The Impact of Paid Family Leave on Families with Health Shocks*

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Abstract

We study the impact of paid family leave (PFL) policies on individuals' labor market and mental health-related responses to their spouses' and children's health shocks using data from the Medical Expenditure Panel Survey and event-study models. We find that PFL access improves job continuity among wives of individuals who are hospitalized or have surgery. Additionally, while PFL access does not affect the wives' self-reported mental health, it reduces their use of mental health-related medication. We find no effects of PFL on parents of children who experience health shocks, suggesting that this policy is less fitting for this group of caregivers.

Keywords: paid family leave, family health shocks, mental health

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1 Introduction

Recent years have witnessed heightened public discourse about the lack of a federal paid family leave (PFL) policy in the United States, as the COVID-19 pandemic amplified the challenges of work-family balance for millions of Americans. Like other forms of social insurance—such as health insurance and disability insurance—PFL can insulate a family from the negative consequences of a health event. Yet while PFL refers to paid time off for workers who have two types of caregiving responsibilities—(i) new parents and (ii) caregivers of ill or temporarily disabled family members—there is much more consensus among Americans across the political spectrum in favor of paid leave for the former group than the latter.[1] The potential merits of paid caregiving leave for individuals who are *not* new parents are under debate among politicians, academics, and policy experts as well. For example, a report commissioned by a collaboration between the American Enterprise Institute and the Brookings Institution indicates that while the group of paid leave experts endorses paid parental leave, the "most contentious discussions centered on caregiving leave" (Mathur et al., 2018).[2] One major reason for this lack of consensus stems from the imbalance in the amounts of empirical evidence regarding the two types of leave. Unlike the volumes of studies documenting the effects of paid parental leave on workers and their children (see Olivetti and Petrongolo, 2017; Rossin-Slater, 2018; Rossin-Slater and Uniat, 2019; Rossin-Slater and Stearns, 2020 for some overviews), the research on paid leave for households who experience non-childbirth-related health shocks is comparably less extensive.

This paper contributes to filling this gap by studying the impact of the implementation of state-level PFL policies in California and New Jersey on individuals' labor market and mental health-related responses to their spouses' and children's health shocks. We use data from the restricted-use version of the Medical Expenditure Panel Survey (MEPS) over years 1999–2013,[3] with information on individuals' states of residence, employment status, and

[1]See, for example, the polls discussed here: `https://www.newamerica.org/better-life-lab/blog/polling-summary-paid-family-and-medical-leave-is-one-of-the-most-popular-planks-in-the-build-back-bett`

[2]Current US president Donald Trump has occasionally indicated support for paid parental leave, but not for paid caregiving leave (see, e.g., `https://19thnews.org/2024/10/harris-trump-voters-paid-parental-sick-leave-policy/`).

[3]As of January 2025, thirteen states and Washington, D.C., have implemented or passed PFL legislation. California and New Jersey were the first two states to do so in 2004 and 2009, respectively, and we can

the precise timing of the health shocks of their spouses and children (Agency for Healthcare Research and Quality, n.d.). We study hospitalizations and surgeries (which can occur in emergency room, inpatient, or outpatient settings) as our measures of health shocks.

The MEPS panel structure allows us to study changes in individuals' outcomes from before to after their spouse or child experiences a health shock. To address concerns regarding bias in difference-in-differences models due to staggered treatment timing and heterogeneous effects (De Chaisemartin and d'Haultfœuille, 2020; Goodman-Bacon, 2021; Borusyak et al., 2021; Sun and Abraham, 2021; Athey and Imbens, 2022; Roth et al., 2023), we use the Callaway and Sant'Anna (2021) event-study models to compare the differences in these pre-to-post-shock changes in outcomes between spouses and parents surveyed before and after PFL implementation, relative to the differences among those in a control group of states that are never treated over the same period. Our regression models include controls for individual and family characteristics, as well as state and year fixed effects. Importantly, in contrast to recent work focused on estimating the causal impacts of health shocks on family members' outcomes (e.g., Fadlon and Nielsen, 2019; Frimmel et al., 2020; Aouad, 2021; Fadlon and Nielsen, 2021; Adhvaryu et al., 2022), our research design does not hinge on the health shocks being exogenous or unanticipated—indeed, we expect that PFL is also useful for anticipated health events, such as scheduled surgeries, when individuals face a caregiving need during recovery. Instead, identification in our setting relies on an assumption that in the absence of PFL, the difference in the evolution of average outcomes measured before and after the shock is the same across PFL and non-PFL states. That is, we allow for PFL and non-PFL states to have different time trends, as long as the difference in time trends is the same before and after the health shock.

Our results indicate that access to PFL improves the employment continuity of women whose spouses experience health shocks. Specifically, we find that among the (healthy) wives in these households, the increase in the probability of reporting leaving a job "to care for

observe individuals in the four years before and four years following PFL implementation in our MEPS data. Rhode Island introduced PFL in 2014, and while we could in principle use MEPS data for the four following years, the sample sizes in Rhode Island are too small to yield meaningful estimates. New York introduced PFL in 2018, which means that the post-treatment window of four years following the policy coincides with the start of the COVID-19 pandemic, complicating this potential analysis. The remaining states introduced PFL too recently to be able to study with the currently-available MEPS data.

home or family" is 6.3 percentage points lower in states and years where PFL is available. This magnitude represents just over half of a standard deviation (SD) of the variation in this differenced outcome in our sample.[4] We note, however, that this estimate is not very precise, and the 95% confidence interval ranges from -0.6 to -12.0 percentage points (or 0.06 to 1.1 SD).

Turning to the mental health impacts on spousal caregivers, we first note they are theoretically ambiguous. On the one hand, if access to PFL increases the amount of time they spend engaging in care work, then we might expect a deterioration in mental health due to known associations between caregiving and depression, anxiety, and stress (e.g., Schulz and Sherwood, 2008). On the other hand, if PFL lowers the likelihood that a caregiver must quit her job in order to take care of her spouse, then it may reduce stress and increase financial stability in the household, thereby improving her mental health. At the same time, irrespective of any potential impacts on underlying mental health, caregivers could experience changes in their utilization of mental healthcare if, for example, they have less time to obtain care for themselves. Our empirical results indicate that while there is no strong impact of PFL on self-reported mental health, women caregivers have a 16.6 percentage point lower difference in the likelihood of using mental health medications between the post-shock and pre-shock survey rounds, which represents about 0.62 SD of the differenced outcome (that ranges from -1 to 1). This effect is driven by both an increased likelihood of women stopping using mental health prescriptions after their spouses' health shocks and a decreased likelihood of them starting any mental health medication. As with the result on job continuity, we have limited statistical power to yield a precisely estimated coefficient. The confidence interval for the change in the likelihood of using any mental health-related medication ranges from -1.3 to -31.9 percentage points (or 0.05 to 1.2 SD). So while the estimate is statistically different from zero, we cannot exclude either very small or very large negative effects. That said, this finding suggests that women caregivers may be delaying their mental healthcare when engaging in care work while on PFL.

[4]We scale our coefficient by the standard deviation instead of the mean because the differenced outcome value ranges from -1 to 1, making the mean mechanically close to zero. The standard deviation reflects the amount of variation in the pre-to-post-shock changes in the outcome in the sample. We measure the changes as the average in the post-shock survey rounds minus the average in the pre-shock survey rounds. There are five survey rounds in total. See Sections 3 and 4 for more details.

We do not find any impacts on the husband caregivers (i.e., men whose spouses experience health shocks). Notably, very few men in our entire spousal caregiver sample report leaving their job to care for home or family, which means there is almost no variation in the outcome for us to study.

When it comes to parents of children who experience a hospitalization or a surgery, we do not find significant impacts on either their job continuity or mental health outcomes.[5] One explanation for these null results is that the PFL policy is less suitable for this group of caregivers. Our data indicate that the majority of children's health shocks are relatively minor and likely do not require an extended period of leave from work for the parents (see Appendix Table A1). Therefore, a policy that provides flexible fully paid leave for a few days (akin to paid sick leave) may be more useful in these situations than the several-week-long partially paid PFL. Alternatively, for cases in which the children's health shocks are severe, it is possible that availability of PFL is not a pivotal factor for parental decisions about working. For example, Adhvaryu et al. (2022) show that in Denmark—a country with generous paid leave policies—a large of share of mothers shift from full- to part-time work or exit the labor force entirely in the aftermath of a child's cancer diagnosis.

We conduct several heterogeneity analyses based on worker and employer characteristics. We find that the effects on job continuity following a spousal health shock are concentrated among women with 12 or fewer years of education, and are mostly non-existent among those with higher educational attainment. Given that less educated women in our sample are twice as likely to report leaving their jobs to care for home or family after their spouse has a health shock compared to women with more than 12 years of schooling, our results suggest that PFL might play an important role in closing this gap. Further, the stronger impact of state PFL programs on less advantaged workers is consistent with other data pointing to substantial socioeconomic inequities in access to employer-provided paid leave.[6]

[5]As we note in Section 3, we are unable to measure family relationships in the data. Thus, we examine the outcomes of adults residing in households in which a child experiences a health shock. While the vast majority of these individuals are likely to be parents, some of them may be grandparents, or aunts and uncles. For simplicity, we refer to these individuals as "parents" throughout.

[6]Only 14 percent of workers in the bottom quartile of the US wage distribution have access to PFL from their employers, while 41 percent of workers in the top quartile do. See: US Bureau of Labor Statistics, National Compensation Survey, March 2023, `https://www.bls.gov/charts/employee-benefits/percent-access-paid-leave-by-wage.htm`. See also Bartel et al. (2019) for evidence on racial and ethnic

Thus, government-provided PFL may serve as a substitute for employer-provided paid leave benefits, thereby reducing socioeconomic inequities in access to paid leave among spousal caregivers.[7]

We also find that the effects on job continuity are larger for women working in firms with 50 or more employees, who are more likely to be eligible for federally-guaranteed job protection while on leave.[8] Additionally, the labor market effects are driven by the wives of individuals who have a medical limitation or condition (and also experience a health shock), consistent with this group being in particular need of access to caregiving leave. Lastly, both the labor market and mental health effects appear to be stronger in California than in New Jersey. While there are many potential reasons for these differences in impacts across states, one explanation is that PFL implementation in New Jersey coincided with the Great Recession, which could have muted its effects.[9]

Our paper contributes to the large literature on PFL policies, which, until recently, primarily focused on the outcomes of new parents and their children. Nearly all of the U.S. evidence comes from studies of California's first-in-the-nation PFL program, documenting impacts on maternal and paternal leave-taking and labor market outcomes, as well as child and maternal health (Rossin-Slater et al., 2013; Huang and Yang, 2015; Das and Polachek, 2015; Baum and Ruhm, 2016; Byker, 2016; Lichtman-Sadot and Bell, 2017; Bartel et al., 2018; Bullinger, 2019; Pihl and Basso, 2019; Stanczyk, 2019; Bailey et al., 2019; Bana et al., 2020).[10,11]

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disparities in paid leave use.

[7] This finding echoes conclusions from previous studies about PFL policies reducing disparities in leave-taking among new mothers (Rossin-Slater et al., 2013). Related, studies of paid sick leave mandates find reductions in inequities in sick leave use, especially among service-sector workers (Harknett and Schneider, 2022).

[8] The federal Family and Medical Leave Act guarantees job protection to eligible workers while they go on leave. One of the eligibility criteria is employment in a firm with 50 or more employees. See Section 2 for more information.

[9] To attempt to account for the state-specific impacts of the Great Recession, we include controls for state-year unemployment rates in all of our models.

[10] Related, Stearns (2015) analyzes the impact of the 1978 Pregnancy Discrimination Act, which mandated that the five states with temporary disability insurance systems provide partially paid maternity leave for birthing mothers, on infant health. Rossin (2011) studies the impact of the federal Family and Medical Leave Act of 1993, which provides *unpaid* leave to eligible workers, on infant health.

[11] There is also an extensive literature on parental leave from countries outside the U.S., which have much longer leave provisions. For example, some studies find that paid maternity leave has positive or zero effects on maternal employment after childbirth (Baker and Milligan, 2008; Kluve and Tamm, 2013; Bergemann

More recently, the literature has expanded to consider caregivers who are not new parents, focusing on outcomes of individuals with family members who have disabilities, chronic health conditions, or are in self-reported poor health.[12] Kang et al. (2019) use data from the Current Population Survey (CPS) to show that the CA PFL policy increases employment among 45 to 64-year-old women with a family member who has a work-limiting disability. Similarly, Bartel et al. (2023) use data from the American Community Survey (ACS) and find that the CA PFL policy increases the employment rate of 45 to 64-year-old individuals with a disabled spouse. Anand et al. (2022) use data from the Survey of Income and Program Participation (SIPP) and show that PFL policies in CA and NJ increase the likelihood that an individual works full-time after the onset of a work-limiting health condition of their spouse.[13] Braga et al. (2022) use data from the Health and Retirement Survey (HRS) and find that PFL policies in CA and NJ increase employment and reduce the likelihood of depression among women with either a spouse in poor health or with a parent in poor health who lives within 10 miles.

We build on these path-breaking studies in three ways. First, we use the MEPS data, which allows us to precisely identify the timing of health shocks based on encounters with the healthcare system and to study changes in outcomes from before to after an individual's family member experiences a health shock. Our results on women being less likely to leave their jobs to care for their family after their spouse has a health shock in states and years with PFL are consistent with the earlier evidence of increases in women's employment, and provide more direct support for the conjecture that these broad employment effects are in

and Riphahn, 2015; Carneiro et al., 2015; Dahl et al., 2016; Stearns, 2016), while others document negative impacts, especially in the long term (Lalive and Zweimüller, 2009; Lequien, 2012; Schönberg and Ludsteck, 2014; Bičáková and Kalíšková, 2016; Canaan, 2017). Studies that compare across countries suggest that provisions of leave up to one year in length typically increase the likelihood of employment shortly after childbirth, whereas longer leave entitlements can negatively affect women's long-term labor market outcomes (Ruhm, 1998; Blau and Kahn, 2013; Thévenon and Solaz, 2013; Olivetti and Petrongolo, 2017). Studies on fathers' outcomes have largely analyzed so-called "Daddy Month" reforms, which earmark a month (or more) of parental leave to fathers only (see, e.g., Duvander and Johansson, 2012; Ekberg et al., 2013; Duvander and Johansson, 2014, 2015; Avdic and Karimi, 2018; Rege and Solli, 2013; Dahl et al., 2014; Cools et al., 2015; Dahl et al., 2016; Eydal and Gíslason, 2008; Schober, 2014; Bünning, 2015; Patnaik, 2019; Farré and González, 2019; Olafsson and Steingrimsdottir, 2020; Andresen and Nix, 2019; Lappegård et al., 2020).

[12] Another relevant study on non-childbirth-related leave is by Arora and Wolf (2018), who examine the impact of California's PFL policy on nursing home use.

[13] Related, Saad-Lessler (2020) also uses data from the SIPP to show that the CA PFL policy increases the likelihood that an unpaid care provider is in the labor force, with the effect being driven by women and those who are more educated.

fact due to increased job continuity afforded by the availability of caregiving leave.

Second, we analyze caregivers' mental health outcomes, examining the effects of PFL access on both self-reported mental health and the use of mental health-related prescription drugs. We thus build on the growing evidence about the mental health impacts of paid leave among new mothers (Bullinger, 2019; Bütikofer et al., 2021; Persson and Rossin-Slater, 2024) by asking whether individuals experience changes in mental health care and outcomes when paid leave enables them to be caregivers to other family members.[14] Our finding that women caregivers have no changes in self-reported mental health but are both more likely to stop and less likely to start using mental health-related prescription medication after their spouse's health shock might suggest the importance of pairing PFL policies with robust supports for caregivers' mental health care.

Third, we expand beyond caregivers of adults to study parents of children who experience health shocks. Our estimated null effects on their employment and mental health outcomes are consistent with other survey evidence that indicates that parents of children with healthcare needs experience large barriers to taking paid leave (even when they have access to it).[15] Moreover, the frequency of less severe health events among children that arguably necessitate parents to only take a few days off work suggests that PFL—which provides only partially paid leave for six to twelve weeks and involves some administrative hassle to apply—may not be the optimal policy tool for this population.

[14]A few studies have used survey data to analyze associations between taking paid leave for caregiving purposes and measures of economic security, well-being, and mental health (Earle and Heymann, 2011; Goodman and Schneider, 2021). However, other differences between workers who are and are not able to take paid leave make causal inference challenging in these research designs. Gimm and Yang (2016) study the impact of CA PFL on the mental health outcomes of self-reported caregivers in the Health and Retirement Survey, focusing on the Center for Epidemiologic Studies (CESD) depression score as the outcome, and finding no significant effects. However, there are some important limitations in this study as it does not include state fixed effects and does not account for clustering of standard errors to account for serial correlation in observations within individuals and states. Moreover, the study treats 2002 as the first policy year, which is not consistent with the fact that California's policy went into effect in July 2004 (the law was passed in 2002).

[15]In general, there is very limited evidence on the impacts of PFL on parents of children who have health care needs. A few surveys of parents of children with special health care needs in Chicago and Los Angeles indicate that parents who are employed report substantial need for having access to paid leave, but experience a variety of barriers to taking such leave (Chung et al., 2007; Schuster et al., 2008; Chung et al., 2012). Another survey of 585 parents of children with special health care needs who reported taking time off for their child's illness during the prior year indicates that the majority of parents experienced positive effects of taking leave on their own and their child's health, but also had leave-related financial challenges (Schuster et al., 2009).

We also build on a long literature documenting the spillover impacts of health shocks on other family members' outcomes, including labor supply, consumption, and health-related behaviors (Altonji et al., 1989; Cochrane, 1991; McClellan, 1998; Wu, 2003; Coile, 2004; García-Gómez et al., 2013; Dalton and LaFave, 2017; Jeon and Pohl, 2017; Dobkin et al., 2018; Bom et al., 2019; Fadlon and Nielsen, 2019; Frimmel et al., 2020; Aouad, 2021; Fadlon and Nielsen, 2021; Adhvaryu et al., 2022). Most relevant to our paper is a study by Arrieta and Li (2023), who use the MEPS data to show that, following a family member's ED visit, women increase their labor supply while men experience a reduction in wages. Our study suggests that access to PFL may be an important determinant of women's labor market responses to their spouses' health shocks. Finally, our paper is relevant to the literature on caregiving and employment (see Bauer and Sousa-Poza, 2015, for a review), in that PFL has the potential to buffer against the adverse effects of caregiving responsibilities on employment and long-term financial well-being.

2 Background

As noted in Section 1, the United States does not have any federal policy providing paid family leave to workers. The federal Family and Medical Leave Act (FMLA) of 1993 provides twelve weeks of *unpaid* job-protected leave for workers caring for a newborn or newly adopted child, an ill family member, or an own serious medical condition.[16] Only about 56 percent of private sector workers meet the FMLA eligibility criteria, which include having worked for at least 1,250 hours for an employer with 50 or more employees during the 12 months before the start of leave (Brown et al., 2020).

At the state level, five states—California, Hawaii, New Jersey, New York, and Rhode Island—have had Temporary Disability Insurance (TDI) programs since the 1940s and 1950s, which provide partially paid leave to workers who need time off due to an own temporary disability or illness. Since the Pregnancy Discrimination Act of 1978, these programs have been required to cover birthing individuals who are preparing for and/or recovering from childbirth.

[16] Job protection refers to an employee's right to return to the same or equivalent job after taking leave.

In 2004, California became the first state in the nation to implement a paid family leave program, which initially provided new parents and caregivers of ill family members with six weeks of leave at a 55 percent wage replacement rate (up to a maximum weekly benefit amount, which varies slightly every year).[17] As of 2025, thirteen additional states and Washington, D.C., have either passed or implemented PFL legislation: New Jersey (in 2009), Rhode Island (in 2014), New York (in 2018), D.C. (in 2020), Washington (in 2020), Massachusetts (in 2021), Connecticut (in 2022), Oregon (in 2023), Colorado (in 2024), Kentucky (in 2024), Maryland (will go into effect in 2026), Delaware (will go into effect in 2026), Minnesota (will go into effect in 2026), and Maine (will go into effect in 2026). These policies are all similar in that they have minimal eligibility requirements—and thus near-universal coverage—and provide partially paid leave for at least two categories of caregivers: those with newborn or newly adopted children and those with ill family members. Policies in states without pre-existing TDI programs also offer leaves for workers' own temporary disabilities or illnesses. Most of these programs are funded by employee payroll taxes. The policies vary substantially in terms of other key parameters, including statutory leave duration, the wage replacement rate, the maximum weekly benefit amount, and the presence or lack of job protection.[18]

We have access to data spanning years 1996–2020, a period that covers the introduction of PFL in four states: California, New Jersey, Rhode Island, and New York. However, we drop households residing in Rhode Island because we have too few observations in MEPS, and drop households residing in New York because its post-period coincides with the beginning of the COVID-19 pandemic in 2020. Therefore, we focus our attention on the impacts of PFL implementation in California and New Jersey.[19] We focus on studying the (healthy) caregivers rather than individuals who need leave for their own illness because both of our analysis states had a pre-existing TDI program at the time of PFL implementation. Thus,

[17]As of 2025, California's PFL policy provides 8 weeks of leave with 70-90 percent of wages replaced, up to a maximum weekly benefit of $1,681.

[18]See `https://www.abetterbalance.org/resources/paid-family-leave-laws-chart/` for an up-to-date chart with details about all current state PFL policies.

[19]As noted above, at the time of implementation, CA-PFL provided six weeks of leave at a 55 percent wage replacement rate, up to a maximum weekly benefit of $728; NJ-PFL provided six weeks of leave with a 66 percent wage replacement rate, up to a maximum weekly benefit of $524. CA-PFL does not have job protection, while NJ-PFL does.

there was no major policy change in the existence of state-provided paid leave for an own health shock during our analysis time frame.

3 Data and Sample

We use data from the restricted-use version of the Medical Expenditure Panel Survey (MEPS) from the Agency for Healthcare Research and Quality, which contains state of residence identifiers. Since 1996, the Household Component survey of MEPS has collected detailed information about the demographic and socioeconomic characteristics, medical conditions, and labor market outcomes of every member of a household in five rounds of interviews over a two-year panel. Each survey panel is designed to capture a representative sample of the U.S. population.

MEPS also collects data on each household member's engagement with the health care system in each round of the panel in the Hospital Inpatient Stay, Emergency Room Visit, and Outpatient Visit event files. We use these files to construct our measure of a health shock: an indicator for experiencing either an inpatient stay or a surgery (in an emergency department, inpatient, or outpatient visit setting). We exclude visits related to pregnancy, birth, or pre- or post-natal maternity care from our analysis. To study how having access to PFL might affect a potential caregiver's mental health, we also use the MEPS Prescribed Medications event files. These files contain U.S. Food Drug and Administration National Drug Codes, which we map into Anatomical Therapeutic Chemical (ATC) Level 5 codes, which can be used to identify the conditions that every drug is typically used to treat.[20] We are thus able to measure the utilization of all mental health-related prescription drugs, as well as prescriptions that are used to treat anxiety and depression specifically.

Analysis Samples. To allow for four years of pre- and post-policy observations in both CA and NJ, we pool all panels of data covering the years 1999 to 2013. We drop individuals who move states during the course of the two-year panel.[21]

[20] We use the NDC-ATC5 crosswalk available here: `https://github.com/fabkury/ndc_map`.

[21] There are a total of 44 individuals in our analysis sample who move across states within any of the two-year MEPS panels.

To study spousal caregivers, we limit our analysis to survey respondents who meet the following three conditions (N=3,870): (i) they are between the ages of 25 and 64 in the first round of the panel, (ii) they report either being employed and at work or that they have a job to return to in the first round of the panel, and (iii) they have a spouse who experiences a health shock after the round 1 interview and before the round 5 interview. To focus on potential caregivers (rather than people who may need paid leave for their own health issues), we additionally drop the 851 individuals who experience an *own* emergency department visit, hospitalization, or surgery in any round of the panel.

To study parent caregivers, we consider individuals who meet criteria (i) and (ii) above, and who additionally have a child under the age of 18 who resides in the same household and experiences a health shock between round 1 and round 5. Note that the MEPS data do not have information on family relationships—i.e., we cannot directly link parents to their own children. Thus, technically, our sample may include not only parents, but also other adults in the household (e.g., grandparents or aunts/uncles). Since households can have multiple children with a health shock during the panel, we construct this sample as a set of adult-child pairs. For simplicity, we often refer to the adult individuals as "parents" throughout the paper, although we acknowledge that our analysis is likely to include other adults residing with children (who also potentially have caregiving responsibilities).

For both of our analysis samples, we study changes in caregivers' outcomes from before to after the shock, as described in more detail below. We collapse the data to a cross-section with one observation per individual and measure control variables using the first round of each panel. Our main spousal analysis sample consists of 3,019 individuals with spouses who experience a health shock, while our main child analysis sample consists of 2,341 adult-child pairs of individuals who are in the same household as a child under 18 who experiences a health shock. As we discuss further below, we conduct our analyses separately by caregiver gender. We have 1,291 spousal caregivers who are women, and 1,041 women caregivers of children who experience health shocks.

Labor Market Outcomes. In the MEPS Household Component file survey, all respondents aged 16 and older are asked their current employment status in each round of the panel. There are four mutually exclusive responses that respondents can provide to describe their employment status as of the interview date: (A) "employed," (B) "have a job to return to" (e.g., on leave or furlough), (C) "had a job" (i.e., worked during the round but did not have a job at the time of the interview date), and (D) "not employed" (did not work during the round and does not have a job to return to). Those who are "not employed" in the current round but were previously employed are asked to report the reason why they are not working.

We use this information to construct three mutually exclusive binary labor market outcomes for each individual in each round: (1) employed, (2) left a job to care for home or family, and (3) left a job for all other reasons. Our first indicator is set to 1 if an individual reports being employed or having a job to return to in that round (options A or B). Our second indicator is equal to 1 if an individual reports being out of work at some point in that round (options C or D) *and* they respond to the question about the reason for not working with the answer "to care for home or family." Our third indicator is equal to 1 if an individual reports being out of work at some point in that round (options C or D), and they either are not asked the question about the reason for not working or they provide another reason, including: "could not find work," "retired," "unable to work because ill/disabled," "on temporary layoff," "maternity/paternity leave," "going to school," "wanted some time off," "waiting to start new job," and "other."[22]

In supplementary analyses, we also examine intensive margin labor market outcomes. These include the reported usual hours worked per week at an individual's current main job, as well as the hourly wage (in 2019 dollars) for all individuals who are not self-employed.[23] Using the number of hours worked and the hourly wage, we also calculate the weekly income.

[22]In some cases, we observe individuals who respond that they "had a job" in one round (option C), and then are "not employed" in the subsequent round (option D). Since the reason for being out of work is not asked with option C, we code the reason for leaving as reported in the subsequent "not employed" round (option D). If an individual who "had a job" in one round (option C) returns to employment by the next round (option A or B), then we code that individual as "leaving a job for all other reasons" in the round in which they are not employed.

[23]Self-employed individuals do not report an hourly wage. Hourly wages in each panel of the Household Component survey are top-coded.

Mental Health Outcomes. We consider both self-reported mental health status and the use of mental health-related prescription drugs. The self-reported mental health outcome is an indicator equal to 1 if an individual states that their mental health is either poor or very poor (a value of 4 or 5 on a 1–5 scale) in the Household Component survey. This question is asked of all survey respondents in every round. We also construct an indicator for using a prescription drug to treat any mental health condition, as well as an indicator for using a prescription drug to treat anxiety or depression specifically, in each round.

Descriptive Statistics. Table 1 presents means of selected characteristics of our main spousal and child shock analysis sample.[24] Columns (1) and (2) present the means of selected characteristics for women and men spousal caregivers, respectively, while columns (3) and (4) present the means for women and men child caregivers (i.e., mothers and fathers, mostly), respectively. All of the reported variables are measured in the first round of each panel.

Within the sample of spousal caregivers, the average age is 47.7 years for women caregivers and 45.5 for men caregivers. The average number of children residing in the household is 0.7 for women caregivers and 1.0 for men caregivers. About 14.7 percent of women spousal caregivers are Hispanic, 10.1 percent are non-Hispanic Black, and 64.8 percent are non-Hispanic white, while 23.0 percent of men spousal caregivers are Hispanic, 11.7 percent are non-Hispanic Black, and 56.5 percent are non-Hispanic white. About 13.0 percent of women spousal caregivers has 11 years or less of education, 59.3 percent has 12 to 15 years of education, and 27.7 percent has 16 or more years of education. Among men spousal caregivers, 18.8 percent has 11 years of less of education, 54.9 percent has 12 to 15 years of education, and 26.2 percent has 16 or more years of education. The bottom panel presents the distribution of medical conditions and limitations among spouses.[25] About 72 percent of the individuals in our sample have a spouse who has at least one medical condition or limitation. The most common condition category—affecting 58.5 percent of spouses of women caregivers—is diabetes, cholesterol, or high blood pressure. 33.2 percent of spouses

[24] The sample sizes reported in Table 1 and used in the estimation of our event-study models are slightly smaller than those reported above due to missing values for some outcomes. The summary statistics table is constructed based on the sample of individuals who are not missing our key outcome of "leaving a job to care for home or family".

[25] Note that the shares do not add up to 100 percent since a respondent can have more than one condition.

of women caregivers have heart or lung conditions, 30.8 percent have arthritis, 9.5 percent have asthma, and 5.9 percent have cancer. In terms of limitations, 33.7 percent of spouses of women caregivers report having a physical limitation, while 11.1 percent report a cognitive limitation.

Table 2 presents the 20 most frequently occurring ICD-9 codes associated with spousal health shocks in our main analysis sample for years 1999 to 2012, when these codes are available.[26] These diagnoses account for about 33.8 percent of all health shocks (i.e., inpatient stays and surgeries in any hospital settings) in the sample. Note that, in our sample, 53 percent of spousal health shocks are inpatient stays that also involve surgeries, 31 percent are inpatient stays that do not involve surgeries, and 16 percent are surgeries in the emergency department or an outpatient setting. The table makes clear that the health shocks we study are quite varied in nature, ranging from heart attacks to pneumonia to joint issues.

Columns 3 and 4 of Table 1 presents summary statistics for our sample of parent caregivers. Compared to spousal caregivers, these individuals are younger (average age is 36.8 years for mothers and 37.7 years for fathers) and have more children (average number of children is 2.3 among mothers and 2.4 among fathers). The bottom half of the table presents rates of medical conditions and limitations recorded in the MEPS data for children in this sample. The rates for most conditions are quite low, with the exception of asthma, which roughly 16.7 percent of children in the sample have. Lastly, Appendix Table A1 presents the 20 most frequently occurring ICD-9 codes associated with child health shocks. These account for about 51.9 percent of all health shocks in the sample. Wounds and injuries are fairly common, but the health shocks we study also include infections, respiratory conditions, and appendicitis. In our sample, 42 percent of child health shocks are surgeries in the outpatient or emergency department setting, 35 percent are inpatient stays without surgeries, and 23 percent are inpatient stays that involve surgeries.

[26] MEPS stopped collecting ICD-code information in the Hospital Inpatient Stay, Emergency Room Visit, and Outpatient Visit event files after 2012.

4 Empirical Design

To measure the effects of access to PFL on the labor market and mental health responses of individuals whose spouses or children experience health shocks, we leverage the state-year variation in PFL access in event-study models. As noted in Section 3, we collapse our panel data into an individual-level cross-sectional data set, in which outcome changes are measured as differences between the averages over values in post-health-shock rounds and averages over values in pre-shock rounds. Thus, we build on the prior literature examining caregiving leave with similar research designs in other data sets (Kang et al., 2019; Anand et al., 2022; Braga et al., 2022; Bartel et al., 2023), except that we use analysis samples in which all individuals experience spousal or child health shocks during the course of the survey panel, and we measure *changes* in individuals' outcomes from before to after the shock.

Specifically, for every outcome, we calculate the change from before to after the shock as the difference between the average of the values in the post-health-shock rounds and the average of the values in the pre-shock rounds (i.e., $\Delta y = \overline{y}_{post} - \overline{y}_{pre}$). For example, consider an individual whose spouse's inpatient stay occurs between the round 2 and round 3 interview dates. We measure the caregiver's post-shock value ($\overline{y}_{post}$) as the average across observations in rounds 3 to 5, and the caregiver's pre-shock value ($\overline{y}_{pre}$) as the average across the observations in rounds 1 and 2. The dependent variable is then constructed as the difference between these two averages. Since our primary outcomes are all binary indicators in each round, the differenced dependent variables used in our regression models range from -1 to 1.

To study the impact of PFL policies on the differenced outcomes of the spouses of individuals who experience health shocks, consider the following event-study model:

$$\Delta Y_{ist} = \alpha + \sum_{k=-4}^{k=4} \beta_k \mathbf{1}[t - PFL^{*}_{st} = k] + \psi' X_i + \zeta u_{st} + \eta_t + \gamma_s + \varepsilon_{ist} \tag{1}$$

for individual i residing in state s and observed in year t. ΔY_{ist} is the change in an outcome of interest, such as the change in the probability that the individual leaves their job to care for home or family, measured as the average of the "job leaving" indicators in the post-shock rounds minus the average of the indicators in the pre-shock rounds. The event-time indicators, $\mathbf{1}[t - PFL^*_{st} = k]$, reflect the year relative to state PFL adoption. The following individual and family characteristics measured in the first round of the panel are included in X_i: indicators for race/ethnicity (non-Hispanic Black, non-Hispanic white, Hispanic, other), indicators for education level (0 to 11 years, 12 to 15 years, 16 years or more), age (in 10-year intervals), and the number of children under age 18 in the household. We additionally include indicators for the type of spousal health shock experienced (inpatient stay or a surgery in any hospital setting) and for whether the spouse has any medical condition or cognitive or physical limitation. u_{st} is the unemployment rate in each state and year. We control for calendar year fixed effects, η_t, which account for aggregate trends in outcomes, and state fixed effects, γ_s, which account for all time-invariant differences between states.[27]

The PFL policy implementation that we study occurs in two states over the course of a fairly long time period. Therefore, we follow the recent literature raising concerns about potential bias in difference-in-differences (DD) models with staggered treatment timing and possible violations of the "parallel trends" assumption (e.g., De Chaisemartin and d'Haultfœuille, 2020; Goodman-Bacon, 2021; Borusyak et al., 2021; Sun and Abraham, 2021; Athey and Imbens, 2022; Roth et al., 2023), and use the Callaway and Sant'Anna (2021) (CS) event-study estimator.

The approach proposed by Callaway and Sant'Anna (2021) is implemented as follows. First, we compute each feasible 2×2 DD combination to obtain the average treatment effect $ATT(g,t)$ for each treated group g (that is first treated in year g) and calendar year t. In our setting, we measure the impacts of two states' PFL policy enactments (i.e., $g = 2004$ for CA and $g = 2009$ for NJ) using data covering years 1999–2013. To measure the effect of California's PFL policy implementation in 2004, we compare outcomes between individuals in CA and in the control states—that are never-treated—observed in each pair of pre-treatment years (i.e., between 1999 and 2000, 2000 and 2001, 2001 and 2002, and 2002 and 2003), as well as between those observed in the first pre-treatment year (2003) and in each post-treatment

[27] While the panel structure of MEPS would theoretically allow us to also include individual fixed effects as in prior studies of family health shocks (e.g., Coile, 2004; Fadlon and Nielsen, 2019; Aouad, 2021; Fadlon and Nielsen, 2021; Arrieta and Li, 2023), this is challenging in our context because MEPS panels are relatively short (2 years) and our key treatment variation—PFL availability—affects a fairly small share of the sample, limiting the statistical power of our analyses.

year (beginning with 2004). We repeat the same procedure to measure the effect of New Jersey's PFL policy implementation in 2009 by comparing outcomes between individuals in NJ and in the control states in each pair of pre-treatment years, as well as between individuals in the first pre-treatment year (2008) and in each post-treatment year (2009 onward). We then aggregate the group-time effects in an event-study plot that uses an event window spanning the period of four years before, the year of, and four years after treatment.

We report the dynamic treatment effects using CS event-study plots, along with a single post-treatment estimate that aggregates the treatment effects in years 0–4 following policy implementation. In our main analysis, we compare outcomes between treated states relative to never-treated states. In sensitivity analyses, we also present results when using both never-treated and not-yet-treated states as the control group. Since we include covariates in equation (1), we use the doubly robust estimator proposed by Sant'Anna and Zhao (2020). We cluster standard errors on the state level.

When studying adults who are in the same household as a child who experiences a health shock, we evaluate the impacts of each household child's health shock on the outcomes of each adult individual in the household (i.e., for each adult-child pair in a household). To obtain the $ATT(g,t)$s in our analysis of child health shocks, we estimate similar specifications as in equation (1), except that the control vector X_i in equation (1) additionally includes an indicator for the individual's marital status.

5 Results

Below, we present our results on the effects of PFL on the labor market and mental health-related responses to family members' health shocks. We begin with the results for spouses, and then present results for parents.

5.1 Effects on Spouses

Figure 1 presents results for the wives of individuals with health shocks from the CS event-study estimator for our three labor market and three mental health-related outcomes, expressed as pre-to-post-shock changes in: (a) the probability of being employed, (b) the probability of leaving one's job to care for home or family, (c) the probability of leaving one's job for all other reasons, (d) the probability of reporting poor mental health, (e) the probability of taking any mental health-related medication, and (f) the probability of taking any anti-anxiety or antidepressant medication. Below each graph, we report the pre-treatment estimate and standard error, the post-treatment estimate and standard error, the standard deviation of the differenced outcome, and the mean of the post-shock level of the outcome. The pre-treatment estimate allows us to assess the parallel trends assumption in the pre-period, while the post-treatment estimate reflects the average treatment effect over the five years from the year of policy implementation. The standard deviation of the differenced outcome is used to benchmark the magnitude of the treatment effect (as the mean is mechanically close to zero due to each outcome ranging from -1 to 1). The post-shock outcome level mean reflects the average rate of each outcome observed in the post-shock rounds of the panel.

We find that PFL access leads to a statistically significant decrease in the change in the probability of wives leaving their jobs to care for home or family (Figure 1b). Specifically, the wives of spouses who have a health shock have a 6.3 percentage point lower increase in the probability of this event when PFL is available, which represents about 0.57 SD of this differenced outcome. For context, about 2.4 percent of women report leaving their job to care for home or family after their spouse's health shock in our sample. We emphasize that our post-treatment estimate is not precise, with a 95% confidence interval that ranges from -0.6 to -12.0 percentage points (or 0.06 to 1.1 SD). We do not find statistically significant effects on our other two labor market outcomes (Figures 1a and c).

With regards to mental health, it appears that there is no significant change in the likelihood of reporting poor mental health (Figure 1d), along with a large and significant decline in the likelihood of taking any mental health-related prescriptions (Figure 1e), including those treating anxiety and depression (Figure 1f). Specifically, we find that the wives of individuals who have a health shock have a 16.6 percentage point lower change in the probability of taking any mental health-related medication, a magnitude reflecting 0.62 SD in this differenced outcome. As with the coefficient for job continuity, the post-treatment estimate in this model is not very precise, with a 95% confidence interval ranging from -1.3 to -31.9

percentage points (or 0.05 to 1.2 SD). In additional analyses, we separately considered binary indicators that captured women who shift from not taking medication to taking medication after the health shock and those who shift from taking medication to not taking it anymore. We find that both of these changes contribute to the overall effect on the differenced outcome.

Appendix Figure A1 presents results for our three intensive margin labor market outcomes for women: the pre-to-post shock changes in hours worked, the real hourly wage, and real weekly income (in 2019$). We do not observe any significant impacts on any of the outcomes, suggesting that PFL mostly influences the extensive margin of labor market participation among women caring for their spouses.

Sensitivity analyses. We conduct several additional analyses testing the sensitivity of our estimates across specifications and samples. We focus on our three key outcomes—change in leaving a job to care for home or family, change in reporting poor mental health, and change in the likelihood of taking any mental health medications—for the women caregiver sample. Appendix Figure A2 uses a stacked event-study model, where each state's policy change is treated as a separate experiment with experiment×year and experiment×state fixed effects, while Appendix Figure A3 uses the Wooldridge (2021) estimator, which makes a stronger assumption about parallel trends than the Callaway and Sant'Anna (2021) estimator but is more efficient. Appendix Figure A4 uses both never-treated and not-yet-treated states in the control group, while Appendix Figure A5 drops states that border treatment states from the control group.[28] The latter specification addresses the concern that eligibility for PFL is determined by the state of one's employer (rather than the state of residence), and dropping individuals living in states bordering those with PFL (who may commute to these states for work) creates a cleaner control group. The results are largely consistent across these different models, with the exception of the result for self-reported mental health in the stacked event-study—the treatment coefficient is negative (whereas it is positive in all other models), but seemingly due to a significant pre-trend in this particular specification.

[28] The border states include states that border Rhode Island and New York.

Effects on men. Figure 2 plots CS post-treatment coefficient estimates and associated 95% confidence intervals from estimating separate models for different sub-samples of our data. We present the results for men whose spouses experience health shocks at the top of each of the sub-figures. We do not find any statistically significant impacts of PFL on men caregivers. We note that there is very limited variation in our key outcome of "leaving a job to care for home or family"—very few men with spouses who have a health shock have a value greater than zero for this outcome.

Heterogeneity in effects of PFL on women. The other rows of Figure 2 investigate heterogeneous treatment effects for different subgroups of women (with the effect in the overall women sample reported in the second row). We consider separate impacts for each of the two states, those with low (0-12 years) and high (13 years or more) education levels, those whose spouses do and do not have a medical condition or limitation, those whose employers do and do not provide paid sick leave benefits, and those employed in firms who have less than 50 or 50+ employees. Paid sick leave benefit provision is designed to proxy for whether employers might offer their own PFL benefits (we unfortunately do not have direct information on employer provision of PFL). The split by firm size is designed to capture differences in effects by FMLA eligibility, which would provide job protection for workers taking state-level paid leave. The spousal medical condition is intended to focus on those workers whose spouses may have had greater caregiving needs following their health shock.

When we focus on the change in leaving a job to care for home or family as the outcome in sub-figure (a), we observe some clear patterns of heterogeneity. First, the effect appears stronger in California than in New Jersey. While there are many plausible explanations for this difference in effects across states, it is possible that the fact that New Jersey's PFL policy implementation coincided with the Great Recession is a factor.

Second, the impact of PFL access on job-leaving is concentrated among women with up to 12 years of education, and is non-existent for those with more years of education. The post-treatment estimate is a statistically significant 17.5 percentage point reduction in the change in this event among less-educated women, and an insignificant 0.1 percentage point increase for the higher-educated group. Given that less educated women are about twice as likely to report leaving their jobs to care for home or family after their spouse's health shock compared to higher-educated women, this result suggests that PFL may significantly reduce this disparity in job continuity.

Third, the effects of PFL access on job leaving for caregiving reasons are larger among those whose spouse has a medical condition or limitation. This might reflect the fact that these spouses have the greatest caregiving needs.

Fourth, we see significant differences by employer size. Specifically, the reduction in the likelihood of job leaving is stronger among women at firms with 50 or more employees, suggesting that they are more likely to benefit from state PFL when they simultaneously qualify for job protection under the FMLA.

There is less heterogeneity in the effects on women's mental health outcomes in sub-figures (b) and (c). It appears that there is an increase in the likelihood of reporting poor mental health when we only focus on California. The coefficients for the change in the probability of taking mental health-related medications are almost entirely in the same direction and with overlapping confidence intervals across the sub-groups.

5.2 Effects on Parents

We present the CS event-study graphs for our sample of parents whose children experience health shocks, using the overall sample in Appendix Figure A6 and mothers (or, more specifically, adult women with children living with them) in Appendix Figure A7. Only one of the post-treatment coefficient estimates reported across the 12 graphs is statistically significant—among women, there is a higher increase in the likelihood of reporting poor mental health after the health shock. However, overall, these results are weaker than those for spousal caregivers.[29]

While our data do not allow us to perfectly understand why parents of children who have health shocks seem less affected by PFL access than spousal caregivers, one conjecture is that these families are less likely to use paid leave even if it is available. Many of the children's health shocks that we observe a relatively minor and do not require an extended

[29] Additionally, in Appendix Figure A7, we are unable to obtain an estimate at event-time 3 due to insufficient sample size in this sample.

period of leave from work.[30] At the same time, for situations with very severe health shocks, it is possible that availability of PFL does not affect parental decisions regarding changing their labor force status (e.g., if a child has a leukemia diagnosis, perhaps one parent will exit the labor force or work part-time regardless of whether they have PFL access or not).

6 Conclusion

This study examines the impact of paid family leave policies on the labor market and mental health outcomes of working-age adults following spousal and child health shocks unrelated to childbirth. Our analysis builds on a burgeoning literature exploring PFL impacts on caregivers who are not new parents; much of the prior evidence focused on parental leaves following the birth of a child.

We use data from the restricted-use Medical Expenditure Panel Survey (MEPS) covering years 1999–2013, and focus on employed working-age spouses of individuals who experience either a surgery or a hospitalization during the course of the panel. Additionally, we study employed working-age adults with children in the household who are under age 18 and experience a surgery or a hospitalization. We analyze the impacts of PFL access in California and New Jersey using the Callaway and Sant'Anna (2021) event-study approach.

We find that PFL access supports employment by increasing job continuity for the wives of individuals who have a health shock. To our knowledge, our study is the first to document this mechanism. We find that the increase in the probability that these women "leave a job to care for home or family" from the pre-shock to the post-shock rounds of the panel is significantly lower when PFL is available.

We also find impacts on mental health outcomes among women caregivers. Specifically, it appears that while their self-reported mental health is unchanged (or potentially worsens), they are both more likely to stop taking mental health-related medications and less likely to start taking them after their spouses' health shocks. These results echo prior research about the mental health burden of care work (Schulz and Sherwood, 2008), and emphasize the importance of pairing mental health supports for caregivers with paid leave.

[30] See Appendix Table A1 for the most common diagnoses associated with the health shocks we study.

Taken together, our results suggest that when married individuals experience a hospitalization or surgery, having PFL access enables their (healthy) wives to care for them while retaining their jobs. We do not find such impacts for husbands in a similar situation. The gendered impacts of PFL among spousal caregivers are consistent with the previous literature that has found that women are substantially more likely to engage in caregiving for their ill spouses than men (e.g., Allen, 1994; Boye, 2015; Sharma et al., 2016; Maestas et al., 2020; Cubas et al., 2021). We also find that the effects of PFL access on job continuity are concentrated among women with low education levels. Thus, our findings suggest that government-provided PFL might reduce pre-existing disparities in leave use and associated outcomes among spousal caregivers.

In contrast to our results for spousal caregivers, we find less consistent evidence that PFL access affects the labor market outcomes of parent caregivers. The lack of impacts of PFL on parent caregivers raises questions about the barriers that these parents may face in using paid leave. Future research should continue to study the needs of working parents whose children experience health shocks and how leave-related policies may better serve these families. Finally, while data limitations did not allow us to examine use of PFL to manage parental health issues, around 30 percent of women are caregivers for a parent or parent-in-law at some point during their 50s (Fahle and McGarry, 2022), indicating that this is an important area for future work on paid leave policies.

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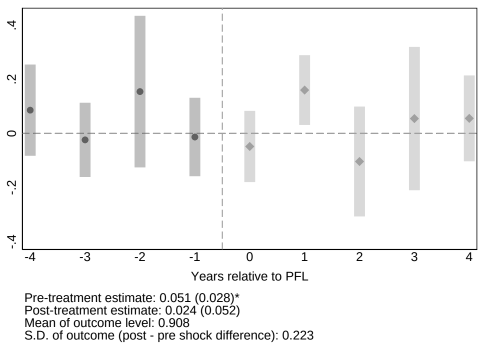
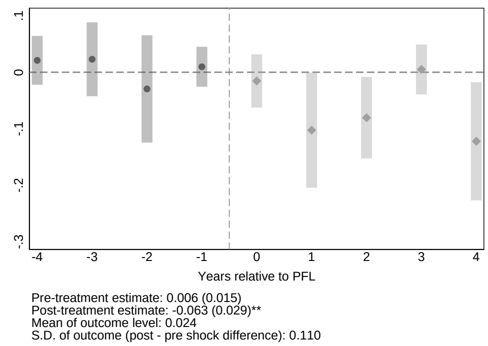
7 Figures

Figure 1: Callaway and Sant'Anna Event-Study Estimates of Effects of PFL on Women with Spouses Who Experience a Health Shock

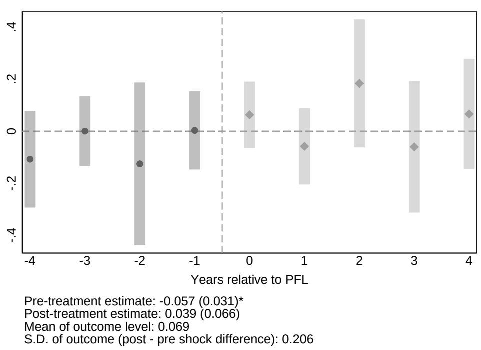
(a) Δ Being Employed



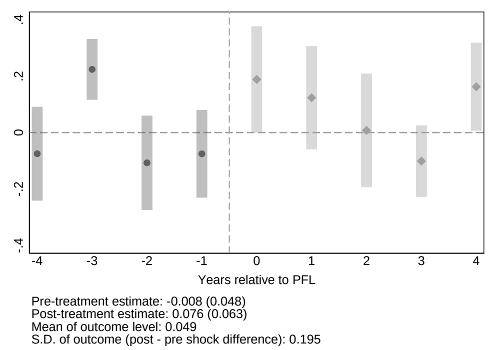
(b) Δ Leaving Job to Care for Home or Family



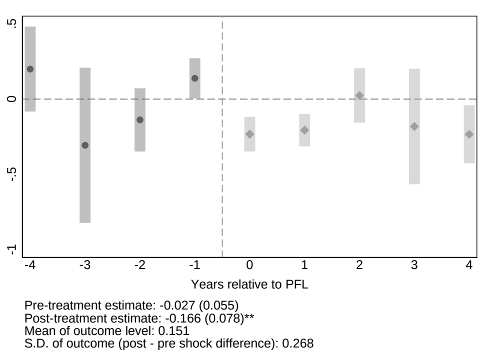
(c) Δ Leaving Job for All Other Reasons



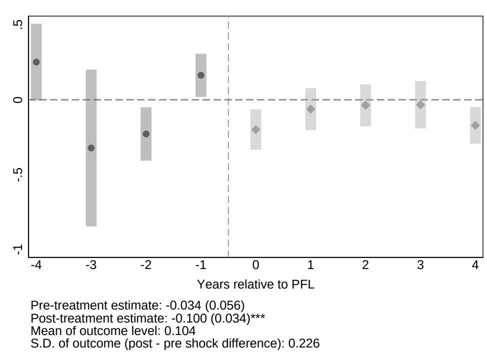
(d) Δ Reporting Poor Mental Health



(e) Δ Having Any MH-Related Medications



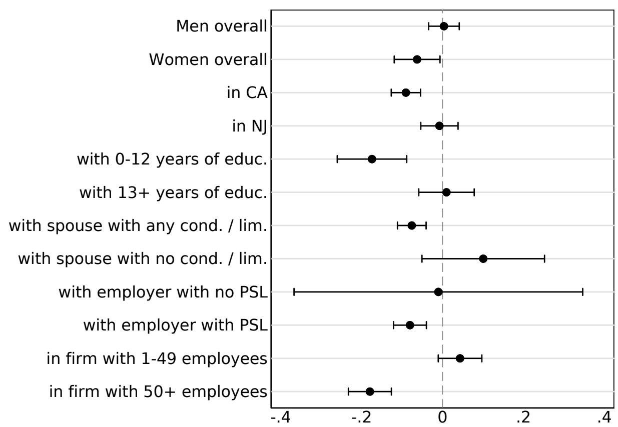
(f) Δ Having Any Anti-Anxiety or Anti-Depressant Medications



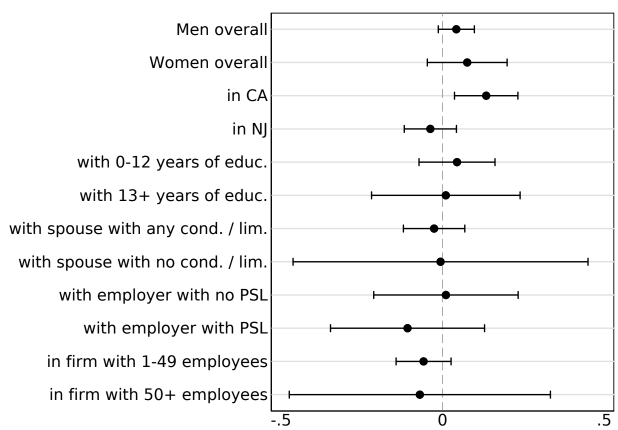
Notes: These figures plot the Callaway and Sant'Anna event-study coefficients and 95% confidence intervals. Each outcome is expressed as a change from before to after the health shock. See equation (1) for details about the regression specification used to obtain the average treatment effects and the notes under Table 1 for information about the analysis sample.

Figure 2: Callaway and Sant'Anna Event-Study Estimates of Effects of PFL on Women with Spouses Who Experience a Health Shock, by Sub-Groups

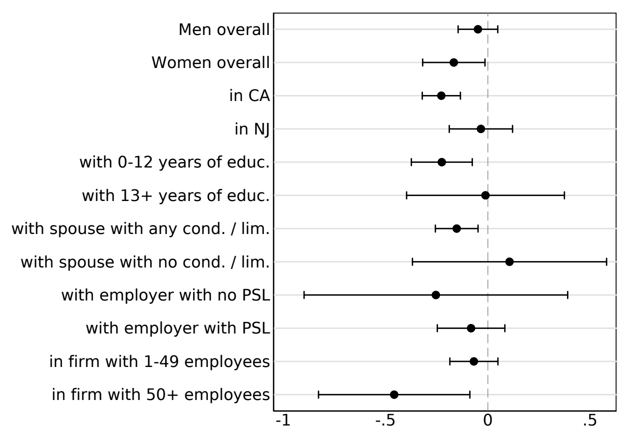
(a) Δ Leaving Job to Care for Home or Family



(b) Δ Reporting Poor Mental Health



(c) Δ Having Any MH-Related Medications



Notes: These figures plot the Callaway and Sant'Anna post-treatment estimates and associated 95% confidence intervals for each subgroup. The point estimates are post-treatment estimates that represent the average treatment effect aggregated over the post-period event window for each subgroup. Each outcome is expressed as a change from before to after the health shock. See equation (1) for details about the regression specification used to obtain the average treatment effects and the notes under Table 1 for information about the analysis sample.

8 Tables

Table 1: Summary Statistics for Individuals with Spouses and Children Who Experience a Health Shock, MEPS 1999–2013

	(1) Women Spousal Caregivers	(2) Men Spousal Caregivers	(3) Women Child Caregivers	(4) Men Child Caregivers
Average age	47.7	45.5	36.8	37.7
Average number of children under 18	0.7	1.0	2.3	2.4
Percent married	99.1%	98.6%	71.3%	87.3%
Percent Hispanic	14.7%	23.0%	23.8%	31.4%
Percent non-Hispanic Black	10.1%	11.7%	15.0%	10.7%
Percent non-Hispanic white	64.8%	56.5%	51.9%	50.2%
Percent 0-11 years of education	13.0%	18.8%	14.3%	22.7%
Percent 12-15 years of education	59.3%	54.9%	57.7%	50.8%
Percent 16+ years of education	27.7%	26.2%	27.8%	25.8%
Percent has spouse with diabetes, cholesterol, or high blood pressure	58.5%	43.5%		
Percent has spouse with heart or lung conditions	33.2%	20.0%		
Percent has spouse with arthritis	30.8%	30.0%		
Percent has spouse with asthma	9.5%	14.5%		
Percent has spouse with cancer	5.9%	5.4%		
Percent has spouse with physical limitation	33.7%	29.5%		
Percent has spouse with cognitive limitation	11.1%	10.2%		
Percent has child with asthma			16.8%	16.7%
Percent has child with any other condition or limitation			<2.5%	2.0%
Observations	1,265	1,673	821	1,200

Notes: This table presents the means of key variables for individuals with spouses in the household in the MEPS data covering years 1999–2013. The sample is further limited to individuals aged 25–64 who are employed or have a job in the first round of the panel, who do not experience an emergency department visit, hospital inpatient stay, or surgery of their own during the panel, and who have a spouse or children in the household who experience a health shock (a hospital inpatient stay or surgery in any hospital setting). The sample excludes individuals who reside in Rhode Island and New York. The heart or lung conditions category includes angina, heart attack, heart disease, emphysema, and stroke.

Table 2: Top 20 ICD-9 Codes Associated with Health Shocks Among Spouses, MEPS 1999–2012

ICD-9 Code	ICD-9 Code Description	Percent of All Health Shocks	Cumulative Percent of All Health Shocks
575	Other disorders of gallbladder	3.01%	3.01%
786	Symptoms involving respiratory system and other chest symptoms	2.37%	5.38%
780	General symptoms	2.28%	7.66%
722	Intervertebral disc disorders	2.13%	9.80%
410	Acute myocardial infarction	2.11%	11.90%
719	Other and unspecified disorders of joint	2.11%	14.01%
486	Pneumonia, organism unspecified	1.99%	15.99%
883	Open wound of finger(s)	1.67%	17.66%
401	Essential hypertension	1.61%	19.27%
959	Injury other and unspecified	1.58%	20.85%
592	Calculus of kidney and ureter	1.49%	22.34%
553	Other hernia of abdominal cavity without mention of obstruction or gangrene	1.46%	23.80%
429	Ill-defined descriptions and complications of heart disease	1.43%	25.23%
427	Cardiac dysrhythmias	1.40%	26.64%
574	Cholelithiasis	1.35%	27.98%
436	Acute, but ill-defined, cerebrovascular disease	1.29%	29.27%
239	Neoplasms of unspecified nature	1.17%	30.44%
530	Diseases of esophagus	1.14%	31.58%
724	Other and unspecified disorders of back	1.14%	32.72%
250	Diabetes mellitus	1.11%	33.83%

Notes: This table presents the 20 most frequently occurring three-digit ICD-9 codes associated with spousal health shocks (defined as either an inpatient stay or a surgery in any hospital setting), using MEPS data covering years 1999–2012. ICD codes are not available in the MEPS data in years after 2012. See notes under Table 1 for additional information about the analysis sample.

ONLINE APPENDIX

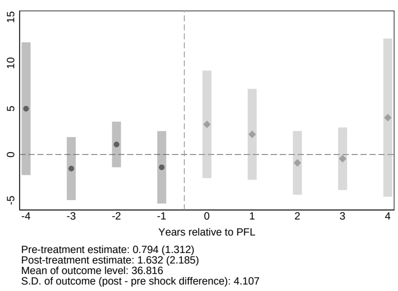
Manuscript Title: The Impact of Paid Family Leave on Families with Health Shocks

Author Names: Courtney Coile, Maya Rossin-Slater, and Amanda Su

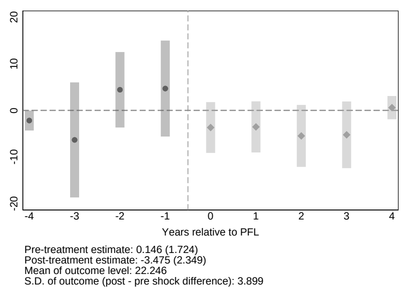
A Appendix Figures

Figure A1: Callaway and Sant'Anna Event-Study Estimates of Effects of PFL on the Intensive Margin Labor Market Outcomes for Women with Spouses Who Experience a Health Shock

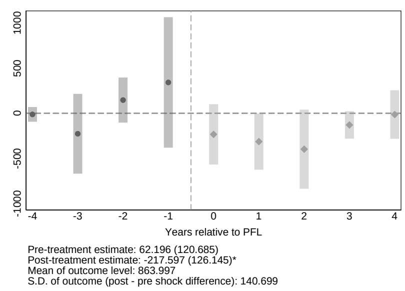
(a) Δ Hours Worked



(b) Δ Real Hourly Wage



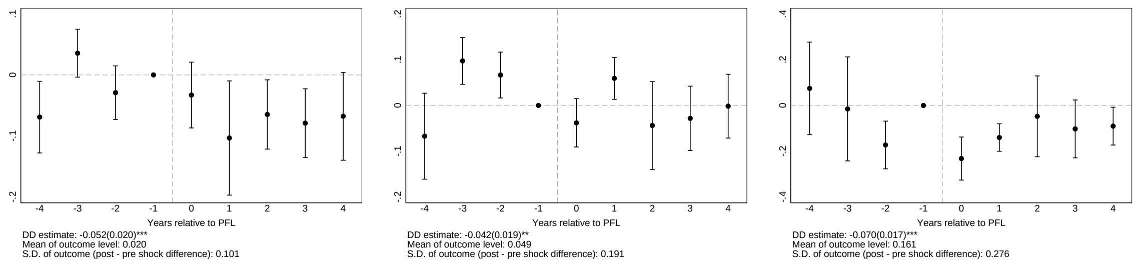
(c) Δ Real Weekly Income



Notes: These figures plot the Callaway and Sant'Anna event-study coefficients and 95% confidence intervals. Each outcome is expressed as a change from before to after the health shock. See equation (1) for details about the regression specification used to obtain the average treatment effects and the notes under Table 1 for information about the analysis sample.

Figure A2: Sensitivity Analysis: Stacked Event-Study Estimates of Effects of PFL on Women with Spouses Who Experience a Health Shock

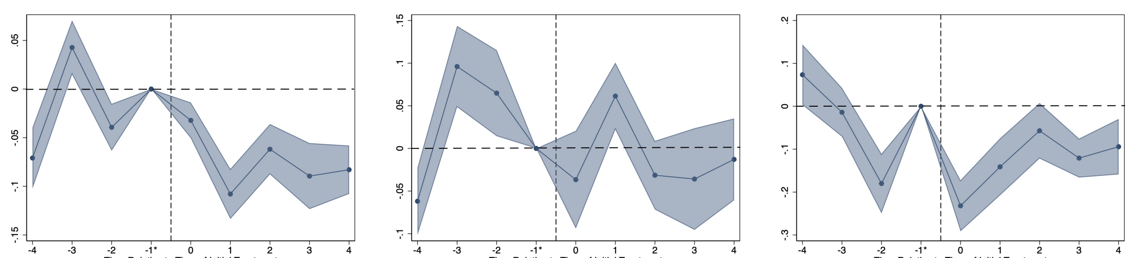
(a) Δ Leaving Job to Care for Home or Family
(b) Δ Reporting Poor Mental Health
(c) Δ Having Any MH-Related Medications



Notes: These figures plot the event-study estimates and 95% confidence intervals using a stacked difference-in-differences estimator that treats each state's policy change as a separate experiment j. The stacked event-study model is represented by: $\Delta Y_{istj} = \beta_0 + \sum_{k=-4}^{k=4} \pi_k 1[t - PFL^*_{stj} = k] + \phi' X_{ij} + \zeta u_{stj} + \eta_{tj} + \gamma_{sj} + \epsilon_{istj}$, where all of the variables are defined similarly to equation (1) except that an observation is now an individual i residing in state s in year t in experiment j. See notes under Table 1 for information about the analysis sample.

Figure A3: Sensitivity Analysis: Wooldridge Event-Study Estimates of Effects of PFL on Women with Spouses Who Experience a Health Shock

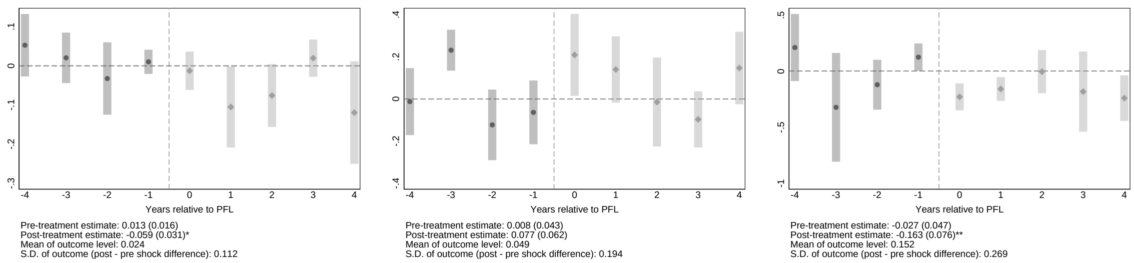
(a) Δ Leaving Job to Care for Home or Family
(b) Δ Reporting Poor Mental Health
(c) Δ Having Any MH-Related Medications



Notes: These figures plot the event-study estimates and 95% confidence interval using the two-way Mundlak estimator described in Wooldridge (2021). In these analyses, we use the same set of covariates as in equation (1). Each outcome is expressed as a change from before to after the health shock. See the notes under Table 1 for information about the analysis sample.

Figure A4: Sensitivity Analysis: Callaway and Sant'Anna Event-Study Estimates of Effects of PFL on Women with Spouses Who Experience a Health Shock, with Not-Yet and Never-Treated Control States

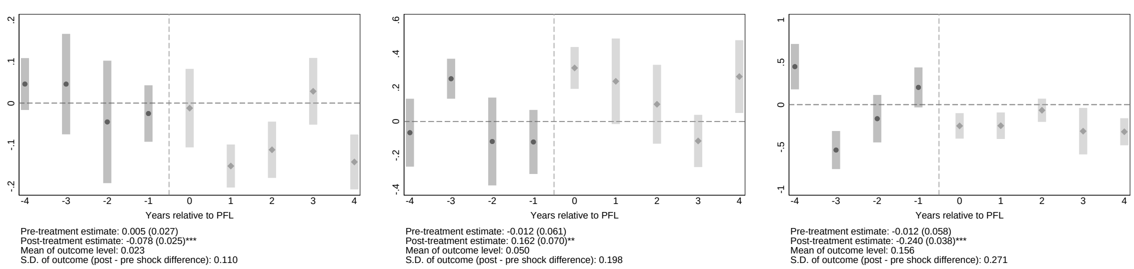
(a) Δ Leaving Job to Care for Home or Family (b) Δ Reporting Poor Mental Health (c) Δ Having Any MH-Related Medications



Notes: These figures plot the Callaway and Sant'Anna event-study coefficients and 95% confidence intervals as in Figure 1, except that the control group includes both never-treated and not-yet-treated states.

Figure A5: Sensitivity Analysis: Callaway and Sant'Anna Event-Study Estimates of Effects of PFL on Women with Spouses Who Experience a Health Shock, with Non-Bordering Control States Only

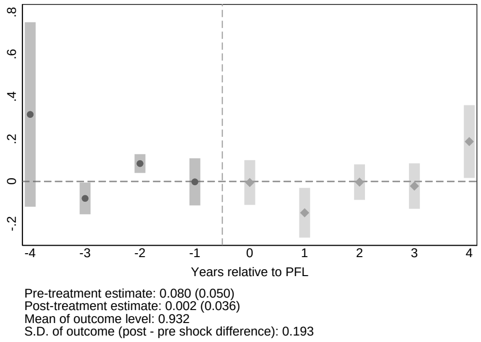
(a) Δ Leaving Job to Care for Home or Family (b) Δ Reporting Poor Mental Health (c) Δ Having Any MH-Related Medications



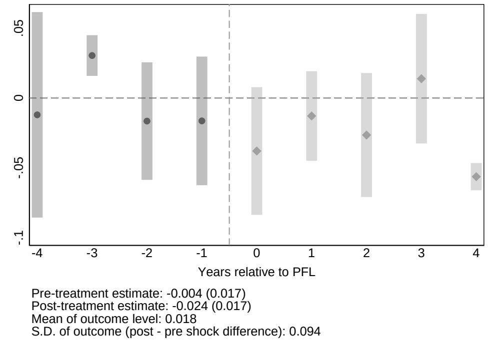
Notes: These figures plot the Callaway and Sant'Anna event-study coefficients and 95% confidence intervals as in Figure 1, except that the control group is further restricted to only include states that do not share a border with California, New Jersey, Rhode Island, or New York. We specifically exclude Arizona, Connecticut, Delaware, Massachusetts, Nevada, New Jersey, New York, Oregon, Pennsylvania, and Vermont from this analysis.

Figure A6: Callaway and Sant'Anna Event-Study Estimates of Effects of PFL on All Individuals with Children Who Experience a Health Shock

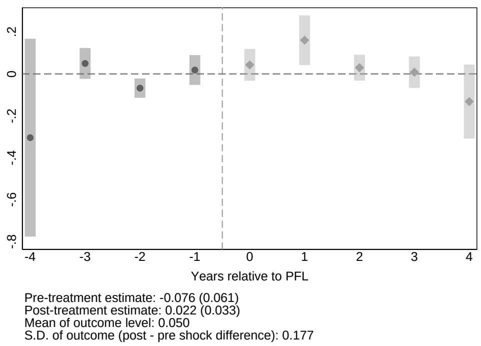
(a) Δ Being Employed



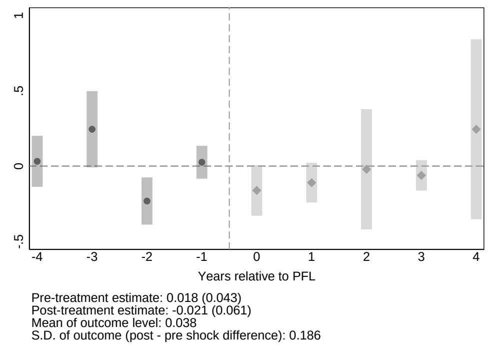
(b) Δ Leaving Job to Care for Home or Family



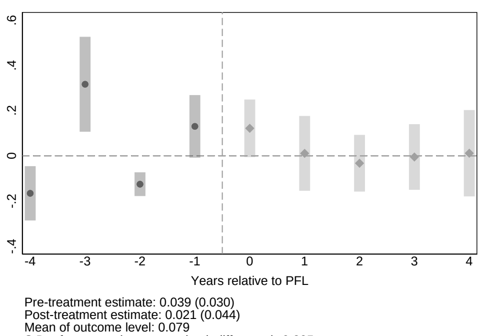
(c) Δ Leaving Job for All Other Reasons



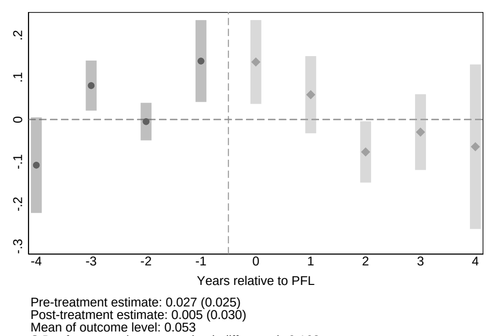
(d) Δ Reporting Poor Mental Health



(e) Δ Having Any MH-Related Medications



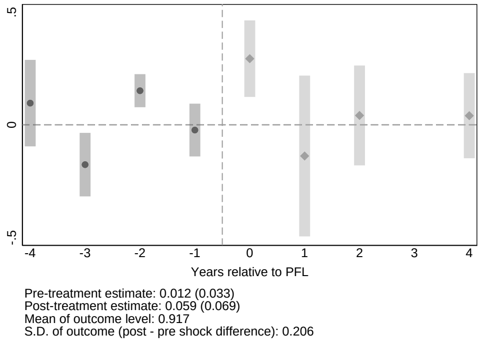
(f) Δ Having Any Anti-Anxiety or Anti-Depressant Medications



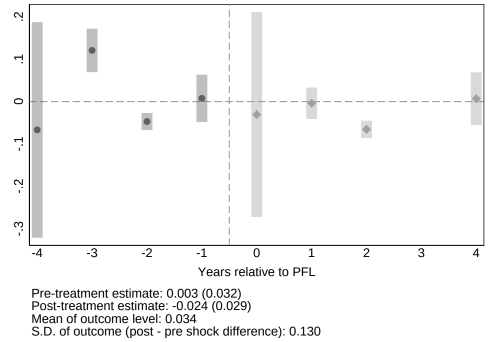
Notes: These figures plot the Callaway and Sant'Anna event-study coefficients and 95% confidence intervals. Each outcome is expressed as a change from before to after the health shock. See equation (1) for details about the regression specification used to obtain the average treatment effects and the notes under Appendix Table 1 for information about the analysis sample.

Figure A7: Callaway and Sant'Anna Event-Study Estimates of Effects of PFL on Adult Women with Children Who Experience a Health Shock

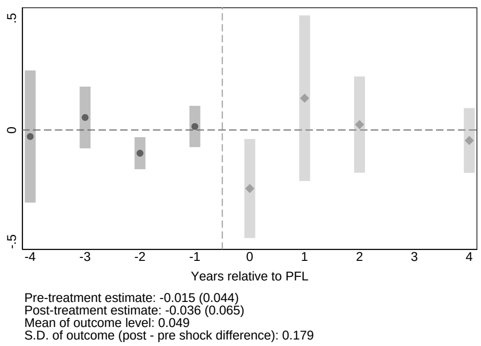
(a) Δ Being Employed



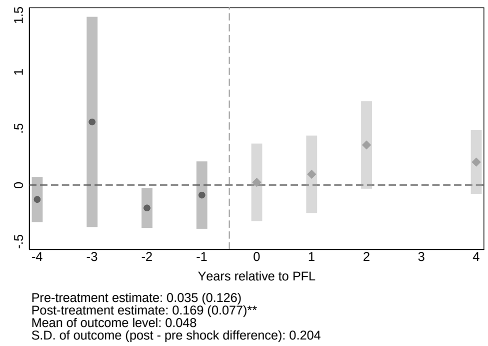
(b) Δ Leaving Job to Care for Home or Family



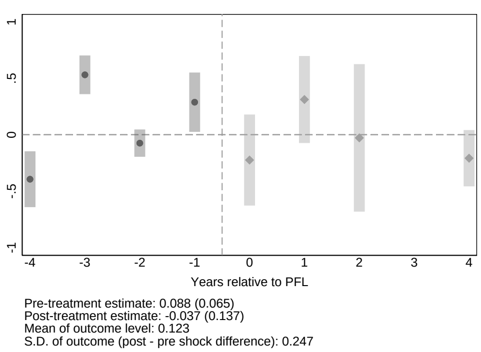
(c) Δ Leaving Job for All Other Reasons



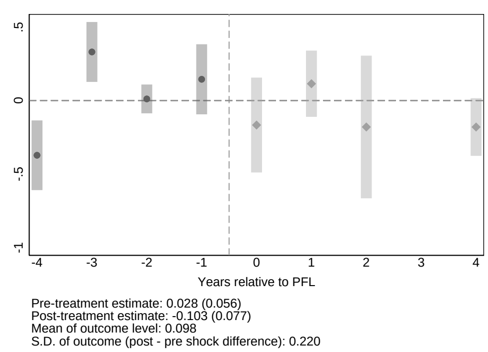
(d) Δ Reporting Poor Mental Health



(e) Δ Having Any MH-Related Medications



(f) Δ Having Any Anti-Anxiety or Anti-Depressant Medications



Notes: These figures plot the Callaway and Sant'Anna event-study coefficients and 95% confidence intervals. Each outcome is expressed as a change from before to after the health shock. See equation (1) for details about the regression specification used to obtain the average treatment effects and the notes under Appendix Table 1 for information about the analysis sample.

B Appendix Tables

Table A1: Top 20 ICD-9 Codes Associated with Health Shocks Among Children, MEPS 1999–2012

ICD-9 Code	ICD-9 Code Description	Percent of All Health Shocks	Cumulative Percent of All Health Shocks
873	Other open wound of head	13.03%	13.03%
541	Appendicitis, unqualified	4.28%	17.31%
891	Open wound of knee leg (except thigh) and ankle	3.54%	20.86%
486	Pneumonia, organism unspecified	3.32%	24.18%
959	Injury other and unspecified	3.21%	27.39%
493	Asthma	2.95%	30.34%
883	Open wound of finger(s)	2.92%	33.26%
780	General symptoms	2.55%	35.81%
079	Viral and chlamydial infection in conditions classified elsewhere and of unspecified site	1.96%	37.76%
008	Intestinal infections due to other organisms	1.85%	39.61%
882	Open wound of hand except finger(s) alone	1.62%	41.23%
892	Open wound of foot except toe(s) alone	1.62%	42.86%
818	Ill-defined fractures of upper limb	1.59%	44.44%
382	Suppurative and unspecified otitis media	1.40%	45.85%
208	Leukemia of unspecified cell type	1.18%	47.03%
276	Disorders of fluid electrolyte and acid-base balance	1.14%	48.17%
463	Acute tonsillitis	0.96%	49.13%
041	Bacterial infection in conditions classified elsewhere and of unspecified site	0.92%	50.06%
474	Chronic disease of tonsils and adenoids	0.92%	50.98%
719	Other and unspecified disorders of joint	0.92%	51.90%

Notes: This table presents the 20 most frequently occurring three-digit ICD-9 codes associated with focal individuals' children's health shocks (defined as either an inpatient stay or a surgery in any hospital setting), using MEPS data covering years 1999–2012. ICD codes are not available in the MEPS data in years after 2012. See notes under Table 1 for additional information about the analysis sample.