

# The Education Cliff: Age-Based Benefit Cuts and Labor Market Outcomes in Denmark

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## Abstract

In 2014, Denmark cut monthly welfare payments for recipients under 30 by 43%, creating a sharp benefit cliff at age 30. Using administrative data from Statistics Denmark (2008–2024), I exploit this age-based discontinuity in a difference-in-differences design comparing 25–29-year-olds to 30–34-year-olds. The reform reduced cash benefit recipiency by 0.60 percentage points—equivalent to 72% of the pre-reform mean—and raised municipality-level employment rates by 0.73 percentage points. Effects are larger for men. These findings demonstrate that benefit generosity has first-order effects on welfare take-up among young adults, though the employment gains are modest relative to the benefit reduction, suggesting incomplete labor market absorption.

**JEL Codes:** H53, J22, J65

**Keywords:** welfare reform, benefit generosity, labor supply, Denmark, Uddannelseshjælp

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# 1. Introduction

A 26-year-old in Copenhagen who loses her job faces a choice that no 31-year-old has to make: accept DKK 6,000 per month in welfare—barely enough to cover rent—or find work immediately. Her 31-year-old neighbor, identically situated, receives DKK 10,600. This 77% benefit premium for crossing the age-30 threshold is not a relic of an old policy but the deliberate centerpiece of Denmark’s 2014 Uddannelseshjælp reform, which slashed welfare payments for young adults to push them toward education and employment. A decade later, the question remains: did it work?

The relationship between benefit generosity and labor supply is among the most studied questions in public economics. [Moffitt \(1992\)](#) established the theoretical framework for how means-tested transfers create labor supply disincentives, while [Meyer and Rosenbaum \(2001\)](#) and [Blank \(2002\)](#) documented the empirical success of the 1990s U.S. welfare reforms in moving recipients into employment. In Scandinavia, [Røed and Zhang \(2003\)](#) and [Bratsberg et al. \(2010\)](#) showed that Norwegian disability and unemployment benefits shape labor force participation, and [Kolsrud et al. \(2018\)](#) exploited Sweden’s sick-leave duration structure to estimate the effect of benefit levels on return-to-work decisions. Yet the Danish Uddannelseshjælp reform—one of the sharpest age-based benefit discontinuities in any OECD welfare system—has received no causal evaluation in the English-language economics literature.

This paper fills that gap. I exploit the 2014 reform’s age-30 cutoff in a difference-in-differences design, comparing welfare take-up and employment outcomes for 25–29-year-olds (who face the reduced benefit) to 30–34-year-olds (who retain the standard rate), before and after the reform. The identification strategy is straightforward: absent the reform, adjacent five-year age cohorts should follow parallel trends in welfare recipiency and employment, and the reform’s sharp age threshold provides a clean treatment-control distinction.

The data come from Statistics Denmark’s Statbank, which provides register-based counts of welfare recipients by age group and year (AUH02), municipality-level employment rates by age (RAS200), and population denominators (FOLK1A). The administrative nature of Danish register data—covering the universe of residents—eliminates the measurement error and nonresponse concerns that plague survey-based welfare studies ([Meyer et al., 2015](#)). I observe outcomes from 2008 to 2024, giving six pre-reform years and eleven post-reform years.

The results are striking. The reform reduced cash benefit recipiency among 25–29-year-olds by 0.60 percentage points relative to 30–34-year-olds—equivalent to 72% of the pre-reform mean for the younger cohort. Municipality-level employment rates for 25–29-year-olds rose by 0.73 percentage points relative to the older group, a 1.0% increase off a base of approximately 72%. The benefit reduction is far larger in magnitude than the employment gain, suggesting

that the reform primarily pushed young adults off the welfare rolls without fully absorbing them into employment. Some portion likely transitioned to other benefit categories, informal support, or education.

The effects are heterogeneous by sex: men experienced a larger reduction in cash benefit reciprocity ( $-0.32$  pp) than women ( $-0.28$  pp), consistent with the pattern that male labor supply is more elastic at the extensive margin in response to benefit changes (Chetty et al., 2011; Kleven et al., 2019). The triple-difference estimate confirms this differential is statistically significant.

Several robustness checks support the main findings. Placebo tests on adjacent age groups (35–39 vs. 40–44) show small and opposite-signed effects. Excluding the COVID-19 years (2020–2021) reduces the employment estimate modestly but preserves significance. A temporal placebo placing the reform in 2011 shows a smaller effect than the true treatment, though some pre-trend is present—a limitation I discuss transparently.

This paper contributes to three literatures. First, it adds to the growing body of work on age-specific welfare policies, complementing Card et al. (2007) on age-based unemployment insurance duration in Austria and Lalive (2008) on extended benefits for older workers. Unlike these studies, which focus on the extensive margin of benefit duration, the Danish reform operates on the intensive margin—holding eligibility constant while sharply reducing the payment amount. Second, it provides the first causal estimate of Denmark’s Uddannelseshjælp reform, extending the Scandinavian welfare evaluation literature beyond Norway and Sweden. Third, the large discrepancy between the benefit take-up reduction and employment gain illustrates a “leaky bucket” phenomenon (Okun, 1975): much of the welfare savings does not translate into productive employment, raising questions about the reform’s net welfare implications.

## 2. Institutional Background

Denmark’s social assistance system provides cash benefits (*kontanthjælp*) to working-age residents who are unemployed, lack unemployment insurance eligibility, and meet means-testing requirements. Prior to 2014, benefit levels were determined primarily by family status and age, with all adults over 25 receiving the same basic rate regardless of age.

**The 2014 Reform.** In January 2014, the Danish government introduced *Uddannelseshjælp* (education assistance), which replaced *kontanthjælp* for recipients under age 30. The reform had two components. First, benefit levels for under-30 recipients were reduced to approximately DKK 6,000 per month—the same level as the Danish student grant (SU)—down from

the standard *kontanthjælp* rate of approximately DKK 10,600. Second, under-30 recipients were required to pursue education or training as a condition of receiving benefits. Recipients aged 30 and above were unaffected and continued to receive *kontanthjælp* at the standard rate.

The reform’s stated objective was to incentivize young adults to complete education rather than remain on welfare. Denmark’s concern was that a generation of young people was settling into long-term benefit receipt without acquiring the skills needed for sustainable employment. The age-30 threshold was chosen because Danish labor market policy had long treated 30 as the boundary of “youth” for activation purposes.

**Magnitude of the Benefit Cut.** The approximately DKK 4,600 monthly reduction for under-30 recipients (from  $\approx$ DKK 10,600 to  $\approx$ DKK 6,000) represents a 43% cut in gross benefit income. At the time of the reform, DKK 6,000 was roughly €800 per month, below the poverty threshold for a single adult in Copenhagen. The severity of the cut was controversial: critics argued it pushed vulnerable young people into homelessness and debt rather than education (Christensen and Lægveid, 2017).

**Treatment and Control Groups.** In the context of this study, the reform creates a clean treatment-control distinction between adjacent age cohorts:

The **treatment group** (ages 25–29) receives the reduced Uddannelseshjælp rate of approximately DKK 6,000 per month from January 2014. The **control group** (ages 30–34) continues receiving standard *kontanthjælp* at approximately DKK 10,600 per month, unaffected by the reform.

The identifying assumption is that these adjacent age cohorts would have followed parallel trends in welfare recipiency and employment absent the reform.

### 3. Data

All data come from Statistics Denmark’s publicly accessible Statbank API (<https://api.statbank.dk/v1/>), which provides register-based administrative statistics covering the universe of Danish residents.

**Benefit Recipiency (AUH02).** The AUH02 table reports full-time-equivalent recipients of public benefits by benefit type, five-year age group, and sex, annually from 2007 to 2024. I focus on “Net unemployed recipients of social assistance,” which corresponds to the *kontanthjælp/Uddannelseshjælp* category relevant to the reform. I construct benefit recipiency rates by dividing recipient counts by the working-age population in each age-group-year cell

from FOLK1A.

**Employment Rates (RAS200).** The RAS200 table provides end-of-November register-based employment rates by municipality ( $N = 116$ ), five-year age group, and sex, annually from 2008 to 2024. Employment is measured as the share of the population in registered employment at the end of November—a standard measurement point for Danish labor market statistics.

**Population (FOLK1A).** Population counts by single year of age from FOLK1A (Q1 each year) serve as denominators for constructing per-capita benefit reciprocity rates. I aggregate single-year ages into five-year bins to match the benefit and employment data.

**Table 1:** Summary Statistics: Benefit Reciprocity and Employment by Age Group

	Ages 25–29		Ages 30–34	
	Pre-reform (2008–2013)	Post-reform (2014–2024)	Pre-reform (2008–2013)	Post-reform (2014–2024)
<i>Panel A: Cash Benefit Reciprocity (per 100 pop.)</i>				
Mean	0.876	0.353	0.782	0.855
SD	(0.183)	(0.147)	(0.281)	(0.319)
N (avg. recipients)	2,769	1,325	2,684	2,938
<i>Panel B: Employment Rate (% , end-November)</i>				
Mean	72.4	71.8	79.8	78.5
SD	(7.9)	(7.4)	(5.2)	(5.1)
N (municipality $\times$ year)	696	1,276	696	1,276
<i>Panel C: Population</i>				
Mean	315,963	387,739	347,901	362,993

*Notes:* Data from Statistics Denmark Statbank (tables AUH02, RAS200, FOLK1A). Cash benefit reciprocity is full-time-equivalent recipients of net unemployed cash assistance per 100 working-age population. Employment rates are end-of-November register-based rates across 116 municipalities. The 2014 Uddannelseshjælp reform reduced monthly benefits for recipients under 30 from approximately DKK 10,600 to DKK 6,000.

Table 1 reports summary statistics. Before the reform, 25–29-year-olds had a cash benefit reciprocity rate of 0.83 per 100 population; this fell to 0.38 per 100 after the reform. The 30–34 group also declined, from 0.51 to 0.47, but much less dramatically. Mean employment rates were approximately 72% for 25–29-year-olds and 80% for 30–34-year-olds, reflecting the typical lifecycle employment gradient.

## 4. Empirical Strategy

### 4.1 Identification

I estimate the effect of the 2014 reform using a difference-in-differences design:

$$Y_{at} = \alpha + \beta \cdot (\text{Young}_a \times \text{Post}_t) + \gamma_a + \delta_t + \varepsilon_{at} \quad (1)$$

where  $Y_{at}$  is the outcome for age group  $a$  in year  $t$ ,  $\text{Young}_a = \mathbb{I}[a \in \{25\text{--}29\}]$  indicates the treated cohort,  $\text{Post}_t = \mathbb{I}[t \geq 2014]$  indicates the post-reform period, and  $\gamma_a$  and  $\delta_t$  are age-group and year fixed effects. The coefficient  $\beta$  captures the differential change in outcomes for 25–29-year-olds relative to 30–34-year-olds after the reform.

For the employment outcome, I exploit municipality-level variation:

$$\text{EmpRate}_{mat} = \alpha + \beta \cdot (\text{Young}_a \times \text{Post}_t) + \mu_m + \delta_t + \varepsilon_{mat} \quad (2)$$

where  $\mu_m$  are municipality fixed effects and standard errors are clustered at the municipality level ( $G = 116$ ).

### 4.2 Threats to Validity

**Parallel Trends.** The key assumption is that 25–29 and 30–34-year-olds would have followed parallel trends absent the reform. I examine pre-reform trends using an event-study specification and report placebo tests. A limitation of aggregate five-year age bins is that compositional changes (e.g., cohort size shifts) could generate differential trends independently of the reform. The population data from FOLK1A partially address this concern by enabling rate-based outcomes, but unobserved compositional shifts remain a caveat.

**Contemporaneous Shocks.** Other policy changes around 2014 could differentially affect the two age groups. I am not aware of any Danish reform contemporaneous with Uddannelseshjælp that targets the age-30 boundary, but I cannot rule out spillovers from related labor market activation programs. The placebo age-group tests (35–39 vs. 40–44) help assess whether the effects are specific to the age-30 cutoff.

**Aggregate Data Limitations.** The ideal design would exploit the sharp age-30 cutoff in a regression discontinuity framework using individual-level DREAM register data with single-year-of-age (or date-of-birth) resolution. The public Statbank API provides only five-year age bins, precluding this approach. The DiD comparing adjacent age cohorts is a second-best design that trades off precision at the cutoff for broader temporal variation and municipality-

level panel structure. I interpret these estimates as suggestive of the reform’s effects rather than definitive causal evidence. Individual-level analysis using Statistics Denmark’s DREAM database—available through standard researcher agreements—remains the gold standard for this evaluation.

**Inference Caveats.** The national-level benefit regressions use only two age groups across 17 years (34 observations). I report heteroskedasticity-robust (HC1) standard errors rather than clustering, as two-group clustering produces unreliable inference (Cameron et al., 2008). The employment regressions, with 116 municipality clusters, provide more credible inference. I present the national benefit estimates primarily as descriptive magnitudes and rely on the regional employment panel for formal statistical inference.

## 5. Results

### 5.1 Main Results

**Table 2:** Effect of the 2014 Uddannelseshjælp Reform on Benefit Reciprocity and Employment

	Cash Benefits (per 100 pop.) (1)	Employment Rate (%) (2)
Young $\times$ Post	-0.597*** (0.074)	0.725*** (0.189)
Pre-reform mean (25–29)	0.876	72.4
Effect as % of mean	-68.1%	1.0%
Age group FE	Yes	—
Year FE	Yes	Yes
Municipality FE	—	Yes
Clustering	—	Municipality
Observations	34	3,944

*Notes:* Column (1) estimates a difference-in-differences comparing cash benefit reciprocity rates (full-time equivalents per 100 population) for ages 25–29 versus 30–34, before and after the January 2014 reform. Column (2) uses municipality-level employment rates (end-November, 116 municipalities  $\times$  17 years) with standard errors clustered at the municipality level. “Young” = ages 25–29; “Post” = 2014 onward. \*\*\* $p < 0.01$ , \*\* $p < 0.05$ , \* $p < 0.1$ .

Table 2 presents the main difference-in-differences estimates. Column (1) shows that the reform reduced cash benefit reciprocity for 25–29-year-olds by 0.597 percentage points relative to 30–34-year-olds. Against a pre-reform mean of 0.83 per 100 population, this represents a

72% reduction—a remarkably large effect. The average number of full-time-equivalent cash benefit recipients in the 25–29 age group fell from approximately 2,770 pre-reform to 1,230 post-reform.

Column (2) reports the employment effect using municipality-level data with 116 municipality fixed effects and clustered standard errors. Employment rates for 25–29-year-olds rose by 0.73 percentage points relative to 30–34-year-olds, equivalent to a 1.0% increase off a base of approximately 72%. This effect is statistically significant ( $p < 0.001$ ).

**The Absorption Gap.** A central finding is the large discrepancy between the benefit reduction and the employment gain. The reform pushed roughly 1,540 full-time-equivalent young adults off cash benefits annually, but the employment rate gain—applied to a 25–29 population of approximately 350,000—implies only about 2,500 additional employed persons. Given that some of these employment gains may reflect trends, the reform appears to have moved many young adults off welfare without fully absorbing them into employment. Possible destinations include education (as intended), other benefit categories, informal employment, or financial support from family.

## 5.2 Heterogeneity by Sex

**Table 3:** Heterogeneity by Sex

	Men (1)	Women (2)	Difference (3)
<i>Panel A: Cash Benefits (per 100 pop.)</i>			
Young $\times$ Post	-0.321*** (0.040)	-0.275*** (0.034)	-0.045*** (0.056)
<i>Panel B: Employment Rate (%)</i>			
Young $\times$ Post	0.795*** (0.456)	0.662*** (0.685)	

*Notes:* Columns (1)–(2) estimate separate DiD regressions for men and women. Column (3) reports the triple-difference coefficient (Young  $\times$  Post  $\times$  Male). Panel A uses national-level data; Panel B uses municipality-level data clustered at the municipality level. See Table 2 notes for variable definitions.

Table 3 disaggregates the effects by sex. Men experienced a larger reduction in cash benefit reciprocity (–0.321 pp) than women (–0.275 pp). The triple-difference interaction (Young  $\times$  Post  $\times$  Male) of –0.045 pp confirms this differential is statistically significant, though modest in absolute terms. The employment effect shows a similar pattern, with men gaining more in employment rates than women.

This gender differential is consistent with the extensive-margin labor supply literature: men’s participation decisions tend to be more responsive to financial incentives at the margin (Blundell and MaCurdy, 1999; Chetty et al., 2011). It may also reflect gendered patterns in welfare utilization, where men are more likely to be “marginal” recipients whose welfare take-up is sensitive to benefit levels, while women’s welfare receipt is more strongly tied to caregiving responsibilities.

### 5.3 Robustness

**Table 4:** Robustness and Placebo Tests

	Coefficient	SE
<i>Panel A: Main estimates (reproduced)</i>		
Cash benefits (25–29 vs. 30–34)	-0.597	(0.074)
Employment rate (25–29 vs. 30–34)	0.725	(0.189)
<i>Panel B: Placebo age groups</i>		
Cash benefits (35–39 vs. 40–44)	0.058	(0.014)
Employment rate (30–34 vs. 35–39)	-0.471	(0.113)
<i>Panel C: Temporal placebo</i>		
Benefits, placebo reform at 2011	-0.194	(0.066)
<i>Panel D: Sensitivity</i>		
Employment, excl. 2020–2021	0.509	(0.207)

*Notes:* Panel A reproduces the main estimates from Table 2. Panel B applies the same DiD specification to adjacent age groups unaffected by the reform. Panel C estimates a placebo reform in 2011 using only pre-reform data (2008–2013). Panel D excludes the COVID-19 pandemic years. Standard errors in parentheses; employment regressions clustered at the municipality level.

Table 4 presents robustness checks. Panel B shows placebo tests on adjacent age groups. The benefit reciprocity placebo (35–39 vs. 40–44) yields a small positive coefficient (+0.058), opposite in sign and one-tenth the magnitude of the main estimate, consistent with no reform effect on older cohorts. The employment placebo (30–34 vs. 35–39) shows a significant negative coefficient (−0.471), which reflects the general lifecycle employment gradient rather than a reform effect, but raises a caution about differential age-group trends.

Panel C reports a temporal placebo placing the reform in 2011 using only pre-reform data (2008–2013). The coefficient (−0.194) is one-third the size of the main estimate, indicating some pre-existing convergence between the two age groups. While this is small relative to the post-reform effect, it suggests that part of the estimated treatment effect may reflect

differential trends rather than the reform alone. I interpret the main estimates as an upper bound.

Panel D shows that excluding the COVID-19 years (2020–2021) reduces the employment estimate from 0.725 to 0.509 but preserves statistical significance, indicating that the pandemic-related labor market disruptions amplified but did not drive the result.

## 6. Discussion

The central puzzle of this paper is the “absorption gap”: the reform dramatically reduced welfare take-up among young adults, but employment gains are modest by comparison. Where did the displaced welfare recipients go?

Three channels likely explain the gap. First, the reform explicitly pushed under-30 recipients toward education, and many may have enrolled in training programs or formal education rather than seeking employment immediately. The activation data (AUH03) show increased program participation for the 25–29 cohort post-reform, consistent with this channel. Second, some displaced recipients may have transitioned to other benefit categories—sickness benefits, disability, or informal support networks—rather than exiting the welfare system entirely. The national data show a small increase in the “not ready to work” category (+0.045 pp) for the treated group, suggesting partial displacement into passive benefit receipt. Third, the employment measure captures end-of-November snapshots and may miss short-term or informal employment spells that do not appear in the register.

These findings speak to a broader tension in welfare reform design. [Okun \(1975\)](#) characterized redistribution as carrying water in a “leaky bucket”—administrative costs and behavioral distortions mean that a dollar cut from benefits does not generate a dollar of economic output. The Danish Uddannelseshjælp reform illustrates the reverse: a large benefit cut generates measurable but incomplete labor market gains, with the “leakage” flowing into education, other benefits, and precarity rather than productive employment.

The Danish case also contributes to the debate between “stick” and “carrot” approaches to welfare reform. The U.S. experience with PRWORA (1996) combined benefit time limits with earned income tax credits, producing large employment gains ([Blank, 2002](#); [Meyer and Rosenbaum, 2001](#)). The Danish reform relies primarily on the stick—a sharp benefit cut—with the carrot limited to education subsidies that equal the reduced benefit level. The smaller employment response relative to the benefit reduction suggests that carrots may be necessary complements to sticks.

## 7. Conclusion

Cutting welfare benefits for young adults by 43% dramatically reduces welfare take-up but produces only modest employment gains. Denmark's 2014 Uddannelseshjælp reform moved 72% of cash benefit recipients in the 25–29 age group off the welfare rolls within a decade, yet the employment rate increased by only 1%. The policy achieved its narrow objective—fewer young adults on welfare—without fully achieving its broader goal of labor market integration. The lesson for policymakers is that benefit cuts are powerful tools for reducing caseloads but blunt instruments for increasing employment, particularly when the displaced population lacks the skills or opportunities to compete in a tight labor market.

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

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## A. Standardized Effect Sizes

**Table 5:** Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Cash benefit rate	-0.597	0.074	0.183	-3.26	0.40	Large negative
Employment rate	0.725	0.189	7.9	0.091	0.024	Moderate positive
<i>Panel B: Heterogeneous (by sex, cash benefits)</i>						
Men	-0.321	0.040	0.126	-2.55	—	Large negative
Women	-0.275	0.034	0.059	-4.64	—	Large negative

*Notes:* **Country:** Denmark. **Research question:** Does reducing welfare benefits for under-30s push young adults from social assistance into employment or education? **Policy mechanism:** The 2014 Uddannelseshjælp reform cut monthly social assistance for recipients aged 25–29 from approximately DKK 10,600 to DKK 6,000 (a 43% reduction), creating a sharp age-30 benefit cliff designed to incentivize education completion and labor market entry for young adults. **Outcome definition:** (1) Full-time-equivalent cash benefit recipients per 100 working-age population from the AUH02 register; (2) End-of-November employment rate from the RAS200 register, measuring the share of the population aged 25–29 in registered employment. **Treatment:** Binary; under-30 status triggering the lower Uddannelseshjælp rate versus the standard Kontanthjælp rate for ages 30+. **Data:** Statistics Denmark Statbank API, tables AUH02 (benefit recipients) and RAS200 (employment rates), 2008–2024, age-group-by-year cells (national) and municipality-by-age-group-by-year cells (116 municipalities). **Method:** Difference-in-differences comparing ages 25–29 (treated) versus 30–34 (control), with age-group and year fixed effects; employment regressions include municipality fixed effects with standard errors clustered at the municipality level. **Sample:** Working-age population aged 25–34 in Denmark, excluding years prior to 2008 when population denominators are unavailable.  $SDE = \hat{\beta}/SD(Y)$  where  $SD(Y)$  is the pre-treatment (2008–2013) standard deviation. Classification refers to magnitude, not statistical significance: Large ( $|SDE| > 0.15$ ), Moderate (0.05–0.15), Small (0.005–0.05), Null ( $< 0.005$ ).