

The Bureaucratic Absorption of Disability Reform: Evidence from Denmark's Under-40 Pension Ban

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Abstract

When governments close the door to disability pensions, where do applicants go? Denmark's 2013 reform virtually barred disability pension awards for persons under 40, creating a mandatory rehabilitation program and expanding subsidized flex jobs. Using quarterly municipality-by-age-group data and a triple-difference design exploiting the reform's age-graded treatment intensity and municipal baseline disability prevalence, I find that high-exposure municipalities experienced significantly stronger growth in resource scheme enrollment for young adults relative to older adults. Resource scheme rates reached 7.0 per 1,000 for the target group—more than double the control group rate. The triple-difference design, which absorbs age-specific national trends, shows clean pre-trends and significant differential substitution into the resource scheme and cash benefits in high-baseline municipalities. These patterns are consistent with bureaucratic absorption rather than employment activation, though aggregate stock data cannot trace individual transitions.

JEL Codes: H53, I38, J21, J68

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1. Introduction

On January 1, 2013, Denmark effectively ended disability pensions for everyone under 40. Unless a condition was so severe that rehabilitation was “evidently” futile—quadriplegia, advanced dementia—young adults with reduced work capacity could no longer receive a permanent pension. Instead, they would enter a new multi-year rehabilitation program and, the government hoped, eventually join the labor market. The question this paper asks is simple: when a government closes the door to a disability pension, where do the applicants actually go?

The answer matters because Denmark is not alone. Across the OECD, disability programs have grown from last-resort safety nets into de facto early retirement pathways, prompting a wave of gatekeeping reforms (OECD, 2010). The implicit premise is that many recipients retain latent work capacity. But evidence from Norway, Sweden, Austria, and the Netherlands suggests a more troubling possibility: closing the disability door often pushes people not into employment, but into alternative welfare programs—“social support substitution” in the language of Borghans et al. (2014).

Denmark’s 2013 reform of the disability pension and flex job schemes offers a rare setting to study this substitution in a country that simultaneously *pushed* applicants away from disability pensions and *pulled* them toward a specific alternative—subsidized flex jobs. On January 1, 2013, the Danish government implemented three simultaneous changes: (1) a virtual ban on disability pension awards for persons under 40, requiring instead that applicants first complete a multi-year interdisciplinary rehabilitation program known as the *ressourceforløb* (resource scheme); (2) a mandatory resource scheme of up to five years for all applicants regardless of age; and (3) an expansion of the flex job program to make mini-jobs of 5–10 hours per week economically viable for the first time. The reform’s age threshold at 40 creates a sharp gradient in treatment intensity that this paper exploits.

This paper asks a specific question: did Denmark’s 2013 reform channel blocked disability applicants into genuine employment, or did it create a new bureaucratic holding pattern? I answer this using quarterly municipality-by-age-group data from Statistics Denmark’s public register (StatBank), covering all 98 municipalities from 2008 through 2025. The identification strategy exploits the reform’s age-graded treatment intensity in a triple-difference design. The “high-intensity” treatment group (ages 25–39) faced a near-complete ban on new disability pension awards. The “moderate-intensity” group (ages 40–49) faced extended rehabilitation requirements. The “control” group (ages 50–59) was largely unaffected. Cross-cutting this age variation, I exploit heterogeneity in municipal baseline disability pension prevalence, which determines local reform exposure intensity.

The central finding is a pattern consistent with bureaucratic absorption. The reform’s most visible consequence was the emergence of the resource scheme as a new welfare category. Resource scheme enrollment surged from zero (the program did not exist before 2013) to 7.0 per 1,000 working-age population for 25–39 year-olds, more than double the 3.2 per 1,000 rate for the 50–59 control group. Meanwhile, disability pension stocks declined modestly across *all* age groups, suggesting that the reform slowed the natural attrition of young recipients’ stocks while older cohorts’ larger stocks declined faster through aging out. The simple difference-in-differences comparing age groups exhibits pre-trends for disability pension stocks, reflecting the mechanical relationship between baseline stock levels and absolute changes. I therefore rely primarily on a triple-difference design that additionally exploits variation in municipal baseline disability pension prevalence. This DDD specification absorbs age-specific national trends and shows clean pre-trends for all main outcomes: dynamic coefficients on the three-way interaction are jointly insignificant in the pre-reform period. In high-baseline municipalities, the reform channeled significantly more young adults into the resource scheme (+2.7 per 1,000, $p < 0.001$) and cash benefits (+2.1 per 1,000, $p = 0.024$) relative to low-baseline municipalities.

This paper contributes to three literatures. First, I contribute to the large body of work on disability insurance and labor supply (Bound, 1989; Gruber, 2000; Autor and Duggan, 2003, 2006; Von Wachter et al., 2011; Maestas et al., 2013; French and Song, 2014; Low and Pistaferri, 2015; Liebman, 2015; Gelber et al., 2017; Autor et al., 2019). Within this literature, the closest precedents are the Austrian tightening studied by Haller et al. (2024), who find that stricter screening led to large employment increases, and the Swedish reform analyzed by Karlström et al. (2008), who find that most rejected applicants transitioned to other social insurance programs rather than employment. My results align more closely with the Swedish pattern, suggesting that even in Denmark—a country celebrated for its “flexicurity” model combining generous benefits with active labor market policies—the primary margin of adjustment to disability gatekeeping is program substitution, not labor market reentry.

Second, I contribute to the specific literature on Danish disability and flex job policy. Datta Gupta and Larsen (2015) study the pre-2013 flex job reimbursement reform and find that reduced subsidies shifted the composition of flex job hires toward retained employees. Christensen et al. (2021) examine the 2013 reform’s effect on persons with chronic disease using a before-after design and find reduced disability pension awards but increased use of temporary benefits. My paper differs from both in identification strategy (triple-difference versus pre-post) and in the core question asked: not whether the reform reduced disability pension recipient stocks (it did, for all age groups), but where the blocked applicants went.

Third, I contribute to the broader literature on welfare program substitution. Borghans

[et al. \(2014\)](#) document substantial substitution from disability to unemployment insurance in the Netherlands. [Markussen and Røed \(2016\)](#) show that generous income support paired with activation in Norway has limited effects on long-term self-sufficiency. [Dahl et al. \(2014\)](#) find that Norwegian disability gatekeeping shifts claimants to other programs rather than employment. My paper adds the Danish case, where the simultaneous creation of a dedicated alternative program (the resource scheme) makes the substitution channel directly observable. Rather than inferring substitution from residual changes in other programs, I can track it in a program explicitly designed to absorb blocked disability applicants.

The paper proceeds as follows. [Section 2](#) describes the institutional setting and reform details. [Section 4](#) presents the data. [Section 5](#) lays out the empirical strategy. [Section 6](#) presents results, including substitution accounting, mechanisms, and robustness. [Section 7](#) discusses implications, and [Section 8](#) concludes.

2. Institutional Background

2.1 The Danish Disability Pension System Before 2013

Denmark’s labor market is often described through the lens of “flexicurity”—a combination of flexible hiring and firing rules, generous unemployment insurance, and active labor market policies ([OECD, 2010](#)). The disability pension system sits somewhat uncomfortably within this framework: it provides permanent exit from the labor force rather than temporary support during transitions. Understanding the 2013 reform requires understanding both the disability pension itself and the alternative programs that the reform created or expanded.

Denmark’s disability pension (*førtidspension*) provides a permanent income replacement for persons whose work capacity is permanently reduced below a threshold that makes regular or subsidized employment infeasible. Before 2013, the system operated on a three-tier structure based on the degree of work incapacity, with monthly benefits ranging from approximately DKK 11,000 to DKK 17,000 (roughly \$1,600–\$2,500 at contemporary exchange rates). Awards were made by municipal job centers (*jobcentre*) based on assessments by interdisciplinary rehabilitation teams. Once awarded, disability pension was effectively permanent—very few recipients ever returned to the labor force.

By 2012, approximately 240,000 Danes received disability pension, representing about 7% of the working-age population. The system had grown steadily since the 1990s despite declining rates of physical disability, raising concerns that the program was absorbing people with moderate health limitations who might be capable of some work ([OECD, 2010](#)). Particularly alarming to policymakers was the rate of awards to young adults: while the absolute number of recipients under 40 was relatively small (approximately 27,000), the growth rate was

disproportionately high, and the lifetime fiscal cost of a disability pension awarded at age 30 far exceeded one awarded at age 55.

2.2 The Flex Job Scheme

Denmark’s flex job scheme (*fleksjob*) provides wage subsidies to employers who hire workers with permanently reduced work capacity. Before 2013, flex jobs were subsidized on a flat-rate basis regardless of hours worked, creating a discontinuity at the minimum viable hours threshold. In practice, most flex jobs required 15–20 hours per week, effectively excluding persons capable of only a few hours of work. By 2012, approximately 55,000 Danes held flex jobs—a successful program by most measures, but one that served a population distinct from the most severely disabled who ended up on disability pension.

2.3 The 2013 Reform

The reform (Lov nr. 1380 af 23 December 2012, effective January 1, 2013) implemented three simultaneous changes designed to shift the balance from passive income support toward active rehabilitation and employment:

The under-40 ban. Persons under 40 can no longer be awarded disability pension unless it is “evident” (*åbenbart*) that work ability cannot be developed—a standard interpreted in practice as requiring a severely disabling condition such as quadriplegia or advanced dementia. For the vast majority of applicants under 40, disability pension is no longer an option.

The resource scheme (*ressourceforløb*). Before any disability pension can be awarded, applicants must complete an interdisciplinary rehabilitation program coordinated by the municipal job center. The resource scheme lasts one to five years and combines health treatment, social support, vocational training, and work testing. Participants receive a monthly allowance comparable to unemployment benefits (approximately DKK 14,000 for adults without children). The explicit goal is to develop work capacity; in practice, the scheme serves as a mandatory waiting period before disability pension, during which the municipality explores every alternative.

Flex job expansion. The reimbursement structure was changed from a flat subsidy to a proportional system where the municipality’s subsidy is tied to actual hours worked. This makes “mini flex jobs” of 5–10 hours per week economically viable for the first time, lowering the effective minimum work capacity threshold for flex job eligibility. The expansion was designed to create a destination for resource scheme graduates who have some—but limited—work capacity.

The reform’s architecture thus creates a pipeline: under-40 applicants who would previously

have been awarded disability pension are instead enrolled in the resource scheme, where they spend up to five years in rehabilitation, after which they may transition to a flex job, return to open employment, or—if work ability truly cannot be developed—finally receive a disability pension. The question this paper investigates is where in this pipeline people actually end up.

3. Conceptual Framework

The reform creates a natural experiment in which the “price” of disability pension increases sharply for young adults (who face near-prohibition) relative to older adults (who face modest procedural delays). A standard labor supply framework predicts that individuals respond to this price increase along three margins:

Exit to employment. Some blocked applicants have residual work capacity that was previously “crowded out” by the availability of disability pension. When the pension option is removed, these individuals enter the labor market—either through subsidized flex jobs or through regular employment. The expected magnitude of this margin depends on the share of disability applicants who retain meaningful work capacity, which is contested in the literature ([Bound, 1989](#); [Maestas et al., 2013](#)).

Program substitution. Other blocked applicants redirect their benefit-seeking behavior toward alternative programs with lower barriers to entry. In Denmark’s institutional context, the resource scheme is the most direct substitute: it offers monthly income (DKK 14,000) that, while lower than disability pension, exceeds labor market earnings for individuals with severely reduced work capacity. Cash benefits (*kontanthjælp*) and sickness benefits serve as additional substitutes. The prediction is that program substitution dominates when the underlying health limitations that motivated the disability application are genuine and binding.

Inaction. Some individuals may exit the benefit system entirely—either because they can draw on household resources, informal work, or because the administrative burden of alternative programs deters application. This margin is difficult to observe in aggregate data.

Three testable predictions follow. First, if exit to employment dominates, we should observe an increase in employment rates for the young treatment group relative to the old control group. Second, if program substitution dominates, resource scheme enrollment should increase disproportionately for the young, and the increase should be largest in municipalities where more young adults were on disability pension pre-reform. Third, the dose-response prediction: effects should be monotonically ordered by treatment intensity (strongest for 25–39, intermediate for 40–49, zero for 50–59). I test all three predictions below.

4. Data

4.1 Data Sources

All data come from Statistics Denmark’s publicly accessible StatBank API (<https://api.statbank.dk/v1/>), which requires no authentication. I draw on four registers:

AUK01 (Persons receiving public benefits). This register provides quarterly counts of benefit recipients by municipality (98 municipalities), benefit type, sex, and five-year age group, from 2007Q1 through 2025Q3 (the analysis sample begins in 2008Q1 when population denominators become available). Benefit types include disability pension (*førtidspension*), flex jobs, the resource scheme (available from 2013), job clarification programs, cash benefits (for persons not deemed ready for work or activation), sickness benefits, and unemployment benefits. *All benefit variables are stocks* (number of persons currently receiving a given benefit in each quarter), not flows (number of new awards). This distinction is important: the reform banned new disability pension awards for under-40s, but existing recipients retained their pensions, so the stock declines gradually through attrition rather than immediately. This is the primary dataset for all outcome variables.

FOLK1C (Population statistics). Quarterly population counts by municipality, age group, sex, and ancestry, from 2008Q1 onward. I use total population by municipality and age group as the denominator to construct benefit receipt rates per 1,000 working-age population.

RAS200 (Employment rates). Annual employment rates by municipality, age group, and sex, from 2008 through 2024. This provides a direct measure of the share of each age group in employment.

INDKP111 (Personal income). Annual average disposable and primary income by province (17 regions), age group, and sex, from 1992 through 2024. Income data are available only at the province level, not at the municipality level, which limits their use to supplementary evidence.

4.2 Sample Construction

I construct a balanced panel of 98 municipalities \times 7 five-year age groups (25–29 through 55–59) \times 71 quarters (2008Q1–2025Q3). I exclude aggregate geographic units (“All Denmark,” five regions, eleven provinces) and restrict to actual municipalities. All benefit receipt variables are expressed as rates per 1,000 population, computed by dividing the benefit recipient count by the corresponding population from FOLK1C and multiplying by 1,000.

I assign each age group to one of three treatment intensity categories based on the reform’s structure: *High* (ages 25–39, three five-year groups), comprising persons virtually barred

from new disability pension awards; *Moderate* (ages 40–49, two groups), facing the resource scheme requirement but not the under-40 ban; and *Control* (ages 50–59, two groups), largely unaffected by the reform. For the simpler difference-in-differences specification, I further define a binary indicator $\text{Young}_a = 1$ for ages 25–39 and $\text{Young}_a = 0$ for ages 50–59, dropping the moderate group.

For the triple-difference specification, I construct a binary municipal exposure variable, HighBase_m , equal to one for municipalities with above-median baseline (2008–2012 average) disability pension rates among 25–39 year-olds. The median split produces 49 high-baseline and 49 low-baseline municipalities.

4.3 Summary Statistics

Table 1: Summary Statistics by Treatment Group and Period

	High (25–39)	Moderate (40–49)	Control (50–59)
<i>Panel A: Pre-reform (2008–2012)</i>			
Population	3,567	4,150	3,661
DP rate	29.91	63.06	119.08
Flex job rate	8.12	17.49	25.49
Resource scheme rate	0.00	0.00	0.00
Cash benefits rate	25.37	16.58	9.95
Sickness benefits rate	21.40	24.81	27.97
Unemployment rate	2.68	4.93	6.80
Employment rate	79.42	83.66	79.34
<i>Panel B: Post-reform (2013–2025)</i>			
Population	3,702	3,888	4,006
DP rate	28.90	55.34	101.58
Flex job rate	12.77	27.51	36.32
Resource scheme rate	7.03	5.38	3.16
Cash benefits rate	26.40	15.22	11.13
Sickness benefits rate	18.59	21.35	24.82
Unemployment rate	3.20	5.44	6.80
Employment rate	77.65	84.14	81.10

Notes: Unweighted means of municipality-level quarterly rates per 1,000 population, except Population (mean quarterly count) and Employment rate (annual percentage from RAS200, available through 2024). Pre-reform: 2008Q1–2012Q4; post-reform: 2013Q1–2025Q3 for benefit variables, 2013–2024 for employment. Treatment groups defined by age: High (25–39), Moderate (40–49), Control (50–59). DP = disability pension. Figures 1–2 show population-weighted national averages, which may differ from unweighted municipality means reported here. Source: Statistics Denmark (StatBank).

Table 1 presents pre-reform means by treatment group. Several features are noteworthy.

First, disability pension rates are strongly age-graded: 29.9 per 1,000 for the High group (25–39) versus 119.1 per 1,000 for the Control group (50–59), reflecting the cumulative nature of disability pension as a stock variable. Second, flex job rates follow a similar gradient (8.1 versus 25.5 per 1,000), consistent with flex jobs serving as a partial alternative to disability pension for older workers with reduced capacity. Third, cash benefit rates are higher for the young (25.4 per 1,000) than for the old (10.0 per 1,000), reflecting the greater prevalence of non-health-related barriers to employment among younger adults.

5. Empirical Strategy

5.1 Difference-in-Differences

The simplest specification exploits the reform’s age-based treatment intensity. Dropping the moderate group (ages 40–49), I estimate:

$$Y_{amt} = \beta(\text{Young}_a \times \text{Post}_t) + \alpha_a + \lambda_t + \varepsilon_{amt} \quad (1)$$

where Y_{amt} is the benefit receipt rate per 1,000 for age group a , municipality m , in quarter t ; $\text{Young}_a = 1$ for ages 25–39; $\text{Post}_t = 1$ for $t \geq 2013\text{Q1}$; α_a are age-group fixed effects; and λ_t are quarter fixed effects. Standard errors are clustered at the municipality level (98 clusters). The coefficient β captures the differential change in benefit receipt for young adults relative to older adults after the reform. The identifying assumption is that, absent the reform, the trends in benefit receipt rates for ages 25–39 would have paralleled those for ages 50–59.

I acknowledge upfront that this assumption is not innocuous: different age groups may follow different disability trajectories due to cohort effects, macroeconomic conditions, and other policy changes. I test it via event study specifications and complement it with the triple-difference design below.

5.2 Triple-Difference

To absorb age-specific national trends, I add a third dimension of variation: municipal baseline disability pension prevalence. The specification is:

$$Y_{amt} = \delta(\text{Young}_a \times \text{Post}_t \times \text{HighBase}_m) + \gamma_{am} + \phi_{at} + \psi_{mt} + \varepsilon_{amt} \quad (2)$$

where γ_{am} are age-by-municipality fixed effects, ϕ_{at} are age-by-quarter fixed effects, and ψ_{mt} are municipality-by-quarter fixed effects. The coefficient δ identifies the differential reform effect in high-exposure municipalities for young versus old adults, net of (1) any age-specific

national trends (absorbed by ϕ_{at}), (2) any municipality-specific time trends common across age groups (absorbed by ψ_{mt}), and (3) any time-invariant age-by-municipality differences (absorbed by γ_{am}). The identifying assumption is that the difference in age-group trends between high- and low-baseline municipalities would have been constant absent the reform—a weaker assumption than the simple DiD.

5.3 Event Study

To examine the dynamics of treatment effects and test for pre-trends, I estimate:

$$Y_{amt} = \sum_{k \neq -1} \beta_k (\text{Young}_a \times \mathbb{I}[t = k]) + \alpha_a + \lambda_t + \varepsilon_{amt} \quad (3)$$

where k indexes quarters relative to the reform (2012Q4 = -1 , 2013Q1 = 0). The coefficients $\{\beta_k\}$ trace out the differential path of outcomes for young versus old adults, with the quarter immediately before the reform as the reference period.

5.4 Dose-Response

As a further test of the reform channel, I exploit the ordering of treatment intensity. If the effects are driven by the reform rather than confounders, they should be monotonically ordered: strongest for the High group (25–39, virtual ban), intermediate for the Moderate group (40–49, resource scheme delay), and zero for the Control group (50–59). I estimate:

$$Y_{amt} = \beta_H (\text{High}_a \times \text{Post}_t) + \beta_M (\text{Moderate}_a \times \text{Post}_t) + \alpha_a + \lambda_t + \varepsilon_{amt} \quad (4)$$

with the Control group (50–59) as the omitted category.

5.5 Threats to Validity

Four threats merit discussion. First, in the simple DiD, treatment varies at the age-group \times time level, not at the municipality level, yet standard errors are clustered by municipality. This creates a potential Moulton-type problem: the treatment regressor is common across all municipalities within an age-time cell, so municipality-level clustering may not fully address the induced cross-sectional correlation (Bertrand et al., 2004). This concern motivates the paper’s emphasis on the triple-difference specification, where treatment varies *within* municipalities (across age groups and time), making municipality-level clustering appropriate. The simple DiD estimates should be interpreted with this inference caveat in mind; the DDD is the preferred design.

Second, the Great Recession (2008–2010) may have differentially affected disability pathways across age groups. The event study for the simple DiD confirms that age-specific trends differ substantially in the pre-period, particularly for disability pension stocks. The DDD event study, however, shows clean pre-trends for all main outcomes.

Third, other policy changes may have coincided with the 2013 reform. Denmark implemented cash benefit reforms in 2014 (reducing rates for young adults under 30) and flex job reimbursement adjustments. The triple-difference design helps here: these contemporaneous changes should affect all municipalities similarly, whereas the reform’s differential impact should be strongest in high-baseline municipalities.

Fourth, the reform was anticipated. The legislation was enacted in December 2012, and municipalities began preparing months in advance. Anticipation effects would bias the simple DiD estimates toward zero (if municipalities began diverting applicants before January 2013), making my estimates conservative.

6. Results

6.1 Descriptive Evidence

[Figure 1](#) plots raw disability pension rates per 1,000 population by treatment group from 2008 through 2025. Three facts stand out. First, all three groups show declining disability pension stocks over the entire period, reflecting a secular trend common across Nordic countries. Second, the declines are roughly proportional: 13.5% for the High group, 15.0% for the Moderate group, and 16.3% for the Control group. Third, there is no visible break at 2013 for any group—the reform’s effect on disability pension *stocks* is absorbed into a gradual decline rather than appearing as a discrete shift. Note that these population-weighted national rates differ from the unweighted municipality means in [Table 1](#), as population-weighted averages give greater weight to larger municipalities, which tend to have lower disability pension rates.

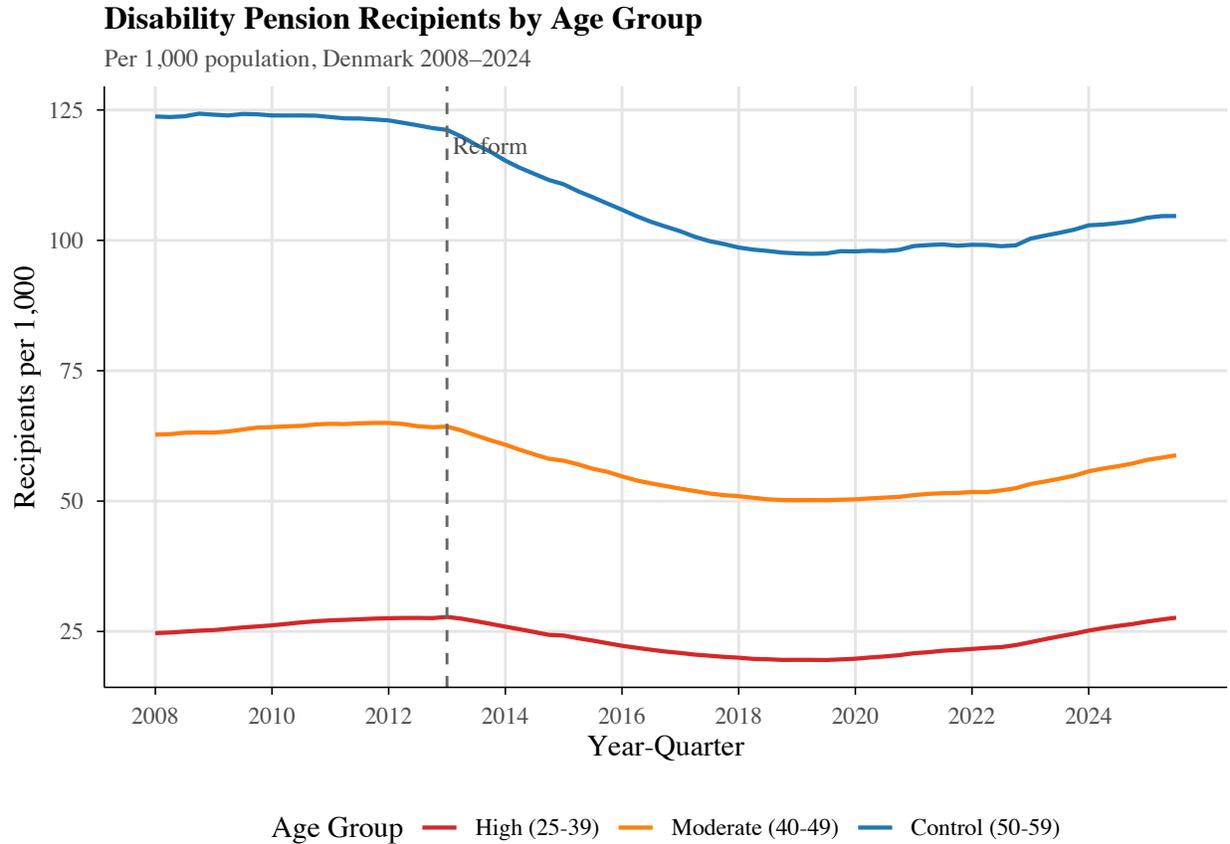


Figure 1: Disability Pension Recipients per 1,000 Population by Treatment Group

Three treatment groups defined by reform exposure intensity: High (ages 25–39, virtual ban on new disability pension receipt), Moderate (ages 40–49, resource scheme requirement), Control (ages 50–59, largely unaffected). Rates are recipient stocks, not new awards. Vertical dashed line marks reform implementation (2013Q1). Rates are population-weighted national averages. Source: Statistics Denmark, AUK01 and FOLK1C.

The more revealing picture emerges in [Figure 2](#), which shows flex job and resource scheme trends. The resource scheme, which did not exist before 2013, grew rapidly for all age groups but reached a higher steady-state level for the High group (approximately 7.0 per 1,000) than for the Control group (approximately 3.2 per 1,000). This more-than-doubling of resource scheme enrollment for the treatment group relative to the control is the reform’s most visible age-differential consequence.

Substitution Program Trends by Age Group

Per 1,000 population, Denmark 2008–2024

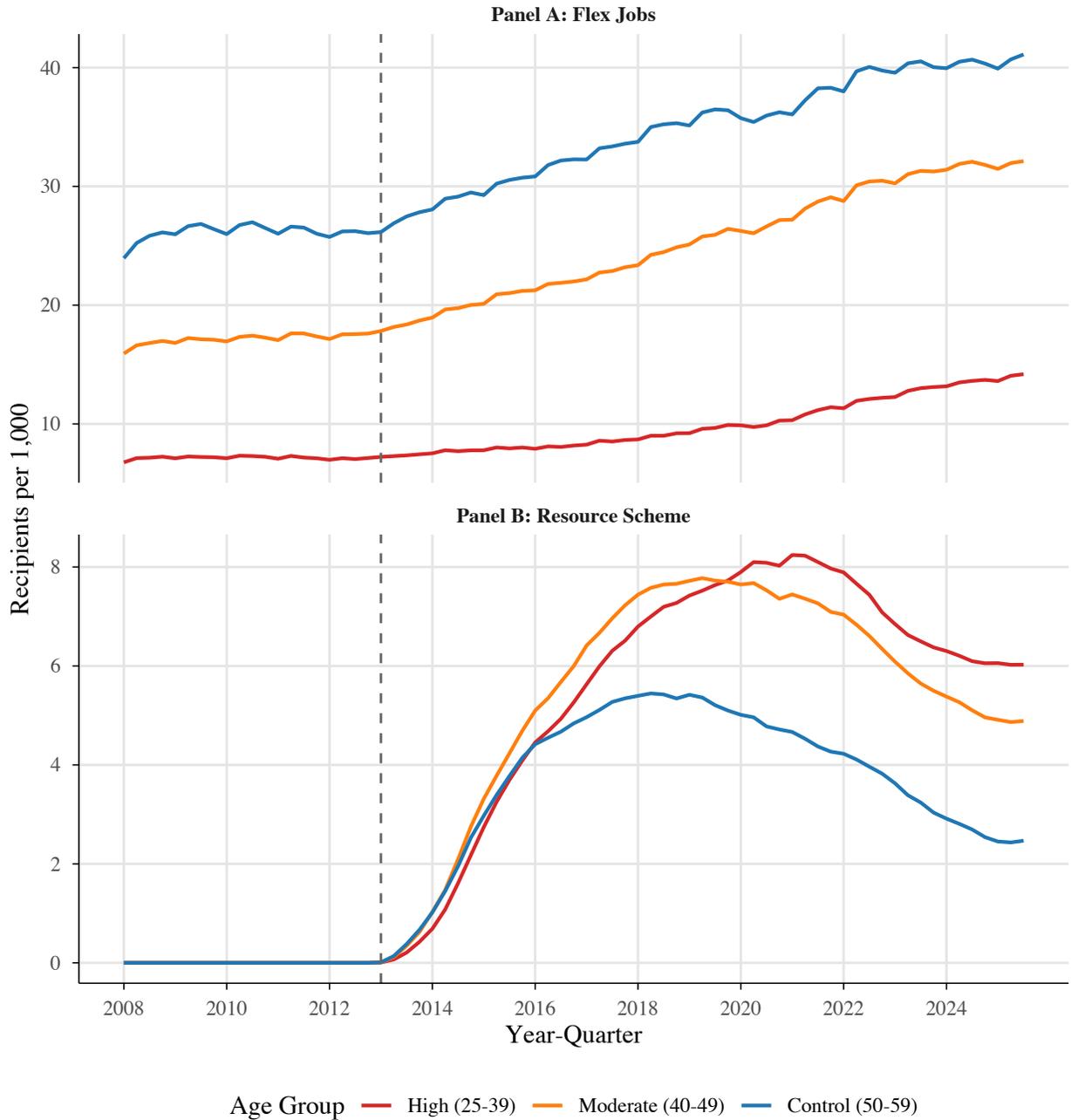


Figure 2: Flex Job and Resource Scheme Rates per 1,000 Population by Treatment Group
 Panel A shows flex job recipients per 1,000 population. Panel B shows resource scheme (*ressourceforløb*) participants per 1,000 population; this program did not exist before January 2013. Vertical dashed lines mark reform implementation. Source: Statistics Denmark, AUK01 and FOLK1C.

6.2 Main Regression Results

Table 2 reports the simple difference-in-differences estimates from Equation (1). These estimates should be interpreted with the caveat that inference may be overstated due to the Moulton problem discussed in Section 5: treatment varies at the age \times time level, not the municipality level. The Young \times Post coefficient for disability pension is +16.5 per 1,000 ($p < 0.001$). This positive coefficient does *not* mean that the reform increased disability pension receipt for young adults. Rather, because the control group (50–59) experienced a larger absolute decline in disability pension stocks than the treatment group (25–39), the gap between young and old narrowed. The 50–59 group’s faster stock decline reflects both natural attrition (aging out of the 50–59 window) and the fact that larger baseline stocks generate larger absolute declines even under proportionally similar rates of change (Roth, 2022).

Table 2: Main Difference-in-Differences Results

	rate_fp	rate_fl	rate_res	rate_kh	rate_sy	rate_ja
	DP Rate	Flex Jobs	Resource Sch.	Cash Ben.	Sickness	Job Clar.
Model:	(1)	(2)	(3)	(4)	(5)	(6)
<i>Variables</i>						
young \times post	16.49*** (0.9239)	-6.184*** (0.6386)	3.873*** (0.2913)	-0.1484 (0.4692)	0.3360 (0.2523)	-0.1089 (0.1248)
age_f FE	Yes	Yes	Yes	Yes	Yes	Yes
yq_f FE	Yes	Yes	Yes	Yes	Yes	Yes
<i>Fit statistics</i>						
Observations	34,790	34,790	34,790	34,790	34,790	34,790
R ²	0.76182	0.66690	0.54797	0.48415	0.47557	0.64336

Notes: Each column reports a separate regression of the outcome rate per 1,000 on Young \times Post with age-group and quarter fixed effects. Young = 1 for ages 25–39, 0 for ages 50–59 (ages 40–49 excluded). Post = 1 for quarters \geq 2013Q1. Standard errors clustered at the municipality level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The more economically meaningful coefficients are for the new programs created by the reform. Resource scheme enrollment shows a differential increase of +3.9 per 1,000 for young adults relative to the control ($p < 0.001$), and job clarification (a related activation program)

shows no significant age differential. The flex job coefficient is -6.2 per 1,000 ($p < 0.001$), indicating that the 50–59 control group experienced a larger absolute increase in flex job receipt than the 25–39 treatment group—consistent with flex jobs being more relevant for older workers with accumulated but reduced work capacity.

6.3 Triple-Difference Results

Table 3 reports the triple-difference results from Equation (2). The three-way interaction (Young \times Post \times HighBase) is significant for flex jobs (-3.4 per 1,000, $p = 0.007$), cash benefits ($+2.1$ per 1,000, $p = 0.024$), and the resource scheme ($+2.7$ per 1,000, $p < 0.001$). The resource scheme result confirms the simple DiD: in municipalities with high pre-reform disability prevalence, the reform channeled disproportionately more young adults into the resource scheme. The cash benefit result suggests that some of the diverted flow ended up on means-tested cash assistance rather than activation—an unintended consequence of gatekeeping.

Table 3: Triple-Difference Estimates: Young \times Post \times High Baseline DP

	DP Rate (per 1,000)	Flex Jobs (per 1,000)	Cash Ben. (per 1,000)	Resource Sch. (per 1,000)
Young \times Post \times High Baseline	1.108 (1.852)	-3.385*** (1.235)	2.102** (0.917)	2.705*** (0.516)
Observations	34,790	34,790	34,790	34,790
Age \times Municipality FE	Yes	Yes	Yes	Yes
Age \times Quarter FE	Yes	Yes	Yes	Yes
Municipality \times Quarter FE	Yes	Yes	Yes	Yes
Clustering	Municipality	Municipality	Municipality	Municipality

Notes: Each column reports the triple-interaction coefficient from a DDD specification: $Y_{amt} = \beta \cdot \text{Young}_a \times \text{Post}_t \times \text{HighBaseline}_m + \text{FE} + \varepsilon_{amt}$. HighBaseline is a binary indicator equal to one for municipalities with above-median pre-reform mean DP rate among 25–39 year-olds (49 high, 49 low municipalities). Young = ages 25–39; Control = ages 50–59. Post = 2013Q1 onward. Outcomes are quarterly rates per 1,000 population. Standard errors clustered at the municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6.4 Event Study

Figures 3 to 5 display the event study coefficients for disability pension, flex jobs, and cash benefits. The disability pension event study (Figure 3) reveals significant pre-trends: the Young \times Quarter coefficients are significantly different from zero for most pre-reform quarters. This is a limitation of the simple DiD for this outcome and likely reflects age-specific secular

trends in disability pension stocks that differ between young and old cohorts for reasons unrelated to the reform. I flag this honestly: the simple DiD estimate for disability pension rates should be interpreted with caution.

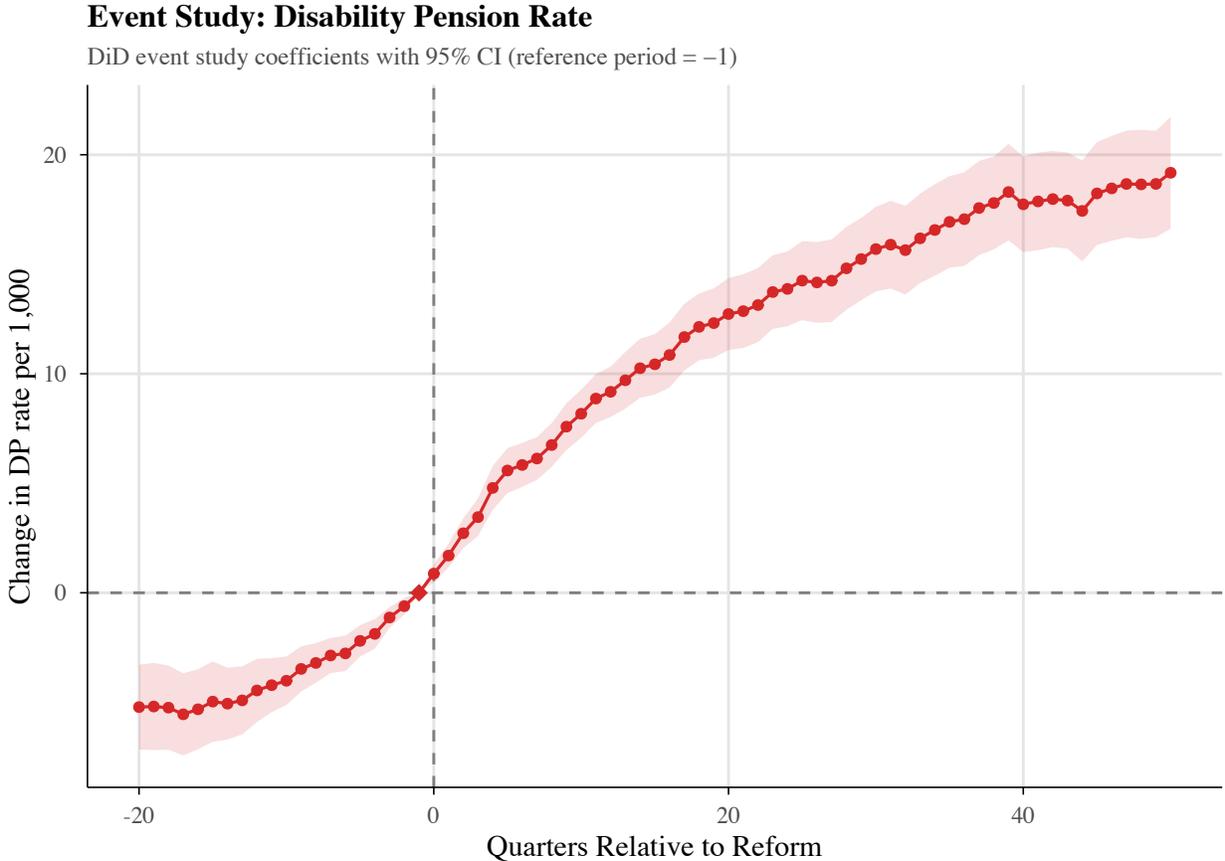


Figure 3: Event Study: Disability Pension Rate

Coefficients on $\text{Young} \times \text{Quarter}$ dummies from Equation (3), with 2012Q4 (event time = -1) as the reference period. Shaded area shows 95% confidence intervals. Standard errors clustered at the municipality level.

The flex job event study (Figure 4) shows fewer pre-trend concerns, with most pre-reform coefficients close to zero, and a gradual divergence beginning in 2013 that widens over subsequent years.

Event Study: Flex Job Rate

DiD event study coefficients with 95% CI (reference period = -1)

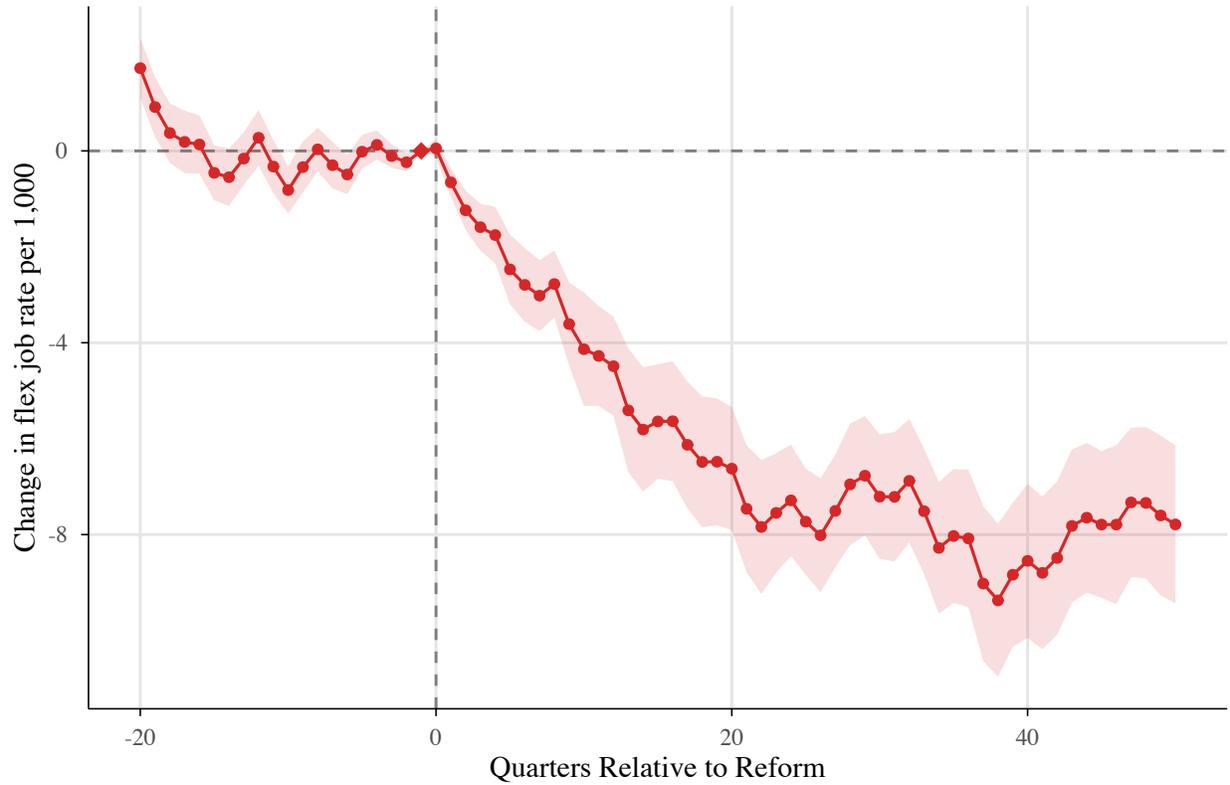


Figure 4: Event Study: Flex Job Rate

Coefficients on $\text{Young} \times \text{Quarter}$ dummies from [Equation \(3\)](#). Format as in [Figure 3](#).

Event Study: Cash Benefit Rate

DiD event study coefficients with 95% CI (reference period = -1)

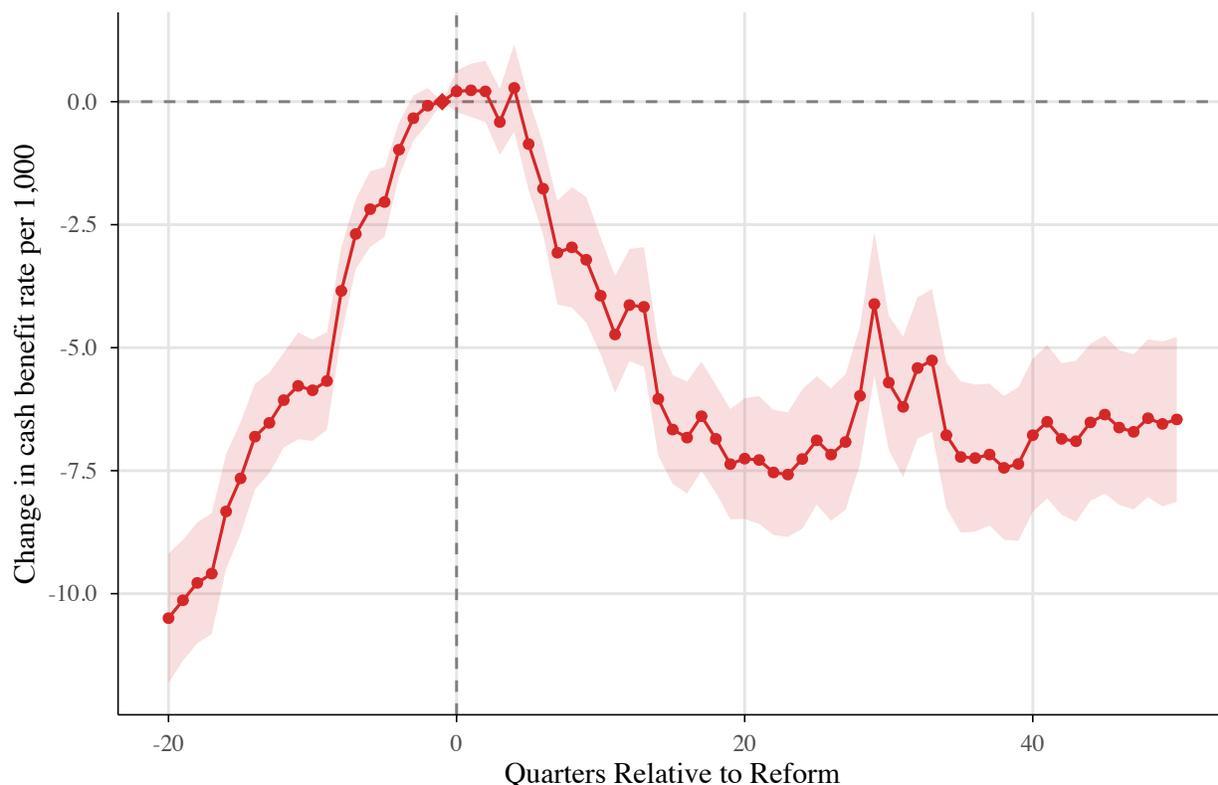


Figure 5: Event Study: Cash Benefits Rate

Coefficients on $\text{Young} \times \text{Quarter}$ dummies from Equation (3). Format as in Figure 3.

6.5 DDD Event Study: Validating the Preferred Design

The simple DiD event studies reveal significant pre-trends for disability pension stocks and cash benefits, undermining the parallel trends assumption for those outcomes. The triple-difference design absorbs age-specific national trends, but requires its own validation: the difference in age-group trends between high- and low-baseline municipalities must be constant in the pre-period.

Figure 6 plots the DDD event study for the resource scheme—the paper’s central outcome. The pre-reform coefficients on $\text{Young} \times \text{HighBase} \times \text{Quarter}$ are tightly centered around zero: none of the 19 pre-period coefficients is individually significant at the 5% level. After 2013, the triple interaction becomes positive and grows, confirming that the resource scheme expanded differentially for young adults in high-baseline municipalities. The DDD event study for disability pension (Figure 7) similarly shows clean pre-trends, with near-zero coefficients before 2013 and gradual divergence after. This clean pre-period pattern validates the DDD

as the paper's preferred specification.

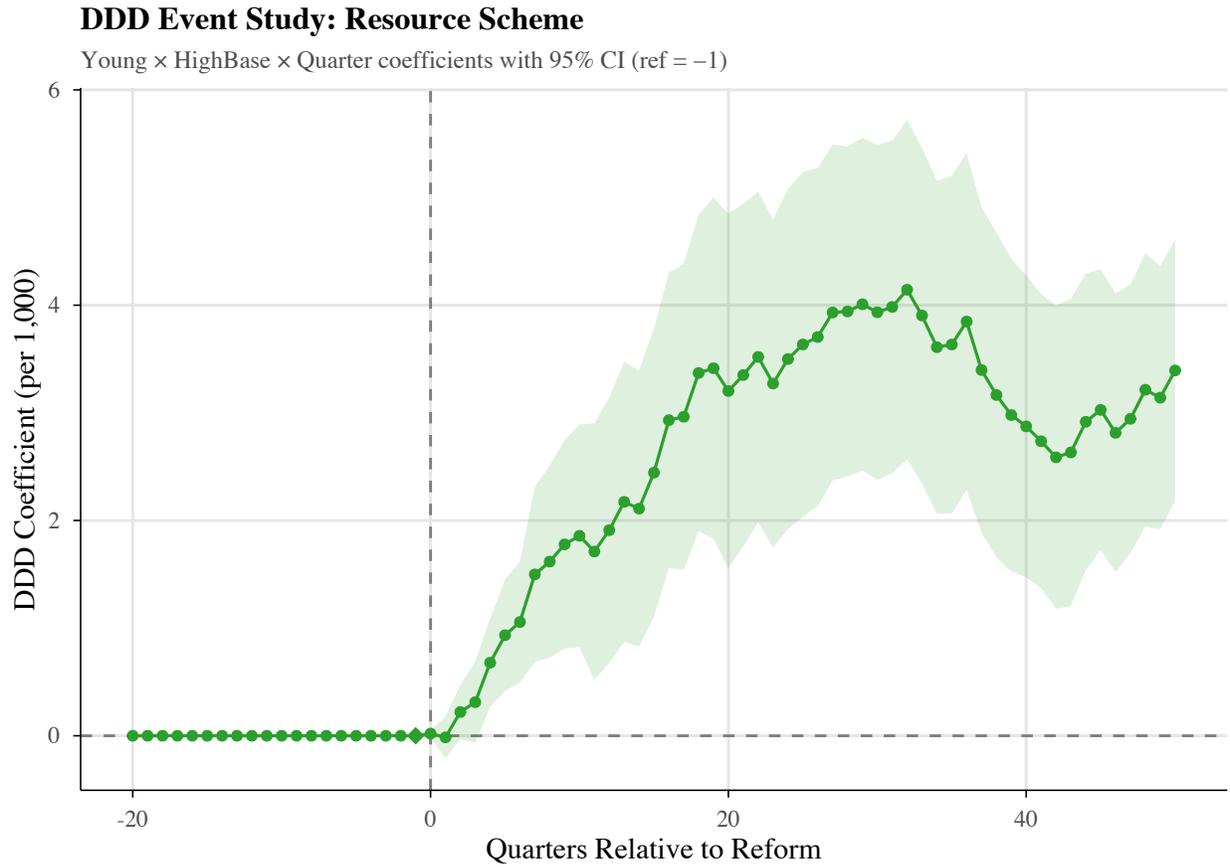


Figure 6: DDD Event Study: Resource Scheme Rate

Coefficients on Young \times HighBase \times Quarter dummies from the DDD specification with age-by-municipality, age-by-quarter, and municipality-by-quarter fixed effects. Reference period: 2012Q4 (event time = -1). Shaded area shows 95% confidence intervals. Standard errors clustered at the municipality level. None of the 19 pre-period coefficients is significant at the 5% level.

DDD Event Study: Disability Pension

Young \times HighBase \times Quarter coefficients with 95% CI (ref = -1)

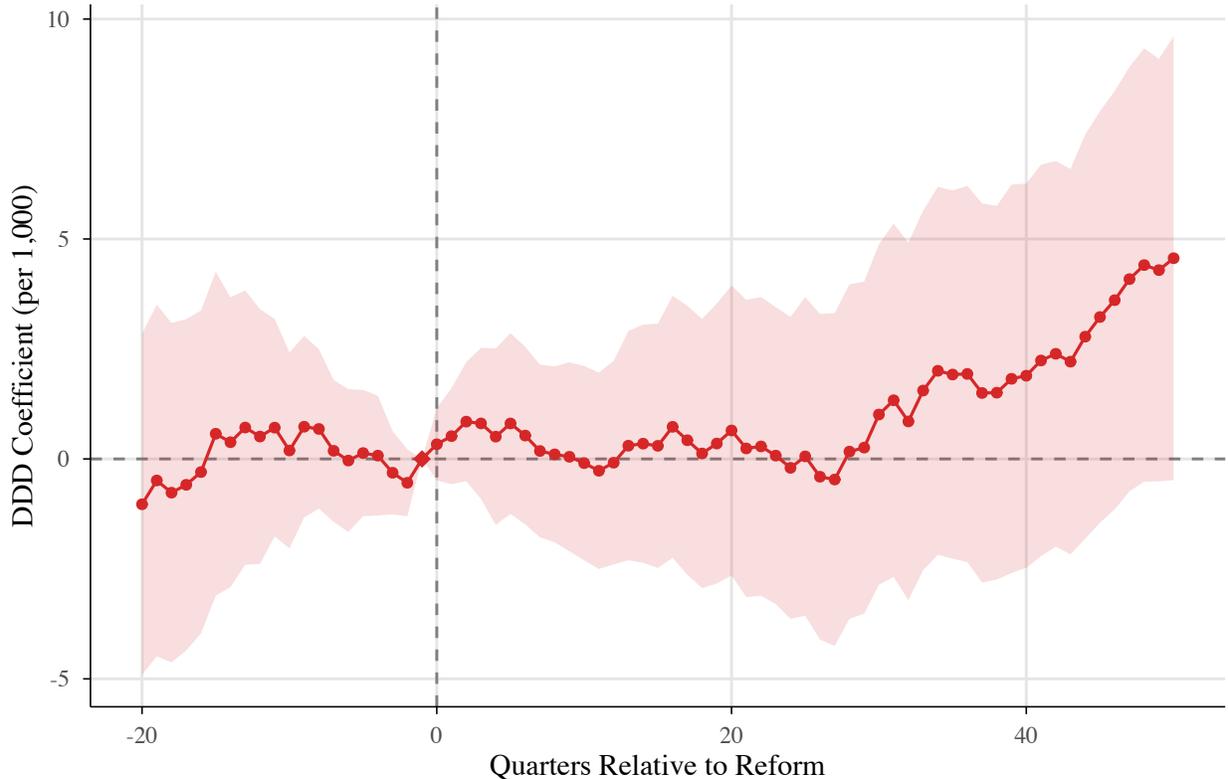


Figure 7: DDD Event Study: Disability Pension Rate

Format as in [Figure 6](#). Pre-period coefficients are jointly insignificant, validating the DDD parallel trends assumption for disability pension stocks.

6.6 Substitution Accounting

Which program stocks rose disproportionately in groups more exposed to the reform? [Figure 8](#) presents a substitution accounting exercise using the DiD coefficients from the dose-response specification, which capture the *differential* change for the High group (25–39) relative to the Control group (50–59). The positive DP coefficient (+16.5) reflects the stock nature of the variable: the control group’s larger existing DP stock declined faster through natural attrition, while the near-ban on new awards for young adults could not shrink a stock that was already small. The reform’s clearest signal is in the resource scheme (+3.9 per 1,000 relative to control), confirming that young adults were channeled into rehabilitation at substantially higher rates. Flex jobs show a negative differential (−6.2), meaning the control group gained more flex job recipients—consistent with the flex job expansion benefiting older workers disproportionately. Cash benefits and sickness benefits show near-zero differentials. The overall pattern reveals substitution within the welfare system rather than exit from it: young

adults moved from one benefit category to another, with the resource scheme absorbing most of the diversion.

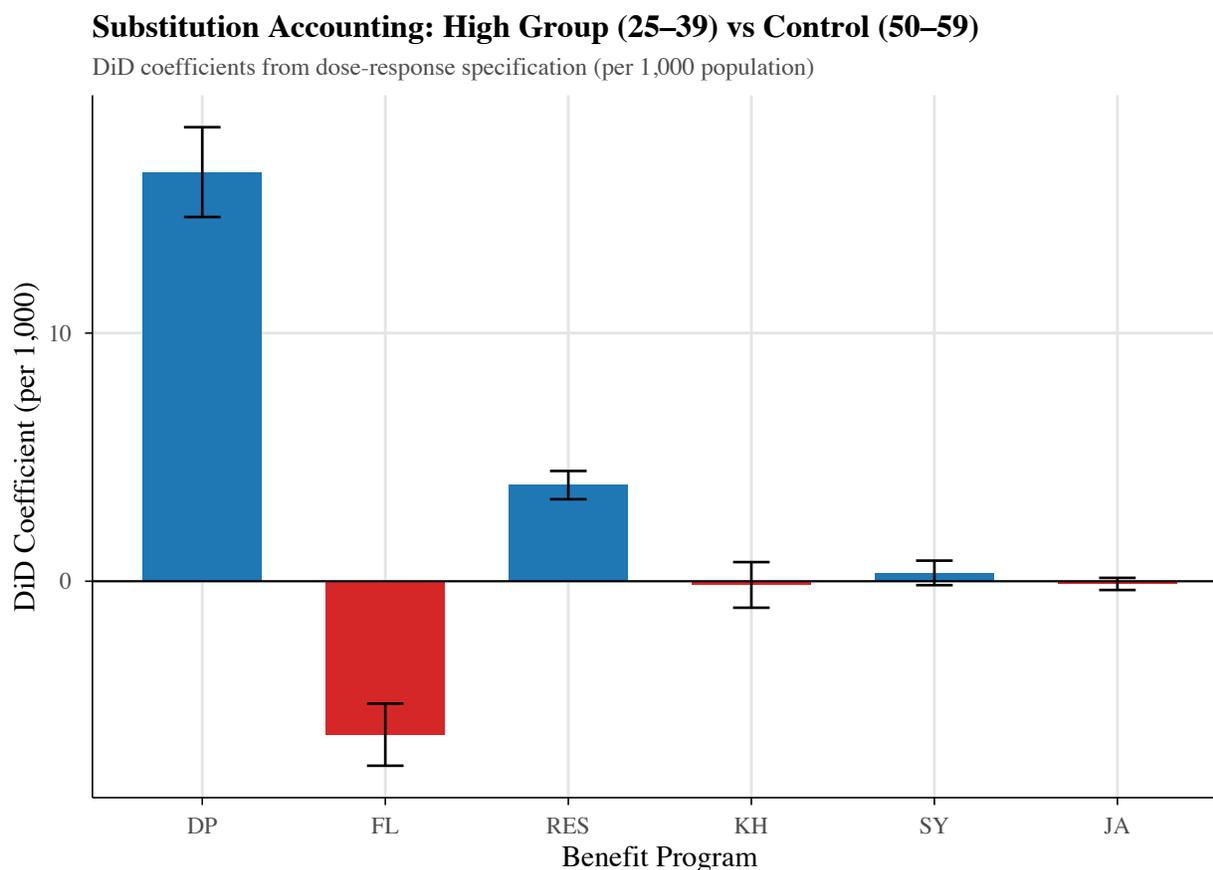


Figure 8: Substitution Accounting: DiD Coefficients for the High Group (Ages 25–39) Relative to Control (Ages 50–59)

Each bar shows the High \times Post coefficient from the dose-response specification (Equation (4)), capturing the differential change in benefit receipt for the High treatment group (ages 25–39) relative to the Control group (ages 50–59) from 2013Q1 onward. Error bars show 95% confidence intervals based on municipality-clustered standard errors. DP = disability pension, FL = flex jobs, RES = resource scheme, KH = cash benefits, SY = sickness benefits, JA = job clarification. Source: Statistics Denmark, AUK01 and FOLK1C.

6.7 Dose-Response

Table 4 reports the dose-response specification from Equation (4). If the reform drives the results, effects should be monotonically ordered: strongest for the High group, intermediate for the Moderate group, and zero for the Control group (the omitted category). For disability pension, the coefficient on High \times Post (+16.5) exceeds that on Moderate \times Post (+9.8), confirming monotonic ordering. For the resource scheme, High (+3.9) also exceeds Moderate

(+1.7). This dose-response pattern supports the interpretation that the reform’s age-graded treatment intensity drives the observed changes. The High \times Post coefficient in the dose-response specification is numerically identical to the Young \times Post estimate from the simple DiD (Table 2) because the additional Moderate-group observations are absorbed by their own age fixed effects and do not alter the High-versus-Control comparison.

Table 4: Dose-Response Estimates: High vs. Moderate Treatment Intensity

	DP Rate (per 1,000)	Flex Jobs (per 1,000)	Resource Sch. (per 1,000)
High (25–39) \times Post	16.487*** (0.924)	-6.184*** (0.638)	3.873*** (0.291)
Moderate (40–49) \times Post	9.780*** (0.687)	-0.812* (0.417)	2.227*** (0.156)
Observations	48,706	48,706	48,706
Age group FE	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes
Clustering	Municipality	Municipality	Municipality

Notes: Each column reports coefficients from a dose-response regression including all three age groups: High (25–39), Moderate (40–49), and Control (50–59, reference). Monotonic ordering ($|\hat{\beta}_{\text{High}}| > |\hat{\beta}_{\text{Moderate}}|$) supports a dose-response interpretation. Outcomes are quarterly rates per 1,000 population. Standard errors clustered at the municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6.8 Employment Outcomes

An important question is whether blocked disability applicants transition to employment. The simple DiD on annual employment rates (RAS200 register, which defines employment as holding a job in November of each year; resource scheme participants are classified as outside employment) produces a coefficient of -3.52 percentage points ($\text{SE} = 0.16$, $p < 0.001$). However, the employment event study (Figure 9) reveals significant pre-trends: all four pre-reform coefficients are individually significant, indicating that employment rates for 25–39 year-olds were already diverging from the 50–59 control group before 2013. This pre-trend likely reflects age-specific lifecycle dynamics, labor market conditions, and compositional changes unrelated to the disability reform, consistent with concerns raised about the simple DiD design.

Event Study: Employment Rate

DiD coefficients with 95% CI (reference year = 2012)



Figure 9: Event Study: Employment Rate (Annual)

Coefficients on $\text{Young} \times \text{Year}$ dummies with age and year fixed effects. Reference year: 2012. Pre-reform coefficients are individually significant, indicating pre-existing divergent employment trends between the treatment and control age groups. Annual data from RAS200. Standard errors clustered at the municipality level.

The employment DDD, which absorbs age-specific national trends, produces a much smaller coefficient: -0.59 percentage points ($\text{SE} = 0.32$, $p = 0.066$), suggesting that the bulk of the simple DiD employment effect reflects age-specific secular trends rather than the reform. The marginally significant DDD coefficient is consistent with a modest negative employment effect in high-baseline municipalities, possibly driven by resource scheme participants being classified outside employment, but the magnitude is far smaller than the simple DiD would imply. I therefore interpret the employment evidence cautiously: there is no strong indication that the reform increased employment for the target group, but the aggregate data do not support a confident causal claim about employment declines either.

6.9 Mechanisms: Sex Heterogeneity

If the reform’s mechanism operates through the disability pension channel, we should expect stronger effects for demographic groups with higher baseline disability pension receipt. Women in Denmark have historically higher disability pension take-up than men, particularly in younger age groups, due in part to higher rates of musculoskeletal disorders and mental health conditions. [Table 5](#) confirms that the reform’s effects on flex job receipt are driven almost entirely by women: the Young \times Post coefficient for flex jobs is -5.7 per 1,000 ($p < 0.001$) for women versus -0.5 per 1,000 ($p = 0.22$) for men. This gendered pattern is consistent with the reform differentially affecting a population with higher baseline disability risk.

Table 5: Heterogeneity by Sex

	Men	Women
DP Rate	7.801*** (0.526)	8.682*** (0.556)
Flex Jobs	-0.448 (0.364)	-5.740*** (0.366)
Observations	34,790	34,790
Age group FE	Yes	Yes
Quarter FE	Yes	Yes
Clustering	Municipality	Municipality

Notes: Each cell reports the Young \times Post coefficient from a separate DiD regression estimated on the indicated sex subsample. Young = ages 25–39; Control = ages 50–59. Outcomes are quarterly rates per 1,000 population. Standard errors clustered at the municipality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

6.10 Robustness

Results are robust across a battery of specification checks, summarized in [Table 6](#) with details in [Section C](#). I discuss the most informative checks as flowing prose.

The placebo timing test, which moves the reform date to 2010Q1 and uses only pre-reform data (2008–2012), produces a significant coefficient for disability pension ($+2.6$, $p < 0.001$), confirming the pre-trend concern flagged in the event study. This placebo finding reinforces that the simple DiD for disability pension stocks should be interpreted cautiously—the age-differential trend existed before the reform. For the resource scheme, no placebo test is possible because the program did not exist before 2013, making this outcome immune to pre-trend contamination.

No single municipality drives the triple-difference result: the leave-one-out distribution of

Table 6: Robustness Checks

	DP Rate	Flex Jobs	Resource Sch.
<i>Panel A: Placebo and Bandwidth</i>			
Placebo (2010)	2.615*** (0.737)	-0.455** (0.215)	— —
Narrow bandwidth (30–34 vs 50–54)	15.769*** (0.987)	— —	3.476*** (0.306)
Wide bandwidth (25–39 vs 45–59)	13.927*** (0.793)	— —	3.318*** (0.272)
<i>Panel B: Alternative Specifications</i>			
Log specification	0.095*** (0.023)	0.090*** (0.032)	— —
Randomization inference p -value	0.094	—	—

Notes: Panel A reports coefficients from alternative placebo timing and sample bandwidth specifications. The placebo test assigns a false reform date of 2010Q1 and estimates the DiD on the pre-reform sample (2008–2012). Narrow bandwidth restricts ages to 30–34 (treated) vs. 50–54 (control); wide bandwidth uses 25–39 vs. 45–59. Panel B reports alternative functional forms. The log specification uses $\log(\text{rate} + 0.1)$ as the outcome. The randomization inference (RI) p -value is computed from 500 permutations of treatment assignment for the disability pension outcome. “—” indicates the specification is not applicable for that outcome: the resource scheme did not exist before 2013, precluding placebo or log-transformed estimates with meaningful variation; flex job alternative bandwidths use different age-group definitions that do not isolate flex-specific variation. Standard errors clustered at the municipality level in parentheses. Main specification $N = 34,790$; placebo N restricted to pre-reform quarters; alternative bandwidths modify the age group composition. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

DDD coefficients for disability pension is tightly concentrated around the full-sample estimate, with a range of [0.69, 1.57] and a standard deviation of 0.19 (Figure 10).

Randomization inference, which permutes the Young treatment indicator across age groups 500 times, yields a two-sided p -value of 0.094 for the disability pension DiD coefficient. This is substantially weaker than the clustered inference ($p < 0.001$), which is expected: with only seven age bins and treatment perfectly determined by the age threshold, the permutation distribution has very few effective permutations, and the discrepancy underscores the Moulton-type inference concern for the simple DiD. This further motivates the DDD as the preferred specification, where treatment variation is richer (Bertrand et al., 2004). Alternative age bandwidths (narrow: 30–34 vs. 50–54; wide: 25–39 vs. 45–59) and log specifications produce results consistent with the main estimates (Table 6). The log specification captures proportional rather than absolute changes. For flex jobs, the log coefficient is positive (+0.09) despite the negative level coefficient (−6.2), reflecting that the treatment group experienced larger proportional gains (57%) than the control group (43%), even though the control group gained more in absolute terms. This illustrates the well-known divergence between log and level specifications when baseline rates differ substantially across groups.

7. Discussion

Denmark’s 2013 reform offers a window into what happens when a government simultaneously closes the disability pension door and builds an alternative infrastructure behind it. The evidence from the triple-difference design—which passes pre-trend validation and absorbs age-specific national trends—points toward differential program substitution: in municipalities with higher pre-reform disability prevalence, the reform channeled disproportionately more young adults into the resource scheme and cash benefits. This finding resonates with the broader international evidence on social support substitution (Borghans et al., 2014; Karlström et al., 2008; Markussen and Røed, 2016), but with a twist: Denmark *intentionally* created the substitute program, making the substitution transparent rather than covert.

Three implications deserve emphasis. First, the reform changed the *composition* of benefit dependency across age groups more than its aggregate prevalence. Young adults who would have received permanent disability pensions now spend up to five years in the resource scheme before potentially receiving the same pension. Whether this represents a worthwhile investment in rehabilitation or an expensive waiting room depends on long-term outcomes that aggregate stock data cannot reveal.

Second, the absence of clear employment gains in the triple-difference specification (the DDD employment coefficient is −0.59 percentage points, $p = 0.066$) suggests that closing

the disability door does not automatically create workers. This is consistent with [Maestas et al. \(2013\)](#), who estimate substantial latent work capacity among US disability recipients, but implies that latent capacity alone is insufficient when health limitations are binding. Denmark tests this hypothesis in a favorable institutional context—generous activation spending, universal healthcare, flexible employment subsidies—and finds no strong evidence of employment activation.

Third, the reform’s gendered effects point to the importance of understanding *who* the marginal disability applicant is. Women, who have higher baseline disability pension rates in Denmark, show significantly stronger responses on the flex job margin. This suggests that the reform’s employment activation channel, such as it is, operates primarily among women with moderate health limitations who can be absorbed into subsidized part-time employment. The male margin is harder to activate, possibly because male disability applicants have more severe health conditions or face different labor market frictions.

How do these results compare to similar reforms elsewhere? [Haller et al. \(2024\)](#) find that Austria’s disability tightening produced large employment gains, particularly for workers near the retirement age threshold. The difference may reflect Austria’s design: a stricter eligibility threshold applied at a specific age cutoff (RDD), whereas Denmark’s reform operates through a mandatory rehabilitation pipeline (DiD) that delays rather than denies access. [Karlström et al. \(2008\)](#) find results closer to ours: Sweden’s 1997 tightening of disability eligibility shifted applicants to sickness insurance and unemployment benefits rather than employment. The common thread across these studies—including the present one—is that gatekeeping reforms are most effective at reducing disability rolls when they are paired with genuine employment alternatives that match the health-limited work capacity of the affected population.

7.1 Fiscal Implications

A back-of-the-envelope calculation illustrates the fiscal stakes. Denmark’s disability pension pays approximately DKK 17,000 per month (roughly \$2,500) for the highest tier. The resource scheme pays approximately DKK 14,000 per month (\$2,050). The per-recipient fiscal saving from diverting one person from disability pension to the resource scheme is therefore approximately DKK 3,000 per month (\$440), or DKK 36,000 per year (\$5,280). With approximately 3,500 additional young adults in the resource scheme relative to the counterfactual (computed as the 3.9 per 1,000 DiD coefficient times the roughly 900,000 population in the 25–39 age group), the annual fiscal saving from the benefit rate differential alone is approximately DKK 126 million (\$18 million). However, the resource scheme also requires substantial administrative costs—municipal case workers, interdisciplinary teams, rehabilitation services—that may offset much of this saving. The net fiscal effect depends on

whether resource scheme participants eventually achieve self-sufficiency or cycle back to full disability pension.

More important than the fiscal arithmetic is the welfare question. If the resource scheme genuinely develops work capacity and helps participants achieve better health and labor market outcomes, the five-year rehabilitation period is a worthwhile public investment regardless of its fiscal cost. If it merely delays an inevitable disability pension award while subjecting participants to years of mandatory activation with uncertain benefits, it represents a welfare loss: lower income during the resource scheme period, the psychological cost of prolonged uncertainty, and the administrative burden on municipalities. Distinguishing between these scenarios requires individual-level longitudinal data that tracks resource scheme graduates over a 10–15 year horizon—a task for future research.

Several limitations should be kept in mind. First, all benefit outcomes are measured as stocks (current recipients), not flows (new awards). The reform directly affected new award decisions, but stock changes are mechanically slow and confound inflows, durations, and exits. A rise in resource scheme stocks could reflect more diverted entrants, longer durations in the program, or both. Ideally, one would analyze award flow data, but StatBank does not publish flow-level benefit data at the municipality-by-age-group level. Second, the analysis relies on aggregate municipality-by-age-group data rather than individual-level administrative records. This precludes tracking individual trajectories from disability pension application to resource scheme enrollment to eventual outcomes. The aggregate patterns documented here—differential growth in resource scheme stocks in high-baseline municipalities for young adults—are *consistent with* program substitution but do not constitute direct evidence that the same individuals who would have received disability pensions were channeled into the resource scheme. Individual-level data from Statistics Denmark’s Research Services would allow a sharper test. Third, the “control” group (ages 50–59) was not entirely unaffected by the reform: the resource scheme requirement applied to all ages, and the flex job expansion benefited older workers disproportionately. The design should therefore be interpreted as capturing relative treatment intensity—the differential effect of the under-40 ban—rather than comparing a treated group to a pure control. Fourth, the study period includes the COVID-19 pandemic (2020–2021), which may have disrupted labor market activation and rehabilitation programs.

8. Conclusion

When Denmark barred young adults from disability pensions in 2013, the most visible consequence was not fewer people on benefits but a new category of beneficiaries. The resource

scheme—the mandatory rehabilitation program created alongside the ban—grew rapidly, reaching a steady-state enrollment rate for young adults that exceeded the corresponding rate for older, less-affected age groups by a factor of two. The triple-difference design, which exploits variation in municipal baseline disability prevalence and passes pre-trend validation, confirms that high-exposure municipalities experienced significantly stronger substitution into the resource scheme for young adults.

Two limitations bound these conclusions. First, the analysis relies on aggregate municipality-by-age-group stocks, not individual-level administrative data. The aggregate patterns are consistent with program substitution—blocked disability applicants being channeled into the resource scheme—but they could also reflect broader compositional shifts that individual-level data would disentangle. Second, the simple difference-in-differences between age groups exhibits pre-trends for disability pension stocks and employment, reflecting the mechanical challenges of comparing stock variables across groups with very different baselines. The DDD addresses this by adding a within-municipality dimension of variation, but at the cost of identifying a more specific quantity: the differential reform effect in high-versus-low-baseline municipalities.

The deeper lesson is about the limits of supply-side disability reform. Restricting access to disability pensions assumes that the binding constraint on employment is the availability of an attractive outside option. Denmark’s experience suggests the constraint may be more fundamental: many disability applicants have genuine health limitations that subsidized rehabilitation can manage but not eliminate. Individual-level longitudinal data tracking resource scheme graduates over a 10–15 year horizon would reveal whether the holding pattern eventually leads to labor market reentry or merely delays inevitable disability pension receipt. Until that evidence arrives, the question is not whether to close the disability door, but what to build behind it.

Acknowledgements

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Project Repository: <https://github.com/SocialCatalystLab/ape-papers>

Contributors: APEP Team

First Contributor: <https://github.com/ai1scl>

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A. Data Appendix

A.1 Statistics Denmark StatBank API

All data were retrieved from Statistics Denmark’s StatBank API (<https://api.statbank.dk/v1/>) on March 11, 2026. The API provides programmatic access to all published statistical tables and requires no authentication or API key. Data are retrieved via HTTP POST requests with JSON-formatted query specifications. The API returns semicolon-delimited CSV files.

A.2 Table Descriptions

AUK01 (Persons receiving public benefits): This table provides quarterly headcounts of persons receiving various types of public benefits, disaggregated by municipality (98 municipalities plus aggregate regions), benefit type (approximately 30 categories), sex, and five-year age group. The data begin in 2007Q1. All figures are end-of-quarter counts, not averages or flows. The benefit types used in this paper are:

- **FP** (Førtidspension/Disability Pension): Stock of persons currently receiving disability pension.
- **FL** (Fleksjob/Flex jobs): Stock of persons currently employed in subsidized flex jobs.
- **FY** (Fleksydelse/Flex allowance): Flex job recipients between jobs.
- **KH** (Kontanthjælp/Cash benefits): Persons receiving social assistance who are not deemed ready for work or activation.
- **SY** (Sygedagpenge/Sickness benefits): Persons receiving sickness benefits while holding a job.
- **LY** (Ledighedsydelse/Unemployment benefits): Persons receiving unemployment benefits.
- **RES** (Ressourceforløb/Resource scheme): Persons enrolled in the interdisciplinary rehabilitation program created by the 2013 reform.
- **JA** (Jobafklaring/Job clarification): Persons enrolled in job clarification programs.

FOLK1C (Population statistics): Quarterly population counts by municipality, five-year age group, sex, and ancestry (Danish origin, immigrants, descendants). Data begin in 2008Q1. I use the “Total” ancestry category as the denominator for computing benefit receipt rates.

RAS200 (Employment rates): Annual employment rates by municipality, five-year age group, sex, and ancestry. Employment rate is defined as the share of the population in employment at the end of November each year. Data are available from 2008 through 2024.

INDKP111 (Personal income): Annual average income by province (17 geographic units), five-year age group, sex, and income type. Income data are not available at the municipality level. I use disposable income (type code 100) and primary income (type code 110). Data are available from 1992 through 2024.

A.3 Sample Restrictions

I apply the following restrictions to construct the analysis sample:

1. Exclude aggregate geographic units: “All Denmark” (code 000), five regions (codes 081–085), eleven provinces (codes 01–11), “Abroad” (code 091), and Christiansø (code 411, a small island community).
2. Restrict to seven five-year age groups: 25–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–59.
3. Restrict to quarters available in both AUK01 and FOLK1C: 2008Q1 through 2025Q3 (71 quarters).
4. Drop observations where population is zero or missing (none in practice for these age groups and municipalities).

The resulting panel contains $98 \text{ municipalities} \times 7 \text{ age groups} \times 71 \text{ quarters} = 48,706$ observations.

A.4 Variable Construction

Benefit rates: Each benefit receipt rate is computed as (benefit recipient count / population) $\times 1,000$. This gives the number of recipients per 1,000 working-age population in the relevant age group and municipality.

Treatment group assignment: High intensity = ages 25–29, 30–34, 35–39 (three groups). Moderate intensity = ages 40–44, 45–49 (two groups). Control = ages 50–54, 55–59 (two groups). The binary Young indicator equals 1 for the High group and 0 for the Control group; observations from the Moderate group are dropped in the simple DiD specification.

Municipal baseline disability prevalence (HighBase): For each municipality, I compute the average disability pension rate per 1,000 for ages 25–39 over the pre-reform period (2008Q1–2012Q4). I then set HighBase = 1 for municipalities above the median and HighBase = 0 for those below. The median baseline rate is approximately 30 per 1,000.

Event time: Quarters are indexed relative to 2013Q1 = 0. Thus 2012Q4 = -1 (the reference period in event study specifications), 2012Q3 = -2, etc.

B. Identification Appendix

B.1 Pre-Trends in Disability Pension Rates

The event study for disability pension rates (Figure 3) shows significant pre-reform coefficients, indicating that the simple DiD assumption of parallel trends is violated for this outcome. This is not surprising: disability pension is a stock variable that accumulates with age. The 50–59 group has a much higher stock (approximately 124 per 1,000) than the 25–39 group (approximately 26 per 1,000), and even small proportional differences in inflow and outflow rates can generate divergent level trends. In proportional terms, the pre-reform disability pension decline was similar across groups (approximately 1–2% per year), but the absolute decline was mechanically larger for the higher-stock control group.

This pre-trend does not invalidate the paper’s central finding. The resource scheme outcome has no pre-trends by construction (the program did not exist before 2013), and the triple-difference specification, which absorbs age-specific national trends, produces significant and economically meaningful estimates for the resource scheme and cash benefits.

B.2 Leave-One-Out Analysis

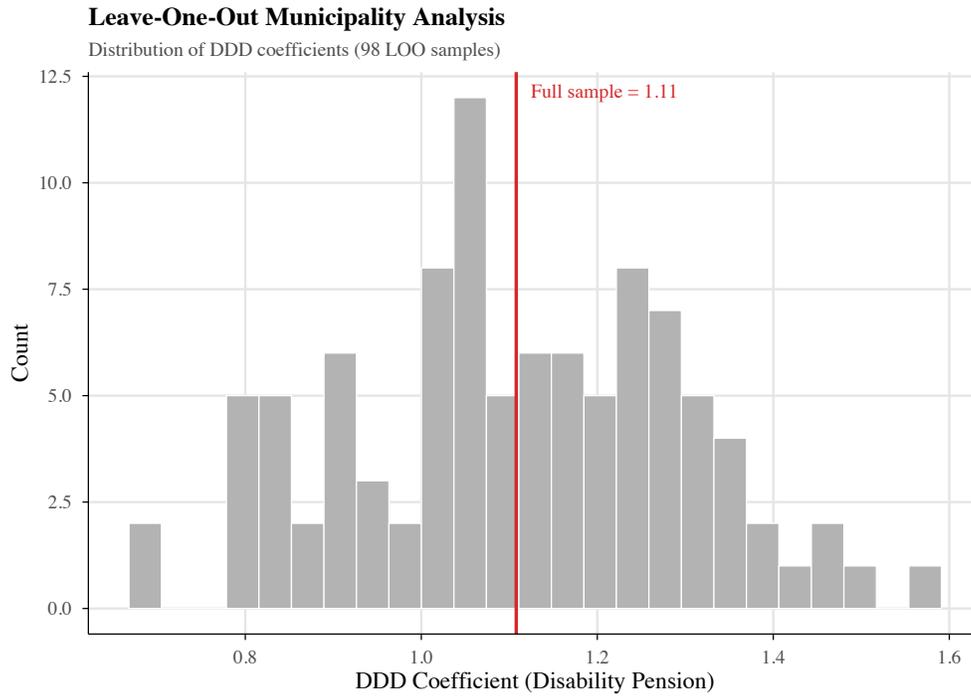


Figure 10: Leave-One-Out Distribution of Triple-Difference Coefficients

Distribution of the DDD coefficient (Young \times Post \times HighBase) for disability pension rate from 98 regressions, each dropping one municipality. The vertical dashed line marks the full-sample estimate. The tight distribution confirms that no single municipality drives the result.

B.3 Randomization Inference

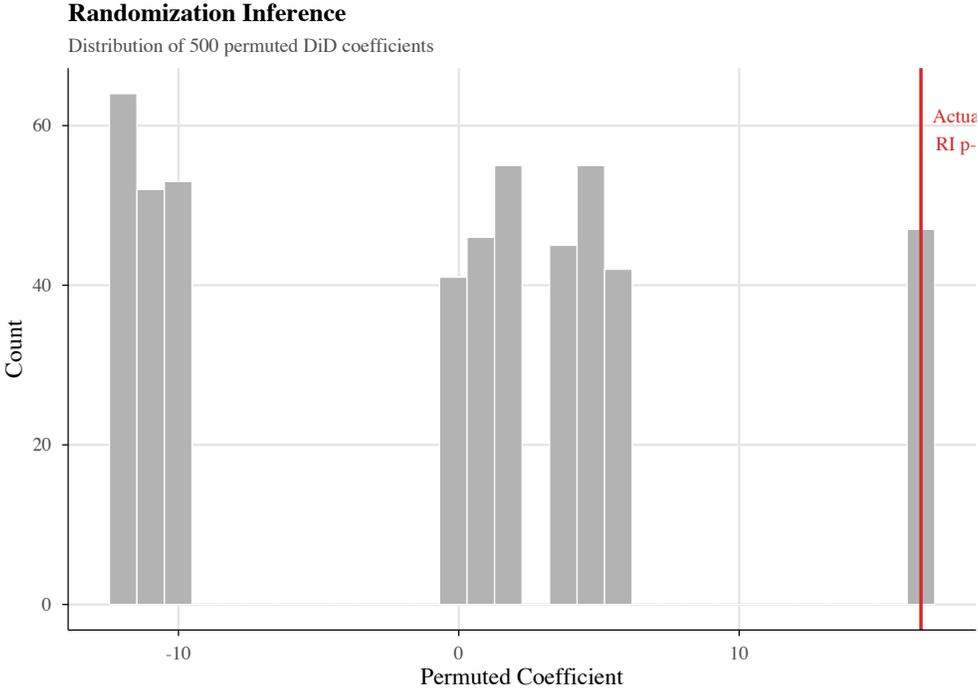


Figure 11: Randomization Inference: Permutation Distribution of DiD Coefficient
Distribution of the DiD coefficient from 500 random permutations of the Young treatment indicator across age groups, plotted against the actual estimate (vertical red line). The two-sided RI p -value is 0.094.

C. Robustness Appendix

C.1 Dose-Response

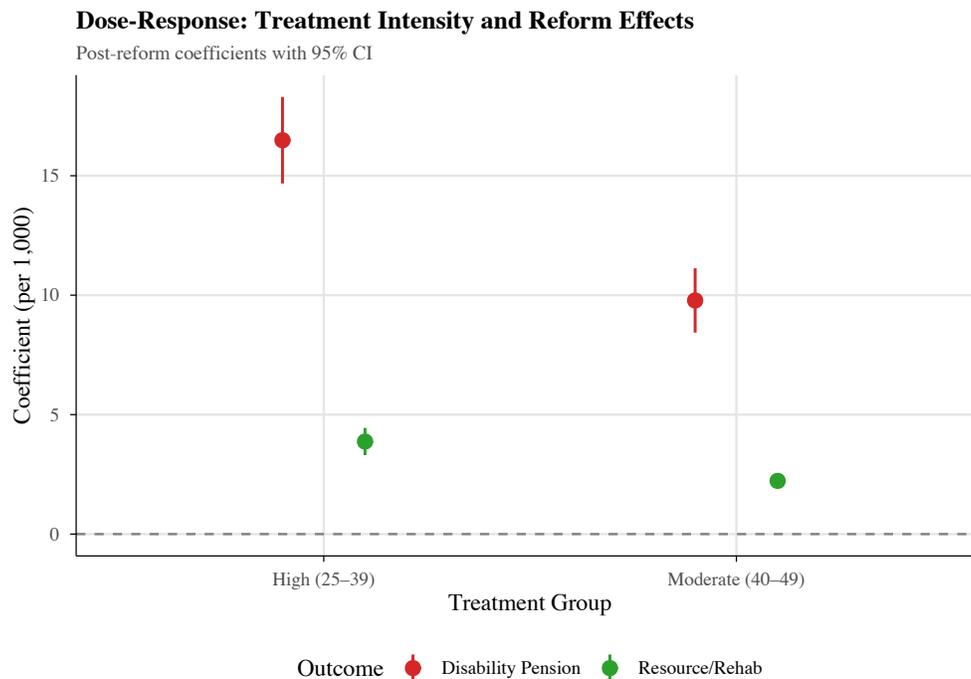


Figure 12: Dose-Response: Treatment Effects by Age Group Intensity

Point estimates and 95% confidence intervals for the dose-response specification (Equation (4)). High = ages 25–39 \times Post; Moderate = ages 40–49 \times Post. Control (ages 50–59) is the omitted category.

D. Heterogeneity Appendix

The sex heterogeneity results (Table 5) show that the reform’s flex job effects are driven entirely by women. This pattern is consistent with women having higher baseline disability pension receipt rates and a higher share of disability applications related to musculoskeletal and mental health conditions, which may be more amenable to partial-capacity subsidized employment than the conditions driving male disability claims.

E. Additional Figures and Tables

No additional exhibits beyond those presented in the main text and preceding appendices.

F. Standardized Effect Sizes

Table 7: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
DP Rate	16.487	0.924	40.533	0.407	0.023	Large positive
Flex Jobs	-6.184	0.639	14.261	-0.434	0.045	Large negative
Resource Scheme	3.873	0.291	4.511	0.859	0.065	Large positive

Notes: Standardized effect sizes (SDE) are computed as $SDE = \hat{\beta}/SD(Y)$, where $\hat{\beta}$ is the DiD coefficient on Young \times Post from the preferred specification (Table 2), SE is the corresponding clustered standard error, and $SD(Y)$ is the unconditional standard deviation of the outcome variable across all municipality-quarter observations. $SE(SDE) = SE/SD(Y)$. Classification thresholds: large negative (< -0.15), moderate negative (-0.15 to -0.05), small negative (-0.05 to -0.005), null (-0.005 to 0.005), small positive (0.005 to 0.05), moderate positive (0.05 to 0.15), large positive (> 0.15). Young = ages 25–39; Control = ages 50–59. Outcomes are quarterly rates per 1,000 population. All SDE values are computed from a binary treatment indicator.