Caregiving and Labor Force Participation: New Evidence from Administrative Data

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1 Introduction

The US population is aging rapidly, with the number of Americans age 65 and older expected to double by 2050. As approximately one third of adults in this age range report multiple functional limitations, the number of people requiring long-term non-acute care is projected to increase significantly (Hagen 2013). Much of the growing demand for long-term care is met by informal (unpaid) caregivers, most commonly family members (Weber-Raley and Smith 2015), and the impacts of caregiving on family caregivers is a topic of growing policy concern. While family-provided care may be an affordable and even preferable alternative to formal care, its toll on the economic well-being of caregivers, many of whom are also formally employed (Freedman and Wolff 2020), is well documented. Evidence from the US suggests that caregiving affects labor supply on both the intensive and extensive margins: caregivers are more likely than non-caregivers to stop working, exit the labor force, or work at reduced hours and wages as a result of their family obligations (Fahle and McGarry 2017; Van Houtven et al. 2013; Skira 2015; Ettner 1996). The opportunity cost of unpaid family care has been estimated at as much as $5 billion in terms of lost wages alone (Chari et al. 2015).

Although it is known that caregiving results in lower labor force attachment, there is less evidence on how rapidly the adverse employment impacts arise once individuals begin providing care.[1] Additionally, given frequently-cited methodological concerns about selection into caregiving based on unobserved characteristics, reverse causality due to labor market shocks, or pre-caregiving anticipation effects biasing estimates of the impacts of caregiving roles on employment outcomes (Van Houtven et al. 2013), there is virtually no evidence documenting dynamic employment trajectories *before* the start of a caregiving spell.[2] Moreover, understanding anticipation effects – changes in employment or human capital investments made by those who expect to be future caregivers– is central when considering the overall costs of family caregiving. A long-term dynamic perspective is key to understanding the role of family caregiving in a lifecycle labor supply framework as well as how the effects of caregiving evolve over time.

These evidence gaps are driven in part by data limitations: observing employment trajectories

[1] Skira (2015) is the only paper that we are aware of that takes a dynamic perspective in the US context, Schmitz and Westphal (2017) studies the dynamics of employment after caregiving in the German context.

[2] Van Houtven et al. (2013) discuss endogeneity concerns in detail with respect to adult children caring for elderly parents, and propose a set of time-varying instruments (related to parental health) to address these concerns. Their objective is to identify the (static) effect of realized caregiving on the outcomes of somebody who is induced into caregiving based on the changing health of their parent, compared to the counterfactual of a similar person who's parents' health did not change.

requires high frequency, longitudinal observations of employment outcomes and caregiving status. We bridge these gaps by combining cross-sectional survey data about family caregiving with longitudinal administrative data on labor market outcomes to estimate the dynamic relationship between work and caregiving for new caregivers.[3] We identify caregivers using the 1996-2008 panels of the Survey of Income and Program participation (SIPP), a nationally representative survey following households for up to six years with interviews every four months. Along with a core longitudinal survey that collects earnings and employment information, the SIPP includes a range of cross-sectional topical modules, including a series of questions about informal caregiving. Using this module, we identify respondents who are family caregivers and determine the year in which they started providing care. We then link SIPP respondents to their Social Security Administration administrative records to create a panel which allows us to observe annual outcomes for caregivers well outside the range of the SIPP survey—up to 15 years before and 10 years after the start of a caregiving spell. Specifically, we analyze trajectories in annual employment, self-employment, and receipt of retirement or disability benefits.

Our objective is to understand the implications of family caregiving for labor supply both after a caregiving role is realized as well as any pre-caregiving impacts driven by the anticipation of a future need or individual or family preferences. We start by estimating event-studies to trace the evolution of labor market outcomes relative to the start of a caregiving spell. The long panel, balanced on event-time, allows us to account for (and distinguish between) both life-cycle and temporal trends using age and year fixed effects. While we do not argue that the onset of a caregiving shock is random, we observe that the start of caregiving coincides with sharp changes in some labor market outcomes. We test for discrete changes in outcomes during the 15 years prior to the start of a caregiving spell that would suggest that pre-caregiving employment shocks that would precede the start of a caregiving spell (in the spirit of an "Ashenfelter dip"). We observe smooth downward trends in employment prior to the start of caregiving, and an upward trend in the likelihood of being eligible for disability benefits. Importantly, we find no evidence of discrete trend breaks before the year in which caregiving starts. To rule out any discrete changes in outcomes that occur within the year that caregiving starts, we take advantage of the longitudinal feature of the SIPP, which allows us to observe employment outcomes quarterly in the year prior to caregiving.

We conclude that the trends we observe are likely driven by life-cycle dynamics among future

[3]We define new caregivers as those who have been caregiving for two years or less.

caregivers, rather than employment disruptions that push individuals into caregiving. Note that this does not imply that there are no anticipation effects whereby future caregivers change their behavior in response to some ex-ante expectation of caregiving, only that, conditional on these expectations, the trends appear smooth. We then we employ a quasi-experimental research design exploiting variation in the timing of caregiving spells among caregivers to estimate the immediate and medium-term employment responses to the realization of a care spell. Specifically, we use the outcomes of future caregivers to construct a comparison group that is similar to the treated group in expectation of caregiving, but that hasn't started caregiving yet in a "stacked" difference-in-differences approach (Fadlon and Nielsen 2017; Deshpande and Li 2017). Our identifying assumption is that, absent the realization of caregiving, outcomes between the treated and control groups would run in parallel. Therefore, what we estimate is the ex-post response to the realization of a caregiving spell. We show that there are no changes in trends between treatment and control groups in the eight years prior to caregiving, suggesting that our assumption that the *timing* of these shocks is random is reasonable.

Using this approach, we show that the likelihood of working falls by an average of two percentage points (3 percent) in the first two years following the start of a caregiving spell, and returns to the pre-caregiving trend within 5 years. We observe no similar change in employment among those who are self-employed, suggesting that the flexibility of self-employment is more compatible with caregiving. We find no increase in retirement hazard, but do observe a 0.7 percentage point (25%) increase in the likelihood of becoming eligible for Social Security Disability Insurance (SSDI) in the year before caregiving starts, suggesting that wage replacement benefits provided by some social insurance programs may facilitate caregiving. We examine heterogeneity in our results and find starkly different trends in some subgroups. In particular, while female caregivers appear to return to the labor market within two years of a caregiving spell, men's employment decrease is larger and doesn't recover in the five years we can observe. We also find that employment effects are concentrated among those age 40-61, which is when family caregiving roles and work are most likely to overlap.

Our study contributes directly to a literature examining trade-offs between providing care to family members with functional or cognitive limitations and formal employment. Most studies on this topic estimate the effect of caregiving on employment-related outcomes, and report a mixed range of results on both the extensive margin of working for pay and the intensive margin of wages

and hours.[4] Other studies instead estimate the effect of changing employment opportunities on the likelihood that work age adults will provide care, and find consistently that family caregiving increases as the opportunity cost of caregiving, in terms of forgone employment opportunities, decreases (Mommaerts and Truskinovsky 2020; He and McHenry 2015; Costa-Font et al. 2016).

We build on the existing literature in several ways. First, most of the aforementioned studies focus on estimating causal effects of caregiving on static employment related outcomes, while our objective is to estimate the employment trajectories of caregivers. Studies addressing the relationship between caregiving and work in the United States, where our study is based, rely primarily on the Health and Retirement Study (HRS), a nationally representative survey of the US population over 50 (Fahle and McGarry 2017; Van Houtven et al. 2013; Skira 2015).[5] The HRS is a biennial panel, and measures both caregiving and employment outcomes at two-year intervals. While the econometric challenges of identifying the causal effects of caregiving on work and related outcomes can be reasonably addressed using panel data techniques and instrumental variables, measuring short- and medium-run *dynamic* responses requires a data set that captures employment and caregiving outcomes at a higher frequency. To this end we construct a novel data linkage between survey and administrative data that allows us to examine higher frequency annual outcomes and allowing us to link short-, medium- and long- term employment outcomes for caregivers.

Understanding the dynamic impact of caregiving, as well as explicitly considering pre-caregiving employment trajectories, is critical to evaluating how employment policies, such as sick leave, family leave, and caregiver tax credits, might help individuals with family caregiving obligations to sustain employment. Many employers and some states offer paid family leave benefits, and many workers in the US have access to unpaid family leave through the Family and Medical Leave Act (FMLA). In general, family leave can be used to provide care for a sick or ailing relative as well as a new child, but family leave benefits are structured to meet the needs of new parents, and new parents make up the majority of leave claimants for both paid and unpaid leave (Bedard and Rossin-Slater 2016).[6] Meanwhile, caregiving to adults may have a different time-use profile

[4]See, for example, Ettner (1996); Carmichael et al. (2010); Van Houtven et al. (2013); Skira (2015); Fahle and McGarry (2017); Bolin et al. (2008); Carmichael and Charles (2003); Schmitz and Westphal (2017) for studies based in the US and Europe.

[5]Exceptions include He and McHenry (2015) who use the SIPP, and Mommaerts and Truskinovsky (2020), who use the American Time Use Survey.

[6]In the first 10 years of California's paid family leave legislation (2004-2014), new parents made up 90% of all PFL claims (California has a separate paid leave program for own medical leave). Much of the empirical evidence on the effectiveness of paid leave similarly focuses on new parents, e.g. Rossin-Slater et al. (2013); Rossin-Slater (2017), and few studies consider the impact of paid leave on family caregivers. Arora and Wolf (2017) found that nursing home

than newborn care, requiring intermittent absences of a few hours, rather than a single absence of several weeks (Maestas 2017). Furthermore, eldercare trajectories are highly heterogeneous, suggesting the need for a more flexible approach to caregiving leave policy (National Academy of Sciences 2016).[7]

We highlight several additional advantages to using the SIPP informal care module to study caregiving. The aforementioned studies largely estimate effects on women over 50 caring for elderly parents. Meanwhile, half of all family caregivers in the US were between the ages of 18 and 49 in 2009 (National Alliance for Caregiving and AARP 2009), and 40% of family caregivers are men. The SIPP is a nationally representative sample of the full US population, providing a more complete picture of caregiving. Additionally, the SIPP surveys respondents who provide any kind of care, including care to friends, neighbors, and extended relatives. As we document, a surprisingly high amount of care is provided to people who are not spouses or parents, a trend that is mirrored in national statistics (National Alliance for Caregiving and AARP 2009; Houser et al. 2015). For example, we find that 20% of caregivers provide care to older family members who are not parents or spouses.

Most closely related to our study is Schmitz and Westphal (2017), who use an annual longitudinal survey in Germany to study the persistence of any employment disruptions due to caregiving. Although they consider dynamics up to 8 years after the start of a caregiving spell, they do not address any pre-caregiving dynamics. To our knowledge, ours is the only study to focus on high frequency, dynamic employment trajectories both before and after the start of a caregiving spell, and to identify caregivers in administrative data in the US context. In addition, the SSA linkage allows us to focus on several previously unexamined outcomes, including the receipt of disability benefits.

Finally, our study is related to a considerable literature looking at the dynamic relationship between caregiving and employment that centers on the effect of bearing and caring for children and on women's life cycle of labor force participation (for example, Goldin (2006); Goldin and Mitchell (2017); Blau and Kahn (2007)). This literature conceptualizes the costs of caregiving as being incurred both after children are born in the form of reduced hours or wages, as well as through decisions made in the anticipation of child care needs, in the form of forgone education

admissions decreased in California following the implementation of paid family leave in 2004, suggesting that paid leave increases the supply of family care.

[7] An additional challenge is defining the family ties that would qualify for elder care. For example, under the Family and Medical Leave Act of 1993, a worker may take protected leave to care for a biological, step, foster, or adoptive parent, but not a parent-in-law.

or employment opportunities (Goldin and Katz 2002; Kleven et al. 2019; Adda et al. 2017; Hotz and Miller 1988). Our long panel and approach allows us to consider the pre-caregiving effect of anticipated care needs ("up-front" costs) separately from the ex post effect of realized caregiving. If the up-front costs are significant, causal estimates of the static effect of caregiving on work could be dramatically under-valuing the costs of family caregiving. In addition, the richness of the long administrative panel combined with detailed survey data allows us to consider various caregiving roles over the life course in tandem and evaluate their cumulative effect on the life-cycle labor supply of caregivers.

The rest of the paper proceeds as follows: In Section 2, we describe the data and provide descriptive statistics for caregivers in the SIPP. In Section 3 we present our event-study approach and provide evidence on the dynamic trajectories of employment related outcomes around the start of a caregiving spell. In section 4 we describe our stacked difference-in-differences approach in detail and present estimates of the effects of caregiving on employment. Section 5 concludes.

2 Data

We use the 1996, 2001, 2004 and 2008 panels of the Survey of Income and Program Participation (SIPP).[8] Each panel follows a nationally representative sample of U.S. households for between two and a half to six consecutive years, with regular interviews at four month intervals (referred to as "waves"). Along with the core longitudinal survey which collects detailed monthly information on demographics, employment, earnings, and program participation, in each wave households respond to topical modules which provide detailed but cross-sectional information on specific topics including informal caregiving.

We identify caregivers from the informal care topical module, which is administered two to three years into the longitudinal panel and asks all respondents the following question: "There are situations in which people provide regular or unpaid care or assistance to a family member or friend who has a long-term illness or a disability. During the past month, did you provide any such care or assistance to a family member or friend living here or living elsewhere?" Respondents who identify as caregivers on the basis of this question then provide details about who they provide care to, and for how many years they have been providing care (less than one year, one year, two years, etc). Focusing on respondents who have been providing care for two years or less, we leverage

[8]Earlier panels of the SIPP do not ask about informal caregiving.

this retrospective information to approximately date the start of a caregiving spell within the SIPP panel, allowing us to construct a longitudinal measure of caregiving outcomes for a subset of SIPP respondents, and to observe labor force participation, earnings, and retirement behavior up to two years before and two years after the start of a caregiving spell.

We next link this sample of caregivers to their SSA administrative records and construct a longitudinal panel following caregivers for 15 years before and ten years after the start of their caregiving spell. In order to match SIPP to administrative earnings and beneficiary data, the Census Bureau must receive consent from respondents. The match is based on the respondent's Social Security number (SSN), which is suppressed for research use. In our sample, 81.9% of respondents match to administrative data. Prior to the 2008 panel, respondents had to provide their SSN, so non-matches are due to a combination of non-consent and failure to recall. Starting with the 2008 Panel, the matching process was automated, so non-matches are due solely to non-consent. Appendix Table A1 compares demographic characteristics for the full SIPP sample and the sub-sample of new caregivers who do and do not have an administrative match. Respondents without an administrative match are younger, more likely to be Hispanic, and have lower levels of education than the matched sample. Among new caregivers, unmatched respondents are more likely to be caring for a non-relative.

We note two important features of our measure of caregiving. First, this measure is subject to selection bias arising from length-biased sampling: the set of individuals who are observed in a caregiving spell at the time of the informal care topical module (a "stock" sample), will tend to over-represent those engaged in longer spells (Kiefer 1988). The retrospective inclusion of individuals who have provided any care in the last month mitigates the degree of bias in comparison to a screening question that would have collected information only from individuals actively providing care, but it is larger than if individuals had been asked to provide information about care ever provided. While the latter screener would more accurately characterize caregivers, it would nonetheless not be possible to link monthly labor supply information for spells that occurred beyond the SIPP sample window. We attempt to mitigate some of the bias by including only individuals who have started a caregiving spell within the last two years ("new caregivers"). The average caregiving spell lasts 4.5 years in most nationally representative estimates, though most estimates of caregiving duration are generated from a similar "stock" sample and will therefore suffer from the same bias (Houser et al. 2015; National Alliance for Caregiving and AARP 2009). In addition, any descriptive information about the details of caregiving, including intensity and tasks

performed (with the exception of duration), only captures the nature of caregiving at the time of the survey. Finally, our sample of caregivers is right-censored: while we observe the reported start of a caregiving spell, we do not observe when it ends.

The second notable feature is that we rely on self-reports to determine caregiver status. Nationally representative surveys that use self-reports of care*givers* often results in significantly higher numbers of caregivers than estimates that rely on reports from their care recipients (Giovannetti and Wolff 2010). Among surveys that measure caregiving based on self-reports, caregiver estimates also vary dramatically based on question wording (Freedman and Wolff 2020). The SIPP wording is precise in that it asks about care provided for a long-term illness or a disability specifically, and thus generates caregiving prevalence estimates that are on the conservative end of the spectrum: 5% of respondents self-identify as caregivers in the SIPP, compared to 17% of respondents in the American Time Use Survey Eldercare Module.[9] However, there is likely significant heterogeneity in individual latent thresholds for reporting a caregiving spell that cannot be identified from this type of survey question.

2.1 Descriptive Statistics

We report descriptive statistics from the informal care module in Table 1, corresponding to wave 9 for the 2008 panel and wave 7 for the 1996, 2001, and 2004 panels. This cross-section includes over 237,000 individuals age 18 and over.[10] Of these, 12,743 individuals, or 5% of the full sample, report having provided unpaid care in the past month.

Table 1 also reports sample statistics about the details of the caregiving spell. Some 15% of the sample is providing care to a spouse, and 22% to a parent or a parent-in-law. Nearly 20% care for another relative, and a quarter of the sample is caring for a non-relative, or somebody with an unspecified relationship.[11] Nearly one quarter of the sample is caring for a child with a long-term illness or disability, underscoring that the definition of informal care in the SIPP is not limited to the elderly. Because we are focused on caregiving beyond the parent-child relationship, going forward we drop individuals who report providing care only to children from the analysis.[12]

The remaining caregivers in this sample provide care for about 11 hours per week, for on average 1.2 care recipients. Nearly 60% of care is provided to recipients outside the caregiver's

[9]Calculated by authors and provided upon request.
[10]The full SIPP sample includes respondents 15 and older.
[11]Percentages do not add up to 1 because individuals can specify up to four care recipients.
[12]We keep respondents providing care both to own children and other adults.

household. 41% of caregivers report provide help with Activities of Daily Living (ADLs), and 92% report providing help with Instrumental Activities of Daily Living (IADLs). Over half of caregivers also provide medical help and 48% are the sole caregiver, highlighting that the majority of caregivers are engaged in significant and time intensive caregiving. Finally, Table 1 also reports sample statistics on the caregiving duration. Some 22% of caregivers started providing care within the last year, 12.0% have been providing care for a year and 15.4% have been providing care for two years. Half the sample have been providing care for three years or longer. The distribution of observed care duration is necessarily biased towards longer spells (the well-known sampling bias problem with "stock" sampling described in the previous subsection), so in our analysis we focus on "new" caregivers, those who have been providing care for two years or less to minimize concerns about length-biased sampling.

We present descriptive statistics for this sample of new caregivers in column 1 of Table 2, and compare to the sample of long-term caregivers (three or more years) in column 2 and to non-caregivers in column 3. Panel A reports demographic characteristics and employment outcomes from the survey data for the wave in which the topical module was administered, and panel B reports employment outcomes from the linked administrative data, measured in the year that the topical module was administered. Starting with the demographic characteristics in panel A, compared with non-caregivers, caregivers are older, more likely to be female, more likely to be white, and more likely to be married. They are also somewhat more likely to have graduated from college. Comparing new and long-term caregivers, new caregivers are younger, more likely to be female and somewhat less likely to be white, married, and college educated than long-term caregivers, highlighting the potential for selection bias from length biased sampling.

The rest of Table 1 reports sample statistics for the variables that enter as outcomes in our analysis. Staying with the survey data in panel A, overall caregivers are less likely to be employed, but conditional on working are more likely to be self-employed than non-caregivers. They are also more likely to be out of the labor force, and more likely to self-identify as retired than non-caregivers. Caregivers also participate less in the labor market on the intensive margin: they are more likely to work part time, and work fewer hours per week than non-caregivers. However, we do not see evidence of large difference in individual monthly earnings, conditional on working. Caregivers live in households with slightly lower family income than non-caregivers.

Comparing new caregivers to long-term caregivers, we find that new caregivers are more likely to be working than long-term caregivers. If they are not working, they are more likely to report

being unemployed and less likely to report being out of the labor force. New caregivers also earn lower monthly wages, conditional on working.

Panel B of Table 1 presents sample statistics for the linked administrative sample. To match the survey data, these outcomes are measured in the year that the informal care topical module was administered. We use data from the Master Earnings File to measure annual employer and self-employment earnings. Employer earnings data derive from IRS Forms W-2, and include wages and tips from all employers. Self-employment earnings data derive from IRS Form Schedule 1040 SE. The earnings variable combines both employer and self-employment earnings, indexed to 2018 dollars. We consider individuals with non-zero, combined earnings as employed in a given year. The self-employment earnings variable includes only the self-employment portion of earnings; individuals with non-zero, self-employment earnings are self-employed in that year.

Employment patterns from administrative records are similar to the survey data. 65% of recent caregivers are employed, compared with 59% of long term caregivers and 71% of non-caregivers. Annual employment measured in the administrative data is higher than monthly employment measured by the survey, pointing to the importance of higher frequency observations to capture within year changes in outcomes. Conversely, rates of self-employment are lower than those reported in the survey data, likely because IRS Form Schedule 1040 SE provides an incomplete picture of self-employment (Abraham et al. 2020). There is no difference in self-employment across these three subgroups. Annual Earnings are lower for caregivers, both overall and conditional on working. Conditional on working, new caregivers earn just under $44,000, compared with $45,300 for long-term caregivers and nearly $49,000 for non-caregivers. Self-employment earnings follow the same pattern.

We use monthly data from the Master Beneficiary Record (MBR) to identify Old Age and Survivors Insurance (OASI, or retirement) beneficiaries and Disability Insurance (SSDI) eligibility. In this study, we focus on OAI beneficiaries, which include retired workers, but not dependents and survivors of workers; SSDI eligible refers to individuals who are have a documented work limiting disability entitling them to benefits, but who have not necessarily started receiving them. We consider individuals as receiving OAI or eligible for DI benefits in a given calendar year if they receive benefits at least one month during the year. 24% of new caregivers are receiving OAI retirement benefits, compared with 29% of long-term caregivers and only 19% of non-caregivers, likely reflecting the age differences between these groups. There do not appear to be large difference in SSDI claiming behavior between caregivers and non-caregivers.

These descriptive statistics underscore that caregivers have different employment outcomes than non-caregivers, but also hint at the dynamic employment trajectories of family caregivers over time. We now turn to describing the methods we employ in the remainder of the paper to map out these trajectories.

3 Employment Trajectories of Caregivers

What is the timing of changes in employment relative to the reported start of a care spell? Does employment decline before caregiving starts? Does exit from the labor market after the start of a spell happen immediately, or gradually as caregivers take on more and more care responsibilities? Is caregiving associated with an increase in retirement hazard, or transitions into self-employment? In this section we present evidence on these relationships using a long panel of annual employment-related outcomes.

3.1 Event-Study Approach

We estimate event-study models to study the dynamic trajectory of employment and retirement outcomes as a function of event-time. We exploit individual variation in the timing of a caregiving spell and trace out the full dynamic trajectory of both anticipatory pre caregiving trends and ex post responses to the start of caregiving. For each respondent, we designate as $\tau = 0$ as the year in which they reported the start of a caregiving spell, and index all other years relative to that year. Our baseline specification considers a balanced panel of new family caregivers whom we observe annually from 15 years before to 10 years after they start caregiving. Our main specifications take the following form:

$$y_{i\tau s} = \sum_{\tau} \mu_r D^{\tau} + \gamma_s + \delta_{is} + \varepsilon_{i\tau} \tag{1}$$

Here $Y_{i\tau}$ is the outcome of interest for individual i in event-time τ. Our coefficients of interest are the μ_r, which are the coefficients on D^{τ}, the indicators for event-time, or time relative to the start of the caregiving spell. We omit the event time dummy at $\tau = -2$, and the μ_r then capture the changes in the outcome variable $Y_{i\tau}$ relative to two years before caregiving started. We include calendar year fixed effects γ_s to control non parametrically for time trends including business cycles, and age fixed effects δ_{is} to control non-parametrically for employment related life-cycle

trends. As we observe respondents starting caregiving at different ages and in different years, we can separately identify these effects in our models.

We reproduce equation 1 using just the SIPP survey data to observe high frequency changes in outcomes in the year before and after the start of a caregiving spell. To ensure out models are balanced around event-time, we can observe at most four waves before and y waves after caregiving starts. In the survey sample, event time is measured as the survey wave relative to the wave in which caregiving started, and the omitted category is μ_{-3}, which corresponds to three waves, or approximately one year before the start of a care spell. Calendar time corresponds to the calendar year and month of the survey, while in the administrative sample, this corresponds to the calendar year.

The results from these event study allow us to visually observe trends in outcomes in the months and years around the reported start of a caregiving spell, and to identify patterns and pre-trends. We do not interpret the coefficients as causal. A causal interpretation would require the timing of the start of a caregiving spell to be uncorrelated with outcomes, and we suspect this is unlikely to be true in many cases. For example, Mommaerts and Truskinovsky (2020) find that changing employment outcomes may precede caregiving for some individuals. It is also likely that individuals time the start of a caregiving spell to coincide with a job transition, such as retirement. Finally, because we rely on retrospective self-reporting about the start of a caregiving spell, it is possible that respondents consider themselves to be a caregiver only once they stop working and start providing care full time, even if they previously performed caregiving duties. However, this perspective allows us to directly examine the pre-caregiving period to visually observe trends and discrete changes.

3.2 Results from Administrative Data

We start by reporting results from the full administrative sample. Figure 1 plots the evolution of employment, self-employment, retirement and SSDI eligibility for 15 years before and 10 years after the reported start of a caregiving spell. Outcomes are binary indicators for any employment, any self employment, any OAI income and SSDI entitlement (i.e. if the respondent has a work-limiting disability that makes them eligible for SSDI, regardless of whether they are actively receiving benefits). For each outcome, we plot the μ_rs from Equation 1. The point estimates and standard errors measure changes relative to the omitted category μ_{-2}, two years before caregiving started, after removing life cycle and time trends. The shaded regions report 95% confidence bands around

the event-time coefficients. Dashed vertical lines are at μ_{-1}, the year before the individual reports any caregiving.

Notably, although all of the outcomes demonstrate substantive (and statistically significant) pre-caregiving patterns, the start of a caregiving spell appears to generate a sharp discontinuity in some cases. Focusing first on employment, the likelihood of being employed falls smoothly and gradually by five percentage points in the 15 years preceding a caregiving spell, and then again sharply in the year that caregiving starts. In the first two years of a caregiving spell, employment falls another five percentage points, and then remains flat for the rest of the observed period. Ten years after a caregiving spell, employment appears to return to its pre-caregiving trend.

Figure 1 suggests that self-employment follows a different trajectory. Self-employment can be a flexible work arrangement that is more compatible with caregiving responsibilities and can also be a transitional form of employment for workers exiting the labor force into retirement. We observe that self employment falls in the pre-period, but by much less than overall employment, and the start of caregiving does not generate similar sharp changes. These initial findings confirm that self-employment is more compatible with caregiving. However, we also don't see increases in self-employment, suggesting that, on average, caregivers are not transitioning into self-employment to remain employed while they are caregiving.

Caregivers leaving the labor market to provide care may compensate for lost earnings through social insurance benefits. In our analysis we focus on two such programs, which are observable in the administrative data: Social Security retired worker benefits (OAI) and Social Security Disability Insurance (SSDI) benefits. The administrative data allows us to observe precisely when an individual begins claiming Social Security and when they become entitled to SSDI benefits. Assessing these two outcomes visually in Figure 1, there is no clear evidence that OAI claiming changes significantly relative to the start of a caregiving spell. On the other hand, SSDI eligibility exhibits a positive, roughly linear trend in the decade before caregiving starts and then increases in the year before the caregiving spell, suggesting the individuals may increase their caregiving behavior after they exit the labor market following the onset of a qualifying disability. Recall that these estimates are adjusted for both life cycle and time fixed effects.

As has already been noted, all of our observed outcomes demonstrate substantive pre-trends in the decade before the reported start of a caregiving spell, which can be roughly approximated by a linear trend. There are several possible reasons for these trends. First, our event study analysis could be capturing selection into caregiving based on employment-related outcomes. However,

the length of the panel (15 years before the reported start of caregiving) and the relatively smooth trends that we observe suggest that this selection is not driven by employment shocks or job loss that induces people into a caregiving role. Alternatively, these could be evidence of anticipatory effects: individuals who anticipate that they will start caregiving in the future may change their market participation ahead of time. This can be conceptualized as costs of caregiving that are incurred prior to the realization of a caregiving spell, and are analogous to women under-investing in human capital or selecting into low-paying "family-friendly" occupations in anticipation of future fertility and childcare needs (Goldin 2006; Goldin and Mitchell 2017; Adda et al. 2017). Finally, it could be a respondent's latent threshold for self-identifying as a caregiving is higher than the threshold at which caregiving duties interfere with employment. In other words, the average caregiver may not report that they are caregiving until they have already exited the labor force to become a caregiver.

3.2.1 Heterogeneity

The average effects presented in the previous section may mask significant heterogeneity between caregivers in the evolution of employment outcomes around a caregiving spell. For example, older workers are more likely to be eligible for social insurance programs such as OAI and SSDI. Older workers may also be more likely to transition into self-employment. Meanwhile, younger workers may have more flexibility to transition between jobs. Due to various caregiving roles over the life cycle, women may be differentially attached to the labor force or may have more flexible jobs than men who become caregivers. Furthermore, men and women may take on different caregiving roles (Van Houtven et al. 2013) or may have different latent thresholds for reporting caregiving. In Figures 2 & 3, we plot the event-time indicators from Equation 1 estimated separately for these key sub-samples.

In Figure 2 we break the sample into three age groups: younger than 40, 40 to 61 and 62 and older.[13]. We focus on these age groups because the interaction of work and caregiving will be different in different life phases. The subgroup age 40 to 61 will have the largest overlap of both caregiving and work responsibility and is most likely to be caring for aging parents. They are also age 25-46 when the panel begins and 50-70 when it ends, so they are the group who is mostly are most likely to be in the labor market for the full event time window. Those aged 62 and older when caregiving starts are more likely to be caring for spouses (Fahle and McGarry 2017), and may be timing caregiving to coincide with existing retirement plans.

[13]Age groups are calculated as age when caregiving starts, so the panel is balanced over event-time

Consistent with this assumption, we find that older caregivers (those age 62 or older when they report caregiving) see the largest pre-caregiving declines in employment that start approximately 10 years before the start of caregiving and continue for approximately four years before leveling off. We see a similar, though smaller in magnitude, change in likelihood of self-employment. SSDI hazard does not appear to change for this group, which is sensible given that they would more likely be accessing Social Security benefits. By contrast we see virtually no change in all four outcomes among the youngest caregivers, those who are younger than 40 when they first report being a caregiver (we omit the youngest group from retirement outcomes figure).

Finally, we consider employment trajectories for respondents who are 40-61 when they start caregiving. For this age group, we see very little evidence of dynamic pre-caregiving trends in employment, and a sharp drop in employment in the first two years after caregiving of approximately 2.5 percentage points. These declines are persistent and average employment does not recover to pre caregiving levels in the study period. Employment remains flat for the remainder of the period. There is little evidence of similar decreases in self-employment. Caregivers in this age group also experience the largest increase in SSDI eligibility around the start of a caregiving spell. Eligibility begins to trend upwards 10 years before a a reported spell and peaks 5 years after.

While women are typically associated with family caregiving responsibilities, nearly 40% of caregivers of adults in the US are men.[14] Existing studies note that caregiving responsibilities have a different impact on men's and women's labor force participation, highlighting the need to look at outcomes separately by gender (Van Houtven et al. 2013). In Figure 3, we present results separately for men and women and find that employment patterns around the start of a caregiving spell vary dramatically by gender. Male caregivers experience the largest pre caregiving declines in employment, while the decline for women is more gradual and smaller in magnitude. Employment falls similarly for men and women in the first two years after the start of a caregiving spell, but women's average employment gradually recovers somewhat, while men's average employment continues to fall and never recovers.

A possible reason for the divergent trends could be that men and women have different latent thresholds for self-identifying as caregivers, as noted in the previous section. Existing studies have found that mean and women engage in different caregiving activities (Houser et al. 2015; Van Houtven et al. 2013). Alternatively men and women may have different reasons for entering into caregiving. For example, Mommaerts and Truskinovsky (2020) consider the role of business cycles

[14] In the SIPP, men make up over 35% of caregivers.

on the propensity to provide care and find that men are responsive to the opportunity cost of caregiving, while women are not. If men take on caregiving following a job loss while women take on caregiving in response to a need, this may explain some of the differences in trends between genders that we observe.

3.3 Results from Survey Data

The results in the previous section allow us to examine year-over-year changes in employment outcomes captured by administrative data. In this section, we turn to the SIPP survey data to study the changes in related outcomes in a shorter time frame and at a higher frequency. Given the structure of the SIPP panel and the timing of the informal care module, we are only able to observe a balanced panel for four waves (16 months) before and 7 waves (28 months) after the start of caregiving. Recall that a survey wave corresponds to four calendar months. We choose to focus on waves rather than months for several reasons. First, the structure of the survey and the limited specificity of the retrospective caregiving information makes it difficult to pinpoint the precise month in which caregiving started, creating some measurement error close to the start of a caregiving spell. Averaging outcomes over four months reduces some of this noise. Second, averaging outcomes over four months reduced some of the issues related to the well-known problem of seam bias in the SIPP.[15] Although this shortened panel does not allow us to observe a lengthy pre-period, we are able to capture any sharp changes in outcomes in the year before caregiving starts. In addition, the rich set of self-reported outcomes allows us to investigate more comprehensively the changes in employment we observe in the administrative data.

Figure 4 plots the μ_r's from Equation (1) in the survey sample. We present results for six self-reported outcomes: working, self-employed, labor force non-participation, unemployed, retired, and not working because of family obligations (including, but not limited to caregiving). All coefficients and standard errors are plotted relative to μ_{-2}, approximately two waves (8 months) before the wave in which a caregiving spell starts. In the figure, the red vertical lines correspond to one wave before and one wave after the start of a caregiving spell. These eight months capture a transitional period where we cannot be certain of caregiving status.

The results plotted in Figure 4 correspond roughly to what we observe in the administrative data. The impact of caregiving on work is visually apparent immediately in the top left panel

[15] Seam bias refers to the empirical fact that employment status changes significantly more between survey waves (at the "seam" where two survey waves meet) than within survey waves.

of the figure, and the likelihood of employment falls for the two years following the start of a caregiving spell. Notably, in the 12 months before caregiving, trends are smooth and relatively flat, supporting our conclusion that caregiving in our sample is not precipitated by discrete changes in employment. Again we see no changes in the likelihood of being self employed. Finally, the decrease in the likelihood of working corresponds to an increase in non-participation.

The the second row of Figure 4, we explore this non participation further. We observe increase in both unemployment and retirement following the start of caregiving, though neither effect is large enough in magnitude to explain the full change in employment. We also observe and increase in the likelihood of not working because of family obligations, which can include providing care to family members. For all outcomes, we again observe no change in outcomes in the year prior to the window where a caregiving spell starts, confirming our conclusion that changes in employment-related outcomes evolve smoothly prior to the start of a caregiving spell.

4 Effects of Caregiving on Employment, Retirement and Disability

In the previous section, we described the dynamics of employment-related outcomes around the start of a caregiving spell. In this section, we turn to estimating the effects of caregiving on labor supply. We leverage plausibly exogenous variation in the start of a caregiving spell among the sample of future caregivers who anticipate a caregiving need but do not know precisely when the need will arise. As such, our target is the ex-post response to the realization of caregiving (the costs borne once caregiving starts) rather than the effects of anticipating the need for caregiving (the pre-caregiving costs). The smoothly evolving employment trends we document in the previous section support this approach.

Our empirical approach uses variation in the timing of caregiving spells to construct a control group to estimate counterfactual trends. In an ideal setting, we could identify a control group of individuals who are identical to caregivers (treated respondents) both in the evolution of their employment outcomes and their ex ante expectations about caregiving, but who do not start providing care within the panel (i.e., they remain untreated). We operationalize this ideal by using a "stacked" difference-in-differences method, following Fadlon and Nielsen (ming) and Deshpande and Li (2017). Specifically, we compare respondents who experience a caregiving event to a control group of individuals who will begin caregiving at some time in the future. This method allows us to exploit the *timing* of caregiving spells among people evolving along similar trajectories, with

similar expectations about future caregiving risk. We rely only on the administrative sample for this part of the analysis, as the survey sample does not provide a balanced panel with sufficient periods before and after the start of a caregiving spell.

4.1 Empirical Approach

4.1.1 Sample Design

This approach requires a different data structure, which we construct as follows: for each calendar year, we take the subset of individuals who begin caregiving in that year and designate them as the treatment group. Every individual who started caregiving at least δ years in the future is assigned to the control group. We then redefine event time r with respect to caregiving for the control group: we define a placebo caregiving spell δ years before they actually report starting caregiving. For the treatment group, r is unchanged, it is the year they report starting a caregiving spell.

We repeat this procedure for each calendar year and then append all the data sets together.[16] The resulting data set is comprised of the original sample of caregivers, all "matched," in calendar time, to a control group of future caregivers. One limitation of this approach is that we can only measure dynamic causal effects $\delta - 1$ periods post-shock (i.e. until the control group becomes treated). We set δ to be 5 years. In Figure 5, we demonstrate this empirical approach visually, graphing the raw plots of the evolution of outcomes in both the treatment and control groups around event-time in the administrative data sample. For most outcomes, we note that the pre-trends in event time are captured reasonably well by the counterfactual trends in the control group. The deviation from these in the treatment group are visually apparent around the year that caregiving started.

4.1.2 Estimating Equation

Having linked every caregiver observation in the sample with a control, we estimate the following dynamic difference-in-differences model:

$$y_{i\tau} = \gamma_t + \beta_0 X_{i\tau} + \beta_1 Treat_i + \sum_{\tau} \mu_r D^{\tau} + \sum_{\tau} \eta_r (Treat_i \times D^{\tau}) + \varepsilon_{i\tau} \tag{2}$$

where $Y_{i\tau}$ is an outcome for individual i at event time τ, $Treat$ is an indicator for if the household

[16] An observation does not serve as its own control, however households do appear in the sample multiple times as controls.

belongs to the treatment group, and as before, the D^{τ} are indicators for event time, or time period relative to the start of the caregiving spell. The coefficients of interest, η_r now represent the dynamic changes in outcomes in the treatment group while controlling for changing outcomes in the control group. For table estimates, which summarize the magnitude and statistical significance of the effects, we estimate a pre-post version of Equation (2), replacing the individual event-time indicators with a single indicator for the post period. Again, we control separately for year zero. All observations are weighted using SIPP-provided survey weights and we cluster standard errors at the individual by treatment group level. This approach captures the average effect of starting a caregiving spell while controlling for trends in event-time as determined by the evolution of outcomes in the control group. The identifying assumption is that, among those who anticipate that they will be caregivers soon, the precise timing of when the caregiving spell starts is as good as randomly assigned. We note that this method does not alleviate the concern that caregiving and labor supply are two outcomes of a joint decision making process, rather than a response to a random shock.

4.2 Stacked Difference-in-Differences

Results from these "stacked" difference-in differences models, which use the outcomes of future caregivers as a counterfactual trend for current caregivers, are presented visually in Figure 6. Because of the data structure, we now have a balanced panel of eight years prior to and five years following the start of a caregiving spell. To summarize magnitudes and conduct inference, we present the results of a variant of Equation 2 using binary indicators for the full caregiving period, as well as for years 0-2 and years 3-5 in Table 3.

Starting as before with employment in the top left panel of Figure 6, we find no difference between the treatment and control groups prior to the start of caregiving, and a discrete drop in the likelihood of being employed in the year that caregiving starts. This deviation from the trend persists for several years before returning to near pre-caregiving levels by the 5th year after caregiving. Although we do observe the end of a caregiving spell, this timing seems consistent with existing estimates suggest that the average spell lasts between 4-5 years. At the end of five years, employment has returned to pre-caregiving *trend*, though not to the pre-caregiving *level*. In Table 3, column 1, we report that employment falls by 2 percentage points following the start of a caregiving spell, or 3 percent from a pre caregiving mean of 72 percent. This effect is concentrated in the first two years.

Given the large differences in trends both pre and post caregiving that we observed our descriptive analysis, in Table 3, we also present results from Equation 2 splitting the sample by gender and age. In Panel A, column 2, we show that effect for men was larger in the post period (2.75 percentage points), and was also larger three to five years after the start of a caregiving spell. Men are 3.75 percentage points, over four percent, less likely to be working three to five years after the start of a caregiving spell. By contrast, the effects for women are concentrated in the first two years, when they are 2.84 percentage points, or 4.1 percent less likely to be working. In years 3-5, female caregivers have returned to the pre caregiving trend. Finally, in Columns 4-6 we break the sample up by age and find that the effects are concentrated among those age 40-61 at the start of a caregiving spell - the subsample for whom work and family caregiving obligation are most likely to overlap. In this group, we find that employment fell by 3.8 percentage points, or nearly five percent from a pre-caregiving mean of 79%.

Unlike these large changes in overall employment, we observe no changes in self-employment or retirement hazard among caregivers in either the dynamic or static estimates. However, there is a large and statistically significant increase in eligibility for SSDI in the year *before* caregiving starts. In Table 3, we report that on average SSDI eligibility increases by 0.75 percentage points, or 25% from a pre-caregiving mean of 3 percent. We find that this effect is present for both men and women, and concentrated among those younger than 62. Combined with the timing, this suggests that receipt of SSDI, as a form of wage replacement, may facilitate caregiving for some families.

5 Discussion and Conclusion

In this paper, we present evidence on caregiving and labor supply from the Survey of Income and Program Participation linked to administrative earnings records from the Social Security Administration. We leverage the retrospective nature of survey responses in the SIPP to create a longitudinal panel of caregiving outcomes. We exploit this longitudinal data construction by employing several different research designs: first we trace out dynamic trajectories of employment related outcomes before and after the start of a caregiving spell, and then we use a "stacked" difference-in-differences design that uses the labor supply of individuals who will become caregivers in the near future as the counterfactual labor supply of individuals who have just started caregiving.

We examine employment and employment-related outcomes for "new" caregivers (those who

start caring within two years of the SIPP survey). We observe annual outcomes for up to 15 years before and ten years after the reported start of a caregiving spell from the administrative data and roughly quarterly outcomes for up to one year before and two years after a the start of a care spell from the survey data.

From our analysis we obtain several key findings. First, we document that the relationship between caregiving and employment-related outcomes is dynamic: We find some evidence of anticipatory effects, as well as changes in outcomes across the post caregiving period. We find that the onset of caregiving leads to a significant–and immediate–reduction in employment on average. Using administrative data, we find that the likelihood of working is 2.1 percentage points (3%) lower after the start of a caregiving spell. In contrast to this decrease in employment, we find no reported changes in self-employment, and no evidence that caregiving spells correspond with immediate transitions into retirement. However, we find that caregiving is associated with increased eligibility for SSDI, suggesting that family caregivers may leverage social insurance to compensate for lost wages associated with caregiving needs. The timing of these effects suggests that SSDI may facilitate family caregiving, rather than caregiving obligations pushing individuals to opt into SSDI.

Heterogeneity analyses reveal stark differences by gender in employment: we find that male caregivers experience large downward trends in employment even before the start of a caregiving spell, and do not appear to recover. Female caregivers, by contrast, return to pre caregiving employment trends within two years of starting a care spell.

While the prior literature has documented a relationship between caregiving activity and employment over a two-year period, our analysis reveals that adverse employment effects are detectable from the very start of the caregiving spell and have a dynamic trajectory. We highlight that this decrease in employment persists for some subgroups of caregivers and reverses for others. We find no change in self employment, suggesting that this form of employment allows caregivers to balance work and caregiving responsibilities. We also document that caregivers, especially men, may be substituting away from paid employment and into disability.

Our results are roughly in line with the existing literature on caregiving to adults and employment in the United States. Looking the the static relationship between caregiving and work, Van Houtven et al. (2013) find no impact on the the likelihood of employment for women, but reductions on hours at the intensive margin. We find that female caregivers do experience reductions on the extensive margin that last for as long as two years. This could be due to labor market frictions

suggested by Skira (2015), who studies the dynamic impacts of caregiving to parents and finds that female caregivers are less likely to receive job offers in the two years following a care spell. In contrast with Van Houtven et al. (2013) we see no effects on claiming of retirement benefits, though this could be due to measurement differences between self-reported retirement status and administrative records. Our results for male caregivers are also in line with the findings of Van Houtven et al. (2013), who find that men reduce employment by 2.4 percentage points. We find a similar magnitude effect and we show that this grows over time, highlighting the differences in employment trajectories between male and female caregivers.

Overall, we find that, as with child care, the costs of family caregiving appear to be incurred both before and after the start of a caregiving spell. It is important to note that while we find caregiving depresses employment among new caregivers, our results do not reveal how much of the employment effect is voluntary versus involuntary. Would new caregivers prefer to continue working if their employers could better accommodate them? The fact that adverse labor supply effects arise immediately following the start of caregiving, and appear to persist over the following months (and years), suggests there could be scope for employment policies designed to help working individuals cope with caregiving demands, which are often idiosyncratic and intermittent over a period of several years, and not typically resolved during a single, 12-week spell of continuous leave. Finally, our analysis relies on self reports of caregiving spells, which may not exactly correspond to the initial onset of caregiving responsibilities. However, self-identification is arguable of greater policy interest if employer and federal policies around caregiving leave and other caregiver respite programs require caregivers to self-identify as such. In future work, we intend to examine the effects of caregiving on the distribution of earnings and family income, and to leverage the long administrative panel combined with rich survey data to tie together caregiving obligations across the life course and their combine effect on the employment trajectories of caregivers.

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FIGURE 1: Caregiving and Employment: Administrative Data



Note: The sample includes all SIPP respondents over 18 who have been providing care for two years or less at the time of the informal care module. Panels display regression coefficients and associated 95% confidence intervals from Equation 1, using SSA administrative data. Coefficients for μ_{-2} are normalized to zero. Standard errors clustered at the individual level.

FIGURE 2: Caregiving and Employment: Administrative Data, by Age



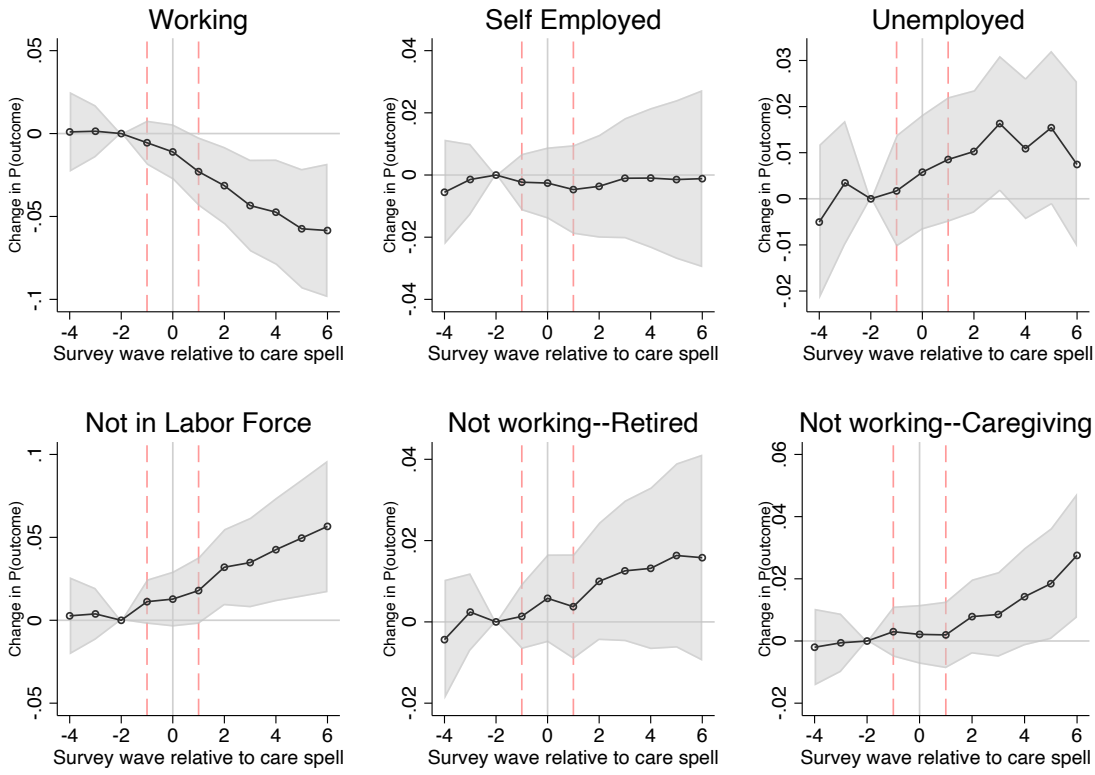
Note: The sample includes all SIPP respondents over 18 who have been providing care for two years or less at the time of the informal care module. Panels display regression coefficients from Equation 1, using the linked SIPP-SSA administrative data. Coefficients for μ_{-1} are normalized to zero. Standard errors clustered at the individual level.

FIGURE 3: Caregiving and Employment: Administrative Data, by Gender



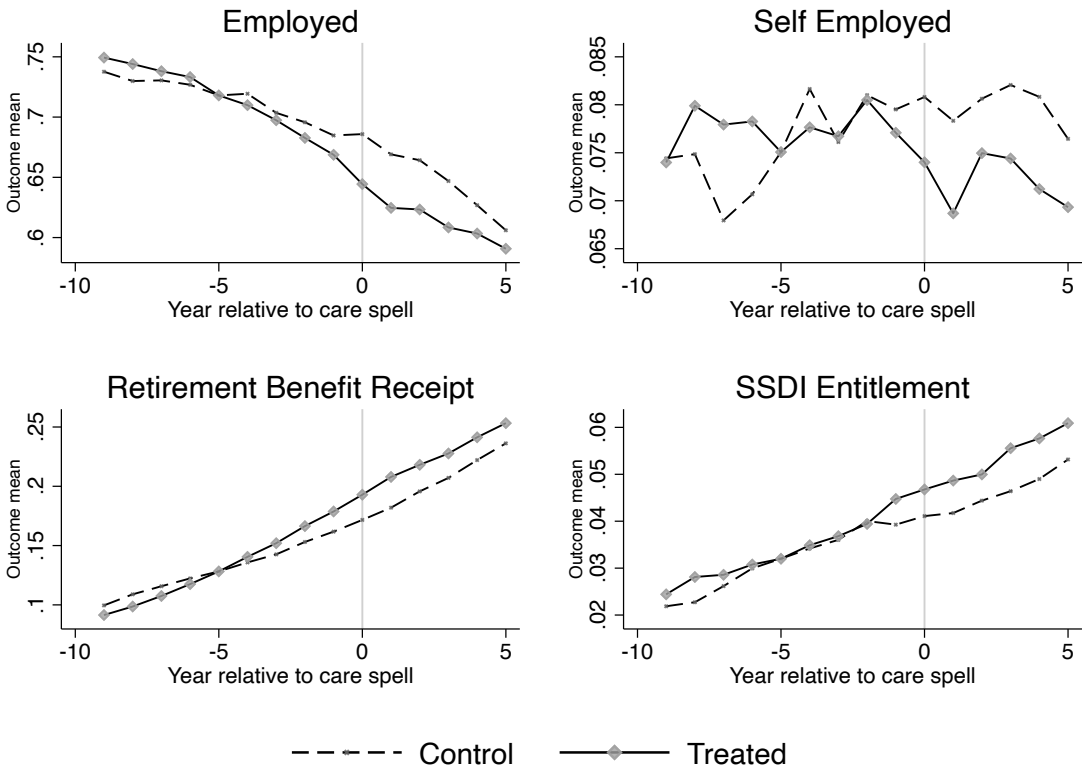
Note: The sample includes all SIPP respondents over 18 who have been providing care for two years or less at the time of the informal care module. Panels display regression coefficients from Equation 1, using the linked SIPP-SSA administrative data. Coefficients for μ_{-1} are normalized to zero. Standard errors clustered at the individual level.

FIGURE 4: Caregiving and Employment: SIPP Data



Note: The sample includes all SIPP respondents over 18 who have been providing care for two years or less at the time of the informal care module. Panels display regression coefficients and associated 95% confidence intervals from Equation 1, using the SIPP survey data. Coefficients for μ_{-3} are normalized to zero. Standard errors clustered at the individual level.

FIGURE 5: UNADJUSTED OUTCOMES IN TREATMENT AND CONTROL GROUPS



Note: Figure plots raw (non-regression-adjusted) average outcomes for caregivers in treatment and control groups. Sample is all caregivers in the SIPP 18+ who have been providing care for two years or less. Treatment group is defined as caregivers who start care spell in the given year; control group is defined as caregivers who will start providing care at least 6 years in the the future

FIGURE 6: Caregiving and Employment: Stacked Administrative data



Note: The sample includes all SIPP respondents over 18 who have been providing care for two years or less at the time of the informal care module linked to the SSA administrative data. Panels display regression coefficients and associated 95% confidence intervals from Equation 2. Coefficients for μ_{-2} are normalized to zero. Standard errors clustered at the individual level.

TABLE 1: DESCRIPTIVE STATISTICS: SIPP INFORMAL CARE TOPICAL MODULE

	Mean
Full Sample	(N= 237,385)
Any caregiving	0.05
Caregiver Sample	(N=12,743)
Caring for:	
Spouse	0.15
Parents or in-laws	0.22
Other relative	0.19
Other	0.25
Child	0.24
Caregiver to Adults Sample	(N=10,209)
Hours of care per week	10.9
Num adults caring for	1.2
Care recipient outside household	0.59
Helps with ADLs	0.41
Helps with IADLs	0.92
Helps with medical care	0.51
Sole caregiver	0.48
Started Care:	
Within the past year	23.0
1 year ago	12.0
2 years ago	15.4
3+ years ago	50.0

Notes: Table reports weighted summary statistics for the pooled 1996, 2001, 2004 and 2008 SIPP surveys. Statistics are measured during the Informal Care module.

TABLE 2: Demographic and Employment Characteristics

	Caregivers <3 years (1)	Caregivers 3+ years (2)	Non Caregivers (3)
Panel A: SIPP survey Sample			
Age	50.35	53.18	43.55
Female	0.64	0.62	0.51
White	0.74	0.75	0.70
Black	0.12	0.11	0.11
Hispanic	0.09	0.09	0.13
Married	0.60	0.62	0.52
College Grad	0.24	0.26	0.23
Working	0.55	0.53	0.61
Self-employed (any)	0.10	0.11	0.09
Unemployed	0.05	0.04	0.04
Not in the Labor Force	0.40	0.42	0.34
Retired	0.20	0.23	0.14
Working Part Time	0.40	0.40	0.32
Usual Hours	30.67	32.27	34.83
Total Monthly Earnings (Unconditional)	1734.02	1691.91	2013.00
Total Monthly Earnings (Conditional)	3390.87	3531.40	3522.77
Family Monthly Income	5921.14	5958.20	6433.80
Panel B: Linked Administrative Data Sample			
Employed	0.65	0.59	0.71
Self-employed (any)	0.08	0.08	0.08
Earnings	28,424	26,814	34,503
Earnings (cond.)	43,974	45,304	48,722
Self-emp earnings (cond.)	19,934	24,714	30,512
Receiving OASI	0.24	0.29	0.19
Receiving DI	0.04	0.05	0.04

Notes: Table reports weighted summary statistics for the pooled 1996, 2001, 2004 and 2008 SIPP surveys in Panel A. Panel B reports summary statistics for the linked pooled SIPP-SSA sample. Statistics are measured during the Informal Care module in panel A and in the same year as the informal care module in Panel B. Column 1 reports statistics for the sample of respondents who report starting care in the previous 2 years, and column 2 reports statistics for respondents providing care for 3 or more years. Column 3 reports statistics for non caregivers.

TABLE 3: STACKED DIFFERENCE IN DIFFERENCES

	Full Sample (1)	Men (2)	Women (3)	Age <40 (4)	Age 40-61 (5)	Age 62 + (6)
			Panel A: Employed			
Treat × Post	-0.0209***	-0.0275**	-0.0179*	-0.0087	-0.0271**	0.0065
	(0.0078)	(0.0129)	(0.0098)	(0.0138)	(0.0117)	(0.0204)
Treat × Post (Year 0-2)	-0.0262***	-0.0232*	-0.0284***	-0.0127	-0.0383***	0.00215
	(0.0079)	(0.0130)	(0.0099)	(0.0145)	(0.0115)	(0.0201)
Treat × Post (Year 3-5)	-0.0146	-0.0327**	-0.0055	-0.0033	-0.0124	0.0124
	(0.0093)	(0.0157)	(0.0117)	(0.0167)	(0.0146)	(0.0243)
Mean	0.72	0.77	0.69	0.83	0.79	0.34
			Panel B: Self-Employed			
Treat × Post	-0.0021	0.0044	-0.0064	0.0054	0.0011	-0.0028
	(0.0050)	(0.0096)	(0.0057)	(0.0097)	(0.0077)	(0.0118)
Treat × Post (Year 0-2)	-0.000709	0.00817	-0.00642	0.00365	0.00288	0.00387
	(0.00533)	(0.0101)	(0.00616)	(0.0101)	(0.00818)	(0.0116)
Treat × Post (Year 3-5)	-0.00379	-0.000146	-0.00638	0.00790	-0.00128	-0.0118
	(0.00596)	(0.0117)	(0.00667)	(0.0119)	(0.00913)	(0.0141)
Mean	0.08	0.11	0.06	0.07	0.09	0.07
			Panel C: Collecting Social Security			
Treat × Post	0.000730	-0.00316	0.00287		-0.00000108	-0.00799
	(0.00374)	(0.00570)	(0.00472)		(0.00612)	(0.0109)
Treat × Post (Year 0-2)	0.00325	-0.000984	0.00500		0.00489	-0.00000786
	(0.00383)	(0.00614)	(0.00473)		(0.00580)	(0.0126)
Treat × Post (Year 3-5)	-0.00231	-0.00587	0.000335		-0.00644	-0.0186*
	(0.00455)	(0.00690)	(0.00586)		(0.00831)	(0.0105)
Mean	0.13	0.17	0.11		0.03	0.69
			Panel D:Eligible for SSDI			
Treat × Post	0.00749**	0.00982	0.00658*	0.0116**	0.0131**	-0.00846
	(0.00329)	(0.00630)	(0.00377)	(0.00483)	(0.00553)	(0.00692)
Treat × Post (Year 0-2)	0.00704**	0.00312	0.00948***	0.0118***	0.0137***	-0.0138*
	(0.00311)	(0.00594)	(0.00357)	(0.00431)	(0.00508)	(0.00801)
Treat × Post (Year 3-5)	0.00803*	0.0181**	0.00312	0.0114*	0.0124*	-0.00138
	(0.00416)	(0.00787)	(0.00481)	(0.00669)	(0.00715)	(0.00634)
Mean	0.03	0.04	0.01		0.05	0.02
N	278535	100250	178284		152330	50413
cN	4280	1533	2767		2123	702

Notes: The sample includes all SIPP respondents over 18 who have been providing care for two years or less at the time of the informal care module linked to SSA administrative data. The first row in each panel reports results from a modified version of Equation 2 and the next two rows report results from a separate indicator for years 0-2 and years 3-5 after caregiving starts interacted with the treatment variable. All columns include individual, state and year fixed effects. Robust standard errors clustered at the individual level are reported in parentheses. (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$)

Appendix

TABLE A1: DESCRIPTIVE STATISTICS: ADMINISTRATIVE MATCH

	Caregiver Sample		Full SIPP	
	No Match (1)	Match (2)	No Match (3)	Match (4)
Age	49	52	44.5	47.6
Female	0.62	0.633	0.533	0.537
Education:				
Less than high school	0.164	0.11	0.193	0.138
High school	0.308	0.271	0.32	0.282
Some college	0.291	0.362	0.281	0.331
Bachelor's degree or more	0.237	0.256	0.206	0.249
Race/Ethnicity:				
Non-Hispanic White	0.707	0.767	0.641	0.747
Non-Hispanic Black	0.124	0.103	0.128	0.112
Hispanic	0.11	0.083	0.167	0.086
Caring for:				
Spouse/partner	0.16	0.15		
Parent	0.238	0.282		
Other relative	0.217	0.246		
Other non-relative	0.401	0.347		
Child	0.011	0.015		
N	952	4297	48,600	185,047

Notes: Table reports descriptive statistics measures at year of the caregiving module for pooled 1996, 2001, 2004 and 2008 sample of SIPP caregivers (columns 1& 2) and the full SIPP sample (columns 3 & 4).