The impact of Covid-19 restrictions on economic activity: evidence from the Italian regional system

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Abstract

Non-pharmaceutical interventions adopted by governments to halt the spread of Sars-Cov2 are thought to have non-trivial consequences for the economy. The purpose of this paper is to estimate the economic impact of non-pharmaceutical interventions in Italy, by taking advantage of timing differences in their implementation across regions. To achieve this, we estimate one-way and two-way fixed effects models on a large sample of Italian provinces. We also isolate a set of well-defined natural experiments in which one region goes from a lower to a higher tier of restrictions, while a neighbouring region remains in the lower tier, for which we can estimate difference-in-differences and continuous treatment models. Moreover, in order to observe whether the impact of restrictions has changed over time, we split the sample around December 2020 and replicate the analysis in each subsample. Our case studies indicate that an Italian province moving from tier 2 to tier 3 in the system of restrictions can expect a fall in mobility of between 12 and 18 percentage points. Thus, we provide evidence of the negative effects of non-pharmaceutical interventions on economic activity. Finally, we provide some evidence that the effectiveness of NPIs in reducing mobility is likely to reduce over time, which has important policy implications.

Keywords: Covid-19; Lockdown; Non-pharmaceutical intervention; Mobility.

JEL Codes: C21; C23; H12; I18; R12.

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1 Introduction

Do lockdowns come with economic costs? As outbreaks of Covid-19 infections spread out around the world, governments set up extraordinary measures to tackle their proliferation. Many countries implemented non-pharmaceutical interventions (NPIs), including personal hygiene advice, social distancing, test and contact tracing, restrictions on school and office attendance, the implementation of stay-at-home policies, and travel bans. These restrictive measures are commonly referred to in public as 'lockdowns', although the term is somewhat vague and has been used to represent different policies in different countries. Independent of their epidemiological efficacy, NPIs are thought to have non-trivial consequences for the economy. In their *World Economic Outlook*, published in October 2020, the International Monetary Fund found that countries that adopted severe restrictions experienced larger drops in GDP, even after controlling for the epidemiological situation (WEO, 2020).

As NPIs are thought to have economic costs, a heated debate around the trade-off between 'saving lives and livelihoods' has emerged (Islamaj et al., 2021). The empirical research on this topic faces serious challenges, particularly regarding how to disentangle the effects of NPIs from those of the epidemic itself. Despite the scale of the challenge, several studies have attempted to identify the impact of NPIs on economic activity. The results are mixed, with some pointing to a greater role of individual behaviour, such as changes in consumption patterns triggered by fear of infection or voluntarily compliance with public guidelines (Sheridan et al., 2020; Maloney and Taskin, 2020; Goolsbee and Syverson, 2021; Mendolia et al., 2021; Kong and Prinz, 2020; Chetty et al., 2020; Guglielminetti and Rondinelli, 2021), while others stressing the role of government policies (Caselli et al., 2020; Coibion et al., 2020; Benzeval et al., 2020; Boone and Ladreit, 2021; Kok, 2020). The first position implies that any trade-off between the incidence of Covid-19 and the economy is limited, while the second tends to emphasise the importance of such a trade-off. A second question considered in the literature is whether or not the effects of NPIs have changed over time, perhaps due to 'lockdown fatigue' (Goldstein et al., 2021). There is, in fact, some evidence that the impact of restrictions on GDP declined over the course of 2020 (OECD, 2021).

Most of the existing studies of the economic effects of NPIs are based on cross-country analyses, and often downplay country-specific heterogeneity in policy implementation. However, after the first wave of infections, many countries started to apply regional or locally-based system of interventions, recognizing the need of balancing the public health benefits of these policies with their economic costs. For instance, Boone and Ladreit (2021) consider the possibility that regional heterogeneity in Spain and the UK might have affected their regression estimates based on aggregate national data. Interestingly, the authors do not mention the case of Italy, which was a prominent example of a country adopting rather different systems of restrictions between its so-called first and second waves of Covid-19. In fact, while the first wave was characterized by a national lockdown, the second wave was approached with a regional-based system of interventions that allowed territories characterized by different epidemiological risks to respond with different levels of restrictions. This system, known colloquially as the color-based system, provides an ideal set of case studies to investigate the impact of NPIs on economic activity.

In this paper, we take advantage of timing differences in the implementation of NPIs in Italian regions to estimate the impact of such policies on economic activity. Broadly speaking, we address the following questions:

1. What is the impact of NPIs on economic activity?
2. Does the effect of NPIs change over time?

In answering these questions, we contribute to the literature in several ways. First of all, we contribute to the strand of literature that attempts to estimate the short-term impact of NPIs on economic activity, and shed light on the debate around the trade-off between public health and the economy. Second, we contribute to the literature investigating whether or not the effect of restrictions have changed over time. Third, we contribute to the growing literature that employs high frequency mobility data to evaluate the economic consequences of the pandemic (Deb et al., 2020). Lastly, this paper add to the empirical literature that estimates locally differentiated restrictions on individual mobility by means of sub-national data (Caselli et al., 2020).

To assess the causal impact of NPIs on economic activity, we employ a diversified empirical approach. We first analyze a large sample of Italian provinces on which we run one way fixed effect and two way fixed effect models. Secondly, to account for possible bias of the two way fixed effect estimator, we isolate a set of case studies characterized by well-defined natural experiments in which one region goes from a lower to a higher level of restrictions, while a neighbouring region remains in the lower tier. We estimate difference-in-differences and continuous treatment models on each of these episodes. In line with a large part of the existing literature, we employ mobility data as a proxy for economic activity at province-level, and control for regional and provincial public health outcomes. Finally, we split the sample around December 2020, and replicate the analysis in each subsample. This method allows us to observe whether the impact of restrictions has changed over time.

Our estimates point to a negative and significant impact of NPIs on economic activity. Specifically, our case study estimates indicate that an Italian province moving from tier 2 to tier 3 in the system of restrictions can expect a significant fall in mobility. Moreover, we observe a systematic decline in the magnitude of the impact of NPIs in the second subsample, suggesting that the impact of NPIs does indeed lessen over time. These findings provide evidence of short-term economic consequences of Covid-19 restrictions on economic activity, indicating the need for policy-makers to implement policies to mitigate the effects on employment and income. Our findings also suggest that the effectiveness of restrictions in reducing mobility is likely to fade away over time, suggesting that rolling systems of restrictions are less likely to be effective than shorter, sharper restrictions, although the evidence is less clear cut in this case.

The remainder of the paper is structured as follows. Section 2 describes the epidemiological context and the Italian regional system of NPIs. Section 3 discusses our methodology and section 4 the data. Section 5 presents the results, and section 6 concludes the paper.

2 Epidemiological context and the Italian regional restriction system

The first two cases of Sars-Cov2 in Italy were two Chinese tourists, who were hospitalized and tested positive on the 30th January 2020 (Vicentini and Galanti, 2021). On the same day, the Italian government interrupted flight connections with China, and on the following day, it declared a state of emergency. Between the 21st and the 22nd of February, significant outbreaks were registered in parts of Lombardy and Veneto, and on the 23rd of February,

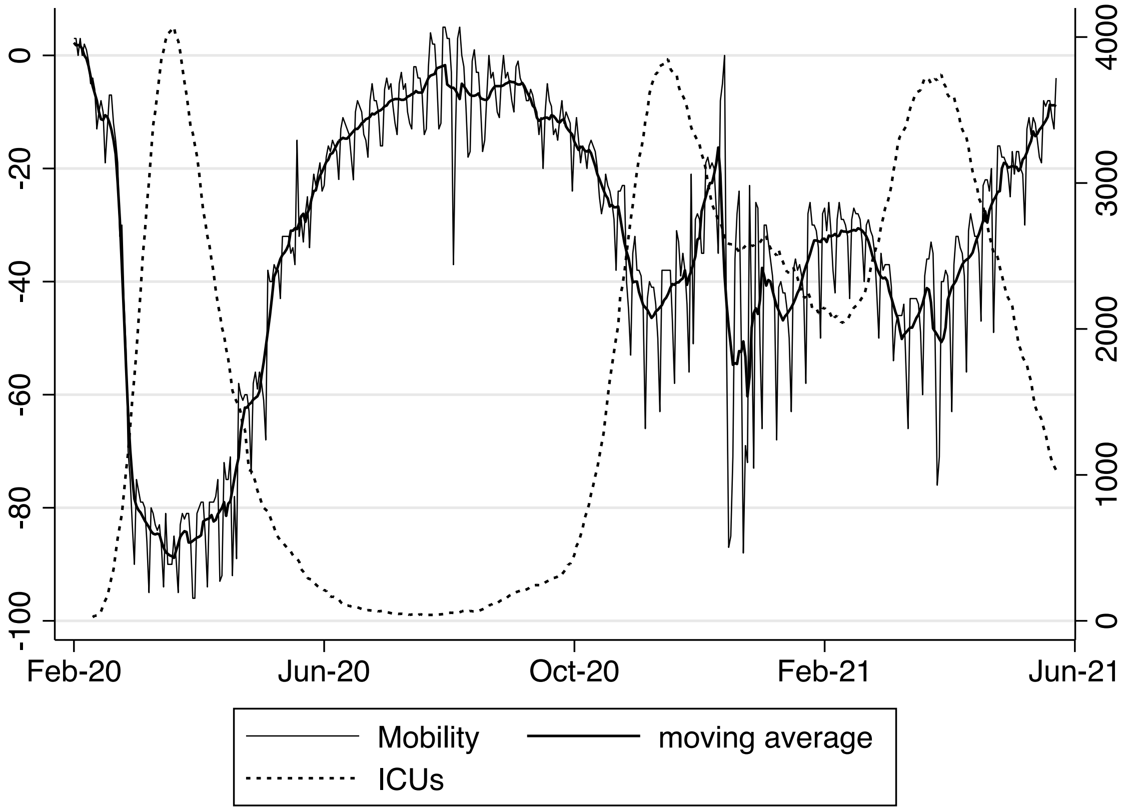

Figure 1: Google mobility index (retail and recreation) and its seven day moving average (left axis), and number of individuals in intensive care units (right axis), between February 2020 and June 2021.

eleven municipalities were put into severe restrictions. By the end of the month, new cases topped 1000 per day, and new measures were put in place to contain the virus on the 4th of March, including closing schools. Ultimately, the government decided to impose a national lockdown on the 9th of March, and was the first to do so outside of China.

The epidemiological situation continued to worsen, and by the 21st of March the daily number of individuals in need of intensive care hospitalization was over 2800. The government imposed further restrictions as a result, and only very essential or strategic activities were allowed to stay open. On the 28th of March daily deaths attributed to Covid-19 peaked at 928 (ISTAT, 2020), and on the 3rd of April hospitalizations peaked with over 4000 people in need of intensive care (see figure 1). From the 14th of April, containment measures were gradually lifted, and with the Prime Ministerial Decree of April 26 the government enacted 'phase-two', which officially ended the lockdown through a plan of gradual reopening of activities (Barbieri and Bonini, 2021).

After a relatively quiet Summer, a new surge in cases occurred in the Autumn of 2020. Initially, between the 14th of October and 5th of November, the extension of restrictions by the Italian government took place at the national level, and included mandatory face masks in outdoor spaces, reduced capacity for recreational venues, and targeted reductions of business opening hours (Manica et al., 2021). From November 6th, however, the government established a three-tiered (and subsequently four tiered) set of restrictions at the

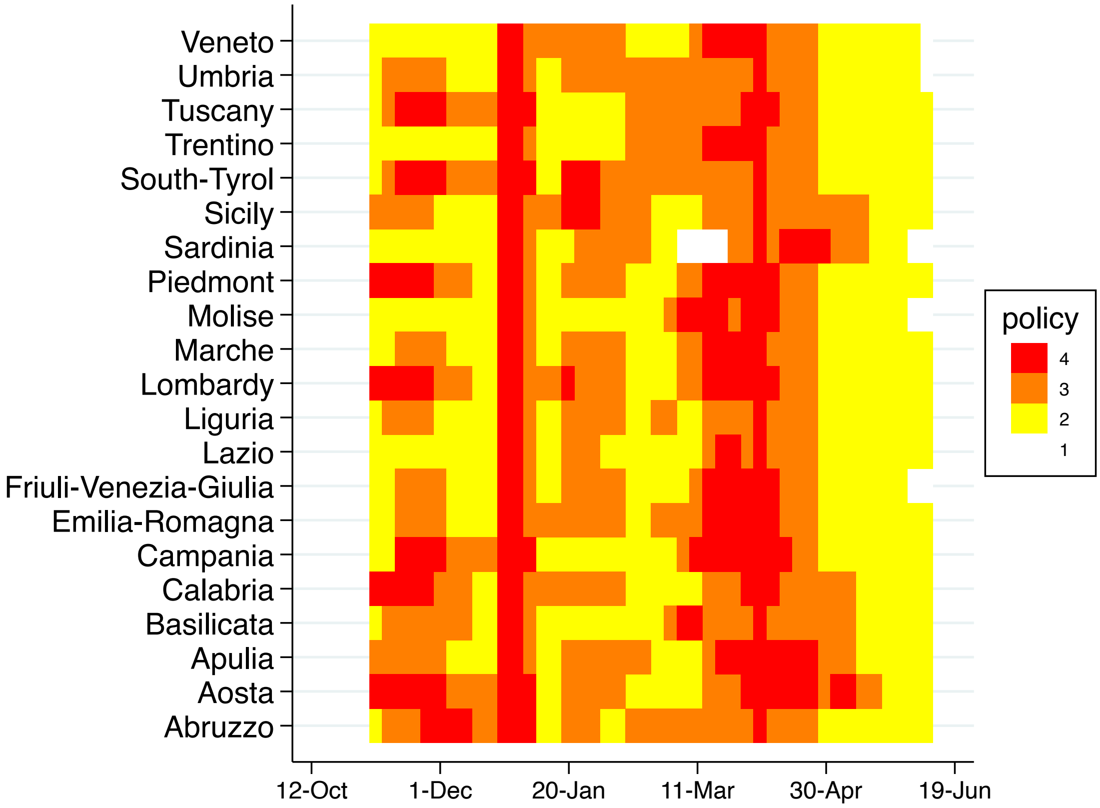

Figure 2: Progression of restrictions in each of 19 Italian regions and the autonomous provinces of Trento and Bolzano between October 2020 and June 2021. White denotes level 1, yellow denotes level 2, orange denotes level 3, and red denotes level 4.

regional level. The tier system was conventionally known as the color-based system, as tiers corresponded to different colors: yellow for low risk, orange for medium risk and red for high risk. Figure 2 illustrates the progression of restrictions in each region from November 2020 up to June 2021.

Although this system was based on local restrictions, the decisions regarding its implementation were taken by the central government. These decisions were based on a number of criteria regarding epidemiological risk, which included recent trends in cases, the number of symptomatic individuals, the number of available hospital beds, and the R number (the reproduction rate). With the decree of the Prime Minister (Decreto del Presidente del Consiglio, DPCM) of the 14th of January, the government introduced an additional parameter consisting of a threshold of 50 cases every 100,000 habitants.

Despite the fact that decision-making was centralized, there were also potential sources of heterogeneity in stringency levels between regions and across time. One source stems from the fact that local authorities enjoyed some degree of discretion, limited to increasing the stringency of government measures. Therefore, while central government could force a region into tier 3 from tier 2, for example, the region could itself decide to impose stricter measures on top of this. This was generally done at province or sub-province level, and there have been a number of episodes in which regions assigned to yellow or orange zones have applied local red zones in some areas. Regions could also increase the stringency of specific rules, for instance, remote schooling. In fact, local authorities had the possibility to impose schools closing also in zones where the general rules allowed for in-presence attendance.

Finally, an additional element of interest regards the time-varying nature of the rules. Overall, the tier system was fairly stable across time. Nevertheless, slight changes that might have had a potential impact on mobility have occurred. For instance, the DPCM of the 14 of January 2021 restricted inter-regional mobility among yellow regions, as well as within-regional and within-province mobility. These potential sources of heterogeneity between regions in apparently equal tiers should be borne in mind in what follows. The key takeaway, however, is that NPIs varied systematically between Italian regions after November 2020, and it is the resulting heterogeneity that we leverage to estimate the effects of Covid-19 restrictions on economic activity.

3 Empirical approach

As discussed in the previous section, we exploit geographic variation in the Italian tier system to identify the effects of NPIs on economic activity proxied by mobility.

One possible model to employ would be a one way fixed effect (FE) model, controlling for time-invariant province specific unobservables and a rich set of epidemiological observables, like the following:

$$Mobility_{irt} = \alpha + \alpha_i + \rho D_{rt}^{orange} + \rho D_{rt}^{red} + \beta' X_{irt} + \epsilon_{irt} \tag{1}$$

Where $Mobility_{irt}$ denotes a mobility index (to be defined in more detail below) for province i in region r at time t; α_i represent cross-section fixed effects. D_{rt}^{orange} is a policy dummy equal to one when region r is subject to tier three of restrictions, and zero otherwise; D_{rt}^{orange} is a policy dummy equal to one when region r is subject to tier four of restrictions, and zero otherwise. X_{irt} is a set of control variables measuring the severity of the Covid-19 pandemic at province, regional and national level. It also includes weekend days dummies to catch seasonality in mobility data.

A second model that we employ is a two way fixed effects (TWFE) with variable treatment timing and intensity, to control for cross-sectionally invariant unobserved time patterns.

$$Mobility_{irt} = \alpha + \alpha_i + \alpha_t + \rho D_{rt}^{orange} + \rho D_{rt}^{red} + \beta' X_{irt} + \epsilon_{irt} \tag{2}$$

Where α_t represent time fixed effects, and X_{irt} only includes Covid-19 control at regional and province level.

If restrictions tiers are conditionally exogenous, given the time-invariant province-level characteristics and observables controls variables, then the FE model should capture the causal effects of NPIs on mobility. If cross-sectionally invariant time-varying unobservables are important, however, then the TWFE model may, but may not, identify the causal effect. This is because our dataset displays a number of peculiar characteristics, some of which have been investigated in the literature as potential sources of bias in a TWFE set-up. In fact, our data is characterized by:

1. Multiple treatments, as each province can be in three different policy tiers;

2. Multiple time periods, as provinces enter different tiers at different dates for different lengths of time;
3. Reversible treatment (or non staggered design), i.e., provinces enter and exit policy tiers;
4. Potential time-variation in the treatment effects.

A growing body of recent literature has highlighted a number of difficulties with the TWFE. In particular, the model is now known to diviate from the canonical 2×2 difference-in-difference (DiD) model in settings with heterogeneous treatment timings and treatment effects, which is often the case in empirical applications. In fact, as shown in Goodman-Bacon (2021), the TWFE yields a variance-weighted average of all possible 2×2 DiD in the data. When in a staggered adoption design units treated at time t are compared with units treated at time $t - 1$, and the treatment determines a change in trend, then the estimator might yield coefficient too small or even wrong signed (Goodman-Bacon, 2021). A similar result is obtained in De Chaisemartin and d'Haultfoeuille (2020), but is generalized for setting in which the treatment might be switched on and off. The authors show that the TWFE estimator is a weighted average of every treatment effect, where the weights sum to one but can be negative even if the ATE is strictly positive. Moreover, de Chaisemartin and D'Haultfoeuille (2020a) extend their results to the case of multiple treatments, reaching two broad conclusions. First, the estimation of individual treatment is contaminated by other treatments. Secondly, the incidence of negative weights can increase with additional treatments.

In combination, these elements rise concerns on the reliability of running a TWFE model on our data. To minimize these concerns, as suggested by Callaway and Sant'Anna (2020), one solution would be to break the sample into all possible episodes for which we can retrieve canonical 2×2 DiD, with a control group remaining in a single tier while a treatment group changes from one tier to the next. We broadly follow this approach with a small number of extra restrictions. Specifically, we select canonical DiD episodes base on the following criteria:

1. A treatment group has to move from a low lockdown tier to a high lockdown tier;
2. A control group has to remain in the same tier;
3. The control group has to be geographically contiguous to the treatment group;
4. The episode has to be repeated in both the November–December 2020 and January–March 2021 subsamples.

The first requirement and second requirements simply define a 2×2 DiD episode that identifies, in principle, the treatment effect of entering a lockdown tier. The third requirement is essentially a 'choice of control donor pool' requirement – geographically contiguous regions are likely to be more similar to each other than more distant regions, and therefore more likely to be suitable controls than more distant regions (see e.g., Abadie, 2021). Finally, the fourth requirement allows us to measure the extent to which the effect of NPIs on economic activity changes over time. To do this effectively, we need to control for unobserved heterogeneity in treatment effects across units, and thus we need to compare the same treatment and control groups at two different points in time.

There are three pairs of regions that satisfy these requirements, thus, we conduct the analysis on these three case studies. The first case involves the regions Emilia-Romagna and Veneto, where the former goes from yellow to orange on the 15th of November 2020, and again on the 21st of February 2021, while the latter stays yellow. The second case involves Toscana and Lazio, with the former going from yellow to orange on the 11th of November 2020, and again on the 14th of February 2021, while the latter stays yellow. The third case involves Lazio as control group and Abruzzo as treated, as the latter passes from yellow to orange on November 11th 2020, and again on February 14th 2021. As is evident from these case studies, a limitation of this approach is that there are no episodes satisfying the conditions above in which a treatment group moves from orange to red. Hence, we focus exclusively on the economic effects of moving from Italy's yellow to orange lockdown tier in our case studies.

For each of these case studies we estimate the following two specifications:

$$Mobility_{irt} = \alpha_i + \alpha_t + \rho D_{rt} + \beta' X_{irt} + \epsilon_{irt}, \tag{3}$$

$$Mobility_{irt} = \alpha_i + \alpha_t + \rho Stringency_{irt} + \beta' X_{irt} + \epsilon_{irt}. \tag{4}$$

Model (3) essentially replicates model (2), a TWFE, in each case study. Model (4) employs as regressor $Stringency_{irt}$, a province level stringency index developed by Banca d'Italia economist Conteduca (2021), which applies a similar methodology of the Oxford Stringency Index of Hale et al. (2020). This index takes into account potential heterogeneity between provinces and across time that might arise as local authorities impose further restriction at province or municipality level, as well as heterogeneity that emerges as characteristics of the tier system change over time. These potential heterogeneities are inevitably omitted in the binary treatment approach.

Finally, we also estimate the following generalized (or event-study) difference-in-differences to measure the post-treatment dynamic evolution of the treatment:

$$Mobility_{irt} = \alpha_i + \alpha_t + \sum_{j=t_0}^{T} \rho_j d_{rt} + \beta' X_{it} + \epsilon_{irt}, \tag{5}$$

in which there is now a sequence of dummy variables d_{rt} that take the value one for a single day in the sample $t = 1, ..., T$, and zero otherwise. Note that, without at least one of coefficients on these dummies restricted to equal zero, the model is unidentified; we return to this point below. Also, by construction we cannot include group (regional) specific variables in model (5).

To validate our empirical strategy, we must be able to motivate the assumption of parallel trends necessary for the standard difference-in-differences model to identify the average treatment effect on treated units. This assumption states that the outcome in the treatment and control groups would have had the same evolution, on average, in the absence of treatment. Hence, any deviation from the trend occurring at the time in which the policy is implemented can be attributed to the policy (the treatment). In a simple 2×2 DiD with pre- and post-treatment periods, evidence in support of the parallel trend assumption can be obtained by visually inspecting the trends prior to treatment. In addition, we have also performed the test for common trends suggested by Mora and Reggio (2015).

Employing DiD methods requires a careful selection of standard error estimators. If heteroskedasticity is rarely an issue for inference (Angrist and Pischke, 2008), our data is likely to feature within-cluster correlation as well as broader forms of cross-sectional dependence, as commonly occurs in spatial panel data. Given that the policy is mostly implemented at regional level (and thus regressors are correlated within regions), if the model fails in fitting within-region correlations then regular estimators will underestimate SEs, even if cluster fixed effects are included (Cameron and Miller, 2015). The ideal solution would be to cluster at the level where the policy is implemented, but this is not feasible in our case studies because we would only have two clusters: the control region and the treated region (Garmann, 2017). Thus, the only option would be to cluster at province-level, which implicitly assumes that errors are uncorrelated between provinces. However, even clustering at province level only implies a number of clusters per case study between nine and sixteen given our sample, which is still thought to be less than optimal (Cameron and Miller, 2015).

In addition, it is also possible that broader forms of cross-sectional dependence might exist in our sample. Specifically, we might expect the existence of spatial correlation between provinces that belong to different regions, particularly as we have imposed geographical proximity as a group selection criteria. Given the foregoing, we estimate our baseline results using Driscoll–Kraay standard errors, which are robust to spatial correlation, autocorrelation and heteroskedasticity (Hoechle, 2007). More importantly, Driscoll–Kraay standard errors were the most conservative from a battery of choices involving clustering at province-level. Effectively, our approach to the choice of SE follows that of Garmann (2017).

4 Data and descriptive statistics

To estimate the impact of NPIs on mobility, we collect data on the 107 Italian provinces from the 28th of October to the 23rd of December 2020, and from the 7th of January until the 15th of March 2021. We have excluded the period spanning December 24th to January 6th, as during this period the regional system was suspended, and national measures were applied for the Christmas holidays. When running our FE and TWFE model we exclude provinces belonging to Sardegna, as it is the only region passing into tier 1 in March 2021. We also exclude the provinces of Aosta, Bolzano and Trento as these represents autonomous administrative entity, and therefore the group level where the policy is implemented coincide with the observational unit level. In addition, the provinces belonging to Piemonte are also excluded because of persistent reporting errors in Covid-19 province cases. However, including Piemonte does not affect our results.

As explained in the previous section, for our DiD specification we only consider 6 specific case studies in which a treatment group goes from level 2 to level 3 (i.e., from yellow to orange) and a control group remains in tier 2. In the reminder of this section, we focus on descriptive statistics for these case studies, as these constitute the weakest identification condition in our battery of empirical approaches.

In principle the sample selection of each individual case study should include a sufficient number of pre-treatment observations for which the common trend assumption holds, and should stop at the time when either the control or treatment group changes policy regime. However, to balance pre-treatment and post-treatment periods, we have restricted the latter to at most three weeks after the policy implementation.

Table 1 provides details on the six specific samples including starting and end date, the date in which the policy is implemented and that when the policy is announced, as well as the date in which the next policy regime change occurs.

Table 1: Sample start and end-points in the six case studies

	t_0	t_1	t_2	T	$T+n$
Veneto - E.R.	Nov 1	Nov 13	Nov 15	Dec 5	Dec 6
	Feb 1	Feb 19	Feb 21	Mar 7	Mar 8
Lazio - Toscana	Oct 28	Nov 10	Nov 11	Nov 14	Nov 15
	Feb 1	Feb 12	Feb 14	Mar 1	Mar 15
Lazio - Abruzzo	Oct 28	Nov 10	Nov 11	Nov 17	Nov 18
	Feb 1	Feb 12	Feb 14	Mar 1	Mar 15

Notes: t_0 is the start of sample, t_1 is the announcement date, t_2 is the implementation date, T is the end of sample, and T+n is the date of the next policy regime change. E.R. stands for Emilia Romagna.

4.1 NPI data

Information on restrictions is taken from the Italian ministry of health. In regards to model (4), we employ a province-level stringency Index developed within Banca d'Italia by Conteduca (2021). This index follows the criteria of the Oxford Stringency Index developed by Hale et al. (2020). The latter is a cross-country and cross-time measure of government pandemic-related interventions, based on a number of standardized metrics such as school closing, stay home requirements and travel restrictions from which the composite index is generated (Hale et al., 2020). Conteduca (2021) improves the Oxford index by taking into account potential asymmetries between regions and across time, as it is built at municipality-level and then aggregated up to province-level.

Figure 3 shows the evolution of the average stringency index by province around the date of the policy implementation in each case study, and reports the minimum and maximum values. In all cases but one, the index displays an identical pattern for treated and control group before the implementation. There is also little apparent variation in restrictions among provinces, with two noticeable exceptions in panel B and F. In fact, in the latter two cases there is significant variation in stringency between provinces after the implementation, which is due to region-specific restrictions being imposed at province-level. For example, when the Italian government imposed the orange tier restrictions for the region of Abruzzo in February, the regional authorities imposed local red zones in the province of Chieti and Pescara from February 14th (Regione Abruzzo, 2021). This is exactly the kind of heterogeneity that is ignored in a binary treatment model.

4.2 Mobility

We obtain mobility data at province-level from the Google Mobility Report, which aggregates anonymized information regarding trends in visits to categorized places from Google

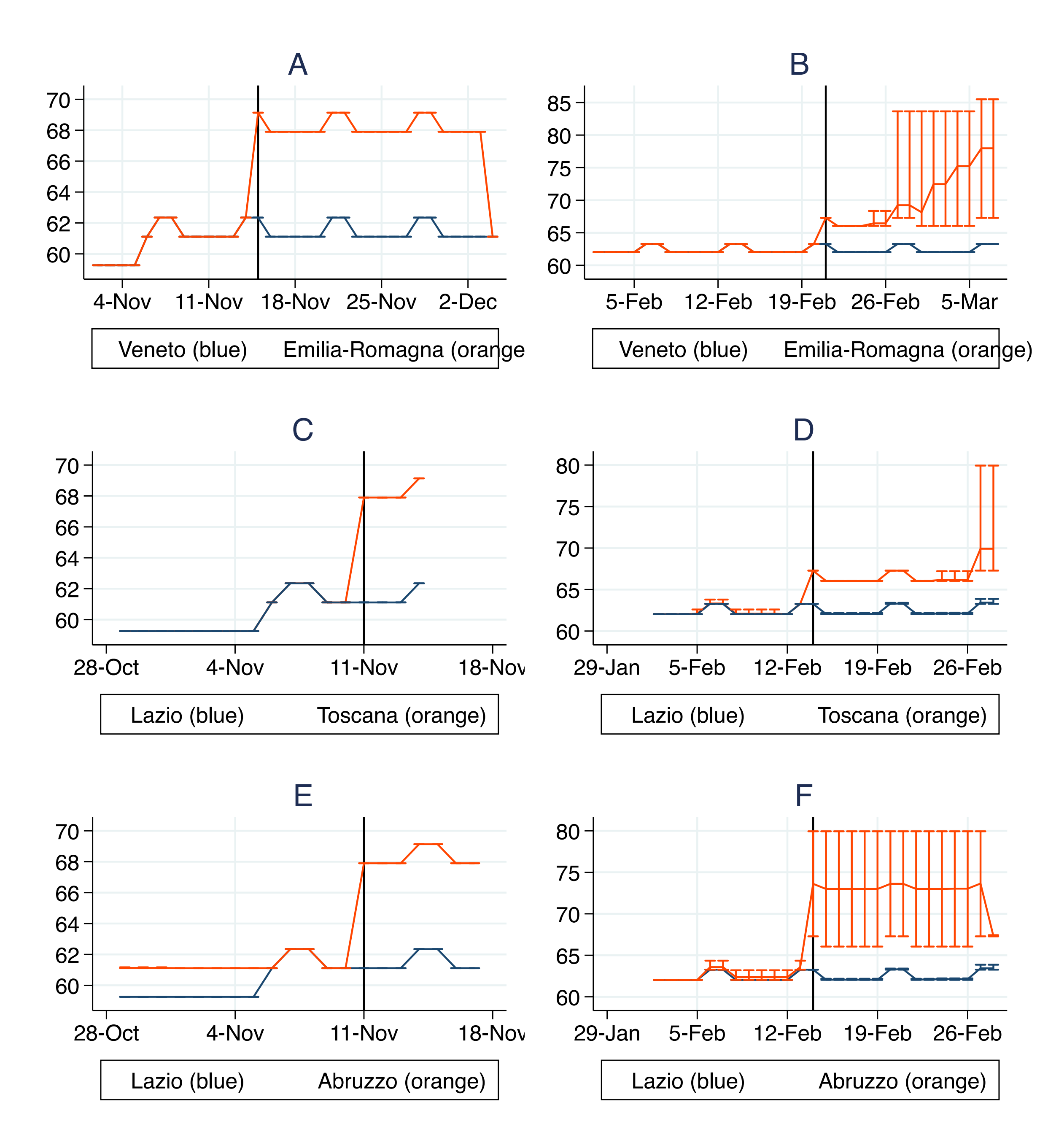

Figure 3: Government Stringency Index. The blue and red lines represent respectively the mean value of provinces belonging to the control group and treatment group. Bands are given by the minimum and maximum value. The black vertical line indicates the treatment date.

users that have opted in using their 'location history' setting [1]. The data are expressed as percent deviations from baseline values that represent norms for each day of the week. Specifically, the baselines are the median values over the five-week period spanning 3rd January to 6th February 2020, i.e., just prior to the onset of the pandemic in Europe. These data are classified into five categories: retail and recreational places, groceries and pharma-

[1] for a detailed explanation of the data aggregation process, see Aktay et al. (2020)

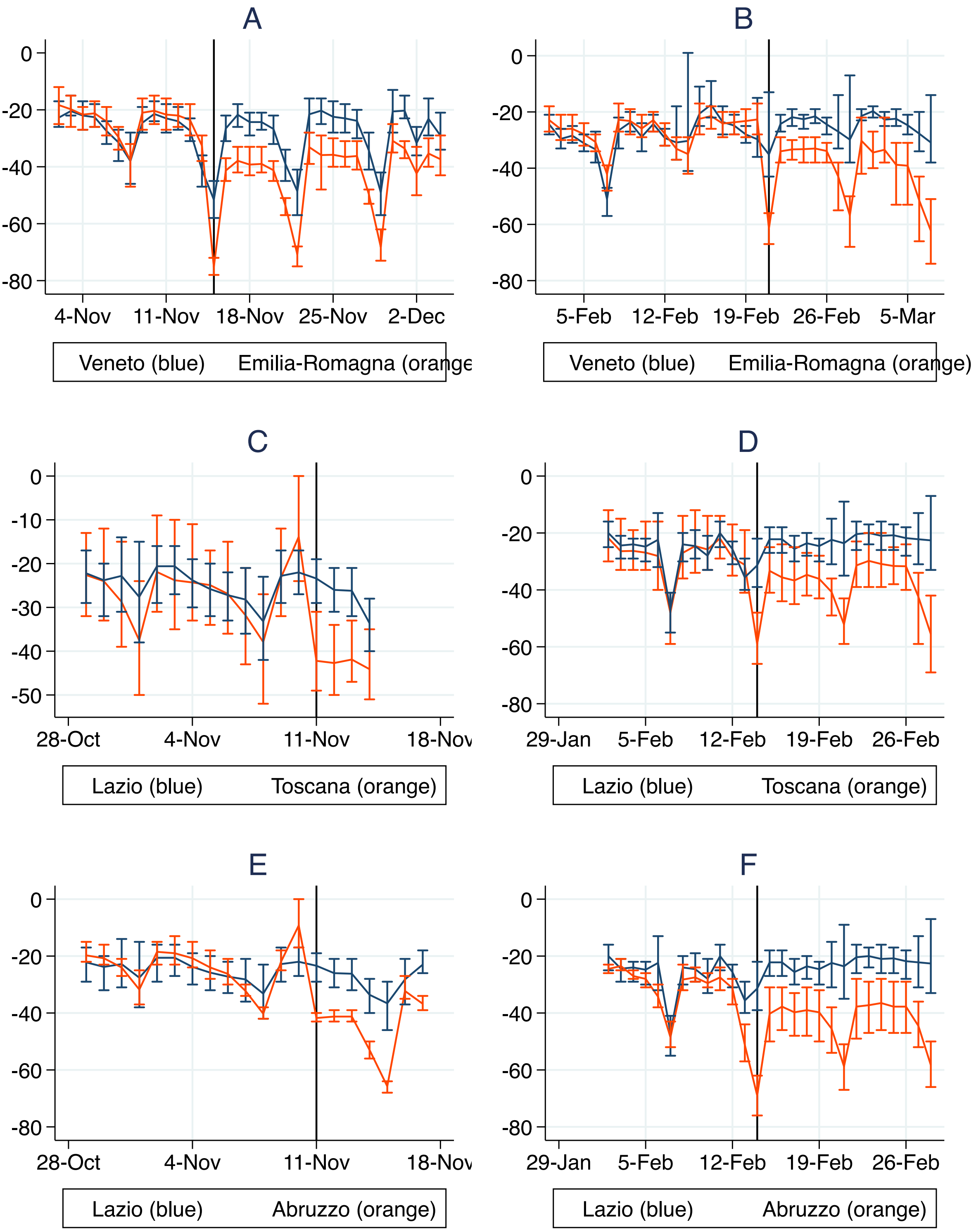

Figure 4: Google Mobility Index. The blue and red lines represent respectively the mean value of provinces belonging to the control group and treatment group. Bands are given by the minimum and maximum value. The black vertical line indicates the treatment date.

cies, parks, transit stations, and places of residence. We use visits to retail and recreational places as our benchmark measure, since it is likely to best capture the effect of restrictions targeting non-essential activities, which was an extremely common feature of most countries' NPIs and of the Italian regional system.

To understand these data, suppose that the retail and recreational places mobility index is equal to minus 10 on a given day in the province of Rome. This means that visits, meaning the number of trips made by Android devices with an activated History Location setting, to recreational places (including restaurants, bars and retailers) in Rome are 10 percent below their pre-pandemic median value for that specific day of the week.

Mobility data have two key advantages for our purposes, namely, that they are available at very high frequency and at a granular level, which makes them particularly suitable for an investigation of the impact of NPIs. In fact, mobility data have been widely used in the economic literature on COVID-19 (WEO, 2020; Deb et al., 2020; Boone and Ladreit, 2021; Buono and Conteduca, 2020). Importantly, the OECD has found strong correlations between mobility, GDP and private consumption in a large sample of countries, with the index able to explain around 75% of cross-country variation in consumption (OECD, 2020). The implication is that mobility is correlated with economic activity, e.g., visits to retailers are correlated with consumption expenditure. In this regard, Buono and Conteduca (2020) employs Google visits to retail and recreational places as proxy for consumption. On the other hand, it is important to note that reductions in mobility will overstate the economic impact if consumers maintain the same level of spending in a smaller number of trips (Goolsbee and Syverson, 2021), and mobility data cannot reveal potential substitution effects occurring as consumers switch from in-person to on-line shopping.

Figure 4 shows the evolution of the benchmark mobility index in the six case studies around the date of the implementation. This figure helps us to visually investigate the existence of common trends in the pre-treatment period. In all cases, the evolution of mobility in the control and treatment group coincides very closely in the pre-treatment periods, and then decouples when the policies are implemented. In same case, for instance in panel F, the decoupling of the two lines appears to begin at the time when the policy is announced. As announcements might play a role in the overall impact of NPIs on activity, it is investigated in our analysis. Allowing for this, however, figure 4 is strongly suggestive that common trends existed in the pre-treatment periods in all of our case studies.

4.3 Epidemiological data

Finally, as NPIs are implemented based on regional epidemiological data, we control for the extent of the Covid-19 pandemic, including a set of variables at the regional level that includes Covid-19 related deaths, cases and ICUs. To account for the impact of voluntary social distancing or fear of infections, we also employ the number of daily new Covid-19 cases per 100.000 people at province level. The latter is obtained as the first difference of cumulative cases, which is the only series available at province level (Morettini et al., 2020). Unfortunately, there are no time series available regarding hospitalizations and fatalities for most of the Italian provinces. In addition, province data for certain units and certain date display reporting errors, as the first difference in cumulative cases delivers negative numbers. In some cases such errors are corrected by extracting the relevant information from local authority sources. In other cases where is not possible to retrieve the information or where data revision occurs, the negative values are substituted with zeros. Finally systematic

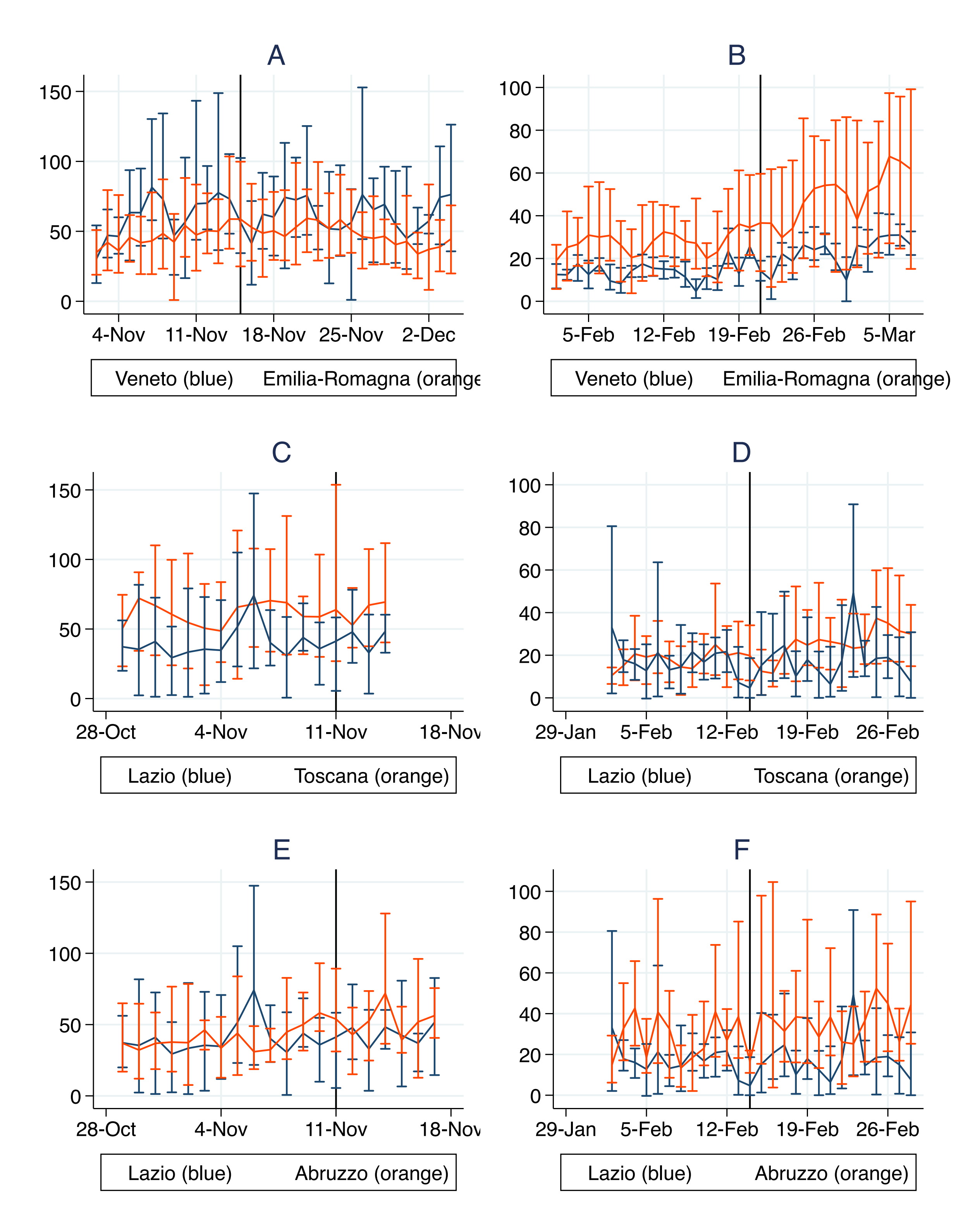

Figure 5: Province COVID-19 cases per 100.000 people. The blue and red lines represent respectively the mean value of provinces belonging to the control group and treatment group. Bands are given by the minimum and maximum value. The black vertical line indicates the treatment date.

reporting errors appear in the series regarding the provinces of Piemonte. Specifically, as these data differ from local authority sources for long periods, we have excluded the region from our full sample specification, with no consequences on our results. Aside from these corrected observations, all the Covid-19 data are taken from Morettini et al. (2020). When we run the regression on the full sample using model (1) we also include a set of Covid-19 controls at national levels, employing cases, fatalities and ICUs. Including national level epidemiological data helps to catch potential correlation between national epidemiological development and local mobility.

Our case studies baseline specifications include seven day moving average of daily province cases per 100.000 people, and seven days lag of the logarithm of regional fatalities. The latter is selected among a richer set of transformed series of regional data included in model (1), as it appears to be the most significant region-level predictor. However, we explore alternative specifications in the online appendices. Figure 5 shows the progression of mean new cases per 100.000 people at province level within our estimation samples in each case study. Figure 5 indicates no obvious connection between surge in infections and policy adoption.

5 Results

5.1 Full sample estimation

We first report the results based on model (1), a FE model, and (2), a TWFE model, in the entire sample of regions. As explained above we split the estimation for the periods November-December 2020, and January-March 2021, to test whether the effect has changed over time. Figure 6 displays the point estimates and confidence intervals of the policy dummies. The entire regression output can be found in table 2 in the appendix. The point estimates represent the effect of NPIs on our Google Mobility benchmark measure, that is, deviations from the pre-pandemic median in visits to retail and recreational places. We can immediately notice that the point estimates are all negative and significant. Moreover, the FE and TWFE models deliver very similar coefficients, particularly when estimating the impact of tier 3 on mobility. The estimates indicate that passing into tier 3 triggers a decline in the mobility index of about 13 p.p. in the first subsample, and 7 p.p. in the January-March period. Instead, the impact of tier 4 on mobility is between -20 p.p. and -22 p.p. in the first period, and between -14 p.p. and -17 p.p. in the second subsample. Therefore, these figures suggest that NPIs negatively impact mobility, even after controlling for the local epidemiological situation. Also, based on these results the impact of restrictions appear to have declined in the second period of analysis, and the difference between the coefficients in the two subsamples is statistically significant.

Table 2 reports also the coefficient of the moving avarage of new cases per 100.000 people, our proxy for individual voluntary social distancing or fear of infection. The estimates are all negative and precisely estimates, suggesting that for every new case per 100.000 people, the expected mobility index decreases between -0.08 to -0.2 percentage points. In addition, the magnitude increases in the second period. This suggest that epidemic-related individual motives are also an important predictor of mobility, particularly in the second subsample, although we have not attempted to identify the causal effect associated with increases in the severity of the pandemic.

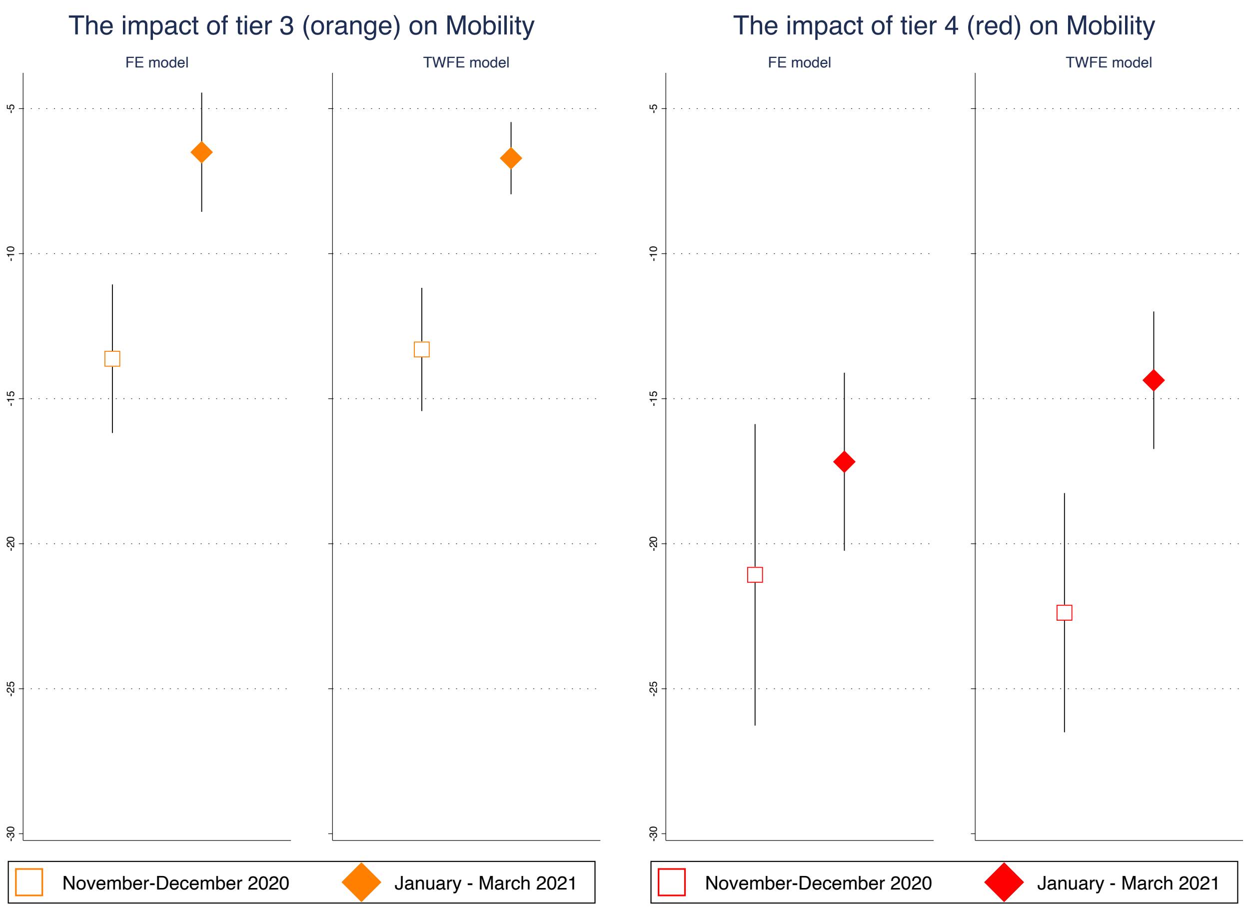

Figure 6: The impact of NPIs on province Google mobility Index estimated with FE and TWFE models. The figure is based on the estimation reported on table 2 in the appendix.

5.2 Case studies

5.2.1 Specification 1: binary treatment

We turn now to our case studies analysis, and report the dynamic DiD results obtained by estimating model (5) on each case study. All point estimates are expressed as percentage point differences of the mobility index with respect to one day before the announcement of new restrictions. The dynamic plots allow us to verify the presence of common trends in the pre-treatment period.

Figure 7 confirms what was suggested in figure 4, as all plots show close-to-zero coefficients prior to the implementation date, followed by significantly negative coefficients after the implementation date. Furthermore, we have tested the common trend assumption via the test proposed in Mora and Reggio (2015). We fail to reject the null of common trend in five out of six cases. The exception is the Veneto and Emilia-Romagna case study in the second period, but this result appears to be driven by one or two pre-treatment observations within a very long pre-treatment period with no obvious secular trend.

The policy effects appear to be heterogeneous across regions, and are not constant within regions over the treatment period. Nevertheless, the estimated effects of NPIs on mobility are clearly large and negative, and appear to be larger in the first period than the second.

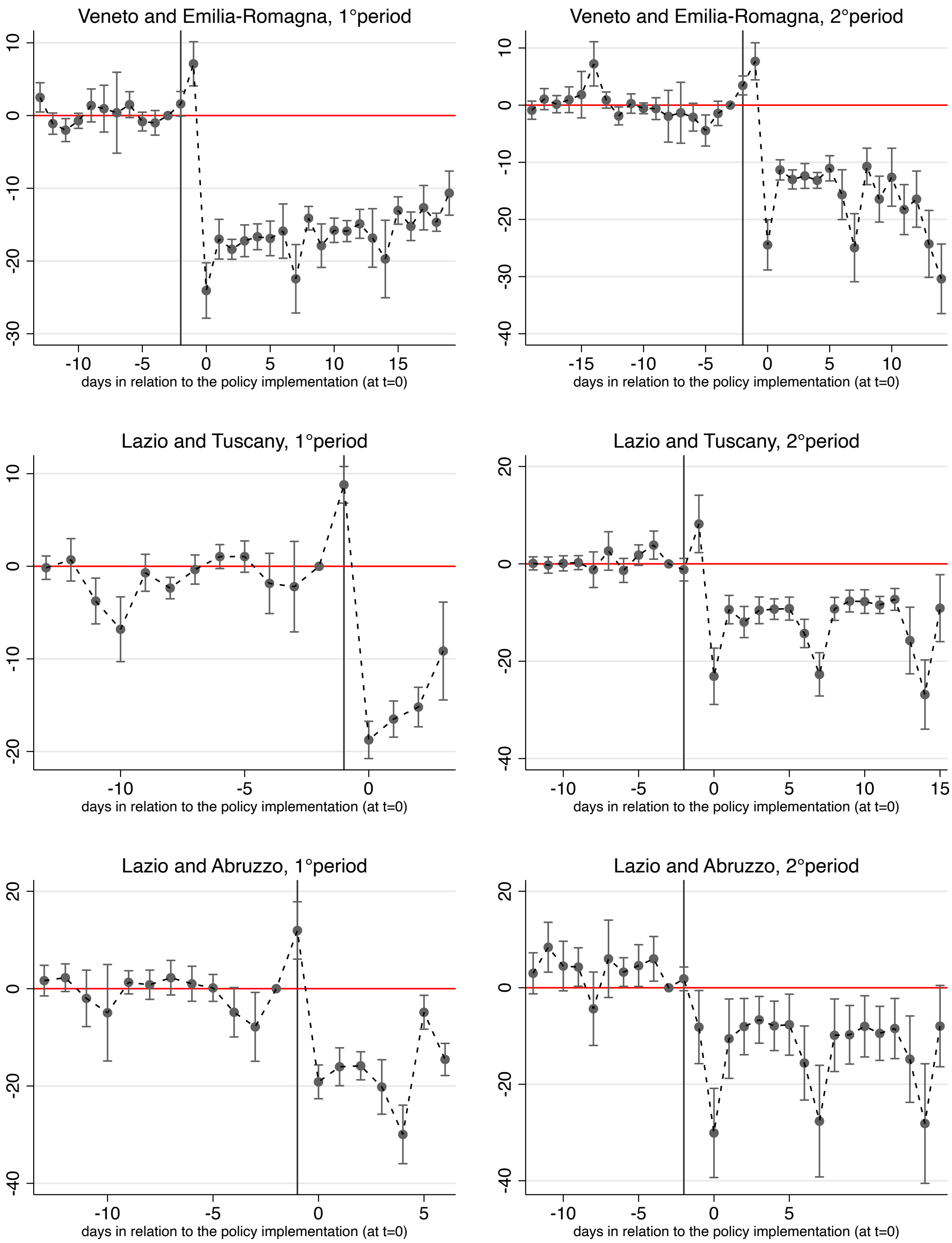

Figure 7: Lead and lag effects of NPIs on mobility. The horizontal axis represent the days in relation to the policy implementation ($t = 0$). The vertical black line represents the day in which the policy is announced.

Among other things, these results indicate that treatment effects are heterogeneous across time and among groups, supporting our choice of isolating a set of well defined 2×2 DiD experiments to test the robustness of model (2) in the entire sample.

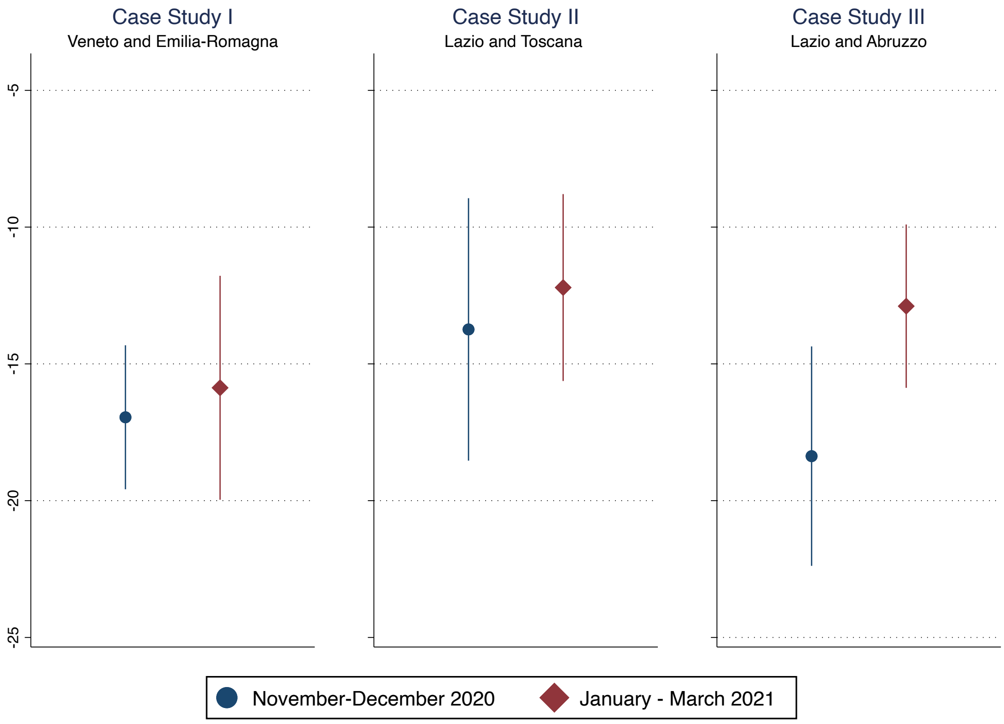

Figure 8: Estimated impact of NPIs on mobility. The figure is based on the estimation reported on table 3 in the appendix.

Another key takeaway from figure 7 is the presence of positive announcement effects, characterized by unusually higher mobility in the day before the implementation. This result does not come as a particular surprise, as individuals anticipating upcoming restrictions adjust their mobility behavior, for instance, by concentrating consumption before business closures. The only exception appears to be the case of Lazio and Abruzzo in the second period.

Figure 8 summarises the dynamic DiD results in figure 7 using the static DiD specification from model (3). The full regression results are presented in table 3. As before, The point estimates represent the effect of NPIs on our Google Mobility benchmark measure, that is, deviations from the pre-pandemic median in visits to retail and recreational places. As we can notice from figure 8, the estimated coefficients are negative and significant, with their magnitude somewhat heterogeneous between cases and ranging from -18 p.p. to -12 p.p.. Nevertheless, it is possible to observe a decreasing effect in the second sub-sample in each of the three case studies, which suggest that the impact of NPIs on province mobility has decreased over time, confirming our findings obtained on the full sample of regions. However, the difference between the two periods is not statistically significant. Finally, we can compare figure 8 with figure 6. It emerges that our case studies estimates are broadly in line with the coefficients obtained in the FE and TWFE in the first period. However, the case studies estimates are significantly higher than the full sample coefficients in the second subsample. Nevertheless, these higher estimates could be explained by the trimmed post-treatment samples in two case studies.

Table 3 in the appendix reports the full regression output of running model (3) in each case. In panel A the model is regressed assuming the treatment starts on the implementation

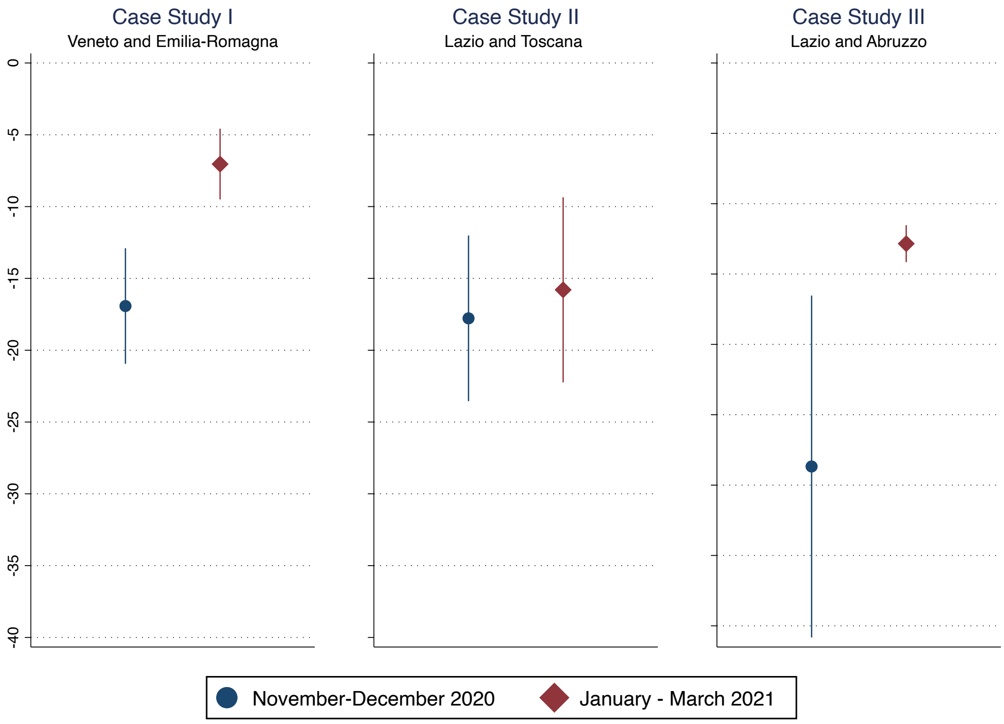

Figure 9: Estimated impact of NPIs on mobility. The figure is based on the estimation reported in table 4 in the appendix.

date. The estimated impact of NPIs on mobility in the first and second subsample is about -17 p.p. and -16 p.p. in the first case study, about -14 p.p. and -12 p.p. in the second case while it is -18 p.p. and about -12 p.p. for the third case study.

In panel B the models assumes the treatment starts at the announcement. As a matter of fact, the positive announcement effect observed in figure 7 are expected to partially offset the negative impact occurring after the policy is implemented. For instance, if announcements induce consumers to stockpile purchases before restrictions are implemented it impacts the overall economic performance. In fact, panel B delivers somewhat different results from panel A. In the first case study the point estimates are about -15 p.p. and about -12 p.p., which are lower than panel A but still decreasing over time. In The second and third case the coefficients are also lower but we do not observe a decreasing impact over time. In fact for Lazio and Toscana the coefficients are -8 p.p. in the first period and -10 p.p. in the second one. For Lazio and Abruzzo the point estimates drops to about -13 p.p. in the first subsample, while in the coefficient is approximately unchanged at -13 p.p. in the second period, indicating absence of announcement effects. Thus by including the announcement effects in the regression we find evidence of a decreasing impact of NPIs over time only in the case of Veneto and Emilia-Romagna.

Table 3 reports the estimates of our province level Covid-19 control, that is, the moving avarage of new cases per 100.000 people. Contrary to our full sample results, the proxy for individual fear or voluntary social distancing is significant only in three case studies, but the estimates are in line with our previous findings.

5.2.2 Specification 2: Continuous treatment

We now turn to our continuous variable specification, i.e. model (4). To compare the estimates of the latter with those obtained with the binary variable specification, we have computed in each case study the marginal difference between the sample mean stringency index for treated and untreated units. The results are summarized in figure 9 and are broadly in line with those estimated with the binary specification. In fact, the estimates point to a significant decline in mobility in each of the six cases as well as suggesting a decreasing impact over time. Also in terms of magnitude the coefficient are mostly comparable with those of figure 8. Major differences in the magnitude of the estimates are the second period in the first case study and the first period in the second case study. As outlined above, by including the stringency index we take into account potential heterogeneity in regards of NPIs severity between regions and over time that are necessarily omitted in the binary specification.

Table 4 contains the full output of the regressions employing the stringency index. The estimated coefficients of NPIs in the first case study indicate that one percent increase in stringency is associated with a drop in mobility of respectively -2.3 and -1 in the first and second subsamples. In the second case study the point estimates are -1.9 and -1.6, while in the third case is -2.5 and -0.9.

6 Conclusion

In this paper we find evidence that NPIs implemented by the Italian government through its regional based system, are associated with a significant decline in province mobility, even after controlling for region specific epidemiological data. We also detected announcement effects prior the implementation of the policy which partially offset the negative decline in economic activity but also have import health implications. In fact, the spikes in mobility around the announcements are likely to be correlated with declining social distancing in contrast to the purpose of the policy. In comparison with available literature, our estimates suggests a stronger impact on mobility than those obtained by Caselli et al. (2020) that, through a fixed effect model, estimate a drop in mobility of 7% in Italian municipalities that experienced the very first wave of restrictions in march 2020. By employing a stringency Index designed on Italian restrictions, our estimates suggest that a 10 p.p. increase in stringency is associated with a decrease of mobility that ranges between 9 p.p. to 23 p.p.. We can compare these estimates with those obtained by Boone and Ladreit (2021). In their cross-country regression of the second wave of COVID-19 they estimate that in Italy a 10 p.p. increase in the stringency index is correlated with around 4 p.p. decline in mobility.

We also investigated whether the impact of restrictions changes over time. Previous literature has underlined that the efficacy of restrictions is likely to be correlated with the duration of the measures, as fatigue motives kicks in over time (Goldstein et al., 2021). By estimating the models in two subsequent subsamples we find consistent evidence of declining impact of NPIs on mobility. However, when we take into account the announcement effects in our case study analysis, the declining impact appears only in one case. Nevertheless, some of these results are affected by the need of balancing pre and post-treatment period, which reduced the latter in some cases yielding higher estimates in the second subsample.

To conclude, our findings suggests that NPIs have non trivial economic costs independently of the specific epidemiological context. Thus, policy makers need to balance economic harm with health costs when implementing NPIs. Moreover, our findings indicate that the efficacy of NPIs is likely to decline over time, suggesting that rolling systems of restrictions are less likely to be effective than shorter lockdowns.

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Appendix

Table 2: One way and two way fixed effect models

	(1)	(2)	(3)	(4)
	DEPENDENT VARIABLE: Mobility			
NPIs (dummy = 1 if Orange)	-12.73***	-13.05***	-6.314***	-6.554***
	(1.232)	(0.984)	(0.931)	(0.588)
NPIs (dummy = 1 if Red)	-20.39***	-22.04***	-17.07***	-14.41***
	(2.421)	(1.879)	(1.480)	(1.191)
m.a. cases per 100.000 people	-0.0846***	-0.0903***	-0.213***	-0.230***
	(0.0114)	(0.0109)	(0.0302)	(0.0329)
1 day lag of log of ICUs (Region)	-0.969	-0.345	-4.199***	-5.289***
	(2.515)	(1.595)	(1.268)	(0.797)
7 days lag of log of ICUs (Region)	-1.645		-4.140*	
	(2.683)		(2.455)	
7 days lag of log of ICUs (Region)	0.0623		1.939	
	(2.307)		(1.865)	
1 day lag of log fatalities (Region)	-0.295		-0.204	
	(0.502)		(0.333)	
7 days lag of log fatalities (Region)	0.199		0.0744	
	(0.377)		(0.312)	
14 days lag of log fatalities (Region)	0.116		-0.0742	
	(0.339)		(0.352)	
m.a. fatalities (National)	-0.0391***		-0.0362***	
	(0.0133)		(0.00987)	
m.a. delta ICUs (National)	-0.0380*		0.00476	
	(0.0214)		(0.0412)	
7days lag of growth of cases (National)	-1.417**		-0.539	
	(0.626)		(0.732)	
dummy = 1 if Saturday	-8.214***		-8.612***	
	(1.069)		(1.682)	
dummy = 1 if Sunday	-20.37***		-21.80***	
	(1.869)		(2.013)	
Constant	21.30	-29.35***	22.64***	7.724**
	(14.75)	(7.110)	(6.419)	(3.667)
R^2 (within)	0.780	0.903	0.749	0.835
Observations	4,443	4,732	5,780	6,006
Number of groups	106	106	106	106
Province FE	Yes	Yes	Yes	Yes
Time FE	No	Yes	No	Yes
SE	Driscoll-Kraay	Driscoll-Kraay	Driscoll-Kraay	Driscoll-Kraay
Period	Nov 1 - Dec 23	Nov 1 - Dec 23	Jan 1 - Mar 15	Jan 1 - Mar 15

Driscoll-Kraay Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3: case studies binary treatment

	(1)	(2)	(3)	(4)	(5)	(6)
	DEPENDENT VARIABLE: Mobility					
	Veneto and Emilia-Romagna		Lazio and Toscana		Lazio and Abruzzo	
	PANEL A: Estimation at the implementation date					
NPIs (dummy =1 if Orange)	-16.95***	-15.87***	-13.74***	-12.21***	-18.37***	-12.89***
	(1.295)	(2.015)	(2.274)	(1.668)	(1.923)	(1.457)
m.a. cases per 100.000 people	-0.0446**	-0.197**	-0.0403	-0.178	-0.00182	-0.300***
- province level	(0.0197)	(0.0903)	(0.0310)	(0.135)	(0.0329)	(0.0745)
7 days lag of log fatalities	-0.735	-1.471	-1.648	-4.333**	1.174	-3.422***
- region level	(0.574)	(1.548)	(1.325)	(1.673)	(1.302)	(0.939)
Constant	-31.18***	-2.802	-15.82***	-19.35***	-20.19***	-13.47***
	(0.833)	(2.948)	(2.562)	(2.828)	(1.918)	(1.692)
R^2 (within)	0.953	0.817	0.880	0.813	0.896	0.789
Treatment date	Nov 15	Feb 21	Nov 11	Feb 14	Nov 11	Feb 14
	PANEL B: Estimation at the announcement date					
NPIs (dummy = 1 if Orange)	-14.64***	-12.24***	-8.521**	-9.773***	-13.07***	-12.84***
	(1.560)	(2.474)	(3.776)	(1.491)	(4.066)	(1.316)
m.a. cases per 100.000 people	-0.0838**	-0.291***	-0.0540	-0.257*	-0.0678	-0.322***
- province level	(0.0333)	(0.0975)	(0.0348)	(0.132)	(0.0590)	(0.0773)
7 days lag of log fatalities	0.0895	-1.938	-2.613	-4.865**	2.128	-3.568***
- region level	(0.950)	(1.743)	(2.189)	(1.967)	(2.321)	(1.076)
Constant	-29.39***	-4.003	-13.71***	-17.65***	-19.47***	-12.89***
	(1.428)	(2.997)	(4.105)	(3.142)	(2.265)	(1.686)
R^2 (within)	0.919	0.769	0.816	0.782	0.799	0.786
Announcement date	Nov 13	Feb 19	Nov 10	Feb 12	Nov 10	Feb 12
Observations	560	560	270	435	173	261
Number of groups	16	16	15	15	9	9
Period	Nov 1 - Dec 5	Feb 1 - Mar 8	Nov 1 - Nov 14	Feb 1 - Mar 1	Nov 1 - Nov 17	Feb 1 - Mar 1

Panel A displays the full regression output obtained by running model (3) in each case studies assuming the treatment begins on the date the policy is implemented. Panel B replicates Panel A assuming treatment starts at the announcement. Columns (1) and (2) report the finding for Veneto and Emilia-Romagna, columns (3) and (4) those of Lazio and Toscana, instead columns (5) and (6) report the findings of Lazio and Abruzzo. Driscoll-Kraay Standard errors in parentheses.
**** p<0.01, ** p<0.05, * p<0.1

Table 4: case studies continuous treatment

	(1)	(2)	(3)	(4)	(5)	(6)
	DEPENDENT VARIABLE: Mobility					
	Veneto and Emilia-Romagna		Lazio and Toscana		Lazio and Abruzzo	
NPIs (Stringency Index)	-2.364***	-0.983***	-2.023***	-1.797***	-2.863***	-0.980***
	(0.287)	(0.176)	(0.335)	(0.374)	(0.619)	(0.162)
m.a. cases per 100.000 people	0.0136	-0.122	-0.0403	-0.166*	-0.00372	-0.207*
- province level	(0.0411)	(0.0895)	(0.0310)	(0.0869)	(0.0455)	(0.106)
7 days lag of log fatalities	-0.802	-1.640	-1.648	-4.416**	0.925	-4.262***
- region level	(0.539)	(1.853)	(1.325)	(1.845)	(1.282)	(1.218)
Constant	106.9***	40.70***	104.1***	92.04***	152.1***	46.29***
	(17.85)	(9.586)	(20.07)	(21.73)	(35.79)	(9.061)
Observations	560	560	270	435	173	261
Number of groups	16	16	15	15	9	9
R^2 (within)	0.943	0.762	0.880	0.825	0.866	0.783
Period	Nov 1 - Dec 5	Feb 1 - Mar 8	Nov 1 - Nov 14	Feb 1 - Mar 1	Nov 1 - Nov 17	Feb 1 - Mar 1
Treatment date	Nov 15	Feb 21	Nov 11	Feb 14	Nov 11	Feb 14

Columns (1) and (2) report the finding for Veneto and Emilia-Romagna, columns (3) and (4) those of Lazio and Toscana, instead columns (5) and (6) report the the findings of Lazio and Abruzzo. Driscoll-Kraay Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1