Economic Costs of Distancing Policy Interventions

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Abstract

Outbreaks of new viruses are increasingly likely in the warming global climate. Distancing policy interventions (DPIs) are expected to be the primary containment strategy in such events before medical treatments become feasible. DPIs, however, affect economic activity as well, making it important to measure the economic impact of DPIs. This paper estimates the effects of DPIs on selected indicators of monthly economic activity in the first year of the COVID-19 pandemic in a sample of 44 countries. The paper contributes policy-relevant economic effects and stronger external validity to existing literature. DPI effects are identified in a two-stage empirical design. The first stage leverages an observable sharp decline in weekly-frequency social interactions after implementing the first DPIs. In a difference-in-differences design, the second stage carries the policy-induced distancing effects over to monthly frequency macroeconomic indicators, such as industrial and manufacturing production, construction output, retail trade, inflation, and unemployment. I find significant output losses due to DPIs but no evidence for inflationary or unemployment effects. Results also demonstrate the relevance of voluntary distancing on output losses. Results suggest that output losses should be the primary concern of governments when considering distancing interventions in a future pandemic.

Keywords: COVID-19; social distancing; regression discontinuity; economic costs

1. Introduction

Outbreaks of new viruses are increasingly likely in the warming global climate.[1] 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.[2] COVID-19 was no exception. DPIs limit social interactions to prevent virus transmission at the cost of impeding economic activity. This paper aims to quantify the short-run economic effects that can be causally linked to distancing policy interventions.

There are two major empirical challenges in studying the economic impacts of DPIs. First, distancing happens not only in compliance with DPIs but also as a voluntary response to idiosyn-

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[1]See for example de Oliveira and Tegally (2023).

[2]Simultaneously referred to as lock-down measures or stay-at-home orders in the literature.

cratic events – such as threatening COVID-related news – confounding the effects of voluntary and policy-induced distancing. The isolation of the policy effects from voluntary distancing effects is highly relevant for policymakers who will not have control over decentralized, voluntary distancing decisions in future pandemics. This challenge is addressed by leveraging a sharp decline in observable social interactions after implementing the first DPIs.

The second challenge is that the sharp decline strategy is credible only on at least weekly or higher frequencies. Comparable macroeconomic indicators, however, are only available on monthly or lower frequencies. Studies, therefore, either focus on a single economy (mostly the US), such as Gupta et al. (2023), Baek et al. (2021), or Bonaccorsi et al. (2020); or use a high-frequency economic outcome, such as unemployment insurance claims (Kong and Prinz 2020), electricity (Chen et al. 2020), or NO2 emissions (Deb et al. 2022, or Keola and Hayakawa 2021). In this paper, I develop a two-stage empirical strategy that carries the sharp decline strategy over to monthly-frequency macroeconomic indicators in a sample of 44 economies. This paper provides two contributions to earlier studies: stronger external validity compared to single-economy studies and higher policy relevance relative to papers using high-frequency proxies of economic activity.

Economic indicators considered are industrial and manufacturing output, retail trade, consumer price index, the producer price index of manufacturing, and the unemployment rate obtained from the Eurostat. The sample size is limited to 44 countries due to data availability. Distancing and other COVID-related interventions are obtained from the widely used publicly available dataset of Hale et al. (2020). Distancing interventions considered are school closures, workplace closures, gathering limits, restrictions on public transportation, stay-at-home orders, within-country travel restrictions, and cancellation of public events. These indicators are summarized by three DPI indicators, taking into account variations in stringency, timing, and within-country coverage. Social mobility data is obtained from Google's COVID-19 Aggregated Mobility Research Dataset. The intensity of social interactions is proxied by a social mobility indicator measuring the average frequency of carry-on devices in NUTS3 areas in a country within one week.[3]

The estimation is carried out in a two-stage empirical design. The first stage identifies the DPI-induced and voluntary components of social mobility by leveraging the sharp decline in social

[3]Only of those who opted for sharing their location data with Google.

mobility observed as a general pattern immediately after the distancing interventions. The key identifying assumption is that voluntary distancing decisions are uncoordinated and thus aggregate to a continuous country-level trend, identifying the sharp discontinuous decline as DPI effects. This identification strategy recovers the voluntary component of social mobility, which can be carried over to the second stage.

The second stage aims to identify the short-run economic effects of DPIs in a difference-in-differences design, comparing the changes in the monthly economy after the first DPIs to their average paths from five years before the pandemic. Because these relative changes can not only driven by DPI-induced but also by voluntary distancing, the first-stage recovered voluntary component is used as the most important control to separate the voluntary distancing-driven economic effects.[4] The second stage design takes several other potential confounders into account, such as international spillovers of distancing effects and the effects of other COVID-related policy interventions, plus time and country fixed effects.

Results show that the introduction of a single DPI decreased the output of sectors between 0.8 and 2.1 percentage points relative to November 2019 levels. The highest losses were detected in the construction sector. 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. These findings suggest that while DPIs were implemented to contain COVID-19 infections, they imposed substantial costs on economic activity.

When testing the effects of later variations in the types of DPIs a country added or revoked from their first DPI mix, I find no evidence of significant additional economic costs. This result suggests that experimenting with different types of interventions to maximize their virus containment effects is mostly costless. However, increasing the stringency of distancing interventions was found to increase the output costs incurred by the first interventions. This suggests that governments should be much more cautious when intensifying DPIs because it comes at the cost of additional output loss.

I find that unemployment responded only moderately and with a few months delay to DPIs. I find no evidence of short-run inflationary effects of DPIs. Therefore, the results presented in this

[4]Using the sharp decline strategy directly on economic indicators would be straightforward but much less credible considering their monthly frequency.

paper suggest that output losses and unemployment should be the primary concern for decision-makers in the short run when considering distancing interventions in a future pandemic. Finally, it is worth noting that policy action is better to rely on a complete cost-benefit analysis that takes into account the health benefits (identified by Chernozhukov et al. 2021, or Rácz 2023 for example), and the additional costs (such as on women rights studied by Ravindran and Shah 2023) DPIs induce.

The rest of the paper is organized as follows. The end of this section details the contribution to related literature. Data sources, sample design, and variable definitions are discussed in Section 2. The empirical strategy is detailed in Section 3. Results are presented in Section 4, and Section 5 concludes the paper.

Literature. The literature studying the economic consequences of distancing interventions (lock-down measures or stay-at-home orders) exploded after COVID-19 escalated into a global pandemic in 2020. This question has been addressed by several scientific methods, such as epidemiological and macroeconomic modeling and macro and microeconometric methods. This paper belongs to the empirical evaluations of non-pharmaceutical interventions (NPI) during the COVID-19 pandemic, surveyed exhaustively by Perra (2021), Ceylan et al. (2020), and Gupta et al. (2023). Within this literature, this paper contributes to the assessment of the economic costs of NPIs, more specifically, distancing interventions (DPIs). Many papers provided early correlative evidence for substantial economic costs brought about by distancing interventions. 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. 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, whether voluntary or policy-compliant, had significant economic effects. The main contribution of this paper to this set of studies is that it aims at the causal identification of DPIs.

Papers identifying the causal impact of distancing interventions have chosen mainly two alternative strategies: either using high-frequency proxies of economic activity and identifying the effects of DPIs as sharp responses after the interventions, or focusing on a single (or a handful of) economies. Bonaccorsi et al. (2020) developed a similar 'sharp decline' strategy to identify the effects of DPIs from high-frequency human mobility data in Italy. They found that DPIs significantly reduce human mobility. Smolyak et al. (2021) provides further evidence from Italy with similar conclusions. 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. Keola and Hayakawa (2021) and Deb et al. (2022) use the ’sharp decline’ strategy to identify the effects of lockdowns on high-frequency NO2 emissions as a proxy of economic activity. Deb et al. (2022) 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." This study employs a similar ’sharp-decline’ strategy but carries its results over to conventional macroeconomic indicators of lower frequencies. Another important difference is that this study, instead of identifying the impacts of distancing in general, focuses on the policy-relevant fraction of distancing by separating the voluntary components that are out of the control of policymakers.

Studies focusing on a single economy, mostly the US, explore other types of identification strategies, such as difference-in-differences exploiting regional variation in the timing of the interventions. 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, identifies 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 similar 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. There are examples of studies focusing on single countries outside of the US that find similar conclusions, such as Ai et al. (2022) for China, and Juranek et al. (2021) for Scandinavian countries, Maloney and Taskin (2020) of Sweden and Italy, or Ravindran and Shah (2023) in India. This study uses a multi-country sample covering 44 mostly developed economies, thus contributing by external validity to this strand of the literature. This paper 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 considers international spillovers of distancing effects as a confounding factor of NPIs and economic outcomes. Some studies document such spillovers. For example, Boranova et al. (2022), or Inoue et al. (2021). Barrot et al. (2021) find GDP responses to distancing through supply chains.

Finally, another branch of literature assessing the economic impact of distancing measures builds on the widely used compartment models of epidemiology. For example, Kempf and Rossignol (2023), Bodenstein et al. (2022), or Kahalé (2020), Mandel and Veetil (2020). These model simulations all suggest significant output losses due to distancing interventions.

2. Data

Data is obtained from several data sources. Economic outcomes are obtained from the Eurostat. COVID-related interventions and infections data is obtained from the widely used Hale et al. (2020) data collection of Oxford University. Social mobility data is obtained from Google's COVID-19 Aggregated Mobility Research Dataset.[5] International trade data for 2019 is obtained from the OECD. Data on weather conditions is obtained from the National Oceanic and Atmospheric Administration (NOAA).

These three data sources are used to build two different estimation samples on different time frequencies. The first sample is a weekly frequency country-level panel dataset. It is used to identify the voluntary and policy-induced components of social mobility. This sample covers every week between the first week of November 2019 and the last week of December 2020. The second sample is a monthly frequency country-level panel dataset. It is used to estimate the economic effects of DPIs. It spans every month between November 2015 and October 2020.

The estimation samples cover the following 44 economies:

[5]For details see Appendix A.1.

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 sample size is limited by data availability.

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
- the unemployment rate.

The first four measure 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 how much output was lost to distancing policy interventions. Intuitively, service sectors, such as personal services, accommodation, food and drink services, or entertainment, must have suffered the most losses under DPI restrictions. These sectors are ignored because of limited data availability, which is a clear limitation of the paper. Results presented in this paper are recommended to be interpreted as lower-bound estimates of the output effects of DPIs.

The next two outcome variables are the consumer price index (CPI) and the producer price index of the manufacturing sector (PPI). Including these price indexes addresses the short-run inflationary costs resulting from DPIs. Finally, the effects on the unemployment rate address how many jobs were lost to DPIs. Other indicators, such as the output of different service sectors, were ignored due to a lack of data availability.

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 five-year benchmark from the pre-COVID years (2015-2019) of the same indicator, indicated by a thick orange line. All seven indicators are normalized by their values from the latest November. This way, all indicators are adjusted for annual changes and become comparable across years.[6] Observations stay close to their five-year benchmarks before the first intervention in all seven graphs. This observation supports the choice of the benchmark.

The figures also reveal that output declined substantially after the first DPI compared to the benchmarks as a general tendency. In the first month following the first intervention, the average decline was around 20%, 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 primary 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 unsurprising as distancing disrupts supply and demand, which have opposing effects on prices. Therefore, the overall impact 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 slightly 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. The second question is how much of these inflation changes can be identified as the causal effects of DPIs.

Unemployment increased after the first DPI in general, but it converged to its benchmark levels nine months later. Unemployment rates were, on average, 2 percentage points higher than their

[6] Original data is seasonally adjusted for all indicators and is fixed price volume indices for sector outputs.

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?

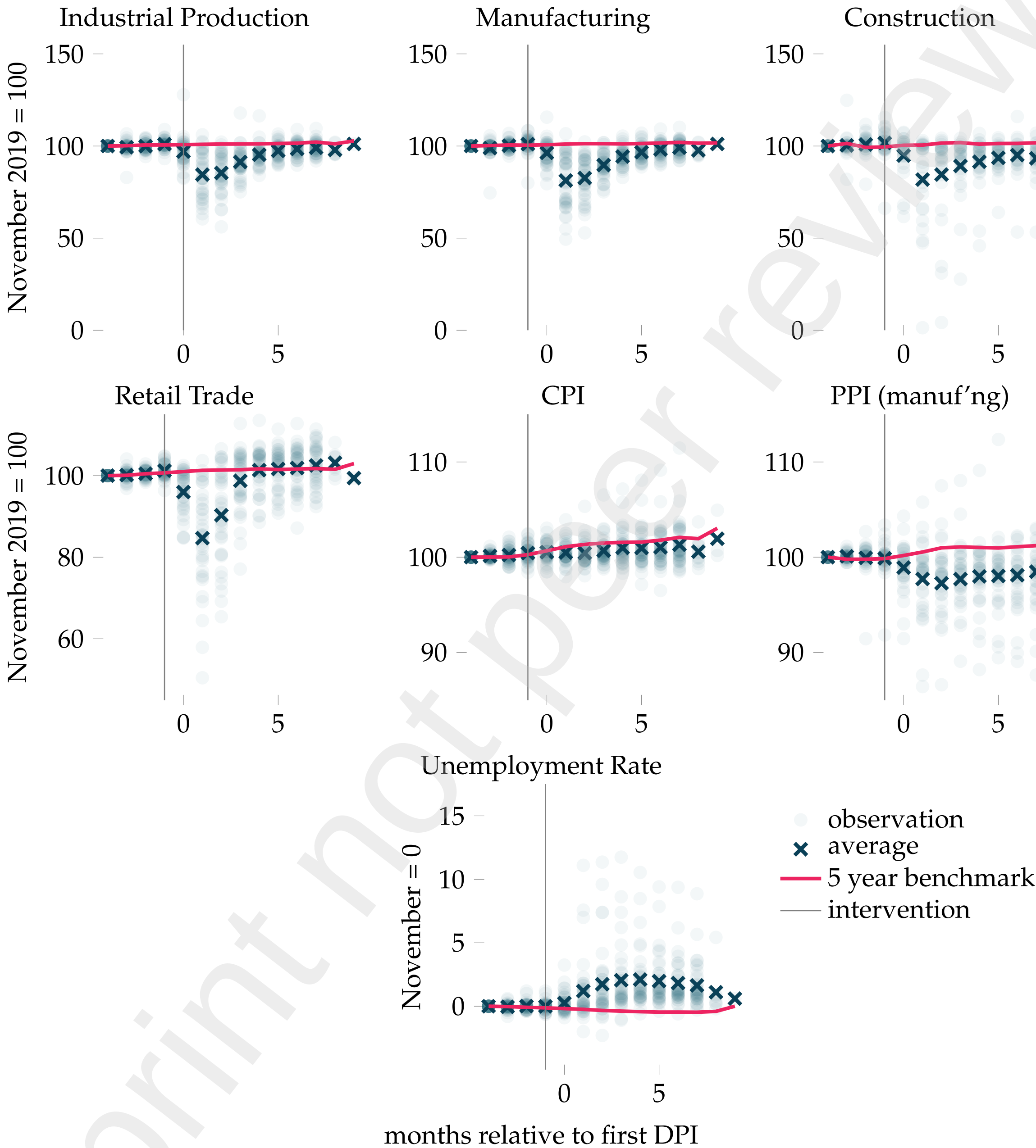

Figure 1: Seven Economic Indicators around DPIs

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

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 a daily basis. I observe seven different types of DPIs:

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

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.[7]

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 could be identified separately. Therefore, I construct an aggregate index of DPIs defined as the sum over all seven types: $\overline{D}_{it} = \sum_j D_{it}^{j}$.[8] This aggregate index takes the value of 3, for example, if there was a level 1 school closure and a level 2 gathering limit (and nothing else) in place in country i during the entire week t. While the individual DPI variables are observed in daily frequencies, $\overline{D}_{it}$ is aggregated to weekly frequency to match the frequency of the rest of the first-stage sample. As a consequence, $\overline{D}_{it}$ is allowed to be non-integer whenever the constituent DPIs change within a week.

I decompose $\overline{D}_{it}$ into three disjoint components. The first component I named as the *treatment* (T_{it}). It captures the first DPIs within a country, with its magnitude remaining constant throughout

[7] 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.

[8] This aggregate index was inspired by the stringency indexes of Hale et al. (2020).

the sample. T_{it} is defined as:

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

that means treatment is by construction 0 before the first ever DPI. It takes the value of $\overline{D}_{it}$ on weeks 0 and 1 of the first-ever intervention, and it is fixed at its first-week value throughout the sample. On week 0, T_{it} potentially 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 the Friday of week 0, $\overline{D}_{i0}$ gets to only 3/7 of $\overline{D}_{i1}$.

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. First DPIs show a substantial heterogeneity across countries in magnitude. Crosses indicate cross-country averages, highlighting the general pattern across countries. It is around 8, revealing that many countries introduced their first DPIs in bundles and started a few already on a level more stringent than level one. This figure also demonstrates the concept of this component being fixed throughout the sample after week 1.

Later deviations in the number or the level of restrictions are absorbed in the other two components: *extensity* (E_{it}) and *intensity* (I_{it}). Extensity captures changes in the number, while intensity captures changes in the stringency level of DPIs after the first intervention. For the formal definition of E_{it} and I_{it} the following two definitions are helpful:

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

where $\mathrm{I}(\ldots)$ is the 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 $L_{it} = 3$,

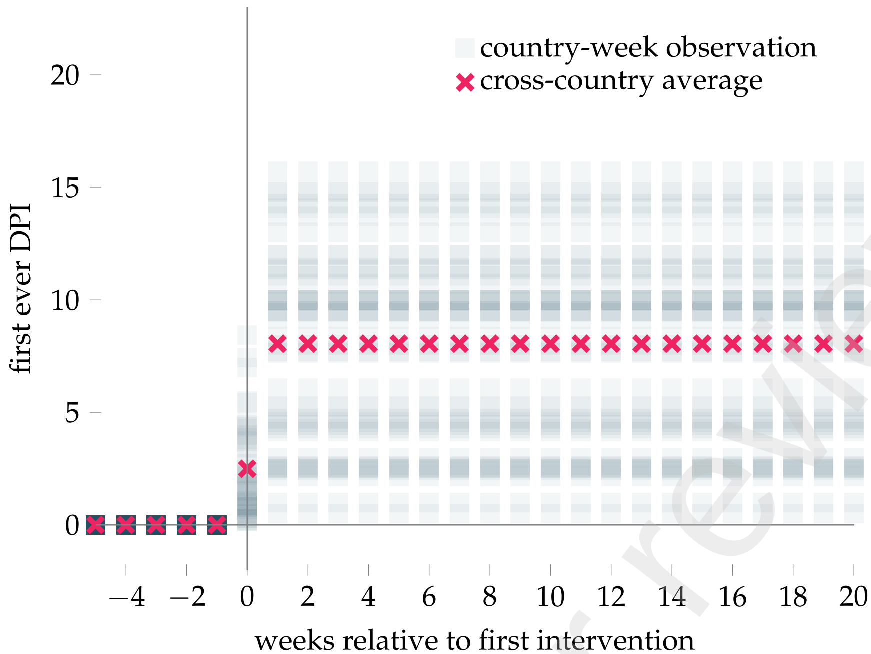

Figure 2: Treatment: T_{it}

Notes: Treatment is the sum of week 0 and week 1 of DPI indicators, holding week 1 values fixed for further weeks. Squares: country-week observations, darker regions show overlapping observations. +: within week averages. vertical line: week 0 of first DPI.

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 recorded by the daily D^j_{it} indicators of the original dataset.

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 grows in the first couple of weeks and starts declining 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 slackening up restrictions after week 10.

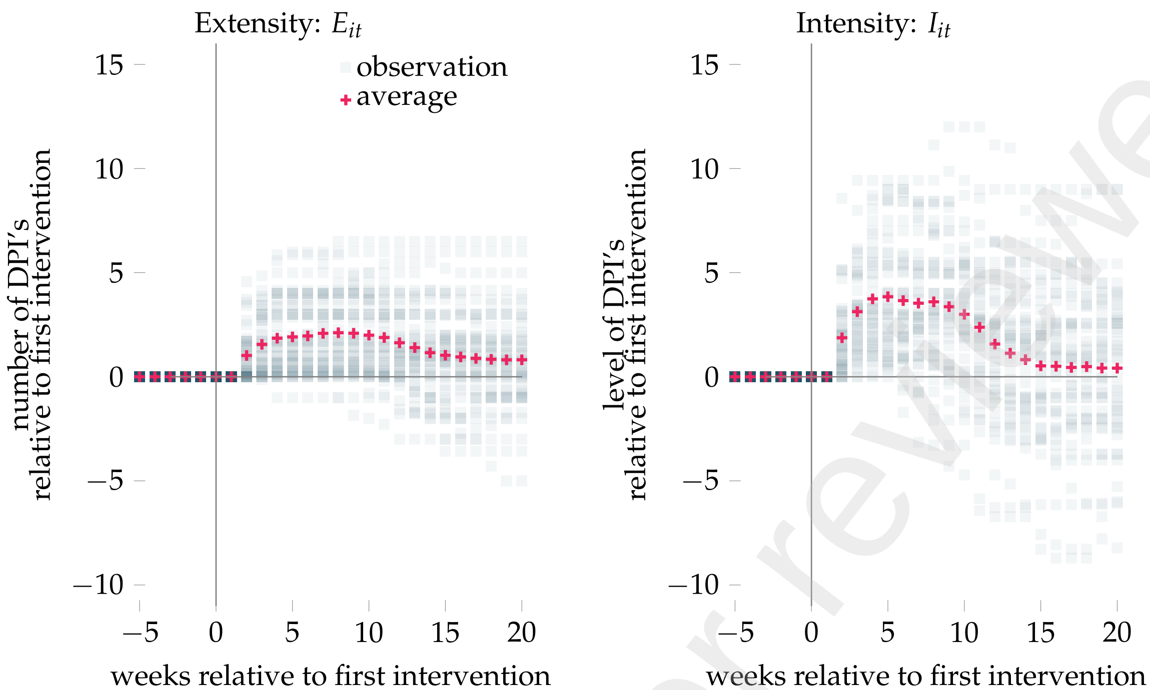

Figure 3: Treatment Dynamics: Extensity and Intensity of DPIs

Squares: country-week observations, darker regions show overlapping observations. +: within week averages. vertical line: shows week 0 of the first intervention.

2.3. Social Mobility Index

Distancing patterns are proxied by a social mobility index (m_{it}) generated using Google's COVID-19 Aggregated Mobility Research Dataset. This dataset provides anonymized records of weekly flows of Google users[9] between NUTS3 areas. This data is available from the first week of November 2019 for every consecutive week until today. The Appendix provides further details regarding this dataset.

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 4 weeks between November 3 and November 30, 2019. This gives m_{it} a unit of percentage deviation from November 2019, consistent with economic outcomes. In the second step, I aggregate these normalized NUTS3 level inflows at the country level by taking their arithmetic mean within a country-week cell. Based on this definition, less social

[9]Only 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, which helps identify when a local business tends to be the most crowded.

mobility (lower m_{it}) indicates more distancing.

Social interactions dropping substantially and suddenly following the first DPIs is a general pattern across countries, as pictured in Figure 4. The figure shows the development of the social mobility index m_{it}. Each circle indicates the average inflow of carry-on devices into NUTS 3 areas within a country on a single week relative to pre-intervention within country averages. This indicator shows the evolution of social behavior when the first DPI was implemented within a country. Darker regions show overlapping observations drawing out the general pattern that social mobility fell by 50 percent in response to the first distancing intervention.

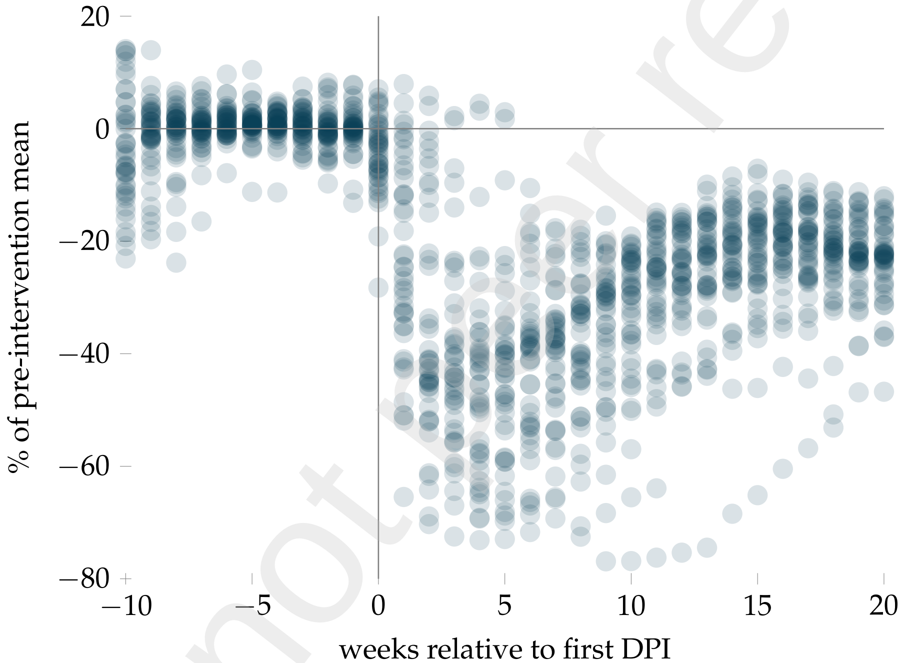

Figure 4: Social Mobility

Each circle indicates the average inflow of carry-on devices into NUTS 3 areas within a country on a single week relative to pre-intervention within country averages; darker: more overlapping observations; vertical line: week of first DPI.

3. Empirical Strategy

The main scope of this paper is to identify the causal effects of distancing policy interventions on selected economic outcomes. This section presents an empirical strategy for identifying these effects. The most prominent empirical challenge is separating the economic effects of DPI-induced and voluntary distancing. 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. The second stage then identifies the causal effects of DPIs from the changes in economic outcomes after the first DPI treatment relative to pre-COVID benchmarks holding voluntary and other potential confounders fixed. The section first discusses the empirical strategy of the second stage, then elaborates on the first stage.

3.1. Economic Effects of DPIs

Identifying the economic effects of DPIs is based on a difference-in-differences approach. They are identified as changes in economic outcomes after the first distancing interventions compared to a COVID-free control period, keeping other confounding factors fixed. This control period is chosen to be the five years preceding the COVID-19 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. The first three variables capture the first DPI intervention (treatment, T_{it}) and further changes in the extensity (E_{it}) and intensity (I_{it}) of DPIs.[10]

X_{it} is a set of covariates that includes all relevant confounders that must be held constant to identify the effect of DPIs: β^T, β^E, and β^I. Potential confounders are tracked down by the Bayesian network of all relevant causal links connecting distancing policy interventions to the economy.[11] Figure 5 shows the Bayesian network. Each arrow represents a causal link. Thick arrows highlight 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 by reducing social mobility, potentially disrupting aggregate demand and supply. It is worth mentioning that DPI effects can be conveyed through two channels. After an intervention, people might reduce their social activity because they comply with the distancing order. Still, they might also cut back on social activities

[10] These three indicators are constructed such that $\sum_j D^j_{it} = T_{it} + E_{it} + I_{it}$, where D^j_{it} is the categorical variable recording a type j (e.g. school closures) distancing intervention. For details, see Section 2.2.

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

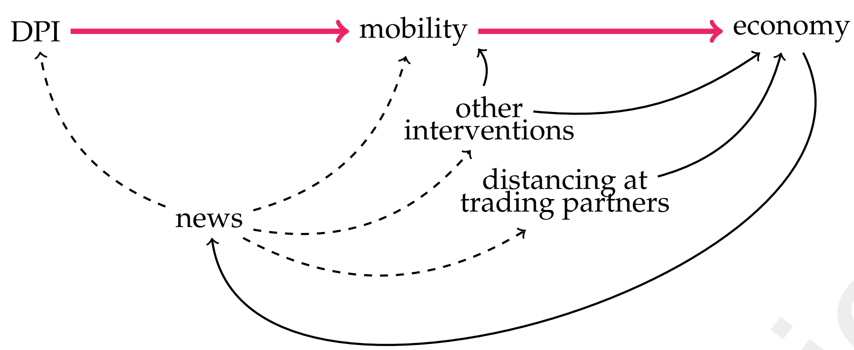

Figure 5: Bayesian Network

Notes: Bayesian network (DAG) of the economic effects of DPIs and potential confounders. Arrows point in the direction of causality. Thick arrows: the path to be identified. Solid line: observed, dashed: unobserved effect.

because they perceive it as a signal of a worsening epidemic. This paper's primary motivation is to inform policymakers about the total effect of distancing interventions, regardless of which channels they are conveyed by. Therefore, the paper intentionally overlooks the separate identification of these two channels.

This network reveals three other paths through which DPIs and economic activities could also be connected. First, DPIs are confounded with social mobility by news, which contains any bits of information about COVID-19 that can alter government and individual distancing decisions simultaneously.[12] For example, the discovery of a large number of COVID-19 infections raises the probability of a DPI and is also likely to discourage people from social activities. Throughout this paper, I will refer to this discouragement effect as *voluntary distancing*.

Second, governments implemented other COVID-related interventions that likely affected 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-19 symptoms, decreasing social activities. Third, distancing in a country impacts domestic markets and can influence trading partners. For example, declining supplies increase import prices. Finally,

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

economic shocks might also have contributed to the news. For example, a negative economic shock 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, local perceptions about the risks of a COVID-19 infection are not recorded in the sample.

To overcome this, I develop a two-stage empirical design. In the first stage, I recover the unobserved voluntary factor of observed distancing behavior. I do that by taking the mobility indicator m_{it}, and separating it into a voluntary and a policy-induced component in a regression discontinuity design.[13] Details of the first-stage empirical strategy are presented in 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 Bayesian network depicted 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} weighted by export and import shares.

Second Stage: Empirical Design. This empirical strategy is formulated by the following equation:

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

where i is a country, and t is a month. T_{it}, E_{it}, and I_{it} are capturing the first DPI intervention and later 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,

[13] m_{it} is derived from Google user mobility data. For details, see Section 2.3.

debt relief programs, international travel controls, and public information campaigns.[14] 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 have the potential to cause omitted variable bias in the coefficients of DPI interventions.

w_{ij}^{X} and w_{ij}^{M} 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 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.

[14]Source: Hale et al. (2020).

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

Table 1: Descriptive Statistics, Second Stage

Notes: 288 country-month observations of 32 countries.

X_{it} contains 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-19 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.[15] The voluntary component, called voluntary mobility, is then defined as the residual of the first-stage regression.

The DPI-induced mobility component 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 and continuous. In contrast, the response to a distancing intervention appears as a sudden discontinuity 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 suggesting 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 also likely that different fractions of society respond with different time lags and intensities based on their different risk assessments of the news. Heterogeneous individual responses tend to aggregate into gradual and smooth (continuous) country-level responses supporting the main identifying assumption of the design. Figure 4 showing the evolution of social mobility provides empirical support for this argument as it shows a sudden drop in social mobility only at the time of the first intervention but smooth changes in other periods.

3.3. First Stage: Empirical Design

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

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

[15] 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).

where δ_t is an event-time coefficient indicating week t after the first DPI was implemented in each country i. It is included to capture the common trend in social mobility around the weeks of an intervention. The effects of the intervention are estimated relative to the final untreated week ($t = -1$), thus δ_{-1} is omitted. 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 treatment.

E_{it}, and I_{it} capture later 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, the 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.

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

Table 2: Descriptive Statistics of Covariates, First Stage

Z_{it} contains 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, a possible confounder 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 second-stage estimations. I start by presenting and discussing the first-stage results. Based on first-stage predictions, I then decompose social mobility into policy-induced and voluntary components. Finally, I present and discuss the main results of 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 the 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 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 before and after the day of the first DPI. These week 0 and 1 effects 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 the effects of more than five weeks after the first DPI, we see a slight reversal of social mobility. This reversal is again attributed to changes in voluntary distancing motives.

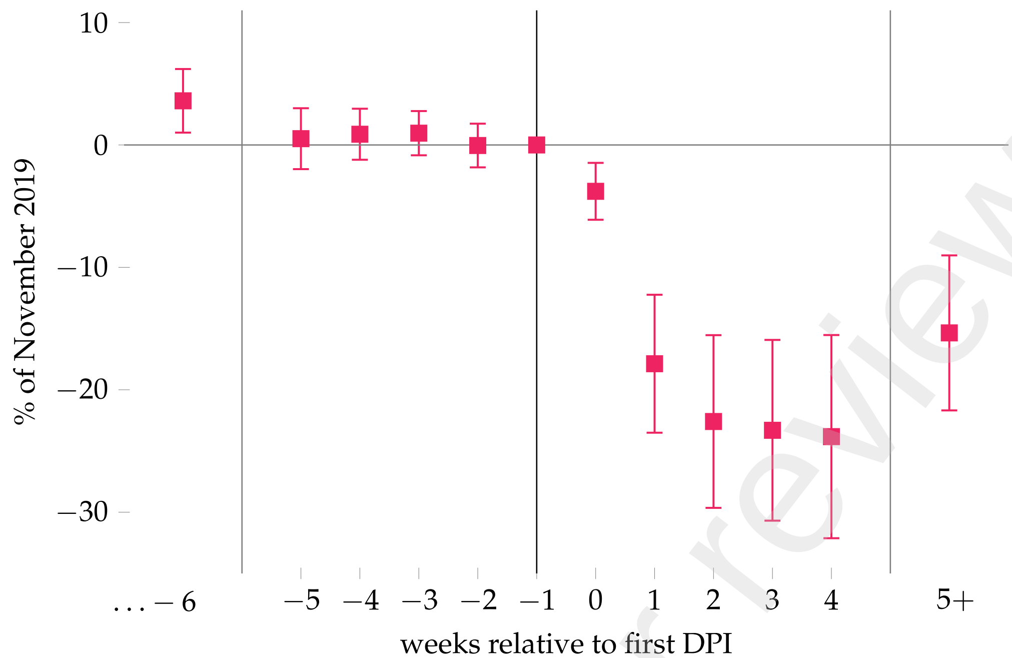

Figure 6: Time Effects on Social Mobility

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

Quantitative results for DPI effects are presented in Table 3.[16] Results are presented for three specifications; the first includes only time effects. The second and third specifications sequentially include extensity and intensity controlling for later changes in the number and severity of DPIs. The top two rows show point estimates for week 0 (δ_0) and week 1 (δ_1), showing standard errors in parentheses. These two coefficients capture the effect of the first DPI. The mobility effects of the first DPI treatment were 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. Given that the average magnitude (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.

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

	(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	•	•	•

Table 3: DPI Effects on Social Mobility

Notes: Point estimates (and their standard errors in parenthesis) of the equation (5) for four selected covariates. Standard errors are allowed to cluster within weeks. *** p<0.01, ** p<0.05, * p<0.1, • – included, ○ – excluded.

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 decreased social mobility by 2.1 percentage points relative to 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. New types of restrictions, however, were found to be ineffective in further enhancing social distancing.

These results suggest that the first-ever distancing interventions have, on average, a strong and significant effect on social mobility. This effect could 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 is beyond the scope of this paper.[17] This paper continues toward its goal of measuring the economic costs of such mobility reductions due to DPIs.

4.2. Voluntary Social Mobility

The goal of the first-stage estimation was to realize a prediction on voluntary distancing because that is a key control for identifying 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 are 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 first-stage point estimates:

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

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 tracks the social mobility indicator m_{it}. Social mobility declined around mid-March, as most countries in the sample intervened for the first time in March 2020. The dominant factor in this decline was the

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

effect of DPIs, although voluntary mobility also made a substantial contribution. Figure **??** 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.

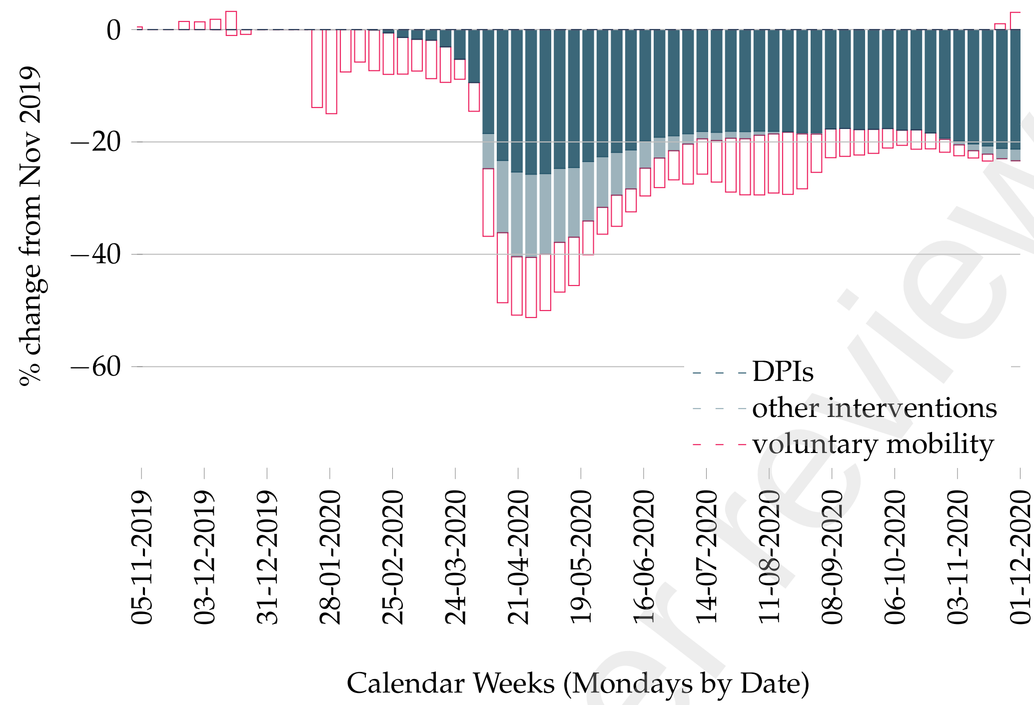

Figure 7: Historical Decomposition of Social Mobility

Notes: cross-country averages of 42 OECD economies. Predicted by specification (3) of Table 3

The first-stage estimation aims to create a proxy for voluntary distancing, an important control in estimating 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, $\widehat{m}_{it}^{V}$, is shown in table 4. Covariates account for 28 percent of the total variance of $\widehat{m}_{it}^{V}$, with fixed effects accounting for another 12 percent. The unexplained component of equation (5) (ν_{it}) accounts for the remaining 60% of its variance, highlighting the importance of unobserved factors potentially confounding voluntary and policy-induced distancing.

4.3. Economic Effects of Distancing Policy Interventions

In this subsection, I present the 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.

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

Table 4: Variance Decomposition of Voluntary Mobility

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.

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, which include voluntary mobility, other interventions, and mobility at trading partners, one by one to investigate the omitted variable bias 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 of only the most complete specification. I start the discussion of output effects by the results for the four sector outputs: industrial and manufacturing production, construction output, and retail trade. I then continue with the analysis of inflationary effects using two price indexes: CPI, and PPI in manufacturing. I conclude the analysis with unemployment effects.

Second Stage Results: Industrial Production. Results for industrial production are presented in Table 5.[18] 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 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 the coefficients of DPIs slightly further, revealing a modest omitted variable bias in previous specifications due to international spillovers. However, the coefficients of these two factors are statistically insignificant.

The most complete specification shows that the first DPIs and further changes in their intensity had a significant effect on industrial production. At the same time, 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. The cross-country average magnitude of the first DPIs was around 8. This result, therefore, translates to a 6.4 percent loss in industrial output due to the typical first-week interventions.

Results also show that a unit change in the intensity of DPIs reduces industrial production by 1.15 percent, while changes in extensity show no significant impact on industrial production. These findings suggest that later adjustments to the severity of already introduced distancing measures could significantly enhance or mitigate the output costs of DPIs while extending the portfolio of distancing measures was a cost-free adjustment. A one-percentage-point decline in voluntary mobility decreases industrial production by 0.4 percent.

Omitted Variable Bias. To highlight the relevance of controlling for the voluntary component of distancing, the most complete specification of the second stage equation (4) was reestimated with all controls except voluntary distancing. The predictions for DPI effects with and without voluntary distancing controls are presented in Figure 8. The solid (teal) line shows the evolution

[18] Estimation results for the rest of the covariates are presented in Section A.3 of the Appendix.

	(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	○	○	●	●

Table 5: Effect of DPIs on Industrial Production

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

of actual industrial production after the first DPI intervention as a percentage of its pre-COVID averages averaged across countries. Industrial production dropped by about 16 percent two months after the first distancing interventions. The orange dotted line shows the prediction of DPI effects from the most complete specification (column 4 of Table 5) of the second stage. It shows that the first DPI mix decreased industrial production by around 10 percent, thus it was responsible for about two-thirds of the observed decline. The red dashed line shows the predictions for DPI effects without controlling for the voluntary component of distancing. It almost matches the total decline of industrial production. The significant difference between the two predictions demonstrates how large the over-prediction of the policy effects can be in a model that omits voluntary distancing. The overestimation of policy effects risks the inefficient use of such policies in future pandemics.

This test, thus, emphasizes the importance of the two-stage design developed in this study.

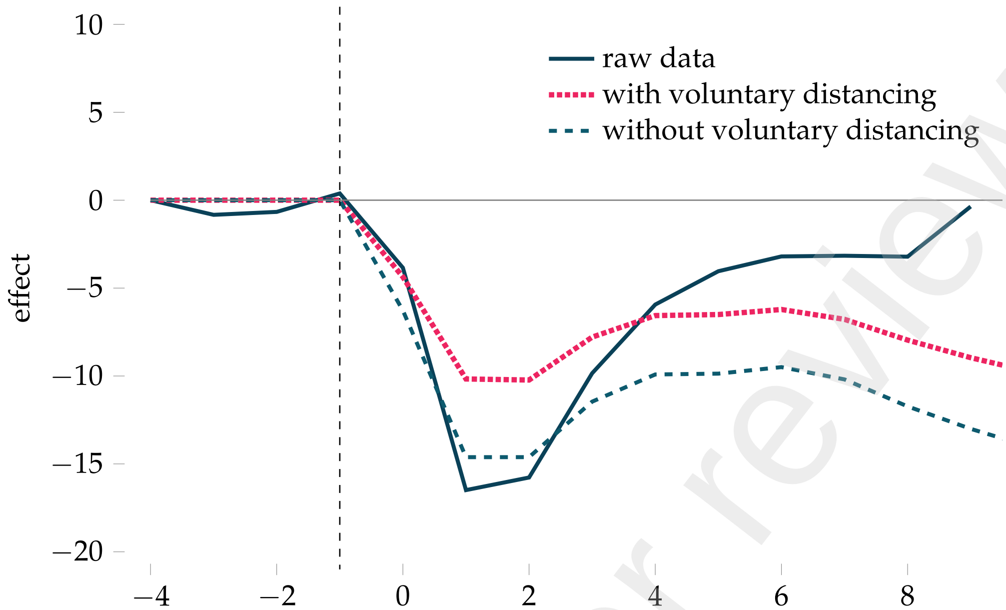

Figure 8: Predicted DPI effects with and without Voluntary Controls

Notes: solid green line: cross country averages; orange dotted line: predicted DPI effects controlled for voluntary distancing; dashed red line: predicted DPI effects without controlling for voluntary distancing, vertical dashed line: last month without a DPI.

Output Losses. Table 6 shows the results of specification 4 for the four different sector outputs. Column 1 repeats the results for industrial production in column 4 of table 5 for comparison. Column 2 shows the effects on manufacturing production. Manufacturing is the largest sub-sector of the total industry sector, and the results are very similar in the first two columns. The first DPI treatment and further changes in DPI intensities significantly negatively affected manufacturing production, but there was no effect from the changes in extensity. Voluntary mobility had a similar impact on manufacturing as 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 smaller set of 21 countries; therefore, its results are not quantitatively comparable with the other columns. Introducing 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

	(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	•	•	•	•

Table 6: Effect of DPIs on Sector Outputs

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 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 decreased 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 spillovers from trading partners' DPI responses. A single level 1 DPI introduced as a first treatment decreases retail trade by 1.2 percentage points. In comparison, 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.

	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

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

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 inconsistent with the first-stage design because the most important policy variable, treatment (T_{it}), is not included. This decision was made in favor of the RDiT design that builds on the key identifying assumption of sudden responses to policy changes. Giving up this design is considered to be a greater cost than the gain from the comparison that a different design would provide.

I work around this problem by picking a different strategy to compare the effects of DPIs and voluntary distancing. 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 (measured by the decline in voluntary mobility), on the other hand, explains only 1.4 percentage points.

The largest negative effect of the first DPI treatment was -27.4 percentage points, and it was identified in the construction sector. 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 contributed the least to total changes. Voluntary mobility decreased retail trade the most, by almost 2 percentage points.

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

[20] As a comparison Deb et al. (2022) find that losses in industrial production were about 10 percent over 30 days following the implementation of containment measures.

Only 72.3% 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 were 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.

Inflationary Effects. Table 8 contains results for consumer prices and producer prices of the manufacturing industry.[21] Effects were found to be consistently negative but statistically insignificant at the standard test sizes. 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 voluntary or DPI-induced distancing effects from domestic markets. On the other hand, distancing in export markets 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, this study could not identify evidence for the inflationary effects of any distancing. This suggests that neither DPIs nor voluntary distancing brings any inflationary costs. 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.

Unemployment Effects. Table 9 presents second-stage results for the unemployment rate in the same four specifications as Table 5 for industrial production. The first specification reveals strong

[21] The rest of the estimation results can be foun in A.3 of the Appendix.

	(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	•	•

Table 8: Effect of DPIs on Prices

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

positive unemployment responses to the first DPI treatment.[22] This strong response is maintained when voluntary mobility is introduced as a control. However, when other COVID interventions are introduced, the coefficient of DPIs collapses and loses significance. Including trading partners (column 4) diminsh the coefficient of the Treatment even further.

These results suggest that the first DPI treatment did not immediately increase unemployment. Thus, rising unemployment rates following the first DPIs must be explained by other factors. First, two of the COVID-related interventions other than direct distancing orders had a positive and marginally significant effect on unemployment: income support programs with less than 50 percent

[22] And a slightly negative response to DPI intensity changes.

replacement rates and debt relief programs. An income support program with a less than 50 percent replacement rate is associated with a 0.45 percentage points average increase of the unemployment rate (with a standard error of 0.168). A narrow debt relief program was found to increase the unemployment rate by 0.59 percentage points (s.e. 0.28), while a broad program by 0.75 percentage points (s.e. 0.34) on average. The average increase in unemployment rates across countries after the first DPIs peaked at around 2 percent, as was shown. These correlations are economically significant. Estimation results for the rest of the covariates, including these figures, are shown in Section A.3 of the Appendix. These findings suggest that insufficient income replacement could not prevent firms from laying off their employees or employees from leaving their jobs. Debt relief, on the other hand, might help people to stick less to their now lower-paying (or not paying) jobs.

	(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 Export Partners				-0.108*
				(0.057)
Mobility $_{t-1}$ at Import Partners				0.008
				(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	○	○	●	●

Table 9: Effect of DPIs on the Unemployment Rate

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

Another explanation is that unemployment responses were delayed. Unemployment rates started to increase only in one month after the first DPIs and peaked around month 4, as has been shown by Figure 1 in Section 2. Firms probably waited with layoffs until the restrictions became permanent and the extent of government support became evident. To test this idea, the second stage equation was reestimated with six consecutive forward values of the unemployment rate on the left-hand side of equation (4).[23] Figure 9 shows the point estimates (and their 95 percent confidence intervals) of the Treatment for these projections. Results show that the effects are already significant and strong in a one-month delay and maintained roughly at the same level until five months when they fall back to zero. These delayed DPI effects are found to be between 0.05 and 0.1 percentage points increase in unemployment rates. These effects are weak compared to the more than 2 percentage point average increase across countries in the first few months.

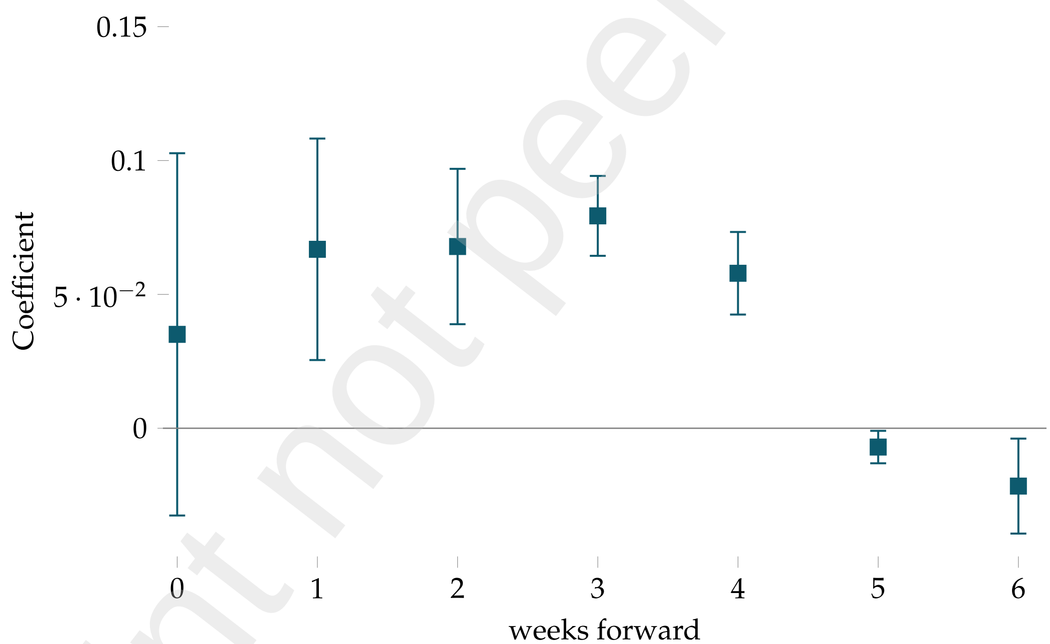

Figure 9: Delayed Unemployment Effects

Notes: Point estimates of β^T of equation (4) by consequent forward values for unemployment ($\widetilde{\Delta} y_{i,t+l}$) with 99% confidence intervals shown around the point estimates with standard errors allowed to cluster within weeks.

[23]This test of dynamic effects employs the idea of local projection models developed by Jordà (2005).

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 behavior 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 also contributed 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, on the other hand, distancing factors altogether predicted more losses than were 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 the inflationary effects of any distancing. This suggests that neither DPIs nor voluntary distancing bring on any inflationary costs. Although a significant hike in unemployment rates can be observed after DPI interventions, 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 for governments planning to implement such interventions during an epidemic. The costs identified here are mainly output losses, while no evidence was found for inflationary costs or unemployment responses. These findings also contribute to a more complete cost-benefit analysis of distancing policy interventions on the cost side. Papers, such as Chernozhukov et al. (2021), or Rácz (2023) found that distancing interventions were an effective containment strategy effectively reducing virus reproduction and saving lives. These two directions of research analyzing the causal impact of distancing interventions outline a policy trade-off decision-makers should consider in a future pandemic. Results already suggest a potential solution to this trade-off as low-intensity measures were already effective in virus containment according to Haug et al. (2020) or Koh et al. (2020); they induced milder output losses according to the results presented in this paper.

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7. Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author used Grammarly Premium to improve the readability and language of the manuscript. After using this service, the author reviewed and edited the content as needed and takes full responsibility for the content of the published article.

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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/pub48778/ 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

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 Neighbors	1.966	2.044	5.517
	(4.821)	(4.207)	(4.573)
Cases $_{t-2}$ at Neighbors	25.366***	24.222***	17.958***
	(6.270)	(6.260)	(6.075)
Deaths $_{t-1}$ at Neighbors	-1,625.307***	-1,538.692***	-1,175.645***
	(356.618)	(346.804)	(294.015)
Deaths $_{t-2}$ at Neighbors	203.146	201.314	214.978
	(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

Table A.10: First Stage Results for Covariates – 1

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

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

Table A.11: First Stage Results for Covariates – 2

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

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

Table A.12: First Stage Results for Covariates – 3

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

	(1) industrial production	(2) manuf'ing production	(3) cons-truction	(4) retail trade	(5) CPI	(6) PPI manuf'ing	(7) 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 Neighbors	0.283**	0.402***	0.075	0.259**	-0.022***	0.006	0.046
	(0.089)	(0.085)	(0.258)	(0.102)	(0.006)	(0.032)	(0.034)
Deaths $_{t-1}$ at Neighbors	-5.005	-6.687	-3.368	-5.061	-0.098	0.792*	-1.160*
	(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 Vaccines	0.350	0.346	2.256	1.663	-0.010	-0.078	0.241
	(0.614)	(0.457)	(1.825)	(1.101)	(0.053)	(0.123)	(0.242)
Investment in Healthcare	0.076**	0.066**	0.332***	-0.016	-0.002	-0.002	0.025**
	(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)

Table A.13: Second Stage Results for Covariates – 1

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

	(1) industrial production	(2) manuf'ing production	(3) cons-truction	(4) retail trade	(5) CPI	(6) PPI manuf'ing	(7) unemployment rate
Income Support Programs							
≤ 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

Table A.14: Second Stage Results for Covariates – 2

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

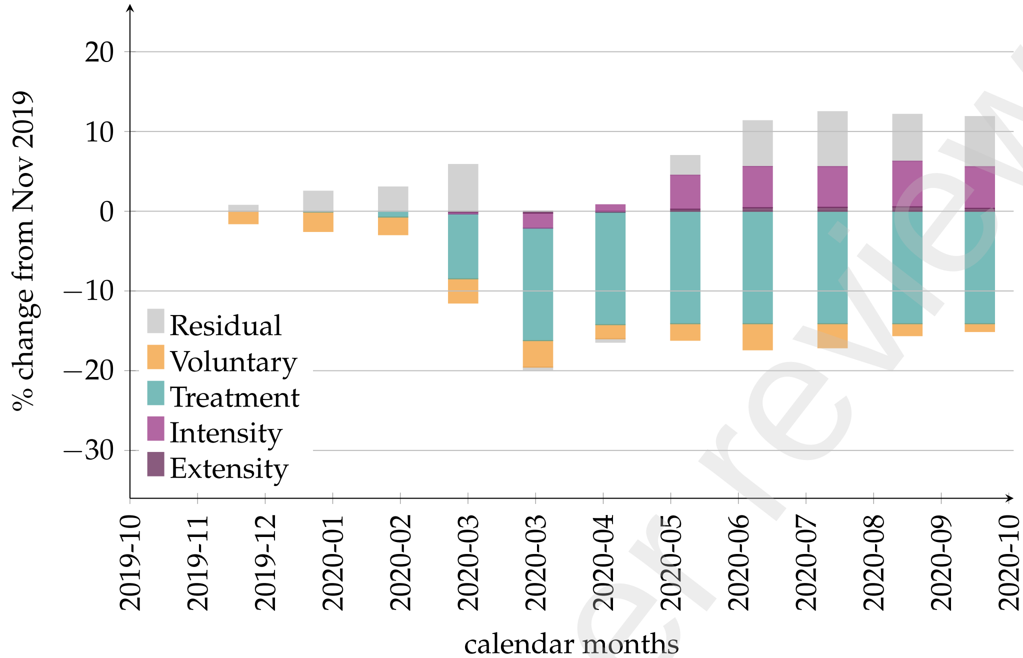

Figure A.10: Historical Decomposition of Industrial Production

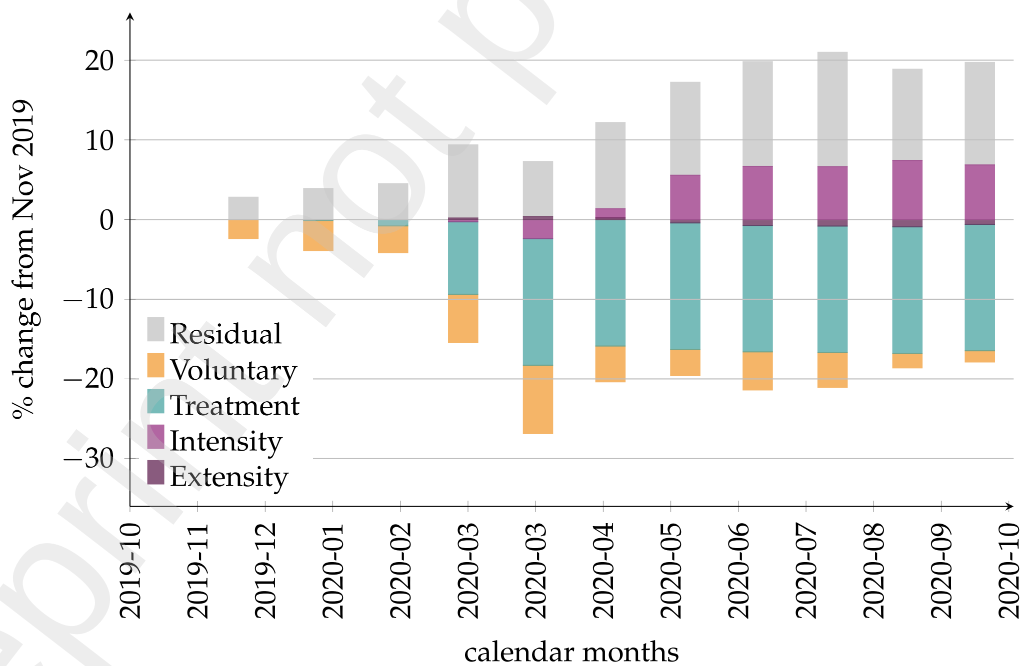

Figure A.11: Historical Decomposition of Manufacturing Production

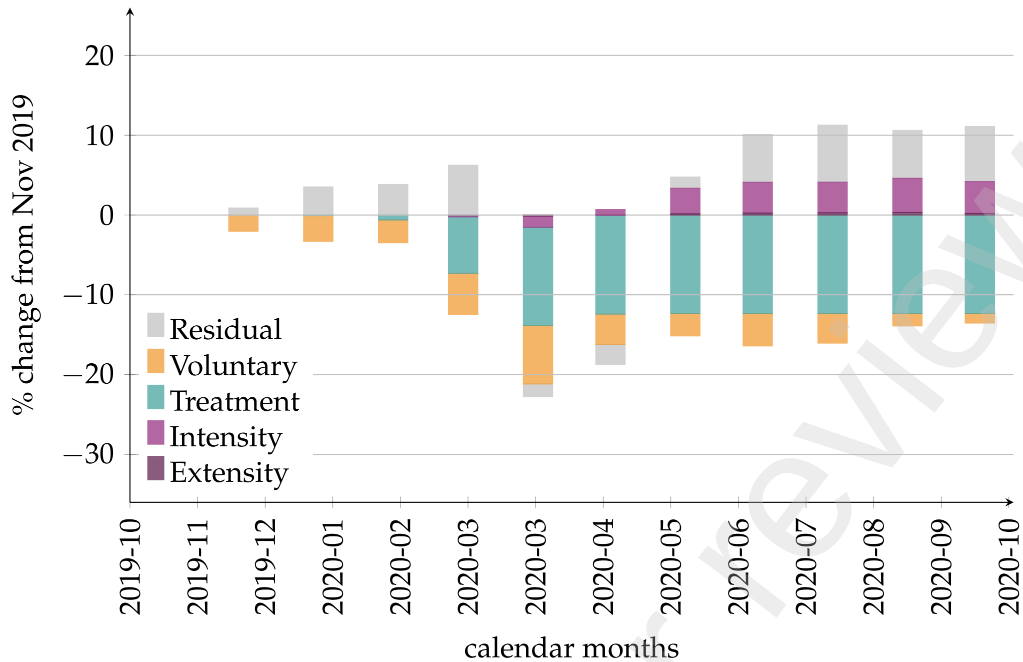

Figure A.12: Historical Decomposition of Construction Output

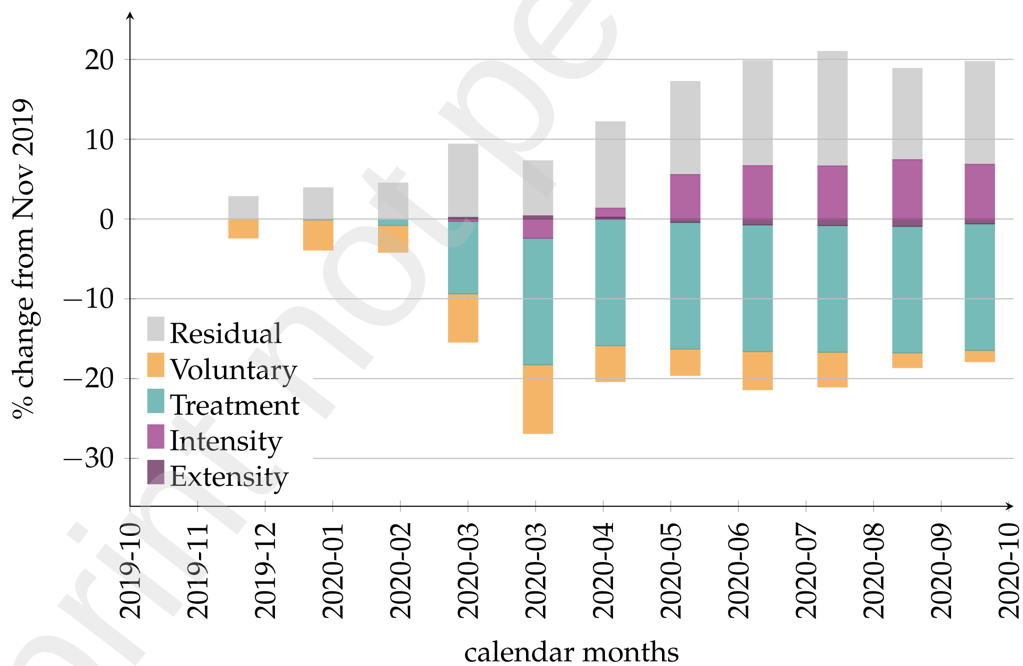

Figure A.13: Historical Decomposition of Retail Trade

A.5. Relevance of Voluntary Distancing

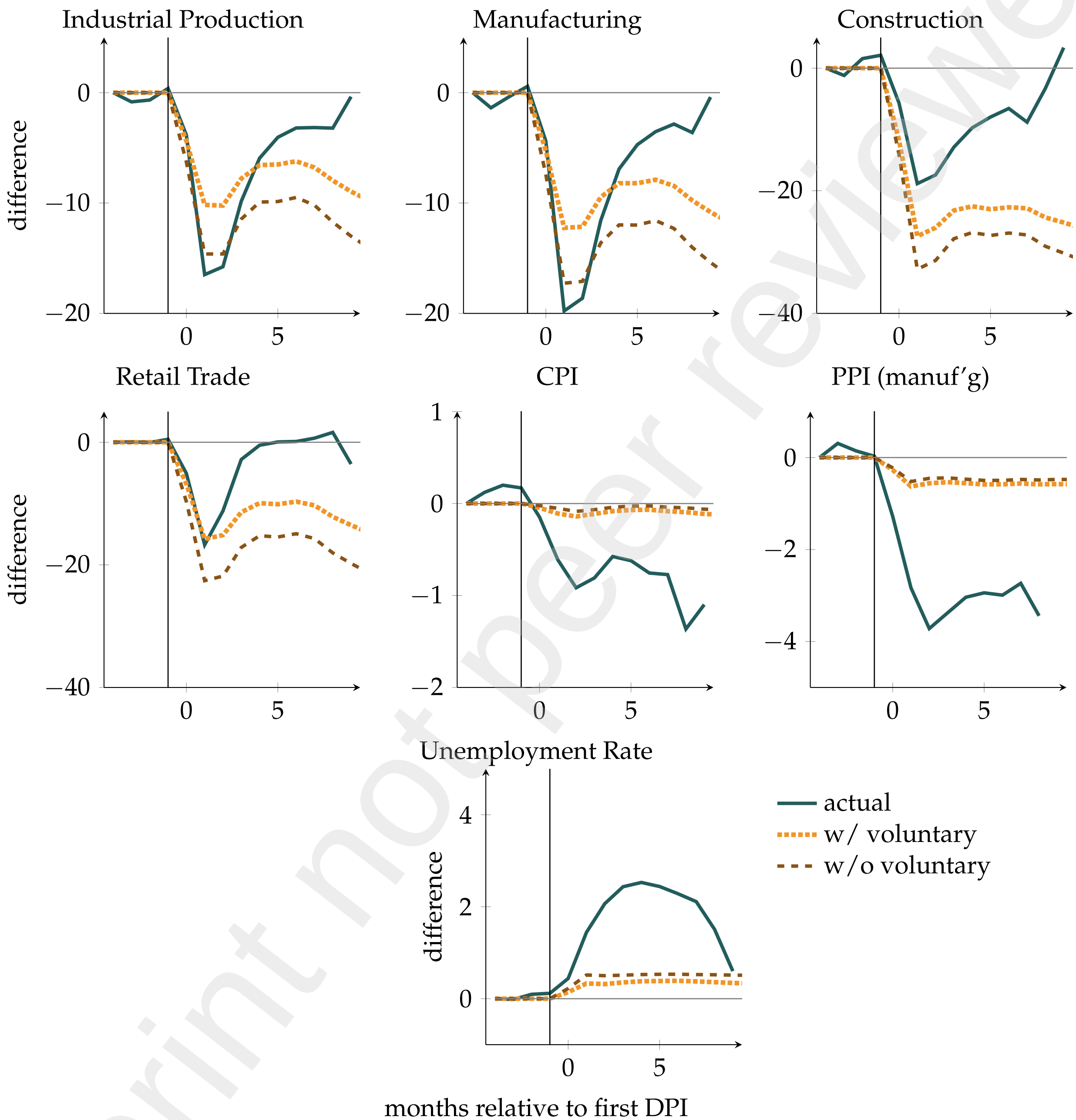

Figure A.14: Predicted DPI effects with and without Voluntary Controls

Notes: solid green line: cross country averages; orange dotted line: predicted DPI effects controlled for voluntary distancing; dashed red line: predicted DPI effects without controlling for voluntary distancing.