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ABSTRACT

There is currently a heated debate about making face masks compulsory in public spaces to contain COVID-19. A key concern is that such policies could lead to risk compensating behaviour and thereby undermine efforts to maintain social distancing and reduce mobility. We provide first evidence on the impact of compulsory face mask policies on community mobility. We exploit the staggered implementation of policies by German states and measure community mobility using geolocated smartphone data. We find no evidence suggesting that compulsory masking policies affect community mobility in Germany. We can rule out even small increases larger than 0.03 standard deviations.

JEL Classification: D9, H12, I12, I18

Keywords: COVID-19, face masks, social distancing, community mobility

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1 Introduction

The ongoing coronavirus disease (COVID-19) pandemic has, as of June 2020, led to the death of over 380,000 people [WHO, 2020] and is expected to trigger a severe economic crisis, with global GDP growth predicted to fall to -3% [IMF, 2020]. One of the main policy objectives in economics is to maximise social welfare. During a pandemic, a key constraint in the maximisation problem is that disease transmission needs to be contained [Budish, 2020]. Governments have attempted to contain the spread of COVID-19 through non-pharmaceutical interventions targeting citizens' behaviour, which centre around reducing citizens' mobility and social contacts in order to disrupt the chain of transmission. Examples include closing schools, banning public gatherings, social distancing rules or lock-downs forbidding individuals to leave their homes [Mellan et al., 2020].

There is currently a heated debate about whether the general public should be required to wear protective face masks to further reduce the spread of COVID-19. For instance, the US Centres for Disease Control [CDC, 2020a] advocate for the use of face masks by the general public, whilst the World Health Organization does not [WHO, 2020].[1] Nonetheless, over 50 countries already require the wearing of face masks in public spaces [Al Jazeera News, 2020]. Those arguing against introducing compulsory face mask policies frequently point to limited evidence on effectiveness, concerns about individuals wearing masks incorrectly, as well as high demand on masks reducing availability for healthcare workers [Feng et al., 2020, Greenhalgh et al., 2020]. Another key argument against making face masks compulsory, which motivates this paper, is the concern that individuals will feel safer and might therefore disregard the most important public-health advice to contain the spread of COVID-19 – which is to reduce mobility and maintain social distancing [Greenhalgh et al., 2020]. This concern has been voiced by key actors in the global response to COVID-19. For example, the coordinator of the White House coronavirus response, Dr Deborah Birx, noted that "asking all Americans to wear masks could inadvertently signal that Americans can abandon social distancing" [The New York Times, 2020]. Similarly, the UK Government's Scientific Advisory Group for Emergencies underlined that face masks "could make people feel invincible and therefore be less likely to adhere to other rules around socialising and staying at home" [The Guardian, 2020b]. Whether compulsory face mask policies are welfare enhancing therefore depends critically on both the direct effect of face masks on disease transmission, as well as

[1] As of June 5$^{\text{th}}$ the WHO recommends that medical face masks should be worn by health workers, people with symptoms or those caring for them. In terms of non-medical (fabric masks) "the WHO does not recommend their widespread use among the public for control of COVID-19" [WHO, 2020], but recommends their use exclusively in spaces where social distancing cannot be maintained.

indirect effects via changes in human behaviour. In this paper we provide first evidence on the effect of face masks on community mobility.

The effect of compulsory face mask policies on citizen's mobility is *a priori* ambiguous. In line with concerns of policymakers [The Guardian, 2020a,b], face masks could increase mobility due to risk compensation. A large economics literature examines behavioural responses to changes in perceived or actual risk [Peltzman, 1976]. Whilst the findings are mixed overall [Godlonton et al., 2016], a number of studies find evidence for risk-compensating behaviour, for instance, more risky sexual behaviour among recipient of HIV or HPV treatments or vaccines [Kapoor, 2008, Eaton and Kalichman, 2007], car accidents as a result of seat belt laws [Blomquist, 1989] and bicycle helmets triggering dangerous driving by cars [Walker, 2007]. Risk compensating behaviour is therefore a plausible mechanism through which protective technologies such as face masks, that reduce personal risk (whether actual or perceived), could lead to an increase in mobility.

In contrast, salience and what we refer to as the "hassle factor" provide reasons to expect that compulsory face mask policies reduce mobility. Face masks differ from previously studied risk-reducing technologies as they need to be worn constantly (unlike one-off treatments such as vaccines). Face masks might therefore serve as a constant reminder to citizens that the COVID-19 pandemic is ongoing and serious. It is therefore possible that compulsory face masks increase the salience of the COVID-19 pandemic in individuals' decision making about their mobility [Van Der Pligt and De Vries, 1998]. Availability bias (where individuals judge events that come to mind more easily to be more likely) potentially exacerbates such an effect. For instance, studies have found that frequent exposure to drug advertisement influences individuals' perceptions about disease prevalence [An, 2008]. Face masks might similarly inflate perceptions about the true prevalence of COVID-19 – which could affect mobility decisions about whether to visit any public space (i.e. not only locations where face masks are required by law). Another way in which face masks differ from previously studied risk-reducing technologies is that that they are bothersome to use (much more so than, for instance, seat belts). Wearing a face mask creates disutility, as wearers suggest that masks can be hot, uncomfortable, humid, itchy and odorous [Li et al., 2005]. This disutility, which we refer to as the "hassle factor", can spoil the fun of non-essential outings and could incentivise individuals to minimise the frequency of essential outings – which could reduce mobility. Due to the extensively studied process of adaptation, through which one quickly adjusts to new or changed circumstances, we expect that any such effects should be short-lived [Dolan and Kahneman, 2008]. In addition, as the hassle factor only comes into play when masks are worn, it should primarily affect mobility in locations where face masks are required by law.[2]

This study provides first evidence on the effect of compulsory face mask policies on community mobility. To isolate the causal effect of such policies, we use a difference-in-differences design, which exploits the staggered introduction of policies requiring the wearing of face masks in shops and public transport by German states (Bundesländer). Saxony was the first state to introduce compulsory face masks on the 20th of April 2020, Schleswig-Holstein was the last to do so on the 29th of April 2020. To measure community mobility, we rely on the Google COVID-19 Community Mobility Reports, which use geo-located smartphone data to provide aggregated (state-level) measures of the number of hours spent at home as well as the number of times public spaces are visited each day. Community mobility has been previously measured in this way in epidemiological studies [Mellan et al., 2020] to estimate the basic reproduction number R_0, which is a key parameter of transmission intensity and therefore highly relevant for containing the spread of COVID-19.

We measure community mobility within each German state between March 23rd and May 21st 2020. Our main outcome is an aggregate measure of mobility in public spaces, which captures visits to grocery and pharmacy shops, workplaces and transport hubs. We focus on an aggregate measure of mobility in public spaces, as we expect policymakers to be more interested in changes in overall mobility patterns. Nonetheless, we also report changes in mobility for specific public locations as well as in places of residence.

We do not find evidence to suggest that compulsory face mask policies affect community mobility in public spaces in Germany. Effect sizes are precisely estimated and we can rule out even small increases in mobility that are larger than 0.03 SD. We only find a small reduction in average community mobility on the day of the policy change (-0.14 SD), but find no longer-term effects thereafter. We also find no evidence suggesting that, beyond a short-term increase during the first four days, compulsory face mask policies affect the number of hours spent at home, which is another "catch-all" measure of community mobility. We take this to suggest that these policies are complementary to interventions aimed at reducing mobility and disrupting the chain of transmission of COVID-19. When we examine mobility in specific locations, we find that mobility patterns are lower in grocery shops and pharmacies for five days following the introduction of compulsory face masks, but that the magnitude of the reduction is modest. We find no effects on mobility patterns in workplaces or transport hubs (subways, buses or train stations).

[2] In a setting where face masks are voluntary, an additional reason why masks could reduce mobility is that individuals perceive masks as a signal for a larger preferred social distance by the wearer, as found by Seres et al. [2020]

This paper makes three main contributions. First, it provides new evidence that is crucial to ongoing policy debates on how to best manage the COVID-19 pandemic. Policymakers and researchers have expressed concerns that making face masks compulsory could lead people to disregard measures that are key for containing COVID-19. We are unable to provide evidence on important individual-level behaviours such as hand-washing or social distancing. However, community mobility plays a key role in reducing the spread of COVID-19 [Mellan et al., 2020] and we find no evidence to suggest that, in Germany, compulsory face mask policies led to an increase in mobility. If anything, we observe a temporary decrease in mobility in grocery shops and pharmacies. This is important information for policy-makers considering the costs and benefits of compulsory face mask policies, as such analyses likely do not have to account for spillovers on mobility.

Second, we contribute to the small but rapidly growing literature using aggregate GPS data to study the effect of policies trying to contain the spread of COVID-19 on mobility patterns [Allcott et al., 2020, Wellenius et al., 2020, Dasgupta et al., 2020]. Using GPS data is one of the main alternatives to using surveys [Briscese et al., 2020, Jørgensen et al., 2020], which likely do not provide reliable data on mobility due to social desirability bias [Daoust et al., 2020].

Finally, our findings speak to the behavioural economics literature on risk compensation [Godlonton et al., 2016, Peltzman, 1976, Kapoor, 2008, Miller and Blomquist, 1989, Walker, 2007]. To our knowledge, only one previous study has examined the effect of face masks on risk compensating behaviour, finding that physical distancing increases by on average 9 cm when individuals wear masks - supposedly because others interpret face masks as a signal for a larger preferred distance [Seres et al., 2020]. Our paper complements the field experiment (N=300) by Seres et al. [2020] by providing first evidence from a large sample. Our study is also the first to investigate the general equilibrium effect of introducing compulsory face masks - where signalling is unlikely to play a role. We show that, even though compulsory face mask policies may reduce personal risk and risk imposed on others, there is no evidence of an undesirable aggregate effect on community mobility.

2 Background

Germany is frequently put forward as a positive example for how to manage the COVID-19 pandemic [The Guardian, 2020c, Stafford, 2020]. As of June 5$^{\text{th}}$ 2020, there have been

183,271 confirmed cases of COVID-19 in Germany and only 8,613 deaths [RKI, 2020].

Germany's 16 states introduced compulsory face mask policies at different times in late April 2020 (see Table 1). Saxony was the first state, on April 20th 2020, followed by Saxony-Anhalt on April 23th and Thuringia on April 24th. Twelve states adopted compulsory face mask policies on April 27th, and Schleswig-Holstein followed suit on April 29th. In all states, the face mask requirement is fulfilled by wearing any type of face covering (including scarves or bandannas) and children under six and people with disabilities are usually exempt. All states except Berlin made face masks compulsory in public transport and in shops at the same time. In Berlin, face masks first became compulsory in public transport (April 27th) and in shops two days later. As of June 5th, compulsory face mask policies remain in place in all German states, although some state governments have expressed a desire to abolish the requirement [Guardian, 2020, Die ZEIT, 2020].

Even though compulsory face mask policies make it illegal not to wear a mask in designated spaces, only nine out of 16 states introduced fines for not wearing masks.[3] Overall, the German approach "seems to be characterised more by appealing on compliance to rules rather than on enforcing them by micromanagement law" [Stafford, 2020].

Table A1 in the Appendix shows when other policies related to COVID-19 (e.g. school, retail and restaurant re-openings as well as lock-downs being relaxed) were implemented, given that these policies may have also affected community mobility in the study period. In some instances, these additional policy changes coincided with the introduction of compulsory face mask policies. Most of the overlap relates to secondary schools re-openings, which coincided with the introduction of compulsory face masks in eleven of the sixteen states. Retail re-openings were implemented on the same day as compulsory face mask policies in only three states, compared to one state for lock-down relaxation and none for restaurant re-openings.

Compulsory face mask policies appear to be widely supported by the German public. Nationally representative survey data suggest that, before the first state-wide introduction in late April 2020, compulsory face mask policies were supported by 86% of the population and support remained high at 79% one month later [BfR, 2020]. The number of individuals who report always wearing masks in public spaces (public transport, supermarkets, shops or main roads) was 11% on April 2nd, 26% on April 24th (as the first compulsory face mask policies were implemented) and to 56% on April 30th (when face masks were compulsory

[3] Fines of varying amounts are in place in Baden-Wuerttemberg, Bavaria, Berlin, Hamburg, Hesse, Lower Saxony, Mecklenburg-West Pomerania, North Rhine-Westphalia and Rhineland-Palatinate. In some cases (e.g. North Rhine-Westphalia), fines vary within the state and are enforced at the discretion of local councils.

across the country) [YouGov, 2020]. As these data are self-reported and likely suffer from social desirability bias, we expect these to be upper-bound estimates of face mask use. As far as we are aware, there are no nationally representative data on actual face mask use. Evidence from a field experiment in Berlin (N=300), conducted before masks became compulsory, found that only 17% of people were wearing face masks in stores, supermarkets or post-offices [Seres et al., 2020]. These estimates are substantially lower than what is self-reported in Berlin (48% for the following week [YouGov, 2020]), although self-reported data are not representative at the state-level.

Table 1: Implementation of compulsory face mask policies by German states in April 2020

	Face masks not compulsory	Face masks compulsory
April 19th	SN, ST, TH, BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL, SH	
April 20th	ST, TH, BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL, SH	SN
April 21st	ST, TH, BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL, SH	SN
April 22nd	ST, TH, BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL, SH	SN
April 23rd	TH, BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL, SH	SN, ST
April 24th	BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL, SH	SN, ST, TH
April 25th	BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL, SH	SN, ST, TH
April 26th	BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL, SH	SN, ST, TH
April 27th	SH	SN, ST, TH, BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL
April 28th	SH	SN, ST, TH, BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL
April 29th		SN, ST, TH, BW, BY, BE, BB, HB, HH, HE, MV, NI, NW, RP, SL, SH

Note: SN (Saxony), ST (Saxony-Anhalt), TH (Thuringia), BW (Baden-Wuerttemberg), BY (Bavaria), BE (Berlin), BB (Brandenburg), HB (Bremen), HH (Hamburg), HE (Hesse), MV (Mecklenburg-West Pomerania), NI (Lower Saxony), NW (North Rhine-Westphalia), RP (Rhineland-Palatinate), SL (Saarland), SH (Schleswig-Holstein).

Several factors could explain why some states implemented compulsory face mask policies earlier than others. First, one could see the staggered introduction as a process of bottom-up policy diffusion. For example, the state of Thuringia implemented compulsory face mask policies after its second-largest city, Jena, became the first city in Germany to do so [Der

Spiegel, 2020]. The federal government largely took a back seat and continued to recommend voluntary face mask use until April 22$^{\text{nd}}$ 2020 [Bundesregierung, 2020]. A second interpretation is that variation in the supply of face masks, and concerns about panic-buying, played a role. For example, the governments of Bavaria, Lower Saxony and North Rhine-Westphalia initially resisted moves to introduce compulsory face masks on these grounds [DW, 2020, Nordbayerischer Kurier, 2020, Aachener Zeitung, 2020]. Third, geographic variation in transmission rates could have prompted some cities (and states) to move earlier than others. For example, Jena was considered a COVID-19 "hotspot" before it introduced compulsory face masks [MDR, 2020].

Even though some evidence from the US suggests that party ideology is associated with support for face masks [Pepinsky, 2020], this does not appear to be the case in Germany. The first city to implement compulsory face mask policies (Jena) is governed by a mayor from the liberal *FDP*. The first state to do so is governed by the centre-right *CDU* and another early mover (Thuringia) is governed by the left-wing *Die Linke*.

3 Data and methods

3.1 Data

To measure community mobility, we use the publicly available Google COVID-19 Community Mobility Reports for Germany.[4] These data capture daily changes in mobility patterns in each German state based on GPS data from Google Account users who have opted-in to the Location History feature. We use mobility data from the period between March 23$^{\text{rd}}$ and May 21$^{\text{st}}$ 2020. We exclude observations from before the national lock-down (which was announced on March 22$^{\text{nd}}$ 2020 and came into force the day after), as mobility reduced drastically in the preceding days, which could distort our estimates (see Figure 1 below).

Google's COVID-19 Community Mobility Reports are disaggregated by place categories. The data capture the number of visits to groceries and pharmacies (grocery markets and food shops, food warehouses, farmers markets, drug stores, and pharmacies), transit stations (transportation hubs including subway, bus, and train stations), parks (local and national parks, beaches, marinas, public gardens) and retail and recreation (restaurants, cafes, theme parks, shopping centres, museums, libraries and cinemas) [Aktay et al., 2020]. The data also

[4] Available at: https://www.google.com/covid19/mobility/

capture mobility patterns for places of work and residence. For workplaces, Google uses the relative frequency of visits, as well as time and duration to calculate how many individuals spent more than one hour at their place of work [Aktay et al., 2020]. A similar process is used to calculate the number of hours spent in places of residence [Aktay et al., 2020].

For each day, the data record the percentage change in the number of visits (or length of stay) relative to a baseline value for that day of the week. This baseline is the median value for the corresponding day of the week in the five-week period between January 3rd and February 6th 2020.[5] The data aggregation process is similar the one used to create"popular times" for places in Google Maps. Observations that do not meet Google's required privacy thresholds are coded as missing by Google (in our study period this is the case for mobility in groceries and pharmacies on three Sundays in Berlin). Importantly, these data are based on Google Account users who opted-in to the Location History feature. This means that the data are not necessarily representative of the German population.

We focus on mobility in public spaces, captured by the percentage change in the number of visits to (or time spent in) groceries and pharmacies (GP), workplaces (W) and transit stations (T). The main outcome of interest is the percentage change in average community mobility in public spaces, equal to $\frac{GP+W+T}{3}$, relative to the baseline. We also use the percentage change in the number of hours spent at home relative to the baseline as an additional catch-all measure. For the sake of simplicity, we use the terms "mobility patterns" or "mobility", to refer to percentage change in the number of visits to (or time spent in) public spaces or number of hours spent at home.

We would like to highlight that the Google data can be used to measure community mobility patterns, but do not provide a good measure of social distancing, as implied in several recent studies [Wellenius et al., 2020, Schrimpf et al., 2020, Ansell, 2020]. The term "social distancing" refers to the physical (Euclidean) distance between two people [CDC, 2020b], which is not directly captured by the Google mobility data. Even though it is plausible that once mobility (i.e. number of visits to public spaces) reaches a certain level, social distancing will be harder to maintain in some locations, it is unclear how this can be accurately inferred from the data.

Google also provides mobility data on parks as well as retail and recreation. However, these

[5] This means there are 7 x 16 baseline values, one for each state and day of the week. Google does not provide data on the baseline total count/number (visits, hours spent), but only percentage changes relative to the (unknown) baseline. We address this issue by including state*day-of-the-week fixed effects in our model specification (see Section 3.3)

locations are less relevant for our analysis. This is because some places that fall within the park category are arguably not relevant for the spread of COVID-19 (for instance national parks, where the risk of transmission is likely extremely low). We also do not consider retail and recreation, as for most of the study period, the places that fall into this category (e.g. restaurants, cafes or cinemas) were required to close.

To create a timeline for when German states introduced compulsory face mask policies, we consulted state-specific secondary legislation (Verordnungen), which are typically published on states' official websites. We also extracted information from the German Catalogue of Fines[6] (Bußgeldkatalog), which records penalties for not wearing face masks in different states, as well as from official announcements made to national and local newspapers. Through the same process, we identified when states implemented other important policies related to the COVID-19 pandemic that could also affect community mobility patterns. We systematically extracted information on the re-opening of schools and shops, as well as the official start and end of state-specific stay-at-home orders (Ausgangsbeschraenkungen).

Finally, we obtain the daily number of new confirmed COVID-19 cases in each state from the Robert Koch Institute (RKI),[7] which is the German federal government agency responsible for disease control and prevention. We use RKI data corresponding to our study period (March 23rd to May 21st 2020).

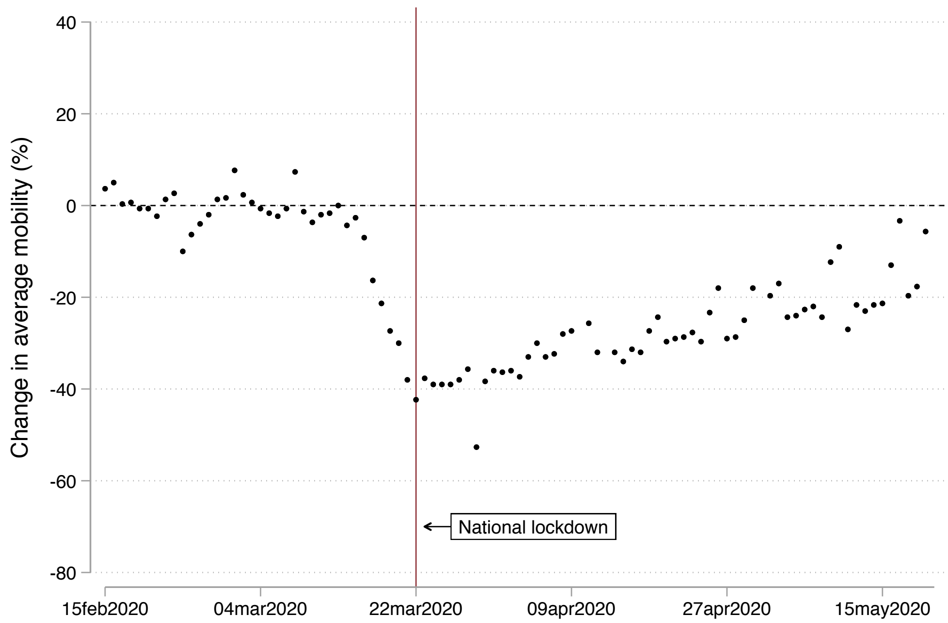
3.2 Mobility trends

Figure 1 provides a descriptive overview of changes in average mobility in public spaces (groceries and pharmacies, workplaces and transit stations). It shows that mobility in public spaces in Germany decreased substantially in the period leading up to the national-level lock-down on March 23rd 2020. As shown in Appendix B, similar patterns can be observed for mobility trends in each state and in specific public spaces (separately for groceries and pharmacies, workplaces and transit station). The number of hours spent in places of residence increased over the same time period, although changes appear less drastic, as individuals already spend a large proportion of their time at home.

[6] Available at: https://www.bussgeldkatalog.org/corona/

[7] Available at: https://npgeo-corona-npgeo-de.hub.arcgis.com/

Figure 1: Average mobility in public spaces in Germany



Note: This graph shows the percentage change in average mobility in public spaces (groceries and pharmacies, workplaces, and transit stations) for each day between Feb 15^{th} and May 21^{st} 2020 relative to the baseline. The baseline is the median value for the corresponding day of the week in the five-week period between Jan 3^{rd} and Feb 6^{th} 2020. *Data:* Google COVID-19 Community Mobility Reports.

3.3 Methods

To isolate the causal effect of compulsory face mask policies, we use a generalised difference-in-differences (DD) design that exploits the staggered introduction of compulsory face mask policies by German states. Intuitively, the DD approach isolates the effect of a policy by comparing changes in outcomes before and after an intervention for a treatment and control group. An attractive feature of the DD approach is that it can account for unobserved time-invariant confounders that differ between states (e.g. health system characteristics) as well as for unobserved time trends shared across states (e.g. national public holidays) [Kreif et al., 2016, Wing et al., 2018]. In this setup, all units are eventually "treated" (i.e. all states implement a compulsory face mask policy), but at different times.

We first use a static DD model:

$$Y_{st} = \alpha_s + \beta_t + \gamma D_{st} + X'_{st} + \eta_0 + \epsilon_{st} \tag{1}$$

where Y_{st} is a measure of community mobility, D_{st} is a treatment indicator equal to one for states and dates where compulsory face mask policies are in place and zero otherwise,[8] α_s denotes state-level fixed effects, β_t denotes date fixed effects, and X'_{st} is a vector of time-varying state-specific controls. The controls are state-specific public holidays (Tag des Sieges in Berlin), an indicator for when states relaxed their stay-at-home orders (Ausgangs-beschraenkungen), the daily new confirmed COVID-19 cases in each state (lagged by one day), an indicator for when states re-opened secondary schools for final year classes, an indicator for when states allowed retail shops $< 800\,\mathrm{m}^2$ to re-open, an indicator for when states allowed retail shops to re-open without any size restrictions, and state*day-of-the-week fixed effects. η_0 is a constant, and ϵ_{st} is an error term. The coefficient of interest is γ, which identifies the effect of compulsory mask policies on community mobility under the parallel trends assumption (i.e. that community mobility trends in treated and untreated states would have developed in parallel in the absence of compulsory face mask policies). We assess the plausibility of the parallel trends assumption by inspecting pre-treatment trends in a "fully dynamic" event study framework (see Equation 2 below).

Given that the static DD estimates can be biased when treatment effects vary over time [Goodman-Bacon, 2018], we use an event study approach that allows us to examine the effect of the policy for the days before and after implementation. In the main event study specification, the data are trimmed so that the panel is balanced in time periods (days) relative to the treatment, as recommended by Abraham and Sun [2018]. Schleswig-Holstein is the last state to receive treatment on April 29th and Google mobility data are available up until May 21st. Our "trimmed" panel therefore contains 22 days before and 22 days after the treatment date in each state.

To investigate pre-trends, we use a "fully dynamic" event study model, which is specified as follows:

$$Y_{st} = \alpha_s + \beta_t + \sum_{\ell=-21}^{-2} \gamma_\ell D_{st}^{\ell} + \sum_{\ell=0}^{22} \gamma_\ell D_{st}^{\ell} + X'_{st} + \epsilon_{st} \tag{2}$$

where $D_{st}^{\ell} = \mathbf{1}\{t - E_s = \ell\}$ is a "switch-on switch-off" indicator for unit s being periods ℓ away from the initial treatment period E_s at calendar time t. In the trimmed specification, distant relative periods (where $|\ \ell\ |> 22$) are excluded so that the panel is balanced in periods relative to the treatment. Furthermore, the first and last treatment lead are set to zero to

[8] For Berlin, we code D_{st}=1 following the introduction of compulsory face masks in public transport on April 27th. The policy was extended to shops two days later.

address under-identification in the fully dynamic model [Borusyak and Jaravel, 2017].

To asses how treatment effects change over time, we instead use a "semi-dynamic" event study model, where all leads are set to zero - following Borusyak and Jaravel [2017]. This specification is robust to event-time treatment effect heterogeneity. Furthermore, it estimates dynamic treatment effects more efficiently than the fully-dynamic model Borusyak and Jaravel [2017]. The semi-dynamic model is specified as follows:

$$Y_{st} = \alpha_s + \beta_t + \sum_{\ell=0}^{22} \gamma_\ell D_{st}^{\ell} + X_{st}' + \epsilon_{st} \tag{3}$$

All models are estimated using OLS with robust standard errors clustered at the state level. We also use a wild cluster bootstrap procedure to obtain more accurate p-values [Roodman et al., 2019]. This is advisable as in a setting with few clusters (16 states) the standard cluster-robust variance estimator may lead to over-rejection of the null and confidence intervals that are too narrow [Bertrand et al., 2004, Cameron et al., 2008]. We report bootsrapped p-values in the main results table and refer to Appendix F.3 for more details on the bootstrap procedure.

4 Results

4.1 Effect of compulsory face masks on mobility in public spaces

We first present results from our static DD specification (Equation 1) which investigates the average effect of introducing compulsory face mask policies on community mobility. As shown in Table 2, we do not find evidence to suggest that compulsory face mask policies affect average mobility in public spaces. Overall, the estimated effects are not statistically significant and relatively small in magnitude, lying between -0.8 percentage points (-0.05 SD) and -1.8 percentage points (-0.11 SD). Column 5 shows results for our preferred model specification, which includes state and date fixed effects and a broad range of state-specific controls: public holidays, the daily number of new COVID-19 cases in each state (lagged by one day), and several policy changes that are likely to affect community mobility (lock-down rules being relaxed, secondary schools and retail re-opening). The main treatment effect is quite precisely estimates and we can rule out even small increases in mobility that are larger than 0.03 SD.

Column 6 shows results for a more flexible model, which includes an interaction between day-of-the-week and state fixed effects. In contrast to all other models, the model suggests a significant negative effect on mobility of -1.8. Our sense is that this is because this static model heavily weighs changes occurring shortly after the treatment - an issue we examine further in the next section.

Table 2: Effect of compulsory face mask policies on mobility in public spaces

	(1)	(2)	(3)	(4)	(5)	(6)
Face mask policy	-0.759	-1.075	-1.074	-1.591	-1.500	-1.763
	(0.703)	(0.692)	(0.700)	(0.946)	(0.924)	(0.605)
	[0.333]	[0.211]	[0.214]	[0.199]	[0.210]	[0.027]
State FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓
State-specific holidays		✓	✓	✓	✓	✓
Lockdown relaxed		✓	✓	✓	✓	✓
COVID-19 cases (t-1)			✓	✓	✓	✓
Sec. school open				✓	✓	✓
Retail open					✓	✓
State * Day-of-week FE						✓
Observations	960	960	960	960	960	960
R-squared	0.965	0.973	0.973	0.973	0.973	0.985
Clusters	16	16	16	16	16	16

Robust clustered standard errors in parentheses.
Wild cluster (state-level) bootstrap p-values in square brackets.
Outcome mean -30.19 and SD 16.94.

Appendix C shows results for specific public locations. We find that the introduction of compulsory face masks leads to a small but statistically significant reduction in mobility for visits to grocery stores and pharmacies of -4.9 percentage points or -0.4 SD (95% CI between -0.28 and -0.10). We also find evidence for a small increase in the number of hours spent at home of 0.08 SD (95% CI between 0.03 SD and 0.13 SD). Our static models do not detect significant effects on mobility in workplaces and transit stations.

4.2 Dynamic effects

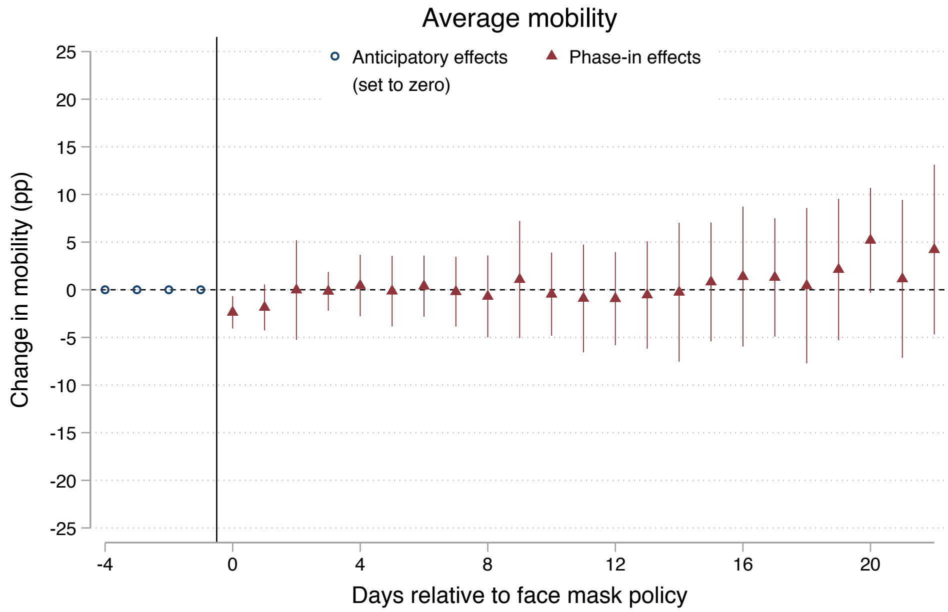
Next, we use event study models to assess parallel trends and examine how compulsory face masks affect mobility patterns over time. All models include controls from our preferred static DD model specification: state-specific public holidays, the daily number of new COVID-19 cases in each state (lagged by one day), and dummies for policy changes that likely affect community mobility (lock-down rules being relaxed, secondary schools and retail re-opening).

In Appendix D, we present results from the fully dynamic specification (Equation 2), which allows us to assess the parallel trends assumption. The absence of apparent pre-treatment trends suggests that our identification strategy is valid.

We use the semi-dynamic specification (Equation 3) to investigate potential over-time effects of compulsory face mask policies - following Borusyak and Jaravel [2017]. Figure 2 below summarises the results from the semi-dynamic model. We do not find evidence to suggest that compulsory face mask policies affect mobility in public spaces over time. There is a significant decrease in mobility on the day compulsory face mask policies are introduced. This decrease is equal to -2.4 percentage points or -0.14 SD (95% CI between -0.24 and -0.04), which is small in magnitude and comparable to the static DD estimate. We do not detect any significant effects on mobility for any other days following the policy change.

In Appendix E we examine over-time effects for mobility patterns in specific public spaces as well as time spent at home. We find that mobility in grocery shops and pharmacies decreases by between -7.7 percentage points (-0.31 SD) and -2.2 percentage points (-0.1 SD) within the first five days of the policy change, which is consistent with static DD estimates. This effect, however, fades out over time. We find only sporadic evidence for a positive over-time effect on mobility in places of work (for instance, a 2.8 percentage point (0.15 SD) increase on the 3$^{\text{rd}}$ day following the change, and a 3.6 percentage point (0.19 SD) increase on the 4$^{\text{th}}$ day). However, point estimated are imprecise and rarely distinguishable from zero. In terms of hours spent at home, we find a small increase within the first four days (between 0.14 and 0.17 SD), but no longer-term effects. We find no significant effects on mobility patterns in transit hubs. Overall, the results suggest that compulsory face mask policies only affect mobility in the very short term, with no detectable medium-term effects.

Figure 2: Semi-dynamic event study estimates



Note: This graph shows the estimated over-time effect of compulsory face mask policies on average mobility in public spaces (groceries and pharmacies, workplaces, and transit stations) for 22 days after the policy change. Point estimates are obtained from a semi-dynamic event study model, where all treatment leads are set to zero and the panel is "trimmed" such that it is balanced in time periods (days) relative to the policy change. The model includes controls from our preferred static DD model specification: state-specific public holidays, the daily number of new COVID-19 cases in each state (lagged by one day), and dummies for several policy changes that are likely to affect community mobility (lock-down rules being relaxed, secondary schools and retail re-opening). Vertical lines represent cluster-robust 95% confidence intervals.

4.3 Robustness checks

We conduct a number of robustness checks. First, we run the fully-dynamic specification using a "binning" approach [Abraham and Sun, 2018], where we replace the first and last switch-on-switch-off leads and lags with switch-on-stay-on indicators (see Equation 4). As shown in Appendix F.1, the main results hold using this alternative event study specification.

Second, we address the potential problem of negative weighting in the static DD setup by using a control group of states that are never exposed to the treatment, but plausibly face the same time effects as the treatment group [Borusyak and Jaravel, 2017]. As shown in Appendix F.2, we also do not find evidence that compulsory face masks affect community mobility using this alternative specification.

Third, we address the potential concern that our null-results are an artefact of too-few clusters [MacKinnon and Webb, 2018]. We show that the main results hold when using a “sub-cluster” wild bootstrap procedure (see Appendix F.3) and robust standard errors clustered at the state-week level (see Appendix F.4).

Finally, we use a synthetic control design. In Appendix F.5 we show that post-treatment mobility patterns do not differ significantly between the first state to implement compulsory face masks (Saxony) and its synthetic control. This is further evidence that compulsory face masks do not appear to affect community mobility.

5 Discussion

There is an ongoing debate about whether to introduce policies requiring the general public to wear protective face masks. A key concern is that individuals could feel safer as a result and, due to risk compensation, increase their mobility. This could undermine the most important public-health advice to contain the spread of COVID-19 – which is to reduce mobility and maintain social distancing [Greenhalgh et al., 2020]. We provide first evidence on the impact of compulsory face mask policies on community mobility. We do not find evidence to suggest that, in Germany, compulsory face mask policies affect mobility in public spaces (groceries and pharmacies, workplaces and transit hubs).

When examining mobility in specific locations, we find a short-term reduction in the number of visits to groceries and pharmacies and a short-term increase in the number of hours spent at home (respectively within five and four days of the policy change). We find no significant over-time effects on mobility in workplaces and transit hubs. Our overall interpretation of the results is that compulsory face mask policies in Germany did not affect community mobility. We do not examine how compulsory face mask policies affect important individual behaviours such as hand-washing and social distancing. However, the findings presented here should to some degree alleviate policy makers’ concerns about compulsory face mask policies leading to an increase in community mobility.

Even though compulsory face mask policies have been introduced in several countries, we currently lack systematic evidence on the effect of face masks on human behaviour. A recent small-scale field experiment implemented in Berlin before face masks became compulsory, finds that masks increase distancing by 9cm on average [Seres et al., 2020] - thereby finding no evidence of risk compensating behaviour. The authors hypothesise that this is due to others perceiving face masks as a signal of a larger preferred physical distance by the wearer. Even though this signalling effect most likely disappears in a setting where face masks are compulsory, we also do not find evidence for risk compensation at the community level.

There are two potential mechanisms which could explain our main finding that compulsory face mask policies have no discernible effect on community mobility. First, it might be that there is simply no risk compensating behaviour when it comes to face masks. One reason for this could be that individuals estimate that the risk of contracting COVID-19 is high, or that face masks do not offer effective protection, which stands in contrast to other settings where risk compensation has been studied - for example, vaccines (where perceived risks are relatively low and protection offered is high [Kapoor, 2008, Eaton and Kalichman, 2007]). Second, it might be that any risk compensation (which would increase mobility) is outweighed by increased salience or the hassle factor (which would decrease mobility). In terms of mobility in specific locations, we find a short-term negative effect on the number of visits to groceries and pharmacies - where face masks are required. Given that the effect occurs immediately and fades out very quickly, we believe that the hassle factor provides a better explanation than increased salience (where negative effects would arguably persist over time). This explanation has intuitive appeal. As face masks are uncomfortable to wear, individuals might initially make fewer visits to locations where face masks have to be worn, until they adapt to the new circumstances. One reason why we observe an effect for groceries and pharmacies but not for transit hubs might be that it is easier and less costly for individuals to change the frequency of visits to grocery shops, but that this is more difficult for transit. As we do not have access to individual-level data, we are unfortunately not in a position to test these hypotheses.

Our results are limited in four main respects. First, we are only able to observe the effect of compulsory face mask policies in the medium-term (up to three weeks after the policy change). It is possible that there are changes to community mobility in the long run that we are not able to detect. However, our results suggest that any changes in mobility fade out within days of the policy change and it is unclear if one would expect additional changes in behaviour after an initial adaptation period. Second, we only examine state-level trends in mobility and are unable to analyse heterogeneity between groups (for instance, those who are more or less likely to transmit COVID-19). Uncovering this heterogeneity would require micro-level mobility data, which are difficult to obtain because of privacy reasons. Third, one concern with the Google COVID-19 Community Mobility Reports is that the data are based on Google Account users who opted-in to Google's Location History feature. It is therefore likely that these data are from a non-random sub-sample of the German population. Whilst we have no data on the number of people using this feature, Germany has very high smartphone penetration. Over 98% of people under 50 years of age and 80% on average use a smartphone, with Android as the main operating system [Statista, 2019]. An additional concern is that the accuracy and coverage of the data vary across sub-national units (e.g. between urban and rural areas) in a systematic manner.

Whilst this paper provides important evidence for current policy debates on how to manage the COVID-19 pandemic, it is unclear if the results can be generalised to other settings. The Google mobility data used in this paper, or other sources of aggregate-level GPS data, could be used to determine the effect of compulsory face mask policies in other countries. Further research is also needed on the impact of compulsory face mask policies on other important behaviours such as hand washing and social distancing.

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6 Online Appendix

A Implementation dates

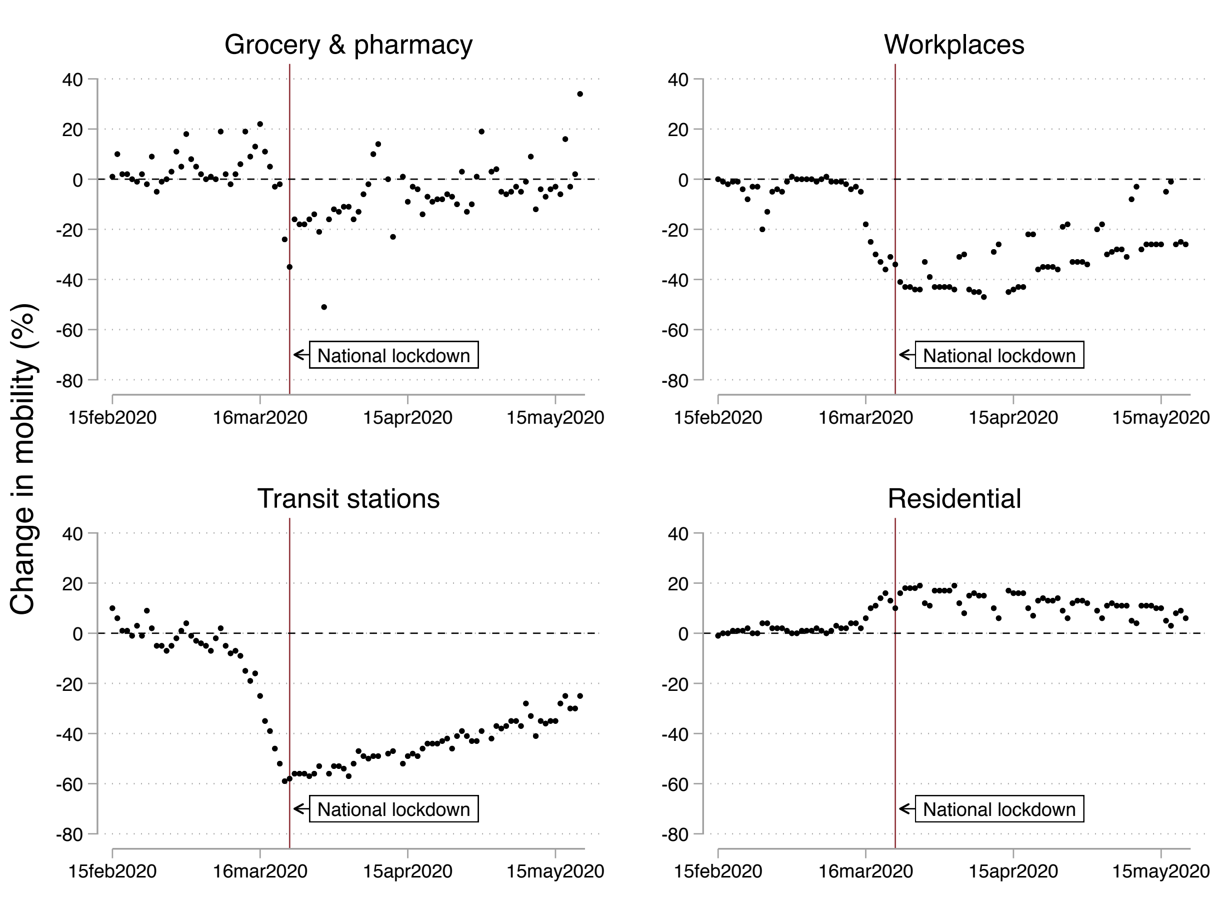
Table A1: Implementation dates for compulsory face mask policies and other COVID-19 measures

State	**Face mask policy**	**Lockdown relaxed**	**Sec. school open**	**Retail open** $< 800\,\mathrm{m}^2$	**All retail open**	**Restaurants open**
Baden-Wuerttemberg	27/04/2020	11/05/2020	04/05/2020	20/04/2020	11/05/2020	18/05/2020
Bayern	27/04/2020	06/05/2020	27/04/2020	27/04/2020	11/05/2020	18/05/2020
Berlin	27/04/2020	09/05/2020	27/04/2020	22/04/2020	09/05/2020	15/05/2020
Brandenburg	27/04/2020	09/05/2020	27/04/2020	22/04/2020	09/05/2020	15/05/2020
Bremen	27/04/2020	13/05/2020	27/04/2020	20/04/2020	13/05/2020	18/05/2020
Hamburg	27/04/2020	13/05/2020	27/04/2020	20/04/2020	13/05/2020	13/05/2020
Hessen	27/04/2020	09/05/2020	27/04/2020	20/04/2020	09/05/2020	15/05/2020
Nieder-Sachsen	27/04/2020	11/05/2020	27/04/2020	20/04/2020	11/05/2020	11/05/2020
Mecklenburg-Vorpommern	27/04/2020	11/05/2020	27/04/2020	20/04/2020	02/05/2020	09/05/2020
Nordrhein-Westphalen	27/04/2020	11/05/2020	23/04/2020	20/04/2020	11/05/2020	11/05/2020
Rheinland-Pfalz	27/04/2020	13/05/2020	27/04/2020	20/04/2020	04/05/2020	13/05/2020
Saarland	27/04/2020	28/04/2020	04/05/2020	20/04/2020	04/05/2020	18/05/2020
Sachsen	20/04/2020	20/04/2020	20/04/2020	20/04/2020	15/05/2020	15/05/2020
Sachsen-Anhalt	23/04/2020	04/05/2020	23/04/2020	20/04/2020	04/05/2020	18/05/2020
Schleswig-Holstein	29/04/2020	09/05/2020	27/04/2020	20/04/2020	09/05/2020	18/05/2020
Thueringen	24/04/2020	13/05/2020	27/04/2020	24/04/2020	04/05/2020	15/05/2020

Note: Berlin made face masks compulsory on public transport on April 27 and in shops two days later.All other states made face masks compulsory on public transport and shops at the same time. *Face mask policy* refers to the first date when a state implemented a compulsory face mask policy (regardless of whether it applied to public transport, shops, or both). *Lockdown relaxed* refers to the first date when a state introduced a first easing of the stay-at-home orders (“Ausgangsbeschränkungen”), which varied somewhat between different states. *Secondary school open* refers to the first date when a state re-opened secondary schools for pupils in their final year (“Abschlussklassen”). The two *Retail open* columns record the first date where a state allowed retail shops $< 800\,\mathrm{m}^2$ and without any size restrictions to re-open. *Restaurants open* refers to the first date when a state allowed restaurants to re-open. *Sources:* Verordnungen der Landesregierungen, Bußgeldkatalog, national and local newspapers.

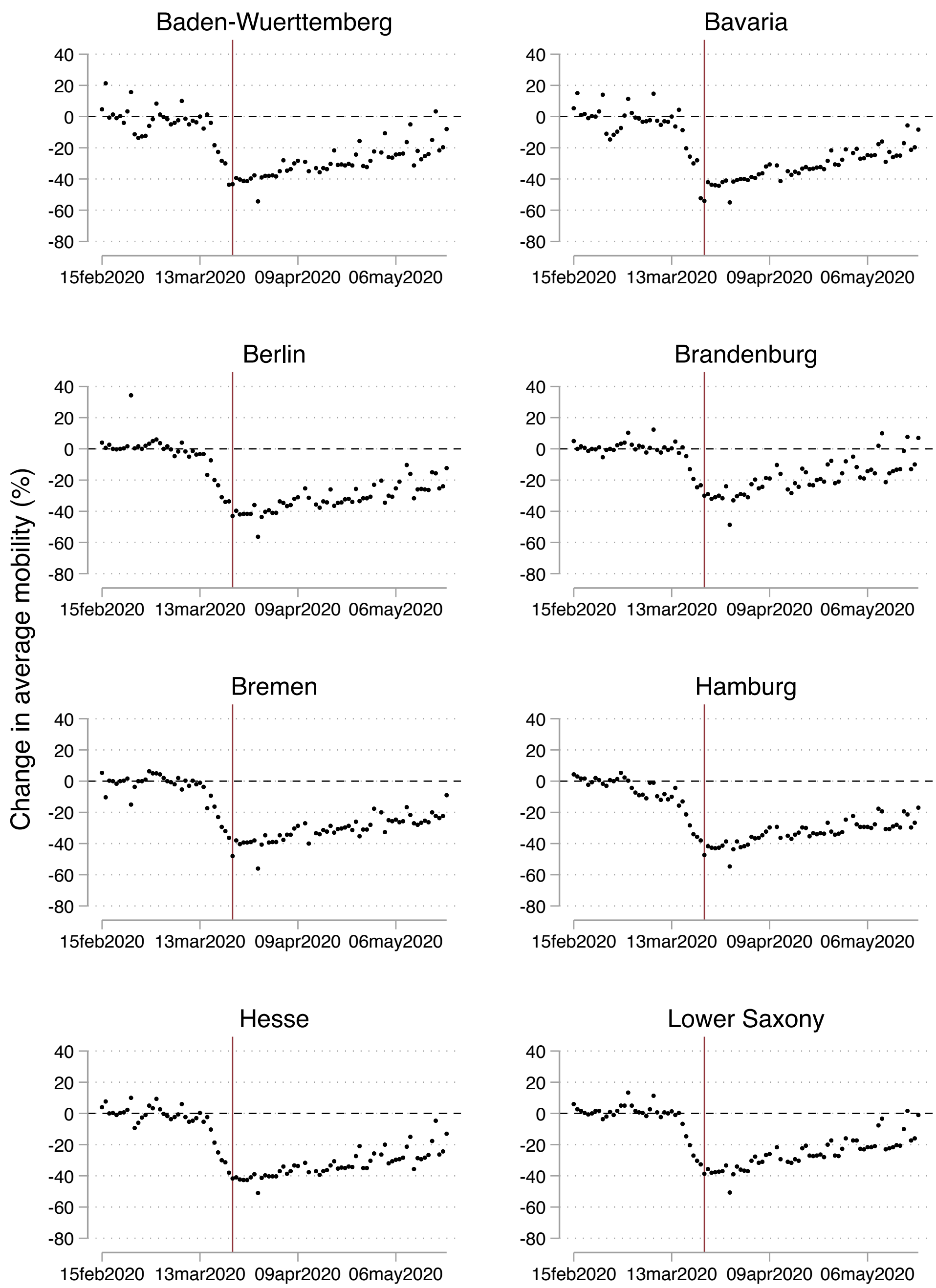
B Mobility trends

Figure A1: Mobility trends in Germany (other mobility measures)



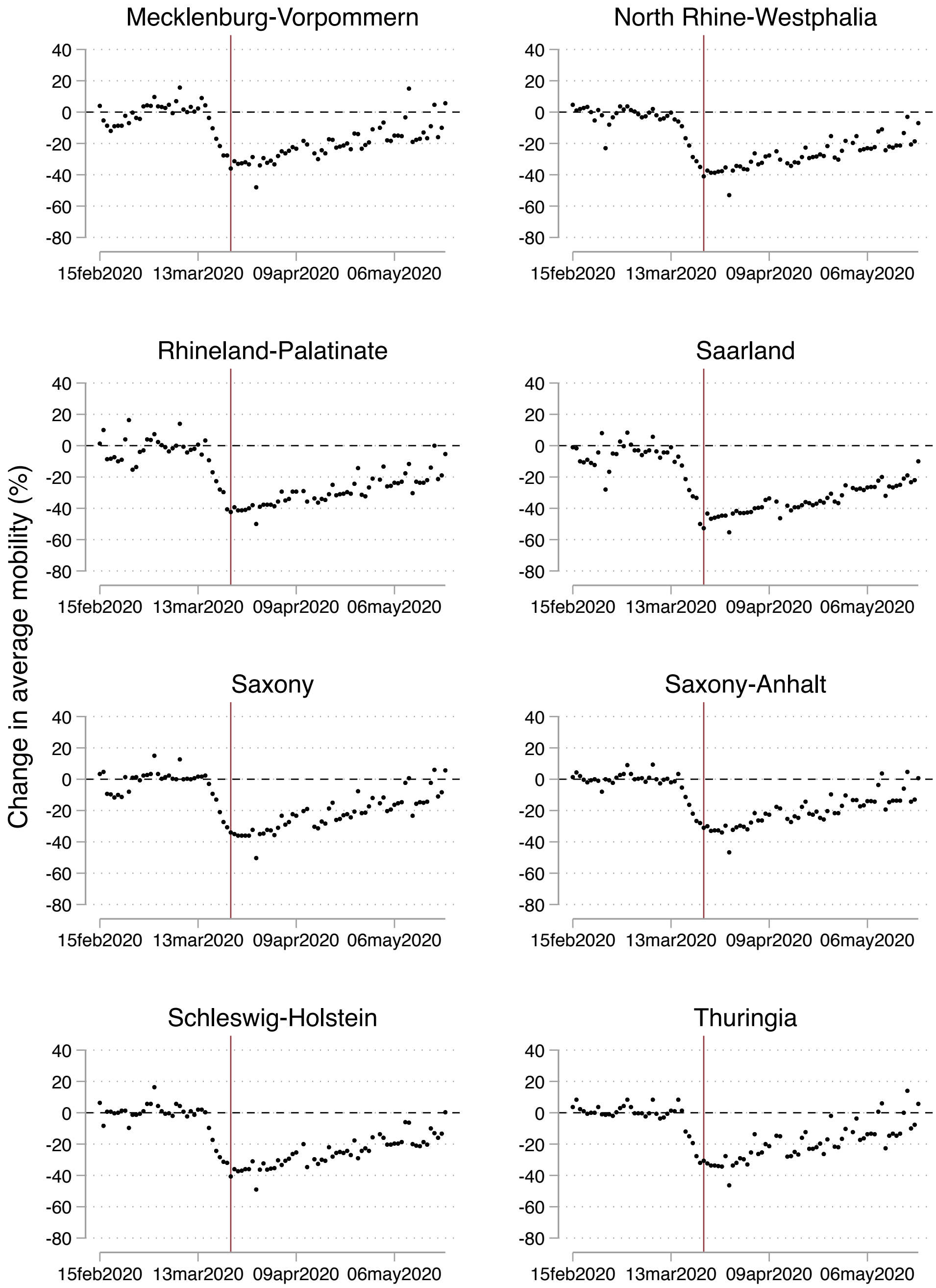
Note: This graph shows the percentage change in mobility (shown separately for goceries and pharmacies, workplaces, places of residence, and transit stations) for each day between February 15th and May 21st 2020 relative to the baseline mobility for that day of the week. The baseline is the median value for the corresponding day of the week in the five-week period between January 3rd and February 6th 2020. The vertical line marks the start of the national lock-down on March 23rd 2020. *Data:* Google COVID-19 Community Mobility Reports.

Figure A2: Average mobility in public spaces in German states



Note: Graphs shows the percentage change in average mobility in public spaces (groceries and pharmacies, workplaces, and transit stations) in each state for each day between February 15$^{\text{th}}$ and May 21$^{\text{st}}$ 2020 relative to the baseline mobility for that day of the week. The baseline is the median value for the corresponding day of the week in the five-week period between January 3$^{\text{rd}}$ and February 6$^{\text{th}}$ 2020. The vertical line marks the start of the national lock-down on March 23$^{\text{rd}}$ 2020. *Data:* Google COVID-19 Community Mobility Reports.

Figure A3: Average mobility in public spaces in German states (cont.)



Note: Graphs shows the percentage change in average mobility in public spaces (groceries and pharmacies, workplaces, and transit stations) in each state for each day between February 15th and May 21st 2020 relative to the baseline mobility for that day of the week. The baseline is the median value for the corresponding day of the week in the five-week period between January 3rd and February 6th 2020. The vertical line marks the start of the national lock-down on March 23rd 2020. *Data:* Google COVID-19 Community Mobility Reports.

C Static DD estimates for other mobility measures

This section presents estimates from the static DD model (Equation 1) for mobility in specific public locations.

Table A2: Effect of compulsory face mask policies on mobility in groceries and pharmacies

	(1)	(2)	(3)	(4)	(5)	(6)
Face mask policy	-4.723	-4.918	-4.902	-4.832	-4.859	-4.896
	(1.091)	(0.963)	(0.971)	(1.034)	(1.051)	(0.789)
State FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓
State-specific holidays		✓	✓	✓	✓	✓
Lockdown relaxed		✓	✓	✓	✓	✓
COVID-19 cases (t-1)			✓	✓	✓	✓
Sec. school open				✓	✓	✓
Retail open					✓	✓
State * Day-of-week FE						✓
Observations	957	957	957	957	957	957
R-squared	0.951	0.962	0.962	0.962	0.962	0.984
Clusters	16	16	16	16	16	16

Robust clustered standard errors in parentheses.

Table A3: Effect of compulsory face mask policies on mobility in workplaces

	(1)	(2)	(3)	(4)	(5)	(6)
Face mask policy	2.129	1.927	1.916	1.275	1.497	0.967
	(0.714)	(0.810)	(0.801)	(0.986)	(1.017)	(0.833)
State FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓
State-specific holidays		✓	✓	✓	✓	✓
Lockdown relaxed		✓	✓	✓	✓	✓
COVID-19 cases (t-1)			✓	✓	✓	✓
Sec. school open				✓	✓	✓
Retail open					✓	✓
State * Day-of-week FE						✓
Observations	960	960	960	960	960	960
R-squared	0.974	0.978	0.979	0.979	0.979	0.989
Clusters	16	16	16	16	16	16

Robust clustered standard errors in parentheses.

Table A4: Effect of compulsory face mask policies on mobility in transit stations

	(1)	(2)	(3)	(4)	(5)	(6)
Face mask policy	-0.094	-0.620	-0.630	-1.642	-1.614	-1.593
	(1.117)	(0.978)	(0.988)	(1.259)	(1.195)	(0.907)
State FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓
State-specific holidays		✓	✓	✓	✓	✓
Lockdown relaxed		✓	✓	✓	✓	✓
COVID-19 cases (t-1)			✓	✓	✓	✓
Sec. school open				✓	✓	✓
Retail open					✓	✓
State * Day-of-week FE						✓
Observations	960	960	960	960	960	960
R-squared	0.917	0.920	0.920	0.921	0.921	0.948
Clusters	16	16	16	16	16	16

Robust clustered standard errors in parentheses.

Table A5: Effect of compulsory face mask policies on time spent at home

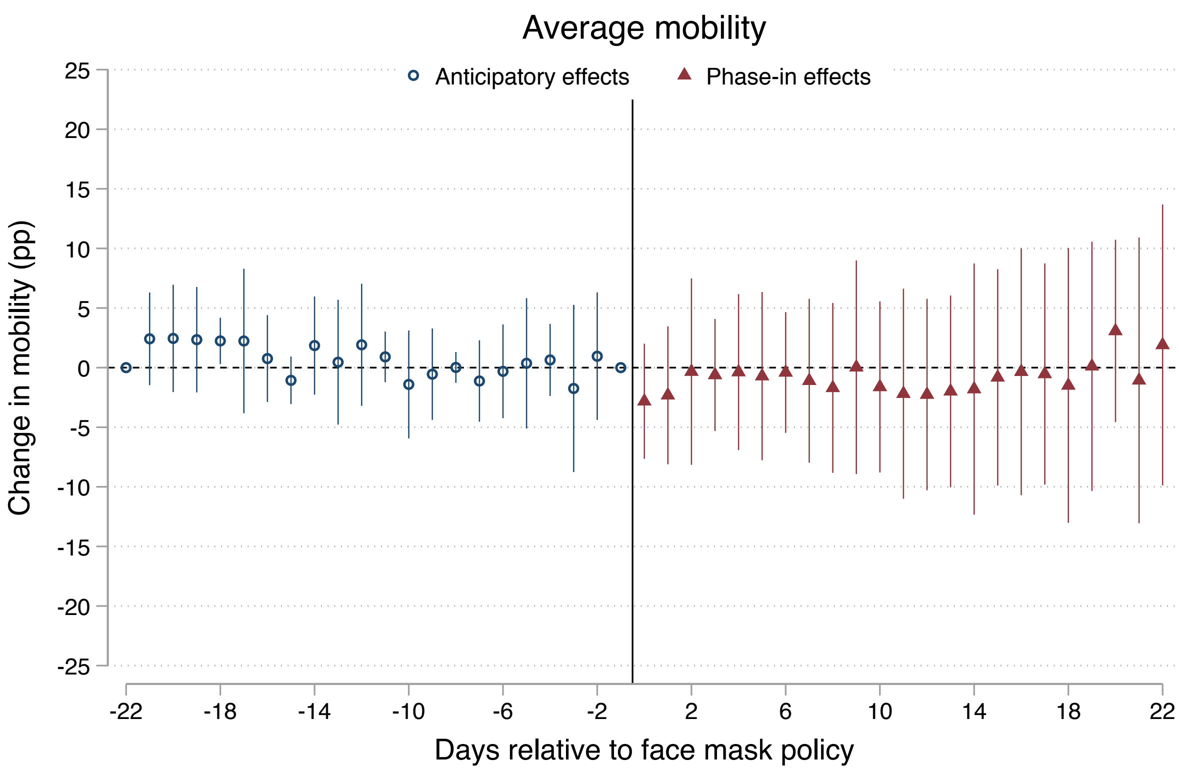
	(1)	(2)	(3)	(4)	(5)	(6)
Face mask policy	0.083	0.257	0.255	0.445	0.450	0.222
	(0.220)	(0.094)	(0.095)	(0.134)	(0.138)	(0.171)
State FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓
State-specific holidays		✓	✓	✓	✓	✓
Lockdown relaxed		✓	✓	✓	✓	✓
COVID-19 cases (t-1)			✓	✓	✓	✓
Sec. school open				✓	✓	✓
Retail open					✓	✓
State * Day-of-week FE						✓
Observations	960	960	960	960	960	960
R-squared	0.973	0.975	0.975	0.975	0.975	0.985
Clusters	16	16	16	16	16	16

Robust clustered standard errors in parentheses.

D Parallel trends

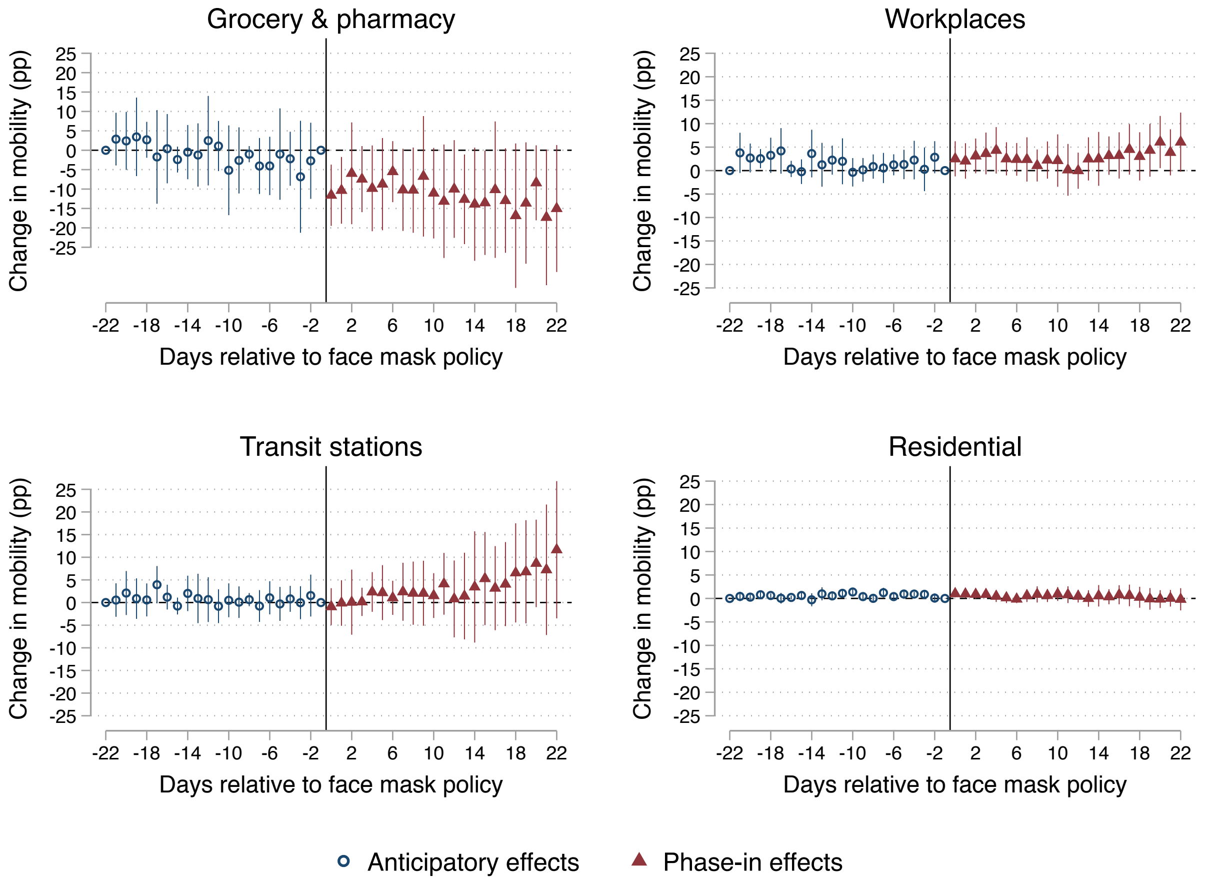
This section examines parallel trends using the fully dynamic event study specification (Equation 2).

Figure A4: Fully dynamic event study estimates for average mobility in public spaces



Note: This graph shows the estimated anticipatory and over-time effects of compulsory face mask policies on average mobility in public spaces (groceries and pharmacies, workplaces, and transit stations) for 22 days before and after the policy change. Point estimates are obtained from a fully dynamic event study model, where the first and last treatment leads are set to zero and the panel is "trimmed" such that it is balanced in time periods (days) relative to the policy change. Vertical lines represent cluster-robust 95% confidence intervals. The model includes controls from our preferred static DD model specification: state-specific public holidays, the daily number of new COVID-19 cases in each state (lagged by one day), and dummies for policy changes that likely affect community mobility (lock-down rules being relaxed, secondary schools and retail re-opening).

Figure A5: Fully dynamic event study estimates for other mobility measures

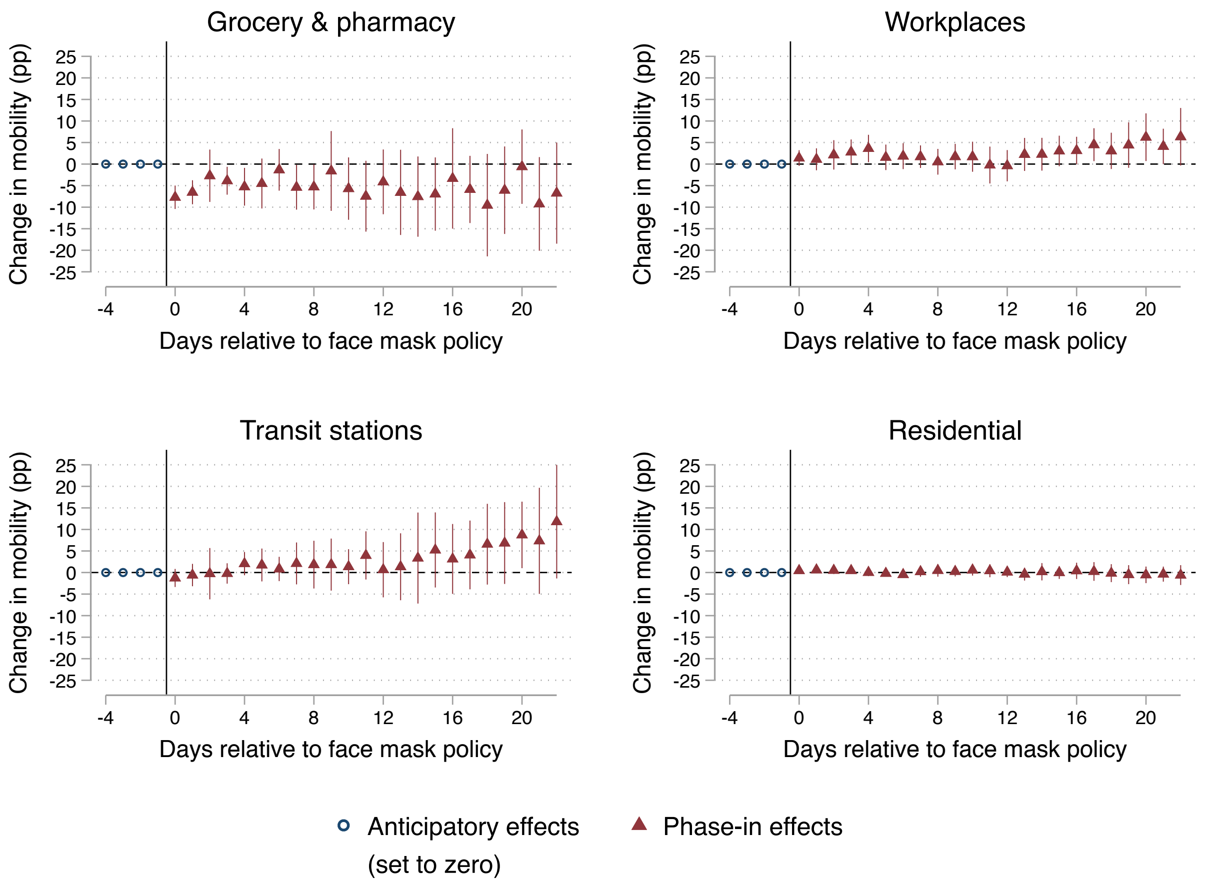


Note: This graph shows the estimated anticipatory and over-time effects of compulsory face mask policies on mobility (shown separately for groceries and pharmacies, workplaces, transit stations, and places of residence) for 22 days before and after the policy change. Point estimates are obtained from a fully dynamic event study model, where the first and last treatment leads are set to zero and the panel is "trimmed" such that it is balanced in time periods (days) relative to the policy change. Vertical lines represent cluster-robust 95% confidence intervals. Models include controls from our preferred static DD model specification: state-specific public holidays, the daily number of new COVID-19 cases in each state (lagged by one day), and dummies for policy changes that likely affect community mobility (lock-down rules being relaxed, secondary schools and retail re-opening).

E Semi-dynamic estimates for other mobility measures

Figure A6 shows semi-dynamic event study (see Equation 3) estimates for measures of mobility in specific public spaces as well as hours spent in places of residence.

Figure A6: Semi-dynamic event study estimates for other mobility measures



Note: This graph shows the estimated over-time effects of compulsory face mask policies on mobility (shown separately for groceries and pharmacies, workplaces, transit stations, and places of residence) for 22 days after the policy change. Point estimates are obtained from a semi-dynamic event study model, where all treatment leads are set to zero and the panel is "trimmed" such that it is balanced in time periods (days) relative to the policy change. Vertical lines represent cluster-robust 95% confidence intervals. Models include controls from our preferred static DD model specification: state-specific public holidays, the daily number of new COVID-19 cases in each state (lagged by one day), and dummies for policy changes that likely affect community mobility (lock-down rules being relaxed, secondary schools and retail re-opening).

F Robustness checks

F.1 Fully dynamic binned specification

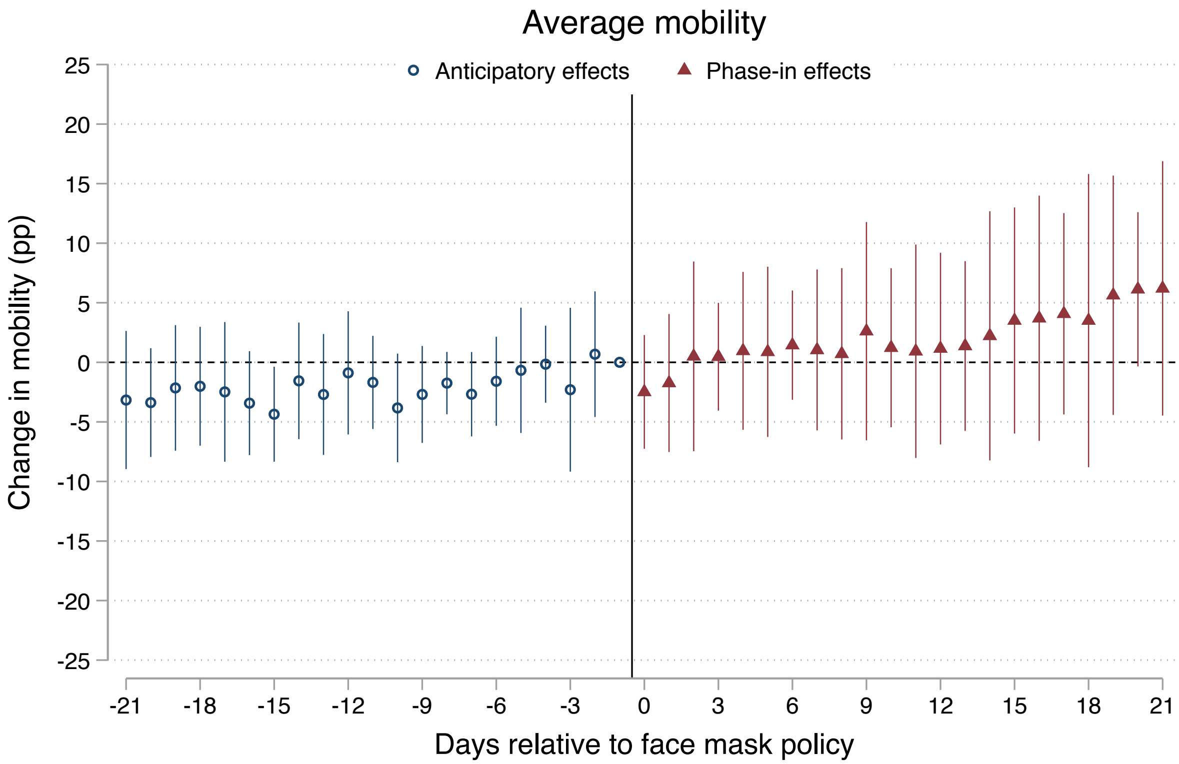
The fully dynamic binned model is specified as follows:

$$Y_{st} = \alpha_s + \beta_t + \mu_{\underline{g}} \sum_{\ell < -21} D_{st}^{\ell} + \sum_{\ell=-21}^{-2} \gamma_\ell D_{st}^{\ell} + \sum_{\ell=0}^{21} \gamma_\ell D_{st}^{\ell} + \mu_{\overline{g}} \sum_{\ell > 21} D_{st}^{\ell} + X_{st}' + \epsilon_{st} \quad (4)$$

where distant relative periods ($\mid \ell \mid > 21$) are binned into $\underline{g} = [-T, -21)$ and $\overline{g} = (21, T]$ and T denotes all available calendar time periods (i.e. dates) in the data. In the binned specification, the panel is balanced in calendar time periods rather than in periods relative to the treatment. Only one lead (where $\ell = -1$) is set to zero.

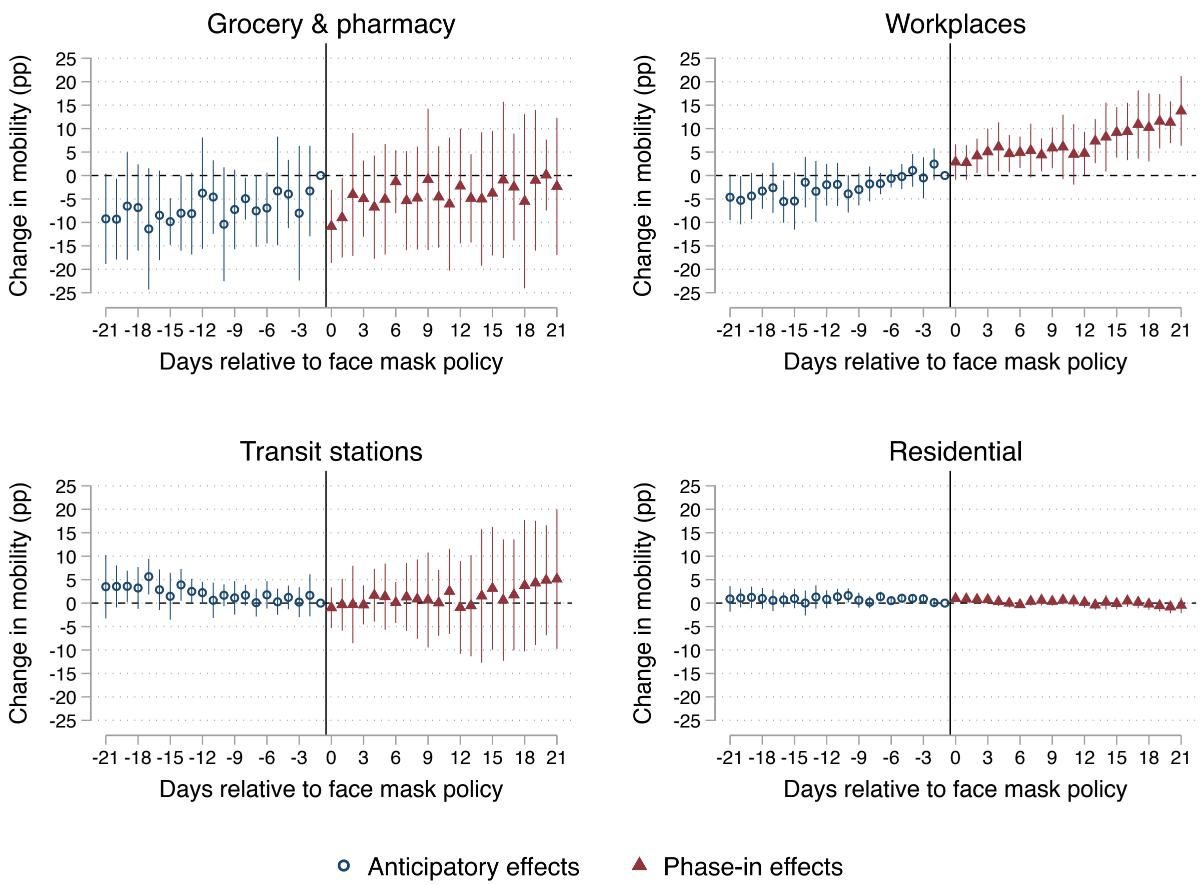
Figures A7 and A8 show, respectively, estimates from fully dynamic "binned" event study models for measures of average mobility and mobility in specific locations.

Figure A7: Fully-dynamic binned event study estimates for average mobility



Note: This graph shows the estimated anticipatory and over-time effects of compulsory face mask policies on average mobility (groceries and pharmacies, workplaces, transit stations) for 22 days before and after the policy change. Point estimates are obtained from a fully-dynamic event study model, where the first treatment lead is set to zero. The most distant leads and lags are "binned" and not displayed. The panel is balanced in calendar time periods. Vertical lines represent cluster-robust 95% confidence intervals. The model includes controls from our preferred static DD model specification: state-specific public holidays, the daily number of new COVID-19 cases in each state (lagged by one day), and dummies for policy changes that likely affect community mobility (lock-down rules being relaxed, secondary schools and retail re-opening).

Figure A8: Fully-dynamic binned event study estimates for other measures of mobility



Note: This graph shows the estimated anticipatory and over-time effects of compulsory face mask policies on mobility (shown separately for groceries and pharmacies, workplaces, transit stations, and places of residence) for 22 days before and after the policy change. Point estimates are obtained from a fully-dynamic event study model, where the first treatment lead is set to zero. The most distant leads and lags are "binned" and not displayed. The panel is balanced in calendar time periods. Vertical lines represent cluster-robust 95% confidence intervals. Models include controls from our preferred static DD model specification: state-specific public holidays, the daily number of new COVID-19 cases in each state (lagged by one day), and dummies for policy changes that likely affect community mobility (lock-down rules being relaxed, secondary schools and retail re-opening).

F.2 Never-treated control group

We re-run the main static DD specification (Equation 1) using a control group of states that are never exposed to the treatment. Given that all states in Germany eventually implemented compulsory face mask policies, we create an "artificial" control group. To this end, we drop all observations from April 27th onward. The three states that made face masks compulsory before April 27th now constitute the treatment group and the remaining thirteen states are part of the artificial never-treated control group. The results are presented in Table A6 below.

Table A6: Effect of compulsory face mask policies on average mobility in public spaces (with never-treated control group)

	(1)	(2)	(3)	(4)	(5)	(6)
Face mask policy	0.860	0.080	0.147	0.136	0.111	-1.090
	(1.013)	(1.463)	(1.495)	(1.714)	(1.697)	(1.455)
State FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓
State-specific holidays		✓	✓	✓	✓	✓
Lockdown relaxed		✓	✓	✓	✓	✓
COVID-19 cases (t-1)			✓	✓	✓	✓
Sec. school open				✓	✓	✓
Retail open					✓	✓
State * Day-of-week FE						✓
Observations	560	560	560	560	560	560
R-squared	0.971	0.971	0.971	0.971	0.971	0.986
Clusters	16	16	16	16	16	16

Robust clustered standard errors in parentheses.

As an additional robustness check, we also run a fully dynamic "binned" event study specification (Equation 5) using the never-treated control group sample, where all observations from April 27$^{\text{th}}$ onward are dropped. We do not employ the "trimming" approach because it is not possible to obtain a panel that is balanced in periods relative to the treatment (as 13 states do not have any post-treatment periods in this sample).

The 6$^{\text{th}}$ lag of the treatment indicator is the last possible lag in the data, given that we have dropped all observations from April 27$^{\text{th}}$ onward and Sachsen was the first state to implement a compulsory face mask policy six days before (on April 20$^{\text{th}}$). The 6$^{\text{th}}$ lag is effectively a "switch-on-stay-on" indicator as the panel ends on April 26$^{\text{th}}$. Equivalently, we treat the 6$^{\text{th}}$ lead of the treatment indicator as the first lead in the data and code it as a "switch-on-stay-on" indicator. As a result, we have a panel that is balanced in calendar periods.

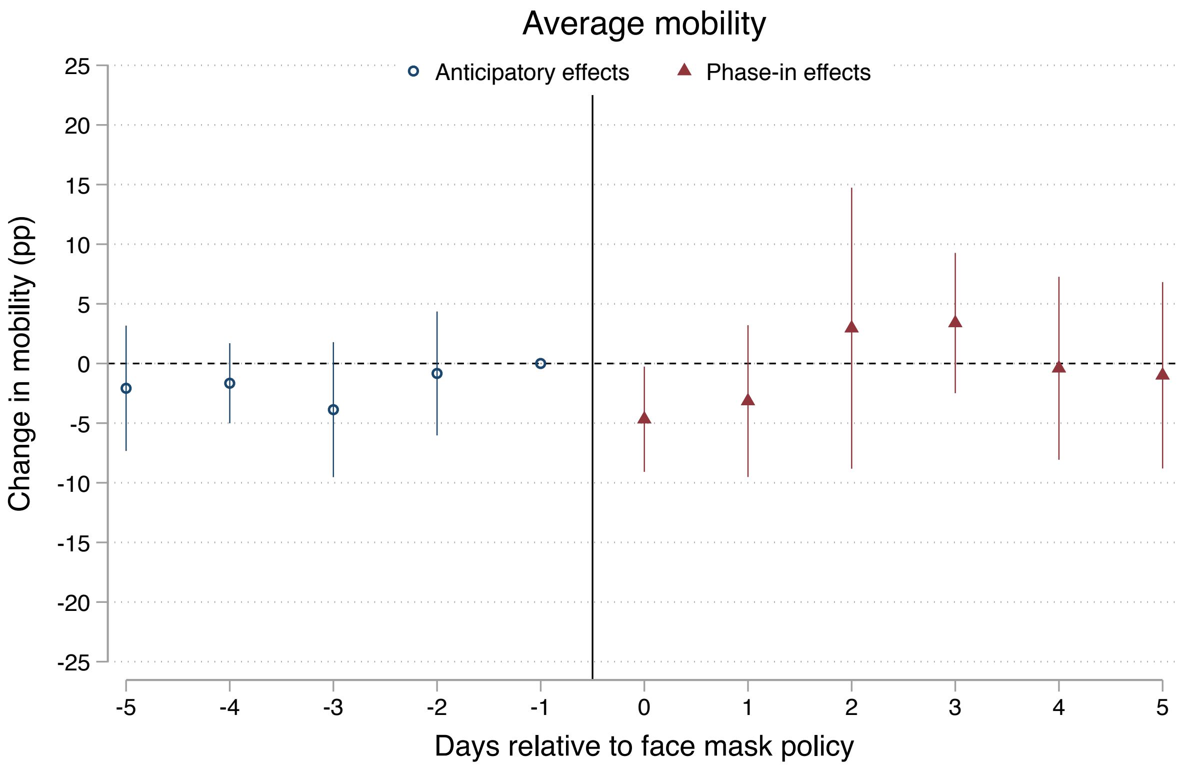
The fully dynamic binned model (with never-treated control group) is specified as follows:

$$Y_{st} = \alpha_s + \beta_t + \mu_{\underline{g}} \sum_{\ell < -5} D_{st}^{\ell} + \sum_{\ell=-5}^{-2} \gamma_\ell D_{st}^{\ell} + \sum_{\ell=0}^{5} \gamma_\ell D_{st}^{\ell} + \mu_{\overline{g}} \sum_{\ell > 5} D_{st}^{\ell} + X_{st}' + \epsilon_{st} \tag{5}$$

where distant relative periods ($\mid \ell \mid > 5$) are binned into $\underline{g} = [-T, -5)$ and $\overline{g} = (5, T]$ and T denotes all available calendar time periods (i.e. dates) in the data. In the binned specification, the panel is balanced in calendar time periods rather than in periods relative to the treatment. Only one lead (where $\ell = -1$) is set to zero.

Figure A9 presents the results from this model specification. As in the static DD analysis, the outcome is average mobility in public spaces in Germany.

Figure A9: Fully-dynamic binned event study estimates for average mobility (with never-treated control group)



Note: This graph shows the estimated anticipatory and over-time effects of compulsory face mask policies on average mobility (groceries and pharmacies, workplaces, transit stations) for 6 days before and after the policy change. Point estimates are obtained from a fully-dynamic event study model, where the first treatment lead is set to zero. The most distant leads and lags are "binned" and not displayed. The panel is balanced in calendar time periods. Vertical lines represent cluster-robust 95% confidence intervals. The model includes controls from our preferred static DD model specification: state-specific public holidays, the daily number of new COVID-19 cases in each state (lagged by one day), and dummies for policy changes that likely affect community mobility (lock-down rules being relaxed, secondary schools and retail re-opening).

F.3 Wild cluster bootstrap

We employ a wild cluster bootstrap procedure to obtain more accurate p-values. Intuitively, the procedure generates many bootstrap samples that mimic the distribution from which the original sample was obtained. It then computes a t-statistic for the coefficient of interest in each bootstrap sample. The refined p-value is the proportion of the bootstrap t-statistics that are more extreme than the t-statistic obtained from the original sample [Angrist and Pischke, 2009].

In a setting with very few treated clusters, the standard wild cluster bootstrap will typically under-reject the null of no treatment effect when the null is imposed (restricted). The restricted specification is the one from which we obtain the refined p-values reported in the main results table (Table 2). In contrast, the standard wild cluster bootstrap will over-reject when the null is not imposed (unrestricted) [MacKinnon and Webb, 2018, Roodman et al., 2019]. To account for this problem, we also employ the "sub-cluster" wild bootstrap procedure proposed by MacKinnon and Webb [2018], where the wild bootstrap data-generating process is clustered at a finer level (i.e. state-date level) than the covariance matrix (i.e. state level).

In Table A7 we report results from static DD models predicting our main outcome (average mobility), with refined p-values from a wild cluster bootstrap procedure, where the data-generating process is clustered at the state-date level (and the null is imposed).

Table A7: Effect of compulsory face mask policies on mobility in public spaces

	(1)	(2)	(3)	(4)	(5)	(6)
Face mask policy	-0.759	-1.075	-1.074	-1.591	-1.500	-1.763
	(0.703)	(0.692)	(0.700)	(0.946)	(0.924)	(0.605)
	[0.366]	[0.211]	[0.214]	[0.211]	[0.228]	[0.040]
State FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓
State-specific holidays		✓	✓	✓	✓	✓
Lockdown relaxed		✓	✓	✓	✓	✓
COVID-19 cases (t-1)			✓	✓	✓	✓
Sec. school open				✓	✓	✓
Retail open					✓	✓
State * Day-of-week FE						✓
Observations	960	960	960	960	960	960
R-squared	0.965	0.973	0.973	0.973	0.973	0.985
Clusters	16	16	16	16	16	16

Robust clustered standard errors in parentheses.
Wild cluster (state-date level) bootstrap p-values in square brackets.

F.4 State-week clustered standard errors

We re-run the main static DD specification (Equation 1) using robust standard errors clustered at the state-week level (rather than the state-level).

Table A8: Effect of compulsory face mask policies on mobility in public spaces (with robust standard errors clustered at the state-week level)

	(1)	(2)	(3)	(4)	(5)	(6)
Face mask policy	-0.759	-1.075	-1.070	-1.588	-1.494	-1.763
	(1.218)	(1.228)	(1.230)	(1.480)	(1.513)	(0.844)
State FE	✓	✓	✓	✓	✓	✓
Date FE	✓	✓	✓	✓	✓	✓
State-specific holidays		✓	✓	✓	✓	✓
Lockdown relaxed		✓	✓	✓	✓	✓
COVID-19 cases (t-1)			✓	✓	✓	✓
Sec. school open				✓	✓	✓
Retail open					✓	✓
State * Day-of-week FE						✓
Observations	960	960	960	960	960	960
R-squared	0.968	0.976	0.976	0.976	0.976	0.987
Clusters	144	144	144	144	144	144

Robust standard errors clustered at the state-week level in parentheses.

F.5 Synthetic control method

The synthetic control (SC) method is an alternative approach for evaluating aggregate-level policy interventions that relaxes the parallel trends assumption of the DD design. Specifically, the synthetic control design allows the effects of unobserved variables on the outcome to vary with time [Abadie et al., 2010]. Intuitively, the synthetic control design weighs outcomes from available control units (often referred to as the "donor pool") so as to construct the counterfactual outcome for the treated unit in the absence of the treatment. A synthetic control unit is defined as the time-invariant weighted average of available control units, which have similar pre-intervention characteristics and outcome trajectories as the treated unit [Kreif et al., 2016].

Weights are chosen so that the resulting synthetic control unit best reproduces the values of a set of predictors of the outcome (community mobility) in the treated unit before the implementation of the compulsory face mask policy. Specifically, weights are chosen so that the mean squared prediction error of the outcome variable in the pre-treatment period (also called pre-treatment root mean squared predictive error or "RMSPE") is minimised [Abadie et al., 2010]. We use the following pre-treatment predictors: population density per km^2, GDP per inhabitant, population aged 25-64 with upper and post secondary education, employment in the service sector as % of total employment, and long-term unemployment as % of the active population. All data are for the year 2014 (we assume that the predictors are relatively stable over time) and come from the latest Quality of Government EU Regional database.[9] In addition, the synthetic control model is augmented by adding average mobility outcomes for three dates prior to the policy change (March 23rd, April 5th, and the last day prior to the policy change) as predictors. Using these predictors results in a synthetic control that has common pre-treatment trends with the treated state.

To obtain a panel with never-treated control units, which is balanced in calendar periods (days) and units (states), we drop all observations from April 27th onward. Only three states made face masks compulsory before April 27th: Saxony (April 20th), Saxony-Anhalt (April 23rd), and Thuringia (April 24th). Here, we focus on comparing Saxony to its synthetic control, given that this state was the first to implement the face mask policy and therefore has the longest available post-treatment period (seven days). The two other early-movers (Saxony-Anhalt and Thuringia) are excluded so that all states in the "donor pool" are never-treated controls. The remaining 13 states represent our "donor pool" from which we construct the synthetic control unit for Saxony. Table A9 shows the means of all pre-treatment predictors used in the synthetic control analysis for the real Saxony, the synthetic Saxony, and all 13 states in the "donor pool". It shows that the synthetic Saxony more closely resembles the real Saxony on all pre-treatment characteristics compared to the rest of Germany.

The results from the synthetic control analysis for Saxony are shown in Figure A10. Overall, the results suggest that the compulsory face mask policy did not affect community mobility

[9] Available at: https://qog.pol.gu.se/data/datadownloads/qogeuregionaldata

patterns in Saxony. This is because the post-treatment mobility trends in Saxony closely track the mobility trends in its synthetic counterpart.

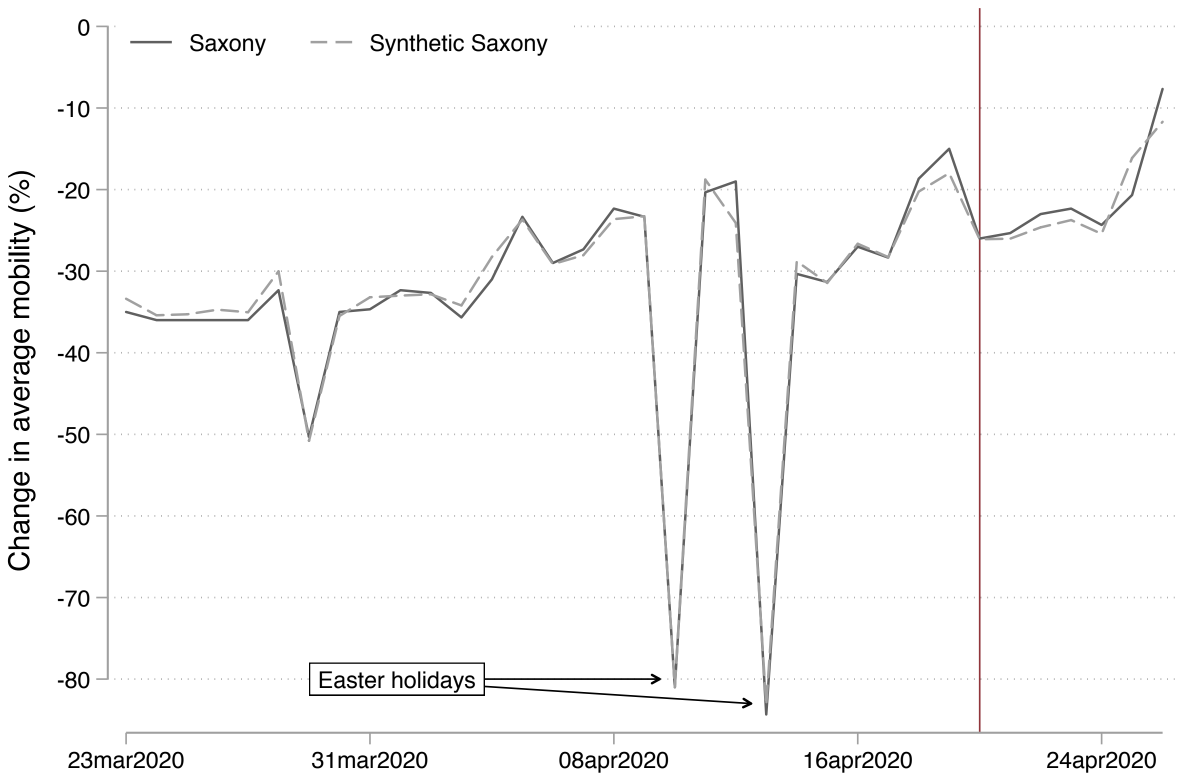
To assess the significance of our synthetic control estimates, we also conduct a series of placebo tests. Specifically, we apply the synthetic control procedure to every potential control state in the sample (i.e. the 13 states in the "donor pool"). This allows us to assess whether the effect estimated by the synthetic control for the treated state is large relative to the effect estimated for a state chosen at random [Abadie et al., 2010, Galiani and Quistorff, 2017]. Figure A11 shows results from the placebo tests. The results suggest that there are no statistically significant differences in post-treatment mobility patterns between the real and the synthetic Saxony.

Table A9: Means of pre-treatment characteristics

	Saxony (mean)	Synthetic Saxony (mean)	Rest of Germany (mean)
Pop. density per km^2	219.9	165.4	738.8
GDP per inhabitant	26900	31379	35979
Pop. aged 25-64 with sec. edu.	67.40	62.70	59.84
% employed in service sector	68.60	69.58	73.44
% long-term unemployed	3.800	2.797	2.571

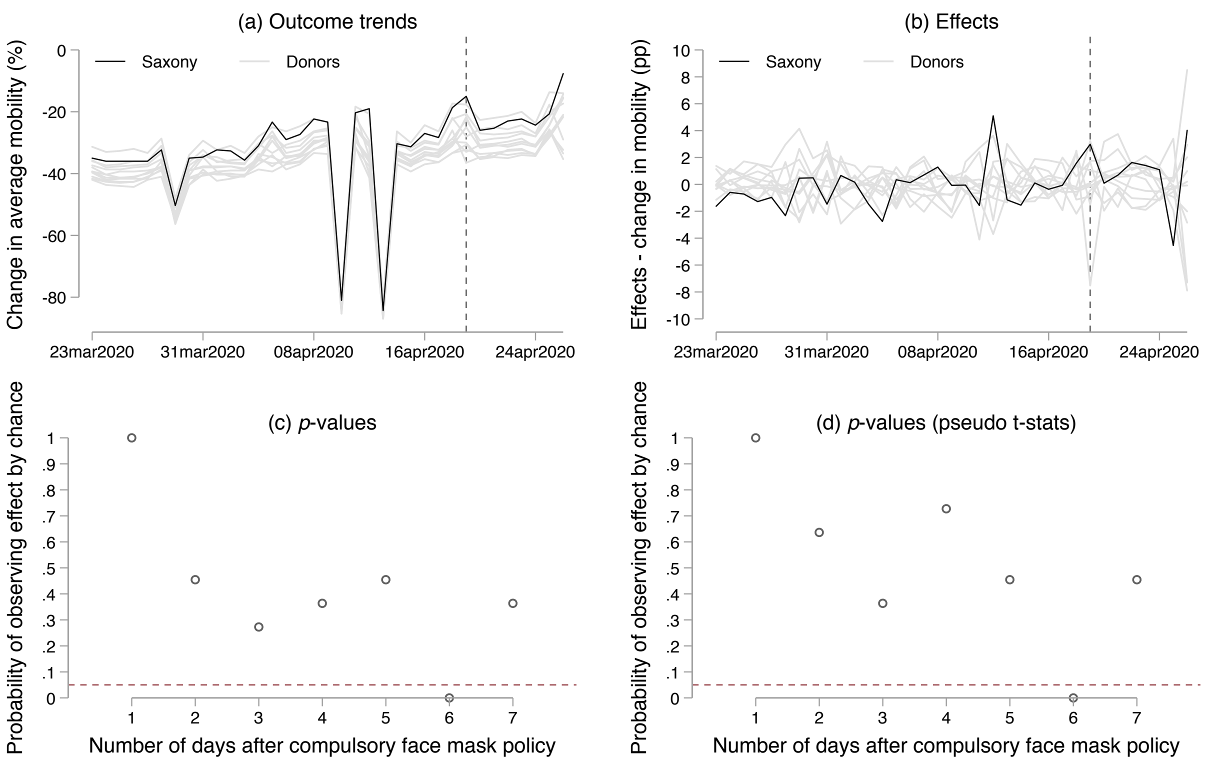
Note: All data are for the year 2014 and from the Quality of Government EU Regional database. The synthetic control model is augmented by adding average mobility outcomes for three dates prior to the policy change (means not reported here). The dates are March 23rd, April 5th, and April 19th. *Rest of Germany* refers to the 13 states in the "donor pool", which excludes Saxony-Anhalt and Thuringia.

Figure A10: Average mobility in Saxony vs. synthetic Saxony



Note: This graph shows the percentage change in average mobility in public spaces (groceries and pharmacies, workplaces, and transit stations) in Saxony (solid line) vs. synthetic Saxony (dashed line) for each day between Mar 23$^{\text{rd}}$ and Apr 27$^{\text{th}}$ 2020 relative to the baseline. The baseline is the median value for the corresponding day of the week in the five-week period between Jan 3$^{\text{rd}}$ and Feb 6$^{\text{th}}$ 2020. The vertical line marks Apr 20$^{\text{th}}$ - when the compulsory face mask policy was implemented in Saxony. Predictors for the synthetic control are population density per km^2, GDP per inhabitant, population aged 25-64 with upper and post secondary education, employment in services as % of total employment, long-term unemployment as % of the active population, change in average mobility on Mar 23$^{\text{rd}}$, Apr 5$^{\text{th}}$ and Apr 19$^{\text{th}}$. *Weights:* Baden-Wuerttemberg = 0.392, Brandenburg = 0.468, Mecklenburg-Vorpommern = 0.140, all other states = 0.

Figure A11: Placebo tests for Saxony



Note: (a) This graph shows the percentage change in average mobility in public spaces (groceries and pharmacies, workplaces, and transit stations) in Saxony (black line) and placebo states (grey lines) for each day between Mar 23rd and Apr 27th 2020 relative to the baseline. The baseline is the median value for the corresponding day of the week in the five-week period between Jan 3rd and Feb 6th 2020. (b) This graph shows the estimated effects (in pp) of the compulsory face mask policy on average mobility for Saxony (black line) and placebo states (grey lines). The dashed vertical lines mark the last pre-treatment period. (c) This graph shows the proportions of placebo effects that are at least as large as the main effect for each post-treatment period. (d) This graph shows the proportions of placebo pseudo t-statistics (unit's effect divided by its pre-treatment RMSPE) that are at least as large as the main pseudo t-statistic for each post-treatment period. The dashed horizontal lines mark the critical value of 0.05. Predictors for the synthetic control are population density per km^2, GDP per inhabitant, population aged 25-64 with upper and post secondary education, employment in services as % of total employment, long-term unemployment as % of the active population, change in average mobility on Mar 23rd, Apr 5th and Apr 19th.

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