Effects of the Covid-19 Pandemic on the Colombian Labor Market: Disentangling the Effect of Sector-Specific Mobility Restrictions

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Abstract

We assess the effect of the Covid-19 pandemic and particularly the sector-specific mobility restrictions on the Colombian labor market. We exploit the sectoral and temporal variation of the restriction policies to identify their effect. Mobility restrictions significantly reduced employment, accounting for approximately a quarter of the total job loss between February and April of 2020. The remaining three-quarters of the job losses could be attributed to the disease's regional patterns and other epidemiological and economic factors affecting the whole country. Therefore, we should expect important employment losses even in the absence of such restrictions. We also assess the effect of restrictions on the intensive margin, finding negative, although smaller effects on the number of hours worked and wages. Most of the employment effect is driven by salaried workers, while self-employment was more responsive to the disease spread. Finally, we find that women are disproportionally affected; mobility restrictions account for a third of the recent increase of the gender gap in salaried employment.

Keywords: Covid-19, mobility restrictions, labor market, employment.

JEL codes: I14, I18, J21

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1. Introduction

The Covid-19 pandemic is one of the most disruptive events the world has faced in recent history. By June 2020, over 10 million people had been infected, almost half a million people had died worldwide and the number was still growing.[1] In order to flatten the contagion curve and improve the health system capacity, most countries implemented strict lockdown policies, with different types of mobility restrictions. The sanitary crisis and the mobility restrictions triggered an unprecedented global economic crisis, with particularly alarming effects on employment.

This paper assesses the pandemic's impact on the Colombian labor market, emphasizing the role of sector-specific mobility restrictions implied by the first wide-reaching lockdown of the country. While some economic sectors, considered essential, were authorized to continue operating, the rest faced severe mobility restrictions. Using this variation source, we estimate difference-in-differences and event-study models to assess the impact of these restrictions on employment, hours worked and wages. Our empirical framework controls for regional variation in the disease spread, time fixed effects accounting for other epidemiological and economic factors affecting the whole country, and sector and city fixed effects capturing their observed and non-observed time-invariant characteristics. The sectorial restrictions were announced in Mid-March 2020 and implemented simultaneously, and no additional sectors were excluded during our period of study, which ends in April 2020.

Our results indicate that sector-specific restrictions had a negative effect on employment. On average, between February and April, employment fell 9.4% more in the restricted sectors, compared to those excluded from the measures. A back-to-the-envelope calculation suggests this effect accounts for almost one-quarter of the total employment loss during this period (-18.3 %). The remaining three-quarters of the job losses could be attributed to the disease's regional patterns and other epidemiological and economic factors affecting the whole country during this period. These factors, captured by the time fixed-effects coefficients, include all common shocks that hit the labor market during the pandemic crisis. For instance, the impact of general mobility restrictions, the average impact of the fear to contagion in the agents' behavioural responses, and the aggregate impact of external macroeconomic shocks as

[1] Data from https://ourworldindata.org/.

commodity prices, trade, or remittances. Therefore, we should expect important employment losses even in the absence of sector-specific restrictions. Furthermore, in the absence of sectoral restrictions, the disease's spread could have been greater, with potentially negative effects on economic activity and employment.

We then assess the impact of the sector-specific restrictions on the intensive margin, estimating the effect of sector-specific restrictions on the average number of hours worked and wages. We also find negative effects, although smaller in magnitude than those found for employment. Further, we investigate whether the estimated effects differ across different segments of the labor market. We find that the effects on employment are mainly driven by salaried workers, while self-employment is more responsive to the disease spread. These results suggest that rigidities in the labor market may amplify the impact of the sector-specific mobility restrictions. Finally, we find considerably larger effects on women, but only in the salaried segment. Mobility restrictions account for a third of the widening of the salaried employment gender gap during the studied period.

Our paper contributes to the growing literature on the labor market effects of lockdown policies. Most of the existing studies indicate that mobility restrictions account for only a fraction of labor markets weakening during the pandemic crisis. Other factors, such as the negative aggregate effect of the disease itself, play an important role (Aum et al., 2020; Forsythe et al., 2020; Gupta et al., 2020; Lozano Rojas et al., 2020; Goolsbee and Syverson, 2021).

Our results are in line with these findings. We provide a complete set of robustness checks confirming the validity of our causal claims regarding the effects of the sector-specific mobility restrictions. First, in the presence of inter-sectoral linkages, mobility restrictions might have affected the employment of excluded sectors as well. We estimate our main specification excluding industries in the control group with strong economic linkages to restricted sectors. The effects of sector-specific restrictions are overall unaltered. Second, contagion risk may vary by industry. We build a measure of potential risk based on physical proximity between workers. We estimate our main specification controlling for the interaction between this measure and the city disease spread, finding similar results. Third, we extend our time framework until June. Since sector-specific restrictions are eliminated progressively, we test for heterogeneous treatment effects following De Chaisemartin and D'Haultfœuille (2020). The estimated effects are similar to those in our main specification.

While most of the existing literature focuses on high-income countries, this is one of the few studying a developing country. This is inherently interesting for at least two reasons. First, developing economies are characterized by a high prevalence of informality. The informal segment of the labor market is usually more flexible than the formal one because official regulations are challenging to enforce. Nevertheless, the informal market's job quality is poor, and informal jobs might be more vulnerable in the pandemic economic crisis (Eslava and Isaacs, 2020). Hence, the response of such segmented labor markets to lockdown policies may differ largely from the response observed in developed countries. Second, in many emerging economies, the strict lockdown policies' timing relative to the disease spread was different with respect to the developed world. In fact, lockdowns were implemented long before disease peaked, a setting that could be more favourable to isolate the effect of sector-specific mobility restrictions from the own effects of disease propagation.

The remaining of the paper is organized as follows. Section 2 summarizes the existing evidence on the pandemic, the lockdown policies, and their impact on the labor market. Section 3 briefly describes the evolution of the disease in Colombia and the adopted mobility restriction measures between March and April 2020. Section 4 describes in detail our data and the empirical strategy. Sections 5 and 6 present the baseline results and the robustness checks, respectively. Finally, Section 7 concludes.

2. Covid-19 pandemic and mobility restriction policies

On December 31 of 2019, Chinese authorities reported to the World Health Organization (WHO) the appearance of rare pneumonia cases in the eastern region of China; the epidemiological origins of this sickness remain unknown. Seven days later, Chinese authorities reported a new virus, initially called 2019-nCoV, which caused the respiratory disease Covid-19. On January 13, the first case of Covid-19 was detected outside of China (in Thailand). By January 31, 18 different countries had already reported the first case of Covid-19 within their borders (Kumar et al., 2020). On March 11, the WHO declared Covid-

19 as a pandemic; at the time, there were over 118 thousand cases detected worldwide. As of June 30, there were over nine million positive cases reported worldwide, causing the death of 505 thousand people.[2]

To flatten the contagion curve and reduce the pressure on the health system, most countries implemented different lockdown policies at different stages of the evolution of the disease. Evidence suggests that these measures effectively reduced mobility in public places, which in turns decreased the number of positive cases and deaths (Bilgin, 2020; Engle et al., 2020; Fang et al., 2020; Glaeser et al., 2020; Kraemer et al., 2020; Yilmazkuday, 2020). However, even in countries without initial mandatory restrictions measures, such as South Korea and Sweden, mobility also decreased, reflecting that individuals also took precautionary measures to prevent contagion (Aum et al., 2020).

Both the presence of the disease and the introduction of lockdown policies may have had adverse effects on the labor markets. On the one hand, the disease itself and the fear of contagion, can increase work absenteeism and reduce the consumption of several goods and services, destroying jobs. On the other hand, lockdown measures may constrain economic activity, both throughout their impact on aggregate demand and supply, hurting employment. Regarding lockdowns, in most countries some sectors deemed essential, such as agriculture and public utilities, were authorized to continue operating. The rest of the economy was restricted, so a differential impact on employment across sectors should be expected. Finally, there is a whole set of external macroeconomic shocks that might have hurt the economy simultaneously, indirectly impacting employment as well. For instance, numerous countries were affected by a sharp reduction in income from international trade and remittances, and instability in commodity prices and exchange rates.

Multiple studies have assessed the effect of the pandemic and the mobility restriction policies on the labor market outcomes.[3] Most of the papers are based on developed countries and conclude that even though lockdown measures have negatively affected the labor market, they only account for a fraction of the crisis (see Baek et al., 2020; Forsythe et al., 2020; Gupta et al., 2020; Kong and Prinz, 2020 and Lozano Rojas et al., 2020 for the United States;

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[2] Data from https://ourworldindata.org/

[3] For a more literature review on the economic effects of the pandemic, see Brodeur et al., (2020).

Barrot et al., 2020 for France; Hupkau and Petrongolo, 2020 for the United Kingdom; Koebel and Pohler, 2020 for Canada, and Fadinger and Schymik, 2020 for Germany, among others). In fact, the disease itself and the precautionary measures taken by individuals also affect employment, regardless of the mobility restrictions policies. Some studies have shown that, even in countries without mobility restrictions, there were important negative effects on the labor market (see Aum et al., 2020 for South Korea, and Juranek et al., 2020 for Sweden relative to other Nordic countries).

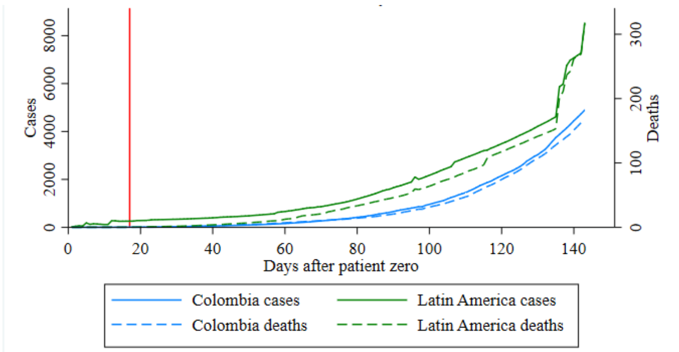
The evidence for developing countries, where lockdowns were often implemented before the disease was widespread, remains scarcer but points in the same direction (see for instance Dang and Nguyen, 2020 for Vietnam; Gottlieb et al., 2020a for 57 countries; Hoehn-Velasco et al., 2021 for Mexico; and Nelson, 2021 for 20 emerging countries). Nevertheless, most of these studies use firm surveys, which omit the impact of the pandemic on self-employment and informal workers, types of employment that hold a considerable share in the labor markets of developing countries.

Some of the recent literature focuses on the heterogeneous effects of the pandemic across different segments of the labor markets. Adams-Prassl et al. (2020), Béland et al., (2020) and Yasenov (2020) analyze differential effects by groups of workers, finding considerably larger impacts on low-skill workers and immigrants. Albanesi and Kim (2021), Alon et al. (2020), Andrew et al. (2020), Kalenkoski and Pabilonia (2020), Del Boca et al. (2020), Lee et al. (2021) and Sevilla and Smith (2020) study the effects on employment by gender. A widening of the gender gaps is documented due to the larger impact in high-contact sectors with higher proportion of female workers and the unequal intra-household distribution of childcare after the closure of schools and care services. Dingel and Neiman (2020), Delaporte and Peña (2020), Gottlieb et al. (2020a, 2020b) and Saltiel (2020) analyze the heterogeneous impact across occupations depending on the ability of perform tasks from home; while Béland et al. (2020) and Goolsbee and Syverson (2021) consider also the exposition to the disease and the physical proximity to coworkers. These studies suggest a reallocation of workers across occupations and sectors, a fact that may imply an increase in both frictional and long-term unemployment (Arango and Flórez, 2020). It would also demand policies to improve the labor force's skills toward digital and technological skills (Farné, 2020).

3. The Colombian Case

In Colombia, the first case of Covid-19 was detected on March 6, 2020 and cases began to raise in late April. By the end of June, there were approximately 95,000 detected cases and 3,200 deaths recorded in the country. While cases we probably underestimated, due to testing limitations during the first months of the pandemic, these numbers remain relatively low compared to the Latin American region, which is partly due to the fact that Colombia implemented lockdown policies early into the pandemic (see Figure 1).

Figure 1: Covid-19 cases and deaths in Colombia and Latin American average



Notes: Cases and deaths per million. The red line indicates the beginning of the lockdown policies in Colombia. Source: Our World in Data https://ourworldindata.org

Mobility restrictions were announced in March 20 and implemented on March 25 (Decree 457 of 2020), when there were only 378 reported positive cases and three deaths[4]. The government enacted a nationwide lockdown, restricting the mobility of all individuals except those working in a small set of sectors classified as essential. These essential sectors included public administration, finance, agriculture and public utilities, and the sectors that were part of their supply chains.[5] Workers in these sectors were authorized to continue working under relatively strict protocols. Everyone else was restricted to leave home, except for some basic activities such as groceries and medical consultations. These exceptions were further regulated by local authorities, which implemented additional restrictions in each city or region. The most common ones were: (i) the restriction to mobility based on gender or the

[4] Before March 20, some regions of the country such as Bogotá, Santander, Boyacá and Nariño decreed mandatory preventive isolation measures. Therefore, in specifications controlling our empirical strategy we include March as the beginning of the treatment period.

[5] A more detailed analysis of the inter-sectoral linkages is presented in section 6.1.

last digit of the national identification number, and (ii) mandatory curfews on weekends and nights.

Both the pandemic and the introduction of mobility restrictions to curb contagion had an enormous impact on the Colombian economy. GDP shrank by 6.8% in 2020, implying that the pandemic originated the worst economic depression in modern Colombian history. Regarding the labor market, in the second quarter of 2020 the country registered its highest urban unemployment rate in recent history, 23% (see Figure A1 in the Appendix). The impact on the labor market was largely heterogeneous across sectors. For instance, Figure A2 in the Appendix presents the annual growth rate of each 1-digit ISIC employment in April 2020. The sectors with the largest decreases were artistic activities and manufacturing, two of the most affected by the mobility restrictions; whereas public utilities, an excluded sector, had a remarkable increase (although its weight is small). We characterize in detail the behavior of employment in both excluded and non-excluded sectors in the next section.

As in most countries, there were numerous additional policies to prevent contagion and deaths, and mitigate the economic crisis. These include large investments in the health system capacity, general mobility restrictions, cash transfers for poor households, and tax breaks and credit lines for firms.[6] While we should not expect health system investments and cash transfers to benefit some economic sectors in particular, tax breaks and credit lines may have disproportionally benefited the industries that were more affected by the mobility restrictions. However, both of these policies were implemented after April. Hence, we argue that our baseline estimates, which consider data only until April, are not contaminated by those policies.

4. Data and empirical strategy

4.1. Data

[6] Additional non-pharmaceutical policy interventions that might have an impact on the labor market include the following. On March 16, the government issued the restriction of transit to non-resident foreigners and closed the borders for 75 days. On March 19, the government announced the suspension of international flights. The government also created special credit lines to severely affected industries, such as tourism or aviation. Likewise, tax subsidies covered up to 40% of the payroll of firms in any economic sector who experienced an income reduction of 20% or more.

Our analysis is based on repeated cross-sections data from the Colombian National Household Survey (GEIH) collected by the Bureau of National Statistics (DANE). The survey includes both formal and informal workers and is the official source for the calculation of the unemployment rate and other labor market statistics in the country. The survey is representative for the 23 cities in Colombia and an aggregate of other municipalities and rural areas on a monthly basis. For our primary analysis, we classify 4-digit ISIC activities as restricted or excluded from the mobility restriction policy following the Decree issued by the Colombian Government. Since the survey is not representative at the 4-digit ISIC and city level, we use more aggregate partitions in our analysis. Specifically, we add-up the employment of excluded and non- excluded sectors in each 1-digit sector and city, which yields a balanced panel with 528 cells per month. To further ensure the representativeness of the sample, our main estimates are based on 2-months moving averages. We also estimate the model using monthly data or and 3-months moving averages in the robustness section, finding similar results.

Since policies changed rapidly during the pandemic and we are interested in its short-run effects, we opt for a relatively narrow window before and after the mobility restrictions were implemented. Therefore, our baseline study period finishes in April 2020, before any changes in the mobility restrictions were made; and our pre-treatment period includes three months. Since May, the group of excluded sectors started to expand gradually, with the reactivation of construction, manufacturing, communications, retail and communications. Real state and professional services joined in June. In section 6.3, we extend the study period until June and estimate the effect of mobility restrictions with heterogenous treatment effects as De Chaisemartin and D'Haultfœuille (2020), finding relatively similar results.

Table 1 presents the summary statistics of the sample we use for our baseline estimations. The study unit is an economic subsector in a given city; the subsector divides a 1-digit ISIC sector into its excluded and non-excluded (by the lockdown policy) components. Summary statistics show that 288 city-sectors belong to the excluded subset, and 240 city-sectors belong to the non-excluded subset. On average, in the excluded subsector, hourly wages were 4,789 COP in January (1.4 USD) and in April, hourly wages were approximately the same, while in the non-excluded subsector they fell from 3,499 (1.1 USD) to 3,366 between the same months. In contrast, the average employment of the non-excluded subsector in January was 48,312 employees and for April it reduced to 40,311.[7]

Table 1. Summary Statistics

	Excluded			Non-excluded		
	Observations	Mean	Std. Dev.	Observations	Mean	Std. Dev.
A. January (2020)						
Hourly Wage	288	4789	2551	240	3499	1582
Employment	288	37471	215989	240	48312	124916
% workers 25-45	288	38,3	1,5	240	38,3	1,5
Deaths per million in working age population	288	0,0	0,0	240	0,0	0,0
Cases per million in working age population	288	0,0	0,0	240	0,0	0,0
B. February (2020)						
Hourly Wage	288	4880	2810	240	3508	1555
Employment	288	37121	211849	240	47525	122731
% workers 25-45	288	38,2	1,5	240	38,2	1,5
Deaths per million in working age population	288	0,0	0,0	240	0,0	0,0
Cases per million in working age population	288	0,0	0,0	240	0,0	0,0
C. March (2020)						
Hourly Wage	288	4905	2643	240	3564	1976
Employment	288	36231	203453	240	45481	117878
% workers 25-45	288	38,0	1,6	240	38,0	1,6
Deaths per million in working age population	288	1,1	1,8	240	1,1	1,8
Cases per million in working age population	288	25,9	21,1	240	25,9	21,1
D. April (2020)						
Hourly Wage	288	4869	3011	240	3366	1941
Employment	288	34709	197636	240	40311	104663
% workers 25-45	288	37,9	1,6	240	37,9	1,6
Deaths per million in working age population	288	8.6	8.5	240	8.6	8.5
Cases per million in working age population	288	162.4	185.3	240	162.4	185.3

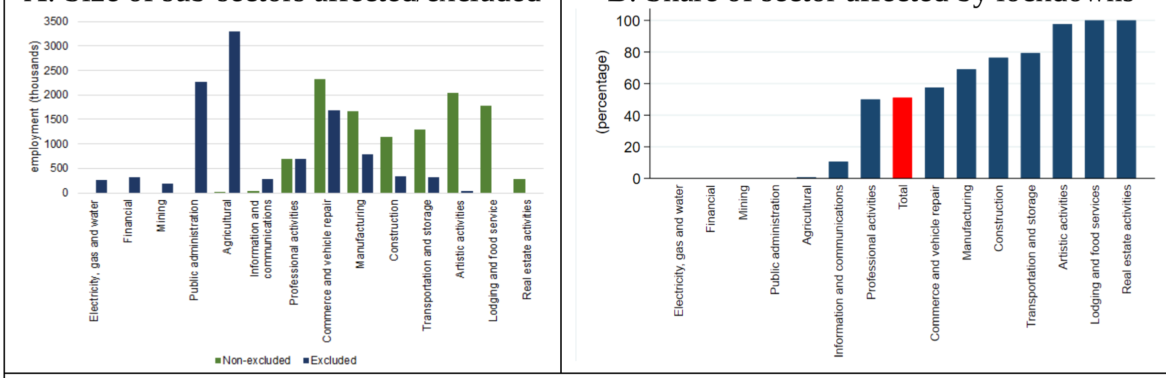
Source: Calculations by the authors based on data from DANE (GEIH). The unit of observation is a partition of the economic sector in a metropolitan area.

To obtain a general overview of the shares of employment possibly affected by mobility restrictions across sectors, we first add-up the pre-pandemic employment of our observations units at the national level, and present their levels for each 1-digit sector in Panel A of Figure 2 (Panel B plots the corresponding shares). As can be seen, the most restricted sectors were artistic activities, lodging and food and real estate, with shares of employment in restricted sub-sectors close to 100%. On the contrary, sectors like public utilities, financial, mining and public administration were completely excluded; suggesting a wide dispersion in the shares of employment possibly affected across sectors.

[7] Our measures of Covid-19 cases and deaths come from the Colombian National Institute of Health (INS acronym in Spanish). Throughout the INS, the Colombian government publishes daily updates on the positive cases and deaths at a national and a regional level, and the media regularly report these statistics. We should, therefore, expect individuals to incorporate this information into their decision-making process.

Figure 2. Intensity of lockdown measured by restricted employment

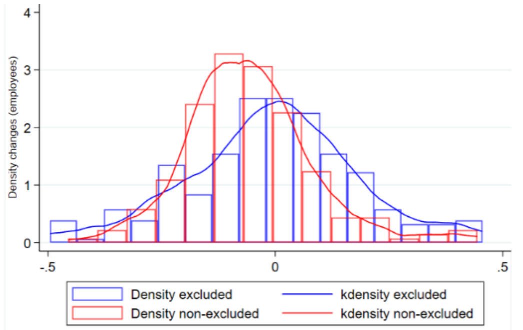
A: Size of sub-sectors affected/excluded | B: Share of sector affected by lockdowns



Notes: Panel A presents for each 1-digit sector, the amount of employment in each subsector (restricted or excluded). Panel B displays the percentage of employment in each sector correspongind to the lockdown policies. **Source:** Calculations by the authors based on data from DANE (GEIH).

In Figure 3, we present the employment growth rate distribution between February and March 2020 for excluded and non-excluded sub-sectors. There is a larger mass of negative growth realizations in excluded sub-sectors relative to non-excluded ones, implying that restricted sectors had on average a worse performance relative to excluded ones. Finally, in Figure A3 in the Appendix we add up employment for both total excluded and total non-excluded sub-sectors and compare their mean growth rates (February-April 2020) for each considered city. The non-excluded sub-sectors experienced more significant employment reductions, which is the case for most of the cities; but important heterogeneities across cities can be observed, that in part justify our choice of observations units. In Table A1 of the Appendix, we present detailed summary statistics of employment, wages, and sickness variables for each of the 24 labor markets we study; the table presents pre- and post-pandemic averages for each city separating by affected or excluded sectors. The table shows that average employment losses from February to April range from from -29.5% to -9.5% (-14.8% to 0.8%) across the cities for affected (excluded) sectors.

Figure 3. Employment growth of excluded and non-excluded sectors



Source: Calculations by the authors based on data from DANE (GEIH).

4.2. Empirical Strategy

We exploit the variation in the excluded and non-excluded sectors and the timing of the restriction policies to disentangle the effect of sector-specific restriction policies from regional variations in disease spread and all other aggregate shocks related to the pandemic. Our baseline specification is the following difference-in-differences (DID) model:

$$y_{jct} = \beta q_j * post_t + \gamma d_{ct} + \delta_t + \phi_{jc} + u_{jct} \qquad (1)$$

Where y_{jct} is the labor market outcome of sector j, in city c, and period t. The differential effect of sector-specific restrictions is captured by β, the coefficient of the interaction between q_j, which takes value one if sector j is restricted and $post_t$, which is equal to one starting March 2020. Controls include d_{ct}, a time-varying measure of the regional variation in disease spread (positive cases or deaths per million) in city c and period t, and time fixed-effects (δ_t), hereafter referred to as the aggregate shock, which accounts for any epidemiologic and economic factor that homogeneously affects the country's labor market in each period. These factors include *i*) The impact of general mobility restrictions, as well as any multiplier effects affecting excluded and non-excluded sectors; *ii)* The average impact of the disease itself on work absenteeism, consumption, and investment decisions; *iii)* The impact of other external macroeconomic shocks related to the pandemic, including sharp variations in commodity prices, trade, and remittances. Since the sanitary crisis began in

March in Colombia, we use February as the reference period. Finally, the models also control for sector-city fixed-effects (ϕ_{jc}), which account for the time-invariant observed and unobserved characteristics of each labor market. Errors are clustered at the sector-city level.

As a complementary analysis, we use events-study models to estimate the differential effect of the sector-specific restrictions in each period. Instead of interacting the restriction term with a post-treatment dummy, we interact it with a set of time dummy variables, excluding February. The estimated equation can be represented as:

$$y_{jct} = \sum_{1}^{T} \beta_t q_j * 1\{period = t\}_t + \gamma d_{ct} + \delta_t + \phi_{jc} + u_{jct} \qquad (2)$$

where, $1\{period = t\}$ is a set of dummy variables equal to one in the respective period and zero otherwise. The reference period is February 2020.

The effect of the sector-specific restrictions can be interpreted as causal as long as the common trends assumption is satisfied, i.e. the excluded and non-excluded sectors have similar employment trends before the policy was implemented. Graphical evidence not presented for the sake of brevity suggests that this is the case; employment in excluded and non-excluded sectors show parallel trends until February, and the difference between them grows in the following months. We provide further confirmation of the common trends assumption with the event study models presented in the following section, which show no significant differences in employment between excluded and non-excluded sectors before February.

We identify at least two sources of potential bias for the regional variation in disease spread. First, the virus testing capacity may vary by region, leading to serious measurement error, whether we use the positive confirmed cases or deaths metrics. Second, it is reasonable to assume that more active labor markets can contribute to the spread of the virus, which would lead to reverse causality. While we cannot make causal claims, results suggest that at least part of the shock is driven by the regional variation in the disease spread. Likewise, the aggregate impact of the broader set of mobility restrictions implied by the lockdown cannot be fully identified from other sources of variation. While it is reasonable to assume that general mobility restrictions have a direct negative effect on employment, they may also slow down the disease spread, which may, in turn, benefit the economy.

Goodman-Bacon and Marcus (2020) point out different sources of potential bias in DID designs in the context of the Covid-19 pandemic. The following are some of the issues that could potentially threat our identification. First, there were multiple policies in place, and some of them could disproportionally benefit the sectors that were more affected by the mobility restrictions. This is particularly true for the tax breaks and credit lines for firms. However, both of these policies were implemented after April, leaving our main estimates unaltered by them.

Second, the estimated effects could be biased by the presence of spillover effects. On one side, there are inter-sectoral linkages; excluded sectors may suffer a reduction of the demand from a sector non-excluded sector, affecting his level of production and employment indirectly. Also, there could be a disruption in the supply of inputs required by excluded sectors produced by the non-excluded ones. In section 6.1, we assess this issue limiting our sample, dropping excluded sectors with important linkages with the restricted ones. Overall, results are similar. On the other hand, there could be regional spillovers in the disease spread. Given that there were strict traveling restrictions during our period of study, we should expect that this is not a big risk to our identification. We further address this point by controlling by the dynamics of cases and deaths in each city.

Third, our estimates could also be biased by unobserved characteristics that simultaneously affect the mobility restrictions and the labor market outcomes. In particular, the covid-19 contagion risk may vary across sectors. We argue that the criteria to exclude sectors from the initial mobility restrictions is more related to how essential the sector is, than the contagion risk itself. In subsection 6.2, we also show that our main results hold in regressions in which we allow the risk of contagion to be heterogeneous across sectors.

Fourth, recent literature has pointed out that multi-period DID designs could be biased because of group-specific heterogeneous effects. The latter is especially true when treatment effects change in time (Callaway and Sant'Anna, 2020; De Chaisemartin and D'Haultfœuille, 2020; Goodman-Bacon and Marcus, 2020; Imai and Kim, 2020). In our baseline specification, we focus on a period in which sectors were restricted simultaneously and there are no major changes in the policy. Therefore, we should not be concerned about this source of heterogeneity. In section 6.4 we extend the timeframe until June. Since restrictions are progressively eliminated during these last months, we estimate with heterogeneous treatment effects following De Chaisemartin and D'Haultfœuille (2020), finding fairly similar results.

Finally, anticipation could be problematic in DID designs. However, in Colombia mobility restrictions were announced and implemented only two weeks after the pandemic was declared, and in a relatively early stage of the disease spread. There were no announcements that could foresee the timing of the measures or which sectors would be excluded from the measures.

5. Results

5.1 Employment

We begin our analysis with the impact on employment in Table 2 (columns 1 to 4). In the first column, we focus on estimation equation (1) of the aggregate shock, including time and sector-city fixed-effects, and the interaction of our interest. The time fixed-effects are negatively and statistically significant for April, with estimated coefficients of -0.13. In this specification, the coefficient that measures the effect of sector-specific restrictions is negative and significant. The estimated coefficient is -0.099, equivalent to almost 9.4% additional jobs loss in the non-excluded sectors relative to the excluded sectors. In a back-to-the-envelope calculation, we approximate the total impact of the sector-specific restrictions by multiplying the estimated coefficient by the share of the labor force in restricted sectors in February (51%).[8] Our results suggest that sector-specific restrictions are responsible for approximately 5 pp, less than a quarter of the total February-April job losses (18.3 %).

In columns 2 and 3, we include the disease spread's regional variation, measured with city-level indicators of Covid-19 confirmed cases and deaths. We consistently find that the time fixed-effects coefficients are smaller in magnitude, while the disease coefficients are negative and significant. When we multiply the estimated coefficients for deaths by the average deaths in March and April (4.8), we find that the regional variation of the disease spread accounts for approximately 2.3pp. While these results cannot be interpreted causally, they suggest that

[8] Recent literature warns about calculations that extrapolate from well-identified elasticities to aggregates because the economic channels and shocks at the level of the variations used to identify the elasticities can differ from those present at the aggregate level (Beraja et al., 2019). For this reason, we consider this calculation as a very approximate decomposition.

a non-trivial part of the pandemic effects on employment is related to the disease itself, which implies that controlling the virus should positively affect employment.

In column 4, we include all the model variables, finding similar coefficients for the sector-specific restrictions, the time fixed-effects, and the regional variation in the disease spread. The estimated coefficients for April fixed effect across all specifications range between -0.13 and -0.092; they account for most of the employment variation during this period. Overall, results suggest that we should expect important employment losses even in the absence of sector-specific restrictions. These findings are consistent with recent papers showing mild effects of the lockdown policies compared to those of the disease itself and other economic factors related to the pandemic (Aum et al., 2020; Forsythe et al., 2020; Gupta et al., 2020; Lozano Rojas et al., 2020; Goolsbee and Syverson, 2021).

We further explore sector-specific restriction policies' dynamic impact using an event study design that interacts q_j with the time fixed-effect variables (equation 2). We use the same time framework used in the DID regressions presented before. As we mentioned in section 4, the post-lockdown period comprises April and March months. In this period, all affected segments were lockdown simultaneously. There are no further exemptions or changes in the lockdown policy during these months, which helps to have a straightforward design. Therefore, we study a relatively narrow window before and after the policy, and we interpret our results only as short-run effects. Policies to tackle the effects of the pandemic evolve very fast; it has been remarked in the literature on DID designs on the effects of the pandemic, the necessity of focus on small windows of time around policy changes (Goodman-Bacon and Marcus, 2020).

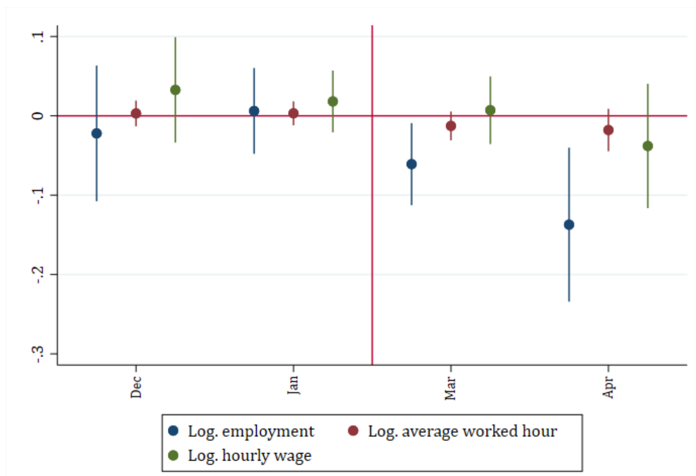
Table 2: Employment, Average Worked Hours and Hourly wage regressions

	Log. Employment				Log. Average hours				Log. Hourly wage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Restricted x Post	-0.0991***	-0.0937***	-0.0944***	-0.0935***	-0.0173**	-0.0173**	-0.0172**	-0.0173**	-0.0316*	-0.0327**	-0.0323*	-0.0326**
	(0.0222)	(0.0229)	(0.0227)	(0.0230)	(0.0083)	(0.0082)	(0.0082)	(0.0082)	(0.0168)	(0.0164)	(0.0166)	(0.0164)
Share reported cases		-0.0003***		-0.0002**		0.0000		0.0000		0.0001		0.0001
		(0.0001)		(0.0001)		(0.0000)		(0.0000)		(0.0001)		(0.0001)
Share reported deaths			-0.0048***	-0.0015			-0.0001	-0.0003			0.0007	-0.0003
			(0.0018)	(0.0015)			(0.0005)	(0.0004)			(0.0012)	(0.0015)
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The variable Restricted x Post represents the interaction between q_j, which takes value one if sector j is restricted, and $post$, which is equal to one starting in March 2020. Share report deaths and Share report cases stand for reported deaths and cases per one million working-age population in each city, respectively. In the fixed-effects, by period, our base month is February (2020). Standard errors are presented in parentheses and clustered at the city-sector level. The regressions are weighted by each sector's share of employment in total employment in the pre-treatment period. In all specifications, we control for study unit and time fixed effects.

Figure 4 presents the event study results for employment regressions and other outcomes we describe further. First, the estimate effects are small in magnitude and statistically insignificant before the policy was enacted; this confirms that there are common trends, and the model assumptions hold. Second, the sector-specific restrictions effects are particularly large in April. Finally, the coefficients are also negative in March, although smaller in magnitude and significance. This latter finding reflects that the policy took place only during the month's last days.

Figure 4. Event Study coefficients log of employment



Notes: Dots represent the point estimates with a 99% confidence interval.

5.2. Worked hours and wages

We assess whether the sanitary crisis and the sector-specific restrictions also affect the number of hours worked. Table 2, columns 5 to 8, presents the effect on average hours worked, where each column displays the results from the same specifications used for employment. We find that the sector-specific restrictions have a negative and significant effect on hours, although smaller than the found for employment: the estimated coefficient is -0.017, equivalent to a reduction of -1.7%. The magnitude of the effect does not change when controlling for the disease's regional intensity variables (share of reported cases or deaths), whose coefficients are no longer significant. We estimate the impact on hourly wages in Table 2, columns 9 to 12, also finding negative and significant effects of sector-specific restrictions, with an estimated effect near -0.032, equivalent to a reduction of 3.1%. When we explore the dynamic impact of sector-specific restriction policies using an event study design, we find that in both cases, hours and hourly wages, the pre-treatment period's effects are not statistically significant (Figure 4). The DID results suggest that the impact of the sector-specific restrictions took place both in the extensive and the intensive margin, with reductions on worked hours or wages.

5.3 Salaried work and self-employment

Colombia, as many other emerging economies, has a segmented labor market, in which there is a strong correlation between informality and self-employment.[9] Previous literature has argued that excessive labor market regulation in these economies might increase the formal segment's rigidity relative to what is observed on the informal sector (Blanchard and Portugal, 2001; Flórez et al., 2020). This is particularly true for Colombia, where non-labor costs remain particularly high and are partly responsible for the high informality rate (Flórez et al., 2020). Given that self-employed workers face considerably fewer regulations, we should expect the sector-specific measures to be less binding in this segment[10].

We test this hypothesis in Table 3, where we estimate the impact on salaried and self-employed employment. As expected, the impact of sector-specific restrictions on employment is entirely driven by the salaried segment. The estimated coefficient for the effect of the restrictions in the specifications controlling for the disease's evolution is around -0.28, equivalent to 24% additional salaried jobs loss in the non-excluded sectors relative to the excluded sectors. Following the same back-of-the-envelope calculation we use for total employment, we find that sector-specific restrictions account for almost half (49%) of the total job loss in this labor market segment. In contrast, sector-specific restrictions do not affect self-employment. These results suggest that labor market rigidities, affecting mainly salaried workers, might amplify the effects of mobility restrictions. Interestingly, the regional variation in the disease spread is particularly relevant for self-employed workers.

[9] Approximately 80% of the self-employed workers do not pay mandatory social security contributions. Given the informal nature of their work, they are not prone to strict compliance with labor regulations.

[10] There is some evidence of yearly transitions from salaried workers to self-employed in the worse months of the crisis (11%). However, the most critical flow is the transition from salaried workers to unemployment (52.5%). In Colombia, as part of the labor market regulations, there is an unemployment insurance system; nevertheless, it is incipient, and workers can only receive the benefit in only one unemployment episode. The unemployment benefit system includes payment of social security taxes up to six months and the payment of one minimum wage distributed monthly and up to six months as well.

Simultaneously, the coefficient is smaller or, in the case of controlling for deaths, statistically insignificant in the salaried segment. These results may reflect that self-employed workers have more flexible jobs and working hours, and thus they might restrict their mobility when local contagion increases.

5.4 Gender gap

Sector-specific restrictions may disproportionally affect female employment if sectors in which female work are predominant were more likely to be restricted (Alon et al., 2020). This compositional effect, along with other channels such as the unequal intra-household distribution of childcare after the closure of schools and care services (Boll and Schüller, 2020; Del Boca et al., 2020; Sevilla and Smith, 2020; Bonilla et al., 2021), could have contributed to the well documented widening of the gender gaps in the labor market due to the pandemic (see Cuesta and Pico, 2020; Garcia-Rojas et al., 2020; and Bonilla et al., 2021 for Colombia; Adams-Prassl et al. 2020; Alon et al., 2020; Albanesi and Kim, 2021; Kalenkoski and Pabilonia, 2020; Lee et al., 2021 for other countries). [11]

To assess the contribution of sector-specific restrictions on the widening of the employment gender gap, we estimate their effect by gender in Panel A of Table 4. We find that sector-specific restrictions only had an impact on total male employment. For men, the estimated coefficient for the effect of the restrictions is -0.10, accounting for around 30% of their total job loss. For women, the estimated coefficient, although negative, is statistically insignificant. A possible explanation for these results is that the composition of employment between salaried and self-employment jobs is different between men and women; an issue that might be driving the results. We thus re-estimate our specifications for the salaried and non-salaried segments for each gender separately. For non-salaried workers, we estimate an insignificant coefficient for both men and women, similar to what we find for both genders in the previous section. Instead, for salaried workers, the estimated coefficients for women are significant and larger in absolute values relative to those for men (Panel B).

[11] In Colombia, during the previous two months to the pandemic declaration, on average, 15,5K women were occupied in excluded sectors, and 22,1K worked in a non-excluded one. In the case of women with children at home (<=12), on average, 6K women were occupied in an excluded sector vs. 8K who worked in a non-excluded one; nevertheless, these differences are not statistically significant.

Using our back-of-the-envelope calculations, based on the model that controls for cases and deaths, we find that the sector-specific restrictions account for 0.88pp of the difference between what the female salaried employment decreased relative to men (2.63pp.). This means that the channel of the sectoral restrictions explains around a third part of the widening of the gender gap in salaried employment. The effect is much smaller when we include self-employment, which is consistent with the fact that other channels, such as the closure of schools and daycare centers, still explain to a considerable part of the widening of the gender gap during the pandemic.

Table 3: Employment Effects for Salaried and Self-employed Workers

	Salaried				Self-employed			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Restricted x Post	-0.2826***	-0.2762***	-0.2776***	-0.2762***	-0.0051	0.0047	0.0036	0.0052
	(0.0612)	(0.0566)	(0.0575)	(0.0565)	(0.0383)	(0.0368)	(0.0372)	(0.0370)
Share reported cases		-0.0003*		-0.0003*		-0.0005***		-0.0003**
		(0.0002)		(0.0002)		(0.0002)		(0.0002)
Share reported deaths			-0.0051	-0.0000			-0.0089***	-0.0033
			(0.0034)	(0.0022)			(0.0033)	(0.0030)
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The variable Restricted x Post represents the interaction between q_j, which takes value one if sector j is restricted, and $post$, equal to one starting in March 2020. Share report deaths and Share report cases stand for reported deaths and cases per one million working-age population in each city, respectively. In the fixed-effects, by period, our base month is February (2020). Standard errors are presented in parentheses and clustered at the city-sector level. The regressions are weighted by each sector's share of employment in total employment in the pre-treatment period.

Table 4: Employment Effects by Gender

	Males				Females			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Log employment								
Restricted x Post	-0.1059***	-0.1012***	-0.1019***	-0.1010***	-0.0822	-0.0794	-0.0791	-0.0790
	(0.0294)	(0.0309)	(0.0306)	(0.0310)	(0.0635)	(0.0636)	(0.0634)	(0.0636)
Share reported cases		-0.0002**		-0.0002*		-0.0001		-0.0000
		(0.0001)		(0.0001)		(0.0002)		(0.0002)
Share reported deaths			-0.0041*	-0.0010			-0.0031	-0.0029
			(0.0022)	(0.0020)			(0.0040)	(0.0032)
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640
B. Log salaried employment								
Restricted x Post	-0.2662***	-0.2604***	-0.2624***	-0.2607***	-0.3496**	-0.3415**	-0.3424**	-0.3411**
	(0.0457)	(0.0439)	(0.0445)	(0.0440)	(0.1649)	(0.1559)	(0.1573)	(0.1557)
Share reported cases		-0.0003		-0.0004**		-0.0004		-0.0003
		(0.0002)		(0.0002)		(0.0004)		(0.0004)
Share reported deaths			-0.0039	0.0020			-0.0073	-0.0027
			(0.0034)	(0.0027)			(0.0075)	(0.0047)
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The variable Restricted x Post represents the interaction between q_j, which takes value one if sector j is restricted, and $post$, equal to one starting in March 2020. Share report deaths and Share report cases stand for reported deaths and cases per one million working-age population in each city, respectively. In the fixed-effects, by period, our base month is February (2020). Standard errors are presented in parentheses and clustered at the city-sector level. The regressions are weighted by each sector's share of employment in total employment in the pre-treatment period.

6. Robustness Checks

In this section, we assess different threats to identification. We begin by evaluating the robustness of our baseline results to alternative measurement of the outcomes. In section 6.2, we test for inter-sectoral spillovers, through which sector-specific restrictions may have indirect effects on the excluded sectors. In section 6.3, we account for potential heterogeneous contagion risk across sectors. Finally, we extend the time frame, and estimate heterogeneous treatment effect models.

6.1. Measurement of main variables

Our baseline results use 2-month moving averages to gain representativeness in our sample for each observation unit. Our estimates are robust to alternative specifications of this data handling. We estimate the model using monthly and 3-month moving averages employment measures in Tables A2 and A3 of the Appendix. As expected, the aggregate effect's magnitude decreases as we smooth our employment measures, and so does the sector-specific restriction coefficient. However, the significance and relative contribution to the total change in employment is similar across specifications.

6.2. Inter-sectoral spillover effects

One of the main threats to our identification strategy is the presence of complementarities between excluded and non-excluded sectors as a result of their input- and output-linkages. Those complementarities violate the assumption that the restrictions to the non-excluded sectors (our "treatment" group) do not affect the performance of excluded sectors (our "control" group). In the presence of strong inter-sectoral linkages, sector-specific restrictions might have affected the employment of excluded sectors as well, either by a reduction in the demand of goods from excluded sectors used as inputs in the production of non-excluded ones; or by the disruption in the supply of inputs required by excluded sectors produced by the non-excluded ones. In both cases, this would lead to underestimating the effect of sector-specific restrictions.

We re-estimate our main specification in a setting in which those linkages between excluded and non-excluded sectors do not threat our identification. For this, we first quantify how important are those linkages using the Colombian input-output matrix.[12] For each sector in the matrix (of a total 68 industries), an index between cero and one is assigned according to the share of pre-pandemic employment that belongs to the affected sub-sectors (according to our classification at the 4-digit level). Most industries in the matrix (54) are classified as entirely excluded or non-excluded from restrictions, so their index is either zero or one, respectively. In contrast, the remaining industries have values in the interval (0,1). Next, for each sector's total intermediate purchases, we compute the share bought from non-excluded sectors, using the sum-product of our index and the values of each inter-sectoral purchase. Similarly, for each sector's total inter-sectoral sales, we compute the share sold to non-excluded sectors. Figure A4 in the Appendix shows the values of those two shares for the 54 industries of the matrix classified as entirely excluded or non-excluded from restrictions. The central insight is that excluded sectors have, on average, a smaller share of both sales to non-excluded sectors and purchases from non-excluded sectors relative to non-excluded sectors. This finding is not surprising since one of the criteria for excluding activities from the mobility restrictions was being part of the supply chain or an essential sector. This fact favors our identification strategy because it makes inter-sectoral linkages deeper within control and treatment groups and less prominent across them.

We drop industries in the control group with strong linkages to restricted sectors to guarantee that the treatment's spillover effects do not affect our control group. For this, we first rank the two computed shares and exclude all subsectors of our initial 4-digit classification that belong to industries that are in the fourth quartiles of both shares. We re-estimate our main specification without those industries in the control group. Results are presented in Panel A of Table A4 in the Appendix.[13] As expected, excluding the control group sectors with strong linkages with the treatment amplifies the effect of sectoral restrictions. However, for all of our outcomes, the magnitudes of the increases are marginal. For instance, the new estimated coefficient for the effect of the restrictions in the specification controlling for cases and deaths is now -0.0936. This effect was -0.0935 in the baseline estimations. In panel B, we

[12] We use the input-output matrix from the Colombian national accounts provided by DANE for the available last year (2017) at the constant prices of 2015.

[13] Notice that columns 1 to 4 depict the same number of observations as in our baseline. This is because we exclude all the 4-digit industries that belong to the input-output matrix's selected industries and add up employment again to the sub-sector and city classification used as the unit of observation. Thus, the number of observations decreases in the only case all its subsectors were dropped for a given final industry.

tighten our criterion and drop from the control group all subsectors with computed shares above both indicators' medians. Most of our estimated coefficients rise in absolute terms, but again the magnitudes of the increases are not considerable. For example, in the specification that controls for cases and deaths, the new estimated coefficient is -0.0961, implying an increase in the additional jobs loss in the non-excluded sectors relative to the excluded sectors of only 0.23 pp compared to the baseline. The only meaningful change is that in the log of hourly wage regression, even though the DID coefficient's magnitude is very similar to the baseline regression, the coefficient is no longer significant in this stricter robustness exercise. Therefore, our estimations seem to be robust to the presence of spillover effects from the treatment, at least regarding the effects of lockdown policies on employment, both in extensive and intensive margins. As we argued, the fact that intermediate inputs of excluded sectors were also excluded from the restrictions makes our strategy less prone to suffer from possible bias related to inter-industry spillovers.

6.3. Heterogeneous contagion risk across sectors

Given the wide variety of activities and technologies, the risk of contagion could differ across sectors. The fact that the risks of spreading the virus within cities and sectors are heterogeneous could configure a reverse causality issue even after controlling for the study units' fixed effects. We address this potential source of bias by allowing the contagion risk to vary across sectors. For this, we measure the physical proximity between workers by sector, following Leibovici et al., (2020).[14] We then interact this measure with the time-varying measures of disease spread (cases and deaths). In addition, we create interaction terms with the measures of spread and the baseline share of sector employment within the city.

We include these interactions as controls in our baseline regressions for all outcomes. Results are presented in Table A5 of the Appendix. Columns 1 to 3 present results, including interactions with the proximity index. Columns 4 to 6 present results including interactions with the share of employment. Finally, columns 7 to 9 present results, including interactions with the proximity index and employment share. Results do not change significantly after including these new controls. For instance, in the regressions with the log of employment as the dependent variable, the coefficient of interest remains around -0.09. It continues to be

[14] For more details of the adaptation of this measure to Colombia see Bonilla et al. (2020).

significant after including the interaction of cases and deaths with physical proximity and the participation of sector employment and including both of them jointly. Therefore, our results are robust to any possible reverse causality bias due to the heterogeneous contagion risk across different sectors.

6.4 Time frame extension and heterogeneous treatment effects

Recent literature on DID estimation has pointed out that in DID designs, when treatment effects vary over time, the two-way fixed effects estimates might be biased, and the sign of the real treatment effect could be the opposite (Goodman-Bacon and Marcus, 2020). In particular, the treatment effect captured by the DID design, with multiple periods, is a weighted average of specific group effects with weights that might be negative. Therefore, there could be the extreme case in which the effect in every particular group are of the same sign, and the DID design estimate an opposite signed effect (Callaway and Sant'Anna, 2020; De Chaisemartin and D'Haultfœuille, 2020; Imai and Kim, 2020). Several robust estimators are offered in this literature to control for this possible source of bias. In these robustness exercises, we implement the correction proposed in De Chaisemartin and D'Haultfœuille, (2020). This method is general and can be applied to staggered and non-staggered designs; also, it provides useful diagnostic tools for identifying negative weights in a particular DID application.

Negative weights are often a problem in designs when treatment effects vary in time. In the baseline estimations presented in section 5, all affected segments of the labor market were simultaneously impacted. The post-policy period includes only the periods in which no additional sectors are excluded from the policy (March and April). Therefore, we expect the possibility of bias to be small. Nevertheless, in May and June, sub-sectors affected in the policy's initial stage became totally or partially excluded; in this case, heterogeneous effects could be a more relevant concern. In May of 2020, Decree 593 and 636 exclude sectors from the mobility restrictions, such as manufacture and construction. In June, Decree 639 and 749 exclude other sectors such as cleaning services, real estate, trade, professional activities, educational and research services.

For the baseline period and the extended period, we compute the diagnostic test and estimate the DID design using the method proposed by De Chaisemartin and D'Haultfœuille 2020)

(Multi-period DID). In all cases, Tables A6 and A7 in the Appendix also show the standard DID estimate. To extend the post-policy period, we follow two approaches. The first approach uses the baseline data structure and classifies as controls in the extended post-period subsectors that became totally or partially excluded. In the second approach, we use a more desegregated structure. In the extended period, the partitions excluded in the extended period are defined as independent industries throughout the entire panel; therefore, control industries in the post-period have no single subsector partially affected. This structure increases the sample and allows identifying with more precision the excluded sectors in the extended post-treatment period but to the cost of reducing the representability of the data.

The diagnostics test proposed in De Chaisemartin and D'Haultfœuille (2020) is based on the computation of the minimal standard deviation of group-specific ATT compatible with a standard DID of opposite sing to the real value under a reasonable amount of treatment effect heterogeneity. The more this standard deviation is computed away from zero, the less the concern of a biased standard DID coefficient. In Tables A6 and A7 of the Appendix, we present a multi-period DID estimation and the diagnostic tests for the baseline and extended study periods, respectively.

In the employment regression, using baseline data structure and specification, the multi-period DID results are very similar to the ones obtained with the standard DID estimation. The multiperiod DID point estimate coefficient of lockdowns' effects is around -6%; the standard DID estimation effect coefficient is included in the robust multi-period DID estimation's 99% confidence interval. The test suggests that standard DID coefficients can be of opposite signs from real value, only under an implausibly large amount of treatment heterogeneity. When we extend the study period to include May and June's months, the point estimate of the traditional DID treatment effect is around 7%. This effect is larger than the effect computed using the multi-period DID (around 4,3%), but it is included in the multi-period 99% confidence interval. As before, the De Chaisemartin and D'Haultfœuille (2020) test suggests that an implausible amount of heterogeneity would be required for the standard DID treatment effects coefficient to be oppositely signed to the real value.

Robustness checks results are similar in regressions with the average hours log as the dependent variable. The standard DID coefficient is around -2% either in the baseline time-framework as in the extended period. The multi-period DID point estimate effect of the lockdown policy is smaller, between 1.1% and 1.3% depending upon the time framework; in all cases, the DID coefficients are statistically significant. Even though the difference between traditional and multi-period is considerable, the De Chaisemartin and D'Haultfœuille (2020) test suggest that the possibility of a biased standard DID coefficient is not a concern. We find that the estimations of the lockdown policy effects in the hourly wage case are not robust to using alternative multi-period methodologies. Neither are they robust to the extension of the study period. The effect coefficient is a reduction of around 3.1% in hourly wages; nevertheless, once we extend the period to include May and June, the Restricted*Post coefficient is no longer significant. Using the multi-period DID method, we obtain no significant coefficients in neither of the two study periods.

Finally, in Table A7 of the Appendix, we estimate a final set of robustness checks in a more disaggregated dataset, varying at the 4-digit ISIC and city level. While this dataset may suffer from representativity problems, particularly in small cities, results are fairly similar. The log of employment coefficients are similar to the ones we present in our baseline estimation; in addition, the negative and significant effect of lockdowns is robust to the multi-period DID methodology in the baseline period and the extended one. The regressions with the log of hours as the dependent variables have similar results to the one we described in the previous paragraph. However, the multi-period DID coefficients are significant only at the 5% level. Finally, as in our baseline sample, we do not find any effects on wages. In the case of employment, our baseline results hold robust to using alternative estimators and expanding the post-period in the extensive and intensive margins.

7. Conclusions

Both the Covid-19 pandemic and the lockdowns required to flatten the contagion curve triggered a global economic crisis with substantial effects on the labor markets. We assess the effect of the sector-specific restrictions implied by the lockdown imposed in March and April in Colombia, a country characterized by a high prevalence of informality and an early implementation of the lockdown, shared features with many other emerging economies. We identify the effect of the sector-specific restrictions with difference-in-differences and event-study models that exploit the variation in the excluded and non-excluded sectors and the timing of the restriction policies. Our main results are robustness to a number of alternative specifications, confirming that we are properly addressing the main threats to identification.

We find that sector-specific restrictions had a negative effect on employment, accounting for approximately one-quarter of the total employment losses between February and April. Therefore, we should expect important employment losses even in the absence of such restrictions. The remaining three-quarters of the variation is plausibly explained by the disease's regional patterns spread and other epidemiological and economic factors homogeneously affecting the country during this period, captured by the time fixed-effects coefficients. Even though we cannot make causal claims about these two factors, we find that the regional variation of the disease spread may explain about a fourth of the total employment variations, suggesting that containing the disease would have significant positive effects on employment. Overall, our findings are consistent with previous literature showing a moderate impact of the sector-specific restrictions implied by the lockdown in developed economies compared to the aggregate shocks implied by the pandemic (Aum et al., 2020; Forsythe et al., 2020; Gupta et al., 2020; Lozano Rojas et al., 2020; Goolsbee and Syverson, 2021).

In the intensive margin, we find that sector-specific restrictions had negative but smaller effects on average worked hours and wages. Our results also show that the impact of the sector-specific restrictions on employment losses was mainly driven by salaried jobs, which suggests that labor market rigidities may be amplifying the effect of sectoral lockdowns. In the salaried segmented, the contribution of the restrictions was around a half of the total job loss. This result has implications for the speed of recovery of employment because in Colombia, as well as in other emerging economies, there are important job creation costs for the salaried segment. Finally, we identify that within the group of salaried workers there is a differential impact of those restrictions by gender. Our results suggest that around a third part of the widening of the salaried employment gender gap could be attributable to the sector-specific restrictions.

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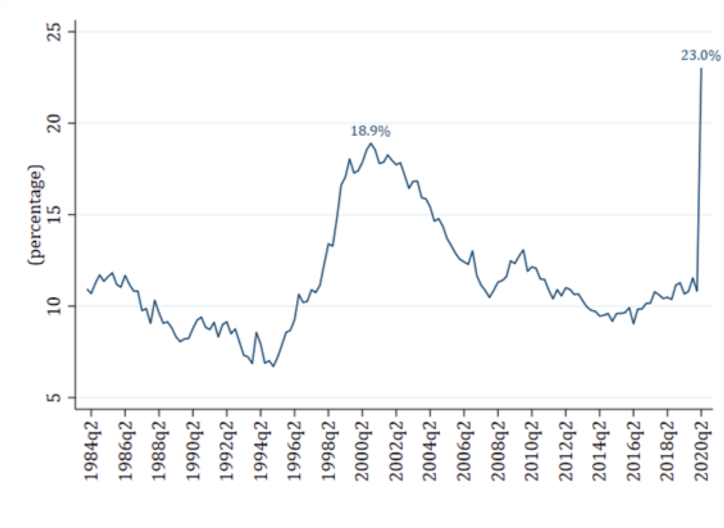
Appendix

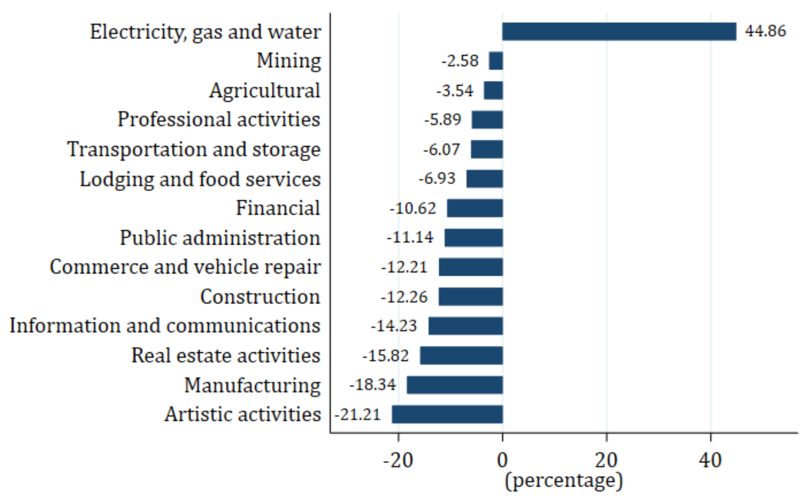
Additional Figures

Figure A1. Urban Unemployment Rate.



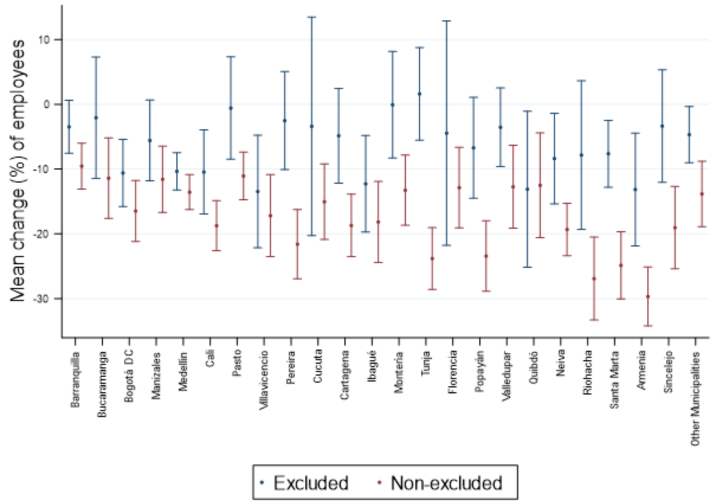
Notes: Seasonally adjusted quarterly moving average. The historical urban unemployment rate is computed for seven cities: Bogotá, Cali, Medellín, Barranquilla, Bucaramanga, Manizales and Pasto. Source: Calculation by the authors based on data from DANE (GEIH).

Figure A2. Annual employment growth by sector in April 2020



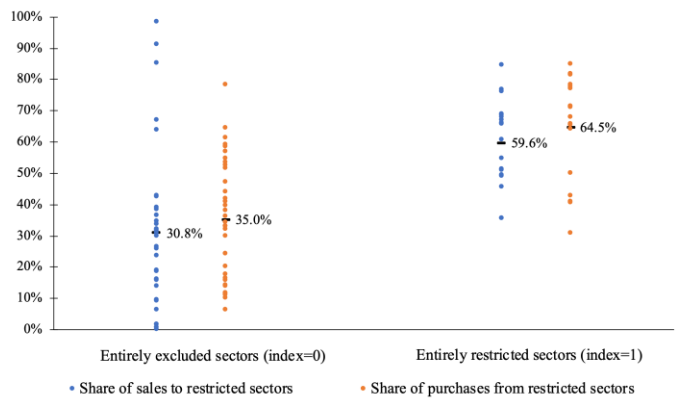
Notes: Seasonally adjusted quarterly moving average. **Source:** Calculations by the authors based on data from DANE (GEIH)

Figure A3. Employment growth of excluded and non-excluded sectors by city (Feb. 2020 – Apr. 2020)



Source: Calculations by the authors based on data from DANE (GEIH). Quarterly moving average. 90% confidence intervals. Weighted average.

Figure A4. Shares of sales and purchases to/from restricted sectors



Notes: Only for the industries of the Colombian input-output matrix that were classified as entirely excluded (index=0) or entirely restricted (index=1) (54 industries of 68). The remaining industries have index values between zero and one. Line markers show averages of each group.

Table A1. Summary Statistics of the city-sectors by city

	Excluded					Non-excluded				
	Observations	February (2020)	April (2020)	Change		Observations	February (2020)	April (2020)	Change	
		Mean	Mean	Number	%		Mean	Mean	Number	%
BARRANQUILLA										
Hourly Wage	12	4.794	4.741	-53	-1,1	10	3.166	3.524	358	11,3
Employment	12	26.437	25.563	-873	-3,3	10	59.538	53.906	-5.632	-9,5
Deaths per million in working age population	12	0,0	11,5	11,5	-	10	0,0	11,5	11,5	-
Cases per million in working age population	12	0,0	152,5	152,5	-	10	0,0	152,5	152,5	-
BUCARAMANGA										
Hourly Wage	12	4.668	5.152	485	10,4	10	3.711	3.391	-320	-8,6
Employment	12	18.268	17.670	-598	-3,3	10	33.295	29.407	-3.888	-11,7
Deaths per million in working age population	12	0,0	2,2	2,2	-	10	0,0	2,2	2,2	-
Cases per million in working age population	12	0,0	20,6	20,6	-	10	0,0	20,6	20,6	-
BOGOTÁ D.C										
Hourly Wage	12	7.062	7.932	871	12,3	10	4.046	4.032	-14	-0,3
Employment	12	146.071	130.207	-15.864	-10,9	10	243.223	203.339	-39.884	-16,4
Deaths per million in working age population	12	0,0	17,0	17,0	-	10	0,0	17,0	17,0	-
Cases per million in working age population	12	0,0	324,4	324,4	-	10	0,0	324,4	324,4	-
MANIZALES										
Hourly Wage	12	4.924	5.436	512	10,4	10	4.020	3.763	-257	-6,4
Employment	12	6.899	6.481	-418	-6,1	10	10.668	9.417	-1.251	-11,7
Deaths per million in working age population	12	0,0	0,0	0,0	-	10	0,0	0,0	0,0	-
Cases per million in working age population	12	0,0	30,4	30,4	-	10	0,0	30,4	30,4	-
MEDELLÍN										
Hourly Wage	12	6.936	6.022	-914	-13,2	10	4.484	4.554	70	1,6
Employment	12	59.883	53.606	-6.277	-10,5	10	111.456	96.202	-15.253	-13,7
Deaths per million in working age population	12	0	1	1	-	10	0	1	1	-
Cases per million in working age population	12	0	95	95	-	10	0	95	95	-
CALI										
Hourly Wage	12	6.357	4.768	-1.588	-25,0	10	3.874	3.747	-128	-3,3
Employment	12	39.476	35.245	-4.231	-10,7	10	79.823	64.792	-15.031	-18,8
Deaths per million in working age population	12	0	20	20	-	10	0	20	20	-
Cases per million in working age population	12	0	328	328	-	10	0	328	328	-
PASTO										
Hourly Wage	12	4.096	4.567	471	11,5	10	3.444	2.757	-687	-20,0
Employment	12	6.866	6.705	-162	-2,4	10	10.455	9.265	-1.190	-11,4
Deaths per million in working age population	12	0	6	6	-	10	0	6	6	-
Cases per million in working age population	12	0	64	64	-	10	0	64	64	-
VILLAVICENCIO										
Hourly Wage	12	5.257	6.102	845	16,1	10	3.424	3.101	-323	-9,4
Employment	12	8.101	7.034	-1.067	-13,2	10	13.008	10.737	-2.271	-17,5
Deaths per million in working age population	12	0	12	12	-	10	0	12	12	-
Cases per million in working age population	12	0	872	872	-	10	0	872	872	-
PEREIRA										
Hourly Wage	12	5.062	4.425	-636	-12,6	10	3.675	3.431	-244	-6,6
Employment	12	9.037	8.822	-214	-2,4	10	17.741	13.860	-3.880	-21,9
Deaths per million in working age population	12	0	11	11	-	10	0	11	11	-
Cases per million in working age population	12	0	304	304	-	10	0	304	304	-
CÚCUTA										
Hourly Wage	12	3.831	4.862	1.030	26,9	10	2.636	2.243	-393	-14,9
Employment	12	10.295	9.862	-433	-4,2	10	21.160	17.946	-3.214	-15,2
Deaths per million in working age population	12	0	7	7	-	10	0	7	7	-
Cases per million in working age population	12	0	69	69	-	10	0	69	69	-
CARTAGENA										
Hourly Wage	12	6.112	4.668	-1.444	-23,6	10	4.052	3.562	-491	-12,1
Employment	12	13.416	12.743	-673	-5,0	10	28.644	23.187	-5.458	-19,1
Deaths per million in working age population	12	0	27	27	-	10	0	27	27	-
Cases per million in working age population	12	0	280	280	-	10	0	280	280	-
IBAGUÉ										
Hourly Wage	12	4.325	4.357	33	0,8	10	3.825	3.597	-227	-5,9
Employment	12	8.174	7.178	-996	-12,2	10	12.539	10.231	-2.308	-18,4
Deaths per million in working age population	12	0	2	2	-	10	0	2	2	-
Cases per million in working age population	12	0	119	119	-	10	0	119	119	-
MONTERÍA										
Hourly Wage	12	4.378	3.730	-648	-14,8	10	3.022	3.252	230	7,6
Employment	12	4.879	4.911	32	0,7	10	9.622	8.331	-1.291	-13,4
Deaths per million in working age population	12	0	3	3	-	10	0	3	3	-
Cases per million in working age population	12	0	54	54	-	10	0	54	54	-
TUNJA										
Hourly Wage	12	5.444	6.111	667	12,2	10	3.387	3.271	-116	-3,4
Employment	12	3.558	3.588	30	0,8	10	4.031	3.068	-963	-23,9
Deaths per million in working age population	12	0	6	6	-	10	0	6	6	-
Cases per million in working age population	12	0	31	31	-	10	0	31	31	-
FLORENCIA										
Hourly Wage	12	4.178	4.069	-108	-2,6	10	3.435	3.145	-291	-8,5
Employment	12	2.395	2.309	-86	-3,6	10	3.443	3.007	-436	-12,7
Deaths per million in working age population	12	0	8	8	-	10	0	8	8	-
Cases per million in working age population	12	0	62	62	-	10	0	62	62	-
POPAYÁN										
Hourly Wage	12	3.881	3.899	19	0,5	10	4.392	3.824	-568	-12,9
Employment	12	4.252	3.962	-290	-6,8	10	5.909	4.520	-1.389	-23,5
Deaths per million in working age population	12	0	0	0	-	10	0	0	0	-
Cases per million in working age population	12	0	46	46	-	10	0	46	46	-
VALLEDUPAR										
Hourly Wage	12	5.092	4.998	-94	-1,8	10	3.441	2.967	-475	-13,8
Employment	12	5.555	5.333	-221	-4,0	10	9.817	8.568	-1.249	-12,7
Deaths per million in working age population	12	0	12	12	-	10	0	12	12	-
Cases per million in working age population	12	0	95	95	-	10	0	95	95	-
QUIBDÓ										
Hourly Wage	12	4.693	4.591	-102	-2,2	10	3.574	2.993	-581	-16,3
Employment	12	1.271	1.083	-188	-14,8	10	1.915	1.651	-264	-13,8
Deaths per million in working age population	12	0	0	0	-	10	0	0	0	-
Cases per million in working age population	12	0	153	153	-	10	0	153	153	-
NEIVA										
Hourly Wage	12	5.135	4.810	-324	-6,3	10	3.341	2.992	-349	-10,5
Employment	12	5.195	4.723	-473	-9,1	10	7.480	6.043	-1.437	-19,2
Deaths per million in working age population	12	0	15	15	-	10	0	15	15	-
Cases per million in working age population	12	0	238	238	-	10	0	238	238	-
RIOHACHA										
Hourly Wage	12	4.782	4.599	-183	-3,8	10	3.153	3.494	342	10,8
Employment	12	3.105	2.902	-203	-6,5	10	5.591	4.086	-1.505	-26,9
Deaths per million in working age population	12	0	5	5	-	10	0	5	5	-
Cases per million in working age population	12	0	27	27	-	10	0	27	27	-
SANTA MARTA										
Hourly Wage	12	4.968	5.156	187	3,8	10	3.057	2.808	-249	-8,2
Employment	12	6.062	5.576	-486	-8,0	10	12.940	9.725	-3.215	-24,8
Deaths per million in working age population	12	0	33	33	-	10	0	33	33	-
Cases per million in working age population	12	0	368	368	-	10	0	368	368	-
ARMENIA										
Hourly Wage	12	4.177	4.934	757	18,1	10	3.119	3.068	-51	-1,6
Employment	12	4.345	3.763	-582	-13,4	10	7.404	5.223	-2.181	-29,5
Deaths per million in working age population	12	0	4	4	-	10	0	4	4	-
Cases per million in working age population	12	0	123	123	-	10	0	123	123	-
SINCELEJO										
Hourly Wage	12	3.321	3.167	-154	-4,6	10	2.959	4.325	1.366	46,2
Employment	12	3.913	3.727	-186	-4,7	10	7.650	6.154	-1.496	-19,6
Deaths per million in working age population	12	0	0	0	-	10	0	0	0	-
Cases per million in working age population	12	0	0	0	-	10	0	0	0	-
OTHER MUNICIPALITIES										
Hourly Wage	12	3.640	3.754	114	3,1	10	2.950	2.942	-9	-0,3
Employment	12	493.460	470.015	-23.444	-4,8	10	423.260	364.797	-58.463	-13,8
Deaths per million in working age population	12	0	3	3	-	10	0	3	3	-
Cases per million in working age population	12	0	40	40	-	10	0	40	40	-

Source: Calculations by the authors based on data from DANE (GEIH).

Table A2. Log of Employment (monthly regression)

	Log. Employment			Log. Average hours			Log. Hourly wage		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Restricted x Post	-0.1309***	-0.1321***	-0.1306***	-0.0147	-0.0149	-0.0148	-0.0433	-0.0425	-0.0430
	(0.0354)	(0.0354)	(0.0354)	(0.0098)	(0.0098)	(0.0098)	(0.0292)	(0.0293)	(0.0292)
Share reported cases	-0.0004***		-0.0003**	-0.0000		-0.0000	0.0001		0.0001
	(0.0002)		(0.0001)	(0.0000)		(0.0000)	(0.0001)		(0.0002)
Share reported deaths		-0.0079***	-0.0024		-0.0000	0.0005		0.0003	-0.0016
		(0.0029)	(0.0023)		(0.0006)	(0.0006)		(0.0026)	(0.0032)
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The variable Restricted x Post represents the interaction between q_j, which takes value one if sector j is restricted and $post$, which is equal to one starting in March 2020. Share report deaths and Share report cases stand for reported deaths and cases per one million working-age population in each city, respectively. In the fixed-effects, by period, our base month is February (2020). Standard errors are presented in parentheses and clustered at the city-sector level. For each outcome, we use the same specifications with control covariates as in Table 2, in which we present baseline estimation results. The regressions are weighted by each sector's share of employment in total employment in the pre-treatment period.

Table A3. Log of Employment (three-month moving average regression)

	Log. Employment			Log. Average hours			Log. Hourly wage		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Restricted x Post	-0.0520***	-0.0526***	-0.0520***	-0.0099*	-0.0098*	-0.0099*	-0.0296**	-0.0293**	-0.0295**
	(0.0183)	(0.0180)	(0.0183)	(0.0055)	(0.0055)	(0.0055)	(0.0127)	(0.0127)	(0.0127)
Share reported cases	-0.0001**		-0.0001**	0.0000		0.0000	0.0000		0.0000
	(0.0001)		(0.0001)	(0.0000)		(0.0000)	(0.0000)		(0.0001)
Share reported deaths		-0.0019	0.0002		-0.0001	-0.0002		-0.0002	-0.0009
		(0.0012)	(0.0012)		(0.0004)	(0.0003)		(0.0009)	(0.0011)
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The variable Restricted x Post represents the interaction between q_j, which takes value one if sector j is restricted and $post$, equal to one starting in March 2020. Share report deaths and Share report cases stand for reported deaths and cases per one million working-age population in each city, respectively. In the fixed-effects, by period, our base month is February (2020). Standard errors are presented in parentheses and clustered at the city-sector level. For each outcome, we use the same specifications with control covariates as in Table 2, in which we present baseline estimation results. The regressions are weighted by each sector's share of employment in total employment in the pre-treatment period.

Table A4. Robustness to spillover effects of treatment

	Log. Employment			Log. Average hours			Log. Hourly wage		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Exclude 4th quartile									
Restricted x Post	-0.0939***	-0.0946***	-0.0936***	-0.0175**	-0.0174**	-0.0175**	-0.0304*	-0.0301*	-0.0304*
	(0.0231)	(0.0229)	(0.0232)	(0.0082)	(0.0082)	(0.0082)	(0.0161)	(0.0162)	(0.0161)
Share reported cases	-0.0003***		-0.0002**	0.0000		0.0000	0.0001		0.0001
	(0.0001)		(0.0001)	(0.0000)		(0.0000)	(0.0001)		(0.0001)
Share reported deaths		-0.0051***	-0.0017		-0.0000	-0.0002		0.0010	-0.0000
		(0.0018)	(0.0015)		(0.0005)	(0.0004)		(0.0012)	(0.0015)
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640
B. Exclude 3rd and 4th quartile									
Restricted x Post	-0.0965***	-0.0970***	-0.0961***	-0.0231***	-0.0230***	-0.0231***	-0.0302	-0.0292	-0.0301
	(0.0246)	(0.0243)	(0.0247)	(0.0081)	(0.0081)	(0.0081)	(0.0190)	(0.0193)	(0.0190)
Share reported cases	-0.0002**		-0.0001	0.0000		0.0000	0.0001		0.0001
	(0.0001)		(0.0001)	(0.0000)		(0.0000)	(0.0001)		(0.0001)
Share reported deaths		-0.0047**	-0.0025		-0.0000	-0.0001		0.0010	-0.0011
		(0.0018)	(0.0015)		(0.0005)	(0.0004)		(0.0014)	(0.0015)
Observations	2,400	2,400	2,400	2,400	2,400	2,400	2,400	2,400	2,400

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Panel A drop from the control group industries in the Colombian input-matrix with both shares of sales to restricted sectors and shares of purchases from restricted sectors in the fourth quartile of their corresponding distributions. Panel B drops industries with both shares above the median of their corresponding distributions. Standard errors are presented in parentheses and clustered at the city-sector level. For each outcome, we use the same specifications with control covariates as in Table 2, in which we present baseline estimation results. The regressions are weighted by each sector's share of employment in total employment in the pre-treatment period.

Table A5. Controlling for heterogeneous risk of contagion across sectors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Log employment									
Restricted x Post	-0.0917***	-0.0907***	-0.0892***	-0.0935***	-0.0938***	-0.0932***	-0.0914***	-0.0904***	-0.0895***
	(0.0239)	(0.0243)	(0.0246)	(0.0232)	(0.0233)	(0.0235)	(0.0242)	(0.0245)	(0.0247)
Share reported cases	0.0019*		-0.0028*	-0.0003***		-0.0002	0.0019*		-0.0028*
	(0.0011)		(0.0016)	(0.0001)		(0.0002)	(0.0011)		(0.0016)
Share reported deaths		0.0468**	0.0917***		-0.0056***	-0.0017		0.0458**	0.0925***
		(0.0192)	(0.0319)		(0.0021)	(0.0032)		(0.0194)	(0.0307)
Interactions with proximity index	YES	YES	YES	NO	NO	NO	YES	YES	YES
Interactions with share employment	NO	NO	NO	YES	YES	YES	YES	YES	YES
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640
B. Log. Average hours									
Restricted x Post	-0.0174**	-0.0172**	-0.0170**	-0.0170**	-0.0165**	-0.0168**	-0.0170**	-0.0165**	-0.0167**
	(0.0082)	(0.0080)	(0.0079)	(0.0076)	(0.0074)	(0.0075)	(0.0076)	(0.0074)	(0.0074)
Share reported cases	-0.0001		-0.0007	-0.0001		-0.0000	-0.0001		-0.0004
	(0.0003)		(0.0005)	(0.0000)		(0.0001)	(0.0003)		(0.0005)
Share reported deaths		0.0008	0.0115		-0.0010*	-0.0008		-0.0012	0.0053
		(0.0061)	(0.0101)		(0.0006)	(0.0007)		(0.0055)	(0.0086)
Interactions with proximity index	YES	YES	YES	NO	NO	NO	YES	YES	YES
Interactions with share employment	NO	NO	NO	YES	YES	YES	YES	YES	YES
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640
C. Log. Hourly wage									
Restricted x Post	-0.0331**	-0.0331**	-0.0335**	-0.0328**	-0.0329**	-0.0334**	-0.0333**	-0.0336**	-0.0340**
	(0.0163)	(0.0164)	(0.0162)	(0.0163)	(0.0163)	(0.0161)	(0.0162)	(0.0161)	(0.0160)
Share reported cases	-0.0004		0.0006	0.0001		0.0000	-0.0004		0.0005
	(0.0012)		(0.0019)	(0.0001)		(0.0002)	(0.0012)		(0.0018)
Share reported deaths		-0.0097	-0.0199		0.0015	0.0013		-0.0083	-0.0153
		(0.0194)	(0.0282)		(0.0016)	(0.0025)		(0.0201)	(0.0284)
Interactions with proximity index	YES	YES	YES	NO	NO	NO	YES	YES	YES
Interactions with share employment	NO	NO	NO	YES	YES	YES	YES	YES	YES
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. Columns 1 to 3 present results including interactions with the proximity index. Columns 4 to 6 present results including interactions with the share of employment. Finally, columns 7 to 9 present results, including interactions with the proximity index and employment share. For each outcome, we use the same specifications with control covariates as in Table 2, in which we present baseline estimation results. The regressions are weighted by each sector's share of employment in total employment in the pre-treatment period.

Table A6. DID with heterogeneous treatment effects

	Log. Employment			Log. Average hours			Log. Hourly wage		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Until April									
Restricted x Post	-0.0937***	-0.0944***	-0.0935***	-0.0173**	-0.0172**	-0.0173**	-0.0327**	-0.0323*	-0.0326**
	(0.0229)	(0.0227)	(0.0230)	(0.0082)	(0.0082)	(0.0082)	(0.0164)	(0.0166)	(0.0164)
TEST	1,4787	1,5438	1,4312	0,2733	0,2818	0,2646	0,5158	0,5292	0,4997
Restricted x Post (did_multiplegt)	-0.0612***	-0.0615***	-0.0612***	-0.0125**	-0.0126**	-0.0125**	0.00708	0.00712	0.00708
se (did_multiplegt)	0.0184	0.0182	0.0184	0.00547	0.00546	0.00549	0.0183	0.0184	0.0183
Observations	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640	2,640
B. Until June									
Restricted x Post	-0.0674***	-0.0673***	-0.0677***	-0.0212***	-0.0212***	-0.0212***	0.0036	0.0039	0.0032
	(0.0224)	(0.0223)	(0.0225)	(0.0061)	(0.0061)	(0.0061)	(0.0429)	(0.0428)	(0.0427)
TEST	0,2960	0,2957	0,2973	0,0931	0,0930	0,0929	0,0156	0,0171	0,0142
Restricted x Post (did_multiplegt)	-0.0439***	-0.0438***	-0.0437***	-0.0106***	-0.0106***	-0.0107***	0.0243	0.0243	0.0246
se (did_multiplegt)	0.0162	0.0162	0.0166	0.00405	0.00397	0.00369	0.0284	0.0282	0.0297
Observations	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696	3,696

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. In panel A of the table, we show regressions results for the baseline period; in panel B, we present results for the extended period. This estimation uses the data's baseline structure; for the extended period, we consider as controls sub-sectors that were totally or partially excluded from the lockdown policy. Columns in each panel mimic the baseline specifications presented for each outcome in Table 2. The coefficient Restricted x Post, represents the standard DID coefficient presented in Table 2. The coefficient Restricted x Post (did_multiplegt) represents the DID estimator using the De Chaisemartin and D'Haultfœuille, (2020) methodology. The test is based on the computation of the minimal standard deviation of group-specific ATE, compatible with a standard DID of opposite sing to the population with a reasonable amount of treatment effect heterogeneity. The more this standard deviation is computed away from zero, the less the concern of a biased standard DID coefficient. De Chaisemartin and D'Haultfœuille (2020) suggest a simple way of evaluating the magnitude of this parameter. Let us call the DID traditional coefficient beta and assume there is a value B which in absolute value is greater than the effect in every group and period. If |beta|<sqrt(3)x, and B<sqrt(3)x, where x represent the value of the test parameter, therefore, x would be an implausibly high amount of treatment effect heterogeneity so beta could be of a different sign than the real treatment effect. In all regressions |beta|<sqrt(3)x, the assumed B value could be several times the estimated beta and the second condition still holds. For each outcome, we use the same specifications with control covariates as in Table 2, in which we present baseline estimation results. The regressions are weighted by each sector's share of employment in total employment in the pre-treatment period.

Table A7. DID with heterogeneous treatment effects (4-digit ISIC and city level data)

	Log. Employment			Log. Average hours			Log. Hourly wage		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Until April									
Restricted x Post	-0.0953***	-0.0959***	-0.0950***	-0.0174**	-0.0172**	-0.0173**	-0.0240	-0.0235	-0.0238
	(0.0231)	(0.0229)	(0.0231)	(0.0084)	(0.0084)	(0.0084)	(0.0166)	(0.0167)	(0.0166)
Test	1,3650	1,4542	1,3316	0,2493	0,2612	0,2427	0,3438	0,3567	0,3337
Restricted x Post (did_multiplegt)	-0.0653***	-0.0656***	-0.0653***	-0.0128**	-0.0129**	-0.0128**	0.0136	0.0137	0.0136
se (did_multiplegt)	0.0232	0.0231	0.0232	0.00767	0.00767	0.00766	0.0136	0.0136	0.0136
Observations	3,290	3,290	3,290	3,290	3,290	3,290	3,290	3,290	3,290
B. Until June									
Restricted x Post	-0.0868***	-0.0867***	-0.0871***	-0.0177***	-0.0177***	-0.0177***	-0.0332	-0.0330	-0.0336
	(0.0214)	(0.0212)	(0.0215)	(0.0062)	(0.0062)	(0.0062)	(0.0373)	(0.0371)	(0.0374)
Test	0,3832	0,3830	0,3842	0,0781	0,7811	0,0781	0,1465	0,1456	0,1483
Restricted x Post (did_multiplegt)	-0.0513***	-0.0511***	-0.0511***	-0.0118**	-0.0117**	-0.0120**	0.00607	0.00596	0.00643
se (did_multiplegt)	0.0218	0.0209	0.0209	0.00667	0.00666	0.00650	0.0246	0.0242	0.0245
Observations	4,606	4,606	4,606	4,606	4,606	4,606	4,606	4,606	4,606

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. In panel A of the table, we show regressions results for the baseline period; in panel B, we present results for the extended period. This estimation uses a modified structure of the data structure in a way that, in the extended period and in all cases, the ones that are excluded have no single subsector partially affected.; for the extended period we consider as controls sub-sectors that were totally or partially excluded from the lockdown policy, Columns in each panel mimic the baseline specifications presented for each outcome in Table 2. The coefficient Restricted x Post, represents the standard DID coefficient presented in Table 2. The coefficient Restricted x Post (did_multiplegt), represents the DID estimator using the De Chaisemartin and D'Haultfœuille, (2020) methodology. The test is based on the computation of the minimal standard deviation of group-specific ATE, compatible with a standard DID of opposite sing to the population with a reasonable amount of treatment effect heterogeneity. The more this standard deviation is computed away from zero, the less the concern of a biased standard DID coefficient. De Chaisemartin and D'Haultfœuille, (2020) suggest a simple way of evaluating the magnitude of this parameter. Let us call the DID traditional coefficient beta and assume there is a value B which in absolute value is greater than the effect in every group and period. If |beta|<sqrt(3)x, and B<sqrt(3)x, where x represent the value of the test parameter, therefore, x would be an implausibly high amount of treatment effect heterogeneity so beta could be of a different sign than the real treatment effect. In all regressions |beta|<sqrt(3)x, the assumed B value could be several times the estimated beta, and the second condition still holds. For each outcome, we use the same specifications with control covariates as in Table 2, in which we present baseline estimation results. The regressions are weighted by each sector's share of employment in total employment in the pre-treatment period.