Student Employment in England after the Maintenance Grant Removal

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July 17, 2024

In 2016 the UK removed a living cost grant for university students. Motivated by a large increase in student employment after the grant was removed, I estimate whether the loss in income pushed students to work during university. I use two Difference-in-Differences designs, the first compares students to the regular labor market, and the second splits students by grant eligibility. Although student employment increased compared to the regular labor market, the increase is common to eligible and ineligible students. My results suggest that the maintenance grant removal did not cause the increase in student employment.

Introduction

We like to think of university as a place of opportunity for all. But we should also remember how expensive it can be. England is a fitting example. Tuition fees tripled within ten years and today the typical cost of studying is £22,000 per year[1]. At the same time, England offers a comprehensive system of student support. The coexistence of high costs and strong support creates a striking setting to study students' choices.

A key issue for poor students are borrowing constraints. Lots of research has focused on borrowing constraints, and found that they increasingly discourage poor students from enrolling in university (Belley and Lochner 2007, Lovenheim 2011, Lochner and Monge-Naranjo 2012). And even if poor students enroll, they are less likely to graduate and more likely to work while studying (Keane and Wolpin 2001, Hotz et al. 2018).

My research relates to the choice of working while studying. I estimate whether student employment has increased after the UK government removed a study grant. The setting is distinctive in two ways. First, the grant covered only living costs, while a separate system of loans covers tuition fees. Working part-time is much more likely to pay regular living costs than large upfront tuition fees, hence the choice to work is particularly relevant in my setting. Second, the UK student support makes borrowing constraints irrelevant. After the grant was removed, it was replaced by government-backed loans available to all students. This means that a student who decided to work did not do so because she couldn't borrow, but because she preferred working to borrowing money.

Now, if a student decides to work during university, is that good or bad for her? Most research has thought about this in two ways: the immediate impact on grades, and the longer-term impact on the labor market transition. While some papers have found that working during school or university lowers grades (Ruhm 1997, Eckstein and Wolpin 1999, Tyler 2003, Stinebrickner and Stinebrickner

1. "Student Money Survey 2023 – Results" 2023.

2003), most recent papers find that working only a moderate amount of hours does not harm grades (Montmarquette, Viennot-Briot, and Dagenais 2007, Buscha et al. 2012, Aucejo, Perry, and Zafar 2023).

The relationship between working during university and the labor market transition is clearer. Several papers show that working raises employment and wages after graduation (Ruhm 1997, Pallais 2014, Ashworth et al. 2021, Le Barbanchon, Ubfal, and Araya 2023), and that is partially due to skills obtained exclusively on-the-job (Heckman, Stixrud, and Urzua 2006, Adhvaryu, Kala, and Nyshadham 2018, Alfonsi et al. 2020).

My paper focuses only on how student employment changed after the academic year 2016/2017 when the grant was removed. Despite constant employment below 20% in the early 2010s, after the grant removal it rose to almost 30%. I scrutinize this rise in a difference-in-differences (DiD) setting, comparing students to people of the same age who never attend university. The classic DiD design shows that student employment increased by 3.5 percentage points. The corresponding event study shows that the effect increases up to 2018/2019, when it reaches 9 percentage points. Unfortunately, multiple pre-treatment coefficients are almost significant at the 5%-level. This questions the parallel trends assumption and makes it difficult to interpret the results.

I construct a second DiD design with a more robust parallel trends assumption. By assigning treatment based on whether someone is eligible for the grant, I'm able to focus on those students whom the grant removal directly impacts. The second design fails to convincingly confirm my first result. Both eligible and ineligible students experience a rise in employment after 2016. But while employment among eligible students rises by 1.6 percentage points more, the difference is statistically insignificant.

I combine the previous two comparisons into a Triple Differences Design. This design allows for a weaker parallel trends assumption by comparing eligible and ineligible students, but additionally adjusting for different employment trends among people who never study. The Triple Differences Design estimates that employment of eligible students fell, but again, the estimate is statistically insignificant.

So far, my analysis ignores the possibility that the maintenance grant removal could have discouraged students from enrolling in university. Such a behavior could introduce a selection bias, in particular, if it mainly affects the enrollment of poor students. I test for selection bias by comparing the enrollment rates of eligible and ineligible potential students. Reassuringly, enrollment rates of both groups are not significantly different after 2016.

The lack of a change in enrollment strengthens my analysis. I can interpret changes in employment as similar students making different choices, rather than different students making the same choice. Nonetheless, my analysis cannot confirm that removing the maintenance grant caused students to work more.

The Maintenance Grant

Students in the UK benefit from a comprehensive system of financial support that is the result of many changes over the last 20 years. The current system has two parts, covering tuition fees and living costs. My analysis will focus on the living costs support, also called maintenance support.

Before 2016[2], maintenance support consisted of a grant and a loan component, both of which depended on a student's parents' household income. Below an income of £25,000, students received the full grant of £3,387 per year. Between an income of £25,000 and £42,620, students receive a partial grant, where every additional £6 in income decreases the eligible amount by roughly £1. The amount is reduced until a household income of £42,620, above this students become ineligible. Figure 1 shows this information graphically.

The grants are complemented by maintenance loans. The maximum amount that can be loaned depends on whether a student lives at home (£4,465), away from home (£5,740), or in London (£8,009). 65% of the amount is available to all students, but the rest is also tied to household income. Lastly, every £1 of maintenance grant received, reduces the loanable amount by £0.50.

2. I refer to each academic year by the calendar year in which it starts: e.g. 2016 for the academic year 2016/2017.

Unlike the grants, the loans need to be repaid. The repayment is part of the tuition fee loan system, where the maintenance loans are just added as additional debt. Students only need to start paying once they graduate, and only if they earn more than £21,000. The exact monthly repayment is a fixed share of their income beyond the £21,000 threshold. The takeaway is that taking on additional debt does not increase how much graduates will have to pay back each month, it only means they will have to pay back for longer. And, in any case, after 25 years the entire debt is written off. In practice, this means that less than a third of students ever pay back their entire loan.

In 2016 the maintenance grants were removed. New students received all their maintenance support through loans. In compensation, the loanable amounts were sharply raised such that total support available increased by roughly 10%.

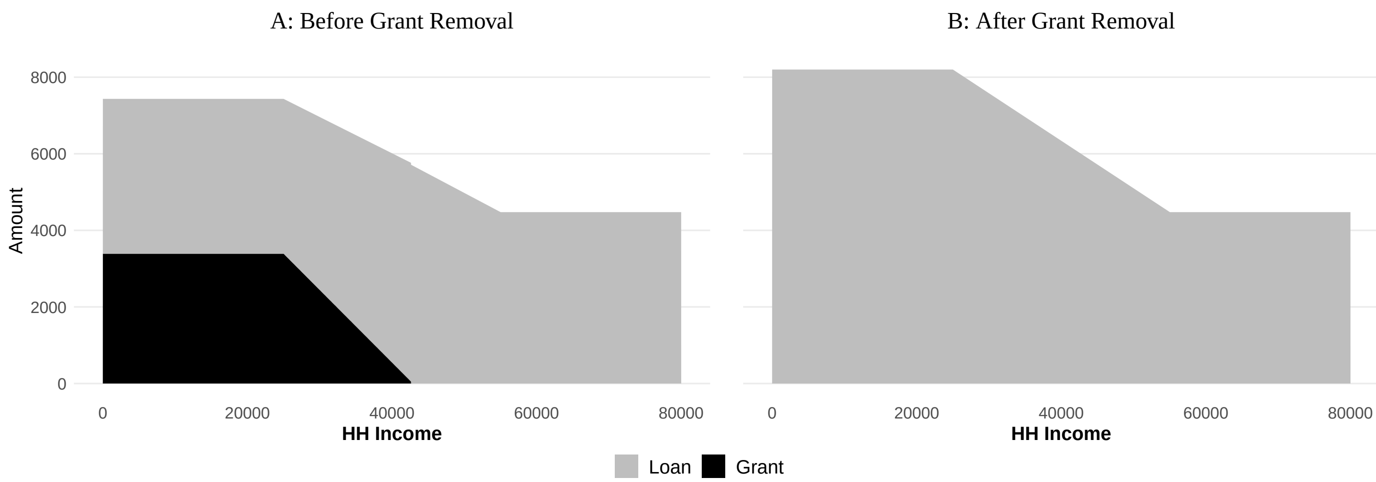

Figure 1: Maintenance Support Before and After the Grant Removal

I use the grant removal in my analysis by assuming that it reduces the liquidity of poor students exogenously. This assumption determines to what extent my analysis is valid. For example, I need to assume that students don't replace the maintenance grant one-for-one with a larger maintenance loan, because then their liquidity wouldn't change. This assumption might seem likely, as taking a loan is clearly less attractive than receiving a grant. On the other hand, I mentioned that only one-third of students pay off their entire student loans. For the remaining two-thirds of students, additional debt is like free money. And if many students think of debt as free money, it's unlikely that they'll face lower liquidity.

In this overview I have described all amounts as they were in 2015 and 2016[3], directly before and after the removal of the maintenance grant. Some amounts were, more or less, raised with inflation in the previous and following years, but no drastic changes occurred. The maintenance grant had been originally introduced in 2006, in an environment of much lower tuition fees. Research has found that its introduction was associated with an increase in enrollment of poorer students (Dearden, Fitzsimons, and Wyness 2011, Dearden, Fitzsimons, and Wyness 2014). Despite that, the numerous tuition fee increases that occurred since 2006 have not affected the income gap in enrollment (Azmat and Simion 2017, Murphy, Scott-Clayton, and Wyness 2019, Murphy and Wyness 2023). The only other paper studying the replacement of grants with loans (Linsenmeier, Rosen, and Rouse 2006) found no negative effects on enrollment. Hence, my paper advances our understanding of the impacts of English university finance as it is the first to study the 2016 maintenance grant removal. It is also the first to analyze the choice to work during university in the UK.

Employment

After the maintenance grant was removed in 2016, student employment increased sharply (Figure 2). Employment was stable at below 20% in the early 2010s, but after 2016 it increased to almost 30%. During Covid it fell, but never reached pre-reform levels again. In my analysis I will examine the increase in employment to estimate the grant removal's causal impact.

3. Source: Student Finance England 2024

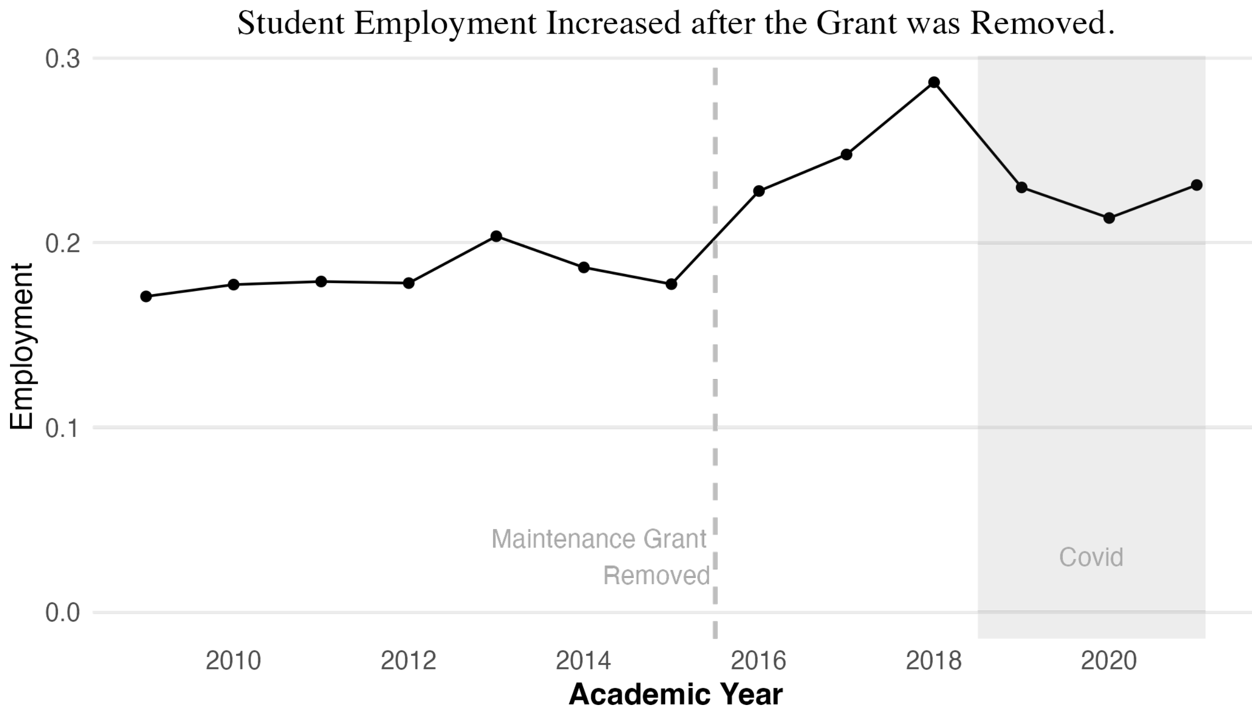

Figure 2: Student Employment Rate 2009-2021

I use data from Understanding Society[4], also known as the UK Household Longitudinal Study. Understanding Society is a panel data set covering 40,000 UK households. It started in 2009 after taking over from the British Household Panel Study, and offers a convenient time frame to study the period before and after the 2016 grant removal. More information can be found in the Understanding Society main survey user guide[5].

Using Understanding Society has several advantages. It contains a wide range of information on demographics, education, and work, and in particular, it enables me to observe employment of people whose main labor force status is to be a student. Many alternative surveys only record whether someone works or studies. A second advantage is the panel structure. I can use it to infer information about a student's educational career that doesn't exist as a separate variable. For example: Is a student in their first year? Or: Did they take a gap year after secondary school, or go straight to university? A final advantage is the possibility to link parents and children. The

4. University of Essex, Institute for Social and Economic Research, Understanding Society: Waves 1-13, 2009-2022 and Harmonised BHPS: Waves 1-18, 1991-2009. [Data Collection]. 18th Edition. UK Data Service. SN: 6614.

5. Institute for Social and Economic Research 2023.

linkage enables me to relate a student's decision to work to their socioeconomic background.

Comparing Student Employment to the Regular Labor Market

The rise in student employment after 2016 could simply reflect a rise in overall UK employment. To understand whether the rise is specific to students, I use a DiD design. This design enables me to compare the employment of undergraduate students, to a control group of non-students of the same age who never attend university.

Allocation to the treatment or control group depends on a choice (to attend university), rather than chance. Hence the people in the treatment and control group are potentially quite different. To judge the quality of my DiD design it is important to understand how different they are.

I compare the treatment and control groups when the maintenance grant was still in place, to avoid confusing differences between groups with differences due to the removal. Table 1 shows that students are younger and from richer backgrounds. They are more likely to be female, and in particular, female students are much more likely to work. Among people who don't study, working is equally common among males and females.

Table 1: Demographics and Labor Market Information on Students and non-Students

	Student			
	No		Yes	
	Unemployed	Employed	Unemployed	Employed
Age	20.5	23.4	20.4	20.7
Share Male	0.49	0.50	0.47	0.34
HH Income Parents (£)	3,171	3,939	4,317	4,019
Labor Income (£)	79	1,279	117	415
Weekly Working Hours	2.3	31.9	3.2	12.1
Employment Rate	0.62		0.18	

Note: Labor Income and Working Hours are measured conditionally on being employed.

More differences emerge when I compare the types of jobs chosen by students and non-students (Figure 3). Students are overrepresented in the sales and services industry, both of which offer many typical student jobs. They are underrepresented in most other industries, in particular the ones involving technical or professional work, such as the legal or financial sector, or public administration. Consequently, students earn much less than non-students.

The differences suggest that student employment is fundamentally different from regular employment. This is bad news for a DiD strategy that compares the employment of students and non-students, and should be kept in mind when interpreting the results. Especially the parallel trends assumption seems questionable. Given a student working in a restaurant and a non-student working in a government agency, would I expect them to be equally affected by a rise in food prices? Clearly not. I will address this issue by showing a specification that controls for the pre-determined differences I showed above: sex, age, and parental income. Adding controls will account for some differences and make the DiD comparison more robust, but it will struggle to hide all differences.

The parallel trends assumption faces also a second threat by the regular employment rate being three times as high as the student employment rate (60% versus 20%). To understand why that is problematic, it is useful to think about what the parallel trends assumption really means. Imagine suddenly food prices in the economy are rising. The assumption says that this shock has an equal impact on regular and student employment. Equal, in this case, means both employment rates change by an equal absolute amount, say five percentage points. But because of the large difference in baseline employment, this equal absolute change implies a very unequal relative change ($\frac{5\text{pp}}{60\text{pp}} = 8.3\%$ versus $\frac{5\text{pp}}{20\text{pp}} = 25\%$).

A solution would be to formulate the parallel trends assumption in relative terms rather than absolute terms, but individual observations and a binary outcome make testing for relative parallel trends impossible. Instead, we should keep the baseline differences in mind, and remember that two equal coefficients imply a much larger relative change for students.

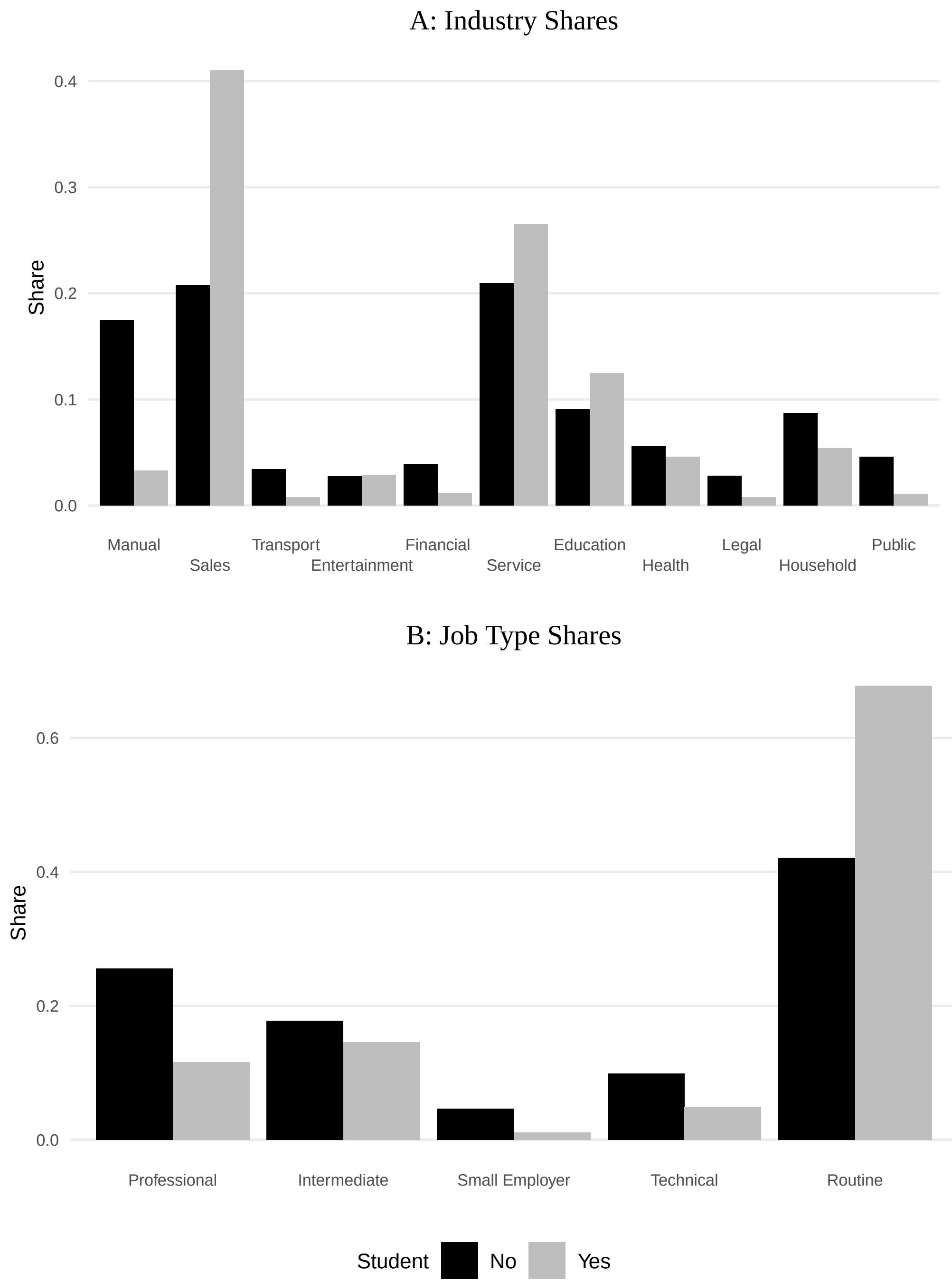

Figure 3: Industry and Job Type of Students and non-Students

The main DiD model is:

$$\begin{aligned}\text{Employment} = \text{Constant} &+ \beta_1 \text{Treatment Group} \times \text{Post-Treatment} \\ &+ \beta_2 \text{Treatment Group} + \beta_3 \text{Post-Treatment} + \epsilon\end{aligned}$$

β_1 estimates the classic DiD treatment effect that controls for a treatment group dummy and a post-treatment dummy. When I estimate this model, I exclude observations from the academic year 2019/2020 and later so that the result is not influenced by Covid. To focus on how employment changed over time I also estimate an event study model:

$$\begin{aligned}\text{Employment} = \text{Constant} + \sum_{y=2009}^{2020} \beta_{1y} \text{Treatment Group} \times \text{Year Dummy}_y \\ + \beta_2 \text{Treatment Group} + \sum_{y=2009}^{2020} \beta_{3y} \text{Year Dummy}_y + \epsilon\end{aligned}$$

Instead of the single treatment coefficient in the classic DiD model, the event study estimates separate treatment coefficients for each academic year. This is useful for two reasons: The post-treatment coefficients reveal whether the employment increase is constant after 2016, or changes over time. And the pre-treatment coefficients reveal whether there are parallel pre-trends, which would be important evidence for the parallel trends assumption.

Using the classic DiD model I estimate that student employment after the grant removal increases by roughly 3.5 percentage points (Table 3). The event study shows that the initial increase in employment is moderate, but increases up until 2018 to reach a maximum of roughly 9 percentage points (Figure 4). This large increase is still slightly smaller than the 11 percentage points increase observed in the raw time series. Hence, it is likely that an increase in general employment after 2016 was responsible for some of the increase in student employment.

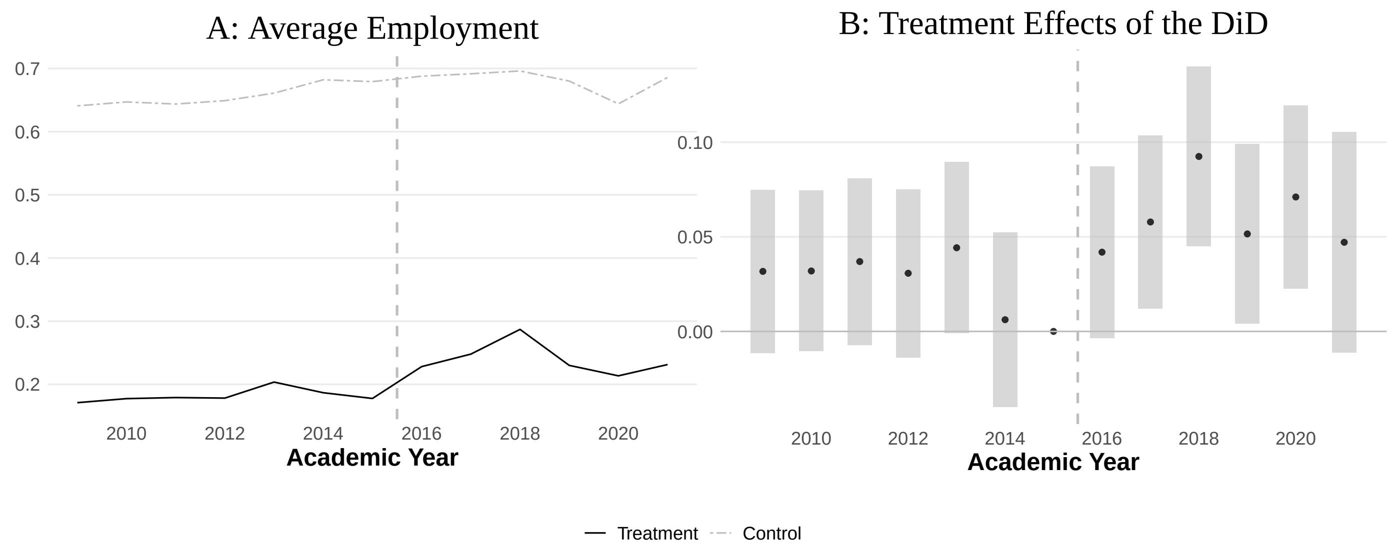

Figure 4: DiD Employment of Students and non-Students

Unfortunately, the employment trends of the treatment and control groups are not parallel before 2016. Instead, employment in the control group grew more quickly than student employment, and multiple pre-treatment coefficients are almost significant at the five percent level. This reinforces my doubt about the parallel trends assumption, however, it could be in line with the relative version of parallel trends that I discussed earlier.

As I described before, I estimate a second model that controls for sex, age, and parents' household income (Figure 5). Adding controls shows a partial improvement. The pre-trends of the treatment and control groups become more similar, and all pre-treatment coefficients are clearly insignificant. However, the controls also increase the standard errors, such that all but the 2018 post-treatment coefficients are now insignificant as well. The estimated increase is also smaller overall, the classic DiD estimates it to be 2.9 percentage points, and the event study estimates a maximum increase of roughly 7 percentage points in 2018.

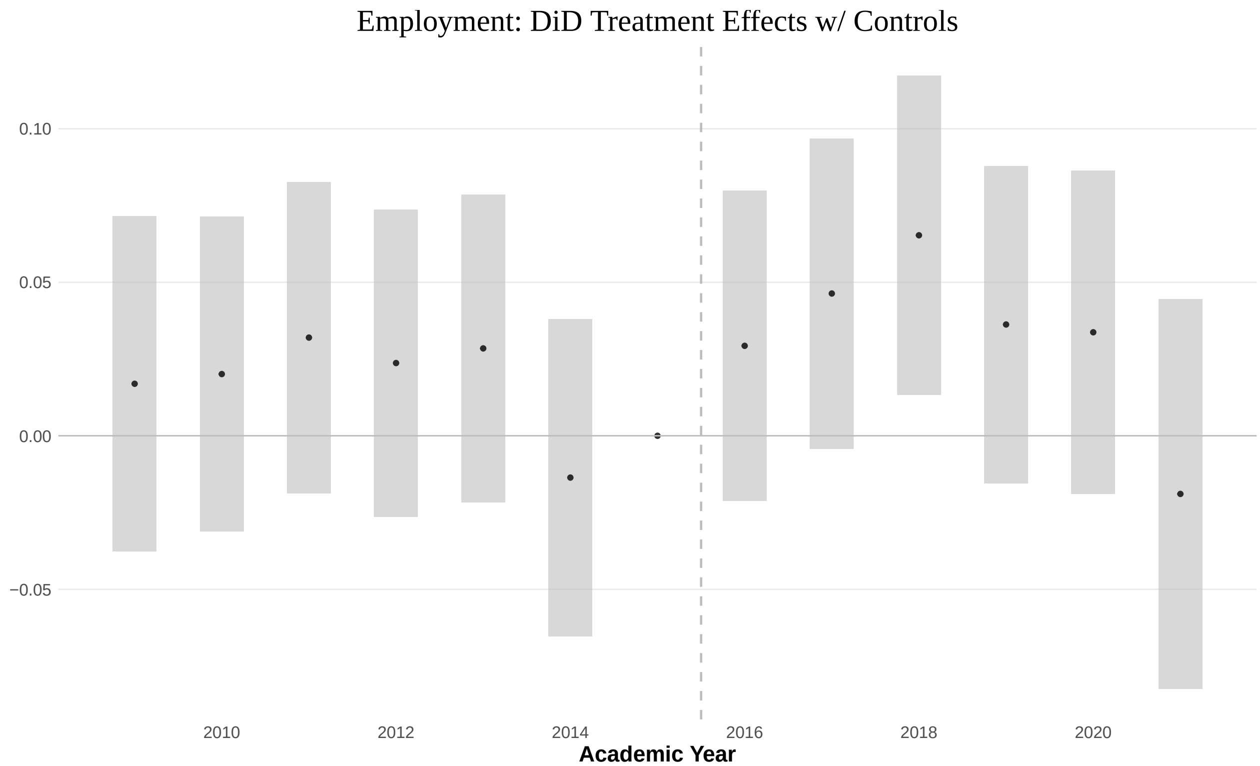

Figure 5: Employment Event Study Coefficients with Controls

The main issue in my analysis so far has been the parallel trends assumption. Thinking about the implications of violating the assumption is speculative, however, if the stronger employment growth in the control group had continued even without the grant removal, then the true treatment effect would be even larger. But beyond such speculation, it would be best to find a more comparable control group.

Comparing Eligible and Ineligible Students

So far I have compared student employment to employment in the regular labor market. Alternatively, I can assign treatment status using the maintenance grant's eligibility rules. The rules let me form a treatment group of students that are either fully or partially eligible, and a control group of ineligible students. The result is a more accurate treatment group, where every treated student is potentially affected by the grant removal, unlike the previous comparison, where the treatment group contained students who were too rich to ever receive the grant. Moreover, I restrict the analysis to students only. This restriction allows me to stop comparing people in student jobs to the regular labor market.

As so often, dealing with one problem will introduce a new one. Splitting the observations by eligibility relieves me from comparing students to non-students, but it does mean that I compare poor students to rich students. A comparison that introduces its own set of differences.

Table 2 shows that eligible (poor) students are older, which indicates that they wait longer to enroll in university. Eligible students are also less likely to be employed, and work and earn less on average. This is in conflict with seeing student work mainly as a way for poorer students to fund themselves.

Table 2: Demographics and Labor Market Information on Employed and Unemployed Students

	Maintenance Grant Eligibility					
	Ineligible		Partial		Full	
Employed	No	Yes	No	Yes	No	Yes
Age	20.4	20.6	20.2	20.5	20.5	21.1
Share Male	0.49	0.36	0.44	0.33	0.48	0.32
HH Income Parents (£)	6,318	5,435	2,914	2,916	1,585	1,643
Labor Income (£)	156	456	104	374	54	389
Weekly Working Hours	4.3	12.6	3.0	11.9	1.5	11.3
Employment Rate	0.20		0.18		0.17	

Note: Labor Income and Working Hours are measured conditionally on being employed.

The typical student jobs I described earlier, are more commonly occupied by eligible (poor) than ineligible students (Figure 6). Hence, there are actually parallels to the students versus non-students comparison. Eligible students are more likely to work in sales and perform tasks classified as less than professional. In contrast, ineligible (rich) students are overrepresented in jobs that perform professional tasks.

Despite these parallels to the earlier comparison, the differences between eligible and ineligible students are much smaller. A particularly important point is that the baseline employment rates

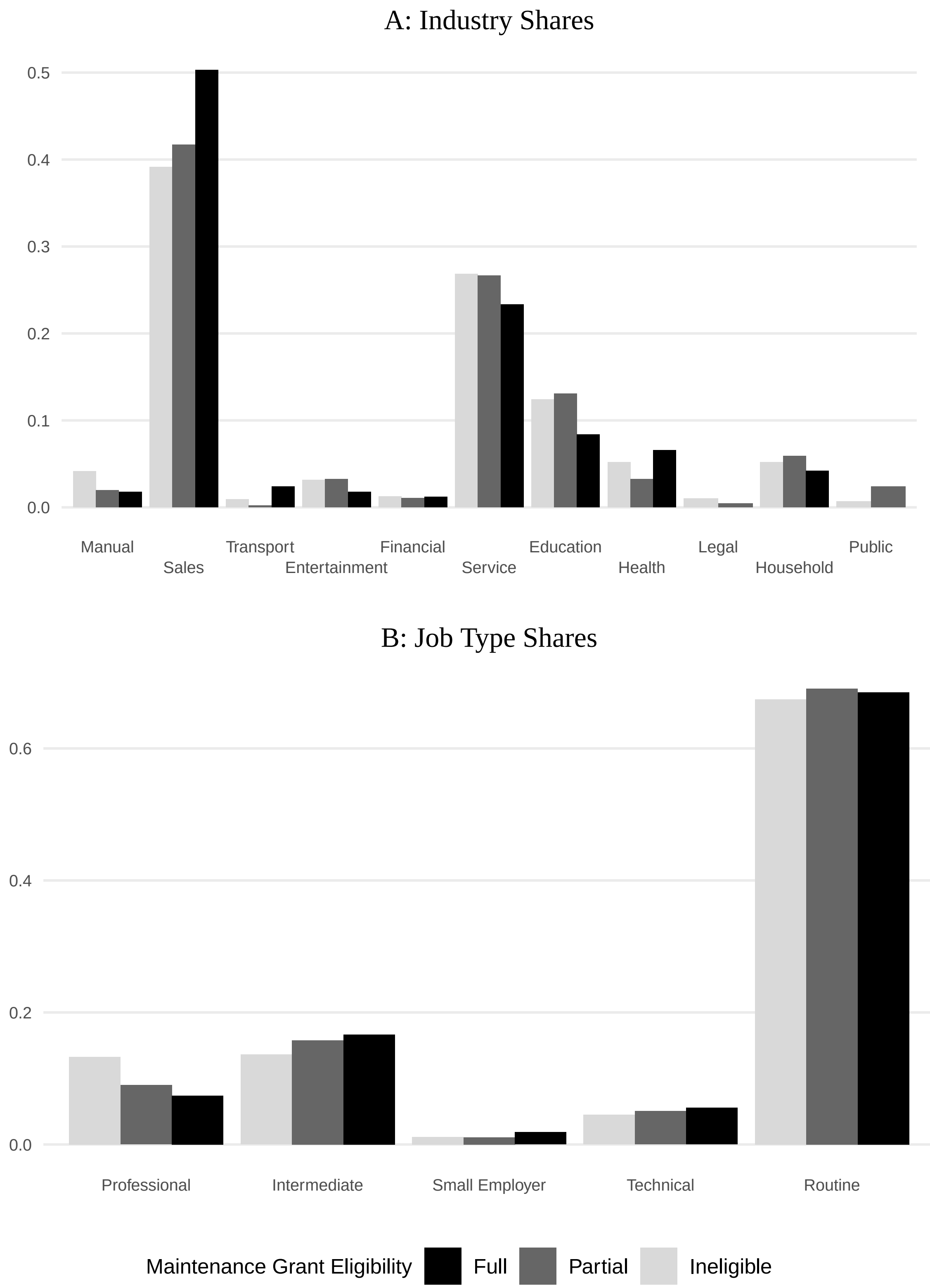

Figure 6: Industry and Job Type Shares of Employed Students

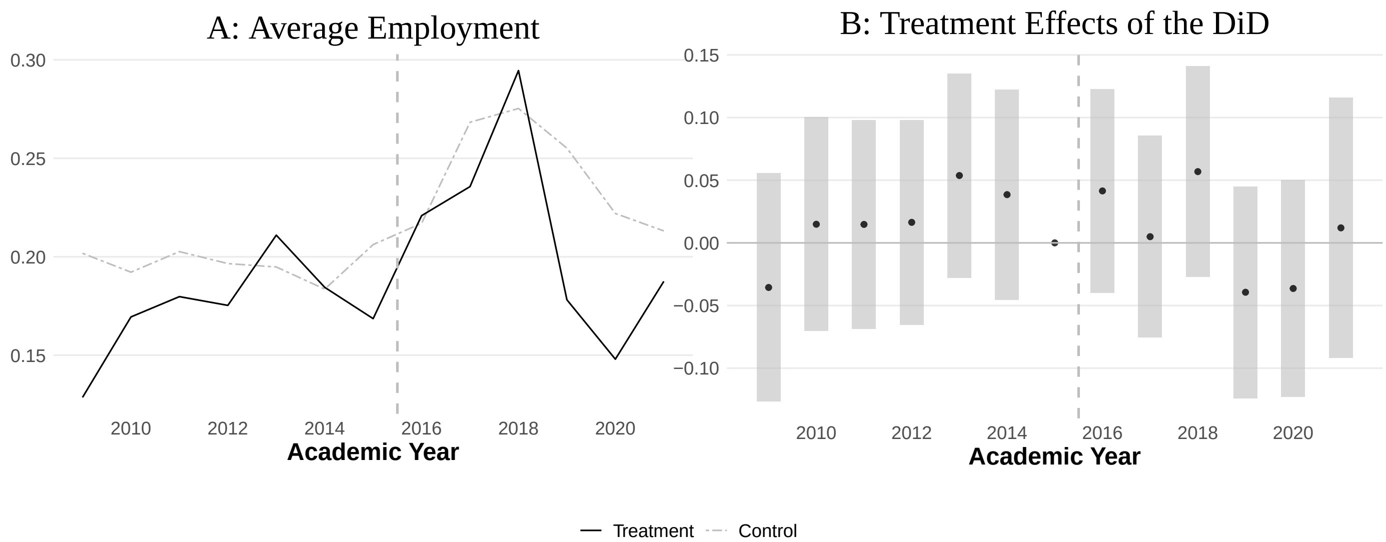

Figure 7: Paid Employment of Eligible and Ineligible Students

of eligible and ineligible students are very similar. This implies that the parallel trends assumption can simultaneously hold in absolute and relative terms.

The DiD analysis shows that employment of eligible students did not increase significantly more than that of ineligible students. The classic DiD model estimates that employment of eligible students increases by 1.6 percentage points more, but the estimate is noisy (Table 3). The event study estimates confirm this conclusion. The difference in employment is highest in 2018 at roughly 5 percentage points, but all estimates are clearly insignificant (Figure 7).

There are two takeaways compared to my previous results. First, the increase in student employment is at least partially experienced by all students, not just the eligible ones. The maintenance grant removal can therefore not explain the entire increase in student employment that I showed earlier. Second, looking only at students decreases my sample size and increases my standard errors. This makes it hard to understand if the grant removal can explain the additional 1.6 percentage point increase in employment of eligible students, or if the difference is just noise. On the other hand, the parallel trends assumption is more reasonable in the eligibility comparison. The event study estimates show parallel pre-trends, not only because of bigger standard errors, but because the estimates are mostly closer to zero.

The final piece of my employment analysis combines the previous two comparisons. I use a triple differences design to compare eligible and ineligible students, but adjust this difference by the employment trends of eligible and ineligible people who never attend university. This allows me to drop the classic parallel trends assumption for a weaker version, that requires that the difference in trends between eligible and ineligible students is the same among those who study, and those who don't. The exact model is:

$$\begin{aligned}\text{Employment} = \text{Constant} &+ \beta_1 \text{Eligible} \times \text{Student} \times \text{Post-Treatment} + \beta_2 \text{Eligible} \\ &\times \text{Student} + \beta_3 \text{Eligible} \times \text{Post-Treatment} + \beta_4 \text{Student} \\ &\times \text{Post-Treatment} + \beta_5 \text{Eligible} + \beta_6 \text{Student} + \beta_7 \text{Post-Treatment} + \epsilon\end{aligned}$$

Also the triple differences model fails to confirm a significant increase in employment for eligible students (Figure 8). In fact, it estimates that removing the grant decreased the employment of eligible students. The estimate is also noisy, and the negative effect is very insignificant. Splitting up the aggregate estimate with an event study reveals that the negative point estimate is produced by a mixture of positive and negative yearly estimates, all of them insignificant. Hence, neither the DiD nor the triple differences model clearly shows that removing the maintenance grant increased the employment rate of eligible students.

Table 3: Regression Results on Employment

	Students vs Non-Students	Eligible vs Ineligible	Triple Difference
	(1)	(2)	(3)
Treatment Effect	0.035	0.016	−0.017
	(0.012, 0.058)	(−0.025, 0.057)	(−0.075, 0.042)
Observations	61,941	7,069	17,802

Note: 95%-confidence intervals in parentheses.
Full event study coefficients in the appendix.

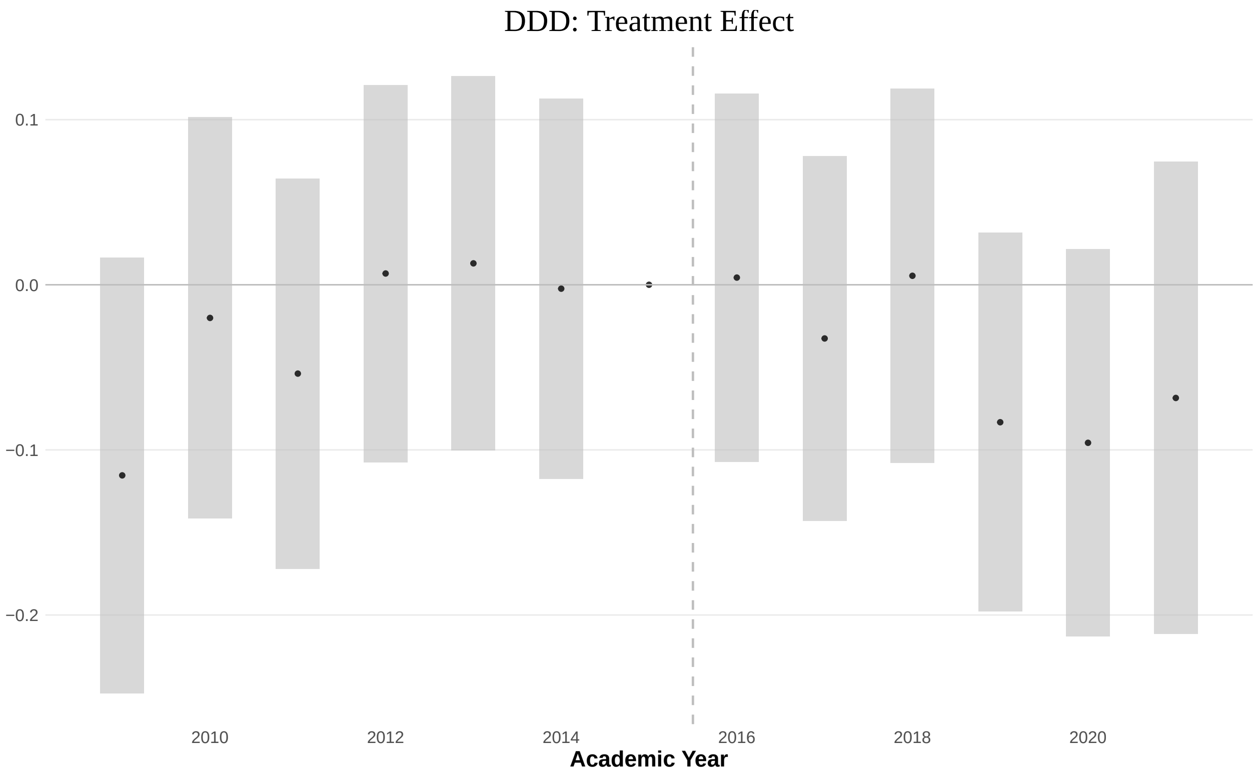

Figure 8: Triple Differences Estimates for Employment

Other Outcomes

Even if removing the maintenance grant did not increase employment it could still have influenced students' work choices. Employment only reflects the decision whether to work (the extensive margin), but students may have decided to work more or in other jobs (the intensive margin). To measure reactions along the intensive margins, I will repeat my analysis looking at weekly working hours and labor income.

I observe both outcomes only if a person is employed, unemployed people appear as missing values. Hence, both outcomes reveal something about the intensive margin but are also impacted by the extensive margin, and I cannot disentangle the two. Furthermore, because of the missing values only employed people are part of the regression sample and my estimates become much noisier.

The DiD design shows that the effect of the grant removal on labor income was similar to its effect on employment (Figure 9). Relative to those who don't study average student income increased by 10% (Table 4), and the aggregate DiD estimate hides dynamic effects of up to roughly 30%. But just as with employment, several of the pre-treatment event study estimates are almost significantly different from zero. In particular in 2013 and 2014 the treatment group started to experience higher income growth than the control group, which makes it difficult to assume parallel trends.

Comparing eligible and ineligible students does not confirm the increase in labor income. Income of eligible students increases by statistically insignificant 6% (standard error is 8.5%). The triple differences design confirms this conclusion with a highly insignificant estimate of -2%.

Weekly working hours also didn't change after the grant was removed (Figure 10). The DiD design estimates that on average students worked 1.2 additional hours after the grant removal but the event study shows that students' hours had been increasing at a higher rate already before 2016. Comparing eligible and ineligible students or using the triple differences design both suggest that hours remained constant after 2016.

The evidence for a change in hours or earnings is even weaker than the evidence on employment, but both outcomes seem to be not affected. Two things make it difficult to be certain about the impact though. First, standard errors are large. Second, I already mentioned that changes in hours and earnings can result from the extensive or intensive margin. It is possible that students who worked before 2016 increased their hours, and students who didn't work before 2016 started working only a few hours. The two impacts could then result in no change in hours on average.

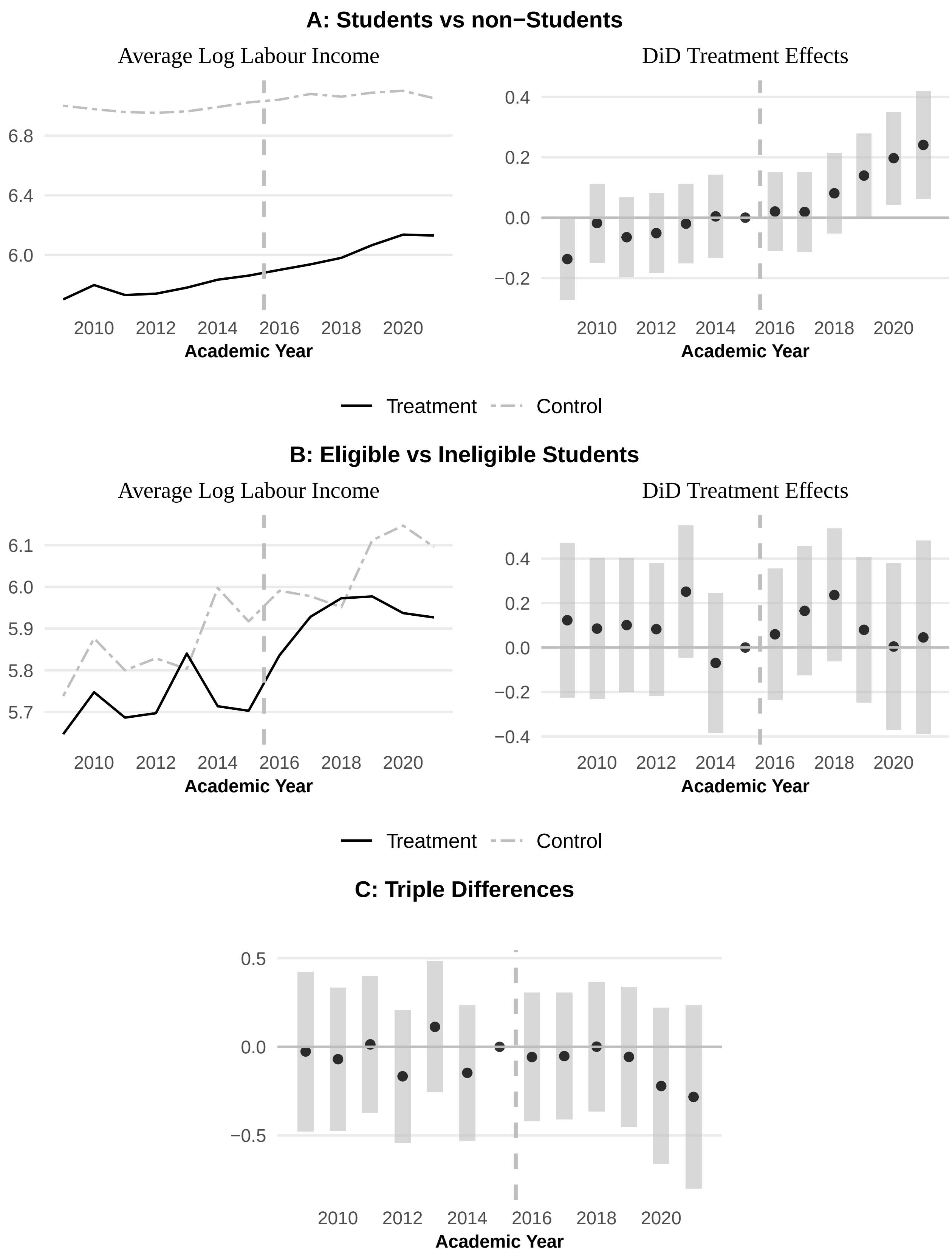

Figure 9: Results for the Labor Income Analysis

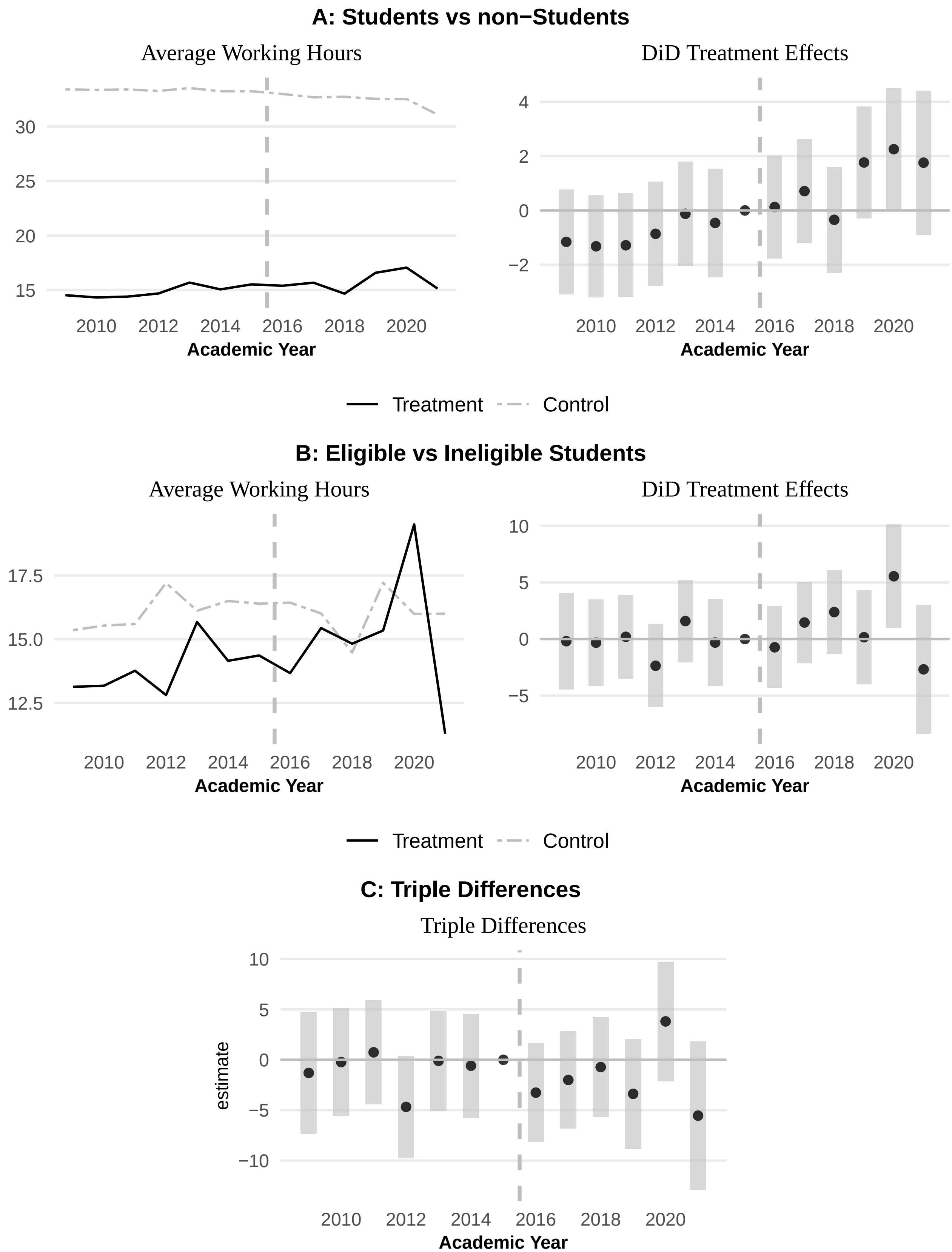

Figure 10: Results for the Working Hours Analysis

Table 4: Regression Results on Log Labor Income and Weekly Working Hours

	Students vs Non-Students	Eligible vs Ineligible	Triple Difference
	(1)	(2)	(3)
Panel A: Log Labor Income			
Treatment Effect	0.080	0.070	−0.067
	(0.016, 0.145)	(−0.073, 0.213)	(−0.257, 0.122)
Observations	44,103	2,837	10,553
Panel B: Hours Worked			
Treatment Effect	0.921	1.272	−1.781
	(−0.014, 1.857)	(−0.457, 3.000)	(−4.297, 0.735)
Observations	42,347	2,426	9,660

Note: 95%-confidence intervals in parentheses. Full event study coefficients in the appendix.

Enrollment

So far, my analysis has focused on how removing the maintenance grant influenced student employment. But to make sure that my analysis is valid, it is important that removing the grant did not influence whether a person becomes part of my sample. Or in other words, whether they enroll in university. The DiD design accounts for enrollment changes common to everybody, but if only eligible people changed their enrollment, my analysis would suffer from selection bias.

Let me illustrate why. Imagine a type of student that never considers working to support his studies. After the grant is removed all students of that type stop enrolling, and the employment rate among the remaining students rises. This is not because any student decided to start working, but simply because unemployed students left the sample. Crucially, this only happens to the group of eligible students. Among the ineligible students, that type of student never enrolled in the first place, and hence their employment doesn't change.

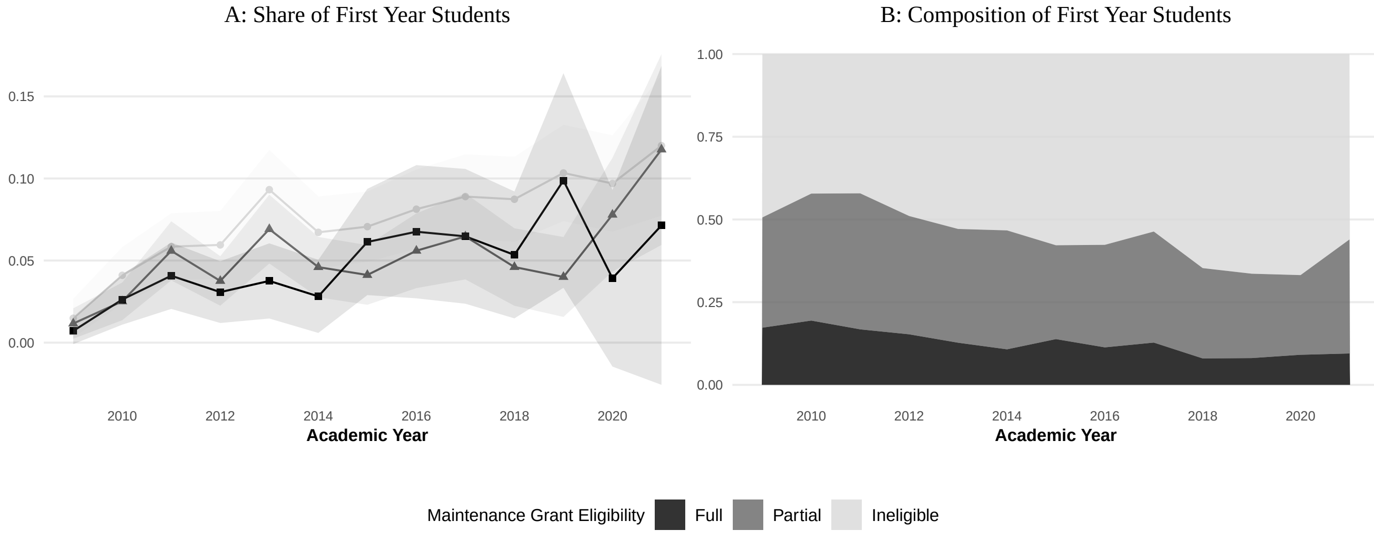

Figure 11: First Year Students 2009-2021

Overall enrollment has been increasing over the entire time span of my analysis and irrespectively of students' financial backgrounds. But, the enrollment gap between eligible and ineligible students has widened (Figure 11). In 2009, 50% of all first-year students would have been ineligible for the 2015 maintenance grant, but recently, this number has risen to 70%. Of course, this could also result from rising incomes overall.

I can test for selection bias by checking whether the enrollment rate of eligible potential students has changed in particular after the maintenance grant was removed. I define potential students as 17 and 18-year-olds because they face the choice to enroll in university for the first time in the year of observation. When older people enroll, this may be the result of delaying enrollment at 18 years to work and save up money. I split the potential students into eligible and ineligible and use the same DiD method as before.

Fortunately, the enrollment rate of eligible potential students is not affected by the grant removal. The DiD design estimates a decrease by very insignificant increase of 0.6 percentage points (Table 5). I repeat my analysis with 19-year-olds and 20-year-olds for robustness, and also estimate the event study design (Figure 12). All specifications confirm no change in enrollment after 2016, and show that before 2016 enrollment rates were moving in parallel. As all pieces of evidence

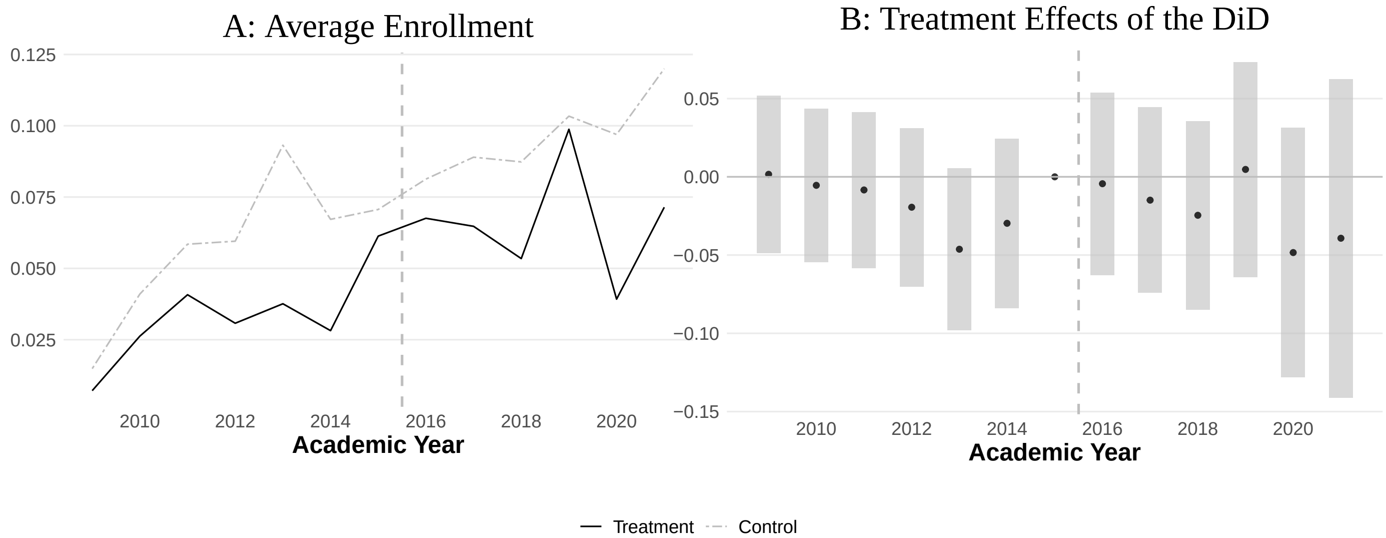

Figure 12: Enrollment of Eligible and Ineligible Potential Students

suggest that enrollment rates have remained constant, I am confident that my analysis of student employment is not affected by selection bias.

Table 5: Regression Results on Enrollment

	17 and 18 Year Olds	19 Year Olds	20 Year Olds
	(1)	(2)	(3)
Treatment Effect	0.006	0.051	−0.018
	(−0.022, 0.033)	(−0.018, 0.121)	(−0.076, 0.040)
Observations	7,613	3,294	3,051

Note: 95%-confidence intervals in parentheses.
Full event study coefficients in the appendix.

Conclusion

My analysis was motivated by a large increase in student employment after the maintenance grant was removed. I show that removing the grant did not affect enrollment rates, which means that my analysis of student employment is not distorted by selection bias. But although student employment increased compared to the regular labor market, the increase is common to both eligible and ineligible students. Therefore, I do not find evidence of a causal relationship.

My findings relate to Keane and Wolpin (2001) in an interesting way. Keane and Wolpin showed that borrowing constraints push students into working during university, and that if the constraints were relaxed, students would work less. But in England borrowing constraints for students are very weak. My findings support their logic by showing that when poor students are able to borrow, they are not pushed into working during university.

There are also a couple of limitations that apply to my analysis. I do not observe how much students borrow, so I don't know how students actually react to the maintenance grant removal. I argued above that they might replace the grant money with a larger maintenance loan. But it is also possible that they simply reduce their living expenses, or receive more support from their parents.

Moreover, few students take part in the Understanding Society survey and my estimates are imprecise. While I cannot prove that removing the maintenance grant increased employment, my estimates are too imprecise to be certain that the removal had no effect. In other words, it is difficult to conclude from a lack of evidence, to evidence for the lack of an effect.

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A — Appendix

Employment Analysis

Table A.1: DiD Results for Employment with Controls

	Students vs Non-Students
Treatment Effect	0.029
	(0.004, 0.055)
Observations	35,737
Note:	95%-confidence intervals in parentheses.

Table A.2: Treatment Effects of the Employment Event Studies

	Students vs Non-Students	Eligible vs Ineligible	Triple Difference
	(1)	(2)	(3)
2009	0.032	−0.036	−0.115
	(−0.011, 0.075)	(−0.127, 0.055)	(−0.247, 0.017)
2010	0.032	0.015	−0.020
	(−0.011, 0.074)	(−0.070, 0.100)	(−0.142, 0.101)
2011	0.037	0.015	−0.054
	(−0.007, 0.081)	(−0.069, 0.098)	(−0.172, 0.064)
2012	0.031	0.016	0.007
	(−0.014, 0.075)	(−0.065, 0.098)	(−0.107, 0.121)
2013	0.044	0.054	0.013
	(−0.001, 0.089)	(−0.028, 0.135)	(−0.100, 0.126)
2014	0.006	0.038	−0.002
	(−0.040, 0.052)	(−0.045, 0.122)	(−0.117, 0.113)
2016	0.042	0.041	0.004
	(−0.003, 0.087)	(−0.040, 0.123)	(−0.107, 0.116)
2017	0.058	0.005	−0.033
	(0.012, 0.104)	(−0.076, 0.085)	(−0.143, 0.078)
2018	0.092	0.057	0.005
	(0.045, 0.140)	(−0.027, 0.141)	(−0.108, 0.119)
2019	0.052	−0.039	−0.083
	(0.004, 0.099)	(−0.124, 0.045)	(−0.198, 0.031)
2020	0.071	−0.036	−0.096
	(0.023, 0.119)	(−0.123, 0.050)	(−0.213, 0.022)
2021	0.047	0.012	−0.069
	(−0.011, 0.105)	(−0.092, 0.116)	(−0.212, 0.074)
Observations	71,409	8,852	22,793

Note: 95%-confidence intervals in parentheses.

Table A.3: Treatment Effects of the Labor Earnings Event Studies

	Students vs Non-Students	Eligible vs Ineligible	Triple Difference
	(1)	(2)	(3)
2009	−0.137	0.123	−0.026
	(−0.271, −0.004)	(−0.225, 0.470)	(−0.477, 0.424)
2010	−0.018	0.085	−0.070
	(−0.149, 0.113)	(−0.231, 0.401)	(−0.473, 0.334)
2011	−0.065	0.101	0.014
	(−0.196, 0.067)	(−0.200, 0.402)	(−0.371, 0.398)
2012	−0.051	0.083	−0.166
	(−0.183, 0.081)	(−0.216, 0.381)	(−0.541, 0.208)
2013	−0.020	0.251	0.113
	(−0.152, 0.112)	(−0.046, 0.548)	(−0.257, 0.482)
2014	0.004	−0.069	−0.147
	(−0.133, 0.142)	(−0.383, 0.244)	(−0.530, 0.237)
2016	0.020	0.059	−0.058
	(−0.110, 0.150)	(−0.235, 0.354)	(−0.421, 0.306)
2017	0.019	0.165	−0.053
	(−0.113, 0.150)	(−0.125, 0.455)	(−0.410, 0.305)
2018	0.081	0.236	0.001
	(−0.053, 0.214)	(−0.063, 0.534)	(−0.365, 0.366)
2019	0.139	0.080	−0.057
	(−0.0001, 0.279)	(−0.248, 0.407)	(−0.452, 0.337)
2020	0.197	0.004	−0.221
	(0.044, 0.351)	(−0.370, 0.379)	(−0.662, 0.220)
2021	0.241	0.045	−0.283
	(0.062, 0.420)	(−0.390, 0.481)	(−0.800, 0.234)
Observations	49,800	3,429	13,750

Note: 95%-confidence intervals in parentheses.

Table A.4: Treatment Effects of the Hours Worked Event Studies

	Students vs Non-Students	Eligible vs Ineligible	Triple Difference
	(1)	(2)	(3)
2009	−1.157	−0.189	−1.307
	(−3.086, 0.773)	(−4.448, 4.070)	(−7.354, 4.741)
2010	−1.321	−0.322	−0.227
	(−3.204, 0.563)	(−4.150, 3.507)	(−5.599, 5.146)
2011	−1.283	0.200	0.739
	(−3.191, 0.626)	(−3.493, 3.892)	(−4.422, 5.900)
2012	−0.860	−2.361	−4.676
	(−2.774, 1.054)	(−6.009, 1.288)	(−9.698, 0.345)
2013	−0.124	1.591	−0.108
	(−2.039, 1.792)	(−2.046, 5.228)	(−5.076, 4.861)
2014	−0.459	−0.309	−0.594
	(−2.460, 1.542)	(−4.152, 3.534)	(−5.752, 4.564)
2016	0.126	−0.728	−3.265
	(−1.769, 2.022)	(−4.345, 2.889)	(−8.147, 1.617)
2017	0.711	1.463	−2.006
	(−1.203, 2.624)	(−2.106, 5.031)	(−6.824, 2.813)
2018	−0.344	2.385	−0.729
	(−2.297, 1.608)	(−1.324, 6.095)	(−5.700, 4.243)
2019	1.766	0.163	−3.384
	(−0.298, 3.829)	(−3.978, 4.305)	(−8.828, 2.060)
2020	2.255	5.550	3.807
	(0.004, 4.507)	(0.969, 10.130)	(−2.123, 9.737)
2021	1.759	−2.676	−5.546
	(−0.897, 4.415)	(−8.366, 3.015)	(−12.900, 1.809)
Observations	47,539	2,901	12,602

Note: 95%-confidence intervals in parentheses.

Enrollment Analysis

Table A.5: Demographics and Socioeconomic Background of Potential Students

	Maintenance Grant Eligibility					
	Ineligible		Partial		Full	
Enrolled	No	Yes	No	Yes	No	Yes
Age	17.46	17.95	17.45	17.96	17.44	17.95
Share Male	0.49	0.46	0.49	0.37	0.51	0.47
Employed	0.32	0.15	0.28	0.19	0.18	0.11
Employed Parent	0.98	0.98	0.93	0.95	0.87	0.88
HH Income Parents (£)	5661.50	7090.20	2936.93	3131.64	1681.45	1633.48

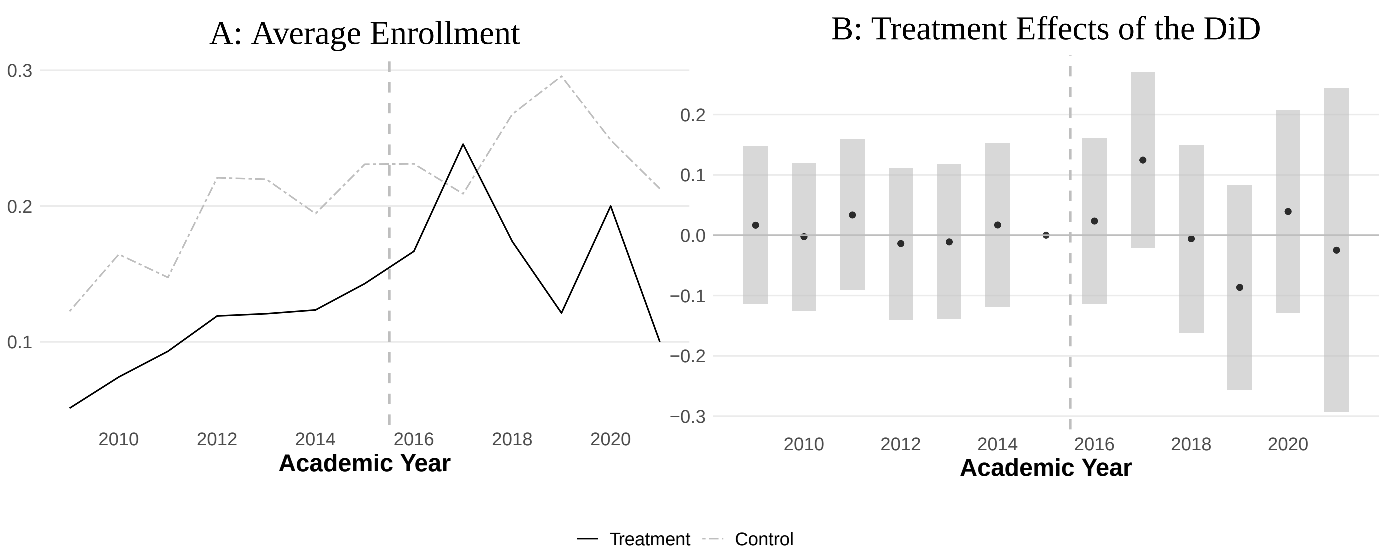

Figure A.1: Enrollment of Eligible and Ineligible 19 Year Olds

Table A.6: Treatment Effects of the Enrollment Event Studies

	17 and 18 Year Olds	19 Year Olds	20 Year Olds
	(1)	(2)	(3)
2009	0.002	0.017	−0.035
	(0.026)	(0.066)	(0.054)
2010	−0.005	−0.002	0.021
	(0.025)	(0.062)	(0.049)
2011	−0.008	0.034	−0.012
	(0.025)	(0.064)	(0.050)
2012	−0.019	−0.014	−0.018
	(0.026)	(0.064)	(0.050)
2013	−0.046	−0.011	0.005
	(0.026)	(0.065)	(0.051)
2014	−0.030	0.017	−0.012
	(0.028)	(0.069)	(0.055)
2016	−0.004	0.023	−0.073
	(0.030)	(0.070)	(0.057)
2017	−0.015	0.125	0.001
	(0.030)	(0.074)	(0.058)
2018	−0.025	−0.006	0.006
	(0.031)	(0.079)	(0.065)
2019	0.005	−0.087	0.035
	(0.035)	(0.086)	(0.066)
2020	−0.048	0.039	0.108
	(0.041)	(0.086)	(0.071)
2021	−0.039	−0.025	0.136
	(0.052)	(0.137)	(0.088)
Observations	8,806	3,881	3,645

Note: NA

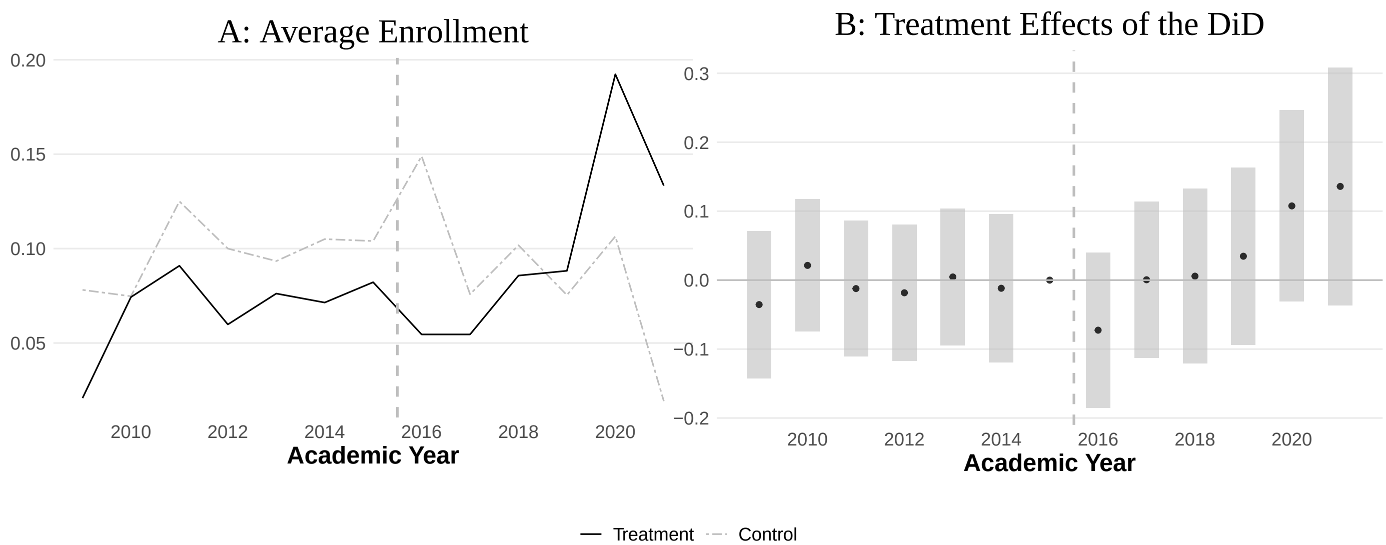

Figure A.2: Enrollment of Eligible and Ineligible 20 Year Olds