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Tanya Byker
Elena Patel
Kristin Smith

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740 15th St. NW 8th Floor
Washington DC 20005
202-545-6002

Fitting the Bill? The First Federal Paid Leave Mandate

Tanya Byker* Elena Patel† Kristin Smith‡

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Abstract

We find that the first federal paid leave mandate in the US, enacted under the Families First Coronavirus Response Act, increased paid leave taking by nearly 200% for eligible workers, easing the constraints of the pandemic. Our estimates are based on a comparison of monthly paid absence from January – June, 2020, relative to these same months in 2018 and 2019 and leverage the eligibility criteria of the policy. We find that these benefits were primarily used for medical rather than child care needs, raising questions for optimal policy design within the broader context of federal paid leave policies.

Keywords : Paid Sick Leave, Leave Taking, Federal Leave Policy
JEL Classification: H51, I38, J08, J38

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*Middlebury College; tbyker@middlebury.edu
†David Eccles School of Business, University of Utah; elena.patel@eccles.utah.edu
‡Dartmouth College; kristin.e.smith@dartmouth.edu

1 Introduction

As the COVID-19 emergency recedes in the rear-view mirror, the first federal mandate for paid sick leave in the U.S., implemented as part of the Families First Coronavirus Response Act (FFCRA), has expired. The FFCRA, which took effect on April 2, 2020, was passed on an unusually fast timeline — just seven days lapsed between when the bill was introduced in the House on March 11 and when the bill was signed into law on March 18 — in a bipartisan attempt to stem the oncoming tide of the pandemic. The "emergency" paid leave provision of the FFCRA, which included at least partial wage replacement for own-illness or care-giving leave related to the pandemic, was an unprecedented social safety net expansion for a country that was ill-equipped to meet the employment challenges of the pandemic. As such, the effect of this emergency paid leave provides important insights for federal, state, and local governments as they weigh post-pandemic policies that mandate access to paid sick leave for U.S. workers.

Despite the sudden increase in leave-taking need and existing evidence on the health benefits of sick leave (Pichler and Ziebarth, 2017; Stearns and White, 2018; Pichler, Wen and Ziebarth, 2021), early indications suggested that take-up of the new policy was underwhelming. For example, public opinion surveys conducted shortly after the enactment of the FFCRA report that these benefits were salient to fewer than 30% of employers and 15% of individuals (Miller and Tankersley, 2020). Consistent with these reports, Jelliffe et al. (2021) finds that, six to eight months after the onset of the pandemic, fewer than 50% of employees were aware of these benefits, and fewer than 10% of respondents indicated that they had taken paid sick leave under the policy. Finally, Goodman (2021) finds, based on employer claims in administrative tax data, that fewer than 50% of likely-eligible firms participated in the program.

This conclusion, however, may be incomplete. First, emergency paid sick leave was targeted towards those workers who were the most likely to exhibit unmet need for paid leave: employees at small employers who were not able to telework. As a result of these restrictions, just 32% of the workforce was likely eligible for benefits, a fact that is often missed by polls and small-scale surveys and is not observable in the employer administrative tax data.[1] Second, statistics based on aggregate, employer level observations mask employee-level treatment effects, especially to the extent that there exists employee-heterogeneity in eligibility by employer.

In this paper, we study the effect of the emergency paid leave policy by utilizing individual-level employment data from the Current Population Survey (CPS) that enables us to model the carve-outs of the policy explicitly. The CPS contains monthly worker outcomes, such as absence from work, paid leave status, and employer and occupation characteristics. The richness of these data allows us to answer unresolved questions about the policy's scope and efficacy. Our analysis is based on a difference-in-differences-in-differences strategy that exploits eligibility criteria to separate the effect of the pandemic from the effect of the policy. We compare monthly paid absence in (1) January–March, 2020 (pre-pandemic) to April–June, 2020 (pandemic) relative to (2) these same months in 2018 and 2019, for (3) individuals eligible and ineligible for emergency paid leave based on firm size. Further, we decompose our results based on additional measures of eligibility, by gender, and by family size to provide evidence supporting empirical identification and mechanisms.

We find that access to emergency paid leave increased the likelihood that an eligible individual took paid leave by nearly 100% just a few short months after implementation. By comparison, our evidence suggests that ineligible employees exhausted all available paid leave immediately after the onset of the pandemic, affecting their ability to take paid leave later in the year. Next, we leverage variation based on employee occupations to study those most likely eligible for the policy (non-telework compatible workers) and those likely ineligible for the policy (telework compatible workers). We find that our result is entirely driven by those workers who were employed in non-telework compatible occupations, who were were more than 200% more likely to take paid leave. These estimates suggest that roughly one million

[1] Statistic based on author's calculation using data drawn from the 2019 Current Population Survey.

workers took up paid leave due to the policy during the first three month of the pandemic.

Next, we examine the effect of the FFCRA—which included partially paid leave for the care of children due to childcare issues—by gender and based on the presence of young children in the home. For example, Heggeness (2020); Collins et al. (2021*b*,*a*) show that school shutdowns increased the need for leave-taking, especially for women. However, we find that the effect of the FFCRA is both economically and statistically similar for men and women and for adults with and without young children. This suggests that leave taken under the FFCRA was primarily used for medical rather than childcare needs, consistent with evidence based on administrative tax data (Goodman, 2021).

Our work contributes to a growing empirical literature studying the effect of expanded access to paid leave in the U.S. labor market. Much of the work in this area has been focused on the effect of access to paid *parental* leave to care for newborn children based on mandates in a handful of states; this literature finds substantial take-up of benefits among women with small-to-negative effects on labor force participation (Rossin-Slater, Ruhm and Waldfogel, 2013; Bailey et al., 2019). The evidence from this literature, however, is of limited generalizability to the context of paid sick leave. A more nascent literature studies the recent expansion of mandated *sick* leave at the state level, finding improved health outcomes, but inconclusive evidence on take-up and labor market outcomes (Ahn and Yelowitz, 2015; Pichler and Ziebarth, 2017; Stearns and White, 2018).

We expand this literature in several important ways that allow us to refine estimated take-up and understand potential mechanisms. First, we analyze employee leave taking using high-frequency data, which offer more detailed information than the quarterly administrative tax data describing employer claiming behavior. This information is crucial in identifying the impact of the FFCRA on leave-taking during the early months of the pandemic. These data are also rich in individual worker characteristics by examining *employee* responses, including based on firm, job, and employee characteristics. We combine monthly CPS data with a convincing empirical strategy that leverages variation in access based on the parameters of the policy and following sharp changes in take-up based on employer size seen in the administrative tax data (Goodman, 2021). We find that the FFCRA was more effective at expanding take-up of paid leave than as was found by Goodman (2021) based on employer-level tax records or by Jelliffe et al. (2021) based on small scale survey data. At the same time, we do not find evidence consistent with the take-up of emergency paid family leave used to care for children whose school or child care facility was closed due to the pandemic, despite well-documented evidence of the crisis of childcare caused by the pandemic. This raises questions about how to improve access or increase awareness about these benefits for future policy design.

2 Background and Data

2.1 COVID-19 Pandemic: Impacts on Labor Market

The first case of SARS-COV2 in the United States was confirmed via testing on January 18, 2020. It would, however, take several more weeks for the U.S. case counts to rise to a level that triggered economic shutdowns. California was the first state to issue mandatory stay-at-home orders—a policy intended to stop the spread of the disease—on March 19, followed by New York on March 20, and Illinois and New Jersey on March 21. By March 25, all U.S. public schools were closed for in-person learning, and by April 1, 35 states had implemented mandatory stay-at-home orders, fundamentally disrupting the US labor and childcare market. As a reflection of these sudden and jarring changes, the U.S. unemployment rate soared to 14.7% in April 2020 — the highest recorded unemployment rate in the post-World War II era.

2.2 Access to Paid Sick and Family Leave

Prior to the pandemic, there was no federal mandate to provide U.S. workers with wage replacement during absences from work. However, many workers had access to paid leave for certain approved health and family reasons that were either voluntarily provided by their employer or mandated by the state or municipality where they worked. The policies and mandates for these paid leaves and the extent of access depended on the leave-taking need.

Paid sick leave provides some wage replacement for absences due to short-term health needs and preventative care. Employees earn or accrue sick days in proportion to the number of hours they have worked. At the beginning of 2020, paid sick leave was mandated in 10 states and 29 localities (Stearns and White, 2018; Maclean, Pichler and Ziebarth, 2020; Al-Sabah and Ouimet, 2021; Byker, Patel and Ramnath, 2023). According to the Bureau of Labor Statistics (BLS) National Compensation Survey (NCS), 75 percent of all private workers had access to at least some paid sick days in 2019. The NCS estimates that workers with access to paid sick leave and one to five years of tenure with a firm had, on average, eight paid sick days per year in 2019 (Bureau of Labor Statistics, 2020).

Paid family and medical leave (PFML) relates to longer-term leave to care for own serious illness (medical leave), to care for a family member with a serious illness (family leave), or to bond with a new child (parental leave). Unlike sick leave, PFMLs typically require a medical authorization, approval, and waiting period. At the beginning of 2020, 5 states had implemented PFML mandates (Byker and Patel, 2021). The duration of leave in these states ranges from 8 to 52 weeks, and wage replacement rates range from 60% to 90%.

2.3 Details of the FFCRA Legislation

The leave mandate under the FFCRA expanded access to paid leave in two ways. The first covered up to two weeks of paid leave for certain qualified COVID-19 related reasons, including own illness or a family member's illness, quarantining, and stay-at-home orders. This type of leave taking, referred to as emergency paid sick leave, was paid at a rate of 100% of an employee's normal wages - up to $511 per day and a maximum of 80 total hours of leave. The second type of paid leave, referred to as paid family leave, provided wage replacement for work absences related to care for children whose school or childcare was closed for pandemic-related reasons. This leave provided wage replacement at a rate of 2/3 of normal wages — up to $200 per day — for up to 10 weeks of absence.

The FFCRA emergency paid leave programs were mandated for eligible employees — those who have been with their employer for at least 30 calendar days — of covered employers — those that employ fewer than 500 employees, beginning April 1, 2020.[2] Moreover, the legislative language explicitly excluded workers who were capable of telework from eligibility. The parameters of the policy are especially important given that (1) workers at small firms have less access to paid family leave, paid medical leave, and paid parental leave (Smith, 2019; Bureau of Labor Statistics, 2020) and (2) jobs that are not telework-compatible are typically lower paying jobs than those that can be preformed remotely (Dingel and Neiman, 2020).

In an unprecedented move, the cost of wage replacement paid under emergency paid sick and family leave was entirely funded by the federal government. This funding mechanism is unusual within the landscape of state mandated paid leave programs, where paid family leave programs are funded through an employer and/or employee payroll tax and paid sick leave programs represent an unfunded mandate on employers to provide this benefit. In the case of emergency paid leave, employers were directed to pay benefits directly to their employees; 100% the cost of these benefits was paid to employers through an immediately refundable payroll tax credit. Employers could either reduce payroll tax deposits in anticipation of the payroll tax credits or they could request an advance payment of the tax credit.

2.4 Sources of Policy Variation

Any empirical analysis of the effectiveness of a single policy enacted at the start of the pandemic is complicated by the rapidly changing policy and economic landscape. Two substantial pieces of legislation were enacted during March of 2020, providing more than $2

[2]Employers with fewer than 50 employees were exempt from this mandate if the requirement would "jeopardize the viability of the business as a going concern." In addition, employers of health care providers and emergency responders could exclude such employees from this program.

trillion in stimulus to support the U.S. economy. First, the Coronavirus Aid, Relief, and Economic Security (CARES) Act, signed into law on March 27, 2020, provided economic relief to businesses, individuals, and state and local governments including direct payments to individuals, expanded unemployment benefits, and small business loans though the Paycheck Protection Program (PPP). Importantly employers may not use the proceeds of a PPP loan to pay for any COVID-19 related leaves that are required to be provided under the FFCRA, negating their ability to take the payroll tax credit to offset the cost of paid leave benefits. Second, the FFCRA provided unemployment benefits in addition to the paid leave benefits that are the focus of this study. In order to isolate the effect of a national emergency paid leave from among these other expansive changes, it is necessary to identify sub-national variation in the policy that does not interact with other stimulus policies.

One approach typically used in the paid leave literature exploits existing variation in access to paid leave across states, especially in light of the fact that 10 states and the District of Columbia had already implemented state mandated paid sick leave policies prior to the pandemic. We do not use variation across states with and without pre-existing paid sick leave or Paid Family and medical Leave mandates to identify the impact of FFCRA for multiple reasons. First, there is not evidence that mandated state *sick* leave policies affected paid sick leave take-up prior to the pandemic (Byker, Patel and Ramnath, 2023). Next, the leave available for childcare disruptions under FFCRA was not previously available in any of the states mandating paid *family* leave. Finally, the each state's experience of the pandemic varied dramatically from month to month due to substantial geographic variation in the health effects of the pandemic and variation in the response of state legislatures. For these reasons, comparing the experience of workers in states with and without paid sick leave mandates does not improve causal identification in this context and may in fact muddy it.

Instead, we exploit the design of the policy, leveraging both employer and employee variation in treatment, to isolate the effect of the emergency paid leave policy on leave taking. As previously described, the mandate of the policy is defined at the employer level based on employee size, where small employers (with fewer than 500 employees) were required to provide access to these benefits. To this end, (Goodman, 2021) studies take-up of the paid leave payroll tax credits by firms using administrative payroll tax data from second quarter of 2020 to the second quarter of 2021. In these administrative tax data, Goodman finds a strong positive correlation between firm size and tax credit take-up for firms with fewer than 500 employees and a sharp discontinuity in the take-up of tax credits for firms with 500 or more employees. We use the discontinuity in take-up of leave credits seen in the administrative tax data to identify workers who had access to emergency paid leave under FFCRA by comparing workers at firms above and below that threshold.

In addition, the legislative language of the emergency paid leave identifies eligible employees as those who are "unable to work" for qualifying reasons related to the pandemic. Importantly, those workers who are able to telework are *ineligible* for these benefits. Updated guidance from the Department of Labor clarifies telework capable workers were eligible for benefits only to the extent that they were unable to perform their work duties due to own illness or caring for a sick family member. By comparison, non-telework compatible employees were unambiguously eligible for emergency paid leave benefits for expanded reasons, including being unable to work because they were quarantining or subject to stay-at-home orders which were especially relevant during our period of analysis in the early pandemic.[3] This variation in eligibility driven by the telework-compatibility of the occupation is masked in employer-level analyses, which combine evidence within a firm and across occupations that are more or less treated by the policy. Our data allows us to use this additional occupation-level variation to identify the effect of the policy.

2.5 Data

We draw data from two components of the CPS from 2018–2020: the basic monthly survey (BMS), which contains information about monthly employment, and the Annual Social

[3] `https://www.dol.gov/agencies/whd/pandemic/ffcra-questions`

and Economic Supplement (ASEC) which is fielded annually and contains detailed employer information. From the BMS, we observe our primary outcome of interest: whether a respondent was absent from work last week and whether this absence was paid or unpaid. In addition, we observe demographic information such as gender, age, education, age, and number and age of children in the household.

From the ASEC, we observe the size of a respondent's employer, measured by total number of employees, and an individual's occupation during the last year.[4] We categorize employers as large and small based on whether an employee reports that their employer has more or fewer than 500 employees. We categorize occupations as telework compatible and incompatible following Dingel and Neiman (2020) and Heggeness and Suri (2021). Dingel and Neiman (2020) design the original telework variable by reviewing the Work Context and the Generalized Work Activities questionnaires in the O*NET database. Occupations, in standard occupational classification (SOC), are defined as non- telework-compatible if respondents indicate that their work requires being outdoors, risking injury or illness, operating machines, performing physical activities, etc. All other occupations are identified as telework-compatible. Heggeness and Suri (2021) establish a binary telework variable by re-coding the SOC format to fit CPS data.

We impose two sample restrictions to these data. First, we limit our analysis to adults aged 21 to 59. Second, we limit our sample to those individuals who were (1) interviewed in the ASEC of the relevant year and (2) worked in the previous year. This latter restriction is required to ensure that we can measure firm size and identify occupation, key determinants of access to paid leave under FFCRA. A practical consequence of this, however, is that we must limit our analysis to observations from January through June of each year. This is because the CPS is structured as a short, interrupted panel – households are interviewed for four months and then they are out of the survey for eight months before being interviewed for another

[4]Specifically, the ASEC asks a respondent to identify the number of persons who worked for their employer during the preceding calendar year across all establishments. Responses are categorized into the following groups: under 10, 10–24, 25–99, 100–499, 500–999, 1000+. The universe is persons age 15+ who worked during the preceding calendar year.

four months. Ensuring that every individual in our sample was observed in the ASEC, our sample includes individuals interviewed from two months before to three months after the ASEC interview in March. Importantly, studying observations from January through June allow us to study the effect of FFCRA on paid leave taking during the early months of the pandemic in 2020 compared to leave-taking patterns before the pandemic in 2018 and 2019.

Table 1 provides summary statistics for the individuals in our sample based on their responses in February of each survey year. Column (1) summarizes the individuals in our sample in 2018 and 2019, and column (2) summarizes the individuals in our sample in 2020. Generally, we see very little difference in the individuals in our sample during this control month (February) across all survey years. To begin, February labor force participation was 96%; this high rate of participation is driven by the restriction that we must make in order to observe firm size: as of March, individuals must have been employed in the previous year. In addition, 47% of our sample are women, 55% are married, 31% have young children (under 12 years old), 40% have a bachelors degree, 42% work in occupations that are telework compatible, and respondents are 40 years old, on average. The primary outcome in our analysis is paid absence from work during the reference week of the survey.[5] Columns (1) and (2) of Table 1 show that individuals in our sample had similar rates of paid leave-taking — roughly 1.5 % — in February across all survey years.[6]

3 Methods

In this section, we describe how we formalize changes in monthly leave taking across 2018, 2018, and 2019 using a difference-in-differences and a difference-in-difference-in-differences methodology. As we will show, a comparison of monthly patterns in 2020 to 2019 and 2018 can identify the effect of the pandemic on leave-taking. Any additional variation in

[5]Specifically, this variable identifies those individuals who were absent from work during the entire reference week with pay.

[6]More specifically, 1.5% of individuals took paid leave during the reference week. This translates to a cumulative probability of an individual taking paid leave at least once over the entire year of 55.6%.

these patterns for employees at small employers, who were eligible for emergency paid leave, compared to employees at large firms, who were ineligible, can expose the effect of the policy. In addition, we decompose our results based on occupation-level telework-compatibility, exposing likely variation in the intent-to-treat of the policy.

First, we study the effect of the pandemic on monthly leave-taking by employing a dynamic difference-in-differences empirical specification:

$$y_{imt} = \lambda_{2020} + \sum_{m \neq -2} \alpha_m \, \mu_m + \sum_{m \neq -2} \beta_m \left(\mu_m \cdot \lambda_{2020}\right) + \theta_s + X_{imt} + u_{imt} \tag{1}$$

where y_{imt} is an indicator variable that identifies monthly paid work absences for individual i in month m in year t. Here, μ_m capture monthly fixed effects, omitting February as the reference category. λ_{2020} is an indicator for 2020, θ_s captures state fixed effects, and X_{imt} includes control variables that explain monthly leave taking including individual characteristics such age, education, the presence of young children in the home, and controls for the evolution of the pandemic such as state-by-month COVID-19 case counts, and state-by-month total Paycheck Protection Program loans.[7] In all cases, we report standard errors clustered at the state level.

The β_m coefficients capture the causal effect of the pandemic on monthly leave taking in April–June of 2020 compared to 2019 and 2018. The onset of the pandemic fundamentally altered the US economic and health landscape. In response, everyone — individuals, local, state, and federal policy makers, employers — altered behavior. For this reason, β_m cannot distinguish between the effect of emergency paid family leave separately and these other confounding factors.

Next, we exploit parameters of the policy by comparing employees that did and did not have emergency paid leave access to identify the effect of the policy on leave-taking behavior. Specifically, employees at firms with fewer than 500 employees (small firms) were mandated

[7] Take-up of PPP loans reduces the ability of employers to claim the emergency paid leave credit under the provisions of both Acts, as described in Section 2. Therefore, we control for the aggregate sum of PPP loans under $250,000 issued in a given state in each month.

to have access to emergency paid family leave (treated employees), whereas employees at firms with more than 500 employees (large firms) were ineligible for emergency paid family leave (control employees). At the same time, the CPS does not identify the *source* of pay for paid absences from among employer-sponsored paid leave, state paid leave, and emergency paid leave. For this reason, comparisons based on differential access reflect the intent-to-treat (ITT) of the policy.

We formally make this comparison based on a dynamic difference-in-differences-in-differences model. This model is a modification of Equation 1 where we add dummy variable Small_i to identify individuals who work for small employers and saturate the model by including all relevant interaction terms:

$$\begin{aligned} y_{imt} = \; & \lambda_{2020} + \sum_{m \neq -2} \alpha_m \, \mu_m + \sum_{m \neq -2} \beta_m \left(\mu_m \cdot \lambda_{2020} \right) + \theta_s + X_{imt} + \\ & + \text{Small}_i + \text{Small}_i \cdot \lambda_{2020} + \sum_{m \neq = -2} \text{Small}_i \cdot \mu_m \\ & + \sum_{m \neq -2} \gamma_m \left(\text{Small}_i \cdot \mu_m \cdot \lambda_{2020} \right) + u_{imt} \qquad (2) \end{aligned}$$

In this specification the coefficients γ_m capture the effect of the policy in each month; in other words, differences in monthly leave taking in each month from January through June relative to February 2020 compared to these same months in 2019 and 2018 for employees at small firms compared to large firms. As before, differences in February leave taking serve as the reference group, and standard errors are clustered at the state level.

In order for these estimates to be interpreted as causally due to the FFCRA, access to emergency paid leave must be the only reason for differences in paid leave take-up during the pandemic across employees at large and small firms. To support this assumption, we provide evidence that treated and control employees were similar across a range of demographic characteristics in February 2020 compared to February 2019 and 2018. Columns (3)–(6) of Table 1 report mean characteristics control and treated employees, respectively. Columns (7) and (8) report estimates of the difference in observable characteristics for control employees compared to treated employees in 2020, relative to these same differences in 2019 and 2018.[8] These estimates show that the observable characteristics of treated and control employees evolved similarly prior to the pandemic. For example, while treated employees are less likely to be female (45% compared with 50% in 2020), the change in this difference is small and statistically insignificant at all conventional levels. Likewise, treated employees are less likely to have a bachelor's degree (35% compared with 48%, cols 4 and 6), but these differences are similar in 2018, 2019, and 2020. Generally, we find evidence of balance based on gender, martial status, age, education, and household composition. Most importantly, differences in the likelihood of paid absence are both economically and statistically insignificant.

4 Analysis

4.1 Effect of the Pandemic on Monthly Leave Taking

Panel (a) of Figure 1 plots the share of individuals absent from work with pay in each month from January through June of 2018 (dashed grey line), of 2019 (solid grey line), and of 2020 (solid black line). These figures draw attention to several important patterns in the data that support our estimation strategy. First, and as previously described, monthly paid absence from work exhibits a baseline cyclicality: workers are almost three times as likely to be absent from work with pay in June compared with January. This could reflect summer vacation behavior, increased care giving needs as school-aged children as the academic school year ends, and/or seasonal trends in illness. Second, this cyclicality was stable across months and years before the arrival of the pandemic: there are neither statistical nor meaningful differences in monthly paid absence in January–March, 2020 compared with these same months in 2019 and 2018 and, likewise, trends in paid leave taking were similar in April–

[8] We estimate these differences based on Equation 1. Column (7) reflects a mean comparison and column (8) includes state fixed effects. Standard errors are clustered at the state level.

June of 2018 and 2019. Third, monthly paid absence diverges sharply in April 2020, and the timing of this divergence exactly coincides with both the onset of the pandemic and the implementation of emergency paid leave and other stimulus measures.

Although the onset of the pandemic and its effect of the US economy arrived in mid-to-late March, it does not appear that this induced notable changes in the share of individuals taking paid leave in March, 2020. While perhaps surprising, differences in paid leave taking in March 2020 will be muted by several factors. First, the BMS is typically administered on a rolling basis throughout the month and references the last week's employment. Second, the administration of the CPS itself was affected by the onset of the pandemic (Ward and Anne Edwards, 2021). Third, and, most importantly, emergency paid leave was not available until April 1st.

Panel (b) of Figure 1 plots difference-in-difference estimates, including 95% confidence intervals, of the effect of the pandemic on monthly paid absence based on Equation 1; all estimates are scaled by monthly paid-leave taking in February of 2018 and 2019. Throughout this analysis all models including controls for age, education, the presence of young children in the home, and state fixed effects, as described in section 3. In addition, we control for pandemic-specific changes based on state-by-month COVID case counts and state-by-month Paycheck Protection Program loan totals. Corresponding point estimates estimates are also reported in column (1) of Table 2.

We find that individuals in our sample were 78% (0.00895/0.0115, Col. 1 Table 2) more likely to be absent from work with pay in April 2020 due to the onset of the pandemic; this estimate is statistically significant at the 1% level. This sharp change in leave-taking was, however, short-lived. We estimate a reversal of the April spike in May and June: paid leave taking returned to pre-pandemic levels in May 2020 and paid leave fell to 50% below typical June paid-leave taking in 2020 (0.00580/0.0115, , Col. 1 Table 2). We interpret this pattern as evidence that workers likely exhausted their stock of paid leave early in the pandemic and were subsequently constrained in taking planned or needed absences as the pandemic wore on. Importantly, these patterns reflect the cumulative effect of the onset of the pandemic and all policy and environmental changes that occurred in April, 2020 rather than isolating the effect of emergency paid leave.

4.2 Effect of Access to Emergency Paid Leave

Next, we exploit the variation in eligibility for emergency paid leave based on firm size in order to identify the effect of the FFCRA. Panels (c) and (d) of Figure 1 depict monthly paid leave-taking rates separately for employees at control and treated firms in 2018–2020, respectively. As previously described, our difference-in-difference-in-differences identification strategy identifies the effect of the policy by comparing pre- and post-pandemic paid leave taking for employees at small firms (the treated group) to employees at large firms (the control group). Panel (e) reports these estimates, scaled by February leave taking rates in 2018 and 2019 among employees at treated firms.

Estimates in panel (e) show that access to emergency paid sick leave reversed the effect of the pandemic on paid leave-taking. While treated employees were no more likely to take paid leave in April, they were 24% more likely to take paid leave in May (0.00242/0.0100, col 2), and they were 89% more likely to take paid leave in June (0.00892/0.0100, col 2). In other words, we find that treated employees were *more* likely to take paid leave as the pandemic wore on.

The monthly pattern of our estimates is consistent with a sudden onset of need for paid absence that arose in April and affected all employees, regardless of firm size, and a policy that alleviated this pressure only for treated workers. US workers had, on average, 8 days of paid sick leave available at the onset of the pandemic according to 2019 National Compensation Survey estimates.[9] However, the sudden change in the employment and care-giving environment likely necessitated leaves of longer than 8 days; indeed, many workers operated under state and local shutdown orders that lasted weeks, if not months. Our

[9] Average number of paid sick leave days available after five years of service, all Civilian workers. Source: National Compensation Survey, 2019

evidence suggests that those employees without access to emergency paid leave were forced to fully exhaust all available paid leave at the start of the pandemic. On the other hand, we find that access to emergency paid leave provided relief for treated employees, and as a result, these employees could accommodate additional leave-taking needs that arose in the following months.

Identification requires that differences in pre- and post-pandemic leave taking would have evolved similarly for employees at small and large firms if not for access to emergency paid leave. We test for a violation of this identifying assumption by comparing differences in paid absence take-up during the pre-pandemic months based on equation 2. We do not find evidence of differential paid leave taking in January, February, and March in 2018, 2019, or 2020, consistent with our underlying parallel trends assumption. Specifically, employees at small firms were 3% (0.000257/0.0100, col. 2) less likely to take paid leave in January compared to February, and were 13% (0.00133/0.0100, col. 2) less likely to take paid leave in March compared to February (Table 2, col. 2). Neither of these estimates are statistically or economically significant, and we fail to reject a null hypothesis of parallel pre-trends with p-value of 0.839.

We further refine our analysis by decomposing our estimates among those workers in telework and non-telework compatible occupations. Workers in non-telework compatible occupations are more likely to have benefited from emergency paid leave because they would not have been subject to the exemption in the mandate for workers who were able to telework as discussed in Section 2. Estimates for the subgroup of workers with telework-compatible occupations thus provide a placebo test for our identification strategy. These results are shown in Figure 2 and reported in columns (3) and (4) of Table 2.

This decomposition reveals that our baseline estimate of the effect of the policy is driven by those employees who work in non-telework compatible occupations. Panels (a) and (b) of Figure 2 plots leave-taking rates for control and treated employees and panel (c) plots scaled estimates for these workers; related point estimates are reported in column (4) of Table

2. We find that workers in non-telework compatible occupations were 189% more likely to take paid absence in June, 2020 (0.182/0.00959, col. 4) due to the emergency paid leave policy; this estimate that is statistically significant at the 1% level with a t-statistic of 4.17. In addition, this sharp increase in leave taking in June of 2020 coincides with start of the first substantial wave of COVID-19 infections, suggesting that eligible employees at treated employers we better able to accommodate the evolving pandemic landscape.

On the other hand, we find that workers in telework-compatible occupations did not benefit from the policy, consistent with the carve-outs of the policy. As seen in panel (f) of Figure 2 and column (3) of Table 2, these workers were just 1% more likely to take paid leave in April and were 20% less like to take paid leave in June; neither of these estimates are statistically significant.

4.3 Heterogeneity by Gender and Kids Under 12

As previously discussed, the FFCRA provided at least partially paid wage replacement for work absences that could be classified as either sick leave — leave due to own illness or a family member's illness — or expanded family leave — leave needed to care for a child whose school or child care provider is closed due to the pandemic. Although this latter expansion of family leave was paid at a two-thirds wage placement rate, whereas emergency sick leave was fully paid, expanded paid family leave due to child care disruptions represents an unprecedented expansion of the US social safety net to meet the unusual needs of the pandemic. In spite of this, evidence of employer claims based administrative tax data shows the vast majority of emergency paid leave claims were for sick leave, rather than childcare issues (Goodman, 2021).

We expand our analysis by investigating heterogeneity in the impact of the policy based on gender and the presence of young children in the household. Although our data cannot identify the type of leave taken, we hypothesize that variation in the effect of the policy by gender and based on the presence of young children in the household would be consistent with leave taken for childcare issues rather than for sick leave. We focus on those workers who are employed in telework-incompatible jobs for this analysis because, based on our baseline analysis, these workers were the most likely to be eligible for the policy. Figure 3 shows scaled results for men and women and for employees with and without children under 12 in the household based on Equation 2. Columns (5) – (8) of Table 2 report point estimates.

Despite concerns that women had greater need for leave for paid absence during the early months of the pandemic, we find that women and men were not substantially more likely to take a paid absence in June, 2020 due to the FFCRA. As seen in panels (a) and (b) of Figure 3, we estimate that men were 165% (0.0176/0.0106, col. 5) more likely to take paid absence, and that women were 231% (0.0183/0.0115, col. 6) more likely to take paid absence. While both estimates are independently statistically significant at the 5% level, these estimates are not statistically distinguishable from each other.

Likewise, we find that paid absences increased in similar ways for workers with and without children (161% compared with 204%, respectively). Our result for employees without young children is statistically significant at 1% level, and our result for employees with young children is statistically significant at the 5% level. Although are estimates are not statistically distinguishable from each other, the magnitude of our estimates are actually larger for those workers *without* young children.

Taken together, our estimates run counter to the concern that women and parents were more likely to need leave to accommodate the remote learning and disrupted care-giving environment of the pandemic. On the other, our estimates provide additional worker-level evidence consistent with Goodman (2021), who shows that the majority of employer tax claims made to offset the cost of emergency paid leave were for sick leave, rather than for childcare issues. In light of this, an outstanding policy question remains as to why emergency expanded family leave benefits, which were designed to alleviate a salient and unique need for work absence during this crisis, were not taken by workers.

4.4 Additional Discussion: Labor Force Participation

Because an individual must be employed in order to take paid absences from work, it is necessary to interpret our results in the context of any simultaneous shifts in employment and labor force participation within our sample. A well-documented narrative of the onset of the pandemic was the historic spike in unemployment rate in April of 2020 that was followed by an unprecedented exodus from the labor market for many individuals. However, whether this behavior was empirically relevant for the individuals in our analysis sample remains an open question.

To shed light on this, we estimate the effect of the pandemic on labor force participation based on Equation 1 and report these results in column (1) of Table 3. We find that the pandemic reduced labor force participation by 2% in April, 2020, and this decline is statistically significant at the 1% level. Labor force participation recovered steadily in May and June. By June, 2020 labor force participation was just 0.6% lower due to the pandemic, and this difference was not statistically significant. In other words, the effect of the pandemic on labor force participation for those in our analysis sample of 21 to 59 year-olds who were employed in March 2020 was small in magnitude and short-lived.

It is possible that policy itself had a direct effect on labor-force attachment, but the sign of this expected impact is ambiguous. On the one hand, access to paid leave should increase attachment for the marginal worker who might otherwise have detached due to leave-taking needs. On the other hand, longer work absences facilitated by the policy could increase separation if, for example, longer leaves impact preferences for working compared with home production. Consistent with this latter pressure, past literature finds that increased access to paid family leave *decreases* employment and earnings for first-time mothers in California (Bailey et al., 2019). In either case, a difference-in-differences-in-differences comparison of labor force participation rates for treated and control employees can speak to these effects.

The remaining columns of Table 3 report estimates of impact of FFCRA on labor force participation for those in our sample based on Equation 2. In general, we find more muted effects on labor force participated for treated workers during the pandemic months. Among our baseline sample, labor force participation decreased by just 1% for treated workers during the pandemic months (col 2). There is some evidence that labor force participation decreased for treated workers among our subgroups (cols 3–8), however these estimates are noisy. All together, this analysis suggests that selection out of the labor force does not drive our estimates of the effect of the policy on paid absences.

5 Conclusion

Early empirical analyses of the effect of the emergency paid leave suggest that the FFCRA provision was less successful than originally envisioned. According to administrative payroll tax data, 45 percent of firms sized 100-300 claimed credits for emergency paid leave through the first quarter of 2021 Goodman (2021); moreover, the Government Accountability Office (GAO) estimates that $9.8 billion in credits were claimed during 2020, which is just 12% of the initial cost estimate made by the Congressional Budget Office(CBO, 2020; GAO, 2022). In addition, a small, nationally representative survey (n=371) conducted at the end of 2020 estimates that 5.4 percent of workers (estimated 8 million) took paid leave under FFCRA Jelliffe et al. (2021). Our analysis expands on these aggregate estimates by studying *who* took up paid leave during the early months of the pandemic, allowing us to address outstanding questions about the reach and effectiveness of the policy. Leveraging the detailed monthly data in the CPS, we find that the act helped eligible workers ease constraints imposed by limited access to paid sick leave on the eve of the pandemic.

We find similar rates of paid absence in the first month the pandemic across eligible and ineligible workers. However, only workers eligible for FFCRA — those at small firms with non-telework compatible jobs — were able to additionally take paid absences later in the year as the pandemic wore on. In contrast, ineligible workers seem to have exhausted any accumulated sick days at the onset of the pandemic and had depressed levels of paid absence later in the year. Monthly data allow us to detect this temporal variation to understand the mechanisms behind the take-up rates seen in the aggregate data. A back-of-the-envelope calculation implies that roughly a million workers took leave due to FFCRA in the first quarter the policy was in effect. This estimate implies that the average affected worker received between $1,200 and $3,300 in wage replacement.[10]

Further, our ability to investigate subgroups of workers by gender and parenthood status allows us to investigate which elements of the FFCRA were effective in meeting worker needs. We interpret homoegeneity in the response of men and women and in the response of parents and non-parents as evidence that the emergency paid *sick* leave was an effective policy tool to accommodate heightened leave taking needs during a sudden public health crisis. This is consistent with evidence in the administrative tax data, which was dominated by paid sick leave claims (Goodman, 2021). On the other hand, the homogeneity that we estimate is inconsistent with access to emergency paid leave meaningfully affecting take up of family care leave during a time of unprecedented disruptions to child care. Goodman hypothesizes this may be due to lower replacement rates and caps for leaves for childcare needs than for sick leave.

The ability to stay home afforded by paid sick leave has been shown to improve public health both before the pandemic Stearns and White (2018) and during the pandemic. Specifically, two papers study the impact of FFCRA on reducing physical mobility using GPS tracking of cellular devices Andersen et al. (2020) and improvements in health outcomes using COVID case counts Pichler, Wen and Ziebarth (2020). Our paper adds to this literature assessing the impact of the first federal mandate for paid leave in US history.

[10]This calculation combines the GAO estimate of the total amount of tax credit claimed with on our estimate of the effect of the policy for the non-telework compatible subgroup. We estimate that 32% of workers in March 2020 were eligible for the policy based on CPS data identifying workers in small firms who who worked in non-telework compatible jobs. In addition, we estimate wage replacement rates based on the average weekly earnings from the CPS for this group of workers in March 2020.

Table 1: Descriptive Statistics and Balance: February, 2018–2020

	Full Sample		Control Large Firms		Treated Small Firms		Large vs Small Balance	
	18–19	20	18–19	20	18–19	20		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Labor Force Participation	0.956	0.954	0.960	0.960	0.951	0.947	-0.00390	-0.00383
							(0.00329)	(0.00329)
Female	0.469	0.476	0.492	0.501	0.447	0.451	-0.00475	-0.00451
							(0.00785)	(0.00785)
Married	0.553	0.552	0.544	0.548	0.561	0.556	-0.00839	-0.00835
							(0.00787)	(0.00786)
Age	39.8	39.9	39.6	39.8	40.1	40.0	-0.301	-0.283
							(0.176)	(0.176)
Bachelors	0.399	0.417	0.457	0.482	0.346	0.354	-0.0170	-0.0154
							(0.00770)	(0.00765)
Young Kids	0.312	0.304	0.302	0.302	0.320	0.304	-0.0156	-0.0158
							(0.00726)	(0.00726)
Unemployment	0.0271	0.0254	0.0219	0.0223	0.0320	0.0283	-0.00412	-0.00419
							(0.00263)	(0.00263)
Telework Compatible	0.418	0.4385	0.453	0.4755	0.384	0.4011	-0.00544	-0.00447
							(0.00791)	(0.00789)
Paid Absence	0.0114	0.0118	0.0131	0.0130	0.00980	0.0106	0.000892	0.000963
							(0.00168)	(0.00168)
State Fixed Effects								✓
Observations	64,464	64,464	30,720	30,720	33,744	33,744	64,464	64,464

Notes: This table reports summary statistics for the individuals in our sample in February of 2018, 2019, and 2020. To be included in our sample, an individual must have been employed in the prior year as reported in the March ASEC. Columns 1 and 2 report summary statistics for our full sample, columns 3 and 4 report summary statistics for individuals working at employers with more than 500 employees in the previous year, and columns 5 and 6 report summary statistics for individuals working at employers with fewer than 500 employees in the previous year. Column 7 reports mean differences for employees at big and small firms in February, 2018–2019 compared to February, 2020. Column 8 reports these same differences based on a regression that includes state fixed effects. Robust standard errors are reported in columns 7 and 8.

Table 2: DD and DDD Estimates: Changes in Paid Absence

	Baseline		Telework DDD		Gender DDD		Children DDD	
	DD	DDD	Yes	No	Male	Female	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Effect								
January	-0.00242**	-0.000257	-0.00556	0.00369	0.00437	0.00176	0.00234	0.00687
	(0.00102)	(0.00246)	(0.00541)	(0.00357)	(0.00508)	(0.00587)	(0.00388)	(0.00645)
March	0.000442	-0.00133	-0.00547	0.00179	-0.00149	0.00522	-0.00229	0.0113**
	(0.00161)	(0.00224)	(0.00364)	(0.00244)	(0.00308)	(0.00478)	(0.00224)	(0.00516)
April	0.00895***	0.0000686	0.000113	-0.000452	-0.00205	0.00199	0.000451	-0.00296
	(0.00254)	(0.00323)	(0.00395)	(0.00455)	(0.00493)	(0.00704)	(0.00396)	(0.00940)
May	0.000939	0.00242	0.00352	0.00168	0.00409	-0.00219	0.00163	0.00139
	(0.00204)	(0.00276)	(0.00493)	(0.00341)	(0.00479)	(0.00667)	(0.00373)	(0.00801)
June	-0.00580**	0.00892*	-0.00222	0.0182***	0.0176**	0.0183**	0.0179***	0.0186**
	(0.00235)	(0.00488)	(0.00930)	(0.00436)	(0.00694)	(0.00767)	(0.00428)	(0.00913)
Control Mean	0.0115	0.0100	0.0108	0.00959	0.0106	0.00791	0.00871	0.0115
Parallel Trends	0.0239	0.839	0.255	0.583	0.430	0.532	0.234	0.0989
N	470,279	470,279	198,947	271,332	157,623	113,709	188,437	82,895

Notes: This table reports estimates of the change in likelihood of taking paid leave due to the pandemic based on equation 1 in column (1) and the effect of access to FFCRA emergency paid leave on the likelihood of paid leave-taking using 2 in columns (2) –(8). Columns (1) and (2) report baseline estimates based on all individuals in our sample, as described in Section 2 and 3. Columns (3) and (4) further restrict our analysis to those individuals who worked in telework ineligible jobs, further leveraging the eligibility criteria of the FFCRA. Columns (5) and (6) report the effect of access to FFCRA separately for men and women who work at non-telework compatible jobs, respectively. Columns (7) and (8) report the effect of access to FFCRA separately for those without and with children under 12 in the household who work at non-telework compatible jobs, respectively.

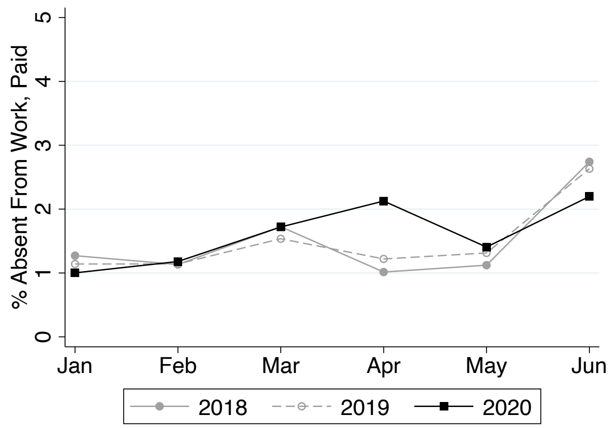
Table 3: DD and DDD Estimates: Changes in Labor Force Participation

	Baseline		Telework DDD		Gender DDD		Children DDD	
	DD	DDD	Yes	No	Male	Female	No	Yes
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment Effect								
January	-0.000190	-0.00428	-0.00311	-0.00488	0.00435	-0.0177*	-0.00577	-0.00272
	(0.00220)	(0.00345)	(0.00430)	(0.00595)	(0.00636)	(0.0102)	(0.00608)	(0.0108)
March	-0.00587***	0.000924	0.00347	-0.000962	0.000421	-0.00385	0.00117	-0.00622
	(0.00136)	(0.00290)	(0.00392)	(0.00410)	(0.00660)	(0.00530)	(0.00653)	(0.00712)
April	-0.0184***	-0.0122***	-0.0130*	-0.00965*	-0.00158	-0.0246**	-0.00991	-0.00942
	(0.00389)	(0.00412)	(0.00657)	(0.00536)	(0.00790)	(0.0105)	(0.00678)	(0.00962)
May	-0.0152***	-0.0000276	-0.00742	0.00879	0.0146	-0.00203	0.0121	0.00136
	(0.00339)	(0.00574)	(0.00721)	(0.0118)	(0.0140)	(0.0172)	(0.0153)	(0.0117)
June	-0.00613	-0.0109**	-0.00478	-0.0123	-0.00272	-0.0284	-0.0166	-0.00123
	(0.00456)	(0.00508)	(0.0100)	(0.00912)	(0.0104)	(0.0225)	(0.0107)	(0.0167)
Control Mean	0.956	0.952	0.961	0.945	0.961	0.921	0.943	0.951
Parallel Trends	0.000374	0.414	0.503	0.713	0.792	0.231	0.561	0.628
N	470,279	470,279	198,947	271,332	157,623	113,709	188,437	82,895

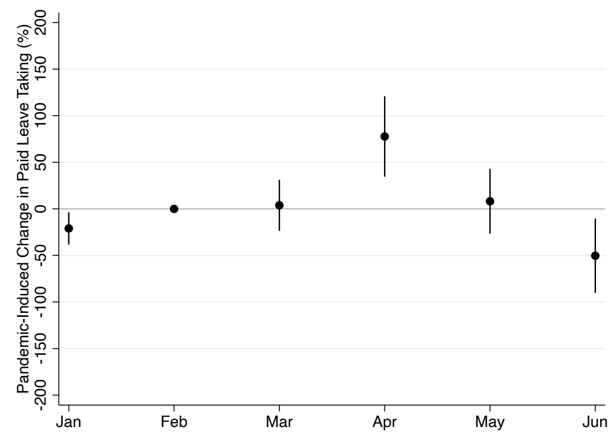
Notes: This table reports estimates of the change in labor force participation due to the pandemic based on equation 1 in column (1) and the effect of access to FFCRA emergency paid leave on the likelihood of paid leave-taking using 2 in columns (2) –(8). See also Table 2 Notes.

Figure 1: Effect of the Policy on Monthly Paid Absence from Work

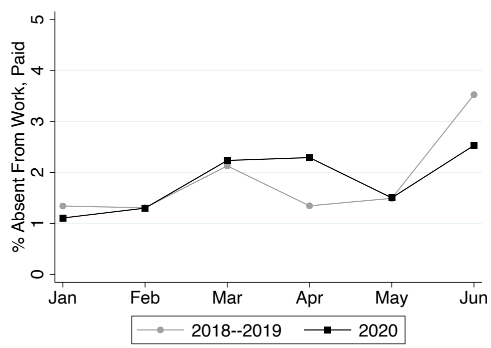
(a) Leave-Taking Rates



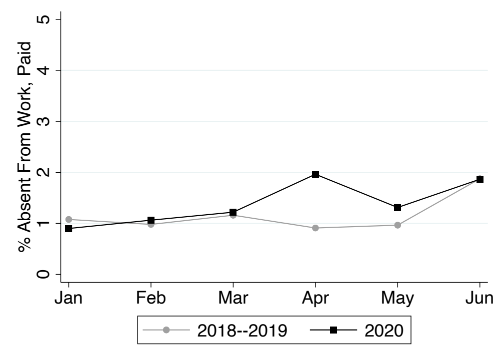
(b) Effect of the Pandemic on Leave Taking



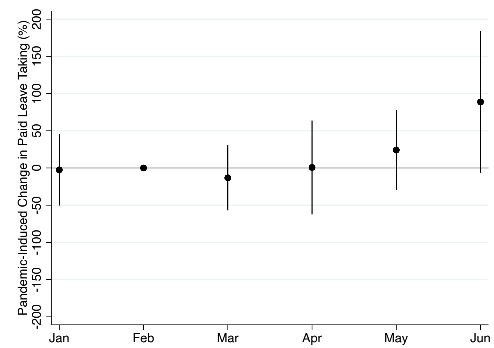
(c) Leave-Taking Rates: Control Firms



(d) Leave-Taking Rates: Treated Firms



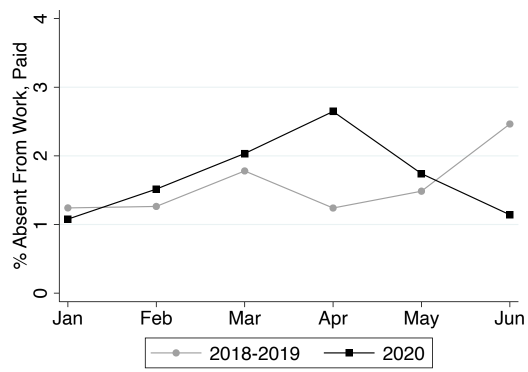
(e) Effect of Policy on Leave Taking



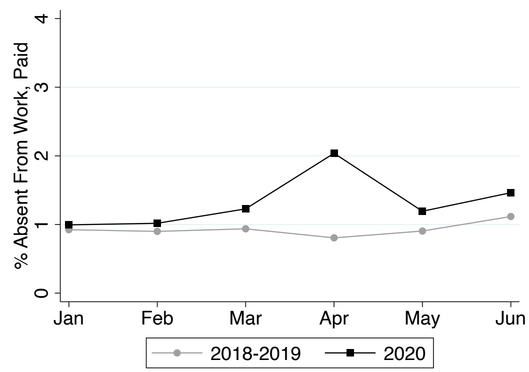
Notes: This figure reports monthly take up of paid leave for those individuals our analysis sample based. See Section 2 for a detailed data description. Panel (a) plots monthly mean take up of paid leave. Panel (b) plots DD estimates—scaled by pre-pandemic average take up—and 95% confidence intervals based Equation 1. Panel (c) plots mean monthly leave taking for individuals employed at large ($\geq$ 500 employees) and small ($<$ 500 employees) Panel (d) plots DDD estimates—scaled by pre-pandemic average take up—and 95% confidence intervals based Equation 2.

Figure 2: Effect of the Policy on Monthly Paid Absence from Work: Telework Compatibility

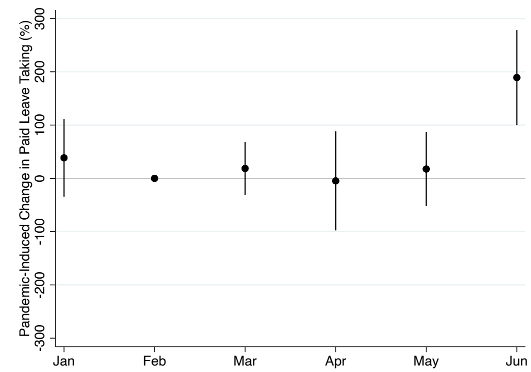
(a) Leave-Taking Rates: Non-Telework Compatible Employees at Control Firms



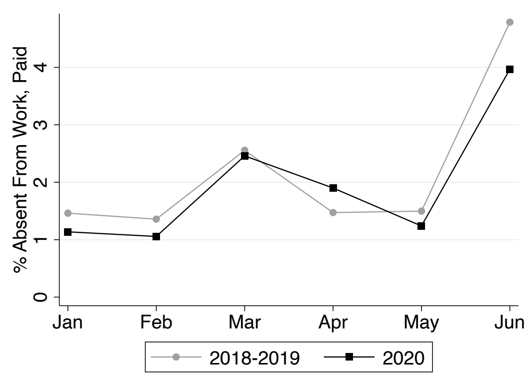
(b) Leave-Taking Rates: Non-Telework Compatible Employees at Treated Firms



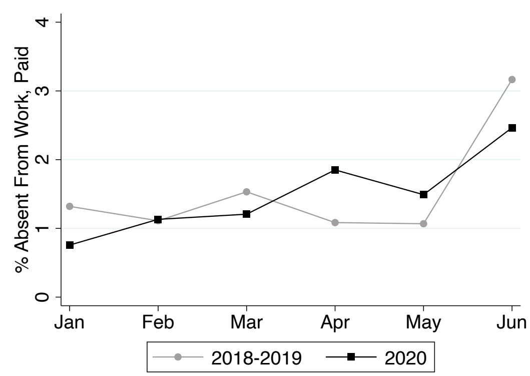
(c) Effect of the Policy on Leave-Taking: Non-Telework-Compatible Employees



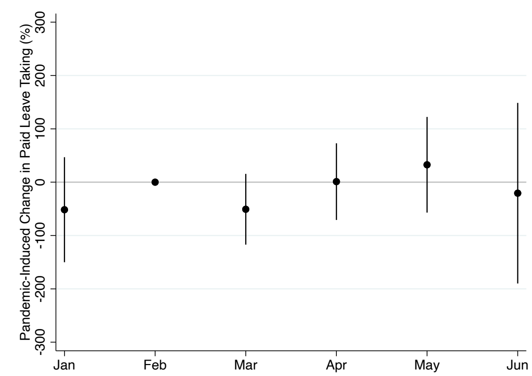
(d) Leave-Taking Rates: Telework Compatible Employees at Control Firms



(e) Leave-Taking Rates: Non-Telework Compatible Employees at Treated Firms

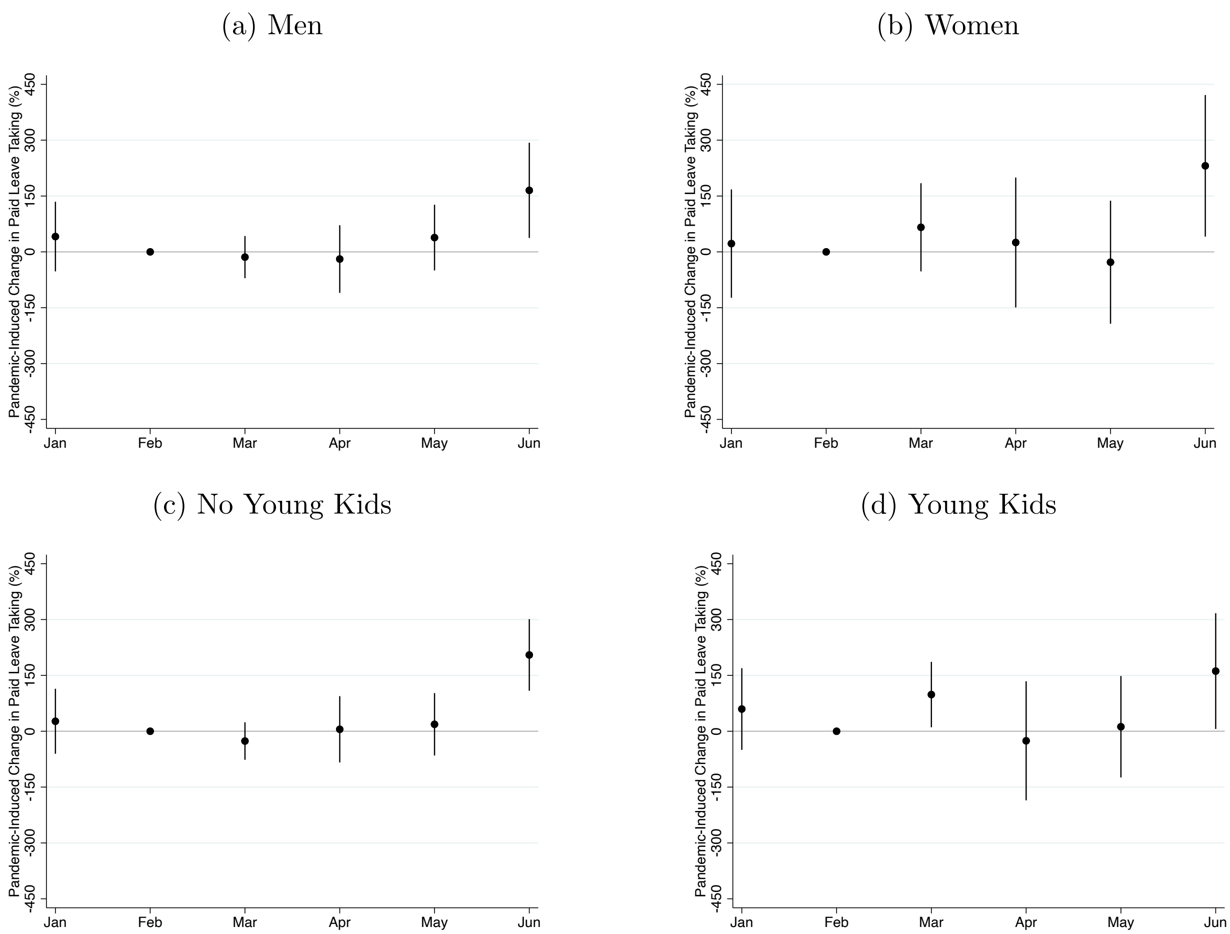


(f) Effect of the Policy on Leave-Taking: Telework-Compatible Employees



Notes: This figure reports monthly take up of paid leave for those individuals our analysis sample based based on the telework-compatibility of their job. See Section 2 for a detailed data description. Panels (a) and (b) plot monthly mean take up of paid leave for employees in non-telework compatible jobs at control firms (more than 500 employees) and treated firms (respectively), respectively. Panel (c) plots the corresponding DDD estimates—scaled by pre-pandemic average take up—and 95% confidence intervals based Equation 2. Panels (d) and (e) plot monthly mean take up of paid leave for employees in telework compatible jobs at control firms (more than 500 employees) and treated firms (respectively), respectively. Panel (f) plots the corresponding DDD estimates—scaled by pre-pandemic average take up—and 95% confidence intervals based Equation 2.

Figure 3: Effect of the Policy on Monthly Paid Absence from Work: Variation by Gender and the Presence of Young Children



Notes: This figure reports our estimate of the effect of access to FFCRA emergency paid leave based on equation 2. Panels (a) and (b) plot estimates for men and women, respectively. Panels (c) and (d) plot estimates for individuals with children under 12 in the household.

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