The effect of compulsory face mask policies on community mobility in Germany

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Abstract

There is an ongoing debate about making face masks compulsory in public spaces to contain COVID-19. A key concern is that such policies could 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 during the first wave of the pandemic and measure mobility using geo-located smartphone data. We find that compulsory masking policies led to a short-term reduction in community mobility, with no significant medium-term effects. We can rule out even small increases in mobility.

Keywords: COVID-19, face masks, social distancing, community mobility
JEL Codes: D9, H12, I12, I18

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1 Introduction

The ongoing coronavirus disease (COVID-19) pandemic has, as of November 2022, led to the death of more than 6.5 million people (WHO 2022) and has had severe economic consequences, as global GDP contracted by 4.9% in 2020 (IMF 2020). Maximising social welfare is arguably one of the main policy objectives in economics and during a pandemic, a key constraint in the maximisation problem is that disease transmission needs to be contained (Budish 2020). One avenue through which governments have attempted to contain the spread of COVID-19 is 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 now familiar policies such as closing schools, banning public gatherings, social distancing rules or lockdowns forbidding individuals to leave their homes (Mellan et al. 2020, Lyu & Wehby 2020).

The requirement to wear face masks in public spaces has proven to be a controversial measure for containing COVID-19. In the earlier stages of the pandemic key international health bodies such as the the US Centres for Disease Control (CDC 2020) strongly advocated for face masks, whilst the World Health Organization (WHO) actively advised against their use (WHO 2020*a*). One reason for this was that, at the time, face masks had not been shown to prevent transmission of COVID-19 (Feng et al. 2020, Greenhalgh et al. 2020), which has now been demonstrated (Ollila et al. 2020, Mitze et al. 2020, Howard et al. 2021). Another key argument against making face masks compulsory, which motivates this paper, has however not yet been addressed: behavioural backlash. It is possible that individuals who wear masks will feel safer and might therefore disregard some of 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 was voiced widely by researchers and policymakers. For instance, 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). Moreover, 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 2020*b*). Importantly, these concerns have not subsided, although compulsory face mask policies have been introduced in numerous countries. The latest position of the WHO is that face masks could create "a false sense of security in the wearer" (WHO 2020*b*). Concerns about face masks creating a false sense of security are also the main reason why some countries such as Sweden have not recommended the use of face masks in public spaces (Reuters 2020). Whether compulsory face mask policies are welfare enhancing depends critically on both the direct effect of face masks on disease transmission, as well as 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 2020*a*,*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 & 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 actual or perceived personal risk, 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. As face masks are easily observable, they might 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 & 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 decisions about visiting public space (i.e. not only locations where face masks are required by law). Moreover, face masks differ from previously studied risk-reducing technologies in that they are bothersome to use (much more so than, for instance, seat belts). Wearing a face mask creates disutility, as 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 effect should be relatively short-lived (Dolan & 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.[1]

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 face masks in shops and public transport by German states (Bundesländer) during the first wave of the COVID-19 pandemic in the spring of 2020. Our results are specific to this particular setting, where masks were introduced following a national lockdown. In this setting, vaccines against COVID-19 were not yet available (as is still the case in many low- and middle-income countries), and reducing mobility was the main policy to contain the spread of COVID-19. The effect of masks on mobility was therefore paramount to policymakers. 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 in our main analysis, 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. These data are available for all individuals who use Google's location history feature - often a default setting for installing apps from Google. As Germany has very high smartphone penetration (80% on average) (Statista 2019), with Android as the main operating system, our sense is that these data therefore have suitable coverage of the German population. 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, but also report changes in mobility for specific locations.

We find that compulsory face mask policies led to a short-term reduction in community mobility in public spaces in Germany. Average community mobility decreased by 2.4 percentage points (-0.14 SD) on the day of the policy change and we find no evidence for a change in average mobility thereafter. Based on our results, we can rule out even small increases in

[1] 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. (2021*a*)

mobility that are larger than 0.03 SD. In terms of mobility in specific public spaces, we find a small increase in the number of hours spent at home – which is another "catch-all" measure of community mobility – as well as a decrease in mobility to grocery shops and pharmacies. We find no evidence suggesting a change in mobility patterns in transit stations and places of work.

As some German districts introduced compulsory face mask policies before state-level changes were implemented (for instance, masks became compulsory in the city of Jena on April 6th 2020 already), we also measure mobility at the district (i.e. NUTS-3) level. District-level data capture mobility in terms of the number of movements in a specific area (i.e. phones switching between radio cells). Results at the district level are analogous to the main results – as we only find a small decrease in mobility on the day of the policy change but no significant medium-term effects.

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 other 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), particularly during a lockdown, or when vaccines are not available. We find no evidence to suggest that compulsory face mask policies led to an increase in mobility in the first wave of the COVID-19 pandemic in Germany. This is important information for policymakers considering the costs and benefits of compulsory face mask policies, as such analyses likely do not have to account for adverse spillovers on mobility (i.e. mobility increasing as a result of the policy change).

Second, we contribute to the rapidly growing literature using aggregate GPS data to study the effect of policies trying to contain the spread of COVID-19 on mobility patterns (Breidenbach & Mitze 2021, Schlosser et al. 2020, Allcott et al. 2020, Wellenius et al. 2020, Dasgupta et al. 2020, Dave et al. 2020, Nguyen 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, 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 approximately 8 cm when individuals wear masks (Seres et al. 2021*a*,*b*). Our paper complements the small-scale field experiment (N=480) by (Seres et al. 2021*a*,*b*) by providing first evidence from a large sample. 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'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 242020ies on April 27th, and Schleswig-Holstein followed suit on April 29th. In all states, the face mask requirement was fulfilled by wearing any type of face covering (including scarves or bandannas) – hence, adherence to the policy was not affected by potential shortages of surgical masks. Children under six and people with disabilities were usually exempt from compulsory masking. 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 September 2022, FFP2 masks need to be worn in hospitals, nursing homes and GPs in all German states, and face mask requirements in public transport remain in place (Bundesregierung 2022).

Even though compulsory face mask policies made it illegal not to wear a mask in designated spaces, only nine out of 16 states introduced fines for not wearing masks in the period of interest.[2] Overall, the German approach to the first wave of the pandemic was characterised "more by appealing on compliance to rules rather than on enforcing them by micromanagement law" (Stafford 2020).

Table A1 in the Online 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 final year classes being allowed

[2]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.

to return to secondary schools, 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.

Before the implementation of mask requirements, a small proportion of the population used face masks, as 11% reported always wearing face masks in public spaces (public transport, supermarkets, shops or main roads) on April 2nd. As far as we are aware, there are no nationally representative data on actual face mask use. Evidence from a field experiment in Berlin, conducted before masks became compulsory, found that only 17% of people were wearing face masks in stores, supermarkets or post-offices (Seres et al. 2021*a*). Reported face mask use increased substantially nation-wide with the introduction of mask requirements, as 26% report always wearing masks in public spaces on April 24th (as the first compulsory face mask policies were implemented) and 56% do so on April 30th (when face masks were compulsory across the country) (YouGov 2020). It is important to note however that these figures do not provide an estimate of compliance with compulsory face mask policies. This is because survey data ask about use of masks in all public spaces, including for example shopping streets, where face masks never became compulsory. In the first wave of the pandemic, compulsory face mask policies appear to have been 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).

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 on April 6th 2020 (Der Spiegel 2020). The federal government largely took a back seat and continued to recommend voluntary face mask use until April 22nd 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 have been the case in Germany. The first city to implement compulsory face mask policies (Jena) was governed by a mayor from the liberal *FDP*. The first state to do so was governed by the centre-right *CDU* and another early mover (Thuringia) was governed by the left-wing *Die Linke*.

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

State	Implementation date
Saxony	20/04/2020
Saxony-Anhalt	23/04/2020
Thuringia	24/04/2020
Baden-Wuerttemberg	27/04/2020
Bavaria	27/04/2020
Berlin	27/04/2020
Brandenburg	27/04/2020
Bremen	27/04/2020
Hamburg	27/04/2020
Hesse	27/04/2020
Lower Saxony	27/04/2020
Mecklenburg-Western Pomerania	27/04/2020
North Rhine-Westphalia	27/04/2020
Rhineland-Palatinate	27/04/2020
Saarland	27/04/2020
Schleswig-Holstein	29/04/2020

Note: The table shows the date compulsory face mask policies were implemented in each German state. These are based on state-specific secondary legislation (Verordnungen), which are typically published on states' official websites.

3 Data and methods

3.1 Data

To measure community mobility, we use the publicly available Google COVID-19 Community Mobility Reports for Germany.[3] These data capture daily changes in mobility patterns in each German state based on GPS data from Google Account users who have enabled the

[3]Available at: https://www.google.com/covid19/mobility/. Accessed May 5th 2020.

Location History feature (which is generally a default setting for installing apps from Google). We use mobility data from the period between March 23rd and May 21st 2020. We exclude observations from before the national lock-down (which was announced on March 22nd 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 capture mobility patterns for places of work and residence. For workplaces, Google uses the number of visits to places of work that last longer than one hour (Aktay et al. 2020). For places of residence, Google captures 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.[4] The data aggregation process is similar to 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 enabled the Location History feature - which is a non-random sub-sample of the German population. However, Germany has very high overall smartphone penetration (98% of people under 50 years of age and 80% on average), with Android as the main operating system (Statista 2019). In addition, even though users have to allow Google to access their location history to appear in the data, this is often the default setting for installing apps from Google. Our sense is therefore that many users will opt-in to this feature due to a default bias (Haan & Linde 2018).

We focus on mobility in public spaces, captured by the percentage change in the number of visits to groceries and pharmacies (GP), workplaces (W) and transit stations (T). Our main outcome of interest is an aggregate measure, which captures the average percentage

[4]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.

change across the three categories, 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 public spaces or number of hours spent at home.[5]

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 (Bußgeldkatalog),[6] 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 partial 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 data on the 7-day COVID-19 incidence rate 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) during the spring of 2020. It shows that mobility in public spaces in Germany decreased substantially in the period leading up to the first national-level lock-down on March 23rd 2020. As shown in Online Online Online Appendix B, similar patterns can be observed for mobility trends in each state and in specific public spaces (i.e. 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

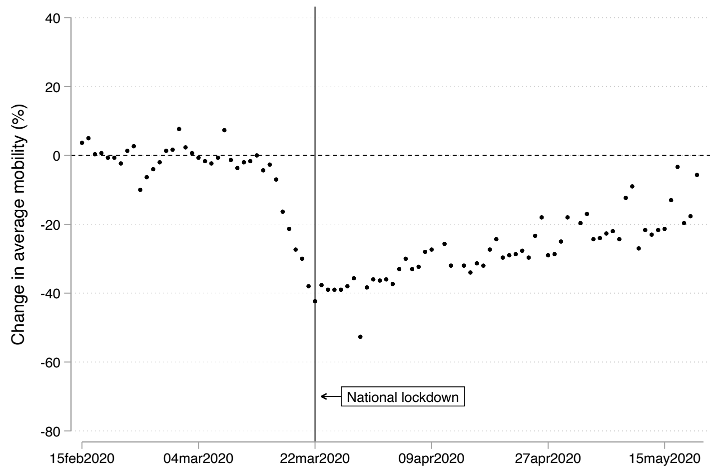
[5] Google also provides mobility data on parks as well as retail and recreation. However, these locations are less relevant for our analysis. This is because some places that fall within the park category are arguably not relevant for the spread of COVID-19 (for instance national parks, where the risk 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.

[6] Available at: https://www.bussgeldkatalog.org/corona/. Accessed May 5th 2020.

[7] Available at: https://npgeo-corona-npgeo-de.hub.arcgis.com/. Accessed May 7th 2020

home.

Figure 1: Average mobility in public spaces in Germany



Note: This graph shows the daily percentage change in average mobility in public spaces (groceries and pharmacies, workplaces, and transit stations) between Feb 15th and May 21st 2020, relative to the baseline. The baseline is the median value for the corresponding day of the week between Jan 3rd and Feb 6th 2020.

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 our case, all units are eventually "treated" (i.e. all states implement a compulsory face mask policy), but at different times.

As German states introduced compulsory face mask policies in close succession (between April 20th and April 29th), we are only able to identify the causal effect on mobility if behaviour change occurs immediately. Our sense is that it is plausible to expect an immediate effect of compulsory face mask policies on behaviour, as risk compensation, increased salience, as well as the "hassle factor" are likely to occur as soon as policies are implemented.

We first use a static DD model:

$$Y_{st} = \alpha_s + \beta_t + \gamma D_{st} + X'_{st} + \epsilon_{st} \quad (1)$$

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 binary indicators state-specific public holidays (Tag des Sieges in Berlin), a binary indicator for when states relaxed their stay-at-home orders (Ausgangsbeschraenkungen), the 7-day COVID-19 incidence rate in each state (the number of new COVID-19 cases in a 7-day period per 100,000 population), a binary indicator for when states re-opened some parts of secondary schools (in most areas only for final year classes) a binary indicator for when states allowed retail shops $< 800\,\mathrm{m}^2$ to re-open, and a binary indicator for when states allowed retail shops to re-open without any size restrictions. 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).

Given that the static DD estimates can be biased when treatment effects vary over time (de Chaisemartin & D'Haultfœuille 2020, Goodman-Bacon 2021), 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 et al. (2021). 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:

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

[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.

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 address under-identification in the fully dynamic model (Borusyak & Jaravel 2021).

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 & Jaravel (2021). This specification is robust to event-time treatment effect heterogeneity. Furthermore, it estimates dynamic treatment effects more efficiently than the fully-dynamic model Borusyak & Jaravel (2021). 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 Online Appendix D.4 for more details on the bootstrap procedure.

In the main analysis we leverage variation that occurs over a relatively short timer period: the period between the the first state adopting compulsory face mask policies (Saxony on April 20^{th}) and the last state doing so (Schleswig-Holstein on April 29^{th}) is 9 days. The variation used in the event study model to estimate over-time effects of the policy change from the 9^{th} day onward comes from switch-on-switch-off indicators (lags) turning on at different calendar times (dates) for different states.[9] We can therefore still interpret these estimates as dynamic treatment effects under the assumption that all states follow the same path of treatment effects, irrespective of when they first got the treatment. In other words, that treatment effects are homogeneous across units (states) and calendar time period (dates) and only vary across relative time period (days since treatment). Nonetheless, we also test

[9]For example, the 9+ lag is equal to 1 on May 6 for all states that adopted compulsory face mask policies 9 days earlier on April 27, but zero for all others. For Saxony for instance, the 9+ lag is equal to one on April 29 and for Saxony-Anhalt on May 2.

whether results are robust to a shorter time window being used and also re-run the analysis at the district level, where there is more temporal variation in the implementation of mask mandates. We also test for negative weights and use an alternative estimation strategy that addresses coefficient estimates being contaminated by other periods.[10]

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. Table 2 shows results from our preferred model specification, which includes state and date fixed effects and a broad range of state-specific controls: public holidays, the 7-day COVID-19 incidence rate in each state, and binary indicators for other policy changes that are likely to affect community mobility (lock-down rules being relaxed, final- year classes of secondary schools and retail re-opening). Results for other specifications are shown in Online Appendix C.

Model 1 examines average mobility in public spaces and does not suggest significant effects. Coefficients are small in magnitude and lie between a 3.4 percentage points (0.2 SD) decrease in mobility and an 0.4 percentage points (0.03 SD) increase in mobility.[11] As shown in Model 2, estimates from our static model suggest that the introduction of compulsory face masks led to a 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 SD and -0.10 SD). 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) and can rule out any reduction in time spent at home (Model 5). Our static models do not detect significant effects on mobility in workplaces and transit stations, coefficients are small in magnitude and we can rule out increases in mobility that are larger than 0.2 and 0.06 SD respectively (Models 3 and 4).

[10] We do not consider spatial spillovers in our analysis, as is done by for example Kosfeld et al. (2021).

[11] The 95% confidence interval of the treatment effect is -3.405 - 0.433. The SD of the outcome is 16.94. Hence, we can rule out increases in mobility that are larger than 0.025 SD (0.433/16.94).

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

	(1) Average	(2) Grocery	(3) Work	(4) Transit	(5) Residential
Face mask policy	-1.486	-4.889	1.518	-1.556	0.457
	(0.900)	(1.067)	(0.987)	(1.115)	(0.149)
	[0.205]	[0.010]	[0.301]	[0.259]	[0.029]
Outcome mean	-30.189	-11.371	-34.366	-44.757	12.397
Outcome SD	16.937	24.978	18.558	13.915	5.919
Observations	960	957	960	960	960
R-squared	0.973	0.962	0.979	0.922	0.975
Clusters	16	16	16	16	16

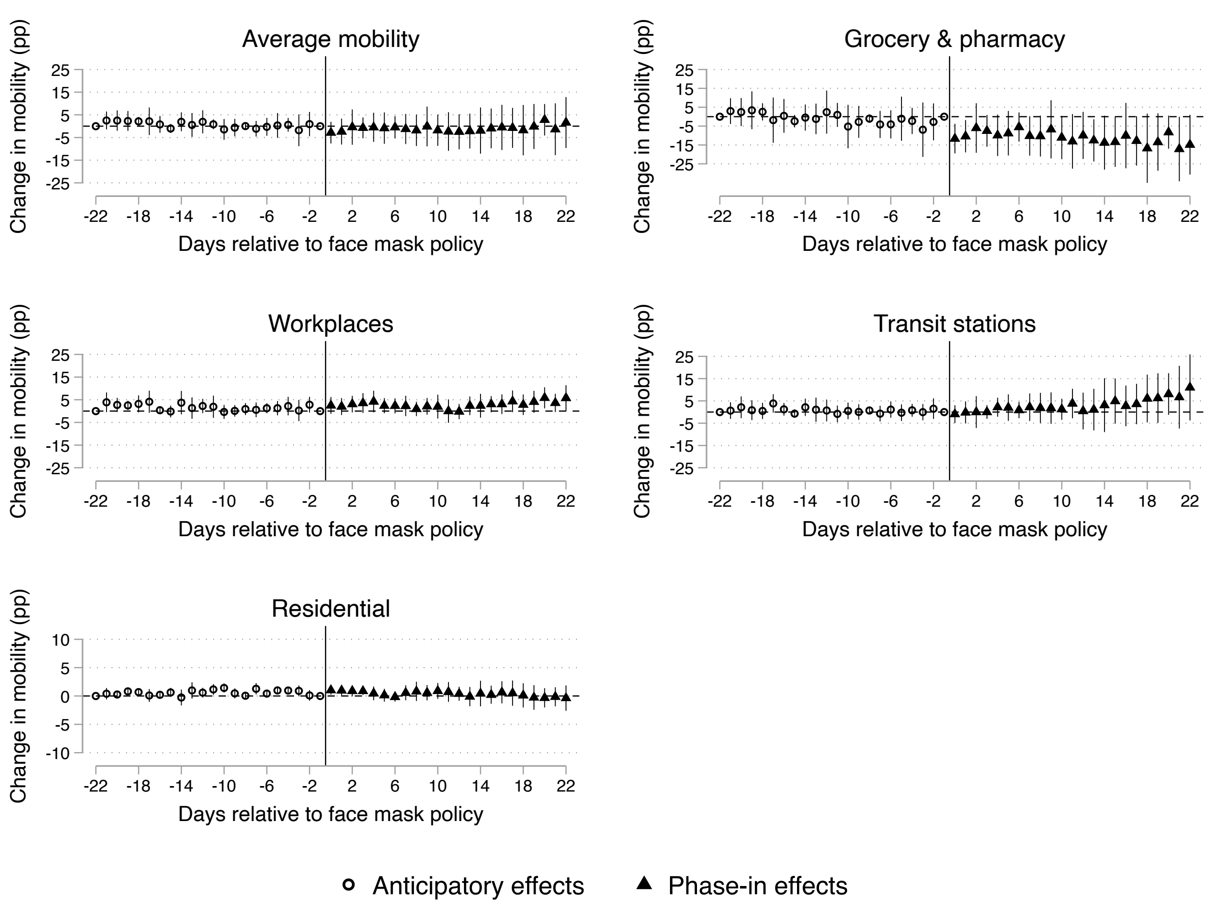
Note: The outcomes are change in state-level average community mobility, mobility in groceries and pharmacies, places of work, transit stations as well as places of residence between March 23rd and May 21st 2020. Models are based on our preferred specification, which includes state and date fixed effects, and controls for state-specific public holidays, an indicator for when state-level lock-downs were relaxed, the 7-day COVID-19 incidence rate, an indicator for when final-year secondary school classes were allowed to return, as well as an indicator for when states allowed retail shops to re-open. Robust clustered standard errors in parentheses. Wild cluster (state-level) bootstrap p-values in square brackets.

4.2 Dynamic effects

Next, we use event study models to investigate pre-treatment trends and examine how compulsory face masks affect mobility patterns over time. All models include controls from our preferred static DD model specification.

We use our fully dynamic specification (Equation 2) to investigate whether there are major deviations from the hypothetical linear trend before policy implementation. Figure 2 shows no apparent pre-treatment trends for our measure of community mobility in public spaces (see top left). Figure 2 also presents results for specific public locations. Whilst there appears to be an overall downward trend in mobility in grocery shops and pharmacies, pre-treatment estimates are not significantly different from zero. For places of work, one of the 22 pre-treatment estimates is significantly different from zero but visually there are no apparent pre-trends. We also find no pre-treatment trends for mobility in transit hubs. For hours spent at home, five of the 22 pre-treatment estimates are significantly different from zero, although, again, there is no apparent pre-treatment trend in outcomes. Overall, we take the absence of significant pre-treatment trends to suggest that there are no major deviations from the hypothetical linear trend before policy implementation.

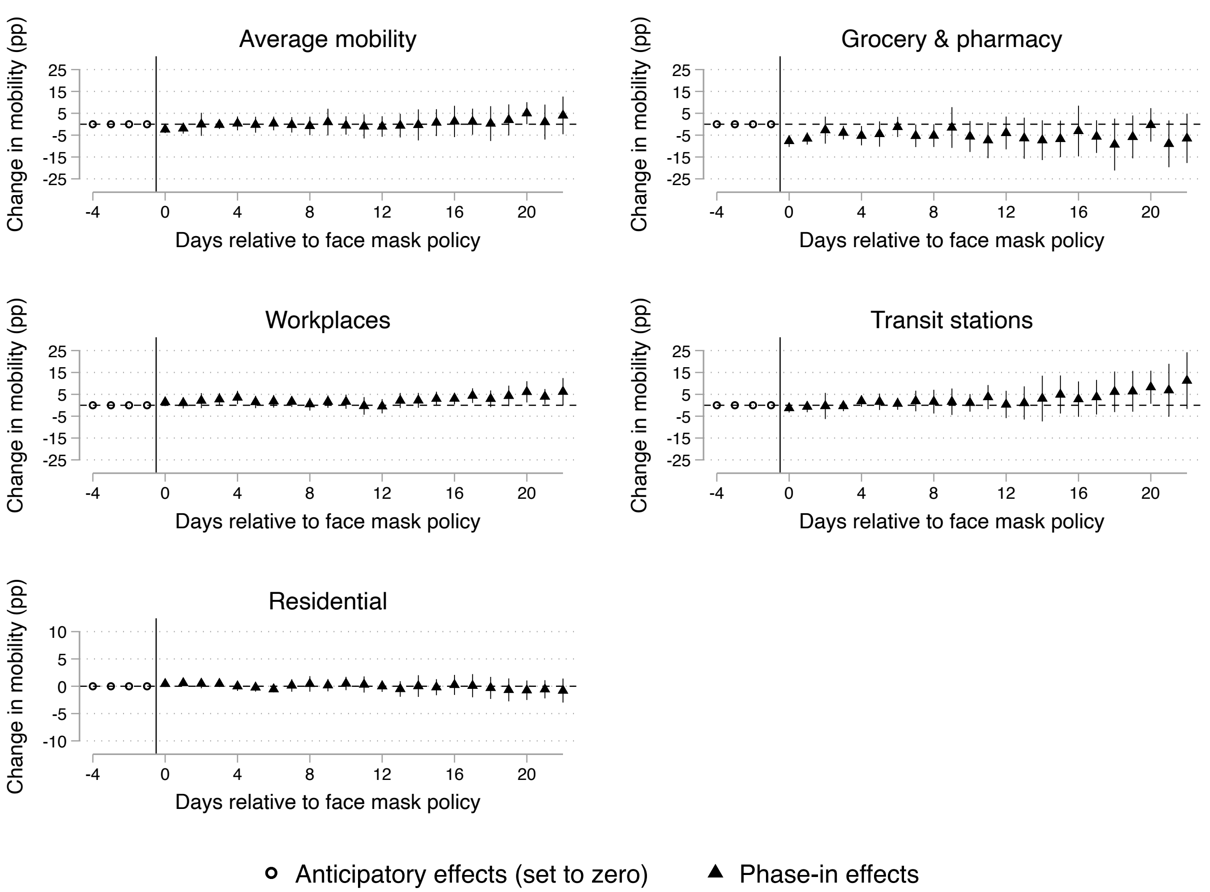
Figure 2: Fully dynamic event study estimates: Face mask policies and mobility in public spaces



Note: This graph shows the estimated anticipatory and over-time effects of compulsory face mask policies on average mobility, as well as mobility in specific public spaces as well as places of residence. Point estimates are obtained from a fully dynamic event study model (equation 2). Vertical lines represent cluster-robust 95% confidence intervals. The model includes controls from our preferred static DD model specification (Table 2).

We use the semi-dynamic specification (Equation 3) to investigate potential over-time effects of compulsory face mask policies - following Borusyak & Jaravel (2021). Figure 3 below summarises the results from the semi-dynamic model for our measure of average mobility in public spaces (top left). We find that the introduction of compulsory face mask policies reduced mobility on the day of the policy change. This decrease is equal to -2.4 percentage points or -0.14 SD (95% CI between -0.24 and -0.04). We do not detect any significant effects on mobility for any other days and can rule out any increase in average mobility in the medium-term.

Figure 3: Semi-dynamic event study estimates: Face mask policies and mobility in public spaces



Note: This graph shows the estimated over-time effect of compulsory face mask policies on average mobility, as well as mobility in specific public spaces as well as places of residence, based on a semi-dynamic event study model (equation 3), where all treatment leads are set to zero.

Figure 3 also shows over-time effects for mobility patterns in specific public spaces as well as time spent at home. We find that the introduction of compulsory face mask policies decreased mobility in grocery shops and pharmacies. Effect sizes lie between -7.7 percentage points (-0.31 SD) and -2.2 percentage points (-0.1 SD), which is consistent with static DD estimates. We find significant decreases in mobility in grocery shops and pharmacies within the first week of the policy change. For the remaining period, coefficients are consistently negative but not significant. In terms of hours spent at home, we find a small (0.1 SD) increase on the day following the implementation of compulsory face mask policies, but no longer-term effects. 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 3rd day following the change, and a 3.6 percentage point (0.19 SD) increase on the 4th day). However, point estimated are imprecise and rarely distinguishable from zero. We find no significant effects on mobility patterns in transit hubs.

Overall, the results do not provide evidence to suggest that compulsory face mask policies increased mobility - a key concern of policymakers. Instead, the introduction of these policies seems to have led to a short-term reduction in average mobility, reduced mobility in grocery shops and pharmacies, as well as a short-term increase in hours spent at home.

4.3 Robustness checks

DD specifications

We explore whether results are robust to a number of different specifications. First, we test for negative weights in our static DD specification. The average treatment effect in static DD models where units are treated at different points in time is equal to the weighted sum of several difference-in-differences that compare the evolution in outcomes between consecutive periods across pairs of groups (de Chaisemartin & D'Haultfœuille 2020). It is possible that some of these comparisons receive negative weights when treatment effects are heterogeneous among groups (de Chaisemartin & D'Haultfœuille 2020). We test how important this issue of negative weights is in our analysis, by using the *negativeweights* package developed by de Chaisemartin & D'Haultfœuille (2020). Results suggest that negative weights are indeed a problem in our analysis. Whilst only 6% of our estimates receive negative weights, the sum of negative weights is equal to -1.3, compared to a sum of 2.3 for positive weights. This provides an additional rationale for not relying solely on static DD estimates, but also using an event study approach and investigating robustness to methods that address the negative weights concerns.

Second, we estimate the event study model using the *eventstudyinteract* package developed by Abraham et al. (2021), as part of the rapidly growing methodological literature on staggered DD designs (de Chaisemartin & D'Haultfœuille 2020, Callaway & Sant'Anna 2021, Goodman-Bacon 2021). Abraham et al. (2021) show that in settings like ours, where dynamic effects are estimated using two-way fixed effects regressions that include leads and lags of the treatment, coefficients on these leads and lags can be contaminated by other periods. Abraham et al. (2021) propose an alternative estimator that is free of this contamination, by using a not-yet-treated control cohort. In the *eventstudyinteract* model, all time periods from when the last cohort receives the treatment need to be excluded from the analysis. It therefore uses a smaller sample than our dynamic (two-way fixed effects) event study model and excludes observations after April 28$^{\text{th}}$, given that Schleswig-Holstein was the last state to adopt compulsory face mask policies on April 29$^{\text{th}}$. As shown in Online Appendix D.1, the results are comparable to our dynamic event study specification when using a balanced window of 8 days before and after the treatment.[12] However, when using an unbalanced window of 22 days before and 8 days after the treatment (i.e. the largest possible window), the *eventstudyinteract* model produces a positive and borderline significant estimate on the second treatment lag (see Figure D2 in Online Appendix D.1). To assess how unusual this estimate is, we conduct a Monte Carlo simulation, which applies the unbalanced *eventstudy-interact* model on 500 simulated datasets that mimic the observed outcome data, but where there are no treatment effects by design. The simulation exercise indicates that a coefficient of the size estimated on the second treatment lag is not uncommon when using the unbalanced *eventstudyinteract* model in settings where no treatment effects are imposed.[13] We conclude that the results from the unbalanced *eventstudyinteract* model do not offer an important caveat to our main results, as the model seems susceptible to produce large coefficients (both positive and negative) on the treatment lags, even when null effects are imposed in the Monte Carlo design.

Third, our main event study analysis investigates mobility effects 22 days before and after the policy change - relying on the parametric assumption that treatment effects are homogeneous across units (states) and calendar time period (dates) and only vary across relative time period (days since treatment). As the gap between the first and last state adopting compulsory masking policies is 9 days, we re-run the main analysis on the time period that lies 8 days before and after the policy change. As shown in Online Appendix D.2, using this shorter time window does not influence our results. Results for static models shown in Table D1 are highly similar to our main specification, although point estimates and standard errors are somewhat larger – likely due to the reduced sample size. As shown in Figure D6 and Figure D7, fully-dynamic and semi-dynamic event study estimates are analogous to our main specification, as we find some reduction in mobility in the short term, but no medium-term effects.

[12] We also use the *didmultiplegt* package developed by de Chaisemartin & D'Haultfœuille (2020), which shows similar results (i.e. generally negative point estimates that are not significant), although point estimates are larger than those calculated using the *eventstudyinteract* package.

[13] The estimated coefficient on the second treatment lag (t+2) is 10.2, which means that, in 500 simulated datasets with no treatment effect imposed, 65% of the most extreme positive coefficients estimated on any treatment lag (t+x) fall below this value and 35% above this value.

Fourth, we run the fully-dynamic specification using a "binning" approach (Abraham et al. 2021), where we replace the first and last switch-on-switch-off leads and lags with switch-on-stay-on indicators (see Equation 4). A necessary and perhaps implausible assumption in this model is that before and beyond the capped leads and lags, anticipatory and phase-in treatment effects are constant (Borusyak & Jaravel 2021). As shown in Online Appendix D.3, we do not find evidence for significant pre-treatment trends using this specification, although estimates are somewhat lower than in our preferred "trimmed" specification. Results for mobility in specific public locations broadly hold, although there appears to be an upward trend in mobility in workplaces for later periods.

Fifth, we address the potential concern that our null-results are an artefact of too-few clusters (MacKinnon & Webb 2018). We show that the main results hold when using a "sub-cluster" wild bootstrap procedure (see Online Appendix D.4) and robust standard errors clustered at the state-week level (see Online Appendix D.5).

Finally, we address the issue that in 11 states, compulsory face mask policies were introduced on the same day as some classes in secondary schools were re-opened (generally only final year classes). The concern with this is that the re-opening of some secondary school classes likely increases mobility, which could outweigh any potential decrease in mobility due to face masks - creating an overall null result. To test for this, we re-run the analysis including only states where compulsory face mask policies were introduced independently. Although we lose power, our point estimates remain stable, as coefficients are negative and of a very similar magnitude (see Online Appendix D.6). In terms of the confidence intervals, in our preferred static specification (Model 5), estimates are non-significant and lie between -0.28 SD and 0.21 SD (compared to -0.2 SD and 0.03 SD for the full sample). Whilst error bands are clearly wider in this specification, the confidence intervals do not shift downwards. This is not what we would expect if the re-opening of some secondary schools increased mobility. If this were the case, we would expect a decrease in the treatment effect in a specification that examines the effect of compulsory face mask policies in isolation, relative to the combined effect with secondary school re-openings. As in most cases only the final grades of secondary school were re-opened, it is likely that the impact on overall mobility trends was modest.

District-level analysis

The main analysis focused on state-level mobility trends. This section re-runs the analysis using district-level (NUTS-3) mobility data. There are 401 districts in Germany, which cover between 150,000 to 800,000 inhabitants.[14] In most cases, districts introduced compulsory face mask policies at the same time as the states in which they are located. However, six districts introduced compulsory face mask policies before state-level changes were implemented - as documented by Mitze et al. (2020). As shown in Table 3, these districts (Rottweil, Main-Kinzig-Kreis, Wolfsburg, Braunschweig, Jena and Nordhausen) are located in four states (Baden-Wuerttemberg, Hessen, Lower Saxony and Thuringia).

Table 3: State and district-level implementation of compulsory face mask policies

State	State-level change	District	District-level change	Difference
Baden-Wuerttemberg	27/04/2020	LK Rottweil	17/04/2020	10 days
Hessen	27/04/2020	Main-Kinzig-Kreis	20/04/2020	7 days
Lower Saxony	27/04/2020	Wolfsburg	20/04/2020	7 days
		Braunschweig	25/04/2020	2 days
Thuringia	24/04/2020	Jena	06/04/2020	18 days
		Nordhausen	14/04/2020	10 days

Note: Based on Mitze et al. (2020). LK stands for Landkreis.

The German Statistical Office provides mobile-phone based data on daily community mobility in each district (Destatis 2021). These data have been used in a number of recent studies (Mitze & Rode 2021, Schlosser et al. 2020, Breidenbach & Mitze 2021) and are made easily available by Schlosser et al. (2020).[15] Unlike the Google COVID-19 Community Mobility Reports, these data do not capture the number of visits to specific public spaces. Instead, they capture the number of movements in a specific area (mobile devices switching from one radio cell into another). Mobility changes are shown in percent and capture differences in mobility between a given date and the monthly average for the corresponding weekday for the same month a year earlier. For instance, a value of -0.05 shows that mobility for a given day was 5% lower than for corresponding weekdays of the month in the previous year. As described in Destatis (2021), further adjustments are made for public holidays.[16] Data are based on all devices accessing the network of the Telefónica telecommunications company, that capture a third of the German mobile phone market. Data are processed by the private service provider Teralytics AG. All data are anonymised and aggregated and contain no personal information from users.

The mobility data provided by the German Statistical Office have the clear advantage of

[14] See: https://www.destatis.de/Europa/EN/Methods/Classifications/OverviewClassification_NUTS

[15] Data are available via an OpenScienceFramework repository: https://osf.io/n53cz/

[16] Mobility changes for public holidays are calculated by comparing mobility to corresponding public holidays a year earlier. For all other days, public holidays are excluded from calculating reference mobility values.

being available for smaller areas (districts rather than states). This allows us to explore additional temporal variation in mask mandates, as there are 23 days between the first district (Jena on April 6th) adopting compulsory face mask policies and the last districts doing so (all district in Schleswig-Holstein on April 29th). As we have information for a much larger number of units, these data also likely offer a cleaner comparison of treatment and control units. However, we see some drawbacks of these data compared to the Google COVID-19 Community Mobility Reports. First, as the data capture movements only when individuals move from one radio cell into another, they only pick up larger movements. Second, data are not disaggregated by location, meaning that we cannot estimate the effect on mobility in specific locations or factor out public spaces that were closed during the national lockdown. Finally, the data only capture movements within the Telefónica network. As Telefónica only captures a third of the German mobile phone market, coverage is likely worse than with Google data.

To examine the effect of compulsory face mask policies on community mobility at the district level, we closely follow the DD design used in the main analysis. As described in detail in Online Appendix D.7, we first use a static DD model (Equation 5). We run the analysis for all states, as well as separately for the four states with early adopting districts. To investigate pre-trends, we use a "fully dynamic" event study model (Equation 6). To asses how treatment effects change over time, we use a "semi-dynamic" event study model, where all leads are set to zero (Equation 7).

Table 4 shows results from our static specification (Equation 5) for the whole sample. We find no evidence that the introduction of compulsory face mask policies significantly affected mobility at the district level, as coefficients are not significant and close to zero. As shown in Table D9 in Online Appendix D.7, results are highly similar when the sample is restricted to the four states with early adopting districts. We can rule out increases in mobility that are larger than 0.07 SD.[17]

[17]In Model 5 in Table 4, the 95% CI for the treatment effect is -.004, .0101. The SD of the outcome is 0.154. Hence, we can rule out decreases in mobility as smaller than 0.03 SD and increases in mobility as larger than 0.06 SD. In Model 5 in Table 4, the 95% CI for the treatment effect is -.004, .010 and the SD of the outcome is .147. Hence we can rule out decreases in mobility as smaller than 0.03 SD and increases in mobility as larger than 0.07 SD.

Table 4: Effect of compulsory face mask policies on district-level mobility

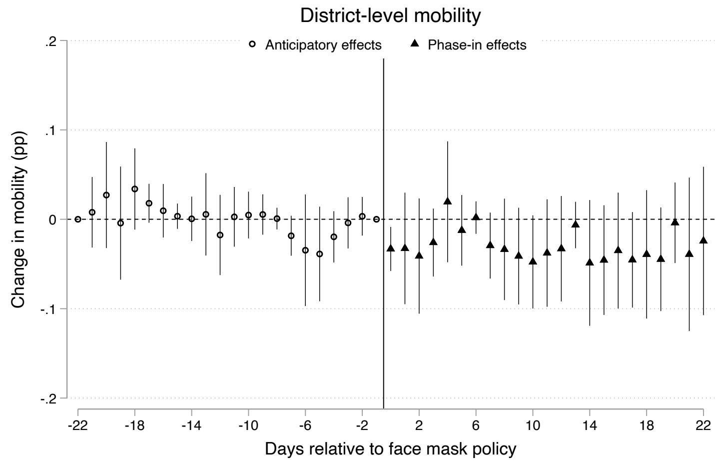
	Mobility in all states (NUTS 3-level)				
	(1)	(2)	(3)	(4)	(5)
Face mask policy	0.003	0.003	0.003	-0.001	0.003
	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Outcome mean	-0.249	-0.249	-0.247	-0.247	-0.247
Average temperature		✓	✓	✓	✓
COVID-19 cases (t-1)			✓	✓	✓
State-level policies				✓	✓
State*date FE					✓
Observations	24,060	23,880	23,482	23,482	23,482
R-squared	0.893	0.894	0.894	0.894	0.936
Number of clusters	401	398	398	398	398

Note: Districts in all German states are included in the analysis. The outcome in all models is change in district-level community mobility in Germany between March 23rd and May 21st 2020. All models control for district and date fixed effects, as well as for states relaxing stay-at-home orders. Robust clustered standard errors in parentheses.

Figure 4 shows results from the fully-dynamic event study model (Equation 6) for all states, which we use to assess the parallel trends assumption. Whilst there appears to be an upward trend in mobility 5 days before the policy change, fluctuations are modest and point estimates are not significantly different from zero. Figure D10 in Online Appendix D.7 shows results from the semi-dynamic event study model (Equation 7), which we use to assess over-time effects. As in the state-level analysis, we find that the introduction of compulsory face mask policies reduced community mobility at the district level on the day of the policy change. Effect sizes are modest at 1.8% or 0.1 SD. We find small increases in mobility for days 6 (3% of 0.2 SD), 13 (3.5% of 0.2 SD) and 20 (4.5% of 0.3 SD). However, these increases in mobility do not suggest a more general upward trend, as estimates are not significant and close to zero or negative for the remainder of the study period. Figures D12 and D11 in Online Appendix D.7 show fully-dynamic and semi-dynamic event study models for mobility at the district-level, focusing only on the four states with early adopting districts. Results are highly similar for this sub-sample.

Overall, the introduction of compulsory face mask policies seemed to have a similar effect on state- and district-level mobility. We find no evidence to suggest that compulsory masking policies significantly increased mobility at the district level. Instead, we find a decrease in mobility in the very short term, with no medium-term effects.

Figure 4: Fully-dynamic event study estimates for district-level mobility



Note: This graph shows the estimated anticipatory and over-time effects of compulsory face mask policies on changes in district-level mobility for 22 days before and after the policy change. Point estimates are obtained from a fully dynamic event study model shown in equation 6. Vertical lines represent cluster-robust 95% confidence intervals. The model includes controls from our preferred static DD model specification shown in Model 5 in Table 4.

Synthetic control

We implement a synthetic control design as a final robustness check. The synthetic control (SC) method is an alternative approach for evaluating the effect of aggregate-level policy interventions and relaxes the parallel trends assumption of the DD design. Specifically, the SC design allows the effects of unobserved variables on the outcome to vary with time (Abadie et al. 2010). Intuitively, the SC design weighs outcomes from available control units (often referred to as the "donor pool") to construct a 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).

We implement the SC method both at the state- and district-level. We focus on the first state to adopt compulsory face mask policies (Saxony) as well as the first district to do so (Jena). In Online Appendix D.8 we show that post-treatment mobility patterns do not differ significantly between the first state to implement compulsory face mask policies (Saxony) and its synthetic control. Similarly, we show that post-treatment mobility trends in the first district to implement compulsory face masks (Jena) closely track the mobility trends in its synthetic counterpart. Hence, results from the synthetic control at the state-level and district-level do not suggest that compulsory face mask policies significantly affected community mobility.

5 Discussion

We find that the introduction of compulsory face mask policies in Germany led to a short-term reduction in mobility in public spaces, with no significant medium-term effects. We can rule out even small increases in mobility larger than 0.03 standard deviations. Although we have no evidence on the effect of compulsory masking policies on other important behaviours such as hand-washing or social distancing, the findings presented here should to some degree alleviate policy makers' lingering concerns about masks increasing community mobility.

Our findings are in line with the only previous study we are aware of that investigates the effect of face masks on social distancing. In a small-scale field experiment implemented in Berlin, Seres et al. (2021*a*,*b*) find that masks increase distancing by approximately 8 cm, which does not indicate risk compensating behaviour. Interestingly, distancing behaviour was similar both before and after the introduction of compulsory face mask policies in Berlin (Seres et al. 2021*a*,*b*). We rely on a much larger sample covering all German states and also do not find evidence for risk compensating behaviour.

This study was conducted in a context where face masks were introduced alongside a national lockdown. Our sense is that policymakers are specifically interested in the effect of face masks on mobility in this setting. During a lockdown, reducing mobility is the main avenue to contain transmission and policymakers will be concerned that masks might undo the benefit of this costly intervention (Greenhalgh et al. 2020, The New York Times 2020, The Guardian 2020*b*, Reuters 2020). In an open society, the effect of face masks on mobility is comparatively less interesting, as the main policy directive is not to reduce mobility. Although lockdowns are no longer the main strategy to contain COVID-19 in many high-income countries (as vaccines have become more available), they continue to be important. At the time of writing, vaccines are only widely available in high-income settings, which means that relying on vaccination alone is not an option for policymakers in many low and middle-income settings (Holder 2021). In addition, countries with very low community transmission rates of COVID-19, such as New Zealand, still rely on lockdowns, and they also continue to be used to contain cluster outbreaks or outbreaks of new variants.

Our analysis is limited in five main respects. First, we only observe the impact of compulsory face mask policies in the medium-term. However, as changes in mobility generally fade out within days of the policy change, it is unclear if one would expect additional changes in behaviour after an initial adaptation period. Second, we only examine state and district-level trends in mobility and are unable to analyse heterogeneity between groups (for instance, high-risk groups). Uncovering this heterogeneity would require micro-level mobility data, which are currently not available due to privacy reasons. Third, one concern with the Google COVID-19 Community Mobility Reports is that the data are based on Google Account users who enabled Google's Location History feature. Whilst users have to allow Google to access their location history, this is often the default setting for installing apps from Google. Due to a default bias (Haan & Linde 2018), our sense is that many users will likely opt-in to this feature. Nonetheless, it is likely that these data are from a non-random sub-sample of the German population.[18] One might for instance assume that fewer users would opt-in to the feature in East German states – given the history of state-sponsored spying which likely has long-term effects on trust and preferences (Alesina & Fuchs-Schündeln 2007, Traps 2009). Whilst we have no data on the number of people using this feature, Germany has very high overall 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). Fourth, compulsory face mask policies may change people's willingness to enter public spaces, but may also change their behaviour in such spaces (e.g. standing closer or touching). We only capture the former margin in the analysis, but not the latter, due to a lack of suitable data. However, our sense is that people's willingness to visit public spaces is important for the transmission of COVID-19 and also has potentially important consequences for the economy by, for instance, influencing consumption behaviour. Finally, we do not have data on face mask use during the study period. Such data, as far as we are aware, do not exist. This means that we are not able to fully determine whether the effect sizes we observe are small or large.

While this paper provides important evidence for current policy debates on how to manage the COVID-19 pandemic, it is unclear to what degree results can be generalised to other settings. Further research is also needed on the impact of compulsory face mask policies on other important behaviours such as hand washing and social distancing.

[18]It is unclear whether older or younger users would be more likely to appear in our data, as younger users might be better able to customise applications according to their preferences, but older users might be less likely to use applications that rely on location history.

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Declarations of interests: None.

Contributions: RK and MD jointly designed the study, conducted the analysis and wrote the first draft of the paper. JT provided guidance on the analysis and reviewed the paper.

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