Banco de México

Working Papers

N° 2022-19

The Impact of the COVID-19 Pandemic on Post-Great Recession Formal Entrants: Evidence from Mexico

Daniel Osuna Gómez
Banco de México

December 2022

Documento de Investigación
2022-19

Working Paper
2022-19

The Impact of the COVID-19 Pandemic on Post-Great Recession Formal Entrants: Evidence from Mexico*

Daniel Osuna Gómez†
Banco de México

Abstract: I study the impact of the COVID-19 pandemic on the formal employment of entrants from each post-Great Recession year. Using longitudinal Mexican social security records and an individual fixed-effects difference-in-differences design, I find that the pandemic caused more recent entrants from each post-Great Recession year to lose formal employment at higher rates than other post-Great Recession entrants that joined the formal sector before them. However, the gap in employment narrowed as the economy recovered. The impact was larger for women and workers with less formal work experience in industries with more interpersonal interactions, like hotels and restaurants.
Keywords: COVID-19 Pandemic, Post-Great Recession Entrants, Formal Labor Markets, Tenure.
JEL Classification: I15, J13, J62, O17

Resumen: Estudio el impacto de la pandemia COVID-19 en el empleo formal de los entrantes de cada año posterior a la Crisis Financiera. Usando registros longitudinales de seguridad social de México, y una estrategia de diferencia en diferencias con efectos fijos a nivel individuo, encuentro que la pandemia causó que los entrantes más recientes de cada año posterior a la Crisis Financiera perdieran su empleo formal a tasas más altas que las de otros entrantes posteriores a la Crisis Financiera que se unieron al sector formal previamente. Sin embargo, las diferencias en empleo se están reduciendo conforme la Economía se recupera. El impacto fue mayor para las mujeres y los trabajadores con menor experiencia laboral formal en industrias con mayor interacción presencial, como hoteles y restaurantes.
Palabras Clave: Pandemia COVID-19, Entrantes del Periodo Posterior a la Crisis Financiera, Mercados Laborales Formales, Experiencia Laboral.

*I am thankful to Dan Garret, Eliza Forsythe, Jorge Pérez Pérez, Chan Yu, León Fernández Bujanda and Alfonso Cebreros for helpful discussions and comments, as well as to seminar participants at Banco de Mexico, CIDE, El Colegio de Mexico, Pontificia Universidad Javeriana and ISI Delhi Annual Conference. I would also like to thank Alejandro Ruiz Ortega for excellent research assistance.
The data used in this paper is confidential and was accessed through the EconLab at Banco de Mexico. The EconLab collected and processed the data as part of its effort to promote evidence-based research and foster ties between Banco de Mexico's research staff and the academic community. Inquiries regarding the terms under which the data can be accessed should be directed to: econlab@banxico.org.mx.

† Dirección General de Investigación Económica. Email: dosuna@banxico.org.mx.

1. Introduction

Previous research shows that individuals with limited work experience have a higher risk of job loss during recessions (Jovanovic 1979; Abraham and Medoff 1984; Bachmann and Felder 2018; Molloy et al. 2021). For these workers, the COVID-19 pandemic was unusual in several dimensions: not only did it severely impact industries that rely heavily on young workers (Costa-Dias et al. 2020; Campa et al. 2021; Banco de Mexico 2022), but post-Great Recession entrants (those who joined the labor market after the Great Recession) were also still struggling employment-wise due to employment hysteresis from the Great Recession (Rothstein 2021).[1] The Great Recession's hysteresis may still have an effect since the job prospects of workers are influenced by initial employment conditions (Beaudry and Dinardo 1991) and their job prospects may take up to 10 years to recover from an unlucky start (Schwandt and von Wachter 2019). Additionally, the labor market changed after the Great Recession: for example, Jaimovich and Siu (2020) and Matias-Cortes and Morris (2022) found that the Great Recession led to a reduction in middle-skill jobs in both the US and Mexico. Since early 2020, the sum of these factors has fueled speculation about the significant pandemic job losses among young workers: was this due to hysteresis effects of the Great Recession or their industry of employment?[2] Was their lack of work experience another potential explanation?

This paper studies how the impact of the COVID-19 pandemic on employment differs for entrants from each post-Great Recession year and explores several hypotheses regarding the causes of larger job losses for recent post-Great Recession entrants (relative to less recent post-Great Recession entrants). Mexico provides a helpful case-study given that, in addition to having suffered one of the largest GDP drops among middle-income countries in 2020 (8.5% annual rate, National Institute of Statistics) and one of the highest rates of COVID-19 deaths worldwide,[3] Mexico adopted almost no new fiscal policy measures directly intended to attenuate the impact of the pandemic, focusing instead on sanitary measures and business

[1] I can share Rothstein's (2021) replication using the Mexican Employment Survey (ENOE) on request.

[2] Illing, Sean. Millennials are getting screwed by the economy. Again. *Vox*. https://www.vox.com/policy-and-politics/2020/4/21/21221273/coronavirus-millennials-great-recession-annie-lowrey.

[3] Which countries have protected both health and the economy during the pandemic? https://ourworldindata.org/covid-ghealth-economy.

closures. The country had allocated 0.5% of its GDP on additional measures to protect firms and households by October 2020, which is low even compared to similar countries (Swarnali et al. 2020). These facts allow for an identification strategy that does not necessitate disentangling the impact of additional fiscal policies. Moreover, focusing on Mexico minimizes the risk that any treatment and control group are different in their remote job adoption rates: few jobs can be performed remotely in Mexico (10.6%, Leyva and Mora 2021), and these are easily identifiable (high-income jobs in high-income areas). Therefore, it is feasible to disentangle any potential impact of remote job adoption rates.

To carry out the analysis, I use the Mexican Social Security Records (IMSS, for its acronym in Spanish). This longitudinal administrative dataset contains monthly information on private formal workers and relies on a data-collecting process that was not interrupted by the pandemic. Because this dataset contains the entire formal employment history for post-Great Recession formal entrants, it allows me to classify workers by year of entry into the formal labor market (a cohort) and follow each worker over time. I then use a difference-in-differences analysis, where I compare each cohort's formal employment with another post-Great Recession cohort that entered the formal labor market a few years earlier, around the onset of the pandemic. For example, I compare the 2016 cohort against the 2012 cohort.

Independently of the pandemic, recent entrants might differ from more experienced workers in the probability of their remaining in the labor market in subsequent years or in terms of other characteristics. This difference between different groups of workers might cause a threat to identification. Therefore, I take the following actions: first, I control for individual time-invariant heterogeneity; second, I compare individuals who differ by a few years (<5) of age and work experience. Because both groups are similar in age and work experience, they have similar observable time-variant IMSS characteristics. For example, it is more reasonable to compare the employment trends and characteristics of someone at 30 with those of someone at 34 than to compare them to someone at 55.[4] Finally, I focus on workers with at least two years of experience in the labor market at the onset of the pandemic. This is because I found that initial attrition dynamics could be a confounding factor for the

[4] This strategy also allows me to avoid using cohorts particularly affected by the Great Recession (2006, for example) as control groups, as these cohorts seem to be different in their composition from post-Great Recession entrants.

labor market dynamics of interest during the initial tenure in formal employment. The overarching identification assumption is that, conditional on remaining in the formal labor market for at least two years, the pre-pandemic probability of remaining for additional years in the formal labor market and with a specific employer was similar for adjacent cohorts. Given similar survival rates since entry, this strategy indirectly measures the relative pandemic shock on workers who differ in their total formal work experience.

The analysis in this paper must also deal with three additional data limitations, namely: the potentially endogenous timing of labor market entry, differences in informal labor market experience, and time-variant unobserved characteristics. I address the first issue using several approaches suggested by Schwandt and von Wachter (2019) and conclude that my analysis is not sensitive to this concern. Regarding the second issue, I limit my sample to individuals who entered the formal market at a young age to limit the effect of differences due to time spent in the informal market. I also vary the age at entry and show that as long as the treatment cohort entered the formal labor market after the control group, the findings of this paper hold. I focus on those who joined the formal labor market at 22 because these workers are still relatively young (their informal labor market experience is still limited) and this is the most common age of college graduation in Mexico (ANUIES, Anuarios Estadisticos de Educacion Superior). Moreover, college educated workers are of interest in this literature, as they are less likely to have skill obsolescence and are less vulnerable to technological changes. This is why this is a common age of reference in the literature. Finally, to minimize the role of time-variant unobservable characteristics, I do the following: I find proxies for such variables in IMSS data (like wages for productivity, and the probability to change employer for match quality) and test for parallel trends between treatment and control group on such outcomes for a year before the onset of the pandemic. I also look for other variables (like working hours) not observed in the IMSS dataset in the National Employment Survey (ENOE) and show that differences among different age-adjacent groups were relatively small before the pandemic. Finally, I perform Oster's (2019) test for assessing bias due to unobservable factors and show that this bias is likely not affecting my coefficients.

My results show that the pandemic caused entrants to later post-Great Recession years to lose formal employment at higher rates than other post-Great Recession cohorts that entered the formal labor market a few years before them. Specifically, job losses are concentrated among those who entered the labor market more recently (workers between the ages of 25 and 30). For example, in June of 2020, the 2016 cohort was 1 percentage point more likely to lose formal employment than the 2012 cohort, whereas the difference between the 2014 and 2012 cohorts was 0.5 percentage points during the same month.

To measure the role of the hysteresis effect from the Great Recession, I divide my sample according to the geography of the Great Recession hysteresis (see Campos-Vazquez et al. 2021). I find that areas that suffered the most job losses during the Great Recession do not have the largest cohort differences during the Pandemic. Instead, areas that suffered the least amount of job losses during the Great Recession have the largest cohort differences during the Pandemic. These results suggest that the Great Recession hysteresis is not a relevant mechanism.

I also find other novel facts that vary for different types of post-Great Recession entrants: first, the pandemic caused more recent cohorts to lose formal employment at higher rates across all industries.[5] Second, among high-earning individuals, the pandemic had no differentiated impact on more recent cohorts' formal employment. Third, I also found that women of more recent entry were particularly affected by the pandemic. Fourth, among workers of the same age and work experience, those with longer firm-specific tenure were more likely to keep their jobs during the pandemic.

This paper highlights that recent post-Great Recession entrants are particularly vulnerable to job loss because of the pandemic. My results align with our understanding of how the pandemic impacted workers: it has been particularly damaging to vulnerable groups (Brodeur et al. 2021). For example, Chetty et al. (2020) found that low-income workers bore the brunt of the recession and Alon et al. (2020) describe the harsher impact on women's

[5] Costa Dias et al. (2020) argued that job loss among the UK youth was mostly driven by the industry composition. Similar arguments were done for other countries too: For Mexico, see Banco de Mexico (2021); for Sweden, see Campa et al (2021); for the US, see Cowan and Shayne-Garcia (2021).

jobs. I focus solely on the effects felt by the group of recent post-Great Recession entrants relative to less recent post-Great Recession entrants.[6][7]

Methodologically, I build on the literature that has found different employment responses to the pandemic by age group (Campa et al. 2021, for example). By also focusing on the entry date, I can observe each worker's formal employment history and compare groups with a similar pre-pandemic probability of staying with their employers. This can minimize the risk of important biases, but it also shows that the tendency of younger workers to job-hop is not the cause of their greater job losses.[8] Furthermore, this paper differs from other projects studying the pandemic's impact on different age groups in that its design allows me to identify the importance of different firm-tenure profiles among workers of the same age.

This analysis studies the impact of recessions on employees in a developing country who have less work experience. Previous literature has focused on developed countries (Jovanovic, 1979; Abraham and Medoff 1984; Bachman and Felder 2018; Molloy et al. 2021). However, those findings may not apply to developing countries for a critical reason: developing countries have lower average human capital, and their firms might assign a different premium to tenure (Levy and Lopez-Calva 2019). In other words, because jobs in developing countries do not require as much specialization, firms in such countries may be indifferent to tenure during recessions. However, my paper finds that tenure is in fact a determinant of job loss in Mexican formal labor markets during recessions. These results help to provide additional external validity to this literature.

My results support the view that the pandemic caused a multi-faceted problem for less-experienced workers. However, these job losses are disappearing as the economy recovers. If this recovery maintains its trajectory, most young workers will return to the labor force. To accelerate such recovery and minimize possible permanent job losses, governments

[6] von Watcher (2020) discusses the potential effect of COVID-19 on new entrants, whereas I focus on workers with work experience. Another notable mention is Chatterji and Li (2021), who study 15- to 24-year-old individuals, whereas I focus on older individuals (25- to 33-year-old workers). The critical difference is that the youngest workers have a different trade-off between work and additional schooling.

[7] Several documents from Banco de Mexico have shown that the pandemic had a different impact on different age groups. See Banxico (2021).

[8] Adkins, Amy. Millennials: The Job-Hopping Generation. Gallup. https://www.gallup.com/workplace/231587/millennials-job-hopping-generation.aspx

can enact policies backed by the literature (job search assistance being the most promising; see Caliendo and Schmild 2016).

The paper proceeds as follows: Section 2 describes the data and Section 3 the Mexican labor market during the pandemic. Section 4 explains the identification strategy and Section 5 describes the results. Section 6 studies heterogeneity and Section 7 summarizes the robustness checks. Section 8 concludes.

2. Data

This paper uses employer–employee panel data from the Mexican Institute of Social Security (IMSS), which covers all formal private-sector workers.[9] Employers must legally report wages, separations, and new hires to the IMSS. This reporting process is remote and, accordingly, this dataset's methodology and coverage were not affected by the pandemic. Thus, these records became a key source of information during the pandemic.[10,11]

The administrative records start in 2005. For each worker, it is possible to know the age, sex, wage, municipality, industry, as well as a unique employer and employee ID.[12] Several features of this dataset make it especially suitable for a cohort-based analysis: first, this dataset is an employer and employee panel. This allows me to know each worker's complete formal employment history; second, this is a census-type dataset, which allows me to compare groups that differ by a few years of work experience and/or age; third, I can estimate total and firm-specific experience (commonly called "tenure").

2.1 Data Limitations

Concerning the IMSS records, it is important to bear in mind the following caveats: I cannot estimate the entry date for anyone before 2006, nor the time spent in the informal labor market. I also cannot see a worker's educational attainment, graduation timing, or

[9] In 2019, 31% of all Mexican workers had IMSS affiliation. 5.5% had ISSTE (this service is for government workers) and 0.6% had other affiliations (for example, PEMEX has its own services). 62.5% had no access to health care services through their employment.

[10] INEGI, the National Statistics Institute, suspended face-to-face interviews for several months during the pandemic and created a new employment survey afterwards (N-ENOE). This new survey uses a reduced sample size and has limited power to offer a post-pandemic analysis.

[11] Even the CPS faced several major disruptions in its data-collection procedures and a large drop in response rates, which generated biases in the estimation of the unemployment rate (Heffetz and Reeves 2020).

[12] This dataset does not include the name of any individual or firm, nor any information that allows the researcher to identify the exact identity of any particular individual or firm. What it does include is an anonymized version of worker and firm identifier. This allows me to create an analysis including firm or individual fixed effects without ever revealing the exact identity of any individual or firm.

occupation. In the following paragraphs, I discuss the actions I took to minimize any bias arising from these limitations.

IMSS administrative data starts in January 2005, and it is possible to follow workers after that date. I follow the literature that uses panel data to study entrants (see Gertler et al. 2022 and Albagli et al. 2020) and define entrants as those workers who first appear in the IMSS dataset one year after the first observation in my dataset (January 2006).[13]

A significant limitation of this analysis is the lack of information regarding the informal market; after all, informal work can provide meaningful human capital accumulation. To reduce any potential bias caused by this unobservable variable, I restrict my sample to individuals who began their first formal employment very young (I vary the definition of "young" as a robustness check). By doing so, I minimize the likelihood that workers in my sample have a significant employment history in the informal market.

A second concern regarding informal employment is that I have no information regarding a worker's destination once they lose their formal job (informal job, unemployment or staying outside of the labor force). Therefore, I simply state that the individuals are not formally employed when they are not in the dataset. Having a formal job is an outcome of interest in itself because it is associated with increased productivity, tax revenues, and wages (Ohnsorge and Yu 2022).

To limit the bias related to the lack of information regarding workers' education, I study workers who entered the labor market at similar ages. The reasoning behind this choice is that, in the absence of large shocks that could affect graduation rates, workers belonging to different cohorts entering at the same age to the workforce should have a similar combination of education profiles (see Appendix 3 for a formal test for endogenous graduation timing). The issue at hand is to choose a particular age at entry to run the analysis (or to not choose an age profile). I choose those who joined the labor market at 22: this is because college graduation before 22 is extremely rare (Anuarios Estadisticos de Educacion Superior, ANUIES),[14] 22 is the most common age for graduation in Mexico[15] and the desired

[13] I do not consider the 2005 cohort because it would not include all the months in that year, which would result in a cohort that is inherently different from the rest.
[14] This is true for both Mexico and the US.
[15] Less than 5% of all individuals leave college before 22 (Higher Education Census 2017).

population in the literature related to entrants is those who have graduated from college (their skill requirements are less likely to become obsolete or change in short periods of time and thus, are better suited for comparisons across time; see Rothstein 2021). Accordingly, for this paper, the unit of study is the cohort which is defined as those workers who entered the formal labor market for the first time in a given year while being 22 years old.[16,17] I recognize that I could have used a different and equally reasonable cohort definition (for example, 19 years old, which is the typical high school graduation age; or 23, to account for those who graduate from college later). However, the conclusions of this paper are not particularly sensitive to changing this definition as long as the treatment group entered the labor market after the control group.

A related issue is the possibility of endogenous labor market entry: some workers might choose when to join the labor market depending on the state of the labor market, creating large group differences. I test for endogenous graduation timing and labor market entry following the tests proposed by Schwandt and von Wachter (2019; Appendix A3) and do not find large effects.

Finally, although there is no information on occupation, I attempt to make indirect approximations (for example, wage-quintile-within-the-firm-and-industry fixed effects) to control for such differences.

3. The Covid-19 Pandemic and the Post-Great Recession Entrants

During the COVID-19 pandemic, Mexico faced challenges similar to those confronting other developing countries. For the most part, most of Mexico's policy actions were sanitary measures (promoting the use of masks, for example),[18] limiting businesses' hours of operation, and some monetary policy actions to maintain favorable conditions in financial markets (Banco de Mexico 2020). However, fiscal policy actions were limited,

[16] For example, the 2016 cohort refers to those who, at 22 years of age, started their first formal job in 2016.
[17] Another benefit of using this cohort definition is research comparability: 22 is a common benchmark age used in many studies related to entrants (for example, Rothstein 2021 and Schwandt and von Wachter 2019).
[18] Medidas de Seguridad Sanitaria. https://coronavirus.gob.mx/medidas-de-seguridad-sanitaria/

concentrating on a few cash transfer programs: some cash transfers for children with disabilities and senior citizens,[19] as well as a limited number of loans to small firms.[20,21]

These measures were not enough to counteract the impact of the COVID-19 pandemic on the Mexican labor market. The labor force participation rate dropped 4.3 percentage points in a single quarter (Figure 1, Panel A), making it the most significant drop in labor force participation rate since the start of the National Employment Survey (ENOE) in 2005.[22] In fact, between the first and third quarters of 2020, 9.5% of workers lost their jobs (approx. 5 million individuals; Figure 1, Panel B). The greatest job losses were among informal workers. However, 5% of all formal workers also lost their jobs. This recession is different from previous ones in that a typical response to job loss during the pandemic was to leave the labor force altogether instead of looking for opportunities in the informal market (ECOVID-ML, INEGI).

During the first three months of the pandemic, only essential businesses were allowed to operate (supermarkets and hospitals, for example). However, by May, the construction and mining industries were allowed to reopen (Diario Oficial de la Federacion 2020). The rest of the economy followed gradually and piecemeal, starting in June 2020 (Banxico 2021). To regulate business operations during the reopening, the government designed a system called *semaforo* ("traffic lights"). This system assigned each state a different color (red, orange, or green) in response to the available number of hospital beds; each category entailed a different set of restrictions. However, the government could not effectively enforce these restrictions. For example, one of the restrictions of a red *semaforo* was to close restaurants, but many restaurants and other businesses did not respect these guidelines.[23]

[19] This program extended its benefits only to a small and unknown number of individuals. Source: Acciones COVID-19 (cdmx.gob.mx)

[20] The loan consisted of $25,000 MXN ($1,250 USD), which is to be paid in three years. However, there is no data on who received these benefits. Source: Expansion. Estos son los apoyos del gobierno para la contingencia. https://politica.expansion.mx/presidencia/2020/05/20/estos-son-los-apoyos-gobierno-amlo-contingencia-sanitaria

[21] For a full list of policies, see: IMF. Policy Responses to COVID-19: Mexico. https://www.imf.org/en/Topics/imf-and-covid19/Policy-Responses-to-COVID-19#M

[22] To make a comparison, during the Great Recession, the labor force participation in Mexico remained at an almost constant value of 60% of the population.

[23] Animal Politico. Restauranteros: Abrimos o morimos. https://www.animalgourmet.com/video/abrimosomorimos/

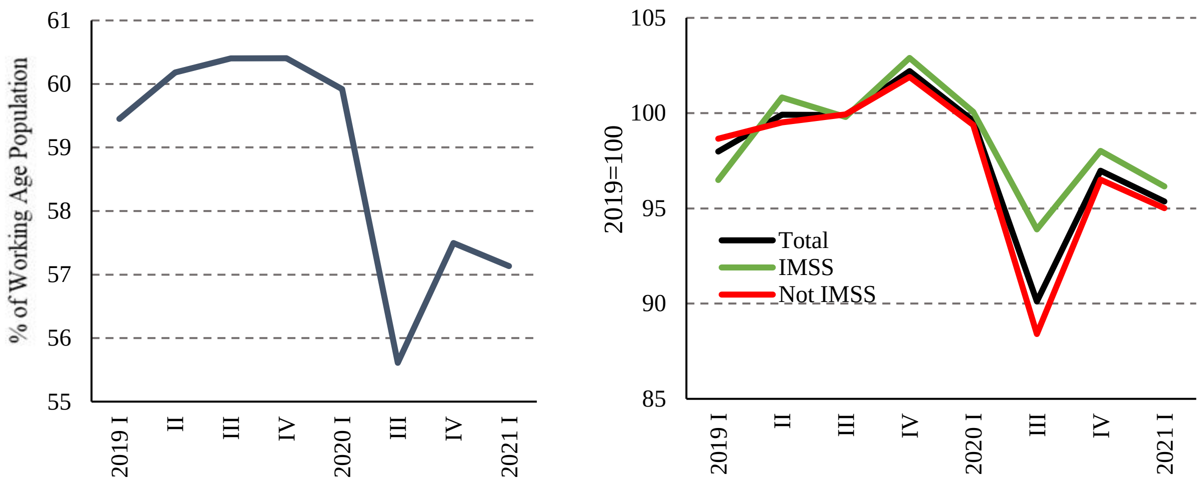
FIGURE 1. EMPLOYMENT IN MEXICO AROUND THE ONSET OF THE PANDEMIC

Panel A. Labor Force Participation

(% of Working Age Population)

Panel B. Number of Workers, by IMSS affiliation

(2019 = 100)



Source: ENOE.

Industries that rely on large gatherings (movie theaters, concert halls and schools, for example) took even longer to reopen. For example, movie theaters and concert halls were unable to reopen until June 2021 and schools reopened in August 2021. [24,25]

New research has shown that, as in previous recessions, the pandemic disproportionately impacted vulnerable groups. For example, Chetty et. al (2020) found a disproportionate impact on lower-income workers. There have been similar patterns in Mexico wherein the most vulnerable workers are the most affected. For example, Mexican women were more likely to leave the labor force (Banxico 2022). This paper focuses on another vulnerable group: post-Great Recession entrants. These workers have some work experience but are still at the beginning of their professional lives.

In Figure 2, I decompose the total IMSS-affiliated population by cohorts, sum all individuals per month, and estimate annual percentage changes (July 2019–July 2020). I find that the 2006–2011 cohorts (aged 31 to 36 years of age in 2020) faced similar employment

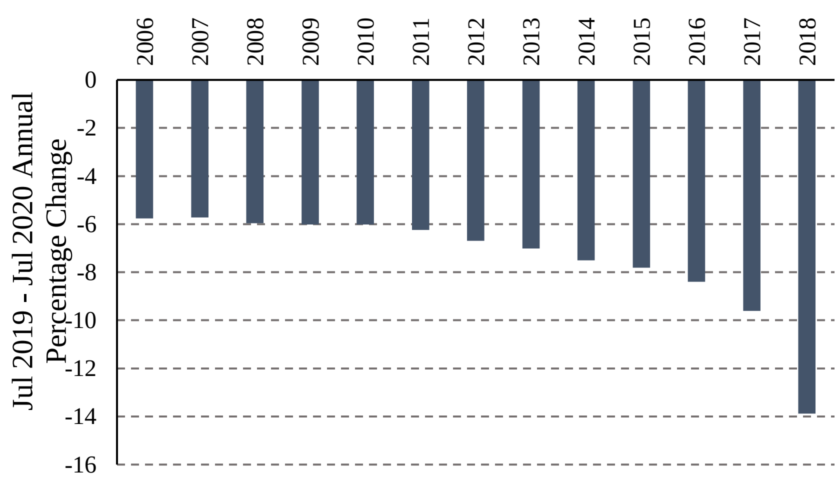
[24] Forbes. 2021. Cines logran recuperase en 2021, pero aún están lejos de los niveles prepandemia https://www.forbes.com.mx/negocios-cines-logran-recuperase-2021-lejos-niveles-prepandemia/

[25] Expansion. 2021. La reapertura de las escuelas en México no puede esperar: UNESCO. https://politica.expansion.mx/mexico/2021/08/26/mexico-posee-las-fortalezas-para-el-regreso-a-aulas

losses, and that every cohort starting in 2012 (aged 30 or younger in 2020) faced a monotonically increasing drop in employment compared to the previous cohorts. In Table A1.1, I included some general descriptive statistics on each cohort (for example, how many individuals there are in each cohort, wages, etc.). This table tells us that younger cohorts were more likely to be employed in certain industries (like restaurants) and to have lower earnings (the wage differences are not statistically significant). Any of these characteristics could potentially explain younger workers' larger pandemic job losses.

The increase in annual job losses starting with the 2012 cohort is particularly interesting because the Great Recession started in 2009 in Mexico, and it took until 2011 for employment levels to surpass pre-recession levels (Banco de Mexico 2012). Therefore, 2012 was the first post-Great Recession cohort. Still, the Great Recession had a hysteresis effect that continued at least until the year preceding the pandemic: polarization increased since 2008 (Matias-Cortes and Morris 2021), and a small number of individuals never returned to the formal job market (Campos-Vazquez et al. 2021). Therefore, the Great Recession hysteresis could also be driving cohort differences in pandemic job losses.

FIGURE 2. FORMAL EMPLOYMENT ANNUAL PERCENTAGE CHANGE (JULY 2019-JULY 2020), BY COHORT



Notes: In this paper, a "cohort" (horizontal axis) is the group of workers that entered the formal labor market during the same year at the same age (22). For this graph, I summed the number of workers per cohort still working by year-month. Then, I calculated the 2019 July – 2020 July percentage change.
Source: IMSS administrative records.

4. Identification Strategy

4.1 Statistical Model

To estimate the reduced-form impact of the pandemic on employment for post-Great Recession entrants, I first focus on an event-study framework where I evaluate how the employment rate changed relative to the initial COVID-19 shock in a specification similar to that of Bacher-Hicks et al. (2021):

(1) $A_{it} = \alpha_i + \omega_{m(t)} + \delta t + \sum_{l=2019\,I}^{2019\,V} \beta_l \times\ 1[e(t) = l]\ \ + \sum_{k=2020\,I}^{2021\,II} \beta_k \times\ 1[e(t) = k] + \mu\ \times\ 1[e(t) < 2019\ I] + \varepsilon_{it}.$

where A_{it} is a dummy equal to 1 if individual i has an IMSS-affiliated job during period t (0 otherwise); α_i are individual fixed effects, $\omega_{m(t)}$ are bimester of the year fixed effects (to account for seasonality)[26] and δt are linear time trends. e(t) represents the bimester-year of reference, and the omitted bimester is 2019 VI (which is the last bimester before the pandemic year).[27] Data prior to 2019-I are used to identify seasonality. ε_{it} is the regression error. This regression is estimated for each cohort separately. I use bimonthly data (the second month of each bimester) due to computing power limitations. The parameters $\beta_{2020\,I}$–$\beta_{2021\,II}$ are the mean difference in employment probability compared to the same months in prior years presented at levels relative to 2019 VI. This is after accounting for a time trend and seasonality, and for individual time-invariant heterogeneity. These parameters identify the causal effect of the pandemic on formal employment under two assumptions, which I discuss in turn.

The first assumption is that no other determinants of employment should coincide with the onset of the COVID-19 crisis. As far as I know, Mexico had no large shocks around the start of the pandemic. The only plausible exception is the raising of the minimum wage in border municipalities in 2019. Although this increase in the minimum wage could have caused a loss of formal jobs, only a small number of municipalities were affected (which I can drop as a robustness check, which I did in a subsequent section). If there were other significant shocks around the onset of the pandemic that I did not accounted for, then the

[26] This takes the form of a series of dummies (i.e., bimesters 1-6)

[27] I set the bimester 2019 VI as the period of reference because I will be using bimonthly data, and there was news about COVID-19 as early as December 31st, 2019 (World Health Organization 2021). I am being cautious about the possibility that businesses responded to the news by late January.

interpretation of β_k from (1) should change, as to also incorporate these additional shocks to the causal interpretation.

The second assumption is that errors should be exogenous to time-dummies (once including the set of controls described in equation 1). I argue that this is plausible because there has not been a recession or any large and persistent drop in formal employment (with seasonally adjusted data) for the previous five quarters before the pandemic (Bank of Mexico, Quarterly Report 2020 I) and because I consider a short time horizon.

Because I am also interested in the relative effect of the pandemic on recent entry cohorts, I estimate the following regression:

(2) $A_{ict} = \alpha_i + \omega_t + \sum_{T \neq 2019VI} \beta_T \times 1(t = T) \times Treatment\_group_{ic} + \varepsilon_{ict}$,

where A_{ict} is a dummy equal to 1 if individual i of cohort c is working in an IMSS-affiliated job during period t (0 otherwise). I control for time (ω_t) and individual fixed effects (α_i). ε_{ict} is the regression error. The coefficients of interest are the β_Ts, which represent the difference between the treatment group (individuals for who $Treatment\_group_{ic}$ is equal to 1)[28] and the control group for each bimester t. I will use different treatment groups in different regression, however, in all cases, the treatment group is a cohort that joined the formal market after the control group (for all regressions, the control group is the 2012 cohort).[29] For example, I run a regression where the treatment group is the 2014 cohort, and another regression where the treatment group is the 2015 cohort. The results are standardized so that there are no differences in formal employment rates between the treatment and control group during 2019 VI. Therefore, $\beta_{2020\,I}$-$\beta_{2021\,II}$ measures the differential formal employment patterns for recent cohorts during the pandemic. Finally, please note that in this regression I include all workers belonging to a cohort, independently of whether they are employed in any specific month. However, to study subsamples of interest (for example, workers with low pre-pandemic wages) I limit my sample to individuals who had employment in 2019 VI and use their last employment during that year to measure workers'

[28] I do not use a continuous treatment because there are non-linear relationships between cohorts and job loss. The variable has a value of 0 for the control group.

[29] This decision was made because the 2012 cohort is the first cohort following the Great Recession in Mexico and do not have a compositional difference from other post-Great Recession cohorts. See Appendix 3 for more details.

characteristics.[30] Also note that whenever I study heterogeneous effects, I use only the 2016 as treatment and the 2012 cohort as control for the sake of brevity (instead of repeating the analysis for all cohorts).

In choosing a clustering strategy, I follow Abadie et al. (2017), who state that clustering standard errors should be design- or experiment-based. Under the first scenario, the problem is that some groups in the population were not sampled. This is not a problem, because I am using census-type data. The second clustering design is problematic because the treatment appears universal; therefore, I argue that treatment was assigned for different individuals with different probabilities and intensities. Thus, I cluster standard errors at the individual level.[31] Because the COVID-19 pandemic had a greater impact on industries that rely on face-to-face interactions, and because certain localities had different *semaforo* ratings, it may be reasonable to cluster at the locality or industry. Therefore, I include these clustering strategies in the Appendix.

4.2 Difference-in-Differences Identification Assumptions

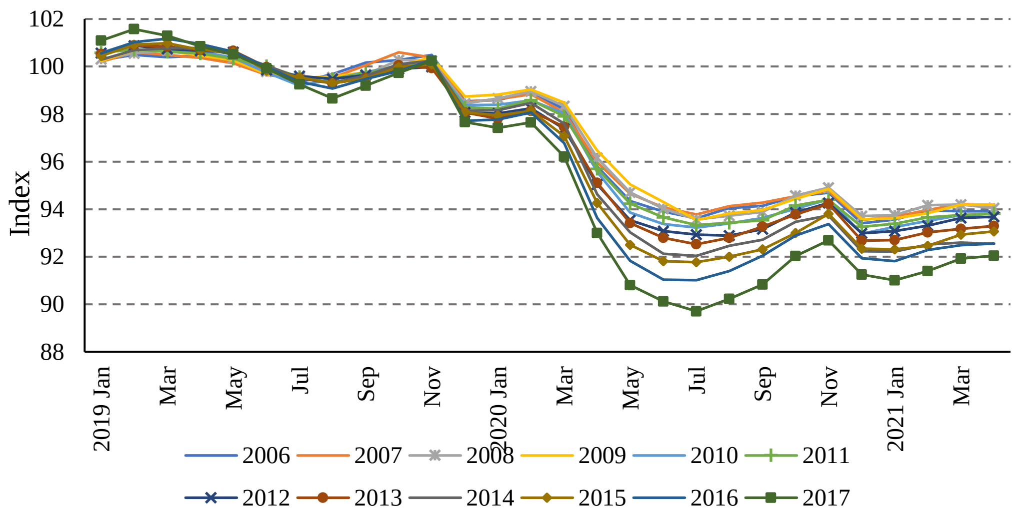
To interpret the results from eq. (2) as causal, I need an additional assumption: in the absence of the pandemic, the differences in formal employment rates between treatment and control cohorts would have been constant over time. This is plausible, given that the cohorts had similar employment trends before the shock and their survival rates (the proportion of workers that stay formally employed) remained stable through time and comparable in magnitude.

Figure 3 illustrates the number of formal workers by cohort and year-month. In this graph, I follow each cohort through time and sum the number of workers formally employed by year-month. I then standardize the number of workers per cohort so that the average for 2019 equals 100. This is a standard “raw-data” view of a parallel trends test for employment. It shows that before the pandemic, all the cohorts in my sample followed parallel trends. But,

[30] This implies that this is an intent-to-treat analysis, where treatment is assigned according to pre-pandemic characteristics.
[31] This clustering strategy is the same one used in Angelucci et al. (2021) and other papers using individual-level panel data to study the impact of the COVID-19 pandemic. The greatest benefit from this strategy is that it allows me to make an analysis that is not conditioned on having observable characteristics in a time period.

just as importantly, it also indicates that these employment trends diverged after the pandemic started in 2020.

FIGURE 3. COHORTS' FORMAL EMPLOYMENT (2019=100)



Notes: For this paper, a "cohort" is a group of workers that entered the formal labor market during the same year at the same age (22). For this graph, I follow each cohort through time and sum the number of workers still employed by date.
Source: IMSS administrative records.

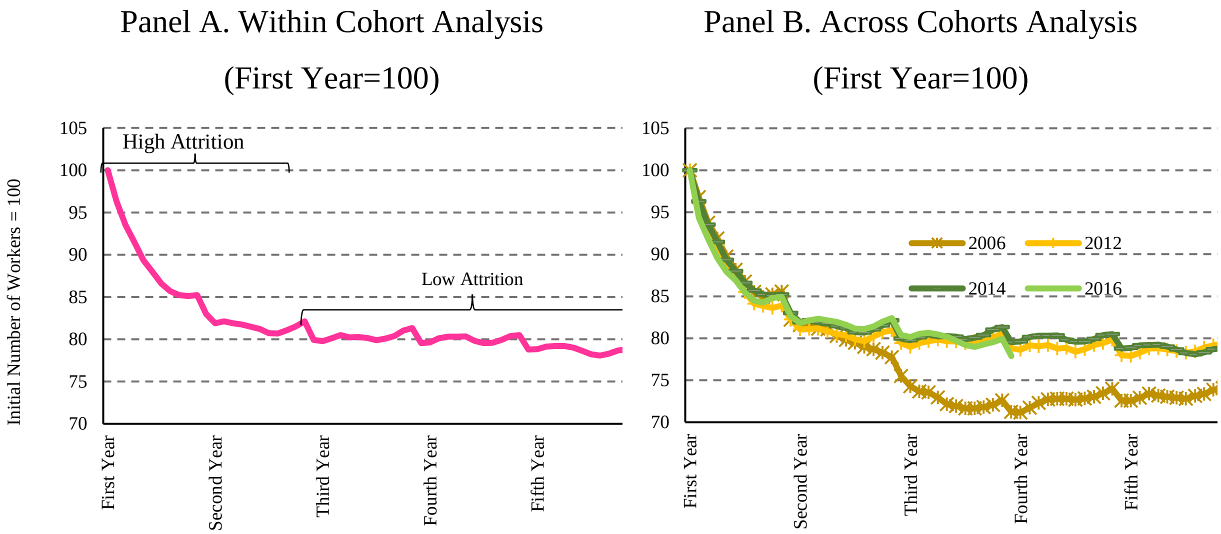
Figure 4 shows the survival rates of some cohorts. Panel A shows an example (the 2014 cohort) of a pattern I observed in the IMSS dataset. Each cohort has an initial period of high attrition (which lasts two years) and then stabilizes. This indicates that I do not need to worry about this attrition process if I study cohorts that have been in the formal labor market for at least two years before the pandemic started. Panel B shows that this pattern is common across cohorts and that their survival rates are similar once they have reached this stable period.[32]

4.3 Possible Sources of Bias

To further validate my research design, I check for common bias sources when comparing cohorts: differences in entry conditions, time-variant characteristics, and firm-match quality.

[32] Please note that the 2006 cohort appears to have permanent job losses caused by the Great Recession. This finding suggests that pre-Great Recession cohorts may not be appropriate control groups.

FIGURE 4. COHORT ATTRITION ACROSS YEARS WITHIN THE LABOR MARKET



Notes: In these graphs, I sum by year-month the number of workers of a specific cohort who are still part of the private formal labor market. In Panel A, I standardized it so that the first month after the entry year, the total number of workers is equal to 100. I did this for the 2014 cohort in Panel A, and added other cohorts in Panel B.
Source: IMSS Administrative Records

First, I explore differences in entry conditions. As each cohort enters the formal market during different years, they may be subject to different selection forces (Borgschulte and Martorell 2018; Schwandt and von Wachter 2019). However, this is not a significant problem if the selection mechanism does not change much from year to year. In Mexico, that selection mechanism is the informal employment rate during a worker's initial working years: during times when the informal employment rate is higher, it is more difficult for entrants to join the formal market (Puggioni et al. 2022). I used ENOE data to graph the informal employment rate of young workers during several pre-pandemic years and found that this rate has been stable (Figure A1.1): for most years, the initial informal rates stayed within approximately +/- 0.5 percentage points of one another (with two exceptions) for adjacent cohorts.

A second common bias when comparing cohorts is that workers' characteristics may vary with time. This is not a serious problem when those characteristics are observable (like wages and industry): for these characteristics, they can be measured at a pre-pandemic date to further study heterogeneity. However, the IMSS dataset does not include many individual-level time-variant characteristics of interest.

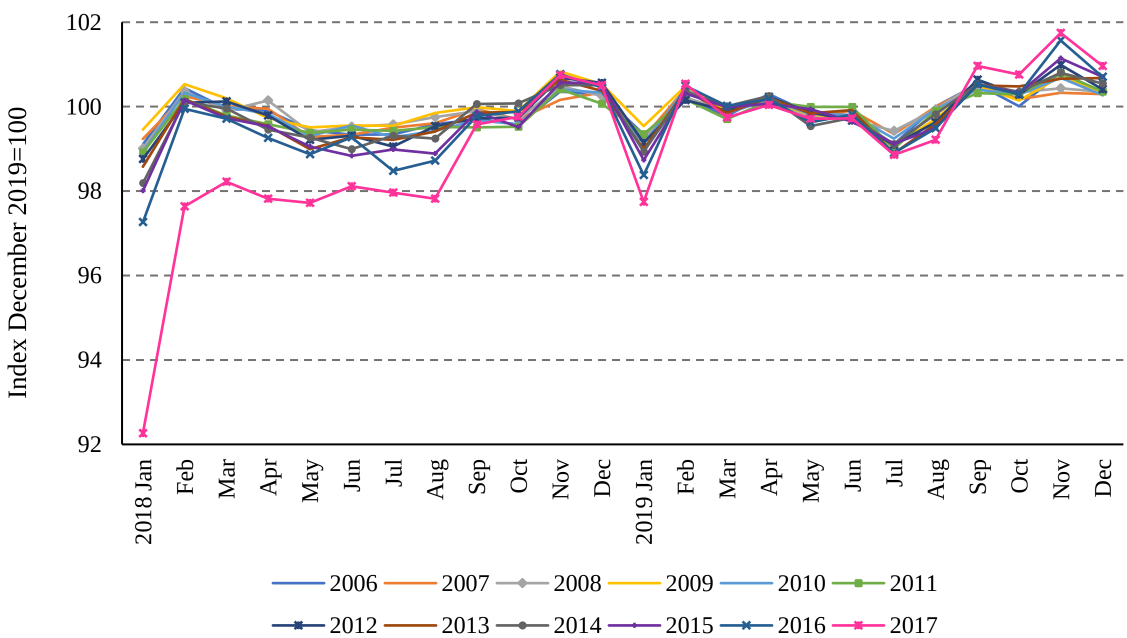
Among all the individual unobserved time-variant characteristics, productivity is likely the most important. Productivity can bias my estimates because individuals who remain longer in the labor market may be more productive (Altonji and Shakotko 1987) or may become more productive as time goes by. Productivity differences that do not vary with time are controlled by adding individual fixed effects. However, time-variant productivity cannot be observed. However, I tested for parallel trends in wages (a proxy variable) between the control and treatment groups, and I showed that their wages had been growing at the same rate for at least one year before the pandemic (Table A2.3). This evidence suggests that differences in productivity are not driving my results.

For some other unobserved time-variant characteristics, I did the following: I used ENOE and the Higher Education Census pre-pandemic waves (2019 and 2017, respectively) to measure these characteristics for different age groups. If these characteristics are not particularly large among different adjacent age groups in 2019, they are less likely to be driving cohort differences in 2020. I performed this analysis for the number of weekly working hours and the proportion of workers with a single job. These are Figures A1.2–A1.3 in Appendix 1. These patterns show that there are systematic differences for workers younger than 25: they work fewer hours and are more likely to have a single job. However, once workers reach ages above 25, their working patterns stabilize. Because the 2016 cohort was 25 in 2019, this indicates that the cohorts I focus on this paper are in age groups with stable working patterns.

To further minimize the risk of omitted variable bias, I perform Oster's (2019) test. Oster (2019) argues that omitted variable bias should be limited if coefficient movements are limited after the inclusion of controls. Oster's (2019) test is a formalization of this approach and estimates a value δ, which assesses the expected relevance that an omitted variable should have to drive a statistically significant coefficient to zero. I estimated these values for all the positive βs and found δ with values larger than one. This suggests that my results are most likely not driven by omitted variable bias.

Finally, I test for differences in "firm–employee match quality." This term refers to the fact that workers who find a high-quality firm match tend to stay longer at that firm (Snell et al. 2018). However, finding a high-quality match can take some time. Therefore, recent cohorts may be more likely to have lower-quality matches. To assess this hypothesis, I created a measure of match quality: the proportion of individuals working for their previous-month employer. In Figure 5, I show this outcome per cohort and find that after working two years in the formal job market, recent entrants are just as likely to stay with their employers as their older counterparts.

FIGURE 5. PROPORTION OF WORKERS RETURNING TO WORK FOR THE PREVIOUS MONTH EMPLOYER (DECEMBER 2019=100), BY COHORT



Source: IMSS Administrative Records.

4.4 Limitations

There are certain limitations that I could not account for. Specifically, I could not account for the role of severance payments and occupational differences among cohorts.

The lack of information regarding severance payments is an issue because severance payments are a function of tenure,[33] and there may be incentives for struggling firms to minimize costs by firing the "cheapest workers to fire." Or it may be that severance payments

[33] According to the Mexican Labor Law (Ley Federal del Trabajo), if a firm fires a worker for reasons that are not related to performance, the worker is entitled to three months of pay and 32 days of pay per year of tenure.

do not matter, because it is common for firms to request a resignation letter in advance during the hiring process or evade paying legal fees for failing to pay severance. [34,35]

Finally, it may be that my results are driven by occupational differences across cohorts. Unfortunately, I do not have information related to occupations. Nevertheless, in subsequent sections, I attempt to make indirect approximations (for example, wage-percentile-within-the-firm-and-industry combinations) to control for such differences.

5. Results

Table 1 shows β_l and β_k the from estimating equation (1) for the 2008, 2010, 2012, 2014 and 2016 cohorts (which were aged 33, 31, 29, 27, 25 years of age in December 2019, respectively). These are estimated employment changes for every bimester around the onset of the pandemic relative to 2019's last bimester. At the top of each column, I mark the studied cohort. Firstly, I would like to highlight that the formal employment probability of each cohort for every bimester before 2019 VI remained constant. However, formal employment decreased for every cohort starting 2020 II. Then, as the economy had begun to recover by the end of 2020, there was modest progress towards reaching pre-pandemic employment levels. The only noticeable difference between cohorts is that more recent cohorts show larger formal employment losses during the pandemic.

As I would like to explore cohort differences, I will show the results of equation (2) in Table 2.[36] At the top of each column, I show a treatment cohort, and each is compared against the 2012 cohort. First, I would like to emphasize that before 2020 II, both treatment and control groups showed parallel trends and only diverged after the pandemic. Second, the pandemic caused a larger loss of employment among more recent Post-Great Recession cohorts. For example, the 2016 cohort (who were 26 in December 2020) saw a relative drop in formal employment of -1% (by 2020-III) relative to the 2012 cohort (29 in December

[34] Confederación de Trabajadores de Mexico. ¿Que hago si me piden mi renuncia voluntaria? http://ctm.mx/que-hago-si-me-piden-firmar-mi-renuncia-voluntaria/

[35] El top de las 10 empresas con mas demandas laborales en el DF. https://www.animalpolitico.com/2011/08/sme-entre-los-que-tienen-mas-demandados-laborales-en-el-df/

[36] Most coefficients become non-statistically significant for cohorts before 2012.

2019).[37] However, the relative drop in employment of the 2014 cohort (relative to the 2012 cohort) was 0.5%.

TABLE 1. INDIVIDUAL LEVEL EVENT STUDY AROUND THE ONSET OF THE PANDEMIC, BY COHORT

	Cohort				
	2008	2010	2012	2014	2016
2019 I	0.002*	0.001	0.001	0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
2019 II	0.001	0.001	-0.001	0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
2019 III	0.001	0.002*	0.001	0.002	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
2019 IV	0.000	0.001	0.001	0.002*	0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
2019 V	0.001	0.001	0.000	0.000	-0.003
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)
2019 VI	.	.	.	.	.
	.	.	.	.	.
2020 I	0.000	-0.001	-0.002	-0.002	-0.002
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
2020 II	-0.013***	-0.014***	-0.018***	-0.019***	-0.023***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
2020 III	-0.019***	-0.021***	-0.024***	-0.029***	-0.035***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
2020 IV	-0.020***	-0.021***	-0.025***	-0.027***	-0.034***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
2020 V	-0.019***	-0.021***	-0.024***	-0.027***	-0.030***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
2020 VI	-0.016***	-0.019***	-0.020***	-0.024***	-0.026***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2021 I	-0.016***	-0.019***	-0.022***	-0.026***	-0.029***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
2021 II	-0.015***	-0.020***	-0.022***	-0.026***	-0.029***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Age in Dec 2019	33	31	29	27	25
# of Workers In Dec 2019	42,455	41,077	51,405	57,659	65,585

Notes: The dependent variable is a dummy equal to 1 if the individual has an IMSS affiliated job in month t, 0 otherwise. All regressions include individual and bimester fixed effects, and a linear time trend controls (as described in equation 1). Observations before 2019 are used to identify seasonality. Regressions are estimated by using the cohort specified at the top of the column. To estimate such regressions, I use the reghdfe Stata command (Correia 2015) on individual level data. 2019 VI is the omitted period and therefore, no value is included for such period. Standard errors are clustered at the individual level.
***p-value<0.01, ** p-value <0.05, * p-value <0.1
Definitions: Each cohort is defined as all 22-year-olds who entered the market in a given year.
Source: IMSS administrative records.

[37] This effect is similar in size to the relative difference caused by the pandemic between men and women labor force participation in Mexico (2%, Juarez and Villaseñor, 2022) and America (1%; Couch et al. 2021).

TABLE 2. RELATIVE EFFECT OF THE PANDEMIC BY COHORTS

	Treatment Cohort				
	2013	2014	2015	2016	2017
2019 I	0.000	-0.001	0.001	0.001	0.002
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
2019 II	0.000	0.000	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2019 III	0.001	0.000	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2019 IV	-0.001	-0.001	-0.001	-0.001	-0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2019 V	0.000	0.000	0.000	0.000	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2019 VI	.	.	.	.	.
	.	.	.	.	.
2020 I	0.000	0.000	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2020 II	0.000	-0.003***	-0.004***	-0.005***	-0.009***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2020 III	-0.002*	-0.005***	-0.006***	-0.010***	-0.013***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2020 IV	-0.001	-0.003***	-0.005***	-0.011***	-0.012***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2020 V	0.000	-0.003***	-0.004***	-0.006***	-0.008***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
2020 VI	-0.002*	-0.004***	-0.004***	-0.005***	-0.008***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.002)
2021 I	-0.002*	-0.005***	-0.005***	-0.006***	-0.008***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
2021 II	-0.002*	-0.006***	-0.005**	-0.006***	-0.007***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
# of workers In December 2019	103,663	109,064	110,706	116,990	117,769
Age of Treatment Group in Dec 2019	28	27	26	25	24
Age of Control Group in Dec 2019	29	29	29	29	29

Notes: The dependent variable is a dummy equal to 1 if the individual has an IMSS affiliated job in month t, 0 otherwise. All regressions include individual and time fixed effects, as described in equation 2. Regressions are estimated by using as treatment the cohort at the top of the column, and as control the 2012 cohort. To estimate such regressions, I use the reghdfe Stata command (Correia, 2015). 2019 VI is the omitted period and therefore, no value is included for such period. Standard errors are clustered at the individual level. ***p<0.01, **p<0.05, *p<0.1
Definitions: Each cohort is defined as all 22-year-olds who entered the market each year.
Source: IMSS administrative records.

6. Heterogeneity

This section explores novel trends for different types of Post-Great Recession entrants. Overall, I find the following: First, cohort differences are the smallest in regions that suffered the largest number of job losses during the Great Recession. Second, recent cohorts are disproportionately fired in all industries. However, they are fired at higher rates in industries that rely on face-to-face interactions. Third, among high-income individuals, fewer and more recent cohorts are equally as likely to keep their jobs. Fourth, recent cohorts of women were more likely to lose their job. Fifth, I find that firm-specific experience ("tenure") was a key determinant of job loss among workers in the same cohort and age.

First, I separate my sample according to the geographic severity of the Great Recession Local Shocks. I replicate the Bartik instrument used in Campos-Vazquez et al. (2021) to measure the geographic distribution of Great Recession shocks.[38] This instrument relies on the fact that all local areas experienced the same secular nationwide shocks, but some local areas experienced more severe Great Recession shocks. For example, municipalities that rely heavily on exports were more affected. First, the geographic distribution of the Great Recession is shown in Figure A1.5. Then, I decompose the population according to the percentile of shock intensity and then run equation (2) for the 2016 and 2012 cohort on each subsample. Results are in Table 3. In columns 1-3, I show the sample decomposition by the size of the Great Recession's initial shock. In contrast, in the rest of the columns, I exclude US-border states from the sample to assess the possible role of the 2019 minimum wage law. Though I see some heterogeneity, it does not align with a story where the Great Recession compounded the effects of the Pandemic. The municipalities most impacted by the Great Recession do not seem to show differences in employment across cohorts. At the same time, I find that differences in employment by cohorts concentrated in the municipalities affected the least by the Great Recession. Finally, the results excluding border States where the minimum wage is higher do not seem particularly different from those using the full sample.

I also explore heterogeneity by industry in Table 4. In each column, I show the results of eq. (2) for individuals that in 2019 were employed in the industry shown at the top of each column. I find that the 2016 cohort lost their jobs at disproportionately higher rates in all sectors. The most unusual case is construction, as it was minimally affected. This is because construction was closed for the shortest time among all industries (3 months). I also observed

[38] The local shock is measured with the following formula:
$Local\ shock_m = \sum_j \left(\frac{emp_{j,m,2007}}{\sum_j emp_{j,m',2007}} \times \frac{\sum_{m' \neq m} emp_{j,m',2009} - \sum_{m' \neq m} emp_{j,m',2007}}{\sum_{m' \neq m} emp_{j,m',2007}}\right)$, for every municipality m and industry j. Emp is the IMSS-affiliated employment in m and j. For this instrument, each municipality's shock is equal to the projected percentage change in formal employment from 2008 to 2010 based on leave-one-municipality-out nationwide changes in employment by four-digit industry categories. See Campos-Vazquez et al. (2021) for more details and robustness tests on this instrument.

that the 2016 cohort's job loss was larger in industries that relied more heavily on face-to-face interactions (retail, restaurants, and hotels; columns 2-3).

TABLE 3. RELATIVE EFFECT OF THE PANDEMIC ON THE 2016 COHORT: HETEROGENEITY BY GEOGRAPHIC INTENSITY OF THE GREAT RECESSION

	25% Most Impacted Municipalities (1)	Percentiles 25-75 (2)	25% Less Impacted (3)	No Border States (4)
2019 I	0.010	-0.004	0.001	0.001
	(0.007)	(0.003)	(0.005)	(0.001)
2019 II	0.009	-0.004	0.004	-0.001
	(0.007)	(0.003)	(0.005)	(0.001)
2019 III	0.007	-0.005	0.008	0.001
	(0.007)	(0.003)	(0.005)	(0.001)
2019 IV	0.010	-0.005	0.001	0.001
	(0.007)	(0.003)	(0.005)	(0.001)
2019 V	0.007	-0.003	0.002	0.000
	(0.006)	(0.002)	(0.004)	(0.001)
2019 VI	.	.	.	.
	.	.	.	.
2020 I	0.004	-0.003	0.003	-0.001
	(0.006)	(0.002)	(0.004)	(0.001)
2020 II	-0.004	-0.006***	-0.007***	-0.005***
	(0.006)	(0.002)	(0.004)	(0.001)
2020 III	-0.008	-0.010***	-0.012***	-0.011***
	(0.006)	(0.002)	(0.004)	(0.001)
2020 IV	-0.007	-0.007***	-0.009***	-0.010***
	(0.006)	(0.002)	(0.004)	(0.001)
2020 V	-0.005	-0.006***	-0.012***	-0.005***
	(0.006)	(0.002)	(0.004)	(0.001)
2020 VI	-0.002	-0.005***	-0.008***	-0.004***
	(0.005)	(0.002)	(0.004)	(0.001)
2021 I	-0.006	-0.005***	-0.008***	-0.005***
	(0.005)	(0.002)	(0.004)	(0.001)
2021 II	-0.003	-0.004**	-0.008***	-0.006***
	(0.005)	(0.002)	(0.003)	(0.002)
# of workers in Dec 2019	17,648	55,237	34,105	106,608

Notes: The dependent variable is a dummy equal to 1 if the individual has an IMSS affiliated job in month t, 0 otherwise. All regressions include individual and time fixed effects, as described in equation 2. To estimate such regressions, I use the reghdfe Stata command (Correia, 2015). The title of the column refers to the subsample being studied. For example, "25% Most Impacted" refers to the subsample that was working in the municipalities that were among the 25% most impacted by the Great Recession (measured using Campos-Vazquez et al. 2021 methodology). The treatment group is the 2016 cohort, and the control group is the 2012 cohort. As Campos-Vazquez et al. 2021 instrument was created using only municipalities with more than 5,000 formal jobs in 2007, the results from columns 1-3 have the same restriction. 2019 VI is the omitted period and therefore, no value is included for such period. Standard errors are clustered at the individual level. ***p<0.01, **p<0.05, *p<0.1
Source: IMSS administrative records.

To explore heterogeneities across the earnings distribution, I categorize all workers by their position within the formal-sector wage distribution in 2019. I show the results in Table 5. Columns 1-3 include those individuals in the bottom quartile, the 25-75 percentile, and the top quartile (respectively). I find that recent cohorts have a lower probability of being employed when the Pandemic occurred for individuals at the lower end of the distribution, although the estimated coefficients are imprecisely estimated. For individuals in the 25-75 percentile, this employment loss of the cohorts closer to the Pandemic is more apparent and statistically significant. Finally, high-income recent cohorts were as likely as their long-tenured counterparts to remain formally employed.

TABLE 4. RELATIVE EFFECT OF THE PANDEMIC ON THE 2016 COHORT: HETEROGENEITY BY INDUSTRY

	Manufacture	Restaurants & Hotels	Retail	Construction
	(1)	(2)	(3)	(4)
2019 I	-0.001	0.007	-0.005	0.011
	(0.005)	(0.011)	(0.005)	(0.012)
2019 II	0.000	0.016	0.000	0.022
	(0.005)	(0.011)	(0.005)	(0.012)
2019 III	0.004	0.014	0.002	0.013
	(0.004)	(0.01)	(0.005)	(0.011)
2019 IV	-0.001	0.008	-0.002	0.020*
	(0.004)	(0.009)	(0.004)	(0.01)
2019 V	0.001	0.000	0.000	0.013
	(0.003)	(0.007)	(0.003)	(0.009)
2019 VI	.	.	.	.
	.	.	.	.
2020 I	-0.005	0.001	-0.004	0.014
	(0.003)	(0.007)	(0.003)	(0.008)
2020 II	-0.008**	-0.014***	-0.018***	-0.005
	(0.004)	(0.009)	(0.004)	(0.01)
2020 III	-0.017***	-0.025***	-0.023***	0.011
	(0.004)	(0.009)	(0.004)	(0.01)
2020 IV	-0.013***	-0.013	-0.019***	0.005
	(0.004)	(0.01)	(0.004)	(0.01)
2020 V	-0.014***	-0.02**	-0.018***	0.01
	(0.004)	(0.01)	(0.004)	(0.01)
2020 VI	-0.018***	-0.028***	-0.018***	0.002
	(0.004)	(0.01)	(0.005)	(0.011)
2021 I	-0.017***	-0.032***	-0.019***	0.011
	(0.005)	(0.01)	(0.005)	(0.011)
2021 II	-0.016***	-0.031***	-0.017***	0.015
	(0.005)	(0.011)	(0.005)	(0.011)
# Workers	36,569	10,146	40,966	10,359

Notes: The dependent variable is a dummy equal to 1 if the individual has an IMSS affiliated job in month t, 0 otherwise. All regressions include individual and time fixed effects, as described in equation 2. To estimate such regressions, I use the reghdfe Stata command (Correia, 2015). The title of the column refers to the subsample being studied. The treatment group is the 2016 cohort, and the control group is the 2012 cohort. 2019 VI is the omitted period and therefore, no value is included for such period. Standard errors are clustered at the individual level.

***p<0.01, **p<0.05, *p<0.1

Source: IMSS administrative records.

TABLE 5. RELATIVE EFFECT OF THE PANDEMIC ON THE 2016 COHORT, BY PRE-PANDEMIC WAGES

	(1) Bottom 25 Population Percentile	(2) 25-75 Population Percentile	(3) Upper 25 Population Percentile
2019 I	0.011	0.000	-0.001
	(0.006)	(0.004)	(0.004)
2019 II	0.010	0.003	-0.007*
	(0.006)	(0.004)	(0.004)
2019 III	0.010	0.005	-0.001
	(0.006)	(0.004)	(0.004)
2019 IV	0.007	-0.001	-0.01
	(0.005)	(0.003)	(0.003)
2019 V	0.002	0.001	-0.002
	(0.004)	(0.002)	(0.003)
2019 VI	.	.	.
	.	.	.
2020 I	0.002	-0.004	0.000
	(0.004)	(0.002)	(0.003)
2020 II	-0.008	-0.011***	-0.001
	(0.005)	(0.003)	(0.003)
2020 III	-0.011***	-0.015***	-0.005
	(0.005)	(0.003)	(0.004)
2020 IV	-0.008	-0.011***	-0.005
	(0.005)	(0.003)	(0.004)
2020 V	-0.008	-0.009***	-0.003
	(0.005)	(0.003)	(0.004)
2020 VI	-0.006	-0.010***	-0.003
	(0.005)	(0.003)	(0.004)
2021 I	-0.012**	-0.010***	0.000
	(0.006)	(0.004)	(0.004)
2021 II	-0.013***	-0.007	-0.001
	(0.006)	(0.004)	(0.004)
# of Workers in Dec 2019	30,552	59,098	27,340

Notes: The dependent variable is a dummy equal to 1 if the individual has an IMSS affiliated job in month t, 0 otherwise. All regressions include individual and time fixed effects, as described in equation 2. To estimate such regressions, I use the reghdfe Stata command (Correia, 2015). The title of the column refers to the subsample being studied. The treatment group is the 2016 cohort, and the control group is the 2012 cohort. 2019 VI is the omitted period and therefore, no value is included for such period. Standard errors are clustered at the individual level. In column 1, I regress equation 2 on workers who were at the bottom 25% earners within the population. In column 2, I show the impact for those who were between the 25-75th percentile of wages within the population. And then, in column 3, I show it for the top 25% of earners. For the top earners, I censored the 5% of wage earners.

***p<0.01, **p<0.05, *p<0.1

Source: IMSS administrative records.

Results by gender are in Table 6 and show that men and women from recent cohorts were more likely to lose employment. However, the pandemic impact was larger for women and it is taking longer for them to recover.

TABLE 6. RELATIVE EFFECT OF THE PANDEMIC ON THE 2016 COHORT, BY SEX

	(1) Men	(2) Women
2019 I	0.002	0.001
	(0.003)	(0.003)
2019 II	0.005	0.003
	(0.003)	(0.003)
2019 III	0.002	0.001
	(0.002)	(0.003)
2019 IV	0.001	-0.003
	(0.002)	(0.003)
2019 V	0.001	-0.001
	(0.002)	(0.002)
2019 VI	.	.
	.	.
2020 I	0.002	-0.002
	(0.002)	(0.002)
2020 II	-0.002	-0.008***
	(0.002)	(0.003)
2020 III	-0.006***	-0.010***
	(0.002)	(0.003)
2020 IV	-0.005***	-0.017***
	(0.002)	(0.003)
2020 V	-0.004**	-0.008***
	(0.002)	(0.003)
2020 VI	-0.001	-0.006***
	(0.003)	(0.003)
2021 I	-0.001	-0.005*
	(0.003)	(0.003)
2021 II	-0.003	-0.006**
	(0.003)	(0.003)
# of Workers in Dec 2019	65,889	51,101

Notes: The dependent variable is a dummy equal to 1 if the individual has an IMSS affiliated job in month t, 0 otherwise. All regressions include individual and time fixed effects, as described in equation 2. To estimate such regressions, I use the reghdfe Stata command (Correia, 2015). The title of the column refers to the subsample being studied. The treatment group is the 2016 cohort, and the control group is the 2012 cohort. 2019 VI is the omitted period and therefore, no value is included for such period. Standard errors are clustered at the individual level.
***p<0.01, **p<0.05, *p<0.1
Source: IMSS administrative records.

Finally, I would like to explore the role of firm-specific job tenure. To do so, I create an alternative identification strategy to study the role of firm-specific tenure among workers with the same entry year and age. I run the following regression:

(3) $A_{ift} = \alpha_t + \vartheta_i + \pi_f + \sum_{T \neq 2019VI} \beta_T \cdot 1(t = T) \times tr_{f=1} + \varepsilon_{ift}$.

Equation (3) is similar to (2), with two key differences: First, all the individuals used in the regression belong to the same cohort (2015 cohort)[39] and have the same age, and second, they differ in firm-specific experience (π_f). Results are shown in Table 7. In column 1, I compare those with 2 years of firm-specific experience (treatment) against those with 3 years (control).[40] In column 2, I compare those with 2 years months of firm-specific experience (treatment) against those with 4 years. In column 3, I compare those with 2 years of firm-specific experience (treatment) against those with more than 4 years. These regressions show that workers from the same cohort and age but with more firm-specific experience were more likely to stay formally employed than those with less firm-specific work experience.

[39] I choose 2015 because it allows me to have multiple years of firm experience between treatment and control.
[40] I do not include those with less than a year of firm-specific experience as they have a higher pre-pandemic initial probability of losing their job than any other group (which makes it impossible to hold parallel trends). This is probably a result of the common practice of using the first year of employment as a test of fit.

TABLE 7. RELATIVE EFFECT OF THE PANDEMIC ON WORKERS FROM THE SAME COHORT WHO DIFFER BY YEARS OF FIRM-SPECIFIC EXPERIENCE

	Cohorts by Year of Entry		
	(1)	(2)	(3)
2019 I	0.000	-0.003	-0.009
	(0.003)	(0.003)	(0.006)
2019 II	-0.001	-0.003	-0.008
	(0.003)	(0.003)	(0.006)
2019 III	-0.002	-0.003	-0.005
	(0.003)	(0.003)	(0.006)
2019 IV	0.000	0.000	-0.004
	(0.002)	(0.003)	(0.005)
2019 V	0.001	-0.001	0.000
	(0.002)	(0.002)	(0.004)
2019 VI	.	.	.
	.	.	.
2020 I	-0.001	-0.004	0.002
	(0.003)	(0.003)	(0.007)
2020 II	-0.014***	-0.021***	-0.035***
	(0.004)	(0.004)	(0.008)
2020 III	-0.027***	-0.039***	-0.054***
	(0.005)	(0.005)	(0.01)
2020 IV	-0.032***	-0.045***	-0.06***
	(0.005)	(0.005)	(0.011)
2020 V	-0.032***	-0.045***	-0.055***
	(0.005)	(0.006)	(0.011)
2020 VI	-0.041***	-0.048***	-0.06***
	(0.005)	(0.006)	(0.012)
2021 I	-0.034***	-0.048***	-0.068***
	(0.005)	(0.006)	(0.012)
2021 II	-0.036***	-0.05***	-0.072***
	(0.006)	(0.006)	(0.012)
# Of workers in 2019 Dec	19,909	17,688	21,704

Notes: The dependent variable is a dummy equal to 1 if the individual has an IMSS affiliated job in month t, 0 otherwise. To estimate these regressions I use individuals from the 2015 cohort and ran equation (3). In all columns, the treatment group are workers with 2 years of firm-specific experience by 2019 VI. The control varies by column: Column 1 uses those with 3 years of firm-specific experience, column 2 uses those with 4 years of firm-specific experience and column 3 uses those with more than 4 years of firm specific experience. All regressions use individual and time fixed effects. ***p<0.01, **p<0.05, *p<0.1

Source: IMSS administrative records.

7. Robustness Checks

For brevity, I will describe the tests I perform in general terms. However, I relegate the analysis to the Appendix.

Varying the Definition of Control and Treatment Group

One of the concerns raised in the previous section concerns the way that treatment and control groups are defined. For example, I focus my attention on those workers that entered the formal labor market at 22. For my analysis to be robust, I should show that my results are similar for workers that entered the formal labor market at 23 or a different age. In order to do this, I simply vary the definition of treatment and control group by varying the age at entry. See Appendix 2.

Vary the definition of the dependent variable

I also vary how the dependent variable is defined. The reason behind this action is that it might be possible to assess how the formal labor market adjusted to the pandemic by changing the dependent variable. For example, suppose I define the dependent variable as the probability of being employed in the last pre-pandemic municipality. In that case, the difference between this outcome and the dependent variable in the main text (a dummy equal to 1 if the individual has an IMSS-affiliated job) should tell us the role of migration. I use different dependent variables (the probability of being formally employed with the last pre-pandemic firm, etc.) and show that migration and re-hires had minimal roles in the recovery. See Appendix 2 for these results.

Parallel Trends, Clustering Strategies and Oster (2019) test

I test for parallel trends in several outcomes (wages, rehires, firm changes). These tests show that adjacent cohorts were receiving wage increases, changing firms and being rehired at similar rates before the onset of the pandemic.

I also change the clustering strategy. The reason to vary the clustering strategy is that the pandemic had a differentiated impact on specific industries and regions. Therefore, it might be reasonable to assume that treatment was given at another combination (industry, municipality, time, etc.). Therefore, I adopt different strategies in order to show that my results are not particularly sensitive to choosing one strategy over the other.

Oster's (2019) test assesses the relevance of omitted variable bias. This test argues that omitted variable bias should be limited if coefficient movements are limited after the inclusion of controls.[41] I estimated this test for all the positive β and found that my results are not likely driven by omitted variable bias.

[41] Under the assumption that selection on observables is informative about selection on unobservables.

Endogenous Labor Market Entry

I follow Schwandt and von Wachter's (2019) suggested endogenous labor market entry tests. These tests are the following: First, I run the percentage of college or high school graduation on an interaction between the informal rate at labor market entry-age and the years since expected graduation. These coefficients (if statistically significant) should tell us if the graduation rate responds to the economic cycle. Second, I test for compositional changes between cohorts. For example, I test if the 2016 cohort has more individuals aged 22 or older. These tests show that the initial informal employment rate has a statistically significant impact on graduation rates. However, such an effect is not large enough to change cohorts' composition.

8. Conclusion

This paper shows that the younger Post-Great Recession entrants (24–29-year-olds in 2020) were more likely to lose employment due to the COVID-19 pandemic than older Post-Great Recession entrants (30 to 35 years-old individuals in 2020). Among my results, I would like to highlight a few:

1. Cohort differences were not the largest in regions that lost the most formal jobs during the Great Recession.
2. More recent cohorts were more likely to lose employment across many industries but were particularly at risk in industries that required face-to-face interactions.
3. Younger women were disproportionately affected.
4. Shorter firm-specific tenure and low pre-pandemic wages were associated with job loss among more recent cohorts during the pandemic.

Overall, my results also show that the employment conditions of recent cohorts are improving as the economy recovers as of date. However, these differences are closing more slowly for vulnerable groups (younger women, for example), and the cohort differences remain as of the writing of this paper. It is still too early to know the long-term impact of the pandemic, but we have seen improvements since the start of 2021.

Finally, this paper also presents evidence of the differentiated impact of recessions on the employment of workers with different tenure profiles in a developing country.

References

Abadie, Alberto, Susan Athey, Guido Imbens, and Jeffrey Wooldridge. 2017. "When Should You Adjust Standard Errors for Clustering?" NBER Working Paper 24003.

Abraham, Katharine G., and James L. Medoff. 1984. "Length of Service and Layoffs in Union and Nonunion Work Groups." *Industrial and Labor Relations Review* 38(1): 87–97.

Albagli, Elías, Gabriela Contreras, Matías Tapia, and Juan M. Wlasiuk. 2018. "Wage Cyclicality of New and Continuing Jobs." Working Paper, Central Bank of Chile.

Alon, Titan, Doepke, Matthias, Olmstead-Rumsey, Jane and Michèle Tertilt. 2020. "The Impact of COVID-19 on Gender Equality." NBER Working Paper No. 26947

Altonji, Joseph G., and Robert A. Shakotko. 1987. "Do Wages Rise with Job Seniority?" *Review of Economic Studies* 54(3): 437–459.

Angelucci, Manuela, Marco Angrisani, Daniel M. Bennett, Arie Kapteyn, and Simone G. Schaner. 2021. "Remote Work and the Heterogeneous Impact of COVID-19 on Employment and Health." NBER Working Paper 27749**.**

Bacher-Hicks, Andrew, Joshua Goodman, and Christine Mulhern. 2021. "Inequality in Household Adaptation to Schooling Shocks: Covid-induced Online Learning Engagement in Real Time." *Journal of Public Economics* 193.

Bachmann, Ronald, and Rahel Felder. 2018. "Job Stability in Europe over the Cycle." *International Labour Review* 157(3): 481-518.

Banco de Mexico. 2012. Banco de Mexico Quarterly Report IV. Mexico. https://www.banxico.org.mx/publicaciones-y-prensa/informes-trimestrales/recuadros/recuadros-informe-trimestral-001.html

Banco de Mexico. 2020. Banco de Mexico Quarterly Report IV. Mexico. https://www.banxico.org.mx/publicaciones-y-prensa/informes-trimestrales/recuadros/recuadros-informe-trimestral-001.html

Banco de Mexico. 2022. Banco de Mexico Quarterly Report 2021 IV. Mexico. https://www.banxico.org.mx/publicaciones-y-prensa/informes-trimestrales/recuadros/recuadros-informe-trimestral-001.html

Beaudry, Paul and John Dinardo. 1991. "The Effect of Implicit Contracts on the Movement of Wages Over the Business Cycle: Evidence from Micro Data." *Journal of Political Economy*. Volume 99, Number 4Aug., 1991

Borgschulte, Mark, and Paco Martorell. 2018. "Paying to Avoid Recession: Using Reenlistment to Estimate the Cost of Unemployment." *American Economic Journal: Applied Economics* 10(3): 101-127.

Brodeur, Abel, David Gray, Anik Islam, and Suraiya Bhuiyam. 2021. "A Literature Review of the Economics of COVID-19." *Journal of Economic Surveys* 35(4): 1007-1044.

Caliendo, Marco, and Ricarda Schmild. 2016. "Youth Unemployment and Active Labor Market Policies in Europe." *IZA Journal of Labor Policy*.

Campa, Pamela, Roine, Jesper and Svante Strömberg. 2021. "Unemployment Inequality in the Pandemic: Evidence from Sweden." *Covid Economics* (83)*:* 1-24.

Campos-Vazquez Raymundo, Goshi, Priyasmita and Eduardo Medina Cortina. 2021. "Long-lasting Consequences of a Depressed Labor Market: Evidence from Mexico After the Great Recession". Working Paper.

Chetty, Raj, John N. Friedman, Nathaniel Hendren, Michael Stepner, and The Opportunity Insights Team. 2020. "The Economic Impacts of COVID-19: Evidence from a New Public Database Built Using Private Sector Data." NBER Working Paper 27431.

Chatterji, Pinka, and Yue Li. 2021. "Recovery from the COVID-19 Recession: Uneven Effects Among Young Workers?" NBER Working Paper 29307.

Costa Dias, Monica, Robert Joyce, and Agnes Norris Keiller. 2020. "COVID-19 and the Career Prospects of Young People." IFS Briefing Note BN299.

Correia, Sergio. 2015. "Linear Models with High-Dimensional Fixed Effects: An Efficient and Feasible Estimator." Working Paper available at http://scorreia.com/research/hdfe.pdf.

Cowan, Benjamin and Kairon Shayne-Garcia. 2021. “How has COVID-19 affected young workers?” CESifo Forum 4 / 2021 July Volume 22.

Couch, Kenneth, Fairlie, Robert, and Huanan Xu. 2021. “The Evolving Impacts of the COVID-19 Pandemic on Gender Inequality in the U.S. Labor Market: The COVID Motherhood Penalty.” NBER Working Paper No. 29426

Diario Oficial de la Federación. Acuerdo del 14 de Mayo de 2020. https://dof.gob.mx/nota_detalle.php?codigo=5593313&fecha=14/05/2020. DOF: 14/05/2020

Gertler, Mark, Huckfeld, Christopher and Antonella Trigar. 2022. “Unemployment Fluctuations, Match Quality, and the Wage Cyclicality of New Hires.” *Review of Economic Studies*. Forthcoming.

Heffetz, Ori, and Daniel Reeves. 2020. “Measuring Unemployment in Crisis: Effects of COVID-19 on Potential Biases in the CPS.” NBER Working Paper 28310.

Jaimovich, Nir and Henry Siu. 2020. “Job polarization and jobless recoveries.” *Review of Economics and Statistics*. 102 (1): 129-147.

Juarez, Laura and Paula Villaseñor. 2022. “Effects of the COVID-19 Pandemic on the Labor Market Outcomes of Women with Children in Mexico.” Working paper.

Jovanovic, Boyan. 1979. “Firm-specific Capital and Turnover.” *Journal of Political Economy* 87(6): 1246–1260.

Levy, Santiago and Luis Felipe Lopez-Calva. 2019. “Persistent Misallocation and the Returns to Education in Mexico.” World Bank Policy Research Working Paper No. 8690.

Leyva, Gustavo and Israel Mora. 2021. “How High (Low) are the Possibilities of Teleworking in Mexico?” Banco de Mexico Working Paper 2021-15.

Matias-Cortes, Guido and Diego M. Morris. 2022. **“**Are Routine Jobs Moving South? Evidence from Changes in the Occupational Structure of Employment in the U.S. and Mexico.” Working Paper.

Molloy, Raven, Christopher, Smith and Abigail Wozniak. 2021. “Changing Stability in U.S. Employment Relationships: A Tale of Two Tails.” *Journal of Human Resources*. 0821-11843; published ahead of print. November 15, 2021.

Ohnsorge, Franziska, and Shu Yu. 2022. “The long shadow of informality: Challenges and policies.” World Bank Publications.

Oster, Emily. 2013. "PSACALC: Stata module to calculate treatment effects and relative degree of selection under proportional selection of observables and unobservables," Statistical Software Components S457677, Boston College Department of Economics, revised 18 Dec 2016.

Oster, Emily. 2019. “Unobservable Selection and Coefficient Stability: Theory and Evidence.” *Journal of Business & Economic Statistics* 37(2): 187-204.

Puggioni, Daniela, Mariana Calderón, Alfonso Cebreros Zurita, León Fernández Bujanda, José Antonio Inguanzo González, and David Jaume. 2022. “Income Dynamics and Inequality: The Case of Mexico.” *Quantitative Economics*, forthcoming.

Rothstein, Jesse. 2021. “The Lost Generation? Labor Market Outcomes for Post Great Recession Entrants.” *Journal of Human Resources* 0920-11206R1.

Schwandt, Hannes, and Till von Wachter. 2019. “Unlucky Cohorts: Estimating the Long-Term Effects of Entering the Labor Market in a Recession in Large Cross-Sectional Data Sets.” *Journal of Labor Economics* 37(1): 161-198.

Snell, Andy, Pedro Martins, Heiko Stüber, and Jonathan P. Thomas. 2018. “Bias in Returns to Tenure When Firm Wages and Employment Comove: A Quantitative Assessment and Solution.” *Journal of Labor Economics* 36(1): 47-74.

Swarnali Ahmed Hannan, Keiko Honjo and Mehdi Raissi. 2020. “Mexico Needs A Fiscal Twist: Response to Covid-19 and Beyond.” International Monetary Fund. https://www.imf.org/-/media/Files/Publications/WP/2020/English/wpiea2020215-print-pdf.ashx

von Watcher, Till. 2020. “Lost Generations: Long-Term Effects of the COVID-19 Crisis on Job Losers and Labour Market Entrants, and Options for Policy.” *Fiscal Studies* 41(3): 549-590.

World Health Organization. Regional Office for Europe. 2021. “A Timeline of WHO’s Response to COVID-19 in the WHO European Region: a Living Document.” Version 2.0 from 31 December 2019 to 31 December 2020). World Health Organization.

Regional Office for Europe. https://apps.who.int/iris/handle/10665/339983. Licencia: CC BY-NC-SA 3.0 IGO

Appendix

Appendix A1. Descriptive Statistics

In this Appendix, I provide descriptive statistics for the dataset of entrants into the formal labor market I created using IMSS administrative data. I also include descriptive data using the ENOE dataset.

Table A1.1 shows various cohorts' demographic and industry characteristics (I start with 2008 for brevity). Panel A includes the characteristics of each cohort at the beginning of their formal labor market history: All cohorts have similar initial gender composition and average wages. However, the industry composition is slightly different: recent cohorts are slightly more likely to be employed in restaurants and hotels. It is also worth noting that recent cohorts tend to be slightly larger. Panel B shows similar characteristics for the same cohorts but in December 2019, the last observation of the previous year before the pandemic. Again, we see a pattern: there is a selection process through time. Older cohorts have a slightly larger proportion of men, and the remaining members are less likely to work in the restaurant industry. Moreover, their wages have increased with time (however, there is large variance in wages, and thus, different cohorts' average wages are not statistically different). This reflects larger issues: an attrition process during the initial years of formal employment tenure, which changes the industry and gender composition.

Figure A1.1 uses ENOE data and graphs the informal rate for young workers (22-27 year-olds) for 2006-2017. It shows that, for all cohorts, the initial informal rate stayed approximately within +/- 1/2 percentage points of each other (with two exceptions) for adjacent cohorts. This implies that the formal market's selection mechanism has been stable over the years, and suggests that cohort composition has been relatively unchanged.

Figures A1.2-A1.3 are similar: they show an indicator of interest, by age, during a pre-pandemic year. The idea is that these indicators vary with time and are not observable in the IMSS data, and if age-adjacent groups are similar on these observables just before the pandemic, then any concern about time-varying characteristics ought to be minimal. Figure A1.2 deals with the number of weekly working hours (ENOE data): we can observe that workers younger than 25 work fewer hours. However, the number of hours stabilizes after

25. Curiously, 25 is the age of the 2016 cohort at the onset of the pandemic. Figure A1.3 shows the percentage of individuals who have a single job. There are two facts to notice concerning this graph: the number of individuals with a single job is relatively high (>95%), and this number is exceptionally high for workers younger than 25.

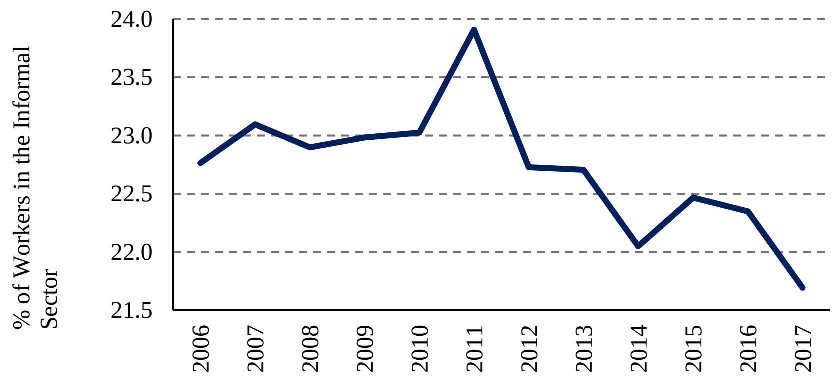
The last Figure in this Appendix replicates the Instrument presented in Campos-Vazquez et al. (2021) and shows the geographic distribution of Great Recession shocks in Mexico. This decomposition is used in the heterogeneity analysis to establish whether the areas affected the most by the Great Recession hysteresis were also the areas where younger workers were more harshly affected.

TABLE A1.1 COHORTS' SUMMARY STATISTICS

	Cohort Year of Entry									
	2008	2009	2010	2011	2012	2013	2014	2015	2016	2017
Panel A. By First Cohorts' December										
%Men	51.86	51.13	51.99	53.18	53.32	53.13	53.57	52.70	51.85	51.34
%Retail	26.32	26.55	26.49	27.16	26.61	27.08	25.65	25.47	24.90	24.04
%Restaurants and Hotels	8.25	8.02	7.94	7.89	8.07	8.34	8.27	8.80	9.22	8.93
Average Wage (Real)	186.12	179.29	184.88	182.72	181.04	177.90	178.52	184.40	187.46	188.57
	(133.33)	(128.29)	(135.31)	(132.30)	(132.46)	(129.80)	(130.81)	(137.19)	(131.87)	(128.42)
Number of Workers	63,865	50,303	60,768	66,792	74,631	74,394	81,232	82,480	89,393	89,490
Year of birth	1986	1987	1988	1989	1990	1991	1992	1993	1994	1995
Panel B. By December 2019										
%Male	57.52	56.83	57.53	58.21	58.26	57.19	57.12	55.96	54.80	53.44
%Retail	21.13	21.39	21.00	21.25	22.07	21.58	21.60	21.62	21.39	21.90
%Restaurants and Hotels	5.44	5.31	5.27	5.42	5.82	6.23	6.44	6.90	7.31	7.73
Average Wage	485.42	474.26	467.04	451.89	425.07	399.64	378.81	355.37	332.41	301.53
	(476.14)	(461.99)	(449.95)	(428.91)	(401.31)	(368.34)	(344.85)	(281.66)	(281.66)	(241.36)
Age	33	32	31	30	29	28	27	26	25	24
Number of Workers	42,455	33,308	41,077	45,807	51,405	52,258	57,659	59,301	65,585	66,364

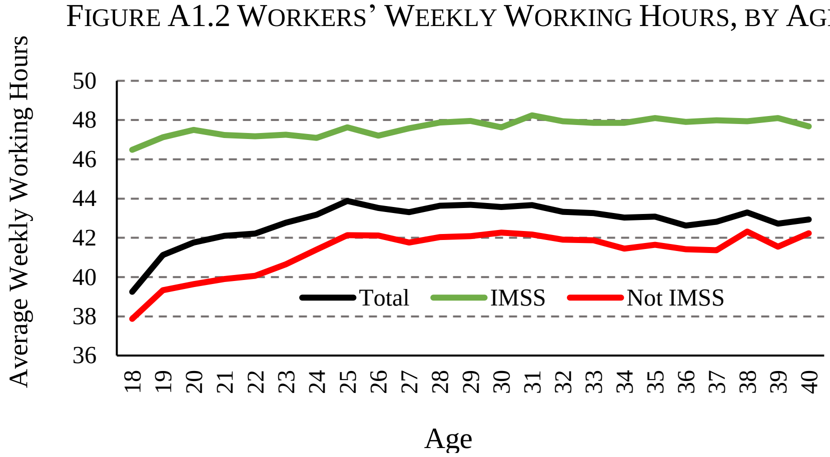
Notes: For this paper, a cohort is a group of workers who, at 22 years of age, entered the formal labor market during a given year. Standard errors for wages in parenthesis.
Sources: IMSS administrative data.

FIGURE A1.1 END OF-YEAR INFORMAL RATE FOR INDIVIDUALS AGED 22-27, BY YEAR



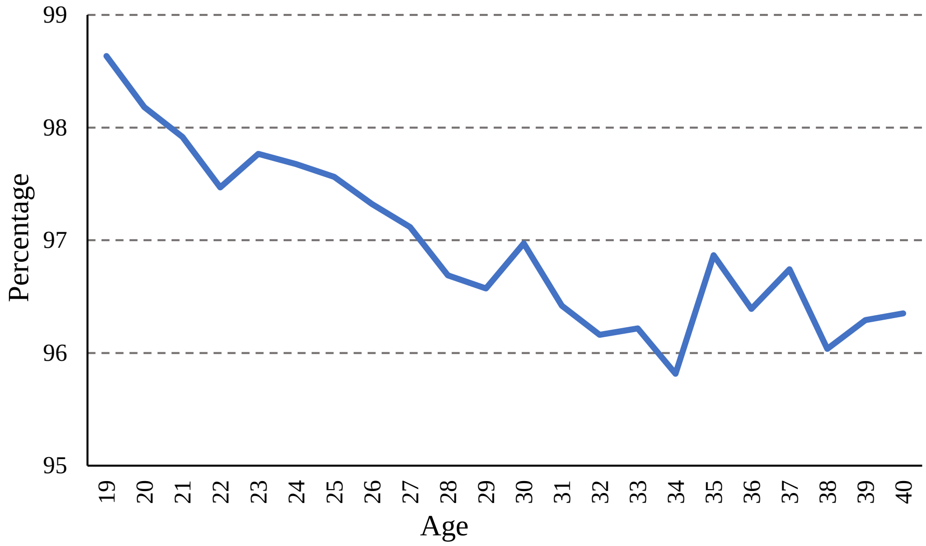
Notes: This is the Informal Employment Rate for workers aged 22-27 in the last quarter of each respective year.
Source: ENOE.

FIGURE A1.2 WORKERS' WEEKLY WORKING HOURS, BY AGE



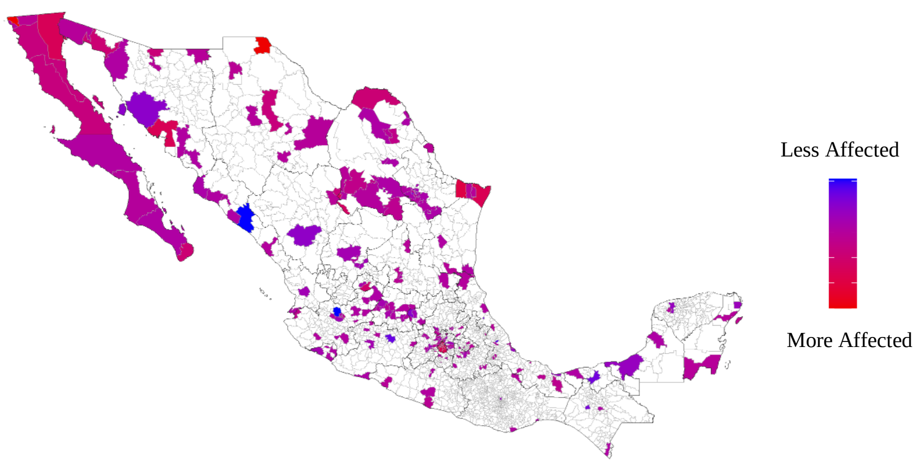
Source: ENOE (2019).

FIGURE A1.3 PROPORTION OF FORMAL WORKERS WITH A SINGLE JOB



Source: ENOE (2019).

FIGURE A1.4 GEOGRAPHIC DISTRIBUTION OF GREAT RECESSION SHOCKS ON LARGE LOCAL LABOR MARKETS



Note: This Figure depicts the geographical distribution of the standardized Great Recession local shocks (in blue, the less affected municipalities; in red, the most affected) in Mexican municipalities considered to be large labor markets, defined as municipalities with 5,000 or more formal workers each month during 2007. The data required for this map was replicated using the methodology from Campos-Vazquez et al. (2021). *Source*: IMSS administrative data (2019 and 2020).

Appendix A2. Robustness Checks

In this Appendix I perform the following Robustness Checks: First, to assess the coefficient sensitivity to changing the definition of treatment and control groups, I vary the age at entry. Second, to assess whether my results are sensitive to the definition of the dependent variable, I change it. Third, I test for parallel trends on other outcomes, try different clustering strategies and perform Oster's (2019) test for omitted variable bias. These tests show that, overall, my results are robust.

Section 1. Varying Treatment and Control Groups

In this section I test for robustness to using different definitions for cohorts. The logic behind these tests is that my results should not be valid contingent on using a specific age at entry. If this were the case, then it would imply that my results were likely driven by an unobservable characteristic of those workers who entered the formal labor market at 22 years of age. In Table A2.1 I show these results. For brevity, I limit my analysis to comparing the 2012 and 2016 cohorts. In column 1, I define each cohort as those individuals that at 21 years of age enter the formal market in a given year. In column 2 I vary the age at entry to 23. In column 3, I show the original cohort definition (22 years old). Column 4 shows everyone that enters the market in a given year. Please note that these results were estimated using a 33% random sample of all IMSS workers.[42] The results show that the magnitudes of the impact of the pandemic vary depending on which definition is used, but qualitatively the results are similar. For example, in all columns there are parallel trends, and the impact of the pandemic starts in 2020 II. Moreover, these differences are not particularly large: for example, in 2020 III, these coefficients vary by a maximum of 0.004.

[42] The reason to do so is limited data storage capability.

TABLE A2.1 RELATIVE EFFECT OF THE PANDEMIC ON THE 2016 COHORT, DIFFERENT COHORT DEFINITIONS

	Cohorts by Year of Entry			
	(1)	(2)	(3)	(4)
2019 I	-0.001	0.003	0.001	0.001
	(0.001)	(0.002)	(0.001)	(0.001)
2019 II	0.000	0.002*	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
2019 III	0.000	0.001	0.001	0.002*
	(0.001)	(0.001)	(0.001)	(0.001)
2019 IV	0.000	0.001	-0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
2019 V	0.002*	0.002*	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
2019 VI	.	.	.	.
	.	.	.	.
2020 I	0.002*	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
2020 II	-0.005***	-0.007***	-0.005***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
2020 III	-0.009***	-0.01***	-0.010***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
2020 IV	-0.009***	-0.009***	-0.011***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
2020 V	-0.005***	-0.007***	-0.006***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
2020 VI	-0.004***	-0.007***	-0.005***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
2021 I	-0.005***	-0.007***	-0.006***	-0.005***
	(0.002)	(0.002)	(0.001)	(0.001)
2021 II	-0.006***	-0.007***	-0.006***	-0.004***
	(0.002)	(0.002)	(0.002)	(0.001)

Notes: See Notes from Table 2. The definition of a cohort changes in each column. Column 1 defines cohort as those 21-year-olds who enter the formal labor market in a given year. Column 2 defines cohort as those 23-year-olds who enter the formal labor market in a given year. Column 3 defines cohort as those 22-year-olds who enter the formal labor market in a given year (same as in the main text). Column 4 includes everyone who enters to the market in a given year. For comparability, I repeated these regressions on the 2016 and 2012 cohort. To estimate such regressions, I use the reghdfe Stata command (Correia, 2015) on a random 33% sample of all IMSS workers. ***p<0.01, **p<0.05, *p<0.1
Source: IMSS administrative records.

Section 2. Varying the outcome of interest.

In this section I vary how the dependent variable is defined. For most of the paper, the dependent variable of interest is a dummy equal to 1 if the worker has an IMSS affiliated job, 0 otherwise. For Table A2.2, I keep this variable for comparison (column 4). However, I compare the results of running equation (2) on the original dependent outcome to the results of running equation (2) on different outcomes. For example, a dummy equal to 1 if the worker has an IMSS affiliated job in the same previous pre-pandemic municipality. This measure, when compared to the dummy of the main analysis (a dummy equal to one if he has an IMSS affiliated job, column 4) should tell us the role of migration. More specifically, I do the following: In column 1, the dependent variable is a dummy equal to 1 if the individual is employed in the same municipality as they were in 2019. In column 2, the dependent variable is the probability of being employed with the last pre-pandemic employer. The difference between the results from this equation and that of column 4 should tell us the role of re-hires. Finally, in column 3, the dependent variable is the probability of being continuously formally employed. The difference between the results from this equation and that of column 4 should tell us how many individuals lost their job and are finding new job opportunities. Overall, column 1 and 4 are quite similar, showing that the role of migration is minimal. Columns 2 and 4 show that coefficients from column 2 are more negative, stating that re-hires had a significant (but still small) effect on the recovery (the recovery was faster for the regression that considers employment with new employers, column 4). Column 3 and 4 show that the probability of being continuously formally employed fell more drastically than the probability of being formally employed, which indicates that most of the recovery comes from individuals who stayed at least a couple of months out of the formal labor force and eventually rejoined.

TABLE A2.2 RELATIVE EFFECT OF THE PANDEMIC ON THE 2016 COHORT, DIFFERENT OUTCOME DEFINITIONS

	(1)	(2)	(3)	(4)
2019 III	0.000	0.000	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
2019 IV	-0.002	-0.002	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
2019 V	0.001	0.001	0.001	0.000
	(0.001)	(0.001)	(0.001)	(0.001)
2019 VI	.	.	.	.
	.	.	.	.
2020 I	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
2020 II	-0.004***	-0.004***	-0.004***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
2020 III	-0.009***	-0.009***	-0.012***	-0.010***
	(0.001)	(0.001)	(0.001)	(0.001)
2020 IV	-0.011***	-0.014***	-0.013***	-0.011***
	(0.001)	(0.001)	(0.001)	(0.001)
2020 V	-0.005***	-0.011***	-0.013***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
2020 VI	-0.004***	-0.013***	-0.014***	-0.005***
	(0.001)	(0.001)	(0.001)	(0.001)
2021 I	-0.005***	-0.012***	-0.013***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)
2021 II	-0.005***	-0.012***	-0.012***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.002)
# of workers in December 2019	116,990	116,990	116,990	116,990

Notes: See Notes on Table 2. I decompose the definition of the dependent variable into the following: the probability of keeping a formal job in the last municipality of employment (column 1, a dummy equal to 1 if the worker is employed as in the last pre-pandemic municipality, 0 otherwise), the probability of being employed by their last pre-pandemic employer (column 2), the probability of staying continuously formally employed (column 3). Finally, a dummy equal to 1 if the worker has a formal job (column 5).
Source: IMSS administrative records.

Section 3. Other tests

In Table A2.3 I present parallel trends for 3 additional outcomes: ln(1+wages),[43] the probability of staying with the previous month's firm (a dummy equal to 1 if the individual stays with the previous month's employer, 0 otherwise) and the probability of staying formally employed in the previous month's municipality (a dummy equal to 1 if the worker is employed in the same municipality, 0 otherwise). I focus on the 2016 and 2012 cohorts for brevity. Because the pandemic caused differences in attrition between cohorts, I do not think I can study causally the impact of the pandemic on these outcomes. Therefore, I focus on the months before the pandemic (where no significant attrition differences occurred). These results show that before the pandemic, wages, firm changes and migration rates were following parallel trends between treatment and control groups.

Table A2.4 performs Oster's (2019) test. This test argues that if omitted variable bias is driving the results, the β-coefficients should be particularly sensitive to using different sets of controls. Therefore, the first part of the test is to change the controls used in the regression and observe changes in β-coefficients and R^2. Column 1 starts with time and cohort fixed effects, and the following columns start adding controls. Column 2 adds the "occupation-proxy fixed effects" (2019 VI industry-by-firms' wage quintile). Column 3 adds municipality fixed effects. Column 4 adds 2019 VI firm fixed effects. Column 5 adds individual fixed effects. The differences between these columns show that β-coefficients are quite stable even under specifications that result in a wide range of R^2.

The last column is a formalization of this argument: I estimate the Oster (2019) δ using the STATA software psacalc and an R-max of 1 on the regression in column. The δ assesses the expected relevance that an omitted variable should have to drive a statistically significant coefficient to zero. For example, a δ=2 implies that for β to be equal to 0, unobservable variables should be at least twice as important as observable variables. I estimated these values for all statistically significant β and found δ with values larger than one. This suggests that my results are not likely driven by omitted variable bias.

[43] IMSS dataset only reports paid employment. That said, this test is not particularly sensitive to using ln(wages) or another variable transformation.

Finally, Table A2.5 tries different clustering strategies. In the main text I argued that there might be reasons to believe that the clustering strategy could be assigned at the industry or locality level. At the top of each column, I show the clustering strategy being used and show that my analysis is not particularly sensitive to using different strategies.

TABLE A2.3 RELATIVE EFFECT OF THE PANDEMIC ON THE 2016 COHORT, OTHER OUTCOMES (PARALLEL TRENDS TEST)

	P(Being in the previous-month firm)	P(Working in a different municipality)	ln(Daily wage)
2019 I	0.001	0.001	-0.013
	(0.001)	(0.001)	(0.008)
2019 II	0.001	0.001	-0.001
	(0.001)	(0.001)	(0.007)
2019 III	0.000	0.000	-0.007
	(0.001)	(0.001)	(0.007)
2019 IV	-0.002	-0.002	-0.011
	(0.001)	(0.001)	(0.007)
2019 V	0.001	0.001	-0.005
	(0.001)	(0.001)	(0.004)
2019 VI	.	.	.
	.	.	.
# of workers In December 2019	116,990	116,990	116,990

Notes: See Notes on Table 2. I tested for parallel trends during 2019 for three outcomes of interest (each described at the top of the column). For months where the individuals were not employed, I input a value of 0 to the monthly wage. To estimate such regressions, I use the reghdfe Stata command (Correia, 2015). ***p<0.01, **p<0.05, *p<0.1
Source: IMSS administrative records.

TABLE A2.4 RELATIVE EFFECT OF THE PANDEMIC ON THE 2016 COHORT: ROBUSTNESS CHECKS BY CHANGING CONTROLS

	Cohort and Time Controls	Cohort and Time Plus Occupation Proxy Fixed Effects	Cohort and Time Plus Municipality Fixed Effects	Cohort and Time, Plus Firm Fixed Effects	Time Plus Individual Fixed Effects	Oster (2019) │δ│ for β in (5) to be equal to 0
	(1)	(2)	(3)	(4)	(5)	(6)
2019 II	0.001	0.000	0.000	-0.001	0.001	
	(0.001)	(0.002)	(0.003)	(0.003)	(0.001)	
2019 III	0.002	0.002	0.002	0.001	0.001	
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
2019 IV	0.001	0.000	0.000	0.000	0.001	
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
2019 V	-0.001	-0.003	-0.003	-0.003	-0.001	
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
2019 VI	.	.	.	.	.	
	.	.	.	.	.	
2020 I	-0.001	-0.001	-0.001	-0.001	-0.001	
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	
2020 II	-0.005***	-0.010***	-0.010***	-0.007***	-0.005***	3.33
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
2020 III	-0.01***	-0.011***	-0.014***	-0.012***	-0.010***	2.48
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
2020 IV	-0.009***	-0.014***	-0.013***	-0.013***	-0.011***	2.13
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
2020 V	-0.006***	-0.013***	-0.010***	-0.008***	-0.006***	2.19
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
2020 VI	-0.005***	-0.011***	-0.012***	-0.009***	-0.005***	2.27
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
2021 I	-0.006***	-0.011***	-0.009***	-0.008***	-0.006***	2.54
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
2021 II	-0.006***	-0.009***	-0.009	-0.008***	-0.004***	1.89
	(0.001)	(0.002)	(0.002)	(0.002)	(0.001)	
R2	0.013	0.04	0.34	0.34	0.75	

Notes: See Notes on Table 2. In this table I run the same regression (equation 2) and vary the controls (the controls are specified at the top of each column, in all cases the dependent variable is a dummy equal to 1 if the individual has an IMSS affiliated job). Occupation Proxy Fixed Effects are an industry-by-firms' wage quintile Fixed Effects. I estimated the Oster (2019) δ using the STATA software psacalc and a R-max of 1 on the regression in column (5). ***p<0.01, **p<0.05, *p<0.1
Source: IMSS administrative records.

TABLE A2.5 RELATIVE IMPACT OF THE COVID-19 PANDEMIC ON THE 2016 COHORT: DIFFERENT CLUSTERING STRATEGIES

	Robust	Municipality	Firm	Municipality-Month-Year
2019 I	0.000	0.000	0.000	0.000
	(0.002)	(0.002)	(0.003)	(0.002)
2019 II	0.002	0.002	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
2019 III	0.000	0.000	0.000	0.000
	(0.002)	(0.002)	(0.002)	(0.002)
2019 IV	-0.003	-0.003	-0.003	-0.003
	(0.002)	(0.002)	(0.002)	(0.002)
2019 V	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.001)	(0.001)	(0.002)
2019 VI	.	.	.	.
	.	.	.	.
2020 I	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.002)
2020 II	-0.011***	-0.011***	-0.011***	-0.011***
	(0.001)	(0.002)	(0.002)	(0.002)
2020 III	-0.016***	-0.016***	-0.016***	-0.016***
	(0.001)	(0.002)	(0.002)	(0.001)
2020 IV	-0.016***	-0.016***	-0.016***	-0.016***
	(0.001)	(0.003)	(0.002)	(0.002)
2020 V	-0.013***	-0.013***	-0.013***	-0.013***
	(0.001)	(0.003)	(0.002)	(0.002)
2020 VI	-0.013***	-0.013***	-0.013***	-0.013***
	(0.001)	(0.003)	(0.002)	(0.002)
2021 I	-0.015***	-0.015***	-0.015***	-0.015***
	(0.002)	(0.003)	(0.002)	(0.002)
2021 II	-0.014***	-0.014***	-0.014***	-0.014***
	(0.002)	(0.003)	(0.002)	(0.002)
# Workers in December 2019	116,990	116,990	116,990	116,990

Notes: See Notes from Table 2. However, these regressions are conditioned to be employed in 2019 VI. The clusters are assigned according to information in 2019 VI and the chosen strategy is at the top of each column. Treatment is the 2016 cohort, and control the 2012 cohort.
***p<0.01, **p<0.05, *p<0.1
Definitions: Each cohort is defined as all 22-year-old who entered the market in a given year.
Source: IMSS administrative records.

Appendix A3. Endogenous Labor Market Entry

In this paper, I treat labor market entry as exogenous. The concern about potentially endogenous labor market entry is that individuals might postpone their entry when the labor market is unfavorable or decide to join prematurely if the economy is booming. The expected bias is zero if labor entry timing is uniformly distributed among new labor market entrants. However, the bias can go either way if there is a selective entry.

I test for potentially endogenous labor market entry following the suggestions of Schwandt and von Wachter (2019) by performing two tests. First, I regress a cohort's share of high-school and college graduates on the informal rate at the expected graduation time. Second, I test for compositional differences in each cohort. The first test tells us whether individuals postpone graduation to avoid an unfavorable market. The second tells us whether certain groups favored specific years to enter the labor market. For example, suppose a specific year sees an increase in younger entrants. In that case, it could mean that individuals might be entering the labor market earlier than usual to take advantage of a booming economy.

To perform the first test, I collapse ENOE data by year-quarter-"year of birth"-State for all pre-pandemic available years (2005-2019), and I run the following regression:

(A3.1) $p_{sta} = \sigma_s + \gamma_t + \delta_{(a-x)} + \beta ir_{s(a=x)} * \delta_{(a-x)} + \varepsilon_{sta}$

Where p_{sta} is the percentage of college or high school graduation in State s and year-quarter t for age group a. The variable x takes the value of 17 or 21, depending on whether we study high school or college graduation: this is because graduation rates are common at 18 and 22, respectively and individuals are more likely to postpone graduation based on labor market conditions when they can still postpone their graduation. Therefore, $\delta_{(a-x)}$, measures years after expected graduation fixed effects. $ir_{s(a=x)}$ is the selection mechanism (the informal employment rate in State s at age x[44]), when the individual is at the age when they

[44] Puggioni et. al (2022) tests several possibilities and finds that the largest selection mechanism into the labor market in Mexico is not the unemployment rate, but the informal employment rate.

might the decision to postpone. This means that β captures the effect of graduating in a recession, given the regular subsequent evolution of the local labor market. To represent population-level relationships, I use sample weights. Finally, standard errors are clustered at the level of age-group by state to account for cohort-specific serial correlation in labor market outcomes.

The results are shown in Table A3.1 and show that college graduation rates do not seem to change (in a statistically significant manner) in response to the informal rate for the first four years since expected graduation. In other words: the informal employment rate affects only those who take the longest to graduate: 1 percentage point increase in the informal rate decreases the probability of graduating from college by 0.13% five years after graduation.

The results for high-school graduation show that an increase in the informal rate increases high-school rates (but this effect comes with a 1-year lag). In other words: individuals respond to worse labor market conditions by gaining more high-school education. That said, the effect is also relatively small: a 1% increase in the informal employment rate increases the high-school education rate by 0.08% two years after expected graduation.

TABLE A3.1 GRADUATION RATE CYCLE SENSITIVITY

Years since expected graduation	Percentage High-School (1)	Percentage College (2)	Percentage High-School (3)	Percentage College (4)
1	-0.027	0.039	-0.025	0.038
	(0.038)	(0.036)	(0.039)	(0.036)
2	0.081***	-0.021	0.08***	-0.02
	(0.038)	(0.035)	(0.038)	(0.035)
3	0.114***	-0.052	0.112***	-0.052
	(0.037)	(0.035)	(0.037)	(0.035)
4	0.107***	-0.07*	0.105***	-0.07
	(0.037)	(0.037)	(0.036)	(0.037)
5	0.13***	-0.135***	0.13***	-0.136***
	(0.038)	(0.038)	(0.037)	(0.038)
6	0.115***	-0.136***	0.116***	-0.136***
	(0.038)	(0.038)	(0.038)	(0.038)
7	0.142***	-0.139***	0.141***	-0.139***
	(0.041)	(0.042)	(0.041)	(0.042)
8	0.129***	-0.119***	0.125***	-0.12***
	(0.041)	(0.042)	(0.041)	(0.042)
9	0.118***	-0.093***	0.114***	-0.093***
	(0.042)	(0.042)	(0.042)	(0.042)
10	0.085***	-0.071***	0.079***	-0.072***
	(0.041)	(0.042)	(0.041)	(0.042)
11	0.13***	-0.083***	0.129***	-0.084***
	(0.043)	(0.042)	(0.043)	(0.042)
12	0.104***	-0.146***	0.104***	-0.147***
	(0.045)	(0.044)	(0.045)	(0.045)
13	0.072***	-0.1***	0.074***	-0.102***
	(0.042)	(0.051)	(0.042)	(0.052)
14	0.043***	-0.157***	0.042***	-0.157***
	(0.064)	(0.056)	(0.064)	(0.056)

Notes: This table shows the β fixed effects from regression A3.1, when the outcome of interest is the one described at the top of the column. All regressions include State, year-quarter and year-of-birth fixed effects. The last two columns differ from the last two, as they are controlling for additional “years-after-expected-graduation” linear time trends.

***p<0.01, **p<0.05, *p<0.1

Source: ENOE.

In my primary analysis, treatment and control groups do not differ by many years, and the informal rate did not change much between those years. Therefore, these results need to be scaled down to assess whether they are relevant for equation (2). In most cases, the informal rate did not change by more than half a percentage point from control year to treatment year. This implies that, even though these results are statistically significant, they are quite small and unlikely to change my results. A method for testing this is to test for compositional changes among entrants to different years. To do so, I use IMSS administrative data, classify all workers by year of entry and only keep information relating to their first formal job during their first month of formal employment ever. Then, following Rothstein (2021), I run the following regression:

(A3.2) $y_{imc} = \sigma_m + \delta_c + \varepsilon_{imc}$

Where y_{imc} is the outcome of individual i, from municipality m, and year of entry c. ε_{imc} is the robust regression error. The coefficients of interest are δ_c, which are the cohort fixed effects. These reflect cohort differences that are not explained by municipality of first employment. I omit the 2012 cohort, so results are relative to this group. If there is no endogenous labor market entry, then, there should be no statistically significant differences between different cohorts.

The results in Table A3.2 show that cohorts tend to have a similar composition across the years with some notable exceptions: 2006-2011 cohorts were more likely to start working at an older age, were more likely to be men and less likely to work in manufacturing.

This is in line with our general understanding that there have been changes in the job market even before the Great Recession (for example, the China Trade shock). However, most Post-Great Recession Entrants seem similar to each other and this suggests a minimal role of endogenous labor market entry after 2011.

Overall, the results from these two tests state that, even though there is a small strategic graduation timing in response to an increase in the informal rate, it was not large enough to change cohort (by year of entry) composition in a statistically significant manner for Post-Great Recession Entrants.

TABLE A3.2 COHORT COMPOSITIONAL DIFFERENCES

Year of entry	P(Age at entry<22)	P(Men)	P(Manufacture)
2006	-0.036***	0.011***	-0.02
	(0.002)	(0.003)	(0.003)
2007	-0.014***	0.02***	0.002
	(0.002)	(0.003)	(0.003)
2008	-0.008***	0.015***	-0.011***
	(0.002)	(0.003)	(0.003)
2009	0.002	0.02***	-0.016***
	(0.002)	(0.004)	(0.003)
2010	0.006***	0.01***	-0.008***
	(0.002)	(0.003)	(0.003)
2011	0.006***	-0.004	-0.008***
	(0.002)	(0.003)	(0.003)
2012	.	.	.
	.	.	.
2013	0.000	-0.002	0.005
	(0.002)	(0.003)	(0.003)
2014	-0.003	0.005	-0.002
	(0.002)	(0.003)	(0.003)
2015	-0.002	0.005	0.005
	(0.002)	(0.003)	(0.003)
2016	-0.003	-0.003	0.000
	(0.002)	(0.003)	(0.003)
# Workers	483,210	483,210	483,210

Notes: This table shows the cohort fixed effects (δ_c) of running regression A3.2 on a dummy equal to one if the individual fits into the category described at the top of the column. The omitted group is the 2012 cohort, and therefore, all results are shown in relationship to this cohort. The data used for this table is of the first formal job the employee had and includes all the formal workers in the IMSS dataset. To create this dataset, I used a random sample of 10% of all the IMSS workers. The 2012 is the omitted cohort and therefore, no value is included for such cohort.

***p<0.01, **p<0.05, *p<0.1

Source: IMSS administrative records.

Appendix A4. Other Outcomes

As job loss was not random, I cannot study causally the evolution of new hires and new wages. However, omitting these outcomes from the analysis would leave out a large portion of the dynamics taking place. For example, even if recent cohorts become employed again, their new job might not be paying as much as their previous employment.

To study these questions, I do a descriptive analysis on the set of workers who lost their job and obtained a new one, and who meet two additional conditions: First, they have been employed for at least 6 months at their new job (the last available observation in this paper is April 2021); Second, they only lost their job once (after February 2020). Then, similar to Puggioni et al. (2022), I run the following regression:

(A4.1) $y_{it} = \omega_t + \gamma_i + \sum_{l=-6}^{-2} \beta_l x \, 1(t - T_0 = l) + \sum_{k=1}^{6} \beta_k x \, 1(t - T_1 = k) + \varepsilon_{it}$,

where ω_t and γ_i are time and individual fixed effects. T_0 is the month of job loss during the pandemic, and T_1 is the month where the individual found a new job opportunity. ε_{it} is the individual clustered regression error. y_{it} is one of the following outcomes: the natural logarithm of the individual's i wage during period t, ln(w_{it}); a dummy equal to 1 if the individual i works during the period t for the previous-period firm (0 otherwise), or a dummy equal to 1 if the individual i works during the period t for the previous-period industry (0 otherwise). Outcomes are standardized so that $\beta_1 - \beta_6$ are expressed in reference to the last pre-job loss period. I show the results of these regressions in Table A4.1

First, I explore the analysis when the outcome is the logarithm of wages. Overall, I see that all cohorts behaved in a similar fashion: before the pandemic, wages were quite stable. After losing their job and finding a new opportunity, I see an initial decrease of about ¼ of the previous job's wage. This initial decrease is disappearing as time goes by. There are two possible reasons for this behavior: First, some workers started their job after the beginning of a month (wages in the IMSS database are calculated as a monthly average, and wages can be lower for the same worker if they do not work the whole month); second, some benefits might take time to be accounted for (IMSS wages are a monthly average plus benefits; and some, like annual vacation stipends, which might take some time to be acknowledged). That said, 6 months after obtaining a new job, workers have about the same wage as they did before the pandemic.

I also studied the probability of an individual working in the last pre-job loss firm (outcome is a dummy equal to 1 if the individual worked in the same firm as in the previous month). Overall, I also see few cohort-differences, but there is a clear pattern: workers who found new job opportunities tended to do so with previous employers (and therefore, industries) at higher rates than they did before the pandemic. This is suggestive evidence that one of the drivers of the employment recovery during the last months of 2020 and first months of 2021 were firms calling back their laid-off workers. Finally, I would like to emphasize that these workers are not all the workers who lost their jobs, but the subset who have rejoined the labor force so far.

TABLE A4.1 WAGES, FIRM AND EMPLOYMENT CHANGES FOR WORKERS WHO LOST THEIR JOB AND EVENTUALLY FOUND A NEW JOB OPPORTUNITY

	Outcome: ln(wage)			Outcome: Dummy equal to 1 if employed with previous-month industry				Outcome: Dummy equal to 1 if employed in previous-month firm
	2012 Cohort	2014 Cohort	2016 Cohort	2016 Cohort	2016 Cohort if used to be in Retail	2016 Cohort if used to be in Manufacture	2016 Cohort if used to be in Services	2016 Cohort
t-6	2.18	-4.08	0.06	0.015	0.012	-0.009	0.013	0.018
	(2.81)	(2.31)	(1.8)	(0.006)	(0.013)	(0.013)	(0.009)	(0.006)
t-5	2.02	2.06	0.5	0.006	0.001	-0.01	-0.001	0.009
	(2.80)	(2.32)	(1.85)	(0.006)	(0.013)	(0.013)	(0.009)	(0.007)
t-4	-0.53	-4.64	0.42	0.004	0.005	-0.008	0.00	-0.008
	(2.83)	(2.43)	(1.88)	(0.006)	(0.013)	(0.012)	(0.009)	(0.007)
t-3	0.00	4.06	-2.52	-0.004	-0.021	-0.016	-0.002	-0.016
	(2.83)	(2.42)	(1.84)	(0.006)	(0.014)	(0.012)	(0.009)	(0.006)
t-2	-1.96	-4.01	-3.02	0.011	0.018	0.018	0.009	0.005
	(2.97)	(2.47)	(1.82)	(0.005)	(0.012)	(0.01)	(0.008)	(0.006)
t-1	.	.	.	.	.	.	.	.
	.	.	.	.	.	.	.	.
t+1	-25.09***	-24.25***	-23.17***	0.096***	0.177***	0.149***	0.134***	0.1***
	(3.48)	(2.93)	(2.20)	(0.005)	(0.013)	(0.014)	(0.009)	(0.005)
t+2	-18.03***	-18.3***	-13.65***	0.092***	0.165***	0.135***	0.116***	0.093***
	(3.59)	(3.00)	(2.35)	(0.005)	(0.014)	(0.014)	(0.009)	(0.005)
t+3	-12.99***	-11.06***	-9.19***	0.075***	0.13***	0.11***	0.089***	0.087***
	(3.67)	(3.01)	(2.40)	(0.005)	(0.014)	(0.014)	(0.009)	(0.006)
t+4	-11.07***	-6.06*	-5.29**	0.073***	0.127***	0.086***	0.089***	0.077***
	(3.92)	(3.19)	(2.51)	(0.006)	(0.013)	(0.014)	(0.01)	(0.006)
t+5	-6.54	-1.34	-0.05	0.076***	0.114***	0.085***	0.086***	0.092***
	(4.01)	(3.36)	(2.79)	(0.006)	(0.015)	(0.013)	(0.008)	(0.007)
t+6	-0.79	-1.55	1.78	0.07***	0.115***	0.09***	0.103***	0.074***
	(4.15)	(3.59)	(2.91)	(0.006)	(0.015)	(0.014)	(0.009)	(0.007)
# Workers	2,329	2,800	3,503	3,503	677	731	1,181	3,503

Notes: These results follow equation (4) and evaluate an event study, where the outcome of the individual (at the top of the column) is compared for six months before losing his job during the pandemic (any month after February 2020) and for six months after gaining a new job. The last observation in this paper is 2021 April. To estimate such regressions, I use the reghdfe Stata command (Correia, 2015). t-1 is the omitted period and therefore, no value is included for such period. ***p<0.01, **p<0.05, *p<0.1
Source: IMSS administrative records.