Improving Educational Pathways to Social Mobility: Evidence from Norway's "Reform 94"*

Marianne Bertrand[†] Magne Mogstad[‡] Jack Mountjoy[§]

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Abstract

We study the impacts of a major reform to vocational secondary education that aimed to move beyond the tradeoff between providing occupational skills and closing off academic opportunities. Norway's Reform 94 integrated more general education into the vocational track, offered vocational students a pathway to college, and increased access to apprenticeships. We identify reform impacts through a difference-in-discontinuity research design applied to linked population registries. The reform substantially increased initial vocational enrollment, but with divergent consequences by gender. Overall, the reform succeeded at improving social mobility, particularly for disadvantaged men, but it somewhat exacerbated the gender gap in adult earnings.

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[†]University of Chicago Booth School of Business; NBER. marianne.bertrand@chicagobooth.edu.

[‡]University of Chicago Department of Economics; Statistics Norway; NBER. magne.mogstad@gmail.com.

[§]University of Chicago Booth School of Business; Statistics Norway; NBER. jack.mountjoy@chicagobooth.edu.

1 Introduction

Academic and vocational secondary education can be viewed as two separate pathways into the labor market. Academic education aims to deliver the general knowledge and learning skills that prepare young people for the demands of college, where they will start making specific human capital investments towards a career. In contrast, vocational education aims to deliver practical knowledge and occupation-specific skills that map directly into entering a particular occupation after high school. As shown in Figure 1, there is tremendous variation across countries in how much they rely on these two types of education in upper secondary. The U.S. is at one extreme of the spectrum, with a vanishingly small share of students concentrating in vocational (also known as career and technical) education in high school compared to other OECD countries.[1] At the other extreme are countries such as Austria, Germany, and Switzerland, where between 60 and 70 percent of high schoolers are enrolled in a vocational program.

Both efficiency and fairness arguments have been made in favor of academic programs. The main appeal of academic secondary education is that the general knowledge it aims to convey might be more portable across occupations, which is particularly valuable in the face of rapid labor market changes such as those induced by technology or globalization (Goldin, 2001). Moreover, by developing habits of learning, general education may make the process of re-skilling in response to shifting labor demand less costly. There has also been, in the U.S. in particular, a more philosophical resistance to vocational education. John Dewey (1916) was famously opposed to the expansion of vocational high school programs. He viewed such efforts to build a two-track (academic vs. vocational) education system as institutionalizing a social class distinction within the design of secondary education. This view is also well captured in the writings of Jeannie Oakes (1985): "[M]any educational scholars agree that an underlying function of vocational education has been to segregate poor and minority students into occupational training programs in order to preserve the academic curriculum for middle- and upper-class students." And indeed, in the U.S. but also other countries, demand for vocational education might be tamped down by the fact that it is often perceived as a dumping ground for underachieving students, those with learning disabilities or other behavioral problems.

Opposite arguments, however, can also be made, under which too little investment in vocational education in high school may contribute to worse labor market outcomes and more constraints on social mobility. First, the immediate marketability of the general skills acquired in an academic high school program might be limited absent the complementary subsequent investment in a specific career in college. This is important in that, while college participation has been on the rise throughout the developed world, it remains true that most young people today do not graduate from college and many do not even enroll. As of 2015, only 42 percent of 25-34 year-olds across the OECD had completed some tertiary education. Also, as has been by now extensively documented, men have been particularly struggling with college: of the six million students across the OECD that obtained a bachelor's degree in 2013, 58% were women.[2] This means that many young people, but especially young men, still enter

[1] Federal government funding under the Perkins Act (the largest source of funding for career and technical education in the U.S.) has been on the decline since the mid-1980s. While American high school students are completing more course work in academic fields such as mathematics, science, English and social studies, the number of vocational education credits has been declining for the last three decades (Hudson, 2013).

[2] http://www.oecd.org/gender/data/gender-gap-in-education.htm.

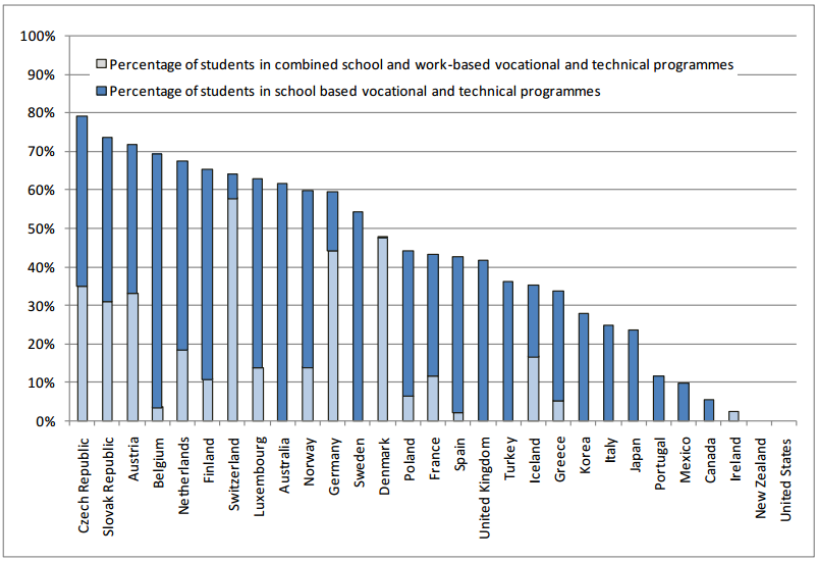


Figure 1: Vocational concentrators as a share of all high school students, 2006

Source: OECD (2008).

the labor market with at most a high school degree and might be better equipped to succeed in this transition from school to work with more occupation-specific skills. Furthermore, the abstract nature of the learning in academic programs may induce more high school dropouts as young people find it hard to grasp the "real-world" value of the curriculum. This suggests the possibility that a pure academic focus in high school may also contribute to fewer years of completed schooling, with more young people out of school (and possibly out of work) at a time when their propensity to engage in risky behavior is at its highest.

Such arguments have been gaining traction in the U.S., where there has been a growing discussion of the failure of the current U.S. secondary education system in addressing the needs of the "forgotten half" (Neumark, 2007) of students who are not on clear pathways toward college. In particular, there has been growing interest in educational reforms that would strengthen vocational education in U.S. high schools but also move away from the rigid two-track model feared since Dewey and offer more flexible pathways to success for students (Symonds et al., 2011).

In this paper, we study the consequences of such an educational reform in Norway. In 1994, Norway implemented changes in its high school vocational programs that were meant to both increase the quality of vocational education and lower the switching costs out of the vocational track. This reform, often referred to as "Reform 94," was meant to address several concerns with the high school system that had emerged by the early 1990s. In particular, Norway faced high drop-out rates and low rates of on-time degree completion. The existing vocational system was characterized by very early specialization, with students on the vocational track having to choose from more than a hundred foundation vocational courses in the first year of high school. The number of quality apprenticeship opportunities for vocational students was limited and the links between school-based learning and industry were poor. Finally, entering the vocational track essentially closed the door to college:

students with a vocational high school degree considering attending college had to restart high school on the academic track.

Starting in the fall of 1994, the number of foundation courses in the first year of the vocational track was streamlined to 10, with further specialization delayed to later years and investments made in improving access to quality apprenticeships in the final two years. Much of the rigidity of the previous two-track system was lifted by making general education a more integral part of the vocational track: vocational students now had to complete a common core in language, math, natural sciences, and social sciences, which allowed them to accumulate credits towards eligibility for tertiary education. Graduates of and students in the vocational track faced reduced costs of transferring to the academic track: vocational graduates could become eligible for university conditional on completing a supplementary six months of general education courses, and current vocational students could transfer to the academic track after the second year of high school and obtain an academic high school degree (and hence be eligible for college) by the end of the third year. This comprehensive educational reform was implemented in one step in the fall of 1994 with a strict eligibility cutoff: those born on or after January 1, 1978 were enrolled into the new regime.

We study the impact of this educational reform on educational attainment, paying close attention to the dynamics of enrollment and completion of different programs (vocational high school, academic high school, and college). We also study the impact of this reform on labor market participation and earnings in adulthood. We complement our study of educational and labor market outcomes with a glance into social outcomes such as criminal behavior, teen parenthood, and marriage. By comparing impacts across gender and family background, we assess this educational reform's implications for social mobility and gender gaps.

We use a difference-in-discontinuity research design to identify causal effects of the reform (e.g. Grembi et al., 2016). In particular, we leverage the sharp eligibility cutoff and compare outcomes for those born right after January 1, 1978 (eligible) to those born right before January 1, 1978 (ineligible). To eliminate the confounding effect of school starting age, we compare outcome discontinuities around the January 1, 1978 reform eligibility threshold to outcome discontinuities around January 1 in other (control) years.

We find that the reform substantially increased enrollment in the vocational track, as well as on-time enrollment in high school overall. Subsequent outcomes, however, differed across groups. Among men, especially those from disadvantaged backgrounds, more completed the vocational track; however, this increase in the share of men with a vocational high school degree is roughly equal in magnitude to the decline in the share of men with an academic high school degree, and hence the reform did not succeed in reducing the male high school dropout rate. Despite the decline in the share of men with an academic high school degree, the reform did not decrease male college attendance and completion, suggesting that the reform brought, on net, men into the vocational track (and out of the academic track) who would not have attended college anyway. Following these students into the labor market, we find that the average annual earnings of disadvantaged men increased by 5 percent post-reform. We also find evidence of reform-induced reductions in criminal charges among disadvantaged men in their teenage years, likely the result of an incapacitation effect due to the higher rates of high school attendance during peak ages of criminal activity.

In contrast, among women, but especially disadvantaged women, the initial surge in vocational enrollment leads to fewer high school dropouts: the share of disadvantaged women without high school credentials dropped by about 6 percentage points, or roughly a 20 percent decrease. The share of women with solely a vocational high school degree or solely an academic high school degree did not change; instead, the reform led to a large increase in the percentage of women holding both vocational and academic high school degrees, virtually all of whom first completed the vocational track and then completed the six-month supplementary academic degree. Yet, despite the greater share of college-eligible women, the reform did not significantly increase the share of women who completed college, and it had only small (and statistically insignificant) impacts on their adult earnings. Much of the difference in terms of how much men financially benefit from accumulating more vocational education compared to women can be traced to sharp gender segregation across vocational fields of study, with men concentrating in higher-paying skilled trade fields and women in lower-paying service fields.

Overall, the reform reduced the gap in adult earnings between disadvantaged and less disadvantaged children by about 20 percent, and it was particularly effective at improving social mobility among men, with the gap in adult earnings between disadvantaged and less disadvantaged men decreasing by close to 30 percent. However, given that male earnings gains dominated female earnings gains, the reform exacerbated the overall gender gap in adult earnings by about 8 percent.

We also perform a Oaxaca-Blinder-type decomposition to quantify how much of the observed labor market gains of the reform can be accounted for by changes in students' educational attainment, holding the earnings payoff to each type of education fixed at the pre-reform levels. We estimate that only about 20 percent of the earnings gains for disadvantaged men can be accounted for by their switching out of academic high school degrees and into vocational degrees, implying that improvement in the quality of the vocational track or better selection into the vocational track play a large role in generating these earnings gains. Among disadvantaged women, on the other hand, nearly all of their (statistically insignificant) earnings gains could be rationalized solely via the change in educational achievement, i.e. fewer women with no high school degrees and more women with both academic and vocational high school degrees.

While there is a vast literature on the returns to schooling, research on the returns to different educational curricula in high school, or different ways to structure educational tracks in high school, is much more limited. Most of the research on the returns to vocational vs. general education in high school is correlational, and hence unable to robustly address selection concerns. The findings of this literature are overall quite mixed. Some papers (Mane, 1999; Bishop and Mane, 2005; Meer, 2007) have reported a positive relationship between vocational secondary education and earnings, including recent quasi-experimental evidence from admissions cutoffs in Connecticut (Brunner et al., 2020) and Finland (Silliman and Virtanen, 2019). City-level, state-level, and national-level quasi-experimental studies commissioned by the U.S. Department of Education as part of a national assessment of vocational education found more mixed evidence on the impact of vocational education on educational and labor market outcomes (U.S. Department of Education, 2014). In a summary of earlier evidence from France, the U.K., and the U.S., Ryan (2001) finds a positive association between vocational education and employment probabilities, but no clear association with labor market earnings. Using individual-level data that span multiple countries, Hanushek et al. (2017) and Hampf and Woessmann (2017)

find that the positive association between employment and vocational education declines with age, consistent with the view that vocational education may ease the transition from school to work but that the specific skills acquired in vocational school become more rapidly obsolete than the general skills acquired in an academic program. A related literature, summarized in Altonji et al. (2012), has studied the correlation between different curriculum choices in high school and earnings, with a heavy focus on the positive association between math course-taking and later-life earnings. LaForest (2017) and Kreisman and Stange (2018) extend this literature by studying the association between the number of vocational courses students take and their earnings in adulthood. Finally, another related literature has studied the effects of school-to-work programs on education and labor market outcomes. Neumark and Rothstein (2006) find some positive benefits on college attendance and employment for men in school-to-work programs that create a direct link to the labor market (including internships and cooperative education); focusing on tech-prep programs, Cellini (2006) finds positive effects of these programs on high-school completion and community-college attendance, but negative effects on 4-year college attendance.

Our research is directly related to a few papers that have also leveraged educational policy reforms to learn about the relative benefits of vocational vs. academic education in high school, and alternative ways to structure high school tracking. Most of these previously studied reforms have centered around forcing more general schooling onto vocational students, and hence are narrower than Reform 94. Malamud and Pop-Eleches (2011) study a reform in Romania in 1973 that shifted a large number of students out of vocational and into general education; using a regression discontinuity design, they find no significant differences in university completion, employment, or earnings between the post- and pre-reform cohorts. Zilic (2018) studies the effect of a high school reform in Croatia in the mid-1970s that reduced tracking in high school by forcing all students to attend two years of general curriculum before entering a vocational school; using a regression discontinuity design, he finds that the reform reduced high school and university completion rates among males but had no adverse effects on females. Oosterbeek and Webbink (2007) use a difference-in-difference approach to study a reform in the Netherlands in 1975 that prolonged three-year vocational tracks with an additional year of general education; using students in tracks that did not change length as the control group, they fail to find any positive effect of the extra year of general schooling on the earnings of vocational students post-reform.

Most related to our paper is Hall (2012, 2016), who studies the pilot phase of a nearly contemporaneous major education reform in Sweden in 1991 that shared similarities with Reform 94 by increasing the academic content of the vocational tracks in upper secondary and giving students graduating from these vocational tracks basic eligibility for university studies. Focusing on students who start in the vocational track and exploiting variation in exposure to the pilot scheme across municipalities, Hall (2012) finds that the reform increased the number of years of completed upper secondary schooling among these students, but did not impact their college outcomes or earnings; furthermore, she finds that the reform increased the probability of dropping out of high school among students with low compulsory school GPAs. Furthermore, Hall (2016) shows that while the Swedish reform overall reduced the risk of experiencing unemployment for vocational students, it increased that risk among students with low GPAs (likely due to their higher drop out rates). While Hall (2012)'s disappointing results

do suggest that more general education and more flexible pathways alone might not benefit vocational students, there are at least two reasons why these results may not generalize to our study. First, the Norwegian reform was distinct in its additional objective of improving the quality of vocational learning, especially the emphasis on increasing apprenticeships. Second, Hall (2012) only considered the impact of the Swedish reform *conditional* on being enrolled in vocational education (either under the old or the new regime) and thus did not assess how enrollment decisions themselves were impacted, another margin of interest in our analysis.

Finally, the reformed educational system in Norway, with its emphasis on the quality of vocational schooling as well as on creating more flexible pathways to success in high school, can be likened to the Career Academies that were established in the U.S. more than 30 years ago. Career Academies combine academic and vocational curricula and provide work-based learning with employers, with the aim of further engaging students in school and preparing them for transition to either tertiary education or work. A randomized controlled trial evaluation conducted by MDRC in 1993 shows that participation in such an educational program increases earnings, employment stability, and hours worked later in life, especially among men; those gains, however, did not appear to be driven by improvement in years of completed schooling (Kemple, 2008).

2 Data

Below we describe our data and sample selection. Details about the data sources and each of the variables are provided in Table A.1.

2.1 Data sources

The starting point for our study is the Central Population Register, which contains records for every Norwegian from 1967 to 2015. Importantly for our analysis, these records include each individual's exact date of birth, as well as basic demographic measures like gender and immigrant status. Using unique identifiers for each individual, we link this data to several other data sources.

Educational enrollments and degrees.
Information on educational enrollments and degrees comes from the national educational registers. We observe the school and track in which students enroll, and which degrees, if any, they complete. This information is available for virtually all students in every school and track for the years 1985-2015. Educational choices and attainment are reported by the schools directly to Statistics Norway, thereby minimizing any measurement error due to misreporting. Starting with the 1985 birth cohort, we also observe each individual's grade point average (GPA) in 10th grade (age 15), the last year of compulsory schooling.

Labor market outcomes and criminal charges.
To perform a comprehensive evaluation of Reform 94, we consider a number of outcome variables in addition to educational choices and attainment. Starting in 1992, we have complete annual records of labor market earnings from tax records, as well as criminal charges from administrative police records.

Starting in 2003, we also observe hours worked and hourly wages in a matched employer-employee annual panel.

Family linkages.
Measures of family background, teenage parenthood, and marriage outcomes start by linking students in our sample to their parents, children, and spouses using family identifiers in the Central Population Register. With these linkages established, we measure parental and spousal outcomes in the same administrative registries as our sample members.

2.2 Sample selection

Our main analysis focuses on individuals who were born in Norway around the years 1975-1981. On average, there are about 50,000 individuals per cohort. We exclude the small number of individuals who emigrated or died by age 30. In addition, we drop the roughly one percent of the sample with missing parental identifiers. This yields an analysis sample of 339,793 individuals whom we can follow from high school entry at age 16 into adulthood. We partition this sample into what we call reform window and control window cohorts. The reform window cohort includes individuals born within ± 6 months of January 1, 1978 (the Reform 94 eligibility cutoff date), i.e. July 1977 through June 1978. The control window cohorts consist of individuals born within ± 6 months of January 1 in the three years prior to the reform window (1975-1977) and the three years after the reform window (1979-1981).

For some outcomes, we restrict attention to a subset of the 1975-1981 analysis cohorts due to data constraints. For example, earnings data coverage ends in 2015, so we drop the younger set of control cohorts when studying earnings at the older ages of 35-37. For criminal charges in the teenage years, we drop the older set of control cohorts, as we do not have information on criminal charges prior to 1992. We also use the 1985 birth cohort for some of the descriptive results, as they are the first to have data on grade point average (GPA) in 10th grade, the last year of compulsory schooling.

We focus many of our main results on individuals with low predicted 10th grade GPA as a summary measure of disadvantage. Low-scoring students are the policy-relevant subpopulation most likely to be affected by the reform, as they are the most likely to participate in the vocational track and are also at the highest risk of dropping out of high school. We do not observe actual 10th grade GPA in our 1975-1981 analysis cohorts, as the first cohort with GPA data is 1985, so we use family background variables to predict GPA in the analysis cohorts using a random forest trained on the 1985 cohort.[3] The top panel of Figure 2 shows that actual 10th grade GPA strongly stratifies the 1985 birth cohort by their probability of obtaining, at most, a vocational high school degree (thus also including dropouts who complete no degrees), and the bottom panel shows that predicted GPA

[3]Specifically, we train a random forest on the 1985 birth cohort using family background covariates available in both samples: mother's education (11 categories), father's education (11 categories), mother's long-run earnings (continuous), father's long-run earnings (continuous), an indicator for immigrant, an indicator for at least one immigrant parent, and number of siblings (continuous). The random forest procedure grows one thousand regression trees in the training sample, each of which uses only a random subset of the covariates, to produce a cross-validated prediction that maps covariates to predicted outcome values (in this case, 10th grade GPA). We use two randomly sampled covariates for each tree according to the rule of thumb $floor[K/3]$, where K is the number of available covariates, equal to seven in our case. To predict 10th grade GPA in the 1975-1981 main analysis cohorts, the random forest averages the predicted outcomes across all of the trees for each observation. Training and prediction are conducted separately by gender.

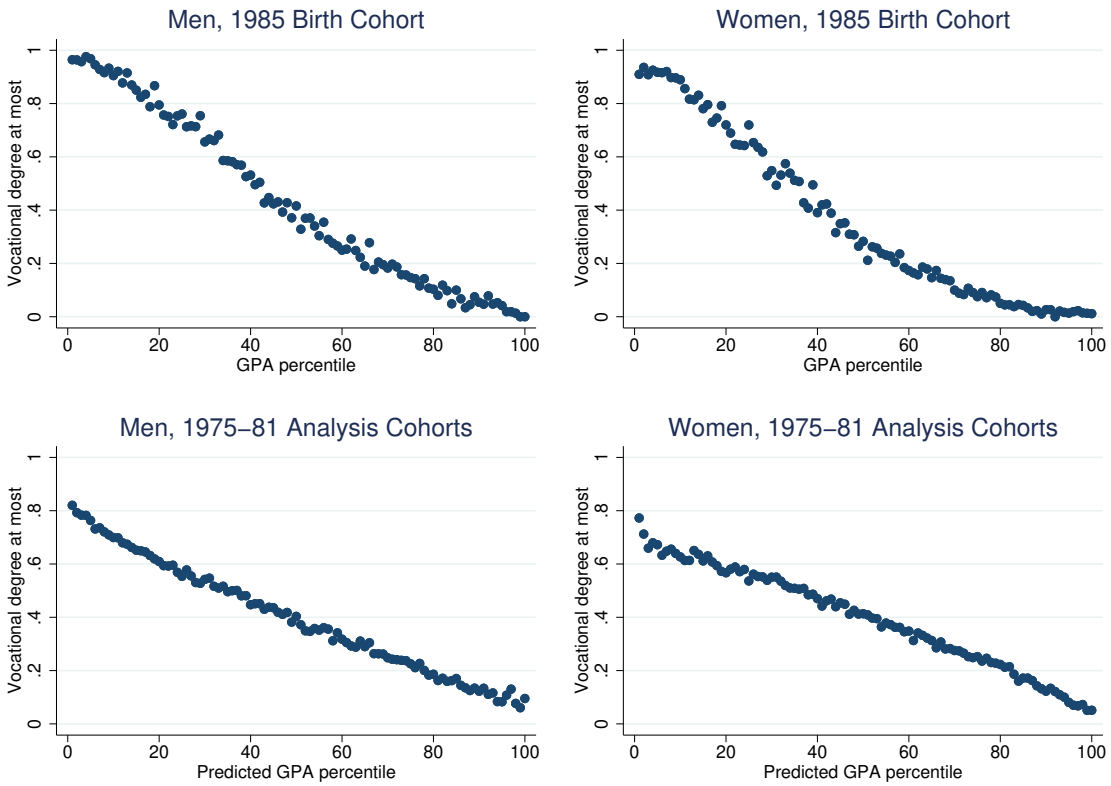


Figure 2: Stratification by actual 10th grade GPA in 1985 cohort (top row) and predicted GPA in analysis cohorts (bottom row)

Notes: The top two figures show the share of men (left) and women (right) who obtain at most a vocational high school degree (including dropouts who obtain no degrees) by actual 10th grade GPA percentile, which is first observed in the 1985 birth cohort. The bottom two figures show the share who obtain at most a vocational degree by *predicted* GPA percentile in the reform analysis cohorts (born 1975-1981), for whom actual GPA is not observed. GPA is predicted via random forest as described in Section 2.2.

reproduces much of this strong stratification within the 1975-81 main analysis cohorts.

Table 1 provides summary statistics for our main analysis sample and subgroups of interest. Disadvantaged men and women in the rightmost columns of the table, defined as being in the bottom third of the distribution of predicted GPA within each gender, come from family backgrounds where a college-educated parent is extremely rare, parental income is substantially lower, and siblings are more numerous compared to the average men and women in our sample. Low predicted GPA is thus synonymous with a lower socio-economic background, so we simply refer to this policy-relevant subgroup as disadvantaged throughout the text. In Section 5.5, we explore stratifications by alternative definitions of disadvantage.

Table 1: Summary statistics

				Disadvantaged	
	Pooled	Men	Women	Men	Women
At least one college-educated parent	0.328	0.329	0.326	0.012	0.021
Parental income	55,281	55,231	55,333	41,454	41,807
(SD)	(20,771)	(20,806)	(20,734)	(13,763)	(14,097)
Immigrant	0.029	0.027	0.031	0.033	0.043
At least one immigrant parent	0.076	0.075	0.077	0.069	0.084
Number of siblings	1.65	1.65	1.65	1.81	1.77
(SD)	(1.07)	(1.07)	(1.07)	(1.24)	(1.23)
Predicted 10th grade GPA percentile	50.5	37.0	64.6	9.9	36.5
(SD)	(28.9)	(26.6)	(24.0)	(5.5)	(13.1)
Enroll in vocational track at 16	0.356	0.426	0.283	0.589	0.415
Complete vocational HS degree	0.284	0.337	0.228	0.417	0.310
Complete academic HS degree	0.558	0.472	0.648	0.269	0.467
Complete college degree	0.356	0.271	0.445	0.124	0.273
Mean earnings over ages 25-34	43,831	50,755	36,586	47,108	31,685
(SD)	(22,683)	(24,348)	(18,163)	(23,344)	(17,039)
Any criminal charges over ages 16-30	0.121	0.192	0.047	0.266	0.072
Teenage parenthood	0.025	0.009	0.042	0.014	0.074
Married by age 34	0.436	0.378	0.496	0.347	0.473
Age at first marriage	28.14	28.87	27.55	28.61	27.06
Spouse has college degree	0.378	0.474	0.302	0.329	0.176
N	339,793	173,741	166,052	57,911	55,347
N for criminal charges	189,319	97,042	92,277	32,346	30,757
N for marriage characteristics	143,818	63,870	79,948	19,375	25,199

Notes: This table presents summary statistics for the main analysis sample. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender. Family background and demographic variables are measured at age 16 just prior to high school entry. Parental income is a long-run average of combined mother and father earnings over the twenty years prior to the child turning 16. Degree completion is measured at age 30. The sample size for criminal charges is smaller than the other outcomes as it excludes the older control cohorts for whom charge data are not available. Age at first marriage and spouse has college degree are conditional on being married by age 34, the last commonly observable age among our main analysis cohorts.

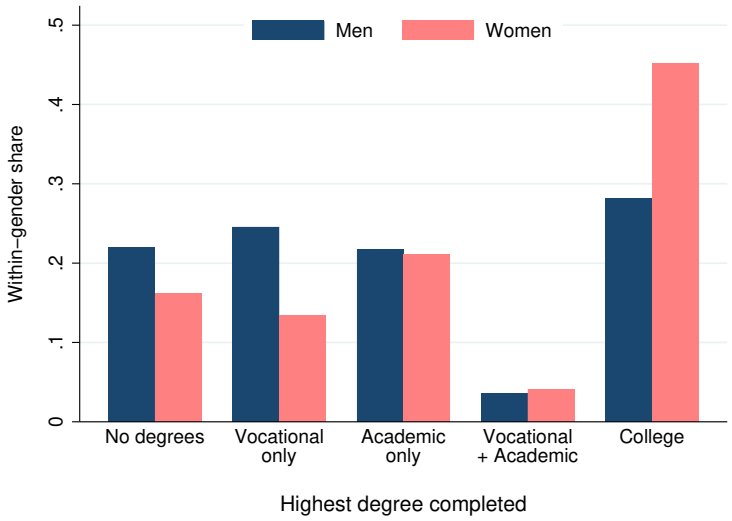


Figure 3: Highest degree completed by gender, pre-reform cohorts

Notes: This figure shows the distribution of highest degree completed, separately for men and women, among the pre-reform cohort born in 1977. These degree categories are mutually exclusive and exhaustive, and measured as of age 30.

3 Institutional setting: Reform 94 and descriptive evidence

3.1 Background and reform

After completing 10 years of compulsory schooling, Norwegian students enter high school (upper secondary) in 11th grade, typically at age 16. While high school is not compulsory, nearly everyone enrolls eventually, if not on-time at 16. There are generally no tuition fees for attending high school. The vast majority of high schools are public and administrated locally by the nineteen counties. Schools may offer academic education, vocational training, or both. Students may apply to several schools and fields within their county. The admission process is centralized with students applying to the combination of a high school and field. Schools or fields for which there is excess demand are filled based on an application score that depends on the student's performance in lower secondary school.

Pre-reform system.

Norway's high school system broadly consists of two tracks. The first is the 3-year academic track, offering theoretical education to prepare and become eligible for higher education at colleges and universities. Curricula at all schools follow a national standard, focusing on language, math, natural sciences, and social studies. Upon completion, students are awarded a general admission certificate for higher education.[4] The second is the vocational track, offering vocational education and training. Prior to Reform 94, this track consisted of a theoretical introduction to a chosen vocational field and practical training in school workshops (or occasionally an apprenticeship), with limited general education. Depending on the subject area, the length of the vocational track varied from 3 to 4 years.

[4]See Kirkeboen et al. (2016) for a description of the admission process to higher education in Norway.

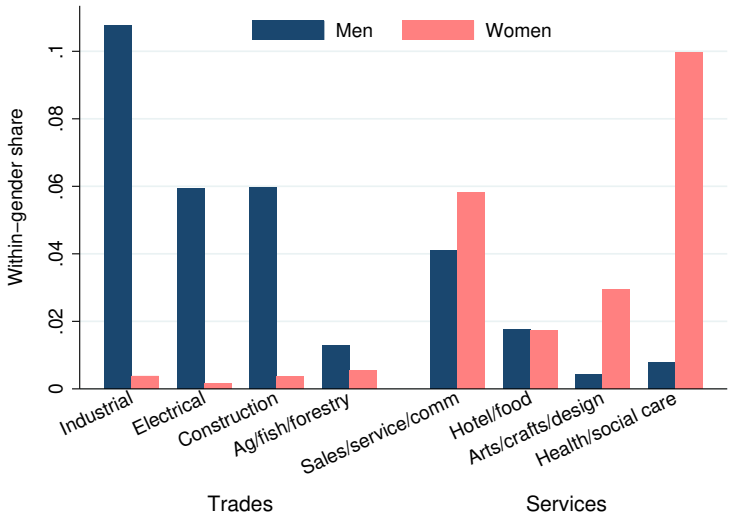


Figure 4: Distribution of high school vocational fields by gender, pre-reform cohorts

Notes: This figure shows the distribution of high school vocational fields completed, separately for men and women, among the pre-reform cohort born in 1977. Completion is measured as of age 30. Within-gender shares in this graph do not sum to one, but rather to the overall within-gender share completing any vocational degree; the complement consists of those who complete an academic degree or no high school degree.

Figure 3 plots the distribution of educational attainment by gender under the pre-reform system. Norway, like much of the developed world, is characterized by higher educational attainment for women. Nearly half of pre-reform women have completed a college degree by age 30, compared to less then 30 percent of men. Among the remaining women who do not complete college, they are more likely to have an academic high school degree as their highest educational attainment relative to a vocational degree or no degrees at all. One quarter of men, in contrast, have a vocational high school degree as their highest attainment—nearly double the share of women in the same category—and men are more likely to drop out with no degrees at all. Only a small share of pre-reform men and women (less than 5 percent) complete both vocational and academic high school degrees (without having also completed college).

Figure 4 shows the distribution of completed vocational fields of study among pre-reform men and women. Men overwhelmingly pursue vocational education in skilled trade fields, while women almost exclusively pursue vocational education in service-based fields. The three more common vocational fields among men are industrial, electrical, and construction. Among women, the most common vocational degrees are in the health and social care sector, followed by sales/service/communication. The only truly gender-mixed vocational fields are in sales/service/communication and hotel/foodservice.

Figure 5 shows that these vocational fields are distinct not only in their content and gender composition, but also in their remuneration. For each field, we plot mean hourly wages among individuals with a vocational degree in that field as their highest educational attainment. Given the stark gender segregation by field observed in Figure 4, we do not report these hourly wages separately

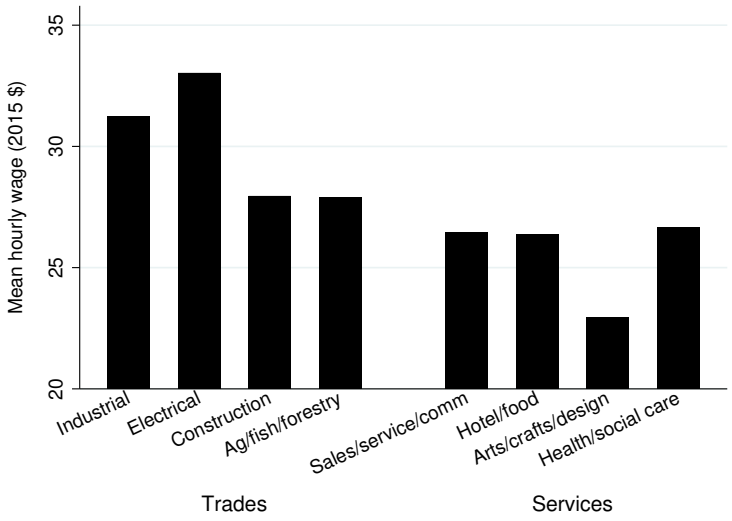


Figure 5: Mean hourly wage by high school vocational field

Notes: This figure shows mean hourly wages associated with each high school vocational field among the pre- and post-reform 1977 and 1978 cohorts. Wages are first averaged within person over ages 28-33, which is the maximal age span for which we can measure wages for all analysis cohorts at the same ages. These means exclude vocational completers who also complete an academic high school degree or a college degree to reflect the wage earning potential of a terminal vocational degree in each field.

by gender.[5] Figure 5 shows that the male-dominated vocational fields in skilled trades are associated with higher hourly wages than the female-dominated service fields. Mean hourly wages are highest for those having pursued vocational education in industrial or electrical trades and lowest for those that have specialized in arts/crafts/design.

Challenges to the system.

In 1994, Norway implemented several changes to its high school system that were meant to both increase the quality of vocational education and lower the switching costs out of the vocational track. The motivation for this reform included several concerns that had emerged by the early 1990s. Norway faced high drop-out rates and low rates of on-time high school degree completion, especially in the vocational track.[6] The existing vocational system was characterized by very early specialization, with students on the vocational track having to pick one of more than a hundred subject areas in the first year of high school. Access to quality apprenticeship opportunities was also limited, with only about half of all completed vocational degrees including an apprenticeship. Finally, entering the vocational track essentially closed the door to college: vocational students seeking college entry credentials had to restart the entire high school process on the academic track.

[5] Within the two fields that are not dominated by one gender—sales/service/communication and hotel/food—men earn roughly 15-20 percent higher hourly wages than women.

[6] Among pre-reform students who started high school in the vocational track at the typical age of 16, 58 percent did not complete a high school degree on time by age 20. Among pre-reform students who ever enrolled in the vocational track, 30 percent never completed a high school degree as of age 30.

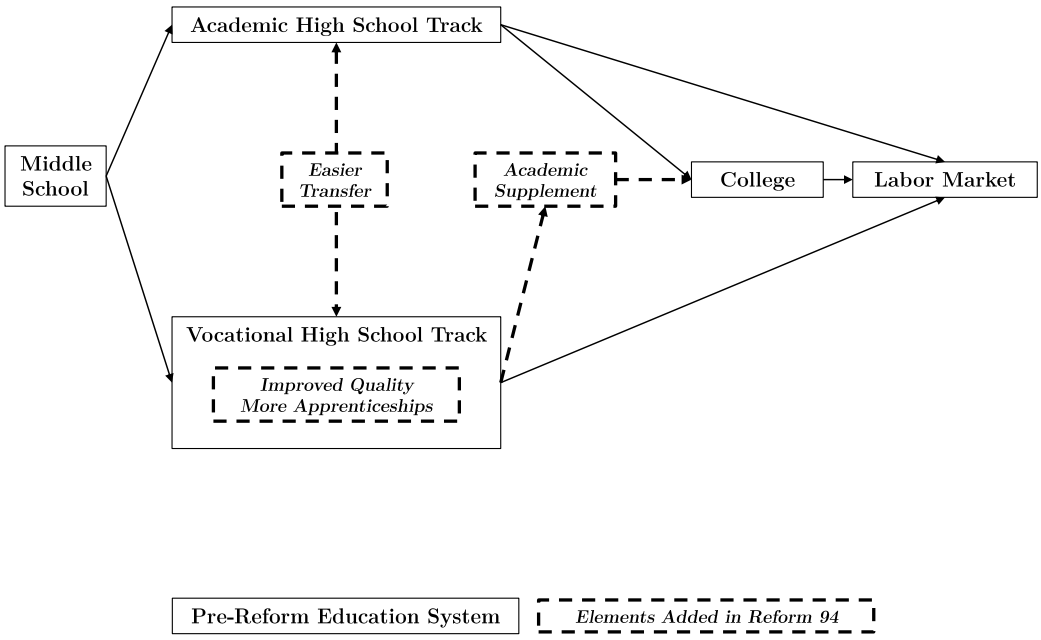


Figure 6: Main pathways in the Norwegian high school system, pre- and post-Reform 94

Key elements of Reform 94.

To address these concerns, the Norwegian government reformed its high school system in a single step the fall of 1994, known as Reform 94.[7] While the academic track remained essentially unchanged, the vocational track experienced three main transformations, as visualized in Figure 6.[8]

First, the effort to improve the quality of the vocational track included significantly increasing the availability of firm-based apprenticeships. Instead of the typical pre-reform path of three years of school-based vocational education, students in the post-reform system became more likely to undertake two years of school-based study followed by two years of employment and training within a firm. To increase the supply of apprenticeships, the government subsidized the wages of apprentices and partnered with firms and trade unions. A second key element of Reform 94 was to broaden the vocational subject areas and integrate more general education, thereby lowering switching costs both between vocational fields and between the vocational and academic tracks. This was done in part by streamlining the first year of the vocational track, reducing the choice set to ten subject areas with further specialization deferred to the later years. Moreover, vocational students now had to complete a common core in language, math, natural sciences, and social sciences, allowing them to accumulate

[7] See Brinch et al. (2012) and Kuczera et al. (2008) for descriptions of the Norwegian educational system in general and Reform 94 in particular. Brinch et al. (2012) also study how the reform may have changed the educational attainment of immigrant youth.

[8] The reform also formally granted students the statutory right to some form of high school education. However, they were not guaranteed admission to a preferred school or track. In reality, over 99 percent of Norwegians eventually attempt (but do not necessarily complete) some form of high school education by age 25, both prior to and after the reform.

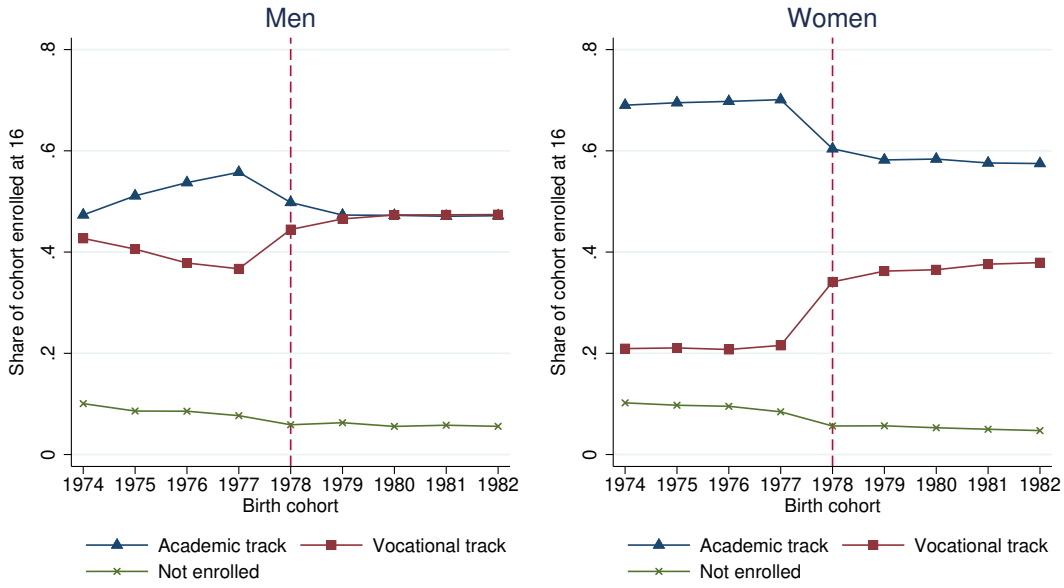


Figure 7: Initial high school track by birth cohort

Notes: This figure shows initial high school track enrollment shares by birth cohort, measured at age 16. Students choose between academic and vocational tracks upon high school entry, usually in the calendar year in which they turn 16, though note that some individuals do not enroll at 16. The dotted 1978 cohort is the first eligible for the reformed high school system.

transferable credits to the academic track after the second year of vocational school. Finally, Reform 94 created a feasible pathway from the vocational track to college: students who completed the reformed vocational track could now earn an academic diploma and eligibility for higher education by completing a supplementary semester of general education courses, rather than having to restart high school entirely.

Taken together, these changes had only a modest impact on government spending on high schools. In fact, reports from Statistics Norway and the Norwegian government suggest little if any change in both spending per student and aggregate expenditure on secondary education (Rivenes et al., 2001; Raabe, 2003). Our results below show that while the reform modestly increased the probability of students starting high school on-time at age 16, this primarily shifted students who otherwise would have started high school at a later age to now do so at an earlier age, which helps understand why overall spending did not see a permanent increase in the years after the reform. Furthermore, the increased costs of subsidizing additional apprenticeships was largely offset by reduced costs from fewer years of classroom instruction in most vocational fields.

3.2 Descriptive evidence

Changes in enrollment and degree completion.

The time series evidence presented in Figure 7 suggests that the reform had a large and immediate

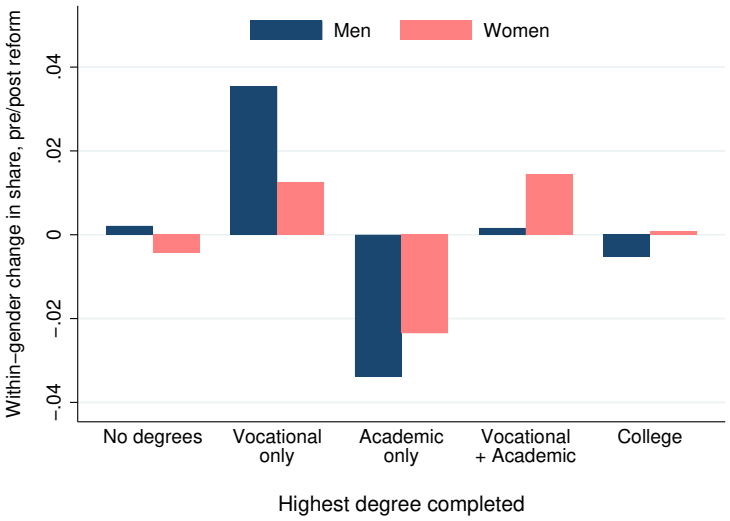


Figure 8: Changes in highest degree completed, pre- vs. post-reform

Notes: This figure shows the change in the share of men and women in each degree category, comparing the pre-reform 1977 cohort to the post-reform 1978 cohort. These degree categories are mutually exclusive and exhaustive, and measured at age 30.

impact on students' educational choices. This figure plots high school track enrollment shares by birth cohort, as measured at age 16 (the age at which students typically begin high school). There is a large and abrupt shift towards the vocational track and away from academic and non-enrollment with the 1978 birth cohort, who are the first to be eligible (by law) for the reformed high school system.

Moving from initial enrollments to final degrees, Figure 8 shows how the ultimate educational attainment of men and women changed in the wake of Reform 94. Men and women both become less likely to end up with only an academic high school degree as their highest attainment. For men, this is almost entirely offset by an increase in the share who have only a vocational degree, while the offsetting increases for women are roughly equally split between having only a vocational degree and having both types of high school degrees, likely driven by women completing the vocational track first and then tacking on the new academic supplement that grants college eligibility.[9] We see little change in the shares of men and women with the highest (college) and lowest (no degrees) levels of educational attainment, suggesting that the reform mainly impacted educational choices on the internal margin of high school specialization, a point we return to below in the main analysis.

Changes in apprenticeships and vocational fields of study.

Within the post-reform increase in vocational degrees lie two important phenomena: a significant increase in vocational students completing apprenticeships (as the reform intended), and changes in the distribution of vocational fields of study. Figure 9 confirms that the reform substantially increased the availability of apprenticeships within the vocational track, as the share of each birth cohort completing a vocational degree with an apprenticeship sharply increases with the 1978 reform cohort while vocational degree completion without an apprenticeship steadily declines.

[9] We probe this particular sequence of high school degree completion in Table A.5 below.

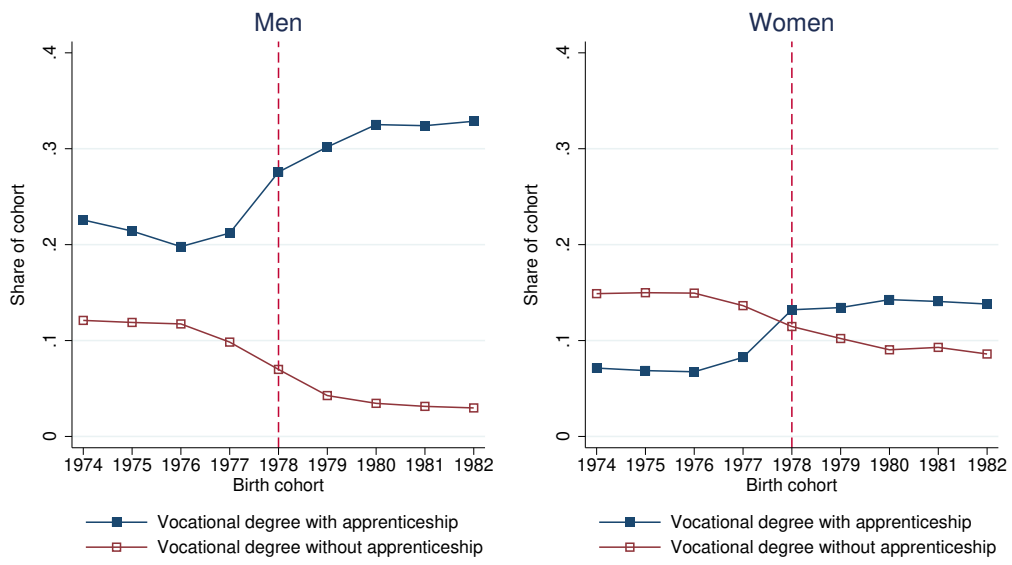


Figure 9: Vocational degree and apprenticeship completion by birth cohort

Notes: This figure shows vocational degree completion shares by birth cohort, split by whether the degree included an apprenticeship. Completion is measured at age 30. The dotted 1978 cohort is the first eligible for the reformed high school system.

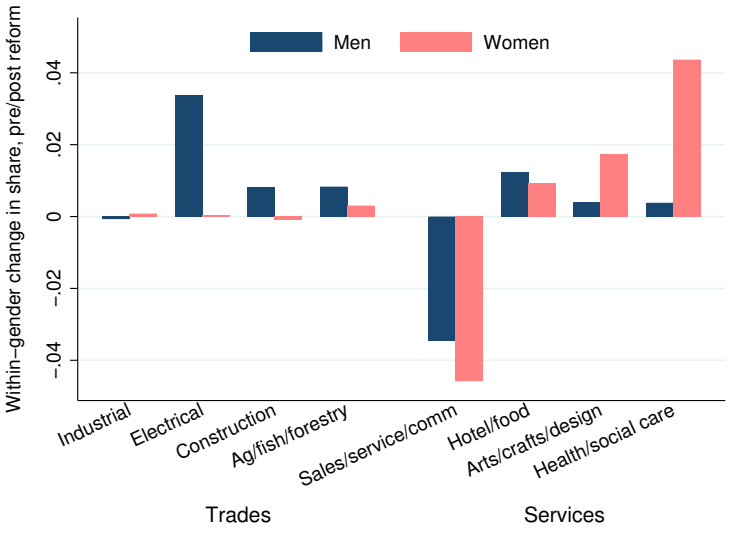


Figure 10: Changes in completed vocational fields, pre- vs. post-reform

Notes: This figure shows the change in the share of men and women completing each vocational field, comparing the pre-reform 1977 cohort to the post-reform 1978 cohort.

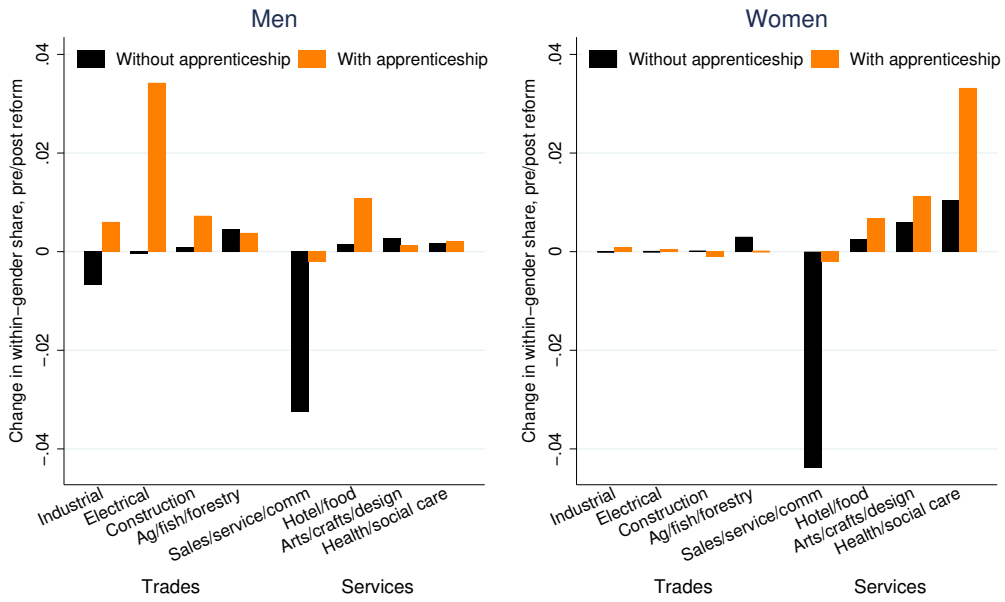


Figure 11: Changes in completed vocational fields by apprenticeship status, pre- vs. post-reform

Notes: This figure shows the change in the share of men and women completing each vocational field with or without an apprenticeship, comparing the pre-reform 1977 cohort to the post-reform 1978 cohort.

Figure 10 decomposes the post-reform increase in vocational degrees into fields of study. Men and women both primarily switch out of sales/service/communication, with men seeing the greatest gains in electrical studies and women seeing the greatest gains in health and social care. Figure 11 further decomposes the post-reform increase in each vocational field into whether the degree was completed with an apprenticeship, showing that both men and women switch out of vocational degrees without apprenticeships and into degrees with apprenticeships. Figure A.1 plots the overall share of degrees within each vocational field that are completed with an apprenticeship, pre-reform versus post-reform. Some of the post-reform increase in apprenticeship completion can be explained by students switching into fields with higher apprenticeship rates, but within-field apprenticeship rates also increased across the majority of fields.

Earnings profiles and premia by highest degree.

So far, we have focused on educational choices and how they may have changed in the wake of Reform 94. Figure 12 complements this analysis by showing the earnings profiles associated with each level of educational attainment. As above, we separate men and women. We also separate birth cohorts that pre-date and post-date Reform 94: the pre-reform cohorts (top row of the figure) include those born before 1978 while the post-reform cohorts (bottom row) include those born in 1978 and after. For both genders and time periods, the earnings of college graduates overtake those of other degree holders by the late 20s. Not surprisingly, those that did not complete high school have the lowest earnings, and their earnings deficit only increases with age. The earnings profiles of women with academic vs. vocational high school degrees do not differ much, and this is true both pre- and post-reform. In contrast, men with a vocational high school degree earn more than men with an academic

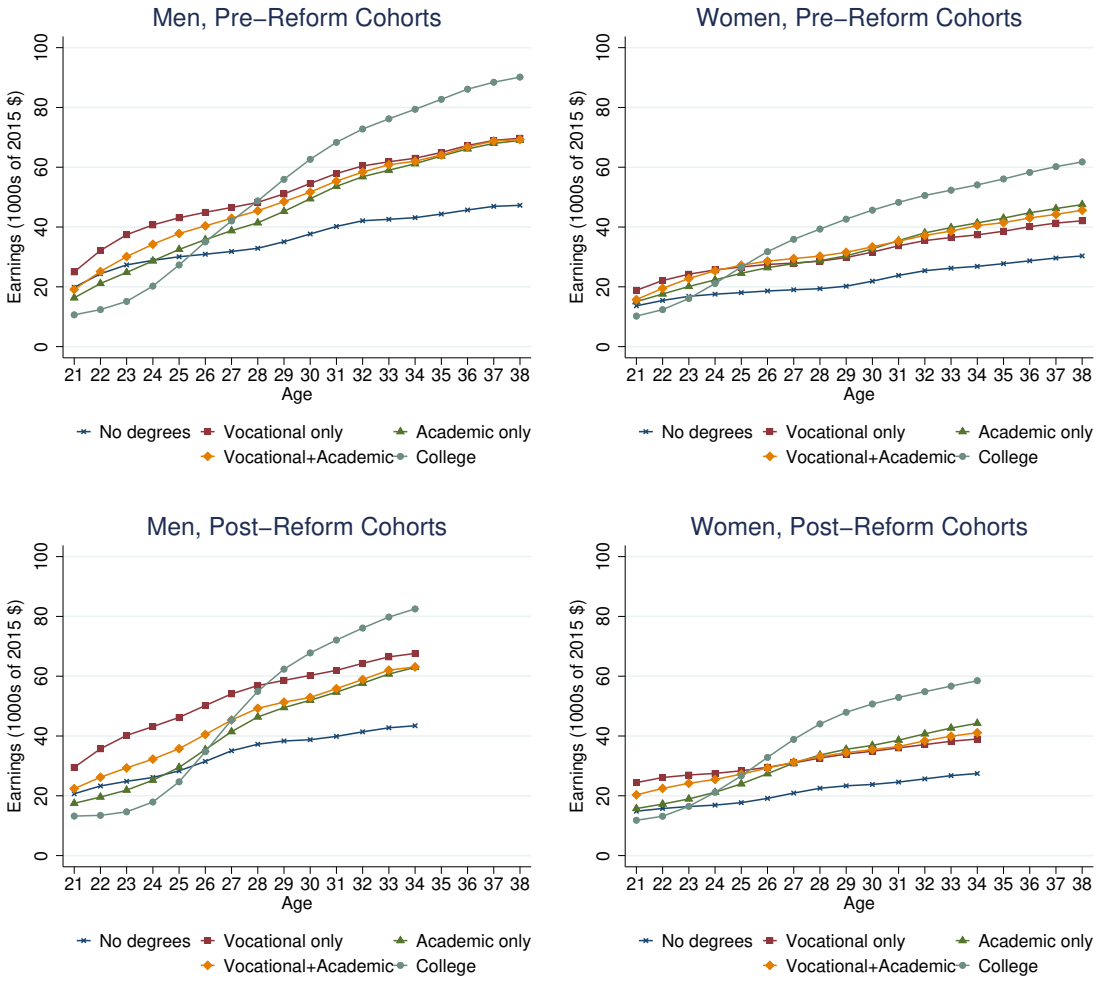


Figure 12: Earnings profiles by highest degree, pre- and post- reform

Notes: This figure shows the mean earnings profiles associated with the mutually exclusive and exhaustive degree categories from Figure 3. Degree completion is measured at age 30, and each individual is assigned to that constant degree category over the lifecycle. Pre-reform cohorts are those born prior to 1978; post-reform cohorts are those born in 1978 and after. The pre-reform profiles end at age 38 as the earnings data end in 2015 and the 1977 cohort turns 38 that year; likewise the post-reform profiles end at age 34 since the 1981 cohort turns 34 in 2015.

high school degree throughout their twenties, with convergence happening in the 30s. Moreover, the earnings premium for men with a vocational high school degree compared to those with an academic high school degree is greater for the post-reform cohorts. While extrapolating beyond the available data suggests eventual convergence in the late 30s, post-reform men with a vocational degree earn substantially more than their counterparts with an academic high school degree throughout their 20s and well into their 30s.

Figure 13 estimates the premia associated with each level of educational attainment (relative to having no degrees) after flexibly controlling for a rich set of covariates: 10th grade GPA, parental

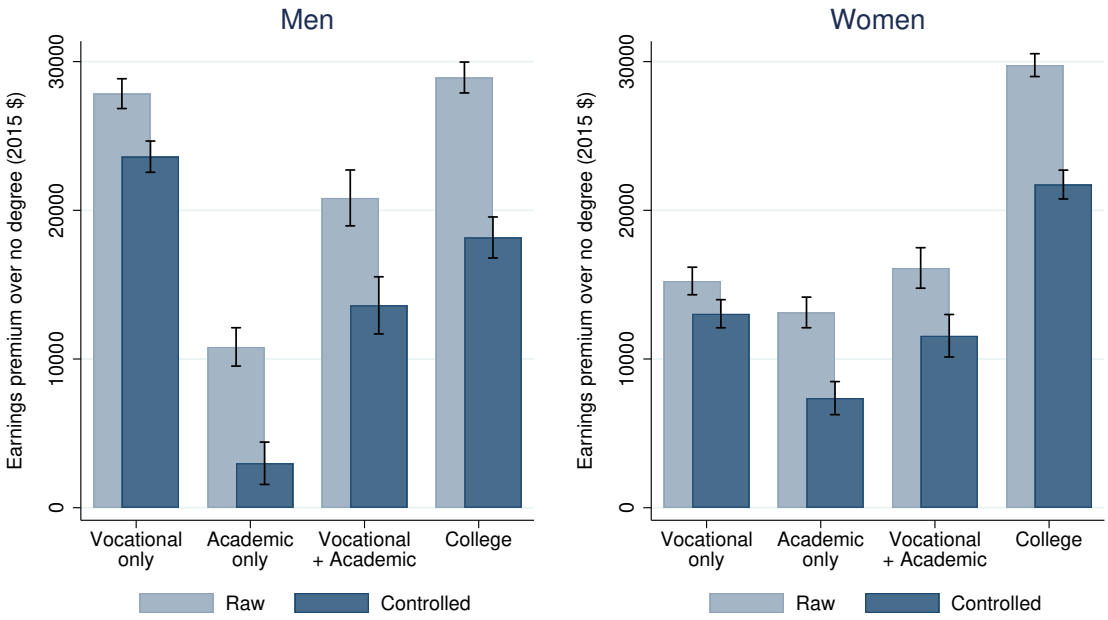


Figure 13: OLS earnings premia for each educational attainment category relative to no degree

Notes: This figure plots raw and controlled earnings premia associated with the degree categories from Figures 3 and 12. The sample is limited to the 1985 birth cohort, who are the first to have 10th grade GPA data. Controlled earnings premia come from a regression of within-person mean earnings over ages 29-30 on degree dummies (omitting the "no degrees" category as the base case) and flexible controls for 10th grade GPA (deciles), mother's education (11 categories), father's education (11 categories), mother's earnings (deciles), father's earnings (deciles), an indicator for at least one immigrant parent, an indicator for the child being an immigrant, and number of siblings (quadratic). Robust 95% confidence intervals appear in brackets.

education, parental income, parent and child immigration status, and number of siblings. We do this for the 1985 birth cohort, which is the oldest cohort for which we have 10th grade GPA data. We focus on earnings averaged over 29 and 30, the latest available ages for this cohort. Figure 13 shows that these detailed controls do not diminish the large vocational-over-academic high school earnings premium for men; in fact, the vocational-academic difference increases with controls, and predicted vocational earnings actually dominate predicted college earnings for men in this age range after controlling for observables.[10] Note that we also observe somewhat greater controlled earnings premia for women with a vocational high school degree compared to those with an academic high school degree, but the magnitude of this earnings premium is much smaller for women than it is for men.

[10] We obtain similar results if we estimate educational premia separately by 10th grade GPA categories.

4 Research design: graphical evidence and regression model

The descriptive evidence presented in the previous section motivates our research design. It suggests that Reform 94 had a large and immediate impact on students' educational choices. One may worry, however, that causal inferences based on these time series are confounded by differences in the unobserved characteristics of students born in different years. To address this concern, our research design leverages the reform's sharp eligibility cutoff by date of birth. In particular, we apply a regression discontinuity design comparing outcomes for those born right after January 1, 1978 (eligible for the new system) and those born right before January 1, 1978 (ineligible). The main idea behind the research design is that individuals born just before this date are good comparisons to those born just after. A concern, however, is that such comparisons confound the impact of Reform 94 with a general effect of being born at the beginning of a calendar year instead of at the end of the previous one. For example, it could be that children born right after January 1st do better because there are direct benefits to starting school at an older age (e.g. Black et al., 2011).

To estimate causal impacts of the reform, we therefore use a difference-in-discontinuity design. This design first estimates the discontinuity in outcomes around the exact eligibility threshold of January 1st, 1978, and then purges any general effect of being born early in the year by differencing out any discontinuity found around January 1st in non-reform control years. Figure 14 provides a graphical depiction of the difference-in-discontinuity design, before turning to the formal econometric model. This figure shows the share of individuals who completed vocational high school by day of birth in windows of ± 180 days surrounding January 1st in different years. In each figure, we plot the unrestricted monthly means and the predicted means using linear regression applied separately to each side of the January 1st thresholds. Whereas the regression lines better illustrate the trends in the data and the size of the jumps at the cutoff, the unrestricted means indicate the underlying noise in the data.

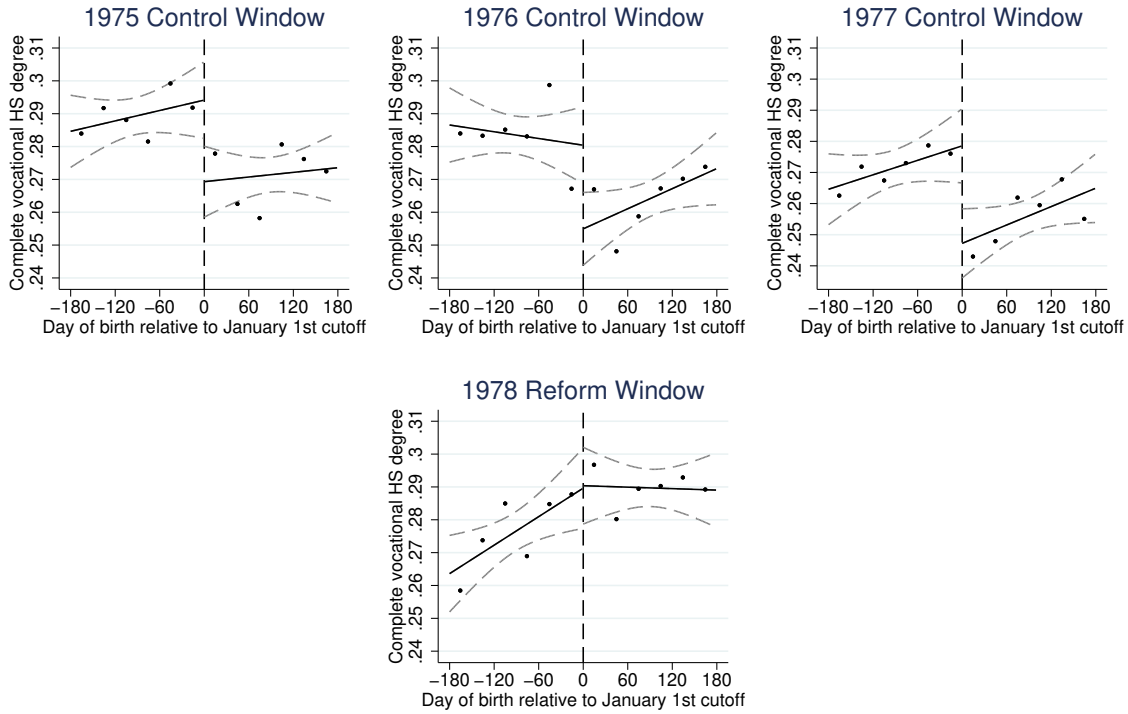
The 1975-1977 windows, shown in Panel A of Figure 14, display how the share of individuals that complete vocational high school varies with date of birth prior to the reform. In each year, students born right after January 1st are less likely to complete a vocational degree as compared to students born right before January 1st. This pattern stands in stark contrast to what we observe for the 1978 window, which includes the individuals who are the first to be treated by the reformed high school system. In particular, individuals born right after January 1, 1978 (and thus treated by the reform) are equally likely to complete vocational high school as those born right before January 1, 1978.

To gain precision, Panel B of Figure 14 pools data for three control years before (1975-1977) and three control years after (1979-1981) the reform year window (1978). The difference-in-discontinuity design identifies the reform effect by comparing the change in the outcome around the January 1st, 1978 reform eligibility cutoff (left graph of Panel B) to the change in the outcome around the January 1st threshold in the years other than the reform year (right graph of Panel B).[11] Interpreted through the lens of this research design, the difference in these discontinuities suggests the reform caused a significant increase in the probability that students complete vocational high school.

Figure 14 motivates and guides our specification of the regression model. Using the pooled data,

[11] For completeness, Figure A.2 presents each discontinuity estimate separately by cohort.

Panel A: Separate control windows



Panel B: Pooled control windows

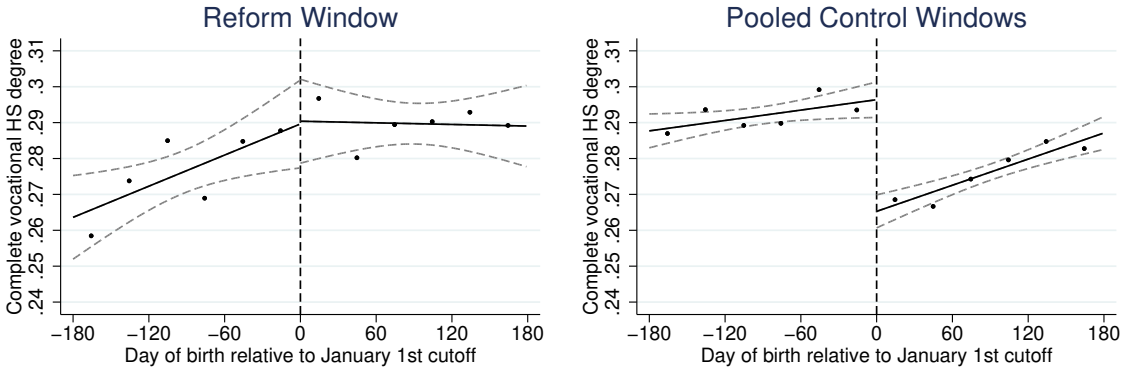


Figure 14: Visualizing the difference-in-discontinuity in vocational degree completion

Notes: Panel A plots mean vocational degree completion (as an example outcome) within 30-day birth bins (dots), linear fits through 180-day bandwidths on each side of each year's January 1st cutoff (solid lines), and robust 95% confidence intervals (curved dotted lines). The 1975, 1976, and 1977 control windows illustrate the school starting age effect that is present each year at the January 1st birthdate cutoff. The 1978 window contains the reform eligibility cutoff. The left plot in Panel B is again the 1978 reform window; the control windows of 1975-1977 and 1979-1981 have been pooled to form the right plot of Panel B. The reform window discontinuity minus the pooled control window discontinuity yields the difference-in-discontinuity estimate of the reform effect.

the model is given by:

$$Y_i = \alpha + \delta_1 Reform_i + \delta_2 Post_i + \beta Reform_i \times Post_i + Day_i(\delta_3 + \delta_4 Reform_i + \delta_5 Post_i + \delta_6 Reform_i \times Post_i) + \epsilon_i \quad (1)$$

where Y_i is an outcome of student i, Day_i is the running variable measured as day of birth minus January 1st within each window, $Reform_i$ is an indicator variable equal to 1 if student i was born in the reform window of $\pm$ 180 days surrounding the cutoff date January 1, 1978, and $Post_i$ is an indicator variable that is equal to 1 if she was born after the January 1st cutoff within each window. β is our main parameter of interest. The interactions with Day_i allow slopes to vary arbitrarily on each side of the January 1st cutoff as well as across reform vs. control windows.

This research design assumes that unobserved characteristics evolve similarly around January 1st, 1978 as compared to the years other than the reform year. In Table A.2, we estimate balancing checks of the reform on pre-determined covariates using our main specification. We find little evidence of differential selection across the reform vs. control window thresholds: out of 25 estimates across covariates and subgroups, only one is marginally significant at the ten percent level. In Section 5.5, we show that our baseline estimates are not sensitive to controlling for pre-determined covariates as one of several robustness checks probing the stability of our estimates to alternative specifications.

5 Results

5.1 How the reform changed educational choices

Effects on enrollment.

We start in Table 2 by studying how Reform 94 impacted initial high school enrollment choices. In this table, as in all subsequent tables, we present pooled results (column 1), results by gender (columns 2 and 3), and results by gender among disadvantaged students (columns 4 and 5). Row 1 shows that the reform resulted in an increase in the share of 16-year-olds who begin high school in the vocational track. This is true overall (column 1) as well as across gender groups. The magnitudes are large. In the pooled sample, vocational enrollment at 16 went up by 8 percentage points, which corresponds to a more than 20 percent increase. Across all groups, the increased enrollment in the vocational track at age 16 was accompanied by a decrease in enrollment in the academic track (row 2). The absolute value of this decrease in academic enrollment is smaller in magnitude, however, than the vocational enrollment increase, so the reform succeeded in increasing overall enrollment in high school at age 16 (row 3). The magnitude is largest for disadvantaged men (column 4), who experienced a 4.6 percentage point decrease in the likelihood of not being enrolled in high school at age 16, comprising close to a 50 percent decrease. Overall, we conclude from Table 2 that Reform 94 succeeded in substantially increasing initial high school enrollment along the vocational track. While part of these gains reflected a switch out of the academic track, total enrollment in high school at age 16 went up, particularly for disadvantaged men.

Table 2: Reform effects on initial high school enrollments

				Disadvantaged	
	Pooled	Men	Women	Men	Women
Enroll in vocational track at 16	0.083***	0.065***	0.100***	0.118***	0.127***
(SE)	(0.010)	(0.014)	(0.013)	(0.024)	(0.025)
Outcome mean	0.356	0.426	0.283	0.589	0.415
Enroll in academic track at 16	-0.059***	-0.038***	-0.080***	-0.073***	-0.110***
	(0.010)	(0.014)	(0.014)	(0.023)	(0.025)
	0.572	0.503	0.644	0.304	0.469
Not enrolled at 16	-0.024***	-0.027***	-0.020**	-0.046***	-0.018
	(0.006)	(0.008)	(0.008)	(0.016)	(0.017)
	0.074	0.073	0.075	0.109	0.117
N	339,793	173,741	166,052	57,911	55,347

Notes: Estimates come from separate difference-in-discontinuity regressions for each outcome and subgroup as described in Section 4. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

Effects on degree completion.

Table 3 reports on the impact of Reform 94 on educational attainments by age 30. We focus here on non-exclusive categories and consider whether someone has completed a particular degree by age 30, not solely the highest educational attainment. Row 1 shows that the reform not only increased the likelihood of initially enrolling in the vocational track but also increased the share of individuals who actually completed a vocational high school degree. This is true for both men and women. However, the rest of the table shows quite different patterns by gender on how the reform impacted other educational attainments. For men, the gains in the share of individuals with a vocational high school degree are roughly equal in magnitude to the declines in the share of individuals having completed an academic high school degree (row 2), and thus the reform did not succeed in ultimately reducing the male high school dropout rate (row 4). Despite declines in the share of men with an academic high school degree, the reform does not appear to have negatively affected men's likelihood of attending college at any point before age 30 or of having completed a college degree (rows 5 and 6, respectively). The results therefore suggest that men did not leverage the new pathways into college opened up by the reform; rather, the reform simply induced a net swap from terminal academic high school degrees to terminal vocational degrees.

The patterns for women, particularly disadvantaged women (column 5), are quite different. Unlike for men, the reform was not accompanied by a parallel decline in the share of women with an academic high school degree. In fact, among disadvantaged women, the reform increased the share who completed such an academic high school degree by 6.1 percentage points (from a mean of .47). The reform also increased the share of disadvantaged women holding both vocational and academic high school degrees by about the same amount (6.4 percentage points), suggesting that most of these new academic high school degree holders also earned a vocational degree. Most logically given the pat-

Table 3: Reform effects on degree completion (non-exclusive categories)

				Disadvantaged	
	Pooled	Men	Women	Men	Women
Vocational degree by 30	0.034***	0.033**	0.034***	0.070***	0.058**
(SE)	(0.009)	(0.014)	(0.012)	(0.024)	(0.024)
Outcome mean	0.284	0.337	0.228	0.417	0.310
Academic degree by 30	-0.009	-0.035**	0.021	-0.061***	0.061**
	(0.010)	(0.014)	(0.014)	(0.022)	(0.025)
	0.558	0.472	0.648	0.269	0.467
Both HS degrees by 30	0.025***	0.005	0.046***	0.004	0.064***
	(0.005)	(0.006)	(0.007)	(0.010)	(0.014)
	0.058	0.050	0.067	0.043	0.076
No HS degree by 30	0.000	0.008	-0.008	-0.004	-0.056**
	(0.008)	(0.012)	(0.011)	(0.023)	(0.023)
	0.217	0.240	0.192	0.357	0.299
Enroll college by 30	0.011	0.014	0.010	-0.018	0.027
	(0.010)	(0.014)	(0.014)	(0.022)	(0.025)
	0.562	0.486	0.641	0.261	0.431
College degree by 30	0.003	0.005	0.003	0.001	0.009
	(0.010)	(0.013)	(0.015)	(0.016)	(0.023)
	0.356	0.271	0.445	0.124	0.273
N	339,793	173,741	166,052	57,911	55,347

Notes: Estimates come from separate difference-in-discontinuity regressions for each outcome and subgroup as described in Section 4. These degree indicators are not mutually exclusive. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

tern in the prior table, these new academic high school degree holders first enrolled in the vocational track, completed it, and then completed the new supplemental coursework that confers an academic degree and college eligibility. We confirm this pattern below. Remarkably, the reform succeeded in substantially reducing the high school dropout rate among disadvantaged women by 5.6 percentage points (from a mean of .30). While the higher percentage of disadvantaged women with an academic high school degree due to the reform also suggests the possibility of a higher share of such women completing college, we do not find strong evidence of this. The impacts of the reform on disadvantaged women's probabilities of college attendance and completion are positive but not statistically significant. The point estimates in column 5 suggest that about half of the additional disadvantaged women with academic high school degrees might have at some point attended college, but that only 15 percent completed college. It might be puzzling as to why women with a vocational degree would also complete an academic high school degree if they do not plan to at least attempt college. While the evidence in Section 3 suggests that the female-dominated high school vocational fields are not as remunerative as the male-dominated fields, the value of completing an academic high school degree

Table 4: Reform effects on highest degree completed (exclusive categories)

				Disadvantaged	
	Pooled	Men	Women	Men	Women
No degrees	-0.016*	-0.010	-0.022**	-0.013	-0.061***
(SE)	(0.008)	(0.012)	(0.011)	(0.023)	(0.023)
Outcome mean	0.197	0.225	0.168	0.349	0.282
Vocational only	0.018**	0.032**	0.003	0.065***	0.011
	(0.008)	(0.013)	(0.010)	(0.024)	(0.021)
	0.207	0.272	0.139	0.361	0.211
Academic only	-0.016**	-0.025**	-0.007	-0.048***	0.003
	(0.008)	(0.011)	(0.012)	(0.017)	(0.019)
	0.198	0.195	0.201	0.132	0.179
Vocational + Academic	0.010**	-0.001	0.023***	-0.005	0.039***
	(0.004)	(0.005)	(0.006)	(0.009)	(0.012)
	0.042	0.037	0.047	0.034	0.056
College	0.003	0.005	0.003	0.001	0.009
	(0.010)	(0.013)	(0.015)	(0.016)	(0.023)
	0.356	0.271	0.445	0.124	0.273
N	339,793	173,741	166,052	57,911	55,347

Notes: Estimates come from separate difference-in-discontinuity regressions for each outcome and subgroup as described in Section 4. These highest degree categories are mutually exclusive and exhaustive. The "No degrees" outcome in this table differs slightly from the "No HS degree by 30" outcome of Table 3 due to the roughly 2 percent of individuals in the sample who never earn a high school degree but manage to earn a college degree. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

and not going on to college does not seem to strongly dominate that of a vocational high school degree for women, at least up to the mid-30s.

Table 4 focuses on the impact of the reform on exclusive and exhaustive categories of highest educational attainment by age 30, helping to understand the linkages between the non-exclusive outcomes in Table 3. Reform 94 led to a greater percentage of men, especially disadvantaged men, finishing their schooling with a vocational high school degree, and nearly all of this increase was compensated by a decrease in the percentage of men finishing their schooling with an academic high school degree. This confirms that the reform did not succeed in reducing the share of men without any high school credentials, nor did it affect the share of men with college degrees, but rather induced a compositional shift in the types of high school degrees earned by non-college-bound men. Among women, but especially disadvantaged women, the reform did not increase the share with solely a vocational high school degree, nor did it increase the share with solely an academic degree. Instead it led to large increases in the percentage of women holding both vocational and academic high school degrees. Finally, echoing the non-exclusive degree results from Table 3, we see that the reform resulted in about a 6 percentage point drop in the share of disadvantaged women without any high school credentials but did not increase the share of women who ultimately complete a college degree.

Table 5: Reform effects on completed vocational fields

				Disadvantaged	
	Pooled	Men	Women	Men	Women
Complete services field	0.009	-0.012*	0.033***	-0.004	0.061***
(SE)	(0.007)	(0.007)	(0.012)	(0.013)	(0.023)
Outcome mean	0.135	0.063	0.210	0.068	0.288
Complete skilled trades field	0.025***	0.043***	0.001	0.076***	0.000
	(0.007)	(0.013)	(0.004)	(0.023)	(0.008)
	0.151	0.278	0.019	0.353	0.024
Complete an apprenticeship	0.042***	0.047***	0.035***	0.067***	0.046**
	(0.008)	(0.012)	(0.009)	(0.023)	(0.018)
	0.182	0.257	0.104	0.326	0.147
N	339,793	173,741	166,052	57,911	55,347

Notes: Estimates come from separate difference-in-discontinuity regressions for each outcome and subgroup as described in Section 4. Outcomes are not mutually exclusive. Services and skilled trades fields are defined in Figure 4. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

Effects on educational pathways.

The remaining results in this subsection explore in greater detail the content of the newly completed vocational degrees under Reform 94 and probe further into which specific educational pathways were impacted by the reform. First, as documented above, fields of specialization in vocational education are very segregated by gender. Table 5 shows that Reform 94 did not change this pattern: in fact, virtually all of the additional vocational degrees completed by women were in service fields, and the additional male degrees continued to concentrate in skilled trades. Table 5 also shows that both men and women, but especially disadvantaged men, were substantially more likely to complete an apprenticeship as part of their high school education; recall that an important aspect of Reform 94 was to increase the supply of quality apprenticeships for vocational students. Table A.3 further links these two results by showing that the increase in apprenticeship completion was driven by men completing more skilled trade degrees with an apprenticeship component and by women completing more service degrees with an apprenticeship component.

Table A.4 shows that Reform 94 also increased persistence on the vocational track. Specifically, the reform increased the joint likelihood of both enrolling in and completing vocational high school, especially among disadvantaged men, and decreased the joint likelihood of both enrolling in and dropping out of vocational high school, especially among disadvantaged women. On the one hand, this shows that Reform 94 succeeded in reducing dropout rates among those that enrolled in high school, which was one key goal of the reform. On the other hand, this is also consistent with our prior inference that few of the additional youth drawn into high school via the strengthening of the vocational track leveraged the added flexibility to transfer out of the vocational track and into the academic track prior to completing their first degree.

Finally, Tables A.5 and A.6 report on how women's, especially disadvantaged women's, educational

trajectories were impacted by Reform 94. Table A.5 shows that virtually all of the net increase in women completing both vocational and academic high school degrees (as seen in Table 4) is driven by women who first obtain their vocational high school degree and then complete the supplemental coursework required to also receive an academic degree and college eligibility. Second, Table A.6 shows that Reform 94, while boosting the share of disadvantaged women with academic high school degrees, did not affect the share of women going beyond such academic high credentials and leveraging their eligibility for tertiary education with a college degree. In other words, while the reform succeeded in inducing more disadvantaged women into completing high school and opening college doors to them, these new opportunities did not translate into higher rates of tertiary education.

5.2 Labor market impacts of the reform

Table 6 presents the effects of Reform 94 on labor market outcomes. As for our analysis of educational outcomes, we present pooled results (column 1), results by gender (columns 2 and 3) and results by gender for disadvantaged students (columns 4 and 5). Our main outcome of interest is average real annual earnings between the ages of 25 and 34 (Panel A), which we obtain from the tax data. Overall, students enrolled under the new educational regime experienced higher earnings, but the magnitude under the pooled specification is not large, corresponding to about a 2 percent increase. The analysis by subgroup, however, shows that the labor market effects were not uniform. In particular, the labor market gains are concentrated among men, and in particular disadvantaged men. The average annual earnings of disadvantaged men increased by 5 percent (2,329 USD). In contrast, we see no average impact for women. When we focus on disadvantaged women (column 5), we estimate a roughly 2 percent increase in earnings, but this effect is not statistically significant. On average across all men and women (columns 2 and 3), the reform thus appears to have exacerbated the gender gap in real annual earnings between the ages of 25 and 34 by about 8 percent.

Panel B of Table 6 focuses on individuals for whom we can observe hours of work, and thus compute hourly wages, in the matched employer-employee administrative data.[12] This data is only available from 2003-2014, so we are constrained to averaging these outcomes over the ages of 28 to 33. The first row of Panel B shows that the annual earnings changes we estimate in the matched employer-employee data among this more limited age range and sample are similar to those estimated in the universal tax data in Panel A, with slightly larger estimated gains for men. Rows 2 and 3 of Panel B show that the male earnings gains can be decomposed into both an increase in average hourly wage and an increase in hours worked, with disadvantaged men working over an hour more per week post-reform.

The fact that our analysis cohorts are around age 16 at the time of the reform in 1994 limits our ability to study labor market effects at ages beyond the mid-30s. Looking at effects at later ages could be relevant, though, as the labor market returns to vocational education might decline more rapidly over time than those of general education (Hanushek et al., 2017; Hampf and Woessmann, 2017). In Panel C of Table 6, we are able to extend the analysis a bit further to age 37 by restricting the control group to the older pre-reform cohorts. Note first that the earnings gains we observe between the ages 25 and 34 (row 1) are similar under this alternative control group specification to those we observed

[12] We find no evidence of reform-induced sample selection bias in hourly wages, as the reform has no impact on appearing in the employer-employee data.

Table 6: Reform effects on labor market outcomes

				Disadvantaged	
	Pooled	Men	Women	Men	Women
Panel A: Full sample					
Annual earnings 25-34	867*	1,322*	196	2,329**	754
(SE)	(465)	(693)	(535)	(1,132)	(871)
Outcome mean	43,831	50,755	36,586	47,108	31,685
N	339,793	173,741	166,052	57,911	55,347
Panel B: Wage sample					
Annual earnings in wage data 28-33	1,197**	1,955***	230	3,167***	875
	(487)	(725)	(547)	(1,113)	(849)
	46,722	54,297	38,850	49,956	34,047
Hourly wage 28-33	0.47*	0.87**	0.02	0.85	0.04
	(0.24)	(0.35)	(0.33)	(0.56)	(0.52)
	30.42	31.77	29.03	29.44	26.83
Hours per week 28-33	0.40**	0.56**	0.16	1.17***	0.37
	(0.19)	(0.23)	(0.27)	(0.42)	(0.50)
	30.49	33.19	27.67	33.04	26.43
N	315,818	160,957	154,861	52,477	50,187
Panel C: Older control cohorts only					
Annual earnings 25-34	943*	1,366*	386	2,754**	748
	(488)	(726)	(561)	(1,181)	(910)
	42,298	49,260	35,023	45,574	30,353
Annual earnings 35-37	596	1,047	-30	2,807	717
	(768)	(1,174)	(874)	(1,810)	(1,363)
	57,380	67,257	47,061	58,521	39,861
N	197,301	100,814	96,487	33,603	32,160

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. Outcomes are averaged within person over the specified ages; age ranges vary across outcomes due to different start and end years of data availability. Panel B is limited to individuals who appear in employer-employee spell data, which excludes informal and self-employment. Panel C is limited to the pre-reform 1975-1977 control window cohorts and the 1978 reform window cohort, excluding the post-reform 1979-1981 control window cohorts to allow for later common ages of earnings measurement. All monetary amounts are measured in real 2015 U.S. dollars. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender. See Table A.1 for more details on data sources and variable definitions.

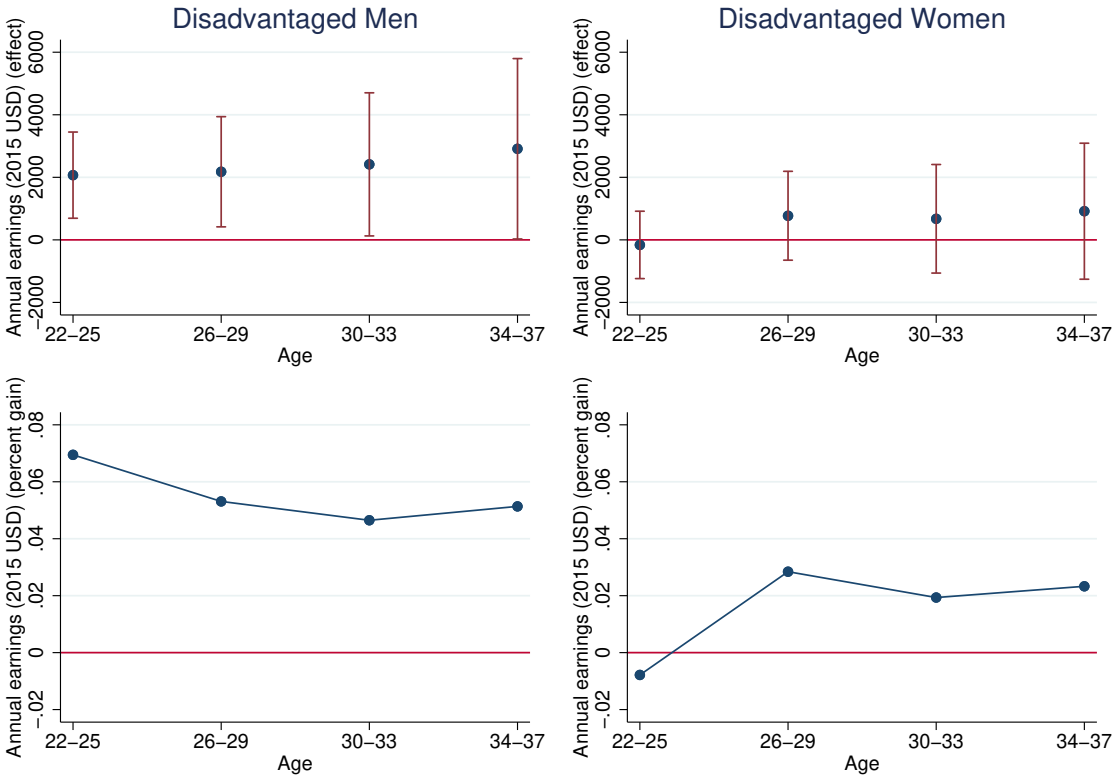


Figure 15: Reform effects on earnings across ages: disadvantaged men and women

Notes: The top figures plot difference-in-discontinuity earnings effect estimates (and robust 90% confidence intervals) separately by age bins. Only the older control cohorts are used at the latest ages (34-37), since earnings data are not yet available for the younger control cohorts at these ages. The bottom figures plot the reform effect estimate divided by the mean counterfactual earnings level at that age to measure the effect in percentage terms. We calculate the mean counterfactual earnings level by evaluating the estimated regression function in Equation (1) at the eligibility cutoff ($Day = 0$) assuming zero reform effect ($\beta = 0$), i.e. $\hat{\alpha} + \hat{\delta}_1 + \hat{\delta}_2$. All monetary amounts are measured in real 2015 U.S. dollars. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

under the main specification. Row 2 then focuses on average earnings between the extended ages of 35 and 37. While more noisily estimated, the magnitudes of the earnings gains for men are comparable at these older ages to those at younger ages. The effects for disadvantaged women remain positive (about a 2 percent gain) but statistically insignificant.

Additional evidence of persistence over time in the labor market gains for disadvantaged men can be gleaned from Figure 15, where we plot the earnings effects by age for disadvantaged men and women. Among disadvantaged men, we observe positive and stable earnings effects across ages 22-37 (top panel), though as a percentage of the mean counterfactual earnings level (bottom panel), the greatest gains occur in the early years after high school and decline somewhat thereafter.[13] In summary,

[13] We calculate the mean counterfactual earnings level by evaluating the estimated regression function in Equation (1) at the eligibility cutoff ($Day = 0$) assuming zero reform effect ($\beta = 0$), i.e. $\hat{\alpha} + \hat{\delta}_1 + \hat{\delta}_2$.

while we of course cannot assess impacts even later in life, Figure 15 confirms that the reform causes disadvantaged men to experience higher earnings throughout the first two decades of their adult life. As expected, we see no statistically significant earnings effects across ages for disadvantaged women, though the point estimates suggest a roughly 2 percent steady gain after an initial transition period in the early 20s.

Figure A.3 replicates Figure 15 for non-disadvantaged men and women, the complement of the disadvantaged sample. It is apparent from all panels that the pooled effects of the reform across all men are very much driven by the subset of disadvantaged men. The effects on non-disadvantaged men's and women's earnings are negligible across all ages. In other words, the reform improved social mobility, especially among men. Based on the point estimates reported in Table A.7, the gap in real annual earnings (ages 25 to 34) between disadvantaged and non-disadvantaged students went down by 20 percent, and by close to 30 percent when we focus on male students only.

5.3 Linking the education and labor market effects

We now link the previous results by exploring how the educational changes induced by the reform map into the documented labor market changes. Recall that, among men, the main effect of the reform was to induce a net switch from completing academic high school to completing vocational high school, with no significant effects on high school or college completion rates. If we assume that the net zero dropout and college effects imply no effects on these margins (i.e., the selection into dropping out and completing college did not change and the returns to dropping out or completing college did not change), then it is clear that the reform increased labor market earnings for men by increasing the net returns to high school. These gains in the returns to high school could be due to a shift of students from the academic track towards the (more remunerative) vocational track, a better sorting of students between the vocational and academic tracks, an increase in the returns to the vocational track (the only educational track whose content was impacted by the reform), or any combination of these three forces.

While we cannot precisely quantify the relative contributions of these three forces, a Oaxaca-Blinder-type decomposition provides some guidance about the relevance of the first force. In particular, we can assess how much of the observed labor market gains of the reform can be accounted for by changes in students' educational attainment, holding the earnings payoffs to each type of education fixed at their pre-reform levels. To do so, we multiply the estimated changes in the probability of each highest degree category (college, both high school degrees, academic high school degree only, vocational high school degree only, no high school completed) from Table 4 by the mean potential earnings associated with each of those degrees in the pre-reform period. We use OLS regressions in the pre-reform period to estimate the mean potential earnings associated with each degree, adding as controls the same battery of family background covariates as in Figure 13 (parental education and income, immigration status, number of siblings) as well as a linear cohort trend. While row 1 of Table 7 reproduces the earnings effect estimates from Table 6, row 2 shows what earnings effects we would have predicted solely due to the educational compositional shifts, holding the earnings payoffs to each type of education fixed at their pre-reform levels.

Only a small share (about 20 percent, 490/2329) of the earnings gains for disadvantaged men can

Table 7: Decomposition of earnings effects

				Disadvantaged	
	Pooled	Men	Women	Men	Women
Actual earnings effect	867	1,322	196	2,329	754
Predicted effect due solely to compositional changes in degrees, holding returns and selection fixed	296	340	259	490	726

Notes: Actual earnings effects come from Table 6. Predicted earnings effects come from multiplying the estimated changes in the probability of each highest degree category (college, both HS, academic HS, vocational HS, none) by estimated mean potential earnings associated with those degrees, where mean potential earnings are estimated via OLS in the pre-reform cohorts with the usual family background controls (parental education and income, immigration, and siblings) and a linear cohort trend.

be accounted for by the switch out of academic high school degrees and into vocational degrees.[14] In other words, we can only make sense of the earnings gains for men if the reform increased the value of vocational degrees in the labor market, or if we allow for the possibility that the marginal men who switched out of academic high school degrees and into vocational degrees were particularly well suited to materially benefit from vocational education.

We perform a similar back-of-the-envelope calculation for women in columns (3) and (5) of Table 7. Recall the reform reduced the likelihood of disadvantaged women dropping out of high school and increased their likelihood of obtaining both an academic and a vocational high school degree, with no measured effect on college completion. Focusing on disadvantaged women where we see the largest (but not significant) earnings gains among women, row 2 suggests that essentially all of these gains (726 vs. 754) could be rationalized only via the change in educational achievement (fewer women with no high school degrees, more women with academic and vocational high school degrees), without a required increase in the returns to vocational high school for women, in contrast with what we observe for men.

5.4 Impacts of the reform on social outcomes

While our focus so far has been on educational and labor market outcomes, the reform may have had broader social benefits. In particular, by inducing young men and women, especially more disadvantaged ones, to spend more time at school in the teenage years, the reform may have reduced the incidence of risky behavior. We examine these additional outcomes in Table 8. The first outcome we consider is involvement with the criminal justice system. Panel A of Table 8 studies the effect of the reform on the likelihood of facing any criminal charge between age 16 and 30.[15] We observe a sharp reduction in criminal activities for disadvantaged men. Not surprisingly, this is the group with the highest propensity to commit crimes, with 26.6% of them facing some criminal charges by age

[14] Disaggregating the vocational degree category by field does not increase the estimates in the second row of Table 7, suggesting that the earnings gains are also not well explained by switching across fields within the vocational track.

[15] Criminal charges include drug, property, economic, and violent crimes, and exclude misdemeanors like traffic violations and drunk driving. We explore these various types of charges in more detail below in Table A.8.

Table 8: Reform effects on social outcomes

				Disadvantaged	
	Pooled	Men	Women	Men	Women
Panel A: Criminal charges sample					
Any criminal charges 16-30	-0.017**	-0.039***	0.004	-0.052**	-0.001
(SE)	(0.007)	(0.012)	(0.007)	(0.023)	(0.014)
Outcome mean	0.121	0.192	0.047	0.266	0.072
N	189,319	97,042	92,277	32,346	30,757
Panel B: Full sample					
Teenage parenthood	-0.004	-0.002	-0.006	-0.004	0.006
	(0.003)	(0.003)	(0.006)	(0.006)	(0.014)
	0.025	0.009	0.042	0.014	0.074
Married by age 34	-0.004	0.004	-0.012	-0.006	-0.026
	(0.010)	(0.014)	(0.015)	(0.023)	(0.025)
	0.436	0.378	0.496	0.347	0.473
N	339,793	173,741	166,052	57,911	55,347
Panel C: Married sample					
Age at first marriage	0.326***	0.059	0.509***	-0.029	1.201***
	(0.116)	(0.162)	(0.160)	(0.312)	(0.298)
	28.136	28.873	27.547	28.605	27.062
Spouse has college degree	0.013	0.030	-0.006	0.018	0.045
	(0.015)	(0.024)	(0.020)	(0.039)	(0.029)
	0.378	0.474	0.302	0.329	0.176
N	143,818	63,870	79,948	19,375	25,199

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. Sample sizes are smaller for criminal charges due to data availability, as criminal charge histories are only available for more recent cohorts. Criminal charges include drug, property, economic, and violent crimes, and exclude misdemeanors like traffic violations and drunk driving. Marriage is measured through age 34 as this is the oldest commonly observable age across our main analysis cohorts. Married sample is conditional on being married by age 34 and spouse appearing in registry data. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

30. The reform significantly reduces this propensity by 5.2 percentage points, which is roughly a 20% decrease. We see no comparable effects on criminal charges for disadvantaged women, who have a much lower rate of participation in criminal activity at baseline (7.2%).

We further explore the decrease in male criminal charges along three dimensions. First, the rows of Table A.8 break out charges by category (violent, property, economic, drug, and other), showing reductions across the board with no clear concentration in any particular type of criminal offenses. In contrast, we see no significant effects of the reform on non-criminal misdemeanor charges (e.g. traffic

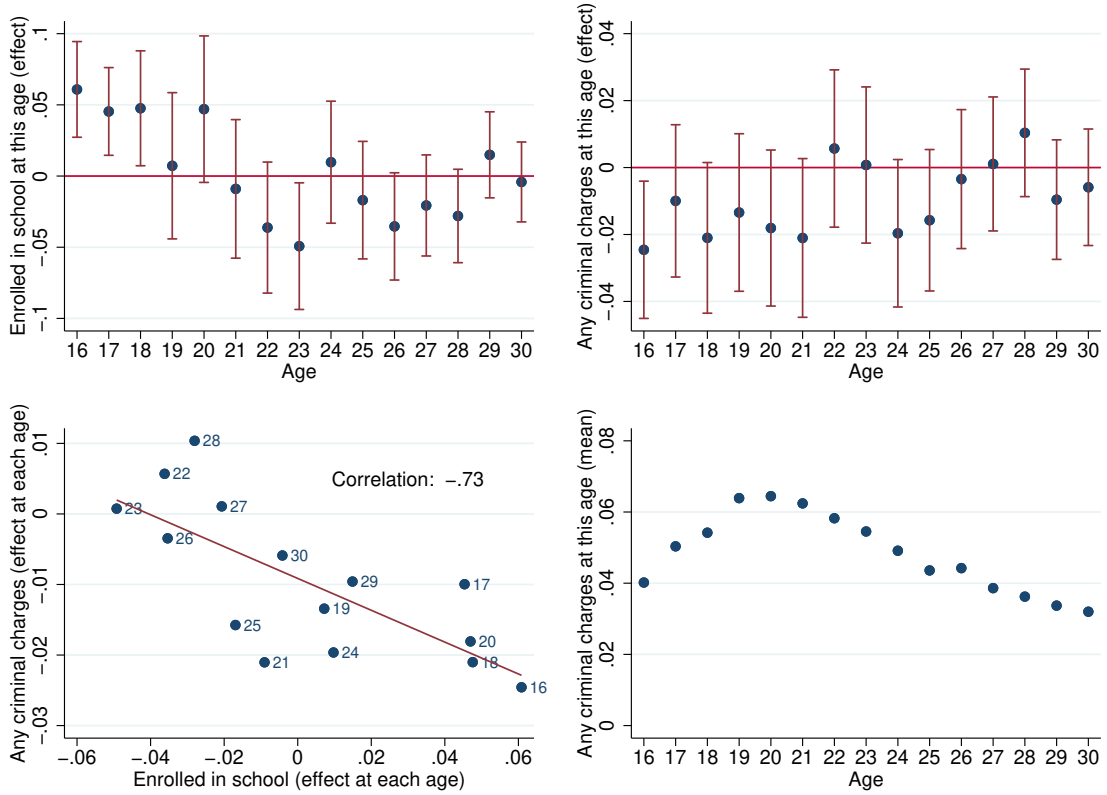


Figure 16: Reform effects on criminal charges and school enrollment at each age: disadvantaged men

Notes: The top left panel plots reform effect estimates on the probability of being enrolled in school at each age, with each estimate coming from a separate difference-in-discontinuity regression as described in Section 4. The top right panel conducts the same exercise for the outcome of facing any criminal charges at that age, and the bottom right panel plots the mean of that outcome at each age. The bottom left panel scatters the top two series against each other by age. Sample is limited to disadvantaged men, i.e. those in the bottom third of the male distribution of predicted GPA.

violations), suggesting that the reform managed to reduce more costly crimes rather than simply petty offenses. Second, the columns of Table A.8 show that just as the reductions in criminal charges tend to be disproportionately driven by men with low predicted GPAs, they also tend to be disproportionately driven by men with high criminal propensity, predicted using the same family background covariates and random forest specification as GPA.[16] Third, Figure 16 offers suggestive evidence that this drop in criminal activity is driven in part by an incapacitation channel. While the reform did not ultimately increase male high school completion by age 30, the top left panel of Figure 16 shows that it did cause many disadvantaged men to start high school earlier, increasing the likelihood of being enrolled in their late teenage years—just as criminal activity builds and peaks (bottom right panel)—and somewhat decreasing the likelihood of being enrolled in their 20s. The reform's effects on criminal charges at each age, plotted in the top right panel, roughly follow the inverse of the schooling pattern, yielding a negative correlation between the two series as confirmed in the bottom left plot.

[16]See footnote 3 for the details of this specification.

The final two panels of Table 8 study the impacts of the reform on teenage parenthood and marriage outcomes. Teen parenthood is relatively rare in Norway, with only 2.5% of individuals becoming parents during their teenage years. The first row of Panel B shows no economically or statistically significant effect of the reform on teen parenthood, even among the most at-risk group (disadvantaged women). We also see no statistically significant effects on the likelihood of being married by age 34, the latest commonly observable age among our analysis cohorts. Conditional on being married, however, the results in Panel C show that women, especially disadvantaged women, significantly delay their age at first marriage in the wake of the reform. Furthermore, while not statistically significant, we find suggestive evidence that the educational gains for disadvantaged women were accompanied by higher quality matches in the marriage market: a greater share of disadvantaged women marry college-educated men post-reform (4.5 percentage point effect, from a baseline of .176). One interpretation is that by reducing the odds of dropping out of high school, the reform exposed these disadvantaged women to a more educated pool of men in the dating market.

5.5 Specification checks

The preceding subsections have offered evidence that Reform 94 substantially impacted the educational pathways and degree attainment of disadvantaged men and women, as well as earnings and criminal charges among disadvantaged men. In this subsection, we conduct several specification checks to investigate the robustness of these main results.

Tables A.9 and A.10 compare our main estimates for disadvantaged men and women, respectively, to those obtained from several alternative specifications. Column (1) reproduces the estimates from our baseline difference-in-discontinuity specification described in Section 4. Column (2) adds the full set of family background controls: categorical indicators for mother's education, father's education, mother's long-run earnings decile, father's long-run earnings decile, child immigrant dummy, parent immigrant dummy, and a quadratic in number of siblings. Column (3) generalizes the baseline specification to allow the school starting age effect to vary across cohorts by including a linear cohort trend and its interaction with the $Post_i$ indicator for birth after January 1st within each RD window. To probe any sensitivity to the selection of control cohorts, column (4) limits the control cohorts to those three born before the 1978 reform window (1975-1977), and column (5) limits the control cohorts to those born after the reform window (1979-1981). All of these alternative specifications deliver similar results to our baseline specification in column (1).

The next set of results explore whether the main estimates are sensitive to our choice of bandwidth in the locally linear difference-in-discontinuity design. From our baseline bandwidth using all six months (roughly 182 days) on each side of the January 1st birthdate cutoff in each RD window, column (6) in Tables A.9 and A.10 reduces this bandwidth to 150 days, and column (7) to 120 days. Figures A.4 and A.5 visualize and expand upon these results by plotting point estimates and 95% confidence intervals across the range of bandwidths from 60 days to 180, showing that our six-month bandwidth improves precision relative to smaller bandwidths without changing the qualitative conclusions of the results.

Tables A.11 and A.12 compare reform effect estimates for disadvantaged men and women across alternative definitions of disadvantage. Compared to our main stratification based on predicted 10th

grade GPA reproduced in column (1), estimates are broadly similar, though in some cases attenuated, when using coarser definitions of disadvantage based on simple categories of parental education in columns (2) and (3), parental income in column (4), and their intersection in column (5).

Finally, in Figures A.6 and A.7, we compare our point estimates to placebo estimates from alternative eligibility cutoffs. Specifically, each placebo estimate comes from a difference-in-discontinuity regression with a placebo date cutoff instead of the actual January 1st cutoff, and we loop over the different birth years to assign placebo "reform" cohorts. The placebo cutoff dates range from 120 to 240 days after January 1st within each calendar birth year, which allows for enough data on each side of each cutoff within each birth year window (at least 120 days) while also avoiding including the actual January 1st cutoff. The results, plotted in Figures A.6 and A.7, reinforce the conclusion that the significant impacts of Reform 94 that we estimate are unlikely to have arisen by chance.

6 Concluding remarks

This paper contributes to an important debate on how secondary education can best be structured to improve social mobility, including addressing the needs of students not on clear pathways toward higher education. This debate is ongoing in the U.S. where, after decades of declines in career and technical education (CTE) funding and enrollments, many states have recently passed laws aimed at slowing or reversing these trends (Jacob, 2017). The push to revive CTE education in the U.S. has been accompanied by efforts to make more apprenticeships accessible to these students during high school.[17] There has also been a resurgence of interest in the decades-old Career Academies model, with federal grants going to school districts that are prepared to develop curricula that combine career/technical learning with academic coursework that meets high school graduation and college entrance requirements.[18]

We believe that the Norwegian experience from the mid-1990s is highly relevant to this current debate, as it uniquely mixed an effort to strengthen vocational education (in particular via access to more apprenticeship opportunities) with a desire, just like under the Career Academies model, to offer vocational students a clear and feasible path to college, departing from more rigid two-track models of high school education.

Our analysis of Reform 94 suggests that the biggest concern raised by Dewey (1916) and his followers, that strengthening vocational education would ultimately hurt disadvantaged youth, appeared unfounded. In particular, Reform 94 did not reduce college-going among disadvantaged youth. Moreover, it provided a higher quality and more attractive high school option that led to higher earnings in adulthood and lower involvement with the criminal justice system among men. Reform 94 also succeeded in reducing high school dropout among disadvantaged women.

On the other hand, we do not find a quantitatively meaningful additional upside in facilitating college enrollment by lowering switching costs between the vocational to academic tracks. Men did not make much use of the flexibility that was added into the system to allow them to convert an initial vocational high school enrollment into a general education. Women, in contrast, did embrace

[17] E.g. https://ctepolicywatch.acteonline.org/2018/05/new-grant-to-connect-apprenticeships-and-secondary-cte.html.
[18] E.g. https://www2.ed.gov/about/overview/budget/budget13/crosscuttingissues/careeracademies.pdf.

this new flexibility, with an increase in the share of women first engaging with high school via the vocational offerings but ultimately completing both vocational and academic high school degrees, and thus an increase in the share of women with the credentials required to attend college. Unfortunately, these gains in college eligibility among disadvantaged women did not translate into meaningful gains in college completion rates.

Our back-of-the-envelope calculation suggests that men mainly benefited from the reform due to improvement in the quality of the vocational track, or perhaps better sorting between the academic and vocational tracks. While nailing down the exact mechanism is not possible, the sharp contrast between our findings for this group and the findings in earlier studies of educational reforms that were mainly focused on bringing more general schooling into vocational tracks (e.g. Oosterbeek and Webbink, 2007; Hall, 2012; Zilic, 2018) leads us to suspect that the gains for men might be more directly related to the stronger apprenticeship system and other improvements in the vocational offerings, rather than the inclusion of more general academic courses into the vocational curriculum.

Much of the difference in terms of how much men financially benefit from accumulating more vocational education compared to women can be traced to sharp gender segregation across vocational fields, with men overwhelmingly concentrating in higher-paying skilled trade jobs and women concentrating almost exclusively in lower-paying service-sector jobs. Such strong sorting by gender is not specific to Norway, nor are the differences in earnings between these male- and female-dominated fields (Carnevale et al., 2012), which suggests that any attempt to make vocational education also "work" for women may need to be accompanied by further efforts to make stereotypically male fields more appealing to young women. It is also possible, however, that changes in the occupational structure of the economy induced by technological change, with manufacturing jobs being more prone to automation than, say, health care jobs, will undo some of these gender differentials in the returns to vocational education, even without changes in gender sorting. In any case, our findings strongly suggest that considerations related to differential benefits by gender should be an integral part of the policy conversation surrounding vocational education.

One of the most disappointing aspects of the Norwegian reform might be that while it succeeded in making a greater share of disadvantaged women college-eligible it failed at ultimately increasing their college completion rates. This result suggests that additional efforts to support the college application process, as well as to encourage persistence while in college, might be needed for the potential of these newly college-eligible women to be fully realized.

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Online appendix

Table A.1: Variable details

Variable	Description
Education	Source: National Education Database
10th grade GPA	Grade point average in the final year of compulsory schooling (10th grade, age 15). Only available for 1985 birth cohort (and younger); predicted in earlier cohorts.
Enroll in vocational/academic track	Indicator for enrolling in a vocational/academic high school track at a given age.
Complete vocational/academic degree	Indicator for completing a vocational/academic high school degree before age 30.
Vocational field of study	For each vocational enrollment/degree, an indicator for one of eight field of study categories: Trades: industrial, electrical, construction, agriculture/fishing/forestry. Services: sales/service/communication, hotel/food, arts/crafts/design, health/social care.
Complete apprenticeship	Indicator for completing an apprenticeship as part of a vocational high school track.
Enroll in college	Indicator for enrolling in college anytime before age 30.
Complete college degree	Indicator for completing a college degree anytime before age 30.
Labor market outcomes	Source: Tax Records and Employer-Employee Matched Panel
Earnings	Annual pre-tax labor earnings, deflated to 2015 USD using Statistics Norway annual CPI and 2015 NOK-USD exchange rate of 8.0739. When taking within-person averages, we ignore outlier observations above the 99.9th percentile and below the 0.1th percentile. The lower cutoff only eliminates negative outliers (not zeros), often self-employment losses. Earnings estimates are very similar if we winsorize these outliers instead of dropping them.
Hours worked per week	Total hours worked each calendar year divided by 52. Available from 2003, when oldest analysis cohort (1975) is age 28, to 2014, when youngest cohort (1981) is age 33.
Hourly wage	Total real earnings in employer-employee data divided by total hours worked that calendar year. Outliers above 99th and below 1st percentile are dropped. Same age restrictions as hours worked.
Social outcomes	Source: Administrative Police Records, Central Population Register
Any criminal charges	Indicator for formal accusation of criminal activity committed in a given calendar year associated with a given age. Only available for younger analysis cohorts due to data beginning in 1992.
Teenage parenthood	Indicator for a new child appearing in the Central Population Register between the ages of 15-19.
Married by age 34	Indicator for any marriage appearing in the Central Population Register by age 34.
Age at first marriage	Conditional on being married by age 34, age at which first marriage appears.
Spouse has college degree	Conditional on being married by age 34, indicator for spouse having a college degree.
Demographics & family background	Source: Central Population Register, National Education Database, Tax Records
Birth date	Exact date of birth in the population register.
Birth year	Calendar year of birth date.
RD window year	Year of birth if born in first half of the year; year of birth plus one if born in the second half.
Age	For educational and criminal charge outcomes, age is year of measurement minus year of birth. For labor market outcomes in difference-in-discontinuity specifications, the year of outcome measurement is held fixed within each RD window (birth dates within ± 6 months of January 1st) to eliminate a discrete change in year of measurement when moving across the cutoff.
Gender	Female or male, as measured at age 16 in Central Population Register.
Parental education	Categories for the highest education level of each parent: compulsory only, vocational high school dropout, academic high school dropout, vocational high school degree, academic high school degree, folk high school degree, post-secondary vocational degree, bachelor's degree, master's degree, doctoral degree. Measured when student is age 16.
Parental earnings	Long-run average of mother and father real earnings over the 20 years prior to child's 16th birthday.
Immigrant	Indicator for individual having a country of birth other than Norway.
Immigrant parent	Indicator for at least one parent having a country of birth other than Norway.
Number of siblings	Count of other individuals in the Central Population Register who have the same mother identifier.

Table A.2: Reform effects on predetermined covariates

				Disadvantaged	
	Pooled	Men	Women	Men	Women
At least one college-educated parent	0.015	0.004	0.027*	-0.002	0.004
(SE)	(0.010)	(0.013)	(0.014)	(0.005)	(0.007)
Outcome mean	0.328	0.329	0.326	0.012	0.021
Parental income	458	129	798	-553	704
	(422)	(596)	(597)	(678)	(699)
	55,281	55,231	55,333	41,454	41,807
At least one immigrant parent	0.000	-0.003	0.002	-0.005	0.011
	(0.003)	(0.005)	(0.005)	(0.009)	(0.011)
	0.029	0.027	0.031	0.033	0.043
Child is an immigrant	0.004	-0.001	0.009	-0.007	0.022
	(0.005)	(0.007)	(0.008)	(0.013)	(0.014)
	0.076	0.075	0.077	0.069	0.084
Number of siblings	0.01	0.04	-0.02	0.09	-0.03
	(0.02)	(0.03)	(0.03)	(0.06)	(0.06)
	1.65	1.65	1.65	1.81	1.77
N	339,793	173,741	166,052	57,911	55,347

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. Family background variables are measured at age 16 just prior to high school entry. Parental income is a long-run average of combined mother and father earnings over the twenty years prior to the child turning 16. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

Table A.3: Reform effects on apprenticeship completion within fields

		All Women	Disadvantaged Women
Complete services field and:	Complete apprenticeship	0.042***	0.055***
	(SE)	(0.009)	(0.018)
	Outcome mean	0.094	0.135
	Do not complete apprenticeship	-0.008	0.006
		(0.009)	(0.018)
		0.116	0.154
		All Men	Disadvantaged Men
Complete skilled trades field and:	Complete apprenticeship	0.035***	0.054**
		(0.012)	(0.022)
		0.226	0.287
	Do not complete apprenticeship	0.008	0.022*
		(0.007)	(0.013)
		0.052	0.065

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. Second column outcomes are mutually exclusive and exhaustive conditional on the first column, providing a decomposition of the reform effect on the first column outcome. Outcomes are measured as of age 30. Services and skilled trades fields are defined in Figure 4. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

Table A.4: Reform effects on vocational persistence

					Disadvantaged	
		Pooled	Men	Women	Men	Women
Ever enroll in vocational and:	Complete vocational	0.034***	0.033**	0.034***	0.070***	0.058**
	(SE)	(0.009)	(0.014)	(0.012)	(0.024)	(0.024)
	Outcome mean	0.284	0.337	0.228	0.417	0.310
	Drop out of vocational	-0.041***	-0.054***	-0.026**	-0.055**	-0.074***
		(0.009)	(0.012)	(0.013)	(0.024)	(0.024)
		0.268	0.263	0.274	0.365	0.367

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. Second column outcomes are mutually exclusive and exhaustive conditional on the first column, providing a decomposition of the reform effect on the first column outcome. Outcomes are measured as of age 30. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

Table A.5: Reform effects on women completing both high school tracks: which order?

		All Women	Disadvantaged Women
	Vocational first	0.051***	0.070***
	(SE)	(0.006)	(0.011)
Complete both vocational & academic:	Outcome mean	0.034	0.041
	Academic first	-0.003	-0.002
		(0.005)	(0.009)
		0.035	0.037

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. Second column outcomes are mutually exclusive and exhaustive conditional on the first column, providing a decomposition of the reform effect on the first column outcome. Outcomes are measured as of age 30. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

Table A.6: Reform effects on disadvantaged women completing high school: on to college as well?

		Disadvantaged Women
	College degree	0.003
	(SE)	(0.022)
Complete high school degree and:	Outcome mean	0.256
	No college degree	0.053**
		(0.025)
		0.446

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. Second column outcomes are mutually exclusive and exhaustive conditional on the first column, providing a decomposition of the reform effect on the first column outcome. Outcomes are measured as of age 30. Disadvantaged is an indicator for being in the bottom third of the distribution of predicted GPA within each gender.

Table A.7: Reform effects on earnings for expanded groups

	Disadvantaged		Not disadvantaged		Disadvantaged	Not disadvantaged
	Men	Women	Men	Women	(both genders)	(both genders)
Earnings 25-34	2,329**	754	689	-305	1,712**	273
(SE)	(1,132)	(871)	(865)	(654)	(771)	(572)
Outcome mean	47,108	31,685	52,578	39,037	39,571	45,961
N	57,911	55,347	115,830	110,705	113,258	226,535

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. All monetary amounts are measured in real 2015 U.S. dollars. Disadvantaged sample is comprised of the bottom third of the distribution of predicted GPA within each gender; not disadvantaged sample is the complement.

Table A.8: Reform effects on male criminal charges: by offense categories and crime propensity

		Stratified by predicted GPA		Stratified by predicted crime	
	All men	Bottom tercile	Top two terciles	Top tercile	Bottom two terciles
Any criminal charges 16-30	-0.039***	-0.052**	-0.030**	-0.044*	-0.017**
(SE)	(0.012)	(0.023)	(0.013)	(0.026)	(0.008)
Outcome mean	0.192	0.266	0.154	0.474	0.050
Any violent crime charges	-0.016**	-0.023	-0.012	-0.029	-0.003
	(0.008)	(0.016)	(0.008)	(0.020)	(0.004)
	0.069	0.106	0.050	0.178	0.014
Any property crime charges	-0.018**	-0.021	-0.015*	-0.027	-0.006
	(0.008)	(0.017)	(0.008)	(0.020)	(0.004)
	0.072	0.115	0.051	0.188	0.015
Any economic crime charges	-0.013**	-0.017	-0.010*	-0.025	-0.003
	(0.006)	(0.012)	(0.006)	(0.015)	(0.003)
	0.035	0.055	0.025	0.091	0.007
Any drug crime charges	-0.025***	-0.029*	-0.022**	-0.032	-0.013**
	(0.008)	(0.016)	(0.010)	(0.021)	(0.006)
	0.092	0.121	0.078	0.226	0.026
Any other crime charges	-0.012	-0.027*	-0.002	-0.012	-0.004
	(0.007)	(0.016)	(0.008)	(0.019)	(0.004)
	0.065	0.098	0.048	0.165	0.015
Any misdemeanor charges 16-30	-0.011	-0.024	0.000	-0.019	0.006
	(0.014)	(0.026)	(0.017)	(0.026)	(0.016)
	0.339	0.434	0.292	0.519	0.249
N	97,042	32,346	64,696	32,350	64,692

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. The criminal charge categories of violent, property, economic, drug, and other exhaustively comprise the "any criminal charges" outcome in the top row, but these categories are not mutually exclusive, since an individual can be charged in more than one category over ages 16-30. Misdemeanor charges in the bottom row are not included in criminal charges. Common offenses in each charge category include the following. Violent: assault, sex offenses, robbery. Property: larceny, motor vehicle theft, trafficking in stolen property. Economic: fraud, forgery and counterfeiting, embezzlement. Drug: all drug-related offenses. Other: vandalism, threats, offenses against public officials. Non-criminal misdemeanors: traffic violations, petty larceny, driving under the influence. Section 2.2 describes GPA prediction via random forest. "Predicted crime" comes from the same random forest prediction algorithm as GPA, but exchanging the GPA outcome with an indicator for any criminal charges over ages 16-30.

Table A.9: Reform effects across different specifications: disadvantaged men

	Baseline	Add control variables	Add cohort trend in starting age effect	Older control cohorts only 1975-1977	Younger control cohorts only 1979-1981	150-day RD bandwidth	120-day RD bandwidth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Enroll in vocational at 16	0.118***	0.113***	0.119***	0.134***	0.103***	0.115***	0.127***
(SE)	(0.024)	(0.024)	(0.024)	(0.026)	(0.026)	(0.027)	(0.030)
Complete vocational HS degree	0.070***	0.063***	0.070***	0.078***	0.061**	0.081***	0.064**
	(0.024)	(0.024)	(0.024)	(0.026)	(0.026)	(0.027)	(0.030)
Complete academic HS degree	-0.061***	-0.059***	-0.061***	-0.076***	-0.046*	-0.064***	-0.079***
	(0.022)	(0.022)	(0.022)	(0.023)	(0.024)	(0.024)	(0.027)
No degrees	-0.013	-0.009	-0.013	-0.007	-0.019	-0.017	0.017
	(0.023)	(0.023)	(0.023)	(0.025)	(0.025)	(0.026)	(0.029)
Earnings 25-34	2,329**	2,223**	2,368**	2,754**	1,975	2,014	1,661
	(1,132)	(1,105)	(1,132)	(1,181)	(1,240)	(1,257)	(1,402)
Any criminal charges 16-30	-0.052**	-0.048**	-0.052*	N/A	-0.052**	-0.064**	-0.075***
	(0.023)	(0.023)	(0.027)	N/A	(0.023)	(0.025)	(0.028)
N	57,911	57,911	57,911	33,603	32,346	47,430	37,771
N for criminal charges 16-30	32,346	32,346	32,346	N/A	32,346	26,519	21,011

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. Estimates are N/A for criminal charges in specification (4) using older control cohorts only since we do not observe criminal charges at younger ages for these older control cohorts; our main specification for criminal charges in Table 8 uses only the younger control cohorts, which is why the criminal charge estimate and sample size in specification (5) are identical to the baseline (1). Disadvantaged sample includes the bottom third of the distribution of predicted GPA within each gender.

Table A.10: Reform effects across different specifications: disadvantaged women

	Baseline	Add control variables	Add cohort trend in starting age effect	Older control cohorts only 1975-1977	Younger control cohorts only 1979-1981	150-day RD bandwidth	120-day RD bandwidth
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Enroll in vocational at 16	0.127***	0.133***	0.130***	0.119***	0.140***	0.114***	0.126***
(SE)	(0.025)	(0.024)	(0.025)	(0.026)	(0.026)	(0.027)	(0.031)
Complete vocational HS degree	0.058**	0.061***	0.059**	0.046*	0.073***	0.051*	0.066**
	(0.024)	(0.024)	(0.024)	(0.025)	(0.025)	(0.026)	(0.029)
Complete academic HS degree	0.061**	0.054**	0.061**	0.062**	0.061**	0.057**	0.054*
	(0.025)	(0.025)	(0.025)	(0.027)	(0.027)	(0.028)	(0.031)
No degrees	-0.061***	-0.057**	-0.062***	-0.058**	-0.067***	-0.048*	-0.053*
	(0.023)	(0.022)	(0.023)	(0.024)	(0.024)	(0.025)	(0.028)
Earnings 25-34	754	466	817	748	865	625	541
	(871)	(853)	(870)	(910)	(942)	(968)	(1,087)
Any criminal charges 16-30	-0.001	-0.001	0.007	N/A	-0.001	-0.007	-0.014
	(0.014)	(0.014)	(0.016)	N/A	(0.014)	(0.015)	(0.017)
N	55,347	55,347	55,347	32,160	30,757	45,223	36,059
N for criminal charges 16-30	30,757	30,757	30,757	N/A	30,757	25,060	20,044

Notes: Estimates come from separate difference-in-discontinuity regressions as described in Section 4. Estimates are N/A for criminal charges in specification (4) using older control cohorts only since we do not observe criminal charges at younger ages for these older control cohorts; our main specification for criminal charges in Table 8 uses only the younger control cohorts, which is why the criminal charge estimate and sample size in specification (5) are identical to the baseline (1). Disadvantaged sample includes the bottom third of the distribution of predicted GPA within each gender.

Table A.11: Reform effects across different definitions of disadvantaged men

	Baseline: Bottom tercile of predicted GPA (1)	Mother's education: Never enrolled in high school (2)	Parental education: Neither mother nor father completed high school (3)	Parental income: Bottom tercile within birth cohort (4)	Parental education and income: Neither completed high school and income in bottom half (5)
Enroll in vocational at 16	0.118***	0.088***	0.116***	0.060**	0.086***
(SE)	(0.024)	(0.027)	(0.022)	(0.025)	(0.027)
Outcome mean	0.589	0.566	0.560	0.544	0.588
Complete vocational HS degree	0.070***	0.059**	0.069***	0.051**	0.077***
	(0.024)	(0.027)	(0.022)	(0.024)	(0.027)
	0.417	0.393	0.411	0.394	0.417
Complete academic HS degree	-0.061***	-0.051**	-0.073***	-0.031	-0.057**
	(0.022)	(0.024)	(0.021)	(0.023)	(0.024)
	0.269	0.283	0.310	0.322	0.272
No degrees	-0.013	-0.010	-0.003	-0.030	-0.028
	(0.023)	(0.026)	(0.021)	(0.023)	(0.026)
	0.349	0.356	0.314	0.320	0.345
Earnings 25-34	2,329**	1,429	1,510	1,549	1,850
	(1,132)	(1,266)	(1,045)	(1,145)	(1,223)
	47,108	47,359	48,817	46,236	46,908
Any criminal charges 16-30	-0.052**	-0.052**	-0.051**	-0.045**	-0.044*
	(0.023)	(0.026)	(0.020)	(0.023)	(0.025)
	0.266	0.272	0.242	0.251	0.258
N	57,911	49,451	70,329	58,143	48,453
N for criminal charges 16-30	32,346	27,664	36,814	32,459	25,508

Notes: Each column presents reform effects on key outcomes for disadvantaged men across different definitions of disadvantage. Estimates come from separate difference-in-discontinuity regressions as described in Section 4.

Table A.12: Reform effects across different definitions of disadvantaged women

	Baseline: Bottom tercile of predicted GPA (1)	Mother's education: Never enrolled in high school (2)	Parental education: Neither mother nor father completed high school (3)	Parental income: Bottom tercile within birth cohort (4)	Parental education and income: Neither completed high school and income in bottom half (5)
Enroll in vocational at 16	0.127***	0.122***	0.106***	0.141***	0.125***
(SE)	(0.025)	(0.027)	(0.022)	(0.024)	(0.027)
Outcome mean	0.415	0.404	0.383	0.378	0.412
Complete vocational HS degree	0.058**	0.054**	0.026	0.070***	0.053**
	(0.024)	(0.026)	(0.021)	(0.023)	(0.026)
	0.310	0.300	0.298	0.282	0.309
Complete academic HS degree	0.061**	0.046*	0.028	0.001	0.016
	(0.025)	(0.028)	(0.023)	(0.025)	(0.028)
	0.467	0.468	0.512	0.520	0.475
No degrees	-0.060***	-0.055**	-0.031	-0.030	-0.035
	(0.023)	(0.025)	(0.020)	(0.022)	(0.024)
	0.282	0.288	0.247	0.252	0.275
Earnings 25-34	754	1,158	779	-71	496
	(871)	(975)	(793)	(878)	(938)
	31,685	32,093	33,362	31,770	31,619
Any criminal charges 16-30	-0.001	-0.013	0.022*	0.006	0.026*
	(0.014)	(0.015)	(0.012)	(0.013)	(0.015)
	0.072	0.074	0.063	0.066	0.068
N	55,347	47,597	67,570	55,119	46,094
N for criminal charges 16-30	30,757	26,578	35,024	30,646	24,172

Notes: Each column presents reform effects on key outcomes for disadvantaged women across different definitions of disadvantage. Estimates come from separate difference-in-discontinuity regressions as described in Section 4.

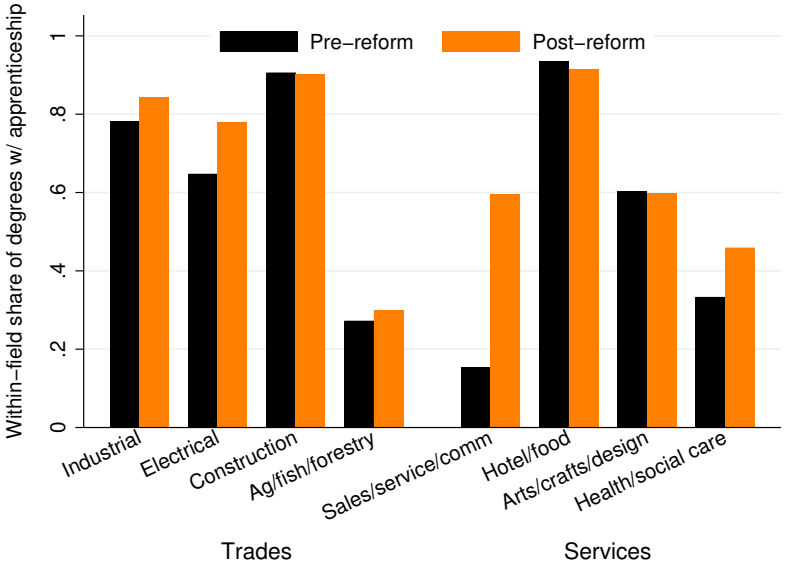

Figure A.1: Share of degrees within each vocational field that are completed with an apprenticeship

Notes: This figure plots the share of degrees within each vocational field that are completed with an apprenticeship, comparing the pre-reform 1977 birth cohort to the post-reform 1978 birth cohort. Sample is conditional on completing any vocational degree.

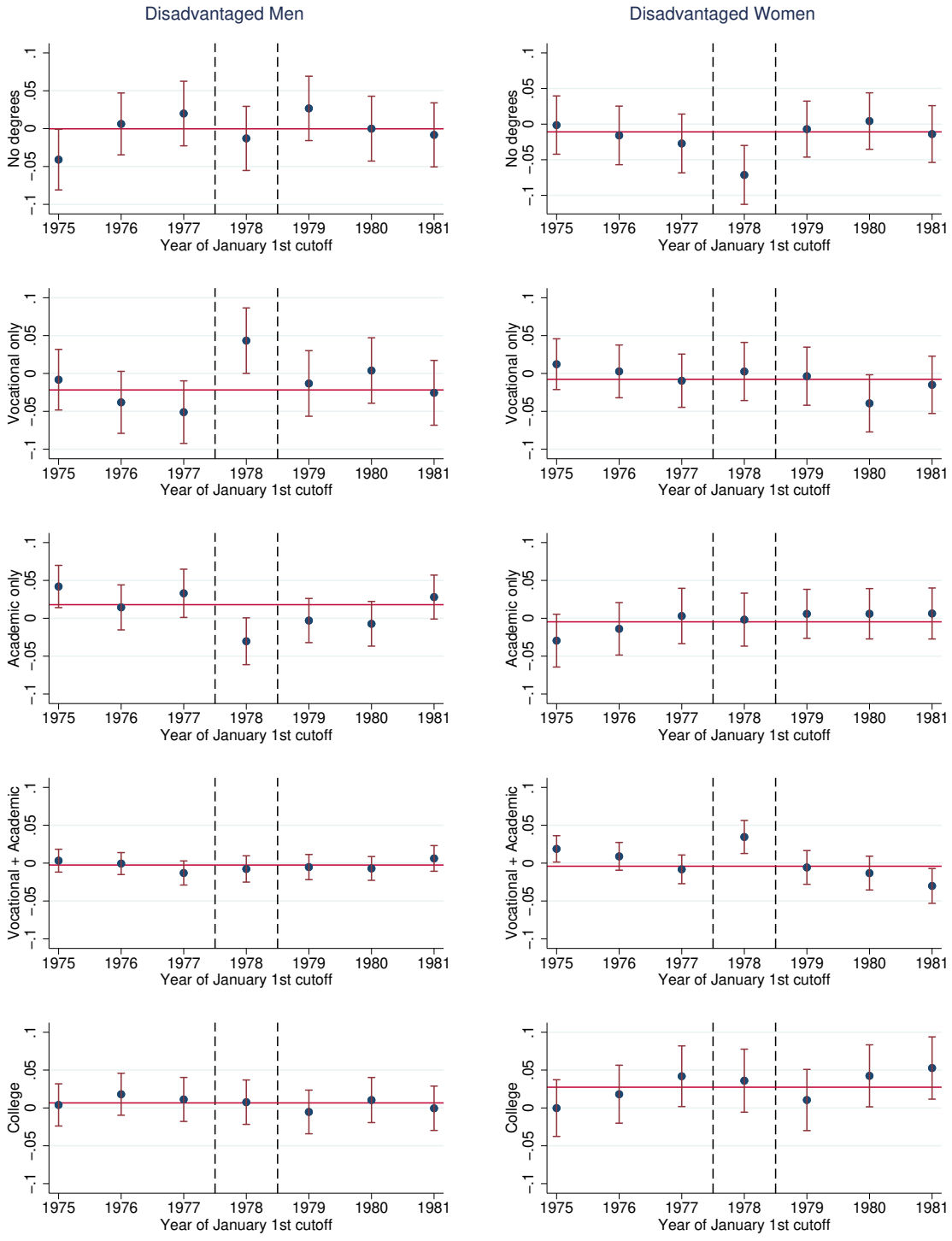

Figure A.2: Discontinuity estimates separately by cohort

Notes: This figure plots point estimates and 95% robust confidence intervals of the January 1st discontinuity in each outcome in Table 4, separately for each birth cohort. The vertical dashed lines highlight the cohort that experienced the discontinuity in eligibility for Reform 94 (1978). Disadvantaged men and women comprise the bottom third of the distribution of predicted GPA within each gender.

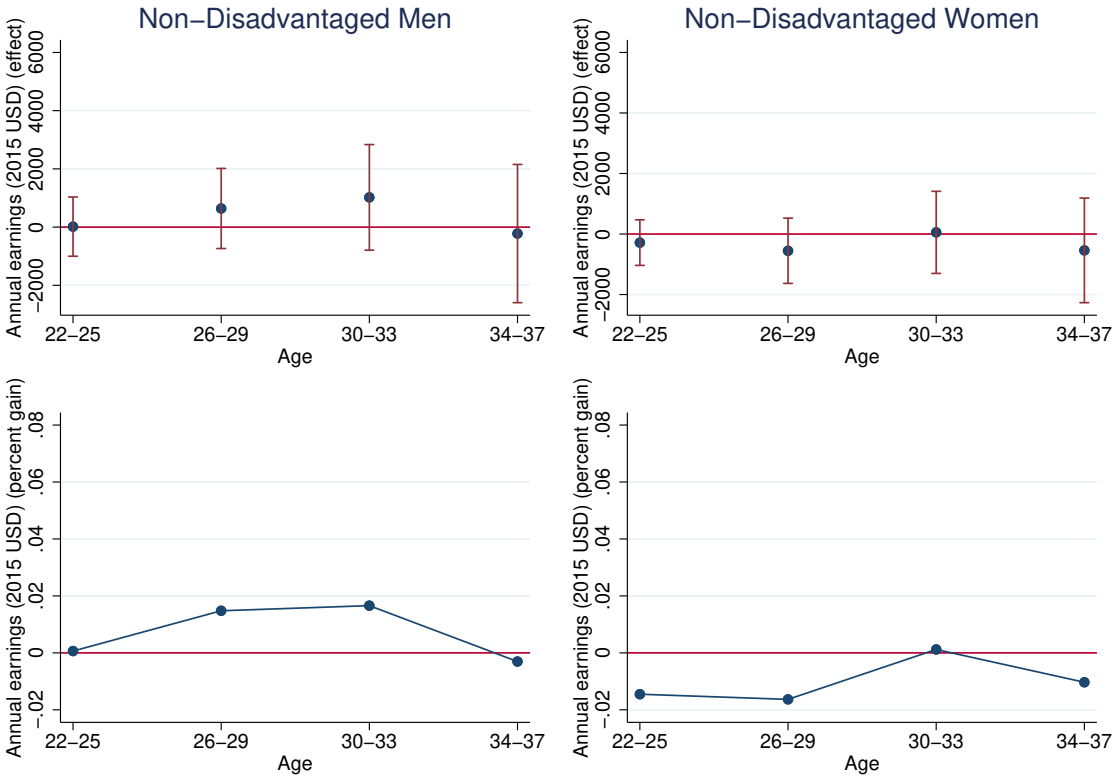

Figure A.3: Reform effects on earnings across ages: non-disadvantaged men and women

Notes: The top figures plot difference-in-discontinuity earnings effect estimates (and robust 90% confidence intervals) separately by age bins. Only the older control cohorts are used at the latest ages (34-37), since earnings data are not yet available for the younger control cohorts at these ages. The bottom figures plot the reform effect estimate divided by the mean counterfactual earnings level at that age to measure the effect in percentage terms. We calculate the mean counterfactual earnings level by evaluating the estimated regression function in Equation (1) at the eligibility cutoff ($Day = 0$) assuming zero reform effect ($\beta = 0$), i.e. $\hat{\alpha} + \hat{\delta}_1 + \hat{\delta}_2$. All monetary amounts are measured in real 2015 U.S. dollars. Non-disadvantaged is defined as being in the top two thirds of the distribution of predicted GPA within each gender.

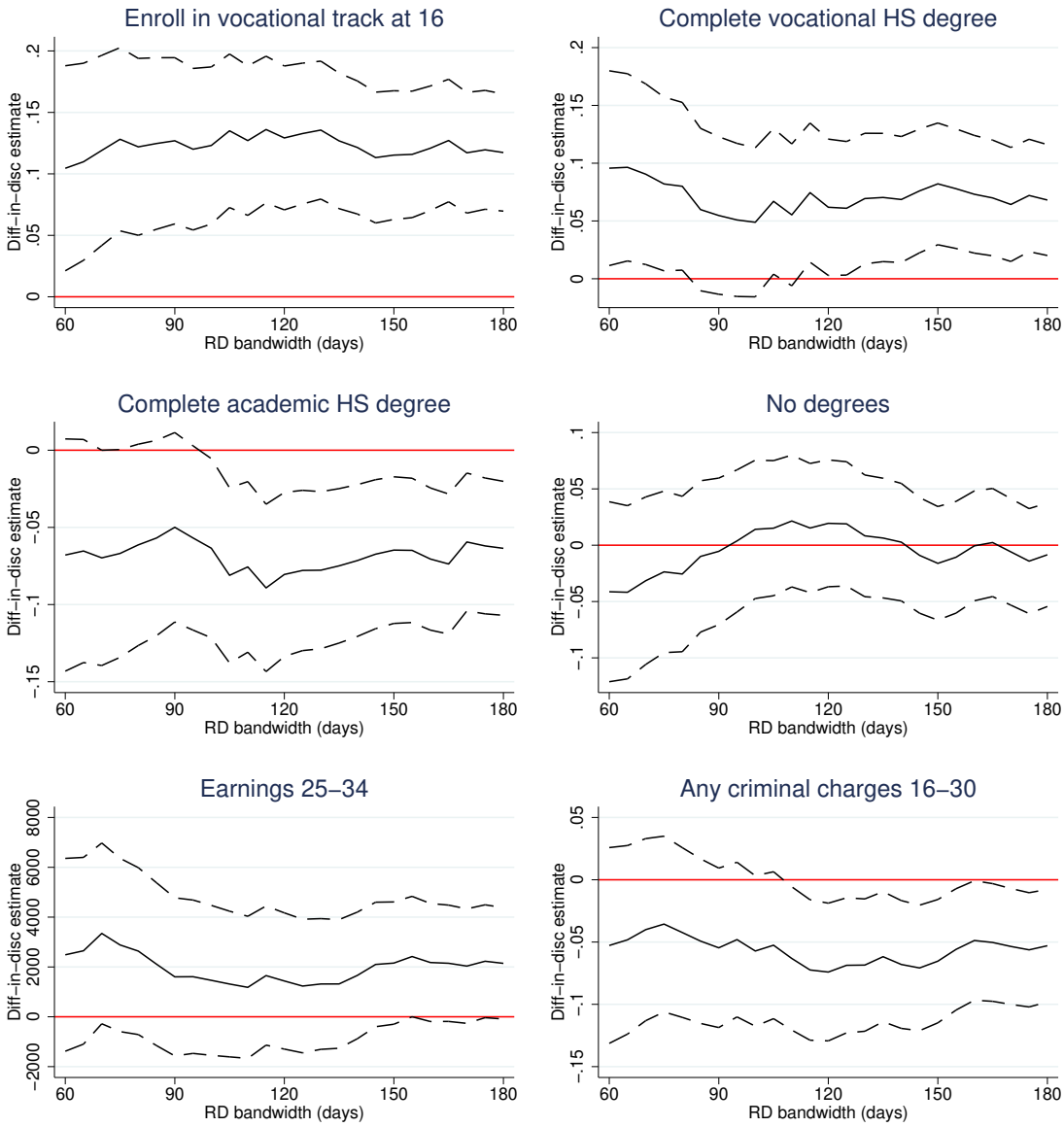

Figure A.4: Reform effect estimates across RD bandwidths: disadvantaged men

Notes: This figure plots disadvantaged male reform effect estimates and 95% robust confidence intervals across different regression discontinuity bandwidths, defined as the number of birth days included on each side of the January 1st cutoff in each cohort window. Disadvantaged sample includes the bottom third of the distribution of predicted GPA within each gender.

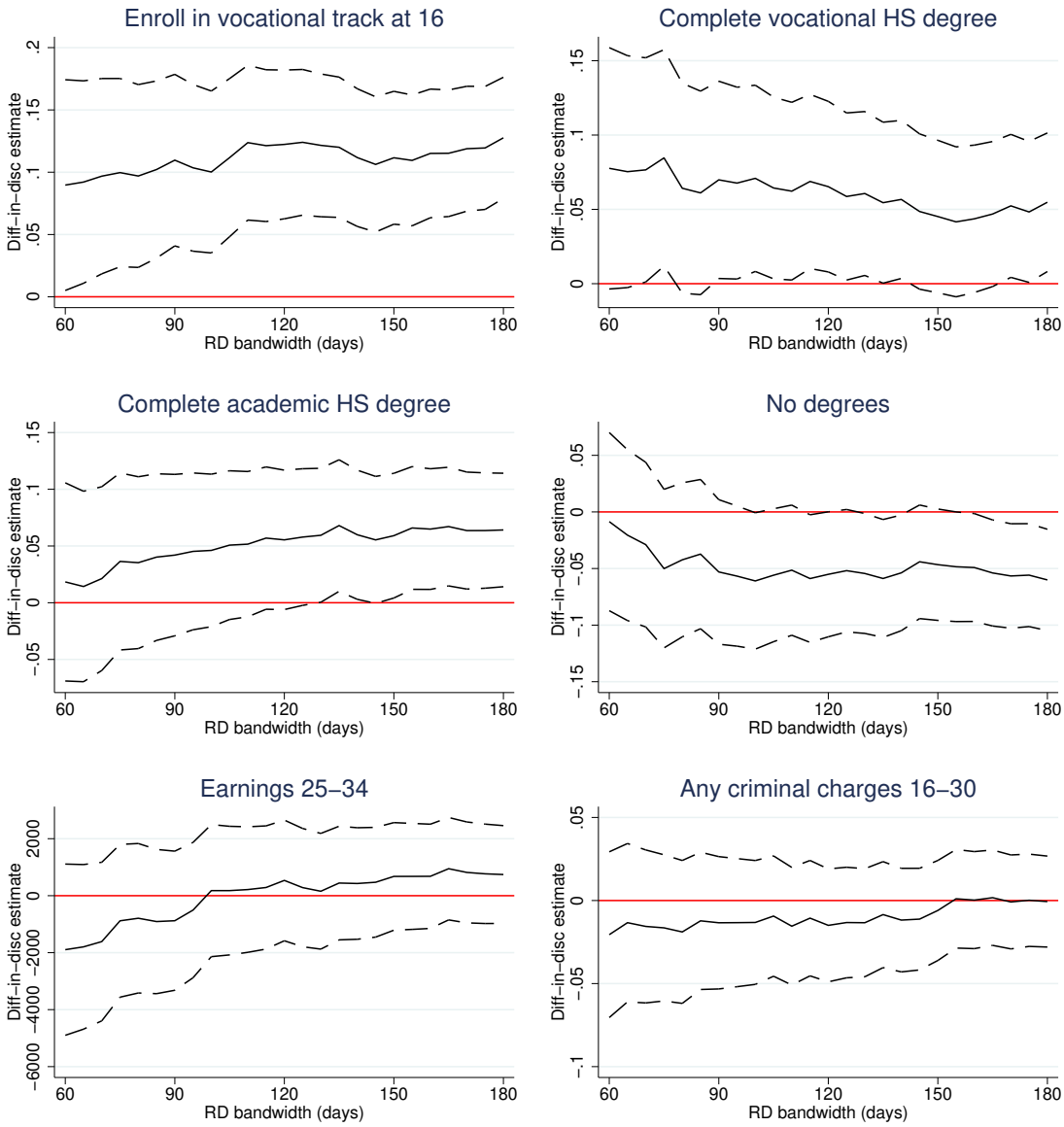

Figure A.5: Reform effect estimates across RD bandwidths: disadvantaged women

Notes: This figure plots disadvantaged female reform effect estimates and 95% robust confidence intervals across different regression discontinuity bandwidths, defined as the number of birth days included on each side of the January 1st cutoff in each cohort window. Disadvantaged sample includes the bottom third of the distribution of predicted GPA within each gender.

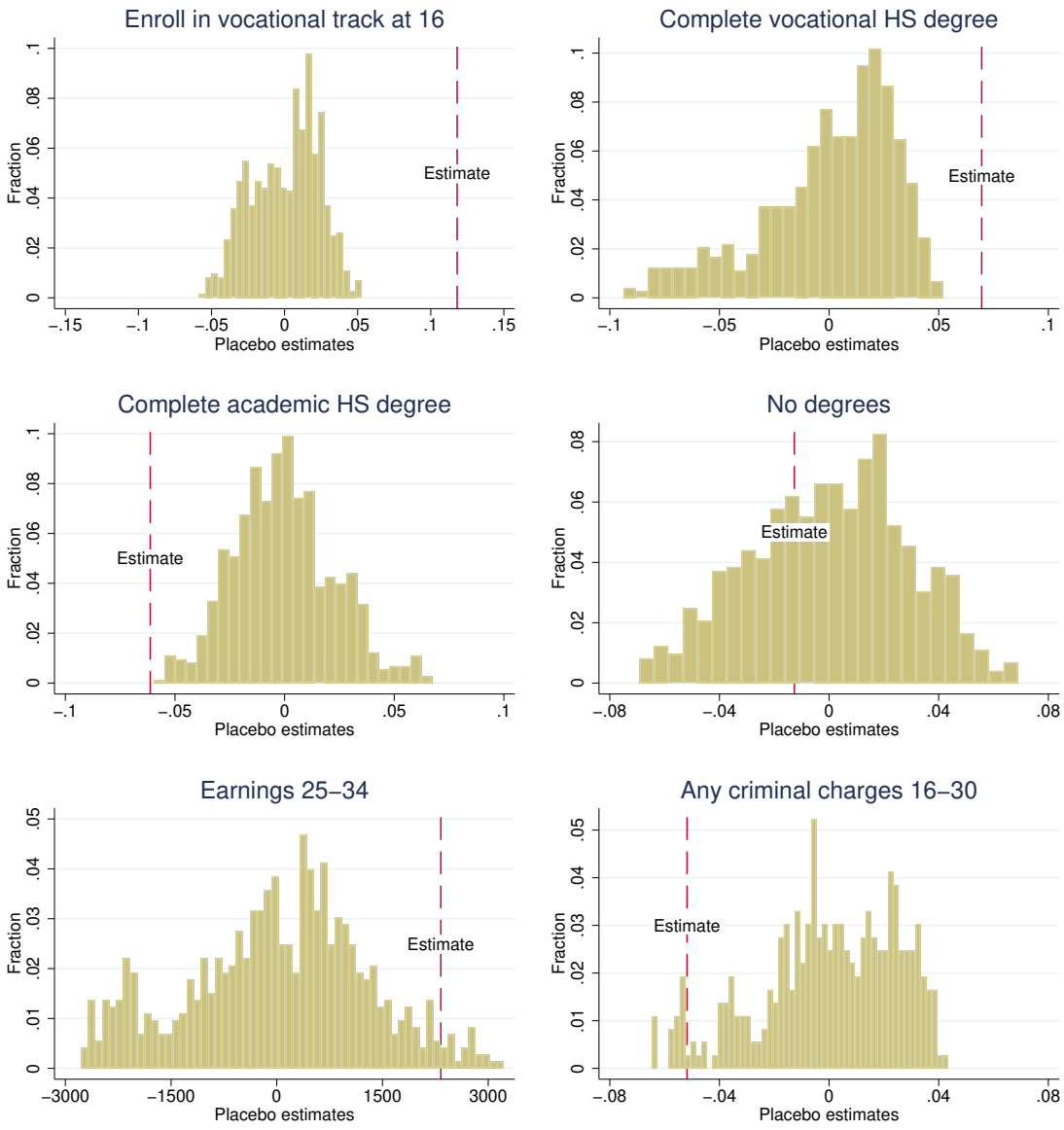

Figure A.6: Actual reform effect estimates versus placebo estimates: disadvantaged men

Notes: This figure plots the actual reform effect estimate for disadvantaged male outcomes versus placebo estimates. Each placebo estimate comes from a difference-in-discontinuity regression with a placebo date cutoff instead of the actual January 1st cutoff, looping over the different birth years to assign placebo "reform" cohorts. The placebo cutoff dates range from 120 to 240 days after January 1st within each calendar birth year to allow for enough data on each side of each cutoff within each birth year window (at least 120 days) and to avoid including the actual January 1st cutoff. Disadvantaged sample includes the bottom third of the distribution of predicted GPA within each gender.

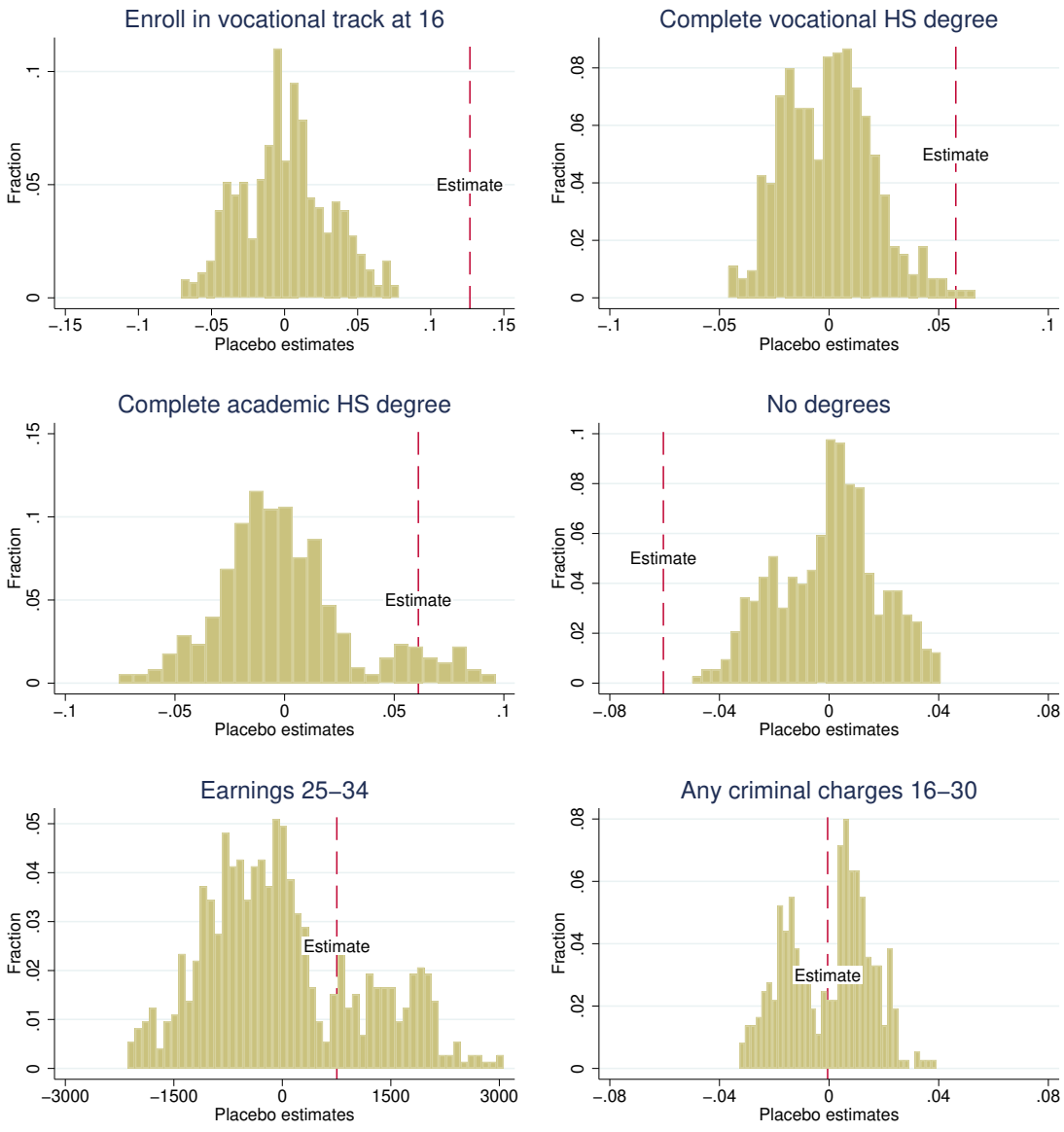

Figure A.7: Actual reform effect estimates versus placebo estimates: disadvantaged women

Notes: This figure plots the actual reform effect estimate for disadvantaged female outcomes versus placebo estimates. Each placebo estimate comes from a difference-in-discontinuity regression with a placebo date cutoff instead of the actual January 1st cutoff, looping over the different birth years to assign placebo "reform" cohorts. The placebo cutoff dates range from 120 to 240 days after January 1st within each calendar birth year to allow for enough data on each side of each cutoff within each birth year window (at least 120 days) and to avoid including the actual January 1st cutoff. Disadvantaged sample includes the bottom third of the distribution of predicted GPA within each gender.