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The health returns of attending university for the marginally eligible student

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Abstract

A key policy question is whether continued expansion of university education is beneficial for the marginally eligible student. In this paper we exploit an arbitrary university eligibility rule combined with regression discontinuity design to estimate the causal effect of university attendance on healthcare utilization amongst young adults in Sweden. We find that the eligibility rule leads to a clear jump in university attendance of between 10% and 14% points for both males and females. 2SLS estimates find that a 10% point increase in university attendance causes a roughly one percentage point increase in hospital admissions due to mental ill health for males, almost exclusively related to alcohol and narcotics. Our findings for females, however, imply the opposite, suggesting that university attendance decreases hospital admissions related to mental health. The results for males sit in contrast to results from previous studies, and suggest that the effect of university education on health for the male student at the margin of eligibility is different to that of the average student.

KEYWORDS
demand for health, regression discontinuity design, the health returns of education

JEL CLASSIFICATION
I10, I23, I26

1 | INTRODUCTION

The expansion of university education in Sweden and most other developed countries over the past decades has led to greater opportunities for many people. However, continued expansion of university education hinges critically on the expectation of the benefits outweighing the costs for students at the margin of eligibility. A student at the margin of eligibility may exhibit very different returns compared with the average university student. In order to make such a calculation, it is important to consider the potential health benefits of university education. Furthermore, and from a more general perspective, an understanding of the impact university education can have on health and health-related behaviors will benefit our understanding of the real causes of the well established correlation between education and health (Mackenbach et al., 2003).

A number of theories support the hypothesis that attending university can improve individual health, even in young adulthood. Increased education may have an effect through its impact on health production, or through its impact on financial resources, preferences or self-empowerment, or through improved understanding or access to information that helps improve health (see e.g., Cutler & Lleras-Muney, 2006; Grossman, 2006). The available quasi-experimental research which estimates the causal effect of years of university education on health relies entirely on the long-term health impacts induced by the Vietnam Draft in the US, and finds important and positive health benefits.[1] However, this evidence may not capture the health returns of university for the student who is at the margin of attending/not attending university - students who are, by definition, lower down the ability distribution than the mean university student. In this paper we investigate the impact of university attendance on health for the student at the margin of eligibility in Sweden. Our estimation strategy employs an arbitrary university eligibility threshold as an instrumental variable for university attendance and we identify the impact of the eligibility threshold by way of Regression Discontinuity Design (RDD).

The present study considers health outcomes in early adulthood. The demand for health models of Grossman (1972) and more recently Grossman (2000) and Galama et al. (2018) suggest that there are only likely small differences in overall health in young adulthood caused by increased knowledge capital. Instead, positive impacts of education are likely to be observed in health-related behaviors, either through the direct effect of improved knowledge or the indirect pathway of influencing discount rates or time preferences as discussed in Cutler and Lleras-Muney (2006). Heckman (2012) notes that investments affecting non-cognitive skills are possibly more effective during the later stages of development. Non-cognitive skills such as self-esteem, self-efficacy, and disenfranchisement are, by their very nature, closely related to measures of mental health, such as depression, anxiety and worry. We therefore consider healthcare use in relation to risky health-related behaviors and mental ill health.

Attending university not only implies the opportunity to gain more years of education, but also an exposure to a potentially different peer group mix compared to what the person may have encountered had they not attended university. Peers could share important health information or impact social norms and adoption of health behaviors, particularly in settings where perceptions are in a formative stage. For example, Robalino and Macy (2018) suggest peers have a significant impact on the prevalence of smoking among high-school students, and Gaviria and Raphael (2001) find peer influence plays an important role in drug use and alcohol consumption among 10th graders. Utilizing the co-existence of two different school systems within the same country, Fischer et al. (2021) analyze the extent to which different socio-economic groups are affected by de-tracking, that is, moving away from the system of separating students by ability in secondary school. Contrary to the results of several previous studies, their findings suggest that de-tracking and the resulting increase in mixed peer groupings, is harmful to health across the board, and not only in higher socioeconomic groups. Overall, university attendance can be said to entail health-behavior effects, due to exposure to a different peer group mix.

To assess the impact of university attendance on healthcare utilization we exploit quasi-experimental variation in university attendance that is caused by an arbitrary rule in Sweden. This rule states that in order to go on to university, students must have a pass mark for at least 90% of their courses that comprise an upper-secondary school program. This arbitrary rule is used as an instrumental variable for university attendance, and is identified using RDD. In combination, this allows us to assess how university attendance impacts various outcomes of health derived medical care use.

The main contribution of the present paper is the credible identification of university attendance for the marginal group affected by the eligibility rule. These individuals are, by definition, individuals at the lower end of the education distribution of those who enroll at university (the 46th percentile and 42nd percentile for males and females, respectively, who were enrolled on the academic track at upper-secondary school). Moreover, they are likely to come from family backgrounds with lower SocioEconomic Status (SES). Theory (Cunha & Heckman, 2007; Heckman, 2012) suggests that the effectiveness of an educational investment is likely to vary depending on the child's stock of capabilities and the background characteristics. Students at the margin of eligibility are generally of lower ability and their background characteristics likely differ in important ways when compared with the average university student, suggesting these students may receive different returns from university education. The margin we estimate is therefore of particular interest to policy makers because it captures the potential egalitarian impact of increasing access to higher education for lower ability individuals, or individuals from lower ranking socioeconomic groups. However, there is currently no evidence for how university affects the health of this marginal group. A further contribution of our analysis is that we also consider males and females separately, as several previous studies have been found important differences across the genders, in terms of their health responses to changes in education (Fischer et al., 2021; Gathmann et al., 2015; Van Kippersluis et al., 2011).

Our analysis sample period covers the universe of individuals who graduated from upper-secondary school between 2003 and 2005, and follows them up until 2013. A further contribution of the paper is our utilization of detailed population-based administrative records of inpatient and outpatient hospital admissions (2003–2013) and prescriptions (2005–2013) linked with education records using a personal identifier. This data provides us with large sample sizes, detailed individual level data, and low measurement errors. We find that achieving university eligibility upon graduating from upper-secondary school leads to a large and significant jump (10%–14% points [pp]) in the proportion of students who go on to study at university, and on the years spent in education. These jumps are substantial in size when compared to years of education effects observed due to compulsory school reforms (Galama et al., 2018).

Our 2SLS results show that a 10% point increase in university attendance is likely to yield a 1% point increase in hospital admissions amongst males, and that this is primarily due to mental illnesses related to alcohol and narcotics. Amongst females, on the other hand, a possible reduction in general mental ill health is observed, although these results are less precisely estimated. The results are local, and therefore only representative for those at the margin of eligibility. No clear impact on overall hospitalizations or prescriptions is found, which is consistent with the general theoretical predictions for the age groups under consideration.

The rest of the paper is structured as follows. In Section 2, we introduce the Swedish education system and the eligibility rule we consider. In Section 3, we introduce the data material we use for the analysis and in Section 4, we explain our empirical approach and test our identifying assumptions. Section 5 presents the results for medical care use and heterogeneity analysis, as well as characterization the compliers and assessment of counter-factual outcomes. Section 6 concludes.

2 | THE SWEDISH EDUCATION SYSTEM

In Sweden, in order to be able to attend university, a student needs to achieve eligibility by passing at least 90% of a full upper-secondary school program.[2] This is most often achieved at graduation from upper-secondary school but can also be achieved at a later stage by completing complementary adult studies. The present study defines eligibility at a particular point in time: graduation from upper-secondary school. We choose to use this definition of university eligibility because it is a well-defined and difficult to manipulate rule that leads to a jump in university attendance, as we will show later. In general, upper-secondary school lasts for 3 years, for ages 16–19 years[3] There are two tracks: the academic track with the explicit aim of going to university, and the vocational track with an explicit focus of getting a job after graduation. This paper focuses on students graduating from the academic track, because that is where the university eligibility threshold has the most significant influence (Nordin et al., 2020). Students are able to choose their preferred track. For both tracks, a full program consists of 2500 course credits for both types of tracks.[4] To receive a diploma of eligibility for university a student needs to pass at least 90% of the full program that is, receive 2250 credits. A program is a collection of courses of which can give either 50, 100, 150, 200 or 250 course credits (with some exceptions for even larger courses). The courses that make up a program are graded on four levels: fail, pass, pass with distinction and pass with special distinction. The credits received are not impacted by how well the student performs; they only have to pass the course. The present study investigates the period beginning in 2003 when the diploma of eligibility for university became much more clearly defined than in the years prior.

3 | DATA

We employ administrative register data for all students who graduated from upper-secondary school between the years 2003 and 2005 and who had previously graduated from Swedish compulsory school.[5] We combine education register data on final grades from compulsory school, grades from upper-secondary school and data on higher education first term attendance and derive total years of schooling using highest level of education achieved. This is then matched with administrative register data on labor market outcomes from the Longitudinal Integration Database for Health Insurance and Labor Market Studies (LISA) and administrative register data on hospital admissions, from the patient register, and with administrative register data on prescriptions, from prescriptions register. Data on labor market outcomes were taken from Statistics Sweden (SCB) and healthcare data were taken from the Swedish Board of Health (Socialstyrelsen). We also use the Multi-generational Register from Statistics Sweden, which links the individuals to their parents. Moreover, the population and housing censuses from years 1985 and 1990 provide us with information on parental education and income during the early childhood of the students in question.

Our medical care use variables are hospital admissions and prescriptions. The study considers both the total number of hospital admissions and the total number of prescriptions in the time between graduation and the year 2013 (our last period of observation). We also consider the probability of hospital admission and the probability of prescription receipt by 2013 by diagnosis International Classification of Diseases (ICD) codes and drug type (Anatomical Therapeutic Chemical [ATC] Classification System codes). Causes of hospitalization and prescription are chosen because of their plausibility of being modified by university attendance: hospitalizations due to external causes, and due to mental illness; prescriptions for mental ill health and opioids.[6] Opioids are considered because they potentially relate to both drug abuse and recovery from severe accidents, which are both common for this age group. We interpret the causes of medical care use as being derived from changes in health but do not rule out the possibility that changes in healthcare seeking behavior may be influenced by university attendance. Summary statistics for our health variables are shown alongside RDD estimates in the results tables. Crucially, our analysis divides the sample by gender because important differences in effects of education on health have been observed across the genders (Fischer et al., 2021; Gathmann et al., 2015; Van Kippersluis et al., 2011).

University attendance is defined as being registered at a university at any point during our sample period. Years of education are defined using highest level of education achieved.[7] Pre-determined characteristics, that are highly correlated with our health outcomes are used as control variables in some specifications, consist of average parental income and education in the years 1985 and 1990 (as recorded in the censuses), age at migration, and compulsory school grades.[8] Dummy variables are also included, in the case of first-generation and second-generation immigrants.[9] Descriptive statistics for these variables can be found in Table A2 in the Appendix, alongside more detailed information on the final sample selection.

4 | METHOD

4.1 | Identifying the causal impact of university attendance

To estimate the effects of university eligibility on health outcomes we use an RDD as our identification strategy. Our causal model of interest is:

$$y_i = \gamma_0 + \gamma_1 Attendance_i + \gamma_2' X_i + f(Coursecredits_i) + \epsilon_i; \tag{1}$$

In this model y_i represents the various health outcomes we consider for individual *i*, *Attendance* is a dummy variable equal to unity for those who attend university, X is a vector of control variables and $Coursecredits_i$ is the running variable, which is measured in terms of distance from the eligibility threshold and expressed as percentage points of a full program. The coefficient γ_1 is the causal effect of university attendance. We estimate Equation (1) using Two Stage Least Squares (2SLS), instrumenting university attendance with university eligibility using the following 2SLS first stage:

$$Attendance_i = \beta_0 + \beta_1 Eligible_i + \beta_2' X_i + f(Coursecredits_i) + \varepsilon_i; \tag{2}$$

where *Eligible* is our instrument for university attendance, a dummy variable equal to unity for those who have passed 90% or more of a full program and are therefore eligible for university.

2SLS regression estimates the Local Average Treatment Effect, which is identified subject to the following conditions: there is a first stage, the exclusion restriction holds, independence (treatment being as good as random) and monotonicity (cf. Angrist & Pischke, 2008). We utilize RDD combined with the arbitrary eligibility rule to provide a first stage (Imbens & Lemieux, 2008; Lee & Lemieux, 2010), and given our RDD strategy we assume independence of university eligibility, that university eligibility leads then only to increases in university attendance and that university eligibility only impacts our outcome variables through its affect on university attendance.

Figure 1 shows the impact of barely passing the cut-off point at graduation from upper-secondary school on the probability of enrolling in university for cohorts graduating between 2003 and 2005. The raw data is graphed as scatter plots of the proportion who attended a first term course of university by the number of achieved credits at upper-secondary school, in bins of 2pp of a full program. Linear regression lines are plotted either side of the cut-off, and 95% confidence intervals are indicated by the shaded area. The vertical dashed line represents the cut-off of university eligibility ($2250 = 2500 * 0.9$). Figure 1 is for men and women studying the academic track and shows that the proportion going on to university is a smooth and increasing function of the percentage completed of a full program at upper-secondary school apart from the discontinuity at the university eligibility threshold of 90% of a full program.[10] It is worth noting that the cut-off and forcing variable are defined at a point in time, which is graduation from upper-secondary school. Even though this is not a high-stakes threshold as students can achieve university eligibility after completing upper-secondary

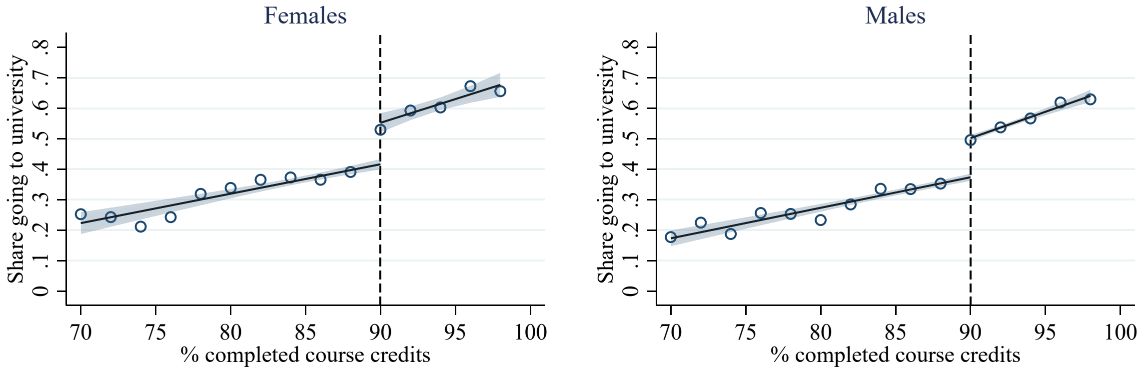

FIGURE 1 Course Credit Profile of University Attendance. This figure plots a scatter of the share who attended a first term of university against percentage completed of a full program. The bin width is 2pp of a full upper-secondary school program (the size of the smallest course) for those graduating upper-secondary school between the years 2003 and 2005, whom we followed until 2013. Linear regression lines are estimated either side of the cut-off with corresponding 95% confidence intervals shown by the shaded area and standard errors clustered at the running variable level. The cut-off for university eligibility is marked by the dashed vertical line at 90pp credits [Colour figure can be viewed at wileyonlinelibrary.com]

school by complementing their studies, we still observe a jump using our definition of the cut-off. That is, students who fail to achieve university eligibility at completion of upper-secondary school are less likely to go on to university despite the possibility for them to achieve eligibility later on.

We use individuals that are very close to either side of this cut-off, just one or two completed courses apart. This is based on the assumption that they are likely to be very similar in all observable and unobservable ways except that those who are above the threshold have easier and more immediate access to university education than those below. We choose a functional form for the forcing variable, $f(Coursecredits_i)$, which is a local low ordered polynomial of $Coursecredits_i$ and an interaction of $Eligibility_i * f(Coursecredits_i)$ so that we have different trends on either side of the cut-off. A low ordered polynomial is important, as higher order polynomials can over fit the data (Gelman & Imbens, 2017). Although we find a single polynomial is sufficient in our empirical application, we provide a second order polynomial as a robustness check. The coefficient β_1 is the discontinuous effect of university eligibility on attendance assuming that our functional form absorbs any potential relationship between $Coursecredits_i$ and ε_i.

A key identifying assumption for RDD analysis of the eligibility threshold is that those at the margin of university eligibility will not have precise control over whether they cross the threshold. We view this as unlikely, but cannot rule it out. Students coming in to the final term of their upper-secondary school program often have seven to eight courses of varying credit sizes to complete, with the smallest worth just 2pp of a full program. Precise manipulation of the threshold would require the more motivated students to understand in advance how many courses they need to pass, and which specific courses they need to focus on in order to just cross the eligibility threshold, which would seem quite a high stakes gamble. As the teachers grade the courses themselves, they may also be aware that a particular student is near the eligibility threshold, and consequently mark up their grades. Although this is possible, in order for teachers to be able to manipulate the threshold *exactly*, they would need to know what the student is likely to achieve in the other seven or so courses they are enrolled in. They would also have to collude with other teachers so that the marginal student crosses the threshold exactly but does not go far beyond it. Overall, then, it would seem very onerous for this degree of collusion to happen so precisely. It is this lack of precise control combined with our understanding that this threshold is not viewed as high-stakes, that leads us to our view that manipulation is unlikely.

The exclusion restriction requires that eligibility only impacts health outcomes via its impact on university attendance. The 2SLS estimates are therefore only valid if there is no pay-off to university eligibility without going on to higher education. Whilst it is possible that eligibility may raise the esteem of the individual, and that eligibility may be seen as a valid cut-off for employers to consider given its importance to universities, we view this as unlikely. We provide analysis of counter-factual outcomes to assess this assumption and find no evidence of violation of the exclusion restriction. We also provide reduced form estimates of our health outcomes as per Equation (2), which do not hinge on the exclusion restriction assumption.

In our RDD analysis, we vary the bandwidth size between 4pp, 8pp and 16/8pp of a full program. This allows us to assess the sensitivity of the results to bandwidth choice.[11] Due to the fact that we use a large sample size so close to the cut-off, we are able to have small bandwidths. The inclusion of linear trends either side of the cut-off means we are in effect modeling a Local Linear Regression with a rectangular kernel (Imbens & Lemieux, 2008). We assess the sensitivity to a rectangular kernel in the sensitivity analysis.

When estimating Equations (1) and (2), we control for a number of pre-determined characteristics in some specifications, characteristics that are highly predictive of our health outcomes. There are two reasons for this. First, as we expand the bandwidth we are including more observations that are not close to the cut-off and the inclusion of covariates may eliminate some bias that results from the inclusion of these observations (Imbens & Lemieux, 2008). Second, it provides an additional test of our identifying assumption that the error term is a smooth function crossing over the eligibility threshold.

In all our main RDD specifications we provide robust standard errors following the recommendation of Kolesár and Rothe (2018). This is because in our empirical application we have a limited number of clusters, up to 12 bins. Kolesár and Rothe (2018) show that in the case of limited clusters, the use of robust clustered along the running variable standard errors can lead to standard errors with poor coverage properties. As a result, robust standard errors are preferred according to Kolesár and Rothe (2018). In a sensitivity analysis we provide estimates with robust clustered along the running variable standard errors as it is not always the case that robust standard errors are the correct standard errors, even in the limited cluster case.[12]

4.2 | The impact of university eligibility on university attendance

In this section we present the estimates of the effect of university eligibility on university attendance, thereby establishing the existence of a first stage. We also present the results of various diagnostic tests which support our assumption that the IV is as good as random given our RDD identification strategy. In Figure 1 we saw that there is a jump in the proportion who attend university at the university eligibility cut-off and in Figure 2 we can see there is also a clear jump in years spent in education for both females and males. The corresponding RDD results are shown in Table 1. Model (1) shows our RDD results using a bandwidth of 4pp and confirms there is a positive jump in the proportion attending university, 11pp for females and 13pp for males. This bandwidth is arguably too small, however, to adequately model the trend in completed school credits. Model (2) is as per model (1), but with double the bandwidth, a bandwidth of 8pp. Model (3) is as per model (2), but with double the left-hand side bandwidth, a bandwidth of 16pp. Model (3) is our preferred specification and corresponds to the linear regression lines in Figure 1. Models (5) and (7) are as per models (2) and (3) but with the addition of pre-determined covariates.[13] The results using a first order polynomial are stable in relation to the choice of bandwidth and suggest that university eligibility leads to a jump in university attendance in the range of 10pp to 14pp for females, and 11pp to 12pp for males. The impact on years of education is in the range 0.14 and 0.27 years for females and 0.1 and 0.17 years for males. Models (4) and (7) provide RDD results using a second order polynomial as a robustness check, which we find support the findings of first order polynomial estimates. If anything, these results suggest the first order polynomials are slightly conservative estimates for females. The F statistics suggest university eligibility provides

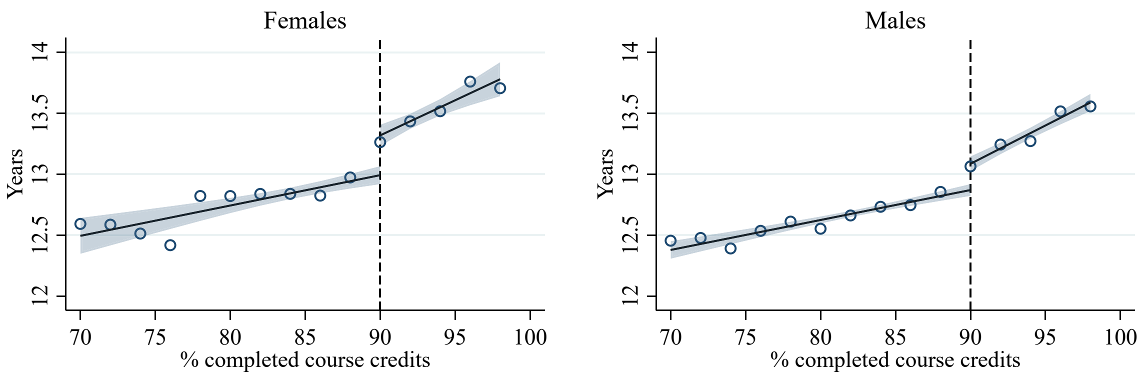

FIGURE 2 Course credit profile of years of schooling. These figures plot mean years of education for each bin of a completed program, in bins of 2pp of a program. The dashed vertical line is the 90% cut-off for university eligibility. See notes for Figure 1 further details [Colour figure can be viewed at wileyonlinelibrary.com]

TABLE 1 Impact of university eligibility on education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Bandwidth	**4pp**	**8pp**	**16pp/8pp**	**16pp/8pp**	**8pp**	**16pp/8pp**	**16pp/8pp**
Female							
1st Term attendance	0.113***	0.139***	0.112***	0.164***	0.135***	0.101***	0.155***
	(0.042)	(0.026)	(0.019)	(0.031)	(0.024)	(0.018)	(0.030)
F-Stat	7.094	29.720	35.001	27.783	30.465	30.528	26.665
Years of schooling	0.140	0.265***	0.236***	0.345***	0.253***	0.202***	0.317***
	(0.110)	(0.067)	(0.049)	(0.081)	(0.063)	(0.047)	(0.078)
F-Stat	1.619	15.793	23.025	18.112	15.907	18.221	16.665
Observations	4685	12,671	13,525	13,525	12,671	13,525	13,525
Male							
1st Term attendance	0.125***	0.119***	0.115***	0.120***	0.116***	0.113***	0.117***
	(0.035)	(0.021)	(0.016)	(0.025)	(0.020)	(0.015)	(0.024)
F-Stat	12.924	33.027	54.412	22.959	32.997	55.500	22.979
Years of schooling	0.101	0.161***	0.170***	0.187***	0.150***	0.163***	0.176***
	(0.086)	(0.052)	(0.039)	(0.062)	(0.051)	(0.038)	(0.061)
F-Stat	1.364	9.672	18.881	9.046	8.826	18.075	8.391
Observations	6573	15,692	17,119	17,119	15,692	17,119	17,119
Polynomial	1	1	1	2	1	1	2
Covariates	N	N	N	N	Y	Y	Y

Note: This table shows the regression discontinuity estimates of the impact of university eligibility on education for those graduating upper-secondary school between the years 2003 and 2005, whom we follow up to 2013. Each estimate is from a separate regression. See text for details for each model (1–6). Robust standard errors are shown in parentheses. Testing the null of the coefficient: ***$p < 0.01$.

a strong first stage for university attendance, based on the rule of thumb for weak instruments of an F statistic above 10 (Stock et al., 2002).

The final analysis of this section considers the credit score distribution of our variables capturing potential manipulation mechanisms and various pre-determined covariates as a test of our identifying assumption. The key identifying assumption is that the students and/or their teachers are not able to manipulate the final credit scores in a systematic way, in a way that is linked to other important characteristics that determine health and medical care use. Our first diagnostic test of manipulation is that we include covariates in the regression estimates in models (5) and (6) in Table 1. The impact of including these covariates is very small, when comparing to results from models (3) and (4). The fact that inclusion of these covariates yields small impacts on our coefficients of interest suggests that unobserved characteristics are in fact a smooth function over the cut-off.

Figure 3 presents three visual tests of cut-off manipulation. The top panel of Figure 3 is a histogram of the population density by credit score plotted with bins of 2pp as suggested by Lee and Lemieux (2010) as a test of manipulation in the spirit of McCrary (2008). A jump in the population just above the cut-off would suggest that individuals were manipulating their position around the threshold, which would violate our identification assumption. The discrete nature of our data means this test is not an ideal fit but we nevertheless observe no obvious jump in density at the university eligibility cut-off. The second panel of Figure 3 shows the final grade plotted against credit score, and the final (third) panel shows the number of failed courses by credit score achieved by graduation. Under no manipulation, these figures should be perfectly co-linear. If manipulation were occurring, we would expect to see a jump in overall upper-secondary grades just above the threshold on account of students trying harder in a number of courses to ensure they fall to the right-hand side of the threshold. Furthermore, if manipulation were occurring we would also expect to see a jump in the number of failed courses at the threshold, as a consequence of attempts to maximize chances of crossing the threshold. This is possible because students can take more courses than needed for a full program. We observe no clear jumps for females or males, however, in any of our visual diagnostic tests.

In Table 2 we present results from a batch of manipulation and balancing tests using RDD. These tests assess whether our diagnostic test variables (as shown in Figure 3), and other pre-determined characteristics (see appendix Figures A1 and A2),

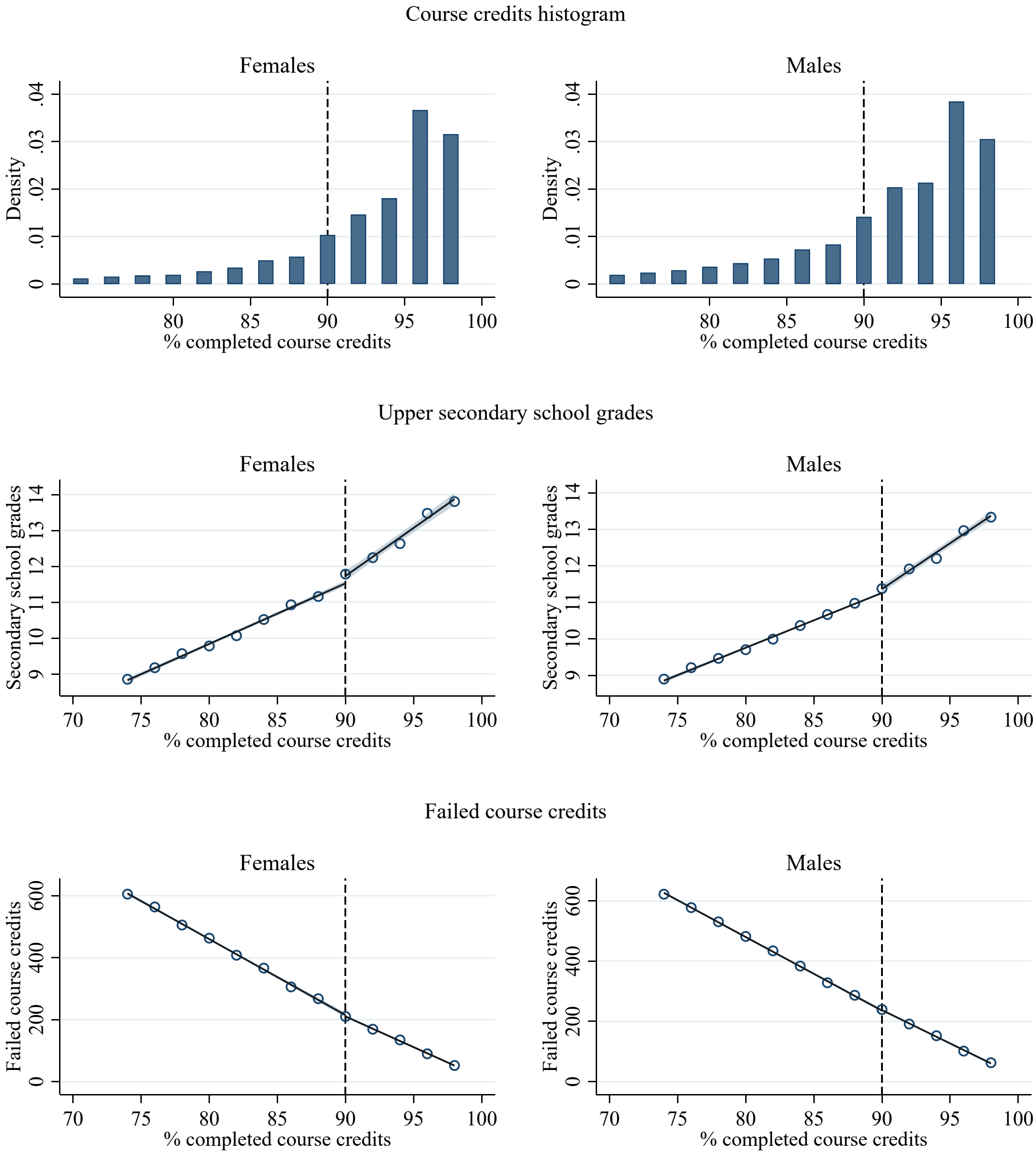

FIGURE 3 Course credit profile of manipulable outcomes. These figures plot various diagnostic tests using percentage of a completed program as the running variable shown in bins of 2pp of a program. For panels 2-4 we present the mean for each bin. The dashed vertical line is the 90% cut-off for university eligibility. See text for further details [Colour figure can be viewed at wileyonlinelibrary.com]

are equally distributed either side of the cut-off. Models (1) and (4) are ordinary least squares (OLS) of the simple association between university eligibility and the covariate and show that university eligibility is highly correlated with all our diagnostic test variables and covariates. However, using our RDD specification to isolate the impact of university eligibility in models (2–3) and (5–6) all coefficients can be observed substantially reducing toward zero and nearly always losing statistical significance. The first row of results are a test of threshold manipulation in the spirit of McCrary (2008), and assesses whether there is a discontinuity in the density around the cut-off. We find no substantive evidence of this even though the very small effect is highly significant. When using the largest bandwidth, we find evidence of a small jump in upper-secondary school grades at the cut-off, but the same cannot be said when using the smaller bandwidth. Whilst the jump is statistically significant it is rather small in relative terms and represents a jump of less than 1pp (mean of circa 12pp). The same result was observed for compulsory school grades. Our RDD balancing test results for mother's and father's income and mother's and father's are all balanced across the cut-off.[14] We test the sensitivity of these balancing

TABLE 2 Balancing tests

	(1)	(2)	(3)	(4)	(5)	(6)
	8pp	8pp	16pp/8pp	8pp	8pp	16pp/8pp
Bandwidth	**Female**			**Male**		
McCrary density test						
Tertiary eligibility	0.020***	−0.001***	−0.000***	0.020***	0.002***	0.002***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Upper secondary grades						
Tertiary eligibility	2.053***	0.097	0.138**	1.741***	0.032	0.079**
	(0.034)	(0.071)	(0.055)	(0.025)	(0.049)	(0.038)
Number of failed credits at upper-secondary school						
Tertiary eligibility	−191.282***	−6.063	−3.966	−191.627***	4.721	3.411
	(2.192)	(4.290)	(3.228)	(1.700)	(3.084)	(2.277)
Compulsory school grades						
Tertiary eligibility	17.352***	1.017	2.734*	13.458***	2.227	2.513**
	(0.901)	(1.994)	(1.506)	(0.668)	(1.483)	(1.125)
Mother's income						
Tertiary eligibility	50.278***	−23.454	2.018	42.992***	−11.376	−5.640
	(10.173)	(22.208)	(16.835)	(8.634)	(18.831)	(13.948)
Father's income						
Tertiary eligibility	114.692***	−1.930	25.995	89.767***	7.763	−13.566
	(17.963)	(40.806)	(28.794)	(18.719)	(35.667)	(27.587)
Father's education						
Tertiary eligibility	0.581***	−0.131	0.034	0.547***	−0.150	−0.027
	(0.102)	(0.228)	(0.171)	(0.086)	(0.188)	(0.142)
Mother's education						
Tertiary eligibility	0.612***	−0.167	−0.022	0.613***	−0.224	−0.017
	(0.094)	(0.213)	(0.161)	(0.078)	(0.173)	(0.130)
Observations	12,671	12,671	13,525	15,692	15,692	17,119
Polynomial	0	1	1	0	1	1

Note: This table shows the regression discontinuity estimates of the impact of university eligibility on a batch of diagnostic variables and pre-determined characteristics, who graduated between the years 2003 and 2005 and who were enrolled on the academic track. Each estimate is from a separate regression. Models (1) and (4) are simple OLS associations of university eligibility and the variable being tested using a bandwidth of 8pp. Models (2) and (5) use a linear trend in course credits either side of the cut-off, and a bandwidth of 8pp of a full program either side of the cut-off. Models (3) and (6) are as models (2) and (5) but with a bandwidth of 16pp before the cut-off, and 8pp after. Robust standard errors are shown in parentheses. Testing the null of the coefficient: *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Abbreviation: OLS, ordinary least squares.

tests to the use of robust standard errors clustered along the running variable and find the conclusions are the same. It must be noted, however, that the McCrary test results lose all significance when the clustered standard errors are used, and so do upper-secondary school grades.

Overall, the fact that our estimates of the impact of university eligibility on university attendance are stable across different model specifications, and also with and without the inclusion of covariates, suggests that both observed and unobserved covariates are a smooth function across the cut-off, and therefore the jumps we observe in our outcome variables are due to the policy effect. Our diagnostic tests serve as further indication that there is no compelling evidence of manipulation to be found. Altogether, this implied that the jumps we see in university attendance are primarily driven by the arbitrary rule, and not by unobserved factors resident in the error term.

5 | RESULTS

5.1 | Hospital admissions

Panel (a) in Figure 4 depicts the completed credit profile of the proportion of students who had a hospital visit on account on the two broad causes relevant to our population: external causes or a mental disorder. These data represent the period between graduation and the year 2013, and are divided by gender. No clear jump is observed in the proportion who were admitted to hospital due to external causes. For the proportion who have had a hospital admission due to mental illness

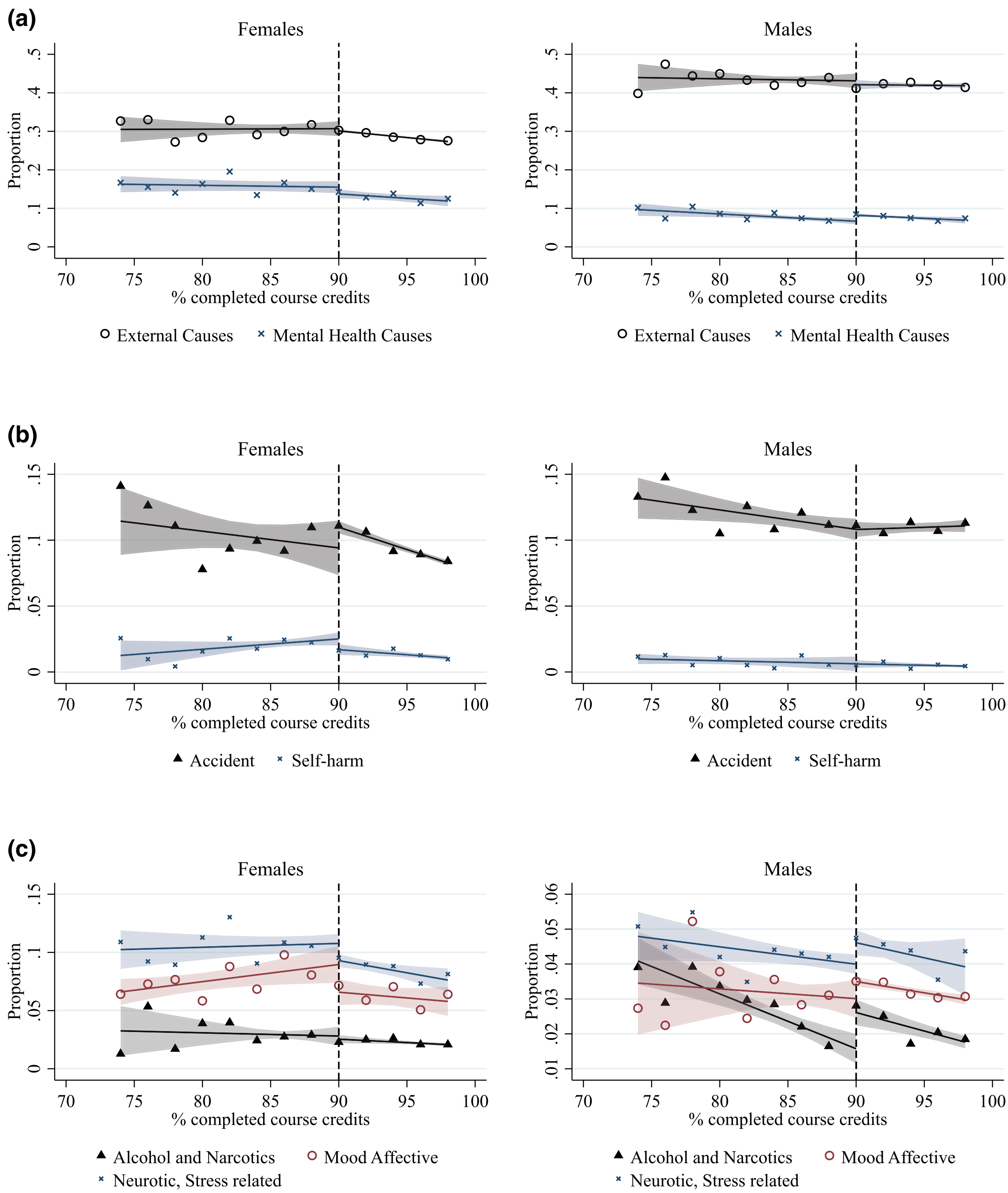

FIGURE 4 Course credit profile of hospital admissions. (a) Broad causes; (b) Specific external causes; (c) Specific mental ill health causes. These figures plot a scatter of probability by cause between graduation until 2013, against percentage completed of a full program with a bin width of 2pp of a full course in each bin. These data represent those who graduated upper-secondary school between the years 2003 and 2005 (academic track), whom we followed until 2013. See notes for Figure 1 [Colour figure can be viewed at wileyonlinelibrary.com]

there appears to be a small decrease for females, in absolute terms. Panel (b) of Figure 4 considers hospital admission due to accidents and self-harm, and for females just above the threshold we potentially observe a slight decrease in self-harm. Panel (c) of Figure 4 considers hospital admissions due to mental ill health, and there appears to be a decrease in mental ill health admissions for females and an increase for males.[15]

The RDD results that correspond to the data presented in Figure 4 can be found in column (3) of Table 3, and support the broad pattern of improved mental health for females that was observed in the raw data, which includes incidences of self-harm. Columns (2) and (4) provide alternative bandwidth and polynomial estimates. For eligible females, we observe a fall in self-harm related admissions of 0.7–0.9pp (reduction of 33%–40% relative to the baseline presented in column [1]). We also observe negative discontinuities in mood affective disorders and neurotic and stress-related disorders, suggesting a general improvement in mental health. The reductions in mental disorders are generally robust to the choice of bandwidth and polynomial, but are imprecisely estimated.

For males, we observe a positive jump in hospital admissions related to a mental disorder exhibiting an increase of 2pp or 28% relative to the baseline presented in column (1). This result is found to be stable to choice of bandwidth or polynomial (columns 2–4). No clear impact is observed on hospital admissions due to external causes. The increase in mental disorders is almost entirely driven by disorders related to abuse of alcohol and narcotics, a result stable to modeling choice and statistically significant at the 5% level. Mood affective disorders, and neurotic and stress related disorders also show positive impacts in line with alcohol and narcotics, suggesting a general increase in mental ill heath amongst males, although these are imprecisely estimated. The positive jumps in mental ill health are part of a general downward trend in course credits suggesting the estimated impacts of eligibility are local to the marginal student. Unlike in the case for females, the impact on incidence of self-harm is almost zero, showing no correspondence with the jump in mental ill health observed for males.

The 2SLS results (columns 6–7) confirm that the positive jumps for males coincide significantly with increased university attendance. The results suggest a 10pp increase in university attendance causes a 1pp increase in hospitalization admissions related to alcohol and narcotics, a relative increase of roughly 60% when compared to the baseline. Smaller but insignificant jumps for males are observed for other causes of mental ill health suggesting a general deterioration in mental ill-health.

Overall, differences in point estimates of university attendance's impact on hospitalizations are observed across genders. For males, a general pattern of worsened mental health is observed, but only for the marginal student. For females, a less precisely estimated pattern of improved mental health is found. Analysis of general hospital admissions finds no clear impacts (see appendix Figure OA.1 and Table OA.1).[16]

5.2 | Pharmaceutical prescriptions

This section considers prescription receipt, an outcome measure which captures a broader part of the health care sector on account of the fact that both health specialists inside and outside of the hospital sector as well as General Practitioners can make up prescriptions. It therefore captures a more prevalent measure of health, but a less severe form of ill-health.

Prescriptions by health category, shown in Figure 5 panel (a), indicate a small fall in opioid prescriptions, and a positive jump in mental health-related prescriptions for males, although this jump may be insignificant. Panel (b) of Figure 5 shows two sub-causes of prescriptions related to mental ill-health where there appears to be a fall in anti-depressives for females and a positive jump for males.[17] These results are in line with what is observed for mental health-related hospitalizations.

The corresponding RDD results are shown in Table 4. The jumps in mental health-related prescriptions are in line with what is observed for hospital admissions but are sensitive to bandwidth and polynomial choice. This is the case for both the reduced form estimates (columns 2–4), and the 2SLS estimates (columns 6–7). In sum, we find no clear robust impacts of university eligibility on overall prescription receipt or prescriptions related specifically to pain relief or mental ill health.[18] Analysis of frequency of prescriptions in general also finds no clear impacts (see appendix Figure OA.1 and Table OA.1).

TABLE 3 Impact of university eligibility on hospital admissions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model	**Mean**	**RDD**	**RDD**	**RDD**	**RDD**	**2SLS**	**2SLS**
Polynomial		**1**	**1**	**2**	**1**	**1**	**2**
Bandwidth		**8pp**	**16/8pp**	**16/8pp**	**16/8pp**	**16/8pp**	**16/8pp**
Females							
External causes	0.317	−0.005	−0.004	−0.026	−0.003	−0.033	−0.160
	(0.017)	(0.024)	(0.018)	(0.030)	(0.018)	(0.162)	(0.185)
Accidents	0.110	0.001	0.017	−0.014	0.017	0.151	−0.083
	(0.011)	(0.016)	(0.012)	(0.020)	(0.012)	(0.112)	(0.123)
Self-harm	0.022	−0.007	−0.009*	−0.013	−0.009	−0.082	−0.078
	(0.005)	(0.008)	(0.005)	(0.009)	(0.005)	(0.051)	(0.058)
Mental disorder	0.151	0.002	−0.011	−0.008	−0.011	−0.102	−0.051
	(0.013)	(0.019)	(0.014)	(0.023)	(0.014)	(0.127)	(0.143)
Alcohol and narcotics	0.029	0.001	−0.003	−0.000	−0.003	−0.024	−0.002
	(0.006)	(0.009)	(0.006)	(0.011)	(0.006)	(0.057)	(0.065)
Mood affective disorders	0.081	−0.014	−0.018*	−0.021	−0.018*	−0.160	−0.127
	(0.010)	(0.014)	(0.011)	(0.017)	(0.011)	(0.098)	(0.109)
Neurotic, stress related disorders	0.106	−0.001	−0.010	−0.009	−0.010	−0.087	−0.052
	(0.011)	(0.016)	(0.012)	(0.020)	(0.012)	(0.107)	(0.121)
N	757	12,671	13,525	13,525	13,525	13,525	13,525
Males							
External causes	0.440	−0.021	−0.013	−0.018	−0.013	−0.114	−0.152
	(0.015)	(0.022)	(0.016)	(0.026)	(0.016)	(0.140)	(0.219)
Accidents	0.112	−0.000	0.002	−0.008	0.002	0.013	−0.067
	(0.010)	(0.014)	(0.010)	(0.017)	(0.010)	(0.091)	(0.141)
Self-harm	0.005	−0.003	−0.000	−0.002	−0.000	−0.001	−0.017
	(0.002)	(0.003)	(0.003)	(0.004)	(0.003)	(0.024)	(0.036)
Mental disorder	0.068	0.019*	0.019**	0.022	0.020**	0.167**	0.184
	(0.008)	(0.011)	(0.009)	(0.014)	(0.009)	(0.079)	(0.123)
Alcohol and narcotics	0.016	0.015**	0.011**	0.020**	0.011**	0.096**	0.164**
	(0.004)	(0.006)	(0.005)	(0.008)	(0.005)	(0.045)	(0.075)
Mood affective disorders	0.031	0.003	0.006	0.010	0.005	0.048	0.084
	(0.005)	(0.007)	(0.006)	(0.009)	(0.006)	(0.050)	(0.080)
Neurotic, stress related disorders	0.042	0.004	0.009	0.002	0.010	0.082	0.021
	(0.006)	(0.009)	(0.007)	(0.011)	(0.007)	(0.059)	(0.090)
N	1094	15,692	17,119	17,119	17,119	17,119	17,119
Covariates	N	N	N	N	Y	N	N

Note: This table shows the regression discontinuity estimates of the impact of university eligibility on hospital admissions by diagnosis from graduation until 2013. These data represent those who graduated upper-secondary school between the years 2003 and 2005 (academic track), whom we followed until 2013. Each coefficient is from a separate regression. Column (1) is the baseline for each outcome for those just below the cut-off. Models (2), (3) and (5) are reduced form RDD estimates of eligibility using a linear trend in credits either side of the cut-off but different bandwidths. Models (6) and (7) are 2SLS estimates of university attendance. Models (4) and (7) use a second polynomial in completed course credits. Model (5) also includes covariates as outlined in table 1. Robust standard errors are shown in parentheses. Testing the null of the coefficient: $*p < 0.1$, $**p < 0.05$.

Abbreviation: RDD, regression discontinuity design.

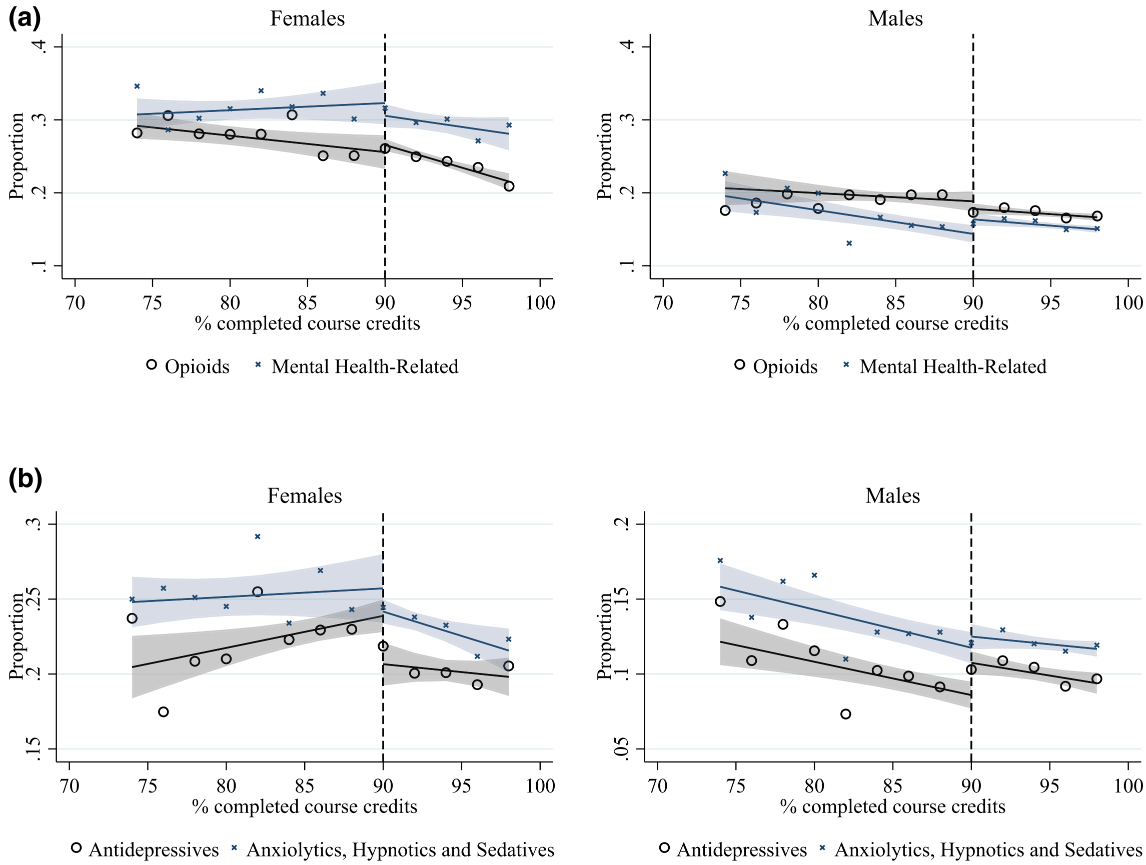

FIGURE 5 Course credit profile of prescriptions. (a) General causes; (b) Prescriptions due to mental ill health. These figures plot a scatter of probability of receiving a prescription from 2005 until 2013 by main cause, against percentage completed of a full program with a bin width of 2pp of a full course in each bin. These data represent those who graduated upper-secondary school between the years 2003 and 2005 (academic track), whom we followed until 2013. See notes for Figure 1 [Colour figure can be viewed at wileyonlinelibrary.com]

5.3 | Heterogeneity

Our main finding, which is the jump in hospital admissions related to alcohol and narcotics due to university attendance amongst males, could potentially be a consequence of peer group composition changes or institutional factors, either of which could influence health behaviors *during* the years spent studying at university. Alternatively, these impacts could be a consequence of the human capital effects of a university education subsequently impacting alcohol consumption patterns *after* university. Consequently, Figure OA.2 in the online appendix assesses the impact on hospitalizations by year after graduation from upper-secondary school. This assessment finds that the positive jump in hospital admissions related to alcohol and narcotics is noted in the first 3–4 years after graduation. It also finds that the jump is not at all observed amongst those on the vocational track. In combination, these findings suggest that the impacts are only observed while attending university and not after. The findings also imply that the overall jump is consistent with the theory of peer composition effects, or institutional factors due to university attendance.

Impacts of university eligibility can also potentially vary by socioeconomic background of the students (Low SES = bottom 25% of household income distribution). For instance, students from a lower socioeconomic background are potentially more susceptible to influences that encourage negative health-related behaviors. Tables OA.4 and OA.5 in the online appendix present the 2SLS impacts split by low/high SES. Although these are less precisely estimated than our main results, the impacts for the low SES group show a more negative effect for males, and a less positive effect for females, which is consistent with our hypothesis.

TABLE 4 Impact of university eligibility on prescription receipt

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Model	**Mean**	**RDD**	**RDD**	**RDD**	**RDD**	**2SLS**	**2SLS**
Polynomial		**1**	**1**	**2**	**1**	**1**	**2**
Bandwidth		**8pp**	**16/8pp**	**16/8pp**	**16/8pp**	**16/8pp**	**16/8pp**
Females							
Mental health-related	0.301	0.019	−0.000	0.010	−0.000	−0.000	0.058
	(0.017)	(0.024)	(0.018)	(0.030)	(0.018)	(0.163)	(0.182)
Antidepressants	0.230	−0.007	−0.023	−0.014	−0.024	−0.209	−0.085
	(0.015)	(0.022)	(0.016)	(0.027)	(0.016)	(0.150)	(0.166)
Anxiolytics, hypnotics and sedatives	0.243	0.014	−0.003	0.005	−0.003	−0.028	0.033
	(0.016)	(0.023)	(0.017)	(0.028)	(0.017)	(0.152)	(0.170)
Opioids	0.251	0.026	0.009	0.032	0.010	0.081	0.195
	(0.016)	(0.023)	(0.017)	(0.028)	(0.017)	(0.155)	(0.178)
N	757	12,671	13,525	13,525	13,525	13,525	13,525
Males							
Mental health-related	0.154	0.002	0.026**	0.002	0.026**	0.223**	0.016
	(0.011)	(0.016)	(0.012)	(0.020)	(0.012)	(0.108)	(0.163)
Antidepressants	0.091	0.009	0.027***	0.005	0.027***	0.237***	0.041
	(0.009)	(0.013)	(0.010)	(0.016)	(0.010)	(0.091)	(0.132)
Anxiolytics, hypnotics and sedatives	0.128	−0.008	0.011	−0.006	0.012	0.097	−0.053
	(0.010)	(0.014)	(0.011)	(0.018)	(0.011)	(0.097)	(0.150)
Opioids	0.197	−0.019	−0.021	−0.025	−0.021	−0.183	−0.209
	(0.012)	(0.017)	(0.013)	(0.021)	(0.013)	(0.113)	(0.178)
N	1094	15,692	17,119	17,119	17,119	17,119	17,119
Covariates	N	N	N	N	Y	N	N

Note: This table shows the regression discontinuity estimates of the impact of university eligibility on frequency of prescriptions and probability of prescriptions from 2005 and up to 2013 by category for those graduating between years 2003 and 2005, academic track only. Each coefficient is from a separate regression. Column (1) is the baseline for each outcome for those just below the cut-off. Models (2), (3) and (5) are reduced form RDD estimates of eligibility using a linear trend in credits either side of the cut-off but different bandwidths. Models (6) and (7) are 2SLS estimates of university attendance. Models (4) and (7) use a second polynomial in completed course credits. Model (5) also includes covariates as outlined in Table 2. Testing the null of the coefficient: $^{**}p < 0.05$, $^{***}p < 0.01$.

Abbreviation: RDD, regression discontinuity design.

5.4 | Sensitivity analysis

The findings presented in Tables 3 and 4 depend on a number of important assumptions, the most important of which is that there are no other unobserved confounders that also jump at the eligibility threshold. To support this claim, we have provided falsification and balancing tests in Section 4. In column (5) of Tables 3 and 4, we also include confounders that are strongly correlated with both the running variable and our outcomes and find that their inclusion does not have any substantive impact on the conclusions we draw. In Tables OA.6 and OA.7 in the online appendix, we provide a placebo test, using a false cut off of 2300 credits instead of 2250 credits, and find no clear jumps in our health outcomes. As a further sensitivity check, we estimate our main results using a triangular kernel in place of a rectangular kernel (see online appendix Tables OA.8 and OA.9) and find that our substantive conclusions remain the same. These results support our argument that observed and unobserved potential confounders are a smooth function of the running variable over the threshold, and that our substantive conclusions are robust to modeling choice.

Robust standard errors are presented in our main analysis. In Tables OA.10–OA.12 shown in the online appendix, we present the same results as shown in Tables 1, 3 and 4 but this time with standard errors clustered along the running variable. The use of clustered standard errors implies increased estimate precision. Our conclusions are therefore based on the conservative choice of standard error estimation.[19] If instead, the results of Table OA.11 are used to draw conclusions, then there is a more clear negative impact of university eligibility on male mental ill health and a more clear positive impact on female mental ill health.

Finally, we assess the risk of false discovery. The results presented in Tables 3 and 4 do not make adjustments for multiple hypothesis testing. In order to control for the false discovery rate across the two null hypotheses of no impact on hospitalizations due to external causes or mental health (as shown in Table 3), we apply the two-step procedure proposed by Benjamini et al. (2006) and find an increase in *p*-values. Furthermore, we observe that the impact on male mental health remains significant, albeit only at the 10% level.[20] The finding that mental ill health increased amongst males is therefore considered to be robust to modeling choices.

5.5 | Counter-factual outcomes and complier characterization

A number of alternative counter-factual outcomes are presented in Table 5 to help provide context to the observed impact of university attendance on healthcare use. The overwhelming majority of those who just miss out on achieving university eligibility (our counter-factual group) go on to adult further education, with a subset that find work and have a higher income. We find no clear impact on immediate unemployment. If the peer composition of adult further education or at the work place differs substantially from that of university then this may influence male attitudes to alcohol and narcotics and explain our jump in male hospitalizations.

Table 1 identifies the size of the complier population: the change in proportion attending university due to eligibility at graduation. Table 6 shows the background characteristics of the complier sub-population and compares them to the total study population. This analysis shows that the complier population have mothers and fathers that are both less educated. The compliers are therefore from a lower socioeconomic background than the mean student in our sample. Comparing across genders shows us that female compliers come from families with much lower levels of education, and have parents who live longer lives than male compliers.[21]

TABLE 5 Alternative outcomes to university attendance

	(1)	(2)	(3)	(4)	(5)	(6)
	8pp	16pp/8pp	16pp/8pp	8pp	16pp/8pp	16pp/8pp
Bandwidth	**Female**			**Male**		
Go onto adult further education						
Tertiary eligibility	−0.162***	−0.198***	−0.204***	−0.239***	−0.247***	−0.248***
	(0.010)	(0.014)	(0.015)	(0.008)	(0.009)	(0.008)
Income year after graduation						
Tertiary eligibility	−53.010	−27.478	−17.312	−87.463***	−52.439**	−54.291**
	(30.134)	(18.598)	(19.042)	(18.853)	(20.368)	(18.392)
Unemployed year after graduation						
Tertiary eligibility	−0.027***	−0.013	−0.008	0.011**	−0.004	−0.003
	(0.006)	(0.009)	(0.010)	(0.004)	(0.007)	(0.007)
Observations	12,671	13,525	13,525	15,692	17,119	17,119
Polynomial	1	1	1	1	1	1
Controls	N	N	Y	N	N	Y

Note: This table shows the regression discontinuity estimates of the impact of university eligibility for those graduating between the years 2003 and 2005 (academic track only) whom we followed until 2013. Each estimate is from a separate regression. Models (1) and (4) use a linear trend in course credits either side of the cut-off and a bandwidth of 8pp. Models (2) and (5) are as per models (1) and (4) but with a bandwidth of 16pp before the cut-off and 8pp after. Models (3) and (6) are as (2) and (5) but include covariates. Robust standard errors clustered at number of credits achieved are shown in parentheses. Testing the null of the coefficient: $^{**}p < 0.05$, $^{***}p < 0.01$.

TABLE 6 Complier characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Females			Males		
		Compliers			Compliers	
	Sample mean	abs	rel	Sample mean	abs	rel
A. Fathers						
High education	0.113	−0.025	−0.223	0.163	0.153	0.939
High income	0.453	0.461	1.018	0.501	0.606	1.209
Died	0.057	0.067	1.179	0.055	0.039	0.704
Swedish Born	0.765	0.861	1.125	0.794	0.870	1.096
B. Mothers						
High education	0.091	0.001	0.009	0.120	0.045	0.374
High income	0.068	0.084	1.241	0.089	0.023	0.261
Died	0.033	0.084	2.536	0.027	0.023	0.835
Swedish born	0.773	0.849	1.098	0.795	0.871	1.096

Note: The table reports complier characteristics for both school reforms. High Education = 1 if years of education>12. High Income = 1 if total annual income ≥ 100,000 Swedish Krona (circa $10,000) in 1990. Columns one and four give the mean background characteristics for the full sample. Columns two and five present the estimated mean for the subpopulation of compliers, that is, $\mathbb{P}(X = 1 | D_1 > D_0)$. Columns 3 and 6 give the relative likelihood that compliers have identical characteristics compared with the overall population $\mathbb{P}(X_1 = 1 | D_1 > D_0) / \mathbb{P}(X_1 = 1)$.

6 | CONCLUSIONS

This paper considers the impact of university attendance on various health-related outcomes. The empirical analysis shows that university eligibility leads to a clear positive discontinuity in the proportion attending university and also to a clear and economically meaningful jump in years spent in education.[22] Utilizing eligibility as an IV for university attendance, we find a suggestive worsening of male mental health, where a 10% point increase in university admission can potentially lead to a 1% point increase in hospitalizations related to mental ill health. These hospitalizations are often due to increased consumption of alcohol and narcotics, a relative increase of roughly 60% compared with the baseline. This implies that males at the margin of eligibility are potentially a sensitive group. For females, the results suggest that university attendance leads to a general reduction in mental ill health, although the health impacts for females are, in general, less precisely estimated. We find no clear impact on overall hospitalizations or prescriptions.

The human capital models of Grossman (2000) and Galama et al. (2018) predict positive but small general health impacts of education among young adults. For these adults, the protective effect of university education is likely observed through its impact on future discounting, and time preferences, which can in turn impact health-related behaviors (Cutler & Lleras-Muney, 2006). The developmental origins theory of Cunha and Heckman (2007) and Heckman (2012) also suggests that education can have a protective effect on health. Investments during adolescence have been found to be effective in improving non-cognitive skills (Cunha & Heckman, 2007), and by association, the same investments are likely to be effective on measures related to non-cognitive skills, such as anxiety and depression. Our findings for females are consistent with these theories, observing a protective effect on measures of mental health-related health care use, although if estimates are not precise enough to allow us to reject the null even though the effects are meaningful in size and consistent across several mental ill health outcomes. Moreover, Nordin et al. (2020) observe that the marginal student is generally accepted into low-quality universities and low-paying fields of education, which may explain why impacts are imprecize. However, our results for males are contradictory to the predictions of these theories.

To help understand the health impacts observed in our study, and why they differ across genders, we have provided a characterization of the compliers and analyzed a number of potential mechanisms, which found key differences across genders. The fact that both males and females observe similar jumps in university attendance and years of schooling, suggests that their differing characteristics and counter-factual outcomes may be the explanation for the opposing mental health impacts we find between the sexes. When compared to the male complier population, female compliers are from less educated but healthier households. Those just under the eligibility threshold generally go on to adult further education, while a subset of males just below the eligibility threshold also move into employment directly after graduating from upper-secondary school. Female and male compliers therefore differ in both their background characteristics and their counter-factual outcomes which potentially explains the differences in health impacts observed between genders.

The fact that male compliers come from less healthy households who often exhibit poor health related behaviors suggests that they are more easily influenced into risky health-related behaviors. The counter-factual of a higher employment rate immediately after graduation for marginally non-eligible male students also implies a different peer group to the marginally eligible. The observed jump in hospital admissions for mental ill health related to alcohol and narcotics amongst males could therefore be related to the effects of peer group composition changes affecting a group more susceptible to the influences of negative health behaviors. This conclusion is supported by results which highlight the negative impact occurring in the first 4 years after graduation, the time when the student is most likely attending university. The female counter factual group appears to go on to study at adult further education almost entirely, exposing the female student to a more similar peer group to that of university. However, it could just as well be that the marginal male student behaves differently at university, when compared with the marginal female student. University clubs and societies are known to encourage binge drinking, especially for males (DeSimone, 2007), and there may be other differing institutional factors which males respond to but females do not.

Our findings for males sit in contrast to those of De Walque (2007); Grimard and Parent (2007), and Buckles et al. (2016) who find that university education has a protective impact on smoking and life expectancy. These findings all use the Vietnam draft risk as an Instrumental Variable for years of university education for males, consider older cohorts, born in the 1940s and early 1950s, and the compliers are potentially specific to those induced by Vietnam draft. On the other hand, our results consider a different margin (university eligibility-induced attendance) and different health outcomes (medical care use amongst younger ages, rather than health behaviors and later life expectancy). In support of our findings, different education-induced health effects across genders have been observed elsewhere in the literature on education and health (Dursun et al., 2018; Fischer et al., 2021; Gathmann et al., 2015; Lindskog & Durevall, 2021). Fischer et al. (2021) for exemple, conclude that there are substantial negative impacts of mixed peer groups for males, but not for females.

Our findings of worsened mental health for males at the margin of university attendance are of particular interest to policy makers, as it is these marginal students who will be affected by the continued expansion of university education. The private gains of education for the *average* student are well documented (Card, 1999), but this is not necessarily the case for the *marginal* student who might be choosing between university education or employment after graduation from upper-secondary school. Nordin et al. (2020) have shown that the labor market gains for the marginal student in Sweden are positive and potentially substantive, but the results presented here show that there are other potentially important, non-market spillovers of university eligibility, which should also be considered when planning the future expansion of the university sector. Our results also show that existing estimates of the health returns of university education, relying entirely on Vietnam draft induced variation and therefore only account for the male population, do not necessarily translate across to females. Indeed, our findings suggest the opposite effect for females.

As a result of our sample and the age range of our relevant cohorts our results have some limitations. First, we have considered health using medical care use as a proxy for health, with a focus on particular causes of hospitalization and prescription that are relevant for the young adults we consider (accidents, injuries and mental health). The causes of hospitalization and prescriptions have been chosen because they are common for the age groups considered and the health outcomes they capture are potentially malleable to changes in education. Our primary interpretation is that changes in these outcomes are due to changes in the health status of the individual. We view this interpretation as credible for the causes we consider. However, we do not rule out other medical care-seeking behaviors and therefore the findings may well capture other interesting channels by which eligibility impacts health care-seeking behavior, such as reduced financial risk of time off to treat mental ill health or changed attitudes to mental ill health. Future analysis could consider more direct measures of health to assess the validity of this assumption. Second, whilst our 2SLS estimates are based upon a strong first stage, our standard errors show that we are only able to reject the null hypothesis of no health effects for relatively large effect sizes. University attendance may have wider impacts on young adult health, yet if these are relatively small effects, we are unable to detect them. Third, we capture health impacts early in adult life because we are only able to identify eligibility for cohorts graduating after 2003. Whilst it is important to understand the immediate health impacts of education to understand mechanisms, we are also interested if impacts are observed over the long-term, yet we are unable to assess this. Future changes in ease of access to university will have to weigh up the health (and labor market) impacts presented in this paper against the costs of providing easier access. This will have to be done whilst acknowledging that the full health impact of eligibility remains to be investigated.

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CONFLICT OF INTEREST

The authors declare that there is no conflict of interest.

DATA AVAILABILITY STATEMENT

This document provides a description of the data used for the article "The Health Returns of Attending University for the Marginally Eligible Student", by Gawain Heckley, Martin Nordin and Ulf Gerdtham. The micro data set on builds on several data sources drawn from Swedish administrative registers. Researchers can gain access to this data at a cost by submitting an application to Statistics Sweden (SCB, scb.se). The application should include a research proposal, a list of registers and the section criteria to be used and a list on the individuals that will use the data and their affiliations. Applications will have to be certified by SCB, and analyses of these micro data sets must be carried out on their secured server. We are happy to assist researchers, who have obtained access to the data and who have an ethical research approval from the Swedish Ethical Review Authority https://etikprovningsmyndigheten.se, to use our do-files to replicate our empirical research.

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ENDNOTES

[1] This research provides credible evidence of the impact of university on health behaviors and long-term health. The results show that increased years of college education reduces smoking initiation, increases chances of smoking cessation (De Walque, 2007; Grimard & Parent, 2007), and increases life-expectancy (Buckles et al., 2016).

[2] The system we describe here was in place between the years 1997–2010. During this period the system was slightly tweaked in 2003

[3] The large majority of students who complete their compulsory schooling choose to continue their studies at upper-secondary school with only 1.7% of students choosing not to continue with their studies. Whilst all students are able to continue their studies at upper-secondary school, there is an eligibility requirement. Those students who do not pass this eligibility requirement enter what is called an individual program with the aim to transfer to the standard upper-secondary school program at some point.

[4] Whilst a large proportion of students went on to study at upper-secondary school, many end up dropping out: for the period under consideration in this paper the drop out rate is about 25%.

[5] Information on prior grades is necessary as a control, and this is only available for those who attended the Swedish school system prior to starting upper-secondary school. We also do not want to include individuals who immigrated to Sweden when they were of secondary school age. We consider the years 2003 onwards because in the years prior to 2003 it was much easier to re-take courses over the summer after graduation. Prior to 2003, then, it was much more difficult to define a student had achieved university education eligibility at graduation, which is our cut-off. Before 2003, measurement errors, and the potential for manipulation of the cut-off, are considered to be significant threats to our identification strategy.

[6] See Appendix A and Table A1 for variable definitions and their codings.

[7] We assign nine for compulsory school, 11 for short high school, 12 for long high school, 14 for short university, 15.5 for long university and 19 for a PhD.

[8] Where parental education and income information is not available, dummy variables are included indicating missing information.

[9] First generation immigrant equals one if an individual is considered born outside Sweden but from EU28 countries, second generation immigrant is defined as one if an individual's parent's are considered born outside Sweden but from EU28 countries, mixed-second generation is defined as one if an individual is considered as having parents from different non-Swedish countries, one foreign born parent is defined as one if an individual is considered having both a Swedish and non-Swedish born parent.

[10] Nordin et al. (2020) show that the jump for those on the vocational track is much smaller and is why we choose to focus on the academic track students.

[11] We model bandwidth by running our linear regressions on the sample within the bandwidth. The discrete nature of the credit score means we are unable to non-parametrically choose the optimal bandwidth as recommended in general by Imbens and Lemieux (2008). 2pp is the smallest course (bin) size making 4pp a natural minimum bandwidth. 10pp is the largest bandwidth on the right hand side because any larger would require us to model 100% of a completed program which is a very large increase.

[12] The concern that clustered standard errors may give rise to poor coverage relates to parametric RDD with large bandwidths and small numbers of clusters. However in our analysis we consider small bandwidths close to the cut-off. This suggests that the use of robust standard errors in our empirical set-up may therefore be too conservative. How standard errors behave when clustering depends not on only the number of clusters, but also amongst other things on the variance within each cluster. In testing the minimum number of clusters (see e.g., Carter et al., 2017; Lee & Steigerwald, 2018) we find the recommended minimum number of clusters to be 4.8, which is less than the number of clusters available (12). Consequently, we present the more conservative robust standard errors in the main text and provide clustered standard errors as a sensitivity in the appendix.

[13] compulsory school grades, mother's and father's education and income plus dummies for missing information on education and income, dummies for European origin of first generation migrants and parents for second generation migrants, age of migration and a dummy for having one parent is a migrant.

[14] Balancing test figures for compulsory school grades, mother's income and father's income are shown in Figure A1.

[15] For scatter plots of individual outcomes not overlayed upon one another see appendix Figures A3 and A4 for external causes and mental ill health related hospitalizations respectively.

[16] In the online appendix (see Table OA.2) we also provide the same RDD results for females and males combined and they show no clear overall impacts on the outcomes we consider.

[17] For scatter plots of individual mental ill health outcomes not overlaid upon one another see appendix Figure A5

[18] In the online appendix (see Table OA.3) we combine the same RDD results for females and males, and find no clear overall impacts on the outcomes under consideration.

[19] Whilst robust standard errors are more honest in the spirit of Kolesár and Rothe (2018) we may be overly conservative in using robust standard errors. The concern of Kolesár and Rothe (2018) is that the coverage rate of clustered standard errors is low if there are few clusters and parametric RDD is used over a large bandwidth. The coverage rate can be improved through the use of a narrow bandwidth subject to good support in each bin or by using non-clustered standard errors. A narrow bandwidth minimizes the potential bias of the estimates. In our analysis we use small bandwidths to minimize the potential bias of our estimates so our use of robust standard errors may be too conservative.

[20] We apply the two-step procedure to the results of column (3) within sexes across the two specific cause hypotheses: External Causes, Mental Health Causes. We do not apply it to all the outcomes we test because we view Accidents, Self-harm, Other external, Alcohol, Mood and Neurotic as exploratory tests of our two main hypotheses rather than alternative hypotheses. Indeed their sum equals External causes and mental health causes respectively. A similar multiple hypothesis adjustment exercise when using the cluster robust estimates of Table OA.11 finds that the increase in mental ill health for males remain significant at the 1% level.

[21] Note that the negative values for females and high educated fathers are a consequence of using a Linear Probability Model to estimate the conditional probabilities. Their interpretation is clear however, showing that female compliers are much less likely to have high educated fathers than the general female population or male compliers.

[22] Jumps in years spent in education are observed in the range 0.19 and 0.27 years for females and 0.15 and 0.17 years for males, jumps which lie toward the upper end of those reported by the literature on the causal effect of education on health (Galama et al., 2018).

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SUPPORTING INFORMATION

Additional supporting information may be found in the online version of the article at the publisher's website.

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APPENDIX

Data description

We define dummy variables for hospital admissions between graduation and the year 2013 due to: *External Causes* or *Mental Health* disorders. Subcategories of external are considered: *Accidents* and *Self-harm*. Subcategories of mental ill health causes are also considered: *Alcohol and Narcotics* related *Mood Affective Disorders* and *Neurotic, Stress Related, Somatoform Disorders*. For prescriptions we consider: *Mental Health Related* prescriptions which include *Anti-depressives* and drugs used to treat *Anxiety, Sleep, Stress*. The exact definitions and their corresponding ICD/ATC codes are provided in Table A1.

TABLE A1 ICD code definitions of causes of hospitalization and Anatomical Therapeutic Chemical (ATC) code definitions of prescriptions

Hospitalizations	
Diagnosis:	*ICD 10 code:*
External causes	= 1 if external cause or diagnosis S,T
Accidents	V, X01-X59
Self-inflicted harm	X60-X84
Mental ill-health causes	F1,F3,F4
Alcohol- & Narcotics-related	F1
Mood affective disorders	F3
Neurotic, stress-related, somatoform disorders	F4
Prescriptions	
Diagnosis:	*ATC code:*
Mental health-related	N06 A, N05 B, N05 C
Anti-depressives	N06 A
Anxiety, sleep, stress	N05 B, N05 C
Opioids	N02 A

Abbreviation: ICD, International Classification of Diseases.

Our sample starts with 128,751 students who graduated from upper-secondary school between the years 2003 and 2005 and who had previously graduated from Swedish compulsory school. We exclude pupils who finish more than one year later (1.3%) or more than one year in advance (only 12 observations). We also exclude those on the individual program as they cannot achieve university eligibility. Most students start upper-secondary school aged 16 and graduate at age 19. It is not uncommon for students to finish upper-secondary school at an older age (12.0%) than the typical graduation age of 19. A small share finish at a younger age (2.8%). There are many common and valid reasons for graduating older than 19 years of age: retaking courses, study breaks, changing programs or studying abroad. Students who graduate before the age of 19 have typically also started compulsory schooling before the mandatory starting age. Keeping students who finish at age 18 or 20 has no impact on the results in this study. This results in sample sizes of between 12,000 and 17,000 depending on the bandwidth and gender chosen. Descriptive statistics for education and pre-determined covariates are shown in Table A2 for the main bandwidths used in the analysis.

TABLE A2 Descriptive statistics

	8pp/8pp Bandwidth		16pp/8pp Bandwidth	
	Females	Males	Females	Males
University attendance	0.580	0.521	0.562	0.497
	(0.004)	(0.004)	(0.004)	(0.004)
Years of education	13.46	13.21	13.41	13.16
	(0.01)	(0.01)	(0.01)	(0.01)
Compulsory school grades	216.13	207.29	214.93	205.99
	(0.35)	(0.28)	(0.34)	(0.27)
First generation EU-28	0.01	0.01	0.01	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
Second generation EU-28	0.01	0.01	0.01	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
Parents of mixed foreign background	0.01	0.01	0.01	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
One foreign born parent	0.10	0.10	0.10	0.10
	(0.00)	(0.00)	(0.00)	(0.00)
Migration age	0.65	0.55	0.67	0.58
	(0.02)	(0.02)	(0.02)	(0.02)
Father's education	10.28	10.91	10.24	10.84
	(0.04)	(0.03)	(0.04)	(0.03)
Mother's education	10.51	11.07	10.45	11.00
	(0.04)	(0.03)	(0.03)	(0.03)
Father's income	1333	1438	1323	1427
	(6.97)	(9.13)	(6.70)	(8.56)
Mother's income	780	827	776	821
	(3.84)	(3.70)	(3.72)	(3.52)
Observations	12,671	15,693	13,525	17,120

Additional figures

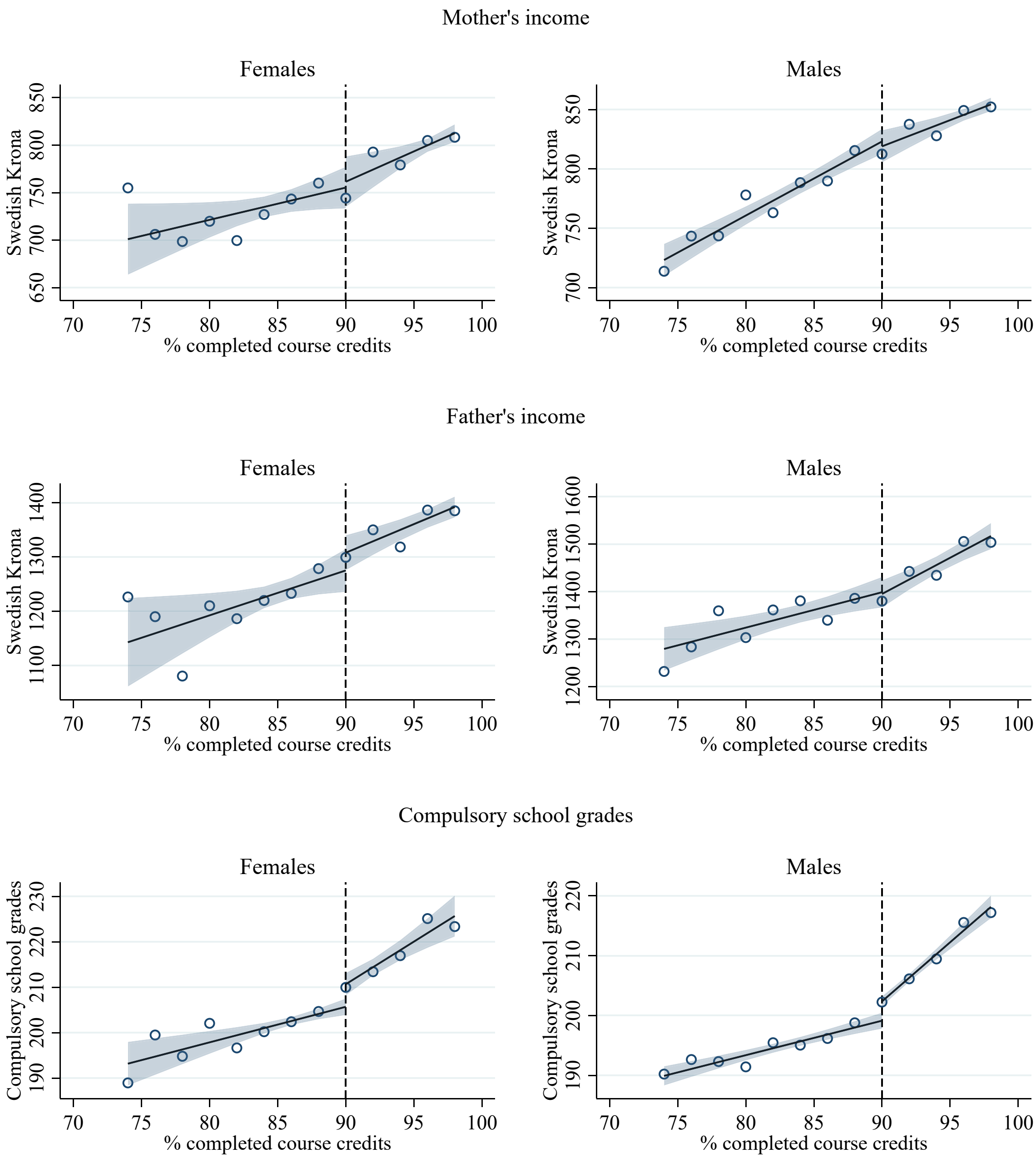

FIGURE A1 Course credit profile of pre-determined characteristics. These figures plot various diagnostic tests using percentage of a completed program as the running variable shown in bins of 2pp of a program. In all figures we present the mean for each bin. The dashed vertical line is the 90% cut-off for university eligibility. See notes for Figure 1 [Colour figure can be viewed at wileyonlinelibrary.com]

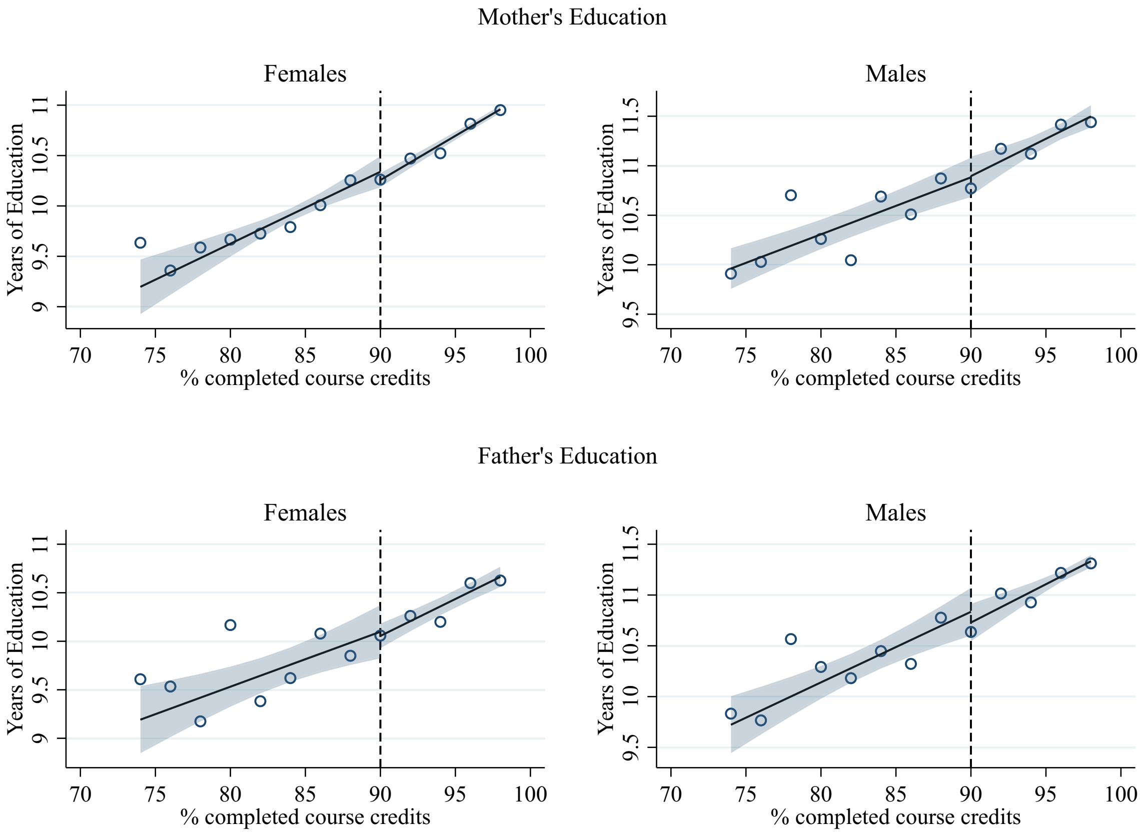

FIGURE A2 Course credit profile of pre-determined characteristics (continued). These figures plot various diagnostic tests using percentage of a completed program as the running variable shown in bins of 2pp of a program. In all figures we present the mean for each bin. The dashed vertical line is the 90% cut-off for university eligibility. See notes for Figure 1 [Colour figure can be viewed at wileyonlinelibrary.com]

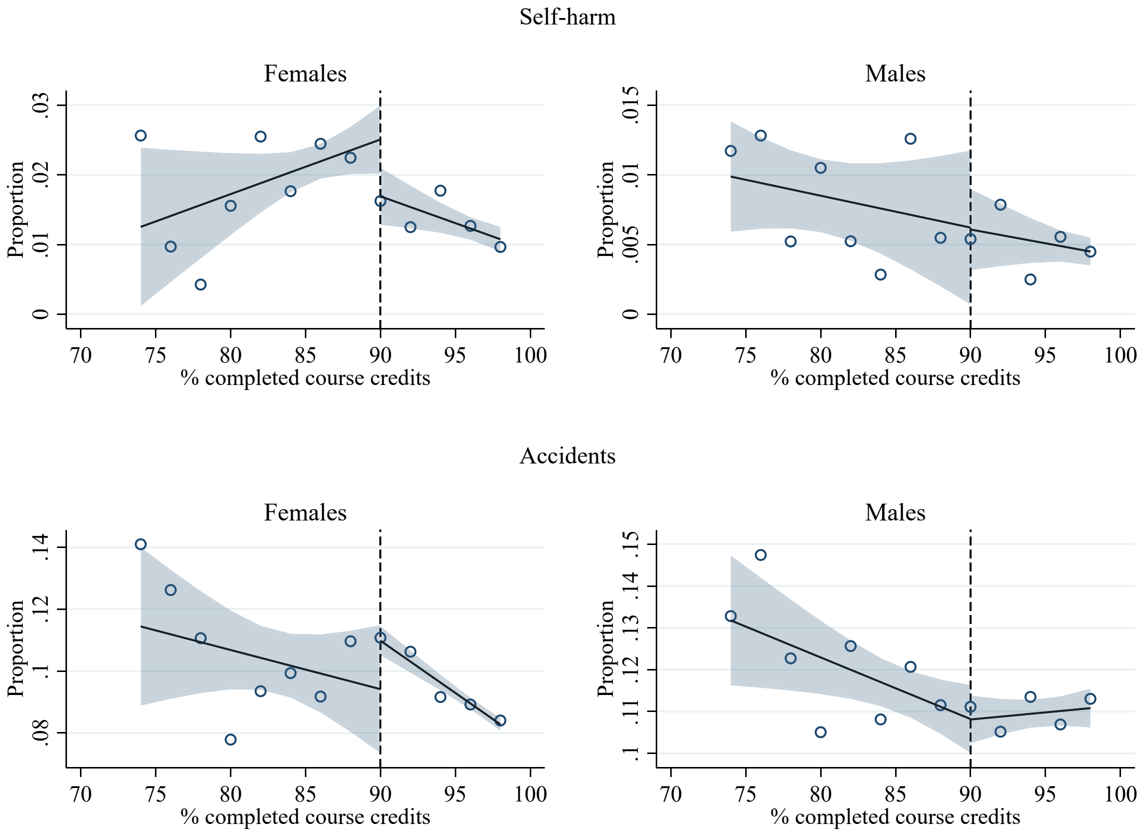

FIGURE A3 Course credit profile of hospital admissions due to external causes. This figure plots a scatter of the proportion diagnosed at hospital between graduation and the year 2013 by diagnosis against percentage completed of a full program with a bin width of 2pp of a full course. These data are for those graduating upper-secondary school between the years 2003 and 2005 (academic track only). See notes for Figure 1 [Colour figure can be viewed at wileyonlinelibrary.com]

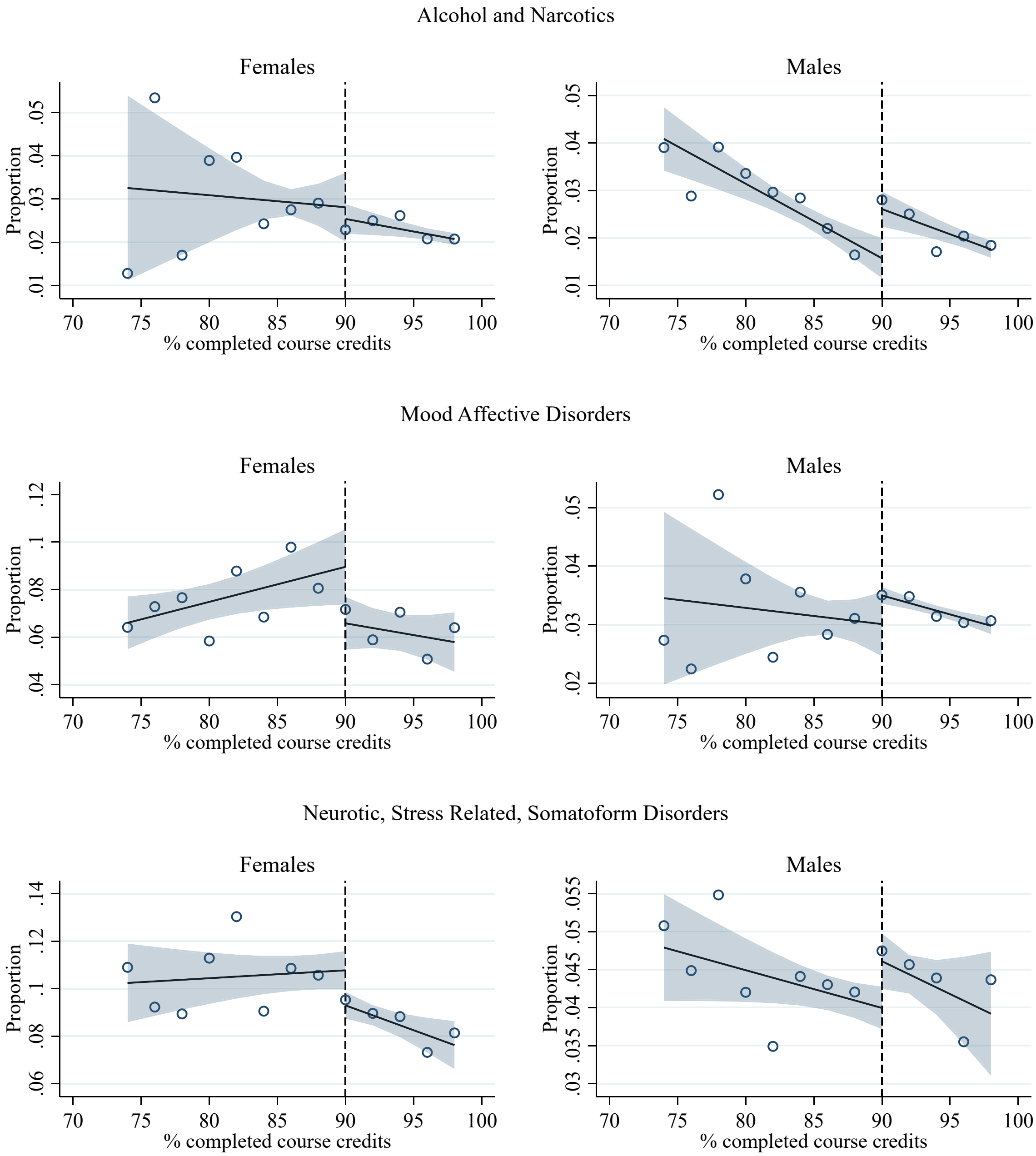

FIGURE A4 Course credit profile of hospital admissions due to mental health. This figure plots a scatter of the proportion diagnosed at hospital between graduation and the year 2013 by diagnosis against percentage completed of a full program with a bin width of 2pp of a full course. These data are for those graduating upper-secondary school between the years 2003 and 2005 (academic track only). See notes for Figure 1 [Colour figure can be viewed at wileyonlinelibrary.com]

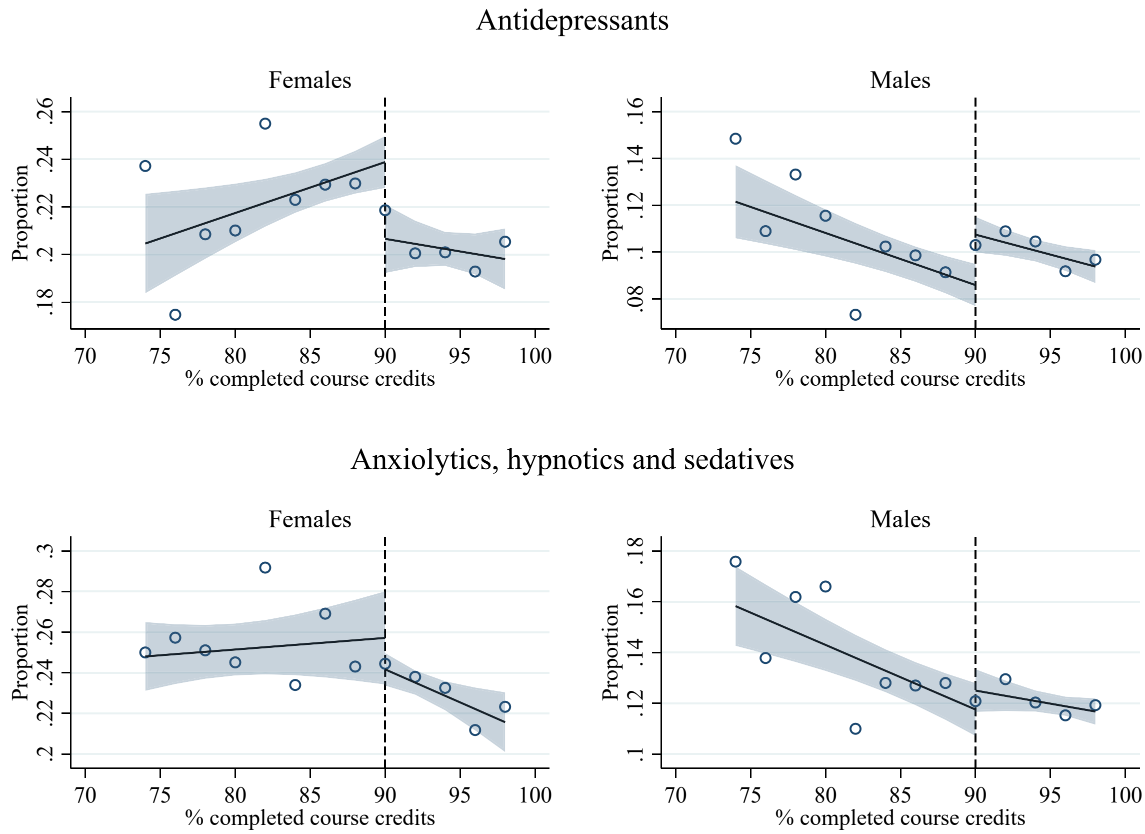

FIGURE A5 Course credit profile of prescriptions due to mental health. This figure plots a scatter of the probability of prescription between graduation and the year 2013 by cause against percentage completed of a full program with a bin width of 2pp of a full course. These data are for those graduating upper-secondary school between the years 2003 and 2005 (academic track only). See notes for Figure 1 [Colour figure can be viewed at wileyonlinelibrary.com]