It's all about what you learn

Estimating the return to human capital in higher education

Sonja Kovacevic[1] and Karl Ingar Kittelsen Røberg[2]

[1]Institute of Economics, University of Oslo
[2]Work Research Institute (AFI), Oslo Metropolitan University

August 29, 2023

Abstract

Returns to higher education can reflect human capital or students signalling higher ability to employers. We exploit a reform of the higher education system in Norway, leading to unplanned and unobserved variation in human capital. The Bologna reform reduced undergraduate study length from four to three years, exogenously varying the length of education for enrolled students. We can rule out selection into treatment since students enrolled before the reform was communicated, shutting down the signalling channel for rewarding higher education. We estimate human-capital driven returns to education in relative time, presenting IV and event-study IV estimates. We address concerns with direct effects of the reform on degree composition by estimating no-assumption and MTS non-parametric bounds on the estimate. We reject zero returns on human capital even with no-assumption bounds and parse out a lower bound of the effect of human capital on earnings of xxx percentage points.

Keywords: Education, Human Capital, Signalling

JEL classification: I23, I25, I26, J24, J31 .

1 Introduction

Should governments subsidize higher education? If higher education boosts human capital and in turn productivity, society as a whole may be better off with policies aiming to increase investment in higher education. However, if education mainly signals candidate quality, giving innately more productive candidate the possibility to inform employers of their ability and receive correspondingly higher compensation, the case for state-subsidized education may be less clear-cut. With rising enrollment shares into higher education across the globe, societies devote increasing resources and time to higher education. Understand how higher education is rewarded in the labor market could help policy-makers design better education policies. We study a setting that allows to isolate the human capital component in the returns to higher education, making use of a retroactive policy change in Norway that forestalls signalling.

Human capital theory, as introduced by Becker, 1962 and Mincer, 1974, argues that education itself increases productivity. Signalling theory, as formulated by Spence, 1973 and Arrow, 1973, points out that employers may use selection into higher education as a signal about unobserved ability. Both theories predict higher wages for individuals with higher levels of education, either because high-ability individuals are choosing higher education or because individuals taking higher education acquire valuable skills. Human-capital driven returns suggest social returns at least at the level of private returns, while signalling-driven private returns correspond to zero social returns. From an empirical perspective, measuring returns to higher education in observational data does not allow to test the two theories against each other since they make identical predictions. However, returns to signalling rely on employers anticipating that higher-ability individuals will choose to acquire the costly signal of higher education. If individuals were not able to decide their level of higher education, signalling theory would predict zero returns, making it possible to measure human capital returns to higher education directly. Returns to such non-chosen higher education support the human capital-enhancing view of higher education, possibly speaking for social returns to higher education.

This paper offers a causal estimate of the human capital component in the returns to higher education. Identifying the returns to human capital requires exogenous assignment of higher education, since employers may otherwise reward higher education as a signal of unobserved ability. We exploit exogenous variation in the length of higher education generated by the implementation of the Bologna reform in Norway to identify the returns to human capital. The Bologna reform led to a difference in the length of higher education, giving the treated cohort up to one non-selected extra year of higher education compared to the subsequent non-affected cohort. The 1999 enrollment cohort was studying towards towards the pre-reform 4-year degree type, but ended up receiving the new 3-year degree instead, when the reform was introduced in 2003. Depending on their study speed, 1999 students will have accumulated up to 4 years worth of higher education by 2003 and effectively receive a 3-year diploma for up to 4 years of higher education. In contrast, the 2000 (and 2001) enrollment cohorts anticipated to complete the 4-year program as well, but had only completed up to 3 years when the new 3-year degree was announced, steering clear of any extra points. Crucially, both cohorts receive the new diplomas, while only the 1999 cohort received up to one extra year of higher education. Since the reform was announced in 2003, several years after enrollment, selection into enrollment cohorts can be ruled out. Thus, the extra education obtained by the 1999 cohort does not convey information about innate ability to potential employers.

We estimate the return to human capital using enrollment cohort as an instrument. Enrollment cohort predicts how much human capital an individual accumulates, whether measured in terms of study time or study points. We then estimate the return to an induced extra year of higher education in the labour market. We establish that cohorts are balanced on observables as they enroll, and that their wage profiles are indistinguishable before enrolling and during higher education.

Since the enrollment cohort of 1999 is on average one year older than the 2000 enrollment cohort, we measure earnings in event time to avoid maturity effects. Estimating 2 SLS for the interactions of cohort-induced differences in study length and relative time dummies allows us to recover the path human-capital induced earnings differences, with years prior to enrollment serving as an implicit test on pre-trends. To address concerns with year-specific shocks, we estimate an alternative specification in real time, using the 2001 cohort along the 2000 cohort to simultaneously offset differences in education vis-à-vis the 1999 cohort and account for age and year fixed effects. We address concerns with a minority of the sample still receiving old degree types by estimating non-parametric bounds on the counterfactual earnings of old degree holders and separately re-estimate the model on the sample with counterfactual upper and lower bounds for old degree holder earnings if they had received the new degree type. We find that a potential sheepskin effect benefitting old degree holders does not drive the results. [ADD RESULTS HERE]

This paper contributes to the literature by isolating the human capital component in the returns to higher education. Specifically, we leverage variation in the amount of higher education that individuals did not self-select into, shutting down the signalling channel.

Several papers attempt to separate the signalling and human capital returns to education. Clark and Martorell, 2014 find little evidence for high school diplomas acting as a signal, while Tyler, 2003 find some signalling returns to the GED and Bedard, 2001 reject a pure human capital model for secondary schooling choices. These papers have in common that they focus on returns to compulsory schooling rather than higher education. Our research estimates the human capital component to higher education, with a lot more variation in individual choice than for compulsory schooling. Additionally, focusing on higher education also offers insights on a type of education where the productivity-enhancing effect does not have the same intuitive appeal. While being able to read and write seem undoubtedly crucial in today's labour market, the case may be less clear-cut for higher education.

For higher education, there are considerably fewer causal studies. Some work pins down the returns to field of study (Bleemer and Mehta, 2022; Hastings et al., 2013; Kirkeboen et al., 2016) or the return to being admitted to a particular program such as medical school (Ketel et al., 2016). Although solidly identified, the returns in these design may reflect the selectivity of a particular course (signal), the match between a candidate and their program of choices and/or monopoly rents for some occupations. Arteaga, 2018 estimates the human capital component in returns to the economics and business degree at an elite university in Columbia. This paper is the closest cousin to our work. However, our framework allows to estimate human capital returns across a broad set of fields, and investigate heterogeneity in the human capital returns by field of study. Additionally, we estimate the human capital returns to all science, social science and humanity degrees at the four universities in Norway, covering human capital returns for a broader set of the student population than at one elite university. Lastly, we can rule out selection into reform cohorts since the reform was announced several years after enrollment, while Arteaga, 2018 relies on the assumption that the university Los Andes would attract all top students both before and after the downgrade of the program, so that student composition would not be affected.

Section 2 describes the Bologna reform and how students of the transitioning cohort received shocks to the length of their education. We show balancing of the treated cohorts and argue why the timing of enrollment and reform announcement made it impossible for students to select into treatment. In section 3, we estimate the returns to randomized human capital. We report IV estimates, with time centered at the enrollment date and show in an event-study fashion how earnings effects evolve over time. As a robustness check, we use an additional cohort to estimate age effects directly. In section 4, we develop an approach to account for students that were still receiving the old degree types, estimating non-parametric bounds on their counterfactual earnings. Section 5 concludes.

2 Bologna-induced human capital

The Bologna reform was a Europe-wide educational reform, aiming at harmonizing and integrating European higher education. All European countries committed to introduce the Bachelor and Master as the main university degrees. In the Norwegian context, this meant replacing the undergraduate degree cand.mag., standardized at 4 years with a 3 year Bachelor and the postgraduate degree hovedfag with a Master degree of equal length, essentially replacing a 4+2 higher education course with a 3+2 system. Similar to other European countries, professional degrees such as medicine and psychology were excluded and additionally, some Norwegian degrees such as nursing and engineering already had a 3+2 structure before the reform. Effectively, around 50 academic subjects underwent the reform, and all the affected degrees were taught at on of the four large universities, while degrees at the community colleges were largely unaffected.

Compared to other European countries, Norway implemented the Bologna reform especially rigorous and wholeheartedly. Starting with the implementation in the fall semester of 2003, students faced a completely overhauled curriculum, with changes both in content, didactic set-up, course structure and exam frequency. More importantly, students that enrolled before the reform in 2003 were also subject to the reform. They were generally not allowed to graduate in the program they had initially enrolled in, but were moved over to the new degree structure. This retroactive implementation of the Bologna reform is the centerpiece of our identification strategy.

Students enrolling before the Bologna reform planned to study for a cand.mag. degree followed by working or a postgraduate degree. All students enrolling before the announcement of the Bologna reform thus share identical signals of enrolling into a 4-year course. However, due to the retroactive implementation of the reform, these otherwise identical students differ in their length of education. We first describe how regular-speed students are affected before discussing how particularly quick and slow students are affected. A student enrolling in 2000, studying at regular speed would complete 3 years of education by 2003 and thus qualify for a bachelor degree. A student enrolling in 1999 would complete 4 years of education by 2003. Thus, they could have graduated with a cand.mag. in 2003 had the Bologna reform not been implemented. However, since as of 2003 only bachelor's degree were possible, 1999 cohort students would receive a Bachelor degree certifying 3 years of education although they had completed 4 years of education. Comparing the 1999 and 2000 enrollment cohort allows to isolate the effect of receiving more higher education without having made any different educational choices. If employers reward this extra education in the labor market, it must be because the 1999 students are more productive due to the extra courses they took and not because they signal some innate ability through their choices.

In the ideal case of students studying exactly at the standard study speed, students from the 1999 cohort will acquire exactly one extra year of education. In practice, study speed varies greatly across some students. Some students study quicker than prescribed while roughly one third studies study at regular speed and the rest studies slower than usual. Using cohort as an instrument, only students that have an average speed of 45 to 60 study points will have accumulated overshooting points by 2003. Slower students from the 1999 cohort will be never-treated while students quicker than that will even receive the old degrees. However, also students with a slower study speed can be affected by the reform, but only if they enroll earlier. A student collecting 40 points a year and enrolling in 1998, for example, would receive 200 points by 2003. Having 20 points more than the 180 required for a bachelor, they would also have extra human capital. For the main specification we will use the 1999 and 2000 cohorts since the first stage is strongest and very few students acquire so many points so early that they can receive the old degrees, thus acquiring a different signal. However, we present estimates for all cohorts where the distribution of study speed would

Point conditions at introduction date 31.07.2003

		Treatment	Control
	Old degree & 240+ p	New degree & 180-240 p	Control: New degree & 180 p
1997	$>$ 40 points/year	30-39 points/year	$\leq$ 30 points/year
1998	$>$ 48 points/year	36-47 points/year	$\leq$ 36 points/year
1999	$>$ 60 points/year	45-59 points/year	$\leq$ 45 points/year
2000	$>$ 80 points/year	60-79 points/year	$\leq$ 60 points/year
2001	$>$ 120 points/year	90-119 points/year	$\leq$ 90 points/year

allow for some first stage, that is all cohorts from 1997 to 2002 as an extension. See below for the expected distribution of students with extra human capital in bachelor degrees, regular bachelor and old degrees by study speed.

[EXTENDED HERE LATER: NORWEGIAN BACKGROUND]

The Norwegian labor market displays an extraordinary degree of wage compression (Barth and Roed, 1999), with extensive labor regulations, strong trade unions and a large public sector all limiting the returns to individual productivity. Human-capital induced increases in productivity may thus show up only partially in labor returns, even if employers recognize them, because wage dispersion among employees is difficult to implement.

3 Theory

4 Empirical Strategy and Data

The Bologna reform pushes people into taking differential education by cohort. That is, students from the 1999 cohort receive more overshooting points than students from the 2000 cohort. Thus, the cohort treatment consists of jointly increasing the length of education and enrolling at a different time. In several evaluations of the returns to education, outcomes are measured within an income year (see for example Bleemer and Mehta, 2022). When comparing programs of equal length, measuring incomes at the same time ensures comparability without any major downsides. However, when measuring the incomes of students from the 1999 and 2000 enrollment cohort in the same income year, students from 1999 will not only have had more education, but will also be one year older. This in itself should make them earn higher wages. Effectively, since students from 1999 study on average four months longer than students in 2000, they will also have eight more months of experience on average. Thus, comparing earnings for 1999 and 2000 students does not isolate the human capital returns of education. Rather, it estimates the combined effect of maturity and substituting some experience for education.

From a policy perspective, one should compare outcomes after a given stretch of time after the intervention. After all, policy makers can only influence how people allocate their time, but they cannot change the total time people have available. In terms of education policy, policy makers can incentivize people to take more education, but that will translate into having less experience at a given age. In order to estimate whether more higher education increases a persons earnings, the counterfactual for receiving more education should be spending the time working (or unemployed) instead and comparing outcomes after a given amount of time. Earnings differences a fixed time span after treatment capture the policy-relevant treatment effect (PRTE). In the setting of the Bologna reform, estimating the PRTE requires comparing outcomes for the 1999 and 2000 cohort at given times since enrollment. Thus, rather than measuring outcomes in calendar time, time should be relative to the enrollment date where cohorts were equivalent, so that any differences in outcome can be attributed to the treatment.

Measuring outcomes in relative time isolates the human-capital driven returns to higher education, shutting down the maturity component of enrolling one year earlier. However, this poses a measurement challenge. When measuring at the same relative time, that is, a given time since enrollment, calendar time differs. For example, measuring outcomes 15 years after enrollment implies measuring incomes for students from the 1999 cohort in 2014 and for students from the 2000 cohort in 2015. Incomes are difficult to compare across calendar years for several reasons. Firstly, in the presence of inflation, identical real wages in relative time will be nominally different across income years. Secondly, in the presence of business cycles, even real wages differ across years. Identical candidates will on average earn as much more in say 2011 compared to 2010 as the average growth rate in the economy, although 2011 and 2010 correspond to identical relative time. Lastly, business cycles in the economy may affect people with different experience differentially across income years. For example, a newly hired graduate may have a higher risk of losing their job in a recession compared to a person with somewhat longer tenure. Since the 2000 cohort has more experience at any given age, their incomes should be less susceptible to economic shocks for a given level of human capital, overestimating their wages in relative time. Lastly, measuring in relative time forces a direct trade-off between returns to education and returns to experience. In the long run, this direct trade-off is desirable because it incorporates the opportunity costs of education and more education would only be worthwhile if the returns to education exceed that of experience. However, upon graduating, incomes grow quickly with experience for the first years in the labour market. Even with a sizable return to an extra year of education, comparing a new graduate to an employee with one year of experience may yield a negative net return. Once the experience curve flattens after many years in the labour market, returns to human capital for the long-run earnings potential should become visible. In the sample of pre-Bologna students in Norway, both study speed and desired degrees vary tremendously, leading to large variation in total study time. Thus, for most time points in the sample, some students will be recent graduates with experience effects dominating, while other have been in the labor market long enough for to make the long-run human capital effects observable, making total effects difficult to interpret.

4.1 Descriptives

[See presentation for preliminary descriptives]

5 Results

[See presentation for some preliminary results]

6 Robustness Checks

6.1 Differential dropout rates across cohorts

Not all students enrolling in higher education complete their envisioned degree. In the Norwegian context, roughly half switch their initial degree, and about 30 percent enrolling in higher education never obtain a degree at all. Many students drop out of university shortly after enrolling, but a sizable fraction of dropouts occur later during studies.

For identifying the causal effect of additional education based on enrollment cohorts, this poses the challenge that the 1999 cohort spend a longer time in university before learning about the reform in 2003. Assuming identically distributed student bodies at enrollment, this leaves more time for the 1999 cohort to drop out of their studies before the treatment occurs. In particular, students in the 2000 cohort that might have dropped out in the fourth year of their studies may continue studying if offered a degree after three years, while corresponding students from the 1999 cohorts will receive this information later and may already have dropped out.

However, if most dropouts in these cohorts realize soon after enrollment or if students drop out for other reasons that the envisaged length of the course, this is not necessarily a problem. The graph below displays the cumulative probability distributions for students from both cohorts for dropping out over time, contrasted with the cumulative probability distribution of students from both cohorts that graduate. While the dropouts occur virtually simultaneous in relative time for both cohorts, students in the 1999 cohort take somewhat longer to graduate. This is in line with the expected effect of the reform for the transitioning cohorts. Checking the t-value for the mean time at university across cohorts for both dropouts and graduates confirms the visual impression. Dropouts across both cohorts are virtually indistinguishable with a t-value of 0.91, while the t-value for graduates' mean study time is at 0.xxxxx.

Although one may expect differential dropping out dependent on the length of available education programs, we do not find any evidence for this in our data. Students from the 1999 cohort that learn later about the possibility to graduate with a bachelor neither drop out more often nor do they drop out at different times. However, graduates from both cohorts are affected in their study length, pointing to a possibility for measuring human capital related effects. Dropping out seems to be (surprisingly) unrelated to study length, so that the treatment does not seem to go through differential probabilities of dropping out. Any differences in average wages across cohorts should thus go through the added human capital of those graduating entering the workforce.

6.2 More robustness

[THE FINISHED PAPER WILL NEED MANY ROBUSTNESS CHECKS]

7 Conclusion

References

Arrow, Kenneth J et al. (1973). "Higher education as a filter." In: *Journal of public economics* 2.3, pp. 193–216.

Arteaga, Carolina (2018). "The effect of human capital on earnings: Evidence from a reform at Colombia's top university." In: *Journal of Public Economics* 157, pp. 212–225.

Barth, Erling and Marianne Roed (1999). "The return to human capital in Norway: a review of the literature." In: *Returns to human capital in Europe: A literature review, ETLA-The Research Institute of the Finnish Economy, Helsinki*, pp. 227–58.

Becker, Gary S (1962). "Investment in human capital: A theoretical analysis." In: *Journal of political economy* 70.5, Part 2, pp. 9–49.

Bedard, Kelly (2001). "Human capital versus signaling models: university access and high school dropouts." In: *Journal of political economy* 109.4, pp. 749–775.

Bleemer, Zachary and Aashish Mehta (2022). "Will studying economics make you rich? A regression discontinuity analysis of the returns to college major." In: *American Economic Journal: Applied Economics* 14.2, pp. 1–22.

Clark, Damon and Paco Martorell (2014). "The signaling value of a high school diploma." In: *Journal of Political Economy* 122.2, pp. 282–318.

Hastings, Justine S, Christopher A Neilson, and Seth D Zimmerman (2013). *Are some degrees worth more than others? Evidence from college admission cutoffs in Chile*. Tech. rep. National Bureau of Economic Research.

Ketel, Nadine, Edwin Leuven, Hessel Oosterbeek, and Bas van der Klaauw (2016). "The returns to medical school: Evidence from admission lotteries." In: *American Economic Journal: Applied Economics* 8.2, pp. 225–254.

Kirkeboen, Lars J, Edwin Leuven, and Magne Mogstad (2016). "Field of study, earnings, and self-selection." In: *The Quarterly Journal of Economics* 131.3, pp. 1057–1111.

Mincer, Jacob A et al. (1974). *Schooling, Experience, and Earnings*. National Bureau of Economic Research.

Spence, Michael (1973). "Job Market Signaling." In: *The Quarterly Journal of Economics*, pp. 355–374.

Tyler, John H (2003). "Economic benefits of the GED: Lessons from recent research." In: *Review of educational research* 73.3, pp. 369–403.