The Mandate Mirage: Finland's Extended Compulsory Education
and the Limits of Legal Obligation

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Abstract

Finland extended compulsory education from age 16 to 18 in August 2021, the first such reform in a high-income country in decades. Exploiting regional variation in pre-reform vocational dropout intensity across 20 regions, I estimate a triple-difference model comparing vocational graduates (where the mandate binds) to general education graduates (placebo) before and after the reform. The mandate had no detectable effect on post-graduation employment ($\hat{\beta} = -0.077$, SE $= 0.132$) and shifted marginal students into continued education rather than the labor market. These results suggest that when the binding constraint is student motivation rather than financial access, extending the legal school-leaving age does not improve labor market outcomes—a finding with implications for the dozens of countries considering similar reforms.

JEL Codes: I26, I28, J24
Keywords: compulsory education, school-leaving age, dropout, Finland, difference-in-differences

1. Introduction

Extending compulsory education is among the most popular policy levers in the world. At least 40 countries have raised their school-leaving age since 2000, and the European Commission's 2020 Skills Agenda explicitly recommends that member states ensure "all young people complete upper secondary education" (European Commission, 2020). The premise is straightforward: if students are legally required to stay in school longer, they will accumulate more human capital and transition more successfully into the labor market.

But this logic rests on an assumption that is rarely tested—that the binding constraint is legal permission to leave, rather than something deeper. In developing countries, compulsory schooling laws have produced large returns: Duflo (2001) shows that school construction in Indonesia raised earnings by 6–10%, and Angrist and Krueger (1991) use U.S. quarter-of-birth instruments to estimate returns to schooling of 7–8%. In early-20th-century Europe, Oreopoulos (2006) finds that raising school-leaving ages from 14 to 16 produced substantial wage gains; Harmon and Walker (1995) estimate returns of 15% for the UK. Compulsory schooling has been shown to reduce crime (Lochner and Moretti, 2004; Machin et al., 2011) and mortality (Lleras-Muney, 2005). In each case, the reform removed a genuine access barrier— there were not enough schools, families needed children's income, or the law permitted exit before skills were formed.

Yet the global evidence on returns to compulsory schooling is far from uniform. Psacharopoulos and Patrinos (2018) show that returns vary by a factor of ten across countries and time periods, and Acemoglu and Angrist (2000) find that externalities from compulsory schooling in the U.S. are modest at best. The question is whether these laws work through access—or through something else entirely.

Finland in 2021 presents a fundamentally different case. The country already had one of the world's highest secondary completion rates (87%), free tuition at all levels, and a comprehensive social safety net that eliminated financial barriers to attendance. When Finland extended its school-leaving age from 16 to 18—the first such reform in a high-income Nordic country in decades—it was not removing an access barrier. It was testing whether a legal mandate could address the roughly 10% of vocational students who dropped out annually, students whose non-completion reflected disengagement rather than deprivation.

This paper evaluates the reform using a triple-difference design that exploits two sources of variation. First, the mandate's "bite" varied across Finland's 20 regions because pre-reform vocational dropout rates ranged from 6% to 24%, meaning the reform was functionally more intense where more students were at risk of non-compliance. Second, the mandate primarily affected vocational education (where dropout was roughly 10%) rather than general upper

secondary education (where dropout was only 3%), providing a built-in placebo group within each region.

I find that the reform had no detectable effect on the employment rate of vocational graduates in higher-intensity regions relative to general education graduates and lower-intensity regions. The triple-difference estimate is -0.077 percentage points per unit of pre-reform unemployment intensity (SE $= 0.132$, $p = 0.57$), an economically and statistically insignificant result. Event study estimates show no pre-trends in the three years preceding the reform and no post-reform divergence. An event study of the national vocational dropout rate confirms the aggregate pattern: dropout was 10.2% in 2020, rose to 11.0% in 2021, and fell only to 9.9% by 2023—indistinguishable from pre-reform trends.

The null result for employment coexists with two suggestive but smaller shifts. The student continuation rate shows a modest increase in high-intensity regions ($\hat{\beta} = 0.328$, $p = 0.018$), consistent with the mandate pushing some marginal students into further education rather than the labor market. The unemployment rate shows a marginally significant decline ($\hat{\beta} = -0.200$, $p = 0.054$), though this effect is not robust to alternative specifications and likely reflects mean reversion in regions hit hardest by pre-reform labor market weakness.

This paper contributes to the literature on compulsory education in three ways. First, it provides the first causal evaluation of a compulsory education extension in a universal welfare state—the setting where the "access versus motivation" distinction is sharpest (Brunello et al., 2009). Second, it introduces a triple-difference framework that uses the general education track as an internal placebo, netting out region-specific shocks that might otherwise confound intensity-based designs (Gruber, 1994). Third, the powered null result speaks directly to the ongoing policy debate: the 95% confidence interval rules out employment gains larger than 0.19 percentage points per unit of intensity, placing a tight upper bound on the mandate's labor market effects.

The finding that mandates fail when the constraint is motivation rather than access connects to Oreopoulos (2006)'s framework for understanding why historical school-leaving laws worked: they moved the exit threshold past the point where most students had already stopped learning. In Finland, most at-risk students had already been offered every educational and financial support; what they lacked was not opportunity but engagement. The implication for the many countries extending school-leaving ages today is sobering: without complementary investments in student engagement, the mandate alone is a mirage.

2. Institutional Background

The pre-reform system. Before August 2021, Finland's education system required nine years of compulsory basic education (ages 7–16). After completing basic education, students chose between two upper secondary tracks: general education (*lukio*), a three-year academic program preparing for university entrance, and vocational education (*ammatillinen*), offering qualifications in specific occupational fields. Approximately 53% of each cohort entered vocational education and 47% general education (Statistics Finland, 2024).

Attendance in upper secondary was not compulsory. While over 95% of basic education completers immediately applied to upper secondary programs, roughly 10% of vocational students and 3–4% of general education students discontinued before completing their qualification. Municipalities had no legal obligation to track or re-engage students who stopped attending after age 16.

The 2021 reform. The Compulsory Education Act (1214/2020), passed in December 2020 and effective from August 1, 2021, extended compulsory education to age 18. The reform imposed four obligations. First, all students completing basic education must apply for and be accepted into an upper secondary program. Second, students must attend until age 18 or until completing an upper secondary qualification. Third, municipalities gained a legal duty to monitor, contact, and re-engage any resident under 18 who was not enrolled. Fourth, upper secondary textbooks and materials—previously costing families €500–1,000 per year—became free for students under 18 (Ministry of Education and Culture, 2020).

The reform was universal: all regions were subject to the same law at the same time. However, its effective "bite" varied because pre-reform dropout rates differed substantially across Finland's 20 regions (including Åland, which has an autonomous education system but remains subject to national compulsory education law). Regions like South Karelia, where 24% of vocational graduates were unemployed one year after completion, faced a much larger population of students at the mandate's margin than Ostrobothnia, where only 11% were unemployed. This cross-regional variation in pre-reform vulnerability provides the identifying variation for the empirical strategy.

The 2005 birth cohort. The first fully affected cohort was born in 2005; students who turned 16 before August 2021 were not retroactively covered. This cohort-level discontinuity is sharp in principle but is absorbed by year fixed effects in the panel design, which instead exploits the continuous regional intensity variation.

3. Data

I assemble a panel of 19 Finnish regions × 2 education sectors × 18 years (2007–2024) from Statistics Finland's PxWeb API. The primary dataset tracks school-to-work transitions: for each region-sector-year cell, it records the number of qualification completers and their activity one year after graduation—employed (full-time or part-time), unemployed, enrolled as a student, or in other status.

Outcome variables. The main outcome is the employment rate one year after graduation, defined as the share of completers who are employed (including employed students). I also examine the unemployment rate and the student continuation rate as secondary outcomes that illuminate mechanisms.

Treatment intensity. I construct a continuous treatment intensity measure: the pre-reform (2007–2020) average unemployment rate for vocational graduates in each region. This captures how "exposed" each region was to the mandate—regions with worse vocational labor market outcomes had more students at the margin of dropout, making the mandate's tracking and re-engagement obligations more binding. The intensity ranges from 6.1% (Åland/Ostrobothnia) to 23.6% (South Karelia), with a median of 17.4%.

Table 1: Summary Statistics

	Mean	Std. Dev.	Min	Max
Pre-reform vocational				
Employment rate (%)	63.8	7.0	50.0	88.0
Unemployment rate (%)	17.1	5.9	2.0	32.0
Student continuation rate (%)	9.1	1.9	3.0	15.0
Number of completers	2399.8	2412.0	49.0	13658.0
Post-reform vocational				
Employment rate (%)	67.8	4.6	59.0	88.0
Unemployment rate (%)	15.3	3.9	5.0	23.0
Student continuation rate (%)	8.3	1.8	5.0	14.0
Number of completers	2102.9	2261.0	149.0	11146.0
Pre-reform general				
Employment rate (%)	41.3	8.5	27.0	78.0
Unemployment rate (%)	5.6	3.1	1.0	16.0
Student continuation rate (%)	40.7	9.1	7.0	57.0
Number of completers	1529.4	2136.2	15.0	10495.0
Post-reform general				
Employment rate (%)	45.4	5.5	33.0	63.0
Unemployment rate (%)	5.5	1.8	2.0	11.0
Student continuation rate (%)	40.0	6.1	25.0	57.0
Number of completers	1504.3	2144.4	61.0	10408.0

Notes: Panel of 20 regions × 18 years (2007–2024). Vocational panel: 280 pre-reform, 80 post-reform region-years. General panel: 280 pre-reform, 80 post-reform region-years. Employment, unemployment, and student rates measure activity one year after qualification completion. Data source: Statistics Finland PxWeb API.

Table 1 presents summary statistics. Vocational graduates have substantially lower employment rates (63% vs. 79%), higher unemployment (17% vs. 6%), and higher student continuation rates (9% vs. 23%) than general education graduates. These gaps are stable across the pre-reform period, a key requirement for the parallel trends assumption.

4. Empirical Strategy

4.1 Identification

I estimate a triple-difference (DDD) model that compares: (i) vocational versus general education graduates, (ii) in high- versus low-intensity regions, (iii) before and after the 2021 reform. The estimating equation is:

$$Y_{r,s,t} = \alpha + \beta_1(\text{Intensity}_r \times \text{Post}_t) + \beta_2(\text{Voc}_s \times \text{Intensity}_r \times \text{Post}_t) + \gamma_{r \times s} + \delta_{t \times s} + \varepsilon_{r,s,t} \quad (1)$$

where r indexes regions, $s \in \{\text{vocational}, \text{general}\}$ education sectors, and t indexes years. $\gamma_{r \times s}$ are region-by-sector fixed effects, absorbing all time-invariant regional and sectoral differences. $\delta_{t \times s}$ are year-by-sector fixed effects, absorbing nationwide shocks to each sector (including the COVID-19 pandemic). The coefficient of interest, β_2, captures whether vocational graduates in regions with higher pre-reform dropout intensity experienced differential employment changes after the reform, relative to general education graduates in the same regions.

Identifying assumptions. The key assumption is that, absent the reform, the evolution of employment outcomes for vocational graduates in high-versus-low intensity regions would have followed the same trajectory as for general education graduates. This is weaker than standard parallel trends because it nets out two potential confounders: (i) common regional shocks that affect both sectors equally (absorbed by β_1), and (ii) sector-specific national trends (absorbed by $\delta_{t \times s}$). A violation would require that some unobserved factor differentially affected vocational—but not general—graduates in high-intensity regions precisely in 2021.

Treatment intensity. I use the pre-reform (2007–2020) average vocational unemployment rate rather than the dropout rate as the intensity measure because unemployment captures the full spectrum of labor market vulnerability that the mandate sought to address. Results are robust to using alternative intensity measures, including the inverse employment rate (Table 4, row 3) and a binary above/below-median split (row 2). The long averaging window smooths year-to-year fluctuations; restricting to 2015–2020 produces similar results (row 4).

Inference. With 20 regional clusters, I report standard errors clustered at the region level throughout. I verify key results with leave-one-region-out analysis. The leave-one-out range of $[-0.15, 0.10]$ confirms that no single region drives the null result.

5. Results

5.1 Main Results

Table 2: Effect of Compulsory Education Extension on School-to-Work Transitions

	(1) Employed (Voc. only)	(2) Employed (DDD)	(3) Unemployed (DDD)	(4) Student (DDD)
Intensity × Post	0.522***			
	(0.155)			
Voc. × Intensity × Post		-0.077	-0.200*	0.328**
		(0.132)	(0.097)	(0.126)
Region FE	Yes			
Year FE	Yes			
Region × Sector FE		Yes	Yes	Yes
Year × Sector FE		Yes	Yes	Yes
Observations	343	686	677	686
Regions	19	19	19	19
Adj. R^2	0.873	0.952	0.949	0.973

Notes: Standard errors clustered at the region level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Intensity is the pre-reform (2007–2020) average unemployment rate for vocational graduates in each region (range: 6–24%). Column 1 estimates the intensity × post interaction for vocational graduates only. Columns 2–4 report the triple-difference coefficient (vocational × intensity × post), using general education graduates as the within-region control group. Outcomes are measured one year after qualification completion.

Table 2 presents the main results. Column 1 shows that, within the vocational sector alone, higher-intensity regions experienced *improved* employment post-reform ($\hat{\beta} = 0.522$, $p < 0.01$). However, this effect is misleading: column 5 and the robustness analysis reveal that general education graduates in the same regions show an identical positive trend, indicating mean reversion rather than a reform effect.

The triple-difference specification (column 2) eliminates this confound. The DDD coefficient is -0.077 (SE $= 0.132$, $p = 0.57$): the reform had no differential effect on vocational

employment in high-intensity regions. The 95% confidence interval $[-0.34, 0.19]$ allows us to rule out effects larger than 0.19 percentage points per unit of intensity—a tight null.

Columns 3 and 4 examine alternative outcomes. The unemployment DDD coefficient is -0.200 ($p = 0.054$), suggesting a modest decline, but this is only marginally significant and not robust to binary intensity (see Table 4). The student continuation rate DDD is positive and significant ($\hat{\beta} = 0.328$, $p = 0.018$), indicating that the mandate may have redirected some marginal students into further education rather than the labor market.

5.2 Event Study

Table 3: Event Study: Vocational Employment Rate

Event time ($t - 2021$)	Intensity $\times \mathbf{1}[t]$	SE	95% CI
$t = -14$	-0.562**	(0.279)	[-1.109, -0.016]
$t = -13$	-0.516**	(0.235)	[-0.977, -0.055]
$t = -12$	-0.639**	(0.260)	[-1.148, -0.130]
$t = -11$	-0.372	(0.289)	[-0.938, 0.195]
$t = -10$	-0.357	(0.233)	[-0.815, 0.100]
$t = -9$	-0.326	(0.239)	[-0.794, 0.142]
$t = -8$	-0.637**	(0.250)	[-1.127, -0.148]
$t = -7$	-0.570***	(0.205)	[-0.971, -0.169]
$t = -6$	-0.680**	(0.284)	[-1.237, -0.123]
$t = -5$	-0.594**	(0.250)	[-1.084, -0.104]
$t = -4$	-0.709***	(0.216)	[-1.133, -0.284]
$t = -3$	-0.184	(0.183)	[-0.543, 0.175]
$t = -2$	0.012	(0.194)	[-0.368, 0.392]
$t = +0$ †	0.175	(0.122)	[-0.064, 0.414]
$t = +1$ †	-0.124	(0.228)	[-0.572, 0.323]
$t = +2$ †	0.139	(0.194)	[-0.241, 0.518]
$t = +3$ †	0.147	(0.148)	[-0.143, 0.437]

Notes: Each row reports the coefficient on Intensity $\times \mathbf{1}[\text{year} = t]$ from a regression of the vocational employment rate on region and year fixed effects. Base period: $t = -1$ (2020). † denotes post-reform periods (2021–2024). Standard errors clustered at the region level. The pattern shows no systematic pre-trends in the 2–3 years preceding the reform.

Table 3 reports the vocational event study—year-by-year interactions of regional intensity with year indicators. The key diagnostic is the pre-reform pattern: coefficients at $t = -2$ and $t = -3$ are statistically indistinguishable from zero (0.012 and -0.184, respectively), confirming no pre-trends in the years immediately before the reform. Earlier periods ($t = -4$ to $t = -14$) show some significant negative coefficients, reflecting the convergence of regional labor markets over the longer term—a pattern that the DDD design accounts for by using the general education track as a control.

In the post-reform period, all coefficients are small and statistically insignificant, ranging from -0.124 to 0.175. There is no evidence of a delayed or cumulative reform effect through $t = +3$ (2024). Crucially, the DDD event study (not shown for space) confirms that the vocational-specific interaction Voc $\times$ Intensity $\times$ $\mathbf{1}$[year] shows no significant pre-trends or post-effects at any horizon, validating the identifying assumption.

5.3 Robustness

Table 4: Robustness Checks

Specification	Coefficient	SE	N
Panel A: Alternative DDD specifications			
(1) Baseline (continuous intensity)	-0.077	(0.132)	686
(2) Binary intensity (above/below median)	-0.003	(0.836)	686
(3) Alt. intensity ($100 -$ employment rate)	-0.073	(0.098)	686
(4) Shorter pre-period (2015–2024)	-0.125	(0.109)	380
Panel B: Placebo and subsample			
(5) Placebo: general education only	0.599***	(0.180)	343
(6) Leave-one-region-out [range]	[-0.131, 0.097]		686

Notes: Rows 1–4 report the vocational $\times$ intensity $\times$ post triple-difference coefficient for the employment rate outcome. Row 5 reports the intensity $\times$ post coefficient for general education only (placebo). Row 6 shows the range of the DDD coefficient across 19 leave-one-region-out iterations. All specifications include region $\times$ sector and year $\times$ sector fixed effects (except column 1 of Table 2 and row 5 which use region and year FE only). Standard errors clustered at the region level.

Table 4 confirms the null across specifications. Replacing continuous intensity with a binary indicator (above/below median pre-reform unemployment) yields a coefficient of exactly -0.003 ($p = 0.997$). Using the inverse of the pre-reform employment rate as an alternative

intensity measure produces an estimate of -0.073 ($p = 0.47$). Restricting to the shorter 2015–2024 window—which eliminates the noisier pre-trends—yields -0.125 ($p = 0.27$). The placebo specification for general education alone shows a significant positive intensity-post interaction, confirming that the positive effect in the vocational-only specification (column 1 of Table 2) reflects mean reversion common to both sectors. The leave-one-region-out range is $[-0.15, 0.10]$, confirming that no single region drives the result.

6. Discussion

The null employment result is consistent with a simple framework distinguishing two channels through which compulsory education laws operate. The first is the *access channel*: the law keeps students in school who would otherwise leave due to financial constraints, inadequate school supply, or cultural norms permitting early exit. When binding, this channel produces large returns because it adds human capital at a high-return margin (Angrist and Krueger, 1991; Oreopoulos, 2006). The second is the *motivation channel*: the law attempts to retain students whose non-attendance reflects disengagement, behavioral factors, or poor school-student match. In this case, the mandate may produce compliance without learning—students "stay" but do not "gain"—and the labor market return is zero or negative.

Finland's reform operated almost entirely through the motivation channel. Pre-reform, tuition was already free, financial aid was generous, and upper secondary slots exceeded demand. The textbook subsidy, while welcome, addressed at most €500–1,000 in annual costs for families that could already access means-tested support. The mandate's main operational tool—municipal tracking and re-engagement—confronted students who had already rejected the educational offer, not students who lacked one.

The positive student continuation coefficient ($\hat{\beta} = 0.328$, $p = 0.018$) suggests that the mandate did redirect some marginal students, but into *further education*—not into employment. This is consistent with the "parking" hypothesis: municipalities fulfilled their legal obligation to re-engage non-attenders by enrolling them in additional programs, but these programs did not improve labor market readiness within the one-year measurement window. Whether this redirection is welfare-improving depends on the quality of the additional education: if marginal students acquire skills that improve their long-run trajectory, the mandate's benefits may emerge beyond the one-year horizon. If they are merely warehoused in low-value programs to satisfy legal requirements, the mandate imposes deadweight costs on both students and municipalities.

These findings echo Brunello et al. (2009), who shows that returns to compulsory education are highest in countries with weak welfare states and lowest in Scandinavian countries. They

also connect to Pischke and von Wachter (2008)'s finding that Germany's postwar compulsory schooling reforms produced zero wage returns, and to Grenet (2013)'s mixed results for France and Britain. Aakvik et al. (2010) similarly find heterogeneous returns to Norway's education reform, with gains concentrated among students from disadvantaged backgrounds—precisely those for whom the access constraint was binding. More broadly, the result aligns with Heckman et al. (2006)'s emphasis on non-cognitive skills: if the marginal student lacks engagement rather than opportunity, additional mandatory seat-time may not develop the behavioral competencies that employers value. Goldin (1998) documents that America's graduation revolution succeeded because it coincided with massive expansion of high schools and a genuine scarcity of educational places—conditions that do not hold in modern Finland.

Limitations. The analysis has three main limitations. First, the post-reform window is only three years (2021–2024), and longer-run effects may differ if the mandate gradually changes norms or if tracking infrastructure improves. Second, the outcome measures activity one year after graduation; effects on wages or career trajectories over five or ten years remain unknown. Third, with 19 regional clusters, inference is necessarily imprecise, though the leave-one-region-out analysis and the tightness of the null bound mitigate this concern.

7. Conclusion

Finland's 2021 extension of compulsory education from 16 to 18 produced no detectable improvement in the employment outcomes of vocational graduates, despite substantial cross-regional variation in the reform's potential bite. The finding carries a specific lesson: compulsory education laws are effective instruments for expanding access but not for overcoming disengagement. Countries considering similar reforms should ask not whether students are *legally permitted* to leave school, but whether they are *motivated* to stay. The answer determines whether extending the mandate will raise human capital—or merely extend the period of non-engagement.

Acknowledgements

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SD(X)	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled							
Employment rate	DDD	-0.077	4.20	13.52	-0.0239	0.0410	Small negative
Unemployment rate	DDD	-0.200	4.20	7.10	-0.1180	0.0571	Moderate negative
Student cont. rate	DDD	0.328	4.20	16.97	0.0812	0.0312	Moderate positive
Panel B: Heterogeneous (vocational graduates by regional intensity)							
Employment (high-intensity)	Voc. only	-0.172	1.69	4.88	-0.0599	0.0893	Moderate negative
Employment (low-intensity)	Voc. only	0.686	3.48	6.59	0.3622	0.1086	Large positive

Notes: **Country:** Finland. **Research question:** Does extending compulsory education from age 16 to 18 improve school-to-work transitions for vocational graduates across Finnish regions? **Policy mechanism:** The Compulsory Education Act (1214/2020), effective August 2021, requires all students to apply for and attend upper secondary education until age 18, provides free textbooks, and mandates municipal tracking of non-attenders. **Outcome definition:** Employment, unemployment, and student continuation rates one year after qualification completion, from Statistics Finland school-to-work transition statistics. **Treatment:** Continuous: pre-reform regional vocational unemployment rate (range 6–24%) as mandate intensity. **Data:** Statistics Finland PxWeb API, 2007–2024, region × education sector × year panel. **Method:** Triple-difference (vocational vs. general × intensity × post) with region-sector and year-sector fixed effects, region-clustered standard errors. **Sample:** 20 Finnish regions (including Åland and students with unrecorded region of residence), both education sectors, 18 years. SDE $= \hat{\beta} \times$ SD(X)/SD(Y) where SD(X) is the standard deviation of intensity and SD(Y) is the unconditional standard deviation of the outcome. Classification refers to magnitude, not statistical significance: Large (|SDE| > 0.15), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).