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Economic Costs of Distancing Policy Interventions*

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Abstract

Distancing policy interventions (DPIs) were aimed at containing the COVID-19 pandemic, but they also likely affected economic activity. This paper estimates the effects of DPIs on selected indicators of monthly economic activity, such as industrial and manufacturing production, construction output, retail trade, inflation, and unemployment. The main contribution of this paper is the isolation of the causal effects of distancing interventions from the effects of voluntary distancing. I use mobility data as a measure of distancing to identify DPI effects on mobility in a regression discontinuity design, specifically as immediate changes in distancing right after the intervention. This strategy identifies the unobserved voluntary component of distancing as well, which is a key control variable in the identification of the economic effects of DPIs. I find significant output losses due to DPIs, but no evidence for inflationary or unemployment effects. Results also show that although voluntary distancing caused significant output losses, their effect was an order of magnitude smaller than that of DPIs.

JEL: C3, C33, C43, C54, E23, E24, E65, H1, H12, H3, H84, I1, I12, I18

Keywords: COVID-19, non-pharmaceutical interventions, causal identification, regression-discontinuity-in-time

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1 Introduction

When a new virus bursts into an epidemic and no vaccines are available, the primary containment strategy is distancing policy interventions, or DPIs in short. COVID-19 was no exception. DPIs limit social interactions in order to prevent virus transmission, but they are also likely to impose high costs on economic activity. This paper quantifies the economic costs in terms of sector output losses, inflationary effects, and unemployment responses caused by DPIs during the COVID-19 pandemic on a country-level weekly panel dataset.

Distancing happens not only in compliance with DPIs but also as a voluntary response to threatening news about the new virus. The disentanglement of the effects of policy-induced and voluntary distancing behaviors is the main contribution of this paper as the main empirical challenge is that these two effects are most likely confounded. I tackle this challenge by observing that, although there is an overall downward trend in social interactions starting after COVID hits a country, right after when a DPI is introduced, social interactions display a sudden and substantial drop. I leverage this stark drop in social behaviors as likely responses to DPIs to separate the policy-induced and voluntary distancing components of overall distancing behaviors. This separation is accomplished through the use of a regression discontinuity in time design, which identifies the policy-induced component as the discontinuity and the voluntary component as the residual. Once I have this voluntary distancing component, I use it as a control in the main estimation of DPI effects of economic activity. These effects are identified from the change in economic activity after DPI interventions, compared to their changes from the same months of five pre-COVID years holding other correlated factors, most importantly voluntary distancing, fixed. Economic activity is measured by seven selected economic outcomes focusing on output, inflation, and unemployment. This empirical strategy has been developed in an earlier paper by the author: Rácz (2023).

The next section of the paper describes data sources and their transformation into two estimation samples. This study uses three main data sources: (i) one for economic outcomes, (ii) another for distancing policy interventions and other COVID-related interventions, (iii) and a third one for social mobility proxying social distancing behaviors. This third data source is Google's COVID-19 Aggregated Mobility Research Dataset.[1] The estimation samples cover 44 countries. The first sample used in the identification of voluntary distancing patterns is a weekly-country level panel dataset spanning every week between November 2019 and December 2020. The second sample used in the identification of the economic effects of DPIs covers every month between November 2015 and October 2020. The second sample contains seven selected economic indicators, such as industrial and manufacturing production, construction output, retail trade, consumer prices, producer prices in manufacturing, and the unemployment rate. Intuitively, service sectors, such as personal services, accommodation, food and drink services, or the entertainment sectors, must have suffered the most losses under DPI restrictions. These sectors are omitted because of a lack of data availability in the current version of this paper. The omission of these specific service sectors

[1] For details, see Section A.1 of the Appendix.

is a clear limitation of the paper, and it suggests the underestimation of the output effects of DPIs.

I define three different DPI indicators using data from Hale et al. (2020): DPI treatment, DPI intensity, and DPI extensity. The DPI treatment captures the average level of the first DPIs within each country. Treatment is held constant until the end of the sample, as if these interventions were kept on all along, even if they were not. This way, DPI treatment resembles a more conventional treatment, making it easier to interpret its effects. Later changes in DPIs are absorbed into two other indicators: DPI extensity and intensity. Because of how DPI treatment is designed, these two indicators capture deviations of DPIs relative to the original interventions. DPI intensity measures deviations in stringency levels, DPI extensity in DPI types, such as school closures or stay-at-home orders.

In Section 3, I present the empirical strategy. I carry out my estimation in a two-stage design. The first stage is the separation of the voluntary and policy-compliant components of social mobility. The second stage is the estimation of the economic effects of DPIs. The first stage identifies DPI effects on a social mobility indicator calculated from mobility data from carry-on devices. It is carried out in a regression-discontinuity-in-time design. The main identifying assumption is that DPI effects must appear suddenly as they impose restrictions on the whole society from one day to the other, while voluntary distancing effects are realized much slower as voluntary decisions are heterogeneous within a society. Patterns observed in raw mobility data give visual support for the discontinuity design. I define a voluntary mobility indicator by residualizing social mobility to first-stage predicted DPI effects. The second stage estimates the economic effect of DPIs, controlling for the first stage's predicted voluntary mobility, other COVID-related policy interventions, and other factors. The second stage design includes controls for distancing patterns at trading partners, anticipating the possibility of international spillovers of economic effects.

Section 4 presents the main results. It starts with first stage results, then it describes the identification of the voluntary mobility indicator. Second stage results about the economic effects of DPIs are presented after that. Second stage results are presented in three separate subsections for output losses, inflationary costs, and unemployment effects.

I find that distancing behaviors that were either voluntary or DPI-compliant generated substantial output losses. I found significant output losses due to DPIs, but no evidence for inflationary and unemployment effects. Findings suggest that DPIs caused substantial output losses. Results also show that although voluntary distancing caused significant losses to sector outputs, its effect was an order of magnitude smaller than that of DPIs. Only 70% of total losses in industry and manufacturing are explained by either voluntary or DPI-induced distancing, implying that other factors, such as other COVID-related interventions contributed substantially to output losses in these sectors. In construction and retail trade, on the other hand, distancing factors altogether predicted more losses than was observed. This finding suggests that other factors, such as fiscal and monetary support programs, could mitigate the short-term costs of distancing in these two sectors.

I find no evidence of significant economic costs resulting from the introduction of new types of DPIs, or from mobility spillovers from trade partners.[2] Changes in the intensity of distancing interventions, such as decreasing the limit for allowed gathering sizes, were found to increase the output costs of the first interventions.

These findings provide evidence that, while DPIs were implemented to contain COVID infections, they imposed substantial costs on economic activity in terms of output loss in industrial production and retail trade. Voluntary distancing induced an order of magnitude lower output losses than DPIs did. Inflationary and unemployment effects were not detected. These results provide both qualitative and quantitative guidance for governments to consider when implementing distancing interventions in times of an epidemic. These findings also contribute to a more complete cost-benefit analysis of distancing policy interventions on the cost side.

Literature

This paper belongs to the empirical evaluation of non-pharmaceutical interventions (NPI) during the COVID-19 pandemic, surveyed exhaustively by Perra (2021). Within this literature, this paper is a contribution to the assessment of the economic costs of NPIs. There are papers that provide correlative evidence between such interventions and economic outcomes. Chen et al. (2020) find that European countries and U.S. states that experienced larger outbreaks also suffered larger economic losses. They find no evidence of NPIs making significant contributions to these losses. Carvalho et al. (2020) consider billions of transactions from Spanish card data and find strong consumption responses to business closures, but smaller effects for capacity restrictions; a steeper decline in spending in rich neighborhoods. Arnon et al. (2020) find that NPIs explain nearly 15 percent of the decline in employment around 3 million jobs over the first three months of the pandemic. Bodenstein et al. (2021) finds that distancing being it either voluntary or policy-compliant had significant economic effects. The main contribution of this study relative to these papers is the identification of causal effects of NPIs.

There are studies that identify causal effects of NPIs similarly to the aim of this paper. There are papers that find strong voluntary effects. Deb et al. (2021) find that containment measures had a significant impact on economic activity for example. Their findings suggest that industrial production losses were around 10% in the 30 days following their implementation, which is very close to the findings of this paper. Berry et al. (2021) find minor but negative economic effects of NPIs. They also stress the importance of voluntary distancing behaviors, when they claim that "Many people had already changed their behaviors before the introduction of shelter-in-place orders." The contribution of this study relative to these papers is the separation of NPI-induced and independent voluntary distancing effects.

An earlier study of the author of this paper is Rácz (2023). In this paper I employ the empirical strategy used in this paper as well to estimate the causal effects of distancing policy interventions on the

[2]Except in construction.

effective reproduction number of COVID-19.

Goolsbee and Syverson (2021) Compare "consumer behavior over the crisis within the same commuting zones but across state and county boundaries with different policy regimes." They find that NPIs account for only a modest share of the documented consumption decline. This comparison, however identify the effect of NPIs decoupled from NPI-induced voluntary effects, which this study considers as relevant consequences of NPIs. Kong and Prinz (2020) find no evidence of unemployment effects of NPIs similarly to this study, but in a sample of US states.

Bodenstein et al. (2021), and Goolsbee and Syverson (2021) stress the importance of voluntary distancing in US states. This study finds that voluntary distancing effects were less important in a global sample. This comparative assessment of the role of voluntary distancing effects is supported by Maloney and Taskin (2020), who use mobility data to identify the effects of NPIs.

This study takes into account international spillovers of distancing effects as a confounding factor of NPIs and economic outcomes. There are studies that document such spillovers. For example, Boranova et al. (2022) provide evidence of international spillovers of distancing effects on car manufacturers. Barrot et al. (2021) find GDP responses to distancing through supply chains.

2 Data

There are three main sets of variables that are used in this study: (i) economic outcomes, (ii) distancing policy interventions and other COVID-related interventions, and (iii) social mobility. These sets of variables are derived from their own three different data sources. These three sets of variables are presented in more detail in the following subsections of this section. Besides these datasets, I also use two more auxiliary data sources. First, I used international trade data from the OECD in 2019 to calculate export and import shares by country pairs. Second, I calculate country-week level averages of various weather indicators using data from the National Oceanic and Atmospheric Administration (NOAA). I merge all this data into two estimation samples: a weekly frequency country-level panel used in the identification of DPIs on social mobility and a monthly frequency country-level panel used in the estimations of the economic effects of DPIs. This section describes data sources and how they are transformed into estimation samples.

The estimation samples cover the following 44 economies:

1. Argentina
2. Australia
3. Austria
4. Belgium
5. Brazil
6. Canada
7. Chile
8. China
9. Colombia
10. Costa Rica
11. Czechia
12. Denmark
13. Estonia
14. Finland
15. France
16. Germany
17. Greece
18. Hungary
19. Indonesia
20. India
21. Ireland
22. Israel
23. Italy
24. Japan

25. Lithuania
26. Luxembourg
27. Latvia
28. Mexico
29. Netherlands
30. Norway
31. Poland
32. Portugal
33. Russia
34. Saudi Arabia
35. Slovakia
36. Slovenia
37. South Africa
38. South Korea
39. Spain
40. Sweden
41. Switzerland
42. Turkey
43. UK
44. USA

The second-stage sample spans every month between November 2015 and October 2020, while the first-stage sample starts from the first week of November 2019 and covers every week until December 2020.

2.1 Economic Outcomes

I estimate the effects of distancing policy interventions on the following seven monthly economic indicators:

- industrial production,
- manufacturing production,
- construction output,
- retail trade,
- consumer price index (CPI),
- producer price index (PPI) in manufacturing, and
- unemployment rate.

The first four of these are measuring the output of different sectors: industrial, and manufacturing production, construction output, and retail trade. Using these as outcome variables in the main estimation addresses the question of output losses due to distancing policy interventions. Intuitively, service sectors, such as personal services, accommodation, food and drink services, or the entertainment sectors, must have suffered the most losses under DPI restrictions. These sectors are omitted because of a lack of data availability in the current version of this paper. The omission of these specific service sectors is a clear limitation of the paper, and it suggests the underestimation of the output effects of DPIs.

The next two outcome variables are price indexes addressing inflationary costs resulting from DPIs. Finally, the unemployment rate indirectly addresses job losses as a result of DPIs.

I normalize all seven indicators by their values from the latest November in order to get numbers that are comparable across years.[3] Figure 1 shows the seven economic outcomes in each country and each month around the first DPI by circle marks. X marks show their cross-country means, highlighting general tendencies. These observations are contrasted with a 5-year benchmark from the pre-COVID years of the same indicator, indicated by a thick orange line. Observations stay close to this benchmark before the first intervention in all seven graphs supporting the choice.

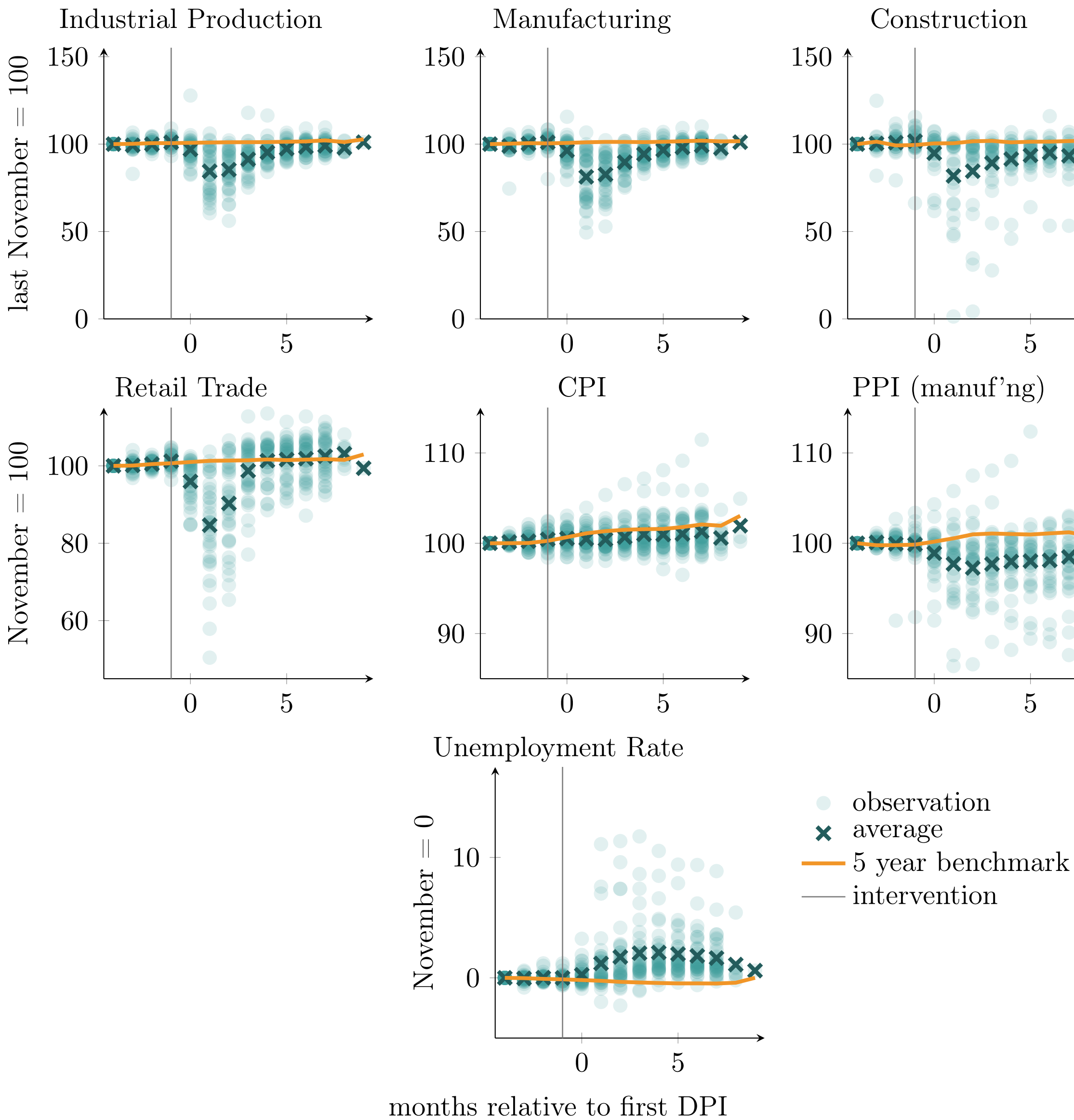
The figures also reveal that all four sector output indicators declined substantially from their benchmarks after the first DPI in almost all countries. In the first month following the first intervention, the

[3] Original data is seasonally adjusted for all indicators, and is fixed price volume indices in the case of sector outputs.

average decline is around 20%, and it is consistent across all four output indicators.Even though this decline was temporary, as all four indicators converge to their benchmarks after roughly 6 months, they do not rise above them, meaning that this decline represents a permanent output loss in these sectors. The main scope of this paper is to quantify what share of this output loss can be causally linked to distancing policy interventions.

The two price variables show a much greater heterogeneity across countries in their price responses to DPIs compared to sector outputs. This observation is not too surprising as distancing disrupts both supply and demand, which have opposite effects on prices. Therefore, the overall effect of DPIs on prices can have varying signs across countries depending on the relative strength of demand and supply disruptions. Although there is this considerable heterogeneity across countries, both price indicators show a slight negative and permanent deviation from their benchmarks, which is a little more pronounced in manufacturing PPI than in CPI. This suggests that distancing was relatively more demand-disruptive, especially for products of the manufacturing industries.

Figure 1: Seven Economic Indicators around DPIs



Notes: cloud: country-month observations around first DPI, darker regions show overlapping observations. X marks: cross country averages around first DPI. Thick orange line: averages of 5 pre-COVID years. Thin line: last month before first intervention.

Unemployment first increased after the first DPI in general, but it converged back to its benchmark levels 9 months later. Unemployment rates were on average 2 percentage points higher than their 5-year benchmarks throughout months 3-6, suggesting a substantial loss of jobs after DPIs were introduced. The question is, how much of this excess unemployment can be attributed directly to DPIs?

2.2 Distancing Policy Interventions (DPIs)

The primary data source for distancing policy interventions and other COVID related interventions is Hale et al. (2020). It is a constantly updated dataset covering almost every country in the world. It reports several different COVID related interventions on daily frequencies.

Distancing policy interventions, abbreviated as DPIs, are the main focus of this study. A DPI of type j is reported as a categorical variable $D_{it}^{j} \in \{0, 1, \ldots k_j\}$, such that 0 signals no intervention and greater integers signal more and more stringent interventions, k_j being the most stringent type j intervention possible. One example is school closures, for which value 1 codes a recommendation, 2 a partial mandate, and 3 a mandate for all levels of education.[4]

I observe seven different DPIs:

- school closures,
- workplace closures,
- gathering limits,
- stay-at-home orders,
- within country travel restrictions, and
- cancellation of public events.

Considering the small sample size plus the fact that most countries in the sample started to intervene in the same month, in March 2020, it is very unlikely that the effects of these seven different interventions can be identified separately. Therefore, I first calculate their sum as: $\overline{D}_{it} = \sum_j D_{it}^{j}$. This variable takes the value of 3, for example, if there was a level 1 school closure and a level 2 gathering limit in place in country i during the entire week t. It can also be non-integer, when a the number or the stringency of DPIs changes within a week. For example, if the level 1 school closure and the level 2 gathering limit was introduced as a first ever intervention on a Wednesday, that would aggregate to a value of $\overline{D}_{it} = 3 \times 5/7 = 2.143$, because the aggregate value of 3 was only in place 5 days in that week.[5]

I decompose $\overline{D}_{it}$ into three distinct components, each of which is defined to be disjoint. The first component I name as the *treatment* (T_{it}). It captures the first DPIs within a country, with its magnitude remaining constant throughout the sample. T_{it} is defined as:

$$T_{it} = \begin{cases} \overline{D}_{it} = 0 & \text{if } t \leq 1 \\ \overline{D}_{it}\big|_{t=1} & \text{if } t > 1 \end{cases}, \tag{1}$$

that means treatment is 0 before the first ever DPI, which happens by construction. It then takes and keeps the retains of $\overline{D}_{it}$ of week 1 from the first ever intervention throughout the sample. On week 0, T_{it} takes a value between 0 and the week 1 value of $\overline{D}_{it}$ depending on the number of days the first ever DPIs were in place on week 0. For example, if the first ever DPIs were introduced on a Monday, the week 0 and week 1 values are the same, but if they were introduced on Friday of week 0, it takes only 3/7 of its week 1 value given it was in place for only 3/7 of the week.

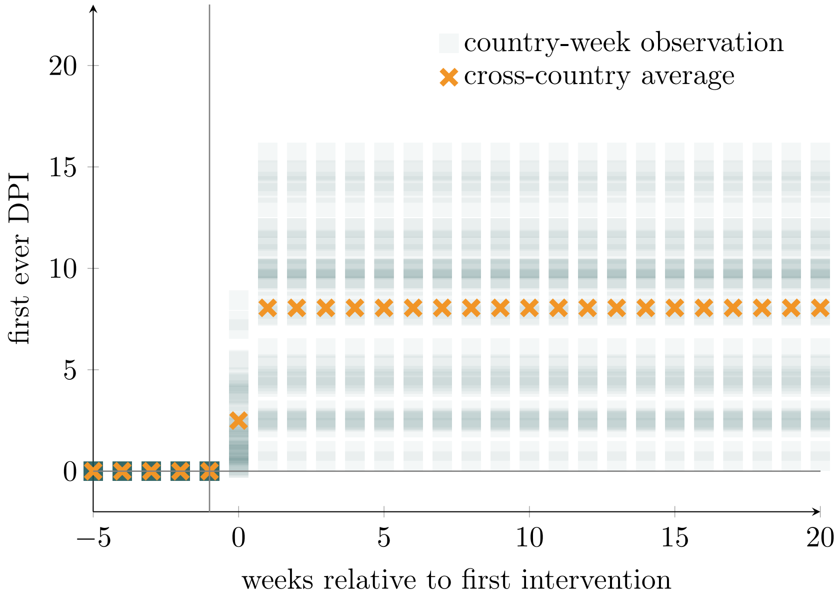
Figure 2 shows the evolution of the treatment (T_{it}) components on weekly frequencies. Squares indicate country-week observations, such that darker regions show overlapping observations. It shows a

[4] They also report a binary indicator for each DPI that indicates if a policy was countrywide or only local. In all my calculations presented here, I deduce 0.5 from a DPI categorical variable if it was only local, meaning a level 3 school closure gets a value of 2.5 if it was only regional.

[5] And the sum of all DPIs was 0 in the first two days of that week.

substantial heterogeneity across countries in the magnitude of their first interventions. Crosses indicate cross country averages highlighting the general pattern across countries, which is around 8, revealing that many countries introduced their first DPIs in bundles and started some on higher than level 1. This figure also confirms the concept of this component as it is defined to be fixed throughout the sample after week 1. Any further alterations in the number or the level of restrictions are absorbed in the other two components.

Figure 2: Treatment: T_{it}



Squares: country-week observations, darker regions show overlapping observations. +: within week averages. vertical line: shows last week before first intervention.

The two other components I define are referred to as *extensity* (E_{it}) and *intensity* (I_{it}). Extensity captures any further changes in the number, intensity in the level of DPIs after the first intervention. For the formal definition of E_{it} and I_{it} the definition of following two numbers is helpful:

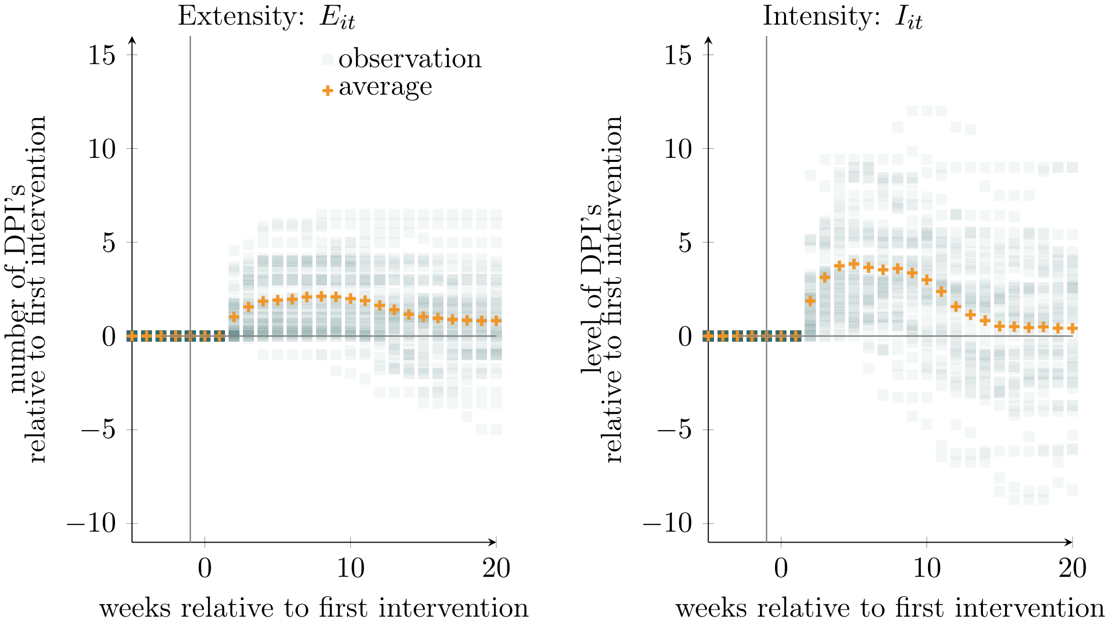
$$N_{it} = \sum_j \mathrm{I}\left(D_{it}^j > 0\right) \qquad\qquad L_{it} = \overline{D}_{it} - N_{it},$$

where $\mathrm{I}(\ldots)$ is an indicator function. N_{it} is the sum of the number of DPIs, while L_{it} sums the level of DPIs above 1. For example if there were only a level 3 school closure and a level 2 gathering limit in a country i on week t, $N_{it} = 2$, because there are only 2 types of DPIs in place, and $L_{it} = 3$, because these DPIs are 3 levels above level 1 in total. E_{it} and I_{it} are formally defined as:

$$E_{it} = \begin{cases} N_{it} - N_{it}|_{t=1} & \text{if } t > 1 \\ 0 & \text{otherwise} \end{cases} \qquad I_{it} = \begin{cases} L_{it} - L_{it}|_{t=1} & \text{if } t > 1 \\ 0 & \text{otherwise} \end{cases} \tag{2}$$

This way $\overline{D}_{it} = T_{it} + E_{it} + I_{it}$, that is they are disjoint and contain all the information coded in D^j_{it}'s.

Figure 3: Treatment Dynamics: Extensity and Intensity of DPIs



Squares: country-week observations, darker regions show overlapping observations. +: within week averages. vertical line: shows last week before first intervention.

The two panels of Figure 3 show the evolution of E_{it} and I_{it}. Squares indicate country-week observations spreading out considerably in both figures showing substantial heterogeneity across countries in both the extensity and intensity of DPIs. Crosses show cross country averages highlighting the general pattern, which is growing in the first couple of weeks and starts to decline between weeks 10 and 20. This pattern shows that after the first interventions countries tended to increase both the number and the level of DPIs in the first couple of weeks and started slacken up restrictions only after 10 weeks.

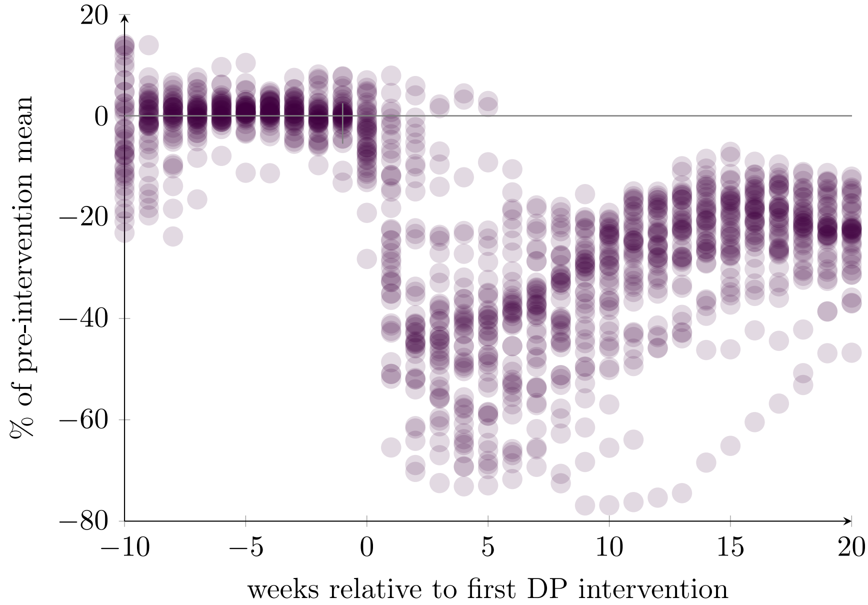
Frequency conversion. Interventions are all reported on daily frequencies, which I convert to weekly and monthly frequencies in my estimation samples. I take the weekly (monthly) averages for all of these variables at weekly (monthly) conversions. In case of a categorical variables this means if that categorical variable switches from category 0 to 1 on a Wednesday of a given week, the weekly conversion of that variable is 5/7=0.714 for that week. Conversion of within month changes happen the same way. I can do that, because categorical variables are ordinal, meaning that if for example an intervention switches from a value of 1 to 2, that means that intervention becomes more restrictive.

2.3 Social Mobility Index

The main contribution of this paper is the identification of the causal effects of DPIs on distancing in isolation from voluntary distancing effects. To track distancing patterns, I create a weekly index of social mobility (m_{it}) using Google's COVID-19 Aggregated Mobility Research Dataset. This dataset provides anonymized records of weekly flows of Google users[6] between NUTS3 areas. This data is available from the first week of November 2019, for every consecutive week until today. Any further details regarding this dataset can be found in the Appendix.

I calculate the social mobility index, m_{it} in two steps. First, I take only inflows into NUTS3 areas and normalize them by their average values for a 4-week period between November 3 and November 30, 2019, because this is the first available month long period, which is as far from the time of the COVID pandemic as possible. This gives m_{it} a unit of percentage deviation from November 2019, similarly to economic outcomes. In the second step, I aggregate these normalized NUTS3 level inflows country level by taking their arithmetic mean within a country-week cell. Based on this definition less social mobility (lower m_{it}) means more distancing.

Figure 4: Social Mobility Index: m_{it}



Squares: country-week observations, darker regions show overlapping observations. +: within week averages. vertical line: shows last week before first intervention.

Figure 4 shows the social mobility index m_{it} around the time of the first DPI. One circle is a single country-week observation, starker regions show overlapping observations. For this figure, I normalized m_{it} by its pre-intervention mean within each country. m_{it} fell by between 10 to 80 percent relative to the pre-intervention period within 2 weeks after the first DPI in almost every country according to the figure. This sharp decline in social mobility right after the first DPI gives a rationale for a discontinuity design in the identification of DPI induced distancing, which is elaborated in the next Section.

[6] Only of users who have turned on the Location History setting, which is off by default. This is similar to the data used to show how busy certain types of places are in Google Maps – helping identify when a local business tends to be the most crowded.

3 Empirical Strategy

The main scope of this paper is to identify the causal effects of distancing policy interventions on selected economic outcomes. An empirical strategy that leads to the identification of these effects is presented in this section. The primary empirical challenge is telling apart the economic effects of DPI-induced and voluntary distancing effects. I use a two-stage empirical strategy in which the first stage identifies voluntary distancing effects by separating social mobility m_{it} into a voluntary and a policy induced component. I use the voluntary mobility component in the second stage as a control to be able to identify the causal effects of DPIs in isolation from voluntary distancing effects. I start with the discussion of the main identification strategy in the second stage. I then elaborate on the identification of voluntary distancing effects on social mobility in the first stage. The empirical strategy outlined in this section has been developed in an earlier paper of the author: Rácz (2023).

3.1 Economic Effects of DPIs

The identification of the economic effects of DPIs is based on a difference-in-differences approach. They are identified as changes in economic outcomes after the implementation of the first DPI compared to a COVID-free control period and holding other confounding factors fixed.[7] This control period is chosen to be the five years that preceded the COVID pandemic: 2015-2019, such that observations are matched by month. This strategy can be formalized by the following equation:

$$\widetilde{\Delta} y_{it} = \underbrace{\beta^T T_{it} + \beta^E E_{it} + \beta^I I_{it}}_{\text{DPI effects}} + \xi' X_{it} + \varepsilon_{it}, \tag{3}$$

where y_{it} is an economic outcome, and $\widetilde{\Delta}$ indicates difference from control period values. T_{it}, E_{it}, and I_{it} are capturing the first DPI intervention and further changes in the extensity and intensity of DPIs.[8]

X_{it} is a set of covariates that includes all relevant confounders that must be held constant in order to identify the effect of DPIs (*beta*). One way to find these confounders is to map out all the relevant causal links connecting distancing policy interventions to the economy.[9] Figure 5 shows the causality map of this paper. Each arrow represents a causal link, with thick arrows emphasizing the link to be identified. Solid lines indicate observed links; dashed lines indicate unobserved links.

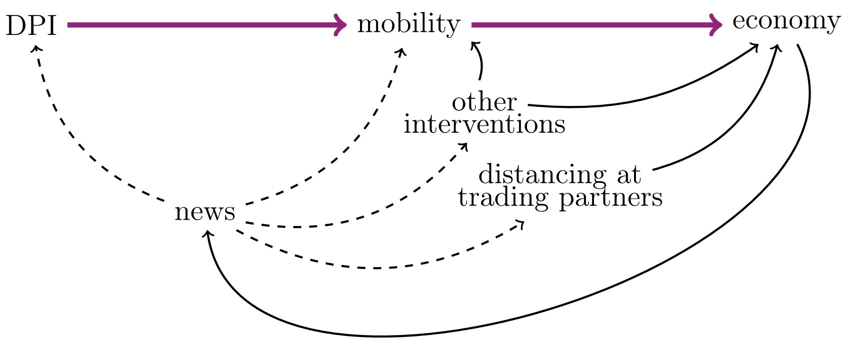
The causality shows that DPIs only indirectly affect the economy through the reduction of social mobility, which has a potentially disruptive effect both on aggregate demand and supply. The effects of DPIs are conveyed by two channels: policy compliant and policy-induced voluntary distancing. People might increase their distancing after the implementation of a restriction because of compliance, but might

[7]The validity of this choice is supported by Figure 1 as it has been discussed in Section 2.1. The main observation there was that treatment period observations concentrate in the close neighborhood of the average of the control period values before the first DPI treatment.

[8]For details see Section 2.2.

[9]This technique is referred to as the DAG method by Cunningham (2021).

Figure 5: Causality Map



Notes: Arrows point in the direction of causality. Thick arrows: the path to be identified Solid line: observed, dashed: unobserved effect.

also because they perceive it as a signal of a worsening epidemic. The primary motivation for this paper is to inform policymakers about the total effect of DPIs. These are realized through both of these channels, I do not aim to identify them separately, therefore, in this paper.

This map reveals three other paths through which DPIs and economic activities are also connected. First, DPIs are confounded with social mobility by news, which is a set containing any bits of information about COVID-19 that has the potential to alter government and individual distancing decisions simultaneously.[10] For example, the discovery of a large number of COVID infections raises the probability of a DPI and is also likely to discourage people from social activities. Throughout this paper, I am going to refer to this discouragement effect as *voluntary distancing*.

Second, governments implemented other COVID-related interventions that likely affected both social and economic activities. For example, income support programs aimed to prevent mass layoffs, but they might as well have encouraged people to stay home when they had COVID symptoms, decreasing social activities. Third, economies are interconnected through international trade links. Distancing in a country thus not only impacts domestic markets but can also have an influence on trading partners. For example, declining supplies increase import prices. Finally, economic shocks might also have contributed to news. For example, a negative shock to an economy could deter the government from the most stringent DPIs.

The easiest way to eliminate the effects of these alternative paths would be to control for news and other interventions. Holding them fixed would identify the economic effect of DPIs. Unfortunately, this strategy is not feasible as news contains unobservable components. For example news about the risk of COVID is not observable in my data sources.

[10] The arrow connecting news to DPI and other interventions acknowledges the fact of endogenous selection of the treatment of this study: DPIs. By closing all backdoor paths that contain this link, I simultaneously eliminate this selection bias.

To overcome this difficulty, I instead construct a two-stage empirical design in which I estimate voluntary distancing in the first stage because it is unobserved. I do that by taking the mobility indicator m_{it}, and separating it into a voluntary and a policy-induced component using a regression discontinuity design.[11] For more details, see Section 3.2. I then estimate the economic effects of DPIs in the second stage, in which I control for other interventions, distancing at trading partners, and voluntary distancing. This design eliminates all alternative paths, including the reverse causality path of economic outcomes, because it is contained by the other three channels through news based on the causality map in Figure 5. I control for voluntary distancing by voluntary mobility predicted by the first stage. I measure distancing at trading partners by averaging social mobility index m_{it} using export and import shares.

Rácz (2023)

3.1.1 Second Stage: Empirical Design

This strategy is formulated by the following equation:

$$\begin{aligned} \widetilde{\Delta} y_{it} = & \underbrace{\beta^T T_{it} + \beta^E E_{it} + \beta^I I_{it}}_{\text{DPI effects}} + \underbrace{\beta^V \widehat{m}_{it}^V}_{\text{voluntary mobility}} + \underbrace{\eta' P_{it}^O}_{\text{other interventions}} \\ & + \underbrace{\lambda_X \sum\nolimits_j w_{ij}^X m_{j,t-1} + \lambda_M \sum\nolimits_j w_{ij}^M m_{k,t-1}}_{\text{distancing at trading partners}} + \underbrace{\xi' X_{it} + \text{FE}_i}_{\text{covariates and FEs}} + \varepsilon_{it}, \end{aligned} \tag{4}$$

where i is a country, and t is a month. T_{it}, E_{it}, and I_{it} are capturing the first DPI intervention and further changes in the extensity and intensity of DPIs. Part A. of Table 1 shows descriptive statistics of these indicators. $\widehat{m}_{it}^V$ is predicted voluntary mobility resulting from the first stage estimation.

P_{it}^O is a set of other COVID interventions, such as COVID-related fiscal spending, investment in vaccines and healthcare, income support programs, debt relief programs, international travel controls, and public information campaigns.[12] Part B. of Table 1 shows descriptive statistics of these interventions.

Information on fiscal and monetary policy interventions that are not directly COVID-related, such as tax or interest rate cuts, is not included among controls. The omission of such controls is a clear limitation of the current version of this paper, because such conventional policy steps were likely to be used to mitigate inflationary and unemployment effects in many countries. Moreover, governments and central banks anticipating higher unemployment or inflationary risks were likely to intervene more strongly. This presumed correlation between conventional policy interventions and outcomes is more likely to be absorbed by COVID related policy interventions that are controlled for, but has the potential to cause omitted variable bias in the coefficients of DPI interventions.

$w_i^X j$and $w_i^M j$are export and import shares from 2019 between countries i and j, which sum to 1 across partner countries denoted by j. The terms with summations therefore measure the average changes in

[11] m_{it} is derived from Google user mobility data. For details, see Section 2.3.
[12] Source: Hale et al. (2020).

mobility at trading partners, capturing the effects of distancing at trading partners. Part D of Table 1 contains descriptive statistics for average social mobility at export and import partners.

Table 1: Descriptive Statistics, Second Stage

Covariate	mean	st.dev.	max.	min.	unit
A. Distancing Policy Interventions (DPIs)					
Treatment	11.19	5.14	18.00	0.00	no. + lvl of DPIs
Extensity	-0.56	1.29	2.97	-4.52	no. of DPIs
Intensity	-1.71	3.12	7.18	-9.00	lvl of DPIs
B. Mobility at Trading Partners					
at Export Partners	19.50	24.41	99.98	2.87	Nov '19=100
at Import Partners	25.20	22.48	100.00	4.02	Nov '19=100
C. Other COVID Interventions					
Fiscal Spending	18.65	138.22	2151.20	0.00	billion USD
Investment in Vaccines	0.06	0.34	4.02	0.00	billion USD
Healthcare Investment	1.87	19.36	306.56	0.00	billion USD
Income Support	1.36	0.78	2.00	0.00	categorical
Debt Relief	1.15	0.79	2.00	0.00	categorical
Internat'l Travel Restr's	2.61	1.15	4.00	0.00	categorical
Information Campaigns	1.86	0.47	2.00	0.00	categorical
D. Other Covariates					
Covid Cases	178.60	335.85	2820.71	0.00	per 10^5 citizen
Covid Deaths	3.94	6.90	55.39	0.00	per 10^5 citizen
Covid Cases at Neighbors	4.64	6.70	40.80	0.00	per 10^5 citizen
Covid Deaths at Neighbors	0.13	0.19	1.21	0.00	per 10^5 citizen

Notes: 288 country-month observations of 32 countries.

X_{it} contain reported COVID cases and related deaths in population shares both domestic and from neighboring countries. These are included to address the direct and spill-over effects of COVID infections on economic activity. Part D of Table 1 presents descriptive statistics of these covariates. Finally country fixed effects are included to absorb the effects of time invariant differences among countries, such as levels of economic development, degree of openness or demographics, that are probably correlated with both government decisions on DPIs and changes in economic outcomes.

3.2 Voluntary Distancing

The first stage estimation identifies the policy-compliant component m_{it}^V of social mobility m_{it} in a regression discontinuity in time (RDiT) design.[13] The voluntary component, called voluntary mobility, is then defined as the residual of the first stage regression.

The DPI induced component of mobility is identified as sudden changes in m_{it} after the first DPI. The identifying assumption is that changes in social mobility due to voluntary distancing are slow, while the response to a distancing intervention is quick, at weekly frequencies. Distancing interventions prescribe a coordinated and sudden reduction in social activities after an intervention. Similarly, coordinated and sudden voluntary responses could only happen if the risk assessment of COVID news were homogeneous within countries. There is anecdotal evidence to assume that nations are much more heterogeneous in this respect, considering the simultaneous presence of virus skeptics and overly cautious people in many countries. It is more likely, therefore, that aggregate voluntary mobility responses are smooth and gradual because different fractions of society respond with different time lags and with different intensities based on their different risk assessments of the news.[14]

3.3 First Stage: Empirical Design

Based on these assumptions, social mobility m_{it} is modeled by the following equation:

$$m_{it} = \delta_t + \gamma^E E_{it} + \gamma^I I_{it} + \theta P_{it}^O + \zeta' Z_{it} + \mathrm{FE}_i + \nu_{it} \tag{5}$$

where δ_t is an event-time coefficient indicating week t after the first DPI was implemented in each country i. δ_t is included to capture the common trend in social mobility around the weeks of a type p intervention. Because they are intended to capture the effects of the intervention relative to the previous week, δ_{-1} is omitted, as δ_0^p represents week 0 of the first ever DPIs. The treatment effects of DPIs are identified by δ_0 and δ_1, because of the main identifying assumption that the first DPIs impact mobility suddenly after their implementation. The rest of the event time dummies are, therefore, assumed to absorb the common trend in voluntary mobility changes in earlier and later weeks relative to the treatment weeks.

E_{it}, and I_{it} are capturing further changes in the extensity and intensity of DPIs after the first DPI. P^O are other interventions that may affect social mobility, such as fiscal spending, population share of vaccinated people, international travel controls, income support and debt relief programs, public information campaigns, testing, contact tracing, mask wearing and vaccination policies, and protection strategies for the elderly population. Parts A and B of Table 2 show the summary statistics of these factors.

[13] A regular RD exploits a discontinuous change in the close neighborhood of a border separating the treated and untreated samples. RDiT is a special case when the running variable is time, which is usually a discrete variable in empirical exercises. This discreteness allows us to identify the effect by event time dummies rather than a discontinuity in a continuous polynomial like in regular RD designs. This design is related to event study designs, but it lacks a control group. For more detail see Hausman and Rapson (2018).

[14] Figure 4, presented in Section 2.3, supports this assumption, as it shows a sudden drop in social mobility at the time of the first intervention, but smooth changes in other periods.

Table 2: Descriptive Statistics of Covariates, First Stage

Covariate	mean	st.dev.	max.	min.	unit
A. Distancing Policy Interventions (DPIs)					
Extensity	1.04	2.03	6.50	-5.00	no. of DPIs
Intensity	1.39	3.11	12.00	-8.71	lvl of DPIs
B. Other COVID interventions					
Fiscal Spending	4.01	62.61	1957.60	0.00	billion USD
Share of Vaccinated	0.00	0.02	0.53	0.00	per citizen
Internat'l Travel Restr's	2.25	1.42	4.00	0.00	categorical
Income Support	1.09	0.87	2.00	0.00	categorical
Debt Relief	0.99	0.84	2.00	0.00	categorical
Public Info' Campaign	1.59	0.79	2.00	0.00	categorical
Testing Policy	1.63	1.03	3.00	0.00	categorical
Contact Tracing	1.24	0.80	2.00	0.00	categorical
Mask Wearing Policy	1.76	1.52	4.00	0.00	categorical
Vaccination Policy	0.32	0.83	5.00	0.00	categorical
Protection of the Elderly	1.53	1.16	3.00	0.00	categorical
C. Covariates of Voluntary Mobility					
Average Temperature	11.45	10.81	39.60	-41.89	Celsius degree
Average Humidity	70.43	16.13	97.08	12.52	percentage
Average Rainfall	14.11	18.50	209.38	0.00	mm
Average Snowfall	0.33	1.18	13.24	0.00	m
Covid Cases	0.68	1.23	9.32	0.00	per 10^5 citizen
Covid Deaths	0.01	0.02	0.20	0.00	per 10^5 citizen
Covid Cases at Neighbors	0.07	0.12	1.33	0.00	per 10^5 citizen
Covid Deaths at Neighbors	0.00	0.00	0.02	0.00	per 10^5 citizen

Z_{it} contain four weekly weather indicators, such as average temperature, humidity, snowfall, and rainfall, to absorb the effects of weather changes on social mobility. It also contains reported COVID cases and related deaths in population shares, both domestic and from neighboring countries. These are included to capture their possibly deterring effects on social mobility, which is a possible con-founder of government and individual decisions on distancing. Part D of Table 2 presents descriptive statistics of these covariates.

Countries differ in demographics, population density, and the quality of political and healthcare institutions, which are likely correlated with interventions, social activity, and reproduction numbers. I address these differences by including country-fixed effects, assuming the invariance of these factors on weekly frequencies.

4 Results

This section presents the results of the first and the second stage estimations. I start with the presentation and discussion of the first stage results. I then continue with a decomposition of social mobility into policy induced and voluntary components based on first stage predictions. Finally I present and discuss the main results about the economic effects of DPIs.

4.1 First Stage

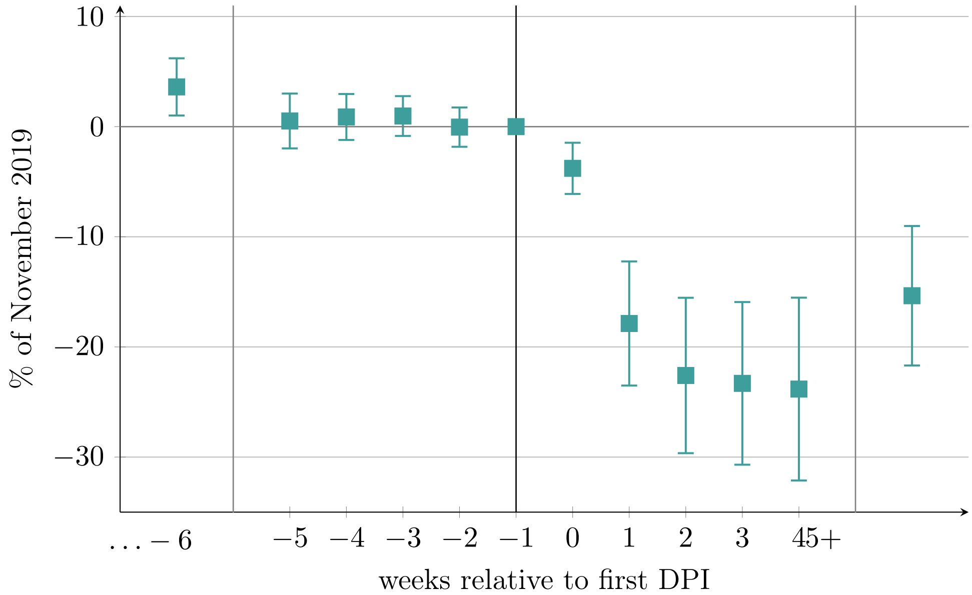
Figure 6 depicts predicted values for δ_t of equation (5), which measures the deviation of the social mobility index m_{it} from its final pre-intervention week values within countries. I interpret results on this figure from left to right. Effects more than five weeks distant from the intervention are grouped, giving two coefficients: one for the distant past, and one for the distant future of interventions. Results show a slight pre-intervention adjustment in social mobility as the effects from more than five weeks before the first intervention are positive and statistically significant at a 1% level. This effect is most likely attributed to voluntary distancing motives.

In the close neighborhood of the first DPI, pre-intervention coefficients are statistically indistinguishable from zero, while post-intervention coefficients indicate strong mobility-reducing effects of the first DPI. This discontinuity in the results supports the choice of the RD strategy. Most of the post-treatment effects happen within the first two weeks, of which week 0 is a mixed week allowed to contain days both from before and after the day of the first DPI. These are the effects that are identified as the treatment effects of the first DPI. Results predict that the first DPI treatment is expected to reduce social mobility by nearly 20 percentage points in November 2019 levels. Finally, looking at effects more than five weeks after the first DPI shows a slight reversal of social mobility. This reversal is again attributed to changes in voluntary distancing motives.

Quantitative results for DPI effects are presented in Table 3.[15] Results are presented for three specifications, the first one excluding further changes in the extensity (number of) and intensity of DPIs after the first intervention. The second and third specifications include these two factors gradually. The top two rows show point estimates for δ_0 and δ_1 with their standard errors in parentheses. These two coefficients capture the effect of the first DPI. The mobility effects of the first DPI treatment were found to be robust to specifications. Based on specification 3, the first DPIs reduced social mobility index m_{it} by 17.8 percentage points measured in November 2019 levels. The magnitude of this coefficient is roughly 1.5 of the standard deviation of m_{it} in the pre-treatment sample: 12.0. Given that the average magnitude

[15]For the estimation results for other covariates see Section A.2 in the Appendix.

Figure 6: Week-Fixed Effects of Social Mobility around the First DPI



Notes: Point estimates of δ_t of equation (5) with 99% confidence intervals. Standard errors allowed to cluster within weeks. Reference period: last week before the intervention. 2 870 country-week observations of 41 countries, R-squared = 0.7543.

(number plus level) of the first DPIs was roughly 8, this result also suggests that introducing a single DPI as a first intervention reduces m_{it} by 2.2 percentage points in November 2019 levels.

Specification 3 provides no statistical evidence for the effects of changes in the extensity of DPIs. However, it is found to significantly reduce mobility significantly in specification 2, where it has been included without the intensity indicator. A possible explanation for the extensity changes losing their significance when controlled for intensity changes is that these two factors are strongly correlated. The correlation between intensity and extensity is 0.79.

Based on specification 3, changing intensities of already introduced DPIs had a significant mobility-reducing effect. Increasing the intensity (total stringency level) of DPIs after the first treatment by 1 was found to decrease m_{it} by 2.1 percentage points measured in November 2019 levels. This result suggests that, although the first interventions were found to be the most effective, governments could significantly increase the distancing effects of DPIs by increasing their stringency levels. The introduction of new types of restrictions, however, was found to be ineffective in the further enhancement of social distancing.

These results suggests that the first ever distancing interventions has on average a strong and significant effect on social mobility. This effect are possible to be fine-tuned by changes in the intensity of but not by changes in the extensity of DPIs. The desired reduction of social mobility depends on how strongly these DPI induced reductions affected the spreading of COVID, which question of outside of the scope of this paper.[16] This paper continues towards its goal of measuring the economic effects of such mobility

[16]See Perra (2021) for a summary of the related literature.

Table 3: Effect of DPIs on Social Mobility

	(1)	(2)	(3)
Week 0	-3.285***	-3.947***	-3.786***
	(1.170)	(1.113)	(1.186)
Week 1	-14.791***	-17.013***	-17.872***
	(3.172)	(2.890)	(2.875)
Extensity		-1.615***	0.545
		(0.359)	(0.490)
Intensity			-2.098***
			(0.320)
Observations	2,870	2,870	2,870
R-squared	0.716	0.728	0.754
Country FE's	•	•	•
Countries	41	41	41
Controls	•	•	•

Notes: *** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses allowed to cluster within weeks. • – included, ∘ – excluded.

reductions due to DPIs.

4.2 Voluntary Social Mobility

The goal of the first stage estimation was realizing a prediction on voluntary distancing, because that is a key control for the identification of the economic effects of DPIs. Voluntary mobility is obtained by residualizing social mobility (m_it) by first-stage predictions for each policy-related covariate.

Predictions for the treatment effect of DPIs is defined as the event-time effects (δ_t) of equation (5) from week 0 and 1, such that it is fixed at the value of δ_1 for $t > 1$:

$$\widehat{m}_{it}^{T} = \begin{cases} \delta_t & \text{if } t \in \{0, 1\} \\ \delta_1 & \text{if } t > 1 \\ 0 & \text{otherwise} \end{cases}$$

Predictions for further changes in the extensity ($\widehat{m}_{it}^{E}$), and the intensity ($\widehat{m}_{it}^{I}$) of DPIs, and other interventions (P_{it}^{O})are simply the product of these variables with their coefficients:

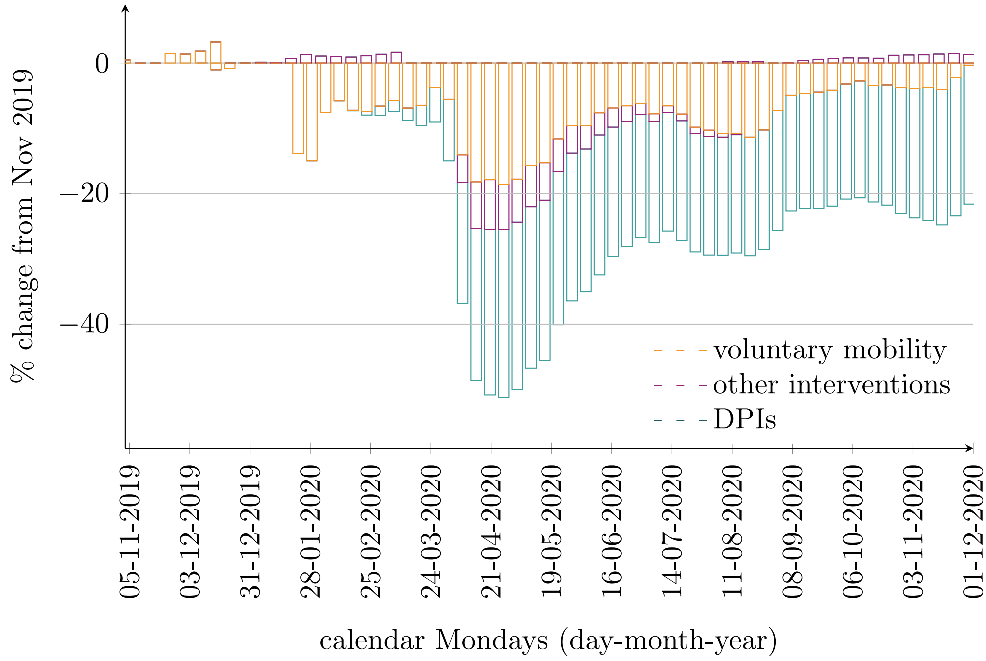
$$\widehat{m}_{it}^{E} = \gamma^{E} E_{it}, \qquad \widehat{m}_{it}^{I} = \gamma^{I} I_{it}, \qquad \widehat{m}_{it}^{O} = \theta P_{it}^{O}$$

A prediction for voluntary mobility ($\widehat{m}_{it}^{V}$) is then obtained as the following residual:

$$\widehat{m}_{it}^{V} = \text{mobility}_{it} - \widehat{m}_{it}^{T} - \widehat{m}_{it}^{E} - \widehat{m}_{it}^{I} - \widehat{m}_{it}^{O}.$$

Figure 7 depict cross-country averages for the predicted voluntary mobility component ($\widehat{m}_{it}^{V}$), the effect of other interventions ($\widehat{m}_{it}^{O}$) and the sum of the DPI related components: $\widehat{m}_{it}^{T}$, $\widehat{m}_{it}^{E}$, and $\widehat{m}_{it}^{I}$ in calendar time. This is a stacked column graph, therefore the sum of the columns track the social mobility indicator m_{it}. Social mobility declines around mid-March as most countries in the sample intervened for the first time in March 2020. The dominant factor in this decline is found to be the effect of DPIs, although voluntary mobility had a substantial contribution as well. Figure 8 breaks down the effect of DPIs of Figure 7 into its three components: $\widehat{m}_{it}^{T}$, $\widehat{m}_{it}^{E}$, and $\widehat{m}_{it}^{I}$. This figure reveals that the effect of DPIs was predominantly due to the first DPIs. This graph also gives visual support for the conclusion that further changes in the intensity of DPIs had a significant effect on mobility, while further changes in the number of DPIs did not.

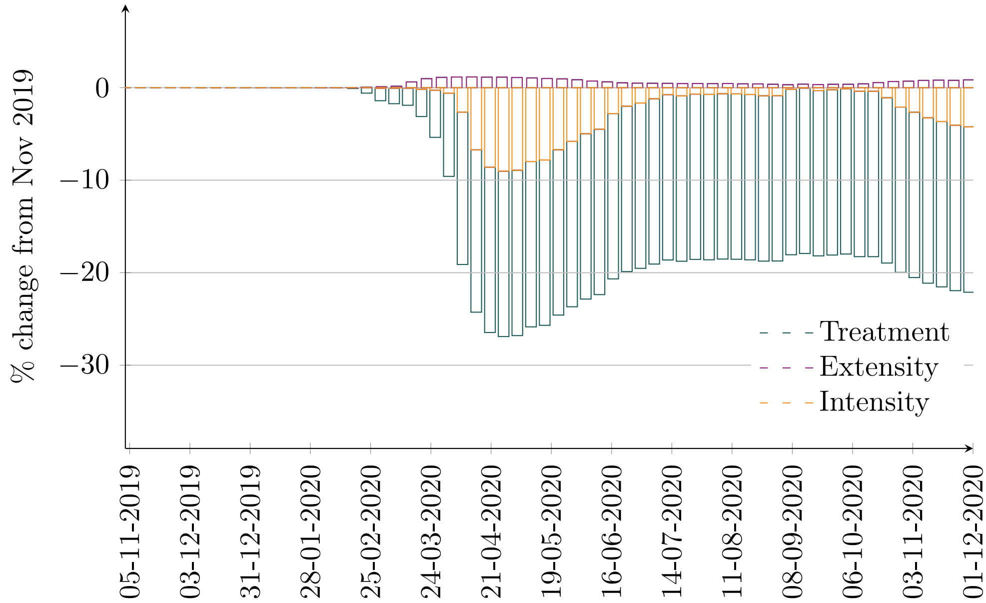
Figure 7: Historical Decomposition of Social Mobility Index in 42 OECD Economies



Notes: cross country averages. Predicted by specification (3) of Table 3

The aim of the first stage estimation is to create a proxy for voluntary distancing, which is an important control in the estimation of the economic effects of DPIs in the second stage. It is the predicted voluntary mobility component $\widehat{m}_{it}^{V}$. It is obtained as a residual, and therefore, it is important to investigate which factors drive the variance of this variable. The contribution of different covariates, country-fixed effects, and the error term to the total variance of voluntary mobility, $widehatmV_it$, is shown in table ref: tab: variance. Covariates account for 28 percent to the total variance of $\widehat{m}_{it}^{V}$, with fixed effects accounting for

Figure 8: Historical Decomposition of Predicted DPI effects in 42 OECD Economies



Notes: cross country averages. Predicted by specification (3) of Table 3

another 12 percent. The unexplained component accounts for the remaining 60% of its variance.

4.3 Economic Effects of Distancing Policy Interventions

In this subsection, I present results of the estimation of equation (4) for seven different economic outcomes. Economic outcomes are measured as a percentage of their most recent November values. The average values of 2015–2019, a COVID-free control period, are subtracted from each indicator. I start with the presentation of three different specifications that include voluntary mobility, other interventions, and mobility at trading partners one-by-one to investigate omitted variable biases caused by these factors. I present these specifications using industrial production as the outcome. After that, I present results for the other six economic outcomes using only the most complete specifications. I first continue with the four sector outputs: industrial, and manufacturing production, construction output, and retail trade. I then continue with the two price indicators: CPI, and PPI in manufacturing. I conclude the analysis with unemployment effects.

4.3.1 Second Stage Results: Industrial Production

Results for industrial production are presented in Table 5.[17] The first specification includes only DPI factors along with country-fixed effects. Treatment and intensity effects are already significant and strong in this simple specification. The second specification includes voluntary social mobility, which is found to

[17]For the estimation results for other covariates see Section A.3 in the Appendix.

Table 4: Variance Decomposition of Voluntary Mobility

Covariate	Variance	Proportion (%)
Average Temperature	2.19	2.21
Average Humidity	1.01	1.03
Average Rainfall	0.14	0.14
Average Snowfall	0.00	0.00
Covid Cases $_{t-1}$	0.03	0.03
Covid Cases $_{t-2}$	0.05	0.05
Covid Deaths $_{t-1}$	6.73	6.80
Covid Deaths $_{t-2}$	2.65	2.67
Covid Cases $_{t-1}$ at Neighbors	0.58	0.59
Covid Cases $_{t-2}$ at Neighbors	3.96	4.00
Covid Deaths $_{t-1}$ at Neighbors	9.58	9.68
Covid Deaths $_{t-2}$ at Neighbors	0.42	0.42
Total of Covariates	27.34	27.62
FE_i	12.21	12.33
Residual	59.43	60.05
Total	98.98	100.00

Notes: Using on results from specification (3) of Table 3. Covid cases and deaths are measured in population shares both for domestic and neighboring countries.

be significant and positively correlated with industrial production. The positive sign of this coefficient is in line with the intuition that less mobility implies lower rates of economic activity. The third specification includes other interventions. These turn out to be important control factors, as their inclusion significantly decreases the coefficients of DPI factors. The fourth specification includes mobility at export and import partners. The inclusion of these two indicators decreases slightly further the coefficients of DPIs, revealing a modest omitted variable bias in previous specifications due to international spillovers. The coefficients of these two factors are statistically insignificant, however.

The most complete specification show that the first DPIs and further changes in their intensity had a significant effect on industrial production, while I found no evidence for the effects of changes in the extensity of DPIs. When compared to its 2015-2019 averages in November values, a single level 1 DPI reduces industrial production by 0.8 percentage points on average. A change in the intensity of DPIs reduces industrial production by 1.15 percent. A one percentage point deviation of voluntary mobility from its November 2019 levels is found to decrease industrial production by .4 percent.

Table 5: Effect of DPIs on Industrial Production

	(1)	(2)	(3)	(4)
Treatment	-1.230***	-1.124***	-0.871***	-0.793***
	(0.203)	(0.132)	(0.244)	(0.231)
Extensity	-0.687	-0.723	-0.628	-0.679
	(0.524)	(0.625)	(0.533)	(0.559)
Intensity	-1.809***	-1.559***	-1.223***	-1.157***
	(0.447)	(0.282)	(0.242)	(0.217)
Voluntary Mobility		0.335**	0.397**	0.400**
		(0.130)	(0.131)	(0.137)
Mobility $_{t-1}$ at Import Partners				0.411
				(0.702)
Mobility $_{t-1}$ at Export Partners				-0.285
				(0.662)
Observations	288	288	288	288
R-squared	0.584	0.623	0.666	0.669
Country FE	•	•	•	•
Countries	32	32	32	32
Other Interventions	◦	◦	•	•

Notes: *** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses allowed to cluster within months. • – included, ◦ – excluded.

4.3.2 Output Losses

Table 6 shows the results of specification 4 for the four different sector outputs.[18] Column 1 simply repeats the results for industrial production in column 4 of table 5 for comparison. Column 2 is manufacturing, which is a sub-sector of the wider industry sector, and as a consequence, the results are very similar in the first two columns. The first DPI treatment and further changes in DPI intensities had a significant negative effect on manufacturing production, but there was no effect from the changes in extensity. Voluntary mobility had a similar impact on manufacturing as it did on the entire industry sector, and no evidence of spillover effects from mobility changes at trading partners was found.

Results for construction output are presented in column 3. This indicator is only available for a substantially smaller set of countries; therefore, its results are not quantitatively comparable with other columns. The introduction of a single level 1 DPI treatment decreases construction output by 2.1 percentage points from November 2019 levels. A further change in DPI intensity is found to decrease it by another 2.7 percentage points. A unit increase in DPI extensity, which is the introduction of a new

[18]For the estimation results for other covariates see Section A.3 in the Appendix.

Table 6: Effect of DPIs on Sector Outputs

	(1) Industrial Production	(2) Manuf'ing Production	(3) Constr' Output	(4) Retail Trade
Treatment	-0.794***	-0.957***	-2.141**	-1.234***
	(0.233)	(0.202)	(0.753)	(0.223)
Extensity	-0.670	-0.302	3.039**	0.938
	(0.557)	(0.589)	(0.941)	(0.695)
Intensity	-1.145***	-1.413***	-2.682***	-2.447***
	(0.218)	(0.202)	(0.436)	(0.473)
Voluntary Mobility	0.402**	0.454**	0.301	0.531***
	(0.136)	(0.143)	(0.208)	(0.112)
Mobility $_{t-1}$ at Import Partners	0.405	0.525	0.126	0.515
	(0.702)	(0.758)	(0.882)	(0.416)
Mobility $_{t-1}$ at Export Partners	-0.278	-0.388	-0.222	-0.552
	(0.663)	(0.723)	(0.947)	(0.424)
Observations	288	288	189	270
R-squared	0.670	0.687	0.683	0.784
Country FE	•	•	•	•
Countries	32	32	21	30
Other Interventions	•	•	•	•

Notes: *** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses allowed to cluster within months. • – included, ◦ – excluded.

type of DPI, had, however, a positive, albeit only marginally significant effect on construction output. Without further investigation, a possible explanation could be that when DPI restrictions extend to more and more building types, such as schools, office buildings, or concert halls, that gives way to more and more reconstructions. A one percentage point decline in mobility was found to decrease construction output by 0.3 percentage points. No evidence was found for spillover effects of mobility changes across trading partners.

Column 4 shows results for retail trade. Retail trade responded significantly to the first DPI treatment, changes in DPI intensity, and voluntary mobility. I found no evidence of significant responses to DPI extensity and mobility spillovers from trade partners. A single level 1 DPI introduced as a first treatment decreases retail trade by 1.2 percentage points, while a unit change in the level of DPIs decreases retail trade by 2.4 percentage points from November 2019 levels. In November 2019 values, a 1% decrease in voluntary mobility reduces retail trade by 0.5 percentage points.

Sector outputs were found to respond strongly to first DPI treatments, changes in DPI intensities, and voluntary mobility. On the other hand, I found no evidence of significant responses to DPI extensity and mobility spillovers from trade partners, except in construction. These findings altogether suggest that distancing behaviors that were either voluntary or DPI-compliant generated substantial output losses.

Table 7: Predicted Distancing Effects by Sectors in Month 2 of the First DPI

	Industrial Production	Manuf'ing Production	Constr' Output	Retail Trade
Treatment	-10.17	-12.25	-27.41	-15.80
	(2.99)	(2.59)	(9.64)	(2.85)
Extensity	-0.22	-0.10	1.01	0.31
	(0.18)	(0.19)	(0.31)	(0.23)
Intensity	0.16	0.20	0.38	0.35
	(-0.03)	(-0.03)	(-0.06)	(-0.07)
Voluntary Mobility	-1.44	-1.62	-1.08	-1.90
	(-0.48)	(-0.51)	(-0.74)	(-0.40)
Total Change	-16.15	-19.20	-19.52	-11.68
Explained by Distancing	-11.67	-13.77	-27.10	-17.04
percent	72.26	71.72	138.83	145.89
Unexplained by Distancing	-4.48	-5.43	7.58	5.36
percent	27.74	28.28	-38.83	-45.89

Notes: Predicted effects. Calculated as changes in cross country averages between month -1 and month 2, and multiplied by the coefficients of column 4 of Table 6. Standard errors in parenthesis are calculated similarly, using the s.e. of the corresponding coefficient.

It is crucial to compare the consequences of voluntary and DPI-induced distancing when forming policy conclusions about DPI efficiency. Voluntary mobility and DPI components are measured in different units, so Table 6 coefficients are not directly comparable between rows. One possible way to address this issue would be to use the estimates for DPIs of equation (5), for example, $\widehat{\gamma}^I I_{it}$ for intensity changes, directly on the right-hand side of equation (4), instead of the policy variables. $\widehat{\gamma}^I I_{it}$ contains the same information as the policy variable, I_{it}, as they differ only in a constant multiplier $\widehat{\gamma}^I$. But this multiplier translates the unit of the policy variable into the unit of voluntary mobility changes, making these two factors comparable. Although this strategy appears simple and straightforward, it is impossible to implement because the most important policy variable, treatment (T_{it}), is not included in the first-stage equation. The reason it is not included is that the effect of the DPI treatment is captured by the RDiT design that builds on the key identifying assumption of sudden responses to policy changes. Giving this design up is considered to be a greater cost than the gain of the comparison that would emerge from a different design would provide.

I work around this problem by picking a different strategy to make the effects of DPIs and voluntary distancing comparable. It is a decomposition of the changes in sector outputs around the months of the first DPI interventions. I calculated predicted values of DPI and voluntary distancing effects by multiplying the changes of these factors from month -1 to month 2 for all factors with their coefficients.[19] Table 7 shows these predicted effects for all four sector outputs averaged across countries. The bottom of the table contains the change of the explained sector outcome and summary calculations about what fraction of this total change could be explained by the predicted distancing effects. Figures show that although voluntary distancing caused significant losses to sector outputs, its effect was an order of magnitude smaller than that of DPIs in the short run. For example, the first DPI treatment explains about 10 percentage points of the total industrial output loss relative to the last month before the first DPI.[20] Voluntary distancing, on the other hand, explains only 1.4 percentage points.

The largest negative effect of the first DPI treatment was identified in the construction sector, -27.4 percentage points. The effect of DPI extensity was found to be significant only in the case of construction output, where it contributed 1 percentage point, offsetting slightly the overall 19,5 percentage point decline observed in the sector. In month 2, the effect of DPI intensity changes was found to contribute the least to total changes.Voluntary mobility was found to decrease retail trade the most, by almost 2 percentage points.

Only 70% of total losses in industry and manufacturing are explained by distancing factors, implying that output losses in these sectors were caused by other factors, such as other COVID-related interventions.In construction and retail trade, on the other hand, distancing factors altogether predicted more losses than was observed. This finding suggests that other factors, such as fiscal and monetary support programs, could mitigate the short-term costs of distancing in these two sectors.

4.3.3 Inflationary Effects

Table 8 contains results for consumer prices and producer prices of the manufacturing industry.[21] I found no evidence of inflationary effects of DPIs except for DPI extensity. Results show that extending the set of DPIs by a new intervention decreases consumer prices by 0.1 percent. Column 6 shows results for producer prices in manufacturing, providing no evidence of either voluntary or DPI-induced distancing effects from domestic markets. Distancing in export markets, on the other hand, is marginally significant, with a one-point increase in social mobility in export markets lowering domestic manufacturing prices by 0.27 percent. The sign of this effect is in contrast with the economic intuition that falling demand reduces prices.

In summary, evidence for inflationary effects of any kind of distancing could not be identified by this study. This suggests that neither DPIs nor voluntary distancing bring on little to no inflationary costs.

[19] I did the same multiplication with the standard errors.

[20] As a comparison Deb et al. (2021) find that losses in industrial production were about 10 percent over 30 days following the implementation of containment measures.

[21] For the estimation results for other covariates see Section A.3 in the Appendix.

One possible explanation for insignificant inflationary effects is the omission of conventional monetary policy interventions, such as rate cuts. Countries anticipating stronger inflationary risks due to their specific mix of DPIs might have cut their rates more strongly, mitigating the inflationary effects of DPIs. Another possible explanation for insignificant inflationary effects is that the typical shock response of prices tends to have a several-quarter time-lag. That might suggest that inflationary effects of DPIs emerge on time horizons, for example, a year later, that are unable to be captured with the current design.

Table 8: Effect of DPIs on Prices

	(1) CPI	(2) PPI manuf'ing
Treatment	-0.009	-0.049
	(0.009)	(0.049)
Extensity	-0.090**	0.178
	(0.028)	(0.157)
Intensity	0.016	-0.077
	(0.015)	(0.058)
Voluntary Mobility	-0.005	-0.008
	(0.003)	(0.022)
Mobility $_{t-1}$ at Export Partners	-0.010	-0.275*
	(0.023)	(0.133)
Mobility $_{t-1}$ at Import Partners	0.024	0.231
	(0.025)	(0.139)
Observations	288	252
R-squared	0.887	0.838
Country FE	•	•
Countries	32	28
Other Policies	•	•

Notes: *** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses allowed to cluster within months. • – included, ∘ – excluded.

4.3.4 Unemployment Effects

Table 9 presents second stage results for the unemployment rate in the same four specifications as Table 5 for industrial production.[22] The first specification reveals strong positive unemployment responses to the first DPI treatment.[23] When voluntary mobility is introduced as a control, this strong response

[22] For the estimation results of other covariates from specification 4 see Section A.3 in the Appendix.
[23] And a slight negative response to DPI intensity changes.

is maintained. However, when other COVID interventions are introduced the coefficient of DPIs collapse and loose significance. This observation about coefficients are maintained when mobility at trading partners are included in the final specification. This finding suggests that the observed hike in unemployment on Figure 1 of Section 2 after the first DPI interventions are explained by other COVID related interventions.[24]

There might be other explanations for the lack of unemployment effects as well. For example, conventional or not COVID-focused fiscal policy interventions are not controlled for in the current version of this paper. For example, governments anticipating higher unemployment risks might choose to relax taxes more intensively compared to other governments, offsetting the unemployment effects of their DPIs. Another possible explanation could be based on anecdotal evidence that labor adjusted on the intensive margins first in the early months of the COVID restrictions, such as lowering working hours, the extension of sick-leaves, or enforced holidays. Employers aimed to keep their employees as they expected the restrictions-induced production halt to be temporary and considered the cost of rehiring to be higher than the cost of labor intensity adjustments.

5 Conclusion

This paper identifies the causal effects of distancing policy interventions (DPIs) on seven short-term economic indicators: industrial and manufacturing production, construction output; retail trade; CPI; PPI in manufacturing; and the unemployment rate. Effects are identified from within-country changes of these indicators from before and after the first ever DPI treatment relative to the averages of a COVID-free control period: 2015–2019. Causal effects are identified by controlling for three important confounding factors: voluntary distancing, other COVID related interventions, and distancing at trading partners.

Among these confounders, voluntary distancing is an unobserved factor. Voluntary distancing is therefore estimated in a regression discontinuity framework using mobility data. It is realized as a residual after the identification of DPI-induced distancing effects as sudden changes in mobility after the first DPI intervention. Results suggest that the first ever distancing intervention had, on average, a strong and significant effect on social mobility. This effect was fine-tuned by changes in the intensity of DPIs. Voluntary motives were also found to contribute to a significant portion of mobility patterns.

I found significant output losses due to DPIs, but no evidence for inflationary and unemployment effects. Findings suggest that DPIs caused substantial output losses. Results also show that although voluntary distancing caused significant losses to sector outputs, its effect was an order of magnitude smaller than that of DPIs. Only 70% of total losses in industry and manufacturing are explained by either voluntary or DPI-induced distancing, implying that other factors, such as other COVID-related interventions contributed substantially to output losses in these sectors. In construction and retail trade,

[24] For a similar result, see Kong and Prinz (2020).

Table 9: Effect of DPIs on the Unemployment Rate

	(1)	(2)	(3)	(4)
Treatment	0.163***	0.171***	0.058	0.026
	(0.031)	(0.029)	(0.069)	(0.070)
Extensity	-0.133*	-0.133*	-0.076	-0.043
	(0.065)	(0.061)	(0.074)	(0.060)
Intensity	0.059	0.073	0.019	-0.006
	(0.100)	(0.095)	(0.090)	(0.084)
Voluntary Mobility		0.017	-0.007	-0.013
		(0.011)	(0.009)	(0.009)
Mobility $_{t-1}$ at				-0.108*
Export Partners				(0.057)
Mobility $_{t-1}$ at				0.008
Import Partners				(0.065)
Observations	279	279	279	279
R-squared	0.745	0.747	0.788	0.810
Country FE	•	•	•	•
Countries	31	31	31	31
Other Interventions	◦	◦	•	•

Notes: *** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses allowed to cluster within months. • – included, ◦ – excluded.

on the other hand, distancing factors altogether predicted more losses than was observed. This finding suggests that other factors, such as fiscal and monetary support programs, could mitigate the short-term costs of distancing in these two sectors.

This study did not identify any evidence for inflationary effects of any kind of distancing. This suggests that neither DPIs nor voluntary distancing bring on little to no inflationary costs. Although a significant hike in unemployment rates can be observed after DPI interventions took place, no evidence was found in support when controlling for voluntary distancing, other COVID interventions, and distancing at trading partners. Findings suggest that the observed hike in unemployment is related to other COVID-related interventions.

These findings provide evidence of the economic cost of DPIs to consider for governments that are planning to implement such interventions during an epidemic. These findings also contribute to a more complete cost-benefit analysis of distancing policy interventions on the cost side. The costs identified here are mainly output losses, while no evidence was found for inflationary costs or unemployment responses.

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Appendix A

A.1 COVID-19 Aggregated Mobility Research Dataset

Description The Google COVID-19 Aggregated Mobility Research Dataset contains anonymized mobility flows aggregated over users who have turned on the Location History setting, which is off by default. This is similar to the data used to show how busy certain types of places are in Google Maps — helping identify when a local business tends to be the most crowded. The dataset aggregates flows of people from region to region, which is here further aggregated at the level of NUTS3 areas, weekly.

To produce this dataset, machine learning is applied to logs data to automatically segment it into semantic trips https://www.nature.com/articles/s41467-019-12809-y. To provide strong privacy guarantees, all trips were anonymized and aggregated using a differentially private mechanism https://research.google/pubs/pub48 to aggregate flows over time (see https://policies.google.com/technologies/anonymization). This research is done on the resulting heavily aggregated and differentially private data. No individual user data was ever manually inspected, only heavily aggregated flows of large populations were handled.

All anonymized trips are processed in aggregate to extract their origin and destination location and time. For example, if users traveled from location a to location b within time interval t, the corresponding cell (a, b, t) in the tensor would be $n \pm err$, where err is Laplacian noise. The automated Laplace mechanism adds random noise drawn from a zero mean Laplace distribution and yields (ϵ, δ)-differential privacy guarantee of $\epsilon = 0.66$ and $\delta = 2.1 \times 10 - 29$ per metric. Specifically, for each week W and each location pair (A, B), we compute the number of unique users who took a trip from location A to location B during week W. To each of these metrics, we add Laplace noise from a zero-mean distribution of scale 1/0.66. We then remove all metrics for which the noisy number of users is lower than 100, following the process described in https://research.google/pubs/pub48778/, and publish the rest. This yields that each metric we publish satisfies (ϵ, δ)-differential privacy with values defined above. The parameter ϵ controls the noise intensity in terms of its variance, while δ represents the deviation from pure ϵ-privacy. The closer they are to zero, the stronger the privacy guarantees.

Limitations These results should be interpreted in light of several important limitations. First, the Google mobility data is limited to smartphone users who have opted in to Google's Location History feature, which is off by default. These data may not be representative of the population as whole, and furthermore their representativeness may vary by location. Importantly, these limited data are only viewed through the lens of differential privacy algorithms, specifically designed to protect user anonymity and obscure fine detail. Moreover, comparisons across rather than within locations are only descriptive since these regions can differ in substantial ways.

Data Availability The Google COVID-19 Aggregated Mobility Research Dataset used for this study is available with permission from Google LLC.

A.2 First Stage Results for Covariates

Table 10: First Stage Results for Covariates – 1

VARIABLES	(1)	(2)	(3)
Cases $_{t-1}$	-0.119	-0.069	-0.224
	(0.636)	(0.596)	(0.512)
Cases $_{t-2}$	-0.416	-0.354	-0.051
	(0.753)	(0.717)	(0.658)
Deaths $_{t-1}$	-122.590**	-125.633***	-108.987***
	(45.508)	(40.947)	(37.613)
Deaths $_{t-2}$	54.836	59.764*	61.981**
	(34.975)	(33.427)	(27.370)
Cases $_{t-1}$ at	1.966	2.044	5.517
Neighbors	(4.821)	(4.207)	(4.573)
Cases $_{t-2}$ at	25.366***	24.222***	17.958***
Neighbors	(6.270)	(6.260)	(6.075)
Deaths $_{t-1}$ at	-1,625.307***	-1,538.692***	-1,175.645***
Neighbors	(356.618)	(346.804)	(294.015)
Deaths $_{t-2}$ at	203.146	201.314	214.978
Neighbors	(315.422)	(308.279)	(269.606)
Fiscal spending	-0.004*	-0.003	-0.004
	(0.002)	(0.002)	(0.002)
Share of vaccinated $_{t-2}$	3.798	7.963	4.707
	(14.339)	(13.505)	(14.963)
Travel Cont's: Screening	6.730**	7.250***	7.329***
	(2.510)	(2.299)	(2.347)
Quarantine	0.005	0.901	0.914
	(2.532)	(2.392)	(2.353)
Trageted Ban	-0.957	0.139	0.611
	(2.956)	(2.785)	(2.729)
Total Ban	-9.701***	-7.600**	-5.435*
	(3.585)	(3.210)	(2.976)
Observations	2,870	2,870	2,870
R-squared	0.716	0.728	0.754
Country FE's	•	•	•
Extensity	◦	•	•
Intensity	◦	◦	•
Countries	41	41	41

Notes: *** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses allowed to cluster within weeks. • – included, ◦ – excluded.

Table 11: First Stage Results for Covariates – 2

VARIABLES	(1)	(2)	(3)
Income Support (≤50%)	-3.882	-3.449	-1.857
	(3.023)	(3.237)	(2.846)
Income Support (>50%)	-2.302	-1.987	-0.869
	(2.464)	(2.582)	(2.359)
Debt Reief: Narrow	-1.628	-1.430	-2.346
	(1.643)	(1.691)	(1.482)
Broad	-2.197	-2.078	-2.544
	(2.136)	(2.133)	(1.844)
Info' Camp'n: Urging	1.371	1.243	0.942
	(1.709)	(1.580)	(1.618)
Coordinated	-0.435	1.318	1.719
	(2.229)	(1.893)	(1.862)
Testing: Symptoms + else	-0.200	0.056	-0.629
	(1.275)	(1.220)	(1.281)
w/ Symptoms	3.681*	2.714	2.369
	(1.896)	(1.733)	(1.864)
Open for All	7.147***	6.017***	5.739**
	(2.169)	(2.005)	(2.169)
Contact Tracing: Limited	-1.306	-1.038	-0.913
	(1.598)	(1.368)	(1.201)
Comprehensive	-0.649	-0.606	-1.731
	(1.427)	(1.240)	(1.214)
Masks: recommended	1.094	1.531	1.843
	(2.333)	(2.430)	(2.059)
specific places	3.231*	3.078*	3.197*
	(1.759)	(1.818)	(1.588)
public places	4.680**	4.615**	4.840***
	(1.953)	(1.977)	(1.744)
everywhere	3.760	5.029**	5.805**
	(2.287)	(2.451)	(2.237)
Observations	2,870	2,870	2,870
R-squared	0.716	0.728	0.754
Country FE's	•	•	•
Extensity	◦	•	•
Intensity	◦	◦	•
Countries	41	41	41

Notes: *** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses allowed to cluster within weeks. • – included, ◦ – excluded.

Table 12: First Stage Results for Covariates – 3

VARIABLES	(1)	(2)	(3)
Vaccination: 1 group	-1.403	-1.466	-1.944
	(1.838)	(1.823)	(1.518)
2 groups	1.216	1.561	1.725
	(1.387)	(1.295)	(1.305)
3 groups	1.359	1.097	2.827*
	(1.324)	(1.297)	(1.505)
3+ groups	5.635	3.814	7.857**
	(3.833)	(3.262)	(3.810)
universal	7.104	4.828	5.007
	(6.505)	(6.063)	(6.852)
Elderly Protection: Recomm'	-1.674	-1.403	-2.794
	(1.842)	(1.884)	(1.730)
Narrow	-6.189***	-4.772***	-4.987***
	(1.961)	(1.763)	(1.495)
Extensive	-8.027***	-5.987***	-4.297**
	(2.183)	(2.145)	(1.699)
Mean Temperature	-0.098	-0.106	-0.124**
	(0.075)	(0.065)	(0.060)
Mean Humidity	0.089**	0.084**	0.079**
	(0.035)	(0.035)	(0.038)
Total Rainfall	-0.029	-0.028	-0.021
	(0.018)	(0.018)	(0.017)
Total Snowfall	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)
Observations	2,870	2,870	2,870
R-squared	0.716	0.728	0.754
Country FE's	•	•	•
Extensity	∘	•	•
Intensity	∘	∘	•
Countries	41	41	41

Notes: *** $p<0.01$, ** $p<0.05$, * $p<0.1$, standard errors in parentheses allowed to cluster within weeks. • – included, ∘ – excluded.

A.3 Second Stage Results for Covariates

Table 13: Second Stage Results for Covariates – 1

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	industrial production	manuf'ing production	cons- truction	retail trade	CPI	PPI manuf'ing	unemployment rate
Cases $_{t-1}$	0.007***	0.007***	0.003	0.002	-0.000***	0.002**	0.001
	(0.002)	(0.002)	(0.005)	(0.003)	(0.000)	(0.001)	(0.001)
Deaths $_{t-1}$	-0.029	-0.057	-0.083	0.121*	0.002	-0.072***	-0.019*
	(0.072)	(0.086)	(0.133)	(0.060)	(0.004)	(0.010)	(0.009)
Cases $_{t-1}$ at	0.283**	0.402***	0.075	0.259**	-0.022***	0.006	0.046
Neighbors	(0.089)	(0.085)	(0.258)	(0.102)	(0.006)	(0.032)	(0.034)
Deaths $_{t-1}$ at	-5.005	-6.687	-3.368	-5.061	-0.098	0.792*	-1.160*
Neighbors	(3.371)	(3.711)	(6.812)	(2.930)	(0.199)	(0.377)	(0.507)
Fiscal Spending	-0.000	-0.008	-0.050***	-0.001	0.000	0.000	-0.005***
	(0.000)	(0.005)	(0.012)	(0.004)	(0.000)	(0.001)	(0.001)
Investment in	0.350	0.346	2.256	1.663	-0.010	-0.078	0.241
Vaccines	(0.614)	(0.457)	(1.825)	(1.101)	(0.053)	(0.123)	(0.242)
Investment in	0.076**	0.066**	0.332***	-0.016	-0.002	-0.002	0.025**
Healthcare	(0.026)	(0.026)	(0.081)	(0.032)	(0.003)	(0.008)	(0.011)
International Travel Controls							
Screening	6.693**	6.738**	4.381	11.743**	-0.107	-1.945**	-1.073***
	(2.179)	(2.446)	(5.329)	(4.865)	(0.137)	(0.685)	(0.211)
Quarantine	1.076	0.842	7.213	5.269	-0.246**	-2.479***	-0.343
	(1.738)	(2.097)	(5.703)	(2.938)	(0.086)	(0.735)	(0.189)
Targeted Ban	-1.226	-1.890	14.420*	5.681*	-0.399***	-3.411***	0.302
	(1.262)	(1.436)	(7.238)	(2.922)	(0.102)	(0.751)	(0.338)
Total Ban	-4.882**	-5.719**	19.837**	3.211	-0.375**	-2.955***	0.444
	(2.027)	(2.204)	(7.352)	(3.311)	(0.132)	(0.786)	(0.564)

Notes: *** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses allowed to cluster within months. • – included, ○ – excluded.

Table 14: Second Stage Results for Covariates – 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	industrial production	manuf'ing production	cons-truction	retail trade	CPI	PPI manuf'ing	unemployment rate
Income Support Programs							
$\leq$ 50%	0.486	0.635	-0.849	0.965	-0.285**	0.045	0.449**
	(3.033)	(2.691)	(2.862)	(2.485)	(0.086)	(0.200)	(0.168)
$<$ 50%	-0.091	0.277	-0.340	4.432	-0.283**	-0.344	0.522
	(2.413)	(2.032)	(2.544)	(2.432)	(0.106)	(0.392)	(0.295)
Debt Relief							
Narrow	-0.253	0.271	1.810	-0.047	0.116	0.240	0.590*
	(0.571)	(0.729)	(2.188)	(1.324)	(0.103)	(0.427)	(0.280)
Broad	-1.510	-1.278	6.137	0.131	-0.075	0.208	0.753*
	(0.993)	(1.217)	(3.614)	(1.890)	(0.147)	(0.383)	(0.336)
Public Information Campaigns							
Officials Urging	-2.068*	-2.190**	1.547	-0.175	-0.079	1.286**	0.069
	(1.012)	(0.828)	(2.563)	(0.831)	(0.123)	(0.543)	(0.476)
Coordinated	1.401	2.213	-2.126	-0.098	-0.109	0.728	-0.032
	(2.196)	(2.220)	(3.782)	(0.966)	(0.096)	(0.728)	(0.403)
Observations	288	288	189	270	288	252	279
R-squared	0.670	0.687	0.683	0.784	0.887	0.838	0.810
Country FE	•	•	•	•	•	•	•
Countries	32	32	21	30	32	28	31

Notes: *** p<0.01, ** p<0.05, * p<0.1, standard errors in parentheses allowed to cluster within months. • – included, ○ – excluded.

A.4 Historical Decompositions

Figure 9: Historical Decomposition of Industrial Production

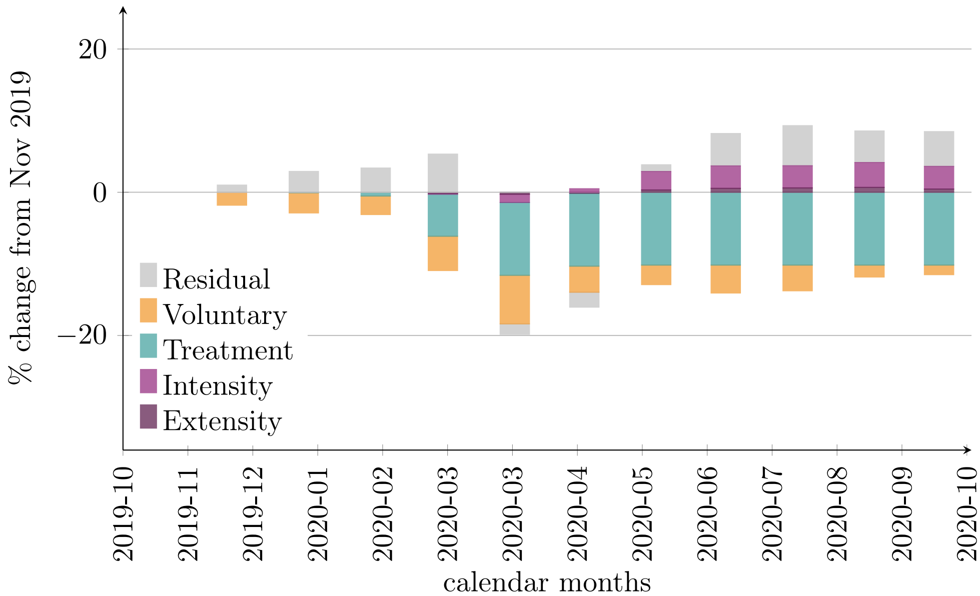

Figure 10: Historical Decomposition of Manufacturing Production

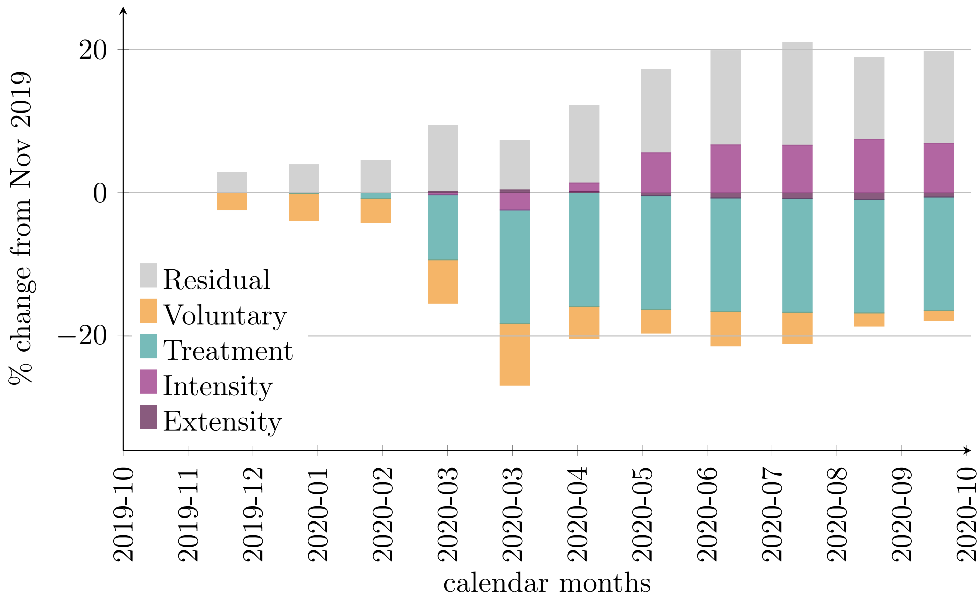

Figure 11: Historical Decomposition of Construction Output

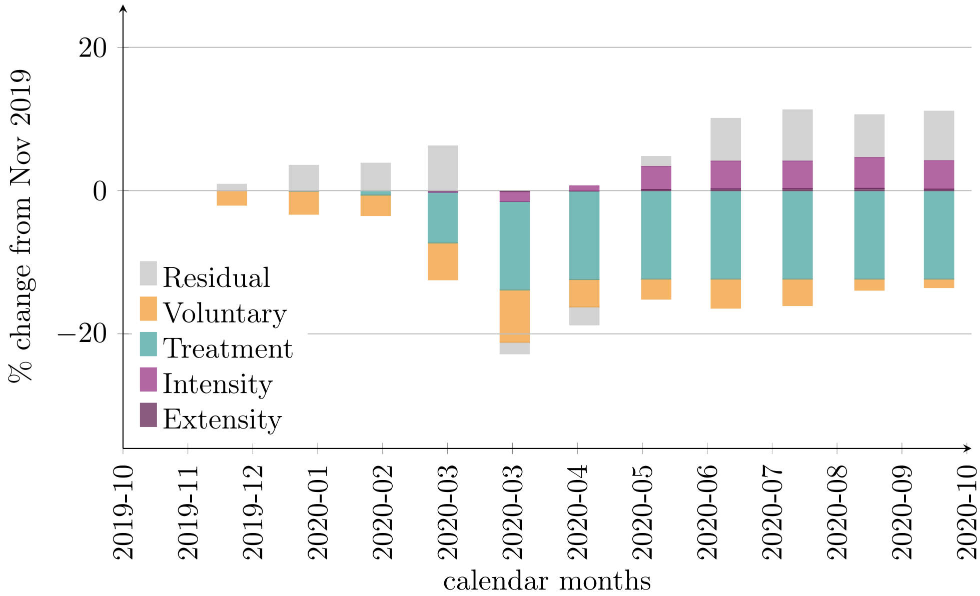

Figure 12: Historical Decomposition of Retail Trade