Do Students Benefit from an Extra Year of High School? A Study of Ontario's Removal of Grade 13

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Abstract

This paper looks at a 2003 education reform in Ontario to measure the value of an extra year of high school. Using data from Statistics Canada's Labour Force Survey and a difference-in-differences technique, I examine grade 13's effect on an individual's wage and educational attainment. I find an initial wage benefit to grade 13 that disappears over time, and potentially turns into a wage penalty ten years after an individual had grade 12. Further, grade 13 significantly increases an individual's likelihood of graduating high school, but has no effect on higher levels of education. Grade 13's failure to add significant long-run value leads me to conclude that we would not benefit from an extra year of high school. This motivates the question: what is the value added by grade 12?

1 Introduction

Students, teachers, and policy-makers alike are concerned with assessing the value of a high-school education. While considerable evidence of a correlation between schooling and earnings exists, the relative importance of each year is unknown, as is the source of this value. Traditionally, education is seen as an investment in human capital, where an individual goes to school to acquire skills in preparation for the labour force. However, economists caution of an 'ability bias,' where individuals pursue a high school diploma and beyond to signal their productivity to employers.

In this paper I look at whether we would benefit from a grade 13. I do so using a unique policy change that took place in my home province of Ontario in 2003, when high school was shortened from five to four years. Before this change occurred, Ontario was the only state or province in North America to require five years of high school. Sufficient time has elapsed to examine how these students have turned out, and by comparing Ontario students before and after the policy change (relative to British Columbia), I can identify the returns to grade 13.

Using nationally representative survey data courtesy of Statistics Canada, I measure grade 13's effect on wage and how it varies over the ten years after an individual had grade 12.[1] Further, I examine how grade 13 affects an individual's likelihood of graduating high school and pursuing higher education. My paper is the first to look at the long-run returns to grade 13, and the first to measure its effect on the labour market. I find that grade 13 results in a significantly higher wage shortly after high school; however, over time this benefit disappears and may even turn into a wage penalty. In the long run, I expect the wages of grade 13 and grade 12 graduates to be approximately the same. With regard to education, I show that grade 13 students are significantly more likely to graduate high school, but fail to stand out at higher education levels.

1 This analysis is based on the Statistics Canada *Labour Force Survey (LFS) Public Use Microdata File*. All computations, use, and interpretation of these data are entirely that of Avinash Moorthy.

Taken together, these results suggest that grade 13 has short-term benefits, but fails to add significant value in the long run. This surprising finding can be explained by both human capital and signalling theory. If high school has diminishing returns, then the productivity gains of grade 13 are cancelled out by its opportunity cost of entering the labour force a year later. Or, if education merely signals ability, then an extra year is redundant. This calls into question the value of the fourth year of high school, and the third year, and so on. I hope that with future research we can identify the appropriate length of high school, carefully weighing the costs and benefits of each individual year.

The structure of this paper is as follows. Section 2 provides institutional background on Ontario's elimination of grade 13 and contextualizes it within the human capital and signalling debate. Section 3 reviews the existing literature on grade 13 as well as related studies on the returns to schooling. Sections 4 and 5 describe my model and the data used to estimate it. Section 6 shows the results, and Section 7 discusses their implications and limitations. Section 8 concludes. Based on my analysis, I argue that students would not benefit from an extra year of high school.

2 Institutional and Theoretical Background

To examine the returns to an extra year of high school I turn to a unique policy change that occurred in the province of Ontario, Canada. Before 2003, Ontario students negotiated five years of high school, the result of a policy imposed in 1921 (Brady & Allingham, 2010). These students were permitted to graduate in four years provided they received the necessary credits; however, the vast majority chose to complete the diploma in five. In fact, only 4 percent of Ontario students in 2000 finished in four years (Sana & Fenesi, 2013). In 1997, the provincial government decided to eliminate the Ontario Academic Credit, as grade 13 was later known, citing cost reduction and conformation to surrounding provinces as motivating factors (Morin, 2013). Consequently, students entering Ontario high schools in 1998 were the last to experience grade 13, producing a 'double cohort' in 2003 when grade 12 and grade 13 students graduated from schools across the province. The number of credits required to graduate was unaffected by the policy change, so the main effect of shortening high school was a change in educational philosophy. Students had fewer courses available to them, and teachers were bound by a rigid new curriculum that emphasized specific learning expectations and prescriptive evaluation (Krashinsky, 2014; Brady & Allingham, 2010). The curriculum compression affected subjects differently so, while math lost much of calculus, biology was more or less unchanged (Morin, 2013).

A change in curriculum suggests a possible human-capital effect, permitting me to assess the returns to grade 13 within the broader debate on high school's value. Whereas a correlation between education and earnings is well established in the literature, the degree of causation is hotly contested. Specifically, economists are split on whether education is an investment in human capital or merely a signal. The esteemed Gary Becker is a major proponent of the former, which he defines as "activities that influence future real income through the embedding of resources in people" (Becker, 1962). He argues that schooling steepens an individual's age-earnings profile, as the person accepts below-potential earnings at an early age (an opportunity cost of schooling) but reaps the benefits over time. Jakob Mincer (1958), another key figure in human-capital theory, offers schooling as an explanation for the earnings distribution's positive skew. Specifically, he observes an increasing marginal benefit of schooling with respect to income. He acknowledges the plausibility of a correlation between schooling and ability, but argues that, even if abilities were distributed in a way which implies a symmetric earnings distribution, positive skewness would appear as soon as choice of schooling were admitted into the model.

Michael Spence (1973) presents signal theory as an alternative to human-capital theory. He describes hiring as a lottery, where an employer uses past experiences to develop conditional probabilistic assessments based on a candidate's observable characteristics, which in turn determine wage. Spence breaks these characteristics down into two categories: unalterable 'indices,' and 'signals' that can be manipulated by the job candidate. Education falls into the latter; as individuals select their optimal level of education by maximizing the difference between expected wage and the cost of signalling. A critical assumption here is that signalling costs are inversely proportional to productivity. That is, more productive individuals face lower costs (monetary, psychic, time, and the like) of acquiring education than those with lower productivity. Without this assumption all individuals would acquire the same amount of education, and it would lose its power as a signal. Spence concludes that, while education may be an effective signal, it does not necessarily increase an individual's marginal product at all.

Weiss (1995) compares human-capital and signalling theory and argues that the coefficient on education in a standard Mincerian earnings function (described in my Model section) captures both. Economists use this coefficient to infer the returns to schooling, ignoring ability differences, and consequently overestimate the productivity gains from schooling. To quantify this 'ability bias,' Weiss reviews studies such as Altonji (1995), Kang and Bishop (1986), and Meyer (1982), which observe no benefit from additional coursework on grades or test scores. Weiss argues that these estimates, taken together, show that learning explains at most one-quarter of the increased earnings associated with high school, and probably less. The rest is due to signalling. Card (1999) similarly reviews the recent literature yet finds a different result. He views the study of twins as the best available measure of ability bias, based on the assumption that identical twins have identical ability. By comparing the results of twin studies to a Mincer prediction of earnings, he finds that the education coefficient of the latter overestimates the average marginal return to schooling by about 10 to 15 percent, implying a small ability bias. Based on Weiss's and Card's findings, education appears to have value as both a signal of ability and a learning tool. That said, it remains unclear which theory better explains its correlation with wage. The Discussion section of this paper addresses human-capital and signalling theory in the context of my findings, which can be explained by both. My results are thus unable to resolve the human capital-signal debate;

nonetheless, they make valuable contributions to the literature on the returns to schooling, described in the following section.

3 Literature Review

Ontario's elimination of grade 13 provides a unique opportunity to assess its value-added, making the lack of existing literature on this policy change quite surprising. That which does exist focuses predominately on the 2003 double cohort, when both grade 13 and grade 12 students graduated high school. Researchers see the double cohort as a natural experiment, where otherwise identical individuals receive either four or five years of high school, making any difference between them a consequence of an extra year of school.

The most cited paper on this policy change, Krashinsky (2014), examines the performance of the double cohort in an Introduction to Management course at the University of Toronto. Krashinsky surveys the approximately 1,000 students in the class and finds that students who received an extra year of high school performed significantly better in the course than their four-year counterparts, receiving grades that were 5 to 8 percent higher. Further, he observes that their GPAs were half a letter grade higher, a difference that remains statistically significant over the double cohort's four years of university, albeit with some attenuation. He recognizes maturity as a possible confound, as grade 13 graduates were up to a full year older upon entering university, and narrows the age range of his sample to minimize its effect. Another concern is non-compliance, specifically the possibility of students 'fast-tracking' or taking a 'victory lap' to avoid the double cohort. These decisions were typically motivated by concerns over enlarged university applicant pools; however, these cases ended up being far and few between. Nonetheless, Krashinsky accounts for the possibility by using a two-stage least squares approach that targets compliant students.

Morin (2013) and Brady and Allingham (2007) similarly examine the double cohort, but find contradictory results. Morin measures subject-specific human capital, focusing on the effect of an extra year of high school math. He does so by comparing math performance in university to biology performance, as the latter was more or less unaffected by the curriculum compression. Morin uses data from the University of Toronto's Life Sciences program, narrowing his age range to account for maturity, and finds a modest (2.3 percent higher grades) yet statistically significant benefit of an extra year of high school math. Brady and Allingham (2007) survey 250 Faculty of Education students at a small Ontario university, and through one-way ANOVA tests find no significant difference between the graduating classes in terms of academic achievement or social adjustment. They conclude that the four-year curriculum effectively replaced the five-year model.

Sana and Fenesi (2013) analyze the aforementioned literature and critique its reliance on small-scale surveys, arguing that these can misrepresent the population. For example, Krashinsky and Morin both look at University of Toronto students, yet the former's sample had a mean high school GPA of 84 percent, which is considerably lower (two standard deviations!) than the latter's average of 91 percent. Along similar lines, Brady and Allingham's sample was 72 percent female, which is far above the national undergraduate average of 62 percent (Statistics Canada, 2015). Additionally, Sana and Fenesi take issue with treating the double cohort as a natural experiment, arguing that the four-year curriculum in 2003 was not a finished product. They use the Youth in Transitions Survey published by Statistics Canada, and eschew the double cohort by instead comparing Ontario graduates in 2000, pre-policy change, to graduates from other provinces. They find that students with grade 13 had a 2.33 percent higher high school GPA than grade 12 graduates, and were 25 percent more likely to pursue university. When in university their GPAs were 0.99 points higher, leading Sana and Fenesi to conclude that individuals with grade 13 are more likely to succeed than those with grade 12.

Similar studies were done using a near-identical reform in Germany. Büttner and Thomsen (2015) look at a double cohort in Saxony-Anhalt to measure the benefits of higher learning intensity. They record grades at high school graduation across twelve secondary schools and find significant differences between the grade 13 and grade 12 graduates in math and English, but no difference in German literature. They conclude that different subjects require different learning intensities and propose a reallocation of instructional time. Dörsam and Lauber (2015) look at double cohorts in Bavaria and Baden-Württemberg to examine curriculum adjustment, using a difference-in-differences technique and data from the University of Konstanz. They find that grade 12 graduates of the double cohort failed first-year exams at a significantly higher frequency than their grade 13 counterparts. However, this finding turned insignificant when comparing the latter to grade 12 graduates of the following year, supporting a theory of curriculum adjustment.

Pischke (2007) and Oreopoulos (2007) examine the long-run effects of changes in the length of schooling, yet find contradictory results. Pischke measures the effect of a shortened school year using a 1966-67 reform in the West German school system that caused most states to move their start date from spring to autumn, losing about two-thirds of a school year. States adjusted at different paces, resulting in substantial heterogeneity in the length of a school year across birth cohorts and states. Pischke finds negative effects of a shortened school year in the early years of schooling, observing higher rates of grade repetition in primary school and less matriculation to secondary school, but fails to find an effect on long-run earnings and employment. He explains this through both signalling and human capital theory. With respect to the former, he makes the case that a shortened school year should not affect its value as a signal. As for the latter, because curriculums were unaffected by the shortened school year, if teachers were able to convey the same material in a reduced period of time, he argues students would receive the same return as prior cohorts. Oreopoulos (2007) measures the effect of state changes in the high school dropout age on educational attainment and earnings. Students who are below the minimum dropout age are forced to remain in school, thus by increasing the dropout age these students receive additional schooling. Oreopoulos uses American Community Survey and Current Population Survey data, matching individuals to the state policy they would have faced in high school.

A difference-in-differences estimator reveals that raising the school leaving age above 16 increases an individual's years of schooling by 0.13 years and raises weekly earnings by 11 percent. This finding contradicts Pischke; however, the two studies look at different populations. Whereas a shortened school year affects all students, changes in the dropout age affect primarily stereotypical 'low-ability' students who are on the fringe of dropping out. This suggests that the optimal length of schooling may vary by ability level.

While the above studies focus on the benefits of schooling, Holmlund et al. (2008) measure its opportunity cost. They look at the effect of gap years in Sweden, where it is common to take at least one year off before university, and find that each additional gap year results in a 2 percent lower wage at age 35, but fails to affect wage after age 40. They present two explanations for this earnings penalty. First, they argue that the returns to post-university work experience are higher than that of a gap year. Second, by postponing university an individual has less time to invest in skills afterward. Due to diminishing marginal returns to post-university work experience the gap year students eventually catch up; nonetheless each gap year results in a lifetime earnings loss. They conclude that the timing of education needs to be considered when studying its returns.

Thus, while there is substantial literature on the returns to schooling, few scholars look at grade 13 and those that do have key limitations. Sample size, curriculum adjustment, maturity, and noncompliance plague the double-cohort studies and, while Sana and Fenesi attempt to account for this, they fail to consider age and regional differences in their analysis. These concerns motivate my decision to eschew the double cohort in my analysis of grade 13. To mitigate the effect of maturity I expand my timeline looking at individuals up to ten years after grade 12 and, drawing from the compulsory schooling literature I measure returns in terms of earnings and educational attainment. I believe that this novel approach, the methodology of which is described below, allows for a comprehensive analysis of the effects of grade 13.

4 Econometric Model

I use a difference-in-differences technique to estimate the benefits of an extra year of high school, comparing the performance of Ontario students graduating before and after the removal of grade 13, relative to students in British Columbia who experienced no such change. My paper is the first to take such an approach, and in doing so I am able to control for provincial differences as well as macroeconomic trends. I measure the returns to grade 13 in terms of wage as well as educational attainment.

4.1 Wage Model

The econometric model here is loosely based on the Mincer (1974) earnings function, considered "one of the most widely used models in empirical economics" (Lemieux, 2006). Mincer expresses the logarithm of wage as a function of schooling and labour market experience. Similarly, equation 1 estimates the logarithm of an individual's real hourly wage in 2017 dollars, the key predictor being a dummy variable for whether an individual had grade 13.[2] For this to be the case an individual must have graduated from an Ontario high school before 2003; these students are compared to British Columbia students as well as post-policy-change students in Ontario – avoiding the double cohort in 2003. Also included are controls for an individual's highest educational attainment, gender, marital status, province of residence, and survey year.

$$ln(Wage) = \beta_0 + \beta_1 Grade13 + \beta_2 Educ + \beta_3 Sex + \beta_4 MaritalStatus + \beta_5 Province + \beta_6 SurveyYear \quad (1)$$

Educational attainment is a factor variable where individuals are coded as receiving either less than eight years of schooling, some secondary school, a high school diploma, some postsecondary schooling, a postsecondary certificate, a bachelor's degree, or a graduate

2 Real hourly wage was calculated using the Consumer Price Index released by Statistics Canada.

degree. To measure the value-added of grade 13, I restrict my sample to individuals with a high school diploma and beyond.

A concern with including university graduates is that an observed wage gap could merely reflect performance differences in university.[3] Related to this point, Krashinsky found a significant GPA gap between grade 13 and grade 12 graduates in first-year university, which need not be explained by grade 13 because maturity and other factors likely play a role as well. To test this theory and isolate the effect of grade 13, I also fit the model to a subsample of individuals whose highest educational attainment was a high school diploma.

Equation 1 additionally contains dummy variables for gender, marital status, survey year and province. A gender variable controls for the gender pay gap observed empirically. For example, Moyer (2017) finds that women earn $0.87 for every dollar earned by men. Similarly, a marital status variable accounts for the significant marriage pay gap observed by Schoeni (1995) in Canada and other developed countries. Marital status is coded as 1 if an individual is married or living in common-law, and 0 if the person is single, divorced, widowed, or separated. Survey year is included to account for macroeconomic trends and events such as the Great Recession, and the dummy variable for province controls for regional differences between Ontario and British Columbia. British Columbia was chosen as a control province due to its similarities with Ontario. They are the two largest English-speaking provinces in Canada by population and have similar population growth rates, both are composed of around 30 percent visible minorities, and both contain a major city – Toronto and Vancouver respectively (Statistics Canada, 2016).

My paper is the first to measure grade 13's effect in the long run. Specifically, I look at how the effect of grade 13 varies up to 10 years after an individual completed grade 12, in 2.5-year intervals. To calculate this time variable I subtract an individual's age from the year he or she was surveyed and add 18, which I assume to be the typical age at grade 12. Given that only about 15 percent of students in Ontario and British Columbia graduated high school

3 Special thanks to Pedro Girardi for recognizing this potential issue.

at age 17 in 2009, this assumption appears empirically valid (Statistics Canada, 2010). As I have defined it, the time variable allows me to compare individuals with and without grade 13 at the same age, which should mitigate the effect of maturity. An alternative approach would have been to look at time since graduation, which facilitates comparison across years of labour market experience. I elect to use the former method, which penalizes grade 13 graduates for entering the labour market a year later, in accordance with the findings of Holmlund et al.'s gap year study. That is, at a given time a grade 13 graduate will have spent one fewer year at work than a grade 12 graduate, and so will receive raises in wage a year later. Thus, I run four separate regressions predicting the effect of grade 13 on an individual's hourly wage 2.5, 5, 7.5 and 10 years after the person would have completed grade 12. The 2.5-year intervals are due to the coding of age in the data, described in Section 5.

When predicting wage I am limited to an unrepresentative subset of the population that receives an hourly wage. To account for this selection bias, I employ a Heckman two-step correction that builds the probability of an individual working into the estimation of log wage. I use current schooling status, educational attainment, gender, marital status, province of residence, and survey year in a probit model to predict an individual's probability of working, which enters equation 1 in the form of an inverse Mills' ratio.

4.2 Educational Attainment Model

Equation 2 examines the effect of grade 13 on a series of educational attainment dummy variables, using logistic regressions to estimate the odds of completing high school, receiving a bachelor's degree, and completing a graduate degree.

$$Educ = \beta_0 + \beta_1 Grade13 + \beta_2 Sex + \beta_3 MaritalStatus + \beta_4 Province + \beta_5 SurveyYear \quad (2)$$

Note, for ease of interpretation, in equation 2 I define educational attainment cumulatively. That is, an individual with a bachelor's degree is also coded as having received a high school degree. Equation 2 also controls for gender, marital status, survey year, and province. The sample is restricted to individuals with at least some secondary schooling; furthermore, I look exclusively at individuals 10 years after they completed grade 12. This leaves me with a pool of individuals who are affected by grade 13 and have completed or are enrolled in their highest intended level of education.

5 Data

To fit the equations described above I use public-use data from the monthly Labour Force Survey conducted by Statistics Canada. A monthly survey is advantageous over a fixed-year survey (such as a census), as the former allows me to track cohorts over time, whereas the latter favours older cohorts who have spent more time in the labour market. The Labour Force Survey provides individual-level data, and each survey consists of approximately 100,000 observations. Each individual is matched to a high-school cohort based on his or her age and is identified as having grade 13 based on age and province of residence.

I use province of residence as a proxy for province of schooling because the latter is not included in the Labour Force Survey. This assumption is not perfect due to interprovincial migration, yet it is frequently made in the literature on U.S. compulsory schooling. Oreopoulos (2007), Acemoglu and Angrist (2000), and Lleras-Muney (2002) find that state of birth and state of residence produce similar estimates of the returns to schooling. This, coupled with the fact that migration is far more prevalent in the U.S. than in Canada—only 59 percent of Americans live in the state of their birth, compared to 85 percent in Canada—suggests that province of residence is a valid proxy for province of schooling (U.S. Census Bureau, 2011; Statistics Canada, 2017a). Using data from the 2011 National Household Survey, which includes information on migration patterns, I observe that 79 percent of Ontario residents age 29 and below lived there 5 years ago, and 87 percent of postsecondary-educated Ontario residents age 29 and below received their highest degree in Ontario, with similar results for British Columbia.[4] This finding supports my decision to ignore migration, yet some individuals will inadvertently be matched to the wrong province. Nonetheless, changes in high school policy are unlikely to cause people to move; thus, any error that exists should be minor and random.[5]

I focus on cohorts that graduated between 2000 and 2002 (before the policy change), and 2004 and 2006 (after the policy change), being careful to avoid the double cohort in 2003. For consistency, I avoid 2003 for British Columbia as well. The year 2006 serves as a natural upper bound, as in 2007 Ontario increased its minimum high school dropout age from 16 to 18. British Columbia's high school dropout age was constant at 16 throughout this time period; thus, by avoiding 2007 I ensure both provinces had the same dropout age. In doing so, I can ignore the positive effect of an increased dropout age found by Oreopoulos and others. For symmetry reasons, I choose 2000 as my lower bound. Cohort year is calculated by subtracting age from survey year, and adding either 18 or 19 depending on the province and year the individual entered high school. The Labour Force Survey codes ages in 2 or 3-year intervals: I exclude individuals whose cohort interval includes the double cohort, but use the average age when calculating the time-elapsed variable, causing it to increment by 2.5 years. The final data set includes 288,655 observations.

Table 1 provides frequency statistics on the various variables used in equations 1 and 2. The majority of the sample comes from Ontario, an unsurprising result as its population is roughly triple that of British Columbia (Statistics Canada, 2017b). Equally unsurprising, most individuals in the sample did not have grade 13, as only pre-2003 Ontario students would have experienced it. The sample is 49.3 percent male and 50.7 percent female, in line with the national averages of 49.6 and 50.4 respectively (Statistics Canada, 2017b). Similarly, the sample's marital status breakdown matches the Canadian average for ages 20 to

4 This analysis is based on the Statistics Canada *2011 National Household Survey Public Use Microdata File*. All computations, use, and interpretation of these data are entirely that of Avinash Moorthy.

5 An email exchange with Philip Oreopoulos, an expert on Canadian education and compulsory schooling, supports this notion. He argues that migration "just creates measurement error," so I should be "safe to ignore it," adding that he has never received much of a problem about it from referees.

29 (Statistics Canada, 2017c). Additionally, the sample includes 64,039 individuals still in school, which is ample for the current schooling variable to be used to estimate probability of work. Lastly, observations are relatively spread out across survey year, which ranges from 2002 to 2016, as well as across different values of the time variable, indicating numerous cohorts with varying amounts of labour market experience. All in all, table 1 shows a sample that is representative of the greater population and appropriate for econometric analysis.

Table 1: Frequency Table

Variable	Count	Percent
Province		
Ontario	206,758	71.6
British Columbia	81,897	28.4
Grade 13		
Yes	103,756	35.9
No	184,899	64.1
Sex		
Male	142,230	49.3
Female	146,425	50.7
Marital Status		
Single	200,491	69.5
Married	88,164	30.5
Currently in School		
Yes	64,039	22.2
No	224,616	77.8
Time		
2.5	61,453	21.3
5	88,286	30.6
7.5	57,302	19.9
10	81,614	28.3
Survey Year		
2002	11,182	3.9
2003	14,775	5.1
2004	19,277	6.7
2005	21,856	7.6
2006	6,560	2.3
2007	26,073	9
2008	30,231	10.5
2009	19,093	6.6
2010	42,504	14.7
2011	28,972	10
2012	14,678	5.1
2013	13,946	4.8
2015	19,247	6.7
2016	20,261	7
Overall	288,655	100

Table 2 describes the relationship between real hourly wage and the various components of equation 1. Note the sample size in table 1 is larger than table 2 as the latter looks exclusively at wage earners. The resulting selection bias necessitates the Heckman two-step procedure described earlier. Table 2's provincial breakdown reveals that individuals from British Columbia receive a higher wage, justifying the inclusion of a provincial control variable in my model. Other results are quite predictable: men are associated with a higher wage than women, married individuals earn more than single individuals, and wages increase with educational attainment as well as with time elapsed. Table 2 also shows wages increasing across survey years even after controlling for inflation, making survey year an obvious control variable. Surprisingly, individuals without grade 13 are associated with a higher wage than those who received grade 13. However, this correlation may be explained by any one of the variables mentioned above.

Table 2: Real Wage Summary Statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Province					
Ontario	142,774	16.79	8.54	2.20	96.15
British Columbia	55,997	17.79	8.60	3.00	86.54
Grade 13					
Yes	72,435	18.80	9.04	3.25	100.57
No	126,336	19.32	9.02	2.48	97.72
Sex					
Male	98,773	20.16	9.40	2.48	100.57
Female	99,998	18.12	8.53	2.62	88.40
Marital Status					
Single	135,557	17.84	8.37	2.64	97.72
Married	63,214	21.91	9.75	2.48	100.57
Educational Attainment					
Some High School	12,256	15.76	6.63	2.48	70.74
High School Graduate	47,987	16.77	7.35	2.64	87.62
Some Postsecondary	33,219	15.53	6.68	2.94	96.78
Postsecondary Certificate	63,526	20.14	8.68	2.62	97.62
Bachelor's Degree	32,949	23.54	10.52	3.42	100.57
Graduate Degree	8,834	26.48	11.44	3.80	88.22
Time					
2.5	39,457	13.84	4.76	2.62	58.21
5	60,325	16.28	6.14	2.48	78.37
7.5	40,721	21.52	9.53	3.28	92.15
10	58,268	23.99	10.28	3.22	100.57
Survey Year					
2002	7,125	13.59	4.62	3.25	44.83
2003	9,378	13.56	4.66	3.52	53.17
2004	13,197	15.63	5.90	3.90	51.23
2005	15,132	15.84	6.04	3.61	48.28
2006	4,739	16.55	6.31	4.29	53.39
2007	17,928	16.85	7.84	3.54	82.83
2008	20,546	17.75	8.26	2.62	82.05
2009	13,556	23.72	10.43	3.39	100.57
2010	29,199	20.21	9.20	2.48	97.62
2011	19,516	18.26	7.89	2.64	89.86
2012	10,339	21.64	9.45	3.28	86.00
2013	9,715	21.59	9.75	3.42	81.81
2015	13,773	24.06	10.25	3.58	96.03
2016	14,628	24.26	10.23	3.22	97.72
Overall	198,771	19.13	9.03	2.48	100.57

Table 3 looks at relevant variables from equation 2 and their relationship with educational attainment. Per equation 2's specification, educational attainment is expressed cumulatively here, so an individual with a bachelor's degree, for example, is coded as receiving a high school diploma as well. Further, I narrow the six education codes from the data to just three: percentage of high school graduates, bachelor's degree recipients, and graduate degree recipients. According to table 3, both provinces have similar high school attainment rates, but Ontario students appear more likely to receive a university education. The relationship between gender and education follows the national trend, which shows women pursuing education more often than men (Ferguson, 2016). Single individuals are associated with a higher dropout rate than those who are married, unsurprising given the age at which one typically drops out. However, single individuals are also more likely to pursue a university education. This puzzling observation might be explained by a theory that individuals delay marriage until they have completed schooling. Lastly, while high school graduation rates appear to stay relatively constant across survey years, table 3 shows a noticeable increase in the percent of individuals with a bachelor's degree over time, corresponding to the national trend (Statistics Canada, 2017d). Individuals with grade 13 have similar high school graduation rates as those without the extra year; however, the latter are associated with a higher percentage of bachelor's degrees. Once again, this could be due to grade 13 or alternatively one of the aforementioned confounds. In sum, tables 1 through 3 are in line with empirical and theoretical evidence suggesting a representative sample. This implies that the findings described in the following section should be generalizable to the greater population.

Table 3: Educational Attainment Summary Statistics

Variable	Obs	% High School	% Bachelor's	% Graduate
Province				
Ontario	56,945	93.43	30.84	9.22
British Columbia	24,669	93.26	28.25	6.19
Grade 13				
Yes	29,669	93.44	28.42	8.16
No	51,945	93.34	30.99	8.55
Sex				
Male	39,593	92.04	24.36	6.71
Female	42,021	94.64	35.42	9.80
Marital Status				
Single	38,797	92.69	30.43	8.54
Married	42,817	94.00	29.72	8.09
Survey Year				
2009	14,979	93.46	28.13	8.88
2010	20,647	92.95	28.20	7.31
2011	6,480	93.06	28.12	5.83
2015	19,247	93.74	31.02	8.32
2016	20,261	93.50	33.07	9.65
Overall	81,614	93.38	30.05	8.30

6 Results

6.1 Wage Model

Table 4 below shows the results of four regressions predicting an individual's log real wage (in 2017 dollars) 2.5, 5, 7.5, and 10 years after grade 12. Using a method of ordinary least squares, I regress log wage on the following set of explanatory variables: whether an individual had grade 13, his or her educational attainment, gender, marital status, province of residence, and survey year. This is seen in the top half of table 4. Note that coefficients for survey year are not reported due to space considerations but are available upon request. By focusing on wage earners who are not representative of the greater population I encounter a selection bias. A Heckman two-step correction is used to account for this. Using a probit model I regress an individual's probability of work on the following explanatory variables:

whether an individual is currently enrolled in school, his or her gender, marital status, educational attainment, province, and survey year. This is shown in the lower half of table 4. The Heckman correction produces an inverse Mills' ratio that is inversely related to the probability of an individual being selected into the sample; its coefficients are presented at the bottom of table 4. The sample is divided into four separate regressions to measure how the effect of grade 13 varies the longer the person is in the work force. The four columns of table 4 show the results of regressions 2.5, 5, 7.5, and 10 years after grade 12, all with sample sizes of at least 50,000 observations. Robust standard errors are used to account for heteroskedasticity, and the observations are weighted to represent the greater Canadian population. This is done using the sampling weights provided by the Labour Force Survey, which detail the number of subjects in the population that each observation represents.

Table 4: Wage Regression Results

	(1) Time = 2.5	(2) Time = 5	(3) Time = 7.5	(4) Time = 10
log(Wage)				
Grade 13: Yes	0.0884***	0.0355***	0.0247	-0.0370***
	(0.0101)	(0.00910)	(0.0131)	(0.0110)
Educ: Some postsecondary	0.000429	-0.0000966	-0.0191*	0.0142
	(0.00434)	(0.00468)	(0.00943)	(0.00963)
Educ: Postsecondary certificate	0.0592***	0.0945***	0.0952***	0.111***
	(0.00527)	(0.00433)	(0.00673)	(0.00643)
Educ: Bachelor's Degree	0.0789***	0.180***	0.214***	0.247***
	(0.0181)	(0.00586)	(0.00777)	(0.00728)
Educ: Graduate Degree	0.0895**	0.172***	0.313***	0.345***
	(0.0343)	(0.0158)	(0.0124)	(0.00926)
Sex: Female	-0.116***	-0.116***	-0.139***	-0.126***
	(0.00385)	(0.00353)	(0.00519)	(0.00457)
Marital Status: Married	0.0756***	0.0756***	0.0608***	0.0624***
	(0.00761)	(0.00429)	(0.00525)	(0.00449)
Prov: British Columbia	0.122***	0.0542***	0.0467***	0.0165*
	(0.00598)	(0.00552)	(0.00792)	(0.00678)
Constant	2.587***	2.731***	2.961***	3.140***
	(0.0119)	(0.0107)	(0.0157)	(0.0146)
Probability of Work				
Currently in School: Yes	-0.887***	-0.802***	-0.831***	-0.670***
	(0.0145)	(0.0126)	(0.0192)	(0.0191)
Educ: Some postsecondary	0.0580***	0.152***	-0.00882	0.151***
	(0.0163)	(0.0165)	(0.0261)	(0.0243)
Educ: Postsecondary certificate	0.112***	0.298***	0.252***	0.327***
	(0.0199)	(0.0156)	(0.0192)	(0.0163)
Educ: Bachelor's Degree	-0.0611	0.209***	0.266***	0.498***
	(0.0518)	(0.0187)	(0.0213)	(0.0182)
Educ: Graduate Degree	0.0913	0.0780	0.304***	0.423***
	(0.104)	(0.0401)	(0.0347)	(0.0242)
Sex: Female	0.157***	0.0814***	0.0407**	-0.114***
	(0.0141)	(0.0119)	(0.0149)	(0.0124)
Marital Status: Married	-0.189***	-0.0445**	-0.0766***	0.0159
	(0.0271)	(0.0152)	(0.0154)	(0.0122)
Prov: British Columbia	0.0286	0.0133	-0.0293	-0.0446**
	(0.0176)	(0.0146)	(0.0181)	(0.0153)
Constant	0.644***	0.657***	0.650***	0.481***
	(0.0204)	(0.0182)	(0.0229)	(0.0199)
Inverse Mills' Ratio	-0.106***	-0.140***	-0.171***	-0.263***
	(0.0047)	(0.0055)	(0.0097)	(0.0109)
Observations	54440	80968	53158	76209

Standard errors in parentheses

Note: Survey year coefficients not shown, results available from the author.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The value added by grade 13 is captured by a dummy variable for whether an individual would have had it, based on age and province of residence. Grade 13 results in an 8.8 percent higher wage 2.5 years after grade 12, which is statistically significant at a 99.9 percent level. This effect remains statistically significant 5 years out but is considerably weaker, amounting to a 3.6 percent higher wage. At 7.5 years the returns to grade 13 become statistically insignificant, and at 10 years they regain significance but in the opposite direction, with grade 13 resulting in a 3.7 percent wage penalty. In short, table 4 shows a significant benefit to grade 13 early on that wanes over time, eventually turning into a penalty. This finding will be explored further in the Discussion section.

The coefficients on education, gender, marital status, province, and survey year are largely significant, justifying their inclusion in the model. For the most part, higher levels of educational attainment result in significantly higher wages. I observe that, relative to persons with a high school diploma, individuals with a postsecondary certificate have an 11.1 percent higher wage 10 years after grade 12 – when an individual should have completed his or her investments in education. Similarly, those with bachelor's and graduate degrees have wages that are 24.7 and 34.5 percent higher. However, individuals with some postsecondary schooling fail to differ significantly from high school graduates. Table 4 shows a statistically significant gender pay gap, where women receive between an 11.6 and 13.9 percent lower wage than men. With regard to marital status, married individuals receive significantly higher wages than those who are single, with the magnitude of the effect ranging from 6.1 to 7.6 percent. These findings are consistent with the empirical and theoretical evidence presented earlier. Lastly, both province of residence and survey year have statistically significant effects on log wage. Residing in British Columbia has a positive effect on wage, relative to Ontario, which makes sense, given Vancouver's high cost of living and inflated housing market compared to Toronto.[6] Survey year coefficients are not particularly interesting and are omitted from table 4 due to space considerations.

6 Cost of living data is from the Numbeo crowd-sourced global database.

The Heckman two-step correction allows me to use the entire sample of individuals with a high school degree and beyond, as opposed to a subsample of just wage earners. The bottom half of table 4 shows the results from the first step of the Heckman process: estimating the probability of an individual working. As expected, individuals in school have a significantly lower probability of working than those who are not. And, with the exception of province of residence, the rest of the controls have statistically significant effects as well. The Heckman correction produces inverse Mills' ratios, the coefficients of which are shown at the bottom of Table 4. They are statistically significant and negative, implying that the unobservable characteristics that increase the probability of an individual working tend to decrease their log wage. This is consistent with Nicodemo (2007), who finds a negative coefficient for most European countries. She argues that individuals with higher probabilities of working tend to have lower reservation wages, which are likely determined by one's pre-existing wealth and savings.

By including individuals with degrees beyond the high school level I run the risk of attributing wage differences caused by university performance to grade 13. For example, if a grade 13 graduate performed better in university due to being a year older, this would be reflected in a higher GPA, which would likely inflate wage upon entering the labour market. To assess the validity of this concern, table 5 below examines the effect of grade 13 on log wage using the same procedure as table 4, but applied to a subsample of individuals with exclusively a high school diploma. This results in a much smaller, but still sufficiently large, sample ranging from 12,657 to 22,593 observations across the four regressions.

Table 5: Wage Regression Results: Only High School Graduates

	(1) Time = 2.5	(2) Time = 5	(3) Time = 7.5	(4) Time = 10
log(Wage)				
Grade 13: Yes	0.0799***	0.0699***	-0.0270	-0.0302
	(0.0161)	(0.0149)	(0.0262)	(0.0242)
Sex: Female	-0.140***	-0.154***	-0.222***	-0.198***
	(0.00592)	(0.00602)	(0.0104)	(0.00997)
Marital Status: Married	0.0796***	0.0657***	0.0308**	0.0537***
	(0.0106)	(0.00701)	(0.0105)	(0.00970)
Prov: British Columbia	0.112***	0.0614***	0.0585***	0.0878***
	(0.00880)	(0.00964)	(0.0159)	(0.0147)
Constant	2.599***	2.690***	3.002***	3.073***
	(0.0182)	(0.0173)	(0.0318)	(0.0300)
Probability of Work				
Currently in School: Yes	-0.877***	-0.794***	-0.729***	-0.704***
	(0.0239)	(0.0258)	(0.0510)	(0.0553)
Sex: Female	0.134***	-0.0553*	-0.134***	-0.173***
	(0.0230)	(0.0222)	(0.0292)	(0.0260)
Marital Status: Married	-0.181***	-0.104***	0.0190	0.0724**
	(0.0395)	(0.0264)	(0.0297)	(0.0258)
Prov: British Columbia	0.150***	0.150***	-0.0322	0.0352
	(0.0284)	(0.0267)	(0.0350)	(0.0318)
Constant	0.730***	0.763***	0.636***	0.455***
	(0.0301)	(0.0305)	(0.0378)	(0.0343)
Inverse Mills' Ratio	-0.103***	-0.109***	-0.048	-0.092***
	(0.0073)	(0.0086)	(0.0270)	(0.0171)
Observations	20568	22593	12657	15892

Standard errors in parentheses

Note: Survey year coefficients not shown, results available from the author.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Similarly to table 4 I observe a positive and statistically significant effect of grade 13 on log wage 2.5 years after grade 12. This effect persists 5 years out, falling from 8 percent to 7 percent, a much smaller change than in table 4. At 7.5 years the effect of grade 13 becomes statistically insignificant, and remains this way 10 years after grade 12. This runs contrary to table 4, which finds a wage penalty after 10 years. The remaining results from table 5's prediction of log wage concur with those of table 4. Table 5 shows a gender wage gap of between 14 and 22 percent, a wage premium of being married ranging from 3.1 to 8 percent, and lastly a higher wage for those residing in British Columbia. Once again, survey year coefficients are omitted but are available upon request.

In table 5's estimation of probability of work, the key predictor of whether an individual is currently enrolled in school is statistically significant at a 99.9 percent level. The coefficient for gender is significant as well, as is the coefficient for marital status in three out of four regressions. Province of residence appears to be a significant predictor up to 5 years after grade 12, but becomes insignificant after this point. The resulting inverse Mills' ratio coefficients are highly significant, with the exception of the regression at 7.5 years after grade 12. As was the case in table 4, the coefficients on this ratio are negative, suggesting that the unobservable factors influencing probability of work affect log wage in the opposite direction.

6.2 Educational Attainment Model

Table 6 shows the results of 3 logit regressions predicting an individual's educational attainment. The explanatory variables are whether an individual had grade 13, his or her gender, marital status, province of residence, and survey year. The dependent variables are the probability of an individual graduating high school, receiving a bachelor's degree, and receiving a graduate degree. These variables are coded cumulatively such that an individual with a graduate degree is assumed to possess the first two as well. I only look at observations with 10 years elapsed since grade 12 to ensure that an individual would have time to pursue their highest intended level of education. To measure the probability of an individual graduating high school, those who dropped out are reintroduced, resulting in a sample size of 81,614 observations. As in the wage model, robust standard errors are used to account for heteroskedasticity, and the observations are weighted to represent the population. Coefficients are displayed in exponentiated form, representing the odds of receiving a specific degree.

Table 6: Educational Attainment Regression Results

	(1) High School	(2) Bachelor's	(3) Graduate
Grade 13: Yes	1.294**	0.944	1.001
	(0.118)	(0.0475)	(0.0918)
Sex: Female	1.593***	1.597***	1.399***
	(0.0577)	(0.0305)	(0.0440)
Marital Status: Married	1.062	0.888***	0.979
	(0.0380)	(0.0168)	(0.0304)
Prov: British Columbia	1.082	0.839***	0.714***
	(0.0619)	(0.0243)	(0.0352)
Survey Year: 2010	1.032	1.030	0.891*
	(0.0601)	(0.0328)	(0.0454)
Survey Year: 2011	1.235*	1.155*	1.051
	(0.125)	(0.0655)	(0.109)
Survey Year: 2015	1.373**	1.232***	1.108
	(0.137)	(0.0672)	(0.109)
Survey Year: 2016	1.341**	1.381***	1.279*
	(0.132)	(0.0751)	(0.124)
Observations	81614	81614	81614

Exponentiated coefficients; Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

According to table 6, grade 13 increases an individual's odds of graduating high school by 29.4 percent, which is statistically significant at a 99.9 percent significance level. However, it fails to have a significant effect on the likelihood of receiving a Bachelor's or Graduate degree. As for the control variables we see mixed results. Women are significantly more likely to pursue education than men; specifically they are 59.3 percent more likely to graduate high school, and are 59.7 and 39.9 percent more likely to receive a Bachelor's and Graduate degree, respectively. Married individuals have a significantly lower probability of receiving a Bachelor's degree than those who are single; however, marital status fails to affect the probability of receiving a high school or graduate degree. Similarly, individuals from British Columbia are significantly less likely to pursue higher education than Ontario residents, but perform roughly the same at the high school level. Lastly, the odds of graduating high school and receiving a Bachelor's degree are observed to increase significantly across survey years. Given that each survey year corresponds to a cohort 10 years after grade 12, this result implies that high school and university attainment increase over the time period examined. The implications of these results, as well as those pertaining to the wage model will be discussed in the following section.

7 Discussion

7.1 Wage Model

Table 4 examines individuals with at least a high school diploma and shows significant returns to a fifth year of high school 2.5 years after grade 12. This effect decreases in magnitude 5 years out yet remains statistically significant. At 7.5 years the benefit of grade 13 becomes insignificant, and at 10 years I actually observe a significant wage penalty. Table 5, which examines individuals with exclusively a high school degree, shows a similar trend, but with a far smaller decrease from 2.5 to 5 years, and no penalty after 10 years. Taken together, the two tables suggest an initial benefit to grade 13 that evaporates over time and potentially turns into a wage penalty.

Tables 4 and 5 coupled with the findings of Krashinsky and others show that grade 13 has a short-term benefit. However, the exact length of this benefit is unclear. For those with just a high school degree, the initial effect is more or less intact 5 years out. However, in table 4, which includes university graduates, the benefit is drastically reduced by this time. One possible explanation is that the effect varies by 'ability' level. Whereas table 4 looks at a range of abilities, table 5 studies stereotypically lower-ability individuals who fail to attain higher education levels. The latter appear to benefit more from an extra year of school, which supports findings of Oreopoulos and others who show that forcing low-ability students to stay in school significantly boosts their wages. Alternatively, confounding factors such as university performance could mask the results of table 4, whereas table 5 may represent the true benefit of grade 13. Regardless, in both tables 4 and 5 the observed benefit disappears 7.5 years after grade 12. So, while grade 13 appears to have short to medium-term value, it does not help significantly in the long run.

The results of tables 4 and 5 diverge once again at 10 years out. Specifically, the latter shows no difference between grade 12 and grade 13 wages, whereas the former suggests a wage penalty of an extra year. On the outset a negative effect may seem improbable, but it can be rationalized by considering the opportunity cost of an extra year. As Holmlund et al. (2008) showed in their study of gap years, delayed entrance into the labour market results in a significant earnings penalty. Because the time variable used in my analysis compares individuals across age, grade-13 students will have spent one fewer year working. As a result, they will be one year behind in experience and consequently will receive raises in wage a year later.

If the returns to grade 13 exceed the opportunity cost we expect a net benefit, as was shown to be the case in the short run. But during a period of sharp earnings growth it is plausible for the opportunity cost to exceed the returns to an extra year. In this case, we would expect a wage penalty. According to a report by the Federal Reserve Bank of New York, the average worker receives his or her biggest upswing in wage between age 25 and 35 (Guvenen et al., 2015). An individual with 10 years elapsed since grade 12 falls squarely in this interval, explaining Table 4's finding of a net wage penalty. Further, the New York Fed report notes that the lowest earners fail to receive much of a boost in wage during this time period. Individuals with just a high school degree typically receive below-average wages, which would explain table 5's finding of no wage penalty 10 years out (Bureau of Labour

Statistics, 2015). Lastly, the report – which uses U.S. Social Security Administration data – finds that earnings growth stagnates after the first 10 years of a career. This suggests that the grade 13 graduates eventually catch up, eliminating the wage penalty observed 10 years after grade 12. Given the recency of the policy change examined in this paper, I am unable to confirm whether this in fact happens. After more time has elapsed, researchers may want to look at whether the wages of grade 13 and grade 12 graduates converge, and when.

In short, tables 4 and 5 show short-run returns to grade 13 that dissipate over time, and perhaps result in a wage penalty. This is likely due to grade 13's opportunity cost of delayed labour market entrance, which is particularly pronounced between the ages of 25 and 35 when the average worker experiences his or her sharpest earnings growth. Ultimately, when this growth stagnates I expect the wage of grade 13 and grade 12 graduates to be approximately the same. Future research is needed to identify this point of convergence.

The findings described above are consistent with theories of both human capital and signalling. An initial benefit to grade 13 might suggest that five-year graduates are more productive from their extra year of learning. However, a signalling explanation could say that an extra year of high school requires greater ability, which manifests itself as a higher wage. The decrease and disappearance of the initial benefit can also be explained by both theories. A signal theorist would argue that other factors such as maturity explain the initial benefit, whereas the signal value of a high school education was unchanged by the extra year. On the other hand, Gary Becker might point to the trade-off between schooling and on-the-job training, which he argues are both investments in human capital. A grade 13 graduate receives an extra year of the former while sacrificing a year of the latter – if their returns are similar, then we expect little to no net benefit of an extra year. All in all, this paper's findings can be explained by both human capital and signalling theories, and thus cannot resolve the debate on the source of education's value.

7.2 Educational Attainment Model

Table 6 reveals that grade 13 significantly increases an individual's likelihood of graduating high school but has no effect on higher levels of education. On the outset this finding seems unintuitive; if anything, the daunting presence of an extra year should encourage higher dropout rates. But Oreopoulos shows a similar result, observing heightened educational attainment as a consequence of increasing the high-school dropout age. Sana and Fenesi find that students with grade 13 had a 2.33 percent higher high-school GPA than grade 12 graduates. Herein lies a possible explanation: if grade 13 students perform better in high school due to a human capital gain, they would be able to get a better job upon graduating, thus raising the opportunity cost of dropping out. Further, if employers believe grade 13 adds human capital value (even if this is not the case!) and pay accordingly, the opportunity cost of dropping out rises yet again. That is, if the sheepskin effect of a 5-year degree is greater than that of a 4-year degree, students in the former system have a stronger incentive to graduate. Table 5's finding of an initial wage premium for five-year high school graduates supports this theory, as the promise of a higher wage serves as a strong incentive to graduate. In sum, grade 13 appears to raise the likelihood of an individual graduating high school, a finding that is plausible given the incentives associated with the initial wage premium of an extra year. However, at education levels beyond high school both grade 13 and grade 12 graduates encounter the same sheepskin effect, and thus face the same incentives. This would explain why grade 13 fails to significantly increase the attainment of higher levels of education.

7.3 Implications and Policy Implications

This paper provides mixed evidence on the value of grade 13. I find definite short-run advantages, specifically a wage premium and an increased high-school graduation rate. Yet these benefits evaporate over time, and may turn into a temporary wage penalty because of delayed entrance into the labour market. Based on the age-earnings profile of a typical individual, I suspect that in the long run grade 13 and grade 12 graduate wages converge.

Both a human capital and a signalling argument can be made to resolve grade 13's lack of long-term benefit; thus, this paper is unable to contribute toward the debate on the source of education's value. That said, this finding does encourage closer examination of high school's importance. If education has learning value, grade 13's insignificant effect on wage suggests diminishing returns to high school. This begs the question: what is the value-added of grade 12? Or grade 11? Similarly, if education works as a signal, then how would one fewer year of high school affect its signalling ability? Given that the annual per-student cost of high school in the U.S. is a whopping $12,509, determining the value of a high school education has major policy implications (NCES, 2017). Grade 13 fails to add long-run value, and thus is not worth funding. But if an analysis of grade 12 produces similar results, then we might be better off without it, too. Clearly this topic needs further research, the specifics of which will be discussed in the following section.

7.4 Limitations and Future Research

Before concluding it is important to address the inherent limitations of my analysis. As discussed earlier, province of residence is an imperfect proxy for province of schooling, resulting in individuals being mismatched due to interprovincial migration. This creates random noise in my data, magnified the longer an individual has been out of school. Similar mismatches occur due to imprecise age estimates. Information on age was provided in intervals, so by averaging them an individual's age will often be incorrect. And by assuming the graduation age is 18, some students, albeit a minority, who graduate at 17 will be assigned to the wrong cohort. Further, information on whether a student graduated in 4 or 5 years is not included in the Labour Force Survey. The vast majority of students followed the rule in place at the time; nonetheless, students who took a victory lap or fast-tracked are mismatched in my analysis. The finding of significant results suggests that these mismatches are not overly concerning; that said, they could understate the significance of coefficients observed.

Another key limitation is with regard to the minimum wage, which my analysis does not control for. Over the course of my timeframe the minimum wage changed several times and differed by province. Changes in the minimum wage would affect high school graduation rates by altering the opportunity cost of school, and would inflate wages for workers who were previously earning below it. Thus the postsecondary educated individuals of table 4 are unlikely to be affected by it, as they receive higher wages to begin with, but the minimum wage might affect the results of table 5, which looks exclusively at high school graduates.

Lastly, my paper is limited by the recency of the policy examined. This is evident in my determination of highest educational attainment. I am able to examine individuals in my sample up to 10 years after grade 12, after which point I expect the majority of them to have completed all of their intended education. Those who have not will be mismatched. For example, it is fairly common to work for a few years before attending graduate school; my timeframe does not extend far enough to account for this possibility. Additionally, my limited timeframe prevents me from verifying that grade 13 and grade 12 wages do indeed converge at some point.

My findings and limitations encourage future research. After more time has elapsed, researchers will want to compare grade 13 and grade 12 wages to identify a convergence point. In doing so they should be able to estimate the lifetime earnings benefit or loss associated with grade 13. And with regard to the value of a high school education, if a state or province were to eliminate grade 12, an analysis akin to mine should be able to measure its value added. Lastly, researchers will want to perform similar analyses on the double cohorts observed in Germany to verify the representativeness of the Ontario findings. With these next steps complete, we should be able to once and for all evaluate the value of a high school education.

8 Conclusion

My paper takes advantage of a unique policy change to measure the returns to an extra year of high school. I first examine the effect of grade 13 on an individual's wage and find a significant short-run benefit that dissipates over time, and potentially turns into a wage penalty. This penalty occurs around the time of an individual's sharpest earnings growth, suggesting that the opportunity cost of grade 13 – entering the labour market a year late – dominates its benefits. Specifically, by spending an extra year in high school, an individual begins working a year later, and thus receives raises in wage a year late. Earnings growth stagnates over time, and so in the long run I expect the grade 13 students to catch up. Grade 13's limited effect on long-term wage implies minimal value-added.

I next look at grade 13's effect on educational attainment and observe significantly higher high-school graduation rates. I explain this through the sheepskin effect: if employers believe an extra year enhances human capital and pay accordingly, grade 13 students have a stronger incentive to graduate high school. In support of this idea, I find that high school graduates with grade 13 receive significantly higher wages than those with grade 12 upon entering the labour market. However, beyond the high-school level, grade 13 fails to affect an individual's likelihood of receiving a degree, limiting the educational benefit of grade 13 to the minority of the population with only a high school degree.

The results of this paper find grade 13 to be largely ineffective. In showing this, my paper motivates a future area of study, as it is reasonable to question the benefits of grade 12. Studying the value-added of individual years of high school has major policy implications, given the exorbitant cost of funding high schools, and future research would go a long way toward determining the optimal length of high school. I hope that-with continued work in this area, governments will be able to provide a productive and cost-effective education for the generations to come. In conclusion, I believe my findings are an important addition to the literature on the returns to schooling, and should cause students across the world to heave a sigh of relief. Clearly, we do not need an extra year of high school.

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