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ABSTRACT

This study quantifies the effect of state reopening policies on daily mobility, travel, and mixing behavior during the COVID-19 pandemic. We harness cell device signal data to examine the effects of the timing and pace of reopening plans in different states. We quantify the increase in mobility patterns during the reopening phase by a broad range of cell-device-based metrics. Soon (four days) after reopening, we observe a 6% to 8% mobility increase. In addition, we find that temperature and precipitation are strongly associated with increased mobility across counties. The mobility measures that reflect visits to a greater variety of locations responds the most to reopening policies, while total time in vs. outside the house remains unchanged. The largest increases in mobility occur in states that were late adopters of closure measures, suggesting that closure policies may have represented more of a binding constraint in those states. Together, these four observations provide an assessment of the extent to which people in the U.S. are resuming movement and physical proximity as the COVID-19 pandemic continues.

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1 Introduction

In March and April 2020, state governments implemented social distancing policies to control the spread of COVID-19 in the United States. These government actions, combined with private responses to the risk of infection, effectively shut down a large share of U.S. economic and social activity. Research based on other epidemics shows that human mobility plays an important role in the spread of many infectious diseases (Wesolowski et al., 2016). In recent work on the COVID-19 epidemic, Gupta et al. (2020) examine the effects of a variety of state and local policy actions (emergency declarations, school closures, restaurant dining-in prohibitions, non-essential business closures, and stay-at-home mandates) on cell phone-based measures of mobility and interaction. Their event studies suggest that state distancing policies led to small reductions in mobility that grew over time, and also that early and information-focused state policies may have had the largest causal influence on mobility patterns. There is also emerging evidence that state shutdown policies have helped reduce the transmission of the SARS-COV-2 virus over the past several weeks (Courtemanche et al., 2020; Dave et al., 2020; Friedson et al., 2020). Inducing higher levels of social distancing and keeping transmission rates low may help protect the viability of local health care systems by reducing peak utilization of limited health care resources like Intensive Care Unit (ICU) beds and ventilators. Thus, there is evidence that social distancing policies yield important social benefits, slowing the pace of the epidemic, preventing surges of healthcare demand, and perhaps ultimately saving lives.

Nevertheless, states' broad-based (non-targeted) social distancing responses likely have very high costs. Over 27 million people have filed new unemployment insurance claims since February, according to the U.S. Department of Labor. Much of this rise appears to be a nationwide response to the epidemic, but research is beginning to emerge on the role of early social distancing policies (Rojas et al., 2020; Kahn et al., 2020). Recent job losses are lower among those whose work can at least partially be completed remotely and among those working in essential industries (Montenovo et al., 2020). But overall the situation is dire: job losses between February and April 2020 already dwarf the job losses from peak to trough of the entire Great Recession. Mass unemployment will place enormous strain on household and national finances, and the experience of unemployment is damaging to mental and physical health (Sullivan and Von Wachter, 2009; Krueger et al., 2011). Given this situation, the pressure on state governments and individual households to restart economic activity is high (Mervosh et al., 2020).

Most states started to lift some of their social distancing policies in late April and early May 2020. But the decision to reopen is controversial. Proponents of reopening contend that strict shutdown policies are unnecessarily harmful to a state's economy, especially if the state appears to have stabilized or experienced declines in daily death or new cases (Tankersley, 2020; Cajner et al., 2020). At the same time, public health officials who advocate for more cautious policy raise concerns about a second wave of infections (Treisman, 2020).

In this study, we examine the early effects of state reopening policies on mobility patterns. We discuss the nature and timing of reopening policies across the country, and we use multiple cell-phone-based data sources to measure various dimensions of mobility. Our paper makes four main empirical contributions. First, we document that there has been a clear increase in mobility levels in most states since mid-April. Whatever the cause of the increase, the data make it clear that a reopening phase really has begun. The resurgence of mobility is small relative to the decline that occurred during the lockdown that took place in early March, but it is observable across a broad range of cell-phone-based metrics.

Second, the size of the increase in mobility across counties is strongly associated with temperature and precipitation patterns. For each county in the country, we computed the change in average mobility between April 15 and May 6. In the average county, mobility increased by about 3 to 30 percentage points depending on the measure of mobility (except a 3% decline in mobility in residential areas). We found that counties where temperature increased by 4 degrees Celsius (1 standard deviation, SD), the change in mobility (mixing index) was about 2% (0.11 SD). That reopening is sensitive to weather conditions may suggest the simple fact that good weather is a complement to mobility-based activities. Various outdoor recreation activities (beaches, pools, and parks) are simply more fun in good weather.

Third, we estimate event study models to trace out the ways in which mobility patterns respond to state reopening policies. These estimates suggest that four days after reopening, reopening policies seem to increase most measures of mobility by 6 to 8%, although the effects vary across outcomes. These results help to highlight the importance of using multiple measures of mobility, as each metric may carry different implications for transmission. Comparing results across pairs of contiguous counties, we find that the changes in mobility are larger in models that account for spillover effects on neighboring counties, indicating that even "untreated" counties experience increases in mobility when a neighboring state reopens.

Fourth, the reopening effects are largest among states that were late adopters of closure policies. This suggests that closure policies may have represented more of a binding constraint in those states. One possibility is that policies in late adopting states distorted private behavior more because people were less willing to engage in social distancing on their own. This could reflect differences in actual or perceived risk of infection, differences in the "affordability" of social distancing in some state populations, or differences in the way that the policies were enforced.

2 Background

2.1 Effects of State Closure Policies

In response to the COVID-19 pandemic, all states took official actions to encourage or mandate social distancing through emergency declarations, school closures, non-essential business closures, restaurant dining-in prohibitions, and stay-at-home orders and advisories. Stay-at-home orders have received a great deal of attention in public debates, but they are typically the final policy in a series of state and local actions (Gupta et al., 2020). Eight states did not issue stay-at-home orders at all: Arkansas, Iowa, Nebraska, North Dakota, South Dakota, Oklahoma, Utah, and Wyoming. These states did take several other policy actions, however. Connecticut, Kentucky, Massachusetts, and New Mexico adopted stay-at-home recommendations but did not actually impose a mandate (Raifman et al., 2020). Adolph et al. (2020) studies the determinants of policy implementation and finds that state policies are spatially correlated: states were more likely to adopt a policy if a neighboring state also adopted it.

To the best of our knowledge, four papers have examined determinants of mobility reductions during state closures (Gupta et al., 2020; Painter and Qiu, 2020; Abouk and Heydari, 2020). Specifically, Gupta et al. (2020) provide a comprehensive overview of the kinds of policies states enacted and their timing. They examine five different measures of mobility from Safegraph and PlaceIQ. Using event study regressions to examine several state information events and policies at both county and state levels, they find little evidence that stay-at-home mandates induce distancing. In contrast, early and information-focused actions have larger effects: first case announcements, emergency declarations, and school closures reduced mobility by 1-5% after 5 days and 7-45% after 20 days. Painter and Qiu (2020) uses Safegraph data on the fraction of cell devices that remain at home all day and finds that there is an immediate 5.1 percentage point increase in this variable following a stay-at-home order, which is 15% of the reported average overall. Andersen (2020) use a difference in difference framework and Safegraph data on number of device visits per county per day to examine the impact of stay-at-home orders, banning gatherings of more than 50 or 500 people, closing schools, restricting dining in restaurants, and closing gyms and entertainment venues. That work suggests that there is a 19.3% change from stay-at-home laws and effects of up to 11% from other laws. Abouk and Heydari (2020) examines mobility indices from Google in a difference in difference framework, studying statewide stay-at-home orders, more limited stay-at-home orders, non-essential business closures, large gathering bans, school closure mandates, and restaurant and bar limits. They find that stay-at-home mandates increase the percent of individuals who are present at home by 600%, with statistically insignificant effect from any other policy.

There are several differences between these existing studies on the largest determinants of mobility slowdowns experienced from early March to early April, and the literature has not fully resolved reasons for differences in conclusions. Studies also emphasize the importance of political variables in understanding mobility responses; the role of weather as an instrument for mobility holds promise in newly emerging research (Kapoor et al., 2020).

There is also evidence that stay-at-home orders, particularly those implemented earlier in the pandemic, reduce COVID-19 case counts and mortality. Dave et al. (2020) demonstrates that early stay-at-home orders reduce COVID-19 case counts and mortality, while later stay-at-home orders have no effect. Using Safegraph mobility data, they also find that stay-at-home orders increase the share of devices that stay home by 5.2%. Similarly, Courtemanche et al. (2020) find that stay-at-home orders reduce the COVID-19 case growth rate by 3.0 percentage points in the first five days after implementation and that the effect increases to an 8.6% reduction after 21 days; closing restaurants has similar effects on the growth of COVID-19 cases, although with a flatter trajectory. Friedson et al. (2020) uses a synthetic control method to estimate the effect of California's stay-at-home order on COVID-19 case counts. In their study, they find that the stay-at-home order reduces COVID-19 cases by approximately 20 to 45 fewer cases per 100,000 two to three weeks after adoption. In log linear models, their estimates indicate that stay-at-home orders reduce COVID-19 case counts by 40 to 50%. Collectively, these papers suggest that stay-at-home orders reduce disease transmission, with the implication that they do so by increasing social distancing.

2.2 State Reopening Policies

By mid May 2020, 36 states had started to unwind some of the policies adopted during the shutdown (Mervosh et al., 2020; Raifman et al., 2020). But the details of the reopening policies vary across states. In this study, we mostly define *reopening* as the first action a state takes to resume non-essential business activity. This is not the only way to measure the concept of reopening, of course. Another option is to use the date when stay-at-home (SAH) orders are lifted. In most states, SAH orders end on the same day that some non-essential businesses are allowed to open. A third approach would focus on how gradually vs. suddenly a state reopens its economy.

One advantage of the first reopening action definition is that – in a difference in difference framework – the first action may serve as a reduced form measure of the collection of reopening actions that follow. Another advantage is that the first reopening step may send a strong signal that the government thinks it is safe to start returning to regular life. It is possible that people may respond more to the initial reopening announcement than to incremental changes in the degree of opening. Even before official state closures, attention to the coronavirus as measured by internet search behavior in a state increased suddenly when the state announced its first positive case (Bento et al., 2020). Earlier work on state closures suggests that mobility effects are largest for information-laden policy actions (Gupta et al., 2020). Of course, this argument is mainly conjecture. It is certainly plausible that opening restaurants open at 20% capacity will have a smaller effect on mobility than opening restaurants at 80% capacity.

The effects of reopening may depend on how binding the various shutdown mandates actually were in practice. If reductions in mobility were primarily driven by "private" responses to the change in public health conditions, then it is possible that lifting a state social distancing mandate will not generate large increases in mobility. People may continue to keep their distance and avoid group gathering places because they are concerned about the health risks of the virus. It is also possible that maintaining high levels of social distancing for a longer period of time may accumulate some curbed demand for various good and services. News reports from Georgia indicate that there were lines of people waiting to get hair cuts after the state allowed hair salons to reopen (Stevens, 2020), complete with visible indicators of pent-up demand due to unkempt hair or fading hair dye. Because these reopenings occur in late spring, the warmer weather may discourage social distancing (Kapoor et al., 2020). It is also likely that individuals will engage in less social distancing after a reopening if they fail to take account of the infection externality that they impose on others (Bethune and Korinek, 2020).

Wisconsin provides a case study of the effects of rapidly reducing social distancing due to two rulings by the state's Supreme Court. The first ruling, on April 6, constrained the use of mail-in voting for the election to be held the next day. The second ruling, on May 14, overturned the governor's extension of the statewide stay-at-home order. Cotti et al. (2020) assesses the effect of the in-person voting mandate and find significant increases in the share of positive COVID-19 tests and new COVID-19 cases in counties that had more in-person voting after the primary. There is no academic literature on the effect of the second court ruling, but CBS News reported that bars and restaurants across the state were "packed" following the ruling (O'Kane, 2020).

3 Data

3.1 State Policies

We collected and coded data on state reopening policies, starting with *New York Times* descriptions of reopening plans. We gathered additional information on the reopening schedules for each state through internet searches. We define the state's reopening date as the earliest date at which that state issued a reopening policy of any type. The dates we arrived at as the first reopening event for each state are identical to the ones depicted in figures used by the *New York Times* article. Panel A of figure 1 lists the states that have reopened on each date since April 20. By May 13, 36 states had officially reopened in some form.

Some states never formally adopted a stay-at-home order, but even these states implemented partial business closures (i.e. restaurant closures) and some non-essential business restrictions. Of course, measures of mobility and economic activity have fallen in these states as well because of private social distancing choices. In addition, the lack of an official closure does not mean that state governments cannot take actions to try to hasten the return to regular levels of activity. For example, South Dakota did not have a statewide stay-at-home order, but the governor announced a "back to normal" plan that set May 1 as the reopening date for many businesses. Our study period commences on April 15 to ensure that we capture reopenings across all states. We provide the information we have compiled from various sources on GitHub.[1]

South Carolina was the first state to adopt a reopening policy, on April 20. It was also one of the last states to adopt a stay-at-home order.[2] This April 20 reopening was partial. It started by allowing retail stores to open at 20% of capacity. By April 30, eight states had reopened to some degree (AL, MS, TN, MT, OK, AK, GA, and SC). Eight more states reopened on May 1; by May 13, a total of 36 states had reopened. In most of our analyses the study period ends on May 15, which means that we are able to estimate impacts for at least 6 days post reopening using variation from 31 state reopening polices; we thus report effects as of 4 days after the policy.

Stay-at-home orders and non-essential business closures are related but distinct. Several states issued 'stay-at-home' mandates after they issued orders closing all non-essential businesses, or after closing some non-essential businesses (such as gyms) and closing restaurants for on-site dining (Panels B and C of Figure 1). Although for the most part, stay-at-home orders coincided with orders to close all non-essential businesses, restaurants and other select categories of business closures started well before stay-at-home orders. Many business closures started in mid-March, along with school closures (see Figure 2.1 of Gupta et al., 2020).

[1] We provide the information we have compiled from various sources at https://github.com/nguyendieuthuy/ReOpeningPlans.

[2] Although it issued an emergency declaration fairly early (March 13), South Carolina did not issue a stay-at-home order until April 7. (See Gupta et al., 2020).

Panel D of Figure 1 shows that by May 13, 63.5% of the U.S. population lived in a state that had adopted some form of reopening policy. However, the pace of reopening has been gradual. Only 36.5% of the population lives in states that opened the retail sector by May 13, and only 33.6% are in states that opened 3 or more sectors. Panel B of Figure 1 shows that by May 18, the last date for which we have policy data as of this writing, most states had implemented some form of reopening. Of the 36 states that reopened by May 13, 16 states reopened across three or more of the seven sectors that we track.[3] However, 20 states pursued a more limited strategy by opening only one or two sectors.[4]

States that either implemented fewer social distancing measures or implemented those measures later also tended to reopen earlier, based on time since the first of four major social distancing measures – non-essential business closures, restaurant closures, social gathering restrictions, and stay-at-home orders or advisories. These results may reflect either a lack of political desire to engage in distancing or a more limited outbreak (Andersen, 2020; Adolph et al., 2020; Allcott et al., 2020) .

3.2 Mobility Measures

We use data from four cell signal aggregators who provide their data for free to support COVID-19 research. Each company has several different measures of mobility, which may provide answers to different questions and have different implications. None of these data sources provides a metric of social mobility that is theoretically ideal in any sense. Each metric may capture a different form of underlying behavior, with different implications for the transmission of the virus and economic activity. For example, the fraction of people who stay at home may be less important than the degree of population mixing that occurs in any given location. In addition, each company collects data from different sets

[3]Following the *New York Times*, we track outdoor recreation, retail, food/drink establishments, personal care establishments, houses of worship, entertainment venues, and industrial areas.

[4]For seven states we could not clearly identify the sectors that would be affected by the reopening decision.

of app users, and it is possible that some of the cell phone panels are more mobile than others. Given these complexities, we think it is particularly important in this literature to compare across several measures of mobility. A multiple measure approach provides a simple way to assess the robustness and generality of a result; it also may provide opportunities to learn from differences in results across measures. In this paper, we examine data on human movement from different data sources, including Apple's Mobility Trends Reports, Google's Community Mobility Reports, PlaceIQ (GitHub repository), and Safegraph (provided upon free research agreement).

Apple Mobility Trends

Apple's Mobility Trends Reports (Site, 2020) are published daily and reflect requests for driving directions in Apple Maps. This measure shows the relative volume of driving directions requests per U.S. state compared to a baseline volume on January 13, 2020; no county-level equivalent is available.

Google Community Mobility Reports

We extract county- and state-level measures of mobility from Google's Community Mobility Reports (Google's Site, 2020). We use the data that reflect the percent change in visits to places within a geographic area, including: grocery and pharmacy; transit stations (public transport hubs such as subway, bus, and train stations); retail and recreation (e.g. restaurants, shopping centers, and theme parks); and residential (places of residence). The baseline for computing these changes is the median level of activity on the corresponding day of the week from January 3 to February 6, 2020.

PlaceIQ

We use two anonymized, aggregated location exposure indices from PlaceIQ data (Couture et al., 2020): (1) a "mixing" index that for a given day detects the likely exposure of a smartphone device in a county or state to other devices that day and (2) out-of-state and out-of-county travel indices that measure among smart devices that pinged in a given geographic location the percent of these devices that pinged in another geographic location at least once during the previous 14 days.

Safegraph

We use Safegraph data to measure the median hours spent at home by devices as well as the number of devices at the census block group level that are detected to be entirely at home during the day or to have left the house. We aggregate these to county or state by-day levels.

3.3 County-Level Characteristics

We collect a vector of county-level covariates to understand heterogeneity in a cross-sectional, descriptive analysis. We collect weather data (temperature and precipitation) from the National Centers for Environmental Information (NCEI, 2020). We estimate the number of nursing home residents in a county from the 2017 Nursing Home Compare database (CMS, 2020). County incarceration rates are obtained from the Incarceration Trends dataset of the U.S. Department of Justice Bureau of Justice Statistics. We derive the socio-demographic data from the 2020 Area Health Resources Files (HRSA, 2020) and County Health Rankings database (County Health Rankings, 2020). We use the latest year available in each original source.

4 Conceptual Framework

It has long been recognized that human mobility affects the dynamics of infectious diseases (Wesolowski et al., 2016). With regard to the COVID-19 pandemic, a recent study has shown that sustained physical distancing interventions are likely to reduce the magnitude of the epidemic's peak (Prem et al., 2020). However, human mobility results from individual decisions regarding costs and benefits related to certain activities. For example, many people are grappling with the decision of when to resume travelling (perhaps by public transport) to locations away from the home. This decision relates to measures captured by our mobility data: the detection of cell phones in far-away states from their usual location. Measures of out-of-state work or leisure travel will likely be shaped by employer reactions to changes in government restrictions, and to consumer perceptions of risk from exposure. Businesses decisions to reopen shape demand for labor, work-travel and leisure travel; following a reopening, we would expect an increase in travel measures. If many businesses remain partially shut down because consumer demand for their services is depressed even when states allow reopening, then work-and leisure related mobility measures may not change as much.

In addition, mobility measures may depend on work and consumption decisions. Mobility might not rise much if governments lift restrictions on non-essential business operations but many consumers do not feel that the 'rents' from shopping in person are sufficient to justify the health risks of the added exposure. Likewise, people in jobs that can be performed remotely or who have other sources of income may opt to continue staying home (Montenovo et al., 2020). Public policies that are not related to reopening, such as the stimulus payment, enhanced unemployment insurance benefits, and paid sick leave may affect decisions about mobility during the reopening phase as well (Andersen et al., 2020).

Geographic variation in the prevalence of essential industry workers may mean that reopening leads to larger changes in some locations than others. In rural areas, the effects of the stay-at-home orders in terms of reduction in mobility was less marked, likely due to the nature of rural work. We might expect that mobility effects would be larger after lockdowns of a longer nature, but there is selection into closing and opening dates: the states that were shut down for longer may proceed more gradually in other ways (especially since the degree of virus spread is information that may lead to policy change).

5 Research Design and Methods

5.1 Event Study Analysis

We use event study regression models to examine how measures of mobility evolve during the period leading up to and following state reopening events. Let E_s be the reopening date in state s. Then $TSE_{st} = t - E_s$ measures the number of days between date t and reopening. For example, five days before reopening, $TSE_{st} = -5$. Five days after reopening, $TSE_{st} = 5$. We set $TSE_{st} = -1$ for states that never reopen. We fit event study regression models with the following structure:

$$Y_{st} = \sum_{a=-15}^{-2} \alpha_a 1\left(TSE_{st} = -a\right) + \sum_{b=0}^{8} \beta_b 1\left(TSE_{st} = b\right) + W_{st}\sigma + \theta_s + \gamma_t + \epsilon_{st} \qquad (1)$$

In the model, Y_{st} is a measure of mobility and θ_s is a set of state fixed effects, which are meant to capture fixed differences in the level of outcomes across states that are stable over the study period. γ_t is a set of date fixed effects, which capture trends in the outcome that are common across all states. ϵ_{st} is a residual error term. α_a and β_b are event study coefficients that trace out deviations from the common trends that states experience in the days leading up to and following a given policy or information event. Specifically, α_a traces out differential pre-event trends in the outcome that are associated with states that go on to experience the policy change or information event examined in the model. β_b traces out differential post-event trends in the outcome that occur after a state adopts the policy or experiences the information shock. The reference period in all event studies is the period before reopening, when $TSE_{st} = -1$.

Our main specifications are based on a balanced panel of states that are observed across the entire range of dates available for the outcome variable. To avoid bias from composition change from one event time coefficient to the next, we set the length of the focal event time window to run from 15 days before the event to 8 days after the event, which keeps compositional variation low across all samples. The data span the period from April 15 to as close to present as possible for each outcome because April 15 appears to be the approximate time when shutdowns had achieved a stable pattern in slowed movement across the nation (Schaul et al., 2020). To adjust for seasonality, we control for state-by-day weather (average temperature and precipitation). These covariates are represented by W_{st} in the regression.

We cluster standard errors at the state level. We do not weight states by population. Our estimates should be interpreted as reflecting the experience of the average state rather than the average person. To help summarize results, we assess the presence of a pre-trend based on the statistical significance of the pre-policy event study coefficients. In our summary results, we say that a measure exhibits a pre-trend if at least 30% of the coefficients in the pre-period were statistically significant.

Subpopulation Analysis

In addition to the state-level event-study analysis, we conduct a series of heteroheneity analyses, by stratifying the sample in several ways. First, we run separate regressions for rural counties and metropolitan counties, expecting that the nature of rural activities might be more essential in nature and less elastic to non-essential business closures. Counties were separated into metropolitan and rural categories using the National Center for Health Statistics Urban-Rural Classification Scheme. Second, we run separate regressions for states with longer and shorter stay-at-home orders, expecting that fatigue maybe captured this way. Longer stay-at-home orders are defined as those implemented more than 25 days prior to re-opening (the median implementation period). Finally, we run heterogeneity analysis by baseline COVID-19 death rates, expecting that where deaths were higher, individuals maybe more reluctant to move even when restrictions are eased. Higher baseline COVID-19 related death rates are defined as those above the median as of April 15th, prior to re-opening.

5.2 Border County Analysis

We also estimate models using border-county pairs that are adjacent to one another but belong to different states.[5] The border county design provides a way to control for local area unobserved factors that may confound the effect of reopening on mobility, and also gives an opportunity to examine the spillover effects that may occur when one state reopens and another does not.

Specifically, for a pair of counties c and c' (in states s and s', respectively), we define the first event date in the pair as $E_{c,c'} = \min(E_s, E_{s'})$ and an indicator $F_{c,c,c'} = 1$ if $E_s = E_{c,c'}$ and 0 otherwise.[6] We let $TSE_{c,c',t} = t - E_{c,c'}$ be the number of days between date t and the first reopening in the county pair. We set $TSE_{c,c',t} = -1$ if neither county has a reopening event in our data. Our county pairs model is a modification of the main event study to include county pair fixed effects and, in some specifications, pair-by-time fixed effects:

$$\begin{aligned} Y_{ct} &= \textstyle\sum_{a=-15}^{-2} (\alpha_a^0 1(TSE_{c,c',t} = a) + \alpha_a^1 1(TSE_{c,c',t} = a) \times F_{c,c,c'}) \\ &+ \textstyle\sum_{b=0}^{8} (\beta_b^0 1(TSE_{c,c',t} = b) + \beta_b^1 1(TSE_{c,c',t} = b) \times F_{c,c,c'}) \\ &+ W_{st}\sigma + \theta_{(c,c')} + \gamma_t + \epsilon_{c,c',t} \end{aligned} \tag{2}$$

In the model, Y_{ct} is a measure of mobility for county c at time t. $\theta_{(c,c')}$ is a set of county pair fixed effects that captures fixed differences in the level of outcomes and timing of reopening across counties. γ_t is a set of date fixed effects that captures trends in outcomes that are common across all counties in the sample. α_a^0 is a set of event study coefficients that trace out how trends in a given pair deviate from the national trend in the lead up to a reopening event in counties that do not reopen, while α_a^1 provide similar estimates for how different the first county to reopen is, relative to the adjacent county that reopens

[5] Our approach is similar to Dube et al. (2010), which uses contiguous counties to study the effects of minimum wages, and Lin and Meissner (2020), which studies how non-pharmaceutical interventions affect distancing.

[6] Since counties can appear in multiple county pairs, a county may be the early county in one pair, but the late county in another pair. It is also possible that both counties reopened at the same time, in which case $F_{c,c,c,'} = 1$ for both counties.

later. β_b^0 traces out changes in counties that did not reopen after the first county reopened. Therefore β_b^0 includes an estimate of the common spillover effect across all county pairs of the first county in the pair reopening. β_b^1 traces the change in outcomes associated with the reopening event in the county that reopened first.

We also estimate a second version of the county pairs model that allows each county pair to have a separate set of date fixed effects. In this model, the α_a^0 and β_b^0 terms are subsumed by the county pair by date fixed effects. These fixed effects flexibly capture trends in the county that reopened later, including the spillover effect on that county from policy changes in its pair county that opened earlier. Therefore we refer to the first model without the county pair by date fixed effects as assuming "no spillovers," while the second model assumes that there are "spillovers." Our sample for the county analysis is constructed in a comparable manner to the main, state-level models, including using a balanced panel. Following the state-level analysis, standard errors are clustered on state and estimates are unweighted.

5.3 County Cross-Sectional Regressions

To understand how the mobility changes we observe during the reopening phase differ across geographic areas with varying non-policy factors, we fit cross sectional regressions of "long differences" in mobility measures at the county level. We estimate the long differences ΔY_c between April 15 and May 6 in 12 measures: mobility to retail/recreation, mobility to grocery/recreation, mobility to transit stations, mobility to workplace, mixing index, out-of-county travel, fraction of devices left home, fraction of devices at workplace, indicator of number of COVID-19 cases above the median (high in cases), and indicator of number of COVID-19 deaths above the median (high in deaths). We link these long differences ΔY_c with a vector of county-level covariates. In order to investigate the overall change in mobility patterns across counties, we fit the following regression:

$$\begin{aligned} \Delta Y_c &= \beta_1 \Delta Weather_c + \beta_2 SES_c + \beta_3 Urban_c + \beta_4 Political_c \\ &\quad + \beta_5 Demography_c + \epsilon_c \end{aligned} \tag{3}$$

In the model, $\Delta Weather_c$ is a vector of covariates depicting the change in daily average precipitation and temperature between April 15 and May 6. $Urban_c$ is a vector of covariates reflecting county population, population density, and urbanicity. SES_c is a vector of covariates describing median household income, poverty rate, health uninsurance rates, number of nursing home residents per capita in 2017, incarcerated rate in 2017, and whether the county is a major destination for recreation or retirement. $Political_c$ is the Republican vote share in the 2016 presidential election, and $Demography_c$ includes demographic composition. We standardized all variables before estimating this cross-sectional regression to make the estimated coefficients more comparable.

6 Results

Figure 2 shows the national and state time series of each mobility measure from February to mid-May. Mobility fell dramatically during the lockdown phase, and there is clear evidence across multiple measures that mobility began to increase in mid-April. The timing of the increase in mobility varies across states and across different measures of the outcome. Although the axis scales differ, the times at which dramatic changes occur are about the same across all measures. To understand the connection between recent increases in mobility and state reopening policies, we turn to event study regressions, county-pair analysis, and cross-sectional regressions of "long differences".

6.1 Event study analyses

Figure 3 plots event study coefficients for the Apple Mobility driving direction requests. There was a large increase in seeking driving directions that grew with time; there is little evidence of a pre-trend.

Figure 4 shows the event study coefficients for the Google Mobility outcomes - Mobility to Retail/Recreation, Grocery/Pharmacy, Transit Stations, and Stay-in Residential Areas. The results suggest that reopening leads to substantial increases in mobility to retail and recreation destinations; there is not much evidence of a pre-trend. There may also have been an increase in mobility to grocery stores and pharmacies and to transit stations, but these estimates are noisier, and it is hard to distinguish the pre-trend and post-trend.

Figure 5 shows the event studies for the Safegraph measures - Fraction left Home and Median Hours at Home. These estimates are noisy, but there is some evidence that reopening policies increase the fraction of people/devices that left the home each day. In contrast, there is no indication that reopening leads to much change in median hours spent at home.

Figure 6 shows the event studies for the PlaceIQ measures - Mixing Index, out-of-state movement, and out-of-county movement. The results indicate that reopening policies generate a substantial increase in the mixing index; there is little evidence of a systematic pre-trend in mixing leading up to reopening. The out-of-state and out-of-county movement measures do not respond much to reopening.

To help summarize the results, the first column of table 1 reports the estimated effect of a reopening policy 4 days after the event for each outcome presented in the event study plots. The effect estimates are presented in percentage terms, relative to the average level on April 15, 2020, to help make the magnitudes as interpretable as possible. Grey shaded cells in the table indicate models in which there is some evidence of a pre-trend; these results should be viewed with some suspicion. We group the outcomes in table 1 into measures of mobility (i.e. driving directions, mixing index, etc.) and measures of the absence of mobility (median hours at home, stay in residential area). We expect a positive sign on the reopening event for measures of mobility and a negative sign on measures of the absence of mobility.

Overall the state-level event study results paint a very clear picture. Four days after reopening, there is a statistically significant increase in most measures of mobility, and most outcomes that are not statistically significant are noisy but also positive. Across all of the mobility outcomes, the effect size ranges from about a 1% to a 22% increase in mobility, and several outcomes cluster around 6% to 8%. Both of the measures of absence of mobility have a negative sign, suggesting that reopening has reduced the tendency to stay at home as well as the number of hours people spend at home each day. However, the reopening effect on these measures appears to be smaller, and we cannot reject the null that reopening has not affected these outcomes at all. One interpretation is that reopening has increased the diversity of options that people have available to them, and they are more likely to visit a variety of locations that they had avoided in previous weeks. At the same time, people have not increased the amount of time they spend outside their home much. One concern is that reopening appears to have a large effect on the mixing index, which is a proxy for actual interactions between people (devices). This may be worrisome if the mixing index represents a particularly relevant proxy for high transmission rates.

In addition to the state-level analysis, we also fit event study specifications to county-level data and stratify the sample in several ways. The second and third columns in table 1 show effect size estimates from models that use only data from rural counties and urban counties, respectively (Appendix B). These results suggest that reopening policies have had broadly similar effects on most measures of mobility in urban vs rural areas. The estimates from urban areas tend to be more precise, but the magnitudes of the effect sizes are similar. One exception is that the increase in the mixing index and in out-of-county movement are large in urban areas and negligible in rural areas.

The fourth and fifth columns show estimates from models that are limited to states that had stay-at-home policies in place for a short vs a long duration (Appendix C). Here we find that the estimated reopening effects are much larger in states that were late adopters of stay-at-home mandates and so were in lockdown for only a short time. This may indicate that voluntary limitation of mobility is lower in areas that took a weaker stance during the lockdown phase. For instance, it is possible that many people in early closure states were and are staying home of their own accord, and so lifting the mandate does not lead to large increases in mobility. In contrast, in states that imposed a lockdown later, restaurant and business closures may have had a binding effect on people's behavior. In late-lockdown states, people may have been more willing to visit restaurants if they had been open. Now that the restaurants are allowed to open, people are returning to them. This account seems logical to us, but it is largely conjecture.

6.2 County Pairs Results

We present event-study-style estimates of the effect of reopening in our county border pairs model (equation (2)) in Appendix D. Figure D1 presents these estimates for our Google Mobility data assuming that there are no spillovers. The first panel presents results for mobility to retail and recreation locations. The pre-trend, which reflects how mobility differs in the first county to reopen in a pair relative to the other county, is flat but somewhat imprecisely estimated. However, after roughly one week, there is a statistically significant increase in mobility to retail and recreation locations of approximately five percent relative to the January 3 to February 6 baseline period. The remaining panels typically provide little evidence for pre-trends, but the estimates are also sufficiently noisy that one cannot make a definitive statement about changes in mobility from these models that assume no spillovers on behavior. Figures D2 and D3 present similar estimates for the fraction of people leaving home, the median time at home, the mixing index, and out-of-county movement. The estimates from these models are too imprecise to permit a reliable interpretation.

Figure D4 presents results for the same Google Mobility data, but now we explicitly take spillover effects into account by allowing for a county-pair-specific set of time fixed effects. The results for retail and recreation mobility indicate that there were increases in mobility immediately after a state reopened, although the effect appears to be slightly smaller than in the no-spillover model. The implication of these two models is that even counties that do not reopen also experience an increase in mobility, which is consistent with people in neighboring counties crossing state lines to go to retail or recreational locations in newly opened states. Results for mobility to groceries, and pharmacies also increase. We also find a reduction in mobility in residential areas, which may indicate that people are spending less time in neighborhoods and more time at formal retail and recreation venues, or groceries.

Figure D5 conducts the same exercise using the fraction of devices that left the home and median hours at home as our measures of mobility. As has been the case in our other models, these estimates are noisy. However, for both measures there is limited, if any, evidence of a pre-trend–so neighboring counties were on similar trajectories in the pre-period–and there are some statistically significant changes in mobility in the post-period. Figure D6 presents results for our mixing index and out-of-county mobility. We find evidence of significant increases in mixing in the post period. This effect appears to get larger over time, so that, after four days, there is an approximately three-point increase in mobility as measured by the mixing index, or 8% of the mean, while there is no change in movement out of the county.

The effects from these event studies are summarized in columns (6) and (7) of Table 1. These estimates indicate that mobility to transit stations in counties that reopened rose by 9% of the April 15^{th} mean level, relative to "closed" counties. In the last column, which allows for spillover effects to untreated counties, we find more precisely estimated and, in many cases, larger estimates for the change in mobility. In all cases, our results are consistent with the state-level analyses and demonstrate that reopening increases mobility in states. Furthermore, there appear to be positive spillover effects onto neighboring counties, since in models that account for spillovers (column 7) our estimated changes in mobility are larger in magnitude.

6.3 County Cross-Sectional Analysis

In the average county, mobility increased by about 3 to 30 percentage points depending on the measure of mobility (except a 3 percentage-point decline in mobility in residential areas) between April 15 and May 6. Figure 7 and Table 2 show standardized regression coefficients (per standard deviation change in the explanatory variable) from models of the total change in mobility measures between April 15 to present. Across the 2,031 counties with complete data, we observe that better weather has important effects on nearly all of mobility measures. As temperature rises in a county, people appear to mix more (a 0.11 SD increase for a one-SD change in temperature) and take more purposeful drives to grocery and retail venues (both increasing $\sim$ 0.2 SD), as well as take more out-of-county trips. The opposite trend is observed with increased precipitation, where we observe people staying at or around home (increase in driving around residential area by 0.23 SD).

In more densely populated counties, we observe an increase in trips to retail locations (0.049 SD) and a marginal decline in trips to grocery locations (-0.073 SD). The size of the metro area affects the magnitude of mixing increase (0.16 in larger areas to 0.057 in smaller areas). In general, we see an observed increase in movement by both white and black communities, with the sharpest increase to retail. Not surprisingly, in recreation counties, we note an increase in overall activity.

We found interesting partisan differences in Americans' response to the COVID-19 pandemic. In counties with a higher Republican vote share, we observe an increase in mixing (0.32 SD) and traveling across counties (0.22 SD) accompanied by a slight increase in mobility to workplace (0.087 SD); these results are per one SD increase in the Republican vote share in the 2016 presidential election.

In counties with higher poverty rates, we see an increase in grocery shopping (0.16 SD) but for the most part still decreased mobility to other venues and a decline in mixing (-0.15 SD) relative to other areas. On the other hand, counties with a higher share of uninsured individuals seem to observe less of an increase in grocery shopping, relative to others.

For the most part, difference in average age across counties does not appear to have a big effect on mobility patterns, except in females ages 55-64 increasing their mobility in general and mixing (0.57 SD). At 65+, however, both females and males appear to become more risk averse, with a marked decline in mobility across all venues and a discernible reduction in mixing in counties with a greater proportion of males 65+ (-0.74 SD). Further research that uses individual-level data is needed to explore the relationship between demography and socioeconomic factors and mobility changes, as there are severe limits to what can be learned through county-level comparisons.

7 Discussion

From the the early phases of the COVID-19 epidemic, social distancing has been a central strategy. Cell phone based metrics show large declines in mobility during the lockdown phase, and evidence suggests that both government policy and private responses played crucial roles. These actions have likely reduced the spread of the virus and therefore have important social benefits. However, maintaining high levels of social distancing places a heavy burden on families, businesses, and governments. As a result, there is enormous pressure to lift some of the social distancing policies that were imposed during the lockdown phase.

State governors have faced difficult decisions when considering reopening policies. On the one hand, residents have experienced economic losses due to business closures and curtailment of other activities. Unemployment rates have skyrocketed (Rojas et al., 2020; Montenovo et al., 2020), and poverty rates in the U.S. have been projected to reach their highest levels in half a century (Parolin and Wimer, 2020). State budgets will likely face extraordinary stress in the coming months, and there have been protests and other signs of political unrest in some states. On the other hand, public health officials caution that it is too early to relax restrictions. The risk of a second wave of infections is still in the horizon, given the estimated population of infected people and an estimated high rate of asymptomatic COVID-19 carriage (Carroll, 2020; Gandhi et al., 0). Early reopening states are now reporting increases an uptick in cases (Slotkin, 2020). There is still no vaccine available, and even under optimistic scenarios, it is unlikely that a vaccine be ready for some time (Yu and Yang, 2020). Further, there is widespread concern regarding high levels of compliance necessary (i.e. masks, maintaining physical distance, handwashing, etc.) that would make reopening safer (Briscese et al., 2020).

Mobility measures from cell signals, a data source fairly new to the wider research community, have been used extensively in the last two months to understand mobility patterns in the COVID-19 era. In this study, we harness these data to study the causal effect of state reopening policies on measures of mobility. Even though state reopening decisions were made fairly recently, close-to-real-time data allow us to study movement patterns relevant to understanding the policies' effects as well as the behavioral decisions that may be guiding individuals and businesses.

Our analysis clearly shows that mobility levels started rising in most states beginning in mid-April. So far, the increase in mobility is still small compared with the declines that occurred during the lockdown phase; activity levels are not back to normal in any meaningful sense. However, the resurgence of mobility is observable across a broad range of indices. We also find that the recent increases in mobility are strongly associated with temperature and precipitation patterns. Researchers should view this result with caution: the seasonal properties of cell-device-based measures of human mobility are yet not well understood. Access to a longer time series of data is not yet available. [7]

The most notable results in our study come from event study regressions. The models suggest that state reopening policies do produce a fairly immediate increase in mobility. After four days of reopening, most measures of mobility increase by 6% to 8%. Reopening effects are the most marked in the states that were late adopters of the major closure measures. Importantly, this suggests that policy acts as a constraint on behavior to a

[7] Safegraph is currently releasing 2019 data, which will provide important insights into seasonal patterns.

different degree in those states.

We note that our study cannot draw conclusions on the extent to which Americans follow social distancing guidelines, as the cell signal data cannot discern the use of situational mitigation strategies such as mask use. States have mostly asked business that reopen to take steps to reduce transmission, and the CDC has issued guidelines on how to open safely. It is possible that these policies and strategies could mean that mobility patterns can increase without incurring a substantial increase in new cases (Hagemann, 2020). At the same time, it is fair to assume that disease transmission is positively related to the mobility measured in this study, and it is important to know to what extent mobility increases are rooted in policy vs. other factors such as fatigue, warmer weather, seasonal expectations, and a waning sense of the dangers of the virus.

Many other important lines of inquiry are emerging as lawmakers assess their COVID-19 strategies (Baqaee et al., 2020). For example, it is crucial to understand how reopening policies may affect a resurgence of cases, hospital capacity, and deaths. There is much concern among lawmakers, public health officials, and the general public that hospital systems may still become overwhelmed from new cases even in areas that have not experienced a surge thus far. Indiana's experience (Carroll, 2020) shows that from a random testing population study, only approximately 2.8% residents may have been infected at some point thus far. As human mobility increases, this raises questions regarding what to expect with regard to transmission rates, hospital capacity, and mortality, as well as for how fast the massive non-COVID societal costs imposed by closures will recover.

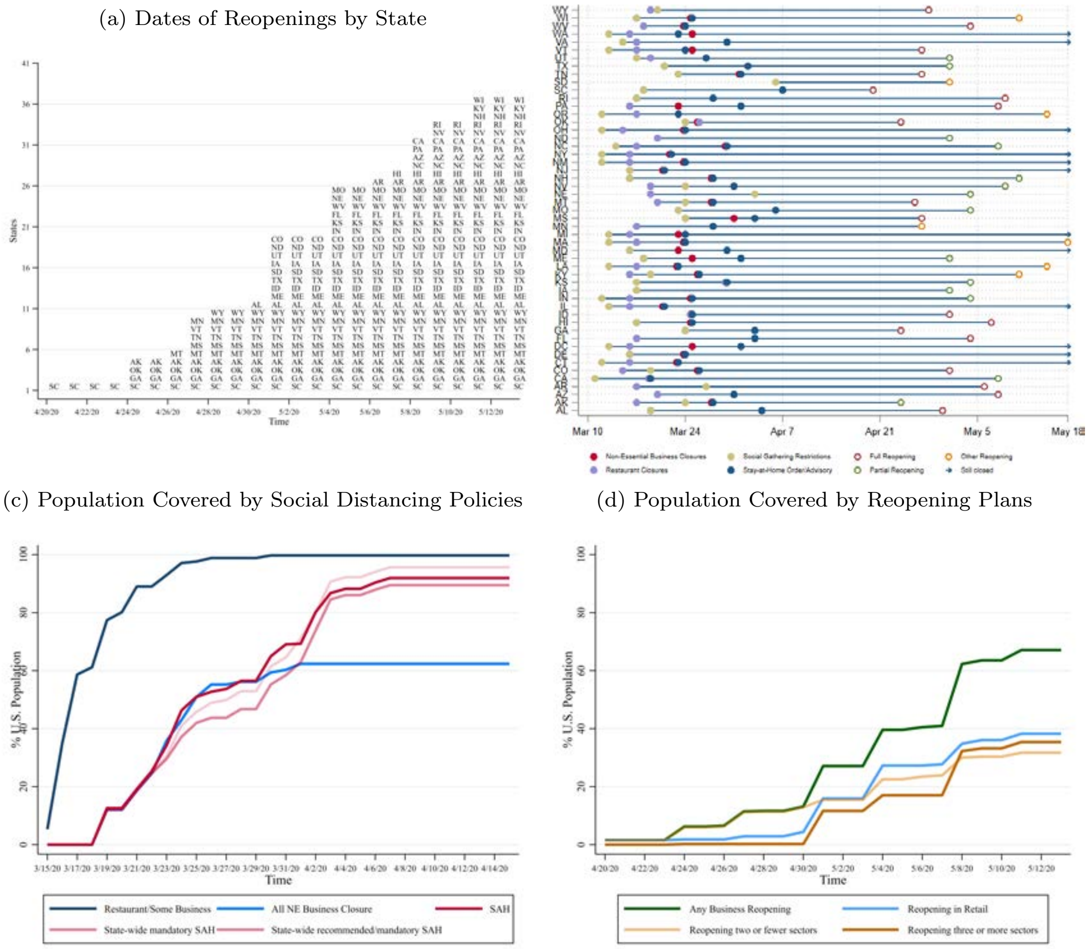
Research on social distancing policy would benefit from a stronger theoretical analysis of the incentives and constraints that shape individual and group choices regarding social distancing. One hypothesis is that people may experience *social distancing fatigue*, which could make it harder to maintain high levels of social distancing over a protracted period of time. Fatigue could stem emotional exhaustion, isolation, or boredom. If the fatigue theory is an important determinant of behavior, then it would be valuable to develop social distancing policy instruments that help mitigate fatigue.

At a more general level, it is likely that social distancing can be viewed through the lens of an economic model of home production. People produce social distance by combining their time with other inputs, and they make decisions about how much social distance to produce relative to other home production activities and other market activities. Understanding more about the complements (such as peer group behavior, media portrayals) and substitutes for social distance and its inputs may be useful for developing more effective social distancing policies. Likewise, understanding the income elasticity of social distancing would be informative.

Finally, it is paramount to dissect the factors that determine individual compliance with social distancing guidelines, something only possible at aggregate area-wide demographic levels with cell data. Some degree of variation may reflect heterogeneous preferences over health and non-health goods. However, it is also possible that people behave differently because they hold different beliefs about about health risks or are exposed to certain types of misinformation.

8 Tables and Figures

Figure 1: Timing and U.S. Population covered by States' COVID-19 Policies



(a) Dates of Reopenings by State

(b) State Policy and Information Timing

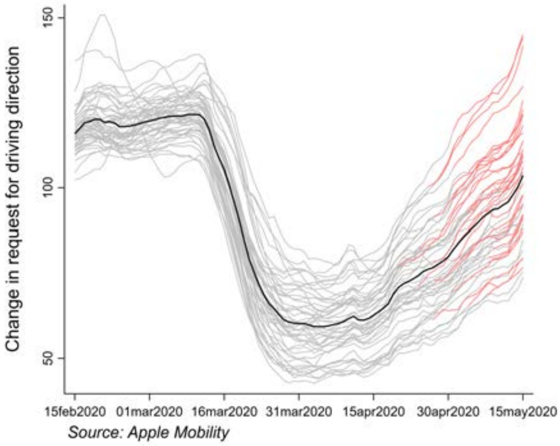
(c) Population Covered by Social Distancing Policies

(d) Population Covered by Reopening Plans

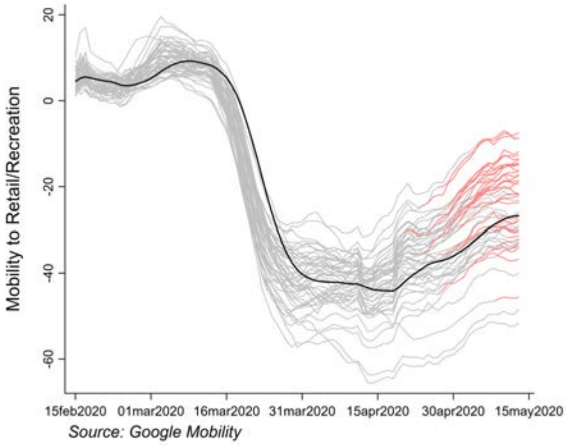
Note: Author's compilations based on New York Times and other media databases. Data covered 4/20/20-5/13/20 for (a) and (d), 1/21/20-5/18/20 for (b), and 3/15/20-4/15/20 for (c). State reopening data are available on GitHub.

Figure 2: Trend in mobility changes.

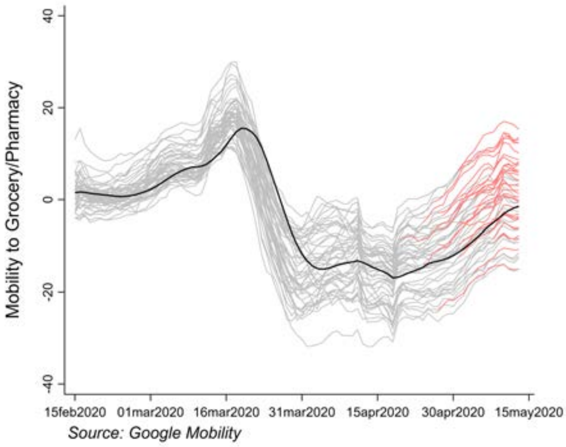
(a) Trend in requests for driving directions



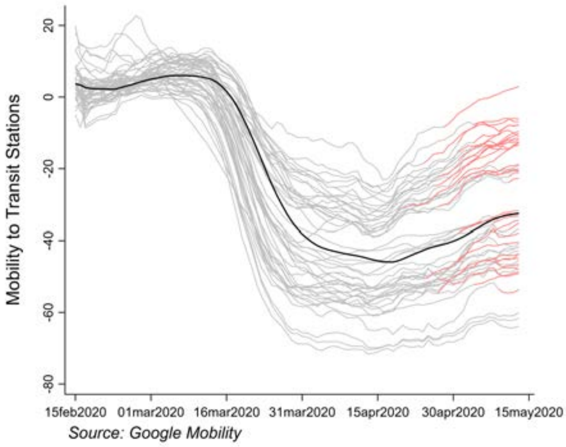
(b) Trend in mobility for retail and recreation



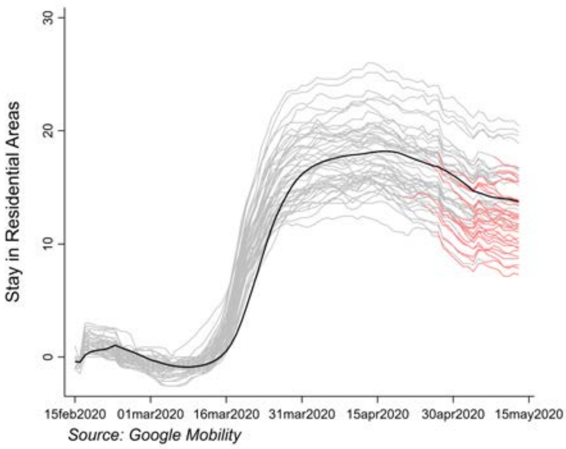
(c) Trend in mobility for grocery and pharmacy



(d) Trend in mobility to transit stations



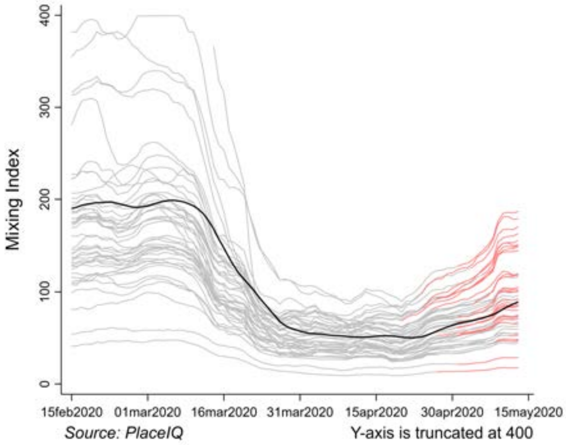
(e) Trend in staying in residential areas.



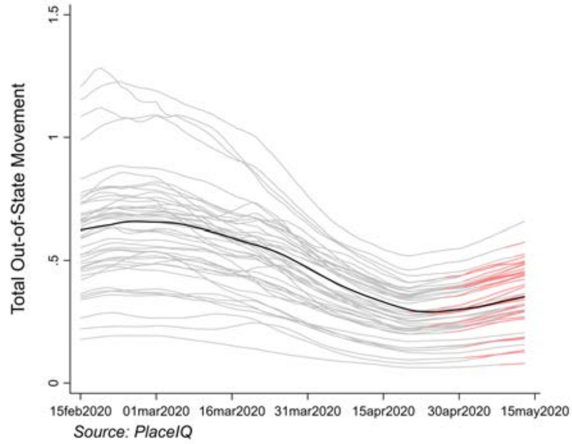
Note: Author's calculation based on data from Apple Mobility, Google Mobility, SafeGraph Aggregated Mobility Metrics and PlaceIQ smart device data. Each grey line represents a state. Red lines represent states which re-opened, for the period after the re-opening. The thick black line represents a "smoothed" 7 day moving average of the states.

Figure 2: (Contd.).

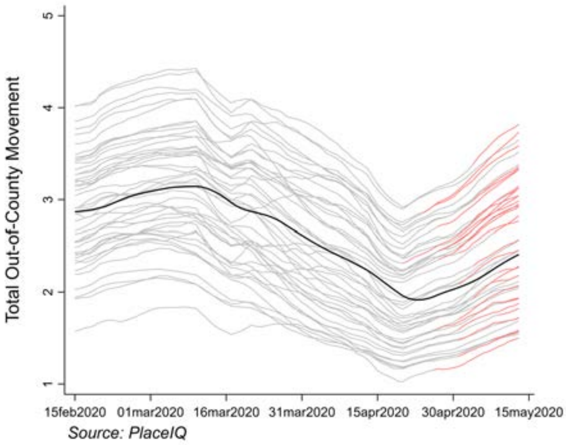
(f) Trend in mixing index.



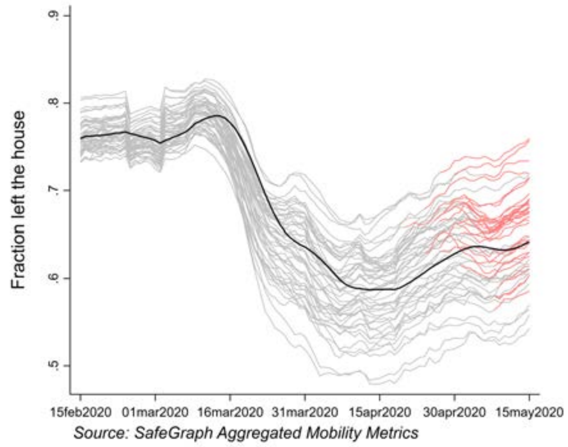
(g) Trend in out-of-state movement.



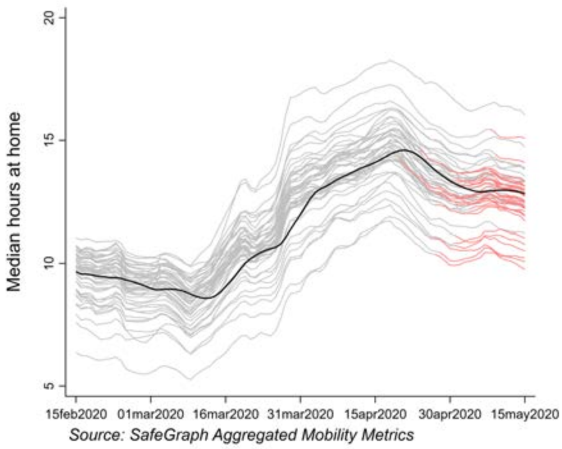
(h) Trend in average out-of-county movement.



(i) Trend in fraction leaving home.

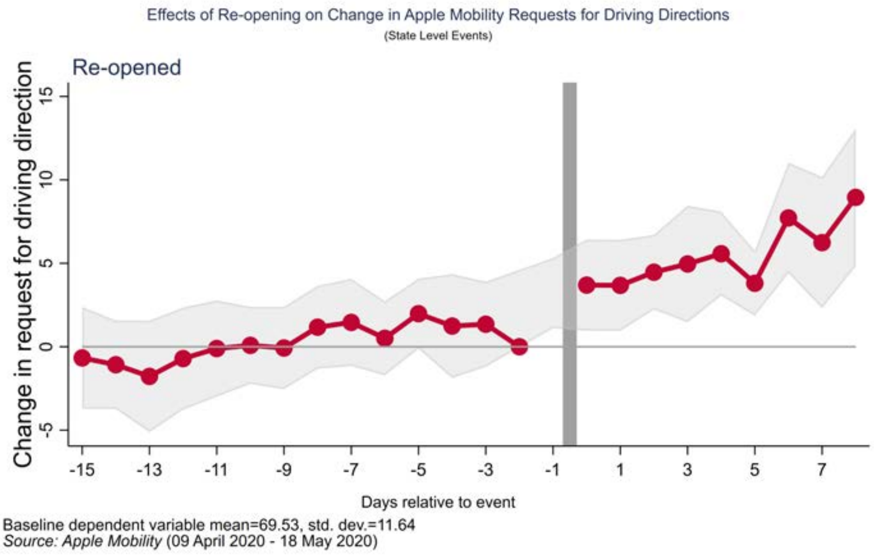


(j) Trend in median hours at home.



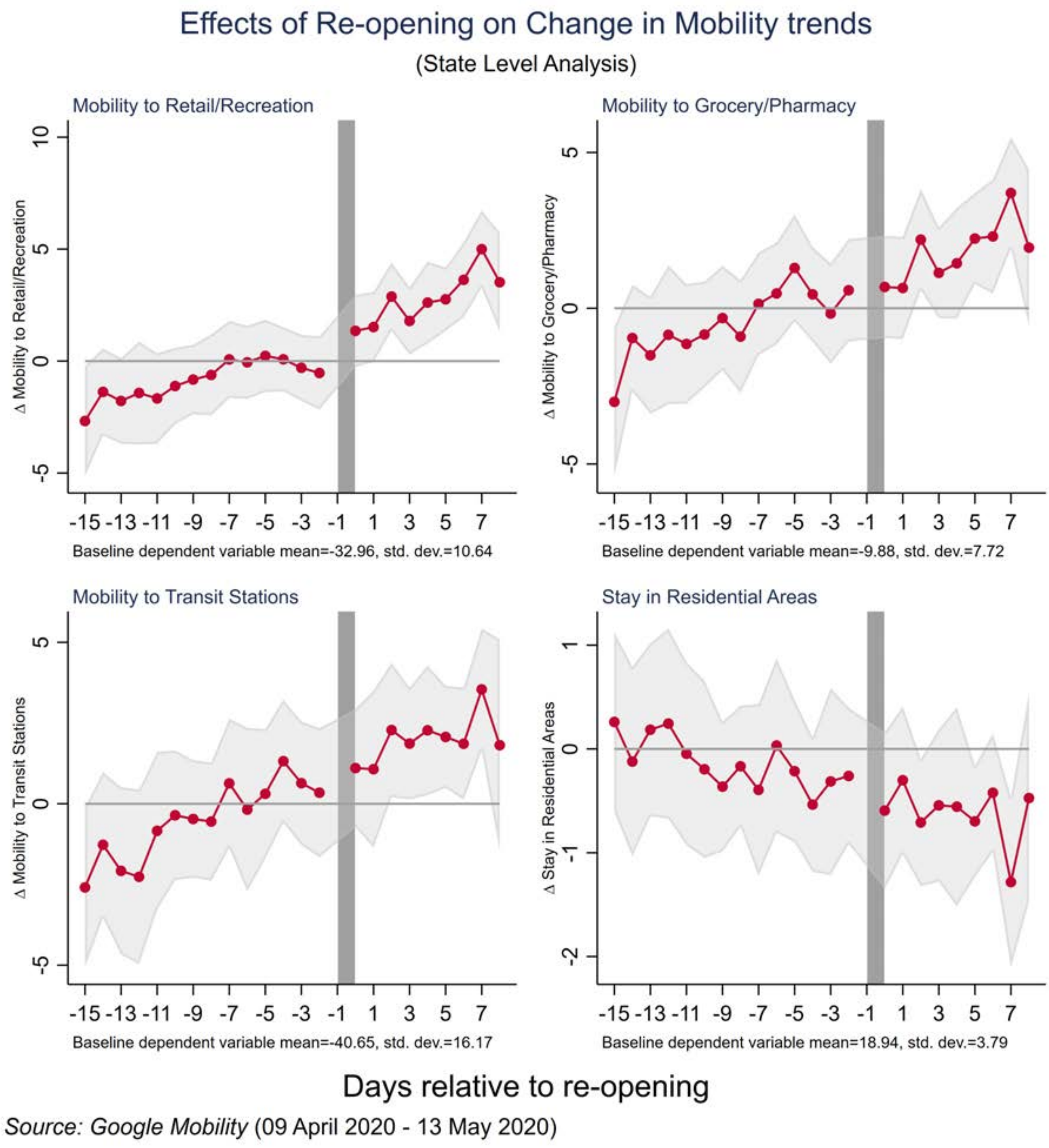
Note: Author's calculation based on data from Apple Mobility, Google Mobility, SafeGraph Aggregated Mobility Metrics and PlaceIQ smart device data. Measures of out-of-state and average out-of-county travel measure 14-day lagged rates of travel outside of the "home state" and "home county". Each grey line represents a state. Red lines represent states which re-opened, for the period after the re-opening. The thick black line represents a "smoothed" 7 day moving average of the states.

Figure 3: Event study regression coefficients and 95 percent confidence interval.



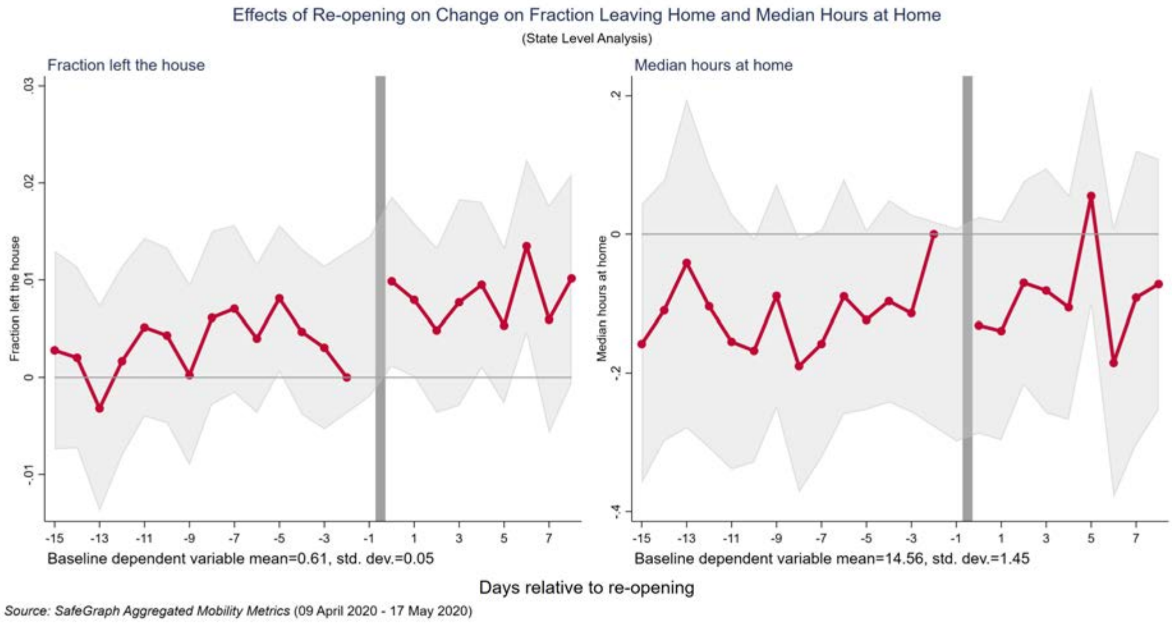
Note: Author's calculation based on smart device movement data from Apple Mobility. Estimation sample window is April 8, 2020-May 18, 2020. N=1680. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

Figure 4: Event study regression coefficients and 95 percent confidence interval.



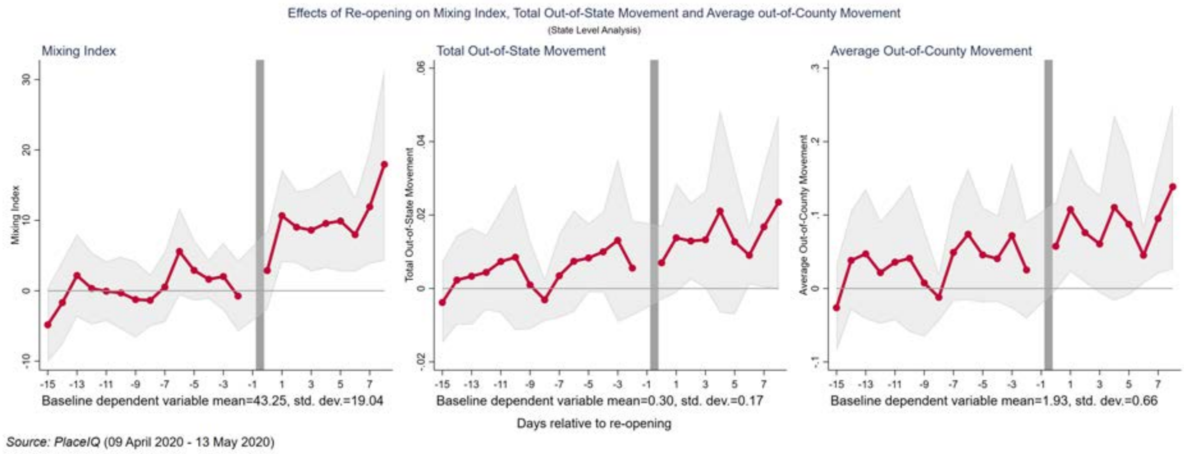
Note: Author's calculation based on smart device movement data from Google Mobility. Each panel is a separate dependent variable. Estimation sample window is April 8, 2020-May 13, 2020. N=1680. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

Figure 5: Event study regression coefficients and 95 percent confidence interval.



Note: Author's calculation based on smart device movement data from SafeGraph Aggregated Mobility Metrics. Each panel is a separate dependent variable. Estimation sample window is April 8, 2020-May 17, 2020.

Figure 6: Event study regression coefficients and 95 percent confidence interval.



Note: Author's calculation based on smart device movement data from PlaceIQ. Each panel is a separate dependent variable. Measure of out-of-state and average out-of-county travel capture 14-day lagged rates of travel outside of the "home state" and "home county". Estimation sample window is April 8, 2020-May 13, 2020. N=1680. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

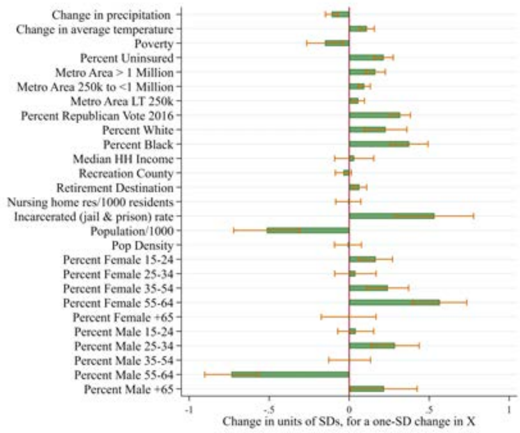
Table 1: Effect Sizes: Percentage magnitude effects of any re-opening on social distancing measures.

	(1) Any Re-Opening	(2) Rural	(3) Urban	(4) Shorter SAH	(5) Longer SAH	(6) County Pairs Analysis: No Spillovers	(7) County Pairs Analysis: Spillovers	(8) High Death Rates	(9) Low Death Rates
Effects After 4 Days									
Geographic Unit	*State*	*County*	*County*	*State*	*State*	*County*	*County*	*State*	*State*
Mobility Measures									
Request for driving directions	7%***	-	-	8%***	6%	-	-	10%*	6%*
Mobility to retail/recreation	8%***	16%	13%**	14%***	2%	12%	13%***	8%*	10%***
Mobility to Grocery/Pharmacy	15%	10%	15%	24%***	6%	24%	35%*	7%	26%
Mobility to Transit Stations	6%**	1%	7%	8%***	4%	8%*	9%*	8%**	4%
Fraction left home	1%	-2%*	3%***	3%***	-0.2%	0.3%	0.4%	2%	1%
Mixing Index	22%***	2%	30%***	14%	6%	8%	8%***	33%**	2%
Out of state movement	7%	-	-	31%***	9%			3%	15%
Out of county movement	7%*	2%	24%**	3%	1%	3%	1%	12%	9%
Absence of Mobility Measures									
Stay in Residential Areas	-3%	1%	-5%	-9%***	5%	-5%	-3%**	-8%*	-1%
Median hours at home	-1%	0%	2%***	-2%*	1%	-0.6%	-1%*	-1%	-0.3%

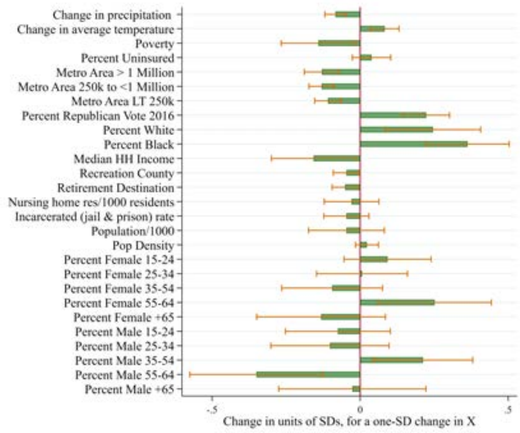
Note: ** and bolded text denotes effect sizes with * $p < 0.1$, ** $p < 0.05$ *** $p < 0.01$. Grey shaded cells denote violation of pre-treatment parallel trends—we do not denote statistical significance for these cells. Effect sizes are estimated using coefficients in the event-study tables, divided by the dependent variable value as of April 15, 2020

Figure 7: County Level Correlates of Mobility Measures

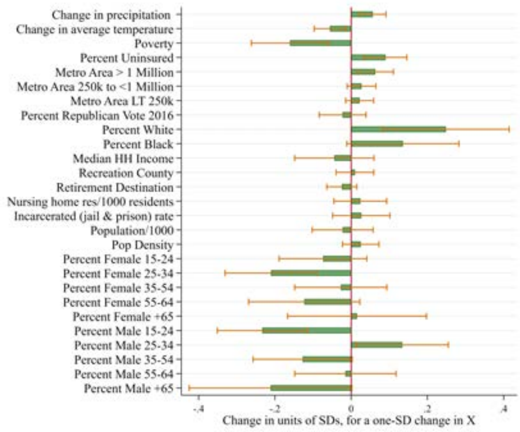
(a) Long difference in mixing index



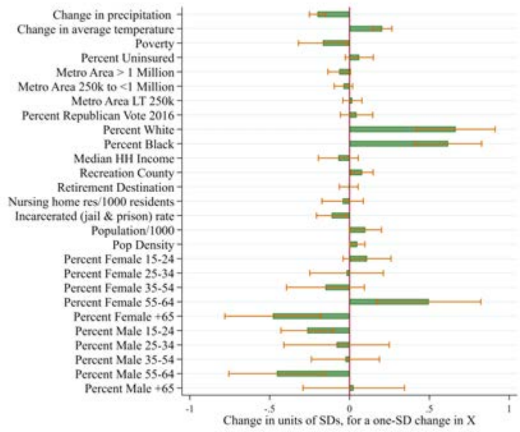
(b) Long difference in Out-of-county movement



(c) Long difference in fraction of devices leaving home



(d) Long difference in retail and recreation



Note: Specification: simple OLS using cross-sectional data at county level. Each figure represents standardized coefficients and their 95% CIs from a separate regression, where the dependent variable is the outcome listed (long differences between April and May 6). Sources of county characteristics: PlaceIQ, Safegraph, Area Health Resource File and County Health Rankings; we use the latest year available in each original source.

Table 2: County Level Correlates of Mobility Measures.

	(1) **Retail** *driving*	(2) **Grocery** *driving*	(3) **Transit** *driving*	(4) **Workplace** *driving*	(5) **Residential** *driving*	(6) **Out-of-county** *movement*	(7) **Mixing** *index*	(8) **Devices** *leaving home*	(9) **Devices leaving** *home & workplace*	(10) **Devices** *at workplace*	(11) **High** *in cases*	(12) **High** *in deaths*
Change in precipitation between April 15 and May 6	-0.20***	-0.072***	-0.096***	-0.13***	0.23***	-0.083***	-0.11***	0.057***	-0.062***	-0.19***	-0.062***	-0.14***
Change in average temperature (C) between April 15 and May 6	0.20***	0.21***	0.018	-0.076***	-0.16***	0.083***	0.11***	-0.056***	0.039*	0.16***	-0.099***	-0.025
Poverty	-0.17**	0.16*	-0.028	0.072	0.29***	-0.14**	-0.15***	-0.16***	-0.13**	0.11**	0.20***	0.22***
Percent Uninsured	0.061	-0.15***	-0.094*	0.016	0.020	0.039	0.22***	0.090***	0.063**	-0.077***	-0.021	-0.065**
Metro Area > 1 Million	-0.065*	-0.0026	-0.0075	-0.023	0.073**	-0.13***	0.16***	0.063***	0.069***	-0.018	0.10***	-0.051
Metro Area 250k to <1 Million	-0.038	-0.023	-0.030	0.016	0.0051	-0.13***	0.095***	0.027	0.032	-0.0036	0.035	-0.032
Metro Area LT 250k	0.018	-0.021	-0.0034	0.0099	0.012	-0.11***	0.057***	0.022	0.032*	0.0044	0.059**	-0.0047
Percent Republican Vote 2016	0.044	-0.054	0.071	0.087***	0.054	0.22***	0.32***	-0.023	0.054*	0.12***	-0.084**	0.039
Percent White	0.66***	0.36***	0.29***	0.32***	-0.63***	0.25***	0.23***	0.25***	0.41***	0.13*	-0.088	-0.10
Percent Black	0.62***	0.21**	0.33***	0.25***	-0.58***	0.36***	0.38***	0.14*	0.39***	0.31***	-0.083	0.14**
Median HH Income	-0.070	-0.019	-0.096	-0.020	0.13*	-0.16**	0.031	-0.044	0.0068	0.092**	0.33***	0.27***
Recreation County	0.079**	0.080**	0.028	0.050**	-0.069**	-0.046**	-0.036	0.0099	0.018	0.0074	-0.053**	0.043*
Retirement Destination	-0.0054	-0.066*	-0.0024	0.026	0.051*	-0.051**	0.064***	-0.025	-0.019	0.018	-0.014	-0.045*
Nursing home res/1000 residents	-0.043	-0.035	-0.11	0.013	0.097	-0.029	-0.0060	0.024	-0.016	-0.067**	0.016	0.087***
Incarcerated (jail & prison) rate	-0.11**	-0.068	-0.094**	-0.062	0.15***	-0.046	0.53***	0.026	0.020	-0.020	0.051	0.019
Population/1000	0.099*	0.070	0.072	0.068	-0.18***	-0.046	-0.52***	-0.022	-0.0037	0.036	-0.095	-0.062
Pop Density	0.049**	-0.073**	0.0032	0.036	-0.057**	0.023	-0.0081	0.025	0.0034	-0.041	0.090*	0.11**
Percent Female 15-24	0.11	0.0072	0.088	0.021	0.094	0.093	0.17***	-0.074	-0.13**	-0.055	0.072	-0.074
Percent Female 25-34	-0.018	0.0022	-0.22**	-0.014	0.060	0.0065	0.039	-0.21***	-0.16***	0.15***	-0.052	-0.10
Percent Female 35-54	-0.15	-0.13	-0.049	0.15**	0.33***	-0.095	0.24***	-0.027	-0.055	-0.029	0.063	0.048
Percent Female 55-64	0.50***	0.44**	0.29	0.18**	-0.24*	0.25**	0.57***	-0.12*	-0.12	0.060	0.11	-0.053
Percent Female +65	-0.48***	-0.42***	-0.56**	-0.30***	0.63***	-0.13	-0.0035	0.015	-0.038	-0.083	0.29***	0.11
Percent Male 15-24	-0.26***	-0.17*	-0.34***	-0.00032	0.23***	-0.075	0.041	-0.23***	-0.24***	0.081	-0.020	-0.0079
Percent Male 25-34	-0.081	-0.13	0.20	0.13	0.20	-0.10	0.29***	0.13**	0.028	-0.21***	-0.025	-0.056
Percent Male 35-54	-0.026	0.0041	-0.21*	-0.081	0.097	0.21**	0.0029	-0.13*	-0.080	0.12**	0.034	-0.071
Percent Male 55-64	-0.46***	-0.42**	-0.22	-0.055	0.30**	-0.35***	-0.74***	-0.015	-0.13*	-0.16**	-0.18**	-0.013
Percent Male +65	0.026	0.13	0.14	0.25**	-0.011	-0.026	0.22**	-0.21*	-0.16	0.17*	-0.15	-0.21**
Dep. Variable Mean	8.18	7.95	9.44	5.27	-3.22	0.65	29.80	0.03	0.03	0.00	0.11	0.11
Dep. Variable SD	6.22	5.97	9.42	3.64	1.65	0.30	17.39	0.04	0.03	0.02	0.31	0.31
Obs.	1001	944	752	1947	997	1531	1531	2030	2030	2030	2031	2031
R-squared	0.22	0.13	0.09	0.10	0.23	0.23	0.44	0.09	0.15	0.14	0.15	0.12

Note: Specification: simple OLS using cross-sectional data at county level. Column represents standardized coefficients from a separate regression, where the dependent variable is the outcome listed (long differences between April 15 and May 6, the number of observations varies across different data samples). Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$ *** $p < 0.01$.

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Appendix

A Regression Tables of Event Study Analyses

Table A1: Effects of any re-opening on requests for driving directions.

	(1) Change in request for driving direction
15 days prior to event	-2.350
	(1.726)
14 days prior to event	-0.669
	(1.558)
13 days prior to event	-1.078
	(1.355)
12 days prior to event	-1.777
	(1.705)
11 days prior to event	-0.710
	(1.564)
10 days prior to event	-0.107
	(1.473)
9 days prior to event	0.0816
	(1.180)
8 days prior to event	-0.0761
	(1.260)
7 days prior to event	1.171
	(1.270)
6 days prior to event	1.457
	(1.334)
5 days prior to event	0.507
	(1.137)
4 days prior to event	1.980*
	(1.073)
3 days prior to event	1.240
	(1.591)
2 days prior to event	1.350
	(1.303)
Day of event	3.215***
	(1.076)
1 day after event	3.693**
	(1.399)
2 days after event	3.678**
	(1.393)
3 days after event	4.466***
	(1.142)
4 days after event	4.959***
	(1.784)
5 days after event	5.575***
	(1.285)
6 days after event	3.803***
	(0.995)
7 days after event	7.719***
	(1.690)
8 days after event	6.240***
	(2.010)
9 days after event	8.950***
	(2.105)
prcp	-0.0914***
	(0.022)
tavg	0.128*
	(0.070)
Observations	1680
Baseline DV mean	69.53

Note: Author's calculation based on smart device movement data from Apple Mobility. Table presents coefficients and standard errors from the the event-study estimation in equation (2). Estimation sample window is April 8, 2020-May 18, 2020. All models include state fixed effects and date fixed effects. Standard errors clustered at state level are presented in parentheses. Baseline dependent variable mean as of April 15, 2020. * $p < 0.1$, ** $p < 0.05$ *** $p < 0.01$.

Table A2: Effects of any re-opening on change in mobility trends to different locations.

	(1) Retail/Recreation	(2) Grocery/Pharmacy	(3) Transit Stations	(4) Workplace	(5) Residential Areas
15 days prior to event	-2.677**	-3.002**	-2.591**	0.144	0.260
	(1.253)	(1.215)	(1.264)	(0.510)	(0.441)
14 days prior to event	-1.376	-0.955	-1.271	0.0738	-0.122
	(0.992)	(0.873)	(1.153)	(0.762)	(0.467)
13 days prior to event	-1.780*	-1.510	-2.079	0.144	0.185
	(0.974)	(0.960)	(1.323)	(0.760)	(0.426)
12 days prior to event	-1.424	-0.853	-2.264	0.0889	0.244
	(1.176)	(1.136)	(1.380)	(0.647)	(0.467)
11 days prior to event	-1.669	-1.146	-0.840	0.607	-0.0463
	(1.030)	(0.978)	(1.247)	(0.623)	(0.450)
10 days prior to event	-1.114	-0.842	-0.357	0.310	-0.196
	(0.872)	(0.866)	(1.025)	(0.603)	(0.436)
9 days prior to event	-0.825	-0.314	-0.471	0.498	-0.363
	(0.791)	(0.857)	(0.929)	(0.536)	(0.319)
8 days prior to event	-0.620	-0.910	-0.551	0.207	-0.167
	(0.923)	(0.925)	(0.936)	(0.353)	(0.298)
7 days prior to event	0.0740	0.141	0.630	0.137	-0.394
	(0.879)	(0.839)	(1.020)	(0.744)	(0.422)
6 days prior to event	-0.0542	0.475	-0.180	0.510	0.0330
	(0.832)	(0.836)	(1.293)	(0.772)	(0.429)
5 days prior to event	0.232	1.293	0.308	0.392	-0.215
	(0.824)	(0.877)	(1.023)	(0.639)	(0.350)
4 days prior to event	0.0780	0.447	1.320	0.808	-0.536
	(0.732)	(0.777)	(0.973)	(0.519)	(0.331)
3 days prior to event	-0.300	-0.171	0.636	0.509	-0.313
	(0.756)	(0.824)	(0.978)	(0.630)	(0.460)
2 days prior to event	-0.532	0.578	0.342	0.391	-0.260
	(0.835)	(0.841)	(1.019)	(0.538)	(0.335)
Day of event	1.349	0.684	1.105	0.693	-0.593
	(0.823)	(0.838)	(0.933)	(0.720)	(0.385)
1 day after event	1.520*	0.650	1.071	1.589**	-0.301
	(0.797)	(0.837)	(1.244)	(0.767)	(0.363)
2 days after event	2.882***	2.202**	2.283**	1.339*	-0.708**
	(0.782)	(0.829)	(1.063)	(0.674)	(0.313)
3 days after event	1.793**	1.136	1.864**	1.517**	-0.544
	(0.767)	(0.743)	(0.883)	(0.577)	(0.375)
4 days after event	2.614***	1.444	2.276**	1.383**	-0.556
	(0.934)	(0.906)	(1.023)	(0.679)	(0.489)
5 days after event	2.759***	2.239***	2.071**	1.316**	-0.697**
	(0.723)	(0.746)	(0.807)	(0.612)	(0.273)
6 days after event	3.629***	2.306**	1.859**	0.903***	-0.422
	(0.855)	(0.934)	(0.887)	(0.298)	(0.291)
7 days after event	5.001***	3.700***	3.543***	1.857***	-1.283***
	(0.881)	(0.913)	(0.962)	(0.654)	(0.419)
8 days after event	3.524***	1.946	1.815	2.334**	-0.473
	(1.132)	(1.280)	(1.672)	(0.962)	(0.511)
Precipitation	-0.0264**	-0.0119	-0.0571***	-0.0185**	0.0316***
	(0.012)	(0.017)	(0.016)	(0.007)	(0.008)
Temperature	-0.00563	0.0660**	0.0133	-0.0549***	-0.0181
	(0.034)	(0.032)	(0.042)	(0.016)	(0.013)
Observations	1680	1680	1680	1680	1680
Baseline DV mean	-32.96	-9.880	-40.65	-47.27	18.94

Note: Author's calculation based on smart device movement data from Google Mobility. Table presents coefficients and standard errors from the the event-study estimation in equation (2). Each column is based on a separate regression. Estimation sample window is April 8, 2020-May 13, 2020. All models include state fixed effects and date fixed effects. Standard errors clustered at state level are presented in parentheses. Baseline dependent variable mean as of April 15, 2020. * $p < 0.1$, ** $p < 0.05$ *** $p < 0.01$.

Table A3: Effects of any re-opening on fraction of devices leaving home and median time at home.

	(1) Fraction Leaving Home	(2) Median Time at Home
15 days prior to event	-0.00383	0.0409
	(0.007)	(0.125)
14 days prior to event	0.00279	-0.158
	(0.005)	(0.103)
13 days prior to event	0.00203	-0.110
	(0.005)	(0.096)
12 days prior to event	-0.00320	-0.0415
	(0.005)	(0.121)
11 days prior to event	0.00167	-0.104
	(0.005)	(0.104)
10 days prior to event	0.00515	-0.155
	(0.005)	(0.094)
9 days prior to event	0.00431	-0.168**
	(0.005)	(0.082)
8 days prior to event	0.000236	-0.0891
	(0.005)	(0.083)
7 days prior to event	0.00612	-0.190**
	(0.005)	(0.093)
6 days prior to event	0.00704	-0.158*
	(0.004)	(0.084)
5 days prior to event	0.00399	-0.0896
	(0.004)	(0.087)
4 days prior to event	0.00811**	-0.123*
	(0.004)	(0.066)
3 days prior to event	0.00469	-0.0966
	(0.004)	(0.074)
2 days prior to event	0.00305	-0.114
	(0.004)	(0.073)
Day of event	0.00620	-0.145*
	(0.004)	(0.078)
1 day after event	0.00986**	-0.131
	(0.004)	(0.080)
2 days after event	0.00795*	-0.139*
	(0.004)	(0.081)
3 days after event	0.00484	-0.0702
	(0.004)	(0.075)
4 days after event	0.00770	-0.0814
	(0.005)	(0.090)
5 days after event	0.00950**	-0.106
	(0.004)	(0.083)
6 days after event	0.00532	0.0553
	(0.004)	(0.081)
7 days after event	0.0135***	-0.185*
	(0.005)	(0.099)
8 days after event	0.00594	-0.0916
	(0.006)	(0.108)
9 days after event	0.0101*	-0.0721
	(0.006)	(0.092)
prcp	-0.000482***	0.00291*
	(0.000)	(0.002)
tavg	-0.000223	-0.00595
	(0.000)	(0.004)
Observations	1776	1776
Baseline DV mean	0.610	14.56

Note: Author's calculation based on smart device movement data from SafeGraph Aggregated Mobility Metrics. Table presents coefficients and standard errors from the the event-study estimation in equation (2). Each column is based on a separate regression. Estimation sample window is April 8, 2020-May 17, 2020. All models include state fixed effects and date fixed effects. Standard errors clustered at state level are presented in parentheses. Baseline dependent variable mean as of April 15, 2020. * $p < 0.1$, ** $p < 0.05$ *** $p < 0.01$.

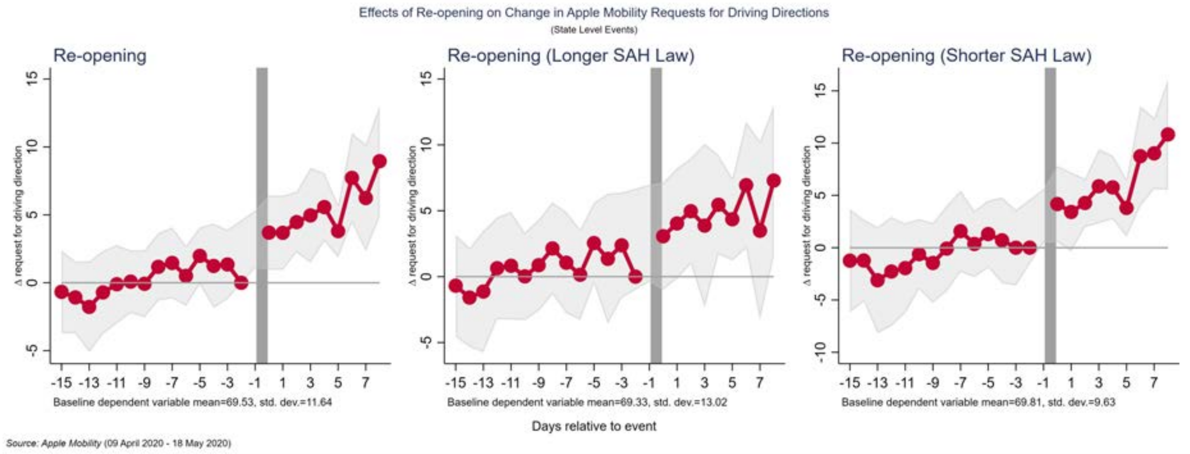
Table A4: Effects of any re-opening on mixing index, out-of-state and out-of-county movement.

	(1) Mixing Index	(2) Total out-of-state movement	(3) Average Out-of-County movement
15 days prior to event	-4.832*	-0.00381	-0.0262
	(2.687)	(0.006)	(0.030)
14 days prior to event	-1.678	0.00228	0.0382
	(3.031)	(0.006)	(0.034)
13 days prior to event	2.181	0.00333	0.0471
	(3.004)	(0.007)	(0.045)
12 days prior to event	0.333	0.00439	0.0216
	(2.618)	(0.005)	(0.036)
11 days prior to event	-0.0540	0.00733	0.0358
	(2.158)	(0.007)	(0.040)
10 days prior to event	-0.297	0.00850	0.0410
	(2.614)	(0.010)	(0.051)
9 days prior to event	-1.244	0.000980	0.00756
	(2.762)	(0.006)	(0.037)
8 days prior to event	-1.350	-0.00309	-0.0120
	(1.866)	(0.003)	(0.016)
7 days prior to event	0.538	0.00346	0.0491
	(2.569)	(0.006)	(0.034)
6 days prior to event	5.597*	0.00741	0.0739
	(3.157)	(0.007)	(0.046)
5 days prior to event	2.913	0.00830*	0.0457
	(2.182)	(0.005)	(0.033)
4 days prior to event	1.641	0.00999*	0.0406
	(1.427)	(0.006)	(0.030)
3 days prior to event	2.031	0.0131	0.0719
	(2.453)	(0.011)	(0.050)
2 days prior to event	-0.743	0.00555	0.0252
	(2.579)	(0.007)	(0.034)
Day of event	2.877	0.00700	0.0574*
	(2.833)	(0.005)	(0.030)
1 day after event	10.67***	0.0138*	0.108**
	(3.361)	(0.008)	(0.043)
2 days after event	9.037***	0.0129**	0.0761**
	(2.593)	(0.005)	(0.035)
3 days after event	8.617***	0.0133*	0.0606*
	(3.009)	(0.007)	(0.034)
4 days after event	9.573***	0.0211	0.110*
	(3.224)	(0.014)	(0.065)
5 days after event	9.917***	0.0127	0.0874*
	(3.653)	(0.010)	(0.049)
6 days after event	7.972***	0.00903**	0.0452**
	(2.674)	(0.004)	(0.020)
7 days after event	11.93***	0.0168*	0.0951**
	(4.129)	(0.008)	(0.038)
8 days after event	17.95**	0.0235*	0.139**
	(6.975)	(0.012)	(0.058)
9 days after event	21.10***	0.0260**	0.161***
	(5.847)	(0.013)	(0.056)
prcp	-0.0423	-0.0000484	-0.000438
	(0.029)	(0.000)	(0.000)
tavg	-0.167	0.000261	-0.000519
	(0.133)	(0.000)	(0.002)
Observations	1680	1680	1680
Baseline DV mean	43.25	0.300	1.930

Note: Author's calculation based on smart device movement data from PlaceIQ. Table presents coefficients and standard errors from the the event-study estimation in equation (2). Each column is based on a separate regression. Measure of out-of-state and average out-of-county travel capture 14-day lagged rates of travel outside of the "home state" and "home county". Estimation sample window is April 8, 2020-May 13, 2020. All models include state fixed effects and date fixed effects. Standard errors clustered at state level are presented in parentheses. Baseline dependent variable mean as of April 15, 2020. * p < 0.1, ** p < 0.05 *** p < 0.01.

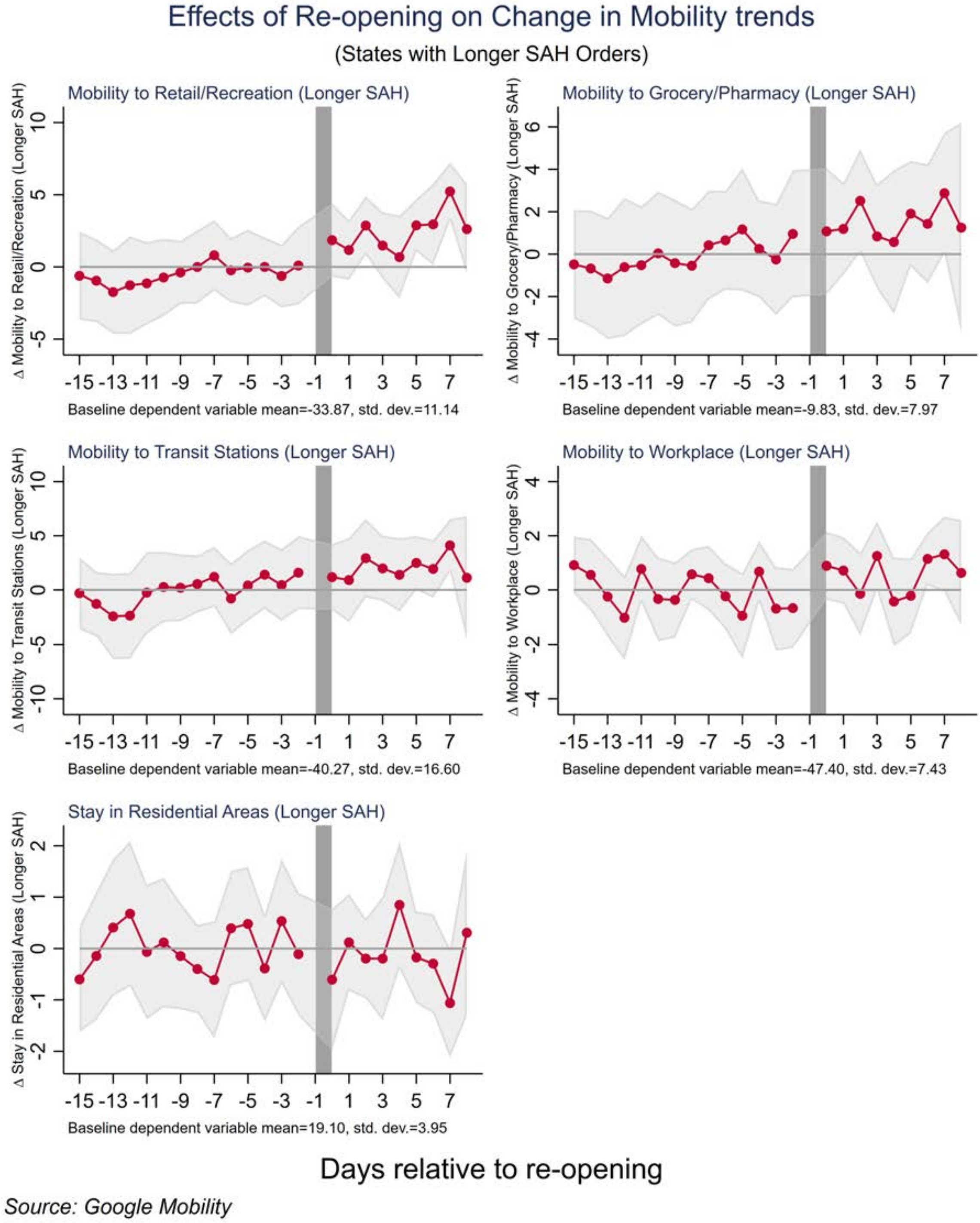
B Duration of Stay-at-Home Orders

Figure B1: Event study regression coefficients and 95 percent confidence interval.



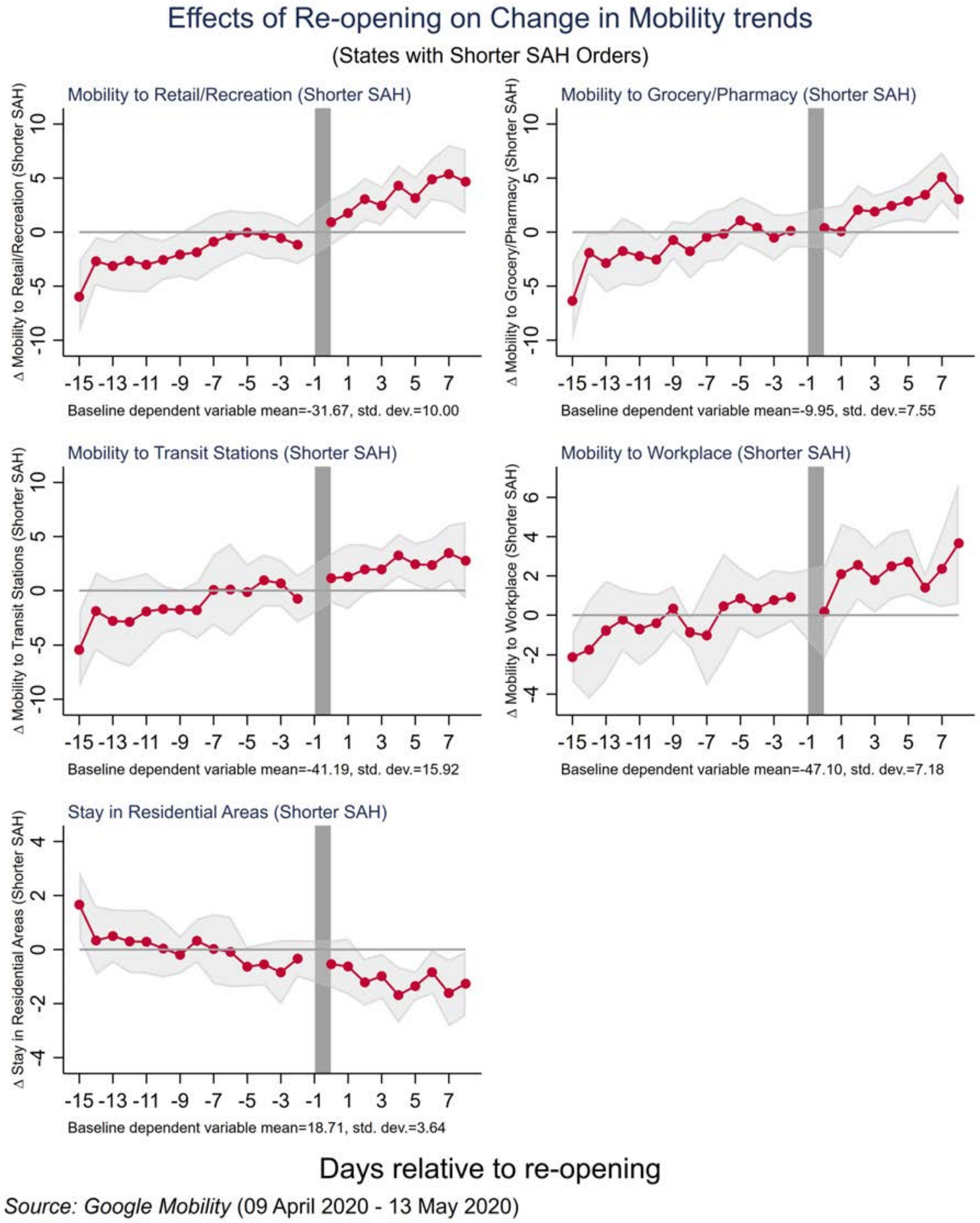
Note: Author's calculation based on smart device movement data from Apple Mobility. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 18, 2020. Longer/shorter Stay-at-home orders are defined as those implemented more/less than the 25 days (median) prior to re-opening. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level.Baseline dependent variable mean as of April 15, 2020.

Figure B2: Event study regression coefficients and 95 percent confidence interval.



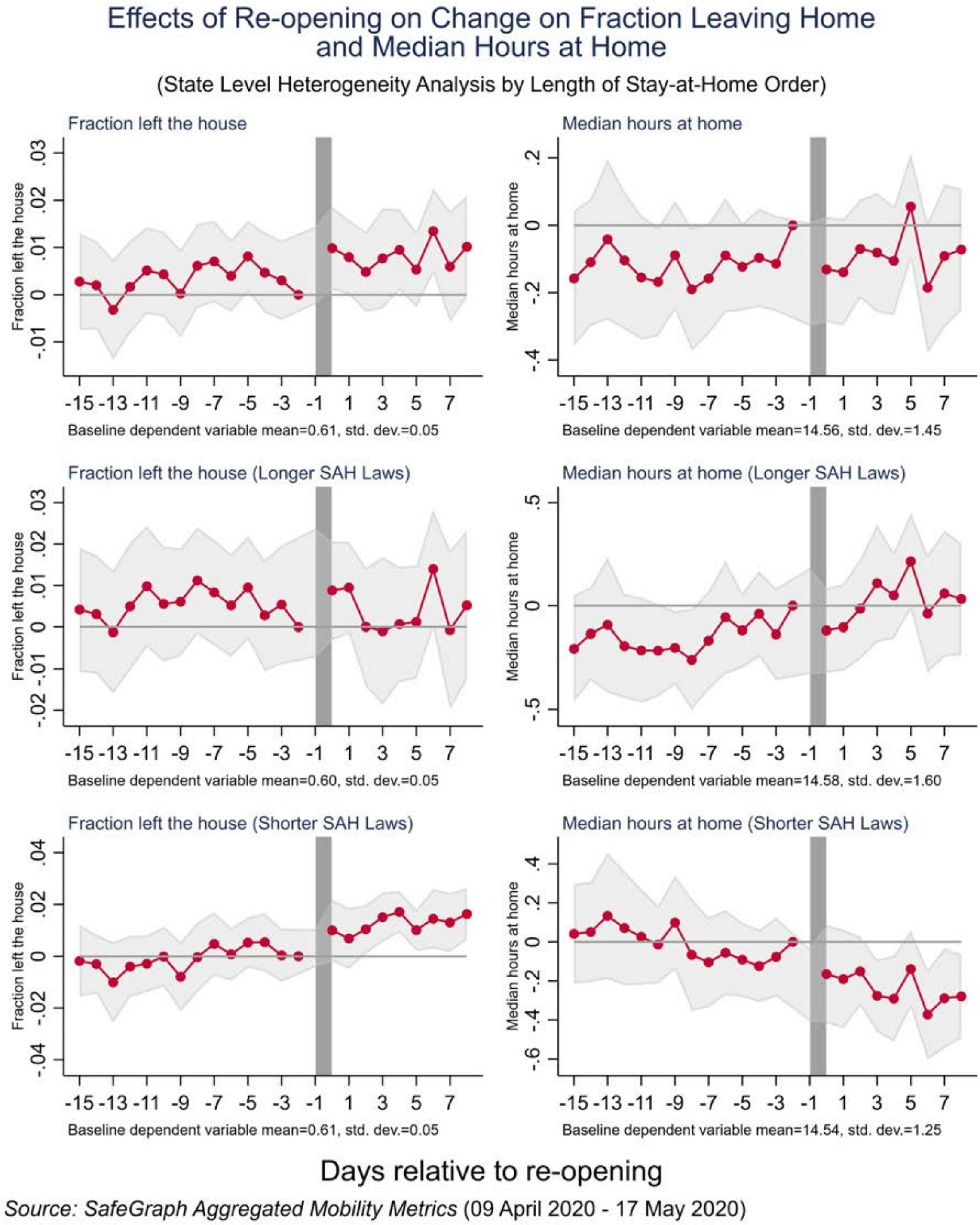
Note: Author's calculation based on smart device movement data from Google Mobility. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 13, 2020. Longer Stay-at-home orders are defined as those implemented more than 25 days (median) prior to re-opening. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

Figure B3: Event study regression coefficients and 95 percent confidence interval.



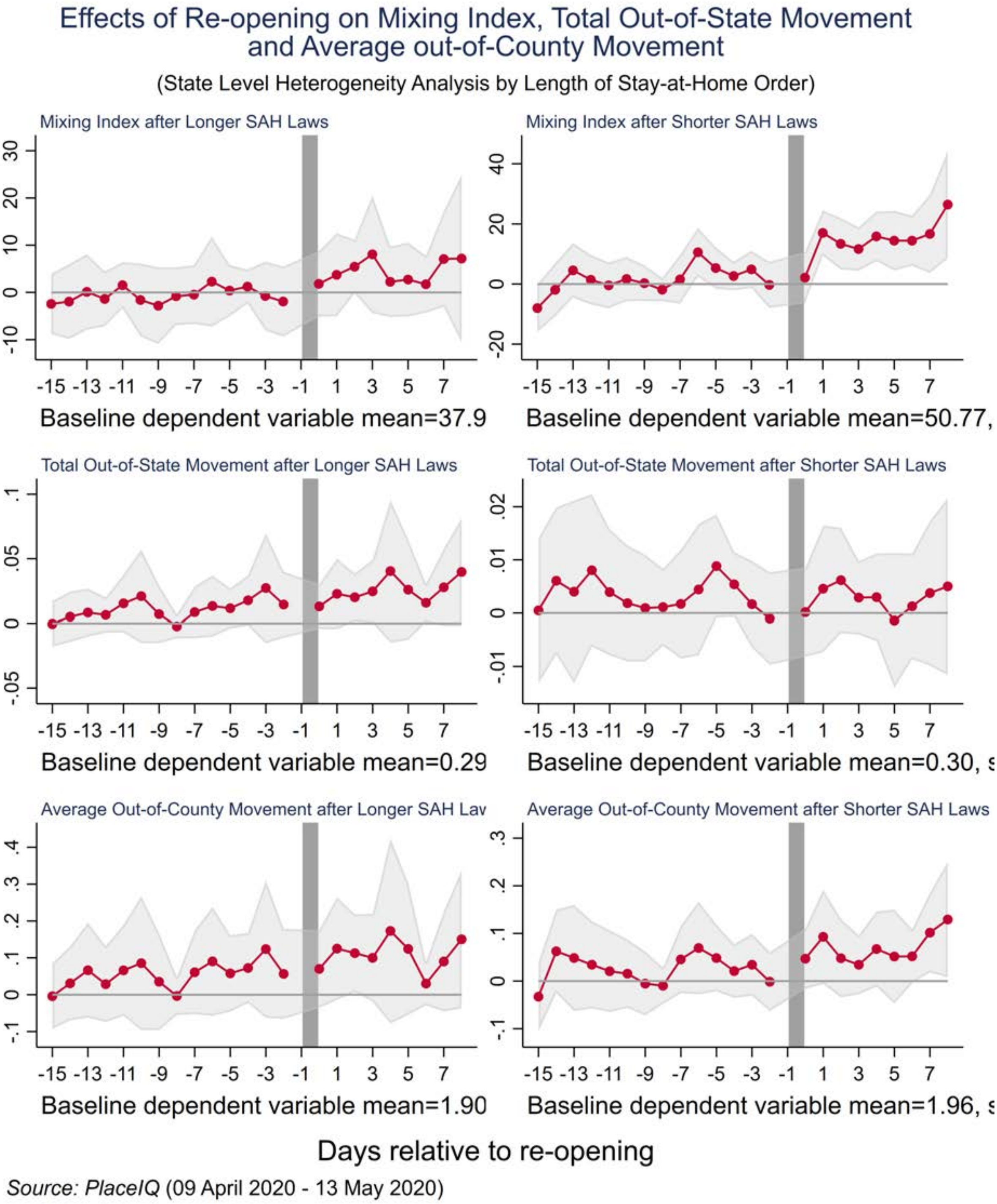
Note: Author's calculation based on smart device movement data from Google Mobility. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 13, 2020. Shorter Stay-at-home orders are defined as those implemented less than 25 days (median) prior to re-opening. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

Figure B4: Event study regression coefficients and 95 percent confidence interval.



Note: Author's calculation based on smart device movement data from SafeGraph Aggregated Mobility Metrics. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 17, 2020. Longer/shorter Stay-at-home orders are defined as those implemented more/less than the 25 days (median) prior to re-opening. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

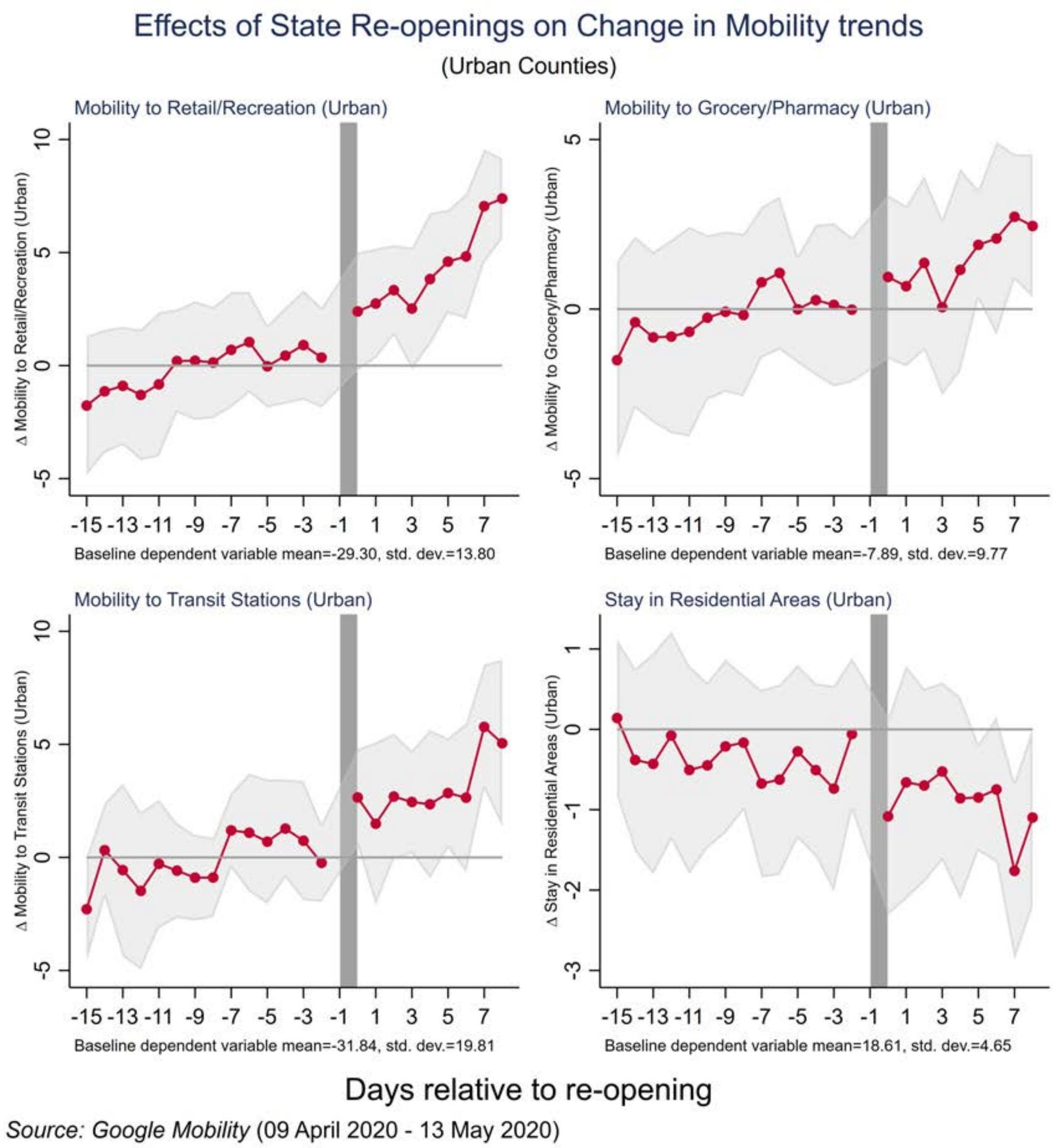
Figure B5: Event study regression coefficients and 95 percent confidence interval.



Note: Author's calculation based on smart device movement data from PlaceIQ. Each panel is based on a separate regression.Measure of out-of-state and average out-of-county travel capture 14-day lagged rates of travel outside of the "home state" and "home county". Estimation sample window is April 8, 2020-May 13, 2020. Longer/shorter Stay-at-home orders are defined as those implemented more/less than the 25 days (median) prior to re-opening. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

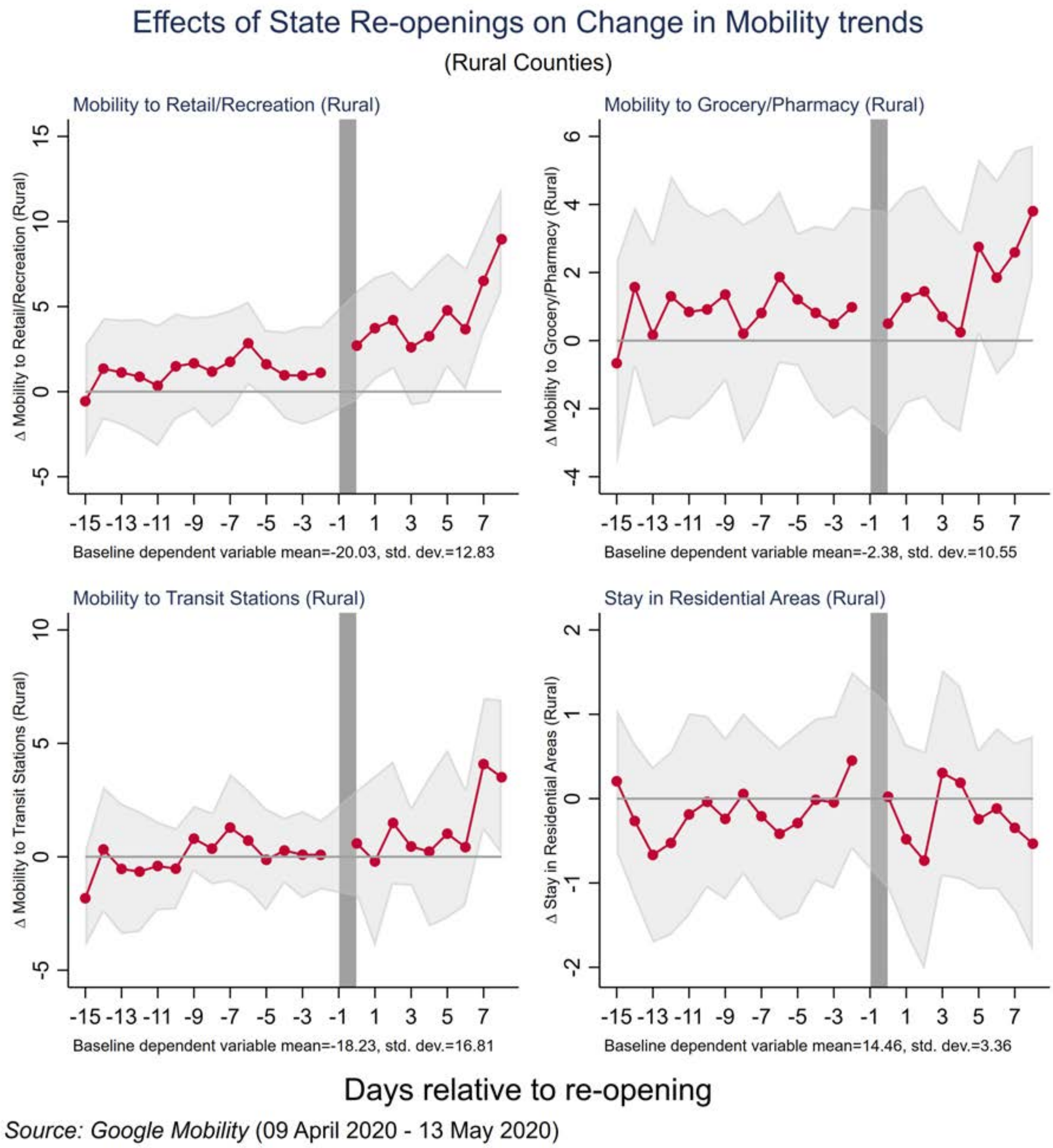
C Urban and Rural Counties

Figure C1: Event study regression coefficients and 95 percent confidence interval.



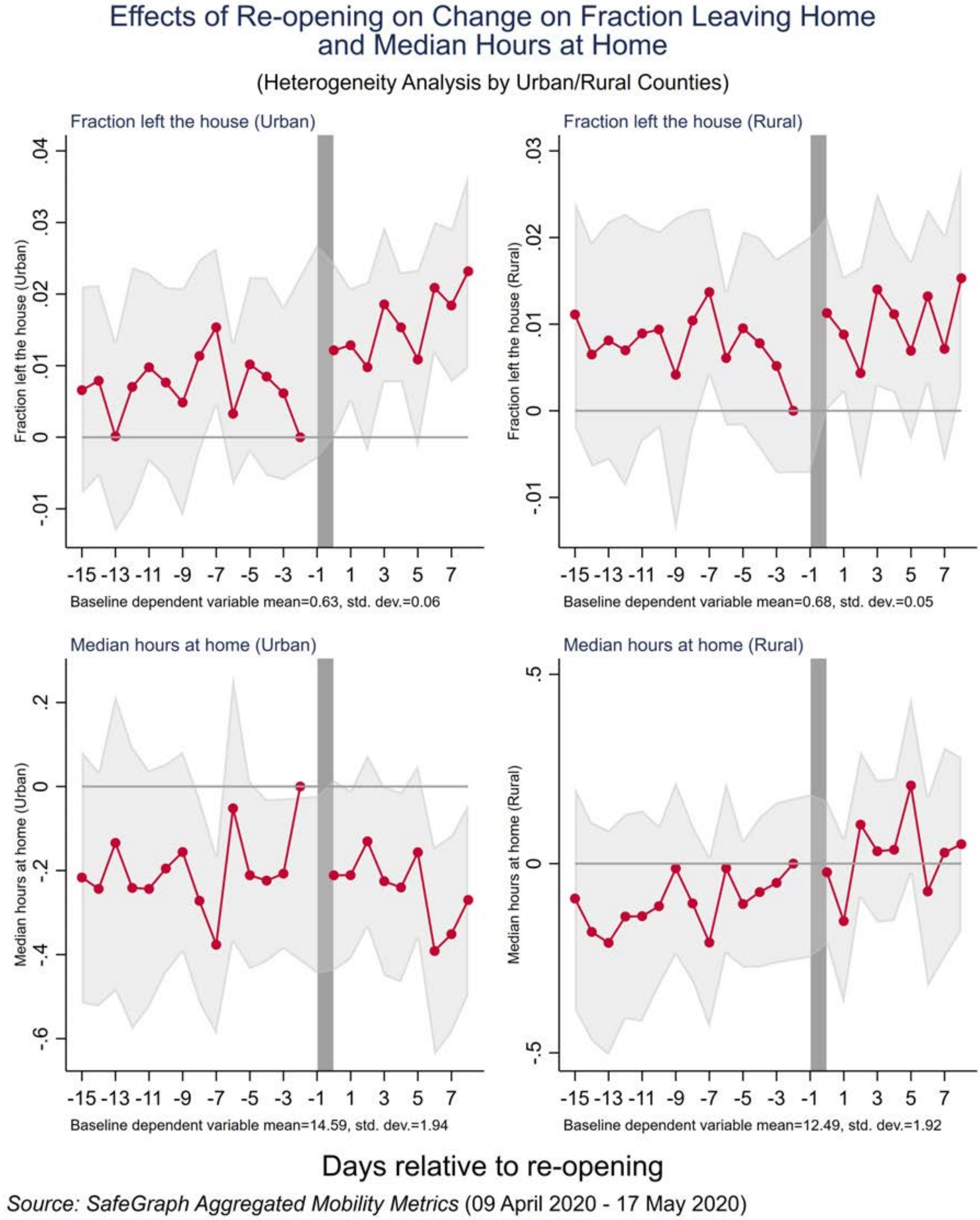
Note: Author's calculation based on smart device movement data from Google Mobility. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 13, 2020. Left panels presents results for urban counties, right panels are for rural counties. Urban/Rural counties defined as metropolitan/non-metropolitan counties. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

Figure C2: Event study regression coefficients and 95 percent confidence interval.



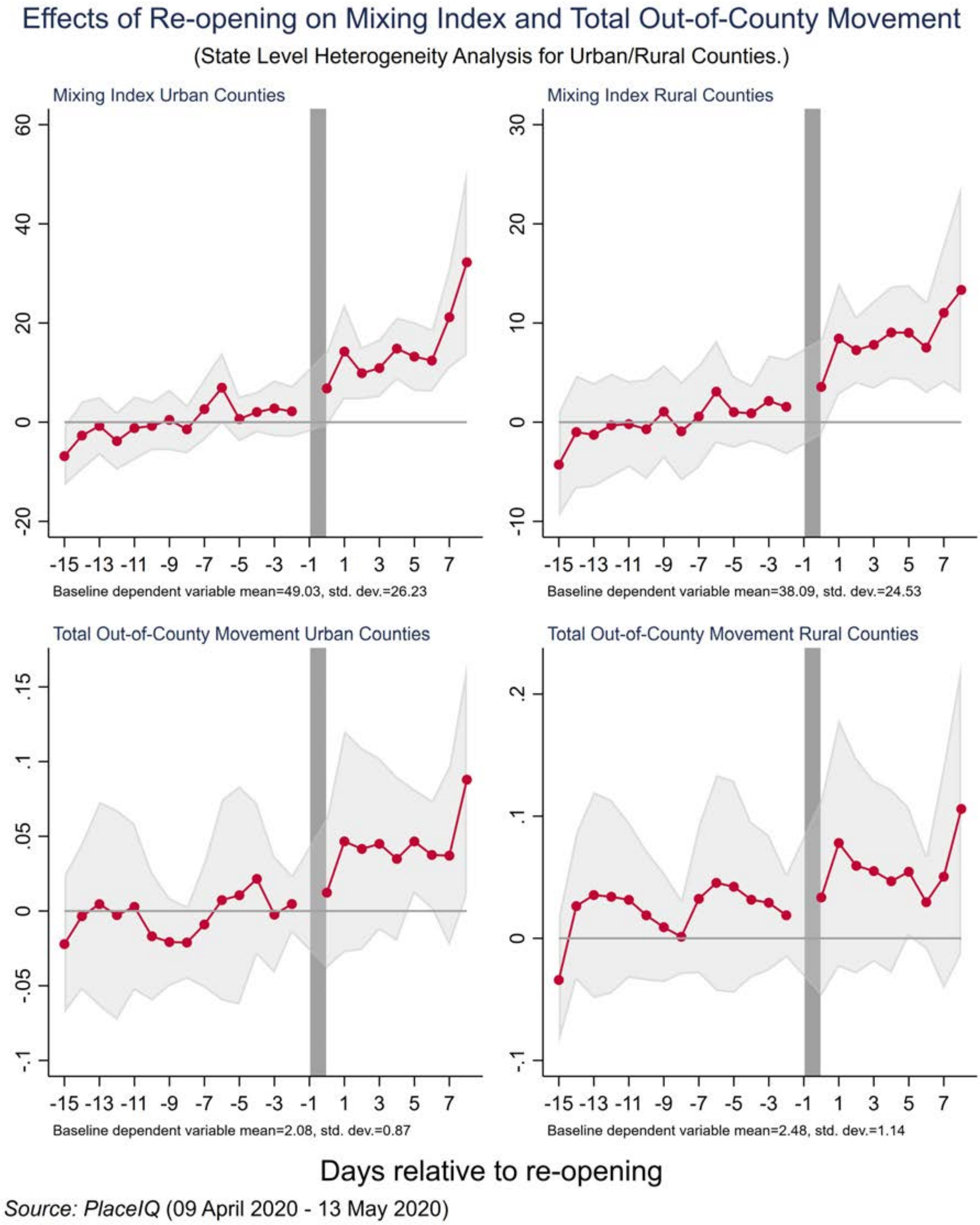
Note: Author's calculation based on smart device movement data from Google Mobility. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 13, 2020. Left panels presents results for urban counties, right panels are for rural counties. Urban/Rural counties defined as metropolitan/non-metropolitan counties. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

Figure C3: Event study regression coefficients and 95 percent confidence interval.



Note: Author's calculation based on smart device movement data from SafeGraph Aggregated Mobility Metrics. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 17, 2020. Left panels presents results for urban counties, right panels are for rural counties. Urban/Rural counties defined as metropolitan/non-metropolitan counties. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

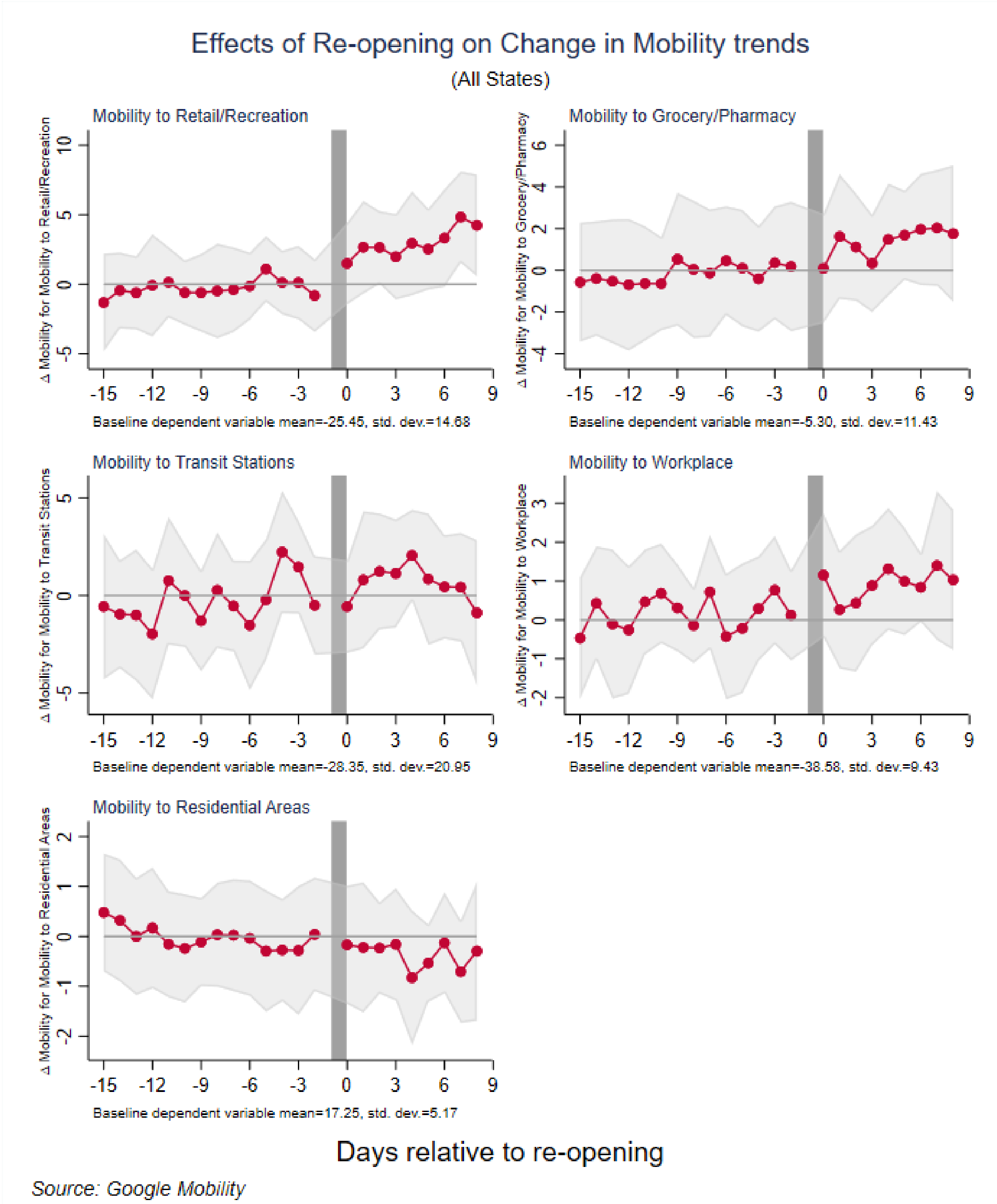
Figure C4: Event study regression coefficients and 95 percent confidence interval.



Note: Author's calculation based on smart device movement data from PlaceIQ. Each panel is based on a separate regression.Measure of out-of-state and average out-of-county travel capture 14-day lagged rates of travel outside of the "home state" and "home county". Estimation sample window is April 8, 2020-May 13, 2020. First panel presents results for urban counties and second column are for rural counties. Urban/Rural counties defined as metropolitan/non-metropolitan counties. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

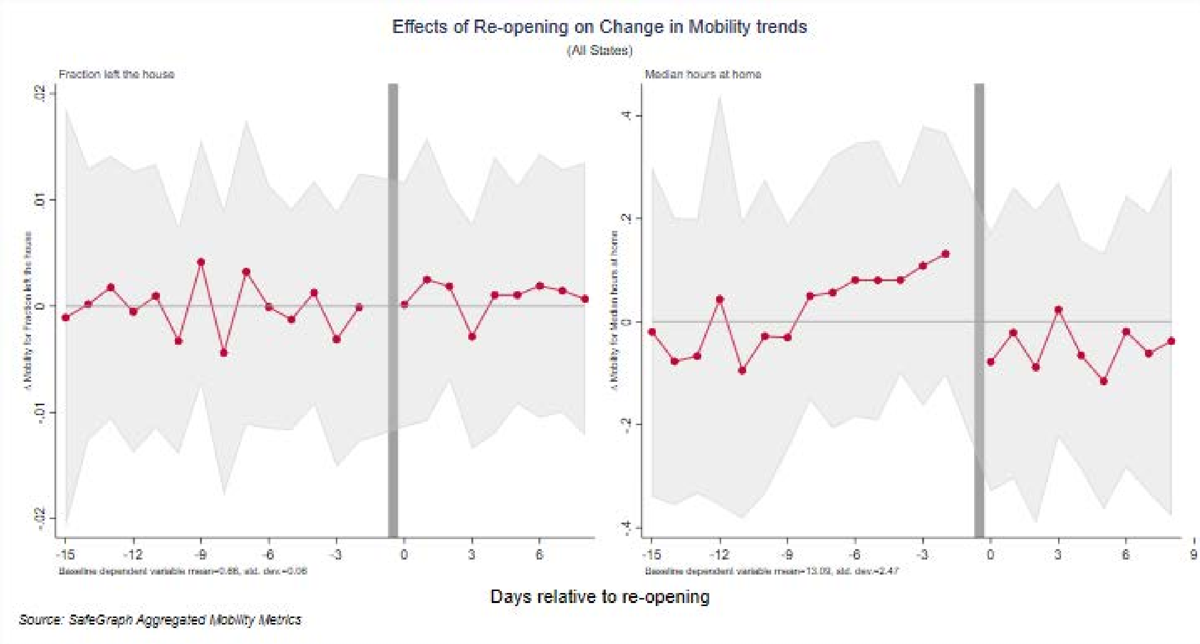
D Border Counties

Figure D1: Event study regression coefficients and 95 percent confidence interval—Border counties, No spillovers.



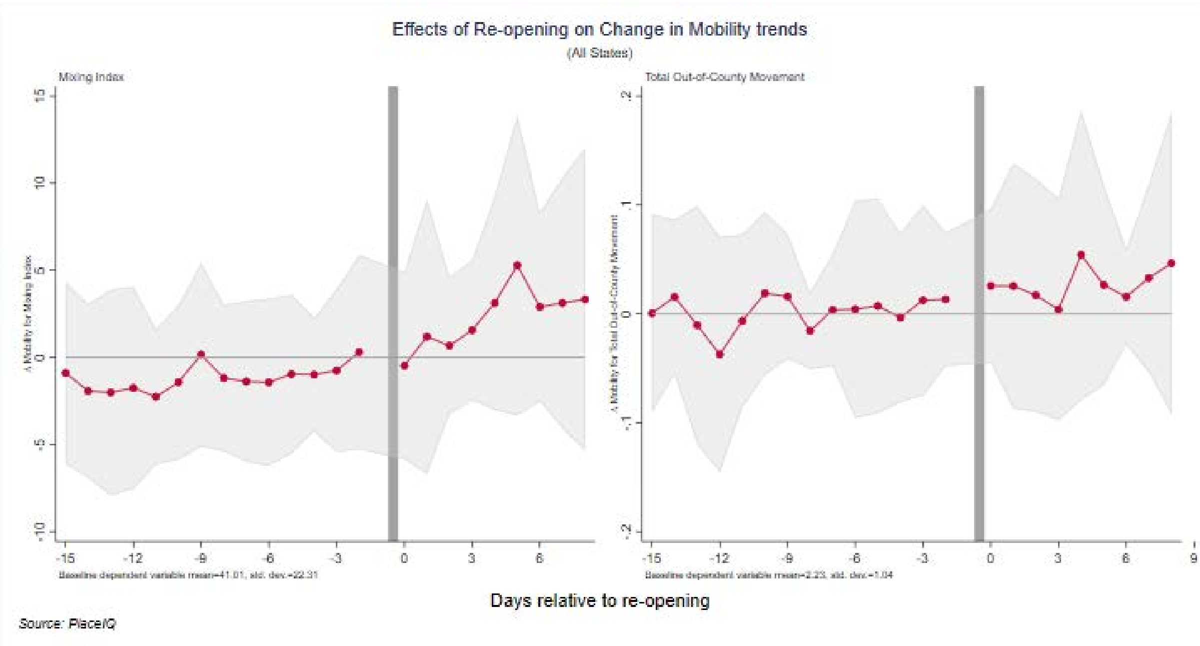
Note: Author's calculation based on smart device movement data from Google Mobility. Each panel is a separate dependent variable. Estimation sample window is April 15, 2020-May 13, 2020. Vertical grey line depicts the day before re-opening. All models include county pair fixed effects, date fixed effects, and county-by-pair fixed effects. Standard errors clustered at state level.

Figure D2: Event study regression coefficients and 95 percent confidence interval—No spillovers.



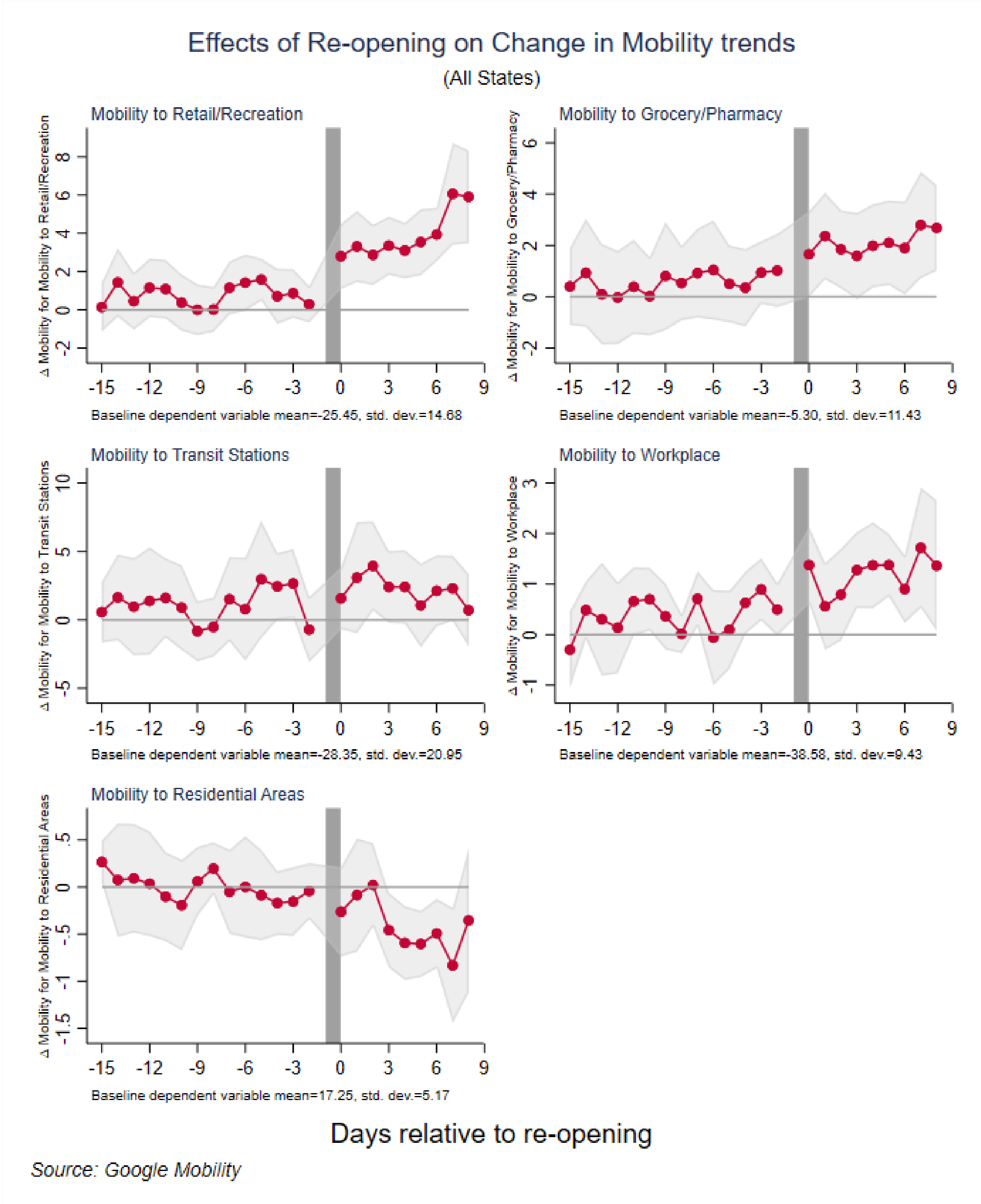
Note: Author's calculation based on smart device movement data from SafeGraph Aggregated Mobility Metrics. Each panel is a separate dependent variable. Estimation sample window is April 15, 2020-May 15, 2020. Vertical grey line depicts the day before re-opening. All models include county pair fixed effects, date fixed effects, and county-by-pair fixed effects. Standard errors clustered at state level.

Figure D3: Event study regression coefficients and 95 percent confidence interval—No spillovers.



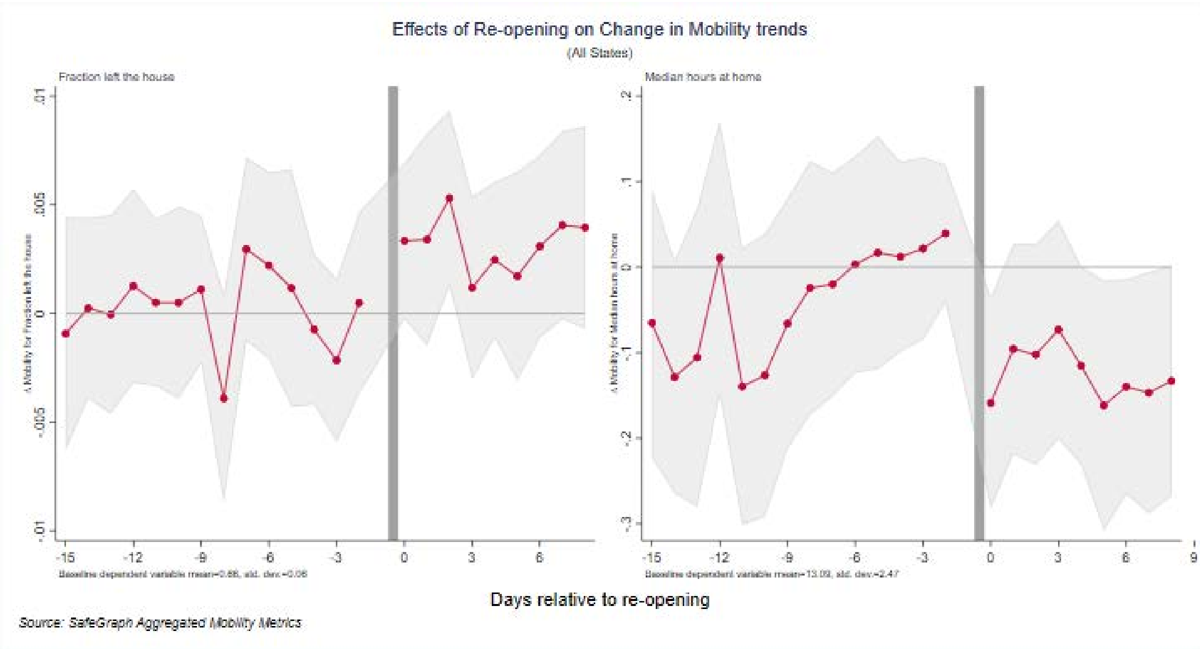
Note: Author's calculation based on smart device movement data from PlaceIQ. Each panel is a separate dependent variable. Estimation sample window is April 15, 2020-May 13, 2020. Vertical grey line depicts the day before re-opening. All models include county pair fixed effects, date fixed effects, and county-by-pair fixed effects. Standard errors clustered at state level.

Figure D4: Event study regression coefficients and 95 percent confidence interval—Spillovers.



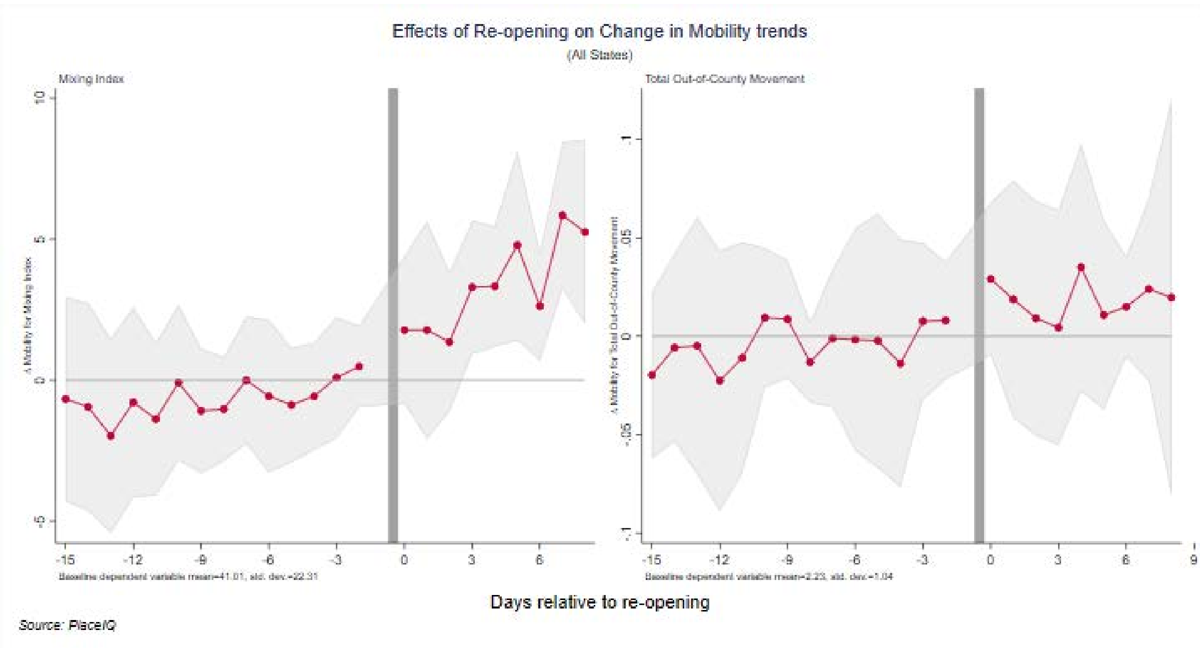
Note: Author's calculation based on smart device movement data from Google Mobility. Each panel is a separate dependent variable. Estimation sample window is April 15, 2020-May 13, 2020. Vertical grey line depicts the day before re-opening. All models include county pair-by-date fixed effects and county-by-pair fixed effects. Standard errors clustered at state level.

Figure D5: Event study regression coefficients and 95 percent confidence interval—Spillovers.



Note: Author's calculation based on smart device movement data from SafeGraph Aggregated Mobility Metrics. Each panel is a separate dependent variable. Estimation sample window is April 15, 2020-May 15, 2020. Vertical grey line depicts the day before re-opening.All models include county pair-by-date fixed effects and county-by-pair fixed effects. Standard errors clustered at state level.

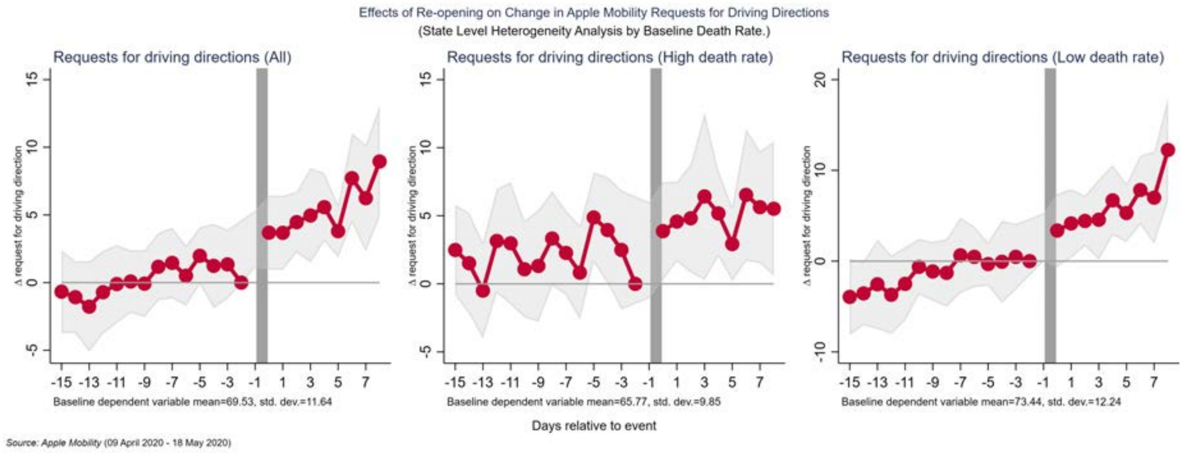
Figure D6: Event study regression coefficients and 95 percent confidence interval—Spillovers.



Note: Author's calculation based on smart device movement data from PlaceIQ. Each panel is a separate dependent variable. Estimation sample window is April 15, 2020-May 13, 2020. Vertical grey line depicts the day before re-opening. All models include county pair-by-date fixed effects and county-by-pair fixed effects. Standard errors clustered at state level.

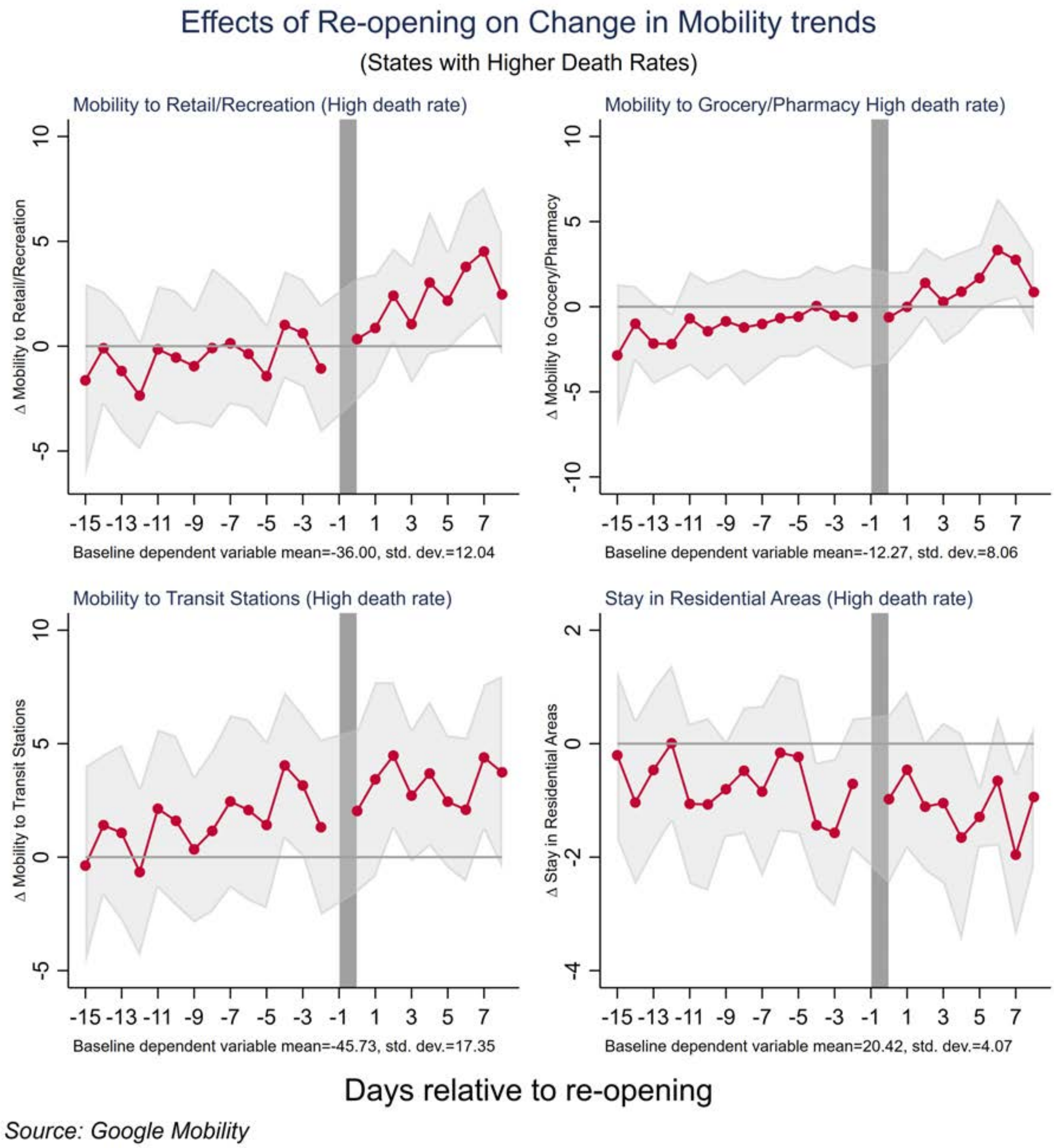
E Baseline COVID-19 related mortality rate

Figure E1: Event study regression coefficients and 95 percent confidence interval.



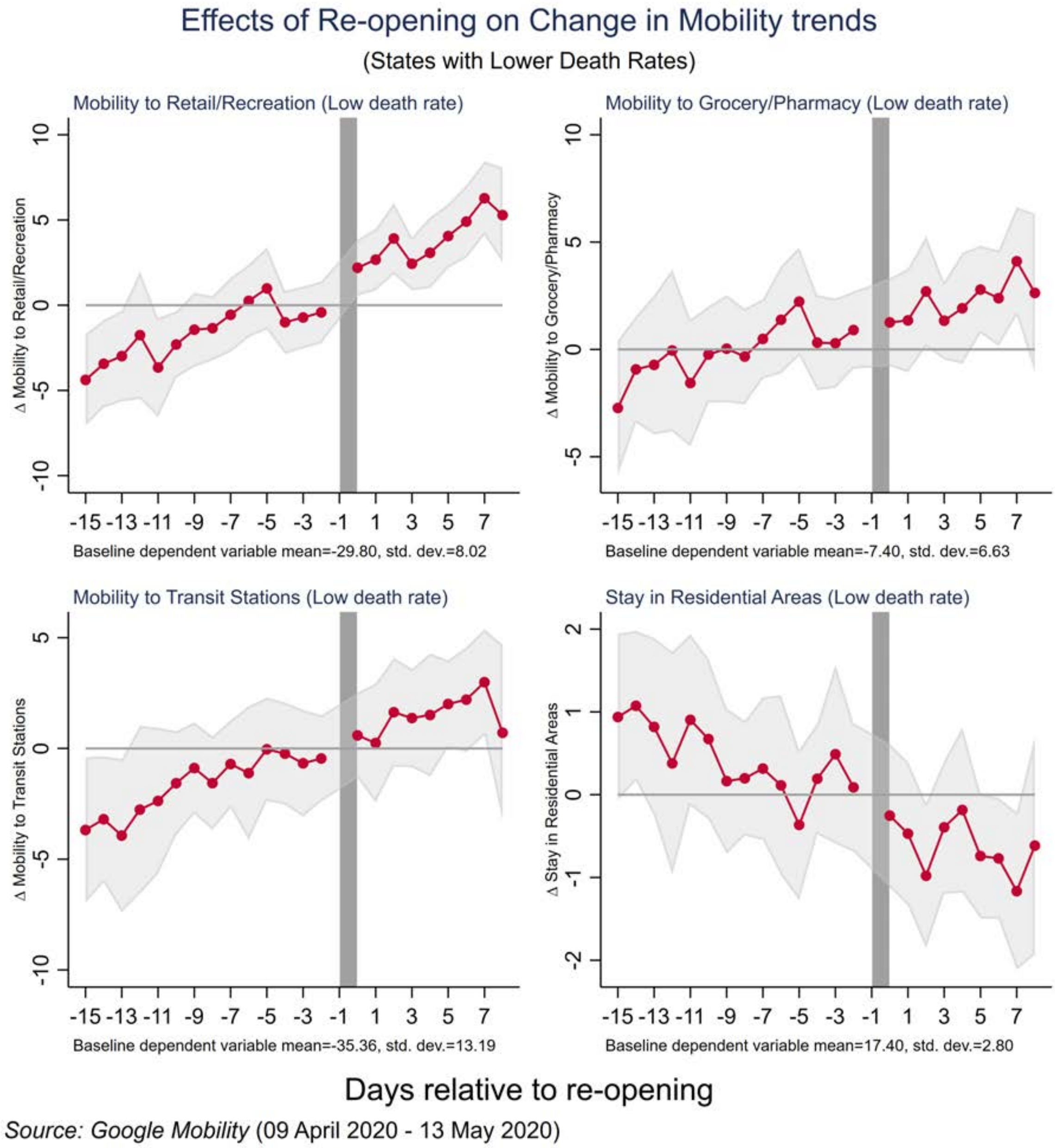
Note: Author's calculation based on smart device movement data from Apple Mobility. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 18, 2020. Higher/lower baseline COVID-19 related death rates are defined as those above/below the median prior to re-opening. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level.Baseline dependent variable mean as of April 15, 2020.

Figure E2: Event study regression coefficients and 95 percent confidence interval.



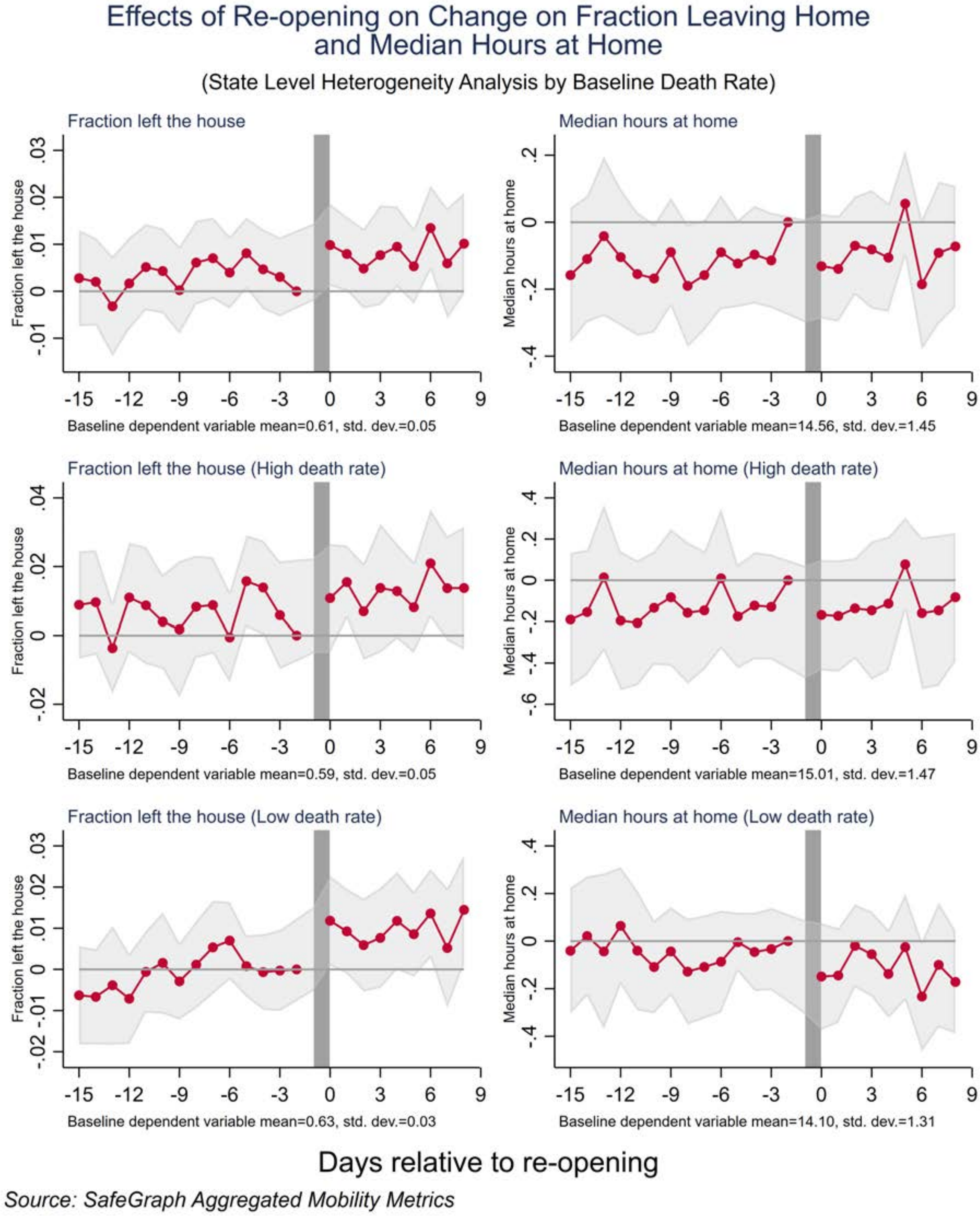
Note: Author's calculation based on smart device movement data from Google Mobility. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 13, 2020. Higher baseline COVID-19 related death rates are defined as those above the median prior to re-opening. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

Figure E3: Event study regression coefficients and 95 percent confidence interval.



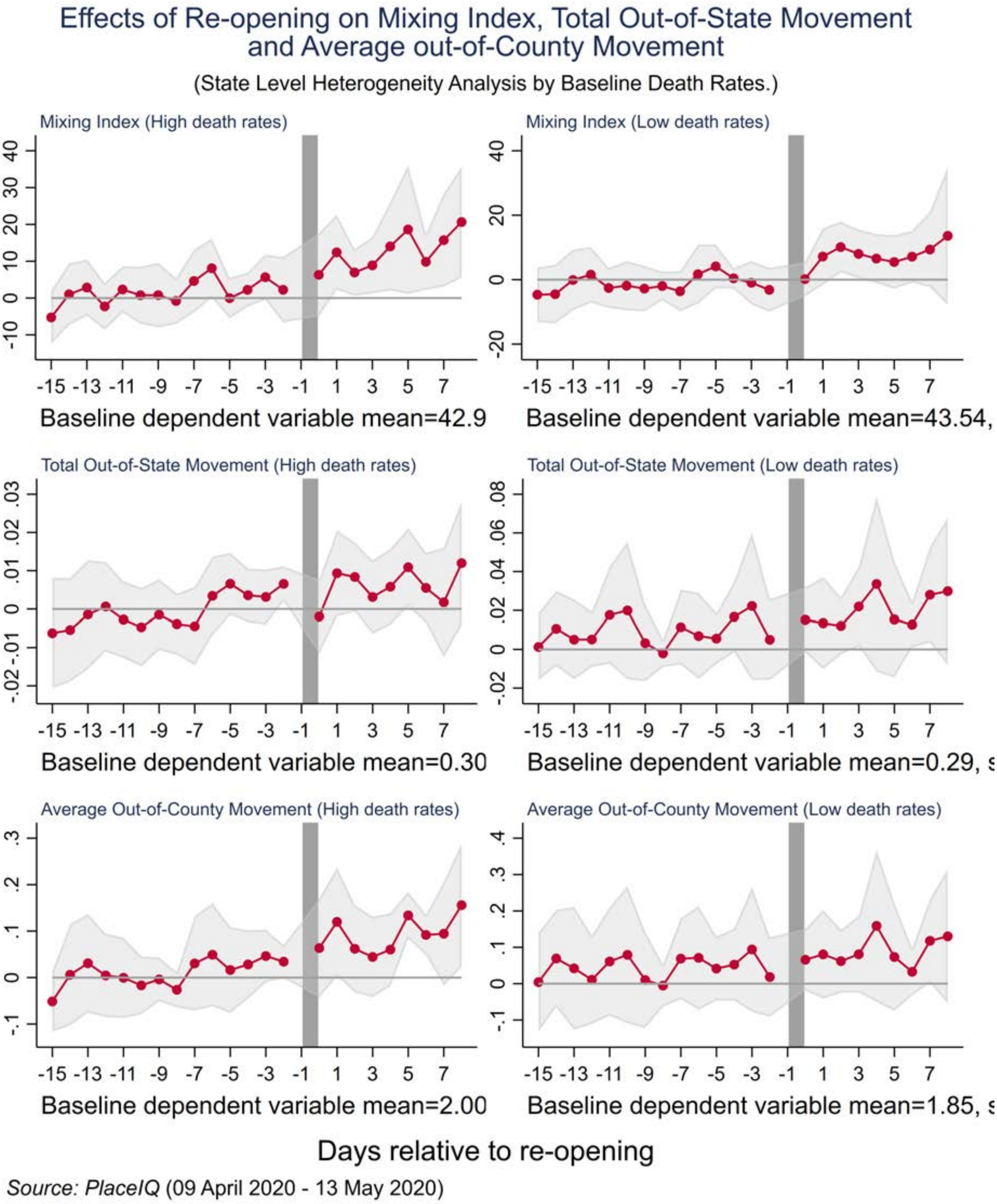
Note: Author's calculation based on smart device movement data from Google Mobility. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 13, 2020. Lower baseline COVID-19 related death rates are defined as those below the median prior to re-opening. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

Figure E4: Event study regression coefficients and 95 percent confidence interval.



Note: Author's calculation based on smart device movement data from SafeGraph Aggregated Mobility Metrics. Each panel is based on a separate regression. Estimation sample window is April 8, 2020-May 17, 2020. Higher/lower baseline COVID-19 related death rates are defined as those above/below the median prior to re-opening. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.

Figure E5: Event study regression coefficients and 95 percent confidence interval.



Note: Author's calculation based on smart device movement data from PlaceIQ. Each panel is based on a separate regression.Measure of out-of-state and average out-of-county travel capture 14-day lagged rates of travel outside of the "home state" and "home county". Estimation sample window is April 8, 2020-May 13, 2020. Higher/lower baseline COVID-19 related death rates are defined as those above/below the median prior to re-opening. Vertical grey line depicts the day before re-opening. All models include state fixed effects and date fixed effects. Standard errors clustered at state level. Baseline dependent variable mean as of April 15, 2020.