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School resources, peer inputs, and student outcomes in adult education

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School resources, peer inputs, and student outcomes in adult education[a]

by

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Abstract

This paper studies a large-scale educational expansion to evaluate whether shocks to school inputs have an impact on the academic achievement of adult education students. I analyze the spillover effects of a Swedish policy that temporarily doubled enrollment in adult education, thus putting considerable strain on school inputs. Since the policy targeted individuals age 25 and over, my analysis focuses on individuals under age 25 to mitigate concerns that changes in student composition drive my findings. First, I establish that students in regions subject to larger enrollment shocks experienced stronger negative shocks to peer quality and school resources such as teacher credentials and per-pupil expenditure. Then, I show that the stronger negative shocks to peer quality and school resources coincided with larger increases in course dropout. Taken together, the two sets of results suggest a causal link between school inputs and course dropout.

Keywords: adult education; educational expansion; per-pupil spending; school resources; student achievement; teacher credentials

JEL codes: I20; I21; I28

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1 Introduction

Policymakers consider increased access to education a crucial tool for improving people's skills, productivity, and well-being. However, an important question is whether access to education can expand without lowering the quality of schooling. The answer depends in part on the relationship between school inputs and student achievement. Large increases in student enrollment put a strain on inputs such as class size, per-pupil spending, peer quality, and teacher quality. For example, to meet the increased demand for teachers, schools may be forced to hire candidates with poor qualifications, thereby lowering the average quality of the teaching staff. If negative shocks to the quality or quantity of inputs matter for student outcomes, policies that increase access to education may be limited in their effectiveness and could even have adverse consequences, particularly for students who would have enrolled in the absence of the intervention.

In this paper, I evaluate how expansion-induced shocks to school inputs affect student performance by investigating the spillover effects of a Swedish policy that temporarily doubled enrollment in adult education. The policy, known as the Adult Education Initiative (AEI), was part of a strategy to reduce high unemployment after a severe economic crisis in the early nineties. Between 1997 and 2002, the government created an additional 100,000 spots in adult education and used generous study allowances to encourage low-educated, unemployed individuals to enroll. A key feature of these study allowances is that they were available only to individuals aged 25 to 55. I exploit this institutional detail by restricting my main analysis to individuals under age 25, thus mitigating concerns that changes in student composition drive my findings. However, I also provide evidence that my results seem to generalize to the older, targeted population of students as well.

With rich administrative data covering all students in adult education and their teachers, I perform two complementary analyses. First, I study how the AEI affected students' exposure to a broad range of inputs that have been shown to influence academic achievement in other settings, including class size (Krueger and Whitmore, 2001; Fredriksson et al., 2013), peer quality (Carrell et al., 2009), teacher experience (Papay and Kraft, 2015), teacher certification (Andersson et al., 2011), and per-pupil expenditure (Jackson et al., 2020). Next, I evaluate whether the changes in peer and school inputs coincided with changes in students' likelihood to complete their courses or achieve good grades. Taken together, the two sets of results provide reduced-form evidence on the relationship between school inputs and student outcomes in adult education.

To estimate the effects of the reform, I rely on the fact that the expansion of adult education was not geographically uniform. For each municipality, I measure the degree of expansion induced by the AEI as the per-capita increase in enrollment among 25–55-year-olds. Then, I classify municipalities as either a higher-expansion or lower-expansion region depending on whether they experienced above- or below-median enrollment shocks. Intuitively, my approach compares the evolution of school inputs and student outcomes in municipalities where enrollment in adult education expanded substantially (i.e., the higher-expansion regions) and municipalities where enrollment in adult education expanded relatively little (i.e., the lower-expansion regions). This difference-in-differences strategy is built on the idea that the amount of strain that the AEI put on school inputs should vary with the intensity of enrollment expansion.[1] Under the premise that larger increases in enrollment coincide with stronger negative shocks to school inputs, it is possible to deduce the relationship between school inputs and student outcomes by studying how academic performance evolves over time in the higher- and lower-expansion regions. If negative shocks to school inputs have a negative effect on students, then academic performance should decline in the higher-expansion regions relative to the lower-expansion regions after the introduction of the AEI.

My first set of results confirms the premise that regions subject to larger enrollment increases experienced greater strains on school inputs. Although the central government provided subsidies to help municipalities finance the expansion, the additional funding was stretched thin in areas where enrollment rose the most. I find that average per-pupil spending on instruction and course materials declined in the higher-expansion regions relative to the lower-expansion regions as a result of the policy. The higher-expansion regions also had a more difficult time recruiting qualified teachers. While there were

[1] For example, areas that experience larger increases in enrollment have a greater need for teachers. If qualified teachers are in short supply, schools in these regions may have to crowd more students into the same classroom or hire a larger share of unqualified, inexperienced teachers from outside the profession.

large declines in the average quality of the teaching staff across both groups, my estimates show that students in the higher-expansion regions experienced a significantly larger drop—approximately five percentage points—in the share of teachers with a formal pedagogical background or prior teaching experience. There is also some evidence that peer quality changed as a result of the reform, with declines in the average cognitive ability of classmates. There were no differential changes in class size, however.

My second set of results shows that, as a consequence of the AEI, students in the higher-expansion regions became approximately three to four percentage points more likely to drop their courses compared to students in the lower-expansion regions. This is a sizeable effect—an increase of over 10% in relation to the baseline probability of dropout. However, conditional on course completion, I find no impact on students' probability to fail their courses or pass with honors. Together with the first set of results, these findings suggest that there is a causal link between school inputs and course dropout. To support this interpretation, I also study the dynamics of the effects over time, showing that the shocks to school inputs and student outcomes both coincide with the introduction of the reform.

While my findings are highly suggestive that school inputs affect the academic outcomes of adult learners, I consider several alternative explanations. Of particular concern is the fact that the composition of the student body is bound to change as a result of the reform, and the compositional changes may be larger in higher-expansion regions. For example, if the AEI created more opportunities for younger students to participate in adult education in the higher-expansion regions, they may be more negatively selected than students in the lower-expansion regions, which could in turn explain the observed increase in course dropout. Indeed, I find that the higher-expansion regions experienced slightly larger enrollment increases among all age groups, not just the target population, particularly towards the end of the reform period. However, I perform a set of balance tests showing that the expansion did not have a differential effect on the overall composition of younger students in the higher- versus lower-expansion regions. Although I cannot rule out that unobserved characteristics changed in a way that would negatively impact student achievement, the majority of my balance tests lend credibility to that assumption and alleviate concerns about negative selection. Furthermore, I show that my main estimates are not sensitive to the inclusion or exclusion of any of the observed background characteristics, and that they are robust to interacting potentially important confounding variables, such as compulsory school grade point average, with year fixed effects to account for important changes over the pre-reform to post-reform period.

In addition to negative selection problems at the individual level, another potential identification issue is that the expansion of adult education across municipalities is not random. For example, higher-expansion regions may have increased capacity in adult education to help counteract negative trends in academic achievement and educational attainment. Moreover, given that the reform took place in the wake of an economic crisis, it is possible that higher-expansion regions had different labor market trends than lower-expansion regions, which may in turn affect student outcomes for reasons unrelated to school input shocks. Indeed, my findings indicate that higher-expansion regions are more negatively selected than lower-expansion regions in terms of labor market outcomes and educational attainment. Nevertheless, I show that despite these baseline differences, trends in average municipal characteristics follow relatively similar patterns over the study period, particularly in the years leading up to the reform. Furthermore, I provide a battery of robustness checks to assess the sensitivity of the estimates when allowing for different underlying time trends in the outcome depending on the pre-reform municipality characteristics. Allowing for different trends related to baseline educational attainment reduces the magnitude of the effects by about one third, but once these trends are controlled for, the estimates remain relatively stable to the inclusion of time trends related to other baseline characteristics.

The findings of this study contribute to a broad body of literature on school inputs and student outcomes. Most existing studies look at the effect of inputs in primary and secondary school. There are relatively few studies at the college level (see, e.g., Ehrenberg and Zhang, 2005; Hoffmann and Oreopoulos, 2009), and to the best of my knowledge, there has been no prior research on adult education outside the higher education system. This is an important omission, given that between 5 and 15% of the adult population in OECD countries participates in formal adult education (OECD, 2017), and many of these adult learners are vulnerable members of society, for example, refugees and high-school dropouts, who may be unable to compensate for poor school environments. Moreover, it is unclear that the results of prior studies are applicable to settings where there tends to be a less traditional study structure and an over-representation of students who have previously struggled to succeed in the formal education system (Skolverket, 2000).

Another key difference between my study and the existing research is that most other studies try to isolate the effect of one particular school input when holding other school inputs constant. While this allows for cleaner causal identification, it does not reflect the reality of most educational interventions, i.e., that many inputs may change at once. One notable exception in the literature is Jepsen and Rivkin (2009), who study a large-scale class-size reduction in California and show that the potential benefits of smaller classes can be offset when schools must hire inexperienced, uncertified teachers in order to meet class size targets. My findings echo these results, suggesting that the benefits of educational expansions may be diminished by resulting shocks to school resources.

As such, my study also contributes to the literature on educational expansions, providing some of the first quasi-experimental evidence that educational expansions have negative spillover effects. In a closely related study, Bianchi (2020) evaluates an expansion of undergraduate STEM education in Italy and finds negative effects on the academic performance of students who were not the target of the policy. Similar results have been found in the literature on cohort size and resource crowding (see, e.g., Bound and Turner, 2007; Babcock et al., 2012). As far as I know, however, none of the existing studies look specifically at general enrollment expansions in the adult education sector. This is a topical issue, as enrollment in adult education is on the rise in most OECD countries, and policymakers have acknowledged that lifelong learning is a key policy tool to cope with technological changes on the labor market.

The rest of the paper proceeds as follows. The next section provides an overview of the Swedish education system and the AEI, in addition to a discussion of the data and the key variables used in my analysis. Section 3 discusses the empirical strategy, sample selection, and descriptive statistics. Section 4 reports the results of the difference-in-differences analysis for school and peer inputs, followed by the corresponding results for students' academic outcomes. Section 5 presents several robustness checks and discusses the credibility and generalizability of the results. Finally, Section 6 concludes the study.

2 Context and data

All facts presented in this section apply specifically to my period of study (1993–2002). I begin with an overview of the education system in Sweden, in particular municipal adult education (*Komvux* in Swedish). Then, I proceed with a discussion of the AEI, the policy intervention that I exploit to study the relationship between school inputs and student outcomes. Finally, I present the data sources and key variables that I use for my empirical analysis.

2.1 The Swedish education system

Following nine years of compulsory education, the majority of students in Sweden choose to enroll in high school. High school education is divided into specialized tracks that are either academic or vocational in nature. Until the mid-1990s, vocational tracks lasted two years and did not grant eligibility for university admission, whereas academic tracks lasted three years and prepared students for higher education. By 1996, the vocational tracks had been converted to three-year programs, and all high school graduates met the general admission requirements for university. Some students, however, had to complete additional courses in order to become eligible for university programs with special entry criteria.[2]

Once individuals complete high school or reach age 20, they are eligible to enroll in municipal adult education in their municipality of residence. They can request to enroll in other municipalities under special circumstances, for example, if their home municipality does not offer certain courses. At the lower-secondary level, admittance is guaranteed to any student who has not finished compulsory school. At the upper-secondary level, admittance is guaranteed only when there is sufficient capacity in a course. If demand for a course exceeds the number of available spots, the school chooses which applicants to admit according to national guidelines; priority is given to those who lack a three-year

[2] For example, medical programs require specific courses in math and science.

high school degree and to those in greatest need of studying the course.[3] If a student is admitted, the municipalities must provide the education free of charge. Moreover, the central government offers various forms of financial aid to help students cover their living expenses and foregone earnings while enrolled.

Enrollment in adult education is quite common in Sweden, with over a third of a birth cohort enrolling during young adulthood (see *Figure A.1*). There are several common reasons for participating. Compulsory- or high-school dropouts may enroll to complete their degree, or graduates of two-year vocational tracks may register for the additional courses required to top up to a three-year degree. Individuals with ambitions to attend a particular university program may enroll to complete courses that were not part of their high school track but are required for admission to their desired field of study. During the period I study, it was also possible for high school graduates to sign up for courses they had already completed in high school in an attempt to improve their final grade and boost their chances of college admission. Finally, students who want additional occupational training may enroll in specialized vocational courses to supplement their previous training.[4]

The vast majority of enrollment in adult education—approximately 85%—occurs in upper-secondary courses, with only 10% in the compulsory-school level and just 5% in supplementary vocational training. Almost all courses follow a syllabus that is similar to—or in the case of upper-secondary courses, identical to—the syllabus in the regular school system. The National Agency for Education (*Skolverket*) determines both the syllabus and the grading criteria. The grading scale varies somewhat by level of instruction and has also changed over time. During the 1993/94 school year, the first year of my study period, some courses were simply graded on a pass/fail basis, and others were graded on a numerical scale of 1–5. Since the 1994/95 school year, teachers at the compulsory level and in supplementary vocational training can instead assign three grades—fail (*I*),

[3] The Ordinance on Municipal Adult Education (*Förordning om kommunal vuxenutbildning, SFS nr. 1992:403*) outlines the admission guidelines in more detail.

[4] These supplementary courses are called *påbyggnadsutbildningar* in Swedish. If the course is a continuation of specific training received in high school or another course in adult education, national guidelines stipulate that grades should be used for admission.

pass (*G*), and pass with distinction (*VG*)—while at the upper-secondary level, teachers also have the possibility to pass a student with high distinction (*MVG*). If teachers lack a sufficient basis to judge a student's mastery of the subject (e.g., due insufficient course participation), the teacher is not supposed to set a formal grade, but should instead enter a mark of *Z* into the grading catalogue.

2.2 The Adult Education Initiative

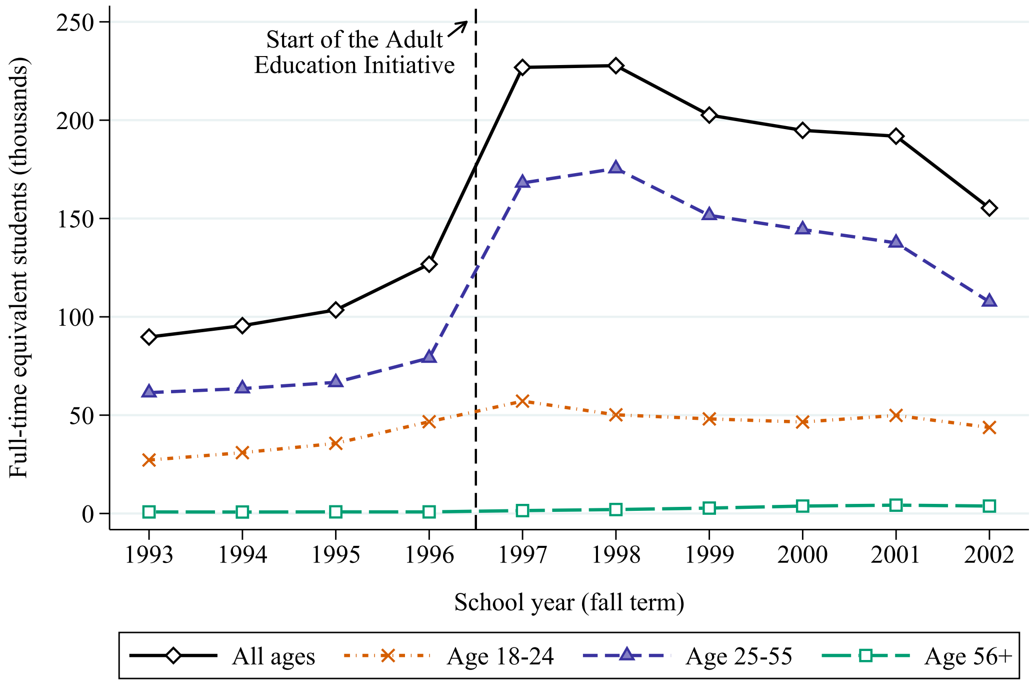
Between 1997 and 2002, the adult education sector underwent a massive expansion as a result of an intervention called the Adult Education Initiative (AEI), or *Kunskapslyftet* in Swedish. The Swedish government implemented the policy in response to a severe financial crisis that caused unemployment to rise from under 2% in 1990 to over 8% by the mid-1990s. The primary aim was to reduce unemployment among low-educated individuals by giving them a chance to obtain stronger academic credentials and raise their appeal to potential employers. In addition, the initiative was intended to revitalize the provision of adult education.

To achieve its goals, the central government financed the creation of 100,000 spots in municipal adult education, primarily at the upper-secondary level. Within just two years of the program's start in July 1997, enrollment in adult education nearly doubled. *Figure 1* shows that much of this increase resulted from a sharp jump in enrollment among individuals between ages 25 and 55. The government specifically targeted this age group with generous study allowances: for up to one year, 25–55-year-olds who were eligible for unemployment benefits could instead receive the same amount in study aid.[5]

Although the central government was in charge of financing the initiative, both in terms of providing financial aid to the students and subsidies to the municipalities, the municipal government bore the ultimate responsibility for implementing the policy. Municipalities had a large degree of freedom in determining which organizational committee

[5]These special study allowances were called UBS or *Särskilt utbildningsbidrag* in Swedish. In order to receive UBS, an individual had to be between 25 and 55 years old, study at the compulsory- or high-school level, and meet the eligibility criteria for unemployment benefits.

Figure 1: Level of enrollment in municipal adult education over time by age group.



Notes: Enrollment levels in municipal adult education (Komvux) are measured in thousands of full-time equivalent (FTE) students. I follow Statistics Sweden's definition and calculate FTEs as the total number of registered lecture hours divided by 540, where the denominator stands for 36 weeks times 15 lecture hours. Age is measured on December 31st of the reported school year. I denote school years according to the fall term, e.g., school year 1997 stands for fall term 1997 and spring term 1998.

would oversee the reform,[6] the number and type of courses that they would offer,[7] and the extent to which they would hire external providers to assist with course instruction. However, all municipalities were subject to several key requirements. First, in order to receive subsidies from the central government, they had to maintain the same "base organization" (i.e., enrollment level) that they had in the years leading up to the expansion, in addition to meeting specific volume requirements set by a central government committee. Second, they had to follow separate ordinances for admitting AEI students and regular Komvux students, at least in principle.[8] In practice, these separate admission procedures were difficult to follow, and there was some arbitrariness in whether students were officially counted as AEI participants or part of the base organization.[9] Consequently, it is not possible to determine the exact extent to which younger students and AEI participants enrolled in the same classes. To provide some idea, *Figure A.2* shows the age composition of classes attended by younger students prior to and during the AEI, and *Figure A.3* shows the share of students in these classes who received special study grants that were introduced for AEI participants.[10] While I cannot rule out the possibility that some AEI-specific classes existed alongside other Komvux classes, these figures suggest that younger students often studied together with the target population. This has two implications for my analysis. First, it highlights the importance of checking for changes in peer composition in addition to school resources. Second, it suggests that the average shocks I estimate for municipal- and school-level inputs are more likely to capture the actual input shocks faced by younger students.[11] If classes had not been integrated, it

[6]Some municipalities created special committees to carry out the administrative oversight, while others relied on the principals already in charge of organizing Komvux.

[7]The initiative aimed to promote the accumulation of general skills rather than vocation-specific skills, but the government encouraged municipalities to adjust course offerings based on the needs and preferences of their residents.

[8]Similar to the rules described earlier, there was a specific order for admitting AEI students to oversubscribed courses. The key difference was that, in the case of AEI students, preference was given to *unemployed* individuals who lacked a three-year high school degree.

[9]An explanation is provided on pages 38–39 of Skolverket's first official annual evaluation of the AEI (Skolverket, 1998). See also Gotlands kommun (2001) for anecdotal evidence.

[10]Not all AEI participants receive these special study grants (UBS), but this type of funding was available only for the target population, and thus receipt of UBS can serve as a proxy to measure AEI participation. Note this is likely a lower bound on the share of AEI participants in the class.

[11]Due to data limitations, I cannot link teachers directly to their students, nor can I see how much money each school spent per student in municipalities with more than one provider of adult education.

would have been easier for schools to target their resources, e.g., more qualified teachers or additional funding, at specific students.

2.3 Data sources and definition of key variables

The analysis uses administrative data from Sweden covering all participants in municipal adult education between 1993 and 2002. Several key variables come from the Komvux registry (*Komvuxregistret*), which contains detailed enrollment history and course transcripts for the full population of students. For each course that a student enrolls in, I observe the total number of lecture hours, an indicator of whether the individual de-registered from the course, and their final grade. The variable for lecture hours allows me to calculate the number of full-time student equivalents and capture the variation in enrollment over time and across regions. This regional and temporal variation in enrollment is key for my identification strategy. The variables for course grades and de-registration provide measures of academic performance that serve as the main student outcomes in my analysis. All of these student outcomes are defined at the course level and are binary variables equal to one if a condition is met and zero otherwise. For example, course dropout equals one if the individual does not earn a grade in the course[12] and zero if they obtain any grade. I also create an indicator for earning credits (i.e., receiving any passing grade) in the course. Finally, for the subsample of course completers from 1994–2002, I analyze the probability of failing, passing, or passing with honors.[13] Note that I cannot study these grade outcomes in 1993 because a different grading scale was in place that year. Moreover, I do not have data on the number of credits earned per course prior to 1997, so I am unable to study credit accumulation.

The Komvux registry includes a school code that allows me to link students to the adult education teachers employed at their school at the start of the academic year. Via the National Teacher Registry (*Lärarregistret*), I obtain annual information on teachers' certification status and accumulated years of teaching experience since 1985. I also extract information on teachers' completed years of schooling via the Integrated Database

[12] For example, if they unregister, or if they fail to participate enough for the teacher to assign them a grade.

[13] Upper-secondary courses have two honors grades—distinction and high distinction—whereas the others only have one honors grade. Thus, for upper-secondary courses, I consider both pass with distinction and pass with high distinction as receiving honors.

for Labor Market Research (*LOUISE*). I use the data on these three teacher characteristics to measure the average teacher qualifications that students are exposed to. I do not observe the exact courses taught by each teacher, so I construct school-by-instruction-level averages of the characteristics, weighing each teacher's characteristic by their percent of employment such that more weight is given to the qualifications of full-time teachers than part-time teachers.[14]

In addition to the school code, the Komvux registry also includes course codes and detailed course information that enable me to approximate classes of students who study a course together (see the data appendix for details). I use this information to measure class-level peer quality and class size, which I define as the number of registrants at the start of the course. I measure peer quality using several different variables, including students' own education level and parents' education level. In addition, I construct a proxy of cognitive ability using compulsory school GPA for younger individuals and imputed with military enlistment test data for older male students.[15] Finally, I study the average age of peers and the share of female peers in a class.

As a complement to the non-financial school inputs that I study, I collect data on each municipality's expenditure on adult education. The data for 1993 through 1998 came in paper form from Statistics Sweden's archive and had to be digitized, whereas the National Agency for Education delivered the data in digital form for school years 1999 through 2002. All variables are reported at the municipal level and are measured as costs per full-time-equivalent students. My analysis studies the log of per-pupil expenditure on instruction, learning materials, and learning facilities.

[14] The teacher registry contains all employees with valid contracts in October. Thus, if schools hire new teachers during the spring term, these teachers are excluded from the averages. Additionally, for a small share of the students (ca. 4%), I can only match to teacher characteristics at the municipal level. My main estimates are not sensitive to the exclusion of these students (results available upon request).

[15] While I do have data on prior academic achievement for the younger students in my main analysis sample, I do not have these measures for the full set of adult education students during the period I study because IFAU's compulsory school and high school registries only date back to the late 1980s. However, for older men, I have information on cognitive ability from mandatory military enlistment tests.

3 Empirical strategy

My identification strategy exploits enrollment shocks induced by the AEI to generate plausibly exogenous variation in school inputs. The crux of the identification strategy is that regions subject to larger increases in enrollment as a result of the AEI experience stronger negative shocks to school inputs. Under this premise, it is possible to assess the impact of school inputs on student outcomes by studying how student performance evolves over time in regions subject to higher versus lower enrollment shocks. If school inputs matter, student outcomes should decline in higher-expansion regions relative to lower-expansion regions after the introduction of the AEI.

A potential issue with this empirical strategy is that the educational expansion I exploit is likely to change the composition of students enrolled in adult education. It is reasonable to expect that the average ability level declines with the influx of new students, and these declines are likely to be stronger in areas where enrollment expands the most. Any observed changes in student performance may therefore reflect changes in students' underlying academic ability.

One crucial feature of the AEI allows me to address concerns related to negative selection. As discussed earlier, the intervention primarily targeted low-educated, unemployed individuals aged 25 to 55 by incentivizing their enrollment with generous study allowances. In contrast, there were no significant changes in the financial incentives or admission rules for younger students. This means that selection issues are likely a considerable concern among the older population but less so for the younger students. I therefore restrict my analysis to individuals under age 25 at the time of enrollment, and in Section 3.3, I provide a formal test to show that—at least on observed characteristics—there are relatively minor compositional changes in the higher- relative to the lower-expansion regions among this subsample. I also perform robustness checks to show that my main estimates are not sensitive to the inclusion or exclusion of various student characteristics as control variables.

3.1 Identifying variation

In order to implement my identification strategy, I need to define a measure that captures regional variation in the intensity of enrollment shock caused by the AEI. Since the AEI targeted individuals aged 25 through 55, I focus on the variation in enrollment for this age group. I measure the intensity of expansion in each municipality as the difference between average per-capita enrollment amongst 25–55-year-olds during the school years that the AEI was in place (1997–2002) and the four school years prior to the AEI (1993–1996). That is, the enrollment shock for a given municipality is defined as:

$$Expansion_m = \sum_{y=1997}^{2002} \frac{1}{6} \cdot \frac{Enrollment25to55_{m,y}}{Population25to55_{m,y}} - \sum_{y=1993}^{1996} \frac{1}{4} \cdot \frac{Enrollment25to55_{m,y}}{Population25to55_{m,y}} \quad (1)$$

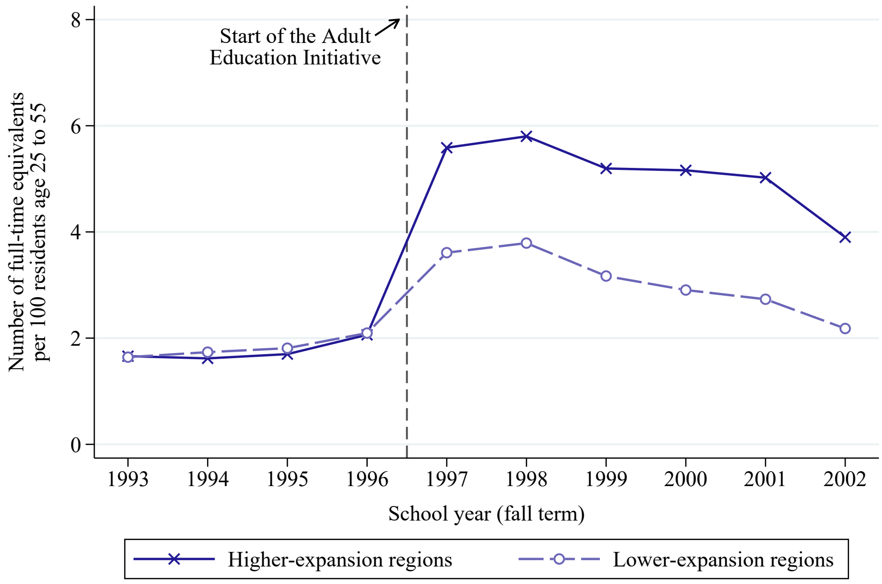
where subscript y indexes the school year and m indexes the municipality. The variable $Enrollment25to55_{m,y}$ is the number of full-time-equivalent students aged 25 through 55 registered in municipality m during school year y, and the variable $Population25to55_{m,y}$ is the number of individuals aged 25 through 55 residing in municipality m (measured in hundreds) during the year. The higher the value of $Expansion_m$, the larger the enrollment shock.

Panel (a) of *Figure A.4* shows the variation in enrollment shocks across different municipalities. For ease of exposition, I divide the municipalities into two groups in the remainder of my empirical analysis. The higher-expansion group consists of the 143 municipalities that experienced above-median enrollment shocks, and the lower-expansion group consists of the 143 municipalities that experienced below-median enrollment shocks. Panel (b) of the figure shows the municipalities according to this binary classification.

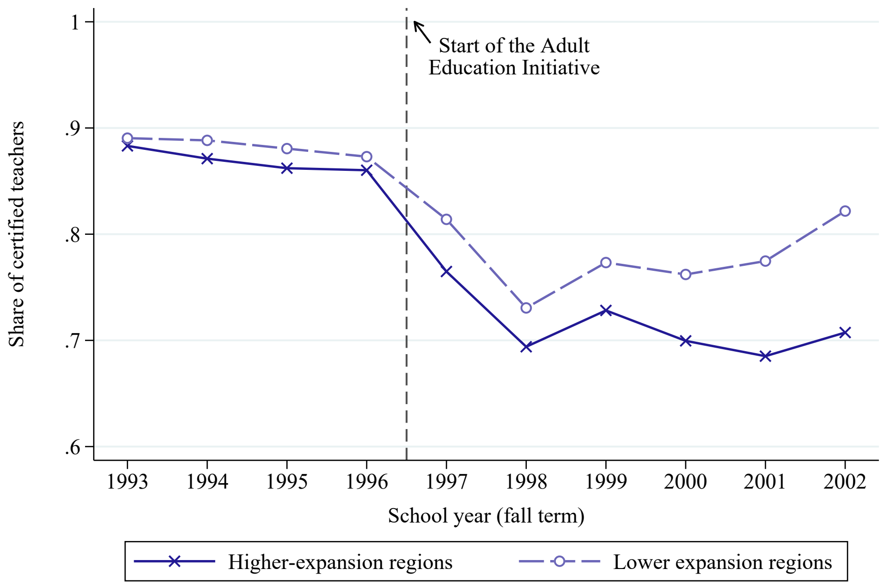
An essential question is whether the higher-expansion municipalities experience sufficiently large enrollment shocks relative to the lower-expansion municipalities, such that we should expect the strain on school inputs to be larger in the higher-expansion regions. I analyze this more formally in the results section, but to preview the results, *Figure 2* plots the strength of the enrollment shocks in the two groups and provides an example

Figure 2: Illustration of the identification strategy.

(a) Enrollment shocks per capita



(b) Students' exposure to certified teachers



Notes: Higher-expansion regions experienced above-median enrollment shocks, and lower-expansion regions experienced below-median enrollment shocks. In panel (b), certified teachers are those with a college degree in pedagogy.

to illustrate that greater increases in enrollment coincide with stronger negative shocks to school inputs. In panel (a), we see that prior to the start of the AEI, enrollment per capita in the higher- and lower-expansion regions was essentially equal. When the AEI began in 1997, the enrollment level jumped in both regions, but the shock was much larger in magnitude in the higher-expansion regions. These differences persisted through the end of the AEI and even widened slightly after 1998 as enrollment tapered off more quickly in the lower-expansion regions. In panel (b), we see a similar pattern in students' exposure to qualified teachers. Prior to the AEI, students in the higher- and lower-expansion regions were taught by teachers with similar credentials: on average, about 85% of teachers in their school were certified. After the introduction of the AEI, this percentage dropped sharply in both regions, as the increased demand for teachers meant that municipalities had to hire teachers without a pedagogical background (see *Figure A.5* in the appendix). However, the declines in teacher qualifications were steeper in the higher-expansion regions, particularly after 1998, when enrollment levels declined relatively faster in the lower-expansion regions.

3.2 Difference-in-differences specification

The preceding example suggests that the binary classification of municipalities by above-median and below-median enrollment expansion accurately captures school input shocks. To more formally compare the evolution of outcomes in higher- and lower-expansion regions, I use the following difference-in-differences model:

$$Outcome_{i,c,s,m,y} = \gamma(HigherExpansion_m \times PostAEI_y) + \alpha_m + \beta_y + \varepsilon_{i,c,s,m,y} \qquad (2)$$

where subscript i indexes an individual, c indexes a course, s indexes a subject, m indexes the municipality of enrollment, and y indexes the school year. In the first part of my analysis, the dependent variable $Outcome_{i,c,s,m,y}$ measures student i's exposure to various school inputs, and in the second part, it measures student i's achievement in course c. The indicator $PostAEI_y$ equals one for all school years after the introduction of the AEI (1997–2002), and $HigherExpansion_m$ equals one for municipalities that experienced an above-median intensity enrollment shock as a result of the policy. For all outcomes, the specification includes municipality fixed effects (α_m) and school-year fixed effects (β_y).

When studying student outcomes, I enrich the model specification in several ways. First, because I pool observations for all courses, I include subject fixed effects (ω_s).[16] These fixed effects account for unobserved heterogeneity due to the fact that some subjects may be inherently harder to pass than others, for example, if some subjects are systematically graded more harshly than others. I also include subject-by-year fixed effects ($\theta_{s,y}$) in order to capture any time-varying subject-specific shocks, for example, changes in the difficulty of tests or course material. Finally, I include a vector of individual-level control variables that may be related to student performance ($X'_{i,y}\psi$): age, sex, civil status, presence of young children in the household, foreign background, parents' highest level of education, and own level of education. I do not include prior academic achievement in the main specification because compulsory school GPA is missing for approximately 10% of the sample; however, I perform robustness checks to show that the results are not sensitive to its inclusion.

The parameter of interest, γ, measures how outcomes evolved in the higher-expansion regions relative to the lower-expansion regions after the introduction of the AEI. It captures the average effect of the AEI on various school inputs and student outcomes under the assumption that the outcomes would have followed parallel paths in the absence of the intervention. To explore the pattern of the effects over time, I also estimate the following dynamic difference-in-differences specification where I replace *PostAEI* in equation (2) with a set of year dummies:

$$Outcome_{i,c,s,m,y} = \sum_{y=1993}^{1995} \lambda_y \cdot HigherExpansion_m + \sum_{y=1997}^{2002} \lambda_y \cdot HigherExpansion_m + \alpha_m + \beta_y + \varepsilon_{i,c,s,m,y} \tag{3}$$

The coefficients of interest, λ_y, are normalized with respect to the year prior to the AEI. In

[16] I define subjects using a combination of the prefix in the course code (e.g., MA for mathematics) and the level of study (e.g., lower secondary or upper secondary). The results are robust to using course fixed effects instead of subject fixed effects, but I prefer to use subject fixed effects in the main specification because some course codes have changed over time, particularly between 1993 and 1994 and between 1999 and 2000.

addition to shedding light on the dynamics of the effects, this specification allows me to evaluate whether the parallel trends assumption is credible; if so, the pre-AEI coefficients λ_{1993}, λ_{1994}, and λ_{1995} should be statistically indistinguishable from zero.

3.3 Sample selection and description of the sample

To construct my sample, I start with the full population of students enrolled in municipal adult education between fall term 1993 and fall term 2002. As discussed earlier, I first restrict my analysis to individuals who were age 18 to 24 in the year of enrollment in order to mitigate concerns that changes in student composition drive my results. Additionally, I drop a small share of individuals who cannot be matched to background characteristics from the national population registers; courses that, by law, do not assign grades or follow a traditional course plan (e.g., introductory courses and individualized courses); classes that cannot be linked to subject codes or have missing information on course duration or other characteristics; classes with fewer than four students; and classes with unreported grades.[17] *Table C.2* in the data appendix documents the number of observations lost at each step of the sample selection. The resulting sample consists of 430,669 individuals, and a total of 3,240,448 observations at the course level. If an individual registers for the same course multiple times, I include all course attempts in the estimation sample. As a robustness check, I show that the results are unchanged if I instead restrict the sample to the individual's first course attempt.

Table A.1 in the appendix shows the descriptive statistics for the sample of students in higher- and lower-expansion regions. The two groups are relatively similar, though students in the higher-expansion regions are slightly less likely to have a foreign background and have slightly weaker performance in both compulsory school and high school. While it is not essential that students' average characteristics are identical in the two groups,

[17] When a student is still registered for a course but does not attend enough lectures or turn in assignments for a final grade, teachers are supposed to record a grade of Z rather than a missing value. However, grades are missing for approximately 10–15% of the observations each year. Some of these are for valid reasons, e.g., introductory courses where students are never assigned grades. However, for most courses, it is impossible to know whether the teacher simply failed to record a grade or whether the student failed to submit the required assignments. To be conservative, I drop all classes with unreported grades, but the main findings are unchanged when I relax this restriction. See the robustness section and data appendix for additional details.

Table 1: Checks for covariate balance over the study period.

	Estimate (1)	Std. Err. (2)	P-value (3)
Age	0.045	0.036	0.211
Female	0.007	0.005	0.215
Married	-0.001	0.003	0.715
Any young children in household	0.003	0.004	0.462
Born in Sweden	-0.003	0.007	0.707
Swedish-born mother	0.005	0.008	0.560
Swedish-born father	0.004	0.008	0.602
Mother's years of schooling	0.008	0.029	0.794
Father's years of schooling	0.014	0.035	0.698
Not a high school graduate	0.002	0.009	0.811
Graduate of academic track	-0.007	0.009	0.453
Graduate of vocational track	0.005	0.007	0.526
High school GPA (std.)	-0.009	0.015	0.537
Compulsory school GPA (std.)	-0.035	0.017	0.043
P-value for test of joint significance: 0.401			

Notes: Each entry of column (1) reports the estimate of the interaction term in equation (2) from separate regressions where the listed characteristic is the outcome variable. Compulsory school and high school GPA are standardized. Standard errors in column (2) are clustered at the municipal level.

an underlying assumption of my identification strategy is that group composition does not change differently across higher- and lower-expansion regions in a way that is correlated with student outcomes. Of particular concern is the fact that higher-expansion regions experienced slightly larger enrollment increases among 18–24-year-olds, not just the target population, especially towards the end of the reform period (see *Figure A.6* in the appendix). Before proceeding to the main empirical analysis, I provide evidence that despite these different enrollment patterns, there were no major differential changes in the composition of students in the higher-expansion and lower-expansion regions. To this end, I estimate the main difference-in-differences model in equation (2) and the dynamic difference-in-differences specification in equation (3) using students' background characteristics as the dependent variable. *Table 1* reports the results of the balance tests for each characteristic, and *Figure A.7* in the appendix plots the dynamics over time.

The test for the joint significance of all characteristics has a p-value of 0.401, indicating no significant overall changes between the two groups. This is confirmed by the separate regressions for each coefficient. The point estimates are rather small in magnitude, and every estimate is statistically indistinguishable from zero at conventional significance levels, with the exception of the estimate for compulsory school GPA. Given the number of variables that I test, this could be due to random chance; indeed, more recent achievement measures like high school completion and high school GPA suggest no differential changes in academic ability between the two groups over time. Nevertheless, the time trends in panel (l) of *Figure A.7* indicate that the negative selection on compulsory school achievement coincides with the introduction of the reform and thus may be an important confounding variable. In light of this, I perform several sensitivity analyses in Section 5 to show that my main estimates are virtually unchanged when I add compulsory school GPA as a control variable in the model, either on its own or interacted with year fixed effects.

3.4 Description of higher- and lower-expansion regions

My empirical strategy uses a geographically-based treatment definition capturing per-capita enrollment increases in adult education at the municipal level over the course of my study period. A key threat to causal identification is the lack of random variation in which municipalities experience stronger or weaker enrollment shocks. The map in *Figure A.4* shows that there is some geographical clustering in the intensity of educational expansion, with fairly large low-expansion areas and high-expansion areas. Before proceeding to the analysis, it is therefore important to investigate the extent to which systematic differences between higher- and lower-expansion regions might be a concern. Note that my main model specification includes municipality fixed effects to control for time-invariant differences across municipalities that might be related to the level of adult education expansion, school inputs, and student outcomes. Thus, it is not necessary for the enrollment shocks that I exploit for identification to be unrelated to municipal characteristics. Nevertheless, significant differences in municipal characteristics could suggest that the time trends in school inputs and student outcomes might have diverged even in the absence of the AEI, which would violate the parallel trends assumption. For example, given that the reform took place in the wake of a severe economic crisis, it is plausible that higher-expansion regions had differential labor market trends, which might in turn affect student outcomes for reasons unrelated to school input shocks.

In order to investigate the extent of this concern, I report the average baseline characteristics of higher- and lower-expansion regions in *Table A.2* in the appendix. I also show the correlation between various municipal characteristics and the continuous measure of enrollment expansion in *Table A.3*. Finally, I plot time trends in various municipal characteristics over the course of the study period in *Figure A.8*. From these results, it is clear that higher-expansion regions are more negatively selected than lower-expansion regions. The intensity of educational expansion has a strong negative correlation with annual labor earnings and employment, in particular the employment rate among 25–55-year-olds. Moreover, it has a strong positive correlation with the share of low-educated individuals, defined as those who have completed less than three years of high school education. These descriptive patterns are not surprising, given that the reform targeted low-educated, unemployed individuals age 25–55.

Despite the significant baseline differences, most trends in municipality characteristics evolve in a fairly parallel fashion over the study period, particularly in the years leading up to the reform. When it comes to labor market trends, employment among individuals age 18–24 seems to increase slightly faster in the higher-expansion regions relative to the lower-expansion regions in the post-AEI period, although this does not seem to be accompanied by faster annual earnings growth (if anything, the opposite). Nevertheless, it would be problematic for my identification strategy if students drop their courses to take advantage of changing labor market prospects, e.g., due to decreased competition from unemployed individuals age 25–55 who now enroll in adult education rather than search for a job. In that case, any effects that I observe could be driven by labor market trends rather than school input shocks. In Section 5, I check whether this seems to be the case by studying labor market outcomes shortly after initial course enrollment. Additionally, in light of the significant differences in important baseline characteristics, I perform a battery of robustness checks to show how the estimated effects change when allowing for different underlying trends in the outcome variables depending on pre-reform municipality characteristics.

4 Results

4.1 Effects on school and class inputs

My empirical strategy rests on the premise that regions subject to larger enrollment increases following the introduction of the AEI experience stronger negative shocks to school inputs. The descriptive plot in *Figure 2* suggests that this is the case, at least for teacher certification status. In this section, I provide formal evidence by estimating the difference-in-differences model in equation (2) using various school inputs as the dependent variable. I analyze several types of inputs: teacher credentials, log per-pupil expenditure, and class characteristics such as class size and peer quality.

Table 2 presents the results of the difference-in-differences analysis for school and class inputs. The first row of each panel reports the estimate of the interaction term, γ, which captures how inputs evolved in regions with higher expansion intensity relative to regions with lower expansion intensity after the start of the AEI. The results confirm that the stronger the enrollment shock, the stronger the negative shock to the input variables. Relative to students in regions that experienced lower enrollment shocks, students in the higher-expansion regions were taught by teachers who were less educated and more likely to be inexperienced and uncertified. The higher-expansion municipalities also spent less money per student on the cost of instruction and learning materials. Although the effects are insignificant, there also appear to be relative declines in per-student expenditure on school facilities. Finally, there were some slight changes in peer composition, with a marginally significant increase in the average age of classmates, in addition to declines in peers' socioeconomic status (measured by parents' education level) and cognitive ability (measured by compulsory school GPA and military enlistment test scores). Class size, however, remained unchanged as a result of the reform.

The dynamic difference-in-differences plots in *Figure 3* and *Figure 4* confirm these findings and shed light on how the inputs evolve pre- and post-reform. The figures report the annual coefficients from the estimation of equation (3) for each school input. All of the pre-reform coefficients for teacher credentials and school resources, as well as the majority of pre-reform coefficients for peer inputs, are statistically indistinguishable from

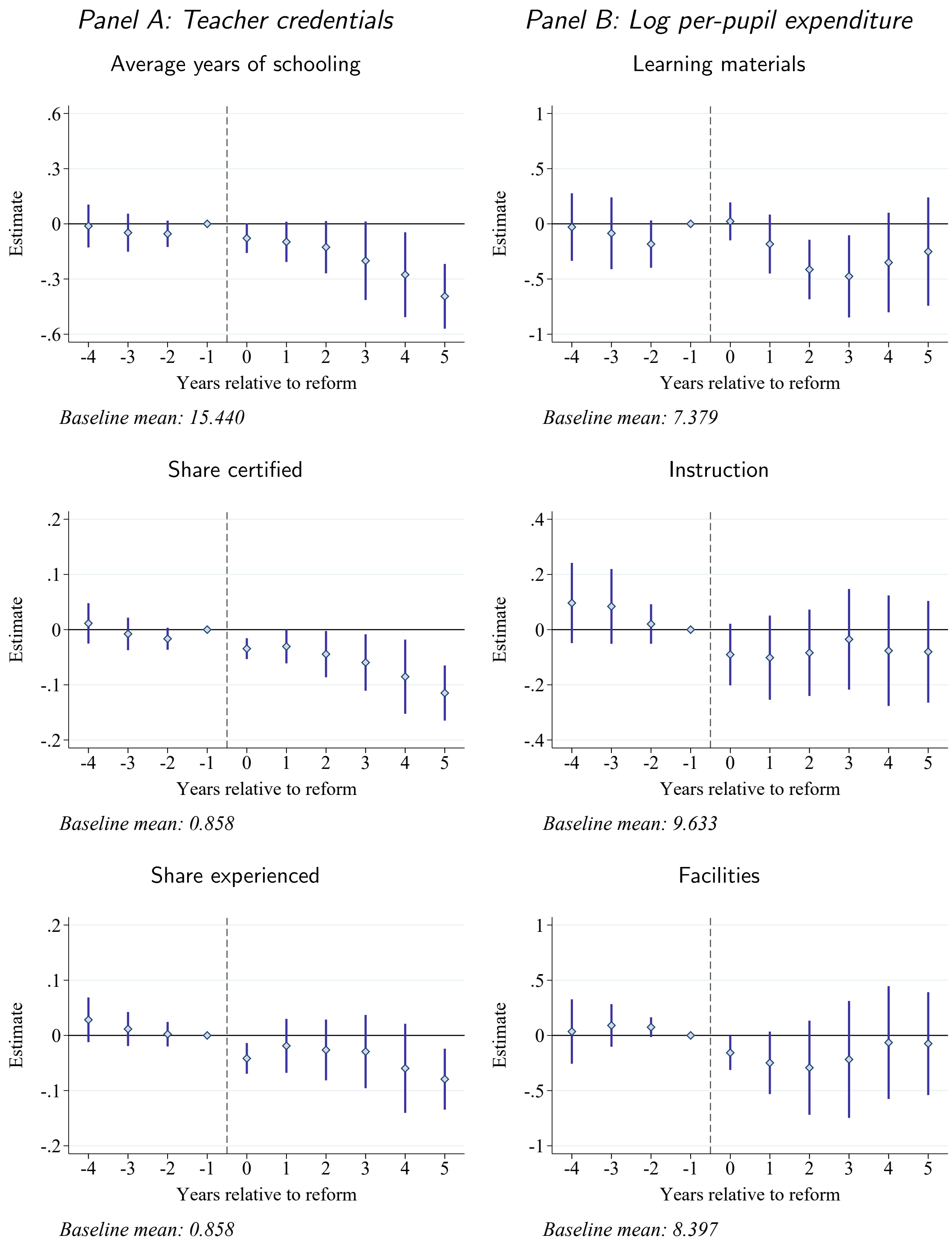
Table 2: Difference-in-differences estimates for school and peer inputs.

	Panel A. School resources					
	Teacher credentials			Log per-pupil expenditure		
	Years of schooling (1)	Share certified (2)	Share experienced (3)	Learning materials (4)	Cost of instruction (5)	School facilities (6)
Diff-in-Diff estimate	-0.150	-0.052	-0.047	-0.189	-0.117	-0.231
	(0.055)***	(0.015)***	(0.020)**	(0.107)*	(0.048)**	(0.153)
Pre-reform averages:						
Higher-expansion areas	15.454	0.860	0.851	7.284	9.702	8.431
Lower-expansion areas	15.524	0.870	0.848	7.131	9.657	8.391
Post-reform averages:						
Higher-expansion areas	15.013	0.739	0.751	7.204	9.665	8.282
Lower-expansion areas	15.245	0.796	0.787	7.286	9.719	8.427
Number of observations	3,230,825	3,230,825	3,230,825	3,157,089	3,169,521	3,160,466

	Panel B. Characteristics of classroom peers					
	Class size (1)	Average age (2)	Share female (3)	Years of schooling (4)	Parents' years of schooling (5)	Cognitive ability (6)
Diff-in-Diff estimate	0.196	0.332	0.003	-0.018	-0.057	-0.034
	(1.602)	(0.184)*	(0.007)	(0.018)	(0.032)*	(0.017)**
Pre-reform averages:						
Higher-expansion areas	39.386	27.528	0.621	11.066	11.049	-0.205
Lower-expansion areas	41.866	27.577	0.624	11.166	11.484	-0.138
Post-reform averages:						
Higher-expansion areas	34.234	29.071	0.635	11.197	11.074	-0.307
Lower-expansion areas	37.776	28.612	0.627	11.332	11.604	-0.190
Number of observations	3,240,448	3,240,448	3,240,448	3,240,286	3,230,990	3,205,804

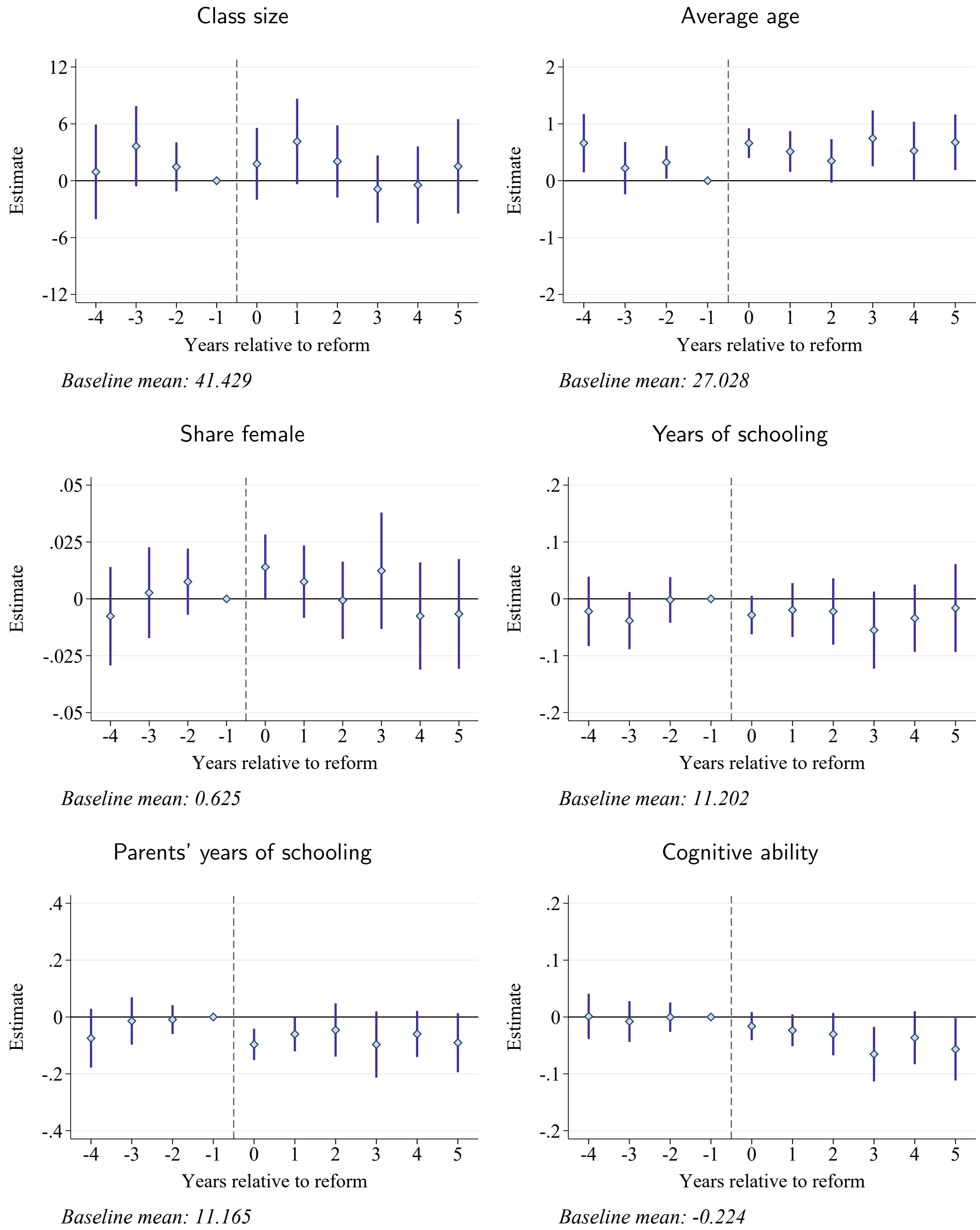
Notes: Students appear in the estimation sample once per registered course attempt. Teacher characteristics are calculated at the school-by-instruction level. Certified teachers are teachers with a college-level degree in pedagogy, and experienced teachers are teachers with 3+ years of teaching experience. Log per-pupil expenditure is calculated at the municipal level and measured in terms of full-time equivalents. Peer characteristics are measured at the class level (see data appendix for details on how classes are approximated). Cognitive ability is proxied by standardized compulsory-school GPA (if available) or military enlistment test score (if available and GPA is missing). Standard errors are shown in parentheses and clustered at the municipal level. Stars denote significance levels: *** for $p < 0.01$; ** for $p < 0.05$; * for $p < 0.10$.

Figure 3: Dynamic difference-in-differences estimates for school inputs.



Notes: In all panels, the dashed vertical line indicates the introduction of the AEI, and the baseline mean refers to the average in the higher-expansion regions in 1996. Each point plots the estimates of λ_y from equation (3), and the vertical bars show the 95% confidence intervals for the estimates when clustering the standard errors at the municipal level. In panel (A), teacher characteristics are calculated at the school-by-instruction level. Certified teachers are those who hold a pedagogy degree. Experienced teachers have at least three years of experience. In panel (B), log per-pupil expenditure is calculated at the municipal level. Individuals appear in the estimation sample once per registered course attempt.

Figure 4: Dynamic difference-in-differences estimates for class inputs.



Notes: In all panels, the dashed vertical line indicates the introduction of the AEI, and the baseline mean refers to the average in the higher-expansion regions in 1996. Each point plots the estimates of λ_y from equation (3), and the vertical bars show the 95% confidence intervals for the estimates when clustering the standard errors at the municipal level. Individuals appear in the estimation sample once per registered course attempt.

zero, suggesting that the inputs would have evolved similarly in the absence of the enrollment shock caused by the AEI. The higher-expansion regions immediately experience a stronger strain on school resources and peer quality after the implementation of the AEI, with some shocks growing slightly stronger over time. If these school inputs matter for student achievement, we should expect to see similar patterns for student outcomes.

4.2 Effects on course outcomes

The previous section established that students in regions where the AEI induced higher enrollment shocks were taught by less-qualified teachers and exposed to more negatively-selected peers than students in regions that experienced lower enrollment shocks. Additionally, per-pupil expenditure declined in the higher-expansion regions relative to the lower-expansion regions. If these school inputs have a positive impact on students' academic achievement, the outcomes of students in the higher-expansion regions should decrease relative to the outcomes of students in the lower-expansion regions after the start of the AEI. To investigate this, I repeat the difference-in-differences analysis specified in equation (2) using different course outcomes as the dependent variable. In particular, I look at the probability of dropping out of a course and earning credit in the course, i.e., receiving any passing grade in the course. For the subsample of course completers, I also look at whether there is any effect on students' grades, including the probability to receive a grade of fail, pass, or pass with honors.

Table 3 reports the estimate of the interaction term γ for each outcome variable. Column (1) shows that as a result of the AEI, students in higher-expansion regions became almost four percentage points more likely to drop out of a course relative to students in lower-expansion regions. This is a sizable effect, approximately a 12% increase over the baseline probability of dropout. Column (2) shows that students in higher-expansion regions also became less likely to earn credit in the course, although this decrease is driven by the increased dropout rate rather than an increase in the probability of failing the course. Columns (3)–(5) show that, conditional on course completion, students' grades are unaffected by the reform.

The estimates reveal that, on average, students in higher-expansion regions had higher

Table 3: Difference-in-differences estimates for student performance.

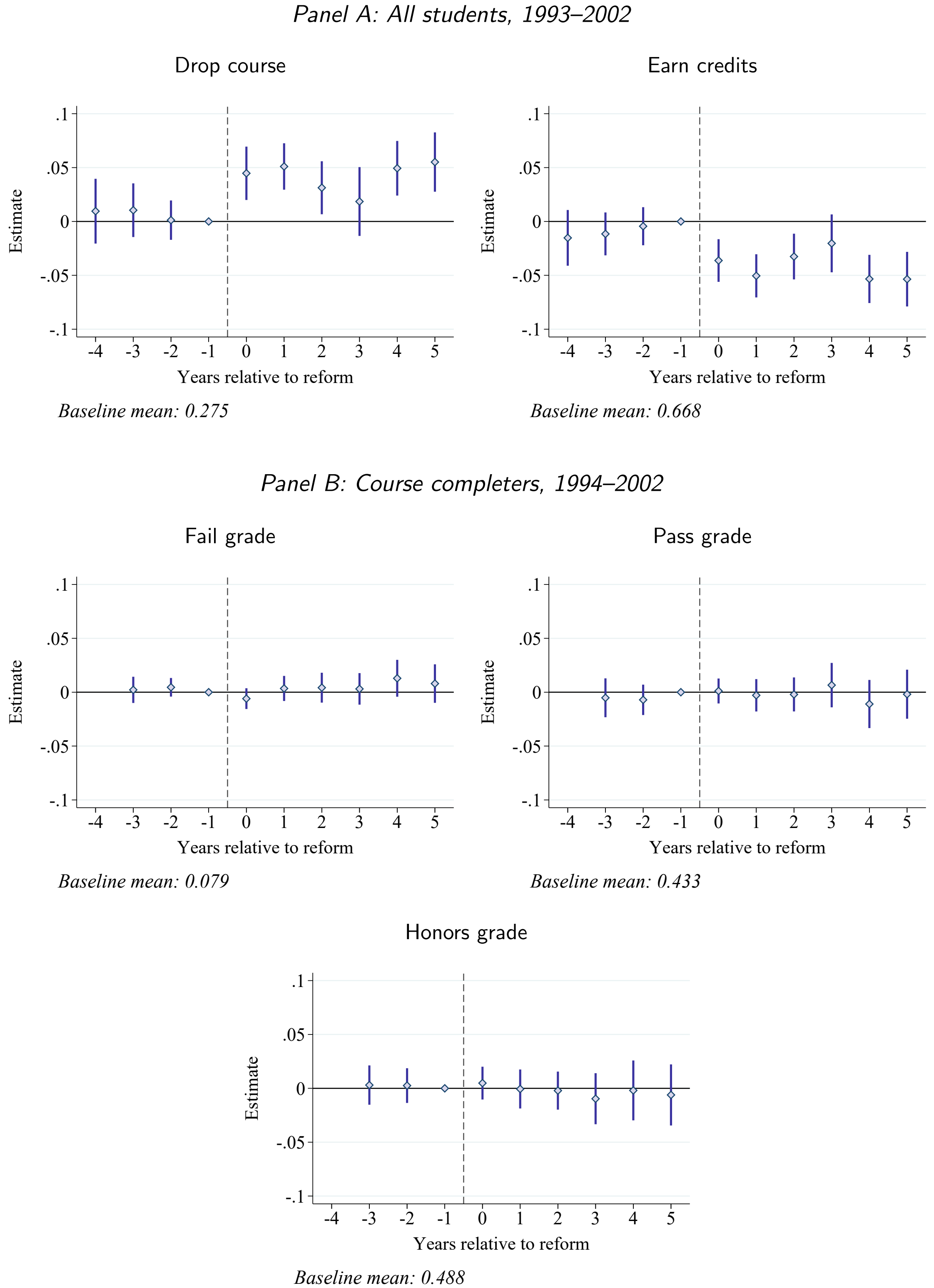
	All students, 1993–2002		*Course completers, 1994–2002*		
	Drop course (1)	Earn credit (2)	Fail grade (3)	Pass grade (4)	Honors grade (5)
Diff-in-Diff estimate	0.037	-0.034	0.001	0.002	-0.004
	(0.007)***	(0.007)***	(0.005)	(0.007)	(0.009)
Pre-reform averages:					
Higher-expansion areas	0.297	0.659	0.072	0.449	0.479
Lower-expansion areas	0.305	0.647	0.080	0.434	0.486
Post-reform averages:					
Higher-expansion areas	0.356	0.584	0.093	0.358	0.549
Lower-expansion areas	0.333	0.595	0.108	0.342	0.550
Number of observations	3,240,448	3,240,448	2,062,652	2,062,652	2,062,652

Notes: Students appear in the estimation sample once per registered course attempt. Outcomes in columns (1)–(2) are unconditional probabilities, and estimates are obtained using the full sample of students from 1993–2002. Outcomes in columns (3)–(5) are conditional probabilities, and estimates are obtained using the sub-sample of course completers from 1994–2002. Year 1993 is dropped from columns (3)–(5) due to a change in grading scale. All regressions include year, municipality, subject, and subject-by-year fixed effects, as well as individual-level controls for age, sex, civil status, presence of young children in the household, foreign background, parents' highest level of education, and own level of education. Standard errors are cluster-robust at the municipal level and shown in parentheses. Stars denote significance levels: *** for $p < 0.01$; ** for $p < 0.05$; * for $p < 0.10$.

dropout rates than students in lower-expansion regions as a result of the AEI. This suggests that school inputs do have some impact on academic outcomes. To investigate this more closely, I study whether the dynamics of the effects for course outcomes line up with the patterns for school inputs observed in *Figure 3* and *Figure 4*. To this end, I estimate the dynamic difference-in-differences specification in equation (3) for each of the course outcomes and plot the coefficients over time in *Figure 5*.

Reassuring for the identification strategy, the coefficients in the pre-reform years are again statistically indistinguishable from zero, which lends credibility to the assumption that students' course outcomes would have evolved similarly in the higher- and lower-expansion regions if they had not been subjected to the enrollment and resource shocks. After the introduction of the AEI, we see immediate declines in student performance, with the effects remaining fairly stable over time. The fact that these effects coincide with the immediate declines in school resources and peer composition is highly suggestive of a

Figure 5: Dynamic difference-in-differences estimates for course outcomes.



Notes: The vertical line indicates the start of the AEI, and the baseline mean refers to the average in the higher-expansion regions in 1996. Each point plots the estimates of λ_y from equation (3), and the vertical bars plot the 95% confidence intervals with standard errors clustered at the municipal level. Outcomes in panel (A) are unconditional probabilities and estimated for the full sample from 1993–2002. Outcomes in panel (B) are conditional probabilities and estimated for the subsample of course completers from 1994–2002. Individuals appear once for every course attempt. Regressions include individual controls and the following fixed effects: municipality, year, subject, and subject by year.

causal link between school inputs and course dropout.

4.3 Heterogeneity analysis

A policy-relevant question is whether the effects of school input shocks are stronger for students who come from disadvantaged backgrounds or who have had low achievement levels in the past. If these students have a harder time compensating for poor resources at school, they may be particularly susceptible to changes in school inputs. Indeed, previous research at the primary and secondary level has shown that students from disadvantaged backgrounds can be more sensitive to changes in school inputs and school quality than students from more advantaged backgrounds (see, e.g., Krueger and Whitmore, 2001; Bloom and Unterman, 2014; Jackson et al., 2016). I investigate whether this also applies to adult learners by performing two different heterogeneity analyses. First, I check whether the results differ by the education level of students' parents, i.e., whether one of their parents has some post-secondary education or not. Additionally, I check whether the results differ for high school graduates and dropouts. I show the results of these two heterogeneity analyses in panels (A) and (B) of *Figure A.9*, respectively. The pattern of effects is quite similar for students with higher- versus lower-educated parents. Similarly, there are no significant differences between high school graduates and dropouts. This suggests that students with more vulnerable socioeconomic or academic backgrounds are not impacted more negatively by school input shocks.

Prior research on the returns to adult education has shown that women have significant benefits from participating in adult education, whereas the returns are weaker or even insignificant for men (see, e.g., Jacobson et al., 2005; Stenberg et al., 2014; Blundell et al., 2020). If women are more likely to drop out and less likely to earn course credit in response to school input shocks, it could thus have particularly negative consequences for them in the longer run. I investigate whether the effects differ for men and women in panel (C) of *Figure A.9*. The magnitude of the estimates is consistently larger for women than for men, suggesting that women's academic performance may be slightly more sensitive to changes in school inputs; however, the estimates for men and women are not statistically different from one another at conventional significance levels.

5 Sensitivity and credibility of the results

5.1 Robustness checks

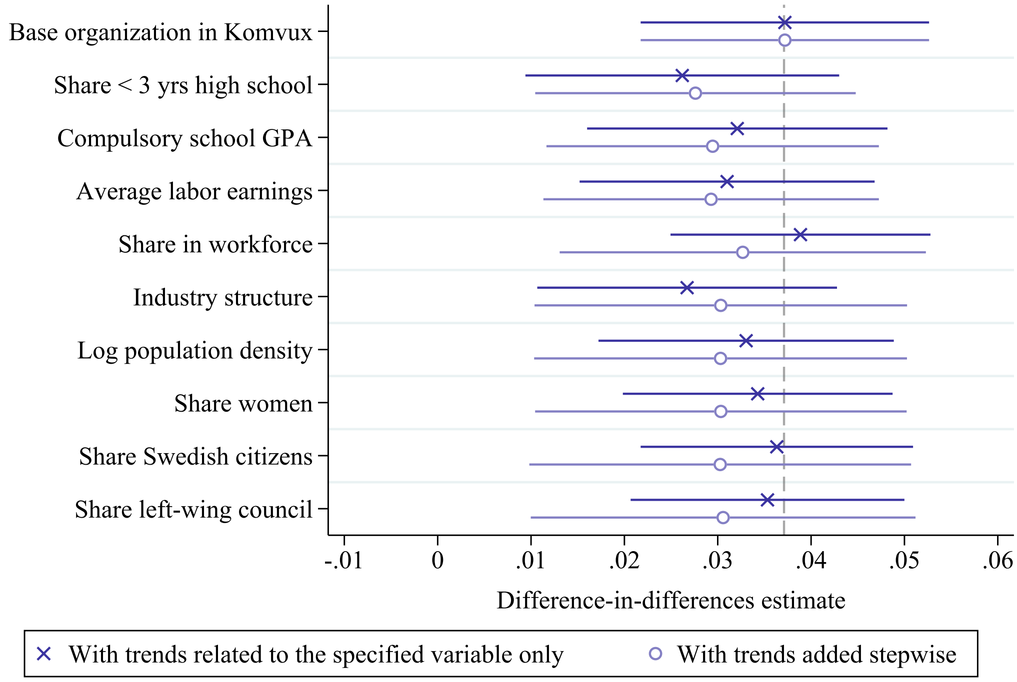
According to national guidelines, teachers are not supposed to assign a grade but should instead report a grade of Z when students fail to complete exams or assignments required to judge their mastery of the subject material. Nevertheless, in the Komvux registry, grades are missing for around 15% of observations where one would expect grades to be reported. It is unclear whether these should have been recorded as grades of Z or whether they are truly missing values. In my main analysis, I take a restrictive approach and only include classes where all students registered at the end of the course have non-missing grades. As a first robustness check of the main results, *Figure A.10* plots the difference-in-differences estimates from equation 2 when dropping classes with different shares of missing grades from the estimation sample. The main point estimates are fairly stable, indicating that my findings are not driven by my sample restriction on classes with missing grades.

I perform several additional specification checks to support my main findings. *Table A.4* reports the results of these checks and includes the estimates from the main specification for comparison. The first four robustness checks test whether my findings are sensitive to the inclusion or exclusion of individual-level background characteristics. Although *Table 1* showed that there were no significant overall changes in the composition of students in higher- relative to lower-expansion regions during the study period, it is concerning that there are some slight imbalances in compulsory school GPA coinciding with the introduction of the reform. Reassuringly, however, panels (B) to (D) of *Table A.4* show that the main estimates for dropping a course and earning course credit are largely unchanged irrespective of whether compulsory school GPA, or any other background characteristics, are included as a control variables. There is also little change in the estimates when interacting compulsory school GPA with the year fixed effects to account for changes over time.

As an additional robustness check, I interact the set of pre-reform municipality characteristics listed in *Table A.2* with year fixed effects to allow for the possibility that there are different underlying trends in student outcomes related to baseline municipality characteristics. The main findings point in the same direction, although the effect sizes drop in magnitude by around a third, which suggests that municipal variables may be an important confounding factor. In order to understand which municipality characteristics drive this decrease, and also to test whether the effects keep decreasing as additional trends are added, I perform two exercises in *Figure 6*. First, I interact the year fixed effects with each one of the variables indicated on the vertical axis, and add these trends to the baseline estimation individually. Second, I use a step-wise procedure to sequentially add underlying trends related to each characteristic—one additional characteristic at a time, moving down the vertical axis—until trends for all characteristics are included in the same model. The bottom point in the graph thus corresponds to the estimate in panel (F), column (1) of *Table A.4*. This exercise reveals that the decrease in the baseline estimate is primarily driven by variables related to education and labor market outcomes. Reassuringly, however, the step-wise procedure shows that after controlling for different trends related to average educational attainment, the estimates remain relatively stable as more underlying trends are added.

Next, I test whether the results are sensitive to the definition of the treatment variable. In panel (G), I use a continuous measure of the enrollment shock in each municipality instead of a binary indicator. When evaluated at the average treatment intensity of 2.1, the point estimates from this specification are nearly identical in magnitude to the main point estimates: 0.0369 and -0.0369 for course dropout and earning credits, respectively, compared to baseline estimates of 0.0371 and -0.0342. In panel (H), I again use a binary treatment indicator that divides the municipalities into groups based on above-median and below-median enrollment shocks; however, instead of measuring enrollment shocks over the full study period, I take the difference between the enrollment level in the peak post-reform school year (1998/99) and the base organization period (1993/94–1996/97) to better capture the immediate shock of the reform. There is a slight reduction in the magnitude of the effects, though they are still sizable and statistically significant at the 1% level. In panel (I), I check whether selective re-location is a problem by assigning individuals to treatment based on their municipality of residence at the start of the sample

Figure 6: Robustness of the point estimates for course dropout to underlying trends in municipality characteristics.



Notes: The dashed vertical line indicates the baseline estimate for course dropout (0.0371). The point estimates marked O are obtained by adding each characteristic sequentially, i.e., the final row allows for differential trends related to all ten characteristics and corresponds to the estimate in panel (F), column (1) of *Table A.4*. Horizontal bars show 95% confidence intervals with standard errors clustered at the municipal level.

period (year 1993). Re-scaling the intent-to-treat estimates by the probability of still living in the same region, the estimates are quite similar to the main specification.

As a final robustness check, I restrict the sample to students' first course attempt in order to check whether the estimates are affected by repeated course-taking. There is no evidence that this is the case.

5.2 Alternative explanations

While the findings presented thus far provide suggestive evidence that shocks to school inputs have a causal effect on course dropout, there are several other plausible explanations to consider. First, the reform may change the type of courses that a student enrolls in or the aggregate course load that they register for. In my main results for course outcomes, I include subject fixed effects to account for the fact that some courses may be more difficult than others and to help alleviate concerns related to changes in course selection. Additionally, I include subject-by-year fixed effects to account for time-varying subject-specific shocks. However, as a robustness check, I also formally test for changes in course composition by using various course characteristics as the dependent variable in my main differences-in-differences model.

Table 4 reports the results of the difference-in-differences analysis when using various course characteristics as the outcome, and *Figure A.11* plots the dynamics over time. The results indicate that there is a slight shift out of academic courses in the higher-expansion regions compared to the lower-expansion regions towards the end of the reform period. However, for all other characteristics, there are no meaningful changes, and the dynamics do not mirror the effects on student outcomes. Overall, this suggests that changes in course composition are unlikely to drive my findings.[18]

Another possibility is that students drop out of their courses to take advantage of improving labor market prospects, especially as competition for job openings may be reduced by the increased flow of unemployed 25–55-year-olds into adult education. To test

Table 4: Changes in course characteristics.

Panel A. Course characteristics.	Estimate	Std. Err.	P-value
Daytime course	-0.005	0.010	0.590
Course duration (in weeks)	0.426	0.599	0.477
Lecture hours per week	-0.159	0.160	0.322
Compulsory-level course	-0.002	0.005	0.734
Academic course	-0.047	0.014	0.001
Number of observations		3,240,448	

Panel B. Overall course load.	Estimate	Std. Err.	P-value
Total number of registered courses	0.112	0.146	0.444
Total lecture hours per week	-0.037	1.144	0.974
Number of observations		679,554	

Notes: In panel (A), each individual appears once per course attempt. In panel (B), each individual appears once per school year. The estimate column reports the difference-in-differences estimate when using course characteristics as the outcome. Standard errors are clustered at the municipal level.

[18] As an additional check, I have verified that controlling for course characteristics (e.g., duration) in the main difference-in-differences model does not affect the main estimates.

whether this is the case, I estimate my main difference-in-differences model for several labor market outcomes in the year after course start, including days unemployed, annual labor earnings, and employment status in November. I include one observation per person and school year in these estimations. The main estimates are reported in columns (1) to (3) of *Table 5*. There is no indication that students drop out of their courses in order to work—in fact, days registered as unemployed increase slightly, although this may be partly driven by negative pre-trends at the start of the study period (see *Figure A.12*).

Finally, it may be the case that students drop their courses because they were accepted into college. However, the estimates in column (4) of *Table 5* and panel (d) of *Figure A.12* show that there is actually a slight reduction in the likelihood of being registered for higher education, particularly at the end of the study period. This indicates that course dropout may have longer-term consequences for students' educational attainment.

Table 5: Effect on study and work situation one year later.

	Days unemployed (1)	Labor earnings (2)	Employed in November (3)	Enrolled in college (4)
Diff-in-Diff estimate	6.679	-0.353	0.010	-0.011
	(3.585)*	(0.966)	(0.007)	(0.005)**
Pre-reform averages:				
Higher-expansion areas	156.864	35.830	0.357	0.145
Lower-expansion areas	133.935	42.245	0.418	0.162
Post-reform averages:				
Higher-expansion areas	115.798	47.113	0.463	0.181
Lower-expansion areas	87.102	53.127	0.511	0.205
Number of observations	676,626	676,626	676,626	676,626

Notes: Each individual appears once per school year that they are registered in Komvux. All outcomes are measured during the following calendar year. Days unemployed refers to total days that the individual was registered with the Public Employment Service during the year. Annual labor earnings is reported in thousands of Swedish crowns (CPI-adjusted to year 1996). Employment status and college enrollment status are measured during the fall. Standard errors are shown in parentheses and clustered at the municipal level. Stars denote significance levels: *** for $p < 0.01$; ** for $p < 0.05$; * for $p < 0.10$.

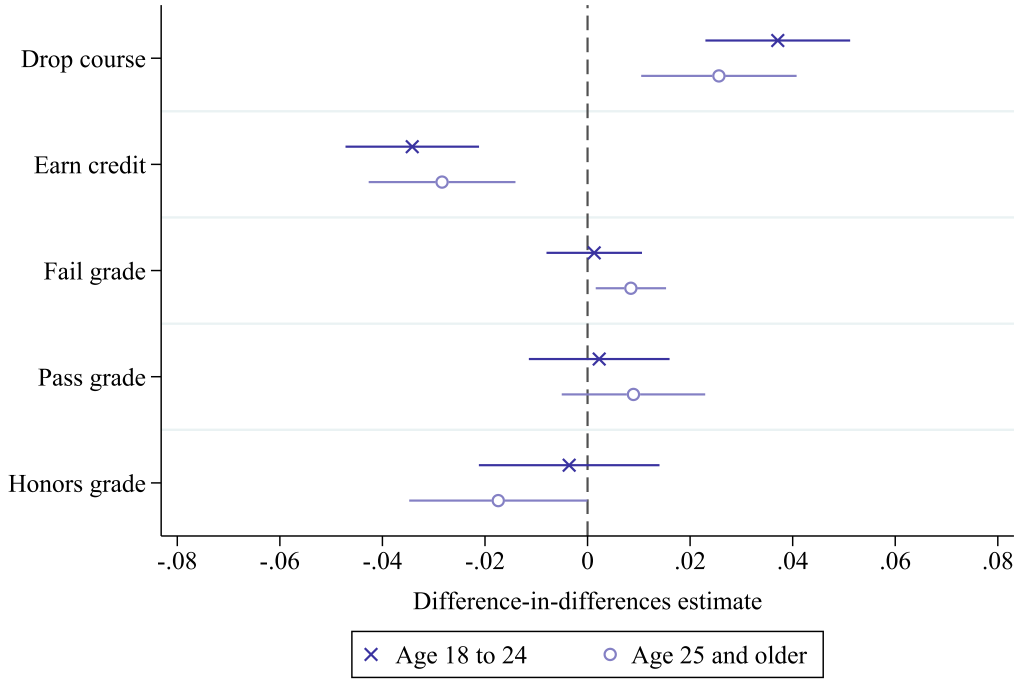
5.3 External validity

My main analysis focuses on young adults, age 18–24, for two reasons. First, individuals under age 25 were not the target population of the AEI reform, and thus, it is likely that negative selection and compositional changes are less severe among this age group. Second, I have better data coverage for younger individuals (e.g., data on prior academic achievement), and thus, I can test and control for important compositional changes over the study period.

Young adults are a particularly relevant subgroup to study in the Swedish context. Over a third of a birth cohort enrolls in municipal adult education before age 25 (see *Figure A.1*), and students under age 25 account for between 25–30% of course enrollment. Nevertheless, I acknowledge that younger students are relatively rarer than the modal student in adult education, and they may differ from older students in several important characteristics that affect the generalizability of my findings. Based on observable characteristics, there are some notable differences between younger and older students. For example, younger students are more likely to be native born, less likely to work while enrolled, and have completed more years of schooling despite their younger age (see *Table A.5*). They are also more likely to enroll in high-school level courses and in academic subjects. Given these differences, it is relevant to consider the extent to which my findings for younger students apply to older individuals, who likely also have different motivations for enrolling in adult education (e.g., re-training for a new profession as opposed to studying courses needed to apply to college).

To shed light on this question, I repeat my difference-in-differences analysis for the sample of students age 25 and older. The resulting estimates are shown in *Figure 7*. The effects on course dropout are somewhat weaker among older students, although not statistically different from the effects for students age 18–24. In contrast to the results for younger students, there is also some evidence of negative impacts on older students' grades conditional on course completion, i.e., slightly increased probability to fail and decreased probability to earn an honors grade. Once again, however, the point estimates are statistically indistinguishable from the point estimates for younger students. Although

Figure 7: Comparison of point estimates for younger and older students.



Notes: Students appear in the estimation sample once per registered course attempt. All regressions control for age, sex, civil status, presence of young children in the household, foreign background, parents' highest level of education, and own level of education.

I cannot rule out that negative selection drives the results for older students, these findings suggest that older adult learners are also negatively impacted by school and peer input shocks.[19]

6 Concluding remarks

One of the most enduring and contentious debates in education research is whether school inputs have an impact on student outcomes. A vast literature examines this question for primary and secondary school students, but we know less about how school inputs affect the outcomes of students beyond high school age. This is an important omission given that a notable percentage of adults are enrolled in formal or informal education in most OECD countries (OECD, 2017).

[19] In *Figure A.13*, I support the credibility of these findings by showing that the results are not very sensitive to the inclusion of ability controls among the sub-sample of men for whom I have military enlistment test data. However, similar to the gender pattern observed for younger students, the point estimates for men seem slightly weaker than average.

In this paper, I contribute to the literature with the first causal evidence on the relationship between school inputs and the academic outcomes of adult education students outside the higher education system. I show that plausibly exogenous shocks to peer inputs and school resources such as average teacher qualifications and per-pupil expenditure coincide with increases in the probability of course dropout, though conditional upon course completion, there is no effect on course grades.

My findings suggest that policies that expand access to education without an adjustment of school resources may have negative consequences. This is a particularly relevant finding in the context of adult education, as policymakers have begun to embrace the concept of lifelong learning as a way to meet the changing demands of the labor market, and enrollment in adult education is on the rise. Since the enrollment shocks that I exploit affect multiple school inputs simultaneously, it is difficult to determine whether one particular input or a certain combination of inputs matters most. Future research could attempt to disentangle the mechanisms and to evaluate whether the short-term effects on course dropout have adverse consequences for students in the long run, for example, decreased educational attainment or worse labor market outcomes.

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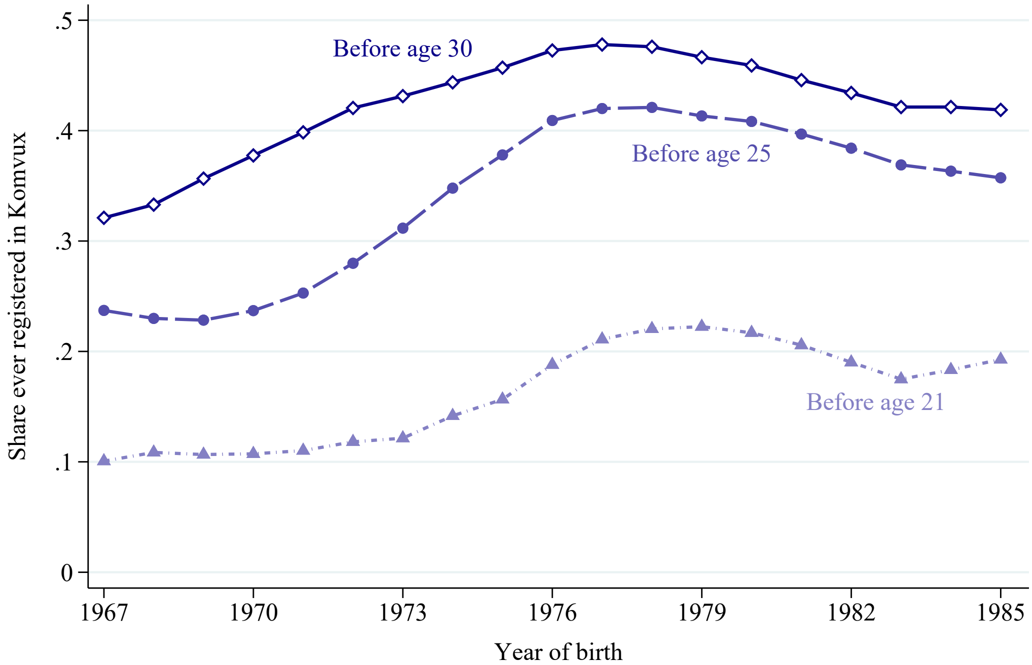
Main appendix

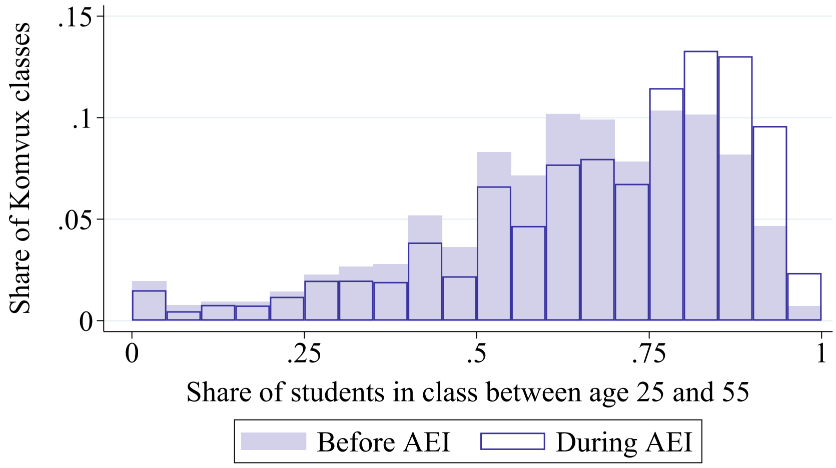
Figures

Figure A.1: Participation in Komvux by age and birth cohort.



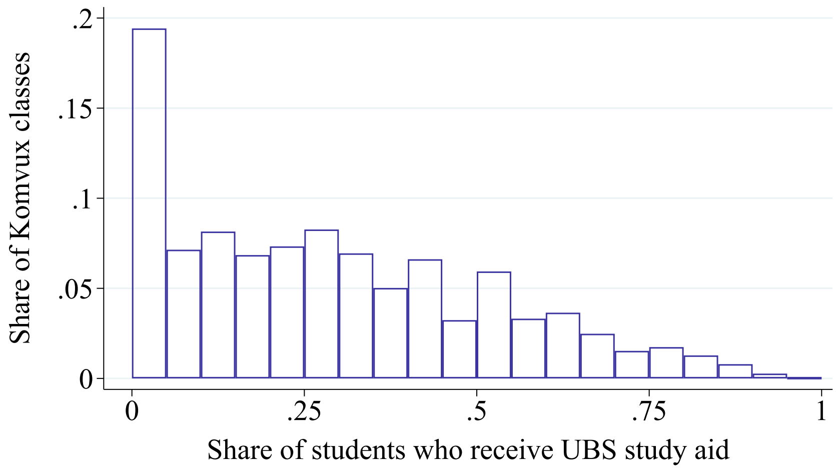
Notes: Each line plots the share of a birth cohort that has ever registered for a course in municipal adult education (Komvux) before a certain age. The bottom/middle/top lines indicate registration before age 21/25/30, respectively. Calculations are based on all individuals in a birth cohort who resided in Sweden at the end of the calendar year they turned 18, regardless of where they resided in previous or subsequent years.

Figure A.2: Age composition of Komvux classes with at least one student under age 25.



Notes: This figure shows how the age composition of Komvux classes changed during the AEI. The sample includes classes with at least four students overall and at least one student under age 25. The before-AEI period spans fall 1993 to spring 1996 and the during-AEI period spans fall 1997 to fall 2002.

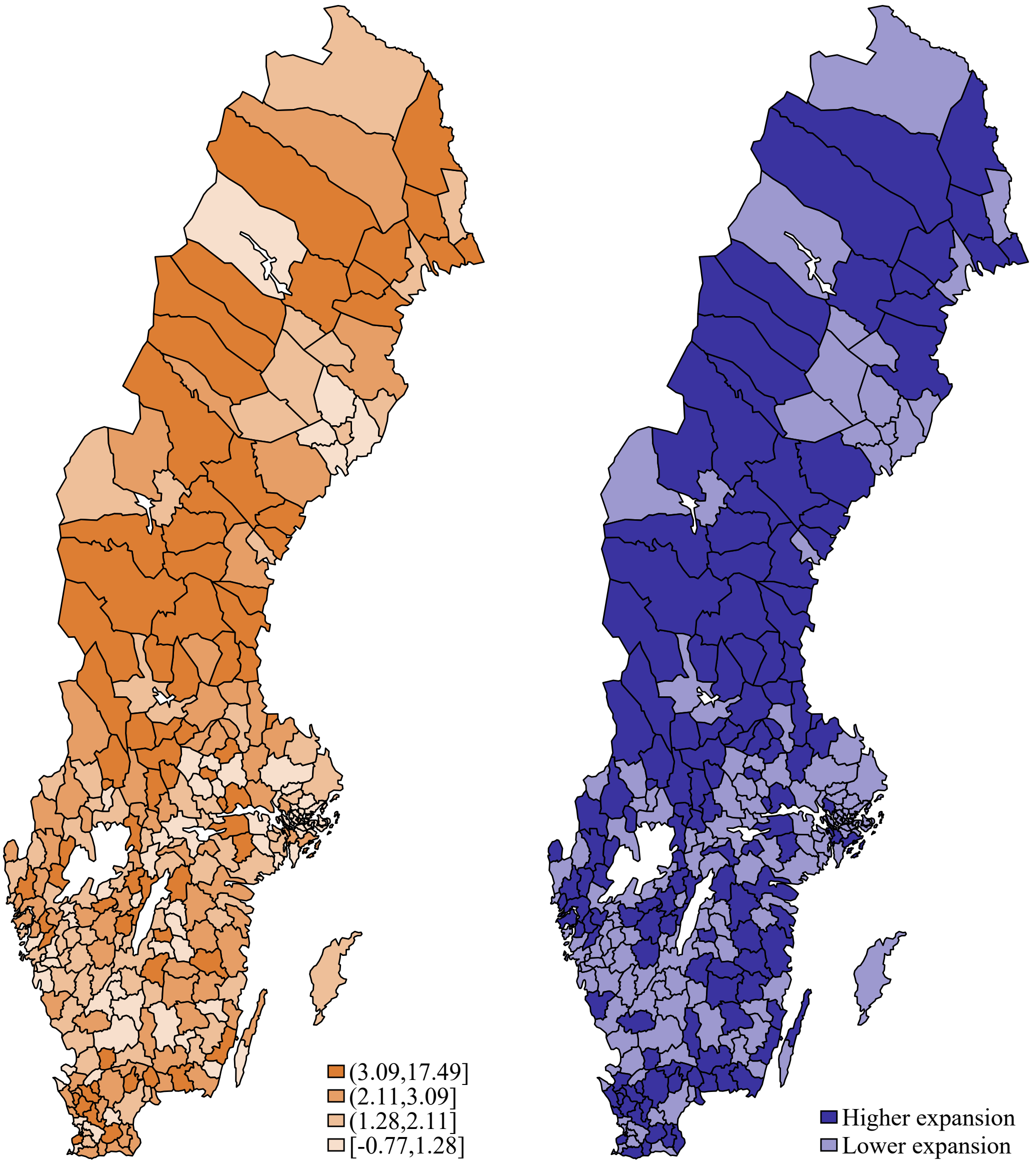
Figure A.3: Share of UBS recipients in classes with at least one student below age 25.



Notes: This figure shows the share of students per class who receive UBS (a study grant introduced in 1997 to encourage enrollment among the AEI target population). The sample includes all classes from fall 1997 to fall 2002 with at least four students overall and at least one student under age 25.

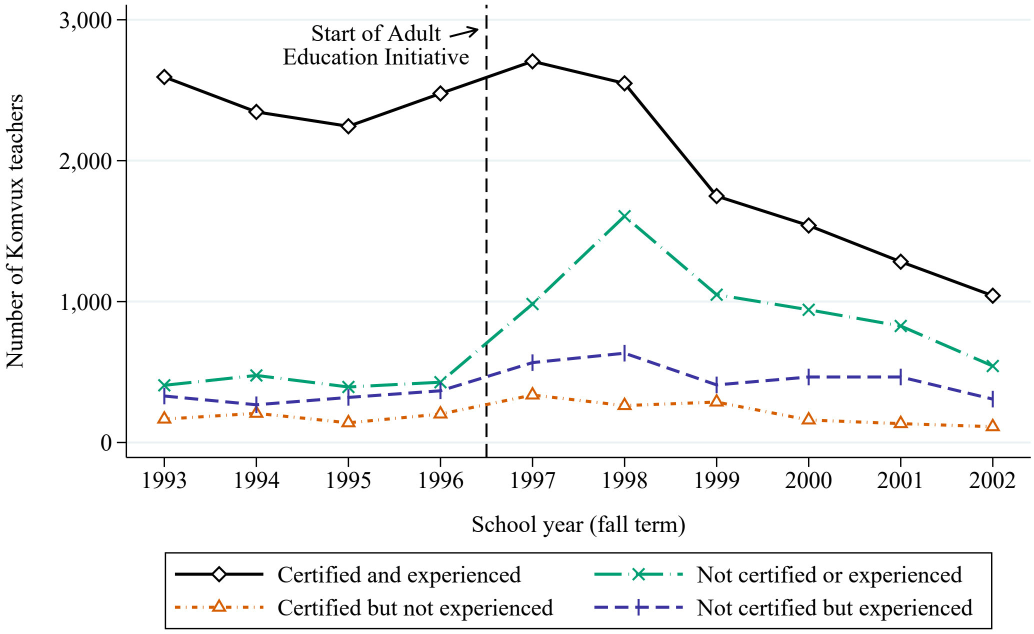
Figure A.4: Variation in the intensity of enrollment shocks across municipalities.

(a) Intensity of enrollment shock **(b)** Binary categorization



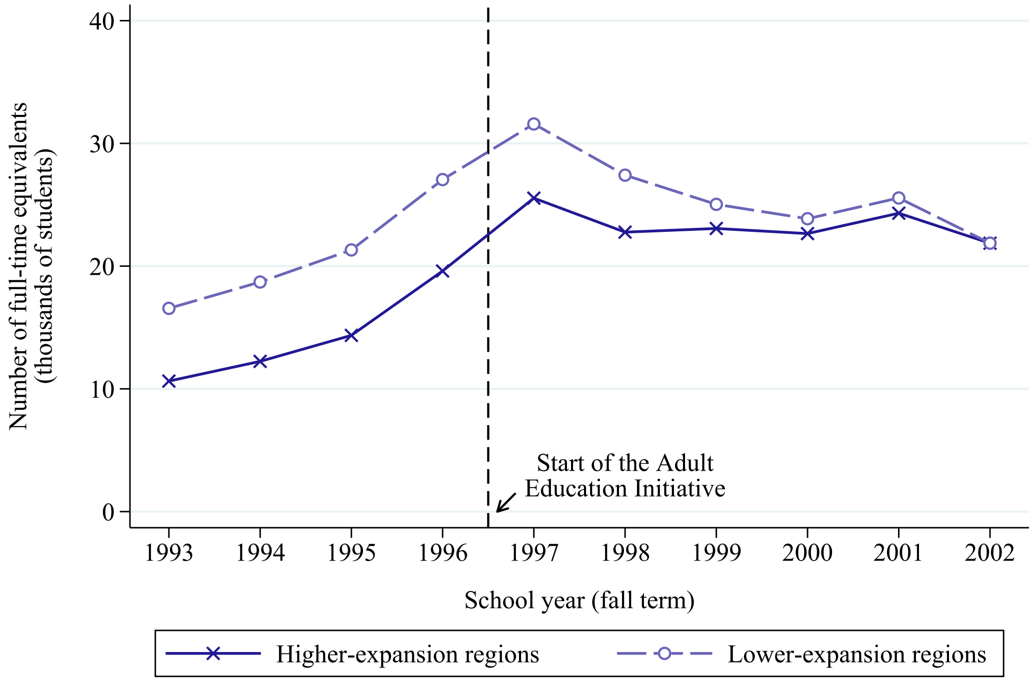
Notes: In panel (a), municipalities are shaded according to the intensity of enrollment shock that they experienced between 1993 and 2002. Each shade represents a different quartile in the distribution of enrollment shocks, with the lowest quartile/weakest shocks represented by the lightest shade and the highest quartile/strongest shocks represent by the darkest shade. In panel (b), higher-expansion areas are defined as municipalities that experienced above-median enrollment shocks (illustrated with darker shading), and lower-expansion areas are defined as municipalities that experienced below-median enrollment shocks (illustrated with lighter shading).

Figure A.5: Inflow of Komvux teachers by prior teaching experience and certification status.



Notes: This figure shows the number of teachers who taught in municipal adult education (Komvux) during the fall term of school year y but not the previous year $y-1$. Each line categorizes teachers according to their prior teaching experience and the type of degree that they have. Experience refers to any teaching experience since 1985, whether in adult education or another level. Certified refers to teachers who have a college degree in pedagogy.

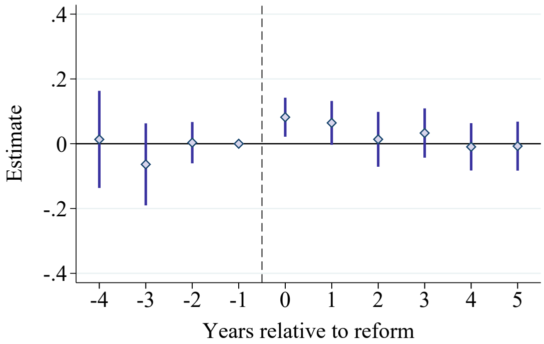
Figure A.6: Enrollment levels in municipal adult education (18- to 24-year olds).



Notes: This figure shows the number of full-time equivalent students age 18 to 24 registered in municipal adult education during a given school year. The darker line plots the enrollment levels in higher-expansion regions, and the lighter line plots the enrollment levels in lower-expansion regions.

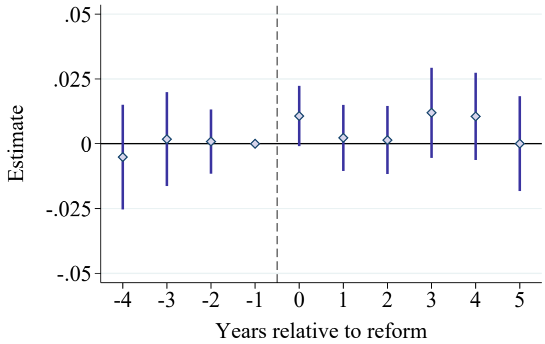
Figure A.7: Dynamic difference-in-differences estimates for student characteristics.

(a) Age



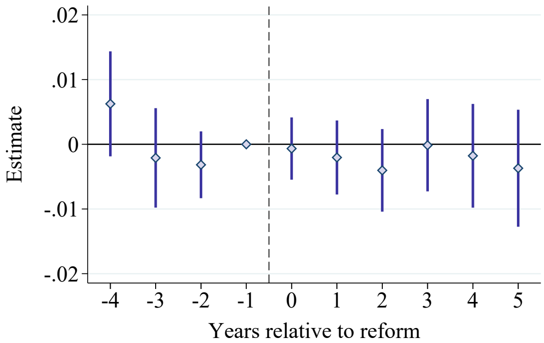
Baseline mean: 21.335

(b) Female



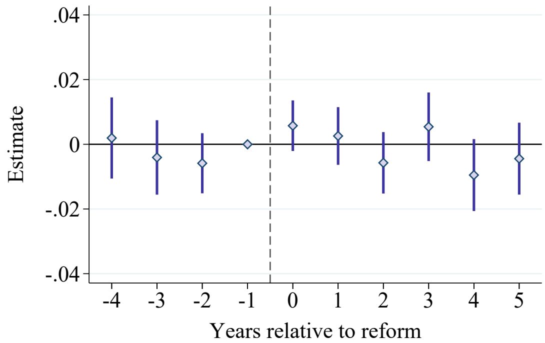
Baseline mean: 0.588

(c) Married



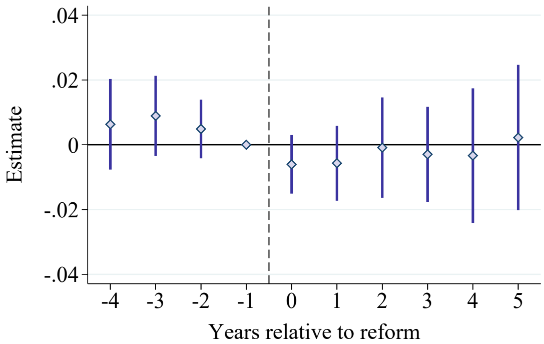
Baseline mean: 0.040

(d) Lives with young children



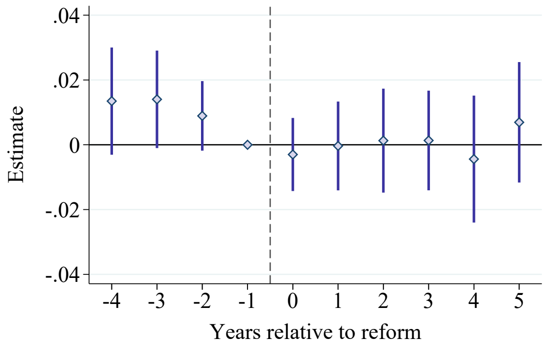
Baseline mean: 0.099

(e) Born in Sweden



Baseline mean: 0.871

(f) Swedish-born parent

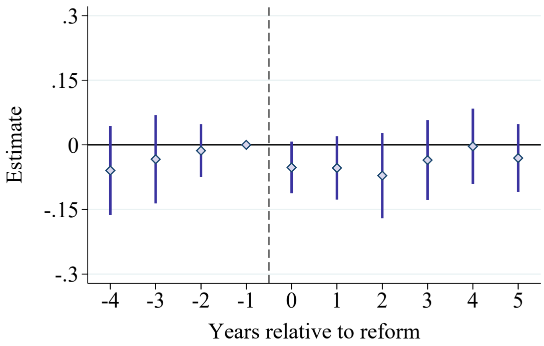


Baseline mean: 0.829

Notes: Figure continues on the next page. The dashed vertical line depicts the start of the AEI in 1997. Individuals appear once per registered course attempt. The vertical bars represent 95% confidence intervals with standard errors clustered at the municipal level.

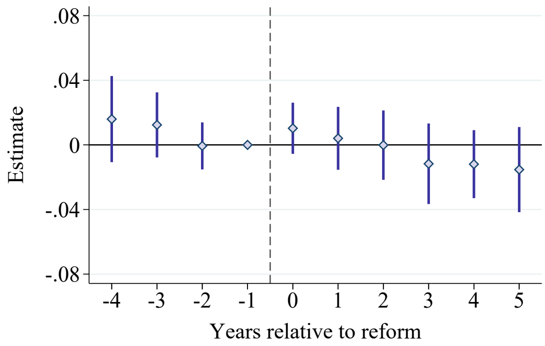
Figure A.7 (continued): Dynamic difference-in-differences estimates for student characteristics.

(g) Parents' years of education



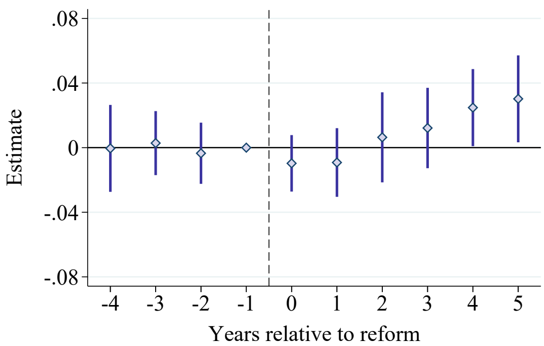
Baseline mean: 11.709

(h) No high school degree



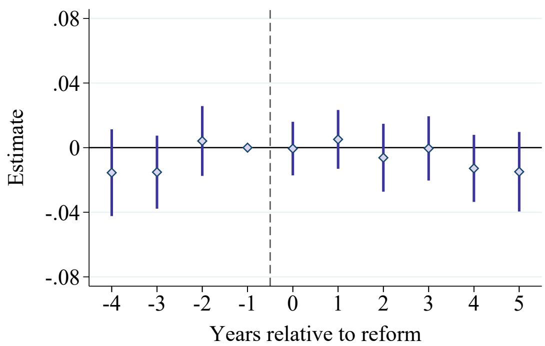
Baseline mean: 0.257

(i) High school degree from academic track



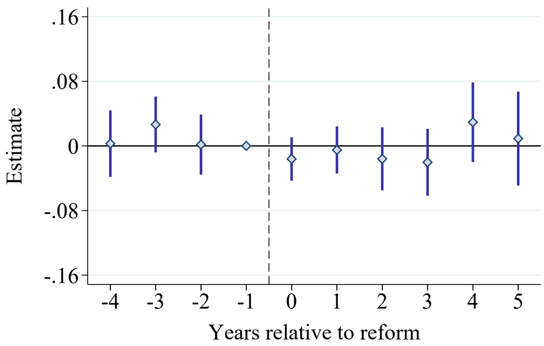
Baseline mean: 0.341

(j) High school degree from vocational track



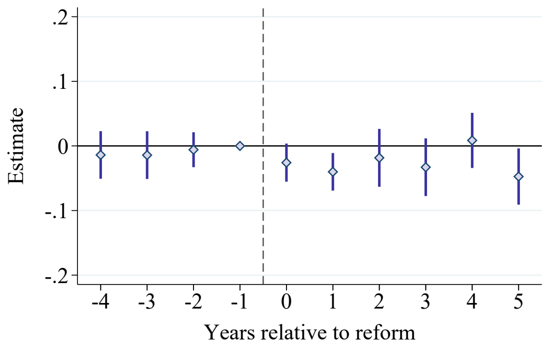
Baseline mean: 0.402

(k) High school GPA



Baseline mean: -0.281

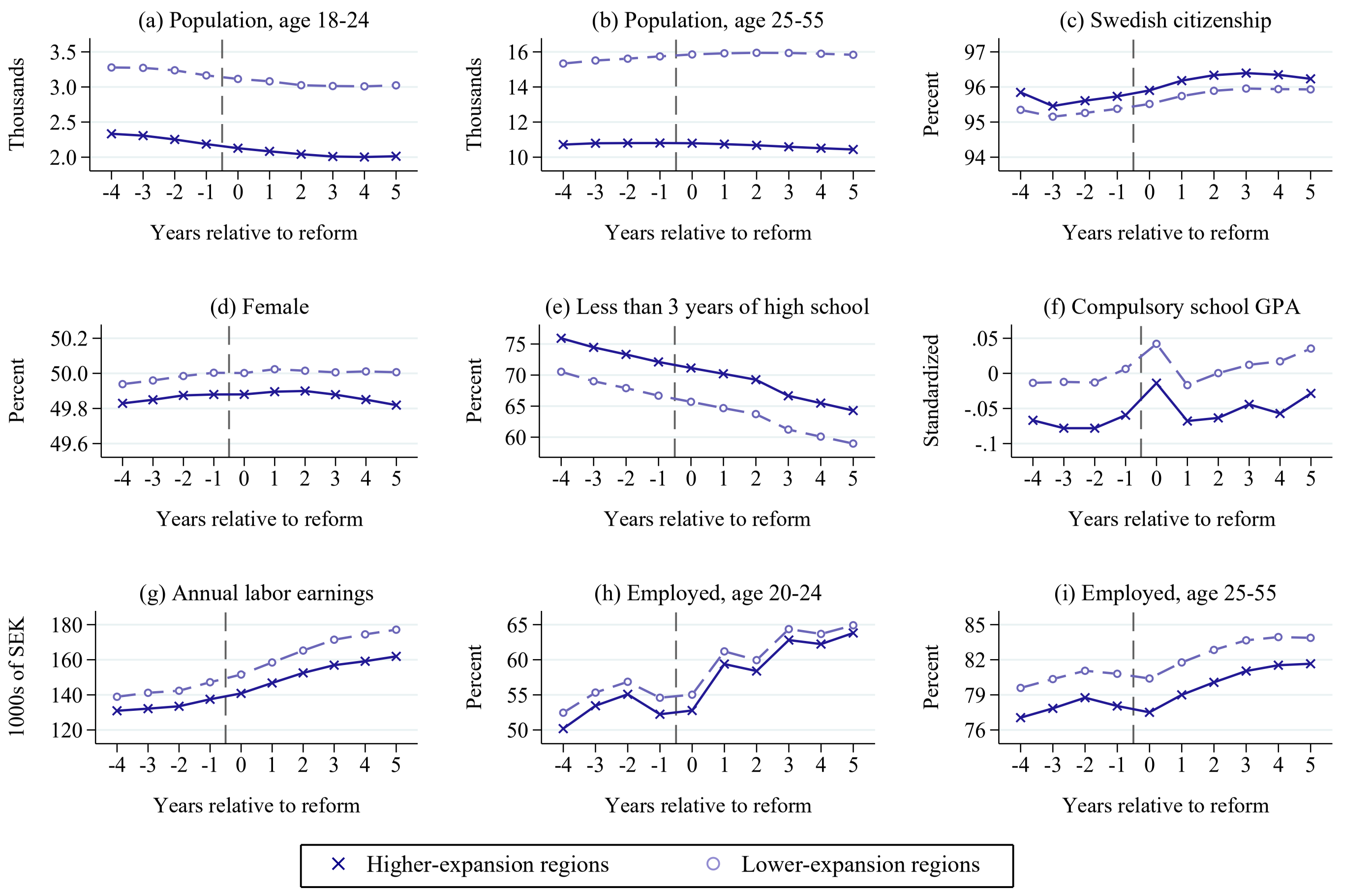
(l) Compulsory school GPA



Baseline mean: -0.197

Notes: The dashed vertical line depicts the start of the AEI in 1997. Individuals appear once for every course that they take. The vertical bars represent 95% confidence intervals with standard errors clustered at the municipal level.

Figure A.8: Population characteristics over time in higher-expansion and lower-expansion regions.

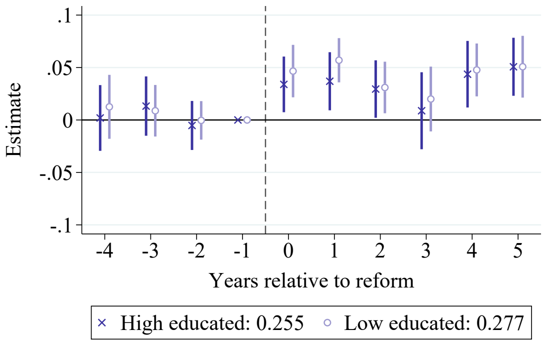


Notes: This figure shows average population characteristics over time in higher-expansion and lower-expansion regions. Each municipality receives equal weight in the average, regardless of population size. The vertical line indicates the introduction of the AEI in 1997. All data comes from Statistics Sweden and the National Agency for Education. In panel (e), there was an improvement in data quality for highest education from time $t = 3$ (year 2000) onward. Panel (f) reports the compulsory school GPA of the current graduation class. There was a change in grading system at $t = 1$ (year 1998).

Figure A.9: Heterogeneity analysis by student characteristics.

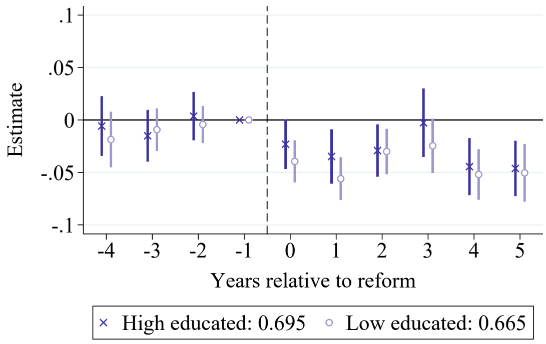
Panel A: Parents' level of education

Drop the course



Baseline mean shown in legend.

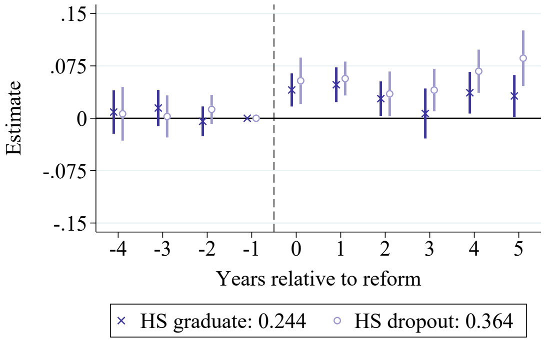
Earn course credits



Baseline mean shown in legend.

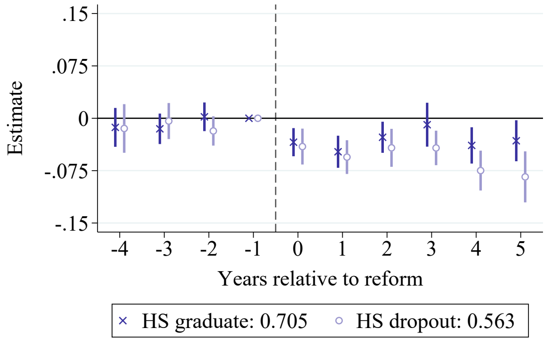
Panel B: High school graduation status

Drop the course



Baseline mean shown in legend.

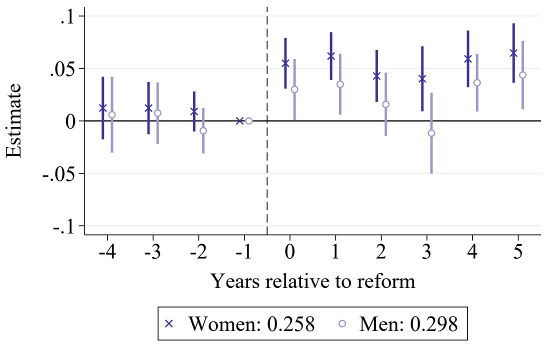
Earn course credits



Baseline mean shown in legend.

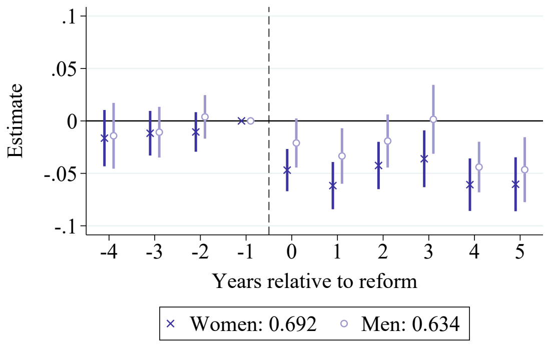
Panel C: Students' gender

Drop the course



Baseline mean shown in legend.

Earn course credits



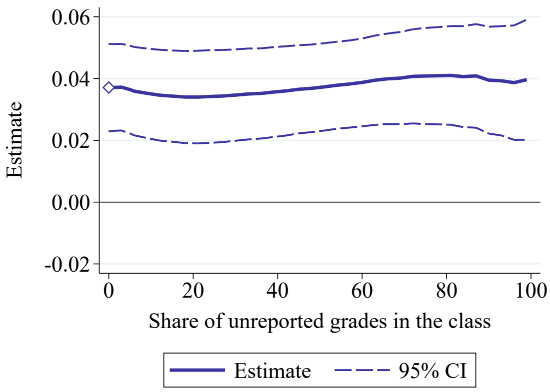
Baseline mean shown in legend.

Notes: The dashed vertical line corresponds to the introduction of the AEI. Vertical bars represent 95% confidence intervals with standard errors clustered at the municipal level. In panel (A), a student is defined as having a high-educated parents if either parent has at least one year of post-secondary education.

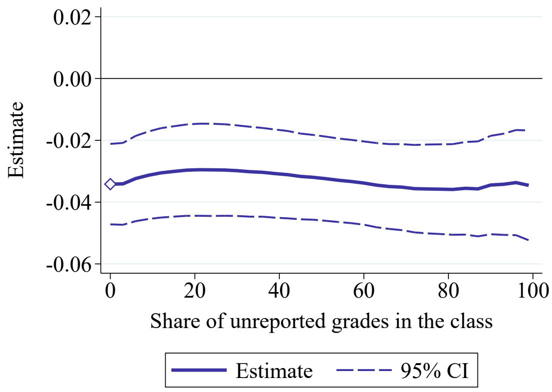
Figure A.10: Sensitivity of the main estimates to inclusion of courses with different shares of unreported grades.

Panel A: All students, 1993–2002

Drop course

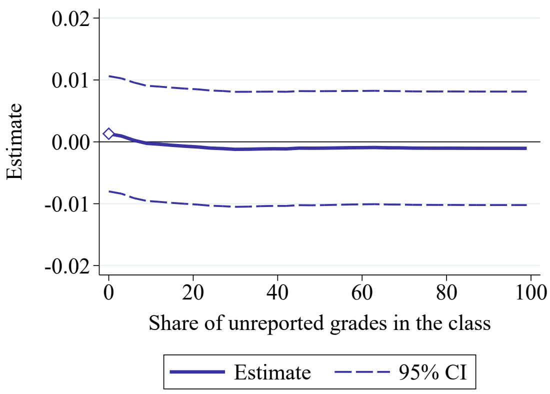


Earn credits

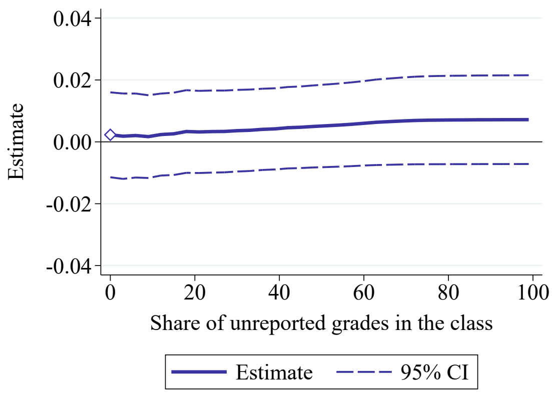


Panel B: Course completers, 1994–2002

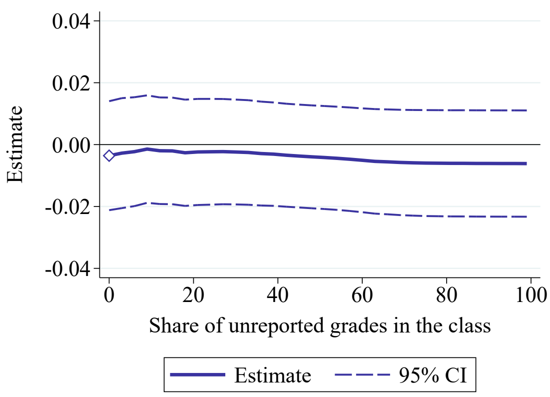
Fail grade



Pass grade



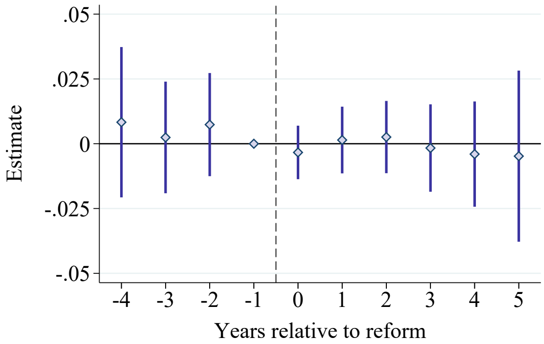
Honors grade



Notes: These figures show how the main estimates change when I relax the sample restriction in which I drop all courses with unreported grades. The leftmost point marked with a diamond corresponds to the main estimate, and the further to the right along the horizontal axis, the higher the share of unreported course grades permitted for an observation to be included in the estimation. If someone has a missing grade, I treat them as a dropout in panel (A).

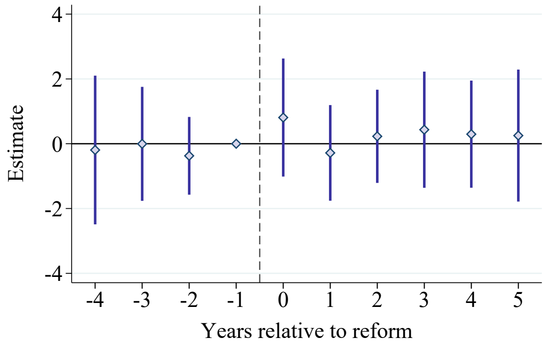
Figure A.11: Dynamic difference-in-differences estimates for course characteristics.

(a) Daytime course



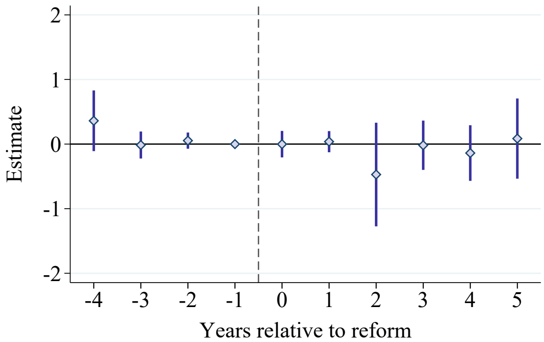
Baseline mean: 0.915

(b) Course duration (weeks)



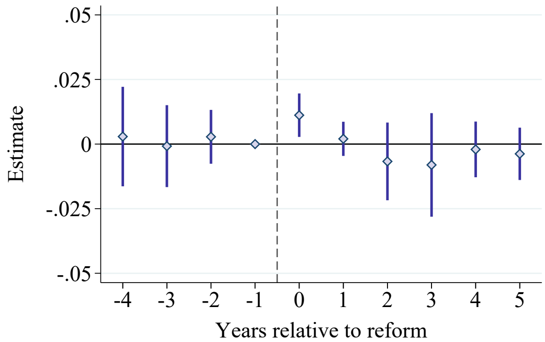
Baseline mean: 27.462

(c) Lecture hours per week



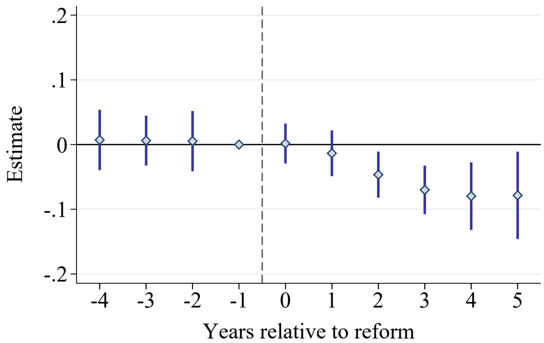
Baseline mean: 2.194

(d) Compulsory level



Baseline mean: 0.055

(e) Academic

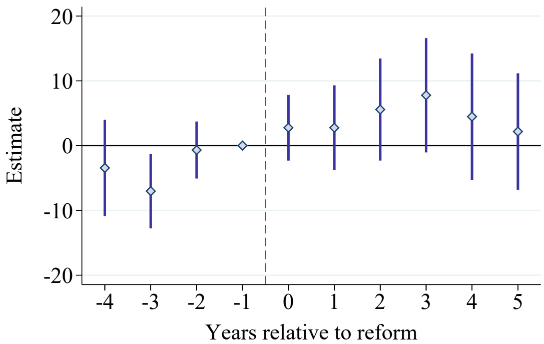


Baseline mean: 0.750

Notes: The vertical line indicates the introduction of the AEI. Vertical bars represent 95% confidence intervals with standard errors clustered at the municipal level. Individuals appear once for every course that they take.

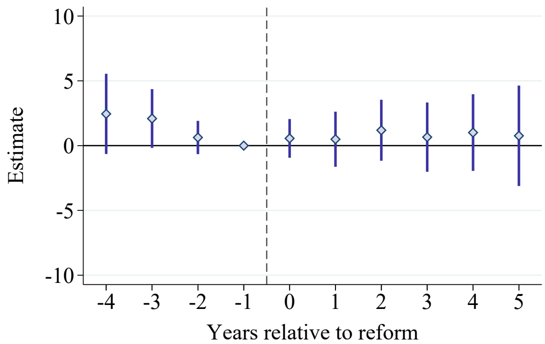
Figure A.12: Dynamic difference-in-differences estimates for work and study situation one year after course start.

(a) Days unemployed during the year



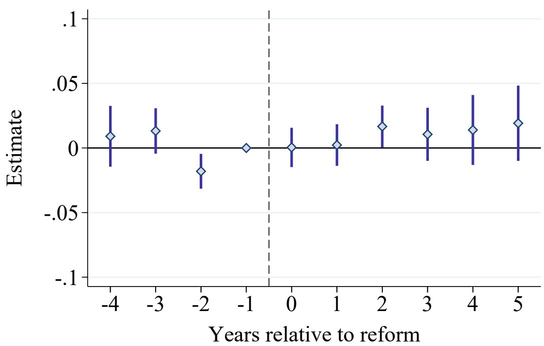
Baseline mean: 153.201

(b) Annual labor earnings



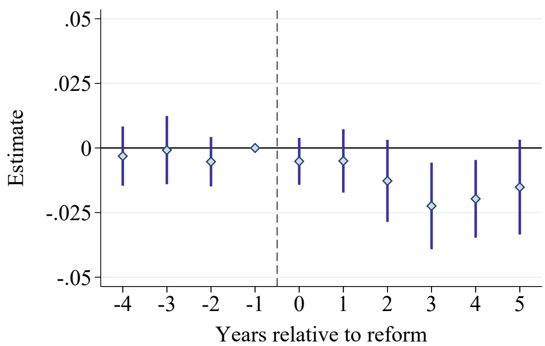
Baseline mean: 35.403

(c) Employed in November



Baseline mean: 0.355

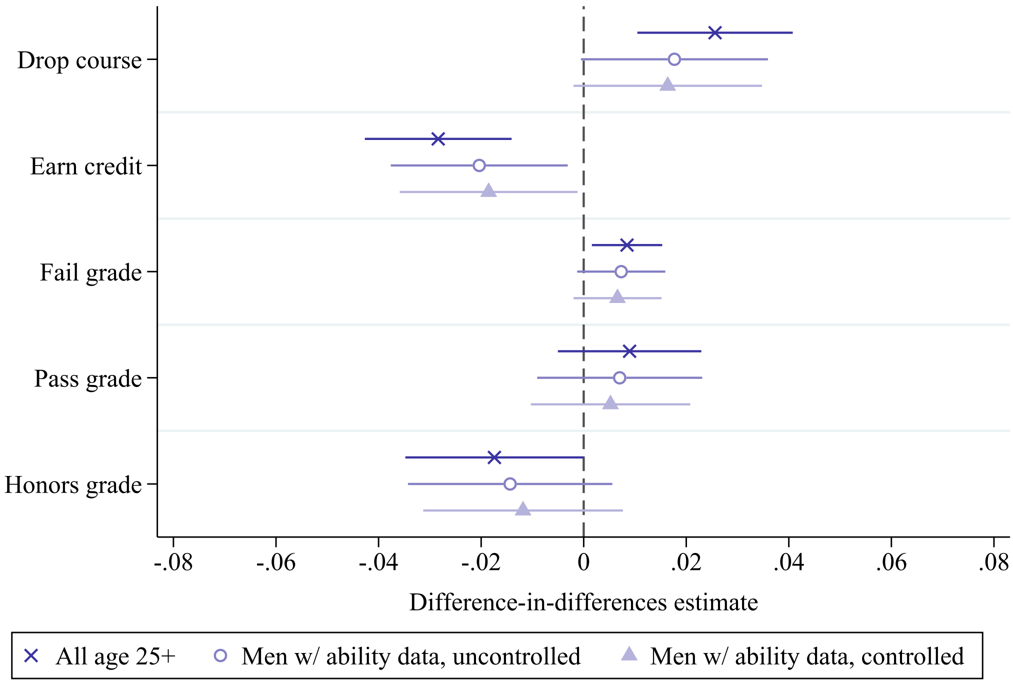
(d) Enrolled in college in fall



Baseline mean: 0.154

Notes: The vertical line indicates the introduction of the AEI. Individuals appear once for every school year that they are registered for Komvux.

Figure A.13: Comparison of estimates for older students with and without ability controls.



Notes: Students appear in the estimation sample once per registered course attempt. All regressions control for age, sex, civil status, presence of young children in the household, foreign background, parents' highest level of education, and own level of education. Ability data refers to cognitive and non-cognitive scores on Sweden's military enlistment test. Horizontal bars show 95% confidence intervals with standard errors clustered at the municipal level.

Tables

Table A.1: Descriptive statistics for the estimation sample.

	Pre-reform		Post-reform	
	Higher expansion	Lower expansion	Higher expansion	Lower expansion
Age	21.225 (1.739)	21.337 (1.705)	21.271 (1.627)	21.365 (1.611)
Female	0.587	0.595	0.587	0.580
Married	0.042	0.040	0.038	0.038
Lives with children under age six	0.104	0.090	0.096	0.075
Born in Sweden	0.869	0.848	0.850	0.828
Swedish-born mother	0.792	0.764	0.766	0.725
Swedish-born father	0.784	0.751	0.759	0.712
Mother's years of schooling	10.750 (2.445)	11.145 (2.575)	10.867 (2.348)	11.291 (2.526)
Father's years of schooling	10.621 (2.722)	11.082 (2.915)	10.815 (2.600)	11.309 (2.847)
Not a graduate of high school	0.291	0.294	0.337	0.331
Graduate of academic track	0.325	0.362	0.292	0.349
Graduate of vocational track	0.384	0.344	0.371	0.320
High school GPA	-0.261 (0.873)	-0.211 (0.881)	-0.407 (0.867)	-0.335 (0.875)
Compulsory school GPA	-0.186 (0.836)	-0.123 (0.826)	-0.273 (0.891)	-0.152 (0.859)
Missing compulsory school GPA	0.088	0.107	0.070	0.079
Number of observations	444,636	655,369	918,213	1,222,230

Notes: Students appear in the sample once per registered course attempt. Pre-reform data covers years 1993–1996, and post-reform data covers years 1997–2002. Higher-expansion regions are those that experienced above-median enrollment shocks during the AEI, and lower-expansion regions are those that experienced below-median enrollment shocks. Standard deviations are shown in parentheses for continuous variables; all other characteristics are binary indicators. Grade point averages (GPAs) are standardized. High school GPA is missing for everyone who is not a high school graduate.

Table A.2: Characteristics of higher-expansion and lower-expansion municipalities.

	Higher expansion	Lower expansion	Difference	P-value
A. Demographics and other characteristics				
Log population density (residents per square kilometer)	2.958	3.746	-0.788	0.000
Percent female	49.880	50.003	-0.124	0.200
Percent Swedish citizens	95.733	95.378	0.355	0.284
Percent in workforce, age 16–64	67.058	70.184	-3.126	0.000
Percent young adults in workforce, age 20–24	52.238	54.593	-2.356	0.006
Percent adults in workforce, age 25–55	78.050	80.804	-2.754	0.000
Annual labor earnings (thousands of SEK)	137.489	147.203	-9.714	0.000
Share with less than three years of high school education	72.119	66.691	5.429	0.000
Average compulsory school GPA (standardized)	-0.060	0.006	-0.066	0.000
Percent left-wing seats in municipal council	58.682	51.935	6.746	0.000
B. Industry structure				
Percent of workforce employed in:				
Agriculture, forestry, and fishing	4.341	3.418	0.924	0.004
Mining and manufacturing	24.978	23.893	1.086	0.317
Construction	6.583	6.116	0.468	0.008
Wholesale and retail trade	10.246	11.803	-1.557	0.000
Accomodation and food services	2.392	2.539	-0.147	0.311
Transport and storage	6.051	6.284	-0.232	0.319
Financial intermediation, real estate, and business activities	7.650	9.826	-2.177	0.000
Public administration, education, health, and social work	33.343	31.733	1.610	0.006
Community and personal services	4.414	4.384	0.030	0.809
Number of observations	143	143	286	

Notes: All characteristics are measured in 1996, the year prior to the introduction of the AEI. Each municipality is weighted equally in the average, regardless of population size. The last column reports the p-value from a test of the null hypothesis that there is no significant difference in the average for the higher-expansion regions and the average for the lower-expansion regions.

Table A.3: Relationship between baseline municipality characteristics and expansion measure.

	Correlation	P-value
A. Demographics & other characteristics		
Log population density (residents per square kilometer)	-0.359	0.000
Percent female	-0.087	0.140
Percent Swedish citizens	0.135	0.022
Percent in workforce, age 16–64	-0.385	0.000
Percent young adults in workforce, age 20–24	-0.076	0.197
Percent adults in workforce, age 25–55	-0.364	0.000
Annual labor earnings (thousands of SEK)	-0.323	0.000
Share with less than three years of high school education	0.362	0.000
Average compulsory school GPA (standardized)	-0.158	0.008
Percent left-wing seats in municipal council	0.321	0.000
B. Percent of workforce employed in each industry		
Agriculture, forestry, and fishing	0.206	0.000
Mining and manufacturing	0.048	0.419
Construction	0.154	0.009
Wholesale and retail trade	-0.289	0.000
Accomodation and food services	0.057	0.335
Transport and storage	-0.062	0.298
Financial intermediation, real estate, and business activities	-0.298	0.000
Public administration, education, health, and social work	0.150	0.011
Community and personal services	0.119	0.045
Number of observations: 286 municipalities		

Notes: This table shows the bivariate correlation between each municipality characteristic listed in the first column and the continuous measurement of enrollment expansion defined in Equation (1). The last column reports the p-value from a test of the null hypothesis that there is no significant correlation between the given characteristic and the expansion measure.

Table A.4: Robustness checks for effects on student outcomes.

	Drop course (1)	Earn credit (2)
A. Main difference-in-differences specification	0.0371	-0.0342
	(0.0072)***	(0.0066)***
B. Excluding vector of individual control variables	0.0367	-0.0335
	(0.0073)***	(0.0071)***
C. Controlling for compulsory school GPA and dummy if missing	0.0341	-0.0300
	(0.0072)***	(0.0066)***
D. Controlling for compulsory school GPA and dropping if missing	0.0366	-0.0336
	(0.0074)***	(0.0068)***
E. Compulsory school GPA interacted with year fixed effects	0.0382	-0.0355
	(0.0075)***	(0.0070)***
F. Pre-reform municipal characteristics interacted with year fixed effects	0.0306	-0.0214
	(0.0105)***	(0.0096)***
G. Using a continuous measure of treatment intensity	0.0176	-0.0176
	(0.0022)***	(0.0018)***
H. Defining treatment by enrollment shock through 1998/99	0.0282	-0.0276
	(0.0076)***	(0.0070)***
I. Assigning treatment by municipality of residence in 1993	0.0260	-0.0240
	(0.0064)***	(0.0060)***
J. Dropping course repeaters from the sample	0.0354	-0.0323
	(0.0076)***	(0.0071)***

Notes: In panels (E) and (F), I interact the year fixed effects with compulsory school GPA and with the pre-reform municipality characteristics listed in *Table A.2*, respectively. In panel (G), the continuous measure used to measure treatment intensity ranges from -0.77 to 17.49, with a median of 2.11. Standard errors are clustered at the municipal level. Stars denote significance levels: *** for $p < 0.01$; ** for $p < 0.05$; * for $p < 0.10$.

Table A.5: Background characteristics of students by age group.

	Age 18–24	Age 25–29	Age 30–39	Age 40+
A. Individual characteristics				
Female	0.592	0.634	0.684	0.692
Born in Sweden	0.829	0.742	0.685	0.725
Married	0.049	0.231	0.450	0.589
Lives with children under age six	0.090	0.345	0.452	0.108
Employed in November	0.408	0.449	0.474	0.560
Less than three years of high school	0.387	0.609	0.656	0.622
B. Course characteristics				
Number of registered courses	5.352	5.222	5.064	4.030
Compulsory level	0.141	0.229	0.268	0.238
High school level	0.907	0.870	0.846	0.840
Supplementary training	0.073	0.048	0.041	0.034
Academic subject	0.897	0.845	0.790	0.632
Vocational subject	0.506	0.557	0.609	0.670
Daytime instruction	0.871	0.832	0.848	0.786

Notes: Data covers the full study period (1993–2002). Each student is counted once per school year that they are registered in Komvux, regardless of the number of courses that they register for. All characteristics except for number of registered courses are binary variables. Categories for course characteristics need not sum to one because students can be registered for multiple types of courses or subjects during the same school year.

Data appendix

Identifying Komvux teachers in the teacher register

Statistics Sweden maintains annual data on all teaching staff employed in the Swedish school system as of October 15th. The database is called the Register of Teaching Personnel (Teacher Register), or *Registret över pedagogisk personal (Lärarregistret)* in Swedish. In my study, I use this database to identify teachers who taught in Komvux in the autumn term over years 1993–2002.

During my period of study, several administrative changes affected the variables and series of codes that can be used to classify teachers by level of instruction. The most notable changes took effect in 1999. Before 1999, a set of six variables called STAD1–STAD6 kept track of the type of instruction that teachers provided at a certain school.[20] With this data structure, multiple teaching positions at the same school—for example, high school instruction and adult education instruction—could appear in the same row. From 1999 onward, this was no longer possible. A single variable called NIVAKOD replaced the set of STAD variables. Thus, if teachers engaged in multiple types of instruction at the same school, they now had to appear in the register multiple times, with one row per type of instruction provided.

Table C.1 summarizes the variables and codes that corresponded to Komvux instruction for each year of my study period. In a given year, I define someone as a Komvux teacher if any of the listed codes appear in any of the variables for instruction type.[21]

[20] Through 1994, the register data includes a code for the "rektorsområde" (principal's area) where a teacher works rather than the school. At the compulsory school level, these principal areas sometimes include multiple schools, for example, when the same principal is responsible for more than one school within a catchment area. At the high school and adult education level, the code for principal area is sufficient to identify a school and link to the student registers.

[21] In contrast, some previous research has relied on the school form variable (SKOLFORM) to determine teachers' level of instruction. However, this method fails to capture the full set of Komvux teachers prior to 1999. The issue arises because SKOLFORM used to be measured by principal area rather than by school for all school forms except compulsory school. Because many principals organized high school and adult education in conjunction with one another, these two different school forms often existed within the same principal's area. In this case, SKOLFORM was always recorded as high school, which meant that adult education teachers in the principal's area would wrongly appear as high school teachers if SKOLFORM was used to classify teachers' instruction level. This changed when the teacher register underwent significant administrative revisions in 1999; since then, different school forms in the same principal's area always receive a unique code and classification. (*Source:* E-mail communication with Statistics Sweden, October 2018.)

Teachers who are currently on leave or do not perform any pedagogical duties are excluded from my analysis.

Table C.1: Variables and codes to identify Komvux teachers.

Year(s)	Variable(s)	Codes
1993	STAD1–STAD6	20, 21, 22, 23
1994	STAD1–STAD6	40, 41, 42, 43, 44, 45
1995	STAD1–STAD6	34, 35, 36, 37, 38
1996–1997	STAD1–STAD6	25, 26, 27, 28, 29
1998	STAD1–STAD6	11, 12
1999–2002	NIVAKOD	11, 12

Notes: Information comes from Statistics Sweden's documentation of the Teacher Register for variables labeled "tjänstgöringsnivå" (level of service). For year 1993, the documentation also lists code 24 as Komvux instruction, but these teachers do not have pedagogical duties in the traditional Komvux system and are thus excluded from my definition of Komvux teachers.

Cleaning the Komvux register

In the Komvux register, enrollment history and course transcripts are reported at the end of each academic term. Grades are left blank for ongoing courses, for courses in which no grades are assigned (e.g., introductory courses), and for students who de-register from the course. If students do not officially de-register but fail to submit the assignments required for a final grade, teachers are supposed to record a grade of Z (*betyg underlag saknas*) on their transcript. However, in some cases, it appears that teachers have forgotten to report grades—either for the entire class or for specific individuals. On average, around 15% of the grades are missing for courses where it appears that final grades should have been recorded based on course end date and student registration status. Often, grades are missing for everyone in the class, but there are also classes with only partial reporting. In my main analysis, I take a restrictive approach and deal with missing values by dropping all classes where a student is missing a grade. However, I also show that my results are robust to different ways of dealing with the missing values. Because the Komvux register only contains a course ID (i.e., specific to a subject) and not a class ID (i.e., specific to a group of students in the same classroom), I rely on the information contained in several other variables to identify a class. Under my definition, a class consists of anyone enrolled in the same course at the same school; furthermore, the course must have the same start and end date, the same number of lecture hours, and be held at the same time of day (i.e., daytime or nighttime).

Table C.2: Sample restrictions for analysis.

Sample restriction	Remaining observations
1. One observation per person-class-year	12,869,391
2. Dropping individuals $<$ 18 or $>$ 24 years old	4,055,384
3. Dropping individuals with missing background characteristics	4,046,356
4. Dropping introductory and individual courses	3,970,600
5. Dropping courses with missing info on subject, duration, etc.	3,968,001
6. Dropping classes with fewer than four students	3,829,188
7. Dropping classes with missing grades	3,240,448

Notes: This table summarizes the sample restrictions that are imposed for the main analysis.