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RESEARCH ARTICLE

Implementation and spillovers of local non-pharmaceutical interventions

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Abstract

In this paper, we analyze economic costs and consequences of local non-pharmaceutical interventions (NPIs) aimed at containing the Covid-19 pandemic. Using comprehensive data on municipal and regional policies in Norway, we implement a difference-in-differences framework identifying impacts of local NPIs from discontinuous differential shifts in outcomes following the implementation of new policies. In treated municipalities, local NPIs lead to persistent reductions in mobility, persistent increases in unemployment, and transient reductions in consumer spending. Analyses of spatial spillovers show that the implementation of local NPIs increases retail mobility in untreated neighboring municipalities. Overall, our findings suggest that local NPIs have economic consequences for local economies and induce residents to shift their consumption of goods and services to neighboring municipalities.

KEYWORDS
Covid-19, mobility, non-pharmaceutical interventions, spillovers, unemployment

JEL CLASSIFICATION
H12, I18, J08, J63

1 | INTRODUCTION

Non-pharmaceutical interventions (NPIs) have been key to managing the Covid-19 pandemic. These policies may have important financial and non-monetary costs. Policymakers face a tradeoff between controlling the number of deaths and limiting the burden of containment policies (Alvarez et al., 2020). In this paper, we consider the economic costs and consequences of local NPIs. Specifically, we implement a difference-in-differences framework, estimating effects of municipal policies on mobility, consumer spending, and unemployment.

Using difference-in-differences methods to evaluate effects of Covid-19 countermeasures is challenging for multiple reasons (Goodman-Bacon & Marcus, 2020). First, the timing of policies is likely a response to Covid-19 incidence. This policy endogeneity could impact analyses of economic outcomes if higher confirmed incidence has an independent effect on economic behaviors. Second, localities may experience multiple treatments: localities may implement a social distancing mandate when rates are rising, relax the rules when incidence is lower, then re-instate the mandate later if contagion levels go back up. Third, the localities that implement NPIs do so at different times; as a result, regression difference-in-differences may yield biased estimates when treatment effects vary over time (Baker et al., 2021; Callaway & Sant'Anna, 2020; Goodman-Bacon, 2021). Fourth, local NPIs may have spillovers to neighboring municipalities, leading violations of the stable unit treatment value assumption that underlies difference-in-differences analysis.

In this paper, we implement a stacked regression estimator (Baker et al., 2021; Sun & Abraham, 2020) as implemented in Cengiz et al. (2019). This model compares outcomes in treated municipalities with outcomes in a set of clean controls, defined as municipalities that do not implement new NPIs during the entire event window. To adjust for observational differences between the treated and control municipalities, we implement a propensity score re-weighting approach. By exclusively leveraging sub-national policy variation, we ensure that the treated municipalities and the municipalities in the comparison group face the same national mandates at any given time. Our approach allows for multiple events per locality, and for treatment effects to vary over time. Moreover, these models let us assess spatial spillovers by assigning treatment status to neighboring municipalities.

Our preferred empirical specification is a set of event study regressions, estimating differential changes in outcomes over time. New NPIs are typically implemented in response to a gradual increase in observed Covid-19 incidence. To the extent that these are effective in reducing contagion, confirmed incidence would likely be gradual and occur with a lag. Conversely, mandated changes in behaviors, and economic impacts of such changes, are likely to happen more abruptly. Discontinuous shifts in the estimated event time coefficients thus point to an independent effect of NPIs on economic outcomes.

We have three main findings. First, we show that local NPIs are typically implemented as a response to high and rising rates of confirmed Covid-19 incidence. Second, we find that local NPIs significantly affect mobility patterns and economic outcomes in the treated municipalities. Our models find abrupt, persistent reductions in mobility, persistent increases in unemployment, and transient reductions in consumer spending. Third, our analysis of spatial spillovers shows that the impact of local NPIs is not limited to the treated municipalities. Rather, we find that retail mobility increases in neighboring municipalities that do not themselves introduce any NPIs during the event window. Overall, our findings suggest that local NPIs induce residents to shift their consumption of goods and services to neighboring municipalities.

Our findings contribute to the rapidly growing literature on the social and economic consequences of the pandemic and its countermeasures. Overall, these studies find mixed results on the effects of local NPIs: while social distancing mandates have been found to significantly shift mobility, studies also find that the effects of the mandates themselves may be limited relative to the voluntary behavioral changes brought about by the pandemic itself (Alexander & Karger, 2021; Allcott et al., 2020; Courtemanche et al., 2020; Cronin & Evans, 2020; Goolsbee & Syverson, 2021; Sears et al., 2020). These papers primarily focus on the impact of NPIs at the beginning of the pandemic, and tend to measure NPIs at a higher level of aggregation. In our setting, meanwhile, there is more variation in the timing of policies, allowing us greater scope for overcoming concerns related to multicollinearity. More generally, the early to mid 2020s was a time when voluntary mobility declines may have been more important. In contrast, we focus on a later period when attention to the pandemic may have been waning.

Most published studies examine effects of NPIs in relatively high-incidence contexts, such as the US or other European countries with more Covid-19 related deaths. In this paper, we provide evidence of the role of NPIs in a lower incidence context, where Covid-19 fatality rates have remained relatively low throughout the pandemic. On the one hand, people may be less likely to voluntarily limit their economic and social activities when perceived risks of death or serious illness are lower; in which case the relative impact of mandates on consumer behavior and the economy may be greater. On the other hand, the content of the NPIs is typically less stringent and the lower incidence could lead to lower compliance with regulations.[1]

Our paper also contributes to the literature on spatial spillovers of local NPIs (Elenev et al., 2021; Holtz et al., 2020). The paper most similar in spirit to our own (Elenev et al., 2021), find that stay-at-home mandates reduce mobility in neighboring counties. This result contrasts with our finding that local NPIs increase mobility in neighboring municipalities, consistent with residents substituting consumption to untreated localities. This difference could reflect the different contexts of the two studies, moreover, the NPIs we study in the present paper are implemented against the backdrop of a national policy response.

The rest of the paper is organized as follows: Section 2 gives an overview of our empirical strategy. In Section 3, we present an analysis linking the adoption of local NPIs to trends in confirmed incidence. Section 4 presents our analysis of the economic impacts of local NPIs, and Section 5 concludes.

2 | EMPIRICAL STRATEGY

The impact of the Covid-19 pandemic in Norway has been geographically uneven. Local and regional policy variation has allowed policymakers to manage contagion while limiting the economic burdens of Covid-19 countermeasures. We leverage this variation to implement a difference-in-differences framework, where we estimate effects of the NPIs by comparing changes in outcomes in treated and untreated municipalities over time.

Below, we present our empirical strategy in detail. First, we briefly review relevant institutions and background. Second, we outline the sample construction and present summary statistics of the sample. Third, we present our formal econometric models.

2.1 | Institutions and background

The first confirmed case of Covid-19 in Norway was reported on February 27. On March 12, 2020, the Norwegian government implemented the most radical public measures in peace time. This immediate national policy response included the closing of schools and non-essential businesses and the prohibition of cultural and sporting events and all indoor recreational activities. Starting in May 2020, the lockdown was gradually lifted and low infection rates throughout September 2020 kept mandates at a minimum until October 2020, when infection rates surged.

Our paper covers the period from November 2020 to September 2021. Our sample period follows a time of very low contagion rates (see Appendix Figure A1 for national trends in Covid-19 incidence). In this period, there were no national restrictions on in-person instruction in universities, schools, preschools or daycare facilities, no national in-person retail restrictions, and no national mask mandate. For shorter periods, national prohibition of larger private gatherings and the serving of alcohol were in effect.

Covid-19 tests were widely available throughout our study period. Vaccines, on the other hand remained limited. The first vaccine was administered on December 27, 2020. Initially, vaccines were restricted to nursing home residents and selected groups of healthcare workers. By the end of April 2021, 23% had received at least one shot, while 6% were fully vaccinated. By October 2021, the end of our sample period, 74% of the population had received at least one shot, and 64% were fully vaccinated. Due to the scarcity of vaccines, NPIs remained the most important intervention in order to contain the spread of the pandemic during most of our study period.

Before the pandemic, employees could be laid-off after a notice of 14 days. The employer had a duty of remuneration the first 15 days and most workers would be eligible for daily unemployment benefits after that. During the pandemic, the rules for temporary lay-offs were altered so that the lay off took effect 2 days after a notice was given. These rules remained in place during our study period.

2.2 | Sample construction

2.2.1 | Defining events

The starting point of our analysis is data on local NPIs collected by Verdens Gang (VG), a large daily newspaper in Norway. Beginning November 19th, 2020, this data represents the most comprehensive source of local and national NPIs.[2] We focus on policies that were implemented between December 17, 2020 and September 3, 2021.[3]

In our analysis, we analyze outcomes in a 6 weeks window around the implementation of local NPIs. We restrict our sample to new NPIs - specifically, we restrict our sample to events where no new mandates, prohibitions, or closures were implemented in the 21 days immediately preceding the event. This restriction allows for a clean analysis of pre-trends. At the same time, we may exclude some municipalities where NPIs are updated more frequently. We make no similar restrictions on policy changes after the initial event, as these are possibly endogenous. Similarly, we do not require a minimum duration of the policy, rather, we keep the event window fixed at plus minus 3 weeks for all events.

The final sample includes 511 events. For each event s, we construct a cohort-specific estimation sample consisting of treated municipalities (new local intervention at time s) and clean controls (no new local interventions in an 6 weeks window around $s, s-21, \ldots, s+20$). In order to estimate spillover effects, we identify neighboring municipalities that share a border with the treated municipalities, and which did not themselves experience a new NPI in the event window. These neighboring municipalities are excluded from the set of clean controls in the main estimation sample. We then stack these cohorts to obtain a balanced data set with 21 days prior to and 21 days post the intervention date. These stacked samples comprise our estimation sample.

2.2.2 | Outcome variables

We construct a municipality by day (week) dataset with our outcome variables. Our analysis covers three broad categories of outcomes: (1) Covid-19 health and test outcomes, (2) behaviors, as proxied by mobility patterns, and (3) financial and economic outcomes.

Data on Covid-19 related outcomes are obtained from the Emergency preparedness register for Covid-19 (Beredt C19). This is a comprehensive database of registers established to give the Norwegian Institute of Public Health an ongoing overview and knowledge of prevalence and consequences of the Covid-19 pandemic in Norway. It comprises individual-level data from a set of linkable administrative registers, including daily updated records on polymerase chain reaction tests and test results (Norwegian Surveillance System for Communicable Diseases) and from the Norwegian Patient Register (Covid-19 related hospitalizations), as well as the National Population Register and sociodemographic registers, including information on income, education, country of birth and housing conditions. Individuals are linked across the registers and to municipalities using unique (de-identified) personal identifiers. These data are used to construct a municipality level dataset with daily numbers of tests, confirmed cases, and hospitalizations.[4]

To assess effects of local NPIs on mobility behavior, we use two sources of data. First, we access mobility data from Google Covid-19 mobility reports (Google, 2021).[5] These reports contain daily municipality level data on mobility trends by category. We include data on mobility related to workplaces and to retail and recreation (restaurants, malls, museums, cinemas etc. Excluding groceries and pharmacies).[6] Data is missing for some of the smaller municipalities. As a consequence, mobility is observed only for a subset of events. Daily mobility is measured relative to a baseline value for the same day of the week, where the baseline is the median value for the corresponding day of the week during the period Jan 3-Feb 6, 2020.

The second mobility data source is all credit/debit card transactions of services and in-person retail for the 1.3 million private customers of DnB, a large Norwegian bank, with a market share of 26%. These data have the advantage of capturing more municipalities. However, the data is available only on a weekly basis. In addition, the data is grouped by cardholders' municipality of residence, and we are therefore not able to identify where transactions take place. Unlike the Google mobility data, which is a measure of how much mobility there is to a certain location in the municipality, the card data is a measure of how much the population living in that municipality moves. This distinction is useful for our analysis of spillovers.

Finally, to capture the economic consequences of local NPIs we include the following two outcomes: registered unemployment and consumer spending. For unemployment, we use publicly available data on the number of unemployed job-seekers measured each Tuesday, published by the Norwegian Labor and Welfare Administration. The data allow us to distinguish between full time and part time unemployment, as well as temporary and permanent unemployment. To measure consumer spending, we use data on all credit/debit card payments from DnB described above. As above, the data is aggregated to the consumer-municipality of residence level, but unlike above, where we apply the number of transactions made by persons living in the municipality, we here measure the total level of spending for people who live in the municipality.

2.2.3 | Descriptives

Table 1 presents summary statistics of the estimation sample. The treated municipalities have higher rates of Covid-19 testing, incidence, and hospitalization relative to the comparison group.[7] Treated municipalities tend to be larger and have a higher share of residents born abroad, higher educational attainment, and higher share of crowded housing.

Appendix Table A1 summarizes the local NPIs in the estimation sample, together with a comparison sample of all local NPIs that were implemented during the sample period. The distribution of NPIs across types of polices and across policy areas is fairly similar in both samples, indicating that the sample NPIs are representative of the NPIs that were in place during the analysis period. There is considerable variation in the timing of events (see appendix Figure A2). While we find one substantial hike in early January 2021 and a smaller hike toward the end of March 2021, local NPIs were implemented throughout the sample period.

The NPIs in our sample cover various areas of economic activity and daily life. The VG data distinguishes between seven distinct policy domains: masks, leisure, social contact, work, education, travel, and Christmas.[8] Appendix Table A2 gives more detail on the types of policies included in each of these categories.

Appendix Table A3 provides more detail on the typical policy transition. On average, 42% of the events in our sample have one or more follow-up mandates in the 3-week window after the initial policy is implemented, while the average event has 0.97 follow-up policies on average for each event. The share of events with follow-up policies appears to vary by policy domain: 55% of education NPIs have one or more follow-up policies, compared to just over 41% for mask mandates. There is considerable variation in the timing of these follow-up policies (see Appendix Figure A3). While the modal follow-up policy is introduced after 14 days, there is no systematic pattern in the timing of these follow-up policies.

In our models, we estimate how outcomes change after the introduction of the initial policy. When interpreting these effects, we should keep in mind that the estimated effects will include the impact of these follow-up policies.[9]

Appendix Figure A5 shows the spatial distributions of the sample events, together with maps showing rates of Covid-19 testing, incidence and hospitalizations. Consistent with the results in Table 1, areas that have been more impacted by Covid-19 have greater number of NPIs. At the same time, the figure shows that NPIs have been implemented in all regions of the country; they are not restricted to the capital region or larger cities.

TABLE 1 Summary statistics

	(1) All	(2) 1+ mandate	(3) No mandate
Incidence (per 100,000)	4.080	8.284	4.020
	(8.955)	(14.22)	(8.843)
Hospitalizations (per 100,000)	0.131	0.279	0.128
	(0.892)	(1.355)	(0.883)
Tests (per 100)	254.2	348.9	252.8
	(208.6)	(237.2)	(207.8)
Age	41.81	40.61	41.82
	(2.514)	(2.282)	(2.513)
Foreign born	0.135	0.175	0.134
	(0.0539)	(0.0653)	(0.0535)
Higher ed	0.312	0.381	0.311
	(0.0895)	(0.110)	(0.0888)
Cramped housing	0.0897	0.113	0.0893
	(0.0356)	(0.0456)	(0.0353)
Low income	0.104	0.114	0.104
	(0.0210)	(0.0268)	(0.0209)
Population (in 1000s)	74.47	184.8	72.89
	(180.9)	(266.5)	(178.8)
Observations	1,740,256	9520	1,730,736

Note: Population-weighted averages; sd in parenthesis.

2.3 | Econometric models

We estimate the following event study regression model:

$$y_{its} = \theta_{is} + \theta_{ts} + \sum_{k=-3,k\neq-1}^{3} \pi_{k(its)}\rho^{k} + \varepsilon_{its} \tag{1}$$

Here, y_{its} is the outcome in municipality i, week t, and event s. π_k is a set of event time variables, indicating that k weeks have passed since the implementation of the mandate. θ_{is} and θ_{ts} are event-specific fixed effects for municipality and calendar time. The calendar time fixed effects will absorb national trends in incidence, as well as any changes in NPIs at the national level.

The primary parameters of interest are the ρ^k attached to the event time dummies. These coefficients will capture the differential changes in outcomes in treated municipalities relative to the comparison group over the event window and relative to the week before, defined as the 7 day-period preceding the mandate. The coefficients attached to event time $k < -1$ indicate estimated pretrends, while the coefficients for $k > 0$ describe how outcomes change after the policy is implemented. Discontinuous shifts in the estimated ρ^k at time 0 or shortly after point to a causal effect of the mandates themselves.

To summarize the model, we also estimate the following specification, grouping event time coefficients into the following week groups: [−3, −1], 0, and [1, 2], from hereon referred to as *PRE*, *DURING*, and *POST*, respectively. This specification is given by:

$$y_{its} = \theta_{is} + \theta_{ts} + \pi_{PRE(its)}\rho^{PRE} + \pi_{DURING(its)}\rho^{DURING} + \pi_{POST(its)}\rho^{POST} + \varepsilon_{its} \tag{2}$$

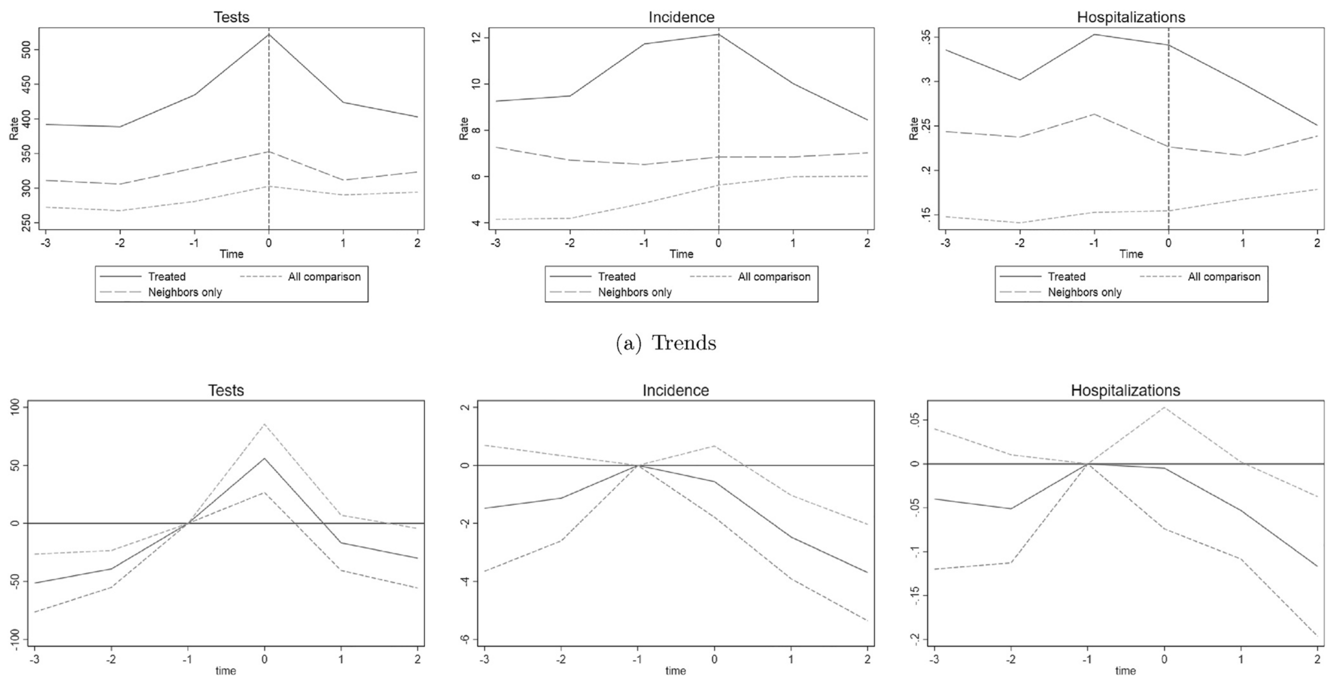
As Table 1 makes clear, municipalities that implement local NPIs are systematically different from the “clean controls” comparison group. The treated municipalities tend to be significantly larger and have higher initial rates of testing, confirmed incidence, and hospitalizations. To account for these differences, we implement a propensity score re-weighting approach to impose balance across treatment and comparison municipalities (Bailey & Goodman-Bacon, 2015; Goodman-Bacon & Marcus, 2020).

To estimate the propensity scores, we include the following pre-determined variables: log of population, the share of employed who were employed in hotels and restaurants as of March 1, 2020, share of population with low income, share of population older than 40 years, share of population living in crowded housing, and share of population with higher education.[10]

3 | COVID-19 INCIDENCE AND THE IMPLEMENTATION OF LOCAL NPIs

Panel (a) in Figure 1 plots average rates of testing, incidence, and hospitalizations in the 6 weeks window around the implementation of new local NPIs. Policies are implemented on day 1 in week 0. Consistent with the summary statistics in Appendix Table 1, treated municipalities have higher rates of testing, incidence, and hospitalization than untreated municipalities, but the differences are smaller between the treated and untreated neighbors compared to the full comparison group. In the weeks leading up to implementation, the gap in incidence between treated and comparison groups grows, relative to at baseline ($t = -3$) indicating that local NPIs are implemented as a response to increasing contagion. After the policy is implemented, the gap shrinks, with a particularly sharp decrease in the first week after the NPI took effect. As the same holds for testing, we cannot exclude that reductions in incidence are caused by reduced rates of testing, however, hospitalizations are also projecting a steep decline, indicating that the underlying prevalence is also falling. At 3 weeks after implementation, hospitalization rates in the treated municipalities have fallen relative to all "clean controls" and is almost similar to that of its "clean control" neighbors.

Panel (b) in Figure 1 presents our estimated event study models of the effects of local NPIs on testing, incidence, and hospitalizations. In estimating these models, we use a propensity score re-weighting approach to account for observational differences between the treated and control municipalities. Even with this adjustment, the estimated models find significant pre-trends in testing. Test rates increase near linearly, reaching their peak during the week the policy is introduced. The sharp



(a) Trends

(b) Event study estimates

FIGURE 1 Covid-19 and the implementation of local non-pharmaceutical interventions (NPIs). Panel (a) plots average rates of testing, incidence and hospitalizations in the 6 weeks window around the implementation of new local NPIs. "All comparison" includes all clean control municipalities (no new mandates, prohibitions or closures in the event window), excluding neighbors. "Neighbors only" includes only municipalities that are clean controls contiguous to the treatment municipalities. The averages are population-weighted. Panel (b) shows the event study estimates of Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

rise and fall in testing can be expected as people were eager to test when NPIs are introduced, both due to increasing contagion and increased awareness. However, once people were tested and the restrictions were in place the need for further testing was substantially reduced.

For incidence, there are suggestive positive pre-trends, however these are not statistically different from zero. We find no significant pre-trends in hospitalizations, suggesting that local NPIs are not preceded by a surge in serious cases. After implementation, that is, weeks 2 - 3 (marked as 1 and 2 in the graphs), there are statistically significant reductions in confirmed incidence and hospitalization rates. The reductions amounts to about 30% and 26%, respectively, relative to pre-intervention levels (see Appendix Table A4).

To sum up, these findings suggests that local NPIs are implemented as a response to increasing confirmed incidence and not increasing hospitalizations. Due to the endogeniety of the introduction of local NPIs with respect to incidence (also evident by the significant pre-trends), our models can not answer to the causal effects of local NPIs on contagion. However, the results on hospitalizations indicate that the local NPIs reduced the prevalence of serious cases.

4 | EFFECTS ON MOBILITY AND THE ECONOMY

4.1 | Mobility and the economy

Our event study estimates of mobility and economic outcomes in the treated municipalities are presented in Figure 2; Table 2 presents the corresponding estimates from Equation (2). The first four panels show effects on mobility. Panel (a) plots estimated models of workplace and retail mobility obtained from Google mobility dashboards, while panel (b) plots estimated models for in-person retail transactions and service transactions based on credit card data. The corresponding trend plots are available in appendix Figure A6.

We find no significant pre-trends in any of the mobility outcomes, suggesting that the mobility outcomes would have trended in parallel in absence of the introduction of new local NPIs. When the local NPIs are introduced, there us a discontinuous downward shift in mobility for all mobility outcomes. The event study graphs in panel (a) show statistically significant and persistent reductions in mobility to workplace and retail locations.

These reductions in mobility arrive in the context of already low mobility levels in the treated municipalities. On average, workplace and retail mobility rates in the pre-implementation period are down by 30% and 21% relative to pre-pandemic baselines. The implementation of NPIs then reduce mobility in these two categories by an additional 2% and 3% respectively.[11]

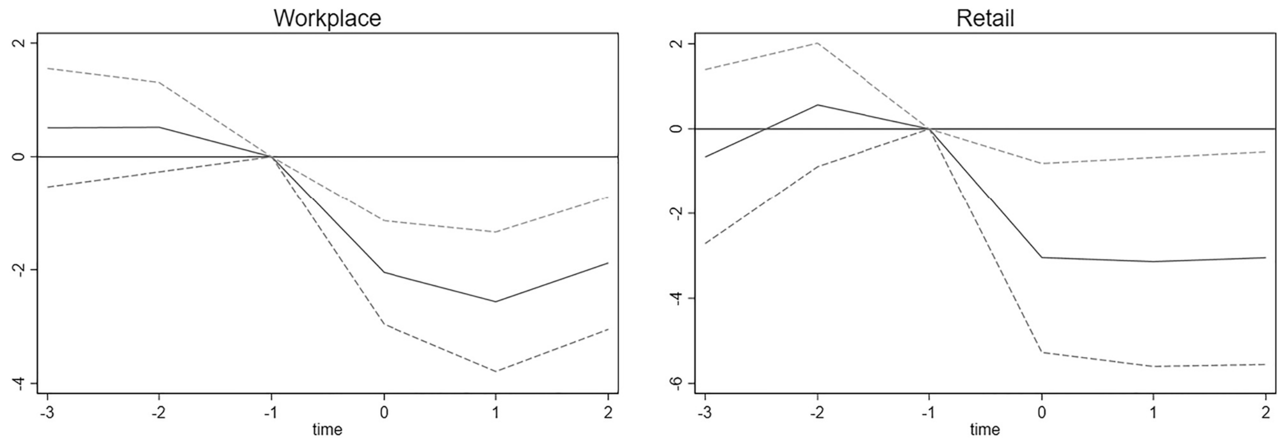
Panel (b) presents the corresponding analyses of in-person card transactions.[12] For both the number of in-person transactions and the number of service transactions, we find significant shifts after the policy is implemented. While the shifts in Google mobility data were persistent, the reduction in numbers of card transactions appears to fade over time. Relative to pre-implementation means, in-person retail and service transactions drop by an average of 5.1% and 7.3%, respectively (see Table 2).

In the results in Figure 2, event time is defined in weeks relative to implementation. However, as the Google mobility data is available on a daily basis, we also present estimates of daily mobility responses using +/− 10 days as estimation windows. These results are presented in Appendix Figure A7. As in the weekly models, the daily models show a discontinuous shift in mobility following the implementation of new local NPIs. Moreover, these results show that the effects on mobility were immediate.

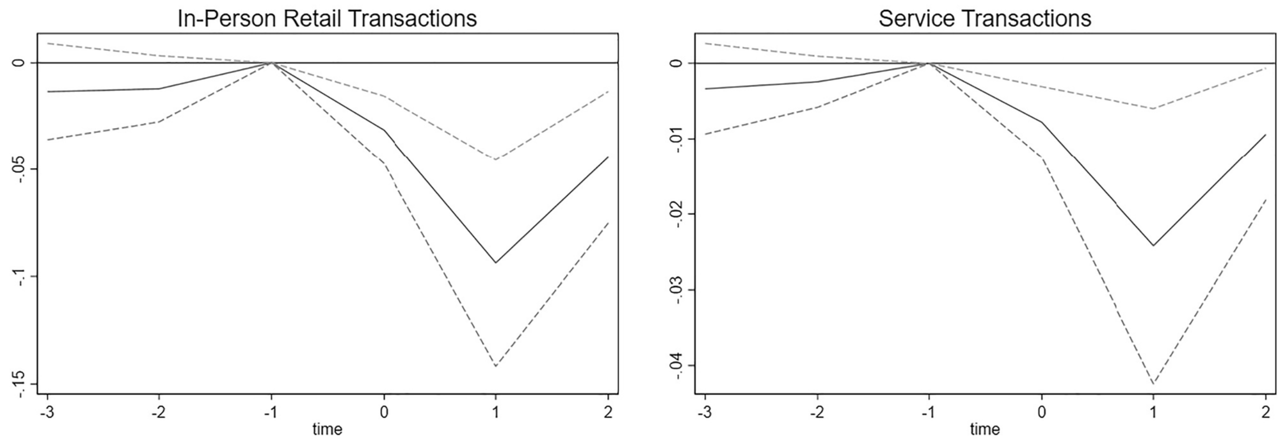
To summarize, the introduction of local NPIs is followed by a substantial reduction in mobility. While this holds for all four mobility outcomes, there is a discrepancy in that the effects using Google mobility data show persistent reductions in mobility, whereas the card transaction data indicate a transient drop. As the Google mobility data defines mobility based on activity in a given geographic area, while the card data defines mobility based on the card holder's municipality of residence, what we see is a persistent drop in mobility to certain places, but people's spending patterns are temporarily reduced. This discrepancy could reflect that people shift to less risky forms of consumption, for example, take-away dinners instead of in-person eating; or a spatial shift where consumers seek out retail and services in neighboring (untreated) municipalities. We will return to the latter possibility in our analysis of spatial spillovers.

Returning to Figure 2, panel (c) presents the effects of the local NPIs on local economies. Here, we consider two outcomes: consumer spending and registered unemployment. The corresponding trend plots are available in Appendix Figure A6.

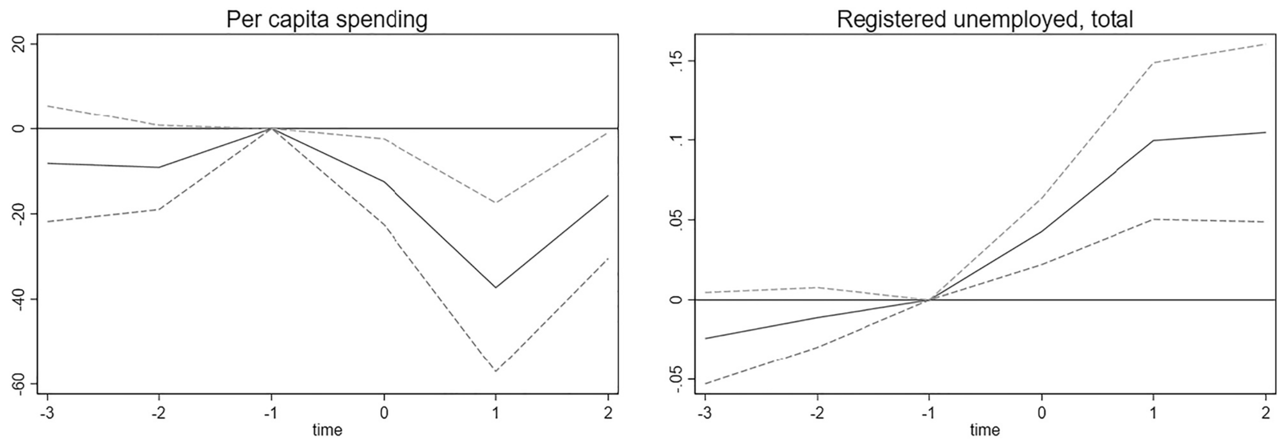
For consumer spending, we see no significant pre-trends before there is a significant drop in spending during the first post-intervention week. In week 3, spending returns approximately to pre-intervention levels, suggesting that the drop in consumer card purchases is temporary. On average we estimate a reduction of about 26 Norwegian Krone/day during weeks 2 and 3, which corresponds to about 4% relative to pre-implementation levels in the treated municipalities (column (5) Table 2).



(a) Mobility data Google



(b) Mobility data card transactions



(c) Local economic outcomes

FIGURE 2 Non-pharmaceutical interventions (NPIs), mobility and local economies. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

We have also estimated models separately for online and in-person purchases. Results from this exercise are presented in Appendix Figure A8 and Appendix Table A5. While there are no significant pre-trends in neither in-person nor online purchases, there is a slight increase in online purchases coupled with a significant drop in in-person transactions. That is, consumers appear to shift some consumption online at the implementation of local NPIs.

TABLE 2 Mobility and the economy in treated municipalities

	(1) Workplace	(2) Retail	(3) In-person transactions	(4) Service transactions	(5) Per capita spending	(6) Unemployment
PRE	0.516	−0.0467	−0.0127	−0.00230	−8.78	−0.0176
	(0.383)	(0.794)	(0.00895)	(0.00218)	(5.75)	(0.0115)
DURING	−2.05***	−3.04***	−0.0313***	−0.00831***	−12.8**	0.0429***
	(0.451)	(1.08)	(0.00783)	(0.00247)	(5.25)	(0.0106)
POST	−2.22***	−3.09***	−0.0670***	−0.0173**	−25.9***	0.102***
	(0.574)	(1.11)	(0.0194)	(0.00687)	(7.96)	(0.0265)
N	63,966	15,582	287,742	273,990	287,742	724,794
Pre-Mean	−29.9	−21.4	1.32	0.235	656	4.66
Relative change	-	-	−0.0509	−0.0736	−0.0396	0.0219

Note: Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

For unemployment, we find no evidence of significant pre-trends. In the week of implementation, registered unemployment increases slightly, before it jumps further in week 2 and stays elevated through week 3. Quantitatively, the effects are small: we estimate an increase in weeks 2 - 3 to be about 2% relative to pre-implementation levels in the treated municipalities. At the same time, these represent significant income losses for the affected workers, in particular because the increase is driven entirely by increases in fully-unemployed persons, with part-time unemployment/hours reduction basically unchanged (see Appendix Figure A9 and Table A6).

The transient drop in consumer spending contrasts with the persistent increase in registered unemployment. One possibility is that the bounce back reflects consumers shifting away from affected businesses (e.g., bars) toward unaffected sectors (e.g., coffee shops) or to establishments in other municipalities.

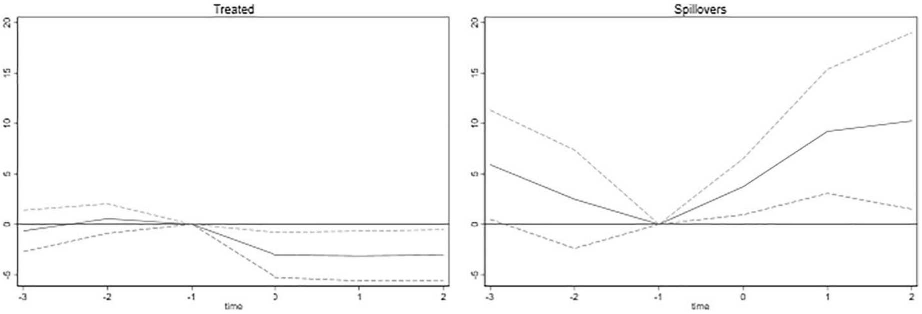
4.2 | Spillovers to neighboring municipalities

Spillovers can happen both through changes in cross-municipality mobility patterns of residents in treated municipalities (e.g., closing bars and restaurants lead residents in treated municipalities to seek out in-person dining in neighboring municipalities), and through changes in behaviors of residents in neighboring municipalities (e.g., mandated closures in the treated municipalities lead to residents in neighboring municipalities staying home more). To assess spillovers to neighboring municipalities, we estimate a version of the event study model where the neighboring municipalities are assigned the treatment dates of the treated municipalities, and the comparison groups consists of all "clean controls" that that are not neighboring a treatment municipality. Results from this exercise are shown in Figure 3; corresponding point estimates from Equation (2) are presented in Table 3.

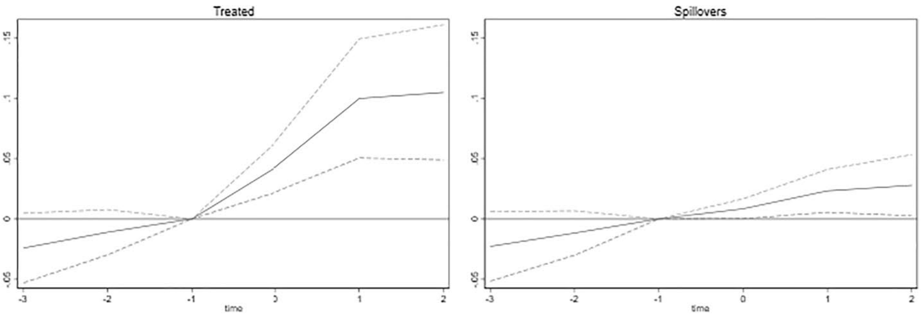
Panel (a) presents our analysis of spillovers in retail, showing estimated event study models of retail mobility in treated (our main results) municipalities to the left and neighboring municipalities to the right. When NPIs are implemented in one municipality, mobility related to retail increases in neighboring municipalities, compared to the rest of the "clean control" municipalities. We estimate that retail mobility in the neighboring municipalities increases by 9.7 points on average; this is considerably larger than the corresponding reduction in retail mobility in the treated municipalities. While this difference may seem puzzling, our empirical strategy implies that we would not necessarily expect a one to one correspondence in estimated effect sizes even when there is perfect substitution.

We place no restrictions on the number of treated municipalities per untreated neighbor. Non-treated neighbors in our estimation sample may share borders with several municipalities that implement mandated business closures in a coordinated manner. Our estimation sample is a nonrandom subset of the universe of treated municipalities and untreated neighbors. Our requirement that events have a 3 week window with no new NPIs implies that municipalities that implemented several policies in quick succession will be underrepresented in our estimation sample. Smaller municipalities with low retail activity are likely to have missing mobility data and are similarly excluded from the sample. As a consequence, even if there is a one to one substitution between retail traffic in the universe of treated and untreated municipalities, this may not be reflected in our sample of events.

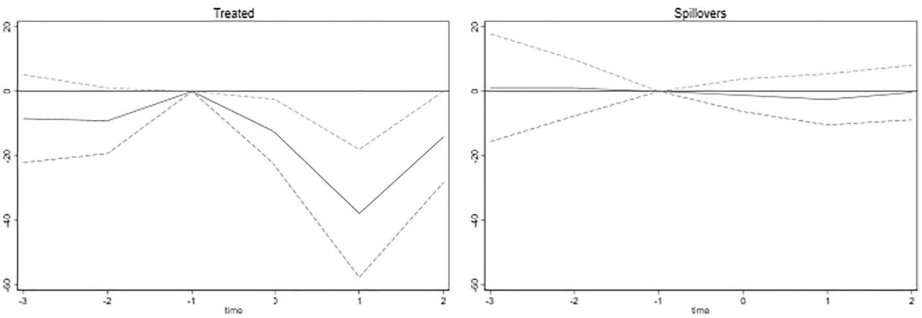
In addition, the mobility metrics are defined relative to a municipality-specific reference week. Compared to treated municipalities, untreated neighbors will typically have lower populations and lower baseline retail activity. If N people leaving a



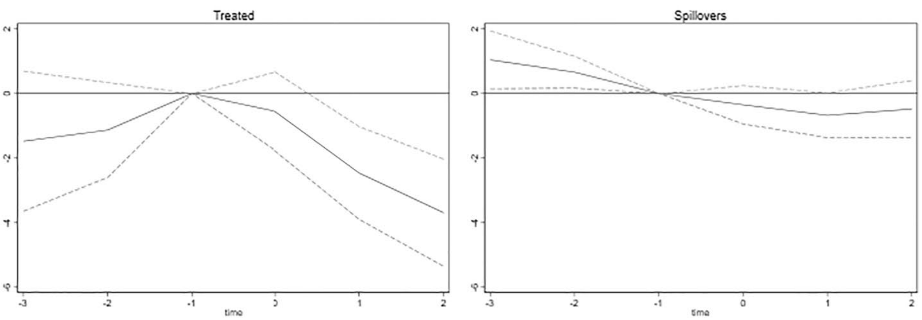
(a) Retail



(b) Unemployment



(c) Per capita spending



(d) Covid-19 incidence

FIGURE 3 Spillovers to neighboring municipalities. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

treated area all seek out retail locations in an untreated neighboring municipality, the resulting relative increase in mobility in the neighboring municipalities will then be larger than the initial relative reduction in mobility in the treated locations. That is, even if there was a one to one substitution in our event sample, the estimated effect sizes would still diverge.

TABLE 3 Spillovers to neighboring municipalities

	(1) Retail	(2) Unemployment	(3) Per capita spending	(4) Incidence
PRE	4.19*	−0.0172	1.11	0.853***
	(2.40)	(0.0113)	(6.05)	(0.324)
DURING	3.73***	0.00842**	−1.20	−0.350
	(1.32)	(0.00417)	(2.58)	(0.302)
POST	9.74***	0.0255**	−1.42	−0.576
	(3.46)	(0.0100)	(4.02)	(0.381)
N	3570	728,574	224,484	5,100,018
Pre-Mean	−29.6	4.26	656	6.83
Relative change	−0.329	0.00599	−0.00217	−0.0843

Note: The table presents estimates of spillover effects in mobility to retail locations, unemployment rate, per capita spending of the municipality inhabitants, and Covid-19 incidence. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

Panels (b) and (c) of Figure 3 show effects on unemployment and consumer spending for residents in neighboring municipalities. The expected direction of spillovers in unemployment are theoretically ambiguous. Unemployment is defined based on workers' municipality of residence, rather than place of work. On the one hand, mandated closures in treated municipalities may drive up demand for retail and services in neighboring municipalities. This in turn could drive up local demand for labor, pushing down the local unemployment rate. At the same time, employers may be unlikely to respond by hiring more workers if the mandates are expected to be temporary. In our context, it seems likely that local labor demand in the neighboring municipalities is fairly stable in the short run. On the other hand, drops in labor demand in treated municipalities could also affect commuters who live in the neighboring municipalities, but work in the treated municipalities. If the latter effect dominates, spillover effects could be net positive.

Overall, we find little evidence of significant spillovers for either unemployment or consumer spending. While we do find a statistically significant increase in registered unemployment in neighboring municipalities the week of implementation, the effect is economically small (0.6% relative to pre-policy mean). Models of workplace mobility and card transactions find no evidence of statistically significant spillover effects (see Appendix Figure A10).

The analysis of spillovers indicate that when local NPIs are introduced in one municipality, people travel across municipality borders. This change in mobility patterns can also explain the discrepancy between permanent increases in unemployment, but only transitory reductions in consumer spending among people living in the affected municipality found in our main results. To find out if this increase in mobility, plausibly by people from higher infection areas, also results in higher infection levels, we also analyze spillovers in Covid-19 incidence. This does not seem to be the case, as panel (d) shows that there were no statistically significant spillovers in Covid-19 incidence.

4.3 | Robustness

In our baseline specification, we account for observational differences using a propensity score re-weighting approach. To assess the robustness of our findings to this choice, we estimate a set of "unweighted" models where observations are instead weighted by the unadjusted population numbers in each municipality. In a related exercise, we augment our baseline specifications in Equations (1) and (2) with the following covariates: municipality population and the share of employed who were employed in hotels and restaurants as of March 1, 2020. The controls are interacted with calendar time.

The estimated event study models are presented in Figure 4, while Table 4 (columns (1) - (2) and (6) - (7)) presents the corresponding difference-in-differences estimates from Equation (2). Qualitatively, results are similar across sepcifications: we estimate significant, transient drops in consumer spending coupled with significant, persistent increases in unemployment. Quantitatively, effects are largely stable across specifications with one exception: covariate adjustment significantly reduces the estimated effect on consumer spending relative to our baseline model (1.6% vs. 4.0% reduction in our preferred specification).

In our baseline models, we use a symmetric +/− 21 days window to define events. This choice is not trivial: while a longer window allows for a more careful analysis of pre-trends and treatment effects, this comes at a cost of excluding more events from the sample. A longer window imposes more stringent criteria for the comparison municipalities, potentially affecting the

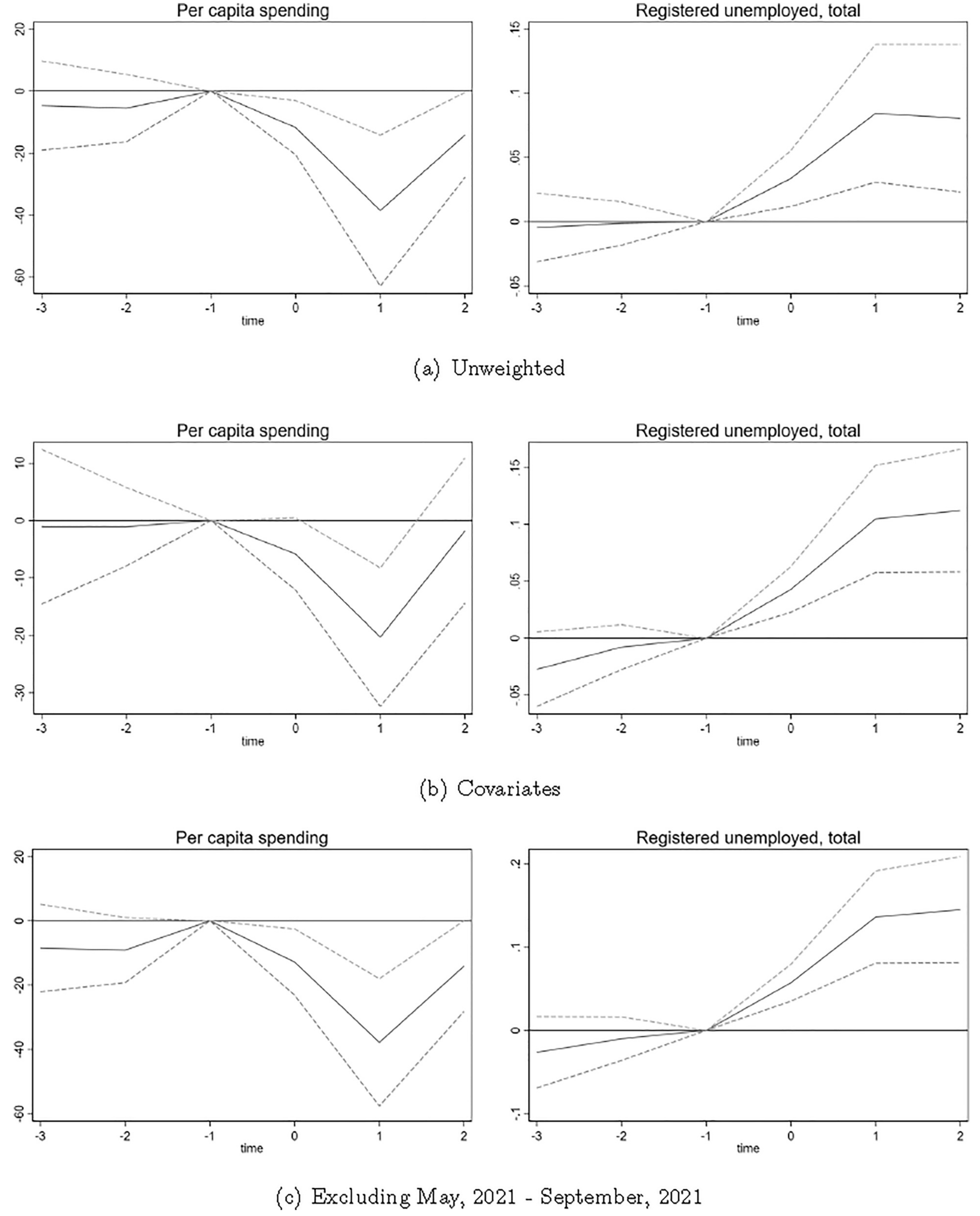


FIGURE 4 Robustness checks. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

TABLE 4 Robustness: reweighting, covariates, and event window

	Per capita spending					Unemployment rate				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Weeks PRE	−5.03	−1.08	−9.69	−13.3	−5.35	−0.00289	−0.0175	0.00861	0.00199	−0.0181
	(6.16)	(4.80)	(7.25)	(34.1)	(6.89)	(0.0104)	(0.0129)	(0.0110)	(0.0161)	(0.0167)
Week 0	−11.7***	−5.87*	−10.3	3.88	−23.1***	0.0337***	0.0428***	0.0820***	0.139***	0.0574***
	(4.45)	(3.22)	(6.66)	(7.23)	(6.57)	(0.0111)	(0.0102)	(0.0145)	(0.0261)	(0.0113)
Weeks POST	−26.3***	−11.1*	−19.0*	−8.26	−37.0***	0.0826***	0.108***	0.197***	0.317***	0.141***
	(9.01)	(5.78)	(11.3)	(9.16)	(8.41)	(0.0280)	(0.0253)	(0.0363)	(0.0578)	(0.0298)
N	287,742	400,440	152,376	115,472	171,372	724,794	724,794	305,992	245,920	508,044
Pre-Mean	687	687	636	687	633	4.67	4.67	4.98	4.67	4.92
Window/Specs.	Pop wt	Covariates	14	28	Pre-vax	Pop wt	Covariates	14	28	Pre-vax
Relative change	−0.0383	−0.0162	−0.0298	−0.0120	−0.0584	0.0177	0.0232	0.0396	0.0678	0.0286

Note: The table presents the different robustness checks: population weighted estimates; covariate-adjusted estimates (Covariates), different estimation window lengths: +14, +/−28 days, and excluding events after May 1st, when the elderly were largely vaccinated, for per capita spending and unemployment. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

size and composition of the comparison group. Requiring no new local policies over a longer time will generally lead to the comparison group being made out of smaller municipalities with lower rates of Covid-19 incidence and hospitalization.

To address the robustness of our findings to the choice of window length, we re-estimate the models in Equations (1) and (2) on samples where the event windows are set to +/− 14 and 28 days, respectively. Appendix Table A7 shows summary statistics of these samples. The shortest window, +/− 14 days with no new mandates, yields treatment and comparison municipalities that are more similar based on observables, whereas in the longest window, +/− 28 days with no new mandates, the comparison municipalities tend to differ more from the treated municipalities.

Results from this exercise are summarized in panels (3)-(4) and (8)-(9) in Table 4.[13] For consumer spending, we estimate a significant reduction using a 14 days window, for +/− 28 days, the effect is not significantly different from zero. These results are in line with event study graphs of our main results (Figure 2) where there is a short term effect on spending that levels off over time. For unemployment, the effect is statistically significant for all choices of event window; quantitatively, effects are larger for the longer window (+/− 28 days).

By the end of April 2021, 91% of the population aged 65+ had gotten at least one vaccine and 39% had two. For the age group 85+ these numbers were 92% percent and 87%, respectively. To see whether people's response to the NPIs changed when the most vulnerable were vaccinated, we re-run the analysis ignoring the months following April 2021. The results from this exercise are presented in panel (c) in Figure 4; see Table 4 for the corresponding point estimates. Excluding the time period when most elderly were vaccinated yields stronger effects for both per capita spending and unemployment, suggesting that people responded more strongly to the NPIs when the risk of serious Covid-19 cases is more pronounced. Non-pharmaceutical interventions passed during this period had approximately 40% larger effects on unemployment and spending relative to the estimated effects on the full sample period.[14]

To assess whether our results are driven by any single events, we implement a leave-one-out approach, re-estimating the regression model of Equation (2) sequentially leaving out each event from the estimation sample. Results from this exercise are shown in Appendix Figure A11. In this figure, results from our main specification is shown as a thick black line. We find some indication that two single events shift the magnitude of the estimated effects. Qualitatively, the pattern remains constant across estimation samples.

4.4 | Effects by type of policy

In the models presented so far, we estimate an average effect across NPIs, weighted by population. Non-pharmaceutical interventions vary across several dimensions, targeting different sectors of the economy and social life. This approach contrasts with, for example, Gupta et al. (2020) and Cronin and Evans (2020), who simultaneously estimate effects of multiple distinct policies. Different policies could have different impacts on mobility and the economy. To analyze this question, we estimate models for six specific sectors of interventions: masks, leisure, social contacts, work, education, and travel. We implement two sets of models. In our preferred specification, we estimate the baseline event study model from Equation (1) separately on a sample of events that have at least one policy in each category j. For this exercise we re-estimate the weights so that the comparison group is observationally similar to the treated municipalities in each policy domain sample.

Results from this exercise are presented in Figure 5. Each panel represents estimates from a separate model estimated only on the sample of events in each category. As a result, the sample sizes are smaller, and the standard errors larger relative to the pooled models. The low precision of the estimates implies that the confidence intervals associated with the different policies tend to overlap. With that caveat, we do see some suggestive evidence of different impacts. In particular, mandates related to work and education are associated with larger increases in registered unemployment and greater reductions in consumer spending (together with travel restrictions). Meanwhile, restrictions on masks, leisure, and social contacts have minimal impacts on either outcome.

In our sample, 65% of the events cover more than one policy domain; the average event covers 2.1 different policy domains. Therefore, these estimates capture the combined impacts of category j mandates in combination with the average mix of other policies that tend to be implemented at the same time (see Appendix Table A3). This could differ from the effect of each policy in isolation. As an alternative specification, we have estimated an augmented version of Equation (1) where the event study indicators are interacted with indicator variables for policy category. These models are estimated on the pooled sample of all events, using the initial re-weighting scheme.

$$y_{its} = \theta_{is} + \theta_{ts} + \sum_{j} \sum_{k=-4,k\neq-1}^{4} \left(pol_j \times \pi_{k(its)}\right)\rho^{k,j} + \varepsilon_{its} \tag{3}$$

Results from this exercise are presented in Appendix Figure A13. Overall, the estimated effects are largely similar to what we find in Figure 5. Results from these models indicate that, in isolation, mask mandates, restrictions on leisure, and social contact have little adverse effects on registered unemployment or consumer spending.

School closures and mandates related to work, meanwhile, have large negative effects on unemployment and spending. This finding might seem puzzling: by nature, school closures are unlikely to directly cause significant increases in unemployment. At the same time, this pattern is consistent with Cronin and Evans (2020)'s finding that school closures have large impacts on foot traffic. School closures could be correlated with mandates that are more wide-reaching or with a longer expected duration. Our models do not take into account how these policies differ in terms of intensity or duration. Moreover, school closures provide a salient signal that contagion rates are going up, in turn inducing increased voluntary social distancing.

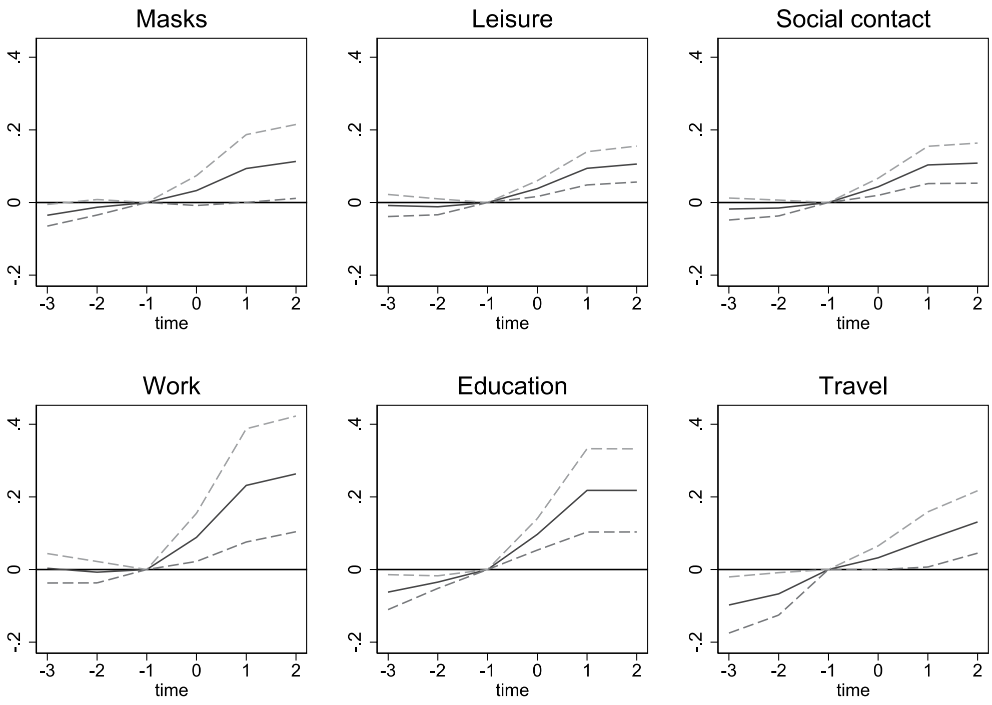
4.5 | Discussion

In this paper, we estimate economic effects of local NPIs in Norwegian municipalities. Using data on registered unemployment and card transactions from a large Norwegian bank, we estimate a series of event study models capturing differential changes in outcomes in treated and comparison municipalities upon the implementation of new NPIs. We find that local NPIs have significant economic impacts on the treated municipalities. Immediately following implementation, there is a significant drop in consumer spending as well as a significant increase in registered unemployment. While the reduction in consumer spending is transitory, the increase in registered unemployment is persistent. We further analyze spillovers to neighboring municipalities, and show that while the implementation of a local NPI reduces mobility to retail locations in the focal municipality, it increases mobility to retail locations in neighboring municipalities that did not implement any NPIs.

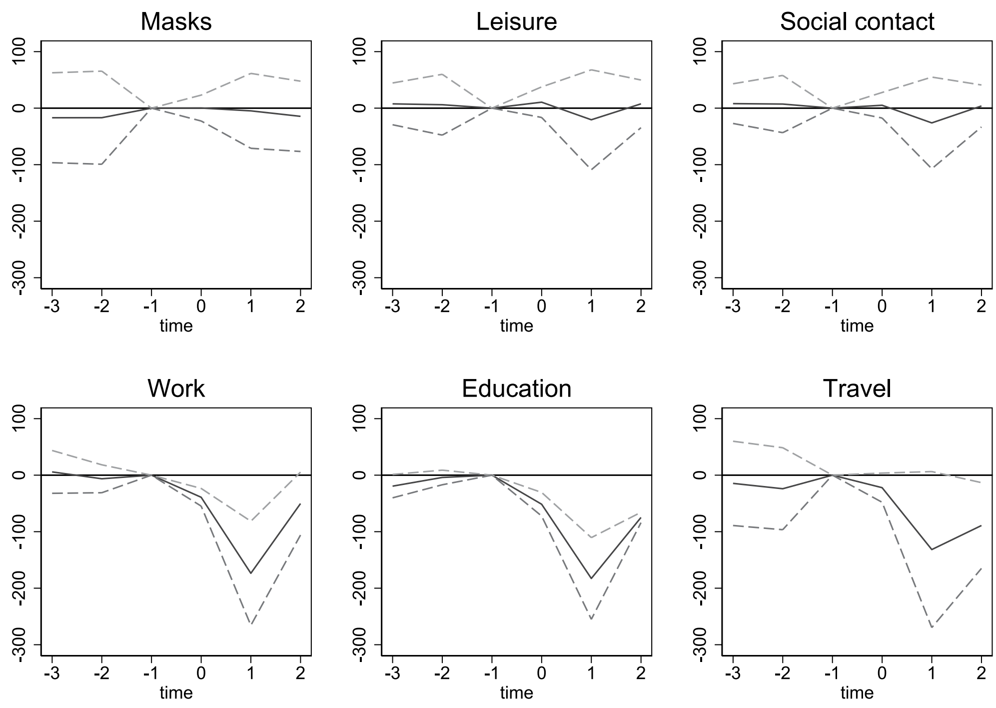
Our results contrast with the findings of Elenev et al. (2021) who find that stay at home mandates reduce mobility in neighboring counties. This difference could reflect differences in settings: their paper estimates effects of stay-at-home orders in the United States, a relatively high-incidence, high fatality setting. Meanwhile, we estimate effects of less far-reaching NPIs in a setting where incidence and fatalities have been relatively low.[15]

The low-fatality setting could also explain the differences between our findings and the conclusions in Goolsbee and Syverson (2021) that individual choices, rather than mandates, was the primary driver of economic decline in the pandemic. In our low fatality setting, the perceived risk of death or serious illness from regular social interactions is likely lower. As a result, the fear of infection may have less relative impact on behaviors. Local NPIs, even if they stop short of a full stay-at-home mandate, may therefore have greater relative effect on mobility and economic outcomes.

To illustrate the implications of our findings, we have carried out a simple policy simulation, using the estimated $\hat{\rho}^{POST}$ to calculate the estimated increases in registered unemployment that result from the local NPIs. Results from this exercise are presented in Appendix Table A8. Our point estimates suggest that the local NPIs led to a total of 13,400 excess unemployed workers over the post-period; the large majority (more than 11,500) of these were laid off full-time.



(a) Unemployment rate



(b) Consumer spending

FIGURE 5 Event study estimates by policy type. Each panel represents estimates of Equation (1) with 95% confidence intervals, estimated on a sample of events where the initial policy included at least one measure in the relevant category. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

Our data does not allow us to identify individual unemployed workers. Meanwhile, numbers from Statistics Norway show that the economic impacts of the pandemic have been highly uneven: immigrants, Norwegian-born children of immigrants, and workers with less formal education have higher rates of job loss during the pandemic (see Appendix Figure A14). Data from past recessions suggests that such job loss could have lasting effects on earnings, employment, health and mortality (von Wachter, 2020; Oreopoulos et al., 2012). Immigrants and their descendants may be more vulnerable to these adverse outcomes due to discrimination in the labor market (Hermansen, 2013; Midtbøen, 2016; Oreopoulos, 2011).

Taken at face value, our point estimates would imply that the NPIs averted approximately 400 confirmed cases and 11 hospitalizations. However, these numbers should be interpreted with caution given the problems with identifying causal impacts on incidence when policies are endogenous. The violation of parallel trends imply that the untreated comparison municipalities are not a valid counterfactual for the evolution of incidence and hospitalizations absent the NPIs. While not statistically significant, the estimated pre-trends in Figure 1 suggest that the true impacts on incidence and hospitalization could be significantly larger.

5 | CONCLUSIONS

The rapidly growing literature on the impacts of NPIs finds mixed results on the relative importance of these policies. Our analysis shows that in the low-incidence context of Norway, local NPIs have statistically significant, economically meaningful effects on local economies.

These findings have implications for further research. In particular, the literature on the lasting effects of job loss on later health and labor market outcomes suggests that the documented increases in unemployment have potentially far-reaching implications. Evaluating the long-term impacts of NPIs should remain a priority as more data becomes available.

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CONFLICT OF INTEREST

The author declares that there is no conflict of interest that could be perceived as prejudicing the impartiality of the research reported.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from The Norwegian Institute of Public Health. Restrictions apply to the availability of these data, which were used under license for this study.

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ENDNOTES

[1] As we discuss in Section 2, the Norwegian policy response stopped short of issuing blanket stay-at-home orders or curfews of the kind observed in several US states and European countries.

[2] See https://www.vg.no/spesial/corona/tiltak/ for details. While local and regional policies have been an important part of the policy response from an early stage of the pandemic, there are currently no complete public databases documenting the timing and nature of these policies.

[3] This restriction follows from our event study setup where we require an 6 weeks window around each event.

[4] For hospitalizations, we use the date of the positive test.

[5] The data were accessed through https://github.com/ActiveConclusion/COVID19_mobility.

[6] The Google mobility data contain other categories as well, but due to a non-trivial amount of missing data in some of these categories, they are left out of the analysis.

[7] See Figure 1 for trends in testing, incidence, and hospitalization for treatment and comparison municipalities.

[8] The low number of Christmas mandates, and the significant national policy restrictions regarding Christmas celebrations makes this latter category difficult to study empirically. In this paper, we focus on the first six policy domains.

[9] Ideally, we would like to compare the effectiveness of a single policy versus municipalities that implement multiple policies sequentially. This is complicated, however, by policy endogeneity, as the decision of whether or not to implement a follow-up policy is likely to depend, at least in part, on early effects of the initial policy.

[10] Common support graphs are presented in Appendix Figure A4.

[11] Note that the Google mobility data is already a normalized measure of mobility relative to pre-pandemic baseline levels.

[12] As the credit/debit card data is available only on a weekly basis (not daily), event time here is defined differently relative to the last full pre-implementation week. That is, event week 0 has between 1 and 7 post implementation days. This is different from in the models using daily data, where the NPIs are implemented on day 1 of week 0. Another issue is that there can be a few days delay in when payments are final and visible on the bank transfers.

[13] See Figure A12 in the Appendix for event study graphs.

[14] However, we note that due to the limited precision of our estimates, these estimates are not statistically significantly different from each other.

[15] See Figure A1 for national trends in Covid-19 incidence and hospitalizations.

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APPENDIX A

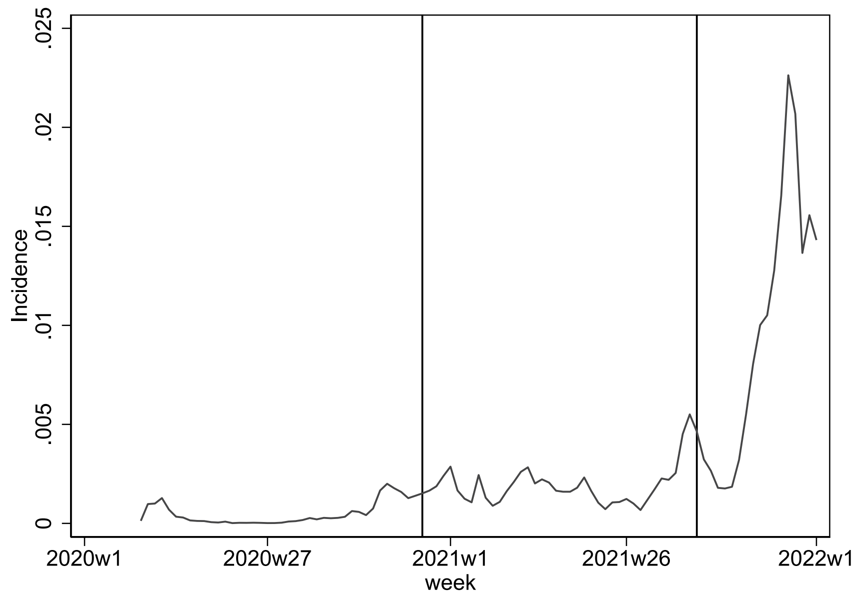


FIGURE A1 Covid-19 in Norway. This figure shows weekly national trends in positive Covid-19 polymerase chain reaction tests from March 2021 to Jan 2022. Our study period, Dec 2020 - Sept 2021, lies within the two vertical lines.

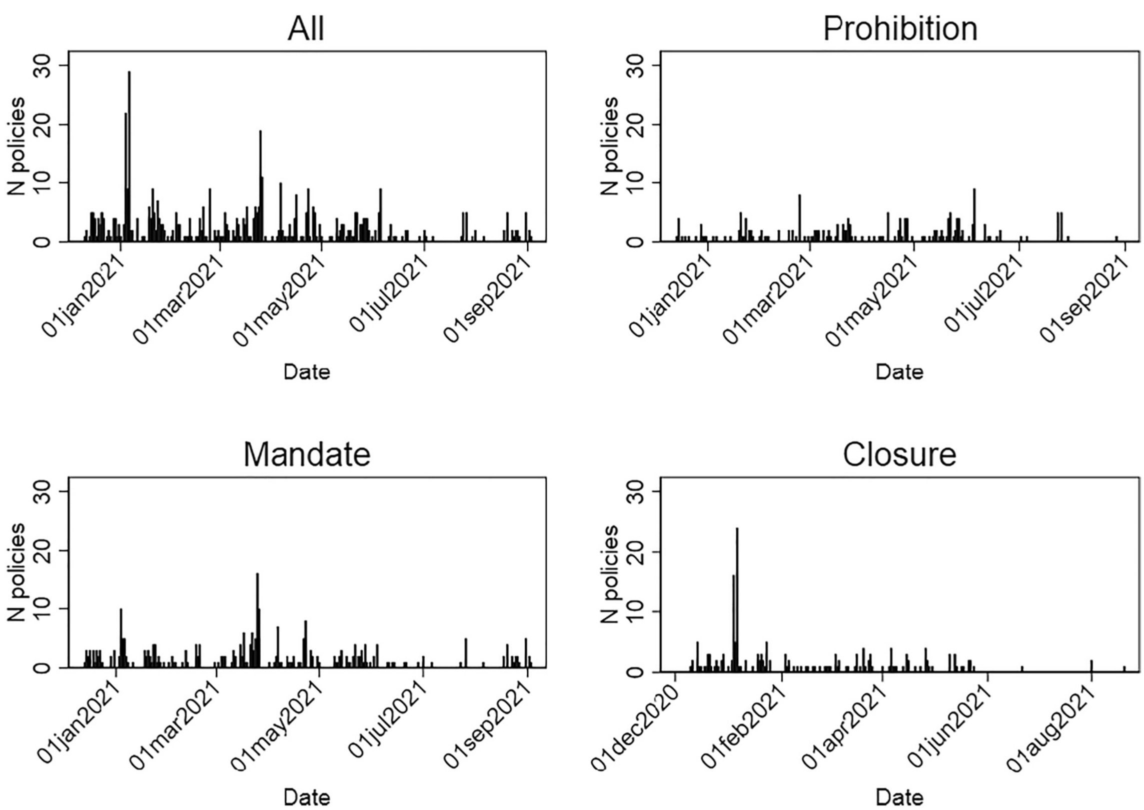


FIGURE A2 Timing of local non-pharmaceutical interventions (NPIs). This figure shows daily number of new local NPIs between December 17, 2020 and September 29, 2021

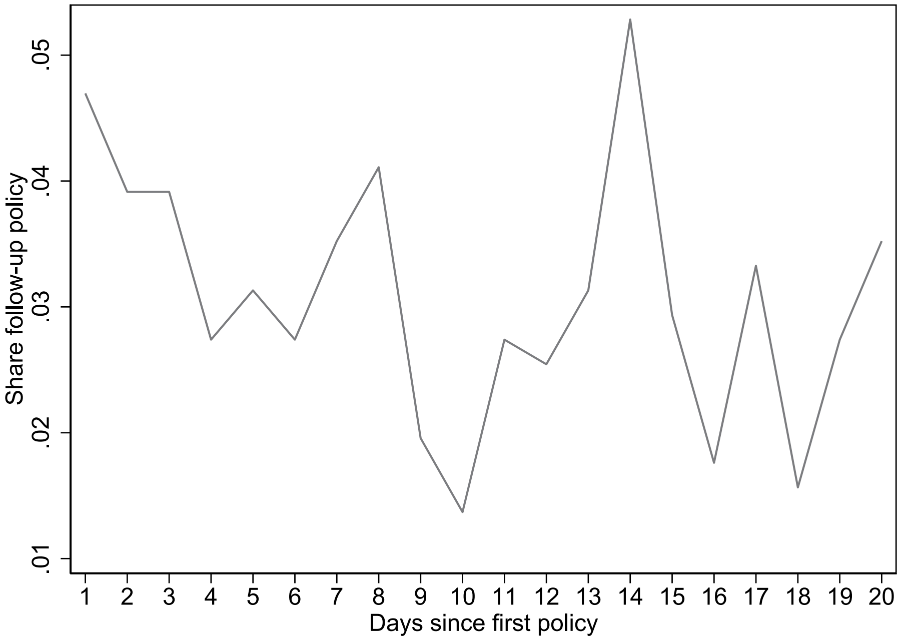


FIGURE A3 Timing of follow-up policies. This figure plots the share of events that introduce one or more follow-up policies each day during the 3 week window after the initial policy is implemented

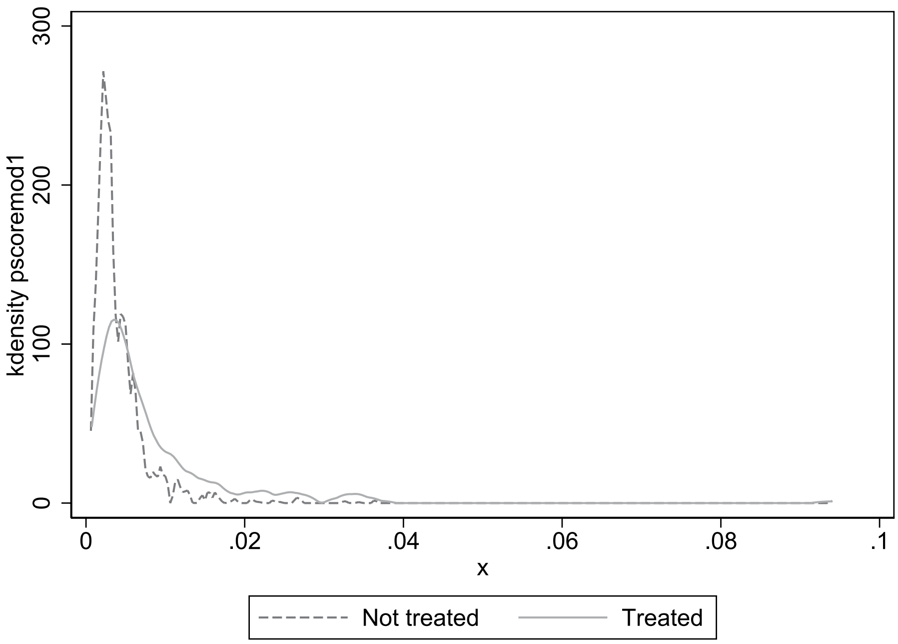


FIGURE A4 Common support propensity weights. The graph shows the common support graphs made by our stacked sample, where the controls can appear multiple times as they can be controls for multiple events. The propensity scores are calculated using the following pre-determined variables: log of population, the share of employed who were employed in hotels and restaurants as of March 1, 2020, municipality centrality class (six dummies), share of population with low income, share of population older than 40 years, share of population living in crowded housing, and share of population with higher education.

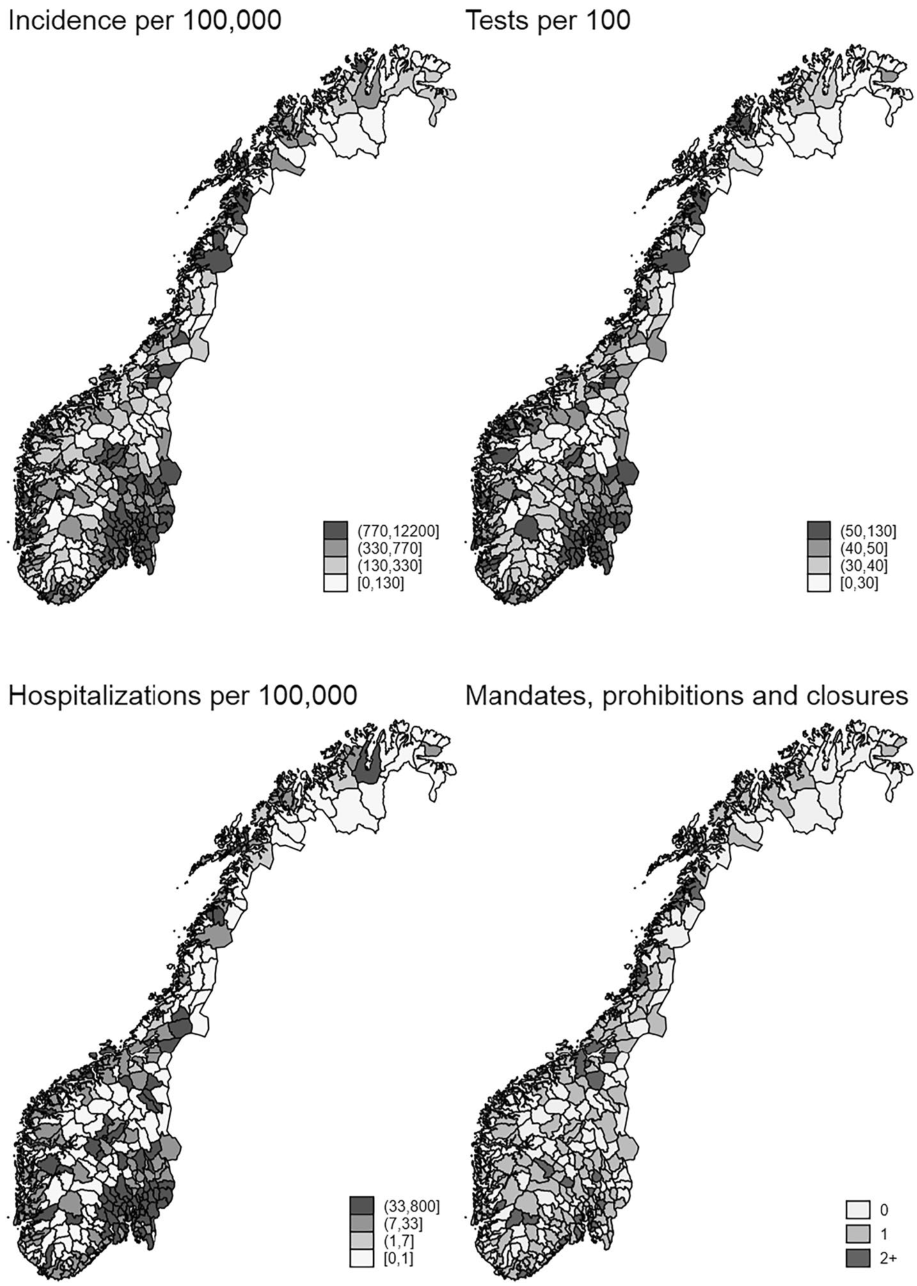


FIGURE A5 Non-pharmaceutical interventions (NPIs), incidence, hospitalizations, and tests. This figure presents the geographic distribution of NPIs and Covid-19 tests, incidence, and hospitalizations across Norwegian municipalities from December 2020 to September 2021.

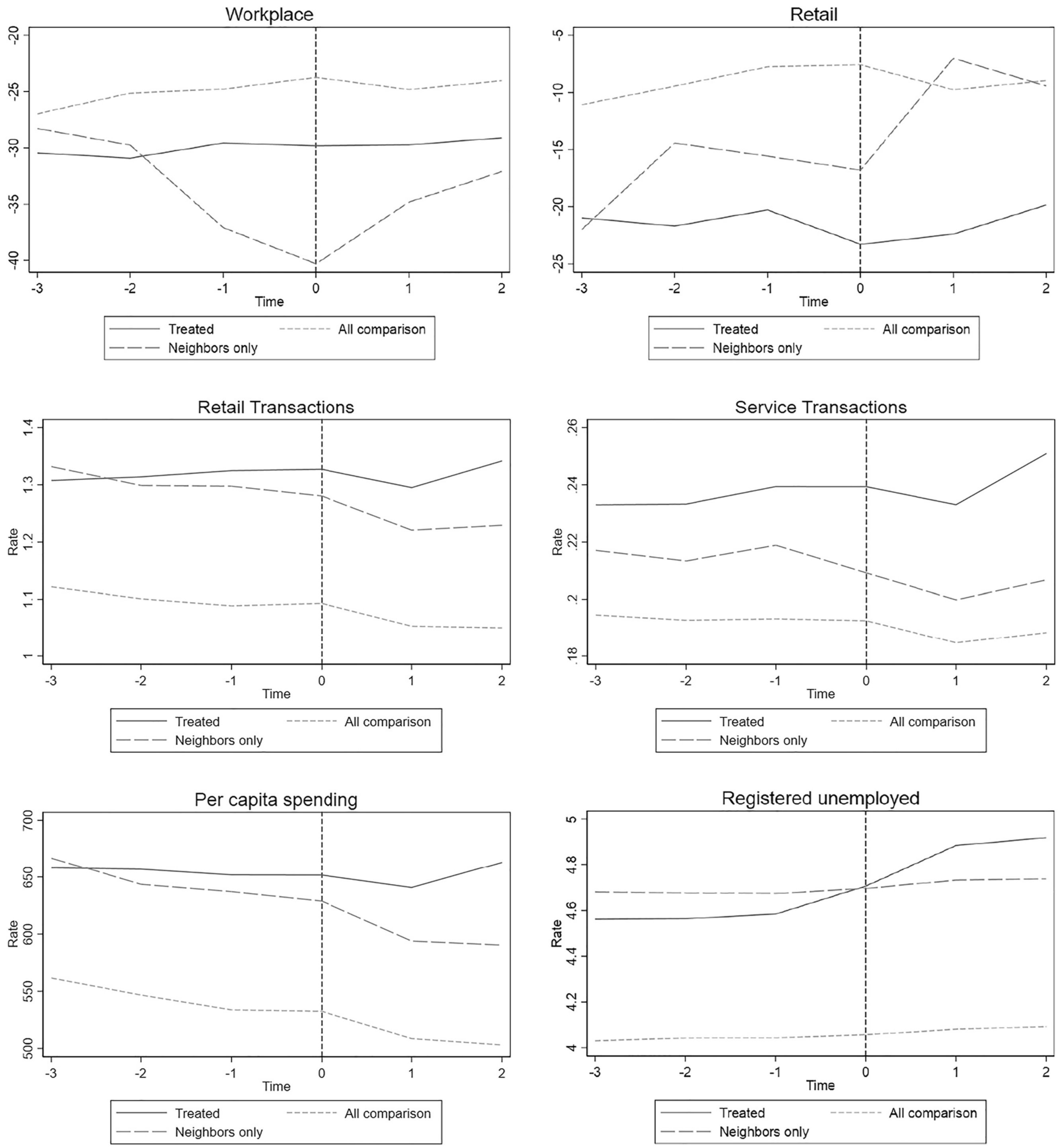


FIGURE A6 Trends in mobility and economic outcomes. This figure shows trends in mobility and economic outcomes. The first two graphs are from Google mobility data, the two middle graphs and the lower left graphs are from DNB transaction data, and the lower right graph is from the Norwegian Labour and Welfare Administration unemployment data.

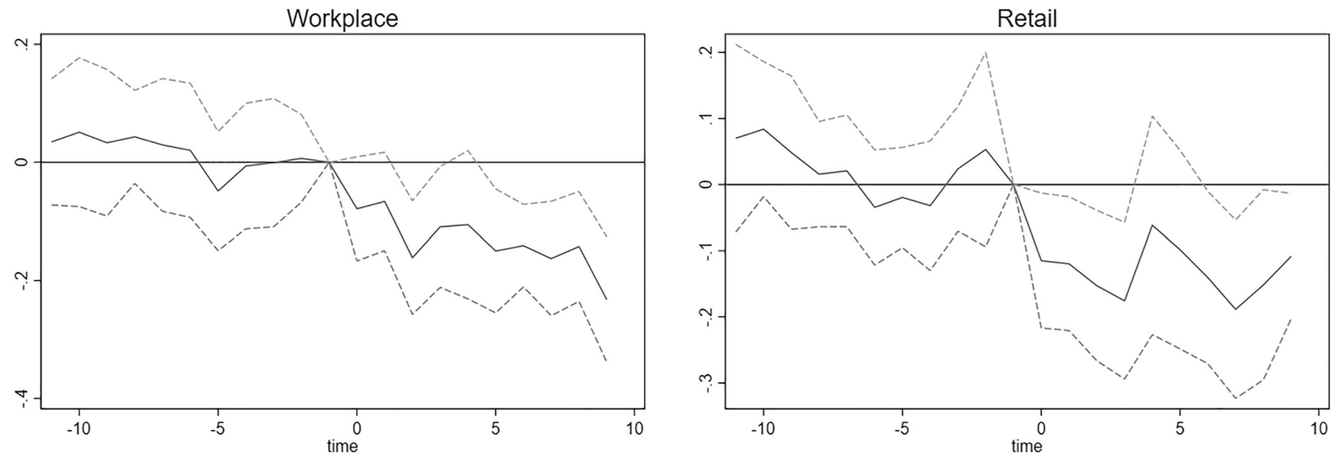


FIGURE A7 Event study estimates: days mobility. This figure shows the event study estimates using a +/− 10 days estimation window. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level

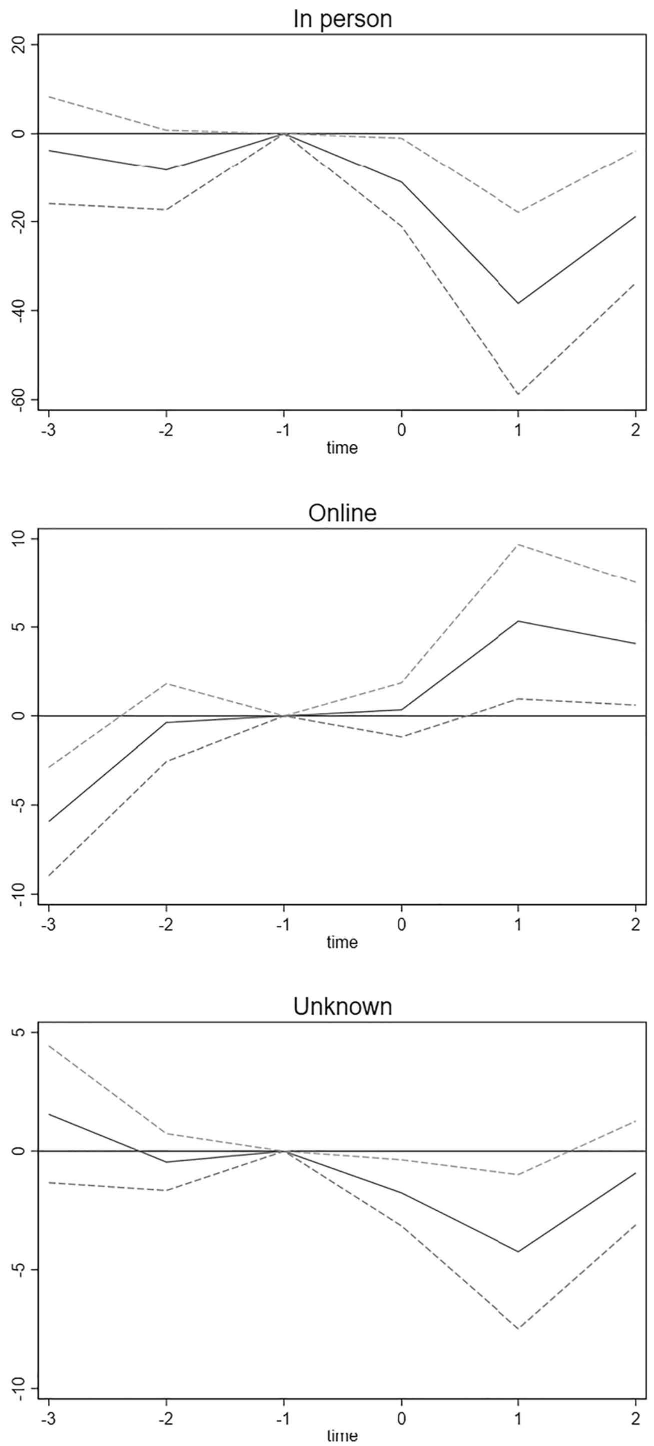


FIGURE A8 Consumer spending - online versus in person. This figure shows the event study estimates using the card data and different types of transactions. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

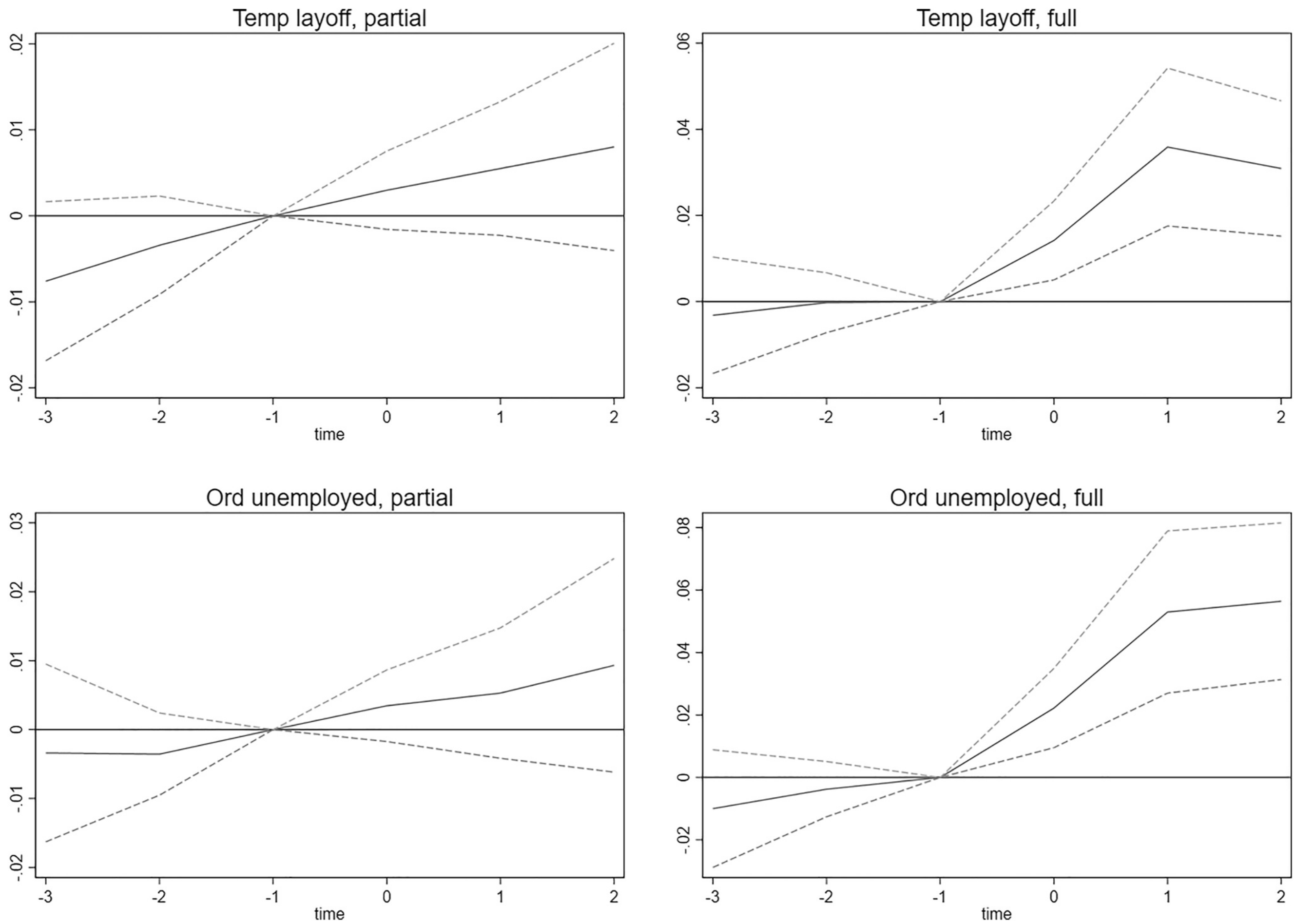
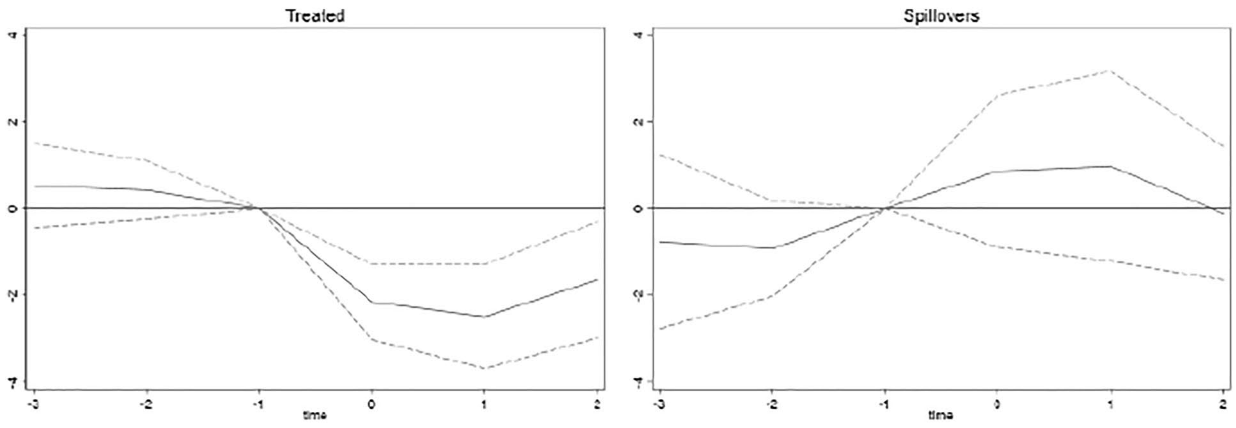
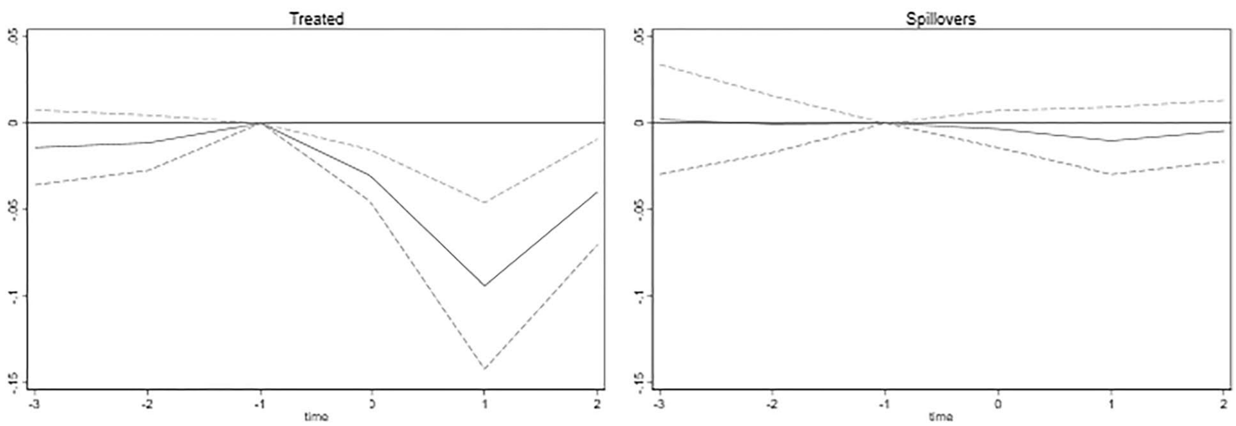


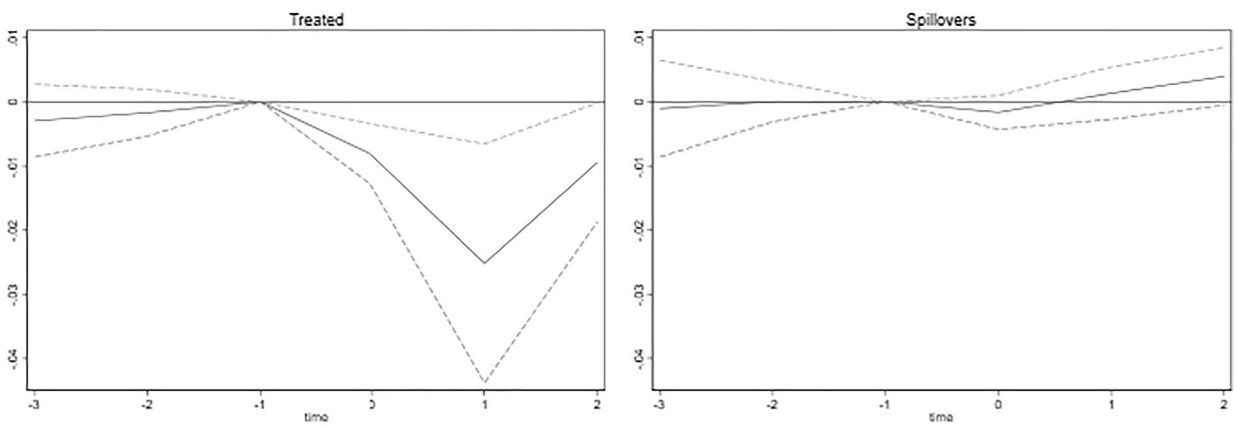
FIGURE A9 Unemployment by category. This figure shows the event study estimates using the unemployment data for different types of unemployment categories. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.



(a) Workplace mobility



(b) Retail transactions



(c) Service transactions

FIGURE A10 Spillovers to neighboring municipalities - additional outcomes. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

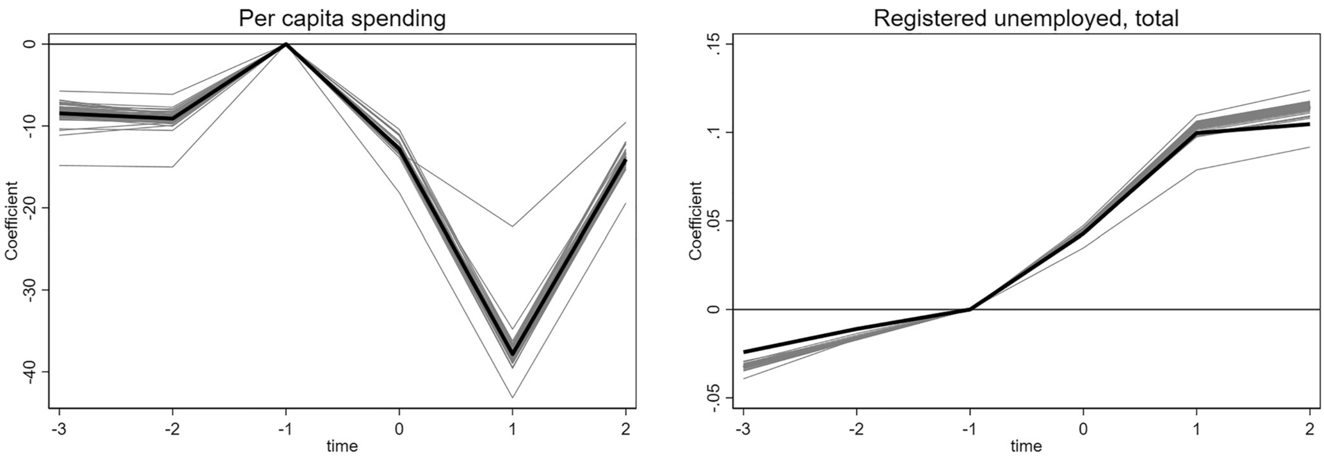


FIGURE A11 Robustness - Leave one out. This figure shows estimates leaving out one by one event. The specification that includes all events is marked as a black line. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

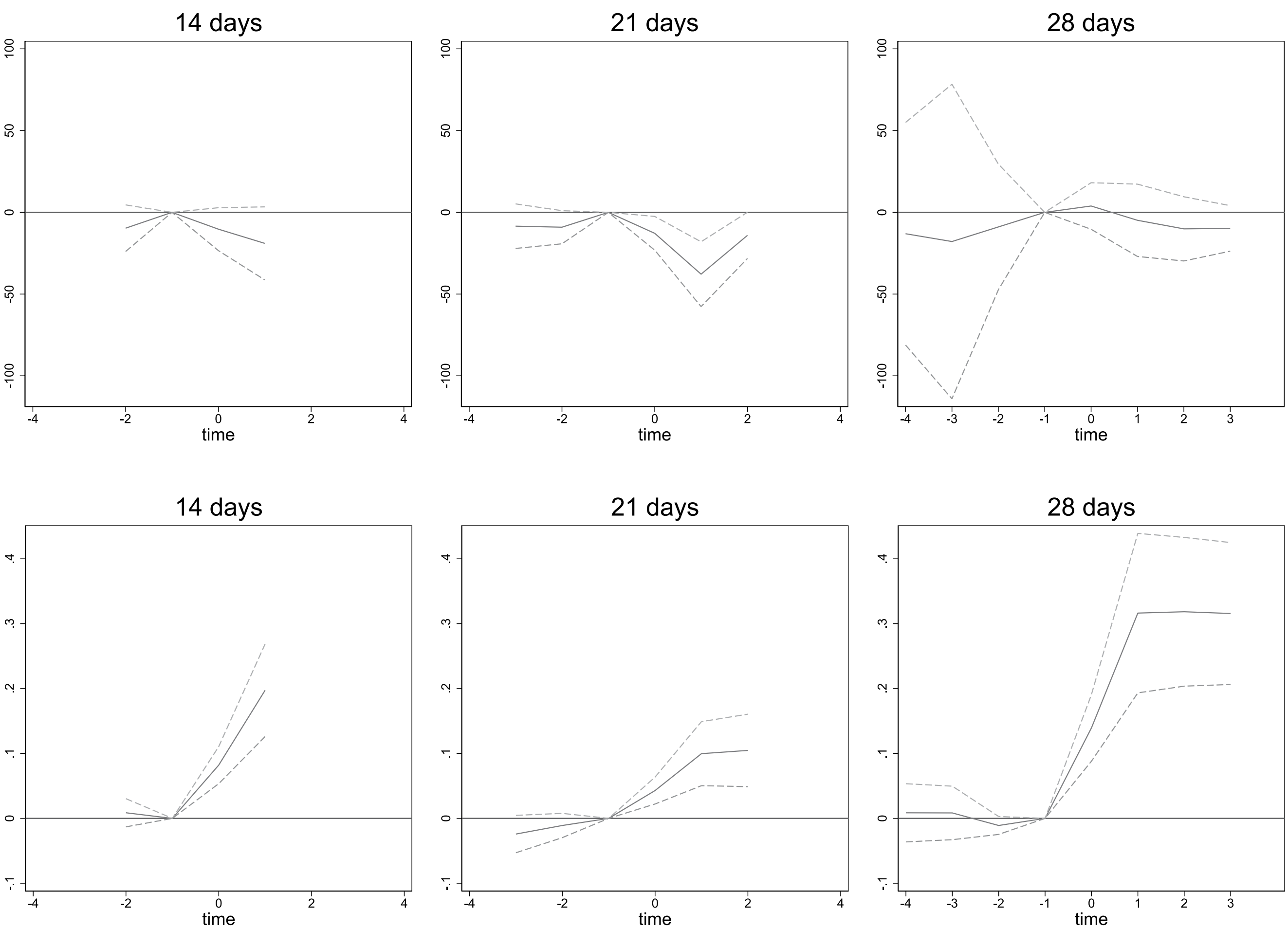


FIGURE A12 Event study - windows. This figure shows estimates for the different estimation windows. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

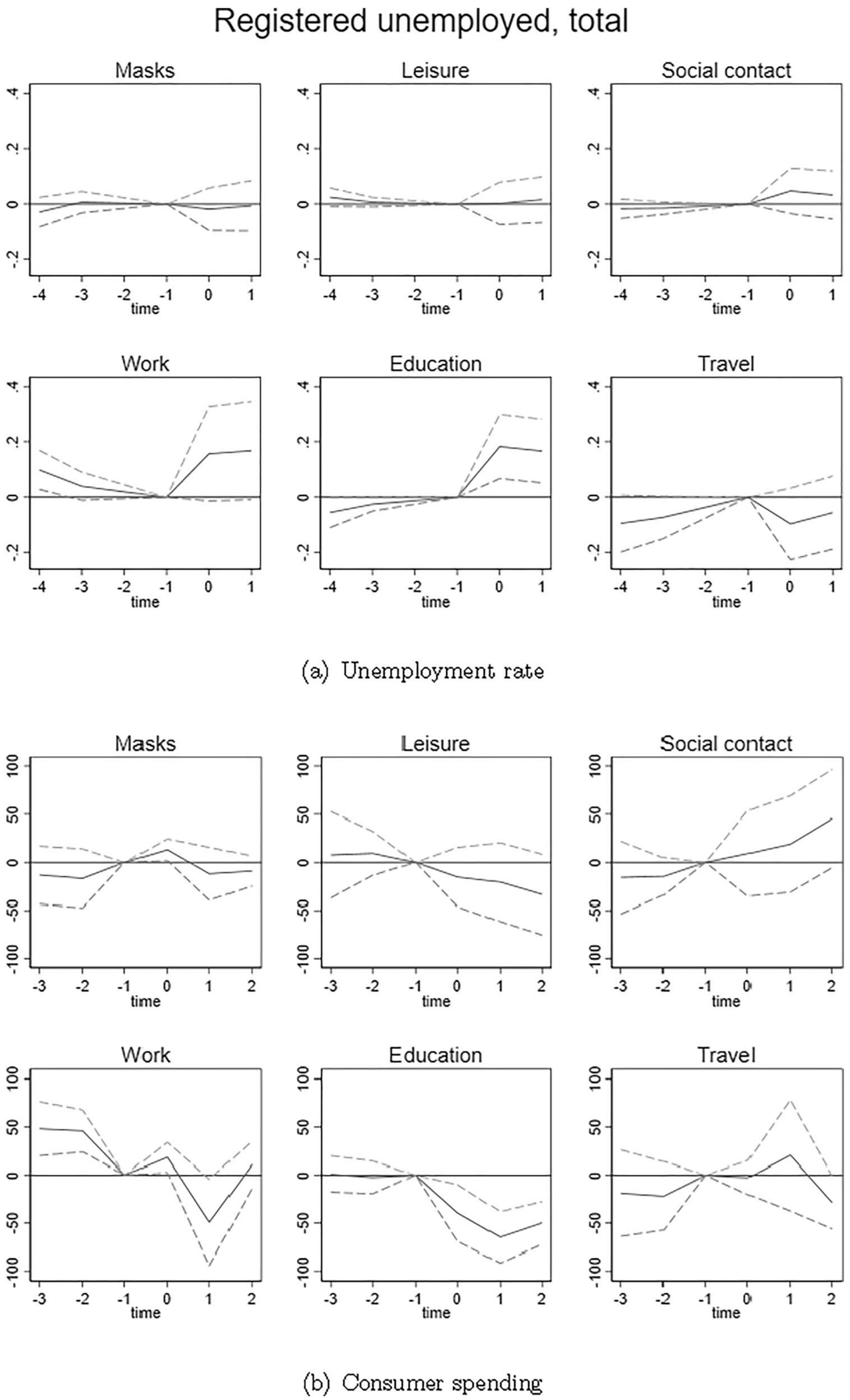


FIGURE A13 Event study estimates by policy type - interaction model. Each panel represents estimates of Equation (3) with 95% confidence intervals, estimated on a sample of events where the initial policy included at least one measure in each category. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

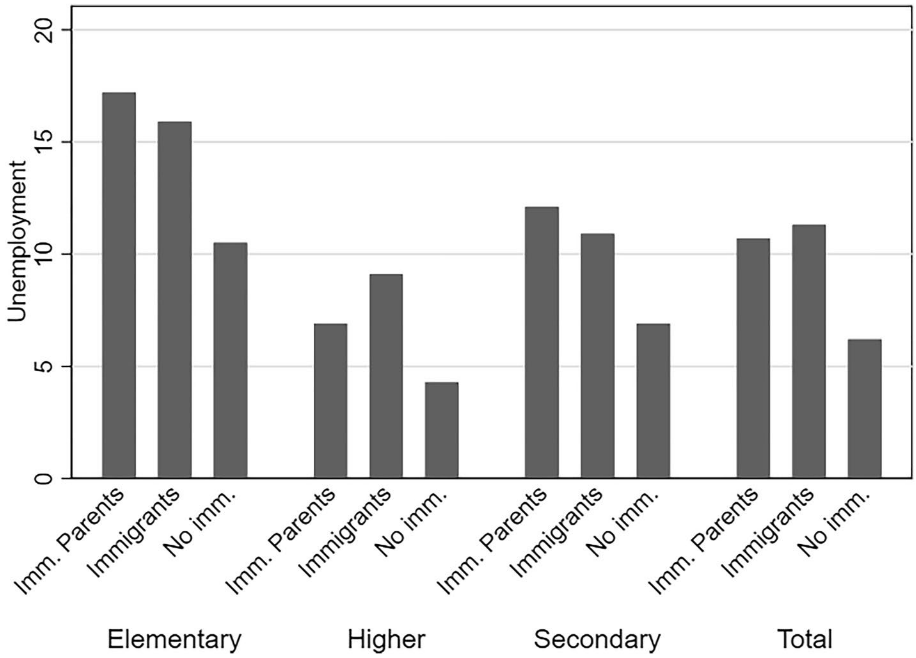


FIGURE A14 Unemployed during the pandemic by education and immigrant group. This figure shows the fraction of people who were employed in 2019, and unemployed in the fourth quarter of 2020. The numbers are divided by educational group and immigration background so that the first bar shows that 17% of those who were employed in 2019 were unemployed in the fourth quarter of 2020 among those who have elementary education and immigrant parents, whereas only 10% of those who have elementary education but native parents lost their job during the same time period. *Source*: Statistics Norway, 2021.

TABLE A1 Types of policies in our sample and in all non-pharmaceutical interventions (NPIs)

	Sample only	All NPIs
Prohibitions	0.378	0.368
Mandates	0.568	0.521
Closures	0.360	0.430
Masks	0.327	0.251
Leisure	0.630	0.656
Social contact	0.661	0.620
Work	0.125	0.148
Education	0.229	0.253
Travel	0.125	0.146
Christmas	0.0157	0.0207
Observations	511	677

Note: This table shows the distribution of local NPIs into types of policies (prohibitions, mandates, and closures) and into policy categories for our sample of NPIs (first column) and for all NPIs implemented during our study period (second column). Mean coefficients.

TABLE A2 Types of policies explained

	Explanation
Masks	Ranges from mandated masks across all public spaces in the whole municipality to mask mandates for health care workers or visitors in nursing homes. The majority is of the form of the former.
Leisure	Limits on the number of guests to concerts or other events, prohibitions of all events and gatherings closures of swimming pools, gyms, restrictions and bans on on-premize alcohol sales.

TABLE A2 (Continued)

	Explanation
Social contact	Restrictions on numbers of visitors to private homes, prohibitions of indoor leisure activities, prohibitions of organized leisure activities, restrictions and bans on on-premize alcohol sales.
Work	Work from home mandates, closure of businesses.
Education	Limited physical presence in kindergartens, schools and universities, mandated online schooling.
Travel	Mandates to uphold physical distance and mask mandates on public transportation, travel bans, testing and quarantining after travels.
Christmas	Restrictions on number of visits and visitors during the holiday season

Note: This table presents an explanation of what the different local NPIs entail.

TABLE A3 Policy transitions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	**Masks**	**Leisure**	**Social contact**	**Work**	**Education**	**Travel**
N inital policies	2.114	2.850	2.562	2.547	3.859	2.564	3.625
	(1.161)	(1.434)	(1.180)	(1.155)	(1.296)	(1.642)	(1.397)
2+ initial policies	0.648	0.760	0.857	0.867	0.953	0.615	0.984
	(0.478)	(0.428)	(0.350)	(0.340)	(0.213)	(0.489)	(0.125)
N follow-up policies	0.969	0.934	0.978	1.065	1.234	1.419	1.063
	(1.397)	(1.367)	(1.415)	(1.436)	(1.640)	(1.683)	(1.622)
1+ followup policies	0.419	0.413	0.425	0.456	0.516	0.547	0.453
	(0.494)	(0.494)	(0.495)	(0.499)	(0.504)	(0.500)	(0.502)
Masks	0.0802	0.0838	0.0776	0.0828	0.141	0.120	0.156
	(0.272)	(0.278)	(0.268)	(0.276)	(0.350)	(0.326)	(0.366)
Leisure	0.303	0.287	0.317	0.334	0.328	0.376	0.281
	(0.460)	(0.454)	(0.466)	(0.472)	(0.473)	(0.486)	(0.453)
Social contact	0.305	0.299	0.304	0.343	0.313	0.402	0.250
	(0.461)	(0.459)	(0.461)	(0.475)	(0.467)	(0.492)	(0.436)
Work	0.0783	0.0659	0.0776	0.0828	0.125	0.154	0.0938
	(0.269)	(0.249)	(0.268)	(0.276)	(0.333)	(0.362)	(0.294)
Education	0.157	0.144	0.161	0.169	0.266	0.299	0.172
	(0.364)	(0.352)	(0.369)	(0.375)	(0.445)	(0.460)	(0.380)
Travel	0.0450	0.0539	0.0404	0.0533	0.0625	0.0684	0.109
	(0.208)	(0.226)	(0.197)	(0.225)	(0.244)	(0.253)	(0.315)
Observations	511	167	322	338	64	117	64

Note: This table summarizes patterns of policy transitions in our main event sample. Row (1) shows the average number of policy domains covered by the initial NPI. Row (2) shows the share of initial NPIs that cover 2 or more policy domains, row (3) shows the number of distinct follow-up policies in the 3 weeks window following the initial implementation. Row (4) shows the share of events that have one or more follow-up policies during the 3 weeks window. Rows (5) - (10) shows the share of sample events with one or more follow-up NPIs in each policy domain. Column (1) shows results for the full sample; columns (2) - (7) show results by initial (non mutually exclusive) policy domain.

TABLE A4 Testing, incidence, and hospitalization Diff-in-Diff

	(1) Incidence	(2) Tests	(3) Hospitalizations
PRE	−1.30	−45.3***	−0.0456
	(0.911)	(9.65)	(0.0329)
DURING	−0.558	56.1***	−0.00481
	(0.624)	(15)	(0.0352)
POST	−3.08***	−23.4**	−0.0849***
	(0.734)	(11.6)	(0.0316)
N	5,073,558	5,073,558	5,073,558
Pre-Mean	10.2	405	0.330
Relative change	−0.303	−0.0577	−0.257

Note: The table presents estimates for the different COVID-19 outcomes. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

TABLE A5 Spending diff-in-diff

	(1) Per capita spending	(2) In person	(3) Online
PRE	−8.78	−6.45	−3.02***
	(5.75)	(5.24)	(1.02)
DURING	−12.8**	−11.0**	0.177
	(5.25)	(5.15)	(0.770)
POST	−25.9***	−27.6***	4.39**
	(7.96)	(8.29)	(2)
N	287,742	287,742	287,742
Pre-Mean	656	498	88.7
Relative change	−0.0396	−0.0555	0.0495

Note: The table presents estimates for different types of card transactions. Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level.

TABLE A6 Registered unemployment diff-in-diff

	(1) Unemp tot	(2) OU- PT	(3) TL - PT	(4) OU-FT	(5) TL - FT
PRE	−0.0176	−0.00347	−0.00551*	−0.00687	−0.00171
	(0.0115)	(0.00416)	(0.00335)	(0.00671)	(0.00498)
DURING	0.0429***	0.00346	0.00299	0.0222***	0.0142***
	(0.0106)	(0.00264)	(0.00232)	(0.00646)	(0.00466)
POST	0.102***	0.00732	0.00677	0.0547***	0.0334***
	(0.0265)	(0.00612)	(0.00492)	(0.0128)	(0.00840)
N	724,794	724,794	724,794	724,794	724,794
Pre-Mean	4.66	1.28	0.640	2.11	0.635
Relative change	0.0219	0.00574	0.0106	0.0260	0.0526

Note: Estimates from Equation (1) with 95% confidence intervals. Models include municipality-by-event and calendar time-by-event fixed effects. Standard errors clustered at the municipality level. OU-PT: ordinary unemployed, part time; TL-PT: temporarily layoff, part time; OU-FT: ordinary unemployed, full time; TL-FT: temporarily layoff, full time.

TABLE A7 Summary statistics, by event window

	(1) 1+ mandate	(2) No mandate	(3) 1+ mandate	(4) No mandate
Incidence (per 100,000)	10.33	5.806	9.613	4.513
	(14.92)	(11.22)	(14.10)	(10.69)
Hospitalizations (per 100,000)	0.328	0.185	0.306	0.138
	(1.211)	(1.021)	(1.192)	(1.057)
Tests (per 100)	432.8	298.5	406.3	270.3
	(298.7)	(242.9)	(297.8)	(238.5)
Age	40.18	41.23	40.35	41.69
	(1.970)	(2.470)	(2.181)	(2.420)
Foreign born	0.175	0.147	0.174	0.133
	(0.0510)	(0.0563)	(0.0568)	(0.0481)
Higher ed	0.389	0.334	0.378	0.312
	(0.0969)	(0.0977)	(0.101)	(0.0871)
Cramped housing	0.109	0.0952	0.109	0.0874
	(0.0363)	(0.0369)	(0.0402)	(0.0303)
Low income	0.114	0.107	0.113	0.103
	(0.0231)	(0.0227)	(0.0243)	(0.0205)
Population (in 1000s)	168.2	100.5	163.7	61.55
	(207.6)	(197.4)	(231.1)	(149.2)
N	18,172	4,770,472	24,472	5,294,968
Window	14	14	28	28
Events	649	649	437	437

Note: Population-weighted averages. Each sample contains treated municipalities and clean controls in a symmetric window around the implementation of new NPIs. See Section 2 for details on sample construction and variable definitions.

TABLE A8 Policy simulations

Outcome	Point estimates	Lower bound	Upper bound
Tests	−3063	−6057	−68
Incidence	−404	−593	−215
Hospitalizations	−11	−19	−3
Registered unemployed, total	13,399	6574	20,224
Temp layoff, partial	888	−380	2155
Temp layoff, full	4378	2214	6543
Ord unemployed, partial	959	−620	2538
Ord unemployed, full	7174	3877	10,470

Note: Table shows results from simple policy simulations showing estimated changes in the absolute number of tests, confirmed cases, hospitalization, and unemployment. Numbers obtained by multiplying the estimated $\hat{\rho}^{POST}$ from Equation (2) by the total population in treated municipalities summed across events: $\Delta_{pol} = \sum_k treatpop_k \times \hat{\rho}^{POST}_{pol}$. Lower and upper bound obtained by the lower and upper bounds of the 95% confidence intervals associated with $\hat{\rho}^{POST}$.