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Lockdown and COVID-19: Brazilian Evidence♦

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Abstract

We estimate the impact of strict social distancing policies on isolation, COVID-19 cases and deaths, and employment in Brazil. Compiling social distancing decrees and combining them with publicly available data, we identify a set of treated municipalities that adopted lockdown between May and June 2021 in the State of São Paulo and build a control group with cities from the same Regional Health Departments. We estimate the lockdown effects using a Difference-in-Differences model with two-way fixed effects and staggered adoption. Our findings suggest that the policy increased social distancing one week after its adoption, decreased cases from two weeks on, reduced deaths from four weeks on, and did not significantly impact employment.

Keywords

COVID-19; Lockdown; Mortality; Employment; Brazil.

Resumo

Nós estimamos o impacto de políticas restritivas de distanciamento social sobre isolamento, casos e mortes de COVID-19 e emprego no Brasil. Compilando decretos de distanciamento social e combinando-os com dados públicos, nós identificamos um conjunto de municípios tratados que adotaram *lockdown* entre maio e junho de 2021 no Estado de São Paulo e construímos um grupo de controle com cidades dos mesmos Departamentos Regionais de Saúde. Nós estimamos os efeitos do *lockdown* usando um modelo de Diferença-em-Diferenças com efeitos fixos de dois níveis e adoção escalonada. Nossas conclusões sugerem que a política aumentou o isolamento social uma semana após sua adoção, diminuiu os casos a partir de duas semanas, reduziu as mortes a partir de quatro semanas e não teve impactos significativos no emprego.

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JEL Classification
H12, I18, J20.

1. Introduction

"We are sorry for all the deaths, but it is everyone's fate."

– Former President Jair Bolsonaro; June 2, 2020.[1]

The fact that the COVID-19 pandemic was poorly controlled in Brazil is globally recognized. The lack of coordination between municipal and state policies, the absence of national leadership in fighting the pandemic, and the negligent behavior of federal government representatives (especially President Jair Bolsonaro) regarding the severity of this global phenomenon resulted in an uncontrolled spread of the virus throughout the country (Castro et al. 2021; Ajzenman, Cavalcanti, and Da Mata 2020). These factors, combined with a delay to acquire vaccines and the emergence of new variants of concern (VOCs) resulted in a frightening scenario in Brazil for almost two years, which reached more than 30 million cases and nearly 700 thousand deaths as a direct result of the pandemic according to the Ministry of Health.[2]

One tool that became commonplace around the world throughout the pandemic to try to stem the growth in cases and deaths associated with COVID-19 was non-pharmaceutical interventions (NPIs) of social distancing, which attempt to decrease infection rates by reducing social interactions. The most strict version of this type of restriction has come to be known as "lockdown", a situation in which all non-essential activities of the economy are suspended, and individuals' ability to move about is limited to strictly necessary activities. Debates about the effectiveness of this type of policy have been intense in both public and academic arenas.

[1] Then, Brazil had more than 30 thousand deaths from COVID-19. Media coverage for this pronunciation here https://noticias.uol.com.br/saude/ultimas-noticias/redacao/2020/06/02/bolsonaro-a-gente-lamenta-todos-os-mortos-mas-e-o-destino-de-todo-mundo.htm, access in 20/11/2022.

[2] https://covid.saude.gov.br/. Access in 13/11/2022.

This paper seeks to contribute to this debate about the impacts of social distancing measures. The importance of this type of analysis cannot be underestimated, especially in the Brazilian context that combined: i) a slow pace of vaccination; ii) the emergence of VOCs; and iii) a heated debate about the effectiveness of NPIs, taking into account the hypothetical trade-off between economic performance and health. The policies of interest here were adopted during the second wave of the pandemic in Brazil when new VOCs began circulating in the country and the number of cases and deaths jumped within a few weeks. Despite the relaxation of measures at the state level in São Paulo, some mayors were forced to adopt stricter policies to decrease the number of infections and hospitalizations in a scenario of few vacancies available in hospitals.

Understanding the effects of lockdowns in the Brazilian context requires an empirical evaluation of the impact of these policies on epidemiological and socioeconomic variables, such as social isolation, cases, deaths, and employment. Natural hypotheses to be tested are that the measures i) *increase* social isolation, ii) *decrease* cases, iii) *decrease* deaths, and iv) *increase* unemployment.

To test these hypotheses, we develop the following econometric framework. First, we identify 15 municipalities in the Regional Health Departments (DRSs) of Araraquara, Barretos, Franca, and Ribeirão Preto that adopted lockdown between May and June 2021. Next, we build a control group using municipalities from the same DRSs – we show that the control group is balanced across different observable characteristics compared to the treatment group. Finally, we use a Differences-in-Differences (DiD) model with two-way fixed effects (TWFE) in an Event Study (ES) design to temporally align policy adoption and estimate the daily municipal-level impact of the lockdown policy on the outcomes of interest.

In summary, our findings suggest that social distancing grows in the week after lockdown adoption, cases fall more strongly three weeks later, deaths fall more strongly five weeks later, and employment does not change significantly between the treatment and control groups. Thus, it appears that the lockdown policy is epidemiologically effective by reducing cases and deaths through increased social isolation and does not have significant employment costs.

Studies of this type contribute to the growing literature dedicated to analyzing the effect of NPIs, discussed in Section 2, and may be useful to inform future decisions in the context of contagious disease pandemics, such as COVID-19. To the best of our knowledge, we provide the first econometrically rigorous evaluation of social distancing policies in Brazil looking at both epidemiological and socioeconomic outcomes. Such an integrated analysis of different outcomes allows one to assess the temporal consistency of the results, using benchmarks from the medical literature about the progression of the virus in the human body, as a robustness check.

This article is divided into five sections other than this introduction. Section 2 assesses the literature on the topic, and how it relates to our study. Section 3 presents the data used in the analysis, Section 4 outlines the empirical framework, Section 5 discusses the results and their robustness, and Section 6 concludes.

2. Literature Review

The number of published studies related to the COVID-19 pandemic is extensive, given its global nature. In the field of economic sciences, one can simplistically divide the studies into three major groups.

The first focuses on developing predictive models of the trajectory of cases and deaths associated with the disease, in order to assist policymakers in their decision-making through the acknowledgment of possible future scenarios – e.g., Zeroual et al. (2020) and Luo (2021). The second seeks to assess the impact of the *pandemic* on different outcomes such as employment and inequality – e.g., Alon et al. (2020), Beland, Brodeur, and Wright (2020), Blundell et al. (2020), and Fairlie, Couch, and Xu (2020). Finally, the third group aims to assess the impacts of *policies* adopted to combat the pandemic on different outcomes such as cases, deaths, social isolation, and employment – e.g., Akim and Ayivodji (2020), Dave et al. (2020), Bargain and Aminjonov (2020), Kong and Prinz (2020), and Goolsbee and Syverson (2021).

Given our goals here, the last group is the one that interests us the most. Below, we list some important works and results from this literature and highlight points to which one should pay attention when analyzing the Brazilian case. We also draw methodological inspiration from these papers, to help us estimate the causal impacts of the lockdown policies.

Notably, despite growing, the literature on the evaluation of NPIs in the context of the COVID-19 pandemic is still largely focused on developed regions such as the United States of America (USA) and the European Union. Nevertheless, it is known that the context of these countries is distinct from those found in less developed countries (LDCs) such as Brazil. It is therefore important to expand the number of rigorous analyses that seek to identify the impact of social distancing measures in LDCs.

A factor that affects the effectiveness of NPIs is the level of civic capital in the country or region, as found by Barrios et al. (2021). Intuitively, the authors show that USA states with higher levels of civic capital have higher levels of social isolation and mask use, even when subjected to similar policies. Another element that appears to alter the impact of NPIs on outcomes such as cases, deaths, and social distancing is the poverty level (Akim and Ayivodji 2020; Bargain and Aminjonov 2020; Wright et al. 2020; Brown and Ravallion 2020). Again, the results are intuitive, given that poor individuals are less likely to have both infrastructure and employment to stay home in isolation. Their results are important to explicit the potential difference in the impact of NPIs between developed and less developed countries, keeping in mind that higher levels of civic capital and lower levels of poverty are positively correlated with development.

In addition to the aforementioned points, which motivate us to evaluate policies in LDCs, other results from the literature that one should keep in mind when evaluating results are cited below. Amuedo-Dorantes, Kaushal, and Muchow (2020) show that the timing of policy adoption is relevant in determining its impacts, i.e., it may be important to compare not only municipalities that adopted and did not adopt lockdown, but also municipalities that adopted early and those that adopted late – the authors suggest a metric to define this concept of policy adoption speed.

The results of Dave et al. (2020) reinforce this idea and show that the impacts of social distancing measures are heterogeneous, suggesting that the earlier the adoption and the more populous the region, the greater its impact. Goolsbee and Syverson (2021), in turn, point to the possibility that policies are not necessarily the most relevant determinants of social distancing, and to some extent of the trajectory of the pandemic. In fact, most behavior of individuals in this context is explained by fear of the pandemic as a whole. Goolsbee and Syverson's strategy for identifying this component is to use the number of deaths from the previous day as an explanatory variable in a regression that looks at a mobility variable. They conclude that too many deaths today increase fear tomorrow, and therefore increase voluntary social distancing tomorrow. These three studies are focused on municipalities or states in the USA and use techniques such as Differences-in-Differences (DiD) with fixed effects.

Furthermore, Fairlie, Couch, and Xu (2020) draw attention to the fact that the impacts of the pandemic and social distancing policies may also depend on skin color. Through a DiD model that interacts the treatment with skin color, looking at data from the USA, the authors show that the gap between the employment level of whites relative to other minorities such as Latinos and Blacks increased during the pandemic. One should be wary, therefore, to explore these heterogeneous effects by skin color when data with this degree of granularity is available.

Regarding the Brazilian case, three works seem to be of special interest to us. Castro et al. (2021) attempt to understand and explain how SARS-CoV-2 spread through Brazil. In short, they find that the combination of i) lack of coordination between municipal and state policies, ii) absence of the federal--level effort to combat the pandemic, and iii) low testing frequency, resulted in an uncontrolled spread of the virus, with no defined pattern, throughout Brazil. Moreover, the authors point to the risk of a second wave even more severe, given the emergence of new VOCs and the slow pace of vaccination in the country, something that unfortunately was confirmed over the first half of 2021.

Ajzenman, Cavalcanti, and Da Mata (2020), in turn, reinforce the idea that the actions of the executive power have not helped in controlling the pandemic, on the contrary. Through an event study type model, the authors show that the speeches and acts of President Jair Bolsonaro with content that disregard the severity of the pandemic resulted in a reduction of social isolation in municipalities in which the politician has majority support.

Lastly, focusing on municipalities in the State of São Paulo, Maia et al. (2021) use an instrumental variable approach (instrumenting isolation with rainfall data) to show that municipalities that had greater social distancing also had decreased cases of COVID-19 and did not suffer more economically, i.e., did not have higher unemployment. This result is remarkable because it contradicts the hypothetical trade-off between health and economics that many use to argue against social distancing policies. Nevertheless, the authors are looking at social distancing behavior instrumented by rain, and not lockdown policies.

Methodologically, we recognize the existence of a new and vast literature on DiD with TWFE and heterogeneous treatment effects, highlighting Goodman-Bacon and Marcus (2020) and Sun and Abraham (2020). These papers point out potential problems in using conventional fixed effects and DiD models in this type of policy evaluation, recommending the use of models in the Event Study (ES) format, and paying attention to which groups are being compared. Goodman-Bacon and Marcus (2020) recommend using unit-specific trends. In our case, as discussed below, we have a panel of treated and control municipalities and compare takers with never-takers. Some studies that adopt this type of approach that will serve as inspiration for our work are Dave et al. (2020), Askitas, Tatsiramos, and Verheyden (2020), and Kong and Prinz (2020).

3. Data

The process of choosing the sample of municipalities used in this work proceeded as follows. We were already interested in evaluating lockdown policies in Brazil, given the political debate around the theme and its importance for public health in that context. During the first semester of 2021, several municipalities in the north and northeast of the State of São Paulo adopted lockdown policies, and this was reported in the media. For economists' eyes, this would be a good opportunity to evaluate such policies, given that not all municipalities adopted them and those that did were close geographically and during the same epidemiological weeks of the pandemic, facilitating the establishment of a reliable control group.

The data on social distancing policies adopted from February to June 2021 in the north and northeast of São Paulo State were obtained through direct consultation of official bulletins of municipal governments and news sources widely regarded as credible – e.g., Folha de S. Paulo, G1, UOL. In the latter case, we always verify the accuracy of the news by reading the specific decree related to the established measure. We have performed a thorough reading of more than 100 municipal decrees, through which it was possible to identify 15 municipalities that adopted policies classified as "lockdown", in the region of interest. We define these policies as those that restricted the circulation of citizens of the municipality and closed most of the commercial establishments, allowing their operation only in the delivery mode.

We apply temporal and spatial restrictions to the policies and municipalities used in the analysis. These restrictions aim to reduce endogeneity in the causal analysis and are discussed below in Section 4.

Some characteristics of the municipalities that adopted lockdown are presented in Table A.1 in the appendix, on the treatment group means column. In summary, these municipalities are concentrated in the North/Northeast region of the State of São Paulo, have an average of approximately 35 cases and 1 COVID-19 death per day, a population of 100,000, economic activities concentrated in services and industry, and an average share of the elderly population of 16%. More details of the policies and the main decrees can be seen in Table 1.

Table 1 - Details of lockdown policies

Municipality	Start	End	Days	Major decrees	
				ID	Date
Altinópolis	25-05-2021	07-06-2021	14	66	22-05-2021
				68	29-05-2021
Araraquara	20-06-2021	27-06-2021	8	12600	17-06-2021
Batatais	15-05-2021	31-05-2021	17	3988	13-05-2021
Bebedouro	20-05-2021	30-05-2021	11	14732	18-05-2021
Brodowski	25-05-2021	06-06-2021	13	4277	26-05-2021
Colômbia	21-05-2021	25-05-2021	5	2027	21-05-2021
Cristais Paulista	28-05-2021	10-06-2021	14	2918	25-05-2021
Franca	27-05-2021	10-06-2021	15	11271	24-05-2021
Itirapuã	27-05-2021	10-06-2021	15	1092	25-05-2021
Jardinópolis	03-06-2021	13-06-2021	11	6424	31-05-2021
Patrocínio Paulista	28-05-2021	10-06-2021	14	3442	26-05-2021
Restinga	27-05-2021	10-06-2021	15	363	25-05-2021
Ribeirão Preto	27-05-2021	02-06-2021	7	118	24-05-2021
				123	31-05-2021
São José da Bela Vista	28-05-2021	10-06-2021	14	1947	25-05-2021
Taiúva	20-05-2021	30-05-2021	11	2803	19-05-2021

Notes: Lockdowns adopted in Araraquara (February/March), Cajuru (April/May), Guará (April), and Ribeirão Preto (March) were excluded from the analysis to allow the policies analyzed to focus on a more similar period of the pandemic, between the months of May and June 2021. The two last columns present information about the most relevant legal decrees related to the policy of interest, identified by a numeric ID and a publication date.

The data on social isolation, cases, and deaths associated with COVID-19 were obtained through publicly available data from the São Paulo State Government.[3] The social isolation index has a daily frequency and is made available by telecommunication service providers (Vivo, Oi, Claro, Tim) through a platform managed by the Brazilian Association of Telecommunication Resources (ABR Telecom). Intuitively, the value of the index should be interpreted as the percentage of inhabitants of the municipality who did not leave their homes on a given day.[4] Unfortunately, the social distance index is not calculated for all municipalities in the State of São Paulo. This limitation should be recognized and taken into considera-

[3] https://www.saopaulo.sp.gov.br/planosp/simi/dados-abertos/. Access in 30/09/2021.

[4] The place that is considered the individual's residence is the place where the mobile phone spent the night. There is a range around the domicile in which the individual can move without considering that she has broken isolation, this prevents inaccurate location signals from being interpreted as breaking social distancing.

tion when discussing the results for this variable. The terms social isolation (or only isolation) and social distancing will be used interchangeably.[5]

The number of cases and deaths associated with COVID-19, on the other hand, are made available by the Secretary of Health of the State of São Paulo. To avoid seasonality problems, we will often use a 7-day moving average of the data described above, which is calculated as a simple arithmetic average of the last 6 days and the current day, always considering 7 periods.

Employment information at the municipal level was calculated using the unidentified microdata from the General Registry for Employed and Unemployed (CAGED) and the Annual Social Information Report (RAIS) made available by the Labor Statistics Dissemination Program (PDET) of the Ministry of Labor. [6] The admissions and dismissals in each municipality were used to calculate the monthly aggregate employment balance.

Other publicly available data used are vaccination, GDP, poverty levels, population, elderly population, and area of the municipality. Vaccine data at the municipal level with daily frequency is calculated with information from the Information System of the National Immunization Program (SI-PNI) referring to the National Vaccination Campaign against COVID-19 of the Ministry of Health.[7] This database contains anonymized data of all vaccines applied in Brazil with various specifications. The municipal code of the vaccinated person's domicile address and the date of vaccine application were used to calculate the number of vaccines applied in each municipality on each day since the beginning of the immunization campaign against COVID-19. The data also allows one to differentiate whether the shot was a first shot, second shot, or a single shot vaccine.

Information about municipal GDP and value-added (VA) by sector is available from the Brazilian Institute of Geography and Statistics (IBGE) and refers to the year 2018.[8] The municipal data used as poverty proxies are from the 2010 Census conducted by IBGE[9] and inform the percentage

5 In Marino, Menezes-Filho and Komatsu (2020), we show that this measure of social distancing is consistent with other indexes available.

6 http://pdet.mte.gov.br/microdados-rais-e-caged. Access in 30/09/2021.

7 https://opendatasus.saude.gov.br/dataset/covid-19-vacinacao/resource/ef3bd0b8-b605-474b-9ae5-c97390c197a8?inner_span=True. Access in 19/07/2021.

8 https://www.ibge.gov.br/estatisticas/economicas/contas-nacionais. Access in 09/06/2021.

9 https://www.ibge.gov.br/estatisticas/multidominio/condicoes-de-vida-desigualdade-e-pobreza. Access in 09/06/2021.

of individuals who live in permanent households and have monthly household income per capita below certain income ranges such as ¼ or ½ of the minimum wage. The population and elderly population data are made available by the São Paulo State Government along with the data on cases and deaths and are calculated by the State System of Data Analysis Foundation (SEADE-SP).

4. Empirical Methods

To identify the causal effect of a social distancing policy on epidemiological and socioeconomic outcomes, one cannot just compare the pre- and post-values in each municipality that adopted the policy, because it is not known whether such a difference would have occurred in the absence of the policy adoption. Nor is it sufficient to compare lockdown municipalities with all municipalities that did not adopt it, since there may be intrinsic differences among these municipalities that explain any change in the dynamics of outcomes before and after the policies. For example, the city of São Paulo, the state capital with over 10 million inhabitants, is clearly not a good control for the municipality of Altinópolis, a municipality with just over 15 thousand inhabitants. It would be hard to believe that what happens in São Paulo after the adoption of lockdown in Altinópolis represents what would have happened in Altinópolis had the mayor not adopted the policy.

In practice, we will never know what would have happened in the treated municipalities if they had not adopted the policy, since we observe the realization of only one state of the world at each point in time; this is the fundamental problem of causal inference. Nevertheless, it is possible to construct a control group that convincingly represents the counterfactual for the treatment group, allowing us to identify the causal effect of the social distance policy by comparing these groups. In other words, we want to find a set of municipalities that did not adopt lockdown and have similar characteristics to the ones that adopted lockdown, convincing us that any differences between the outcomes of interest after the policy adoption are caused by the lockdown itself.

The strategy adopted here for the empirical analysis consists of temporal and spatial restrictions in the sample, followed by a Difference-in-Differences model with an event study design, to align the different timing of policy adoption across municipalities. In addition, two robustness tests are performed, namely, the use of propensity score matching (PSM) to select a control group – even more similar to the treated one – and the application of a placebo treatment.

4.1. *Control group*

The main steps taken to find a control group have already been mentioned above. First, the policies to be evaluated were restricted to the period between May and June of 2021 to guarantee that the municipalities are all in a similar moment of the COVID-19 pandemic. Second, the sample was restricted to municipalities on the Regional Health Departments (DRSs) of Araraquara, Barretos, Franca, and Ribeirão Preto to ensure that they are similar in unobservable characteristics such as habits, culture, and historical institutions in general, as they are neighbors. This second step reduces the number of municipalities in the analysis: only 88 of the 645 municipalities in the State of São Paulo are in the mentioned DRSs, and, from these, only 15 adopted lockdowns in the restricted sample period.

In the main analysis, we use all municipalities that did not adopt lockdown in these DRSs as the control group (Figure 1). As a robustness check, we present results for a control group that is constructed by applying Propensity Score Matching (PSM) on this sample, resulting in a subset of 18 control municipalities, which are even more similar to the treated ones. The results are similar in both approaches.

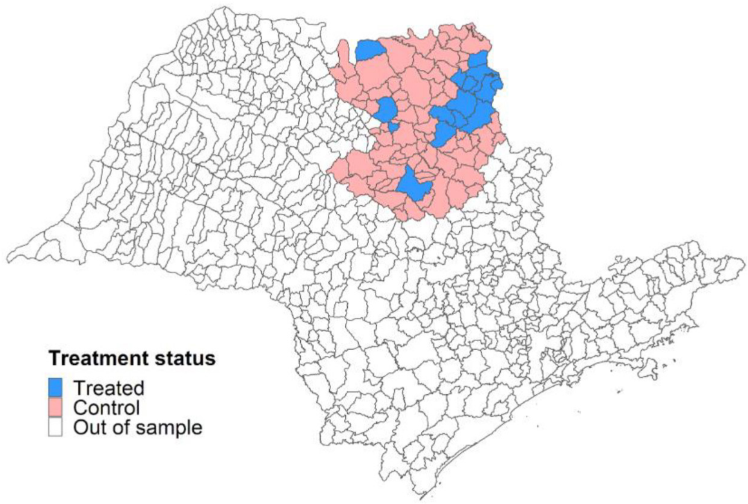


Figure 1 - Municipalities by treatment status

Notes: The treatment group is composed by the following municipalities: Altinópolis, Araraquara, Batatais, Bebedouro, Brodowski, Colômbia, Cristais Paulista, Franca, Itirapuã, Jardinópolis, Patrocínio Paulista, Restinga, Ribeirão Preto, São José da Bela Vista, Taiúva. The control group is composed by all the other municipalities in the Regional Health Departments of Araraquara, Barretos, Franca, or Ribeirão Preto.

Table A.1 presents the mean tests for different characteristics of the treatment and control municipalities, for the unmatched (main) and matched (with PSM) approaches. The balance test results show that for most variables the treatment and control groups are notably similar, with no statistically significant differences in the means. For the variables with significant differences, such as population, and some value-added measures, the magnitude of the differences is small and should not compromise the analysis. With the PSM, all the differences are not significant, and the results are similar to what we find in the unmatched sample.

Our preferred specification is the one without PSM, since it makes the analysis more straightforward, and gives us more statistical power. We believe that, given the sample restrictions, any difference in the outcomes between the treated and control groups after the policy adoption is caused by the lockdown policy.

4.2. Event Study

Given the intuition discussed above, let us formally present the identification strategy adopted to estimate the effects of the lockdown policies on the outcomes of interest (Y), namely, social isolation, cases, deaths, and employment. To this end, we use a DiD framework, which compares the difference in the average outcome before and after the intervention for the treatment groups (T) and the control groups (C) (Equation 1), for our primary estimation procedure.

$$\hat{\beta}_{DiD} = \left(\overline{Y}^{T}_{post} - \overline{Y}^{T}_{pre}\right)\left(\overline{Y}^{C}_{post} - \overline{Y}^{C}_{pre}\right) \tag{1}$$

The idea here, as suggested before, is that the difference in the outcome of the control group before and after the policy represents what would have happened for the municipalities in the treatment group if they had not adopted the policy. The difference between the observed change in the treatment group and in the control group, thus, should give us the impact of the lockdown over Y. More formally, writing Equation 1 in terms of conditional expectations and adding zero by summing and subtracting the expected outcome for the treated if they had not adopted the policy, $E[Y^{T}_{x=0}|post]$, one can find Equation 2, where x is an indicator variable that identifies whether the municipality received or did not receive the treatment – have adopted or have not adopted lockdown.

$$\begin{aligned}\hat{\beta}_{DiD} = \left(\mathbb{E}[\mathrm{Y}^{\mathrm{T}}_{\mathrm{x=1}}|post] - \mathbb{E}[\mathrm{Y}^{\mathrm{T}}_{\mathrm{x=0}}|pre]\right) - \left(\mathbb{E}[\mathrm{Y}^{C}_{\mathrm{x=0}}|post] - \mathbb{E}[\mathrm{Y}^{C}_{\mathrm{x=0}}|pre]\right) \\ +\left(\mathbb{E}[\mathrm{Y}^{\mathrm{T}}_{\mathrm{x=0}}|post] - \mathbb{E}[\mathrm{Y}^{\mathrm{T}}_{\mathrm{x=0}}|post]\right)\end{aligned} \tag{2}$$

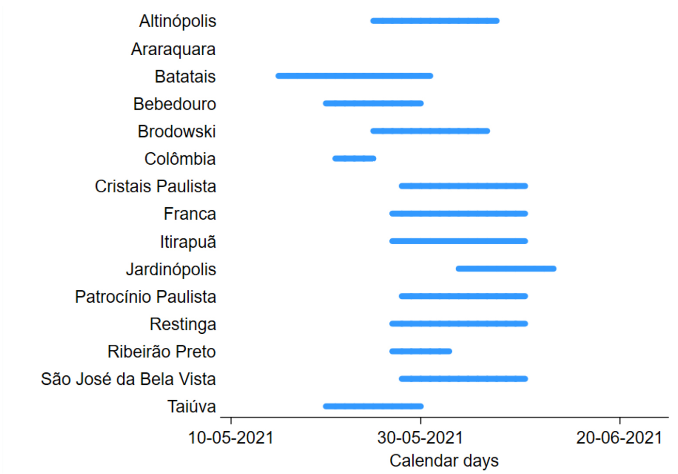
Rearranging Equation 2, one can find Equation 3, where the structure of the DiD estimator is clear. The first line displays exactly what we were looking for, that is, the difference in the outcome of the treated municipalities Y^{T} if they were treated ($x = 1$) and if they were not treated ($x = 0$) both *after* the policy adoption, in expectation terms. This is the so-called Average Treatment Effect (ATT). The second line represents the difference in the variation of the variable Y before and after the policy between the treatment and the control groups when both do not receive the treatment, i.e., when none of them adopted the policy (Cunningham 2021).

$$\begin{aligned}\hat{\beta}_{DiD} = &\left(\mathbb{E}[\mathrm{Y}^{\mathrm{T}}_{\mathrm{x=1}}|post] - \mathbb{E}[\mathrm{Y}^{\mathrm{T}}_{\mathrm{x=0}}|post]\right) \\ &+\left(\mathbb{E}[\mathrm{Y}^{\mathrm{T}}_{\mathrm{x=0}}|post] - \mathbb{E}[\mathrm{Y}^{\mathrm{T}}_{\mathrm{x=0}}|pre]\right) - \left(\mathbb{E}[\mathrm{Y}^{C}_{\mathrm{x=0}}|post] - \mathbb{E}[\mathrm{Y}^{C}_{\mathrm{x=0}}|pre]\right)\end{aligned} \tag{3}$$

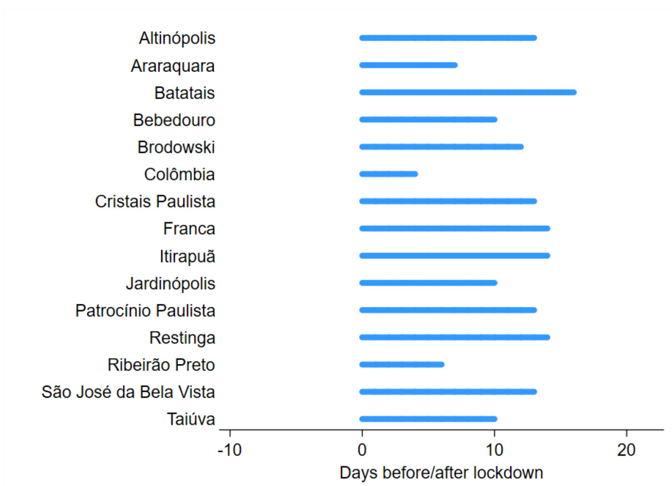
As argued above, by constructing a control group with the methods discussed, we hope to convince the reader that this difference (second line of Equation 3) equals zero and that, consequently, the DiD estimator equals the ATT. This is the main assumption behind the DiD design: the "parallel trends" assumption. In words, we assume that the *trajectory* or *variation* (not to be confounded with the level) of the outcome would have been the same between the two groups if the treated municipalities had not adopted the policy. The best one can do to evaluate the validity of the parallel trends assumption is to check whether the trajectories of Y are similar between the groups before the intervention; this will be discussed below. One premise that supported our decision is that, even of short duration, a lockdown can affect case developments by elevating social distancing. One drawback of this approach is that it does not inform us about the optimal duration of such policies, a question that we do not claim to answer, although we recognize its value.

The DiD design explained above is intuitive, especially when there is a unique adoption date for the intervention, a scenario in which the pre- and post-policy periods are easily defined. Nevertheless, this is not the most common case, since if one wants to analyze different units that have adopted a policy, it is likely that they have done so at different points in time. The careful reader should have realized that this is the case here: the municipal lockdown policies to be studied were adopted at different moments in time (see Table 1). In other words, there is a staggered adoption of the policy.

This is where the event study framework becomes handy. The idea here is to run a DiD analysis but look at the time relative to the policy rather than the calendar time when thinking about the pre- and post-policy periods, imposing a temporal alignment between the treated units regarding the intervention. For example, if A adopted lockdown on May 20, 2021, and B did so on May 25, the pre- and post-policy periods are not aligned when we look at the calendar. However, if we think about policy-related days, we have that day 0 will be May 20 for A and May 25 for B; day 1, in turn, will be May 21 for A and May 26 for B, and so on. Figure 2 illustrates the temporal alignment that the event study design enables, by showing the lockdown policy duration for each municipality and the differences when we look for calendar days and policy-related days.



(a) Calendar time



(b) Time relative to policy

Figure 2 - Event study illustration for lockdown policies

It is worth mentioning that, although the policies have different durations, we chose not to explore this feature and simply classify municipalities between treated and non-treated, in a binary way, independent of the lockdown duration. In part, this decision was made to simplify the analysis and interpretation of the results. Moreover, we believe that this may only lead us to underestimate the effects of lockdown on the outcomes of interest, if we assume that there is a monotonic positive relationship between impact and policy duration, giving us greater confidence in the effects we find. One drawback of this approach, however, is that it does not inform us about the optimal duration of such policies, a question that we do not claim to answer, although we recognize its value.

Finally, let us formally present our main specification. Equation 4 refers to the estimation of the daily impact of lockdown on the variables of social isolation, COVID-19 cases, and COVID-19 deaths. Equation 5 refers to the estimation of the monthly impact of lockdown on employment. All variables are analyzed at the municipal level, the variables of cases, deaths, and employment are considered relative to 100,000 inhabitants, and the variables of social isolation, cases, and deaths are used as 7-day moving averages to avoid seasonality. All linear regressions were estimated with weighted least squares, where the weight is the municipalities' population.

$$Y_{i,t} = \sum_{\substack{d=-14 \\ d\neq -1}}^{50} \beta_d \times 1_d(d = t - t_i^*) + \delta_t + \gamma_i + t \times \gamma_i + \varepsilon_t \tag{4}$$

$$E_{i,t} = \sum_{\substack{m=-3 \\ m\neq -1}}^{3} \beta_m \times 1_m(m = t - t_i^*) + \delta_t + \gamma_i + t \times \gamma_i + \varepsilon_t \tag{5}$$

In Equation 4, the subscripts i and t index each municipality and date of the year, respectively. Further on, Y is the dependent variable (isolation, cases, or deaths); t_i^* is the date of lockdown adoption in municipality i; $1_d(d = t - t_i^*)$ are 64 indicator variables that are triggered on day d for treated municipalities; δ_t are date of the year fixed effects; γ_i are municipal fixed effects; $t \times \gamma_i$ are municipal-specific trends; and ε_t is a robust error term.

The regression samples are defined as follows. The sample period starts on May 1, 2021, 14 days before the first municipality adopted lockdown (Batatais on May 15); and ends on August 9, 2021, 50 days after the last municipality adopted the policy (Araraquara on June 20). Therefore, we have 101 calendar days in the sample. For treated municipalities, we use only observations from 14 days before to 50 days after policy adoption (65 observations)[10], but for controls, we use all observations in the sam-

[10] For example, if treated municipalities A and B adopted lockdown on dates *x* and *y*, the sample period for A is in [*x*-14,*x*+50], and for B, in [*y*-14,*y*+50].

ple period (101 observations). Including a larger number of observations from untreated units means that we do not place weight on comparisons between treated units that adopted the policy at different times, mitigating econometric complications raised by Goodman-Bacon and Marcus (2020) and Sun and Abraham (2020) related to variation in treatment adoption time. The time fixed effects and the municipal-specific trends comprehend the whole sample period.

For the case of the regressions by weeks, we use only one observation before policy adoption for the treated units to have the same baseline for treatment effects as we have in the regressions by day (event period -1), giving us 52 observations for the treated municipalities.

The use of unit and time fixed effects is known as two-way fixed effects (TWFE) and it captures any aggregate variation by day of the year – such as WHO announcements and presidential speeches – or municipal-specific characteristics that are constant over the sample period – such as population, GDP, and the number of hospitals. The municipal-specific trends control for any trend in the municipalities before or after the policy adoption. This type of analysis is more robust than a standard DiD model and the parallel trends before the intervention give us confidence in the estimated results (Dave et al. 2020; Sun and Abraham 2020; Goodman-Bacon and Marcus 2020).[11]

Given the geographic constraint of the municipalities being in the same DRSs, the time constraint of the policies being adopted at a similar time of the pandemic, the TWFE, and the municipal-specific trends, we assume that the β_d coefficients identify the daily causal impact of the lockdown policy on the dependent variable Y. The interpretation of these coefficients can be performed as explained below.

The $1_d(d = t - t_i^*)$ variables equal 1 on day d and 0 on the other days for the treated municipalities, for the control they always equal 0.[12] The dummy variable for the day immediately before the policy $(d = -1)$ is

[11] Using the terminology of the recent literature on DiD with staggered adoption and heterogeneous treatment effects, we are comparing adopters with never-adopters. We defined the treated group with an indicator variable that always equals one and the event-time is only defined in the range of interest. In this way, we are not comparing early- with late-adopters, which could cause problems.

[12] An example: if a municipality adopted lockdown on May 20, we have $t_A^* =$ 20-05-2021 and d will be defined for every t as the day relative to the policy, i.e., in the day before the policy, we have $d = t - t_A^* =$ 19-05-2021 - 20-05-2021 =-1. This logic is analogous to the other days of the year.

omitted to serve as a baseline against the other daily dummies since one period must be omitted to avoid linear colinearity. Therefore, each β_d represents the difference on day d in outcome Y between the treated and control municipalities relative to this difference in the day before the policy adoption, which is the baseline period.[13]

Intuitively, β_d with $d \in \{z \in Z; -14 \leq z \leq -2\}$ allows us to check for parallel trends, given that it represents the difference in the trajectory between the treatment and control groups *before* the policy, which should be zero. On the other hand, β_d with $d \in \{z \in Z; 0 \leq z \leq 50\}$ gives us the daily impact of the lockdown policy from the adoption day $(d = 0)$ until 7 weeks, plus 1 day, after $(d = 50)$.

There is an asymmetry between the periods considered before and after the adoption of the policy because they have different purposes. While the pre--adoption coefficients are meant to inform us about the existence of parallel trends, the post-adoption coefficients are meant to identify treatment effects. We add two weeks before adoption to evaluate pre-trends because this is a common interval in the literature. On the other hand, we add 50 days after adoption because it is known that there can be a time lag between policy adoption and some identifiable variation in deaths, given the incubation period of the virus and other epidemiological factors. With these lags, we have a balanced panel, where all the units have observations for all event periods.

We present results with and without fixed effects and municipal-specific trends. The interpretation of Equation 5 is analogous, with the only difference being that the frequency of the employment variable is monthly, as is the definition of all other time-related variables.

It is important to recognize what the main threat to our identification strategy is and explain why we think it is not happening. If there are unobservable factors that vary over time and municipalities – therefore not captured by fixed effects –, that are correlated with lockdown adoption, and that significantly affect the outcomes of interest, we could be looking at the impact of these unknown factors and crediting it to the lockdown. An example would be if the federal government started giving financial aid to treated municipalities, because they were in a critical situation, at a time close to the adoption

[13] We do not report the constant term of these regressions because the use of multiple fixed effects does not allow for an intuitive interpretation of the constant.

of lockdown. In this case, we would not know how to separate the effects of the lockdown and the effects of the federal transfer.

Based on our institutional knowledge, we believe that these confounders are not present. First, it is unlikely that there would be factors exclusively affecting municipalities in the treatment group. As shown in the results, these municipalities are very similar before the policies, in terms of COVID-19 cases and deaths and other characteristics. We believe that what determines the adoption of the policy in some municipalities and others not is the positioning of the mayors and their technical teams, and not something intrinsic and unique to the treated municipalities. Second, as will be discussed below, the way the results present themselves over time is very convincing and these are robust to a different control group.

5. Results and Discussion

Here, we present and discuss the estimation results from the event study model outlined above. As is the convention in the literature, the main results are presented in figures with the daily ATT point estimates and their 95% confidence interval. We also present the results in tables, aggregating the effects by week.

5.1. Results

The results of the analysis to identify the daily impact of the lockdown policy on social isolation are presented in Figure 4. Before analyzing these results, it is important to highlight that for this outcome, our sample is restricted due to the lack of social distancing measures for most of the municipalities. Therefore, for the estimates below, we use only 16 municipalities, 5 treated, and 11 control[14]. The municipalities where this measure is available are not randomly selected, they have a higher average population, so the results must be interpreted with caution. Nevertheless, we

[14] The treated municipalities are: Araraquara, Batatais, Bebedouro, Franca, Ribeirão Preto. The control municipalities are: Barretos, Ibitinga, Jaboticabal, Matão, Monte Alto, Olímpia, Porto Ferreira, Sertãozinho, São Carlos, São Joaquim da Barra, and Taquaritinga.

think they provide valuable insights for the interpretation of the impacts of the policy in general.

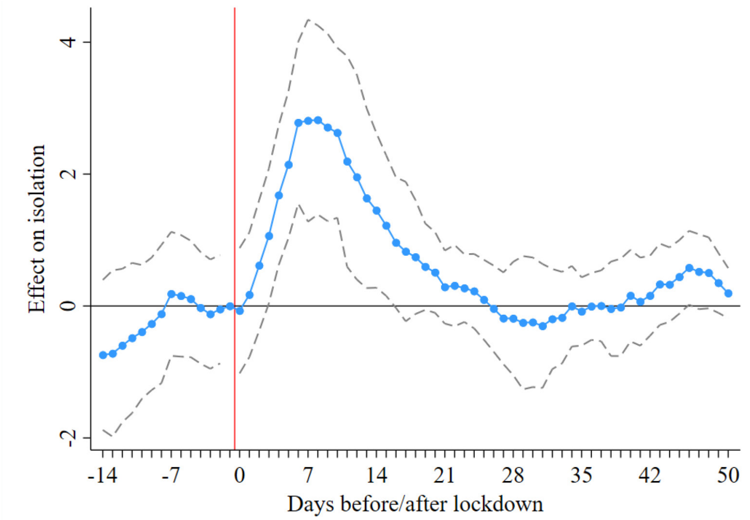


Figure 4 - Lockdown effect on social isolation

Notes: The regression used to elaborate this graph contains one indicator variable for each day from 2 weeks before the lockdown adoption until 7 weeks later and was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as a 7-day moving average. The regression controls for municipality and day fixed effects and municipal-specific trends. Robust standard errors were used to construct the 95% confidence interval represented by the dashed lines.

Two main points must be highlighted in Figure 4. First, the impact of the policy in the 7 days before its adoption is null. As discussed in Section 4, the non-significance of these coefficients before the policy adoption gives us confidence that the parallel trends assumption is being respected. Second, observing the trajectory of the impact after the lockdown adoption, one can see that social isolation significantly increased in the treatment group from days 4 to 11 after the policy, reaching its peak one week after the lockdown.

It is worth highlighting that the plotted coefficients give us the difference between the treatment and control groups relative to the day before the policy. Therefore, Figure 4 shows us that 7 days after the policy adoption social distancing increased by approximately 2.5 percentage points in the treatment group compared to the control one relative to the day before the lockdown[15]. This effect, however, does not persist from day 14 onward, being statistically not different from 0 at the 5% level. The short-term feature of this effect is aligned with the length of the policies, which ranges from 5 to 17 days in the treatment group, as shown in Table 1.

Table 2 - Lockdown effect on social isolation, by week

Independent variables	Dependent variable: Social isolation		
	(1)	(2)	(3)
Week 1	-1.978***	1.699***	1.592**
	(0.476)	(0.421)	(0.568)
Week 2	-0.637	2.380***	2.096**
	(0.465)	(0.446)	(0.655)
Week 3	-3.651***	0.880*	0.417
	(0.502)	(0.392)	(0.779)
Week 4	-4.191***	0.223	-0.409
	(0.535)	(0.381)	(0.916)
Week 5	-4.205***	-0.218	-1.022
	(0.519)	(0.408)	(1.159)
Week 6	-4.458***	0.069	-0.927
	(0.516)	(0.389)	(1.315)
Week 7	-4.391***	0.317	-0.947
	(0.475)	(0.382)	(1.514)
Municipal FE	No	Yes	Yes
Date FE	No	Yes	Yes
Municipal-specific trends	No	No	Yes
R-squared	0.116	0.979	0.982
Observations	1371	1371	1371

Notes: Robust standard errors are in parenthesis. The regression was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as a 7-day moving average.

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

Table 2 confirms that the effects of the policy on social distancing were positive in the treatment group, as the coefficients for weeks 1 to 3 are positive in the most reliable specifications. In column (3), the impact in the first week after the lockdown is significant at the 10% level. It is easy to see why the 5% significance does not hold when we aggregate the

[15] This interpretation follows from the fact that the social distance variable is measured as percentage points, going from 0 to 100.

results by week: Figure 4 shows us that the impact of the policy in the days immediately after the policy adoption (days 0 to 3) were not statistically different from 0 at the 5% level, reducing the average effect when we aggregate it by week. Lastly, one should note that the coefficients highlighted here have a similar magnitude when we go from column (2) to (3), the main difference is that the standard errors increase in the specification with municipal-specific trends.

The results for COVID-19 cases are presented in Figure 5, and the interpretation of the results is analogous to the one developed above for social distancing. In this case, we find three marginally significant coefficients at the 5% level in the pre-policy period. We believe that this is not a strong violation of the parallel trends assumption because: (i) the magnitude of these coefficients is small, (ii) there are no clear trends, and (iii) the results hold with the PSM specification in the appendix, where there are no pre-trends.

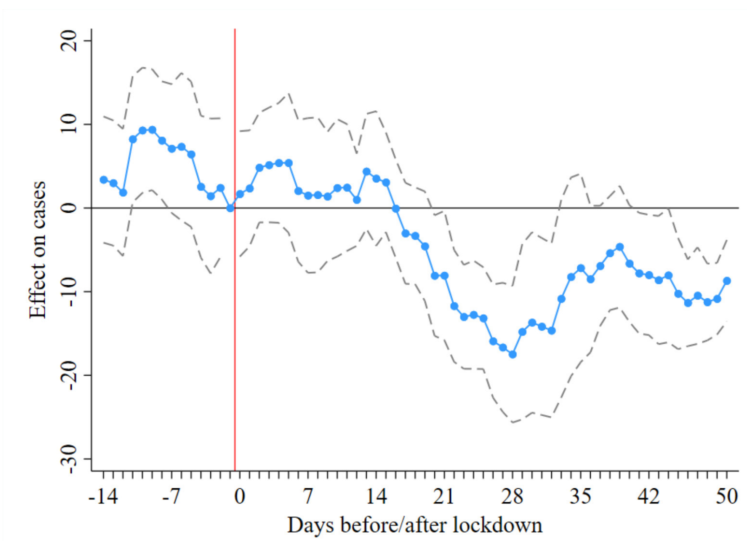


Figure 5 - Lockdown effect on cases

Notes: The regression used to elaborate this graph contains one indicator variable for each day from 2 weeks before the lockdown adoption until 7 weeks later and was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as a 7-day moving average per 100,000 inhabitants. The regression controls for municipality and day fixed effects and municipal-specific trends. Robust standard errors were used to construct the 95% confidence interval represented by the dashed lines.

Looking for the daily impact of the lockdown on the 7-day moving average of COVID-19 cases per 100,000 inhabitants in the treatment group, it is clear that there is a drop in the cases trajectory starting two weeks after the policy adoption. From 15 days onward after the lockdown, all the coefficients are negative and from days 22 to 33, and 42 to 50, they are also statistically different from 0 at the 5% level. Four weeks after the lockdown, on day 28, for example, there were approximately 17 fewer COVID-19 cases per 100,000 inhabitants in the treatment group compared to the control one relative to the day before the policy adoption. To have a benchmark for these effects, one can think about the averages in the month before the policy adoption of COVID-19 cases, presented in Table A.1. The average number of cases in the month prior to the policy in the treatment and control groups was approximately 35 and 38, respectively.

Table 3 - Lockdown effect on cases per 100k inhabitants, by week

Independent variables	Dependent variable: Cases		
	(1)	(2)	(3)
Week 1	-1.035	1.283	1.188
	(2.633)	(6.140)	(3.210)
Week 2	-0.483	1.678	1.330
	(2.322)	(6.128)	(4.121)
Week 3	-0.610	-2.628	-3.497
	(1.718)	(6.078)	(5.166)
Week 4	-4.581***	-11.177	-12.641
	(1.245)	(6.002)	(6.594)
Week 5	-4.445*	-6.934	-8.790
	(1.823)	(6.239)	(7.942)
Week 6	-7.465***	-0.075	-2.491
	(0.994)	(6.098)	(9.457)
Week 7	-12.429***	-0.513	-3.644
	(1.055)	(6.026)	(10.804)
Municipal FE	No	Yes	Yes
Date FE	No	Yes	Yes
Municipal-specific trends	No	No	Yes
R-squared	0.012	0.448	0.602
Observations	8153	8153	8153

Notes: Robust standard errors are in parenthesis. The regression was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as a 7-day moving average per 100,000 inhabitants.

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

These results are quite intuitive when combined with those found for social isolation. If social isolation rose one week after the lockdown was adopted, it is reasonable that cases would begin to fall two weeks after the policy, since there must be a time lag between the increase in isolation and the decrease in cases when we take into account the days to present symptoms and to report positive tests. One should also note that the cases in the treatment group remain at a lower level compared to the control one even after the policy is over. The weekly impact of the policy is presented in Table 3. Column (3) confirms the results obtained before by showing negative coefficients from week 3 onward after the policy. Nevertheless, these coefficients are not significantly different from zero at conventional levels, this may happen because the aggregation of the impact by week might hide the heterogeneous impact of the policy for each day in the 7-day intervals. Again, the coefficients have similar magnitudes when comparing columns (2) and (3).

The daily impact of the lockdown policy on the 7-day moving average of COVID-19-related deaths per 100,000 inhabitants is presented in Figure 6. One more time, the parallel trends seem to be respected, given that the coefficients for the days before the policy are statistically not different from zero at the 5% level. One month after the lockdown, in turn, the deaths begin to drop in the treatment group and this difference is significantly different from zero at 5% between days 37 and 44, that is, in the sixth to seventh week after the policy adoption. On the 41st day after the lockdown, the 7-day moving average of deaths per 100,000 inhabitants in the treatment group was approximately 0.5 lower than in the control group relative to the day before the policy adoption. The average deaths in the month before the policy for treatment and control were roughly 0.9 and 1.4, respectively (see Table A.1). This effect, however, is modest in magnitude and does not seem to be maintained over time, since from day 45 the difference seems to be zero again.

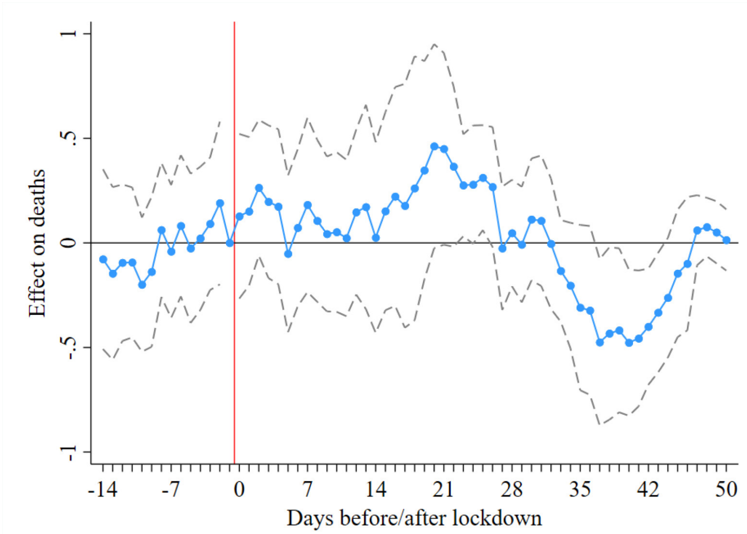


Figure 6 - Lockdown effect on deaths

Notes: The regression used to elaborate this graph contains one indicator variable for each day from 2 weeks before the lockdown adoption until 7 weeks later and was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as a 7-day moving average per 100,000 inhabitants. The regression controls for municipality and day fixed effects and municipal-specific trends. Robust standard errors were used to construct the 95% confidence interval represented by the dashed lines.

Table 4, in the specification with all the controls, column (3), confirms the results discussed above for the death outcome. The coefficients for weeks 5, 6, and 7 are all negatives. These are not significant at the conventional levels probably because the effects are modest, and the days of greater impact are divided between weeks 6 and 7 which also contain days with no significant impact, as shown in Figure 6. Again, these results are notably intuitive if combined with the ones previously presented. The lockdown increased the social distancing 1 week after its adoption, decreased the cases 2 weeks after, especially on week 4, and the deaths started to drop after week 4, reaching its lowest point in the treatment group on weeks 6 and 7, within 14 days after the minimum number of daily cases. The time intervals between the impacts of the variables of interest add up when we consider the medical literature, and this will be discussed further below.

Table 4 - Lockdown effect on deaths per 100k inhabitants, by week

Independent variables	Dependent variable: Deaths		
	(1)	(2)	(3)
Week 1	0.541***	0.035	0.027
	(0.099)	(0.150)	(0.169)
Week 2	0.277**	0.006	-0.015
	(0.089)	(0.158)	(0.216)
Week 3	0.660***	0.189	0.150
	(0.156)	(0.173)	(0.288)
Week 4	0.389***	0.145	0.058
	(0.092)	(0.148)	(0.350)
Week 5	-0.004	-0.131	-0.221
	(0.077)	(0.153)	(0.424)
Week 6	-0.431***	-0.511**	-0.597
	(0.044)	(0.160)	(0.508)
Week 7	-0.427***	-0.174	-0.253
	(0.033)	(0.153)	(0.595)
Municipal FE	No	Yes	Yes
Date FE	No	Yes	Yes
Municipal-specific trends	No	No	Yes
R-squared	0.041	0.325	0.409
Observations	8153	8153	8153

Notes: Robust standard errors are in parenthesis. The regression was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as a 7-day moving average per 100,000 inhabitants.

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

Finally, the monthly impact of the lockdown policy on employment per 100,000 inhabitants is presented in Figure 7. One should be aware that the estimates for this variable contain a considerably smaller number of observations, given that its frequency is lower. This is clear when we see the wide confidence intervals. The coefficients for the months -3 and -2 before the policy are close to and statistically not different from 0 at the 5% level. Although these are only two months, it gives us some confidence in the existence of parallel trends. After the policy, in turn, the coefficients are positive, but not significant at the conventional levels. In other words, these results suggest that the lockdown did not significantly affect employment in the treated municipalities compared to the control ones relative to the month before the policy. Table 5 confirms these results by presenting positive coefficients for almost all the months after the policy and no significance for all of them at the 5% level.

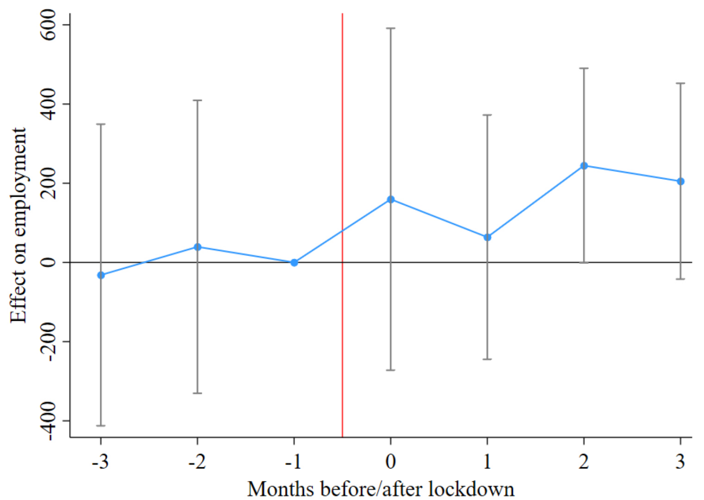


Figure 7 - Lockdown effect on employment

Notes: The regression used to elaborate this graph contains one indicator variable for each month from 1 month before the lockdown adoption until 4 months later and was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as per 100,000 inhabitants. The regression controls for municipality and day fixed effects. Robust standard errors were used to construct the 95% confidence interval represented by the vertical capped lines.

Table 5 - Lockdown effect on employment per 100k inhabitants, by month

Independent variables	Dependent variable: Employment		
	(1)	(2)	(3)
Month 1	221.886	157.871	118.273
	(195.116)	(222.475)	(266.938)
Month 2	109.582	62.031	-3.543
	(113.177)	(154.563)	(260.990)
Month 3	221.821*	242.705*	150.03
	(96.911)	(120.960)	(330.724)
Month 4	162.166*	202.642	45.214
	(64.950)	(122.937)	(453.238)
Municipal FE	No	Yes	Yes
Date FE	No	Yes	Yes
Municipal-specific trends	No	No	Yes
R-squared	0.014	0.123	0.317
Observations	614	614	614

Notes: Robust standard errors are in parenthesis. The regression was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as per 100,000 inhabitants. The sample consists of 7 observations for each municipality, one per month, from 3 months before to 3 after the policy. Two municipalities (Araraquara and Jardinópolis) do not have observations for the last month because our data stop in August.

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

5.2. Discussion

The results presented above point to the epidemiological effectiveness of lockdown policies, which decrease cases and deaths when accounting for the appropriate time lag. We find no clear economic cost of the policies, in terms of increased unemployment in the municipalities that adopted them. Our results undermine the supposed trade-off between economics and health often raised in the debate about methods of fighting the pandemic in Brazil. This is in line with the results obtained by Maia et al. (2021) and shows how social isolation policies could have been used more widely in the country to combat the COVID-19 pandemic, despite the resistance of President Jair Bolsonaro (Ajzenman, Cavalcanti, and Da Mata 2020; Castro et al. 2021).

An important caveat is that we are only looking at one economic variable and, as said before, these estimates are suggestive, given the lower frequency of the data. For example, even without having increased layoffs, the policies may have had an economic impact on workers and or employers through a reduction in their income (Arndt et al. 2020).

Additionally, our results add to the vast literature that points to the positive epidemiological impact of lockdown policies, such as Dave et al. (2020), despite the expectation of a lower or null impact of this type of policy in less developed countries due to poverty (Akim and Ayivodji 2020; Bargain and Aminjonov 2020; Wright et al. 2020; Brown and Ravallion 2020) and lower levels of civic capital (Barrios et al. 2021).

One way to assess whether the results found here are reliable, and not a work of chance, is to think about whether they fit the timeline established in the medical literature about the development of the COVID-19 virus. Assuming that the lockdown policy affects epidemiological outcomes through social isolation, it would not make sense if cases had dropped before social isolation increased. It would make even less sense if deaths had dropped before a decrease in cases a few days earlier, if that was true the explanation would likely be related to a new treatment method rather than a social distancing policy.

However, the results presented above are in line with what is known about the progression of COVID-19 in the human body. The medical literature points to the fact that, on average, the progression of the COVID-19 virus occurs as follows: symptoms take about 5 days from the date of infection to manifest themselves, and deaths usually occur within 18 to 28 days, that is, between 2.5 and 4 weeks after infection (Wang et al. 2020; Guan et al. 2020; Huang et al. 2020; Zhou et al. 2020; Yang et al. 2020).

Recalling the timing of the results presented here, we find that social isolation increased significantly in the week after the policy was adopted. This makes sense if we consider that there are a few days of adjustment to the policy, in which citizens realize that there is indeed enforcement and the measures must be respected. The cases, in turn, began to fall from 15 days after the adoption of the policy, in other words, one week after the largest increase in social isolation. Considering what was stated above, that the average delay for the manifestation of the symptoms is 5 days, it is intuitive to think that the reporting of cases fell after a week of the increase in social isolation, and not immediately after. Between this increase in isolation and the fall in cases, some cases that were contracted before were still being reported with this 5-day lag, this is the time it took for the patients to have the symptoms. The largest drop in cases happened 4 weeks after the adoption of the policy. This persistence of the reduction is also intuitive because, besides the decrease in social interactions, the lower number of cases implies a slowdown of the transmission rate in the municipality.

Finally, the results for deaths pointed to the onset of the decline one month after the lockdown began and with the greatest impact, in absolute terms, occurring 40 days after the policy. In other words, the onset of the decline in deaths occurred within 3 to 4 weeks after the onset of the increase in social distancing, and the greatest decline occurred within 2 weeks after the greatest decrease in cases. Again, this is in agreement with the aforementioned medical literature, which points to an average time between 2.5 and 4 weeks between infection and death.

5.3. Robustness

We present two tests to assess the robustness of our findings. In the first one, we repeat the analysis above, but using a different control group, built with a PSM procedure.

The variables used to estimate the PSM were the following: daily cases and deaths of COVID-19, monthly employment balance, daily vaccinations, population, elderly population, VA by sector (agriculture, industry, services, and administration), GDP per capita, and share of the population in households with monthly per capita income below ¼ of the minimum wage.[16] This last variable is intended to serve as a proxy for poverty in the municipalities, the variables for cases, deaths, employment, and vaccinations are per 100,000 inhabitants and were used as an average for the one month before the policy, from April 14 to May 14, 2021; given that the first municipality in the sample to adopt lockdown did so on May 15, 2021. The control group was constructed using the nearest neighbors for each treated unit, with replacement – that is, a municipality can be a control for more than one treated one.

The PSM improves the balance between the treatment and control group, as shown in Table A.1. Moreover, the main results are presented in Figure 8 and as expected they are roughly the same as the ones from the main analysis. More details on this control group and the results in table format are presented in Appendix A.

On a second robustness test, we estimated the placebo effect of a non-existent lockdown policy for the same treatment and control groups used in the main analysis. The idea here is to estimate the same models used to construct the figures discussed above, but now as if the lockdown policy in the treated municipalities had been adopted 1 year earlier. Intuitively, the results found should not persist in a scenario in which there was no policy. That is, this test suggests the robustness of the results when the placebo effect of the policy at a hypothetical adoption date is not consistent with the main analysis. This is exactly what is found in the test, as one can see in more detail in Appendix B. Consequently, this test also suggests the robustness of the results here obtained.

Finally, one could think about potential spillovers of lockdowns. As in, the trajectory of cases in one municipality being affected by the neighboring municipality, due to commuting and different transmission rates. If this scenario is true, the results presented here are a lower bound for lockdown policy effects, as reducing cases in the treated units would also be reducing cases in the controls, narrowing the gap between them.

[16] The social isolation variable was not used in the PSM because data is not available for all municipalities.

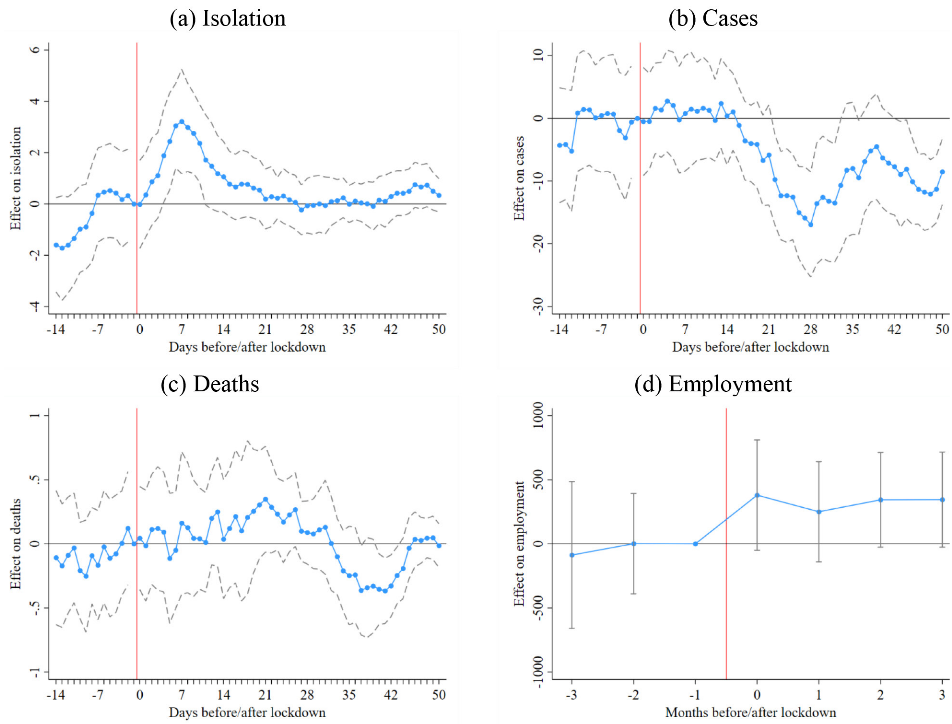


Figure 8 - Results with PSM

Notes: The regressions used to elaborate these graphs contain one indicator variable for each period before and after the lockdown adoption and were estimated with weighted least squares, where the weight is the municipality's population. From (a) to (c), the dependent variable is calculated as a 7-day moving average. In (b) to (d), the dependent variable is calculated as per 100,000 inhabitants. The regressions control for municipality and time fixed effects. Robust standard errors were used to construct the 95% confidence interval represented by the vertical capped lines. The control group is selected using propensity score matching (PSM).

6. Conclusion

This paper studies the effectiveness of non-pharmacological interventions (NPIs) of social distancing adopted to fight the COVID-19 pandemic in Brazil. Temporally, we look at the second wave of the pandemic in the country, which occurred in the first half of 2021; geographically, we analyze municipalities in the North/Northeast region of São Paulo State. We evaluate the effect of lockdown policies, in which establishments are closed to in-person business and the movement of people is restricted to reduce viral transmission. We find that these policies had the desired epidemiological effects without significant negative impacts on employment.

The goal was to evaluate the impact of the policies both in epidemiological and socioeconomic outcomes, such as social isolation, COVID-19 cases and deaths, and employment. Some hypotheses raised about the impact of the policy were that it would: i) *increase* social isolation, ii) decrease cases, iii) *decrease* deaths, and iv) *increase* unemployment.

To test these hypotheses, we leverage different sources of municipal-level data and use a Differences-in-Differences model with two-way fixed effects in an Event Study design. To construct a control group for these estimates, we restrict the sample to municipalities within the same set of Regional Health Departments of the treated ones and look only at policies that were adopted in a given period of the pandemic.

The results obtained add to the literature evaluating NPIs suggesting a positive impact of the policy in epidemiological terms. Social isolation increases in the week after the lockdown, cases drop two weeks later, and deaths drop one month later, all results are significant when evaluating the daily impact of the policy. Surprisingly, employment levels in treated municipalities are not reduced when compared to controls in the months after the lockdown, contradicting the idea of a trade-off between economy and health in this type of policy, at least in terms of employment. In summary, of the four hypotheses listed above, the first three are not rejected and the last one is rejected.

The findings are intuitive when we think of the time lags of the impacts on the different outcomes, taking into consideration the results in the medical literature regarding the progression of the COVID-19 virus. Furthermore, the results are robust to a different control group, built with a PSM procedure, and we find no such effects when we perform a placebo test (Appendices A and B).

This article can serve as a basis and motivation for some further research. It might be interesting to evaluate the lockdown impact using microdata that allows identifying whether the effects vary according to the citizens' skin color or educational level. Moreover, one can assess the heterogeneity of impact according to the timing of lockdown adoption by comparing early- and late-adopters, or the duration of the policy.

Finally, we hope that this study will inform healthcare policies and the public debate in general. The trade-off between health and the economy was taken as true by many, even though there was no clear evidence for it during the COVID-19 pandemic. Moreover, Jair Bolsonaro, the president of Brazil at the time, advocated strongly against these policies and spread misinformation about their effects, and about COVID-19 in general. We contribute to the discussion of the effects of lockdown policies in Brazil through a detailed study of the decisions made at the peak of the pandemic when a small number of vaccines were available, and many lives were being lost every day. Hopefully, we can help mitigate the harms of similar events in the future.

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Appendix A. Propensity Score Matching – PSM

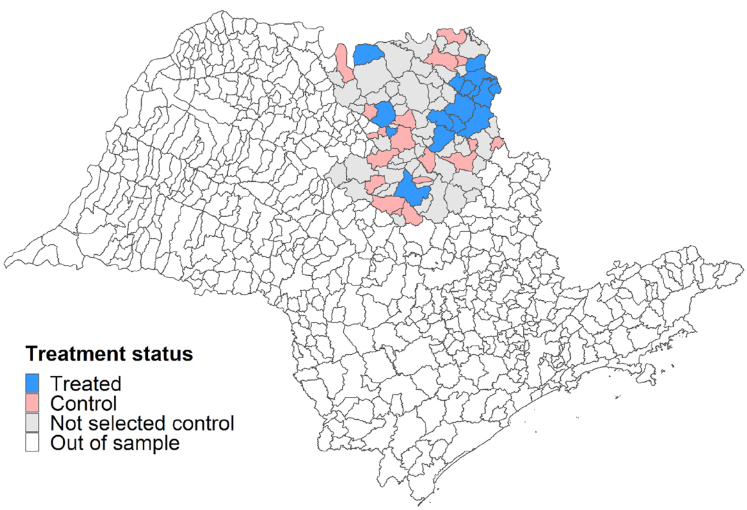

Figure A.1 - Municipalities by treatment status

Notes: The treatment group is composed by the following municipalities: Altinópolis, Araraquara, Batatais, Bebedouro, Brodowski, Colômbia, Cristais Paulista, Franca, Itirapuã, Jardinópolis, Patrocínio Paulista, Restinga, Ribeirão Preto, São José da Bela Vista, Taiúva. The control group is composed by the following municipalities: Aramina, Boa Esperança do Sul, Guaraci, Igarapava, Ituverava, Itápolis, Jaborandi, Jaboticabal, Jeriquara, Monte Azul Paulista, Morro Agudo, Motuca, Santa Cruz da Esperança, Santa Rita do Passa Quatro, Serra Azul, Serrana, Tabatinga, Vista Alegre do Alto, Tabatinga, Vista Alegre do Alto. All municipalities that are not ``Out of sample'' are in the Regional Health Departments of Araraquara, Barretos, Franca or Ribeirão Preto.

Table A.1 - Mean test before/after propensity score matching

Variable	Unmatched / Matched	Mean Treated	Mean Control	Diff	Mean test t	Mean test p-value
Cases per 100k inhabitants	U	34.668	38.15	-15.1	-0.48	0.629
	M		42.532	-34.2	-0.9	0.375
Deaths per 100k inhabitants	U	0.938	1.384	-60.3	-1.75	0.083*
	M		0.671	36	1.09	0.284
Employment per 100k inhabitants	U	720.72	159.09	34.1	1.81	0.074*
	M		84.069	38.6	1.07	0.293
Daily vaccines per 100k inhabitants	U	443.35	449.51	-5.2	-0.16	0.875
	M		397.63	38.4	1.14	0.264
Population	U	100000	27615	55	3.18	0.002***
	M		28606	54.3	1.5	0.145
Elderly population	U	16701	4346.7	56.6	3.26	0.002***
	M		4739.7	54.8	1.51	0.142
VA Agriculture	U	84997	65595	37.4	1.24	0.219
	M		91565	-12.7	-0.34	0.734
VA Industry	U	580000	280000	36.1	1.71	0.091*
	M		160000	50.3	1.48	0.149
VA Services	U	2700000	450000	50.3	3.01	0.003***
	M		470000	50.2	1.37	0.181
VA Administration	U	420000	120000	54.1	3.13	0.002***
	M		120000	52.9	1.46	0.155
GDP per capita	U	32187	33783	-8	-0.24	0.809
	M		27626	22.9	1.1	0.28
Pop. with income under 1/4 mw (%)	U	5.615	4.84	31.8	1.34	0.183
	M		5.4729	5.8	0.15	0.88
Inhabitants/km2	U	1.608	1.238	14.5	0.56	0.576
	M		2.718	-43.6	-0.89	0.379

Notes: The value added (VA) data by sector and GDP per capita are from 2018. The data on share of population with income under ¼ of a minimum-wage (mw) are from the 2010 Census. For the variables that are not constant over time, an average was calculated for the period before the policy, from April 14 to May 14, 2021.

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.2 - Lockdown effect on social isolation, by week

Independent variables	Dependent variable: Social isolation (1)	(2)	(3)
Week 1	1.659**	1.869**	1.796*
	(0.503)	(0.667)	(0.847)
Week 2	3.000***	1.819**	1.614
	(0.493)	(0.695)	(1.023)
Week 3	-0.013	0.364	0.016
	(0.528)	(0.637)	(1.203)
Week 4	-0.553	0.152	-0.320
	(0.561)	(0.607)	(1.392)
Week 5	-0.567	-0.417	-1.274
	(0.545)	(0.676)	(1.795)
Week 6	-0.820	-0.123	-1.380
	(0.542)	(0.650)	(2.038)
Week 7	-0.753	0.398	-1.446
	(0.502)	(0.639)	(2.309)
Municipal FE	No	Yes	Yes
Date FE	No	Yes	Yes
Municipal-specific trends	No	No	Yes
R-squared	0.096	0.967	0.974
Observations	361	348	348

Notes: Robust standard errors are in parenthesis. The regression was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as a 7-day moving average. The control group is selected using propensity score matching (PSM).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.3 - Lockdown effect on cases per 100k inhabitants, by week

Independent variables	Dependent variable: Cases (1)	(2)	(3)
Week 1	4.684	0.033	0.403
	(2.739)	(6.099)	(3.700)
Week 2	5.235*	1.469	2.091
	(2.441)	(6.094)	(4.514)
Week 3	5.108**	-1.679	-1.863
	(1.874)	(6.061)	(5.380)
Week 4	1.138	-8.467	-9.609
	(1.452)	(6.063)	(6.773)
Week 5	1.274	-4.328	-6.067
	(1.971)	(6.243)	(8.024)
Week 6	-1.746	2.67	-0.184
	(1.243)	(6.236)	(9.484)
Week 7	-6.711***	2.877	-1.828
	(1.243)	(6.236)	(9.484)
Municipal FE	No	Yes	Yes
Date FE	No	Yes	Yes
Municipal-specific trends	No	No	Yes
R-squared	0.023	0.436	0.653
Observations	2598	2598	2598

Notes: Robust standard errors are in parenthesis. The regression was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as a 7-day moving average per 100,000 inhabitants. The control group is selected using propensity score matching (PSM).
*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.4 - Lockdown effect on deaths per 100k inhabitants, by week

Independent variables	Dependent variable: Deaths		
	(1)	(2)	(3)
Week 1	0.758***	-0.020	-0.057
	(0.104)	(0.168)	(0.168)
Week 2	0.494***	0.141	0.048
	(0.095)	(0.178)	(0.219)
Week 3	0.877***	0.192	0.029
	(0.159)	(0.183)	(0.285)
Week 4	0.606***	0.284	0.003
	(0.097)	(0.166)	(0.339)
Week 5	0.213*	0.090	-0.206
	(0.084)	(0.172)	(0.412)
Week 6	-0.214***	-0.224	-0.515
	(0.054)	(0.173)	(0.490)
Week 7	-0.210***	0.074	-0.208
	(0.046)	(0.170)	(0.567)
Municipal FE	No	Yes	Yes
Date FE	No	Yes	Yes
Municipal-specific trends	No	No	Yes
R-squared	0.143	0.458	0.575
Observations	2598	2598	2598

Notes: Robust standard errors are in parenthesis. The regression was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as a 7-day moving average per 100,000 inhabitants. The control group is selected using propensity score matching (PSM).

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A.5 - Lockdown effect on employment per 100k inhabitants, by month

Independent variables	Dependent variable: Employment		
	(1)	(2)	(3)
Month 1	241.603	398.307	277.082
	(200.595)	(216.512)	(293.385)
Month 2	129.299	274.046	42.049
	(120.929)	(203.106)	(366.294)
Month 3	241.537*	367.578	7.864
	(105.635)	(196.225)	(494.867)
Month 4	181.882*	365.685	-206.110
	(76.896)	(201.599)	(697.903)
Municipal FE	No	Yes	Yes
Date FE	No	Yes	Yes
Municipal-specific trends	No	No	Yes
R-squared	0.022	0.134	0.346
Observations	229	229	229

Notes: Robust standard errors are in parenthesis. The regression was estimated with weighted least squares, where the weight is the municipality's population. The dependent variable is calculated as per 100,000 inhabitants. The control group is selected using propensity score matching (PSM).

*** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Appendix B. Placebo Lockdown

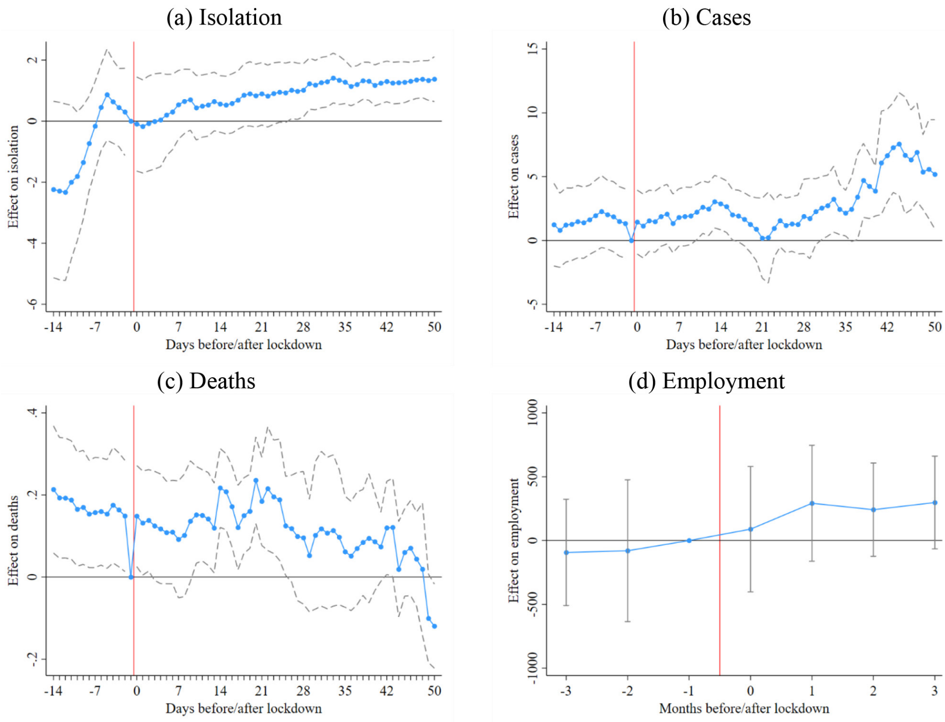

Figure B.1 - Results with placebo lockdown

Notes: These figures replicate the main analysis but using a placebo lockdown policy "adopted" one year before the actual policy. The regressions control for municipality and day fixed effects and municipal--specific trends. Robust standard errors were used to construct the 95% confidence interval represented by the dashed lines.