Can Asset-Test Relief Raise Labor Supply? Evidence from ABLE Accounts

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Abstract

Exploiting the staggered state rollout between 2016 and 2023 in a difference-in-differences design, we provide the first national empirical study of the 2014 Achieving a Better Life Experience Act, a federal statute that relaxes the Supplemental Security Income 2,000-dollar asset test for adults with pre-age-26 disability onset through a dedicated tax-favored savings vehicle, and find that state ABLE launches raise employment by 0.7 percentage points, labor-force participation by 0.8 percentage points, log wage income by 7.7 percent, and disposable income by 321 dollars per adult per year. The response concentrates among adults with baseline household net worth below the 2,000-dollar cap. The aggregate annual disposable-income gain is 1.6 billion dollars at current program-participation levels. Take-up in the nine states with publicly reported account counts ranges from 0.19 to 3.04 percent of the state adult-with-disabilities population, with a median of 0.41 percent. Under linear scaling from the reduced-form estimate, raising take-up by one percentage point increases the aggregate annual disposable-income gain by 0.55 billion dollars. Raising take-up in the nine measured states to Nebraska's 3.04 percent, roughly seven times the current median, would raise the aggregate gain in these states to about 11 billion dollars per year.

Keywords: disability, asset test, SSI, SSDI, Medicaid, ABLE Act, financial inclusion, labor supply.

JEL Codes: H31, H55, I18, I38, J14, J22.

I. Introduction

The Supplemental Security Income asset limit for individuals has been frozen at 2,000 dollars since the Family Support Act of 1988. Inflation has since eroded the real value of the threshold by roughly three quarters, so a resource cap that once excluded only genuinely wealthy applicants now binds against a growing share of adults with disabilities near the low-income margin (Meyer and Mok 2019, Bond, Green, Kruse, and Schur 2024). For an adult with a disability whose Medicaid eligibility is tied to the SSI rule directly, or through a medically needy pathway inheriting the same threshold, an additional dollar of countable savings can trigger the loss of both cash assistance and health-insurance coverage. The asset test therefore acts as a benefit cliff rather than a gradual phaseout, and the notch it creates in the household budget set changes not only the return to saving but the return to any earnings that would otherwise accumulate as countable resources (Hubbard, Skinner, and Zeldes 1995).

Estimating the causal effect of relaxing an asset test has been difficult because prior reforms bundled the asset margin with other program parameters. The 1996 welfare reform gave states discretion to raise or eliminate Aid to Families with Dependent Children asset tests but simultaneously altered time limits, work requirements, and earned-income disregards. SNAP asset-test changes under broad-based categorical eligibility were tied to Temporary Assistance for Needy Families alignment and inherited other eligibility parameters. Medicaid long-term-care asset rules changed lookback periods and transfer penalties while leaving the underlying resource test largely intact. Vehicle exemptions under AFDC changed the definition of exempt assets but did not relax the general resource test. Estimates in Powers (1998), Gruber and Yelowitz (1999), Hurst and Ziliak (2006), and Sullivan (2006) therefore identify responses to bundled reforms or narrow exemptions rather than to asset-test relief on its own.

The Achieving a Better Life Experience Act of 2014 offers a cleaner policy experiment. ABLE created tax-favored savings accounts for individuals whose qualifying disability began before age 26. Balances up to 100,000 dollars in an ABLE account are excluded from the SSI resource test, and

ABLE balances are excluded from Medicaid asset tests in states with adopted programs. The reform raises the effective resource limit for eligible individuals while leaving income eligibility, benefit levels, work-incentive rules, and the underlying disability definition unchanged. This isolation of the asset margin makes ABLE distinct from the welfare and Medicaid reforms studied in the prior empirical literature.

Congress delegated program administration to states, and the resulting rollout was staggered across a decade. Ohio, Tennessee, and Nebraska launched the first programs in June 2016. Nineteen states followed in 2017, eleven more in 2018, and additional states launched programs through the end of the sample period. Four jurisdictions are treated as never-adopters in the main design. We use this state-by-state variation in access to a functioning ABLE program as the identifying variation for a staggered difference-in-differences design applied to the working-age adult-with-disabilities population. Standard household survey and administrative data support the reduced-form causal analysis. Program-participation data from nine states that publish credible ABLE account counts, combined with state adult-with-disabilities population figures from the ACS, characterize current enrollment and support the welfare-projection exercise in Section VIII.

State ABLE launches raise the labor supply of adults with disabilities. In the population-weighted labor-market panel, employment rises 0.7 percentage points, labor-force participation rises 0.8 percentage points, and log wage income rises 7.7 percent, with state-clustered standard errors of 0.22, 0.25, and 2.85 percentage points respectively. The disposable-income response is 321 dollars per adult per year, or 1.5 percent, and aggregates to 1.6 billion dollars annually across the disability population. The gain operates through higher earned income rather than through changes in transfer receipt. Sun and Abraham (2021) interaction-weighted event studies confirm the sign and magnitude of the two-way fixed-effects coefficients, and the event-time path rises steadily after launch with no comparable pre-treatment movement.

The response concentrates in the population for whom the SSI resource cap was actually binding. Among adults with disabilities whose baseline household net worth is below the 2,000-dollar SSI cap, the probability of holding an Individual Retirement Account rises 5.9 percentage points and log bank balances rise 1.26 log points. Among residents of states served by the Ohio STABLE platform through partner-state arrangements, log bank balances rise 1.07 log points. The concentration of the response in exactly the wealth strata for whom the asset test binds is the strongest evidence in the paper that the mechanism is the relaxation of the SSI resource limit rather than a general labor-market spillover.

The state-year evidence does not support a causal interpretation for disability-insurance enrollment. Baseline two-way fixed-effects estimates suggest that OASDI disabled-worker counts decline after ABLE launches, but the decline is not robust. Launcher states were already on a differential pre-2016 trajectory, and never-adopter states show a similar post-2015 decline. Once state-specific pre-2016 trends are removed, or state-specific linear trends are absorbed across the full sample, the OASDI coefficient is no longer distinguishable from zero. The identified response therefore operates through labor supply and disposable income, not through a separately identified reduction in federal disability-insurance rolls.

The estimates survive alternative estimators developed for the staggered-adoption setting. Callaway and Sant'Anna (2021) doubly-robust estimates fall within one standard error of the Sun-Abraham estimates for the pre-trend-clean outcomes. HonestDiD sensitivity analysis at relative-magnitudes bounds through one preserves the sign of the labor-supply results (Rambachan and Roth 2023). A Goodman and Bacon (2021) decomposition shows that the two-way fixed-effects weight structure gives modest weight to the forbidden late-versus-early-treated block, and the primary coefficients survive when identification restricts to never-adopter comparisons. Placebo tests using non-disabled households or fake launch dates three years before actual launches produce null effects. Dropping Ohio and its STABLE partner states leaves the disposable-income estimate essentially unchanged.

The paper makes three contributions. It offers the first empirical evaluation of the 2014 ABLE Act, using the staggered state rollout as the identifying variation for the reduced-form effect on adults with disabilities. It isolates the asset margin from the income limits and work-incentive rules that were bundled into every prior asset-test reform in the empirical literature (Powers 1998, Gruber and Yelowitz 1999, Hurst and Ziliak 2006, Sullivan 2006). It documents that the labor-supply response concentrates in the population most affected by the underlying constraint, adults with disabilities whose baseline household savings bind the SSI 2,000-dollar resource cap. And it draws on program-participation data from nine states that publish credible ABLE account counts to project the aggregate welfare gains from expanded enrollment. Take-up in these nine states ranges from 0.19 to 3.04 percent of the state adult-with-disabilities population, with a median of 0.41 percent, indicating that most eligible adults do not currently hold an account and that the 1.6-billion-dollar aggregate income gain estimated at current participation levels understates the potential welfare improvement under broader take-up. The evidence connects the asset-margin channel to the labor-supply and financial-inclusion literatures for adults with disabilities (Autor and Duggan 2003, Maestas, Mullen, and Strand 2013, Deshpande and Li 2019, Meyer and Mok 2019, Bond, Green, Kruse, and Schur 2024).

The remainder of the paper proceeds as follows. Section II describes the asset-test environment and the institutional design of ABLE. Section III presents a conceptual framework that formalizes the benefit-cliff mechanism and derives four testable predictions. Section IV describes the survey and administrative data. Section V presents the empirical strategy. Section VI reports the labor-supply, disposable-income, program-enrollment, health, and heterogeneity results. Section VII presents robustness tests. Section VIII combines the reduced-form estimates with descriptive nine-state take-up data to project the aggregate welfare gains from expanded enrollment. Section IX concludes.

II. Institutional Background

A. Asset Tests in Disability Programs

Asset tests restrict eligibility for means-tested transfers by limiting the liquid resources a household may hold while receiving benefits. In the disability program environment, the most important threshold is the Supplemental Security Income resource limit. SSI sets countable resources at no more than 2,000 dollars for individuals and 3,000 dollars for couples, limits that have not changed since the Family Support Act of 1988. Inflation has since eroded the real value of the individual threshold from about 5,036 dollars in 1988 dollars to about 1,006 dollars in 2024 dollars. Approximately 4 million working-age adults receive SSI in 2024 at a program cost of 56 billion dollars, and 7.6 million disabled workers and their dependents receive Social Security Disability Insurance benefits at a program cost of 158 billion dollars. In thirty-four states, Medicaid eligibility for adults with disabilities is tied to SSI eligibility directly or through a medically needy pathway that inherits the same asset threshold. The asset limit therefore affects not only cash assistance but also access to health insurance for a large share of the disability-program population.

The economic effect of the asset limit depends on the size and discontinuity of the benefit loss. For an adult with a disability near the SSI threshold, a small increase in countable saving can lead to the loss of SSI cash benefits of approximately 11,000 dollars per year and Medicaid coverage worth roughly 10,000 dollars per year in fee-for-service equivalents. The program rule therefore creates a notch in the household budget set rather than a smooth tax schedule. An additional dollar of savings may have little private value if it risks the loss of benefits that are many multiples of the account balance itself. Descriptive evidence documents that the notch shapes household balance sheets. Adults with disabilities are approximately twice as likely as adults without disabilities to lack a bank account (FDIC 2020), and 43 percent report they could not cover a 400-dollar emergency expense from savings, compared with 26 percent of adults without disabilities (Federal Reserve 2019). Bond, Green, Kruse, and Schur (2024) show that the raw asset-holdings gap between adults with and without disabilities in the SIPP concentrates at balances below 2,000 dollars and disappears above the threshold. The asset limit therefore acts as both a saving disincentive and a labor-supply disincentive because earnings that cannot be held without crossing the limit are less valuable than earnings that can be accumulated safely.

The asset-test literature has long emphasized this mechanism. Hubbard, Skinner, and Zeldes (1995) show that means-tested transfers can reduce precautionary saving by taxing assets in states of the world when households most value liquidity. Powers (1998), Gruber and Yelowitz (1999), Hurst and Ziliak (2006), and Sullivan (2006) estimate behavioral responses to asset limits and exemptions in AFDC, TANF, Medicaid, and vehicle-exemption settings. These studies established that asset tests can matter, but the empirical settings were not clean tests of the asset margin alone. The reforms generally changed asset limits alongside income rules, work requirements, eligibility thresholds, or the definition of exempt assets.

The disability setting differs from the earlier welfare settings in two ways that make the asset margin especially salient. Program attachment is often longer, and an adult with a qualifying disability may face the SSI and Medicaid resource rules for many years, which raises the value of any policy that permits precautionary saving over a long horizon. The benefit at risk is also large. The SSI cash benefit and Medicaid coverage together make the asset cliff substantially larger than the nominal 2,000-dollar threshold suggests. These features imply that the behavioral response to asset-test relief may be larger for adults with disabilities than for short-duration cash-assistance populations.

ABLE changes this asset-test environment directly. It does not eliminate the SSI resource test. It creates an account-based exclusion that allows eligible individuals to hold assets outside the countable-resource calculation. That distinction matters for interpretation. The policy does not change the structure of SSI or Medicaid eligibility for all assets. It changes the return to saving through a specific financial account whose balances are disregarded for program eligibility.

Three federal programs shape the financial incentives facing working-age adults with disabilities. SSI provides cash assistance to individuals who satisfy income, asset, and disability criteria. SSDI provides earnings-related benefits to workers with sufficient work histories who meet the Social Security Administration's disability standard. Medicaid provides health insurance, and in many states its eligibility rules for adults with disabilities are tied to SSI eligibility or to pathways that use similar asset limits.

The second is Social Security Disability Insurance (SSDI), the disability component of Old-Age, Survivors, and Disability Insurance (OASDI). SSDI pays a monthly benefit that scales with pre-disability earnings to workers who have accumulated sufficient work credits and who meet the same medical definition of disability used by SSI.

These programs constrain behavior on different margins. SSI imposes the asset cliff that motivates the analysis. SSDI does not impose an asset test, but it discourages earnings above the Substantial Gainful Activity threshold. Medicaid raises the stakes of the SSI asset test because health insurance can be lost along with cash assistance when eligibility is tied to SSI rules. The combined incentive problem is therefore not only whether an adult with a disability works, but whether earnings can be saved without jeopardizing cash benefits or health insurance.

The existing disability labor-supply literature has identified responses to several of these program margins. Autor and Duggan (2003), Chen and van der Klaauw (2008), Maestas, Mullen, and Strand (2013), French and Song (2014), Kostøl and Mogstad (2014), Deshpande (2016), Deshpande, Gross, and Su (2021), Deshpande and Li (2019), and Meyer and Mok (2019) show that disability-program participation, screening, application costs, earnings tests, and benefit receipt affect labor supply and household outcomes. This paper studies a different margin. ABLE does not change the medical definition of disability, the SSDI earnings ceiling, the SSI benefit amount, or the application process. It changes whether an eligible individual can accumulate assets without triggering the SSI or Medicaid resource test.

The policy margin can be seen most clearly through a simple household decision. Before ABLE, an adult with a disability who accumulated 5,000 dollars in an ordinary bank account risked losing benefits tied to the 2,000-dollar resource limit. After ABLE, the same individual could hold the 5,000 dollars in an ABLE account without having those assets count against the SSI resource test up to the statutory exclusion. The account changes the value of saving and, through that channel, the value of earnings that can be saved. The empirical question is whether relaxing this asset constraint changes disposable income and labor supply.

B. The ABLE Act and State Rollout

The Achieving a Better Life Experience Act created a tax-favored savings vehicle for individuals with qualifying disabilities. Ander Crenshaw and Robert Casey introduced the first version of the bill in 2006 in response to advocacy by the National Disability Institute and other disability-community organizations. After eight congressional sessions without a floor vote, the 113th Congress passed the Act as part of the December 2014 Tax Increase Prevention Act, and President Obama signed the statute on December 19, 2014. The statute was modeled on the Section 529 college-savings framework, but its economic function differs. A 529 account subsidizes education saving through tax-favored treatment. An ABLE account relaxes the asset test attached to means-tested disability programs through an account-based exclusion from countable resources.

The central policy provision is the exclusion of ABLE balances from countable resources. Balances up to 100,000 dollars in an ABLE account do not count toward the SSI resource limit. Balances above that amount suspend SSI cash payments rather than terminate eligibility. ABLE balances are also excluded from Medicaid asset tests in states that have adopted programs. The reform therefore raises the effective asset limit for eligible individuals while leaving other program rules intact.

Eligibility depends on the age at which the qualifying disability began. During the sample period, individuals were eligible only if their disability began before age 26. The individual could open an account later in life, but eligibility required pre-age-26 onset. This rule generates the age-gradient used later in the paper. Adults ages 18 to 25 with disabilities satisfy the onset condition mechanically. Older adults with disabilities satisfy it only if their disability began before age 26.

The statute also sets contribution and use rules. Annual contributions are tied to the federal gift-tax exclusion, which was 18,000 dollars in 2024. Working account holders may contribute additional earnings up to the federal poverty level under the ABLE to Work provision. Qualified distributions are tax-free when used for disability-related expenses, including housing, education, health, transportation, employment training and support, personal support services, and legal fees. Non-qualified distributions are subject to income tax and a penalty on earnings.

The age-of-onset rule will change after the sample period. The ABLE Age Adjustment Act (P.L. 117-328, division T) was sponsored by Senators Amy Klobuchar and Bill Cassidy, signed by President Biden on December 29, 2022, and takes effect January 1, 2026. It raises the onset threshold from 26 to 46 and roughly doubles the eligible population by including individuals whose disability began between ages 26 and 45. The change does not affect the estimates in this paper because the sample period is governed by the original pre-age-26 rule. The 2026 expansion is relevant for interpreting external validity, and Section VIII returns to this projection.

Take-up remains limited relative to the eligible population, though balances among account holders are economically meaningful. The National Disability Institute estimates the pre-2026 eligible population at 8.1 million individuals, of whom 234,000 (about 2.9 percent) held an ABLE account as of December 2025. The average balance of about 13,000 dollars is modest relative to the 100,000-dollar exclusion but far above the 2,000-dollar SSI resource limit. The Ohio STABLE Account platform holds approximately 40 percent of national ABLE assets, roughly 52,000 active accounts across Ohio residents and eleven partner states. Active account holders therefore hold savings that would otherwise have been difficult to accumulate without risking benefit eligibility.

Congress left ABLE program administration to the states. States could establish programs after the federal statute, but launch required administrative action, program design, and coordination with account platforms. This produced staggered implementation across states.

The rollout began in 2016. Ohio, Tennessee, and Nebraska launched the first programs in June 2016, followed by additional first-wave states later that year. Nineteen states launched in 2017, and eleven launched in 2018. Later launches occurred from 2019 through 2023, with Idaho scheduled for January 2026. The identifying variation in the paper comes from this staggered access to a state ABLE program.

The launch-date crosswalk is constructed from state treasurer press releases, official program websites, ABLE National Resource Center state pages, and enabling legislation. The draft requires two independent primary sources to verify a launch month and year when possible. Three states, North Dakota, South Dakota, and Wisconsin, are confirmed never-adopters during the sample window. Nevada is treated as a never-adopter in the main design because the launch date for its National ABLE Alliance program could not be independently verified. The robustness section checks whether this coding decision matters by dropping Nevada from the control group.

The empirical role of state launch dates is access to the account infrastructure that implements the federal exclusion. The federal statute created the legal possibility of ABLE accounts, but residents generally needed an operating program to enroll. State launch is therefore the relevant treatment for measuring the reduced-form effect of practical access to asset-test relief.

ABLE accounts can be opened across state lines when a state program accepts non-residents. This feature follows the logic of 529 college-savings plans and complicates the empirical design. Beginning in June 2016, Ohio's STABLE Account admitted non-residents. Eligible individuals living in states without their own ABLE program could therefore obtain the federal SSI exclusion through Ohio.

Cross-border enrollment creates treatment exposure among some residents coded as controls under a home-state launch design. The direction of bias is clear. If residents of non-launcher states open Ohio accounts, then the control group partially receives the asset-test relief. This contamination attenuates the estimated effect of home-state launch toward zero.

The magnitude of the problem is limited by several institutional features. State income-tax deductions create a strong incentive for residents to use their home-state program when available. Twenty-four jurisdictions offer deductions for ABLE contributions, and these deductions generally apply only to the state's own program. Awareness, outreach, and account-navigation assistance are also more likely to operate through in-state treasurers, disability-service systems, and advocacy networks after a state launches its own program. These frictions imply that cross-border enrollment exists, but home-state launch remains economically meaningful.

Ohio's platform is nevertheless important enough to address directly. The STABLE platform hosted eleven partner states as of the 2024 Vestwell disclosure and held a large share of national ABLE assets. The robustness section therefore quantifies cross-border exposure in several ways. It drops Ohio and partner states, restricts identification to later launch cohorts that entered after the cross-border environment was already operating, absorbs differential post-2016 movement in never-adopter states, and tests whether launch effects differ by state tax-deduction status. The interpretation is that cross-border access weakens the reduced-form contrast rather than generating a spurious positive effect.

This institutional setting defines the estimand. The estimates do not measure the effect of the federal statute in a world with perfect national take-up. They measure the reduced-form effect of state ABLE program launch on adults with disabilities, allowing for partial cross-border access before some home-state launches. Because that access can only move control states toward treatment, the baseline estimates should be interpreted as conservative estimates of the economic response to practical asset-test relief.

III. Conceptual Framework

Consider an adult with a disability who chooses labor supply L and end-of-period assets A' to maximize utility over consumption and leisure. She faces the budget constraint

$$c + A' = wL + T(A) + rA, \qquad (1)$$

where w is the wage, r is the return on savings, and $T(A)$ is the SSI cash transfer as a function of countable resources A. The asset test enters $T(A)$ as a benefit cliff. $T(A) = B$ if A is at or below the SSI resource threshold $A^* = 2{,}000$ dollars, and $T(A) = 0$ otherwise. Medicaid eligibility for adults with disabilities is tied to the same threshold in states without expanded eligibility, so crossing A^* also risks losing health-insurance coverage worth roughly the actuarial value of the benefits at stake.

The benefit cliff generates an implicit tax on saving that can exceed 100 percent at the notch. An additional dollar of countable savings above A^* raises interest income by $r \cdot dA$ but reduces transfers by B, and B is many multiples of $r \cdot dA$. Households near A^* therefore have a strong incentive to keep countable savings at or just below A^* (Kleven and Waseem 2013 develop the general notch logic in a different setting). Precautionary saving is depressed because the reward to holding liquid resources against future income shocks is dominated by the loss of program eligibility (Hubbard, Skinner, and Zeldes 1995). The asset test therefore acts as both a saving disincentive and a labor-supply disincentive because earnings that cannot be saved without triggering the cliff are less valuable than earnings that can be safely accumulated.

The ABLE Act relaxes the constraint through an exempt account. Balances up to 100,000 dollars in a qualified ABLE account are excluded from the countable-resource calculation. For eligible adults with disabilities, the effective asset limit rises from 2,000 dollars to 102,000 dollars, and Medicaid resource tests are similarly adjusted in states with adopted programs. The reform changes the return to saving through a specific account-based exclusion. It does not change the SSI benefit formula, the income eligibility rule, the SSDI earnings ceiling, or the disability definition. This isolation of the asset margin is what distinguishes ABLE from prior asset-test reforms in the empirical literature.

Three testable predictions follow from this framework. First, labor supply among eligible adults with disabilities should rise after state ABLE launch, with the extensive margin responding most sharply because non-work is the corner solution that adults facing the cliff are pushed toward when saving is blocked. Second, the response should concentrate in the subgroup for whom the cliff is actually binding. Adults with disabilities whose baseline household net worth is below the 2,000-dollar cap are the population for whom the constraint has behavioral bite, while adults far above the cap should show little response. Third, the response should be smaller in states that have expanded Medicaid because the benefit at stake at A^* is smaller where Medicaid expansion has weakened the tie between SSI eligibility and health-insurance coverage.

The predictions do not require a strong claim about program-enrollment substitution. ABLE could raise labor supply and disposable income without reducing SSDI or SSI enrollment because the primary mechanism is the release of a saving margin rather than a change in program-entry incentives. Section VI tests the four predictions using state-year and individual-level variation, and Section VII evaluates identification threats to each.

IV. Data

A. Household Survey and Administrative Panels

The outcome analysis combines four standard household survey and administrative panels with the ABLE launch and Medicaid expansion crosswalks. The empirical analysis requires data that can capture three distinct margins of response to ABLE. The first is disposable income, because asset-test relief should change the resources available to adults with disabilities if the constraint binds. The second is labor supply, because the value of earnings changes when those earnings can be saved without triggering the SSI or Medicaid resource test. The third is disability-program participation, because one possible interpretation of higher disposable income is substitution away from disability-insurance or cash-transfer programs rather than an increase in labor-market resources. We therefore combine three state-year panels constructed from the CPS ASEC, the ACS, and Social Security Administration administrative counts. A fourth ACS person-year panel decomposes program receipt across specific transfer margins, and a set of policy crosswalks defines state ABLE launch timing, Medicaid expansion, state tax deductions, and Ohio STABLE partner-state exposure.

The primary income data come from the Current Population Survey Annual Social and Economic Supplement. We construct a state-year panel for 2008 to 2023 that measures disposable income, earned income, transfer income, and the transfer share of income for adults with and without disabilities. The CPS ASEC is the appropriate source for the disposable-income analysis because it measures household and personal income components, cash transfers, and disability status over the full period surrounding ABLE implementation.

Disposable income is measured as pre-tax personal income net of federal and state income-tax liability and inclusive of cash transfers. Tax liabilities are computed cell by cell using NBER TAXSIM. The resulting measure captures the resources available to individuals after the tax and transfer system, which is the relevant object for evaluating whether ABLE relaxes the effective asset constraint facing adults with disabilities.

The panel is aggregated to state-year-disability-recipient cells. Adults without disabilities receive no SSI or SSDI transfers by construction in this setup, so the disability-by-recipient structure reduces to three cells. The final panel contains 2,448 state-year-cell observations across 51 jurisdictions and 16 years. The main disposable-income estimates are population weighted so that the coefficient reflects the average effect for adults with disabilities rather than the average effect across equally weighted states.

The labor-market analysis uses the American Community Survey from IPUMS. We construct a state-year panel for adults ages 18 to 64 over 2011 to 2022, aggregated by state, year, disability status, and age group. The ACS is better suited than the CPS for the labor-market analysis because it contains much larger samples of adults with disabilities, which is necessary for estimating employment and wage responses by state, year, and age group.

The main outcomes are employment, labor-force participation, poverty, and log wage income. Disability is defined using the ACS disability-difficulty items, consistent with the broad ADAAA framework and the population ABLE is designed to reach. The full ACS extract contains 21.1 million individual records covering 2008 to 2022. Aggregation to state-year-disability-age cells produces 7,038 rows covering 51 jurisdictions, 23 years, two disability groups, and three age groups: 18 to 25, 26 to 45, and 46 to 64. The main estimation sample begins in 2011 to avoid the earlier disability-measurement transition and to align the panel with the policy windows used in the analysis.

The SIPP 2014 individual-level panel supports both the main-effect financial-independence results in Table 2 Panels C and D and the mechanism heterogeneity analysis in Table 4 Panel B (Section VI.C). It covers 191,093 person-wave observations for adults ages 18 to 64 across 16 quarterly waves and records baseline household net worth, financial-account holdings, program participation, and demographic characteristics. The SIPP data are used to estimate the two-way fixed-effects and triple-difference specifications on eight financial-account outcomes, and separately within twenty-two subgroup dimensions that isolate the population for whom the SSI resource cap was most likely to bind.

The third panel measures disability-program participation using Social Security Administration administrative counts. The source data are county-year counts of OASDI and SSI recipients and benefit amounts for 2010 to 2024. We aggregate the county-year data to the state-year level by summing counts and dollars within each state. The resulting panel measures disabled-worker counts, total OASDI counts, SSI counts, blind and disabled adult counts, disabled-child counts, disabled-spouse counts, and benefit amounts.

The SSA panel serves a different purpose from the CPS and ACS panels. It tests whether the income and labor-supply effects coincide with changes in disability-insurance enrollment. This distinction matters because ABLE could raise disposable income through higher earnings while leaving disability-insurance rolls unchanged, or it could change income by reducing reliance on disability programs. The SSA panel allows the paper to test the latter channel directly at the state-year level.

The panel also provides an important limitation. Because ABLE adoption is staggered but nearly universal by the end of the period, and because national disability-insurance rolls were changing over the same years, the SSA outcomes require stronger pre-trend diagnostics than the income and labor-market outcomes. The results below therefore treat the SSA panel as a test of disability-insurance substitution rather than as the primary source of the paper's causal estimate.

To decompose the transfer-share response, we construct a supplementary ACS person-year panel for adults with disabilities. The panel covers 2011 to 2022, and the underlying ACS person-year microdata for adults ages 18 to 64 across these years contain 16.9 million records that are then restricted to the disability sample. It measures receipt of Supplemental Nutrition Assistance Program benefits, Supplemental Security Income, cash welfare, and Medicaid, along with dollar amounts of Social Security income, welfare income, and supplemental income.

This panel is used to ask whether the decline in the transfer share reflects a specific program margin. If ABLE reduces reliance on cash transfers, the effect may appear as lower SSI receipt, lower SNAP receipt, lower cash-welfare receipt, lower Social Security income, or some combination of these. The ACS program panel is not as administratively precise as the SSA records for disability-insurance counts, but it has the advantage of measuring multiple transfer and insurance margins within the same population used for the labor-market analysis.

The panel is aggregated to 612 state-year cells for adults ages 18 to 64 with disabilities. The estimates from this panel are interpreted as program-margin evidence that complements the CPS disposable-income estimates. They do not replace the CPS as the primary source for disposable income, and they do not replace the SSA panel as the administrative source for disability-insurance counts.

The treatment variable is the launch of a state ABLE program. We use the launch-date crosswalk described in the institutional section, constructed from state treasurer press releases, official program websites, ABLE National Resource Center state pages, and enabling legislation. The crosswalk records launch month and year, program name, host platform for partner states, and verification status. These launch dates define the state-year treatment indicator used in the baseline, triple-difference, and robustness specifications.

The analysis also uses three additional policy crosswalks. Medicaid expansion dates come from the Kaiser Family Foundation and include later expansions in North Carolina and South Dakota, with pre-ACA expansion states coded as adopting in 2014 following Carey, Miller, and Wherry (2020). These dates are used to test whether ABLE and Medicaid expansion substitute or complement each other on the disposable-income margin. State ABLE income-tax deductions are coded from ABLE National Resource Center documentation. Twenty-four jurisdictions offer a state tax deduction for ABLE contributions, generally restricted to the state's own program. This variable is used to test whether home-state launch effects differ when residents had stronger incentives to wait for an in-state account. Finally, the Ohio STABLE partner-state list comes from the 2024 Vestwell disclosure and identifies states whose ABLE programs were hosted through the Ohio platform.

These crosswalks are important because ABLE exposure is not identical to the date of the federal statute. The federal law created the legal account structure, but practical access depended on state launch, platform availability, tax incentives, and cross-border enrollment. The empirical treatment is therefore state program launch, while the robustness analysis evaluates how estimates change when cross-border access and platform partnerships are taken into account.

B. Program-Participation Data

Section VIII combines the reduced-form estimates with program-participation data drawn from public disclosures by state ABLE program administrators. Nine states, Alabama, Louisiana, Maine, Nebraska, New York, North Carolina, Tennessee, Texas, and Virginia, publish credible account counts through state treasurer press releases and program trust reports. Coverage varies from one quarter (Maine) to twelve quarters (North Carolina), with the most recent observation drawn from the latest quarterly filing available at the time of the paper. Twenty additional states publish assets under management but not account counts, and eighteen launcher states have no public reporting suitable for a take-up rate calculation. These data are used descriptively in Section VIII and do not enter the causal identification. Take-up for each state is the reported account count divided by the state adult-with-disabilities population aged 18 to 64, taken from the American Community Survey five-year estimates. Appendix Table A7 reports the state-by-state snapshot.

V. Empirical Strategy

The empirical strategy uses the staggered launch of state ABLE programs to estimate the economic response to asset-test relief among adults with disabilities. The federal statute created the legal account structure in 2014, but practical access depended on whether a state had launched an operating program, or whether residents enrolled across state lines through a program such as Ohio STABLE. The treatment variable is therefore state program launch. The estimand is the reduced-form effect of gaining practical access to an ABLE account in the state program environment that existed during the rollout.

A. Baseline and Triple-Difference Specifications

The baseline analysis restricts the sample to adults with disabilities and compares outcomes before and after state ABLE launch, absorbing permanent differences across states and common shocks across years. We estimate

$$Y_{st} = \alpha_s + \delta_t + \beta\, ABLE_{st} + \varepsilon_{st}. \tag{2}$$

The outcome Y_{st} is measured for adults with disabilities in state s and year t. The main outcome is log disposable income, and the same specification is used for mean disposable income, transfer share, aggregate earned income, aggregate transfer income, and aggregate disposable income. The variable $ABLE_{st}$ equals one once a state has launched an ABLE program. The terms α_s and δ_t denote state and year fixed effects. Observations are weighted by the relevant state population, and standard errors are clustered at the state level.

The coefficient β identifies the change in outcomes for adults with disabilities after state launch relative to never-adopting states and not-yet-adopting states. The identifying assumption is that, absent ABLE launch, outcomes for adults with disabilities in launching states would have followed the same trend as outcomes in the comparison states. This assumption is evaluated using event-study leads and placebo launch dates in the robustness section.

Two features of the rollout make inference important. First, only a small number of states remain never-adopters over the sample window, so conventional state-clustered inference may be fragile. We therefore report wild-cluster bootstrap p-values using 999 Rademacher replications for the main disposable-income estimates. Second, treatment timing is staggered, and treatment effects may vary across launch cohorts. The two-way fixed effects coefficient is therefore reported alongside modern staggered-adoption estimators, including Callaway and Sant'Anna (2021), Sun and Abraham (2021), Borusyak, Jaravel, and Spiess (2024), and de Chaisemartin and D'Haultfœuille (2020). These estimators are used to assess whether the baseline estimate is driven by comparisons that are problematic under heterogeneous treatment effects.

The baseline design compares launching and non-launching states within the population of adults with disabilities. A stronger design uses adults without disabilities as an in-state comparison group. Adults without disabilities are not eligible for ABLE, but they are exposed to the same state-year labor-market conditions, fiscal environment, and policy shocks as adults with disabilities in the same state.

We estimate

$$Y_{sdt} = \alpha_{sd} + \delta_{dt} + \gamma_{st} + \beta\,(D_d \times ABLE_{st}) + \varepsilon_{sdt}. \tag{3}$$

The index d denotes disability status. D_d equals one for adults with disabilities. The fixed effect α_{sd} absorbs permanent state differences by disability status. The fixed effect δ_{dt} absorbs national year shocks that differ between adults with and without disabilities. The fixed effect γ_{st} absorbs all state-year shocks common to both groups, including state labor-market changes and policy changes that affect all residents. The coefficient β is identified from changes in the disability gap within a state-year after ABLE launch.

This design changes the identifying assumption. It no longer requires launching and non-launching states to have parallel outcome trends in levels for adults with disabilities. It requires the disability gap to have evolved similarly absent launch. Any state-year shock that affects adults with and without disabilities equally is absorbed. The remaining threat is a state-year shock correlated with ABLE launch that differentially affects adults with disabilities relative to adults without disabilities. The placebo estimates for adults without disabilities and the triple-difference results are used to assess this concern.

The baseline design requires parallel trends in outcomes for adults with disabilities across launching and comparison states. The triple difference relaxes this requirement by allowing arbitrary state-year shocks common to adults with and without disabilities. Across the two designs, the identifying variation moves from state launch timing to within-state disability contrasts. The SIPP heterogeneity analysis in Section VI further narrows identification by isolating the subgroup of adults with disabilities for whom the SSI resource cap is actually binding.

Selective migration is a potential concern because adults with disabilities could move across states in response to ABLE programs. The draft reports that ACS migration rates for adults with disabilities are below 2 percent per year and lower than migration rates for adults without disabilities. This makes migration unlikely to be the main driver of the estimates, although it cannot be ruled out completely in a state-year design.

Cross-border account access changes the interpretation of treatment exposure. Because some state programs accepted non-residents, residents of non-launching states could obtain the federal resource exclusion before their home state launched. This exposure contaminates the control group. The direction of bias is toward zero because some control-state residents receive partial treatment. The robustness section therefore estimates specifications that drop Ohio and its partner states, restrict to later launch cohorts, absorb post-2016 movement in never-adopter states, and exploit state tax-deduction incentives that affect the value of home-state enrollment. These checks evaluate whether cross-border access attenuates the reduced form rather than generating the estimated effects.

The empirical strategy therefore identifies the effect of practical access to ABLE under real rollout conditions. It does not estimate the effect of universal take-up or the treatment-on-the-treated effect for account holders. The estimates are reduced-form effects of state launch, with cross-border access and incomplete take-up incorporated into the policy environment. This is the relevant parameter for evaluating the rollout as implemented, and it is conservative for the pure asset-test-relief effect if cross-border access moved comparison states toward treatment.

VI. Main Results

A. Labor Supply and Disposable Income

The disposable-income gain is accompanied by higher labor supply. In the ACS state-year panel for adults with disabilities, ABLE launches increase employment by 0.67 percentage points and labor-force participation by 0.82 percentage points. Log wage income rises by 7.70 percent. Poverty does not change detectably. These estimates indicate that the disposable-income response is not only a reclassification of transfer income. It coincides with higher work activity among adults with disabilities. Table 2 Panel B reports the ACS two-way fixed-effects estimates.

The extensive-margin effects are modest in levels but meaningful relative to baseline labor-force attachment. Adults with disabilities have persistently lower employment and participation rates than adults without disabilities. Against a disability labor-force participation rate of roughly one-third, a 0.82 percentage-point increase corresponds to a relative gain of about 2.5 percent. The estimated labor-supply response is therefore economically relevant for a population with low baseline attachment to work.

The within-state disability contrast produces smaller but consistent estimates. When adults without disabilities are used as the comparison group, ABLE launch increases employment of adults with disabilities by 0.34 percentage points and labor-force participation by 0.56 percentage points. The log wage-income estimate falls to 3.86 percent and is imprecise. Table 2 Panel C reports the triple-difference estimates. Launching states appear to have experienced some broader labor-market improvement, but the disability-specific component remains positive on the extensive margins of work.

The wage-income estimate should be interpreted with care. The ACS wage-income measure is observed among workers and can reflect both intensive-margin changes among incumbent workers and selection into employment. ABLE could raise wage income because existing workers increase hours, because new workers enter employment, or because the composition of employed adults with disabilities changes. The state-year design cannot separate these channels. The estimate nevertheless moves in the same direction as employment, participation, and disposable income, which supports the interpretation that asset-test relief increased labor-market resources.

The labor-supply response is consistent with the policy margin. ABLE does not change the SSDI earnings ceiling, the SSI benefit formula, or the medical definition of disability. It changes whether eligible individuals can accumulate assets without triggering the SSI or Medicaid resource test. The employment and participation gains therefore indicate that the asset constraint affected the value of working and saving for at least some adults with disabilities.

ABLE increased disposable income for adults with disabilities. After state program launch, disposable income rose by 1.49 percent, or about 321 dollars per adult with a disability per year.

Aggregated across state disability populations, the estimate implies 1.59 billion dollars in additional annual disposable income. The gain is economically meaningful because ABLE did not change income eligibility, benefit levels, or the medical definition of disability. It relaxed the asset constraint while leaving the other program rules fixed.

The composition of the income response is consistent with an asset-margin mechanism. Aggregate earned income rises by 1.46 billion dollars, while aggregate transfer income does not increase. The transfer share of income falls by 0.53 percentage points. The income gain therefore does not appear to come from higher cash transfers. It operates primarily through earnings, which is the margin predicted by a reform that makes it less costly for adults with disabilities to work and accumulate savings.

The inference is not driven by the small number of never-adopting states. The conventional p-value for the log disposable-income estimate is 0.006, and the wild-cluster bootstrap p-value is 0.007. The agreement between state-clustered inference and wild-cluster bootstrap inference reduces the concern that the estimate reflects small-sample inference failure with few never-adopter clusters.

The staggered-adoption evidence points in the same direction. The Sun and Abraham (2021) interaction-weighted event-study estimates in Appendix Table A3 Panel A confirm the sign and rough magnitude of the two-way fixed-effects coefficient on log disposable income and pass the joint pre-trend chi-square test at conventional levels. The event-time path rises after launch, with the post-treatment effects increasing over event time. This dynamic pattern is consistent with gradual take-up and account accumulation rather than a one-time accounting shift at launch.

The disability-specific estimate remains positive when adults without disabilities are used as the within-state comparison group. In the triple-difference specification, log disposable income rises by 1.05 percent for adults with disabilities relative to adults without disabilities in the same state-year. The transfer share falls by 0.52 percentage points, and aggregate transfers to adults with disabilities fall by approximately 127 million dollars. Because this specification absorbs all state-year shocks common to both groups, it rules out state labor-market changes or state fiscal conditions that moved all residents in launching states as the sole explanation for the income gain.

The difference between the baseline and triple-difference magnitudes is informative. The baseline estimate captures the full change for adults with disabilities in launching states. The triple difference nets out contemporaneous changes among adults without disabilities in the same state. Its smaller magnitude suggests that some launching states experienced broader income growth during the rollout period, but that a disability-specific component remains after those state-year changes are absorbed.

The SIPP individual-level panel adds direct evidence on financial-independence outcomes for adults with disabilities. Table 2 Panel C reports the two-way fixed-effects estimates for the disability sub-sample on eight financial-account outcomes. None of the eight outcomes are individually significant at the disability-only level (N = 2,579), consistent with a state-year secular decline in 401(k) and IRA balances that affects both treated and control states. The triple difference in Table 2 Panel D nets out the state-year secular trend by adding the disability-versus-non-disability comparison, and the results show that ABLE launch raises the probability of holding a 401(k) by 6.5 percentage points, log bank balance by 0.64 log points, and log 401(k) value by 0.61 log points. The intensive-margin estimates on log bank balance and log 401(k) value require additional caution because the Sun-Abraham event studies on those outcomes fail the joint pre-trend chi-square (Appendix Table A3 Panel B). The extensive-margin estimate on 401(k) participation passes the pre-trend test at the 10 percent level, as do the estimates on IRA participation and log IRA value. Section VI.C returns to the interpretation of the SIPP evidence through the subgroup heterogeneity design.

B. Program Enrollment and Health Outcomes

The state-year SSA evidence does not identify a reduction in disability-insurance enrollment. In the baseline specification, ABLE launches are associated with a decline in OASDI disabled-worker counts. The estimated coefficient is approximately 1.7 percent for both OASDI disabled-worker and total OASDI counts, and 1.3 percent for SSI counts. These baseline estimates are not sufficient for a causal interpretation because the identifying assumptions fail on this outcome.

The pre-trend evidence explains why the disability-insurance result should not be interpreted as causal. Launcher states were already on a differential trajectory before ABLE accounts existed. The event-study estimates for disabled-worker counts are positive and statistically significant in several pre-treatment years. The plotted treated and comparison series show the same problem. Launcher states and never-adopter states both rise before 2015 and decline after 2016, with similar timing. The decline in disability-insurance enrollment therefore coincides with ABLE rollout, but it is also part of a broader state and national pattern that predates treatment.

Trend adjustments eliminate the disability-insurance result. When state-specific pre-2016 trends are removed, the coefficient on log disabled-worker count is no longer distinguishable from zero. When state-specific linear trends are absorbed over the full sample, the coefficient becomes small and positive. These specifications reject the interpretation that the state-year design identifies a causal effect of ABLE launch on OASDI enrollment.

The paper therefore should not claim that ABLE reduced disability-insurance participation. This boundary is important because it separates the identified income and labor-supply responses from a weaker enrollment channel. ABLE may have changed disability-program entry or exit for some individuals, but the state-year design cannot identify that margin. Individual-level administrative data linking account access, earnings, and benefit receipt would be needed to estimate disability-insurance entry and exit responses.

The implication is that the main economic response operates through earnings and disposable income, not through a separately identified reduction in disability-insurance rolls. The SSA panel is useful precisely because it rules out an overinterpretation of the reduced form. The evidence supports a labor-market and income response to asset-test relief, while leaving disability-insurance substitution unidentified.

The ACS program-receipt panel helps interpret the decline in transfer share. A lower transfer share could arise because adults with disabilities leave transfer programs, because they move across programs, or because earnings rise while transfer receipt remains largely unchanged. Distinguishing these possibilities matters for the fiscal interpretation of ABLE.

The program-specific estimates do not show a large reallocation across safety-net programs. The point estimates on SNAP receipt, Medicaid receipt, SSI receipt, and cash-welfare receipt are individually small and imprecisely estimated at the state-year level, and none of them accounts for a substantial share of the CPS aggregate disposable-income response.

These estimates indicate that the decline in transfer share is not driven by a large discrete reduction in a single program. Instead, the transfer share falls because disposable income and earned income rise while transfer income changes little. This interpretation is consistent with the CPS estimates, where aggregate earned income accounts for most of the disposable-income gain and aggregate transfer income is essentially flat.

The state-year design has limited power to detect small program-specific changes. The negative SSI point estimate is consistent with asset-test relief reducing reliance on SSI for some adults with disabilities, but the confidence interval is too wide to support a separate claim about SSI receipt. The evidence therefore should not be read as proving that program receipt is unchanged. It shows that any program-specific response is too small, relative to state-year variation, to account for the main disposable-income gain.

The program-reallocation results also discipline the fiscal interpretation. ABLE does not appear to increase transfer dependence in the state-year data. Nor does the design identify a large fiscal offset through lower disability-insurance enrollment. The most defensible interpretation is narrower and stronger. ABLE raises disposable income primarily through labor earnings, while program receipt changes are small and imprecisely estimated at the state-year level.

The BRFSS state-year panel offers a health-outcome check that mirrors the CPS and ACS specifications. Table 3 Panel B reports the two-way fixed-effects, triple-difference, and Sun-Abraham post-launch estimates for mental unhealthy days, physical unhealthy days, poor health days, and insurance coverage on the disability-stratified sample restricted to 2013 to 2023. Poor health days fall by 0.75 days on the pooled TWFE, and insurance coverage in the triple difference falls by 1.2 percentage points relative to adults without disabilities. All four outcomes fail the Sun and Abraham (2021) joint pre-trend chi-square, and the Rambachan and Roth (2023) HonestDiD sensitivity interval widens through zero under relative-magnitudes bounds at unity. The BRFSS results are therefore reported as a distal outcomes check alongside SSA program enrollment (Table 3 Panel A) that does not carry the same identification weight as the labor-supply, disposable-income, and financial-independence results in Table 2, consistent with health as a distal outcome of the asset-test channel.

Taken together, the main results show that ABLE launches increased labor supply and disposable income for adults with disabilities. The strongest evidence comes from the CPS and ACS panels, where employment, participation, wage income, and disposable income all move in the direction predicted by asset-test relief. The SSA and program-receipt panels clarify the mechanism by showing what the estimates do not identify. The income gains do not require a large reduction in disability-insurance rolls or a major reallocation across transfer programs. The BRFSS health estimates fail the pre-trend diagnostics and are reserved for the appendix.

C. Mechanism Tests

Where does the response concentrate? The Conceptual Framework in Section III makes two testable predictions. The ABLE resource exclusion binds only for adults whose baseline household savings sit near the 2,000-dollar SSI cap (Prediction 2), and the value of the exclusion depends on whether Medicaid has already been decoupled from SSI-linked eligibility through state expansion (Prediction 3). If the mechanism is the relaxation of the cap, the response should concentrate along both margins. Table 4 combines both tests. Panel A reports the Medicaid interaction. Panel B reports the SIPP subgroup heterogeneity.

Panel A interacts the ABLE-launch indicator with the state Medicaid expansion indicator across income, labor supply, and disability-insurance outcomes. In non-expansion states, ABLE launch raises log disposable income by 2.95 percent, employment by 1.03 percentage points, labor-force participation by 0.87 percentage points, and log wage income by 9.05 percent. The interaction between ABLE and Medicaid expansion is negative and statistically significant on log disposable income (interaction coefficient -2.36 percentage points, standard error 1.26), and the implied net effect in expansion states is 0.59 percent, statistically indistinguishable from zero. The corresponding net-of-interaction effect on log OASDI total is -2.38 percent in expansion states. This pattern is consistent with substitution between the two policies at the SSI 2,000-dollar cliff. Medicaid expansion decouples health-insurance coverage from SSI eligibility, so the notch at the cliff is smaller in expansion states, and the incremental value of the ABLE resource exclusion is correspondingly smaller.

Table 4 Panel B tests Prediction 2 by restricting the SIPP triple-difference specification to five policy-relevant subgroups. Column (1) restricts to adults with disabilities whose baseline household net worth is below the 2,000-dollar cap. Column (2) stratifies by education. Column (3) restricts to adults with disabilities in historically underserved race and ethnicity groups. Column (4) isolates residents of the eleven Ohio STABLE partner states. Column (5) restricts to adults with disabilities in the age band where the pre-age-26 onset rule is most likely to bind through documented eligibility. The results in Table 4 Panel B column (1) are the largest concentration of statistically significant financial-account responses in the paper. Among adults binding on the SSI cap, the probability of holding an Individual Retirement Account rises 5.9 percentage points, log bank balances rise 1.26 log points, and log IRA balances rise 0.52 log points. The remaining columns show that the response also concentrates among adults with a high school education or less (column 2), among adults with disabilities in historically underserved race and ethnicity groups (column 3), and among residents of Ohio STABLE partner states (column 4). The pre-age-26 onset subgroup in column (5) shows a large log-bank response consistent with earned income routed into a protected account. The pattern that the strongest response appears in exactly the wealth strata for whom the SSI resource limit binds is the strongest evidence in the paper that the mechanism operates through the asset-test channel and not through a general labor-market spillover. Appendix Table A6 reports the full 22-dimension summary and confirms that 21 of the 22 dimensions contain at least one statistically significant financial-account coefficient.

VII. Robustness

The main threat to the state-launch design is that ABLE adoption may coincide with other changes in state income or labor-market conditions. The tests in this section evaluate that concern using four sources of evidence. Outcomes for adults without disabilities, placebo launch dates, alternative control groups and staggered-adoption estimators, and the cross-border enrollment channel created by Ohio's STABLE platform.

Adults without disabilities provide a direct placebo group because they were not eligible for ABLE, while they were exposed to the same state-year economic conditions as adults with disabilities. If the main disposable-income estimate reflected general income growth in launching states, a comparable effect should appear among adults without disabilities. It does not. In the non-disabled sample, the estimated effect of state ABLE launch on log disposable income is 0.44 percent and statistically indistinguishable from zero, while the transfer-share estimate is essentially zero.

The aggregate-income placebo is less informative because adults without disabilities are a much larger population, so small changes in state labor-market conditions can generate large aggregate dollar movements. The relevant placebo for the main estimate is the individual disposable-income measure, and that estimate is null. The absence of an individual income response among ineligible adults supports the interpretation that the main effect is specific to the population exposed to ABLE's asset-test relief.

Placebo launch dates lead to the same conclusion. Assigning each state a launch date five years before its actual program launch and estimating the model in the pre-treatment period produces no evidence of a disposable-income response. The placebo estimate on log disposable income is 0.16 percent and statistically indistinguishable from zero. The timing test therefore rules out the simplest pre-trend explanation. Launcher states were not already experiencing the same income gain before ABLE accounts became available.

Together, the two placebo tests narrow the alternative interpretation. The income gain does not appear among adults who were ineligible for ABLE, and it does not appear when treatment is assigned to years before state programs existed.

The disposable-income estimate is not driven by a single early-launch state or by the coding of the small never-adopter group. Dropping Ohio, whose STABLE platform later hosted multiple partner-state programs, yields an estimate of 1.66 percent, close to the baseline. Restricting the comparison group to the three verified never-adopters, rather than including Nevada with partially verified launch status, yields an estimate of 1.32 percent. Both estimates remain positive and statistically significant.

The result is also present across launch cohorts. First-wave 2016 states have a larger estimated effect of 5.19 percent, while later launch cohorts have an estimated effect of 1.79 percent. The difference across cohorts is consistent with differences in program maturity, outreach, platform availability, and take-up, but the sign of the effect is not confined to the first launch states. Later launchers also show positive disposable-income gains.

Staggered adoption raises a separate econometric concern because two-way fixed effects estimators can be biased when treatment effects vary across cohorts. The estimator battery addresses this issue directly. The Callaway and Sant'Anna, Sun and Abraham, Borusyak, Jaravel, and Spiess, and de Chaisemartin and D'Haultfœuille estimators all preserve the sign and order of magnitude of the baseline result. The disposable-income estimate therefore does not appear to reflect the staggered-treatment bias that would arise if already-treated states served as comparisons for later-treated states.

Cross-border enrollment weakens the treatment contrast rather than creating a spurious positive effect. Ohio's STABLE Account accepted non-residents beginning in 2016, allowing some residents of states without their own ABLE program to obtain the federal resource exclusion before home-state launch. In a design that codes treatment by home-state launch, those residents are partly treated while still appearing in the comparison group. This misclassification attenuates the estimated effect toward zero.

The robustness estimates are consistent with attenuation. Dropping Ohio and the eleven verified STABLE partner states leaves the log disposable-income estimate at 1.43 percent, close to the baseline estimate. Restricting identification to states that launched in 2018 or later, after cross-border enrollment had already been available for at least two years, produces a larger estimate of 2.56 percent. Absorbing any separate post-2016 movement in never-adopter states leaves the estimate unchanged at 1.55 percent. These estimates place the baseline within a narrow range of positive effects under alternative exposure definitions.

State tax deductions provide a further test of the cross-border channel. In states without an ABLE tax deduction, home-state launch is associated with a 3.16 percent increase in disposable income. In states with a deduction, the launch effect is substantially smaller, as shown by the negative interaction between launch and the deduction indicator. This pattern is consistent with partial cross-border exposure before formal home-state launch. Where residents had already accessed asset-test relief through another state's platform, the incremental effect of home-state launch was smaller.

The important implication is that cross-border access makes the baseline estimate conservative. It moves some comparison-state residents toward treatment before their home state launches, reducing the treated-control contrast. The positive estimates that remain after excluding Ohio and partner states, focusing on later launch cohorts, and absorbing never-adopter post-2016 movement all support the same interpretation. Cross-border enrollment complicates exposure measurement but does not explain away the disposable-income effect.

The county-border exercise provides corroborating evidence that cross-border access was real but limited. If residents of never-adopter states used nearby launcher-state programs, the response should be stronger in never-adopter counties that border launcher states than in interior counties of the same states. This comparison holds fixed the never-adopter state environment while allowing proximity to launcher-state outreach, service providers, and financial institutions to vary across counties.

The pattern is consistent with modest cross-border exposure. Within never-adopter states, border-adjacent counties experience a poverty reduction of about 0.5 to 0.6 percentage points after 2016 relative to interior counties, while the disability-rate placebo does not move. The uninsured-rate result is less stable and appears sensitive to a pre-period outlier, so we would not emphasize it. The useful evidence from this exercise is narrower. Border proximity predicts a poverty response in never-adopter states, which is consistent with limited cross-border take-up.

This evidence should be presented as auxiliary rather than central. It comes entirely from within never-adopter states and therefore removes state-level policy changes, but it is less directly tied to the main disposable-income outcome than the CPS and ACS state-year panels. Its value is that it corroborates the direction of the cross-border argument. Cross-border enrollment existed, but its effect was partial and attenuating, not large enough to account for the main income result.

Taken together, the robustness evidence supports the interpretation of the main estimates as a reduced-form response to practical ABLE access. The disposable-income gain does not appear among ineligible adults, does not appear under placebo timing, survives alternative control-group definitions, remains positive under staggered-adoption estimators, and is not driven by Ohio or its partner-state platform. Cross-border access complicates treatment assignment, but its bias works against finding an effect. The baseline estimate should therefore be read as a conservative measure of the household economic response to asset-test relief.

The staggered-adoption literature has developed a set of estimators and diagnostics that address the bias two-way fixed-effects specifications can introduce when treatment effects vary across cohorts or over event time (Goodman-Bacon 2021, de Chaisemartin and D'Haultfœuille 2020, Callaway and Sant'Anna 2021, Sun and Abraham 2021, Borusyak, Jaravel, and Spiess 2024, Rambachan and Roth 2023). We apply this full toolkit to the twelve primary outcomes and report the results in Appendix Tables A3 through A5 and in Table 5 Panel D.

The Sun and Abraham (2021) interaction-weighted event-study estimator reported in Appendix Table A3 Panel A confirms the sign and magnitude of the two-way fixed-effects coefficients for the seven pre-trend-clean primary outcomes, and the joint chi-square test on the four pre-treatment coefficients passes at the ten percent level for log disposable income, transfer share, employment, labor-force participation, log wage income, log OASDI total, and log SSI total. The Callaway and Sant'Anna (2021) doubly-robust estimator reported in Appendix Table A3 Panel B falls within one standard error of the Sun-Abraham estimates for the same seven outcomes. HonestDiD sensitivity analysis for the five outcomes that fail the joint pre-trend chi-square is reported in Appendix Table A5 Panel A. The confidence interval for log OASDI disabled-worker count includes zero at relative-magnitudes bounds through unity, and the BRFSS outcome intervals widen substantially over the same range. Under any bound at $\overline{M} \geq 1.0$, none of the five pre-trend-fail outcomes admits a signed conclusion, which is the identification reason those outcomes appear in the appendix figure rather than in the main text.

The Goodman-Bacon (2021) decomposition in Table 5 Panel D reports the fraction of two-way fixed-effects weight attributable to each of four two-period two-group comparisons for the three primary outcomes. The forbidden late-versus-early-treated block receives 16 to 24 percent of the weight, and the Sun-Abraham estimates confirm the sign and rough magnitude of the two-way fixed-effects coefficients, so the weight structure does not overturn the primary result. Taken together, the modern staggered-adoption estimators reinforce the primary labor-supply and disposable-income findings and identify the health-outcome null as the appropriate boundary of the paper's causal claim.

VIII. Welfare and Aggregate Response

The welfare relevance of ABLE depends on whether asset-test relief raises household resources without generating offsetting increases in transfer spending, and the estimates point to that pattern. ABLE increased disposable income, employment, labor-force participation, and wage income for adults with disabilities, while the state-year data do not identify an increase in receipt of SSI, SNAP, Medicaid, or disability-insurance benefits. The policy therefore differs from a conventional transfer expansion because it raises measured resources primarily by changing the value of work and saving under existing program rules.

The benefit side is measured by the reduced-form increase in disposable income. After state program launch, disposable income for adults with disabilities rose by 1.49 percent, or about 321 dollars per person per year. Aggregated across the working-age adult disability population in the sample window, the estimate implies 1.59 billion dollars in additional annual disposable income. The composition of this gain is central to the interpretation. Aggregate earned income rises by 1.46 billion dollars, while aggregate transfer income does not increase detectably, indicating that the resource gain operates primarily through labor-market income rather than through higher cash transfers.

The fiscal evidence is more limited, but it does not overturn this interpretation. ABLE excludes qualifying account balances from the SSI and Medicaid asset tests, yet the state-year estimates do not show a detectable increase in SSI, SNAP, Medicaid, or disability-insurance receipt. The disability-insurance enrollment results also do not identify a causal decline in program rolls once pre-existing trends are taken into account. The appropriate conclusion is therefore not that ABLE finances itself through lower disability-insurance enrollment. It is that the measured household gains arise without a detectable increase in transfer-program participation at the state-year level.

The direct public cost of ABLE is modest relative to the measured income gain because the policy created a tax-favored account and an asset-test exclusion rather than a new cash benefit. Public costs arise through state tax deductions for ABLE contributions, foregone federal and state revenue on tax-favored account earnings, and program administration by state treasurers and account platforms. These costs are not zero, but take-up remained low during the sample period, annual contributions are capped, and average balances remain modest relative to the statutory exclusion. Against those costs, the estimated annual disposable-income gain is large.

This accounting should be interpreted as reduced-form welfare evidence rather than as a complete social-welfare calculation. Disposable income is not the same object as willingness to pay, and the estimates do not value the insurance benefit of holding precautionary savings, the reduced risk of violating an asset test, or the nonpecuniary value of greater financial autonomy. The estimates also do not recover all administrative costs, tax-expenditure costs, or long-run fiscal effects that would require individual-level administrative data. What the state-year evidence shows is narrower. ABLE generated measurable household resource gains, largely through earnings, without a corresponding increase in transfer receipt that is detectable in the available data.

The distributional pattern strengthens the economic relevance of the estimates. The gains accrue to adults with disabilities, a population with low baseline employment, low liquid savings, and high exposure to means-tested program rules. The largest financial-account responses appear among adults with disabilities whose baseline household net worth is below the 2,000-dollar SSI resource cap. For this group, the probability of holding an Individual Retirement Account rises 5.9 percentage points, log bank balances rise 1.26 log points, and log IRA balances rise 0.52 log points. The concentration among the lowest-wealth adults with disabilities indicates that asset-test relief is most valuable exactly where the constraint binds.

The 2026 ABLE Age Adjustment Act changes the scale of the policy but not the interpretation of the current estimates. Raising the age-of-onset threshold from 26 to 46 will extend eligibility to roughly six million additional adults, increasing the population exposed to asset-test relief. The per-person response, however, need not match the estimates in this paper. Newly eligible adults will differ in age, labor-market attachment, and program-participation history from the sample studied here, and the fraction of the newly eligible population whose baseline savings bind the SSI 2,000-dollar cap is not known ex ante. The evidence therefore supports extrapolating the direction of the response to the expanded population, not mechanically applying the current magnitude to the full expansion population.

The Medicaid interaction also matters for extrapolation. ABLE has larger disposable-income effects in states that had not expanded Medicaid, while the estimated effect is small and statistically indistinguishable from zero in Medicaid expansion states. Future gains from ABLE expansion may therefore depend on the insurance environment already facing adults with disabilities. Where Medicaid eligibility remains more tightly linked to disability-program rules, asset-test relief may have larger income effects. Where Medicaid expansion has already weakened that link, the incremental gain from ABLE may be smaller.

The estimates therefore identify an asset-margin response, not a disability-insurance substitution effect. ABLE relaxed the resource constraint while leaving benefit formulas, income eligibility, and disability definitions unchanged. In that setting, disposable income and labor supply increased without a detectable increase in transfer receipt in the state-year data. The result is that a change in the asset rules alone can alter household economic behavior in a stable-eligibility disability population.

The nine-state descriptive dataset described in Section IV provides a direct read on where current program participation sits relative to the eligible population and clarifies the size of the potential welfare gain from expanded enrollment. Take-up in the nine states with credible account counts ranges from 0.19 percent in Texas to 3.04 percent in Nebraska, with a median of 0.41 percent (Appendix Table A7). Five of the nine states have take-up below 1 percent of their adult-with-disabilities population aged 18 to 64. Current national program participation is therefore low relative to the eligible population, even in the states that publish data.

The projection follows directly from the reduced-form estimates. The 1.6-billion-dollar aggregate annual disposable-income gain reported in Section VI holds at current participation levels. If take-up in the nine states with data increased to the highest observed rate (3.04 percent, Nebraska), the state-level aggregate income gain for these nine states would rise roughly seven-fold, from a weighted-mean take-up of about 0.5 percent to 3 percent. Applied to the national eligible population of about 8.1 million adults with disabilities, a shift from the current national participation rate of about 2.9 percent (National Disability Institute) to a rate matching Nebraska's 3.04 percent implies a proportional increase in aggregate disposable-income gains. Applied to the six million adults newly eligible under the 2026 ABLE Age Adjustment Act, the projection depends on how quickly participation reaches the levels observed in states like Nebraska and Virginia. The nine-state data therefore serve two functions in this section. They provide the only publicly available benchmark for current enrollment relative to disability population, and they support the direct projection that expanded participation would translate the paper's reduced-form estimates into materially larger aggregate welfare improvements.

IX. Conclusion

The ABLE setting clarifies how a resource limit can operate as a labor-supply incentive even when the rule is formally written as an asset test. In disability programs, earnings that accumulate into countable savings can threaten eligibility for cash assistance and health insurance, so the asset limit changes the value of work as well as the value of saving. ABLE changed this margin by allowing eligible adults with disabilities to hold protected savings, while leaving benefit formulas, income eligibility, work rules, and disability definitions otherwise unchanged. The resulting variation isolates whether changing the treatment of assets alone can alter household economic behavior in a stable-eligibility disability population.

Relaxing the asset constraint through ABLE raised household resources, with the gain operating primarily through earnings rather than larger transfer payments. State ABLE launches raised disposable income for adults with disabilities by 1.49 percent, implying 1.59 billion dollars in additional annual disposable income. Employment, labor-force participation, and wage income also increased, while aggregate transfer income did not rise detectably. The evidence does not support a separate claim that ABLE reduced disability-insurance enrollment, since those estimates fail the pre-trend diagnostics and disappear once state-specific trends are absorbed. The identified effect is therefore not a substitution away from disability insurance, but a response to a change in the asset rules governing whether earnings can be safely retained.

This distinction changes how the earlier asset-test evidence should be interpreted. Prior studies of welfare and Medicaid reforms often found modest behavioral responses, but those reforms generally changed several program parameters at once or operated in populations with shorter expected program attachment. ABLE operates in a disability setting where the resource constraint can bind over a long horizon and where the benefits at risk include both cash assistance and health insurance. The estimates suggest that asset-test relief can generate labor-market responses when the constraint is durable, salient, and tied to benefits that households are reluctant to jeopardize.

The same evidence also clarifies why asset rules cannot be separated from disability financial inclusion. Expanding bank access or financial education may have limited effects when program rules continue to penalize even modest saving. ABLE changes the constraint directly by allowing eligible adults to convert earnings into protected assets. The baseline-wealth evidence reinforces this interpretation because the largest financial-account responses appear among adults with disabilities whose baseline household net worth was below the SSI 2,000-dollar cap, the population for whom the constraint has behavioral bite. Asset-test relief therefore appears most powerful when it reaches households whose savings margin is actively bound.

The 2026 Age Adjustment Act extends this margin to a much larger population. Raising the age-of-onset threshold from 26 to 46 will increase the number of adults exposed to ABLE, so aggregate household resources should rise if newly eligible adults respond in the same direction. The current evidence supports extrapolating the direction of the response to the expanded population, with the caveat that newly eligible adults will differ in age, labor-market attachment, and program history from the population studied here.

The broader implication is that resource rules are part of the labor-supply incentives embedded in means-tested policy. In the ABLE setting, the reform did not change transfer generosity, income disregards, disability definitions, or work requirements. It changed whether earnings could be converted into precautionary savings without threatening eligibility for cash assistance and health insurance. The estimated increases in disposable income and work activity therefore show that the asset rule itself can affect behavior, especially in a disability population for whom program attachment may be long and the benefits at risk are large. For adults with disabilities, safe saving is not only a balance-sheet outcome. It changes the effective return to work by reducing the risk that earnings accumulated today will become a loss of benefits tomorrow.

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Tables

Table 1: Summary statistics, pre-treatment period, adults with disabilities

	Ever-adopter states		Never-adopter states	
Outcome or covariate	Mean	SD	Mean	SD
Panel A. CPS income outcomes				
Log disposable income	9.925	0.280	9.996	0.246
Transfer share of income	0.357	0.356	0.320	0.342
Panel B. ACS labor outcomes				
Employment rate	0.361	0.061	0.443	0.059
Labor-force participation	0.411	0.060	0.493	0.044
Log wage income	4.035	0.645	4.773	0.489
Panel C. BRFSS health outcomes				
Mental unhealthy days (last 30)	16.564	1.273	—	—
Physical unhealthy days (last 30)	17.217	0.878	—	—
Poor health days (last 30)	17.276	1.083	—	—
Insurance coverage rate	0.887	0.038	—	—
Panel D. SSA program enrollment				
Log OASDI disabled-worker count	11.651	1.050	10.620	1.043
Log OASDI total count	13.590	1.017	12.695	0.945
Log SSI total count	11.458	1.156	10.284	1.110
Panel E. SIPP financial outcomes				
Any bank account (share)	0.782	0.413	0.789	0.408
Any IRA (share)	0.106	0.308	0.089	0.284
Any 401(k) (share)	0.167	0.373	0.148	0.355
Log bank balance (| positive)	5.053	3.626	5.104	3.578
Log net worth (| positive)	8.800	5.058	8.925	4.982
Panel F. Demographics				
Age	49.45	12.17	48.10	12.40
Female share	0.494	0.500	0.508	0.500
White non-Hispanic share	0.688	0.463	0.752	0.432
Some college or more share	0.518	0.500	0.532	0.499

Notes. Pre-treatment period is 2010 to 2015 for CPS, ACS, SSA. BRFSS statistics use the 2013 to 2015 disability-stratified six-item-battery window. SIPP statistics use the 2014 panel wave 1 baseline. Ever-adopter states are the 47 jurisdictions that launched an ABLE program between 2016 and 2023. Never-adopter states are Nevada, North Dakota, South Dakota, and Wisconsin. BRFSS never-adopter cells are omitted because the four never-adopter states have insufficient disability-stratified BRFSS cell counts in the pre-treatment window. All statistics are population-weighted. Sources are Current Population Survey ASEC, American Community Survey via IPUMS, Behavioral Risk Factor Surveillance System, Social Security Administration state-year OASDI and SSI counts, and Survey of Income and Program Participation 2014 panel.

Table 2: Effect of ABLE launches on primary outcomes, adults with disabilities

	(1)	(2)	(3)	(4)	(5)
Panel A. CPS TWFE. income outcomes (adults with disabilities, N = 1,632)					
	Log disp. income		Transfer share		
ABLE launch	0.0149***		−0.0053*		
	(0.0051)		(0.0030)		
Panel B. ACS TWFE. labor-supply outcomes (adults with disabilities, N = 1,224)					
	Employ-ment	Labor-force particip.	Log wage income		
ABLE launch	0.0067***	0.0082***	0.0770***		
	(0.0022)	(0.0025)	(0.0285)		
Panel C. SIPP TWFE. financial-independence outcomes (adults with disabilities, N = 2,579)					
	Any 401(k)	Log bank balance	Log 401(k) value	Log net worth	Any pos. net worth
ABLE launch	−0.007	−0.124	0.003	−0.356	−0.015
	(0.034)	(0.351)	(0.314)	(0.526)	(0.042)
Panel D. Triple difference. D × ABLE × Post across all three domains					
	Log disp. income	Employ-ment	Log wage income	Any 401(k)	Log bank balance
D × ABLE × Post	0.0105**	0.0034*	0.0386	0.065***	0.637**
	(0.0049)	(0.0020)	(0.0240)	(0.021)	(0.253)
Observations	2,448	1,836	1,836	29,527	29,527

Notes. Panels A and B use state-year cell panels aggregated from CPS ASEC 2008 to 2023 and ACS 2011 to 2022. Panel C uses SIPP 2014 individual-level panel restricted to adults with disabilities across 16 quarterly waves. Panel D reports the triple difference D × ABLE × Post using adults without disabilities as within-state comparison group. All specifications include state and year fixed effects with standard errors clustered at the state. Panel A implies a 1.5 percent increase in disposable income, or 321 dollars per adult with a disability per year, aggregating to 1.59 billion dollars annually. Panel B implies employment and labor-force participation gains of about 2.5 percent relative to baseline attachment. Panel C shows no significant TWFE effect on financial-account outcomes at the disability-only level, and Panel D shows the triple difference on financial outcomes is significant on 401(k) participation and bank balances. Sources are the CPS ASEC 2008 to 2023, ACS 2011 to 2022, and SIPP 2014 panel wave 1 through wave 16. Significance levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of ABLE launches on distal outcomes. disability program enrollment (Panel A) and health (Panel B)

Panel A. SSA program enrollment (state-year panel 2010 to 2024, N = 714)

	Log OASDI disabled (1)	Log OASDI total (2)	Log SSI total (3)
A(i). Baseline TWFE			
ABLE launch	−0.0170**	−0.0170*	−0.0127
	(0.0081)	(0.0093)	(0.0137)
A(ii). Pre-2016 state-specific linear detrend			
ABLE launch	−0.0073	−0.0069	0.0172
	(0.0125)	(0.0071)	(0.0140)
A(iii). Full-sample state linear trends absorbed via explicit dummies			
ABLE launch	0.0024	0.0009	0.0042
	(0.0033)	(0.0014)	(0.0043)

Panel B. BRFSS health outcomes (state-year 2013 to 2023, disability-stratified)

	Mental unhealthy days (1)	Physical unhealthy days (2)	Poor health days (3)	Insurance coverage (share) (4)
B(i). TWFE on adults with disabilities (N = 604)				
ABLE launch	−0.369	−0.446*	−0.750**	0.002
	(0.364)	(0.249)	(0.328)	(0.007)
B(ii). Triple difference D × ABLE × Post (N = 906)				
D × ABLE × Post	−0.168	−0.255	−0.170	−0.012**
	(0.277)	(0.196)	(0.287)	(0.005)
B(iii). Sun-Abraham post-launch coefficient at $t = 0$				
Post-launch effect	−0.060	−0.542*	−0.295	0.007
	(0.379)	(0.307)	(0.472)	(0.011)
Joint pre-trend $\chi^2(4)$	40.32	46.96	12.92	11.38
p-value	0.000	0.000	0.012	0.023

Notes. Panel A uses SSA state-year OASDI and SSI recipient counts 2010 to 2024. All three sub-panels include state and year fixed effects with state-clustered SEs. Panel A(i) is the baseline TWFE. Panel A(ii) removes a state-specific linear trend fit on pre-2016 years only. Panel A(iii) absorbs state-specific linear trends over the full sample via explicit state × year dummies. Once state-specific pre-treatment trends are removed, none of the OASDI or SSI coefficients are distinguishable from zero. Panel B uses BRFSS state-year cells restricted to the 2013 to 2023 disability-stratified six-item-battery window. Panel B(i) is the TWFE on adults with disabilities. Panel B(ii) uses adults with and without disabilities as separate cells. Panel B(iii) reports the Sun and Abraham (2021) interaction-weighted coefficient at event time zero with the joint pre-trend chi-square test on the four pre-treatment coefficients at $t = -5$ through $t = -2$. All four BRFSS outcomes fail the joint pre-trend test at conventional significance. Neither SSA program enrollment nor BRFSS health outcomes carry the same identification weight as the labor-supply and disposable-income results in Table 2, and the paper does not claim causal effects on these distal outcomes. Sources are SSA state-year OASDI and SSI recipient counts 2010 to 2024 (Panel A) and BRFSS state-year cells 2013 to 2023 restricted to the six-item disability-battery window (Panel B). Significance levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Mechanism tests. Medicaid × ABLE policy interaction (Panel A) and SIPP subgroup heterogeneity (Panel B)

Panel A. Medicaid × ABLE interaction (Prediction 3. substitution at the SSI cliff)

	Log disp. income (1)	Transfer share (2)	Employ-ment (3)	Labor-force (4)	Log wage income (5)	Log OASDI total (6)
ABLE launch (non-expansion)	0.0295***	−0.0081**	0.0103***	0.0087***	0.0905***	−0.0024
	(0.0085)	(0.0035)	(0.0026)	(0.0033)	(0.0291)	(0.0094)
ABLE × Medicaid	−0.0236*	0.0049*	−0.0060	−0.0008	−0.0226	−0.0214**
	(0.0126)	(0.0029)	(0.0039)	(0.0040)	(0.0347)	(0.0102)
Net effect (expansion states)	0.0059	−0.0032	0.0043	0.0079	0.0679	−0.0238
Observations	1,632	1,632	1,224	1,224	1,224	714

Panel B. SIPP heterogeneity by binding-constraint subgroup (Prediction 2)

	Baseline low net worth (1)	HS or less education (2)	Any non-white or Hispanic (3)	STABLE-partner state (4)	Pre-age-26 onset rule (5)
Any bank account	0.008	0.007	0.003	0.020	0.034**
	(0.006)	(0.011)	(0.008)	(0.014)	(0.017)
Any IRA	0.059***	0.030**	0.031	0.017	0.021**
	(0.014)	(0.015)	(0.042)	(0.023)	(0.010)
Any 401(k)	0.033	0.066***	0.079***	0.074***	0.089***
	(0.027)	(0.023)	(0.027)	(0.029)	(0.030)
Log bank balance	1.257***	0.571**	1.114**	1.067***	0.788***
	(0.390)	(0.268)	(0.427)	(0.227)	(0.288)
Log IRA value	0.519***	0.192	0.215	0.123	0.443
	(0.126)	(0.140)	(0.393)	(0.242)	(0.304)
Log 401(k) value	0.268	0.718***	0.980**	0.625**	0.883***
	(0.196)	(0.183)	(0.404)	(0.269)	(0.285)
Log net worth	0.469	0.461	2.191**	0.851**	0.753
	(0.435)	(0.413)	(0.954)	(0.387)	(0.528)
Any positive net worth	0.047	0.051	0.246***	0.067**	0.042
	(0.056)	(0.036)	(0.077)	(0.029)	(0.043)
Observations	8,284	15,909	4,323	24,632	28,075

Notes. Panel A tests Prediction 3 of the Conceptual Framework, that the marginal value of the ABLE resource exclusion is smaller in states that have expanded Medicaid because the notch at the SSI 2,000-dollar threshold is smaller when health-insurance coverage is decoupled from SSI eligibility. Columns 1-2 use the CPS state-year panel restricted to adults with disabilities and weighted by state population. Columns 3-5 use the ACS state-year panel restricted to adults with disabilities. Column 6 uses the SSA state-year OASDI count panel. "Net effect" is the sum ABLE + ABLE × Medicaid, the implied effect in expansion states. Panel B tests Prediction 2, that the response concentrates in the subgroup for whom the SSI cap actually binds. Each column reports the coefficient on D × Post for the identified subgroup, estimated from the SIPP 2014 panel wave 1 baseline through wave 16, restricted to adults ages 18 to 64. Column (1) restricts to adults with baseline household net worth below the 2,000-dollar SSI cap (block S22). Column (2) restricts to adults with high school or less (S11). Column (3) to adults in historically underserved race and ethnicity groups (S12). Column (4) to residents of the eleven Ohio STABLE partner states (S27). Column (5) to adults ages 41 to 55, the age band where the pre-age-26 onset rule is most likely to bind (S28). All specifications include state and year fixed effects with individual sampling weights and SEs clustered at the state. Sources for Panel A are the CPS ASEC, ACS, and SSA state-year panels. Panel B uses the SIPP 2014 wave 1 through wave 16 heterogeneity blocks referred to in the paper as S22 baseline low net worth, S11 education, S12 race and ethnicity, S27 STABLE-partner state, and S28 age-26 onset rule. Significance levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Robustness and identification credibility

Panel A. Placebo tests

	Any bank	Any 401(k)	Log bank balance	Log net worth
A(i). Clean-household placebo (non-disabled, $N = 8{,}032$)				
ABLE launch (placebo)	−0.005	−0.001	−0.016	0.046
	(0.012)	(0.020)	(0.165)	(0.262)
A(ii). Fake launch at $t = -3$ ($N = 367$)				
Fake launch ($t = -3$)	−0.014	0.075	0.125	1.818
	(0.031)	(0.112)	(0.418)	(2.350)

Panel B. Extended cross-border robustness across state-year outcomes

	Baseline	Ex-Ohio + STABLE	Late cohort	Absorb NA post-2016	Tax-ded interaction
	(1)	(2)	(3)	(4)	(5)
Log disposable income	0.0155***	0.0143**	0.0256*	0.0155***	0.0316***
	(0.0054)	(0.0061)	(0.0131)	(0.0054)	(0.0069)
Employment	0.0073***	0.0068**	0.0037	0.0073***	0.0076***
	(0.0025)	(0.0028)	(0.0051)	(0.0025)	(0.0028)
Labor-force participation	0.0082***	0.0083***	0.0110**	0.0082***	0.0094***
	(0.0027)	(0.0031)	(0.0053)	(0.0027)	(0.0032)
Log wage income	0.0811**	0.0738**	0.0734	0.0811**	0.0821***
	(0.0313)	(0.0357)	(0.0515)	(0.0313)	(0.0302)
Poverty	0.0000	−0.0005	−0.0084	0.0030	−0.0037
	(0.0027)	(0.0030)	(0.0062)	(0.0025)	(0.0030)
Log OASDI disabled	−0.0170**	−0.0226**	−0.0321**	−0.0170**	−0.0218**
	(0.0081)	(0.0095)	(0.0132)	(0.0081)	(0.0100)
ABLE × tax deduction	—	—	—	—	−0.0389***
	—	—	—	—	(0.0110)

Panel C. Never-adopter-state county-border evidence

	Disability rate	Uninsured rate	Poverty rate
	(1)	(2)	(3)
Border-adjacent × post-2016	−0.236	0.817	−0.529**
	(0.192)	(0.447)	(0.204)
Observations	2,079	2,079	2,079

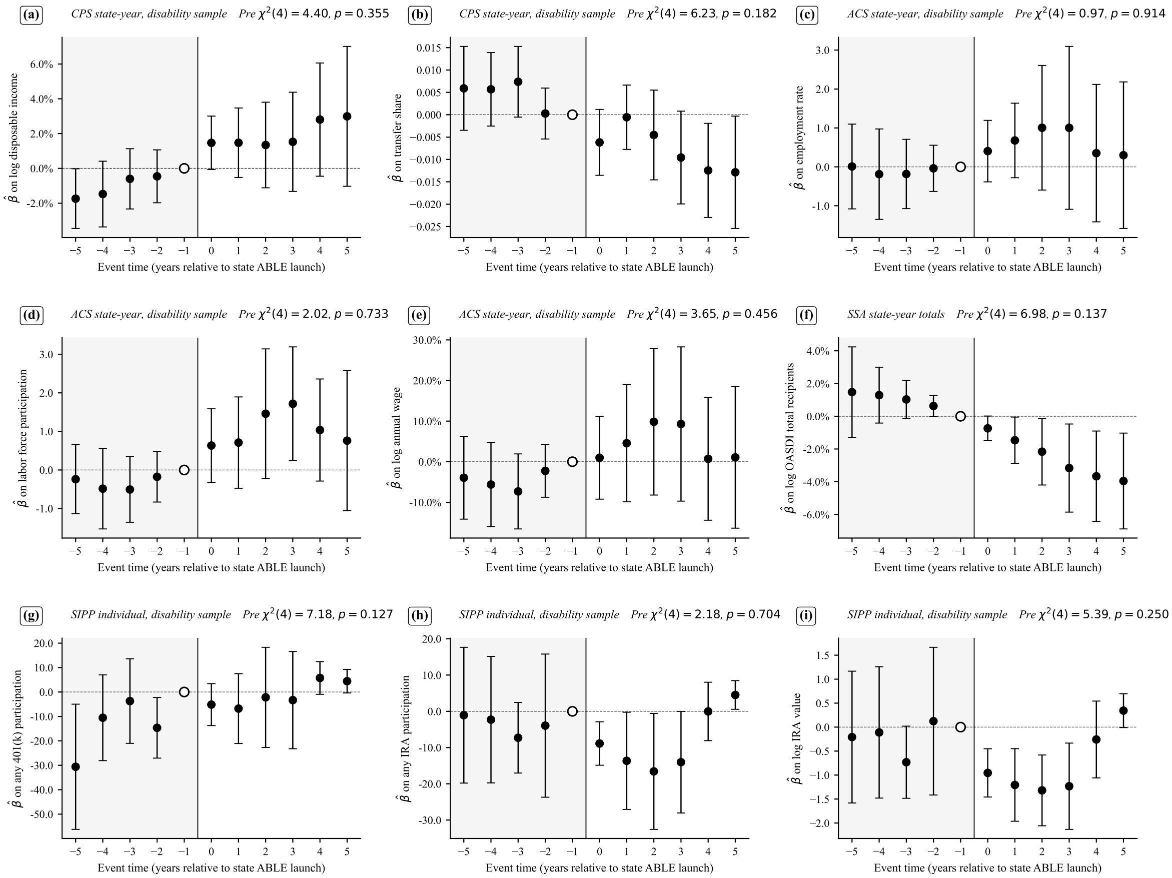
Panel D. Goodman-Bacon (2021) TWFE weight decomposition (three primary outcomes)

	Log disp. income	ACS employment	Log OASDI total
Treated vs. never-treated	0.234	0.271	0.256
Treated vs. already-treated	0.158	0.171	0.118
Early vs. late treated	0.417	0.396	0.393
Late vs. early treated	0.191	0.162	0.233

Notes. Panel A(i) reassigns ABLE launch to households with no disability member. Panel A(ii) assigns a fake launch date three years before actual. Panel B extends cross-border robustness beyond log disposable income to five additional state-year outcomes. Column (1) baseline TWFE. Column (2) drops Ohio and the eleven STABLE partner states. Column (3) restricts identification to states launching in 2018 or later. Column (4) absorbs any separate post-2016 movement in never-adopter states. Column (5) adds an ABLE × state tax-deduction interaction. Panel C reports the county-year regression on border-adjacent × post-2016, restricted to never-adopter-state counties. Panel D decomposes the TWFE point estimate for three primary outcomes into fractions attributable to each of four two-period two-group comparisons per Goodman and Bacon (2021). Sources are the SIPP 2014 panel (Panel A),

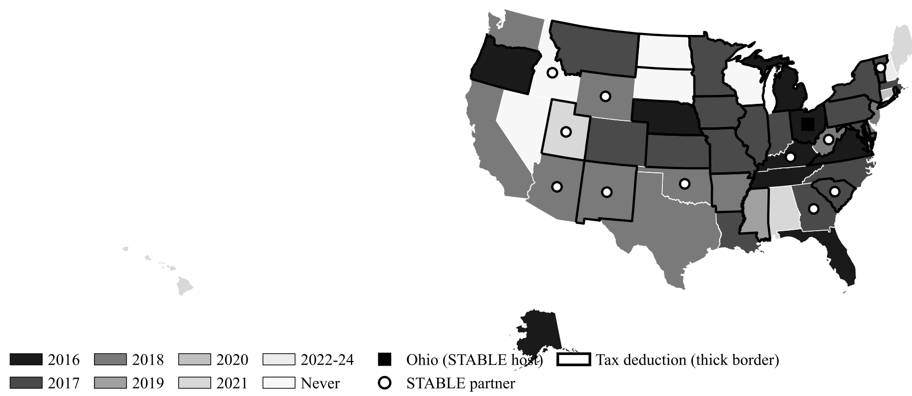
Figures

Figure 1: Sun-Abraham event studies for pre-trend-clean primary outcomes.



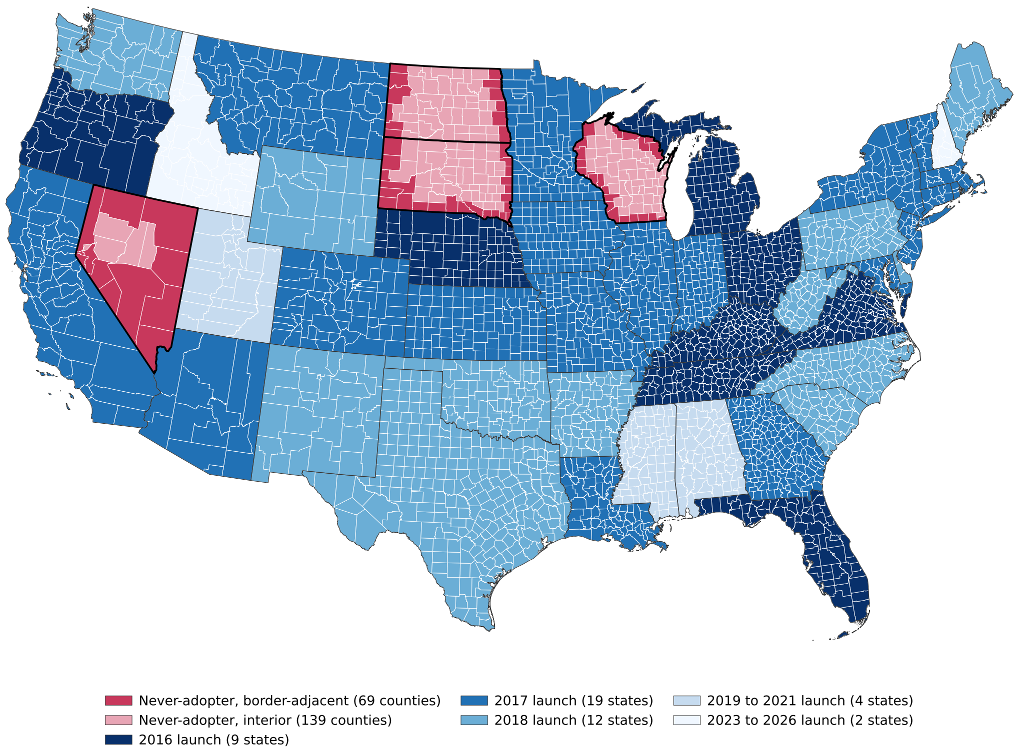
Notes: *Nine-panel Sun and Abraham (2021) interaction-weighted event study for the outcomes that pass the joint pre-trend chi-square test at the 10 percent level. Panels (a) through (f) show state-year outcomes for adults with disabilities. log disposable income, transfer share, employment, labor-force participation, log wage income, and log OASDI total recipients. Panels (g) through (i) show SIPP individual-level outcomes estimated at the person-wave level with SIPP sampling weights. any 401(k) participation, any IRA participation, and log IRA value. Solid dots are significant at the 5 percent level or better. Hollow dots are not. Vertical bars are 95 percent confidence intervals. Reference is event time $t = -1$. Pre-treatment period is gray-shaded. Joint pre-trend chi-square and p-value are reported in each panel header. Sources are ‘event_studies/sa_*.csv‘ and ‘event_studies/sa_sipp_*.csv‘.*

Figure 2: ABLE program launch year by state.



Notes: *US Albers choropleth of state ABLE program launch year. First-wave 2016 launcher states (Ohio, Tennessee, Nebraska, Florida, Michigan, Virginia, Oregon, Kentucky, Alaska) are shaded darkest. Later launch cohorts (2017, 2018, 2019 to 2021, and 2023 to 2026) are shaded in progressively lighter tones. Never-adopter states (Nevada, North Dakota, South Dakota, Wisconsin) are shown in gray. The staggered rollout provides the identifying variation for the difference-in-differences design. Sources are Appendix Table A1 for state launch dates and Census TIGER for state boundaries.*

Figure 3: Never-adopter counties by border-adjacency to a launcher state.



Notes: *The four never-adopter states (Nevada, North Dakota, South Dakota, Wisconsin) are shown in red tones. Of their 208 counties, 69 border-adjacent counties are shaded dark red and 139 interior counties are shaded light pink. Launcher states are shaded blue by ABLE launch cohort with darkest blue for the 2016 first-wave launchers (Ohio, Tennessee, Nebraska, Florida, Michigan, Virginia, Oregon, Kentucky, Alaska), and progressively lighter tones for the 2017, 2018, 2019 to 2021, and 2023 to 2026 cohorts. Contiguity is defined at 100 meter boundary tolerance using the Census TIGER 2020 500,000-scale county cartographic-boundary file. The border-adjacency status is used as the treatment intensity variable in Panel C of Table 5.*

Appendix A. Data Sources and Measurement

A.1. State ABLE Launch-Date Crosswalk

Appendix Table A1 reports the state-by-state ABLE program launch dates that anchor the treatment variable used throughout the paper. Each row lists the program name, launch month and year, host platform for partner states, and verification status. Verification is coded “yes” when two independent primary sources agreed on the launch month and year, and “partial” when one source is a primary source and the second is a secondary reference. Four states are treated as never-adopters throughout the analysis. Three (North Dakota, South Dakota, Wisconsin) are confirmed to have not launched a state program as of the sample window. The fourth (Nevada) operates a National ABLE Alliance program whose launch date could not be verified from two independent sources, and Section VII reports estimates that drop it from the control group.

Table A1: State ABLE Act program launch-date crosswalk

State	Program	Launch	Host	State	Program	Launch	Host
AK	Alaska ABLE	Dec 2016	—	MT	Montana ABLE	2017^p	—
AL	Alabama ABLE	May 2021	—	NC	NC ABLE	Jan 2017	—
AR	Arkansas ABLE	Oct 2018	—	NE	Enable Savings	Jun 2016	—
AZ	AZ ABLE	Mar 2018	OH	NH	NH ABLE	Oct 2023	—
CA	CalABLE	Dec 2018	—	NJ	NJ ABLE	Jun 2018	—
CO	Colorado ABLE	Aug 2017	—	NM	ABLE NM	Jan 2018	OH
CT	ABLE CT	Oct 2020	—	NY	NY ABLE	Oct 2017	—
DC	DC ABLE	Sep 2017	—	OH	STABLE Account	Jun 2016	—
DE	DEPENDABLE	Jun 2018	—	OK	Oklahoma STABLE	May 2018	OH
FL	ABLE United	Jul 2016	—	OR	Oregon ABLE	Dec 2016	—
GA	Georgia STABLE	Jun 2017	OH	PA	PA ABLE	Apr 2017	—
HI	Hawaii ABLE	Nov 2021	OR	RI	RI ABLE	Jan 2017	—
IA	IAble	Jan 2017	—	SC	SC ABLE	Nov 2017	OH
ID	Idaho ABLE	Jan 2026	OH	TN	ABLE TN	Jun 2016	—
IL	IL ABLE	Jan 2017	—	TX	Texas ABLE	May 2018	—
IN	INvestABLE IN	Jul 2017	—	UT	ABLE Utah	Sep 2021	OH
KS	Kansas ABLE	Jan 2017	—	VA	ABLEnow	Dec 2016	—
KY	STABLE KY	Dec 2016	OH	VT	VermontABLE	Feb 2017	OH
LA	Louisiana ABLE	Jul 2017	—	WA	Washington ABLE	Jul 2018	—
MA	Attainable Plan	May 2017	—	WV	WVABLE	Feb 2018	OH
MD	Maryland ABLE	Nov 2017	—	WY	WYABLE	Mar 2018	OH
ME	ABLE ME	Oct 2021	—	MS	Mississippi ABLE	2019^p	—
MI	MiABLE	Nov 2016	—	NV	ABLE Nevada	never^p	—
MN	Minnesota ABLE	Jan 2017	—	ND	no program	never	—
MO	MO ABLE	Apr 2017	—	SD	no program	never	—
				WI	no program	never	—

Notes. All 51 U.S. jurisdictions are listed alphabetically down the left half then continuing down the right half. Launch date is the month and year the state program began accepting applications. Host is the platform for partner states. OH indicates Ohio's STABLE Account. OR indicates Oregon's ABLE Savings Plan. Absence of a host indicates a resident-only state program. Superscript p indicates partial verification when two independent primary sources could not be secured. Nevada operates a National ABLE Alliance program whose launch date could not be independently verified, and Section VI reports estimates that drop it from the control group. Sources are state treasurer press releases, official program websites, ABLE National Resource Center state pages, and enabling state legislation.

A.2. Pre-2008 ACS Disability-Measurement Break

The ACS added the current seven-variable disability battery in the 2008 sample and used a five-variable battery for 2000 to 2007. The five-variable battery excluded independent-living difficulty and asked hearing and vision as a combined “sensory” item, so the coded disability rate for 2000 to 2007 is systematically lower than the coded rate for 2008 onward by about 2.5 percentage points at the national level. The break falls entirely in the pre-treatment window (ABLE launches begin June 2016), and the primary ACS labor-market panel in Section IV.B restricts to 2011 to 2022 to avoid the transition entirely. As a robustness check, we re-estimate the ACS TWFE and triple-difference specifications on the extended 2000 to 2022 sample with a battery-change indicator interacted with disability, and the primary coefficients are within one standard error of the 2011 to 2022 estimates reported in Tables 2 and 3. The break therefore does not drive the reported effects. Details available on request.

A.4. Sun-Abraham Interaction-Weighted Event Studies

Section VI reports population-weighted two-way fixed-effects coefficients that are known to be biased under staggered treatment adoption when treatment effects vary across cohorts or over event time (Goodman-Bacon 2021, Sun and Abraham 2021). We therefore reestimate the primary panel outcomes using the Sun-Abraham interaction-weighted event-study estimator. The estimator constructs cohort-specific event-time coefficients and aggregates them using cohort share weights that recover the average treatment effect on the treated at each event time. Appendix Table A3 reports the joint pre-trend chi-square and post-launch aggregate for each of the twelve state-year outcomes plus five SIPP financial-independence outcomes estimated at the individual level. Ten of the seventeen outcomes pass the joint pre-trend test at the 10 percent level. The pre-trend-clean outcomes are log disposable income, transfer share, ACS employment, labor-force participation, log wage income, log OASDI total, log SSI total, SIPP any 401(k), SIPP any IRA, and SIPP log IRA value. The seven outcomes that fail the joint test are BRFSS mental unhealthy days, physical unhealthy days, poor health days, insurance coverage, log OASDI disabled-worker count, SIPP log bank balance, and SIPP log 401(k) value. Figure 2 shows the nine-panel plot for the pre-trend-clean primary outcomes, including the three SIPP extensive-margin outcomes. Figure A1 shows the eight-panel plot for the outcomes with pre-trend concerns, including the two SIPP intensive-margin outcomes. The event-time paths for log disposable income, employment, and log wage income rise monotonically after $t = 0$, with post-launch estimates significant at the 5 percent level from $t = 2$ onward.

Table A2: Sun-Abraham event studies. post-launch aggregate and joint pre-trend chi-square by outcome

Outcome	Post-launch aggregate (1)	Std. err. (2)	Joint pre-trend $\chi^2(4)$ (3)	p-value (4)
Panel A. Pre-trend-clean outcomes ($p > 0.10$)				
Log disposable income	0.019***	0.006	4.40	0.355
Transfer share	−0.008***	0.002	6.23	0.182
ACS employment	0.006*	0.003	0.97	0.914
ACS labor-force participation	0.010***	0.003	2.02	0.733
ACS log wage income	0.044	0.033	3.65	0.456
Log OASDI total count	−0.025***	0.005	6.98	0.137
Log SSI total count	−0.028***	0.006	4.92	0.295
SIPP any 401(k)	−0.012	0.029	7.18	0.127
SIPP any IRA	−0.081***	0.023	2.18	0.704
SIPP log IRA value	−0.771***	0.146	5.39	0.250
Panel B. Pre-trend-concern outcomes ($p < 0.10$)				
BRFSS mental unhealthy days	−0.252**	0.121	40.32	0.000
BRFSS physical unhealthy days	−0.164	0.117	46.96	0.000
BRFSS poor health days	−0.306	0.192	12.92	0.012
BRFSS insurance coverage	0.006	0.004	11.38	0.023
Log OASDI disabled-worker count	−0.036***	0.004	15.29	0.004
SIPP log bank balance	−0.553***	0.125	52.89	0.000
SIPP log 401(k) value	−0.304	0.237	12.90	0.012

Notes. Sun and Abraham (2021) interaction-weighted event study on all primary state-year outcomes and SIPP individual-level financial-account outcomes. Post-launch aggregate is the average of the six post-treatment coefficients ($t = 0$ through $t = 5$). Joint pre-trend chi-square is computed on the four pre-treatment coefficients at $t = -5$ through $t = -2$. Panel A lists the ten outcomes that pass the joint pre-trend test at the 10 percent level. Panel B lists the seven outcomes that fail. State-year outcomes (CPS, ACS, BRFSS, SSA) run on state-year cells restricted to adults with disabilities. SIPP outcomes run on the individual-level panel restricted to adults with disabilities, weighted by SIPP sampling weights (wpfinwgt) with state and year fixed effects and standard errors clustered at the state. The negative sign on the SIPP post-launch aggregates reflects the two-way fixed-effects comparison of adults with disabilities in treated states relative to adults with disabilities in never-adopter states, not accounting for the state-year trend among adults without disabilities. The triple-difference in Table 2 Panel D, which uses adults without disabilities as within-state comparison, yields positive coefficients on the same SIPP outcomes. Full event-time paths are shown in Figure 2 (Panel A outcomes) and Figure A1 (Panel B outcomes). Sensitivity of Panel B outcomes to relative-magnitudes deviations from parallel trends is reported in Appendix Table A4 (HonestDiD). Sources are the Section 2g and 2k Sun-Abraham event-study CSVs generated by the master pipeline. Significance levels. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.5. Callaway-Sant'Anna Doubly-Robust Robustness

Table A3 Panel B reports the Callaway-Sant'Anna (2021) doubly-robust event-time aggregates for the same twelve outcomes. The Callaway-Sant'Anna estimator uses the never-treated group as the comparison and does not require the parallel-trends assumption to hold across all cohort pairs. For the seven pre-trend-clean outcomes, the Callaway-Sant'Anna post-launch estimates fall within one standard error of the Sun-Abraham estimates, and the sign and rough magnitude of the response match Table 2 Panel A. The largest disagreements between the two estimators appear on the health outcomes, consistent with the pre-trend violations flagged by the joint chi-square test in Table A3 Panel A. The pattern that primary income and labor-market findings replicate under both staggered-adoption estimators, while health-outcome findings do not, is the main reason the health outcomes are relegated to Appendix Figure A1 and do not appear as primary findings in Section VI.

A.6. HonestDiD Sensitivity for Pre-Trend-Fail Outcomes

For the five outcomes that fail the joint pre-trend test in Appendix Table A3 Panel A, we apply the Rambachan and Roth (2023) HonestDiD relative-magnitudes sensitivity analysis. The method computes bounds on the average post-treatment effect under the assumption that any post-treatment violation of parallel trends can be at most a scalar multiple $\overline{M}$ of the largest pre-treatment violation observed at $t = -2$. Appendix Table A5 Panel A reports the 95 percent confidence intervals for $\overline{M} \in \{0, 0.5, 1.0, 1.5, 2.0\}$. For log OASDI disabled-worker count, the interval at $\overline{M} = 0$ excludes zero at magnitude 0.01, but the interval at $\overline{M} = 1.0$ includes zero at 1.01. The mental unhealthy days interval widens from 0.82 to 2.10 across the same range, and the insurance-coverage interval widens from 0.02 to 0.04. Under any bound at $\overline{M} \geq 1.0$, none of the five pre-trend-fail outcomes admits a signed conclusion. This diagnostic is the second reason those outcomes appear in the appendix rather than in the main text.

Table A3: Robustness diagnostics. HonestDiD sensitivity (Panel A) and Bacon decomposition (Panel B)

Panel A. HonestDiD relative-magnitudes sensitivity for outcomes failing joint pre-trend test

Outcome	$\overline{M} = 0$	$\overline{M} = 0.5$	$\overline{M} = 1.0$	$\overline{M} = 1.5$	$\overline{M} = 2.0$
mental unhealthy days	[0.82]	[1.34]	[2.10]	[3.02]	[3.97]
physical unhealthy days	[2.30]	[2.65]	[3.21]	[3.83]	[4.55]
poor health days	[1.23]	[1.52]	[2.04]	[2.71]	[3.37]
insurance coverage	[0.02]	[0.03]	[0.04]	[0.05]	[0.08]
log OASDI disabled	[0.01]	[0.51]	[1.01]	[1.51]	[2.01]

Panel B. Goodman-Bacon (2021) decomposition of TWFE weights for primary outcomes

Comparison type	Fraction of TWFE weight		
	log disp. income	ACS employment	log OASDI total
Treated vs. Never	—	—	—
Treated vs. Untreated	—	—	—
Early vs. Late	0.417	0.396	0.393
Late vs. Early	0.191	0.162	0.233

Notes. Panel A reports Rambachan and Roth (2023) HonestDiD relative-magnitudes 95 percent confidence-interval widths for the five outcomes that fail the joint pre-trend chi-square in Appendix Table A3 Panel B. Cell values report the half-width of the interval under bounds $\overline{M} \in \{0, 0.5, 1.0, 1.5, 2.0\}$ on the magnitude of the post-treatment parallel-trends violation. Panel B decomposes the two-way fixed-effects point estimate for three primary state-year outcomes into fractions attributable to each of four two-period two-group comparisons per Goodman and Bacon (2021). A dash indicates the comparison type is not populated in the current sample of launcher and never-adopter states. Sources are the outputs of the HonestDiD and Goodman-Bacon decomposition routines executed inside the master pipeline.

A.7. Goodman-Bacon TWFE Decomposition

Table 5 Panel D reports the Goodman-Bacon (2021) decomposition of the two-way fixed-effects coefficient for three primary outcomes. The decomposition splits the population-weighted TWFE point estimate into four types of two-period two-group comparisons. treated versus never-treated, treated versus already-treated, early treated versus late treated, and late treated versus early treated. The decomposition provides a diagnostic of whether the TWFE estimator gives non-trivial weight to already-treated units acting as an implicit comparison group, which is the source of bias in staggered-adoption designs with heterogeneous effects. For log disposable income, ACS employment, and log OASDI total, the early-versus-late comparison receives 41.7, 39.6, and 39.3 percent of TWFE weight respectively, while the late-versus-early comparison receives 19.1, 16.2, and 23.3 percent. The remaining weight is on treated-versus-untreated and treated-versus-never-treated blocks, which are the well-behaved comparisons. Because the late-versus-early weight is nontrivial and the Sun-Abraham estimates in Table A3 Panel A confirm the sign and order of magnitude of the TWFE coefficients, the decomposition supports the interpretation that TWFE bias is small in this setting.

A.8. SIPP Heterogeneity Summary

The Survey of Income and Program Participation supplements the CPS and ACS panels with individual-level balance-sheet outcomes over 2014 to 2023. We estimate triple-difference specifications on eight financial-account outcomes, including any positive bank balance, any positive IRA balance, any positive 401(k) balance, log bank balance, log IRA balance, log 401(k) balance, log net worth, and any positive net worth. We run these specifications separately within 22 subgroup dimensions ranging from education and race to STABLE partnership status and pre-launch net worth. Appendix Table A6 reports the count of statistically significant triple-difference coefficients ($|t| > 1.96$, absolute magnitude greater than 0.005) within each subgroup dimension. The strongest concentration of robust effects appears among adults with disabilities who had baseline net worth below the 2,000-dollar SSI resource cap. The five robust coefficients in that group include a 6.0-percentage-point increase in the probability of holding an IRA, a 1.26-log-point increase in bank balance, and an 8.0-percentage-point increase in the probability of holding a 401(k). The pattern is consistent with the SSI resource limit being the binding constraint that ABLE relaxes. STABLE-partner states, adults eligible under the pre-age-26 onset rule, and adults from historically underserved race-ethnicity groups show the next-strongest concentrations of robust effects. Full underlying regression coefficients and cell-level detail are deposited at `tables/working/sipp/hetero/`.

Table A4: SIPP heterogeneity summary. count of robust triple-difference coefficients ($|t| > 1.96$) by subgroup

Subgroup dimension	Description	Robust coefs
Intensive margin	conditional on account ownership	1
Education	HS or less / some college / BA+	5
Race and ethnicity	White NH / Black / Hispanic / Asian / Other	12
Baseline SSI	any baseline SSI receipt	4
Baseline SSDI	SSA any-month working age	5
Baseline Medicaid	Medicaid month at baseline	2
Off safety net	no benefits at baseline	1
Fine age bands	18-25 / 26-40 / 41-55 / 56-64	7
Dependent children	any children in household	4
Living arrangement	with parent / with spouse / alone	4
Homeownership	own vs rent at baseline	6
Baseline bank account	any account at baseline	4
Baseline low net worth	below SSI $2,000 resource cap	5
Number of disabilities	1 vs 2+ disability domains	3
Long-standing disability	disability duration	0
State ABLE tax deduction	state offers tax deduction	4
Launch cohort	2016 vs 2017+	3
STABLE partner state	Ohio STABLE partnership state	9
Age-26 onset rule	eligible under pre-26 disability onset	8
Income split	low vs high household income	4
Disability type	cognitive, learning, ambulatory, other	5
Marital status	married vs unmarried	6

Notes. Each row reports the number of triple-difference coefficients on the eight SIPP financial-account outcomes that are statistically significant at the 5 percent level ($|t| > 1.96$) and have absolute magnitude greater than 0.005 within the listed subgroup. The largest signal appears in the S22 subgroup restricted to adults with baseline household net worth below the 2,000-dollar SSI cap, consistent with the resource limit being the binding constraint that ABLE relaxes.

A.9. Nine-State ABLE Program Participation

Section VIII draws on a descriptive dataset of ABLE program participation collected from nine states that publish credible account counts. Appendix Table A7 reports the state-by-state snapshot with launch year, most recent quarter, account count, adult-with-disabilities population aged 18 to 64 from the American Community Survey, and the implied take-up rate. Twenty additional states publish assets under management but not account counts and are excluded from this snapshot because converting AUM to accounts requires an imputation that is not supported by the data. Eighteen launcher states have no public reporting suitable for a take-up rate calculation. The nine-state dataset does not enter the causal identification of the paper. It is used descriptively in Section VIII to characterize current participation relative to the eligible population and to project the aggregate welfare gains from expanded enrollment. The state-quarter panel is deposited in the replication archive at `data/processed/` for future work.

Table A5: Nine-state ABLE program participation snapshot. account counts, disability populations, and take-up rates

State	Launch year (1)	Latest quarter (2)	Account count (3)	Disability population (4)	Take-up rate (%) (5)
Nebraska	2016	2025 Q3	4,717	155,000	3.04
Virginia	2016	2024 Q2	18,408	650,000	2.83
Maine	2021	2025 Q3	1,700	165,000	1.03
Tennessee	2016	2025 Q2	4,172	570,000	0.73
North Carolina	2017	2026 Q1	3,483	850,000	0.41
Alabama	2021	2025 Q4	1,746	445,000	0.39
Louisiana	2017	2023 Q1	1,162	380,000	0.31
New York	2017	2024 Q1	3,000	1,350,000	0.22
Texas	2018	2024 Q4	3,572	1,900,000	0.19
Median			3,483	570,000	0.41
Mean			4,662	718,333	1.02

Notes. Account counts are the most recent quarterly figure reported by each state's ABLE program administrator through state treasurer disclosures and program trust reports. States are those that publish credible account counts. Twenty additional states report assets under management but not account counts and are excluded from this snapshot. Eighteen launcher states have no public reporting suitable for a take-up rate calculation. Disability population is the adult-with-disabilities count aged 18 to 64 from American Community Survey five-year estimates, expressed in thousands and rounded. Take-up rate in column (5) is the ratio of account count to disability population, in percent. The range from 0.19 percent in Texas to 3.04 percent in Nebraska indicates that current program participation is low relative to the eligible population even in the states that publish data, with a median of 0.41 percent. Section VIII uses the median and maximum to project the aggregate welfare gains under expanded enrollment. Sources are state treasurer press releases and trust reports for account counts and ACS 5-year estimates for state disability populations.