Caught in the Cycle: Economic Conditions at Enrollment and Labor Market Outcomes of College Graduates*

Alena Bičáková
CERGE-EI

Guido Matias Cortes
York University

Jacopo Mazza
University of Essex

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Abstract

We find robust evidence that cohorts of male graduates who start college during worse economic times earn higher average wages than those who start during better times. This gap is not explained by differences in selection into employment, in economic conditions at the time of college graduation, or in field of study choices. Graduates who enroll in bad times are not more positively selected based on their high-school outcomes, but they achieve higher college grades, sort into higher-paying occupations, and earn higher wages conditional on their grades. We find similar but less robust patterns for female graduates. Our results suggest that individuals who enroll during economic downturns exert more effort during their studies.

Keywords: Business Cycle, Higher Education, Cohort Effects

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1 Introduction

Do business cycle fluctuations have long-lasting impacts on individual outcomes? The answer to this question is crucial for our understanding of the welfare consequences of recessions. Most of the literature on this topic has focused on the long-term earnings losses induced by recessions, due to job separations or decreased job finding probabilities.[1] Recessions, however, also impact long-term individual outcomes through a separate and equally crucial channel, namely by influencing investment in human capital. Several contributions to the literature have shown that enrollment in higher education tends to increase during economic downturns.[2] In spite of this well-documented link, little is known about how college graduates who enroll during downturns end up performing once they enter the labor market. Analyzing how these cohorts perform is essential if we want to understand the long-term impacts of recessions that operate via changes in human capital investment decisions.

There are strong reasons to expect that the labor market outcomes of graduates who enrolled during adverse economic times will differ from those who enrolled during periods of low unemployment. The increase in college enrollment observed during recessions is likely associated with a change in the size as well as the composition of skills of the cohort of graduates. At the same time, the resources available per student may vary over the business cycle.[3] Finally, the recession may affect students' career choices, or induce changes in the time and effort that individuals allocate towards their studies.

This paper analyzes the link between economic conditions at the time of college enrollment and future labor market outcomes using data from fifty-eight cohorts of college graduates in the United Kingdom. Our key finding is that male graduates that start university during worse economic times have systematically better average labor market outcomes than those who start during better times. This gap is not explained by differences in the economic conditions at the time of college graduation or by

[1] Davis and von Wachter (2011), for example, show that workers who exogenously lose their job during times of high unemployment experience substantially larger permanent earnings losses than those who experience a similar shock when unemployment rates are lower. A number of papers show that entering the labor market during a recession has long-lasting negative effects on individuals' career outcomes (Aslund and Rooth, 2007; Kahn, 2010; Oreopoulos et al., 2012; Altonji et al., 2016b; Liu et al., 2016; Schwandt and von Wachter, 2019).

[2] See, among others, Betts and McFarland (1995); Dellas and Sakellaris (2003); Clark (2011); Méndez and Sepúlveda (2012); Johnson (2013); Barr and Turner (2013, 2015); Sievertsen (2016); Atkin (2016) and Charles et al. (2018).

[3] Kane et al. (2005) show that, due to balanced budget requirements, state appropriations for higher education in the U.S. tend to fall during economic downturns. Note that even if funding remained constant, the increase in enrollment during downturns would lead to a decline in resources per student. Bound et al. (2010) show that resources per student outside of the most selective universities have declined over time as enrollment cohorts have become larger.

changes in the composition of the cohorts in terms of field of study. It also does not come at the expense of worse labor market outcomes for women from these cohorts, as they show similar, although weaker, patterns of improvement. Using information on nationally comparable measures of academic achievement at both the high school and the college level, we show that the wage differentials cannot be explained by changes in the composition of graduates at the point of college entry. Rather, the results point towards an increase in the effort provided by graduates during (and potentially also after) their college years.

Our analysis relies on data from the UK Quarterly Labour Force Survey from 1998 to 2019. By exploiting information on the timing of graduation, we construct a long series of cohorts based on their year of college enrollment, ranging from 1957 to 2014.[4] Our empirical strategy compares wage outcomes across cohorts of college graduates who enroll at different points in the business cycle. Relying on the rich cross-sectional and time dimensions of our data, and the fact that an individual's enrollment cohort is not a perfect function of their age and the calendar year when their wages are observed (because individuals may enroll into college at different ages), our identification approach controls for time and age effects, and identifies the effect of business cycle conditions at entry based on the deviations of cohort quality from a quadratic long-run trend. Business cycle conditions are proxied by the national unemployment rate in the year of enrollment.

We find that, conditional on age effects, time effects, and the long-run trend in cohort quality, a 3 percentage point increase in the unemployment rate at the time of enrollment (approximately one standard deviation in the sample), increases average male cohort wages by around 1.8%. For women, the magnitude and significance of the estimated coefficient on unemployment at enrollment is to some extent sensitive to the type of cohort trends that we control for. However, the point estimate is consistently positive. The results are also robust to an alternative estimation approach which exploits regional variation in unemployment rates within cohorts.

We consider four mechanisms that could potentially explain our results: (i) selection into employment, (ii) the correlation between unemployment at entry and unemployment at exit from college; (iii) the impact of the business cycle on students' major choices; and (iv) changes in selection into different industries or occupations across cohorts entering university at different phases of the business cycle. We do not find any evidence in our data supporting the first three channels. Male graduates who enrolled at different points in the cycle are not differentially selected into full-time employment; female graduates, however, are somewhat more likely to work part-time

[4]Throughout the paper we follow the convention in the literature to refer to university as "college". The group that would normally be referred to as college graduates in the UK (those who completed A-levels) are referred to, in this paper, as high-school graduates. More details about the UK education system are provided in Section 2.1.

instead of full-time. Regarding conditions at graduation, we show that unemployment rates at enrollment and graduation are positively correlated over our estimation period, implying that individuals who enroll during bad times tend to, on average, also enter the labor market during relatively bad times. We also do not observe a large shift towards higher paying majors among students who enroll during periods of poor macroeconomic conditions. In terms of the final channel, we find that the wage differential can be partially explained by sorting into higher paying occupations, but a substantial wage gap remains even when conditioning on detailed occupation fixed effects.

Our results provide strong evidence that the ex-post quality of graduates who enrolled during bad times is higher than that of graduates who enrolled during good times (particularly among men). This implies that there is either better ex-ante selection at the time of college entry, or there are changes in human capital accumulation occurring during the cohort's college years. To distinguish between these two potential explanations, we leverage information on academic achievements at both the high-school and the university level.

Consistent with the intuition that marginal students who decide to enroll during bad times are drawn from the lower end of the ability distribution of potential college-goers, we find that the high school outcomes of college graduates who enroll during bad times are similar, or if anything slightly worse, than those of college graduates who enroll in good times. It is important to note that this is true for both men and women and that we are only focusing on those who *completed* their undergraduate degrees. Hence, we conclude that our wage differentials cannot be explained by an improvement in *ex-ante* cohort quality due to changes in selection at the time of college entry, or by differential selection in terms of which students graduate from different enrollment cohorts.

Our measures of academic achievement in university do, however, confirm that graduates who enroll during bad times are of better quality at the point of undergraduate study completion. Specifically, we find that, in spite of the lack of advantage at the high-school level, the cohorts of graduates who enrolled during periods of higher unemployment achieve higher university grades, particularly in the case of men and, remarkably, earn higher wages even conditional on their university grade point average.

Absent any clear evidence of an increase in the quality of education during downturns, we interpret these findings as suggesting that graduates who enroll in university during bad times improve their human capital acquisition by exerting more effort during their university studies (and potentially also after they enter the labor market), particularly in the case of men. Effort adjustments in response to adverse economic conditions have been observed in other contexts by Lazear et al. (2016), Mukoyama et al. (2018) and Griffith et al. (2016). Blom et al. (2015) find that students in the

US who enroll during worse economic times pursue more challenging majors, which is also consistent with an increase in effort. Given the institutional features that limit students' ability to change majors in the UK, our findings suggest that the increased effort among UK students enrolling during adverse economic conditions manifests itself within, rather than between majors.

We propose three potential channels through which the increase in effort might arise. First, the increase in cohort size due to countercyclical enrollment would lead to increased competition, which might encourage higher effort (see Morin, 2015, for evidence on the relationship between cohort size and effort among male university students). Second, the lack of (part-time) employment opportunities might allow students to dedicate an increased proportion of their time towards their academic studies (see Darolia, 2014; Neyt et al., 2019, for evidence on the relationship between employment and student outcomes). Finally, as suggested by the impressionable years hypothesis (Krosnick and Alwin, 1989), the experience of poor economic conditions during early adulthood might generate a change in attitudes among the students that enroll in bad times (at least among the ones who do complete their studies), leading them to adjust their effort levels in university and thereafter. While assessing the relative importance of the three mechanisms is of high interest and policy relevance, it is beyond the scope of this paper and is left for future research.

This paper provides a number of important contributions to several streams of the literature. It is the first study that directly analyzes how the composition of college graduates varies according to the business cycle conditions prevailing at enrollment, and what the implications of these changes are for their future labor market outcomes.[5] We contribute to the rich line of research on the implications of macroeconomic conditions for workers' current and future economic achievements (see e.g. Beaudry and DiNardo, 1991; Baker et al., 1994; Gibbons and Waldman, 2006; Hagedorn and Manovskii, 2013), by highlighting the previously disregarded link operating via the increase in college enrollment that is induced by weak aggregate conditions. Our findings also complement the numerous studies on "scarring" effects (Kahn, 2010; Oreopoulos et al., 2012; Altonji et al., 2016b; Liu et al., 2016; Schwandt and von Wachter, 2019) by emphasizing the salience of entering as well as exiting conditions for college graduates' future payoffs.

Our finding that student effort increases during adverse times, and results in improvements in future labor market outcomes (particularly for men), has at least two crucial implications. First, it calls into question the external validity of instruments for schooling based on labor market conditions at the time of enrollment. Second, it provides supportive evidence for the interpretation of education as enhancing human

[5] The only papers that we are aware of that directly analyze changing selection into post-secondary education over the business cycle are Alessandrini (2018) and Arenas and Malgouyres (2018), who consider the impact of these changes for intergenerational educational mobility.

capital, rather than merely serving as a signal of individuals' innate ability.

The rest of the paper is organized as follows. Section 2 describes our dataset and our empirical strategy. Section 3 presents the key results in terms of wage outcomes across cohorts as well as a number of robustness checks. Section 4 explores various potential mechanisms through which these cohort-level wage differences may arise. Section 5 investigates the merit of the two possible interpretations of our findings in terms of ex-ante ability vs. effort. Finally, Section 6 presents the conclusions.

2 Data and Empirical Strategy

2.1 Background: Higher Education System in the UK

In the UK, students attend secondary school until the age of 16, at which point they take a General Certificate of Secondary Education (GCSE) examination. This marks the end of compulsory education.[6] The GCSE diploma is required to continue on to post-compulsory studies, which involve two additional years of education leading to a standardized school-leaving qualification called 'A-levels' (short for General Certificate of Education – Advanced level). Students can choose the subjects that they wish to take A-level exams in. Most universities require at least three A-levels for admission.

After A-levels, around age 18, students can choose to pursue further studies at university level. Undergraduate degrees (often referred to as *first degrees* in the British Higher Education system) normally involve three years of studies in England, Wales and Northern Ireland, with some exceptions for degrees such as Medicine. In Scotland, the standard length of an undergraduate degree is four years. At graduation, students are classified according to five possible degree classes which, in descending order, are: first-class, second-class upper division, second-class lower division, third class, and ordinary degree otherwise called a "pass". Which degree is awarded depends on the weighted average of the grades obtained during the course of study (with a higher weight usually assigned to grades obtained in the later years).

Throughout the paper, and following the convention in the literature, we use the term 'college graduates' to refer to individuals who are awarded a university-level Undergraduate (Bachelor's) degree.

[6] In England, compulsory education or training has been extended to age 18 for those born on, or after, 1 September 1997.

2.2 Data

2.2.1 Individual-Level Data

Our analysis is based on the UK Quarterly Labour Force Survey (LFS). The LFS is a widely used survey covering around 60,000 households living in the UK in each survey. It is managed by the Office of National Statistics (ONS) and has been conducted quarterly since 1992. We concentrate our analysis on 85 quarterly samples from 1998 to 2019, for which our key variables of interest are available.[7] The LFS sampling has a rotating panel design, where individuals stay in the sample for five consecutive waves or quarters. Earnings questions are asked during individuals' first and fifth quarter in the sample, and we restrict our analysis to individuals in these waves. We also restrict our sample to university graduates aged 25-65, and exclude self-employed workers, employees with missing wage information, and a small number of employees with outlier weekly wages (below £25 or above £3000 per week in real 2015 pounds).

Schooling variables – The LFS presents several advantages for our analysis. In addition to recording individuals' highest level of education it collects information on the year of graduation,[8] the major studied in college and, since the last wave of 2005, two measures of educational performance: the number of GCSE exams passed in high-school and the degree class achieved at the end of university. This unique feature of the data allows us to observe educational performance at different stages for a large sample of individuals from a wide range of cohorts.

Being able to identify the exact moment at which individuals achieve their highest level of education is crucial for our purposes, as it allows us to infer, with a fair degree of accuracy, when the individual enrolled into tertiary education, and hence the macroeconomic conditions that prevailed at the time of enrollment. This is in contrast to most datasets which only record individuals' highest achieved education level, but not when they obtained this degree. Researchers who use such datasets and are interested in the impact of macroeconomic conditions at the time of college entry (or graduation) must make the assumption that individuals started their studies at the standard age of high-school graduation (see e.g. Blom et al., 2015). This assumption is not innocuous. According to Barr and Turner (2013), only about 54% of undergraduate students in the US were of traditional college age in 2010. Moreover, and importantly for our purposes, cyclical shocks may have differential effects across age groups in terms of post-secondary enrollment, making the assumption particularly

[7] This includes all quarters from 1998 to 2019, with the exception of the first quarter of 2001, for which no earnings data is available, the first quarter of 2004, for which no information on educational levels is available, and the first quarter of 2005, for which education variables are inconsistent.

[8] This information has been collected starting in 2001. Before then, respondents were asked to declare the age when they left full time education. For the 1998, 1999 and 2000 waves we use this information to impute the year of graduation.

problematic for business cycle analysis. Having information on the year of graduation for each individual in our sample is therefore an important advantage of our dataset.

Construction of cohorts by enrollment year – We impute the year of enrollment as the year of graduation minus three for all major categories except for graduates in Medicine for which the normal course of study takes five years. In Scotland, the length of a standard undergraduate degree is four years. Unfortunately, the publicly available LFS data do not provide information on where individuals obtained their undergraduate degree. For the waves from 2001 onward we do, however, know whether individuals were born in Scotland. Analysis of restricted-use LFS data from April-June 2017 supplied by the Data Advice and Relations Team at the ONS shows that nearly 85% of undergraduate degree holders who were born in Scotland also studied at a Scottish university. Hence, when information on the location of birth is available, we impute the year of enrollment as the year of graduation minus four for individuals born in Scotland. We also check the robustness of our results to excluding the Scottish born. Another limitation of the LFS is that it only provides information on the timing of completion of individuals' highest degree. Hence, for individuals who have completed a postgraduate degree, we impute their year of undergraduate enrollment based on the year in which they turned 18.

We drop observations whose imputed year of enrollment in college is inconsistent (e.g. before the individual turned 16) and observations where the age at university completion is over 45. Individuals for whom the year of university entry is less than four years prior to being interviewed are also omitted, as they may still be pursuing further studies. Finally, we exclude foreign nationals who obtained their college degree before the year in which they arrived in the UK, as they would not have been directly affected by the macroeconomic conditions that prevailed in the UK at the time of their enrollment.

The assignment of enrollment years allows us to group individuals into cohorts according to their year of enrollment, ranging from 1957 until 2014. Although we only observe labor market outcomes after 1998, we are able to infer the business cycle conditions that prevailed at the time of enrollment for all of these cohorts. Naturally, the observed labor market outcomes will be affected by time, cohort, and life cycle effects. Section 2.3 provides a detailed discussion of how our empirical strategy identifies the effects of business cycle conditions at enrollment while accounting for time, cohort, and life-cycle patterns in wages.

Our imputation procedure opens up some concerns of misclassification, as some students might exceed the normal length of their university course due to slower progress, part-time study or study breaks after enrollment. If that is the case, we would be assigning the wrong starting date, and therefore the wrong unemployment rate, to the delayed students. Our main specification focuses on the economic conditions prevailing in the (imputed) year of enrollment, but we check the robustness of our main result in different ways, including: (i) using unemployment rates observed in the years immediately preceding and following the enrollment year, (ii) excluding individuals with postgraduate degrees, given that the imputation procedure is much more precise for those who only have an undergraduate degree, and (iii) restricting the sample to those who, based on their age at graduation, would not have exceeded the normal length of studies for their degree.

Sample selection – Our analysis focuses on individuals who have completed an undergraduate degree. In this respect, we focus on the same group of individuals as the literature exploring the effect of economic conditions at the time of graduation, and complement the findings in that literature by providing evidence on the impact of economic conditions at the time of college *entry*.

Naturally, the composition of this group of graduates varies over time and over the business cycle according to selection into university and in terms of who graduates. Our goal is to understand these patterns of selection by analyzing how the cohorts of graduates who enroll during bad times differ from those who enroll during good times.[9] Hence, our analysis speaks to the question of how economic conditions impact future labor market outcomes for individuals who *enroll in tertiary education and complete their degrees*. Given our use of retrospective information on the timing of enrollment, and the lack of direct evidence on individuals who drop out of college, our findings cannot speak to the question of what happens to the broader cohort of students enrolling at different points in the cycle (i.e. without conditioning on graduation). Whether the composition of our sample of individuals who enroll in bad times and complete their degrees is differentially selected in terms of pre-university ability compared to cohorts of graduates who enroll in good times is something we directly investigate in Section 5.1.

Our main outcome variable of interest is real weekly earnings for full-time workers. Hence, there is a further margin of selection in terms of who is in full-time employment, something we directly analyze in Section 4.1.

2.2.2 Macroeconomic Conditions at the Time of University Enrollment: Unemployment Rate Data

To capture aggregate labor market conditions, we focus primarily on the national unemployment rate, as measured by the Office for National Statistics (ONS).[10] We

[9] Our identification strategy is discussed in detail in Section 2.3.

[10] See https://www.ons.gov.uk/employmentandlabourmarket/peoplenotinwork/unemployment, last accessed 29/05/2020. The survey-based series has only been available on a consistent basis since 1971. Since our data includes cohorts of university graduates who enrolled between 1957 and 2014, we resort to unemployment figures based on administrative sources for the years before 1971. These are available from Denman and McDonald (1996).

consider the national unemployment rate to be the relevant indicator for our population of reference. Wozniak (2010) finds that highly educated workers can smooth labor market shocks through migration more easily than other individuals. This is especially relevant for the UK context where local labor markets are often geographically contiguous. Additionally, given the salience of the national unemployment rate, it is likely that families and individuals take it into consideration when choosing whether to enter college.[11] Finally, as national (rather than local) unemployment rates are regularly discussed in media, the information is more easily observed by individuals and more likely to attract their attention.

Data constraints also limit our ability to allocate relevant local unemployment rates to individuals. Specifically, we have no direct information on where individuals studied or where they lived in the years leading up to college enrollment. In spite of these limitations, in Section 3.3 we check the robustness of our results using local unemployment rates data based either on UK country of birth (England, Wales, Scotland or Northern Ireland), or on individuals' more disaggregated current region of residence. Data on local unemployment rates is also obtained from the ONS.[12] As the official local unemployment rate series start only in 1992, we follow the approach of Clark (2011) and use historical data on unemployment benefit claimants (claimant count data) that is available since 1975. Specifically, we use annual series of the ratio of the number of claimants who are resident in each area as a percentage of workforce jobs plus the claimant count as a proxy for the local unemployment rate.[13]

Panel A of Figure 1 plots the UK national unemployment rate for 1957–2019. The Figure shows the well documented increase in unemployment in the 1970's and early 1980's and the negative impact of the economic recession of the early 1990's and the financial crisis of 2008–2009. It also shows that even during more recent periods of strong growth, the very low levels of unemployment that the UK enjoyed in the aftermath of World War II were never recovered. Our empirical strategy, discussed in detail in Section 2.3, will control for long-run trends and exploit only shorter-term fluctuations in our data.

Panel B plots the local unemployment rates (based on the claimant count data as described above) for the four UK countries between 1975-2019. The four series move very much in parallel, and in a similar fashion as the national unemployment rate over the overlapping time period plotted in Panel A.

[11] Altonji et al. (2016b) also argue that national economic conditions are likely more relevant for college graduates than local conditions; however, in their analysis they use census division unemployment rates in order to obtain additional variation.

[12] The data were extracted from `https://www.nomisweb.co.uk/` on April 2 and 6, 2020.

[13] We compared the series with the local unemployment rates available from the ONS since 1992 and the correlation was close to 1.

2.2.3 Descriptive Statistics

Throughout the paper we analyze outcomes for men and women separately. Panel A of Table 1 shows descriptive statistics for our main sample, which includes 96,543 observations for men and 76,784 observations for women. Individuals in the sample are predominantly white and only a small minority is born outside of the UK. Average real weekly wages in 2015 pounds are higher on average for men compared to women. For both samples, around a third of graduates hold a postgraduate degree and around three-quarters enrolled between 1980 and 2009.

In Panel B we show the composition of the sample across university majors. This sample is smaller given that information on undergraduate field of study is not available for individuals with a postgraduate degree (and is sometimes missing for individuals whose highest degree is an undergraduate). To categorize university majors we use UNESCO's International Standard Classification of Education (ISCED), 2013 update. The table shows that men tend to favor degrees in engineering, natural sciences and business and law, while women's most frequent choice is arts and humanities, followed by business and law, natural sciences and social sciences. In line with existing evidence, women are severely underrepresented in engineering.

For our analysis that considers individuals' academic performance, we must restrict our attention to post-2005 observations. Information for this sample is presented in Panel C of Table 1. High-school performance, measured as the number of GCSEs, tends to be slightly better for women, among whom the share with 8 or more GCSEs above a C grade is two percentage points higher than among men. Unsurprisingly, the majority of college graduates belong to the highest high-school achievement category. The table also shows that women outperform men in college as well. In fact, while the share of graduates with "first class" degrees is the same (11%) for both men and women, the share with "upper second class" degrees is eight percentage points higher for women than for men.

2.3 Empirical Strategy

The literature on cohort effects specifies labor market outcomes as being a function of: (i) current labor market conditions, (ii) individual labor market experience, and (iii) the cohort that the individual belongs to. Identifying all three factors separately is typically challenging, as fixing two dimensions generally determines the third. For example, when estimating a wage regression with controls for calendar year and time since entry into the labor market, it is not possible to identify cohort effects associated with the year of labor market entry, as this is equal to the calendar year minus the number of years since entry. Similarly, if one controls for calendar year effects and age, it is not possible to identify birth cohort effects.

This identification problem is typically overcome by imposing restrictions on the cohort effects. Hall (1971) and Berndt et al. (1995) suggest dropping one cohort effect and restricting the remaining cohort effects to add up to zero. Oreopoulos et al. (2012), in their analysis of how economic conditions at the time of labor market entry affect career outcomes, exclude two cohort effects from their analysis (see their footnote 10). In a similar fashion, Kwon et al. (2010) implement an identification approach that omits two cohort effects. They specifically choose to omit the effects for the first and the last cohort in their sample, as this allows them to identify the evolution of the non-linear component of the cohort effects, around a (non-identified) long-run linear trend. An alternative approach is to control for one of the factors in a non-linear fashion, given that the multicollinearity arises only among the linear terms. This is the approach adopted by Antonczyk et al. (2018).[14]

In this paper, we are interested in how labor market outcomes vary across cohorts that enroll into college at different stages of the business cycle. This implies that, in our context, cohorts are defined by the year of college enrollment. We would therefore not be able to simultaneously identify enrollment cohort effects, calendar year effects, and the effect of years since enrollment. We can, however, simultaneously identify enrollment cohort effects, calendar year effects, and *age* effects. This is due to the fact that not all students enter college immediately after high school. Hence, among individuals of age a observed at time t, there is variation in the enrollment cohort that they belong to, as students enroll at different ages (and hence in different years). This is one of the advantages of using the UK LFS, where we have information on when each individual graduated. Many studies in the literature impute the year of graduation based on the normal age of college exit. In such a context, by construction, all individuals of age a observed at time t would be imputed to have enrolled in the same year, and hence there would be no independent variation to allow identification of cohort effects, calendar year effects, and age effects.

Having variation in the age at enrollment means that we can simultaneously control for: (i) the current stage of the business cycle (captured by calendar year), (ii) potential labor market experience (captured by age), and (iii) cohort effects (without imposing additional restrictions as the previous literature does).

Although we could, in principle, fully identify enrollment cohort fixed effects (due to the variation in age at enrollment), we would not, at the same time, be able to estimate the impact of our primary variable of interest – the national unemployment

[14]Other papers that analyze the effect of entry conditions on future labor market outcomes perform their estimation without controlling for (long-run) trends in cohort effects. In particular, the specification used by Altonji et al. (2016b) to estimate the overall (non-major-specific) effect of economic conditions excludes graduation year fixed effects (though they state that including them does not affect their results). Kahn (2010) also excludes controls for college graduation year when estimating the effects of entry conditions using national unemployment rates.

rate at the time of college entry – as this varies only by enrollment cohorts and would be subsumed by the cohort fixed effects. However, our focus is on the short term fluctuations in cohort outcomes that are systematically related to business cycle conditions at enrollment. Hence, we assume that the long-term component of cohort quality evolves over time in a smooth fashion, and we identify the wage effects of enrolling at different phases of the business cycle based on the deviations of cohort outcomes around this long-term cohort trend.

Our benchmark estimation takes on the following form:

$$w_{it} = \alpha + \beta U_{c_i} + f\left(a_{it}\right) + g\left(c_i\right) + \tau_t + \gamma x_{it} + \epsilon_{it}, \tag{1}$$

where w_{it} is the labor market outcome of individual i observed in year t (generally log real weekly earnings), α is a constant term, $f\left(a_{it}\right)$ is a function of the age of individual i at time t, $g\left(c_i\right)$ is the long-term trend in cohort quality, with c_i indicating the year of college enrollment for individual i, τ_t captures calendar year fixed effects (for the year in which the labor market outcome is observed), x_{it} is the remaining set of individual-specific characteristics and ϵ_{it} is a standard error term. We consider various specifications for the functions $f(.)$ and $g(.)$, including linear and quadratic parametrizations, as discussed in Section 3.1. β, the coefficient of interest, captures the impact of the unemployment rate in the year that individual i enrolled into college (U_{c_i}). Having controlled for long-term cohort trends (as well as age and calendar year effects), the identification of β is driven solely by cross-cohort differences in outcomes that are systematically related to the business cycle conditions experienced at the time of enrollment.

Our specification hinges on the assumptions that: (1) the unemployment rate at college entry only induces cohort-specific deviations from a long-term trend in cohort quality which evolves smoothly over time, and (2) the age profile of labor market outcomes is constant across cohorts (an assumption that is widespread in any standard specification of the Mincerian wage equation). Based on these assumptions and the variation in age at enrollment, we are able to identify our main coefficient of interest β.

Note that by capturing labor market experience through age (rather than years since graduation) we assume that the effect of an additional year of experience on earnings does not depend on whether it occurred prior to, or after completing college.[15] Moreover, although the inclusion of "non-standard" students (i.e. students

[15] Although we do have independent variation in years since graduation conditional on the current calendar year and the enrollment cohort, this variation is very limited, as it arises solely from the fact that some individuals enrolling in the same year graduate at different times due to differences in program length. Hence, we prefer to control for age rather than years since graduation in our analysis.

who enroll at later ages) is crucial in driving identification in our benchmark specification, in Section 3.2 we show that if we restrict attention to "standard" students and adopt an identification approach akin to what has been used in previous literature, we obtain very similar results. This group of "standard" students have exactly the same amount of (potential) experience after graduation. The fact that we obtain very similar results for this sample suggests that our key results are not driven by differential returns for labor market experience before vs after college.

Relying on the national unemployment rate limits the flexibility that we can allow for when modeling cohort effects. An alternative approach that is often used in the literature to control for cohort effects is to exploit within-cohort variation across geographical locations by using regional unemployment rates (e.g. Oreopoulos et al., 2012). In our context, we are limited in the extent to which we can exploit geographic variation within cohorts. Although we have information on individuals' place of birth, this is only available at a very aggregate level. Specifically, we only know in which of the four UK countries (England, Wales, Scotland or Northern Ireland) individuals were born (and only for the waves from 2001 onwards). While studies using US data are able to exploit variation across states, having within-cohort variation only across four regions in our context is quite restrictive. As shown in Panel B of Figure 1, the sub-national unemployment rates closely follow each other. Starting in the early 1990s between-country differences nearly disappear. This reduced within-cohort variation is an important disadvantage of using country-level unemployment rates.

We also have information on individuals' current region of residence (12 regions), which could provide additional variation. However, individuals' current region of residence is clearly an outcome (ex-post choice) variable, so the validity of assigning unemployment rates to individuals based on their current region of residence may be questionable.

In spite of these limitation, in Section 3.3 we consider the robustness of our results to a specification that allows for cohort fixed effects and where identification is achieved solely from within-cohort variation across locations (based on either UK country of birth or current region of residence). This specification is as follows:

$$w_{it} = \alpha + \beta U_{r_i c_i} + f\left(a_{it}\right) + \chi_{c_i} + \theta_{r_i} + \tau_t + \gamma x_{it} + \epsilon_{it}, \tag{2}$$

where r_i indicates individual i's UK country of birth or region of residence (depending on the specification), χ_{c_i} are a full set of cohort of enrollment fixed effects, and θ_{r_i} represent UK country or region of residence fixed effects. The other terms are analogous to those defined for Equation (1). The key difference relative to Equation (1) is the inclusion of cohort fixed effects, which limit identification of β, our coefficient of interest, to regional variation in unemployment rates within cohorts. Given that both sub-national specifications present important shortcomings, our preferred estimates are those based on national unemployment rates. The results based on Equation (2), however, allow us to show that our benchmark results are not particularly reliant on the functional form specification for the cohort effects.

3 Results: Unemployment at Enrollment and Wages

3.1 Benchmark Results

Our benchmark specification estimates Equation (1) for our main sample, using log real weekly wages as our outcome variable of interest. The additional control variables included in x_{it} are a race dummy, a dummy for foreign nationals, and a set of 19 region of residence dummies. Results for men are presented in the top panel of Table 2; results for women are in the bottom panel. In all cases, observations are weighted using person weights provided in the dataset, and standard errors are clustered by year of enrollment.[16]

Column (1) begins with a specification that allows for a linear cohort trend and a linear function of age. Columns (2) through (4) allow for more flexibility in the parametrization of the age and cohort effects. In particular, Column (2) allows for a quadratic function of age, while Column (3) allows for a quadratic cohort trend. In Column (4), we consider a specification with a linear cohort trend, plus a set of decade of enrollment dummies. The results in the top panel show that, allowing for non-linearities in either the age or the cohort effects leads to an estimated coefficient of around 0.006 for men. This implies that male cohorts enrolling in times when the unemployment rate is 1p.p. higher have wages that are on average 0.6% higher, after controlling for age effects, calendar year effects, and long-run changes in cohort quality. This estimated impact for men is not sensitive to the dimension of non-linearity chosen. The results for women, on the other hand, are sensitive to the type of non-linearity that is allowed for. While a specification with a linear cohort trend and a quadratic function of age yields a statistically significant coefficient of 0.007, specifications that allow for non-linearities in the cohort effects yield smaller and statistically insignificant coefficients.

Given the sensitivity of the results for women and the robustness of the results for men, and to be conservative in our estimated impacts, we consider the specification in Column (3), which allows for a quadratic cohort trend and a linear function of age as our preferred specification. To gain insight into the magnitude of the effect, consider an increase of 3p.p. in the unemployment rate at the time of enrollment

[16] In most specifications, we have a total of 58 clusters, which is above the rule-of-thumb threshold considered appropriate for reliable inference using a standard cluster adjustment (see e.g. Cameron et al., 2008; Angrist and Pischke, 2009).

– approximately one standard deviation in the sample. The estimated coefficient in Column (3) of Table 2 implies that male graduates who enrolled in college when unemployment was 3p.p. higher earn approximately 1.8% more on average. Given average real weekly gross wages in the sample of £890 (in 2015 pounds), this implies that male cohorts that enroll when unemployment is one standard deviation higher (and complete their degrees) can expect to earn roughly £16 more per week, or £830 more per year, for every working year.

Column (5) considers whether the result might be affected by changes in tuition fees. In 2006 and 2012 there were increases in tuition fees in the UK. This may have changed the patterns of selection into university, with implications for average wage levels across cohorts. The timing of the introduction of the fees could potentially be correlated with the business cycle. To account for this, in Column (5) we add a dummy for the 2005 and the 2011 enrollment cohorts (where the composition could differ in anticipation of the introduction of the new fees), while also allowing for discontinuities in the cohort trend in 2006 and 2012. The results show that allowing for these discontinuities in outcomes across cohorts due to tuition fees does not affect our main result.

In Column (6) we allow the impact of unemployment at enrollment to partly operate through the choice of enrolling in post-graduate education and/or through the choice of region of residence. We do this by removing these variables as controls. Removing these controls does not lead to any noticeable changes in the estimated coefficient of interest. Hence, the wage differential that we have identified does not appear to be driven by selection into postgraduate education or by the choice of region of residence.[17]

In the following sections we explore several channels that may explain the wage differential that we have identified. These include individuals' field of study choices and their academic achievements prior to and during university. Since this information is only available for individuals whose highest degree is an undergraduate and is not available in all years of the survey, Table 3 replicates our benchmark results for these relevant subsamples to provide benchmark results that are directly comparable with those obtained when exploring these different channels. First, in Columns (1) and (4) we exclude all individuals with a postgraduate degree. This serves as a further check of the robustness of our main result using the group for whom we are able to better infer the year of undergraduate enrollment. The results for men are nearly identical to those obtained for the full sample in Table 2. For women, we obtain a somewhat higher estimated coefficient which is statistically significant at the 10% level. Columns (2) and (5) further exclude a relatively small number of obser-

[17] Recall, however, that our procedure to impute the year of enrollment is much less accurate for individuals with postgraduate degrees, so we do not view this as conclusive evidence that there is no effect on postgraduate enrollment.

vations with missing information on the field of study at university, thus presenting our benchmark results using the sample from Panel B of Table 1. Columns (3) and (6) exclude individuals with missing information on their academic achievements at either the high school or the university level. This is the sample from Panel C of Table 1, which excludes individuals interviewed in LFS waves prior to the last quarter of 2005, when these questions were not asked. Once again the results for men remain very consistent, while for women the estimated coefficient becomes larger and statistically significant at the 5% level.

Overall, our results consistently indicate that the average wages of cohorts of male graduates who enroll into university when aggregate economic conditions are poor are higher than those of cohorts who enroll during better economic conditions. In the case of women, although no robust positive effect is found, there is also no indication that the average wages of cohorts who enroll during bad times are *worse* than those of cohorts who enroll during good times. This result is striking, given that enrollment into university tends to increase when macroeconomic conditions deteriorate, prompting us to expect worse selection in terms of quality for these cohorts as well as downward pressure on wages due to the higher supply of graduates. On the contrary, our results show that the cohorts that enroll during worse macroeconomic conditions end up performing better in the labor market. In Section 4 we explore various mechanisms that might account for this pattern, and in Section 5 we analyze whether the pattern is likely to be driven by changes in selection into higher education and/or in terms of which students graduate according to the business cycle conditions prevailing at enrollment, or by behavioral changes among students. Before this, we show that our results are not driven by non-standard students, we explore the robustness of our results to a set of specifications that only exploit within-cohort regional variation in unemployment rates, and we analyze how our results vary when we use different years to proxy economic conditions at enrollment.[18]

[18] As mentioned above, our imputation of the year of enrollment assumes that all individuals born in Scotland study in Scotland and hence complete their degrees over a four year period. Based on the analysis of restricted-use LFS data provided by the ONS we know that this assumption is incorrect for around 15% of the Scottish born sample. Hence, in Appendix Table A.1, we replicate our main results for the period from 2001 onwards (for which information on location of birth is available), and show that excluding the Scottish-born has no noticeable effect on our coefficient of interest. Note also that we would impute the wrong year of enrollment for students born in England, Wales or Northern Ireland if they studied in Scotland. However, the ONS analysis mentioned above shows that only around 2% of English and Welsh undergraduate degree holders obtained their degrees in Scotland, so this would only be of minor concern.

3.2 Excluding "Non-Standard" Students

The source of variation that enables us to identify the effect of unemployment at enrollment, while simultaneously controlling for age, cohort, and time effects is the fact that individuals enroll into college at different ages. The higher wage observed among male cohorts who enroll during times of higher unemployment may be reflecting differential selection of "non-standard" students (i.e. those who enroll into university at later ages) among these cohorts. In this section we explore whether this is driving our results, or whether similar patterns are observed when we focus only on "standard" students (i.e. those who who enroll at age 18 or 19).

The estimates presented in Table 4 reproduce our baseline estimation using only individuals for whom we can clearly infer from the data that they enrolled into college at age 18 or 19. We therefore exclude individuals with a postgraduate degree (for whom the imputation procedure is imprecise) as well as those that, based on their age at graduation, would have enrolled into college after some time in the labor market. In these regressions, year of enrollment is a perfect function of calendar year and age, so we cannot control for all three dimensions, and we exclude the cohort trend. Conversely, we allow for more flexibility in the age effects by controlling for a quadratic function of age. In Columns (1) and (3) of Table 4 we only consider individuals who enrolled at age 18. The results for this sub-sample are remarkably similar to our benchmark estimation in the case of men. For women, instead, we estimate a positive and significant effect even larger than the one obtained for their male counterparts. When we restrict the sample to individuals who enrolled at age 19 in Columns (2) and (4), we obtain a large and significant effect for males and a smaller and insignificant effect for females.

While excluding "non-standard" students from our benchmark specification provides useful insights, the omission of any cohort effects is very restrictive. As noted by Kwon et al. (2010), in a setting where cohort is a linear function of calendar year and age, the deviations of the cohort effects from a (non-identified) linear trend can still be identified by estimating a regression that controls for age, calendar year, and cohort fixed effects, where the first and last cohort effects are set to 0. To provide further evidence of the impact of economic conditions at enrollment among "standard" students, we implement this procedure using individuals who enroll either at age 18 or at age 19.

In Figure 2 we plot the cohort effects estimated with this methodology for the two sub-samples of individuals who enrolled at age 18 and at age 19, respectively, together with the unemployment rate for our period of interest. Looking at the results for men in Panel (a), we see that, even if we focus only on individuals who chose to enroll into college without delays, the correlation between the (detrended) cohort effect in wages and the unemployment rate at entry is positive, at 0.65 and 0.50 for the 18 and 19 year old group, respectively. In Panel (b) we see a positive correlation (0.61) between unemployment rates and cohort wage effects for female graduates who enrolled at 18. We calculate a much weaker correlation (0.27) for those who enrolled at 19.

Overall, we conclude that the positive correlation between unemployment at enrollment and cohort wages that we have identified for men is not (solely) driven by the inclusion of non-standard students in our analysis. For women we estimate a positive correlation for those who enroll at 18, but this correlation appears to be weaker or absent for those who enroll at older ages.

3.3 Using Variation Across UK Countries or Regions

As discussed in Section 2.3, we explore the robustness of our results to using a specification that controls for cohort fixed effects and exploits sub-national variation in unemployment rates (see Equation (2)). This specification presents some limitations, as discussed above, but allows us to check the extent to which our benchmark results are reliant on functional form assumptions about the cohort effects.

The results are presented in Figure 3. The figure plots the estimated coefficient on the unemployment rate at enrollment for males (top panel) and females (bottom panel) for five different specifications. In the left panel, for reference purposes, we reproduce the estimates from our benchmark model where we use the national unemployment rate and control for cohort effects through the inclusion of a quadratic term. In the middle panel we report two estimates where unemployment rates are assigned to individuals based on their UK country of birth. The first estimate, represented by a dark circle, is based on a specification that includes country of birth fixed effects and a quadratic cohort trend. For the second estimate, represented by a gray square, we replace the quadratic term with a full set of cohort fixed effects, thus exploiting only within-cohort variation in economic conditions across UK countries. In the rightmost panel, we assign unemployment rates to individuals based on their more disaggregated current region of residence. Once again we show estimates from two specifications, both of which include region of residence fixed effects. The first one controls for a quadratic cohort trend, and the second one replaces this term with a full set of cohort fixed effects.

The results displayed in Figure 3 show a remarkable stability of our coefficient of interest. Introducing cohort fixed effects has almost no effect on the size of the estimated impact of unemployment at enrollment for men, though the estimates become less precise as indicated by the widening of the confidence intervals. This is not surprising given the limited amount of within-cohort variation that these estimates rely upon.

The results for women are also quite stable across specifications. One notable difference is with regards to the coefficient estimated in the regional specification with cohort fixed effects reported in the bottom right panel of Figure 3. In this specification, the effect of the unemployment rate at enrollment on wages is positive and statistically significant at the 5% level.

Overall, despite the severe limitations of the estimation based on local unemployment rates (as discussed in Section 2.3), the results of these robustness checks offer an indication that our benchmark results are not primarily driven by the functional form assumptions on cohort trends that we have adopted.

3.4 Changing the Relevant Unemployment Rate

Our benchmark specifications use the unemployment rate in the (imputed) year of enrollment and show that conditions at enrollment are relevant for future wages. In this section we inspect whether labor market performance is also influenced by the unemployment rates around the enrollment year.

This is an important check for at least two reasons. First, by focusing on the years around the year of enrollment we can get a sense of when macroeconomic conditions start shaping future outcomes. Focusing on the macroeconomic conditions in the enrollment year seems the most natural choice, but it is not *a priori* clear at which point economic conditions affect college entrants the most as different behavioral mechanisms may affect people at different ages (as we discuss further in Section 5.3). Second, this is an important check for the plausibility of our suggested link between macroeconomic conditions at enrollment and future outcomes. Should we find that unemployment rates in years away from college entry matter for future wages, we might be concerned that the effect that we find is driven by factors other than the ones we suggest.

We perform these tests by re-estimating our benchmark model, replacing the unemployment rate in the year of (imputed) enrollment (t_0) with the national unemployment rate in years before or after college entry. In Figure 4 we show the estimates of our coefficient of interest for men and women when varying the unemployment rate from t_{-7} to t_{+7} (i.e. up to seven years before/after enrollment).

A general pattern emerges from this figure: For both men and women the strongest effects on wages are found for the years immediately before enrollment. For men we estimate the largest coefficient at t_{-2}, and for women at t_{-3}. The coefficients move smoothly around these peaks; coefficients in contiguous years are similar, but converge towards zero as we move to years further before or after enrollment. Quite reassuringly, unemployment rates that should be irrelevant for college graduates' labor market outcomes are in fact irrelevant.

The finding that the estimated coefficients on unemployment before enrollment are systematically larger than the estimated coefficients on unemployment in the year of enrollment or in the years after enrollment provides suggestive evidence that conditions *before* enrollment matter more for future wages than conditions during individuals' undergraduate studies. The differences between the estimated coefficients, however, are generally not statistically significant. The results should also be interpreted with caution given that, as discussed in Section 2.2.1, our procedure to impute the year of enrollment may assign an incorrect year, and consequently an incorrect unemployment rate, to some students (in particular, individuals with postgraduate degrees and those who took longer than the standard program length to complete their degrees). As we expect this measurement error to stay mostly within a close neighborhood around the true enrollment year, it may not be particularly surprising that the effect does not disappear sharply when moving away from the imputed enrollment year. Moreover, the inherent autocorrelation of unemployment data makes it difficult to precisely isolate how the impact varies over time.

An additional implication of the results displayed in Figure 4 is that, if we were to use the years immediately before enrollment instead of the year of enrollment in our benchmark specification for women, we would find a positive and statistically significant effect of unemployment on wages also for this sample.

4 Potential Channels

In this section we explore four potential channels that could explain the wage differences documented above: selection into employment, variation in economic conditions at the time of graduation, changing selection into different fields of study, and changing selection into occupations or industries.

4.1 Selection into Employment

One potential explanation for the cross-cohort wage differences that we have identified could be selection into employment. If cohorts who enroll during worse economic conditions have lower employment probabilities, the subset of full-time workers from these cohorts might be more positively selected than full-time workers from cohorts who enroll into university during better aggregate conditions. We explore this possibility in Table 5. Columns (1) through (3) estimate a series of linear probability models using the same set of controls as our baseline specification from Column (3) of Table 2. The outcome variable in Column (1) is the probability of full-time employment (i.e. the probability of being in our sample from Table 2), while the outcome variables in Columns (2) and (3) are the probability of part-time employment and non-employment, respectively. On average, the probability of full-time employment in our sample is 81% for men and 58% for women. The probability of part-time employment is 5% for men and 23% for women.

The results in the top panel of Table 5 in Columns (1)-(3) show that there is no statistically significant relationship between aggregate conditions at the time of university enrollment and the probability of working (either full time or part time) for men. Although positive, the estimated coefficient for the probability of full time employment is quite small and not statistically significant. For women, worse economic conditions at the time of enrollment are associated with slightly lower probabilities of working full time. This is driven by an increase in the probability of part-time employment.

Column (4) of Table 5 shows the result from a regression analogous to our baseline specification from Column (3) of Table 2, but where we use hourly, rather than weekly, wages, and where we include both full-time and part-time workers. In line with our baseline results, we estimate a positive and significant coefficient for men, and no correlation for women.

Finally, in Column (5) of Table 5 we once again estimate a regression analogous to our baseline specification from Column (3) of Table 2, but where we now use weekly earnings in levels (rather than logs), and include part-time workers and individuals with zero earnings along with the full-time workers from our baseline sample. The results confirm the strong and statistically significant positive correlation between unemployment at enrollment and earnings for men, and the much weaker relationship for women.

Based on this analysis, we conclude that, although it is possible that the pool of women in full-time employment is more positively selected among those who enroll in university during downturns, in the case of men, where we see much stronger evidence of positive wage effects, there is no evidence that this is driven by differential selection into full-time employment.

4.2 Economic Conditions at Time of Graduation

There is strong evidence in the literature that economic conditions at the time of graduation have large and long-lasting effects on labor market outcomes for university graduates (Kahn, 2010; Oreopoulos et al., 2012; Altonji et al., 2016b; Liu et al., 2016).[19] Our finding regarding differences in average cohort-level wages could potentially arise if cohorts that enroll in bad times tend to graduate in good times, and hence avoid these negative graduation effects. This would be the case if the unemployment rate at enrollment were negatively correlated with the unemployment rate

[19] See also Cribb et al. (2017) and Schwandt and von Wachter (2019), who show that adverse effects are also observed for workers without a college degree.

at graduation.

In Figure 5 we plot the correlation between detrended unemployment rates at the time of enrollment (college entry) and at the time of graduation (college exit) experienced by each cohort (detrended using a quadratic trend in both cases). The two unemployment rates are clearly positively correlated, although there is also independent variation in the two. Overall, the positive correlation implies that individuals who enroll in bad times tend to, on average, also graduate during relatively bad times, which would go against the intuition described above.

To investigate this channel more formally, we re-estimate our benchmark specification, but add the unemployment rate at graduation as an additional control. The results are presented in Column (1) of Table 6. Remarkably, the estimated coefficient on the unemployment rate at enrollment is very similar to the one from our benchmark specification, while the estimated coefficient on the unemployment rate at graduation is statistically insignificant. The puzzling lack of a negative impact of the unemployment rate at graduation on wages seems to be driven by the fact that the evidence for scarring effects in the UK among university graduates is not robust to the inclusion of cohort trends. This result is not inconsistent with the evidence in the existing literature for the UK and is discussed in more detail in Appendix A.[20]

4.3 Major Choice

There is recent evidence in the literature suggesting that economic conditions at the time of enrollment have an impact on students' field of study preferences and choices (Bradley, 2012; Goulas and Megalokonomou, 2019; Blom et al., 2015). It is also well known that wages vary substantially across majors (e.g. Altonji et al., 2012; Lemieux, 2014; Altonji et al., 2016a). Therefore, a potential explanation for the wage differences that we have documented would be that students who enroll when economic conditions are poorer tend to select into higher paying majors, thus increasing average wages at the cohort level.

To explore evidence for this mechanism, we proceed in two stages. First, we analyze whether we observe changes in field of study choices over the business cycle in our dataset. To the best of our knowledge, this is the first paper to explore the effects of the business cycle on the composition of majors in the UK. Then, we return to our wage regression to determine whether changes in the field of study composition across cohorts can account for the differences in wages. This section uses the sample in Panel B of Table 1, which excludes individuals with postgraduate degrees given that their undergraduate major choice is not available for all years in our data

[20]We thank Jonathan Cribb and Robert Joyce for a discussion of the (lack of) evidence of scarring effects in the UK.

To determine whether the composition of fields of study varies according to the business cycle, we estimate a series of linear probability models of students' major choices. The models are estimated separately for each major category. As individual controls we include race, nationality and region of residence. To account for long-run changes in the composition of majors across cohorts, we allow for a quadratic cohort trend in the enrollment probability into each major. As before, our regressor of interest is the unemployment rate in the year of enrollment.

We plot the results for the estimated coefficients on the unemployment rate at the time of enrollment in Figure 6. Our estimates suggest that in periods of higher unemployment, more students (both male and female) select into Engineering. Men tend to select out of Business, Law and Information and Communication Technologies, whereas women tend to move away from Health and Welfare fields. The estimated effects, however, are small. Our estimates imply that a 5 percentage point increase in the national unemployment rate – a historical swing, only experienced twice in the last 55 years in the UK – would increase the share of graduates in Engineering, the most responsive category, by around 4.5 percentage points for men and only 1 percentage point for women – a rather modest change.[21] Moreover, although Engineering – which is clearly a high-paying field – grows in recessions, other high-paying fields such as Business and Law, for men, and Health, for women, tend to shrink. It is also not obvious what the changes in field of study composition would imply for cohort-level wages, given that marginal students who change their field of study decisions due to the business cycle might not earn wages that are similar to the average wages in their new field of choice.

To determine more directly whether changes in field of study composition explain the differences in wages across cohorts, we return to our benchmark wage regression and add controls for fields of study. The relevant benchmark results for this sample are in Columns (2) and (4) of Table 3. Column (2) of Table 6 replaces the simple calendar year fixed effects with fully interacted field of study-calendar year fixed effects. This controls for changes in the return to different fields and limits identification to variation within field-year cells. To the extent that the effect of unemployment on wages that we were finding was due to differences in field of study composition and the differences in rewards across fields, these new fixed effects should eliminate the effect that we find. Interestingly, adding these field-specific calendar year fixed effects has no noticeable effect on our result of interest, implying that the wage differentials are observed *within* fields of study.

[21] This contrasts with the results for the US in Blom et al. (2015), which show a much stronger responsiveness of major composition to the business cycle, and is likely due to the fact that selection of majors is much more rigid in the UK system, where students' choices are more limited by their course of study during their A-levels. It may also reflect less flexibility at the departmental level to change enrollment as a response to changes in application volumes.

Appendix Table A.4 explores whether the positive wage effect is concentrated within certain fields of study. We show results for our benchmark specification, but where unemployment at enrollment is interacted with a full set of field of study dummies.[22] The results show widespread positive effects for men across most fields, the main exception being Health and Welfare. Interestingly, statistically significant positive wage effects are observed in a number of high-paying fields, including Natural Sciences, Mathematics and Statistics (for both men and women), and Engineering (for men). This implies that the cohorts selecting into these highly remunerated fields when aggregate economic conditions deteriorate have higher average wages than those who select into these fields in better times. Again, this contrasts with our expectation that a field like Engineering would attract lower quality marginal students as it tends to expand in response to worsening aggregate economic conditions.

Overall, the results provide robust evidence that the increase in wages observed for cohorts who select into college during worse economic conditions is *not* driven by reallocation across fields of study. Instead, there appears to be an improvement in cohort quality among male college graduates within many fields, including high-paying fields such as Natural Sciences, Mathematics and Statistics, and Engineering.

4.4 Occupation and Industry Sorting

The wage differential that we have found for cohorts who enroll in university during worse macroeconomic conditions could be, to some extent, driven by differential sorting into higher paying occupations or industries. For example, Liu et al. (2016) show that business cycle conditions at graduation have important implications for the quality of graduates' initial industry match, and this can explain some of the persistent wage losses from graduating in a recession.

Here we explore if differences in the occupation and industry composition of different cohorts can explain the wage differences that we have identified. We do this by adding a set of controls for occupations and industries and determining the extent to which the coefficient on unemployment at enrollment is reduced.

In Column (3) of Table 6 we add a set of nine broad occupation dummies, interacted with calendar year. This accounts for variation in the return to different occupations over time.[23] The coefficient on unemployment at enrollment is still statistically significant for men, implying that cohorts who enroll into university during periods of higher unemployment have higher wages, even within occupations. Comparing it to our benchmark coefficient in Column (3) of Table 2, the additional controls

[22] Age effects and calendar year effects are also allowed to vary by field of study in this specification.

[23] Having these occupation-time interactions also implies that we do not need to be concerned about changes in the occupational coding schemes over time, given that identification is solely within occupation-year cells.

reduce the coefficient by around one-third, implying that part of the cohort-level wage differences are due to differences in selection into different occupations, though only to a limited extent.

In Column (4) we instead include a full set of occupation fixed effects at the most detailed level available in the LFS.[24] This reduces the estimated coefficient by half, relative to our benchmark specification.

Columns (5) and (6) of Table 6 repeat the analysis using broad and detailed industry categories, instead of occupation groups. The results are similar with regards to industry sorting: more than half of the wage differential for men is observed within industries.

5 Understanding the Wage Differences: Ex-Ante Selection or Increased Effort?

Our results so far provide strong evidence of ex-post differences in the average quality (unobserved ability) of male cohorts of graduates that enrolled at different points in the business cycle. In this section, we explore whether this can be explained by better selection among graduates who enrolled during poor economic conditions – implying that these cohorts are of better quality *ex-ante* (i.e. in terms of their pre-university achievements) – or whether the differences in quality only arise *ex-post*.

Carneiro et al. (2011) and Carneiro and Lee (2011) show that increases in college enrollment in the US between 1960 and 2000 led to a decline in the average quality of college graduates. A similar logic would lead us to expect that the expansion of enrollment that occurs during bad times would be associated with a lowering of the average cohort-level ability. The additional marginal students who enroll in bad times may not be as well-suited for higher education and may even negatively impact the achievements of their peers. There may, however, be differential selection in terms of which students graduate. If dropout rates are higher among men who enroll in bad times, then the pool of graduates from these cohorts may indeed be more positively selected. Evidence from the US, for example, shows that expansions in enrollment rates are not necessarily matched by increases in graduation rates, particularly among lower ability students (Bound et al., 2010).

While average cohort quality is not directly observable, our data includes information on school performance before college entry and during college. This can be used to shed some light on how the observed ex-ante and ex-post cohort ability of graduates varies with the economic conditions at entry. The two measures of educa-

[24] We include a separate set of fixed effects for the codes from each of the three coding schemes used over time in the data.

tional performance that we will exploit are: (i) the number of GCSE exams passed in high school, an *ex-ante* measure of performance providing us with an indication of the average level of graduates' cohort quality at the time of college entry;[25] and (ii) the "degree class" achieved in university, which is a function of students' Grade Point Average, providing us with an indication of the same cohort's *ex-post* quality as they exit college and enter the labor market. For the analysis in this section, we focus on the sample from Panel C of Table 1 which, as mentioned earlier, excludes individuals with postgraduate degrees as well as individuals surveyed prior to the last quarter of 2005. The relevant benchmark results for this sample are in Columns (3) and (6) of Table 3 for men and women, respectively.

5.1 Ex-Ante Selection: Academic Performance in High School

We begin by analyzing how the average *ex-ante* quality of cohorts varies, by determining whether graduates who enrolled at different points in the business cycle differ in terms of the number of GCSEs that they passed in high school. The LFS records the number of GCSEs passed with a grade of C or above using the following interval categories: one to two, three to four, five to seven, or more than eight. We construct a continuous measure using the mid-points of each interval (where we assign a value of nine for the "more then eight" category), and we also present results based on linear probability models, where we use a dummy for each of the possible intervals as the dependent variable.[26]

The regression results are presented in Table 7. Columns (1) through (5) restrict attention to the sample used in our wage regressions (full-time working individuals), while Column (6) shows results for the broader sample that includes those who are working part time as well as those who are not working. In all specifications, we allow for a quadratic trend in the outcome variable across cohorts, which controls for long-term patterns in GCSE achievement levels. All regressions also include individual-level controls for race, nationality, and region of residence at the time of the survey. Columns (1) and (6) use the continuous measure of GCSE achievement as the dependent variable, while the remaining columns show results based on the linear probability regressions for each possible outcome.

The estimates reveal that graduates who entered college in high unemployment

[25] A variable recording the number of A-levels, another measure of pre-university achievements, is also available in the LFS, but it has very limited granularity, only recording whether the individual has zero, or one or more A-levels. Given that a key prerequisite for university admission is the number of A-levels, this variable presents almost no variation in our sample.

[26] GCSE exams were introduced in 1988, replacing the O-level exams in England, Wales and Northern Ireland. For individuals who finished high school before 1988, the LFS records the number of O-level exams passed with a grade of C or above.

years have on average passed *fewer* GCSE exams than those entering in boom periods. This is driven by a lower probability of having passed eight or more GCSEs for both men and women, as seen in Column (5), though the difference is not statistically significant at conventional levels in the case of men. Comparing the estimated coefficients in Columns (1) and (6), we see that the somewhat lower high school achievement for graduates that enrolled in bad times is observed for the cohort as a whole, as well as for those in full-time employment. In the case of women, the evidence of worse GCSE outcomes for cohorts enrolling in periods of higher unemployment is stronger when we focus on those in full-time employment, rather than the cohort as a whole.

Overall, our estimates suggest that the average ex-ante quality of graduates who entered college during periods of higher unemployment is, if anything, lower than the average quality of those who entered during periods of lower unemployment. This is consistent with evidence on selection into education worsening as cohort size expands, and rules out the possibility that differential dropout rates lead to a more positively selected group of graduates among cohorts enrolling in bad times. Hence, the positive wage effects that we find for these cohorts cannot be explained by more positive selection in terms of their *ex-ante* academic achievements.[27] We next explore whether the positive wage effects can be explained by better average quality at the time of university exit by analyzing cohorts' academic performance in college.

5.2 Ex-Post Quality: Academic Performance in University

To analyze ex-post cohort quality we focus on individuals' degree classifications obtained in university which, as mentioned in Section 2.1, are a function of students' university grades. The UK uses a system of external examiners with the aim of standardizing degree classifications and making them comparable across UK universities. As with the GCSE variable, we perform our analysis using a continuous measure which ranges from one to five based on the five degree class categories, where one corresponds to the lowest GPA outcome ("pass") and five to the highest ("first class"). In the Appendix, we present results based on linear probability models for each of the possible degree class outcomes. As before, we allow for quadratic cohort trends in the outcome variable. These cohort trends capture overall trends in the quality of university students and/or in "grade inflation" patterns.

The results based on the continuous degree class measure are presented in Table 8. All specifications include individual-level controls for race, nationality, and region of residence at the time of the survey and, as in all of our specifications, standard errors are clustered by year of enrollment. The results in Column (1) indicate that

[27]We also check whether graduates who enrolled in periods of high vs. low unemployment differ in terms of observable characteristics and find no strong evidence of any meaningful differences.

male graduates who enroll during times of higher unemployment have, on average, higher GPAs. This provides a first piece of evidence supporting the idea that these cohorts end up being of better quality by the end of their studies.

The specification in Column (1) does not control for the fact that some students return to university at older ages. Since older students might be more mature and/or motivated to pursue their studies, in Column (2) we add a control for age at graduation. Our coefficient of interest remains essentially unchanged. As the grade distribution is likely to differ across college majors, in Column (3) we add field of study fixed effects. This would control for the possibility that individuals who decide to enroll into college in times of higher unemployment might select majors where higher grades are easier to achieve. Adding field fixed effects, however, has no impact on our main coefficient (consistent with our earlier finding of limited reallocation across fields over the business cycle). In Column (5) we add a full set of dummies for the number of GCSEs passed in high school (in intervaled categories, as discussed in the previous subsection). Not surprisingly, given the evidence that cohorts who enroll during periods of higher unemployment are not very different in terms of their GCSE achievements, controlling for *ex-ante* achievement measures has little effect on the *ex-post* achievement gap across cohorts.

In the case of women, we also see that those who enroll during periods of higher unemployment obtain higher grade point averages at graduation, though the difference is not statistically significant when focusing on women in full-time employment. Importantly, this result rules out the possibility that the improved grade outcomes for men occur at the expense of lower grade outcomes for women. Instead, the grade distribution shifts up for the entire cohort of graduates enrolling in bad times.[28]

We now return to our wage regressions to determine whether the higher average wages for men who enroll during periods of higher unemployment are explained by their better academic performance in university. Recall that the relevant benchmark results for this sample are in Columns (3) and (6) of Table 3 for men and women, respectively.

In Columns (1) and (4) of Table 9 we add controls for the number of GCSEs. The significant positive coefficient on the GCSE variable confirms that high school performance is a relevant wage determinant. However, given that graduates that enroll at different points in the business cycle do not differ much in terms of their GCSE outcomes, the coefficient on unemployment at enrollment remains approximately the

[28]Appendix Table A.5 shows the results obtained from running separate linear probability regressions for each possible degree class outcome. Male graduates that enrolled during times of higher unemployment are significantly more likely to obtain a first class degree, everything else equal, and significantly less likely to obtain a lower second class degree. Once again we confirm that these outcomes are not at the expense of women, as female graduates are also more likely to obtain first class degrees (though the coefficient is not statistically significant).

same as in the benchmark specification. In Columns (2) and (5) we add controls for degree classification, in the form of a full set of degree class fixed effects. The estimated coefficient on the unemployment rate at enrollment for men declines by about one-sixth. In Columns (3) and (6) we replace the simple degree class fixed effects with fully interacted degree class and calendar year fixed effects. This allows the return to different degree classes to vary over time. Our coefficient of interest for men declines by an additional one-sixth. For women, the coefficient remains approximately the same as in the benchmark specification.

These results imply that the increased attainment in terms of degree classification only partially accounts for the wage differences across graduates enrolling at different points in the business cycle. Specifically, increases in academic achievement account for approximately one-third of the estimated impact for men. Hence, even conditional on degree class, graduates who enroll into university during times of higher unemployment earn higher wages. The higher average wages of these cohorts therefore seem to be driven both by skills that are reflected in academic ability as well as by further unobservable skills not captured by academic ability. Moreover, these higher skills are observed for cohorts of individuals whose academic performance in earlier years (prior to college entry) is at best equal to, and possibly worse, than that of cohorts enrolling in periods of economic expansion.

5.3 Discussion: Increased Effort

Our findings suggest that male graduates who entered university during poor economic conditions more than compensate for their initial lower quality and obtain higher grades in university, and also earn higher wages conditional on their grades. Though less robust, the evidence for female graduates points in the same direction. What could account for this improvement in cohort quality that arises during their university years? One possibility is that the quality of education improves during downturns. This could occur if there is an improvement in instructor quality due to changes in selection into (or retention in) higher education teaching following periods of high unemployment. Böhm and Watzinger (2015), for example, find an improvement in selection into academia among PhD economists graduating in a recession.[29] On the other hand, there is evidence that government expenditures on education decrease during periods of high unemployment. Data on expenditures on tertiary education in the UK over the period 1971–2015 from UNESCO shows that the correlation between the expenditures and the national unemployment rate is -0.13 with a p-value of 0.43, implying that there is no statistically significant relationship between the two variables. If anything, the UK government tends to invest less into

[29] See also Nagler et al. (2020), who find evidence of higher effectiveness among primary school teachers who select into the teaching profession during downturns.

tertiary education during recessions, not more. Kane et al. (2005) and Barr and Turner (2013) also find evidence of declining public expenditures on education during downturns in the US. These funding reductions might offset any potential gains derived from changes in instructor quality.

Given the lack of clear evidence pointing towards an improvement in the quality of education during downturns, we interpret our results as indicating that there is an increase in the effort that graduates who enroll during bad times exert during their university studies. Effort adjustments in response to adverse economic conditions have also been observed in other contexts. Lazear et al. (2016) find that the Great Recession induced US workers to exert more effort and that this increased effort explains most of the gains in productivity experienced by US firms in that period. The results in Blom et al. (2015), which show that students in the US who enroll during worse economic times pursue more challenging majors, can also be interpreted as reflecting increased effort during downturns among university students in the US. Given the institutional features that limit students' ability to change majors in the UK, our findings suggest that while the increased effort among US students enrolling during adverse economic conditions operates through major changes, the analogous adjustment in effort among UK students operates within, rather than between majors.[30]

Why would cohorts who enroll during periods of higher unemployment exert more effort during their studies and in the labor market? We believe that there are at least three potential explanations.

1. Increased competitive pressure – The fact that university enrollment tends to be countercyclical implies that individuals who enroll in university during times of higher unemployment would be part of larger cohorts. This would mean that in order to excel in class – particularly if grading is to some extent done on a curve – students would have to exert extra effort. This extra effort could translate both into higher grades, and even if not reflected entirely in their grades, in higher human capital accumulation that is later reflected in the form of higher wages, conditional on university grades. Evidence of effort adjustments in response to changes in cohort size is provided in a different context by Morin (2015), who exploits a natural experiment that exogenously led to a substantial increase in the size of an enrollment cohort at Ontario universities, and shows evidence of an increase in the relative effort exerted by male students as a reaction to increased competition for university grades.[31]

[30]Effort adjustments during downturns have been documented in very different contexts by Mukoyama et al. (2018), who find that search effort increases during downturns, and Griffith et al. (2016), who find that individuals adjust their food expenditures while maintaining similar nutrition levels by increasing their shopping effort during the Great Recession.

[31]Roth (2017) finds that apprenticeship graduates who are part of larger cohorts in Germany are able to find jobs faster, without compromising the quality of the jobs that they take. A number of papers in the literature instead find that overall cohort sizes tend to be correlated with worse labor

2. Increased focus on academic achievement due to lower employment opportunities – Another reason why effort may increase for cohorts who enroll during poorer economic conditions is reduced opportunities for (part-time) employment during their studies, freeing up larger proportion of their time to be dedicated to their academic activities. Using data from the American Time Use Surveys, Kalenkoski and Pabilonia (2012) show that working time crowds out time devoted to school-related tasks among high school students. Stinebrickner and Stinebrickner (2004, 2008) show that study time has an important effect on grades and other educational outcomes, while Darolia (2014) and Neyt et al. (2019) find that working has a negative impact on students' academic performance.

To explore whether this mechanism might be at play, we focus on individuals who are surveyed in the LFS while they are full time students, and test whether poor labor market conditions are correlated with lower employment rates among this group. The sample used for this set of regressions differs from that used in the previous analysis as here we look at full-time university students who are observed in the survey while still at university. In the absence of retrospective information on labor market participation, we are forced to restrict our analysis to the period 1998-2019 directly covered by the LFS. The results of this analysis are presented in Appendix Table A.6. We find that a 1p.p. increase in the current unemployment rate is associated with a 1p.p. increase in the probability that a student is not working in the case of men. Interestingly for women we do not find an effect. These results suggest that cohorts of men, but not women, enrolling in a trough might indeed dedicate more of their time to their education, as it is harder for them to find a part-time job while studying.

3. Changing attitudes – The experience of reaching early adulthood during a time of poor macroeconomic conditions may have a direct impact on the attitudes of individuals who select into college during bad times. This interpretation is consistent with a social psychology hypothesis known as the "impressionable years hypothesis" (Krosnick and Alwin, 1989), which suggests that core attitudes, beliefs, and values crystallize during early adulthood. This hypothesis has already proven useful for explaining differences across cohorts in preferences for redistribution and risk attitudes (Giuliano and Spilimbergo, 2014; Malmendier and Nagel, 2011; Shigeoka, 2019), and in explaining how individuals form expectations about inflation (Malmendier and Nagel, 2016). Following a similar logic, we hypothesize that individuals who select into college during bad times may be particularly susceptible to concerns regarding economic outcomes, and for this reason may be particularly motivated to excel in their studies. The higher grades and wages that we identify for graduates from these cohorts would be consistent with a change in their educational attitudes due to their experience of poor economic conditions during their key impressionable years. It may

market outcomes, mainly attributed to the saturation of the labor market; see for example Welch (1979); Berger (1985); Wright (1991); Brunello (2010); Agarwal et al. (2017).

also induce them to compete for and select into better-paying occupations, consistent with our findings.

While it would be tempting to further explore the evidence for this type of channel using data on high-school graduates who decide not to enroll into college (as they would also be impacted through the experience of poor economic conditions during early adulthood), this type of analysis would be challenging. For individuals who choose to enter the labor market directly after high school, the labor market conditions that they experience during their late teenage years (which might cause an impression on their attitudes) would be the same conditions that they experience when entering the labor market. It would therefore not be possible to separately identify business cycle impacts due to potential changes in attitudes from the impact of the conditions at entry among this sample.

Overall, although we are unable to provide direct evidence of a change in effort among graduates who enrolled during bad times, we believe that there are several pieces of evidence which make this interpretation quite plausible. It is also worth noting that our analysis on the impact of unemployment rates around the enrollment year discussed in Section 3.4 suggests that unemployment rates before enrollment have slightly stronger effects than unemployment rates during individuals' studies. While not conclusive, this suggests that arguments along the lines of increased competition or the impressionable years hypothesis may be somewhat stronger than those related to decreased employment opportunities during college. It is also worth noting that Morin (2015) finds that increased competition increases relative effort exerted by male students, which would be consistent with our finding of stronger effects among male graduates.

Our finding of increased effort has at least two crucial implications for the literature. First, variables such as average wages or unemployment rates, often at the local level, have been widely used as instruments for schooling (Cameron and Heckman, 1998; Cameron and Taber, 2004; Carneiro et al., 2011, 2013), given their potential impact on the opportunity cost of education. Our results imply that unemployment at choice affects later wages by inducing increased effort during university among those who choose to enroll (and complete their studies). These individuals would be the "compliers" in the instrumental variable (IV) setting. As IV strategies identify Local Average Treatment Effects (LATE) for the compliers, the estimated effect would include the effort boost, which would be absent for other cohorts. As a result, generalizing the estimates of the returns to schooling obtained from this IV strategy for the broader population would seem problematic.

Second, by underscoring the importance of effort on later outcomes, we contribute to the debate on whether obtaining an educational degree increases individuals' human capital or merely serves as a signal of their underlying innate (predetermined) ability – a persistent debate in the literature on the returns to education (Groot and Oosterbeek, 1994; Weiss, 1995; Chevalier et al., 2004). Our results provide supportive evidence for the interpretation of education as enhancing human capital. A signaling model would be able to rationalize our results only if employers would interpret the choice of enrolling into tertiary education in a bad economy as a signal of higher ability, which seems highly unlikely.

6 Conclusions

Economic downturns tend to attract additional students into higher education. Economic intuition would suggest that these marginal students would reduce the average wage outcomes among graduates who enroll during bad times, unless there are differential changes in dropout rates leading to improved selection among graduates from these cohorts. Our findings, based on UK data, show that male graduates who enrolled in college during periods of higher unemployment earn significantly higher wages ex-post, compared to graduates who enrolled during periods of lower unemployment. These cohorts of graduates are not more positively selected in terms of their high-school outcomes, implying that the marginal "enroller effect" dominates any other potential effect from differential dropout rates. We find evidence that suggests that there is a genuine improvement, during their college years, in the quality of graduates who select into college during adverse macroeconomic times. This is reflected in better college degree attainment, increased sorting towards higher paying occupations and industries, and higher wages conditional on occupation, industry, or GPA. While our findings are stronger for male graduates, there is no evidence that men's gains are at the expense of worse outcomes for women: The results for female graduates are less robust but point in the same direction.

We interpret our findings as reflecting an increase in effort among graduates who enroll in times of higher unemployment. Although the specific drivers of this increase in effort merit further investigation, we hypothesize that this may be due to increased competition, reduced opportunities for part-time employment, or changes in attitudes consistent with the impressionable years hypothesis from social psychology. Devising empirical strategies to identify these different channels would be a promising avenue for future research.

Regardless of the driving force behind the improvement in the academic and labor market outcomes for those who start higher education in bad economic times, our findings send a clear signal to policymakers that it is not a good idea to limit funding for education or curb enrollment to tertiary-level education during a recession.

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Table 1: Summary Statistics

Panel A: Main Sample	*Men*		*Women*	
Average Log Real Weekly Earnings	6.68	(0.51)	6.45	(0.47)
Share White	0.92	(0.27)	0.91	(0.28)
Share Foreign	0.05	(0.21)	0.06	(0.24)
Average Age	40.67	(9.84)	38.61	(9.77)
Share with Postgraduate Degree	0.35	(0.48)	0.36	(0.48)
Average Unemployment at Enrollment	7.20	(2.76)	7.24	(2.52)
Enrollment Decade (Shares):				
1950s & 1960s	0.07	(0.25)	0.04	(0.19)
1970s	0.19	(0.39)	0.14	(0.34)
1980s	0.27	(0.44)	0.23	(0.42)
1990s	0.31	(0.46)	0.35	(0.48)
2000s	0.15	(0.36)	0.22	(0.41)
2010s	0.02	(0.13)	0.03	(0.16)
Obs.	96,543		76,784	
Panel B: Field of Study Sample	*Men*		*Women*	
Health & Welfare	0.04	(0.20)	0.14	(0.35)
Soc. Sci., Journ. and Info.	0.12	(0.32)	0.15	(0.36)
Business, Admin. & Law	0.18	(0.38)	0.18	(0.39)
Arts & Humanities	0.15	(0.36)	0.23	(0.42)
Education	0.02	(0.14)	0.09	(0.29)
Nat. Sci., Maths & Stat.	0.20	(0.40)	0.15	(0.36)
Veterinary & Agriculture	0.01	(0.11)	0.01	(0.11)
Info & Comm. Tech.	0.07	(0.25)	0.01	(0.11)
Engineering & Techn.	0.21	(0.41)	0.03	(0.16)
Obs.	60,439		46,605	
Panel C: Academic Achievements Sample	*Men*		*Women*	
Number of GCSEs:				
1 or 2	0.02	(0.13)	0.02	(0.13)
3 or 4	0.04	(0.20)	0.04	(0.20)
5 to 7	0.24	(0.43)	0.22	(0.41)
8 or more	0.70	(0.46)	0.72	(0.45)
Degree Classification:				
Ordinary	0.06	(0.24)	0.06	(0.24)
Third	0.06	(0.24)	0.03	(0.16)
Lower Second	0.33	(0.47)	0.28	(0.45)
Upper Second	0.44	(0.50)	0.52	(0.50)
First	0.11	(0.31)	0.11	(0.31)
Obs.	34,842		28,325	

Note: All statistics are weighted using person weights from the LFS. Standard deviations in parenthesis.

Table 2: Wages and Economic Conditions at Time of College Enrollment

	Outcome: Log real weekly wages					
	(1)	(2)	(3)	(4)	(5)	(6)
Men:						
Unemp at enrollment	0.022***	0.006***	0.006***	0.007**	0.006***	0.006***
	(0.002)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)
Obs.	96,543	96,543	96,543	96,543	96,543	96,543
R^2	0.162	0.195	0.169	0.167	0.169	0.123
Nr. of Clusters	58	58	58	58	58	58
Women:						
Unemp at enrollment	0.016***	0.007***	0.002	0.002	0.001	0.002
	(0.002)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)
Obs.	76,784	76,784	76,784	76,784	76,784	76,784
R^2	0.160	0.179	0.167	0.166	0.167	0.123
Nr. of Clusters	58	58	58	58	58	58
Age (Linear)	✓		✓	✓	✓	✓
Age (Quadratic)		✓				
Cohort (Linear)	✓	✓		✓		
Cohort (Quadratic)			✓		✓	✓
Cohort Decade				✓		
Fee Years					✓	
Calendar Year FE	✓	✓	✓	✓	✓	✓
Race & Nationality	✓	✓	✓	✓	✓	✓
Postgrad Dummy	✓	✓	✓	✓	✓	
Region	✓	✓	✓	✓	✓	

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment. The regressions use all observations for full-time workers with non-missing weekly wage data.

Table 3: Benchmark Results for Different Sub-Samples

	Outcome: Log real weekly wages					
	Men			*Women*		
	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Undergrad Only	Field of Study	Academic Achievements	Undergrad Only	Field of Study	Academic Achievements
Unemp at enrollment	0.006***	0.005***	0.006***	0.003*	0.004**	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Obs.	62,692	60,439	34,842	48,873	46,605	28,325
R^2	0.181	0.181	0.191	0.156	0.149	0.158
Nr. of Clusters	58	58	55	58	58	55
Age (Linear)	✓	✓	✓	✓	✓	✓
Cohort (Quadratic)	✓	✓	✓	✓	✓	✓
Calendar Year FE	✓	✓	✓	✓	✓	✓

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. All regressions include a race dummy, a dummy for foreign nationals and 19 region of residence dummies. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment. Columns (1) and (4) use observations for full-time workers with non-missing weekly wage data whose highest level of education is an undergraduate degree. Columns (2) and (5) use the sample from Panel B of Table 1, which excludes individuals with missing field of study information. Columns (3) and (6) use the sample from Panel C of Table 1, which excludes individuals with missing academic achievement information (only collected in the LFS from the last quarter of 2005 onwards).

Table 4: Wages and Economic Conditions at Time of College Enrollment: Excluding Non-Standard Students

	Outcome: Log real weekly wages			
	Men		*Women*	
	(1)	(2)	(3)	(4)
Age at enrollment:	18 y.o.	19 y.o.	18 y.o.	19 y.o.
Unemp at enrollment	0.005***	0.009***	0.008***	0.003
	(0.002)	(0.002)	(0.002)	(0.002)
Obs.	19,341	15,653	14,507	11,483
R^2	0.227	0.223	0.183	0.186
Nr. of Clusters	56	57	55	56
Age (Quadratic)	✓	✓	✓	✓
Calendar Year FE	✓	✓	✓	✓

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. All regressions include a race dummy, a dummy for foreign nationals, and 19 region of residence dummies. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment. The regressions use observations for full-time workers with non-missing weekly wage data whose highest level of education is an undergraduate degree and who enrolled into university at age 18 or 19.

Table 5: Selection into Employment

	Outcome:				
	Prob Full-Time	Prob Part-Time	Prob Non-Empl	Log Hourly Wage (incl PT)	Weekly Earn (incl zeros)
	(1)	(2)	(3)	(4)	(5)
Men:					
Unemp at enrollment	0.001	0.000	-0.002	0.006***	10.624***
	(0.002)	(0.000)	(0.001)	(0.001)	(2.099)
Obs.	119,345	119,345	119,345	101,421	119,345
R^2	0.145	0.018	0.124	0.152	0.104
Nr. of Clusters	58	58	58	58	58
Women:					
Unemp at enrollment	-0.003**	0.004***	-0.001	0.000	2.168*
	(0.002)	(0.001)	(0.001)	(0.001)	(1.158)
Obs.	134,067	134,067	134,067	107,196	134,067
R^2	0.071	0.022	0.060	0.130	0.056
Nr. of Clusters	58	58	58	58	58
Age (Linear)	✓	✓	✓	✓	✓
Cohort (Quadratic)	✓	✓	✓	✓	✓
Calendar Year FE	✓	✓	✓	✓	✓

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. All regressions include a race dummy, a dummy for foreign nationals, 19 region of residence dummies, and a dummy for individuals with a post-graduate degree. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment.

Table 6: Mechanisms

	Outcome: Log real weekly wages					
	(1)	(2)	(3)	(4)	(5)	(6)
Men:						
Unemp at enrollment	0.005***	0.005***	0.004***	0.003***	0.005***	0.004***
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Unemp at graduation	0.002					
	(0.001)					
Obs.	96,543	60,439	96,506	92,076	96,417	96,419
R^2	0.169	0.212	0.264	0.379	0.204	0.284
Nr. of Clusters	58	58	58	58	58	58
Women:						
Unemp at enrollment	0.001	0.004**	0.002**	0.002**	0.001	0.002*
	(0.001)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Unemp at graduation	0.001					
	(0.001)					
Obs.	76,784	46,605	76,762	73,177	76,683	76,685
R^2	0.167	0.175	0.291	0.395	0.191	0.259
Nr. of Clusters	58	58	58	58	58	58
Age (Linear)	✓	✓	✓	✓	✓	✓
Cohort (Quadratic)	✓	✓	✓	✓	✓	✓
Calendar Year FE	✓			✓		✓
Field-Year FE		✓				
Occ (Broad)-Year FE			✓			
Occ (Detailed) FE				✓		
Ind (Broad)-Year FE					✓	
Ind (Detailed) FE						✓

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. All regressions include a race dummy, a dummy for foreign nationals, 19 region of residence dummies, and a dummy for individuals with a post-graduate degree (except in Column (2) which excludes individuals with post-graduate degrees). All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment.

Table 7: Academic Performance in High School

		Dummies for Nr. of GCSEs				
	Continuous	1–2	3–4	5–7	8+	Continuous
	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Full-Time Workers					All
Men:						
Unemp at enrollment	-0.015	0.000	-0.000	0.005**	-0.005	-0.016
	(0.012)	(0.001)	(0.001)	(0.002)	(0.003)	(0.013)
Obs.	34,842	34,842	34,842	34,842	34,842	43,245
R^2	0.020	0.003	0.004	0.026	0.027	0.019
Nr. of Clusters	55	55	55	55	55	57
Women:						
Unemp at enrollment	-0.024*	0.000	0.001	0.007**	-0.007**	-0.018*
	(0.012)	(0.000)	(0.001)	(0.003)	(0.003)	(0.010)
Obs.	28,325	28,325	28,325	28,325	28,325	51,719
R^2	0.021	0.005	0.005	0.028	0.028	0.019
Nr. of Clusters	55	55	55	55	55	56
Cohort (Quadratic)	✓	✓	✓	✓	✓	✓

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. The dependent variable is based on the number of GCSE exams passed with a score of C or higher. This information is only collected starting in the third quarter of 2005. Columns (1) and (6) use a continuous measure of the number of GCSEs passed, while the remaining columns use indicator variables for the corresponding intervals. All regressions include a race dummy, a dummy for foreign nationals, and 19 region of residence dummies. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment.

Table 8: Academic Performance in University

	Outcome: University Degree Class (Continuous)				
	(1)	(2)	(3)	(4)	(5)
Sample:	Full-Time Workers				All
Men:					
Unemp at enrollment	0.011***	0.012***	0.012***	0.012***	0.013***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Obs.	34,842	34,842	34,750	34,750	43,245
R^2	0.047	0.048	0.056	0.062	0.052
Nr. of Clusters	55	55	55	55	57
Women:					
Unemp at enrollment	0.006	0.006	0.005	0.006	0.009***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Obs.	28,325	28,325	28,258	28,258	51,719
R^2	0.074	0.074	0.082	0.091	0.076
Nr. of Clusters	55	55	55	55	56
Cohort (Quadratic)	✓	✓	✓	✓	✓
Age at graduation		✓	✓	✓	✓
Field of study FE			✓	✓	
Nr of GCSEs FE				✓	

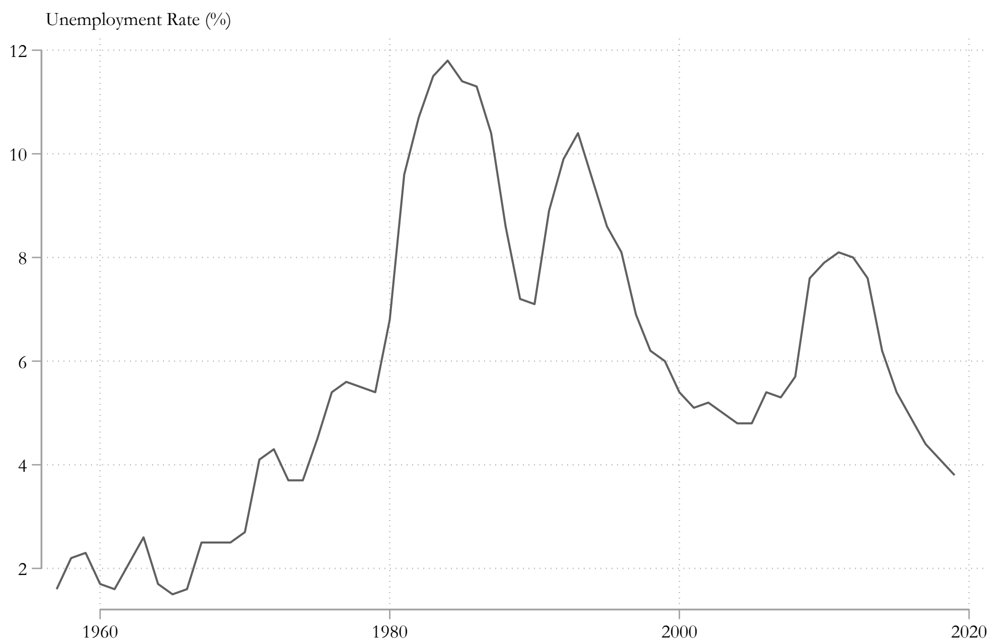
Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. The dependent variable is a continuous measure based on university degree class, with higher values corresponding to higher GPAs. All regressions include a race dummy, a dummy for foreign nationals, and 19 region of residence dummies. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment. In Columns (1) to (4), the sample is restricted to individuals working full-time. Column (5) includes all individuals (including those working part time or not working).

Table 9: Academic Performance and Wages

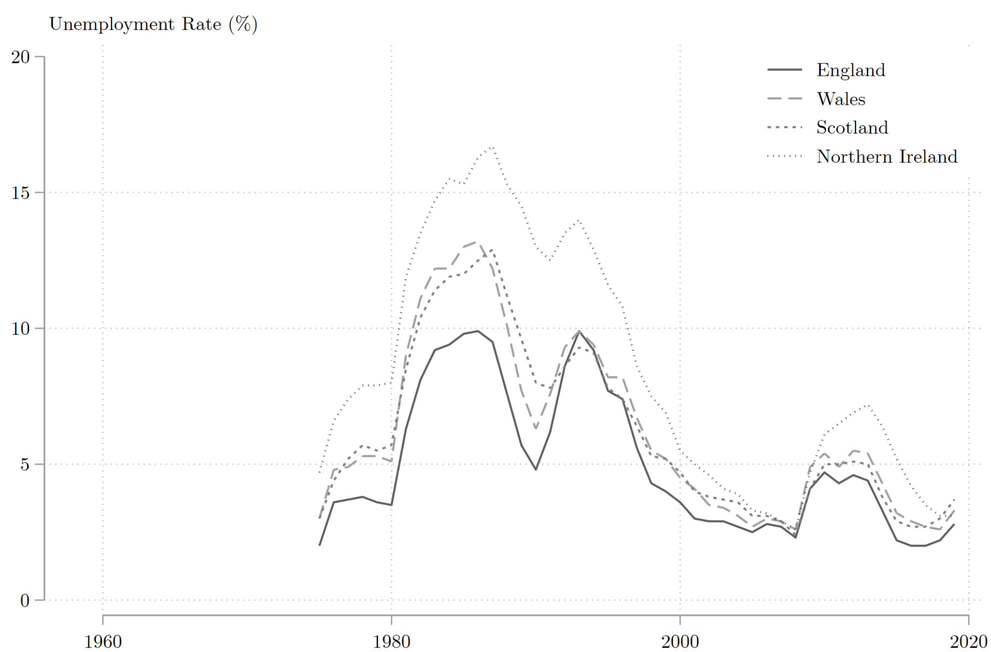
	Outcome: Log real weekly wages					
	Men			*Women*		
	(1)	(2)	(3)	(4)	(5)	(6)
Unemp at enrollment	0.006***	0.005***	0.004***	0.005**	0.005***	0.004**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Nr of GCSEs	0.032***	0.029***	0.029***	0.029***	0.026***	0.026***
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)
First Class		0.152***			0.065***	
		(0.017)			(0.017)	
Upper Second Class		0.059***			-0.010	
		(0.016)			(0.016)	
Lower Second Class		-0.031*			-0.102***	
		(0.016)			(0.015)	
Third Class		-0.087***			-0.161***	
		(0.019)			(0.025)	
Obs.	34,842	34,842	34,842	28,325	28,325	28,325
R^2	0.201	0.215	0.217	0.168	0.181	0.184
Nr. of Clusters	55	55	55	55	55	55
Age (Linear)	✓	✓	✓	✓	✓	✓
Cohort (Quadratic)	✓	✓	✓	✓	✓	✓
Calendar Year FE	✓	✓		✓	✓	
Degree Class-Year FE			✓			✓

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. All regressions include a race dummy, a dummy for foreign nationals, and 19 region of residence dummies. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment.

Figure 1: Unemployment Rates



(a) National Unemployment Rate

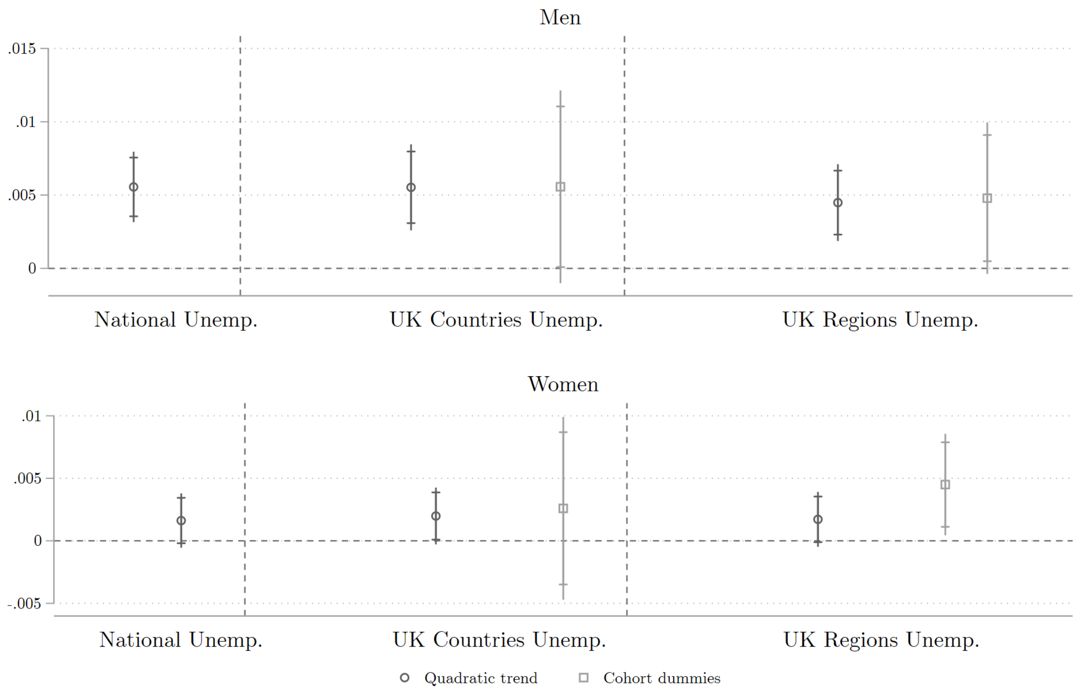


(b) Unemployment Rates for UK Countries

Figure 2: Detrended Cohort Effects in Wages. Age at Enrollment: 18 and 19

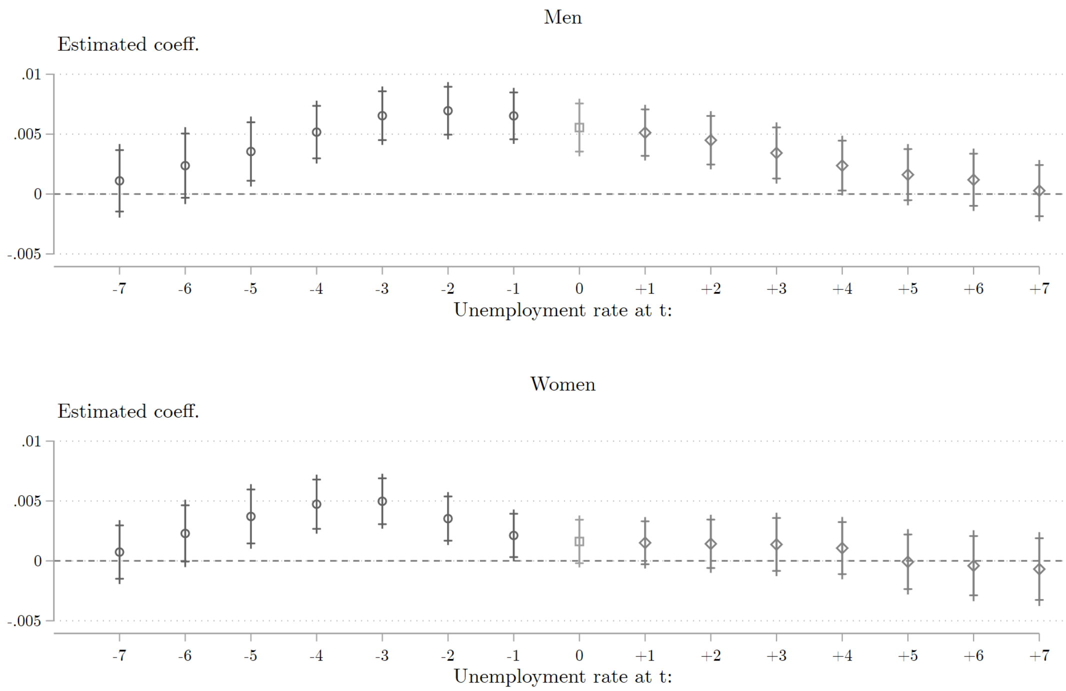
Note: The figure plots estimated cohort effects based on the Kwon et al. (2010) approach, where the first and last cohort effects are set to 0 and the estimates therefore represent deviations from a (non-identified) linear cohort trend. The line labeled "Cohort effect 18" is based on an estimation which uses only individuals who enroll at age 18, while the line labeled "Cohort effect 19" is based on an estimation which uses only individuals who enroll at age 19. The estimated effects are based on regressions of log real weekly earnings which include a full set of age and calendar year fixed effects, a race dummy, a dummy for foreign nationals, and 19 region of residence dummies. The regressions are weighted using person weights from the LFS, and they use all observations for full-time workers with non-missing weekly wage data who enroll at the relevant age. Due to limited sample size when restricting to specific enrollment ages, we exclude cohorts who enrolled at 18 before 1959 or after 2011 and before 1964 and after 2011 for those who enrolled at 19 for this part of the analysis.

Figure 3: Coefficient on Unemployment at Enrollment at Different Levels of Geographic Aggregation



Note: The black circles represent the coefficient for the unemployment rate at entry on the logarithm of real wages in a regression including a quadratic trend for cohort effects. The gray squares represent the coefficient for the U.K. country unemployment rate at entry on the logarithm of real wages in a regression including cohort specific effects. The black and gray lines represent the 95% confidence intervals. The cap on each line represent the 90% confidence intervals. All regressions include calendar year fixed effects, a linear term in age, a race dummy, a dummy for foreign nationals, and a dummy for postgraduate students. All regressions are weighted using the LFS personal weights. Standard errors are clustered by year of enrollment.

Figure 4: Effect of Unemployment in the Year of Enrollment and Adjacent Years

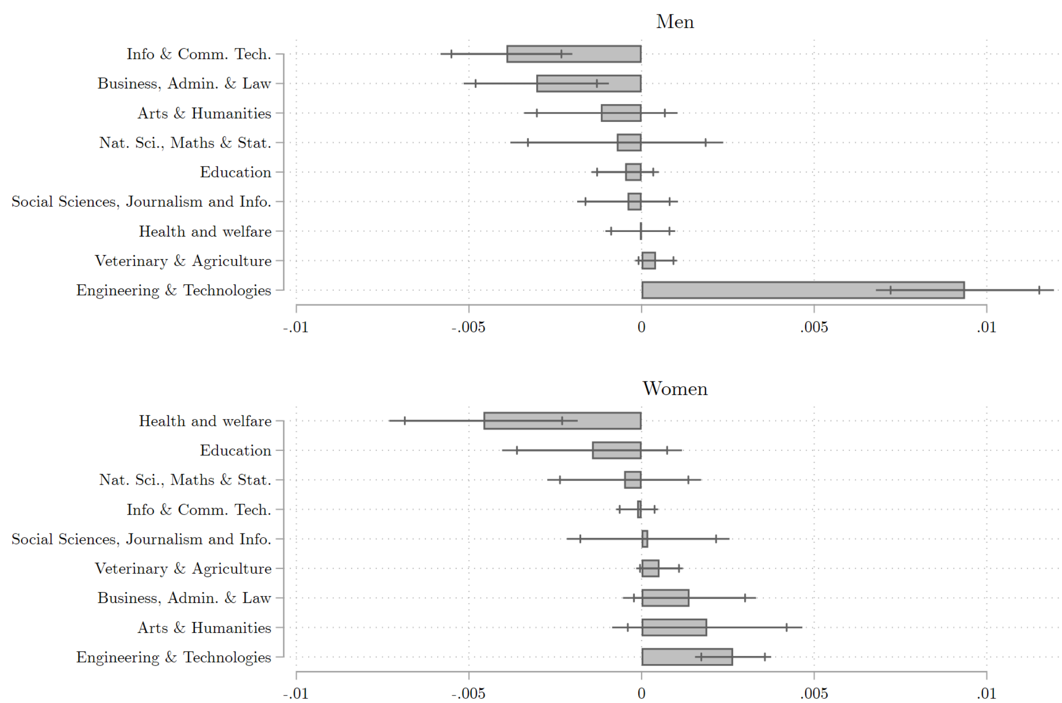


Note: The markers represent the estimated effect of the unemployment rate in year t on the logarithm of real wages, where $t = 0$ is the (imputed) year of enrollment. The lines represent the 95% confidence intervals. The cap on each line represents the 90% confidence interval. All regressions include a quadratic cohort trend, calendar year fixed effects, a linear term in age, a race dummy, a dummy for foreign nationals, and a dummy for postgraduate students. All regressions are weighted using the LFS personal weights. Standard errors are clustered by year of enrollment.

Figure 5: Unemployment Rates at the Time of College Enrollment and College Graduation, by Cohort

Note: Each dot represents a cohort defined by the year of college enrollment. The x-axis indicates the detrended unemployment rate at college enrollment, and the y-axis the detrended unemployment rate at the time of college graduation (removing a quadratic trend in both cases).

Figure 6: Change in Major Selection Probabilities



Note: Bars represent the estimated coefficients for the effect of unemployment rate at college enrollment on the probability of selecting each of the nine major categories. The lines represent the 95% confidence intervals. The cap on each line represents the 90% confidence interval. Regressions include controls for race, nationality, region of residence and a quadratic cohort trend. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment.

Online Appendix for:

Caught in the Cycle:
Economic Conditions at Enrollment and Labor Market Outcomes of College Graduates

Alena Bičáková (CERGE-EI)
Guido Matias Cortes (York University and RCEA)
Jacopo Mazza (University of Essex)

Appendix A Scarring Effects in the UK

In Section 4.2 we control for the unemployment rate at the time of graduation in our benchmark specification (see Column (1) of Table 6) and find that the estimated coefficient on this variable is statistically insignificant and positive – a result that seems puzzling in light of the literature on scarring effects. In this Appendix we further explore the evidence for scarring effects in the UK.

Using our the male sample, Column (1) of Table A.2 considers a standard scarring effects specification, which controls for the unemployment rate at graduation, and its interaction with years since graduation, without allowing for a cohort trend. The result shows the standard pattern in the literature: cohorts that graduate during periods of higher unemployment earn lower wages, and this wage differential is slowly eliminated with time in the labor market. Column (1) of Table A.3 shows a similar result for women.

Columns (2) and (3) of Tables A.2 and A.3, however, show that this result is not robust to the inclusion of a cohort trend – linear or quadratic. Once cohort trends are included, the coefficients on unemployment at graduation and its interaction with years since graduation change signs. Columns (4) and (5) show the results without the interaction term, once again including either a linear or a quadratic cohort trend. These results confirm the lack of evidence for a negative relationship between unemployment at graduation and wages, once cohort trends are included.

In Columns (6) and (7), where we control for both unemployment at enrollment and unemployment at graduation, we see that the positive and significant coefficient on unemployment at graduation observed in Columns (4) and (5) disappears, whereas the coefficient on unemployment at enrollment is positive and statistically significant (except in the case of women when we allow for a quadratic cohort trend, as discussed in the body of the paper). In Column (7) we control for years since graduation instead of age and obtain a nearly identical result.[1]

Our results therefore suggest that, when focusing on UK university graduates, there is no robust evidence for the standard type of scarring effects often discussed in the literature. If anything, it appears to be the case that individuals graduating during downturns earn higher wages, but this is entirely explained by the fact that these individuals also tend to enroll when unemployment rates are high (see Figure 5). The finding that (male) graduates enrolling during downturns earn higher wages remains robust.

Although our finding regarding the lack of scarring effects for UK university graduates seems surprising, it does not contradict any existing evidence in the literature. Most of the literature on this topic has used data for the US or for a few other countries such as Sweden. We are not aware of any analysis that explores scarring effects in the UK except for Cribb et al. (2017). While scarring effects are not the main focus of their paper, Table 2 of their Appendix presents a regression of log earnings for a sample of tertiary education graduates that also includes unemployment at the time of graduation as one of the regressors. They also find that the coefficient is not statistically significant, suggesting zero scarring effects for college graduates in the UK. Further exploring why the effect of unemployment at graduation would be different in the UK than in other countries such as the US seems like an interesting avenue for future research.

[1] Note that variation in age conditional on calendar year effects and cohort trends remains in our data due to variation in the age of enrollment, as discussed in the paper. In the case of years since graduation, variation remains conditional on calendar year effects and cohort trends due to variation in program lengths (e.g. medicine vs others; Scotland vs the rest of the UK).

Table A.1: Wages and Economic Conditions at Time of College Enrollment: Scotland Adjustment

	Outcome: Log real weekly wages					
	(1) All	(2) Excl. Scot	(3) All	(4) Excl. Scot	(5) All	(6) Excl. Scot
Men:						
Unemp at enrollment	0.006***	0.006***	0.006***	0.006***	0.008*	0.008*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.005)	(0.005)
Obs.	84,267	77,193	84,267	77,193	84,267	77,193
R^2	0.195	0.195	0.172	0.172	0.169	0.169
Nr. of Clusters	58	58	58	58	58	58
Women:						
Unemp at enrollment	0.005***	0.005***	0.002	0.002	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)	(0.003)	(0.003)
Obs.	68,839	62,419	68,839	62,419	68,839	62,419
R^2	0.180	0.180	0.170	0.170	0.167	0.168
Nr. of Clusters	58	58	58	58	58	58
Age (Linear)			✓	✓	✓	✓
Age (Quadratic)	✓	✓				
Cohort (Linear)	✓	✓			✓	✓
Cohort (Quadratic)			✓	✓		
Cohort Decade					✓	✓
Calendar Year FE	✓	✓	✓	✓	✓	✓

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. All regressions include a race dummy, a dummy for foreign nationals, a dummy for individuals with a postgraduate degree, and 19 region of residence dummies. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment. The regressions use observations for full-time workers with non-missing wage data for the waves from 2001 onwards (which is the period for which we have information on location of birth). Columns (2), (4) and (6) exclude individuals who were born in Scotland.

Table A.2: Scarring Effects – Males

	Outcome: Log real weekly wages							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemp at graduation	-0.029***	0.017***	0.015**	0.005***	0.003***	0.001	0.002	0.002
	(0.003)	(0.004)	(0.007)	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)
Unemp at grad × Yrs since grad	0.002***	-0.001***	-0.001					
	(0.000)	(0.000)	(0.000)					
Unemp at enrollment						0.006***	0.005***	0.005***
						(0.001)	(0.001)	(0.001)
Age (Linear)	✓	✓	✓	✓	✓	✓	✓	
Yrs since Grad (Linear)								✓
Cohort Trend (Linear)		✓		✓		✓		
Cohort Trend (Quadratic)			✓		✓		✓	✓
Calendar Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Obs.	96,543	96,543	96,543	96,543	96,543	96,543	96,543	96,543
R^2	0.183	0.195	0.169	0.195	0.168	0.195	0.169	0.171
Nr. of Clusters	58	58	58	58	58	58	58	58

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. All regressions include a race dummy, a dummy for foreign nationals, a dummy for individuals with a postgraduate degree, and 19 region of residence dummies. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment. The regressions use all observations for full-time male workers with non-missing weekly wage data.

Table A.3: Scarring Effects – Females

	Outcome: Log real weekly wages							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Unemp at graduation	-0.025***	0.011**	0.017***	0.005***	0.001	0.001	0.001	0.001
	(0.003)	(0.005)	(0.005)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Unemp at grad × Yrs since grad	0.002***	-0.000	-0.001***					
	(0.000)	(0.000)	(0.000)					
Unemp at enrollment						0.006***	0.001	0.002
						(0.002)	(0.001)	(0.001)
Age (Linear)	✓	✓	✓	✓	✓	✓	✓	
Yrs since Grad (Linear)								✓
Cohort Trend (Linear)		✓		✓		✓		
Cohort Trend (Quadratic)			✓		✓		✓	✓
Calendar Year FE	✓	✓	✓	✓	✓	✓	✓	✓
Obs.	76,784	76,784	76,784	76,784	76,784	76,784	76,784	76,784
R^2	0.170	0.179	0.169	0.178	0.167	0.179	0.167	0.171
Nr. of Clusters	58	58	58	58	58	58	58	58

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. All regressions include a race dummy, a dummy for foreign nationals, a dummy for individuals with a postgraduate degree, and 19 region of residence dummies. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment. The regressions use all observations for full-time female workers with non-missing weekly wage data.

Table A.4: Wage Regressions by Field of Study

	Men	*Women*
	(1)	(2)
Health & Welfare	-0.011**	-0.005*
Social Sciences, Journalism & Info	-0.002	0.004
Business, Admin & Law	0.006**	0.004
Arts & Humanities	0.004	0.004
Education	0.005	0.005*
Nat Sci, Math & Stat	0.006***	0.011***
Veterinary & Agriculture	0.012	-0.007
Info & Comm Tech	0.010**	0.010
Engineering & Technologies	0.006**	-0.001
Age x Field	✓	✓
Cohort Trend (Quadratic)	✓	✓
Calendar Year x Field	✓	✓
Obs.	60,439	46,605
R^2	0.214	0.176
Nr. of Clusters	58	58

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. The dependent variable is log real weekly wages. All regressions include a race dummy, a dummy for foreign nationals, and 19 region of residence dummies. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment.

Table A.5: University Degree Class Probability

	(1) Ordinary	(2) Third	(3) Lower Second	(4) Upper Second	(5) First
Men:					
Unemp at enrollment	-0.000	-0.000	-0.004**	-0.002	0.006***
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Obs.	34,842	34,842	34,842	34,842	34,842
R^2	0.068	0.010	0.021	0.015	0.018
Nr. of Clusters	55	55	55	55	55
Women:					
Unemp at enrollment	-0.002**	0.000	0.002	-0.002	0.002
	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
Obs.	28,325	28,325	28,325	28,325	28,325
R^2	0.106	0.006	0.029	0.022	0.026
Nr. of Clusters	55	55	55	55	55
Cohort (Quadratic)	✓	✓	✓	✓	✓
Age at graduation	✓	✓	✓	✓	✓

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. The dependent variables are indicator variables for the degree class obtained at university. All regressions include a quadratic cohort trend, a control for age at graduation, a race dummy, a dummy for foreign nationals, and 19 region of residence dummies. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year of enrollment.

Table A.6: Probability of non-employment during full time college studies

	Males			*Females*		
	(1)	(2)	(3)	(4)	(5)	(6)
Unemp at enrollment	0.011***	0.006**	0.005*	0.003*	-0.003	-0.002
	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)
Year × Age FE	✓	✓	✓	✓	✓	✓
GCSE FE × Age FE			✓			✓
Age FE	✓	✓	✓	✓	✓	✓
GCSE FE		✓	✓		✓	✓
Obs.	68,538	44,317	44,317	80,898	53,594	53,594
R^2	0.034	0.035	0.039	0.033	0.033	0.036
Nr. of Clusters	22	15	15	22	15	15

Note: ***, ** and * denote statistical significance at the one, five and ten percent levels, respectively. The dependent variable is an indicator variable for non-employment at the time of the survey. The sample is restricted to full-time students. All regressions include a race dummy, a dummy for foreign nationals, and 19 region of residence dummies, as well as a full set of age fixed effects, a quadratic time trend and an age specific linear time trend. All regressions are weighted using person weights from the LFS. Standard errors are clustered by year. Columns (2), (3), (5) and (6) further restrict the sample to individuals with information on their GCSE score; this information is only collected starting in the third quarter of 2005. Columns (2) and (5) include four GCSE performance group dummies, while Columns (3) and (6) add an age-specific GCSE group trend.