The Gig Economy and Crime*

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Abstract

This study investigates how the expansion of gig work affects the level and geographic distribution of crime by leveraging the rapid expansion of the largest food delivery platform in Brazil. While a positive labor market shock can increase the opportunity cost of crime, greater work flexibility may make it easier to shift between legal work and criminal activity. Moreover, delivery work could alter the spatial incidence of crime by hiring low-skilled workers to deliver goods across the city. Exploiting the staggered rollout of the delivery platform alongside 6.2 million geocoded property crime and robbery incident reports from São Paulo state, I find that the rise in gig work reduced municipality-level criminal incidents by 9.8%. The effect is largest at peak demand hours, when the returns to delivery work are highest, providing rare direct evidence for the opportunity cost channel central to economic models of crime. Within cities, the effect is concentrated in lower-income neighborhoods where delivery workers disproportionately reside. I find no evidence that crime is displaced toward high-income neighborhoods or areas with high delivery demand.

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1 Introduction

The digital revolution of the twenty-first century has led to new, nontraditional work arrangements, commonly referred to as "gig" work. These jobs are mediated by digital platforms and allow workers to flexibly choose when and how much to work. The increase in gig work, and in particular jobs provided by transportation and delivery platforms, has quickly transformed labor markets for low-skilled workers. In Brazil, the setting of this study, transportation and delivery apps employed approximately 1.5 million drivers in 2022—a figure that grew by 984% over the previous five years (IPEA, 2022). This represents 31% of the population employed in the transportation sector, roughly 1% of the total workforce, and a much larger share of low-skilled employment.

This paper studies the expansion of delivery work to examine how the rapid rise of flexible employment affects crime. *Ex ante*, the relationship is ambiguous. On one hand, economic models of crime treat criminal activity and legal work as mutually exclusive and predict that positive labor market shocks reduce crime by raising its opportunity cost (Becker, 1968; Ehrlich, 1973). Since delivery work creates opportunities for young, low-skilled men—precisely the group most likely to commit "blue-collar" crime—the rise of gig work may have reduced criminal activity. On the other hand, flexible gig work may make employment easier to combine with criminal activity, thereby increasing crime (Jacob and Lefgren, 2003; Dahl and DellaVigna, 2009). Moreover, delivery work in particular may change not only the *level* of crime but also its geographic distribution. By moving low-skilled employment out of low-income neighborhoods and spreading it throughout the city, delivery work could relocate crime by exposing workers to new opportunities for criminal activity, especially in segregated cities where mobility is limited. It is therefore necessary to turn to data in order to understand how the rise of gig work affects the level and geographic incidence of crime.

I exploit the staggered expansion of Brazil's largest food delivery platform, iFood, across municipalities in the state of São Paulo to examine how the growth of platform work affects both the level and spatial distribution of crime. iFood was the first delivery platform introduced in Brazil and accounts for nearly the entire food-delivery market in the country, so its entry into a municipality effectively marks the arrival of platform delivery work itself. Moreover, São Paulo provides an ideal setting: it contains numerous large cities with high crime rates and strong demand for delivery services, and platform work grew substantially from 2012 onwards, the sample period of this study.

To measure criminal activity, I take advantage of the fact that São Paulo's Department of Public Safety publishes unusually rich incident-level data on financially motivated crime, the category most likely to respond to changes in labor market opportunities. These data include 6.2 million incident reports over ten years and cover both property crime, in which the offender does not directly confront the victim (e.g., burglary or theft of a parked vehicle), and violent crime, in which force or the threat of force is used (e.g., mugging or armed robbery). Incident reports are geocoded and time-stamped, so I can investigate how the effect of gig work on crime varies across neighborhoods within each municipality and across hours of the day. Time-stamped crime data are particularly useful for

determining whether the change in the opportunity cost of crime is an important mechanism. Since the returns to delivery work vary systematically throughout the day due to heightened demand at mealtimes, and since gig workers themselves choose when to work, I can test whether criminal activity responds to hourly variation in opportunity costs. Despite the popularity of opportunity cost models, this mechanism has proven difficult to isolate using variation in traditional jobs, where economic shocks (e.g., mass layoffs) affect many potential determinants of crime and where working hours are imposed rather than chosen (Jacob and Lefgren, 2003; Rose, 2018).

Before turning to the effect of gig work on crime, I first verify that iFood's entry increased delivery work. Using Brazil's national household survey, PNAD Contínua, I identify individuals whose occupation is self-employed motorcycle delivery driver. Because municipality-level employment data do not exist for Brazil, I estimate this effect at the state level, exploiting variation in the population-weighted share of each state's residents living in municipalities where iFood was already operating. I find that the full rollout of iFood within a state is associated with approximately 32 additional self-employed motorcycle delivery drivers per 100,000 residents, and this relationship is not preceded by any pre-existing trend in delivery employment.

The main results examine how an expansion in delivery work affects crime. The empirical strategy compares crime trends in municipalities before and after iFood's entry, exploiting variation in the timing of entry across all municipalities within São Paulo to construct a difference-in-differences estimator. The key identifying assumption is that, absent iFood's entry, treated municipalities would have followed the same crime trends as later-treated and never-treated municipalities; that is, entry timing is uncorrelated with unobserved determinants of municipal crime trends. I provide support for this assumption by showing that treated and comparison municipalities exhibit parallel pre-trends in criminal activity and by drawing on qualitative interviews with platform officials.

I find that the introduction of the delivery platform reduced municipality-level criminal offenses by 9.8%, equivalent to 437 fewer offenses per municipality-year on average. The result is similar (12.9%) when crime is measured per capita using time-varying population, which rules out the possibility that the decline in counts reflects population dynamics rather than a genuine reduction in crime. Benchmarked against the number of delivery jobs estimated above, this magnitude implies 3.6 fewer crimes per year for each delivery worker. This negative effect is largest for property crime, which declines by 15.8%, but also extends to violent crime, which declines by 4.0%. I find no evidence of spatial spillovers to neighboring municipalities. A reduction in crime of this size is noteworthy, given how few policies have been shown to improve public safety in Latin America and other developing regions where crime is persistently high (IADB, 2015; IPA, 2021).

The timing of the effect closely tracks the expansion of the delivery market after iFood's entry. Event-study estimates show no preexisting differences in crime between treatment and control (i.e., later- and never-treated) municipalities prior to the entry of iFood. A negative effect emerges in the year after iFood's entry and grows over time, consistent with the ramp-up in supply and demand for delivery services after iFood's arrival. This pattern is robust to estimators that account for

heterogeneous treatment effects under staggered adoption (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021). In sensitivity analyses, I show that the results are not affected by municipality-level trends correlated with population or income, and that they are similar across various sample restrictions, including after dropping municipalities below a series of population thresholds.

Having documented that the expansion of delivery work reduces crime, I turn to the mechanisms behind the effect. Economic models of crime contend that positive labor market shocks reduce crime by raising its opportunity cost (Becker, 1968; Ehrlich, 1973), but this channel is typically challenging to isolate for two reasons. First, labor market shocks also change other determinants of crime, such as leisure time, mental well-being, and income (Rose, 2018). Second, because working hours are usually imposed rather than chosen, a reduction in crime during those hours could reflect incapacitation rather than any response to the time-varying returns to work (Jacob and Lefgren, 2003). For the same reason, outside of working hours, or for those unable to find work, time spent on crime comes at the expense of leisure rather than paid work, so the prevailing wage does not measure the opportunity cost of crime. The features of delivery work described above address both challenges. Because the returns to delivery work vary across the day, comparing effects across hours separates the opportunity cost of crime from changes in income, well-being, and other determinants of crime that are unlikely to vary sharply from one hour to the next. Because drivers choose when to be active on the app and whether to accept each order, time spent working reflects a decision to work rather than an imposed schedule. If a higher opportunity cost of crime is driving the results, effects should be stronger when the returns to delivery work are larger.

Using information on the exact time when each offense was committed, I disaggregate the main effect by time of day to test whether effects are larger in magnitude when the opportunity cost of crime is higher. To measure hourly variation in opportunity costs, I use e-receipt data purchased from a large data provider; intuitively, high demand hours are concentrated around mealtimes. I find that effects are significantly larger when delivery demand is strongest. For violent crime, the negative effect of iFood appears only at high-demand hours, consistent with potential offenders responding to the hour-by-hour opportunity cost of crime. Effects on property crime are also present when delivery demand is low, suggesting that for less serious offenses the total income from delivery work is enough to deter offending. The results are not driven by the fact that consumers stay at home when ordering delivery, which could reduce outdoor victimization opportunities: using information on the location of each crime from the criminal incident reports, I show that the effect is similar for both indoor and outdoor offenses. The results are also not driven by other factors correlated with the baseline distribution of crime across hours: the effects are very similar after controlling for the baseline hour-level crime rate interacted with the treatment indicator. Together, these results indicate that potential offenders factor in hour-by-hour opportunity costs.

Next, I investigate how the introduction of delivery work affects the spatial incidence of crime. While the municipality-level effect of gig work is negative, it may have had heterogeneous and even opposite-signed effects across neighborhoods. Delivery drivers tend to be low-skilled workers who

reside in lower-income neighborhoods. Thus, the reduction in crime may be concentrated in poorer neighborhoods. At the same time, delivery work is spatially mobile: drivers move throughout the city rather than working from a fixed location, and could carry crime with them toward the areas where delivery activity is most intense. This mobility could also present new opportunities for criminal activity. In particular, delivery work may give potential offenders information about neighborhoods they would otherwise rarely visit, in the same way that shocks to commuting and neighborhood exploration can permanently affect economic behavior (Larcom et al., 2017). Brazilian media outlets regularly report crimes committed by individuals wearing delivery uniforms (Figure A1), and the phenomenon is prominent enough that iFood maintains a standing public statement addressing it (Figure A2). Both possibilities are consistent with a well-established literature in criminology that documents that offenders tend to commit crime close to where they live or work, and in places they know from their routine activities (e.g., Brantingham and Brantingham, 1981; Bernasco, 2010; Bernasco et al., 2015; Menting et al., 2020; Kirchmaier et al., 2024).

To test whether the main effect is concentrated in low-income neighborhoods, I combine the geocoded location of each crime with gridded data on income per capita and estimate the effect of iFood's introduction separately for grid cells in each quartile of the income distribution. The effect is largest in the lowest-income neighborhoods, where it is roughly three times the magnitude of the effect in the highest-income neighborhoods. This pattern is especially pronounced for violent crime: while the effect on property crime is present across the income distribution, the effect on violent crime is concentrated almost entirely in the two poorest quartiles and is close to zero in richer neighborhoods. Thus, the main result is driven by a reduction in crime in low income neighborhoods, where delivery workers tend to live and would likely otherwise be employed. There is no evidence of offsetting, positive effects in high-income neighborhoods. Since the lowest-income neighborhoods experience substantially more crime—and especially violent crime—the rise of delivery work narrows the preexisting gap in neighborhood safety.

To test whether delivery work's spatial mobility shifts crime toward the areas where drivers work, I combine the geocoded location of each crime with geocoded delivery addresses in the iFood e-receipt data, and divide the state into grid cells of roughly 1 km^2 to identify local areas where delivery activity took place during the sample period. I then estimate the effect of iFood's introduction separately for cells with recorded deliveries and cells with none. If drivers' new mobility were shifting crime toward the places they now frequent, the effect should be smaller, or reversed, in the cells with deliveries. Instead, the effect is virtually identical across the two groups. To ensure these average differences are not still masking differential effects in the grid cells with the most delivery activity, I restrict the sample to grid cells with progressively more orders and re-estimate the effect. The reduction in crime is very similar throughout, even in local areas above the 90th percentile of iFood deliveries. This pattern also holds in municipalities with above-median income inequality, where residential segregation implies that delivery work brings drivers into contact with neighborhoods that differ more substantially from those in which they would otherwise have worked.

I therefore find no evidence that crime relocates toward high-delivery areas.

Taken together, the results indicate that the expansion of delivery work generated a meaningful and persistent reduction in crime, rather an increase due to potential synergies between gig work and criminal activity. The effect is strongest during hours when delivery work is most lucrative—consistent with criminal activity declining when its opportunity cost increases—and is concentrated in the lower-income neighborhoods where drivers disproportionately reside. Unlike many existing policies designed to increase public safety, the expansion of platform work seems to have reduced violence without displacing criminal activity across neighborhoods or regions.

Related Literature This study is related to past work examining the economic impacts of gig work (e.g., Chen et al., 2019; Angrist et al., 2021; Cook et al., 2021; Scarelli, 2024; Papp, 2024; Plotkin, 2024). Prior studies have documented that workers value the real-time flexibility and payment structure of ride-hailing platforms (Chen et al., 2019; Angrist et al., 2021; Scarelli, 2024), that gig work's flexibility helps close gender pay gaps (Cook et al., 2021), and that delivery platform adoption reshapes formal employment and the distribution of earnings across workers in Brazil (Plotkin, 2024).[1] I investigate the effect of gig work on crime, which is important given the rapid and widespread rise of flexible jobs, especially in developing countries with high crime rates.

This study also builds on existing evidence on the relationship between labor market conditions and crime. Financially motivated crime has been shown to increase following mass layoff events (Khanna et al., 2021; Britto et al., 2022) and episodes of trade liberalization (Dix-Carneiro et al., 2018; Dell et al., 2019).[2] Crime and violence also respond to changes in the returns to legal work driven by commodity price shocks (Dube and Vargas, 2013) and to youth summer employment programs (Gelber et al., 2016; Davis and Heller, 2020). Most related, concurrent work by Allouard et al. (2026) studies the introduction of the UberEats platform in France and finds that increased rider availability reduces drug-related crime.

This paper makes three contributions to this literature. First, it examines the market-level effect of a large shock to flexible jobs, which could be very different from the effect of shocks to traditional, full-time employment. It does so in a developing-country setting, where gig work has expanded rapidly and crime is a protracted policy challenge. Second, time-stamped crime data allow me to isolate the opportunity-cost channel—the primary mechanism through which improved labor market conditions reduce crime in economic models of criminal behavior—providing a rare test of this central mechanism. Third, it characterizes neighborhood-level effects and how shifting the location of low-skilled work affects the spatial distribution of crime.[3] This is of central importance

[1] For a recent review of this literature, see Mas and Pallais (2020).

[2] Focusing on high-income countries, Machin and Meghir (2004) find that decreases in low-skilled wages during the 1970s in the United Kingdom increased crime and Gould et al. (2002) show a negative relationship between the labor market prospects of unskilled American men during the 1980s-1990s and crime rates. Other work investigates whether these effects can be mediated by benefit payments (Watson et al., 2020; Foley, 2011; Khanna et al., 2023).

[3] This also relates to a smaller body of work that investigates how density and transportation affect the spatial distribution of crime within cities (Glaeser et al., 1996; Khanna et al., 2022; Lafrogne-Joussier and Rollet, 2023).

given the possibility that gig work may have competing (and even opposite-signed) effects across neighborhoods. Indeed, Brazilian media coverage has linked the growth of delivery work to a *rise* in crime in high-income neighborhoods (Henrique, 2022; Saleme, 2024), and urban policies to reduce segregation often face resistance due to fear of exactly these negative spillovers (e.g., Skogan, 1995).

Finally, this paper relates to existing work on the causes of Latin America's persistently high crime rates and the effects of public policies aimed at reducing them (e.g., Di Tella and Schargrodsky, 2004; Soares and Naritomi, 2010; Mejia and Restrepo, 2013; Blattman et al., 2021; Sviatschi, 2022). Several studies investigate this topic in Brazil in particular (Monteiro et al., 2020; Ferraz and Schiavon, 2022; Pezzuchi et al., 2024; Guerra et al., 2025; Soares and Souza, 2025; Mancha et al., 2025). Most policies that successfully reduce crime rely on increasing police presence or pacifying gang-controlled territories, but these strategies are costly and can displace crime to other areas or backfire (see Dell, 2015; Bellégo and Drouard, 2024; Pereira et al., 2024). The finding that the expansion of gig jobs significantly reduces crime suggests an alternative way to improve public safety by expanding access to a flexible and easily accessible source of income to low-skilled individuals.

The paper is organized as follows. Section 2 describes the delivery platform and the data. Section 3 estimates the effect of iFood's entry on delivery work. Section 4 introduces the empirical strategy and reports the main results. Section 5 investigates mechanisms, and the last section concludes.

2 Background and Data

2.1 The Delivery Platform

iFood introduced its delivery platform in 2012, the first of its kind in Brazil, and has grown rapidly since. It currently has about 63 million users and accounts for 87% of the food-delivery market.[4] This dominance matters for the analysis: because iFood accounts for nearly all platform-based food delivery in Brazil and was also the first firm to enter, a comparison of municipalities with and without iFood is effectively a comparison of municipalities with and without delivery-platform work, and the estimates are largely uncontaminated by the presence of competing platforms.[5] In 2020, iFood employed 537,964 delivery drivers (roughly 0.5% of the Brazilian workforce) (Haddad et al., 2023). Registration is straightforward: anyone with a vehicle (motorcycle or bicycle) and an identification document can sign up. Drivers freely choose when to accept orders and are paid per delivery. Previous research shows that these jobs created opportunities for unemployed workers and raised hourly wages for those previously in other occupations. According to Callil and Picanço (2023), about one-third of delivery drivers were unemployed before joining a platform. Hourly earnings are 65% higher than in alternative jobs; on average, drivers earn 25 reais per hour (∼$7

[4] According to Statista estimates. See the link here.

[5] During the study period, iFood shared the market with two other large players: UberEats and Rappi. Both entered the Brazilian market in 2017, much later than iFood, and only served a handful of municipalities that already had iFood services.

USD), and those relying solely on delivery work make 2.5 times the monthly minimum wage. A 2022 survey of drivers reinforces this finding: earnings were the second most cited advantage, confirming the financial appeal of delivery work.[6]

This expansion represented a positive labor market shock disproportionately available to young, low-skilled men. According to the most recent national prison census (Brazil, Ministério da Justiça e Segurança Pública, Departamento Penitenciário Nacional , DEPEN), 95% of individuals incarcerated for theft and 97% of those incarcerated for robbery in Brazil are male. The same census indicates that 58% of incarcerated men nationally are between 18 and 34 years old; the data are not cross-tabulated by age and offense type jointly, so this reflects the incarcerated population as a whole rather than property offenders specifically, though it is suggestive that the population most relevant here skews young as well. In 2020, 95% of iFood drivers were male, with an average age of 29; about half had not finished high school, and only 4% had some tertiary education (Callil and Picanço, 2023). These characteristics closely mirror those of individuals incarcerated for financially motivated crime, the focus of this study. This demographic overlap means delivery work represents a labor market opportunity specifically available to individuals at elevated risk of offending; its introduction should therefore be expected to reduce crime to the extent that it raises their income.

However, the expansion of delivery work could also make crime *more* likely. First, the flexibility of the job means that potential offenders may be able to switch back and forth more easily between criminal activity and employment. They can stop work as soon as a criminal opportunity presents itself, and return to work thereafter. Moreover, the job's structure means drivers have idle time between deliveries, the motorcycle allows a quick approach and escape, and the uniform provides a form of cover: a person on a motorcycle wearing an iFood bag attracts no attention in any neighborhood in the city, at any hour. Second, delivery brings young, low-income workers into neighborhoods they might otherwise rarely visit. This could also increase opportunities for criminal activity or shift the geographic distribution of crime toward higher-income, high-demand neighborhoods. Indeed, news reports frequently highlight crimes committed in delivery uniforms (Figure A1), especially in high-income zones. This narrative has become so prominent that iFood maintains an official statement on its website addressing it (Figure A2).[7] Anecdotal evidence therefore suggests that the entry of iFood could increase crime in some neighborhoods, underscoring the need for neighborhood-level estimates to understand how the rapid growth of delivery work affects crime.

2.2 Data

The analysis requires data on criminal activity, the expansion of iFood, and a range of other neighborhood and municipality characteristics. Here, I describe each in turn.

[6]The survey appears in Callil and Picanço (2023).

[7]The statement was issued after the killing of a São Paulo resident by a robber posing as a delivery driver; iFood confirmed the suspect had never been registered as a driver on the platform. See iFood's statement.

Crime Crime data come from São Paulo State's Department of Public Safety (SSP-SP) and cover all precinct-filed incident reports of financially motivated crime involving at least one vehicle or cell phone. This includes property crime, in which the offender takes something without directly confronting the victim (pickpocketing, shoplifting, burglary, theft of a parked car or motorcycle, or personal items lifted from a person) and violent crime, in which the victim is confronted and force or a weapon is used (muggings, armed robbery, carjacking, home invasion, etc.). Each record lists the offense type, date and time, address, location, and geographic coordinates of the occurrence. The reports also indicate whether the crime occurred in an indoor establishment or outdoors in a public space or street. I compile all reports from 2010–2019 by cleaning and merging separate monthly, yearly, and crime-type files to build a dataset that records the total number of crimes in a municipality and year. The majority of all incidents are also geolocated (87%) and time-stamped (74%), making it possible to assign each incident to a specific neighborhood and hour of the day.[8][9]

I focus on financially motivated crime for two reasons. First, it is both the most prevalent category of offense and the one most likely to respond to changes in labor market conditions. By volume, my data alone record an average of over 600,000 offenses per year in São Paulo state, compared to 153,222 cases of intentional bodily injury (assault) and 2,630 homicides recorded in the state in 2024, for example (Fórum Brasileiro de Segurança Pública, 2025).[10] In addition, this type of crime is, by definition, committed for material gain. Second, it is the only crime category for which SSP-SP incident-level data are available consistently over the full sample period.

Several factors suggest these data are likely to accurately capture patterns of criminal activity. First, reporting costs are low: victims had the option of submitting an online report throughout the study period without the need to travel to a precinct. Second, victims of financially motivated crime often have an incentive to report, for instance to file insurance claims or justify absences from work, which is another advantage of focusing on this type of crime. Third, incident reports do not depend directly on law-enforcement quality, helping ensure data consistency across neighborhoods with varying enforcement capacity. This matters for the analyses in Sections 5.2.1 and 5.2.2, which compare the effect of iFood's introduction across neighborhoods of different income levels and across locations with and without delivery activity.[11] That said, results are robust to controlling for differential trends by municipality GDP and population and to dropping smaller municipalities where data quality may be lower, and the effect remains negative when restricting to violent crime, for which reporting is more likely to be complete.

[8] About two-thirds of incidents are geocoded in the raw data, and I geocode 58% of the remainder from the address of occurrence. Incidents without an exact address are excluded from the within-municipality analyses in Section 5.

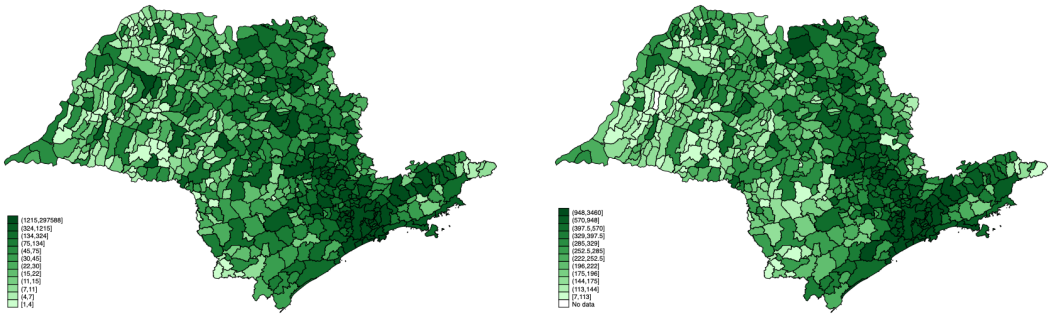
[9] Incidents without a time stamp still record the period of the day. Their distribution across periods is similar to that of time-stamped incidents, so the time-of-day analysis is drawn from a representative subsample (Figure A8).

[10] Unfortunately, SSP-SP does not make incident-level data available for these other types of offenses. Although homicide data are available, they cover only a three-year period, which is insufficient for a panel analysis.

[11] Reporting rates may vary with factors such as trust in the police, which could correlate with municipal wealth (Soares, 2004). Municipality fixed effects absorb time-invariant differences in reporting, so identification would be affected only if iFood's entry itself changed reporting behavior. I am not aware of a channel through which it would do so, and results are robust to controlling for differential crime trends by municipality GDP per capita (Table A1).

Figure 1: Crime Across Municipalities in São Paulo

(A) Count per year **(B) Count per year per 100,000 people**



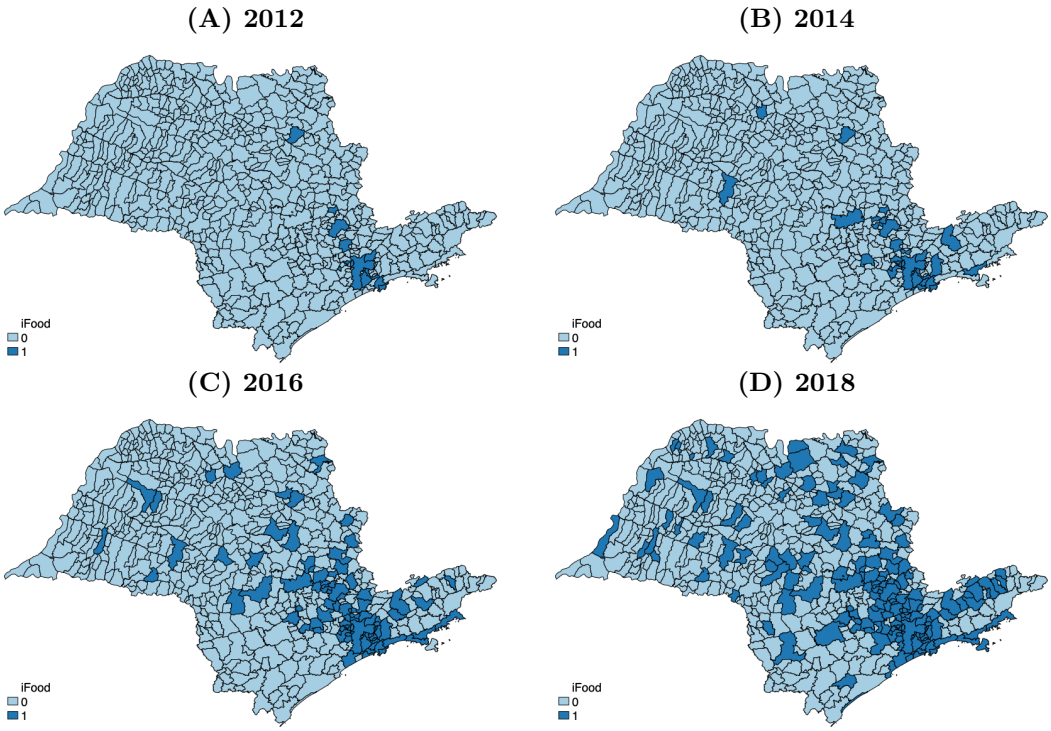
This figure shows the distribution of crime across municipalities in the state of São Paulo. In Panel 1a, municipalities are shaded according to the average count of criminal offenses registered by precincts per year from 2010 to 2019. In Panel 1b, municipalities are shaded according to the average count of criminal offenses per 100,000 people per year registered by precincts from 2010 to 2019.

The final dataset records 6.2 million criminal offenses over 10 years. Figure 1 illustrates the data by showing the dispersion of crime across municipalities in the state of São Paulo. Panel 1a plots the average annual number of offenses by municipality from 2010–2019, while Panel 1b plots the average per 100,000 residents. Darker shading indicates higher crime rates.

Delivery Platform Entry I use proprietary information from iFood on its expansion, including the year in which it entered each municipality. When iFood starts serving a municipality, both commercial establishments and households within the municipality are granted access to the app. The rollout began in 2012, when the first cities received its services, and continues until the present. A total of 227 municipalities had received services by 2019, indicating that they were treated during the sample period. Figure 2 displays maps of São Paulo with the municipalities served by iFood in 2012, 2014, 2016, and 2018, to illustrate the rollout.

Labor Market Outcomes I use microdata from Brazil's Pesquisa Nacional por Amostra de Domicílios Contínua (PNAD), a quarterly household survey conducted by the Brazilian Institute for Geography and Statistics (IBGE), to estimate the number of self-employed motorcycle delivery drivers in each state and year. I identify delivery drivers as individuals whose occupation is coded as motorcycle driver, whose main economic activity is cargo transport or courier and parcel delivery services, and who report being self-employed. For each state-quarter, I sum the survey weights of respondents meeting this definition to obtain a population-weighted estimate of the number of delivery drivers, and average the four quarterly estimates within each year to construct an annual state-level series. These data are available from 2012, when the survey's quarterly redesign was first introduced. I use these data to investigate the impact of iFood coverage on delivery employment.

Figure 2: Rollout of iFood Over Time



This figure shows the municipalities that have iFood services in 2012 (Panel 2a), 2014 (Panel 2b), 2016 (Panel 2c), and 2018 (Panel 2d).

Delivery Platform Demand I use e-receipt data for iFood purchased from Measurable AI, the largest provider of e-receipt data across emerging markets, to measure delivery demand across hours of the day and delivery activity across neighborhoods. Measurable AI owns MailTime, an email productivity app which helps de-clutter mailboxes and prioritize emails with an interface formatted to imitate text-messaging. They aggregate and anonymize transactional data from the mobile devices of their app's users and translate it into data that contain information on the date and time of the transaction, the commercial establishment (pickup) address, and the geocoded delivery address. The final dataset contains about 400,000 purchases on the iFood platform over 8 years.

Municipality and Neighborhood Characteristics Additional information on municipality and neighborhood characteristics comes from various sources. Municipality shapefiles and municipality population are retrieved from the Brazilian Institute for Geography and Statistics (IBGE), and are used to make maps of the distribution of crime across municipalities (Figure 1) and the expansion of iFood over time (Figure 2). To investigate heterogeneous effects across neighborhoods with different

socioeconomic status, I use 2010 gridded GDP data from Kummu et al. (2025)[12] and population data from the Center for International Earth Science Information Network (CIESIN, 2018).

3 The Effect of Delivery Platform Entry on Gig Work

Before turning to the effect of iFood's entry on crime, I examine whether it in fact increased delivery work, and by how much. This exercise serves two purposes: it validates that individuals are taking up delivery work in response to the platform's entry, and it provides a benchmark for interpreting the magnitude of the crime results in Section 4. The ideal test would mirror the empirical strategy in Section 4.1, examining employment at the municipality level. However, panel data on employment at the municipality level do not exist for São Paulo or for Brazil. Instead, Brazil's national household survey, the Pesquisa Nacional por Amostra de Domicílios Contínua (PNAD Contínua) identifies only the state of each respondent.

Using these data, I estimate a second-best specification at the state level, exploiting variation over time in the population-weighted share of each state's residents living in municipalities that had received iFood services. I construct Coverage_{st}, the share of state s's baseline (2010) population residing in municipalities where iFood was operating as of year t, using the same municipality-level rollout data and treatment-timing definition as the main analysis. I then estimate

$$Y_{st} = \alpha_s + \delta_t + \beta \cdot \text{Coverage}_{st} + \epsilon_{st}, \tag{1}$$

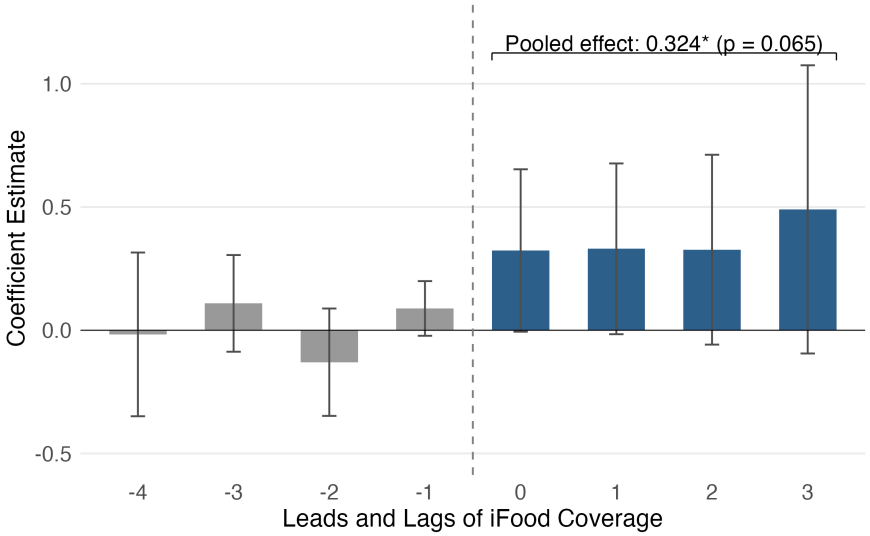
where Y_{st} is the number of self-employed motorcycle delivery drivers per 100,000 residents in state s in year t, and α_s and δ_t are state and year fixed effects. I identify delivery drivers in PNAD Contínua using each respondent's reported occupation and activity codes: individuals whose primary occupation is coded as motorcycle driver, whose main activity is cargo transport or courier and parcel delivery, and who report being self-employed.[13] The sample covers all Brazilian states from 2012 to 2019, with the regression weighted by state population to make the estimate more representative of larger urbanized states like São Paulo, where the main analysis takes place, and to ensure the results are not driven by small states. Standard errors are clustered by state.

The estimated coefficient on Coverage_{st} is 0.324 ($p = 0.065$), implying that a full increase in iFood coverage from zero to 100 percent translates into approximately 32 additional delivery workers per 100,000 residents. This estimate is likely to be a lower bound for two reasons. First, PNAD Contínua's occupation classification applies only to each respondent's main job by hours worked, so individuals for whom delivery work is a secondary or supplementary source of income are not

[12] Starting from subnational GDP-per-capita data, the authors use machine-learning models incorporating population, urbanisation, income inequality, and accessibility to spatially downscale GDP, which is then combined with gridded population data to estimate total GDP at the grid-cell level.

[13] PNAD Contínua is a quarterly rotating panel survey; I average the four quarterly estimates within each year to construct Y_{st}.

Figure 3: Leads and Lags: Effect of iFood Coverage on Delivery Work



This figure shows the relationship between state-level iFood population coverage and the share of the population employed as self-employed motorcycle delivery drivers, estimated separately for each period relative to the current year. Each coefficient comes from a separate regression of delivery-worker share (per 100,000 residents) on the lead or lag of iFood coverage (in percentage points), controlling for state and year fixed effects and weighting by state population. The sample is Brazilian states, 2012–2019, using PNAD Contínua data. Standard errors are clustered by state and 95% confidence intervals are reported.

captured. Second, the CBO occupation code used to identify delivery drivers (8321) covers motorcycle drivers specifically; delivery workers who use bicycles are not captured in this measure.[14] Nevertheless, this measure provides a useful benchmark for approximating the number of full-time delivery workers generated by iFood's expansion.

Figure 3 presents the same relationship estimated separately for each year leading up to and following a state's own increase in iFood coverage. That is, each bar represents a separate estimate of Equation 1 in which I replace Coverage_{st} with its leading or lagged value. Leading coefficients are small, statistically insignificant, and show no systematic trend, consistent with the identification assumption that treatment timing is not driven by a pre-existing trajectory in delivery-sector labor supply. Coefficients in the periods following are of a similar order of magnitude to the pooled estimate and increase slightly over time.

[14]This measure also excludes any indirect employment the platform may generate, for instance in restaurants that expand in response to delivery demand. Reassuringly, Plotkin (2024) finds that the expansion in other restaurant employment is very limited.

4 Main Results

4.1 Empirical Strategy

This section investigates the effect of the introduction of flexible delivery work on crime in São Paulo state. The identification strategy compares the difference in crime before and after the introduction of iFood in municipalities that receive delivery platform services earlier versus later. The main estimating equation is:

$$\text{Crime}_{mt} = \exp\{\alpha_m + \delta_t + \beta \cdot \mathbb{1}_{mt}^{Post} + \epsilon_{mt}\}, \tag{2}$$

where the unit of observation is a municipality m in year t, and α_m and δ_t are municipality and time fixed effects, respectively. $\mathbb{1}_{mt}^{Post}$ is a binary variable that equals zero before iFood is introduced in the municipality and one after. The dependent variable is the count of crime in a municipality-year, and as a result the preferred specification is estimated using Poisson pseudo-maximum likelihood (see Wooldridge, 2023). I verify that results are similar using Ordinary Least Squares (OLS), with the dependent variable measured as the number of criminal offenses per 100,000 residents. Standard errors are clustered at the municipality level.

The coefficient of interest is β, which captures the effect of the introduction of iFood on crime. The sign of β could hypothetically be positive or negative, depending on the relative strength of the forces associated with an increase in delivery jobs. Increases in total income and the opportunity cost of criminal activity should decrease crime, whereas potential increases in leisure time, the fact that a flexible job can be combined with criminal activity, and the shift in employment location as a result of delivery work are reasons crime might increase.

To keep the sample of municipalities consistent across the main results and mechanism sections—which requires variation in criminal activity across neighborhoods and across hours of the day—and to ensure that the results are not driven by municipalities with very low crime rates, the baseline sample is restricted to municipalities with at least 10 crimes per year. This yields 135 municipalities, of which 114 receive iFood by 2019 and 21 do not. Figures A6 and A7 show that results are not sensitive to this restriction, and also remain very similar if I impose more stringent thresholds.

The identification assumption is that municipalities that receive iFood earlier and later would be on the same crime trend absent the introduction of the delivery platform. Consistent with the identification assumption, I find no indication in personal interviews with practitioners that crime or other social outcomes affected platform entry decisions, which were only concerned with the size of the potential market for their services.[15] Accordingly, I also verify that the results are not driven by trends correlated with several proxies for market size. Empirically, the lack of preexisting differences in crime between municipalities "treated" earlier and later also provides support for this assumption.

[15]Personal interviews conducted with M. Biggi, former manager at UberEats (one of iFood's main competitors during the study period), May 31st, 2023, and iFood, May 15th, 2025.

Table 1: Effect on Crime

	Dependent Variable is		
	All Crime	**Property Crime**	**Violent Crime**
	(1)	(2)	(3)
	Panel A: Count of Crime (PPML)		
Post iFood (=1)	-0.103***	-0.172***	-0.0404*
	(0.0180)	(0.0319)	(0.0223)
Municipality Fixed Effects	yes	yes	yes
Year Fixed Effects	yes	yes	yes
Dependent Variable Mean	4463	1940	2524
	Panel B: Crime per Capita (OLS)		
Post iFood (=1)	-116.5***	-83.55***	-32.91*
	(31.13)	(16.89)	(19.79)
Municipality Fixed Effects	yes	yes	yes
Year Fixed Effects	yes	yes	yes
Dependent Variable Mean	905	454	451
Observations	1,350	1,350	1,350

The unit of observation is a municipality–year. The sample includes all municipalities that record at least 10 criminal offenses in every year of the sample period. The dependent variable in column 1 includes all types of crime, columns 2 and 3 restrict to property and violent offenses, respectively. Panel A estimates are obtained by Poisson pseudo-maximum likelihood, with the dependent variable measured as a raw count. Panel B estimates are obtained by OLS, with the dependent variable measured as crime per 100,000 residents, using time-varying annual population estimates. "Post iFood" equals one for municipality-years after iFood is introduced in the municipality and zero otherwise. All specifications include municipality and year fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Following recent recommendations on staggered difference-in-differences designs, results are also estimated using the methods of Callaway and Sant'Anna (2021) and Sun and Abraham (2021), which allow for heterogeneous effects across municipalities and over time.

4.2 Gig Work Reduces Crime

4.2.1 Baseline Results

Table 1 reports the main results, estimates of Equation 2. Panel A uses the annual count of criminal offenses by municipality as the dependent variable. In column 1, the coefficient of interest is negative and statistically significant, indicating that the introduction of the delivery platform reduces financially motivated crime by 9.8% on average—equivalent to about 437 fewer crimes per municipality-year. Panel B, column 1 re-estimates the baseline specification using the number of criminal offenses per 100,000 residents, constructed with time-varying annual population estimates, as the dependent variable in an OLS regression. This addresses two distinct concerns simultaneously:

that the results are driven by more populous municipalities with higher crime counts, and that population growth over the sample period confounds results on crime counts. The results closely match those in Panel A, with the baseline estimate implying a 12.9% decline in crime per capita.

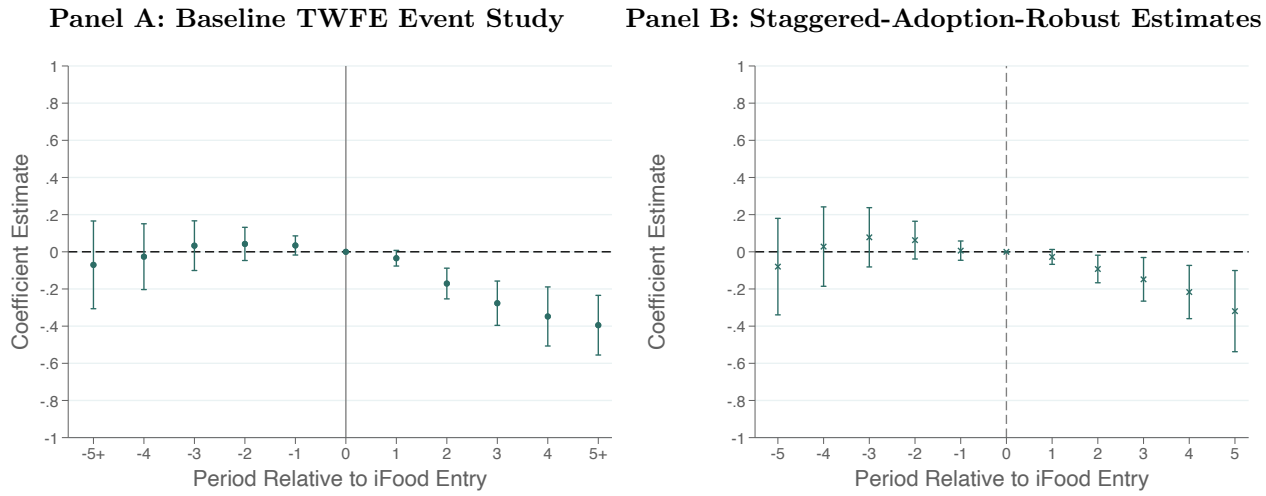
Columns 2 and 3 investigate which types of crime drive the main result. Using information from the incident reports, I separately estimate the effects for property crime, which does not involve direct confrontation with the victim, and violent crime, which involves direct confrontation and the use or threat of force or a weapon. I find that the effect is larger in magnitude for property crime, which falls by 15.8% ($p<0.01$), than for violent crime, which falls by 4.0% ($p=0.07$). This pattern is consistent with the shock having a greater impact on individuals at the margin of committing less serious offenses, resulting in a larger reduction in these crimes relative to violent ones. Panel B confirms this pattern using crime per capita, with property crime falling by 18.4% and violent crime by 7.3%. Although the effect on violent crime is smaller on average, in Section 5.2.1 I show evidence of a large decline in violent crime in the lowest-income neighborhoods, where the overall effect is also concentrated.

The incident reports also record where each offense occurred, which lets me rule out an alternative explanation: that crime falls because food delivery keeps consumers at home, reducing opportunities for victimization. If so, the effect should be concentrated in outdoor crime, particularly thefts of personal items; instead, Table A3 shows that the effects on indoor and outdoor crime are virtually identical, including for thefts of personal items. Thus, the results are not driven by the fact that delivery work makes it more likely for consumers to stay at home.

Using the estimate of the number of delivery drivers per 100,000 residents from Section 3, I can benchmark this decrease in crime against the amount of work the platform's rollout actually created. Full iFood coverage is associated with approximately 32 additional self-employed motorcycle delivery drivers per 100,000 residents, while Panel B, column 1 implies a decline of 116.5 crimes per 100,000 residents—approximately 3.6 fewer crimes for each additional delivery worker per year.[16] The effect is most plausibly driven by a small number of would-be offenders who each account for many crimes per year, rather than each delivery driver deterring roughly 3.6 crimes. This magnitude is not out of line with related evidence from Brazil. Britto et al. (2022) find that job loss increases the probability of committing crime by 23% among displaced male workers, and Dix-Carneiro et al. (2018) find that regions facing larger tariff-induced reductions in employment during Brazil's trade liberalization experienced relative increases in crime of up to 46%. The 9.8% decline in crime that I estimate is smaller in magnitude, as expected: both papers study involuntary negative labor market shocks experienced directly by every affected worker, whereas I study the emergence of a new, voluntary work opportunity that only a subset of residents in a treated municipality choose to take up.

[16]Because the first-stage estimate is likely a lower bound on the true increase in delivery work, as discussed in Section 3, this figure should be read as an upper bound on the number of crimes averted per additional worker.

Figure 4: Event Studies: Effect on Crime



This figure shows treatment effects over time. Both panels are estimated using Poisson pseudo-maximum likelihood, with the dependent variable equal to the number of criminal offenses and the entry year as the reference period. Panel 4a reports a two-way fixed effects event study. Panel 4b reports the interaction-weighted estimator of Sun and Abraham (2021), following Wooldridge (2023): a separate coefficient is estimated for each treatment cohort and event time, using municipalities not treated by 2019 as controls, and coefficients are aggregated across cohorts weighting by cohort size. In both panels, endpoints are binned at ±5 years, the widest symmetric window in which every coefficient is identified from at least three treatment cohorts and 30 treated municipalities. The sample is all municipalities with at least 10 crimes per year, the unit of observation is a municipality-year, standard errors are clustered by municipality, and 95% confidence intervals are reported.

Dynamic Effects Next, I examine the dynamic effects of delivery work on crime. Figure 4 plots event studies associated with Equation 2. Panel 4a reports estimates using Poisson pseudo-maximum likelihood and binning endpoints with sparse data, following Schmidheiny and Siegloch (2023). Consistent with the identification assumption, there are no preexisting differences in crime between municipalities that receive iFood's services earlier versus later: the coefficients for pre-treatment years are small and indistinguishable from zero. The negative effect sets in the first full year of iFood coverage and grows over time, reflecting the continued addition of merchants and consumers, as shown in Figure A5. A version of the event study without binning the first and last years shows the same patterns with wider confidence intervals at the endpoints, where few cohorts contribute to the estimate (Figure A3).

Panel 4b addresses the possibility that the two-way fixed effects estimates are biased under staggered adoption, because already-treated municipalities serve as controls for later-treated ones. It reports the estimator of Sun and Abraham (2021) estimated by Poisson pseudo-maximum likelihood following Wooldridge (2023), so that the coefficients are directly comparable to Panel 4a. The panels are similar: pre-treatment coefficients are small and statistically indistinguishable from zero, and the post-treatment effect grows from 3 percent after one year to 27 percent five years after entry, tracking the two-way fixed effects estimates at every horizon. These patterns also hold when the

outcome is crime per capita, as in Panel B of Table 1, using the estimator of Callaway and Sant'Anna (2021) (Figure A4). The main findings are therefore robust to heterogeneous treatment effects, and the growing magnitude of the effect is not an artifact of the two-way fixed effects estimator.

4.2.2 Robustness and Additional Results

Sensitivity to main sample definition I first confirm that the results are not specific to the baseline sample of municipalities with at least 10 criminal offenses in every year. Figure A6 shows that lowering the threshold to 5, or raising it to 20 or 30 offenses per year, yields very similar coefficients, in both magnitude and precision, for both the count-based and per capita specifications. Figure A7 illustrates why this restriction matters in the first place. I drop the minimum crime count restriction entirely and instead restrict the sample by population, starting from municipalities with at least 50,000 residents and lowering the threshold in five increments until no restriction remains. Each step adds smaller municipalities, which have fewer and more volatile crime counts, so the sequence shows how the estimate behaves as the sample moves from large municipalities with substantial crime to all municipalities in the state. As smaller municipalities are added, the percentage effect grows larger in magnitude while the implied effect on the *level* of crime shrinks—consistent with small crime counts inflating percentage effects. These results suggest that once municipalities record enough crimes for the estimate not to be driven by small, potentially idiosyncratic changes in the underlying count, the effect stabilizes around a 10 percent decrease, supporting the interpretation that this magnitude reflects the effect of iFood's introduction.

Robustness to controls for differential trends by market size The next test probes whether the results are driven by underlying differences between the municipalities iFood enters first and those it enters later. As discussed in Section 4.1, the only factor iFood reports considering in its expansion decisions is market size. If larger markets both received iFood earlier and were on a different crime trajectory for unrelated reasons, the baseline estimates would conflate the two. Table A1 addresses this by interacting year fixed effects with three proxies for market size—municipality GDP, GDP per capita, and population—which allows municipalities of each size to follow their own time trend in crime, so that the effect of iFood's entry is identified only from comparisons among municipalities on the same market-size-specific trend. The estimated effect is very similar across specifications, indicating that differential trends by market size are not driving the results. This also controls for any potential trends in data quality that vary with municipality GDP (Soares, 2004).

Geographic spillover effects Crime could also fall in municipalities adjacent to those that receive iFood, either because some offenders would otherwise have crossed municipal borders to commit crimes, or because residents of untreated municipalities commute to neighboring ones to work as drivers. To test for this, I estimate Equation 2 on municipalities that do not themselves receive iFood through 2018 and that record at least 10 crimes per year, as in the baseline sample, replacing

the treatment indicator with one that turns on once the municipality's earliest-treated neighbor has entered.[17] Table A2 reports the results, with columns 1 and 2 corresponding to the count and per capita specifications of Table 1. Both point estimates are close to zero and statistically insignificant, although imprecisely estimated given the small sample. This is consistent with municipalities being relatively large administrative units, so that crossing municipal borders to commit crime or to work is relatively uncommon. Moreover, to the extent that spillovers are negative, they would only lead the baseline estimates to understate the effect of iFood on crime.

5 Mechanisms

5.1 Opportunity Cost of Crime

Economic models of crime argue that positive labor market shocks reduce crime by raising its opportunity cost: the choice between employment and crime is mutually exclusive, and the opportunity cost of crime is the foregone income from work (Becker, 1968; Ehrlich, 1973). Despite the popularity of these models, there is little direct empirical evidence for the opportunity cost channel, for two reasons. First, large labor market shocks usually affect other determinants of crime, including leisure time, mental well-being, and income, making it impossible to isolate the effect of the opportunity cost itself (Rose, 2018).[18] Second, in most settings time spent working is set by the employer rather than chosen by the worker, so a reduction in crime during working hours could reflect incapacitation (individuals simply being more occupied) rather than an ongoing response to the returns to work. This is also the mechanism through which compulsory schooling and incarceration are thought to reduce crime (Jacob and Lefgren, 2003).

Two features of delivery work allow me to overcome these challenges and develop a test for the opportunity cost mechanism. First, drivers choose each hour whether to be active on the app and whether to accept each order, so time spent working reflects a decision to work rather than offend at that hour, which is precisely the opportunity cost channel. Moreover, delivery work does not physically preclude offending, since drivers move through the city on motorcycles with idle time between orders, and the cost of switching between delivery work and crime within an hour is very low. Second, the returns to delivery work vary predictably across the day. Because iFood is a food delivery platform, demand is heavily concentrated at mealtimes: about half of the orders in the e-receipt data are placed in the three-hour windows around lunch and dinner, so the returns to working are much higher in those windows than at other times. If the opportunity cost channel is operative, we would therefore expect larger reductions in crime when the returns to delivery work

[17] The sample is restricted to municipalities untreated through 2018, rather than 2019, to ensure there are at least 20 municipalities in the sample.

[18] For example, most existing papers that investigate the effect of employment on crime leverage mass layoffs for identification, which also affect the amount of leisure time and mental well-being (e.g., Dix-Carneiro et al., 2018; Dell et al., 2019; Khanna et al., 2021).

are highest. Uniform decreases across the day would instead indicate that potential offenders are not responding to within-day differences in opportunity cost, and would be more consistent with total income from delivery work being sufficient to deter offending at all times.

To test whether increases in the opportunity cost of crime are driving the main result, I separately estimate the effect of the introduction of the delivery platform for each hour of the day, exploiting time stamps in the criminal incident reports. The estimating equation is:

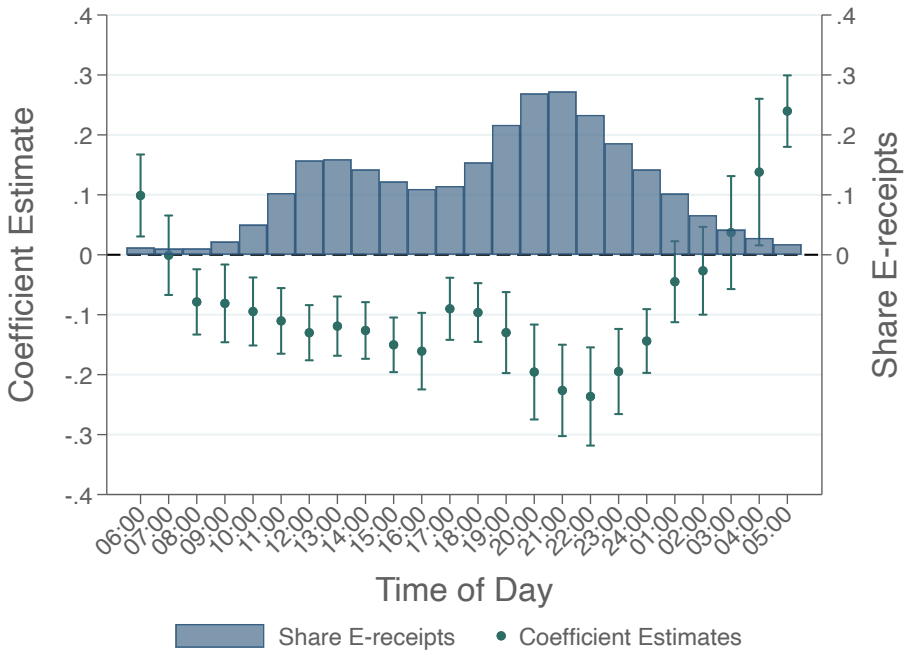
$$\text{Crime}_{hmt} = \exp\{\alpha_h + \delta_m + \phi_t + \sum_{r=1}^{24} \beta^r (\mathbb{1}_{mt}^{Post} \cdot \text{Time}_h^r) + \epsilon_{hmt}\} \tag{3}$$

The unit of observation is a time of day (i.e., hour) by municipality and year, so that $\mathbb{1}_{mt}^{Post}$ can be interacted with an indicator that equals one for each time of day, disaggregating the effect into one-hour windows. The specification controls for municipality, year, and time of day fixed effects, and standard errors are clustered at the municipality level.

Figure 5 overlays the estimates from Equation 3 on a histogram showing the share of orders (e-receipts) at each time of day. The histogram shows that delivery demand is very low during early morning hours, grows steadily from 9:00 a.m. onward until it peaks during lunchtime, declines in the afternoon and peaks once more during dinnertime, when demand is greatest. The effects on crime by time of day, displayed in green, closely mirror fluctuations in delivery demand: the point estimates are negative and larger in magnitude when delivery demand is highest. The point estimates are *positive* from 4 to 6 a.m., when demand is lowest, indicating potential displacement to these hours, though this effect represents only a handful of offenses since crime counts are very low at these times. Such displacement would be consistent with delivery work revealing new opportunities for crime, which potential offenders exploit at the hours when the returns to delivery work are lowest. This pattern suggests that effects are stronger when gains from work are higher. Because the model is estimated using PPML, the larger coefficients at certain times of day reflect proportionally larger reductions in crime, rather than a mechanical effect arising from higher baseline crime levels. That effects are strongest when drivers are most active is also reassuring, as it is unclear *ex ante* why any omitted trend would generate this precise daily pattern of crime reduction.

Table 2 tests formally whether the reduction in crime varies with delivery demand over the course of the day. I estimate a separate regression for each demand quartile, restricting the sample to the time-of-day observations belonging to that quartile alone. This allows the fixed effects—municipality by time of day, and time of day by year—to be estimated separately within each quartile, rather than imposing that their effect on crime is common across quartiles of very different demand intensity. The unit of observation is a time of day by municipality and year, and standard errors are clustered by municipality. The dependent variable in each column is the count of all criminal offenses. In Panel A, quartiles are defined over all 24 hours of the day. The effect is monotonically increasing in magnitude with delivery demand, from a statistically insignificant 5.3% decline in the least busy

Figure 5: Effect on Crime by Time of Day



The blue histogram plots the share of e-receipts belonging to each one-hour interval of the day. The green dots plot the coefficient estimates from a single regression. It is estimated using Poisson pseudo-maximum likelihood and the sample is all municipalities with at least 10 crimes per year. The unit of observation is a time of day by municipality and year. The dependent variable is the number of criminal offenses. Each dot is the coefficient on an interaction between "Post iFood" (a binary variable that equals zero before iFood is introduced and one after) and a binary variable that equals one for each time of day. The regression controls for time of day, municipality, and year fixed effects. Standard errors are clustered by municipality and 95% confidence intervals are reported.

quartile to a 14.7% decline in the busiest quartile of the day, and effects are negative and significant in all but the least busy quartile. Panel B recomputes the quartiles over waking hours only, from 7 a.m. to 10 p.m., so that each quartile compares hours in which both offenders and drivers are plausibly active. The pattern is the same and, if anything, sharper: the effect rises monotonically from a 6.5% decline in the least busy daytime quartile to a 15.6% decline in the busiest. This confirms that the magnitude of the effects increases with the returns to delivery work.

Figure 6 presents the results of the same specification separately for property crime and violent crime. The two respond differently to delivery demand. For property crime (Panel 6a), the effect is negative and significant across all four quartiles, and does not increase monotonically with demand the way the pooled effect in Table 2 does. Delivery job expansion thus reduces property crime even when the opportunity cost of crime is low, consistent with a broad effect of total income on criminal participation rather than one operating only through hour-by-hour changes in the returns to legal work. For violent crime (Panel 6b), by contrast, the effect is indistinguishable from zero in the two

Table 2: Effect on Crime by Quartiles of Delivery Demand

	Dependent Variable is **Crime (count)**			
	Demand Quartile 1	Demand Quartile 2	Demand Quartile 3	Demand Quartile 4
	(1)	(2)	(3)	(4)
	Panel A: All 24 hours			
Post iFood (=1)	-0.0549	-0.0619**	-0.128***	-0.159***
	(0.0344)	(0.0256)	(0.0224)	(0.0332)
Municipality X Time of Day FE	yes	yes	yes	yes
Time of Day X Year FE	yes	yes	yes	yes
Dependent Variable Mean	109	103	139	231
Observations	7,550	7,560	7,560	7,560
	Panel B: Waking hours (7 a.m. - 10 p.m.)			
Post iFood (=1)	-0.0674**	-0.110***	-0.134***	-0.170***
	(0.0276)	(0.0276)	(0.0220)	(0.0351)
Municipality X Time of Day FE	yes	yes	yes	yes
Time of Day X Year FE	yes	yes	yes	yes
Dependent Variable Mean	120	140	144	263
Observations	5,040	5,040	5,040	5,040

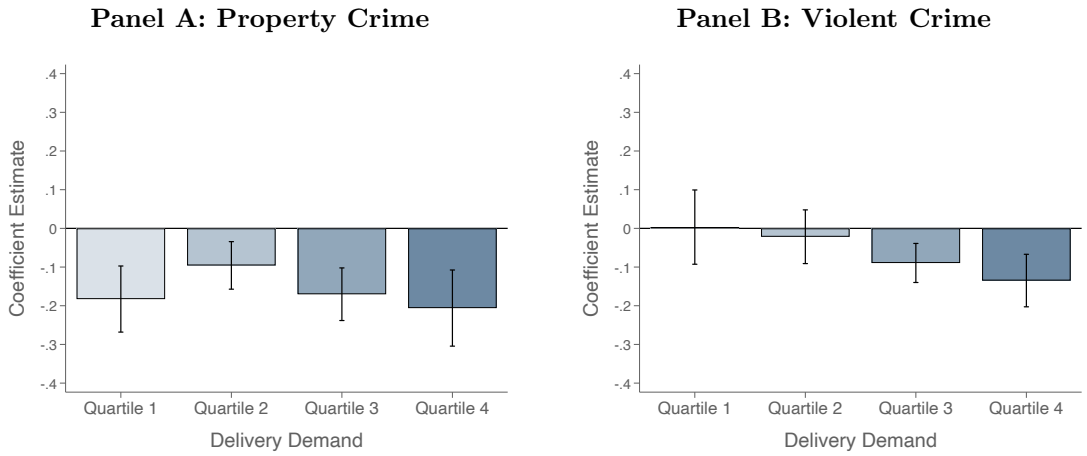
All regressions are estimated using Poisson pseudo-maximum likelihood and the sample is all municipalities with at least 10 crimes per year. The unit of observation is a time of day by municipality and year. The dependent variable is the number of criminal offenses. Each column reports a separate regression estimated on the subsample of time-of-day observations belonging to that quartile of delivery demand, where quartiles are defined by the share of e-receipts in each one-hour interval. In Panel A, quartiles are computed over all 24 hours of the day; in Panel B, quartiles are computed over waking hours only (7 a.m. to 10 p.m.), and overnight hours are excluded from the sample. "Post iFood" is a binary variable that equals zero before iFood is introduced and one after. All columns control for municipality by time of day and time of day by year fixed effects. Standard errors are clustered by municipality. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

least busy quartiles and becomes negative and significant only in the two busiest. Offenders appear to refrain from violent crime only when the opportunity cost of crime is highest. These results suggest that models assuming a mutually exclusive choice between crime and legal work based on the relative returns to each may be better suited to explaining participation in more serious offenses.

Alternative mechanisms I next verify that the time-of-day pattern is not instead driven by fewer opportunities for victimization, or by other factors correlated with how much crime occurs at each hour. The first alternative mechanism is a change in consumer behavior: if households order delivery rather than going out, delivery demand reduces opportunities for outdoor victimization. This channel makes a sharp prediction: the effect should be concentrated in outdoor offenses, and increasingly so at times of high delivery demand. Figure A9 tests this by estimating the quartile-of-delivery-demand specification separately for indoor and outdoor offenses. The effect grows with delivery demand in both panels, and the effect on outdoor offenses is never meaningfully larger than on indoor offenses, which is inconsistent with the consumer-behavior channel.

The second alternative is that hours of high delivery demand differ from other hours in ways

Figure 6: Effect by Delivery Demand Quartile and Type of Offense



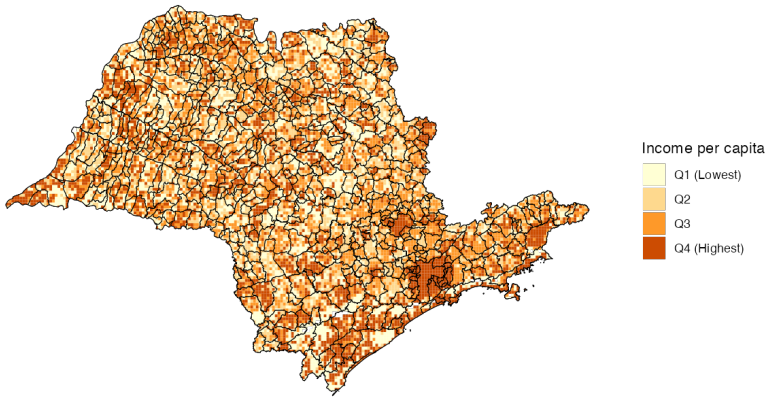
All regressions are estimated using Poisson pseudo-maximum likelihood and the sample is all municipalities with at least 10 crimes per year. Each bar plots the coefficient from a separate regression estimated on the subsample of time-of-day observations belonging to that quartile of delivery demand. The unit of observation is a time of day by municipality and year. The dependent variable is the number of property crimes in Panel A and the number of violent crimes in Panel B. "Post iFood" is a binary variable that equals zero before iFood is introduced and one after. All regressions control for municipality by time of day and time of day by year fixed effects. Standard errors are clustered by municipality. 95% confidence intervals are reported.

unrelated to delivery—for instance, in the overall level of crime—and that these differences, rather than delivery demand, generate the pattern. To address this, I estimate a version of Equation 3 in which $\mathbb{1}_{mt}^{Post}$ is interacted with binary variables for each quartile of delivery demand, and additionally with binary variables for each quartile of the baseline crime distribution across hours. The second set of interactions absorbs any variation in the effect across hours that is driven by factors correlated with the level of crime in each hour. Table A5 reports the results. The coefficients on the delivery-demand interactions closely mirror Table 2 and Figure 6: the effect grows monotonically with delivery demand for total and violent crime, and the difference between the first and fourth quartiles is significant in all three columns ($p < 0.001$). Together, these tests support the interpretation that crime is responding to differences in the opportunity cost of crime over the day.

5.2 Spatial Distribution of Crime

This section examines whether the reduction in crime is spread evenly across neighborhoods within treated municipalities, or concentrated in some and absent, or even reversed, in others. There are two reasons to expect it to be uneven, both resting on the well-documented tendency of offenders to commit crime close to where they spend their time, a pattern known as distance decay (e.g., Brantingham and Brantingham, 1981; Rengert et al., 1999; Kirchmaier et al., 2024). First, delivery drivers are drawn from a population that disproportionately lives in lower-income neighborhoods, suggesting that the reduction in crime may be concentrated where those individuals

Figure 7: São Paulo State Shaded by Income per Capita



This figure shows a map of São Paulo state divided into 0.04° × 0.04° grid cells and shaded according to quartiles of income per capita. Darker shades represent higher-income grid cells.

would otherwise have offended. Second, delivery work is spatially mobile: drivers move throughout the city rather than working from a fixed location, spending time in neighborhoods they might otherwise rarely visit, which could shift crime toward the areas where delivery activity is most intense—a possibility that receives considerable attention in the Brazilian press (Figure A1).

Section 5.2.1 tests the first hypothesis by estimating the effect separately across neighborhoods of different income levels. Section 5.2.2 tests the second by comparing the effect in local areas with varying levels of delivery activity.

5.2.1 Effects Across Neighborhood Income

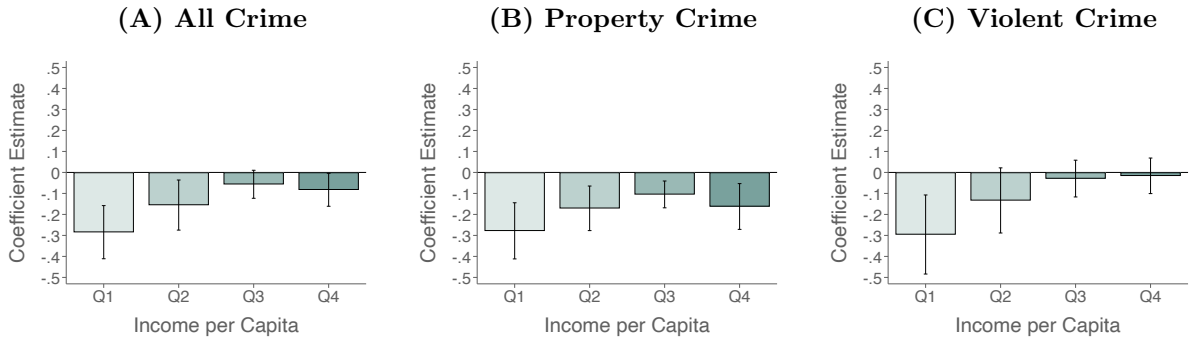
I first examine whether the effect is stronger in lower-income neighborhoods, where delivery drivers disproportionately live and might otherwise have worked. To do this, I divide São Paulo state into 0.04° × 0.04° grid cells (∼20 km² each). The cell size is chosen so that the number of cells per municipality approximates the average number of neighborhoods per municipality while keeping gridded income and population aggregation feasible. I compute income per capita for each cell using gridded data from Kummu et al. (2025). Figure 7 shows São Paulo divided into grid cells and shaded by GDP per capita. The estimating equation is:

$$\text{Crime}_{\ell mt} = \exp\{\alpha_{\ell m} + \delta_t + \rho \cdot \mathbb{1}_{mt}^{Post} + \epsilon_{\ell mt}\}, \tag{4}$$

where ℓ indexes grid cells, m indexes municipality, and t indexes year. Effects are estimated separately for neighborhoods belonging to each of the four quartiles of income per capita.

Figure 8 displays the results. In Panel 8a, the dependent variable is the number of criminal

Figure 8: Effects by Neighborhood Income



All regressions are estimated using Poisson pseudo-maximum likelihood and the sample is all municipalities with at least 10 crimes per year. Each bar plots the coefficient from a separate regression estimated on the subsample of grid cells belonging to that quartile of income per capita. The unit of observation is a grid cell by municipality and year. The dependent variable is the number of criminal offenses in Panel A, the number of property crimes in Panel B, and the number of violent crimes in Panel C. "Post iFood" is a binary variable that equals zero before iFood is introduced and one after. All regressions control for grid cell by municipality and year fixed effects. Standard errors are clustered by municipality and 95% confidence intervals are reported.

offenses; Panels 8b and 8c report the same specification separately for property and violent crime. Given that delivery drivers are predominantly low-skilled young men earning close to the minimum wage in alternative occupations, the expectation is that the effect should be concentrated in the lowest-income neighborhoods, where this population disproportionately lives. This is what Panel 8a shows: the effect is negative in all four quartiles, but is largest in the poorest, where it is 17 percentage points larger than in the richest (p=0.01). Appendix Figure A10 confirms these results by estimating effects over time for above-median- and below-median-income neighborhoods. In neither group is crime trending downward in the years leading up to entry; after entry, the effect size is much larger for lower-income neighborhoods. This pattern mirrors a substantial baseline gap in crime across neighborhoods: the poorest quartile of grid cells records more than twice as much crime per capita as the richest (Table A4). The introduction of the delivery platform therefore alleviates the geographic inequality in safety across neighborhoods.

The concentration of the effect in low-income neighborhoods is especially pronounced for violent crime. In Panel 8b, the effect on property crime is largest in the poorest quartile but remains negative and significant even in the two richest. In Panel 8c, by contrast, the effect on violent crime is concentrated almost entirely in the two poorest quartiles, with estimates in the richer quartiles close to zero and statistically insignificant. These patterns hold, and are if anything more pronounced, when quartiles of income are instead computed within each municipality, capturing relative income differences across neighborhoods in the same municipality (Figure A11). To the extent that offenders commit crime close to where they live, this suggests that violent crime in richer neighborhoods may be committed by a population with little overlap with delivery workers, which would explain why the effect of the platform on violent crime is more sharply concentrated in

Table 3: Effect on Crime in Places with High and Low Delivery Propensity

	Dependent Variable is **Count of**		
	All Crime	**Property Crime**	**Violent Crime**
	(1)	(2)	(3)
	Panel A: High Delivery Propensity Grid Cells		
Post iFood (=1)	-0.107***	-0.188***	-0.0450
	(0.0213)	(0.0311)	(0.0308)
Dependent Variable Mean	183	80.55	102.5
Observations	21,420	21,420	21,410
	Panel B: Low Delivery Propensity Grid Cells		
Post iFood (=1)	-0.128***	-0.192***	-0.0624
	(0.0351)	(0.0334)	(0.0426)
Dependent Variable Mean	53.71	20.48	33.26
Observations	18,100	18,100	18,080
p-value, Panel A = Panel B	0.61	0.93	0.74

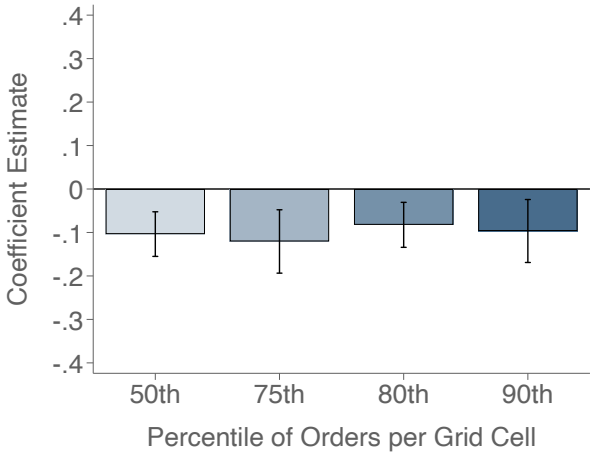
The unit of observation is a grid cell by municipality and year. All regressions are estimated using Poisson pseudo-maximum likelihood. The sample is 1 km^2 grid cells that record at least one crime in every year, in municipalities with at least 10 crimes per year. Panel A restricts to grid cells that register at least one iFood delivery in the e-receipt data during the sample period; Panel B restricts to grid cells that register none. The dependent variable is the number of criminal offenses in column 1, the number of property crimes in column 2, and the number of violent crimes in column 3. "Post iFood" is a binary variable that equals zero before iFood is introduced and one after. All columns control for grid cell by municipality and year fixed effects. Standard errors are clustered by municipality. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

poor neighborhoods. These findings also show that the average effect on violent crime documented in Section 4.2 conceals a much larger effect in the poorest neighborhoods, reinforcing the idea that the introduction of delivery work narrows the gap in safety across neighborhoods.

5.2.2 Delivery Activity and Displacement of Crime

A second possibility that might affect the spatial distribution of crime is that the introduction of the delivery platform shifts offenses toward areas where delivery drivers work. This channel is plausible because delivery work is unusually spatially mobile relative to the jobs typically available to young, low-skilled men. According to PNAD Contínua, the most common other jobs held by drivers are in construction, security, retail and commerce, or manufacturing—occupations tied to a single, fixed location for the duration of employment. Delivery work, by contrast, takes workers throughout the city. If offenders commit crime close to where they spend their time, as the distance-decay literature discussed above suggests, then this shift from static to mobile employment could in principle displace crime toward the new locations where delivery workers spend their time. This mechanism is particularly relevant in more segregated cities, where mobile delivery jobs represent a bigger change in the employment locations of low-skilled workers.

Figure 9: Effect on Crime by Intensity of Delivery Activity



This figure reports the estimated effect of iFood's introduction on crime, restricting the sample to grid cells with delivery activity at or above increasingly high percentiles of orders across grid cells. Each bar plots the coefficient from a separate Poisson pseudo-maximum likelihood regression of the number of criminal offenses on an indicator equal to one after iFood's introduction and zero before, restricted to grid cells at or above the 50th, 75th, 80th, and 90th percentiles of total orders, respectively. The sample is all grid cells that registered at least one crime in every year, in municipalities with at least 10 crimes per year. All regressions control for grid cell by municipality and year fixed effects. Standard errors are clustered by municipality, and 95% confidence intervals are reported.

To estimate effects separately in local areas with varying levels of delivery activity, I divide São Paulo state into $0.008^\circ \times 0.008^\circ$ grid cells ($\sim$1 km^2) and use geocoded drop-off locations from the e-receipt data to measure the number of orders per cell. I restrict the sample to cells that record at least one crime in every year, so that the comparison does not simply capture cells with and without residents. I then conduct two tests. The first is on the extensive margin: I classify cells that record at least one delivery as high delivery propensity (about 15% of all grid cells in the state) and cells that record none as low propensity, and estimate the effect of iFood's introduction separately for each group. If crime is displaced toward areas with delivery activity, the effect should be smaller, or reversed, in high-propensity cells. The second is on the intensive margin, and asks whether crime rises in the busiest delivery destinations: I progressively restrict the sample to cells at or above the 50th, 75th, 80th, and 90th percentiles of iFood orders, and re-estimate the effect at each threshold.[19]

[19]Because delivery propensity is measured with post-entry data, cells with delivery activity could in principle be selected on favorable crime dynamics. This is unlikely to affect the extensive-margin classification, since recording at least one order over eight years reflects whether an area has residents or businesses rather than year-to-year crime trends. The intensive-margin thresholds are more exposed to such selection but provide a sharper test of displacement; that the two exercises agree is therefore reassuring.

Table 3 reports the extensive-margin results, with Panels A and B corresponding to high- and low-delivery-propensity cells; column 1 reports the effect on all crime, and columns 2 and 3 disaggregate into property and violent crime. The introduction of the platform reduces crime in high-propensity cells by 10.2% (Panel A, column 1), close to both the baseline effect of 9.8% (Table 1, column 1) and the 12.0% effect in low-propensity cells (Panel B, column 1), and the difference between the two groups is not statistically significant (p=0.61). The effects on property and violent crime are similar across high- and low-propensity cells, with differences that are not statistically significant (p=0.93 and p=0.74, respectively). The same pattern holds in high-inequality municipalities, with above-median Gini coefficient (Figure A12): the effects in high- and low-propensity cells remain statistically indistinguishable (p=0.41, 0.44, and 0.97, Table A6). Figure 9 reports the intensive-margin results. The effect is stable as the sample narrows to cells with more intense delivery activity, providing no evidence of displacement toward even the busiest delivery destinations.

Together, these findings suggest that the expansion of work opportunities for low-skilled workers does not lead to the relocation of crime toward new work sites, even in more segregated municipalities where delivery drivers are exposed to the starkest change in the income of the neighborhood they work in. Instead, as shown in Section 5.2.1, there appears to have been a reduction across all neighborhoods, with larger effects in lower-income areas because, absent the expansion in delivery jobs, potential offenders would disproportionately commit crime in these places.

6 Conclusion

This study leverages the staggered rollout of Brazil's largest food delivery platform to provide new evidence on how the expansion of gig work affects crime. I find that the introduction of the platform reduced crime by 9.8 percent on average, equivalent to about 437 fewer offenses per municipality per year, or 3.6 fewer offenses per year for each delivery job created. The effect extends to violent crime, though it is smaller on average, and grows over the years following iFood's entry as the platform expands. This is the first study to show that expanding flexible work opportunities can generate sustained reductions in crime in a developing country, where crime is high and effective public safety interventions are scarce.

Turning to mechanisms, I find that reductions in crime are larger at times of day when delivery work is most lucrative, consistent with the increase in the opportunity cost of crime being an important channel; this pattern holds even after controlling for the level of crime typically observed at each hour of the day, and is not explained by a reduction in outdoor victimization opportunities as consumers stay home more often. However, effects are also present at times when delivery demand is low, particularly for property crime, suggesting that the total income gain from gig work also drives crime down independently of hour-by-hour changes in opportunity cost.

In addition, I find that the spatial incidence of the effect is not even, consistent with the tendency of offenders to commit crime close to where they live. Reductions are largest in lower-income

neighborhoods, where delivery drivers disproportionately live and where crime rates are highest at baseline, so that the introduction of the platform narrows preexisting gaps in neighborhood safety. This is especially true for violent crime, which declines substantially in low-income neighborhoods. At the same time, there is no evidence that crime is displaced toward higher-income neighborhoods or areas with greater delivery activity.

Delivery work has potential synergies with crime—by increasing free time, mobility, and opportunities for concealment—that would predict a positive effect on crime. These synergies have been the subject of numerous press reports in Brazil, the setting of this study. Instead, I find a sizable, persistent reduction in crime. The fact that the reduction is concentrated in the lowest-income neighborhoods without any displacement toward the areas drivers now work in suggests that connecting low-skilled workers to jobs in wealthier areas can narrow gaps in neighborhood safety without relocating crime across places. These findings also indicate that gig work functions as an informal safety net for the population most at risk of offending: a job with no hiring process and immediate pay gives young, low-skilled men a fallback source of income. To the extent that this is the mechanism, the findings should extend to other forms of flexible work, which are growing worldwide and were accelerated by the COVID-19 pandemic. That the effect is nonetheless largest when the returns to delivery work are highest suggests that programs offering alternative income could increase their impact by targeting the hours and days when crime peaks.

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Appendix Figures and Tables

Figure A1: News Articles Reporting on Delivery Drivers Committing Crime



Falso entregador de aplicativo é preso menos de dois meses após sair da cadeia e voltar a assaltar no Rio

Homem usava roupas e mochila de entregador para praticar assaltos, segundo a polícia. Ele é suspeito de balear um cliente de uma barbearia no final de janeiro, na Tijuca, durante um assalto.

Por Alice Portes, Henrique Coelho, TV Globo e g1 Rio
02/02/2024 09h26 · Atualizado há uma semana

São Paulo

Vídeos flagram assaltos de falsos entregadores em bairros ricos de SP

Em meio à alta de roubos em bairros ricos de SP, vídeos de câmeras de segurança mostram assaltos de falsos entregadores de delivery

Felipe Resk
20/01/2024 05:15, atualizado 20/01/2024 10:30

This figure shows recent news stories about people using iFood uniforms as disguises to commit crime. The first headline translates to "Fake app deliveryman is arrested less than two months after leaving prison in Rio." The second headline translates to "Videos capture robberies by fake deliverymen in wealthy neighborhoods of São Paulo."

Figure A2: iFood Blog Post on Delivery Drivers and Crime



27/04/2022 | Delivery drivers, Releases

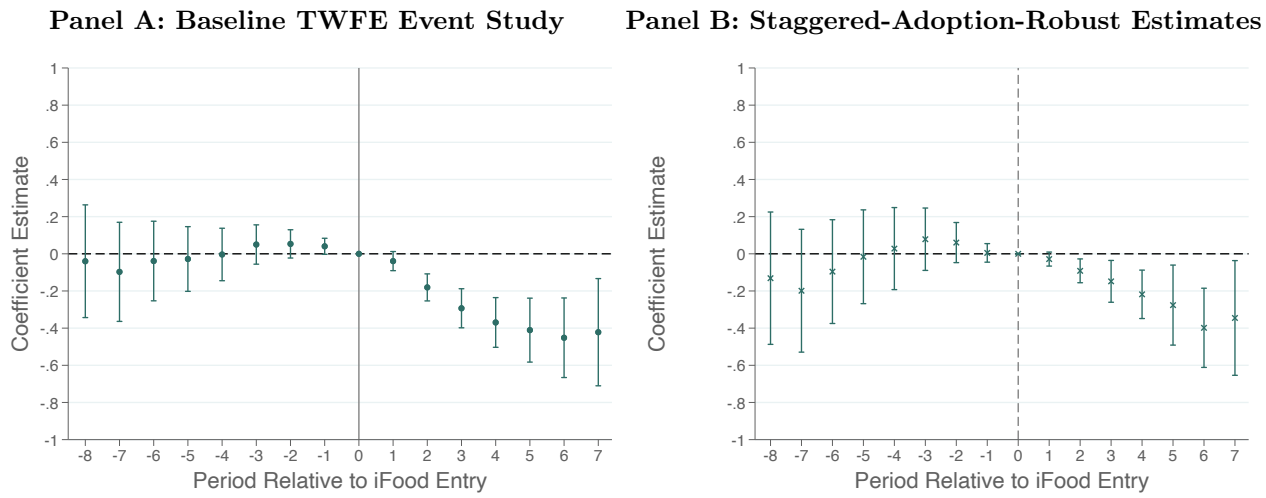
Position on crimes committed by people using iFood bags

Comunicado

ifood news

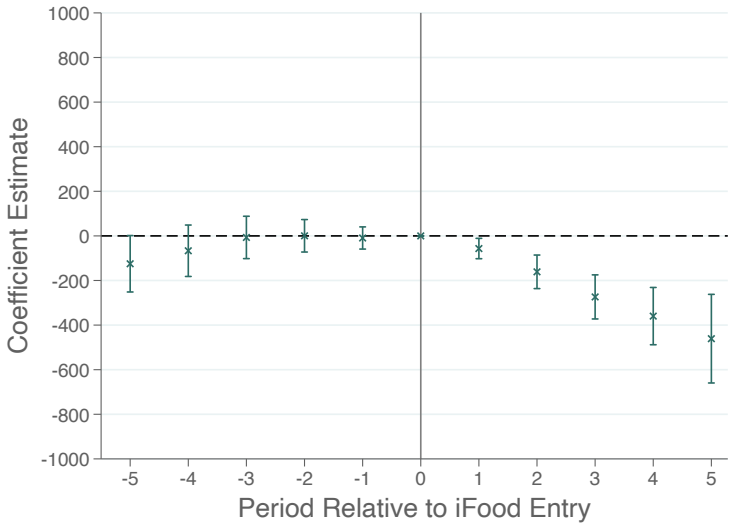
This figure shows a screenshot of a blog post published by iFood in 2022 describing the steps it takes to mitigate crime committed by people wearing delivery driver uniforms. The blog post can be found under this link.

Figure A3: Event Studies without Binning Endpoints



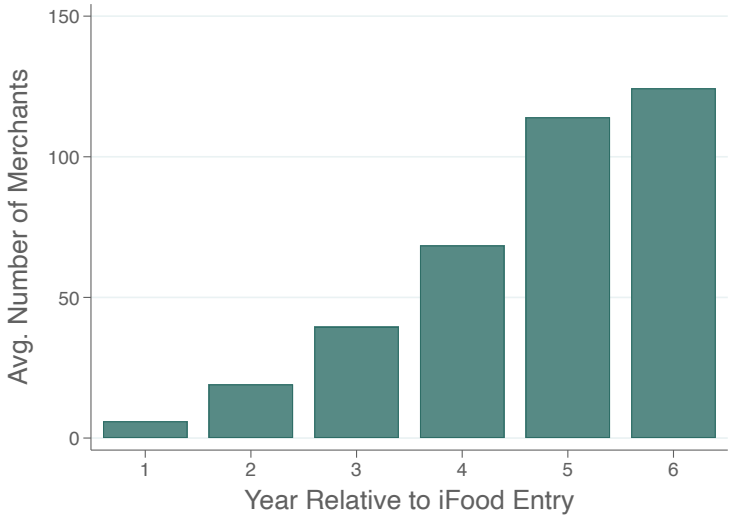
This figure shows dynamic treatment effects over the full event window, without binning the endpoints. Panel A reports the two-way fixed effects event study; Panel B reports the interaction-weighted estimator of Sun and Abraham (2021), estimated following Wooldridge (2023), which estimates a separate coefficient for each treatment cohort and event time using municipalities not treated by 2019 as controls and aggregates across cohorts weighting by cohort size. Both panels are estimated using Poisson pseudo-maximum likelihood, with the dependent variable equal to the number of criminal offenses and the entry year as the reference period. The sample is all municipalities with at least 10 crimes per year and the unit of observation is a municipality-year. Standard errors are clustered by municipality and 95% confidence intervals are shown; intervals extending beyond the plotted range are truncated at the axis limits. Coefficients at the extremes of the window are identified from a small number of municipalities: fewer than 20 contribute beyond event time 5 and fewer than 50 before event time -6.

Figure A4: Staggered-Adoption-Robust Event Study: Crime per Capita



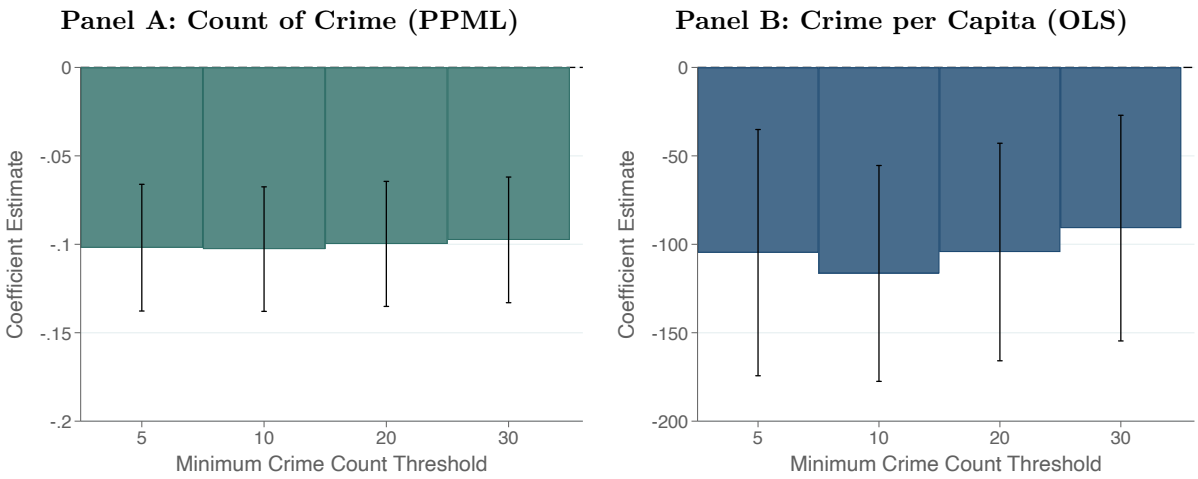
This figure shows dynamic treatment effects, estimated using the group-time average treatment effect estimator of Callaway and Sant'Anna (2021) with municipalities not treated by 2019 as controls. The dependent variable is the number of criminal offenses per 100,000 residents, using time-varying annual population estimates. The sample is all municipalities with at least 10 crimes per year, and the unit of observation is a municipality-year. Standard errors are clustered by municipality and 95% confidence intervals are shown.

Figure A5: Number of Merchants Registered with iFood Over Time



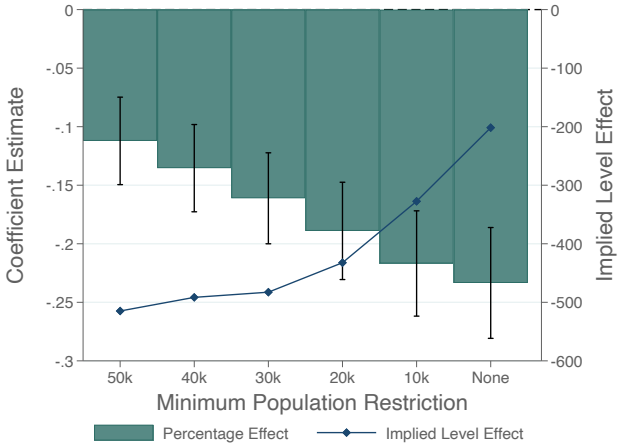
This figure displays the average number of merchants (commercial establishments) per 100,000 residents registered with iFood in each municipality over time.

Figure A6: Effect on Crime: Relaxing Minimum Crime Count Restriction



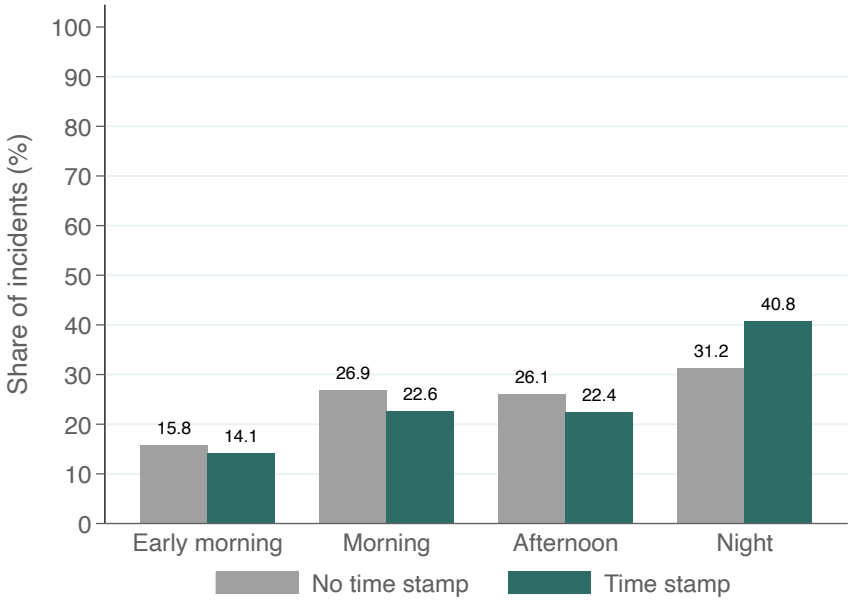
This figure reports estimates of the effect on crime as the minimum crime count restriction is relaxed. Each estimate is from a separate regression of crime on an indicator equal to one after iFood's introduction and zero before, restricted to municipalities that record at least the threshold number of criminal offenses in every year of the sample period, with municipality and year fixed effects and standard errors clustered by municipality. Panel A reports Poisson pseudo-maximum likelihood estimates, with the dependent variable measured as a raw count. Panel B reports OLS estimates, with the dependent variable measured as crime per 100,000 residents, using time-varying annual population estimates. Bars report the estimated coefficient with 95% confidence intervals.

Figure A7: Effect on Crime: Relaxing Population Restriction



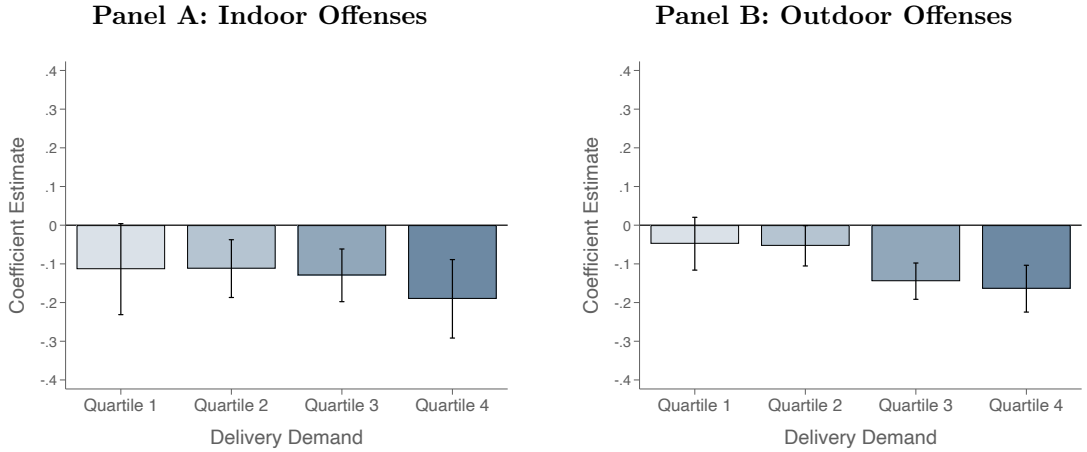
This figure reports estimates of the effect on crime counts as the sample's minimum population restriction is relaxed. Each estimate is from a separate Poisson pseudo-maximum likelihood regression of the number of criminal offenses on an indicator equal to one after iFood's introduction and zero before, with municipality and year fixed effects and standard errors clustered by municipality. Bars report the estimated coefficient with 95% confidence intervals (left axis). The connected line reports the implied effect on the level of crime, calculated as the sample mean of the dependent variable multiplied by $(\exp(\hat{\beta}) - 1)$ (right axis).

Figure A8: Period of Day for Incidents With and Without a Time Stamp



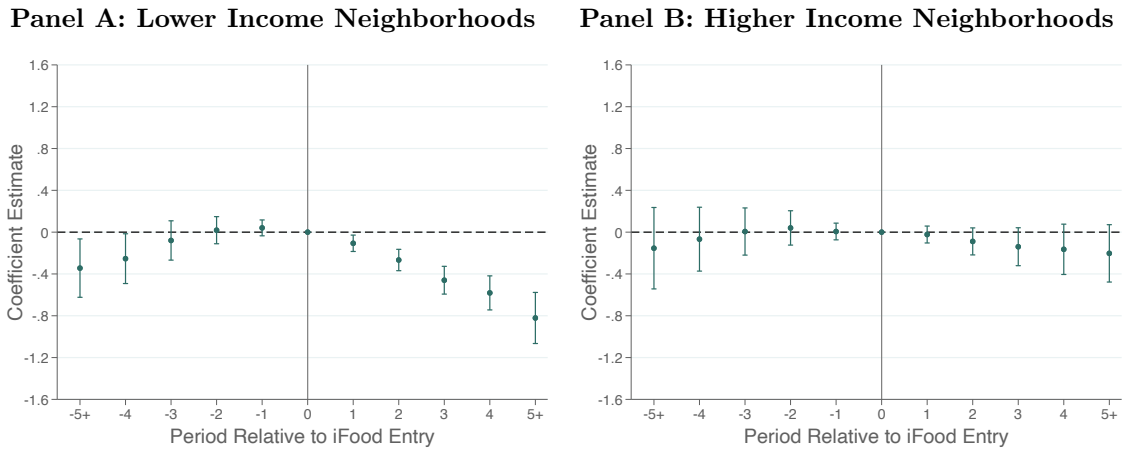
This figure compares the distribution of incidents across periods of the day for incident reports that record an exact time of occurrence and for those that record only the period of the day. Periods are as classified by the reporting officer: middle of the night (12 a.m. to 6 a.m., *madrugada*), morning (6 a.m. to 12 p.m.), afternoon (12 p.m. to 6 p.m.), and night (6 p.m. to 12 a.m.). The time-of-day analysis in Section 5.1 uses only incidents with an exact time stamp.

Figure A9: Effect by Delivery Demand Quartile and Location of Offense



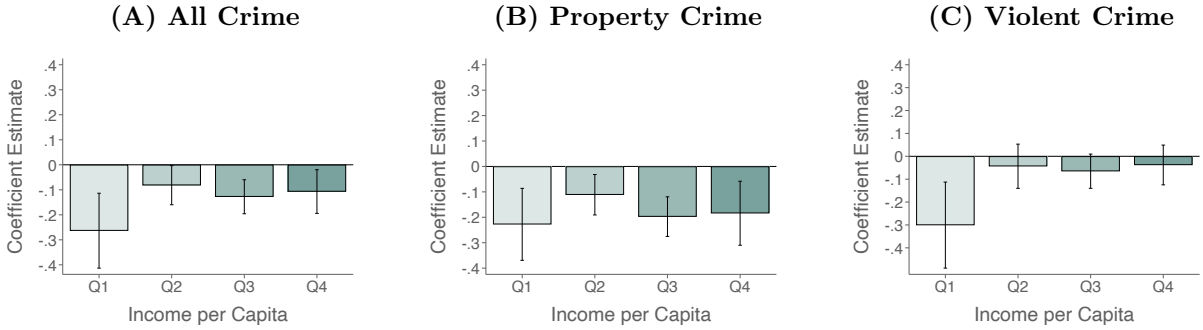
All regressions are estimated using Poisson pseudo-maximum likelihood and the sample is all municipalities with at least 10 crimes per year. Each bar plots the coefficient from a separate regression estimated on the subsample of time-of-day observations belonging to that quartile of delivery demand. The unit of observation is a time of day by municipality and year. The dependent variable is the number of criminal offenses occurring indoors in Panel A and outdoors in Panel B. "Post iFood" is a binary variable that equals zero before iFood is introduced and one after. All regressions control for municipality by time of day and time of day by year fixed effects. Standard errors are clustered by municipality. 95% confidence intervals are reported.

Figure A10: Event Studies: Effect on Crime by Neighborhood Income



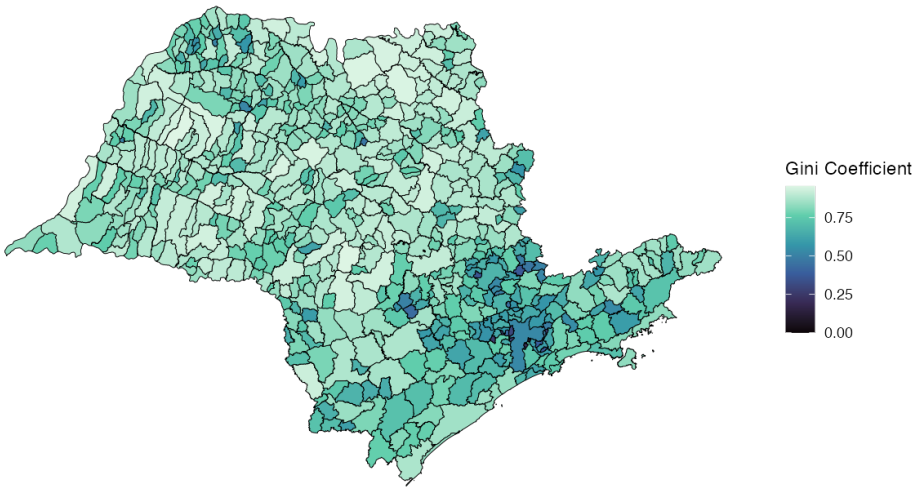
This figure shows treatment effects over time separately for grid cells below and above the median GDP per capita. Panel A reports estimates for below-median grid cells and Panel B reports estimates for above-median grid cells. Both panels report a binned event-study specification estimated using Poisson pseudo-maximum likelihood, with the dependent variable equal to the number of criminal offenses, as in Figure 4. The unit of observation is a grid cell-year, the sample is all grid cells within municipalities with at least 10 crimes per year, standard errors are clustered by municipality, and 95% confidence intervals are reported.

Figure A11: Effects by Within-Municipality Neighborhood Income



All regressions are estimated using Poisson pseudo-maximum likelihood and the sample is all municipalities with at least 10 crimes per year. Quartiles of income per capita are defined within each municipality, so that each grid cell is ranked relative to other grid cells in the same municipality. Each bar plots the coefficient from a separate regression estimated on the subsample of grid cells belonging to that within-municipality quartile. The unit of observation is a grid cell by municipality and year. The dependent variable is the number of criminal offenses in Panel A, the number of property crimes in Panel B, and the number of violent crimes in Panel C. "Post iFood" is a binary variable that equals zero before iFood is introduced and one after. All regressions control for grid cell by municipality and year fixed effects. Standard errors are clustered by municipality and 95% confidence intervals are reported.

Figure A12: São Paulo State Shaded by Gini Coefficient



This figure shows a map of São Paulo state with municipalities shaded by their Gini coefficient, calculated from the income distribution of $0.04^\circ \times 0.04^\circ$ grid cells within each municipality. Lighter shades indicate higher Gini coefficients and greater income inequality.

Table A1: Effect on Crime: Controlling for Differential Trends by Market Size

	Dependent Variable is **Count of Crime**			
	(1)	(2)	(3)	(4)
Post iFood (=1)	-0.103***	-0.0857***	-0.104***	-0.0839***
	(0.0180)	(0.0250)	(0.0262)	(0.0256)
Municipality Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	-	-	-
Year FE X Pre-period Municipality GDP	-	yes	yes	yes
Year FE X Pre-period Municipality GDP per Capita	-	-	yes	yes
Year FE X Population	-	-	-	yes
Dependent Variable Mean	4463	4463	4463	4463
Observations	1,350	1,350	1,350	1,350

The unit of observation is a municipality-year and the sample is all municipalities with at least 10 crimes per year. All regressions are estimated using Poisson pseudo-maximum likelihood. The dependent variable is the number of criminal offenses. "Post iFood" is a binary variable that equals zero before iFood is introduced and one after. Column 1 controls for municipality and year fixed effects. Columns 2, 3, and 4 additionally control for year fixed effects interacted with municipality GDP, municipality GDP per capita, and municipality population, respectively. Standard errors are clustered by municipality. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A2: Spillover Effects on Crime in Neighboring Municipalities

	Dependent Variable is	
	Crime (count)	**Crime per Capita**
	(1)	(2)
Post Neighbor's iFood (=1)	-0.0334	-13.70
	(0.2798)	(288.1)
Municipality Fixed Effects	yes	yes
Year Fixed Effects	yes	yes
Dependent Variable Mean	158	439
Observations	189	198

The unit of observation is a municipality-year. The sample is all municipalities that did not receive iFood services through 2018 and that record at least 10 criminal offenses in every year. Column 1 reports Poisson pseudo-maximum likelihood estimates, with the dependent variable measured as the count of criminal offenses. Column 2 reports OLS estimates, with the dependent variable measured as crime per 100,000 residents, using time-varying annual population estimates. "Post Neighbor's iFood" is a binary variable that equals zero before iFood is introduced in any bordering municipality and one once at least one bordering municipality receives iFood services. All columns control for municipality and year fixed effects. Standard errors are clustered by municipality. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A3: Effects on Crime by Location and Type of Item Stolen

	Dependent Variable is **Count of Crime** involving:			
	Any Item, Indoors	**Any Item, Outdoors**	**Personal Item, Indoors**	**Personal Item, Outdoors**
	(1)	(2)	(3)	(4)
Post iFood (=1)	-0.155***	-0.166***	-0.150***	-0.157***
	(0.0303)	(0.0259)	(0.0321)	(0.0300)
Municipality Fixed Effects	yes	yes	yes	yes
Year Fixed Effects	yes	yes	yes	yes
Dependent Variable Mean	763	2878	527	1309
Observations	1,350	1,350	1,350	1,350

The unit of observation is a municipality-year. The sample is all municipalities with at least 10 crimes per year. All regressions are estimated using Poisson pseudo-maximum likelihood. In column 1, the dependent variable is the number of criminal offenses committed indoors, and in column 2, it is the number committed outdoors. Columns 3 and 4 focus specifically on offenses involving the theft of a personal item, with column 3 showing those committed indoors and column 4 those committed outdoors. "Post iFood" is a binary variable that equals zero before iFood is introduced and one after. All columns control for municipality and year fixed effects. Standard errors are clustered by municipality. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A4: Average Crime by Quartile of Income per Capita

Quartile of GDP per Capita	**Average Crime per Capita**	**Average Property Crime per Capita**	**Average Violent Crime per Capita**
1	439	150	289
2	188	93	95
3	296	117	179
4	192	87	104
Ratio, Q1/Q4	2.29	1.72	2.78

This table shows average crime per capita, defined as the number of criminal offenses per 100,000 residents, in grid cells belonging to each quartile of income per capita. Column 1 reports all criminal offenses, column 2 property crime, and column 3 violent crime. Quartiles are defined using grid-cell GDP per capita from Kummu et al. (2025), computed across all grid cells in municipalities that record at least 10 criminal offenses in every year of the sample period. Quartile 1 corresponds to the lowest income and Quartile 4 to the highest. The final row reports the ratio of average crime in Quartile 1 to that in Quartile 4.

Table A5: Effect by Quartiles of Delivery Demand Controlling for Crime Level

	Dependent Variable is **Count of**		
	All Crime	**Property Crime**	**Violent Crime**
	(1)	(2)	(3)
1st Quartile Delivery Demand X Post iFood (=1)	0.00852	-0.0934***	0.0455
	(0.0245)	(0.0302)	(0.0315)
2nd Quartile Delivery Demand X Post iFood (=1)	-0.139***	-0.143***	-0.119***
	(0.0243)	(0.0304)	(0.0345)
3rd Quartile Delivery Demand X Post iFood (=1)	-0.147***	-0.119***	-0.146***
	(0.0232)	(0.0267)	(0.0263)
4th Quartile Delivery Demand X Post iFood (=1)	-0.191***	-0.233***	-0.167***
	(0.0363)	(0.0363)	(0.0327)
p-value, Q1=Q4	< 0.001	< 0.001	< 0.001
Dependent Variable Mean	145	40	105
Observations	30,240	30,240	30,240

All regressions are estimated using Poisson pseudo-maximum likelihood and the sample is all municipalities with at least 10 crimes per year. The unit of observation is a time of day by municipality and year. The dependent variable is the number of criminal offenses in column 1, property crimes in column 2, and violent crimes in column 3. "Post iFood" is a binary variable that equals zero before iFood is introduced and one after. It is interacted with binary variables that equal one when the time of day belongs to that quartile of delivery demand. All columns also control for the "Post iFood" indicator interacted with binary variables that equal one if the time of day belongs to that quartile of crime (not shown), as well as time of day, municipality, and year fixed effects. Standard errors are clustered by municipality. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A6: Effects in High and Low Delivery Propensity Locations in Unequal Municipalities

	Dependent Variable is **Count of**		
	All Crime	**Property Crime**	**Violent Crime**
	(1)	(2)	(3)
	Panel A: High Delivery Propensity Grid Cells		
Post iFood (=1)	-0.141***	-0.168***	-0.123**
	(0.0478)	(0.0415)	(0.0598)
Dependent Variable Mean	68.47	38.53	29.98
Observations	7,570	7,570	7,560
	Panel B: Low Delivery Propensity Grid Cells		
Post iFood (=1)	-0.196***	-0.218***	-0.120*
	(0.0469)	(0.0489)	(0.0662)
Dependent Variable Mean	32.60	17.83	14.80
Observations	9,450	9,450	9,430
p-value, Panel A = Panel B	0.41	0.44	0.97

The unit of observation is a grid cell by municipality and year. All regressions are estimated using Poisson pseudo-maximum likelihood. The sample is 1 km^2 grid cells that record at least one crime in every year, in municipalities with at least 10 crimes per year and above-median income inequality as measured by the Gini coefficient (see Figure A12). Panel A restricts to grid cells that register at least one iFood delivery in the e-receipt data during the sample period; Panel B restricts to grid cells that register none. The dependent variable is the number of criminal offenses in column 1, the number of property crimes in column 2, and the number of violent crimes in column 3. "Post iFood" is a binary variable that equals zero before iFood is introduced and one after. All columns control for grid cell by municipality and year fixed effects. Standard errors are clustered by municipality. *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.