USING A NATURAL EXPERIMENT TO ASSESS THE SHORT-RUN COSTS AND EFFECTIVENESS OF INTENSIFYING SOCIAL ISOLATION AT THE BEGINNING OF THE COVID-19 PANDEMIC[1]

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After imposing restrictive measures when the first cases of covid-19 were identified in the state, the government of Rio Grande do Sul, through State Decree No. 55,184 of April 15, 2020, gave discretionary power to the municipalities to adopt or abolish restrictive measures to economic activities, in particular, related to the retail of goods and services with face-to-face customer service. This article uses the natural experiment created by this decree to evaluate the costs and benefits of intensifying social isolation when there is no identified outbreak. To achieve this goal, this article uses sales information with electronic invoices and covid-19 cases, hospitalizations, and deaths to estimate models of difference in differences with weekly panel data from March 22 to May 16, 2020. The results show that the short-run economic costs of intensifying social isolation were around BRL 889 thousand per day. At the same time, the short-run benefits of reducing cases, hospitalizations, and deaths caused by covid-19 were small and not statistically significant. Therefore, the article concludes that, without an outbreak, the economic costs of intensifying social isolation were relevant and consistent. At the same time, their benefits in terms of health outcomes were small and uncertain.

Keywords: covid-19; social isolation; difference in differences; cost-benefit analysis.

USANDO UM EXPERIMENTO NATURAL PARA AVALIAR OS CUSTOS E EFETIVIDADE DE CURTO PRAZO DA INTENSIFICAÇÃO DO ISOLAMENTO SOCIAL NO INÍCIO DA PANDEMIA DE COVID-19

Após impor medidas restritivas quando os primeiros casos de covid-19 foram identificados no estado, o governo do Rio Grande do Sul, por meio do Decreto Estadual nº 55.184, de 15 de abril de 2020, deu poder discricionário aos municípios para adotar ou abolir medidas restritivas a atividades econômicas, em especial, relacionadas ao comércio varejista de bens e serviços com atendimento presencial. Este artigo utiliza o experimento natural criado por esse decreto para avaliar os custos e benefícios da intensificação do isolamento social quando não há um surto identificado. Para atingir esse objetivo, este artigo utiliza informações de vendas com notas fiscais eletrônicas e casos, internações hospitalares e óbitos por covid-19 para estimar modelos de diferença em diferenças com dados de painel semanais de 22 de março a 16 de maio de 2020. Os resultados mostram que, no curto prazo, os custos econômicos executados com a intensificação do isolamento social ficaram em torno de R$ 889 mil por dia. Ao mesmo tempo, os benefícios de curto prazo da

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redução de casos, hospitalizações e mortes por covid-19 foram pequenos e não estatisticamente significativos. Dessa maneira, o artigo conclui que, sem um surto, os custos econômicos da intensificação do isolamento social foram relevantes e consistentes, enquanto seus benefícios em termos de resultados na saúde pública foram pequenos e incertos nos municípios tratados.

Palavras-chave: covid-19; isolamento social; diferença em diferenças; análise custo-benefício.

USANDO UN EXPERIMENTO NATURAL PARA EVALUAR LA COSTUMBRE Y LA EFICACIA DEL CORTO PLAZO DE INTENSIFICACIÓN DEL AISLAMIENTO SOCIAL DESDE EL INICIO DE LA PANDEMIA DE COVID-19

Después de imponer medidas restrictivas cuando se identificaron los primeros casos de covid-19 en el estado, el gobierno de Rio Grande do Sul, a través del decreto estatal 55.184, de 15 de abril de 2020, otorgó discrecionalidad a los municipios para adoptar o derogar medidas restrictivas. actividades económicas, en particular, relacionadas con el comercio al por menor de bienes y servicios con atención presencial. Este artículo utiliza el experimento natural creado por este decreto para evaluar los costos y beneficios de intensificar el aislamiento social cuando no hay un brote identificado. Para lograr este objetivo, este artículo utiliza información de ventas con facturas electrónicas y casos de covid-19, ingresos hospitalarios y muertes para estimar modelos de diferencias en diferencias con datos de panel semanales del 22 de marzo al 16 de mayo de 2020. Los resultados muestran que en el a corto plazo, los costos económicos incurridos con la intensificación del aislamiento social rondaron los R$ 889 mil por día. Al mismo tiempo, los beneficios a corto plazo de reducir los casos, las hospitalizaciones y las muertes por covid-19 fueron pequeños y no estadísticamente significativos. De esta forma, el artículo concluye que, sin un brote, los costos económicos de intensificar el aislamiento social fueron relevantes y consistentes, mientras que sus beneficios en términos de resultados de salud pública fueron pequeños e inciertos.

Palabras clave: covid-19; aislamiento social; diferencia en diferencias; análisis costo-beneficio.

JEL: C23; D61; I18.

1 INTRODUCTION

The expansion of covid-19 represents one of the most challenging economic and health crises the world has recently experienced. From China, the SARS-CoV-2 virus has spread to many countries, generating enormous costs in terms of lives lost and economic losses. At the beginning of the pandemic, governments worldwide have implemented a mix of non-pharmaceutical interventions (hereafter NPIs) to prepare health services for increased demand and thus reduce losses caused by a possible inability to provide adequate care to covid-19 patients. From a theoretical point of view, the use of such policies seems obvious because, by restricting social interactions, there would be a consequent reduction in the spread of the virus and because, as shown by Eichenbaum, Rebelo and Trabandt (2020), people fail to internalize the costs they impose on others when they are potential virus transmitters. Therefore, restrictive measures imposed by governments would be a way to internalize these externalities and redistribute the social costs of the pandemic such that interventions would be socially optimal (Alvarez, Argente and Lippi, 2021; Farboodi, Jarosch and Shimer, 2021).

Following this reasoning, governments have imposed substantial restrictions on economic activities and people's mobility during the pandemic. This has caused widespread disruption in most economies on at least two fronts. On the one hand, governments applied measures imposing the closure of entire sectors of their economies that were considered at greater risk of spreading the virus or sectors that were considered "non-essential." On the other hand, faced with the fear of contamination by the virus, people have restricted or changed their work activities and consumption habits. Therefore, even in the absence of restrictive measures, it is likely that there would be a contraction in (economic) activities because people would voluntarily reduce social interactions and stay more at home out of self-interest, motivated by the fear of contamination by the new virus.

From an economic perspective, the unexpected nature of the virus' arrival, combined with the changes in behavior and restrictive measures, exposes the economy to supply and demand shocks. Most sectors of the economy are exposed to both types of shocks. For example, sectors such as transport are more exposed to demand-side reductions, while other sectors with little capacity to adapt to established protocols and with limitations to introduce homework, such as manufacturing and retail sectors, are probably more exposed to supply-side shocks. However, other sectors, such as entertainment, restaurants, and hotels, experienced strong supply and demand shocks and have almost disappeared throughout the first months of the pandemic. Nevertheless, the substantial impacts on some sectors cannot be seen as a localized problem in one sector, which is considered non-essential, as it cannot be ignored that shocks in supply and demand in some sectors spread to others. In this sense, Guerrieri et al. (2022) argue that reducing supply in some sectors would generally reduce the demand in sectors not initially affected.

Considering these economic costs, with many still unknown, it is a fact that in deciding what level of restrictions on economic activities they should impose, governments at all levels have faced a trade-off between containing the virus spread and preventing the short-run collapse of the local economy. In Rio Grande do Sul, the situation was no different, and at the beginning of the pandemic in Brazil, in mid-March 2020, when the first covid-19 cases were registered, state decrees with restrictive measures were published. In this sense, the most relevant in the first months were the State Decrees No. 55,128 on March 19, 2020, and No. 55,154 on April 1, 2020, which imposed restrictive measures to limit people's mobility and prohibit public gatherings to contain the spread of SARS-CoV-2. These restrictive measures have generated high economic costs for the state. Oliveira (2020b) estimates that the losses in terms of sales would be BRL 43.34 billion (more than 40% of expected monthly sales) and, in terms of tax collection losses, it would be around BRL 1.56 billion (more than 50% of expected monthly tax collection) in the first 27 days when these measures were in force.

Thus, despite the crucial importance from a public policy point of view, even after a while, many doubts remain, and the empirical assessment of these restrictive measures implemented at the beginning of the pandemic in terms of cost-benefit is still limited.[5] The main question is to what extent restrictive measures aimed at encouraging the reduction of social interactions effectively contain and delay the spread of covid-19 when the pandemic is just at the beginning and there is no outbreak.[6] In addition, it is important to estimate the costs generated for the economy due to these restrictive measures.

So, these and many other questions still need to be answered, and only a small share of empirical studies used research designs that allow causal inference. Despite the greater data availability as the pandemic progressed, this scarcity is probably due to the difficulty of finding natural experiments or exogenous variation sources to evaluate such measures when almost all places in the world have restrictions in force (were treated) at some point. In this context, the period at the pandemic's beginning may help source places capable of being used as control groups. These are the cases of Friedson et al. (2021), who use synthetic control to assess social isolation measures in California, the first American state to adopt restrictive measures, and Born, Dietrich and Müller (2021), who use the same method to assess the absence of restrictive measures in Sweden. In addition to these studies, there are at least three other articles that use natural experiments to apply the difference in differences methods using data from the beginning of the pandemic (Dave et al., 2021; Fang, Wang and Yan, 2020; Gupta et al., 2021) and one study that use instrumental variables (Bjørnskov, 2021) to assess whether non-pharmaceutical interventions can reduce interactions, and covid-19 related cases, hospitalizations, or deaths.

Gupta et al. (2021) use mobility data in the United States to evaluate the impacts of restrictive measures on observed social mobility and, secondly, to assess the effects of restrictive measures on cases and deaths recorded in American states and counties. The authors conclude that state and municipal orders to stay at home and other policies, such as state emergency declarations and news of the first health threats at the state and municipal levels, have led to declines in mobility. The authors did not find public health benefits from these measures regarding reductions in recorded cases and deaths in the study. These results differ

5. According to Weill et al. (2021), until March 2021, it was possible to find about 23 thousand studies on the subject on Google Scholar without reaching a definitive conclusion on, for example, its efficiency.

6. Data from the Health Department of Rio Grande do Sul shows that these initial restrictive measures were implemented during a period of extremely low number of covid-19 cases. The registered cases by day moving average were around 300 in our study sample period. However, after that, numbers were always high. So far, the peak was achieved in March 2021 with a moving average of around 9500 cases per day, which led the hospital sector to collapse. The state population in 2020 was about 11.29 million inhabitants. Updated information about the pandemic statistics in the state can be seen (in Portuguese) on the official page of the Health Department of the State available at: https://ti.saude.rs.gov.br/covid19/.

from those of Dave et al. (2021), who used daily American data from covid-19 cases at the state level and with the same methodology, found that approximately three weeks after the adoption of a restrictive measure of the type *Shelter in Place Orders, hereafter SIPO* cumulative cases of covid-19 were reduced by about 44%.

With Chinese data, Fang, Wang and Yang (2020) seek to identify the causal impact of the people's mobility restriction measures, particularly of the lockdown in the city of Wuhan initiated on January 23, 2020, to contain the spread of covid-19 in the Chinese Hubei province. Their results show that the city lockdown has significantly reduced the number of infection cases outside Wuhan, even considering the social distancing measures later imposed by other Chinese cities. Herby, Jonung and Hanke (2022) summarize restrictive measures' efficacy on covid-19 mortality through a meta-analysis that includes 24 difference in differences or instrumental variables studies after three screening levels in a sample that started with 18,590 studies. The authors find that stringency index studies find that lockdowns in Europe and the United States only reduced mortality by 0.2% on average and that SIPOs only reduced covid-19 mortality by 2.9% on average. Following these results, the authors concluded that restrictive measures, also known as lockdowns, have had little to no effect on mortality associated with the virus.

Bjørnskov (2021) tries to solve a significant problem presented in the studies that use the difference in differences methods, the simultaneity between restrictive measures and covid-19 cases. It is plausible to state that places facing a potential outbreak will implement restrictive measures that reduce social interactions. The next section will cover this significant caveat in more detail when we present our identification strategy. So, the author uses the discretionary power granted by the constitutional emergency provisions in 24 European countries as an instrument for restrictive measures. Since the limits to establish restrictions were determined years before the pandemic by countries' Constitutions, it provides the exogenous variation necessary to assess the impacts of restriction measures on the mortality caused by covid-19. The author finds no clear association between such restrictive measures and mortality evolution.

However, a weakness of these studies is that they make a partial welfare evaluation of these measures. In other words, they do not simultaneously evaluate the costs and benefits, which, although helpful, is insufficient to assist policymakers in decision-making. The exception is Friedson et al. (2021), which shows that saving lives can have a high cost. Even though they show that California SIPO measures led to less than 1,661 deaths under covid-19 during the study period, they also show that the cost of these measures was about 400 job losses for each life saved during a short post-treatment period at the beginning of the pandemic.

This article seeks to contribute to the literature by identifying the causal effect of social isolation intensification on health and economic variables. This allows us to do a limited and short-run cost-benefit analysis of this treatment in some Rio Grande do Sul municipalities. For this purpose, it leverages the natural experiment created by State Decree No. 55,184 of April 15, 2020, at the onset of the pandemic. This decree revoked State Decree No. 55,128 of March 19, 2020, which had imposed various restrictions on mobility and economic activities. The new decree granted municipalities the discretionary authority to implement or lift measures restricting economic activities, particularly in the retail sector, which relies heavily on in-person customer interactions. However, as these measures adopted by municipalities are challenging to observe and are, at best, an intention to treat, this study uses a measure of social isolation obtained through the monitoring of mobile phones, which allows the identification of control and treatment groups according to the effective treatment observed (intensification of social isolation) after this decree in the municipalities of Rio Grande do Sul.

Thus, with this research design, the difference in differences models controlling for two-way fixed effects are estimated with the use of information on new cases, hospitalizations, and deaths caused by covid-19 and sales with the help of Electronic Invoice Documents to evaluate the short-run benefits and costs of social isolation intensification at the beginning of the pandemic when there is not an apparent outbreak. To deal with the potential endogeneity problem between observed social isolation and health variables in difference in differences models, we used a database that considers the onset date of symptoms. This allows us to work with data closer to the actual number of cases per week than those based on registration dates and to work with data unknown to policymakers and municipality residents when decisions about social isolation were taken. Our main argument is that both make decisions based on information at least three weeks out of date, in a way that is not a current relationship between outcome variables and the treatment assignment mechanism.

The article is structured as follows. Besides this introduction, the article has three more sections. The following section presents the data used, the strategy used to identify the groups, and the methodology used to identify the causal effects in the face of some problems that arise in the modeling. The third section shows the results and their discussion. At the end of the article, some concluding remarks are presented.

2 IDENTIFICATION STRATEGY

2.1 Data

The data used in this study comes from three sources.[7] Information related to the healthy outcome variables, that is, new cases, hospitalizations, and deaths by covid-19 according to the date of onset of symptoms, comes from a survey carried out by the Health Department of the State of Rio Grande do Sul based on information from the SRAG-E-SUS, from e-SUS Notify, which surveys cases of flu syndrome that do not require hospitalization, and from the Influenza Epidemiological Surveillance Information System (Sistema de Informação da Vigilância Epidemiológica da Influenza – Sivep-Influenza), which monitors the patients treated in healthy units.

As is well known, new cases have underreporting problems caused by the fact that many people with SARS-CoV-2 are asymptomatic. At the pandemic's beginning, some protocols defined who would be tested. In Brazil, according to the protocol established by the Ministry of Health, until March 19, 2020, only patients with the symptoms of covid-19 who had returned from travel abroad or had contact with someone who had traveled abroad were tested. From this date on, the protocol was changed, and it was determined that all patients with symptoms of covid-19 for more than eight days should be tested.

It should be noted that under-reporting can be a source of measurement error that can cause bias in estimations (Gupta et al., 2021). However, this problem is mitigated if (non) testing rates are similar among municipalities. This study seeks to minimize this problem by limiting the analysis by using data from new cases and deaths only after March 22, i.e., the new protocol that expanded the testing to a larger sample. The aim is to use a period with more reliable data and enough variation to estimate an econometric model since it was the pandemic's beginning. Before this period, it is possible to observe many zeros in the sample.

The social isolation index (SII) was obtained from "In Loco" company. This index is calculated from the proportion of users who did not leave their place of residence, who "stayed at home" on the day in a particular region, which can be a municipality or state. This is calculated by the proportion of cell phones displaced during the day less than 450 meters from the place during the night/dawn period. This monitoring was done through GPS information from mobile devices of more than 60 million application users that used the company's technology, approximately 30% of the Brazilian population. In the Rio Grande do Sul, besides its limitations (available in only 270 of 496 municipalities), the state's health authorities used the index to monitor people's mobility. This study uses it to determine the control and

7. In the appendix A, table A.1 presents a descriptive statistic of these variables.

treatment groups. But we must deal with other limitations. Although the index was available since February 1, 2020, information from 73 municipalities was unavailable for all periods because it had only started to be calculated later. Therefore, these municipalities had to be excluded from the sample to keep our panel balanced. Besides, another 12 municipalities with more than 200,000 inhabitants had to be excluded from the sample to obtain a good control group capable of attending to parallel trends assumption. Thus, the sample used in this study has 185 municipalities.[8]

The sales per municipality are nonpublic data from the State Department of Treasure. They are obtained through Electronic Invoice Documents (Documentos Fiscais Eletrônicos – DF-e), which comprise the sum of Electronic Invoices (Notas Fiscais Eletrônicas – NF-e), which are issued in all business operations involving the circulation of goods, such as sale to a legal entity, return, transfer, among others, and Consumer Electronic Invoices (Notas Fiscais de Consumidor Eletrônica – NFC-e), which are issued in operations of face-to-face sale or delivery to the final consumer. The article also uses the Electronic Transport Invoice (Conhecimento de Transporte Eletrônico – CT-e), issued to cover cargo transport service operations, and the Electronic Tickets (Bilhetes de Passagem Eletrônico – BP-e), issued to document passenger transport service provision, such as road, waterway, and rail transport. The daily information is converted into weekly daily averages, and the available periodicity of these data is six weeks from the starting date of this study, March 22.

2.2 Group's identification

To identify the municipalities in the control and treatment groups, this study uses the changes observed in the SII, which, as already commented, began to be calculated on February 1, 2020. However, the initial date of our analysis is the following day, which marks the beginning of the first week with complete information that allows obtaining the daily average of the index by week. So, the week between February 2 and 8 is the starting week of this study.[9] Thus, considering that the enactment of State Decree No. 55,185 occurred on April 15 (the eleventh week), it is possible to define the beginning of the treatment period as the twelfth week.

With this index, it is possible to observe the changes in the population's behavior living in these municipalities during the first months of the pandemic. Figure 1 shows that in the fourth week (February 23 to 29), almost all the municipalities included in the sample maintained a lower SII than in the eleventh week. That is, with few cases of covid-19 registered, and in the absence of restrictive measures imposed by municipal and state decrees, the people's mobility followed its ordinary course.

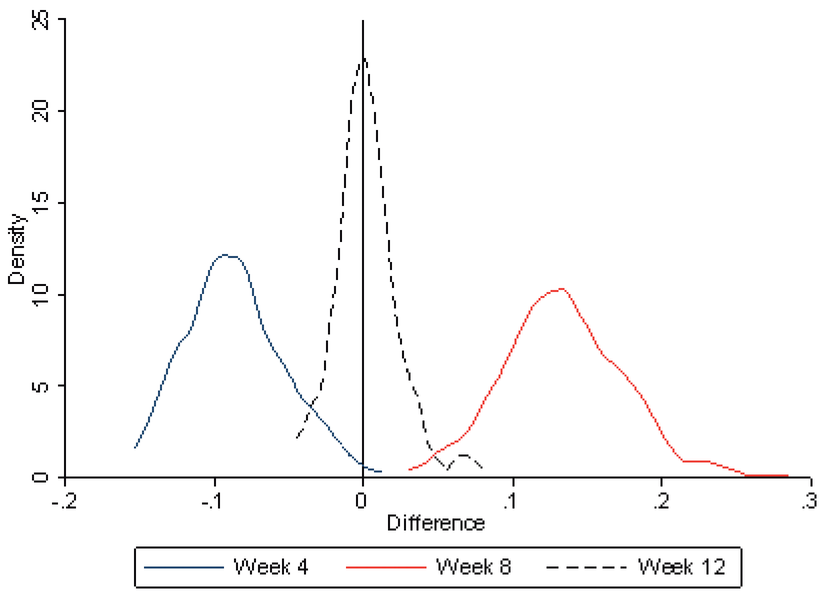
8. Figure A.1 in the appendix A shows a map where it's possible to observe the representativity of our sample.

9. It should be noted that this formatting serves only as a temporal reference because the estimated models consider information from the eighth week, which begins on March 22, 2020.

However, this scenario has changed a lot since the State Decree No. 55,128 of March 19, 2020,[10] which, among other measures, established the prohibition of public and private interstate bus circulation and transportation, the closure of stores in shopping malls, limitation on attendance provided by supermarkets, pharmacies, and restaurants, the prohibition of events and public gatherings of any nature, public or private, including excursions, face-to-face courses, religious cults, with more than 30 people, limitations on inter-municipal transportation, and urban and rural public transportation buses, in addition to other measures with less impact on the people's mobility.

The result of this decree can be observed in the following week (the eighth week, from March 22 to 28). All the municipalities included in the sample had a higher SII than in the eleventh week. All municipalities could be considered treated in statistical language to intensify social isolation. Therefore, it is difficult, not to say impossible, to make a causal inference regarding the effects of social isolation since, until this period, it was only possible to observe all municipalities with and without treatment.

FIGURE 1
Differences in the SII from the reference week



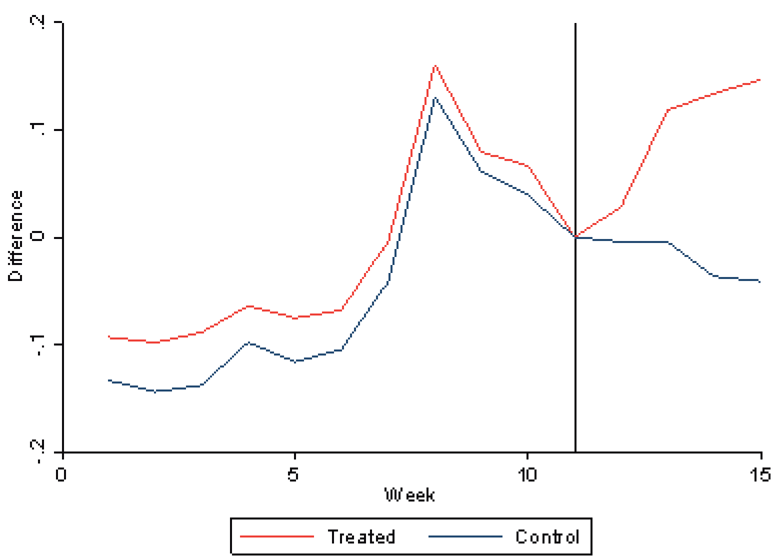
Source: SII from the company "In Loco."
Authors' elaboration.
Obs.: 1. Week 4 refers to the interval between February 23 and 29, 2020; week 8 refers to the interval between March 22 and 28, 2020; and week 12 refers to the interval between April 19 and 25, 2020.
2. Figure whose layout and texts could not be formatted due to the technical characteristics of the original files (Publisher's note).

10. The restrictive measures were further intensified by a second decree, State Decree No. 55,154 of April 1, which determined the business closure throughout the whole state of Rio Grande do Sul according to classification between non-essential and essential activities, which are described in art. 17 of the same decree. All non-essential businesses should remain closed, and the essential ones should obey some restrictive criteria for compliance.

The third decree altered this behavior, the State Decree No. 55,184 of April 15, which established discretionary powers for state municipalities to adopt or abolish measures restricting economic activities.[11] It generated a group's division in the observed SII, as shown in figure 1, in the twelfth week, April 19 to 25. So, some municipalities maintained the measures restricting economic activities, and their residents intensified their precautionary behavior (deepened their social isolation), especially from the fourteenth week, our treatment group, while other municipalities relaxed some measures, and their residents increased their mobility (reduced social isolation). The difference in behavior between the groups can be better observed in figure 2.

The two groups had similar behavior until the week of the third decree, with the treated group having a slightly higher average SII than the control group. At least until the eleventh week, both were in the same group. All the municipalities were in the control group until the seventh week and treated from this week until the eleventh week. However, from the eleventh week, there is a difference in the group's strategy, observed in terms of the SII, i.e., one group showing an increase in the SII (with 67 municipalities), and the other showing a decrease (with 118 municipalities) in subsequent weeks.

FIGURE 2
The difference in SII about the eleventh week by groups of municipalities



Authors' elaboration.
Obs.: 1. Values are the difference in the average weekly daily isolation compared to the eleventh week (April 12 to 18).
2. Figure whose layout and texts could not be formatted due to the technical characteristics of the original files (Publisher's note).

11. Basically, the decree provides the discretionary power to authorize activities in the service sector, especially face-to-face customer service. In paragraph 4, the Decree states, "the commercial establishments referred to in the caption of this article may have their opening to the public authorized, using a substantiated act of the competent municipal authorities, supported by scientific evidence and analysis of strategic health information" (Rio Grande do Sul, 2020, art. 5, § 4, our translation).

Although the separation of groups regarding social isolation behavior, many problems remain to be overcome to identify treatment effects (intensification of social isolation). First, treatment is heterogeneous and continuous. Municipalities applied different levels of social isolation over time. However, identifying continuous treatment effects is a challenge still being studied by very recent literature, and well-established solutions still need to be presented (Callaway, Goodman-Bacon and Sant'anna, 2021). So, even recognizing a loss of an important source of variability, in this article, we opted for a simple separation into two groups with an irreversible binary treatment that started at the same time (eleventh week) for all treated municipalities since this design allows the estimation of treatment effects in a way that is well established in the literature (Callaway and Sant'anna, 2021). Second, potential endogeneity problems should be addressed since, in theory, the municipalities select themselves. We argue that restriction measures are not enough to induce social isolation and that the observed social isolation results from, among other things, voluntary adherence by residents and the enforcement capacity of municipalities' authorities. In other words, nobody has perfect control over the treatment assignment. However, one cannot initially determine whether the assignment depends on the outcome variables. Our main argument against this potential source of endogeneity is that this self-selection decision is based on outdated information regarding the stage of the pandemic in the municipality.

According to Anderson et al. (2020), the delay for symptoms to appear in the United States varies from 2 to 14 days, with an average of 5 days of virus incubation. These numbers are corroborated by Lauer et al. (2020), who estimate from Chinese data that the mean incubation period is 5.1 days and that 97.5% of Chinese patients developed symptoms within 11.5 days. After the onset of symptoms, since the effectiveness of the tests depends on the stage of infection, there are some recommendations to achieve more precise test results. Molecular tests such as PCR are recommended to be applied three to seven days after symptom onset. In contrast, rapid antibody tests such as IgG and IgM are recommended to be used after the eleventh day of onset of symptoms. Rapid tests are released on the same day, while PCR tests depend on the laboratories' capacity to process the tests. RT-PCR tests also give results on the same day, but most laboratories were unprepared for this new demand at the pandemic's beginning. So, there were delays not only in Brazil but also in many countries. These delays in disclosing test results resulted in delays in registering new cases. Anderson et al. (2020) conclude that the average period to register a new case in the United States officially would be around 14 days after infection.

In Rio Grande do Sul, identified positive cases were immediately reported by the laboratory or health unit to the health state authority since the beginning of the pandemic. Still, in the studied period, the time for a covid-19 positive case to become an official record in Rio Grande do Sul was around 14 days after the symptom onset as seen in table 1.

TABLE 1
Time for a covid-19 positive case to become an official record in the state of Rio Grande do Sul according to the type of test and total
(In 1 day)

Test type	Mean	Standard Deviation	N[1]
PCR	8.3668	13.1720	4,053
Rapid test (IgG and IgM)	19.8013	21.5649	4,701
Other	5.7487	16.3378	199
Total	**14.3126**	**19.0276**	**8,953**

Source: e-SUS. Available at: https://ti.saude.rs.gov.br/covid19/download?2020. Accessed on: Jul. 14, 2021.
Authors' elaboration.
Note: [1] N represents the number of positive tests.
Obs.: The sample period runs from the first week, February 2nd, to the fifteenth week, ending May 16th, 2020.

In the first months of the pandemic, rapid tests were the most common way to detect covid-19 cases in the state. The data in table 1 shows that these tests were, on average, applied about 20 days after the onset of symptoms. If, on the one hand, this period increases its effectiveness in determining the presence of the virus by this test type, on the other hand, it generates a relevant delay in the detection of cases by the health authorities. However, the main point of our argument is that real-time covid-19 cases were unknown to policymakers and the municipality's residents since they used, at best, information about the number of compelling new covid-19 cases three weeks before the intervention. Considering the time between contamination and the appearance of symptoms plus the time to perform a test and have its result registered by the health authority, the total time would be two weeks for PCR tests, four weeks for rapid tests, and three weeks on average. In other words, at the time of the State Decree No. 55,184 of April 15, 2020, policymakers and the municipality residents made decisions about restrictive measures and social isolation, considering only a few recorded covid-19 cases provided by average information, three weeks out of date.

So, we have confidence in the independence between treatment assignment and outcome variables arising from the temporal disconnect between the timing of policy decisions and the availability of real-time covid-19 data. Given the significant lag between symptom onset, testing, and case registration – averaging three weeks – policymakers relied on outdated information when implementing interventions. This delay ensures that the assignment of treatments, such as restrictive measures, was not contemporaneously influenced by the immediate state of key outcome variables, such as average daily sales, new cases, hospitalizations, and deaths. The study minimizes the risk of reverse causality by anchoring the analysis on data based on symptom onset rather than recorded cases. It ensures that outcomes reflect the true timeline of disease progression. This methodological approach enhances the robustness of the analysis by isolating the causal effects of policy measures from contemporaneous feedback loops or distortions arising from data lags.

Therefore, when the state government of Rio Grande do Sul provided discretionary power to the municipalities to decide which strategy to adopt, there was a change in the probabilities of treatment assignment that, combined with the outdated information used by the relevant agents, allowed us to use this decree as a natural experiment capable of identifying the treatment attribution without significant concerns about endogeneity problems, since there is no direct relationship between current cases, hospitalizations and deaths (by symptom onset) and sales and the treatment attribution, which is likely related to covid-19 cases occurred, at least, three weeks before. With a causal design, these characteristics allow this study to evaluate the impacts of intensifying social isolation on economic and health variables at the beginning of the pandemic and with no apparent covid-19 outbreak.

Finally, it is worth mentioning that this discretionary power on the part of the municipalities was in force for a short time since, through State Decree No. 55,240 of May 10, 2020, a new policy, the so-called "controlled distance model", was established based on a system of flag colors, which came into force the day after its enactment. The new policy restricted discretionary powers to municipalities. It provided four levels of restrictions, represented by flags in the colors yellow, orange, red, and black, which vary according to the spread of the disease and the health system capacity in each of the 20 predetermined regions, constituted in such a way that there were reference hospitals with Intensive Care Unit (ICU) beds within each of these regions. Therefore, using more updated and endogenous criteria (based on the availability of hospital beds) made it difficult to deal with endogeneity problems and recover the randomness of the treatment assignment mechanism.

For these reasons, in order not to suffer the influences of this new decree or the current cases, this study limits the use of economic information to two weeks after the third decree and health information to four weeks after this decree since it is expected that there will be a period between the time when the measure was imposed and its effects in terms of reduction of the people interactions and later on the records of new cases, hospitalizations, and deaths at the same time that the current covid-19 cases, hospitalizations do not define treatment assignment and deaths observed by the policymakers and by the municipality residents. In other words, we limited the horizon of analysis of our study so that, simultaneously, the treatment of intensifying social isolation has time to produce some effect, and the decision to be treated is not influenced by the outcome variables used in this study.

2.3 Methods

Once the control and treatment groups have been defined, the empirical strategy to obtain the effects of treatment (intensification of social isolation) on the variables related to the economy (sales) and health (new cases, hospitalizations, and deaths by covid-19) is the use of the difference in differences models. In theory, the treatment is expected to negatively impact the supply and demand for goods and services in the municipalities treated and reduce the speed of virus spread to reduce the number of new cases, hospitalizations, and deaths caused by covid-19. The general model to be estimated is given by:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{l=0}^{L} \delta_l D_{it}^l + \beta X_{it} + \mu_{it} \quad (1)$$

in which i indicate the municipality of the state of Rio Grande do Sul, t the period in weeks, Y_{it} represents the variables of interest of this study, i.e., average daily sales per week, new cases, hospitalizations, and deaths per week, D_{it} represents the municipalities treated from the twelfth week, X_{it} represents the weekly number of covid-19 cases in the last month, α_i represents the fixed effects of the municipalities, λ_t represents the fixed effects in time.

In general, any study in which units can select themselves for treatment based on unobservable characteristics that correlate with their potential results constitutes an assignment mechanism that is not random. This study is no different, local policymakers can establish restrictive measures, and municipalities' residents can isolate themselves according to unobservable characteristics. As is well known, this is a source of bias for estimates that cannot be formally tested (Lechner, 2011). However, in this study, considering that the data periodicity is weekly, it is plausible to assume that many of these unobservable characteristics, such as the economic characteristics of the municipality (urbanization, sector shares etc.), are fixed over time. Therefore, the bias can be eliminated by controlling these fixed effects. However, as argued before, it is also plausible to assume that policymakers and the population respond with more restrictive measures and less mobility as the number of covid-19 cases increases in the municipality. But, as previously argued, they had access to outdated information, so there is no current relationship between outcome variables and the treatment assignment. Still, there is probably a relationship with previously registered cases, so a more significant number of previous instances increases the probability of being in the treated group. Thus, we use a covariate that undoubtedly is not affected by the treatment, the weekly number of covid-19 cases in the last month (four weeks), to restore the randomness of treatment.

Including this covariate requires an additional assumption for the model that estimates the effects of the treatment in new cases. This is because a relationship between the treatment and previous values of the outcome variable could bias the estimates if the treatment is anticipated. In other words, if people can anticipate that the increase in the number of cases today will imply more intensive social isolation in the future, there will be a problem of simultaneity where the treatment assignment also depends on the outcome variable. However, it is not a plausible statement if we consider an environment where there needs to be more information about measures that could be taken in the future, especially by the state government. Therefore, specifically in these models, it is assumed that there are no anticipatory effects of treatment. This assumption is not necessary for the other models.

In addition, this study assesses the dynamics of the treatment effect. While the impact of intensified social isolation on sales is expected to be almost immediate due to economic constraints on both the supply and demand sides of municipalities, the effects of the treatment on new cases, hospitalizations, and deaths are very likely to have a distinct dynamic transition. As mentioned before, a transition period is required between the period in which the individual was exposed and infected by the virus and the effective registration as a patient with the disease caused by it. Previews studies such as Oliveira (2020a) estimate that the most relevant impact of social isolation in the cases of covid-19 in Brazil occurs on the eleventh day, therefore, in the second week after a change. These results are similar to those of Fang, Wang e Yang (2020), who found a peak on the eleventh day for cities in Hubei province and the fourteenth day outside this Chinese province. The statistically significant impacts of social isolation on the deaths observed in Oliveira (2020a) appear on the 23rd day, close to the three weeks that Flaxman et al. (2020) estimated. Thus, since this study limits the evaluation of the impacts of intensified social isolation in new cases, hospitalizations, and deaths (by symptoms onset) to four weeks after the start of treatment, this would be sufficient time, according to the literature, for the treatment to take effect and not be influenced by the measures of State Decree No. 55,240, which removed the powers of the municipalities to decide on restrictive measures. It came into force on May 11, 2020.

3 RESULTS

As described in the previous section, this article evaluates the impacts of intensifying social isolation on four (outcome) variables of interest: sales, new cases, hospitalizations, and deaths caused by covid-19. These impacts are evaluated in a static (with a unique average effect over the periods) and dynamic form (with the impact varying over the weeks after treatment).

The results shown in table 2 indicate that the intensification of social isolation after April 15 resulted in a loss in sales of BRL 889,000 per day in the two weeks following the decree's entry into force. This represents about 27% of potential sales[12] in the treated municipalities. In turn, the intensification of social isolation did not significantly impact the treated group's new cases, hospitalizations, and deaths. On average, the treatment effects on the treated are negative for new cases and hospitalizations and positive for deaths over the four weeks evaluated. Still,

12. Potential sales were obtained by adding the sales observed after treatment with the estimated treatment effect.

the coefficients show values that are not even close to one, which means the intensification of social isolation could not reduce a single case or hospitalization by covid-19 in the treated municipalities in a week. The coefficients are close to zero and show a high dispersion, resulting in no statistically significant treatment effects.

TABLE 2
Effects of the intensification of social isolation after State Decree No. 55,184 of April 15, 2020, on sales, new cases, new hospitalizations, and new deaths by covid-19

	Dependent variable							
	Sales		Cases		Hospitalizations		Deaths	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Treatment_{Average}$	-0.889*** (0.2862)	-	-0.141 (1.5266)	-	-0.209 (0.2056)	-	0.013 (0.0550)	-
Treatment	-	-0.022 (0.1495)	-	-0.326 (1.5353)	-	-0.055 (0.3708)	-	-0.025 (0.0667)
$Treatment_{t+1}$	-	-1.757*** (0.5376)	-	-1.065 (1.7834)	-	-0.127 (0.2311)	-	0.111 (0.1045)
$Treatment_{t+2}$	-	-	-	-1.061 (1.3210)	-	-0.406 (0.2570)	-	-0.063 (0.0547)
$Treatment_{t+3}$	-	-	-	1.888 (3.2826)	-	-0.250 (0.1853)	-	0.029 (0.0526)
n (municipalities)	185	185	185	185	185	185	185	185
t (weeks)	6	6	8	8	8	8	8	8
Test F[1]	15.83 (0.000)	13.83 (0.000)	9.88 (0.000)	7.58 (0.000)	3.90 (0.000)	2.96 (0.001)	4.33 (0.000)	3.81 (0.000)
R^2 within	0.1852	0.1958	0.1977	0.1998	0.0517	0.0529	0.0804	0.0855
R^2 between	0.1587	0.1588	0.8314	0.8314	0.1026	0.1127	0.6072	0.6072
R^2 overall	0.0634	0.0647	0.3810	0.3819	0.0365	0.0380	0.2007	0.2031

Authors' elaboration.
Note: [1] In F tests, the values shown in parentheses are *p*-values.
Obs.: 1. *** $p < 0.01$; ** $p < 0.05$; and * $p < 0.1$.
2. Entries are the average treatment effects on the treated municipalities with social isolation intensification estimated by Ordinary Least Squares obtained through a difference in differences model.
3. All models have control over time for fixed effects per municipality.
4. The standard errors shown in parentheses are clustered by municipalities.

However, these estimated coefficients, shown in columns (1), (3), (5), and (7), represent an average impact over the post-treatment periods. Still, the effects on new cases, hospitalizations, and deaths may have dynamics. Therefore, the dynamics of these treatment effects should be evaluated since periods that would not have a priorly expected impact are considered. However, when this is done, the results change little concerning new cases, hospitalizations, and deaths, and what is observed is a minor economic loss in the first week after treatment, but genuinely relevant and significant in the second week, about BRL 1.75 million per day, which represents about 43% of potential sales of the treated municipalities in a week.

These results are somewhat disappointing considering the expectation that policymakers and the population had to control the spread of the virus through behavioral changes imposed by restrictive measures and campaigns for people to stay at home in the state. However, assessing their validity before further discussing these results is essential. To ensure the validity of these estimations, several robustness checks are performed. Besides the traditional and essential evaluation of the existence of parallel trends among the outcome variables, this study uses as falsification tests a placebo event in a different period (from the State Decree No. 55,154 of April 1) and uses placebo variables that, in theory, were not affected by the decree.

Before presenting these results, it should be noted that one limitation of this study is the lack of weekly information at the municipal level, which may affect the treatment assignment mechanism. Thus, it is impossible to test the impacts of treatment on these variables, although this is crucial to assess the robustness of difference in differences in models. However, at the same time, it is plausible to assume that most of these variables are fixed over time. Therefore, estimation by fixed effects can eliminate the bias caused by omitting such variables, making this evaluation unnecessary.

TABLE 3
Tests for parallel trends

	Dependent variable			
	Sales	Cases	Hospitalizations	Deaths
t-4	-0.005 (0.3112)	-0.247 (1.7009)	-0.163 (0.3840)	-0.014 (0.0754)
t-3	0.044 (0.2012)	0.104 (1.6499)	-0.052 (0.3916)	0.022 (0.0756)
t-2	0.323 (0.2091)	0.656 (1.3124)	0.1453 (0.2904)	0.006 (0.0686)
t-1	-0.182 (0.2019)	0.875 (1.079)	0.054 (0.3107)	0.061 (0.0513)
F test[1]	1.08 (0.3695)	1.87 (0.1177)	1.35 (0.2533)	1.08 (0.3698)
R^2 within	0.0634	0.1558	0.1379	0.1294
R^2 between	0.1024	0.4028	0.3116	0.2601
R^2 overall	0.0288	0.2309	0.1943	0.1712

Authors' elaboration.
Note: [1] The F tests evaluate the hypothesis that the trend coefficients in the three periods are jointly equal to zero. The values shown in parentheses below these F tests are *p*-values.
Obs.: 1. *** $p < 0.01$; ** $p < 0.05$; and * $p < 0.1$.
2. Entries are the differences in the trend between the municipalities belonging to the control group and those treated before the treatment estimated by Ordinary Least Squares.
3. All models have control over time for fixed effects per municipality.
4. The standard errors shown in parentheses are clustered by municipalities.

Thus, we test parallel trends with the procedure suggested by Angrist and Pischke (2008), which includes the interaction between the treated group and the trend with the time trend variable. If the interaction term coefficient is statistically not different from zero, one can reasonably expect that the parallel trend assumptions are satisfied. The results shown in table 3 indicate that, according to the *t-tests* of these estimated coefficients, it is impossible to reject the hypothesis that they are equal to zero at the 5% significance level. Furthermore, the F test for the hypothesis that these coefficients are jointly equal to zero shows that this hypothesis cannot be rejected in any model.[13]

The following procedure uses a different period for the treatment to assess whether there were no longer differences between the groups in the periods before the treatment, as Bertrand, Duflo and Mullainathan (2004) suggest. For this purpose, the period of the second decree was chosen, the State Decree No. 55,154 of April 1, 2020, which imposes many restrictive measures, such as the closure of non-essential business throughout the whole state of Rio Grande do Sul.

The results show that this decree did not significantly impact the treated group. It was expected since all municipalities were under intense social isolation in this period. Many of them had their laws establishing restrictions on the mobility of people and economic activities. It was a situation where every municipality was treated with intense social isolation. With State Decree No. 55,184 of April 15, 2020, municipalities began to adopt different social isolation strategies and policies.

TABLE 4
Falsification tests with the use of a different period

	Dependent variable			
	Sales	Cases	Hospitalizations	Deaths
Treatment	0.064 (0.1874)	0.489 (0.6658)	0.125 (0.1391)	0.017 (0.0349)
F Test[1]	6.18 (0.0000)	5.18 (0.0002)	3.05 (0.0114)	1.46 (0.2045)
R^2 within	0.0547	0.0678	0.0476	0.0509
R^2 between	0.0819	0.3587	0.2266	0.1173
R^2 overall	0.0159	0.1041	0.0704	0.0681

Authors' elaboration.
Note: [1] In F tests, the values shown in parentheses are *p*-values.
Obs.: 1. $p < 0.01$; ** $p < 0.05$; and * $p < 0.1$.
2. Entries are the average treatment effects on treated municipalities estimated by Ordinary Least Squares.
3. All models are difference in differences and control fixed effects per municipality and over time.
4. The standard errors shown in parentheses are clustered by municipalities.

13. In the appendix A, figure A.2 are shown the estimated coefficients for the trend of the two groups. It is possible to observe that these have a common trajectory, which increases the confidence that the hypothesis of parallel trends is not violated in any of the models presented in this study.

The last procedure to evaluate robustness is using two placebo variables, represented by the values in sales using an BP-e and the CT-e. The transport of inter municipalities passengers, which is the issuer of BP-e notes, suffered restrictions since the first State Decree No. 55,128 of March 19, 2020, such as the limitation of operating with only half of its standard capacity, which lasted until the end of the period assessed in this study. In turn, the cargo transportation, which is the issuer of CT-e notes, could operate without any restrictions in any of the state decrees, and, as far as we know, the same occurs at the municipal level since there was a fear of shortages of essential products if this was done. Therefore, there is no reason to expect these sectors to be influenced by the treatment (intensifying social isolation) and serve to carry out a falsification test of the treatment effects. The results shown in table 5 indicate that coefficients are close to zero, so it is not possible to reject the null hypothesis that the effect of treatment (intensification of social isolation) in the treated municipalities is equal to zero.

TABLE 5
Falsification tests with the use of placebos

	Dependent variable			
	Intermunicipal transport ticket sales		Cargo transportation services	
	(1)	(2)	(3)	(4)
Treatment	-0.0001 (0.0001)	-	-0.0124 (0.0100)	-
Treatment	-	-0.00008 (0.00008)	-	0.0098 (0.01062)
$Treatment_{t+1}$	-	-0.00014 (0.00015)	-	-0.0346 (0.0215)
Test F	0.59 (0.7647)	0.58 (0.7947)	5.59 (0.0000)	5.43 (0.0000)
R^2 within	0.0153	0.0156	0.0620	0.0646
R^2 between	0.0038	0.0038	0.2004	0.2005
R^2 overall	0.0019	0.0020	0.0353	0.0354

Authors' elaboration.
Note: [1] In F tests, the values shown in parentheses are *p*-values.
Obs.: 1. $p < 0.01$; ** $p < 0.05$; and * $p < 0.1$.
2. Entries are the average treatment effects on treated municipalities estimated by Ordinary Least Squares.
3. All models are difference in differences and control fixed effects per municipality and over time.
4. The standard errors shown in parentheses are clustered by municipalities.

3.1 Discussion

Considering all the results, we are confident that the estimated treatment effects are robust and, therefore, can be discussed in greater detail. Before that, it is essential to remember that any well-designed empirical study with causal inference can identify treatment effects when they exist but cannot necessarily identify the causal mechanisms. To this end, we need theory.

The fact is that, at the beginning of the pandemic, restrictive measures were taken without previously having a good knowledge of the risks and, therefore, of the costs and benefits – which Sunstein (2014) defined as a condition of ignorance. So, doubts persist as to whether these restrictive measures to economic activities and the reduction of people's mobility at that time were exaggerated responses motivated by the moment's panic (Hammitt, 2013; Shogren and Thunström, 2016). In this context, our results aimed to assess the impacts of intensifying social isolation on variables related to the economy and health from a research design that allowed us to make a causal inference.

Taken together, it enables us to conclude that the intensification of measures restricting business activities at the beginning of the pandemic, when there was no apparent outbreak, generated economic losses and benefits in health outcomes that are doubtful in the best scenario. That is, they show that the benefits are small and highly dispersed. The most plausible explanation for this result is that there were not enough cases, hospitalizations, and deaths for restrictive measures and behavioral changes by municipality residents to change these outcomes.

Moreover, it is essential to clarify that the cost-benefit analysis of intensifying social isolation depends on individual and collective results. The distribution of costs is relevant and can determine their outcome. Imposing measures restricting business operations and access to work in a developing country like Brazil can disproportionately impact low-income workers and small businesses. These groups often lack the financial capacity to sustain long closure periods without alternative income sources. Given their significant representation in the total workforce, such restrictions can undermine the effectiveness of these policies. In this sense, our results are illustrative since they show significant losses in the formal sector that represent almost half of the sales expected for the weeks after the measures come into force. However, the distribution of these losses was not equal. Essential services, such as pharmacies and supermarkets, were allowed to open despite the limitation of the attendance capacity. In contrast, other services and sectors were kept closed with zero revenue and fixed costs to cover. This situation drove many businesses, especially those without savings or access to credit, into immediate bankruptcy and led others to default after exhausting their financial reserves or credit capacity during the first wave of restrictive measures. Without economic compensation automatically linked to such restrictions, many businesses were left with no viable alternatives, forcing them to disregard the measures in subsequent waves of closures when new outbreaks occurred and additional shutdowns were applied.

4 CONCLUDING REMARKS

This study aimed to assess the impacts of intensifying social isolation on economic and health variables from a research design that allowed a causal inference. To this end, it used the natural experiment generated by State Decree No. 55,184 of April 15, 2020, which provided discretionary power to municipalities to adopt or abolish measures restricting economic activities, especially those related to retail with face-to-face customer service, to identify the effects of this treatment. It is assumed that there is a relationship between the adoption of these measures and the mobility of people (measured by the index of social isolation). It is assessed whether these measures have a causal relationship with the health variables (new cases, hospitalizations, and deaths) through an increase in the population susceptible to contamination by the virus and with the economic variable (sales) through supply and demand shocks. As far as we know, it is the first study that evaluates, even partially, the costs and health benefits of social isolation in Brazil with such a research design.

However, it is important to disclose that this study has several limitations. The proposed design does not allow, for instance, to assess each restrictive measure or its intensity in each municipality since there is no complete information about these measures at the municipality level in Brazil. Therefore, it does not allow us to infer the binary adoption of social isolation as a strategy to contain the spread of the SARS-CoV-2 since all the municipalities included in our sample were under the effects of several types of restrictive measures, such as the limitation of the attendance capacity in supermarkets, pharmacies, and restaurants, the prohibition of events and gatherings of any nature, public or private, including excursions, religious services and with face-to-face classes suspended in schools and universities.

A real threat to the validity of these results is the presence of (unobservable) externalities caused by people mobility who have moved between municipalities belonging to control and treatment groups. However, this is only a source of bias if there has been a change in residents' behavior. Otherwise, this mobility is captured by fixed effects. Furthermore, Goodman-Bacon and Marcus (2020) show that in the context of infectious diseases, the repercussions of travel typically lead to bias in estimates of the difference in differences to zero because the externality in one direction tends to annul the externality in the opposite direction.

However, it cannot be ruled out that closing retail services in one municipality leads its residents to buy in neighboring municipalities with relaxed restrictive measures. This would potentially generate a positive bias in sales (increasing differences between groups) and a negative bias in new cases, hospitalizations, and deaths (reducing the difference between groups) since, in theory, these displaced residents would be increasing their susceptibility to the virus having contact with other people in places (retail services) with potential for contamination. Yet there are reasons to believe this bias is small, if any. This is because the results shown in the robustness check showed no changes in the sales of intercity transport tickets, so this increase in people's mobility would have to have occurred by car. Moreover, it is worth remembering that the treatment evaluated in this study is the intensification of social isolation, so it is not plausible to assume that there was an increase in the mobility of residents between municipalities while a more significant proportion of residents of the treated municipalities stayed in their homes.

Another limitation is that the economic costs evaluated in this study involve only the losses in the formal sector of the economy that uses some electronic system to pay taxes. Therefore, the model does not allow for the estimation of losses, for example, in the informal sectors of the state economy, possibly even more affected by the restrictive measures. In addition, it does not allow evaluation of other economic costs imposed by the intensification of social isolation, such as job and income losses by workers, and health costs, such as physical and mental health problems caused by social isolation, which are not yet correctly recorded in the statistics. Moreover, the difference in differences models have a robust internal validity, but their external validity is limited. This means that while the causal effect is well identified, as suggested by the robustness checks in this study, the validity of these results for other sites or periods during the pandemic cannot be guaranteed. In this context, it would be essential to conduct similar studies for different periods and Brazilian states, especially in states with more cases and losses of lives caused by covid-19.

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APPENDIX A

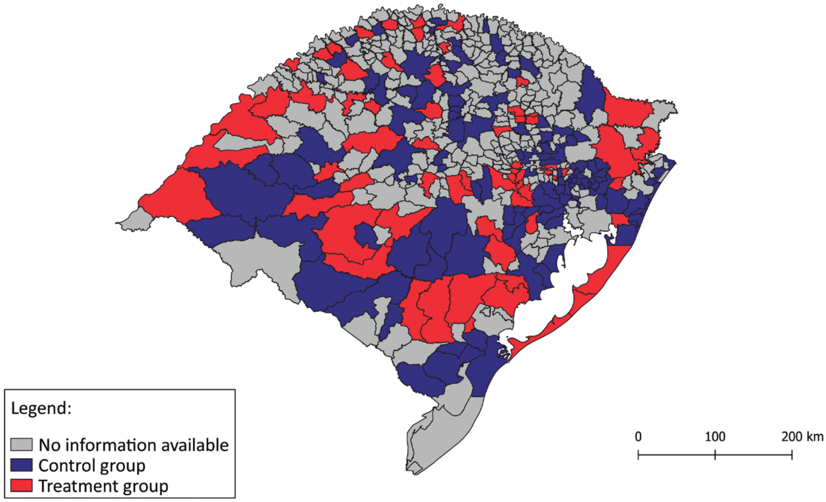
TABLE A.1
Descriptive statistics

Variables		Groups	
		Control	Treated
Sales (million BRL per day)	Mean	5,2496	2,5904
	Standard deviation	7,2459	2,7840
	N	708	402
Electronic passenger ticket (million BRL per day)	Mean	0,0004	0,0001
	Standard deviation	0,0032	0,0010
	N	708	402
Electronic cargo transportation invoice (million BRL per day)	Mean	0,1772	0,1150
	Standard deviation	0,3745	0,2036
	N	708	402
Cases of covid-19 (By week)	Mean	4,0105	3.4421
	Standard deviation	12,282	14.994
	N	944	536
	Mean	0.5741	0,4813
Hospitalizations by covid-19	Standard deviation	2.0437	1.7666
(By week)	N	944	536
Deaths by covid-19 (By week)	Mean	0,0815	0,0858
	Standard deviation	0,3577	0,4815
	N	944	536
Social isolation index (Weekly daily average)	Mean	0,4558	0,4370
	Standard deviation	0,0619	0,0579
	N	944	536
Number of municipalities	-	118	67

Authors’ elaboration.

FIGURE A.1

Rio Grande do Sul map with municipalities included and not included in the sample



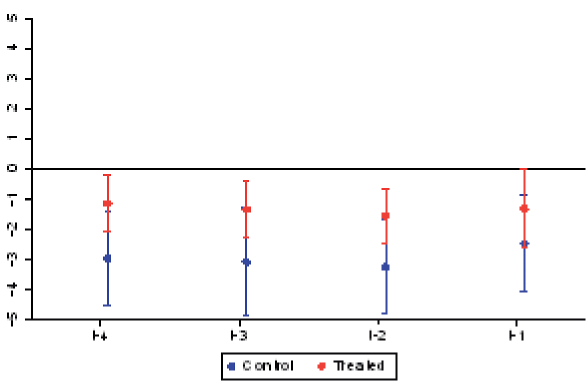
Authors' elaboration.

Obs.: Figure whose layout and texts could not be formatted due to the technical characteristics of the original files (Publisher's note).

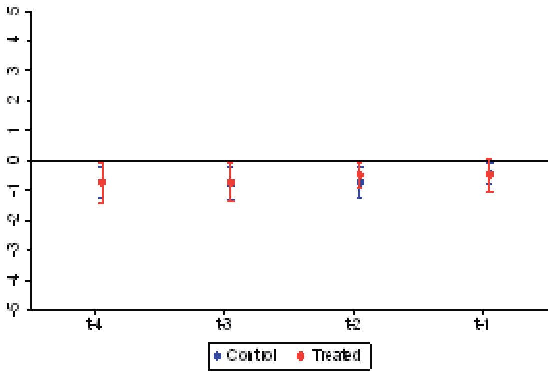
FIGURE A.2

Further evaluation of parallel trends

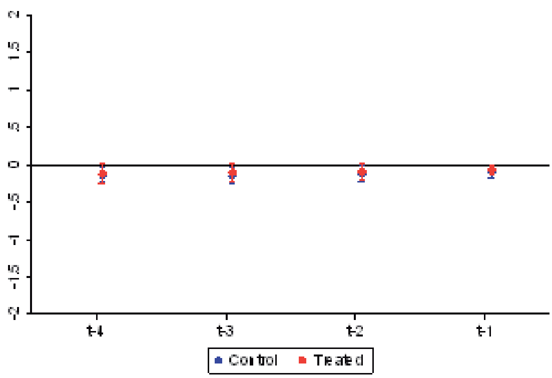
A.2A – Cases



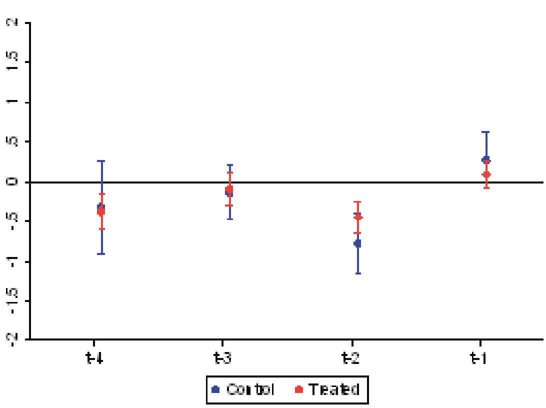
A.2B – Hospitalizations



A.2C – Deaths



A.2D – Sales



Authors' elaboration.

Obs.: 1. Entries are the estimated coefficients for the trend of the municipalities belonging to the control group and treated before treatment estimated by Ordinary Least Squares.

2. All models have control for fixed effects by the municipality.

3. Confidence intervals are constructed from standard errors clustered by municipalities.

4. Figure whose layout and texts could not be formatted due to the technical characteristics of the original files (Publisher's note).

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