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The effects of non-pharmaceutical interventions on mobility - evidence from the early stage of the pandemic in Germany

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List of Acronyms

ATT	Average treatment effect on treated
AGS	Amtlicher Gemeindeschlüssel
DiD	Difference-in-Difference
FE	Fixed effects
IO	Intervening opportunities
M08	Policy restricting gastronomy
M10	Policy restricting nightlife activities
M14	Policy restricting internal movement
M17	Policy restricting workplaces
NPI	Non pharmaceutical intervention
NA	Missing value
OSM	Open street maps
PPML	Pseudo Poisson maximum likelihood
SLX	Spatial cross regressive

List of Symbols

A	Balancing factor for Gravity model
i, j	County indices
n	Number of counties
N	Number of county pairs ($n \times n$)
t	Time index
T	Number of total periods
M	Mass of a county measured as population count
D	Distance between two counties
V	Mobility flux between two counties
P	Policy dummy
K	Covariates
s	Index of covariate
S	Total number of covariates
ϵ	Additive error term for gravity model
$\tilde{\epsilon}$	Multiplicative error term
δ	Coefficient for distance
β	Coefficient for county mass
κ	Coefficient for covariate
γ	Coefficient for Policy dummy
α	County or county pair fixed effect
μ	Time fixed effect
λ	Abbreviation for all time variant covariates
f	Abbreviation for all used fixed effects
W_o	Origin based spatial weight matrix
W_d	Destination based spatial weight matrix
ω	Indicates spatially lagged version of the variable it is an superscript of

1 Introduction

In early 2020 the Covid-19 pandemic began to severely hit the first countries in Europe and spread rapidly throughout the world. While the possibility of deadly pandemics was known, it was not part of the public debate at that time. However, many governments all over the globe reacted quickly and employed heavy non-pharmaceutical interventions (NPIs) with the aim of reducing human mobility and minimizing the spread of the disease. Since most restriction policies impose significant economic costs onto societies and ineffective and inefficient policies need to be curbed, a growing field of research has evolved since then and is now evaluating the effectiveness of NPIs which are meant to reduce population mobility. [61, 64, 20, 1, 13].
Recently, telecommunication data from various sources has become available to a broad audience. Providing unmatched spatio-temporal resolution, this data can be used to infer about real-life mobility patterns with high precision. This new data availability can be leveraged to precisely assess the performance of Covid-policies. Since population mobility has been shown to be closely related to the growth of Covid-19 cases in the community [64] and considering that NPIs are associated with economic costs, this research plays an important role in the efficient management of future pan- and epidemics of airborne pathogens. In Germany, the competence with respect to most NPIs was at the level of federal states during the Spring of 2020. Such that, similar to the United States, a natural experiment with large variations in terms of the implementation of NPIs occurred. [1].

In this thesis, mobile phone data provided by Teralytics is used to measure the effects of selected NPIs on mobility patterns in Germany during the early stages of the pandemic at the level of 400 counties. Controlling for the pandemic situation, home office potential and election results as proxy for government obedience, the following NPIs are considered: restrictions for gastronomy (**M08**), interventions on nightlife activities (**M10**), internal mobility restrictions (**M14**), regulations for workplaces (**M17**). The aim of this thesis is to answer the question of to what extent these selected policies helped to reduce human mobility in Germany. Further, it is of interest what role spatial spillover effects had for the effectiveness of these policies.

To answer these questions, a spatial interaction model is employed following the gravity approach. To measure the impact of policy change on population mobility, a two-way fixed effects Difference-in-Difference (DiD) study design is used. Reliable estimation of the coefficients, in the presence of zero mobility flows between counties, is made possible by utilizing the Pseudo-Poisson Maximum Likelihood estimator[52]. Most studies so far have neglected spatial spillover effects in the context of mobility restricting policies. This is especially worrying since the framework of DiD estimation can be heavily biased if spillover effects are present and not controlled for [38]. That is the reason why in this study the regression is augmented with spatial lags for the NPIs such that the final model falls into the category of cross-regressive spatial models.

In Section 2 the relevant literature leading up to this study is evaluated and the dynamics of the early pandemic is summarized. Section 3 revisits the theoretical background of the gravity model for modeling spatial interactions. Section 4 describes the data base of this study and offers some descriptive statistics to put this analysis into perspective. Section 5 deals with the challenges of econometric modeling and includes dedicated subsections from study design to estimation strategy. The regression results are then presented and discussed in Section 6. The last section concludes.

2 Previous Research on Mobility Behavior Before and During Covid-19 Pandemic

2.1 Literature Overview

The effects of NPIs on mobility patterns and Covid infections are of great interest for governments all over the world. Thus, it is not surprising that the corresponding literature is growing rapidly. A general overview for potential effects of Covid-19 is provided by [18]. The article of [59] offers a description of the mobility behavior of people during the early stages of the pandemic. Although being only descriptive, this article provides a basis for mobility analysis during the Covid-19 pandemic. A widespread decline in human mobility across many countries is reported.
Another study by [20] used GPS mobility data from Unacast, available for the US during the early phase of the pandemic, to estimate how the change in distance traveled by citizens is related to the perceived risk of contracting the disease and government restriction orders. They found that higher infection rates lead to a decline in mobility. However, the decline associated with stay-at-home restriction orders is magnitudes higher. Additionally, they found that regions with a lower share of votes for the republican party are more responsive to restriction orders.
The study of [37] evaluated the mobility behavior of US citizens during the 7-month period from march 2020 to September 2020. Using a spline model they found that people sharply reduced their mobility during the first wave ranging from march to June. But quickly recovered to pre-Covid mobility levels. During this phase counties with restriction orders in place reduced mobility more strongly than those without restrictions. During the remaining period up to September 2020 only little mobility reduction was observed.
Using a Difference-in-Difference approach with fixed effects for US counties and time,

[1] examined to what extent policy interventions affected mobility patterns during the beginning of the Covid-19 pandemic. The authors found that stay-at-home orders were most effective at reducing mobility, especially when adopted early. Restrictions on bars and restaurants also had a moderate effect. The effect associated with the closure of non-essential businesses was small. The other NPIs considered did not lead to a significant reduction in mobility (limited stay-at-home order, ban on large gatherings, mandate of school closure). For many states, a reduction in mobility was observed before policies were implemented. This implies that part of the response to the Covid-19 pandemic is due to voluntary reduction of social contacts and thus also of mobility.
Using a different approach, called regression discontinuity analysis, [61] studied the effects of policy interventions on mobility as well as the effect on Covid case growth in the US during Spring 2020. Their analysis yields results comparable to those of [1]. Limits in bars and restaurants were found to be the most effective policy intervention here, while school closures and bans on large gatherings did not show significant effects on mobility. It should be noted that the estimated effects on mobility varied substantially between states. Only seven states imposed a third level of mobility restriction, which was a shelter-in-place order. These policies resulted in a further decline in mobility similar to that occurring when imposing social distancing policies. Interestingly, the authors note that the declaration of state emergency was associated with a 11.5% decrease in mobility. This is surprising since the declaration did not impose any legal obligations onto the citizens, again hinting toward a voluntary component of the populations response towards Covid 19.
This finding is consistent with the results of [28]. Using a flexible event study approach, they found that state declarations had a measurable effect on mobility reduction. They further found that other information events without legal obligations, such as the reporting of the first case or death related to Covid-19, can have effects on population mobility. Unlike other studies, a significant reduction in mobility was associated with school closures. In general, the authors report that the observed effects started small and increased over a period of 20 days.
The paper of [33] used mobile device location data for the US at the county level to employ a big data approach for modeling human mobility trends. A generalized additive mixed model was used to evaluate the mobility effects of NPIs. It was found that restriction policies helped to reduce travel. However, the effect was relatively small and more pronounced for more strict policies (3.5% to 7.9%). Similarly to other studies a quick rebound from mobility reduction was observed and it was found that counties with more democrats were more likely to reduce their mobility. Unlike many other studies, spatio-temporal auto-correlation was not neglected in this study. Using a spatial interaction term the authors aimed to reduce the bias of their estimates. However, the spatial effects themselves were not given much attention

other than that.

Similar studies for Europe were conducted as well. In the article of [19] it is described how in early 2020, the overall mobility in The UK was greatly reduced. Using Google mobility data it was found that different areas reacted differently to restriction policies and a mild form of "tiredness" from Covid-lockdown was observed. Hinting on the one hand to the importance of heterogeneous traits of regions, such as partisanship and wealth distribution etc. as proposed by other studies. And on the other hand, giving further evidence for the short-lived mobility effects of NPIs.
The study of [16] evaluated the effects of the relatively mild restriction policies implemented in Sweden during the early pandemic. In contrast to most other countries, Sweden relied on mostly voluntary mobility reduction. Using mobile phone mobility data the authors deployed a Difference-in-Difference model and found that even mild restriction can significantly decrease population mobility. Especially longer travel distances were reduced in Sweden during the time frame of this study. Among the most influential policies in Sweden were recommendations for working from home.
Similarly, the study of [65] found that non-mandatory NPIs greatly reduced urban mobility in Tokyo during the early phase of the pandemic.
The authors of [64] deployed a pre-post comparison with a linear mixed-effects model to assess the effectiveness of social distancing policies in Europe. In addition to providing additional evidence for voluntary mobility reduction, the authors found that the greatest reduction in mobility was observed for mandatory stay-at-home orders. Workplace closures also led to a significant decrease in mobility, while other measures did not contribute significantly to the reduction in mobility. In general, they found that mandatory policies were more effective than voluntary ones. The authors further observed a near one-to-one relationship between mobility reduction and fall in Covid case counts.

Other studies did not limit themselves to single coherent geographic locations. Instead, the effects of social distancing policies on mobility were evaluated for a large number of countries.
Using google mobility data for many countries during the early stage of the pandemic in combination with a fixed effects model, the authors of [45] found that mobility was significantly reduced by a voluntary component. This voluntary reduction accounts for 14% of the total mobility reduction. However, government imposed mobility restrictions were magnitudes more effective and reduced mobility by up to 50% cumulatively.
The study of [6] examined the effect of lockdowns on population mobility and Covid incidence. They highlighted the importance to control for concurrent policies to avoid potential biases. Using a country-level multiple event study, it was found that

the cancellation of public events, as well as restrictions on private gatherings and closure of schools had the greatest impact on reduction of mobility and Covid case counts. Workplace and stay-at-home requirements have been reported to have a moderate effect, and controls for international travel, as well as public transport restrictions and internal mobility restrictions did not have a significant effect. Policies that were not directed at crowded and frequently visited places were not associated with a decline in total case counts, even if they had effects on mobility. The study was later extended to 175 countries and published.[7]
Another country-level study conducted in early 2020 is [36]. Using a natural experiment, an interrupted time series model was built to assess the effectiveness of physical distancing policies in preventing Covid-19 transmission. The results suggest that social distancing policies are indeed effective at reducing Covid transmission. The finding from [6, 7] that restrictions on public transport do not have a significant effect on the dynamic of the Covid-19 pandemic is supported. It is also supported that interventions were generally associated with a greater effect if implemented earlier rather than later.

The authors of [50] used mobile phone data to evaluate structural changes to the mobility networks in Germany. Similar to studies from other regions it was found that mobility was strongly reduced as restrictions were in place but afterwards recovered to pre-pandemic levels. Furthermore, it was found that especially the number of long-distance trips as well as mobility in large cities was reduced. This reduction in long-distance travel is highly important for evaluation of restrictions policies since cross-regional spread of Covid-19 can be prevented effectively this way.
For this thesis, a highly relevant piece of literature is [25]. In this study, the effects of policy interventions on population mobility are evaluated for Germany at county level. The data set used for this study was provided by Teralytics and is identical to the data used in this thesis. Using an event-study approach, the authors found that people in Germany started to reduce their mobility after the appeal of the government. This finding is consistent with the results of studies from abroad. Mobility reduction occurs without legal obligations. However, the most drastic decline in mobility was observed after schools and retail stores closed on March 16.

So far only studies that examined the behavior of the population as a whole were considered; however, there is evidence that the response to policy interventions differs between socioeconomic groups. The authors of [12] found that women and young people are generally more affected by social distancing policies. This is problematic since especially young people rely heavily on work-related income, which is closely linked to the need for mobility. Other evidence suggests that wealthy people respond more strongly to policy interventions than less wealthy people [24, 60, 37, 33]. This implies that less wealthy people are more vulnerable to the spread of disease, since reduction of mobility is often not possible.

The work by [14] suggests that regions with high trust in the government but low confidence in their health system have greater adherence to social distancing policies. [27] found that for the US democratic leaning counties had a more pronounced response to governors recommendation when compared to republican leaning counties. Furthermore, it was observed that democratic leaning counties reacted even stronger if the governor announcing the recommendation is of republican partisanship.

In summary, it can be concluded from this review of the literature that policy interventions are associated with a large decline in population mobility and a following rebound effect. The response of the people, however, differed substantially even at the regional level and across socioeconomic groups. Furthermore, the adherence to policy interventions seems to be related to political partisanship. For different geographic regions, different interventions were found to be most effective. Almost all studies evaluated here found some evidence for voluntary reduction of mobility, some as a response to local information events. However, the greatest decline in mobility was observed for mandatory policies aimed directly at occupations where many people frequently meet (ban of large gatherings, school closure, stay-at-home/shelter-in-place orders). It was also established that reducing population mobility does coincide with a drop in Covid case counts.

2.2 The Pandemic in Germany

As mentioned above, a similar study has been conducted by [25]. In this paper a time line for the Covid-19 pandemic in Germany was presented. I will quickly review it here with some additional notes.
In Germany, the first cases of Covid-19 were recorded in late February of 2020. In the following weeks, the situation worsened quickly and governments started to react. During the first weeks, only few restrictions were imposed, such as bans on large gatherings or quarantine of infected individuals. During the first response, some counties acted autonomously and implemented their own policies to combat the spread of Covid-19. However, these were quickly overruled by state-level policies. On March 12 the government appealed to its citizens to reduce contacts and shortly after some state governments implemented mobility restrictions, including some of the policies analyzed in this paper. Later on March 22 a country wide contact ban was issued. This date marks the point from which on the grid of NPIs began to unify across Germany. Up to this point, most regulations were issued in the respective states. From April 20 on, governments started to lift restrictions again.

3 Gravity Model for Mobility

For more than a century, research concerning human mobility behavior has frequently relied on the principles of the physics inspired gravity model. The simple rationale is that the interaction between two sites is directly proportional to the respective masses and inversely proportional to their distance. This model, first introduced by Ravenstein [47], was later extensively refined (see [30] for an overview) and remains highly relevant as a workhorse for research, not only in modeling human mobility, but also in modeling international trade flows [32].

In addition to the gravity model, the model of intervening opportunities (IO) has received a lot of attention in this research field [30]. In contrast to the gravity model, this approach assumes that the main driver of interaction between two masses is the number of opportunities at the destination. The impediment factor here is the number of intervening opportunities in between, instead of geographic or cultural distance as for the gravity model [56]. The IO model has also been extended and adapted multiple times [30, 41, 22]. A derivative of the IO model is the so-called radiation model, which takes a different physics-based approach to model human mobility. In this model, people are emitted from an origin and are absorbed by the next fitting destination [53, 48, 43]. Others have attempted to predict commuting flows using machine learning [54].

However since the IO model as well as the radiation model rely on solid theoretical motivation for the observed mobility, such as shopping, working and holiday visits and since the machine learning models are not suited to infer about determinants of mobility due to their black-box character, I will focus on the gravitational model in this thesis.

The gravity model for the panel data is specified in analogy to [62]. Let V_{ijt} be the mobility flow between two regions $i = 1, ..., n, j = 1, ..., n$ at time $t = 1, ..., T$. Note that it is explicitly allowed for $i = j$ and thus mobility that occurs within regions is included. To simplify notation, the number of observations per period t is denoted by $N = n \times n$. The most simple gravity model for mobility only includes a measure for the masses of the origin and destination region (M_i, M_j), a deterrence parameter (D_{ij}) and a gravitational constant (A_{ij}). Since the distance between two regions and the respective masses are assumed to be time invariant, no time index is needed for those.

$$V_{ijt} = A_{ij} M_i^{\beta_1} M_j^{\beta_2} D_{ij}^{\delta} \tag{1}$$

For modeling human mobility, masses are commonly measured as the population residing in the respective region or as total inflows and outflows of mobility thereof [30]. The deterrence parameter is usually just the distance between the two regions.

To model NPIs I include dummies for these policies. P_{it}, P_{jt} being one if the policy is active in region i or j at time t and zero otherwise.

$$V_{ijt} = A_{ij} M_i^{\beta_1} M_j^{\beta_2} D_{ij}^{\delta} \cdot exp[\gamma_1 P_{it} + \gamma_2 P_{jt}] \tag{2}$$

To complete the model an error term ϵ_{ijt} and S additional regressors specific to the origin or destination $K_{s,it}, K_{s,jt}$ are included. Note that these additional regressors may or may not be time-varying meaning that for some s $K_{s,it} = K_{s,i(t+1)} = ... = K_{s,iT}$ and $K_{s,jt} = K_{s,j(t+1)} = ... = K_{s,jT}$.

$$V_{ijt} = A_{ij} D_{ij}^{\delta} M_i^{\beta_1} M_j^{\beta_2} K_{s,it}^{\kappa_1} K_{s,jt}^{\kappa_2} \cdot exp[\gamma_1 P_{it} + \gamma_2 P_{jt}] + \epsilon_{ijt} \tag{3}$$

To obtain a multiplicative model, integrate ϵ_{ijt} into the first term. To do so, abbreviate the first term of the sum as C. By letting $\tilde{\epsilon}_{ijt} = 1 + \epsilon_{ijt}/C$ the multiplicative model can be written as

$$V_{ijt} = A_{ij} D_{ij}^{\delta} M_i^{\beta_1} M_j^{\beta_2} K_{s,it}^{\kappa_1} K_{s,jt}^{\kappa_2} \cdot exp[\gamma_1 P_{it} + \gamma_2 P_{jt}] \tilde{\epsilon}_{ijt} \tag{4}$$

4 The Data

4.1 Telecommunication Data

The unique data set provided by Teralytics leverages the switches made by cell phones when entering an area served by a different cell tower than before to track human mobility. To reasonably infer about trips, machine learning algorithms are used to stitch switches between cell towers together if they meet certain criteria. For example, short stops in between are ignored so that coherent movements are detected as one single trip. Additionally, since only one telecom provider is used as a data source, the raw data must be extrapolated to reflect the behavior of the whole population [58]. The mobility data for 513 regions, corresponding to Open Street Maps (OSM) relation IDs are available, grouped by mode of transport (road, train, plane, not classified), for every day starting January 2019 up to December of 2021. For the analysis, the 513 regions were aggregated to the 400 German counties.[1] While this data set contains no personal information such as socioeconomic status or demographics, it allows for an analysis with very high spatio-temporal resolution.

[1] Since no adequate matching table was available the translation from relation IDs to the German AGS5 was done using web scraping from www.openstreetmap.org/relation/... where "..." represents the relation ID which is to be matched.

The data provided by Teralytics only reports traffic sizes of 5 observations per day or more; this leads to a large number of zeros in the dependent variable.[2]

4.2 Pandemic Data

Information about Covid policies, infection rates and other pandemic related variables is sourced from [35]. This platform was built by infas Institute on behalf of the BMWK (German resort for economy and climate). The policy data set contains daily entries starting from March 1st, 2020, for each German county. A one indicates that the corresponding policy was active during this day and a zero indicates that it was not active. From the set of all policies implemented, four were chosen for this analysis. First, there are restrictions on gastronomy (M08) and restrictions on nightlife activities (M10). Both policies were found to be relevant in previous studies and were among the first policies to be implemented by many counties. Second, there are restrictions on internal movement (M14) and restrictions on working places (M17). Although the literature suggests that (M17) may have effects on mobility, (M14) was found by several studies to have no effect. Both restrictions were first implemented only by few counties and many counties never adopted these two policies. See the appendix B2 for implementation dates on state level. Note that the policies evaluated here are decided on at state level not on county level.

Data about the seven-day incidence are available for each day starting on March 7, 2020, at county level. This data restriction and the fact that new implementations of policies after the month of April are sparse define the boundaries of the time frame examined in this study. The focus is on the time between March 7 and April 30 in the year 2020 where there was a high dynamic in terms of both infections and political action.

4.3 Election Results

The data on the election results was sourced from [9] and reflects the results of the Bundestagswahl 2017 at state level. The constituencies are not nested within counties in Germany and, therefore, cannot be aggregated easily.

4.4 Home-Office Potential

The variable that gauges home office potential is modeled following [2] where the values are calculated using administrative and qualitative survey data. The calculations used here were provided at county level by IAB-Nord. The paper of [2] found that on average in Germany about 56% of the jobs can be done from home. For large

2 In the raw data no entry is made for these zero observations. To obtain a balanced panel, the zero values were filled in.

cities, this potential for home office increases up to 65 %. They also found that a large portion of home office potential remained untapped prior to the pandemic. In this analysis, I will use the home office potential, which was calculated using industry structure, to examine how this untapped potential for home office affects mobility patterns.

4.5 Distance Measure

To build the weight matrix, meaningful measures of the distances between and within counties are needed. To accomplish this, the distances between counties were calculated using the coordinates of the geometric center of each county. Using the OSM routing service, where information about streets, traffic signs/lights and speed limits are available, the distances can be computed as the time needed to travel by car.

These travel durations form the basis of the weight matrix. However, the intra-county distances have to be approximated. The problem of intra-regional distances is not new, and some workarounds have been proposed. In this paper, I will use the approach used by [46] that builds on the work on intra-city distances by [31]. It is suggested to approximate the distances using a scaling factor $\frac{1}{\sqrt{\pi}}$ and multiplying by the square root of the area of the respective region. This approach yields a reasonable estimate of intra-regional distance.

4.6 Other Data Sources

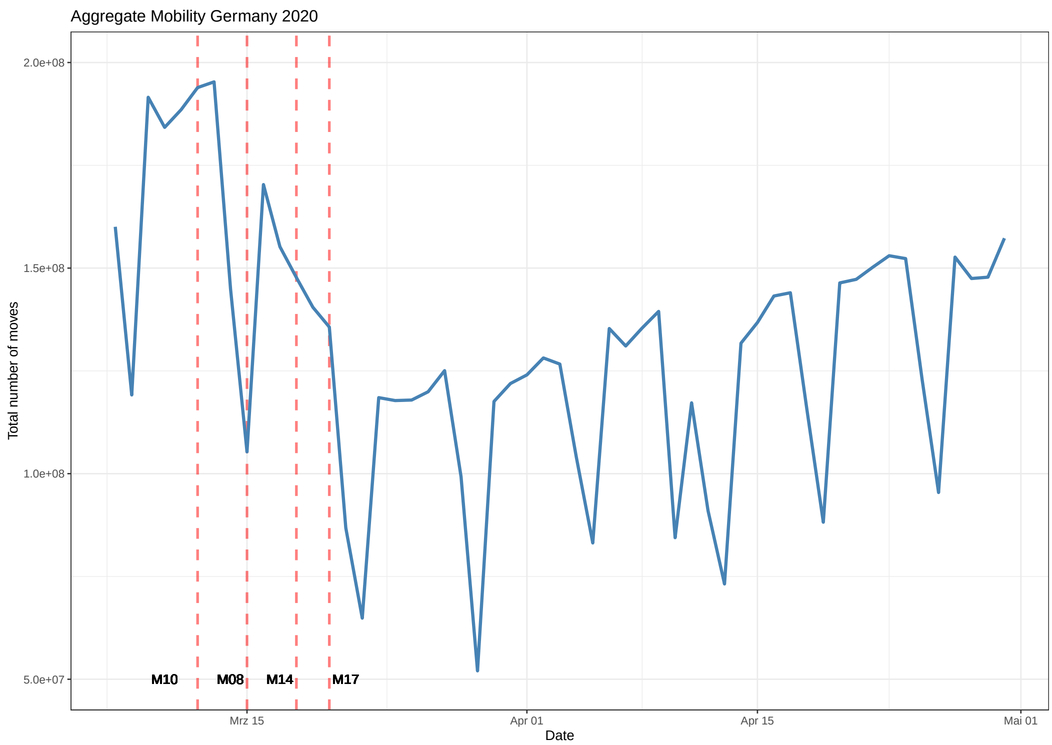
Data on county-specific characteristics such as area and population are sourced from the Bundesamt für Statistik [55].

4.7 Descriptives

When looking at the data, it becomes obvious that the majority of county pairs only have very small linkages in terms of mobility flux. For many pairs of regions, many days do not have any registered movement between these two regions. Furthermore, most of the movement happens internally. These observations are not surprising and provide first evidence for the adequacy of the gravity model. On average about 815 trips occur between two regions on a given day. However, this mean is very misleading since for example Berlin, which is not segregated into smaller counties exhibits intra-county mobility with values way above 6 Million moves per day. 75% of the observed mobility flows were smaller than or equal to 11, while the median is zero. Even when concentrating on moves that occur between counties and not within, the problem persists. Although the average and maximum values are significantly smaller, the distribution is still heavily skewed to the left. Finally, the distribution of intra-county moves is much more evenly distributed. The range spans from 14181 moves in the county of Schwabach to more than six million in Berlin. For intra-county mobility the median lies at 180000 moves and about 75% of the connections exhibited less than 300000 moves per day. Further characteristics of the distribution and a geo-referenced visualization of mobility networks before and 2 weeks after first treatment, can be found in the appendices [3,4,5].

When examining the time trend of aggregated moves in Germany, it can be observed that in accordance to the literature, mobility declines strongly from mid-March to the beginning of April. Afterwards, the trend reverses and mobility starts to rise again. The following graphic depicts the time trend from March to May. The dates at which the different policies were implemented for the first time are highlighted [1]. While I already discussed the dynamics of policy implementations in Germany, it is

Figure 1: Aggregated Time Trend of Population Mobility

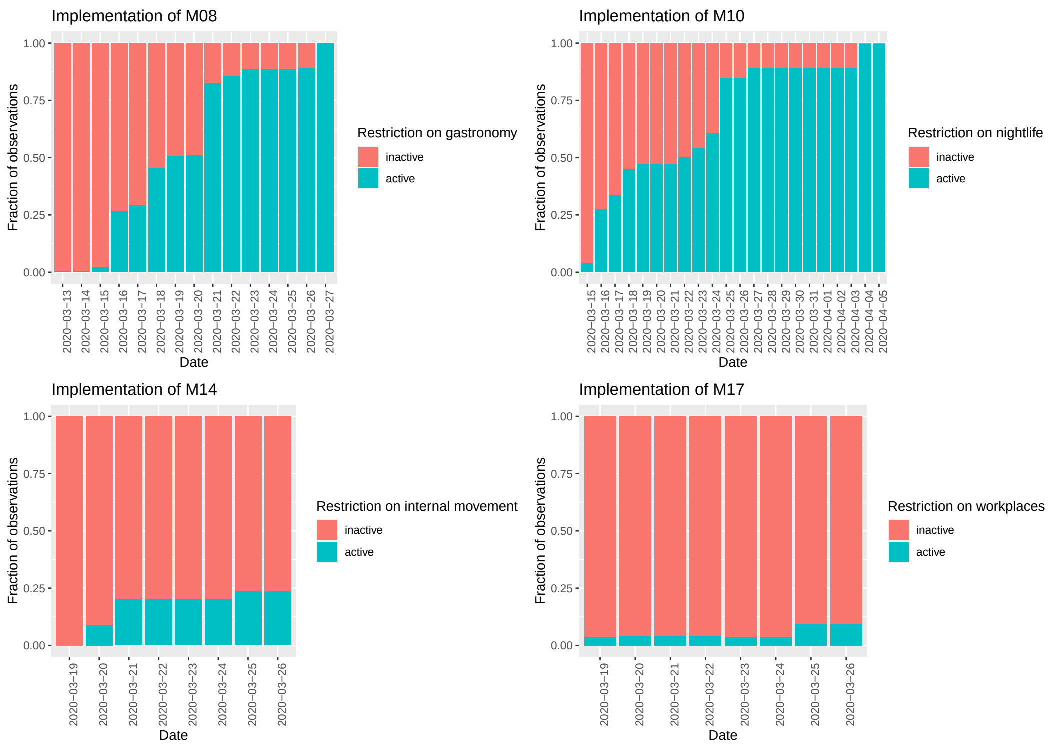


Own representation with data provided by Teralytics

recommended for a better understanding to examine the data used in this analysis. Looking at the figure [2] below, it can be seen that the first two policies (restrictions on gastronomy and restriction on night life activities) start to get implemented around March 13 to 15. Subsequently, all counties implement those policies within the following 3 weeks. This allows for sufficient heterogeneity across regions to deduce meaningful inference. However, it has to be kept in mind that eventually, no controls that are not exposed to treatment are available. For the other two policies, things are a little bit different. The policies which impose restrictions on working

places and on internal movement were first implemented from the 19th to the 20th of March. In contrast to before, these two policies do not get implemented by all counties, at least not during the observed time frame. Within one week after first implementation, about a quarter of all counties have adopted restrictions on internal movement and about 10% imposed restrictions on workplaces.
This study relies thus on the quasi-natural experiment that emerged when state governments acted semi-autonomously and counties in different states experienced differences in the number and severity of NPIs imposed on them. Leveraging the Difference-in-Difference study design the causal effect of NPIs on population mobility can be identified.

Figure 2: Policy Implementation



Own representation with Data from [35]

5 Econometric Methods

5.1 Fixed Effects

The fixed effects model is a regression technique for panel data that leverages the richness of multilevel data to improve standard OLS and allows estimates of the causal effect of treatment, the only assumption being that there is no unobserved unit-specific heterogeneity in the data [8]. Specific to this thesis a panel data set is used where the individual level is the link between two regions rather than the regional units themselves. Since individual characteristics are often related to other features of a regression, they induce bias and should be dealt with accordingly. Compared to OLS the FE model splits the equation error into two parts. The first part captures all unobserved time-invariant individual or group specific characteristics, the second part consists of the characteristics which vary over time and between individuals/groups. Although this decomposition of the error term is possible even when working with cross-sectional data, it is only identified when repeated observations for individuals/groups are available [8].
Consistency of the FE estimator relies on the assumption that the time-varying part of the error term is strictly exogenous with respect to the time-constant part and regression features. However, no assumption regarding the relation between the stable characteristics and the regressors must be made [8]. For panel data, the rather strong assumption of strict exogeneity can be relaxed by allowing untreated subjects to contribute to the estimate through time fixed effects.

The standard FE estimator emerges when performing OLS on demeaned pooled panel data. This equals an OLS regression with intercepts estimated for each individual [8]. The fixed effects approach could thus be written as

$$y_{it} = x_{it}\beta + \alpha_i + \epsilon_{it}$$

Where α_i is the intercept for observational unit i and ϵ_{it} is the time variant error component.
FE estimation allows to wipe out group-specific heterogeneity; therefore, it is applicable to include fixed effects for the origin, the destination and the individuals (region link). This way, heterogeneity that resides at the group level of counties as well as at the level of region pairs can be removed [8]. By denoting fixed effects α_i, α_j for region-specific characteristics and α_{ij} for pairwise characteristics of the masses, all regressors without time index and the distance variable are thus dropped from the equation.

$$V_{ijt} = K_{s,it}^{\kappa_1} K_{s,jt}^{\kappa_2} \cdot exp[\gamma_1 P_{it} + \gamma_2 P_{jt} + \alpha_i + \alpha_j + \alpha_{ij}]\tilde{\epsilon}_{ijt} \tag{5}$$

Formally, one could test the appropriateness of the fixed effects model in comparison to the Random Effects and/or the Mixed Effects model which can be more efficient if appropriate. The test used for this is the Hausman test [29]. Since I am pursuing a DiD framework and since it is of great advantage to remove group-level heterogeneity which is likely to be correlated with the dependent variable, I will continue using fixed effects.

5.2 Difference-in-Difference

A common method to perform causal inference in the presence of policy changes in social sciences is the Difference-in-Difference approach. This approach allows to eliminate both selection bias and general time trends and is therefore well suited for panel data problems [4]. The basic intuition is as follows: Consider data points for two groups at two different time periods. The first period is set before treatment for one of the groups is implemented and the second period is set after treatment has taken place. The first difference observed is for each group the difference in outcome over time. The Difference-in-Difference is then the difference between the treated and untreated group regarding the first difference [49]. The question asked is thus: By how much differ the changes in outcome which occur over the course of a certain time frame between untreated and treated group on average?

To give an overview, let me paraphrase the concepts of DiD as advocated in popular textbooks [15, 4].
We start by acknowledging that each individual has two potential outcomes in the post-treatment period.

$$y_{pot} = \begin{cases} y_{1i} & \text{if } D_i = 1 \\ y_{0i} & \text{if } D_i = 0 \end{cases}$$

Here, the potential outcome y_{pot} is y_{1i} if the individual is treated in the post-treatment period ($D_i = 1$) or y_{0i} if it is not treated ($D_i = 0$). Of interest is the difference between these two potential outcomes. The observed result can be expressed as an equation of both potential outcomes.

$$y_i = y_{0i} + \underbrace{(y_{1i} - y_{0i})}_{\text{Causal effect of treatment}} D_i$$

The fundamental problem here is that both potential outcomes for any individual can never be observed. Therefore, resort to comparing the average outcomes of those who were treated with those who were not. This leads to the following relationship:

$$\begin{aligned}E[y_i|D_i = 1] - E[y_i|D_i = 0] = &\underbrace{E[y_{1i}|D_i = 1] - E[y_{0i}|D_i = 1]}_{\text{ATT}} \\ &+ \underbrace{E[y_{0i}|D_i = 1] - E[y_{0i}|D_i = 0]}_{\text{selection Bias}}\end{aligned}$$

By extending this expression by zero, as shown by [15], it can be seen that the classical 2x2 Difference-in-Difference estimator consists of the average treatment effect on the treated (ATT) and the bias that occurs if the trends of the control and treatment group differ from each other. This is why the most fundamental assumption ensuring the validity of DiD inference is the so-called Parallel Trends Assumption.
This assumption states that there must not be a pre-existing difference in trends between the treated and the untreated group. That is, in a counterfactual scenario in which the treatment did not occur, both groups would have evolved in the same way [38]. In a regression context, covariates that contribute to differential trends for the treatment and control group can be included to ensure parallel trends [38, 49]. Since covariates and also fixed effects ideally have explanatory power for the dependent variable, including them also helps to reduce standard errors and increase the precision of estimates [15]. Another important aspect that is implied is that the treatment must not be endogenous. Keep in mind that the Parallel Trends Assumption makes statements about counter-factual scenarios and is therefore by definition untestable. Economists typically conduct an event study to provide some form of evidence for parallel trends. If the coefficients for the treatment effect do not differ significantly from zero before treatment has taken place, then at least during the pre-treatment period the trends of the treated and untreated subjects were approximately the same [15]. Nevertheless, it is advised to back up the claim of parallel trends by economical reasoning. The appropriateness of this Assumption for this thesis is evaluated in appendix **E**.

Another important assumption is that each individual can only exclusively belong to either the treatment or control group. This assumption implies that the outcome for members of the control group is not affected by treatment. Therefore, spillover effects are ruled out by this configuration of the DiD approach [38]. A recent methodological paper has addressed this issue [34] and developed a novel framework to relax this assumption by assuming it to be satisfied on an aggregate level and thus allow for spillover effects on the individual level. Another article proposed using a set of indicators for concentric rings around a region instead of a single indicator to determine whether two regions are neighbors or not [10]. It is, however, sufficient to include simple spatial lags to recover unbiased DiD estimates. The spillover effect then needs to be interpreted as a weighted average of all spillover effects that occur at different distances from the treated group.

Furthermore, it is assumed that there is no effect for the treatment group before treatment has been established. This means that no anticipatory effects are present. Although this assumption might be justified for the early stages of the pandemic where implementation of mobility restricting policies is novel for many individuals, it has to be questioned for later stages, especially when policies are reimplemented.

Given that these assumptions hold, the standard DiD model can be expressed as a two-way fixed effects regression [49].

$$V_{ijt} = \alpha_i + \alpha_j + \alpha_{ij} + \mu_t + P_{it}\gamma_1 + P_{jt}\gamma_2 + \epsilon_{ijt}$$

Where γ_1, γ_2 are the coefficients of interest since the treatment variables P_{it}, P_{jt} are coded as an interaction between time and treatment status.
To introduce this concept to the model of this thesis, fixed effects for each period t are denoted by μ_t. Fixed time effects μ_t will capture all underlying time variant heterogeneity common to region pairs

$$V_{ijt} = K_{s,it}^{\kappa_1} K_{s,jt}^{\kappa_2} \cdot exp[\gamma_1 P_{it} + \gamma_2 P_{jt} + \alpha_i + \alpha_j + \alpha_{ij} + \mu_t]\tilde{\epsilon}_{ijt} \tag{6}$$

For ease of notation denote

$$\lambda_{ijt} = K_{s,it}^{\kappa_1} K_{s,jt}^{\kappa_2}$$

and

$$f_{ijt} = \alpha_i + \alpha_j + \alpha_{ij} + \mu_t$$

such that the model becomes

$$V_{ijt} = \lambda_{ijt} \cdot exp[\gamma_1 P_{it} + \gamma_2 P_{jt} + f_{ijt}]\tilde{\epsilon}_{ijt} \tag{7}$$

Recent advances in the field of DiD literature have made it clear that if a policy is implemented in a staggered manner rather than simultaneously to all treated units, the simple DiD estimator can be severely biased. The bias occurs when the treatment effect varies over time. This is because the simple DiD estimator in the case of staggered implementation yields an estimate of the weighted average of the different comparisons between the early treated and untreated, the late treated and untreated, as well as the subjects early treated and late treated [3] [26]. Several methods have been proposed to deal with this model setup mainly relying on careful

[3] This is commonly referred to as the *Bacon decomposition*.

specification of reference groups [11, 57]. Others have suggested that two-way fixed effects regression can handle staggered implementation very well when specifying the model equation accordingly [63].

To recall the descriptive part concerning treatment variation, see [2]. For the treatments, the phase of implementation spans only one to two weeks. Since the pandemic situation was still not fully understood at this point in time, at least in terms of the population, assume that the treatment effect did not vary substantially during this rather short time period, and that anticipatory effects did not hinder the analysis. For this matter continuing with the classical DiD estimator seems plausible.

5.3 Cross Regressive Spatial Model

In order to account for spatial structure, a weight matrix W is introduced. In this study, the weights are set to one if the geographic centers of two regions are less than a threshold distance apart and zero else. The diagonal of this matrix is zero by convention [5], since no region is a neighbor to itself. Then this neighbor matrix is standardized by row to facilitate computation and ease interpretation [5]. In the base specification, the distance threshold is set to 90 minutes. This value appears to be rather arbitrary, but as shown by [40] the exact specification of the weight matrix is not too important for this kind of study. To include spillover effects in the model, a spatial cross-regressive model (SLX) is used. This type of model is among the simplest methods used to examine spatial dependencies, it requires no special estimation technique. The SLX model can be seen as a special case of more general spatial models, where spatial dependence for the dependent variable and the error term are allowed for. In contrast to those, the SLX approach only includes spatial effects for the independent variables [23].

Since the model discussed here is of a spatial origin-destination flow type, the weights need to be modified similarly as for the spatial lag model in [39]. To model spillovers from origin and destination specific variables respectively, two different weight matrices are needed.

$$W_o = W \otimes I_n$$
$$W_d = I_n \otimes W$$

Where I_n is an $n \times n$ identity matrix. Since it is assumed that the spatial weights remain constant over time, it is possible to compute for each period a single spatial lag vector of dimension $1 \times N$ and then stack these lag vectors together such that one ends up with a $1 \times (N \cdot T)$ spatial lag vector. The values in this spatial lag vector then depict a weighted average of the variable of interest for all neighbors of

the respective region.[4] To see why refer to appendix **D**.
Since the weights are row-standardized and the treatment variables are coded as zero - one dummies, the spatial lag vector will contain the fraction of neighbors of the respective region for which the treatment was active on that day.

For estimation of the spatial spillovers, simply add the spatial lags as additional regressors to the model equation. Therefore, let the superscript w denote the spatially lagged regressors and let λ_{ijt}^{w} contain additionally to the covariates in λ_{ijt} the spatially lagged covariates of interest.

$$V_{ijt} = \lambda_{ijt}^{w} \cdot exp[\gamma_1 P_{it} + \gamma_2 P_{jt} + \iota_1 P_{it}^{w} + \iota_2 P_{jt}^{w} + f_{ijt}]\tilde{\epsilon}_{ijt} \tag{8}$$

5.4 Pseudo-Poisson Maximum Likelihood (PPML)

Traditionally, the gravity model is estimated using OLS. To do so, the multiplicative model is linearized using the natural logarithm.

$$ln(V_{ijt}) = ln(\lambda_{ijt}^{w}) + \gamma_1 P_{it} + \gamma_2 P_{jt} + \iota_1 P_{it}^{w} + \iota_2 P_{jt}^{w} + f_{ijt} + ln(\tilde{\epsilon}_{ijt}) \tag{9}$$

However, this approach has several shortcomings, especially in the presence of zeros in the dependent variable. It is therefore state of the art to estimate gravity-type models using other methods than OLS. It has been shown that due to Jensen's inequality, the estimation of log linearized models, can lead to inconsistent estimates. Especially in case of heteroskedasticity, it is advised to use other approaches [52]. The PPML estimation offers a natural way to cope with zero values for the dependent variable since the model is estimated in its multiplicative form rather than in its log-linearized form. Simultaneously, the method is easy to implement and the interpretation of coefficients is straight forward. As for OLS, coefficients for regressors entered in logarithms can be interpreted as elasticities, coefficients for regressors entered in levels can be interpreted as semi-elasticities [62]. Using the quasi-maximum likelihood approach following [44] for this estimator to be consistent, the only thing needed is that the model is specified correctly [52]. The data does not have to be Poisson distributed or even be an integer. Further evidence for the adequacy of the PPML method, especially in the presence of zeros in the dependent variable, has been established by [51, 42].
To make the estimation of the model operational, it needs to be transformed back into the multiplicative form. To do so, simply apply the exponential function.

$$V_{ijt} = exp[ln(\lambda_{ijt}^{w}) + \gamma_1 P_{it} + \gamma_2 P_{jt} + \iota_1 P_{it}^{w} + \iota_2 P_{jt}^{w} + f_{ijt} + ln(\tilde{\epsilon}_{ijt})] \tag{10}$$

[4] Usually one would form a Kronecker product with an identity matrix of dimensions T to incorporate the time dimension [5]. Due to limited computational resources, the computation was chunked.

Note that just as for the interpretation of log-linear models the interpretation of semi-elasticities can be eased by approximations. In this thesis the vast majority of coefficients are comparatively small which is why the interpretations for these coefficients in chapter 6 are to be understood as approximations even if not explicitly stated.

In the case of pseudo-poisson maximum likelihood the inclusion of fixed effects for the origin and destination regions automatically implies additivity of the estimated mobility flows. This means that the sum of all estimated mobility flows does not exceed the sum of all observed mobility flows. This property is unique to the PPML estimation technique [21]. Since for this thesis information on both the inflows and outflows for each region is available and thus a doubly constrained gravity model which accounts for inward and outward multilateral resistances is appropriate, this property is desirable [3, 30]. The asymptotic consistency of the PPML estimator in the presence of fixed effects has been established by [66].

6 Regression Results

6.1 Base Models

6.1.1 Controls

The estimated coefficient for the logarithm of distance remains in line with the theory underlying the classical gravity models; see [F4]. Distance remains a highly significant predictor of reduced mobility between two counties. The estimates indicate that if the time needed to travel between two counties increases by 1%, the number of trips between these two counties that is observed is on average reduced by 2.8% to 3.8%. The first model seems to underestimate the effect of distance on mobility. The inclusion of fixed effects for the origin and destination helps to eliminate county-specific characteristics, which encourage mobility, thus leading to a stronger estimate for the deterrence effect of distance.

In addition to the logarithm of distance, the base model of gravity theory includes the logarithm of the masses for both the origin and destination of the interaction; see [F4]. Here, this mass enters in the form of the county population. Again, not too surprisingly, the coefficient is highly significant and hovers below but close to one. The estimate is slightly higher for the population in the origin. An increase in mobility between two counties by 0.94 to 0.95 percent on average if either in the origin or destination region the population increases by one percent can be observed. Compared to models that did not include any other covariates, the effects for distance and population are slightly elevated.

Moving beyond the traditional gravity model, continue to build the model by including additional time-invariant covariates[5]; see [F5,F6,F7,F8]. This way, evidence for the adequacy of the home office potential calculated by the IAB following the framework of [2] can be acquired.
It can be seen that for an increase in the share of industry that exhibits home-office capabilities at the origin or destination by 1%, mobility flows between these two counties are reduced by more than 8% on average. These estimates are highly significant and by far the strongest relationship uncovered by this regression.
Interestingly, the effect is slightly larger for changes in home-office capabilities in the destination region. This asymmetry could be due to large cities, where the potential for home-office is on average higher than in the rest of the country [2]. Generally speaking, large cities have large catch areas [17] and therefore more movement is occurring toward the cities than moving out from within the city. In addition, incentives to travel outward of cities are reduced, as cities are well equipped with a wide variety of infrastructure. Thus, for the case of metropolitan areas, a higher home

5 For a variable overview refer to Appendix **A**.

office potential and higher inward mobility occurs, which could be the reason for the higher estimates of home office potential in the destination region.

As mentioned in the literature overview, there is some evidence that partisanship might be related to government obedience. This is the reason why the model includes an indicator on whether the share of votes for the AfD in the state of the respective county is higher than the country average. The voting shares considered are from the 2017 national election. It is that the AfD is known for strictly opposing the positions of established parties, which makes it particularly suited to control for non-compliance during the Covid-19 pandemic. In a similar fashion, an indicator for government obedience using the election results of the ruling parties (CDU/SPD)is included.

For the simple model without fixed effects, these dummies reflect expectations. The coefficients remain highly significant and positive for all policies considered when looking at the disobedience dummy (AfD).
It can further be observed that the coefficient is consistently higher for the start region and for the policies M14 and M17 compared to M08 and M10. Therefore, if the share of votes for the AfD is above average in the origin region, the number of trips between origin and destination is on average significantly increased.
Although less pronounced, this effect also holds true when the share of votes for the AfD is above average in the destination region. The reason for the more pronounced effect induced by M14 and M17 could be that for these two policies it is easier to choose whether or not to follow them. Even if internal mobility restrictions are imposed, it is still quite unlikely that violations will be prosecuted.
M08 and M10 are legally binding and businesses are required to close.

Analogously, for the dummy measuring compliance (CDU / SPD), negative coefficients are estimated, implying a negative relationship between government approval and mobility. The reduction in mobility appears to be roughly twice as strong when the share of votes exceeds the average in the destination region compared to the same scenario in the origin region.
Furthermore, the reduction is more pronounced for M14 and M17, further illustrating the difference in compliance compared to the AfD voting parts of the population. This finding also substantiates the findings of previous studies that partisanship can be a meaningful proxy for government compliance.
Further interpretation seems a little far fetched for now since county-specific heterogeneity is not yet accounted for and thus the result could be simply due to eastern states having generally higher votes for government opposing parties. This discussion will be continued later when evaluating the interaction between (dis)obedience and

policy interventions.

To control for differences in the dynamics of the Covid-19 pandemic between counties, a variable is included that measures the incidence of Covid. In contrast to the previous features, this variable is time-variant and will thus be part of the model even after inclusion of fixed effects. The variable is incremented by one[6] and enters the model in terms of logs.
The coefficient for this feature remains highly significant and positive in the first model with only time fixed effects. This rather surprising relationship holds for all policies considered. However, when controlling for time-invariant county-specific aspects, the coefficients lose significance. Conversely, standard errors are reduced and significance is restored when in addition fixed effects for county pairs are considered. I then obtain small negative effects which are in absolute values smaller than 0.01% per one percent higher incidence. The effects are approximately equal when comparing the start region with the destination region.
It is now evident that high incidences are not the main driver of mobility reductions during the early stage of the Covid-19 pandemic in Germany.

Additionally all models include a spatial lag of Covid-19 incidence. Reporting and Interpretation of these spillover effects and the effects of incidence are delegated to the appendices **F** and **G**.

In the following the first working models of this thesis will be discussed. Using the DiD framework, I attempt to infer about the causal effects of policy changes on population mobility. First, consider a model that includes the covariates discussed above and fixed daily effects. Subsequently, fixed effects for the origin and destination region, as well as for county pairs, are included. In the last step, I will compare the results with the final model, where spatial spillover effects are considered.

6.1.2 Policy Effects

Looking at restrictions imposed on gastronomy, models 1 and 2 cannot identify significant effects on mobility. However, as soon as fixed effects for county pairs are included, the significance increases. The results imply that if a government imposes this policy on a county, the number of trips originating from this county is reduced by approximately 2.3% on average and the number of trips that have it as destination is reduced by approximately 3.5% on average.

[6] This is necessary since the PPML model takes logged variables as input. Alternatively, observations with zero value could be discarded, but this would lead to loss of information and potentially disrupt the time series.

Similarly, for restriction on nightlife activities the first two models fail to find significant effects. The third specification yields, at least for when the policy is active in the destination region, a significant coefficient. This effect is negative and approximately half the size of M08 (-1.62%).

In the first model specification rather large positive and significant coefficients for M14 are obtained (9.93% for origin, 15.89% for destination). For models 2 and 3 the coefficients remain insignificant and thus no causal effects of M14 on mobility can be observed.

By far the largest significant coefficients are obtained for M17 in the first model specification with only fixed time effects. The coefficient implies a change in mobility by -32.55% if the policy is active in the origin and -30.37% if it is active in the destination. However, these estimates seem to be severely downward biased due to unobserved confounders. Thus, the second model specification does not yield significant coefficients for M17. The coefficient obtained in the third model specification implies that the number of trips which have a county as destination which has active restrictions on workplaces is reduced by approximately 3.03% on average.

It appears that for most models the effect of policy intervention is more pronounced if the intervention occurs in the destination region. This result seems sensible for policies that impose restrictions on possible travel destinations only within the respective county. However, M14 explicitly contains limitations on crossing state borders and imposes maximum travel distances from the point of residence. This is why strong effects on the number of originating trips are expected, at least for M14. But it seems that the policy is not effective in reducing mobility.

6.1.3 Interactions With Partisanship

As mentioned before, I would like to infer about implications of partisanship on compliance regarding Covid-19 policies. This is why an interaction term between the DiD indicators and the dummies representing approval/disapproval of the governments decisions is included. Keep in mind that the measure used for partisanship is time constant and thus the coefficients estimated are not exactly reflecting the response to the policy implementations, but rather describe the general difference in mobility during the observed time frame compared to regions where the interaction dummy is zero.

Starting with measuring the effect of disapproval, it can be observed that for the interaction which includes M08, only in the third model significant coefficients are obtained. Here, the estimated effect for when the policy is active and the share of

AfD votes is above average in the destination region is positive and about twice as large (4.29%) as for the same situation in the origin region (2.09%). This result is in line with expectations. The policies implemented are aimed at reducing population mobility and the AfD is known for opposing policies advocated by the established parties. Thus, facing restrictions on gastronomy, counties that have a higher share of AfD voting inhabitants are likely to exhibit higher mobility compared to counties that have an average or below average share of AfD voting inhabitants. The higher coefficient for when the dummy is active in the destination region rather than in the origin region, again, could be attributed to the fact that the policy is targeted at destination sites in the respective country.
The results for the interaction with restrictions on nightlife activities are very similar.

Surprisingly, the coefficient in the first model specification for the interaction including M14 is highly significant and negative; however, when controlling for origin-destination fixed effects, the significance vanishes. Further, including county pair fixed effects the coefficient changes sign and becomes significantly positive. Here, the estimated effect appears to be approximately of equal size, regardless of whether the scenario is manifesting in the origin or destination region (2.91%,3.35%).

Looking at the interaction with M17, significant positive coefficients for the first model can be observed, which again become insignificant once origin-destination fixed effects are included and regain significance as county pair effects are added to the model. In the third specification, an effect of 1.24% on mobility is estimated for the origin region and, more than 4 times as large, 5.98% for the destination region. In summary, counties with an above average share of AfD voters tend to reduce mobility less compared to other regions when faced with restriction policies.

To complete the analysis, now look at the interaction between policies and the share of votes of the ruling parties.
For the interaction with M08 only the base model without fixed effects for counties or county pairs yields significant estimates, which are negative.

The situation is similar for the restrictions on nightlife activities, except that in the third model a small significant effect of 0.9% for when the dummy is active in the destination region can be estimated.

The first model specification using M14 as the policy variable only yields a significant estimate, which is negative for the case where the interaction dummy is active for the destination region. Inclusion of fixed effects for the origin and destination regions extinguishes the significance. When in addition controlling for origin-destination fixed effects, the sign of both coefficients switches and they become significant again.

Although the effects are small, it is still worth noting that for both the interaction with restrictions on nightlife activities as well as for those with restrictions on internal mobility, the number of trips to counties that have above average share of voters for the ruling parties is elevated compared to counties where they only got average or below average share of votes.

Interpretation of the effect of M17 is hardly possible since most coefficients regarding the policy-government interaction are dropped due to colinearity. Only the coefficient of the interaction in the destination region for the third model is not omitted. It is positive and statistically significant. By inspecting the results, it becomes clear that while AfD voting schemes work fine in modeling non-compliance, the voting pattern for the ruling parties seems to have unclear effects on compliance.

6.2 Single Policy Models

Finally arriving at the full model; see table 1. As mentioned earlier, the DiD approach rules out spillover effects; this is why it has to be controlled for in order to obtain unbiased estimates for the ATT. In this implementation, the value of the spatial lag for the policies is the fraction of neighbors that have implemented the same policy. The results confirm the importance of controlling for spatial effects.

6.2.1 Policy Effects

As soon as spatial lags for the policies are included, nearly all DiD estimates lose significance. Solely restrictions on workplaces (M17) appear to retain their causal effect on mobility of -1.39% in the origin and -2.22% in the destination region. For most DiD estimates, significance is 'sucked up' by the spatial lags. On the contrary, all coefficients that capture spillover effects, except one, are statistically significant.

6.2.2 Spatial Spillover Effects

The interpretation of the *origin*-based spillover effect is that if the *origin* county has only neighbors with the respective policy activated, then the number of trips *originating* in it changes by $\hat{\beta} \cdot 100\%$ on average. looking at the *destination*-based spatial lag, if the *destination* county is only neighbored by counties with the policy activated the number of trips directed *towards* it changes by $\hat{\beta} \cdot 100\%$ on average. In reality it is also possible that not all but some neighbors have implemented the policy. The county is then only partially exposed to the spatial lag and thus the coefficient could be multiplied with the fraction of neighbors which have activated the policy.

Regarding M08 I find that if all neighboring counties have M08 active, a reduction in the number of trips having this county as destination by 6.81% on average can be observed. The number of trips originating from this county is only reduced by 2.42%.

Turning towards M10, only the spillover effect for the destination region is significant. Thus, it can be observed that the spillover effect induces an average reduction in the number of trips going to this county by 2.37%.

Surprisingly, the data reveal a positive origin-based spillover effect for M14 indicating an increase of 1.63% in the number of trips originating from a county with only neighbors participating in the implementation of M14. The destination spillover effect is similar in size but negative.

For M17, the spillover effect for the destination region (4.07%) is more than three times as large as for the origin region (1.11%). These last positive coefficients are very interesting and thus require special attention.

Keep in mind that the policy M14 was mostly active for clustered regions with very high Covid incidence. It thus seems plausible that agents within these clusters substituted trips which would have had counties as a destination which are farther away than what restrictions allow, with trips towards other regions nearby including the domestic county. In other words, it is possible that mobility within these clusters of regions with M14 increased due to a reduction of trips leaving the cluster. Simultaneously, mobility going towards counties with many neighbors with M14 active is reduced.
This reduction may be partially due to fear of infection from people traveling from far away and partially due to less interaction with neighboring counties in response to the implications of the policies. The case of M17 is not easily entangled and may require further research. However, a possible explanation for the positive spillover effects could also be substitution effects. Firms could relocate their employees toward workplaces where restrictions are less severe or simply to reduce the number of people working on one site.

6.2.3 Interactions With Partisanship

The coefficients for the policy interactions with AfD and the ruling parties, in general, stay the same. The coefficient shrinks in size, but significance does not change when compared to the model specification without spillover effects. However, it should be noted that no government interaction effect could be estimated for M17 due to colinearity.

Table 1: Final Model With Spatial Component for M08 - M17

Dependent Variable:	Population mobility			
Policy:	(M08)	(M10)	(M14)	(M17)
Variables				
Policy_start	0.0033	0.0071	0.0012	-0.0137***
	(0.0063)	(0.0058)	(0.0058)	(0.0052)
Policy_end	-0.0094	-0.0045	-0.0003	-0.0222***
	(0.0062)	(0.0058)	(0.0058)	(0.0052)
Policy_start_W	-0.0242**	-0.0032	0.0163**	0.0111*
	(0.0122)	(0.0084)	(0.0066)	(0.0066)
Policy_end_W	-0.0681***	-0.0237***	-0.0131**	0.0407***
	(0.0125)	(0.0086	(0.0066)	(0.0066)
Policy_start ×AfD_start	0.0157***	0.0119**	0.0256***	0.0139**
	(0.0060)	(0.0060	(0.0072)	(0.0058)
Policy_end ×AfD_end	0.0431***	0.0376***	0.0271***	0.0319***
	(0.0060)	(0.0060)	(0.0072)	(0.0058)
Policy_start ×government_start	-0.0077	-0.0054	0.0148**	COLIN
	(0.0051)	(0.0051)	(0.0061)	
Policy_end ×government_end	0.0055	0.0085*	0.0146**	COLIN
	(0.0051)	(0.0051)	(0.0061)	
Fixed effects: time, origin, destination, origin-destination				
Observations	8,083,405	8,083,405	8,083,405	8,083,405
Squared Correlation	0.99675	0.99674	0.99669	0.99671

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

6.3 Effects on Short Versus Long Travel Distances

In this section the final model of the previous section is deployed onto two sub-samples; see [2]. First, the set of county pairs where the travel time does not exceed 120 minutes, and second, the set that contains only those pairs that are a minimum of 120 minutes driving time apart. Using these sub-samples, I aim to uncover hidden dynamics of the effects of the policy interventions on mobility patterns. Keep in mind that with a number of almost 6.9 million, most data entries are for long-distance trips compared to only 1.2 million observations for short-distance trips. At the same time, recall the descriptive analysis and look again at figure 3. The average number of total moves per data entry is much higher for intra-county connections, which are mostly part of the short-distance data set. In fact, when summing up the moves occurring in each of the data sets, only 57 million moves belong to the category of long-distance trips and over 7 billion movements belong to the short-distance data set. Therefore, even if stronger percentage changes for long-distance travel data are observed, absolute mobility changes are likely to be higher for short-distance travel.

6.3.1 Policy Effects

Starting with the effects of M08, it can be observed that in contrast to the results obtained using the full data set, significant estimates are obtained for the ATT on population mobility when using either of the sub-samples. The regression results suggest that when only considering short travel distances, the number of trips originating from a county with M08 being active is reduced by approximately 1.5%. The destination-based effect is slightly smaller. The effects are much more pronounced for trips that are longer than 120 minutes. For this category of trips, the implementation of M08 in a county appears to cause a reduction by 15% on average in the number of trips originating in this county. The number of long-distance trips directed towards this county is reduced by 22% on average.
These differences in effect size could be explained by the fact that M08 is aimed at mobility related to touristic and recreational purposes. It is likely that these kinds of trip are overrepresented in the long-distance data when compared to the original data set and thus a stronger reduction in those occurs after implementation.

Considering now the estimates for the ATT of M10 on population mobility, it immediately stands out that, in contrast to M08, no significant effect can be identified when only short-distance trips are considered. Nonetheless, the implementation of M10 exerts a causal effect on long-distance mobility directed at and originated from a county. The effect remains negative and is approximately the same size as the corresponding coefficients obtained for M08. Even more than M08 the restrictions on nightlife mainly focus on leisure-related mobility and thus a large decline for long-distance trips is observed.

When M14 is implemented in a county, the number of trips that exceed 120 minutes of driving and have this county as the destination is reduced by approximately 5.6% on average. The number of trips originating from this county is reduced by 7.9% on average. Compared to M08 and M10, much stronger effects of M14 on long trips are observed. It can be ssen that M14 reliably reduces long-distance mobility, but for short distances, no significant effect is observed.
This seems plausible since mobility within a short distance from home is not targeted by M14. However, as mentioned in the literature review, it is not only of interest if mobility is reduced but also what kind of mobility is reduced. Since M14 is

not targeted at specific destinations, it may not be as effective as other policies in preventing infections, even if mobility is reduced [6].

The ATT of M17 on population mobility is highly significant for both samples; however, the effect size varies substantially. The number of trips originating from a county that has M17 active is on average reduced by approximately 4% when considering short distances. The effect on long trips is nearly four times as large. The average reduction of trips that have this county as destination is approximately 2% for short trips and 2.4% for long trips, respectively. Compared to the original estimates, most of these coefficients are magnitudes larger, only the coefficient obtained for the destination-based effect on short trips is very close to the original estimate. This reduction in long-distance mobility indicates the potential to reduce work-related traffic by making use of home-office regulations. However, this is only based on the premise that business continues even if workers do not commute these long distances and instead work from home. It is reassuring to find these results since workplace restrictions such as home-office obligations are repeatedly discussed, especially in the context of reducing CO_2 emissions from commuting and saving fuel, in order to carry through with sanctions imposed on Russian exporters as a response to the invasion of Ukraine in 2022.

6.3.2 Spatial Spillover Effects

While keeping in mind that in the short-distance data set, a large portion of observations are neighbor-to-neighbor connections, now moving on to the spatial effects. Concerning M08, it can be seen that for short distances there is still a significant negative estimate. For any county that only has neighbors that have M08 active, the destination-based spillover effect amounts to a reduction in mobility by 3.6% while the origin-based spillover effect reduced the number of trips originating from it by 5.4% on average. The origin-based spillover effect is insignificant for long-duration trips. However, the destination-based spillover on long-distance trips is approximately -12.55% on average.
Since many observations in the short-distance data are movements between neighbors, it makes sense that a high share of neighbors which have implemented the policy translates to a lower level of interaction between them, assuming that the policy reduces travel on its own. On the other hand, people who decide to travel to a county from a farther distance seem to factor in the state of implementation of the surrounding area as well.

For short distances, no significant spillover effects of M10 could be recovered. This result is rather surprising since one would expect that people reduce leisure-related trips if surrounding counties have restrictions on nightlife activities. However, the

destination-based spillover effect for long trips is highly significant and approximately three times larger than the estimate under the whole sample. On average, the number of long-distance trips going towards a county is decreased by approximately 7.4% if all neighbors have activated M10.
These findings on M08 and M10 are particularly interesting for county administration since obviously these two policies significantly impact touristic travel even if the policy is not implemented in the respective county.

Short-distance trips are not affected by the spillover effects of M14. However, the coefficients estimated for the long-distance data set are highly significant, relatively large and negative. The average spillover effects amount to -9.2% in the origin and almost twice the size, -17.57%, in the destination region.

The estimates show that on average the number of long trips originating from a county increases by approximately 21.07% if all neighbors have activated M17. The number of trips directed towards it is increased by 30.45% on average. In contrast, the estimated spillover effects for short distances stay close to the original estimates. It seems that a high share of neighbors with M17 active implies stronger interactions with destinations that are farther away.
This finding might be due to substitution effects, where business-related trips which were directed to the surrounding area are reoriented at counties which are nearby. On the flip side, people from within a county avoid going to neighbors which have implemented M17 and instead stay either within their county or seek destinations far away where restrictions are less severe.

6.3.3 Interactions With Partisanship

When comparing the results of the short-distance sub-sample with the original estimates obtained in the joint sample, the coefficients for the interaction between M08 and the AfD dummy remain significant, but are slightly elevated. Longer trips are much more affected by this interaction. If the interaction dummy is active for a county, then on average the number of long-distance trips originating from it increases by 23%. The number of trips directed towards this county increases by as much as 33% compared to regions where the interaction does not hold.

For long trips, it can be observed that the effect size of the interaction with M10 is approximately the same as for M08. For short trips, the effect does not differ much from the original estimates, which are based on the full data set.

As for previous policies, the interaction effect of M14 with the AfD dummy is many times stronger when only looking at long-distance trips. A significant increase of approximately 2% on average in both the number of trips that go to and those that originate from a county where the interaction dummy is active can be observed. Therefore, this effect is approximately eight times more pronounced than those obtained using the whole sample. In contrast, the estimates for short-distance trips differ only marginally from the original estimates.

It is observed that if the interaction with M17 is active in the origin region, then the number of short trips increases approximately by 4.7% on average when compared to regions where the dummy is inactive. This is more than three times the original estimate. The same scenario does not produce a significant estimate for a longer trip duration. If the dummy is active in the destination, a relatively large increase in the number of long trips directed toward it (11%) occurs. The destination-based effect on short trips is roughly the same as for the whole sample.

While for M08 no significant effect on mobility for an above average share of voters for the ruling parties was evident in the full data set, for both sub-samples a significant positive coefficient is estimated. The effect size remains well below that for the AfD interaction and the familiar pattern of stronger effects on long-duration trips when compared to shorter trips remains visible. This result suggests that both an above average approval and an above average disapproval of the government are associated with a higher level of mobility compared to other regions.
This result might also be a hint that the government dummy does not always work as intended. It could be that the rather large time discrepancy between the election results used here from 2017 and the pandemic in 2020 is causing the dummy to become watery. In addition, the pandemic can be viewed as a "black swan event". That is, it was not anticipated in 2017 and people did not factor in the suitability of parties to cope with a major health crisis when deciding who to give their vote. The dummy measuring non-compliance does not suffer from this watering-down effect since the AfD is still an opposing party and people who vote for them believe that they would do a better job than the established parties without practical evidence for this belief.

The estimates for the interaction of M10 and the government dummy are also very similar to those obtained for M08. Since M08 and M10 are relatively similar in terms of what type of mobility is restricted, the same arguments as for M08 apply.

For M14 the interaction with the government dummy yields similar results as for the interaction with the AfD dummy. The coefficient remains positive, indicating that an above average share of votes for the ruling parties in combination with M14 is associated with greater mobility compared to other counties. The number of long-distance trips originating from a county increases on average by approximately 7.4%

compared to regions where the dummy is not active. The other coefficients for long- and short-distance trips are rather similar to the original estimates.
Again, disobedience can be captured well by using the AfD dummy interaction, but measuring obedience using the government dummy fails to find a stronger adherence to restriction policies when compared to counties with supposedly lower levels of government approval.

Due to collinearity, it is not possible to obtain significant estimates for the interaction term of M17 with the government dummy.

Table 2: Final Model With Different Trip Distances

Dependent Variable:	Population mobility							
Policy:	M08		M10		M14		M17	
Distance:	< 2 h	>2 h	< 2 h	>2 h	< 2 h	>2 h	< 2 h	>2 h
Variables								
Policy_start	-0.0151***	-0.1515***	-0.0034	-0.1547***	0.0011	0.0563***	-0.0437**	-0.1620***
	(0.0056)	(0.0081)	(0.0051)	(0.0078)	(0.0064)	(0.0067)	(0.0183)	(0.0085)
Policy_end	-0.0145***	-0.2244***	-0.0038	-0.2182***	-0.0007	0.0794***	-0.0200***	-0.2376***
	(0.0055)	(0.0100)	(0.0051)	(0.0095)	(0.0064)	(0.0066)	(0.0054)	(0.0080)
Policy_start_W	-0.0540***	0.0084	-0.0194	0.0112	0.0036	-0.0920***	0.0156*	0.2107***
	(0.0193)	(0.0085)	(0.0128)	(0.0075)	(0.0084)	(0.0083)	(0.0086)	(0.0100)
Policy_end_W	-0.0361*	-0.1255***	-0.0058	-0.0738***	-0.0004	-0.1757***	0.0360***	0.3045***
	(0.0196)	(0.0090)	(0.0129)	(0.0078)	(0.0084)	(0.0083)	(0.0087)	(0.0094)
Policy_start ×AfD_start	0.0308***	0.2293***	0.0197***	0.2304***	0.0243***	0.1990***	0.0465**	-0.0002
	(0.0058)	(0.0080)	(0.0058)	(0.0080)	(0.0082)	(0.0089)	(0.0208)	(0.0079)
Policy_end ×AfD_end	0.0523***	0.3326***	0.0401***	0.3281***	0.0277***	0.2086***	0.0287***	0.1107***
	(0.0058)	(0.0090)	(0.0058)	(0.0090)	(0.0082)	(0.0095)	(0.0066)	(0.0076)
Policy_start ×government_start	0.0177***	0.1544***	0.0091**	0.1540***	0.0132*	0.0741***	0.0276	COLIN
	(0.0046)	(0.0061)	(0.0046)	(0.0061)	(0.0068)	(0.0062)	(0.0212)	

Dependent Variable:	Population mobility							
Policy:	M08		M10		M14		M17	
Distance:	< 2 h	>2 h	< 2 h	>2 h	< 2 h	>2 h	< 2 h	>2 h
Policy_end ×government_end	0.0191***	0.2245***	0.0111**	0.2290***	0.0156**	0.0142**	COLIN	COLIN
	(0.0046)	(0.0074)	(0.0046)	(0.0074)	(0.0068)	(0.0062)		
Fixed effects: time, origin, destination, origin-destination								
Observations	1,196,360	6,885,395	1,196,360	6,885,395	1,196,360	6,885,395	1,196,360	6,885,395
Squared Correlation	0.99687	0.84078	0.99679	0.84079	0.99668	0.82195	0.99671	0.82152

Heteroskedasticity-robust standard-errors in parentheses

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

6.4 Combined Model

Now I will evaluate the regression results for the model which is identical to the full model from before with the exception that now all four policies and interactions thereof are controlled for; see [3] for only policy related variables and [F9] for all estimates. The bias from the unobserved effects of policies that were active simultaneously can therefore be, at least partially, eliminated. For example, it could be that before positive coefficients for a policy were found, but since no other policies were included, these coefficient estimates were caused by one or multiple other policies which where not accounted for at that moment.

6.4.1 Policy Effects

For the model that only includes M08 as a policy variable, no significant ATT could be found. However, this new estimate suggests that the implementation of M08 in a county reduces the number of trips directed toward it by 1.64%.

With a value of 0.8%, the destination-based ATT of M10 becomes significant and positive. Therefore, it is implied that the implementation of M10 in a county leads to a significant increase in the number of trips toward it. Since M08 and M10 are both policies which were often activated close in time, it seems plausible to think that the insignificant estimates for M08 and M10 during the single-policy estimations were caused by counteracting effects of those policies such that the net effects become insignificant.
Similar to the single-policy model, the ATT of M14 remains insignificant.

For this extended model, the origin-based ATT of M17 loses significance and the destination-based ATT shrinks in size from -2.22% to -1.59%. Apparently, the ATTs of M17 were overestimated in the single policy estimation.

In summary, it can be concluded that M08 and M17 were effective in reducing the interaction between counties. In contrast, M10 appears to slightly increase mobility. This seems rather counter-intuitive but might be explained by a psychological effect, which leads people to perceive counties with closed night-time activities as more secure and thus a more desirable destination compared to counties without these safety measures. Restrictions on internal mobility appear to have no meaningful effect on population mobility, at least not during the time frame of this study.

6.4.2 Spatial Spillover Effects

Moving on to spatial spillover effects, the coefficient for M08 remains highly significant and negative for both the origin and destination regions. Compared to the single policy model, the coefficient increases substantially from 2.42% to 3.92% for the origin-based effect and from 3.92% to 10.11% for the destination-based effect.

When using the single-policy model, the destination-based spillover effect of M10 was negative and the origin-based effect remained insignificant. Now, two significant positive estimates are obtained. With 3.38% the destination-based spillover is slightly larger than the origin-based spillover with 2.14%.
Again, it might be possible that the effects of M08 and M10 cancel each other out and the estimates from the single policy regressions might thus have been biased.

Similar to the single-policy model, the origin-based spillover effect of M14 is positive, while the destination-based effect is negative. The size of both coefficients is slightly increased in the model that includes all policies. The positive coefficient for M14 spillovers implies counterproductive effects of M14. While a neighborhood full of counties with restrictions on internal movement reduces the number of trips going towards a region, it also increases the number of trips originating from it.
One could imagine scenarios where a person resides in a county without M14 but works in a county with this restriction. Some people could use secondary accommodations in counties with active M14 to continue working and avoid loss of income.

For M17, the origin-based spillover effect loses significance, the destination-based effect doubles in size but changes sign and is now positive (2.78%). It can thus be seen that mobility going towards a country is slightly increased if all neighboring counties impose restrictions of workplaces on their citizens.
Again, this could be caused by the fact that the respective county becomes relatively more attractive to firms with multiple locations, which then move employees to counties with less severe restrictions when compared to its neighbors. This provides further evidence for the need to consider spatial spillover effects when faced with human mobility issues.

6.4.3 Interactions With Partisanship

Next take a look at the interaction effects between the policies and the dummy indicating an above average share of AfD voting inhabitants. The destination-based interaction effect of M08 loses significance. The origin-based effect remains positive, but is slightly smaller compared to the single-policy model.

No significant estimates of the interaction effect with M10 are obtained from the regression that includes all policy variables.

When looking at the interaction effects of M14 it becomes clear that the destination-based effect loses significance. Only the origin-based effect is significantly positive and slightly smaller compared to the estimate from the single-policy model.

In contrast, for M17, only the destination-based effect remains significantly positive. The estimate implies that counties with activated M17 and an above-average share of AfD voters exhibit on average a by 1.22% elevated number of trips going toward them. This effect is roughly half of that of the single-policy model.
It seems that when other policies are controlled for, the estimated effect of AfD partisanship on compliance shrinks consistently for all policies and many estimates lose significance. However, there is still some evidence that regions with an above average share of AfD voters are on average less likely to reduce mobility as strongly as other regions when faced with restrictive policies.

Concerning the interaction effect between M08 and the dummy indicating the above average share of votes for the ruling parties, just as for the single policy model, no significant effect is recovered.

Surprisingly, there are now not only for M17 but also for M10 issues of colinearity when working with its interaction effects. Although small positive effects were obtained in the single-policy model, no coefficient for the interaction effect of M10 could be estimated for the final model specification, which includes all policies.

Moving on, it becomes evident that if a county has an above average share of votes for the ruling parties and M14 is activated, then on average mobility increases by 2.7% when going outward and by 1.61% when going inward. It seems that the government dummy only uncovers some of the dynamics regarding restrictions on internal movement.

Similarly to the single-policy model, no estimates can be obtained for the interaction of M17 due to colinearity. Again, the effects of this interaction are not in line with expectations when trying to model government obedience.

Table 3: Regression Results for All Policies Simultaneously

Dependent Variable:	Population mobility	
Model:	(1)	(2)
Variables		
M08_start	0.0149***	0.0005
	(0.0048)	(0.0073)

Dependent Variable:	Population mobility	
Model:	(1)	(2)
M08_end	-0.0495***	-0.0164**
	(0.0065)	(0.0072)
M10_start	-0.0330***	0.0044
	(0.0052)	(0.0037)
M10_end	0.0190***	0.0080**
	(0.0033)	(0.0037)
M14_start	-0.0078	-0.0080
	(0.0065)	(0.0071)
M14_end	-0.0051	0.0027
	(0.0065)	(0.0071)
M17_start	0.0009	-0.0082
	(0.0039)	(0.0053)
M17_end	0.0005	-0.0159***
	(0.0039)	(0.0053)
M08_start_W		-0.0392***
		(0.0132)
M08_end_W		-0.1011***
		(0.0134)
M10_start_W		0.0214***
		(0.0074)
M10_end_W		0.0338***
		(0.0074)
M14_start_W		0.0223***
		(0.0066)
M14_end_W		-0.0207***
		(0.0066)
M17_start_W		0.0032
		(0.0067)
M17_end_W		0.0278***
		(0.0067)
Fixed effects: time, origin, destination, origin-destination		
Observations	8,083,405	8,083,405
Squared Correlation	0.99667	0.99675

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

6.5 Effects on Short Versus Long Travel Distances for the Combined Model

Moving on, the same differentiation between long and short distances is made for the model including all policy variables; see [4] for a table of only the policy related variables and [F10,F11] for a table containing all estimates.

6.5.1 Policy Effects

While the origin-based ATT of M08 was insignificant for the whole sample, now a 7.45% reduction in short trips originating in a county with activated M08 can be observed. The destination-based effect is almost identical. For the model without spatial effects, the origin-based ATT of M08 on long trips is significantly positive but becomes negative once spatial effects are included. Therefore, the implementation of M08 reduces the number of long trips originating in a county by 4.8% on average. However, this effect is dwarfed by an average reduction in long trips to this county of 33.62% when M08 is activated.

It becomes evident that the estimated coefficients are much more pronounced for the sub-sets than for the full data set. The most intriguing thing is the strong reduction in long-distance trips. These results have significant implications for tourist activities in regions with restrictions on gastronomy. When a county imposes M08 on its citizens, then the number of trips from far away is reduced by more than a third compared to counties which have not implemented it. This leaves two things to consider.

First, M08 seems to be a great policy to reduce interaction between counties which are far apart, and thus nation-wide implementation of it can help very effectively to lessen the spread of Covid 19 across the country. Second, the implementation of M08 obviously comes at the cost that touristic businesses are closed and people working in this sector may suffer losses of substantial parts of their income. Businesses without enough reserves may even have to close permanently. Another group of people who suffer are young people in education who rely, for example, on low-wage jobs in gastronomy to finance their studies. Other industries that rely on traveling guests may be damaged indirectly through this policy.

Compared to the original estimates using all data points, the estimated ATTs of M10 on short-distance trips are greatly increased. Both the origin-based effect and the destination-based effect imply an increase in the number of short trips of approximately 6% on average compared to counties where M10 is not active. Moving on to the estimation results based only on long-duration trips, the origin-based ATT

switches sign and becomes strongly negative (-18.45%). The destination-based effect on long-duration trips remains positive, but small (2.52%).
The positive estimates for the effect of M10 on mobility may be due to a feeling of security for residents if restrictions on nightlife activities are in place. At the same time, people in counties with activated M10 might be cautious to travel far away, since they have already been sensitized to the dangers of Covid-19. This would explain the steep decline in long-distance traveling originating from counties which have activated M10.

Using only short-distance trips, no significant estimates for the variables related to M14 could be found. The ATT on long-duration mobility, however, is significant for both the origin and the destination region. Surprisingly, if a county has implemented M14, the number of long trips originating there increases by 27.04%, while the destination-based effect is even greater, with 37.38%. Thus, restrictions on internal mobility are not effective in reducing short-distance mobility. Instead, a steep incline in the number of long-distance trips to and from counties with active M14 can be observed.
This result is rather interesting since M14 is the only variable directly targeted at mobility and legal sanctions are to be expected when violating the policy.

Concerning the ATT of M17 on short trips, it can be observed that the destination-based effect is almost identical to that of the full sample estimation. However, the origin-based effect gains significance and is negative (-1.26%). For long trips, the origin-based ATT is -5.96%. The destination-based effect (-13.33%) is many times stronger than the original estimation (-1.59%). Long trips seem to be greatly reduced when workplace restrictions are imposed.
This finding seems sensible and provides further evidence for the suitability of workplace regulations, such as mandatory home office, to reduce commute mobility.

6.5.2 Spatial Spillover Effects

Compared to the estimates obtained using the whole sample, the coefficient for the origin-based spillover effect of M08 is associated with a stronger decrease in the number of short trips (- 4.88%). The destination-based effect is drastically reduced from -10.11% to -4.1%. Interestingly, when only long-duration trips are considered, the origin-based spillover effect loses significance. The destination-based estimate of the spillover effect becomes even more negative and changes to -14.31% when only considering trips that are longer than 120 minutes.

No significant effects for the spillover effects of M10 are found. Neither for short nor long trips.

Although in the original estimation the spatial spillover effects based on the origin of M14 were positive, the estimated average effect on long trips is now -9.88% for the origin-based spillover and -19.34% for the destination-based spillover.

The estimated spillover effects on short-distance travel of M17 remain fairly close to those of the full sample estimation. In contrast, the estimated destination-based spillover of M17 on long-distance trips (15.42%) is much larger than for the estimation using the whole data set (2.78%). The origin-based effect gains significance and is approximately half the size of the destination-based effect.
For this final iteration of the regression model, some new dynamics are uncovered. While restrictions on gastronomy continue to exhibit large negative spillover effects, it becomes evident that predominantly long-distance trips going towards regions with many counties that have activated M08 are reduced. Highlighting yet again the relevance and effectiveness of this policy.
Furthermore, it seems that the spillover effect of restrictions on internal movement has changed. Now, at least long-distance trips are reduced if a region has a neighborhood full of counties with restrictions on internal movement. In this regard, restrictions on internal movement may be suited to dampen the spread of Covid-19 across long distances. The number of long-distance trips going towards regions which are surrounded by counties with restrictions on workplaces is strongly increased. Again, it might be possible that firms relocate workplaces if possible to avoid strict regulations.

6.5.3 Interactions With Partisanship

Now consider the estimates for the interaction between the policies and the dummy indicating the above average share of AfD voters.
Starting off with M08 it becomes visible that, in contrast to full sample estimation, both coefficients remain significant when only analyzing short-duration trips. The estimated effect of the origin-based interaction suggests an average increase in mobility of 7.25% compared to counties which have not activated the policy M08 or where the share of AFD votes is below average. The destination-based effect of 9.41% is even stronger. For long-duration trips, the interaction with AfD voting patterns yields very high estimates. While the origin-based estimate implies an average increase in mobility by 14%, the average effect suggested by the destination-based estimate is a 46.67% increase in mobility.

Although for M10 the interaction effect in the original estimation was only positively significant for the origin, for short distances the effect is now highly significant for both the origin and destination. For short distances, the effect of the origin-based interaction amounts to a mobility reduction by almost 5% on average. When looking at the estimates for long-duration trips, another detail of the heterogeneity among the policies is uncovered. The destination-based interaction effect using AfD voting patterns remains insignificant; however, a large positive estimate is obtained for the origin-based effect (23.11%).

As mentioned above, the M14 related variables were not significant for the short-distance data set. However, for long trips, the interaction effect with AfD voting patterns is reversed compared to the original estimation. On average, long-distance trips are reduced by 9.84% or 17.16% respectively if the interaction holds true either in the origin or the destination region.

Considering the interactions of M17, it can be observed that for short distances, no significant interaction is found for AfD. The number of long trips originating in a county where the interaction is true is reduced by 10.05% on average. The destination-based effect is fairly similar to the one obtained in the original estimation.
For the single policy estimations so far, mostly positive coefficients for this interaction were found. Now, when all policies are included at once, the effect is not as clear-cut as it appeared before. Only the coefficient of the interaction with restrictions on gastronomy is positive for both short and long trips. It becomes clear that partisanship can have very strong implications for the adherence to some policy interventions. However, the response to policies is highly dependent on the type of policy. Although some policies are rejected by the population and mobility is almost 50% above the level of the reference group, other policies seem to be followed without resistance such that regions with an above average share of AfD votes exhibit 9% to 17% fewer trips compared to the reference group.

While the full sample did not reveal any significant estimates for the interaction effect of voting in favor of the ruling parties and the implementation of M08, short trips are increased almost as strong as when the interaction with the AfD dummy holds. Similarly, the coefficients for the long-distance data remain below but fairly close to those of the AfD interaction.

For M10, a similar picture emerges. In general, the effects on short-distance mobility remain close to those of the AfD interaction, albeit somewhat stronger. Again issues of colinearity with this policy persist; only the origin-based interaction effect can be estimated. This estimated effect amounts to 18.63% and is thus slightly smaller than the corresponding coefficient for the interaction with the AfD dummy.

The only negative estimates for the effect of the interaction with the government dummy on long-duration trips can be found for M14. If the interaction holds in a county, then on average the incoming mobility is reduced by 26.57% and the outgoing mobility is decreased by 14.02%.

M17 does not produce estimates for this coefficient due to colinearity.
Overall the results for the interaction with votes for the ruling parties are similar to those for the interaction with AFD votes. The response towards policies seems to be dependent on partisanship, but also on the type of policies. While for most policy-government interactions, the coefficient remains similar to those of the policy-AfD interaction, the results suggest that people in counties with above average share of voters for the ruling parties seem to follow restrictions on internal movement much more strictly when compared to the reference group.

Table 4: Final Model With All Policies and Different Trip Distances

Dependent Variable:	Population mobility		
Model:	Short-distance	Long-distance	Full data
Variables			
M08_start	-0.0745***	-0.0480***	0.0005
	(0.0150)	(0.0181)	(0.0073)
M08_end	-0.0714***	-0.3362***	-0.0164**
	(0.0150)	(0.0134)	(0.0072)
M10_start	0.0629***	-0.1845***	0.0044
	(0.0143)	(0.0186)	(0.0037)
M10_end	0.0603***	0.0252***	0.0080**
	(0.0143)	(0.0062)	(0.0037)
M14_start	0.0020	0.2702***	-0.0080
	(0.0064)	(0.0097)	(0.0071)
M14_end	0.0020	0.3738***	0.0027
	(0.0064)	(0.0111)	(0.0071)
M17_start	-0.0126**	-0.0596***	0.0128
	(0.0057)	(0.0089)	(109.0)
M17_end	-0.0157***	-0.1333***	-0.0159***
	(0.0057)	(0.0084)	(0.0053)
M08_start_W	-0.0488**	-0.0097	-0.0392***
	(0.0209)	(0.0107)	(0.0132)
M08_end_W	-0.0410*	-0.1431***	-0.1011***
	(0.0210)	(0.0115)	(0.0134)
M10_start_W	-3.44×10^{-5}	0.0078	0.0214***
	(0.0104)	(0.0090)	(0.0074)

Dependent Variable:	Population mobility		
Model:	Short-distance	Long-distance	Full data
M10_end_W	0.0092	0.0021	0.0338***
	(0.0104)	(0.0095)	(0.0074)
M14_start_W	0.0085	-0.0988***	0.0223***
	(0.0085)	(0.0079)	(0.0066)
M14_end_W	-0.0025	-0.1943***	-0.0207***
	(0.0085)	(0.0080)	(0.0066)
M17_start_W	0.0045	0.0814***	0.0032
	(0.0088)	(0.0104)	(0.0067)
M17_end_W	0.0252***	0.1542***	0.0278***
	(0.0088)	(0.0097)	(0.0067)
Fixed effects: time, origin, destination, origin-destination			
Observations	1,196,360	6,885,395	8,083,405
Squared Correlation	0.99694	0.84703	0.99675

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

7 Conclusion

The reason for this analysis was to evaluate the effectiveness of selected NPIs to reduce mobility in Germany during the early phase of the pandemic and to capture potential spatial spillover effects of these policies. To do so a gravitational model of mobility was built using Fixed Effects. Enhancing this model with spatial lags for the different policies and then proceeding with Difference-in-Difference methods leads to the desired estimator.

The first part, where the policies were included in the regression one by one, provided strong evidence for the presence of spatial spillover effects. However, the direct effect of the implementation of policies on the level of mobility in counties was rendered insignificant for all policies except for restrictions on workplaces. Surprisingly, the estimates for the spillover effects of restrictions on workplaces and internal mobility were positive.

When differentiating between short and long travel distances, several DiD estimates for different policies gained significance. Especially for long-distance trips, the analysis yields very strong significant estimates for all four policies. With the exception of restrictions on internal movement, all ATTs were negative. The strongest impact on long-distance trips was found for restrictions on workplaces in the destination region where a reduction in mobility by more than 23 percent was observed. The short-distance data set revealed significant negative estimates for restrictions on gastronomy and for restrictions on workplaces. However, the effects remain well below the corresponding estimates for the long-distance data set. Compared to the first part, some spillover effects become insignificant. The estimated spillover of restrictions on internal movement becomes negative.

The next step of the analysis included the four policies and their interactions simultaneously. This configuration allowed for more precise estimates since potential effects of competing policies are no longer part of the remainder and instead are factored into the estimation.
Although all origin-based DiD estimates lost significance, restrictions on gastronomy and workplaces in the destination county decreased the number of trips to this destination by roughly 1.6%. Restrictions on nightlife activities at the destination slightly increased the number of trips to there. Only for restrictions on gastronomy, both origin- and destination-based spillover effects were strongly negative. Although most estimates of spillover effects remained significant, many estimates become positive, implying mobility encouraging effects on neighboring counties. These positive spillover effects, however, remained comparatively small.

Finally, when the travel distance is again differentiated between short- and long-distance travel, the pattern of strong estimates for the long-distance data set continued. Restrictions on gastronomy have a negative impact on both the number of short- and long-distance trips going towards or coming from counties where they are active. Restrictions on nightlife have positive effects on short-distance travel, but the number of trips originating from counties with the activated policy is heavily reduced. Restrictions on internal movement were shown to be ineffective since no significant reduction in short-distance travel could be observed. At the same time, long-distance interactions with counties with this policy in place are strongly increased. Workplace restrictions are found to be an effective tool to reduce both short- and long-distance travel.

In addition to these primary results, it was found that partisanship has a significant influence on the reduction of mobility in response to different restriction policies. Although the expectation that an above average share of votes for AfD is associated with less mobility reduction compared to other regions has been supported, it could not be found that regions with an above average share of votes for the ruling parties coincide with stronger adherence to mobility restricting policies. Instead, regions with strong election results for the ruling parties were found to reduce their mobility level less than other regions.

Further evidence for the importance of spillover effects when working with Difference-in-Difference studies has been established. Especially for the case where spatial interactions are modeled, it is not advisable to neglect these spillover effects. Furthermore, this study joins the existing literature and provides evidence that during the early stage of the Covid-19 pandemic NPIs, some better than others, can be effective tools to reduce population mobility. Although the approach followed by this thesis is well suited for the task at hand, multiple shortcomings need to be pointed out.

The most dominant flaw in this thesis, which could not be solved due to resource limitations, is that a simple DiD was used without considering the implementation of the policy in stages. While it seems plausible to assume constant treatment effects, this assumption remains somewhat speculative and I would highly recommend following one of the approaches[7] to deal with this kind of problem. This is especially advisable when the time frame of this study is extended in order to evaluate other policy implementations and/or to examine possible re-implementations.
Further research should then also address the assumption of no anticipatory effects. From an econometric point of view, it would also be interesting to control for spillover effects in the dependent variable and thus further refine the estimates precision. Fur-

[7] For example the Goodman-Bacon decomposition.

thermore, I would recommend to use time-varying data on election results. Monthly or weekly surveys conducted by polling institutes might serve this purpose well.
It could also be useful to experiment with different indicators to gauge the dynamics of the pandemic. For example, one could include the death incidence related to Covid-19 or hospitalization rate in the analysis.

Other less severe shortcomings are that the decisions to differentiate between short- and long-distance travel, as well as between neighbors and not neighbors, were rather arbitrary. Also, the intra-county travel distance could only be estimated.
One shortcoming that is not easily solved is that the data set provided by Teralytics does not allow to differentiate between socioeconomic groups. Simultaneously, the data would allow to focus on different modes of transport and thus refine the findings on policy implications for population mobility.

These shortcomings could potentially bias the results obtained in this study, and therefore, I highly recommend identifying the most serious problems and dealing with them in some way before advising political decision makers.

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Appendices

A Variable Overview

8

Table A1: Variable Overview

Population mobility	Number of trips between two counties as detected by Teralytics data collection for every day from March 7th 2020 to April 30th 2020. Intra-county movements are included
Distance	Distance between two counties in minutes needed to drive by car. For intra-county distance the approximation mentioned above is used.
Population	Population count for each county from 31.12.2020.
share_homeoffice_WZ	Home office potential as share of industry with home office capabilities. Calculation provided by the IAB following [2].
government	Indicator whether the combined share of votes of CDU/CSU and SPD from the Bundestagswahl 2017 in the state of the respective county exceeds the federal average.
AfD	Indicator whether the share of votes of the AfD from the Bundestagswahl 2017 in the state of the respective county exceeds the federal average.
log(incidence)	Logarithm naturalis applied to daily data on Covid-19 seven day incidence per 100K inhabitants at the level of counties.
log(incidence)_W	Spatial lag of log(incidence).
M08	Daily data on the status of restrictions on gastronomy at the county level.
M10	Daily data on the status of restrictions on nightlife activities at the county level.
M14	Daily data on the status of restrictions of internal movement at the county level.
M17	Daily data on the status of restrictions on workplaces at the county level.
M08_W	Spatial lag to the respective policy.
M10_W	Spatial lag to the respective policy.
M14_W	Spatial lag to the respective policy.
M17_W	Spatial lag to the respective policy.

8 In order to avoid infinite values each entry for the incidence was incremented by one before applying the logarithm.

B Implementation Dates

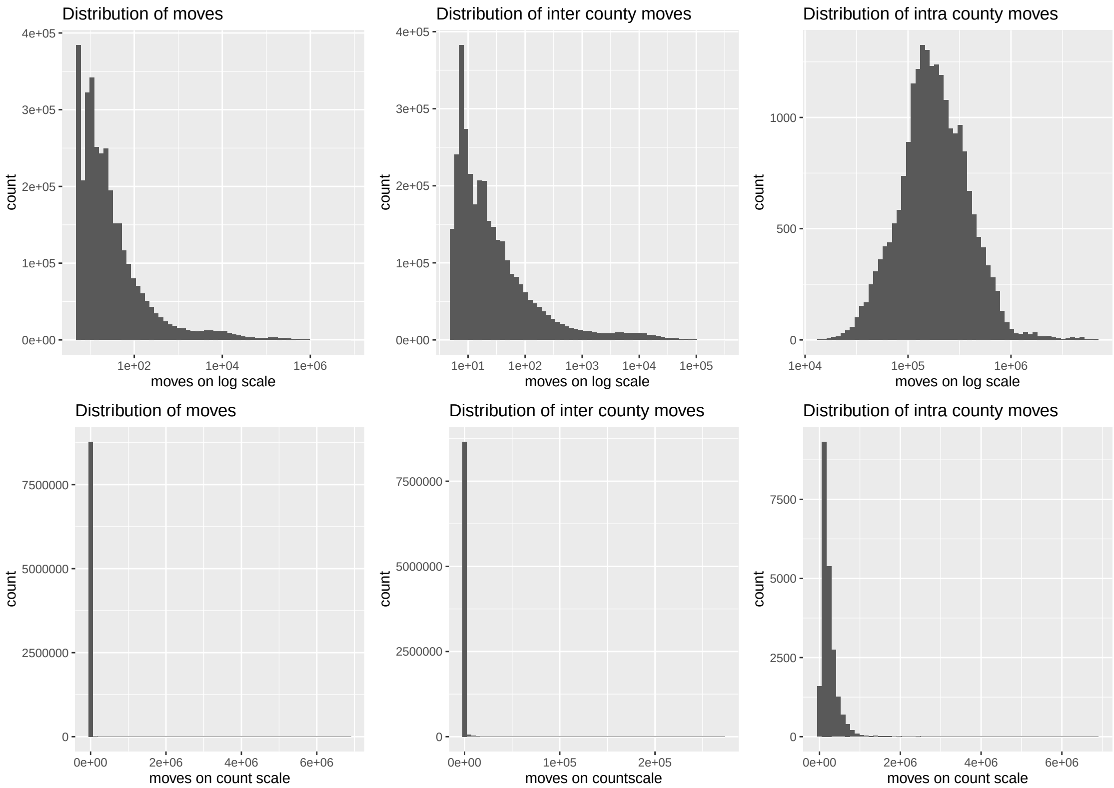
Table B2: First Implementation of Policies

Bundesland	**M08**	**M10**	**M14**	**M17**
Schleswig-Holstein	2020-03-15	2020-03-12	NA	NA
Hamburg	2020-03-15	2020-03-15	NA	2021-01-11
Niedersachsen	2020-03-17	2020-03-17	NA	NA
Nordrhein-Westfalen	2020-03-16	2020-03-16	NA	2020-04-20
Hessen	2020-03-18	2020-03-18	NA	NA
Rheinland-Pfalz	2020-03-17	2020-03-17	2020-03-20	NA
Baden-Württemberg	2020-03-16	2020-03-16	2020-03-21	2021-06-07
Freistaat Bayern	2020-03-21	2020-03-25	2021-01-13	2021-02-19
Saarland	2020-03-18	2020-03-13	NA	2020-06-05
Berlin	2020-03-17	2020-03-17	NA	2020-12-16
Brandenburg	2020-03-23	2020-03-23	NA	2021-01-23
Mecklenburg-Vorpommern	2020-03-17	2020-03-17	NA	2021-06-26
Sachsen	2020-03-22	2020-03-22	NA	2020-03-18
Sachsen-Anhalt	2020-03-18	2020-03-18	2020-03-25	2021-09-14
Thüringen	2020-03-27	2020-03-27	2021-01-11	2020-03-25
Bremen	2020-03-20	2020-03-20	2020-05-13	2020-07-02

Data from [35]

C Descriptive Statistics

Figure 3: Distribution of Moves



Own representation with data provided by Teralytics.

Table C3: Summary statistics

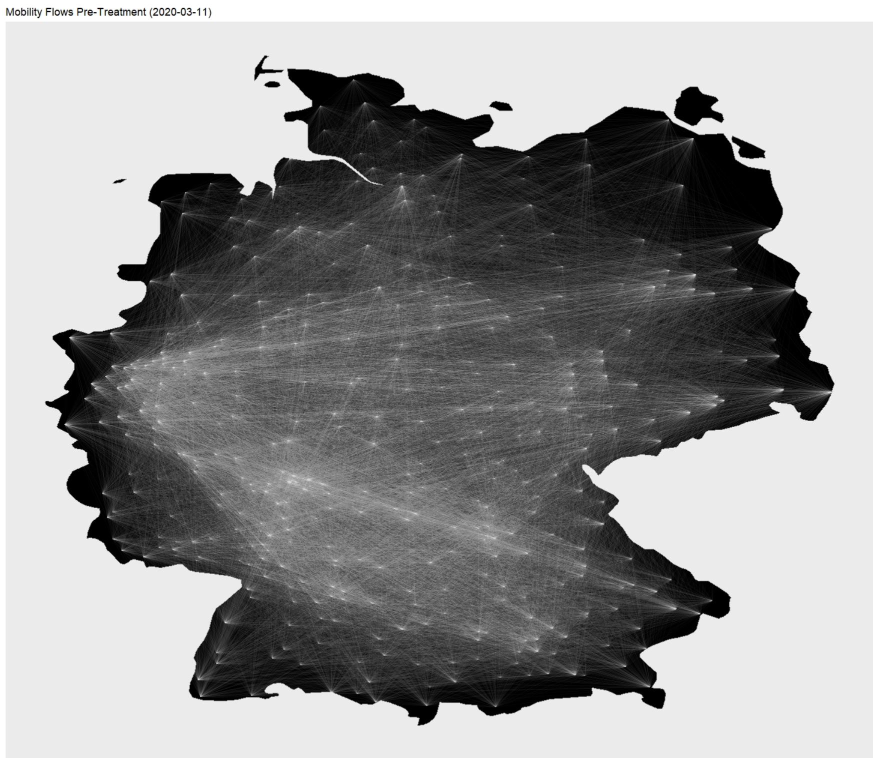
Statistic	N	Mean	St. Dev.	Median	Pctl(25)	Pctl(75)	Min	Max
Population mobility	8,800,000	815.831	19,926.750	0	0	11	0	6,856,353
Distance	8,800,000	248.401	112.996	242.100	163.800	325.900	3.371	643.300
Population	8,800,000	207,887.600	244,839.100	156,418	103,627	241,378.5	34,001	3,664,088
share_homeoffice_WZ	8,800,000	0.539	0.025	0.533	0.522	0.553	0.496	0.630
CDU_CSU	8,800,000	33.975	3.706	34.400	32.400	35.900	22.700	38.800
SPD	8,800,000	19.911	5.331	17.600	15.300	26.000	10.500	27.400
AfD	8,800,000	13.020	4.592	12.200	9.400	12.400	7.800	27.000
log(incidence)	8,800,000	2.632	1.218	2.815	1.887	3.503	0.000	6.358
log(incidence)_W	8,800,000	2.639	1.015	2.823	2.073	3.365	0.000	4.862
M08	8,800,000	0.767	0.423	1	1	1	0	1
M10	8,800,000	0.727	0.446	1	0	1	0	1
M14	8,800,000	0.156	0.363	0	0	0	0	1
M17	8,800,000	0.091	0.287	0	0	0	0	1
M08_W	8,800,000	0.768	0.397	1.000	0.700	1.000	0.000	1.000
M10_W	8,800,000	0.727	0.411	1.000	0.356	1.000	0.000	1.000
M14_W	8,800,000	0.155	0.267	0.000	0.000	0.202	0.000	1.000
M17_W	8,800,000	0.090	0.219	0.000	0.000	0.000	0.000	1.000

Own calculations based on data provided by Teralytics.

C.1 Mobility Network

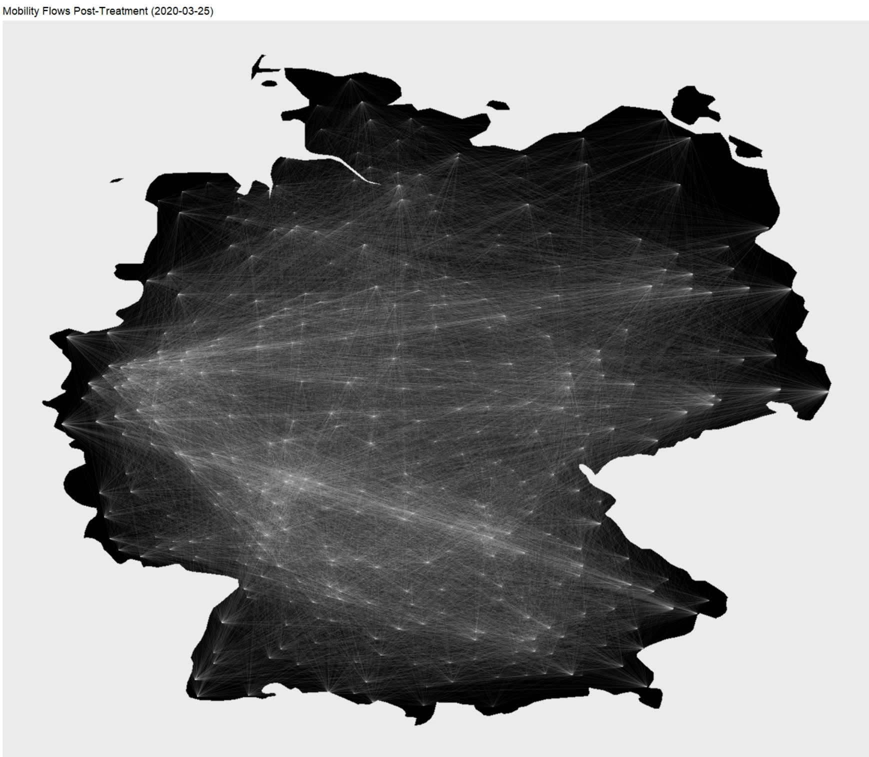
The following graphs show the intensity of the interaction between counties before and two weeks after the first implementation of any of the policies considered in this thesis. Each centroid is linked with any other centroid by a white line which becomes more visible the stronger the interaction between these two counties was at this day. A strong reduction in mobility is observed. Further, it becomes visible that long-distance interactions were reduced the most.

Figure 4: Mobility Network Pre-Treatment



Own representation with data provided by Teralytics

Figure 5: Mobility Network Post-Treatment



Own representation with data provided by Teralytics

D Spatial Weights

Consider an example of three regions:

$$W = \begin{bmatrix} 0 & w_{ab} & w_{ac} \\ w_{ba} & 0 & w_{bc} \\ w_{ca} & w_{cb} & 0 \end{bmatrix}$$

To obtain W_o calculate $W \otimes I_3$. Denote a 3×3 square matrix of zeros as 0_3 and let w_{ij} denote a 3×3 square matrix with the weight w_{ij} on the diagonal, the upper and lower triangles are filled with zeros. The following expression can then be obtained:

$$W_o = \begin{bmatrix} 0_3 & w_{ab} & w_{ac} \\ w_{ba} & 0_3 & w_{bc} \\ w_{ca} & w_{cb} & 0_3 \end{bmatrix}$$

Due to the origin centric arranged data the vector for the origin specific variable looks like this

$$K_s = \begin{bmatrix} x_a & x_a & x_a & x_b & x_b & x_b & x_c & x_c & x_c \end{bmatrix}'$$

Thus if one multiplies the weight matrix with the vector K_s, for the first element of the resulting vector the following sum is obtained:

$$\begin{aligned} W_o \cdot K_s &= 0 \cdot x_a + 0 \cdot x_a + 0 \cdot x_a + w_{ab} \cdot x_b + 0 \cdot x_b + 0 \cdot x_b + w_{ac} \cdot x_c + 0 \cdot x_b + 0 \cdot x_b \\ &= w_{ab} \cdot x_b + w_{ac} \cdot x_c \end{aligned}$$

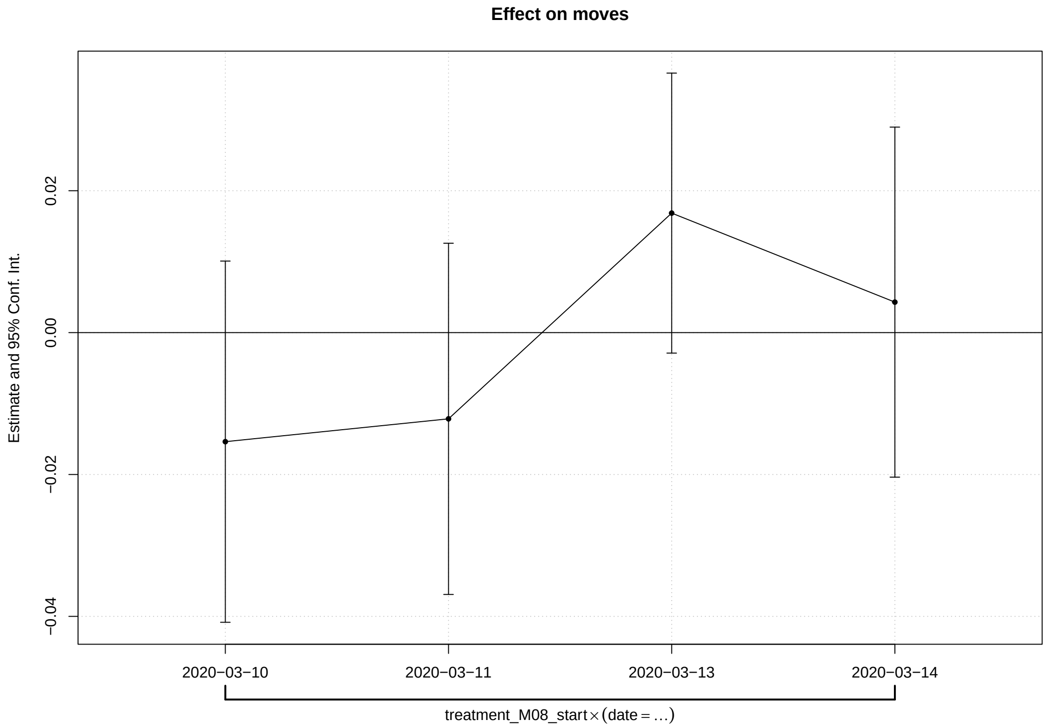
The other elements can be calculated accordingly.

E Parallel Trends Assumption

To assess the assumption of parallel trends, refer to the event study plots below. It can be seen how the treatment effect of the different policies develops two periods prior to and two periods after the first implementation of the respective policy. Since the policies M08 and M10 are eventually implemented in all 400 counties, it needs to be defined, to make this event study operational, a cut-off point. As a consequence for these two policies, the counties that implemented the respective policy 10 days after it was first implemented were used as the control group. Therefore, the graphs depict the difference in trends between early adopters and late adopters. The other policies do not suffer from this problem and do not need to be modified in any way. As seen in the graphs, the coefficients did not differ significantly from zero for any of the policies considered here prior to treatment; see Figures 6,7,8,9. In fact, except for restrictions on workplaces, none of the policies showed significant effects in the two periods after implementation. Only when the start region had implemented M17 a significant decrease in mobility can be observed immediately afterward. This is rather surprising since one would expect that M14, which is the only policy which restricts mobility directly and where legal sanctions are to be expected when people fail to comply, would have an immediate effect as well.

These first results support the assumption of parallel trends for this analysis. But why would one assume parallel trends from a theoretical perspective? To address this question, it is advised to first rule out endogeneity of the treatment variables, since this would immediately invalidate the assumption. Looking at the appendix [B2], one can observe that there is in fact some systematic underlying the implementation of NPIs. The last states to implement restrictions on gastronomy (M08) and nightlife (M10) activities were Thuringia (TH), Bavaria (BY), Saxony (SN) and Brandenburg (BB). This hints towards some difference how governments react and divides Germany into a group of fast actors and a group of slow acting governments in the south east. Especially Schleswig-Holstein (SH) and Saarland (SL) were quick to implement restrictions on nightlife activities. M08 and M10 were among the first heavy restrictions imposed during the early stages of the Covid-19 pandemic.
The evaluated NPIs are decided at the federal state or country level [35]. This implies that in order to avoid endogeneity, it has to be controlled for differences between the federal states. By controlling for time constant heterogeneity through county fixed effects as well as time fixed effects, electoral results and the difference in pandemic situation, a large portion of possible endogeneity is dealt with.
Restrictions on internal mobility and workplace regulations were implemented much later. In 2020 only four states put restrictions on mobility, namely Rhineland-Palatinate(RP), Saxony-Anhalt(ST), Bremen(HB), as well as Baden Wurttemberg (BW). The relative autonomy of the federal states offers the rare opportunity of a

large-scale natural experiment. Due to the cultural proximity between federal states and the paramount national institutions it seems reasonable to assume similar trends regarding population mobility across states.



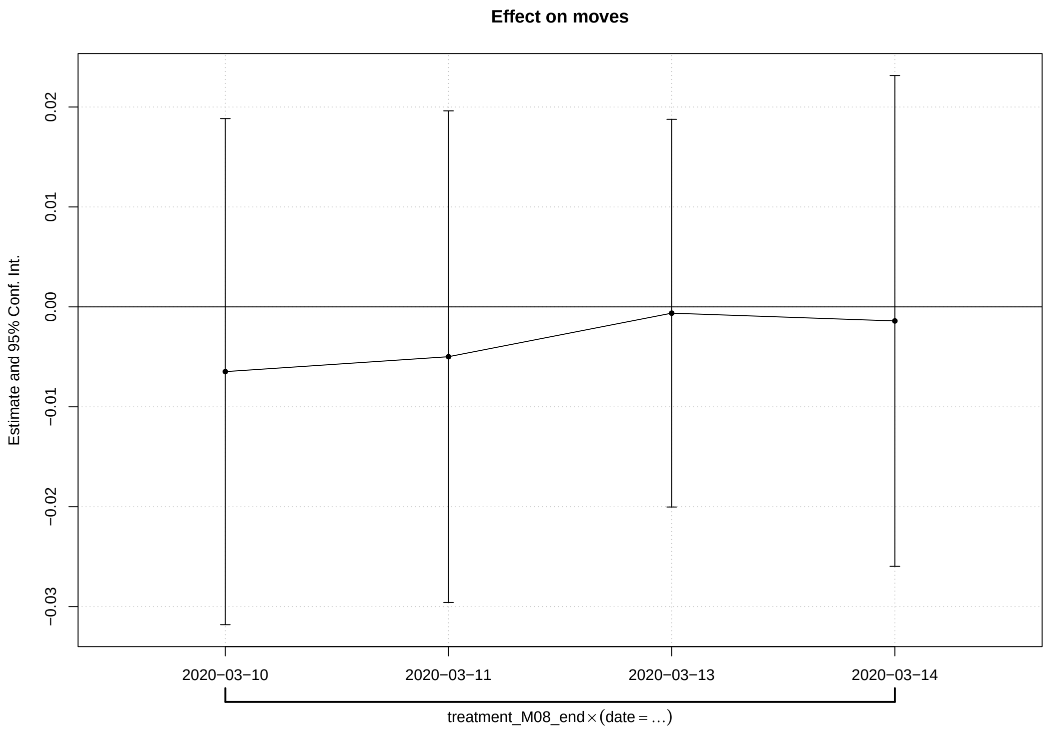
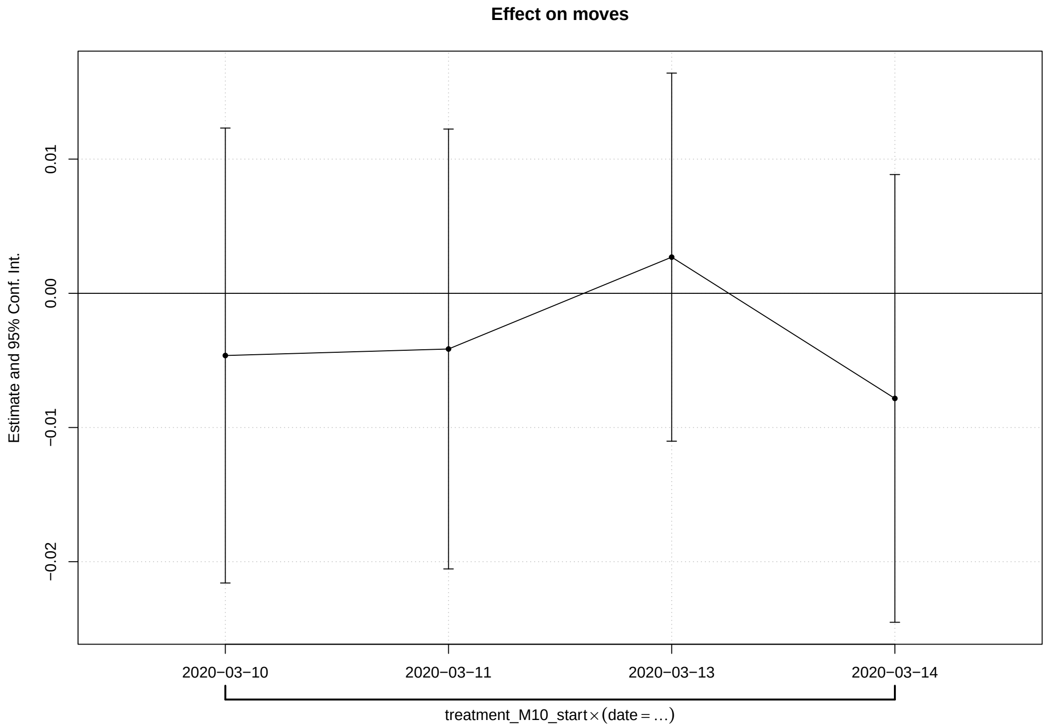


Figure 6: Effect Size of Restrictions on Gastronomy; Reference Date: 2020-03-12



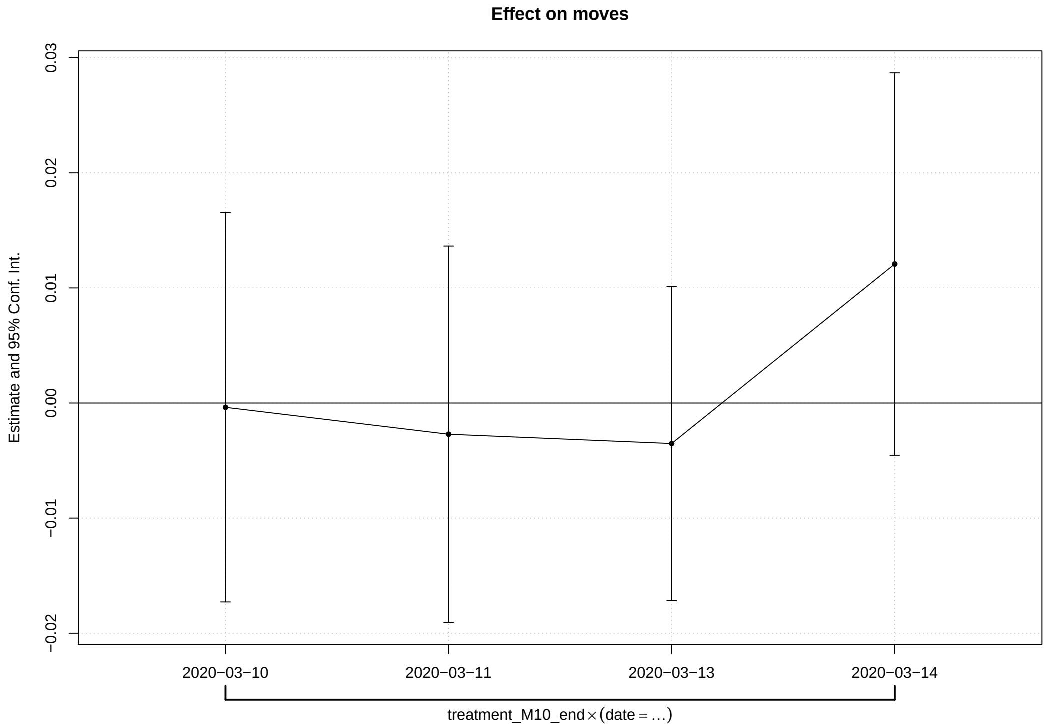
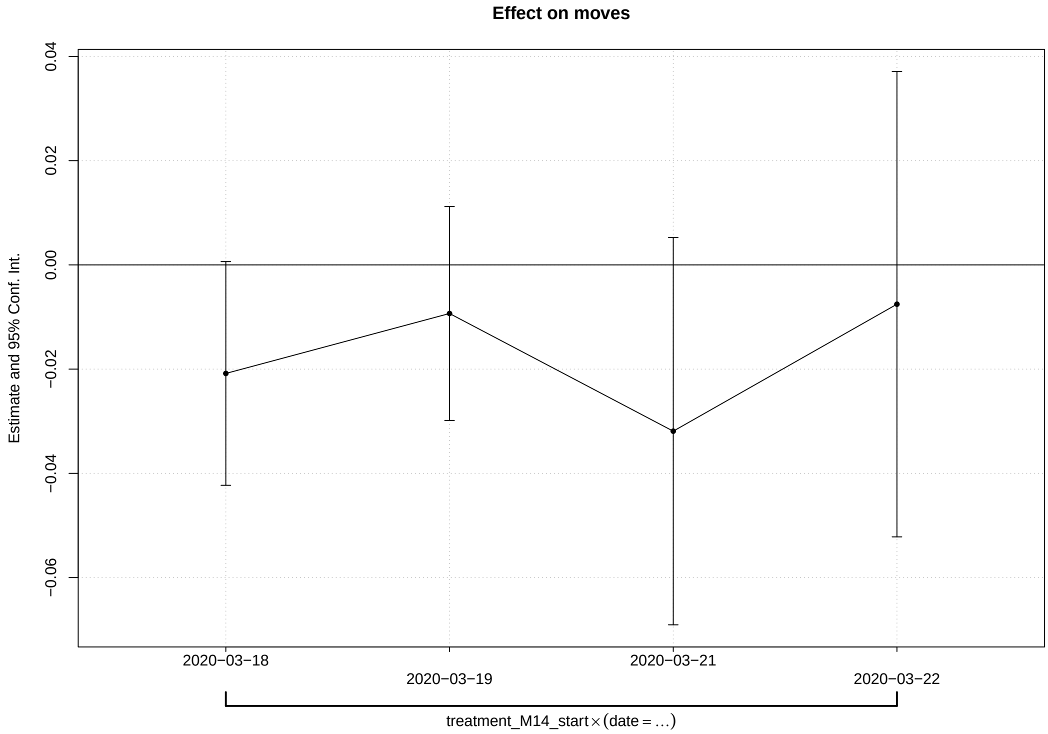


Figure 7: Effect Size of Restrictions on Nightlife Activities, Reference Date: 2020-03-12



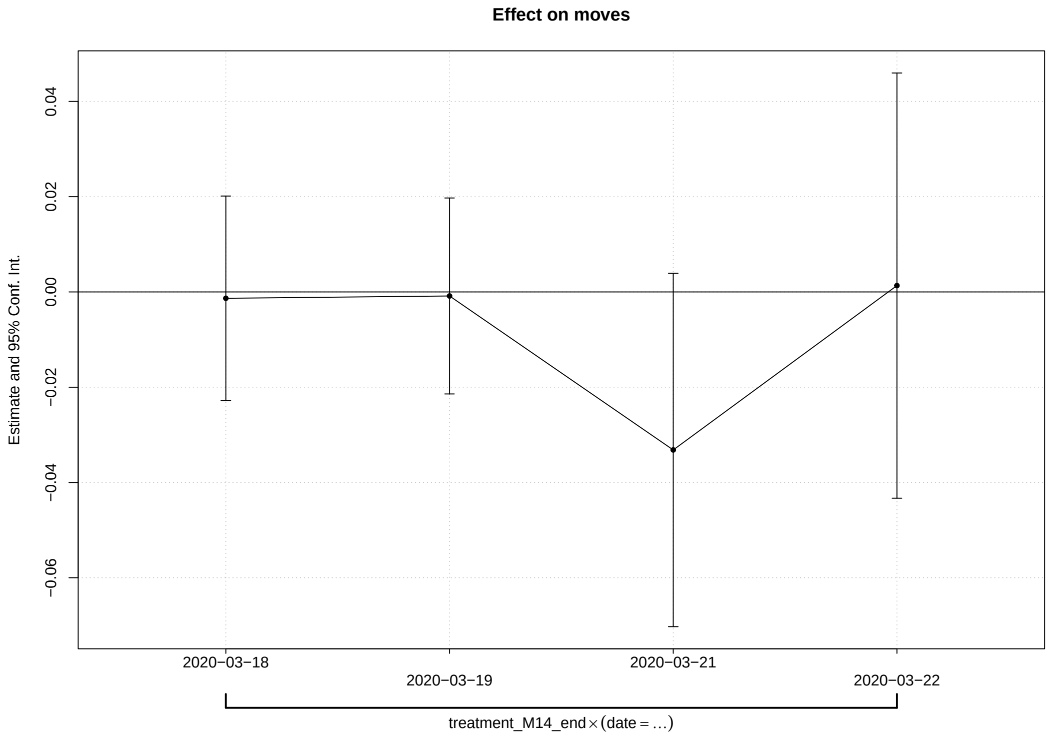
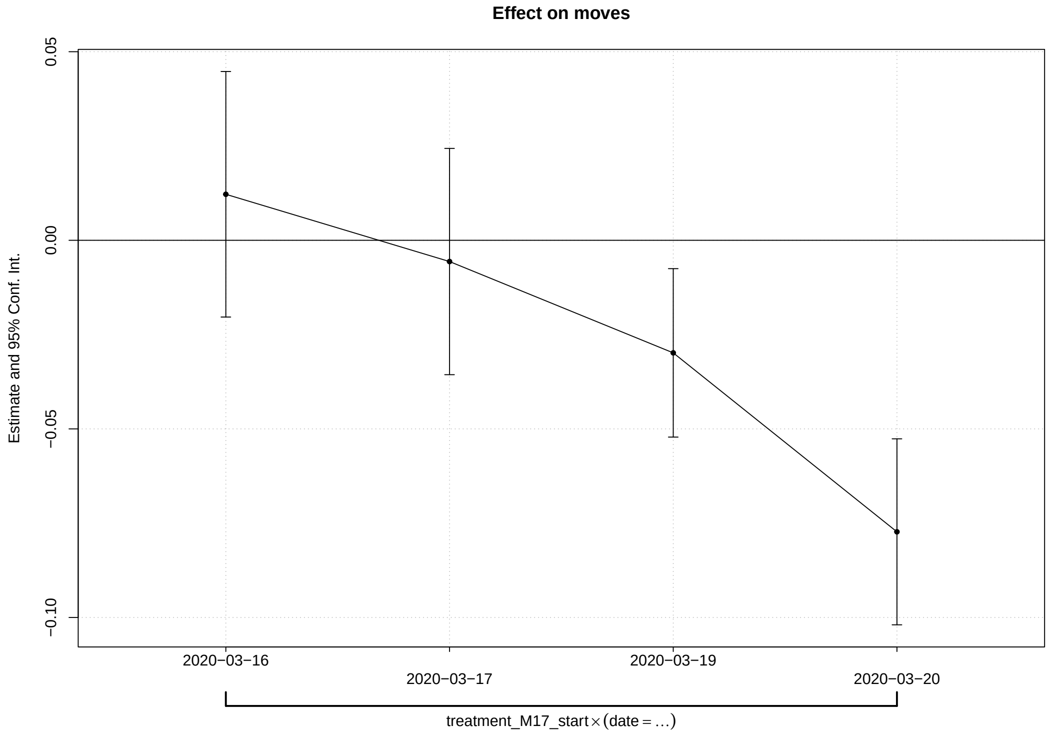


Figure 8: Effect Size Restriction on Workplaces, Reference Date: 2020-03-20



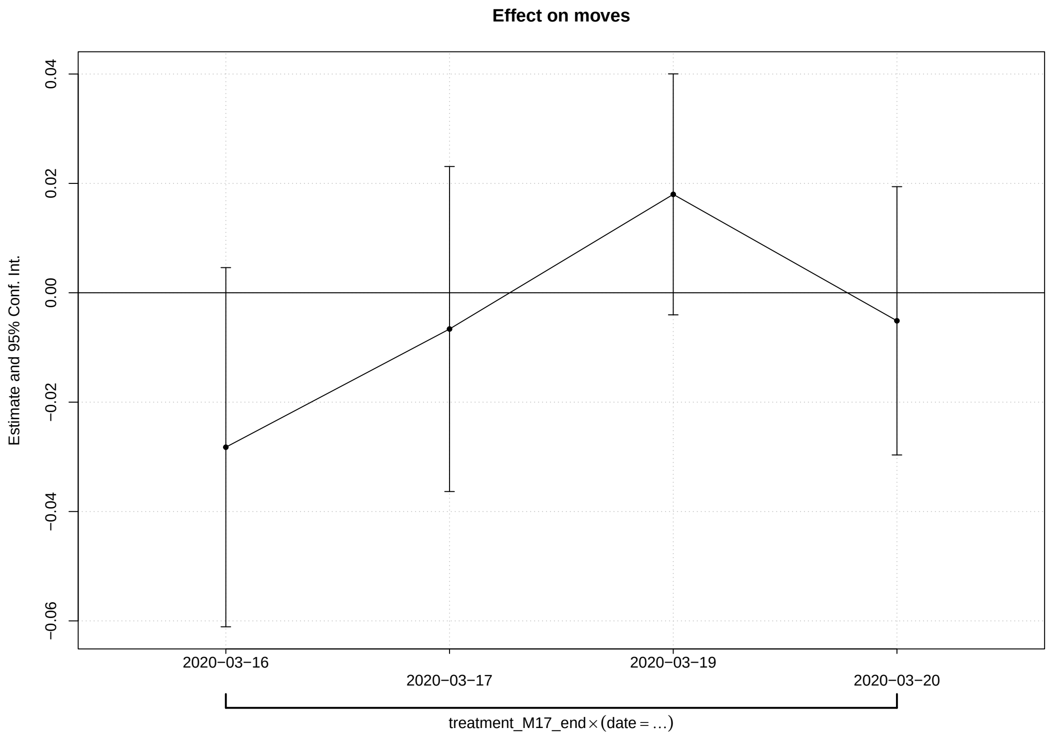


Figure 9: Effect Size of Restrictions on Internal Movement, Reference Date: 2020-03-18

F Regression results

Table F4: Regression Results Gravity Base Model

Dependent Variable:	Population mobility	
Model:	(1)	(2)
Variables		
(Intercept)	0.3542***	
	(0.1354)	
log(distance)	-2.503***	-2.503***
	(0.0040)	(0.0041)
log(population_start)	0.7067***	0.7067***
	(0.0062)	(0.0058)
log(population_end)	0.7172***	0.7172***
	(0.0062)	(0.0058)
Fixed effects		
time		Yes
Fit statistics		
Observations	8,800,000	8,800,000
Squared Correlation	0.47033	0.49603

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table F5: Regression Results for Restrictions on Gastronomy (M08)

Dependent Variable:	Population mobility			
Model:	(1)	(2)	(3)	(4)
Variables				
(Intercept)				
log(distance)	-2.771***	-3.847***		
	(0.0068)	(0.0031)		
log(population_start)	0.9417***			
	(0.0084)			
log(population_end)	0.9336***			
	(0.0084)			
log(incidence_start)	0.0636***	-0.0055	-0.0055***	-0.0036***
	(0.0069)	(0.0059)	(0.0009)	(0.0009)
log(incidence_end)	0.0655***	-0.0045	-0.0042***	-0.0014
	(0.0069)	(0.0059)	(0.0009)	(0.0009)
log(share_homeoffice_WZ_start)	-8.319***			
	(0.1255)			
log(share_homeoffice_WZ_end)	-8.530***			
	(0.1294)			
AfD_start	0.2534***			
	(0.0416)			
AfD_end	0.1465***			
	(0.0403)			
government_start	-0.0922*			
	(0.0492)			
government_end	-0.2293***			
	(0.0480)			
M08_start	-0.0517	-0.0244	-0.0228***	0.0033
	(0.0532)	(0.0422)	(0.0052)	(0.0063)
M08_end	-0.0022	-0.0343	-0.0349***	-0.0094
	(0.0521)	(0.0421)	(0.0052)	(0.0062)
AfD_start × M08_start	-0.0219	0.0221	0.0209***	0.0157***
	(0.0452)	(0.0417)	(0.0059)	(0.0060)
AfD_end × M08_end	-0.0452	0.0418	0.0429***	0.0431***
	(0.0437)	(0.0417)	(0.0059)	(0.0060)
government_start ×M08_start	-0.1445***	-0.0060	-0.0072	-0.0077
	(0.0543)	(0.0384)	(0.0050)	(0.0051)

government_end × M08_end	-0.1631***	0.0033	0.0050	0.0055
	(0.0530)	(0.0383)	(0.0050)	(0.0051)
M08_start_W				-0.0242**
				(0.0122)
M08_end_W				-0.0681***
				(0.0125)
log(incidence_start)_W				-0.0316***
				(0.0041)
log(incidence_end)_W				-0.0011
				(0.0041)
Fixed effects				
time	Yes	Yes	Yes	Yes
origin		Yes	Yes	Yes
destination		Yes	Yes	Yes
origin-destination			Yes	Yes
Fit statistics				
Observations	8,800,000	8,800,000	8,083,405	8,083,405
Squared Correlation	0.54226	0.94154	0.99670	0.99675

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table F6: Regression Results for Restrictions on Nightlife Activities (M10)

Dependent Variable:	Population mobility			
Model:	(1)	(2)	(3)	(4)
Variables				
(Intercept)				
log(distance)	-2.774***	-3.847***		
	(0.0068)	(0.0031)		
log(population_start)	0.9458***			
	(0.0085)			
log(population_end)	0.9377***			
	(0.0085)			
log(incidence_start)	0.0653***	-0.0063	-0.0063***	-0.0039***
	(0.0069)	(0.0059)	(0.0009)	(0.0009)
log(incidence_end)	0.0675***	-0.0054	-0.0050***	-0.0017*
	(0.0069)	(0.0059)	(0.0009)	(0.0009)
log(share_homeoffice_WZ_start)	-8.342***			
	(0.1255)			
log(share_homeoffice_WZ_end)	-8.558***			
	(0.1294)			
AfD_start	0.2611***			
	(0.0417)			
AfD_end	0.1521***			
	(0.0404)			
government_start	-0.0804*			
	(0.0474)			
government_end	-0.2131***			
	(0.0461)			
M10_start	-0.0682	-0.0060	-0.0049	0.0071
	(0.0529)	(0.0403)	(0.0052)	(0.0058)
M10_end	-0.0223	-0.0151	-0.0162***	-0.0045
	(0.0517)	(0.0403)	(0.0052)	(0.0058)
AfD_start × M10_start	-0.0257	0.0204	0.0192***	0.0119**
	(0.0454)	(0.0414)	(0.0059)	(0.0060)
AfD_end × M10_end	-0.0476	0.0391	0.0402***	0.0376***
	(0.0439)	(0.0413)	(0.0058)	(0.0060)
government_start ×M10_start	-0.1627***	-0.0026	-0.0042	-0.0054
	(0.0527)	(0.0383)	(0.0050)	(0.0051)

government_end ×M10_end	-0.1862***	0.0071	0.0090*	0.0085*
	(0.0513)	(0.0383)	(0.0050)	(0.0051)
M10_start_W				-0.0032
				(0.0084)
M10_end_W				-0.0237***
				(0.0086)
log(incidence_start)_W				-0.0355***
				(0.0041)
log(incidence_end)_W				-0.0065
				(0.0041)
Fixed effects				
time	Yes	Yes	Yes	Yes
origin		Yes	Yes	Yes
destination		Yes	Yes	Yes
origin-destination			Yes	Yes
Fit statistics				
Observations	8,800,000	8,800,000	8,083,405	8,083,405
Squared Correlation	0.54431	0.94151	0.99666	0.99674

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table F7: Regression Results for Restrictions on Internal Movement (M14)

Dependent Variable:	Population mobility			
Model:	(1)	(2)	(3)	(4)
Variables				
(Intercept)				
log(distance)	-2.773***	-3.847***		
	(0.0069)	(0.0031)		
log(population_start)	0.9502***			
	(0.0088)			
log(population_end)	0.9429***			
	(0.0088)			
log(incidence_start)	0.0524***	-0.0073	-0.0072***	-0.0040***
	(0.0070)	(0.0059)	(0.0009)	(0.0009)
log(incidence_end)	0.0562***	-0.0062	-0.0058***	-0.0018**
	(0.0070)	(0.0059)	(0.0009)	(0.0009)
log(share_homeoffice_WZ_start)	-8.337***			
	(0.1248)			
log(share_homeoffice_WZ_end)	-8.521***			
	(0.1278)			
AfD_start	0.2866***			
	(0.0294)			
AfD_end	0.1955***			
	(0.0287)			
government_start	-0.1655***			
	(0.0333)			
government_end	-0.2656***			
	(0.0324)			
M14_start	0.0993**	-0.0015	0.0007	0.0012
	(0.0484)	(0.0493)	(0.0052)	(0.0058)
M14_end	0.1589***	-0.0048	-0.0071	-0.0003
	(0.0472)	(0.0492)	(0.0052)	(0.0058)
AfD_start × M14_start	-0.1567***	0.0321	0.0291***	0.0256***
	(0.0552)	(0.0794)	(0.0071)	(0.0072)
AfD_end × M14_end	-0.2145***	0.0344	0.0335***	0.0271***
	(0.0537)	(0.0790)	(0.0071)	(0.0072)
government_start ×M14_start	-0.0622	0.0179	0.0160***	0.0148**
	(0.0521)	(0.0528)	(0.0060)	(0.0061)

government_end ×M14_end	-0.3031***	0.0185	0.0196***	0.0146**
	(0.0555)	(0.0529)	(0.0060)	(0.0061)
M14_start_W				0.0163**
				(0.0066)
M14_end_W				-0.0131**
				(0.0066)
log(incidence_start)_W				-0.0380***
				(0.0040)
log(incidence_end)_W				-0.0133***
				(0.0040)
Fixed effects				
time	Yes	Yes	Yes	Yes
origin		Yes	Yes	Yes
destination		Yes	Yes	Yes
origin-destination			Yes	Yes
Fit statistics				
Observations	8,800,000	8,800,000	8,083,405	8,083,405
Squared Correlation	0.53393	0.94142	0.99656	0.99669

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table F8: Regression Results for Restrictions on Workplaces (M17)

Dependent Variable:	Population mobility			
Model:	(1)	(2)	(3)	(4)
Variables				
(Intercept)				
log(distance)	-2.781***	-3.847***		
	(0.0067)	(0.0031)		
log(population_start)	0.9562***			
	(0.0086)			
log(population_end)	0.9482***			
	(0.0086)			
log(incidence_start)	0.0706***	-0.0075	-0.0074***	-0.0043***
	(0.0071)	(0.0059)	(0.0009)	(0.0009)
log(incidence_end)	0.0715***	-0.0064	-0.0060***	-0.0020**
	(0.0071)	(0.0059)	(0.0009)	(0.0009)
log(share_homeoffice_WZ_start)	-8.457***			
	(0.1254)			
log(share_homeoffice_WZ_end)	-8.667***			
	(0.1294)			
AfD_start	0.3181***			
	(0.0264)			
AfD_end	0.1936***			
	(0.0259)			
government_start	-0.1535***			
	(0.0275)			
government_end	-0.3074***			
	(0.0269)			
M17_start	-0.3255***	0.0021	0.0022	-0.0137***
	(0.0218)	(0.0259)	(0.0039)	(0.0052)
M17_end	-0.3037***	0.0014	-0.0303***	-0.0222***
	(0.0219)	(0.0259)	(0.0055)	(0.0052)
AfD_start × M17_start	0.1668***	0.0170	0.0124**	0.0139**
	(0.0283)	(0.0612)	(0.0058)	(0.0058)
AfD_end × M17_end	0.1343***	0.0246	0.0598***	0.0319***
	(0.0284)	(0.0613)	(0.0073)	(0.0058)
government_end × M17_end			0.0315***	
			(0.0018)	

M17_start_W				0.0111*
				(0.0066)
M17_end_W				0.0407***
				(0.0066)
log(incidence_start)_W				-0.0380***
				(0.0040)
log(incidence_end)_W				-0.0121***
				(0.0040)
Fixed effects				
time	Yes	Yes	Yes	Yes
origin		Yes	Yes	Yes
destination		Yes	Yes	Yes
origin-destination			Yes	Yes
Fit statistics				
Observations	8,800,000	8,800,000	8,083,405	8,083,405
Squared Correlation	0.55168	0.94144	0.99658	0.99671

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table F9: Regression Results for All Policies Simultaneously

Dependent Variable:	Population mobility	
Model:	(1)	(2)
Variables		
log(incidence_start)	-0.0057***	-0.0039***
	(0.0009)	(0.0009)
log(incidence_end)	-0.0043***	-0.0015*
	(0.0009)	(0.0009)
M08_start	0.0149***	0.0005
	(0.0048)	(0.0073)
M08_end	-0.0495***	-0.0164**
	(0.0065)	(0.0072)
M10_start	-0.0330***	0.0044
	(0.0052)	(0.0037)
M10_end	0.0190***	0.0080**
	(0.0033)	(0.0037)
M14_start	-0.0078	-0.0080
	(0.0065)	(0.0071)
M14_end	-0.0051	0.0027
	(0.0065)	(0.0071)
M17_start	0.0009	-0.0082
	(0.0039)	(0.0053)
M17_end	0.0005	-0.0159***
	(0.0039)	(0.0053)
AfD_start × M08_start	-0.0242***	-4.32×10^{-5}
	(0.0089)	(0.0127)
AfD_end × M08_end	0.0538***	0.0351***
	(0.0126)	(0.0126)
government_start × M08_start	-0.0602***	-0.0103
	(0.0047)	(0.0065)
government_end × M08_end	0.0027	0.0057
	(0.0064)	(0.0065)
AfD_start × M10_start	0.0406***	0.0089
	(0.0095)	(0.0114)
AfD_end × M10_end	-0.0167	0.0024
	(0.0113)	(0.0113)
government_start × M10_start	0.0483***	
	(0.0049)	

AfD_start × M14_start	0.0179**	0.0204**
	(0.0078)	(0.0081)
AfD_end × M14_end	0.0156**	0.0113
	(0.0078)	(0.0080)
government_start × M14_start	0.0288***	0.0270***
	(0.0073)	(0.0075)
government_end × M14_end	0.0219***	0.0161**
	(0.0073)	(0.0075)
AfD_start × M17_start	-0.0073	-0.0012
	(0.0068)	(0.0070)
AfD_end × M17_end	0.0047	0.0122*
	(0.0068)	(0.0069)
M08_start_W		-0.0392***
		(0.0132)
M08_end_W		-0.1011***
		(0.0134)
M10_start_W		0.0214***
		(0.0074)
M10_end_W		0.0338***
		(0.0074)
M14_start_W		0.0223***
		(0.0066)
M14_end_W		-0.0207***
		(0.0066)
M17_start_W		0.0032
		(0.0067)
M17_end_W		0.0278***
		(0.0067)
log(incidence_start)_W)		-0.0335***
		(0.0041)
log(incidence_end)_W)		-0.0032
		(0.0041)
Fixed effects: time, origin, destination, origin-destination		
Observations	8,083,405	8,083,405
Squared Correlation	0.99667	0.99675

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table F10: Regression Results for All Policies Simultaneously, Only Short Trips

Dependent Variable:	Population mobility	
Model:	(1)	(2)
Variables		
log(incidence_start)	-0.0040***	-0.0028***
	(0.0009)	(0.0009)
log(incidence_end)	-0.0037***	-0.0022**
	(0.0009)	(0.0009)
M08_start	-0.1039***	-0.0745***
	(0.0145)	(0.0150)
M08_end	-0.1013***	-0.0714***
	(0.0145)	(0.0150)
M10_start	0.0663***	0.0629***
	(0.0140)	(0.0143)
M10_end	0.0646***	0.0603***
	(0.0141)	(0.0143)
M14_start	0.0050	0.0020
	(0.0059)	(0.0064)
M14_end	0.0033	0.0020
	(0.0059)	(0.0064)
M17_start	-0.0023	-0.0126**
	(0.0047)	(0.0057)
M17_end	-0.0034	-0.0157***
	(0.0047)	(0.0057)
AfD_start × M08_start	0.0921***	0.0725***
	(0.0185)	(0.0186)
AfD_end × M08_end	0.1124***	0.0941***
	(0.0184)	(0.0186)
government_start × M08_start	0.0870***	0.0759***
	(0.0148)	(0.0149)
government_end × M08_end	0.0850***	0.0735***
	(0.0148)	(0.0149)
AfD_start × M10_start	-0.0596***	-0.0486***
	(0.0181)	(0.0182)
AfD_end × M10_end	-0.0596***	-0.0479***
	(0.0181)	(0.0181)
government_start × M10_start	-0.0655***	-0.0610***
	(0.0144)	(0.0145)

government_end × M10_end	-0.0618***	-0.0568***
	(0.0144)	(0.0145)
AfD_start × M14_start	0.0063	0.0101
	(0.0081)	(0.0084)
AfD_end × M14_end	0.0080	0.0095
	(0.0081)	(0.0083)
government_start × M14_start	0.0063	0.0102
	(0.0068)	(0.0069)
government_end × M14_end	0.0081	0.0104
	(0.0068)	(0.0069)
AfD_start × M17_start	0.0024	0.0084
	(0.0077)	(0.0078)
AfD_end × M17_end	0.0032	0.0077
	(0.0076)	(0.0078)
M08_start_W		-0.0488**
		(0.0209)
M08_end_W		-0.0410*
		(0.0210)
M10_start_W		-3.44×10^{-5}
		(0.0104)
M10_end_W		0.0092
		(0.0104)
M14_start_W		0.0085
		(0.0085)
M14_end_W		-0.0025
		(0.0085)
M17_start_W		0.0045
		(0.0088)
M17_end_W		0.0252***
		(0.0088)
log(incidence_start)_W)		-0.0159***
		(0.0051)
log(incidence_end)_W)		-0.0102**
		(0.0051)
Fixed effects: time, origin, destination, origin-destination		
Observations	1,196,360	1,196,360
Squared Correlation	0.99690	0.99694

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table F11: Regression Results for All Policies Simultaneously, Only Long Trips

Dependent Variable:	Population mobility	
Model:	(1)	(2)
Variables		
log(incidence_start)	-0.0199***	-0.0089***
	(0.0016)	(0.0016)
log(incidence_end)	0.0013	0.0019
	(0.0015)	(0.0016)
M08_start	0.0274**	-0.0480***
	(0.0121)	(0.0181)
M08_end	-0.3959***	-0.3362***
	(0.0114)	(0.0134)
M10_start	-0.2734***	-0.1845***
	(0.0156)	(0.0186)
M10_end	0.0221***	0.0252***
	(0.0041)	(0.0062)
M14_start	0.1800***	0.2702***
	(0.0080)	(0.0097)
M14_end	0.2190***	0.3738***
	(0.0098)	(0.0111)
M17_start	-0.2493***	-0.0596***
	(0.0161)	(0.0089)
M17_end	-0.0053	-0.1333***
	(0.0041)	(0.0084)
AfD_start × M08_start	0.0557***	0.1400***
	(0.0120)	(0.0115)
AfD_end × M08_end	0.4597***	0.4667***
	(0.0274)	(0.0275)
government_start × M08_start	-0.0096	0.0873***
	(0.0144)	(0.0200)
government_end × M08_end	0.3643***	0.3724***
	(0.0115)	(0.0118)
AfD_start × M10_start	0.3366***	0.2301***
	(0.0172)	(0.0127)
AfD_end × M10_end	-0.0039	-0.0061
	(0.0244)	(0.0241)
government_start × M10_start	0.2729***	0.1863***
	(0.0168)	(0.0200)

AfD_start × M14_start	-0.0380***	-0.0984***
	(0.0120)	(0.0124)
AfD_end × M14_end	-0.0559***	-0.1716***
	(0.0131)	(0.0136)
government_start × M14_start	-0.1027***	-0.1402***
	(0.0093)	(0.0095)
government_end × M14_end	-0.2132***	-0.2657***
	(0.0109)	(0.0112)
AfD_start × M17_start	0.1291***	-0.1005***
	(0.0076)	(0.0100)
AfD_end × M17_end	-0.0241***	0.0185*
	(0.0092)	(0.0095)
government_start × M17_start	0.2537***	
	(0.0166)	
M08_start_W		-0.0097
		(0.0107)
M08_end_W		-0.1431***
		(0.0115)
M10_start_W		0.0078
		(0.0090)
M10_end_W		0.0021
		(0.0095)
M14_start_W		-0.0988***
		(0.0079)
M14_end_W		-0.1943***
		(0.0080)
M17_start_W		0.0814***
		(0.0104)
M17_end_W		0.1542***
		(0.0097)
log(incidence_start)_W)		-0.0792***
		(0.0055)
log(incidence_end)_W)		0.0218***
		(0.0049)
Fixed effects: time, origin, destination, origin-destination		
Observations	6,885,395	6,885,395
Squared Correlation	0.84736	0.84703

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

G Interpretation Auxiliary results

Additionally to the policy related variables I report some estimates of the effects of incidence and its spillover effects on population mobility. The spatial lag of incidence takes the form a weighted average of the logarithm of the incidence in all neighboring counties.

G.1 Single Policy Model Whole Sample

For the single policy model using the whole sample, the spatial spillover of the incidence for the origin region is relatively consistent across all four policies. The effect is significant and the estimates imply that a 1% increase in the weighted average of the incidence of neighboring counties is associated with a reduction in the number of trips originating from a county by 3% to 3.8%.
In contrast, the destination spillover of incidence remains insignificant for M08 and M10.
For M14 and M17 a significant negative coefficient is reported, which is about a third in size of that for the origin spillover (-1.33% for M14 and -1.21% for M17).
For the variable *log_incidence_end* in the model containing M08, the coefficient becomes insignificant. It can be seen that it is not only of interest for individuals how high the incidence in a particular county is when making mobility decisions. Instead, more attention is paid to the pandemic situation in the general neighborhood of a potential destination. Also, when deciding to leave a county, the situation in the surrounding counties appears to be more important.

G.2 Single Policy Model Sub-samples

Moving on to the single policy model using the sub samples, it can be seen that a higher incidence is associated with a reduction in mobility for the whole sample and both sub samples. While the coefficients for short trips do not differ much from the original estimates, the incidence in the destination region appears to have no significant effect on long trips. In contrast, the effect of incidence in the origin region on long trips is magnitudes higher than the original estimates.
This result could be an indicator that people voluntarily reduce mobility if high incidences are present in their county. In order to avoid prolonging of transmission chains long trips are avoided.

A higher weighted average of incidence of neighboring counties is most of the time associated with a moderate decrease in mobility going toward or leaving this county. Only for long trips a significant positive coefficient is estimated for the destination-based spillover effect for the models which include M08 or M10.
It seems plausible that in general areas with high incidence limit the mobility going

towards them. At the same time, it might be possible that people intentionally choose destinations with restrictions in place because they feel more safe there compared to other regions where no restrictions on free-time activities are imposed.

G.3 Combined Model Whole Sample

For the combined model which uses the whole sample, the coefficients for incidence remain significant but relatively small and negative.

The spillover effects of incidence are similar to those estimated for the single policy models that include M08 or M10. Only the origin-based spillover is significantly negative (-3.35%). This indicates that models that only include M14 or M17 suffer from bias due to omitting M08 and M10.

G.4 Combined Model Sub-samples

Finally when moving on to the combined model used with the sub samples it can be seen that concerning short duration trips, the effects of incidence remain fairly similar to those obtained for the original estimation using the whole sample. The destination-based effect of incidence on long trips is rendered insignificant. However, the origin-based effect is more pronounced than for the original estimation (-1.99%). People seem to avoid longer trips if the incidence is high in their home county.

Moving on to the spatial spillover effects of incidence, for short trips both origin- (-1.59%) and destination- (-1.02%) based spillover effects are significant and negative. The origin-based effect on long-duration trips remains negative and is, with -7.92%, twice as large as the estimated effect on all trips. If the incidence is high in neighboring regions, people avoid traveling to these neighboring regions. However, long trips are much more affected by a neighborhood of counties with high incidence. A 1% higher weighted average of neighboring incidences for the destination region is associated with an increase in long trips toward it of 2.18% on average.
This positive effect could again be explained by the increase in relative attractiveness of counties that are surrounded by high incidences.

H R packages

Table H12: Used R-Packages

Feather	Fast and native data reading and writing with far superior writing and reading times compared to CSV format.
Readr and Haven	Packages to read and write external data formats like CSV,XLSX and DTA.
tidyverse	Bundle of packages for efficient data manipulation. Especially important are 'Dplyr' for data transformations. And 'ggplot2' for visualization.
reshape2	R package used to efficiently "melt" weight matrices into paired lists.
collapse	R package for efficient data aggregation.
rvest	R Package for webscraping and facilated interface between R and json, HTML as well as javascript.
spml pml spdep spatialreg	These packages are needed to work with spatial panel data. E.g. to set time and spatial indexes.
fixest	Very fast implementation for fixed effects estimation. This package is the main workhorse used for the estimations presented here in this theses.

I Technical Difficulties

In this section I want to highlight some technical difficulties which were overcome and made the study as presented possible.

- Data from Teralytics is provided as aggregate at the level of relation IDs from OSM and only a limited number of days of data can be downloaded simultaneously. Further the data contains entries for different modes of transport. Since the differentiation between modes of transport would increase computational cost drastically, Moritz Meister kindly provided me with the raw numbers on total trips between OSM relation ID regions for the time frame of this study. Since no adequate matching table between OSM relation IDs and german counties were available this needed to be done by me. After a lot of trial and error were I tried to complete the matching using relational data bases which form the foundation of OSM I finally wrote a script in R which created web links with the relation IDs automatically and then scraped the corresponding AGS5 and the county name from the website of OSM.

- Another challenge were the distances between counties. I set up a local image of OSM for Germany. Using the R package 'OSRM' I was able to send queries to this local image which contained the coordinates of the geometric center of each county. The OSM routing service which is called by these functions then uses a variation of the Djikstra algorithm to calculate the distances and/or time needed to travel between points by car. From this I was then able to build the distance matrix which captures all distances between region pairs.

- Further down the line it became evident that working with traditional spatial weight matrices would require high performance servers with hundreds of gigabyte of ram to compute spatial lags. To cope with this problem, my implementation uses the R package 'Matrix' and 'spdep'. With the help of these packages I was able to build sparse matrices which only store non zero values and can be processed using specifically designed handling functions. With these sparse matrices the computational burden could be lowered substantially such that the incorporation of spatial lags became feasible.

- General data wrangling and reshaping of the data set as well as filling in missing values were performed using the boundle of R packages called 'tidyverse'.

Affirmation

I hereby declare that I have composed my Master's thesis *"The effects of non-pharmaceutical interventions on mobility - evidence from the early stage of the pandemic in Germany"* independently using only those resources mentioned, and that I have as such identified all passages which I have taken from publications verbatim or in substance. I agree that the work will be reviewed using plagiarism testing software. Neither this paper, nor any extract of it, has been previously submitted to an examining authority, in this or a similar form.

I have ensured that the written version of this thesis is identical to the version saved on the enclosed storage medium.

25.11.2022, P. Hinrichs

(Date, Signature)