NBER WORKING PAPER SERIES

EFFECT OF A FEDERAL PAID SICK LEAVE MANDATE ON WORKING AND STAYING AT HOME DURING THE COVID-19 PANDEMIC:EVIDENCE FROM CELLULAR DEVICE DATA

Martin Andersen
Johanna Catherine Maclean
Michael F. Pesko
Kosali I. Simon

Working Paper 27138
http://www.nber.org/papers/w27138

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
May 2020

Authors listed in alphabetical order. All authors contributed equally to the manuscript. Special thanks to SafeGraph, a data company that aggregates anonymized location data from numerous applications in order to provide insights about physical places, for provide the data for this study. We thank Douglas Webber for helpful comments. All errors are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Effect of a Federal Paid Sick Leave Mandate on Working and Staying at Home During the COVID-19 Pandemic: Evidence from Cellular Device Data
Martin Andersen, Johanna Catherine Maclean, Michael F. Pesko, and Kosali I. Simon
NBER Working Paper No. 27138
May 2020, Revised in October 2020
JEL No. H0,I1,J0,K0

ABSTRACT

We study the effects of the temporary federal paid sick leave mandate that became effective April 1st, 2020 on 'social distancing,' as proxied by individuals' physical mobility behavior gleaned from cellular devices. The national paid leave policy was implemented in response to the COVID-19 outbreak and provided many private and public workers with up to two weeks of paid leave for own or family illness or dependent care. We study the impact of this policy using difference-in-differences methods leveraging pre-FFCRA county-level differences in the share of workers likely eligible for FFCRA benefits. We find that FFCRA increased the average number of hours at home, and reduced the share of the individuals likely at work. In particular, comparing the county with the lowest to highest FFCRA exposure, we find that the average daily hours at home per day increased 4.2% while the average hours not at home per day and working decreased by 7.7% and 6.1% post-policy.

Martin Andersen
Department of Economics
University of North Carolina at Greensboro
Bryan, Room 448
Greensboro, NC 27402
msander4@uncg.edu

Johanna Catherine Maclean
Department of Economics
Temple University
Ritter Annex 869
Philadelphia, PA 19122
and NBER
catherine.maclean@temple.edu

Michael F. Pesko
Department of Economics
Andrew Young School of Policy Studies
Georgia State University
PO Box 3992
Atlanta, GA 30302-3992
mpesko@gsu.edu

Kosali I. Simon
O'Neill School of Public and Environmental Affairs
Indiana University
1315 East Tenth Street
Bloomington, IN 47405-1701
and NBER
simonkos@indiana.edu

1. Introduction

As of October 2nd, 2020 there were over 34 million confirmed global cases of the novel coronavirus 2019 (COVID-19) and more than one million deaths (World Health Organization 2020b). The United States, the focus of our study, accounts for 21% of confirmed cases and 20% 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 before displaying symptoms (e.g., dry cough and fever), thus increasing the importance of strategies to enable sick workers to remain at home.

Currently, there is no cure or vaccine for COVID-19.[1] Thus, public health measures are therefore the primary means to mitigate disease spread. The World Health Organization suggests that individuals infected with or exposed to COVID-19 self-isolate for 14 days, and all people (symptomatic and non-symptomatic) practice social distancing. A study using Israeli data collected in the lead-up to the COVID-19 outbreak shows that 97% of adults report that they would comply with a government mandate to self-quarantine if their wages were compensated, but compliance falls to 57% without compensation (Bodas and Peleg 2020), suggesting the importance of financial protection for effective containment of COVID-19.

The U.S. does not have a universal, national paid sick leave (PSL) policy. Thus, how effectively the country may be able to advance a meaningful mitigation strategy based on isolation among infected individuals is unclear. Indeed, working while sick is common in the

[1] At the time of writing, there are ongoing clinical trials for potential vaccines and therapies. See, for example, the World Health Organization (2020c) listing of potential vaccines. Early data suggests that the drug Remdesivir may reduce mortality risk and time to recovery (National Institutes of Health 2020; Beigel et al. 2020). On May 1st, 2020, the Food and Drug Agency approved through an emergency use authorization Remdesivir to treat hospitalized COVID-19 patients with severe disease and on August 28th, 2020 the Agency expanded this approval to include all hospitalized adult and pediatric patients, regardless of disease severity (Food and Drug Administration 2020).

U.S.: pre-COVID-19 survey data suggest that 90% of workers report coming to work while sick (Accountemps 2019), possibly due to fear of income or job loss.

In response to the surge in confirmed COVID-19 cases and deaths, the U.S. federal government adopted a *temporary* national PSL policy: the Families First Coronavirus Response Act (FFCRA) on March 18th, 2020 (116th Congress of the United States 2020). This Act, which became effective April 1st, 2020 and will sunset at the end of 2020, compels many private and public employers to offer up to two weeks of temporary emergency sick leave to workers for COVID-19-related treatment, isolation, childcare due to school/daycare closures, or care for dependents impacted by COVID-19. The objective of this Act is to provide financial support to stay home for those with COVID-19 or caring for children/dependents during the pandemic, and ultimately reduce disease spread within the population.

We provide the first evidence on the impact of the federal FFCRA on 'physical mobility' measured using GPS tracking of cellular devices. Specifically, we consider cellular device movements that plausibly reflect the time individuals spend in their homes and at work. We view these variables as proxies for individuals' social distancing and quarantining behaviors, in particular the ability to stay at home from work when sick, watching children whose school or daycare is closed due to COVID-19 reasons, or when caring for a dependent who is sick.[2] We exploit the fact that 'essential workers' – e.g., healthcare workers and those working in food services – are exempt from FFCRA benefits and that the share of such workers varies across U.S. counties. This variation allows us to estimate a modified difference-in-differences style (DD) model that leverages differential treatment 'doses' based variation in the share of the county

[2] In particular, we consider our measures to be proxy measures for social distancing, but not physical distancing, which is the requirement to stay at least six feet apart from those outside your household. Social distancing encompasses more than just physical distancing and one may engage in physical distancing, but not social distancing. In our data, this kind of behavior would be captured as a violation of social distancing.

workforce that is employed in an ‘essential’ job. The intuition of this empirical strategy is that counties with higher shares of *non-essential* workers pre-FFCRA should be *more exposed* to the policy than counties with lower shares. We find that FFCRA increases the average number of hours at home, and reduces the average hours away from home and the share of people likely at work (i.e., away from their home for eight or more hours per day). In particular, post-FFCRA comparing the county with the lowest share of non-essential workers in our data to the county with the highest share of non-essential workers, the average number of hours at home increases (compared to pre-FFCRA values) by 4.2% while the average number of hours away from home decreases by 7.7% and the share of individuals away from home for more than eight hours per day declines by 6.1%. These results are confirmed through event-study comparisons -- the differences between highly exposed counties and counties with limited exposure are none-existent prior to policy, and increase starkly after the policy start date, and through placebo testing against 2019 data and numerous other robustness checks.

The paper is organized as follows. Section 2 briefly outlines the related literatures on PSL mandates and COVID-19. FFCRA is discussed in Section 3. Data, variables, and methods are described in Section 4. Results are listed in Section 5 and Section 6 concludes.

2. Related literature

2.1. PSL mandate effects

Several studies examine the effect of PSL mandates on labor market outcomes. Many of the early studies focus on Europe, where mandates have been in place for longer relative to newer state and local mandates in the U.S. Mandated PSL generosity in Sweden and Italy increases work absences (Henrekson and Persson 2004; Scognamiglio 2019). Puhani and Sonderhof (2010), Ziebarth and Karlsson (2010), and Ziebarth and Karlsson (2014) investigate

German legislation that decreased sick pay from 100% to 80% of wages for two years, and then reinstated wages to 100%. Sick days decreased by 2.4 days during the two-year period in which PSL benefits were less generous (Puhani and Sonderhof 2010), and 6% to 8% more workers reported taking no days off work during this time (Ziebarth and Karlsson 2010). Finally, one study finds that sickness leave payments incentivize the length of sickness absence, with higher wage replacement rates increasing absences (Böckerman, Kanninen, and Suoniemi 2018).

In the U.S., studies find that PSL mandates increase PSL coverage, especially for workers in industries historically lacking such benefits (Maclean, Pichler, and Ziebarth 2020; Callison and Pesko 2020). These mandates do not reduce employment, wages, or non-mandated benefits (Maclean, Pichler, and Ziebarth 2020; Pichler and Ziebarth 2020). However, PSL mandates increase workplace absences overall (Maclean, Pichler, and Ziebarth 2020; Callison and Pesko 2020; Schneider 2020; Colla et al. 2014; Ahn and Yelowitz 2016), and several studies are able to evaluate heterogeneity in which types of workplace absences increase post-mandate. Stearns and White (2018) find that PSL mandates adopted in Connecticut and Washington, DC increase illness-related work absences, but do not increase work absences for non-illness reasons (e.g., childcare). Callison and Pesko (2020) do not find evidence that PSL mandates increase work absences nationally for own illness, but the authors document increases in leave-taking for a broader group of absences including child care problems or other personal/family obligations, and these effects were disproportionately higher for households with children. One possible explanation is that PSL mandates are used to care for a sick child. Additionally, Callison and Pesko (2020) find evidence that PSL mandates reduce presenteeism (i.e., working while sick) by 4.5 percentage points (ppts). A study shows that the Washington state PSL mandate reduces presenteeism by eight ppts for workers in the retail and food service industries (Schneider 2020).

A few studies examine the effect of PSL mandates on measures of health or healthcare utilization. The temporary decrease in the generosity of German PSL mandate reduced hospitalizations and hospital visits but had no effect on self-reported health (Puhani and Sonderhof 2010). Similarly, restoring the PSL mandate generosity had no effect on self-reported health satisfaction (Ziebarth and Karlsson 2014). Pichler and Ziebarth (2017) use high-frequency Google influenza data in the U.S. to show that population-level influenza-like disease rates (as measured by searches related to the illness or its symptoms) decrease after workers gain access to PSL following mandate adoption, suggesting PSL mandates have positive spillover effects by preventing the disease spread. In a follow-up study using administrative data on physician-certified influenza, Pichler, Wen, and Ziebarth (2020) confirm this finding.

2.2. Analyses of COVID-19 and associated policies

There is a rapidly emerging literature evaluating effects of the COVID-19 pandemic, and associated policies on economic and health outcomes. Fully describing this literature is beyond the scope of our study. Instead, we mention a few studies that investigate the effect of the pandemic and legislation this virus has precipitated on social distancing-related behaviors. We also refer readers to an excellent review of COVID-19 studies by Brodeur et al. (2020).

An early study finds that the full lockdown of Wuhan, China (the city in which the virus was first identified) reduced the flows of people into, out of and with Wuhan, thus reducing infections outside of that city (Fang, Wang, and Yang 2020). Another study using Chinese data finds that mandatory, but not voluntary, social distancing is effective in flattening the pandemic curve (Chudik, Pesaran, and Rebucci 2020).

A U.S. study uses aggregate human mobility and location trends published by Google for the month of March 2020 to explore the effect of six different types of orders: statewide stay-at-home order, other stay home orders, non-essential business closure, large gatherings ban, school closure, and restaurant/bar limits (Abouk and Heydari 2020). State-wide stay-at-home orders appear to have the largest effect on reducing mobility.

Sehra et al. (2020) use Google data and demonstrate that reductions in mobility (captured by the movement of cellular devices) are associated with a lower incidence of COVID-19 cases after five, ten, and 15 days. Effects are particularly pronounced in areas that continued to have high rates of mobility to retail establishments and workplaces, and on transit systems. Conversely, localities with higher rates of individuals staying in residential areas experience lower confirmed COVID-19 case growth.

Several studies use SafeGraph data (the same data that we use in our analysis) to assess the impact of policies and area-level demographics on social distancing. Income and high-speed internet predict people's ability to obey social distancing directives (Chiou and Tucker 2020). People living in areas with more Republicans engage in less social distancing behaviors that residents in other areas (Allcott et al. 2020; Andersen 2020). Gupta et al. (2020) estimate that a state or county policy change or informational event each reduces mobility by 2% to 8%, with policies of a more information nature explaining in total up to half of the declines in mobility experienced from early March to early April 2020.

Friedson et al. (2020) show that California's stay-at-home order – the first such policy in the U.S. – was effective in encouraging people to remain in their homes. The policy also reduced COVID-19 cases and deaths, but lead to job losses. Early stay-at-home orders and those adopted in high population-density localities appear to be the most impactful (Dave, Friedson, Matsuzawa, and Sabia 2020). Courtemanche et al. (2020) use administrative data to show that Kentucky's stay-at-home order reduced the number of confirmed cases in that state relative to other Southern and Midwest states. Lyu and Wehby (2020) estimate the effects of state-level mandates that require individuals to wear face coverings or masks when in public; covering the face can reduce disease spread, in particular when social distancing is not feasible. The authors demonstrate that mandates reduce the number of cases by 2.0 ppts three weeks post-mandate.

Several studies examine the impact of large public gatherings on disease spread. There is heterogeneity in COVID-19 effects with some gatherings increasing confirmed cases (Dave, Friedson, McNichols, et al. 2020) and others not leading to substantial changes (Dave, Friedson, Matsuzawa, Sabia, et al. 2020). Public gatherings differ in terms of social distancing and mask-wearing – both among gathering participants and non-participants (i.e., local residents), and along with other factors that may impact disease spread, leaving to heterogeneous effects.

Overall, studies suggest that government policies that target social distancing policies (e.g., staying at home) and mask wearing in public places reduce disease spread. Further, voluntary actions by individuals also play an important role.

3. U.S. paid sick leave and policies

3.1. PSL coverage in the U.S., and state and local paid sick leave mandates

Providing PSL benefits have largely been left to employers in the U.S. In March 2019, 76% of civilian workers had access to PSL through their employer, ranging from 73% among private workers to 91% of government workers (Bureau of Labor Statistics 2020a). The average number of PSL days available to workers was eight days per year in 2019 (Bureau of Labor Statistics 2020b), thus less than the recommended 14 days of self-quarantine recommended following exposure to an individual infected with COVID-19.

These averages conceal substantial heterogeneity in access to PSL (see Appendix Table 1; based on tabulations listed in Bureau of Labor Statistics (2020a)). The PSL coverage rate is

94% among workers in management, business, and financial occupations while the rate is 59% among workers in construction, extraction, farming, fishing, and forestry occupations. 86% of full-time workers have access to PSL and 43% of part time workers have access, and coverage rates are 94% among the highest 10% of wage earners and 31% among the lowest 10% of wage earners. The coverage rate among large employers (500 or more workers) is 91% while the coverage rate is 64% among small employers (50 or fewer workers).[3]

The general pattern that emerges from Appendix Table 1 is that workers in 'good jobs' – i.e., prestigious, full-time, and high wage jobs at larger employers– are more likely to have access to PSL than other workers. As documented by Maclean, Pichler, and Ziebarth (2020), coverage rates are particularly low in the food preparation and serving occupations (25%), and retail trade (53%) and accommodation and food services (27%) industries. Low coverage rates in these segments of the labor market are troubling in the context of disease spread given the substantial face-to-face contact between workers and clients involved in such jobs.

Beginning with San Francisco, California in February 2007, 34 U.S. localities have passed mandates to expand access to PSL among workers, see Appendix Table 2 (A Better Balance 2020).[4] Eleven of the mandates are at the state-level. All PSL policies are employer mandates. While the specifics vary across PSL mandate, in general the mandates to date require workers to work for a specified period of time with the employer before gaining eligibility to the benefit. Most mandates compel private employers to provide approximately seven days of PSL annually, unused days can be rolled over to the next calendar year. There are exemptions to PSL

---

[3] Using data from the National Health Interview Survey (NHIS), Callison and Pesko (2020) find below the mean PSL coverage rates for workers in the agriculture/forestry/fishing, construction, arts/entertainment, and accommodation/food services industries.

[4] Other localities have proposed PSL legislation. For example, on August 31st, 2020 the Governor of Pennsylvania called for that state's General Assembly to pass PSL legislation (https://www.governor.pa.gov/newsroom/gov-wolf-calls-for-paid-sick-and-family-leave-for-workers/; last accessed October 2nd, 2020).

mandates. Small employers are often exempt, and some mandates exclude entire industries. Of note, the benefits conferred by state and local PSL mandates -- up to seven days (A Better Balance 2020) -- are likely not sufficient to allow for effective isolation in the context of COVID-19. Further, many workers may not have worked at their employer long enough to have accrued a meaningful amount of PSL and/or workers may have ‘used up’ PSL pre-pandemic as anticipating the severity of COVID-19 was not likely feasible.

3.2. FFCRA

FFCRA compels certain private employers with less than 500 workers and some public employers to offer temporary paid leave to workers (Federal Resgister 2020).[5] FFCRA applies to the gig economy (e.g., Uber) but exempts many small employers with fewer than 50 workers. Qualifying reasons for PSL include: (i) 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. Early estimates suggest that FFCRA will cover 17% to 47% of U.S. workers (Glynn 2020).

FFCRA provides eligible workers who are unable to work because they are in quarantine or are experiencing COVID-19 symptoms and seeking a diagnosis with two weeks (up to a maximum of 80 hours) 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. Workers who are caring for children whose schools or daycares have closed due to COVID-19 or who are tending to

[5] We are not aware of data on the number of individuals who have taken up FFCRA benefits. We suspect that the lack of data is attributable to the newness of this policy.

dependents with COVID-19 are eligible for two weeks (up to a maximum of 80 hours) of PSL at two-thirds 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 period for FFCRA benefits.

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 (EFMLEA), which predates COVID-19 and extends Title I of the Family and Medical Leave Act, an Act that provides *unpaid* leave to qualifying workers. Such workers are eligible for an additional ten weeks of paid expanded family and medical leave at two-thirds the worker's regular rate of pay if the worker is not able to work due to COVID-19 symptoms and/or must care for a child whose school or daycare is closed, or childcare provider is not available (Department of Labor 2020). Employers must post notices in the workplace such that employees know about the benefit.

Thus, FFCRA is arguably more generous, in terms of covered workers and benefits, than state and local PSLMs described in Section 3.1. However, the federal Act is temporary, is limited to COVID-19 sickness and responsibilities, arguably affects different workers and employers than the PSL mandates, and is implemented during a global pandemic. Thus, the extent to which we can extrapolate from previous PSL work to FFCRA is unclear.

Of particular relevance to our study, 'essential workers' are exempt from receiving FFCRA benefits (Federal Resgister 2020). President Trump, through an executive order, used the Defense Production Act to compel essential workers to continue to work during the pandemic and thus such workers are not eligible for FFCRA benefits. The Department of Labor (DOL) has not explicitly defined an 'essential worker.'[6] Within the FFCRA legislation, the DOL states that

[6] The DOL uses the term 'essential responder.' We use the common colloquial term 'essential worker.'

essential workers are individuals who ‘(1) interact with and aid individuals with physical or mental health issues, including those who are or may be suffering from COVID-19; (2) ensure the welfare and safety of our communities and of our Nation; (3) have specialized training relevant to emergency response; and (4) provide essential services relevant to the American people's health and wellbeing’ (Federal Resgister 2020). The DOL delegates the exact definition of essential workers to states: ‘Therefore, the definition allows for the highest official of a state or territory to identify other categories of emergency responders, as necessary’ (Federal Resgister 2020). As we discuss in Section 4.3, we use a definition of essential workers developed by Blau, Koebe, and Meyerhofer (2020), and leverage differences across counties in the share of the workforce that is likely classified as a *non-essential* worker.[7]

FFCRA is designed to provide paid leave for both workers who become infected with COVID-19 (or have good reason to believe that they may be infected) and workers who must care for dependents impacted by COVID-19 (e.g., children who cannot attend school due to closures and/or who are themselves ill with the virus). We are not able to study the reasons *why* physical mobility – which we observe as a cellular device in a particular location – may change post-FFCRA. Thus, we cannot assess – if we observe changes in device locations – whether individual behaviors are altered by FFCRA. Nonetheless, we contend that any reductions are useful in mitigating COVID-19 spread. On the one hand, if an individual with COVID-19 (or who has good reason to believe she is infected with the virus) is able to stay home from work, this change should directly allow the individual to comply with CDC guidelines and thus reduce spread. Similarly, if an individual is caring for a dependent with COVID-19, that individual is exposed to the virus through the dependent and staying home from work should reduce spread.

[7] We note that employers with less than 50 workers or 500 or more workers are also exempt in from FFCRA.

Finally, allowing a parent/guardian to take time away from work without losing pay can prevent a child whose school or daycare is closed due to COVID-19 from being placed in an alternative care setting (e.g., with a babysitter outside the household); reducing such interactions with 'new people' outside the immediate household is recommend by CDC to minimize the spread of COVID-19. For these reasons, we suspect that reductions in physical mobility that we consider could allow, to varying degrees, the U.S. to establish a more effective COVID-19 response.

4. Data, outcome variables, and methods

4.1. SafeGraph Inc.

We use aggregated, high frequency geolocation data from SafeGraph Inc. (a company that aggregates anonymized location data from numerous cellular applications) covering the period covering March 13th, 2020 through April 30th, 2020 on a daily basis.[8] We exclude earlier days in March given that multiple policies were adopted and information related to COVID-19 became available during this period. March 13th, 2020 was also the date on which President Trump 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. 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, as we show in robustness checking (Section 5.7), our results are not sensitive to alternative study start and end dates.

SafeGraph data cover over 20 million cellular devices and are freely available to researchers. 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.

[8] Please see www.safegraph.com (last accessed October 2nd, 2020).

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 six week period. Because SafeGraph data are based on users of cellular applications who have opted in to location sharing, the number of devices in the sample changes over time.[9] Given our short study period, the above-noted six week requirement, and the kinds 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 Census block group information with fewer than five active devices on a given day. We aggregate the number of active devices in each county, the average time devices remained at home or away,[10] and the fraction of devices that were away for eight or more hours, from census block groups to the relevant county. 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 2020). Appendix Table 2 lists localities with a pre-FFCRA PSL mandate. The study sample includes 2,757 counties and county equivalents out of a total of 3,143 in the country; we do not differentiate between counties and equivalents.[11] We observe each county in each of the 48 days in our study period, thus the sample is balanced, but we exclude weekends as most work and school/daycare activities occur within the standard work week (although as we show in robustness checking in Section 5.7, results are not sensitive to including such days).

[9] Examples of application types include weather and mobile retail applications. As such devices are likely actively transmitting their locations throughout the day if a device is moving.

[10] SafeGraph reports median hours. Given this data reporting structure, we calculate mean time at home and away from binned data using the midpoints of the bins.

[11] One may view counties with a pre-FFCRA PSL mandate as a potential comparison group that could be leveraged in a reverse DD specification (Gruber 1994). However, the pre-FFCRA policies are quite different in terms of the workers covered, conditions for which the benefits can be used, duration of benefits, accrual period, and the fact that state and local PSL mandates are permanent pieces of legislation while FFCRA is a temporary policy which expires at the end of 2020. For these reasons, we are not confident in estimating such a reverse DD specification.

4.2. Outcomes

We consider three physical mobility outcomes. We view these outcomes as proxies for the ability to remain at home while sick and/or caring for dependents during COVID-19, all of which should help mitigate the spread of the disease. While ideally we would specifically measure time at work for individual workers, such data are not available at the high-frequency level, and in a very recent time period, required for our empirical strategy (see Section 4.3).

The measures we consider are based on movement of *cellular devices* within U.S. counties and may therefore not fully reflect physical mobility patterns of *individuals*.[12] We measure the average number of hours per day that the cellular device spends (i) at home and (ii) not at home in each county. We also examine the share of devices that are not at home more than eight hours per day; we select eight 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.[13] Thus, while we use terms such as 'individual' when discussing our results, we note that we are in fact tracking cellular devices which are, presumably, carried by an individual.

We note that our variables that relate to time away from home do not necessarily isolate time at work and such time could be used for other activities conducted away from home (e.g., healthcare needs, shopping). We suspect that, after conditioning on social distancing policies and other controls (see Section 4.3), we are able to, at least partially, net out other behaviors.

[12] For example, if an individual does not take their cellular device with them when they left their home for work, then we will not capture this working behavior and instead we would, erroneously code this individual as at home. We cannot envision any reason why the propensity to carry a cellular device, vs. leaving the device at home, should be correlated with FFCRA implementation over our relatively short study period. We note that in an earlier version of this manuscript we used alternative proxies. Previously SafeGraph constructed a measure they deemed 'time at work.' However, this measure is now viewed by SafeGraph as unreliable due to an error in how visit duration (time spent at one location) is calculated. Hence, we no longer examine this outcome.

[13] Individuals must be 13 years or older to be included in the SafeGraph sample. Thus, elementary and middle school students are not included in the sample, but high school and college students may be included.

Further, other activities should not be expected to change discretely on the FFCRA effective date. Our measure that captures cellular devices that are away from the home more than eight hours per day is constructed to mimic a standard working day. Nonetheless, we acknowledge that our measures may capture other non-work activities that take place away from the SafeGraph-defined home. For example, traveling to the hospital to see a sick child or dependent.

4.3. Methods

To estimate FFCRA effects, we estimate a modified DD-style model (Alpert, Powell, and Pacula 2018; Courtemanche et al. 2017; Powell and Pacula 2020; Finkelstein 2007; Beheshti 2019; Powell, Alpert, and Pacula 2019; Argys et al. 2020; Park and Powell 2020). This model leverages variation in treatment intensity that is attributable to differences in pre-treatment characteristics across counties. The intuition is that we should observe a larger effects of FFCRA, in terms of our physical mobility measures, in counties which at baseline had higher shares of non-essential 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 benefits.

In particular, 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 'non-essential worker' job in the first quarter of 2019 using data from the Bureau of Labor Statistics' Quarterly Census of Employment and Wages (QCEW). The QCEW captures the near universe of establishments in the U.S. An establishment as: 'A single physical location where business is conducted or where services or industrial operations are performed.' A limitation of our approach is that we use establishments rather than workers themselves to proxy non-essential workers.[14] While not an

[14] 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.

ideal proxy, we note that this approach is common in the COVID-19 literature to date (Brodeur et al. 2020). Essential workers are not eligible for FFCRA benefits and counties with greater shares of non-essential 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 provides a high-level description and leaves the final decisions on how to define this construct to states. There is heterogeneity across states (but not across counties within a state) in the effective definition. In our main analysis, we follow the definition outlined by Blau, Koebe, and Meyerhofer (2020), although our results are robust to using an alternative definition.[15]

The regression model for our modified DD-style model is outlined in Equation (1):

(1) $$Y_{c,s,t} = \pi_0 + \pi_1 FFCRA_t * Non-essential_{c,s} + P_{s,t}\pi_2 + D_{c,s,t}\pi_3 + \delta_c + \theta_t + \eta_{c,s,t}$$

Where $FFCRA_t$ is the post-FFCRA indicator and $Non-essential_{c,s}$ is the fraction of non-essential worker establishments in county c in state *s* the first quarter of 2019. $P_{s,t}$ is a vector of state-level COVID-19-related policies (public school closures, stay at home orders, non-essential business closures, and prohibition on in-restaurant dining (Raifman 2020)) and $D_{c,s,t}$ is a vector of county-level weather variables,[16] 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.

[15] 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.

[16] Weather variables accessed at https://github.com/jbayham/gridMETr (last accessed October 2nd, 2020).

$Non - essential_{c,s}$ is time-invariant and thus we do not include the main effect as this variable is perfectly collinear with county fixed-effects.

We estimate least squares regression. The data are weighted by the county population. We cluster standard errors at the county level (Bertrand, Duflo, and Mullainathan 2004).

Given that we leverage county-level variation in non-essential workers, discussing the type of worker affected by FFCRA is worthwhile. Blau, Koebe, and Meyerhofer (2020) carefully examine demographics of essential and non-essential workers. Non-essential 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 non-essential and essential workers across broad occupational groupings.

5. Results

5.1. Summary statistics and trends

Table 1 provides summary statistics in the pre-FFCRA period. The average number of hours at home and away from home are 9.3 and 4.6 respectively. We note that these two variables do not sum to 24 hours. The difference is attributable to devices that are either not turned on or not transmitting location data for all 24 hours of the day, and measurement error since SafeGraph reports time at home and time away in ranges.[17] 29.1% of individuals in a county are away from home more than eight hours per day and 65.2% of establishments are non-essential. Pre-FFCRA, 21.4% of county-days are under a public school closure order and 4.7% are under a stay at home order. Similarly, over this time period, in 5.8% and 18.1% of county-

[17] Time at home is reported in buckets of one hour or less, one to six hours, six to 12 hours, 12 to 18 hours, and more than 18 hours. Time away from home is reported in buckets of 20 minutes or less, 21 to 45 minutes, 46 minutes to one hour, one to two hours, two to three hours, three to four hours, four to five hours, five to six hours, six to seven hours, seven to eight hours, eight to nine hours, nine to ten hours, ten to 11 hours, 11 to 12 hours, 12 to 14 hours, 14 to 16 hours, 16 to 18 hours, 18 to 20 hours, 20 to 22 hours, 22 hours or more.

days non-essential business are ordered closed and in-person restaurant dining is prohibited. While there is no other policy that occurred nationwide on April 1st, 2020 that might confound out effects, controlling for state social distancing policies allows us to arguably better isolate FFCRA effects. Weather controls indicate that our data span a relatively dry period, with moderate levels of humidity and typical spring weather.

Figures 1A and 1B report variation in non-essential worker establishments. The share of non-essential worker establishments varies across U.S. counties (Figure 1A). We note that several states have low shares of non-essential workers while others have higher shares. Indeed, the two states that arguably appear to be the most discordant border each other: California and Oregon. Apart from these states, the distribution of non-essential 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 non-essential worker establishments. 33.6% of the establishments in Fresno county California, for example, are non-essential, while in neighboring Mono county, California we classify 85.3% of establishments as non-essential. Figure 1B depicts a histogram of the share of non-essential worker establishments, the distribution is roughly bell-shaped but does display a modest right skew. The range of values is 23.1% to 92.1% non-essential worker establishments. The average share of non-essential worker establishments across quartiles is as follows: 55.4%, 63.8%, 68.0%, and 74.1%. Thus, the 4th quartile has a 33.8% higher share of non-essential worker establishments than the 1st quartile.

Trends in our three outcome variables over our study period are reported in Figures 2A, 2B, and 2C. We report the difference between 2020 and 2019 for each variable (we use comparable dates in each year[18]) and aggregate to the weekly level to smooth out noise inherent in high-frequency data. We report trends for the full sample, counties in the 1st quartile of the non-essential worker establishment distribution, and counties in the 4th quarter of the non-essential worker establishment distribution. The intuition for examining the 1st and 4th quartiles is that our design is based on the premise that counties with higher shares of non-essential worker establishments should be more exposed to FFCRA than those counties with lower shares. We can test this hypothesis by examining unadjusted changes for the 1st and 4th quartiles. Across all three measures we observe a sharp change on April 1st. In particular, the average number of hours a home increase, the average number of hours not at home decrease, and the share of individuals away from home for more than eight hours per day decline. The changes occurring on April 1st are largest for counties in the 4th quartile of the non-essential worker establishment distribution, this pattern of results offers suggestive evidence in support of our research design: FFCRA effects are largest in the most exposed counties.

Appendix Figures 1A and 1B report trends over time in the U.S. in confirmed COVID-19 cases and deaths. These figures display several patterns related to our study. First, both confirmed cases and deaths are increasing over time. Second, our study period (March 13th, 2020 to April 30th, 2020) falls within a relatively early period of the pandemic. Thus, our findings are likely local to this period when, while both outcomes were rising rapidly, total confirmed cases and deaths were much lower than they are at the time of writing.

5.2. Effect of FFCRA on physical mobility

[18] We construct comparable dates using epidemiological weeks—the number of weeks that have elapsed since the first Sunday of a week containing at least four days of a given year—and the day of the week. For example, April 1st, 2020 is the Wednesday of the 13th epidemiological week; the corresponding date in 2019 is April 3rd.

Results based on our baseline specification are reported in Table 2. We observe that for a hypothetical county that moved from having 0% non-essential worker establishments to 100% non-essential worker establishments, average hours at home increased in the post-FFCRA period, while the average hours not at home and the share of devices not at home for more than eight hours per day in the post-FFCRA period decreased. We do not observe such a county in our data and thus transform the coefficient estimate to reflect a pattern that we do observe in real-world U.S. counties (see Figure 1A). In particular, we scale our estimates by 0.689, this reflects the difference in exposure in the county with the lowest (23.1%) and the county with the highest (92.1%) share of non-essential worker establishments in 2019 quarter 1. Using this transformation, we find that FFCRA leads to a 0.44 hour or 26.6 minute increase in average time at home, a 0.33 hour or 19.8 minute decrease in average time away from home, and a 1.6 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 4.2%, 7.7%, and 6.1% changes in the three outcomes respectively. FFCRA has been estimated to affect up to 47% of the U.S. workforce (Glynn 2020) and during our study period the share of Americans with COVID-19 was relatively low (see, for example, Appendix Figures 1A and 1B for confirmed cases and deaths over time), which could contribute – in combination with other factors such as school closures, fear of losing a job, and limited knowledge of a new benefit -- to the relatively small effect sizes that we estimate.

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 non-essential worker establishments and (ii) moving from the 10^{th} (56.1%) to 90^{th} (74.0%) percentile of the non-essential worker establishment distribution (see Figure 1B). We report these alternative transformations for transparency given that there is no standard approach to scaling of which we are aware. These transformations, necessarily, imply smaller changes in outcomes post-FFCRA as we are comparing more similar counties.

During our study period, states were active in implementing social distancing policies. Similar to other studies in the literature (see Section 2.2), we observe that state-level social distancing policies generally increase time at home and decrease time away from home, with some variation in terms of both the magnitude and statistical significance across policies.[19]

5.3. Internal validity

We next probe the robustness of our design to various threats to identification. First, we explore the ability of our data to satisfy parallel trends. Second, we investigate the importance of unobserved confounders. Finally, we conduct falsification exercises to ensure that we are not erroneously capturing the effect of some other policy or factor that was also adopted nationwide on April 1st, 2020; of note we are not aware of any such policy or factor.

Parallel trends: We estimate a modified event-study model to explore the ability of our data to satisfy the parallel trends assumption that is necessary for DD-style models to estimate causal effects. In particular, we interact the county pre-FFCRA share of non-essential worker establishments with indicators for the weeks beginning March 18th, 25th, April 1st, April 8th, April 15th, April 22nd, and April 29th. We select the week of March 18th as the omitted category as it is the earliest period in our sample (Lovenheim 2009). Otherwise, the event-study equation is identical to Equation (1). Results are reported graphically in Figures 3A, 3B, and 3C; note these figures depict the coefficient estimates and are not scaled in any way. Broadly, these figures

[19] We observe no clear pattern of results from interactions between FFCRA and social distancing policies (not reported but available on request), suggesting no conclusive evidence on whether FFCRA enhanced the effectiveness of these policies.

suggest that counties with higher and lower shares of non-essential worker establishments followed similar trends pre-FFCRA. However, beginning on April 1st, 2020, counties with higher shares of non-essential worker establishments experience sharp increases in average hours at home, and decreases in average hours not at home and the share of individuals away from home eight or more hours per day. This pattern of results suggests that our data can satisfy parallel trends and that FFCRA effects are observable precisely at the effective date.

Unobserved confounders: We conduct a test to explore the importance of unobserved confounders. We report results excluding time-varying controls from Equation (1). If results change when we include and exclude the time-varying controls, this pattern of results suggests that unobservable confounders do not drive our findings (Altonji, Elder, and Taber 2005). Our coefficient estimates are not appreciably different with or without controls (Table 3).

Falsification: W conduct two falsification tests to further probe our design. First, we estimate Equation (1) using data from 2019, we us the same dates but one 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 are able to replicate our findings in the earlier year, this pattern of results would call to question the internal validity of our design. Results are listed in Table 4: coefficient estimates are small in magnitude (6.3% to 8.7% of the comparable our main estimates which are reported in Table 2) and are not statistically different from zero.

Second, we randomly re-shuffle our treatment variable across counties and dates, and re-estimate Equation (1) 100 times, thereby generating 100 placebo estimates.[20] We compare our main coefficient estimate to the distribution of placebo estimates, if we are capturing the 'true'

[20] In our re-assignment of the treatment variable, the treatment is assigned to a date.

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 4A, 4B, and 4C, we present a scatter plot (Panel A) and a histogram (Panel B) of the estimates. In all three figures, our main coefficient estimate is an outlier.

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 that occurred nationwide on April 1st, 2020. Thus, we proceed with use of the DD-style estimator.

5.4. Heterogeneity in FFCRA effects across education, race/ethnicity, and industry

While COVID-19 has affected all of the U.S., particular groups have been especially hard-hit., e.g., rates of cases and deaths have been very high among African Americans (Villarosa 2020). Similarly, as documented in Appendix Table 1, workers in less desirable jobs are less likely to have access to PSL through their employer, we therefore expect FFCRA effects to be larger among this group of workers.

To explore hypotheses related to disparate impacts, we interact the FFCRA with the share of the county that does not have college degree, is African American, is other race, is Hispanic, and works in a blue collar occupation[21] using data from the 2014 to 2018 American Community Survey.[22] We de-mean the county-shares for ease of interpretation. Results are reported in Table 5. Broadly, we find limited evidence of heterogeneity in FFCRA effects across counties with different demographic profiles. An exception is that counties with higher shares of non-

[21] We use service; sales; office and administration support; farming, fishing, and forestry; construction and extraction; installation maintenance and repair; production; and transportation and material moving occupations.
[22] Data available through https://nhgis.org/ (last accessed October 2nd, 2020).

college educated groups are more impacted by the policy, the elevated impact in such counties is potentially attributable to lower levels of PSL pre-FFCRA for such individuals.

5.5. Interactions between FFCRA and pre-FFCRA PSL mandates

As discussed in Section 2.1., 34 localities across the country had a PSL mandate in place prior to FFCRA. We exclude counties covered by a pre-FFCRA PSL mandate in our main analysis to allow for clean identification of FFCRA effects. While the benefits conferred by FFCRA are arguably quite different from those made available by the PSL mandates, we hypothesize that localities with a mandate in place prior to FFCRA may be better able to support effective implementation of the federal Act. On the other hand, with access to PSL pre-FFCRA, workers in such counties may have less need for additional PSL through FFCRA. To explore this question, we interact our 'bite' variable with an indicator for a pre-FFCRA PSL mandate.

Prior to exploring heterogeneity in treatment effects across counties with and without a pre-FFCRA PSL mandate, we first include those counties with such a mandate in the sample and re-estimate Equation (1). Results are reported in Table 6 and are not appreciably different from our main findings (Table 2). Coefficient estimates are slightly smaller in size, but the 95% confidence intervals overlap substantially and thus we are reluctant to overstate differences. Table 7 reports results based on the interacted models. The primary variable of interest ($FFCRA_t * Non-essential_{c,s}$) carries coefficient estimates across specifications that are very similar to our main results (Table 2). The coefficient estimate on the interaction between ($FFCRA_t * Non-essential_{c,s}$) and indicator for a pre-FFCRA PSL mandate is not statistically distinguishable from zero in any of the specifications. Collectively, these results do not suggest that counties with a pre-FFCRA mandate are differentially impacted by the federal policy.

5.7. Robustness and extensions

We next conduct a series of robustness checks to ensure that our results are stable, and we examine the impact of FFCRA on confirmed COVID-19 cases. We find that our results do not appreciably change across the different specifications and samples, thus we summarize this analysis and note what we view as particularly important findings.

While FFRCA became effective April 1st, 2020 the DOL did not begin to officially enforce the policy until April 18th, 2020. Thus, between April 1st and April 17th, employers did not face a penalty for non-enforcement. However, to the best of our knowledge there is no evidence that employers did not conform with FFCRA during this period.[23] To explore the empirical importance of enforcement, we include an interaction term between an indicator for the period April 18th to April 30th, 2020; this is the period in which we expect the effect of the policy to be most substantial as FFCRA is in place and DOL is actively enforcing the policy. The main effect coefficient estimates are similar to our core results (Table 2), but the interaction terms with the April 18th to April 30th, 2020 indicator suggest that FFCRA effects increased when the DOL began to enforce the policy. In particular, post-April 18th, 2020 average time at home increases by 33.5 minutes, and average time not at home decreases by 26.2 minutes while the share of individuals not at home more than eight hours a day increases by 2.3 ppts (Appendix Table 3).[24] We also define the FFCRA 'effective' date as April 18th, 2020 (Appendix Table 4). We test whether the signing of FFCRA by President Trump on March 18th, 2020 can be viewed as having an impact, perhaps by sending a signal to employers and workers on the importance of staying home while sick or caring for dependents. To implement this test, we treat March 18th, 2020 as the 'effective date.' Results (reported in Appendix Table 5) do not suggest strong signaling

[23] See, for example, http://www.wbiw.com/2020/05/13/indiana-trucking-company-pays-back-wages-to-worker-denied-paid-sick-leave-while-experiencing-covid-19-and-seeking-diagnosis/ (last accessed October 2nd, 2020).
[24] We take the sum of the main effect and the interaction term coefficient estimate.

effects: coefficient estimates are smaller than those reported in Table 2 and are not statistically distinguishable from zero.

We combine SafeGraph data from March 13th to April 30th in 2020 and the equivalent period from 2019,[25] and conduct an alternative DD model (Appendix Table 6). We do not use variation in non-essential worker establishments in this analysis, instead we conduct a standard DD analysis. In this specification, observations in 2020 comprise the treatment group and observations in 2019 comprise the comparison group; April 1st to April 30th in both years comprise the 'post' period; and observations observed between April 1st, 2020 and April 30th, 2020 comprise the treatment*post indicator. Results were not appreciably different to our main results when using this alternative modelling strategy.

We also apply an interrupted time series analysis (ITSA) to study FFCRA effects (Appendix Table 7). Broadly, ITSA parametrically constructs a counterfactual trend for the U.S. had FFCRA not been adopted, then compares the actual and counterfactual trends to estimate treatment effects. The ITSA results suggest that FFCRA effects lead to a discrete change in social distancing outcomes and that effects may decline over time. For example, at the time of policy adoption, average time away from home decreases by 0.51 hours (30.4 minutes) and this change dissipates by 0.8 hours (5.0 minutes) per week day.[26] We have added a control for the enforcement period in the ITSA model (results available on request). Findings, similar to results listed in Appendix Table 3, suggest that effects are enhanced when the DOL enforces the policy, but after some time to begin to decline. Thus, we do not interpret our findings from our ITSA specification to be discordant with results reported in Appendix Table 3.

---

[25] 2019 data cover the period from March 15th to May 2nd.

[26] In an earlier version of this manuscript we emphasized the ITSA findings. Based on feedback from helpful readers, we have elected to focus on the DD-style model in the current version.

We add controls for the first confirmed COVID-19 case and death in the state and county, these variables may convey new information regarding the seriousness of the pandemic to Americans (Appendix Table 8). We also interact our treatment variable with an indicator for the first confirmed case per 10,000 county residents (Appendix Table 9). The main coefficient estimates are not appreciably different from our core results (Table 2) and the coefficient estimate on the interaction with cases per 10,000 residents is imprecise. An exception to this pattern is observed in the average hours at home specification: the interaction term coefficient estimate in this case is roughly the same size as the main coefficient estimate, suggesting that part of the effect of FFCRA on staying home accrues once COVID outbreaks occur in an area.

Our results are stable across alternative sample periods. We use a longer pre- and post-FFCRA period (Appendix Table 10): February 1st, 2020 through July 17th, 2020. We also zero in on the effective date by using a shorter post-treatment time period: March 13th, 2020 through April 15th, 2020 (Appendix Table 11). The longer period arguably allows us to explore how effects may vary as time passes and the pandemic proceeds, which is useful because (presumably) as more individuals become sick they are more likely to take advantage of FFCRA benefits and knowledge of the policy among both workers and employers likely increases over time. However, a cost of using the longer study period is that (as noted earlier in the manuscript), many states began re-opening their economies in May, 2020 which could confound effects. On the other hand, while zeroing in on the FFCRA effective date potentially offers the cleanest design (as a shorter study period arguably allows for us to mitigate secular changes in the pandemic in March and April, 2020), we lose the period in which the DOL began to enforce the policy and cannot allow for learning about the policy to occur. We include weekends (Appendix Table 12), we exclude these days in our main analysis as work and school/daycare responsibilities tend to occur during the work-week for most individuals. Nonetheless, our findings are stable – although we note not identical – across alternative study periods.

As noted earlier in the manuscript, to date the DOL has not provided a specific definition of essential workers. In our main analysis, we rely on a definition developed by Blau, Koebe, and Meyerhofer (2020). Next, we use a definition proposed by Tomer and Kane (2020), and re-estimate Equation (1). Results, reported in Appendix Table 13, are not appreciably different.

We cluster standard errors at the day level (Appendix Table 14) and use heteroscedasticity robust standard errors (Appendix Table 15). We also estimate unweighted regression (Appendix Table 16). In our main analysis, we emphasize variation in treatment effects generated by non-essential worker shares pre-FFCRA. Another dimension of heterogeneity in exposure to the policy is employer size: employers with 50 or less or 500 or more employers are exempt (Glynn 2020). Next, we use County Business Patterns (CBP) data from the U.S. Census (which covers the week of March 12th). The most recent data from the CBP is 2018.[27] We construct the share of establishments exempt based on size in each county and interact that share with our non-essential worker share variable. We report results in Appendix Table 17. Results are very similar.

We next investigate the impact of FFCRA on confirmed COVID-19 cases. To do so, we use data from the Johns Hopkins University Coronavirus Resource Center and construct the logarithm of the number of new cases in the *next* seven days in each county-day of our study period. We use the future seven days that staying at home cannot impact past confirmed cases. Before reporting our main specification, we report an event-study in Appendix Figure 2. Pre-

[27] As noted earlier in footnote 13, employer size is very limited at the county level in the QCEW, hence we do not use this information in our primary specification. The Census Bureau infuses noise into the worker variable – roughly 67% of the observations have ‘medium’ or ‘high’ noise – which leads us to report this analysis as a check rather than in our main specification.

FFCRA counties with different levels of non-essential worker establishments appear to have followed the same trend in confirmed cases. Appendix Table 18 reports results based on Equation (1). We observe a decline of 59.6%[28] in the next week's new number of cases post-FFCRA (comparing the counties with the lowest and highest shares of non-essential workers in our sample). While our coefficient estimate implies a large effect size, our confidence intervals are somewhat wide and the upper tail of our 95% confidence interval implies a 27.8% decrease.

6. Discussion

On January 30th, 2020 the World Health Organization (WHO) declared the coronavirus 2019 (COVID-19) outbreak a Public Health Emergency of International Concern (PHEIC) and on March 11th, 2020 the organization officially declared it a global pandemic (World Health Organization 2020a). The COVID-19 pandemic has caused large losses in lives and decreased morbidity, and severely and adversely affected labor markets. In addition, the pandemic has reignited discussions of perceived inadequacies in U.S. social policy, including a lack of a national, universal PSL mandate (Cain Miller 2020).

We offer the first evidence on the impact of FFCRA on physical mobility (presence at home and at work), a proxy for social distancing which is a key component in CDC guidelines for mitigating the pandemic through public health efforts as, to date, there is no established medical treatment or vaccine. By providing many public and private sector workers up to two weeks of PSL to those who are sick (whether confirmed as COVID-19 infected or not) and/or must care for children who cannot attend school or daycare due to pandemic-related closures, or tend to sick family members, FFCRA represents the first national PSL policy in the U.S. Importantly for our study, 'essential' workers are not eligible for FFCRA benefits. This

[28] We calculate this number as follows: percent change = exp(-1.316*0.689)-1.

exemption offers us a plausibly exogenous source of variation. In particular, we use pre-FFCRA variation across U.S. counties in the share of non-essential workers and apply a modified difference-in-differences methods that leverages this variation. The intuition of this empirical approach is that counties with lower shares of non-essential workers should be more exposed to FFCRA and thus experience a larger 'dose' of treatment. 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 DD-style approach to estimate FFCRA effects 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 non-essential workers to the county with the highest share of non-essential workers in the U.S., the average number of hours at home increases (compared to pre-FFCRA values) by 4.2% while the average number of hours away from home decreases by 7.7% and the share of individuals away from the home for more than eight hours per day declines by 6.1%.

Our findings contribute to three policy-relevant literatures. First, our work adds to the literature that explores the impact of PSL mandates in the U.S. Our work complements previous work, which has focused on state and local mandates, by examining an Act that affected the nation. In addition, unlike existing PSL mandates, FFCRA is a temporary Act that is designed to offer immediate, but tailored, support to workers and their families, and society at large, during an unprecedented outbreak of a highly infectious disease. Second, we add to the recent surge in economic research on government responses to infectious disease. A theme in this literature is to study the impact of policies that encourage social distancing. In that spirit, we consider how providing workers with financial support impacts social distancing.

Our study has limitations. Our proxy variables have many shortcomings in terms of reflecting the medically advised social distancing concept we would ideally study (i.e., staying home from work when infected with COVID-19 or having a credible perception of exposure to someone infected with COVID-19). Another important caveat is that we are not able to isolate *why* individuals take leave: to recover from COVID-19 or to care for dependents. Future work from surveys that discern the reasons for staying away from work would enable further understanding regarding the mechanism through which leave policies might affect future COVID-19 cases. However, as we discussed earlier in the manuscript, increases in time at home and decreases in time away from home likely impacted by FFCRA are all likely conducive to better mitigation of COVID-19 spread through social distancing policies established by the CDC.

Despite limitations, we offer crucial timely first evidence on the impact of FFCRA on physical mobility, a proxy for social distancing, a behavior that is critical if the U.S. is to adopt a meaningful public health policy that can mitigate disease spread. Since the aim of this temporary PSL law is to reduce externalities in workplace illness and to reduce caregiver burdens, understanding whether workers responded by decreased time in the workplace and increased time at home is vital first step to assessing the effects of the law.

Table 1. Summary statistics pre-FFCRA

Variable	Mean/proportion
County-level outcomes	
Average time at home	9.286
Average time away from home	4.609
% away from home > 8 hours	0.291
County-level establishments	
Share non-essential worker establishments	0.652
State-level social distancing policies	
Public school closure order	0.214
Stay-at-home order	0.047
Non-essential business closure	0.058
Restaurant dining-in prohibited	0.181
County-level weather controls	
Precipitation (mm)	3.386
Maximum daily relative humidity (%)	84.826
Minimum daily relative humidity (%)	44.736
Surface downwelling solar radiation (W/m^2)	151.699
Maximum daily temperature (°F)	33.908
Minimum daily temperature (°F)	54.747
Mean daily wind speed (miles per hour)	10.054
*N (county * day)*	164841

Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through March 31st, 2020. Data are weighted by the county population. The unit of observation is a county in a day. The average time away from home and the average time at home do not sum to 24 hours for two reasons. (1) Devices are not tracked 24 hours per day by SafeGraph (e.g., devices are not tracked when they are turned off). (2) The hours data are reported by SafeGraph in hourly intervals, not exact minutes/seconds, which leads to measurement error.

Table 2. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Baseline specification

Outcome:	Average hours at home	Average hours not at home	Away from home >8 hours
Post-FFCRA* %non-essential establishments	0.644***	-0.482***	-0.023***
	(0.151)	(0.121)	(0.006)
Range observed in data	0.444	-0.332	-0.016
[1 SD increase]	[0.047]	[-0.035]	[-0.002]
(10th to 90th percentile Δ)	(0.109)	(-0.081)	(-0.004)
State-level social distancing policies			
Public school closure	0.084***	-0.053***	-0.004***
	(0.016)	(0.013)	(0.001)
Stay-at-home order	0.035*	-0.038***	-0.002***
	(0.014)	(0.010)	(0.001)
Non-essential business closure	0.030	-0.015	-0.001*
	(0.017)	(0.012)	(0.001)
Restaurant dining-in prohibited	0.075***	-0.014	0.000
	(0.011)	(0.009)	(0.000)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in the sample	2757	2757	2757

Notes: 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 models 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.

Table 3. 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 at home	Average hours not at home	Away from home >8 hours
Post-FFCRA* % non-essential establishments	0.674^{***}	-0.528^{***}	-0.025^{***}
	(0.161)	(0.125)	(0.006)
Range observed in data	0.464	-0.364	-0.017
[1 SD increase]	[0.049]	[-0.039]	[-0.002]
(10th to 90th percentile Δ)	(0.114)	(-0.089)	(-0.004)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in the sample	2757	2757	2757

Notes: 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 models 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.

Table 4. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Falsification testing using 2019 data

Outcome:	Average hours at home	Average hours not at home	Away from home >8 hours
Post-FFCRA*% non-essential establishments	0.052 (0.092)	0.042 (0.072)	-0.002 (0.005)
Range observed in data	0.036	0.029	-0.001
[1 SD increase]	[0.004]	[0.003]	[-0.000]
(10th to 90th percentile Δ)	(0.009)	(0.007)	(-0.000)
Pre-FFCRA mean	9.272	4.030	0.252
Number of counties in sample	2757	2757	2757

Notes: 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 models 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.

Table 5. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Allow for interactions between education, race, ethnicity, and blue-collar employment share and FFCRA

Outcome:	Average hours at home	Average hours not at home	Away from home >8 hours
Post-FFCRA*% non-essential establishments*non-college educated	4.035*	-5.509***	-0.384***
	(1.983)	(1.623)	(0.095)
Range observed in data	2.780	-3.796	-0.265
[1 SD increase]	[0.294]	[-0.402]	[-0.028]
(10th to 90th percentile Δ)	(0.682)	(-0.931)	(-0.065)
Post-FFCRA*% non-essential establishments*African American	3.685	-1.060	0.026
	(2.836)	(2.125)	(0.117)
Range observed in data	2.539	-0.730	0.018
[1 SD increase]	[0.269]	[-0.077]	[0.002]
(10th to 90th percentile Δ)	(0.623)	(-0.179)	(0.004)
Post-FFCRA*% non-essential establishments*other race	-1.538	-0.212	-0.062
	(1.406)	(1.056)	(0.059)
Range observed in data	-1.060	-0.146	-0.043
[1 SD increase]	[-0.112]	[-0.015]	[-0.005]
(10th to 90th percentile Δ)	(-0.260)	(-0.036)	(-0.011)
Post-FFCRA*% non-essential establishments*Hispanic	-0.574	-0.021	-0.049
	(0.933)	(0.656)	(0.040)
Range observed in data	-0.395	-0.014	-0.034
[1 SD increase]	[-0.042]	[-0.002]	[-0.004]
(10th to 90th percentile Δ)	(-0.097)	(-0.004)	(-0.008)
Post-FFCRA*% non-essential establishments*blue collar workers	-1.538	-0.212	-0.062
	(1.406)	(1.056)	(0.059)
Range observed in data	-1.060	-0.146	-0.043
[1 SD increase]	[-0.112]	[-0.015]	[-0.005]
(10th to 90th percentile Δ)	(-0.260)	(-0.036)	(-0.011)
Social distancing policies			
School closure order	0.070***	-0.043***	-0.003***
	(0.015)	(0.012)	(0.001)
Stay-at-home order	0.053***	-0.038***	-0.002***
	(0.013)	(0.010)	(0.001)
Non-essential business closure	0.019	-0.019	-0.002**
	(0.016)	(0.011)	(0.001)
Restaurant dining-in prohibited	0.074***	-0.016	-0.000
	(0.011)	(0.010)	(0.001)
Pre-FFCRA mean	10.530	4.303	0.263
Number of counties in sample	2756	2756	2756

Notes: 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 models 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. We lose one observation due to missing data. We suppress interactions with the share of blue collar workers in the county and main effects and two-way interactions for ease of viewing. Full results available on request.
***;**;* = statistically different from zero at the 0.1%, 1%, 5% level.

Table 6. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Include counties with a pre-FFCRA PSL mandate

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
FFCRA* non-essential	0.423***	-0.348***	-0.021***
establishments	(0.088)	(0.053)	(0.003)
Range observed in data	0.291	-0.240	-0.014
[1 SD increase]	[0.031]	[-0.025]	[-0.002]
(10th to 90th percentile Δ)	(0.071)	(-0.059)	(-0.003)
State-level social distancing policies			-0.002**
Public school closure	0.027	-0.027*	(0.001)
	(0.018)	(0.013)	-0.001
Stay-at-home order	-0.009	-0.023*	(0.001)
	(0.015)	(0.010)	-0.001*
Non-essential business closure	0.014	-0.010	(0.001)
	(0.017)	(0.011)	0.000
Restaurant dining-in prohibited	0.046**	0.001	(0.001)
	(0.015)	(0.010)	-0.021***
Pre-FFCRA mean	10.692	4.143	0.253
Number of counties in the sample	3104	3104	3104

Notes: 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 models 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.

Table 7. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Allow for interaction between pre-FFCRA PSL mandate with FFCRA

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA*% non-essential establishments	0.654*** (0.155)	-0.487*** (0.123)	-0.025*** (0.006)
Range observed in data	0.451	-0.336	-0.017
[1 SD increase]	[0.048]	[-0.035]	[-0.002]
(10th to 90th percentile Δ)	(0.111)	(-0.082)	(-0.004)
PSLM*Post-FFCRA*% non-essential establishments	-0.341 (0.198)	0.173 (0.142)	0.006 (0.007)
Range observed in data	-0.235	0.119	0.004
[1 SD increase]	[-0.025]	[0.013]	[0.000]
(10th to 90th percentile Δ)	(-0.058)	(0.029)	(0.001)
*PSLM * post-FFCRA period*			
PSLM*post-FFCRA	0.220	-0.127	-0.004
	(0.128)	(0.093)	(0.005)
State-level social distancing policies			
School closure order	0.026	-0.028*	-0.002**
	(0.018)	(0.013)	(0.001)
Stay-at-home order	-0.010	-0.023*	-0.001
	(0.015)	(0.011)	(0.001)
Non-essential business closure	0.017	-0.011	-0.001*
	(0.017)	(0.011)	(0.001)
Restaurant dining-in prohibited	0.045**	-0.001	0.000
	(0.015)	(0.010)	(0.001)
Pre-FFCRA mean	10.692	4.143	0.253
Number of counties in sample	3104	3104	3104

Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The sample includes counties with a pre-FFCRA PSL mandate and is therefore larger than the baseline sample which excludes these counties. See Appendix Table 2. Data are weighted by the county population. The unit of observation is a county in a day. All models 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.

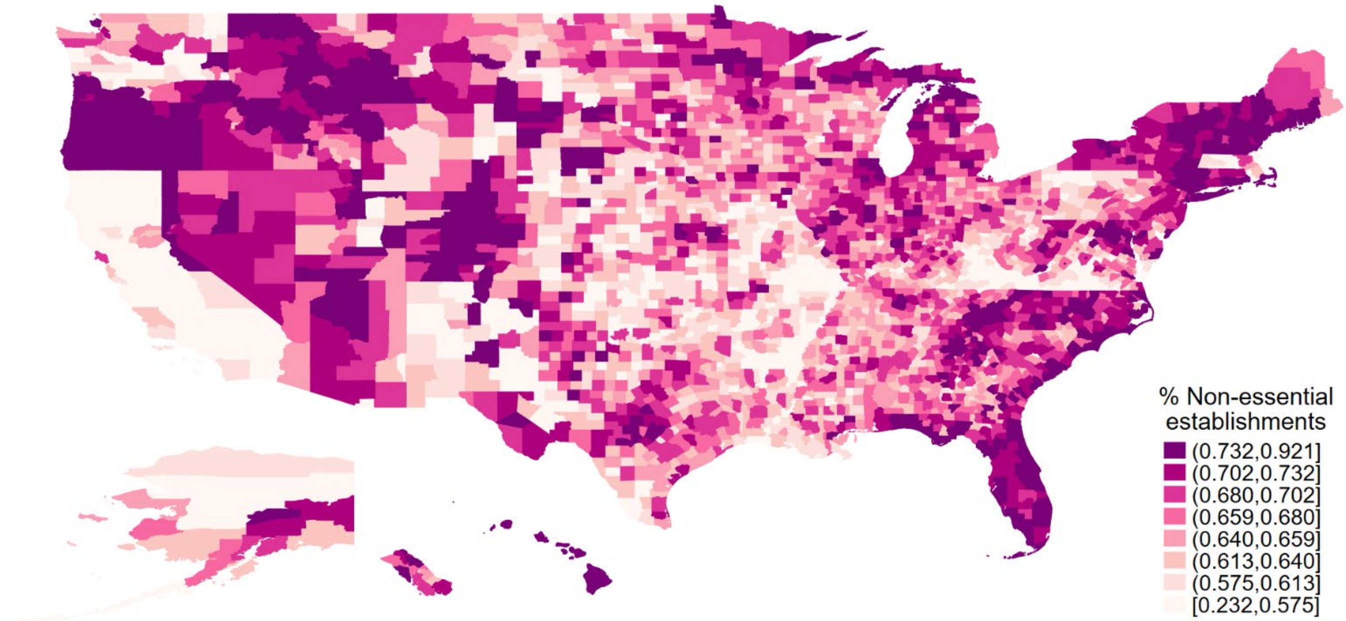
Table 8. Effect of FFCRA on the logarithm of new weekly confirmed cases in the following seven days using a difference-in-differences style model

Outcome:	New confirmed cases
Post-FFCRA* %non-essential establishments	-1.316**
	(0.430)
Range observed in data	0.596┼
[1 SD increase]	[-0.096]┼
(10th to 90th percentile Δ)	(-0.222)┼
State-level social distancing policies	
Public school closure	-0.014
	(0.052)
Stay-at-home order	-0.093*
	(0.044)
Non-essential business closure	0.015
	(0.042)
Restaurant dining-in prohibited	0.025
	(0.039)
Pre-FFCRA mean	-2.269
Number of counties in the sample	2757

Notes: 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 models 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.

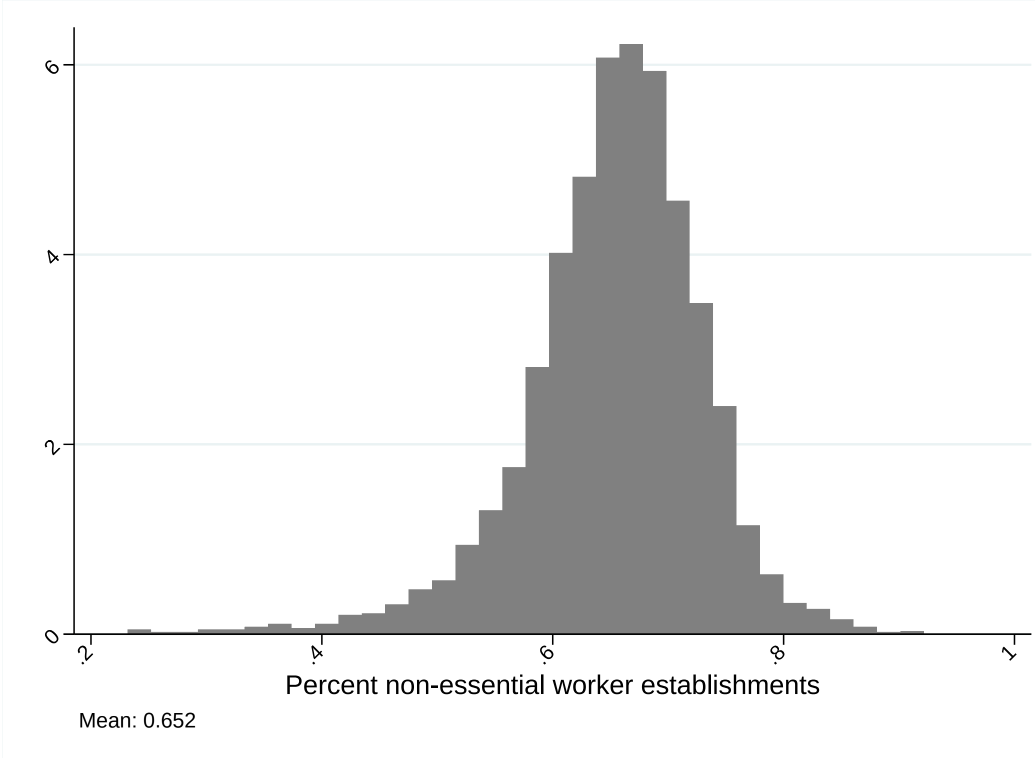
┼ We calculate this number as follows: percent change = $\exp(-\hat{\beta}*0.689)-1$.

Figure 1A. Geographic distribution of non-essential worker establishments across U.S. counties



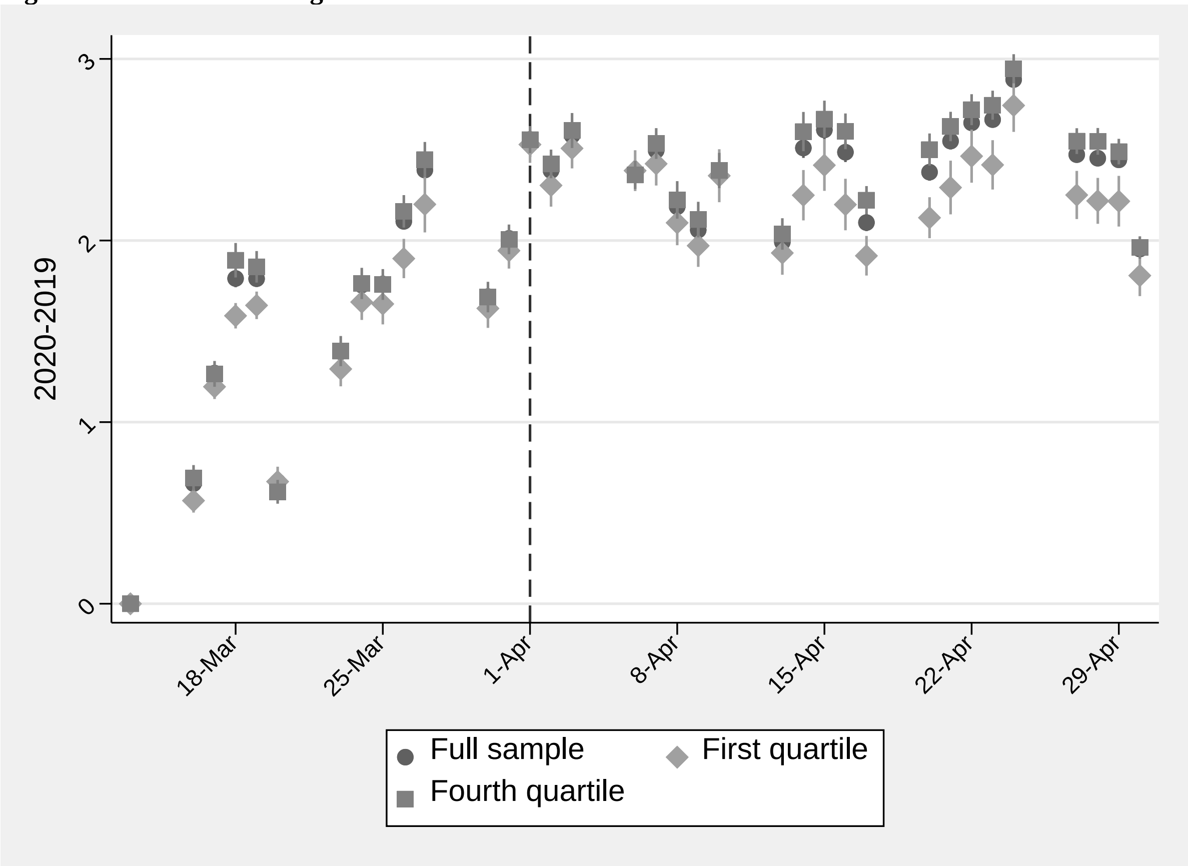
Notes: Data source is Quarterly Census of Employment and Wages 2019.

Figure 1B. Frequency distribution of non-essential worker establishments across U.S. counties



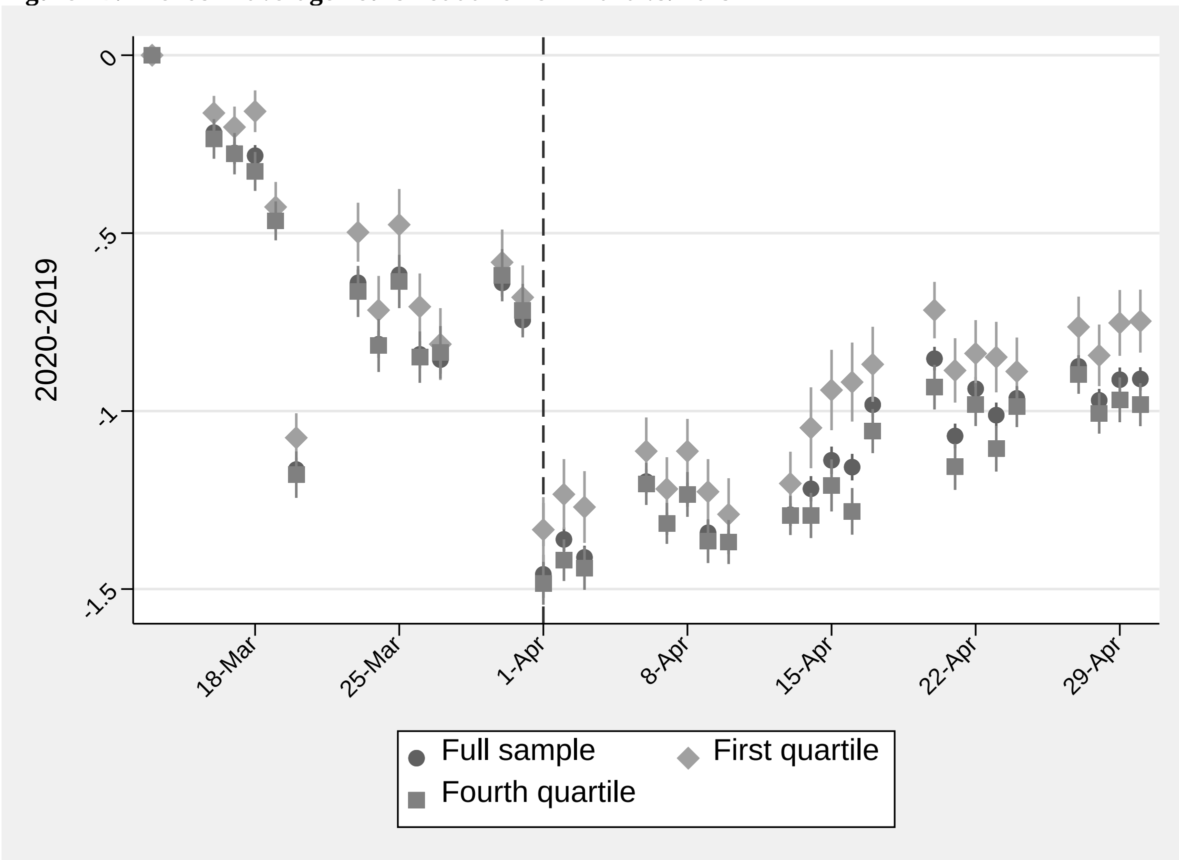
Notes: Data source is Quarterly Census of Employment and Wages 2019 quarter 1.

Figure 2A. Trends in average hours at home in 2020 vs. 2019



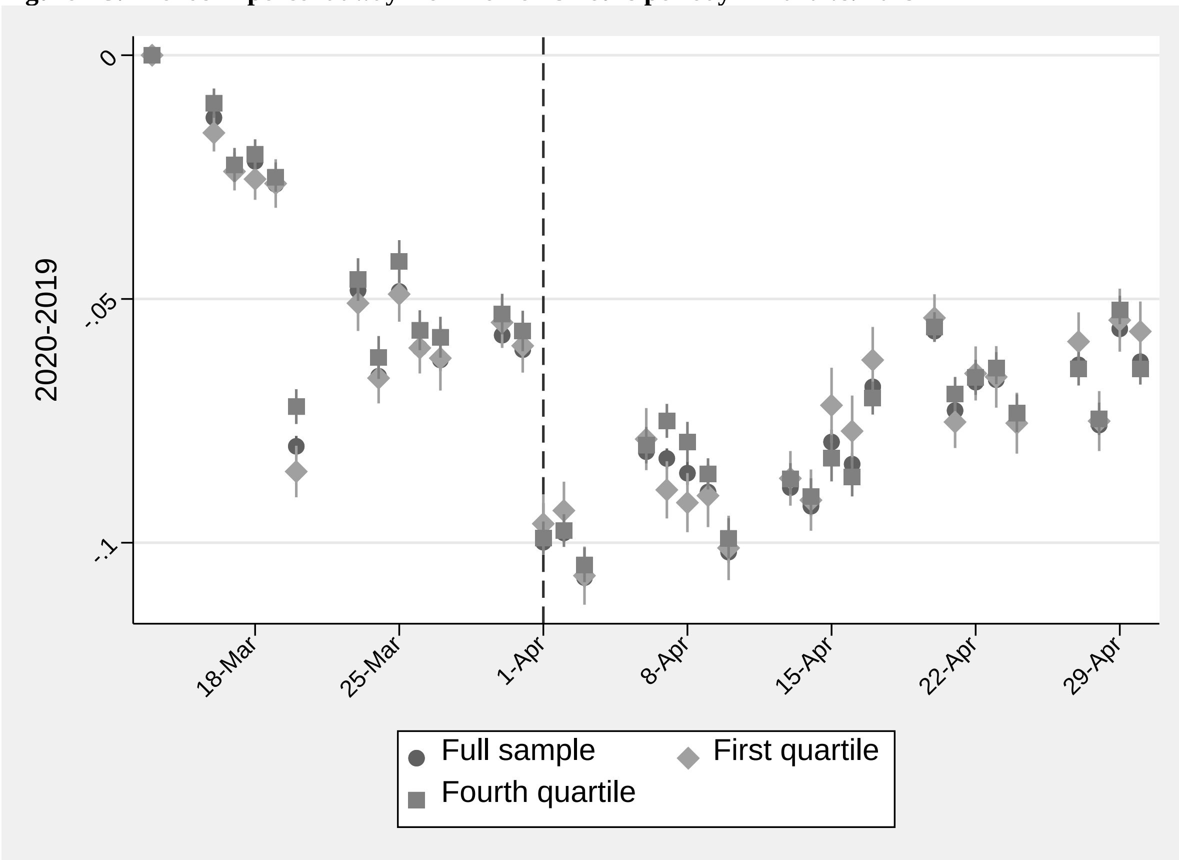
Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a week. Data are demeaned using the grand mean over the full study period. The vertical dashed line indicates April 1, 2020. Quartiles refer to the quartile of the non-essential worker distribution across U.S. counties.

Figure 2B. Trends in average hours not at home in 2020 vs. 2019



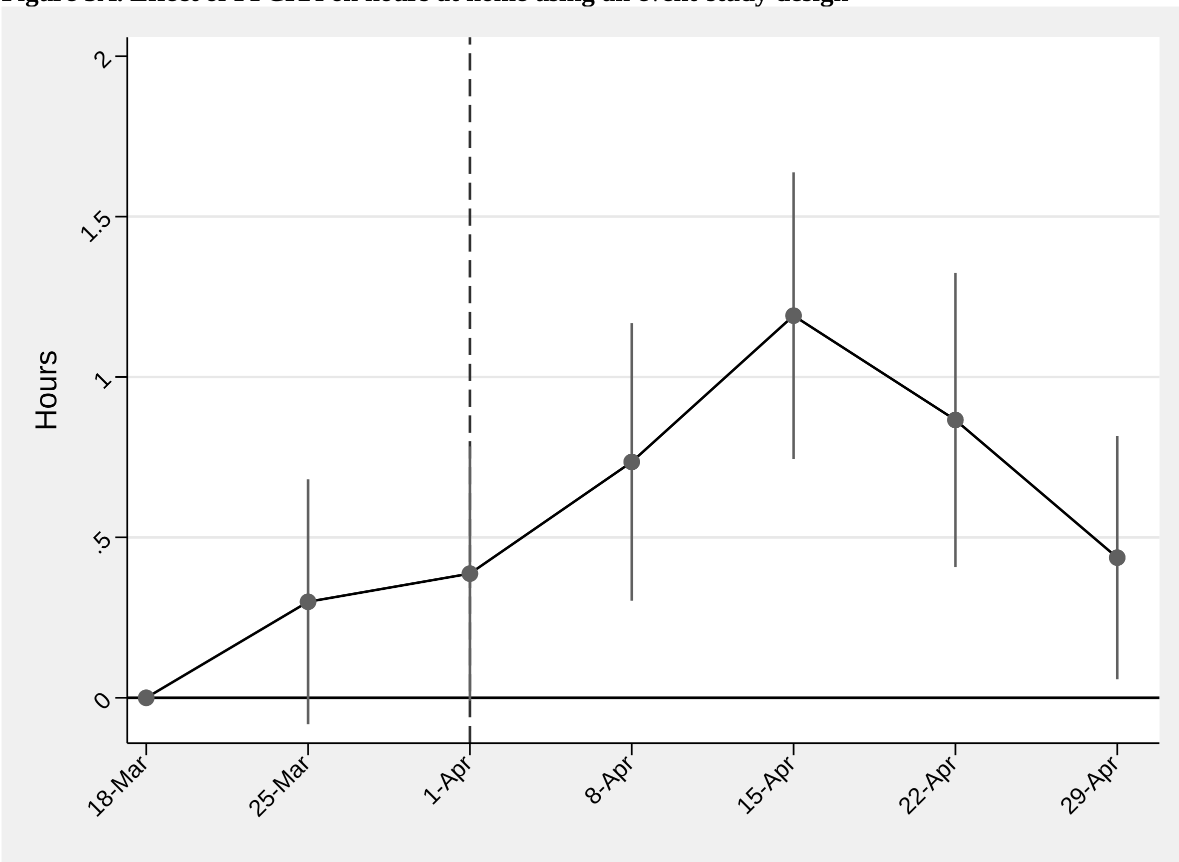
Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a week. Data are demeaned using the grand mean over the full study period. The vertical dashed line indicates April 1, 2020. Quartiles refer to the quartile of the non-essential worker distribution across U.S. counties.

Figure 2C. Trends in percent away from home >8 hours per day in 2020 vs. 2019



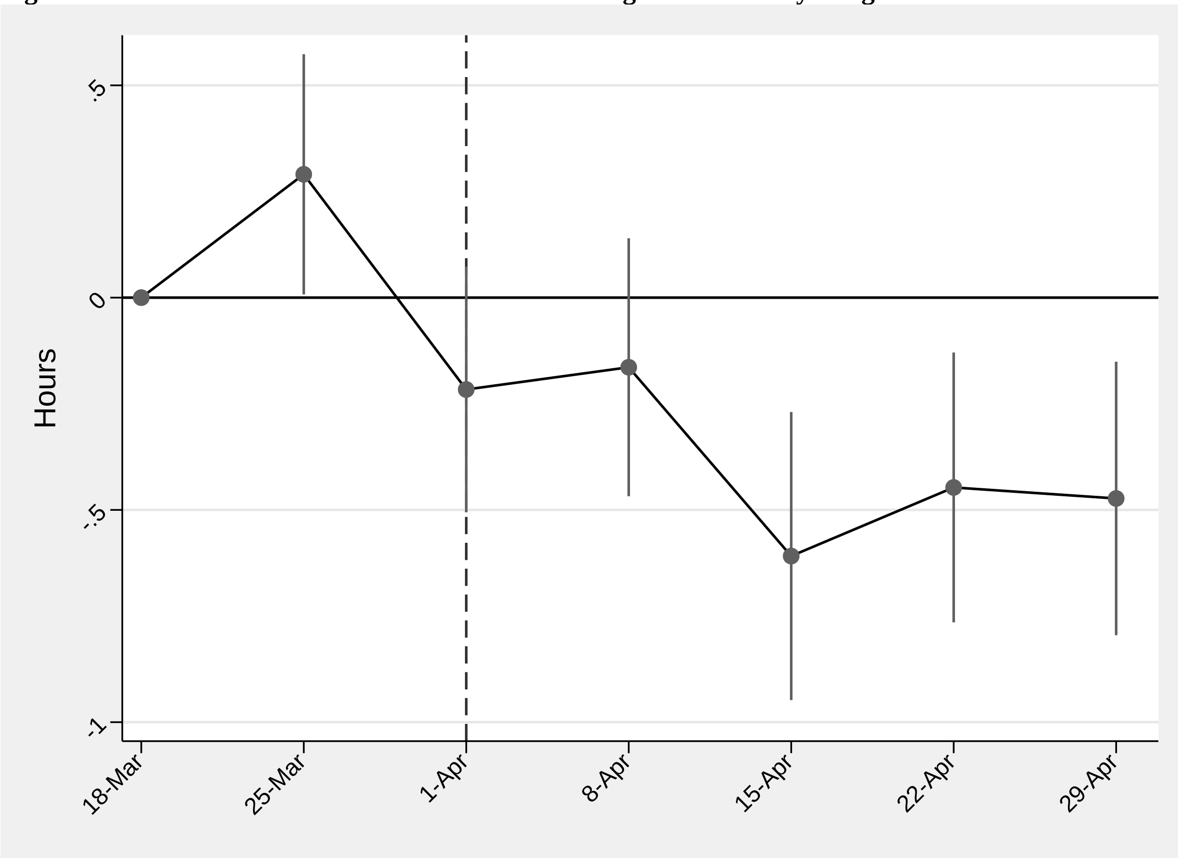
Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a week. Data are demeaned using the grand mean over the full study period. The vertical dashed line indicates April 1, 2020. Quartiles refer to the quartile of the non-essential worker distribution across U.S. counties.

Figure 3A. Effect of FFCRA on hours at home using an event-study design



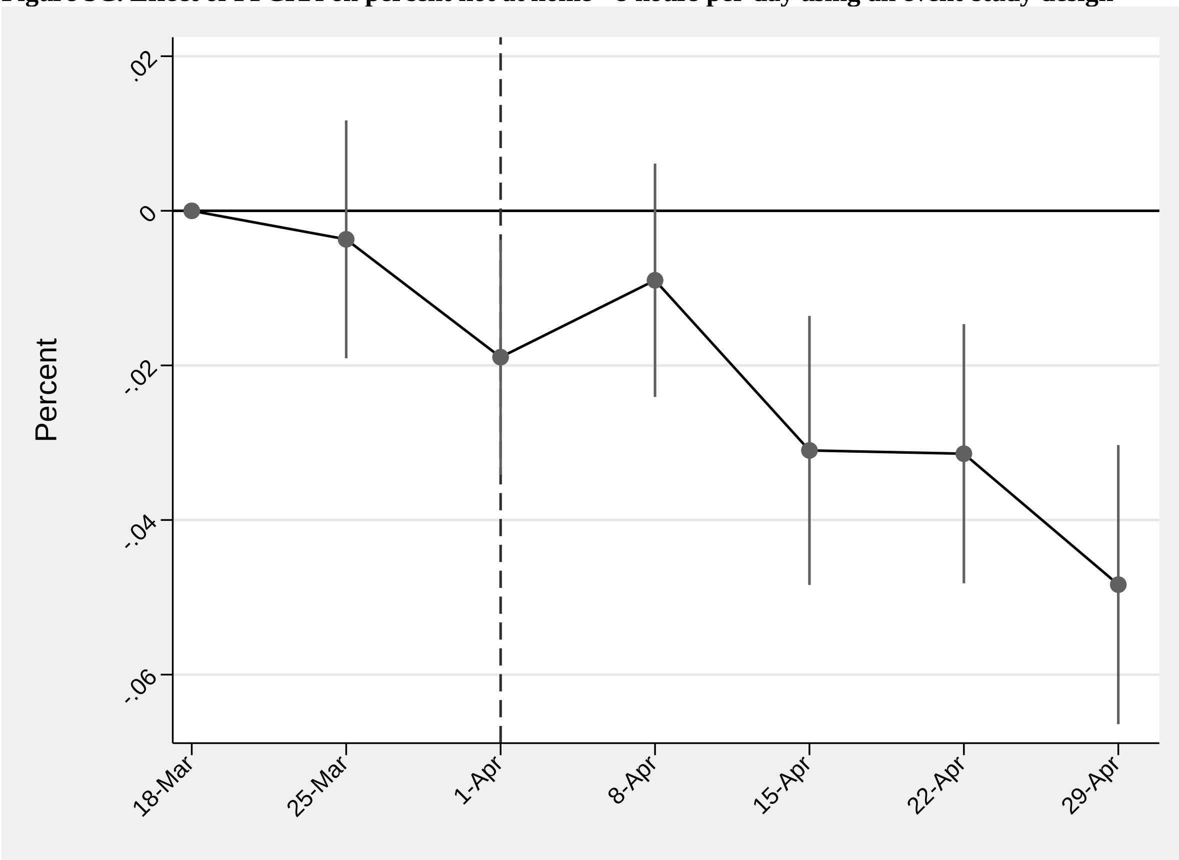
Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All models are estimated with least squares and control for weather covariates, county fixed-effects, and date fixed-effects. Coefficient estimates are reported with black circles. The omitted category is March 13th, 2020 to March 24th, 2020. 95% confidence intervals account for within-county clustering and are reported with vertical lines. The vertical dashed line indicates April 1st, 2020.

Figure 3B. Effect of FFCRA on hours not at home using an event-study design



Notes: 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 models are estimated with least squares and control for weather covariates, county fixed-effects, and date fixed-effects. Coefficient estimates are reported with black circles. The omitted category is March 13th, 2020 to March 24th, 2020. 95% confidence intervals account for within-county clustering and are reported with vertical lines. The vertical dashed line indicates April 1st, 2020.

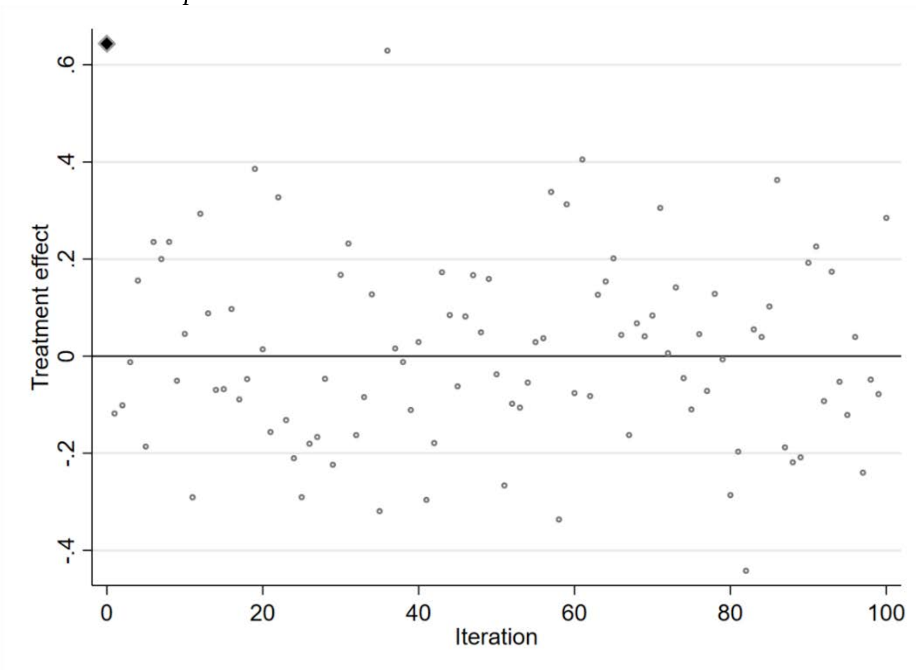
Figure 3C. Effect of FFCRA on percent not at home >8 hours per day using an event-study design



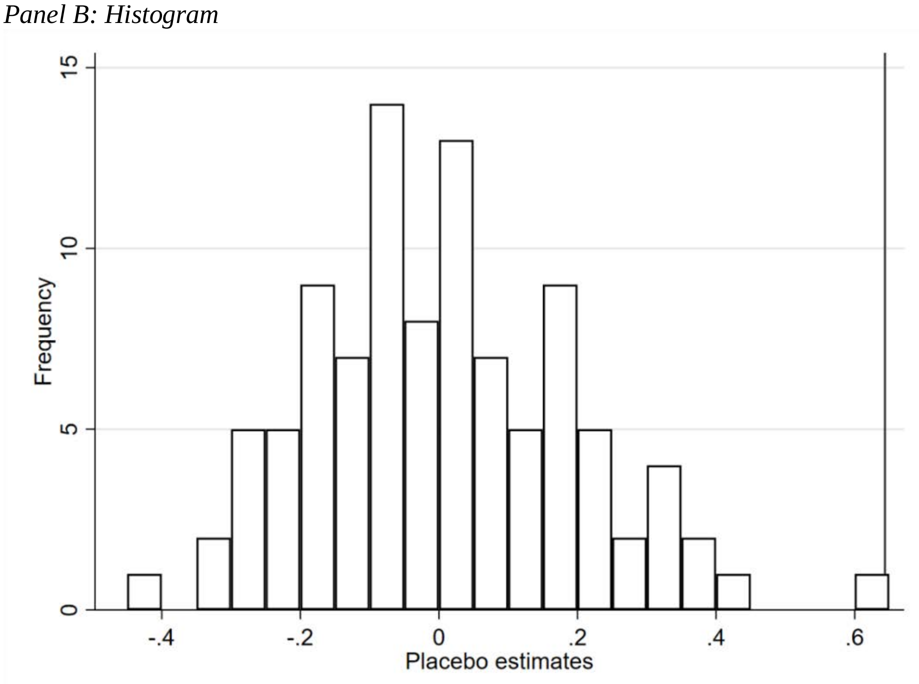
Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All models are estimated with least squares and control for weather covariates, county fixed-effects, and date fixed-effects. Coefficient estimates are reported with black circles. The omitted category is March 13th, 2020 to March 24th, 2020. 95% confidence intervals account for within-county clustering and are reported with vertical lines. The vertical dashed line indicates April 1st, 2020.

Figure 4A. Effect of FFCRA on average hours at home using a difference-in-differences style model: Falsification testing

Panel A: Scatter plot



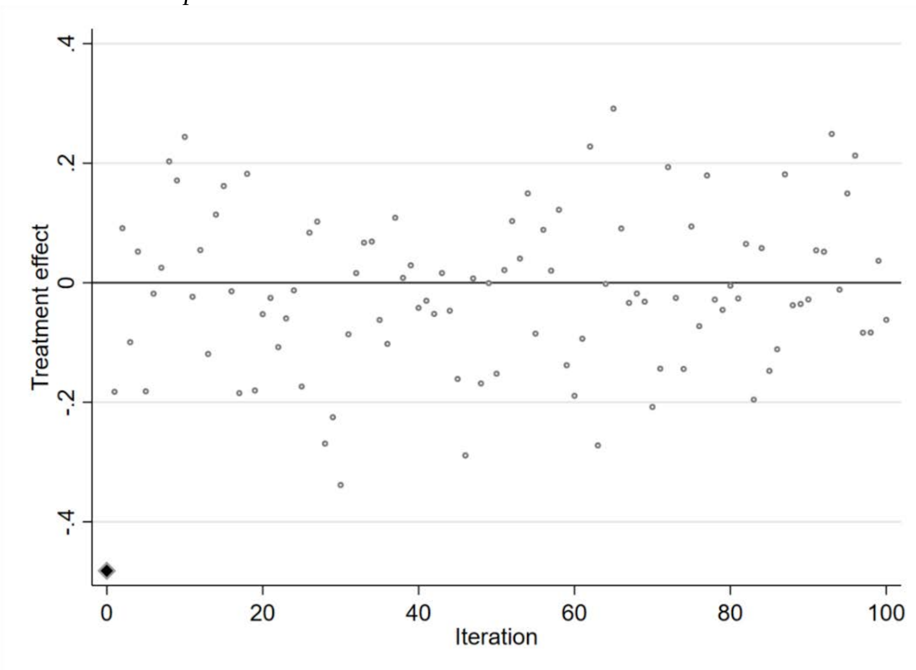
Panel B: Histogram



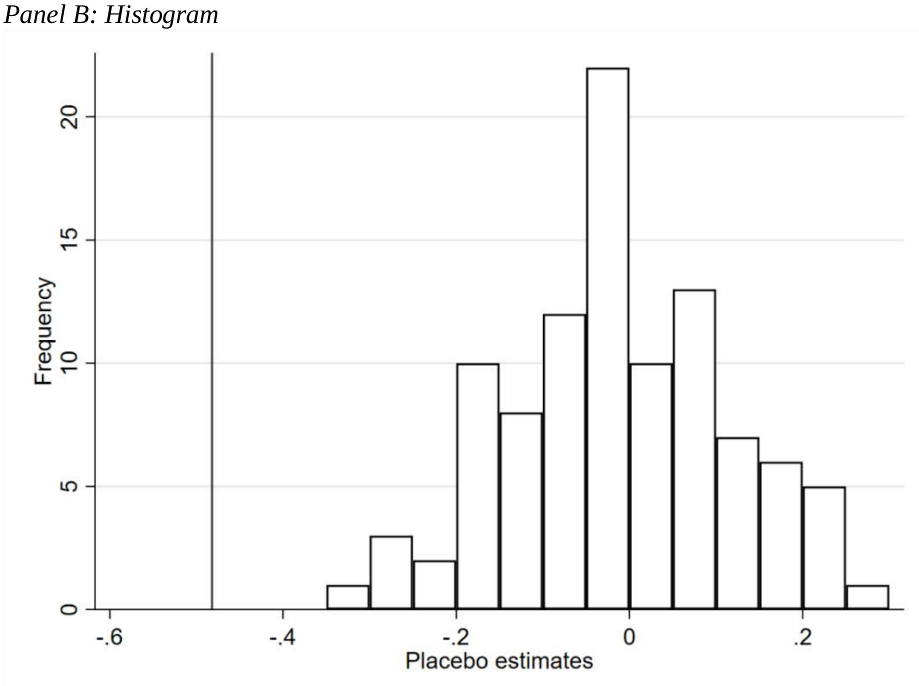
Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a day. All models are estimated with least squares and control for weather covariates, county fixed-effects, and date fixed-effects. 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*% non-essential worker establishments) across counties and dates.

Figure 4B. Effect of FFCRA on average hours not at home using a difference-in-differences style model: Falsification testing

Panel A: Scatter plot



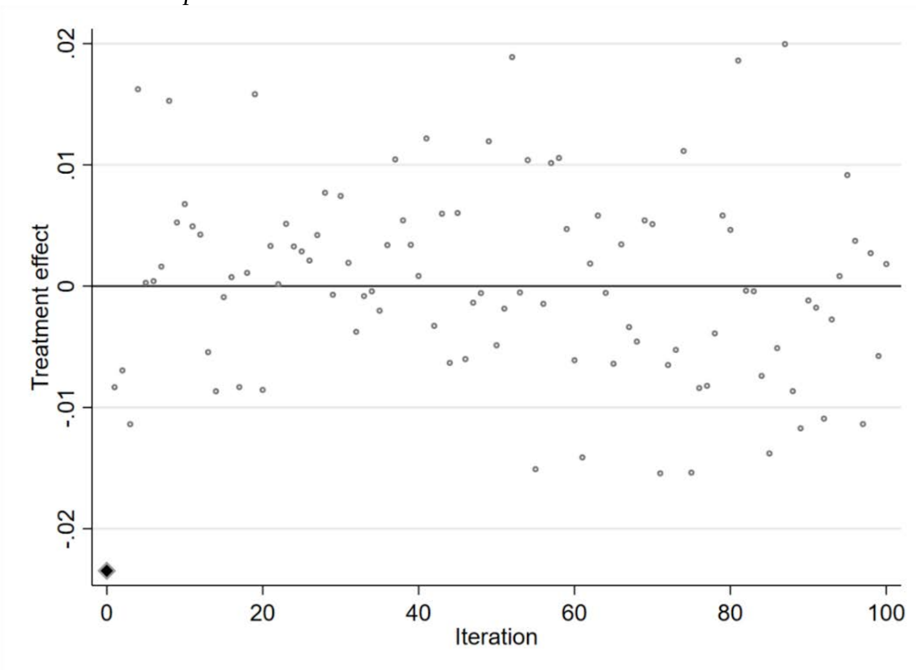
Panel B: Histogram



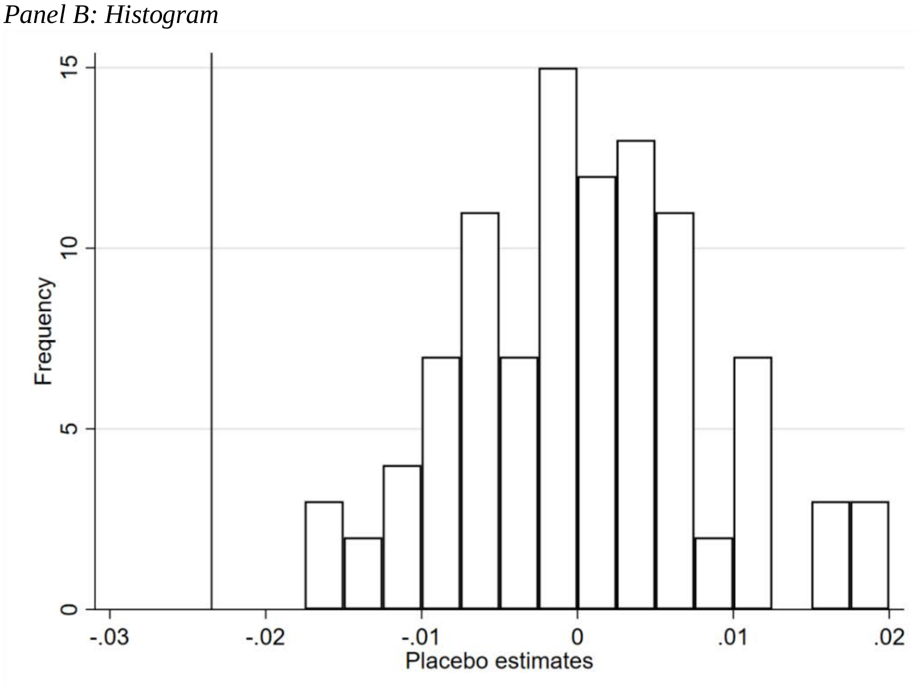
Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a day. All models are estimated with least squares and control for weather covariates, county fixed-effects, and date fixed-effects. 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*% non-essential worker establishments) across counties and dates.

Figure 4C. Effect of FFCRA on share away from home for >8 hours using a difference-in-differences style model: Falsification testing

Panel A: Scatter plot



Panel B: Histogram



Notes: 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 models are estimated with least squares and control for 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*% non-essential worker establishments) across counties and dates.

Appendix Table 1. Access to PSL in the U.S. among civilian workers

Category	Percent with access to PSL
All workers	76
Worker occupation	
Management, professional, and related	91
Management, business, and financial	94
Professional and related	90
Teachers	87
Primary, secondary, and special education school teachers	96
Registered nurses	90
Service	61
Protective service	83
Sales and office	76
Sales and related	64
Office and administrative support	83
Natural resources, construction, and maintenance	68
Construction, extraction, farming, fishing, and forestry	59
Installation, maintenance, and repair	77
Production, transportation, and material moving	70
Production	68
Transportation and material moving	72
Worker job characteristics	
Full-time	86
Part time	43
Union	91
Nonunion	73
Worker wage group	
Lowest 25 percent	51
Lowest 10 percent	31
Second 25 percent	79
Third 25 percent	88
Highest 25 percent	92
Highest 10 percent	94
Employer industry	
Goods-producing industries	72
Service-providing industries	76
Education and health services	87
Educational services	90
Elementary and secondary schools	93
Junior colleges, colleges, and universities	89
Health care and social assistance	85
Hospitals	94
Public administration	92
Employer size (number of workers)	
1 to 99	66
1 to 49	64
50 to 99	71
100 or more	85
100 to 499	81
500 or more	91

Notes: Data source is the 2019 National Compensation Survey, Bureau of Labor Statistics calculations https://www.bls.gov/ncs/ebs/benefits/2019/ownership/civilian/table31a.pdf (last accessed October 2nd, 2020).

Appendix Table 2. Localities with pre-FFCRA PSLM

Type of locality	Specific locality name
States	Arizona
	California
	Connecticut
	Massachusetts
	Maryland
	Michigan
	New Jersey
	Oregon
	Rhode Island
	Vermont
	Washington
Cites and counties	Berkeley, California
	Emeryville, California
	Los Angeles, California
	Oakland, California
	San Diego, California
	San Francisco, California
	Santa Monica, California
	Washington, DC
	Chicago, Illinois
	Cook County, Illinois
	Montgomery County, Maryland
	Duluth, Michigan
	Minneapolis, Minnesota
	Saint Paul, Minnesota
	New York City, New York
	Westchester County, New York
	Philadelphia, Pennsylvania
	Pittsburgh, Pennsylvania
	Seattle, Washington
	Tacoma, Washington
	Austin, Texas
	Dallas, Texas
	San Antonio, Texas

Notes: Data source: A Better Balance (2020).

Appendix Table 3. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Interact an indicator for more than 14 days post-FFCRA with non-essential workers

Outcome:	Average hours at home	Average hours not at home	Away from home >8 hours
Post-FFCRA*% non-essential establishments	0.443** (0.151)	-0.303** (0.116)	-0.013* (0.006)
Range observed in data [1 SD increase] (10th to 90th percentile Δ)	0.305 [0.032] (0.075)	-0.209 [-0.022] (-0.051)	-0.009 [-0.001] (-0.002)
>14 days Post-FFCRA*% non-essential establishments	0.369*** (0.085)	-0.329*** (0.050)	-0.020*** (0.003)
Range observed in data [1 SD increase] (10th to 90th percentile Δ)	0.254 [0.027] (0.062)	-0.227 [-0.024] (-0.056)	-0.014 [-0.001] (-0.003)
State-level social distancing policies			
Public school closure	0.052 (0.038)	-0.019 (0.028)	-0.004*** (0.001)
Stay-at-home order	-0.079 (0.055)	0.010 (0.037)	-0.002*** (0.001)
Non-essential business closure	0.063 (0.059)	-0.066 (0.035)	-0.001* (0.001)
Restaurant dining-in prohibited	0.034 (0.038)	-0.029 (0.027)	0.000 (0.000)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in the sample	2757	2757	2757

Notes: 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 models 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.

Appendix Table 4. Effect of FFCRA on physical mobility outcomes a difference-in-differences style model: Define April 18 2020 as the FFCRA effective date

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA*% non-essential establishments	0.410*** (0.113)	-0.408*** (0.074)	-0.021*** (0.004)
Range observed in data	0.282	-0.281	-0.014
[1 SD increase]	[0.030]	[-0.030]	[-0.002]
(10th to 90th percentile Δ)	(0.069)	(-0.069)	(-0.004)
State-level social distancing policies			
Public school closure	0.081***	-0.051***	-0.003***
	(0.016)	(0.013)	(0.001)
Stay-at-home order	0.040**	-0.042***	-0.002***
	(0.013)	(0.010)	(0.000)
Non-essential business closure	0.027	-0.013	-0.001*
	(0.017)	(0.011)	(0.001)
Restaurant dining-in prohibited	0.072***	-0.012	0.000
	(0.010)	(0.009)	(0.000)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in the sample	2757	2757	2757

Notes: 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 models 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. The Department of Labor did not officially being enforcing FFCRA on April 18th, 2020.
***;**;* = statistically different from zero at the 0.1%, 1%, 5% level.

Appendix Table 5. Effect of FFCRA on physical mobility outcomes a difference-in-differences style model: Define March 18 2020 as the FFCRA effective date

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA*% non-essential establishments	0.398	-0.241	-0.005
	(0.250)	(0.202)	(0.011)
Range observed in data	0.274	-0.166	-0.003
[1 SD increase]	[0.029]	[-0.018]	[-0.000]
(10th to 90th percentile Δ)	(0.067)	(-0.041)	(-0.001)
State-level social distancing policies			
Public school closure	0.080***	-0.050***	-0.003***
	(0.017)	(0.013)	(0.001)
Stay-at-home order	0.040**	-0.042***	-0.002***
	(0.013)	(0.010)	(0.000)
Non-essential business closure	0.026	-0.012	-0.001
	(0.017)	(0.011)	(0.001)
Restaurant dining-in prohibited	0.071***	-0.011	0.000
	(0.010)	(0.009)	(0.000)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in the sample	2757	2757	2757

Notes: 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 models 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. President Trump signed FFCRA on March 18th, 2020.
***;**;* = statistically different from zero at the 0.1%, 1%, 5% level.

Appendix Table 6. Effect of FFCRA on physical mobility outcomes using difference-in-difference from the comparable date in 2019

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA	0.721***	-0.452***	-0.025***
	(0.039)	(0.027)	(0.002)
2020	0.739***	0.536***	0.030***
	(0.048)	(0.025)	(0.001)
State-level social distancing policies			
Public school closure	0.508***	-0.221***	-0.016***
	(0.041)	(0.025)	(0.002)
Stay-at-home order	0.010	-0.024	-0.003
	(0.051)	(0.031)	(0.002)
Non-essential business closure	0.075	-0.064*	-0.008***
	(0.061)	(0.033)	(0.002)
Restaurant dining-in prohibited	0.222***	-0.117***	-0.010***
	(0.039)	(0.025)	(0.002)
Pre-FFCRA mean	10.529	4.303	0.255
Number of counties in sample	2757	2757	2757

Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020 and March 15th, 2019 to May 2nd, 2019; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All models are estimated with least squares and control for weather covariates, county fixed-effects, and epidemiological week-by-day-of-week fixed-effects. Epidemiological week-by-day-of-week fixed-effects control for equivalent dates in 2019 and 2020, so that we compare, for example, the Wednesday of the 13th week of 2020 with the Wednesday of the 13th week of 2019. Standard errors are clustered at the county-level and are reported in parentheses.
***;**;* = statistically different from zero at the 0.1%, 1%, 5% level.

Appendix Table 7. Effect of FFCRA on physical mobility outcomes using an interrupted time-series specification

Outcome:	Average hours at home	Average hours not at home	Away from home >8 hours
Post-FFCRA	0.507***	-0.627***	-0.035***
	(0.011)	(0.011)	(0.001)
Time (relative to April 1, 2020)	0.063***	-0.022***	-0.002***
	(0.002)	(0.001)	(0.000)
Post-FFCRA * Time	-0.083***	0.042***	0.003***
	(0.002)	(0.001)	(0.000)
State-level social distancing policies			
Public school closure	0.299***	-0.182***	-0.013***
	(0.019)	(0.014)	(0.001)
Stay-at-home order	0.159***	-0.021*	-0.001**
	(0.015)	(0.010)	(0.000)
Non-essential business	0.008	0.024*	0.001
closure	(0.020)	(0.012)	(0.001)
Restaurant dining-in	0.195***	-0.024*	-0.002*
prohibited	(0.018)	(0.011)	(0.001)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in the sample	2757	2757	2757

Notes: 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 models are estimated with least squares and control for weather covariates, and county fixed-effects. We do not include date fixed-effects in ITSA regression models as we include a linear time trend instead. Standard errors are clustered at the county-level and are reported in parentheses.

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

Appendix Table 8. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Control for information events as proxied by first confirmed case and death in the county and state

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA*% non-essential establishments	0.626*** (0.156)	-0.475*** (0.127)	-0.024*** (0.006)
Range observed in data	0.431	-0.327	-0.017
[1 SD increase]	[0.046]	[-0.035]	[-0.002]
(10th to 90th percentile Δ)	(0.106)	(-0.080)	(-0.004)
State-level social distancing policies			
School closure order	0.081***	-0.053***	-0.003***
	(0.016)	(0.013)	(0.001)
Stay-at-home order	0.034*	-0.040***	-0.002***
	(0.014)	(0.010)	(0.001)
Non-essential business closure	0.030	-0.009	-0.001
	(0.017)	(0.012)	(0.001)
Restaurant dining-in prohibited	0.077***	-0.016	-0.000
	(0.011)	(0.009)	(0.000)
Confirmed COVID-19 cases and deaths			
At least one case in county	0.007	0.013	-0.000
	(0.015)	(0.012)	(0.001)
At least one death in county	0.035***	-0.010	-0.001
	(0.009)	(0.008)	(0.000)
At least one case in state	-0.223***	0.062	0.006**
	(0.040)	(0.032)	(0.002)
At least one death in state	0.011	-0.043***	-0.002*
	(0.015)	(0.012)	(0.001)
Pre-FFCA mean	10.529	4.303	0.263
Number of counties in sample	2757	2757	2757

Notes: 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 models 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.

Appendix Table 9. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Allowing interactions with confirmed cases

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA*% essential worker establishments	0.416** (0.149)	-0.375** (0.123)	-0.021** (0.007)
Range observed in data	0.287	-0.258	-0.014
[1 SD increase]	[0.030]	[-0.027]	[-0.002]
(10th to 90th percentile Δ)	(0.070)	(-0.063)	(-0.004)
>1 case per 10,000*% essential worker establishments	-0.180 (0.225)	0.317 (0.208)	0.008 (0.013)
Range observed in data	-0.124	0.218	0.006
[1 SD increase]	[-0.013]	[0.023]	[0.001]
(10th to 90th percentile Δ)	(-0.030)	(0.054)	(0.001)
Post-FFCRA*>1 case per 10,000* % essential worker establishments	0.412* (0.193)	-0.325 (0.192)	-0.009 (0.011)
Range observed in data	0.284	-0.224	-0.006
[1 SD increase]	[0.030]	[-0.024]	[-0.001]
(10th to 90th percentile Δ)	(0.070)	(-0.055)	(-0.001)
County confirmed cases main effects and interacted with post-FFCRA period			
>1 case per 10,000	0.021	0.008	-0.000
	(0.012)	(0.012)	(0.001)
Post-FFCRA*>1 case per 10,000	-0.023	-0.011	0.000
	(0.015)	(0.016)	(0.001)
State-level social distancing policies			
School closure order	0.084***	-0.053***	-0.003***
	(0.016)	(0.013)	(0.001)
Stay-at-home order	0.035*	-0.038***	-0.002***
	(0.014)	(0.010)	(0.001)
Non-essential business closure	0.030	-0.016	-0.001*
	(0.017)	(0.012)	(0.001)
Restaurant dining-in prohibited	0.075***	-0.013	0.000
	(0.011)	(0.009)	(0.000)
Pre-period mean	10.529	4.303	0.263
Number of counties in sample	2757	2757	2757

Notes: 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 models 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.

Appendix Table 10. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Use a longer pre- and post-treatment period

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA*% non-essential establishments	1.089***	-1.328***	-0.061***
	(0.234)	(0.205)	(0.013)
Range observed in data	0.750	-0.915	-0.042
[1 SD increase]	[0.079]	[-0.097]	[-0.004]
(10th to 90th percentile Δ)	(0.184)	(-0.225)	(-0.010)
State-level social distancing policies			
School closure order	0.108***	-0.039	-0.002*
	(0.023)	(0.023)	(0.001)
Stay-at-home order	0.074***	-0.039*	-0.002**
	(0.018)	(0.017)	(0.001)
Non-essential business closure	0.081**	-0.061***	-0.006***
	(0.028)	(0.018)	(0.001)
Restaurant dining-in prohibited	0.094***	-0.058***	-0.003***
	(0.013)	(0.012)	(0.001)
Pre-FFCRA mean	9.640	4.653	0.291
Number of counties in sample	2757	2757	2757

Notes: Data source is SafeGraph Social Distancing Metrics files February 1st, 2020 through July 17th, 2020; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All models 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.

Appendix Table 11. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Use a shorter post-treatment period

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA*% non-essential establishments	0.508***	-0.326**	-0.017**
	(0.153)	(0.118)	(0.006)
Range observed in data	0.350	-0.225	-0.012
[1 SD increase]	[0.037]	[-0.024]	[-0.001]
(10th to 90th percentile Δ)	(0.086)	(-0.055)	(-0.003)
State-level social distancing policies			
School closure order	0.087***	-0.060***	-0.003***
	(0.016)	(0.012)	(0.001)
Stay-at-home order	0.048**	-0.042***	-0.002***
	(0.015)	(0.010)	(0.001)
Non-essential business closure	0.019	-0.016	-0.001*
	(0.020)	(0.013)	(0.001)
Restaurant dining-in prohibited	0.099***	-0.011	-0.000
	(0.012)	(0.010)	(0.001)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in sample	2757	2757	2757

Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 15th, 2020; weekends are omitted. Data are weighted by the county population. The unit of observation is a county in a day. All models 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.

Appendix Table 12. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Including weekends

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA*% non-essential establishments	0.710*** (0.151)	-0.582*** (0.120)	-0.027*** (0.006)
Range observed in data	0.489	-0.401	-0.019
[1 SD increase]	[0.052]	[-0.042]	[-0.002]
(10th to 90th percentile Δ)	(0.120)	(-0.098)	(-0.005)
State-level social distancing policies			
School closure order	0.054**	-0.045***	-0.003***
	(0.017)	(0.011)	(0.001)
Stay-at-home order	0.014	-0.031**	-0.002**
	(0.015)	(0.010)	(0.001)
Non-essential business closure	0.012	-0.004	-0.002**
	(0.017)	(0.012)	(0.001)
Restaurant dining-in prohibited	0.057***	-0.003	0.000
	(0.010)	(0.008)	(0.000)
Pre-FFCRA mean	10.608	4.165	0.252
Number of counties in sample	2757	2757	2757

Notes: 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 models 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.

Appendix Table 13. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Use an alternative measure of non-essential establishments

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA* %non-essential establishments	0.798***	-0.782***	-0.042***
	(0.173)	(0.126)	(0.006)
Range observed in data	0.550	-0.539	-0.029
[1 SD increase]	[0.053]	[-0.052]	[-0.003]
(10th to 90th percentile Δ)	(0.130)	(-0.127)	(-0.007)
State-level social distancing policies			
Public school closure	0.085***	-0.055***	-0.004***
	(0.016)	(0.013)	(0.001)
Stay-at-home order	0.039**	-0.041***	-0.002***
	(0.014)	(0.010)	(0.001)
Non-essential business closure	0.025	-0.012	-0.001
	(0.017)	(0.012)	(0.001)
Restaurant dining-in prohibited	0.074***	-0.013	0.000
	(0.011)	(0.009)	(0.000)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in the sample	2757	2757	2757

Notes: 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 models 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. The alternative measure of non-essential establishments is based on the definition of essential workers from Blau, Koebe, and Meyerhoefer (2020).
***;**;* = statistically different from zero at the 0.1%, 1%, 5% level.

Appendix Table 14. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Cluster standard errors at the day level

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA*% non-essential establishments	0.644***	-0.482***	-0.023***
	(0.175)	(0.093)	(0.005)
Range observed in data	0.444	-0.332	-0.016
[1 SD increase]	[0.047]	[-0.035]	[-0.002]
(10th to 90th percentile Δ)	(0.109)	(-0.081)	(-0.004)
State-level social distancing policies			
School closure order	0.084***	-0.053***	-0.004***
	(0.022)	(0.010)	(0.001)
Stay-at-home order	0.035*	-0.038**	-0.002*
	(0.015)	(0.012)	(0.001)
Non-essential business closure	0.030	-0.015	-0.001
	(0.017)	(0.015)	(0.001)
Restaurant dining-in prohibited	0.075***	-0.014	0.000
	(0.018)	(0.013)	(0.001)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in sample	2757	2757	2757

Notes: 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 models are estimated with least squares and control for weather covariates, county fixed-effects, and date fixed-effects. Standard errors are clustered at the day-level and are reported in parentheses.
***;**;* = statistically different from zero at the 0.1%, 1%, 5% level.

Appendix Table 15. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Heteroscedasticity robust standard errors

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA*% non-essential establishments	0.644*** (0.060)	-0.482*** (0.044)	-0.023*** (0.003)
Range observed in data	0.444	-0.332	-0.016
[1 SD increase]	[0.047]	[-0.035]	[-0.002]
(10th to 90th percentile Δ)	(0.109)	(-0.081)	(-0.004)
State-level social distancing policies			
School closure order	0.084***	-0.053***	-0.004***
	(0.011)	(0.008)	(0.000)
Stay-at-home order	0.035***	-0.038***	-0.002***
	(0.007)	(0.005)	(0.000)
Non-essential business closure	0.030***	-0.015**	-0.001***
	(0.008)	(0.005)	(0.000)
Restaurant dining-in prohibited	0.075***	-0.014*	0.000
	(0.008)	(0.006)	(0.000)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in sample	2757	2757	2757

Notes: 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 models are estimated with least squares and control for weather covariates, county fixed-effects, and date fixed-effects. Heteroskedasticity robust standard errors are reported in parentheses.
***;**;* = statistically different from zero at the 0.1%, 1%, 5% level.

Appendix Table 16. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Unweighted regression

Outcome:	Average hours at home	Average hours not at home	Away from home >8 hours
Post-FFCRA* %non-essential establishments	0.634*** (0.125)	-0.521*** (0.090)	-0.020*** (0.005)
Range observed in data	0.437	-0.359	-0.014
[1 SD increase]	[0.046]	[-0.038]	[-0.001]
(10th to 90th percentile Δ)	(0.107)	(-0.088)	(-0.003)
State-level social distancing policies			
Public school closure	0.053***	-0.034***	-0.004***
	(0.015)	(0.010)	(0.001)
Stay-at-home order	0.058***	-0.057***	-0.003***
	(0.011)	(0.009)	(0.001)
Non-essential business closure	0.074*** (0.011)	-0.009 (0.010)	-0.002** (0.001)
Restaurant dining-in prohibited	0.042*** (0.010)	0.008 (0.008)	0.003*** (0.001)
Number of counties in the sample	2757	2757	0.285
Pre-FFCRA mean	9.756	4.534	2757

Notes: 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 unweighted. All models 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.

Appendix Table 17. Effect of FFCRA on physical mobility outcomes using a difference-in-differences style model: Incorporate employer size exemptions

Outcome:	**Average hours at home**	**Average hours not at home**	**Away from home >8 hours**
Post-FFCRA* %non-essential establishments	0.914***	-0.726***	-0.033***
	(0.126)	(0.113)	(0.006)
Range observed in data	0.630	0.500	0.023
[1 SD increase]	[0.075]	[-0.060]	[-0.003]
(10th to 90th percentile Δ)	(0.195)	(-0.155)	(-0.007)
State-level social distancing policies			
Public school closure	0.087***	-0.056***	-0.004***
	(0.017)	(0.013)	(0.001)
Stay-at-home order	0.029*	-0.033**	-0.002**
	(0.013)	(0.010)	(0.001)
Non-essential business closure	0.033	-0.018	-0.001*
	(0.018)	(0.012)	(0.001)
Restaurant dining-in prohibited	0.081***	-0.018*	-0.000
	(0.011)	(0.009)	(0.000)
Pre-FFCRA mean	10.529	4.303	0.263
Number of counties in the sample	2756	2756	2756

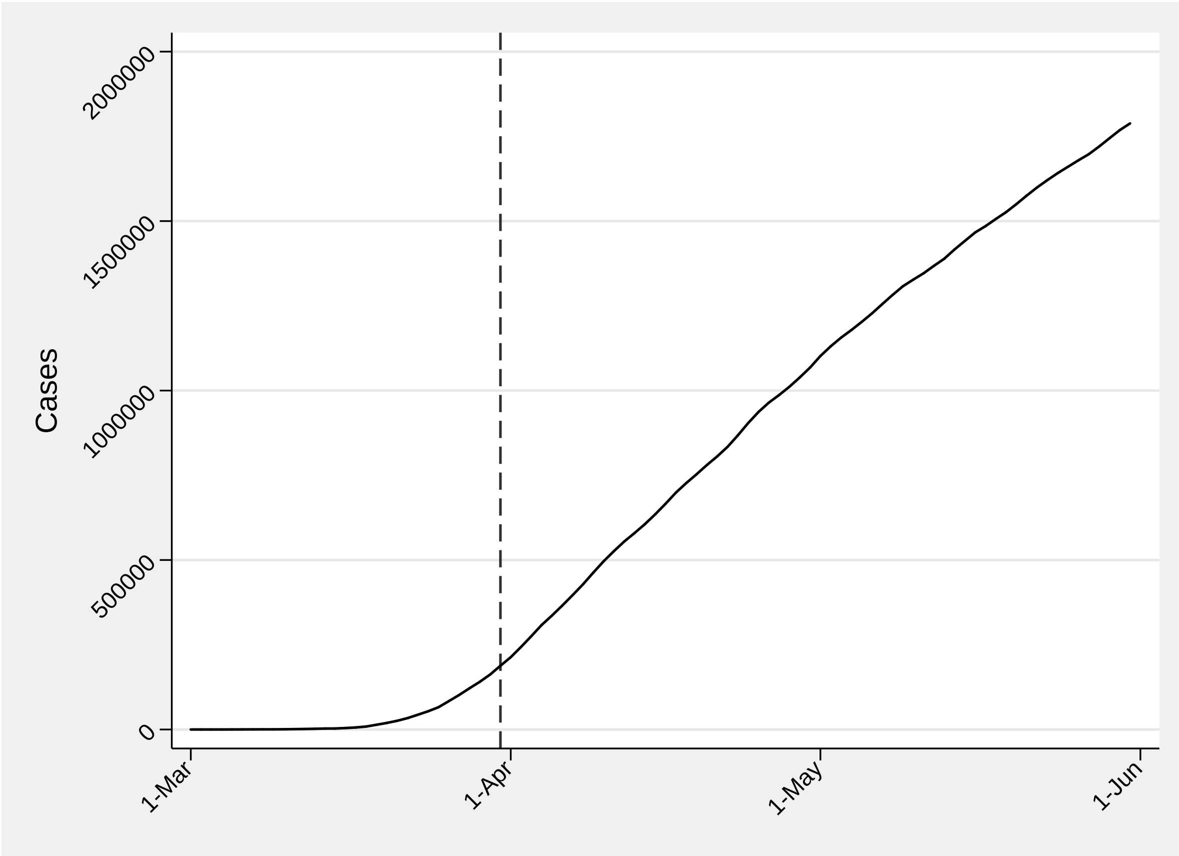
Notes: Data source is SafeGraph Social Distancing Metrics files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a day. Data are weighted by the county population. All models are estimated with least squares and control for weather covariates, county 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.

Appendix Table 18. Effect of FFCRA on the logarithm of new weekly confirmed cases in the following seven days using a difference-in-differences style model

Outcome:	**New confirmed cases**
Post-FFCRA* %non-essential	-1.316**
establishments	(0.430)
Range observed in data	0.596┼
[1 SD increase]	[-0.096]┼
(10th to 90th percentile Δ)	(-0.222)┼
State-level social distancing policies	
Public school closure	-0.014
	(0.052)
Stay-at-home order	-0.093*
	(0.044)
Non-essential business	0.015
closure	(0.042)
Restaurant dining-in	0.025
prohibited	(0.039)
Pre-FFCRA mean	-2.269
Number of counties in the sample	2757

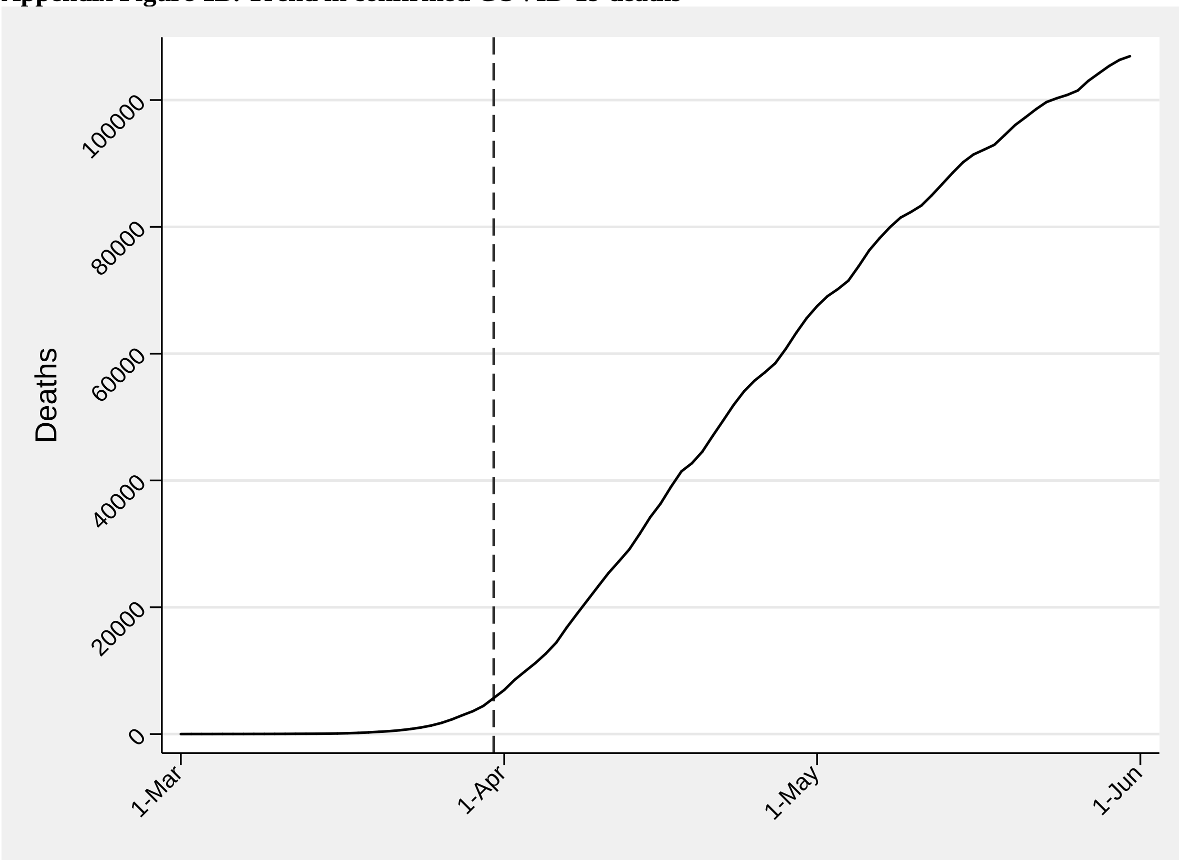
Notes: Data source is Johns Hopkins University Coronavirus Resource Center files March 13th, 2020 through April 30th, 2020; weekends are omitted. The unit of observation is a county in a day. Data are weighted by the county population. All models are estimated with least squares and control for weather 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.
┼ We calculate this number as follows: percent change = exp(-$\hat{\beta}$*0.689)-1.

Appendix Figure 1A. Trend in confirmed COVID-19 cases



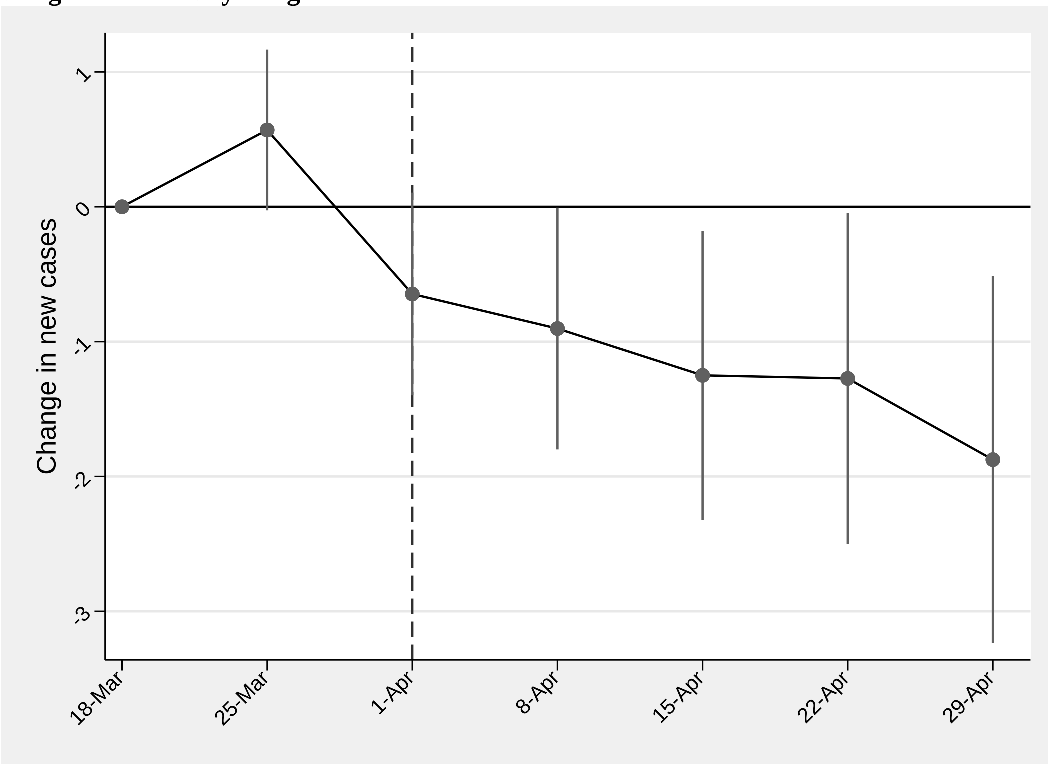
Notes: Data source is Johns Hopkins University Coronavirus Resource Center confirmed COVID-19 cases March 1st, 2020 to June 1st, 2020; weekends omitted. The unit of observation is a county in a day. The vertical dashed line indicates April 1st, 2020.

Appendix Figure 1B. Trend in confirmed COVID-19 deaths



Notes: Data source is Johns Hopkins University Coronavirus Resource Center confirmed COVID-19 deaths March 1st, 2020 to June 1st, 2020; weekends omitted. The unit of observation is a county in a day. The vertical dashed line indicates April 1st, 2020.

Appendix Figure 2. Effect of FFCRA on the logarithm of new confirmed weekly cases in the following week using an event-study design



Notes: Data source is Johns Hopkins University Coronavirus Resource Center 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 models are estimated with least squares and control for weather covariates, county fixed-effects, and date fixed-effects. Coefficient estimates are reported with black circles. The omitted category is March 13th, 2020 to March 24th, 2020. 95% confidence intervals account for within-county clustering and are reported with vertical lines. The vertical dashed line indicates April 1st, 2020.

References:

116th Congress of the United States. 2020. H.R.6201 - families first coronavirus response act.

A Better Balance. 2020. Overview of paid sick time laws in the united states. In *A better balance: The work and family legal center*.

Abouk, R., and B. Heydari. 2020. The immediate effect of covid-19 policies on social distancing behavior in the united states. In *Social Science Research Network Working Paper Series*: Social Science Research Network.

Accountemps. 2019. 9 in 10 employees come to work sick, survey shows. Menlo Park, CA: Robert Half.

Ahn, T., and A. Yelowitz. 2016. Paid sick leave and absenteeism: The first evidence from the us. In *Social Science Research Network Working Paper Series*: Social Science Reserach Network.

Allcott, H., L. Boxell, J. Conway, M. Gentzkow, M. Thaler, and D.Y. Yang. 2020. Polarization and public health: Partisan differences in social distancing during the coronavirus pandemic. In *National Bureau of Economic Resarch Working Paper Series*. Cambridge, MA: National Bureau of Economic Resarch.

Alpert, A., D. Powell, and R. Pacula. 2018. "Supply-side drug policy in the presence of substitutes: Evidence from the introduction of abuse-deterrent opioids." *American Economic Journal-Economic Policy* 10 (4):1-35.

Altonji, J., T. Elder, and C. Taber. 2005. "Selection on observed and unobserved variables: Assessing the effectiveness of catholic schools." *Journal of Political Economy* 113 (1):151-184.

Andersen, M. 2020. "Early evidence on social distancing in response to covid-19 in the united states." *Available at SSRN 3569368*.

Argys, L.M., A. Friedson, M.M. Pitts, and D.S. Tello-Trillo. 2020. "The financial instability cost of shrinking public health insurance." *Journal of Public Economics* Forthcoming.

Beheshti, D. 2019. "Adverse health effects of abuse-deterrent opioids: Evidence from the reformulation of oxycontin." *Health Economics* 28 (12):1449-1461.

Beigel, J.H., K.M. Tomashek, L.E. Dodd, A.K. Mehta, B.S. Zingman, A.C. Kalil, E. Hohmann, H.Y. Chu, A. Luetkemeyer, S. Kline, D. Lopez de Castilla, R.W. Finberg, K. Dierberg, V. Tapson, L. Hsieh, T.F. Patterson, R. Paredes, D.A. Sweeney, W.R. Short, G. Touloumi, D.C. Lye, N. Ohmagari, M.-d. Oh, G.M. Ruiz-Palacios, T. Benfield, G. Fätkenheuer, M.G. Kortepeter, R.L. Atmar, C.B. Creech, J. Lundgren, A.G. Babiker, S. Pett, J.D. Neaton, T.H. Burgess, T. Bonnett, M. Green, M. Makowski, A. Osinusi, S. Nayak, and H.C. Lane. 2020. "Remdesivir for the treatment of covid-19 — preliminary report." *New England Journal of Medicine*.

Bertrand, M., E. Duflo, and S. Mullainathan. 2004. "How much should we trust differences-in-differences estimates?" *Quarterly Journal of Economics* 119 (1):249-275.

Blau, F.D., J. Koebe, and P.A. Meyerhofer. 2020. Essential and frontline workers in the covid-19 crisis. Econofact.

Böckerman, P., O. Kanninen, and I. Suoniemi. 2018. "A kink that makes you sick: The effect of sick pay on absence." *Journal of Applied Econometrics* 33 (4):568-579.

Bodas, M., and K. Peleg. 2020. "Self-isolation compliance in the covid-19 era influenced by compensation: Findings from a recent survey in israel." *Health Affairs* 39 (6):936-941.

Brodeur, A., D.M. Gray, A. Islam, and S. Bhuiyan. 2020. A literature review of the economics of covid-19. In *IZA Discussion Paper Series*. Bonn, Germany: IZA.

Bureau of Labor Statistics. 2020a. Leave benefits: Access, civilian workers. Washtingon, DC: Bureau of Labor Statistics.

———. 2020b. Paid sick leave: What is available to workers? Washington, DC: Bureau of Labor Statistics.

Cain Miller, C. 2020. "Coronavirus brings a new legislative push for paid sick leave." *New York Times*.

Callison, K., and M. Pesko. 2020. "The effect of paid sick leave mandates on access to paid leave and work absences." *Journal of Human Resources* Accepted.

Chiou, L., and C. Tucker. 2020. Social distancing, internet access and inequality. National Bureau of Economic Research.

Chudik, A., M.H. Pesaran, and A. Rebucci. 2020. Voluntary and mandatory social distancing: Evidence on covid-19 exposure rates from chinese provinces and selected countries. In *National Bureau of Economic Research Working Paper Series*. Cambridge, MA: National Bureau of Economic Research.

Colla, C.H., W.H. Dow, A. Dube, and V. Lovell. 2014. "Early effects of the san francisco paid sick leave policy." *American Journal of Public Health* 104 (12):2453-2460.

Courtemanche, C., J. Garuccio, A. Le, J.C. Pinkston, and A. Yelowitz. 2020. Did social-distancing measures in kentucky help to flatten the covid-19 curve? In *Institute for the Study of Free Enterprise Working Paper Series*. Lexington, KY.

Courtemanche, C., J. Marton, B. Ukert, A. Yelowitz, and D. Zapata. 2017. "Early impacts of the affordable care act on health insurance coverage in medicaid expansion and non-expansion states." *Journal of Policy Analysis and Management* 36 (1):178-210.

Dave, D., A. Friedson, D. McNichols, and J.J. Sabia. 2020. The contagion externality of a superspreading event: The sturgis motorcycle rally and covid-19. In *Center for Health Economics & Policy Studies Working Paper Series*. San Diego, CA: Center for Health Economics & Policy Studies.

Dave, D.M., A.I. Friedson, K. Matsuzawa, and J.J. Sabia. 2020. When do shelter-in-place orders fight covid-19 best? Policy heterogeneity across states and adoption time. In *National Bureau of Economic Research Working Paper Series*. Cambridge, MA: National Bureau of Economic Research.

Dave, D.M., A.I. Friedson, K. Matsuzawa, J.J. Sabia, and S. Safford. 2020. Black lives matter protests, social distancing, and covid-19. In *National Bureau of Economic Research Working Paper Series*. Cambridge, MA: National Bureau of Economic Research.

Department of Labor. 2020. Families first coronavirus response act: Employee paid leave rights. Washington, DC: Department of Labor.

Fang, H., L. Wang, and Y. Yang. 2020. Human mobility restrictions and the spread of the novel coronavirus (2019-ncov) in china. In *National Bureau of Economic Research Working Paper Series*. Cambridge, MA: National Bureau of Economic Research.

Federal Emergency Management Agency. 2020. Covid-19 disaster declarations.

Federal Resgister. 2020. Paid leave under the families first coronavirus response act. Washington, DC: National Archives.

Finkelstein, A. 2007. "The aggregate effects of health insurance: Evidence from the introduction of medicare." *Quarterly Journal of Economics* 122 (1):1-37.

Food and Drug Administration. 2020. Covid-19 update: Fda broadens emergency use authorization for veklury (remdesivir) to include all hospitalized patients for treatment of covid-19.

Friedson, A., D. McNichols, J.J. Sabia, and D. Dave. 2020. Did california’s shelter in place order work? Early evidence on coronavirus-related health benefits. In *Center for Health Economics & Policy Studies Working Paper Series*. San Deigo, CA.

Glynn, S.J. 2020. Coronavirus paid leave exemptions exclude millions of workers from coverage. Washington, DC: Center for American Progress.

Gruber, J. 1994. "The incidence of mandated maternity benefits." *American Economic Review* 84 (3):622-641.

Gupta, S., T.D. Nguyen, S. Raman, B. Lee, F.L. Rojas, A. Bento, K. Simon, and C. Wing. 2020. Tracking public and private responses to the covid-19 epidemic: Evidence from state and local government actions.

Henrekson, M., and M. Persson. 2004. "The effects on sick leave of changes in the sickness insurance system." *Journal of Labor Economics* 22 (1):87-113.

Internal Revenue Service. 2020. Covid-19-related tax credits for required paid leave provided by small and midsize businesses faqs. Washtington, DC: Internal Revenue Service.

Lovenheim, M.F. 2009. "The effect of teachers’ unions on education production: Evidence from union election certifications in three midwestern states." *Journal of Labor Economics* 27 (4):525-587.

Lyu, W., and G.L. Wehby. 2020. "Community use of face masks and covid-19: Evidence from a natural experiment of state mandates in the us: Study examines impact on covid-19 growth rates associated with state government mandates requiring face mask use in public." *Health Affairs* 39 (8):1419-1425.

Maclean, J.C., S. Pichler, and N.R. Ziebarth. 2020. Mandated sick pay: Coverage, utilization, and welfare effects. National Bureau of Economic Research.

National Institutes of Health. 2020. Nih clinical trial shows remdesivir accelerates recovery from advanced covid-19. Bethesda, MD: National Institutes of Health.

Park, S., and D. Powell. 2020. Is the rise in illicit opioids affecting labor supply and disability claiming rates? In *National Bureau of Economic Research Working Paper Series*. Cambridge, MA: National Bureau of Economic Research.

Pichler, S., K. Wen, and N. Ziebarth. 2020. Positive health externalities of mandating paid sick leave. In *Cornell University Working Paper*.

Pichler, S., and N.R. Ziebarth. 2017. "The pros and cons of sick pay schemes: Testing for contagious presenteeism and noncontagious absenteeism behavior." *Journal of Public Economics* 156:14-33.

———. 2020. "Labor market effects of us sick pay mandates." *Journal of Human Resources* 55 (2):611-659.

Powell, D., A. Alpert, and R.L. Pacula. 2019. "A transitioning epidemic: How the opioid crisis is driving the rise in hepatitis c." *Health Affairs* 38 (2):287-294.

Powell, D., and R.L. Pacula. 2020. The evolving consequences of oxycontin reformulation on drug overdoses. In *National Bureau of Economic Research Working Paper Series*. Cambridge, MA: National Bureau of Economic Research.

Puhani, P.A., and K. Sonderhof. 2010. "The effects of a sick pay reform on absence and on health-related outcomes." *Journal of Health Economics* 29 (2):285-302.

Raifman, J. 2020. Covid-19 us state policy database. Boston, MA: Boston University.

Schneider, D. 2020. "Paid sick leave in washington state: Evidence on employee outcomes, 2016–2018." *American Journal of Public Health* 110 (4):499-504.

Scognamiglio, A. 2019. "Paid sick leave and employee absences." *LABOUR*.

Sehra, S.T., M. George, D.J. Wiebe, S. Fundin, and J.F. Baker. 2020. "Cell phone activity in categories of places and associations with growth in cases of covid-19 in the us." *JAMA Internal Medicine*.

Stearns, J., and C. White. 2018. "Can paid sick leave mandates reduce leave-taking?" *Labour Economics* 51:227-246.

The Council of State Governments. 2020. State reopen plans. Lexington, KY: The Council of State Governments.

Tomer, A., and J.W. Kane. 2020. How to protect essential workers during covid-19. In *Brookings Report*. Washington, DC: Brookings Institute.

Villarosa, L. 2020. "'A terrible price': The deadly racial disparities of covid-19 in america." *New York Times* April 29, 2020.

World Health Organization. 2020a. Coronavirus disease (covid-19) outbreak. Geneva, Switzerland: World Health Organization Western Pacific.

———. 2020b. Coronavirus disease (covid-19) pandemic. Geneva, Switzerland: World Health Organization.

———. 2020c. Draft landscape of covid-19 candidate vaccines – 11 april 2020. Geneva, Switzerland: World Health Organization.

Ziebarth, N.R., and M. Karlsson. 2010. "A natural experiment on sick pay cuts, sickness absence, and labor costs." *Journal of Public Economics* 94 (11):1108-1122.

———. 2014. "The effects of expanding the generosity of the statutory sickness insurance system." *Journal of Applied Econometrics* 29 (2):208-230.