Are we #Stayinghome to Flatten the Curve?

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Abstract

The recent spread of COVID-19 across the U.S. led to concerted efforts by states to "flatten the curve" through the adoption of stay-at-home mandates that encouraged individuals to reduce travel and maintain social distance. Combining data on changes in travel activity and human encounter rates with state policy adoption timing, we first characterize the overall changes in mobility patterns that accompanied COVID-19's spread. We find evidence of dramatic nationwide declines in mobility and human encounters prior to adoption of any statewide mandates. Then, using difference-in-differences along with weighted and unweighted event study methods, we isolate the portion of those reductions directly attributable to statewide mandates. Once states adopt a mandate, we estimate further mandate-induced declines between 2.1 and 7.0 percentage points relative to pre-COVID-19 baseline levels. While residents of mandate states soon returned to prior business visitation patterns, the impacts on distances traveled and human encounter rates persisted throughout the observed mandate periods. Our estimates of early mobility reductions and the responses to statewide stay-at-home policies convey important policy implications for the persistence of mobility behavior changes and states' future re-openings.

I. Introduction

Beginning December 2019, the novel coronavirus SARS-CoV-2 (COVID-19) spread rapidly around the world and in the U.S., prompting dramatic policy responses. Local, state, and national governments around the world faced an extensive set of policy instruments with which to fight the pandemic and limit the virus' impact on their constituents. Because many regions exhibited exponential growth in coronavirus cases, policymakers increasingly implemented aggressive stay-at-home mandates that sought to reduce transmission through human interaction and "flatten the curve" (Mervosh, Lu, and Swales 2020).

By March 31, the U.S. reported the highest number of confirmed COVID-19 cases globally, with over 67% more cases than the next closest country (*Coronavirus disease (COVID-19) dashboard* 2020). 30 states had implemented stay-at-home policies by this date, with 42 states and Washington D.C. ultimately adopting a stay-at-home order. Understanding how effective these policies were at modifying residents' behavior (and ultimately reducing the propensity for COVID-19 transmission) is of key importance for the maintenance of current mandates and the design of future pandemic control policies.

The relevant benefits of non-pharmaceutical interventions (NPI) – such as quarantining infected households, closing schools, and banning social events or large gatherings – have largely been informed by existing mathematical models (Hatchett, Mecher, and Lipsitch 2007). In addition, some anecdotal and historical evidence supports their efficacy. In California's San Francisco Bay Area, the first area of the country to implement stay-at-home mandates, doctors reported "fewer cases than expected" after two weeks of social distancing (Kahn and Marinucci 2020). Analysis of internet-connected thermometers suggests that new fever rates on March 23 were below those at the start of the month, while state hospitalization rates showed a commensurate decline in growth rates. Washington state officials reported similar reductions in COVID-19 transmission as a result of the state's containment strategies (Baker 2020). Exploration of death

rates and NPI roll-out in 17 U.S. cities during the 1918 influenza pandemic support these claims, finding that implementation of multiple social distancing practices intended to reduce infectious contacts early in the outbreak led to 50% lower peak death rates and flatter epidemic curves relative to cities that did not implement such policies (Hatchett, Mecher, and Lipsitch 2007).

Recent simulations provide further insight into the benefits of social distancing. While epidemiological models of the U.K. and U.S. suggest that techniques for mitigating exposure of those most at risk may drastically reduce peak load on the healthcare system and cut COVID-19 deaths by half, such techniques on their own might not be enough to prevent the healthcare system from being overwhelmed. Some argue that, in this case, a combination of social distancing, self-quarantine of infected people, and suspension of schools would need to be maintained until a vaccine is available to prevent a rebound (Ferguson et al. 2020). Other experts call for widespread testing coupled with digital contact tracing as a means to reduce viral spread while minimizing harmful social and economic side-effects. Simulations based on a moderate mitigation policy (comprising 7-day isolation following any symptoms, a 14-day quarantine for the household, and social distancing for all citizens over age 70) find that, had it been implemented in late March, it would have reduced potential U.S. deaths by 1.76 million (Greenstone and Nigam 2020). Given that this simulated policy is less stringent and maintained for a shorter duration than many of the policies that states actually implemented, the benefits from existing stay-at-home mandates (either directly from reduced COVID-19 deaths or indirectly due to decreased transmission of other illnesses) could be substantially larger.

Careful empirical study of these stay-at-home mandates is critical for understanding the benefits to the policies, given the visibility and extent of their costs. Even before mandates limited economic activity, GDP forecasts suggested an economic contraction in the U.S. of 24% (McCabe 2020). Concerns over these costs prompted comments from the executive branch regarding relaxation of restrictions and allowing non-essential businesses to reopen, which quickly prompted opposition from public health experts (Finucane and Andersen 2020) and many economists (IGM

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2020).

This paper contributes to the existing literature first by documenting widespread changes in U.S. mobility patterns surrounding the COVID-19 pandemic. Using cellular location data, we examine the ways in which travel behavior changed across the United States in response to the virus' spread (Unacast 2020). We show that tremendous nationwide reductions in travel activity levels and human encounter rates occurred prior to statewide mandates, suggesting residents were already responding to perceived risks and more local policies. Prior to any state implementing a statewide mandate, average travel distances had already fallen by 16 percentage points, the human encounter rate by 63 percentage points, and non-essential visits by 39 percentage points relative to pre-COVID-19 levels, providing evidence that extensive social distancing preceded many of the policies designed to induce such behavior.

Next, we provide empirical evidence that statewide stay-at-home mandates induced further reductions in travel behavior and greater increases in social distancing. States' policies combined closures of non-essential businesses with instructions for all residents to remain at home except for the purchase of necessities (i.e. groceries or medicine), with the goal of limiting "unnecessary person-to-person contact" (New Jersey 2020) and to "mitigate the impact of COVID-19" (California 2020). We first estimate a differences-in-differences model that isolates the effect of statewide mandates by comparing differences before and after mandate implementation within states and between early-adopting, later-adopting, and control states. Using this framework, we test whether states' stay-at-home policies induced further changes in mobility and daily human encounters. Decomposition of the treatment effect estimates reveals that a majority of estimate weights fall on comparisons between mandate and non-mandate states, with effect heterogeneity primarily found in the size of travel reductions.

After discussing results from the difference-in-differences model, we present estimates from event study models that directly examine the dynamic effects of stay-at-home mandates. In addition to typical unweighted event studies, we implement weighted event studies that directly

balance states on differences in pre-adoption mobility trends (Ben-Michael, Feller, and Rothstein 2019). These methods show that while visits to non-essential businesses returned to previous levels within ten days of mandate implementation, reductions in average travel distance and human encounter rates persisted throughout the observed portions of mandates. Both event study methods yield similar overall treatment effect estimates as in the difference-in-differences model, supporting the finding that statewide mandates induced further reductions in travel activity even after considerable pre-mandate reductions.

Across all methods, we find evidence that residents reduced daily activity, even before mandates, and that patterns differed by state. Moreover, we estimate significant additional reductions in travel and increased social distancing in response to stay-at-home mandates. We estimate a 7.0 percentage point reduction in average distance traveled, a 2.1 percentage point decline in non-essential visits, and a 3.5 percentage point reduction in the daily rate of human encounters after the average stay-at-home mandate was implemented. These represent meaningful changes, as non-essential visits and human encounter rates fell a further 27–31% relative to the reductions observed before states implemented mandates, with average travel distances changing by an additional 140%.

Taken together, our results provide evidence that the implementation of non-pharmaceutical interventions in the form of statewide stay-at-home mandates encouraged additional social distancing and reduced the opportunities for person-to-person transmission. Our estimates of mandate-induced behavior changes provide important insight into the effectiveness of recent policies designed to "flatten the curve" and stop the spread of COVID-19. We provide evidence that these policies further modified travel behavior and reduced the frequency of in-person contact. Further, our findings of substantial reductions in mobility prior to state-level policies convey important implications for the relaxation and eventual removal of these pandemic policies.

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II.   Materials and Methods

A.   MOBILITY DATA

We obtain travel activity and social distancing data from the analytics company Unacast. To understand how well different communities are social distancing, Unacast uses cellular location data for 15-17 million identifiers per day to construct three measures of behavior in response to COVID-19 policies (Unacast 2020). Each measure is aggregated to the state-by-day level and is defined as the daily percentage point change relative to that day of week's average for the pre-COVID-19 period of February 10 through March 8 (henceforth referred to as the baseline period). While all data are published publicly to their Social Distancing Dashboard in the form of figures and maps (Unacast 2020), we obtained the balanced panel of state-by-day observations for the period of February 24 through April 29, 2020 directly from Unacast.

Unacast receives location data from millions of mobile devices. Location information is received through authorized applications, Wi-Fi or Bluetooth connections, and A-GPS positions. Obtained information includes the location of the device at a given point in time (latitude, longitude, and elevation) along with the mobile device make, model, and operating system, the corresponding application gathering the data, GPS accuracy value, and the direction and rate of travel. Each state-day observation we use is calculated using position information. Taken together, these three measures paint a comprehensive picture of behavior changes in response to states' stay-at-home mandates. See Appendix A for more details on the data collection process, the equations used to construct each measure, and further discussion on sample composition and potential biases or measurement errors.

The first metric we use, the change in average distances traveled ($\dot{ADT}$), provides a measure of overall changes in travel activity during the COVID-19 period. To create this measure, the average distance traveled across all devices assigned to a state on a given day is compared to the state's average for that day of the week during the pre-COVID-19 baseline period. A value of

$\dot{ADT}_{it} = 0$ indicates that the average distance traveled for individuals assigned to state i on date t was identical to the baseline distance for that day of the week, while a value of -7 conveys that, on average, devices assigned to the state traveled an average distance 7 percentage points less than typical levels. As a result, $\dot{ADT}$ measures the percentage point change in average distance traveled relative to more typical behavior prior to the pandemic, accounting for pre-existing differences in states' average mobility patterns or residents' propensity to travel throughout the week.

Changes to average distances traveled give a sense of broad transformations to travel behavior. Declines in relative activity will yield negative values of $\dot{ADT}$, which can reflect both intensive margin (shorter distances traveled for the same frequency of trips) and extensive margin (some trips foregone entirely) adjustments. Reductions in $\dot{ADT}$ following mandate implementation reflect compliance on average with states' guidances to work from and stay at home except for essential activities, while positive or nonexistent changes would suggest a disconnect between private behavior and public policy.

The second metric we use is the change in visits to non-essential businesses, defined as Non-Essential Visits ($\dot{NEV}$). To the extent that non-essential businesses closed following stay-at-home mandates, we expect to see reductions in the number of trips residents take to these types of retail or service businesses. Our utilized measure of the change in visits to non-essential businesses ($\dot{NEV}$) offers a similar comparison to $\dot{ADT}$ that is targeted at travel to the types of businesses most heavily impacted by stay-at-home mandates. Businesses likely to be deemed "non-essential" include department stores, spas and salons, fitness facilities, event spaces, and many others. To improve accuracy, non-essential businesses are defined according to group definitions in both the Unacast SDK and the OpenStreetMaps POI's.[1] $\dot{NEV}$ is calculated by dividing a state's average number of visits to non-essential businesses by its day-of-week

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1. See Table 1 in the online Appendix A for a complete list of all retail and service categories classified as non-essential.

baseline level. A value of $N\dot{E}V_{it} = 2$ indicates a two percentage point increase in visitations to non-essential businesses relative to baseline norms for that weekday in a given state.

Finally, we use changes in the rate of unique human encounters ($E\dot{N}C$) as a measure of social distancing. While $A\dot{D}T$ and $N\dot{E}V$ provide information on two potential margins for adjusting travel behavior, neither directly captures changes in human-to-human interaction. As COVID-19 transmission primarily occurs through "close contact from person-to-person," having a measure of potential human encounters allows us to further understand whether reductions in travel distance and business visitations translate into fewer opportunities for viral transmission (CDC 2020).

$E\dot{N}C$ measures the change in the rate of unique human encounters per square kilometer relative to the state's baseline levels. Following (Pepe et al. 2020), one unique encounter is produced every time two devices assigned to a given state are observed within a 50 meter radius circle of each other for no more than 60 minutes.[2] Dividing the state-level sum of encounters for the day by the state's square kilometer of land area provides the state's daily rate of unique human encounters. Finally, this encounter rate is normalized by the state's average encounter rate for that day of the week during the baseline period.[3] As a result, an encounter rate equal to the state's baseline rate for that day of the week results in a value of $E\dot{N}C_{it} = 0$, while a value of $E\dot{N}C_{it} = -12$ indicates a 12 percentage point reduction in the encounter rate for state i on date t relative to the state's pre-COVID-19 level.

Table 1 provides summary statistics for the three mobility measures across the sample period.

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2. For data quality reasons, we drop observations for Washington D.C. from our analysis of human encounter rates. The point estimates reported in this paper are unchanged when D.C. is included, albeit with larger standard errors.

3. In contrast to $A\dot{D}T$ and $N\dot{E}V$, which are normalized by the state's day-of-week average from the entire pre-COVID-19 baseline period of February 10 through March 8, due to data limitations $E\dot{N}C$ is only normalized using the state's day of week average for February 24 through March 8. For more discussion, see Section 1.4 of online Appendix A.

The table is divided into four panels. The first panel corresponds to the majority of the pre-COVID-19 baseline period of February 24 to March 8, 2020.[4] The second panel summarizes behavioral changes for the rest of March, during which the majority of states enacted their stay-at-home policies. The third panel covers the period from April 1-29, 2020, which includes adoption of the final ten mandates and the observed post-adoption period. The final panel provides the average, median, and standard deviation for the total sample. The three columns report summary statistics for the changes in average distance traveled ($\dot{ADT}$), non-essential visits ($\dot{NEV}$), and the unique human encounter rate ($\dot{ENC}$).

In the top panel, covering February 24 through March 8, 2020, we see that the daily average distance traveled was larger than pre-COVID-19 baseline levels by 0.79 percentage points. Non-essential visits are 1.78 percentage points lower than the pre-COVID-19 baseline, with the rate of human encounters on average at baseline in the third column. Looking beyond the mean or median provides evidence of substantially heterogeneity across states, with the first quartile for all measures reflecting reduced activity while the third quartile reveals higher travel.

For the final three weeks of March 2020, all three mobility measures experience large decreases relative to pre-COVID-19 levels, attesting to average reductions in both travel and social interactions. During this period we observe 75th percentile changes of 12.93 percentage point reductions in average distances traveled, 21.06 percentage point reductions for non-essential visits, and encounter rate declines of 48.66 percentage points below baseline levels. Travel reductions become even more dramatic in the month of April; average reductions in all three travel activity measures during April exceed in magnitude the equivalent March declines.

Our utilized travel measures display very high correlations with travel data produced by other sources. Comparing our utilized travel change variables with the measure of state-level changes

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4. While the full baseline period used for normalization extends back to February 10, our state-by-day mobility panel only begins on February 24 and does not include daily observations for February 10-23. However, Figure 1 shows that travel patterns were largely indistinguishable from baseline levels prior to early March, providing evidence that behavior did not change substantively during the month of February.

Table 1: Summary Statistics on Travel Behavior and Social Distancing

	$\dot{ADT}$ Distance Traveled	$\dot{NEV}$ Non-Essential Visits	$\dot{ENC}$ Human Encounters
Before Mandates, February 24th to March 8th			
Average	0.79	-1.78	0.00
25th percentile	-1.59	-5.09	-10.79
Median	0.77	-1.93	-2.35
75th percentile	3.54	1.63	9.92
March 8 to March 31st			
Average	-25.43	-41.26	-60.14
25th percentile	-37.51	-59.51	-77.40
Median	-25.38	-48.21	-67.85
75th percentile	-12.93	-21.06	-48.66
April 1 to April 29th			
Average	-40.92	-59.02	-78.85
25th percentile	-47.87	-65.44	-85.25
Median	-39.98	-58.92	-80.53
75th percentile	-33.33	-53.13	-73.58
Total Sample			
Average	-26.59	-40.43	-55.22
Median	-30.93	-51.75	-71.59
Standard Deviation	19.98	26.23	34.87
N	3366	3366	3300

Source: Unacast. This table reports summary statistics for the changes in the average distance traveled $\dot{ADT}$ (Column 1), non-essential visits $\dot{NEV}$ (Column 2) and rate of unique human encounters $\dot{ENC}$ (Column 3). Data cover the period from February 24th to April 29th. Each observation is measured at the state-by-day level and represents an aggregate of mobile device-level travel and social distancing behavior on a given day.

in retail and recreation travel from Google's COVID-19 Community Mobility Reports, we find average correlations of 0.95 with $A\dot{D}T$ and 0.98 with $N\dot{E}V$ (Google 2020). Changes in non-essential visits are nearly perfectly correlated with the Google measure, with no state exhibiting a correlation below 0.96. Correlations for $A\dot{D}T$ remain high but exhibit greater variation across states. 31 states have correlations above 0.95, including California and New York at 0.97 and 0.98, respectively. Wyoming displays the lowest correlation at 0.75. These strong relationships across data providers suggest that our results are indicative of widespread changes to general mobility patterns and not spurious results arising from anomalies in our chosen data source.[5]

B. STAY-AT-HOME MANDATE DATA

To denote periods before or after a state implemented a "stay-at-home order," we obtain the date each statewide policy was issued for all fifty states and the District of Columbia (Mervosh, Lu, and Swales 2020). We define our early adopters as the first four states to pass a stay-at-home mandate: California, Illinois, New Jersey, and New York. The second group comprises the 38 late adoption states and D.C.. The last group comprises the eight remaining states that never implemented statewide mandates: Arkansas, Iowa, Nebraska, North Dakota, Oklahoma, South Dakota, Utah, and Wyoming.[6] The observed stay-at-home mandates all consisted of a mix of specific non-pharmaceutical interventions; each observed policy closed or placed considerable limits on non-essential businesses and required residents to stay at home except for essential activities. Essential services include grocery stores, gas stations, pharmacies, banks, laundry services, and business essential to government functions (covid19.ca.gov 2020). Throughout this paper, we refer to all mandates that implement this combination of policies as a "stay-at-home mandate."

While we focus our attention on statewide stay-at-home policies, many county and local

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5. See the data Appendix A for all state-specific correlations between the Unacast measures and the retail and recreation measures from Google's COVID-19 Community Mobility Reports.

6. For information on the timing of each individual policy, see Appendix A.2.

policies had already been implemented and were already affecting individual-level mobility. Six San Francisco Bay Area counties required residents to stay-at-home beginning March 17, two days prior to the statewide mandate. By mid-March, schools of all levels had begun closing their doors and transitioning to online instruction. On March 9, Stanford University moved classes online "to the extent possible," with Harvard and many other institutions swiftly following suit (Kadvany 2020). Further, business leaders including Google, Microsoft, Twitter, Facebook, and Amazon transitioned some or all of their employees to working remotely well before statewide mandates entered into effect (Aten 2020). As a result, the behavioral changes following statewide stay-at-home mandate adoption represent only a partial response to the suite of actions and policies undertaken to combat the spread of COVID-19. Our estimated "mandate effects" that follow therefore capture the behavioral responses specific to statewide stay-at-home mandates and underestimate the effect of all combined policies. If local, county, and business policies had already incentivized residents to stay at home, then we would expect a reduced response to later statewide mandates (which would be reflected in small magnitude estimates in our models). Any estimated mandate effects that follow reflect mobility responses in addition to those already realized by pre-existing policies.

C. EMPIRICAL STRATEGY

C.1 DIFFERENCE-IN-DIFFERENCES UNDER STAGGERED ADOPTION To determine the effect of statewide stay-at-home mandates on the outcome of interest, we begin by estimating the following difference-in-differences model under staggered adoption:

$$Y_{sd} = \alpha + \beta \; SAH_{sd} + \eta_s + \delta_d + \varepsilon_{sd} \tag{1}$$

The outcome Y_{sd} denotes the change in a given measure of travel activity ($\dot{ADT}$, $\dot{NEV}$, or $\dot{ENC}$) for state s on date d relative to the state's baseline level for that day of the week. Each outcome is expressed as a function of a constant α, whether a state has a statewide mandate in effect, and both time and unit fixed effects. SAH is an indicator equal to one if state s

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has a stay-at-home mandate in place on date d and zero otherwise. In the sections that follow, we consider the two cases where SAH includes variation for just the first four states to adopt statewide mandates and for all states that ever adopted a statewide mandate. The vector of state fixed effects η_s controls for time-invariant characteristics of states that affect the outcome, while date fixed effects δ_d control for factors affecting the outcome on a given date common to all states (i.e. executive branch press conferences or daily changes in worldwide COVID-19 deaths/hospitalizations). The term ε is an idiosyncratic error comprised of unobserved determinants of changes in the outcome that are not controlled for by the variables specified in the linear Eq 1. Rather than include state-specific time trends, our preferred specifications use outcomes residualized of timing cohort pre-trends (Goodman-Bacon 2018).

The coefficient β measures the difference in the change in average outcome for states that implemented a stay-at-home mandate relative to the change in activity in states that had yet to implement or never implemented such policies, controlling for state and time-varying factors that also correlate with the outcome of interest. In this way $\hat{\beta}$ provides an estimate of the average treatment effect for treated states (ATT). Models are estimated using data for the entire sample period of February 24 through April 29, 2020, covering the observed pre-mandate, adoption, and post-adoption periods.

This empirical approach allows us to identify the relationship between stay-at-home mandates and daily changes in each of the outcomes of interest while explicitly controlling for other confounding factors that are specific to each state or date. Employment rates prior to COVID-19 or the shares of a given local population previously working from home are controlled for with η, while day-to-day changes in factors common to all states – motivated by new information on the virus' spread and nationwide media coverage or federal appeals for social distancing – are absorbed by δ. The mandate effect β is identified under the assumption that, after controlling for cohort-specific pre-trends, common day-to-day trends, and time-invariant state characteristics, stay-at-home mandates are as good as random. Equivalently, the day-to-day outcome changes

from typical pre-COVID-19 levels in states that had yet to adopt or never adopted a mandate are what the change in the outcome would have been for treated states absent a stay-at-home mandate. Given the time-varying nature of adoption, we can express this underlying assumption as the weighted average of parallel trends for each simple two-by-two difference-in-differences estimators (Goodman-Bacon 2018). Our approach is identified using changes in the outcome that differ from typical pre-COVID-19 levels for a state and the state's average change during the COVID-19 time. A remaining source of bias would be if the early mandate states were trending differently than the control states before March 18 in ways that differed from trends after March 18, or if similar differences in trends exist across all mandate states. Standard errors of the estimated parameters are clustered by state to account for variation in state policies potentially affecting the variance of the residual ε.

C.2 UNWEIGHTED AND WEIGHTED EVENT STUDIES To directly model the dynamic nature of mobility responses to statewide stay-at-home mandates and relax assumptions of the difference-in-differences approach, we employ two event study methods. First, we estimate traditional event studies equivalent to the preferred difference-in-differences specifications:

$$Y_{sd} = \alpha + \sum_{k=\underline{k}}^{\bar{k}} \beta_k \cdot \text{Days Since}_{sd}^k + \eta_s + \delta_d + \varepsilon_{sd} \tag{2}$$

where

$$\text{Days Since}_{sd}^k = \begin{cases} \mathbb{1}\{d \leq SAH_s + k\} \text{ if } k = \underline{k} \\ \mathbb{1}\{d = SAH_s + k\} \text{ if } \underline{k} < k < \bar{k} \\ \mathbb{1}\{d \geq SAH_s + k\} \text{ if } k = \bar{k} \end{cases} \tag{3}$$

In this fashion the difference-in-differences estimator is decomposed into $\bar{k} - \underline{k} + 1$ individual coefficients relating the impact of being k days relative to when a state adopted its statewide mandate on date SAH_s. Event-time effects are identified under identical conditions as the

staggered difference in differences model of Eq. 1 while relaxing the assumption of a time-invariant treatment effect. To ensure that dynamic treatment effects are identified separately from time trends even in models that omit never-treated states, the endpoints $\underline{k}$ and $\bar{k}$ are binned to include all dates that fall either before $\underline{k}$ or after $\bar{k}$ (Schmidheiny and Siegloch 2019). Following convention, we drop the $k = -1$ bin and normalize all event-time effects relative to the day prior to mandate adoption.

Second, we estimate weighted event studies to further account for differences in pre-adoption mobility between states (Ben-Michael, Feller, and Rothstein 2019). Weighted event studies extend the synthetic control method (Abadie, Diamond, and Hainmueller 2010) to the staggered adoption event study framework and cleanly nest within the fixed effects approaches of Eq. 1 and 2. To correct for imperfect pre-treatment balance, weighted event study augments a "partially pooled" SCM estimator with a fixed effects outcome model. Synthetic controls are constructed based on the balance of residualized pre-treatment outcomes; in this way, the approach softens the difference-in-differences identifying assumptions to balance of treated units against a weighted combination of still-untreated states and builds upon recent research on doubly-robust estimators with an extension to the staggered adoption setting (Sun and Abraham 2020; Arkhangelsky and Imbens 2019; Ben-Michael, Feller, and Rothstein 2019; Chernozhukov, Wuthrich, and Zhu 2020).[7]

We report results for both traditional unweighted event studies and weighted event studies in the form of event study graphs. Each figure presents the estimates for the event-time coefficients,

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7. For each treated unit k, $\widehat{ATT}_{jk}$ can be thought of as a doubly-weighted difference in differences estimator, wherein the change in the treatment unit j is obtained as the difference between the treatment unit's outcome k periods post-adoption and its pre-period average, and the change in the control group is the average for equivalent changes for all donor units, weighted by partially pooled synthetic control weights. Averaging $\widehat{ATT}_{jk}$ across all treated units at a given point in event time yields a period-specific treatment effect $\widehat{ATT}_k$ that can be thought of as equivalent to the dynamic ATT obtained from an unweighted event study design. Standard errors are obtained using a jackknife approach (Arkhangelsky and Imbens 2019).

along with 95% standard errors clustered at the state level for unweighted event studies and jackknife 95% standard errors for weighted event studies (Arkhangelsky and Imbens 2019).

III. Results

A. OVERALL CHANGES TO MOBILITY PATTERNS

Across the United States, COVID-19 upended daily routines. As a result of revised work-from-home guidelines, school closures, family needs, layoffs, and state policies, travel behavior changed dramatically in the U.S. from February through April 2020. Figure 1 plots over time the changes in average distance traveled ($\dot{ADT}$), visits to non-essential businesses ($\dot{NEV}$), and the unique human encounter rate ($\dot{ENC}$) per day for all U.S. states, measured as the percentage point change relative to typical pre-COVID-19 baseline levels. The solid line plots the average for the first four states to implement mandatory stay-at-home policies: California (implemented March 19), Illinois (March 21), New Jersey (March 21), and New York (March 22). The dotted line plots the average for the 39 states that adopted stay-at-home mandates later in the sample period, while the dashed line plots the daily average for the remaining 8 states that had yet to adopt a stay-at-home mandate by April 29th.[8]

Travel behavior and social interactions in late February and through the first week of March look largely typical nationwide. Distance traveled and visits to non-essential businesses exhibit only small fluctuations relative to baseline activity levels for all states and groups. The change in the human encounter rate exhibits much greater variation throughout the week, increasing over the course of the work week before falling considerably over the weekend. Despite this greater within-week variation, the average human encounter rate for all states finishes the work week of March 2-6 above baseline levels.

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8. Massachusetts adopted a stay-at-home "advisory" that recommended but did not require that residents stayed home. Our reported results include Massachusetts as a stay-at-home state, and the findings are robust to the exclusion of Massachusetts' policy.
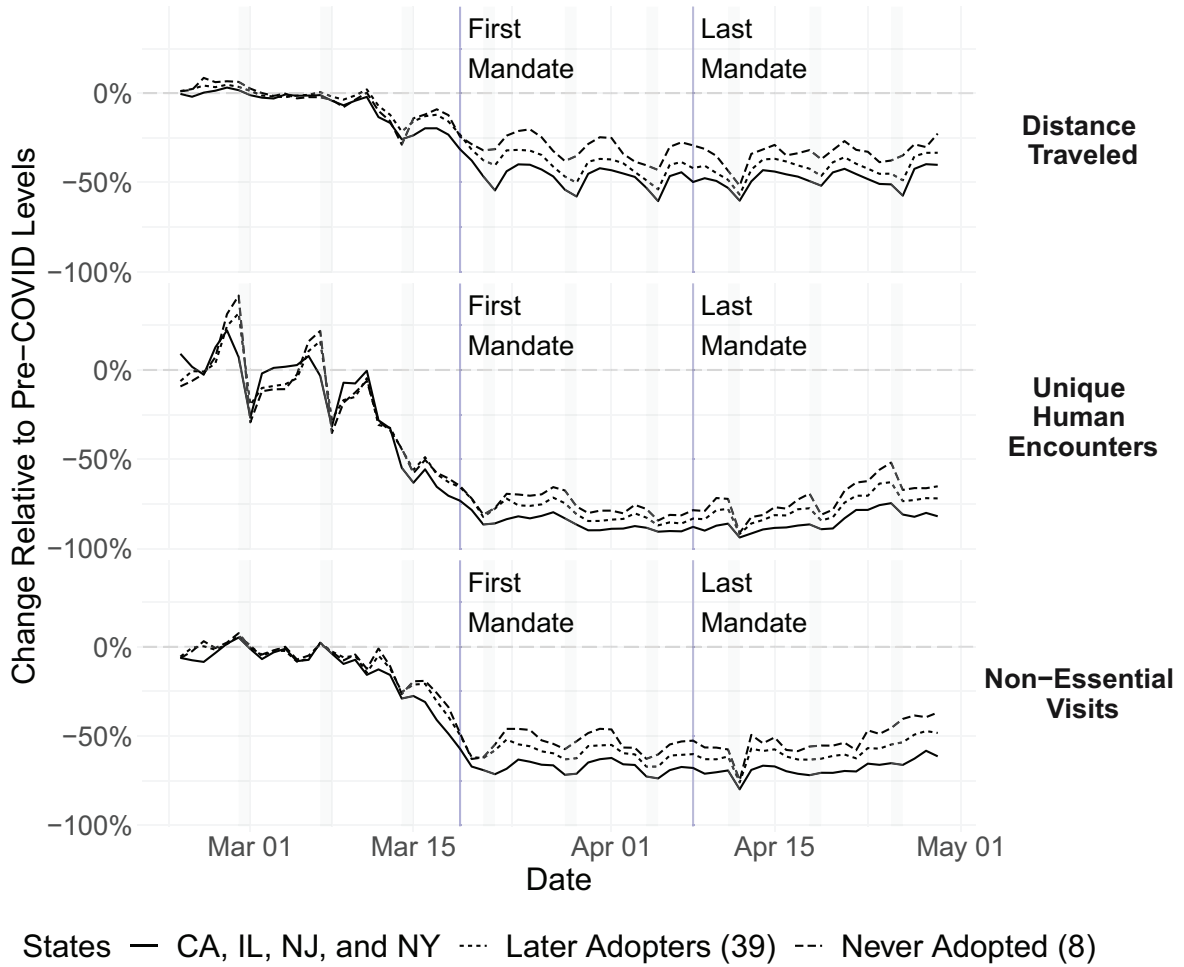
Figure 1: Changes in travel activity and social distancing. Each series represents the change in each day's mobility measure relative to pre-COVID-19 levels for the given group of states. The solid line corresponds to the average change for the four states that implemented stay-at-home mandates by end-of-day March 22 (California, Illinois, New Jersey, and New York). The dotted line plots the average for the 39 states that adopted statewide mandates at later points, while the dashed line represents the average for the eight states that never adopted a statewide mandate. The first panel plots changes in average distance traveled, the second changes in unique human encounters per square kilometer, and the third changes in visits to non-essential businesses. The gray bars designate weekend days. The vertical lines indicate the dates of the first and last statewide stay-at-home mandates (March 19 and April 8).

Beginning the week of March 9, residents across the country began deviating from typical travel patterns. By Wednesday March 11, residents of all states had begun reducing their distances traveled, trips to non-essential businesses, and encounters with others relative to pre-COVID-19 norms. Initially, changes to mobility patterns in early-adoption states are largely

indistinguishable from those for other states; by March 15, residents across all three groups had reduced travel distance by 8 to 13 percentage points, unique human encounters by 28 to 29 percentage points, and visits to non-essential businesses by 12 to 17 percentage points.

By March 18, before the first statewide mandate went into effect, the rate of travel activity decline had grown considerably in. The change in travel distances fell further, to between -12 and -23 percentage points, with changes between -34 to -49 percentage points for non-essential visits. Unique human encounters had already fallen between -61 and -71 percentage points relative to pre-COVID-19 baseline levels, a dramatic indicator of extensive social distancing occurring even before statewide orders required such behavior. By the start of April, travel behavior and social interactions had largely stabilized at levels 35 to 65 percentage points below previous norms, with within-week patterns and gaps between early adopter, later adopter, and never-adopter states remaining stable as well.

Figure 1 provides initial evidence that changes in travel behavior are correlated with the decisions of whether and when to adopt stay-at-home mandates. Following the start of statewide mandate adoption on March 19, residents of early adopter states exhibit larger magnitude reductions every single day through April 29 across all three measures. Each week during this period, mean encounter rates in early adoption states are consistently 10 to 16 percentage points lower than in states that never adopted mandates, with a larger weekly gap in travel distance (between 13 and 19 percentage points) and a similar 12 to 22 percentage point gap for non-essential visits. Trends for later-adopting states fall between early and never-adopting states, with late adopters displaying declines between 5 and 7 percentage points larger in magnitude than never-adopters for human encounter rates, between 7 and 10 percentage points for travel distance, and between 6 and 10 percentage points for non-essential visits.

B. EFFECT OF STAY-AT-HOME MANDATES ON DAILY MOBILITY AND SOCIAL DISTANCING

Figure 1 provides preliminary visual evidence that residents across the country drastically reduced travel activity and engaged in extensive social distancing prior to the adoption of statewide stay-at-home mandates. We next present estimates of empirical models designed to identify any additional changes in mobility and social distancing patterns attributable to states' stay-at-home mandates. We begin by presenting results of the staggered difference-in-differences treatment effect estimates in Table 2 before discussing the unweighted and weighted event study results in Figures 2 to 4.

Table 2 presents difference-in-differences estimates following Eq. 1 for the effect of stay-at-home mandates on travel activity across all three mobility measures. Columns (1) - (4) report coefficient estimates for the treatment effect restricted to only the first four adopters' mandates (CA, IL, NJ, and NY compared to all other states) while columns (5) - (8) report estimates using variation from all 43 adopting areas to identify the treatment effect. Columns (1) and (5) report treatment effects with state and date fixed effects, while columns (2) - (4) and (6) - (8) utilize dependent variables residualized of timing cohort pre-trends (Goodman-Bacon 2018).

Comparing column (1) to column (2) illustrates the bias present when models fail to control for cohort-specific trends in the presence of staggered adoption timing and dynamic treatment effects. In column (1) we estimate a -4.5 percentage point change in average distance traveled due to the first four states' early mandates (for the sake of brevity we report equivalent models for $N\dot{E}V$ and $E\dot{N}C$ that yield similar magnitude estimates in online Appendix C.) Adding controls for adopters' pre-trends across timing cohorts in column (2), the treatment effect estimate on SAH_{it} changes sign and loses all statistical significance.[9] Once we account for these early differences in COVID-19 outbreak trajectories between early and later-adopting cohorts, we fail to identify any differential effect of early adopters' mandates relative to later policies across all

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9. The same patterns hold true for both $N\dot{E}V$ and $E\dot{N}C$ when cohort pre-trends are included.

Table 2: Statewide Stay-at-Home Mandates, Travel Activity, and Social Distancing

	Early SAH States				All SAH States			
	$\dot{ADT}$ (1)	$\dot{ADT}$ (2)	$\dot{NEV}$ (3)	$\dot{ENC}$ (4)	$\dot{ADT}$ (5)	$\dot{ADT}$ (6)	$\dot{NEV}$ (7)	$\dot{ENC}$ (8)
SAH_{it}	−4.454**	2.495	2.629	3.050	−5.508***	−6.992***	−2.149**	−3.506***
	(2.074)	(2.113)	(1.927)	(4.598)	(1.036)	(1.454)	(0.880)	(0.981)
$\bar{Y}$	-26.59	-29.29	-36.23	-53.57	-26.59	-29.29	-36.23	-53.57
Pre-Period $\bar{Y}$	-3.98	-4.98	-7.02	-14.85	-3.98	-4.98	-7.02	-14.85
State + Date FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort Pre-Trends	No	Yes	Yes	Yes	No	Yes	Yes	Yes
N	3,366	3,366	3,366	3,300	3,366	3,366	3,366	3,300
Adjusted R^2	0.926	0.896	0.955	0.954	0.929	0.901	0.955	0.954

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors are clustered at the state level. These models estimate the effect of statewide stay-at-home mandates on travel activity and social distancing. The dependent variables measure the percentage point changes in average distances traveled, visits to non-essential businesses, and unique human encounters for the same day of the week relative to the pre-COVID-19 baseline level (average of Feb 10 - Mar 8). A coefficient of one indicates a marginal effect of a 1 percentage point increase in travel relative to pre-COVID-19 levels, controlling for time and the average COVID-19 mobility change in the state during the sample period. *Cohort Pre-Trends* indicates that outcomes have been residualized of timing cohort pre-trends (Goodman-Bacon 2018).

measures in columns (2) - (4).

Columns (5) to (8) report equivalent estimates using adoption of all statewide mandates to identify the treatment effect estimate $\widehat{ATT}^{SAH}$. Here, with much greater variation in adoption timing, ATT estimates are identified both through comparisons of changes in treated units to changes in states that never adopted a mandate and through comparisons between various pairs of states treated at different times. Comparing column (5) with column (6), we once again see the change in point estimates when accounting for cohort pre-trends. In this case, however,

controlling for differences in pre-trends does not eliminate statistical significance and yields an ATT estimate statistically indistinguishable from that in column (5) (p = 0.15).

In our preferred specifications for all statewide stay-at-home mandates in columns (6) to (8), we estimate large magnitude treatment effects relative to pre-mandate behavior changes. Looking at estimates for changes in average distances traveled, we observe a treatment effect estimate of -6.99 percentage points in column (6), statistically significant beyond the 1% level. That is, once a statewide mandate is implemented, we estimate a 6.99 percentage point reduction in the change in average distance traveled relative to control states. This represents an additional 140% decline relative to the pre-mandate change of -4.98, and an additional 24% reduction relative to average changes over the entire sample period (-29.29). In column (7) we estimate a -2.15 percentage point change in visits to non-essential businesses per day relative to control states (corresponding to 31% and 6% additional reductions relative to pre-mandate and full sample average changes, respectively). Turning next to changes in human encounter rates in column (8), we obtain an ATT estimate of a -3.506 percentage point decline per day after a mandate is implemented. Once again the treatment effects are statistically significant beyond the 1% level. This mandate effect corresponds to a 27% additional reduction relative to pre-mandate average changes of -14.85, and a 7% reduction relative to the -53.57 average change observed over the full sample period.

B.1 DECOMPOSING THE DIFFERENCE-IN-DIFFERENCES TREATMENT EF-FECT A potential concern of the difference-in-differences estimator relates to the weighting of individual periods. Under staggered adoption, the estimated treatment effect can be expressed as a weighted average of all unique two-period by two-group difference-in-difference estimators (Goodman-Bacon 2018). Weights are implicitly assigned to each timing cohort and unit, proportional to the variance of the treatment indicator in each period and the size of each cross-sectional group. A key implication of these weights is a favoring of units treated near the middle of the

sample period, with non-convexity indicating a potential for negative weights (Borusyak and Jaravel 2017; de Chaisemartin and D'Haultfœuille 2020; Sun and Abraham 2020). A resulting consequence is that negative (positive) treatment effects could also be obtained even when the effects of stay-at-home mandates for all adopting states are positive (negative) (Callaway and Sant'Anna 2020).

To shed light on the implicit weighting of the difference-in-differences ATT estimates presented in Table 2, we decompose the treatment effect estimates from columns (6) to (8) of Table 2 into their component two-by-two comparisons (Goodman-Bacon 2018). We find that over half the overall difference-in-differences estimate's weight is placed on comparisons of mandate states to never-adopter states. With adoption timing spanning March 19 to April 8, two-by-two comparisons can be made across many more cohorts and donor pools; in total, 18 comparisons are made between treatment cohorts and never-treated states, with 306 different comparisons between early and later adopters. More than half the overall ATT weight is given to comparisons of treatment cohorts versus pure control units, comprising 56-57% of the estimate across activity measures. The remaining weight is split evenly between comparisons of timing cohorts, with 21-22% of ATT weight given to comparisons of early treated units against later treated units still in the donor pool, and to later treated units post-treatment relative to previously-treated states. Except for early versus later treated units for average distances traveled, we observe consistently negative average ATT estimates across all three mobility measures, showing that treatment effect heterogeneity primarily concerns the size of reductions in travel activity rather than the sign of activity changes. See online Appendix B for a detailed presentation of the decomposition results and a more thorough discussion of the approach.

C. UNWEIGHTED AND WEIGHTED EVENT STUDIES OF MOBILITY AND SO-CIAL DISTANCING

To investigate the dynamic nature of mobility and social distancing responses to stay-at-home mandates and to address concerns regarding imbalances in changes to mobility patterns during the pre-mandate period, we present results of unweighted and weighted event studies. First, unweighted event studies avoid the implicit weighting concerns of the difference-in-differences estimator and allow an understanding of how treatment effects evolve and persist over time. Next, weighted event studies extend these benefits and allow for the comparison of stay-at-home states to a synthetic control state balanced on pre-mandate mobility change trajectories.

Figures 2, 3, and 4 plot unweighted and weighted event study graphs for average distances traveled, visits to non-essential businesses, and the rate of human encounters. The left panel in each figure reports coefficient estimates and 95% confidence intervals from an unweighted event study following Eq. 2 using outcomes residualized of cohort pre-trends. The right panel reports results from an equivalent weighted event study with the weight between separate and pooled synthetic control weights set equal to $\nu = \sqrt{q^{pool}}/\sqrt{q^{sep}}$, the ratio of the square roots of pooled to separate SCM imbalance (Ben-Michael, Feller, and Rothstein 2019).[10] The x-axis of each plot reports event time, indicating the number of days elapsed since a state's stay-at-home mandate entered into effect. An event time of zero indicates the first full day a state's mandate was in effect. The unweighted event study relies on the parallel trends assumption required for the overall difference-in-differences approach, while the doubly-robust approach of the weighted event study necessarily imposes balance on changes in pre-treatment outcomes.

The unweighted event study (left panel) plots day-to-day ATT estimates averaged across all

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10. See online Appendix D for overall ATT estimates obtained across the entire space of ν. While an interior ν of $0.01 - 0.99$ offers substantial imbalance reductions relative to the pooled or separate SCM cases, the optimal choice of ν is not immediately obvious. Estimating weighted event studies over the range of ν allows us to better understand how sensitive the overall ATT estimate is to the shift in weight from separate SCM for each state to a purely pooled SCM approach.

adopting states. These estimates are obtained from a two-way fixed effects control approach akin to columns (6), (7), and (8) in Table 2, with a vector of dummy variables for being each of $k \in \{-24, 21\}$ days relative to mandate adoption. The day prior to adoption ($k = -1$) is normalized to zero, such that all point estimates are interpreted as a differential change in a given travel outcome on the kth day since mandate adoption relative to the day immediately preceding adoption. 95% confidence intervals clustered at the state level are reported in the gray band. Estimates statistically distinguishable from zero in the post-period measure the daily treatment effect of stay-at-home mandates on mobility patterns. Non-zero estimates in the pre-mandate period ($k < 0$) are evidence that the difference-in-differences parallel trends assumption is likely violated and that residents of adopting states were already differentially modifying their travel behavior relative to residents of control states prior to stay-at-home mandates requiring such modification.

Figure 2: Unweighted and weighted event studies for changes in average distances traveled. The unweighted event study (left panel) plots regression coefficients for dummy variables equal to one for being k days away from the first effective date of each statewide stay-at-home mandate, with 95% confidence intervals represented in the gray band. A point estimate of -10 indicates a 10 percentage point greater decline in the average distance traveled per day for a state k days since mandate adoption relative to the day prior to mandate adoption ($k = -1$). The right panel plots equivalent point estimates and jackknife 95% confidence intervals from a weighted event study, with partially pooled synthetic controls constructed to match treated units on residualized pre-treatment outcomes with 60% of synthetic control weights obtained from pooled versus individual synthetic control weights ($\nu = 0.6$).

The unweighted event study for average distance traveled (left panel of Figure 2) reveals the extent of differences in pre-trends for mandate versus control states. Despite controlling for cohort pre-trends, the unweighted event study (left panel) still displays large pre-treatment differences between mandate and control states, reflecting the patterns seen in Figure 1. In all periods 24 to 10 days prior to mandate adoption, adopting states display markedly greater

average travel distances between 4 and 14 percentage points higher relative to control states. Pre-period point estimates remain positive but lose statistical significance for periods -9 to -2. Once a mandate is adopted, we observe an immediate reduction of 4 percentage points. This effect remains stable for 9 days before gradually attenuating over the remaining post periods and becoming indistinguishable from zero after 18 days.

The weighted event study results for average distance traveled (right panel of Figure 2) demonstrate the improved comparison achieved by internalizing pre-trends and its impact on treatment effect estimates. Using control units matched on prior mobility changes with $\nu = 0.6$ (60% of the weight given to the pooled synthetic controls and 40% to the individual controls) produces a greatly-improved overall pre-treatment match, with balance achieved in ten additional periods relative to the unweighted event study. Immediately after mandate adoption, average travel distances fall discontinuously by 10 percentage points in mandate states. In contrast to the unweighted event study, this estimated effect persists across all post-mandate periods. Averaging event day-specific ATT estimates across the entire mandate period yields an overall weighted event study ATT estimate of -8.97. Comparing to the previous estimates from Table 2, we find that eliminating the implicit weighting of difference-in-differences estimators and internalizing pre-trends yields an ATT estimates roughly 25-60% greater in magnitude.

Turning to estimates for changes in non-essential visits in Figure 3 and human encounter rates in Figure 4, we observe greater similarity between unweighted and weighted event study estimates. Pre-treatment point estimates are statistically insignificant in all periods for non-essential visits and in 17 of 23 periods for human encounter rates. The unweighted event study for non-essential visits in the left panel of Figure 3 shows a 6 percentage point reduction on the first day of mandate adoption that rapidly attenuates and becomes statistically insignificant after 8 days. The weighted event study in the right panel displays greater balance with narrow confidence intervals in all pre-treatment periods, with smaller magnitude point estimates that are lower variance but statistically indistinguishable from those in the unweighted event study. The

unweighted event study for human encounter rate changes in Figure 4 presents some evidence of differential trends, followed by an immediate and persistent treatment effect between -5.5 to -4.5 percentage points per day throughout the post-adoption period. Pre-treatment balance once again improves in the weighted event study, with no distinguishable difference between mandate states and their synthetic controls in any pre-treatment period. The weighted event study yields treatment effect point estimates that begin slightly larger in magnitude than but statistically indistinguishable from those in the unweighted event study. The weighted model estimates statistically significant declines in human encounter rates between -3 and -6 percentage points for the first ten days following mandate adoption. Although point estimate magnitudes never rise above -2.5 for days 11 to 21, increased noise relative to the unweighted event study leads to statistically insignificant estimates in all but two of these eleven periods.

Once again overall weighted event study ATT estimates are in line with our findings from difference-in-differences models. The weighted event study for non-essential visits yields an overall mandate effect of -0.84 (compared to -2.15 for column (7) of Table 2), with an overall ATT of -4.14 for encounter rates (compared to -3.51 for column (8) of Table 2). These estimates are highly robust to the specific choice of ν; overall ATT estimates fall between -9.94 and -8.96 for average distance traveled, between -1.08 and -0.81 for non-essential visits, and between -4.17 and -3.51 for human encounter rates across the space of $\nu \in [0, 1]$.[11]

The consistency of both weighted and unweighted event study estimates with results from difference-in-differences approaches provides confirming evidence that statewide stay-at-home mandates elicited further reductions in travel activity by affected residents. Event studies yield additional detail as to how these responses evolved, showing that reductions occurred immediately upon policy implementation and largely persisted even as residents were subject to the policies for three full weeks. This pattern is especially true for average distance traveled and human encounters, suggesting that residents of mandate states continued to socially distance,

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11. See Appendix D for plots of ATT estimates for each 0.01 value of ν.

Unweighted Event Study

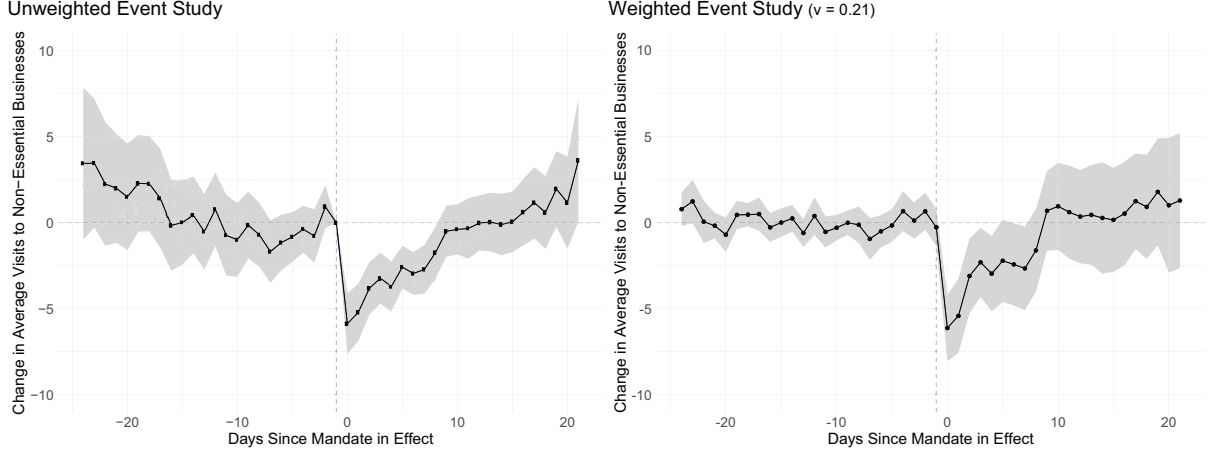**Weighted Event Study** (v = 0.21)

Figure 3: Unweighted and weighted event studies for changes in visits to non-essential businesses. The unweighted event study (left panel) plots regression coefficients for dummy variables equal to one for being k days away from the first effective date of each statewide stay-at-home mandate, with 95% confidence intervals represented in the gray band. A point estimate of -10 indicates a 10 percentage point greater decline in the average distance traveled per day for a state k days since mandate adoption relative to the day prior to mandate adoption ($k = -1$). The right panel plots equivalent point estimates and jackknife 95% confidence intervals from a weighted event study, with partially pooled synthetic controls constructed to match treated units on residualized pre-treatment outcomes with 21% of synthetic control weights obtained from pooled versus individual synthetic control weights ($\nu = 0.21$).

Figure 4: Unweighted and weighted event studies for changes in the unique human encounter rate. The unweighted event study (left panel) plots regression coefficients for dummy variables equal to one for being k days away from the first effective date of each statewide stay-at-home mandate, with 95% confidence intervals represented in the gray band. A point estimate of -10 indicates a 10 percentage point greater decline in the average distance traveled per day for a state k days since mandate adoption relative to the day prior to mandate adoption ($k = -1$). The right panel plots equivalent point estimates and jackknife 95% confidence intervals from a weighted event study, with partially pooled synthetic controls constructed to match treated units on residualized pre-treatment outcomes with 18% of synthetic control weights obtained from pooled versus individual synthetic control weights ($\nu = 0.18$).

a key avenue for reducing the potential transmission of COVID-19. The persistence of mandate effects under the weighted event study approach is confirmation that our findings actual, behavioral responses and not merely the result of pre-trend imbalance.

D.   Policy Take Away

Taken together, Figure 1 and the subsequent analyses reveal two main facts. First, that people across the U.S. decreased their travel and rate of human encounters early in the pandemic, preempting statewide requirements. Second, we find that statewide stay-at-home mandates are related to additional reductions in all our measures of travel activity and social distancing.

Conclusion

Temporarily closing non-essential businesses and mandating that residents stay at home except for essential activity is the prime policy instrument currently employed by states to promote social distancing and slow the spread of COVID-19. If effective, these policies will have reduced strain on the medical system and provided much-needed time for the development of pharmaceutical treatments that can reduce transmission rates and end the pandemic. If unsuccessful, states will have incurred large economic costs with few lives saved. Whether these mandates cause people to stay at home and engage in social distancing is a key requirement of a successful policy. Knowing whether such policies will have their intended effect is of increasing policy relevance, as all but eight states eventually adopted such policies. Understanding whether and how individuals reduce travel activity and maintain social distance in response to stay-at-home mandates is the primary empirical question we tackle in this paper. We establish two empirical findings.

First, we find that, by the time the average adopter had implemented its statewide mandate, residents had already reduced travel by considerable amounts relative to pre-COVID-19 levels. Average travel distances had already fallen by 16 percentage points, human encounter rates by 63 percentage points, and non-essential visits by 39 percentage points before the first statewide mandate came into effect, providing evidence of extensive social distancing occurring even before such behavior was required by statewide orders.

Second, we find evidence that adoption of state-level stay-at-home mandates induced further reductions across three travel activity measures. The staggered difference-in-differences models estimate a reduction in average distance traveled of 5.51 percentage points, a decline in visits to non-essential businesses of 2.15 percentage points, and a decrease in the rate of unique human encounters of 3.51 percentage points relative to pre-COVID-19 baselines. Estimated magnitudes remain highly comparable when directly accounting for differences in pre-mandate behavior for treatment and control states. Through the weighted event studies that construct control units to balance pre-treatment travel behavior net of state fixed effects, we find large, statistically significant drops immediately following mandate implementation across all measures that persist for the duration of the sample period for distances traveled and human encounters. Resulting estimates of the overall mandate effects mirror those obtained from the difference-in-differences models, and are similarly sized for any mix of pooled and separate synthetic control weights.

As travel activity is a main source of social interaction beyond one's immediate family (Silvis and D'Souza 2006) and travel to non-work locations increases the probability of co-location with others, these reductions in distances traveled likely reflect commensurate decreases in physical interactions with those outside of one's immediate family. Our estimates for changes in unique human encounters support this notion, providing evidence of further social distancing once states adopted a stay-at-home mandate. Further, these findings are not limited to the Unacast mobility measures; use of Google's COVID-19 Community Mobility Reports estimates similarly large and statistically significant effects of statewide mandates.[12] All this provides consistent evidence that, during their first three weeks, stay-at-home mandates have had the intended effect of inducing greater social distancing than would have occurred otherwise, helping to reduce the opportunities for communication of COVID-19 within adopting states.

Our work complements that of Gupta et al. (forthcoming), who similarly explore changes in mobility patterns in response to COVID-19 policies. The authors find comparably large changes

12. See online Appendix F for replications using the complete Google mobility trends data.

to a broad set of mobility measures in March 2020 prior to the adoption of county and state-level stay-at-home mandates (Gupta et al., In press). Some state and many county-level event studies that they estimate reveal parallel trend violations; our work demonstrates techniques to correct for differences in pre-period mobility trends. Our weighted event study results show the sensitivity of treatment effect estimates to potential imbalances in comparison groups in traditional difference-in-difference and event study methods.

Overall, our estimates suggest that residents subject to stay-at-home mandates on average responded as desired to curb the spread of COVID-19. Our empirical approaches isolate the mandate effect from other drivers of daily changes in travel activity levels and from pre-existing trends, and control for a host of potential confounding factors that differ between states that adopted policies relative to other states and those yet to adopt policies. Under these rigorous control approaches, we find persistent evidence of state mandates inducing further reductions in travel activity even after considerable earlier declines around the country. Further, our estimates are average treatment effects in response to statewide mandates only. Given the extent of prior school closures, new work from home abilities, and county-level stay-at-home policies, our findings represent only a portion of the way individuals responded to COVID-19 policies. As a result, our estimates represent a considerable lower bound on individuals' comprehensive responses to all COVID-19 policies.

These findings have important policy implications for the fight against COVID-19. First, individuals on average responded as intended to statewide mandates. Despite considerable prior reductions, residents heeded their states' directives and reduced travel activity. Second, the declines in economic activity directly attributable to statewide mandates may be much smaller than previously thought. Because individuals around the country had already more than halved the quantity of trips taken to non-essential retail and service businesses, much of the lost business and resulting unemployment would have likely still occurred even if states had not adopted their stay-at-home policies. Further, as the mandate-induced reductions in visits to non-essential

businesses amount to only one-tenth of the overall reductions since COVID-19 arose, it is likely that loosening or removing statewide policies may not be sufficient to induce mobility patterns to quickly return to pre-COVID-19 levels. Further policies will be needed to ensure that individuals can safely resume activity and return to local businesses.

Future work can establish whether these changes in mobility have significant health effects, taking into account the benefits from avoided hospitalizations and other indirect health benefits from reduced travel activity and social distancing. Because reductions in travel distance and increased social distance likely decrease exposure to other potentially deadly illnesses, estimates of the health benefits due to stay-at-home policies likely underestimate their overall impact. We support continued efforts to obtain accurate counts of the mortality and morbidity consequences from COVID-19 to help ensure future research can provide sufficient policy guidance in the case of future pandemics.

Acknowledgments

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