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Deliver Us from Crime? Gig Jobs, Labor-Market Opportunities, and Offending

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Deliver Us from Crime? Gig Jobs, Labor-Market Opportunities, and Offending^{*}

Abstract

We study how access to low-barrier gig work affects labor markets and crime, exploiting the staggered rollout of food-delivery platforms across France. Platform entry increases delivery-worker registrations, reduces unemployment among men and inactivity among male migrants, and lowers crime, especially vandalism, petty theft, and drug offenses. Crime falls among young adults just above the legal minimum age of 18 for delivery work, but not among minors just below it. Using national criminal records, we also find that reoffending declines among offenders whose records permit platform work, but not among others. Lowering barriers to legal income can reduce low-skill offending.

JEL classification

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Keywords

gig economy, crime, labor-market opportunities, digital platforms

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1 Introduction

We study how access to gig jobs affects local labor markets and crime. Food-delivery platforms create self-employment opportunities with low barriers to entry, attracting ethnic minorities and disadvantaged youth with limited access to wage employment. In a 2021 U.S. survey, 7% of adults reported earning income from delivery work. However, participation was higher among Hispanic, Black, young, and low-income respondents than among White, prime-age, and high-income respondents.¹ These job opportunities may matter because disadvantaged youth, ethnic minorities, and people with migrant backgrounds face persistent barriers to legal employment and are overrepresented in arrest and conviction records (Hjalmarsson et al., 2024; Marie and Pinotti, 2024). By expanding access to legal income for workers on the margin of the labor market, gig jobs may reduce low-skill crime.

Fifteen years ago, jobs performed by self-employed contractors on digital platforms were almost nonexistent. By 2021, there were an estimated 4.9 million gig workers in the United States (Garin et al., 2023) and up to 4.1 million in the European Union.² This growth stems overwhelmingly from food-delivery platforms. Food-delivery work is the archetypal task-based gig job, which provides limited unemployment benefits and job security, but offers flexibility (Einav et al., 2016; Mas and Pallais, 2017; Hall and Krueger, 2018; Chen et al., 2019) and very low barriers to entry. Registration is free and upfront investment is minimal. In Europe, e.g., riders often use only a basic bicycle. Platforms typically do not require CVs, photographs, formal education, credentials, or language proficiency. The resulting low-screening, largely non-discriminatory hiring process makes delivery jobs accessible to groups otherwise excluded from the labor market.

Can access to gig jobs help reduce crime? Becker (1968) frames crime as a cost-benefit calculation under uncertainty: individuals weigh the expected returns to illegal activity against the earnings available in the legal sector. Readily accessible gig jobs raise legal earnings, thereby increasing the opportunity cost of crime. The Beckerian economic deterrence mechanism resonates with the testimony of an ethnic minority migrant in

¹The Pew Research Center reports the following statistics in “The State of Gig Work in 2021:” Hispanics (16%), Blacks (10%), 18-29-year-olds (18%), and low-income earners (12%) versus Whites (4%), 30-49-year-olds (7%), and high-income earners (2%).

²See the EU Directive 2024/2831 on improving working conditions in platform work. In China, Meituan, a major delivery platform, uses 7.5 million riders who are paid \$11 billion a year; see The Economist, 4 April 2025, last accessed January 2026.

France, who “needed work” and who felt that “riding a bike, even on precarious terms, was better than more nefarious ways of making money like selling drugs”.³ A complementary mechanism for the reduction in crime is called self-incapacitation: time spent working may reduce the amount of unstructured time available for criminal activity.

Existing evidence links aggregate unemployment and low-skill wages to crime (Grogger, 1998; Raphael and Winter-Ebmer, 2001; Machin and Meghir, 2004; Draca and Machin, 2015). Other studies examine labor-market opportunities after prison release and policies that raise employment income or income support (Yang, 2017; Agan and Makowsky, 2023; Ludwig and Schnepel, 2025). We study a different margin: access to low-barrier legal work. Food-delivery platforms offer legal work to people who face barriers to wage employment. Unlike re-entry programs, platform work is not restricted to people leaving prison and involves little employer screening. Relative to income support policies, platform work changes both income opportunities and time use.

France provides a useful setting for examining the job access margin. Disadvantaged youth and migrants face persistent discrimination and high inactivity and unemployment (Cahuc et al., 2013; Glover et al., 2017). During the pre-pandemic period, from 2012 to 2019, unemployment was 8 to 14 percentage points higher for youth (15–24), immigrants, and low-skilled workers than for prime-age (25–49), natives, and high-skilled workers.⁴

We exploit the staggered rollout of Uber Eats and Deliveroo, the two largest food-delivery platforms in France.⁵ Both platforms began operating in eleven areas in 2015 and then expanded sequentially. We combine platform-entry dates with administrative records on rider take-up, labor-market outcomes, police-recorded crime, and court records on offender age. These data show which groups take-up delivery work and allow us to compare crime responses between adults and minors, who cannot register as delivery riders. French law does not allow crime records to be linked to rider or employment records at the individual level (Section 5.3). At the aggregate level, however, we observe rider take-up and crime in the same geographic units, which allows us to estimate area-level responses to platform entry. Using a one-in-eight sample of the French workforce that we can follow over time, we find that 61 percent of riders held no job in the month before they registered, and for one in four, delivery was their first formal job.

³New York Times, 16 June 2019, last accessed July 2026.

⁴See section 2.1 for sources and details.

⁵See <https://dashmote.com/blog/food-delivery-race-europe>, last accessed July 2026.

We estimate the area-level responses using a staggered difference-in-differences design that compares changes after platform entry with contemporaneous changes in not-yet- and never-treated areas. The causal interpretation requires that, absent platform entry, these areas would have followed parallel trends in labor-market outcomes and crime. However, platform rollout was not random. They selected larger markets with more restaurants, customers and potential riders. The main threat is that factors driving this selection may have generated different outcome trends. We address this threat by adding controls for observed local conditions (population, restaurants per capita, and male unemployment and inactivity). We also use alternative estimators and assess the plausibility of the parallel-trends assumption by examining pre-entry event-study coefficients for rider supply, labor-market outcomes, and crime.

Our estimates show that platform entry leads to a sharp rise in rider registrations, with take-up concentrated among men (about 93 percent of riders) and migrant (about a third). This first-stage response, measured using administrative registrations, identifies which demographic groups are most exposed to the arrival of gig work opportunities. Because delivery incomes are self-reported and substantially under-declared, we use the stock of active riders, rather than earnings, to measure the extensive margin of access to delivery work. This stock does not capture delivery work performed under rented accounts and therefore understates the expansion of gig work (see Section 3).

We next examine whether the groups with the strongest rider take-up also experience changes in local labor-market outcomes. Although aggregate unemployment and inactivity do not change significantly, labor-market activation is visible for the groups driving rider take-up: male unemployment declines and inactivity falls among migrant men in the years following platform entry.

The question is whether labor-market changes affect crime. Our estimates show that crime declines in jurisdictions where delivery gig jobs become available. First, low-skill and income-generating crimes, such as street drug dealing and shoplifting, decline, suggesting that legal earnings raise the opportunity cost of illicit activities. Second, the decline in violence and property destruction suggests a self-incapacitation channel, whereby delivery work keeps young people occupied, including in the evenings and on weekends, when delivery demand peaks. In contrast, skill-intensive property crimes, such as burglary and vehicle theft, do not show significant change. This pattern is consistent with the idea

that these offenses require more specialized skills, planning, and criminal networks, which make them less responsive to low-skill employment opportunities.

Two complementary tests examine whether crime declines are concentrated among people eligible for delivery work. First, we exploit the legal age requirement that delivery workers must be at least 18. Court records report offender age, allowing us to compare offenses for minors aged 15-17, who cannot register as riders, and young adults aged 18-25, among whom most riders work. Property destruction and low-skill theft are at least as common among minors as young adults, making minors an informative comparison group. Neither offense changes after entry for minors, but both decline significantly for young adults: property destruction by 8 percent and low-skill theft by 9 percent. Declines are at least as large for ages 18-20, just above the threshold, though less precisely estimated because this narrow window contains fewer offenses, and they persist for wider adult windows through age 39. Because each age group is compared across treated and not-yet-treated areas in the same years, age-related declines in offending and other changes at 18, including adult criminal responsibility, are differenced out. Police or court treatment of minors would bias the comparison only if it changed in treated areas when platforms entered. Absent a change in such treatment, the age discontinuity points to a mechanism operating through work eligibility, rather than through factors that would affect minors and young adults alike, such as increased policing, “eyes-on-the-street” presence of riders, or broader urban dynamics.

Second, we exploit an additional eligibility margin: criminal records. Platforms run a background check on the record extract available to private employers, which in France lists only the most serious convictions, mainly unsuspended prison sentences of more than two years (Section 3). Most convictions therefore do not prevent delivery work, even though they appear on the more complete extracts checked by public employers and regulated occupations. Record extracts are not disclosed, so we reconstruct eligibility from each adult offender’s conviction history, using a deliberately conservative rule. Offenders with any prior prison sentence, even suspended, are classified as ineligible, and those with only non-custodial sentences, such as fines or community service, as eligible. Reoffending falls by 13 percent among eligible offenders and changes little among ineligible ones. Although the criminal-record data use coarser spatial aggregation than the rest of the paper, this pattern supports the job access mechanism.

Evidence on whether lowering criminal-record barriers reduces reoffending comes mainly from the United States, where policies such as “ban the box” and record expungement aim to improve employment (Agan and Starr, 2018; Doleac and Hansen, 2020).⁶ In particular, reclassifying eligible felony convictions as misdemeanors increases participation in platform gig work (Agan et al., 2024). Outside the United States, Bhuller et al. (2020) and Hjalmarsson et al. (2025) document how incarceration and criminal records affect employment and recidivism. Our findings address the other side of this margin: when delivery work becomes available, offenders eligible to work based on their records reoffend less. This provides rare causal evidence outside the United States that jobs accessible to people with criminal records can reduce reoffending.

Our paper adds to other strands of the literature. First, digital platforms offer a novel alternative for marginalized, low-skilled, unemployed and disabled workers (Agrawal et al., 2015; Burtch et al., 2018; Huang et al., 2020; Laitenberger et al., 2023; Guo et al., 2024; Chen et al., 2026). Gig work can offset income losses after job loss and alter labor supply (Fos et al., 2025; Jackson, 2022). It can also affect employment, earnings, and labor demand (Abraham et al., 2024; Plotkin, 2026). We provide causal evidence on how the rollout of low-barrier gig-job opportunities affects local labor-market attachment and area-level crime.

Second, we contribute to research on job opportunities and crime (Becker, 1968; Grogger, 1998; Machin and Meghir, 2004; Draca and Machin, 2015; Hjalmarsson et al., 2024). Stronger low-skill labor markets and income support reduce property crime or recidivism (Raphael and Winter-Ebmer, 2001; Yang, 2017; Agan and Makowsky, 2023; Ludwig and Schnepel, 2025). We focus on the extensive margin of legal employment access: how low-barrier gig jobs affect offending among people with weak attachment to the formal labor market. Delivery work has minimal screening and low entry costs, expanding access to legal income for disadvantaged workers. This focus complements evidence that discrimination against ethnic minorities and people with criminal records can restrict employment access and sustain labor-market exclusion and criminal involvement (Kline et al., 2022; Cullen et al., 2023).

Finally, our paper relates to work on digital platforms and crime. Previous work links

⁶“Increasing the availability of jobs for reentering offenders reduces recidivism rates (Yang, 2017; Schnepel, 2018). But finding employment remains difficult for this group” (Doleac and Hansen, 2020).

ride-hailing availability to victimization outcomes.⁷ Uber’s rollout in the United States raises auto-theft arrests but lowers assaults (Dills and Mulholland, 2018), with mixed effects on overall personal crime (Weber, 2019). Rai et al. (2024) rely on the staggered rollout of background check laws introduced by Uber and Lyft in the US to study their impact on local crime. They show that crime increased in the states where the policy was introduced. A concomitant study based on São Paulo shows crime falls where the iFood delivery platform expands (Frankenthal, 2025). We study a developed-country setting in which gig work constitutes formal employment and can be linked to administrative labor-market records and residential locations. This allows us to observe who takes up delivery work, where they live, and how access to legal jobs for disadvantaged populations affects labor-market attachment and criminal behavior. In addition, the legal minimum age for delivery work creates a sharp eligibility threshold that helps us separate the effect of job access on offending from alternative channels such as the probability of detection or shifts in consumer behavior. We examine whether crime responses differ across offenses, distinguishing opportunistic, low-skill offenses from crimes requiring greater planning or specialization.

We do not claim that gig jobs represent a comprehensive solution to crime or labor-market exclusion. We study a narrower margin: access to legal work. The results show that lowering entry barriers to legal income, even through temporary, flexible, and low-wage work, can generate behavioral responses among individuals at the boundary between legal and illegal income opportunities. Our evidence concerns one form of low-barrier work, food delivery. Whether other jobs open to the same workers would have similar effects remains an open question.

The paper is organized as follows. Section 2 describes the labor-market context and presents the main insights of our conceptual framework. Section 3 documents the rollout of food delivery platforms in France and explains the requirements to access gig work. Section 4 characterizes the demographic profile of delivery riders using administrative data. Section 5 presents our empirical strategy and estimation approach. Section 6 reports the main results on the labor-market and crime outcomes. Section 7 provides evidence on mechanisms and robustness checks. Section 8 concludes.

⁷Other studies use ride-hailing rollouts to study traffic fatalities, housing costs, and consumption (Anderson and Davis, 2026; Gorback, 2024; Norris and Xiong, 2023). In France, ride-hailing created few jobs in our period (Appendix Figure A5), entered earlier against strong taxi opposition, and requires clean criminal records (see Section 3).

2 Labor-Market Exclusion, Gig Work, and Crime

2.1 Labor-Market Exclusion and Crime

A large economic literature links labor-market exclusion to crime ([Grogger, 1998](#); [Raphael and Winter-Ebmer, 2001](#); [Machin and Meghir, 2004](#); [Draca and Machin, 2015](#)). In France, unemployment and inactivity remain high among young people and individuals of immigrant origin. Between 2012 and 2019, unemployment rates averaged 15.9% among first-generation migrants and 13.7% among second-generation migrants, compared with 8.1% among natives. The unemployment rate for young people aged 15-24 averaged a high 22.7%.⁸ Foreign-born youth experienced even higher unemployment rates during the same period. In 2019, the unemployment rate was 30.2% among foreign-born youth and 20.7% among all youth.⁹ The share of people not in employment, education, or training (NEET) was much higher among foreign-born youth. In 2019, the NEET rate was 19.1% among foreign-born youth, 1.8 times the 10.6% among all youth (see Appendix Figure A2). Labor-market disparities vary by age, migration background, and country or region of origin (see Appendix Figure A3 and Tables A1 and A2).

Correspondence studies document persistent hiring discrimination in France: applicants with North African-sounding names receive 30-45% fewer callbacks than otherwise identical applicants ([Breda et al., 2021](#)). This ethnic penalty declines with job qualification, suggesting stronger discrimination in low-skill segments of the labor market, where many young men of immigrant background are concentrated.

The demographic profile of convicted individuals overlaps with the groups most exposed to labor-market exclusion. When delivery platforms entered the French market in 2015, 67% of convicted individuals were between 18 and 40 years of age, 89.8% were male, and 13.6% were foreign nationals, about twice their 6.7% share of France’s resident population.¹⁰ These descriptive patterns do not imply that immigrant origin raises offending.

⁸Authors’ calculations based on [INSEE data](#) (dataset FPORSOC21-F13). The averages cover 2012, 2014, 2016, 2018, and 2019, except those by migration status, for which 2012 data is not available. Following INSEE’s classification, first-generation migrants are immigrants, second-generation migrants are descendants of immigrants, and natives are people without direct migrant ancestry.

⁹The unemployment gap runs in the opposite direction in the United States. Among youth aged 16-24, unemployment was lower for the foreign-born than for the native-born in both 2012 (14.3% versus 16.4%) and 2019 (6.7% versus 8.5%). Source: U.S. Bureau of Labor Statistics, Current Population Survey annual averages, ages 16-24. Foreign-born estimates come from *Labor Force Characteristics of Foreign-Born Workers*, Table 1.

¹⁰French Ministry of Justice [data](#). The population benchmark comes from [INSEE](#). The comparison is descriptive. Judicial statistics may include foreign nationals who do not reside in France, whereas the

The association between immigrant population shares and crime rates in France disappears after accounting for immigrants’ economic circumstances ([Aoki and Todo, 2009](#)). This result is consistent with broader evidence that access to legal status and work reduces immigrants’ involvement in crime ([Marie and Pinotti, 2024](#)).

French evidence documents weak labor-market attachment and low educational attainment among people entering prison.¹¹ In 2019-20, 52.1% of inmates had no diploma, and 90% had not completed the equivalent of a high school diploma (*baccalauréat*) when they entered prison.¹²

These facts point to a tight overlap between labor-market exclusion and criminal involvement. This overlap motivates examining whether market developments that lower barriers to low-skill legal work affect crime.

2.2 Gig Work as Low-barrier Legal Employment

Food-delivery platforms mitigate traditional recruitment frictions through three features: low entry costs, minimal skill requirements, and limited screening. Riders need only basic equipment, and administrative registration is free and fast. They need no formal qualifications, interviews, or résumés, and most criminal convictions do not disqualify applicants. Entry involves little job search or employer discretion. Hiring decisions do not depend on subjective assessments of past employment histories. These features distinguish delivery gig work from standard low-skill jobs, where hiring frictions, screening, and discrimination can exclude individuals with limited education, weak labor market attachment, or criminal records. Delivery gig work is also flexible. Riders can combine it with schooling, informal activity, or other employment. Even partial or irregular engagement can therefore provide legal income and change time use.¹³

Evidence from related platform settings supports the view that platforms mitigate traditional hiring constraints. For example, [Lambin and Palikot \(2026\)](#) show that reputation-

population benchmark covers residents, and nationality does not capture immigrant origin.

¹¹A [2021 survey](#) of 1,119 people held in 71 French prisons finds that 53% reported being employed before incarceration, compared with 66% in the general population. Among respondents aged 17-27, only 42% reported being employed before incarceration (see, pp. 21-23).

¹²French Ministry of Justice, Direction de l’administration pénitentiaire, [Bilan annuel de l’enseignement en milieu pénitentiaire, 2019-2020](#), p. 31. The estimates come from the *Enquête hebdomadaire S49 2019, pôle enseignement*, which covers incarcerated people participating in prison education and records the highest education level they declared at prison entry.

¹³We do not observe riders’ working hours and therefore cannot measure the intensity or timing of delivery work.

based mechanisms in ride-sharing platforms attenuate ethnic penalties in customer evaluations. Self-employment and gig work are used disproportionately by individuals with criminal justice involvement, consistent with their limited reliance on formal screening (Finlay et al., 2023). Complementary evidence from record-remediation policies shows that even after formal criminal-record barriers are removed, regular wage employment responds little, likely because disrupted employment histories remain a stigma, whereas participation in platform-based gig work increases sharply (Agan et al., 2024). Together, these findings suggest that gig jobs remain accessible when traditional employment opportunities are constrained.

Our analysis focuses on access to gig work, rather than evaluating its overall job quality. By lowering barriers related to skills, screening, discrimination, and prior employment records, delivery platforms expand access to legal income opportunities. Our conceptual framework, formalized in Appendix A, illustrates how the arrival of gig opportunities affects the trade-off between legal and illegal work for low-skilled individuals by lowering barriers to legal employment. As access to legal work improves, the expected utility of legal activity increases relative to illegal alternatives, implying that higher illicit returns are required for crime to remain attractive.

Two mechanisms are central in this framework. First, in the Beckerian deterrence mechanism (Becker, 1968), readily accessible gig jobs raise expected legal earnings and reduce uncertainty about immediate legal income, which increases the opportunity cost of crime. Second, self-incapacitation operates through time allocation. Time spent working reduces opportunities to engage in offending. This mechanism has been documented in settings where participation in time-absorbing activities crowds out criminal behavior, including schooling (Jacob and Lefgren, 2003), movie attendance (Dahl and DellaVigna, 2009), and large sporting events (Marie, 2016).¹⁴ Neither mechanism requires full labor-market activation. Intermittent delivery work can provide legal income and occupy time otherwise available for offending.

¹⁴In some of these settings, crime reductions coexist with concentration effects, whereby aggregating individuals with elevated baseline offending risk or heightened arousal in the same place can increase violent interactions. This concern is less relevant in our context: delivery work is individually performed, spatially dispersed, and does not generate dense peer interactions.

3 Platform Rollout and Access to Gig Work

3.1 Platform Rollout

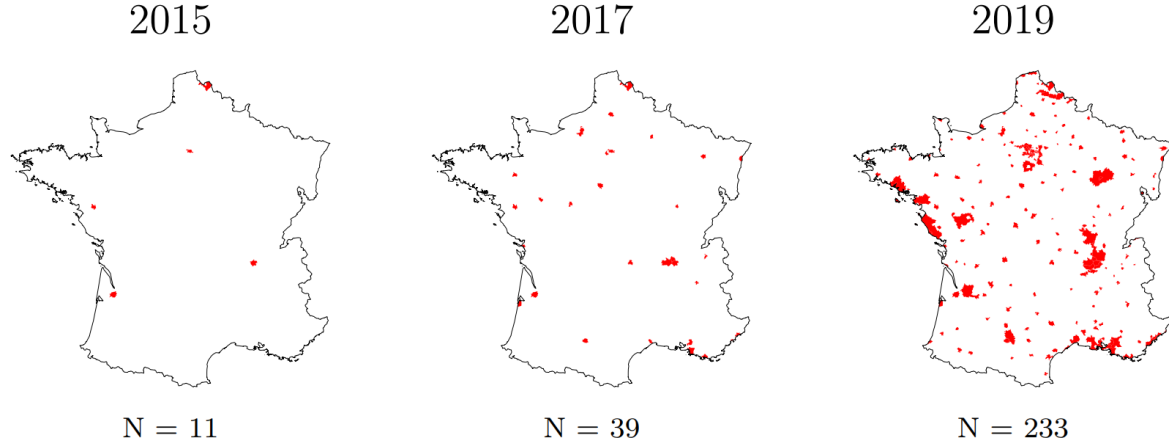
Uber Eats and Deliveroo are the two dominant food-delivery platforms in France. Both entered the French market in 2015, first in large metropolitan areas such as Paris and Lyon, before expanding to smaller cities and peri-urban areas. Their expansion was geographically staggered, generating substantial variation in the timing of entry across local markets.

The platforms organized their expansion around what they call ‘reference areas’: spatial units defined by the joint availability of restaurants (supply), consumers (demand), and potential riders. These areas are not limited to administrative city boundaries and may encompass multiple municipalities, including suburban neighborhoods. According to Uber Eats France’s Head of Expansion, platform entry depends on three local criteria: the population catchment area, the restaurant base, and the pool of potential riders.¹⁵ The criteria point to market size as the main determinant of rollout timing. Since market size changes relatively slowly over time, our estimations condition on area fixed effects that absorb persistent differences across local markets. The analysis also uses proxies for market size and local market conditions, including population size and density, restaurants per capita, and the unemployment and inactivity rates. Importantly for identification, platform entry is not subject to binding regulatory constraints, reducing concerns that rollout timing reflects municipal policy choices correlated with local crime or labor-market trends.

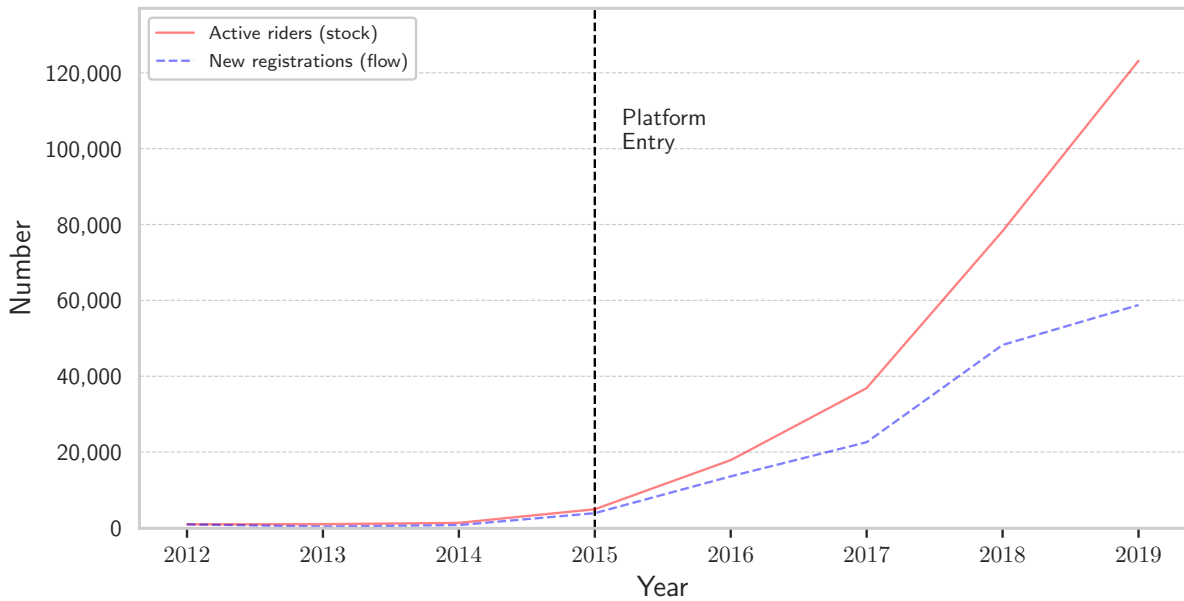
By 2019, Uber Eats and Deliveroo operated in 2,114 reference areas, covering more than half of the French population. We aggregate these reference areas to police jurisdictions, the most granular level at which crime data are consistently observed. The resulting sample contains 681 police jurisdictions (see Appendix E for data construction). Panel A of Figure 1 maps the staggered rollout between 2015 and 2019 at the police-jurisdiction level, our main spatial unit of analysis. Panel B shows the evolution of new registrations of self-employed workers in food delivery and the stock of active riders, defined as registered micro-entrepreneurs that have declared positive turnover in at least one of the previous

¹⁵To the question “How does Uber Eats select the cities in which it operates?” Pierre Estagnasié, Head of Expansion for Uber Eats France, answered: “nous étudions les bassins de populations, la base de restaurants et le réservoir de coursiers autour de chaque ville.” See actu.fr, last accessed July 2026.

Figure 1: Platform Rollout and Food-Delivery Workers in France



Panel A: Platform Rollout by Year and Police Jurisdiction, 2015–2019



Panel B: New Registrations (Flow) and Active Riders (Stock), 2012–2019

Notes: Panel A displays the staggered rollout of Uber Eats and Deliveroo across France between 2015 and 2019, aggregated at the level of police jurisdictions. Red indicates the first year of platform entry. Number of police jurisdictions in which platforms operate: 11 (2015), 39 (2017) and 233 (2019). Panel B shows annual counts of food-delivery workers. The dashed line reports new registrations (the annual inflow of newly registered riders). The solid line reports the stock of active riders, defined as registered micro-entrepreneurs that have declared positive turnover in at least one of the previous two years, which excludes dormant registrations. The vertical dashed line marks the entry of major food-delivery platforms in 2015. See Appendix Figure A5 for the number of riders and ride-hailing drivers after 2019. *Sources:* Authors' calculations using Uber Eats and Deliveroo entry dates and the INSEE *Sirene* business registry.

two years, which excludes dormant registrations (see Section 4 and Appendix F for details on registration status and income reporting).

Platform entry also requires the participation of local restaurants. Only restaurants located within a covered area can join a delivery platform and receive orders. Participation is voluntary and involves a simple application process and brief training on platform-specific software; once approved, restaurants manage orders through a tablet interface provided by the platform. Similarly, consumers face low friction. All they need is to download the app and provide basic information to place an order.

The expansion of food-delivery platforms did not face organized resistance from incumbent firms. Unlike the entry of ride-hailing services into regulated taxi markets, which triggered protests and attempts to block entry in some areas,¹⁶ restaurants generally welcomed platform access as a source of additional demand. As a result, platform rollout proceeded smoothly within covered areas and translated into expanded access to delivery services and gig work, without evidence of local backlash or regulatory obstruction.

Having described how food-delivery platforms entered local markets, we now turn to the institutional conditions governing access to delivery work, that is, who can become a rider once a platform operates in an area.

3.2 Legal Access and Conditions of Delivery Work

Food-delivery platforms impose few binding requirements. Riders must be at least 18 and register as self-employed. The registration procedure as self-employed is free and simple. Beyond registration, riders need a smartphone and a branded delivery bag. For bicycle delivery, the most common mode during our period of study, platforms require no license, certification, or formal training. These rules keep financial and administrative entry costs low.

Access also depends on legal status. Riders must present valid identification, either a passport issued by a European Union (EU) member state, a European Economic Area country, or Switzerland, or a residence permit authorizing non-salaried activity.¹⁷ Because undocumented migrants cannot register as self-employed, a shadow market has emerged in which rider accounts are rented. Platforms try to limit such practices. For example,

¹⁶See, for example, Politico Europe, “[France braces for new taxi-Uber war](#)”, 26 January 2016.

¹⁷See [Can a foreigner set up a company in France?](#), last accessed July 2026.

Uber Eats suspended about 2,500 accounts in 2022.¹⁸ The persistence of this shadow market points to excess demand for delivery work among individuals who lack legal status. Account rentals imply that administrative employment statistics underestimate delivery work. Therefore, the measured increase in rider take-up is a lower bound on the expansion of delivery work following platform entry. Crime outcomes capture behavioral responses within treated areas, even if workers do not appear in formal employment records.

Legal constraints related to criminal records are relatively light. Platforms check only the record extract available to private employers, which mainly lists unsuspended prison sentences of more than two years, so most other convictions do not bar access to delivery work. Applicants can therefore work as riders with criminal records that would exclude them from other regulated or screened occupations.¹⁹

For instance, food-delivery work has much lower entry requirements than other platform-based transport services. To operate as a ride-hailing driver in France, applicants must present clean criminal records,²⁰ complete mandatory training, and bear substantially higher capital costs, including access to a suitable vehicle. These additional legal, financial, and administrative barriers exclude individuals with misdemeanor convictions, recent criminal justice involvement, or limited resources. Food delivery therefore remains accessible to a much broader segment of the population, including young adults and individuals facing legal or reputational barriers in alternative gig or wage employment.

Delivery work offers flexible scheduling and piece-rate compensation. Although work intensity and earnings vary across locations, platforms, and individuals, surveys suggest that, between 2015 and 2019, riders could earn hourly revenues comparable to, or slightly above, the minimum wage, typically in the €10-15 range (Daugareilh, 2022; Dablanc et al., 2019).²¹ Earnings are an important dimension for assessing the long-run viability and career prospects of platform work, given the income volatility and limited progression paths documented for self-employment and gig jobs (Boeri et al., 2020). We focus on ac-

¹⁸See [Le Parisien](#), 3 October 2022, last accessed January 2026.

¹⁹France maintains three nested criminal-record extracts (“bulletins”) that differ in scope and accessibility (see [service-public.gouv.fr](#)). Bulletin B1 is the full criminal record, accessible to judicial authorities. Bulletin B2 is a restricted extract available to certain employers and public bodies. Bulletin B3 includes only the most serious convictions, mainly unsuspended prison sentences of more than two years, and is the extract individuals obtain themselves to show private employers. Food-delivery platforms require only a clean B3 record.

²⁰Applicants for ride-hailing work must present clean B2 and B3 records, see footnote 19.

²¹In 2018, Uber Eats cited €10-15 per hour and Deliveroo reported an average of €13 (see [New York Times](#), 16 June 2019).

cess to legal income opportunities rather than earnings dynamics. This emphasis reflects both the role of labor-market participation for the populations we study and data constraints. In particular, we do not observe hours worked, which vary widely among riders. More fundamentally, no reliable administrative measure of riders’ earnings exists, by design of the income-reporting system. Throughout our study period, micro-entrepreneurs self-reported their turnover to the social-security agency (URSSAF), and platforms were not required to report transaction amounts to the tax authorities until 2019 (see Appendix F for the registration and income-reporting rules). Comparing self-reports with platform records, the French Observatory on Undeclared Work documents massive under-declaration, and many riders report zero or very low turnover.²² Analyzing the extensive margin, whether individuals work at all, therefore better matches the available data and the paper’s focus on labor-market attachment and crime.

4 Riders

To characterize who the delivery riders are and where they come from, we combine three complementary administrative sources from INSEE: the *Sirene* business registry, the *Self-Employed Database* (*Base Non-Salariés*), and the *All Active Workers Panel* (*Panel Tous Actifs*). These administrative data allow us to document riders’ demographic profiles, geographic location, and labor-market attachment prior to entry into delivery work (see Appendix H for details on data construction and coverage).

4.1 Who Are They? Demographic Characteristics

We use the *Sirene* database to describe delivery riders’ demographic characteristics. It lists 153,322 unique individuals registered under NAF code 53.20Z (“Other postal and courier activities”) between 2012 and 2019.²³ Panel B of Figure 1 shows that new registrations under this code rise sharply from 2015 onward. This rise coincides with the entry and rapid expansion of food-delivery platforms. By 2019, new rider registrations reach about 60,000 per year and the stock reaches roughly 130,000 active riders. Appendix Figure A5

²²See [Les Echos, 14 December 2022](#) and [23 November 2021](#), last accessed January 2026.

²³Although code 53.20Z may include other rider activities, registrations were rare before food-delivery platforms arrived. Provided that such activities do not respond to local platform entry, their inclusion adds measurement error but does not bias the estimated take-up response.

compares food-delivery registrations with ride-hailing registrations and extends the series beyond 2019. It shows that ride-hailing remains quantitatively small, while food-delivery work expands rapidly afterward. The comparison indicates that delivery, not ride-hailing, is the relevant margin of gig-job creation in our estimation window.

To shed light on riders’ backgrounds, we use first names recorded in *Sirene* to infer broad geographic name origins.²⁴ Using a probabilistic classification approach based on large language models (see Appendix D.3), we estimate that 48.6% of riders have a non-European-sounding first name, most of which are classified as Arabic-sounding. Although name-based inference is necessarily imperfect, these patterns suggest overrepresentation of individuals likely to face discrimination in the labor market.

We complement this information with the *Base Non-Salariés*, which reports age, gender, place of birth, and municipality of residence for self-employed workers. This source identifies 147,923 unique riders aged 16-54 during the period 2012-2019, of whom 97% also appear in the *Sirene* data. Riders are overwhelmingly male (93%) and young, with an average age at first registration of 25 years (s.d. 6.6). About 32% are foreign-born.²⁵

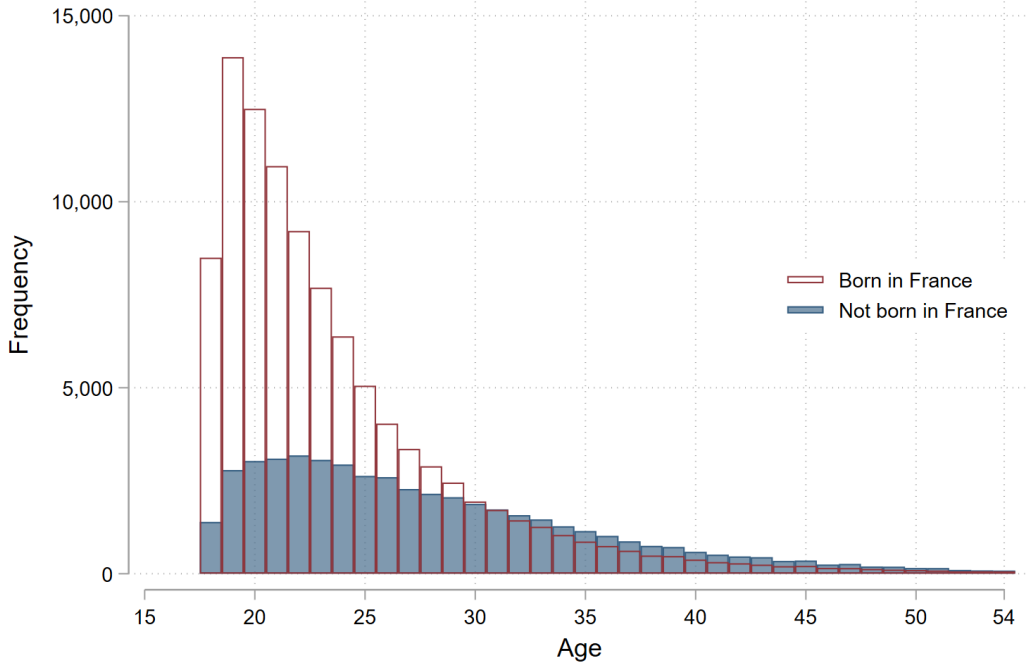
Figure 2 plots the age distribution of riders by nativity. French-born riders are highly concentrated in the 18-24 age range, with participation declining thereafter. In contrast, foreign-born riders display a flatter age profile, indicating more persistent engagement in delivery work beyond early adulthood.

These descriptive statistics, complemented by survey evidence reported in Appendix Table A3, point to a clear rider profile. Riders are predominantly young men, with a substantial share of foreign-born individuals and, among those born in France, a high prevalence of non-European-sounding names. These patterns are consistent with the view that food-delivery platforms disproportionately attract individuals from groups facing weaker attachment to the formal labor market and greater barriers to standard employment.

²⁴We exclude observations without a recorded first name (37,612) and drop unique first names that could allow identification (12,929), following INSEE anonymization guidelines.

²⁵French administrative data do not permit the construction of ethnic or ancestry-based statistics, even when names are available, so administrative records can only distinguish individuals born in France from those born abroad. As a result, second-generation immigrants, who face substantial discrimination and barriers in the French labor market (see Section 2.1), cannot be separately identified in these data. Our name-based measures therefore provide complementary evidence on the likely ethnic and second-generation composition of the rider population.

Figure 2: Rider Age by Nativity (Foreign vs. French-born)



Notes: Age distribution of riders by nativity (foreign vs. French-born). The sample includes 147,963 riders registered as micro-entrepreneurs (code 53.20Z) between 2012 and 2019. “Born in France” (hollow bars, $n = 100,128$) denotes riders born in France. “Not born in France” (filled bars, $n = 47,835$) includes all foreign-born riders. Age is measured at the time of registration and the sample is restricted to riders aged 15-54 with valid birthplace information.

4.2 Where Do They Come From? Spatial and Labor-Market Origins

Using geolocation information from the *Sirene* dataset, we compare the residential location of riders to that of the general population across neighborhoods and cities, using socioeconomic measures that include income, education, unemployment, and inactivity (see Section 5.3 for definitions).

Riders are concentrated in disadvantaged areas, and for some characteristics the differences are striking (see Appendix Table A4). The starkest contrast concerns migrant concentration. In 2019, 40.9% of riders lived in the top 5% of cities by immigrant population share, more than twice the 18.2% of the overall population residing there. Riders were also about 1.6 times as likely as the average resident to live in a “Priority Neighborhood”²⁶ (15.3% versus 9.8%) or in a city with the lowest educational attainment (6.9%

²⁶Priority Neighborhoods (or *Quartiers prioritaires de la politique de la ville* in French) were established by the 2014 planning law on urban affairs and cohesion to reduce spatial inequalities. Their boundaries are defined by the National Agency for Territorial Cohesion based on concentrations of low-income households. As of January 2015, there were 1,514 Priority Neighborhoods across 859 cities.

versus 4.5%), and 1.4 times as likely to live in one of the poorest cities (13.7% versus 9.8%). These spatial patterns reinforce the individual-level evidence that delivery work attracts populations facing weaker labor-market opportunities.

Next, we examine riders' labor-market position before they took up delivery work. The registries used above do not record employment histories, so we use the *Panel Tous Actifs* (All Active Workers Panel; see Appendix H.1.3). This panel follows only about one in eight workers of the French workforce, selected by date of birth. Within this subsample, we identify 16,805 individuals who registered as self-employed couriers without employees (activity code 53.20Z) between 2015 and 2019, roughly one-eighth of all riders.

Most riders enter delivery work without leaving an existing job. In the month before registration, 61% were not employed, while 39% held a job. Among all riders, 24% had no prior record of salaried employment, self-employment, or unemployment benefits, and another 9% were unemployed and receiving benefits in the month before registration. Among those employed before entry, one third worked part-time. Delivery work could therefore have replaced an existing job for at most 39% of riders, and likely fewer (Appendix Table A5). Non-employment before registration is most common among young riders: two thirds of riders under 26 held no job in the month before registration, compared with half of older riders. For many young people, delivery work was their first formal job: more than half of riders aged 19 or under had no job or benefit spell recorded at any point before registration.

Survey evidence further suggests that, for a non-negligible share of riders, delivery work represented the only feasible employment option at the time of entry (see Appendix C).

Despite the smaller sample size of the one-eighth administrative panel, these patterns are highly informative about the relevant margins of adjustment. They indicate that food-delivery platforms primarily draw workers from inactivity and weak labor-market attachment, rather than reallocating workers from existing jobs.

Because this evidence points to entry from outside regular employment, we expect the primary labor-market responses to occur through reductions in inactivity and unemployment. Accordingly, our empirical analysis focuses on these margins, which best reflect the pre-entry status of individuals taking up delivery work.

5 Empirical Strategy and Outcome Measurement

5.1 Identification Strategy

We examine how the staggered rollout of food-delivery platforms across France affects labor-market activation and crime. Our identification strategy exploits variation in the timing of platform entry across areas using a staggered difference-in-differences (DiD) design:

$$Y_{zt} = \alpha_z + \lambda_t + \sum_{\tau=-7, \tau \neq -1}^4 \beta_\tau \text{Entry}_{zt}^\tau + \epsilon_{zt}, \quad (1)$$

where Y_{zt} denotes the outcome of interest, rider supply, labor market outcomes, or crime rates in year t and area z (defined in Section 5.2). Entry_{zt}^τ is an event-time indicator equal to one if year t is τ years relative to the first entry of a food-delivery platform in area z . The omitted event time is $\tau = -1$, so each β_τ measures the change in Y_{zt} relative to the year before platform entry. We include area fixed effects α_z to absorb time-invariant differences across areas and year fixed effects λ_t to control for common aggregate shocks. The usual error term is ϵ_{zt} , and we cluster standard errors at the police-jurisdiction level.

After accounting for permanent area differences and aggregate year shocks, identifying variation comes from differences in the timing of platform entry across areas. The main threat is that platforms enter areas already on different labor-market or crime trends. First, We assess this threat using pre-entry event study coefficients and controls for observable determinants of platform entry. Section 3 shows that platforms base their entry decisions on the population catchment area, restaurant base, and pool of potential riders. Area fixed effects absorb persistent differences in these factors, but do not rule out differential trends across local markets. As a sensitivity check, we add time-varying measures: population aged 15-54, restaurants per capita, male unemployment, and male inactivity to Equation 1 (see Appendix N).

Second, we examine potential departures from parallel trends in labor-market and crime outcomes using the formal sensitivity analysis of [Rambachan and Roth \(2023\)](#) (see Appendix R). Because visual inspection of pre-entry event-study coefficients alone has limited power to assess this assumption, we report confidence intervals that allow the largest post-entry deviation from parallel trends between consecutive periods to be a multiple of the largest observed pre-entry deviation. We also examine robustness by excluding the largest urban areas (O) and varying the exposure buffer (Q).

Our main estimates come from the two-stage staggered DiD estimator of [Gardner et al. \(2026\)](#), which is designed to accommodate treatment-effect heterogeneity across cohorts and over time. We implement it with the *did2s* package ([Butts and Gardner, 2022](#)). The estimator first estimates the area and year fixed effects using observations not yet exposed to platform entry and then estimates dynamic treatment effects from the residualized outcome. This two-stage procedure avoids the negative-weight and contamination issues that can arise in two-way fixed-effects models with staggered adoption. We use this estimator as our preferred specification because it combines heterogeneity-robust estimation of dynamic effects with a regression-based implementation that accommodates time-varying covariates.²⁷ To assess the robustness of our findings, we also report estimates from the OLS two-way fixed effects and from the estimators of [Callaway and Sant’Anna \(2021\)](#) and [de Chaisemartin and d’Haultfoeuille \(2025\)](#). All four estimators yield very similar event-study coefficients for labor-market and crime outcomes (Appendix P).

5.2 Treated Areas, Spatial Aggregation, and Spillovers

A first empirical challenge is the definition of the treated area z . We observe platform entry, rider counts, and labor-market indicators at the municipality level, but crime statistics are available only at the police-jurisdiction level, the most granular geographic unit available for crime statistics in France. We aggregate the municipality-level measures to police jurisdictions.²⁸ Each municipality belongs to one police jurisdiction, which typically spans multiple municipalities. We assign each rider to the municipality of their registered address and hence to one police jurisdiction, even when a platform’s reference area spans several jurisdictions. Our sample include 681 police jurisdictions, each covering on average about 50 municipalities.²⁹

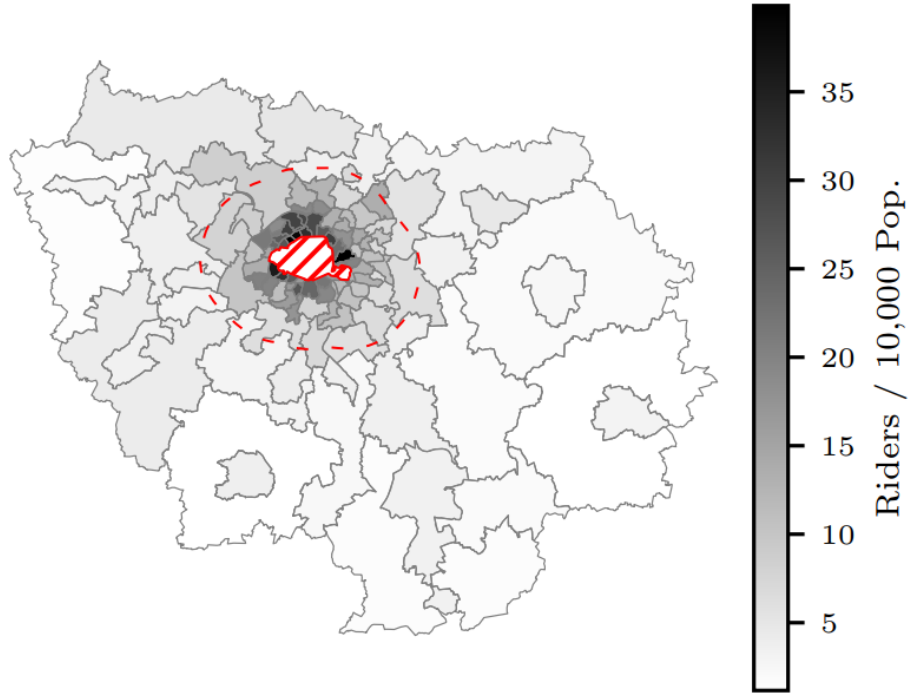
A second challenge is that not all municipalities within a given jurisdiction are treated at the same time. We therefore define a police jurisdiction as treated in the year a platform first enters any municipality within that jurisdiction. This timing typically coincides with entry into the most populous municipality in the jurisdiction, which platforms target as their primary reference market. This definition dates treatment from residents’ first access

²⁷The estimator also permits conventional GMM-based inference without resampling. When treatment effects are homogeneous, [Gardner et al. \(2026\)](#)’s estimator yields standard errors similar to those from OLS two-way fixed effects, indicating little loss of precision from allowing for treatment-effect heterogeneity.

²⁸See Appendix Figure J for an illustration of the geographical units.

²⁹In France, 88% of municipalities are rural or small with fewer than 2,000 inhabitants.

Figure 3: Riders in the Surrounding Area of Paris (2016)



Notes: The figure maps the police jurisdictions of region Île-de-France. The red hatched area shows the Paris police jurisdiction. The red dashed line shows the 15-km buffer used to define Paris's catchment area. Shading reports rider density (riders per 10,000 residents aged 15-54) in 2016, with darker areas representing higher density. Paris is excluded from the color scale to improve contrast for nearby jurisdictions and to highlight potential spillover effects. Within Paris, rider density is about 50 per 10,000 residents aged 15-54.

to platform-based gig-work opportunities.

A further challenge is that riders may live, work, and potentially engage in criminal activity in different locations. In particular, individuals may reside in one municipality while delivering food or offending in nearby areas. To account for such spatial spillovers, we allow platform entry to affect outcomes beyond municipal boundaries by defining treatment exposure using a 15-kilometer buffer around each police jurisdiction. Under this exposure definition, a jurisdiction is treated in the first year in which a platform enters either the jurisdiction itself or a municipality within 15 kilometers of its boundary.

In robustness checks, we vary the size of the buffer to assess the sensitivity of the estimates to the exposure definition. The 15-kilometer buffer aims to capture nearby jurisdictions whose residents can access platform work by commuting, while limiting the inclusion of more distant, weakly exposed jurisdictions. Smaller buffers may classify exposed jurisdictions as untreated, whereas larger buffers may blur the comparison between treated and untreated jurisdictions.

Figure 3 illustrates the spatial motivation for this exposure definition. It shows the

police jurisdictions of region Île-de-France in 2016. Deliveroo and Uber Eats began operating in Paris (red hatched area) in 2015. Some of their riders lived in the surrounding suburbs and commuted into the city. Rider density is highest in the darker suburban jurisdictions, measured as riders per 10,000 residents. To capture Paris’s catchment area, the figure also displays a 15-km buffer around Paris (red dashed line). Appendix Table A8 reports average demographic characteristics, labor-market indicators, and crime rates for police jurisdictions that are eventually treated and those that are never treated, measured in 2014 and 2019. Before entry, in 2014, treated jurisdictions are substantially more populous than never-treated jurisdictions, whereas labor-market indicators and crime rates are similar across the two groups. By 2019, delivery-worker registrations are markedly higher in treated jurisdictions, as expected following platform entry.

5.3 Rider Supply, Labor-Market, and Crime Measures

We examine how the rollout of food-delivery platforms affects rider supply, local labor-market conditions, and crime. Rider supply captures the most immediate response to entry: whether platforms attract workers. Labor-market outcomes capture changes in unemployment and inactivity, whereas crime outcomes measure changes in criminal activity. All outcomes are aggregated annually at the police-jurisdiction level, the spatial unit used in administrative crime statistics. Our analysis covers 681 police jurisdictions between 2012 and 2019, yielding a balanced panel of 5,448 jurisdiction-year observations.

Rider Supply. We measure rider supply using administrative business records of self-employed micro-entrepreneurs operating under the food-delivery activity code (see Section 4 and Appendix H). For each police jurisdiction and year, we compute the annual stock of active riders, weighting each registration by the fraction of the year for which the business is active, based on creation and closure dates. For example, a registration active for six months receives a 0.5 weight. The data identify each rider’s gender and nativity (French-born versus foreign-born), which lets us estimate whether platform entry attracts demographic groups at different rates.

Labor-Market: Unemployment and Inactivity. Labor-market and population indicators come from INSEE administrative census statistics.³⁰ The data cover unemploy-

³⁰See [Employment and Labor Force Participation in 2019 - Population Census](#).

ment and inactivity, two relevant outcomes because gig work attracts individuals with weak prior attachment to the labor market. In the empirical analysis, we report effects on both outcomes for the overall working-age population, and then by gender and nativity. This disaggregation matches the rider demographics documented above, with men and migrants overrepresented among riders.

Groups are defined by gender and by place of birth (French-born or foreign-born), and by age where indicated. We measure the unemployment of specific groups using each group’s unemployment share as the number of unemployed individuals in a group divided by the group population. Unlike the standard unemployment rate, the denominator is the group population rather than the labor force, so it is not mechanically affected by changes in labor force participation. We measure the group inactivity as the number of inactive members, excluding students and (pre-)retirees, divided by the group population. This restriction focuses the analysis on the population available to participate in the labor market.

Crime: Police and Courts. We draw on two complementary administrative sources to measure criminal activity (see Appendix H).

- *Police-recorded offenses.*³¹ The French Ministry of Interior reports offenses at the police-jurisdiction level using the *État 4001* classification, which covers property crimes (e.g., theft and burglary), drug-related offenses (e.g., possession and dealing), and violent crimes (e.g., assault). The data allow us to study how platform entry affects overall crime and specific offense categories. We focus on offenses plausibly responsive to changes in low-skill legal income opportunities, such as shoplifting, drug possession, and minor theft.
- *Court records and age-based eligibility.*³² To exploit the legal minimum age requirement for platform work, we complement police data with case-level court records that report the age of the offender at the time of the offense. Court data have a narrower scope, excluding the most severe offenses and dismissals, but they let us compare crime responses for individuals below and above the minimum age threshold for gig work. This age-based comparison provides a key test of whether crime

³¹The data can be downloaded from [DataGouv.fr](https://data.gouv.fr) (last updated February 1, 2022). See Appendix H.2.1.

³²See Appendix H.2.2 for details.

changes operate through labor-market access, rather than through mechanisms that would affect offenders of all ages.

None of the crime data can be linked to rider, employment, or business records at the individual level. Police statistics are released only as offense counts by jurisdiction. Court and criminal-record files use justice-system-specific pseudonymized identifiers that cannot be matched to those used in labor-market and business registries. French law also prohibits linking the national criminal record to any file held outside the Ministry of Justice (Article 777-3 of the Code of Criminal Procedure). We therefore cannot observe whether individual riders offend, and we estimate the effect of access to platform work on crime at the area level. The eligibility tests for age and criminal record in Sections 7.1 and 7.2) use the individual characteristics that the justice data record, offenders' age and prior convictions, to bring the analysis as close to the individual level as the data allow.

6 Main Results

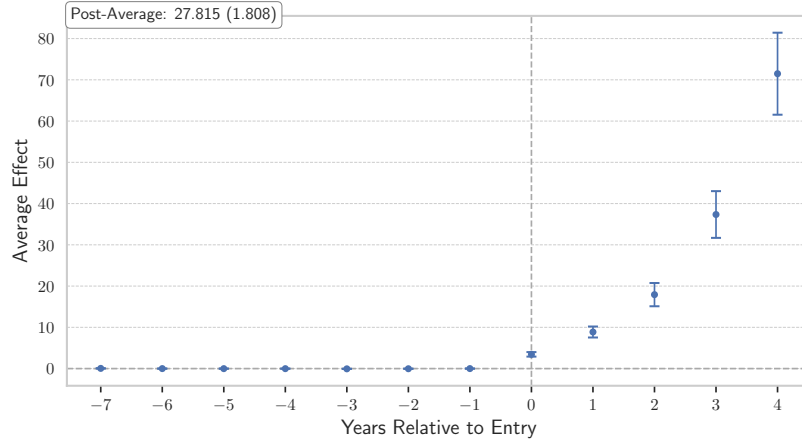
6.1 Platform Entry and Rider Supply

We begin by examining how the arrival of food-delivery platforms affects the stock of active riders in treated areas. To assess whether entry translates into rider take-up, we estimate the event-study specification in Equation 1 using rider registrations as the outcome. Figure 4 shows a clear increase in rider registrations following platform entry, but the response is not immediate. Rather than a sharp jump at event time 0, the stock of active riders rises gradually over subsequent years. This gradual response may reflect the time needed for information to spread and for platforms to recruit and onboard riders, even when formal entry costs are low.

Once uptake begins, growth is sustained. On average, platform entry leads to an increase of approximately 28 active riders per 10,000 working-age residents in the post-entry period (Panel A). This aggregate effect, however, masks substantial heterogeneity across demographic groups. Panel B shows that the stock of active riders rises almost exclusively among men: rider supply increases by roughly 54 per 10,000 male residents, compared to only about 3 per 10,000 female residents. Panel C shows that take-up also differs by nativity among men. Platform entry increases the stock of foreign-born male riders by approximately 37 per 10,000 foreign-born male residents, whereas the

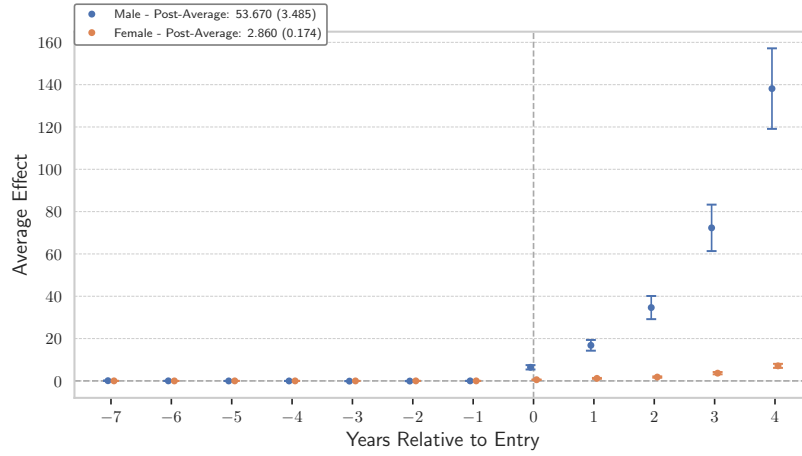
Figure 4: Impact of Platform Entry on Stock of Active Riders

A. Overall



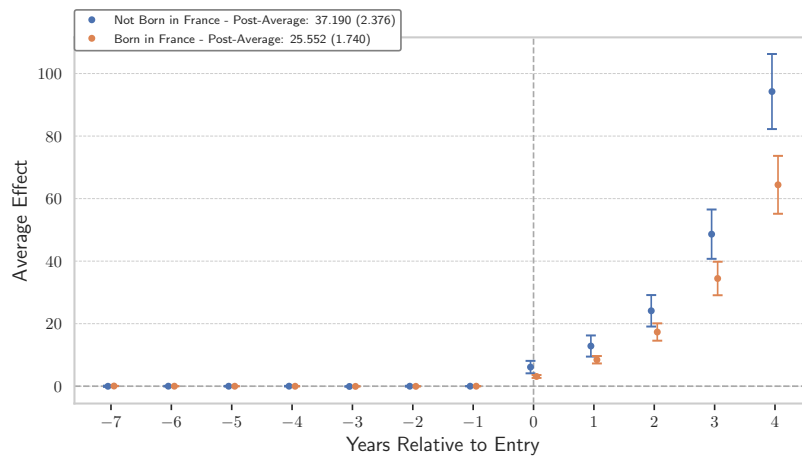
Post-entry average per 10,000: +28 riders

B. Male vs. Female



Post-entry average per 10,000: Male: +54; Female: +3

C. Male: Foreign-born vs. French-born



Post-entry average per 10,000: Foreign-born: +37; French-born: +26

Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a platform operates within a police jurisdiction or its 15-km buffer. The vertical dashed line marks event time 0. Outcomes are annual rider registrations per 10,000 residents aged 15-54: Panel A: all riders; Panel B: gender-specific (per 10,000 men and women); Panel C: nativity-specific (per 10,000 foreign- and French-born residents). Post-entry values: averages across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

corresponding increase among French-born men is about 26 per 10,000. These magnitudes indicate that platform expansion attracts men, while take-up among women is small. The higher take-up among foreign-born men may reflect weaker attachment to the formal labor market. Importantly, all effects are scaled relative to the size of each group’s population, so the stronger responses reflect higher participation rates rather than differences in group size.

Across all panels, pre-entry coefficients are statistically indistinguishable from zero, providing no evidence of differential pre-entry trends in rider supply. After entry, the stock of active riders rises steadily, showing that platform entry is followed by sustained rider take-up.

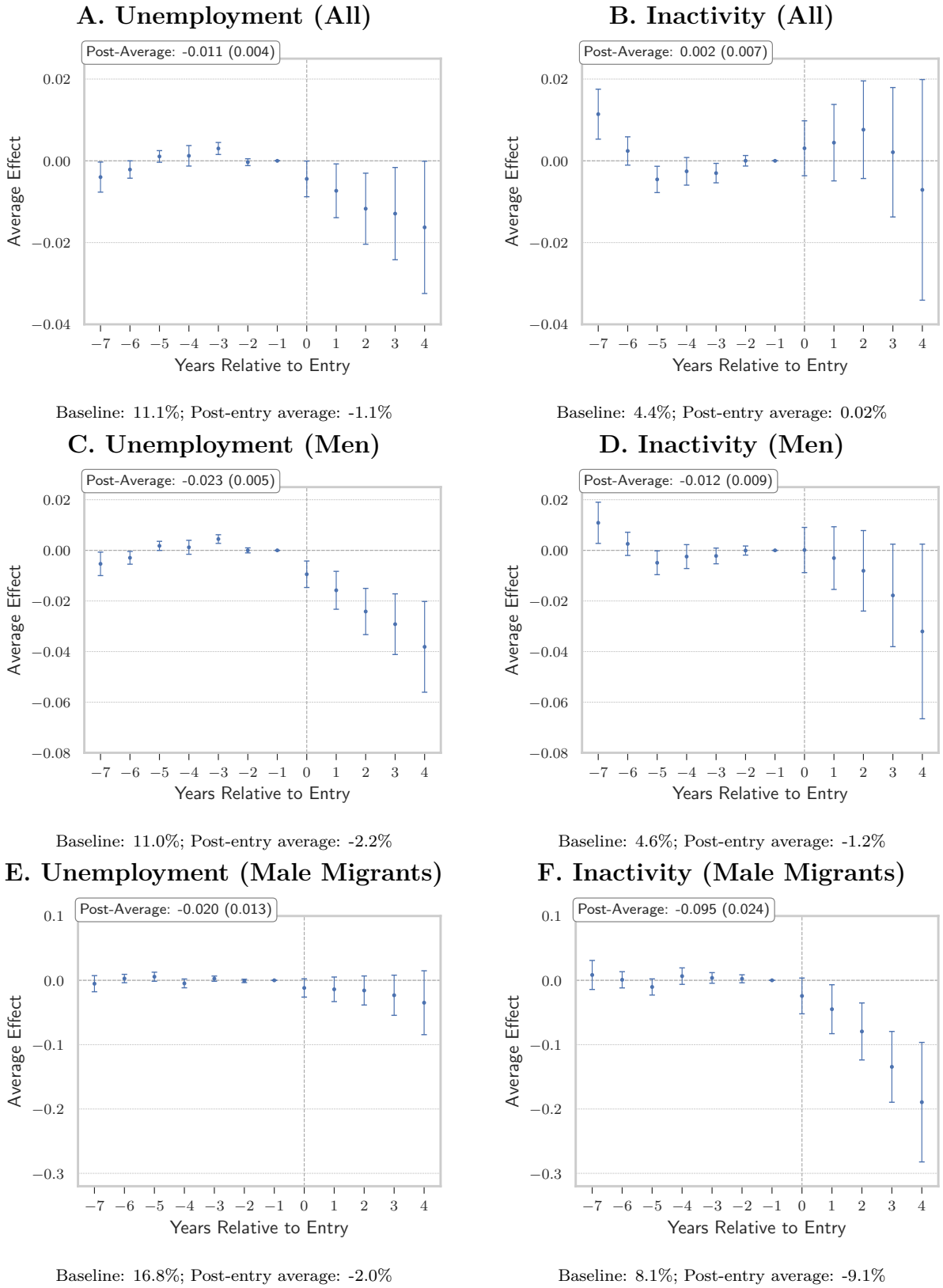
6.2 Labor-Market Outcomes

We now examine whether the expansion of platform-based gig work translates into broader changes in local labor-market outcomes. We estimate the event-study specification in Equation 1 using unemployment or inactivity rates as outcomes. Platform entry increases the stock of active riders by about 28 per 10,000 working-age residents (aged 15-54). While sizable relative to the pre-existing rider population, this increase represents a modest shock to the local labor market as a whole, so we would not expect large aggregate effects on unemployment or inactivity. Panels A and B of Figure 5 show that aggregate unemployment falls by 1.1% in the post-entry period, equivalent to 0.1 percentage point relative to its 2014 baseline, while aggregate inactivity changes little.

The modest aggregate response masks substantial heterogeneity. Panels C and D focus on men, who account for the vast majority of rider take-up. Male unemployment falls by about 2.2% on average in the post-entry period. The decline emerges gradually after entry, mirroring the sustained increase in the stock of active riders documented above. This effect points to labor-market activation rather than short-run reallocation. Male inactivity also declines by about 1.2%, but this estimate is imprecise.

The strongest response is the decline in inactivity among foreign-born men (Panel F of Figure 5). Their unemployment share (Panel E) falls by about 2.0%, similar to the 2.2% decline among men overall, but the estimate is imprecise. By contrast, their inactivity share falls by about 9.1%, equivalent to roughly 0.7 percentage point from a 2014 baseline of 8.1%. We also expect the response among male migrants to decline with age. Appendix

Figure 5: Impact of Platform Entry on Labor-Market Outcomes



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a food-delivery platform operates within a police jurisdiction or its 15-km buffer. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Baselines are from 2014. Migrants are defined as foreign-born. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals. Number of jurisdictions excluded due to zero values in the outcome variable: 1 (Panel E) and 3 (Panel F).

Figure A9 confirms this age gradient. The decline in inactivity is larger for younger foreign-born men: about 13% among those aged 15-24 and 8% among those aged 25-54, with no detectable change among those aged 55-65.

The decline in inactivity is consistent with some foreign-born men entering the labor force from nonparticipation. This interpretation aligns with the descriptive evidence (see Section 4.2). Appendix Table A4 shows that riders are overrepresented in areas with high immigrant shares and weak labor-market conditions. Meanwhile, Appendix Table A5 shows that many riders enter delivery work from outside regular employment. The age gradient also matches the rider profile documented above, with riders predominantly young, male, and often foreign-born.

We also study female labor-market outcomes, although the small number of women registering as riders suggests that any effects should be limited. Unemployment effects are indeed close to zero for women overall and for foreign-born women (see Panels A and C of Appendix Figure A10). Effects on inactivity are slightly larger and mixed. Inactivity among women overall increases by about 1.8%, equivalent to 0.08 percentage point (p.p.) from a 4.3% baseline, whereas inactivity among foreign-born women declines by 2.3%. This decrease is equivalent to 0.16 p.p. from a 7% baseline and is imprecisely estimated (see Panels B and D of Appendix Figure A10). One potential concern raised by the small increase in female inactivity is whether platform entry reallocates labor demand away from jobs that employ many women, such as restaurant service and supermarket retail, toward male-dominated delivery work.

We therefore examine employment responses in restaurants and supermarkets. These sectors employ similar worker profiles and are closely tied to local consumption demand. Appendix Figure A11 shows that restaurant employment changes little (-0.5%), whereas supermarket employment falls by 1.7%; both estimates are however imprecise. At the same time, the number of restaurants and supermarkets per capita rises by about 2.1% and 4.5%, respectively. These sectoral estimates do not provide clear evidence of employment displacement. The significant increase in the number of establishments without corresponding employment growth instead points to a reorganization of local businesses, potentially toward smaller or more delivery-oriented establishments.

Overall, the labor-market evidence indicates that platform entry operates through activation at the extensive margin. It offers work opportunities to men, especially male

migrants, into work who might otherwise remain inactive. The absence of aggregate employment effects reflects the modest scale of platform jobs relative to local labor markets, not the absence of meaningful adjustment for the populations most affected by platform access. These patterns directly motivate the analysis of crime outcomes that follows.

6.3 Crime Outcomes

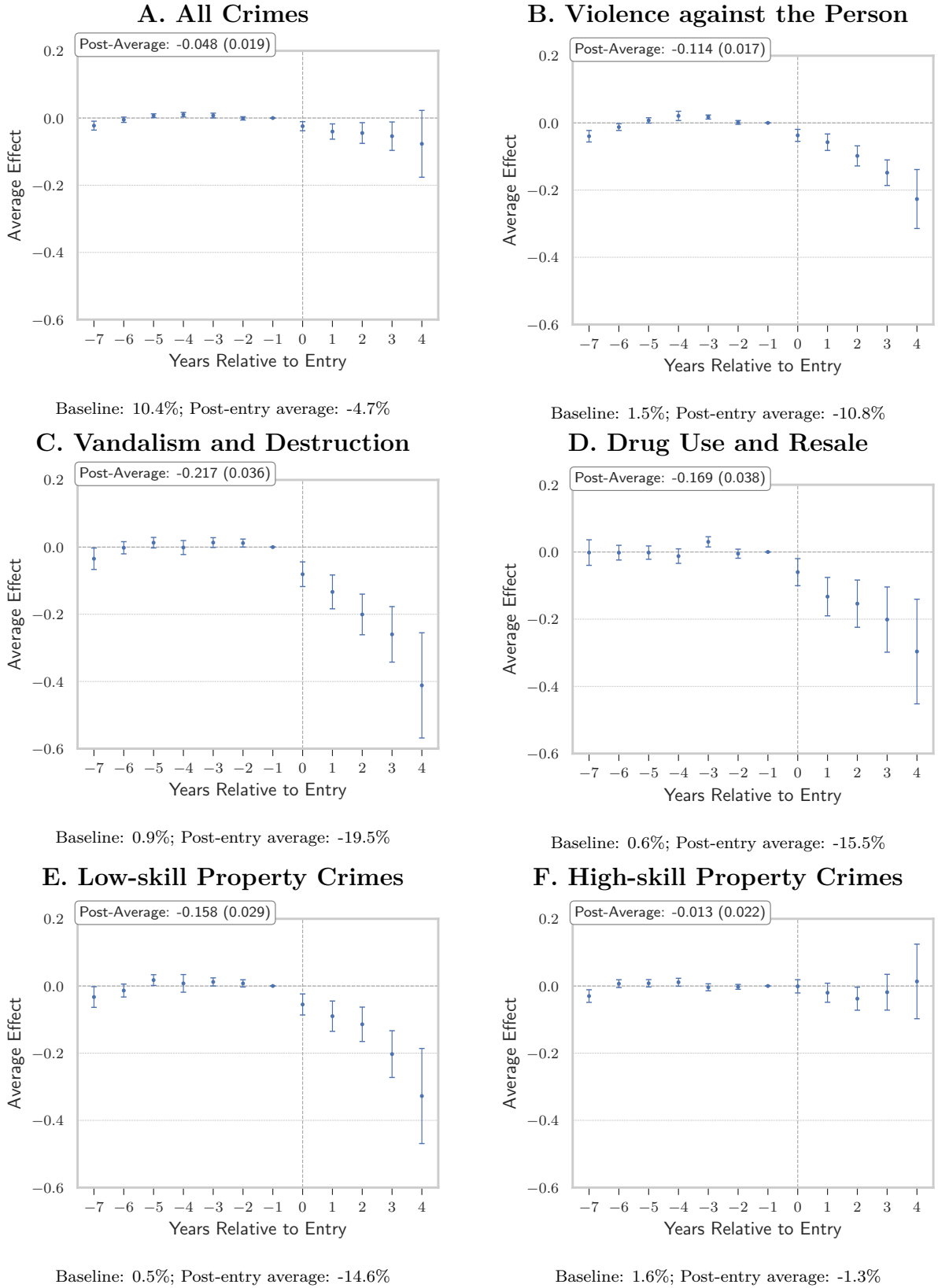
We now investigate how the entry of food-delivery platforms affects criminal activity. Figure 6 presents event-study estimates for a range of crime categories. The pre-entry coefficients do not display a trend that mirrors the post-entry declines. The [Rambachan and Roth \(2023\)](#) sensitivity analysis confirms that the main crime estimates remain robust to meaningful departures from parallel trends (see Appendix R).

Following entry, recorded crime falls across several categories, with the largest declines in destruction of property, drug offenses, and low-skill property crime. These reductions are consistent with both mechanisms outlined in Section 2: economic deterrence and self-incapacitation. Access to legal earnings raises the opportunity cost of crime, whereas time spent working reduces opportunities to offend.

Panel A of Figure 6 shows that overall recorded crime declines by about 4.7% after platform entry. The decline is more pronounced for violence against the person, a major subcategory of recorded offenses (-10.8%; Panel B). The 95% confidence intervals for both post-entry averages exclude zero. The larger decline in violence is consistent with self-incapacitation. Time spent on delivery work may reduce exposure to situations in which interpersonal conflict occurs.

The largest percentage declines occur for vandalism and destruction of property (-19.5%; Panel C) and for drug-related offenses (-15.5%; Panel D). The 95% confidence intervals exclude zero for both estimates. The decline in vandalism is consistent with self-incapacitation because these offenses are concentrated among adolescents and young adults and may respond to a reduction in unstructured time. The reduction in drug offenses aligns more closely with an income-substitution mechanism. For individuals facing barriers to formal employment, platform work provides a legal, accessible alternative to low-level drug dealing. This interpretation echoes the qualitative motivation discussed in the introduction, where a rider describes delivery work as preferable to “more nefarious ways of making money” (New York Times, June 16, 2019).

Figure 6: Impact of Platform Entry on Crime



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a food-delivery platform operates within a police jurisdiction or its 15-km buffer. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. The vertical dashed line marks event time 0. Baselines are from 2014. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals. One jurisdiction is excluded due to zero values in the outcome variable (Panel E).

Distinguishing property crimes by skill intensity provides a further comparison. Low-skill property crimes such as shoplifting and street robbery fall by about 14.6% following platform entry (Panel E). These offenses require little planning or specialization. In contrast, high-skill property crimes such as burglary and vehicle theft have a post-entry estimate of -1.3%, with a 95% confidence interval that includes zero (Panel F). These crimes require more planning, coordination, or technical know-how. This is consistent with a larger response among opportunistic, non-specialized offenses than among crimes requiring more planning.

Overall, the pattern of results aligns well with the mechanisms outlined in our conceptual framework (Section 2). Platform work increases the relative attractiveness of legal income for individuals facing labor-market exclusion (*economic deterrence*) and reduces unstructured time during evenings and weekends when opportunistic crime is most likely (*self-incapacitation*). The reduction in low-skill, short-horizon offenses, but not in more organized or skill-intensive crimes, is consistent with platform entry shifting behavior among individuals at the margin between illicit and legal income sources.

Crime Effects in Light of Rider Take-Up

To assess whether the estimated crime effects are economically meaningful, we relate the magnitude of rider take-up to the implied reductions in criminal activity. Table 1 reports post-entry average effects and translates the log estimates into absolute changes using baseline crime rates per 10,000 residents. Platform entry increases the stock of active riders by about 28 per 10,000 working-age residents, and by roughly 54 per 10,000 among men. Although this expansion is modest relative to the size of local labor markets, it is concentrated among men, especially foreign-born.

As documented in Section 4.2, riders are men aged 18-25, frequently inactive or unemployed prior to entry, and disproportionately from migrant backgrounds. Because French administrative crime data do not permit the construction of statistics by ethnicity or ancestry, we rely on a national security survey to characterize differential exposure to police contact across groups. The survey reports that young men perceived as Black, Arab, or North African are four times as likely to report an identity check and 12 times as likely to report an in-depth check as the rest of the population.³³ The survey postdates our sam-

³³2025 report of the Defender of Rights (*Défenseur des droits*) on [Police/citizen relations: identity checks and filing complaints](#).

Table 1: Translating Estimated Effects into Absolute Crime Reductions

	Post-average Effect	Baseline per 10,000 population	Absolute Effect
Active riders	+27.8***	—	+27.8
Active male riders	+53.7***	—	+53.7
Active foreign-born male riders	+37.2***	—	+37.2
All crimes	-4.7%**	1038	-48.8
Violence against the person	-10.8%***	153	-16.5
Vandalism/destruction of property	-19.5%***	90	-17.5
Drug use and resale	-15.5%***	59	-9.1
Low-skill property crimes	-14.6%***	51	-7.4

Notes: Baselines represent average crime rates per 10,000 population aged 15-54 in 2014. Absolute effects are computed by multiplying the post-average effect by the baseline values. Estimates for active riders are extracted from regressions presented in Figure 4: (A) all ; (B) male, and (C) foreign-born. Crime estimates correspond to Figure 6: (A) all; (B) violence against the person; (C) vandalism and destruction of property; (D) Drug use and resale; (E) low-skill property crimes. Significance levels: * $p < 0.1$; ** $p < 0.05$ and *** $p < 0.01$.

ple period, so it is informative about relative exposure across groups rather than about levels in 2014-2019. The survey therefore provides a proxy for differences in exposure to police contact and arrest. The same groups exhibit the largest take-up of platform work following entry.

Table 1 shows that the largest percentage declines among the crime subcategories occur for vandalism, drug-related offenses, and low-skill property crime. These offenses require relatively little capital or specialization and can generate income to meet short-run needs. A shift of several dozen young men per 10,000 residents into delivery work, which offers immediate employment and predictable daily earnings, could plausibly generate reductions of this size. The magnitudes are thus consistent with targeted responses among populations at the margin between informal and illegal work. For these groups, access to low-barrier gig jobs can expand the set of feasible earning opportunities.

Comparing magnitudes side by side, however, cannot establish that the labor-market and crime responses are connected: both could, in principle, reflect distinct developments that happen to coincide in treated areas. To link them, we conduct a mediation analysis that decomposes the total effect of platform entry into a component operating through the platform-induced expansion of riders and restaurants and a residual direct component (see Appendix S for the framework, specifications, and full results). The decomposition ties the two responses together. The expansion of rider and restaurant activity accounts

for 32% to 50% of the estimated crime declines and 46% to 73% of the labor-market improvements, and more than 98% of this mediated effect operates through riders rather than restaurants (Appendix Table A9).

A second step asks whether the crime response to rider supply itself passes through measured labor-market activation. Using entry timing as the source of variation in the local stock of riders, we find that changes in unemployment and inactivity, primarily among men and male immigrants, account for 11% to 20% of the effect of rider supply on crime, with the remainder not explained by these measured outcomes (Appendix Table A10). This pattern supports the dual mechanism outlined in Section 2: part of the crime response reflects economic deterrence through improved labor-market positions, whereas the sizable direct component is consistent with self-incapacitation among individuals whose measured employment status does not change, such as those combining delivery work with other activities.

Drug offenses are the exception in these decompositions. Their reduced-form decline is among the largest, yet little of it passes through the measured rider or labor-market channels. One possible reason is that drug markets were simultaneously reorganizing, with a shift toward online ordering and delivery-based distribution that reduces recorded street-level offenses through channels unrelated to local labor supply. We do not push this interpretation further, but it aligns with the visibility and detection concerns examined in Section 7.4. For the other crime categories, the mediation results are consistent with a common role for the platform-induced expansion of gig employment in the labor-market and crime responses.

7 Mechanism and Robustness

7.1 Age-Based Eligibility Test

A key implication of the mechanisms outlined in Section 2 is that crime reductions should be concentrated among individuals who are eligible to take up platform work. Food-delivery platforms require riders to be at least 18 years old to register as micro-entrepreneurs. This institutional feature provides a sharp test of the mechanism: if platform entry affects crime through access to legal earning opportunities and changes in time allocation, reductions should occur among offenders aged 18 and above, with no

comparable response among minors.

The age comparison is informative because minors account for a substantial share of the offenses studied in France. Using 2019 police records, INSEE reports that the rate at which people are implicated by police peaks at ages 15-19 and then declines with age (INSEE, 2021).

To implement this test, we use court records that report each offender’s age at the time of the offense. We focus on two crime categories: destruction of property and low-skill theft. They satisfy three criteria. First, both can be mapped to police-reported offenses analyzed in our main estimations (Section 6.3), and both are tried in correctional and juvenile courts. Second, both are common among youth. Appendix T.1 shows that offending in these categories peaks at age 16 and remains high through age 20. Offense rates are in fact higher among minors aged 15-17 than among young adults aged 18-20: 5.4 versus 4.3 destructions of property and 5.8 versus 4.9 low-skill thefts per 10,000 residents aged 15-54 in 2014, for age groups of nearly identical size. Minors therefore provide an informative comparison group. They are involved in these crimes but cannot take up platform work. Because the event studies compare each age group in treated areas with the same age group in not-yet-treated areas, the decline of offending with age is common to both groups and does not drive the estimates. Third, both are low-capital, opportunistic offenses that can generate income to meet short-run needs, the margin on which access to legal earnings should operate.

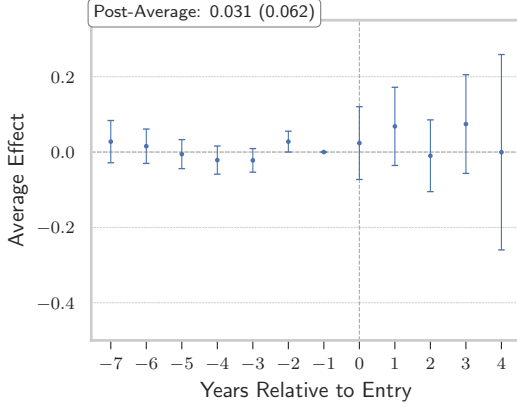
Destruction of property provides the closest match between police and court records, whereas court data understate low-skill theft, as shoplifting and pickpocketing offenders are rarely caught.³⁴

Figure 7 presents event-study estimates separately for minors aged 15 to 17 and young adults aged 18 to 25. We define the adult group to match the population that takes up delivery work: riders register at an average age of 25, and French-born riders are highly concentrated in the 18-24 age range (Section 4.1). Riders under 26 are also those most

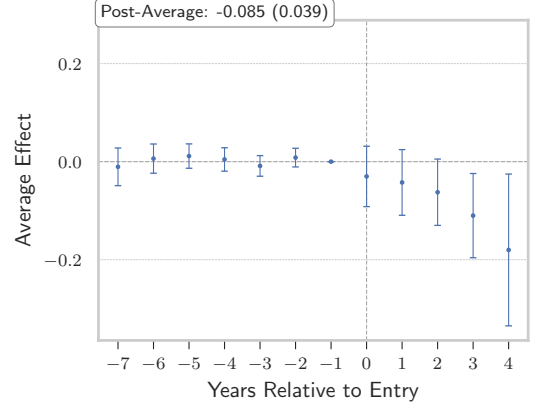
³⁴Drug-related offenses are not suitable for this test because police and court categories do not align well: the distinction between drug use and supply is not comparable across the two sources, and many drug offenses never reach court, as alternative sanctions such as on-the-spot fines for consumption were used during our period of study. Violence against the person shows a similar pattern in court data: no change among minors after platform entry (+2.0%) and declines among adults (−6.1% for ages 18-20, and −6.4% and −5.4% for ages 18-25 and 18-39, both significant at the 5% level). We do not include it in this test because its age profile is less concentrated around the age-18 threshold and its most serious offenses are tried in assize courts, which our court data do not cover.

Figure 7: Crime Effects by Eligibility for Gig Work (Minors vs. Adults)

A. Property Destruction (Aged 15-17) B. Property Destruction (Aged 18-25)

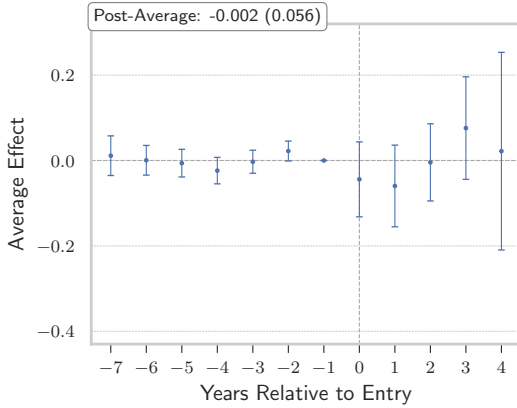


Baseline: 5.4; Post-entry average: 3.1%



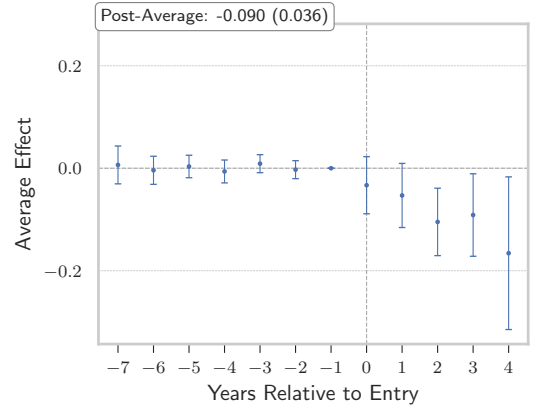
Baseline: 8.7; Post-entry average: -8.2%

C. Low-skill Theft (Aged 15-17)



Baseline: 5.8; Post-entry average: -0.2%

D. Low-skill Theft (Aged 18-25)



Baseline: 10.2; Post-entry average: -8.6%

Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a food-delivery platform operates within a police jurisdiction or its 15-km buffer. Outcomes are the inverse hyperbolic sine of annual court-recorded offenses committed by minors aged 15-17 and by young adults aged 18-25 (the age range in which most riders work; see Section 4.1) per 10,000 residents aged 15-54, at the jurisdiction level. The vertical dashed line marks event time 0. Baselines are 2014 means of these rates, i.e., offenses per 10,000 residents aged 15-54 (not per 10,000 residents of the age group itself; Appendix Figure A25 reports offense counts by single year of age). Post-entry averages equal $(\exp(\text{Post-Average}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals. Offender age distributions and estimates for the narrowest (18-20) and widest (18-39) adult windows are reported in Appendix T.

often drawn from non-employment: two thirds held no job in the month before registration, compared with half of older riders (Section 4.2). For both offense categories, the estimated offense rates for minors show no post-entry change, even though minors commit these offenses at higher rates than young adults just above the threshold. In contrast, estimated offense rates for young adults decline after entry: destruction of property falls by 8.2% and low-skill theft by 8.6%, both significant at the 5% level. The pre-entry coefficients show no evidence of systematic differential pre-trends in either age group.

The results are very similar when we focus on the narrowest window just above the threshold. For young adults aged 18 to 20, destruction of property falls by 11.0% and low-skill theft by 10.1% (Appendix T). These estimates are less precise, as this three-year window contains only about four-fifths as many offenses as the minors' window (77% for destruction and 81% for low-skill theft; Appendix T.1). The declines also remain of similar magnitude when the adult window is widened to ages 18-39. The response to platform entry is thus present just above the eligibility threshold, absent just below it, and stable across the adult age range in which riders are found.

Changes in policing intensity, reporting practices, or broader area-level dynamics would not predict a discontinuity precisely at the legal working-age cutoff.³⁵ Instead, the evidence is consistent with a mechanism in which platform entry expands access to flexible, low-barrier legal work for individuals old enough to register, shifting income opportunities and potentially reallocating time away from activities associated with opportunistic offending. The age-based eligibility test therefore provides the cleanest and most informative quasi-experimental evidence on the mechanism linking platform entry to crime.³⁶

7.2 Criminal-Record Eligibility Test

Age is not the only margin that determines who can take up platform work. Access also depends on an applicant's criminal record. As described in Section 3, platforms check only the B3 extract, the record extract available to private employers, which mainly lists

³⁵We attempted a difference-in-discontinuities design at age 18. However, because the court records report age only in completed years rather than exact birth dates, the running variable has too few support points near the cutoff for reliable local-polynomial estimation.

³⁶We are unable to conduct an analogous age-based test by gender using court data. Women account for less than 10 percent of court-recorded offenses in our sample. The resulting cells are too small to support reliable event-study estimates at the police-jurisdiction level.

unsuspended prison sentences of more than two years. They therefore accept applicants with most other convictions, which nonetheless appear on the B2 extract checked by public employers and regulated occupations (see footnote 19). If expanded access to legal work drives the crime reductions, reoffending should fall among offenders whose records permit them to register as riders, and much less among offenders whose records exclude them. This provides a second test of the mechanism, using a different eligibility rule and a different data source.

We use the statistical file of the national criminal record (*Casier judiciaire national*), accessed through the CASD. The file covers all convictions pronounced in France and includes a pseudonymized person identifier, which allows us to reconstruct each offender’s conviction history between 2012 and 2019. For every adult conviction, we observe whether the offender had previously been convicted and whether any prior conviction was for a crime-level offense or carried a custodial sentence. We classify repeat offenders with such a conviction in their prior record as *ineligible* for platform work. This is our proxy for the B3 extract that platforms check. We classify all other repeat offenders as *eligible* (minor prior convictions only).

The sample covers 4.2 million adult convictions. Because offenses committed before age 18 are tried by juvenile courts, every offender in this sample was old enough for platform work. The criminal record is therefore the only eligibility margin that varies. We infer B3 status, which is not disclosed in the data, from conviction history, accounting for sentence length, suspension, and later removal from the record.³⁷ Our deliberately broad proxy counts suspended prison sentences, which do not appear on the B3 extract, so many offenders we classify as ineligible could in fact register as riders. Thus, two thirds of the 1.7 million convictions of offenders with a prior conviction fall in the ineligible group. If this misclassification is unrelated to platform entry, it attenuates the difference between the two groups. The estimated difference therefore provides a lower bound on the true difference in reoffending responses. Appendix U describes the data, the construction of conviction histories, and the validation of the person identifier. Appendix Figure A28 provides a first check. Adult convictions and the convictions of repeat offenders both fall after platform entry, with no pre-entry trends.

This test is rougher than the age-based test, because the two data sources geolocate

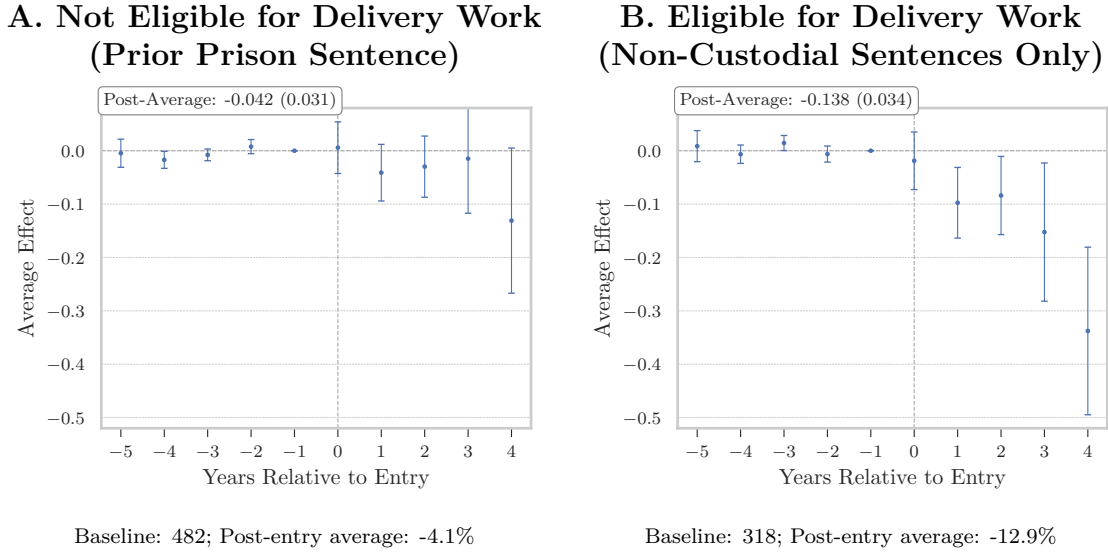
³⁷No official statistics report how many people have a conviction recorded on the B3 extract, and statisticians at the Ministry of Justice we contacted could not point us to any such figure.

offenses differently. The court records used in Section 7.1 come from the courts’ case-management system (*Cassiopée*), which records the municipality where each offense was committed. This allows us to aggregate them to the 681 police jurisdictions used throughout the paper. The criminal-record file is instead a register of persons and does not record where the offense took place or where the offender lives. The only geographic information it contains is the court that pronounced the conviction. We therefore aggregate convictions to the catchment areas of first-instance courts (*tribunaux judiciaires*), 160 areas in mainland France. This geographic aggregation preserves a local link to each case. A criminal court can try an offense only if it was committed within its area, or if the defendant lives or was arrested there. We date platform entry by the year in which a platform first operated in the city where the court sits. Appendix Figure A27 maps these court areas and their entry dates. Court areas are much larger than police jurisdictions and measure entry less precisely. Both features should attenuate the estimated effects. Otherwise, we use the same staggered difference-in-differences estimator, and we date each conviction by the year in which the offense was committed.

We first verify that the criminal-record data reproduce our main findings. After platform entry, adult convictions fall by 12.7% and convictions of offenders with a prior conviction by 11.3%, with no evidence of pre-entry trends (see Appendix Figure A28). Figure 8 then separates reoffending by type of prior record. Among offenders whose records permit platform work, reoffending falls by 12.9% on average after entry, and the decline deepens every year, reaching about 29% four years after entry. Among offenders whose records exclude them, reoffending declines by only 4.1%, and this estimate is not statistically significant. A decline appears in this group only four years after entry, and it remains imprecise. This late decline is consistent with our classification rule. In our data, an offender remains ineligible once a qualifying conviction is observed, whereas in practice convictions are removed from the criminal record after statutory rehabilitation periods. Many offenders we classify as ineligible can therefore register as riders, including those whose only qualifying sentence was suspended. Their share grows as more time passes since conviction. The pattern mirrors the age-based test: reoffending more among offenders classified as eligible for platform work than among those classified as ineligible.

These results speak to the question of whether access to work reduces reoffending among people with criminal records. In the United States, [Agan et al. \(2024\)](#) find that

Figure 8: Reoffense Convictions by Eligibility for Gig Work (Prior Prison vs. Non-Custodial Sentences)



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Data: statistical file of the national criminal record (*Casier judiciaire national*), 2012-2019. Unit of observation: 160 catchment areas of first-instance courts (*tribunaux judiciaires*) by year of offense. Platform entry: first year a food-delivery platform operates in the city where the court sits. Outcomes are the log number of adult convictions of offenders with at least one prior conviction observed since 2012. Panel A includes offenders with a prior prison sentence, suspended or not, or a prior crime-level conviction. These criteria provide a broad proxy for a nonblank B3 extract (ineligible for platform work). Panel B includes all other repeat offenders (eligible for platform work). Baselines are 2014 mean counts per court area. Event time is binned at -5 and $+4$, with year -1 as the reference. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the court-area level. Bars show 95% confidence intervals. Joint tests that all pre-entry coefficients are zero: $p = 0.11$ (Panel A) and $p = 0.22$ (Panel B). Appendix [U](#) describes the data construction.

people whose felony convictions are reclassified as misdemeanors turn to platform-based gig work rather than to regular wage employment. This result suggests that gig work can provide legal income for people with criminal records. Our estimates show that when gig work becomes locally available, offenders whose records allow them to take it reoffend less. The contrast with offenders whose records exclude them is consistent with access to legal work reducing reoffending, rather than other changes in treated areas.

Three features of the data make this test conservative. First, our classification is broader than the B3 extract and does not account for the removal of convictions over time, so many offenders classified as ineligible are in fact eligible. Second, we observe conviction histories only from 2012 onward, so some offenders with older prison sentences are classified as eligible. Both forms of misclassification push the two estimates toward each other, so the true gap between eligible and ineligible offenders is likely larger than we estimate. Third, the coarser geography attenuates both estimates.

7.3 Platform Entry Beyond Major Urban Districts

A concern is that the estimated effects are driven by France’s largest urban centers, where food-delivery platforms first entered. Labor-market conditions, policing intensity, and crime dynamics may differ from those in smaller jurisdictions. If platform rollout coincided with broader economic or institutional changes specific to these urban cores, our estimates could reflect such confounding trends rather than the expansion of access to gig work.

To address this concern, we re-estimate our baseline specifications after excluding the police jurisdictions covering the urban cores of Paris, Marseille, and Lyon, the three most populous cities in France. Together, these cities account for nearly three million working-age residents. We retain suburban police jurisdictions located within 15 km of the boundaries of these cities. Although delivery demand and restaurant coverage are concentrated in city centers, many riders reside in surrounding suburbs and commute into the urban core to work. As shown in the spatial rider maps in Appendix J, the residential distribution of riders is more spatially dispersed than the geography of platform restaurant operations. These suburban jurisdictions remain exposed to platform entry. Residents gain access to platform work when a platform enters a nearby city, even if it has not entered a municipality within their own jurisdiction. This feature allows us to isolate the labor-supply channel from confounding changes in local commercial activity, foot traffic, or city-center policing.

Re-estimating our labor-market and crime specifications on this restricted sample yields estimates close to the baseline. Platform entry continues to generate a strong increase in rider registrations, and declines in male unemployment and inactivity among foreign-born men. The estimates for destruction of property, drug offenses, and low-skill property crime also remain similar. Appendix Figures A14 and A15 report the estimates.

This robustness exercise shows that the main results are not driven by dynamics specific to major metropolitan centers, nor by changes in city-center delivery markets or consumption patterns. Instead, the evidence supports a mechanism operating where riders live: platform entry expands access to flexible, low-barrier legal work in residential areas, and this expansion underlies the observed reductions in crime.

7.4 Could Changes in Movement or Detection Explain Crime Reductions?

An alternative interpretation of the crime reductions documented above is that they stem from changes in opportunities for offending or in the visibility of offenses, rather than from changes in underlying criminal behavior. Platform entry could shift public activity if consumers stay home and order meals instead of going out. It could also affect detection and reporting. Riders circulating in public space may act as eyes on the street, raising the risk that offenders are seen ([Matheson et al., 2026](#)). Such guardianship would deter minors and adults alike, yet offending falls only among adults eligible for delivery work (see Section 7.1). We therefore examine outcomes that would respond mechanically to changes in public activity or detection.

A first possibility is that platform rollout changes public activity. Such a shift could mechanically affect crime by changing the supply of potential victims rather than offenders' behavior. Two patterns argue against this interpretation. First, burglary rates remain flat following platform entry (see Appendix Figure A29a). If more people were staying at home, burglaries should fall because dwellings would be less often unattended, as shown by the decline in burglary that followed the rise of working from home ([Matheson et al., 2026](#)). The absence of any such decline argues against a large, purely mechanical “stay-at-home” effect.

Moreover, we observe marked declines in shoplifting and other low-skill theft offenses (see Appendix Figure A29b). Unlike street crime, shoplifting does not require potential victims to be present in public space. The decline in shoplifting is therefore difficult to attribute solely to reduced victim exposure and is more consistent with a change in offending behavior.

A related concern is selective residential sorting. Potential riders, particularly young migrant men, might move to jurisdictions where gig-work becomes available. Such sorting could mechanically affect local outcomes. Appendix Figure A29c addresses this concern by plotting an event study of the log population of male migrants aged 15-54. Population levels are flat before and after entry, with no detectable post-entry response. This result provides no evidence that selective in-migration drives our results.

A second possibility is that declines in drug-related offenses reflect changes in how drugs are distributed rather than changes in the offenses themselves. Journalistic accounts

suggest that app-based ordering and delivery of cannabis expanded during the late 2010s and the COVID-19 period, potentially reducing the visibility of transactions and lowering recorded offenses even if the underlying activity remained unchanged. Although this interpretation cannot be ruled out entirely, several features of the data make it unlikely to explain our findings. The declines in drug offenses appear immediately following platform entry, whereas the diffusion of app-based drug delivery is documented to lag by several years. Moreover, we observe contemporaneous reductions in vandalism, low-skill theft, and violence—offenses that are not plausibly affected by changes in drug distribution technology. The alignment of effects across these categories points to a common behavioral mechanism rather than to changes in detection specific to drug markets.

Both remaining measurement concerns, moreover, push in the same direction: toward understating the true effects. First, registrations do not capture all platform work. Some individuals deliver under accounts rented from registered riders (Section 3), so the true expansion of gig work—and hence the treatment whose effect we measure—is larger than our administrative registration counts. Second, while Appendix Figure A29c shows no significant change in the male migrant population of treated areas, the point estimates are mildly positive; any such compositional inflow of young men would, all else equal, raise local offense counts. Under either scenario, our estimates understate the crime-reducing effect of access to gig work and are best read as lower bounds.

These findings mitigate concerns that changes in public movement, residential sorting, or offense-specific visibility explain the full pattern of crime declines. The declines across several offense categories remain consistent with platform entry expanding access to legal earnings for young men with weak labor-market attachment and changing how they allocate their time.

8 Conclusion

This paper examines how food-delivery platforms shape local labor markets and crime in France. Using staggered platform entry and administrative data, we show that rollout increases participation in gig-work, particularly among young men from immigrant origin with weak prior attachment to wage employment. Crime rates fall after entry. The declines concentrate on offenses that are most prevalent among this group, such as drug

offenses and low-skill property crime.

The labor-market and crime effects closely align. Platform entry expands access to legal earning opportunities for groups facing structural barriers to formal employment. The resulting activation into work is visible in the decline of inactivity and unemployment among male migrants, but does not significantly impact overall employment or unemployment rates. Correspondingly, crime declines most in destruction of property, drug offenses, and low-skill property crime, a pattern consistent with economic deterrence and self-incapacitation. We find no clear evidence of displacement from traditional low-skill sectors: neither restaurant nor supermarket employment declines. On the contrary, establishment counts in these sectors slightly increase, suggesting mild commercial expansion rather than job crowd-out.

Robustness checks reduce concerns that unrelated urban changes explain the crime decline. Excluding the three largest cities, while retaining their surrounding commuter suburbs where riders live, yields similar labor-market and crime estimates. Results are also similar across alternative staggered difference-in-differences estimators. Crime declines are also not driven by shifts in public activity or policing intensity: burglaries remain flat, bar activity is stable, and shoplifting (which does not depend on victim presence) declines markedly. Although a shift toward less visible drug transactions cannot be entirely ruled out for drug offenses, the declines among offenders just above the age-eligibility threshold but not among minors remain consistent with economic deterrence and self-incapacitation.

The evidence points to a specific role played by gig work in contexts of labor-market exclusion. Platforms lower the barriers to legal employment by reducing screening frictions and credential requirements. For individuals at the margin of legal and illegal income opportunities, the availability of flexible, low-barrier work shifts behavior away from criminal activity. In this sense, gig jobs function as a short-run outside option that reduces involvement in entry-level crime. However, these short-run benefits should not be interpreted as evidence of upward labor-market mobility. Recent work by [Adermon and Hensvik \(2022\)](#) shows that, for applicants whose names signal majority-group status, gig experience raises callback rates relative to unemployment but less than traditional work experience. However, neither type of experience raises callback rates for applicants whose names signal minority-group status (Arabic-sounding names in their study). Gig work may therefore improve job prospects relative to unemployment for some workers, but it

is unlikely to resolve deeper structural barriers in the labor market.

This pattern suggests a broader implication: improving access to legal income opportunities for disadvantaged groups may reduce crime, even when those opportunities are temporary, flexible, and low-paid. The criminal-record test supports this implication. Re-offending falls among offenders whose records still allow them to work on platforms, and much less among those whose records exclude them. Policies that keep legal jobs open to people with criminal records may therefore reduce reoffending, a conclusion that has so far rested almost entirely on evidence from the United States. The longer-run question is whether such jobs can lead to stable employment. [Degenhardt and Nimczik \(2025\)](#) find that access to food-delivery gig jobs improves initial employment for refugees but yields no lasting employment advantage. This result underscores the need for further research on how short-run access to gig work can be paired with durable integration policies.

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Appendix

A A Simple Conceptual Framework

This simple framework illustrates how delivery-platform entry can reduce participation in low-skill, income-generating crime through economic deterrence and self-incapacitation, represented by lower barriers to legal work and a higher time-related cost of offending.

Delivery Platforms — There are $z = \{1, \dots, Z\}$ economic zones, each representing a distinct location of varying size. Delivery platforms evaluate these locations for potential entry based on the availability of restaurants (supply), consumers (demand), and riders.

Riders — A key difference between delivery platforms and other digital platforms, such as micro-tasking or homestays, is the role of the rider, a gig worker, who connects supply and demand. This gig job is characterized by low barriers to entry and low investment that benefit a specific group g of workers: young, low-skilled, and often migrants.

Group g workers encounter significant barriers to employment in the legal sector (see Appendix B). Let U_L denote the expected utility from low-skilled legal work:

$$U_L = \omega_L - f.$$

where ω_L is the minimum wage, and f summarizes labor-market frictions as a disutility and cost (e.g., hiring-stage discrimination, documentation requirements such as a CV, and search time).

Confronted with barriers to employment, g workers may turn to low-skill and income-generating crime, such as street-level drug dealing and shoplifting. This substitution may happen despite the greater regularity, safety, and benefits of legal jobs. Let U_I denote the expected utility from illegal work:

$$U_I = \omega_I - pC - h,$$

where ω_I is a random variable, with cumulative distribution F , representing the illegal wage derived from low-skill and income-generating crime, p is the probability of getting caught, C is the associated cost (monetary or otherwise, e.g., incarceration), and $h \geq 0$ denotes the disutility of committing offenses during peak-crime hours (evenings/weekends, which overlap with peak delivery hours).³⁸

An individual chooses the illegal job if its expected utility exceeds that of the low-skilled job; that is,

$$\omega_I > \omega_L - f + pC + h.$$

Let $\tau \equiv \omega_L - f + pC + h$ denote the threshold. An individual chooses illegal work when potential illegal earnings exceed this threshold. The probability of choosing illegal work is

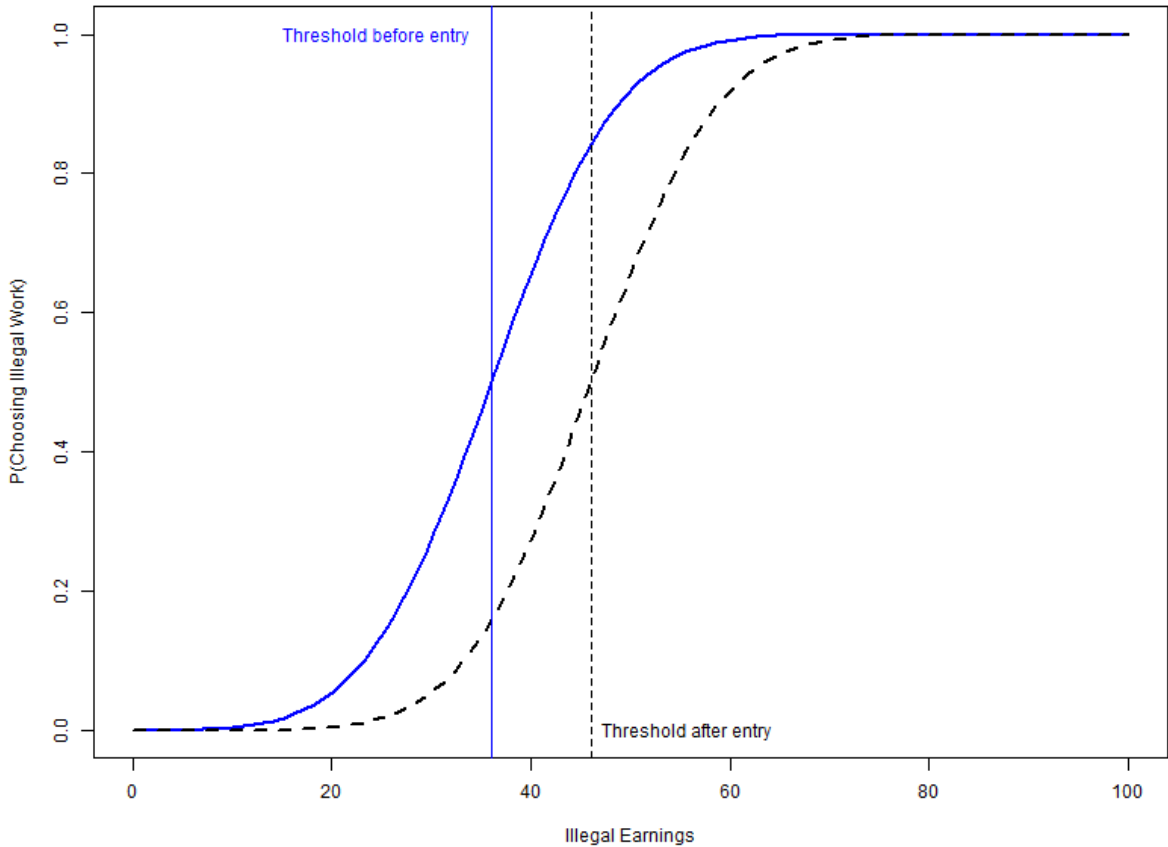
$$P_I = \mathbb{P}(\omega_I > \tau) = 1 - F(\tau),$$

that is, the likelihood that potential illegal earnings exceed the net payoff from legal work.

³⁸For the sake of illustrating the partial effect of platform entry, we assume simple linear utility functions. For instance, the effort/disutility from work is assumed to be equal across activities, and the offender retains ω_I even if caught.

Platform Entry — Entry and availability of a delivery platform in location z generates two effects: (1) economic deterrence by lowering barriers to employment (reducing f), and (2) self-incapacitation effect by occupying peak-crime hours with delivery work (raising h). Both effects increase the threshold τ reducing P_I . As a consequence, in Figure A1, the threshold shifts right: illegal work must offer a higher wage to be preferred to legal work. This visual representation underscores the potential benefits of policies aimed at enhancing legal job opportunities for at-risk populations.

Figure A1: Conceptual Model – Shift in Crime Threshold



Notes: The solid blue curve represents the cumulative distribution of illegal earnings before the entry (or availability) of delivery platforms in a location; the dashed black curve represents the distribution after the entry. The vertical lines indicate the thresholds before (solid blue) and after (dashed black) the entry of platforms.

The conceptual framework motivates our empirical difference-in-differences approach, which compares pre- and post-platform entry in z to a control location z' , where the platform has not entered. We expect not only more riders in z but also a decrease in crime. Let G be the size of group g . The expected number of individuals choosing illegal work is $G_I = pG[1 - F(\tau)]$. Because $\partial\tau/\partial f = -1$, we have $\partial G_I/\partial f > 0$, holding F and p fixed and assuming positive density at the threshold: reducing barriers to employment f (via platform entry) raises τ and lowers G_I . Likewise, $\partial G_I/\partial h < 0$: an increase in the time-related cost of offending h , representing self-incapacitation, reduces the expected number of individuals choosing illegal work.

B French Labor-Market Features

Table A1: Labor-Market Outcomes by Migration Status, 2022 (in %)

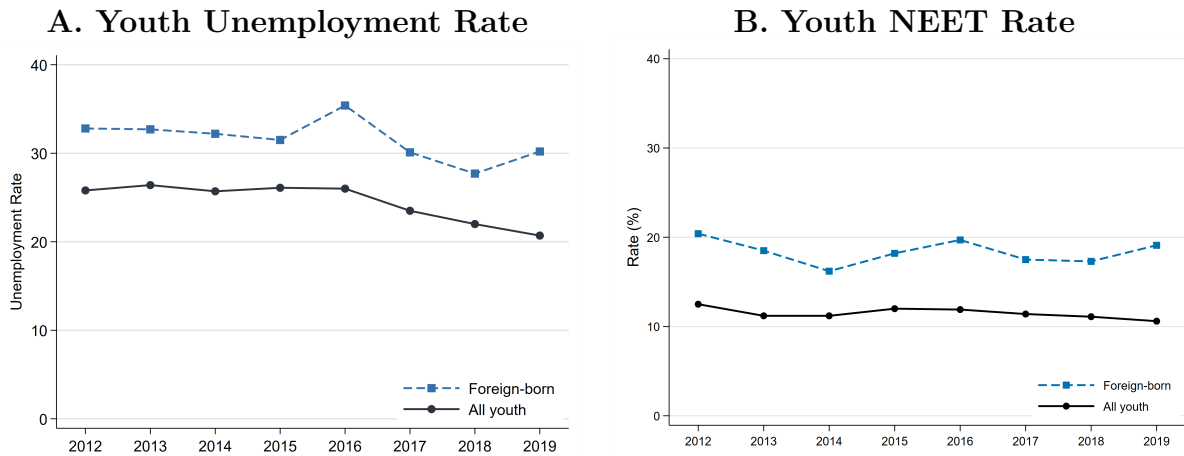
	Inactivity Rate	Employment Rate	Unemployment Rate
First-generation immigrant ^a	30.0	61.8	11.7
Second-generation immigrant ^b	32.2	60.5	10.8
Non-immigrant ^c	25.1	70.2	6.3
Total population	26.5	68.1	7.3

Notes: All rates are computed for individuals aged 15–64 living in France (excluding Mayotte). Inactive rate = inactive (neither employed nor unemployed) ÷ total population. Employment rate = employed ÷ total population. Unemployment rate = unemployed ÷ labor force (employed + unemployed). In 2022, among first-generation immigrants aged 15 to 64, 30.0% were inactive and 61.8% were employed. Among those active in the labor force, 11.7% were unemployed.

^aFirst-generation immigrant: born abroad and residing in France. Individuals continue to be considered immigrants even if they acquire French nationality. ^bSecond-generation immigrant: native-born with at least one foreign-born parent. ^cNon-immigrant: native-born with two native-born parents.

Source: INSEE, [Labor Force Survey](#) (accessed August 2024). Note that the INSEE website provides only the latest available year at any given time.

Figure A2: Youth Labor Market Outcomes in France, 2012-2019



Notes: Panel A reports annual unemployment rates for people aged 15-24. The unemployment rate equals the number of unemployed people divided by the labor force. Panel B reports the share of people aged 15-24 who are neither employed nor in formal or non-formal education or training (NEET). Foreign-born refers to people born outside France. *Source:* Eurostat, datasets `une_rt_a`, `lfsa_urgacob`, `edat_lfse_20`, and `edat_lfse_28`.

Table A2: Inactivity, Employment, and Unemployment Rates of Immigrants by Geographic Origin, 2022 (in %)

	First Generation ^a			Second Generation ^b		
	Inactivity Rate	Emp. Rate	Unemp. Rate	Inactivity Rate	Emp. Rate	Unemp. Rate
Africa	31.3	59.3	13.7	38.0	52.6	15.2
Maghreb*	34.4	56.6	13.7	37.0	53.8	14.5
Other	26.2	63.7	13.7	41.0	48.8	17.3
Asia	31.3	60.8	11.5	37.6	55.3	11.4
Turkey, Middle East	31.8	58.3	14.5	39.4	51.4	15.2
Other	30.9	62.7	9.1	35.8	59.2	7.8
Southeast Asia	24.2	70.0	7.7	27.2	66.6	8.5
Americas, Oceania	31.8	59.4	12.8	45.3	47.2	13.7
Europe	26.3	67.7	8.1	22.5	72.5	6.3
Southern Europe [†]	25.2	70.7	5.6	19.2	76.2	5.7
Other EU-27	23.4	69.5	9.3	32.0	62.1	8.7
Other	30.9	61.5	10.9	32.7	61.9	8.0
Non-immigrant ^c	25.1	70.2	6.3	25.1	70.2	6.3

Notes: All rates are computed for individuals aged 15–64 living in France (excluding Mayotte). Inactive rate = inactive (neither employed nor unemployed) ÷ total population. Emp. (Employment) rate = employed ÷ total population. (Unemp.) Unemployment rate = unemployed ÷ labor force (employed + unemployed). In 2022, among first-generation immigrants from Africa aged 15 to 64, 31.3% were inactive and 59.3% were employed. Among those active in the labor force, 13.7% were unemployed.

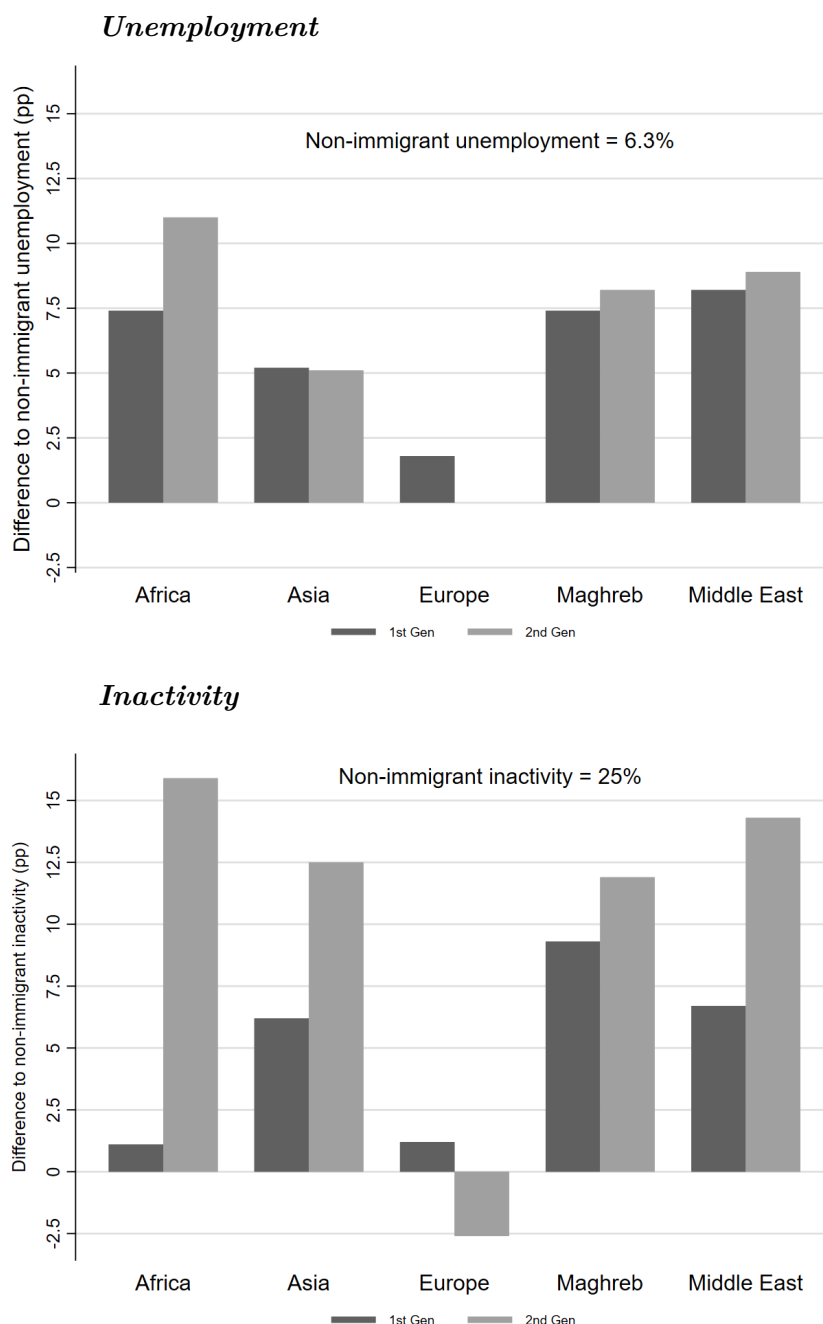
The country of origin is the country of birth of the immigrant parent if there is only one. When both parents are immigrants, the father’s origin is used by convention.

^aFirst-generation immigrants: born abroad and residing in France. Individuals continue to be considered immigrants even if they acquire French nationality. ^bSecond-generation immigrants: native-born with at least one foreign-born parent. ^cNon-immigrant: native-born with two native-born parents.

*Maghreb: Algeria, Morocco, Tunisia. [†]Southern Europe: Portugal, Spain, Italy. “Other” represents other countries within each region.

Source: INSEE, [Labor Force Survey](#) (accessed August 2024). Note that the INSEE website provides only the latest available year at any given time.

Figure A3: Unemployment and Inactivity by Origin, 2022



Notes: All rates are computed for individuals aged 15–64 living in France (excluding Mayotte), and differences are measured relative to non-immigrants (native-born with two native-born parents). See Appendix Tables A1 and A2 for raw numbers. Inactive rate = inactive (neither employed nor unemployed) ÷ total population. Unemployment rate = unemployed ÷ labor force (employed + unemployed). The country of origin is the country of birth of the immigrant parent if there is only one. When both parents are immigrants, the father’s origin is used by convention. “1st Generation”: born abroad and residing in France. “2nd Generation”: native-born with at least one foreign-born parent. Origin groups: Africa (excluding Maghreb); Asia; Europe (excluding France); Maghreb (Algeria, Morocco, Tunisia); Middle East (including Turkey).

Source: INSEE, [Labor Force Survey](#) (accessed August 2024). Note that the INSEE website provides only the latest available year at any given time.

C Survey Evidence on Food Riders in Paris, 2016-2019

This appendix presents the data from three successive rider surveys conducted in Eastern Paris from 2016 to 2019 (see [Dablanc et al., 2019, 2020](#)). The first surveyed 96 riders (Oct–Dec 2016); the second, 107 riders (Jan–Mar 2018); the third, 300 riders (Dec 2019–Jan 2020).

Table A3: Descriptive Evidence of Three Rider Surveys in Eastern Paris

	2016 (1)	2018 (2)	2019 (3)
<i>Demographics (%)</i>			
Women	2	5	2
Age < 35	92	94	88
No diploma or middle school education	30	33	66 ^(a)
Bicycle as main mode of delivery	78	73	62
<i>Weekly hours distribution (%)</i>			
< 10 hours	53	56	30
10–20 hours	36	40	60
> 20 hours	6	11	10
<i>Main reasons for being a rider (%)</i>			
Only job I could find	—	17	^(b)
Wanted high autonomy	—	40	—
<i>Self-declared nationality (%)</i>			
French	—	—	14
Foreigner	—	—	86
Observations	96	107	300

Notes: Percentages as reported in the original surveys. In 2019: ^(a) among the 215 full-time riders, 50% report no diploma; ^(b) 43% of riders answered “yes” to the question, “I can’t find another job.”

Source: Three rider surveys conducted in Eastern Paris ([Dablanc et al., 2019, 2020](#)). (1): Oct–Dec 2016; (2): Jan–Mar 2018; (3): Dec 2019–Jan 2020.

The survey evidence in Appendix Table A3 points to a clear rider profile: overwhelmingly male (2–5% women), very young (88–94% under 35), and largely first-generation migrants (86% foreign nationals in 2019). Formal education is low, about one-third report no diploma or only middle-school education in 2016–2018, rising to two-thirds in 2019. Bicycles remain the predominant mode of transport (62–78%). While most riders work fewer than 20 hours per week, they cite both autonomy (40% in 2018) and lack of alternatives (17% in 2018; 43% in 2019 said they “can’t find another job”).

D Rider’s Origins

D.1 Residential Locations

Table A4 uses the geolocation information from the *Sirene* dataset (see Section H.1.1) to compare where riders live to where the general population lives. The comparison covers the 5,280 French cities with at least 2,000 inhabitants and uses the 2019 stock of riders. We assign each rider to the city recorded as their residence at registration.

Table A4: Rider Representation Across Disadvantaged Areas

	% of Population	% of Riders
Priority Neighborhoods ^a	9.80	15.29
Cities by ^b		
Low median income (bottom 5%)	9.78	13.66
Low-education (bottom 5%) ^b	4.46	6.94
Immigrant rate (top 5%)	18.22	40.93
Male unemployment rate (top 5%) ^c	9.98	11.45
Inactivity rate (top 5%) ^c	8.23	9.63
Welfare claimant rate (top 5%) ^c	9.50	10.27

Notes: ^aPriority Neighborhoods (*Quartiers prioritaires de la politique de la ville*) are areas characterized by a high concentration of low-income households. Some riders could not be precisely geolocated within cities and are not included in the QPV estimates. The analysis includes only cities with more than 2,000 inhabitants (5,280 in total) and uses rider counts from 2019. ^bLow education refers to the share of individuals without a diploma or, for those who left school before 1989, without a Primary School Certificate (*Certificat d’Études Primaires*). ^cUnemployment, inactivity, welfare claimant rates are based on 2014 population data to avoid conditioning on outcomes that platform entry may affect.

Each row of Table A4 compares the shares of riders and the population living in areas with different local socioeconomic conditions. The first row examines Priority Neighborhoods.³⁹ The population share is computed for this row with the 2018 population of these neighborhoods. We exclude riders whose addresses cannot be precisely located within their city. Each remaining row defines a group of cities based on one local characteristic. The income row, for example, covers the 5% of cities with the lowest median income. The education row covers the 5% of cities at the bottom of the education distribution. The immigration, male-unemployment, inactivity, and welfare-claimant rows each cover the 5% of cities with the highest value of the relevant measure. Columns (1) and (2) report the shares of the general population and riders who live in the selected cities. The city characteristics come from INSEE municipal statistics. We measure unemployment, inactivity, and welfare-claimant rates in 2014, before platform entry, to avoid conditioning on outcomes that entry may affect. The immigrant share is the share of immigrants in the population aged 15-64.

Riders are overrepresented across all measures. The largest gap concerns migrant concentration: 40.9% of riders live in the top 5% of cities by immigrant population share, compared with 18.2% of the overall population. Riders are also more likely than the

³⁹Priority Neighborhoods (or *Quartiers prioritaires de la politique de la ville* in French) were established by the 2014 planning law on urban affairs and cohesion to reduce spatial inequalities. Their boundaries are defined by the National Agency for Territorial Cohesion based on concentrations of low-income households. As of January 2015, there were 1,514 Priority Neighborhoods across 859 cities.

average resident to live in a Priority Neighborhood (15.3% versus 9.8%), in the least-educated cities (6.9% versus 4.5%), and in the poorest cities (13.7% versus 9.8%).

D.2 Labor-Market Status Before Registration

Table A5 reports riders' labor-market status in the month before registration, using the *All Active Workers Panel* (see Appendix H.1.3). The panel follows only individuals born on selected dates and represents about one-eighth of the French workforce. The panel therefore covers a subsample of riders: 16,805 individuals who registered as self-employed riders between 2015 and 2019, compared with 147,923 riders in the full *Self-employed Database*. Because selection into the panel depends only on date of birth, this subsample is not selected on labor-market history. We classify each rider by the record observed in the month before registration: a salaried job (full-time or part-time), unemployment benefits, other self-employment, or no record. For riders with no record that month, we distinguish those with an earlier salaried, self-employed, or unemployment-benefit spell from those without any record. We do not break these patterns down by country of birth, because for foreign-born riders the absence of earlier records may reflect recent arrival in France rather than non-employment, and the panel does not record the date of arrival.

Table A5: Labor-Market Status Before Delivery Registration

In the month before	Number	Share (%)
A. Not working	10,288	61.2
No job or benefit record ever before ^a	4,026	24.0
Unemployed (receiving benefits)	1,588	9.4
Worked or on benefits earlier, not in that month	4,674	27.8
B. Working	6,517	38.8
Full-time employee	4,222	25.1
Part-time employee	2,232	13.3
Other self-employment ^c	63	0.4
Total	16,805	100.0

Notes: Labor-market status in the month before registration as a self-employed rider (activity code 53.20Z), for the 16,805 riders who registered between 2015 and 2019 and appear in the All Active Workers Panel (*Panel Tous Actifs*). The panel covers only a one-eighth sample of salaried and self-employed workers in France, selected by date of birth. For riders in Panel A, delivery work marks entry into employment; only for riders in Panel B can it replace an existing job. ^aNo salaried, self-employed, or unemployment-benefit spell observed in the panel at any date before registration. ^bSelf-employed, home workers, and a small number of individuals with missing employment status.

D.3 Name-Based Origin Classification

Appendix B documents that first- and second-generation immigrants experience higher rates of inactivity and unemployment than natives, with larger disparities among non-European groups, such as African migrants. INSEE produces these statistics from official sources, such as the census and dedicated surveys. By contrast, French data protection rules consider revealing racial or ethnic origin as sensitive information and restrict their

collection and use.⁴⁰ Given these restrictions, we use an alternative approach to infer riders’ likely origins and their potential exposure to labor-market discrimination before entering delivery work.

To comply with French data-protection rules, we construct a non-identifying list of first names. The *Sirene* dataset lists 153,322 unique riders registered under NAF code 53.20Z between 2012 and 2019. From this list, we exclude 37,612 riders without a recorded first name. To preserve anonymity, we then drop 12,929 first names that occur only once and could therefore allow identification.⁴¹

The resulting dataset contains only nonunique first names and no other information that could identify a rider. We then use [Claude](#), a large language model (LLM) accessed through its paid web interface, to classify the list of first names into four broad name-origin categories: Arabic, Asian, European, and sub-Saharan African.⁴² Two points clarify how we use these classifications. First, we do not merge these name-origin classifications back with the original *Sirene* database. Second, these categories reflect geographic associations in naming conventions rather than ancestry. This methodology has inherent limitations. It oversimplifies diversity, misses regional specificity, and fails to capture the complex geographic landscape within and across groups. We therefore use them only as a proxy for the share of riders likely to face name-based discrimination in the labor market.

E Uber Eats and Deliveroo Rollout in France

Uber Eats and Deliveroo are the two largest delivery platforms in France, and thus the most likely to affect local labor markets and crime. Uber began piloting meal delivery in Los Angeles in August 2014 and introduced the service in Paris through the Uber app in October 2015.⁴³ Deliveroo launched in 2013 in the United Kingdom and entered France in 2015.⁴⁴

We assembled entry dates for Uber Eats and Deliveroo from multiple sources. First, we collected reports of platform launches from national and regional media. Second, for Uber Eats, we retrieved city-specific entry dates from archived versions of the Uber Eats website using the Wayback Machine.⁴⁵ Third, we cross-checked and supplemented these data with information from Collectif Data + Local.⁴⁶ Finally, Uber provided data on its “reference areas,” allowing us to verify their geographic coverage and entry dates since 2015. We also check whether city-level rider registrations rise around each city’s first recorded platform-entry date (see Figure 1).

⁴⁰See the [CNIL guidance on sensitive data in scientific research](#), last accessed July 2026.

⁴¹For a unique compound first name, we retain only its first component if that component is not unique.

⁴²We performed the classification on September 27, 2025; the classified list is available on request.

⁴³See Uber’s accounts of the [Los Angeles pilot](#) and the [Paris launch](#), last accessed July 2026. Uber’s later [French timeline](#) dates the launch of Uber Eats in France to March 2016. We define entry by the first availability of meal delivery, which occurred in Paris in October 2015.

⁴⁴In July 2015, Deliveroo reported partnerships with 250 restaurants in France (see [Les Echos](#), last accessed January 2026).

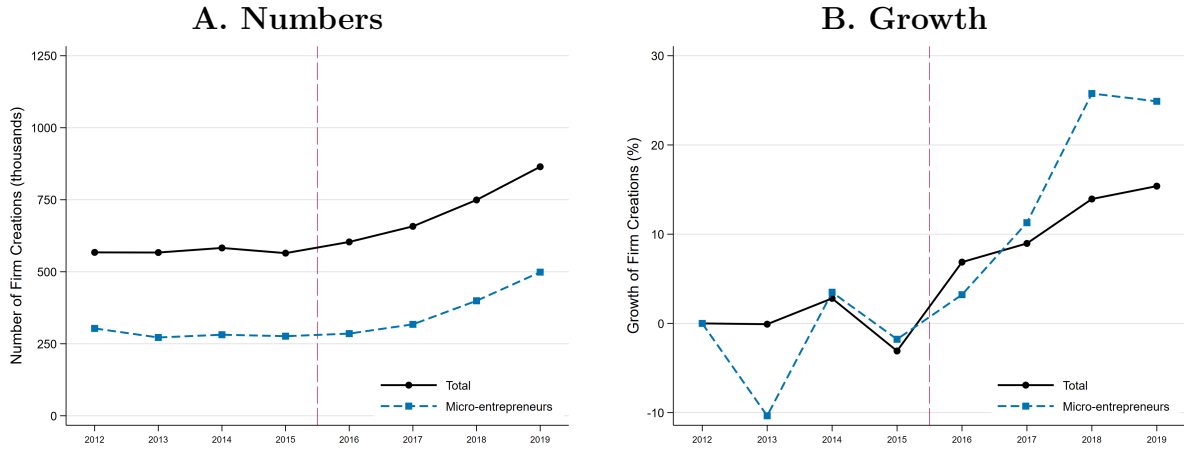
⁴⁵Because Deliveroo does not provide a complete archive of city-entry dates, we rely on national and regional press and Twitter archives.

⁴⁶Collectif Data + Local is a consortium of journalists from major French local media; see [Collectif Data + Local](#).

F Self-employment Registration in France

In France, riders are required to register as self-employed. This micro-entrepreneur status, introduced in 2009, was designed to simplify the creation and management of small individual businesses, particularly those with low turnover.⁴⁷ Appendix Figure A4 shows that micro-entrepreneurs account for a large share of firm creation in France. Between 2012 and 2019, annual registrations under this regime rose from 303,300 to 498,700 and accounted for about two-thirds of the increase in total firm creation. For riders, the regime lowers administrative entry costs because registration requires few formalities. We identify riders using the French NAF classification’s APE code 53.20Z (“Other postal and courier activities”).⁴⁸

Figure A4: Business Creations in France, 2012-2019



Notes: Annual legal-unit creations, in thousands, for all market activities except agriculture. Micro-entrepreneur creations refer to registrations under the micro-entrepreneur regime. The vertical dashed line marks the entry date of the first delivery platforms in France. *Source:* INSEE, SIDE, , dataset IP2092.

During our study period, riders could register as micro-entrepreneurs online or at a *Centre de formalités des entreprises* (CFE). Registration involves the following information and requirements:

- Identity: full name, date and place of birth, nationality, and address.
- Nature of activity: “Bike delivery” or “transportation of goods/people using a two-wheeled vehicle.” The activity is a commercial service activity that generates industrial and commercial profits (*bénéfices industriels et commerciaux*, BIC) within the micro-entrepreneur regime.
- Start date: desired start date of the activity.
- Registration identifiers: INSEE assigns a nine-digit SIREN number to the business and a 14-digit SIRET number to its establishment after registration.
- Bank account: before 2019, riders could use a personal bank account for professional transactions; since 2019, a separate bank account dedicated to the activity is required if annual turnover exceeds €10,000 for two consecutive calendar years.⁴⁹

⁴⁷<https://www.insee.fr/en/metadonnees/definition/c1500>, last accessed July 2026.

⁴⁸<https://www.insee.fr/en/metadonnees/nafr2/sousClasse/53.20z>, last accessed July 2026.

⁴⁹See www.urssaf.fr, last accessed July 2026.

- Honor statement: a simple *déclaration sur l'honneur de non-condamnation* attesting the absence of disqualifying criminal or commercial convictions.

Registration does not necessarily imply current activity. Micro-entrepreneurs must report turnover monthly or quarterly to URSSAF (the French social welfare collection agency), even when it is zero.⁵⁰ Two consecutive calendar years without turnover or declaration can lead the social-security authority to presume that the business is inactive. Before 2018, eligibility for the micro-entrepreneur regime was subject to an annual turnover ceiling of approximately €33,000. In 2018, the annual turnover ceiling rose to €70,000.⁵¹ Beyond these thresholds, a rider exits the simplified micro-entrepreneur scheme to a more complex tax and social-security regime.

Within the simplified micro-entrepreneur scheme, riders may choose between the *versement libératoire de l'impôt sur le revenu*, paying income tax as a fixed percentage of turnover (1.7%), and the default micro-fiscal regime, which applies a 50% standard expense allowance and taxes the remaining 50% at the household's progressive income-tax rate. The same social contributions apply in both cases. Until 2017, the *Régime social des indépendants* administered riders' basic social protection, including health and retirement coverage. A two-year transition that began in 2018 integrated self-employed workers into the general social-security system by 2020.⁵²

Throughout our study period (2012–2019), riders self-reported turnover to URSSAF. Starting with transactions in 2019, platforms also reported users' transaction amounts to the tax authorities.⁵³ Self-reporting helps explain why turnover figures for riders in INSEE's *Base non-salariés* are incomplete or underestimated. A 2021 government report of the Observatory on Undeclared Work (*Observatoire du travail dissimulé*) compares turnover declared to URSSAF with platform-reported transactions.⁵⁴ It estimates that, in 2019, 38.4% of delivery micro-entrepreneurs who used platforms reported no turnover to URSSAF despite having platform transactions, and 47.4% reported positive turnover, but less than 95% of the amount generated on the platforms. For these 85.8% of delivery micro-entrepreneurs, the gap between the turnover recorded by platforms and that declared to URSSAF amounts to €189.7 million, corresponding to an estimated €24.3 million in unpaid contributions. The report cautions that these estimates rely on the available platform data and remain preliminary. Delivery nonetheless stands out even within the platform economy: the estimated effective contribution-evasion rate for delivery riders reaches 68%, about twice the rate estimated for micro-entrepreneurs outside platform-based transport (33%) ([Haut Conseil du financement de la protection sociale \(HCFiPS\), 2021](#)). The same platform transaction data also imply a larger population of economically active riders than declaration-based statistics record, because official counts retain only micro-entrepreneurs who declared positive revenue at least once over two years. Declaration-based measures of rider activity, including our stock of active riders, are therefore conservative.

G Food-Delivery and Ride-Hailing in France

⁵⁰See [French Ministry of the Economy](#), last accessed July 2026.

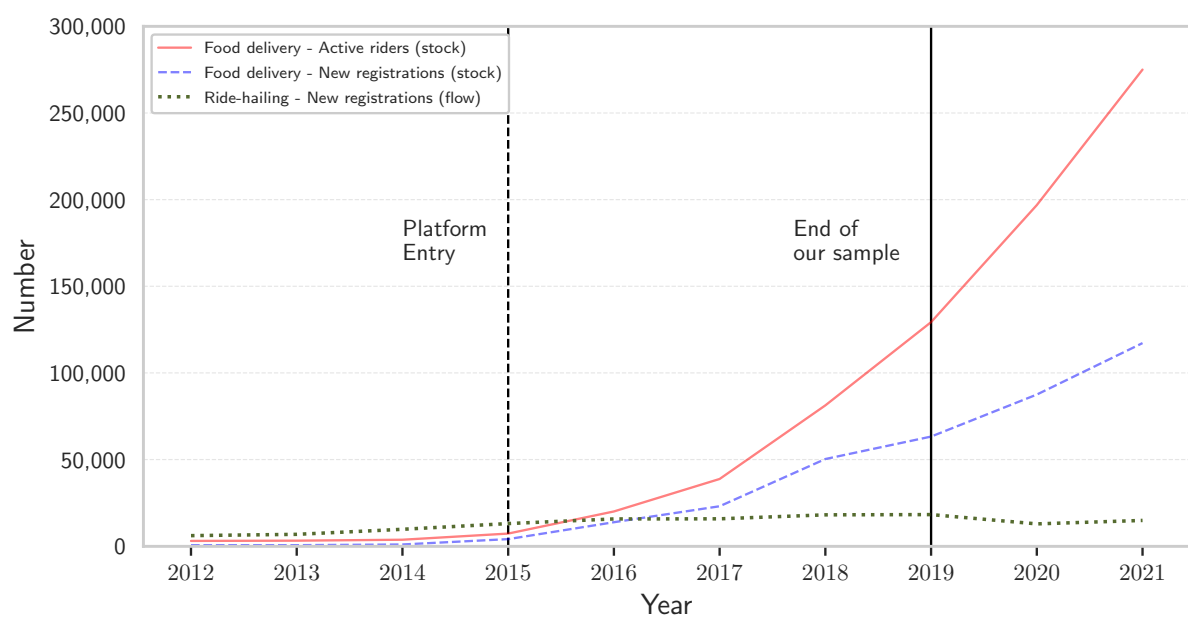
⁵¹Article 50-0 of the French Tax Code, version in force on January 1, 2018; see [Legifrance](#).

⁵²See the reform of social protection for self-employed workers at [securite-sociale-independants](#), last accessed July 2026.

⁵³Article 242 bis of the French Tax Code; see [Legifrance](#).

⁵⁴See [Observatoire du travail dissimulé](#), last accessed July 2026.

Figure A5: Food-Delivery and Ride-Hailing Registrations in France, 2012–2021



New (flow) and Registered (stock) Food-Delivery Workers and Ride-Hail Drivers

Notes: This figure extends Panel B of Figure 1 by adding ride-hailing registrations and by continuing the series beyond 2019. It reports annual new registrations and the stock of registered active food-delivery workers, together with new ride-hailing registrations. The solid vertical line at 2019 marks the end of the estimation sample and the onset of the COVID-related expansion. Ride-hailing registrations grow very little prior to 2019, whereas food-delivery registrations increase sharply following platform entry, indicating that delivery, not ride-hailing, drives the expansion of gig work during our sample period. *Sources:* Authors' calculations using the INSEE *Sirene* business registry.

H Data: Definitions and Sources

H.1 Delivery Riders

To identify delivery riders and obtain their demographic characteristics, we combine information from three complementary French administrative databases: the Sirene business registry, the Self-employed Database (*Bases Non-Salariés*), and the All Active Workers Panel (*Panel Tout Actif*). These datasets provide different but overlapping perspectives on the population of micro-entrepreneurs operating in the delivery sector, allowing us to construct a comprehensive picture of rider demographics and employment.

H.1.1 Sirene

The [Sirene](#) administrative database, which is France’s business registry, is openly accessible online. It is organized at the legal unit level, encompassing both legal persons (distinct from their owners) and natural persons conducting economic activity. We identify delivery riders as establishments classified under NACE code 53.20Z (“Other postal and courier activities”) with no employees (self-employed). The database provides (i) establishment-level information, including registration year, legal status, activity codes, and geographic location (city), as well as (ii) information about natural persons, such as first names and registered address. However, names and precise geolocation (finer than the city level) of a small number of riders are not released (see the main text for precise numbers).

H.1.2 Self-employed Database (SED)

The [Self-employed Database](#) (SED) complements Sirene by providing annual demographics and employment information for self-employed workers. It reports administrative data from social-security declarations. While Sirene is openly accessible, SED requires authorization and must be accessed within the CASD ([Centre d’accès sécurisé aux données](#)) secure environment.

SED uses the same firm identifier as Sirene, enabling us to match the two datasets. Between 2012 and 2019, we identify 147,923 unique riders aged 16-54, 97% of whom are also listed in Sirene. SED adds information on riders’ turnover and demographics (French-born, foreign-born, age). However, as noted in Section F, turnover figures are severely misreported during our study period.

H.1.3 All Active Workers Panel (AAWP)

The [All Active Workers Panel](#) (AAWP) allows us to document riders’ job trajectories prior to delivery work. This longitudinal dataset follows one-eighth of the active population (employees and self-employed) through spells of employment and self-employment.⁵⁵ Like the Self-employed Database, AAWP requires authorization and must be accessed within the CASD.

We identify individuals who created a micro-entrepreneur under activity code 53.20Z with no employees after 2015. Using their unique identifiers, we reconstruct their complete employment history before and after their delivery work. Because the AAWP covers only

⁵⁵The panel includes individuals born in selected reference months, representing roughly one-eighth of the French working population. This is therefore a panel based on sampling by birth dates, rather than interpolation of missing values.

one-eighth of active workers, it offers a representative but incomplete picture of riders. Even so, these worker trajectories enable us to analyze patterns of entry into delivery work and the professional backgrounds of platform workers.

H.2 Police and Court Data

We use two complementary administrative sources of crime data. First, police records provide annual counts of offenses recorded by police units.⁵⁶ Second, the *Cassiopée* court records complement police records with individual-level information on people prosecuted in criminal cases. However, *Cassiopée* excludes dismissed cases and cases tried in assize courts or on appeal.

Given these complementary strengths and limits, we rely primarily on police data for overall crime estimations and use court data to analyze heterogeneity across specific population groups.

H.2.1 Police Data

We use annual police-recorded data to construct local crime rates since 2012.⁵⁷ The records cover offenses regardless of subsequent judicial proceedings. They include offenses reported by victims or third parties as well as offenses identified through patrols, stops, or investigations.

Police data are recorded at the police jurisdiction level, the most granular geographic unit available. Jurisdictions align with administrative boundaries and may cover parts of a municipality or groups of municipalities across metropolitan France. On average, one jurisdiction comprises about 50 municipalities and 93,844 inhabitants.⁵⁸

We aggregate offenses by police jurisdiction into six categories: all crimes, violence against the person, destruction of property, low-skill theft, high-skill theft, and drug offenses (see Table A6). We distinguish low- from high-skill theft by required expertise: low-skill offenses are opportunistic and non-specialized (e.g., shoplifting, bicycle theft), whereas high-skill theft requires greater planning, coordination, or technical know-how (e.g., burglary).

H.2.2 Court Data

The court data come from the judicial information system *Cassiopée*, which records decisions issued by magistrates in two courts of first instance: Juvenile Court and Criminal Court (see Figure A7).⁵⁹ The records include information on cases and offenders, including the offender’s age at the time of the crime, nationality, and gender. Because of their

⁵⁶For simplicity, we use “police” to denote two different units: gendarmerie and police (or “commissariats”). They perform similar missions (law enforcement, public safety, and crime prevention), although they operate in distinct jurisdictions and fall under different ministries (the Ministry of the Interior of police stations, and the Ministry of Defense for gendarmeries operate).

⁵⁷The dataset is publicly available on the [INSEE website](#).

⁵⁸Between 2012 and 2019, some jurisdictions were merged following municipal reforms and police reorganizations. To ensure consistent geographic units over time, we harmonize police-jurisdiction boundaries in every year to the most aggregated boundaries observed during the period.

⁵⁹Courts of first instance are the trial courts where cases are initially filed and adjudicated, before any appeal to higher courts (see Figure A7). Note that the Criminal Court is split between an adult division, trying adult defendants for misdemeanors, and a juvenile division, trying minors under specialized procedures and sanctions.

Table A6: Crime Categories in Police Recorded Data (État 4001 codes)

Crime Category	Code
ALL CRIMES	
From “Settling of scores between criminals” to “Other offenses” (excluding codes 42-43)	1-107
VIOLENCE AGAINST THE PERSON	
Settling of scores between criminals; Homicides and attempted homicides for thefts or other reasons; Voluntary assault and battery resulting in death; Other voluntary criminal or correctional assault and battery; Hostage-taking during thefts or for other purposes; Kidnappings; Threats or blackmail for extortion or other purposes; Armed robberies with firearms or bladed weapons against financial, industrial or commercial establishments, cash-in-transit companies, or individuals at home; Other armed robberies with firearms; Violent thefts without weapons on public roads or in other public places; Rapes or attempted rapes of adults or minors; Sexual harassment and other sexual assaults against adults or minors; Homicides of children under 15; Violence, abuse and abandonment of children; Violence against public officials	1-12, 15-26, 46-49, 51-52, 73
DESTRUCTION OF PROPERTY	
Arson of public or private property; Other destruction and damage to public or private property; Destruction and damage to private vehicles	62, 63, 66-68
ALL THEFTS	
Violent thefts without weapons on public roads or other public places; Burglaries of primary or secondary residences, industrial, commercial or financial premises, and other places; Theft from vehicles; Shoplifting; Theft of cars or other vehicles; Armed robberies; Receiving stolen goods; Simple thefts against public or private establishments or individuals in public or private places.	25-41
LOW-SKILL THEFTS	
Violent thefts without weapons on public roads or other public places Shoplifting	25, 26 33
HIGH-SKILL THEFTS	
Burglaries of primary or secondary residences; Burglaries of industrial, commercial or financial premises, or of other places Theft of cars or other vehicles	27, 28, 29, 30 35-36
DRUG CRIMES	
Drug use and resale Drug trafficking and resale without use; Drug use; Other drug law violations	56 55, 57, 58

Notes: État 4001 refers to the French national crime statistics classification system. Some crime codes may appear in multiple categories.

sensitive nature, these data are accessible only within the CASD secure environment. The availability of offender characteristics allows us to analyze heterogeneity in criminal behavior across demographic and age groups.

Table A7: French Criminal Justice System

First-Instance Courts		
Police Court <i>(Tribunal de Police)</i>	Criminal Court <i>(Tribunal Correctionnel)</i>	Assize Court <i>(Cour d'Assises)</i>
CONTRAVENTIONS Classes 1–5	MISDEMEANORS (<i>Délits</i>)	FELONIES (<i>Crimes</i>)
No prison sentence Fines: up to €3,000	Prison: less than 10 years Fines: over €3,750	Prison: 10+ years to life Tried by jury and judges
Examples: • Traffic violations • Minor public disorder • Petty vandalism • Parking violations	Examples: • Theft without violence • Fraud • Simple assault • Drunk driving • Drug possession	Examples: • Murder/manslaughter • Rape • Armed robbery • Terrorism • Major drug trafficking
Appeal Courts		
Court of Appeal <i>(Cour d'Appel)</i>	Assize Appeal Court <i>(Cour d'Assises d'Appel)</i>	
Reviews facts and law for the Police Court and the Criminal Court	Reviews facts and law for the Assize Court, with a new jury	
Supreme Court (<i>Cour de Cassation</i>)		
Reviews legal procedure only, not facts; final court of appeal		

Notes: Schematic of the French criminal justice system. Cases move from first-instance courts to appeal courts, and finally to the Supreme Court (*Cour de Cassation*), which reviews legal procedure only. Our court data cover two of the first-instance criminal jurisdictions: the Police Court and the Criminal Court (adult and juvenile divisions).

Some limitations are worth mentioning. First, *Cassiopée* does not cover the most serious criminal cases tried in the assize court (court of first instance), the court of appeals, or the supreme court (see Figure A7).

Second, it records only completed cases and omits dismissals (*non-lieu*). Because most offenses committed between 2012 and 2019 were likely resolved by 2022 (our last update), this should not bias our sample. Nevertheless, excluding dismissals creates a discrepancy with police data, which include all reported incidents regardless of prosecution status.

Third, *Cassiopée* classifies cases with NATAFF codes, where as police uses État 4001 codes. NATAFF codes are assigned at registration from the initial file and are not revised. They describe the *case* and not each offender. Despite this taxonomy difference, we are able to map court records to the police records. Using this mapping, we aggregate case counts (originally dated and recorded at the municipality level) to the same spatial and temporal units as the police data to ensure comparability in estimation.

I Average Statistics of Police Jurisdictions

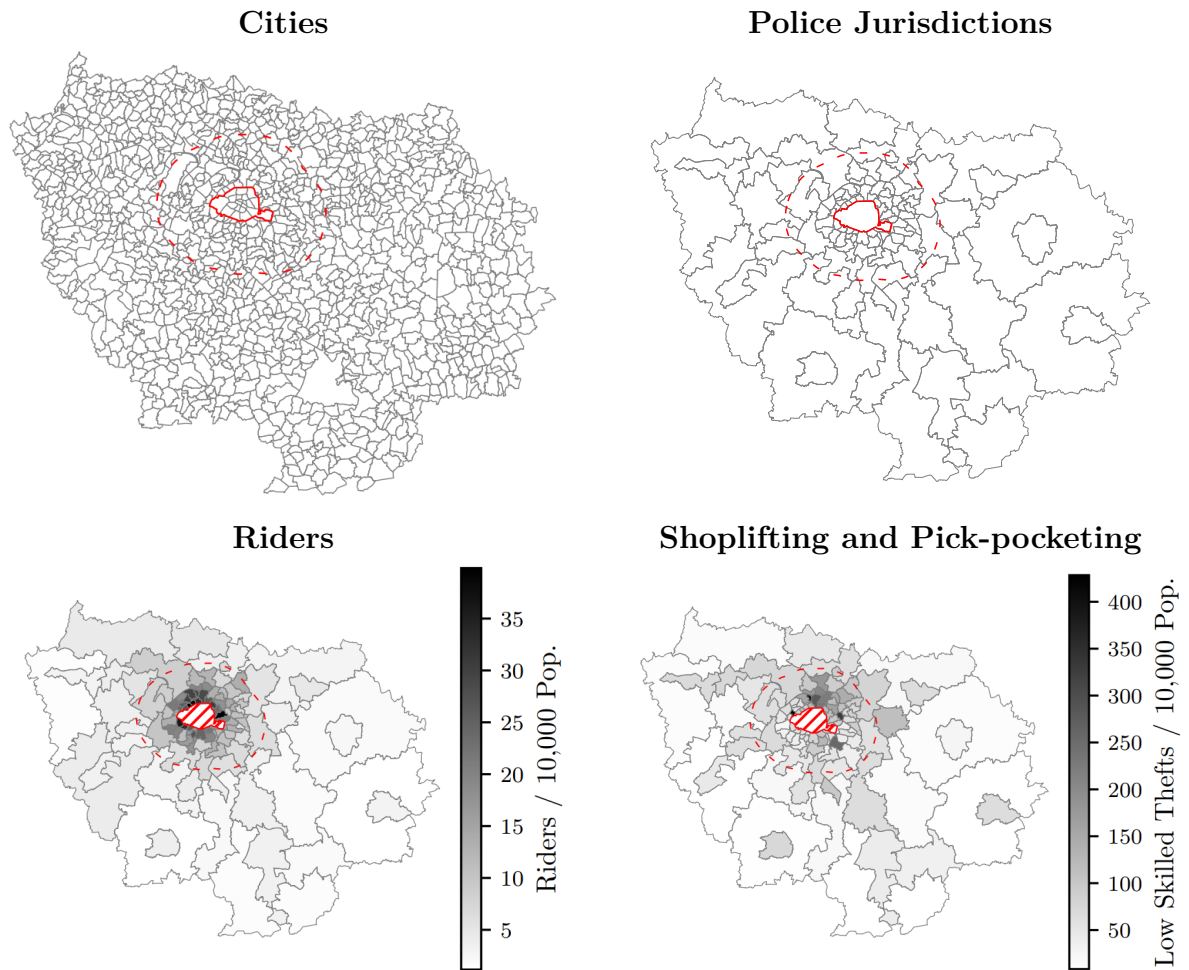
Table A8: Average Statistics of Police Jurisdictions by Treatment Status and Year

	Never Treated		Treated ^a	
	2014	2019	2014	2019
Population (aged 15-54) ^b	15,436	14,753	50,094	49,820
	Population Statistics (% population, 15-54)			
Men	50.37	50.52	49.87	49.90
Young (aged 15-24)	23.09	23.44	22.54	22.70
Migrants	8.61	9.68	9.94	10.76
Male migrants	4.23	4.84	4.79	5.22
	Labor-Market Statistics (% population, 15-54)			
Delivery workers	0.01	0.03	0.01	0.17
Unemployment	12.51	11.89	11.39	10.87
Inactivity	5.76	6.49	4.68	5.20
Male unemployment	6.35	6.03	5.61	5.30
Male inactivity	3.04	3.51	2.40	2.72
Migrant unemployment	1.58	1.70	1.78	1.82
Migrant inactivity	0.72	0.99	0.77	0.92
Male migrant unemployment	0.76	0.80	0.81	0.80
Male migrant inactivity	0.36	0.55	0.39	0.48
	Crime Statistics (% population, 15-54)			
All crimes	10.48	11.89	9.96	10.63
Physical violence	1.48	2.26	1.44	1.88
Vandalism and destruction	1.17	1.17	0.89	0.84
Shoplifting and pick-pocketing	0.43	0.38	0.43	0.36
Burglary and vehicle theft	1.40	1.32	1.59	1.46
Drug use and resale	0.74	1.06	0.62	0.67

Notes: ^aTreated: A jurisdiction is treated between 2015 and 2019 if a food-delivery platform operates within the police jurisdiction, including a 15-km buffer around its boundary. ^bPopulation refers to the average number of residents aged 15-54. Other statistics are average rates per 100 inhabitants aged 15-54. See Section 5.3 and Appendix H for sources and definitions of population, labor-market and crime statistics.

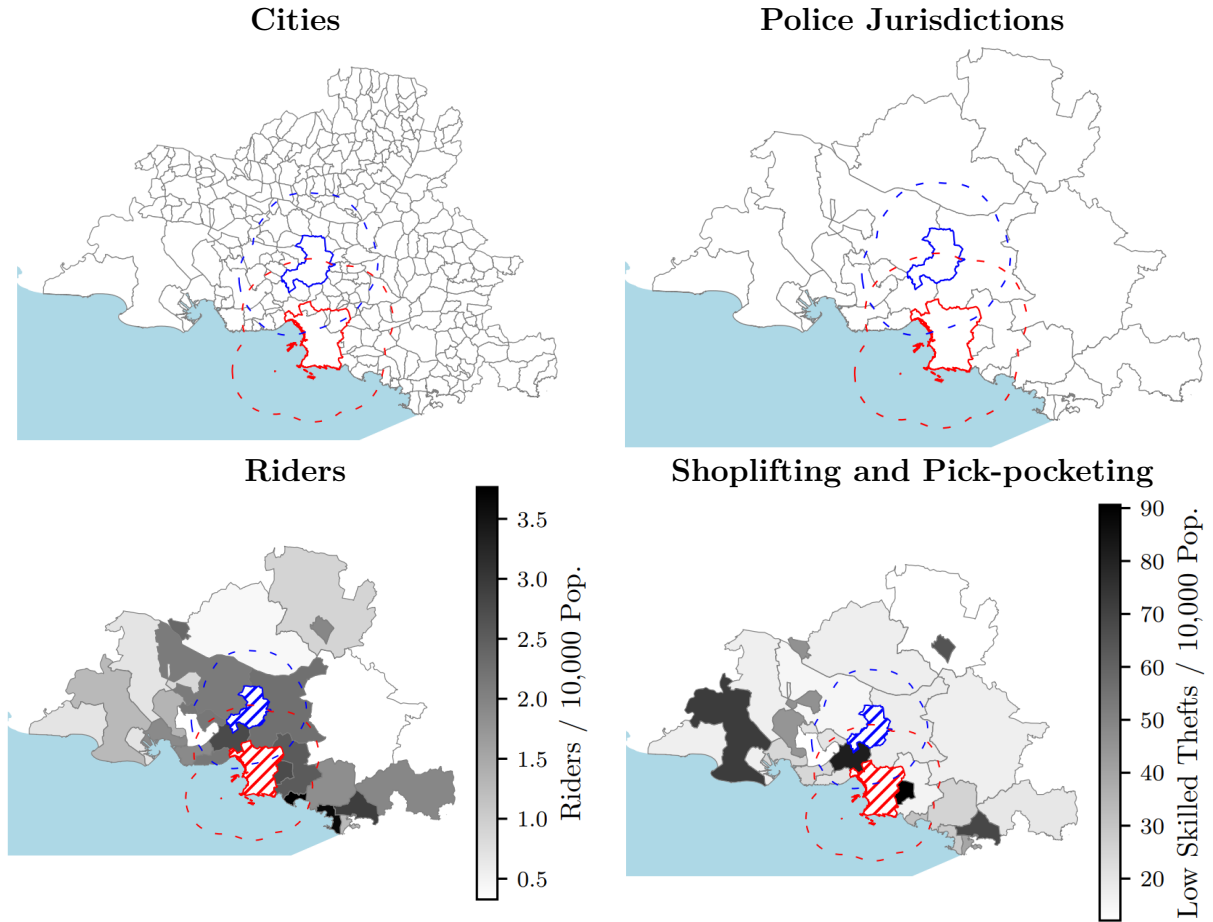
J Geographical Units and Outcomes

Figure A6: Paris and the Surrounding Area (2016)



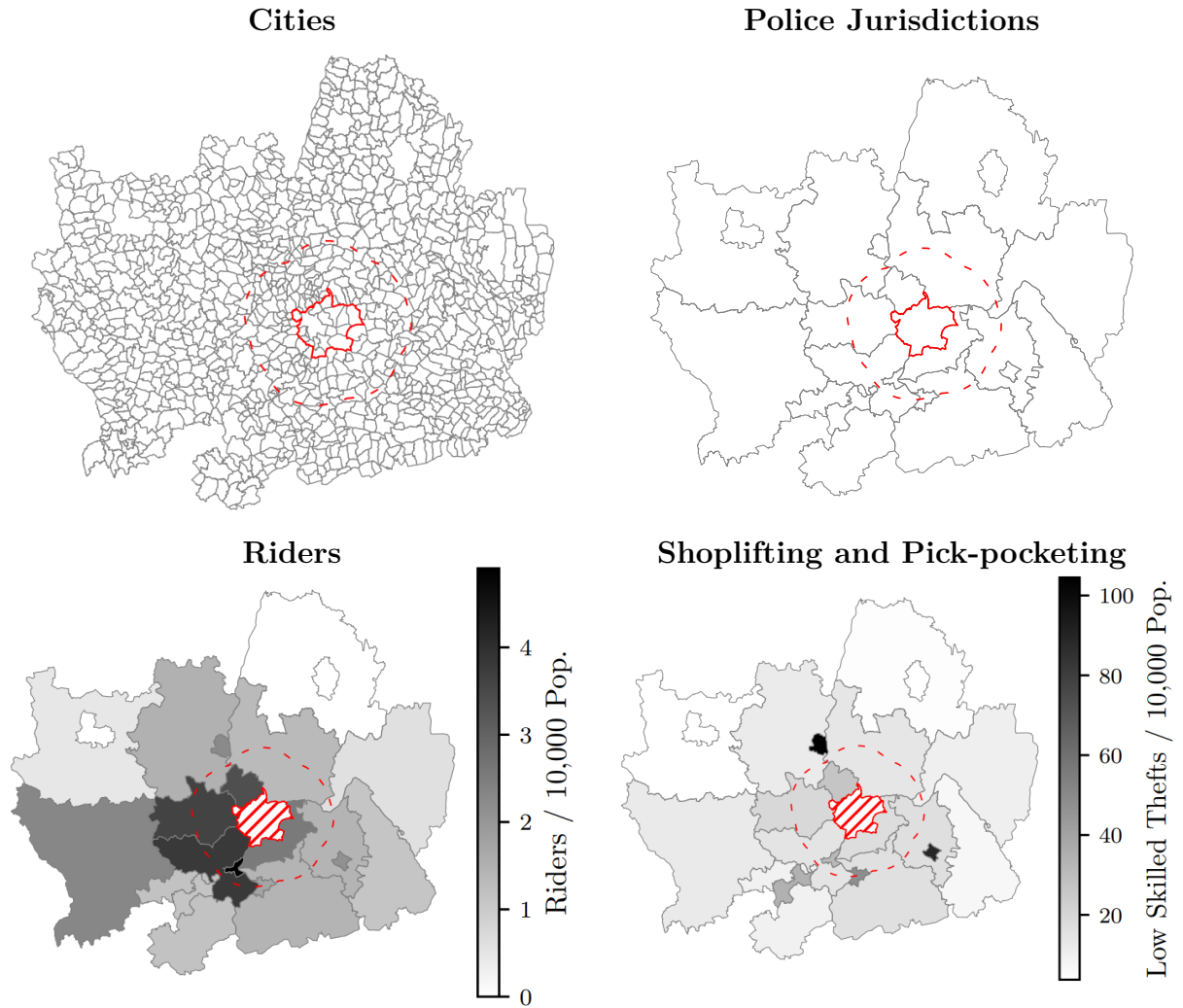
Notes: The red hatched area denotes the police jurisdiction of Paris, while the dashed red line represents a 15-kilometer buffer extending from that boundary. Grey lines denote city boundaries in the left graph of the first panel, while in the other maps they indicate police jurisdiction borders. The population aged 15–54 is used to compute the statistics presented in the second panel. Data on riders, shoplifting and pick-pocketing crimes, and population correspond to 2016, the year following the entry of delivery platforms into Paris. Statistics for Paris are excluded from the graphical representation to better highlight neighboring jurisdictions and potential spillover effects. Within the Paris police jurisdiction, there are approximately 50 riders and 122 shoplifting and pick-pocketing crimes per 10,000 inhabitants.

Figure A7: Marseille and the Surrounding Area (2017)



Notes: The red hatched area denotes the police jurisdiction of Marseille, while the dashed red line represents a 15-kilometer buffer extending from that boundary. Similarly, the solid blue line indicates the boundary of Aix-en-Provence, while the dashed blue line represents a 15-kilometer buffer extending from that boundary. Grey lines denote city boundaries in the left graph of the first panel, while in the other maps they indicate police jurisdiction borders. The population aged 15–54 is used to compute the statistics presented in the second panel. Data on riders, shoplifting and pick-pocketing crimes and population correspond to 2017, the year following the entry of delivery platforms into Marseille and Aix-en-Provence. Statistics for these cities are excluded from the graphical representation to better highlight neighboring jurisdictions and potential spillover effects. Within the Marseille police jurisdiction, there are approximately 11 riders and 81 shoplifting and pick-pocketing crimes per 10,000 inhabitants. Within the Aix-en-Provence police jurisdiction, there are approximately 15 riders and 56 shoplifting and pick-pocketing crimes per 10,000 inhabitants.

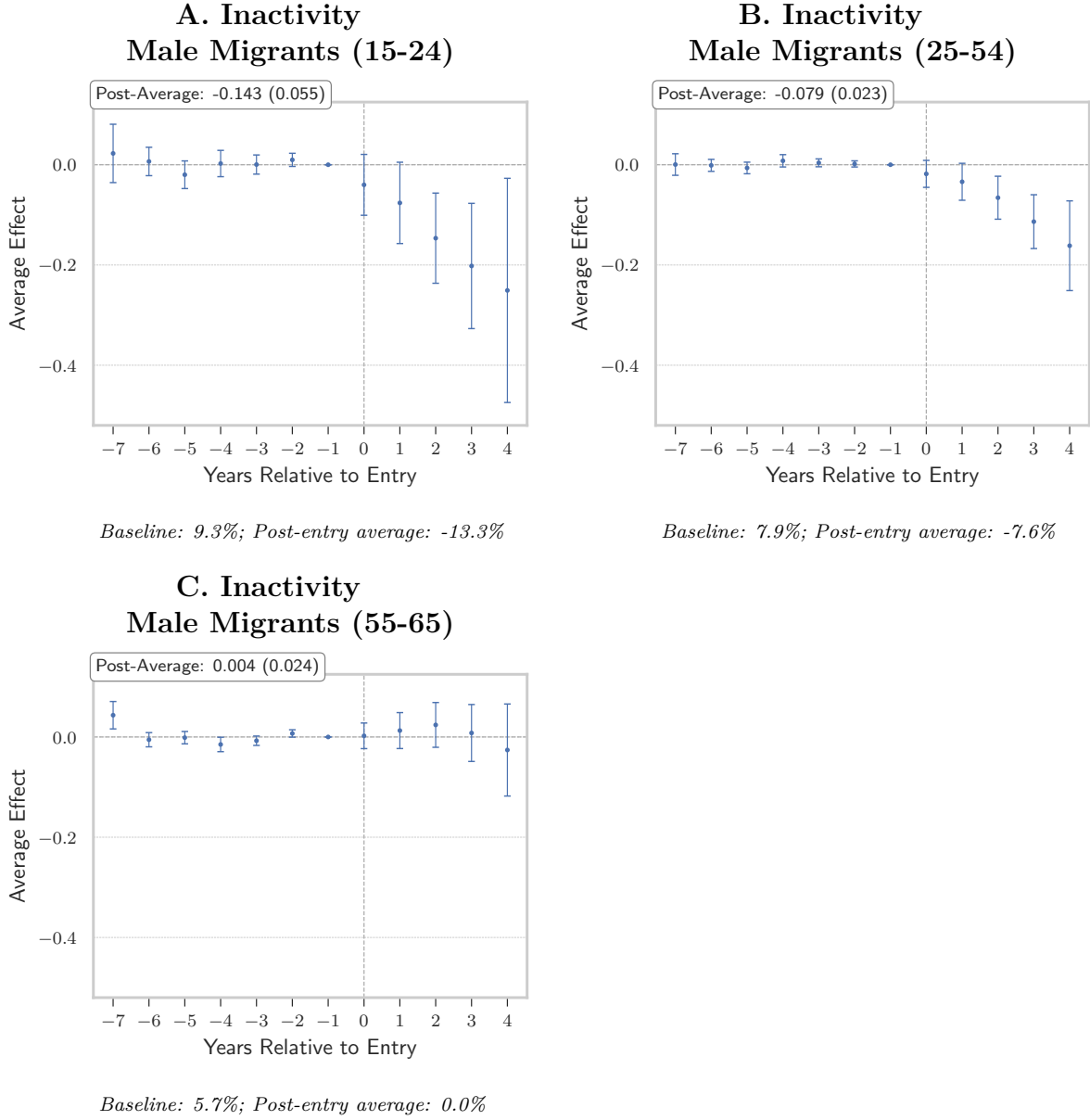
Figure A8: Lyon and the Surrounding Area (2016)



Notes: The red hatched area denotes the police jurisdiction of Lyon, while the dashed red line represents a 15-kilometer buffer extending from that boundary. Grey lines denote city boundaries in the left graph of the first panel, while in the other maps they indicate police jurisdiction borders. The population aged 15–54 is used to compute the statistics presented in the second panel. Data on riders, shoplifting and pick-pocketing crimes and population correspond to 2016, the year following the entry of delivery platforms into Lyon. Statistics for Lyon are excluded from the graphical representation to better highlight neighboring jurisdictions and potential spillover effects. Within the Lyon police jurisdiction, there are approximately 24 riders and 88 shoplifting and pick-pocketing crimes per 10,000 inhabitants.

K Male Migrant Inactivity by Age Category

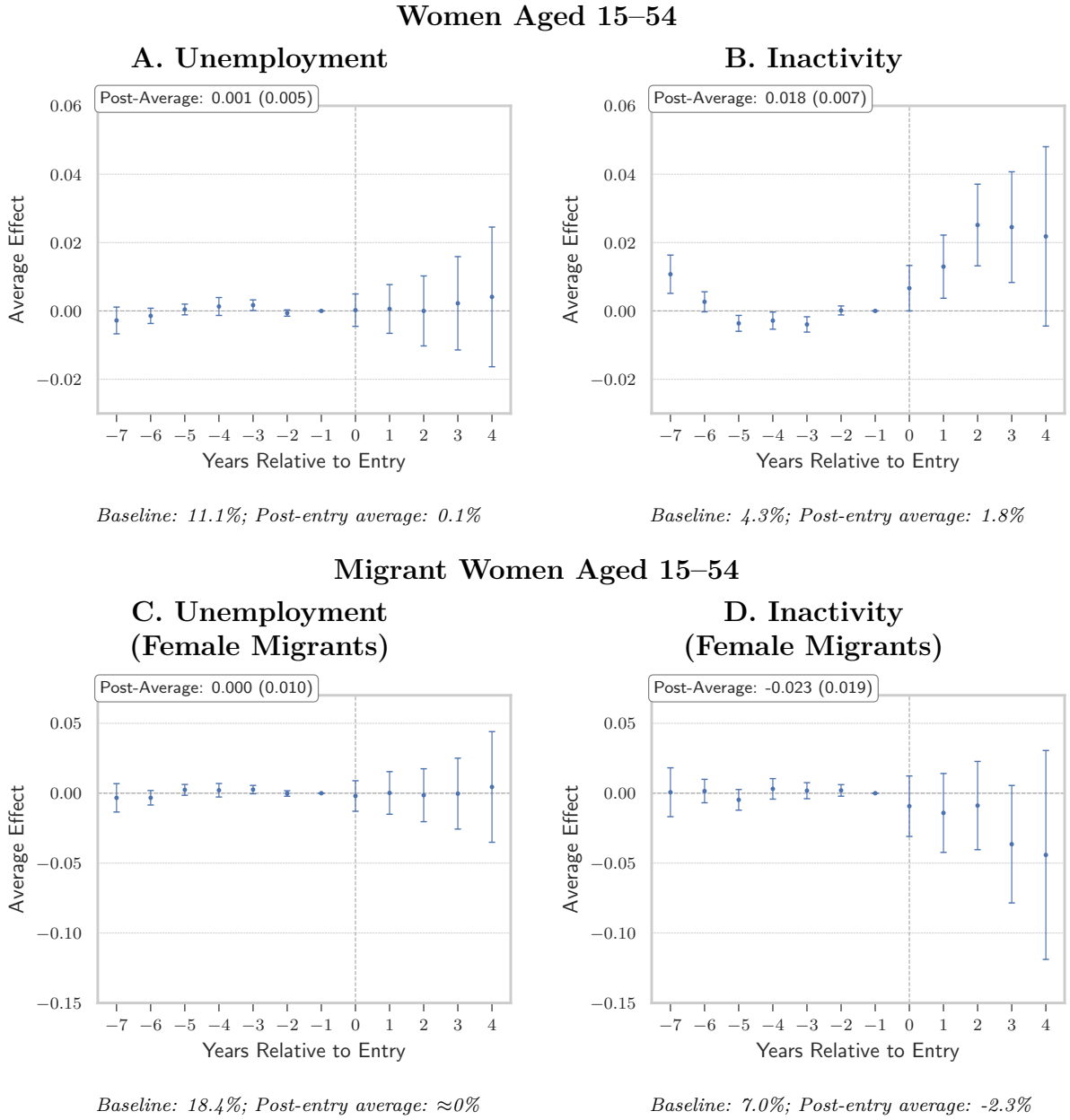
Figure A9: Event Study: Inactivity of Male Migrants by Age Category



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a food-delivery platform operates within a police jurisdiction, including a 15-km buffer around its boundary. Logged annual outcomes at the jurisdiction level for the age group shown in each panel. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Baselines are from 2014. Standard errors are clustered at the police jurisdiction level. The figure shows 95% confidence intervals. Jurisdictions excluded due to zero values in the outcome variable: 79 (Panel A), 4 (Panel B) and 8 (Panel C).

L Female Labor-Market Outcomes

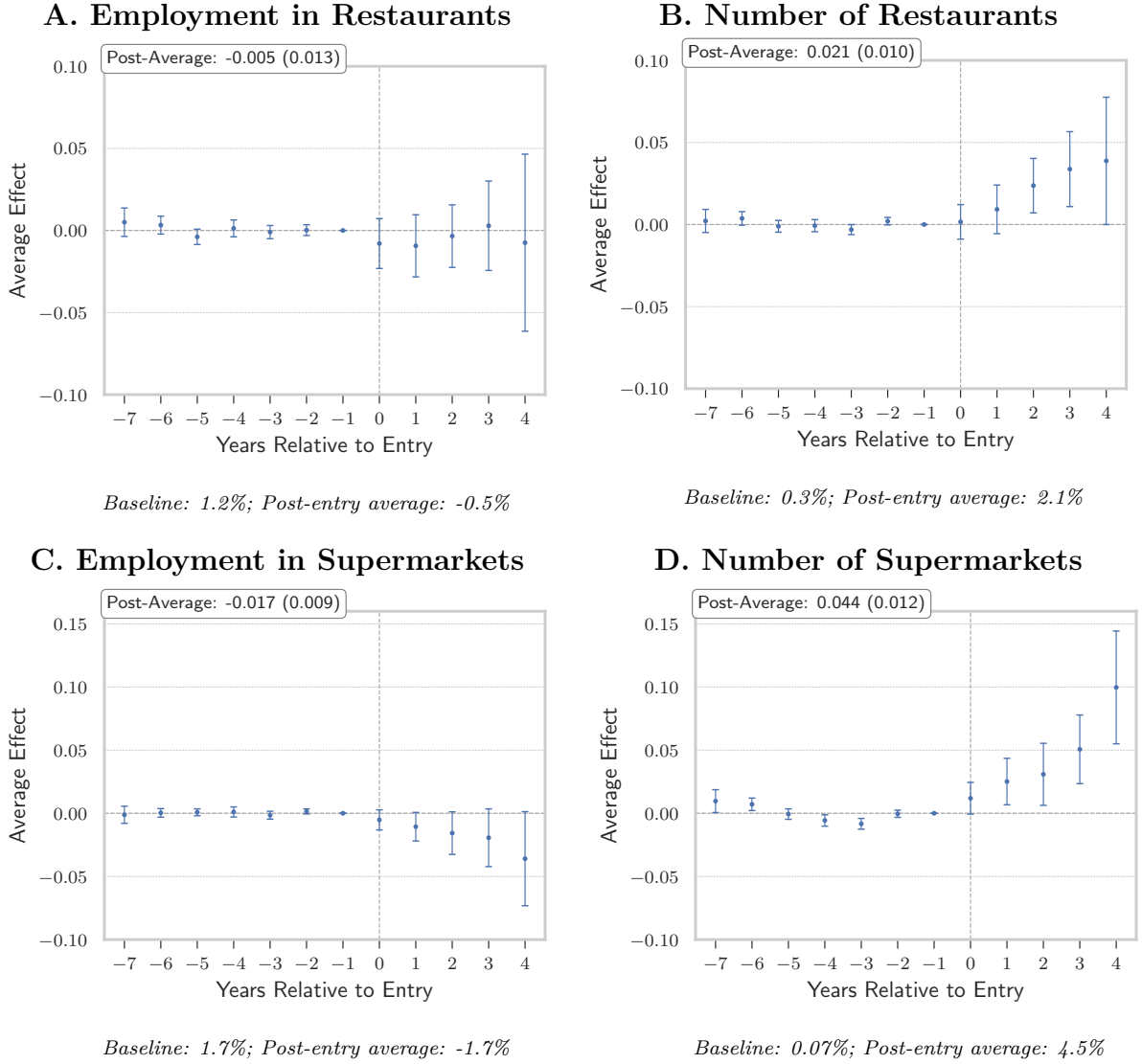
Figure A10: Event Study: Female Labor-Market Outcomes



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a delivery platform operates within a police jurisdiction or its 15-km buffer. The vertical dashed line marks event time 0. Pre-entry coefficients (event times -7 to -1) assess parallel trends. Outcomes are in logs. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. values show average treatment effects across post-entry years. Standard errors are clustered at the police jurisdiction level. The figure shows 95% confidence intervals. Number of police jurisdictions excluded due to zero values in the outcome variable: 1 (Panel C) and 1 (Panel D).

M Local Shops

Figure A11: Event Study: Local Shops



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a food-delivery platform operates within a police jurisdiction or its 15-km buffer. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. The omitted reference period is event time -1 . The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals. Jurisdictions excluded due to zero values in the outcome variable: 3 (Panel C) and 3 (Panel D).

N Robustness: Controls for Platform Entry

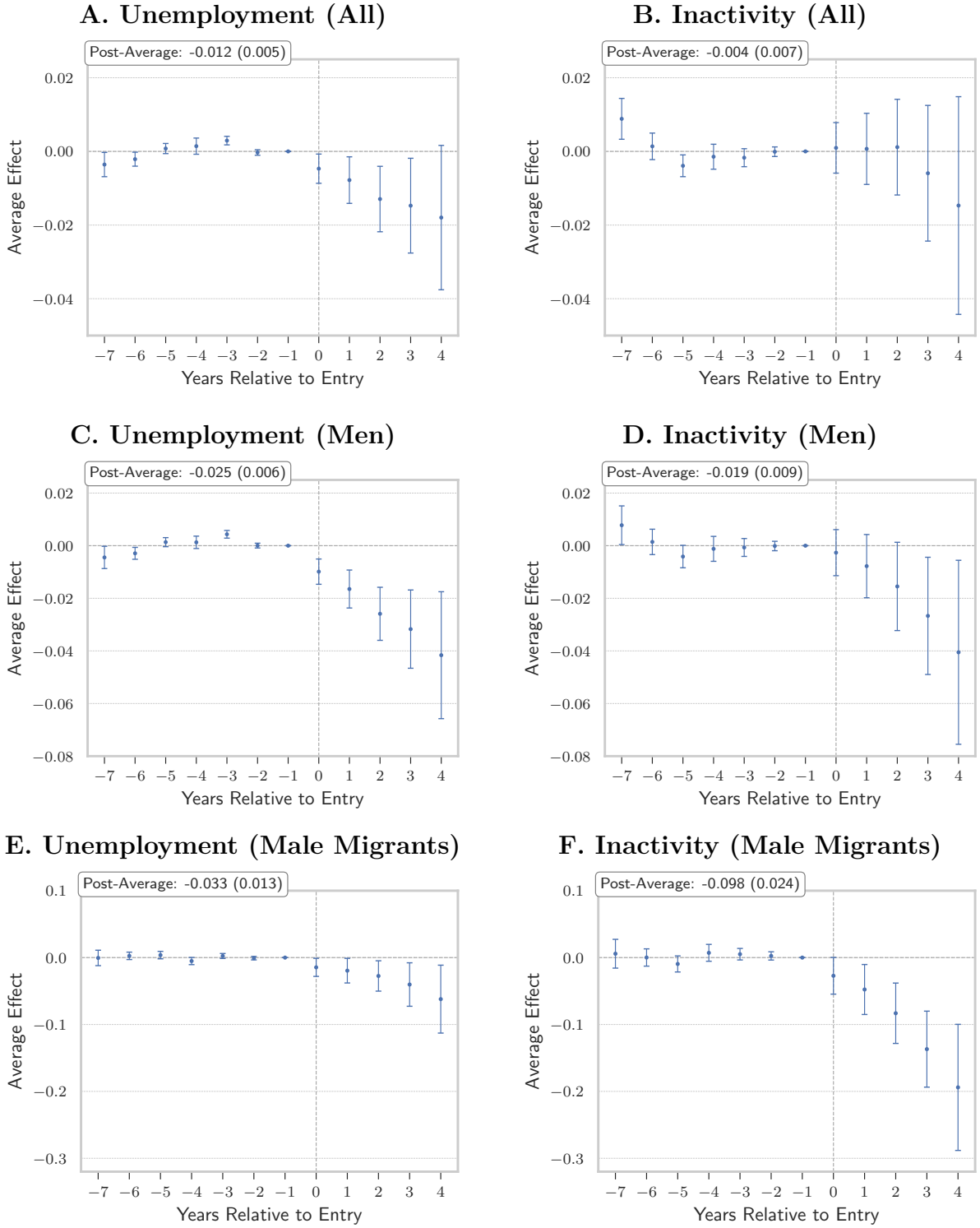
We add covariates to our staggered DiD estimator to assess whether observed local factors associated with platform rollout account for our estimates. If platforms enter jurisdictions in response to local characteristics that also predict labor-market or crime outcomes, our estimates could partly reflect those characteristics rather than platform entry. According to Uber Eats France’s Head of Expansion, the decision to enter an area depends on its population catchment area, restaurant base, and pool of potential riders. We therefore control for the population aged 15-54, the number of restaurants per capita, and the male unemployment and inactivity shares among individuals aged 15-54, which proxy for the pool of potential riders. To avoid mechanical correlation between the outcome and the control variables, we omit the male unemployment share from unemployment specifications and the male inactivity share from inactivity specifications.

Figure [A12](#) presents the results for labor-market outcomes when these covariates are included. The estimates are broadly similar to the baseline estimates. The main difference is that the decline in unemployment among male migrants, which is imprecisely estimated in the baseline specification, becomes statistically significant after the covariates are included.

Figure [A13](#) shows that the crime estimates also remain similar to the baseline estimates.

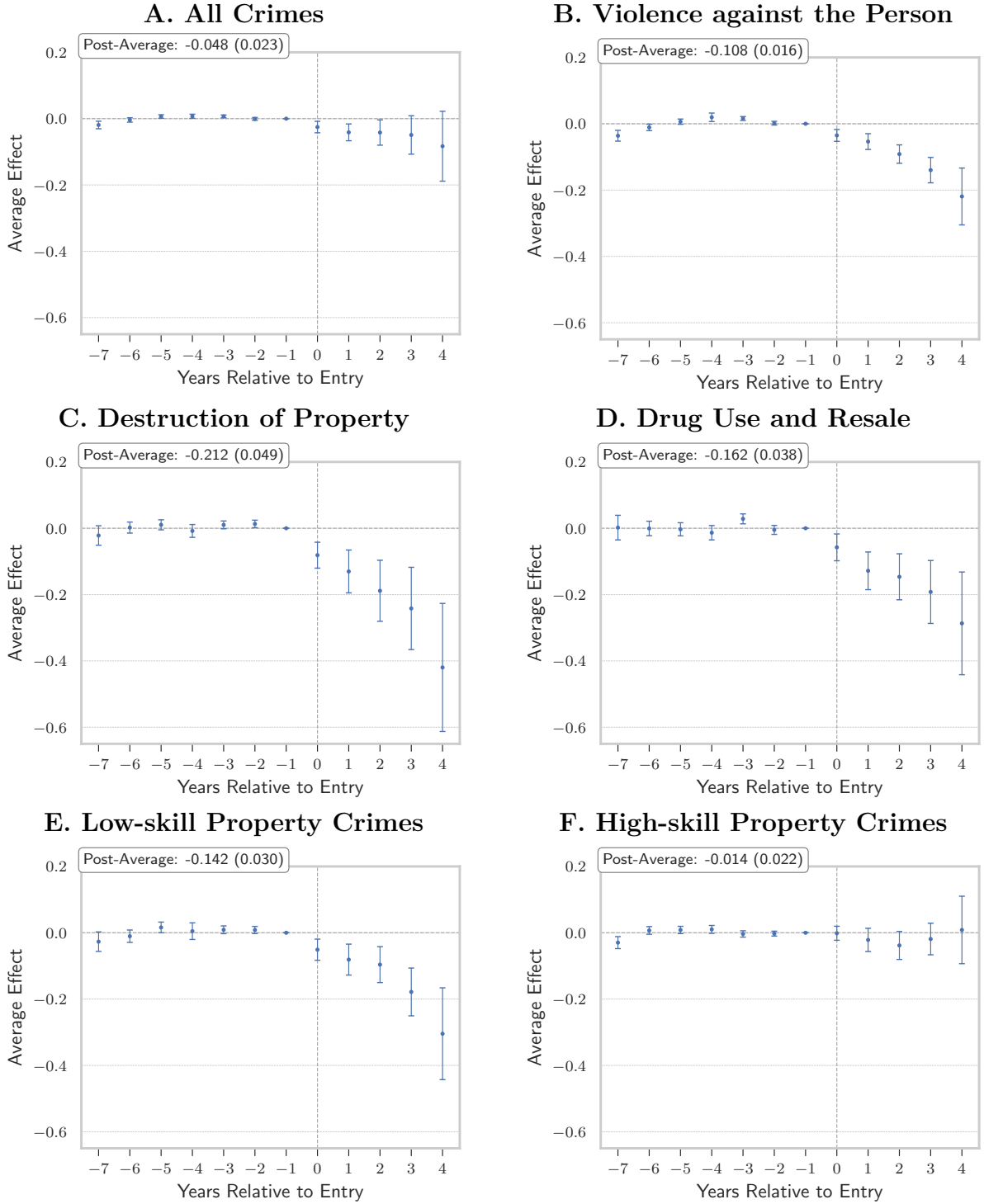
Overall, the estimates are robust to conditioning on observed factors associated with platform rollout. This robustness check reinforces the idea that, conditional on jurisdiction and year fixed effects, platform entry is orthogonal to local crime and labor-market dynamics.

Figure A12: Event Study with Controls for Platform Entry: Labor-Market Outcomes



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a food-delivery platform operates within a police jurisdiction or its 15-km buffer. All specifications control for the population aged 15-54 and the number of restaurants per capita. Unemployment specifications also control for the male inactivity share, and inactivity specifications control for the male unemployment share. Outcomes are logged annual unemployment and inactivity shares at the jurisdiction level for individuals aged 15-54. The omitted reference period is event time -1 . The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

Figure A13: Event Study with Controls for Platform Entry: Crime Outcomes



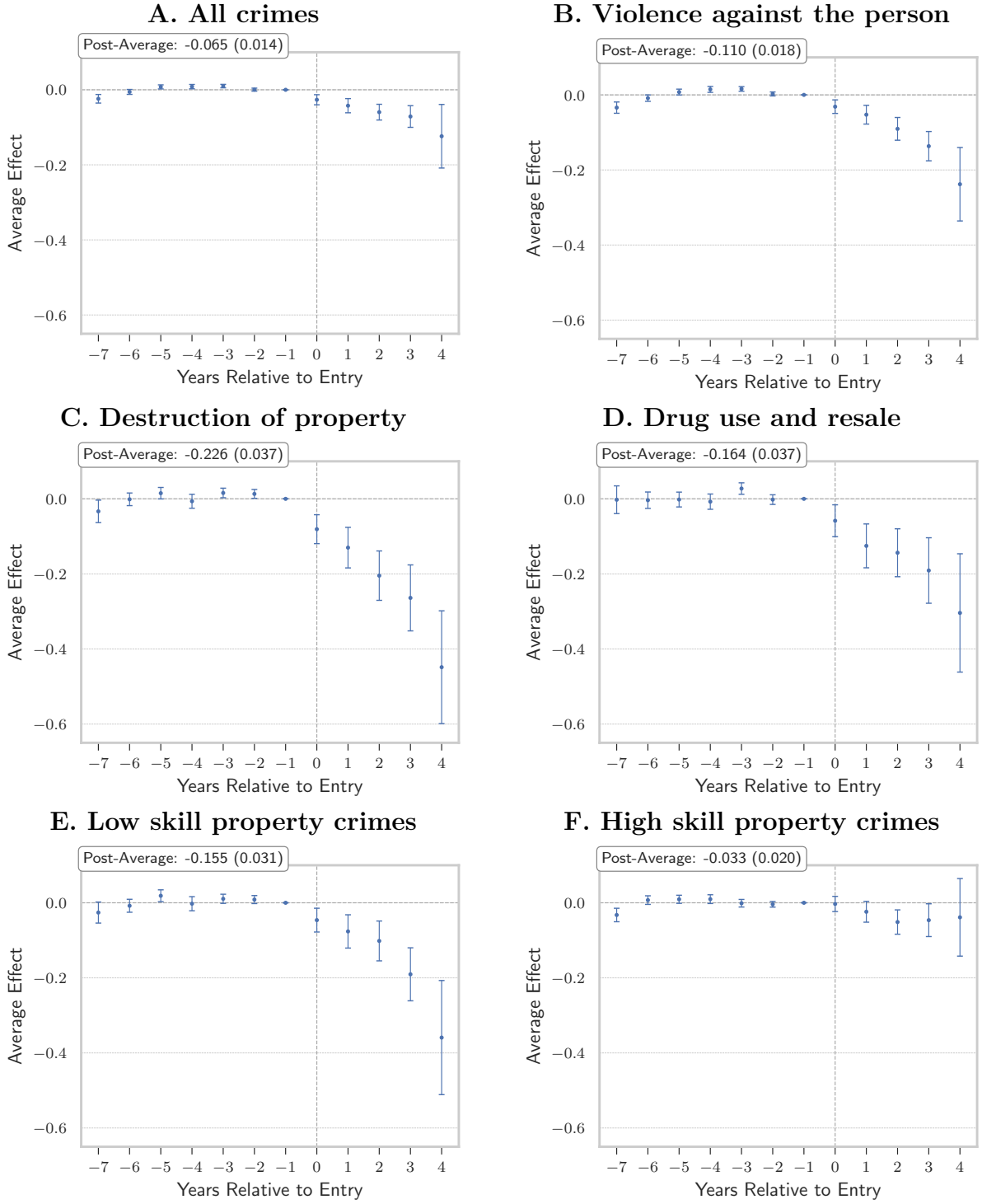
Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a food-delivery platform operates within a police jurisdiction or its 15-km buffer. All specifications control for the population aged 15-54, the number of restaurants per capita, and the male unemployment and inactivity shares among individuals aged 15-54. Outcomes are logged annual crime rates per 10,000 residents at the jurisdiction level. The omitted reference period is event time -1 . The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\text{Post-Average}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

O Robustness: Drop Big Cities (Not Surrounding Areas)

Food-delivery platforms entered France’s largest markets first, where consumer demand, restaurant supply, and the pool of potential riders are likely larger. To account for this pattern, our staggered DiD analysis weights police jurisdictions by their 2012 population. These weights give to jurisdictions with more inhabitants more influence, raising the concern that a few large urban cores may drive the results. We therefore re-estimate our model after excluding the police jurisdictions covering Paris, Marseille, and Lyon, France’s three most populous cities. In 2012, these jurisdictions represent 2.87 million working-age inhabitants out of 40.37 million across the 681 jurisdictions.

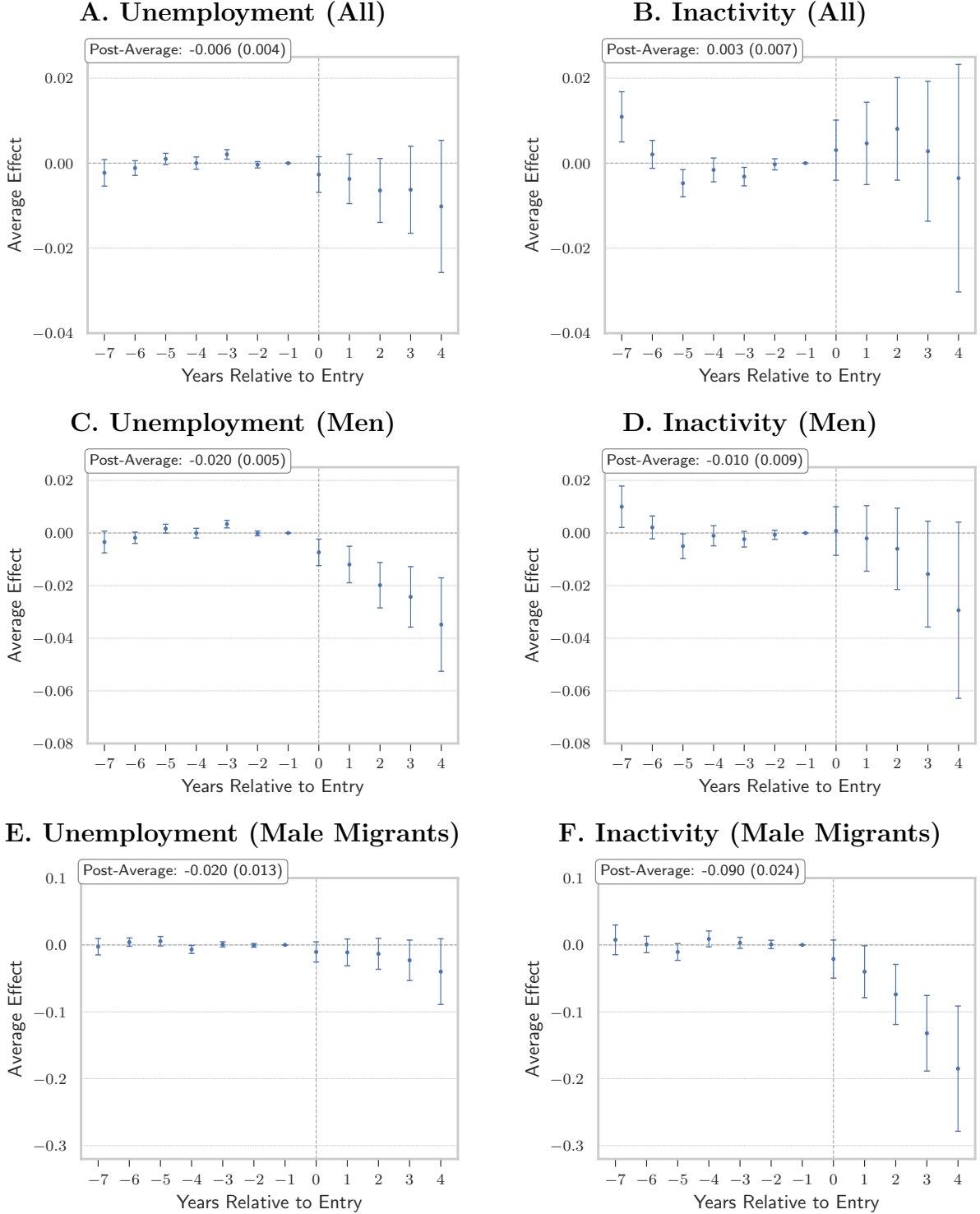
Figures [A14](#) and [A15](#) report the results after excluding these three major cities. The sample still retains treated jurisdictions within 15 km of these cities, as well as all other jurisdictions, consistent with the main estimation strategy. The labor-market and crime estimates remain close to the baseline, reducing concern that trends specific to the three major cities drive the main findings.

Figure A15: Event Study with Big Cities Excluded: Crime Outcomes



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a food-delivery platform operates within a police jurisdiction or its 15-km buffer. Police jurisdictions of Paris, Marseille, and Lyon are excluded. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\overline{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

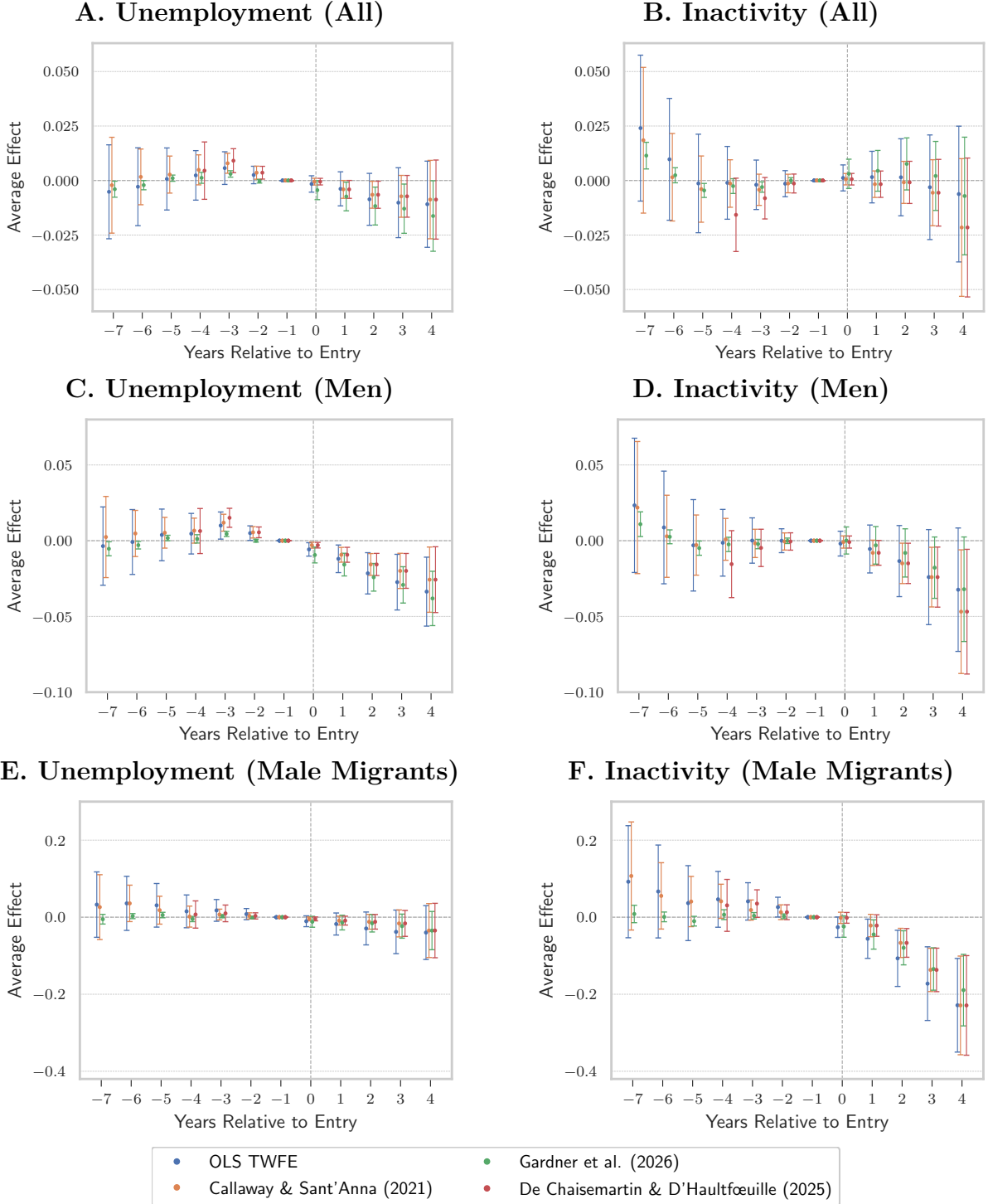
Figure A14: Event Study with Big Cities Excluded: Labor-Market Outcomes



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry is the first year in which a food-delivery platform operates within a police jurisdiction or its 15-km buffer. Police jurisdictions covering the three largest French cities (Paris, Marseille, and Lyon) are excluded. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

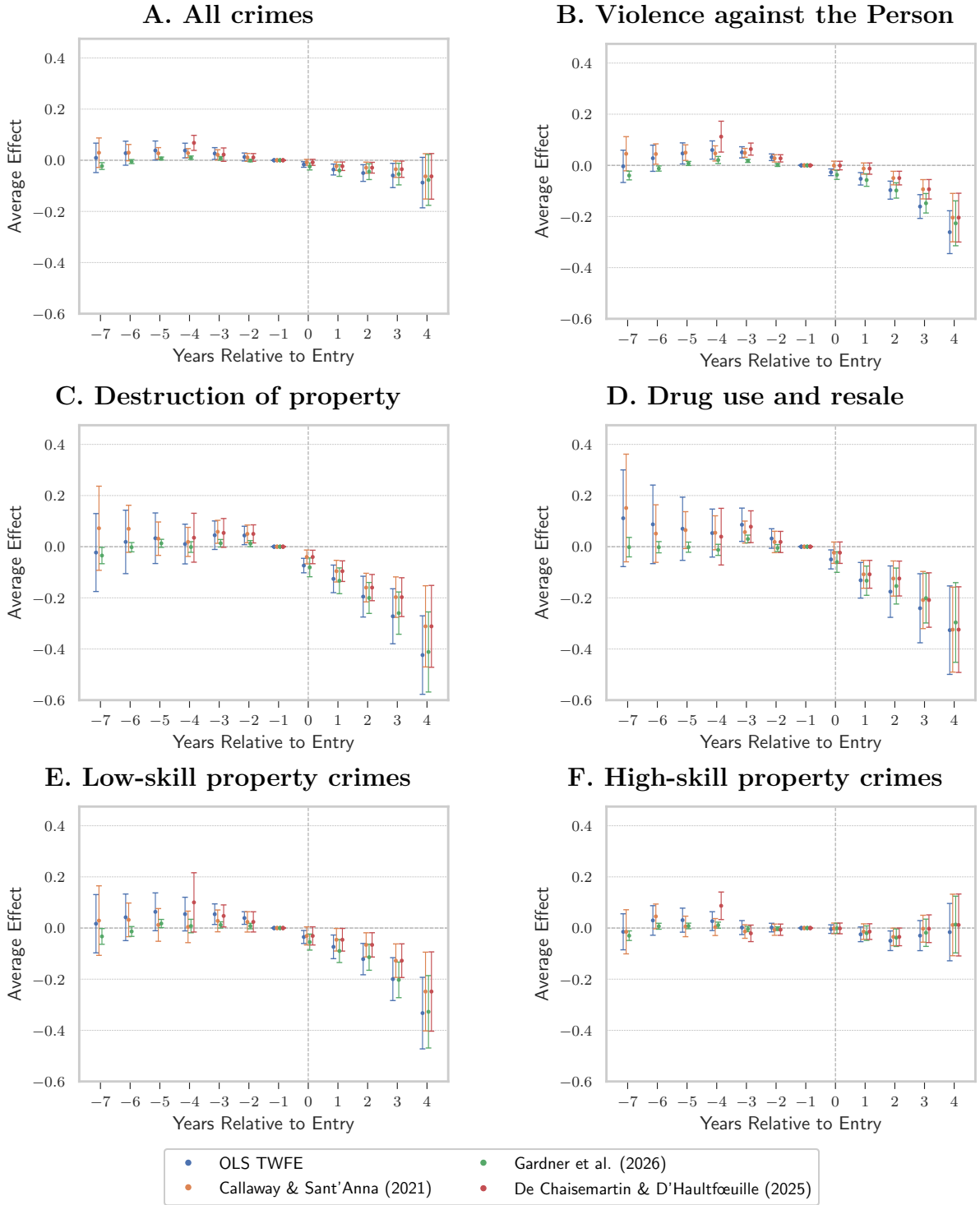
P Robustness: Alternative Estimators

Figure A16: Event Study with Alternative Estimators: Labor-Market Outcomes



Notes: The four estimators give very similar pre-entry and post-entry estimates. Event-study estimates using OLS TWFE (blue) and staggered DiD estimators: [Callaway and Sant'Anna \(2021\)](#) (orange), [Gardner et al. \(2026\)](#) (green), and [de Chaisemartin and D'Haultfœuille \(2025\)](#) (red). Platform entry: first year a food-delivery platform operates within a police jurisdiction, including a 15-km buffer around its boundary. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\text{Post-Average}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

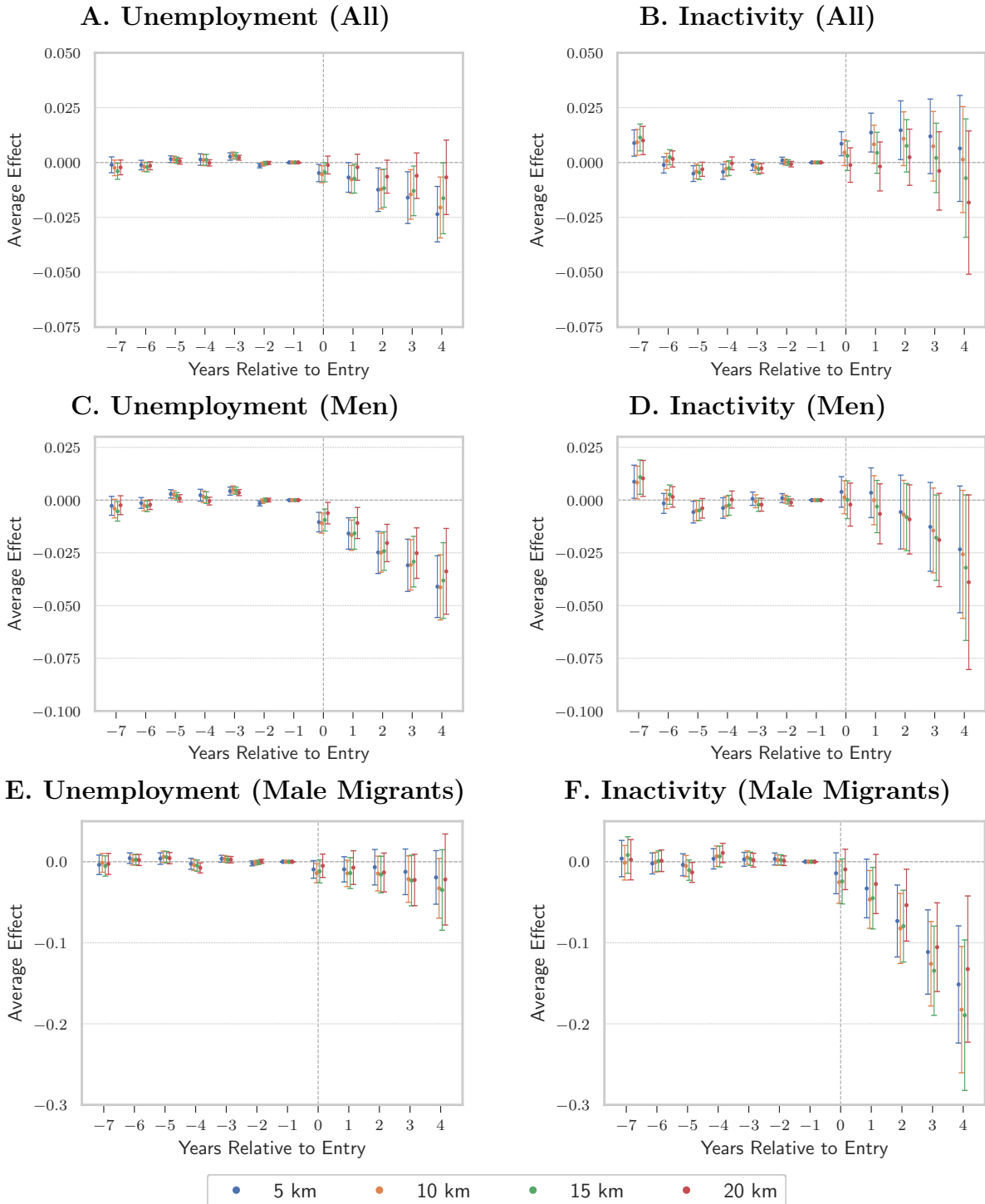
Figure A17: Event Study with Alternative Estimators: Crime Outcomes



Notes: The four estimators give very similar pre-entry and post-entry estimates. Event-study estimates using OLS TWFE (blue) and staggered DiD estimators: [Callaway and Sant'Anna \(2021\)](#) (orange), [Gardner et al. \(2026\)](#) (green), and [de Chaisemartin and d'Haultfœuille \(2025\)](#) (red). Platform entry: first year a food-delivery platform operates within a police jurisdiction, including a 15-km buffer around its boundary. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

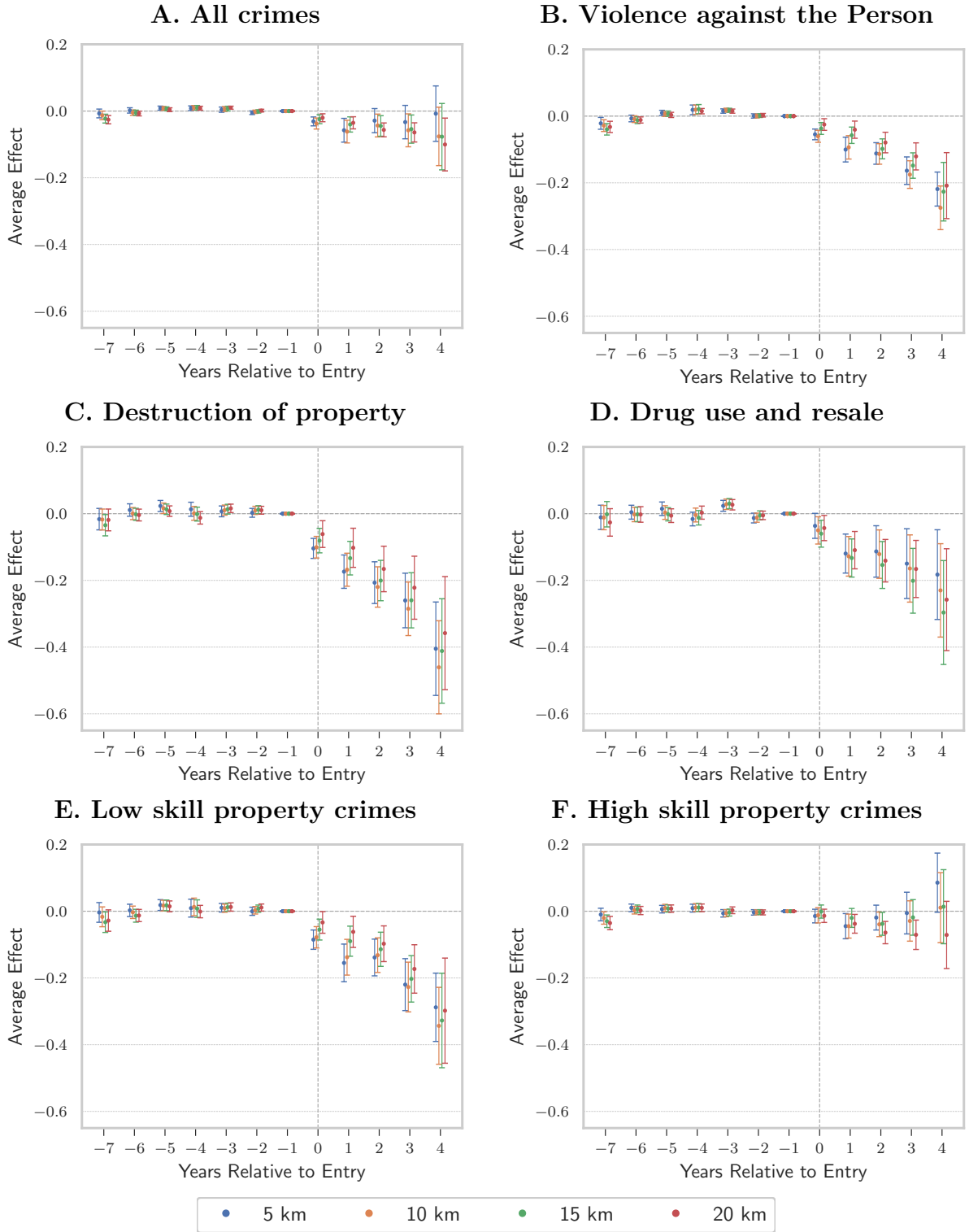
Q Robustness: Alternative Distance Buffers

Figure A18: Event Study with Alternative Buffers: Labor-Market Outcomes



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a food-delivery platform operates within a police jurisdiction, including a 5, 10, 15, or 20-km buffer around its boundary. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

Figure A19: Event Study with Alternative Buffers: Crime Outcomes



Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Platform entry: first year a food-delivery platform operates within a police jurisdiction, including a 5, 10, 15, or 20-km buffer around its boundary. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

R Robustness: Honest Difference-in-Differences

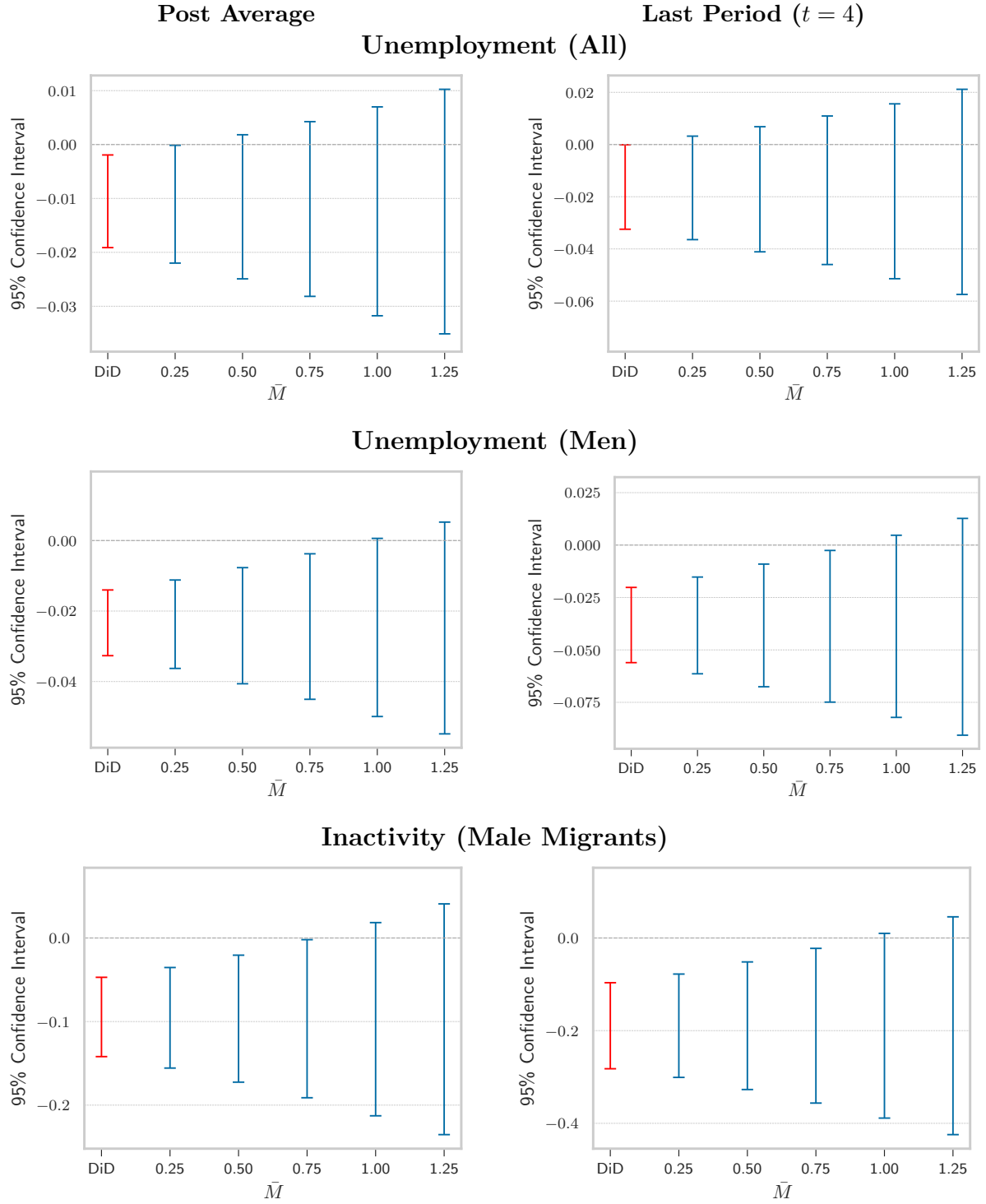
To assess the sensitivity of our findings to violations of the parallel trends assumption, we apply the method of [Rambachan and Roth \(2023\)](#) to our baseline estimates. The method imposes a relative-magnitude (RM) restriction: the largest post-treatment violation of parallel trends between consecutive periods cannot exceed $\bar{M}$ times the largest pre-treatment violation. Thus, $\bar{M} = 0.25$ permits a post-treatment violation up to one-quarter as large as the largest pre-treatment violation, $\bar{M} = 1$ permits one equally large, and $\bar{M} = 1.25$ permits one up to 25% larger. We report robust 95% confidence intervals for values of $\bar{M}$ ranging from 0.25 to 1.25 for both the average post-treatment effect and for the effect at the last observed event time ($t = 4$).

Figure [A20](#) displays the labor-market results. We focus on the outcomes whose baseline 95% confidence intervals exclude zero in Figure [5](#), i.e., Panels A (unemployment–all), C (unemployment–men), and F (inactivity–male migrants). We examine how their confidence intervals widen as we allow for increasingly large deviations from parallel trends. The results are reassuring. For male unemployment and male migrant inactivity, the estimates remain statistically significant up to $\bar{M} = 0.75$, indicating that the parallel trends assumption would have to be violated by a sizable margin before these findings disappear. The overall unemployment rate is the exception. Its baseline post-entry estimate in Figure [5](#) is small (-0.011 , with a standard error of 0.004), so its confidence interval already lies relatively close to zero under exact parallel trends. The HonestDiD interval therefore includes zero at lower values of $\bar{M}$.

Turning to crime outcomes, Figure [A21](#) reports the same exercise for outcomes whose baseline 95% confidence intervals exclude zero in Figure [6](#). This includes all panels except those for high-skill property crimes. The picture is broadly similar: violence against the person, vandalism and destruction, and low-skill property crimes remain statistically significant through $\bar{M} = 0.75$. Thus, allowing post-treatment deviations from parallel trends up to 0.75 times the largest observed pre-treatment deviation does not change the conclusion that these effects differ from zero. The intervals for drug use and resale exclude zero through $\bar{M} = 0.5$. The all-crime aggregate, which captures a mix of heterogeneous subcategories, is most sensitive: its robust intervals include zero by $\bar{M} = 0.25$.

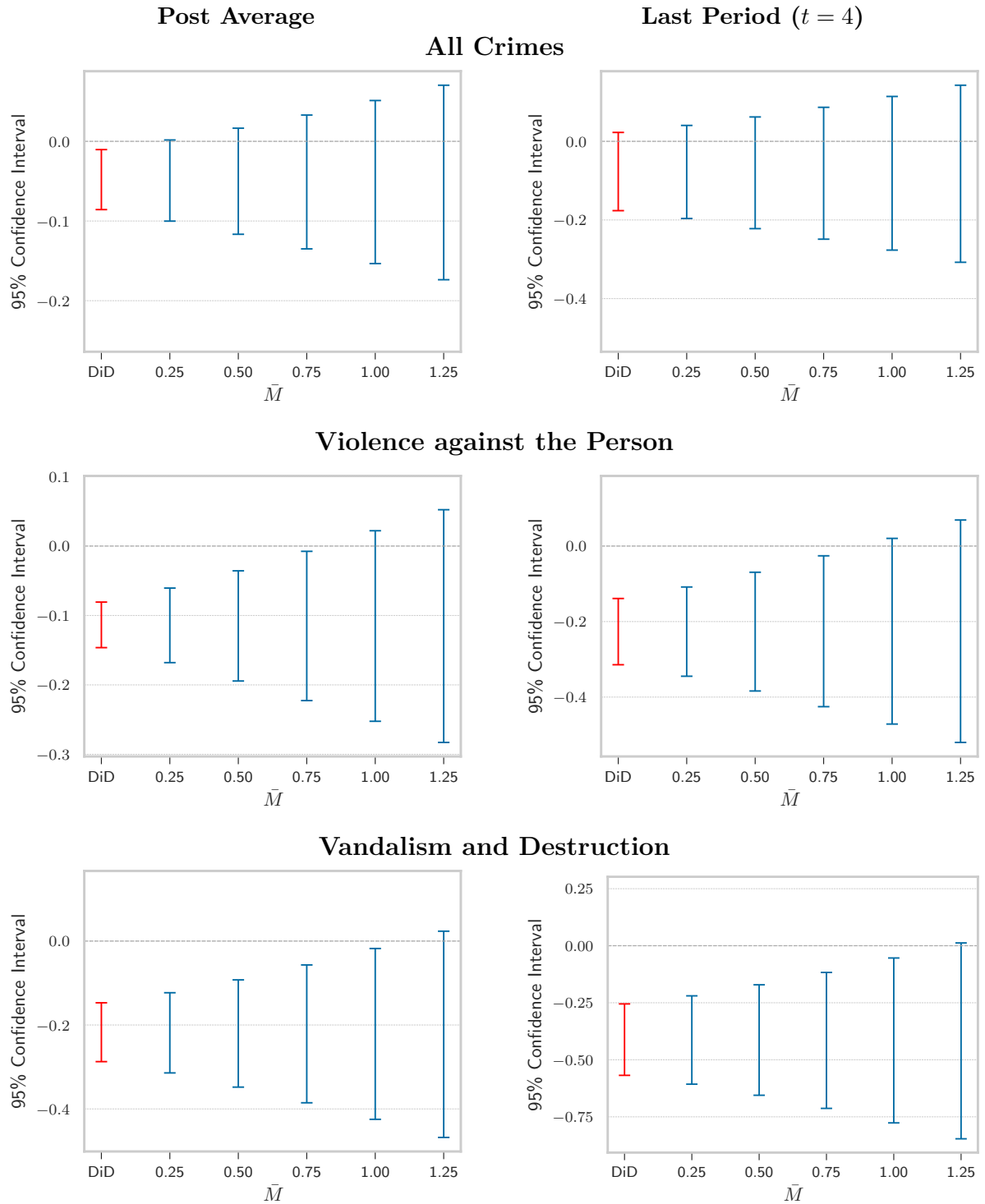
Taken together, this sensitivity analysis shows that several main estimates remain statistically distinguishable from zero under departures from exact parallel trends calibrated to the pre-treatment violations, while overall unemployment and drug offenses are more sensitive. This exercise quantifies how the conclusions change as the allowed post-treatment violation grows relative to the pre-treatment violations.

Figure A20: HonestDiD Sensitivity Analysis: Labor-Market Outcomes



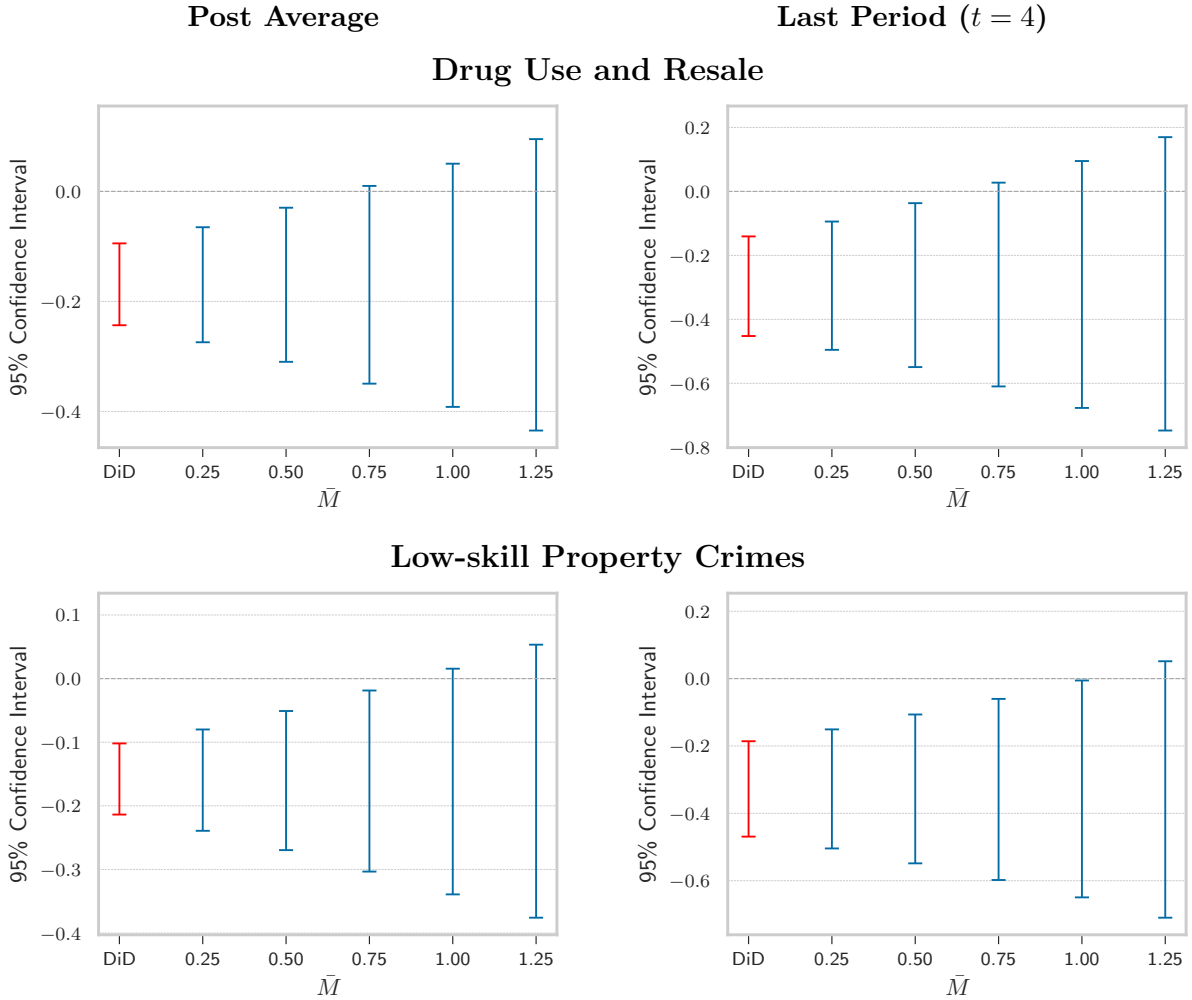
Notes: Robust 95% confidence intervals from the HonestDiD sensitivity analysis of [Rambachan and Roth \(2023\)](#), applied to the baseline staggered DiD estimates of Panels A, C, and F in Figure 5. We impose the *relative magnitudes* (RM) restriction, which bounds the degree to which the slope of parallel trends may change between consecutive periods by a factor $\bar{M}$ relative to the maximum pre-treatment violation. The DiD interval reports the baseline staggered DiD confidence interval from Figure 5. *Post Average* reports the confidence interval for the average effect across event times 0 to 4; *Last Period* ($t = 4$) reports the confidence interval for the last observed post-treatment event time. The dashed horizontal line marks zero.

Figure A21: HonestDiD Sensitivity Analysis: Crime Outcomes



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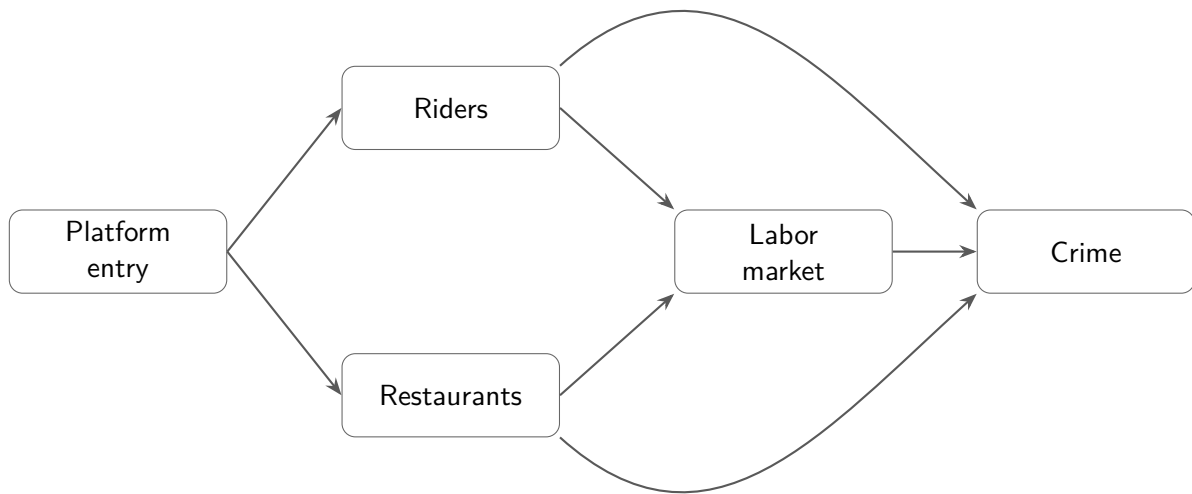


Notes: Robust 95% confidence intervals from the honest difference-in-differences sensitivity analysis of [Rambachan and Roth \(2023\)](#), applied to Panels A to F in Figure 6. We impose the *relative magnitudes* (RM) restriction, which bounds the degree to which the slope of parallel trends may change between consecutive periods by a factor $\bar{M}$ relative to the maximum pre-treatment violation. The DiD interval reports the baseline staggered DiD confidence interval from Figure 6. *Post Average* reports the confidence interval for the average effect across event times 0 to 4; *Last Period* ($t = 4$) reports the confidence interval for the last observed post-treatment event time. The dashed horizontal line marks zero.

S Mediation Analysis

We use an exploratory mediation analysis to examine the pathways linking platform entry, rider supply, restaurant participation, labor-market conditions, and crime. As Figure A22 illustrates, platform entry affects rider supply and restaurant participation, which may in turn influence unemployment, inactivity, and crime directly; unemployment and inactivity may then affect crime through a further, indirect channel. The conceptual framework identifies two channels through which gig work may reduce crime: economic deterrence and self-incapacitation (Section 2). The mediation exercise cannot distinguish between them. Unemployment and inactivity capture labor-market activation but do not map uniquely to either mechanism. These labor-market outcomes are therefore relevant, but incomplete, mediators of the deterrence and incapacitation channels.

Figure A22: Potential Pathways from Platform Entry to Crime



A full causal decomposition would require identifying each post-entry link in Figure A22. Rider supply and restaurant participation complicate this task: platform entry affects both, and each may in turn influence labor-market conditions and crime. Rider supply and restaurant participation therefore act as “exposure-induced confounders” of the relationship between labor-market conditions and crime. Omitting these confounders from the model leaves the relationship confounded, whereas conditioning on them can absorb part of the platform-entry effect. Standard mediation methods therefore cannot identify the causal contribution of each post-entry link without further assumptions (Imai and Yamamoto, 2013).⁶⁰

Although this identification issue limits the interpretation of the mediation analysis, it does not make the exercise uninformative. The analysis goes beyond documenting

⁶⁰One might try to address this identification problem by instrumenting rider supply with platform entry. The instrument isolates the variation in rider supply induced by platform entry, but it does not identify the full causal sequence. Platform entry may also affect crime through restaurant participation or other channels. Without further restrictions, standard instrumental variables estimation therefore does not identify the causal effect of labor-market conditions on crime or separate the rider effect into direct and indirect components (Dippel et al., 2017).

separate entry effects on riders, labor-market outcomes, and crime. It examines whether the successive links predicted by the conceptual framework appear in the data. It identifies which post-entry variables produce the largest changes in the estimated coefficients, and shows which labor-market outcomes contribute most to the rider-crime relationship.

We therefore use two complementary steps to assess the empirical relevance of the proposed mechanism. Step 1 measures how the estimated responses of labor-market and crime outcomes change after adding rider and restaurant activity. Step 2 focuses on the pathway from platform-induced rider supply to crime through observed changes in unemployment and inactivity. Evidence pointing in the same direction across the two steps strengthens the interpretation that expanded access to gig work links platform entry to labor-market activation and lower crime. The estimates remain informative about the direction and relative empirical importance of the proposed pathways, even though they do not deliver causal path-specific shares or separate economic deterrence from self-incapacitation.

Step 1: Decomposing the Platform Entry Coefficient

Using the staggered DiD estimator of [Gardner et al. \(2026\)](#), we first estimate the total effect of platform entry:

$$Y_{zt} = \alpha_z + \lambda_t + \sum_{\tau=-7, \tau \neq -1}^4 \tau_{\tau}^{\text{Tot}} \text{Entry}_{zt}^{\tau} + \epsilon_{zt}, \quad (2)$$

and the average post-treatment total effect is $\hat{\tau}^{\text{Tot}} = \frac{1}{5} \sum_{\tau=0}^4 \hat{\tau}_{\tau}^{\text{Tot}}$.

Second, we add the post-entry rider and restaurant rates to obtain the direct effect of platform entry:

$$Y_{zt} = \alpha_z + \lambda_t + \sum_{\tau=-7, \tau \neq -1}^4 \delta_{\tau} \text{Entry}_{zt}^{\tau} + \beta_1 M_{zt}^{\text{rider}} + \beta_2 M_{zt}^{\text{rest}} + \epsilon_{zt}, \quad (3)$$

where M_{zt}^{rider} is the rate of riders per capita and M_{zt}^{rest} is the rate of restaurants per capita. The average post-treatment direct effect is $\hat{\tau}^{\text{Dir}} = \frac{1}{5} \sum_{\tau=0}^4 \hat{\delta}_{\tau}$.

Third, the *indirect effect*, i.e., the share mediated through riders and restaurants, is:

$$\hat{\tau}^{\text{Indir}} = \hat{\tau}^{\text{Tot}} - \hat{\tau}^{\text{Dir}}. \quad (4)$$

To decompose the indirect effect by mediator, we estimate the effect of platform entry on each mediator M_{zt}^k separately:

$$M_{zt}^k = \alpha_z + \lambda_t + \sum_{\tau=-7, \tau \neq -1}^4 \omega_{\tau}^k \text{Entry}_{zt}^{\tau} + \nu_{zt}, \quad (5)$$

and the mediator-specific indirect contribution is $\hat{\beta}_k \times \hat{\omega}^k$, where $\hat{\alpha}^k = \frac{1}{5} \sum_{\tau=0}^4 \hat{\omega}_{\tau}^k$ and $\hat{\beta}_k$ is the coefficient on M^k from Equation (3).

Figure A23 displays the total effect of platform entry, the indirect effect resulting from mediation through riders and restaurants, and the direct effect on labor-market outcomes for each relative year following entry. The figure includes outcomes that are statistically significant in the main estimates. For all outcomes, a non-trivial share of the total effect

is mediated through the number of restaurants and riders operating in the local labor market. The only outcome for which the indirect effect is not statistically significant is male inactivity, but this is simply because the total effect itself is not significant for this outcome. These results show that the estimated reductions in inactivity and unemployment among men and male immigrants are partly mediated through the changes in the number of riders and restaurants following platform entry. Significant indirect effects through these two channels indicate that platform-induced growth in riders and restaurant participation accounts for part of the estimated improvement in local labor-market outcomes for these demographic groups.

Figure A24 presents the same decomposition for crime outcomes. With the exception of drug usage and resale, all estimates indicate that riders and restaurants mediate a significant share of the effect of platform entry on local crime. These findings suggest that the decrease in criminal activity following platform entry operates, at least in part, through the expansion of the delivery workforce and restaurant network, rather than through alternative channels unrelated to the gig economy labor market.

Table A9 reports the average post-treatment effect, the total mediated share, and the contribution of riders to the total mediated effect, with the remainder attributable to restaurants. For labor-market outcomes, the indirect effect accounts for 46% to 73% of the total effect. For crime outcomes, excluding drug usage and resale, the indirect effect accounts for 32% to 50% of the total effect. The lower mediated share for crime relative to labor-market outcomes may reflect the spatial mismatch between where riders and restaurants are located and where crimes occur. Across all outcomes, more than 98% of the mediated effect is attributable to the increase in the number of riders, indicating that most of the indirect effect operates through the gig economy workforce rather than through the restaurant channel, which is consistent with the stable restaurant workforce after platform entry in panel A of Appendix Figure A11.

S.1 Step 2: Probing the Labor-Market Mediation

The effect of riders and restaurants on crime estimated in Step 1 bundles the impact of platform activity with an effect operating through the labor market. This second step examines directly whether labor-market conditions contribute to the link between platform activity and crime. We use platform entry as an instrument for the local stock of riders.



We estimate whether labor-market outcomes mediate the effect of riders on crime with two-stage least squares (2SLS), instrumenting the number of riders per capita with relative-time-to-entry indicators. We proceed in three steps. First, the *total effect* of riders on crime is estimated from:

$$Y_{zt} = \alpha_z + \lambda_t + \tau^{\text{Tot}} \log(\text{Riders}_{zt}) + \epsilon_{zt}, \quad (6)$$

where $\log(\text{Riders}_{zt})$ is instrumented by the post-entry indicators $\{\text{Entry}_{zt}^\tau\}_{\tau=0}^4$.

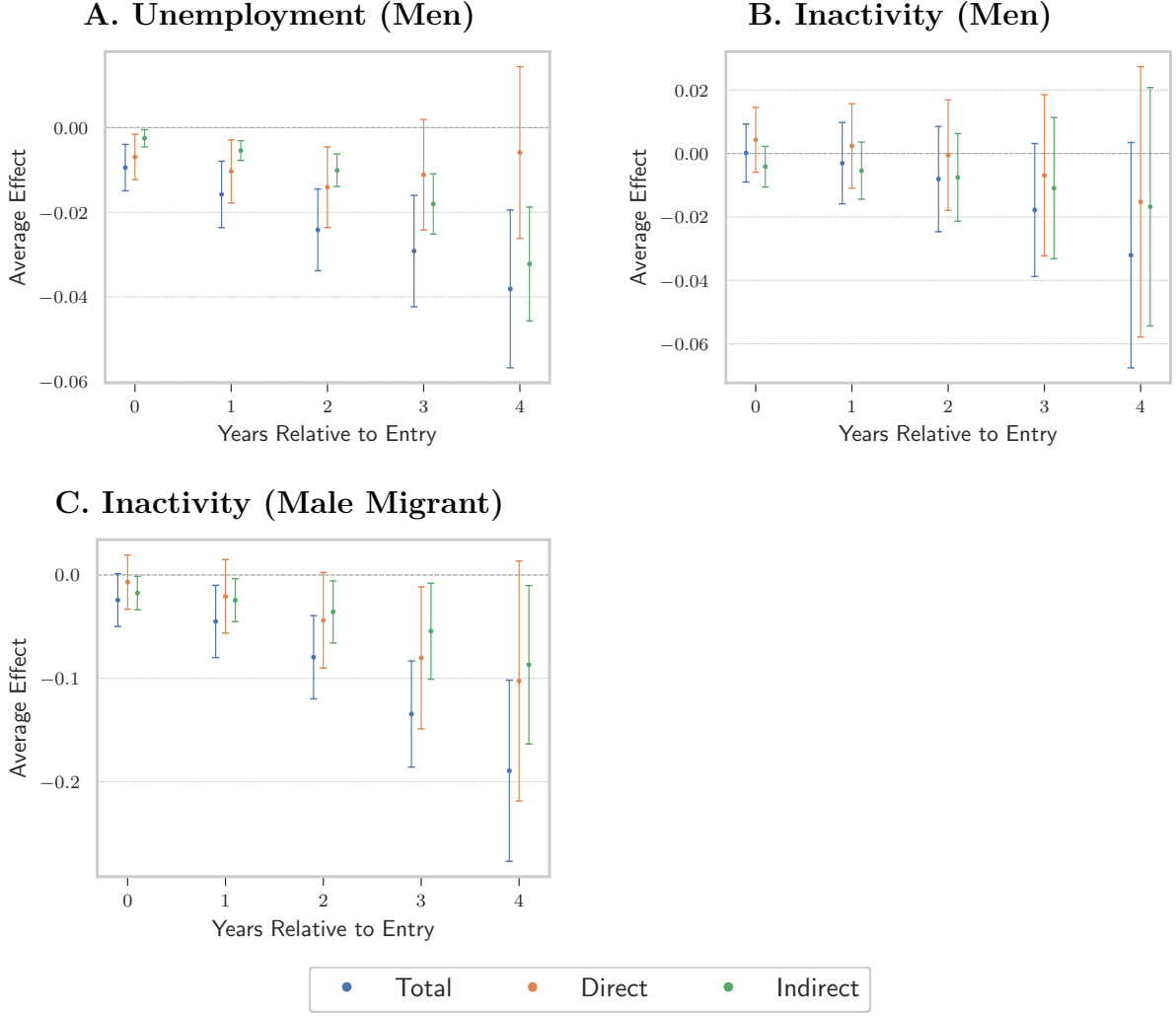
Second, we obtain the direct effect, i.e., the portion not operating through labor-

Table A9: Mediation Analysis: Average Post-Treatment Effects

	Indirect	Direct	Total	% Mediated	Rider Share
Violence against the Person	-0.046*** (0.013)	-0.067*** (0.024)	-0.114*** (0.017)	40.58	99.75
Destruction and Vandalism	-0.070** (0.030)	-0.148*** (0.043)	-0.217*** (0.036)	32.01	100.11
Drug Use and Resale	0.020 (0.032)	-0.189*** (0.043)	-0.169*** (0.040)	-11.63	100.28
Low-skill property crimes	-0.079*** (0.024)	-0.079** (0.040)	-0.158*** (0.027)	50.23	99.72
Male Unemployment	-0.014*** (0.003)	-0.010* (0.005)	-0.023*** (0.005)	58.52	99.80
Male Inactivity	-0.009 (0.009)	-0.003 (0.010)	-0.012 (0.009)	73.73	98.11
Inactivity of Male Migrants	-0.044** (0.019)	-0.051* (0.028)	-0.095*** (0.022)	46.26	98.48

Notes: Estimates obtained using [Gardner et al. \(2026\)](#) staggered DiD estimator. Each row reports post-average treatment effects (mean over $t = 0$ to $t = 4$). *% Mediated* reports $100 \times \text{Indirect}/\text{Total}$. *Rider Share* reports the share of the indirect effect attributable to delivery workers, and the remainder is attributable to restaurants. Bootstrapped standard errors in parentheses, clustered at the jurisdiction level. Significance levels: * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Figure A23: Event Study with Mediation Analysis: Labor-Market Outcomes



Notes: Mediation analysis using the staggered DiD estimator of [Gardner et al. \(2026\)](#). The figure displays the total effect of platform entry, the direct effect, and the indirect effect driven by the increase in the rate of delivery workers and restaurants per capita. Only post-treatment estimates are shown. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. Bootstrapped standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

market channels, by adding the mediators to the specification:

$$Y_{zt} = \alpha_z + \lambda_t + \delta \log(\text{Riders}_{zt}) + \sum_k \beta_k \log(M_{zt}^k) + \epsilon_{zt}, \quad (7)$$

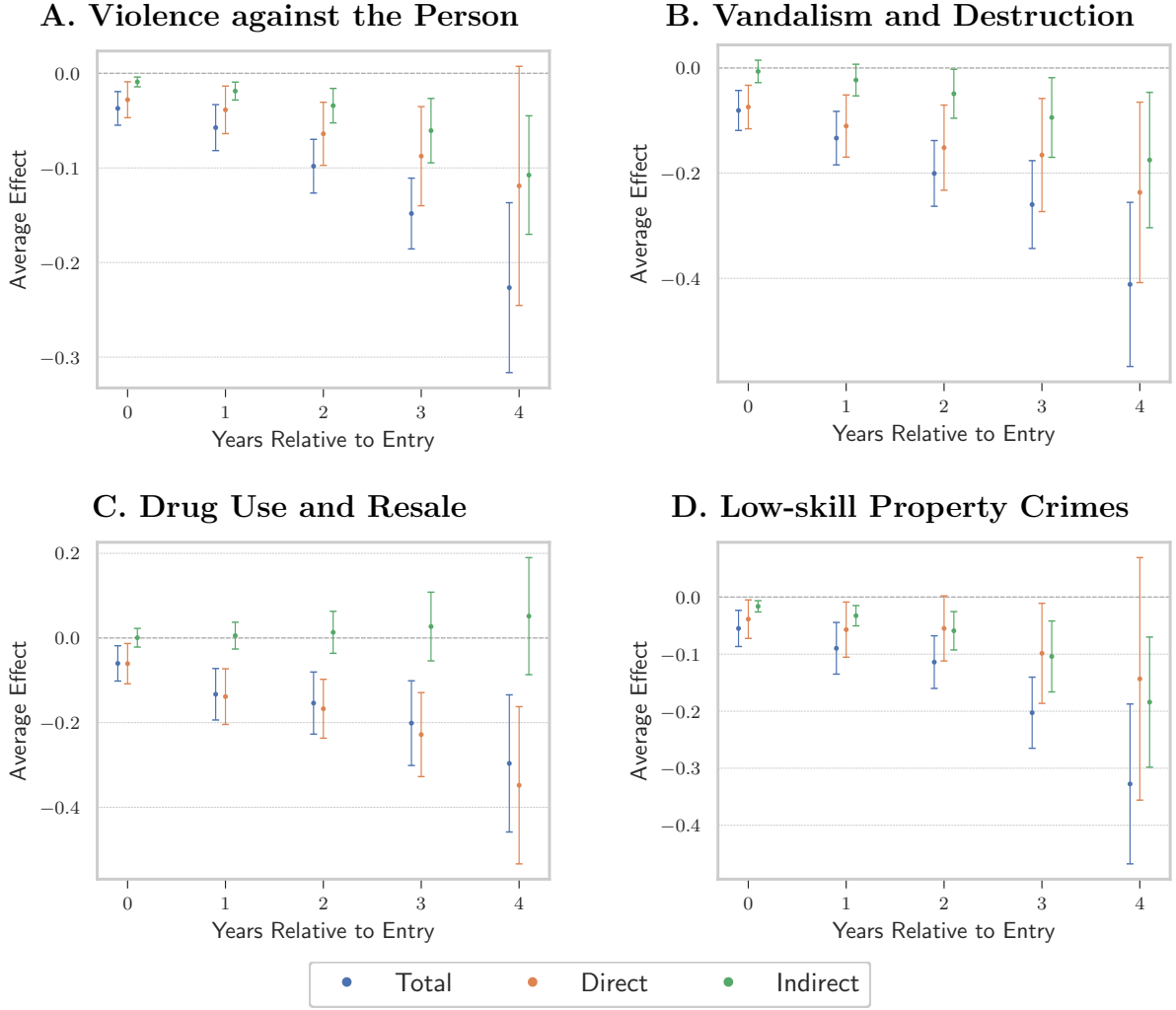
where $\log(M_{zt}^k)$ captures labor-market measures for men—since they constitute the vast majority of delivery workers—including male inactivity, male immigrant inactivity, male unemployment, and male immigrant unemployment. The direct effect is $\hat{\tau}^{\text{Dir}} = \hat{\delta}$.

Third, the *indirect effect*, i.e., the share mediated through labor-market outcomes, is:

$$\hat{\tau}^{\text{Indir}} = \hat{\tau}^{\text{Tot}} - \hat{\tau}^{\text{Dir}}. \quad (8)$$

To decompose the indirect effect by mediator, we estimate the impact of riders on

Figure A24: Event Study with Mediation Analysis: Crime Outcomes



Notes: Mediation analysis using the staggered DiD estimator of [Gardner et al. \(2026\)](#). The figure displays the total effect of platform entry, the direct effect, and the indirect effect driven by the increase in the rate of delivery workers and restaurants per capita. Only post-treatment estimates are shown. Logged annual outcomes at the jurisdiction level for individuals aged 15-54. Bootstrapped standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.

each mediator M_{zt}^k separately:

$$M_{zt}^k = \alpha_z + \lambda_t + \omega^k \log(\text{Riders}_{zt}) + \nu_{zt}, \quad (9)$$

where $\log(\text{Riders}_{zt})$ is again instrumented by the entry indicators. The mediator-specific indirect contribution is then $\hat{\beta}_k \times \hat{\omega}^k$, where $\hat{\omega}^k$ is the coefficient from Equation (9) and $\hat{\beta}_k$ is the coefficient on $\log(M^k)$ from Equation (7). Standard errors for the total, direct, and indirect effects are obtained from a bootstrap clustered at the police-jurisdiction level.

Table A10 reports the total, direct, and indirect effects of riders on crime, along with the mediator-specific contributions. For all crime categories except drug usage and resale, the total indirect effect operating through the labor market is negative and statistically significant, accounting for 11% to 20% of the total effect. The largest contributions primarily come from male unemployment and male immigrant inactivity, consistent with the findings from the main difference-in-differences estimates. These results provide evidence for a dual mechanism through which gig economy employment affects crime: a self-

incapacitation channel, whereby time spent working reduces the opportunity to engage in criminal activity, and an economic deterrence channel, whereby improved labor-market prospects raise the opportunity cost of crime.

Table A10: Causal Mediation Analysis - Indirect Effect of the Labor Market on Crimes

	All Crimes	Low-skill Property Crimes	Violence against the Person	Drug Use and Resale	Vandalism and Destruction
Total	-0.020* (0.012)	-0.081*** (0.014)	-0.066*** (0.008)	-0.085*** (0.021)	-0.124*** (0.017)
Direct	-0.016 (0.011)	-0.064*** (0.014)	-0.058*** (0.008)	-0.084*** (0.022)	-0.110*** (0.016)
Indirect	-0.003** (0.001)	-0.016*** (0.004)	-0.008*** (0.002)	-0.000 (0.004)	-0.014*** (0.003)
Mediated (%)	17.45	20.33	12.02	0.59	11.14
Inactivity (%)	5.30	2.61	1.22	-1.35	3.48
Migrant Inactivity (%)	6.04	8.79	3.71	-3.61	4.80
Unemployment (%)	5.73	7.43	5.86	5.44	7.23
Migrant Unemployment (%)	0.38	1.50	1.23	0.11	-4.39

Notes: The rider stock is instrumented with event-time indicators for years 0 to 4 after platform entry. All specifications include police-jurisdiction and year fixed effects. Total is the IV coefficient on riders from Eq. (6). Direct is the coefficient after adding the post-entry labor-market outcomes in Eq. (7). Indirect equals Total minus Direct. Mediated (%) reports $100 \times \text{Indirect}/\text{Total}$. Each remaining percentage row reports $100 \times (\hat{\beta}_k \hat{\omega}^k)/\text{Total}$ for the indicated labor-market outcome; these rows sum to Mediated (%). These quantities form an IV-based regression decomposition, not causal direct and indirect effects. Bootstrap standard errors in parentheses are clustered by police jurisdiction. Significance levels: * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

This mediation analysis is best read as evidence consistent with the *presence and direction* of the economic deterrence and self-incapacitation channels, rather than as a complete causal accounting of their relative contributions.

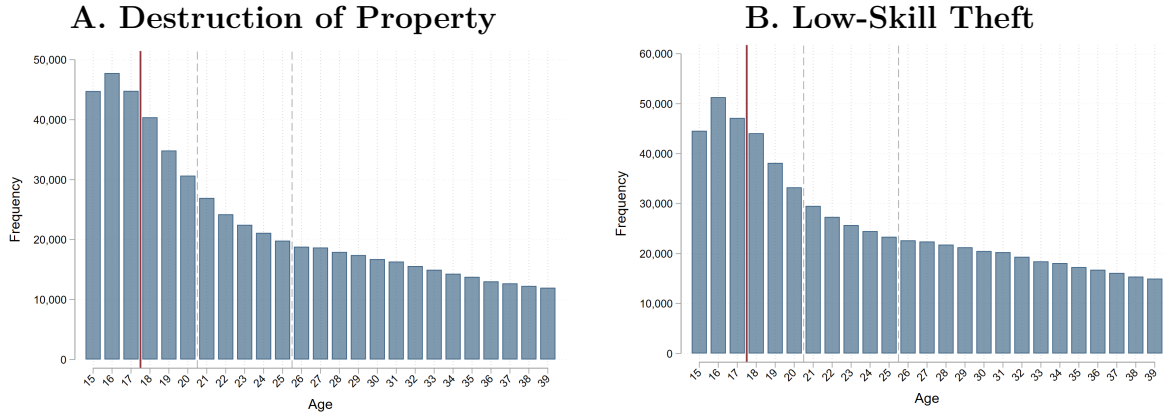
T Age Profiles of Crime and Alternative Adult Age Windows

This appendix supports the age-based eligibility test in Section 7.1. It first documents the age profiles of the three court-recorded offense categories, which motivate both the comparison of minors with young adults around the age-18 threshold and the emphasis on destruction of property and low-skill theft as quintessential youth crimes. It then shows that the adult crime declines are not specific to the 18-25 window used in the main text, using both the narrowest window just above the threshold (18-20) and the widest (18-39).

T.1 Offender Age Distributions

Figure A25 plots the age distribution of offenders for the two categories used in the age-based test. Destruction of property and low-skill theft are heavily concentrated among teenagers and young adults: offending peaks at age 16, remains high on both sides of the age-18 threshold, and falls by about a third between ages 18 and 21. These profiles imply that minors provide an informative comparison group precisely where the offenses most responsive to platform entry are concentrated. They also show that ages 15-17 are the highest-density segment of the age-crime profile: the three-year window just above the threshold (18-20) contains about four-fifths as many offenses (77% for destruction of property and 81% for low-skill theft), while the 18-25 window used in Figure 7 contains about 1.6-1.7 times as many.

Figure A25: Crime Age Distribution



Notes: Age distribution of offenders, men and women combined, for destruction of property (A) and low-skill theft (B). Court records (*Cassiopée*), 2012-2019 pooled; age is measured at the time of the offense. The x-axis shows age in years (15 to 39, the widest adult window used in the analysis), and the y-axis shows the number of offenses. The solid red line marks the legal age-18 threshold for platform work, separating ages 15-17 from 18 and above. The dashed gray lines mark the upper bounds of the 18-20 and 18-25 adult windows used in Figure 7 and Appendix Figure A26.

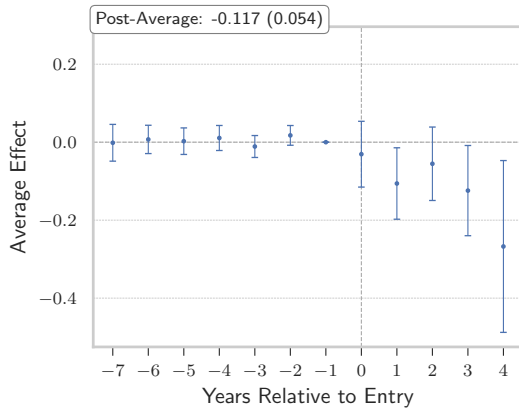
T.2 Alternative Adult Age Windows

The age-based eligibility test in Section 7.1 compares minors (15-17) with young adults aged 18-25, the age range in which most riders work. This subsection shows that the adult crime declines do not depend on this choice. Figure A26 re-estimates the event studies for the narrowest window just above the legal threshold, ages 18-20, and for the

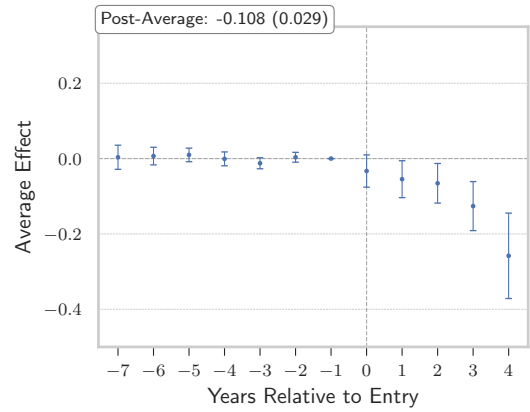
widest window we consider, ages 18-39. The post-entry declines are remarkably stable across windows: post-average estimates are -11.0% (18-20), -8.2% (18-25), and -10.2% (18-39) for destruction of property, and -10.1% , -8.6% , and -11.6% for low-skill theft, with estimates for all intermediate upper bounds (18-27 through 18-37) in the same range. The 18-20 estimates are less precise because this three-year window contains only about four-fifths as many offenses as the minors' window (Figure A25). The stability of the proportional declines as older cohorts are added implies that offenders across the adult age range below 40 respond to platform entry, consistent with the age profile of riders documented in Section 4.1, and confirms that the choice of adult comparison window does not drive the eligibility results.

Figure A26: Adult Crime Effects for Alternative Age Windows

A. Property Destruction (Aged 18-20) B. Property Destruction (Aged 18-39)

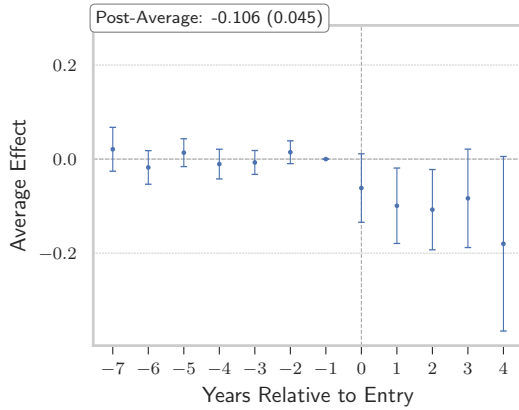


Baseline: 4.3; Post-entry average: -11.0%



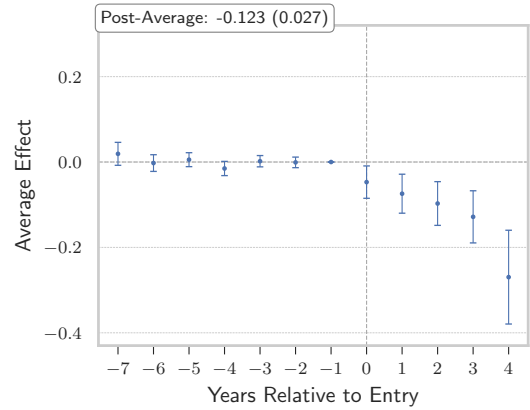
Baseline: 16.5; Post-entry average: -10.2%

C. Low-skill Theft (Aged 18-20)



Baseline: 4.9; Post-entry average: -10.1%

D. Low-skill Theft (Aged 18-39)



Baseline: 20.9; Post-entry average: -11.6%

Notes: Event-study estimates using [Gardner et al. \(2026\)](#), complementing the 18-25 window in Figure 7. Platform entry: first year a food-delivery platform operates within a police jurisdiction or its 15-km buffer. Outcomes are the inverse hyperbolic sine of annual court-recorded offenses committed by adults aged 18-20 (left column) and 18-39 (right column) per 10,000 residents aged 15-54, at the jurisdiction level. Baselines are 2014 means of these rates. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals. Estimates for intermediate upper age bounds (18-27 through 18-37) are similar and available on request.

U Criminal-Record Data and Eligibility Test

This appendix describes the data used for the criminal-record eligibility test in Section 7.2.

U.1 Data Source and Sample

We use the statistical file of the national criminal record (*fichier statistique du Casier judiciaire national*), produced by the statistical service of the French Ministry of Justice and accessed through the CASD secure environment. The file records every conviction pronounced by French courts. We use the annual event files from 2012 to 2019, in which each observation is a conviction and reports the offender’s characteristics, the principal offense, and the principal sentence. We keep convictions for misdemeanors and crimes (*délits* and *crimes*) pronounced in mainland France by first-instance courts, excluding appeal and assize courts, and we drop records that the data producer adds to estimate incomplete collection. The resulting file contains about 4.5 million convictions. Our analysis uses the 4,205,881 convictions of adults pronounced by correctional courts, which involve 2.57 million distinct individuals, all assigned to one of 160 court areas. French courts assign cases based on the offender’s age at the time of the offense, and offenses committed before age 18 are tried by juvenile courts, so every conviction in the sample concerns an offense committed at age 18 or older and all offenders meet the platforms’ age requirement. Men account for 89% of adult convictions and foreign-born offenders for 20%; the median age at conviction is 31.

U.2 Conviction Histories

Each record carries a pseudonymized person identifier (*numéro de dossier*), which corresponds to an individual’s file in the national criminal record and is stable across annual files. For 98.8% of identifiers that appear more than once, the associated date of birth is constant. This pattern supports the interpretation that the identifier follows the same person across annual files. The identifier allows us to reconstruct each offender’s sequence of convictions over 2012-2019. Among the 2.57 million adult offenders, 30% appear more than once in this window, with 1.6 convictions per person on average. We date each conviction by the date of the most recent offense it covers rather than by the judgment date, because court delays would otherwise blur the timing of platform entry.

We define a conviction as a reoffense when the offender has a previous conviction in the data and the new offense was committed after that previous judgment. This definition excludes new judgments on the same facts, such as retrials after judgments in absentia. As a validation, 41% of adult convictions concern an offender with a prior conviction observed since 2012, close to the 42% of people convicted in 2020 who had a prior conviction according to Ministry of Justice statistics.⁶¹ Because histories start in 2012, prior convictions pronounced before 2012 are not observed. This left-censoring is common to all areas in a given year and is absorbed by year fixed effects, but it implies that some offenders with older convictions are classified as having no prior record.

We classify an offender as ineligible for platform work if any earlier conviction was for a crime-level offense or carried a prison sentence, suspended or not, as its principal

⁶¹Ministry of Justice, *Références statistiques justice, année 2020*, Section 7.7. The Ministry counts people rather than convictions and uses complete conviction histories. See [Ministry of Justice](#), last accessed September 2026.

sentence. We classify as eligible offenders whose earlier sentences were all non-custodial, such as fines or community service. Of the 1,706,592 adult convictions of offenders with a prior conviction, 1,133,592 (66%) concern offenders classified as ineligible and 573,000 (34%) offenders classified as eligible. This classification is a proxy for the convictions recorded on the B3 criminal-record extract, which food-delivery platforms require to be clean (see footnote 19). B3 status is not recorded in the microdata. So we reconstruct this status from each offender’s conviction history, and this reconstruction is necessarily rough. It is broader than the legal definition because it treats suspended prison sentences as disqualifying. It is permanent, whereas convictions are removed from the criminal record after statutory rehabilitation periods. It also misses convictions pronounced before 2012. These limitations blur the distinction between the two groups and attenuate the estimated difference in their reoffending responses.

U.3 Geography and Treatment

The two judicial sources used in this paper geolocate offenses differently. The court records of Section 7.1 come from *Cassiopée*, the case-management system of the courts, which registers each case with the municipality where the offense was committed (Appendix H.2.2). We aggregate them to the 681 police jurisdictions, exactly like the police data. The criminal-record file serves a different purpose: it is a register of persons, kept to certify each individual’s convictions. It does not report where offenses took place or where offenders live. The only geographic information it contains is the court that pronounced the conviction.

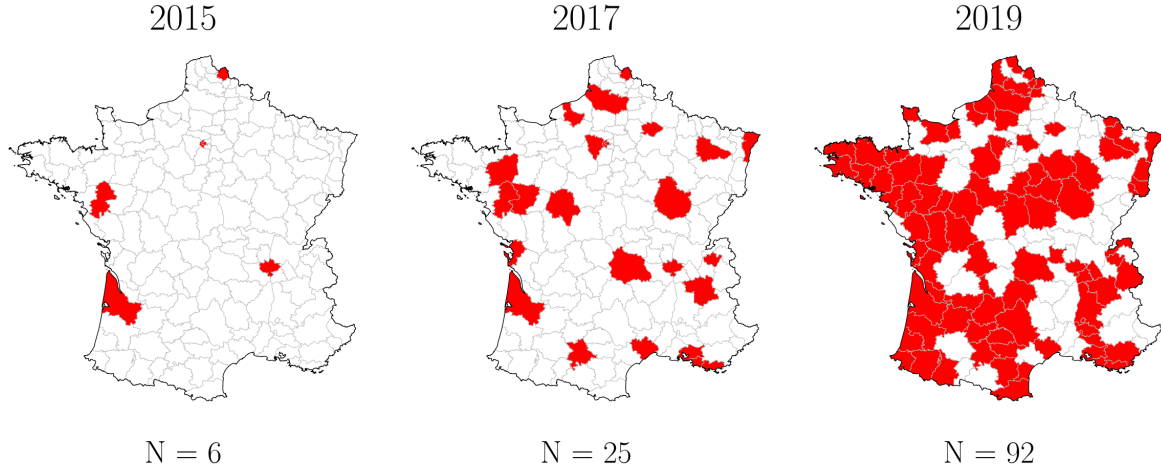
We identify each court by its municipality and map it to the catchment area (*ressort*) of its first-instance court (*tribunal judiciaire*) using the official correspondence between municipalities and courts published by the Ministry of Justice. This mapping assigns detached court chambers to their parent court and yields 160 court areas. Each court area retains a local link to the conviction. French criminal procedure gives jurisdiction to the court of the place where the offense was committed, where the defendant resides, or where the defendant was arrested (Article 382 of the Code of Criminal Procedure). We define platform entry in a court area as the first year in which a food-delivery platform operated in the city where the court sits. Court seats are usually the largest city of their area, so this date typically coincides with the first platform entry in the area.

Figure A27 shows the resulting treatment rollout, in the same format as the rollout across police jurisdictions in Figure 1. Platforms operated in 6 court areas in 2015, 25 in 2017, and 92 in 2019, compared with 11, 39, and 233 of the 681 police jurisdictions. Court areas are much larger than police jurisdictions. Once platforms enter the seat city, the whole area is treated, including municipalities that platforms did not reach. We therefore interpret the resulting estimates as conservative.

U.4 Estimation and Additional Results

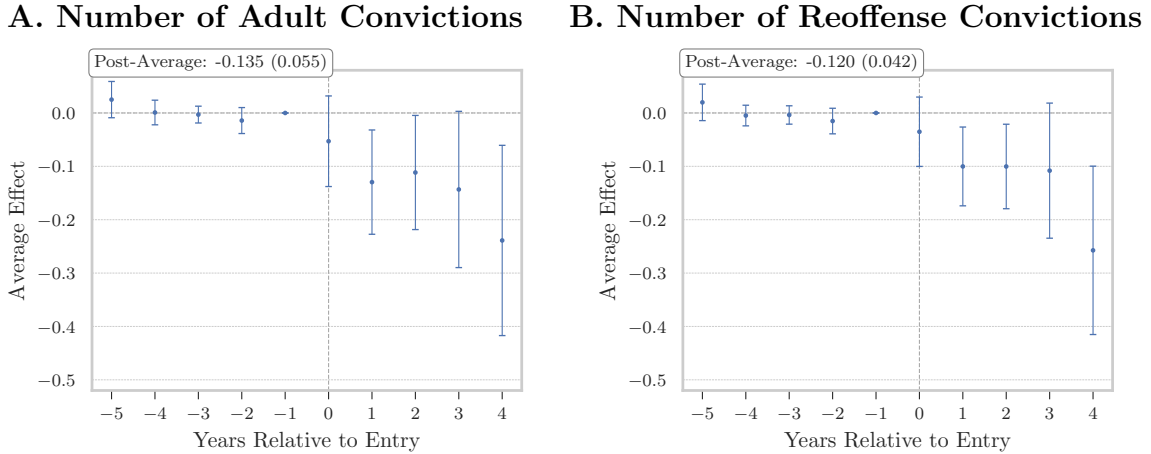
We estimate event studies using the staggered difference-in-differences estimator of Gardner et al. (2026), including court-area and year fixed effects, event time binned at -5 and $+4$, year -1 as the reference period, and standard errors clustered at the court-area level. Outcomes are the logarithm of conviction counts per court area and year. Figure A28 reports the results for the number of adult convictions and the number of reoffense convictions.

Figure A27: Platform Rollout by Year and Court Area, 2015-2019



Notes: Catchment areas of the first-instance courts (*tribunaux judiciaires*) of mainland France (Corsica not shown), the geographic unit of the criminal-record eligibility test (Section 7.2). Grey lines show court-area boundaries, and the black line shows the national border. Red areas are treated: a food-delivery platform operates in the city where the court sits by the year shown. N is the number of treated court areas. Boundaries are built from the official 2026 correspondence between municipalities and courts published by the Ministry of Justice. Municipalities absent from it because of later mergers are assigned to the nearest court area. Compare with the rollout across the 681 police jurisdictions in Figure 1.

Figure A28: Convictions and Reoffending after Platform Entry: Criminal-Record Data



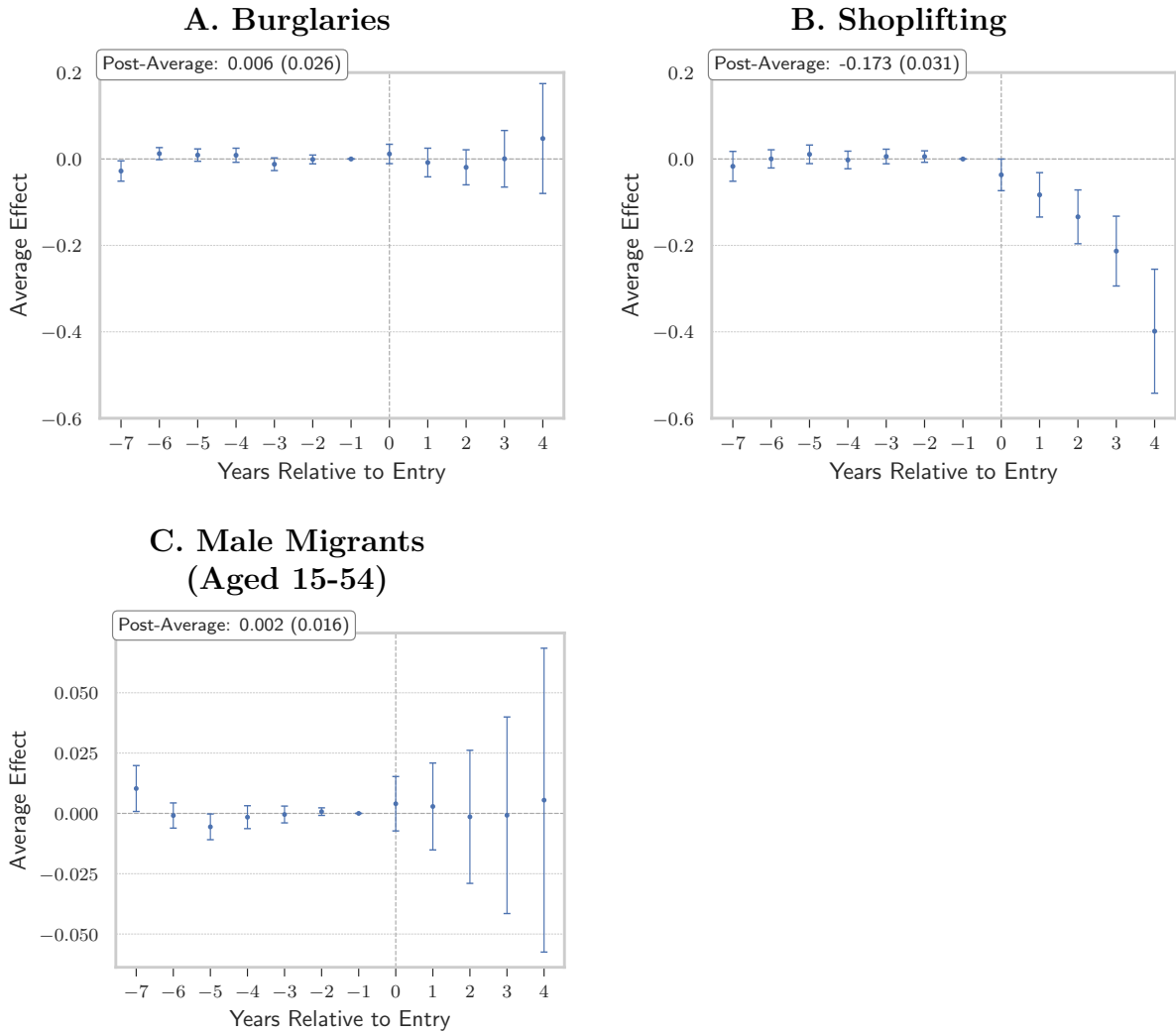
Baseline: 3,232; Post-entry average: -12.7%

Baseline: 795; Post-entry average: -11.3%

Notes: Event-study estimates using [Gardner et al. \(2026\)](#). Data: statistical file of the national criminal record, 2012-2019, adult convictions. Unit of observation: 160 court areas by year of offense. Platform entry: first year a food-delivery platform operates in the city where the court sits. Outcomes are the log number of convictions per court area: all adult convictions (Panel A) and convictions for offenses committed after a previous conviction (Panel B). Baselines are 2014 mean counts per court area. Event time is binned at -5 and $+4$, with year -1 as the reference. Standard errors are clustered at the court-area level. Bars show 95% confidence intervals. Joint tests that all pre-entry coefficients are zero: $p = 0.53$ (Panel A) and $p = 0.63$ (Panel B).

V Movement as Confounder

Figure A29: Event-Study Evidence on Movement-Related Confounders



Notes: Event-study estimates from the [Gardner et al. \(2026\)](#) staggered DiD estimator. Platform entry: first year a food-delivery platform operates within a police jurisdiction or its 15-km buffer. Outcomes are logged annual jurisdiction-level measures for individuals aged 15-54. Panel C reports the log population of male migrants, testing for selective residential sorting following platform entry. The vertical dashed line marks event time 0. Post-entry averages equal $(\exp(\widehat{\text{Post-Average}}) - 1) \times 100$ across all post-entry years. Standard errors are clustered at the police jurisdiction level. Bars show 95% confidence intervals.