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Who bears the brunt: Tuition fees and educational mismatch

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Abstract: Exploiting the quasi-experimental introduction of tuition fees in selected federal states of Germany between 2006 and 2014, this study investigates how these fees affect the likelihood of overeducation based on data from the SOEP. Reporting lower bound estimates, the findings show that graduates from fee-charging states are significantly more likely to experience overeducation. There is suggestive evidence that the Intention to Treat Effect (ITT) may be heterogeneous depending on occupational choice after graduation, socioeconomic background and duration of the treatment. Moreover, dynamic analyses reveal that the increase in the likelihood of overeducation due to tuition fee exposure is not short-term but persists for even up to ten years after degree completion.

Keywords: Tuition fees; Overeducation; Quasi-experiment; Germany

JEL Classification: I21, I23, I28

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1 Introduction

Access to higher education is often shaped by financial barriers, making it a key concern in both education economics and policy. Human Capital Theory suggests that individuals invest in education to yield higher future earnings (Becker, 1975). However, higher education costs can pose substantial obstacles to human capital accumulation, particularly for individuals with limited financial resources and borrowing constraints. This is particularly relevant in countries such as the US which rely heavily on tuition fees to finance the tertiary education system.[1] This is supported by numerous studies showing that student subsidies, loans, and grants are important measures to buffer the consequences of tuition fees, and might help to equalise opportunities among students from different backgrounds (e.g., Bettinger et al., 2019; Castleman and Long, 2016; Denning, 2019; Dynarski, 2008; Dynarski et al., 2021; Fack and Grenet, 2015; Page et al., 2019; Solis, 2017). In contrast, less is known about the effects of introducing tuition fees in countries where higher education has historically been free. As one of these states, Germany offers a unique example where tuition fees of EUR 500 were temporarily introduced in seven federal states between 2006 and 2014, while all other federal states kept tertiary education free of charge.[2]

Using this quasi-experimental setting, previous research has focused on the impact of tuition fees on enrolment rates and intentions, educational attainment in the population and student mobility (Bahrs and Siedler, 2019; Bietenbeck et al., 2023; Bruckmeier et al., 2013; Dwenger et al., 2012; Hübner, 2012; Minor, 2023). However, long-term consequences are less researched. Some studies suggest that the monetary pressure caused by tuition fees could influence individuals' career choices (e.g., Rothstein and Rouse, 2011). Such an impact on career choices might induce them to accept jobs below their qualifications at higher rates, for example, to accelerate the transition between university and labour market entry (Gervais and Ziebarth, 2019; Minicozzi, 2005). In addition to this mechanism, tuition fees could also deter academic performance or extracurricular engagement, which are important signals in the application process. Weaker signals could, in turn, reduce employment prospects and increase the likelihood of working in jobs that require less education than the attained level (Spence, 1973; Thurow, 1975). If these mechanisms materialised, this could imply substantial long-term consequences for individuals as well as firms and might produce inefficiencies in the labour market. However, empirical studies on

[1]According to the OECD, the annual average costs amounted to USD 9,596 for Bachelor students at public universities and USD 34,041 at private institutions in the US in the academic year of 2022/2023 (for details see Table C5.1 in OECD, 2024).

[2]Importantly, tuition fees are paid directly to the university and differ from term contributions (*Semesterbeitrag*). The latter generally cover administrative costs such as student union contributions or public transport fees and are charged regardless of tuition fees.

such a link are missing. Hence, a potential, important consequence of tuition fees has not yet been investigated.

This study fills this gap by asking: First, do tuition fees affect individuals' likelihood of working in jobs below their qualifications post-graduation? Second, does this link vary across subgroups? And third, does it persist over time? To answer these questions, I operationalise jobs below individuals' qualifications using the concept of overeducation.[3] Overeducation forms part of underemployment (Feldman, 1996) and defines the state in which individuals' formal education exceeds the job requirements (Freeman, 1976).[4] Moreover, I rely on the quasi-experimental setting in Germany to identify the impact of tuition fees and use data on graduates from the Socio-Economic Panel for the years 1985 to 2021 (DIW, 2024; Goebel et al., 2019). The evidence reveals that affected graduates are indeed significantly more likely to be overeducated post-graduation. This result holds robust to a series of sensitivity checks, and concerns regarding sources of selection biases are tackled. Suggestive evidence indicates that the treatment effect is less pronounced among individuals in high-skill occupations, with one parent owning upper secondary education or those treated for a shorter period. Finally, by leveraging the panel structure of the SOEP, this study shows that individuals exposed to tuition fees report a higher likelihood of overeducation not only in the short run but also in the ten years following graduation.

This study adds to the literature in three ways. First, the study extends our knowledge of the consequences of higher education costs. Most related studies evaluate the role of subsidies (e.g., Castleman and Long, 2016; Denning, 2019; Dynarski, 2008; Dynarski et al., 2021; Fack and Grenet, 2015; Marx and Turner, 2019; Monks, 2001; Page et al., 2019; Solis, 2017)[5] or tuition fees (e.g., Andrews and Stange, 2019; Garibaldi et al., 2012; Hassani-Nezhad et al., 2021; Molina and Rivadeneyra, 2021; Neill, 2009) in countries where tuition fees are common. In contrast, the present study evaluates the introduction of tuition fees in Germany, where higher education was generally free before the intervention. This enables an analysis of tuition fees as a binary policy change. Moreover, by linking this reform to post-graduate overeducation, the study expands on previous research by Bahrs and Siedler (2019), Bietenbeck et al. (2023), Bruckmeier et al. (2013), Dwenger et al. (2012), Fischer and Wigger (2016), Hübner (2012), and Minor (2023) that has largely focused on

[3]This study focuses on the likelihood of overeducation. Undereducation is not considered because it is, by definition, highly unlikely among graduates of tertiary education who were affected by the policy change.

[4]Distinct concepts cover overskilling or horizontal educational mismatches, focusing either on whether individuals' skills exceed the job requirements or whether individuals work in positions outside of their field of study (McGuinness et al., 2018; Robst, 1995, 2007).

[5]Other studies evaluate the impact of support systems to finance education in primary or secondary education (e.g., Angrist et al., 2002; Burlando, 2023).

pre-graduation measures like enrolment decisions and educational intentions.

Second, it complements the literature on the antecedents of overeducation, which has, to a large extent, focused on demographic or job-related factors (e.g., Aleksynska and Tritah, 2013; Battu et al., 1999; Belfield, 2010; Blásquez and Budría, 2012; Caroleo and Pastore, 2018; Carroll and Tani, 2015; Davia et al., 2017; Diem, 2015; Diem and Wolter, 2014; Dolton and Silles, 2008; Fleming and Kler, 2008; Turmo-Garuz and Bartual-Figueras, 2019; Verhaest and Omey, 2010). This study extends this strand of literature by leveraging a quasi-experimental setting that enables the application of more nuanced research designs. While the available data and setting do not allow causal claims, such quasi-experimental approaches are rare in the educational mismatch literature and provide an important extension to predominantly correlational analyses.[6]

Third, the dynamic analysis provided at the end of this study builds on the literature evaluating Career Mobility Theory as an explanation of overeducation. Following this theory, individuals might enter overeducation after graduation to acquire working experience and further training, allowing them to proceed to better-matched jobs in the aftermath (Sicherman, 1991; Sicherman and Galor, 1990). Previous research has mainly estimated whether individuals in overeducation are more likely to participate in training measures, to switch their jobs, or to stay in overeducation for a longer period (e.g., Baert et al., 2013; Büchel and Mertens, 2004; Grunau and Pecoraro, 2017; Kiersztyn, 2013; Roller et al., 2020; Sicherman, 1991; Wen and Maani, 2019). Adding to that, this study evaluates whether the increase in the likelihood of overeducation caused by tuition fees is a short-term phenomenon – helping individuals to transition to matched jobs – or whether it endures even several years after graduation – trapping individuals in overeducation.

The study proceeds as follows: Section 2 explains the institutional background and develops the expectations. Section 3 describes the dataset, the variables, summarises the sample, and explains the identification strategy. Section 4 comprises the main findings, a series of robustness checks, a discussion of potential selection issues, the heterogeneity analyses, and the dynamic analysis. Section 5 concludes the study.

[6] As an exception, Ordine and Rose (2017) investigate the link between the supply of graduates in the labour market and their likelihood of overeducation using a quasi-natural experiment in Italy.

2 Institutional background and expectations

2.1 Quasi-experimental setting

Studying in Germany has generally been free of charge at public universities since the early 1970s for first and consecutive study programmes. This Germany-wide tuition fee exemption was enshrined in the legislation in 2002 when the German parliament (*Bundestag*) passed a law banning tuition fees for all first and consecutive study programmes in Germany.[7] However, following a legal complaint by the federal states of Bavaria, Baden-Württemberg, Hamburg, Saarland, Saxony, and Saxony-Anhalt, the Federal Constitutional Court (*Bundesverfassungsgericht*) ruled that the law violated the German Constitution (*Grundgesetz*) that delegates control over the education system to federal states.[8] As a result of this ruling, seven out of the sixteen federal states in Germany introduced tuition fees of up to EUR 500 per term between 2006 and 2014.

Lower Saxony and North Rhine-Westphalia were the first states to implement fees in the winter term of 2006/07. Subsequently, Bavaria, Hamburg, and Baden-Württemberg followed in the summer term of 2007, and Hesse and Saarland in the winter term of 2007/08. Hesse abolished the fees again after two terms, while Lower Saxony was the last state to remove the fees after the summer term of 2014 and charged tuition fees for over 16 terms.[9] Hence, the periods for which the fees were active varied largely across the federal states (see Figure 1).[10]

This partial and time-wise introduction of tuition fees in Germany provides a quasi-experimental setting enabling to study the influence of education costs on students' labour market outcomes. The following section outlines the mechanisms through which tuition fees may affect these outcomes, particularly with regard to overeducation.

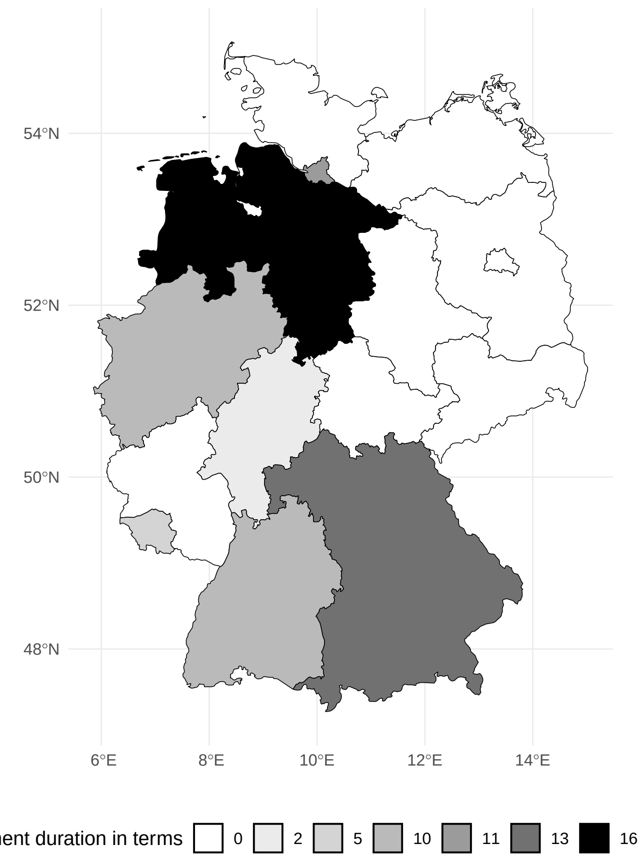
[7] Sechstes Gesetz zur Änderung des Hochschulrahmengesetzes (6. HRGÄndG, Sixth Law Amending the Higher Education Framework Act) Art. 1 Nr. 3.

[8] Bundesverfassungsgericht, Urteil des Zweiten Senats (Federal Constitutional Court, Verdict of the Second Senate), January 26, 2005 - 2 BvF 1/03.

[9] See e.g., Baden-Württemberg Landeshochschulgebührengesetz (Higher Education Fees Act), December 19, 2005, §§3-4, Bayerisches Hochschulgesetz (Bavarian Higher Education Act), May 23, 2006, Art. 71, Hamburgisches Hochschulgesetz (Hamburg Higher Education Act), September 4, 2006, §6b, Hessisches Studienbeitragsgesetz (Hessian Tuition Fees Act), October 16, 2006, §§1-3, Niedersächsisches Hochschulgesetz (Lower Saxony Higher Education Act), December 15, 2005, §11, Hochschulabgabengesetz NRW (Higher Education Fees Act NRW), March 21, 2006, §2, Saarländisches Hochschulgebührengesetz (Saarland Higher Education Fees Act), July 12, 2006, §2.

[10] After fees were abolished, many states maintained fees for individuals pursuing a second university degree or for long-term students. Still, such regulations are also implemented in states that never introduced tuition fees for first and consecutive studies between 2006 and 2014. Saxony, for example, regulates that students who exceed the standard period of study by more than four terms due to their own responsibility have to pay EUR 500 for each following term (Sächsisches Hochschulgesetz (Saxon Higher Education Act), May 31, 2023, §13 (2).

Figure 1: Tuition fees in German federal states



Note: The map displays the federal states of Germany. States that introduced tuition fees between 2006 and 2014 are displayed in distinct shades of grey. The shades are determined by the length of the treatment period counted in terms. The treatment states are: Lower Saxony (winter term 2006/07 - summer term 2014), North Rhine-Westphalia (winter term 2006/07 - summer term 2011), Bavaria (summer term 2007 - summer term 2013), Hamburg (summer term 2007 - summer term 2012), Baden-Württemberg (summer term 2007 - winter term 2011/12), Hesse (winter term 2007/08 - summer term 2008), and Saarland (winter term 2007/08 - winter term 2009/10). Darker shades represent a longer period. Federal states depicted in white never introduced tuition fees and therefore exhibit a treatment duration of zero.

2.2 Expectations

Assuming that the educational attainment of tertiary graduates is fixed, overeducation can generally arise through two mechanisms: First, individuals could voluntarily apply for jobs below their qualifications; Or second, firms could not hire individuals for jobs requiring their qualifications. This implies that overeducation can result from both the individual's and the firm's choices in the application process.

Regarding the individual's choice, previous research reported that monetary burdens, such as tuition fees or financial support mechanisms during studies, might influence occupational and labour market outcomes not only indirectly but also directly (see, e.g., Bettinger et al., 2019; Black et al., 2023; Field, 2009; Rothstein and Rouse, 2011; Zhang, 2013). Particularly, the financial burden of loan or grant repayments could motivate individuals to enter the labour market faster. Such acceleration could induce individuals to accept jobs that do not align with their qualifications at higher rates (Gervais and Ziebarth, 2019; Minicozzi, 2005). Molina and Rivadeneyra (2021) provide evidence in line with this supposition and link tuition fees to occupational choice. They examine the abolition of fees in Ecuador in 2008 and demonstrate that individuals transitioned to higher-skilled jobs at higher rates once fees were eliminated.[11] If such a pattern translated to the German case, and individuals affected by tuition fees systematically selected into lower-skilled jobs, this would directly increase overeducation rates among the treated individuals.

Regarding the firm's choice, the conceptual work of Lazear et al. (2018) and Dolado et al. (2000) combined with standard Signalling (Spence, 1973) and Job Competition Theory (Thurow, 1975) suggest that graduates' likelihood of overeducation rises with increasing competition from equally or slightly better-qualified graduates. The reason for this being that firms will select the best-qualified graduate for the vacant position. Thus, for tuition fees to affect the likelihood of overeducation through this mechanism, *ceteris paribus*, the applications of treated individuals would have to convey signals of lower quality compared to those of untreated individuals. This would require tuition fees to affect performance indicators, such as the GPA or time-to-completion, and that the amount of EUR 500 per term was sufficient to provoke such changes. One plausible behavioural response to the introduction of tuition fees, which may affect graduates' signals, is that students might seek to minimise their total expenses and accelerate their studies (cf., Bietenbeck et al., 2023; Garibaldi et al., 2012). This could affect academic outcomes if students decided to take more exams at a time to complete their studies faster. On the one hand, such an increase in speed could translate into less time for each exam and could induce higher stress levels, possibly harming academic performance. On the other hand, studying for multiple courses at a time may also generate synergies, potentially improving overall performance. Empirical evidence by Garibaldi et al. (2012) on Bocconi University in Italy shows that higher fees reduce the time-to-completion. However, they did not affect the dropouts or academic performance of students. Partly contrary to that, Fricke (2018) investigates a sudden increase in tuition fees in Switzerland and does not find an effect on the likelihood of on-time graduation or academic performance indicators. Overall, the empirical evidence does not suggest that the performance of graduates reduces with rising costs because they aim to complete their studies faster. Hence, this mechanism should not affect graduates signals and thus, their likelihood of overeducation.

Alternatively, affected students could also adjust their behaviour by seeking ad-

[11] This effect was observed primarily among individuals from higher socioeconomic backgrounds. Molina and Rivadeneyra (2021) relate this to two explanations: First, individuals from lower socioeconomic backgrounds faced relatively low fees even before their abolition, and second, disparities in pre-university educational quality might deter university enrolment among them.

ditional funding for their studies. This may include external sources (e.g., from family or student loans) but also increasing their labour income (cf., Denning, 2019; Kalenkoski and Pabilonia, 2010; Neill, 2015). Transfers by external sources, e.g., parents, could ease students' academic performance as they can spend their time studying and do not have to invest more hours into working. Empirically, anyhow, while students receiving such transfers were shown to work less, study more, and report a higher likelihood of graduating, their academic performance indeed tends to be worse (Bachmann and Boes, 2014; Bodvarsson and Walker, 2004; Hamilton, 2013; Sauer, 2004). As a result, since worse academic performance likely is a negative signal for firms in the application process, one could expect higher likelihoods of overeducation among treated individuals based on this mechanism. In contrast, external subsidies could incentivise students to perform better, particularly when scholarships or grants are tied to academic achievements (e.g., Bernal and Penney, 2019; Montalbán, 2023). While this would be expected to diminish the likelihood of overeducation, purely performance-based subsidies are less prevalent in Germany compared to countries like the United States. If individuals increased their financial resources by working longer hours instead, this would reduce the time available for studying, leisure, or both. This could potentially harm academic performance (see e.g., Baert et al., 2018; Body et al., 2014; Darolia, 2014; Kalenkoski and Pabilonia, 2010; McKee-Ryan et al., 2009; McVicar and McKee, 2002; Montmarquette et al., 2007; Oettinger, 1999; Stinebrickner and Stinebrickner, 2003) and raise individuals' likelihood of overeducation. From another perspective, though, acquiring labour market experience besides studying may also serve as a positive signal in the application process, potentially buffering the risk of overeducation, particularly if gathered in a field related to one's studies (cf., Diem and Wolter, 2014; Verhaest and Omey, 2010).

Beyond behavioural changes, tuition fees might also affect students psychologically, which in turn could be negatively linked to individuals' academic performance. Previous research, for example, found a link between student loans and worse psychological outcomes (see e.g., Walsemann et al., 2015; Zhang, 2013). Taking a more nuanced perspective in their literature review, McCloud and Bann (2019) suggest that not the height of the debt but more subjective measures such as financial concerns are linked to lower mental health. Notably, such psychological deterioration can impede academic performance and success (see e.g., Bruffaerts et al., 2018; Eisenberg et al., 2009). As this would again deter individuals' signals, the likelihood of overeducation could be expected to rise.

Additionally, even if tuition fees neither triggered behavioural nor psychological responses, changes in educational performance and, hence, the likelihood of overeducation could still stem from university-level improvements. If tuition fees rise, they can spend more money to buy better teaching materials, hire more qualified teachers or create better learning spaces. Such improvements could translate into individuals' academic performance and might diminish graduates' risk of overeducation. Anyhow, as with the individual behavioural responses, it remains unclear whether EUR 500 per term and student are sufficient to finance such improvements and if the time interval is sufficiently large to realise them.

To summarise, expectations regarding the impact of tuition fees on the likelihood of overeducation are ambiguous. Consistent with theory, the likelihood of overeducation could be influenced, either by individuals choosing other occupations or by being hired for other jobs. The latter mechanism would require that tuition fees influence individuals' signals in the application process. However, the direction of this relationship is unclear, as tuition fees could deter or improve the academic performance of graduates, depending on the behavioural and psychological responses of individuals and the educational spending of universities.

3 Data and methodology

Examining how the introduction of tuition fees affects graduates' overeducation requires data that enables the identification of the type, timing, and place of the obtained degree, as well as the labour situation after graduation. For this reason, I use data from the SOEP (DIW, 2024; Goebel et al., 2019), which allows for the parallel assessment of individuals' labour market outcomes and academic careers. Its panel structure enables the identification of students across different periods, including the treatment period. Finally, it provides information on the socioeconomic background of individuals, which is crucial when evaluating policies that monetarily affect students.

3.1 Dependent variable

To define overeducation, I follow Clogg and Shockey (1984) and Verdugo and Verdugo (1989) and use a data-driven approach (MEAN measure) to calculate the required level of education for a job. This measure bears two major advantages in the context of the present study: Firstly, the variable can be implemented by only requiring information on the years of education and occupation of individuals, thereby avoiding any further reduction in sample size. Secondly, the variable allows an adjustment for time trends in education caused, for example, by educational inflation and changing occupational requirements. Since the SOEP contains observations on individuals from four different decades, a time-adjusted measure prevents false conclusions based on contextual inconsistencies (for a discussion, see e.g., Capsada-Munsech, 2019; Verhaest and Omey, 2010).

To apply this measure, the average level of education obtained by workers in the same occupational group is defined as the reference value for the level of required education. As proposed by Blásquez and Budría (2012), the three-digit ISCO classification offers a sufficiently narrow differentiation of occupations, while still allowing for a sufficient size of clusters. Within these clusters, individuals are defined to be overeducated if their years of education x_{it} exceed the average years of education $\overline{x_{ot}}$ in occupation o in survey wave t by more than one standard deviation σ_{ot}. Everyone whose attained education falls within the one standard deviation range around the mean is considered matched (reference category). More formally:

$$OE_{ito} = \begin{cases} 1 & \text{if } x_{it} > \overline{x_{ot}} + \sigma_{ot} \\ 0 & \text{if } \overline{x_{ot}} - \sigma_{ot} < x_{it} < \overline{x_{ot}} + \sigma_{ot} \end{cases} \quad (1)$$

3.2 Treatment

Individuals are defined to be treated if they obtained their degree in a state that charged fees in the respective period. Therefore, I first extract the year and month in which the degree was obtained from the data.[12] Furthermore, the definition of the treatment variable requires information on the state in which an individual obtained the degree. The dataset contains direct information on this for a subsample of individuals. For the remaining individuals, I look at the federal state of residence in the survey wave in which the degree was obtained.[13] Two critical assumptions are that individuals did not commute to university and that their indicated place of residence equals their true place of residence. If these assumptions were violated, individuals would be considered treated, although they did not pay tuition fees. Such inclusion of non-treated individuals in the treatment group would imply a non-differential misclassification. This would bias the estimated treatment effect toward zero. Hence, the estimates likely represent conservative lower bounds of the true effect (see also e.g., Bahrs and Siedler, 2019; Bietenbeck et al., 2023; Hübner, 2012). However, to properly account for this and as the pattern cannot be tested empirically, the effects are referred to as Intention to Treat Effects (ITT).[14]

Combining information on the timing of the degree and the federal state in which the degree was obtained, I can identify individuals affected by the tuition fee

[12] Direct information on this is available only for a subset of individuals. The remaining respondents are asked each year whether they received a degree since the previous year, and if so, what kind of degree they completed in which month.

[13] Note that this reduces the sample to those for whom direct information is available or to people who participated in the survey in the year in which they graduated, resulting in a small sample size.

[14] For a formalisation on how the “home bias” and spill-over effects threaten the identification of the true Average Treatment Effect on the Treated see Hübner (2012).

legislation. These individuals form the treatment group, whereas those who obtained a degree outside this period or the treatment states form part of the control group.

3.3 Covariates

The estimations include several covariates that are arguably determined before degree completion and related to both the likelihood of overeducation and of being treated. The first set contains demographic covariates, including age, sex, and migration background. Additionally, I include proxies for socioeconomic background. These cover, firstly, the education of one's father and mother as they may simultaneously impact the likelihood of studying and post-graduation labour market outcomes. Moreover, parents' educational background arguably proxies the financial resources of young adults (e.g., De Haan, 2011; Dickson et al., 2016; Sikhova, 2023). Secondly, the number of siblings is considered to proxy the distribution of economic resources within families (Downey, 1995). Regional variation is accounted for through fixed effects covering the federal state of residence and the federal state where the individual obtained the degree. Finally, I include survey year fixed effects to rule out time effects.[15]

3.4 Sample

Treatment is conditional on visiting a university or college, restricting the sample to graduates. Moreover, I exclusively keep individuals with valid information on the dependent variable. This implies that I drop unemployed or self-employed individuals. Additionally, individuals who pursue studies later in life, for example, in part-time or after having started to work in the labour market, are likely less affected by the monetary burden of tuition fees. Therefore, I restrict the sample to individuals aged 35 and under at the time of degree completion. Also, I exclude the lower 2.5% of the age-at-degree distribution, restricting the sample to individuals who completed their degrees at age 19 at the earliest, as it is uncommon to complete a tertiary degree at even younger ages. As the treatment is time-invariant within individuals, I cannot account for individual time-invariant unobserved heterogeneity. For this reason, the main sample is restricted to the first observation per individual after the degree was obtained. Finally, I make sure that all observed persons have obtained their degree at least one year before the respective survey wave. This avoids overeducation being detected in periods when people are still studying but are already working part-time alongside their studies.

Following this approach, I observe one to at most three treated individuals in

[15] The year in which the degree was obtained is not included in the analysis due to its near-perfect correlation with the survey year ($\rho = 0.989^{**}$).

Saarland, Hesse, and Hamburg. To avoid biases stemming from this pattern, I only consider the control observations for these federal states. As a result, the treatment effect is identified based on individuals who graduated in Bavaria, Baden-Württemberg, Lower Saxony, and North Rhine-Westphalia. All individuals with missing values are dropped.

3.5 Descriptives

The sample composition is displayed in Table 1. Within the sample, 40% of the individuals are overeducated. This number differs from studies relying on German panel data (e.g., Bauer, 2002; Blásquez and Budría, 2012) due to the restriction to graduates, among whom overeducation is, by definition, most likely. Moreover, 11% of the respondents are treated.

The sample is well-balanced regarding sex, as 48% of the respondents are female. The individuals are born between 1953 and 1997, yielding an age range at the time of the survey from 20 to 43 years. Notably, the average age at degree completion is 25.35, aligning with administrative data from the Federal Statistical Office noting the average age at study completion (master's degree) to be 26.5 (Statistisches Bundesamt, 2024b). Individuals have between 0 to 7 siblings, with an average of slightly above 1. The vast majority of them (93%) are natives. 3% (4%) of the individuals have a direct (indirect) migration background. Regarding parental education, I observe 1% of fathers and mothers with no education, 32% (33%) with general education, 28% (41%) with secondary education, and 39% (25%) with upper secondary education degrees.

To identify the effect of tuition fees on the likelihood of overeducation, individuals in treatment and control states should be comparable before the policy change. Otherwise, the estimates might be influenced by factors independent of the actual intervention. For this reason, Table 2 compares the differences in means between treated and control states exclusively using the observations of individuals who obtained their degrees before the first tuition fee introduction in 2006.[16] This causes a reduced sample size compared to Table 1 and implies that Table 2 relies on control group observations exclusively. The results indicate no statistically significant differences in the majority of variables. Still, individuals in the treatment states tend to have a slightly higher number of siblings on average. While this difference is statistically significant, it remains relatively minor, with the average number of siblings being between 1 and 1.5 in both groups. Moreover, the proportion of indi-

[16]Note that individuals are observed at least one year after graduation, such that 2006 would be expected to be the last year of observation in Table 2. However, for few individuals there is a time lack between the year in which they graduated and that in which they are observed for the first time after graduation. Hence, Table 2 covers the years 1985 to 2012.

Table 1: Summary statistics

	N	Mean	Min	Max
Overeducated	966	0.40	0.00	1.00
Treated	966	0.11	0.00	1.00
Female	966	0.48	0.00	1.00
Number of siblings	966	1.32	0.00	7.00
Birth year	966	1975.59	1953.00	1997.00
Age	966	26.79	20.00	43.00
Age at degree	966	25.35	19.00	35.00
No migrant	966	0.93	0.00	1.00
Direct migrant	966	0.03	0.00	1.00
Indirect migrant	966	0.04	0.00	1.00
Father educ.: no	966	0.01	0.00	1.00
Father educ.: general	966	0.32	0.00	1.00
Father educ.: secondary	966	0.28	0.00	1.00
Father educ.: upper secondary	966	0.39	0.00	1.00
Mother educ.: no	966	0.01	0.00	1.00
Mother educ.: general	966	0.33	0.00	1.00
Mother educ.: secondary	966	0.41	0.00	1.00
Mother educ.: upper secondary	966	0.25	0.00	1.00
Survey year	966	2002.37	1985.00	2021.00
Degree year	966	2000.94	1984.00	2020.00

Note: The sample is based on SOEP data on graduates from 1985 to 2021. Observations are restricted to the first observation per individual. SOEP weights applied.

viduals whose fathers attained secondary education is higher in the control states. This discrepancy may be due to the different school systems in the Federal Republic of Germany and the former German Democratic Republic, which constitutes a large portion of the control group (for a detailed comparison, see, e.g., Riphahn and Trübswetter, 2013).

3.6 Identification strategy

I estimate the following linear probability model for the quasi-experimental setting of the introduction of tuition fees in Germany to identify their ITT on the likelihood of overeducation (compare e.g., Bahrs and Siedler, 2019).

$$OE_i = \alpha_0 + \beta_1 treated_i + \beta_2 X_i' + \lambda_t + \gamma_e + \delta_s + \epsilon_i \tag{2}$$

OE_i captures overeducation for individual i. α_0 is the intercept. β_1 is the coefficient of interest and identifies the ITT of tuition fee exposure. $treated_i$ equals 1 if the individual obtained a degree in a state and in a year in which tuition fees were charged. X_i' is a vector covering the covariates for each individual i. λ_t, γ_e, and δ_s cover the fixed effects for the survey years, the federal states where the degree was obtained, and the federal state of residence at the time of the interview. ϵ_i is the error term.[17]

[17] All estimations are pursued with the software R (version 4.2.3).

Table 2: Differences in means pre-treatment

	Control states		Treatment states		T-test	
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. in Means	Std. Error
Overeducated	0.450	0.498	0.392	0.489	-0.058	0.050
Female	0.436	0.497	0.358	0.480	-0.079	0.049
Number of siblings	1.166	1.096	1.357	1.141	0.191+	0.112
Birth year	1970.582	7.824	1969.947	7.068	-0.636	0.771
Age	27.399	4.439	27.575	4.130	0.175	0.456
Age at degree	25.896	4.594	25.986	4.019	0.090	0.440
No migrant	0.949	0.221	0.941	0.235	-0.007	0.025
Direct migrant	0.030	0.171	0.039	0.194	0.009	0.020
Indirect migrant	0.021	0.144	0.020	0.139	-0.002	0.015
Father educ.: no	0.005	0.073	0.001	0.032	-0.004	0.003
Father educ.: general	0.357	0.480	0.416	0.494	0.059	0.049
Father educ.: secondary	0.346	0.477	0.239	0.427	-0.107*	0.047
Father educ.: upper secondary	0.291	0.455	0.344	0.476	0.053	0.046
Mother educ.: no	0.004	0.064	0.006	0.076	0.002	0.004
Mother educ.: general	0.423	0.495	0.471	0.500	0.048	0.050
Mother educ.: secondary	0.341	0.475	0.357	0.480	0.016	0.047
Mother educ.: upper secondary	0.232	0.423	0.167	0.373	-0.065	0.042
Survey year	1997.982	5.800	1997.522	5.555	-0.460	0.589
Degree year	1996.478	5.655	1995.933	5.596	-0.545	0.594
Num. obs.	293		396			

Note: The sample is based on SOEP data on graduates from 1985 to 2012. Observations are restricted to the first observation per individual and to individuals who obtained their degrees before the introduction of tuition fees in 2006. The treatment states cover states that introduced tuition fees in 2006 or 2007. SOEP weights applied.

4 Results

4.1 Main results

Table 3 reports the ITT of being affected by tuition fees on the likelihood of overeducation from linear probability models. Column (1) estimates a baseline model including fixed effects for the survey year, federal state of residence, and federal state where the degree was obtained. Column (2) adds demographic covariates, and column (3) conditions on the proxies for socioeconomic background. Using the specification with fixed effects exclusively, the likelihood of overeducation is 11.9 percentage points larger for the treated group. The coefficient increases marginally to 12.8 percentage points once conditioned on the covariates. This corresponds to 28% of the pre-treatment mean of the control group (see Table 2). Table A.1 in the Appendix reports the results for the covariates. The likelihood of overeducation increases with the age of the graduate and marginally decreases with the number of siblings. All remaining covariates yield insignificant estimates.[18]

[18]The results are not sensitive to the inclusion of age at graduation as a covariate instead of age (see Table A.2), to the restriction of the sample to individuals who are employed part-time or full-time (see Table A.3), or to the use of the panel structure by including all repeated observations for the individuals covered in the main estimations (see Table A.4).

Table 3: ITT of tuition fees on the likelihood of overeducation

	(1)	(2)	(3)
Treated	0.119+	0.133*	0.128*
	(0.060)	(0.050)	(0.049)
λ_t, γ_e, δ_s	X	X	X
Demographics		X	X
Socioeconomic background			X
Num. obs.	966	966	966
R2	0.080	0.096	0.102
R2 Adj.	0.013	0.025	0.024

Note: Columns (1) to (3) contain results from linear probability models estimating the ITT of exposure to tuition fees on the likelihood of being overeducated. The sample is based on SOEP data on graduates from 1985 to 2021. Observations are restricted to the first observation per individual. Column (1) reports estimates including fixed effects for the survey year (λ_t), the federal state where the degree was obtained (γ_e), and the federal state of residence (δ_s). Column (2) adds further demographic covariates and column (3) conditions on proxies for the socio-economic status, respectively. Standard errors are clustered at the level of the state where the degree was obtained and reported in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

4.2 Robustness

Due to the small sample size and the data limitations, it is critical to ensure the robustness of the findings. Therefore, I employ a series of sensitivity tests in the following section.

4.2.1 Identification strategy

First, I conduct a placebo test (see e.g., Dwenger et al., 2012; Hübner, 2012) altering the treatment period (column (1) of Table B.1). Doing so ensures that the ITT presented in Table 3 does not capture differences between treatment and control regions that materialised independently of the policy change. Second, I re-estimate the base model using heteroskedasticity-robust standard errors as proposed in Wooldridge (2010) (column (2) of Table B.1). This addresses concerns that a small number of clusters may lead to overly precise standard errors in the main specification. Third, I estimate the base model using a sample of individuals who obtained their degrees between 2006 and 2014 (column (3) of Table B.1). While this shrinks the size of the control group, it enhances comparability between treated and control individuals by ensuring they attended university during the same period. Notably, this approach also accounts for the changes in the educational system brought about by the Bologna reform. Before the reform, students in Germany pursued *Magister*- or *Diplom*-degrees, which took four to six years to complete. In contrast to the two-tier system implemented with the Bologna reform, students did not face the option to exit early from the degree while still holding an academic qualification. Including earlier cohorts in the base model may therefore lead to a downward bias in the estimated tuition fee effect, as students affected by tuition fees could respond by exiting early with a Bachelor's degree. While this additionally supports the earlier notion of a conservative lower bound estimate, such early exits may be driven not only by the risk of overeducation but also by the financial burden of tuition fees and should therefore be tackled.[19] Fourth, linear probability models are not without criticism because predicted probabilities can exceed the range of 0 to 1. For this reason, column (4) of Table B.1 reports Average Marginal Effects (AME) from logit estimations, while column (5) applies a trimming approach based on Horrace and Oaxaca (2006). Fifth, column (6) of Table B.1 reports results from Difference-in-Differences (DiD) estimations to explicitly test the common trends assumption.[20] In all cases, results align qualitatively with the base results.

4.2.2 Matching and weighting

The identification of the effect of tuition fees on overeducation is threatened if treated individuals differ from their counterfactual in factors other than treatment assignment. While structural differences based on region are partially tackled in the placebo test contained above, I also use matching and weighting methods to explicitly control for differences in observables. Column (7) of Table B.1 shows results from exact matching, where treated and control units are exactly matched on confounders, only allowing for differences in treatment assignment. This procedure reduces the number of observations to 145. Column (8) uses propensity score weighting based on treatment probabilities estimated via a Generalised Linear Model (GLM). Column (9) uses weights from the SuperLearner algorithm, which combines multiple machine-learning models for improved accuracy. While the estimations lack precision in the exact matching procedure, they are qualitatively unaffected in all cases.[21]

4.2.3 Omitted variable bias

As the sample includes the first observation per individual after treatment only, the period between treatment assignment and the transition to the labour market is

[19]Note that there is no significant difference in the years of education between the treatment and control group. The average years of education obtained are 15.76 in the control, and 15.99 in the treatment group. This additionally limits concerns that treated individuals drop out of university earlier at a higher rate.

[20]The estimation equation is $OE_i = \alpha_0 + \beta_1 treat_e + \beta_2 post_y + \beta_3 treat_e \times post_y + \beta_4 X_i' + \lambda_t + \gamma_e + \delta_s + \epsilon_e$ where $treat_e$ equals one if the degree was obtained in a fee-charging state e and $post_y$ captures whether it was obtained after fee introduction in the respective state. δ_3 is the coefficient of interest covering the Difference-in-Difference estimator. To account for the stepwise abolition of fees in the federal states, the sample drops all individuals who obtained their degrees after the fees were again abolished.

[21]Exact matching is implemented using the *MatchIt*-package based on Ho et al. (2011). All weighting techniques are performed with the *WeightIt*-package by Greifer (2023).

usually short. Therefore, I cannot rule out that local labour market factors could simultaneously impact the decision to move to a state for studies and the likelihood of overeducation. To account for such potential omitted variable bias, I use additional information from the *Indikatoren und Karten zur Raum- und Stadtentwicklung* (INKAR) database, providing a broad range of statistics on the regional level as well as from the Federal Statistical Office. To capture the impact of the local labour market and economic conditions, I extract information on the local unemployment rate and GDP (columns (1) and (2) of Table B.2). Additionally, I use the annual number of enrolled students per 1,000 inhabitants within each state to capture educational capacity (column (3) of Table B.2). Finally, the introduction of tuition fees implied a monetary burden to students but increased financial resources of the charging states for educational purposes. This might have ameliorated educational quality as formulated in Section 2. In turn, this could affect individuals' choice to study in a fee-charging state as well as their job prospects post-graduation. Thus, column (4) of Table B.2 controls for the expenditure on education in each federal state, normalised by the number of students. Including these additional regional indicators does not affect the results qualitatively.

4.3 Selection concerns

Despite the provided robustness checks, the results presented in Table 3 may still be subject to selection bias. As outlined earlier, the data enable a treatment definition based on direct information about the state in which the degree was obtained or the state of residence when the degree was earned. Still, identification may not be perfectly accurate if, for example, individuals moved between fee-charging and control states without adequately indicating their new place of residence in the survey.[22] Dwenger et al. (2012) provide an in-depth discussion of this issue, noting that internal migration primarily occurs within West or East German federal states, with minimal migration between the two regions. With this in mind, they suggest restricting the control group to East German states, none of which introduced tuition fees. Still, applying this restriction reduces the sample drastically. For this reason, I adopt an alternative strategy by incorporating data on internal migration provided by INKAR since 1995. Internal migration captures the net flow of individuals moving in and out of a federal state, while migration for educational purposes focuses specifically on the mobility of individuals aged 18 to 25. Negative values of the indicators indicate a net outflow, and positive values signal a net inflow.

[22]Note that in Germany, people who change their place of residence are generally obliged to register their new address within two weeks. However, students who move to study could decide to register their new home as a secondary residence and thus keep their parents' house as their official main residence.

Based on this, I create dummy variables that take the value of 1 if the indicator is larger than or equal to 0, and 0 otherwise. While these do not directly approach individual-level selection, they reflect variation in overall internal migration patterns across federal states. Thus, they can be interpreted as a proxy for the average likelihood of mobility associated with living in a particular federal state in a certain year. States that experience a steady outflow of individuals could offer structurally weaker labour markets or fewer higher education opportunities, which could both increase the probability of students leaving the state and raise the likelihood of overeducation after graduation. By including this proxy, I partially account for potential biases arising from differential migration behaviour. Also, these measures make it possible to consider internal migration patterns but prevent a large reduction in the sample size. The results remain qualitatively unaffected (see Table C.1).

A second selection concern is the possibility that individuals who usually would have considered applying to university decide not to study or to delay their application due to the introduced tuition fees. In this case, the results discussed in Table 3 would be subject to sample selection bias, as these individuals would no longer appear in the graduate dataset. As this concern cannot be approached using the SOEP data, I analyse administrative data on first-year students provided by the Federal Statistical Office in the following (Statistisches Bundesamt, 2024a). The observation period spans from the winter term of 1998/99 to the winter term of 2023/24 providing 400 federal state-year observations ($25 \times 16 = 400$). If these data revealed substantial deviations in student enrolment trends between treated and control states, it would suggest the presence of an outselection bias. Given that treated and control states differ in the total number of higher education institutions and students, I compute the year-over-year percentage change in first-year student enrolments for each treated state and, collectively, for the control states. Figure C.1 and Figure C.1 illustrate these trends over time.

For Lower Saxony, Hamburg, and Hesse (Figure C.1d, Figure C.1e, Figure C.1f), the trajectories align closely with those of the control states, particularly around the period when tuition fees were first implemented. However, in Bavaria, Baden-Württemberg, Saarland, and North Rhine-Westphalia (Figure C.1a, Figure C.1b, Figure C.1c, Figure C.1g), a slight divergence emerges during the initial year of tuition fee introduction, with a noticeable gap developing between the treated and the control states. Nonetheless, in all cases, the percentage change in first-year student enrolments eventually surpasses that of the control group, suggesting that any initial decline relative to the control states was offset by a subsequent increase.

To test this more thoroughly, Figure C.2 estimates an event study approach evaluating the effect of fee introduction on the number of students following Sun and Abraham (2021). While the dynamic analysis reveals a small reduction in the number of students in the first period, the number of students in the treatment states subsequently exceeds that in the control states, aligning with the descriptive evidence. Importantly, these estimates are never statistically significant at the 10%-level, indicating no long-term effect on the number of students (for supportive evidence, see e.g., Bruckmeier et al., 2013; Havranek et al., 2018).

The above-presented robustness and selection analyses support the validity of the main results. However, concerns regarding a potential selection on unobservables remain untackled. For this reason, I additionally use the approach proposed by Oster (2019). This approach allows for the estimation of the degree of selection on unobservables relative to observables that would be necessary to reduce the base coefficient (β_1 in Equation 2) to zero, $\delta*$.[23] Absolute values of $|\delta*| < 1$ hereby hint at problems with selection on unobservables, while larger values limit such concerns. Applying this approach, $\delta*$ is estimated to be -17.1. This indicates that selection on unobservables would need to be both extremely strong and act in the opposite direction of selection on observables to nullify the effect.[24] This extends the above presented evidence and furthermore limits concerns about selection bias in the base results.

4.4 Heterogeneity

After ensuring the robustness of the presented findings and limiting concerns regarding selection bias, it remains open whether one of the mechanisms outlined in Section 2 can be observed in the data. While not all of them can be tested with the data at hand, the following heterogeneity analyses enable me to approach some of them. These heterogeneity analyses use the following form to investigate if the ITT of tuition fees on the likelihood of overeducation is particularly pronounced among certain subgroups:

$$OE_i = \alpha_0 + \beta_1 treated_i + \beta_2 \kappa_i + \beta_3 treated_i \times \kappa_i + \beta_4 X_i' + \lambda_t + \gamma_e + \delta_s + \epsilon_i \quad (3)$$

where κ_i denotes the indicators for diverging sources of heterogeneity across subgroups. The coefficient β_3 is the main coefficient of interest and captures this heterogeneity in the ITT of tuition fees on the likelihood of overeducation.

[23]The estimates are performed using the *"robomit"* package of Schaub (2025). Following Oster (2019) *Rmax* is estimated based on the *R-squared* of the estimation including all covariates multiplied by 1.3. $\beta*$ is set equal to zero.

[24]For detailed explanations of the interpretation of negative $\delta*$ as well as for further recent studies reporting such, see e.g., Aldén and Hammarstedt (2016), Graham et al. (2017), Lee and Ohtake (2021), Luo (2022).

4.4.1 Occupational choice

First, one way in which tuition fees may influence the likelihood of overeducation is through systematic differences in individuals' career choices. As mentioned in Minicozzi (2005) and Gervais and Ziebarth (2019), unsuitable career choices may result from an accelerated transition into the labour market after graduation to cover the financial burden accumulated during studies. Molina and Rivadeneyra (2021) already provided direct evidence on the link between tuition fees and occupational choice and reported that individuals who were no longer affected by tuition fees selected into high-skill occupations at higher rates. If this translated to the German setting, and treated individuals selected into low-skill occupations at higher rates, the ITT of tuition fees on the likelihood of overeducation could be expected to be larger among individuals working in low-skill jobs. To identify this heterogeneity, I use information on individuals' occupations and distinguish the respective ISCO codes into high- and low-skill occupations, where the first three ISCO major groups (managers, professionals, associate professionals/technicians) represent the high-skill occupations (International Labour Office, 2012).[25]

Figure D.1 presents the respective results. As expected and in line with the definition of overeducation, individuals in high-skill occupations report a lower likelihood of overeducation. Also, aligning with the base results (see Table 3), the ITT of being exposed to tuition fees is positive and significant. Moreover, as expected, the interaction term between working in a high-skill occupation and being treated is negative. This would indicate that, indeed, the exposure to tuition fees increases the likelihood of overeducation for individuals working in low-skill occupations, while being less influential among individuals in high-skill occupations. Still, this evidence is only suggestive due to two major reasons: The coefficient of the interaction term is insignificantly estimated, limiting firm conclusions about heterogeneity across occupational choice. In addition, this heterogeneity analysis attempts to explicitly take into account career choice after treatment. However, the differences in the ITT between groups may not only be due to occupational choice, but could also capture certain systematic differences within occupations. Although the base coefficient of the occupation dummy partially accounts for such differences, it might still not encompass all relevant variation.

4.4.2 Socioeconomic background

Second, and as formulated in Section 2, individuals could potentially be affected differently by tuition fees depending on their socioeconomic background and, for

[25]Table D.1 shows the ITT of exposure to tuition fees on the likelihood of working in a high-skill occupation. The coefficient is negative, which aligns with the expectations, but is small and insignificant.

example, the monetary support received from their parents. Aligning with this supposition, Neill (2009) shows that enrolment rates are most strongly affected among individuals whose parents obtained medium levels of education in Canada. Moreover, using the German case, Bahrs and Siedler (2019) find that the negative effect of tuition fees on educational intentions is stronger among individuals from low-income households. While the SOEP does not provide information on parental income or financial support by parents for a sufficient number of observations, I again employ the education attained by parents as a proxy for socioeconomic background. To maintain interpretability in the interaction setting, I define dummies equaling one if the individuals' father (mother) obtained upper secondary education. Moreover, I create a combined indicator as the sum of these dummies ranging between zero – when neither parent obtained upper secondary education – and two – when both obtained upper secondary education.[26]

Figure D.2a and Figure D.2b report the results separately for the education obtained by mothers and fathers. In both cases, it does not matter for individuals' likelihood of overeducation if the father (mother) obtained upper secondary education, while the ITT of being exposed to tuition fees is positive and significant as before. As indicated by the insignificant interaction terms, this positive ITT does not vary by the educational attainment of either fathers or mothers. Figure D.4c uses the categorical indicator for the joint education obtained by fathers and mothers. As before, whether one or both parental parts obtained upper secondary education is not statistically related to the likelihood of overeducation, while the treatment indicator remains positive and statistically significant. But, in contrast to the previous estimations, I observe a significantly lower likelihood of overeducation for treated individuals with one parental part owning upper secondary education. This aligns with the expectations that a higher socioeconomic background could buffer the influence of the tuition fee introduction. But, in contrast to that, the ITT of tuition fees on the likelihood of overeducation for individuals with two parents owning upper secondary education does not significantly differ from the reference group.

4.4.3 Employment during studies

Third, as outlined in Section 2, the likelihood of overeducation might be affected by tuition fees if individuals worked besides their studies to cover the additional expenses. But, theoretical expectations about the direction of the effect were ambiguous. This is, as working besides studies could harm individuals' performance, but could also improve individuals' signals in terms of working experience, poten-

[26]To avoid multicollinearity issues, these heterogeneity checks do not include the categorical assessment of education obtained by mother and father as separate covariates, but solely include the interaction term and the newly created dummies.

tially reducing the likelihood of overeducation (Diem and Wolter, 2014; Verhaest and Omey, 2010). If the latter of both materialised, one would expect the ITT of tuition fees to be most pronounced among individuals who were not employed. This is because their lack of working experience would disadvantage them in the application process. In contrast, if the performance and signal of individuals were harmed by working besides studying due to reductions in the time available for studying, the opposite, hence, a more pronounced ITT among individuals who worked besides their studies, would be expected. Using information on the employment status of individuals one year before their degree completion, I distinguish between graduates who were not employed during their studies and those who were in either full-time, part-time, or marginal employment.

Figure D.5 presents the results. Notably, having worked besides the studies is significantly linked to a higher likelihood of overeducation. This aligns more with the perspective that working besides studying reduces the time available and might harm academic outcomes and thus, the transition to the labour market. Through adding the interaction of the treatment indicator and the employment variable, the ITT of tuition fees on the likelihood of overeducation becomes insignificant, though remaining positive. Also, the interaction term is positive but imprecisely estimated. Hence, although the evidence aligns more with the perspective that employment besides studies is harmful instead of helpful, the analysis does not provide evidence that the treatment effect is driven by this group.[27]

In addition to the previous sources of heterogeneity, two further aspects should be considered: The duration of the treatment exposure and the federal states. Importantly, these two aspects are inevitably linked to the treatment, as, for example, only treated individuals can differ in the duration of their exposure. For this reason, the interaction approach presented in Equation 3 is not applicable here. For the following two heterogeneity analyses, I hence estimate the interaction regression without the baseline effects for the treatment indicator or the source of heterogeneity due to multicollinearity with the interaction term. The regression equation hence

[27]To validate that the treatment indicator loses significance due to the introduction of the interaction term, I re-estimated the baseline regression on the reduced sample used in Figure D.5. Moreover, I also estimated the baseline regression introducing the employment measure as a covariate without interacting it with the treatment. In both cases, the ITT of exposure to tuition fees is positive and significant. Additionally, as the vanishing significance of the treatment variable could raise the question of whether the treatment effect is mediated by employment besides studying, I regressed the employment indicator on the treatment. The coefficient is close to zero and insignificant, and does, thus, not indicate that the ITT might work through an impact of tuition fees on the employment of students. The estimations are contained in Table D.2.

reads as follows:[28]

$$OE_i = \alpha_0 + \beta_1 treated_i \times \kappa_i + \beta_2 X_i' + \lambda_t + \gamma_e + \delta_s + \epsilon_i \tag{4}$$

4.4.4 Treatment duration

Although the data do not allow for the identification of the exact duration of the degree, they enable me to approximate whether individuals were at least treated for the full period of a standard Bachelor's degree. Notably, this approach allows to account for two questions at once. On the one hand, it proxies differences in the total amount of tuition fees paid. On the other hand, it assesses differences in the timing of treatment, as people who already started their studies with tuition fees may have been affected differently by the tuition fees than those who started without tuition fees and were surprised by the introduction during their studies. As a standard Bachelor's degree takes at least six terms in most cases, every individual who graduated a minimum of six terms after the introduction of the tuition fees but before their abolition in the respective state can be assumed to be fully treated. Using this approach, 46% of the previously treated individuals were exposed to tuition fees for the full duration of a standard undergraduate programme.

Figure D.6 shows the results from interacting the treatment indicator with the dummy indicating if individuals were treated for at least six terms. Notably, the coefficient for those considered fully treated is slightly larger in magnitude than the base coefficient (see Table 3) and is statistically significant. In contrast, the coefficient for those who were treated less than these six terms is quantitatively smaller and imprecisely estimated. These results align conceptually with the anticipation that individuals with longer treatment duration and higher costs report a stronger ITT of tuition fees on the likelihood of overeducation. However, the confidence intervals overlap, preventing the identification of a significant difference in the ITT between both groups.

4.4.5 Federal states

Fifth, differences could arise, for example, if financial resources gathered through the fees were used in different ways by the federal states. They could invest them directly in improving the quality of teaching, or use them for infrastructural investments, while other federal states might only make such investments later. In

[28] Importantly, due to the changes in the regression function, the interpretation of the plot changes. In Figure D.1 to Figure D.5, the interaction terms indicate whether the ITT of tuition fees statistically differs between groups. In contrast, in Figure D.6 and Figure D.7, overlapping confidence intervals indicate non-statistically significant differences between the groups, while non-overlapping confidence intervals hint at statistically significant differences.

addition, structural differences between the federal states could induce heterogeneity in the influence of tuition fees on the likelihood of overeducation. While the robustness section already approached whether the main results persist when conditioning for educational expenses on the state-level, Figure D.7 plots interactions of the treatment indicator and the federal states, revealing the ITT for each of them. The abbreviations on the x-axis indicate the federal states such that LS = Lower Saxony, NRW = North Rhine-Westphalia, BW = Baden-Württemberg, and BY = Bavaria.[29] All remaining states are part of the control group for which the interaction term yields zero. The positive ITT reported in Table 3 can be observed in Lower Saxony, Baden-Württemberg and Bavaria, but is insignificant in North Rhine-Westphalia. Albeit this, the analysis provides little evidence that structural state differences or differences in the educational system would drive the results.

In sum, the heterogeneity analyses provide suggestive evidence that the treatment may be less influential among individuals who chose high-skill occupations post-graduation, who have one parent with upper secondary education and for those who were treated for a shorter period. But, in most cases, the data do not allow to identify significant differences between groups. For this reason, although results align with the suppositions in the majority of cases, the evidence is interpreted with caution.

4.5 Dynamic analysis

Due to the sample restrictions outlined above, the ITT of tuition fees on the likelihood of overeducation likely applies exclusively to individuals' first jobs post-graduation. Hence, it remains open whether this is a short- or long-term effect. Theoretical considerations related to Career Mobility Theory argue that selecting an overeducated position post-graduation may serve as a temporary state, allowing one to gather work experience to achieve a matched position afterwards (see e.g., Sicherman, 1991; Sicherman and Galor, 1990). The empirical literature, however, reports contradicting results. Several papers report evidence aligning with Career Mobility Theory as overeducated individuals tend to participate more in training, are more likely to switch jobs, or report higher wage growth rates over time (e.g., Grunau and Pecoraro, 2017; Roller et al., 2020; Sicherman, 1991). In contrast, other studies identify overeducation as a "trap," increasing the probability of staying overeducated in the long run (e.g., Baert et al., 2013; Blásquez and Budría, 2012; Büchel and Mertens, 2004; Kiersztyn, 2013; Wen and Maani, 2019). As reported in Table 3, tuition fees increase the likelihood of overeducation among treated individuals. If this effect was only short-term, potential negative consequences for individuals and

[29] As discussed in Section 3.4, the ITT cannot be identified for Hesse, Hamburg, and Saarland.

society would be limited. However, if the increased likelihood of overeducation persisted for several years, what would align more with the trap-perspective, this would not only imply an inefficient distribution of formal qualifications in the labour market. Even more, it might also expose individuals to certain disadvantages attributed to overeducation such as the overeducation-pay-penalty or potentially a lower job satisfaction (cf., Allen and van der Velden, 2001; Belfield, 2010; Caroleo and Pastore, 2018; Duncan and Hoffman, 1981; Peiró et al., 2010; Verdugo and Verdugo, 1989).

To investigate whether the ITT of being exposed to tuition fees on the likelihood of overeducation persists, I make use of the panel structure of the SOEP in the following. For each individual, I define the years since the degree completion as the distance between the year in which the degree was obtained and the respective survey year. Using this, I estimate Equation 5 where β_1 captures the unique ITT at each distinct time-interval since the degree completion, i.e., the ITT of being treated 1 (2, 3, ...) years post-graduation, while β_2 controls for general trends in the likelihood of overeducation with increasing years since graduation.[30]

$$OE_{it} = \alpha_0 + \beta_1 treated_i \times yearssince_{it} + \beta_2 yearssince_{it} + \beta_3 X_i' + \lambda_t + \gamma_e + \delta_s + \epsilon_i \quad (5)$$

To estimate this regression, the following analysis uses all observations of the individuals contained in the data after they completed their degrees.[31] To ensure a sufficient number of observations in both the treatment and control groups across all values of the variable capturing years since degree completion, the analysis is restricted to individuals observed within ten years post-graduation.[32] This results in a sample of 5,889 observations of 965 individuals.[33] Figure 2 shows the results for the interaction effect, β_1. While the point estimate is positive and substantial in each of the periods, it is precisely estimated from the fourth interval onward. Moreover, as the time since graduation increases, the point estimate gradually strengthens until around the eighth year and declines slightly afterwards.[34]

In sum, the evidence from the dynamic analyses is more in favour of a trap perspective. The findings indicate that individuals exposed to tuition fees are more

[30]Note that years since degree completion can be defined for individuals independent of their treatment assignment, while the interaction term is directly linked to the treatment. For this reason, the treatment cannot be included as separate covariate.

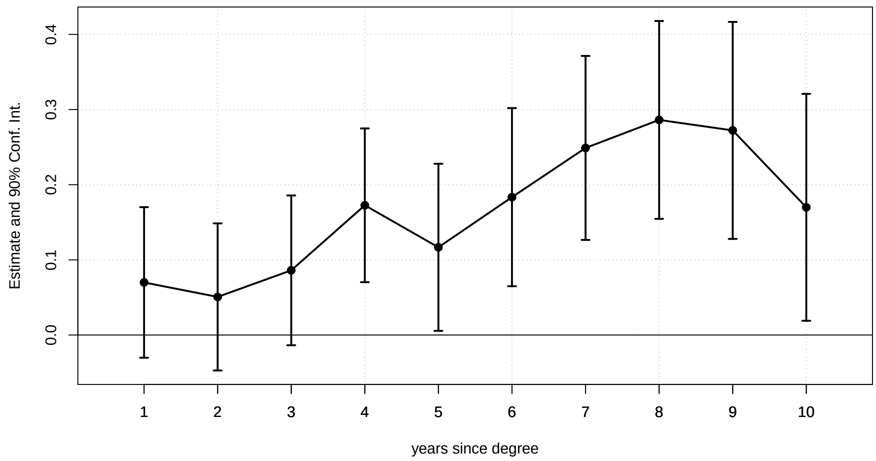
[31]Note that the use of the panel structure and the inclusion of all observations for the years surveyed in Table 3 extends the survey years to 2022.

[32]One individual included in the baseline estimations was only observed ten years after graduation and is therefore dropped from the sample.

[33]The results presented in Table A.4 remain qualitatively the same when using this restricted sample.

[34]Figure E.1 in the Appendix additionally plots the baseline coefficients for years since degree completion (β_2) revealing insignificant estimates in all periods.

Figure 2: ITT of tuition fees on the likelihood of overeducation: Dynamic analysis



Note: The graph displays the ITT by the years since degree completion based on the interaction of the treatment indicator and the years since degree completion. The regression is based on Equation 5 and estimates the ITT conditional on general trends in overeducation with rising years since degree completion. The years since degree completion are not significantly related to the likelihood of overeducation (see Figure E.1). The sample is based on SOEP data using all observations of graduates referred to in Table 3 survey between 1985 to 2022. It is restricted to individuals observed within ten years following degree completion. All covariates and fixed effects of Table 3 are included.

likely to experience overeducation even up until ten years after graduation. This suggests enduring consequences of the introduction of tuition fees on labour market outcomes, particularly job match quality, for affected individuals.

5 Conclusion

Research on the effect of higher education costs is extensive in contexts where tuition fees are common (e.g., Castleman and Long, 2016; Denning, 2019; Dynarski, 2008; Dynarski et al., 2021; Fack and Grenet, 2015; Page et al., 2019; Solis, 2017). In contrast, evidence on rises in tuition fees from countries with historically low or non-existent tuition fees is limited and focuses on educational performance and enrolment (see, e.g., Badillo-Amador and Vila, 2013; Barr, 2019; Bruckmeier et al., 2013; Bruckmeier and Wigger, 2014; Elliott and Soo, 2013; Hübner, 2012; Kane, 2007; McPherson and Schapiro, 1991; Minor, 2023; Neill, 2009; Riphahn, 2012; Savoca, 1990). Consequently, there is a lack of evidence on the impact of tuition fees on post-graduate outcomes, particularly in systems that historically do not rely on such fees. This study provides the first empirical evidence linking higher education costs to post-graduation outcomes in the German setting. Specifically, it exploits the quasi-experimental setting of the introduction of tuition fees in several federal states between 2006 and 2014 on the likelihood of overeducation post-graduation.

Using data on graduates from the SOEP for the period 1985 to 2021 reveals that exposure to tuition fees significantly increases the likelihood of overeducation. This result is not sensitive to the empirical specification. Moreover, concerns regarding potential selection issues can be limited empirically using additional data from INKAR and the Federal Statistical Office, as well as by implementing the approach suggested by Oster (2019). Heterogeneity analyses hint at potential differences based on individuals' occupational choice, socioeconomic background and treatment duration. In dynamic terms, the findings suggest a persistent effect of tuition fees on the likelihood of overeducation. More precisely, the positive link persists in the ten years post-graduation.

Albeit these findings hint at severe and long-lasting impacts of tuition fees on individuals' labour market outcomes, several limitations of the study should be acknowledged. Identifying a sufficiently large sample of treated and control individuals imposes challenges, as the analysis requires participants to be tertiary graduates with identifiable graduation dates, all while being observed after entering the labour market. These requirements, combined with the sample selection criteria, yield a comparably small sample size, potentially limiting generalisability. However, comparisons with similar studies using the same dataset indicate that the sample size is within a typical range (see, e.g., Bahrs and Siedler, 2019). Furthermore, this study focuses on the ITT rather than the average treatment effect (ATE), as it cannot be ruled out that persons in the treated group are actually untreated due to a discrepancy between their assumed and actual place of study. Although undesirable, such non-differential misclassification would introduce a downward bias in the coefficients. This implies that the coefficients are conservative lower bounds and likely even underestimate the true effect (e.g., Bahrs and Siedler, 2019; Bietenbeck et al., 2023; Hübner, 2012). Finally, the ITT is identified by four out of seven states that introduced tuition fees in the respective period due to data limitations. It should be noted that prior studies reported heterogeneity in effect directions, particularly regarding Hamburg and Saarland (Bruckmeier et al., 2013). Therefore, it is emphasised that the evidence obtained in this study cannot be generalised to these states but needs to be supplemented with further results.

Despite these limitations, the insights gained through this study are informative for the debate about the gains and costs of tuition fees. In particular, the study supports the thesis that the introduction of tuition fees cannot only influence the decision for or against higher education but also the individual's outcomes after graduation. While this may harm individuals' careers (Baert et al., 2013), it could also impede labour earnings (Allen and van der Velden, 2001; Dolton and Vignoles, 2000; Duncan and Hoffman, 1981; Rumberger, 1987) and could prevent an efficient allocation of the labour force.

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Appendix

A Main results

Table A.1: ITT: Extended

	(1)	(2)	(3)
Treated	0.119+	0.133*	0.128*
	(0.060)	(0.050)	(0.049)
Age		0.015***	0.017***
		(0.003)	(0.003)
Female		0.041	0.045
		(0.037)	(0.038)
Direct migrant		0.026	0.015
		(0.072)	(0.082)
Indirect migrant		-0.061	-0.066
		(0.066)	(0.063)
Number of siblings			-0.022+
			(0.011)
Father educ.: general			-0.006
			(0.321)
Father educ.: secondary			-0.036
			(0.296)
Father educ.: upper secondary			-0.044
			(0.305)
Mother educ.: general			-0.114
			(0.311)
Mother educ.: secondary			-0.024
			(0.300)
Mother educ.: upper secondary			-0.025
			(0.300)
λ_t, γ_e, δ_s	X	X	X
Num. obs.	966	966	966
R2	0.080	0.096	0.102
R2 Adj.	0.013	0.025	0.024

Note: Columns (1) to (3) contain results from linear probability models estimating the ITT of exposure to tuition fees on the likelihood of being overeducated. The sample is based on SOEP data on graduates from 1985 to 2021. Observations are restricted to the first observation per individual. Column (1) reports estimates, including fixed effects for the survey year (λ_t), the federal state where the degree was obtained (γ_e), and the federal state of residence (δ_s). Column (2) adds further demographic covariates including age, sex and migration background. Column (3) furthermore conditions on proxies for the socio-economic status by including the number of siblings, and dummies, differentiating the education attained by mothers and fathers, respectively. Standard errors are clustered at the level of the state where the degree was obtained. Standard errors are reported in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A.2: ITT: Age at degree

	(1)	(2)	(3)
Treated	0.119+	0.127*	0.122*
	(0.060)	(0.053)	(0.052)
Age at degree		0.014***	0.015***
		(0.003)	(0.003)
λ_t, γ_e, δ_s	X	X	X
Remaining demographics		X	X
Socioeconomic background			X
Num.Obs.	966	966	966
R2	0.080	0.094	0.100
R2 Adj.	0.013	0.023	0.022

Note: Columns (1) to (3) contain results from linear probability models estimating the ITT of exposure to tuition fees on the likelihood of being overeducated. The sample is based on SOEP data on graduates from 1985 to 2021. Observations are restricted to the first observation per individual. Column (1) reports estimates, including fixed effects for the survey year (λ_t), the federal state where the degree was obtained (γ_e), and the federal state of residence (δ_s). Column (2) adds further demographic covariates including age at degree (in exchange for age), sex and migration background. Column (3) furthermore conditions on proxies for the socio-economic status by including the number of siblings, and dummies, differentiating the education attained by mothers and fathers, respectively. Standard errors are clustered at the level of the state where the degree was obtained. Standard errors are reported in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A.3: ITT: Only full- and part-time employed

	(1)	(2)	(3)
Treated	0.135+	0.148*	0.141*
	(0.075)	(0.067)	(0.064)
λ_t, γ_e, δ_s	X	X	X
Demographics		X	X
Socioeconomic background			X
Num.Obs.	770	770	770
R2	0.085	0.100	0.111
R2 Adj.	0.001	0.011	0.014

Note: Columns (1) to (3) contain results from linear probability models estimating the ITT of exposure to tuition fees on the likelihood of being overeducated. The sample is based on SOEP data on graduates from 1985 to 2021 working in full- or part-time. Observations are restricted to the first observation per individual. Column (1) reports estimates, including fixed effects for the survey year (λ_t), the federal state where the degree was obtained (γ_e), and the federal state of residence (δ_s). Column (2) adds further demographic covariates including age, sex and migration background. Column (3) furthermore conditions on proxies for the socio-economic status by including the number of siblings, and dummies, differentiating the education attained by mothers and fathers, respectively. Standard errors are clustered at the level of the state where the degree was obtained. Standard errors are reported in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A.4: ITT: Using panel structure

	(1)	(2)	(3)
Treated	0.094*	0.121*	0.121*
	(0.046)	(0.048)	(0.048)
λ_t, γ_e, δ_s	X	X	X
Demographics		X	X
Socioeconomic background			X
Num.Obs.	8654	8654	8654
R2	0.042	0.045	0.054
R2 Adj.	0.035	0.037	0.046

Note: Columns (1) to (3) contain results from linear probability models estimating the ITT of exposure to tuition fees on the likelihood of being overeducated. The sample is based on SOEP data on graduates from 1985 to 2021. The sample is based on the same individuals observed in Table 3 but includes all repeated observations for these individuals. Column (1) reports estimates, including fixed effects for the survey year (λ_t), the federal state where the degree was obtained (γ_e), and the federal state of residence (δ_s). Column (2) adds further demographic covariates including age, sex and migration background. Column (3) furthermore conditions on proxies for the socio-economic status by including the number of siblings, and dummies, differentiating the education attained by mothers and fathers, respectively. Standard errors are clustered at the level of the state where the degree was obtained interacted with the individual identifier. Standard errors are reported in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

B Robustness

Table B.1: Robustness: Identification strategy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
			Identification & Estimation					Matching & Weighting	
	Placebo	SE	2006 - 2014	Logit	Trimming	DiD	Exact	PSM	SuperLearner
Placebo	-0.003								
	(0.029)								
Treated/ treated × post		0.128+	0.237***	0.131*	0.138*	0.109+	0.283	0.116+	0.127+
		(0.076)	(0.055)	(0.060)	(0.050)	(0.063)	(0.223)	(0.062)	(0.062)
Post						-0.096			
						(0.193)			
Treat						-0.540			
						(1.000)			
λ_t, γ_e, δ_s	X	X	X	X	X	X	X	X	X
Demographics	X	X	X	X	X	X	X	X	X
Socioeconomic background	X	X	X	X	X	X	X	X	X
Num. obs.	966	966	244	966	956	928	145	966	966
Effective obs.								927.93	931,71
R2/ McFadden Pseudo R2	0.099	0.102	0.137	0.021	0.103	0.109	0.328	0.116	0.098
R2 Adj.	0.021	0.024	-0.070		0.025	0.027	-0.240	0.039	0.020

Note: Columns (1) to (3) and (5) to (9) contain results from linear probability models estimating the ITT of exposure to tuition fees on the likelihood of being overeducated. Column (4) estimates a logit model. The sample is based on SOEP data on graduates from 1985 to 2021. Observations are restricted to the first observation per individual. Column (1) reports estimates using a placebo treatment using different treatment periods. Column (2) uses heteroskedasticity robust standard errors (see Wooldridge, 2010). Column (3) restricts the sample to individuals who obtained their degrees between 2006 and 2014. Column (4) presents Average Marginal Effects (AME) from logit specifications. Column (5) trims the sample following the suggestion of Horrace and Oaxaca (2006) to account for biases in the linear probability specification. Column (6) applies a classical Difference-in-Difference (DiD) approach where the treatment equals 1 if the individual obtained the degree in one of the fee-charging states and the post-variable identifies degrees obtained after introducing fees. Individuals who obtained degrees after the abolition of fees in the fee-charging states are dropped to avoid biases. Columns (7) to (9) report estimates based on the baseline identification strategy, applying weights to correct the covariate balance between the treated and control groups. Column (7) uses exact matching methods. Column (8) applies propensity score matching (PSM) based on a parametric GLM estimating the predicted probability of being in the treatment or the control group. Column (9) uses propensity score weights estimated with the SuperLearner algorithm that combines multiple machine learning models to improve prediction accuracy and flexibility. The covariates used to estimate the weights are the demographic confounders. All covariates and fixed effects are included. Standard errors are clustered at the level of the state where the degree was obtained in columns (1) and (3) to (9) and are reported in parentheses. McFadden Pseudo R2 reported in column (4). + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table B.2: Robustness: Omitted variables

	(1) Unemployment	(2) GDP	(3) # students/1,000 inhabitants	(4) Expenditure/student
Treated	0.150**	0.117*	0.150**	0.167**
	(0.050)	(0.055)	(0.048)	(0.042)
Unemployment rate	0.007			
	(0.015)			
GDP		0.000		
		(0.000)		
# students (norm.)			0.001	
			(0.007)	
Expenditures (norm.)				-0.013
				(0.017)
λ_t, γ_e, δ_s	X	X	X	X
Demographics	X	X	X	X
Socioeconomic background	X	X	X	X
Num. obs.	667	795	738	618
R2	0.111	0.112	0.114	0.117
R2 Adj.	0.017	0.028	0.026	0.015

Note: Columns (1) to (4) contain results from linear probability models estimating the ITT of exposure to tuition fees on the likelihood of being overeducated. The sample is based on SOEP data on university graduates from 1998 to 2021 in column (1), from 1995 to 2021 in column (2), from 1996 to 2021 in column (3), and from 1999 to 2021 in column (4). Observations are restricted to the first observation per individual. Column (1) adds the local unemployment rate. Column (2) contains the GDP, and column (3) additionally conditions on the local number of students per 1,000 inhabitants. Column (4) adds higher education expenditure in EUR 1,000, standardised to the number of students in the respective federal state. The data on the local unemployment rate, GDP, and the number of students per 1,000 inhabitants are extracted from INKAR. Data on the absolute number of students in each federal state, which are used for standardisation, and on expenditure on higher education in each federal state come from the Federal Statistical Office. All covariates and fixed effects are included. Standard errors are clustered at the level of the state where the degree was obtained and are reported in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

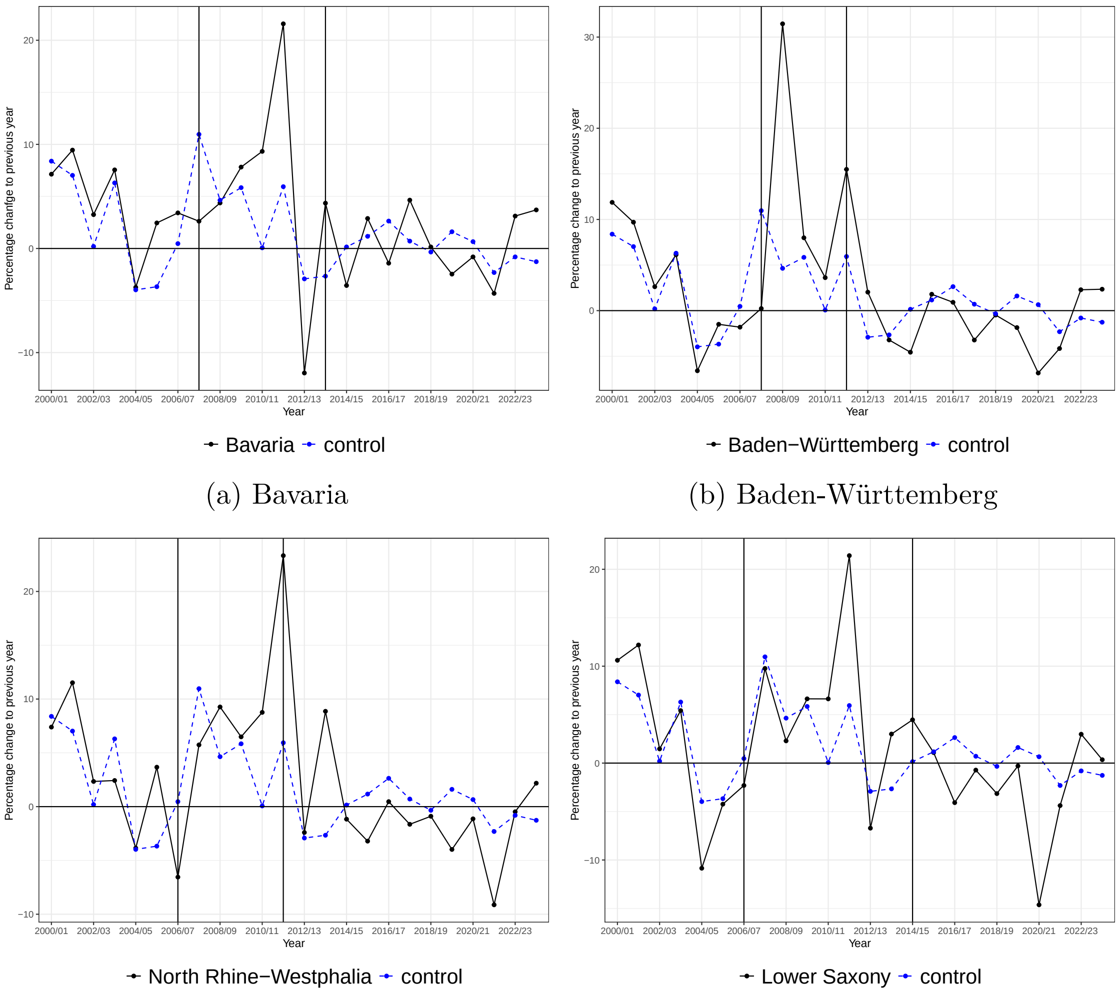
C Selection concerns

Table C.1: Selection: Internal migration

	(1) For education	(2) General
Treated	0.147*	0.150**
	(0.050)	(0.051)
Int. migration inflow	-0.021	0.000
	(0.053)	(0.036)
λ_t, γ_e, δ_s	X	X
Demographics	X	X
Socioeconomic background	X	X
Num. obs.	795	795
R2	0.092	0.091
R2 Adj.	0.006	0.006

Note: Columns (1) to (2) contain results from linear probability models estimating the ITT of exposure to tuition fees on the likelihood of being overeducated. The sample is based on SOEP data on graduates from 1995 to 2021. Observations are restricted to the first observation per individual. Columns (1) to (2) use information from INKAR on internal migration and add dummies equaling one if the federal state experienced a net inflow of internal migrants in the respective year. This indicator is based on individuals aged 18 to 25 in column (1) capturing migration for educational purposes, while the indicator considers all migration streams in column (2). All covariates and fixed effects are included. Standard errors are clustered at the level of the state where the degree was obtained and are reported in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure C.1: Percentage change in the average number of first-year students



(a) Bavaria

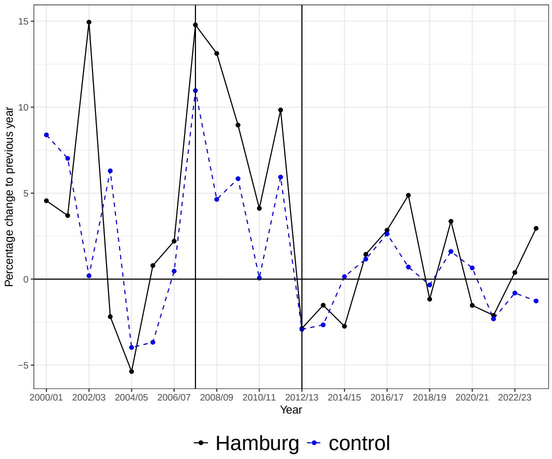
(b) Baden-Württemberg

(c) North Rhine-Westphalia

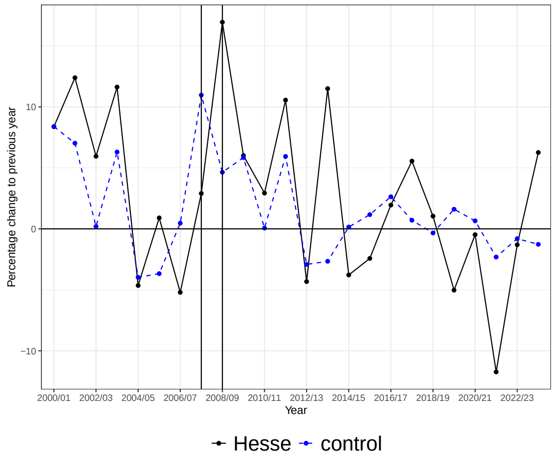
(d) Lower Saxony

Note: This figure is based on administrative data by the Federal Statistical Office (Statistisches Bundesamt, 2024a) on first-year students from winter term 1998/1999 to winter term 2023/24. It displays the percentage change of the average number of first-year students compared to the previous winter term for the treated (Bavaria, Baden-Württemberg, North Rhine-Westphalia, Lower Saxony) in black (solid line) compared to the aggregate of the control states in blue (dotted line). Values above 0 indicate an increase in the number of first-year students compared to the previous winter term, while negative numbers represent a decrease. Due to the estimation of the percentage change compared to the previous winter term, the first observation period (winter term 1998/99) is not included. The reference lines mark the first introduction of tuition fees and the last abolition of fees.

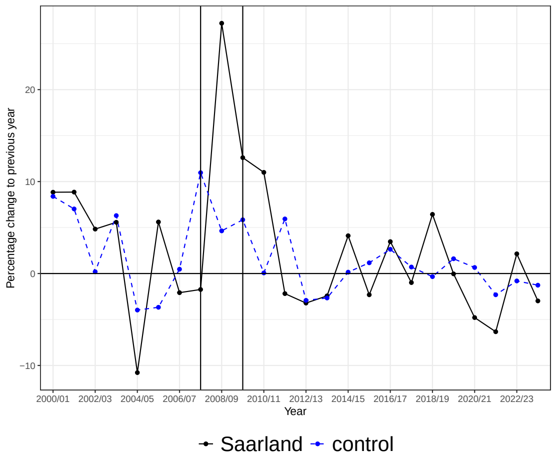
Figure C.1: Percentage change in the average number of first-year students (cont.)



(e) Hamburg



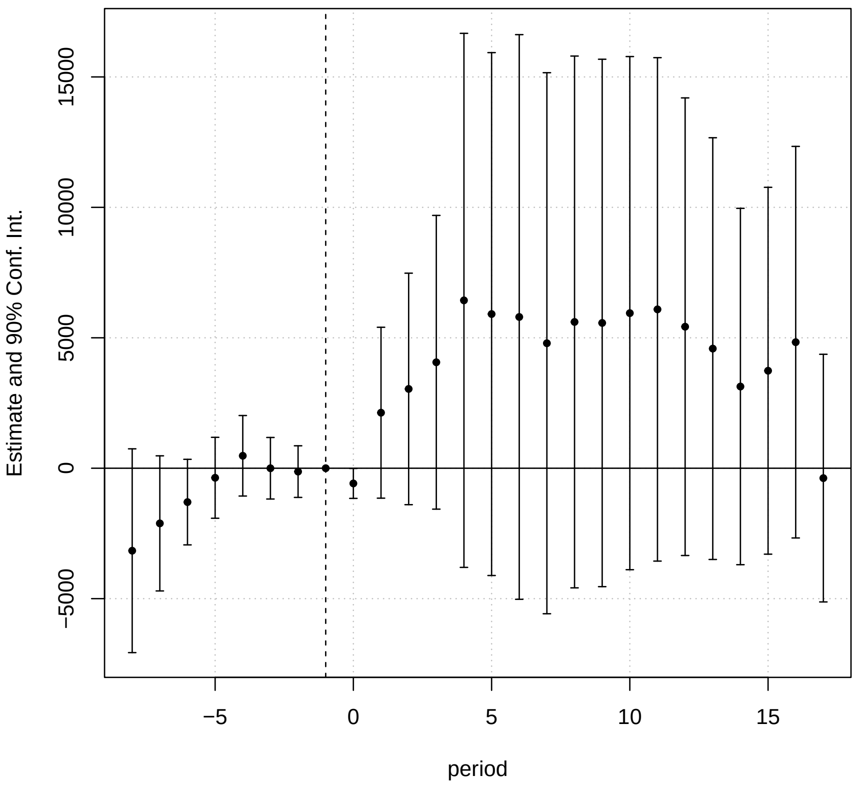
(f) Hesse



(g) Saarland

Note: This figure is based on administrative data by the Federal Statistical Office (Statistisches Bundesamt, 2024a) on first-year students from winter term 1998/1999 to winter term 2023/24. It displays the percentage change of the average number of first-year students compared to the previous winter term for the treated (Hamburg, Hesse, and Saarland) in black (solid line) compared to the aggregate of the control states in blue (dotted line). Values above 0 indicate an increase in the number of first-year students compared to the previous winter term, while negative numbers represent a decrease. Due to the estimation of the percentage change compared to the previous winter term, the first observation period (winter term 1998/99) is not included. The reference lines mark the first introduction of tuition fees and the last abolition of fees.

Figure C.2: Event study: Treatment effect on the number of students



Note: This figure is based on administrative data by the Federal Statistical Office (Statistisches Bundesamt, 2024a) on first-year students from winter term 1998/1999 to winter term 2023/24. It displays results from an event study approach based on Sun and Abraham (2021). To calculate the event study the period is defined based on the year in which the respective winter term started (e.g., winter term 2007/08 translates into cohort= 2007). The cohort equals 2007 for Baden-Württemberg, Saarland, Hesse, Hamburg and Bavaria, and 2006 for Lower Saxony and North-Rhine Westphalia. Federal state and year fixed effects included. Standard errors are clustered at the state level. Average Treatment Effect on the Treated (ATT) = 4452.67.

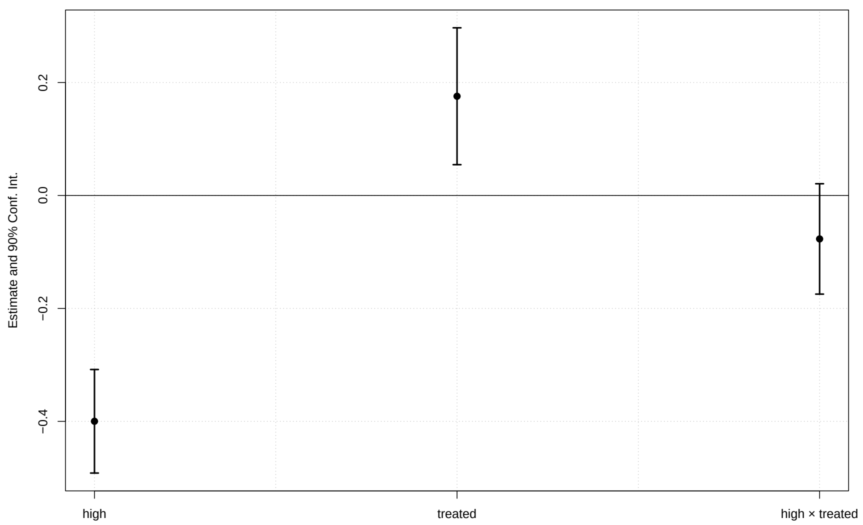
D Heterogeneity

Table D.1: ITT: Occupational choice

	(1)
Treated	-0.018
	(0.068)
λ_t, γ_e, δ_s	X
Demographics	X
Socioeconomic background	X
Num.Obs.	966
R2	0.201
R2 Adj.	0.132

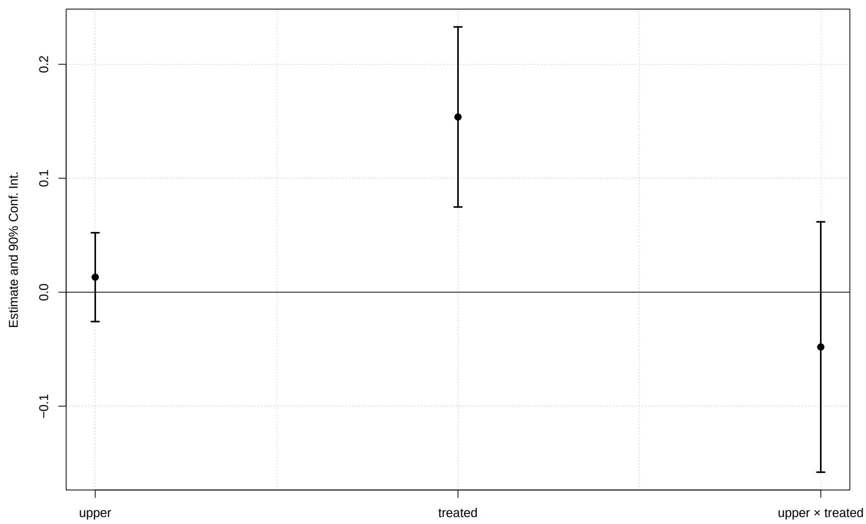
Note: Column (1) contains results from linear probability models estimating the ITT of exposure to tuition fees on the likelihood of working in a high-skill occupation. The sample is based on SOEP data on graduates from 1985 to 2021. Observations are restricted to the first observation per individual. All covariates and fixed effects are included. Standard errors are clustered at the level of the state where the degree was obtained. Standard errors are reported in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure D.1: Heterogeneity by occupation

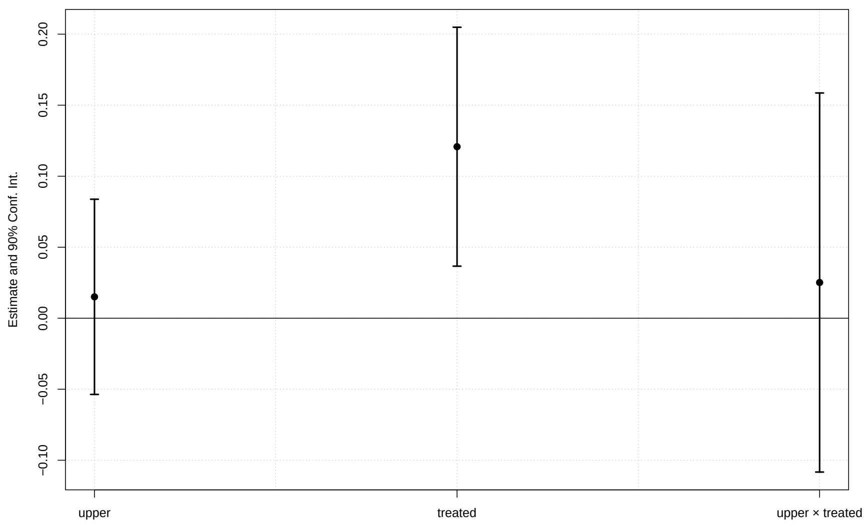


Note: The graph plots the coefficients based on Equation 3 focusing on the heterogeneity across individuals working in high- and low-skill occupations. The sample and covariates are the same as in Table 3. 90%-confidence intervals plotted.

Figure D.2: Heterogeneity by parental education



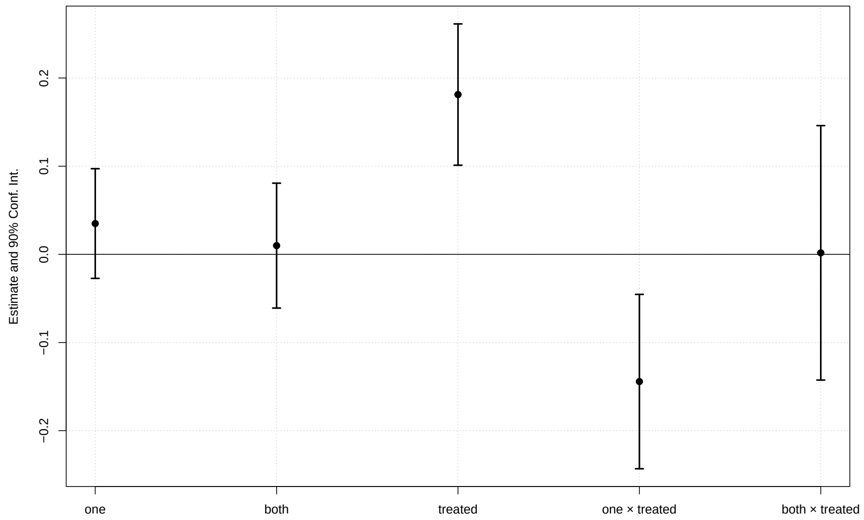
(a) By father education



(b) By mother education

Note: The graph plots the coefficients based on Equation 3 focusing on the heterogeneity across parental education. In Figure D.2a (Figure D.2b) "upper" indicates that the individual's father (mother) obtained upper secondary education. Due to multicollinearity issues, the regression does not additionally include the categorical indicators for father and mother education while the remaining covariates are the same as in Table 3. 90%-confidence intervals plotted.

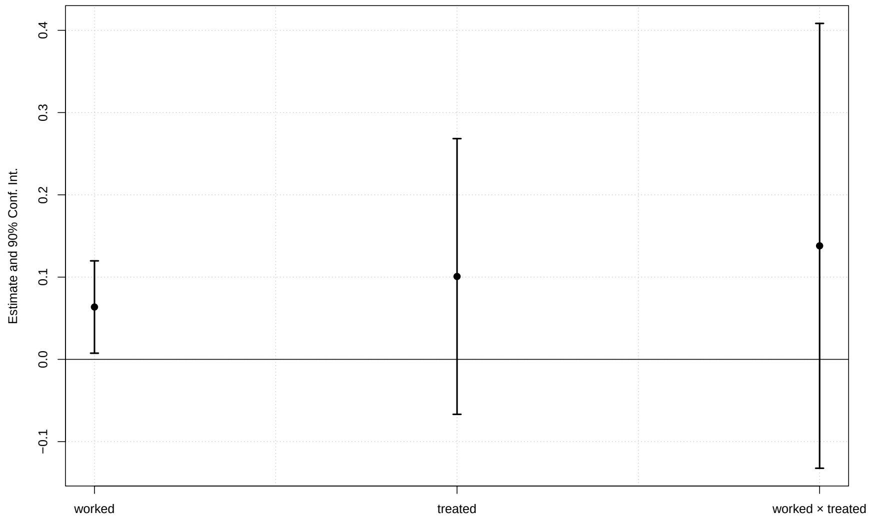
Figure D.4: Heterogeneity by parental education (cont.)



(c) By combination of mother and father education

Note: The graph plots the coefficients based on Equation 3 focusing on the heterogeneity across parental education. In Figure D.4c "one" ("both") indicates that one (both) of the parents obtained upper secondary education. Due to multicollinearity issues, the regression does not additionally include the categorical indicators for father and mother education while the remaining covariates are the same as in Table 3. 90%-confidence intervals plotted.

Figure D.5: Heterogeneity by employment status during studies



Note: The graph plots the coefficients based on Equation 3 focusing on the heterogeneity across employment status during studies. "Working" indicates that the individual was either marginally, part-time or full-time employed one year before university graduation. Due to missing information on the employment status of some individuals one year prior to graduation, the sample size reduces to 737 in this estimation. The covariates are the same as in Table 3. 90%-confidence intervals plotted.

Table D.2: Heterogeneity by employment status: Additional analyses

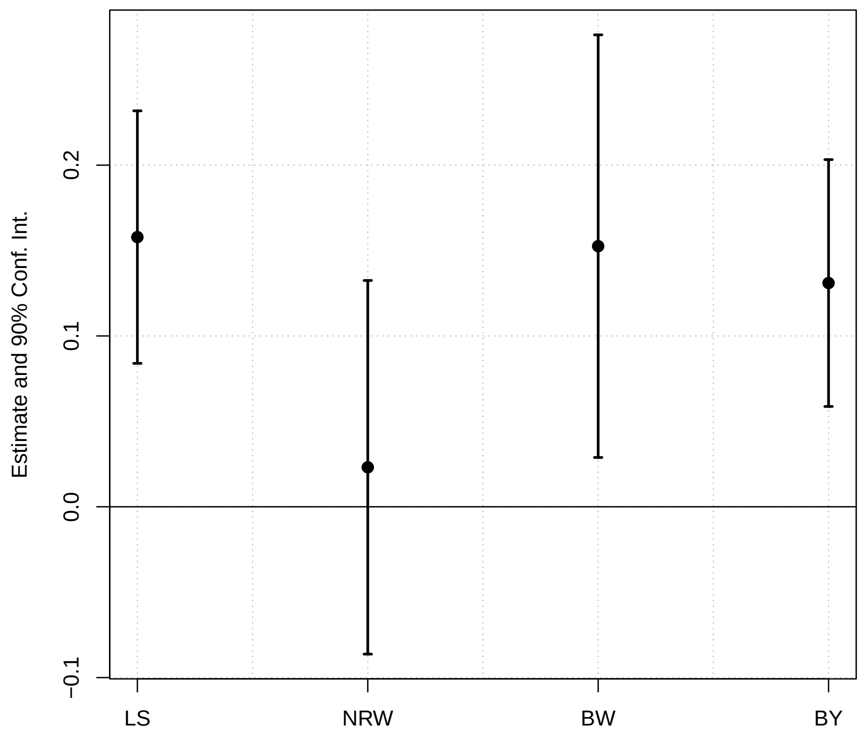
	(1)	(2)	(3)
Dependent	Overeducation		Worked
Treated	0.133+	0.133+	-0.003
	(0.064)	(0.063)	(0.047)
Worked		0.075*	
		(0.028)	
λ_t, γ_e, δ_s	X	X	X
Demographics	X	X	X
Socioeconomic background	X	X	X
Num.Obs.	737	737	737
R2	0.112	0.116	0.212
R2 Adj.	0.011	0.014	0.122

Note: Columns (1) to (3) contain results from linear probability models. Column (1) estimates the same regression as Table 3 on the reduced sample used in Figure D.5, while column (2) additionally controls for whether the individuals worked besides studying. Column 3 regresses the treatment indicator on the likelihood of having worked while studying. The sample is based on SOEP data on graduates from 1985 to 2021. Observations are restricted to the first observation per individual. All covariates and fixed effects are included. Standard errors are clustered at the level of the state where the degree was obtained and are reported in parentheses. + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Figure D.6: Heterogeneity by treatment duration

Note: The graph displays the ITT for individuals who were treated at least for a period of six terms (Bachelor's degree) compared to those who were treated shorter based on Equation 4. The sample and covariates are the same as in Table 3. 90%-confidence intervals plotted.

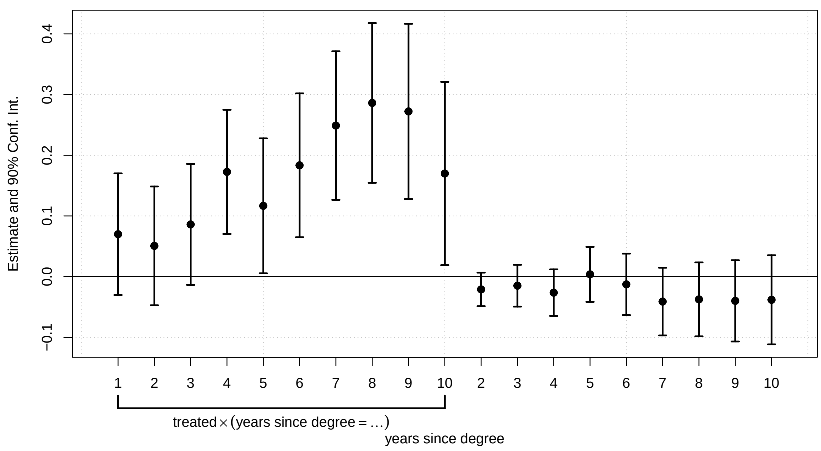
Figure D.7: ITT by federal state



Note: The graph displays the ITT for each federal state based on the interaction of the treatment indicator and the federal state in which the degree was obtained based on Equation 4. The sample and the remainder of the covariates are the same as in Table 3. 90%-confidence intervals plotted.

E Dynamic analysis

Figure E.1: ITT of exposure to tuition fees: Dynamic analysis (extended)



Note: The graph displays the ITT by the years since degree completion based on the interaction of the treatment indicator and the years since degree completion. I.e., the first ten coefficients are the same coefficients are displayed in Figure 2. The remaining coefficients represent the baseline coefficients for the link between years since degree completion and the likelihood of overeducation (the first year after graduation forms the reference category). The regression is based on Equation 5. The sample is based on SOEP data using all repeated observations of graduates referred to in Table 3 surveyed between 1985 to 2022. It is restricted to individuals observed within ten years following degree completion. All covariates and fixed effects of Table 3 are included.

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