Disability Heterogeneity in the Impact of the ACA's Medicaid Expansions on Disability Employment

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Abstract

Objectives To test for heterogeneous treatment effects in the impact of the Affordable Care Act's (ACA) Medicaid expansion on the employment of people with disabilities.
Methods Using difference-in-difference approaches, we estimate the impact of the ACA's Medicaid expansion on employment outcomes for various subgroups of people with disabilities. Using the Current Population Survey (CPS) from June 2008 to December 2019, we segment the disabled population by disability type, disability recency and labor force attachment, leveraging the longitudinal aspect of the CPS.
Results Among persons with higher labor force attachment, we find that Medicaid expansion reduced the employment rate of persons with new disabilities by a statistically significant -3.2%, while there was a precisely estimated null effect for persons with ongoing disabilities. Among those with lower labor attachment, we find suggestive evidence of offsetting treatment effects among persons with new versus ongoing disabilities. Medicaid expansion increased the employment rate of persons with ongoing disabilities by 10.5% but decreased the employment rate of persons with new disabilities by -9.2%. However, these latter estimates for persons with lower labor force attachment are imprecisely estimated, limiting the conclusions that can be drawn from them.
Conclusions Existing literature on the disability employment effects of Medicaid expansion is mixed in part due to different study designs picking up different effects on distinct groups of people with disabilities. We show that accounting for disability heterogeneity allows for more precise estimates of policy impacts for some populations while providing suggestive evidence of countervailing treatment effects for others.

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A large literature has examined the impact of the Affordable Care Act (ACA)'s Medicaid expansion on a variety of policy-relevant outcomes (Mazurenko et al., 2018). However, the existing literature on the impact of Medicaid expansion on the employment of people with disabilities is decisively mixed. Hall, Shartzer, Kurth, & Thomas (2017; 2018) found that the ACA's Medicaid expansion significantly improved employment outcomes for people with disabilities in expansion states relative to people in states that did not expand Medicaid. In contrast, Sevak & Hyde (2021) found no evidence of a change in employment trends for people with disabilities in expanding states as compared to non-expanding states.

Research on participation in disability income support programs (receipt of which requires an inability to engage in any substantial gainful activity, thus precluding employment above a certain threshold) also points in different directions. Burns & Dague (2017) found that pre-ACA Medicaid expansions reduced Supplemental Security Income (SSI) participation by 7%. Maestas, Mullen, & Strand (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, Burns, Dague, & Simon (2017) found a 3.3% decrease in SSI participation when evaluating the early ACA Medicaid expansions. In contrast, Anand, Hyde, Colby, & O'Leary (2018) found suggestive evidence of increased SSI applications in expansion states relative to non-expansion states, though their results were inconclusive for SSDI due to concerns about differing pre-expansion trends for expansion and non-expansion states. Schmidt, Shore-Sheppard, & Watson (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.

Those seeking to understand these different findings often point to the heterogeneous constructs researchers choose to examine. For example, some studies have made use of the American Community Survey (ACS) to measure employment outcomes for people with disabilities, while others have used other survey data sources, such as the Urban Institute's Health Reform Monitoring Survey (HRMS) (e.g., Hall et al., 2017, 2018). 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 (question phrasing in appendix), 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, Mullen, & Rennane, 2019; Burkhauser, Houtenville, & Tennant, 2014).

Similarly, in the realm of disability program participation, researchers have studied both application and participation rates, which may reflect different phenomena both because not every applicant is successful and because the former do not incorporate rates of program exit. If Medicaid expansion impacts those with more recent or less severe disabilities differently than those with longstanding or more severe disabilities, application and participation trends may be impacted in different directions by the same policy change.

It is possible that different study designs may pick up different signals reflecting Medicaid expansion's distinct impacts on different groups of people with disabilities. In this paper, we examine the impact of the ACA's Medicaid expansion on different populations of people with disabilities, with the purpose of assessing whether disability heterogeneity may help explain the divergent findings in prior work. To do so, we rely on a different data source altogether: the Current Population Survey (CPS). Like the ACS, the CPS uses a six-question sequence to identify people with disabilities. Unlike the ACS, the CPS has a longitudinal component that asks about respondents' disability status on two occasions that are usually a year apart from each other.[1]

Prior work has established that responses to the disability questions frequently change between the first and second times they are asked (Ward et al., 2017). Field surveys relying on the same 6-question 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), suggesting that different patterns of responses on the repeated disability

[1] Households are asked the disability questions when they first enter the sample and then when they return from the break in sampling. For households that are present in both the first and fifth months of sampling, the two responses to the disability questions would always be 1 year apart. But, lack of household response can result in the two responses being under 1 year apart (disability questions asked in months 3 and 5 of sampling would be 10 months apart) or over 1 year apart (disability questions asked in months 1 and 7 of sampling would be 14 months apart).

questions asked by the CPS identify people with disabilities in distinct life circumstances (Myers et al, 2020). Each wave of the CPS also collects information on respondents’ current employment status. Thus, the CPS provides us with an opportunity to subset the disabled population along two dimensions: a) whether a person with a disability has a new or an ongoing disability, and b) whether a person with a disability has higher or lower labor force attachment.

There are strong theoretical reasons to believe that Medicaid expansion may have different impacts on different parts of the disability community. An increase in employment for individuals with ongoing disabilities might be consistent with a change in work incentives brought about by Medicaid expansion. In all but ten states, SSI – which serves persons with no or minimal prior work history and is thus primarily focused on those with longstanding disabilities - comes with categorical eligibility for Medicaid, making enrollment in income support programs an important eligibility pathway for accessing public insurance (Rupp & Riley, 2016). By offering an alternative pathway, Medicaid expansion may reduce the incentive for people with longstanding disabilities to apply for income support programs (and exit the labor force altogether). It may also make it more attractive for those already enrolled in income support programs to re-enter the labor force by offering an alternative pathway into public insurance benefits. For this latter group, Section 1619(b) already permits existing SSI recipients to retain Medicaid eligibility well above 138% FPL, but relatively few SSI recipients are aware of the existence of this program even among the sizable subset of recipients who desire to participate in the workforce (Livermore & Prenovitz, 2009). It is likely that recipients would be more aware of their states’ choice to expand Medicaid. Conversely, a decrease in employment for individuals with new disabilities could represent a variation on the “job lock” mechanism, whereby individuals retain their attachment to the labor force largely due to their need for employer-sponsored insurance (ESI), and under which expansions in non-ESI options may result in reduced labor supply. Medicaid expansion might incentivize individuals who have lower labor force attachment to exit the labor force altogether when they become newly disabled rather than seek new employment for purposes of acquiring health insurance benefits. Alternatively, the act of exiting the labor force for health-related reasons may change a person's self-identification as a person with a disability.

Methods

Data Sources

The Current Population Survey (CPS) is a monthly household survey administered by the US Census Bureau whose sample is nationally representative of the civilian, non-institutional population. Though its primary goal is to measure employment status, the CPS also collects social and demographic information about the US population. The CPS began asking respondents a sequence of questions about disability status in June 2008. Respondents are considered to have a disability if they report having any of six disability types: i) hearing disability, ii) vision disability, iii) cognitive disability, iv) physical disability, v) self-care disability, and vi) independent living disability.

Respondent households are included in the 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 after the eight-month hiatus, enabling observation of self-reported changes in disability status one year after a respondents' first inquiry. Approximately three-fourths of the CPS sample eligible for resurvey a year later are retained from one year to the next (Rivera Drew, Flood, & Warren, 2014). Data on current employment status is collected in every wave. We illustrate the CPS's longitudinal data collection process with respect to our variables of interest in Figure 1.

We use data from the 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 required of expanding states as of 2014. We supplement our Medicaid expansion data with early expansion data from Schmidt, Shore-Sheppard, & Watson (2020). We consider states to be *Medicaid expanders* starting the first year that coverage is expanded, even if the eligibility limit is below 138% FPL. We check the data on early expanders in Schmidt, Shore-Sheppard, & Watson (2020) against other literature and information from state waiver applications available on the CMS website. In doing so, we confirm the timing of early expansions in DC and Connecticut (Medicaid State Plan Amendments, 2022). However, we chose to drop Delaware, Massachusetts, New York and Vermont from our specification, as they had robust early expansions prior to the passage of the ACA (Denham & Veazie, 2019). We also code Arizona as expanding in 2014, rather than 2010 as it is in Schmidt, Shore-Sheppard, & Watson (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 (Vitalyst Health, 2017). We also test a version of our primary specification including Delaware, Massachusetts, New York and Vermont as 2014 expanders.

Sample

We utilize CPS data for respondents ages 18-64 who are in their fifth through eighth sampling months between June 2008 and December 2019 (n= 4,881,109). Unlike prior work, we do not analyze employment outcomes for the disabled population as a whole. Instead, we vary our sample to specific groups of people with disabilities in order to explore the possibility of heterogeneous treatment effects, making use of the longitudinal nature of the CPS to divide the disabled population into different groups. The longitudinal nature of the CPS is somewhat underutilized, but it represents an extremely valuable source of information for characterizing within-person change over time. We build on descriptive work from Ward, Myers, Wong, & Ravesloot (2017) and Sage, Ward, Myers, & Ravesloot (2019) who first proposed the use of the longitudinal nature of the CPS to distinguish between persons with new and ongoing disabilities. Ameri, Ali, Schur, & Kruse (2019) also make use of the longitudinal nature of the CPS to note changes in disability status in their descriptive study of the relationship between unionization and employment outcomes for people with disabilities. We expand on their work by also subdividing the disabled population by employment status one year previously, used here as a measure of labor force attachment. To our knowledge our study is the first to use these subgroup definitions with quasi-experimental methods to estimate heterogeneous treatment effects of a policy change on different parts of the disability community.

In our first set of analyses, we focus on the subset of respondents who report a disability and who were interviewed twelve months prior (i.e., in the first through fourth waves of sampling). We then create measures of disability recency and labor force attachment (defined via respondent's employment status one year prior). Our disability recency measures are constructed from individuals' first and second set of responses to the disability sequence which typically take place in the first and fifth wave of the CPS. Respondents who report a disability when asked the second time but did not report a disability a year prior when first asked are classified as *newly disabled* (n=134,598), whereas respondents who report having a disability when asked both times are classified as *ongoing disabled* (n=200,679). Figure 1 shows the timing of the disability questions within the CPS's longitudinal data collection scheme and relative to the monthly labor supply questions.

A respondent's labor force attachment is based on their employment status at their interview twelve months prior (e.g., for a respondent in their seventh month in sample, we use their employment status during the third month in sample). We classify respondents as having *higher labor force attachment* (n=107,215) if they reported any form of employment (full-time or part-time) in the prior year; those who were either previously unemployed or out of the labor force are classified as having *lower labor force attachment* (n=228,062). We then subset the CPS data to create four subsamples: *ongoing disabled & higher labor force attachment* (n=42,258), *ongoing disabled & lower labor force attachment* (n=158,421), *newly disabled & higher labor force attachment* (n=64,957), and *newly disabled & lower labor force attachment* (n=69,641).

As individuals are surveyed about their employment status in each successive month, a single individual can contribute up to four sample observations if they are in sample for all of their fifth through eighth waves of the CPS.

Table 1 reports descriptive statistics for each of these population subgroups. Persons with ongoing disabilities and higher labor force attachment make up 11.8% of the disabled population and have the highest employment rate (averaged over our study period) at 78.6%. In contrast, persons with ongoing disabilities and lower labor force attachment make up the largest portion of the disabled population at 47.7% but had the lowest average employment rate at 3.7%. For persons with new disabilities, those with higher labor force attachment make up 18.9% of the disabled population while having an average employment rate of 72.1%, while those with lower labor force attachment make up 21.6% of the disabled population and have an average employment rate of 8.4%. For all four populations of people with disabilities, most respondents were between the ages of 50-64. Respondents with lower labor force attachment were more likely to be non-White or Hispanic and were less likely to have a Bachelor's degree.

Our second set of analyses restricts the sample of people with disabilities by disability type. We subset to respondents ages 18-64 in months five through eight of sampling who report having each one of the six specific disability types, respectively. Since this analysis does not use the longitudinal aspect of the CPS, there are no inclusion criteria based on respondent's prior response records. Additionally, respondents may be included in more than one of the six subsamples if they report having more than one disability type. Sample sizes for each disability category are provided in the appendix.

Table 2 reports descriptive statistics by disability type. As respondents can indicate they have more than one disability, the proportion of the disabled sample each group represents sums up to more than 100%. The majority of the disabled sample – 56% of respondents – have a physical disability, with the next largest groups being persons with cognitive disabilities (37.7%), independent living disabilities (34.9%), hearing disabilities (17.7%), self-care disabilities (17.0%) and vision disabilities (12.6%). Employment rates are highest for persons with hearing disabilities (47.5%) and lowest for persons with self-care disabilities (10.0%). The majority of the disabled sample is in the 50-64 age range for all disability groups except persons with cognitive disabilities, for whom only 44.5% of respondents are in the 50-64 age range category and who possess the highest proportion of respondents in the 18-34 range (28.1%).

Study Variables

We collapse the CPS microdata to the state-year level, using person-level weights, to calculate the percentage of the population who are employed in each subsample of interest (employment-to-population ratio). We include demographic covariates to control for compositional change over time in the samples. In particular, we include sex, race (white and Non-Hispanic, Black and Non-Hispanic, Hispanic, or other), age group (18-34, 35-49, 50-64) and educational attainment (Bachelor’s degree or no Bachelor’s degree). We also include state-year-level “Bartik shift-share” variables to control for state-level changes in labor demand (independent of changes in labor supply), since any health effects caused by insurance expansions might increase labor supply even if labor demand were unchanged. Constructed from American Community Survey (ACS) data, the shift-share variables predict state-level employment growth rates by multiplying state industry shares in 2008 by *national* industry growth rates between 2008 and any given sample year, where the state of interest is excluded from the growth rate calculation (Goldsmith-Pinkham, Sorkin, & Swift, 2020).

Statistical Analyses

We use a stacked difference-in-differences (DID) model to address the staggered expansion of Medicaid eligibility across states. We create sub-experiments for each of the six expansion cohorts (2010, 2011, 2012, 2014, 2015, 2016). There is no 2013 expansion cohort. We exclude the 2019 expansion cohort (Maine and Virginia) from our analyses, as there is no post-expansion period in our time frame, and we exclude 2020 onwards in order to avoid confounding effects of the COVID-19 public health emergency. The "stacked" DID approach is adapted from Deshpande and Li (2019) and is intended to address potential biases identified by Goodman-Bacon (2021). By classifying each Medicaid expansion cohort as a sub-experiment and including experiment fixed effects, we ensure that comparisons are only made between newly treated and not-yet or never-treated states, avoiding the confounding effects of using already-treated states as controls. Each sub-experiment dataset is restricted to within five years of the expansion year. For the expansion cohort in year *t*, states that expand Medicaid prior to year *t* are dropped from the sub-experiment dataset and any state that has not expanded Medicaid by year *t* is treated as a control. The sub-experiment datasets are appended, and a standard difference-in-difference model is fitted to the "stacked" data using the following specification:

$$ln(y_{s,t,e}) = \beta_0 + \beta_1 M_{s,e} + \beta_2 P_{t,e} + \beta_3 (M_{s,e} \times P_{t,e}) + \alpha_s + \gamma_t + \delta_e + \theta' X_{s,t} + \epsilon_{s,t,e} \quad (1)$$

We log-transform the dependent variable, $\boldsymbol{y_{s,t,e}}$, the employment-to-population ratio in state *s* in year *t* in sub-experiment *e*, so that the coefficients approximate the percent change in the employment-to-population ratio. This facilitates comparison of coefficients across subsamples with very different baseline employment levels. (We also report results using an unlogged dependent variable in the Appendix.) $\boldsymbol{M_{s,e}}$ is an indicator that state *s* is an expansion state for sub-experiment *e*, and $\boldsymbol{P_{t,e}}$ is an indicator that year *t* is in the post-expansion period for sub-experiment *e*. Our coefficient of interest is the DID estimator, $\boldsymbol{\beta_3}$, which is the average effect of Medicaid expansion on the employment rate in expansion states compared to non-expansion states. We include state ($\boldsymbol{\alpha_s}$), year ($\boldsymbol{\gamma_t}$), and sub-experiment ($\boldsymbol{\delta_e}$) fixed effects and a set of covariates consisting of demographic characteristics and our Bartik shift-share variable ($\boldsymbol{X_{s,t}}$) for state *s* in year *t*. All models cluster standard errors at the state-level.

We supplement our stacked DID models with a second empirical approach: the event study, to diagnosis whether the assumption of parallel trends (a requirement for the validity of the DID estimator) between expansion and non-expansion states holds. We utilize the Sun & Abraham (2021) interaction-weighted estimator for the event study [*eventstudyinteract* command in Stata, Version 16.1] in order to avoid bias from time-varying treatment effects across cohorts. Once again, we restrict the study window to five lead and five lag periods relative to the state's Medicaid expansion. Such restriction is particularly necessary under the Sun & Abraham estimator to limit the impact of distant time periods as the coefficients for each lead/lag are influenced by the set of leads and lags included. Consistent with our approach to the stacked DID, we exclude the 2019 expansion cohort, include state and year fixed effects and the full set of demographic and Bartik shift-share covariates in our models and cluster all standard errors at the state-level. Though we design the event study with as similar a specification to our stacked DID model as possible, the two estimation strategies measure slightly different things and are thus likely to deliver slightly different results. The lead and lag coefficients of the event study are relative to the year prior Medicaid expansion (t = -1), while the stacked DID estimate captures the totality of the difference between treated and control units in their respective changes between the post-expansion period compared to the pre-expansion period. Additionally, the Sun & Abraham estimator uses states who never expand Medicaid as the control group, whereas the stacked DID sub-experiments use as control groups both states that never expand and states that have yet to expand in any given cohort. As such, we incorporate the event study solely for the purpose of diagnosing whether the parallel trends assumption holds and do not use it to estimate causal impact.

Results

We find evidence of heterogeneous treatment effects in Medicaid expansion's impact on the employment of people with disabilities. Table 3 presents the regression results from the stacked DID models for the subsamples defined by disability recency and labor force attachment. We find a relatively precisely estimated null effect of -0.5% (p=0.708) for persons with ongoing disabilities and higher labor force attachment, for whom a 95% confidence interval rules out increases in employment greater than 2.2% or decreases larger than -3.1%. In contrast, the employment rate of persons with higher labor force attachment who experienced new disabilities declined by -3.2% (p= 0.048) as a result of Medicaid expansion, with a 95% confidence interval ruling out any employment increase for this population and placing a range on the size of the decrease from 0.0% to -6.3%.

For persons with lower labor force attachment, we find suggestive evidence of opposing effects of Medicaid expansion by disability recency, though these are estimated less precisely and do not meet conventional significance levels. The employment of persons with lower labor force attachment and ongoing disabilities rose by 10.5% (p=0.140) due to Medicaid expansion, while persons with lower labor attachment and new disabilities experienced a decrease in employment of -9.2% (p=0.327). The 95% confidence interval for low-attachment persons with ongoing disabilities ranges from 24.6% to -3.6%, and for low-attachment persons with new disabilities from a 9.5% to -27.8%. As indicated, estimates for persons with stronger labor force attachment are more precisely estimated than estimates for persons without lower labor force attachment, likely owing to the larger size of the former population.

While estimates for persons with lower labor force attachment are suggestive of meaningful treatment effects in opposite directions, they must be approached with caution given their lack of precision. In contrast, the comparably precise estimates for persons with higher labor force attachment provide more compelling evidence of Medicaid expansion having a significant negative effect on the employment of such persons with new disabilities and a compelling null effect on such persons with ongoing disabilities. Event study estimates shown Appendix C offer evidence in support of the “parallel trends” assumption in all subgroups of interest (one of the necessary preconditions for the validity of the DID study design).

We find null results when our subsamples were defined by disability type. The disability type stacked DID results are shown in Table 4 and corresponding event study plots are displayed in

Appendix C. No effect sizes are statistically significant at the p<0.05 level. Our event study specifications support the parallel trends assumption.

In light of the evidence of heterogeneous treatment effects in Table 3, we illustrate the value of examining treatment effects with disability subsamples by showing estimates for broader groups in Table 5. Rather than dividing respondents with disabilities into four subsamples along both dimensions of longitudinal disability status and labor force attachment as we did in Table 3, we test including the pooled disability sample (*any disability*) as well as subsetting by only one of the longitudinal variables at a time, resulting in subsamples by either disability recency (*newly disabled* or *ongoing disabled*) or labor force attachment (*disabled and higher labor force attachment* & *disabled and lower labor force attachment).*

The results of Table 5's pooled analyses alongside those in Table 3 give insight into the detectability of different treatment effects when accounting for one as opposed to two dimensions of disability heterogeneity. In the pooled disability sample, we find a statistically insignificant negative effect of 1.2% (p=0.563). But for respondents with ongoing disabilities, we find a statistically insignificant positive effect of 3.2% (p=0.279), whereas for respondents with new disabilities, we find a statistically insignificant negative effect of -3.8% (p=0.121). The opposite direction of these two effects is substantively interesting, despite their imprecision, and obscured in the pooled disability sample because they offset one another.

For disabled respondents with higher labor force attachment, we find a statistically significant negative impact of -2.3% (p=0.04), likely driven by such respondents with new disabilities for whom a statistically significant result was also reported in Table 3. For disabled respondents with lower labor force attachment, we find a statistically insignificant negative impact of -0.6% (p=0.919). This coefficient is not precisely estimated as the opposing treatment effects of the newly and ongoing disabled groups in Table 3 largely cancel each other out. We report event study specifications for these subsamples in Appendix C.

Robustness Checks

We conduct a series of robustness checks to assess the degree to which our findings persist under alternative specifications. Specifically, we test how pre-ACA expansion states, early expanding states (i.e., those that expand post-ACA but pre-2014), and states expanding in 2019 are treated in the model.

As articulated in the methods section, our main specifications exclude the four states that had long standing Medicaid expansion programs before the passage of the ACA in 2010 (Delaware, Massachusetts, New York, Vermont). We estimate a version of our models where these states are coded as expanding in 2014, when these programs then conformed to the 138% FPL threshold and other applicable federal requirements. Next, we run a version of the stacked DID and event study models where the early expanding cohorts—states that expanded in 2010, 2011, and 2012—are dropped from our analyses, since their Medicaid expansions were often substantially less expansive than the 138% FPL required as of 2014. We also run a version where early expanding states are coded as 2014 expanders (similar to our alternative specification for the four pre-ACA expanding states), once their Medicaid programs were subject to the same eligibility criteria as any later expanding state. Lastly, we evaluate the treatment of the 2019 expansion cohort by running a set of models where the 2019 expansion cohort is included, resulting in an additional expansion cohort.

Ultimately, our robustness checks are substantively similar to our primary results in all of the above instances. Interestingly, the positive employment effect for those with lower labor force attachment and ongoing disabilities increases in magnitude to approximately a 20% increase and becomes statistically significant at the $p<0.05$ level in both models that completely exclude the early expansion cohorts (2010, 2011, 2012). This is unsurprising, as the early expansions were typically to a lower threshold than the 138% FPL required from 2014 onwards, suggesting that including these earlier cohorts diluted the treatment effect by introducing partially treated units. As a result, this robustness check strengthens the suggestive evidence we report in our primary results that Medicaid expansion had a meaningful positive effect on the employment of persons with lower labor force attachment and ongoing disabilities.

Discussion

People with disabilities are a broad category whose commonalities nonetheless mask considerable differences. Not only do persons with different types of functional impairment have different labor market experiences, but the recency of a person's disability and their degree of labor force attachment may play an important role in shaping how they respond to the change in labor market incentives brought about by Medicaid expansion. The disability experience of a person who has recently acquired a mobility impairment in a car accident and must reorganize their career accordingly is likely distinct from the experience of someone born with cerebral palsy for whom access barriers and societal bias have been present throughout their lifetime.

We show that Medicaid expansion had no effect on the employment of persons with greater labor force attachment and ongoing disabilities, reinforcing prior work by Sevak & Hyde (2021) and providing new evidence that the null effect is most relevant to this particular group of people with disabilities. We also demonstrate a significant negative effect of Medicaid expansion on the employment of persons with strong labor force attachment who acquire new disabilities, illustrating the importance of accounting for disability heterogeneity in such analyses.

Finally, we show suggestive evidence that Medicaid expansion may have opposing effects for persons with lower labor force attachment. Among this group, Medicaid expansion may have increased the employment of persons with ongoing disabilities by 10.5% but decreased the employment of persons with new disabilities by 9.2%, although neither effect was statistically significant. In pooled analyses that do not account for the heterogeneity of the disabled population, we are unable to detect many of these signals as treatment effects for some subgroups attenuate or cancel out those of others. This may explain the conflicted state of the existing literature on Medicaid expansion and disability employment, with different study designs and data choices picking up distinct signals pointing in different directions.

Our findings of negative effects for persons with new disabilities offers an important new contribution to another mixed literature: that of the effect of changes to public insurance eligibility on “job lock” – a phenomenon whereby workers remain in employment primarily to retain health insurance benefits when they might otherwise change jobs or drop out of the labor force altogether. Prior work on the impact of the ACA’s Medicaid expansion on employment has generally not found evidence of job lock. Gooptu, Moriya, Simon, & Sommers (2016) find that the ACA's Medicaid expansion did not result in significant employment effects in its first year of implementation. Kaestner, Garrett, Chen, Gangopadhyaya, & Fleming (2017) examine the ACA's Medicaid expansion over the course of its first two and a half years and find no evidence of a reduction in employment, with point estimates indicating a statistically insignificant increase instead. Leung & Mas (2018) find that the ACA's Medicaid expansion had no significant effect on the employment of childless adults. Similarly, Kandilov & Kandilov (2022) find that Medicaid expansion had no impact on the labor supply of agricultural workers, despite their experiencing a 12-percentage point (24%) increase in the likelihood of having health insurance.

These null findings contradict some prior work dating from before the ACA. Garthwaite, Gross, & Notowidigdo (2014) find that a pre-ACA elimination of Medicaid coverage for childless adults in Tennessee resulted in increases in employment consistent with a "job lock" mechanism with effects concentrated among individuals in less-than-excellent health. Similarly, Dague, DeLeire, & Leininger (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, Finkelstein, Song, & Taubman, 2014). In short, much like the literature on Medicaid expansion and disability employment, the literature on job lock is very much mixed.

Our findings are the first to show that the ACA's Medicaid expansion released individuals from job lock. However, this finding is specific to a particular population: persons with newly acquired disabilities, a group for whom early retirement while retaining access to health insurance may be particularly valuable. The resulting negative effects on employment may not be detectable in study designs focused on the general population. Due to the potential existence of countervailing positive effects for individuals with ongoing disabilities, they may even be hard to detect in study designs focused on people with disabilities. However, by accounting for disability heterogeneity via exploiting the longitudinal nature of the CPS, we confirm that Medicaid expansion released persons with higher labor force attachment and new disabilities from job lock and provide suggestive evidence of such an effect for persons with lower labor force attachment and new disabilities. This is precisely the group theory predicts would be most responsive to the introduction of an affordable health insurance option that is not tied to one's employment.

The broad range of the confidence intervals for the estimates for individuals with lower labor force attachment suggests that a state-year DID approach may be insufficiently powered to detect the impact of Medicaid expansion. If so, one potential solution would be to examine employment variation in sub-state units (though this may also raise power concerns when working with disability subgroups depending on the data source), as Schmidt, Shore-Sheppard, & Watson (2020) and Sevak & Hyde (2021) both do in their studies on Medicaid expansion's impact on this population. Unfortunately, existing data limitations make this approach infeasible when seeking to explore disability subgroups defined by longitudinal data. Unlike the ACS, the CPS is not designed for and lacks sufficient sample size to deliver consistently reliable estimates on the county level. However, unlike the CPS, the ACS lacks a longitudinal component to its data collection, rendering it impossible to observe changes in disability or employment status within the same individual over time. While both surveys rely on the same 6-question to define disability, the longitudinal nature of the CPS permits a far more nuanced understanding of disability subgroups even as its smaller sample size renders it less useful than the ACS for quasi-experimental work that relies on geographic units smaller than a state.

Our study design offers a promising new dimension through which to examine disability heterogeneity in future work. Making use of the longitudinal nature of the CPS opens new horizons for disability policy researchers seeking to examine the diverse nature of the disability community and evaluate the extent to which public policies impact different groups of people with disabilities in different ways. Existing research practices have largely approached disability as a binary, condensing considerable variation within the disability community into a single indicator variable that fails to capture the complexity and nuances of different disability experiences. While the benefits of subsetting to smaller disability subgroups must be balanced against power considerations, researchers might consider making use of this method for examining disability heterogeneity in both descriptive and quasi-experimental work.

However, the CPS's limitations point to the need to introduce additional measures of disability heterogeneity in other forms of disability data collection. Querying respondents on the timing of disability onset could represent a useful addition to the existing 6-question disability sequence within the ACS and other forms of non-longitudinal disability data collection. Similarly, researchers might consider deploying field surveys to better understand how the 6-question disability sequence and other existing survey questions on respondent demographics correspond to specific clinical diagnoses in order to identify the nature of the population captured by current disability data collection efforts. For example, Kruse, Park, van der Meulen Rodgers, & Schur (2022) make use of Behavioral Risk Factor Surveillance System (BRFSS) data to build a model to impute cancer status in the CPS using the 6-question disability sequence alongside gender, age, race/ethnicity, and employment status. Future work might consider further leveraging of disability questions alongside non-disability demographic covariates to impute more granular forms of disability status than are collected through direct measures. Similarly, machine learning techniques may be useful for better understanding the distinct clusters that occur in existing disability data. Such analyses could heavily inform future revisions to existing disability data collection efforts. However, the extent to which imputation strategies relying on existing data are sufficient for use in quasi-experimental studies regarding disability subgroups remains to be seen.

Researchers are often well advised to account for disability heterogeneity in their analyses, though this must be balanced against power considerations. By leveraging the longitudinal nature of the CPS to segment the disabled population by disability recency and prior employment status, we have articulated a promising option to do just this in future work. In addition, we offer evidence that Medicaid expansion has heterogeneous treatment effects on the employment of different groups of people with disabilities, helping to explain inconsistencies in prior work on this topic.

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Figure 1: CPS Interview Schedule for a Given Rotation Group

Month	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
Month in Sample	1st	2nd	3rd	4th									5th	6th	7th	8th
Employment Status Question	X	X	X	X									X	X	X	X
Disability Status Questions	X												X			

Note: Households are asked about employment status every month that they are interviewed. The six-question disability sequence is only asked when households first enter the sample (typically 1st month in sample) and when they return from the break in interviewing (typically 5th month in sample).

Table 1. Descriptive Statistics for Disability Recency and Labor Force Attachment Groups

	Ongoing Disabled		Newly Disabled	
	Higher Labor Force Attachment	Lower Labor Force Attachment	Higher Labor Force Attachment	Lower Labor Force Attachment
Proportion of sample (%)	11.8%	47.7%	18.9%	21.6%
Employment				
Average employment rate (%)	78.6%	3.7%	72.1%	8.4%
Sex				
Male (%)	53.3%	48.7%	52.9%	47.3%
Female (%)	46.7%	51.3%	47.1%	52.7%
Race				
White, Non-Hispanic (%)	77.0%	65.9%	69.6%	60.9%
Black, Non-Hispanic (%)	9.0%	16.8%	11.8%	18.9%
Hispanic (%)	8.3%	11.8%	12.8%	14.4%
Other (%)	5.7%	5.5%	5.8%	5.8%
Age				
18-34 (%)	17.1%	15.2%	18.0%	19.3%
35-49 (%)	26.2%	22.3%	28.8%	23.5%
50-64 (%)	56.8%	62.5%	53.2%	57.2%
Educational Attainment				
Bachelor's degree (%)	24.0%	10.0%	24.6%	12.7%
No Bachelor's degree (%)	76.0%	90.0%	75.4%	87.3%
N	42,258	158,421	64,957	158,421

Notes: Sample is working age respondents, ages 18-64, in their fifth through eighth months of sampling who report a disability and who were interviewed twelve months prior.

Table 2. Descriptive Statistics for Disability Types

	Hearing Difficulty	Vision Difficulty	Cognitive Difficulty	Physical Difficulty	Self-Care Difficulty	Independent Living Difficulty
Proportion of disabled sample (%)	17.7%	12.6%	37.7%	56.0%	17.0%	34.9%
Employment						
Average employment rate (%)	47.5%	32.4%	19.5%	17.9%	10.0%	10.7%
Sex						
Male (%)	60.1%	48.8%	51.6%	45.0%	48.2%	46.8%
Female (%)	39.9%	51.2%	48.4%	55.0%	51.8%	53.2%
Race						
White, Non-Hispanic (%)	74.9%	60.8%	66.0%	65.1%	61.3%	62.6%
Black, Non-Hispanic (%)	8.7%	17.5%	15.8%	17.6%	18.0%	17.9%
Hispanic (%)	10.6%	15.7%	12.3%	11.8%	14.8%	13.4%
Other (%)	5.8%	6.1%	5.9%	5.5%	5.9%	6.1%
Age						
18-34 (%)	12.9%	16.5%	28.1%	8.7%	15.6%	20.8%
35-49 (%)	21.9%	24.5%	27.5%	22.5%	24.1%	25.3%
50-64 (%)	65.2%	59.0%	44.5%	68.8%	60.3%	53.9%
Educational Attainment						
Bachelor's degree (%)	19.9%	15.0%	12.0%	13.9%	13.3%	11.9%
No Bachelor's degree (%)	80.1%	85.0%	88.0%	86.1%	86.7%	88.1%
N	68,665	45,923	135,263	205,688	59,975	122,814

Notes: Sample is working age respondents, ages 18-64, in their fifth through eighth months of sampling who answer affirmatively to having any disability type.

Table 3. Stacked Difference-in-Difference Models by Disability Recency and Labor Force Attachment

		Ongoing Disabled				Newly Disabled			
		Higher Labor Force Attachment		Lower Labor Force Attachment		Higher Labor Force Attachment		Lower Labor Force Attachment	
Expansion State X Medicaid Expanded	*Estimate*	-0.005		0.105		-0.032	**	-0.092	
	Standard Error	0.013		0.070		0.016		0.092	
	P-Value	0.708		0.140		0.048		0.327	
Expansion State	*Estimate*	-0.001		-0.061	*	0.011		0.042	
	Standard Error	0.007		0.035		0.007		0.042	
	P-Value	0.918		0.090		0.146		0.313	
Medicaid Expanded	*Estimate*	-0.009	**	-0.038		0.007		0.004	
	Standard Error	0.004		0.023		0.006		0.036	
	P-Value	0.026		0.107		0.250		0.913	
N			1,849		1,803		1,849		1,794

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcome variable is logged employment-to-population rate of disability group. Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Table 4. Stacked Difference-in-Difference Models by Disability Type

		Hearing Difficulty	Vision Difficulty	Cognitive Difficulty	Ambulatory Difficulty	Self-Care Difficulty	Independent Living Difficulty
Expansion State X Medicaid Expanded	*Estimate*	0.012	-0.039	-0.021	-0.050	-0.042	-0.001
	Standard Error	0.028	0.049	0.042	0.037	0.091	0.073
	P-Value	0.670	0.429	0.626	0.190	0.649	0.987
Expansion State	*Estimate*	-0.007	0.008	-0.002	0.006	0.024	0.001
	Standard Error	0.010	0.028	0.024	0.015	0.039	0.032
	P-Value	0.444	0.770	0.938	0.672	0.548	0.986
Medicaid Expanded	*Estimate*	0.002	0.020	0.008	0.012	-0.001	-0.011
	Standard Error	0.010	0.018	0.015	0.014	0.030	0.029
	P-Value	0.828	0.266	0.616	0.414	0.982	0.699
N		1,849	1,837	1,849	1,849	1,742	1,835

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Outcome variable is logged employment-to-population rate of disability group. Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Table 5. Stacked Difference-in-Difference Models by Labor Force Attachment or Disability Recency

		Disabled	Ongoing Disabled	Newly Disabled	Disabled and Higher Labor Force Attachment	Disabled and Lower Labor Force Attachment
Expansion State X Medicaid Expanded	Estimate	-0.012	0.035	-0.038	-0.024 **	-0.006
	Standard Error	0.020	0.032	0.024	0.011	0.058
	P-Value	0.563	0.279	0.121	0.04	0.919
Expansion State	Estimate	0.000	-0.017	0.014	0.007	0.005
	Standard Error	0.009	0.013	0.011	0.005	0.028
	P-Value	0.995	0.191	0.201	0.226	0.859
Medicaid Expanded	Estimate	0.004	-0.013	0.013	0.003	-0.021
	Standard Error	0.008	0.012	0.010	0.004	0.024
	P-Value	0.579	0.312	0.204	0.368	0.382
N		1,849	1,849	1,849	1,849	1,839

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01. Outcome variable is logged employment-to-population rate of disability group. Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Appendix Contents

Appendix A: Medicaid Expansion Cohorts

Cohort	States	Sub-Experiment Time Frame
Pre-ACA	Delaware, Massachusetts, New York, Vermont	N/A
2010	Connecticut, District of Columbia	2009-2015
2011	Minnesota	2009-2016
2012	California, New Jersey	2009-2017
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	2009-2019
2015	Alaska, Indiana, Pennsylvania	2010-2019
2016	Louisiana, Montana	2011-2019
2019	Maine, Virginia	N/A
Never Expand	Alabama, Florida, Georgia, Hawaii*, Kansas, Mississippi, Missouri^, Nebraska*, North Carolina, Oklahoma^, South Carolina, South Dakota, Tennessee, Texas, Utah*, Wyoming	N/A

Notes: ~ 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. * Expand in 2020. ^ Expand in 2021.

Appendix B: Sample Size of Disability Groups

Table B1. Sample Size by Labor Force Attachment and Disability Recency

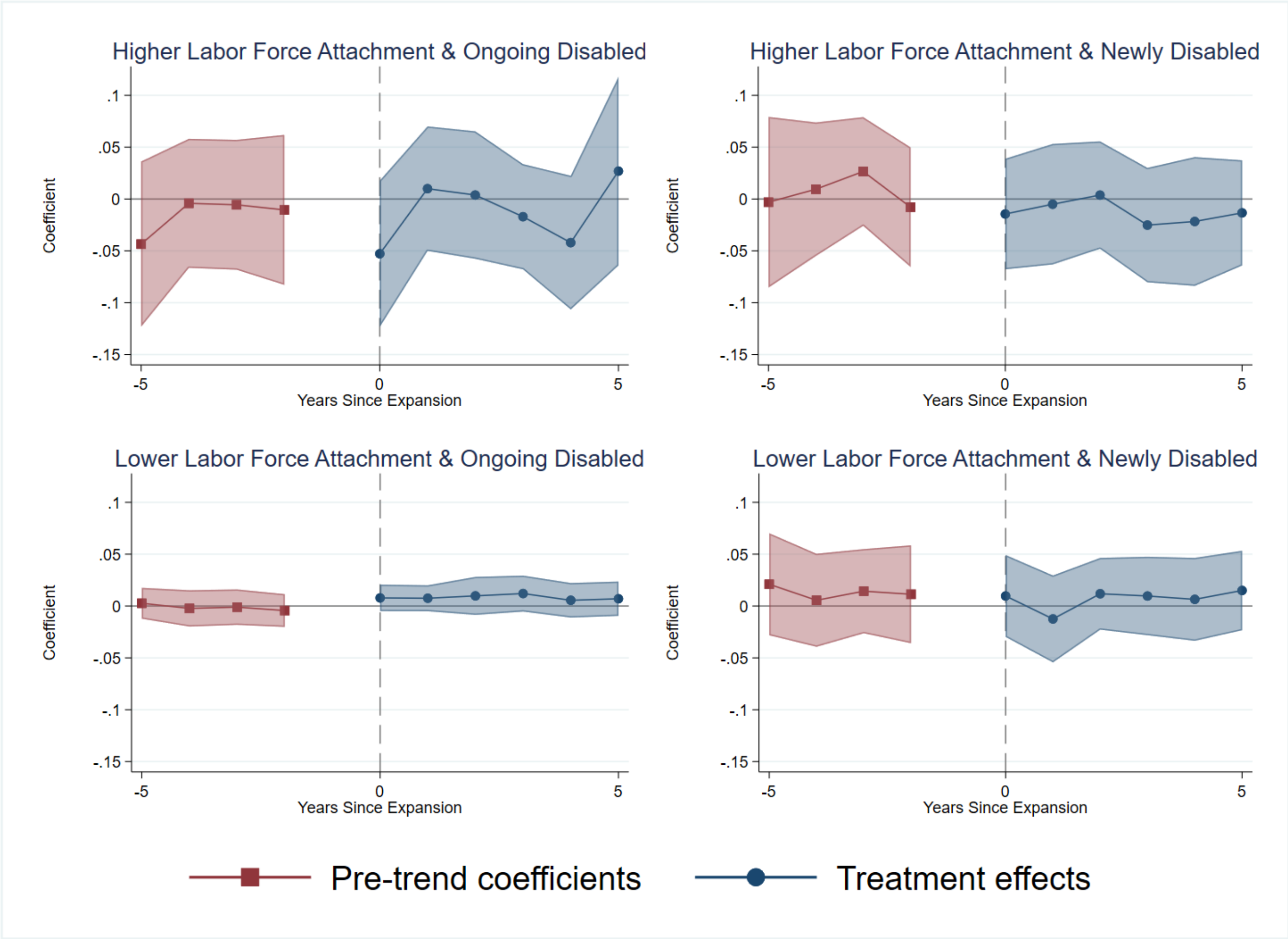
	Higher Labor Force Attachment	Lower Labor Force Attachment	
Ongoing Disabled	42,258	158,421	200,679
Newly Disabled	64,957	69,641	134,598
	107,215	228,062	335,277

Table B2. Sample Size by Disability Category

Disability Category	**N**
Any Disability	366,462
Hearing Disability	68,665
Vision Disability	45,923
Cognitive Disability	135,263
Physical Disability	205,688
Self-care Disability	59,975
Independent Living Disability	122,814
Sample is working age (18-64) respondents in their fifth through eighth months of sampling.	

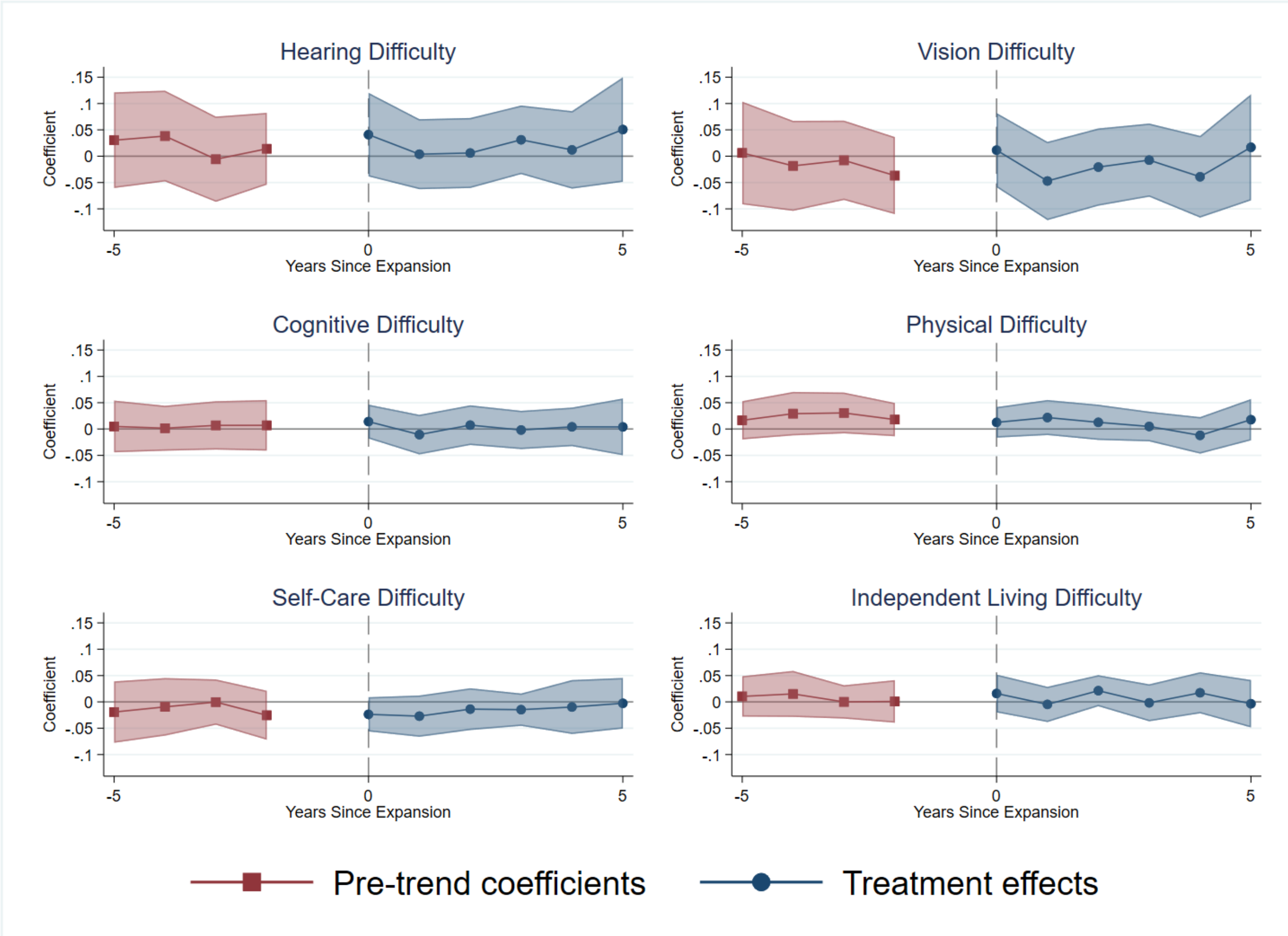
Appendix C: Testing for Parallel Pre-Trends: Event Study Plots by Disability Subgroup

Figure C1. Trends in Employment-to-Population Rate Relative to State Medicaid Expansion by Disability Recency and Labor Force Attachment



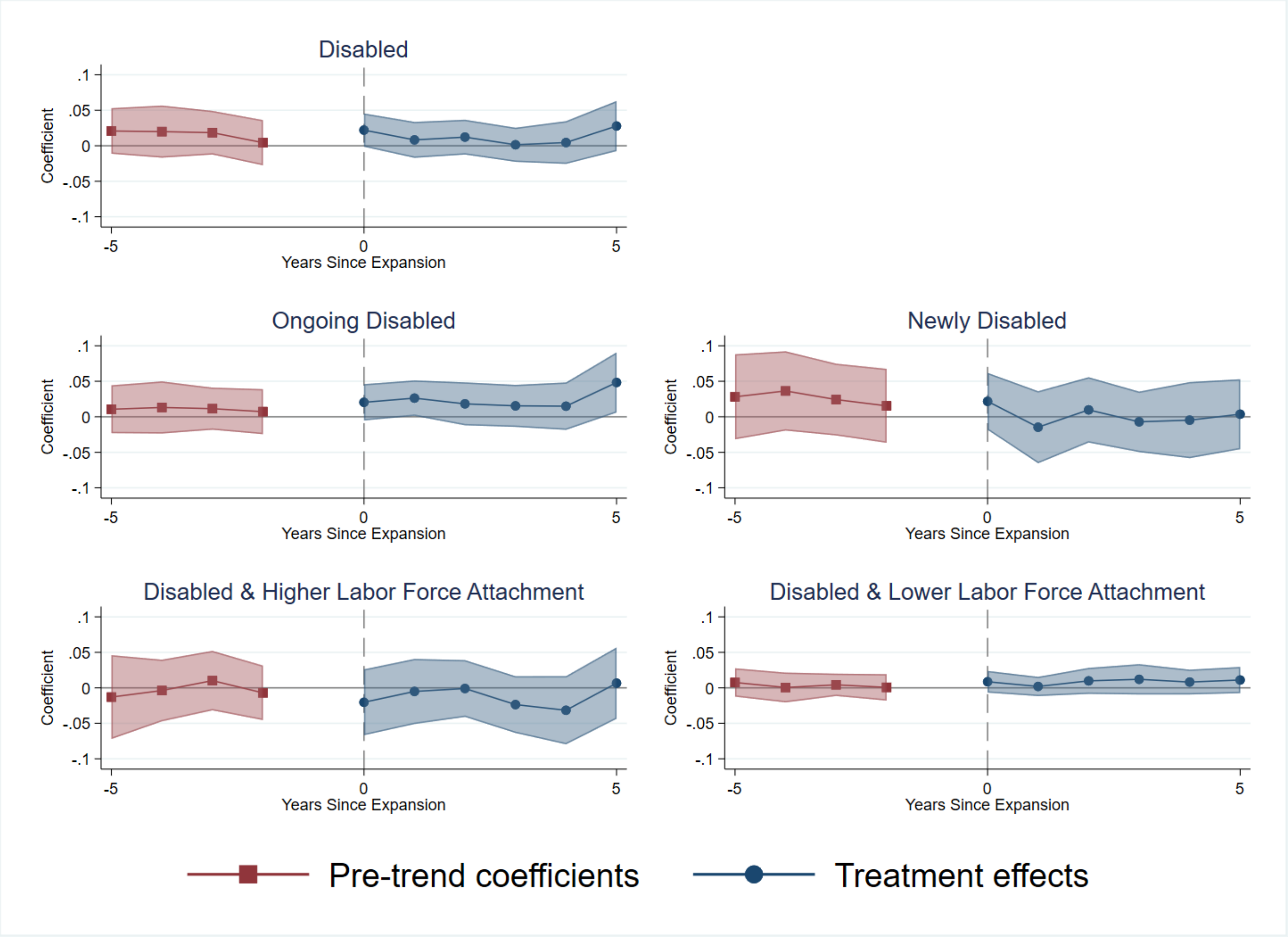
Notes: The outcome variable is employment-to-population rate of disability group. Models include state FE, calendar year FE, and demographic covariates (sex, race, age, college education).

Figure C2. Trends in Employment-to-Population Rate Relative to State Medicaid Expansion by Disability Type



Notes: The outcome variable is employment-to-population rate of disability group. Models include state FE, calendar year FE, and demographic covariates (sex, race, age, college education).

Figure C3. Trends in Employment-to-Population Rate Relative to State Medicaid Expansion by Disability Recency or Labor Force Attachment



Notes: The outcome variable is employment-to-population rate of disability group. Models include state FE, calendar year FE, and demographic covariates (sex, race, age, college education).

Appendix D: Unlogged Employment-Population Ratio Models

Table D1. Stacked Difference-in-Difference Models by Labor Force Attachment and Disability Recency with E/P Ratio Outcome

		Ongoing Disabled				Newly Disabled			
		Higher Labor Force Attachment		Lower Labor Force Attachment		Higher Labor Force Attachment		Lower Labor Force Attachment	
Expansion State X Medicaid Expanded	Estimate	-0.004		0.004		-0.023	**	-0.005	
	Standard Error	0.010		0.003		0.011		0.008	
	P-Value	0.671		0.170		0.045		0.562	
Expansion State	Estimate	0.000		-0.002		0.008		0.002	
	Standard Error	0.005		0.002		0.005		0.004	
	P-Value	0.937		0.165		0.111		0.617	
Medicaid Expanded	Estimate	-0.007	**	-0.002	*	0.004		-0.001	
	Standard Error	0.003		0.001		0.004		0.003	
	P-Value	0.028		0.087		0.313		0.633	
Average Employment Rate		0.786		0.037		0.721		0.084	
N		1,849		1,803		1,849		1,794	

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01. Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Table D2. Stacked Difference-in-Difference Models by Disability Type with E/P Ratio Outcome

		Hearing Disability	Vision Disability	Cognitive Disability	Physical Disability	Self-Care Disability	Independent Living Disability
Expansion State X Medicaid Expanded	Estimate	0.006	-0.010	-0.001	-0.012	-0.005	-0.004
	Standard Error	0.012	0.014	0.009	0.007	0.009	0.008
	P-Value	0.604	0.489	0.898	0.104	0.623	0.596
Expansion State	Estimate	-0.004	0.003	-0.001	0.003	0.003	0.000
	Standard Error	0.004	0.007	0.005	0.003	0.004	0.004
	P-Value	0.399	0.707	0.773	0.277	0.455	0.954
Medicaid Expanded	Estimate	0.000	0.005	0.000	0.002	0.000	0.000
	Standard Error	0.004	0.005	0.003	0.002	0.003	0.003
	P-Value	0.958	0.352	0.950	0.417	0.926	0.902
Average Employment Rate		0.177	0.126	0.377	0.560	0.170	0.349
N		1,849	1,837	1,849	1,849	1,742	1,835

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Table D3. Stacked Difference-in-Difference Models by Labor Force Attachment or Disability Recency with E/P Ratio Outcome

		Disabled		Ongoing Disabled		Newly Disabled		Disabled and Higher Labor Force Attachment		Disabled and Lower Labor Force Attachment	
Expansion State X Medicaid Expanded	Estimate	-0.004		0.005		-0.015		-0.017	**	0.001	
	Standard Error	0.006		0.007		0.010		0.008		0.003	
	P-Value	0.513		0.453		0.152		0.047		0.702	
Expansion State	Estimate	0.000		-0.002		0.004		0.005		-0.001	
	Standard Error	0.002		0.003		0.004		0.004		0.002	
	P-Value	0.923		0.477		0.314		0.209		0.684	
Medicaid Expanded	Estimate	0.001		-0.002		0.004		0.002		-0.002	
	Standard Error	0.002		0.002		0.004		0.003		0.001	
	P-Value	0.713		0.285		0.242		0.438		0.159	
Average Employment Rate		0.265		0.186		0.382		0.746		0.052	
N		1,849		1,849		1,849		1,849		1,839	

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01. Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Appendix E: Alternate Model Specifications – Early Expansion and 2019 Expansion Cohorts

Table E1. Alternate Difference-in-Difference Models by Specification of Early Expansion States and 2019 Expansion Cohort for Higher Labor Force Attachment and Ongoing Disabled Subgroup

		Primary Specification					
		(1)	(2)	(3)	(4)	(5)	(6)
Early Expansion States		Expand as Early as 2010		Expand in 2014		Dropped	
2019 Expansion Cohort		Excluded	Included	Excluded	Included	Excluded	Included
Expansion State X Medicaid Expanded	*Estimate*	-0.005	0.004	-0.001	-0.002	0.004	0.004
	Standard Error	0.013	0.013	0.018	0.017	0.019	0.018
	P-Value	0.708	0.792	0.950	0.907	0.826	0.833
Expansion State	*Estimate*	-0.001	-0.001	-0.009	-0.006	-0.011	-0.008
	Standard Error	0.007	0.007	0.011	0.009	0.011	0.010
	P-Value	0.918	0.831	0.396	0.544	0.320	0.420
Medicaid Expanded	*Estimate*	-0.010	-0.006	-0.019	-0.006	-0.019	-0.008
	Standard Error	0.004	0.005	0.014	0.011	0.014	0.012
	P-Value	0.026	0.262	0.199	0.641	0.195	0.505
N		1,849	1,993	886	1,060	812	980

Notes: Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Table E2. Alternate Difference-in-Difference Models by Specification of Early Expansion States and 2019 Expansion Cohort for Lower Labor Force Attachment and Ongoing Disabled Subgroup

		Primary Specification					
		(1)	(2)	(3)	(4)	(5)	(6)
Early Expansion States		Expand as Early as 2010		Expand in 2014		Dropped	
2019 Expansion Cohort		Excluded	Included	Excluded	Included	Excluded	Included
Expansion State X Medicaid Expanded	*Estimate*	0.105	0.103	0.138	0.120	0.218	0.191
	Standard Error	0.070	0.072	0.095	0.089	0.096	0.090
	P-Value	0.140	0.159	0.151	0.184	0.029	0.041
Expansion State	*Estimate*	-0.061	-0.041	-0.090	-0.064	-0.132	-0.094
	Standard Error	0.035	0.043	0.052	0.046	0.053	0.048
	P-Value	0.090	0.345	0.092	0.176	0.017	0.056
Medicaid Expanded	*Estimate*	-0.038	-0.021	0.075	0.068	0.047	0.051
	Standard Error	0.023	0.019	0.069	0.048	0.067	0.046
	P-Value	0.107	0.270	0.285	0.161	0.482	0.277
N		1,803	1,948	867	1,040	796	963

Notes: Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Table E3. Alternate Difference-in-Difference Models by Specification of Early Expansion States and 2019 Expansion Cohort for Higher Labor Force Attachment and Newly Disabled Subgroup

		Primary Specification					
		(1)	(2)	(3)	(4)	(5)	(6)
Early Expansion States		Expand as Early as 2010		Expand in 2014		Dropped	
2019 Expansion Cohort		Excluded	Included	Excluded	Included	Excluded	Included
Expansion State X Medicaid Expanded	*Estimate*	-0.032	-0.041	-0.030	-0.035	-0.035	-0.041
	Standard Error	0.016	0.014	0.021	0.019	0.022	0.020
	P-Value	0.048	0.007	0.167	0.072	0.122	0.052
Expansion State	*Estimate*	0.011	0.017	0.014	0.021	0.015	0.021
	Standard Error	0.007	0.006	0.012	0.008	0.013	0.009
	P-Value	0.146	0.010	0.258	0.019	0.242	0.024
Medicaid Expanded	*Estimate*	0.007	0.008	0.017	0.012	0.007	0.006
	Standard Error	0.006	0.004	0.015	0.009	0.014	0.009
	P-Value	0.250	0.038	0.245	0.197	0.628	0.501
N		1,849	1,993	886	1,060	812	980

Notes: Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Table E4. Alternate Difference-in-Difference Models by Specification of Early Expansion States and 2019 Expansion Cohort for Lower Labor Force Attachment and Newly Disabled Subgroup

		Primary Specification					
		(1)	(2)	(3)	(4)	(5)	(6)
Early Expansion States		Expand as Early as 2010		Expand in 2014		Dropped	
2019 Expansion Cohort		Excluded	Included	Excluded	Included	Excluded	Included
Expansion State X Medicaid Expanded	*Estimate*	-0.092	-0.038	-0.056	-0.007	-0.063	-0.009
	Standard Error	0.092	0.091	0.126	0.118	0.126	0.119
	P-Value	0.327	0.675	0.661	0.955	0.619	0.940
Expansion State	*Estimate*	0.042	-0.016	0.022	-0.044	0.023	-0.049
	Standard Error	0.042	0.047	0.082	0.066	0.080	0.066
	P-Value	0.313	0.726	0.787	0.510	0.780	0.458
Medicaid Expanded	*Estimate*	0.004	0.026	-0.048	-0.029	-0.052	-0.028
	Standard Error	0.036	0.025	0.080	0.050	0.080	0.049
	P-Value	0.913	0.306	0.554	0.567	0.517	0.565
N		1,794	1,938	864	1,036	791	957

Notes: Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Appendix F: Alternate Model Specifications – Pre-ACA Expansion Cohort

Table F1. Alternate Difference-in-Difference Models by Treatment of Pre-ACA Expansion Cohort for Higher Labor Force Attachment and Ongoing Disabled Subgroup

		Primary Specification	
		(1)	(2)
Pre-ACA Expanders		Excluded	2014 Expanders
Expansion State X Medicaid Expanded	*Estimate*	-0.005	-0.007
	Standard Error	0.013	0.012
	P-Value	0.708	0.542
Expansion State	*Estimate*	-0.001	0.002
	Standard Error	0.007	0.006
	P-Value	0.918	0.729
Medicaid Expanded	*Estimate*	-0.009	-0.007
	Standard Error	0.004	0.004
	P-Value	0.026	0.101
N		1,849	1,989

Notes: Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Table F2. Alternate Difference-in-Difference Models by Treatment of Pre-ACA Expansion Cohort for Lower Labor Force Attachment and Ongoing Disabled Subgroup

		Primary Specification	
		(1)	(2)
Pre-ACA Expanders		Excluded	2014 Expanders
Expansion State X Medicaid Expanded	*Estimate*	0.105	0.094
	Standard Error	0.070	0.064
	P-Value	0.140	0.149
Expansion State	*Estimate*	-0.061	-0.061
	Standard Error	0.035	0.031
	P-Value	0.090	0.058
Medicaid Expanded	*Estimate*	-0.038	-0.028
	Standard Error	0.023	0.023
	P-Value	0.107	0.243
N		1,803	1,936

Notes: Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Table F3. Alternate Difference-in-Difference Models by Treatment of Pre-ACA Expansion Cohort for Higher Labor Force Attachment and Newly Disabled Subgroup

		Primary Specification	
		(1)	(2)
Pre-ACA Expanders		Excluded	2014 Expanders
Expansion State X Medicaid Expanded	*Estimate*	-0.032	-0.031
	Standard Error	0.016	0.014
	P-Value	0.048	0.033
Expansion State	*Estimate*	0.011	0.009
	Standard Error	0.007	0.007
	P-Value	0.146	0.168
Medicaid Expanded	*Estimate*	0.007	0.008
	Standard Error	0.006	0.006
	P-Value	0.250	0.177
N		1,849	1,989

Notes: Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.

Table F4. Alternate Difference-in-Difference Models by Treatment of Pre-ACA Expansion Cohort for Lower Labor Force Attachment and Newly Disabled Subgroup

		Primary Specification	
		(1)	(2)
Pre-ACA Expanders		Excluded	2014 Expanders
Expansion State X Medicaid Expanded	*Estimate*	-0.092	-0.077
	Standard Error	0.092	0.087
	P-Value	0.327	0.383
Expansion State	*Estimate*	0.042	0.036
	Standard Error	0.042	0.038
	P-Value	0.313	0.346
Medicaid Expanded	*Estimate*	0.004	0.006
	Standard Error	0.036	0.036
	P-Value	0.913	0.874
N		1,794	1,934

Notes: Models include state FE, calendar year FE, experiment FE, demographic covariates (sex, race, age, college education). Standard errors clustered on the state-level.