Disability Insurance in the United States and Multiple-Earner Households[1]

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## Abstract

How does Disability Insurance (DI) interact with household labor supply as a mechanism for insuring against health shocks? In this paper, I argue that several features of DI programs in the United States (SSDI and SSI) may drive such interactions, with implications for who decides to apply for DI. In the first part of this paper, I look at how the decision to apply for DI and the resulting outcomes of DI applications depend on the characteristics of one's household. Using the SIPP Gold Standard File from 1984 to 2014, I document little in the way of evolutions in spousal labor supply or income over the application process. I further find little in the way of spousal labor supply responses to the allowance or rejection of an applicant from DI. I find that nearly all applicants linkable to time-of-filing information in the SIPP-SSA data are allowed onto DI. I conclude that this reflects serious limitations in the utility of the SIPP-SSA Gold Standard File, which includes limited timing information on only *most recent* applications, for examining DI application decisions. I find that non-married applicants are less likely to have ever applied for DI, but this is explained by differences in age between married and non-married SIPP participants. Accounting for differences in age, non-married applicants are significantly *more* likely to have ever applied for DI. With non-married applicants also showing weaker labor force attachment, I conclude that these facts are consistent with marital status being negatively correlated with (unobservable) individual health. The main conclusion of this section is that having *only* DI application dates from an applicant's *most recent* applications is insufficient to study joint work and application decisions within households. This is because people who choose to apply may submit multiple DI applications.

In the second part of this paper, I examine how Medicaid expansion shut down one mechanism driving the relationship between DI take-up and household work: health insurance access. Using state-level Medicaid expansions of the Affordable Care Act from 2013 to 2016, I find a substantial 1.3 percentage point increase in SSDI take-up among individuals living alone as a consequence of Medicaid expansion. Effects increase to as large as 2 or 3 percentage points in later years. This increase is not observed in other household structures, and is not explained by contemporaneous changes in average employment. I find no detectable effects on SSI receipt, which provides recipients with fewer potential gaps in health insurance coverage than SSDI. This suggests that the lack of access to health insurance while applying for DI may have dissuaded some single individuals from applying for DI benefits. CPS estimates indicate that individuals living alone make up about 19% of the overall U.S. population between 18 and 65 during the Medicaid expansion years.

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## 1 Background

How do individuals jointly make the decision to apply for disability insurance (DI) benefits along with the labor supply decisions of their household? This paper attempts to answer this question by examining how individuals from different types of households choose to apply for disability insurance benefits and how the work decisions of applicant households evolve over the DI application process. It secondly examines how health insurance access may be one mechanism by which household labor supply drives DI applications, exploiting variation in health insurance access induced by Medicaid expansions. This paper is related to two deep literatures in economics: one on DI programs in the United States and other developed countries, and another on the capacity for households to self-insure themselves against negative shocks.

In the former, Autor and Duggan have thoroughly explored the importance of many factors to explaining the rise in SSDI applications and SSDI rolls from 1980 onward in two well-known papers–including the role of rising female labor supply (D. H. Autor & Duggan, 2003, 2006; Duggan & Imberman, 2009). The literature has largely examined disability with an implicit assumption that individual work decisions are separately determined ((Low & Pistaferri, 2015), (Maestas, Mullen, & Strand, 2013),(D. H. Autor & Duggan, 2006)). However, the key idea of this paper is that male and female labor supply and disability application decisions may be *jointly* determined through a shared households. This paper is, to my knowledge, the first attempt to examine the joint determination of work and DI application decisions within US households. However, Autor et al. (2017) find evidence of these interactions in a causal and more narrow context in Norway (D. Autor, Kostol, Mogstad, & Setzler, 2017). They exploit the random assignment of DI appellant cases (which were initially rejected from DI) to DI judges who differ in the stringency with which they judge cases, finding that rejection from DI causes greater spousal labor force participation.

In examining household ex-post responses to disability onset, this paper also relates to the literature on household mechanisms for insuring against adverse health and income shocks. With the many ways in which negative health shocks may affect household preferences and budget sets, the optimal labor supply responses of households (with multiple members, or even single-person households) to disability shocks is an ex-ante ambiguous empirical exercise. This is true even in the absence of public insurance programs. There is evidence that transfer programs crowd out an insurance function for spousal labor supply in unemployment insurance, and evidence for spousal labor supply insuring households against fatal health shocks (Cullen & Gruber, 2000; D. Autor et al., 2017). The onset of work-limiting disability, though, may be more akin to a non-fatal adverse health shock than a fatal one or a pure employment shock. Spousal labor supply does not appear responsive to acute and non-fatal health shocks in Denmark (which more thoroughly insures individuals against such shocks than does the U.S.), nor does it seem responsive to hospital admissions in the United States (Fadlon & Nielsen, 2015; Dobkin, Finkelstein, Kluender, & Notowidigdo, 2018). However, the types of disabilities which qualify an individual for DI benefits are longer-term. One may expect greater responses for these more long-term shocks. These longer-term disabilities may require formal or informal care that, on the other hand, restricts spousal labor supply.

Where I examine the role of health insurance in driving differential responses to disability risk across household structures, my empirical strategy relates to the literature on the consequences of Medicaid expansion. This is not the first paper to examine the effect of Medicaid expansion on DI program applications; Hyde et al. (2017) use SSA administrative data to estimate (mostly

positive) average effects of expansion on DI applications in expanding states . The contribution of this paper is to document heterogeneity in the effects of expansion as a function of household structure, motivated by the idea that single-person households have fewer options for maintaining private health insurance coverage (while applying for DI and during the Medicare waiting period for SSDI recipients) and maintaining income (while applying for DI). The types of individuals for which these matters of health insurance access are relevant represent a modest share of the United States population and changes in their average behavior are of some consequence. Approximately 19% of the population belongs to these single households in 2014 and onward, according to CPS estimates.

### 1.1 U.S. Disability Programs

Applying for disability insurance benefits requires that an individual be earning below "Substantial Gainful Activity" (SGA, updated annually with a value of $300 monthly in 1984 and $1,070 in 2014), which puts nontrivial constraints on work hours for even minimum wage workers. This restriction applies both during the application process and while receiving benefits, but it does not apply to the earnings of household members. DI payments may be made through two programs: the larger Social Security Disability Insurance (SSDI) program which puts no restrictions on assets and which makes payments as a function of an individual's wage history, and the smaller asset-tested Supplemental Security Income (SSI) program which pays fixed benefits.[3] Payments through SSDI follow the same formula as standard Social Security Retirement benefits. An individual may receive benefits through both SSI and SSDI if they qualify for SSDI on their work history and qualify for SSI by means tests, though SSI benefits are reduced as a function of SSDI benefits. SSDI benefits disregard other household members altogether, but assets and income of a working spouse will count against means tests for SSI and will reduce SSI payments.

Applying for DI (SSI and/or SSDI) benefits is a potentially costly process. Initial DI decision waiting periods in 2005 were 2.8 and 3.0 months for those initially allowed and initially denied respectively (D. H. Autor, Maestas, Mullen, & Strand, 2015). Only approximately a third of applicants are initially allowed in the 1990's and 2000's, with about two thirds of the initially denied appealing their claim.[4] Those initially denied faced average total wait times of over two years in 2005 if they choose to appeal or reapply (D. H. Autor et al., 2015). Conditional on choosing to apply for DI, applicants often appeal and reapply for benefits until their case is eventually allowed; only about 20% of applicants in the 1990's ended their process with a final rejection within two years of their filing (French & Song, 2014).

SSDI beneficiaries also become eligible for Medicare only after 24 months of SSDI benefit entitlement. With health insurance largely being employer-provided, this exposes DI applicants to a long gap in health insurance coverage in the absence of alternatives to employer-provided insurance, even if benefits are awarded quickly. SSI benefits are not subject to these restrictions; SSI beneficiaries immediately begin receiving payments and Medicaid coverage upon DI allowance.

A DI beneficiary may leave DI receipt voluntarily by returning to work; an individual will

[3] $721 and $1,082 were the federal amounts for an individual and couple respectively in 2014, reduced by other income. Most states supplement the federal benefit for at least some beneficiaries.

[4] The initially denied may appeal their decision through several stages: reconsideration in the original DDS office by different staff, appeal to an administrative law judge, review by the SSA Appeals Council, and review by Circuit Court of Appeals.

continue receiving benefits during the first 9 months of work (not necessarily consecutive). After those 9 months, benefits are provided for three years in any month in which the disabled individual earns less than SGA. Beginning in the 37th month, the individual is made ineligible for benefits if they earn more than SGA in any month. If a beneficiary stops receiving SSI benefits due to excess income, they may restart benefits without reapplication within 5 years of benefit cessation. A disabled individual may also be removed involuntarily from either program by continuing disability review, though these disability reviews were extremely rare from the mid 1980's through 2000 (less so in recent years), and well below 10% lead to removals from DI. (Bureau, 2015)

## 2 Research Design, Methods, and Data Analysis

### 2.1 Households and the Application Process

The first part of this paper consists of examining several differences over types of households: in the decision to apply for disability insurance, in behavior over the application process, in application outcomes, and in behavior after application decisions have been made. For this part of the paper I use limited access to version 7 of the SIPP Gold Standard File through Census validation of results on the SIPP Synthetic Beta. The SIPP Gold Standard File is a Census Bureau restricted-use data file composed of select information from the Survey of Income and Program Participation (SIPP) panels 1984-2008, linked with administrative data on earnings histories from the IRS and select administrative data on DI applications from the SSA from 1984-2014 (Reeder, Trageser, & Vilhuber, 2018). Key information for my analysis includes the time when DI applications are filed, the time when application decisions are made, and the final outcome of the DI applications. In the Gold Standard File, timing information is available only for the most recently filed applications by individuals who have not yet claimed retirement benefits by 2014. The result is that I only observe this information on the *most recent* applications for DI, for a sub-sample of individuals who consented to linking their survey information to their social security number (about 75%) and who tend to belong to more recent birth cohorts.

Household links in this data come from the SIPP survey, and are measured at the time of interview in the SIPP. Part of my analyses involve drawing comparisons across observably distinct types of households. I first separate married individuals in the SIPP from non-married individuals in the SIPP Gold Standard File.[5]. I compare demographic characteristics and DI application rates across these two groups.

I construct two data panels, for households containing an individual who applies for DI benefits before the age of 59: one surrounding the filing of the individual's *most recent* DI application and one surrounding the allowance decision for that same application (with dates measured in years, these two events are different to the extent that they occur in different calendar years). These panels are designed to include the same exact samples of households, by restricting to a balanced sample of households with observable earnings information in the 4 years before filing the application through the 4 years after receiving an allowance decision. I compare the characteristics of households in this sample to individuals in the overall SIPP sample. In addition to applying standard SIPP survey weights (weighing up the overall SIPP panels to be cross-sectionally representative of the US civilian non-institutionalized population), I apply a reweighting procedure to address

[5]Other information on family and household ties is not available in the SIPP Gold Standard File

differences in the distribution of birth cohorts across married and non-married applicants. This addresses the fact that non-married applicants tend to come from younger birth cohorts than married applicants.

I then cut the married and non-married applicant groups into sub-groups, based on labor force attachment in the years prior to their most recent application for DI. I separate non-married applicants into those with strong labor force attachment (earning at least SGA in all 4 years prior to applying for DI) and those with weak labor force attachment (the remaining non-married applicants). I make two cuts of the married sample: first by their own labor force attachment (as for the single applicants) and then separately by the labor force attachment of their spouses.

### 2.2 The Health Insurance Mechanism: Medicaid Expansion

In this section, exploit state-level expansions of Medicaid to examine the effect of health insurance access on DI take-up as a function of an individual's household structure. To do this, I will use cross-sectional data from the Current Population Survey (CPS), comparing outcomes among individuals who live in expanding states to individuals living in non-expanding states. One potential drawback of this approach is that I cannot condition on the stability of household environment; I only observe it cross-sectionally.

I use data from the Current Population Survey (CPS) Annual Social and Economic Supplement (ASEC), 2000-2017. The CPS provides a representative sample of U.S. non-institutionalized civilian households in these years, with a sampling frame drawn from address lists from the Decennial Census. (*Design and Methodology, Current Population Survey*, 2006) While the CPS is longitudinal in nature and survey information is collected annually, information on self-reported disability and disability benefit receipt is only collected in the ASEC. With panels lasting 12 months, I cannot exploit the panel nature of the CPS for studying these outcomes.

Social Security payments are reported for each individual in the household from 2000 onward, including the reason for receiving those payments (e.g., SSDI, SSI, or retirement). Reports are also provided for each individual indicating whether or not they had "a health problem or a disability which prevents him/her from working or which limits the kind or amount of work." I classify individuals into three household environments based on information at the time of survey: those not married and living with no relatives (potentially living with boarders or roommates), those married and living with a spouse (along with potentially other relatives), and those not married and living with relatives. I drop individuals who are married and not living with a spouse, and unmarried individuals living with non-relatives in the interest of focusing on comparing individuals living independently against individuals living with a long-term partner. I make no restrictions on age (the CPS surveys individuals age 18-65), sex, or relation to the sampled household except for the restrictions already described in defining household structures.

I follow Hyde et al. (2017) in excluding 10 states which either substantially expanded Medicaid eligibility to adults without dependents well in the late 1990's or early 2000's, or which had state-funded health insurance programs covering low income individuals without dependents.[6] Individuals in these states arguably did not face the potential for a substantial change in access to health insurance coverage independent of DI receipt with the passage of the Affordable Care Act.

My difference-in-differences strategy will hinge on the assumption:

[6]Those states are California, Delaware, Hawaii, Iowa, Maryland, Massachusetts, Minnesota, New York, Vermont, and Washington D.C.

**Assumption 1.** *Individuals are as-good-as randomly assigned to states of residence, conditional on education, birth cohort, race, gender, and state policies regarding interactions between disability and Medicaid.*

with which I may identify an average effect of Medicaid access on household outcomes, $\beta$, in expressions of the following form:

$$Y_i = \alpha'_{h(i)} X_i + \sum_t \lambda_{h(i)t} D_i^t + \beta_{h(i)} Post_i \times Expansion_i + \epsilon_i \tag{1}$$

where $h(i)$ denotes the household structure associated with individual $i$, and $X_i$ is a vector of controls including dummies for state residence, age group, sex, race, and education. $D_i^t$ is an indicator that individual $i$ was surveyed in calendar year $t$, and $Expansion_i$ is an indicator for living in an expanding state. I also estimate an expression providing year-by-year differences between individuals in expanding and non-expanding states:

$$Y_i = \alpha'_{h(i)} X_i + \sum_t \lambda_{h(i)t} D_{it} + \sum_\tau \delta_{h(i)\tau} D_{i\tau} \times Expansion_i + \eta_i \tag{2}$$

Medicaid expansion occurs in 2014, 2015, and 2016 for different sets of states; the coefficients $\delta_{h(i)\tau}$ are identified off of variation in average outcomes in expanding states $\tau$ years after (or before) expansion relative to contemporaneous control states. The estimates I present for parameters of interest $\delta_{h(i)\tau}$ and $\beta_{h(i)}$ represent weighted averages of treatment effects after pooling across the year-of-expansion-specific difference-in-differences. I weigh the data so that $\hat{\beta}_h$ and $\hat{\delta}_{h(i)t}$ estimate the average effect of expansion on individuals living in expanding states; the average treatment effect on the treated. This includes reweighing the data so that individuals living in the same household structure in expanding and non-expanding states match on the distribution of age, education, race, and exposure to certain Medicaid-related state policies. The statistics on these observable characteristics are reported before and after reweighing for expanding and non-expanding states in Table A3.

The state policies used in the weighting procedure are borrowed from Hyde et al. (2017): the presence of a Medicaid Medically Needy program (covering high medical expense for individuals who do not qualify for Medicaid on financial grounds), and whether whether state SSI recipients are automatically enrolled in Medicaid (state's Section 1634 status) . All standard errors are clustered at the state level.

I will examine the effect of Medicaid expansion on both SSI and SSDI take-up, and on the characteristics of the beneficiaries of these programs. The institutional details described in Section 1.1 give good reason to believe that health insurance access will interact mores strongly with SSDI than SSI.

## 3 Results

### 3.1 Households and the Application Process

I begin by comparing demographic characteristics and DI application rates across married and non-married individuals in the SIPP sample who have been successfully linked to administrative data.

This information is reported in Table A1, which reports characteristics for the unweighted SIPP sample, and for the SIPP sample after applying weights. Non-married SIPP participants slightly less likely to have applied for SSDI, are less likely to be allowed on SSDI if applied, are born a full 20 years later than married participants, are less educated, are slightly more female, are less white, and are less likely to be dead in administrative data. Many non-married participants were married at some point prior to interview in the SIPP (20%). Of those, most marriages ended in divorce (about 60%), though some (about 34%) ended in widowhood. The remaining participants were married but not matched to a cohabitant spouse, and remain part of the non-married sample. In the third column of this table (and in the following event studies) I apply weight adjustments according to the distribution of birth cohort over the original survey weights. Differences in birth cohort do not explain other demographic differences between married and non-married participants. However, non-married SIPP participants are *more* likely to have applied for DI, and differences in DI allowance conditional on applying mostly vanish.

Table A2 focuses in on the SIPP participants with available information on a DI application submitted before the age of 60, who will form the sample for the event studies to follow. Non-married applicants tend to apply at earlier ages and are born about 6 years later than married applicants on average. While the majority of all applicants tend to have high labor force attachment (earning over SGA in each of the four years prior to applying), married applicants are about 7 percentage points more likely to have high attachment than non-married applicants. Over half of married applicants also have spouses with high labor force attachment. A greater share of non-married applicants applicants are female. There are no substantial differences in education between married and non-married applicants; all applicants tend to be fairly low-educated with the median applicant having a high school degree. All applicants are on average white, but non-married applicants are about 12 percentage points less likely to be white than married applicants. Relative to the overall sample, non-married applicants are much more likely to be divorcees or widows. As in Table A1, the last column of Table A2 includes reweighing on the distribution of cohort. These weights were constructed to match the distribution of birth cohort in the overall SIPP sample though, so it is a non-mechanical result that the average birth cohort among married and non-married *applicants* are both around 1950. Even after adjusting for the distribution of birth cohort in this way, non-married applicants tend to apply about 2 years earlier in their lives than married applicants. Differences in age do not appear to explain differences in applicant labor force attachment prior to applying for DI.

In Figure A1, I report the cumulative distribution function for the years required to process DI applications by applicant marital status; I see no differences in processing times between any of the sub-groups by marital status or labor force attachment (which I do not report here).

To examine how spousal work evolves as married applicants wait for the decision on their DI application, I report the share with employed spouses (earning over SGA) in the years before and after filing the DI application (restricting to households which have not yet received their application decision) in Figure A2. I then report average spousal employment earnings among those working spouses separately for the spousal labor force attachment groups in Figure A3. These figures reveal very stable average employment surrounding the filing of the DI application, and a steady decline in spousal employment as years progress from the filing of the application. Based on these analyses alone, though, it is unclear if the trends in work and earnings after filing are due to changes in behavior or changes in the composition of the applicant sample (applicants drop over time from this event study as they receive their allowance decisions).

I report the same event studies of own employment income and earnings separately for married and non-married individuals in Figure A4. The great majority of applicants are working and earning over SGA in the years before filing the application. However, employment rates are dropping for both married and non-married applicants over that time (from about 90 percent to about 80 percent). In that time, unmarried applicant workers earn on average around $\$15,000$ annually while married applicant workers on average earn over $\$20,000$ annually–both facing a small dip in the year just before filing. Unsurprisingly, the share of applicants working drops precipitously in the year of filing, with about 20% of applicants working while awaiting the result of their application. This share increases over time for non-married applicants, but not for married applicants. Earnings while awaiting the application decision are also low (about $\$5,000$ for unmarried applicants and $\$6,000$ for married applicants in the year after filing). They increase slightly over time–moreso for married applicants than unmarried ones. Unmarried applicants who are still waiting their allowance decision earn nearly $\$10,000$. However, this is a very select sub-sample of applicants.

I next examine the outcomes of DI applications, reporting in Figure A5 the share of applicants receiving DI benefits in the years surrounding the allowance decision for married and non-married applicants split by the applicant labor force attachment groups. These plots are striking for two reasons. First, many applicants are receiving DI in the years before determination of the DI applications defining my panel. Supposing this does not reflect administrative error or misinterpretation of the administrative data, this suggests that these applications belong to individuals who had been recently removed from disability insurance. The fact that fewer applicants with strong labor force attachment were receiving benefits in years prior to the allowance decision is consistent with this interpretation, though the shares remain high with over 40% having benefits in the prior year. The second remarkable feature is that virtually all applicants are receiving DI benefits in the years following their most recently filed DI application. This means there are no differences in DI receipt between married and non-married applicants (as we might expect if individuals are strategically choosing to apply for DI) or between applicants with high versus low labor force attachment. While striking, both these features are consistent with the evidence of French and Song (2014) that DI applicants in the United States repeatedly apply for benefits until they are admitted. If applicants appeal and reapply until either they are awarded benefits or they age into eligibility for retirement benefits, then this would explain the very few rejections among the set of applications eligible for linking in the SIPP Gold Standard Files.

I lastly examine applicant and spousal work in the years surrounding the allowance decision of the DI application. In these figures, I restrict to applicants with strong labor force attachment. This is to better focus on applicants who were neither participating in a long string of reapplications for DI nor tied closely to recent benefit receipt. In Figure A6 I report the share of non-married applicants employed at or above SGA, and the average earnings among those employed applicants. I report these values separately for allowed and rejected applicants. Both rejected and allowed applicants were likely to be employed (earning over SGA) in the years before their allowance decision, with declining employment over time as observed previously in igure A4. However, rejected applicants were more likely to work before their allowance decision (on the order of 8 percentage points in the year before the allowance decision, with about 75 percent of allowed applicants working at that time). Further, rejected applicants who worked tended to earn more than allowed applicants who worked; on the order of $1,000 more annually on average. Earnings dip before the allowance decision for both rejected and allowed applicants. This dip is much greater than the one observed in the event studies around the filing of applications, which suggests it

can be attributed to the fact that some applicants waited more than one year for their allowance decisions (and earned less income while awaiting that decision). After rejection from DI, non-married applicant employment continues to decline steadily from about 70% in the year following rejection to about 50% four years later. This may partially reflect natural exits from employment as individuals age over the life cycle. The sample has been selected to include few individuals eligible for social security retirement benefits by the end of the panel (applicants must have filed their applications before age 59), though, so any such exits would be considered early retirement. Among those rejected applicants who work after rejection, earnings bounce back to about $20,000 per year on average after about two or three years. Against the decline in employment over time, though, overall individual employment income among rejected applicants is steadily declining over time after rejection.

Figure A7 reports the same information for married applicants. It shows results on own employment and earnings around the allowance decision which are virtually identical to the results for non-married applicants, with one exception. Differences in earnings among allowed and rejected married applicants are not economically or statistically significant in the years prior to DI allowance.

In Figure A8 I report the share of married applicants whose spouse earns at or above SGA and the average earnings among those spouses. Rejected applicants tend to have spouses who are significantly more likely to work (by about 5 percentage points) and who tend to earn significantly more income conditional on working (about $\$2,000$ to $\$3,000$ annually) over the interval before the DI allowance decision takes place. Strikingly, I see no evidence of upward adjustments in spousal labor supply nearer to the DI allowance decision or after allowed applicants begin receiving benefits, which would be implied by the existence of added worker effects. The pre-allowance trends in spousal employment and earnings persist through the allowance decision for both allowed and rejected applicants. The absence of a noticeable drop in spousal employment among allowed applicants two years after benefit receipt suggests that spousal employment responds very little when Medicare coverage is made available to DI beneficiaries. If anything, spousal employment responds *positively* to Medicare access, if we use rejected applicants to perform a difference-in-difference for spousal employment outcomes in years three and four. However, this stems from spousal employment of *rejected* applicants (not allowed applicants) deviating slightly from their previous linear trend. I estimate this difference-in-difference (not reported here) and the resulting estimate is not statistically significant.

The differences in spousal employment outcomes between rejected and allowed applicants prior to the allowance decision is consistent with selection on spousal work capacity into applying for DI. Individuals who have spouses with better earnings capacity may be willing to apply for DI when they face lower odds of receiving it (perhaps due to better employment prospects or better health), because the application process is comparatively less costly for them. However, this hypothesis would imply differences across spousal labor force attachment in the share of applicants who were allowed onto DI, which I did not detect. On the other hand, these differences could be attributable to age differences between allowed and rejected applicants. Applicants who are rejected in their *most recent* DI application may chose to stop applying for benefits because they are older and anticipate collecting retirement benefits in the near future, whereas younger applicants who are rejected choose to reapply for benefits. This hypothesis seems somewhat at odds (though not necessarily contradictory) with the trends in *own* employment outcomes for rejected married applicants. Both of these trends cannot be explained by common life cycle trends affecting the

work and earnings of both rejected applicants and the spouses of rejected and allowed spouses. However, one might not expect those trends to be common to the extent that rejected DI applicants face some kind of work-limiting disability which could affect their life cycle work outcomes.

### 3.2 Medicaid Expansion - Results

Figure A9 depicts the year-by-year event study of average SSDI benefit receipt associated with Medicaid expansion, grouping states by the year in which they expanded Medicaid. The bottom panel of Figure A9 presents the difference-in-difference coefficients estimated according to Equation 2 for individuals living without family or a partner. SSDI receipt does increase by approximately two to three percentage points in the three years strictly after Medicaid expansion for individuals living alone. The pooled effect of Medicaid expansion on individuals living alone (1.3 percentage points) is significantly different from the (zero) effect on married individuals ($p < 0.01$). This is a substantial increase in SSDI take-up in both relative and absolute terms; the average SSDI take-up among individuals living alone in non-expanding states is 6%. I estimate no economically meaningful effect of Medicaid expansion on SSDI receipt for married individuals and individuals living with family. These differential effects do not appear to be explained by contemporaneous changes in labor demand that uniquely impacted single individuals. I find Medicaid expansion associated with no aggregate changes in employment or earnings in expanding states among individuals living alone (or in the other household structures, see Figure A10). Further, I fail to reject the null hypothesis in a test for parallel trends in the difference-in-difference plot of the lower panel of Figure A9. This suggests that the parallel trends assumption necessary for a difference-in-differences strategy hold.

Figure A11 presents results for the same event study and difference-in-difference equations, taking SSI receipt as the outcome of interest. I find no effects of Medicaid expansion on SSI benefit receipt. Given that SSI applicants are not subject to the two-year waiting period for health care access, and given that SSI applicants tend to be poorer individuals who may not have access to health insurance to begin with, the absence of a detectable effect for SSI receipt is unsurprising.

Interestingly, self-reports of work-limiting disabilities (which were, if anything, decreasing over time in expanding states relative to non-expanding states prior to Medicaid expansion) also increase significantly with Medicaid expansion for individuals living alone and non-married individuals living with family. Figure A12 plots the event studies and the difference-in-difference coefficients for individuals living alone (they are comparable to non-married individuals living with family). These plots show an increase of about 3 percentage points in work-limiting disability, over a baseline of about 15 percent reporting them in non-expanding states. With little evidence of measurable declines in health coincident with the expansion of Medicaid, this result suggests substantial endogeneity in the self-reporting of work-limiting disabilities. This result may be useful for judging the reliability of the self-reported measures of the CPS, in addition to similar self-reported measures in surveys like the SIPP and the Panel Study of Income Dynamics (PSID).

Next, I consider how the *characteristics* of SSDI beneficiaries change as a consequence of Medicaid expansion. This allows me to say something about how the targeting of SSDI is affected by Medicaid expansion. I use the same estimating equation to identify difference-in-difference effects of expansion on education and household income of SSDI beneficiaries by household structure (Figures A13 and A14 respectively). I find no significant changes in household income; this is not surprising for individuals living alone by definition of their household structure. With no

family or partners and limits on own employment income, the variation in household income of DI beneficiaries living alone should come largely from variation in DI benefits, other government transfers, and other non-work income. I find no effect on education level of beneficiaries either.

## 4 Discussion and Implications

### 4.1 Households and the Application Process

The examination of the linked SIPP-SSA data revealed several insights into a limited snapshot of the interactions between households and US disability insurance programs.

First, non-married SIPP participants are marginally less likely to have ever applied for SSDI than married SIPP participants. This is apparently attributable to differences in the ages of the married and non-married; the non-married are several percentage points *more* likely to have ever applied for DI when we adjust for differences in the distribution of age. However, it is likely that marital status (especially as individuals age) may be correlated with health. This is consistent with the fact that non-married applicants have weaker labor force attachment than married ones, despite the fact that they are younger. Unfortunately, the SIPP Gold Standard Files are limited in their capacity to address those concerns. Applicants do not appear to face different processing times for their applications as a function of their marital status, own employment histories, or spousal employment histories. This is despite the fact that we might expect more contentious applications to take longer to process, and we might expect applications arising from certain types of households to be more contentious than those arising from other types of households.

Second, there are no detectable differences in the outcomes of DI applications as a function of these characteristics either. This is because nearly all applicants are receiving benefits after the allowance decision, regardless of marital status or employment histories of the applicant or their spouse.

Third, many applicants are receiving DI benefits at some point in the years prior to the filing the applications which were linked to date of filing and adjudication information in the SIPP-SSA Gold Standard File. This suggests that many cases in this data are reapplications of individuals who were previously removed from DI benefits. If true, it together with the second point suggests that the most recent DI applications submitted by an individual only provides a small snapshot into a broader strategy on the part of the household. It suggests household behavior surrounding *earlier* applications may be essential for understanding the interactions between households and the decision to apply for DI benefits in the United States. That would pose deep challenges for the utility of current iterations of the SIPP-SSA Gold Standard Files in examining the decision to apply for DI benefits. The Gold Standard Files include timing information on only the *most recent* applications to DI for SIPP participants who have not yet claimed Social Security retirement benefits.

Fourth, applicant employment and earnings (married or unmarried) evolving over the application process in an unsurprising way; applicants tend to be mostly employed in the years before applying, with a slight decline in both just prior to filing the application and a precipitous drop in both while the application is being processed. We see employment among rejected applicants steadily declining in the years following rejection, while average earnings seem to recover roughly within two years to the levels enjoyed two years before the allowance decision. However, this is among the smaller selected sub-sample who remain employed at that time; own employment in-

come among rejected applicants is falling over time. This is unlikely to be explained by aging into Social Security retirement, given the manner in which the sample was selected on applicant age at the time of filing.

Fifth, married applicants tend to have spouses who participate actively in the labor force, but we detect no clear dynamic adjustments in spousal labor supply over the DI application process. Spouses of rejected applicants tend to work more and (conditional on working) earn more money than allowed applicants. This could be consistent with applicants that have more work-capable spouses being healthier or more able to work themselves (or at least more likely to be judged so by examiners at the Social Security Administration). It could alternatively be explained by any number of other characteristics, like age, which could differ between applicants whose *last* recorded application is a rejection and applicants whose *last* recorded application is an allowance. Age or other life cycle considerations may be particularly important, as older rejected applicants may forgo reapplying in anticipation of collecting retirement benefits in the near future.

The analysis in this report suggests there could be great value in revisiting these questions with a dataset which includes richer information on the full history of interactions between households and disability insurance programs, and better information on health. This is a direction of research I am actively pursuing through the Census Restricted Data Center. Supposing that interactions between DI application and household labor supply exist, then I will pursue deeper research questions regarding the implications this has for the welfare value of disability insurance–and how that may change with policies that affect allowance rules for DI programs or affect health insurance access for disabled individuals.

### 4.2 Medicaid Expansion

Examining the effect of Medicaid expansion on disability insurance take-up, I find large increases in SSDI receipt associated with Medicaid expansion among individuals living alone, but not among individuals in other household settings. In the years following Medicaid expansion, SSDI receipt among individuals living alone increased by almost 2 percentage points more than in non-expanding states, on average. This increase appears unique to SSDI and to single individuals, which is consistent with limited health insurance access during the application process screening out these individuals and not others. Alternative explanations for this result exist. For instance, the effect of Medicaid expansion on SSDI take-up of single individuals may instead operate through effects on low-skill employment which disproportionately affect single individuals. However, Medicaid expansion did not appear to impact average employment in expanding relative to non-expanding states for these individuals. Overall, the results of the Medicaid expansion difference-in-difference analyses suggest that lack of health insurance access may have been heavily screening individuals living alone out of SSDI receipt.

I also find a large increase in self-reported work-limiting disabilities as a consequence of Medicaid expansion. This result appears to suggest the self-reporting of work-limiting disabilities are somewhat endogenous, confounding an analysis of *responses* to self-reported disability in similar survey data.

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Table A1: Observable Characteristics of Married and Non-Married SIPP Participants

| | Unweighted | | Survey Weighted | | Survey Weighted, Cohort-Adjusted | |
|---|---|---|---|---|---|---|
| N ≈ | 326000 | 260000 | 326000 | 260000 | 326000 | 260000 |
| Variable | Unmarried | Married | Unmarried | Married | Unmarried | Married |
| Ever Applied for SSDI | .1435 | .1526 | .1451 | .1539 | .1962 | .1232 |
| | (.0006) | (.0007) | (.0006) | (.0007) | (.0007) | (.0006) |
| Ever Allowed SSDI (if applied) | .505 | .6413 | .5105 | .651 | .5692 | .5943 |
| | (.0023) | (.0024) | (.0023) | (.0024) | (.0023) | (.0025) |
| Birth Cohort | 1971 | 1951 | 1969 | 1949 | 1960 | 1959 |
| | (.042) | (.0328) | (.0423) | (.0329) | (.041) | (.0443) |
| Male | .4667 | .5026 | .4682 | .5028 | .4429 | .4741 |
| | (.0009) | (.001) | (.0009) | (.001) | (.0009) | (.001) |
| Educ. - No HS Degree | .3106 | .1479 | .3125 | .1544 | .2643 | .1788 |
| | (.0012) | (.0007) | (.0012) | (.0008) | (.0011) | (.0008) |
| Educ. - HS Degree | .2808 | .3207 | .2835 | .3322 | .3041 | .3111 |
| | (.0011) | (.001) | (.0011) | (.001) | (.0012) | (.001) |
| Educ. - Some College | .2706 | .2803 | .2617 | .2674 | .2666 | .2806 |
| | (.0011) | (.0009) | (.0011) | (.0009) | (.0011) | (.0009) |
| Educ. - College Degree | .0966 | .1635 | .1001 | .1606 | .1114 | .1565 |
| | (.0007) | (.0008) | (.0008) | (.0008) | (.0008) | (.0008) |
| Educ. - Grad Degree | .0414 | .0876 | .0422 | .0854 | .0535 | .073 |
| | (.0005) | (.0006) | (.0005) | (.0006) | (.0006) | (.0005) |
| Race - White | .7807 | .8787 | .8004 | .8892 | .7999 | .8761 |
| | (.0007) | (.0006) | (.0007) | (.0006) | (.0007) | (.0006) |
| Race - Black | .1624 | .0729 | .1483 | .0666 | .1545 | .0711 |
| | (.0006) | (.0005) | (.0006) | (.0005) | (.0006) | (.0005) |
| Race - Other | .0569 | .0484 | .0513 | .0442 | .0457 | .0528 |
| | (.0004) | (.0004) | (.0004) | (.0004) | (.0004) | (.0004) |
| Hispanic | .1051 | .0749 | .0955 | .0688 | .0846 | .0905 |
| | (.0005) | (.0005) | (.0005) | (.0005) | (.0005) | (.0006) |
| Ever Married | .2107 | . | .2122 | . | .3333 | . |
| | (.0007) | (.) | (.0007) | (.) | (.0008) | (.) |
| Ever Divorced (if Married) | .5959 | .1991 | .5941 | .1945 | .6384 | .1582 |
| | (.0019) | (.0008) | (.0019) | (.0008) | (.0018) | (.0007) |
| Ever Widowed (if Married) | .3245 | .0451 | .3423 | .0456 | .3099 | .0377 |
| | (.0018) | (.0004) | (.0018) | (.0004) | (.0018) | (.0004) |
| Died | .1088 | .1854 | .1251 | .215 | .1784 | .1643 |
| | (.0005) | (.0008) | (.0006) | (.0008) | (.0007) | (.0007) |
| Year of Death (if dead) | 2005 | 2006 | 2005 | 2005 | 2006 | 2005 |
| | (.0397) | (.034) | (.0401) | (.0344) | (.0388) | (.0356) |

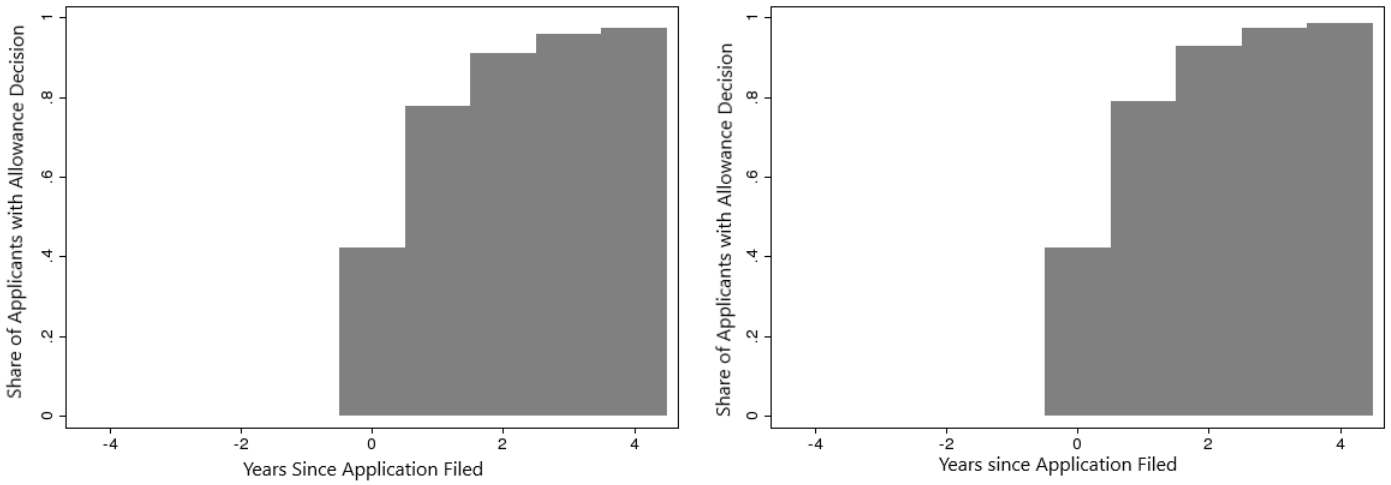
This table restricts to SIPP participants who are linked to SSA administrative data. Means are censored when they do not meet U.S. Census Bureau sample size disclosure standards. For binary outcome variables, means are censored if either of the two cells implied by the binary variable contains fewer than 15 observations. Other outcome variables are censored if the overall sample size for the average contains fewer than 15 observations. Any mean which would indirectly disclose the mean for a censored statistic is also censored.

Table A2: Observable Characteristics of Married and Non-Married DI Applicant Sample

| | Unweighted | | Survey Weighted | | Survey Weighted, Cohort-Adjusted | |
|---|---|---|---|---|---|---|
| $N \approx$ | 12500 | 14000 | 12500 | 14000 | 12500 | 14000 |
| Variable | Unmarried | Married | Unmarried | Married | Unmarried | Married |
| Applicant Allowed DI | .9265 | .9486 | .9303 | .9523 | .9399 | .9477 |
| | (.0023) | (.0019) | (.0023) | (.0018) | (.0021) | (.0019) |
| Age at Filing | 43.27 | 48.66 | 43.27 | 49.02 | 45.75 | 47.69 |
| | (.1006) | (.0726) | (.1008) | (.071) | (.0911) | (.0807) |
| Age at Adjudication | 44.48 | 49.62 | 44.47 | 49.95 | 46.89 | 48.63 |
| | (.0984) | (.0717) | (.0986) | (.07) | (.0891) | (.0798) |
| Years Applicant Working | 3.335 | .9769 | 3.339 | .9843 | 3.361 | .9562 |
| | (.0097) | (.0128) | (.0097) | (.0128) | (.0097) | (.0127) |
| Years Spouse Working | 0 | .3723 | 0 | .3686 | 0 | .3777 |
| | (0) | (.0075) | (0) | (.0074) | (0) | (.0074) |
| Years Both Working | 0 | 2.504 | 0 | 2.5 | 0 | 2.519 |
| | (0) | (.014) | (0) | (.014) | (0) | (.0139) |
| High Labor Force Attachment | .6516 | .7283 | .6532 | .7299 | .6681 | .7248 |
| | (.0042) | (.0038) | (.0042) | (.0038) | (.0042) | (.0038) |
| High Spousal Labor Force Attachment | . | .558 | . | .5603 | . | .5592 |
| | (.) | (.0042) | (.) | (.0042) | (.) | (.0042) |
| Birth Cohort | 1955 | 1949 | 1954 | 1948 | 1951 | 1950 |
| | (.1214) | (.0952) | (.1228) | (.0954) | (.1068) | (.1095) |
| Male | .4631 | .5791 | .4682 | .5797 | .4558 | .5697 |
| | (.0044) | (.0042) | (.0044) | (.0042) | (.0044) | (.0042) |
| Educ. - No HS Degree | .2751 | .2636 | .2829 | .2702 | .276 | .2646 |
| | (.0045) | (.004) | (.0046) | (.004) | (.0045) | (.004) |
| Educ. - HS Degree | .3608 | .3862 | .3654 | .3956 | .365 | .3941 |
| | (.0049) | (.0044) | (.0049) | (.0045) | (.0049) | (.0045) |
| Educ. - Some College | .285 | .2679 | .2706 | .2513 | .2726 | .26 |
| | (.0046) | (.004) | (.0045) | (.004) | (.0045) | (.004) |
| Educ. - College Degree | .0582 | .0601 | .0596 | .0606 | .0625 | .0598 |
| | (.0024) | (.0022) | (.0024) | (.0022) | (.0025) | (.0022) |
| Educ. - Grad Degree | .0209 | .0222 | .0215 | .0223 | .0238 | .0215 |
| | (.0015) | (.0013) | (.0015) | (.0013) | (.0015) | (.0013) |
| Race - White | .7175 | .8378 | .7369 | .8519 | .7334 | .8535 |
| | (.004) | (.0031) | (.0039) | (.003) | (.0039) | (.003) |
| Race - Black | .2435 | .1281 | .2292 | .1181 | .2331 | .1164 |
| | (.0038) | (.0028) | (.0037) | (.0028) | (.0038) | (.0027) |
| Race - Other | .039 | .0342 | .0339 | .03 | .0334 | .0301 |
| | (.0017) | (.0015) | (.0016) | (.0015) | (.0016) | (.0015) |
| Hispanic | .0725 | .0775 | .071 | .0728 | .0696 | .0739 |
| | (.0023) | (.0023) | (.0023) | (.0022) | (.0023) | (.0022) |
| Ever Married | .442 | . | .4166 | . | .4763 | . |
| | (.0044) | (.) | (.0044) | (.) | (.0044) | (.) |
| Ever Divorced (if Married) | .7841 | .2942 | .8014 | .286 | .8044 | .2783 |
| | (.0055) | (.0039) | (.0053) | (.0039) | (.0053) | (.0038) |
| Ever Widowed (if Married) | .1525 | .0499 | .1461 | .0455 | .1476 | .0444 |
| | (.0048) | (.0019) | (.0047) | (.0018) | (.0047) | (.0018) |
| Died | .3111 | .3637 | .3357 | .3975 | .3701 | .3769 |
| | (.0041) | (.0041) | (.0042) | (.0042) | (.0043) | (.0041) |
| Year of Death (if dead) | 2007 | 2007 | 2007 | 2006 | 2006 | 2006 |
| | (.106) | (.1) | (.1085) | (.1022) | (.1089) | (.1024) |

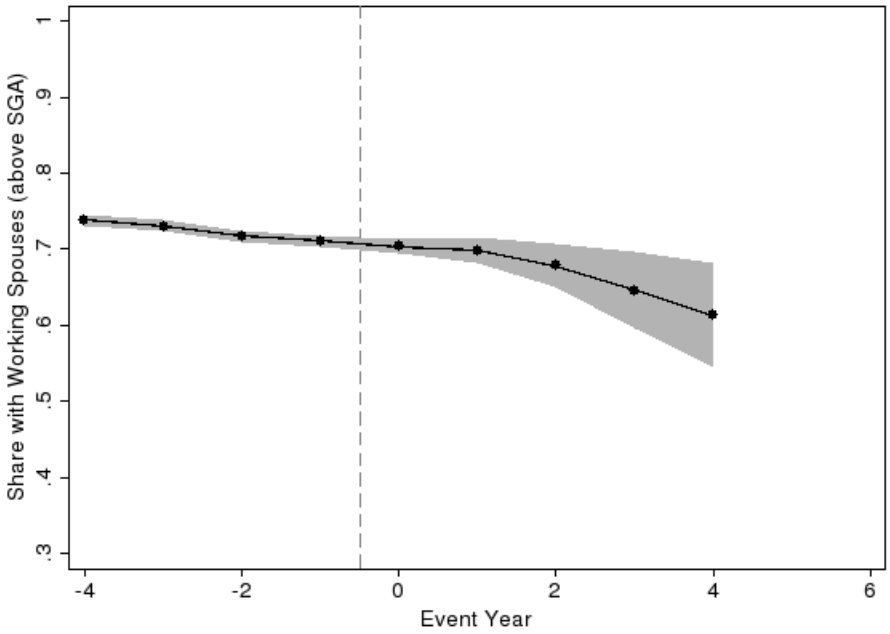
Means are censored when they do not meet U.S. Census Bureau sample size disclosure standards. For binary outcome variables, means are censored if either of the two cells implied by the binary variable contains fewer than 15 observations. Other outcome variables are censored if the overall sample size for the average contains fewer than 15 observations. Any mean which would indirectly disclose the mean for a censored statistic is also censored.

Figure A1: Distribution of Application Wait Times, Married and Non-Married Applicants



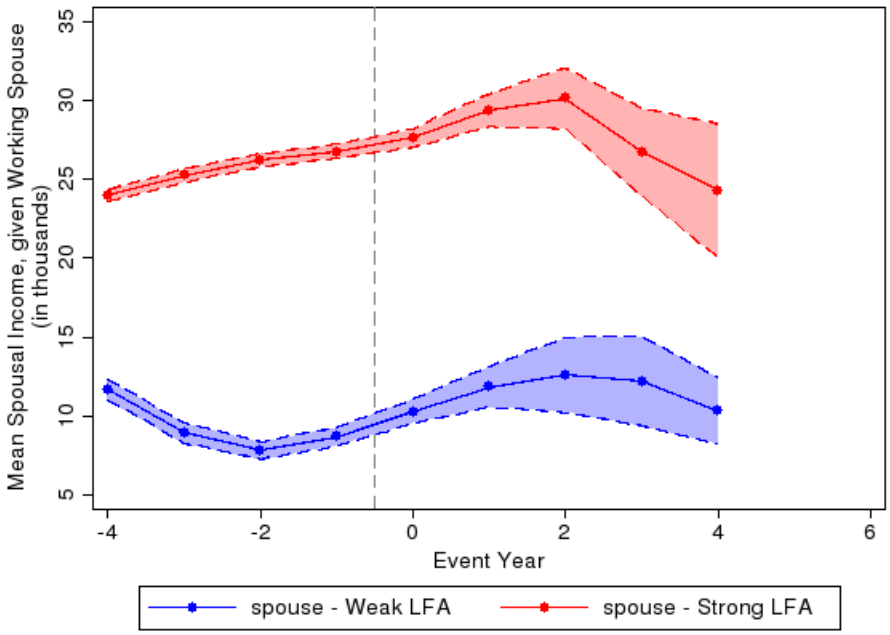

Means are censored when they do not meet U.S. Census Bureau sample size disclosure standards. For binary outcome variables, means are censored if either of the two cells implied by the binary variable contains fewer than 15 observations. Other outcome variables are censored if the overall sample size for the average contains fewer than 15 observations. Any mean which would indirectly disclose the mean for a censored statistic is also censored.

Figure A2: Share of Married Applicants with Working Spouse (over SGA) in Years Surrounding DI Application and Before Receiving an Allowance Decision




Means are censored when they do not meet U.S. Census Bureau sample size disclosure standards. For binary outcome variables, means are censored if either of the two cells implied by the binary variable contains fewer than 15 observations. Other outcome variables are censored if the overall sample size for the average contains fewer than 15 observations. Any mean which would indirectly disclose the mean for a censored statistic is also censored.

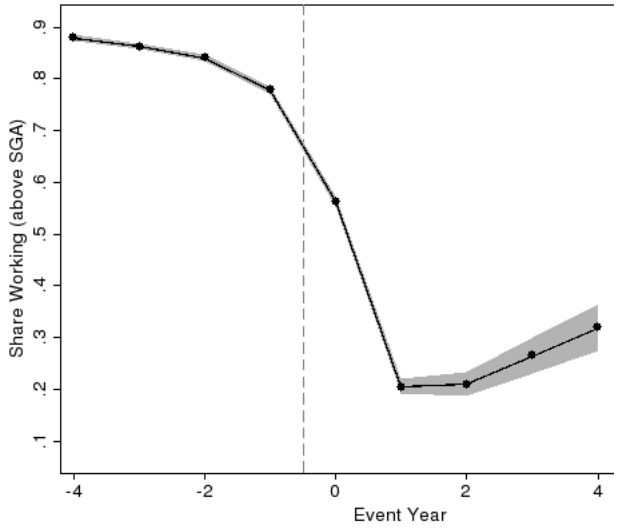
Figure A3: Employment Income of Working Spouses (over SGA) in Years Surrounding DI Application



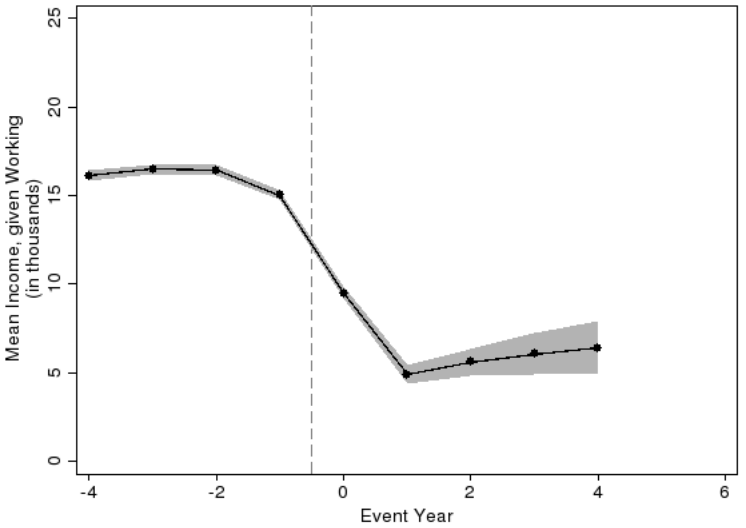

Means are censored when they do not meet U.S. Census Bureau sample size disclosure standards. For binary outcome variables, means are censored if either of the two cells implied by the binary variable contains fewer than 15 observations. Other outcome variables are censored if the overall sample size for the average contains fewer than 15 observations. Any mean which would indirectly disclose the mean for a censored statistic is also censored.

Figure A4: Own Employment Outcomes Over the While Filing DI Application, by Marital Status

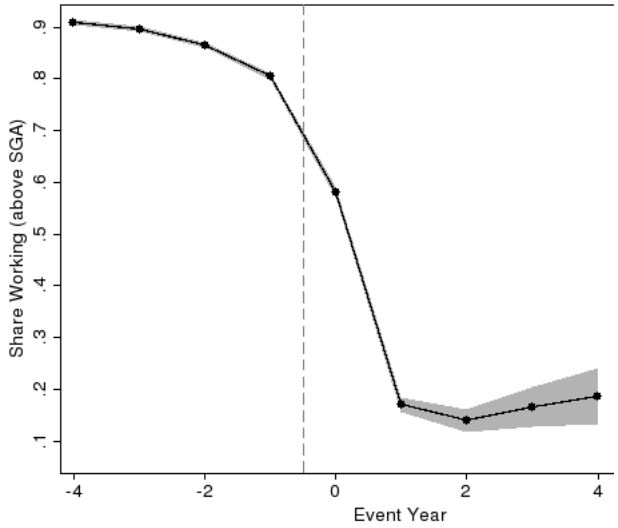
(a) Share Working, Non-Married Applicants



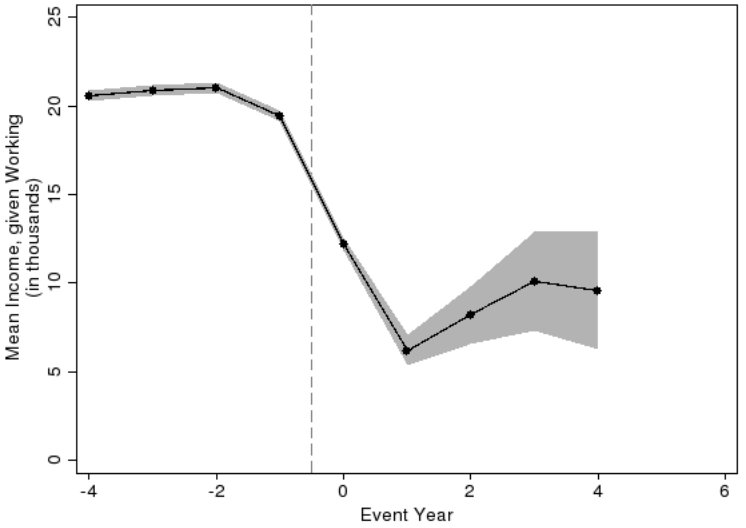

(b) Average Earnings Among Non-Married Applicants




(c) Share Working, Married Applicants



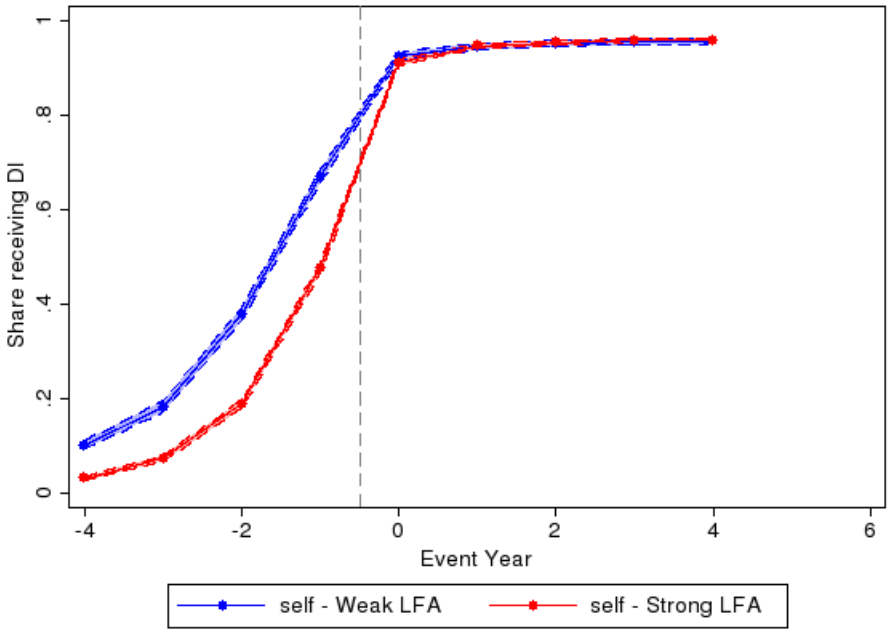

(d) Average Earnings Among Married Applicants



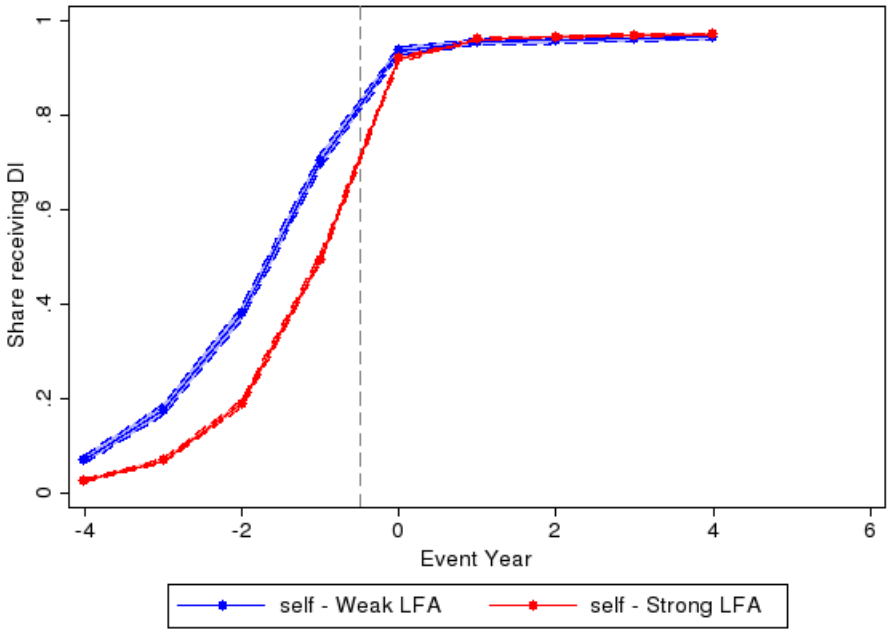

Means are censored when they do not meet U.S. Census Bureau sample size disclosure standards. For binary outcome variables, means are censored if either of the two cells implied by the binary variable contains fewer than 15 observations. Other outcome variables are censored if the overall sample size for the average contains fewer than 15 observations. Any mean which would indirectly disclose the mean for a censored statistic is also censored.

Figure A5

(a) Share Working, Non-Married Applicants



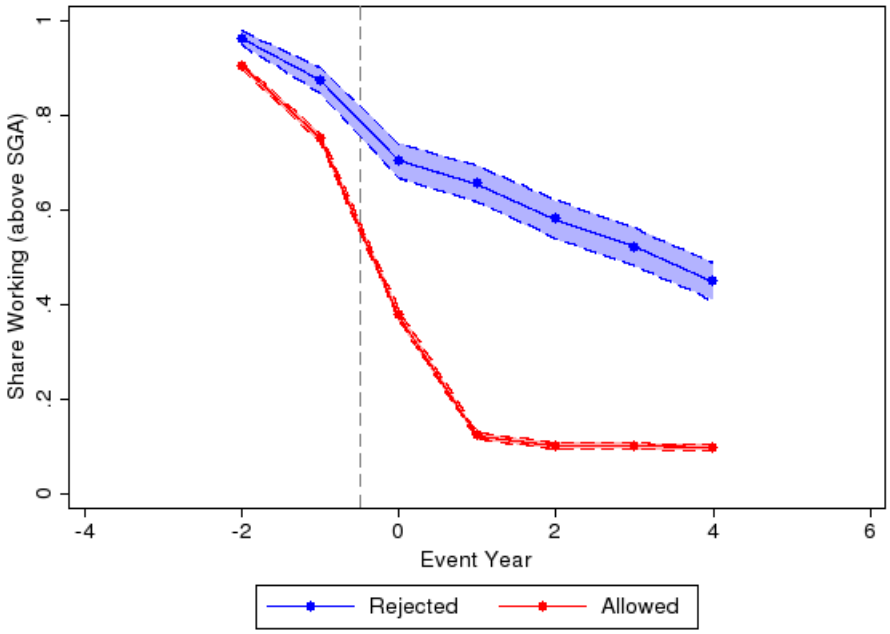

(b) Share Working, Non-Married Applicants



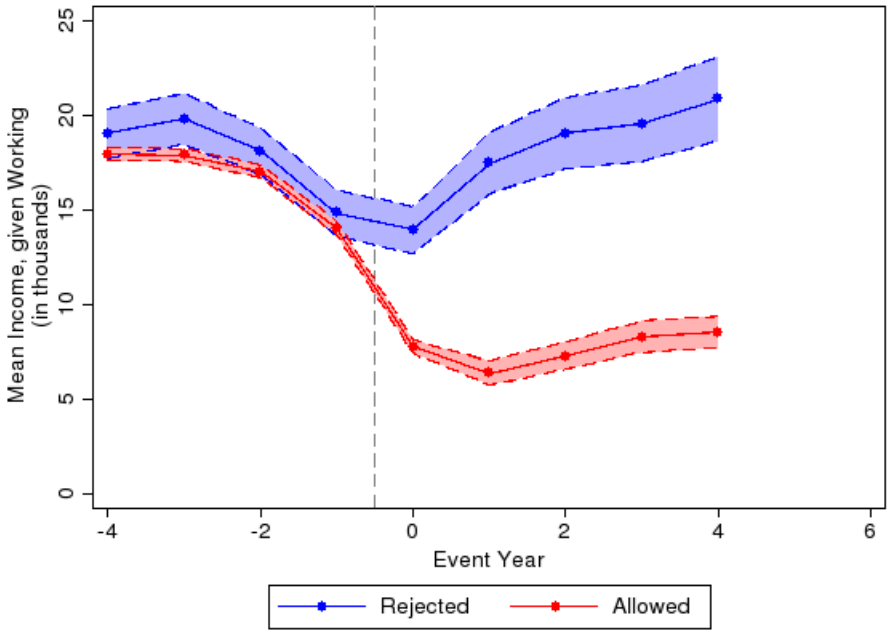

Means are censored when they do not meet U.S. Census Bureau sample size disclosure standards. For binary outcome variables, means are censored if either of the two cells implied by the binary variable contains fewer than 15 observations. Other outcome variables are censored if the overall sample size for the average contains fewer than 15 observations. Any mean which would indirectly disclose the mean for a censored statistic is also censored.

Figure A6: Applicant Employment Outcomes Before and After DI Allowance Decision: Non-Married Applicants with High Labor Force Attachment

(a) Share Working, Non-Married Applicants



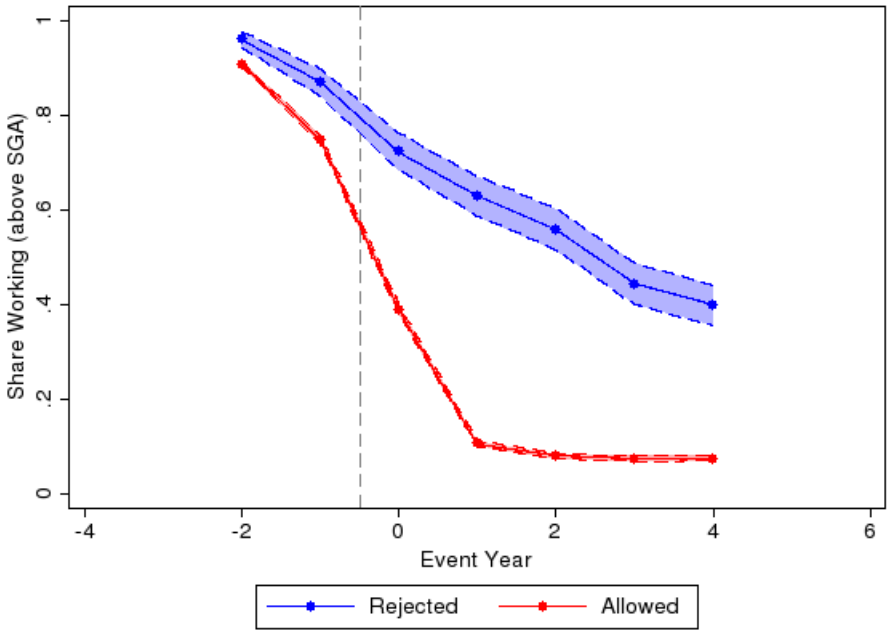

(b) Average Earnings, Non-Married Applicants



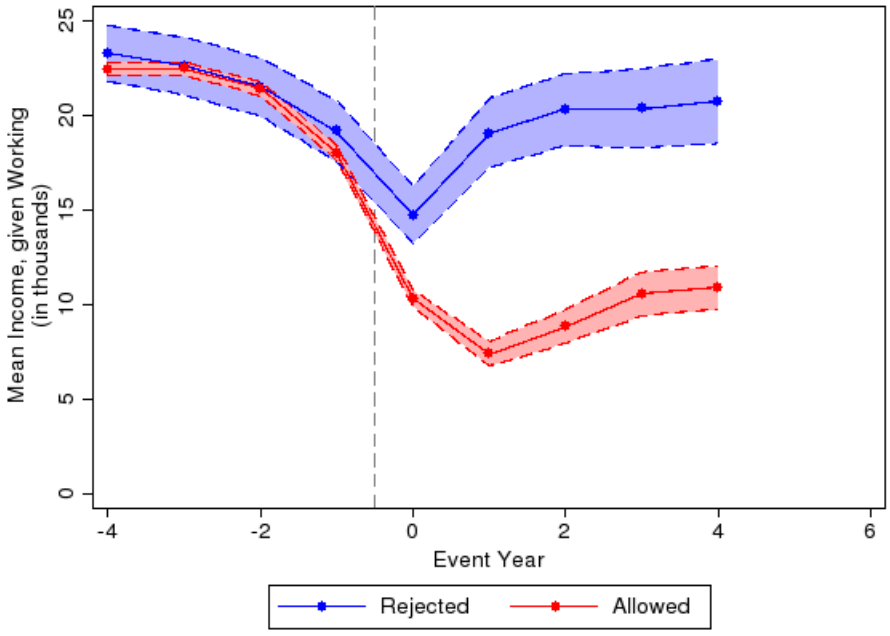

Means are censored when they do not meet U.S. Census Bureau sample size disclosure standards. For binary outcome variables, means are censored if either of the two cells implied by the binary variable contains fewer than 15 observations. Other outcome variables are censored if the overall sample size for the average contains fewer than 15 observations. Any mean which would indirectly disclose the mean for a censored statistic is also censored.

Figure A7: Applicant Employment Outcomes Before and After DI Allowance Decision: Married Applicants with High Labor Force Attachment

(a) Share Working, Married Applicants



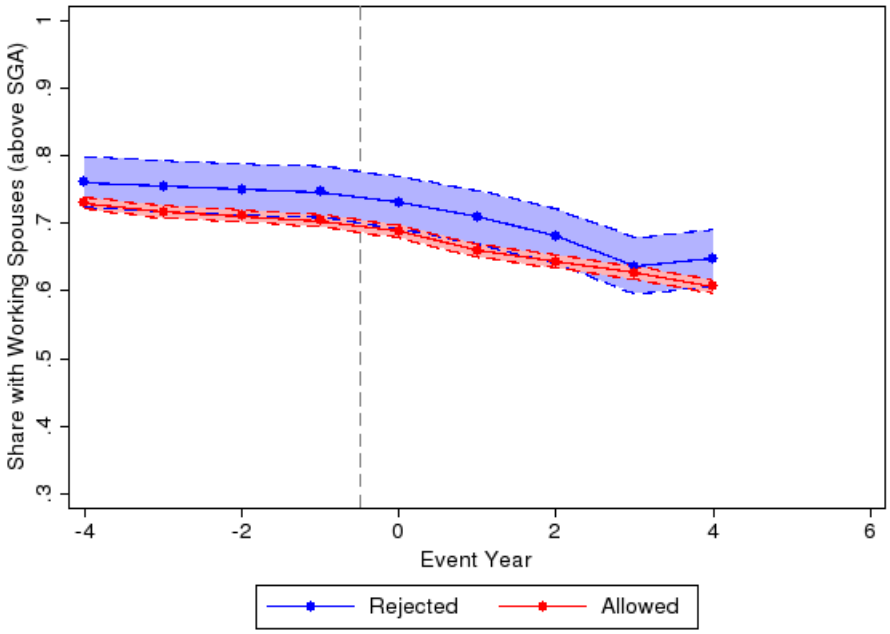

(b) Average Earnings, Married Applicants



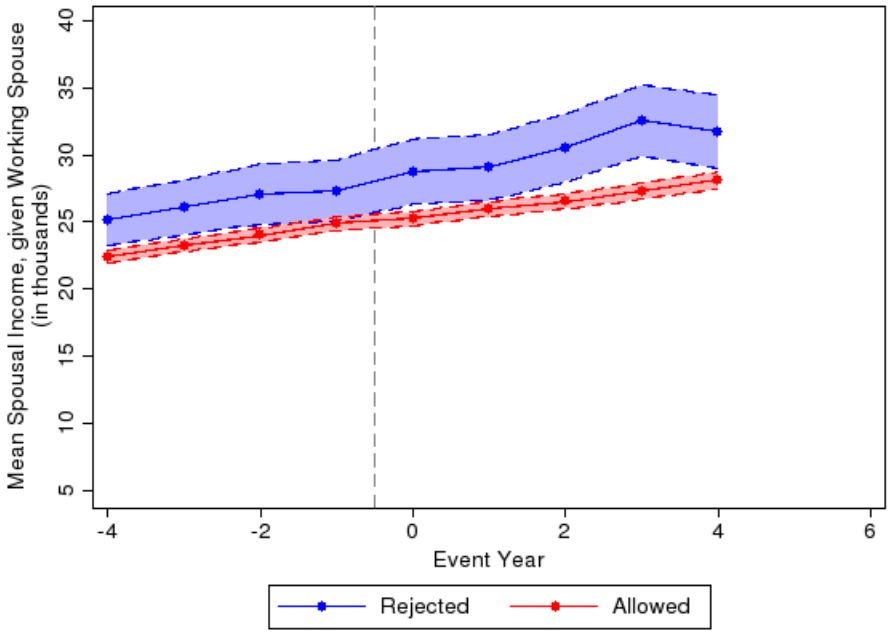

Means are censored when they do not meet U.S. Census Bureau sample size disclosure standards. For binary outcome variables, means are censored if either of the two cells implied by the binary variable contains fewer than 15 observations. Other outcome variables are censored if the overall sample size for the average contains fewer than 15 observations. Any mean which would indirectly disclose the mean for a censored statistic is also censored.

Figure A8: Spousal Employment Outcomes Before and After DI Allowance Decision: Married Applicants with High Labor Force Attachment

(a) Share Working, Spouses of Married Applicants



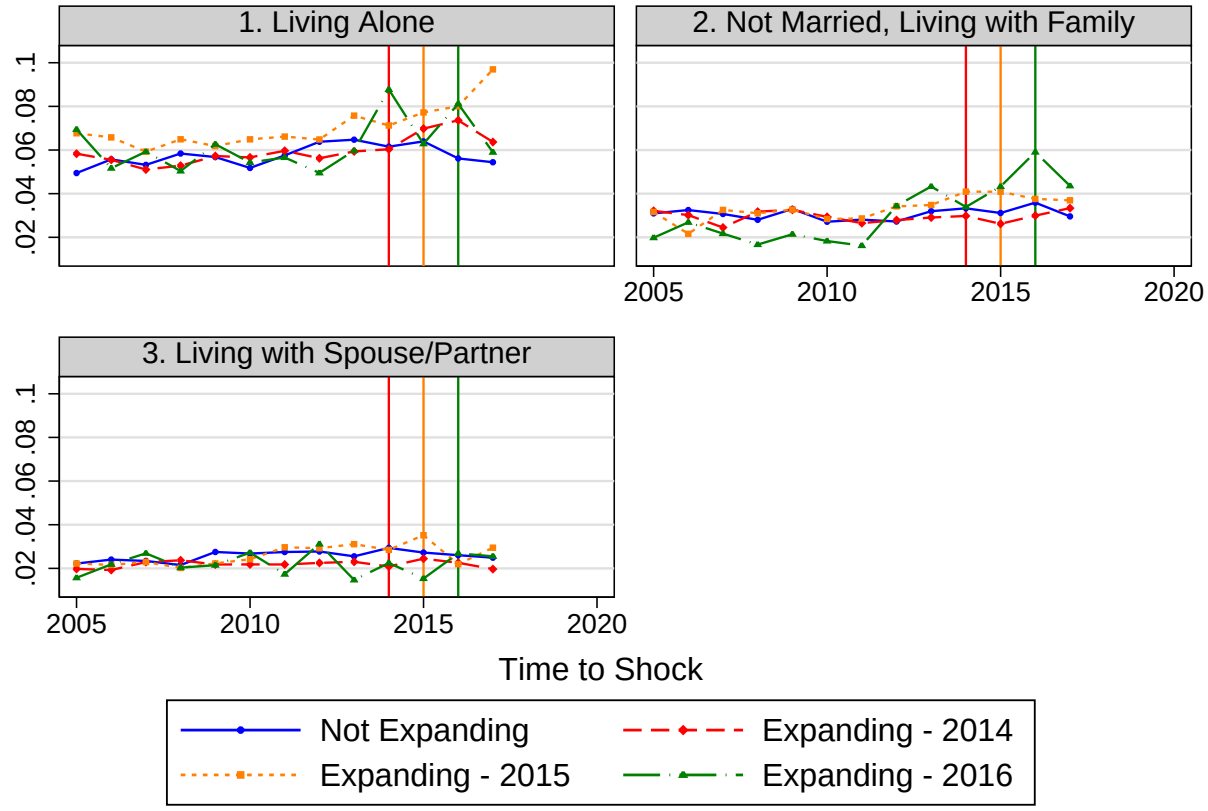

(b) Average Earnings, Spouses of Married Applicants




Means are censored when they do not meet U.S. Census Bureau sample size disclosure standards. For binary outcome variables, means are censored if either of the two cells implied by the binary variable contains fewer than 15 observations. Other outcome variables are censored if the overall sample size for the average contains fewer than 15 observations. Any mean which would indirectly disclose the mean for a censored statistic is also censored.

Figure A9: Share Receiving SSDI in Expanding versus Non-Expanding States

(a) Event Studies




(b) Difference-in-Differences, Living Alone

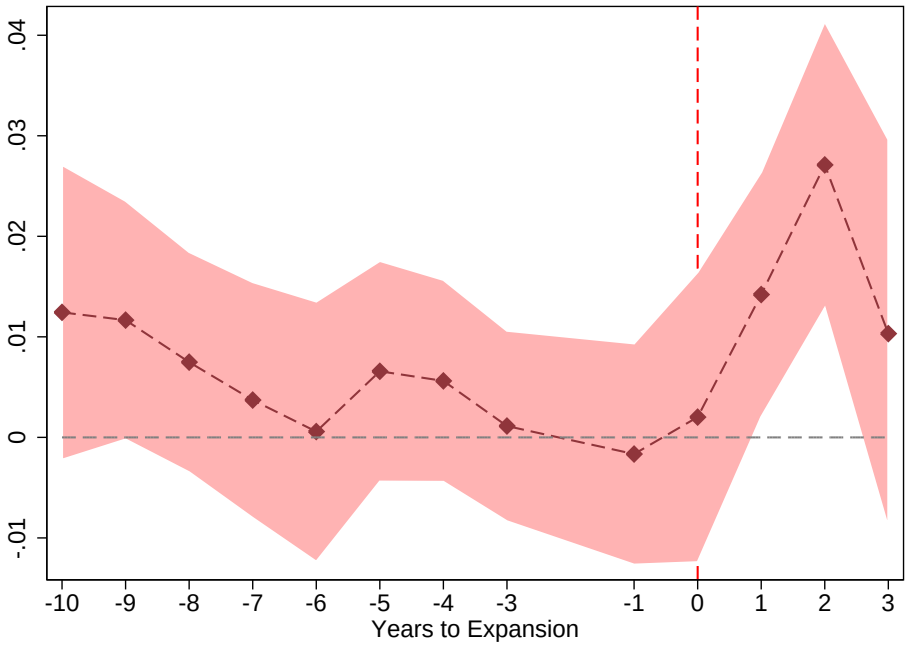
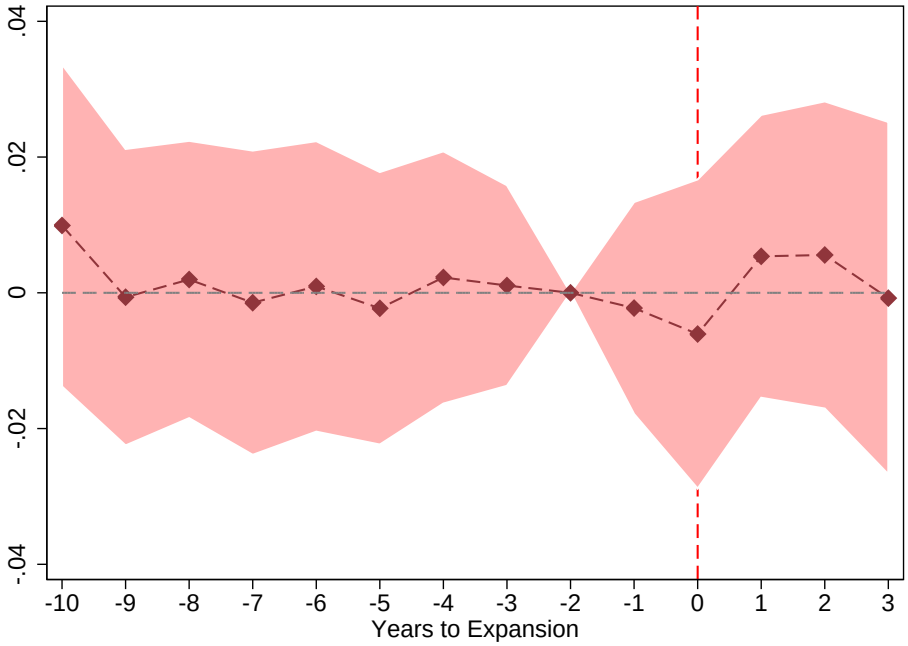


Figure A10: Share Working in Expanding versus Non-Expanding States

(a) Difference-in-Differences, Living with Partner




(b) Difference-in-Differences, Living Alone

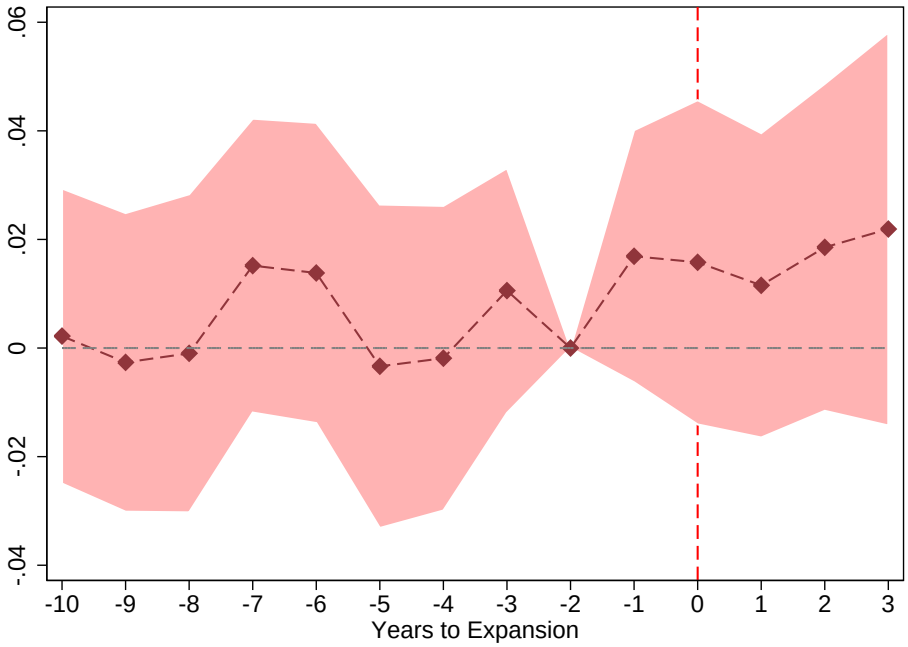
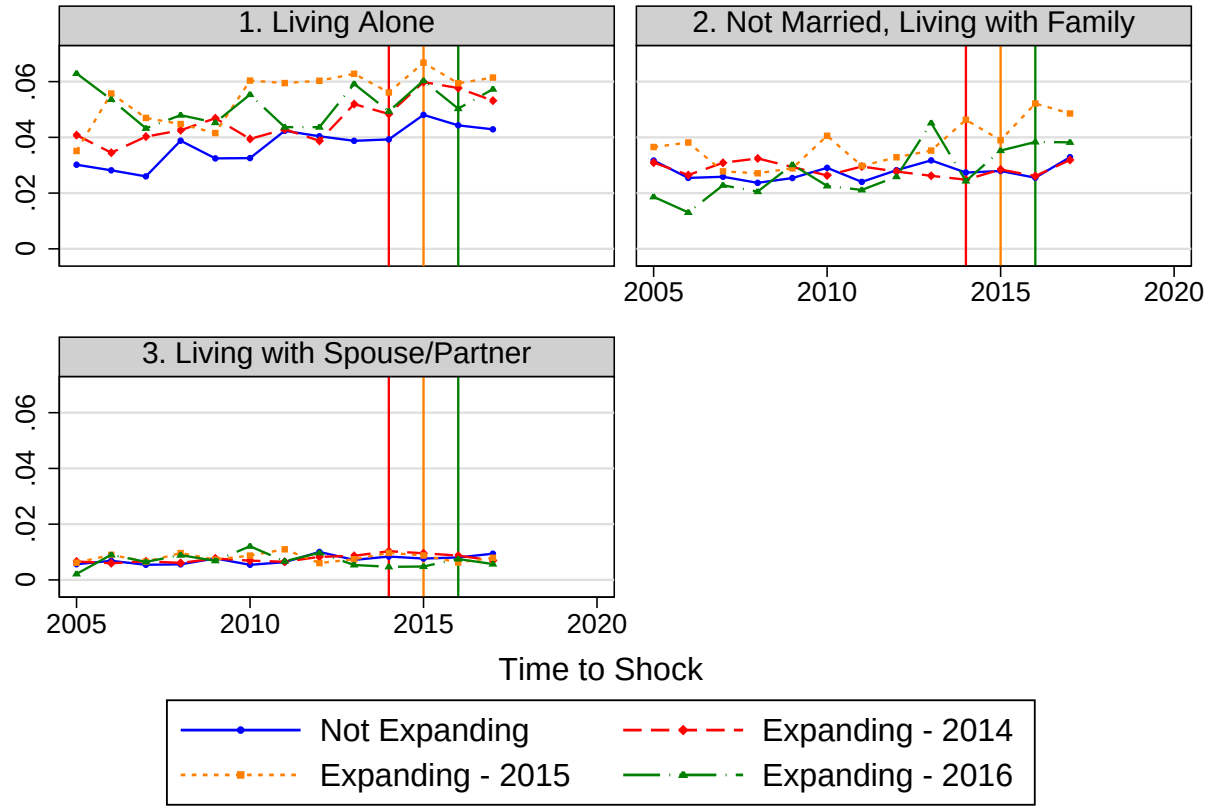


Figure A11: Share Receiving SSI in Expanding versus Non-Expanding States

(a) Event Studies




(b) Difference-in-Differences, Living Alone

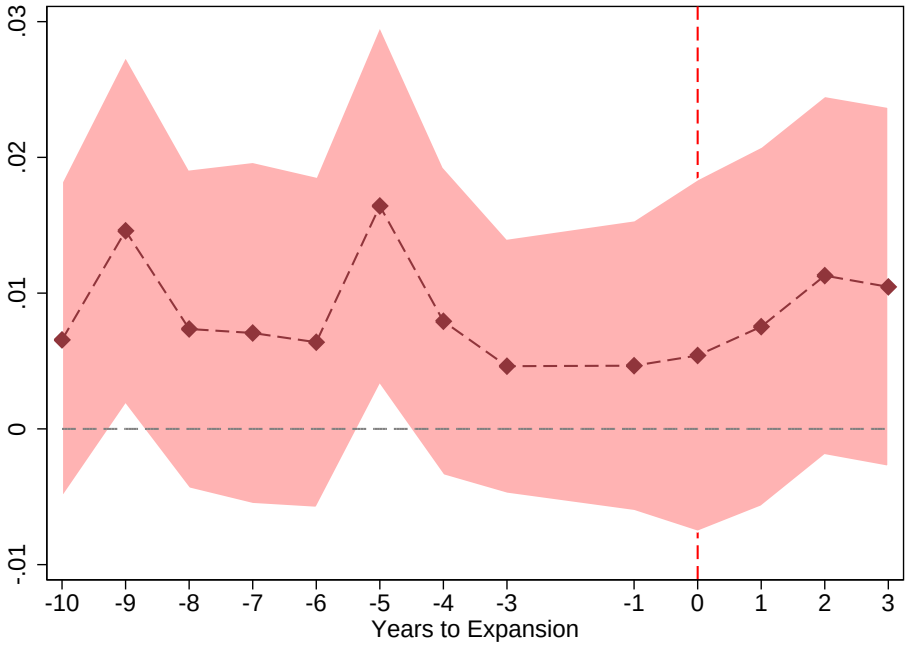
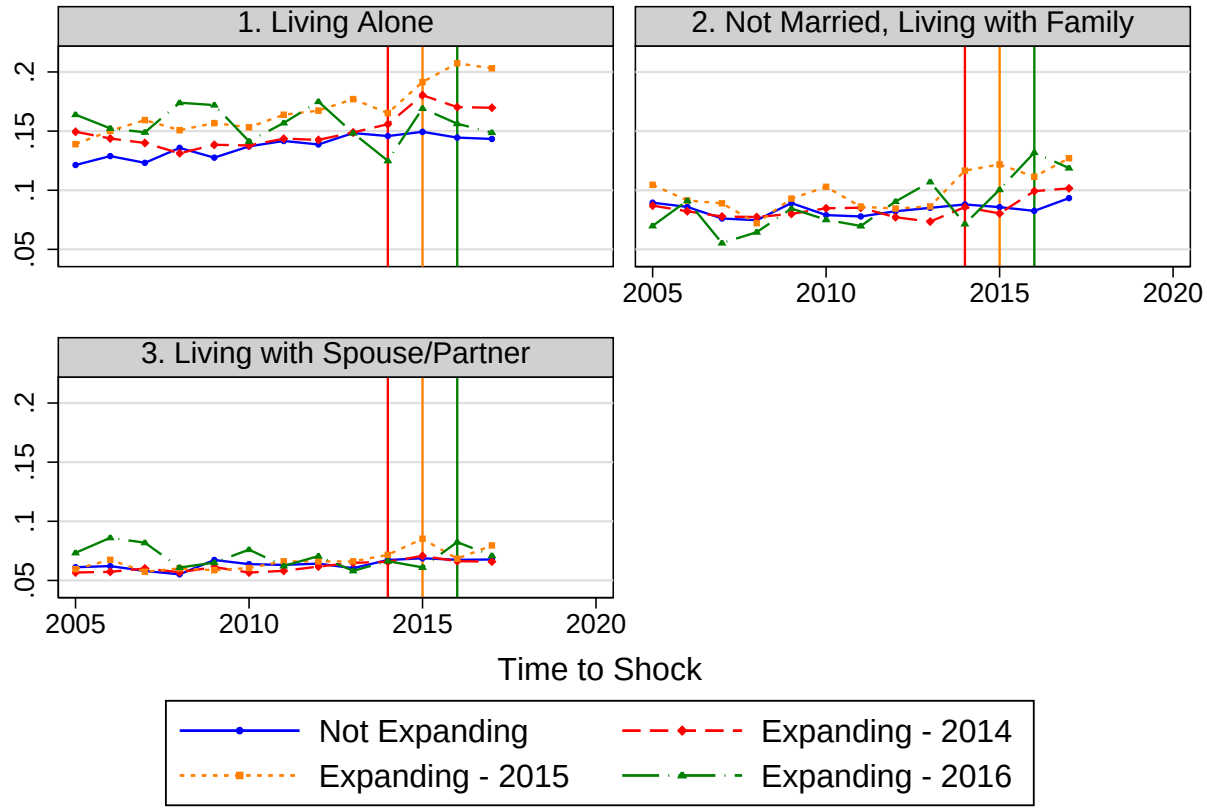


Figure A12: Share Self-Reporting Work-limiting Disabilities in Expanding versus Non-Expanding States

(a) Event Studies




(b) Difference-in-Differences, Living Alone

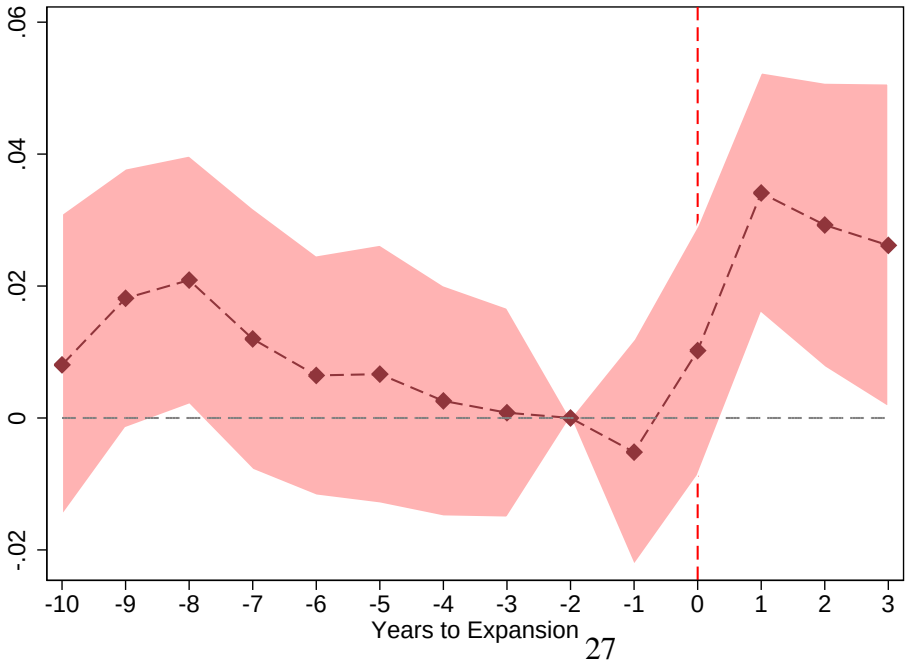
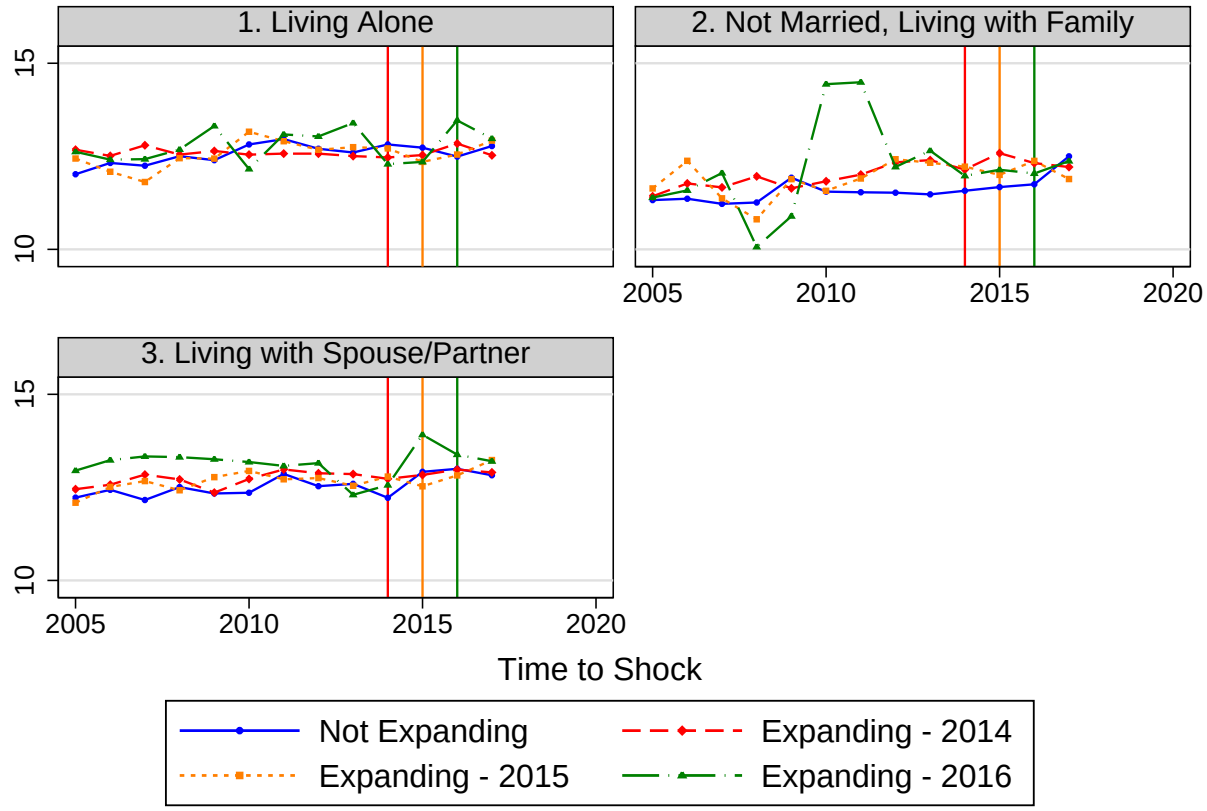


Figure A13: Characteristics of SSDI Beneficiaries in Expanding and Non-Expanding States - Years of Education

(a) Event Studies




(b) Difference-in-Differences, Living Alone

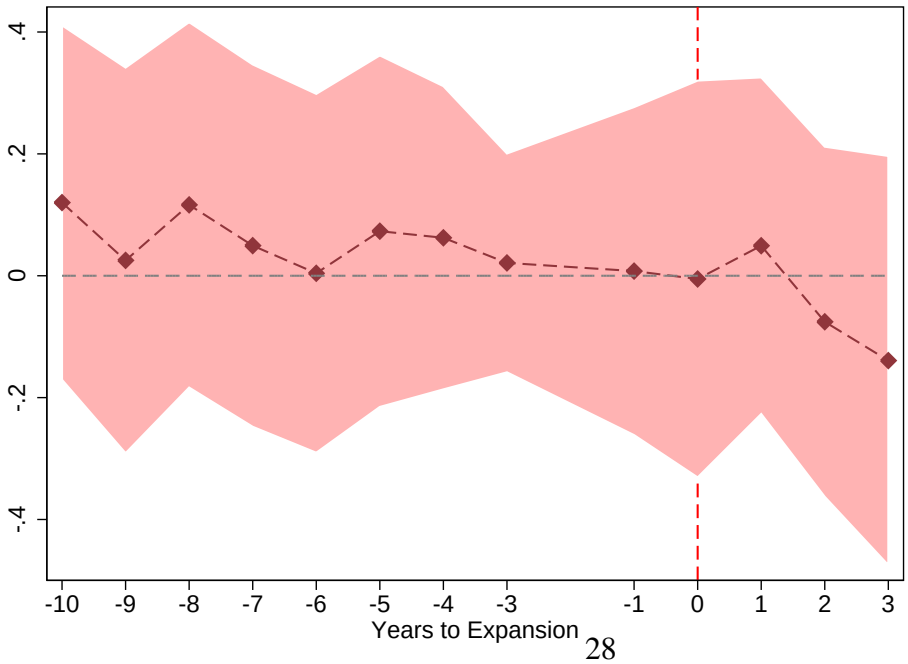
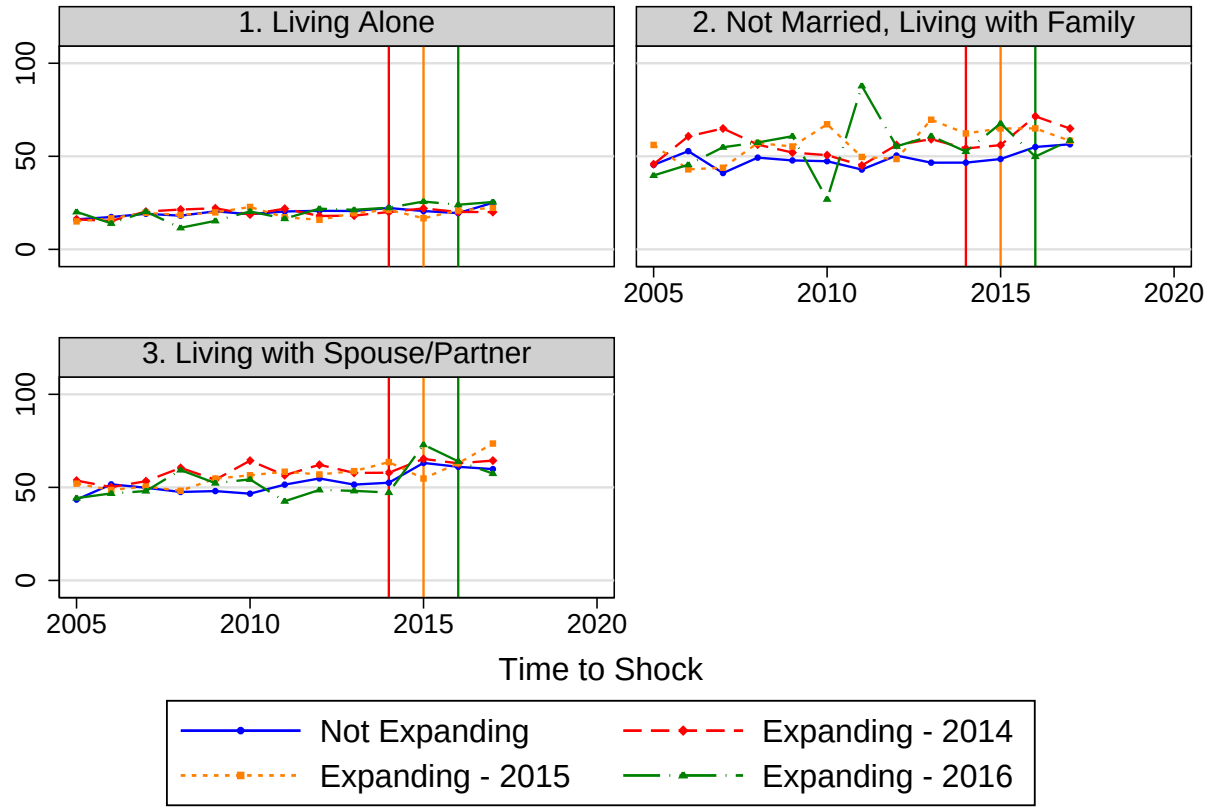


Figure A14: Characteristics of SSDI Beneficiaries in Expanding and Non-Expanding States - Household Income (in Thousands of Dollars)

(a) Event Studies




(b) Difference-in-Differences, Living Alone

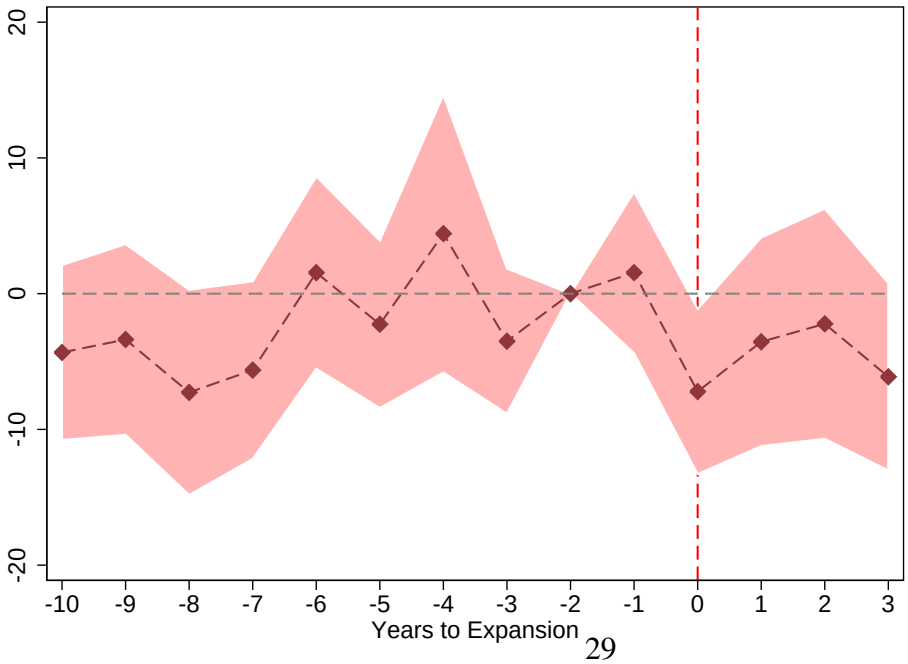


Table A3: Covariate Balance across Household Structures, Expanding and Non-Expanding States

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | Non-Married Households | | Married Households | | Change in Partner | |
| | Unweighted | Weighted | Unweighted | Weighted | Unweighted | Weighted |
| Ages 18-34 | | | | | | |
| Non-Expanding | 0.337*** | 0.330*** | 0.705*** | 0.732*** | 0.237*** | 0.205*** |
| | (0.00307) | (0.00368) | (0.00260) | (0.00273) | (0.00266) | (0.00274) |
| Expanding | 0.323*** | 0.330*** | 0.727*** | 0.732*** | 0.211*** | 0.205*** |
| | (0.00230) | (0.00247) | (0.00214) | (0.00230) | (0.00184) | (0.00185) |
| Ages 35-44 | | | | | | |
| Non-Expanding | 0.227*** | 0.205*** | 0.107*** | 0.0975*** | 0.295*** | 0.262*** |
| | (0.00198) | (0.00254) | (0.00125) | (0.00143) | (0.00162) | (0.00217) |
| Expanding | 0.231*** | 0.205*** | 0.0978*** | 0.0975*** | 0.303*** | 0.262*** |
| | (0.00202) | (0.00232) | (0.00111) | (0.00126) | (0.00181) | (0.00187) |
| Ages 45-49 | | | | | | |
| Non-Expanding | 0.111*** | 0.108*** | 0.0532*** | 0.0476*** | 0.143*** | 0.140*** |
| | (0.00129) | (0.00169) | (0.000839) | (0.00106) | (0.000959) | (0.000945) |
| Expanding | 0.113*** | 0.108*** | 0.0503*** | 0.0476*** | 0.150*** | 0.140*** |
| | (0.00115) | (0.00136) | (0.000769) | (0.000906) | (0.000873) | (0.000939) |
| Ages 50-54 | | | | | | |
| Non-Expanding | 0.107*** | 0.113*** | 0.0506*** | 0.0462*** | 0.123*** | 0.140*** |
| | (0.00118) | (0.00152) | (0.000782) | (0.000891) | (0.000929) | (0.00106) |
| Expanding | 0.110*** | 0.113*** | 0.0467*** | 0.0462*** | 0.131*** | 0.140*** |
| | (0.00102) | (0.00122) | (0.000694) | (0.000841) | (0.000844) | (0.000983) |
| Ages 55-65 | | | | | | |
| Non-Expanding | 0.217*** | 0.245*** | 0.0836*** | 0.0769*** | 0.201*** | 0.254*** |
| | (0.00309) | (0.00427) | (0.00139) | (0.00157) | (0.00234) | (0.00349) |
| Expanding | 0.223*** | 0.245*** | 0.0780*** | 0.0769*** | 0.204*** | 0.254*** |
| | (0.00285) | (0.00332) | (0.00116) | (0.00128) | (0.00218) | (0.00263) |
| Male | | | | | | |
| Non-Expanding | 0.421*** | 0.451*** | 0.469*** | 0.513*** | 0.490*** | 0.488*** |
| | (0.00246) | (0.00275) | (0.00177) | (0.00175) | (0.000246) | (0.000368) |
| Expanding | 0.426*** | 0.451*** | 0.479*** | 0.513*** | 0.490*** | 0.488*** |
| | (0.00226) | (0.00229) | (0.00159) | (0.00157) | (0.000176) | (0.000224) |
| Race - White | | | | | | |
| Non-Expanding | 0.709*** | 0.804*** | 0.710*** | 0.773*** | 0.858*** | 0.887*** |
| | (0.00855) | (0.00554) | (0.00917) | (0.00587) | (0.00412) | (0.00278) |
| Expanding | 0.787*** | 0.804*** | 0.780*** | 0.773*** | 0.886*** | 0.887*** |
| | (0.00515) | (0.00413) | (0.00519) | (0.00524) | (0.00280) | (0.00297) |
| Race - Black | | | | | | |
| Non-Expanding | 0.246*** | 0.159*** | 0.227*** | 0.165*** | 0.0919*** | 0.0555*** |

| | | | | | | |
|---|---|---|---|---|---|---|
| | (0.00900) | (0.00581) | (0.00955) | (0.00648) | (0.00388) | (0.00207) |
| Expanding | 0.161*** | 0.159*** | 0.143*** | 0.165*** | 0.0519*** | 0.0555*** |
| | (0.00582) | (0.00477) | (0.00562) | (0.00571) | (0.00194) | (0.00185) |
| Race - Asian | | | | | | |
| Non-Expanding | 0.0128*** | 0.0131*** | 0.0220*** | 0.0254*** | 0.0258*** | 0.0362*** |
| | (0.000674) | (0.000798) | (0.00127) | (0.00140) | (0.00145) | (0.00185) |
| Expanding | 0.0143*** | 0.0131*** | 0.0275*** | 0.0254*** | 0.0345*** | 0.0362*** |
| | (0.000747) | (0.000781) | (0.00141) | (0.00148) | (0.00169) | (0.00210) |
| Race - Other | | | | | | |
| Non-Expanding | 0.0325*** | 0.0239*** | 0.0417*** | 0.0369*** | 0.0245*** | 0.0210*** |
| | (0.00135) | (0.00140) | (0.00259) | (0.00233) | (0.000950) | (0.00129) |
| Expanding | 0.0371*** | 0.0239*** | 0.0494*** | 0.0369*** | 0.0281*** | 0.0210*** |
| | (0.00204) | (0.00117) | (0.00289) | (0.00217) | (0.00150) | (0.00112) |
| Education - Less than HS | | | | | | |
| Non-Expanding | 0.120*** | 0.0924*** | 0.244*** | 0.188*** | 0.101*** | 0.0759*** |
| | (0.00250) | (0.00191) | (0.00315) | (0.00299) | (0.00406) | (0.00170) |
| Expanding | 0.103*** | 0.0924*** | 0.217*** | 0.188*** | 0.0817*** | 0.0759*** |
| | (0.00184) | (0.00177) | (0.00241) | (0.00269) | (0.00164) | (0.00141) |
| Education - HS Grad | | | | | | |
| Non-Expanding | 0.300*** | 0.306*** | 0.324*** | 0.334*** | 0.289*** | 0.306*** |
| | (0.00242) | (0.00412) | (0.00206) | (0.00291) | (0.00231) | (0.00369) |
| Expanding | 0.307*** | 0.306*** | 0.329*** | 0.334*** | 0.297*** | 0.306*** |
| | (0.00304) | (0.00389) | (0.00208) | (0.00283) | (0.00297) | (0.00362) |
| Education - Some College | | | | | | |
| Non-Expanding | 0.331*** | 0.320*** | 0.343*** | 0.363*** | 0.289*** | 0.270*** |
| | (0.00221) | (0.00232) | (0.00240) | (0.00304) | (0.00228) | (0.00174) |
| Expanding | 0.326*** | 0.320*** | 0.353*** | 0.363*** | 0.275*** | 0.270*** |
| | (0.00259) | (0.00292) | (0.00240) | (0.00288) | (0.00228) | (0.00259) |
| Education - College Grad | | | | | | |
| Non-Expanding | 0.175*** | 0.194*** | 0.0736*** | 0.0973*** | 0.216*** | 0.221*** |
| | (0.00192) | (0.00303) | (0.00146) | (0.00218) | (0.00172) | (0.00218) |
| Expanding | 0.182*** | 0.194*** | 0.0846*** | 0.0973*** | 0.222*** | 0.221*** |
| | (0.00225) | (0.00268) | (0.00167) | (0.00250) | (0.00220) | (0.00243) |
| Education - More than Coll. | | | | | | |
| Non-Expanding | 0.0745*** | 0.0875*** | 0.0155*** | 0.0178*** | 0.106*** | 0.127*** |
| | (0.00132) | (0.00162) | (0.000498) | (0.000765) | (0.00172) | (0.00197) |
| Expanding | 0.0820*** | 0.0875*** | 0.0167*** | 0.0178*** | 0.124*** | 0.127*** |
| | (0.00124) | (0.00157) | (0.000491) | (0.000719) | (0.00181) | (0.00202) |
| Year of Survey | | | | | | |
| Non-Expanding | 2009.0*** | 2009.4*** | 2009.3*** | 2009.6*** | 2008.7*** | 2009.2*** |

| | | | | | | |
|---|---|---|---|---|---|---|
| | (0.326) | (0.387) | (0.348) | (0.401) | (0.323) | (0.386) |
| Expanding | 2008.7*** | 2009.4*** | 2008.9*** | 2009.6*** | 2008.4*** | 2009.2*** |
| | (0.265) | (0.312) | (0.275) | (0.323) | (0.262) | (0.313) |
| Medicaid Auto-Enroll | | | | | | |
| Non-Expanding | 0.861*** | 0.688*** | 0.868*** | 0.695*** | 0.857*** | 0.689*** |
| | (0.0193) | (0.0387) | (0.0192) | (0.0397) | (0.0199) | (0.0384) |
| Expanding | 0.705*** | 0.688*** | 0.702*** | 0.695*** | 0.691*** | 0.689*** |
| | (0.0256) | (0.0322) | (0.0267) | (0.0326) | (0.0261) | (0.0314) |
| Medically Needy Program | | | | | | |
| Non-Expanding | 0.485*** | 0.605*** | 0.480*** | 0.659*** | 0.476*** | 0.621*** |
| | (0.0337) | (0.0384) | (0.0360) | (0.0375) | (0.0331) | (0.0373) |
| Expanding | 0.610*** | 0.605*** | 0.645*** | 0.659*** | 0.624*** | 0.621*** |
| | (0.0261) | (0.0312) | (0.0256) | (0.0301) | (0.0258) | (0.0304) |
| $N$ | 222161 | 222161 | 244047 | 244047 | 871372 | 871372 |

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$