Does paid sick leave encourage staying at home? Evidence from the United States during a pandemic

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Keywords: infectious disease spread | paid sick leave | pandemic disease | physical mobility

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RESEARCH ARTICLE

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Does paid sick leave encourage staying at home? Evidence from the United States during a pandemic

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Abstract

We study the impact of a temporary U.S. paid sick leave mandate that became effective April 1st, 2020 on self-quarantining, proxied by physical mobility behaviors gleaned from cellular devices. We study this policy using generalized difference-in-differences methods, leveraging pre-policy county-level heterogeneity in the share of workers likely eligible for paid sick leave benefits. We find that the policy leads to increased self-quarantining as proxied by staying home. We also find that COVID-19 confirmed cases decline post-policy.

KEYWORDS
infectious disease spread, paid sick leave, pandemic disease, physical mobility

JEL CLASSIFICATION
I12, I18, J32

1 | INTRODUCTION

In contemporary economically interconnected societies, infectious diseases can emerge and spread rapidly and lead to global sickness, death, economic decline, and numerous other social costs (see, e.g., Adda (2016) and Agüero and Beleche (2017)). How best to curtail disease spread is an important question as the macroeconomic role of pandemics and other crises is increasingly recognized, and as pandemics become more common (World Health Organization, 2018). Scientists develop vaccinations and medical treatments in response to new diseases, but these innovations take time and distribution plans must be established to widely deploy pharmacological and other medical responses to the population, a non-trivial task even in advanced economies. At least initially, infected and exposed individuals can self-quarantine (e.g., stay home) to prevent further disease spread.

Authors listed in alphabetical order. All authors contributed equally to the manuscript.

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Infectious diseases can also increase family responsibilities, such as caring for dependents, and exacerbate existing labor market inequities between workers of different races, ethnicities, sexes, and socioeconomic statues (e.g., Alon et al. (2020)).

Public health measures are important to contain disease spread in the early stages of an infectious disease outbreak and remain vital even after pharmacological and other medical responses are developed. However, these measures are effective only when viewed through a lens of economic motivations that serve to promote or deter people's willingness or ability to follow public health recommendations. In particular, if individuals face financial barriers to taking time off when sick/exposed to disease or attending to family responsibilities, public health measures may not be adequately leveraged, and disease spread may be exacerbated. A possible policy response to promote effective self-quarantining and meeting family responsibilities during pandemic (and non-pandemic) times is providing workers with paid sick leave (PSL). Most developed countries mandate PSL to workers, and evidence suggests that individuals are more likely to self-quarantine when offered financial protection to do so (Bodas & Peleg, 2020).

To study the importance of PSL policy for promoting self-quarantining, we use data from the U.S., which is one of three Organization for Economic Development (OECD) countries that does not have a national PSL policy, during the COVID-19 pandemic. The U.S. represents a unique case as it is an advanced economy that may face challenges to effectively deploying a public health response to infectious diseases as working while sick is common, potentially due to limited access to PSL among workers. Without a federal mandate, provision of PSL benefits has largely been left to employers. Nearly 40% of employed U.S. adults report that they do not have any PSL through their employer (Asfaw et al., 2019).

As specific examples to demonstrate that limited PSL in the U.S. contributes to the spread of infectious disease, even in non-pandemic times, survey data suggest that 90% of workers report working while sick (Accountemps, 2019) and each week approximately three million Americans go to work while sick (Susser & Ziebarth, 2016), including with infectious conditions (Smith, 2008), possibly due to fear of income or job loss (NPR, Kaiser Family Foundation, and Harvard School of Public Health, 2008). Foregoing wages when sick is likely non-trivial for many Americans: in 2019 the cost of losing a day of wages was $155 for the median worker (Gould, 2020). Similarly, one in five Americans are caregivers and 61% of caregivers work outside the home (The National Alliance for Caregiving and AARP, 2020), suggesting that PSL could allow many people to better balance work and family responsibilities, especially with widespread school and daycare closures and general sickness that can occur during pandemic periods.

In response to the COVID-19 pandemic, the U.S. federal government adopted a *temporary* national PSL policy designed to allow infected workers to stay home for two to 12 weeks when sick and/or when caring for dependents who became sick or children whose schools and daycares were shuttered. The initial 2 weeks were at full pay, and the additional 10 weeks of paid leave carried a two-thirds replacement rate, but both provisions had important exemptions in terms of industry which we exploit for constructing a causal identification strategy. The policy, the Families First Coronavirus Response Act (FFCRA), was adopted on April 1st, 2020, and expired on December 31st, 2020.[1] The benefit could only be claimed once, although days could be taken intermittently (Department of Labor, 2020b).

We provide the first evidence on the impact of any U.S. PSL mandates on "physical mobility," a proxy for self-quarantining, and a behavioral outcome targeted by the policies, measured using proxies from GPS tracking of cellular devices. We consider cellular device movements that plausibly capture time individuals spend at home, reflecting their ability to stay away from work when sick, self-quarantine, and to meet family responsibilities.

To study this question, we exploit a unique feature of the federal PSL policy is that "essential workers" were not eligible for FFCRA benefits.[2] Thus, counties with higher shares of *nonessential* workers pre-FFCRA should be *more exposed* to the policy.[3] While there are counties with very low (22.7%) and very high (89.9%) shares of nonessential establishments, in half of all counties, nonessential establishments account for 61.0%–69.9% of the total. This variation allows us to estimate a generalized difference-in-differences (DD) regression that leverages differential treatment "doses"-based variation in the share of the county workforce that is employed in an "essential" job.

We find that FFCRA reduced time away from the home. Post-FFCRA, comparing the county with the lowest (22.7%) share of nonessential workers in our data to the county with the highest share (89.9%), the average number of hours away from home decreases (compared to pre-FFCRA values) by 8.9% and the share of individuals away from home for more than 8 hours per day declines by 6.9%. In addition, counties more exposed to the policy experience larger declines in confirmed COVID-19 cases. We make use of structural break tests to establish that April 1st, 2020 represents a meaningful discontinuity in trends for our physical mobility measures. Moreover, there is no other policy or institutional change we know of that happened on that date that would affect counties differentially based on their pre-policy share of nonessential workers, although we acknowledge that the pandemic led to many behavioral changes that we likely cannot measure. Nevertheless, our results are robust to controlling for local area unemployment—suggesting that we are not simply picking up a spurious association between economic activity changes that occurred during our study period (Congressional Research Service, 2021), and are confirmed through event study

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comparisons—the differences between highly exposed counties and counties with limited exposure are nonexistent prior to policy and increase after the policy start date, and through placebo testing against 2019 data and numerous other robustness checks. This analysis allows us to conclude that the PSL policy is effective in encouraging people to stay home.

The paper proceeds as follows. Section 2 describes the COVID-19 pandemic. The details of FFCRA are discussed in Section 3. Data and methods are described in Section 4. Results are reported in Section 5. Supplementary Appendix S1 includes a review of policies adopted during the COVID-19 pandemic and additional FFCRA details, an extensive set of robustness checks (including using longer and shorter study period windows, inclusion of various alternative fixed-effects, and complementary approaches to statistical inference) that we conduct to assess the stability of our findings. Section 6 offers a discussion and conclusion.

2 | RELATED LITERATURE

2.1 | PSL mandate effects

There is a growing literature on PSL mandates. Much of the literature has focused on non-U.S. settings as the U.S. has only recently adopted PSL mandates, and only at the city, county, and state levels. We focus on U.S. PSL mandates as they are most relevant to our work.

Studies find that U.S. city and state PSL mandates in general increase PSL coverage among workers (Callison & Pesko, 2022; Maclean et al., 2020). However, most studies to date suggest that following a city or state PSL mandate, workers take additional paid leave (Ahn & Yelowitz, 2016; Callison & Pesko, 2022; Colla et al., 2014; Maclean et al., 2020; Schneider, 2020). We note that Stearns and White (2018), using variation from Connecticut and Washington DC, show that overall leave-taking *declines* post-mandate.

FFCRA benefits are fully financed by the federal government, thus we might expect that employers would be more likely (relative to the employer-financed mandates standard in the U.S. at the time of writing) to encourage employees take time off work when sick as employer costs are reduced. Further, this policy was federal and was implemented during a time period in which many workers were infected with COVID-19 (or concerned about such infections for themselves and their dependents) and the public's attention was focused on this policy to a greater degree that might be the case for the non-federal policies currently in place in the U.S. Collectively, these features of our study might suggest that a federal PSL policy could have differential impacts on worker behavior than a more localized (i.e., state, city, or county) policy during a non-pandemic time.

Three studies that exploit variation generated by U.S. city and state PSL mandates shows that these policies reduce infectious disease spread in non-pandemic times. Pichler and Ziebarth (2017), and Maclean et al. (2020) use high-frequency Google influenza data in the U.S. to show that population-level influenza-like disease rates decrease after workers gain access to PSL following a city or state mandate adoption, suggesting PSL mandates have positive spillover effects by preventing disease spread. Pichler et al. (2021) confirm this finding using administrative data on physician-certified influenza.

Our study is arguably most closely linked with Pichler et al. (2020) who show that FFCRA is associated with a reduction in COVID-19 cases. Our study complements this previous and important work by examining a key mechanism for the documented decline in cases. Put differently, the aggregate impact of PSL on mobility (which is not examined by Pichler et al. as that study focuses exclusively on COVID-19 cases) is likely the most relevant mechanism by which PSL can reduce COVID-19 incidence. In essence, if an infected or exposed worker stays away from home due to financial protection afforded by PSL but does not decrease mobility in total, then the effectiveness of PSL mandates as policy tools for reducing community spread of an infectious disease is likely reduced. Our study is able to speak directly to these issues and thus provides a mechanism for the findings of Pichler et al. (2020), who suggest such behaviors as a key pathway from the policy to their documented declines in COIVD-19 cases. Collectively, the two studies shed light on the role of PSL in curbing disease spread.

2.2 | COVID-19

The COVID-19 pandemic is the largest pandemic in modern history and thus offers a unique opportunity to study the importance of PSL policy for promoting basic public health measures such as staying home while sick/exposed. As of October 28th, 2022 there were nearly 626 million confirmed global COVID-19 cases and nearly 6.6 million deaths (World Health Organization, 2022). On that date, the U.S. accounted for 15% of confirmed cases and 16% of deaths globally. COVID-19 is a viral disease caused by infection with the virus SARS-CoV-2. Infected individuals are contagious for a period of up to 14 days and

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before displaying symptoms, thus increasing the importance of strategies to enable sick workers to remain at home and for workers to take time away from work to care for exposed or sick dependents.

The U.S. response to curtailing COVID-19 through public health measures has been criticized (Altman, 2020; Yong, 2020). Lack of PSL potentially played a role as has been established in previous pandemics. Using a simulation model, Kumar et al. (2012) estimate that 5 million Americans contracted influenza during the 2009 H1NI outbreak directly due to a lack of PSL. Indeed, major international organizations recommend that countries adopt mandated PSL for workers specifically as a means to curtail COVID-19 spread (OECD, 2020).

3 | FFCRA

FFCRA compels certain private employers with 50–500 workers and some public employers to offer temporary paid leave to workers (Federal Register 2020). FFCRA also applies to the gig economy. Qualifying reasons for PSL include: (i) the worker is subject to a federal, state, or local quarantine or isolation order; (ii) a healthcare professional has recommended that the worker self-quarantine; (iii) the worker is experiencing COVID-19 symptoms or similar symptoms and is currently seeking a diagnosis from a healthcare professional; (iv) the worker is caring for an individual(s) subject to (i) or (ii); and (v) the worker is caring for a child whose school or daycare is closed, or whose childcare provider is not available for reasons related to COVID-19. Similar to most mandates (for PSL and for other benefits such as health insurance) in the U.S., there are exemptions for employers that meet specific criteria (Federal Register, 2020). We account for employer size exemptions in our analysis, but many exemptions are quite nuanced, and we do not address all of them in our measure of exposure to FFCRA, which likely leads to some measurement error.

FFCRA provides eligible workers with a qualifying own or family illness and, as we discuss later in the manuscript, additional pandemic-related reasons with 2 weeks (up to a maximum of 80 h) of PSL at the worker's regular rate of pay or the applicable minimum wage (whichever is higher), up to a maximum of $511 per day, once, during 2020. Employers are not permitted to reduce other benefits following FFCRA adoption and FFCRA benefits can be used in conjunction with previously provided paid leave. Workers who are caring for children whose schools or daycares have closed due to COVID-19 or who are tending to dependents with COVID-19 are also eligible for 2 weeks (up to a maximum of 80 h) of PSL at the full cost of the worker's regular rate of pay, or the applicable minimum wage, up to $200 per day. Employers initially pay the benefits, but later receive federal reimbursable tax credits (Internal Revenue Service, 2020). There is no accrual rate or waiting period for benefits; such features are common among U.S. city and state mandates to date (e.g., most city and state mandates impose a waiting period of 60 or more days).

Additional benefits are available to some workers (who have worked for the employer for more than 30 days) under The Emergency Family and Medical Leave Expansion Act, which predates COVID-19 and extends Title I of the Family and Medical Leave Act that provides 12 weeks *unpaid* leave to qualifying workers. FFCRA gives employers reimbursements (up to two-thirds of the regular wages) for 10 weeks of such leave, making FMLA into a *paid* sick leave policy (Department of Labor, 2020a). Employers are mandated to post notices in the workplace so that workers know about the benefit.

Of particular relevance to our study, "essential workers" who work in one of 16 "critical infrastructure sectors" (U.S. Cybersecurity and Infrastructure Security Agency, 2020) and are employed by employers with 50–500 workers are exempt from receiving FFCRA benefits (Federal Register, 2020). The sectors include chemical; commercial facilities; communications; critical manufacturing; dams; defense industrial base; emergency services; energy; financial services; food and agriculture; government facilities; healthcare and public health; information technology; nuclear reactors, materials, and waste; transportation systems; and water and wastewater systems. The Department of Labor (DOL) has not explicitly defined an "essential worker,"[4] although they must work in one of the 16 above-noted sectors. The DOL states that essential workers are individuals who "(i) interact with and aid individuals with physical or mental health issues, including those who are or may be suffering from COVID-19; (ii) ensure the welfare and safety of our communities and of our Nation; (iii) have specialized training relevant to emergency response; and (iv) provide essential services relevant to the American people's health and wellbeing" (Federal Register, 2020). The DOL delegates the exact definition of essential workers to states: "…the definition allows for the highest official of a state or territory to identify other categories of emergency responders, as necessary." As we describe in Section 4.3, we use a definition of essential workers developed by Blau et al. (2020), and leverage differences across counties in the share of the workforce that is likely classified as a *nonessential* worker.

Early estimates suggested that FFCRA would cover 17%–47% of U.S. workers (Glynn, 2020). However, to the best of our knowledge, there is limited information on FFCRA benefit take-up to date, instead the small FFCRA literature has focused on quantifying the effects of the policy on COVID-19 outcomes or describing the workers likely to gain access to the benefit

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(see Pichler et al. (2020) and Blau et al. (2020)). One novel study conducted a nationally representative telephone survey in late 2020 to assess self-reported FFCRA take-up among U.S. employees over our analysis period (Jelliffe et al., 2021). The authors find that approximately eight million employed Americans used FFCRA benefits in the first six to 8 months of the policy. Further, the Congressional Budget Office (2021) estimates that between April 1st and December 31st, 2020, employers claimed $5.4 billion in tax credits and the Government Accountability Office (2021) reports that in the second and third quarters of 2020 (April through September), 382,727 employers claimed FFCRA tax credits. Using preliminary IRS tax returns through the second quarter of 2021, Goodman (2021) finds that take-up of FFCRA for paid leave tax credits was nearly 50% among employers most likely eligible while take-up for family leave was lower at roughly 15%. Goodman notes that, because the tax data analyzed due not include amended tax returns and due to delayed tax filings during the pandemic, take-up rates may be "non-trivially" underestimated. These numbers are lower than initial estimates reported by these government agencies but suggest that both employees and employers made use of the FFCRA benefits.

As discussed above, a county's exposure to FFCRA depends on the share of workers who are eligible for FFCRA benefits. We approximate this value using the share of workers in nonessential industries. However, since employment data is frequently suppressed in the Quarterly Census of Employment and Wages (QCEW) at the county-by-industry level that we need, we employ an iterative proportional fitting (IPV) procedure (Bacharach, 1965) to estimate employment at the county-by-industry level (see Supplementary Appendix S1 for full details). We report results based on specifications that use (i) nonessential worker establishments (directly observed in the QCEW) and (ii) nonessential workers that we derive from our IPV estimates. We view both measures as proxies for the number of nonessential workers in a county. Our findings are not sensitive to the proxy we employ. In complementary analyses, we make use of our IPV estimates that incorporate employer size to more accurately identify the share of workers employed by nonessential employers with 50–500 workers. We chose not to use this measure as our primary proxy as we must impute both industry and employer size. However, we have re-estimated all analyses reported in this paper using the "doubly imputed" IPV measure and results (available on request) are very similar.

4 | DATA AND METHODS

4.1 | SafeGraph Inc

We use aggregated, high frequency (daily) geolocation data from SafeGraph Inc. (a company that aggregates anonymized location data from numerous cellular applications) covering the period March 13th, 2020, through April 30th, 2020. We select March 13th, 2020 as the start date of our study window as this is the date on which the President declared a national emergency (Federal Emergency Management Agency, 2020) and potentially reflects a meaningful change in the understanding of the COVID-19 pandemic among Americans.

Our methods rely on common trends in our outcomes. COVID-19 was a shock to the U.S. and counties likely had differential responses in terms of mobility due to labor markets, social norms, understanding of/belief in public health experts, and so forth (Allcott et al., 2020). Therefore, in our main analysis, we focus on trends in the "early COVID-19 environment" as that period is most salient to our study and, in particular, to the validity of our research design. We close the study period on April 30th, 2020 as several states began the process of re-opening their economy in early May 2020 (The Council of State Governments, 2020). However, results are highly robust to using both longer and shorter study periods (see Supplementary Appendix S1).

SafeGraph data cover over 20 million cellular devices. These data allow us to accurately locate individual cellular devices and track the share of devices that leave the home area in near real-time and are, therefore, ideal for our study. SafeGraph identifies locations for a device using a GeoHash-7 encoding algorithm that covers the globe with a grid that is approximately 500 feet per side. Devices are included in the sample if SafeGraph can identify a home location for the device, which requires a device to be on and consistently present at a location during nighttime hours for a 6-week period. Home locations are updated at the start of each month. Because SafeGraph data are based on users of cellular applications who have opted into location sharing, the number of devices in the sample changes over time.[5] Given our short study period, the above-noted 6-week requirement in our main analysis, and the types of applications that provide location data, we do not expect that the sample of cellular devices to be a function of FFCRA implementation.

SafeGraph excludes data from Census block groups in any day with information from fewer than five active devices on a given day.[6] We aggregate the number of active devices in each county,[7] the average time devices remained away from home,[8] and the fraction of devices that were away for eight or more hours, from census block groups to the relevant county. Because of the manner in which SafeGraph identifies devices as away from home, the hours at home and away from home for any given device will not add up to 24 h. The specific hours for which devices are observed and the duration for which each device is observed are not reported, therefore we are unable to adjust our estimates for the relative duration of each day that we observe a device

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nor for the distribution of hours (particularly work vs. non-work hours) that a device is observed. This feature of the SafeGraph data implies that our proxy for share of devices away from home (reported later) is lower than would be expected if devices were turned on and tracked 24 h per day. To the best of our knowledge, there is no data set that offers such coverage for the nation.

To isolate FFCRA effects, in our main analysis we use counties that were not covered by a PSL mandate prior to FFCRA (A Better Balance, 2021). The study sample includes 2757 counties and county equivalents out of a total of 3143 in the country; we do not differentiate between counties and county equivalents. We observe each county in each of the 48 days in our study period, thus the sample is balanced. We exclude weekends as most work and school/daycare activities occur within the standard work week (although as we show in robustness checking reported in Supplementary Appendix S1, results are not sensitive to including such days).

4.2 | Outcomes

We consider two physical mobility outcomes based on the movement of cellular devices (not individuals) that proxy for the ability to remain at home while sick/exposed and/or caring for dependents. Ideally, we would measure actual time at home, but high-frequency data are not available for the period required for our empirical strategy.[9]

We measure the average number of hours per day that the cellular device spends away from the SafeGraph-determined home in each county. We also examine the share of devices that are way from this home more than 8 hours per day. We select 8 hours as this duration plausibly captures a work or daycare/school day; both of these behaviors could be impacted by FFCRA as the policy provides benefits for parents/guardians who are caring for children not at daycare/school or who are sick.[10] While we use terms such as "individual" when discussing our results, we are in fact tracking cellular devices which are, presumably, carried by individuals.

4.3 | Methods

To estimate the impact of FFCRA on our outcomes, we estimate a generalized DD regression (Alpert et al., 2018; Argys et al., 2020; Beheshti, 2019; Courtemanche et al., 2017; Finkelstein, 2007; Park & Powell, 2021; Powell et al., 2019; Powell & Pacula, 2021). This regression leverages variation in treatment intensity that is attributable to differences in pre-treatment characteristics across counties. The intuition is that we should observe larger effects of FFCRA on our outcomes in counties which had higher shares of nonessential workers pre-FFCRA as these are the workers who are potentially eligible for policy benefits. Put differently, there is likely to be more policy "bite" in such counties as a greater share of the workforce is eligible for FFCRA.

We interact an indicator for the post-FFCRA period (April 1st, 2020, through April 30th, 2020) with the share of workers in a county employed in a "nonessential worker" job in the first quarter of 2019 using data from the Bureau of Labor Statistics' QCEW. The QCEW captures the near universe of establishments in the U.S. and is measured at the establishment level. An establishment is: "a single physical location where business is conducted or where services or industrial operations are performed." A limitation of this variable is that we use establishments rather than workers themselves to proxy nonessential workers.[11] Thus, as described earlier in Section 3 and Supplementary Appendix S1, we construct a statistical estimate of employment by industry within counties. We also construct estimates of the number of establishments and employees within county-industry-employer size cells. Both measures—which we view as proxies for the share of the county impacted by FFCRA—have benefits and costs, therefore, we report results using both.

Essential workers are not eligible for FFCRA benefits and counties with greater shares of nonessential workers should be more exposed to the policy and, correspondingly, if FFCRA impacts our proxies, should experience larger changes in outcomes post-policy. The DOL has not established a definition of essential workers, but instead provided a high-level description and leaves the exact definition to states (see Section 3). There is heterogeneity across states (but not across counties within a state) in the effective definition. We follow the definition outlined by Blau et al. (2020), although our results are robust to using an alternative definition proposed by Tomer and Kane (2020), see Supplementary Appendix S1.

The generalized DD regression is outlined in Equation (1):

$$Y_{c,s,t} = \pi_0 + \pi_1 \text{FFCRA}_t * \text{nonessential}_c + P_{s,t}\pi_2 + D_{c,s,t}\pi_3 + \delta_c + \theta_t + \eta_{c,s,t} \tag{1}$$

Where $Y_{c,s,t}$ is a physical mobility outcome (i.e., hours away from home and away from home eight or more hours) in county c in state s in time t. FFCRA_t is the post-FFCRA indicator and $nonessential_c$ is the fraction of nonessential workers in county c in the first quarter of 2019 (using one of our two proxies: nonessential worker establishments or a probabilistic measure of nonessential workers). $P_{s,t}$ is a vector of state-level COVID-19-related policies (public school closures, stay at home orders, nonessential business closures, and prohibition on in-restaurant dining (Raifman, 2020)) and $D_{c,s,t}$ is a vector of county-level

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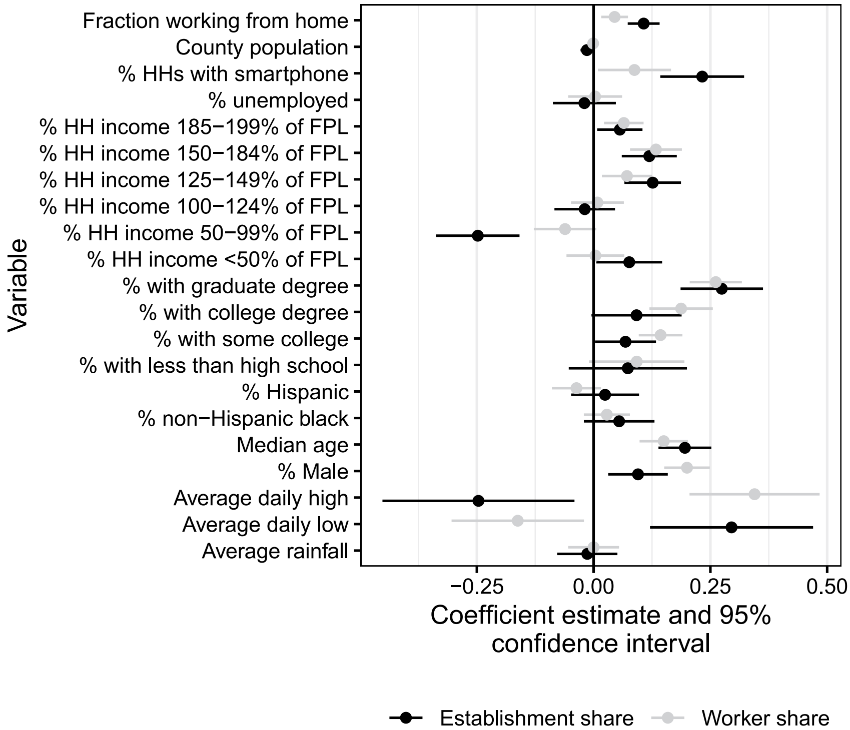

FIGURE 1 Correlates of county-level nonessential workers. See text for data sources. Data are weighted by the county population. The unit of observation is a county, there is observation per county. All regressions are estimated with least squares. Omitted categories for demographics are female and white race. All variables converted to standard deviations prior to estimation. Coefficient estimates are reported with circles. 95% confidence intervals that account for within-county clustering are reported with horizontal lines.

weather variables,[12] the latter of which likely impact our physical mobility measures independent of an pandemic. δ_c and θ_t are county and day fixed-effects, respectively. nonessential$_c$ is time-invariant and, thus, we do not include the main effect as this variable is perfectly collinear with county fixed-effects. Indeed, our county fixed-effects will control for all time-invariant factors over our study period (e.g., labor market structure and state-specific definitions of essential workers).

We do not control for confirmed cases or deaths attributable to COVID-19 in our main specifications as we are concerned that these variables would be endogenous if changes in mobility affect COVID-19 incidence. In addition, given the high-frequency and geographic granularity of our data (recall we measure outcomes at the daily level in the county) many variables that we would like to include in the regression (e.g., daily county-level unemployment rates) are not available to us, though our $P_{s,t}$ vector includes a great number of potential sources of within county, time-varying omitted variable bias. We estimate least squares regressions. The data are weighted by the county population. We cluster standard errors at the county level (Bertrand et al., 2004).[13]

Given that we leverage county-level variation in nonessential workers, discussing the type of worker affected by FFCRA is worthwhile. Blau et al. (2020) carefully examine demographics of essential and nonessential workers. Nonessential workers (vs. essential workers) are slightly more likely to be male, have similar wages, are more likely to be racial or ethnicity minority, and have lower education. Further, the authors note that there is a similar distribution of nonessential and essential workers across broad occupational groupings.

We also explore county-level correlates of the share of nonessential workers. To this end, we regress this share on the following county-level variables: share of households with a cellular phone, unemployment rate, household income as a percent of the Federal Poverty Level (FPL), educational attainment, median age, sex-distribution, race and ethnicity, and weather. These regressions do not control for county-fixed effects and are thus simple (adjusted) correlations. Demographic variables are drawn from the 2015 to 2019 American Community Survey. We report results for both nonessential worker proxies (Figure 1).

We observe that the share of nonessential establishments is correlated with the share of households with many of these covariates. However, we control for county fixed-effects in all regressions which, because our study period is short, likely account for many differences across counties. We also include time-varying (weather-related) covariates in our regression models.

4.4 | Summary statistics

Table 1 provides summary statistics in the pre-FFCRA period. For each census block group, SafeGraph provides the number of devices in bins of hours away from home, assigning the midpoint of each range to the bin, the average number of hours away from home is 4.3 h[14] 26.3% of individuals in a county are away from home more than 8 hours per day and 68.8% (57.8%) of

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TABLE 1 Summary statistics pre-FFCRA.

Variable	Mean/proportion
County-level outcomes	
Average hours away from home	4.303
% away from home >8 h	0.263
County-level share of affected workers	
Share nonessential worker establishments	0.688
Share nonessential workers	0.578
State-level social distancing policies	
Public school closure order	0.826
Stay-at-home order	0.162
Nonessential business closure	0.214
Restaurant dining-in prohibited	0.657
County-level weather controls	
Precipitation (mm)	3.923
Minimum daily temperature (°F)	46.24
Maximum daily temperature (°F)	65.16
N (county * day)	35841

Note: Means and standard deviations across counties in the United States for the period from February 1, 2020 until March 31, 2020. Data are weighted by the county population. The unit of observation is a county in a day.

establishments (workers) are nonessential. Our employment proxy, the fraction of devices that are away from home for eight or more hours a day, is substantially lower in the pre-period (26.3%) than the labor force participation rate (63.4% in February of 2020) or the employment-to-population ratio (61.2% in February of 2020). A reasonable question is why we observe such differences across these metrics.

One explanation for the discordance between the employment and labor force participation rates, and our metrics is that SafeGraph's algorithm likely undercounts time away from home. SafeGraph receives intermittent "pings" when a device is in use. These pings are clustered into visits with the duration of each visit based on the first and last consecutive ping in a location. To prevent inadvertently attributing very long duration visits to a location, SafeGraph requires that the two pings be within 6 hours of one another, otherwise a visit is split into two pieces and the 6-hour period between the two pings is discarded. Therefore, individuals would not be counted as away from home in the data we receive from SafeGraph if they turn off, or do not use, their device for a long period of time while away from home, or if they leave their phone at home. In Supplementary Appendix S1, we show different, less restrictive, cut-offs (i.e., more than 2, 4, and 6 hours away from home per day) and results are robust. We choose to use the eight-hour threshold in our main analyses to be conservative and consistent with the standard workday/school day, but as we show in Supplementary Appendix S1, our findings are not tied to this decision. However, we recognize that this variable is an imperfect proxy of employment and labor force participation, and very likely underestimates the actual number of people at work.

Appendix Figures A1 and A2 report variation in our nonessential worker proxies. The share of nonessential workers varies substantially across U.S. counties (Appendix Figure A1). We note that several states have low shares of nonessential workers while others have higher shares. The two states that arguably appear to be the most discordant border each other: California and Oregon. Apart from these states, the distribution of nonessential worker establishment does not show a strong geographic trend, with most states including counties with both very high and very low shares of such establishments. Indeed, adjacent counties within the same state often have very different levels of nonessential worker establishments: 34.0% of the establishments in Fresno County California, for example, are nonessential, while in neighboring Mono County, California we classify 85.1% of establishments as nonessential. Appendix Figure A2 depicts a histogram of the share of nonessential workers; the distribution is roughly bell-shaped with a modest right skew. The range is 23.1%–92.1% nonessential workers.

A critical component of our analysis is determining the pre- and post-policy periods. Put differently, is April 1st, 2020 a meaningful break? This is the date on which FFCRA became effective, which supports our definition of the pre- and post-periods. Further, as we show in robustness checking, we cannot replicate our findings using placebo effective dates. Finally, we have conducted formal tests for structural breaks. In particular, we estimate a linear-in-time structural break model from the macroeconomics literature, and following Cronin and Evans (2020), for each date from March 20th to April 23rd, 2020. We compute the F-statistic on an indicator for a level-shift on each date and a shift in the slope of a linear time trend on that date. These

F-statistics, which are plotted in Appendix Figures A3a,b, demonstrate that there was a large break in the time trend on April 1st, 2020, which supports our empirical design.

5 | RESULTS

5.1 | Effect of FFCRA on measures of physical mobility

Results based on our baseline specification are reported in Table 2 (using nonessential worker establishments) and Table 3 (using nonessential workers). We observe that for a hypothetical county that moved from having 0% nonessential workers to 100% nonessential workers, the average hours away from home and the share of devices away from home for more than 8 hours per day in the post-FFCRA period decreased. No such county with extreme values of nonessential workers exists in the U.S. and thus we transform the coefficient estimates to reflect a pattern that we do observe (see Appendix Figure A1). We scale our estimates by 0.671 in our nonessential worker establishment specifications. This scaling factor reflects the difference in exposure in the county with the lowest (22.7%) and the county with the highest (89.9%) share of nonessential establishments in 2019 quarter one. For our probabilistic measure of nonessential workers, we use one as the scaling factor, as the minimum and maximum of the employment share variable are zero and one respectively.

Using these transformations, we find that FFCRA leads to a 0.38 h or 22.9-min decrease in average time away from home and a 1.8 ppt decrease in the share of devices away from home eight more hours per day. In relative terms (calculated by comparing the coefficient estimates to the pre-FFCRA sample means), our findings imply 8.9% and 6.9% changes in the outcomes. Results using the share of nonessential employees in the county are broadly similar, with a 0.273 h decrease in time away from home (16.4 min) between counties with 0% versus 100% nonessential workers. The reduction in devices away from home for 8 hours or more is slightly smaller using the employment share, as opposed to establishment share (1.1. vs. 1.8 ppts). Relative to the average levels of these variables in the pre-period, we find changes of −6.3% and −4.2%, respectively.

We also report two alternative transformations in all regression tables that display coefficient estimates generated in Equation (1): (i) a one standard deviation (SD) increase in the share of nonessential establishments (6.0%) or workers (7.6%) and

TABLE 2 Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Baseline specification using the share of nonessential worker establishments to measure treatment intensity.

Outcome	Average hours away from home	Away from home >8 h
Post-FFCRA* % nonessential	−0.568***	−0.027***
	(0.112)	(0.005)
Range observed in data		
[1 SD increase]	[−0.042]	[−0.002]
<10th to 90th percentile Δ>	<−0.099>	<−0.005>
State-level social distancing policies		
Public school closure	−0.071***	−0.004***
	(0.019)	(0.001)
Stay-at-home order	−0.033**	−0.002**
	(0.010)	(0.001)
Nonessential business closure	−0.019	−0.002**
	(0.012)	(0.001)
Restaurant dining-in prohibited	−0.015	−0.000
	(0.009)	(0.001)
Pre-FFCRA mean	4.303	0.263
Number of counties in the sample	2757	2757

Note: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a day. Data are weighted by the county population. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

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TABLE 3 Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Baseline specification using the share of nonessential workers to measure treatment intensity.

Outcome	Average hours away from home	Away from home >8 h
Post-FFCRA* % nonessential	−0.273**	−0.011*
	(0.089)	(0.004)
Range observed in data		
[1 SD increase]	[−0.031]	[−0.001]
<10th to 90th percentile Δ>	<−0.075>	<−0.003>
State-level social distancing policies		
Public school closure	−0.066***	−0.004***
	(0.019)	(0.001)
Stay-at-home order	−0.036***	−0.002***
	(0.010)	(0.000)
Nonessential business closure	−0.017	−0.001*
	(0.011)	(0.001)
Restaurant dining-in prohibited	−0.009	0.000
prohibited	(0.009)	(0.000)
Pre-FFCRA mean	4.303	0.263
Number of counties in the sample	2757	2757

Note: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a day. Data are weighted by the county population. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

(ii) moving from the 10th (61.3% and 48.3% for nonessential worker establishments and workers) to 90th (75.5% and 67.3% for nonessential worker establishments and workers) percentile of the empirical distribution (see Appendix Figure A2). We report these alternative transformations for transparency given that there is no standard scaling approach. These transformations necessarily imply smaller changes post-FFCRA as we are comparing more similar counties.[15]

In Table 4, we report results using IPV measures that incorporate employer size, namely nonessential employers with 50–500 workers (panel A) and nonessential workers employed at employers with 50–500 workers (panel B). As noted earlier, this measure—while more accurate in terms of isolating employer size (recall that FFCRA impacts employers with 50–500 workers)—requires that we impute *both* industry and employer size. Thus, we are concerned that "doubly" imputing may inflate measurement error and associated bias. However, the results reported in Table 4 are similar to those reported earlier (i.e., Tables 2 and 3). After scaling,[16] post-FFCRA (comparing the most to the least exposed county in the U.S.) the average hours away from home decline by 0.282 h while the share of cellular devices away from home eight or more hours per day declines by 1.3 ppts using the nonessential employer exposure measure. The comparable (scaled) post-FFCRA declines are 0.166 h and 1.0 ppts when using the IPV variable that measures employees.

A concern with our analysis thus far is that our measure of nonessential workers is simply correlated with county-level unemployment, which changed over our study period (Congressional Research Service, 2021), and therefore our findings reflect employment changes rather than changes in physical mobility associated with FFCRA. To gauge the empirical importance of this concern, we use monthly county-level unemployment data from the Bureau of Labor Statistics Local Area Unemployment Statistics for March and April 2020 and assign the relevant monthly unemployment rate to each county in our sample. We then control for this variable in Equation (1). Results are reported in Table 5. Our results are not appreciably changed when we include this unemployment proxy. We conclude from this analysis that our findings are not simply capturing economic activity changes.[17]

In Supplementary Appendix S1, we conduct an analysis of heterogeneity in treatment effects across county-level demographics (race, ethnicity, education, and occupation). We find that in counties with higher shares of non-college educated workers the impact of FFCRA on our outcomes is stronger.

TABLE 4 Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: IPV measure imputing both industry and employer size.

Outcome	Average hours away from home	Away from home >8 h
Panel A: Nonessential worker establishments with 50–500 workers used to measure treatment intensity		
Post-FFCRA* % nonessential	−7.237***	−0.345***
	(1.534)	(0.076)
Range observed in data		
[1 SD increase]	[−0.041]	[−0.002]
<10th to 90th percentile Δ>	<−0.106>	<−0.005>
Panel B: Nonessential workers employed at employers with 50–500 workers used to measure treatment intensity		
Post-FFCRA* % nonessential	−0.318	−0.020*
	(0.176)	(0.009)
Range observed in data		
[1 SD increase]	[−0.016]	[−0.001]
<10th to 90th percentile Δ>	<−0.040>	<−0.002>
Pre-FFCRA mean	4.303	0.263
Number of counties in the sample	2757	2757

Note: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

TABLE 5 Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Controlling for local unemployment.

Outcome	Average hours away from home	Away from home >8 h
Panel A: Nonessential worker establishments used to measure treatment intensity		
Post-FFCRA* % nonessential	−0.537***	−0.025***
	(0.113)	(0.005)
Range observed in data		
[1 SD increase]	[−0.040]	[−0.002]
<10th to 90th percentile Δ>	(−0.093)	(−0.004)
Local unemployment rate	−0.004*	−0.000***
	(0.002)	(0.000)
Panel B: Nonessential workers used to measure treatment intensity		
Post-FFCRA* % nonessential	−0.272**	−0.011*
	(0.087)	(0.004)
Range observed in data		
[1 SD increase]	[−0.031]	[−0.001]
<10th to 90th percentile Δ>	(−0.074)	(−0.003)
Local unemployment rate	−0.006**	−0.000***
	(0.002)	(0.000)
Pre-FFCRA mean	4.303	0.263
Number of counties in the sample	2757	2757

Note: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

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5.2 | Internal validity and robustness checking

We next probe the robustness of our design to various threats to identification and robustness checking. First, we explore the ability of our data to satisfy parallel trends. Second, we investigate the importance of unobserved confounders. Third, we conduct falsification exercises to ensure that we are not erroneously capturing the effect of some other policy or factor. Fourth, we consider the appropriateness of using daily data rather than more aggregated data, and we explore aggregating the data to the community zone (vs. county) level. Finally, we conduct a wide range of sensitivity analyses in which we alter the specification, sample, and so forth to assess the stability of our findings. Our results are highly robust to all sensitivity checks. See Supplementary Appendix S1 for details and results.

5.2.1 | Parallel trends

We estimate a modified event study to explore the ability of our data to satisfy the parallel trends assumption that is necessary for generalized DD regressions to estimate causal effects. In particular, we interact the county pre-FFCRA share of nonessential establishments with indicators for each week beginning with March 4th through April 29th (we use Wednesdays because April 1st was a Wednesday). We select the period of March 25th through March 31st as the omitted category as it is the final period in our sample before FFCRA took effect (Lovenheim, 2009). We aggregate the event time bins to the week-level to smooth out noise, but we leave the underlying data at the date-level. Otherwise, the event study equation is identical to Equation (1). Results (unscaled) are reported graphically in Figure 2, panel A reports results for hours away from work per day and panel B reports the share of devices away from home eight or more hours per day (all event study figures for our main outcomes reported in the paper follow this structure). These figures suggest that counties with higher and lower shares of nonessential establishments follow similar trends pre-FFCRA. Beginning on April 1st, 2020, counties with higher shares of nonessential establishments and workers experience sharp decreases in average hours not at home and the share of individuals away from home eight or more hours per day. This pattern suggests that our data can satisfy parallel trends and FFCRA effects are observable precisely at the effective date.

5.2.2 | Unobserved confounders

We conduct a test to explore the importance of unobserved confounders. We first report results excluding time-varying controls and second, we include additional controls (in particular, confirmed COVID-19 cases and deaths). If results change when we include and exclude the time-varying controls, this pattern of results suggests that unobservable confounders may not drive our findings (Altonji et al., 2005). Our coefficient estimates are not appreciably different across specifications (Tables 6 and 7).

A potentially important omitted variable is the ability to work from home, which may vary across counties. Such a variable is not available for our time period.[18] However, this issue remains a concern and we attempt to address it as best we can.[19] We estimate regressions in which we separate out the sixteen different industry codes, interacting each with the post-FFCRA dummy[20] (rather than all 16 measures); constructing the exposure variable in this manner (rather than using the overall nonessential worker share) largely holds constant the ability to work from home (see Figure 3). A secondary objective of this exercise is to attempt to understand which (if any) of the sixteen nonessential industries is empirically the most important. Interestingly, education, real estate, information, arts and entertainment, and accommodation and food industries appear to be particularly important industries. As described in footnote 15, we have constructed exposure measures using groups of essential industries that appear (based on the nature of work common in these industries) to be similar in terms of the ability to work from home. Results (which are reported in the footnote) are similar to our main findings.

We next apply logic from the omitted variable bias formula to generate intuition on the sign of bias attributable to the ability to work from home. First, we correlate our exposure variable with a proxy for the ability to work from home available in the May Current Population Survey (CPS) onward (Flood et al., 2022): working at home for pay at any point during the previous 4 weeks due to the COVID-19 pandemic ("telework").[21] We use data from May through December 2020. After adjusting for fixed-effects and other controls in our regression (Table 8), we find that an additional hour away from home and the percentage point increase in the fraction of workers away from home more than 8 hours per day are negatively associated with the probability of telework.[22] We find that our exposure variable is negatively correlated with our outcome variables (Table 8). Thus, our main coefficient estimates are likely biased toward zero, suggesting that we provide a lower bound estimate of the true effect.

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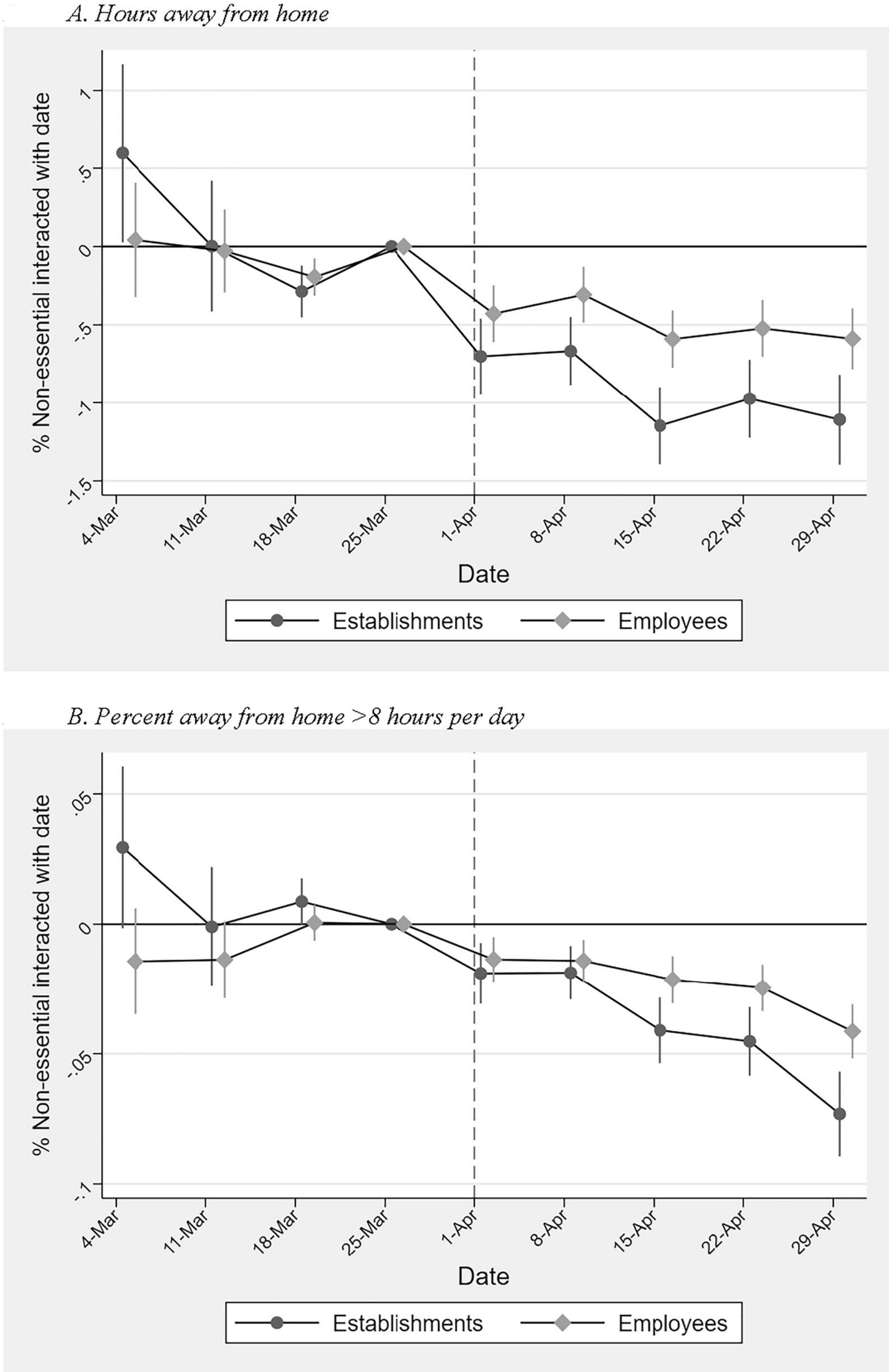

FIGURE 2 Effect of FFCRA on physical mobility outcomes using an event study design. Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Coefficient estimates are reported with circles. 95% confidence intervals that account for within-county clustering are reported with vertical lines. The omitted category is March 13th, 2020 to March 24th, 2020. The vertical dashed line indicates April 1st, 2020 which is the first day that FFCRA became effective.

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TABLE 6 Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Exclude the state-level social distancing policies and weather covariates.

Outcome	Average hours away from home	Away from home >8 h
Panel A: Nonessential worker establishments used to measure treatment intensity		
Post-FFCRA* % nonessential	−0.587***	−0.027***
	(0.119)	(0.006)
Range observed in data		
[1 SD increase]	[−0.043]	[−0.002]
<10th to 90th percentile Δ>	<−0.102>	<−0.005>
Panel B: Nonessential workers used to measure treatment intensity		
Post-FFCRA* % nonessential	−0.282**	−0.010*
	(0.093)	(0.005)
Range observed in data		
[1 SD increase]	[−0.032]	[−0.001]
<10th to 90th percentile Δ>	<−0.077>	<−0.003>
Pre-FFCRA mean	4.303	0.263
Number of counties in the sample	2757	2757

Note: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All regressions are estimated with least squares and control for county fixed-effects and date fixed-effects. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

TABLE 7 Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Including COVID-19 cases and deaths.

Outcome	Average hours away from home	Away from home >8 h
Panel A: Nonessential worker establishments used to measure treatment intensity		
Post-FFCRA* % nonessential	−0.547***	−0.027***
	(0.119)	(0.006)
Range observed in data		
[1 SD increase]	[−0.040]	[−0.002]
<10th to 90th percentile Δ>	(−0.095)	(−0.005)
Panel B: Nonessential workers used to measure treatment intensity		
Post-FFCRA* % nonessential	−0.243**	−0.010*
	(0.091)	(0.005)
Range observed in data		
[1 SD increase]	[−0.027]	[−0.001]
<10th to 90th percentile Δ>	(−0.066)	(−0.003)
Pre-FFCRA mean	4.303	0.263
Number of counties in the sample	2757	2757

Note: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All regressions are estimated with least squares and control for confirmed COVID-19 cases and deaths, social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

5.2.3 | Aggregation

Our regression uses daily data that captures both social distancing (for those not concerned about having come into contact with COVID-19 but wishing to reduce community transmission) and quarantining behaviors (for individuals coming into contact with COVID-19 and beginning their CDC-recommended quarantine procedure). Ideally, we would like to consider

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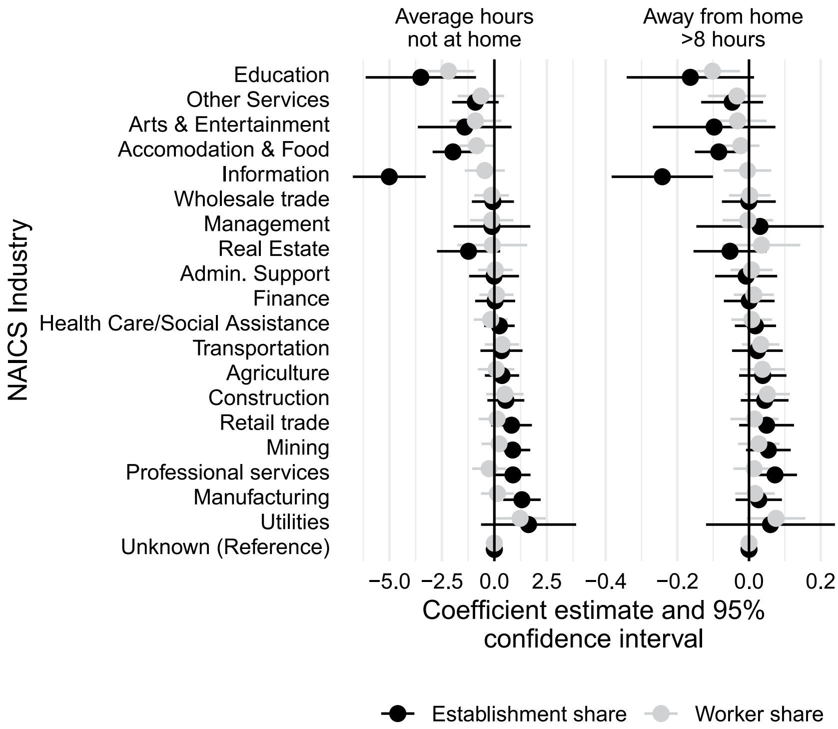

FIGURE 3 Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Allow for separate industry code interaction with nonessential worker industry codes. Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a day. Data are weighted by the county population. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Coefficient estimates are reported in circles. 95% confidence intervals account for within-county clustering and are reported with horizontal lines.

TABLE 8 Association of SafeGraph metrics with self-reported employment and telework from the Current Population Survey.

Outcome	Telework	Telework	Telework
Hours away from home	−0.123***		
	(0.020)		
Fraction away more than 8 h		−2.19***	
		(0.287)	
% nonessential			1.531***
			(0.185)
Mean	0.257	0.257	0.257
Number of counties in sample	326	326	326

Note: Data sources are SafeGraph metrics and the Current Population Survey (May-December of 2020). Share nonessential worker establishments used to measure treatment intensity. Data are weighted using county population. The unit of observation is a county. All regressions are estimated with least squares and control for month and state. Standard errors are clustered at the state-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

both, in particular as adequate quarantining includes multiple days. One method to separate successful quarantining from social distancing specifically is to create a new variable that shows if people left home at all and studying if people followed this for 1-week and 2-week periods of time. We choose the daily metrics as our primary outcomes because using longer timeframes implies that we must form our averages based on different devices as the SafeGraph data are an unbalanced panel in devices, this issue is less of a concern when aggregating to the daily level. However, to explore the robustness of our findings to metrics measured over a longer period of time, we construct averages at the week and 2-week (vs. day) level as these timeframes may better align with the actual amount of time infected or exposed persons allocated to quarantining. Results are reported in Table 9 (using a daily model with a 1-week lookback period) and 7 (using a daily model with a 2-week lookback period) and are not appreciably different from our main findings. We also create a variable that indicates the share of devices that did not leave the SafeGraph defined home at all over these two lookback periods (see Tables 9 and 10). The results are in line with our main metrics: the share of devices at home over these durations increased post-FFCRA more in counties more (vs. less) highly exposed to the policy.

In a related aggregation issue, some people may not reside in the same county in which they work, which could lead to measurement error in our county-level exposure variable. To address this issue, we have aggregated our data to the commuting zone level and re-estimate Equation (1), we replace county fixed-effects with commuting zone fixed-effects. Results

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TABLE 9 Effect of FFCRA on average physical mobility outcomes over a 1-week lookback period using a difference-in-differences style model.

Outcome	Average hours away from home	Away from home >8 h	Always home
Panel A: Nonessential worker establishments used to measure treatment intensity			
Post-FFCRA* % nonessential	−0.693***	−0.031***	0.174***
	(0.121)	(0.006)	(0.017)
Range observed in data			
[1 SD increase]	[−0.051]	[−0.002]	[0.013]
<10th to 90th percentile Δ>	<−0.120>	<−0.005>	<0.030>
Panel B: Nonessential workers used to measure treatment intensity			
Post-FFCRA* % nonessential	−0.309***	−0.010*	0.083***
	(0.092)	(0.005)	(0.011)
Range observed in data			
[1 SD increase]	[−0.035]	[−0.001]	[0.009]
<10th to 90th percentile Δ>	<−0.084>	<−0.003>	<0.023>
Pre-FFCRA mean	4.284	0.261	0.312
Number of counties in the sample	2757	2757	2757

Note: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Share nonessential worker establishments used to measure treatment intensity. Data are weighted by the county population. The unit of observation is a county in a day. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

TABLE 10 Effect of FFCRA on average physical mobility outcomes over a 2-week lookback period using a difference-in-differences style model.

Outcome	Average hours away from home	Away from home >8 h	Always home
Panel A: Nonessential worker establishments used to measure treatment intensity			
Post-FFCRA* % nonessential	−0.776***	−0.032***	0.226***
	(0.148)	(0.008)	(0.023)
Range observed in data			
[1 SD increase]	[−0.057]	[−0.002]	[0.017]
<10th to 90th percentile Δ>	<−0.135>	<−0.006>	<0.039>
Panel B: Nonessential workers used to measure treatment intensity			
Post-FFCRA* % nonessential	−0.316**	−0.005	0.104***
	(0.102)	(0.006)	(0.013)
Range observed in data			
[1 SD increase]	[−0.036]	[−0.001]	[0.012]
<10th to 90th percentile Δ>	<−0.086>	<−0.001>	<0.028>
Pre-FFCRA mean	4.425	0.271	0.283
Number of counties in the sample	2757	2757	2757

Note: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Share nonessential worker establishments used to measure treatment intensity. Data are weighted by the county population. The unit of observation is a county in a day. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

(reported in Table 11) are very similar to our main findings, although less precise, the drop in precision is perhaps not surprising as we have 2757 counties and just 646 commuting zones, thus we lose a substantial amount of variation in our exposure measure.

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TABLE 11 Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Aggregate to the commuting-zone level.

Outcome	**Average hours away from home**	**Away from home >8 h**
Post-FFCRA* % nonessential	−0.433*	−0.018*
	(0.194)	(0.009)
Range observed in data		
[1 SD increase]	[−0.027]	[−0.001]
<10th to 90th percentile Δ>	<−0.065>	<−0.003>
Pre-FFCRA mean	4.309	0.264
Number of counties in the sample	646	646

Note: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Share nonessential worker establishments used to measure treatment intensity. The unit of observation is a commuting-zone in a day. Data are weighted by the commuting-zone population. All regressions are estimated with least squares and control for social distancing policies, weather covariates, commuting-zone fixed-effects, and date fixed-effects. Standard errors are clustered at the commuting-zone-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

TABLE 12 Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Falsification testing using 2019 data.

Outcome	**Average hours away from home**	**Away from home >8 h**
Panel A: Nonessential worker establishments used to measure treatment intensity		
Post-FFCRA* % nonessential	0.086	−0.000
	(0.072)	(0.005)
Range observed in data		
[1 SD increase]	[0.006]	[−0.000]
<10th to 90th percentile Δ>	<0.015>	<−0.000>
Panel B: Nonessential workers used to measure treatment intensity		
Post-FFCRA* % nonessential	0.152**	0.007
	(0.056)	(0.004)
Range observed in data		
[1 SD increase]	[0.017]	[0.001]
<10th to 90th percentile Δ>	<0.042>	<0.002>
Pre-FFCRA mean	4.030	0.252
Number of counties in the sample	2757	2757

Note: Data source is SafeGraph Social Distancing Metrics files March 15th, 2019 through May 2nd, 2019; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All regressions are estimated with least squares and control for weather covariates, county fixed-effects, and date fixed-effects. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

5.2.4 | Falsification

We conduct three falsification tests to further probe our design. First, we estimate Equation (1) using data from 2019: we use the same dates but 1 year prior to the pandemic. Note that we cannot control for state-level social-distancing policies, as they were not in place in 2019; otherwise, the specification is identical to Equation (1). If we were able to replicate our findings in the earlier year, this pattern of results would call to question the internal validity of our design, for example, such a finding might suggest that there are simply breaks in physical mobility that occur each year in the U.S. at roughly April 1st. Results are listed in Table 12: coefficient estimates are generally small in magnitude and not statically different from zero, an exception is one coefficient estimate for average hours away from home that is statistically significant in the opposite direction to our baseline results.

Second, we randomly re-shuffle our treatment variable across counties and dates, and re-estimate Equation (1) 100 times, thereby generating 100 placebo estimates (treatment is assigned to a date). We compare our main coefficient estimate to the

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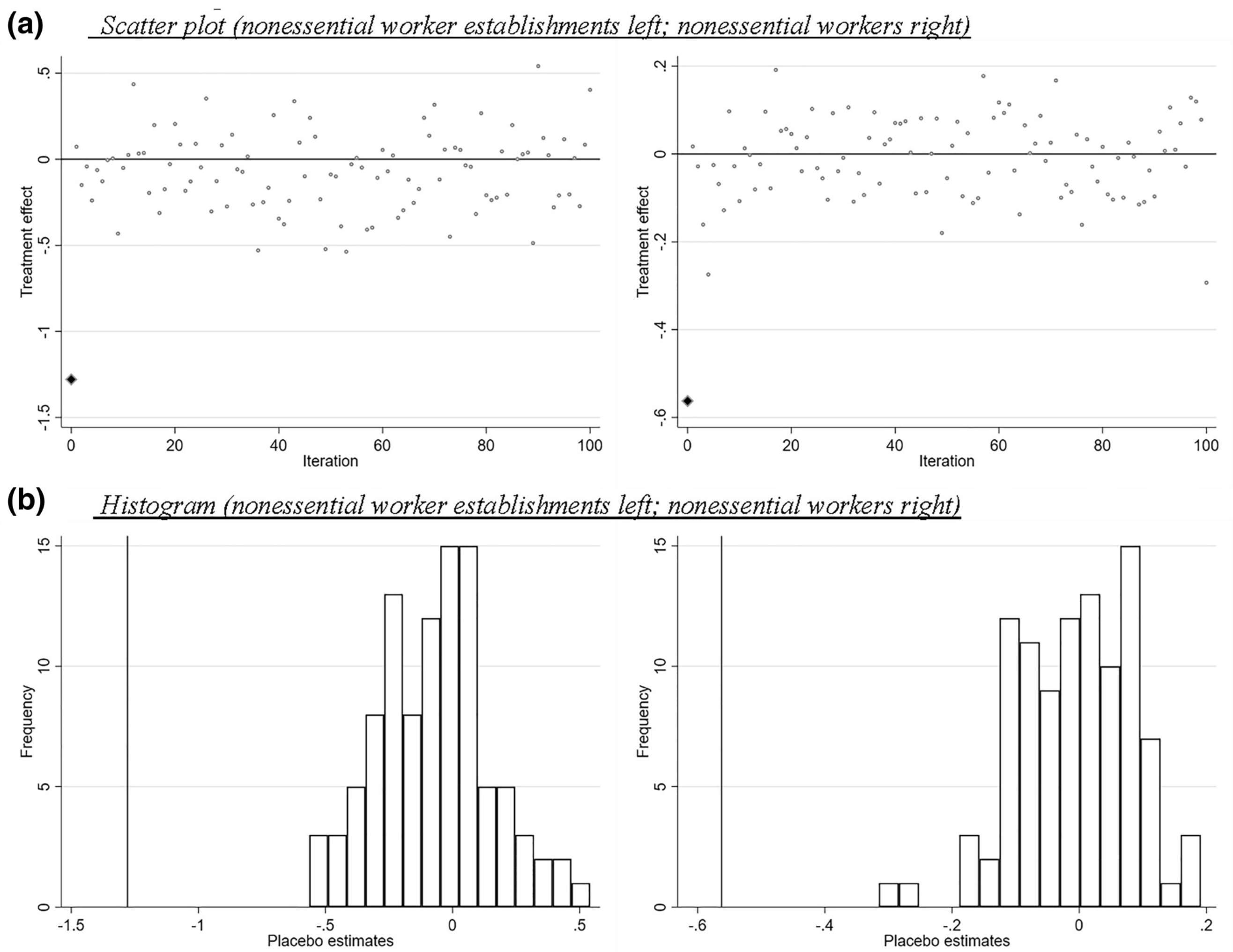

FIGURE 4 The outcome is the average hours away from home. Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a day. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Panel A: black diamond is the coefficient estimate from our preferred specification and small white circles capture coefficient estimates generated in equation (1) after randomly re-shuffling the treatment variable (Post FFCRA*% nonessential worker establishments) across counties and dates.

distribution of placebo estimates. If we are capturing the "true" FFCRA effect rather than some other co-occurring policy or factor that changed on April 1st, 2020, then our main coefficient estimate should be an outlier. We report results graphically in Figures 4 and 5: we present a scatter plot (panel A) and a histogram (panel B) of the estimates. In all figures, our main coefficient estimate is an outlier.

Third, we estimate Equation (1) using only those counties that had a pre-FFCRA PSL mandate (Table 13), these counties are excluded from our main analyses. Such counties could experience less benefit from the federal PSL policy as many workers already had access to PSL pre-FFCRA. Results are not statistically different from zero, suggesting that localities without such policies benefited from PSL provided by FFCRA while those counties that had such a policy may not have benefited from FFCRA. We also include all counties in the sample (those with and without a pre-FFCRA PSL mandate) and interact the pre-existing PSL mandate with FFCRA, this analysis further suggests that the counties without PSL prior to FFCRA are disproportionately affected by the policy.

We note that in panel A (but not panel B) the coefficient estimates in the sample of counties with a pre-FFCRA city or state PSL mandate are statistically distinguishable from zero, but smaller in magnitude than our main coefficient estimates. While we lack the data to explore this finding, we can offer some hypotheses. First, the pandemic may have increased attention on public health policies generally and PSL specifically, suggesting that there was better knowledge of FFCRA than earlier city or state PSL policies. Second, there is some pre-FFCRA evidence that employers do not fully comply with city or state PSL policies, Maclean et al. (2020) show that PSL coverage increases following a state PSL mandate, but the coverage rate does not reach

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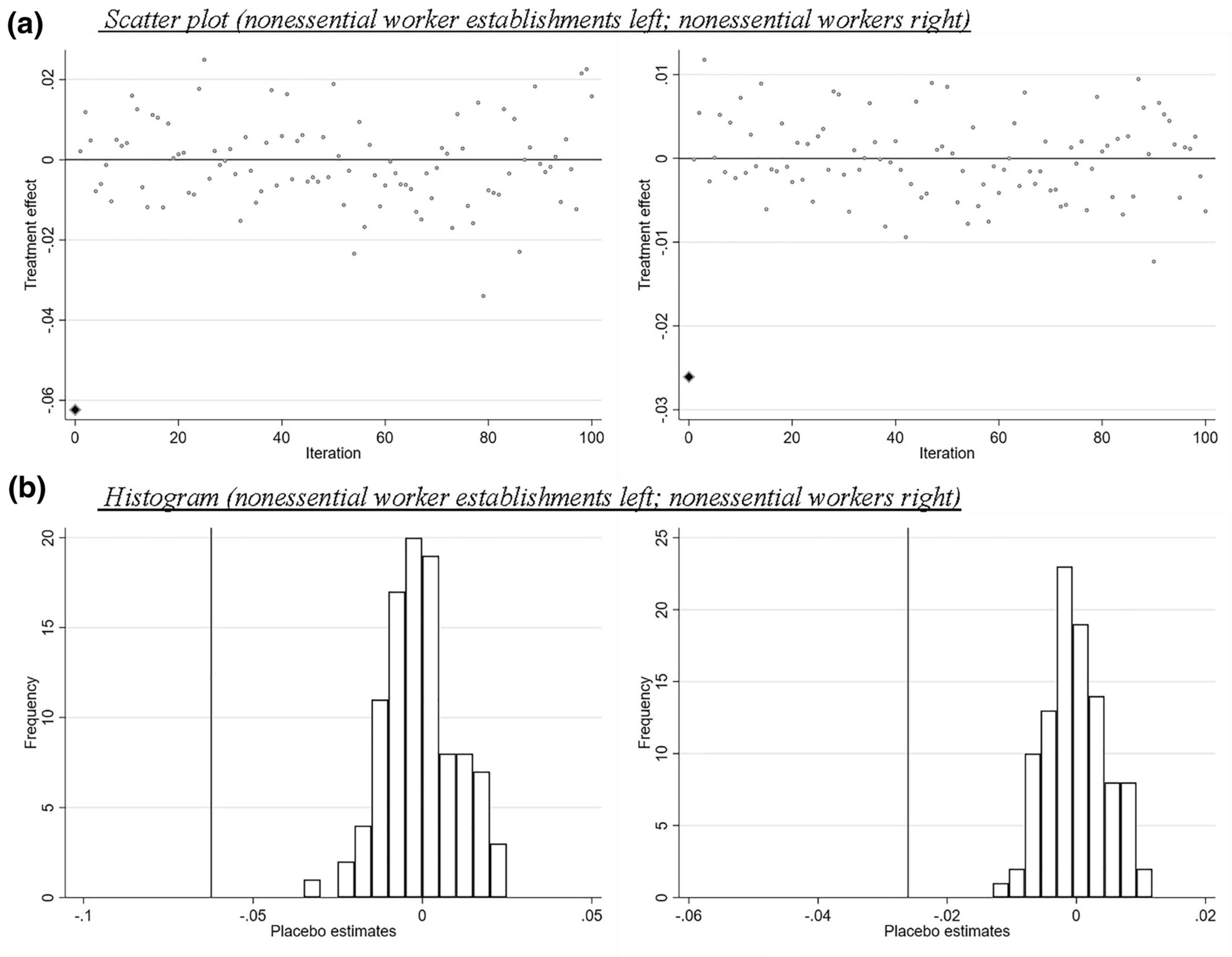

FIGURE 5 The outcome is the share of devices away from home ≥8 hr. Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a day. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Panel A: black diamond is the coefficient estimate from our preferred specification and small white circles capture coefficient estimates generated in equation (1) after randomly re-shuffling the treatment variable (Post FFCRA*% nonessential worker establishments) across counties and dates.

100%, the authors raise employer non-compliance as a potential reason for less than full coverage rates. The federal government fully funded all FFCRA benefits through tax credits, thus employers may have been more likely to comply as compliance was less costly (vs. city or state PSL policies, which do not offer tax credits to employers). A concern among employers with city or state PSL is costs, these perceived costs may have been reduced with FFCRA. Both of these factors could suggest that FFCRA could be better understood and enforced than earlier city or state PSL. Further, FFCRA benefits could be used when children were out of school due to closures and the ability to use FFCRA benefits to care for children, combined with limited childcare options during the early COVID-19 period, may have also led to FFCRA greater awareness of FFCRA than earlier city or state mandates.

We have also conducted a review of the literature to determine if any other policies or factors related to social distancing or quarantining occurred over our study period. Our review does not suggest that such a policy or factor changed, although we cannot with certainty rule out that possibility given myriad changes the pandemic imposed on society. See Supplementary Appendix S1.

5.2.5 | Assessment of internal validity

We view our testing of the design as providing suggestive evidence that are main coefficient estimates are not attributable to a violation of parallel trends, unobserved confounders, or some other policy or factor. Further, as we show in Table 5 when

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TABLE 13 Effect of FFCRA on physical mobility outcomes in a difference-in-differences style model: Heterogeneity by pre-FFCRA paid sick leave mandate.

Outcome	Average hours away from home		Away from home >8 h	
Panel A: Nonessential worker establishments used to measure treatment intensity				
Post-FFCRA* % nonessential	−0.231**	−0.296***	−0.012**	−0.016***
	(0.079)	(0.075)	(0.004)	(0.004)
Range observed in data				
[1 SD increase]	[−0.017]	[−0.022]	[−0.001]	[−0.001]
<10th to 90th percentile Δ>	<−0.040>	<−0.051>	<−0.002>	<−0.003>
* no prior PSL		−0.279*		−0.013
		(0.136)		(0.007)
Range observed in data				
[1 SD increase]		[−0.021]		[−0.001]
<10th to 90th percentile Δ>		<−0.049>		<−0.002>
Panel B: Nonessential workers used to measure treatment intensity				
Post-FFCRA* % nonessential	−0.048	−0.067	0.003	0.002
	(0.126)	(0.129)	(0.007)	(0.007)
Range observed in data				
[1 SD increase]	[−0.005]	[−0.008]	[0.000]	[0.000]
<10th to 90th percentile Δ>	<−0.013>	<−0.018>	<0.001>	<0.001>
* no prior PSL		−0.206		−0.013
		(0.156)		(0.008)
Range observed in data				
[1 SD increase]		[−0.023]		[−0.001]
<10th to 90th percentile Δ>		<−0.056>		<−0.004>
Sample	Counties with prior PSL mandate	All	Counties with prior PSL mandate	All
Pre-FFCRA mean	3.866	4.143	0.236	0.253

Note: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. All variables in models with all counties are interacted with an indicator for a county not having a paid sick law in effect prior to the pandemic. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

we control for county-level unemployment rates, we do not believe that our measure of nonessential worker share is simply capturing changes in economic activity. While we cannot fully address bias from omitting the ability to work from home from our regression, this omission should lead us to under-state FFCRA effects.

5.3 | Effect of FFCRA on confirmed COVID-19 cases

Finally, we investigate the impact of FFCRA on confirmed COVID-19 cases following Pichler et al. (2020), although we use a different design. To do so, we utilize data from the Johns Hopkins University Coronavirus Resource Center and construct the logarithm of the number of new cases in the *next* 7 days in each county-day of our study period. Table 14 reports results based on Equation (1).[23] We observe a decline of 55.0%[24] in the next week's new number of cases post-FFCRA (comparing the counties with the lowest and highest shares of nonessential workers in our sample). While our coefficient estimate implies a large effect, our confidence intervals are somewhat wide and the upper tail of our 95% confidence interval implies a 27.8% decrease. We also note that Pichler et al. document substantial effect sizes: over a 100% reduction in new cases per day post-FFCRA.[25]

We can also put this decline in context by comparing our estimated change in cases to the change in mobility. Our findings suggest that the weekly COVID-19 incidence decreased by 7.7 log points post-FFCRA, using a one standard deviation change in the nonessential establishment share. On a daily basis this coefficient estimate corresponds to a 1.1 log point reduction in incidence. On that same basis, we find a 0.7% reduction in mean hours away from home which, if one assumes that is linearly

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TABLE 14 Effect of FFCRA on the logarithm of new weekly confirmed COVID-19 cases in the following 7 days using a difference-in-differences style model.

Treatment intensity defined using	Nonessential worker establishments	Nonessential workers
Post-FFCRA* % nonessential	−1.052*	−1.159***
	(0.440)	(0.350)
Range observed in data		
[1 SD increase]	[−0.077]	[−0.131]
<10th to 90th percentile Δ>	(−0.183)	(−0.316)
Pre-FFCRA mean (cases per 100,000)	10.3	10.3
Number of counties in the sample	2757	2757

Note: Data source is Johns Hopkins University Coronavirus Resource Center files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a day. Data are weighted by the county population. All regressions are estimated with least squares and control for social distancing policies, weather covariates, county fixed-effects, and date fixed-effects. Standard errors are clustered at the county-level and are reported in parentheses.

***; **; *, statistically different from zero at the 0.1%, 1%, 5% level.

related to contacts, implies that each newly infected individual would produce approximately 1.5 new cases, and this implied change in cases is well within conventional estimates of R_0 for (Ke et al., 2021).

6 | DISCUSSION

We offer the first evidence on the impact of the federal COVID-19 paid sick leave policy on self-quarantining and family responsibilities during a global pandemic. These behaviors are key components of front-line public health efforts to curb disease spread and address labor market inequities across workers with differential levels of family responsibilities (Avery et al., 2020). To study this question, we take advantage of a unique confluence of events: we study how a temporary PSL policy in the United States—a large, interconnected economy that lacks universal and nationally mandated PSL—during the COVID-19 pandemic is associated with physical mobility within the population. The temporary policy provided a large share of workers (pre-policy estimates suggest as high as 47% of workers (Glynn, 2020)) with approximately 2 weeks of fully compensated PSL, with some workers eligible for substantially more leave. While the federal PSL policy impacted the nation as a whole, differences in eligibility for the benefits differed across U.S. counties, thereby allowing us to estimate a modified difference-in-differences method that exploits heterogeneity in treatment intensity. We combine high frequency data based on more than 20-million cellular devices' (individuals') GPS locations to track physical mobility measured at the county-level with this generalized DD regression approach to estimate the effect of FFCRA on proxies for social distancing.

Following the federal Act, those individuals are more likely to stay home and less likely to work. In particular, post-FFCRA, comparing the county with the lowest share of nonessential workers to the county with the highest share of nonessential workers in the U.S., the average number of hours away from home decreases by 8.9% and the share of individuals away from the home for more than 8 hours per day declines by 6.9%. Our standard errors are relatively small, and we can rule out effect sizes larger (smaller) than 12.3% (5.4%) and 9.4% (4.4%) in these outcomes respectively with 95% confidence. Further, counties more exposed to the policy experienced larger declines in confirmed COVID-19 cases. We offer evidence that our research design is valid and show that our results are robust to numerous sensitivity checks.

For the average county, our estimates indicate that FFCRA reduced total hours away from home in the county by approximately 30.7 million hours each day (96% of Americans had a cellular device over our study period (Pew Research Center, 2021). Assuming that the reduction in hours away from home came from nonessential workers only indicates that the average nonessential worker in the average county spent 0.79 fewer hours away from home post FFCRA. Since FFCRA is limited a 14-day allotment of PSL (10 business days), FFCRA may have reduced aggregate time away from home by 307 million hours, or a little under *one* hour per American. Taken in this light, while our relative effect sizes may appear modest in size, the number of hours at home induced by FFCRA appears non-trivial.

In summary, we provide evidence on a novel question, whether paid sick leave policy (i.e., FFCRA) affects physical mobility, a quarantining proxy. Our findings suggest that providing paid sick leave aids workers in following public health guidelines.

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CONFLICT OF INTEREST STATEMENT

Michael F. Pesko reports research funding for the proposed work from the National Cancer Institute, and unrelated consulting revenue from the National Institutes of Health, Virginia Foundation for Healthy Youth, and the University of Kentucky's Institute for the Study of Free Enterprise. Kosali Simon receives journal editorial compensation (Journal of Health Economics and Journal of Human Resources) and compensation from grants from National Institutes of Health. Simon serves on unpaid capacity as advisor to various government and private organizations. Maclean reports research funding from Washington Center for Equitable Growth. Other authors declares no conflict of interest.

DATA AVAILABILITY STATEMENT

We use SafeGraph data through a data use agreement (DUA) with the data provider. We cannot share the data due to our DUA, but we are happy to provide guidance to researchers interested in accessing these data.

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ENDNOTES

[1] Later legislation resulted in these benefits becoming voluntary for employers to provide as of 2021 (Congressional Budget Office, 2021; Government Accountability Office, 2021), but the mandatory provision of these benefits was not continued beyond December 31st, 2020. We discuss our focus on the policy implementation in Supplementary Appendix S1.

[2] The federal government defines an essential worker who is employed in one of "…16 critical infrastructure sectors whose assets, systems, and networks, whether physical or virtual, are considered so vital to the United States that their incapacitation or destruction would have a debilitating effect on security, national economic security, national public health or safety, or any combination thereof" (U.S. Cybersecurity and Infrastructure Security Agency, 2020).

[3] Small and large employers are exempt. We exploit this source of variation in our empirical models as well.

[4] The DOL uses the term "essential responder." We use the common colloquial term "essential worker."

[5] Examples of application types include weather and mobile retail applications.

[6] Less than 0.2% of Census block groups (<0.1% by population) are missing during our study period.

[7] Aggregating to the county-level yields a correlation of 0.967 during our study period between the number of devices in the county and county population, which is substantially greater than the 0.782 correlation we observe at the Census block group level.

[8] SafeGraph reports median hours. We calculate time away from the home using the midpoint of binned categories.

[9] For example, if an individual does not take their cellular device with them when they leave their home for work, then we will not capture this working behavior and instead we would, erroneously code this individual as at home.

[10] Individuals must be 13 years or older to be included in the SafeGraph sample.

[11] While the QCEW contains some information on the number of workers, there is substantial suppression at the county-industry level due to privacy concerns. Hence, we cannot use this information.

[12] Weather variables accessed at https://github.com/jbayham/gridMETr (last accessed 4/7/2021).

[13] While all units in our sample are treated at the same time (FFCRA became effective at the same time for all U.S. counties), we have differences in treatment dose (i.e., counties with higher shares of nonessential workers are more exposed to, or received a higher dose of, FFCRA relative to counties with lower shares). As noted by Callaway, Goodman-Bacon, and Sant'Anna (2021), this setting requires additional assumptions to the canonical DD setting. We discuss these assumptions in Supplementary Appendix S1.

[14] SafeGraph reports time away from home is reported in buckets of 20 min or less, 21 to <45 min, 46 min to <1 h, 1 to <2 h, 2 to <3 h, 3 to <4 h, 4 to <5 h, 5 to <6 h, 6 to <7 h, 7 to <8 h, 8 to <9 h, 9 to <10 h, 10 to <11 h, 11 to <12 h, 12 to <14 h, 14 to <16 h, 16 to <18 h, 18 to <20 h, 20 to <22 h, and 22 h or more.

[15] We also check the sensitivity of our results in Table 3 to using a version of the nonessential share that does not include NAICS codes for education, since many educators are nonessential employees but were moved online by school and university closings during the COVID-19 pandemic. Using this variable, the coefficient estimate is very similar to our main estimate in this case (average hours away from home coefficient estimate = −0.213; standard error (SE) = 0.089; 1 SD difference = −0.024; 90-10 difference = −0.058; away from home >8 h coefficient

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estimate = −0.008; SE = 0.004; 1 SD difference = −0.001; 90−10 difference = −0.002). We also construct another version of the nonessential share for industries that may have particular difficulty moving to remote or online work: accommodation and food, transportation, agriculture, construction, mining, manufacturing, and utilities. Here, effect sizes are larger than in Table 3 (average hours away from home coefficient estimate = −0.297; SE = 0.055; 1 SD difference = −0.050; 90−10 difference = −0.133; away from home >8 h coefficient estimate = −0.012; SE = 0.003; 1 SD difference = −0.002; 90−10 difference = −0.006). These results demonstrate robustness to including and excluding key industries that plausibly had greater leeway to adopt online and remote work approaches during COVID-19.

[16] The scaling factor (comparing the counties with the lowest and highest shares of exposure observed in the data) for panel A in Table 3 is 0.039 while the comparable scaling factor in panel B is 0.520.

[17] Ideally, we would have daily unemployment data for each county. Unfortunately, such data do not exist.

[18] We acknowledge that there are proxies for realized working from home, but what is of interest in our setting is the potential ability to perform work required to complete job tasks from home. This could include, for example, broadband Internet access and industry composition that provides greater or lesser ability to work from home.

[19] Further, working from home is potentially difficult for essential workers as well as nonessential workers, and working from home increased significantly during the pandemic for all groups.

[20] The omitted category is industry code 99, which is missing industry.

[21] In the CPS, smaller counties are suppressed for privacy, thus our sample includes larger U.S. counties (identified individually), and, for the remaining counties, we have pooled them into one "rest of state" pseudo-county per state.

[22] In our analysis of the CPS, we cluster standard errors at the state-level (vs. county-level) to be conservative in estimation of standard errors as we assign all suppressed counties a single state-level average value.

[23] We have estimated an event study for this outcome and do not observe evidence of differential pre-trends. See Supplementary Appendix S1 for full details.

[24] We calculate this number as follows: percent change = exp(−1.159*0.689)−1.

[25] In particular, the sample mean number of new confirmed cases per day per state is 353 (Exhibit 1 in the Pichler et al. study) and the coefficients estimates from DD regressions range from −375.583 to −494.868 (Exhibit 2 in the Pichler et al. study). Comparing the coefficient estimates to the sample mean implies relative declines of 106%–140% in new daily confirmed cases per state post-FFCRA for states without a preexisting PSL mandate to those with such a PSL mandate.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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APPENDIX

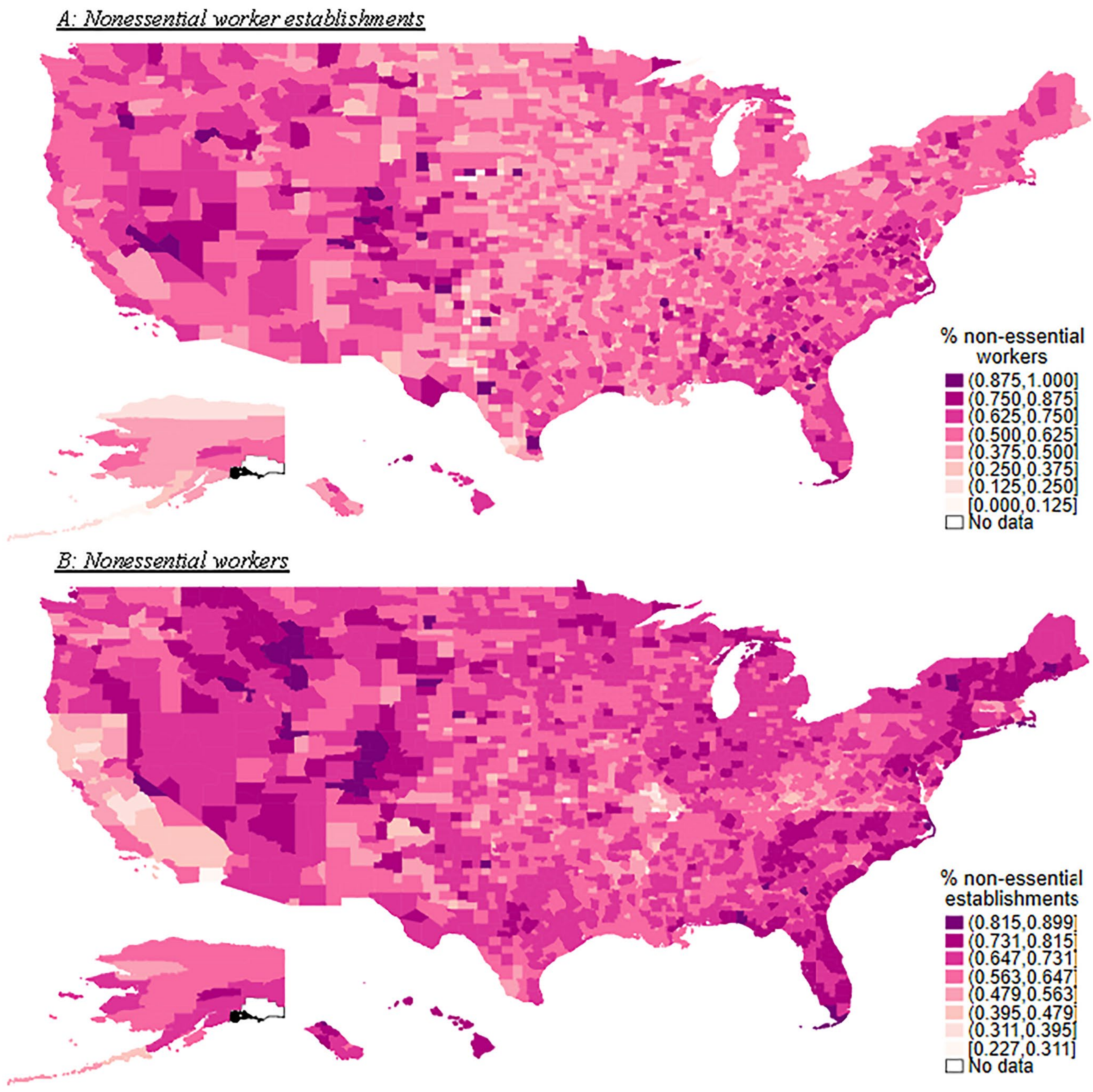

FIGURE A1 Geographic distribution of nonessential worker establishments and workers across U.S. counties. Data source is Quarterly Census of Employment and Wages 2019. We include the following NAICS codes: 111, 112, 114, 115, 22, 23, 311, 3121, 3221, 32222, 32223, 32229, 3251, 3253, 3254, 3256, 3259, 33311, 3341, 3342, 3343, 3345, 3344, 3346, 3352, 3391, 4233, 4234, 4237, 4241, 4242, 4244, 4245, 4246, 4247, 4248, 4249, 4441, 44511, 44512, 4452, 4453, 4523, 454110, 44611, 447, 481, 482, 484, 4851, 4852, 4853, 4854, 4855, 4859, 491, 492, 493, 51111, 515, 517, 5182, 51913, 521, 52211, 52219, 52212, 52213, 5222, 5223, 523, 5241, 5412, 5415, 5416, 5417, 54194, 5525, 5617, 56173, 562, 616, 6211, 6212, 6213, 6214, 6215, 6216, 6219, 6221, 6222, 6223, 6231, 6232, 6233, 6239, 6241, 6242, 6244, 7211, 722, 8111, 8112, 8113, 8122, 8123, 92111, 92112, 92113, 92114, 92115, 92119, 922, 923, 924, 925, 926, 927, and 928. We exclude the following NAICS codes: 311811, 42491, 44413, 517311, 56173, 62131, 62132, 7224, and 811192. [Colour figure can be viewed at wileyonlinelibrary.com]

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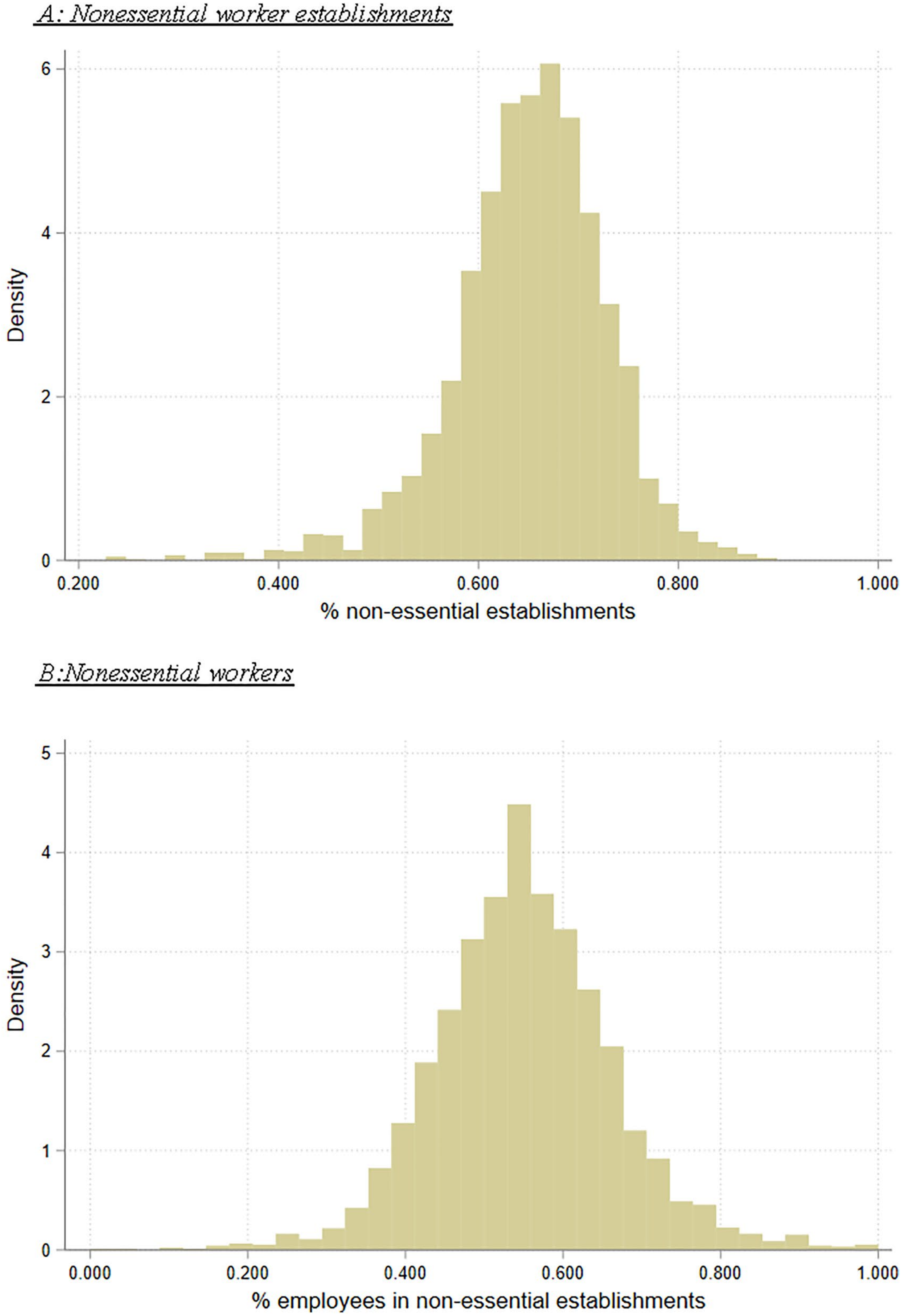

FIGURE A2 Frequency distribution of nonessential worker establishments and workers across U.S. counties. Data source is Quarterly Census of Employment and Wages 2019 quarter 1. We include the following NAICS codes: 111, 112, 114, 115, 22, 23, 311, 3121, 3221, 32222, 32223, 32229, 3251, 3253, 3254, 3256, 3259, 33311, 3341, 3342, 3343, 3345, 3344, 3346, 3352, 3391, 4233, 4234, 4237, 4241, 4242, 4244, 4245, 4246, 4247, 4248, 4249, 4441, 44511, 44512, 4452, 4453, 4523, 454110, 44611, 447, 481, 482, 484, 4851, 4852, 4853, 4854, 4855, 4859, 491, 492, 493, 51111, 515, 517, 5182, 51913, 521, 52211, 52219, 52212, 52213, 5222, 5223, 523, 5241, 5412, 5415, 5416, 5417, 54194, 5525, 5617, 56173, 562, 616, 6211, 6212, 6213, 6214, 6215, 6216, 6219, 6221, 6222, 6223, 6231, 6232, 6233, 6239, 6241, 6242, 6244, 7211, 722, 8111, 8112, 8113, 8122, 8123, 92111, 92112, 92113, 92114, 92115, 92119, 922, 923, 924, 925, 926, 927, and 928. We exclude the following NAICS codes: 311811, 42491, 44413, 517311, 56173, 62131, 62132, 7224, and 811192. [Colour figure can be viewed at wileyonlinelibrary.com]

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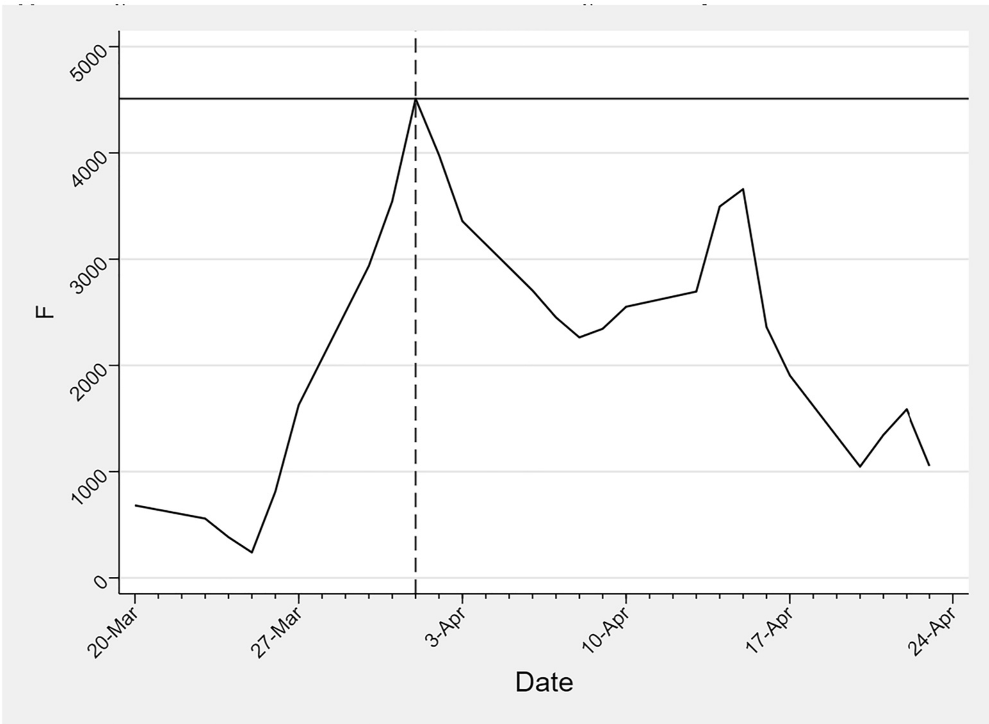
(a)



(b)

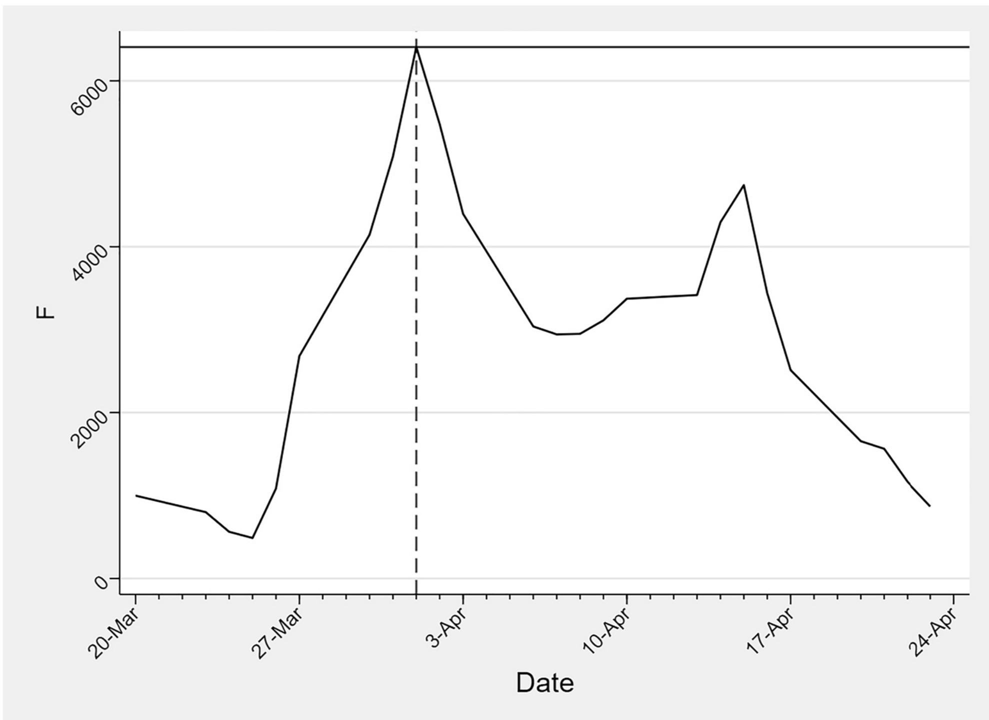


FIGURE A3 (a) *F*-statistics for structural breaks for average hours away from home. (b) *F*-statistics for structural breaks for fraction away from home more than 8 hours. Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Lines are the *F*-statistic from a test for a level- and slope-shift in a linear time trend at the indicated date in models controlling for county fixed-effects, weather, and state policies. *F*-statistic based on covariance matrix that is clustered on county.

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