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HOW DOES MEDICAID EXPANSION IMPACT INCOME SUPPORT PROGRAM PARTICIPATION AND EMPLOYMENT FOR DIFFERENT TYPES OF PEOPLE WITH DISABILITIES?

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How Does Medicaid Expansion Impact Income Support Program Participation and Employment for Different Types of People with Disabilities?
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ABSTRACT

Social Security Disability Insurance and Supplemental Security Income, the United States' two primary disability income support programs, each offer a pathway to public health insurance in addition to cash benefits. This implies that expansions in public health insurance availability, such as the ACA's Medicaid expansions, may impact disability program participation and employment of people with disabilities. However, prior research has yielded mixed results as to the impact of Medicaid expansion on these outcomes. Using a stacked difference-in-differences design and data from the Current Population Survey, we demonstrate that the ACA's Medicaid expansions increased SSDI receipt among individuals ages 50-64 with physical, self-care and independent living disabilities, consistent with a "job unlock" mechanism. Exploiting the longitudinal nature of the CPS, we show that treatment effects are heterogeneous and concentrated among persons with ongoing disabilities (as opposed to new disabilities) as reported on the CPS's 6-question functional impairment sequence. We also show suggestive evidence of a reduction in SSI, but find that it is sensitive to specification and data preparation choices, which we illustrate through comparison with other recent work. Effects on employment are inconclusive. Our findings provide further evidence of work capacity among SSDI beneficiaries.

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1 Introduction

By design, the two major civilian disability income support programs in the U.S.– Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI)[1] – are tightly linked with public health insurance. SSDI beneficiaries receive Medicare coverage twenty-four months after they become entitled to benefits and SSI recipients are categorically eligible for Medicaid in most of the country.[2,3] For both programs, public health insurance is an important part of their value for people with disabilities. As such, the Affordable Care Act's expansion of Medicaid to low-income individuals—regardless of their eligibility for SSDI or SSI—may have impacted disability program participation.

The structure of each program suggests potential mechanisms by which Medicaid expansion might impact enrollment and subsequent labor supply decisions by people with disabilities. The long waiting period before SSDI applicants receive Medicare could serve as a disincentive for persons with high anticipated healthcare costs to exit the labor force due to loss of employer-sponsored insurance (ESI) and insufficient affordable alternative coverage options. By offering public insurance, Medicaid expansion could increase SSDI enrollment and reduce employment among people with disabilities. Such a mechanism is the reverse of the well known "job lock" phenomenon, whereby persons may remain in a job longer than they desire in order to retain access to ESI or other job amenities (see Ross, 1958; Mitchell, 1982; Madrian, 1994). We refer to persons exiting the labor force due to an expansion in insurance availability as "job unlock."

Alternatively, because both SSDI and SSI offer a pathway into public health insurance, Medicaid expansion could reduce the value of receiving benefits under either program by offering an alternative pathway into public insurance coverage that does not require people with disabilities to limit their income in order to retain access to benefits. Disability advocates have long contended that work disincentives built into the structure of both SSDI and SSI serve as a major barrier to increasing labor force participation for people with disabilities (TenBroek and Matson, 1966; Longmore, 2003; Ne'eman, 2020). This is particularly the case for persons who place high value on access to public health insurance, either because other

[1] In 2021, approximately 8.3 million working-age people with disabilities were enrolled in SSDI while 4.4 million received SSI. 1.1 million persons were concurrently enrolled in both programs.

[2] Even in those states that have stricter financial eligibility standards for Medicaid than SSI, SSI receipt still serves as a pathway to Medicaid eligibility and results in Medicaid enrollment for the majority of working-age SSI recipients (Rupp and Riley, 2016)

[3] SSDI and SSI use the same medical standard to identify qualifying disabilities, but they differ on non-medical criteria. To receive either, applicants must demonstrate that they are unable to engage in substantial gainful activity (defined as $1,470 in monthly income as of 2023) due to a medically determinable impairment lasting more than one year or expected to result in death. SSI recipients are also subject to a $2,000 asset test, which has not been updated since 1989 and is not indexed to inflation. There is no asset test for SSDI.

coverage options are not available to them or because other available options have important coverage limitations compared to public insurance.

For example, people with disabilities who require substantial long-term services and supports generally have no alternative to Medicaid coverage and must retain it even if they have concurrent access to ESI or the individual market options created by the ACA's guaranteed issue and community rating requirements. Under this mechanism, Medicaid expansion would reduce participation in income support programs and potentially increase employment for people with disabilities owing to the availability of an eligibility pathway onto public insurance with less restrictive financial eligibility requirements than SSDI or SSI. Though a potential factor for both programs, this mechanism is particularly likely to be relevant to SSI participation as SSI offers immediate enrollment (in most states) in the same public insurance program offered by Medicaid expansion.[4]

Surprisingly, however, the literature on the effect of Medicaid expansion on disability income support programs and employment is mixed. Burns and Dague (2017) found that pre-ACA Medicaid expansions reduced Supplemental Security Income (SSI) participation by 7%. Maestas et al. (2014)'s work on the effect of the Massachusetts health reform law – which became a model for the ACA – found a 6% decrease in SSI applications but a 5-6% increase in Social Security Disability Insurance (SSDI) applications; both effects were temporary. Soni et al. (2017) found a 3.3% decrease in SSI participation when evaluating the early ACA Medicaid expansions. However, other work has found contrasting or null results. Anand et al. (2018) found suggestive evidence of increased SSI applications in expansion states relative to non-expansion states. Most recently, Schmidt et al. (2020) used a state border design to find no significant impact of expansion on either SSI or SSDI applications, doing so with sufficient precision to rule out economically meaningful effects in either direction. With respect to employment, Hall et al. (2017, 2018) found that the ACA's Medicaid expansion significantly increased employment for people with disabilities in expansion states relative to people in states that did not expand Medicaid. In contrast, Sevak and Schimmel Hyde (2021) found no evidence of a change in employment trends for people with disabilities in expanding states as compared to non-expanding states.

One potential explanation for these different findings may be found in the populations researchers choose to examine. Research on SSDI and SSI program participation has generally estimated treatment effects in the general working-age or prime-age population. This is especially necessary when using SSA administrative data, since these data sets only include information on SSDI/SSI recipients and applicants. Although researchers can use SSA data to

[4] In related settings, loss of health insurance coverage has been shown to increase SSI applications (and to a lesser extent, awards) (e.g., see Levere et al. (2021) who study the SSI applications and awards at age 26, when the ACA's dependent coverage mandate expires).

subset their numerator (those receiving or applying for SSDI and SSI) into relevant subgroups (e.g., disability type), there is no available denominator other than Census estimates for the general population; thus, it is not possible to similarly subset the denominator into relevant "at-risk" subgroups (e.g., those whose disabilities place them on the margin of SSDI/SSI application or receipt). This forces researchers to estimate treatment effects relative to the general population as a whole. However, disability program participation is concentrated among people with the most severe and longstanding functional impairments. Treatment effects for this population, which is much more likely to be at the margin of entering SSDI or SSI or already enrolled in the programs, may be quite different than treatment effects for the general, predominantly non-disabled population.[5]

In this paper, we examine the impact of Medicaid expansion on disability benefits and employment and test the hypothesis that prior literature may be mixed in part because of the presence of heterogeneous treatment effects. Unlike prior work on disability program participation, we test for treatment effects on both the general population and on persons identified by a 6-question sequence on functional impairment commonly used to identify people with disabilities in federal population surveys, including the data we analyze here, the Bureau of Labor Statistics' Current Population Survey (CPS). Although people with disabilities identified by the 6-question sequence (hereafter 6Q) are only 7.5% of CPS respondents, they make up 60.1% of SSDI and SSI beneficiaries. We find strong evidence that Medicaid expansion resulted in an increase in SSDI participation among people with disabilities, an effect that is detectable but attenuated in the general population. We show that the increase in SSDI enrollment was driven by persons with physical, self-care and independent living disabilities. We also find suggestive evidence of a decrease in SSI participation, and inconclusive results with respect to employment outcomes.

We further exploit the longitudinal nature of the CPS to segment the population of people with disabilities by their disability recency (ongoing versus new disabilities). We show

[5]Similarly, prior research on employment outcomes, which does subset data to look at treatment effects specific to people with disabilities, relies on different disability definitions from different data sources. For example, Sevak and Schimmel Hyde (2021) make use of the American Community Survey (ACS). In contrast, Hall et al. (2017, 2018) made use of the Urban Institute's Health Reform Monitoring Survey (HRMS). Different survey tools may capture different populations, particularly because they rely on different questions for identifying people with disabilities. While the ACS identifies people with disabilities using a six-question sequence that asks about functional impairment in hearing, vision, cognition, physical activity, self-care, and independent living (see Figure 1), the HRMS identifies people with disabilities by a single question inquiring if a respondent had "a physical or mental condition, impairment, or disability that affects your daily activities OR that requires you to use special equipment or devices, such as a wheelchair, TDD, or communications device" (Hall et al., 2017). It is likely that these questions capture individuals with very different disability experiences in terms of severity, recency or other dimensions of variation. Disability identification varies significantly based on question wording and order, making it likely that different survey approaches yield different disabled populations (Maestas et al., 2019; Burkhauser et al., 2014b)

that the increase in SSDI participation is strongest among people with ongoing disabilities, particularly persons aged 50-64. This finding would be consistent with a "job unlock" mechanism, whereby Medicaid expansion permits persons who had previously remained in the labor force primarily to retain access to employer-sponsored insurance (ESI) to leave and enter SSDI when Medicaid expansion offers them an alternative source of coverage that can "bridge" the two-year waiting period until SSDI renders them eligible for Medicare.

The finding of a positive increase on SSDI participation is particularly noteworthy given the presence of a nationwide decline in SSDI enrollment over this time period, caused by a 2011 administrative change that made it more difficult for applicants to receive benefits. The ability of Medicaid expansion to tip people with disabilities into SSDI enrollment even during a time of tightening eligibility standards reinforces prior literature showing that persons on SSDI often retain significant work capacity, even if only for part-time work (Maestas et al., 2013).

2 Data and Methods

2.1 Identifying People with Disabilities in Survey Data

On March 13th, 1998, President Clinton issued Executive Order 13078 on Increasing Employment of Adults With Disabilities. Among other components, the order directed the Bureau of Labor Statistics (BLS) and the Census Bureau to work collaboratively to develop a means to measure the employment rate of adults with disabilities (McMenamin and Hipple, 2014). This directive reflected a broader effort, following the passage of the Americans with Disabilities Act (ADA), to define the disabled population in terms other than an inability to work or substantial limitation in work ability. Given the ADA's stated purpose of bringing people with disabilities into the labor force through civil rights protections, it no longer made sense to characterize disability primarily with respect to participation in income support programs. The ADA defined disability in terms of any physical or mental impairment that substantially limits one or more major life activities, an inclusive standard intended to be interpreted broadly and beyond just those with work disabilities who are enrolled in SSDI and/or SSI.

To accomplish this directive, the federal government embarked on a ten-year process culminating in the addition of 6 questions to the BLS's Current Population Survey in June 2008. These six questions have become the most common means of identifying people with disabilities in federal population surveys. A person is classified as having a disability if they answer "yes" to any of the six questions about difficulties with specific functional activities (none of which explicitly reference work). Although they do not identify all SSDI and SSI recipients, prior work finds they capture approximately two-thirds of those enrolled in these disability income support programs (Burkhauser et al., 2014a,b). Thus, the subgroup of people with functional impairments identified by the 6Q sequence may be a particularly relevant population of interest for research studies using disability program participation as an outcome.

As shown in Table 1, the six-question (6Q) sequence asks about functional impairment rather than whether the respondent self-identifies with the word "disability." As "disability" is interpreted in different ways in different respondent populations, survey designers opted to ask instead about functional impairment (McMenamin and Hipple, 2014). Recent work has demonstrated that responses to the 6Q sequence can be used to measure changes in disability status over time within the same individual (Ward et al., 2017) . Ward et al. (2017) found that approximately 63% of respondents who report a disability according to the 6Q sequence did so one year previously (i.e., had an ongoing as opposed to a new disability). Field surveys relying on the 6Q sequence used by the CPS found that transitions into disability status are associated with lower health-related quality of life (while transitions out of it are associated with higher levels of the same), indicating that different response patterns on the repeated disability questions asked by the CPS likely identify people with disabilities in distinct life circumstances and levels of disability severity (Myers et al., 2020).[6] This suggests that a substantial population exists at the margin of self-reported disability status, and therefore using question responses at a single point in time to identify people with disabilities captures a heterogeneous population that may exhibit heterogeneous treatment effects.

To address this, we make use of the longitudinal nature of the CPS. Respondent households are included in the CPS's sample for four consecutive months, out of sample for eight months, and then return to the sample for another four months. Consequently, there is one calendar year between a household's first month in the sample and fifth month in the sample. Disability questions are included in the interview when households first enter the sample and when households reenter the sample (usually in month 5 in sample) after the eight-month hiatus, enabling observation of self-reported changes in disability status one year after a respondents' first survey response. Approximately three-fourths of the CPS sample eligible for resurvey a year later are retained from one year to the next (Rivera Drew et al., 2014). As illustrated in Figure 1, this enables us to subdivide the disabled population identified by the CPS into persons with ongoing and new disabilities. This builds on prior descriptive work

[6]Disability self-report may also be influenced by changes in the availability of services, supports and medical care – for example, Abouk et al. (2023) find that state legalization of recreational marijuana use reduces rates of self-reported work-limiting disabilities along with reductions in workers' compensation benefit receipt (presumably due to the availability of more effective pain management for persons with chronic conditions). This reinforces the need to test and control for compositional changes in the disabled population, something we do in this paper.

from Ward et al. (2017) and Sage et al. (2019), who first proposed the use of the longitudinal aspect of the CPS to distinguish between persons with new and ongoing disabilities, and Ameri et al. (2019) who use changes in disability status to investigate the relationship between unionization and employment outcomes for people with disabilities.

Persons with ongoing and new disabilities likely have different relationships with income support programs. Program rules require participants to have a long-lasting impairment, meaning respondents without an impairment prior to reporting a disability in the 6Q sequence may not yet be eligible for SSDI/SSI. Even among respondents for whom reporting a disability in the 6Q sequence reflects the worsening of a longstanding impairment (consistent with prior work showing that the 6Q sequence captures changes in health-related quality of life), people with ongoing disabilities are likely to have more severe impairments and less connection to the labor force. Given these differences, we theorize they will also have distinct benefit participation and employment profiles and will respond to the expansion of public insurance in different ways.

We use of the CPS's Annual Social and Economic Supplement (ASEC), which is fielded every March and collects a broad array of variables on the social and economic characteristics of households and individual respondents. These include data on income support program participation (including SSI and SSDI), Medicaid enrollment and household income. We subset to respondents ages 18-64 who have two ASEC observations in years 2010-2020. This yields 308,249 respondents. Our analyses focus on either those with disabilities (N=23,322), defined as reporting difficulty with any of the functional activities in the 6Q sequence at their second ASEC observation, or the subset of this population with ongoing disabilities (N=13,899). We find that we are able to link 74% of respondents who report having a disability with an ASEC response in their first four months of CPS participation with a second ASEC response. This is consistent with prior work showing that approximately three-fourths of CPS respondents can be linked across years (Rivera Drew et al., 2014). Data on employment is taken from the core monthly CPS. In our primary specification, we account for intermittent employment by taking the average of a respondent's binary employment status over months 5 through 8 of their CPS participation.

We use data from the Kaiser Family Foundation (Kaiser Family Foundation, 2022) to track state Medicaid expansion decisions. The earliest Medicaid expansions under the ACA guidelines became effective January 1st, 2014. However, several states started expanding Medicaid eligibility as early as 2010, often with income eligibility limits well below 138% of the Federal Poverty Level (FPL), which was only required of expanding states as of 2014. This means we were unable to construct an adequate pre-period to assess parallel trends (owing to the CPS only adding disability questions in late 2008) for early expander states.

Owing to this and their status as partial treatments, we exclude them from our specification.

We supplement the Medicaid expansion dates from KFF with expansion dates obtained from Schmidt et al. (2020), which includes early expansion dates. We classify states as Medicaid expanders starting the first calendar year in which coverage was expanded. We check the dates of early expansions in Schmidt et al. (2020) against other literature and information from state waiver applications available on the CMS website (Medicaid.gov, 2022).[7]

Table 2 presents summary statistics for the general population for both the entire population of people with disabilities ("All Disabled"), the subsets of this population with ongoing and new disabilities, those who reported a disability previously but do not now ("previous disability") and those who never reported a disability ("never disability"). Consistent with prior work, 59.6% of people with disabilities have an ongoing disability while 40.4% have new disabilities. As we hypothesized, these two populations have very different profiles of employment and benefit participation. Persons with ongoing disabilities have roughly half the employment rate and are enrolled in SSDI and SSI at more than double the rate of persons with new disabilities. Persons with new disabilities also have approximately 6 percentage points higher educational attainment than persons with ongoing disabilities. The populations with ongoing, new and previous disabilities all have substantially lower employment and higher SSI and SSDI participation than the never-disabled population.

Appendix Table A-1 shows the portion of all SSDI, SSI and concurrent recipients with ongoing disabilities, new disabilities, previous disabilities and who never had a disability. Consistent with prior work, we show that 60.1% of SSDI and SSI recipients are identified by a point-in-time 6Q sequence.[8] As shown by the descriptive statistics reported in Table 2, the majority of SSDI and SSI respondents are in the ongoing disability category, which includes 46.6% of all SSDI/SSI enrollees.

[7]We also dropped Delaware, Massachusetts, New York and Vermont from our specification, as they had robust early expansions prior to the passage of the ACA (Denham and Veazie, 2019). We also code Arizona as expanding in 2014, rather than 2010 as it is in Schmidt et al. (2020). Arizona had a pre-ACA expansion in 2000 for childless adults earning up to 100% FPL. In 2011, the state froze enrollment due to cost pressures, leading to a sudden drop in enrollment of over 100,000 people from 2011-2013. Arizona lifted this freeze in 2014 and adopted Medicaid expansion, adding approximately 200,000 enrollees from 2014-2016 (Shafer and Kelly, 2017).

[8]Interestingly, incorporating those with previous disabilities increases this percentage to 74.7%. Prior work from Burkhauser et al. (2014a) argues for the addition of a seventh question on work-limitations to achieve a higher level of SSI/SSDI coverage. Our result suggests that an improved level of sensitivity can be achieved with existing CPS data by taking into account prior year responses to the 6Q sequence.

2.2 Research Design

We make use of a stacked difference-in-difference (DiD) design to estimate the causal effect of Medicaid expansion on disability-related outcomes. This approach is adapted from Deshpande and Li (2019) and other similar work, and it addresses potential biases identified by Goodman-Bacon (2021). Under a stacked DID framework, units subject to time-varying interventions are split into different cohorts or "sub-experiments," with each cohort receiving treatment at a different time. A distinct window is established for the pre-period and post-period, respectively, defining the number of time periods clean controls must be available within for each cohort to be included within the sub-experiment. Separate data sets are then constructed for each cohort consisting of the treated units as well as units that remain untreated through the entirety of the pre- and post-periods, ensuring that treated units are only compared to never-treated and not-yet-treated units (and not compared to already-treated states).

In our stacked DiD, each sub-experiment dataset is restricted to +/- 4 years from expansion year t. States that expanded Medicaid prior to year t are dropped from the sub-experiment dataset and any state that had not expanded Medicaid by year t+4 is treated as a control. The sub-experiment data sets are then appended to each other, and estimation proceeds with sub-experiment by state and sub-experiment by year fixed effects, to avoid making comparisons across sub-experiments. A standard difference-in-differences model is fitted to the "stacked" data using the following specification:

$$y_{i,s,t,e} = \alpha + (M_{s,e} * P_{t,e})\beta + \gamma_{s,e} + \delta_{t,e} + X_i\eta + B_{s,t}\zeta + \epsilon_{i,s,t,e}$$

Where $y_{i,s,t,e}$ represents person $i's$ disability program participation or employment outcome, $M_{s,e}$ is an indicator for state s being an expansion state in sub-experiment e, $P_{t,e}$ is an indicator that year t is post-expansion in sub-experiment e, $\gamma_{s,e}$ is a state-by-sub-experiment fixed effect, $\delta_{t,e}$ is a year-by-sub-experiment fixed effect, X_i are individual-level covariates (sex, race, age, education level, prior year household income below 150% FPL) and $\epsilon_{i,s,t,e}$ is an idiosyncratic error term. The term $B_{s,t}$ is a Bartik shift-share variable, which we include to control for any state- and time-varying labor demand conditions that may affect disability-related outcomes.[9] The coefficient of interest is β, which represents the causal

[9]The Bartik shift-share variable was constructed at the state-level using data from the American Community Survey. It was constructed by summing the product across industries of each state's initial industry share in the base year of 2007 and the national employment level for that industry (leaving out the contribution of the state for which the shift-share variable is being constructed for). When used in a regression including state fixed effects (thereby removing level differences between states), this produces a measure of labor demand under the counterfactual scenario that local employment grew only in proportion to national industry growth, removing the endogenous impact of local labor supply factors.

effect of Medicaid expansion on selected disability program participation and employment outcomes.

Medicaid expansion cohorts are included if there are at least four pre-period and four post-period years of data. This criteria results in the inclusion of the 2014, 2015 and 2016 Medicaid expansion cohorts, making up the majority of Medicaid expansion states. As noted above, we do not include early expander states owing to both concerns about partial treatment and lack of a sufficient pre-period. The 2019 cohort is excluded due to an insufficient post-period and concerns regarding the impact of the COVID-19 public health emergency contaminating the post-period. The control states for each cohort are all other states that are "clean controls" – i.e., they were not treated at any point within the pre- or post-periods for the sub-experiment. As shown in Appendix Figure A-1, this results in the 2019, 2020 and never-treated cohorts serving as controls for the 2014 expansion cohort, the 2020 and the never-treated cohorts serving as controls for the 2015 expansion cohort, and the never-treated cohort alone serving as a control for the 2016 expansion cohort.

We also test subdividing our sample by household income status (above/below 150% of the federal poverty level), age (18-49/50-64) and education level (High School or below/Any College). There are strong theoretical reasons to believe that these further subdivisions may be relevant when measuring the effects of Medicaid expansion. Because the Affordable Care Act's Medicaid expansion was only available to persons whose household income is below 138% of the federal poverty level, low-income households would be disproportionately likely to benefit from Medicaid expansion.[10] Similarly, older adults nearer to the retirement age may be more likely to opt into SSDI because of relaxed eligibility standards at older ages and/or as a means of early retirement. Educational attainment may also shape treatment effects by influencing the scope of occupational choices available to people with disabilities seeking employment as opposed to income support.

In notation, the shift-share variable can be expressed as follows:

$$z_{t,s} = \sum_{k} \frac{w_{k,07,s}}{w_{07,s}} * e_{t,k,s}$$

where $z_{t,s}$ is the shift-share variable for in year t for state s, $w_{s,k,07}$ is employment in industry k in 2007 in state s, $w_{07,s}$ is total employment in 2007 in state s and $e_{t,k,s}$ is national employment in year t in industry k excluding employment in state s.

[10]Unfortunately, the ASEC records household income status in relation to the poverty level in terms of those below FPL, 100-124% FPL, 125-149% FPL and > 150% FPL. As such, we subdivide by household income at 150% FPL.

2.3 Validity Tests

To assess the validity of the stacked DiD research design, we performed two sets of validity tests. The first set tests for evidence of compositional change in the population of people reporting disabilities by testing whether the prevalence of any disability subgroups changed in response to Medicaid expansion. The second set tests for parallel pre-trends in the treated versus control states for each of our outcomes of interest. We describe each in turn.

Before testing our outcomes of interest, we tested for the possibility that Medicaid expansion may have resulted in compositional shifts in each of our disabled populations by implementing our preferred specification with ongoing disability status, new disability status, previous disability status and never-disabled status as the dependent variable in turn. In Appendix Table A-3 and Appendix Figure A-2, we show DiD estimates and event study plots testing whether the prevalence of either disability subcategory changed because of Medicaid expansion (which would suggest potential unobserved compositional change that would threaten the validity of causal estimates on our outcomes of interest). Event studies show strong evidence of parallel trends in the pre-period for all groups. We find suggestive evidence that Medicaid expansion increased new disability by 9.7% relative to the average rate in our sample. There is a post-expansion decrease in the never-disabled population that is matched in timing by a post-expansion increase in the newly disabled population. This suggests Medicaid expansion may have increased rates of new disability, potentially due to previously uninsured persons learning of chronic conditions they would not have been aware of without Medicaid coverage and its accompanying access to medical care, thereby shifting their perception of their own abilities relative to those of their peers. In the context of our research design, this could indicate compositional change in unobserved factors that might bias estimates of the effect of Medicaid expansion for persons with new disabilities. In contrast, we do not find evidence that Medicaid expansion altered the frequency of ongoing disability (as the DiD estimate is nonsignificant and the event study shows no consistent pattern in the post-period). As such, we focus primarily on persons with ongoing disabilities going forward. Fortunately, this population is of greatest substantive interest, as it makes up the overwhelming plurality of SSI and SSDI enrollment.

We also test the validity of a key assumption for the DiD study design: that outcomes in treated units would have proceeded along parallel trends to outcomes in untreated units in the absence of treatment. While this is not directly testable, we test for differences in the outcome trends of treated and untreated units prior to the intervention using event studies. In Figure 2 (first row), we find strong evidence for parallel pre-trends for the SSDI outcome when estimating treatment effects in the general population, among all disabled respondents, and for ongoing disabled respondents. In Figure 3 (first row), we find similar evidence of parallel pre-trends for the SSI outcome. In Figure 4 (first row), we show evidence of parallel pre-trends for the employment outcome in the general population and to a lesser extent among all disabled respondents, but we find evidence of a pre-trend among ongoing disabled respondents that raises validity concerns about using the DiD study design to assess employment impacts on this subpopulation. We return to the lower rows of the event study figures in the heterogeneity section below, when we present the treatment effects by subgroup.

3 Results

We first present estimates of the effect of Medicaid expansion on Medicaid enrollment. This serves to confirm that Medicaid expansion increased Medicaid coverage among the population under study, an informal equivalent to the "first-stage" test of relevance in instrumental variable study designs. We then present our main estimates for our primary outcomes of interest: SSDI enrollment, SSI enrollment and employment. Data is at the person-level. Although we define disability recency using multiple observations, we make use of only one observation per individual, attributing person-level observations to the year of the person's second ASEC response. All standard errors are clustered at the state level.

3.1 Medicaid Enrollment

We present DiD estimates of the effect of Medicaid expansion on Medicaid enrollment for people with disabilities, for persons with ongoing disabilities, and for subgroups of people with ongoing disabilities defined by household income, age and educational attainment in Appendix Table A-4. Appendix Figure A-4 shows event study plots for these estimates while Appendix Figure A-5 shows trends in the raw data used in the stacked DiD estimation in event time. Column 1 of Appendix Table A-4 shows that the general population saw a statistically significant increase in Medicaid enrollment of 2.6 percentage points (27.1% relative to their average Medicaid enrollment), while Column 2 shows that people with disabilities saw a statistically significant increase of 7.0 percentage points (21.1% relative to their average Medicaid enrollment).

In contrast, columns 3-7 of Appendix Table A-4 show that among persons with ongoing disabilities the effect of Medicaid expansion on Medicaid enrollment was positive, but not statistically significant in any subgroup. This is explained, however, by review of the event study graphs shown in Appendix Figure A-4. These show that much of the increase in Medicaid enrollment for persons with ongoing disabilities took place one year prior to

Medicaid expansion.[11] In subsets of persons with ongoing disabilities in households below 150% of FPL, ages 18-49, or with educational attainment of a high school diploma or below, the increase in Medicaid enrollment takes place between event study coefficient -2 and event study coefficient -1. This anticipation effect is logical given that many persons with ongoing disabilities would already be categorically eligible for Medicaid enrollment via an existing pre-expansion pathway but may not have been aware of it prior to the publicity surrounding Medicaid expansion.

Such a mechanism is consistent with prior work. For example, Frean et al. (2017) find that half of Medicaid expansion's increase came from "woodwork effects" (sometimes also referred to as "welcome mat" effects) whereby previously eligible persons newly enrolled in response to a combination of increased awareness of the benefit, the removal of administrative barriers and other similar factors. Prior work on pre-ACA Medicaid expansions have also documented a substantial welcome mat effect on the previously eligible and analyses of Medicaid expansion's likely effects prior to implementation suggested that such woodwork effects would likely be substantial (Sonier et al., 2013; Sommers and Epstein, 2011). In short, we find that Medicaid expansion also increased Medicaid enrollment for persons with ongoing disabilities, with this increase coming primarily from a likely "welcome mat" effect on individuals already eligible for Medicaid.

3.2 Social Security Disability Insurance

Table 3 shows DiD estimates of Medicaid expansion's impact on SSDI receipt. We show that expansion increased SSDI receipt in the general population by 0.8 percentage points (22.2%), among people with disabilities by 5.1 percentage points (18.1%) and among persons with ongoing disabilities by 6.4 percentage points (17.7%). Appendix Figure A-6 shows trends in the raw data used in the stacked DiD estimation in event time. It reveals that for low-income households, persons ages 50-64 and those with any college education the treatment effect from Medicaid expansion is attributable both to an increase in SSDI receipt in expansion states and to a broader decline in SSDI receipt in non-expansion states. In the absence of a compelling explanation for why this decline would have impacted both expansion and non-expansion states equally in the absence of Medicaid expansion, this could represent a

[11] This anticipation effect attenuates the DiD coefficient and the treatment effects visible in event study plots. Consistent with established practice, the event studies shown in Appendix Figure A-4 use the period immediately prior to Medicaid expansion as the reference period, thereby removing from the subsequent event study coefficient differences between expansion and non-expansion states at that point. Similarly, DiD estimation removes the difference between expansion and non-expansion states in the pre-period from the DiD estimate (as these differences are loaded onto the β_1 term). This means that the DiD estimates for Medicaid expansion's impact on Medicaid enrollment for people with ongoing disabilities understate its true effect.

threat to the parallel trends assumption upon which DiD study designs depend. Fortunately, we understand the reason for this decline and know that it applied equally across the country. In 2011, the Social Security Administration undertook a series of policy initiatives designed to improve consistency and quality in disability case reviews by Administrative Law Judges. This resulted in a significant decline in the appellate allowance rate, contributing to a broader national decline in SSDI enrollment during our study period (Hoynes et al., 2023). This tightening of eligibility would have been equally relevant across expansion and non-expansion states, providing a clear explanation for the decline in control states that does not violate the parallel trends assumption. Indeed, it makes the large treatment effect Medicaid expansion appears to have had on SSDI enrollment all the more remarkable in that it took place during a time of tightening eligibility standards during which it was much more difficult for marginal applicants to enter the SSDI program.

In Appendix Table A-5, we show that these treatment effects are not significant for either those with previous disabilities or the never-disabled populations. This reinforces the importance of estimating treatment effects for persons at the margin of disability program participation, as general population treatment effects are attenuated as effects are concentrated among those with disabilities, and in particular, those with ongoing disabilities.

3.3 Supplemental Security Income

Table 4 shows DiD estimates of Medicaid expansion's impact on SSI receipt. We do not find any statistically significant treatment effect. However, Figure 3 shows that event study plots for this outcome do indicate a downward trend in the post-period, particularly among low-income households. The presence of a noisy pre-period with respect to persons with ongoing disabilities and various subgroups of persons with ongoing disabilities, however, and the absence of a statistically significant DiD coefficient mean that any evidence of a decline in SSI enrollment is purely suggestive in nature and cannot be firmly established.

3.4 Employment

Table 5 shows DiD results testing Medicaid expansion's impact on employment for people with disabilities. Although the DiD estimate for persons with ongoing disabilities in low-income households shows a statistically significant decline in employment as a result of Medicaid expansion, review of the event study plots in Figure 4 shows the absence of parallel trends in this and each of our other subgroups. As such, we must treat our results regarding employment outcomes as inconclusive and cannot draw any clear conclusions from them.

3.5 Heterogeneity by Disability Type, Age, and Education

To further explore heterogeneous treatment effects, we also test subdividing the ongoing disability category by household income status (above/below 150% of the federal poverty level), age (18-49/50-64) and education level (High School or below/Any College).

Table 3 shows that Medicaid expansion's effect on SSDI appears to be concentrated in low-income households and among persons ages 50-64. Rows 2 and 3 of Figure 2 confirm parallel pre-trends between treated and untreated units in these subgroups, reinforcing the validity of the treatment effects seen in the DiD estimates. In Appendix Table A-6, we also analyze these results by disability type, finding that the increase in SSDI enrollment is driven by persons with physical, self-care and/or independent living disabilities with no significant treatment effects for persons with cognitive, vision or hearing disabilities. In Appendix Table A-7, we provide a correlation matrix that shows these disability types—physical, self-care and independent living disabilities—tend to co-occur. Treatment effects are substantively similar regardless of educational attainment.

As discussed above, event study plots in Figure 3 suggest negative effects of Medicaid expansion on SSI receipt among certain subgroups. However, DiD estimates for SSI receipt are not significant, meaning these point estimates must be viewed as only suggestive of a negative effect on SSI receipt. Figure 4 shows very noisy pre-trends for the employment outcome in all subgroup analysis, leading us to further conclude that results for employment must be viewed as inconclusive.

3.6 Robustness

As a robustness check, we test the exclusion of one or more expansion cohorts in Appendix Tables A-9 to A-12. Across all tests, we find substantively similar point estimates. We also test an alternative definition of employment that relies only on respondents' employment status in the March in which they respond to the ASEC for the second time (rather than their average employment status across months 5-8 of the core monthly CPS). This too yields substantively identical results (see Appendix Table A-13). We also test a specification in which we only include respondents aged 61 or below, to remove any confounding effect of the availability of early retirement benefits at age 62 in the Social Security retirement program for older adults.[12] This yields substantively identical results, though effect sizes are

[12] Although individuals can claim Social Security Old Age Insurance benefits as early as age 62, such early claiming (prior to full retirement age) comes with an actuarial reduction in monthly benefits (up to 30%) to account for a longer period of expected benefit receipt. In contrast, SSDI benefits are not reduced to account for claiming before full retirement age. However, the greater administrative burden, scrutiny and time costs associated with SSDI application and receipt may result in some people with disabilities choosing

slightly larger (see Appendix Table A-14).

Prior research suggests that people with disabilities often confuse SSI and SSDI status when reporting their own program participation in survey data. Giefer et al. (2015) use SSA administrative data linked to the 2014 Survey of Income and Program Participation to find that as many as one-half of persons reporting SSI receipt were actually receiving Old Age Insurance or SSDI payments (though this figure includes older adults not included in our analysis). While confusion takes place in both directions between SSI and SSDI, the problem is worse in SSI owing to the smaller size of the program – i.e., a relatively small number of SSDI recipients inaccurately reporting SSI introduces far more noise in SSI treatment effect estimates than the opposite error does in SSDI treatment effect estimates. As a robustness check, we test removing persons with implausibly high SSI benefit levels[13] and those who switched from receiving SSDI income in their first ASEC observation to receiving SSI income in their second ASEC observation from the SSI outcome. We also include as a control an indicator variable for whether a state has categorical Medicaid eligibility for SSI recipients. This yields substantively similar results (see Appendix Table A-15).

4 Comparison with Recent Work

A contemporaneous working paper from Staiger et al. (2023) also makes use of CPS data to assess the impact of Medicaid expansion on SSI and SSDI take-up among people with disabilities. Whereas we focus on examining disability heterogeneity, they focus on differences in treatment effects on the basis of race and ethnicity. Like us, they find that Medicaid expansion increased SSDI enrollment for the broader population of people with disabilities (though their treatment effect is smaller than ours: 2.0 percentage points as compared to our 5.1). Unlike us, they find a statistically significant reduction in SSI takeup, whereas our results are only suggestive of such a reduction for some subpopulations of persons with ongoing disabilities. In this section, we explore multiple possible reasons for our different results.[14]

early retirement benefits instead.

[13]We define as implausibly high persons who report monthly benefit levels that exceed the federal benefit rate by more than $400. While many states offer an SSI State Supplement, they are typically far below $400.

[14]Huntington-Klein et al. (2021) have recently highlighted the problem of "researcher degrees of freedom" by providing multiple replication teams with identical data, research questions and broad identification strategies, then asked them to produce results using their typical research practices. Large differences in data cleaning and analysis decisions were found between teams, resulting in different sample sizes, statistical significance and, in one of the studies, a flipped sign. As our work and Staiger et al. (2023)'s address the same research questions (differing only in the sub-populations we each examine to assess heterogeneous treatment effects) and use the same data, our lack of knowledge of each other's work presents a natural instance of the parallel research processes Huntington-Klein et al. produced by experiment.

First, the two studies define disability in different ways. Whereas we use the 6Q sequence to identify people with disabilities, Staiger et al. (2023) add a seventh question related to work disability. While the addition of a seventh question identifies a larger percentage of SSDI and SSI recipients than the 6Q sequence alone, a major change to the wording of the work disability question took place in 2015. Prior to 2015, the ASEC question asked respondents about a disability or health problem at the time of the interview. From 2015 onward, the question asks about a disability or health problem at any time in the prior calendar year, and newly directs respondents to include disabilities that may have been temporary. This results in an increase in the population identified by the work disability question, taking place almost immediately after the 2014 Medicaid expansions. Since people with temporary disabilities do not qualify for SSI, the reduction in SSI receipt could in part reflect compositional change.

The two studies also use different quasi-experimental setups. Staiger et al. (2023) implement a DiD design using only the 2014 expansion cohort, whereas we use a stacked DiD design to assess the impact of multiple waves of expansion. In addition, there are substantial differences across the studies in the set of states included in the 2014 expansion cohort. Staiger et al. (2023) include Delaware, Massachusetts, New York, Vermont, Connecticut, DC, Minnesota, California and New Jersey in the 2014 expansion cohort, while we exclude these states because each had substantial Medicaid expansions prior to 2014.[15] On the other hand, Staiger et al. (2023) do not include Wisconsin in the 2014 expansion cohort (instead designating it as a control state), whereas we follow Schmidt et al. (2020) in classifying Wisconsin as a 2014 expansion state due to a state-funded expansion up to 100% FPL, even though the state did not embark upon the ACA's full Medicaid expansion up to 138% FPL. Lastly, Staiger et al. (2023) code Arizona as having expanded Medicaid in 2015, while we classify Arizona as having implemented a complete expansion in 2014.[16]

Several other differences exist. Most notably, the two studies use different control variables. Our specification controls for age, sex, whether a respondent is above or below 150% FPL in their first ASEC response (to control for the effect of expansion-related changes in income), and a Bartik shift-share variable to control for changes in labor demand, while their

[15] For example, New York had expanded Medicaid up to 100% FPL in 2001, Massachusetts had done so in 2006 and California implemented expansions in multiple high-population counties at different FPL thresholds in 2011 (Aliu et al., 2014; Maestas et al., 2014; Golberstein et al., 2015). While the 2014 expansion resulted in many of these states expanding eligibility further up to 138% FPL, we attributed these states to the year of their initial expansions (see Appendix Table A-2). That said, because the CPS only began asking the 6Q disability sequence in June 2008, we were unable to include pre-2014 expansion cohorts in our stacked DiD owing to the lack of an adequate pre-period and the study design's requirement of a balanced panel.

[16] Although Arizona had a pre-ACA expansion, the state had implemented an enrollment freeze in 2011 that had removed the vast majority of childless adult enrollees via attrition by the time it adopted the 2014 expansion, causing us to include it in the 2014 cohort. (Kaiser Family Foundation, 2015; Arizona Health Care Cost Containment System, 2013)

specification controls for marital status, state poverty rates, and non-metropolitan residency. We both control for race and college education. Staiger et al. (2023) lag their models, attributing ASEC observations to the year before the question was asked, since the ASEC asked about prior year benefits enrollment. We chose not to lag after concluding respondents were more commonly answering about their current year status. They also drop the year of expansion (2014 in their study) to remove partial effects during a transitional period (which we do not do). In terms of sample selection, Staiger et al. (2023) exclude respondents with household income at the 90th percentile or above, while we do not do. However, we only include respondents who were present in two successive ASEC observations, whereas their sample only requires respondents to be present in one ASEC observation. We maintain a single observation per respondent (their second ASEC observation, because we condition on disability status from the first ASEC observation), whereas their specification allows up to two observations per respondent. These sample selection choices mean their sample is much larger than ours.

To shed light on the differences in our results, we replicated their sample, data structure and specifications for SSI and SSDI receipt among the overall disabled population, and obtained near-identical point estimates, standard errors and sample size. We then implement each study difference one at a time (see Appendix Tables A-16 and A-17). Because the different samples and data structures are not directly comparable, we do the same for our specifications for the overall disabled population, iteratively adding changes to make our specification more like theirs, albeit built on our sample and data structure (see Appendix Tables A-18 and A-19). We also show the impact of modifying the covariate structure of each of our primary specifications (see Appendix Tables A-20 and A-21).[17]

Several design choices appear to be particularly impactful. Covariate selection plays an important role in the different results for SSI takeup (see Appendix Table A-20). Shifting from their covariate structure to ours while maintaining their sample and data structure attenuates their SSI result by approximately one-third, resulting in a loss of statistical significance. Similarly, shifting from our controls to theirs with our sample and data structure shifts from an insignificant positive point estimate to an insignificant negative one when using our full stacked setup and dramatically increases the size of the negative point estimate when estimating the effect of the 2014 expansion alone under both our and their definition of the 2014 expansion cohort. This appears to be primarily because of our use of a Bartik shift-share

[17] Although they do not specify a weighting scheme, we assume that Staiger et al. (2023) use the standard ASEC weight, whereas we use a weight offered by the ASEC for observations linked longitudinally across years. Similarly, because their sample selection does not permit observing the prior year characteristics of all respondents, we use current rather than lagged income as a covariate when working with their data structure and sample.

variable to control for labor demand changes across states and over time. This suggests that the decline in SSI participation found by Staiger et al. (2023) may be at least partially the result of stronger labor markets in expansion states.

Another impactful choice appears to have been the selection of which states were included in the 2014 expansion cohort. That this would matter is unsurprising, as our specification removes several high population states from the 2014 expansion cohort due to pre-2014 expansions, including California, New York, Massachusetts and New Jersey. However, as these states already extended Medicaid eligibility to most low-income persons not enrolled in SSI prior to 2014, we believe they are more appropriately assigned to a different expansion year and represent only partial treatments in 2014. Nonetheless, such partial treatments may have decreased SSI takeup by raising awareness of Medicaid expansion as an alternative pathway to Medicaid eligibility. As such, we believe both approaches are defensible—but caution that the impact of Medicaid expansion on SSI receipt appears to be sensitive to which way the 2014 expansion cohort is defined.

Differences in sample size play a smaller role. Even after adopting the same data structure and (much larger) sample as Staiger et al. (2023), we still obtain smaller point estimates that are not statistically significant when we use our 2014 cohort definition rather than theirs. This indicates our different findings are not simply the result of weaker statistical power.

With respect to SSDI, most specifications yielded significant positive point estimates (see Appendix Tables A-17 and A-19). Our larger effect size appears to be the result of a combination of our covariate selection, our definition of the 2014 cohort and our use of a full stacked setup. For example, had Staiger et al. (2023) adopted our covariate structure, the full stacked setup we employ and our definition of the 2014 cohort while retaining their sample and data structure, they would have found Medicaid expansion results in a 4.1 percentage point increase in SSDI, comparable to our treatment effect of 5.1 percentage points. Similarly, had we adopted their covariate structure, the 2014-only approach they employ and their definition of the 2014 cohort while retaining our sample and data structure, we would have found a point estimate indicating a 2.4 percentage point increase in SSDI (statistically insignificant, likely due to power), similar to their 2 percentage point treatment effect. Both examples can be found in Appendix Table A-21.

5 Discussion and Conclusion

We find that Medicaid expansion increased SSDI receipt. Treatment effects were driven by persons identified by the six-question sequence used by the CPS, particularly persons with ongoing disabilities ages 50-64. These findings suggest a "job unlock" mechanism whereby

Medicaid expansion permits persons who had remained in the labor force primarily to retain access to ESI to take early retirement through the SSDI program. In this scenario, Medicaid expansion serves as a bridge over the 29-month waiting period (from the onset of a person's disability) before they can access Medicare coverage. Although we find a corresponding decline in employment among people with disabilities, the parallel trends assumption was not satisfied for the employment outcome, and therefore our results are inconclusive for that outcome. However, the SSDI treatment effect is particularly robust among persons with ongoing disabilities ages 50-64 and those in low-income households—two groups likely to be particularly susceptible to "job unlock." By comparison with Staiger et al. (2023), we also show that our treatment effect for SSDI is robust to a broad range of specification and data cleaning choices.

With respect to SSI, our results are suggestive of a decrease in SSI enrollment consistent with theory but the estimate is not statistically significant. One potential reason for the greater noisiness of our SSI event studies may be found in a longstanding literature showing that respondents reporting benefit receipt in survey questions frequently confuse SSDI and SSI (Huynh et al., 2002). Our attempt to address this by removing plausibly false SSI self-reports did not yield substantively different results, but this may not have fully addressed misreporting in our data. We also show by comparison with Staiger et al. (2023) that under some plausible specifications and data cleaning approaches larger negative effects on SSI receipt can be found, indicating results for SSI are sensitive to study design choices (as might be expected for a smaller treatment effect).

Our findings suggest that the ACA's Medicaid expansion may have released some individuals with disabilities from job lock. Until recently, prior work on the impact of the ACA's Medicaid expansion on employment has generally not found evidence of job lock (Gooptu et al., 2016; Kaestner et al., 2017; Leung and Mas, 2018; Kandilov and Kandilov, 2022; Schmidt et al., 2020). Work dating from before the ACA is mixed. Garthwaite et al. (2014) found that a pre-ACA elimination of Medicaid coverage for childless adults in Tennessee resulted in increases in employment consistent with a "job lock" mechanism. Similarly, Dague et al. (2017) exploit the imposition of an enrollment cap on public insurance for childless adults to find that enrollment in public insurance reduced employment by 5 percentage points, a 12% decline. In contrast, the Oregon Health Insurance Experiment found no evidence that the experiment's Medicaid expansion had any effect on employment, earnings or SSDI receipt Baicker et al. (2014).

Our findings also demonstrate the value of estimating treatment effects among those persons who may be on the margin of employment and disability program entry, as identified by the 6Q disability sequence used by the CPS and other federal surveys, rather than among the general population. While a tradeoff exists with statistical power, this population is most likely to place a high value on access to health insurance owing to the presence of predictable and ongoing medical expenditures. Although the 6Q sequence identifies a small proportion of the general population, it represents more than half of SSDI and SSI enrollment, meaning that a treatment effect in this subgroup is likely to be economically significant. When using the longitudinal aspect of the CPS, the 6Q sequence also permits further subdividing of the disabled population based on disability recency, which we demonstrate is a highly relevant domain in disability program participation research.

This reflects an important and heretofore unconsidered advantage of relying on survey data as distinct from administrative data in research on disability income support programs. Administrative data has generally been viewed as the "gold standard" for research on program participation, given well documented problems of misreporting in survey research (Giefer et al., 2015; Huynh et al., 2002). Administrative data from SSA avoids this problem and provides a rich set of variables to characterize those receiving or who have applied for SSI or SSDI. However, this data provides no information on persons who are not receiving and who have not applied for benefits, but who may be "at risk" of applying in the future. This forces researchers to estimate treatment effects in the general population alone, as they cannot subset their numerator and denominator in the same way in order to explore subgroup effects. In contrast, survey data permits exploring treatment effect heterogeneity to a far greater extent, helping to understand precisely who is impacted by a policy change. Although we found that the increase in SSDI receipt was detectable in the general population, the ability to further subset our data to explore treatment effect heterogeneity yielded valuable insight about mechanisms, identifying low-income persons with ongoing physical, self-care and independent living disabilities near retirement age as the population induced into SSDI receipt by Medicaid expansion. This population is precisely the group theory implies would be job locked prior to expansion.

Finally, an increase in SSDI receipt is particularly remarkable given that expansion took place at a time of tightening SSDI eligibility standards and declining enrollment in the SSDI program nationally. Although Medicaid expansion prompted applicants on the margin of SSDI receipt to enter the program, their ability to access the program at a time when applications received greater scrutiny than in prior years suggests that their disabilities were not mild in nature. This reinforces a longstanding literature showing that recipients of disability income support programs often have significant work capacity (Maestas et al., 2013). Prior efforts to acknowledge this reality have generally taken two forms. In the 1980s, the Reagan Administration sought to forcibly remove SSDI enrollees from the program in the name of addressing program fraud, prompting considerable harm to many recipients who depended on SSDI for income support and a public backlash that led to legislative and legal changes that set the stage for further program expansion (Erkulwater, 2002). More recently, SSA has endeavored to encourage both SSI and SSDI enrollees to return to the workforce through additional services (i.e., the Ticket to Work program) and changes to program rules (e.g., Medicaid Buy-In programs, Section 1619(b) Continued Medicaid Eligibility, SSDI Extended Period of Eligibility for Medicare coverage).

Though these approaches are each distinct, they have an important commonality: they rely upon strategies that take effect only after individuals enter the program. But the application process for disability benefits itself adversely impacts work capacity on the part of applicants (even if they are not successful) because they must largely exit the labor force in order to apply (Autor et al., 2015). This reflects the binary definition of disability embedded in the SSDI and SSI programs; one can either be disabled or not, with persons in the latter category assumed not to require any income support. Our findings suggest this policy approach may not adequately capture the nuances of disability and work capacity. Our work adds to a growing body of evidence finding that disability and income support needs are not necessarily binary, and suggests the potential utility of institutional arrangements that might recognize both work capacity and income support needs among those on the margin of disability program entry.

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Figure 1: Identifying the Ongoing and Newly Disabled Subgroups in the CPS

Panel A: Ongoing Disabled

Month	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Month in Sample	1	2	3	4								5	6	7	8
6 Disability Questions	Y											Y			

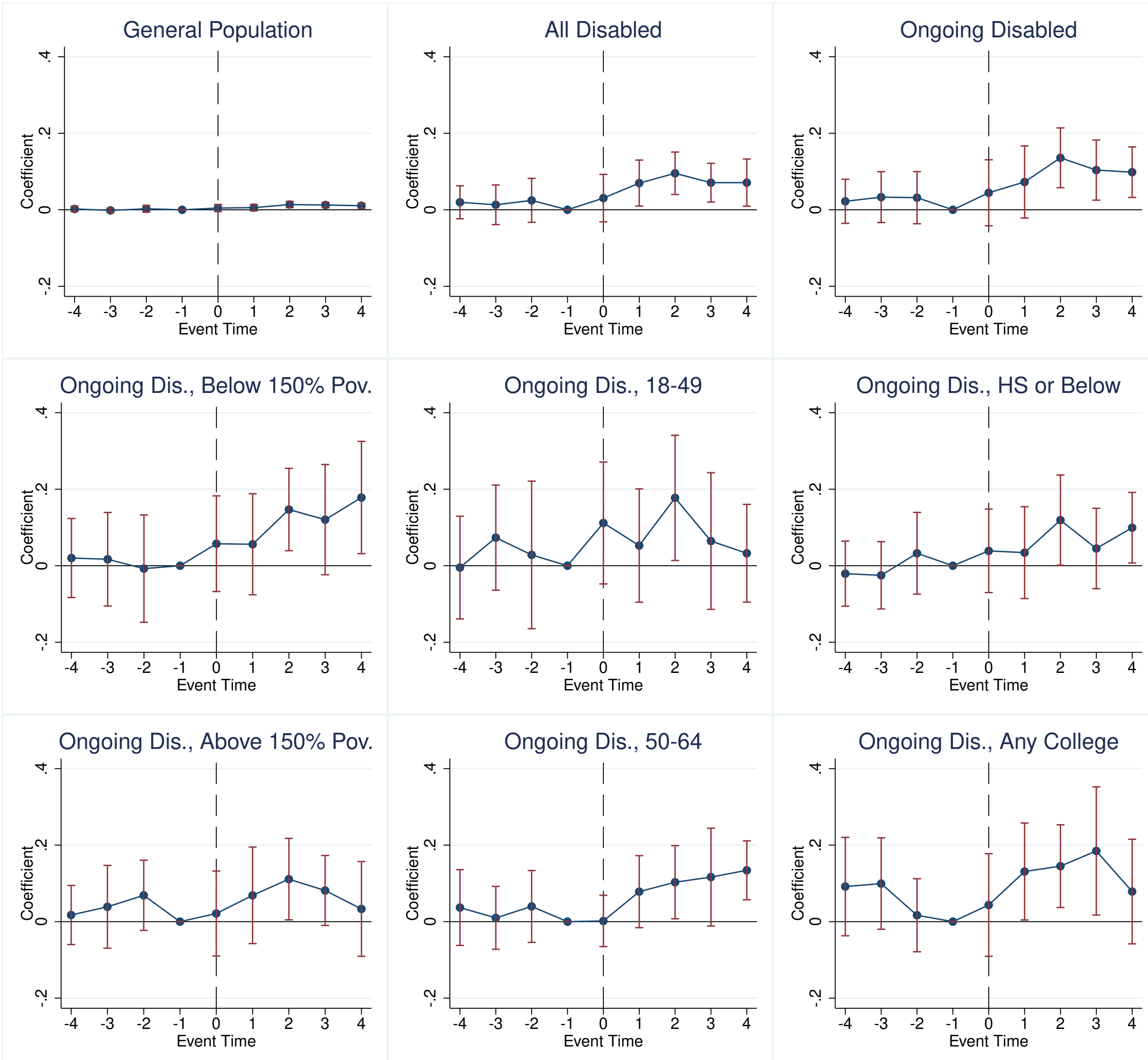
Respondents who indicate having a disability (“Y”) in both the first and second administrations of the disability questions are “ongoing disabled.”

Panel B: Newly Disabled

Month	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Month in Sample	1	2	3	4								5	6	7	8
6 Disability Questions	N											Y			

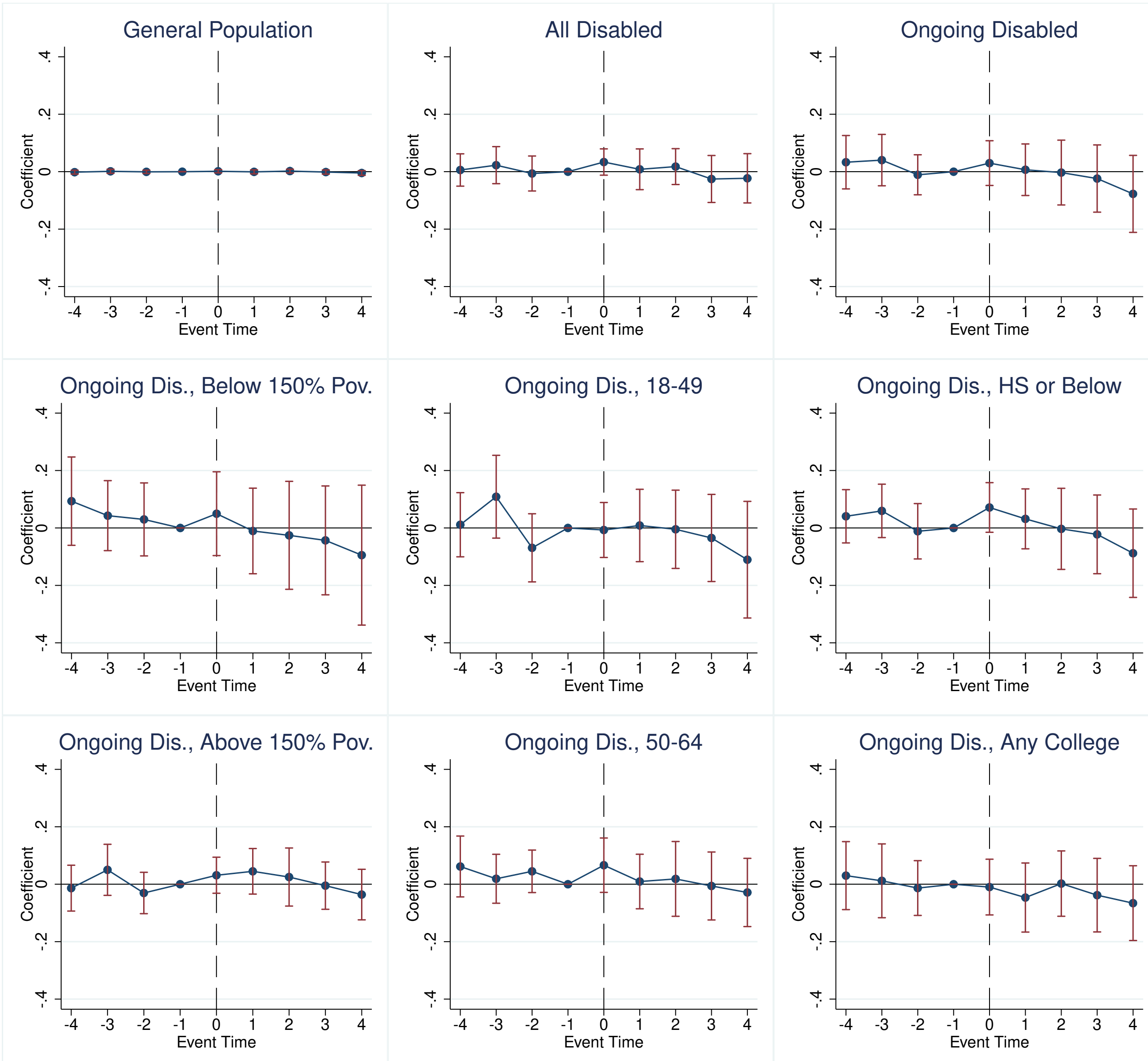
Respondents who did not indicate having a disability (“N”) in the first administration of the disability questions but did indicate having a disability in the second administration are “newly disabled.”

Figure 2: Event Study Estimates of Medicaid Expansion's Impact on SSDI Receipt



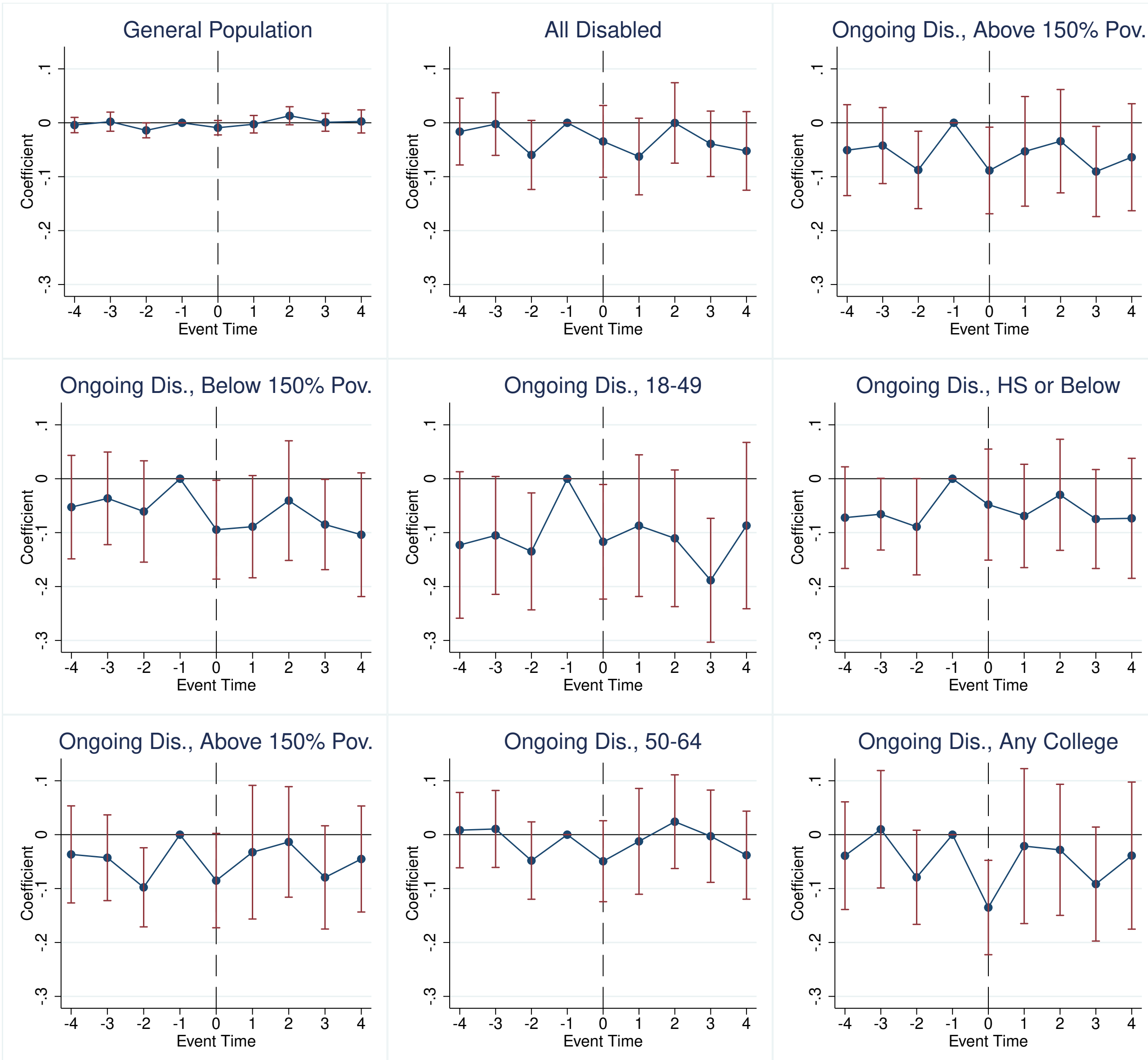
Notes: In all models, the dependent variable is SSDI receipt as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share control variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. 95% confidence intervals are shown.

Figure 3: Event Study Estimates of Medicaid Expansion's Impact on SSI Receipt



Notes: In all models, the dependent variable is SSI receipt on the basis of disability, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share control variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. 95% confidence intervals are shown.

Figure 4: Event Study Estimates of Medicaid Expansion's Impact on Employment Rates



Notes: In all models, the dependent variable is individual employment rate for the fifth through eighth months in the CPS sample. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. 95% confidence intervals are shown.

Table 1: Six Question Sequence for Identifying People with Disabilities in Survey Data

Disability Type	**Prompt**	**Universe**
Hearing Disability	Is this person deaf or does he/she have serious difficulty hearing?	All Persons
Vision Disability	Is this person blind or does he/she have serious difficulty seeing even when wearing glasses?	All Persons
Cognitive Disability	Because of a physical, mental, or emotional condition, does this person have serious difficulty concentrating, remembering, or making decisions?	Persons Aged 5+
Physical Disability	Does this person have serious difficulty walking or climbing stairs?	Persons Aged 5+
Self-Care Disability	Does this person have difficulty dressing or bathing?	Persons Aged 5+
Independent Living Disability	Because of a physical, mental, or emotional condition, does this person have difficulty doing errands alone such as visiting a doctor's office or shopping?	Persons Aged 15+

Source: Flood et al. (2022) Notes: An answer of "yes" to any of the six question prompts signifies the respondent has a disability.

Table 2: Descriptive Statistics

	All	Disabled	Ongoing Disabled	Newly Disabled	Previously Disabled	Never Disabled
Proportion of Sample	100.0%	7.5%	4.5%	3.0%	3.8%	88.7%
Employment/Benefit Participation						
Avg. employment rate(%)	71.0%	26.7%	19.0%	38.1%	47.7%	75.7%
Avg. SSI rate (%)	2.2%	18.2 %	23.2%	10.6%	8.4%	0.6%
Avg. SSDI rate (%)	3.3%	27.0%	35.2%	14.8%	12.7%	0.9%
Sex						
Female (%)	50.3%	49.2%	48.9%	49.5%	50.7%	50.4%
Race/Ethnicity						
White, Non-Hispanic (%)	69.9%	73.7%	74.7%	72.2%	72.4%	69.5%
Black, Non-Hispanic (%)	10.3%	13.1%	13.0%	13.4%	12.7%	10.1%
Hispanic (%)	12.8%	8.8%	8.2%	9.6%	10.2%	13.3%
Other (%)	6.9%	4.4%	4.1%	4.8%	4.8%	7.2%
Age						
18-34 (%)	35.9%	18.7%	17.5%	20.5%	23.0%	37.9%
35-49 (%)	28.8%	22.9%	22.4%	23.7%	24.3%	29.5%
50-64 (%)	35.2%	58.4%	60.1%	55.8%	52.7%	32.5%
Educational Attainment						
Bachelor's Degree (%)	32.2%	14.7%	12.5%	18.0%	19.4%	34.2%
N	308,249	23,322	13,899	9,423	11,958	272,969

Notes: Data for all working age adults (18-64) with at least two ASEC observations pooled years 2010-2020, weighted using the longitudinal weight for two adjacent years provided by the CPS.

Table 3: Impact of Medicaid Expansion on SSDI Receipt

	General Population	All Disabled	Ongoing Disabled	Ongoing Disabled by Income		Ongoing Disabled by Age		Ongoing Disabled by Education	
				<150% FPL	≥150% FPL	18-49	50-64	HS or Below	Any College
DID Estimate	0.008***	0.051***	0.064**	0.095**	0.029	0.066	0.053**	0.062	0.063
SE	(0.002)	(0.018)	(0.029)	(0.037)	(0.034)	(0.054)	(0.025)	(0.045)	(0.041)
P-Value	0.002	0.007	0.035	0.015	0.404	0.234	0.036	0.177	0.130
Mean Dep. Var	0.036	0.281	0.362	0.372	0.355	0.306	0.398	0.373	0.346
Effect Rel. to Avg Rate	22.2%	18.1%	17.7%	25.5%	8.2%	21.6%	13.3%	16.6%	18.2%
Observations in Stacked Dataset	333,332	26,635	16,024	6,936	9,088	5,688	10,336	9,419	6,605

Notes: In all models, the dependent variable is SSDI receipt, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *p < 0.10, **p < 0.05, ***p < 0.01.

Table 4: Impact of Medicaid Expansion on SSI Receipt

	General Population	All Disabled	Ongoing Disabled	Ongoing Disabled by Income		Ongoing Disabled by Age		Ongoing Disabled by Education	
				<150% FPL	≥150% FPL	18-49	50-64	HS or Below	Any College
DID Estimate	0.000	0.003	-0.018	-0.050	0.020	-0.028	-0.010	-0.007	-0.034
SE	(0.002)	(0.017)	(0.026)	(0.047)	(0.028)	(0.042)	(0.027)	(0.037)	(0.022)
P-Value	0.995	0.881	0.506	0.287	0.484	0.512	0.717	0.857	0.130
Mean Dep. Var	0.023	0.180	0.229	0.325	0.159	0.268	0.204	0.279	0.155
Effect Rel. to Avg Rate	0.0%	1.7%	-7.9%	-15.4%	12.6%	-10.4%	-4.9%	-2.5%	-21.9%
Observations in Stacked Dataset	333,332	26,635	16,024	6,936	9,088	5,688	10,336	9,419	6,605

Notes: In all models, the dependent variable is SSI receipt on the basis of disability, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *p < 0.10, **p < 0.05, ***p < 0.01.

Table 5: Impact of Medicaid Expansion on Employment Rates

	General Population	All Disabled	Ongoing Disabled	Ongoing Disabled by Income		Ongoing Disabled by Age		Ongoing Disabled by Education	
				<150% FPL	≥150% FPL	18-49	50-64	HS or Below	Any College
DID Estimate	0.003	-0.020	-0.022	-0.048**	-0.007	-0.032	-0.009	-0.005	-0.040
SE	(0.007)	(0.016)	(0.025)	(0.020)	(0.031)	(0.032)	(0.026)	(0.029)	(0.033)
P-Value	0.645	0.228	0.382	0.021	0.828	0.334	0.722	0.870	0.238
Mean Dep. Var	0.709	0.260	0.182	0.080	0.256	0.211	0.164	0.135	0.253
Effect Rel. to Avg Rate	0.4%	-7.7%	-12.1%	-60.0%	-2.7%	-15.2%	-5.5%	-3.7%	-15.8%
Observations in Stacked Dataset	331,994	26,635	16,024	6,936	9,088	5,688	10,336	9,419	6,605

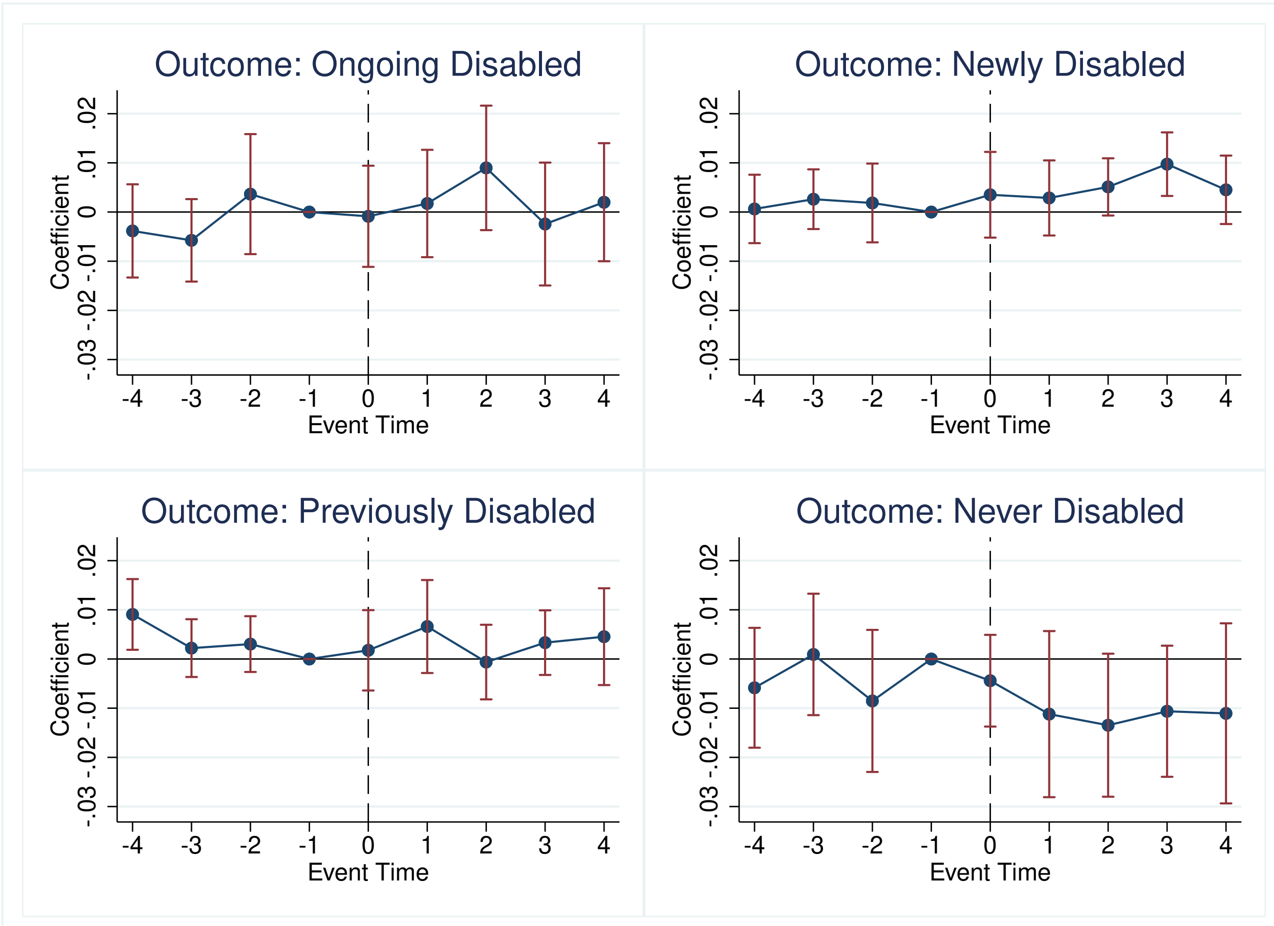
Notes: In all models, the dependent variable is individual employment rate for the fifth through eighth months in the CPS sample. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *p < 0.10, **p < 0.05, ***p < 0.01.

Appendix

Figure A-1: Illustrating the Stacked DiD Structure

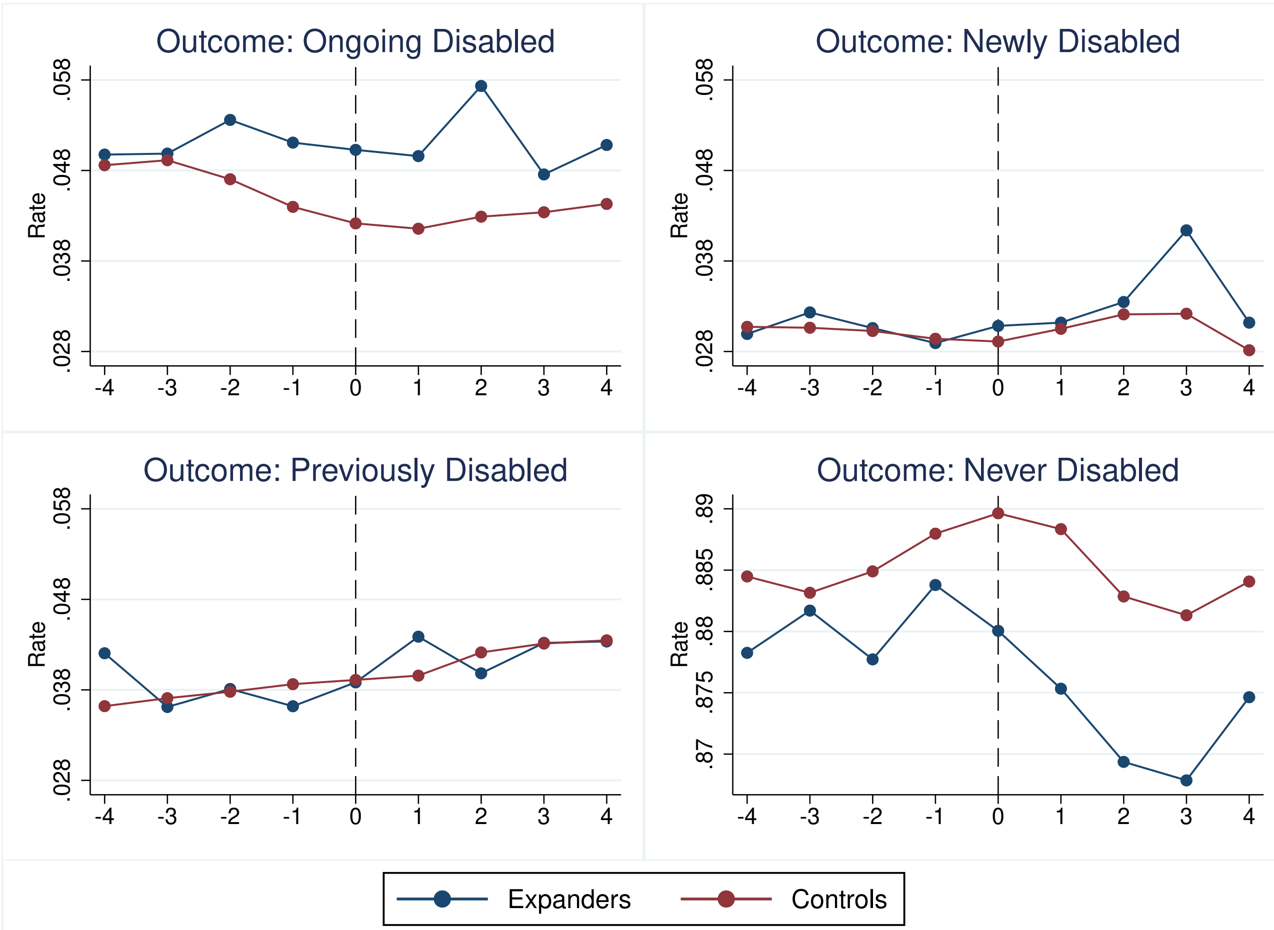
<table>
<tr><th>Sub-Experiment</th><th>Expansion Cohort</th><th>Years Included</th><th>2010</th><th>2011</th><th>2012</th><th>2013</th><th>2014</th><th>2015</th><th>2016</th><th>2017</th><th>2018</th><th>2019</th><th>2020</th><th>Control Cohorts</th></tr>
<tr><td>1</td><td>2014</td><td>2010-2018</td><td colspan="4">Pre-Period</td><td>Exp.</td><td colspan="4">Post-Period</td><td></td><td></td><td>2019
2020
Never</td></tr>
<tr><td>2</td><td>2015</td><td>2011- 2019</td><td></td><td colspan="4">Pre-Period</td><td>Exp.</td><td colspan="4">Post-Period</td><td></td><td>2020
Never</td></tr>
<tr><td>3</td><td>2016</td><td>2012-2020</td><td></td><td></td><td colspan="4">Pre-Period</td><td>Exp.</td><td colspan="4">Post-Period</td><td>Never</td></tr>
</table>

Figure A-2: Event Study Estimates of Medicaid Expansion's Impact on Disability Subgroup Prevalence



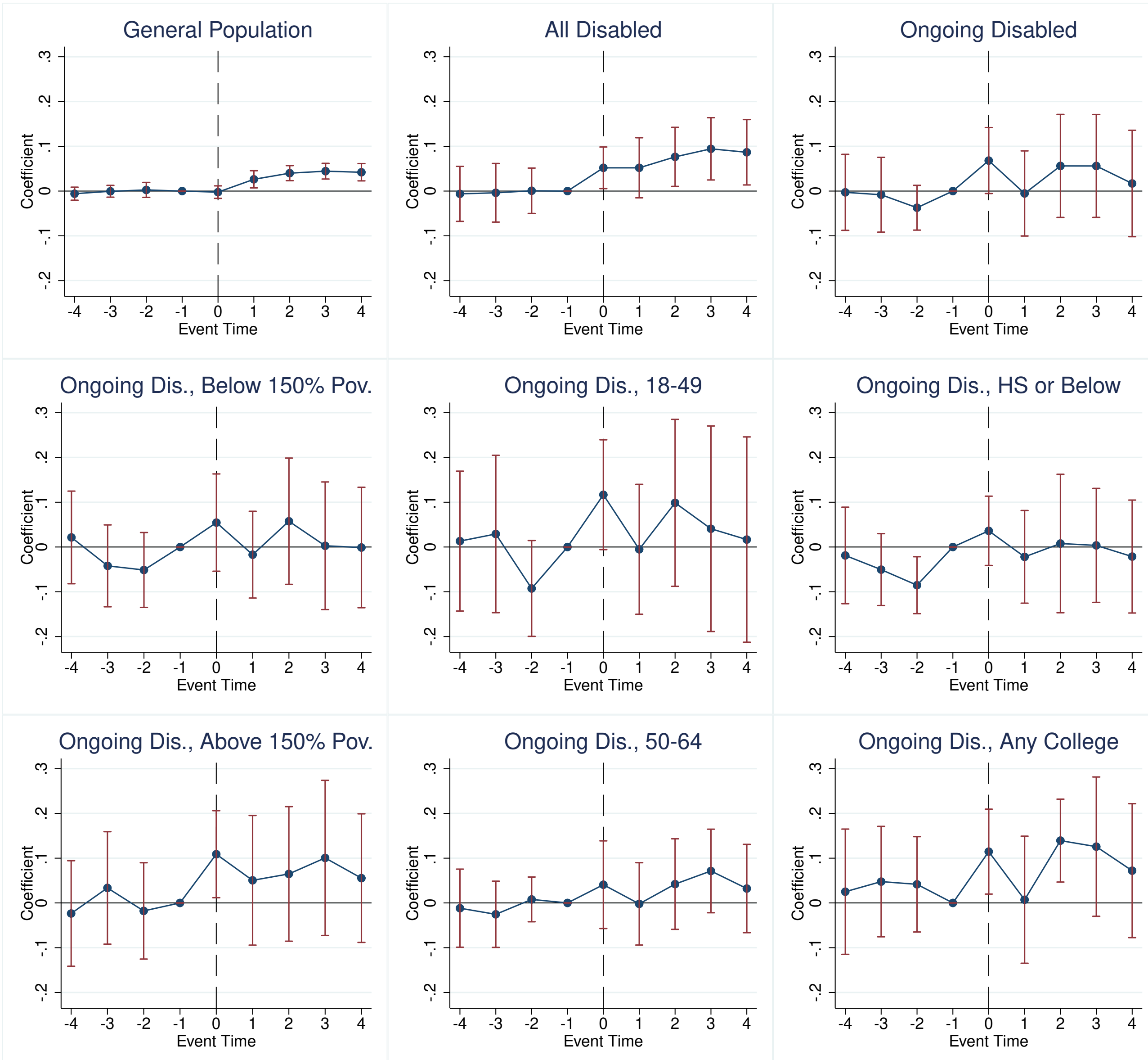
Notes: In all models, the dependent variable is the relevant disability status as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share control variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. 95% confidence intervals are shown.

Figure A-3: Raw Data Trends in Disability Subgroup Prevalence in Event Time



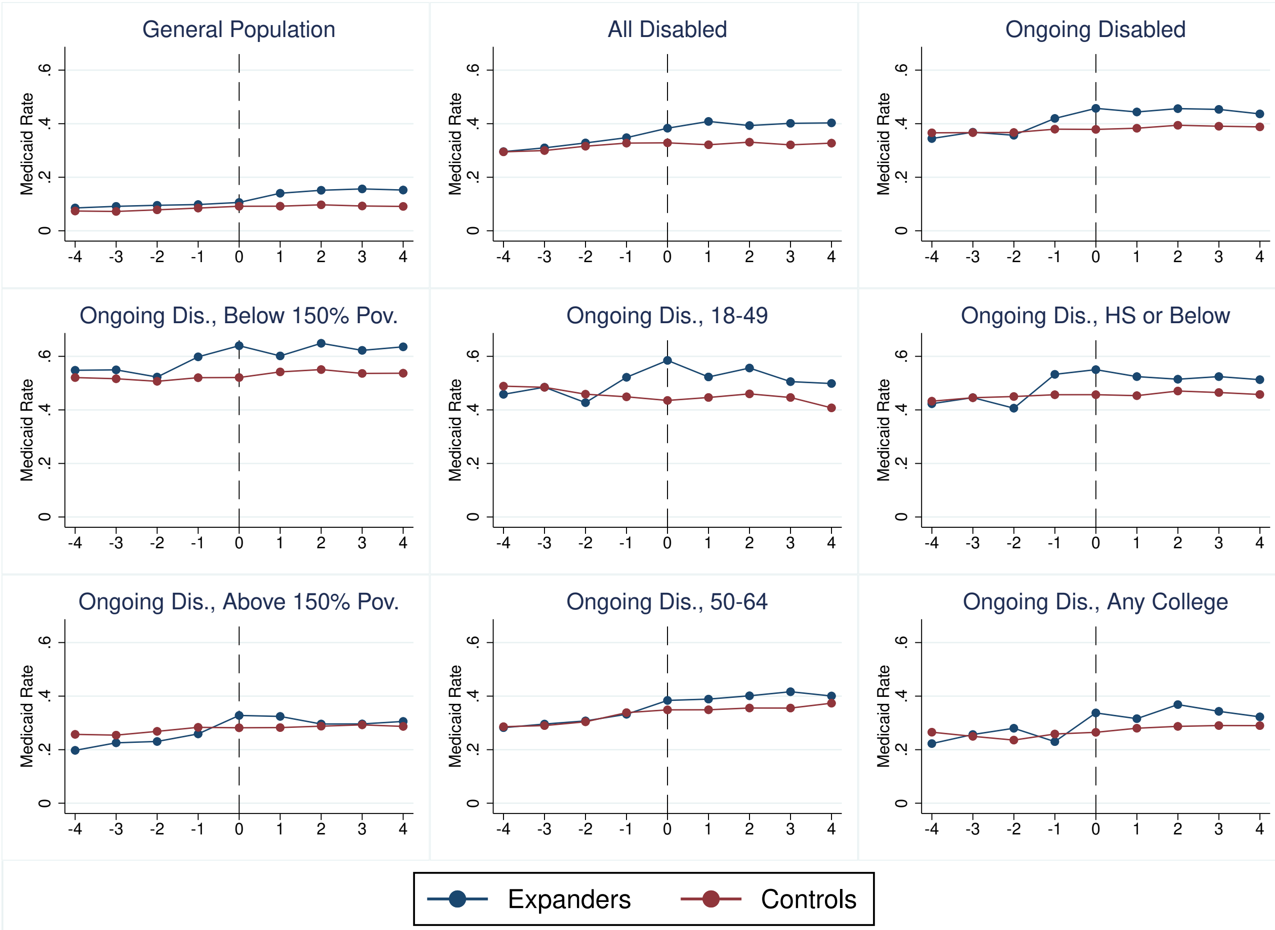
Notes: All plots show average rates of the relevant disability status for both expansion states and control states, shown in event-time (i.e. time defined in relation to expansion timing within a given sub-experiment). Observations are weighted using the CPS longitudinal weight for two adjacent years.

Figure A-4: Event Study Estimates of Medicaid Expansion's Impact on Medicaid Enrollment



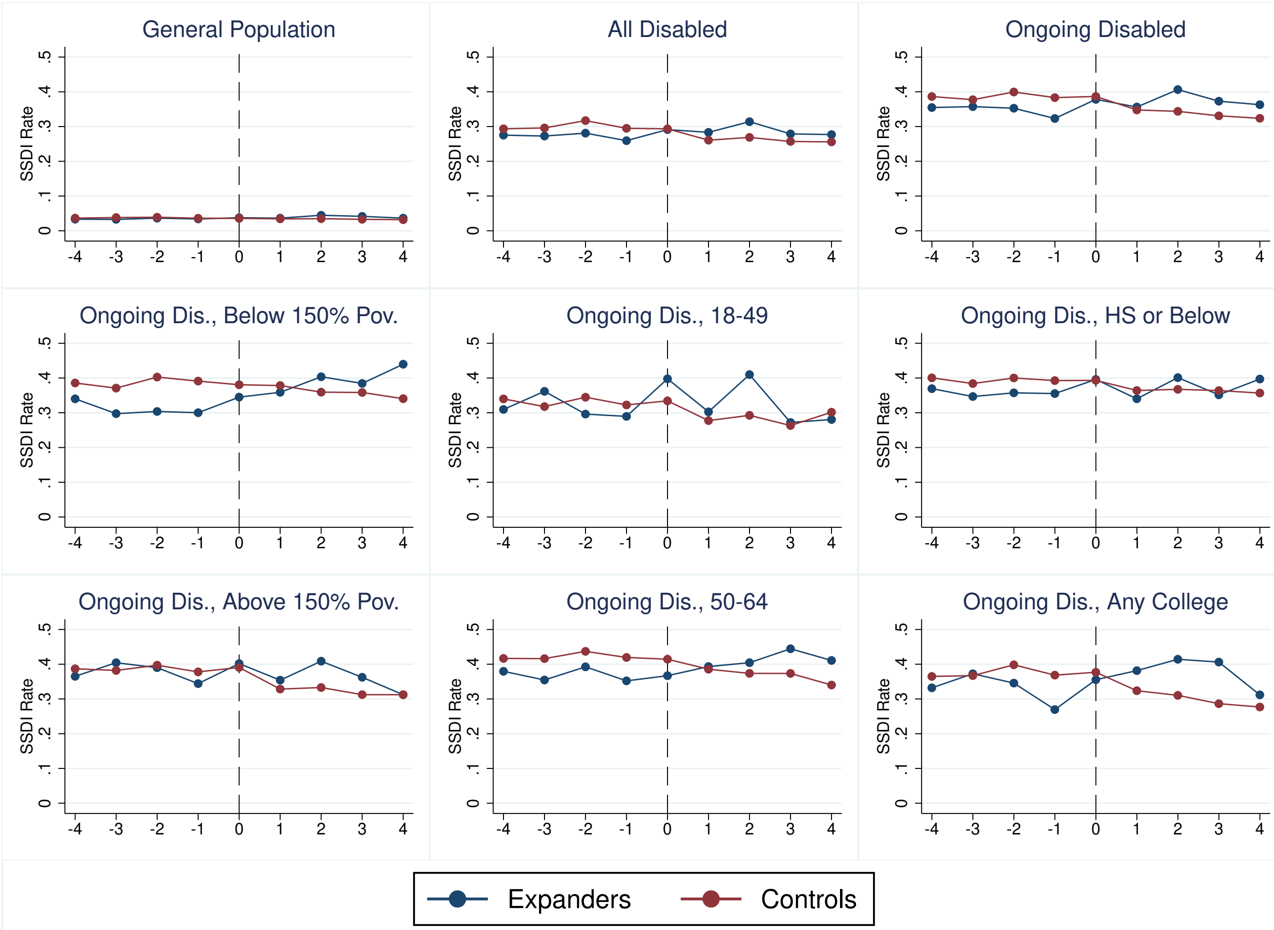
Notes: In all models, the dependent variable is Medicaid enrollment as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share control variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. 95% confidence intervals are included.

Figure A-5: Raw Data Trends in Medicaid Enrollment in Event Time



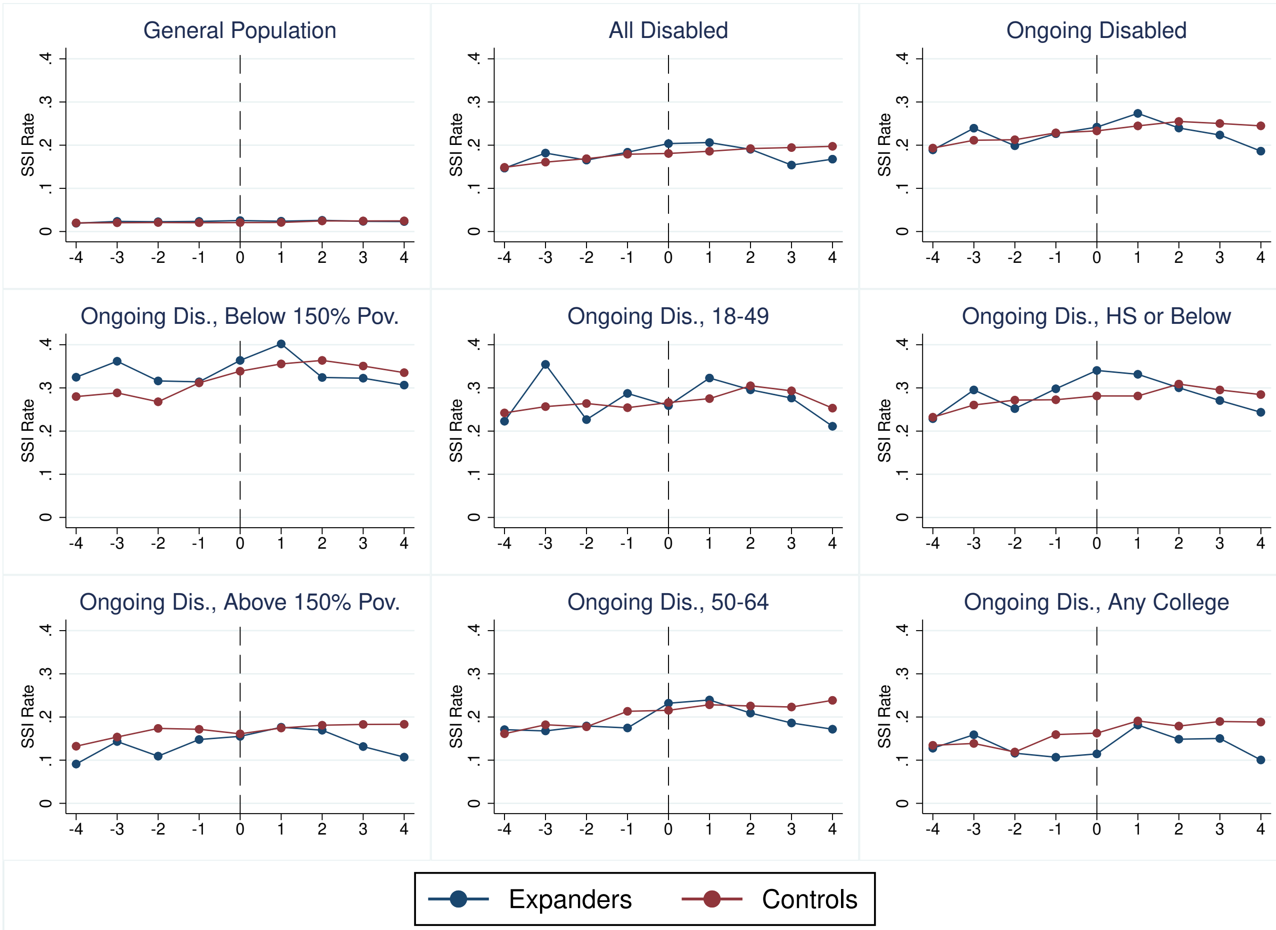
Notes: All plots show average rates of Medicaid enrollment for both expansion states and control states, shown in event-time (i.e. time defined in relation to expansion timing within a given sub-experiment). Observations are weighted using the CPS longitudinal weight for two adjacent years.

Figure A-6: Raw Data Trends in SSDI Receipt in Event Time



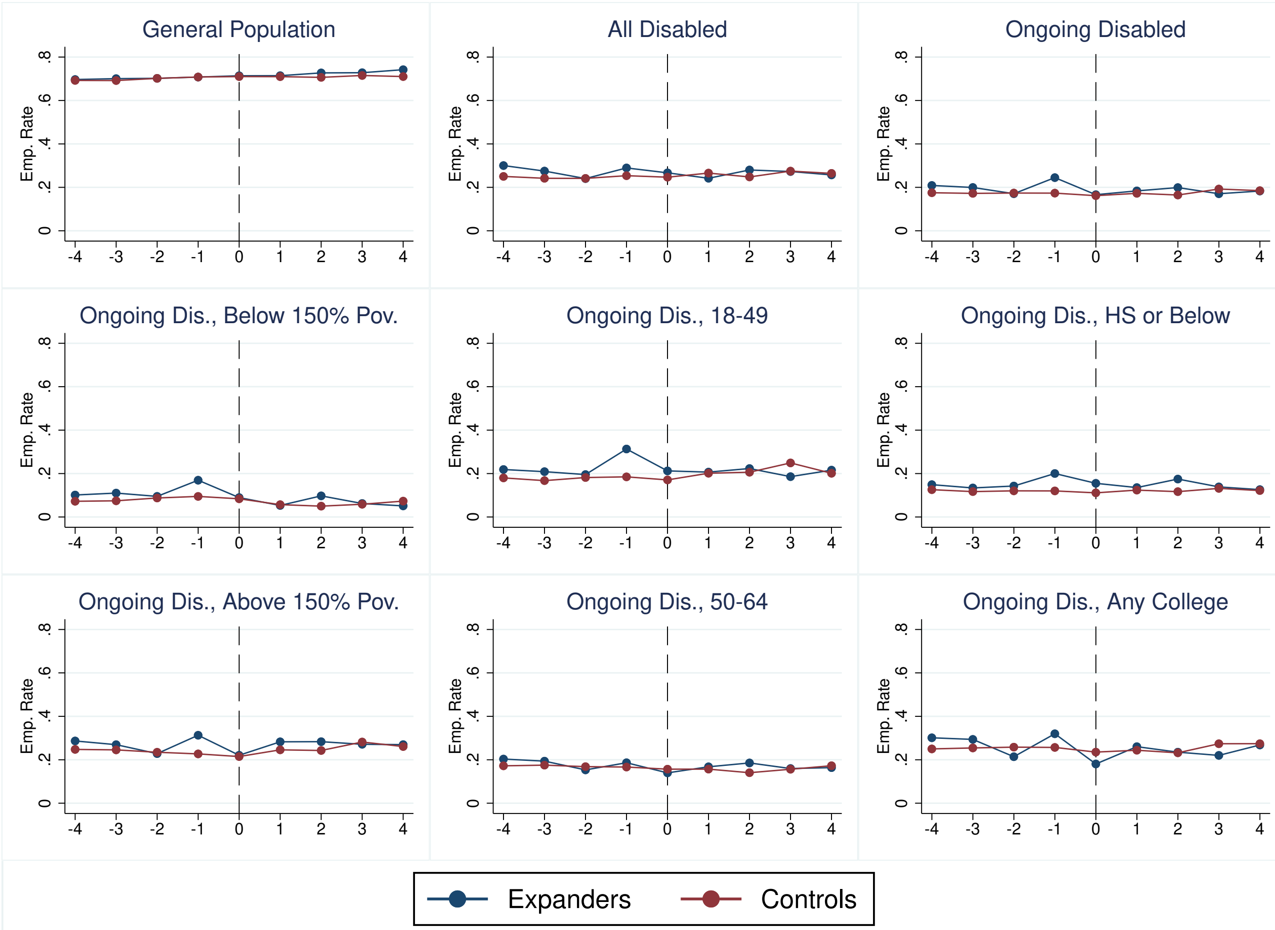
Notes: All plots show average rates of SSDI receipt for both expansion states and control states, shown in event-time (i.e. time defined in relation to expansion timing within a given sub-experiment). Observations are weighted using the CPS longitudinal weight for two adjacent years.

Figure A-7: Raw Data Trends in SSI Receipt in Event Time



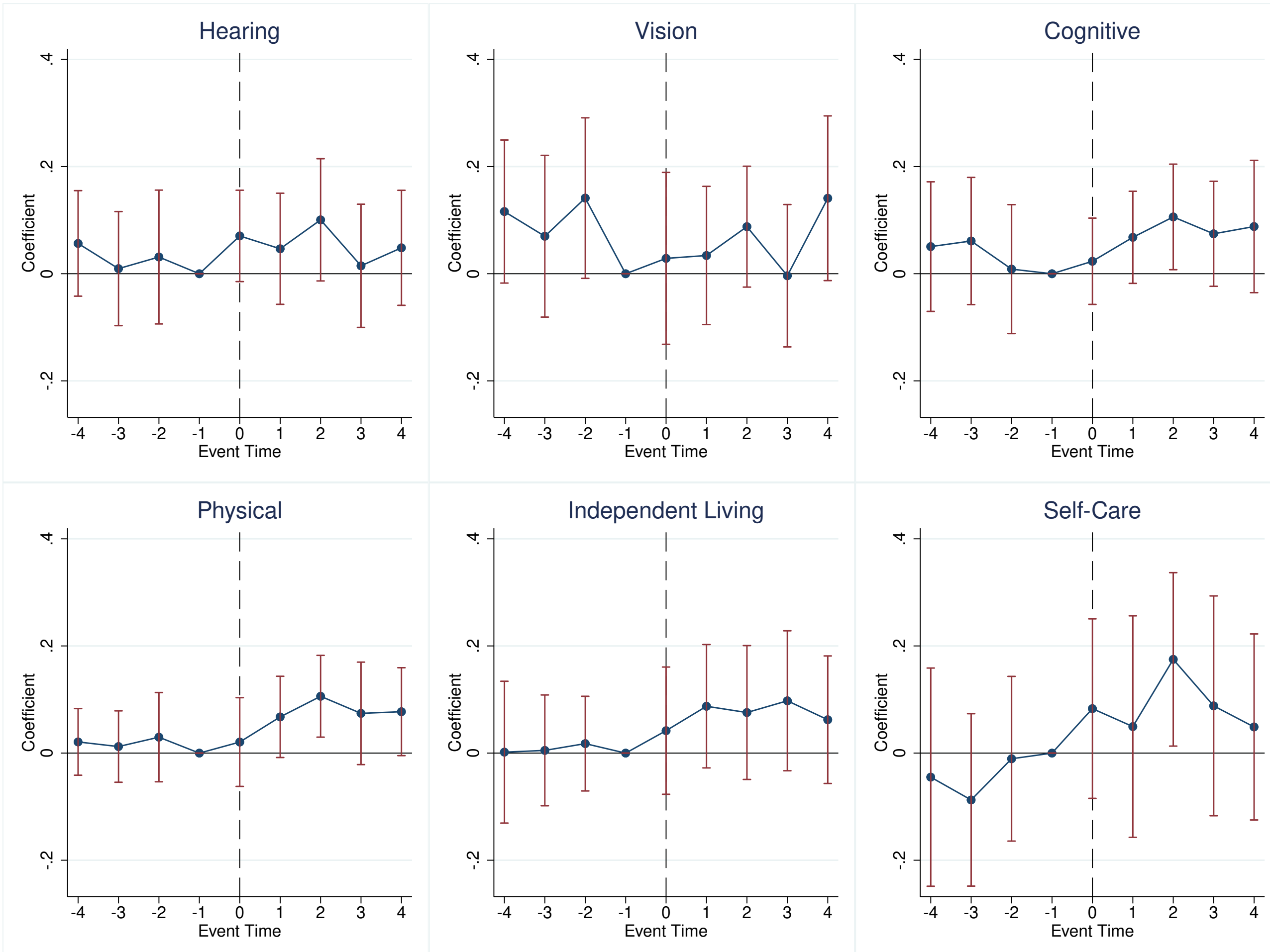
Notes: All plots show average rates of SSI receipt on the basis of disability for both expansion states and control states, shown in event-time (i.e. time defined in relation to expansion timing within a given sub-experiment). Observations are weighted using the CPS longitudinal weight for two adjacent years.

Figure A-8: Raw Data Trends in Employment Rates in Event Time



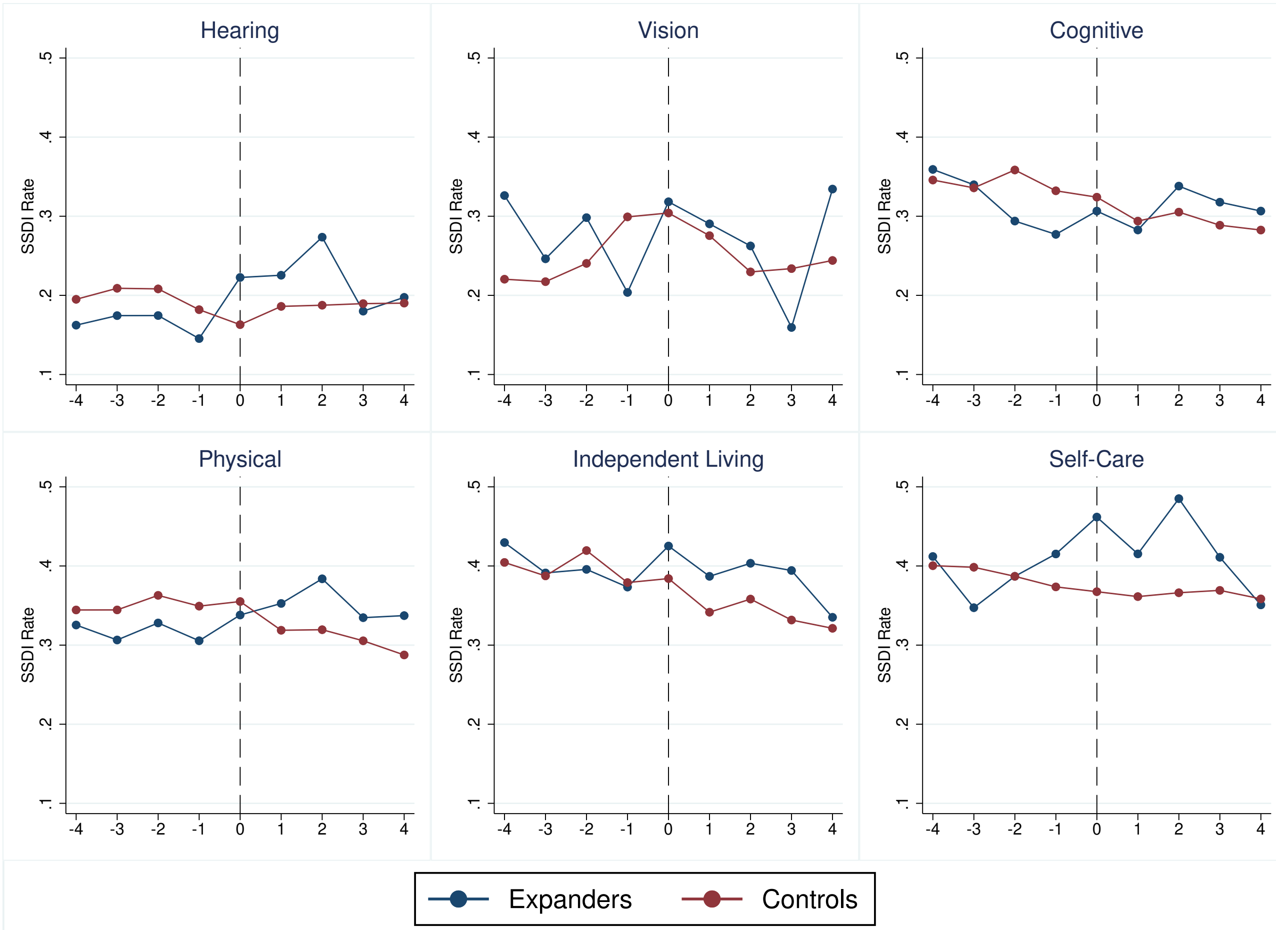
Notes: All plots show average rates of employment for both expansion states and control states, shown in event-time (i.e. time defined in relation to expansion timing within a given sub-experiment). Observations are weighted using the CPS longitudinal weight for two adjacent years.

Figure A-9: Event Study Estimates of Expansion's Impact on SSDI Receipt, by Disability Type



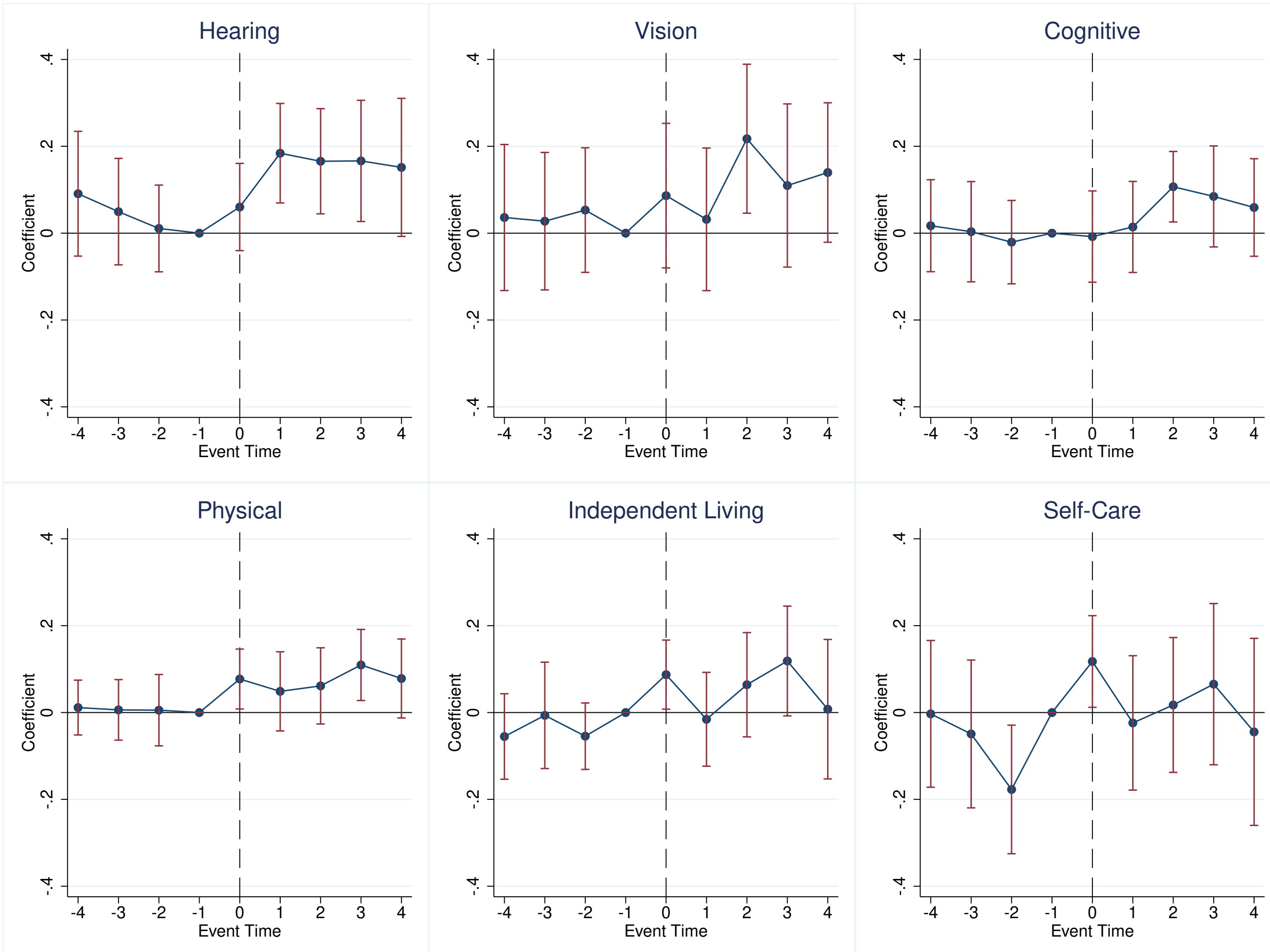
Notes: In all models, the dependent variable is SSDI receipt as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share control variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. 95% confidence intervals are shown.

Figure A-10: Raw Data Trends in SSDI Receipt in Event Time, by Disability Type



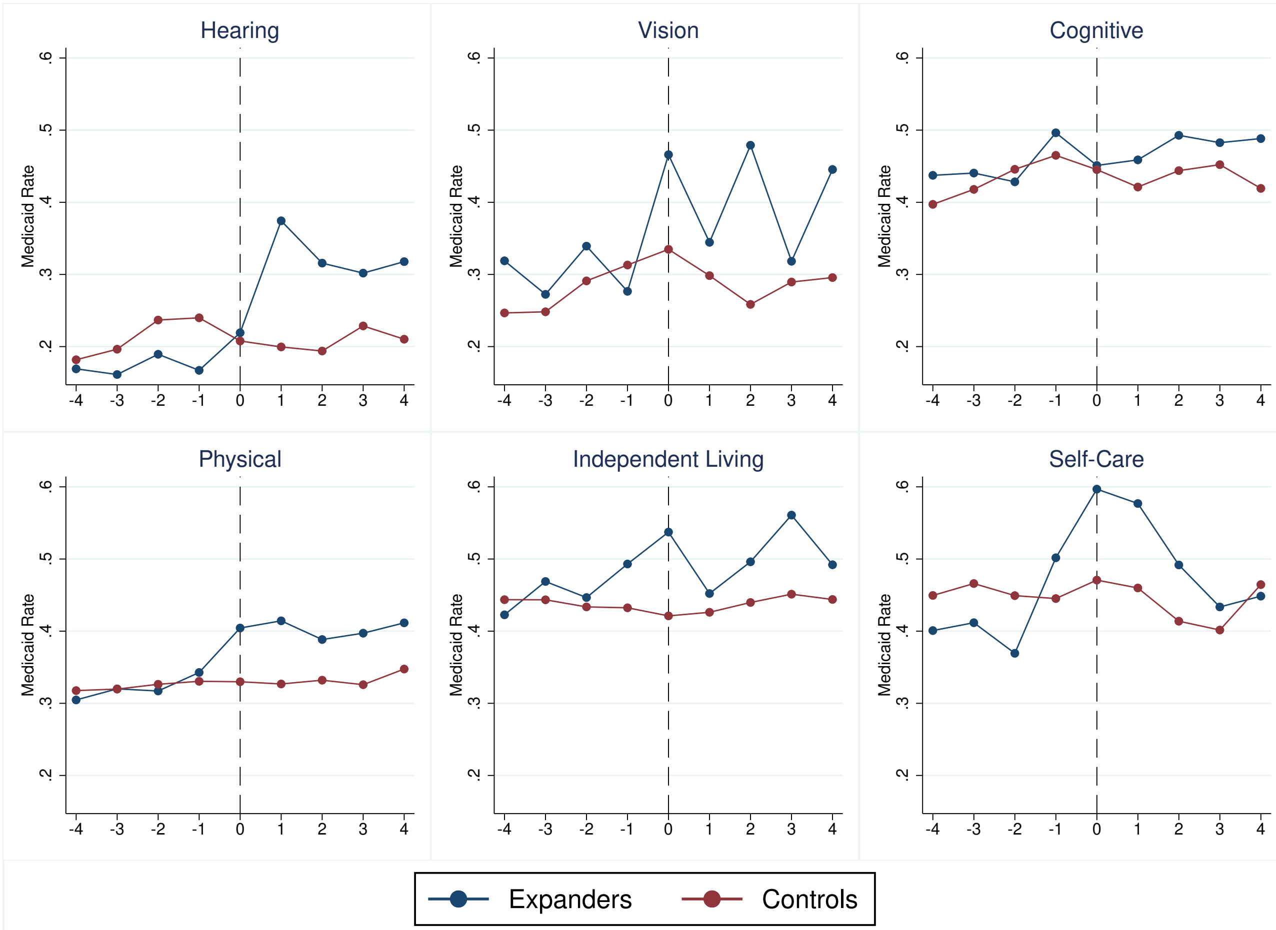
Notes: All plots show average rates of SSDI receipt for both expansion states and control states, reflected in event-time (i.e.: time defined in relation to expansion timing within a given sub-experiment). Observations are weighted using the CPS longitudinal weight for two adjacent years.

Figure A-11: Event Study Estimates of Medicaid Expansion's Impact on Medicaid Enrollment, by Disability Type



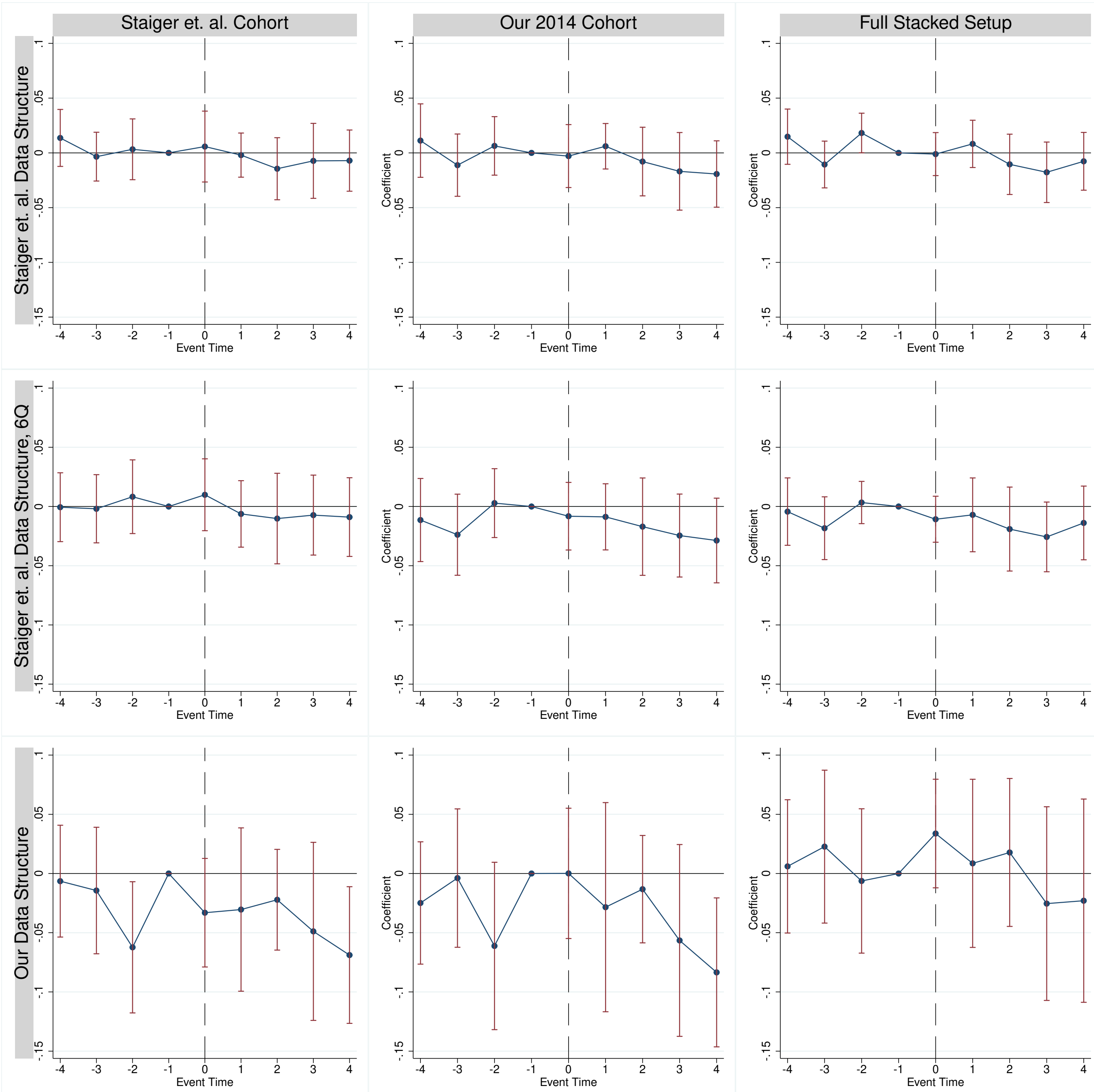
Notes: In all models, the dependent variable is Medicaid enrollment as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share control variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. 95% confidence intervals are shown.

Figure A-12: Raw Data Trends in Medicaid Enrollment in Event Time, by Disability Type



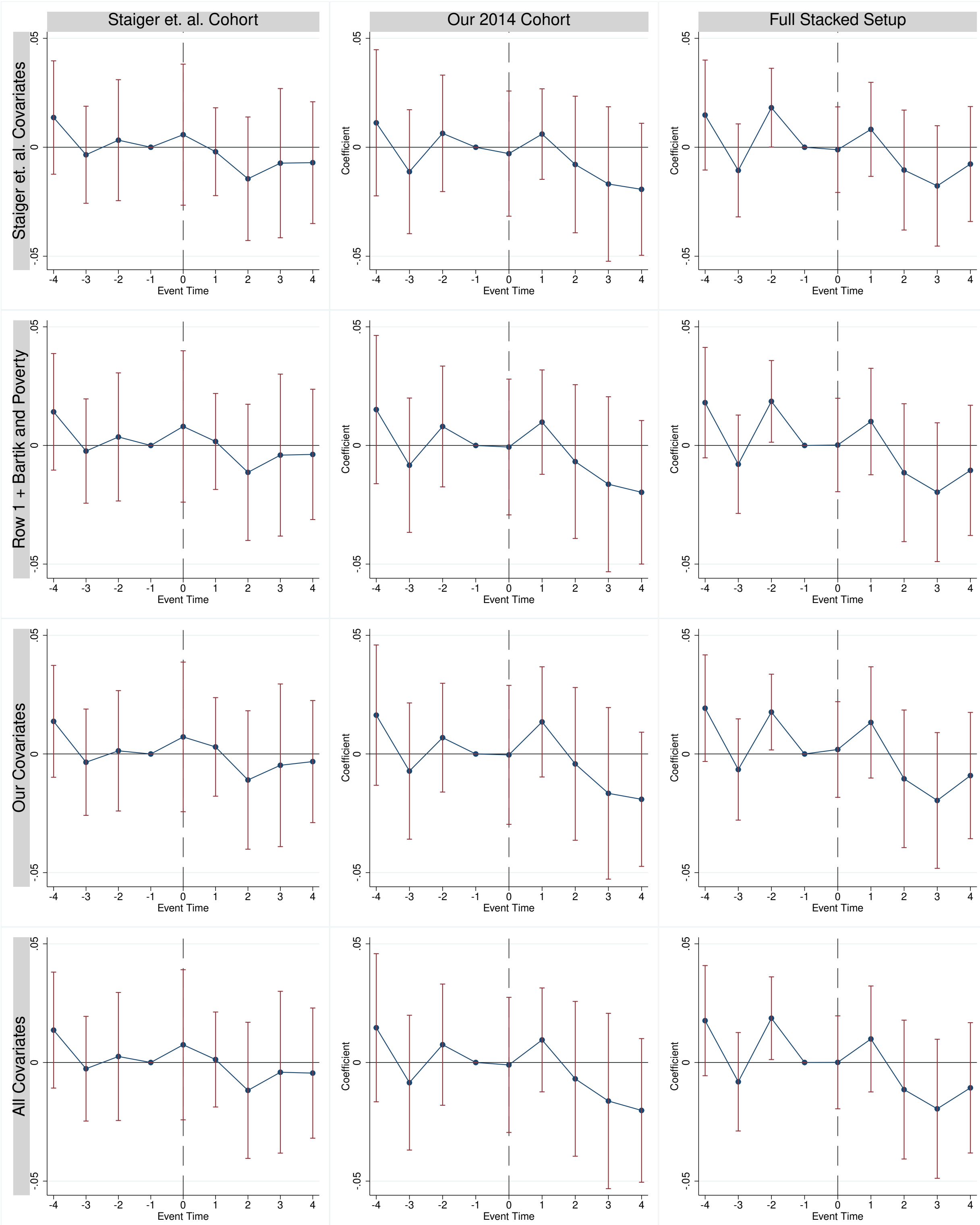
Notes: All plots show average rates of Medicaid enrollment for both expansion states and control states, reflected in event-time (i.e.: time defined in relation to expansion timing within a given sub-experiment). Observations are weighted using the CPS longitudinal weight for two adjacent years.

Figure A-13: Event Study Estimates comparing Staiger et al. (2023) Data Structure and Cohorts with Our Own, SSI Receipt



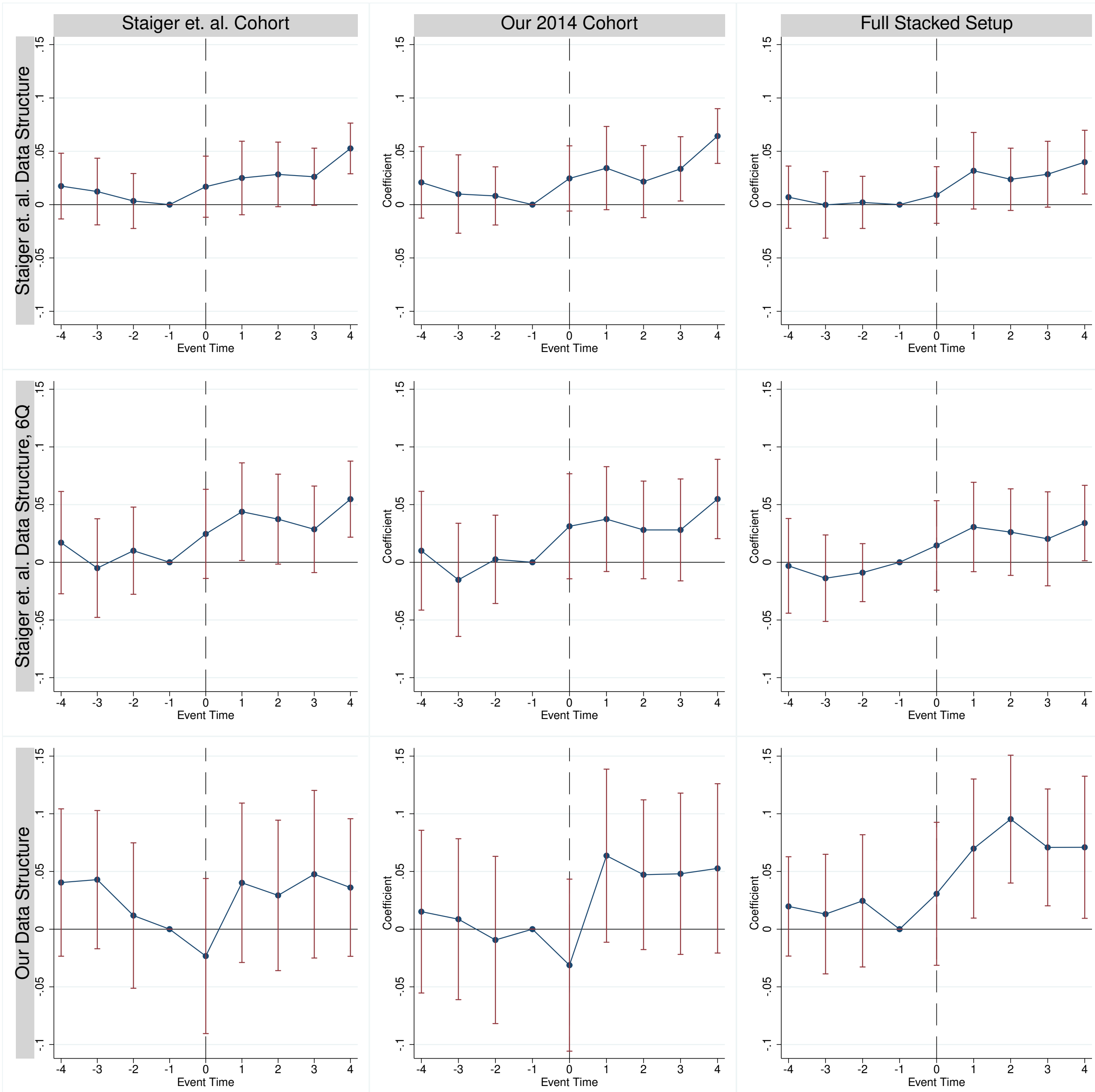
Notes: In all models, the dependent variable is SSI Receipt on the basis on disability, as a binary indicator. Models include covariates based on the specified data structure. Staiger et al. (2023) covariates include race, college education, marital status, state poverty rates, non-metropolitan residency, and state and calendar year fixed effects. Our covariates include sex, race, age group, college education, prior income level, Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the person level ASEC weight under the Staiger et al. (2023) data structure and the CPS longitudinal weight for two adjacent years under our data structure. 95% confidence intervals are shown.

Figure A-14: Event Study Estimates Comparing Staiger et al. (2023) Covariate Structure with Our Own, SSI Receipt



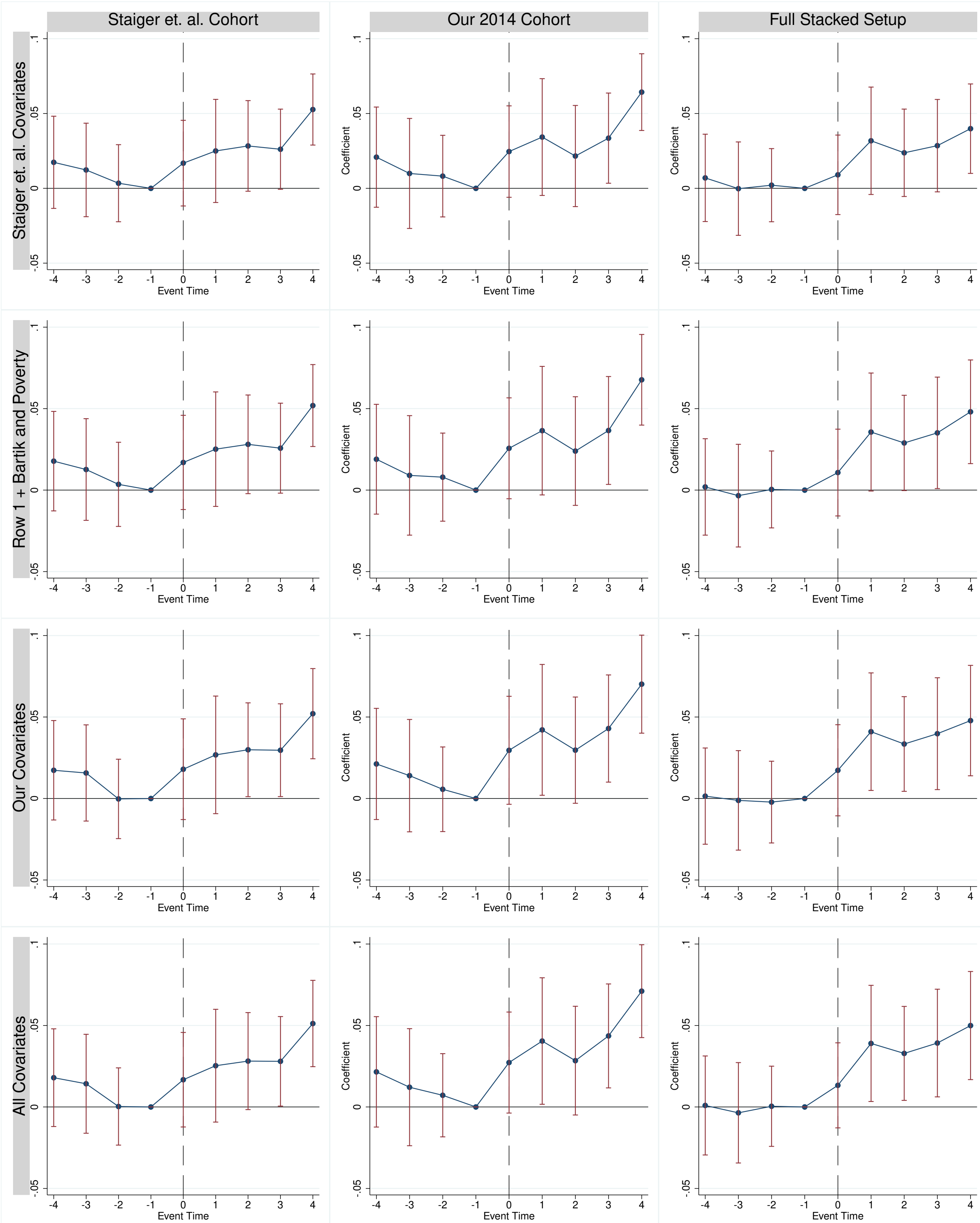
Notes: In all models, the dependent variable is SSI Receipt on the basis of disability, as a binary indicator. 95% confidence intervals are shown. Staiger et al. (2023) covariates include race, college education, marital status, state poverty rates, non-metropolitan residency, and state and calendar year fixed effects. Our covariates include sex, race, age group, college education, prior income level, Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the person level ASEC weight under the Staiger et al. (2023) data structure and the CPS longitudinal weight for two adjacent years under our data structure. 95% confidence intervals are shown.

Figure A-15: Event Study Estimates Comparing Staiger et al. (2023) Data Structure and Cohorts with Our Own, SSDI Receipt



Notes: In all models, the dependent variable is SSDI Receipt, as a binary indicator. Models include covariates based on the specified data structure. Staiger et al. (2023) covariates include race, college education, marital status, state poverty rates, non-metropolitan residency, and state and calendar year fixed effects. Our covariates include sex, race, age group, college education, prior income level, Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the person level ASEC weight under the Staiger et al. (2023) data structure and the CPS longitudinal weight for two adjacent years under our data structure. 95% confidence intervals are shown.

Figure A-16: Event Study Estimates Comparing Staiger et al. (2023) Covariate Structure with Our Own, SSDI Receipt



Notes: In all models, the dependent variable is SSDI Receipt, as a binary indicator. Staiger et al. (2023) covariates include race, college education, marital status, state poverty rates, non-metropolitan residency, and state and calendar year fixed effects. Our covariates include sex, race, age group, college education, prior income level, Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the person level ASEC weight under the Staiger et al. (2023) data structure and the CPS longitudinal weight for two adjacent years under our data structure. 95% confidence intervals are shown.

Table A-1: SSDI/SSI Enrollment by Disability Recency

	SSI	SSDI	Concurrent SSI and SSDI	SSI and/or SSDI
Ongoing	3105	4863	681	7287
	(46.5%)	(47.5%)	(54.2%)	(46.6%)
Newly	932	1362	168	2126
	(14.0%)	(13.3%)	(13.4%)	(13.6%)
Previously	985	1480	179	2286
	(14.8%)	(14.5%)	(14.3%)	(14.6%)
Never	1654	2530	228	3956
	(24.8%)	(24.7%)	(18.2%)	(25.3%)
Total	6676	10235	1256	15655
	(100.0%)	(100.0%)	(100.0%)	(100.0%)

Unweighted counts and percentages for working age adults (18-64) with at least two ASEC observations, pooled years 2010-2020.

Table A-2: Medicaid Expansion Cohorts

Cohort	**States**
Pre-ACA	Delaware, Massachusetts, New York, Vermont
2010	Connecticut, District of Columbia
2011	Minnesota
2012	California, New Jersey
2014	Arkansas, Arizona*, Colorado, Hawaii, Illinois, Iowa, Kentucky, Maryland, Michigan, Nevada, New Hampshire, New Mexico, North Dakota, Ohio, Oregon, Rhode Island, Washington, West Virginia, Wisconsin†
2015	Alaska, Indiana, Pennsylvania
2016	Louisiana, Montana
2019	Maine, Virginia
2020	Hawaii, Nebraska, Utah
Never Expand	Alabama, Florida, Georgia, Kansas, Mississippi, Missouri, North Carolina, Oklahoma, South Carolina, South Dakota, Tennessee, Texas, Wyoming

*Arizona had a pre-ACA expansion in 2000 for childless adults up to 100% FPL. However, there was an enrollment freeze starting 2011. The enrollment freeze was lifted in 2014 when Arizona adopted Medicaid expansion consistent with ACA guidelines. Missouri and Oklahoma expanded Medicaid in 2021.

† Although Wisconsin did not participate in the ACA's full Medicaid expansion, the state conducted a state-funded expansion of Medicaid up to 100% FPL that took place in 2014. Consistent with Schmidt, Shore-Sheppard, & Watson (2020), we classify them as a 2014 expander.

Table A-3: DiD Estimates with Disability Status as Outcome

	Ongoing Disabled	Newly Disabled	Previously Disabled	Never Disabled
DiD Estimate	0.003	0.003	0.000	-0.007
Standard Error	(0.003)	(0.002)	(0.003)	(0.004)
P-Value	0.398	0.100	0.924	0.140
Average Rate	0.047	0.031	0.040	0.881
Effect Relative to Avg. Rate	6.4%	9.7%	0.0%	-0.8%
Observations in Stacked Dataset	333,332	333,332	333,332	333,332

Notes: In all models, the dependent variable is the relevant disability status as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years.*$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A-4: Impact of Medicaid Expansion on Medicaid Enrollment

	General Population	All Disabled	Ongoing Disabled	Ongoing Disabled by Income		Ongoing Disabled by Age		Ongoing Disabled by Education	
				$<$150% FPL	$\geq$150% FPL	18-49	50-64	HS or Below	Any College
DID Estimate	0.026***	0.070***	0.049	0.038	0.078	0.070	0.039	0.040	0.059*
SE	(0.006)	(0.025)	(0.035)	(0.038)	(0.049)	(0.057)	(0.029)	(0.046)	(0.032)
P-Value	0.000	0.007	0.169	0.313	0.123	0.226	0.180	0.392	0.070
Mean Dep. Var	0.096	0.331	0.390	0.547	0.277	0.472	0.337	0.465	0.279
Effect Rel. to Avg Rate	27.1%	21.1%	12.6%	6.9%	28.2%	14.8%	11.6%	8.6%	21.1%
Observations in Stacked Dataset	333,328	26,635	16,024	6,936	9,088	5,688	10,336	9,419	6,605

Notes: In all models, the dependent variable is Medicaid receipt, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A-5: Impact of Medicaid Expansion on Outcomes of Interest, for Previous Disability and Never Disability Subgroups

	General Population	Previous Disability	Never Disability
Outcome: Medicaid Enrollment			
DID Estimate	0.026***	0.061**	0.019***
SE	(0.006)	(0.025)	(0.006)
P-Value	0.000	0.020	0.002
Avg. Medicaid Rate	0.096	0.201	0.070
Effect Rel. to Avg. Rate	27.1%	30.3%	27.1%
Observations in Stacked Dataset	333,328	13,791	292,902
Outcome: SSDI Receipt			
DID Estimate	0.008***	-0.001	0.002
SE	(0.002)	(0.020)	(0.001)
P-Value	0.002	0.973	0.119
Avg. SSDI Rate	0.036	0.135	0.010
Effect Rel. to Avg. Rate	22.2%	-0.7%	20.0%
Observations in Stacked Dataset	333,332	13,791	292,906
Outcome: SSI Receipt			
DID Estimate	0.000	-0.005	-0.001
SE	(0.002)	(0.016)	(0.001)
P-Value	0.995	0.762	0.355
Avg. SSI Rate	0.023	0.080	0.006
Effect Rel. to Avg. Rate	0.0%	-6.3%	-16.7%
Observations in Stacked Dataset	333,332	13,791	292,906
Outcome: Employment			
DID Estimate	0.003	-0.015	0.009
SE	(0.007)	(0.022)	(0.008)
P-Value	0.645	0.498	0.235
Avg. Employment Rate	0.709	0.474	0.760
Effect Rel to Avg Rate	0.4%	-3.2%	1.2%
Observations in Stacked Dataset	331,994	13,782	291,577

Notes: Models include demographic covariates (sex, race, age group, college education, prior income level), Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years *p < 0.10, **p < 0.05, ***p < 0.01.

Table A-6: Impact of Medicaid Expansion on SSDI Receipt, by Disability Type

	All	Disability Type					
	Disabled	Hearing	Vision	Cognitive	Physical	Ind. Living	Self-Care
DID Estimate	0.051***	0.036	-0.021	0.041	0.049*	0.065*	0.120*
SE	(0.018)	(0.027)	(0.052)	(0.037)	(0.024)	(0.037)	(0.071)
P-Value	0.007	0.192	0.689	0.278	0.053	0.090	0.098
Avg. SSDI Rate	0.281	0.191	0.255	0.314	0.333	0.375	0.384
Effect Rel to avg Rate	18.1%	18.8%	-8.2%	13.1%	14.7%	17.3%	31.3%
Observations in Stacked Dataset	26,635	5,004	3,484	9,317	15,630	8,934	4,438

Notes: Respondents may indicate having any of the six disability types, which are not mutually exclusive. In all models, the dependent variable is SSDI receipt, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A-7: Correlation Between Disability Types

	Hearing	Vision	Cognitive	Physical	Independent	Self-Care
Hearing	1.000					
Vision	0.024	1.000				
Cognitive	-0.152	-0.069	1.000			
Physical	-0.243	-0.121	-0.276	1.000		
Independent	-0.159	-0.018	0.179	0.114	1.000	
Self-Care	-0.070	-0.002	0.052	0.290	0.392	1.000

Notes: Correlation between disability types for working age respondents (ages 18-64) who indicate having a disability in their second ASEC observation, pooled for years 2010-2020 (N=23,322). Results are weighted using the CPS longitudinal weight for two adjacent years.

Table A-8: Impact of Medicaid Expansion on Medicaid Enrollment, by Disability Type

	All	Disability Type					
	Disabled	Hearing	Vision	Cognitive	Physical	Ind Living	Self-Care
DID Estimate	0.070***	0.108***	0.082**	0.044	0.049*	0.075	0.088
SE	(0.025)	(0.036)	(0.038)	(0.040)	(0.024)	(0.047)	(0.056)
P-Value	0.007	0.005	0.038	0.281	0.053	0.166	0.120
Avg. Medicaid Rate	0.331	0.219	0.310	0.439	0.333	0.447	0.457
Effect Rel to avg Rate	21.1%	49.3%	26.5%	10.0%	14.7%	16.8%	19.3%
Observations in Stacked Dataset	26,635	5,004	3,484	9,317	15,630	8,934	4,438

Notes: Respondents may report any of the six disability types, which are not mutually exclusive. In all models, the dependent variable is Medicaid receipt, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years *p < 0.10, **p < 0.05, ***p < 0.01.

Table A-9: Impact of Medicaid Expansion on Medicaid Enrollment, Testing Inclusion of Expansion Cohorts

	Expansion Cohorts Included						
	2014 2015 2016	2014	2015	2016	2014 2015	2014 2016	2015 2016
General Population							
DID Estimate	0.026***	0.026***	0.015*	0.056***	0.024***	0.030***	0.026**
SE	(0.006)	(0.007)	(0.007)	(0.013)	(0.007)	(0.007)	(0.009)
P-Value	0.000	0.001	0.054	0.001	0.001	0.000	0.012
Avg. Medicaid Rate	0.096	0.100	0.091	0.094	0.097	0.098	0.093
Effect Rel to Avg. Rate	27.1%	26.0%	16.5%	59.6%	24.7%	30.6%	28.0%
Observations in Stacked Dataset	333,328	170,882	90,243	72,203	261,125	243,085	162,446
Overall Disabled							
DID Estimate	0.070***	0.061**	0.051*	0.143***	0.059**	0.074***	0.082***
SE	(0.025)	(0.030)	(0.028)	(0.025)	(0.026)	(0.027)	(0.026)
P-Value	0.007	0.047	0.081	0.000	0.030	0.008	0.005
Avg. Medicaid Rate	0.331	0.333	0.330	0.328	0.332	0.331	0.329
Effect Rel to Avg. Rate	21.1%	18.3%	15.5%	43.6%	17.8%	22.4%	24.9%
Observations in Stacked Dataset	26,635	13,616	7,202	5,817	20,818	19,433	13,019
Ongoing Disabled							
DID Estimate	0.049	0.032	0.012	0.230***	0.029	0.059	0.076
SE	(0.035)	(0.037)	(0.036)	(0.031)	(0.034)	(0.038)	(0.052)
P-Value	0.169	0.384	0.742	0.000	0.396	0.126	0.157
Avg. SSDI Rate	0.390	0.389	0.395	0.386	0.391	0.388	0.391
Effect Rel. to Avg. Rate	12.6%	8.2%	3.0%	59.6%	7.4%	15.2%	19.4%
Observations in Stacked Dataset	16,024	8,222	4,339	3,463	12,561	11,685	7,802

Notes: In all models, the dependent variable is Medicaid receipt, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A-10: Impact of Medicaid Expansion on SSDI Receipt, Testing Inclusion of Expansion Cohorts

	Expansion Cohorts Included						
	2014 2015 2016	2014	2015	2016	2014 2015	2014 2016	2015 2016
General Population							
DID Estimate	0.008***	0.004	0.013***	0.022***	0.006**	0.006**	0.016***
SE	(0.002)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
P-Value	0.002	0.163	0.001	0.000	0.022	0.019	0.000
Avg. SSDI Rate	0.036	0.036	0.037	0.036	0.036	0.036	0.036
Effect Rel to Avg. Rate	22.2%	11.1%	35.1%	61.1%	16.7%	16.7%	44.4%
Observations in Stacked Dataset	333,332	170,882	90,244	72,206	261,126	243,088	162,450
Overall Disabled							
DID Estimate	0.051***	0.034	0.073***	0.127***	0.041**	0.046**	0.093***
SE	(0.018)	(0.021)	(0.024)	(0.023)	(0.018)	(0.020)	(0.023)
P-Value	0.007	0.109	0.007	0.000	0.027	0.027	0.001
Avg. SSDI Rate	0.281	0.283	0.280	0.278	0.282	0.281	0.279
Effect Rel to Avg. Rate	18.1%	12.0%	26.1%	45.7%	14.5%	16.4%	33.3%
Observations in Stacked Dataset	26,635	13,616	7,202	5,817	20,818	19,433	13,019
Ongoing Disabled							
DID Estimate	0.064**	0.037	0.085**	0.213***	0.047	0.059*	0.124***
SE	(0.029)	(0.031)	(0.032)	(0.026)	(0.029)	(0.032)	(0.037)
P-Value	0.035	0.240	0.016	0.000	0.110	0.076	0.003
Avg. SSDI Rate	0.362	0.366	0.361	0.357	0.364	0.363	0.359
Effect Rel. to Avg. Rate	17.7%	10.1%	23.5%	59.7%	12.9%	16.3%	34.5%
Observations in Stacked Dataset	16,024	8,222	4,339	3,463	12,561	11,685	7,802

Notes: In all models, the dependent variable is SSDI receipt, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A-11: Impact of Medicaid Expansion on SSI Receipt, Testing Inclusion of Expansion Cohorts

	Expansion Cohorts Included						
	2014 2015 2016	2014	2015	2016	2014 2015	2014 2016	2015 2016
General Population							
DID Estimate	0.000	-0.001	-0.001	0.010***	-0.001	0.000	0.002
SE	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
P-Value	0.995	0.793	0.506	0.001	0.699	0.862	0.619
Avg. SSI Rate	0.023	0.022	0.023	0.024	0.022	0.023	0.024
Effect Rel to Avg. Rate	0.0%	-4.5%	4.3%	41.7%	-4.5%	0.0%	8.3%
Observations in Stacked Dataset	333,332	170,882	90,244	72,206	261,126	243,088	162,450
Overall Disabled							
DID Estimate	0.003	-0.008	-0.000	0.087***	-0.006	0.003	0.025
SE	(0.017)	(0.018)	(0.024)	(0.025)	(0.016)	(0.018)	(0.029)
P-Value	0.881	0.645	0.998	0.004	0.726	0.875	0.400
Avg. SSI Rate	0.180	0.173	0.184	0.188	0.177	0.178	0.186
Effect Rel to Avg. Rate	1.7%	-4.6%	-0.0%	46.3%	-3.4%	1.7%	13.4%
Observations in Stacked Dataset	26,635	13,616	7,202	5,817	20,818	19,433	13,019
Ongoing Disabled							
DID Estimate	-0.018	-0.028	-0.052	0.147***	-0.033	-0.009	0.003
SE	(0.026)	(0.025)	(0.036)	(0.032)	(0.023)	(0.028)	(0.055)
P-Value	0.506	0.255	0.170	0.000	0.148	0.764	0.961
Avg. SSI Rate	0.229	0.224	0.234	0.234	0.227	0.227	0.234
Effect Rel. to Avg. Rate	-7.9%	-12.5%	-22.2%	62.8%	-14.5%	-4.0%	1.3%
Observations in Stacked Dataset	16,024	8,222	4,339	3,463	12,561	11,685	7,802

Notes: In all models the dependent variable is SSI receipt on the basis of disability, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A-12: Impact of Medicaid Expansion on Employment, Testing Inclusion of Expansion Cohorts

	Expansion Cohorts Included						
	2014 2015 2016	2014	2015	2016	2014 2015	2014 2016	2015 2016
General Population							
DID Estimate	0.003	-0.004	0.018	0.016	0.003	-0.001	0.016
SE	(0.007)	(0.006)	(0.023)	(0.014)	(0.008)	(0.006)	(0.018)
P-Value	0.645	0.530	0.433	0.289	0.752	0.833	0.379
Avg. Employment Rate	0.709	0.711	0.711	0.702	0.711	0.708	0.707
Effect Rel to Avg. Rate	0.4%	-0.6%	2.5%	2.3%	0.4%	-0.1%	2.3%
Observations in Stacked Dataset	331,994	170,179	89,881	71,934	260,060	242,113	161,815
Overall Disabled							
DID Estimate	-0.020	-0.036**	0.025	-0.055*	-0.020	-0.032*	0.005
SE	(0.016)	(0.016)	(0.032)	(0.028)	(0.016)	(0.016)	(0.032)
P-Value	0.228	0.034	0.438	0.067	0.235	0.051	0.871
Avg. Employment Rate	0.260	0.264	0.261	0.248	0.263	0.259	0.255
Effect Rel to Avg. Rate	-7.7%	-13.6%	9.6%	-22.2%	-7.6%	-12.4%	2.0%
Observations in Stacked Dataset	26,635	13,616	7,202	5,817	20,818	19,433	13,019
Ongoing Disabled							
DID Estimate	-0.022	-0.017	-0.005	-0.159***	-0.012	-0.027	-0.045
SE	(0.025)	(0.022)	(0.034)	(0.041)	(0.021)	(0.028)	(0.049)
P-Value	0.382	0.451	0.886	0.002	0.569	0.344	0.377
Avg. Employment Rate	0.182	0.187	0.180	0.175	0.184	0.184	0.178
Effect Rel. to Avg. Rate	-12.1%	-9.1%	-2.8%	-90.9%	-6.5%	-14.7%	-25.3%
Observations in Stacked Dataset	16,024	8,222	4,339	3,463	12,561	11,685	7,802

Notes: In all models, the dependent variable is individual employment rate for the fifth through eighth months in the CPS sample. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *$p < 0.10$, **$p < 0.05$, ***$p < 0.01$.

Table A-13: Impact of Expansion on Disabled Employment, Alternate Definition of Employment

	General Population	All Disabled	Ongoing Disabled	Ongoing Disabled by Income		Ongoing Disabled by Age		Ongoing Disabled by Education	
				<150% FPL	≥150% FPL	18-49	50-61	HS or Below	Any College
DID Estimate	0.008	-0.016	-0.020	-0.039*	-0.012	-0.017	-0.015	-0.005	-0.035
SE	(0.007)	(0.017)	(0.028)	(0.022)	(0.035)	(0.040)	(0.024)	(0.032)	(0.038)
P-Value	0.318	0.344	0.477	0.080	0.727	0.664	0.523	0.865	0.353
Avg. Employment Rate	0.709	0.206	0.185	0.081	0.260	0.217	0.164	0.139	0.253
Effect Rel. to Avg Rate	1.1%	-6.2%	-10.8%	-48.1%	-4.6%	-7.8%	-9.1%	-3.6%	-13.8%
Observations in Stacked Dataset	331,854	26,635	16,024	6,936	9,088	5,688	10,336	9,419	6,605

Notes: In all models, the dependent variable is employed, as a binary indicator, in March of the second ASEC observation. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. * p < 0.10, ** p < 0.05, *** p < 0.01.

Table A-14: Impact of Expansion on SSDI Receipt, Alternate Definition of Working Age

	General Population	All Disabled	Ongoing Disabled	Ongoing Disabled by Income		Ongoing Disabled by Age		Ongoing Disabled by Education	
				<150% FPL	≥150% FPL	18-49	50-61	HS or Below	Any College
DID Estimate	0.007***	0.057***	0.075**	0.077**	0.063*	0.065	0.073**	0.070	0.070
SE	(0.002)	(0.019)	(0.031)	(0.035)	(0.037)	(0.055)	(0.033)	(0.046)	(0.046)
P-Value	0.003	0.004	0.020	0.035	0.098	0.239	0.032	0.133	0.137
Avg. SSDI Rate	0.033	0.278	0.360	0.372	0.352	0.306	0.405	0.369	0.347
Effect Rel. to Avg Rate	21.2%	20.5%	20.8%	20.7%	17.9%	21.2%	18.0%	19.0%	20.2%
Observations in Stacked Dataset	309,868	22,812	13,647	6,031	7,616	5,688	7,959	8,170	5,477

Notes: In all models, the dependent variable is SSDI, as a binary indicator, in March of the second ASEC observation. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-15: Impact of Medicaid Expansion on SSI Receipt, Tests to Improve SSI Specificity

	General Population	All Disabled	Ongoing Disabled	Ongoing Disabled by Income		Ongoing Disabled by Age		Ongoing Disabled by Education	
				<150% FPL	≥150% FPL	18-49	50-61	HS or Below	Any College
DID Estimate	-0.000	-0.005	-0.023	-0.067	0.022	-0.008	-0.029	-0.005	-0.048**
SE	(0.002)	(0.017)	(0.024)	(0.047)	(0.023)	(0.041)	(0.023)	(0.030)	(0.022)
P-Value	0.999	0.773	0.348	0.160	0.360	0.851	0.213	0.868	0.032
Avg. SSI Rate	0.015	0.128	0.162	0.257	0.095	0.206	0.134	0.208	0.096
Effect Rel. to Avg Rate	0.0%	-3.9%	-14.2%	26.1%	23.2%	-3.9%	-21.6%	-2.4%	-50.0%
Observations in Stacked Dataset	330,563	25,067	14,765	6,318	8,477	5,242	9,523	8,573	6,192

Notes: Respondents who have implausibly high SSI income and who switch from reporting SSDI income in the first ASEC administration to reporting SSI income in the second ASEC administration (i.e. SSDI to SSI switchers) are excluded from analyses. In all models, the dependent variable is SSI receipt on the basis of disability, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), a Bartik shift-share variable, an indicator for 209(b) status, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A-16: Staiger et al. (2023) Data Structure DiD Estimates, SSI Receipt

	Staiger et al. Cohort	Our 2014 Cohort	Our Full Stacked Setup
Staiger et al. Sample	-0.015**	-0.011	-0.012
	(0.006)	(0.008)	(0.008)
6 Question Sequence	-0.015**	-0.013	-0.012
	(0.007)	(0.009)	(0.009)
No Lag added to Sample	-0.013**	-0.004	-0.011
	(0.006)	(0.008)	(0.008)
Year 0 Kept in Sample	-0.011	0.001	-0.006
	(0.007)	(0.008)	(0.007)
Households with income > 90^{th} percentile added	-0.011*	0.000	-0.006
	(0.007)	(0.008)	(0.006)

Notes: Changes to the study sample are listed in the left most column and are cumulative down the rows so the sample becomes progressively more similar to our sample under the Staiger et al. (2023) data structure. In all models, the dependent variable is SSI receipt on the basis of disability, as a binary indicator. Models include covariates determined by the Staiger et al. (2023) model which include race, college education, marital status, state poverty rates, nonmetropolitan residency, and state and calendar year fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the ASEC person level weight. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A-17: Staiger et al. (2023) Data Structure DiD Estimates, SSDI Receipt

	Staiger et al. Cohort	Our 2014 Cohort	Our Full Stacked Setup
Staiger et al. Sample	0.020***	0.030***	0.029***
	(0.006)	(0.008)	(0.009)
6 Question Sequence	0.031***	0.037***	0.033***
	(0.007)	(0.009)	(0.012)
No Lag added to Sample	0.028***	0.026***	0.030***
	(0.006)	(0.007)	(0.010)
Year 0 Kept in Sample	0.019**	0.016*	0.023***
	(0.007)	(0.009)	(0.008)
Household's with income > 90^{th} percentile added	0.019***	0.017*	0.023***
	(0.007)	(0.008)	(0.007)

Notes: Changes to the study sample are listed in the left most column and are cumulative down the rows so the sample becomes progressively more similar to our sample under the Staiger et al. (2023) data structure. In all models, the dependent variable is SSDI receipt on the basis of disability, as a binary indicator. Models include covariates determined by the Staiger et al. (2023) model which include race, college education, marital status, state poverty rates, non-metropolitan residency, and state and calendar year fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the ASEC person level weight. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A-18: Our Data Structure DiD Estimates, SSI Receipt

	Staiger et. al. Cohort	Our 2014 Cohort	Our Full Stacked Setup
Our Sample	-0.016	-0.008	0.003
	(0.011)	(0.018)	(0.017)
7 Question Sequence	-0.018	-0.015	-0.012
	(0.011)	(0.017)	(0.016)
Sample lagged by 1 year	-0.008	-0.029*	-0.025
	(0.012)	(0.015)	(0.015)
Year 0 Removed from Sample	-0.008	-0.033**	-0.027
	(0.011)	(0.014)	(0.018)
Households with income > 90^{th} percentile removed	-0.011	-0.033**	-0.027
	(0.011)	(0.015)	(0.018)

Notes: Changes to the study sample are listed in the left most column and are cumulative down the rows so the sample becomes progressively more similar to the Staiger et al. (2023) sample under our data structure. In all models, the dependent variable is SSI receipt on the basis of disability, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *p < 0.10, **p < 0.05, ***p < 0.01

Table A-19: Our Data Structure DiD Estimates, SSDI Receipt

	Staiger et al. Cohort	Our 2014 Cohort	Our Full Stacked Setup
Our Sample	0.020	0.034	0.051***
	(0.019)	(0.021)	(0.018)
7 Question Sequence	0.009	0.015	0.040**
	(0.013)	(0.018)	(0.017)
Sample lagged by 1 year	0.020	0.042**	0.050***
	(0.014)	(0.020)	(0.016)
Year 0 Removed from Sample	0.020	0.045**	0.061***
	(0.014)	(0.020)	(0.020)
Households with income > 90^{th}	0.018	0.041*	0.058***
percentile removed	(0.014)	(0.020)	(0.021)

Notes: Changes to the study sample are listed in the left most column and are cumulative down the rows so the sample becomes progressively more similar to the Staiger et al. (2023) sample under our data structure. In all models, the dependent variable is SSDI receipt on the basis of disability, as a binary indicator. Models include demographic covariates (sex, race, age group, college education, prior income level), Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. Standard errors are clustered on the state-level. Observations are weighted using the CPS longitudinal weight for two adjacent years. *p < 0.10, **p < 0.05, ***p < 0.01

Table A-20: Staiger et al. (2023) Covariate Comparisons DiD Estimates, SSI Receipt

	Staiger et al. Cohort	Our 2014 Cohort	Our Full Stacked Setup
Staiger et al. Data Structure			
Staiger et al. Covariates	-0.015**	-0.011	-0.012
	(0.006)	(0.008)	(0.008)
Staiger et al. Covariates + Income	-0.014**	-0.009	-0.010
	(0.006)	(0.007)	(0.008)
Staiger et al. Covariates + Income + Bartik	-0.011*	-0.009	-0.013
	(0.006)	(0.010)	(0.010)
Our Covariates	-0.010	-0.007	-0.011
	(0.006)	(0.010)	(0.010)
Our Data Structure			
Staiger et al. Covariates	-0.026*	-0.019	-0.011
	(0.013)	(0.017)	(0.016)
Staiger et al. Covariates + Income	-0.025*	-0.020	-0.010
	(0.013)	(0.017)	(0.016)
Staiger et al. Covariates + Income + Bartik	-0.019	-0.009	0.004
	(0.012)	(0.018)	(0.017)
Our Covariates	-0.016	-0.008	0.003
	(0.011)	(0.018)	(0.017)

Notes: The Staiger et al. (2023) covariates include race, college education, marital status, state poverty rates, non-metropolitan residency, and state and calendar year fixed effects. Our covariates include sex, race, age group, college education, prior income level, Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. The covariate for prior income level requires the longitudinal data structure exploited in our study. However, when applied to the Staiger et al. (2023) study this covariate becomes present income level, rather than prior income level. All models have dependent variable of SSI receipt on the basis of disability, as a binary indicator. Standard errors are clustered on the state-level. Observations are weighted using the person level ASEC weight under Staiger et al. (2023) data structure and the CPS longitudinal weight for two adjacent years under our data structure. *p < 0.10, **p < 0.05, ***p < 0.01.

Table A-21: Staiger et al. (2023) Covariate Comparisons DiD Estimates, SSDI Receipt

	Staiger et al. Cohort	Our 2014 Cohort	Our Full Stacked Setup
Staiger et al. Data Structure			
Staiger et al. Covariates	0.020***	0.030***	0.029***
	(0.006)	(0.008)	(0.009)
Staiger et al. Covariates + Income	0.020***	0.031***	0.029***
	(0.006)	(0.008)	(0.009)
Staiger et al. Covariates + Income + Bartik	0.023***	0.034***	0.036***
	(0.008)	(0.010)	(0.010)
Our Covariates	0.025***	0.039***	0.041***
	(0.009)	(0.011)	(0.011)
Our Data Structure			
Staiger et al. Covariates	0.024	0.029	0.050**
	(0.017)	(0.023)	(0.022)
Staiger et al. Covariates + Income	0.025	0.029	0.051**
	(0.017)	(0.022)	(0.021)
Staiger et al. Covariates + Income + Bartik	0.019	0.034	0.050***
	(0.018)	(0.022)	(0.018)
Our Covariates	0.020	0.034	0.051***
	(0.019)	(0.021)	(0.018)

Notes: The Staiger et al. (2023) covariates include race, college education, marital status, state poverty rates, non-metropolitan residency, and state and calendar year fixed effects. Our covariates include sex, race, age group, college education, prior income level, Bartik shift-share variable, state by experiment fixed effects, and calendar year by experiment fixed effects. The covariate for prior income level requires the longitudinal data structure exploited in our study. However, when applied to the Staiger et al. (2023) study this covariate becomes present income level, rather than prior income level. All models have dependent variable of SSDI receipt on the basis of disability, as a binary indicator. Standard errors are clustered on the state-level. Observations are weighted using the person level ASEC weight under Staiger et al. (2023) data structure and the CPS longitudinal weight for two adjacent years under our data structure. *p < 0.10, **p < 0.05, ***p < 0.01.