Pontifícia Universidade Católica do Rio de Janeiro

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Gig Economy and Informality: Evidence from Brazil

Dissertação de Mestrado

Masters dissertation presented to the Programa de Pós–graduação em Economia, do Departamento de Economia da PUC-Rio in partial fulfillment of the requirements for the degree of Mestre em Economia.

Advisor : Prof. Gustavo Gonzaga
Co-advisor: Prof. Ursula Mello

Rio de Janeiro
April 2024

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Abstract

Campos, Bruno Daré Riotto Malta; Gonzaga, Gustavo (Advisor); Mello, Ursula (Co-Advisor). **Gig Economy and Informality: Evidence from Brazil**. Rio de Janeiro, 2024. 38p. Dissertação de Mestrado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

About one out of every fifty workers in the labor force worked in the gig economy, in 2019 within capital cities in Brazil. The gig sector allows workers to flexibly enter or exit the market and to freely choose how many hours to supply. This paper answers whether gig work benefits workers by serving as an insurance, or a buffer, against unemployment (by absorbing those who are non-employed while they cannot find traditional work) in a context of large informality. Longitudinal quarterly household survey data from 2012 to 2019 allows me to map individual transitions between gig categories and other labor statuses. To answer my question, I run an event-study that takes advantage of the staggered entry of a major ride-sharing company (Uber) across municipalities on different individual labor market statuses. I estimate that the gig economy increases one's probability of becoming a gig worker but it does not increase one's probability of leaving non-employment. This differs from what Jackson (2022) finds for the United States, arguably because, for Brazil, the informal sector alone provided the type of flexibility that buffered those out of work into informal work. I show that most people who relocate into the gig economy were previously skilled blue-collar workers, both formal and informal, but there is also a share of out of the labor force people absorbed into gig work. I find no effect of gig availability on wages. A municipality-level regression allows me to uncover longer-term impacts of the gig economy and estimates suggest that gig work may crowd-out people from formal employment in the long-run. When a settled informal sector exists, the unemployment buffer effect of the gig economy is limited.

Keywords

Labor Market; Gig Economy; Informality; Self-Employment; Brazil.

Resumo

Campos, Bruno Daré Riotto Malta; Gonzaga, Gustavo; Mello, Ursula. **Economia Gig e Informalidade: Evidência do Brasil**. Rio de Janeiro, 2024. 38p. Dissertação de Mestrado – Departamento de Economia, Pontifícia Universidade Católica do Rio de Janeiro.

Um de cada cinquenta trabalhadores na força de trabalho atuou na economia gig, em 2019 nas capitais do Brasil. Este setor gig permite que trabalhadores entrem ou saiam do mercado com flexibilidade e escolham livremente quantas horas ofertar. Este artigo responde se o trabalho gig beneficia trabalhadores ao servir como um seguro, ou um colchão, contra o desemprego (por absorver pessoas que estão sem emprego enquanto elas não encontram trabalhos tradicionais) num contexto de grande informalidade. Dados trimestrais longitudinais de pesquisas domiciliares de 2012 a 2019 me permitem mapear transições entre trabalhos gig para outros status no mercado de trabalho. Para responder minha pergunta, eu conduzo um estudo de evento que aproveita a entrada escalonada de uma grande empresa de transporte por aplicativo (Uber) em diferentes municípios em diferentes status individuais no mercado de trabalho. Estimo que a economia gig aumenta a probabilidade de uma pessoa se tornar um motorista de aplicativo mas não aumenta de deixar o não-emprego. Isso difere do que Jackson (2022) encontra para os Estados Unidos, possivelmente porque, no Brasil, o setor informal sozinho já fornecia o tipo de flexibilidade que absorvia aqueles sem emprego para trabalhos informais. Mostro que a maioria das pessoas que entram para a economia gig eram trabalhadores qualificados de colarinho azul, tanto formais quanto informais, enquanto há uma parcela de pessoas fora da força de trabalho que são absorvidas para trabalhos gig. Não encontro efeitos da disponibilidade de trabalho gig sobre salários. Regressões a nível municipal permitem estimar impactos de longo prazo da economia gig e as estimativas sugerem que o trabalho gig pode afastar pessoas do emprego formal a longo prazo. Quando existe um setor informal estabelecido, o efeito colchão contra o desemprego da economia gig é limitado.

Palavras-chave

Mercado de Trabalho; Economia Gig; Informalidade; Conta-própria; Brasil.

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1
Introduction

"Gig work" surged into widespread popularity over the last decade. In Brazil, the number of workers notably increased in the ride-sharing and food-delivery markets, together amounting to about half a million workers in 2019 just within capital cities. These gig labor sectors are particular in that workers and consumers are matched through an online platform (the firm). The workers may also flexibly choose how many hours to supply (at the intensive margin) and may easily enter or exit the market (extensive margin).

The distinct feature of flexibility may benefit workers by absorbing those who lost a job while they cannot find traditional work, shortening unemployment spells, and providing an insurance (or a buffer) against unemployment. Jackson (2022) shows, with administrative tax data from the United States, that the availability of gig work increased earnings in the short-run but crowded-out traditional employment in the long-run. However, these findings may not be generalizable to an economy with a large informal sector, because the informal sector itself may function already as an unemployment buffer in developing economies (Dix-Carneiro & Kovak (2019), Dix-Carneiro et al. (2021), Ponczek & Ulyssea (2022)).

Does the gig economy increase people's chances of working, even in the presence of an informal sector? I answer this question by exploiting the staggered entry of a major ride-sharing company (namely, Uber) across municipalities in Brazil and by focusing on drivers. Brazilian quarterly survey data uncovers labor and socioeconomic characteristics of workers and allows me to observe individual transitions between formality, informality, self-employment, gig employment and non-employment. Brazil is a good setting to address this question not only due to data availability of formal and informal workers, but also because it is a developing economy with a particularly large share of self-employment and gig work.

My main empirical strategy consists of an event-study design that accommodates dynamic effects over time on outcomes that denote different labor market statuses, hence estimating the effect of the gig economy on the probability of having a particular status. I also report average differences-in-differences estimates, linear combinations of the dynamic event-study coefficients. Potential biases might arise from OLS estimates on a two-way fixed effects setting when effects are heterogeneous or dynamic, and this is dealt with by implementing the strategy laid out in Sun & Abraham (2021).

I find that gig availability increases one's probability of becoming a gig worker by 0.04-0.13 percentage points but it does not, however, increase one's probability of getting any job (that is, of being out of non-employment). I argue that this is the case in a context of informality because there is no additional buffering introduced by the gig economy. Furthermore, I show evidence that workers are substituting traditional informal work for gig work, meaning they would otherwise find informal work in the absence of a gig economy, rather than simply staying out of employment.

Furthermore, I investigate who are those who transition into being gig workers. This is done by conditioning the probabilities of being a gig worker on previous statuses they held before the introduction of a gig economy at their municipality. That is, I estimate the effect of the gig economy on the probability of transitioning, for example, from unemployment into gig work, taking advantage of the panel quality of the data. I find that those who become gig workers were mostly working a quarter before (formally and informally), with a substantial share also coming from out of the labor force. From those working, they were primarily skilled blue-collar workers. They also mostly had completed high school.

Moreover, I employ event-study strategies to asses how wages were affected by the introduction of the gig economy but do not find statistically significant effects, suggesting that employment is the relevant margin of adjustment through which the gig economy affects markets. Finally, by structuring my data in a municipality-level fashion to assess longer-term impacts of the gig economy, event-study estimates show that gig work might crowd-out formal employment in the long-run, consistent with what Jackson (2022) found.

This paper relates to a large literature on the implications informality has on development (see Porta & Shleifer (2014) and Ulyssea (2020) for a review). Informality has been linked to lower output, productivity and welfare (McKinsey (1998), Ulyssea (2018)) but recent research has highlighted that informality can buffer unemployment, as mentioned above. Another positive byproduct of informality, as shown by Gerard & Gonzaga (2021), is that it may reduce the efficiency cost of social programs by allowing people to work (informally) while they receive unemployment insurance. Donovan, Lu & Schoellman (2023) documents that workers' transitions from and to informality and self-employment accounts for higher labor market flows in developing economies.

There is also a growing stream of papers that aims to understand alternative work arrangements and self-employment (Katz & Krueger (2017), Mas & Pallais (2017), Mas & Pallais (2020), Boeri et al. (2020), Narita (2020)). Recent papers have investigated how ride-sharing services from the gig economy has impacted taxi drivers' earnings, but conclusions are mixed — Chang (2017) and Berger, Chen & Frey (2018) find that the introduction of a ride-sharing company reduced taxi drivers' earnings in Taiwan and United States, while Oliveira & Machado (2021) find no such negative impact for taxi drivers in Brazil. Adermon & Hensvik (2022) conducts an experiment in Sweden and find that gig-experience is more valuable to firms than unemployment, but less useful than traditional employment. Garin et al. (2023) documents the rise of the gig economy in the United States.

To the best of my knowledge, this is the first paper to investigate the interaction between the gig economy and employment in a context of informality, with data on informal workers. My main contribution to the literature is to add that the buffer mechanism found by Jackson (2022) does not hold in an economy with informality, plausibly because the informal sector served as a buffer even in the absence of the gig economy.

The remainder of this paper is structured as follows. Chapter 2 describes the data and the institutional setting. Chapter 3 presents stylized facts about the gig economy in Brazil. Chapter 4 characterizes the quasi-experiment setting and explains the identification strategy. Chapter 5 presents the results. Chapter 6 concludes.

2
Data and Institutional Background

This paper uses longitudinal quarterly household survey data from *Pesquisa Nacional por Amostra de Domicílios Contínua*[1] spanning from 2012 to 2019. The data is structured as a rotating panel, such that households are surveyed for five consecutive quarters[2], with about 100,000 households interviewed per quarter. The resulting dataset excludes individuals younger than 14 in at least one of the interviews. Moreover, I exclude individuals who lived outside capitals, both because this allows for my identification strategy (this will be clarified later) and because the gig phenomena is largely metropolitan — in fact, 538,094 out of the 1,384,317 gig workers lived in capitals (about 40%), as of June 2019.

Brazil has two popular gig economy sectors — (I) food-, package- or merchandise-delivery and (II) ride-sharing services. Following Goés, Firmino & Martins (2022), the gig economy is measured by mapping workers into gig categories by combining their reports on job activity (*passenger transportation*, *cargo transportation* and *courier and delivery activities*) and their reports on job occupation (*motorcycle conductors*, *automobile, taxi and truck conductors* and *conductors of vehicles powered by pedal or arms*). Table 2.1 displays details on how this is accomplished. The data does not allow me to distinguish between taxi drivers and app drivers since they would both report equal occupation and activity, hence this is not a perfect measure of the gig economy in that not every driver is working through an online platform. However, throughout the paper I show evidence that the results are driven by app workers, that is, by gig workers.

Table 2.1: Gig Workers

		Occupation		
		Motorcycle Conductors (8321)	Automobile, Taxi and Truck Conductors (8322)	Conductors of Vehicles Powered by Pedal or Arms (9331)
Activity	Passenger Transportation (49030)	*Motorcycle Driver*	*Driver*	
	Cargo Transportation (49040)	*Delivery*		*Delivery*
	Courier and Delivery Activities (53002)	*Delivery*	*Delivery*	*Delivery*

Notes: This table is reproduced from Goés, Firmino & Martins (2022). The corresponding *Classificação Brasileira de Ocupações* (CBO) and *Classificação Nacional de Atividades Econômicas* (CNAE) codes are displayed in parentheses.

Gig workers then are either app/taxi drivers, food/package-delivery workers or motorcycle drivers. The remaining population, outside gig work, is

[1] PNAD-Contínua hereon.

[2] Individual identifiers across time are not made publicly available by *Instituto Brasileiro de Geografia e Estatistica* and, to overcome this obstacle, Ribas & Soares (2008) developed a methodology that combines household identifiers across time with dates of birth and gender to recover the panel quality of the data.

categorized into one of seven different labor statuses: (I) formal employment, (II) informal employment, (III) non-gig self-employment, (IV) employer, (V) other employment (which includes military and public sector workers, for example), (VI) unemployment and (VII) out of the labor force. Because of the nature of my empirical strategy, the analysis is restricted to individuals within capital cities and is concentrated on the drivers' share of the gig economy.

Those informally employed by a firm or self-employed, both without an official work permit and hence uncovered by Brazilian labor legislation[3], are commonly defined as informal workers, as in Meghir, Narita & Robin (2015). Solo self-employed workers may choose to partially formalize their statuses by registering as a firm (as an individual microentrepreneur, under the *Microempreendedor Individual* modality) by paying a monthly amount of 5% of the minimum wage[4]. This allows self-employed workers to have social security coverage. Nevertheless, this paper also regards as informal those self-employed who choose to register as a firm, since they are not subject to labor legislation and hold no formal employer-employee contract, thus they do not enjoy the same benefits that formal workers do, such as paid rest or unemployment insurance.

Founded in 2010, the ride-sharing company Uber arrived in Brazil through Rio de Janeiro during the 2014 FIFA World Cup, followed then by the city of São Paulo months later. The company, then, entered in a staggered fashion throughout all remaining 25 capital cities, between 2014 and 2017. Entry dates on capital cities were collected from news articles. Table A.1, in the appendix, provides sources.

(a) Population and Entry

(b) Geographic Rollout

Notes: Log-population is relative to 2014, when the company first began operating in the country. "S1" is meant to represent the first semester, while "S2" represents the second semester of a given year.

Figure 2.1: Uber Rollout

Figure 2.1 shows the geographic distribution of the rollout, making it clear that there is substantial variation in timing across regions. The exact criteria adopted to begin operating at a given city is unknown, but population size was likely an important deciding factor. Figure 2.1a shows that Uber

[3] *Consolidação das Leis do Trabalho.*

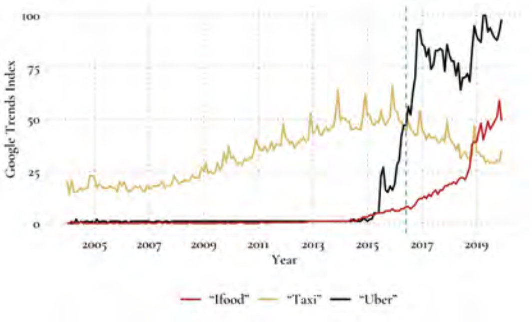
[4] Martins et al. (2023)

arrived earlier at more populous capital cities. This alone is handled by including fixed effects in a regression.

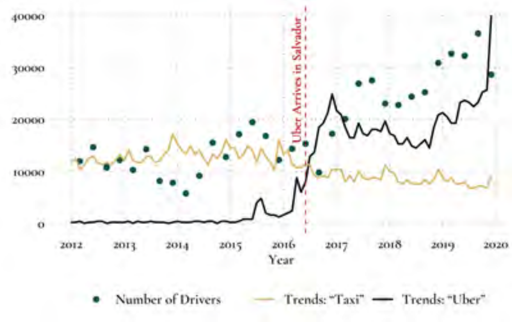
During the first years of operation, Uber was under controversy regarding its legal status in Brazil and it was even criticized by the then president Dilma Roussef. Taxi drivers protested against the company's activity, claiming it was unfair or illegal competition, lobbying to either ban or regulate Uber. The backlash grasped policymakers' attention and the discussions reached local governments, which resulted on Uber being temporarily banned from São Paulo, Rio de Janeiro and Brasília in 2015, but the bans were soon overruled, as detailed in Defossez (2017). Brazil's Supreme Federal Court decided that Uber was legal only in 2019, after many years of the company operating normally. The debate surrounding regulation of the company's activity and of the workers' informal status persists to this day.

3
Stylized Facts

Figure 3.1 pictures the growth of popularity of Uber once it started operating in Brazil and, as an illustration, in the city of Salvador. Figure 3.1a shows the popularity of search, on Google, for the words “Taxi”, “Uber” and “Ifood” (a well-known food-delivery company) over the last years and for the entire country. The ride-sharing app rapidly became popular relative to the standard alternative, taxis, but also relative to the food-delivery market, whose popularity arrived, and to a lesser extent, years later. Figure 3.1b displays, for the capital city of Salvador in the state of Bahia, how Uber’s popularity[1] increased precisely after the company arrived in the city and, at the same time, how the number of drivers in Salvador accompanied those trends.



(a) Brazil



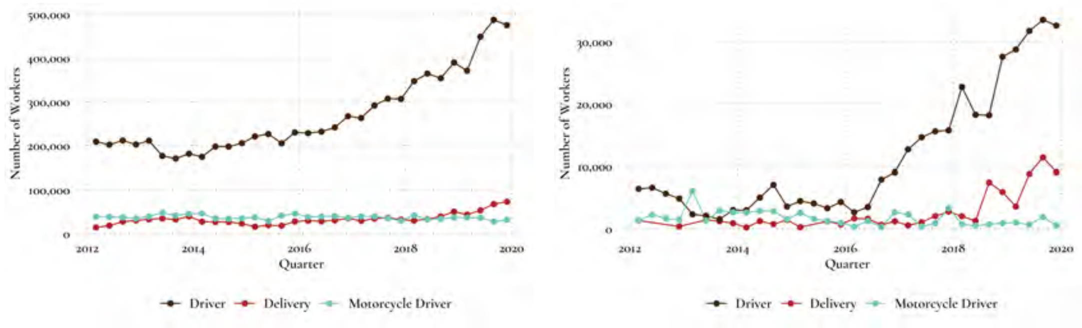
(b) An Example: Salvador (Bahia)

Notes: Google Trends data was obtained from *trends.google.com/trends/explore* and the number of drivers from PNAD-Contínua. The green dashed line indicates when the word “Uber” overcomes “Taxi” in searches, while the red dashed line represents the date when the company arrived in Salvador.

Figure 3.1: Ride-Sharing Popularity

Figure 3.2 exhibits the size of the gig economy (within capitals, as mentioned before). The number of drivers grew significantly from 2016 onwards, more than doubling and peaking at 487,121 people. The same is true for people’s second job and, interestingly, very few were driving as a second job before 2016. Still, not as many workers engage in the gig economy as a side job, suggesting perhaps that the introduction of gig work might not be a major source of additional income to already employed workers. For this reason, everything that follows restricts the analysis to workers’ main job. The number of delivery workers also grew, modestly, amounting to 72,997 people at the end of 2019.

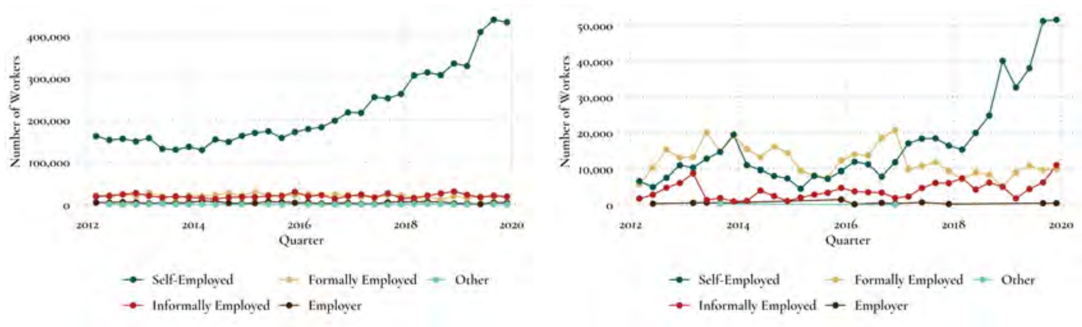
[1]Measured by searches for it on Google, in Bahia.



(a) Main Job (b) Second Job

Notes: The figure shows the number of workers within capitals, by gig economy sector. An individual's main job is the one they normally work the most hours.

Figure 3.2: Gig Economy's Size

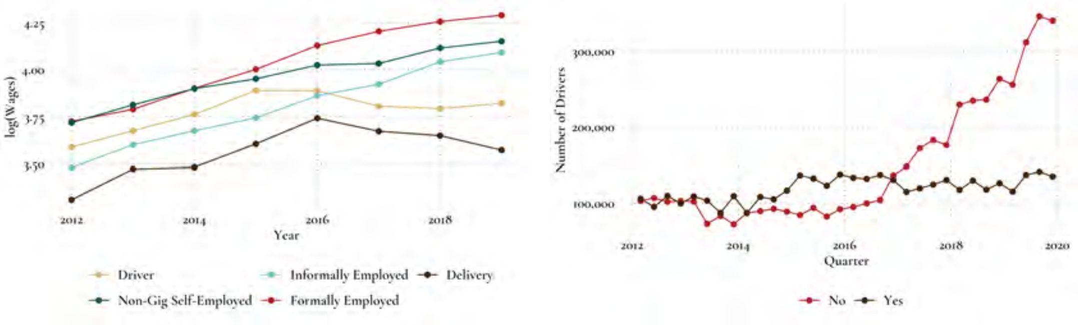


(a) Drivers (b) Delivery Workers

Notes: The figure shows the number of workers (drivers or delivery workers) within capitals, by type of labor relationship.

Figure 3.3: Gig Workers By Type Of Labor Relationship

Figure 3.3a plots drivers by type of labor relationship, showing they are overwhelmingly self-employed, and were even before the gig economy arrived. Figure 3.3b shows this was not the case for the delivery market, with a similar share of self-employed and formally employed workers. From 2018 onwards, formality shrinked while the self-employment expanded in the delivery market, possibly because of delivery apps' popularity, as evidenced by Figure 3.1a.



(a) Wages (b) Social Security

Notes: Log of wages are computed for those who reported positive earnings and hours, which are in nominal rather than real terms. A particular individual (a delivery worker) was removed from the sample because their earnings were likely misreported — they reported earning twenty times more than their colleagues' average, with an implied hourly wage of R$7,000. "Yes" on social security indicates that a driver contributed to a pension institute.

Figure 3.4: *Drivers:* Wages and Social Security Contribution Over Time

Figure 3.4 underscores two important new facts about drivers that emerged also around 2016. First, drivers' implied wages (calculated by taking their reported monthly earnings over their reported weekly hours) lagged behind compared to other workers' wages. Drivers' wages were already below other self-employed workers' wages by the beginning of the decade, and by the end of this period they also had, on average, lower wages than the informally employed. Another evident phenomenon is how the observed growth in the number of drivers, around 2017, is pushed by drivers who are not covered by social security. Figure A.1, in the appendix, shows that the increase on the number of drivers is entirely driven by self-employed workers uncovered by social security.

Table 3.1 describes how drivers and delivery workers are different, on average, from other non-gig workers on socioeconomic and labor characteristics, both before (by the second quarter of 2013) and after (by the second quarter of 2019) the popularization of the gig economy.

Drivers and delivery workers are predominantly male and mostly non-white (before and after the presence of a gig economy). This might appear puzzling because, first, part-time work is linked to female employment (Blau & Kahn (2013)) and, second, for all other informal jobs, female employment is sizeable. McCrate (2005), however, found that women sort into activities with flexible schedules no more than men.

Drivers report working more hours, on average, which might explain why their earnings were mechanically the highest in 2013—Q2, despite their lower wages, compared to the formally employed and the non-gig self-employed. Another possibility is that behavioral biases might arise, when reporting hours, from lack of clarity when distinguishing working hours from transportation time from and to work, for example. Pires (2022) argues that gig workers might experience biased memory. These reported hours and earnings, hence, should be interpreted cautiously.

Also noteworthy is how drivers' social security coverage fell from just

Table 3.1: Descriptive Statistics

Panel A: Socioeconomic Characteristics								
	2013—Q2				2019—Q2			
	Education	Age	Male	White	Education	Age	Male	White
Driver	10.38	45.99	0.93	0.48	11.56	43.24	0.93	0.45
Non-Gig Self-Employed	10.16	43.65	0.58	0.45	11.19	43.75	0.57	0.43
Informally Employed	10.33	35.31	0.44	0.39	11.36	36.63	0.45	0.39
Formally Employed	11.63	35.34	0.55	0.46	12.37	36.98	0.53	0.43
Delivery	11.15	34.01	0.98	0.54	11.05	33.22	0.98	0.41
Panel B: Labor Characteristics								
	2013—Q2				2019—Q2			
	Income	Hours	Insurance	N	Income	Hours	Insurance	N
Driver	1999	49.78	0.58	176,178	2270	49.11	0.30	448,555
Non-Gig Self-Employed	1715	38.92	0.27	4,119,176	2245	36.45	0.28	5,091,928
Informally Employed	1314	35.92	0.20	3,471,442	2099	34.44	0.23	3,736,502
Formally Employed	1839	42.42	1	10,867,315	2925	42.04	1	9,501,407
Delivery	1565	41.99	0.77	34,922	1488	43.63	0.43	53,252

Notes: All reported statistics are averages. Income stands for nominal monthly earnings while hours stands for weekly hours. Insurance corresponds to the share of workers covered by social security. Education stands for years of schooling. The number of workers is represented by N, which is not sample size, but weighted sample size.

under 60% to 30% within those six years, and from 77% to 43% for delivery workers. These numbers remained remarkably stable for the other informal workers, suggesting this decline in contribution is a phenomenon particular to the gig economy.

To understand further who are these new workers who became drivers (roughly a quarter million), Figure 3.5 investigates which was their previous statuses by concentrating on those individuals observed for two consecutive quarters, switching from a given status to driving. I want to learn, in other words, from which labor market status drivers come from, over time. Figure 3.5 plots, for every quarter, the proportion of drivers that had a given labor position three months before. By illustration: 21% of the people who switched into driver during 2019—Q2 were previously unemployed, the quarter before (2019—Q1). Figure A.2, in the appendix, shows drivers' previous labor market occupations.

The most striking phenomenon observed is that the share of drivers that were previously without a job (either unemployed or out of the labor force) grows significantly. By the end of 2019, more than a third of drivers came from non-employment, compared to just 13% by the end of 2012.

That the drivers' category is absorbing more jobless people, around 2016, could suggest that becoming an app driver is easier (or more attractive) to the non-employed, compared to becoming a taxi driver. The flexibility of entry into

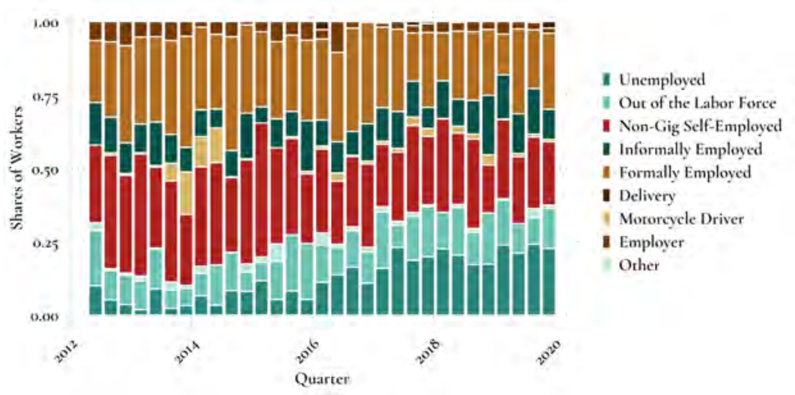


Figure 3.5: Labor Market Statuses Before Becoming a Driver

Notes: For every quarter, for those workers observed to transition into a driver (that is, who were not drivers already), each bar represents what proportion of those drivers came from a given labor market status.

app driving work differs from the taxi business, that requires one to obtain a government license, which is both financially costly and time-consuming, since it requires extensive training to an exam.

4
Empirical Strategy

An ideal experiment would randomize the availability of gig-driving on different labor markets. Without such experiment, one can estimate the causal effect by conducting an event-study taking advantage of the differential timing in treatment, imposing a few identification hypothesis. The relevant assumption is that the treated units would experience parallel trends relative to the non-treated units, in the absence of treatment.

Observations are at the individual- ($\mathbf{i}$), quarter- ($\mathbf{t}$) and capital-level ($\mathbf{c}$), and $\mathbf{q}$ will denote how many quarters an observation is distant from the event (in my case, the arrival of Uber). The sample is restricted to individuals who are identified across at least two different interviews, which enables me to add individual fixed effects in my regressions. Data is trimmed to exclude observations who were more than three quarters distant from the event (hence implying $\mathbf{q} \in$ [-3, 3]) so that an individual who completed all five interviews has witnessed the event at some point.

Controlling for individual fixed effects is important because PNAD-Contínua is a rotating panel of individuals, meaning that the group of individuals interviewed at a given municipality is different every quarter — that is, the sample is balanced with respect to municipalities but unbalanced with respect to individuals. Hence, including individual fixed effects accounts for how the characteristics of the interviewed sample at each municipality changes over the years. Nevertheless, this event-study is also estimated on a municipality-level sample in a subsection below.

The standard event-study equation I estimate is given by the following:

$$\mathbf{y_{itq}} = \sum_{\mathbf{k \neq -1}} \mathbf{b_k} \cdot \mathbf{1}\{\mathbf{k} = \mathbf{q}\} + \psi_\mathbf{t} + \varphi_\mathbf{i} + \delta \mathbf{x_{it}} + \epsilon_\mathbf{itq}. \tag{4-1}$$

The set of individual ($\varphi_\mathbf{i}$) and quarter ($\psi_\mathbf{t}$) fixed effects are included in all main specifications, while the time-varying controls ($\mathbf{x_{it}}$) represents schooling years and age. Outcomes of interest ($\mathbf{y_{itq}}$) are dummies denoting different labor market status, yielding a natural interpretation to the event-time dummies: coefficients $\mathbf{b_k}$ denote the causal percentage point increase in the probability of having $\mathbf{y_{itq}} = 1$ at a given distance $\mathbf{k}$ from the event[1].

OLS estimates of two-way fixed effects models might be biased in the presence of either heterogeneous or dynamic effects, as recently shown in the literature (Chaisemartin & D'Haultfœuille (2020), Goodman-Bacon (2021), Callaway & Sant'Anna (2021), Sun & Abraham (2021)). I report the OLS results on the appendix and show main results robust to the methodology proposed by Sun & Abraham (2021) (S&A hereon), appropriate to an event-study design. This method uses the last municipalities to be treated as the control group, rather than not-yet-treated municipalities.

[1] Relative to -1, omitted

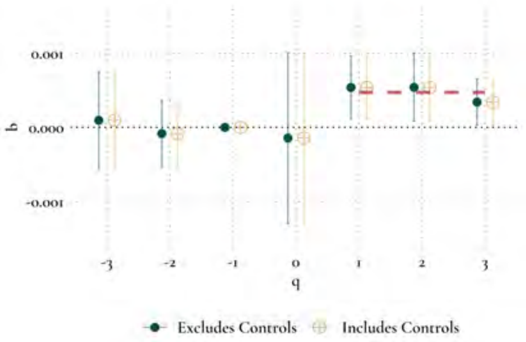
5
Results

5.1
Main Results

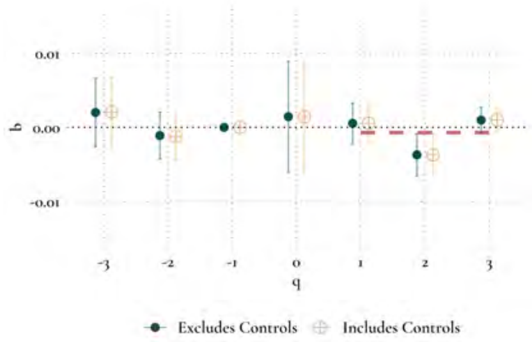
Figure 5.1 plots the event-time dummies' coefficients $\mathbf{b_k}$, estimated via S&A, on four different outcomes of interest. Specifications with and without time-varying controls are displayed and labeled accordingly. Differences-in-differences estimates are reported in red, each calculated as a linear combination (that is, an average) of the coefficients on $\mathbf{b_k}$ over the first, second and third quarters after treatment[1]. Figure A.3 in the appendix plots these same figures but estimated via OLS, and it yields very similar results.

Figure 5.1a shows that the availability of gig-driving increases the probability that someone becomes a driver during the following year by around 0.05 percentage points. This piece of evidence supports that, indeed, becoming an app driver is, in some sense, easier than becoming a traditional taxi driver. For the other outcomes, however, estimates are statistically insignificant without significant pre-trends. That is, although the probability that one is working as a driver increases, the overall probability that one is working remains unchanged.

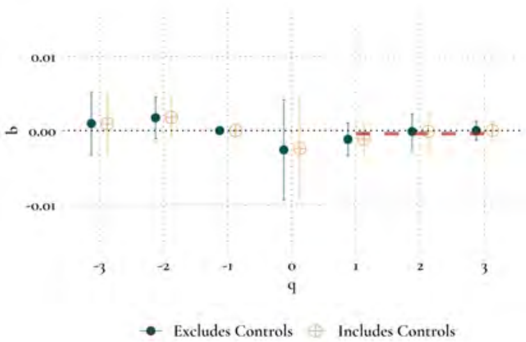
[1]The relative quarter "zero" is not included because there is observed delay in treatment.



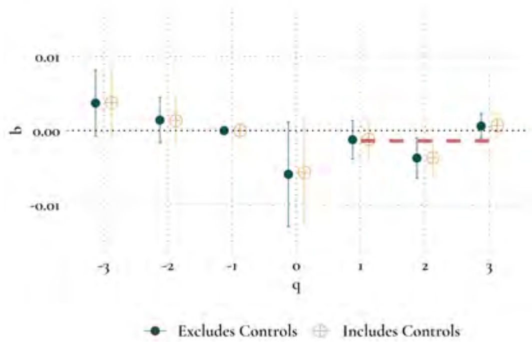
(a) P(*Driver)*



(b) P(*Informal)*



(c) P(*Formally Employed)*



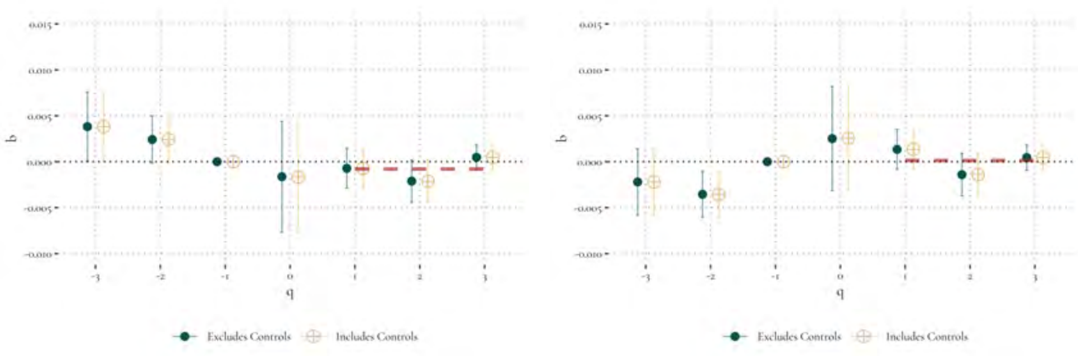
(d) P(*Any Work)*

Notes: Coefficients estimated from Equation 4-1 are plotted, excluding or including age and schooling years as controls for different specifications. These are estimated on a rotating, unbalanced, panel of people that responded to at least two interviews, observed at most three quarters distant from Uber's entry. The status named *Informal* comprises both the informally employed and the non-gig self-employed. The statuses *Formally Employed* and *Informal* excludes drivers, delivery workers and motorcycle drivers. The status named *Any Work* includes any type of work, that is, it includes everyone who is neither unemployed nor out of the labor force. 95% confidence intervals are displayed, estimated with robust standard errors. I report (in red) the differences-in-differences estimates, an aggregation of the dynamic $\mathbf{b_k}$ estimates, with respect to the specification with time-varying controls.

Figure 5.1: S&A Event-Study Estimates

This is different from what Jackson (2022) found, also in the short-run, but in a context of very little informality. My interpretation for why this contrast in results arises is that, when informality plays an important role in the labor market, the introduction of gig work offers no additional buffering mechanism to the non-employed.

What remains to be answered, then, is where these new drivers would otherwise be, in the absence of a gig economy. If the reason why I find no effect on employment is because informality already provides the type of flexibility that buffers those without formal employment, then I should observe a negative effect of the gig economy on some type of informal work. Figure 5.2 plots S&A event-study estimates on two informal statuses, non-gig self-employment and informal employment, thus decomposing the informal sector.



(a) P(*Informally Employed*) (b) P(*Non-Gig Self-Employed*)

Notes: Coefficients estimated from Equation 4-1 are plotted, excluding or including age and schooling years as controls for different specifications. These are estimated on a rotating, unbalanced, panel of people that responded to at least two interviews, observed at most three quarters distant from Uber's entry. 95% confidence intervals are displayed, estimated with robust standard errors. I report (in red) the differences-in-differences estimates in percentage points, an aggregation of the dynamic $\mathbf{b_k}$ estimates, with respect to the specification with time-varying controls.

Figure 5.2: *Informality:* S&A Event-Study Estimates

Figure 5.2a shows estimates that point to the plausible explanation that a share of workers are substituting informal employment for driving when gig work becomes available. Putting it differently, if the gig economy had not arrived, those who become drivers would not be non-employed, but informally employed. This explanation, however, should be interpreted with caution since there are sizable pre-trends and the effects are small and statistically insignificant at 95% confidence intervals.

Table 5.1 displays the summarized differences-in-differences estimates for multiple specifications. I consistently find a positive effect on the probability of becoming a driver but no effect on the probability of getting any work. The size of the point estimates are remarkably smaller for S&A than for OLS, but those two takeaways are robust across specifications. The explanation that drivers would otherwise be informally employed, however, is sensitive to the specification choice.

5.2
Transitions Into Gig Work

This section presents results on transition probabilities rather than statuses probabilities. The standard event-study equation to be estimated is the same as in Equation 4-1 without time-varying controls, but with different outcomes of interest: now $\mathbf{y_{itq}}$ is constructed as the interaction between the driver dummy and a dummy[2] that identifies some given status at -1.

Hence, when interested in the effect of Uber's entry on the probability of an unemployed person to transition into a driver, I generate a dummy that equals one if they are a driver and were unemployed at -1, and zero otherwise.

[2] This requires subsampling to those who were interviewed at -1.

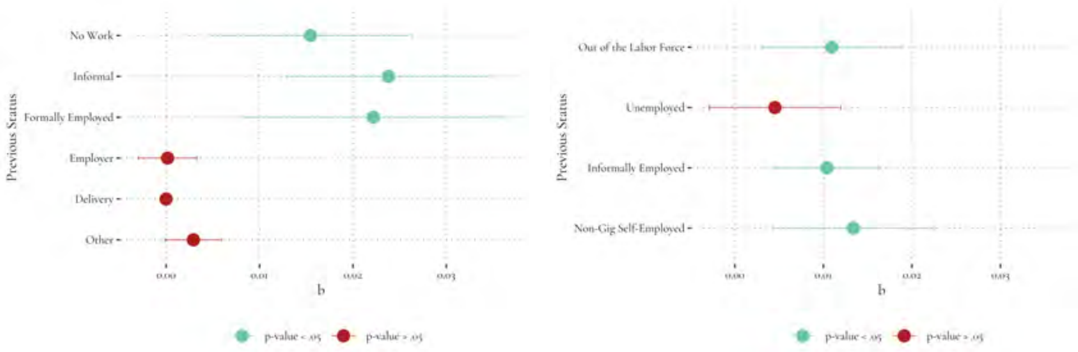
Table 5.1: Differences-In-Differences Estimates

	(I)	(II)	(III)	(IV)
	OLS	OLS	S&A	S&A
Driver	0.13***	0.11**	0.04**	0.04**
	(0.03)	(0.03)	(0.01)	(0.01)
Informal	-0.09	-0.20	-0.07	-0.07
	(0.21)	(0.24)	(0.09)	(0.09)
Formally Employed	-0.26	-0.14	-0.04	-0.04
	(0.18)	(0.21)	(0.07)	(0.07)
Any Work	-0.17	-0.15	-0.14	-0.13
	(0.20)	(0.23)	(0.08)	(0.08)
Informally Employed	-0.38**	-0.40**	-0.07	-0.07
	(0.17)	(0.19)	(0.07)	(0.07)
Non-Gig Self-Employed	0.24	0.17	0.01	0.01
	(0.16)	(0.19)	(0.07)	(0.07)
Quarter FEs	x	x	x	x
Region FEs				
Individual FEs	x	x	x	x
Time-Varying Controls		x		x
Observations	585,619	585,619	492,023	492,023
Sum of Weights ($\times$ 10^3)	221,520	221,520	205,940	205,940

Notes: Differences-in-differences estimates are reported in percentage points, an aggregation of the dynamic $\mathbf{b_k}$ estimates over the first, second and third quarters after treatment. These are estimated on a rotating, unbalanced, panel of people that responded to at least two interviews, observed at most three quarters distant from Uber's entry. Robust standard errors in parentheses. By *regions* I mean capital cities. The number of observations (actual sample size) differs from the sum of weights because PNAD-Contínua is a representative survey, hence the half million observations are meant to be representative of two hundred million. *** p<0.01, ** $p < 0.05$, * $p < 0.1$

.

This compares the number of transitions of the type “unemployed-driver” between the treatment and control municipalities in a diff-in-diff fashion. This is done to multiple distinct statuses at -1, an exercise with the aim to understand who are those who transition into being drivers.



(a) All Statuses (b) Decomposing “Informal” and “No Work”

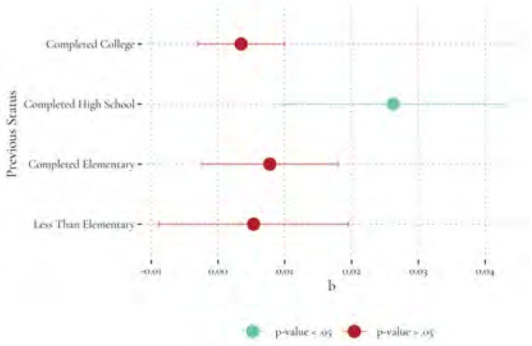
Notes: Average coefficients estimated via S&A from Equation 4-1 are plotted for different transition outcomes, without age and schooling years as controls. These are estimated on a rotating, unbalanced, panel of people that responded to at least two interviews, observed at most three quarters distant from Uber’s entry, but who necessarily responded to an interview at -1. 95% confidence intervals are displayed, estimated with robust standard errors.

Figure 5.3: *Transitions Into Driver:* Previous Statuses

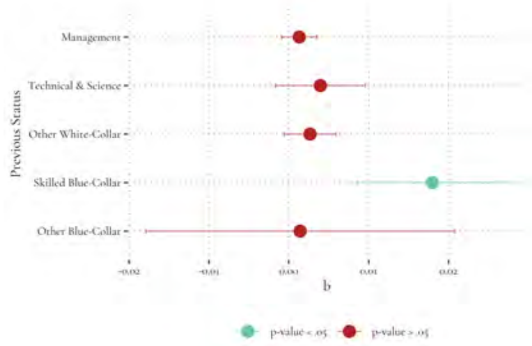
Figure 5.3 displays the average estimates (linear combinations on $\mathbf{b_k}$ over the first, second and third quarters after treatment) of those different previous statuses into driving, employing S&A.

What this reveals is that those who become drivers were just as likely formally working as they were informally working. A smaller, but relevant, number of drivers were previously without work. Decomposing those informal workers and those without work in Figure 5.3b shows that, from the informal ones, the self-employed slice was the one that shifted into driving most, and from those without work, those out of the labor force, interestingly, shifted most. This finding indicates that the flexibility of gig work has brought people from out of the labor force into working.

To further investigate who are those that transition into drivers, I conduct a similar empirical exercise. With this same empirical strategy, instead of conditioning on statuses at -1, Figure 5.4a shows coefficients conditioning on individuals’ education level at -1 and Figure 5.4b conditions, for those who worked at -1, on previous labor market occupations. These results in Figure 5.4 uncover that those who shift into drivers were typically skilled blue-collar type of workers who had high school degrees.



(a) Schooling Instruction Level



(b) Labor Market Occupations

Notes: Average coefficients estimated via S&A from Equation 4-1 are plotted for different transition outcomes, without age and schooling years as controls. These are estimated on a rotating, unbalanced, panel of people that responded to at least two interviews, observed at most three quarters distant from Uber's entry, but who necessarily responded to an interview at -1. 95% confidence intervals are displayed, estimated with robust standard errors. These occupations are aggregations of CBO-Domiciliar categories.

Figure 5.4: *Transitions Into Driver:* Previous Schooling and Occupation

5.3 Wages

An important aspect of this phenomena, other than employment statuses, is wages. This is relevant when assessing whether people are better off in the presence of a gig economy, for example, or in understanding how markets might adjust in this dimension. The goal of this section is to understand how the gig economy changed wages of those who became drivers, of those who were already drivers and stayed drivers and of those who were informal and stayed informal. This is accomplished by constructing three different groups and running an event-study on each of them.

Groups are categorized by their statuses on $\mathbf{q} < \mathbf{0}$, before the arrival of Uber on their municipality, and by their statuses on $\mathbf{q} \geq \mathbf{0}$, after the event. The first group, "new driver", consists of individuals who were observed working all interviewed quarters before the event and were drivers all quarters after the event. The second group, "driver stayer", consists of individuals who were drivers during all interviews, before and after the event. Similarly, the third group, "informal stayer", consists of individuals who were informal for all their interviews. Those restrictions reduce the sample substantially, leaving each group with 1,190 observations, 802 observations and 25,822 observations, respectively.

The event-study equation to be estimated via S&A for each group is the same as in Equation 4-1 without time-varying controls, comparing those groups in treated versus last-treated municipalities. The outcome of interest now is the wage of each individual at each quarter, and as before, wages are constructed as reported monthly earnings divided by reported weekly worked hours. Figure 5.5 plots the event-time dummies' coefficients $\mathbf{b_k}$ for each group and show no detectable effects on wages for either group, with small statistically insignificant estimates.

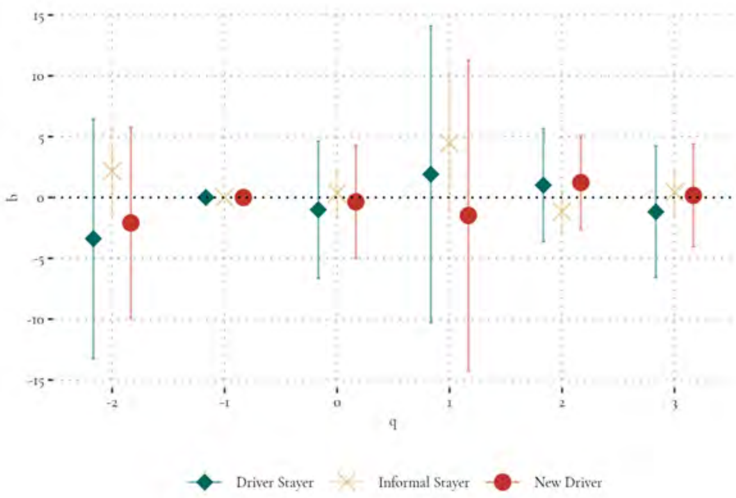


Figure 5.5: *Wages:* S&A Event-Study Estimates for Different Groups

Notes: Coefficients estimated from Equation 4-1 are plotted, without age and schooling years as controls. These are estimated on a rotating, unbalanced, panel of people that responded to at least two interviews, observed at most three quarters distant from Uber's entry. Groups are subsampled as explained in the text. 95% confidence intervals are displayed, estimated with robust standard errors. Wages are calculated as reported monthly earnings over their reported weekly worked hours.

Hence, the arrival of a gig economy has no apparent impact on wages of workers who were drivers and stayed that way, a group that can be thought of as a mix of taxi drivers who remained taxi drivers and taxi drivers who transitioned into app drivers. There is also no wage gain for workers who choose to become drivers once the gig economy arrives or for workers in the informal sector.

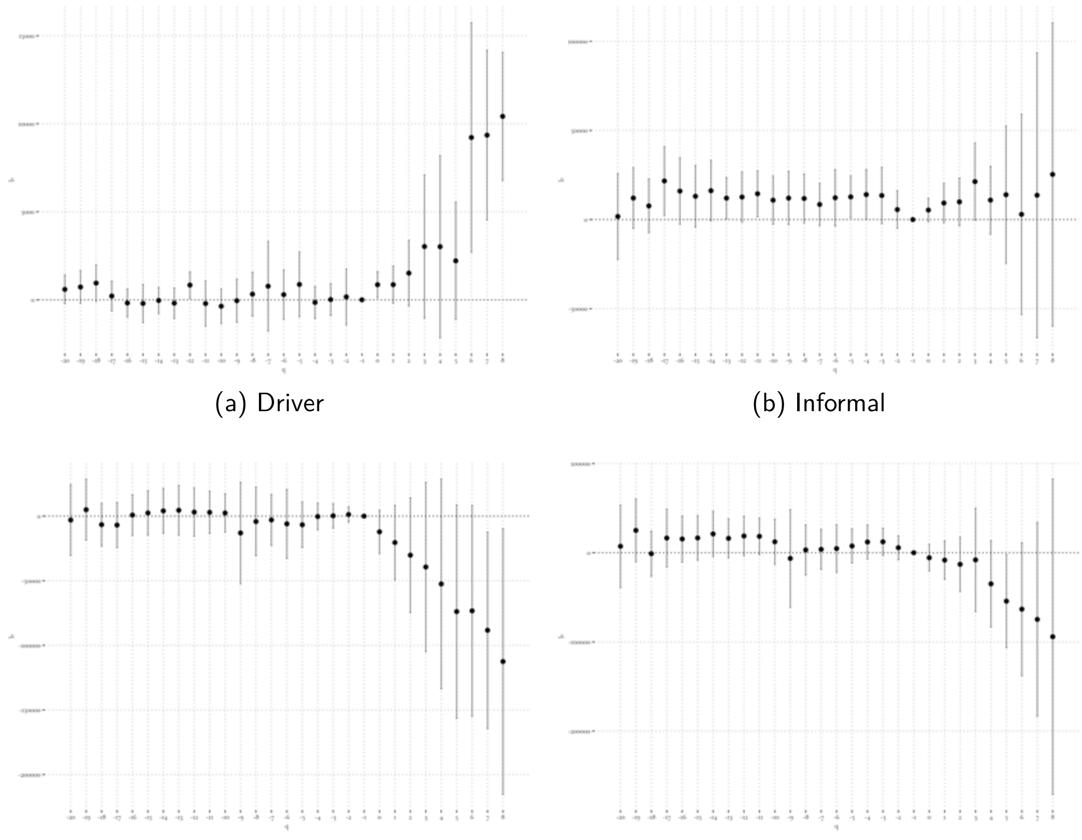
5.4 Municipality-level Results

Running the event-study at the municipality-level allows me to observe effects for a longer period of time, but comes at a cost of not controlling for individual characteristics. Observations here are at the municipality- ($\mathbf{m}$), quarter- ($\mathbf{t}$) and capital-level ($\mathbf{c}$), and again $\mathbf{q}$ will denote how many quarters an observation is distant from the event. Different from the individual-level results, here the sample includes individuals who responded to a single interview, since I am not including individual fixed-effects. Data is trimmed to observe municipalities up to two years after the event, implying $\mathbf{q \leq 8}$.

The equation I estimate is given by the following:

$$\mathbf{Y_{mtq}} = \sum_{\mathbf{k \neq -1}} \mathbf{b_k} \cdot \mathbf{1\{k = q\}} + \psi_{\mathbf{t}} + \phi_{\mathbf{m}} + \epsilon_{\mathbf{mtq}}. \tag{5-1}$$

The set of municipality ($\phi_{\mathbf{m}}$) and quarter ($\psi_{\mathbf{t}}$) fixed effects are included in all regressions. Outcomes of interest ($\mathbf{Y_{mtq}}$) are the sum of individual dummies denoting different labor market status ($\mathbf{y_{itq}}$) for each municipality, weighted by PNAD-Contínua weights. In other words, an outcome $\mathbf{Y_{mtq}}$ is the number of people in a given labor market status in municipality $\mathbf{m}$ and period $\mathbf{t}$. Therefore, the coefficients $\mathbf{b_k}$ denote the causal increase (or decrease) in the number of people having a given status at a distance $\mathbf{k}$ from the event. Rather than using the last-treated units as controls, in this exercise it is the not-yet-treated municipalities that are used as controls, as proposed in Callaway & Sant'Anna (2021) (C&S hereon).



(a) Driver

(b) Informal

(c) Formally Employed

(d) Any Work

Notes: Coefficients estimated from Equation 5-1 are plotted. These are estimated on a rotating, balanced, panel of municipalities, observed at most eight quarters distant from Uber's entry. The status named *Informal* comprises both the informally employed and the non-gig self-employed. The statuses *Formally Employed* and *Informal* excludes drivers, delivery workers and motorcycle drivers. The status named *Any Work* includes any type of work, that is, it includes everyone who is neither unemployed nor out of the labor force. 95% confidence intervals are displayed, estimated with robust standard errors.

Figure 5.6: *Long-run:* C&S Event-Study Estimates

Figure 5.6 plots the event-time dummies' coefficients on four outcomes of interest, repeating those same labor market statuses of interest in the individual-level main results.

Figures 5.6a and 5.6d are consistent with my previous main results in that they show that making gig-driving available increases the number of drivers but does not increase the number of people with work — if anything, that appears to decrease, but standard errors are noisier in this municipality-level specification and are interpreted as statistical zeroes. Hence, there is an increase in the number of drivers of about 10,000 people two years after the event and no statistically significant change in the number of people with any work. Again, there is no additional unemployment buffer introduced by the gig economy.

Figure 5.6c, however, points to something different from the individual-level estimates. Here, the gig economy appears to reduce the number of people formally employed in the medium- to long-run, about two years after the gig economy is introduced. This is similar to what Jackson (2022) found for the United States: that gig availability crowded-out traditional employment in the long-run. Jackson (2022) reasons this result by saying that non-employed people who take up gig employment stay in gig employment while, in the absence of that option, they would eventually return to traditional employment. Nevertheless, in this scenario laid out by Figure 5.6, there is no buffer in the short-run.

This implication of reduced formal employment should be interpreted with caution since not controlling for the changing individual characteristics of the sample might be a problem for identification. Despite that reduction observed in formal employment, there is no evidence of effect on overall employment, meaning that this strategy further corroborates the conclusion that the flexibility of the gig economy did not introduce an unemployment buffer mechanism.

6
Conclusion

Taken together, the evidence suggests that the probability of getting any work was no higher when the gig economy was introduced, although the probability of working in the gig economy raised by between 0.04 and 0.13 percentage points. My evidence deviates from the buffer mechanism found by Jackson (2022), arguably because in my setting the informal sector alone provided the type of flexibility that buffered the non-employed into work.

By breaking down the informal sector into the self-employed and the informally employed, this paper provides evidence that in the absence of a gig economy these gig workers would likely be informally employed, not non-employed. This further supports that the flexibility of a gig economy does not benefit the labor market by buffering the non-employed because a settled informal sector exists.

Further, I show that those who shift into the gig economy (once it exists) were typically skilled blue-collar workers, both formal and informal, with high school diplomas, while some substation portion of them were out of the labor force. Wage estimates are not statistically significant, hence, there are no detectable changes in wages for people who become drivers, who are drivers or who are informal.

Finally, municipality-level results are consistent with individual-level results in that there is a positive effect in the number of drivers but no statistically significant impact in the number of people with any work. The longer-term estimates point that gig availability may crowd-out traditional employment, about two years after gig work becomes available.

Policy implications and future research emerge from these findings. When one thinks about the formalization of these workers, that comes at the cost of reducing the flexibility of their work, it is important to recognize that the flexibility of the gig economy served as a buffer no more than informality to assess that cost. Documenting whether workers are better off in the gig economy rather than in traditional informality, not only through wages, but also through other amenities is an interesting avenue for future research. Another potential avenue of research is understanding why workers in the gig economy are mostly uncovered by social security, an important problem to tackle if one wants to design social insurance programs.

7
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A
Appendix

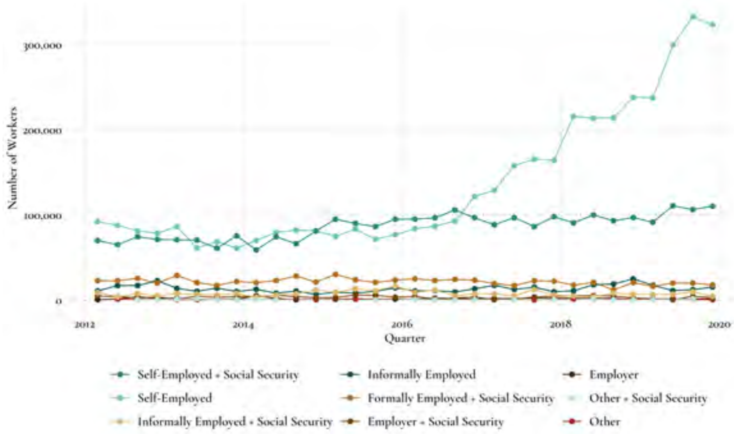


Figure A.1: Drivers By Type Of Labor Relationship and Social Security Contribution

Notes: When "+ Social Security" is omitted, that is a group on that given status that does not contribute to social security.

Table A.1: Entry Dates

Brazilian Capital Cities (States)	**News Source**	**Date**
Rio de Janeiro (Rio de Janeiro)	*Uber*	15/05/2014
São Paulo (São Paulo)	*Globo*	06/26/2014
Belo Horizonte (Minas Gerais)	*Uber*	09/12/2014
Brasília (Distrito Federal)	*Correio Braziliense*	02/26/2015
Porto Alegre (Rio Grande do Sul)	*Globo*	11/19/2015
Goiânia (Goiás)	*Globo*	01/29/2016
Recife (Pernambuco)	*Globo*	03/03/2016
Curitiba (Paraná)	*Globo*	03/18/2016
Salvador (Bahia)	*Globo*	04/07/2016
Fortaleza (Ceará)	*Globo*	04/29/2016
Natal (Rio Grande do Norte)	*Globo*	08/26/2016
Vitória (Espírito Santo)	*Globo*	09/13/2016
João Pessoa (Paraíba)	*Globo*	09/21/2016
Campo Grande (Mato Grosso do Sul)	*Globo*	09/22/2016
Florianópolis (Santa Catarina)	*Globo*	09/30/2016
Maceió (Alagoas)	*Globo*	10/06/2016
Teresina (Piauí)	*Globo*	11/24/2016
Cuiabá (Mato Grosso)	*Globo*	11/25/2016
Aracaju (Sergipe)	*Globo*	12/13/2016
Belém (Pará)	*Globo*	02/09/2017
São Luís (Maranhão)	*O Imparcial*	02/21/2017
Palmas (Tocantins)	*Globo*	03/31/2017
Manaus (Amazonas)	*Globo*	04/12/2017
Porto Velho (Rondônia)	*Globo*	05/17/2017
Rio Branco (Acre)	*Globo*	06/07/2017
Boa Vista (Roraima)	*Globo*	06/21/2017
Macapá (Amapá)	*Globo*	06/28/2017

Notes: News sources are clickable. The corresponding states of each capital city are in parentheses.

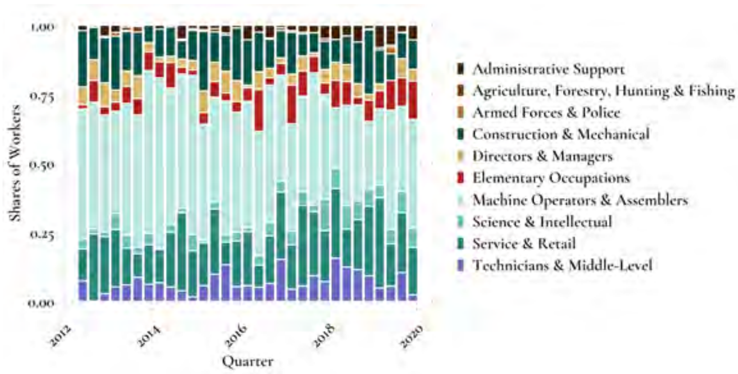
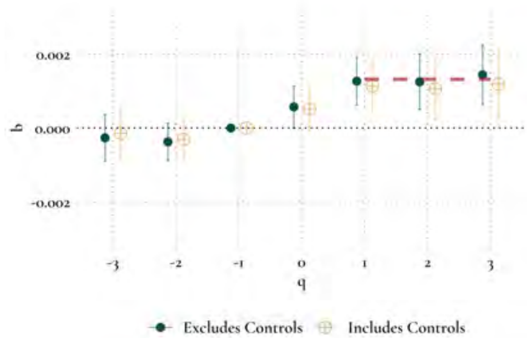
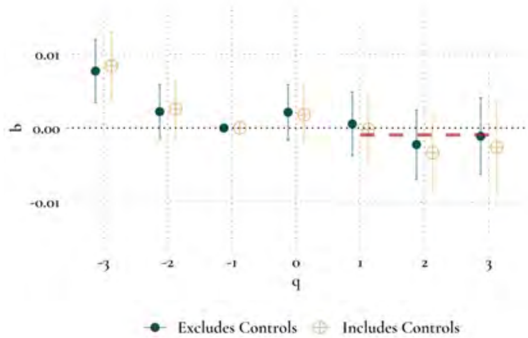


Figure A.2: Labor Market Occupations Before Becoming a Driver

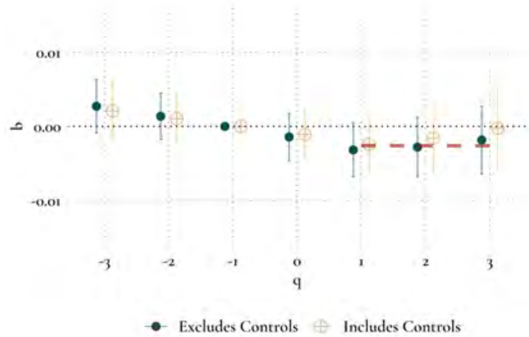
Notes: For every quarter, for those workers observed to transition into a driver (that is, who were not drivers already), each bar represents what proportion of those drivers came from a given occupation at their previous work. I exclude those who were previously unemployed since I am interested in occupations.



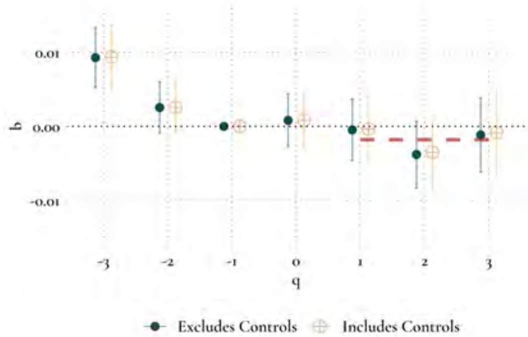
(a) $P($*Driver*$)$



(b) $P($*Informal*$)$



(c) $P($*Formally Employed*$)$



(d) $P($*Any Work*$)$

Notes: Coefficients estimated from Equation 4-1 are plotted, excluding or including age and schooling years as controls for different specifications. These are estimated on a rotating, unbalanced, panel of people that responded to at least two interviews, observed at most three quarters distant from Uber's entry. The status named *Informal* comprises both the informally employed and the non-gig self-employed. The statuses *Formally Employed* and *Informal* excludes drivers, delivery workers and motorcycle drivers. The status named *Any Work* includes any type of work, that is, it includes everyone who is neither unemployed or out of the labor force. 95% confidence intervals are displayed, estimated with robust standard errors. I report (in red) the differences-in-differences estimates, an aggregation of the dynamic $\mathbf{b_k}$ estimates, with respect to the specification with time-varying controls.

Figure A.3: OLS Event-Study Estimates