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Abstract

Local policies can have substantial spillovers both across geographies and markets. We estimate U.S. county level direct and spillover effects of Stay-at-Home-Orders (SHOs) aimed at containing the spread of COVID-19 on mobility and social interaction measures. We propose a modified difference-in-difference regression design, based on contiguous-county triplets. This approach compares treated counties, which adopted the SHO, and neighbors, to the neighbor's neighbors, which we term hinterland, counties. We find that mobility in neighboring counties declined by a third to a half as much as in the treated locations. These spillover effects are concentrated in neighbors that share media markets with treated counties. Using directional mobility data, we decompose the spillover decline in mobility into reductions in external visits coming from the treated county and an even stronger voluntary decline in the neighbor county's own traffic. Together, our results provide strong evidence that SHOs operate through information sharing and illustrate the quantitative importance of voluntary social distancing. The finding that the estimated spillovers are in the same direction as the direct effects casts doubt on the prevailing narrative that a more nationally coordinated policy response would have accomplished a greater reduction in mobility and contacts.

JEL Classification: H73, I18, R12

Keywords: COVID-19, Smart-phone-based Mobility Data, Media Markets, Non Pharmaceutical Interventions, Place-Based Policies, Spillovers, Stay-at-Home Orders, voluntary social distancing

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Direct and Spillover Effects from Staggered Adoption of Health Policies: Evidence from COVID-19 Stay-at-Home Orders*

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October 6, 2021

Local policies can have substantial spillovers both across geographies and markets. Little is known about the impact of public health regulations across administrative borders. We estimate U.S. county level direct and spillover effects of Stay-at-Home-Orders (SHOs) aimed at containing the spread of COVID-19 on mobility and social interaction measures. We propose a modified difference-in-difference regression design, based on contiguous-county triplets. This approach compares treated counties, which adopted the SHO, and neighbors, to the neighbor's neighbors, which we term *hinterland*, counties. We find that mobility in neighboring counties declined by a third to a half as much as in the treated locations. These spillover effects are concentrated in neighbors that share media markets with treated counties. Using directional mobility data, we decompose the spillover decline in mobility into reductions in external visits coming from the treated county and an even stronger voluntary decline in the neighbor county's own traffic. Together, our results provide strong evidence that SHOs operate through information sharing and illustrate the quantitative importance of voluntary social distancing. The finding that the estimated spillovers are in the same direction as the direct effects casts doubt on the prevailing narrative that a more nationally coordinated policy response would have accomplished a greater reduction in mobility and contacts.

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1 Introduction

The proximity and connectedness amongst administrative jurisdictions may result in effects of local policies going beyond target areas and spilling over onto neighboring ones. Understanding these spillovers is important for several reasons. First, properly assessing the spillover effects is necessary for adequate quantification of the direct effect in the adopting jurisdictions. Second, it broadens our understanding of these policies' total impact and their optimal design. Finally, it sheds light on the mechanisms through which these policies work. In this paper, we investigate the direct and spillover effects of one particular public health policy - the Stay-at-Home Orders (SHOs) - intended to slow the spread of COVID-19 in the United States.[1]

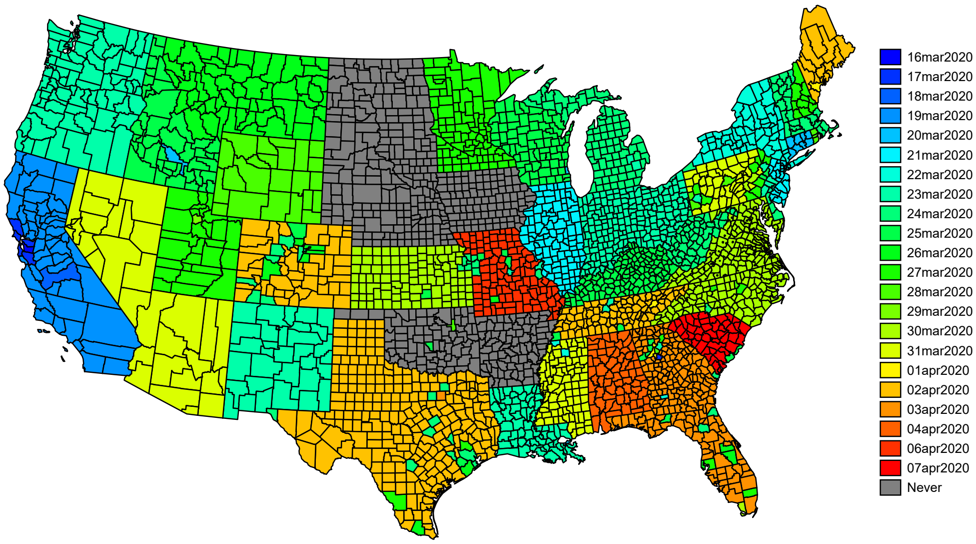
In the United States, non-pharmaceutical interventions (NPIs), such as Stay-at-Home Orders were implemented by local jurisdictions in a staggered manner over time and geographies, without national coordination. Figure 1 shows the staggered SHO adoption dates at the onset of the epidemic across U.S. counties. The figure uses color/shading to highlight the county-level implementation date of SHOs between March 16, 2020 and April 7, 2020, which is the sample period we focus on. With staggered implementation of SHOs across space and over time there is ample scope for spillovers. The intensity of social interaction can change in one county in response to another county's adoption of more stringent NPIs. Spillovers can also arise from lack of compliance, which has been documented empirically by Wright et al. (2020), as non-binding restrictions may induce residents of adopting counties to shift normal activities to neighboring countries. Theoretically, spillovers in policy implementation can occur as agents' actions or behaviors affect indirectly other agents' outcomes, for example through peer effects, strategic interactions, externalities, and other coordination issues.[2]

It is an empirical question whether the staggered NPI implementation across U.S. countries entailed spillovers and in which direction they predominantly went. First, traffic from a SHO

[1] We focus on SHOs because they were the most widely adopted NPI with substantial variation in adoption both over time and across counties. Moreover, in states that "shut down" gradually, SHOs were the most restrictive and often the NPI of last resort. As a result, we would expect them to have the strongest direct effects but, relatively speaking, the weakest spillover effects, making our estimates of spillover effects a conservative lower bound.

[2] This is very important when jurisdictions are very connected, which is very common across US counties. The median county has 15% of its traffic originating outside of its borders.

Figure 1: County-Level Implementation of Stay-at-Home Orders.



Note. The map shows the time distribution of the first stay-at-home order adoption in each county between March 16, 2020 and April 7, 2020. Deep blue/colder tones indicate earlier implementation dates.

implementing county to a neighboring county can be affected. On one hand, an enforced shutdown of establishments in one county could lead its residents to frequent establishments in a neighboring county instead. This perverse spillover has often been cited as the cost of America's decentralized approach to the COVID-19 fight (Holtz et al., 2020). Conversely, spillovers can work in the same direction as the SHO. Residents who traveled to the neighboring county from the SHO implementing county could reduce such visits in voluntary compliance with the order in their own county. Second, internal movement in neighboring counties can also be affected, as residents update their beliefs about the severity of the infection risk in their county as a result of the SHO adopting county's actions.

This study offers the first robust empirical estimates of the direct and spillover effects of Stay-at-Home Orders in the United States and of the mechanisms driving these spillovers. To identify direct and spillover effects of NPIs on mobility, we adopt an identification strategy based on comparing adjacent counties in contiguous county triplets, in which one county adopted a SHO before others. We find evidence of spillovers in the same direction as the direct effects: in response to a

SHO, traffic in neighboring counties drops by about a third to a half as much as traffic in adopting counties, when compared to those further afield, which we call hinterland counties. These results have two major implications. First, a comparison of adopters with their neighbors significantly understates the direct impact of SHOs. Second, the total effect of these policies goes beyond the adopting jurisdiction's borders. We identify mechanisms and further show that the spillover happens both because residents of the SHO adopting counties reduce (rather than increase) visits to non-adopting neighbors (the external traffic channel), and, more significantly, because the neighbor's own residents voluntarily change their behavior in response to the adopting county's SHO by reducing visits within their own county (the information channel). Finally, we demonstrate that the estimated spillover effects are more pronounced when adopting and neighbor counties share local news information. This confirms the presence of an information channel through which SHOs spill over by sending a signal to residents of neighboring counties.

Our results have important policy implications. Our estimates indicate that even before adopting any policy of their own, jurisdictions experience mobility reductions as a reaction to their neighbors' SHOs. This implies that, contrary to conventional wisdom, coordinated adoption could reduce overall treatment intensity by removing the additional rounds of informational treatment we document. Criticisms of the staggered adoption usually contrast it with a coordinated early adoption of SHOs, which confounds the effect of earlier adoption and coordination. A more reasonable benchmark would be a situation with coordinated policy interventions happening closer to the average SHO implementation date.[3] Under same direction spillovers, this coordinated option would be less effective than a staggered (uncoordinated) approach such as the one followed in the US.

The rest of the paper is organized as follows. In the next section we discuss the paper's contribution in the context of the literature. Section 3 presents the data. Next, we discuss the methodology. Section 5 first reports the estimated direct and spillovers effects and then investigates the mechanisms driving the main result. The last section concludes.

[3] In this case, late adopters like Lee County, SC, which declared a SHO on April 7th, would adopt earlier but early adopters like Alameda County, CA, which declared a SHO on March 16th, would adopt later, converging in the average implementation date. The average implementation date of SHOs during the first wave of NPIs was March 27th

2 Related Literature

This study stands at the intersection of the literatures on place-based local policies, policy spillovers, and the recent and growing research on COVID-19 related policy interventions and their effects on mobility, economic activity, and health outcomes [4].

Several studies raise the possibility of NPI spillovers from a theoretical perspective. For example, Bethune and Korinek (2020) show theoretically that externalities play an important role in limiting voluntary self-isolation in response to COVID-19 as the impact of the individual choice on the aggregate infection risk is not internalized. A similar mechanism can apply to local authorities deciding on NPI implementation with respect to neighboring jurisdictions, with a potential spillover effect from residents of the late-adopter to the early-adopter. Cui et al. (2020) model how the policy of one state influences the incentives that other states face to adopt similar policies. If enough states engage in social distancing, they will tip others to do the same by shifting the Nash equilibrium with respect to the number of states engaging in costly social distancing. Similarly, Beck and Wagner (2020) develop a theoretical model to show that uncoordinated interventions can have spillovers across countries. Whether or not spillovers are significant depends on the degree of social and economic integration across countries. We show empirical evidence of strong spillovers across counties in the US that occur in the same direction as the direct effects.

Our work is closely related to recent studies on mobility during the COVID-19 epidemic in the US. Coven et al. (2020) analyze inter-county mobility changes as a result of the pandemic. They focus on long-term exodus and re-locations across cities in response the local COVID-19 outbreak. We focus on changes in short-term intra- and inter-county mobility, visits, in response to policies designed to mitigate the outbreak. Goolsbee and Syverson (2021) evaluate the effect of SHOs over common commuting zones across state and county boundaries, but do not estimate spillovers. Lin and Meissner (2020) estimate the effect of SHOs at the state level with and without including nearby state SHOs, and conclude that the change in the estimates between these two approaches suggest the presence of spillovers, without estimating them directly. Both papers highlight the modest effect

[4] See Brodeur et al. (2020), Gupta et al. (2020), and Avery et al. (2020) for surveys of the early COVID-19 literature, including on NPI impact evaluation.

they find for SHOs effect on county mobility. This is consistent with a bias coming from positive spillovers on the neighboring counties used as controls to estimate direct policy effects. We estimate both direct and spillover effects of county SHOs, which allows us to account for the downwards bias in an adjacent-county estimation of direct effects and, more importantly, also provide an estimate of the spillovers. We also decompose the spillovers into external traffic and information channels.

Holtz et al. (2020) also study spillovers from SHOs. There are several important methodological differences between our studies that lead to different conclusions, mainly that we find consistent spillovers in the same direction as direct effects, while they find spillovers in different directions depending on the specification. The two studies adopt different approaches with respect to identification. Holtz et al. (2020) attempt to identify average effects of NPIs on mobility for the nation as a whole, while we focus on getting unbiased estimates and identifying specific mechanisms free of selection and other confounders. Specifically, our methods are robust to different parts of the country experiencing different Covid dynamics, and to SHOs causing spillovers of different intensities across time. This also allows us to investigate the mechanism through which these spillovers operate and quantify the relative contributions of two distinct channels. In this investigation, our work contrasts as well by using improved directional data that both controls for measurement errors coming from confusing still and disappearing devices, and by using more accurate measures of directional traffic that differentiate between actual visits to establishments and displacements of people just driving through a location.

Our work contributes to the literature on policy evaluation in the presence of spillovers.[5] The applied econometrics literature has shown that spillovers can lead to biased estimates if they are not properly accounted for. For example, Abadie et al. (2010) evaluates a tobacco control program using synthetic control methods, and Cao and Dowd (2019) show that spillovers can bias direct treatment effects in that setting. Kalenkoski and Lacombe (2013) demonstrate that analyses of minimum wage changes, like the seminal work of Card and Krueger (1994), can suffer from bias

[5] A number of studies have documented spillovers from local policies stemming mainly from mobility, similar to our work. This includes competition effects where local jurisdictions implement policies to compete with welfare policies in nearby locations for fear of losing population (Baicker, 2005; Isen, 2014); displacement effects where implementing jurisdiction displace negative phenomena addressed by the policy to neighboring locations Blattman et al. (2021); and expectation effects where neighboring jurisdictions see policy implementation in nearby locations as a sign that similar policies might be implemented locally in the future (Galletta, 2017).

when spillovers are ignored, especially when contiguous counties are selected as controls due to spatial heterogeneous trends, as in Dube et al. (2010) and Neumark and Kolko (2010). Hanson and Rohlin (2013) shows how using nearby areas as controls to evaluate policies, as analyzed for de-regulation policies for labor in Holmes (1998), banking in Huang (2008), and crime prevention in Blattman et al. (2017), can present upward biases when spillovers are negative. This study contributes to the literature by proposing a jurisdiction-level regression design to control for spillover effects exploiting geographic proximity and connectedness. Many studies have opted for focusing on results that remove neighboring locations to the place-based policy to assuage the concern (Kline and Moretti, 2014). Our analysis provides measures of both the spillover and the direct effect net of the spillover bias; it also quantifies the mechanisms underlying the spillover effects.

3 Data

Our empirical analysis relies primarily on smart phone location data collected by SafeGraph.[6] Couture et al. (2021) show that smartphone mobility data are representative of movement patterns in the United States and match well conventional survey data. SafeGraph location data comes from signals, or 'pings', that identify the location of a particular smartphone at a moment in time. In January 2020, this data contained information on more than 45 million devices, with an approximate coverage of 16% of all smartphones in the United States. Tracking the devices' pings, the data identifies visits to any point of interest (POI). Visits to POIs are aggregated by category (e.g. restaurants, bars, schools, etc.) using Google Places classification, daily or weekly, and, for weekly data, by geographic origin (Goolsbee and Syverson, 2021). Safegraph provides comprehensive coverage of all areas in the US. We describe our county selection process in section 4.1. Data coverage is similar in and out of sample, with 0.107 devices tracked per capita in counties out of sample and 0.113 in-sample, based on pre-pandemic coverage during March and April 2019.

We measure visits flows across locations by tracking the devices' *home location* and the POI visited location. A user's origin, or home, location is defined as a 153m x 153m square where a

[6]The primary source of the data is publicly available at https://www.safegraph.com All constructed variables are documented and available together in the paper replication package on the authors' websites.

user spends her nighttime hours (6pm-7am) over a 6-week period. SafeGraph aggregates these locations in census block groups. We use these residence locations to determine whether visits to establishments are from county residents or external county visitors. Using these home locations, together with the location of POIs visited, we build within- and between-*county* traffic flows and a measure of "connectedness " between any two counties.

We construct both aggregate total mobility indicators and indicators by POI category, at *daily* frequency. Specifically, the mobility variables we construct are: (i) total visits to all non-medical establishments, which captures interactions in consumption and leisure activities; (ii) visits to restaurants; and (iii) visits to retail locations.[7]

We also consider (iv) hours spent at home, which is a broader indicator of social distancing. For each device, SafeGraph data provides the total amount of time spent at home during the day (whether or not these were contiguous), and also reports the median across all devices in a given census block. To construct hours spent at home, we take a device count-weighted average across census blocks to aggregate at the county level. Next, we proxy for interactions at-work by using (v) the fraction of devices working away from home. This variable is constructed by adding the devices that exhibit either part-time or full-time working behavior in a given day and then dividing by the number of all devices tracked. The part-time or full-time working behaviors are identified as devices spending more than 3 hours at one location other than the device's home base (geohash-7) during the 8am-6pm local time period. Finally, we consider (vi) the fraction of households that stay completely at home, without leaving the geohash-7 location in which their home is located. This variable captures both overall social distancing but also reduction in work interactions. Results are based on county-level daily observations over the period from 1 March to 30 April 2020, or the same period in 2019.

For total visits to all non-medical establishments (outcome i above), we also construct *weekly* measures of within-county mobility as well as directional traffic between each pair of counties for the same outcome variables. Both in selecting control counties (which we will call hinterland counties)

[7] In unreported regressions, we also used payroll data for a subsample of establishments to confirm that the decline in visits to restaurants and retail establishments is strongly correlated with a decline in hours worked at such establishments. These results are available upon request.

for our main regression design and for identifying the effect of SHOs on inter-county traffic flows when we inspect the mechanisms, we rely on the pre-pandemic value of a measure of "connectedness " between any two counties. For any two origin (o) and destination (d) counties, we measure overall connectedness between county o and d, C_{od}, as follows:

$$C_{od} = \frac{\omega_{od} + \omega_{do}}{\sum_d (\omega_{od} + \omega_{do})}, \tag{1}$$

where ω_{od} is the total number of visits originated from location o, travelling to destination d. We construct this measure of connectedness between all counties based on pre-COVID traffic flows, adding up weekly traffic between the two locations in March and April in 2019.

We focus our analysis on stay-at-home orders throughout the paper. SHOs are advisory notices that people should shelter at home except for essential reasons. These notices have also been referred to as shelter-in-place orders or lock downs. We focus on SHOs because they were the most widely adopted NPI with substantial variation in adoption both over time and across counties. The data set on the county-level implementation of SHOs merges county-specific and state-specific adoption dates from Keystone Strategy, a consulting firm, and the Johns Hopkins Coronavirus Resource Center data assembled in Killeen et al. (2020).[8] Figure 1 shows the SHO implementation dates. The first counties to adopt a SHO were in the San Francisco Bay area, shaded in deep blues. Over the next several weeks, most counties followed suit. We can see the pattern of statewide implementation, but also substantial intra-state variation, mostly driven by metro areas adopting NPIs ahead of the whole state.

In our investigation of the mechanisms, we include weather controls: temperature, precipitation and snowfall. We obtain these data from NOAA weather stations matched to the closest county based on proximity to its centroid and data availability. When multiple weather stations are available, we use the one with the best coverage. Lastly, we use population data from the 2018 Community Survey to weigh observations in specifications to test for robustness.

[8]Keystone data are available at https://github.com/Keystone-Strategy/COVID19-intervention-data.

4 Methodology

Our main goal is to evaluate the effects of the staggered introduction of NPIs on mobility in US counties. We are interested in measuring the effect on mobility as a proxy for contacts and interactions, which are the fundamental driver of COVID-19 transmissions and whose reduction has been the main objective of NPIs. In an ideal research design, there would be sets of ex-ante identical counties. Simultaneously in each set, one county would be randomly treated with a SHO. Subsequent differences in outcomes of interest could then be unambiguously attributed to the order. Changes in outcome variables in other counties would measure spillovers, while changes in outcome variables in the treated county would measure the direct effects.

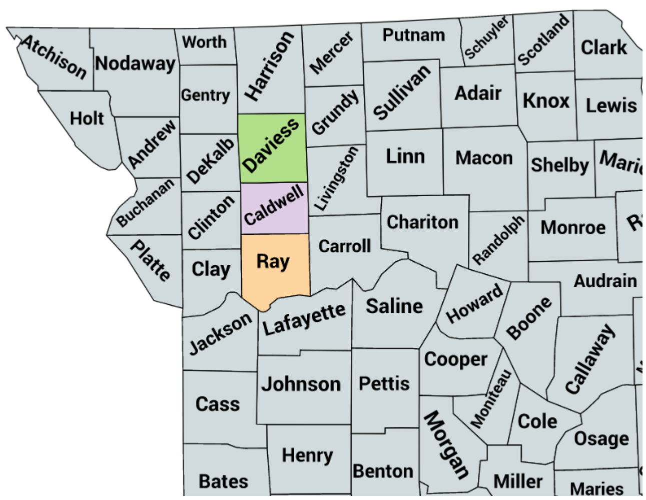
Locations adopt NPIs when the expected benefits of restricting mobility in terms of public health outweigh the expected costs from reductions in economic activity. Differences in SHO adoption and the timing of these orders are driven by two channels: either some locations are expecting worse health outcomes, or their economies are less likely to be affected by the restrictions, or both. As a result, a naive comparison of mobility before and after adoption could attribute an impact to a specific county's SHO, whereas in fact it was caused by a national trend, a seasonal pattern given geography, differences in economic activity or the perceived costs of shut-downs, or another related factor. Locations that are connected geographically (contiguous counties) are more likely to experience similar economic activity and similar (perceived) public health conditions.

4.1 Identifying Spillovers

To identify direct and spillover effects of NPIs on mobility, we adopt an identification strategy based on comparing adjacent counties in contiguous county triplets, in which one county adopted a SHO *earlier* than others. We call this research design identification by contiguous counties for brevity. The critical identifying assumption is that contiguous counties share unobserved time-varying characteristics and thus residual variation in ex-post outcome variables can be attributed to the SHO treatment.

Our design relies on comparing outcomes in counties that implemented a SHO with adjacent and non-adjacent nearby counties that did not for *at least* 4 days after the treated county's implementation. Following Huang (2008), we identify sets of three counties, or triplets. The *treated* county in the triplet adopts the SHO. The *neighbor* county shares a border with the treated county but does not adopt a SHO until at least 4 days after the treated county's adoption date. The hinterland or *control* county shares a border with the neighbor county but does not share a border with the treated county, nor does it share a border with any other county that adopts a SHO for at least four days after the treated county's adoption date.

Figure 2: Contiguous County Triplet Example



NOTE. This map shows a subset of counties in Missouri with one of the county triplets we select for our empirical analysis as an example. Ray County is a treated county because it adopted a stay-at-home order on March 25, 2020. Caldwell county did not adopt a stay-at-home within 4 days (March 29). We call it a neighbor to the treated county, and it is potentially subject to spillover effects from Ray county's order. Daviess county borders Caldwell county, but it does not border Ray County or any other county that adopted a SHO on or before March 29, nor did it adopt itself a SHO by that deadline. Thus, Daviess is selected as the control county, and we call it hinterland.

Figure 2 zooms in on one such triplet in the northwest of Missouri. Ray County is a treated county because it adopted a stay-at-home order on March 25, 2020. Caldwell county did not adopt a stay-at-home until April 6. Yet, because it shares a border with Ray county, its outcome variables could be subject to spillover effects from Ray county's order on March 25. We call Ray a neighbor county. Daviess county borders Caldwell county, but it does not border Ray county or any other county that adopted a stay-at-home order on or before March 29. It also had not adopted a SHO by March 25, and did not do so until April 6 - not within 4 days from the treated. As a result, we select it as the control county and we call it a hinterland county. It is "close enough" to Ray and Caldwell counties to share their regional characteristics, but is "far enough" from Ray County to be minimally affected by its SHO.[9] To the extent that outcomes change in Ray county more than in Daviess county after March 29, we assume this is a direct effect of Ray County's order. To the extent in which they change in Caldwell county more than in Daviess county after March 29, we assume that this is a spillover effect of Ray county's order.

To construct the set of country triplets, we begin with a list of all adjacent county pairs from the U.S. Census Bureau. We then eliminate adjacencies over large bodies of water (e.g. counties in Wisconsin bordering counties in Michigan across Lake Michigan). This step leaves us with 18,870 county pairs. Next, we retain only those pairs in which one county adopted a SHO at least 4 days after the other, if it did it all. The choice of time interval between adoption days involves a trade-off. Selecting a small number results in fewer days over which we can estimate a spillover effect, because the neighbor county may receive direct treatment right after. This not only reduces power, but may misstate the total effect if it takes several days to unfold. Selecting a large number of days results in fewer triplets, significantly reducing power. Taking both concerns into account, we choose 4 days.[10] This second step leaves us with 4,289 county pairs. The early adopter is the *treated* county. The late adopter, if it adopted at all, is the *neighbor* county. We then identify hinterland counties from adjacent counties to the neighbor that have not implemented a SHO and that do not qualify as neighbors themselves.

For some pairs, no such county exists – all of the neighbor's adjacent counties either adopted an order within 4 days, or border with others that did so. We eliminate these pairs. For other pairs, there are one or more counties satisfying the criteria to be a hinterland. In this case, we select the one that is "least connected" to the treated county, in the sense of having the lowest flow of traffic between the two counties, pre-pandemic i.e. in March-April 2019. Connectedness is measured as described in section 3, using normal traffic between the two counties in the same

[9]When multiple candidate hinterland counties exist, we select the one that is least connected to the treated county using the connectedness measure discussed above (see more details below).

[10]In the Appendix, we re-do the analysis for 3-day and 5-day intervals, and confirm that our results are robust.

period pre-pandemic. This third step leaves us with 856 triplets. As we already mentioned, to ensure that the hinterland county has not been affected by other counties' prior orders, we further eliminate all pairs in which the hinterland county served as neighbor county in a different pair with an earlier treated date. This fourth step leaves us with 733 triplets.

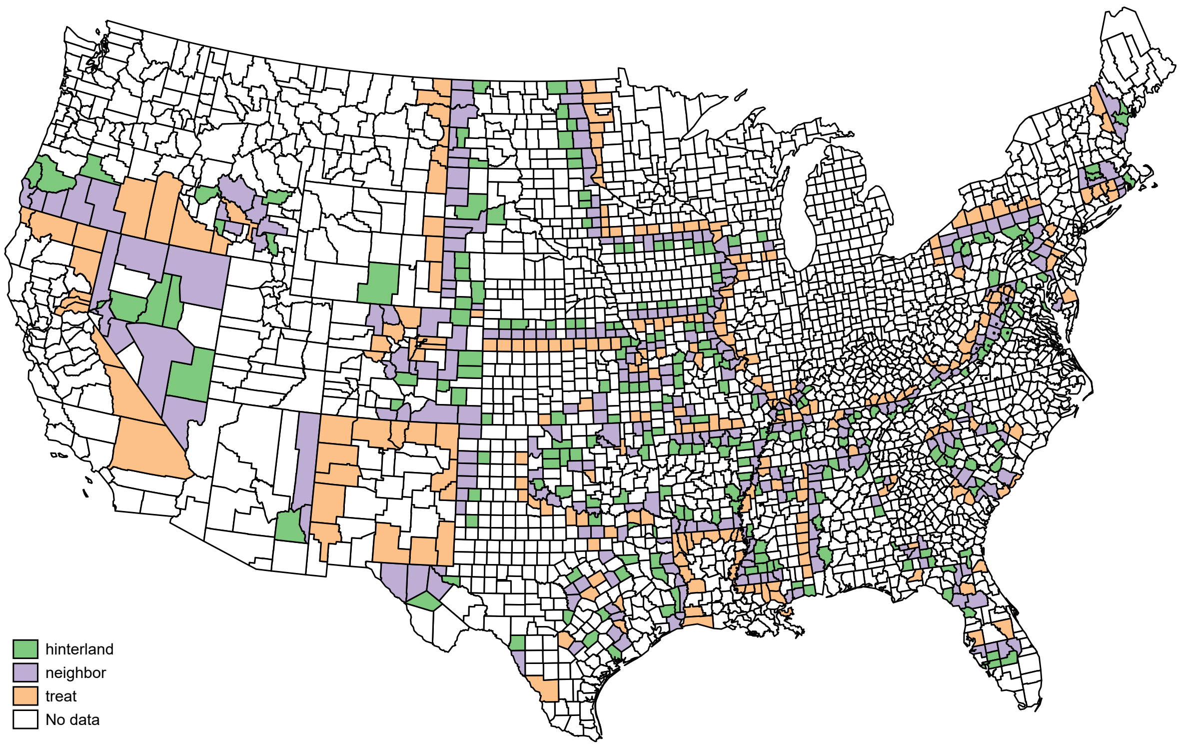
Next, we recognize that we cannot completely rule out the possibility that the hinterland county receives *some* spillover. To minimize this, if a hinterland county appears in multiple triplets, we retain only the triplet(s) with the earliest treatment date. This fifth step leaves us with 659 triplets. Finally, to minimize the likelihood that hinterland counties receive a spillover, we eliminate triplets in which the hinterland is connected to the treated county using the pre-pandemic measure of connectedness described above. This last step leaves us with 474 triplets, which form the basis of our analysis.

The final sample covers 799 counties, about a quarter of all continental US counties, containing approximately a fifth of the total U.S. population and are spread out throughout the country. Figure 3 shows each treated, neighbor, and hinterland county in the final sample. The triplets satisfying the identifying conditions above and making up our sample often lie along state borders, but not always. Table B.4 shows characteristics for the resulting 799 counties from the 2018 American Community Survey, along with the comparable characteristics for those counties that are left out of our sample after our triplet selection procedure. The counties in our sample are more rural: density and transit scores show economically significant differences.[11] The samples are otherwise broadly comparable. Other statistically significant differences at conventional levels reflect tightly estimated small differences in the variables of interest. For example, the counties in our sample present lower median income and higher share of households below the poverty line. They have a higher share of African American population, and lower share of Hispanics. These differences might warrant caution regarding external validity, however we must balance this against the advantage of a more reliable identification. Remaining external validity concerns are assuaged by the directional results, which are estimated on a comprehensive nationwide sample of county

[11] Transit scores come from the Center for Neighborhood Technology. It measures how well a location is served by public transit on a scale from 0 to 10, where 10 indicates higher accessibility. This index incorporates information on scheduled bus, rail, or ferry service.

pairs in equation 4.

Figure 3: Contiguous County Triplet Map



NOTE. The map shows each treated (green-shaded), neighbor (purple-shaded), and hinterland (orange-shaded) counties in the final sample.

Table 1 reports summary statistics for socio-demographic characteristics and all mobility measures in our sample of triplets. The first column shows the mean of the respective variable across all counties in the triplet sample. In columns 4-6 we report the means (with standard deviations in parentheses) for the three types of counties separately. The last two columns show differences in means (with standard errors in parentheses) between the neighbor and the hinterland and the treated and the hinterland counties. The socio-demographic characteristics come from the American Community Survey 2018. Based on these characteristics, the counties that first adopt SHOs in the triplets are more densely populated and more urban than the neighbors and the hinterland counties. The hinterland counties are on average less populated, and they have a higher proportion of individuals aged over 65. However, there are no substantial differences in the proportion of minorities and in the average economic conditions, as measured by median incomes and poverty rates.

Treated counties have more college graduates. They also have more ICU beds. Overall, hinterland and neighbor counties are very similar on most characteristics. On average, devices in the treated and neighbor counties visited more establishments in 2019 relative to hinterland counties. This is consistent with a younger population and a higher transit score. There are no significant differences in the share of devices visiting workplaces and hours spent at home across the three subsamples. In the last row, we show the proportion of treated, hinterland and neighbor counties that adopted SHOs during our observation window. By construction, all treated counties implemented an SHO. About seventy percent of neighbor and hinterland counties did so eventually.

Given this sample of county triplets, we estimate a modified difference-in-differences regression comparing treated (denoted T) to neighbor (denoted N) counties, both relative to the hinterland county. The *treated* county captures the direct effect of the SHOs, while *neighbor* county differential behavior captures the spillover effect. The regression specification is as follows.

$$\begin{aligned} y_{prt} &= \alpha_p + \psi_{c(pr)} + \delta_{\mathrm{T}} T_{pr} + \delta_{\mathrm{N}} N_{pr} + \eta Post_{pt} + \\ &\quad \beta_{\mathrm{T}}(T_{pr} \times Post_{pt}) + \beta_{\mathrm{N}}(N_{pr} \times Post_{pt}) + X_{prt} + \epsilon_{prt}, \end{aligned} \tag{2}$$

where p denotes the county triplet, $r \in \{T, N, H\}$ denotes a given county's role within the triplet, $c(pr)$ is the county specified by the triplet and its role, and $t \in [-4, 4]$ denotes the number of days since the treated county in the triplet adopted its SHO. Here, y_{prt} denotes any of the outcome variables we consider. T_{pr} and N_{pr} are two dummies equal to 1 if the given county is ”treated” ($r = T$) or ”neighbor” ($r = N$) in the given triplet, respectively. $Post_{pt}$ is a dummy equal to 1 after the treated county in triplet p implements the order. X_{prt} is a vector of weather-related controls – temperature, precipitation, and snowfall. We include triplet and county fixed effects.[12] To take into account heterogeneous county triplet size, we weigh observations by total population in the triplet as recorded in the 2018 American Community Survey. Results remain significant when we do not weight the observations. Clustering of the standard errors is at the triplet level.

In support of our basic results, we also conduct an event study, to assess pre-trends and the

[12]Note that some counties may appear in multiple triplets, and so the county fixed effects are not collinear with the pair and role fixed effects.

Table 1: County Sample Characteristics

	All Counties		Subsamples			Differences	
	Mean	St.Dev.	Hinterland	Neighbor	Treated	Neighbor	Treated
Non-Med Visits	6.782	3.727	6.194	6.943	7.109	0.749**	0.914***
			(0.193)	(0.220)	(0.256)	(0.293)	(0.320)
Restaurant Visits	1.588	1.312	1.396	1.666	1.665	0.270**	0.269***
			(0.0551)	(0.0935)	(0.0734)	(0.109)	(0.0918)
Retail Visits	2.312	1.421	2.069	2.334	2.503	0.265**	0.434***
			(0.0793)	(0.0796)	(0.0991)	(0.112)	(0.127)
Hrs Home	9.264	1.207	9.190	9.312	9.270	0.122	0.0793
			(0.0833)	(0.0687)	(0.0714)	(0.108)	(0.110)
Share Completely Home	30.32	4.426	29.92	30.09	30.97	0.171	1.047***
			(0.286)	(0.254)	(0.273)	(0.382)	(0.395)
Share Work	14.59	2.169	14.61	14.66	14.48	0.0597	-0.124
			(0.145)	(0.127)	(0.128)	(0.193)	(0.194)
Population	83866.1	212279.8	36813.7	72176.2	140561.6	35362.6***	103747.9***
			(4010.3)	(9848.7)	(19312.6)	(10633.9)	(19724.6)
Share 65+	19.60	4.470	20.12	19.46	19.29	-0.658*	-0.824**
			(0.269)	(0.243)	(0.310)	(0.362)	(0.411)
Share Black	11.16	16.03	10.86	10.87	11.78	0.0135	0.926
			(1.075)	(0.899)	(1.000)	(1.401)	(1.468)
Share Hispanic	9.296	12.98	8.224	9.439	10.08	1.215	1.858
			(0.725)	(0.763)	(0.869)	(1.053)	(1.132)
Share College+	20.32	8.495	18.98	19.65	22.36	0.668	3.377***
			(0.498)	(0.400)	(0.648)	(0.639)	(0.818)
Median HH Income	50566.3	12604.4	49660.7	50616.2	51317.8	955.5	1657.1
			(801.6)	(648.0)	(890.5)	(1030.8)	(1198.2)
Share Poverty	15.22	5.938	15.47	14.86	15.44	-0.612	-0.0296
			(0.404)	(0.314)	(0.389)	(0.512)	(0.561)
Density (Housing Units)	48.93	106.6	31.73	38.02	77.78	6.292	46.05***
			(4.925)	(3.888)	(9.620)	(6.275)	(10.81)
Transit Score	5.922	13.32	3.043	4.879	9.987	1.836**	6.944***
			(0.566)	(0.634)	(1.146)	(0.850)	(1.278)
ICU Beds / 100K	11.29	16.75	8.609	10.82	14.27	2.216	5.657***
			(0.990)	(0.916)	(1.149)	(1.349)	(1.517)
Ever Adopted SHO	0.793	0.405	0.703	0.692	1.000	-0.0110	0.297***
			(0.0303)	(0.0261)		(0.0399)	(0.0303)
N	799		229	315	255		

Robust standard errors in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

Note. The table reports triple counties' summary statistics for mobility measures (top panel) as well as relevant demographic, economic, and geographic characteristics of sample counties (bottom panel). Mobility measures are computed for March-April 2019 to estimate typical pre-SHO mobility during the time of year in our main sample. Numbers of visits are per 100 people of the destination county's 2018 population. Shares are in percent. "Ever Adopted SHO" is an indicator for whether the county adopted a SHO at any point during March-April 2020. It is equal to 1 for all treated counties by definition. The table reports the sample average, standard deviation, the 1st and the 99th percentiles for the whole sample. It also reports conditional means for each county type, as well as differences in sample means.

dynamics of policy impact. We use the following dynamic specification:

$$
\begin{aligned}
y_{prt} \;=\;& \alpha_p + \psi_{c(pr)} + \delta_{\mathrm{T}} T_{pr} + \delta_{\mathrm{N}} N_{pr} + \sum_{s \in \mathcal{S}-\{-1\}} \eta_s D_{p(t+s)} + \\
& \sum_{s \in \mathcal{S}} \beta_{\mathrm{T},s}(T_{pr} \times D_{p(t+s)}) + \sum_{s \in \mathcal{S}} \beta_{\mathrm{N},s}(N_{pr} \times D_{p(t+s)}) + X_{prt} + \epsilon_{prt} \qquad (3)
\end{aligned}
$$

where $s \in \mathcal{S} = \{-4, -3, \ldots, 4\}$ indexes the day relative to the treated county's treatment date, with un-interacted $D_{p(t-1)}$ dummies omitted. Clustering and weighting is consistent with the main specification.

4.2 Identifying the Mechanisms

When a treated county implements a SHO, it can affect the mobility and social interactions in the neighbor county through two distinct channels. First, the SHO can affect the visits to the establishments in the neighbor county originating in the treated county. We call this the external traffic channel. The sign of this first effect is a priori ambiguous. On the one hand, the closure of establishments in the treated county may lead its residents to seek alternatives nearby, increasing visits to the neighbor county. On the other hand, its residents may choose to, in fact, stay at home, reducing visits both to establishments within its own county and to neighboring ones. Second, the SHO can also induce changes in internal mobility, which are visits to the establishments in the neighbor county originated in the same neighbor county itself, through voluntary social distancing as a reaction to learning about the SHO implemented in the treated county. We label this the information channel. We expect the sign of this second effect to be in line with the direct effect – for example because the SHO raises the neighbor county residents' awareness about the pandemic.

In section 5.2, we investigate these two alternative channels and quantify their relative importance by estimating two additional regression specifications. First, to assess the presence of the information channel, we split the triplet sample into two sub-samples. The first sub-sample consists of triplets where the treated and neighbor counties share the same local television news networks, i.e. they are in the same designated market area (DMA).[13] The remaining triplets are in the sec-

[13]DMAs, often referred to also as "media markets," are areas that share the same local TV network affiliates, newspapers, and other media. Locations in the same media market are expected to share exposure to common media,

ond sub-sample. We then run the same analysis described above on each sample and compare the results. If our information channel is present, we should see stronger spillovers in the sample where treated and neighbor counties are in the same DMA.

Second, we analyze directional traffic flows. The DMA analysis enables us to see the extent to which information transmission drives the spillover effects. But it does not let us compare the relative strengths of the external traffic and information channels because we do not know where the visits to a county's establishments originate. To compare the relative strength of the two channels, we exploit an alternative set of mobility data from Safegraph where we can observe directional mobility flows from one county to another. Unfortunately, these data are available only at the weekly, rather than the daily, frequency. This prevents us from using our demanding $\pm$ 4 day criterion for selecting contiguous county triplets. Were we to construct a sample relying on sufficient distance between adoption times in *weeks*, we would lose all our statistical power.To overcome this challenge, we adopt a staggered difference-in-difference approach in a panel setting where the cross-sectional unit of observation is a county pair p. We then evaluate the change in directional traffic – traffic from the first (origin) county in the pair to the second (destination) one – as a result of either county's adoption of SHOs.

We construct a set of all non-same county pairs in the United States (approximately 9 million) and restrict this sample as follows. First, we eliminate *disconnected* pairs i.e. pairs that had no inter-county traffic in the corresponding March-April pre-pandemic period in 2019. This step leaves us with 93,647 county pairs. As long as we can detect any traffic between a pair of counties in the period March-April 2019, that pair is retained in this sample. Next, we restrict the sample to all pairs in which the destination implements a SHO in the week following origin at the earliest, if it ever does. This is the most comparable conditioning to the contiguous county design because we are considering the effect of a county's SHO on other counties who have not yet adopted their own. This second step leaves us with 28,593 county pairs. Here, origin county, the early adopter, is the *treated* county. Destination county, the late (if ever) adopter, is the control county. While this specification cannot identify spillovers as sharply as the contiguous county regression design,

which can change how they react to neighboring SHOs (Bursztyn et al., 2020) it has the distinct advantage of speaking to the question of why we observe the spillovers that we estimate, including magnitude and sign.

To implement this strategy, we look at two dependent variables measuring traffic in March-April 2019 and March-April 2020. First, we consider traffic flows from origin to destination counties in each pair p. Traffic flow from origin county $o(p)$ to destination county $d(p)$ is the sum of visits to all non-medical POIs located in county $d(p)$ by devices whose home location has been identified to be inside county $o(p)$. Effects of SHOs in this variable is evidence of the external traffic channel of spillovers. Next, we consider the destination county's own internal traffic, defined as the sum of all non-medical visits to POIs located inside a county $d(p)$ from devices with an identified home location also located inside that county, as described in Section 3. Impacts on this variable are consistent with the information channel, also explored in the DMA split mentioned earlier.

The regression specification is as follows.

$$y_{pt} \quad = \quad \beta_o Post_{o(p)t} + \beta_d Post_{d(p)t} + \eta_{pw(t)} + \xi_{r(d(p))t} + \gamma X_{d(p)t} + \epsilon_{pt}, \tag{4}$$

where p denotes the county pair, $o(p)$ and $d(p)$ denote the pair's origin and destination counties, respectively. $r(d(p))$ denotes the Census Division, to which the county $d(p)$ belongs. t denotes the calendar week (e.g. 2019 Week 10), while $w(t)$ denotes the ordinal week of the year (e.g. week 10).

Here, $\text{Post}_{o(p)t}$ is a dummy equal to one if the origin county adopts the SHO within week t or after. For each of the two outcome variables, its coefficient β_o is our primary coefficient of interest. It represents the effect of the origin county's SHO on the outcome variable. Similarly, $\text{Post}_{d(p)t}$ is a dummy that takes the value of one when the destination county $d(p)$ in the pair adopts a SHO within week t or after, which necessarily happens after the origin county (if at all) by construction. It is important to account for a destination's county own SHO so that we do not incorrectly attribute any of the post-origin SHO decline in traffic to the origin's order when it is caused by the destination's own later order.

We include two sets of fixed effects to account for confounding sources of variation. First, $\eta_{pw(t)}$ is a pair $\times$ week fixed effects, representing the average traffic from origin to destination of a given pair in a given ordinal week (e.g. traffic from Ray to Caldwell County in Week 10 of a year). Including this fixed effect accounts for pair-specific seasonal patterns affecting traffic.[14] Second, $\xi_{r(d(p))t}$ is a destination county's census division $\times$ time fixed effect, representing the average traffic to a given census's division county in a given calendar week (e.g. traffic to counties in West North Central Census Division in Week 10 of 2020). Including this fixed effect controls non-parametrically for regional time trends in mobility, which is important to the extent that in 2020 individuals reduced their mobility voluntarily at different rates in different regions as COVID-19 spread. Lastly, $X_{d(p)t}$ are parametric weather controls: weekly averages of temperature, precipitation, and snowfall in the destination county.

We present results for two weighting schemes. In one, we weigh observations equally. Coefficients from the unweighted regression can be interpreted as average effects. In the other, we weigh observations by our connectedness measure between county o and d, C_{od}, described in equation 1. In other words, county pairs that were more connected in 2019 receive higher weights. Coefficients from the weighted regression can be interpreted as aggregate effects. We present both sets of results to show robustness. Errors are clustered at the destination county level in all specifications.

5 Results

First, we report and discuss estimated direct and spillover effects of SHOs based on the contiguous counties regression design specified in equation 2. Next, we explore the transmission mechanisms.

5.1 Direct and Spillover Effects of Stay-at-Home Orders

Table 2 reports the results for our main specification, which uses tight intervals of 4 days around the adoption date to estimate the mean change in the outcome variables between the pre-period (-4 to -1 days before treated county's SHO) and the post-period (0 to 4 days after). The coefficients of interest are β_T and β_N from equation 2. They measure the direct and spillover effects on the outcomes of interest, y_{prt}, of SHOs implemented in the treated county in the triplet p (direct effect), and in the neighbor county (spillover effect), relative to the hinterland county that serves as control.

In the implementing (treated) county, a SHO directly leads to lower mobility and interactions as

[14] Recall each ordinal week includes observations for 2019 and 2020.

Table 2: Contiguous Counties Estimation Results for Stay-at-Home Orders

	(1) **Log Total Visits**	(2) **Log Restaurant Visits**	(3) **Log Retail Visits**	(4) **Hrs Home**	(5) **Frac Work**	(6) **Frac Completely Home**
$Post_{pt}$	0.011	0.029	-0.024	0.957***	0.003	0.005
	(0.022)	(0.033)	(0.022)	(0.130)	(0.002)	(0.003)
$T_{pr} \times Post_{pt}$	-0.104***	-0.090***	-0.105***	0.587***	-0.011***	0.020***
	(0.011)	(0.013)	(0.012)	(0.074)	(0.001)	(0.0018)
$N_{pr} \times Post_{pt}$	-0.044***	-0.051***	-0.043***	0.297***	-0.006***	0.008***
	(0.015)	(0.017)	(0.016)	(0.071)	(0.001)	(0.001)
No. of Obs.	12,798	12,726	12,780	12,798	12,798	12,798
R^2	0.990	0.985	0.991	0.810	0.241	0.684
R^2 (within)	0.071	0.058	0.076	0.223	0.022	0.148

Notes: The specification is given by equation 2. All regressions include county and triplet fixed effects, as described in the text. Coefficients on T_{pr} and N_{pr} are omitted because they are collinear with county fixed effects. Coefficients on weather controls are also omitted. Standard errors clustered at the triplet level are in parentheses. Sample period: [-4,4] days around treated county's SHO order adoption in March and April 2020. Restaurant and retail visits data is missing for a small number of county-days, explaining the lower number of observations in those columns. * p<.10, ** p<.05, *** p<.01.

measured by total visits to non-medical establishments, more time spent at home, a higher fraction of households spending the day at home, and a smaller fraction of time at work, either part- or full-time (first table row). Visits to restaurants and retail establishments decline as well. These effects are highly significant statistically and have the expected sign. For instance, for the average treated county, the estimated coefficient implies a 10.4 percent decline in total visits after the order.[15] The magnitude of the estimated *direct* effects are higher than comparable estimates in the literature that do not take spillovers into account, (Crucini and O'Flaherty, 2020) , Lin and Meissner (2020), and Goolsbee and Syverson (2021). The post-treatment difference between treated and control counties is indeed due to a response in the treated, rather, than control counties. The coefficients on $Post_{pt}$, are an order of magnitude smaller and insignificant for all outcome variables except for hours home. The positive coefficient for hours home indicates that all triplet residents spent an hour more at home after the order, but residents of treated counties spent an additional 35 minutes at home relative to residents of hinterland counties.

The spillover effects are also sizeable and statistically significant for all outcome variables, ranging from a third to a half the size of the corresponding direct effects.[16] Importantly, the

[15] Here observations are weighted by the triplet population.

[16] In Appendix A we explore the robustness of these estimates by varying the event window (3 day and 5 day

direction or sign of the spillover effects is the same as the direct effects for all outcomes. There is no aggregate evidence of "perverse" spillovers in the sense of increases in mobility in neighbor counties in response to a SHO in the treated counties. This implies that the net effect of the two channels spelled out above, the external traffic channel and the information channel, is a reduction in traffic to neighbor counties.

This result is important because it provides evidence against the commonly held view that a staggered and uncoordinated adoption of SHOs across U.S. counties might have contributed to a higher levels of social interaction in the United States than in countries with a more centralized government (e.g., Lin and Meissner (2020)). While it is true that coordinated early adoption of SHOs would have promoted greater social distancing and likely led to fewer cases and deaths (Renne et al., 2020), this counterfactual confounds the time of adoption and the coordination in adoption times. A more adequate comparison to evaluate the role of coordination is a mean-preserving contraction of adoption times, a situation in which SHOs are implemented with less variance in dates, but with the same average date. This contraction deters from the the social distancing spillover effects of the SHOs, as some of the spillover effect is lost from the earlier counties implementing closer to the average date.

We now turn to the discussion of dynamic effects. Figure 4 shows the event study results for each outcome variable, based on the dynamic specification in equation 3 and the final sample of county triplets.[17] It reports the estimated direct and spillover effects four days before and four days after the SHO adoption. The estimated impacts are the coefficients from the regression of our outcome variables on the interaction between the county status in the triplet and eight different implementation dummy variables. These variables take value one on each of the (four) days before and after the day in which the SHO is implemented in the treated county. The implementation day is denoted with 0. Implementation dummies interacted with the Treated county are plotted as direct effect. Those interacted with the Neighbor county are plotted as spillover effect.

The figure shows that after the SHO implementation, spillover effects are detectable in all

intervals) and by restricting the analysis to subsets of triplets. The estimates are broadly the same as those reported in Table 2.

[17]Table B.6 in the appendix shows the regression coefficients.

outcomes and are statistically different than zero in all cases for at least one or two days after the implementation. For total visits, hours at home, the fraction of devices that stay completely at home, visits to restaurants, and visits to retail establishments, the spillover effects are smaller than the direct effects but are significant and in the same order of magnitude. More importantly, they occur in the same direction, i.e. the SHOs' direct effect is to decrease people's mobility and hence social interactions; the spillover effect also decreases the mobility and overall social interactions of neighboring counties. For the fraction of devices that can be traced to a workplace, not only do the spillover effects go in the same direction, but they are as large as the direct effects.

While the sign of the spillover effects is aligned with the sign of the direct effects, the identification by contiguous county triplets does not speak to the specific mechanisms through which they come about. We are also interested in establishing whether the underlying channels push in the same or opposite direction.We now turn to unpacking the mechanisms behind the spillover effects.

5.2 Stay-at-Home Orders Spillovers: External Traffic and Information Channels

In this section we explore the mechanisms driving the estimated spillover effects and assess the relative importance of the two channels discussed. We rely on two different research designs. First, we investigate the information channel directly by repeating our contiguous county analysis in section 5.1 on two separate sub-samples: one where treated and neighbor counties share local news, and those in which they do not. Second, we exploit directional mobility data to identify where visitors of a given county are coming from, and assess the relative magnitudes of reductions in external traffic and voluntary reduction in local traffic in explaining spillovers. Because precise directional data that identifies visits to establishments, as opposed to people passing by the county, is not available at a daily frequency, this requires us to set aside the contiguous county ("triplet") strategy and adopt a staggered difference-in-difference strategy.

5.2.1 Stay-at-Home Orders Spillovers and Media Markets Areas

We conjecture that sharing a media market with, and hence receiving the same local news as a treated county, can make residents of a neighbor county more aware of the treated county's SHO, as well as the public health circumstances leading to its adoption, likely leading them to *voluntarily* intensify social distancing behavior. By considering county triplets whose treated and neighbor counties are located in the same DMA, we can control for information-sharing. DMAs are local media markets used to determine advertisement targets and for other business purposes. The areas are determined by Nielsen, a major national marketing corporation, aggregating locations that share the same local television stations and in which local TV stations have a significant share of the total market in terms of total hours viewed. There are 210 DMA regions, covering the entire continental United States, Hawaii, and parts of Alaska. Despite the surge in online access to news, recent Pew Research Center surveys show that nearly 50% of households still get their news from local TV stations (Pew Research Center, 2019).

We split our triplet sample into those in which neighbor and treated county share the same media market and those that do not.[18] We end up with two similarly sized subsamples of 254 and 230 triplets, respectively. We then run the same regression as in section 5.1, equation 2, for the two subsamples.

Table 3 reports the results, comparing them with those obtained with the full sample for each outcome variable for ease of comparison. While the direct effects are similar in both subsamples, these estimates show that the spillover effects are driven by counties that share the same DMAs, confirming the presence of an information channel in our main results. Total visits drop by a highly significant 5.8% in the neighbor county when the neighbor is in the same DMA as the treated county, while only dropping by 1.8% when they are in different DMAs, significant only at a 10% level. The same discrepancy exists for restaurant visits and hours home, while the spillovers to neighbor counties in different DMAs are entirely insignificant for retail visits, with point estimates

[18]Note here that hinterland county is ignored by the criterion for the sample split. The criterion identifies whether or not the neighbor and the treated counties share the same source of local news. All but one of the hinterland counties we consider are in DMAs that are different from the treated and the spillover county DMAs. Results from specifications omitting the triplet in which the hinterland shares DMA with the other two counties are extremely similar and available upon request.

Table 3: Stay-at-Home Direct and Spillover Effects by Designated Market Area

	(1) All	(2) Same DMA	(3) Different DMAs	(4) All	(5) Same DMA	(6) Different DMAs	(7) All	(8) Same DMA	(9) Different DMAs
	Log Total Visits			**Log Restaurant Visits**			**Log Retail Visits**		
$Post_{pt}$	0.0110	-0.00145	0.0359	0.0290	0.00897	0.0694	-0.0236	-0.0392	0.00799
	(0.0221)	(0.0304)	(0.0225)	(0.0328)	(0.0458)	(0.0264)	(0.0221)	(0.0307)	(0.0200)
$T_{pr} \times Post_{pt}$	-0.104***	-0.109***	-0.0971***	-0.0906***	-0.0884***	-0.0970***	-0.105***	-0.107***	-0.102***
	(0.0110)	(0.0155)	(0.0111)	(0.0136)	(0.0193)	(0.0126)	(0.0126)	(0.0173)	(0.0149)
$N_{pr} \times Post_{pt}$	-0.0446***	-0.0578***	-0.0180*	-0.0513***	-0.0636***	-0.0266*	-0.0436***	-0.0576***	-0.0148
	(0.0158)	(0.0220)	(0.0109)	(0.0173)	(0.0242)	(0.0135)	(0.0162)	(0.0221)	(0.0142)
No. of Obs.	12,798	8,208	4,590	12,726	8,163	4,563	12,780	8,208	4,572
No. of Triplets	474	304	170	474	304	170	474	304	170
R^2	0.990	0.990	0.989	0.985	0.986	0.982	0.991	0.992	0.991
R^2 (within)	0.0710	0.0845	0.0538	0.0589	0.0680	0.0523	0.0769	0.0934	0.0532
	Hrs Home			**Frac Work**			**Frac Completely Home**		
$Post_{pt}$	0.957***	0.915***	1.030***	0.00314	0.00410	0.000946	0.00528	0.00675	0.00233
	(0.130)	(0.175)	(0.134)	(0.00220)	(0.00284)	(0.00330)	(0.00337)	(0.00457)	(0.00398)
$T_{pr} \times Post_{pt}$	0.587***	0.543***	0.655***	-0.0119***	-0.0132***	-0.00934***	0.0205***	0.0211***	0.0191***
	(0.0745)	(0.0914)	(0.122)	(0.00160)	(0.00220)	(0.00131)	(0.00184)	(0.00248)	(0.00234)
$N_{pr} \times Post_{pt}$	0.297***	0.360***	0.137*	-0.00674***	-0.00828***	-0.00356***	0.00844***	0.0100***	0.00498***
	(0.0716)	(0.0945)	(0.0756)	(0.00154)	(0.00211)	(0.00123)	(0.00182)	(0.00247)	(0.00174)
No. of Obs.	12,798	8,208	4,590	12,798	8,208	4,590	12,798	8,208	4,590
No. of Triplets	474	304	170	474	304	170	474	304	170
R^2	0.821	0.820	0.823	0.241	0.246	0.230	0.684	0.685	0.684
R^2 (within)	0.223	0.206	0.286	0.0229	0.0276	0.0150	0.148	0.162	0.122

Notes: The table reports the results from the estimation of equation 4 splitting the county triplet sample in two: one sub-sample that includes all triplets in which the treated and neighbor counties share the same designated marketing areas (DMAs), and one in which they don't. All regressions include county and triplet fixed effects described in the text. Coefficients on T_{pr} and N_{pr} are omitted because they are collinear with county fixed effects. Coefficients on weather controls are also omitted. Standard errors clustered at the triplet level are in parentheses. * p<.10, ** p<.05, *** p<.01. Sample period: March and April 2020. Restaurant and retail visits data is missing for a small number of county-days, explaining the lower number of observations in those columns.

less than a third of those for same DMAs. The spillovers remain significant for fraction working and fraction completely at home, but the point estimates are less than half of those for same DMAs.

In contrast, direct effects remain highly significant and similar in both subsamples, suggesting that the two subsamples are not substantially different along some other unobservable dimensions that affect mobility. This evidence implies that when a county implements a SHO, their positive spillover effects on the mobility of surrounding locations is driven largely by an information channel.

5.2.2 Spillovers and Directional Mobility

To evaluate the relative importance of the external traffic and information channels we use weekly directional mobility data that takes the POIs and the device home locations into account. The results are based on the specification in equation 4 and estimated on a sample of origin-destination county pairs. Table 4 reports the results. The table reports results for two outcome variables, measured as the log of weekly visits: total non-medical visits by devices residing in the origin county to POIs in the destination county (columns 1 and 2), and visits of devices residing in a destination county to POIs in that destination county (columns 3 and 4). We consider the impact of the origin county adopting a SHO (row 1) and of the destination county adopting a SHO after origin (row 2). We report results with and without weighting observations using pre-pandemic connectedness between origin and destination measured as we discussed previously.

Let's consider first the estimates of the impact of a SHO on the external traffic from origin to destination. The results are shown in columns 1 and 2 of Table 4. The coefficients on the first table row measure the directional spillover effect of SHOs implemented in the origin county on destination counties that have not yet adopted the SHO. The estimate for this outcome implies a reduction of around 21-25% in the external traffic from that adopting location. In this case, there is little difference between weighted and unweighted results (columns 1 and 2). As we can see from the coefficient in the second row, the destination county further contains incoming mobility from the origin county when it implements its own SHO, even when it implements after origin, with an estimated additional decline in traffic of about 28-32%. Because we are restricting the sample to destinations that implement SHOs after origins, this is an incremental additional reduction

Table 4: Impact of Stay-at-Home Order on Internal and External Mobility

	(1)	(2)	(3)	(4)
	Log Visits From Origin To Destination	**Log Visits From Origin To Destination**	**Log Internal Visits In Destination**	**Log Internal Visits In Destination**
Origin adopted SHO	-0.212***	-0.253***	-0.0935***	-0.0839***
	(0.0549)	(0.0854)	(0.0153)	(0.0203)
Destination adopted SHO	-0.276**	-0.324**	-0.112***	-0.124***
	(0.108)	(0.156)	(0.0324)	(0.0318)
N	651,420	651,420	651,420	651,420
R^2	0.860	0.967	0.996	0.995
R^2 (within)	0.00361	0.0217	0.0195	0.0255
Weighted	N	Y	N	Y

NOTE. The table reports the impact of SHOs on directional mobility measures based on the POIs and the device's locations. The results are based on the specification in equation 4. Origin is the implementing county in the pair. Destination is the neighboring county in the pair. The outcome variable in Columns 1 and 2 is the log of the sum of visits of devices residing in the origin county to POIs in the destination county. The outcome variable Columns 3 and 4 is the log of sum of visits of devices residing in a destination county to POIs in that destination county. We consider the impact of the county origin adopting a SHO (first coefficient row) controlling for the destination county adopting a SHO after origin (second coefficient row). We also include parametric controls for temperature, snowfall, and precipitation, county pair × ordinal week fixed effects, and census division × calendar week fixed effects. The sample is restricted to county pairs where destinations implement the SHO after origin, if ever. We report both weighted and unweighted results. Observations are weighted by connectedness measure between origin and destination measured in pre-pandemic times in March and April 2019. Columns (1) and (3) reports weighted results. Errors are clustered at the destination county level in all specifications.

conditional on the spillover effect.

Let's now look at the spillover effects on internal traffic in the destination county (i.e. visits originating and ending in the same county) as a way to complement the DMA evidence on the information channel (columns 3 and 4). The adoption of a SHO in the *origin* county reduces internal traffic in the *destination* county by 8%-9%. This is strong evidence of the information channel at work: it's not just that residents of counties implementing SHOs stop visiting their neighbors; the results also show that the neighbor's own residents stay home more, voluntarily, when connected counties implement SHOs. The evidence in fact is fully consistent with our DMA results suggesting that a SHO in a county sends a signal that affects behavior in connected counties. The impact is larger for the specifications that do not weight observation by pre-pandemic connectedness. This suggests that this channel does not necessarily rely on the destination's residents seeing an unusual decline in visits from an otherwise strongly connected county.

The relative decline of external visits to the destination county from the SHO implementing origin is 2-3 times larger than the decline in the destination county's own internal visits. But to compare these magnitudes, we must take into account the fact that most visits to a county's POIs are internal, i.e by its own residents. The median county experiences 85% of visits by its own residents. Using unweighted estimates, the total spillover effect is $(-0.212)(1-0.85)+(-0.0935)(0.85) = -0.1112$. Of this 11.1% decline, the external traffic channel contributes $(0.212)(1-0.85) = 3.18\%$, while the information channel contributes $(0.0935)(0.85) = 7.95\%$, more than twice as much. Using weighted estimates, the corresponding decomposition yields 3.80% and 7.13% declines, with the information channel still dominating.

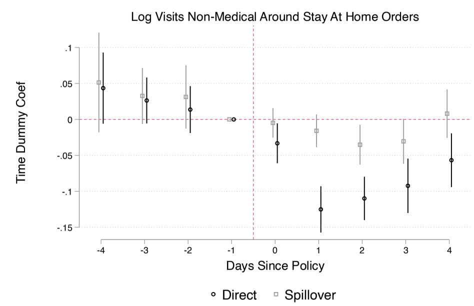
In summary, in terms of the mechanism and its channels, these directional mobility results suggest that both information and external traffic channels are at work. The implementation of a SHO by a county has the effect of reducing the interactions in neighboring counties both by reducing the visitors they send to them, and by inducing a decline in internal mobility. Moreover, taken together with our DMA sample split results, the directional results suggest that the internal mobility decline can be driven by an information channel, consistent with behavioral epidemiological models and the evidence in the literature on voluntary social distancing (Goolsbee and Syverson, 2021).

5.3 Policy Implications

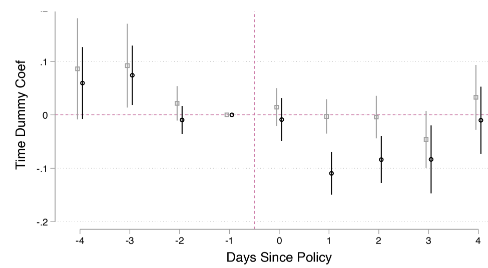
Our results yield two important policy implications. First, NPI policy evaluation ought to control for spillover effects, because they are sizable. Spillovers in the same direction as the main effect, as we find, imply that the total benefits of SHOs are underestimated if spillovers accruing to non-implementing locations are ignored. This is even more critical for methodologies that use nearest neighboring locations as control. In our context of spillovers, this may underestimate the local effect of SHOs in the implementing location. This may explain estimated effects in the literature that are considerably lower than those we find. The intuition and methodology of our analysis applies to the evaluation of health-related and other local policies in which movements of goods and people may imply spillovers.

Second, in the absence of tight national and international coordination, NPI policy design should leverage the signalling role of early adoption that can affect voluntary social distancing. Our results refute a common narrative about perverse effects of staggered adoption of NPIs. A large body of evidence suggests that non-pharmaceutical interventions have had a strong impact on social distancing in the United States. Their staggered implementation has been criticized for potentially detracting from their overall effectiveness as a result of these coordination failures. While it is true that coordinated early adoption of SHOs may have promoted greater social distancing earlier on, and likely led to fewer cases and deaths, this confounds the coordination and earlier adoption aspects. Instead, one can consider the role of coordination exclusively as a mean-preserving contraction of adoption times, a situation in which SHOs are implemented with less time in between, but where the average implementation date is maintained. In this situation, counties that implemented SHOs earlier, like Alameda County, CA where measures were taken on March 16th, would implement closer to the average implementation date, which was March 27th for the first wave of SHOs in spring of 2020. Counties that implemented last, like Lee County, SC with a SHO implemented on April 7th, would implement earlier. Figure 5 shows the observed distribution of the SHOs date under the staggered adoption. The *coordinated early adoption* bars show the distribution that would have been observed had all counties (that implemented a SHO during this first wave) implemented them during the weekend when Alameda County did. This is the benchmark that confounds early adoption and coordination. The *coordinated adoption* bars show the distribution that would have been observed had these counties implemented them during the weekend around the mean adoption date. Our evidence of spillovers working in the same direction as the direct effects of SHOs suggests that this more coordinated policy does not deliver the additional waves of treatment that the spillover brings. In contrast, the staggered implementation itself – which can be seen as a mean-preserving spread of adoption times – promotes, rather than detracts, from the policy goal of reducing interactions.

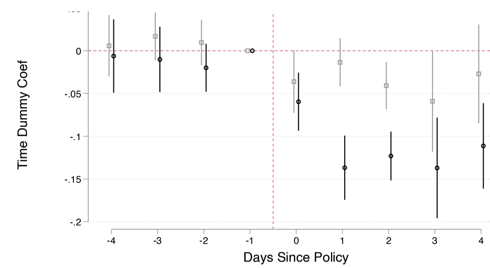
Figure 4: Stay-at-Home Order Direct and Spillover Effects: Event Study Results



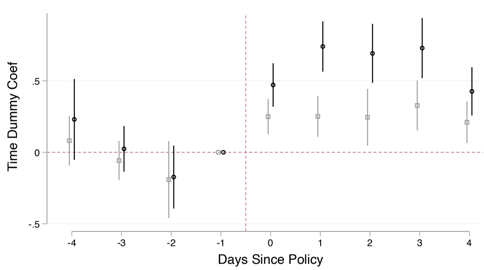
(a) Total Visits



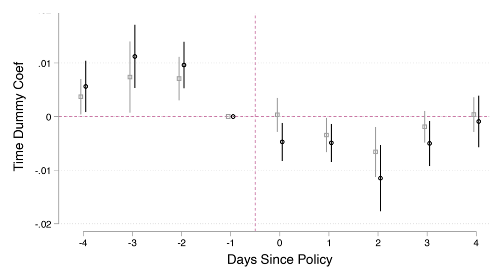
(b) Restaurant Visits



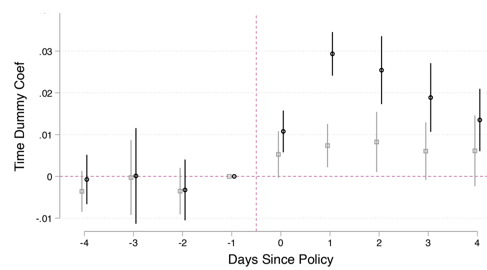
(c) Retail Visits



(d) Hours Home



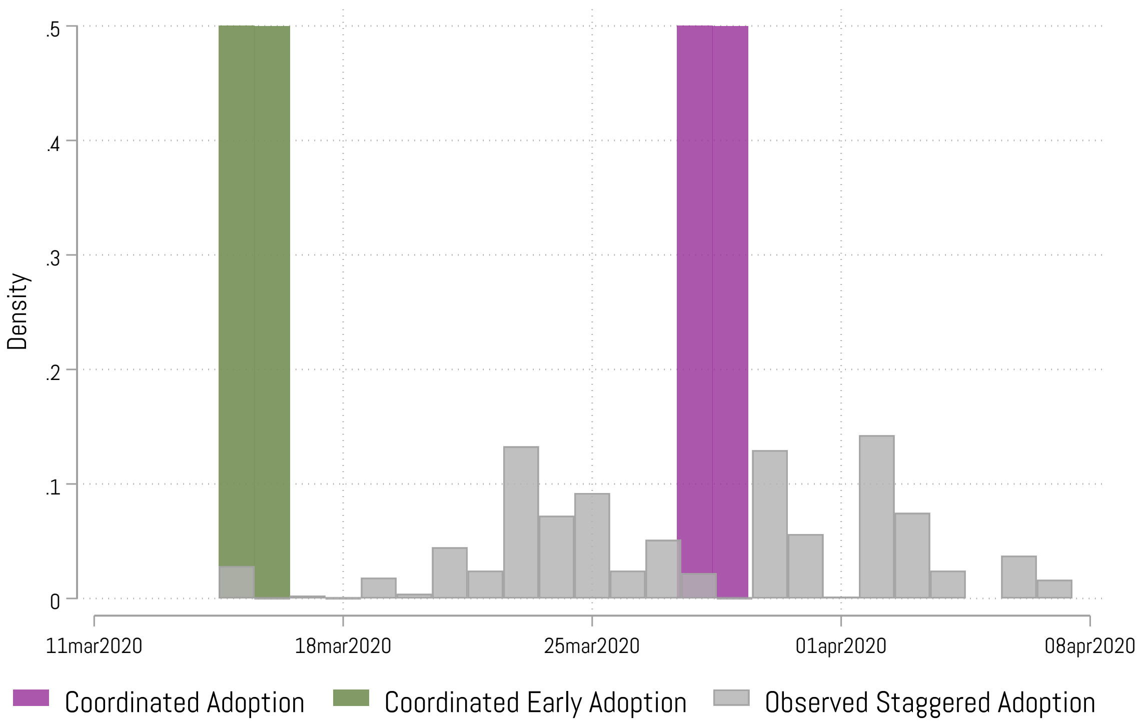
(e) Fraction of Visiting Workplaces



(f) Fraction Completely at Home

NOTE. The figure plots estimated direct and spillover effects in event study format for all outcome variables. The time frequency is daily. The x-axis is in days. $t = 0$ is the implementation day. The y-axis is in percent. The regression specification is explained in the text and it includes county triplet and county fixed effects, as well as weather controls. Observations are weighted by county triplet population. Regression includes County Triple and County FEs. Obs weighted by county triple population. t=-1 Coefs Omitted.

Figure 5: Coordinated, Staggered, and Early Coordinated Adoption of Stay-at-Home Orders Compared



NOTE. Coordinated Adoption shows the situation when all counties would adopt during the weeked when the mean adoption date occurred (March 27th, 2020). Coordinated Early Adoption shows adoption over a weekend, but earlier on when the first SHOs were implemented (March 15th, 2020). The Observed Staggered Adoption shows the distribution of actual SHO implementation dates.

6 Conclusion

In this paper, we estimate the direct and spillover effects of COVID-19 Stay-Home-Orders implemented at the county level in March and April 2020 on smartphone-based mobility data indicators. We propose a modified difference-in-difference research design based on contiguous county triplets in which SHO adoption in one treated county can spillover onto a neighboring non-adopting county, and both are assessed relative to a hinterland control county. We find spillover effects from SHO implementation at the county level that work in the same direction as their direct effects, i.e spillovers reduce the mobility in neighboring counties. These effects are substantial, between a half and a third of the estimated direct effects. We also investigate the mechanism behind these spillovers, testing two potential channels. One direct channel is due to inter-county traffic. The other, indirect, channel is based on information sharing. We explore the information channel by measuring the SHOs spillovers coming from counties that share common media markets and those that do not. We also exploit directional weekly mobility data to estimate the strength of these two channels.

We find that a county's SHO order keeps its residents at home. They do not go out in their own county, nor do they flock to neighboring counties, even if the destination has looser restrictions. This reduction in visits from the adopting county decreases overall mobility of the neighboring counties. This is the external channel of spillovers. The residents of counties neighboring the SHO-implementing one also reduce their own internal traffic, even if they are not directly mandated to do so by a SHO. This information channel is much stronger when information sharing has fewer frictions, which we capture by the presence of common media markets. This is evidence that SHO adoption by surrounding counties promotes awareness of the pandemic's severity and signals or renders salient the recommendation to socially distance, even in the absence of binding mobility restrictions. This evidence is consistent with behavioral models of social distancing in which individuals respond to information about the risks of infection. In sum, we find strong evidence that SHOs promote social distancing not just in the counties in which they are adopted but also in neighboring counties through both an external mobility channel and an information channel affecting *voluntary* behavior. We find that the information channel contributes twice as much to the spillover reduction in mobility implied by the SHO, highlighting important potential benefits of voluntary behaviors in this situation.

Our results yield two important policy implications. First, NPI policy evaluation yields biased estimates unless it controls for spillover effects, especially in methodologies that use nearest neighboring locations as controls. Furthermore, spillovers imply that optimal policy design benefits from incorporating impacts that go beyond local measured impacts. The approach presented in our study can benefit the evaluation of health and other local policies. Second, in the presence of same direction spillovers, a staggered implementation benefits rather than detracts from the overall impact of the policy, through the additional layers of informational treatment that increases voluntary social distancing. This cuts against the common criticism of staggered NPI adoption that took place in the US, often compared with an alternative coordinated early adoption. Such a counterfactual confounds early adoption and coordination. Compared instead to pure coordination, where early adopters adopt later and vice versa, a staggered adoption would achieve a stronger overall impact.

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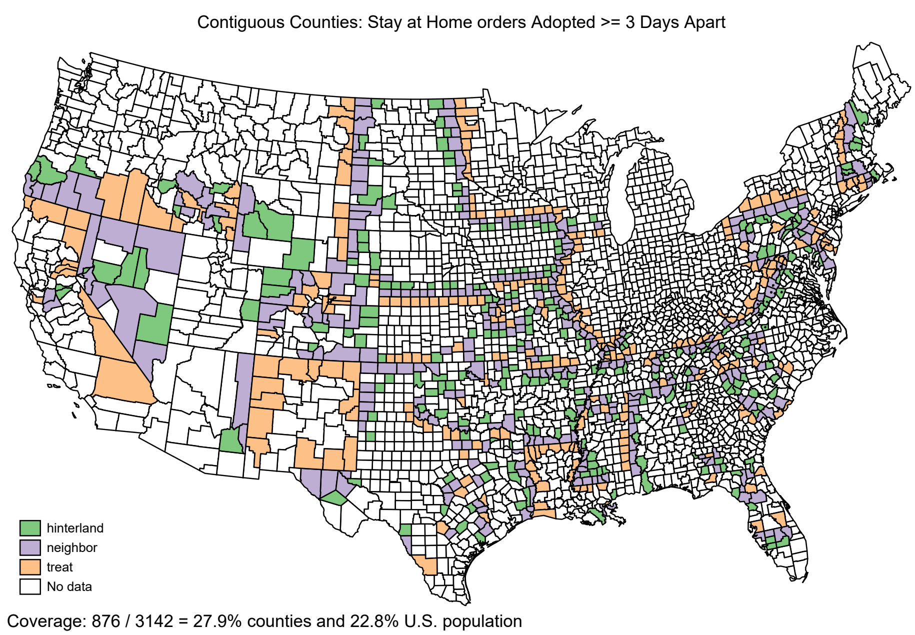
A Robustness Checks

In this section, we present evidence that our results are robust to alternative selection criteria for restricting the set of county triplets used in our main results (Table 2). First, we vary the minimum number of days that need to have elapsed between SHO adoption by the treated county in the triplet, and the neighbor. In the baseline regression, we use 4 days. In Figure A.1 and Table A.1, we show the set of triplets and regression results, when the minimum interval is 3 days. This is a less restrictive criterion that results in more triplets. But, it also requires a shorter post-treatment period to avoid contamination of our results by some of the neighbors adopting their own SHO on day 4. Nevertheless, our results are qualitatively the same, and quantitatively only slightly different.

Figure A.1: Contiguous County Triplets: 3 Day Minimum Gap Between Treated and Neighbor Stay-at-Home Order Adoption Dates



NOTE. The map shows each treated (green-shaded), neighbor (purple-shaded), and hinterland (orange-shaded) counties in the final sample.

Table A.1: Contiguous County Triplets: 3 Day Minimum Gap Between Treated and Neighbor Stay-at-Home Order Adoption Dates

	(1) **Log Total Visits**	(2) **Log Restaurant Visits**	(3) **Log Retail Visits**	(4) **Hrs Home**	(5) **Frac Work**	(6) **Frac Completely Home**
$Post_{pt}$	0.00196	0.0135	-0.0273	0.546***	0.00667**	0.000277
	(0.0234)	(0.0336)	(0.0223)	(0.128)	(0.00300)	(0.00415)
$T_{cp} \times Post_{pt}$	-0.0993***	-0.0928***	-0.101***	0.680***	-0.0120***	0.0232***
	(0.0103)	(0.0140)	(0.0128)	(0.0793)	(0.00195)	(0.00248)
$N_{cp} \times Post_{pt}$	-0.0340***	-0.0388***	-0.0393***	0.330***	-0.00640***	0.00805***
	(0.0126)	(0.0141)	(0.0136)	(0.0823)	(0.00188)	(0.00166)
No. of Obs.	10,941	10,885	10,927	10,941	10,941	10,941
R^2	0.990	0.986	0.992	0.837	0.253	0.672
R^2 (within)	0.0631	0.0503	0.0723	0.157	0.0246	0.140

Notes: These results are equivalent to Table 2 in the paper, but with the sample constructed using a 3-day minimum difference between adoption dates of treated and neighbor counties, instead of the 4 days used in the paper. The specification is given by equation 2. All regressions include county and triplet fixed effects, as described in the text. T_{cp} and N_{cp} are omitted because they are collinear with county fixed effects. Coefficients on weather controls are also omitted. Standard errors clustered at the triplet level are in parentheses. Sample period: [-4,4] days around treated county's SHO order adoption in March and April 2020. Restaurant and retail visits data is missing for a small number of county-days, explaining the lower number of observations in those columns. * p<.10, ** p<.05, *** p<.01.

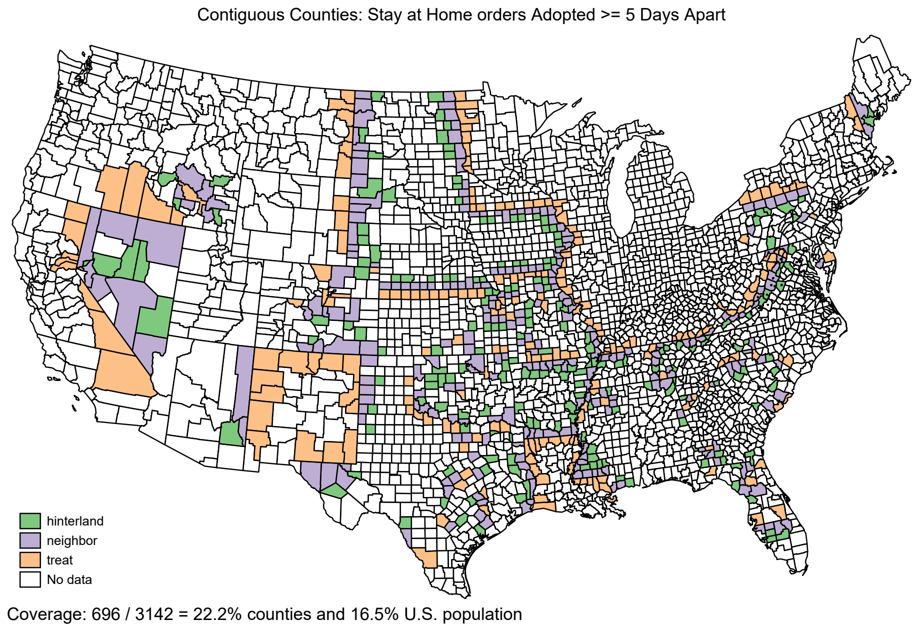
In Figure A.2 and Table A.2, we repeat this analysis for 5 day intervals. This is a more restrictive criterion that results in fewer triplets and a longer sample. Again, results are robust.

Table A.2: Contiguous County Triplets: 5 Day Minimum Gap Between Treated and Neighbor Stay-at-Home Order Adoption Dates

	(1) **Log Total Visits**	(2) **Log Restaurant Visits**	(3) **Log Retail Visits**	(4) **Hrs Home**	(5) **Frac Work**	(6) **Frac Completely Home**
$Post_{pt}$	0.0121	0.0159	-0.0260	1.267***	-0.000884	0.0116***
	(0.0231)	(0.0406)	(0.0257)	(0.139)	(0.00151)	(0.00284)
$T_{cp} \times Post_{pt}$	-0.108***	-0.0737***	-0.108***	0.496***	-0.0104***	0.0216***
	(0.0125)	(0.0139)	(0.0108)	(0.0894)	(0.00130)	(0.00208)
$N_{cp} \times Post_{pt}$	-0.0606***	-0.0560***	-0.0524***	0.314***	-0.00695***	0.0112***
	(0.0225)	(0.0215)	(0.0182)	(0.0772)	(0.00141)	(0.00268)
No. of Obs.	13,563	13,475	13,541	13,563	13,563	13,563
R^2	0.990	0.984	0.992	0.818	0.224	0.656
R^2 (within)	0.0830	0.0704	0.0828	0.297	0.0273	0.157

Notes: These results are equivalent to Table 2 in the paper, but with the sample constructed using a 5-day minimum difference between adoption dates of treated and neighbor counties, instead of the 4 days used in the paper. The specification is given by equation 2. All regressions include county and triplet fixed effects, as described in the text. T_{cp} and N_{cp} are omitted because they are collinear with county fixed effects. Coefficients on weather controls are also omitted. Standard errors clustered at the triplet level are in parentheses. Sample period: [-4,4] days around treated county's SHO order adoption in March and April 2020. Restaurant and retail visits data is missing for a small number of county-days, explaining the lower number of observations in those columns. * p<.10, ** p<.05, *** p<.01.

Figure A.2: Contiguous County Triplets: 5 Day Minimum Gap Between Treated and Neighbor Stay-at-Home Order Adoption Dates



NOTE. The map shows each treated (green-shaded), neighbor (purple-shaded), and hinterland (orange-shaded) counties in the final sample.

Next, we address concerns that could arise from the differences in some demographic, economic, or geographic characteristics between treated, neighbor, and hinterland counties, as reported in Table 1. These differences pose an identification challenge if they cause differential trends in mobility, which could lead us to mistakenly attribute differential post-treatment dynamics to treatment. If this were the case, then triplets whose counties are most heterogeneous in their fixed characteristics should have strongest estimated treatment and spillover effects.

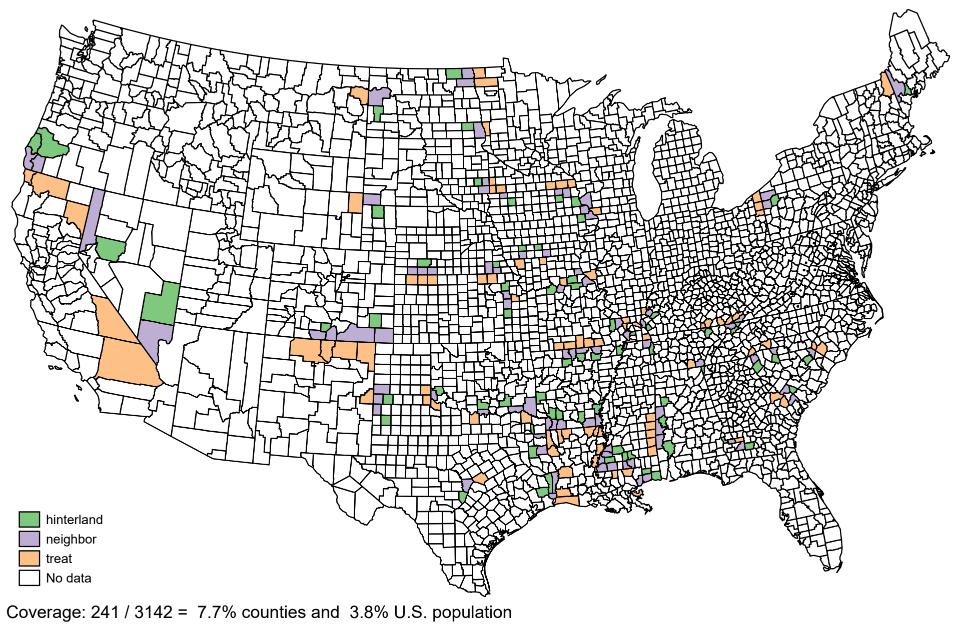
We show the opposite is true using k-means cluster analysis.[19] We assign each county in the overall sample (not just in our triplets) to one of 5 clusters based on its fixed characteristics. We include all variables in Table 1. We take logs of one plus population, transit score, and density to deal with the asymmetric and fat-right-tailed distributions of these variables. We then retain

[19]This is a standard method in data mining used to group observations into clusters based on similarity measurement criteria. When dealing with only two dimensions, the process is intuitive. k points are selected randomly and observations are assigned to these k according to their similarity. Euclidean distance is a common similarity measure. Next, centroids are computed for the k groups and taken as the new points. Reassignment happens again. This is iterated until no more reallocation of observations into clusters happens. Bishop (2006) provides more details of this method.

in our sample of triplets only those triplets where all three counties belong to the same cluster. Given the relatively large number of clusters, this ensures that the sample contains only the most homogeneous triplets – 118 rather than 474 in the baseline. We re-run our main regression on this restricted sample. Figure A.3 and Table A.3 shows a map of remaining triplets as well as presents the results. The point estimates are larger, and despite a much smaller sample, still significant. County heterogeneity is not driving our results.

In unreported regressions, we split each of the three alternative samples described above into same-DMA and different-DMA triplets and confirm that the results in Table 3 are robust as well.

Figure A.3: Contiguous County Triplets: Homogeneous County Clusters Only



Note. The map shows each treated (green-shaded), neighbor (purple-shaded), and hinterland (orange-shaded) counties in the final sample.

Table A.3: Contiguous County Triplets: Homogeneous County Clusters Only

	(1) **Log Total Visits**	(2) **Log Restaurant Visits**	(3) **Log Retail Visits**	(4) **Hrs Home**	(5) **Frac Work**	(6) **Frac Completely Home**
$Post_{pt}$	-0.106	-0.132*	-0.143**	0.162	0.00387	0.0177*
	(0.0692)	(0.111)	(0.0667)	(0.385)	(0.00291)	(0.00961)
$T_{cp} \times Post_{pt}$	-0.155***	-0.0969**	-0.142***	0.125	-0.0197***	0.0183***
	(0.0288)	(0.0483)	(0.0175)	(0.147)	(0.00546)	(0.00399)
$N_{cp} \times Post_{pt}$	-0.137**	-0.127**	-0.110*	0.225	-0.0157***	0.0162***
	(0.0617)	(0.0618)	(0.0608)	(0.182)	(0.00548)	(0.00620)
No. of Obs.	3,186	3,186	3,186	3,186	3,186	3,186
R^2	0.991	0.986	0.992	0.842	0.293	0.635
R^2 (within)	0.198	0.170	0.208	0.0238	0.0484	0.188

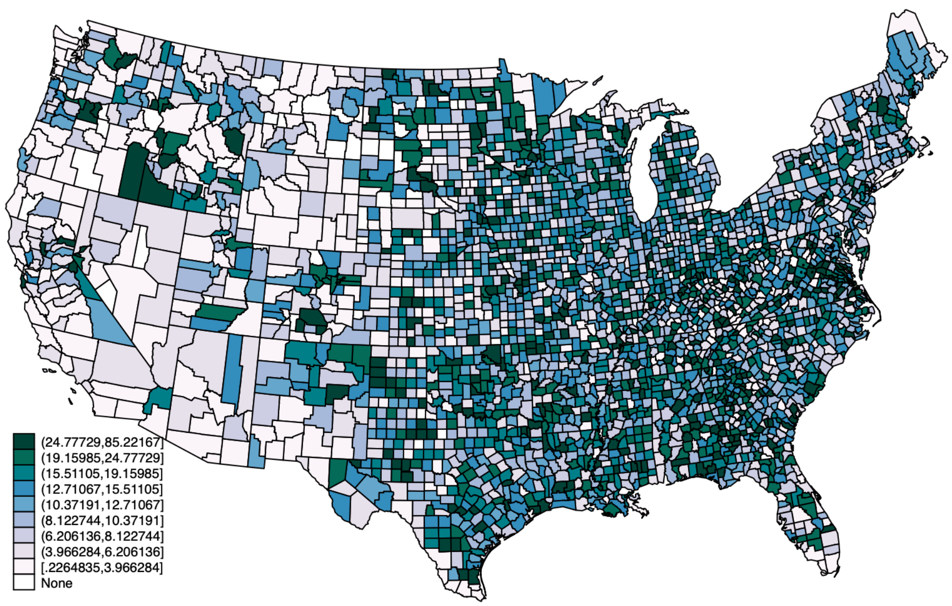
Notes: These results are equivalent to Table 2 in the paper, but with the sample constructed using only triplets, in which all counties share similar demographic, economic, and geographic characteristics given $k = 5$-means cluster analysis. The specification is given by equation 2. All regressions include county and triplet fixed effects, as described in the text. T_{cp} and N_{cp} are omitted because they are collinear with county fixed effects. Coefficients on weather controls are also omitted. Standard errors clustered at the triplet level are in parentheses. Sample period: [-4,4] days around treated county's SHO order adoption in March and April 2020. * p<.10, ** p<.05, *** p<.01.

B Additional Results

Figure B.4 shows the percentage of all mobility in a county that originates from phone devices that reside outside of the county. Mobility is measured by the number of visits detected to all non-residential POIs. Darker tones indicate a higher share of traffic coming from out of the county.

Table B.4 compares triplet member counties to other counties in the U.S. Table B.5 reports summary statistics for all mobility measures over the sample used in our directional regressions, the results of which are presented in Table 4. Table B.6 reports regression coefficients for the dynamic event studies presented in graphical form Figure 4.

Figure B.4: Percentage of Visits in a County Originating From All Other Counties



NOTE. The map shows the percentage of all mobility in a county that originates from phone devices that reside outside of the county. Mobility is measured by the number of visits detected to all non-residential POIs. Darker tones indicate a higher share of traffic coming from out of the county.

Table B.4: Outcome Variables: Summary Statistics

	Unconditional				Subsamples		
	Mean	St.Dev.	1-tile	99-tile	Not In Triplet	In Triplet	Difference
Non-Med Visits	6.615	3.813	0.305	18.13	6.560	6.782	0.222
					(0.0781)	(0.132)	(0.153)
Restaurant Visits	1.612	1.167	0.0396	5.335	1.620	1.588	-0.0311
					(0.0229)	(0.0465)	(0.0519)
Retail Visits	2.318	1.536	0.0918	6.993	2.320	2.312	-0.00726
					(0.0322)	(0.0503)	(0.0597)
Hrs Home	9.203	1.191	5.556	11.39	9.183	9.264	0.0803
					(0.0241)	(0.0427)	(0.0490)
Share Completely Home	30.83	4.646	21.28	43.99	31.00	30.32	-0.681***
					(0.0957)	(0.157)	(0.183)
Share Work	14.43	2.256	9.465	20.04	14.38	14.59	0.214**
					(0.0464)	(0.0767)	(0.0897)
Population	102,661	329,729	928	1,259,201	108868.5	83866.1	-25002.4**
					(7320.8)	(7507.6)	(10486.1)
Share 65+	19.26	4.691	9.455	32.22	19.15	19.60	0.451**
					(0.0984)	(0.158)	(0.186)
Share Black	10.22	14.61	0.587	64.86	9.906	11.16	1.252**
					(0.291)	(0.567)	(0.637)
Share Hispanic	9.638	13.80	0.892	69.06	9.756	9.296	-0.460
					(0.291)	(0.459)	(0.543)
Share College+	21.57	9.365	8.800	53.50	21.98	20.32	-1.656***
					(0.195)	(0.300)	(0.358)
Median HH Income	52792.3	13881.8	30029	100887	53552.0	50566.3	-2985.7***
					(293.8)	(445.8)	(533.9)
Share Poverty	14.56	5.648	5.070	32.22	14.33	15.22	0.891***
					(0.114)	(0.210)	(0.239)
Density (Housing Units)	123.9	821.8	0.400	1512.3	148.6	48.93	-99.70***
					(19.21)	(3.774)	(19.58)
Transit Score	6.752	14.99	0	65.70	7.037	5.922	-1.115*
					(0.325)	(0.475)	(0.576)
ICU Beds / 100K	13.38	54.14	0	85.10	14.10	11.29	-2.808**
					(1.280)	(0.592)	(1.410)
Ever Adopted SHO	0.857	0.350	0	1	0.878	0.793	-0.0841***
					(0.00667)	(0.0143)	(0.0158)
N	3218				2419	799	

Standard errors in parentheses

* $p < .10$, ** $p < .05$, *** $p < .01$

Note. The table reports summary statistics for mobility measures (top panel) as well as relevant demographic, economic, and geographic characteristics of sample counties (bottom panel). Mobility measures are computed for March-April 2019 to estimate typical pre-SHO mobility during the time of year in our main sample. Numbers of visits are per 100 people of the destination county's 2018 population. Shares are in percent. "Ever Adopted SHO" is an indicator for whether the county adopted a SHO at any point during March-April 2020. It is equal to 1 for all treated counties by definition. The table reports the sample average, standard deviation, the 1st and the 99th percentiles for the whole sample. It also reports means conditional on whether the county is in our sample (i.e., a member of a triplet) or not, as well as differences in sample means.

Table B.5: Outcome Variables for Directional Results: Summary Statistics

	Obs.	Mean	St. Dev.	Median	1-tile	99-tile
2019 and 2020 Sample						
Visits From Origin To Destination	651,420	0.11	0.50	0.01	0.00	1.80
Internal Visits In Destination	651,420	30.57	13.83	28.18	6.64	63.00
2019 Sample						
	Obs.	Mean	St. Dev.	Median	1-tile	99-tile
Visits From Origin To Destination	325,710	0.13	0.57	0.02	0.00	2.08
Internal Visits In Destination	325,710	35.77	13.11	36.42	9.31	64.03
2020 Sample						
	Obs.	Mean	St. Dev.	Median	1-tile	99-tile
Visits From Origin To Destination	325,710	0.08	0.42	0.01	0.00	1.52
Internal Visits In Destination	325,710	25.36	12.51	22.76	5.73	61.81

NOTE. The table reports summary statistics for all outcome variables defined in the text. Numbers of visits are per 100 people of the destination county's 2018 population. The table reports the sample average, standard deviation, median, the 1st and the 99th percentiles. The top panel reports statistics for the total sample used in directional county-pair regressions. The next two panels report statistics separately for the 2020 and corresponding 2019 pre-pandemic periods.

Table B.6: Contiguous Counties Estimation Results for Stay-at-Home Orders

	(1) **Log Total Visits**	(2) **Log Restaurant Visits**	(3) **Log Retail Visits**	(4) **Hrs Home**	(5) **Frac Work**	(6) **Frac Completely Home**
-4	0.0133	0.0248	0.0620**	-1.089***	-0.00344	-0.0168***
	(0.0295)	(0.0311)	(0.0297)	(0.131)	(0.00458)	(0.00442)
-3	-0.0718***	-0.113***	-0.0215	-0.984***	-0.0175***	0.000967
	(0.0274)	(0.0321)	(0.0310)	(0.125)	(0.00481)	(0.00736)
-2	-0.0735***	-0.0547**	-0.0250	-0.173	-0.0204***	0.00944
	(0.0193)	(0.0221)	(0.0222)	(0.127)	(0.00401)	(0.00615)
0	-0.0165	-0.0388*	0.00265	-0.168*	0.00138	-0.00725**
	(0.0187)	(0.0231)	(0.0176)	(0.0950)	(0.00305)	(0.00330)
1	-0.0286	-0.0249	-0.0443*	-0.0194	-0.00446	-0.00118
	(0.0260)	(0.0295)	(0.0250)	(0.165)	(0.00390)	(0.00387)
2	-0.0285	-0.0159	-0.0569	0.312**	-0.00272	0.00548
	(0.0307)	(0.0437)	(0.0369)	(0.126)	(0.00380)	(0.00474)
3	-0.0191	0.0388	-0.0137	0.666***	-0.00945**	0.00866*
	(0.0292)	(0.0485)	(0.0180)	(0.128)	(0.00428)	(0.00450)
4	-0.0153	0.0137	0.0168	1.246***	-0.0223***	0.0134***
	(0.0309)	(0.0499)	(0.0184)	(0.139)	(0.00484)	(0.00440)
T_{pr} × -4	0.0435*	0.0595*	-0.00622	0.230	0.00562**	-0.000738
	(0.0252)	(0.0343)	(0.0218)	(0.144)	(0.00245)	(0.00301)
T_{pr} × -3	0.0263	0.0741***	-0.0102	0.0239	0.0112***	0.000113
	(0.0163)	(0.0283)	(0.0195)	(0.0812)	(0.00301)	(0.00583)
T_{pr} × -2	0.0137	-0.00950	-0.0199	-0.173	0.00961***	-0.00324
	(0.0166)	(0.0134)	(0.0143)	(0.112)	(0.00221)	(0.00370)
T_{pr} × 0	-0.0332**	-0.00885	-0.0596***	0.471***	-0.00471***	0.0108***
	(0.0141)	(0.0205)	(0.0173)	(0.0774)	(0.00181)	(0.00255)
T_{pr} × 1	-0.125***	-0.109***	-0.137***	0.740***	-0.00490***	0.0293***
	(0.0164)	(0.0203)	(0.0191)	(0.0896)	(0.00180)	(0.00266)
T_{pr} × 2	-0.110***	-0.0838***	-0.123***	0.692***	-0.0115***	0.0254***
	(0.0154)	(0.0224)	(0.0145)	(0.105)	(0.00315)	(0.00414)
T_{pr} × 3	-0.0924***	-0.0834**	-0.137***	0.729***	-0.00501**	0.0189***
	(0.0193)	(0.0324)	(0.0300)	(0.107)	(0.00215)	(0.00419)
T_{pr} × 4	-0.0568***	-0.0101	-0.111***	0.426***	-0.000919	0.0135***
	(0.0190)	(0.0320)	(0.0254)	(0.0860)	(0.00246)	(0.00380)
N_{pr} × -4	0.0514	0.0862*	0.00581	0.0811	0.00369**	-0.00358
	(0.0353)	(0.0482)	(0.0183)	(0.0879)	(0.00168)	(0.00250)
N_{pr} × -3	0.0327	0.0921**	0.0169	-0.0574	0.00737**	-0.000284
	(0.0199)	(0.0399)	(0.0142)	(0.0698)	(0.00338)	(0.00456)
N_{pr} × -2	0.0313	0.0215	0.00968	-0.191	0.00707***	-0.00352
	(0.0224)	(0.0164)	(0.0135)	(0.137)	(0.00207)	(0.00283)
N_{pr} × 0	-0.00487	0.0144	-0.0360*	0.249***	0.000324	0.00527*
	(0.0104)	(0.0181)	(0.0186)	(0.0628)	(0.00160)	(0.00283)
N_{pr} × 1	-0.0159	-0.00302	-0.0136	0.251***	-0.00344**	0.00738***
	(0.0116)	(0.0163)	(0.0144)	(0.0727)	(0.00164)	(0.00264)
N_{pr} × 2	-0.0352**	-0.00410	-0.0408***	0.245**	-0.00659***	0.00824**
	(0.0141)	(0.0203)	(0.0140)	(0.101)	(0.00238)	(0.00367)
N_{pr} × 3	-0.0305*	-0.0460*	-0.0591**	0.327***	-0.00191	0.00604*
	(0.0159)	(0.0272)	(0.0300)	(0.0893)	(0.00150)	(0.00351)
N_{pr} × 4	0.00799	0.0329	-0.0270	0.210***	0.000353	0.00612
	(0.0172)	(0.0309)	(0.0294)	(0.0741)	(0.00164)	(0.00432)
No. of Obs.	12798	12726	12780	12798	12798	12798
R^2	0.990	0.985	0.992	0.850	0.288	0.706
R^2 (within)	0.101	0.0898	0.110	0.348	0.0836	0.206

Notes: The specification is given by equation 3. All regressions include county and triplet fixed effects, as described in the text. Coefficients on T_{pr} and N_{pr} are omitted because they are collinear with county fixed effects. Coefficients on weather controls are also omitted. Standard errors clustered at the triplet level are in parentheses. * p<.10, ** p<.05, *** p<.01. Sample period: March and April 2020. Restaurant and retail visits data is missing for a small number of county-days, explaining the lower number of observations in those columns.