

Employment Effects of the Workforce Innovation and Opportunity Act for Youth with Disabilities

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Abstract

The Workforce Innovation and Opportunity Act (2014) requires state vocational rehabilitation agencies to reserve at least 15 percent of their federal funds for Pre-Employment Transition Services (Pre-ETS), the largest categorical youth-disability mandate in federal workforce policy. Using cohort variation in Pre-ETS age eligibility at implementation and American Community Survey data, we find that eligibility reduced the pre-pandemic youth disability employment gap by 2.5 percentage points, about 6.5 percent of the pre-WIOA gap. The gain is concentrated among youth with cognitive disabilities and does not scale with states' discretionary Title I Youth spending. Implied earnings gains approach 2.9 billion dollars yearly.

Keywords: WIOA. Pre-Employment Transition Services. Youth with disabilities. Vocational rehabilitation. Workforce development

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I. Introduction

For youth with disabilities, the transition from secondary school to employment or postsecondary education is a critical margin on which long-run labor-force attachment is formed. The Workforce Innovation and Opportunity Act of 2014 (WIOA), the largest federal reform of workforce policy in a generation, made this transition a central federal policy target by creating a dedicated funding stream for school-to-work transition services. Yet, ten years after enactment, there is still no national causal evaluation of that reform. Existing studies document WIOA’s design, state implementation, service provision, and outcomes for specific disability groups (McDonnall et al. 2024; Rooney-Kron et al. 2024; Roux et al. 2019; Taylor et al. 2022, 2025), but they do not estimate whether the law improved educational or labor-market outcomes at the national scale. The closest rigorous evidence comes from related but distinct settings. Andersson, Holzer, Lane, Rosenblum, and Smith (2024) provide national quasi-experimental evidence on training under the Workforce Investment Act and WIOA, but their analysis does not isolate WIOA’s disability-specific provisions or the transition-age population targeted by Pre-ETS. Experimental demonstrations for youth receiving Supplemental Security Income provide evidence on intensive transition interventions for a related population (Fraker et al. 2018; Mamun et al. 2019), while studies of vocational rehabilitation services document returns to specific services at the individual level (Dean et al. 2019). None of this evidence evaluates the categorical federal mandate for youth with disabilities that WIOA introduced.

The absence of national evidence matters because working-age adults with disabilities constitute the largest group outside the U.S. labor force. In 2025, the employment rate among working-age individuals with disabilities was roughly 40 percent, compared with 75 percent among those without disabilities (Bureau of Labor Statistics 2026), and much of this gap emerges during the transition from school to work (Dean et al. 2019; Yin et al. 2023). Each cohort that leaves secondary school without a pathway into employment or postsecondary education adds to the stock of adults outside the labor force. WIOA is the federal government’s primary policy for improving that transition, and this paper provides the first national causal evidence on whether it achieved that objective for the population it was designed to serve.

WIOA's disability-specific innovation was Pre-Employment Transition Services, or Pre-ETS, a federally mandated set-aside requiring state vocational rehabilitation agencies to dedicate at least 15 percent of federal vocational rehabilitation funds to transition services for students with disabilities. This structure made WIOA an unusually large and binding federal intervention in the school-to-work transition of youth with disabilities, and it distinguished the law from the more discretionary Title I youth programs that characterized federal workforce policy under the Workforce Investment Act of 1998.

The categorical structure of Pre-ETS is also central to current policy debates. Reauthorization proposals, including A Stronger Workforce for America Act, would reshape the federal workforce system, while broader proposals to consolidate workforce programs into state block grants could weaken or eliminate the dedicated funding stream that supports Pre-ETS. At the same time, although the Department of Labor withdrew its proposed rule to phase out Section 14(c) subminimum wage certificates in July 2025, ongoing state-level elimination of subminimum wage employment raises the stakes for preparing youth with disabilities for competitive integrated employment. These pressures are emerging just as many entry-level occupations that have historically provided a foothold in the labor market face growing exposure to automation, artificial intelligence, and other emerging technologies that are reshaping task demand and occupational pathways (Acemoglu and Restrepo 2020; Autor et al. 2024; Acemoglu, Autor, and Johnson 2026; Demirel et al. 2026; Yin and Ogun 2026). Together, these changes make the effectiveness of school-based labor-market preparation an increasingly important empirical question.

Because WIOA applied nationwide, no untreated jurisdiction can serve as a clean comparison for the national effect of the law. Our main design therefore exploits variation within the target population itself, using age-based eligibility for Pre-ETS. In the American Community Survey, we compare cohorts who were young enough to be eligible for Pre-ETS when implementation began, ages 14 to 21 in 2015, with cohorts who had already aged out, ages 25 to 35 in 2015, and measure outcomes after both groups have completed schooling. The main threat to this design is that disability-related labor-market gaps may evolve across cohorts for reasons unrelated to WIOA.

We address this concern using three complementary strategies. First, we use cohorts that were never eligible for Pre-ETS to estimate how disability-related labor-market gaps evolve across birth cohorts in the absence of program exposure, and then remove those predicted trends before estimating effects for eligible cohorts. Second, we tighten the comparison by adding cohorts who were ages 22 to 24 in 2015. These individuals had just aged out of Pre-ETS eligibility but remained eligible for the broader WIOA youth system, which allows us to separate the incremental effect of the Pre-ETS set-aside from the effect of general youth services. Because this comparison uses adjacent age cohorts, it also reduces the scope for smooth cohort trends to drive the estimates. Third, we leverage cross-state variation in the speed and extent of WIOA implementation (Carlson et al. 2020; Deutsch et al. 2021; Rooney-Kron et al. 2024) to test whether estimated effects are larger in states that moved more quickly or more fully to implement the Pre-ETS provisions. This provides a separate check on whether the national cohort patterns align with the timing and intensity of state-level implementation.

The post-2015 implementation window overlaps with the post-2020 increase in disability employment, a period shaped by the pandemic-era shift to remote work and a historically tight labor market, both of which are developments unrelated to WIOA (Bloom, Dahl, and Rooth 2024). We therefore evaluate the law separately in the pre- and post-pandemic periods. The pre-pandemic period, 2011 to 2019, provides the cleanest setting for the cohort design, while the post-pandemic period, 2021 to 2024, captures a labor market in which the disability employment gap closed sharply for reasons that likely affected treated and comparison cohorts alike. Because the pandemic changed the disability labor market through channels outside WIOA, separating these periods is necessary for obtaining credible national estimates of the law's effects.

The national disability employment gap was stable before WIOA, narrowed modestly after 2015, and then closed sharply after 2020. The stability of the pre-WIOA series supports the cohort design, while the size and timing of the post-2020 closure make it implausible to attribute that change to the law. In the pre-pandemic period, Pre-ETS eligibility reduced the disability employment gap by 2.5 percentage points, equivalent to about 6.5 percent of the pre-WIOA gap of roughly 39 percentage points between youth with and without disabilities. The gain is concentrated

among cohorts eligible for Pre-ETS. Cohorts that were just too old for Pre-ETS but still eligible for general WIOA youth services show no comparable improvement, indicating that the effect comes from Pre-ETS rather than from the broader youth system. The employment effect is robust across placebo tests, alternative detrending specifications, two-way clustering, and wild-cluster bootstrap inference. Effects on earnings and high-school completion are positive in the national data but are not robust across the same checks, so we interpret them as suggestive.

This interpretation is further supported by evidence from state implementation and national vocational rehabilitation records. Employment gains do not scale with states' discretionary youth spending, which points away from general youth expenditure as the operative margin and toward the binding Pre-ETS set-aside. Consistent with that interpretation, national vocational rehabilitation records show that WIOA shifted service delivery toward employment-focused activities, with work-based learning and workplace-readiness training predicting competitive integrated employment.

These findings contribute to three strands of the economics of workforce policy. First, the paper provides the first nationally representative causal evidence on WIOA ten years after enactment and at a moment when reauthorization is under active congressional debate. This contribution complements existing evidence on training participation under WIA and WIOA by evaluating a different policy margin, the categorical disability specific structure of WIOA itself. Second, the paper advances the empirical study of specific provisions embedded inside bundled national reforms. By combining an age-eligibility design with a just-ineligible comparison group, the analysis separates the effect of the Pre-ETS set-aside from the broader Title I youth system in which it was embedded, while also accounting for the overlapping macroeconomic shock created by the pandemic-era closure of the disability employment gap. Third, the paper provides policy-relevant benefit-cost estimates that speak directly to the reauthorization debate. Applying the 2.5 percentage point employment gain to the annual stock of transition-age youth with disabilities implies aggregate earnings gains on the order of 2.9 billion dollars per year, compared with federal Pre-ETS spending of approximately 570 million dollars. The implied benefit-cost ratio exceeds five, and the evidence points to the categorical 15 percent Pre-ETS mandate rather than discretionary

Title I youth spending as the relevant policy margin. That distinction is directly relevant to proposals to convert WIOA into a block grant or reduce the disability specific set-aside.

The remainder of the paper proceeds as follows. The next section reviews WIOA's institutional background and disability provisions. We then present the national cohort analysis, including the descriptive national evidence, empirical strategy, and causal estimates before and after the pandemic, along with robustness checks and heterogeneity analyses. Subsequent sections examine variation in state implementation intensity and analyze vocational rehabilitation service delivery and mechanisms using national RSA-911 data. The final section concludes.

II. Background

WIOA created two youth-facing federal margins that operate through different funding rules. The Title I Youth program provides discretionary services to disadvantaged youth ages 14 to 24, and states retain broad authority over how to allocate those funds within the statutory rules. The Pre-Employment Transition Services (Pre-ETS) provision layers on top of this system a disability-specific mandate. It requires state vocational rehabilitation agencies to reserve at least 15 percent of their federal vocational rehabilitation allotment for transition services delivered to students with disabilities ages 14 to 21. The estimand of this paper is therefore not the effect of WIOA in general. It is the incremental effect of the Pre-ETS set-aside relative to the broader discretionary youth-service system that already existed for these cohorts.

A. The Workforce System

Understanding WIOA requires distinguishing the broader public workforce system from the disability specific vocational rehabilitation system. The Workforce Investment Act of 1998 established the basic architecture of federally funded employment and training services delivered through state and local workforce systems, including youth programs for disadvantaged young people (Barnow and Smith 2015). Although disability was one barrier that could qualify a young person for services, the earlier system was not organized around the school-to-work transition of students with disabilities. Workforce development programs and state vocational rehabilitation agencies often operated through separate administrative structures, with limited coordination across education, employment, and disability services (Government Accountability Office 2019).

As a result, access to effective transition support depended heavily on local relationships among schools, workforce boards, and vocational rehabilitation agencies.

WIOA sought to make this system more integrated and more accountable by strengthening the one-stop service delivery model, expanding performance accountability requirements, and placing greater emphasis on job seekers facing substantial barriers to employment (Congressional Research Service 2022). Title I Youth formula grants continued to fund services for low-income youth ages 14 to 24 who face barriers such as low basic skills, dropout status, pregnancy or parenting, foster care involvement, homelessness, justice system involvement, or disability. These services remain part of the broader youth workforce system and are administered through state and local workforce boards, leaving considerable discretion in how states and local areas define priorities, allocate resources, and deliver services.

For youth with disabilities, the more consequential institutional change came through Title IV and the vocational rehabilitation system. Rather than relying only on the discretionary Title I youth system, WIOA created a disability-specific transition mandate administered by state vocational rehabilitation agencies. This distinction is central to the analysis below. WIOA did not replace the existing youth workforce system with a single new program. It layered a federally mandated disability transition provision on top of a broader youth system that remained more discretionary. The empirical question is therefore not only whether youth outcomes changed after WIOA, but whether the disability-specific Pre-ETS mandate generated effects beyond those associated with the general Title I youth system.

WIOA's implementation timeline involves two distinct anchor dates that we use in different components of the analysis. The statute was enacted in July 2014 and became effective on July 1, 2015, which is the reference year for cohort eligibility in our analysis. The vocational rehabilitation regulations implementing Pre-ETS were finalized in August 2016, and the first full vocational rehabilitation program year under WIOA rules was program year 2016 (July 1, 2016 to June 30, 2017), which is the reference date used for our mechanism analysis. The 2015 anchor governs statutory eligibility in the population panel. The 2016 anchor governs administrative implementation in the vocational rehabilitation program. The one-year gap between statutory

effective date and full VR-program implementation is standard for federal rulemaking and does not affect the cohort analysis below, which is identified by age-based eligibility rather than by service receipt.

B. Pre-ETS and Section 14(c)

The disability-specific mandate in WIOA operated through Pre-Employment Transition Services, or Pre-ETS. State vocational rehabilitation agencies are required to reserve at least 15 percent of their federal vocational rehabilitation allotment for students with disabilities ages 14 to 21. The required services are defined around the transition from school to work and include job exploration counseling, work-based learning experiences, counseling on postsecondary education opportunities, workplace-readiness training, and instruction in self-advocacy (Rehabilitation Act of 1973 § 110(d)(1), as amended by the Workforce Innovation and Opportunity Act, 29 U.S.C. § 730(d)(1), 34 C.F.R. § 361.65(a)(3)(i), with Pre-ETS activities enumerated at 29 U.S.C. § 733). The policy logic was to move employment preparation earlier in the life cycle, before students with disabilities left the education system and before labor-market detachment became more difficult to reverse.

Pre-ETS also interacted with WIOA's restrictions on subminimum wage employment under Section 14(c) of the Fair Labor Standards Act. Before WIOA, youth with disabilities could move directly from school into subminimum wage employment, often in sheltered workshop settings. WIOA changed that pathway by requiring youth to receive transition services and vocational rehabilitation counseling before entering subminimum wage work. This requirement reflected a broader federal shift toward competitive integrated employment, defined as work in typical workplace settings alongside workers without disabilities and at comparable wages (29 U.S.C. § 3102(11), Butterworth et al. 2007). In institutional terms, the two provisions worked together. Pre-ETS expanded early access to employment preparation, while the Section 14(c) restrictions made it harder for youth to move into segregated, subminimum wage employment without first receiving transition support. National evidence indicates that eliminating Section 14(c) certificates reduced subminimum-wage employment among workers with disabilities without reducing their overall employment (Yin, Seo, and Vu 2026).

C. Youth Program Funding

Federal Title I Youth allotments are distributed to states through a statutory formula based on unemployment and the number of economically disadvantaged youth ages 16 to 21 (29 U.S.C. § 3162(b)(1)). The allocation formula did not change in the transition from the Workforce Investment Act to WIOA, which makes the spending patterns after implementation especially informative. WIOA did not primarily change how federal youth funds were allocated across states. It changed the rules governing how states were expected to use those funds, including stronger requirements around service priorities, performance accountability, and populations facing barriers to employment.

Federal Youth allotments were relatively stable over this period, averaging approximately \$799 million annually under WIA from 2011 to 2015 and increasing modestly to \$860 million under WIOA from 2016 to 2022. Yet states spent a smaller share of available funds after WIOA took effect. Average expenditure rates declined from 74.6 percent under WIA to 63.1 percent under WIOA, with nearly all states experiencing reductions (Appendix Figure A1). This decline is consistent with the implementation challenges documented by states and federal oversight agencies as workforce systems adjusted to the new statutory requirements.

One important source of adjustment was WIOA's stronger emphasis on out-of-school youth. The law increased the required share of Title I Youth funds that must be spent on out-of-school youth from 30 percent to 75 percent, a shift that many states reported as administratively difficult (Government Accountability Office 2018, 29 U.S.C. § 3164(a)(4)(A)). Because out-of-school youth and youth with disabilities often require more intensive outreach, coordination, and individualized services, the decline in expenditure rates may reflect more than administrative transition costs. It may also capture differences in how states translated WIOA's new priorities into service delivery for youth with more complex barriers to employment.

These funding and spending patterns are important for the empirical analysis because they separate statutory funding from implementation intensity. The federal allocation formula was largely stable, but states differed in how fully they spent available youth funds and how quickly they adapted to WIOA's new service requirements. We use this variation in the state-spending

analysis below to examine whether more intensive implementation of youth services translated into better outcomes for youth with disabilities, and whether any such relationship is distinct from the national effect of the Pre-ETS mandate.

III. Data

A. American Community Survey

The primary data source is the American Community Survey (ACS), accessed through IPUMS USA, for survey years 2011 through 2024. The ACS is the largest annual household survey in the United States, covering roughly one percent of the population, or about three million individuals, each year. It is also the only nationally representative data source that combines disability status, employment, schooling, and earnings measures with samples large enough to distinguish narrow birth cohorts within the disability population across the full pre- and post-WIOA window. These features make the ACS well suited to the cohort design used below. Vocational rehabilitation records observe only youth who enter the VR system, while smaller household surveys such as the Current Population Survey do not provide sufficient sample sizes to estimate single-year cohort effects within the disability population.

The ACS identifies disability using six standardized functional-limitation items: hearing difficulty, vision difficulty, cognitive difficulty, ambulatory difficulty, self-care difficulty, and independent-living difficulty. We classify an individual as having a disability if they report any of the six limitations. The analytical sample is restricted to individuals ages 24 to 35 at the time of observation. The lower bound ensures that outcomes are measured after most respondents have completed formal schooling, so that employment and high-school completion reflect post-transition outcomes rather than contemporaneous enrollment decisions. It also allows the youngest Pre-ETS-eligible cohorts to be observed at comparable post-schooling ages. The upper bound keeps the comparison cohorts close in age to the treated cohorts while still providing enough pre-WIOA observations to estimate cohort trends.

Cohorts are defined by age in 2015, the first year of nationwide WIOA implementation. Individuals ages 14 to 21 in 2015 were young enough to be exposed to Pre-ETS during the transition period, while older cohorts had already aged out of eligibility. Appendix Table A1

reports which cohorts are observed at ages 24 and older in each survey year. The main outcomes are employment, high-school completion, and annual earnings. Employment is measured as currently working. High-school completion is measured as attainment of a high-school diploma or equivalent. Earnings are measured as the log of annual wage and salary income among individuals with positive earnings. Individuals in military occupations are excluded. All estimates use ACS person weights.

Table 1 Panel A reports summary statistics for the analytical sample by disability status. Individuals with disabilities are employed at substantially lower rates than their non-disabled peers, 46 percent compared with 80 percent, complete high school less often, 82 percent compared with 91 percent, and earn less among those with positive earnings, \$27,800 compared with \$41,500. These differences define the national disability gaps in employment, schooling, and earnings that the empirical analysis examines.

B. State Youth Expenditures

We supplement the ACS with state-level administrative data on WIOA Title I Youth programs from the Department of Labor’s Quarterly State Expenditure Reports (Department of Labor 2024). These data are used to construct a measure of implementation intensity for the state-level analysis. For each state and program year, we define the Youth expenditure rate as actual Title I Youth program expenditures divided by total Youth funds available to the state, where available funds include the current-year allotment and carry-in from prior years. The measure captures the share of available federal resources that a state actually deployed, distinguishing states that drew down their youth funds from those that allowed funds to lapse or carry forward.

The expenditure data cover program years 2011 through 2022, spanning the transition from the Workforce Investment Act to WIOA. Because the state implementation analysis depends on this administrative measure, that analysis uses the 2011 to 2022 window rather than the full ACS panel. We demean each state’s expenditure rate relative to the national state-year average. In the triple-difference specification below, this normalization implies that the main WIOA coefficient is evaluated at average implementation intensity, while the interaction coefficient measures whether outcomes changed more in states with above-average expenditure intensity.

This measure is not a direct measure of Pre-ETS spending, which is administered through the vocational rehabilitation system rather than Title I Youth. Its value is instead diagnostic. If the estimated national effects are driven by the broader youth workforce system, they should be larger in states that spent a greater share of available Title I Youth funds after WIOA. If the effects are instead driven by the categorical Pre-ETS mandate, which applied nationally through the VR system, the relationship with discretionary Title I Youth expenditure intensity should be weaker. The state-spending analysis therefore helps distinguish the broader WIOA youth system from the disability-specific policy margin at the center of the paper.

C. RSA-911 Records

To study mechanisms, we use RSA-911 administrative records, the case-level universe of vocational rehabilitation case closures that state agencies submit annually to the Rehabilitation Services Administration. Each record reports participant demographics, primary and secondary disabilities, public-benefit receipt, services received during the VR spell, and employment and wage outcomes at case closure. We use program years 2014 through 2019, a window that brackets the implementation of Pre-ETS while ending before the pandemic disrupted both service delivery and the labor market. The sample is restricted to transition-age youth ages 14 to 21 at application, the population targeted by Pre-ETS, and excludes U.S. territories. After requiring valid eligibility and closure dates, the final sample contains 313,890 cases.

The RSA-911 records are useful because they observe service receipt directly. We measure Pre-ETS receipt using a composite indicator and four separately recorded service components, work-based learning, workplace-readiness training, counseling on postsecondary enrollment, and self-advocacy instruction. Job exploration counseling, the fifth statutory Pre-ETS component, is not separately recorded in these files. The outcome of interest is competitive integrated employment at closure, defined as work in an integrated setting at or above the minimum wage.

The strength of the RSA-911 data is also their limitation. They provide detailed information on services and case outcomes, but only for youth who entered the VR system. They do not represent all youth with disabilities, and they omit a growing group of students who received Pre-ETS without opening a full VR case, a population WIOA specifically sought to reach. For that

reason, the RSA-911 analysis complements the population-level ACS estimates rather than replacing them. We interpret the service-outcome relationships in these records as descriptive evidence on channels through which WIOA may have affected employment, not as causal estimates of the national policy effect.

Different components of the RSA-911 analysis use different subsamples of the 313,890 transition-age youth cases. Table 1, Panel B, summarizes the sample derivation. The Table 5 caseload total (560,306) exceeds the transition-age youth total of 313,890 because it draws on program years 2016 to 2019 while the main sample uses 2014 to 2019 and applies the closure-date restriction. Youth who appear in the case-flow universe may not appear in the outcome-restricted samples, which is standard for RSA-911 analyses of this kind.

IV. Empirical Strategy

WIOA was implemented nationally, so the statute does not provide untreated states that can identify the national effect of the law. The identifying variation comes instead from age eligibility for Pre-ETS within the target population. We compare birth cohorts that were young enough to be eligible for Pre-ETS when implementation began with cohorts that had already aged out, and with a just-ineligible group that remained eligible for Title I Youth services but not Pre-ETS. The parameter of interest is an intent-to-treat effect of Pre-ETS age eligibility. The ACS does not observe service receipt, and eligibility does not imply that every student received Pre-ETS. The design therefore estimates whether cohorts that were eligible for the disability-specific set-aside experienced a differential narrowing of the disability employment gap after WIOA.

The main threat to identification is that the disability employment gap may evolve smoothly across birth cohorts for reasons unrelated to Pre-ETS. We address this concern in three ways. First, we estimate disability-specific cohort trends among the never-eligible cohorts, ages 25 to 35 in 2015, and remove the predicted trend before estimating effects for the Pre-ETS-eligible cohorts. Second, we add cohorts ages 22 to 24 in 2015 as an adjacent comparison group. These cohorts were too old for Pre-ETS but remained eligible for the broader Title I Youth system, which separates the incremental effect of the Pre-ETS set-aside from general youth service eligibility. Third, we estimate a within-control placebo that assigns false treatment to never-eligible cohorts ages 25 to

27 relative to never-eligible cohorts ages 28 to 35. Since neither group was eligible for WIOA youth provisions, a nonzero placebo estimate would indicate that the detrending procedure has not removed smooth cohort differences in the disability gap.

We implement the estimator in two steps. Among never-eligible cohorts, we first estimate the disability-specific cohort trend,

$$Y_i = \delta D_i + \pi c_i + \varphi(D_i \times c_i) + X_i' \gamma + \theta_s + \theta_a + u_i \quad (1)$$

The outcome Y_i is employment, high-school completion, or log annual earnings. D_i indicates disability status. c_i is birth cohort centered on the mean of the never-eligible cohorts. X_i includes sex, race and ethnicity, the presence of young children, and state-year controls for Medicaid expansion, the minimum wage, the Earned Income Tax Credit, log personal income per capita, and the house-price index. In the employment and earnings models, X_i also includes educational attainment. The terms θ_s and θ_a denote state and age fixed effects. The coefficient φ captures the disability-specific cohort trend estimated on cohorts that were never eligible for WIOA youth provisions.

We use this estimate to construct a detrended outcome, $\tilde{Y}_i = Y_i - \varphi(D_i \times c_i)$. The preferred specification removes a linear disability-specific cohort trend. A quadratic cohort trend is used as a robustness check. We then estimate the difference-in-differences contrast between Pre-ETS-eligible and never-eligible cohorts,

$$\tilde{Y}_i = \beta (E_i \times D_i) + \delta D_i + X_i' \gamma + \theta_b + \theta_s + \theta_a + \varepsilon_i \quad (2)$$

The variable E_i indicates cohorts ages 14 to 21 in 2015, who were young enough to be eligible for Pre-ETS. The term θ_b denotes birth-cohort fixed effects. Standard errors are clustered by state. A positive value of β means that the disability gap narrowed for the Pre-ETS-eligible cohorts relative to the never-eligible cohorts.

The preferred specification adds the just-ineligible cohorts,

$$\tilde{Y}_i = \beta_E (E_i \times D_i) + \beta_P (P_i \times D_i) + \delta D_i + X_i' \gamma + \theta_b + \theta_s + \theta_a + \varepsilon_i \quad (3)$$

The variable P_i indicates cohorts ages 22 to 24 in 2015. These cohorts were eligible for general Title I Youth services but not for Pre-ETS. The incremental Pre-ETS effect is $\beta_E - \beta_P$. This contrast removes any effect shared by Pre-ETS-eligible cohorts and the just-ineligible cohorts, including exposure to the broader youth system and smooth cohort differences in the disability employment gap.

The design does not separately estimate unrestricted age, period, and cohort effects. Age at observation, birth cohort, and survey year are mechanically linked. Instead, the design uses the institutional cutoff to isolate the discrete change in Pre-ETS eligibility. Age fixed effects absorb differences across observation ages. Birth-cohort fixed effects absorb level differences across cohorts. The detrending step removes the smooth disability-specific cohort gradient estimated among cohorts that were never eligible. The three-arm contrast then compares adjacent cohorts that differ in Pre-ETS eligibility while remaining close in birth year and exposed to the same non-Pre-ETS youth system.

We estimate the model separately for the pre-pandemic period, 2011 to 2019, and the post-pandemic period, 2021 to 2024. We drop 2020 because of ACS data-quality concerns and the acute disruption of that year. The pre-pandemic period is the primary estimating window because it precedes the pandemic-era reallocation of work arrangements and labor demand. After 2020, the disability employment gap closed sharply in a labor market shaped by remote work and unusually strong labor demand, both of which affected workers with disabilities through channels outside WIOA. The post-pandemic estimates therefore show how the design behaves under a changed labor market, but they are not the primary source of the paper’s quantitative claim.

V. Main Results

A. National Trends

The disability employment gap was stable before WIOA, narrowed modestly after implementation, and closed sharply after 2020. Figure 1 plots annual estimates of the employment gap between young adults with and without disabilities, relative to 2014, after controlling for demographics,

state policies, local economic conditions, and state and age fixed effects. The pre-WIOA series is essentially flat. A joint test for the 2011 to 2013 coefficients does not reject equality with zero. This stability supports the cohort design because a pre-existing narrowing of the disability employment gap for younger cohorts would weaken the interpretation of the age-eligibility contrast.

The level of the pre-WIOA gap makes the estimated effects economically meaningful. In the analytical sample, young adults ages 24 to 35 with disabilities were employed at roughly 38 percent before WIOA, compared with 77 percent among young adults without disabilities. The baseline disability employment gap was therefore about 39 percentage points. The estimates below should be interpreted against this large pre-existing gap.

The post-2020 pattern differs from the earlier implementation period. The disability employment gap closed much more sharply after the pandemic than it did in the first years after WIOA. This later closure is difficult to interpret as a WIOA effect because it occurs after the main implementation window and coincides with a broader labor-market shock that changed employment opportunities for workers with disabilities. For this reason, the pre-pandemic period provides the main window for estimating the effect of Pre-ETS eligibility. The post-pandemic estimates are useful primarily for assessing how the cohort design performs once the disability labor market changes for reasons outside the statute.

The high-school completion and earnings gaps follow a similar aggregate pattern. Both gaps narrow after WIOA and then more sharply after 2020. These descriptive series provide context for the national environment in which WIOA was implemented. They do not, by themselves, identify the effect of the Pre-ETS set-aside. The cohort comparisons below carry the causal interpretation.

B. Employment Effects

Table 2 presents the main employment estimates. Pre-ETS eligibility reduced the disability employment gap before the pandemic. The preferred estimate comes from the three-arm design, which compares Pre-ETS-eligible cohorts with adjacent cohorts that were too old for Pre-ETS but still eligible for general Title I Youth services. In the pre-pandemic period, this contrast implies that Pre-ETS eligibility reduced the disability employment gap by 2.54 percentage points. The equality

test for the exposed and partial coefficients gives $F = 5.60$ and $p = 0.018$. Relative to the pre-WIOA employment gap of about 39 percentage points, the estimate closes roughly 7 percent of the gap.

The magnitude is modest in absolute terms but meaningful for a national eligibility rule. WIOA did not assign youth to an intensive demonstration program or guarantee the individualized case management evaluated in earlier transition demonstrations (Schochet 2021). It changed the federal funding structure by requiring state vocational rehabilitation agencies to reserve a share of federal funds for transition services. A 2.5 percentage-point employment effect from this type of statutory mandate is large enough to matter for aggregate employment, while remaining plausible given the scale and implementation complexity of the reform.

The just-ineligible comparison is the key evidence that the employment effect comes from Pre-ETS rather than from WIOA's broader youth system. The Pre-ETS-eligible cohorts, ages 14 to 21 in 2015, experienced an employment gain of 2.56 percentage points relative to the never-eligible cohorts. The adjacent cohorts ages 22 to 24, who were too old for Pre-ETS but remained eligible for Title I Youth services, show essentially no gain. The estimated partial-cohort effect is 0.02 percentage points. The difference between these two estimates isolates the part of the employment improvement that cannot be attributed to general youth-service eligibility. The relevant margin is therefore not exposure to WIOA youth services in general. It is eligibility for the disability-specific Pre-ETS set-aside.

The two-group estimates point to the same interpretation but are less decisive. Comparing Pre-ETS eligible cohorts directly to never-eligible cohorts yields effects of 2.55 percentage points under linear detrending and 3.19 percentage points under quadratic detrending. These estimates show that the result is not sensitive to the functional form used to remove smooth cohort trends. The three-arm estimate remains preferred because it also nets out the broader Title I Youth margin and compares adjacent birth cohorts.

The post-pandemic estimates reinforce the decision to make the pre-pandemic period the main quantitative window. In the two-group specification, the estimates become unstable after 2020. The linear-detrended estimate is essentially zero, at negative 0.06 percentage points, while the quadratic-detrended estimate is implausibly large, at 17.66 percentage points. The within-control

placebo also rejects in this period. These patterns are consistent with a post-2020 change in disability employment that varied across cohorts and weakened the two-group comparison. The adjacent-cohort estimator is less exposed to this concern and produces a positive but smaller estimate of 0.68 percentage points, which is not statistically distinguishable from zero. We therefore treat the post-pandemic estimates as evidence on the stability of the design under a changed labor market rather than as the preferred estimate of WIOA.

The results for high-school completion and earnings are weaker (Appendix Tables A2 and A3). The two-group design shows a positive pre-pandemic effect on high-school completion of 1.32 percentage points, but the three-arm design does not attribute that gain to the Pre-ETS set-aside. The just-ineligible cohorts show similar schooling gains, 1.07 percentage points compared with 1.36 percentage points for the eligible cohorts, yielding a small and statistically insignificant Pre-ETS contrast. Earnings estimates are imprecise across specifications. The paper's causal claim should therefore remain centered on employment. Schooling and earnings are secondary outcomes that are directionally consistent with improvement but not separately identified as Pre-ETS effects.

C. Robustness

The employment estimate is not explained by smooth cohort trends. The main concern in the cohort design is that disability employment gaps may have been narrowing for younger cohorts even without WIOA. The within-control placebo directly tests this concern by assigning false treatment to never-eligible cohorts ages 25 to 27 relative to never-eligible cohorts ages 28 to 35. In the pre-pandemic period, the placebo estimate is small and statistically insignificant, at 0.24 percentage points. This result, reported in Table 2, supports the detrending procedure. It removes pre-existing cohort gradients rather than creating a spurious treatment effect.

The estimate is also stable across detrending specifications. The two-group estimate is 2.55 percentage points under a linear cohort trend and 3.19 percentage points under a quadratic trend. The preferred three-arm Pre-ETS estimate is 2.54 percentage points. The timing evidence points in the same direction. The national employment gap is flat before the reform and begins to narrow after the 2015 implementation year rather than in 2014, when the statute was signed and when other contemporaneous policy changes were already in place.

Alternative inference procedures do not overturn the result. Because the identifying variation is organized by cohort and state, conventional state-clustered standard errors may understate dependence across cells. The estimate remains statistically significant with two-way clustering by state and birth cohort, and it also survives wild-cluster bootstrap inference. The bootstrap p-value is 0.010, and the corresponding confidence set excludes zero. Adding the state-year unemployment rate and its interaction with disability also does not attenuate the estimate. If anything, the coefficient rises to 3.4 percentage points, which indicates that the result is not explained by differential gains for workers with disabilities during a tightening labor market.

The cohort event study provides an additional check on the cutoff interpretation. We estimate employment on disability interacted with age-in-2015 indicators, along with the demographic controls, state-year controls, state fixed effects, age fixed effects, and year fixed effects. The omitted category in the estimation is age 25 in 2015, the youngest never-eligible cohort. For interpretation, we report differences relative to age 20 in 2015, the youngest cohort observed at age 24 or older within the pre-pandemic window.

The coefficients trace a monotonic pattern in age at implementation. Relative to the age-20 cohort, the estimated disability employment gap widens by 2.2 percentage points at age 21, 3.9 percentage points at age 22, 6.5 percentage points at age 24, 8.0 percentage points at age 25, 10.8 percentage points at age 30, and 12.8 percentage points at age 35. The largest single-year increase occurs at the age-21-to-22 boundary, where Pre-ETS eligibility ends. The gradient among Pre-ETS-eligible cohorts is smaller than the gradient among never-eligible cohorts, which is consistent with Pre-ETS compressing the disability employment gap for cohorts with greater exposure to the set-aside.

Rambachan and Roth (2023) sensitivity bounds reinforce this conclusion. We recast the cohort comparison as an event study of the detrended disability employment gap by age in 2015, calibrating the cohort trend on the never-eligible cohorts (ages 26 to 35) and taking the Pre-ETS-eligible cohorts as the post-reference periods. The robust 95 percent confidence interval on the Pre-ETS employment effect is 0.015 to 0.051 under exact parallel trends. The interval remains positive when violations of parallel trends are allowed up to the size of the largest observed pre-trend, with

a lower bound of 0.004 at $M = 1.0$, and it first includes zero near $M = 1.1$. Because only the two oldest eligible cohorts (ages 20 and 21 in 2015) are observable at age 24 or older before the pandemic, the sensitivity is estimated on the least-exposed eligible cohorts and should be read as a conservative check. Table A4 reports the full set of bounds.

D. Heterogeneity

Table 3 reports the Pre-ETS employment effect across subgroups. The employment effect is concentrated among youth with cognitive disabilities, the group for whom Pre-ETS is most likely to change the school-to-work transition. The estimated effect for youth with cognitive disabilities is 3.49 percentage points and statistically significant. The estimates for youth with physical disabilities and sensory disabilities are smaller and statistically insignificant, at 0.19 and 1.72 percentage points. This pattern is informative about the policy margin. Pre-ETS emphasizes job exploration, work-based learning, workplace readiness, postsecondary counseling, and self-advocacy. These services are likely to matter most when the binding constraint is early work exposure, transition planning, employer interaction, or preparation for workplace routines. They are less likely, by themselves, to remove accommodation, transportation, or accessibility barriers that may be more salient for some youth with physical or sensory disabilities.

The demographic heterogeneity estimates are consistent with the average effect but less central to the interpretation. Estimated gains are larger for young men than for young women, at 3.10 percentage points compared with 1.24 percentage points. By race and ethnicity, the estimate is largest for Hispanic youth, at 3.78 percentage points, marginally significant for white youth, at 2.17 percentage points, and positive but imprecise for Black youth, at 2.30 percentage points. These subgroup estimates should be interpreted cautiously because the samples are smaller and the confidence intervals are wider. Their main value is to show that the average employment gain is not driven by a single demographic group.

The heterogeneity evidence supports the program interpretation. The effect is largest for the disability group most directly targeted by transition-oriented services and weaker for groups whose employment barriers may require different policy instruments. This pattern is difficult to reconcile

with a diffuse cohort trend. It is more consistent with a reform that operated through early, school-based labor-market preparation.

VI. Mechanisms

WIOA created two youth-facing margins that can affect youth with disabilities through different institutional channels. The first is the broader Title I Youth system, whose intensity depends on how states and local workforce boards spend available youth funds. The second is the Pre-ETS set-aside, which required every state vocational rehabilitation agency to reserve at least 15 percent of its federal vocational rehabilitation allotment for transition services to students with disabilities. The ACS estimates identify an employment gain for cohorts eligible for Pre-ETS. The remaining question is whether that gain is more consistent with the discretionary youth system or with the binding disability-specific set-aside.

The evidence points to the Pre-ETS margin. Employment gains do not scale with discretionary Title I Youth spending. VR administrative records show that WIOA changed both the population served by vocational rehabilitation and the services delivered to transition-age youth. The ACS program-margin estimates move in the direction predicted by employment entry, with reductions in Medicaid and SSI receipt and an increase in employer-sponsored insurance. None of these analyses separately identifies service receipt as a causal mediator. Their role is to discipline the interpretation of the ACS employment estimate.

A. Discretionary Spending

The state-spending test asks whether the ACS employment effect is larger in states that spent more of their available Title I Youth funds. If the employment gain reflects the broader discretionary youth system, higher spending states should exhibit larger reductions in the disability employment gap among Pre-ETS-eligible cohorts. If the employment gain reflects the Pre-ETS set-aside, the relationship with Title I Youth spending should be weaker because the set-aside applied through the vocational rehabilitation system and was mandatory across states (Appendix Table A5).

We test this prediction by interacting the baseline disability-by-exposure term with the state-year Title I Youth expenditure rate. The expenditure rate is demeaned relative to the

national state-year average, so the lower-order exposure coefficient is evaluated at average spending intensity. The estimating equation is

$$y_{ist} = \alpha + \beta_1 (D_i \times E_i \times Z_{st}) + \beta_2 (D_i \times E_i) + \beta_3 (D_i \times Z_{st}) + \beta_4 (E_i \times Z_{st}) + X_{it}' \Gamma + \tau_s + \phi_t + \epsilon_{ist}. \quad (4)$$

Here Z_{st} is the state-year Title I Youth expenditure rate. The coefficient β_1 measures whether the disability gap narrowed more for exposed cohorts in states with higher youth-spending intensity. The specification includes the individual-level and state-level controls used in the baseline models, along with state and birth-year fixed effects. Standard errors are clustered by state. The event-study versions reported in Appendix Figures A2 to A4 show no evidence that spending intensity was differentially related to pre-existing disability-gap trends.

The spending-intensity estimates differ sharply across outcomes, reported in Table 4. For high-school completion, the triple interaction is positive and statistically significant. States that spent a larger share of available Youth funds experienced larger reductions in the disability graduation gap among exposed cohorts. This pattern is consistent with a role for discretionary youth spending on educational attainment, where local workforce programming may help students remain attached to school or complete a credential.

The employment result is different. The triple interaction for employment is close to zero and statistically insignificant. Reductions in the disability employment gap do not scale with the share of Youth funds that states spent. The earnings interaction is also statistically insignificant. These null relationships do not prove that Title I Youth services had no role in transition outcomes. They do rule against the most direct alternative interpretation of the ACS employment result, which is that the estimated employment gain is simply larger in states that deployed more discretionary youth funds.

This distinction is central to the paper's interpretation. The schooling evidence is more consistent with the broader youth system. The employment evidence is more consistent with a structural feature common to all states, the mandatory Pre-ETS set-aside. We therefore interpret the state-spending analysis as separating two policy margins. Discretionary Title I Youth spending

appears relevant for schooling, while the employment effect does not load on that discretionary spending margin.

B. Vocational Rehabilitation Service Delivery

RSA-911 records provide descriptive evidence on how WIOA changed the vocational rehabilitation system serving transition-age youth. These records are not population data. They observe youth who entered the VR system, and they do not include all students who received Pre-ETS without opening a full VR case. For that reason, the RSA-911 estimates should not be interpreted as causal estimates of the national policy effect. They are useful because they show whether the institutional channel implied by the ACS results is visible in the service-delivery system that implemented Pre-ETS. (Appendix Table A8)

The post-WIOA VR caseload shifted toward youth with greater transition-service needs (Table 5). Transition-age VR recipients were more likely after WIOA to be Black or Hispanic, to receive Medicaid, and to have cognitive or psychosocial disabilities. This compositional change is consistent with the ACS heterogeneity results. The ACS employment gain is largest for youth with cognitive disabilities, and the demographic estimates show detectable gains for Hispanic youth and positive but imprecise gains for Black youth. The administrative records therefore suggest that WIOA changed not only the services delivered by VR agencies but also the population that reached those services. One channel is access: because administrative and application barriers shape who reaches disability programs, screening out those with fewer resources (Deshpande and Li 2019), delivering Pre-ETS in schools rather than through VR intake should reach youth who would otherwise stay outside the system. That a race-neutral set-aside widened access for minority youth speaks to concerns that WIOA underserves Black workers (Camardelle 2021).

Service receipt also shifted toward employment-focused activities. Job placement assistance, VR counseling and guidance, job search assistance, supported employment, and job readiness training all increased after WIOA. At the same time, approximately one-fifth of post-WIOA transition-age youth in the VR records received at least one recorded Pre-ETS activity. Work-based learning and workplace-readiness training were the most common recorded Pre-ETS components, at 14 percent and 15 percent. Postsecondary-enrollment counseling and self-advocacy instruction

were each recorded for about 12 percent of cases. Diagnosis and treatment, the one explicitly health-related service category in the RSA-911 files, remained a small part of the case mix (Appendix Table A6).

The components most directly tied to employment content are also the components most strongly associated with competitive integrated employment at closure. Among WIOA-era VR participants with an Individualized Plan for Employment, work-based learning and workplace-readiness training predict competitive integrated employment, after controlling for demographics, disability severity, benefits receipt, education, state fixed effects, and eligibility-year fixed effects. Postsecondary-enrollment counseling and self-advocacy instruction do not show the same relationship. These associations are descriptive because service receipt is not randomly assigned. They nevertheless align with the ACS interpretation. The labor-market effect appears most connected to services that create work exposure, workplace preparation, and employer-facing transition activity.

The ACS and RSA-911 analyses therefore identify different layers of the reform. The ACS estimates the population-level effect of Pre-ETS age eligibility on the disability employment gap. The RSA-911 records show how the VR system changed among youth who entered that system. The administrative evidence cannot by itself identify the national effect of Pre-ETS, but it supports the mechanism implied by the cohort design. WIOA moved VR service delivery toward earlier and more employment-focused engagement, and the employment-oriented Pre-ETS components are the components most closely associated with competitive integrated employment.

C. Program Margins

Table 6 reports other program-margin estimates using ACS to provide a final check on the interpretation of the employment effect. If Pre-ETS eligibility moved youth with disabilities into employment, then public-program participation should shift in ways consistent with labor-market entry. Public coverage and disability-benefit receipt should fall, while employer-sponsored coverage should rise.

The three-arm estimates show this pattern. Pre-ETS eligibility reduces Medicaid enrollment by

2.41 percentage points and SSI receipt by 2.07 percentage points. Employer-sponsored insurance rises by 1.58 percentage points, although the estimate is less precise. The estimates for any welfare income and any Social Security income are close to zero and statistically insignificant. The Medicaid reduction is similar in magnitude to the 2.5 percentage-point employment effect, and the SSI reduction is large enough to matter for the fiscal accounting in the next section.

These program-margin estimates should be interpreted as corroborating evidence, not as a separate causal welfare analysis. We use the same three-arm cohort design as the employment estimates, and they move in the direction predicted by employment entry. They do not identify the full household insurance response, the persistence of benefit changes, or the longer-run pathway into SSDI. What they establish is narrower and sufficient for the paper's interpretation. The employment gain is accompanied by reductions in public coverage and SSI receipt and by an increase in employer-sponsored insurance.

The channel evidence is therefore internally consistent. Discretionary Title I Youth spending does not predict the employment effect. The VR system shifted toward employment-focused transition supports. The Pre-ETS components most directly connected to labor-market preparation are associated with competitive integrated employment at closure. Public-program margins move in the direction implied by employment entry. The evidence supports the interpretation that the binding categorical Pre-ETS mandate, rather than the broader discretionary youth system, is the operative margin behind the ACS employment result.

VII. Welfare Implications

The employment estimates imply economically meaningful gains relative to the federal cost of the Pre-ETS set-aside. This section reports reduced-form welfare accounting rather than a full welfare analysis. The estimates do not identify persistence, general-equilibrium effects, or the value of all nonmarket outcomes. They do, however, allow the direct earnings gains and first-order fiscal offsets to be compared with the federal expenditure required by the mandate.

The statutory cost is transparent. Pre-ETS requires state vocational rehabilitation agencies to reserve 15 percent of federal vocational rehabilitation funds for transition services to students with disabilities ages 14 to 21. Federal VR appropriations averaged roughly 3.8 billion dollars annually

over the sample period, which implies an annual Pre-ETS set-aside of approximately 570 million dollars.

The direct earnings calculation applies the estimated employment effect to the annual stock of transition-age youth with disabilities in secondary school. That population is approximately 4.2 million individuals in steady state. A 2.5 percentage-point employment gain implies roughly 105,000 additional employed young adults with disabilities. Weighted mean annual earnings among employed young adults with disabilities in the ACS are approximately 27,700 dollars. Multiplying the induced employment count by this earnings measure gives aggregate annual earnings gains of about 2.9 billion dollars.

This calculation implies a benefit-cost ratio above five when additional earnings are valued at their full amount. A more conservative assumption that values additional earnings at 60 percent, allowing for foregone leisure and displacement margins that the reduced-form design does not separately identify, still yields a ratio above three. The direct earnings gain therefore exceeds the annual federal set-aside under conservative parameter values.

The fiscal margin reinforces this conclusion. Transition-age youth with disabilities who leave school without competitive employment are a source of future SSI and SSDI participation (Deshpande 2016). The ACS estimates show that Pre-ETS eligibility reduces SSI receipt by 2.07 percentage points among eligible cohorts. Applied to the same transition-age population, this estimate implies roughly 87,000 fewer SSI recipients. At an average annual SSI benefit of 8,400 dollars, the implied reduction in federal transfers is approximately 730 million dollars per year. This fiscal offset is comparable in magnitude to the 570 million dollar federal Pre-ETS set-aside. The aggregate SSA trend-break evidence in Appendix Table A7 is consistent with this direction, but it is descriptive because all states were treated at the same time.

The same accounting can be expressed as a marginal value of public funds (MVPF, Hendren and Sprung Keyser 2020). In this setting, the numerator is the value of the earnings gain to youth with disabilities. The denominator is the net government cost, defined as the Pre-ETS set-aside net of fiscal offsets from reduced disability-benefit receipt. With a full value on earnings, the numerator is 2.9 billion dollars. With the SSI offset estimated from the ACS, the net government cost is

approximately negative 160 million dollars. Under this parameterization, the program pays for itself through reduced SSI receipt before counting Medicaid savings or SSDI-related margins. Under the more conservative case that values earnings at 60 percent and assigns no fiscal offset, the numerator is about 1.7 billion dollars and the denominator is 570 million dollars, which implies an MVPF of about 3.1.

Table 7 reports the same calculation over a range of assumptions. The rows vary the shadow value on additional earnings from 0.4 to 1.0. The columns vary the assumed SSI or SSDI diversion rate from zero to 30 percent. The benefit-cost ratio exceeds one in every cell and ranges from 2.04 under the most conservative displayed case to 5.73 under the least conservative displayed case. Moderate assumptions place the ratio between about 3.5 and 4.5.

These calculations should be read as policy accounting anchored in the reduced-form estimates, not as a complete welfare model. The ACS design identifies the effect of Pre-ETS eligibility on employment and related program margins. It does not identify how long the employment gains persist, how induced employment affects non-disabled workers, how families value the transition from public benefits to employment, or whether later SSDI claims are permanently reduced. The evidence nevertheless supports a strong first-order conclusion. The direct earnings gain alone exceeds the federal cost of the set-aside by several multiples, and observed reductions in SSI receipt strengthen rather than weaken the economic case for preserving the categorical mandate.

VIII. Conclusion

WIOA made the school-to-work transition of youth with disabilities a central federal workforce-policy objective. Its most distinctive disability provision was not simply an expansion of youth services, but a categorical mandate requiring state vocational rehabilitation agencies to reserve at least 15 percent of federal vocational rehabilitation funds for Pre-Employment Transition Services. This paper estimates the population effect of that mandate using age-based eligibility for Pre-ETS and a just-ineligible comparison group that remained eligible for Title I Youth services but not for Pre-ETS.

The main finding is that Pre-ETS eligibility reduced the youth disability employment gap before the pandemic. Pre-pandemic, the cohort analysis implies a 2.5 percentage-point reduction in the disability employment gap, equal to about 6.5 percent of the pre-WIOA gap. The three-arm decomposition attributes this gain to Pre-ETS eligibility rather than to the broader Title I Youth system. Cohorts that were eligible for general youth services but too old for Pre-ETS show no comparable employment improvement. The effect is concentrated among youth with cognitive disabilities, the group for whom early work exposure, workplace-readiness training, and transition planning are most likely to change the path from school to employment.

The interpretation is supported by three additional pieces of evidence. Employment gains do not scale with states' discretionary Title I Youth spending, which points away from general youth expenditures as the operative margin. Vocational rehabilitation records show that WIOA shifted service delivery toward employment-focused transition supports, and the Pre-ETS components most directly connected to work preparation are associated with competitive integrated employment at closure. Program-margin estimates also move in the direction predicted by employment entry. Pre-ETS eligibility reduces Medicaid and SSI receipt and increases employer-sponsored insurance.

The evidence should be interpreted with the design in mind. The main estimates are intent-to-treat effects of age-based eligibility rather than treatment-on-the-treated effects of service receipt. The ACS data do not observe whether a student received Pre-ETS. The cohort design also relies on the assumption that, absent WIOA, disability gaps would not have changed discretely at the Pre-ETS eligibility cutoff. We support this assumption with pre-period detrending, a within-control placebo, flat national pre-trends, alternative detrending specifications, two-way clustering, wild-cluster bootstrap inference, and cohort event-study sensitivity analysis. The estimates for earnings and high-school completion are less robust than the employment estimates and should be interpreted as secondary outcomes. The administrative analyses identify plausible mechanisms, but they are descriptive because RSA-911 records observe only youth who enter the vocational rehabilitation system.

The post-pandemic estimates reinforce the importance of separating the pre- and post-2020 labor markets. After 2020, the disability employment gap closed sharply during a period of remote-work expansion and strong labor demand. That shift affected workers with disabilities through channels outside WIOA and weakens the interpretation of later cohort contrasts as estimates of the statute. The pre-pandemic estimate is therefore the paper's primary quantitative claim. The post-pandemic analysis shows that the adjacent-cohort design is more stable than the two-group comparison, but it does not provide the preferred estimate of the Pre-ETS effect.

The policy implication follows directly from the identified margin. The employment gain is tied to the categorical Pre-ETS set-aside, not to discretionary Title I Youth spending. Reauthorization proposals that preserve the set-aside preserve the statutory feature associated with the estimated employment effect. Proposals that consolidate WIOA funds into a more discretionary block-grant structure would change that margin. The welfare accounting is consistent with the same conclusion. Applying the 2.5 percentage-point employment effect to the annual stock of transition-age youth with disabilities implies aggregate earnings gains near 2.9 billion dollars per year, compared with an annual federal Pre-ETS set-aside of roughly 570 million dollars. Reductions in SSI receipt provide an additional fiscal offset.

The broader contribution is to the economics of workforce and disability policy. National reforms often bundle several provisions and leave no untreated jurisdiction for a standard state-level comparison. This paper shows that age-based eligibility, combined with a just-ineligible comparison group, can isolate a specific provision inside such a reform when the institutional cutoff is sharp and economically meaningful. In this setting, that provision is the Pre-ETS set-aside. The evidence shows that a modest but binding disability specific mandate can move employment at population scale, even when broader discretionary youth spending does not predict the same labor-market gain.

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Tables

Table 1. Analytical Samples

Panel A. ACS analytical sample summary statistics (ages 24 to 35, survey years 2011 to 2024).

Variable	No Disability	Any Disability
Age	29.48	29.55
Female	0.50	0.46
White	0.71	0.72
Black	0.15	0.19
Hispanic	0.21	0.18
Asian	0.08	0.04
Cognitive disability	0.00	0.62
Physical disability	0.00	0.27
Sensory disability	0.00	0.30
Less than high school	0.09	0.18
High school / GED	0.23	0.35
Some college	0.30	0.31
Bachelor's or higher	0.38	0.16
Employed	0.80	0.46
Completed high school	0.91	0.82
Annual earnings (2017 \$)	\$41,479	\$27,800
Observations	4,464,680	369,406

Panel B. RSA-911 sample derivation (transition-age youth, ages 14 to 21 at application).

Analytical subsample	Definition	N
Transition-age youth cases	Program years 2014–2019	313,890
Eligible and closed cases	Valid eligibility and closure dates, U.S. states	272,283
WIOA-era IPE sample	Individualized Plan for Employment	160,768
Wage sample	Non-missing log hourly wage	111,593
Post-WIOA case-composition sample	Program years 2017–2019	560,306

Notes: Panel A reports ACS means, weighted by person weights, for Pre-ETS-eligible cohorts (ages 14–21 in 2015) and never-eligible cohorts (ages 25–35), observed at ages 24–35, 2011–2024; earnings are conditional on positive earnings. Panel B reports the RSA-911 case-derivation sequence and the analytic subsamples used in the VR tables.

Table 2. WIOA and the Disability Employment Gap, Before and After the Pandemic

	Pre-COVID (2011–2019)	Post-COVID (2021–2024)
A. Two-group cohort DiD (ages 14–21 vs. 25–35)		
Exposed × Disabled, linear detrend	0.025*** (0.009)	–0.001 (0.006)
Exposed × Disabled, quadratic detrend	0.032*** (0.009)	0.177*** (0.006)
Within-control placebo (25–27 vs. 28–35)	0.002 (0.003)	–0.018*** (0.007)
B. Three-arm decomposition		
Exposed × Disabled (14–21)	0.026*** (0.009)	–0.001 (0.006)
Partial × Disabled (22–24)	0.000 (0.005)	–0.007 (0.005)
Pre-ETS effect (Exposed – Partial)	0.025** (0.011)	0.007 (0.005)

Notes: Difference-in-differences estimates of the disability employment gap (ACS, ages 24–35). “Partial” denotes cohorts ages 22–24 in 2015, eligible for Title I youth services but not Pre-ETS. Models include state and birth-year fixed effects. Standard errors clustered by state in parentheses; stars on the Pre-ETS effect test equality of the Exposed and Partial coefficients. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3. Heterogeneity in the Pre-ETS Employment Effect

Subgroup	Pre-ETS effect (Exposed – Partial)
Panel A. By disability type	
Cognitive	0.035*** (0.011)
Physical	0.002 (0.020)
Sensory	0.017 (0.014)
Panel B. By gender and race / ethnicity	
Male	0.031** (0.012)
Female	0.012 (0.013)
White	0.027** (0.010)
Black	0.023 (0.022)
Hispanic	0.038*** (0.013)

Notes: Each cell is the Pre-ETS employment effect (Exposed minus Partial) from the three-arm cohort design, pre-COVID (2011–2019), ages 24–35. Standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4. State WIOA Youth Spending Intensity and Disability Gaps (DDD Estimates)

	(1) HS Graduation	(2) Employment	(3) Log Earnings
Exposed × Disabled	0.024*** (0.004)	0.049*** (0.005)	0.073*** (0.012)
Exposed × Disabled × Spending (DDD)	0.068*** (0.025)	–0.046 (0.043)	–0.101 (0.127)
Baseline mean	0.920	0.779	10.11
Observations	2,677,909	2,677,909	2,159,791
R-squared	0.052	0.105	0.146

Notes: Triple-difference estimates of disability × Pre-ETS exposure × the demeaned state-year Title I Youth expenditure rate. Models include state and birth-year fixed effects and the individual and state controls described in the text. Standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5. RSA-911 Transition-Age Youth Cases, Pre- and Post-WIOA

	Pre-WIOA	Post-WIOA
Panel A. Caseload composition (means)		
Age at application	17.6	18.1
Female	0.41	0.38
White	0.77	0.74
Black	0.20	0.23
Hispanic	0.14	0.18
Cognitive	0.59	0.63
Psychosocial	0.17	0.22
Orthopedic or neurological	0.053	0.026
Other physical	0.051	0.033
Sensory or communication	0.085	0.034
Significantly disabled	0.36	0.35
Most significantly disabled	0.62	0.63
Receives SSDI	0.031	0.027
Receives SSI	0.19	0.17
Medicaid at application	0.34	0.42
N (cases)	113,318	200,572
Panel B. Service receipt (share of cases)		
Had an IPE	0.989	0.815
VR counseling and guidance	0.233	0.315
Diagnosis and treatment	0.039	0.049
Assessment services	0.120	0.140
Vocational / occupational training	0.011	0.012
Community college training	0.021	0.010
On-the-job training	0.009	0.016
Job readiness training	0.028	0.036
Job search assistance	0.082	0.099
Job placement assistance	0.057	0.083
Supported employment	0.025	0.033
Any Pre-ETS (post only)	-	0.201
Pre-ETS work-based learning	-	0.142
Pre-ETS enrollment counseling	-	0.125
Pre-ETS workplace readiness	-	0.149
Pre-ETS self-advocacy	-	0.116

Notes: RSA-911 national VR case-closure records for transition-age youth (ages 14–21 at application). Pre-WIOA is program years 2014–2015; post-WIOA is 2016–2019. All Panel A and B differences are significant at the 1 percent level, except for the vocational and occupational training row. Pre-ETS did not exist under WIA and is reported for post-WIOA cases only.

Table 6. Corroboration from Non-Employment Margins in the ACS

Outcome	Exposed × Disabled	Partial × Disabled	Pre-ETS effect (E – P)
Medicaid enrollment	–0.034*** (0.006)	–0.010** (0.004)	–0.024*** (0.006)
SSI receipt	–0.032*** (0.006)	–0.012*** (0.004)	–0.021*** (0.006)
Employer-sponsored insurance	+0.022*** (0.007)	+0.006 (0.005)	+0.016* (0.009)
Any welfare income	–0.002 (0.004)	+0.002 (0.002)	–0.004 (0.004)
Any Social Security income	+0.004 (0.004)	+0.003 (0.003)	+0.001 (0.005)
Observations			3,611,589

Notes: Three-arm cohort difference-in-differences estimates (pre-COVID ACS, 2011–2019, ages 24–35) with two-stage linear detrending on the never-eligible cohorts. Exposed cohorts are ages 14–21 in 2015; Partial cohorts are ages 22–24. Controls, fixed effects, and weights are as in Table 2. Standard errors clustered by state in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7. Benefit-Cost Ratio of the Pre-ETS Mandate Under Alternative Parameters

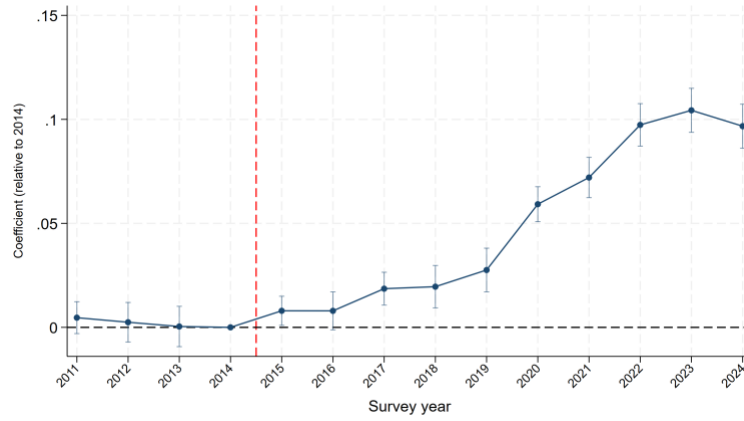
Shadow value on earnings	No fiscal offset	10% diversion	20% diversion	30% diversion
0.4	2.04	2.25	2.46	2.67
0.6	3.06	3.27	3.48	3.69
0.8	4.08	4.29	4.50	4.71
1.0 (full)	5.10	5.31	5.52	5.73

Notes: Benefit-cost ratio equals annual benefit divided by the \$570 million annual federal Pre-ETS outlay. Benefit equals (shadow value) × (\$2.9 billion earnings gain) + (diversion rate) × (105,000 induced-employed) × (\$11,400 average annual SSI/SSDI benefit). Population: 4.2 million transition-age youth with disabilities in secondary school (IDEA data). Employment effect is 2.5 percentage points (Table 2); mean earnings \$27,700 (Table 1).

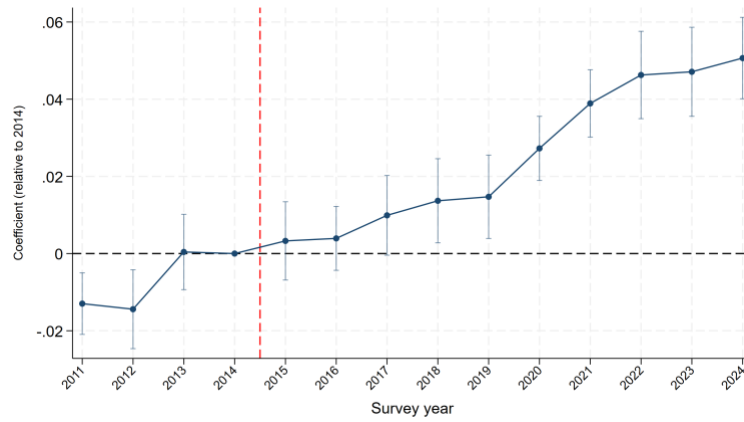
Figures

Figure 1. Event Studies of the Disability Gap Around WIOA, 2011 to 2024

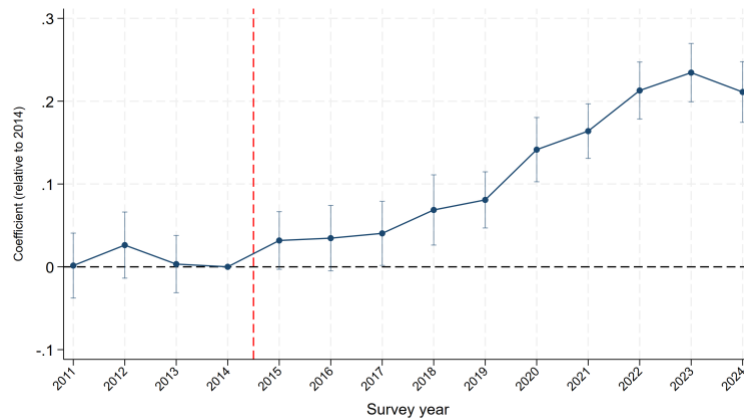
Panel A. Employment.



Panel B. High-school completion.



Panel C. Log earnings.



Notes: Event-study estimates of the disability gap in the ACS relative to 2014, ages 24 to 35, controlling for demographics and state-year policy controls with state and age fixed effects. Vertical dashed line marks WIOA implementation (2015). Panel A shows employment. Panel B shows high-school completion. Panel C shows log annual earnings among adults with positive earnings.