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Academic cost of student mobility: COVID-19 restrictions as a natural experiment

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Luis Rumert*

Academic Cost of Student Mobility: COVID-19 Restrictions as a Natural Experiment

Abstract

The COVID-19 pandemic forced university students to transition to online learning due to mobility restrictions and campus closures. When in-person teaching resumed, many students had to commute or move closer to campus and adapt to a new learning and social environment. This paper examines how this mid-study return to campus impacted academic performance and whether all students had to bear the same costs. Using administrative student data from a public university in North Rhine-Westphalia, Germany and a difference-in-differences approach, the results show an overall significant but small decrease in passed credit points and the number of registered exams. The effects increase over time and reach a 14 percent decrease in passed credit points and a 13 percent decrease in registered exams after five semesters. Additionally, the overall dropout probability decreases by 33 percent. The estimated effects are heterogenous with respect to cohorts, sex, and migration background. Moreover, the cost of student mobility increases by distance.

JEL-Codes: I23, R23, I38

Keywords: Student mobility; reopening of universities; academic performance; difference-in-differences

March 2025

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1 Introduction

During the COVID-19 pandemic, university students were required to study from home as mobility was severely restricted and campuses were closed. Many students struggled with the abrupt disruption of their social lives and the increased isolation caused by the risk of infection and recurring public lockdowns between 2020 and 2022. While currently enroled students had to transition to online learning, first-year students had to begin their academic education remotely, missing out on the traditional transition to higher education. When campuses reopened, and in-person teaching resumed, some students faced a new social and learning environment with the necessity of commuting or moving to attend classes.

In this study, I investigate how this shift back to campus affected university students' academic performance, utilizing administrative student data from a typical public university in Germany. Investigating the relationship between student mobility and academic performance poses challenges due to selection bias. For example, students who graduated from high schools in regions not close to the university can differ in intrinsic motivation or resilience compared to those who enroled in universities closer to home. Therefore, a direct comparison between these groups would be misleading. However, the unique circumstances created by the COVID-restrictions provide an opportunity to address this issue: the administrative student panel data employed in this study contains academic performance records from before students were required to relocate to the university. Since university campuses had to close down and offer teaching online[1], first-year students most likely stayed at their parents' homes after high school graduation. After the reopening of universities, students who lived close to campus did not have to move, while students who lived further away had to adjust, either by moving closer to campus or by having to bear higher commuting costs. This allows for a difference-in-differences (DiD) approach, where I can use pre-treatment data from students who enroled under the restrictions to identify how student mobility affects academic performance.

My results show that student mobility leads to a statistically significant decrease of 0.56 in passed credit points (a 5 percent decline compared to the pre-treatment control group average) and 0.29 in the number of registered exams (7 percent decrease). Surprisingly, I also observe a significant reduction in the dropout probability by 1.7 percentage points (33 percent). The decrease in academic performance and dropout probability can be explained by study progression. Second-year students drive the reduction in academic performance, while first-year students drive the reduction in dropout probability. The estimates for credit points and registered exams increase in magnitude over time, increasing up to a 14 percent decrease in passed credit points and a 13 percent decrease in registered exams. Furthermore, my

[1] Only some practical courses were reorganized and offered on campus in a form that was compliant with the federal hygiene rules. Examples are machinery project work for engineering students or lab sessions for science students. Exams were mostly written on campus. Student life and on-campus events were abolished almost completely.

results show that female students react stronger to the treatment than male students. While students without a migration background experience a decline in registered exams and passed credit points, the estimates for students with a migration background show a decrease only in dropout probability. Finally, my findings suggest that greater distances amplify the treatment effects of student mobility.

I expect the treatment to negatively impact academic performance through two potential channels: *(1)* students who commute may spend less time studying, or *(2)* students who move to the university city may allocate time to work more to finance the additional cost of living or establish a new social and learning environment, reducing study time. The dataset does not contain direct observational data on changes in student residence, which means that a precise disentanglement of these mechanisms is not possible. However, since new students had no reason to relocate to campus under the restrictions, I can utilize information on the high school graduation district as a proxy to determine whether a student experienced the treatment.

Multiple studies have examined how geographic factors influence educational access and outcomes, such as socio-economic neighborhood quality (Agostinelli et al., 2024; Laliberté, 2021; Weinhardt, 2014) or macroeconomic factors like local public spending and economic trends (Marchand & Weber, 2020; Litschig & Morrison, 2013). However, this paper focuses on the cost of mobility of students itself rather than structural differences between regions. In the high school context, Cordes et al. (2019) rely on administrative data to estimate the effects of residential mobility on the performance of New York City public school students. They find that short-distance moves improve student performance, whereas long-distance moves negatively impact school outcomes.

In higher education, the most closely related literature examines the relationship between academic outcomes and geographic or spatial variations in proximity. For example, Vieira et al. (2018) show that greater distance between a university and a student's family residence negatively affects academic performance. Alm & Winters (2009) find that a shorter distance to the nearest college increases the likelihood of enrolling in the local higher education system. Similarly, Do (2004) uses the proximity of high-quality colleges to demonstrate that low-income students who grow up closer to such institutions are more likely to attend higher-quality colleges. Additionally, my paper relates to the literature on the effects of college transfers, as student mobility is a common consequence of these transitions. Notably, Miller (2023) employs regression discontinuity designs to estimate the impact of transfers on educational and labor market outcomes, showing that lateral transfers between non-flagship, four-year institutions increase bachelor's degree completion rates.

Similar to my empirical setting, there have been multiple studies over the past years that utilize the COVID-19 pandemic as an exogenous event in a DiD study to estimate changes in academic performance (Bird et al., 2022; De Paola et al., 2023; Gonzalez et al., 2020). However, this study presents a quasi-experimental setting constructed differently than most other DiD-based approaches. I

exclusively investigate cohorts who enroled under pandemic restrictions and utilize the abolition rather than the introduction of restrictions as an exogenous event. In addition to the abolition of COVID-19 restrictions, and similar to Rodríguez-Planas (2022), I condition the treatment on a second variable: distance between the in this paper investigated university and the high school district of graduation. Lastly, and similar to Schwerter et al. (2024), I apply the DiD approach of Callaway & Sant'Anna (2021) in the context of a German university to address the potential biases of the two-way fixed effects (TWFE) estimator.

The remainder of this study is structured as follows: Chapter 2 describes the data. Chapter 3 explains the empirical setting in more detail and discusses the identification and estimation strategy. Chapter 4 provides the main results, with additional figures and tables in the Appendix. Chapter 5 summarizes my findings, and I discuss the limitations of my study and potential further research.

2 Data

The analysis utilizes standardized administrative data collected at the student level. This student data has to be collected at every German university (§3 HStatG) and includes demographic variables, information on university entrance qualifications, and examination outcomes. The raw dataset includes historical data on 68,124 students over a time period from 2007 to 2024 and is based on a standard public university with ten different faculties, offering a wide variety of study programs. The university is located at the centre of North Rhine-Westphalia, Germanys most populated state, and it benefits from the connectivity of urban areas, fostering a diverse student body.[2]

I restricted the dataset to only include the four student cohorts who enroled under the university's COVID-19 restrictions: summer term 2020 (ST20), winter term 2020 (WT20), summer term 2021 (ST21), and winter term 2021 (WT21). Only bachelor students are included in the analysis, and I removed exchange students and visiting students since they are only temporarily enroled. Furthermore, to receive estimates representative of the average student population, I additionally removed students older than 30 years at time of matriculation, students who already received a bachelor's degree, students who passed more than 45 credit points in a semester[3], and students who, until the treatment, stayed completely inactive (never registered for a single exam). Due to the lack of tuition fees, German universities have significant shares of completely inactive students (benefiting from student subsidies, such as cheap public transportation tickets) without actively pursuing a degree (Berens et al., 2024).[4] The resulting sample includes 4,629 students and the data is shown in Table 1.

[2] The Appendix includes additional information on the institutional framework.

[3] The workload suggested by the university is 30 credit points per semester, but most students pass fewer. For students who pass significantly more credits, it is very likely that they transferred credits from other institutions, so I remove these outliers.

[4] These students would bias the results making them less representative of "actual" students.

Table 1
Descriptive table of variables.

Variables	Mean	SD	Min	Max
(a) Demographic				
Male	0.43	0.50	0.00	1.00
Migration background	0.49	0.50	0.00	1.00
Age at matriculation	19.78	2.04	17.00	30.00
Private health insurance	0.09	0.29	0.00	1.00
HS GPA	2.48	0.57	1.00	4.00
Distance HS district (km)	35.84	56.80	0.00	522.70
Low income HS district uni	0.34	0.47	0.00	1.00
Academic HS degree	0.68	0.47	0.00	1.00
German HS degree	1.00	0.03	0.00	1.00
(b) Program				
Dropout prob. degree	0.49	0.14	0.15	1.00
Enroled STEM program	0.54	0.50	0.00	1.00
Switched study program	0.14	0.35	0.00	1.00
Winter enrollment	0.98	0.15	0.00	1.00
(c) Academic				
Passed exams per semester	4.13	2.78	0.00	17.00
Passed credits per semester	15.36	10.14	0.00	45.00
Average GPA exams	3.13	1.19	1.00	5.00
Registered exams per semester	5.15	2.47	0.00	17.0
No. Students 4,629				

Notes: The table describes the sample of students that was used for the estimations in the following sections. All students enroled in the university under the COVID-19 restrictions. High school is abbreviated with HS. For the empirical analysis, a few additional data cleaning steps were required, i.e., the removal of three observations where the sex was undefined, imputing empty cells for passed credit points and registered exams in a semester with 0, imputing empty cells for average grade in a semester with 5.0 (which is the failing grade and represents no study progression), and removing four students with missing information on the district of their high school but no foreign high school degree.

While citizenship is included in the data, there is no information on student's migration history. However, a name analysis allowed me to estimate the probability of a second-generation migration background (Berens et al., 2019). [5] A continuous enrolment at the university defines the "status" of a student, i.e., if a student is labeled as a graduate, dropout, or active student. If a student leaves the university for at least one semester without receiving a degree, the student is labeled as a dropout. A student is labeled as a graduate if that student receives a bachelor's degree (if the student is enroled in multiple programs, I label that student as a graduate for receiving a degree in one of the programs). A new observation is added to the dataset if the student re-enrols in a bachelor program. Lastly, I collected administrative data on the district levels provided by the Federal Statistical Office of Germany. The data

[5] I thank Jörg Michael for providing us with the database.

is used to add information on whether the student graduated from a high school in a district with a public university, graduated in a city with more than 100,000 inhabitants, and to add information on the district average income of private households.

For the treatment definition in this study, the distance (kilometer) between the high school district and the university is essential. It is measured using the direct distance between the centroid of the district where the university is located and the centroid of the high school district where the student graduated, provided by the geolocation service *Nominatim*.[6] Districts in Germany are administrative areas larger than municipalities but smaller than states. Almost all large cities (i.e., cities with more than 100.000 inhabitants) are districts, and Germany has around 400 districts.

Citizenship is used to identify a first-generation migration background, while the dummy variable second-generation migration background is determined based on citizenship and a name analysis. The variable low-income high school district equals one if the student graduated from a district in the bottom quartile of the distribution for average household income. Although there are multiple pathways to obtaining a university entrance qualification in Germany, most university entrants attend an academic high school (Gymnasium). The variable STEM enrolment is set to one if a student is enroled in a STEM program in the first semester. The variable switched program equals one if students change their major during their studies.

3 Empirical Strategy

Setting

To estimate how student mobility affected students, I exploit the abolition of university teaching restrictions as an exogenous event in a quasi-experimental DiD setting. Students who received their high school degree in a different district than the one where the university is located become treated after the abolition of university restrictions. Since there is no information on student's addresses, I have to assume that the high school district (and the distance between the university and the high school district) is a good proxy for the district where the student lives and studies until the abolition of restrictions.[7] I view this assumption as valid because I only focus on students who enroled under COVID-19 restrictions. These students likely stayed at their parents' homes until high school graduation and also afterward since campuses were closed. Students who graduated in the city of the university are the control group. In Figure 1, the district of control students is marked by a blue border. The darker shading shows that most students enroled at the university come from districts nearby.

I adopt a DiD framework instead of a simple OLS model because students from the city where the university is located may systematically differ from students from other regions. One indicator is the variation in observable demographic characteristics, as shown in Table A.1 in the Appendix. Differences

[6] The estimation was performed in Python (3.9) with the geopy package (2.4.1).

[7] In her study, Miller (2023) also uses high school location as a proxy for a student's hometown.

in economic criteria could also drive this bias. For instance, the university's city lies in the bottom quarter of the district average income of private households' distribution. In contrast, only 18 percent of students in the treatment group come from low-income districts (see Table A.1 in the Appendix). Students from lower-income households have less access to educational resources, which could affect their academic performance at the university. Moreover, there might be additional unobserved differences influencing academic outcomes. This suggests that assuming unconfoundedness for causal inference is not appropriate.

Fig. 1. Home districts of university students.
Notes: The figure shows the city where the university is located, marked in blue, and the surrounding German districts. University campus locations are marked as red dots. The colour of the districts indicates the number of students who received the high school diploma in that district. A darker colour indicates more students.

Under the COVID-restrictions, four different cohorts enroled. Of these cohorts, the students who graduated from high school outside the university city received the treatment in different semesters. The first student cohort was treated in their fifth semester (summer cohort 2020/ group five), and the last cohort who enroled under COVID-19 restrictions was treated in their second semester (winter cohort 2021/ group two). Students who graduated in the university city are never-treated students, independent of their cohort. This results in an unbalanced panel where every cohort has five outcome periods, but a varying number of pre-treatment periods. The left panel of Figure 2 visualizes the timing of the treatment for the respective cohorts. All cohorts were affected at the same time (at the start of the summer term 2022) and observed until the end of the summer semester of 2024.

However, if I use a simple DiD model and compare all treated students with all never treated students, I observe some deviation in pre-trends. Alternatively, I can alter the panel structure to compare students on a semester level resulting in a setting where students experienced the treatment in a staggered manner.[8] This results in the panel structure shown in the right panel of Figure 2. For a staggered DID estimation, I have to include a covariate in my model that indicates the abolition of the COVID-19

[8] A similar approach is proposed in Berens et al. (2024).

restrictions, which allows me to differentiate between students who *(1)* graduated from high school outside the city where the university is located (Treatment) and those who *(2)* only experienced the abolition of the COVID-19 restrictions (Event).

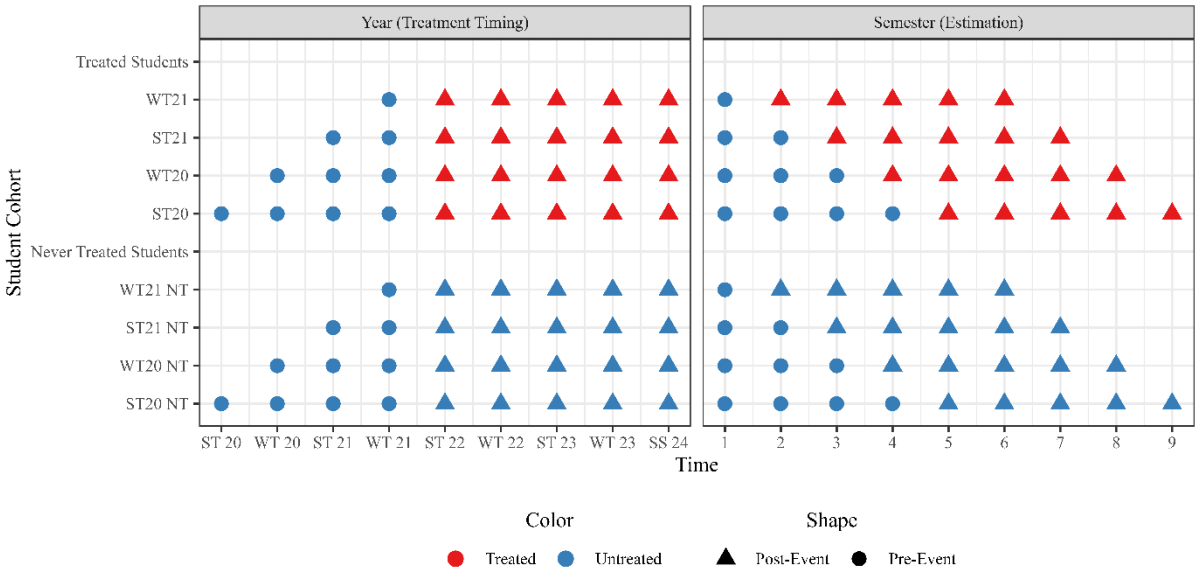

Fig. 2. Treatment and event timing for every cohort in relative and absolute semesters.
Notes: The figure shows the absolute and relative treatment and event timing for treated and never-treated students of all four cohorts. Shape shows if a group of students experienced the event, which is the abolition of the COVID-19 restrictions at the university. Color shows if a group of students experienced the treatment.

To estimate the potential effect of student mobility, I compare students who became treated (students with red triangles) with students who experienced the event (blue triangles) in the respective semester. Otherwise my estimates would be biased by the pre- post-difference of the abolition of the COVID-19 restrictions. Estimating the outcomes using this staggered setting has the advantage of comparing students at the same level of study progress (as in the right panel of Figure 2). One possible approach in this setting is the (Dynamic-) TWFE estimator. However, this approach has estimation issues if there are heterogenous treatment effects between groups or over time in the static estimation (e.g., Goodman-Bacon, 2021; Chaisemartin & D'Haultfoeuille, 2020), as well as in the dynamic estimation (Sun & Abraham, 2021). The difference-in-differences estimator suggested by Callaway & Sant'Anna (2021) was developed to solve the issues related to the TWFE estimator and allows the estimation with an unbalanced panel.

Estimation and Identification

The method proposed by Callaway & Sant'Anna (2021) estimates the average treatment effect on the treated (ATT) in a classical 2x2 DiD manner for different groups and treatment periods, ATT(g, t), where g is the semester a group of students experience the shift to face-to-face teaching and t is the semester (ranges from one to eight). For the estimation, I use never-treated students as the control group.[9]

[9] In my specific case, due to conditioning treatment on the event, not-yet-treated students would never be relevant in the post treatment estimation. This is because not-yet-treated students never experience the event

Under the canonical parallel trends assumption in equation (1), the ATT in a simple 2x2 DiD model can be estimated by equation (2),

$$E[Y_t(0) - Y_{t-1}(0)|\mathrm{D} = 1] = E[Y_t(0) - Y_{t-1}(0)|\mathrm{D} = 0], \qquad (1)$$

$$ATT = E[Y_t - Y_{t-1}|\mathrm{D} = 1] - E[Y_t - Y_{t-1}|\mathrm{D} = 0], \qquad (2)$$

where D equals 1 if students are in the treatment group and $Y_t(0)$ is defined as the (potential) untreated outcome. Callaway & Sant'Anna (2021) augment the canonical parallel trends assumption in equation (3) to allow the inclusion of never-treated students as the control group and to enable the inclusion of covariates X,

$$E\big[Y_t(0) - Y_{t-1}(0)|X, G_g = 1\big] = E[Y_t(0) - Y_{t-1}(0)|X, C = 1], \qquad (3)$$

where G_g equals one if students in group g are treated, C equals one if a student is never-treated, and $t \in T$ with $t \geq g$. Therefore, the main identification assumption states that the difference in the untreated outcome of students in group g is equal to the difference in the untreated outcome of never treated students, conditional on covariates. Based on this assumption, without anticipation of the treatment, the $\mathrm{ATT}(g,t)$ for the treated students using never-treated students as the control group can be estimated in a non-parametric way by equation (4), using inverse-probability weighting to account for covariates,

$$ATT(g,t) = E\left[\left(\frac{G_g}{E\big[G_g\big]} - \frac{\frac{p_g(X)\,\mathrm{C}}{1-p_g(X)}}{E\left[\frac{p_g(X)\,\mathrm{C}}{1-p_g(X)}\right]}\right)\big(Y_t - Y_{g-1}\big)\right], \qquad (4)$$

where $p_g(X)$ is the probability of being first treated in period g conditional on covariates. Afterwards, the $\mathrm{ATT}(g,t)$ can be aggregated into various weighted estimates of interest. In this study, I present overall post-treatment averages, estimates aggregated by student cohorts, and estimates aggregated into an event-study.

As mentioned above, one challenge to my identification of the effect of student mobility is the lack of residential data. For the identification, I assume that treated students did not move to the city where the university is located before the abolition of the university restrictions since almost all courses were taught online from the ST in 2020 until the ST of 2022 and most events related to university life were cancelled during that time. Therefore, student mobility should (almost) exclusively occur after the return to face-to-face lectures. However, the lack of information on actual residence means that some students might violate the assumption. Therefore, my estimates should be interpreted as an intention to treat.

without experiencing the treatment. In my view, never-treated students yield a cleaner comparison because they are used for the pre and post treatment estimation. In the end, using not-yet-treated students as the control group results in only marginal differences.

Several studies have addressed the drawbacks of the TWFE estimator in a continuous treatment setting (e.g., Callaway et al., 2024; Chaisemartin & d'Haultfoeuille, 2024; Chaisemartin et al., 2022). However, since I use distance to define treated students in a discretized form, estimating the effect of student mobility using a continuous treatment is an intuitive extension. Hence, I address potential treatment intensity in the result section below.

4 Results

In this section, I first describe the general results. Afterward, I will address treatment intensity and the potential heterogeneity of my estimates. Figure 3 presents average dynamic pre- and post-treatment estimates at the 95% confidence level for all outcomes. They show no statistically significant deviation in pre-trends for passed credit points, average grade, registered exams, and dropout probability in the baseline specification.[10] To test the validity of my estimates, I define additional specifications where the control group consists of students who received their high school degree in a district closer than 20 and, alternatively, closer than 40 kilometers (km) to the university city.[11] In general, the alternative specifications show a decrease in significance and size of the estimates, which most likely comes from the alignment in post-treatment trends after the inclusion of students who lived outside of the city of the university (and are now included in the control group). Still, most of my estimates are robust to alternative specifications.

Table 2 presents the main results, and the focus lies on the baseline specification, i.e., the specification where the control group consists only of students from the university city (Base in the Tables). On average, students affected by student mobility show a marginally significant 0.56 decrease in passed credit points. This corresponds to a 5 percent decrease after treatment compared to the pre-treatment average in the control group. The outcome is driven by the last period, which shows a decrease of 1,76 credit points (14 percent). Since the baseline specification shows a marginally significant deviation before treatment, and my estimates lose significance over the alternative specifications, the estimate should be interpreted carefully. Registered exams show an average decrease of 0.29 (7 percent), while the last period shows a decrease of 0.86 (13 percent).

[10] All cohort-specific and average dynamic plots in this paper include covariates on sex, migration background, age at matriculation, type of high school degree, high school GPA, the average dropout probability of the study program, and a variable that indicates the abolition of COVID-19 restrictions.

[11] Figure A.1. in the Appendix visualizes the additional control districts. Figures A.2. and A.3. show event-study plots in the alternative specifications. They show that, on average, there are only marginal differences in the pre-trends between the three specifications.

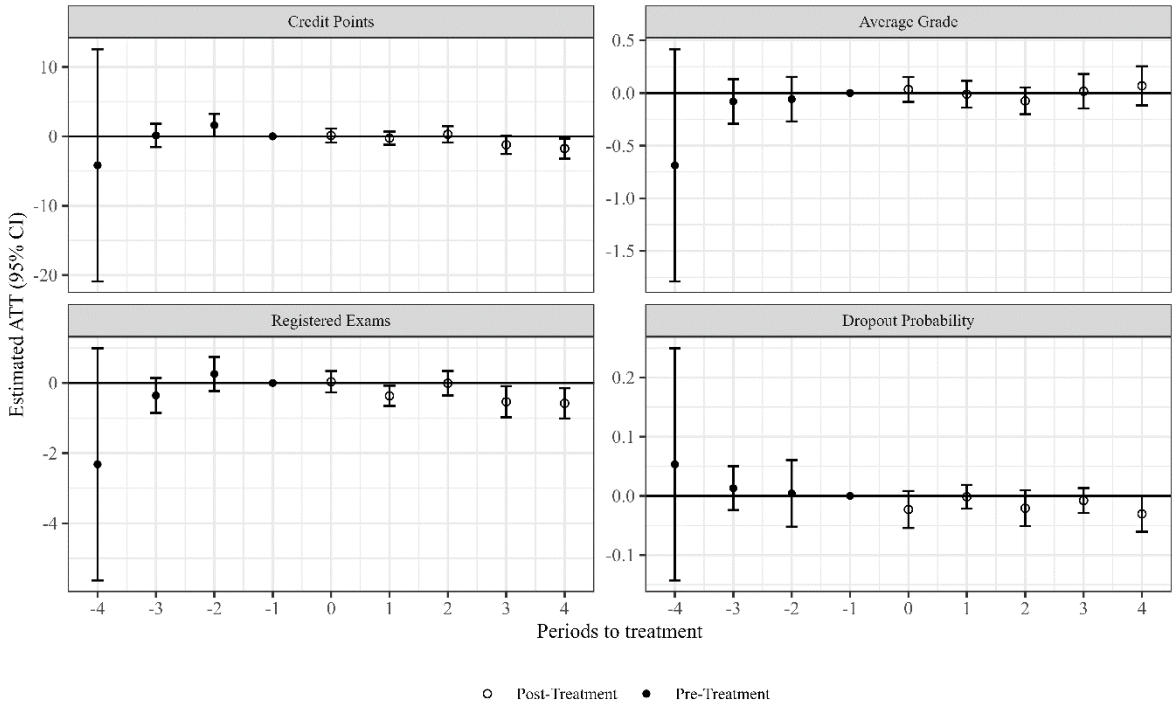

Fig. 3. Average dynamic pre-trends and outcomes in the baseline specification at the 95% confidence level.
Notes: Graphical representation of pre-trends to assess the validity of the parallel trends assumption for all outcomes of interest in the baseline specification. All point estimates before the treatment are marked by filled circles.

In contrast, the results show that student mobility did not affect average grades. Overall, my results indicate that students experienced a long-term decrease in study workload but not outcome quality, as measured by grades. Lastly, I observe a relatively large and statistically significant decrease of 1.7 percentage points (33 percent) in the probability of dropping out of the university. Similar to the other outcomes, the estimates increase over time, with the last period showing a decrease of 3.1 percentage points (60 percent).

Table 2
Estimates for dynamic pre-trends and outcomes for all three specifications.

Period	Credit Points			Average Grade			Registered Exams			Dropout Probability		
	Base	20 km	40 km	Base	20 km	40 km	Base	20 km	40 km	Base	20 km	40 km
Mean Pre	12.42	13.59	14.63	3.45	3.36	3.29	4.41	4.68	5.00	0.052	0.054	0.055
-4	-4.18	-7.91	-5.77	-0.69	0.66	-0.58	-2.32	-2.12	-3.65	-0.053	0.138	0.274
	(6.51)	(6.03)	(5.83)	(0.42)	(0.83)	(1.22)	(1.21)	(2.46)	(2.55)	(0.073)	(0.124)	(0.144)
-3	0.13	-0.11	0.31	-0.08	-0.10	-0.04	-0.36	-0.34	-0.31	0.013	-0.003	-0.010
	(0.65)	(0.55)	(0.59)	(0.08)	(0.07)	(0.08)	(0.18)	(0.15)	(0.19)	(0.014)	(0.011)	(0.013)
-2	1.61*	1.07	1.45*	-0.06	-0.06	-0.12	0.26	0.08	0.03	0.004	0.005	0.008
	(0.63)	(0.50)	(0.60)	(0.08)	(0.06)	(0.08)	(0.18)	(0.16)	(0.18)	(0.021)	(0.015)	(0.018)
-1	-	-	-	-	-	-	-	-	-	-	-	-
0	0.13	-0.10	0.30	0.03	-0.01	0.02	0.04	-0.00	-0.02	-0.023	-0.016	-0.015
	(0.39)	(0.34)	(0.42)	(0.05)	(0.04)	(0.05)	(0.11)	(0.09)	(0.12)	(0.012)	(0.009)	(0.011)
+1	-0.26	-0.06	0.44	-0.01	-0.01	0.02	-0.37***	-0.35***	-0.25	-0.001	-0.005	0.007
	(0.37)	(0.33)	(0.43)	(0.05)	(0.04)	(0.05)	(0.11)	(0.09)	(0.13)	(0.007)	(0.007)	(0.009)
+2	0.29	0.23	0.31	-0.08	-0.10**	-0.11	-0.01	-0.03	-0.08	-0.021	-0.006	-0.008
	(0.46)	(0.36)	(0.46)	(0.05)	(0.04)	(0.05)	(0.13)	(0.10)	(0.13)	(0.011)	(0.009)	(0.011)
+3	-1.21	-0.53	-0.61	0.02	-0.00	0.03	-0.53***	-0.37**	-0.45**	-0.008	-0.012	-0.002
	(0.52)	(0.41)	(0.52)	(0.06)	(0.05)	(0.06)	(0.16)	(0.13)	(0.16)	(0.008)	(0.007)	(0.009)
+4	-1.76**	-1.04	-0.64	0.07	-0.01	-0.02	-0.58***	-0.46***	-0.45**	-0.031**	-0.015	-0.025**
	(0.56)	(0.43)	(0.52)	(0.07)	(0.05)	(0.07)	(0.16)	(0.12)	(0.17)	(0.011)	(0.008)	(0.009)
Avg. Post	-0.56*	-0.30	-0.21	0.01	-0.03	-0.01	-0.29***	-0.24***	-0.25**	-0.017**	-0.010*	-0.009
	(0.34)	(0.28)	(0.33)	(0.04)	(0.04)	(0.04)	(0.09)	(0.08)	(0.11)	(0.008)	(0.006)	(0.008)

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. (Robust) Standard errors are presented in parenthesis. Mean Pre shows the outcome over all cohorts and semesters before treatment in the control group. Period -1 is the base period and never-treated students are the control group. The model includes covariates on sex, migration background, age at matriculation, type of high school degree, high school GPA, the average dropout probability of the study program, and a variable that indicates the abolition of COVID-19 restrictions.

One reason for the smaller estimates in earlier post-treatment periods could be the stickiness of returning to regular teaching practices. Online learning resources were likely still provided to the students after the campus re-opened, and the return to exclusive face-to-face teaching was completed only after some semesters. Also, observing a reduction in academic performance and dropout probability at the same time seems surprising at first. However, Figure 4 suggests that cohorts reacted differently to the treatment, depending on their study progression. Students who show a decrease in passed credit points and registered exams express a small reduction in dropout probability but do not drive the overall estimates.

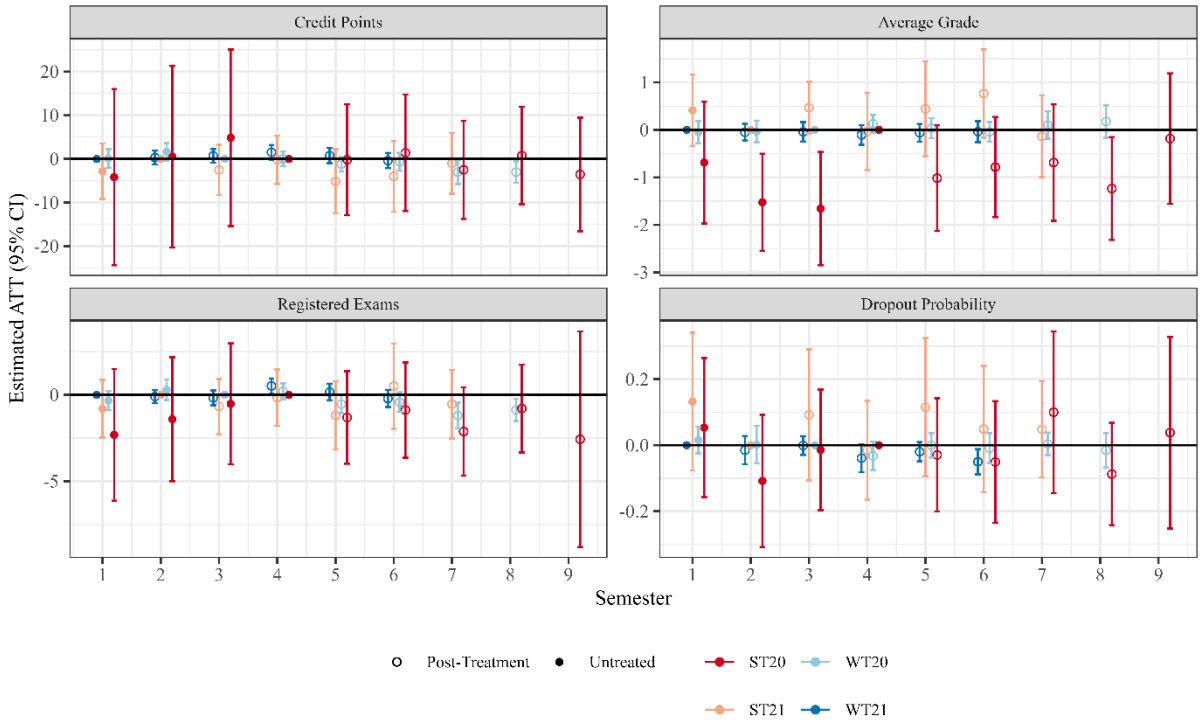

Fig. 4. Cohort-specific pre-trends and outcomes in the baseline specification at the 95% confidence level.
Notes: Cohort-specific graphical representation of pre-trends and outcomes to assess the validity of the parallel trends assumption for all outcomes of interest in the baseline specification. As shown in Figure 2, every cohort is treated in a different semester and is observed over a varying number of semesters. All point estimates before the treatment are marked by filled circles, and a different color is used for every cohort.

Figure 4 shows no statistically significant deviation in pre-trends for credit points, registered exams, and dropout probability for all cohorts at the 95% confidence level.[12] Only the ST20 cohort shows a deviation for the average grade. Students from the WT20 cohort drive the reduction in passed credit points and registered exams. Students in this cohort had to study from home for a long time (compared to the WT21 cohort) and, therefore, seem to have struggled to adjust to the new social and learning environment. On the other hand, the WT21 cohort drives the reduction in dropout probability. These students studied under the restrictions for only one semester and seemingly benefitted from the return to campus.

After moving to the university city, students possibly incurred (sunk) costs and might have been less willing to make that decision again and stayed enroled. Another explanation for the reduction in dropout probabilities could be a higher social integration at the university for students who moved there after campus life restarted (Tinto, 1975).[13] These explanations are supported by the findings presented below,

[12] The confidence intervals of the summer cohorts are wider than the ones of the winter cohorts, which can be explained by the comparably low enrolment number of students. Most study programs start in the winter semester. The cohort sizes are ST20: 55 students, WT20: 2,420 students, ST21: 50 students, WT21: 2,226 students. The Appendix includes Figures A.4. and A.5., which show plots for pre-trends and outcomes at the 95% confidence level in the alternative 20 and 40 km specifications for each group.

[13] Tinto's model of student retention resolves around institutional integration. There are two main channels: academic integration (through good academic performance and intellectual development) and social integration (through peer and faculty-staff interactions).

which show that treatment effects increase for students who live further away and are more likely to move. Students from the closest districts show smaller and less significant outcomes.

To address treatment intensity without a continuous treatment, the control group stays unadjusted in the alternative specifications. For example, in the 20 km specification, I do not include districts closer than 20 km to the city of the university in the control group. Still, the treatment group comprises students from districts further away than 20 km (analogously for the 40 km specification). Figure 5 visualizes the adjustment of the treatment group.[14] Table 3 shows how my results change over distances.

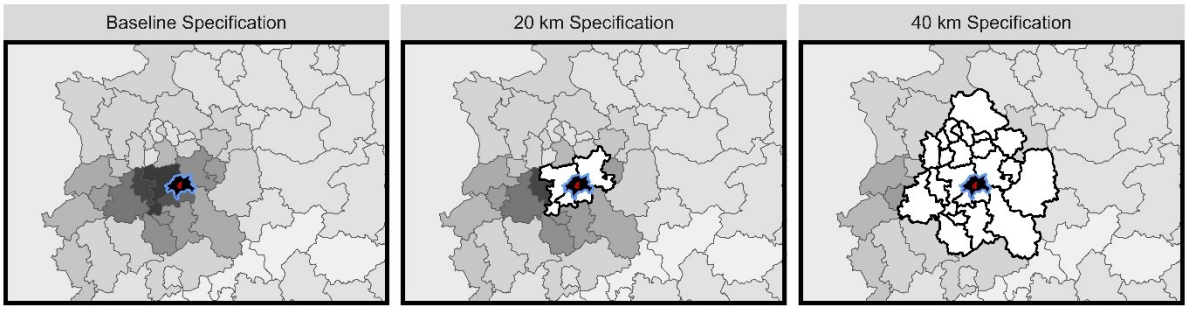

Fig. 5. Districts removed for the treatment intensity setting.
Notes: The graph shows which districts are removed from the treatment group over the 20 km and 40 km specifications. Therefore, treated students have to relocate from further away.

Overall, results show that the increase in distance leads to stronger estimates for passed credit points, registered exams, and dropout probability. This is true especially for those periods with significant or relatively large estimates. In the last period, students who have to relocate from more than 40 km away show an additional decrease of 2 percentage points in passed credit points, 4 percentage points in registered exams, and 13 percentage points in the dropout probability compared to the baseline specification. Therefore, my results suggest that treatment intensity affects student performance through distance.

Additionally, I can remove students from the treatment group who received their high school diploma in a district further away than 20 km or 40 km. Therefore, I only keep students in the treatment group who can commute to the university relatively easily (visualized in Figure A.8. in the Appendix). Table A.2 in the Appendix shows the resulting estimates. The results show that the estimates get smaller and become less significant as I remove students who graduated in districts further away, which aligns with the results presented in Table 3. In the 40 km setting, most estimates persist in size and overall significance. If I remove students from districts between 20 km and 40 km away, the estimates get smaller, and some become insignificant.

[14] Figures A.6. and A.7. in the Appendix show the average dynamic pre- and post-treatment estimates at the 95% confidence level for all outcomes in the intensity setting.

Table 3
Estimates for dynamic pre-trends and outcomes for all three specifications in the intensity setting.

Period	Credit Points			Average Grade			Registered Exams			Dropout Probability		
	Base	20 km	40 km	Base	20 km	40 km	Base	20 km	40 km	Base	20 km	40 km
Mean Pre	12.42	12.42	12.42	3.45	3.45	3.45	4.41	4.41	4.41	0.052	0.052	0.052
-4	-4.18	-1.39	-6.77	-0.69	-0.59	-1.41	-2.32	-1.68	-3.97	-0.053	0.122	0.278
	(6.51)	(6.98)	(5.09)	(0.42)	(0.63)	(0.91)	(1.21)	(1.82)	(2.24)	(0.073)	(0.129)	(0.158)
-3	0.13	-0.07	-0.30	-0.08	-0.08	-0.01	-0.36	-0.41	-0.48	0.013	-0.008	0.014
	(0.65)	(0.94)	(0.94)	(0.08)	(0.09)	(0.11)	(0.18)	(0.20)	(0.27)	(0.014)	(0.012)	(0.019)
-2	1.61*	1.83*	2.20**	-0.06	-0.06	-0.07	0.26	0.26	0.22	0.004	0.008	0.012
	(0.63)	(0.70)	(0.81)	(0.08)	(0.09)	(0.12)	(0.18)	(0.21)	(0.27)	(0.021)	(0.019)	(0.024)
-1	-	-	-	-	-	-	-	-	-	-	-	-
0	0.13	0.05	0.42	0.03	-0.02	0.04	0.04	0.10	0.18	-0.023	-0.025	-0.027
	(0.39)	(0.45)	(0.56)	(0.05)	(0.05)	(0.06)	(0.11)	(0.12)	(0.15)	(0.012)	(0.012)	(0.013)
+1	-0.26	-0.27	0.20	-0.01	-0.01	0.01	-0.37***	-0.38**	-0.32	-0.001	-0.003	0.007
	(0.37)	(0.57)	(0.57)	(0.05)	(0.05)	(0.06)	(0.11)	(0.12)	(0.16)	(0.007)	(0.008)	(0.010)
+2	0.29	0.22	0.20	-0.08	-0.08	-0.10	-0.01	-0.02	0.02	-0.021	-0.021	-0.027
	(0.46)	(0.52)	(0.57)	(0.05)	(0.05)	(0.06)	(0.13)	(0.14)	(0.19)	(0.011)	(0.011)	(0.014)
+3	-1.21	-1.37*	-1.75	0.02	-0.01	0.04	-0.53***	-0.64***	-0.82***	-0.008	-0.009	-0.003
	(0.52)	(0.56)	(0.75)	(0.06)	(0.06)	(0.08)	(0.16)	(0.17)	(0.16)	(0.008)	(0.008)	(0.010)
+4	-1.76**	-1.88***	-1.95**	0.07	-0.03	-0.03	-0.58***	-0.66***	-0.73***	-0.031**	-0.029*	-0.038**
	(0.56)	(0.60)	(0.76)	(0.07)	(0.07)	(0.08)	(0.16)	(0.18)	(0.21)	(0.011)	(0.012)	(0.014)
Avg. Post	-0.56*	-0.65*	-0.58	0.01	-0.01	0.01	-0.29***	-0.31***	-0.33**	-0.017**	-0.017**	-0.018*
	(0.34)	(0.37)	(0.46)	(0.04)	(0.04)	(0.05)	(0.09)	(0.11)	(0.14)	(0.008)	(0.008)	(0.009)

Notes: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. (Robust) Standard errors are presented in parenthesis. Mean Pre shows the average outcome over all cohorts and semesters before treatment in the control group. Never-treated students are the control group. In this table, only the treatment group changes over the three specifications (this also means that the Mean Pre does not change over the three different specifications). The model includes covariates on sex, migration background, age at matriculation, type of high school degree, high school GPA, the average dropout probability of the study program, and a variable that indicates the abolition of COVID-19 restrictions. The number of observations changes over the specifications since I remove students who graduated in a high school district closer than 20 km (40 km) to the city where the university is located.

Lastly, I investigate the potential heterogeneity of my results. Figure 6 presents estimates over student characteristics for all outcomes of interest. The green Base estimate is similar to the average post estimate shown in Table 2 but without covariates. The first thing to notice is the comparably high responsiveness of older students compared to younger ones. One explanation might be that younger students are more adaptable to new environments and disruptions, showing a weaker reaction to the treatment. Additionally, Base estimates are close to younger students' estimates since they comprise the largest part of my sample.

Female students react stronger to student mobility than male students and show a decrease in passed credit points, registered exams, as well as dropout probability. Male students stay relatively unaffected. The variable migration background shows a different dynamic. While students without a migration background experienced a decrease in passed credit points and registered exams, students with a migration background showed a decrease in the dropout probability.

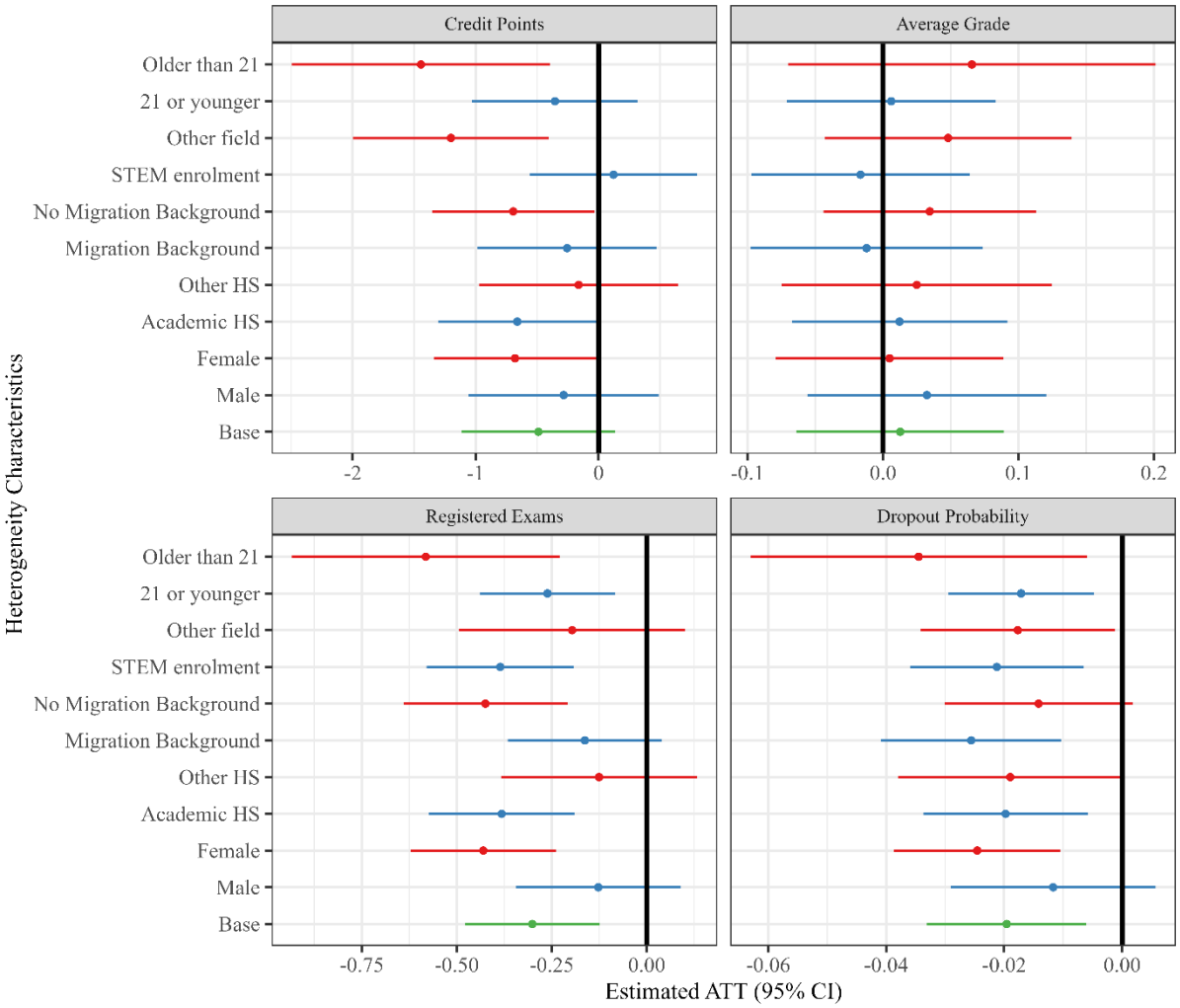

Fig. 6. Heterogeneity over student characteristics in the baseline specification at the 95% confidence level.
Notes: The graph shows post average estimates over different student characteristics without control variables. For the estimation, I only include students from the control and treatment group that fit the respective characteristic on the y-axis. Estimates can be compared to their respective counterpart or the base estimate.

Figure 7 presents heterogeneity estimates over high school districts for all outcomes of interest. For passed credit points, especially students who relocate from smaller cities and rural areas show a decrease. Regarding dropout probability, students from a low-income district[15] show a decrease in their dropout probability. In contrast to my initial expectation, students from districts where a public university is located do not show an increase in their dropout probability. Since students could enrol and attend classes from anywhere during the pandemic, I expected that students close to another public institution would try to enrol there and, consequently, drop out. However, my estimates do not support this hypothesis.

[15] High- and low-income districts correspond to the top and bottom quartiles of the district average income of private households distribution.

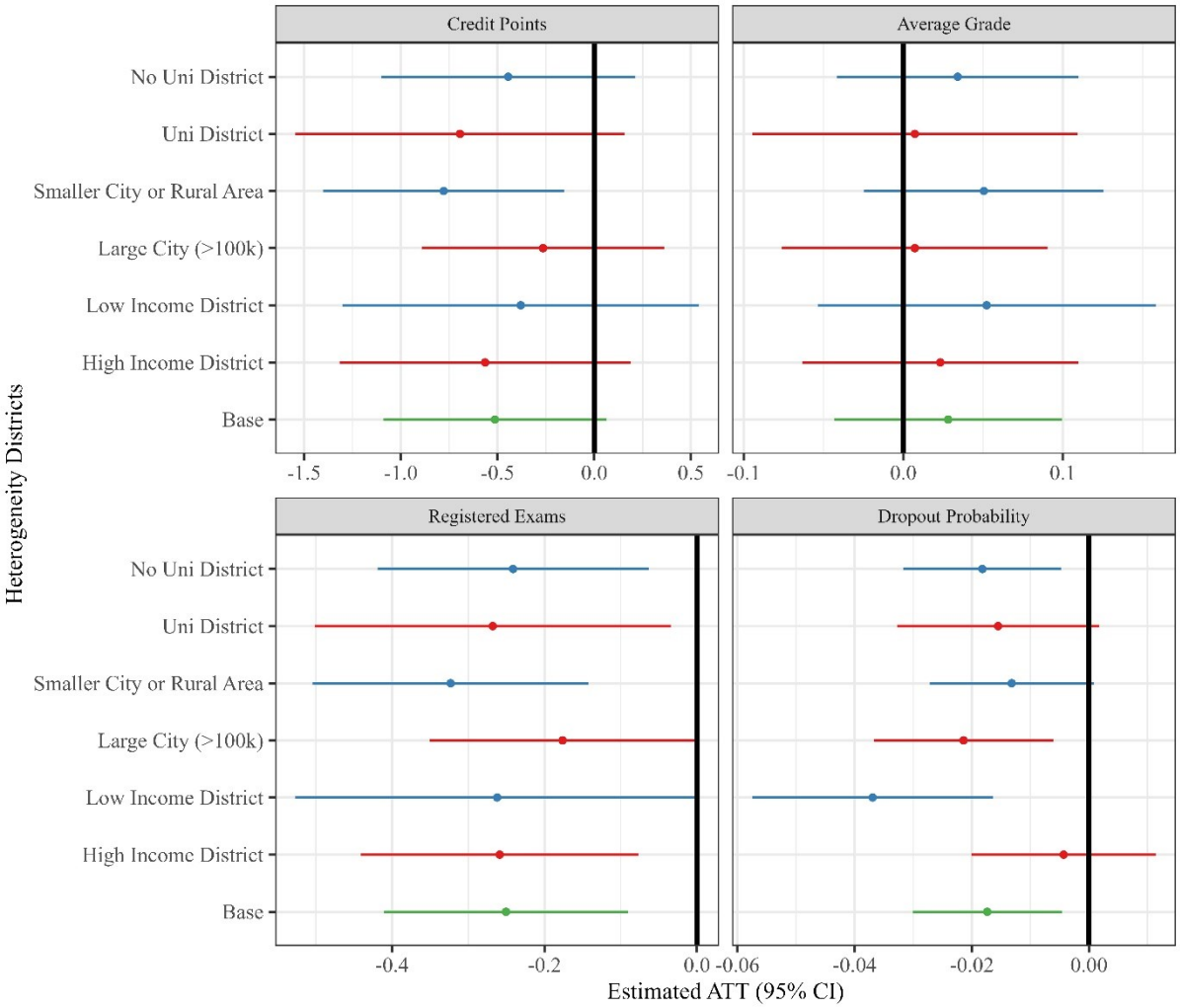

Fig. 7. Heterogeneity over district characteristics in the baseline specification at the 95% confidence level.
Notes: The graph shows post average estimates over different district characteristics without control variables. For the estimation, I include all students from the control group and only students from the treatment group that graduated from high school in a district that fits the respective characteristic on the y-axis. Estimates can be compared to their respective counterpart or the base estimate.

5 Conclusion

This paper investigates the impact of student mobility on academic performance, utilizing administrative data from a public university and exploiting teaching restrictions as a quasi-natural experiment. Overall, student mobility led to a decrease of 0.56 (5 percent) in the number of passed credit points and a decrease of 0.29 (7 percent) in registered exams. These estimates increase over time, showing a decrease of 1.76 (14 percent) for passed credit points and 0.58 (13 percent) for registered exams in the last period. Overall, average grades stay unaffected. The dropout probability shows the most prominent effect, with an overall decrease of 1.7 percentage points (33 percent). The winter cohort of 2020 drives the estimates for passed credit points and registered exams, while students from the winter cohort of 2021 drive the reduction in dropout probability. Heterogeneity estimates show that female students were affected stronger than male students. Students without a migration background experienced a decrease in

registered exams and passed credit points, while students with a migration background showed a decrease in their dropout probability.

In addition, my results indicate that the effects of student mobility are primarily driven by students from districts located farther from the university. In contrast, students from nearby districts exhibit small and statistically insignificant effects, suggesting that those who move to the university city account for the observed outcomes. This implies that the provision of student housing could be an effective strategy to support students and improve retention rates. Since student housing programs are common at most universities, such measures can be implemented easily and rolled out quickly. Both winter cohorts experience a reduction in their dropout probabilities, although the cohort treated earlier in their studies benefits more from the intervention. Prioritizing first-year students in the allocation of housing could therefore improve the efficiency of such programs.

One limitation of this study is the need for observational residential data. Further research could use this information to improve the estimation and come closer to a causal effect of student mobility. In addition, the comparably short pre-treatment time horizon limits the strength of my argument for parallel trends and, subsequently, a causal interpretation of my estimates. Another limitation of my study is external validity, as with most single university datasets. It would be interesting to see if similar studies to this one would lead to the same results.

Since most studies investigate the switch from face-to-face to online teaching, further research could focus on how students who started their studies online and had to switch to face-to-face teaching adapted to their new learning environment. Students who started their studies under “normal” circumstances returned to a familiar setting when the pandemic ended. On the other hand, students who enroled under COVID-19 restrictions had to navigate an unfamiliar setting without the help of generous examination support and, potentially, without an intact social network comprised of other students. These students might need (different) supporting measures compared to those already familiar with day-to-day campus life. I could not investigate this research question with my sample since crucial identification assumptions were violated. However, this violation could be specific to my sample, and data from another institution could be used to investigate this research question further.

Declaration of competing interest

The author has no competing interest that need to be declared.

Data availability

The data used is confidential and can only be used for research purpose by staff of the respective institution.

Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work the author used ChatGPT & Grammarly in order to improve the grammar and the readability of the manuscript. After using this tool/service, the author reviewed and edited the content as needed and takes full responsibility for the content of the published article.

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Appendix

Additional Information on the Institutional Framework

The German higher education system encompasses three primary categories of institutions: public universities, universities of applied sciences, and colleges (of arts and music). Public universities primarily emphasize theoretical and research-oriented education and charge a biannual administrative fee, typically around 300 euros. These fees cover student services and a public transportation pass and are significantly cheaper than tuition fees in countries like the United States. Bachelor's degree programs typically last six semesters in Germany, while master's programs typically span four semesters. On average, university bachelor's students take 7.9 semesters (13.3 for master's students) to graduate (Maaz et al., 2022).

When the COVID-19 pandemic reached Germany in the spring of 2020, most universities followed legal guidelines implemented by their state governments and switched from face-to-face to online teaching. The campus was closed around the middle of March, and the start of the summer semester of 2020 was postponed to the last third of April. In early April, the federal government announced that all teaching would shift to online formats due to the high insecurity of continued lockdowns and social restrictions. Consequently, university teaching was primarily organized as online semesters from the summer semester of 2020 until the summer semester of 2022. At the university investigated here, the mandatory supply of online teaching materials was abolished at the start of the summer semester of 2022, and face-to-face teaching was officially announced to be the regular format again.

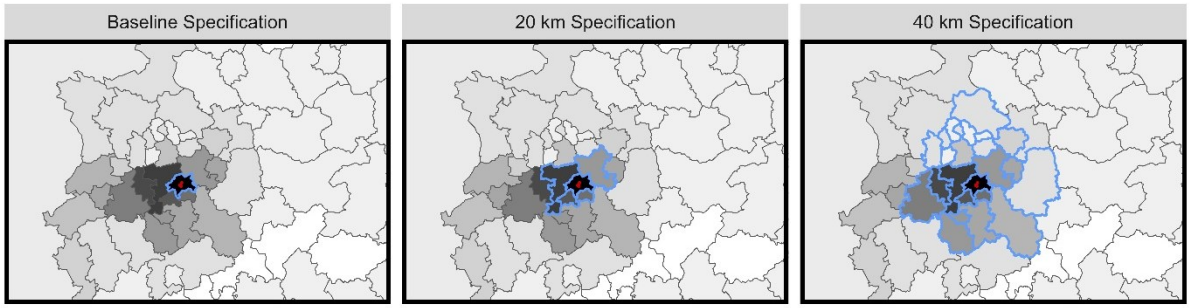

Fig. A.1. Additional districts included in the control group.
Notes: The graph shows which additional districts are included in the control group in the alternative specifications. I include additional districts to test the robustness of my estimates.

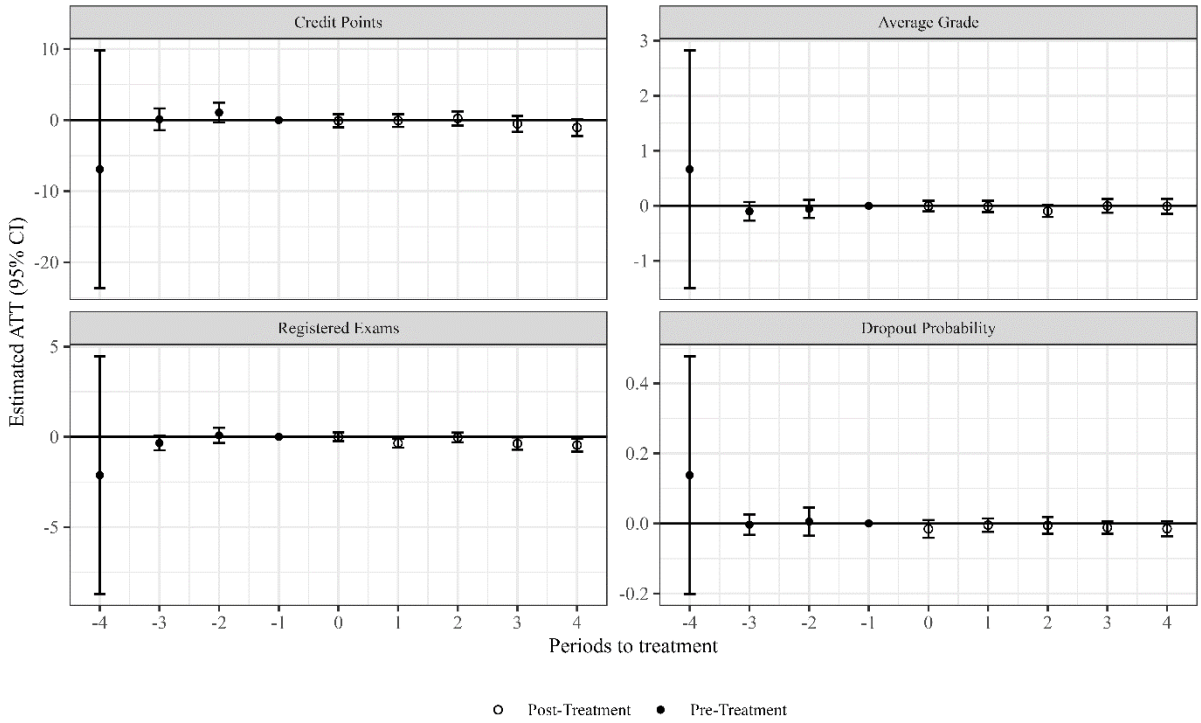

Fig. A.2. Dynamic pre-trends and outcomes in the 20 km specification at the 95% confidence level.
Notes: Graphical representation of pre-trends to assess the validity of the parallel trends assumption for all outcomes of interest in the 20 km specification. All point estimates before the treatment are marked by filled circles.

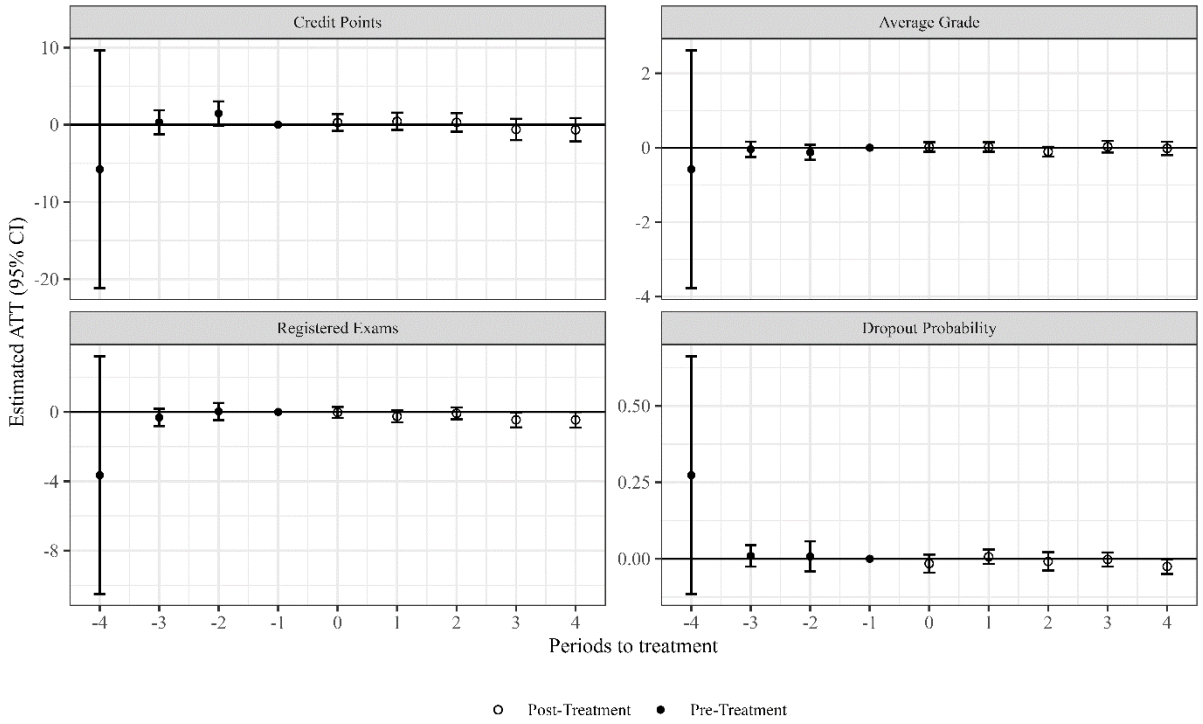

Fig. A.3. Dynamic pre-trends and outcomes in the 40 km specification at the 95% confidence level.
Notes: Graphical representation of pre-trends to assess the validity of the parallel trends assumption for all outcomes of interest in the 40 km specification. All point estimates before the treatment are marked by filled circles.

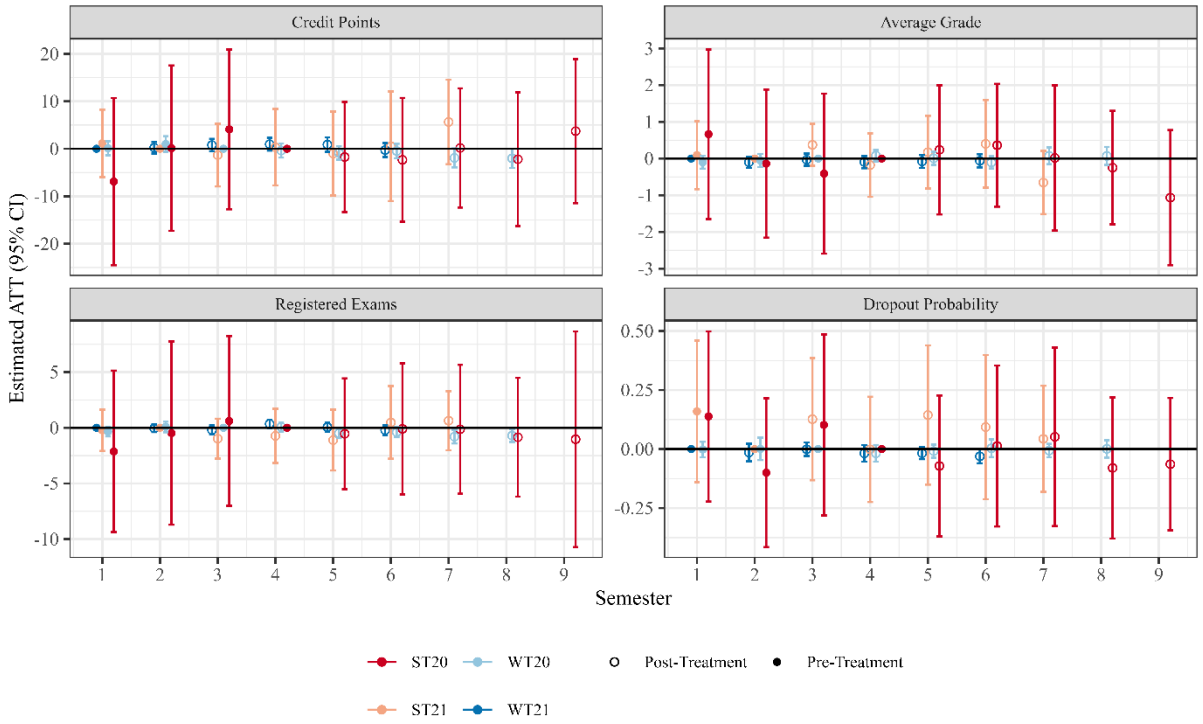

Fig. A.4. Cohort-specific pre-trends and outcomes in the 20 km specification at the 95% confidence level.
Notes: Cohort-specific graphical representation of pre-trends to assess the validity of the parallel trends assumption for all outcomes of interest in the 20 km specification. All point estimates before the treatment are marked by filled circles, and a different color is used for every cohort.

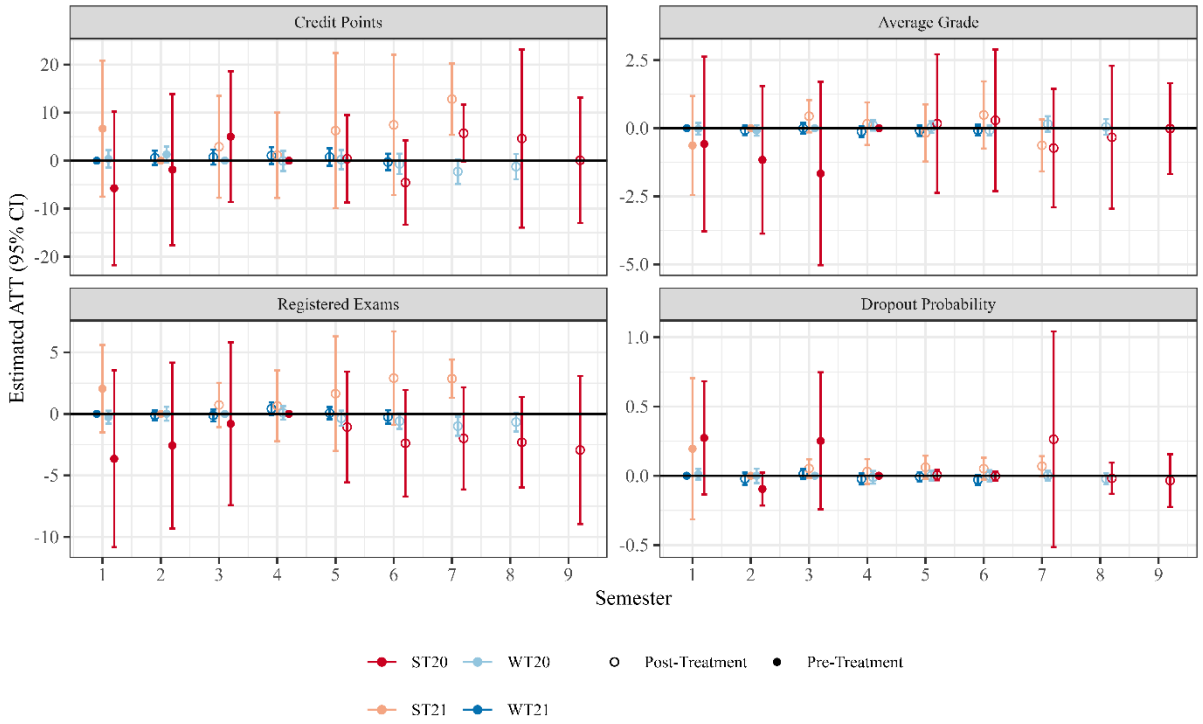

Fig. A.5. Cohort-specific pre- trends and outcomes in the 40 km specification at the 95% confidence level.
Notes: Cohort-specific graphical representation of pre-trends to assess the validity of the parallel trends assumption for all outcomes of interest in the 40 km specification. All point estimates before the treatment are marked by filled circles, and a different color is used for every cohort.

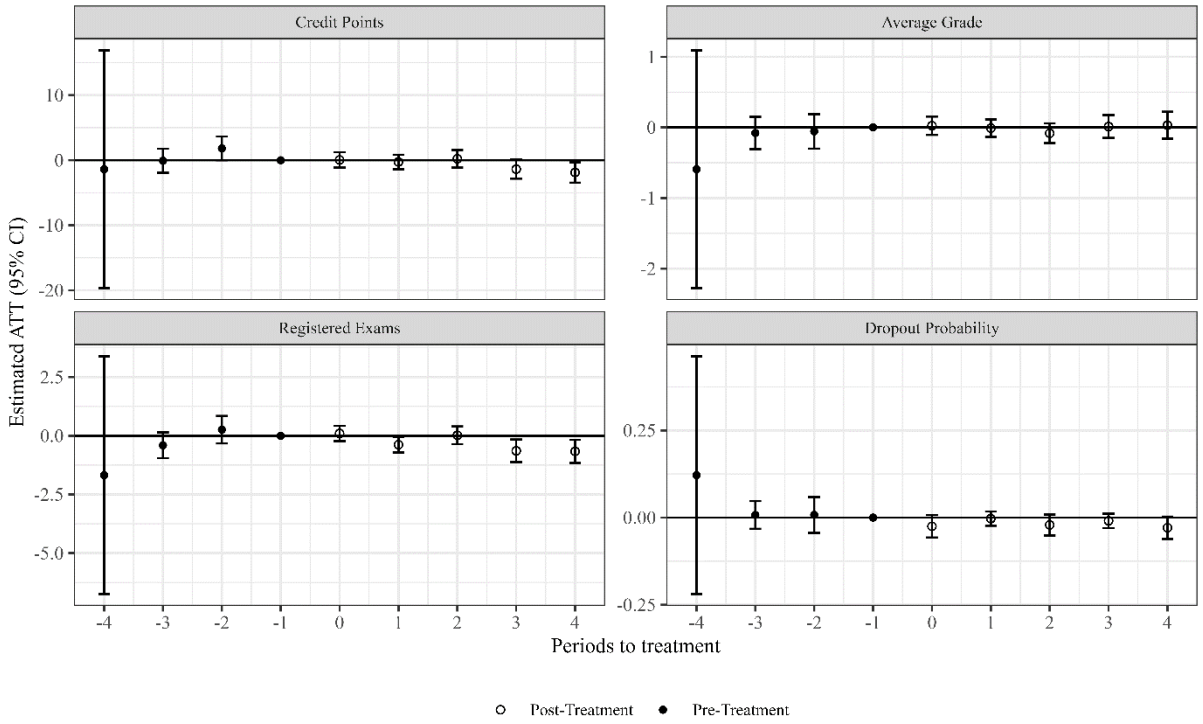

Fig. A.6. Dynamic pre-trends and outcomes in the 20 km specification treatment intensity setting at the 95% confidence level.
Notes: Graphical representation of pre-trends to assess the validity of the parallel trends assumption for all outcomes of interest in the intensity setting. The graph shows the 20 km specification. All point estimates before the treatment are marked by filled circles.

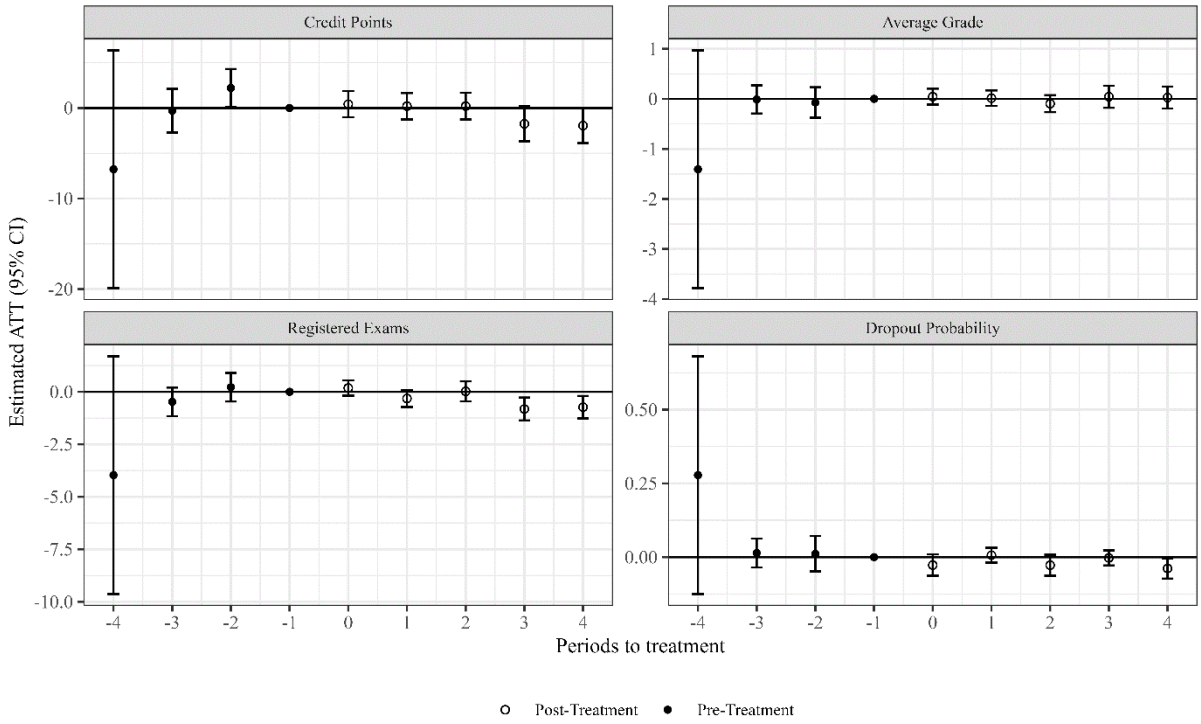

Fig. A.7. Dynamic pre-trends and outcomes in the 40 km specification treatment intensity setting at the 95% confidence level.
Notes: Graphical representation of pre-trends to assess the validity of the parallel trends assumption for all outcomes of interest in the intensity setting. The graph shows the 40 km specification. All point estimates before the treatment are marked by filled circles.

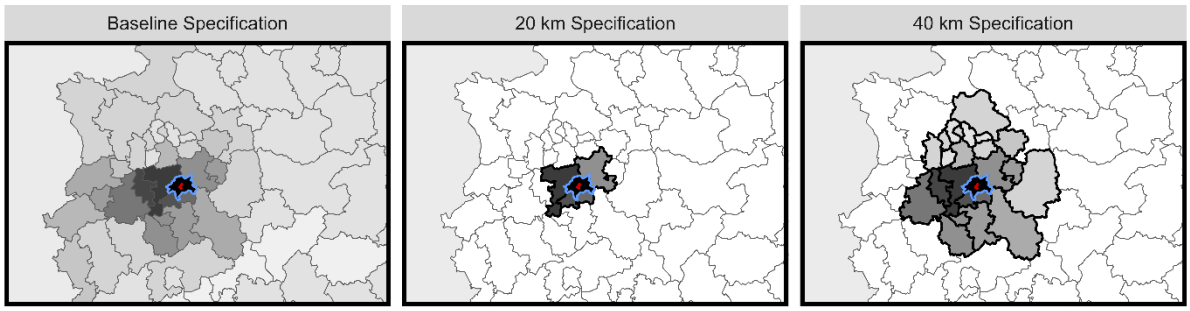

Fig. A.8. Districts removed for the commuting setting.
Notes: The graph shows which districts are removed from the treatment group over the 20 km and 40 km specifications. Therefore, treated students have to relocate from closer by.

Table A.1
Descriptive table of variables for treatment and control group in the baseline specification.

Baseline Specification:	**Control**		**Treatment**	
Variables	**Mean**	**SE**	**Mean**	**SE**
(a) Demographic				
Male	0.56	0.02	0.40	0.01
Migration Background	0.57	0.02	0.47	0.01
Age at Matriculation	19.83	0.07	19.77	0.03
Private Health Insurance	0.08	0.01	0.10	0.00
HS GPA	2.49	0.02	2.48	0.01
Distance HS District Uni (km)	0.00	0.00	44.62	0.99
Low Income HS District	1.00	0.00	0.18	0.01
Academic HS Degree	0.60	0.02	0.70	0.01
German HS Degree	1.00	0.00	1.00	0.00
(b) Program				
Dropout Prob. Degree	0.49	0.00	0.49	0.00
Enroled STEM Program	0.54	0.02	0.54	0.01
Switched Study Program	0.17	0.01	0.14	0.01
Winter Enrollment	0.98	0.00	0.98	0.00
(c) Academic				
Passed Exams per Semester	3.13	0.09	4.37	0.05
Passed Credits per Semester	12.70	0.35	16.01	0.17
Average GPA passed Exams	3.36	0.04	3.07	0.02
Registered Exams per Semester	4.51	0.07	5.30	0.04
No. Students	911		3,718	

Notes: The table describes the sample divided into students who received their high school degree in the university city and students who received their degree outside of the city. In the baseline specification of my empirical analysis, these two groups of students resemble the control and treatment group. Students from the control group have an average of one for low-income high school district because the university city is a low-income high school district itself. All students enroled in the university under the COVID-19 restrictions. High school is abbreviated with HS.

Table A.2
Estimates for dynamic pre-trends and outcomes for all three specifications in the commuting setting.

Period	Credit Points			Average Grade			Registered Exams			Dropout Probability		
	Base	20 km	40 km	Base	20 km	40 km	Base	20 km	40 km	Base	20 km	40 km
Mean Pre	12.42	12.42	12.42	3.45	3.45	3.45	4.41	4.41	4.41	0.052	0.052	0.052
-4	-4.18	-6.17	-2.07	-0.69	-0.60	-0.38	-2.32	-2.82	-1.75	-0.053	-0.028	0.009
	(6.51)	(7.11)	(6.39)	(0.42)	(0.35)	(0.44)	(1.21)	(1.34)	(1.24)	(0.073)	(0.129)	(0.073)
-3	0.13	-0.51	0.29	-0.08	-0.06	-0.10	-0.36	-0.22	-0.31	0.013	0.022	0.013
	(0.65)	(0.74)	(0.65)	(0.08)	(0.09)	(0.08)	(0.18)	(0.21)	(0.18)	(0.014)	(0.015)	(0.013)
-2	1.61*	1.18	1.44	-0.06	-0.03	-0.04	0.26	0.26	0.27	0.004	-0.002	0.003
	(0.63)	(0.70)	(0.61)	(0.08)	(0.08)	(0.07)	(0.18)	(0.22)	(0.18)	(0.021)	(0.024)	(0.019)
-1	-	-	-	-	-	-	-	-	-	-	-	-
0	0.13	0.27	0.04	0.03	0.07	0.03	0.04	-0.09	0.01	-0.023	-0.018	-0.022
	(0.39)	(0.44)	(0.40)	(0.05)	(0.06)	(0.04)	(0.11)	(0.13)	(0.10)	(0.012)	(0.016)	(0.012)
+1	-0.26	-0.27	-0.43	-0.01	-0.01	-0.02	-0.37***	-0.33	-0.39***	-0.001	-0.002	-0.004
	(0.37)	(0.46)	(0.57)	(0.05)	(0.06)	(0.05)	(0.11)	(0.12)	(0.11)	(0.007)	(0.011)	(0.007)
+2	0.29	0.48	0.35	-0.08	-0.04	-0.06	-0.01	-0.06	-0.02	-0.021	-0.020	-0.027
	(0.46)	(0.55)	(0.43)	(0.05)	(0.06)	(0.05)	(0.13)	(0.16)	(0.12)	(0.011)	(0.014)	(0.014)
+3	-1.21	-0.72	-1.00	0.02	0.02	0.01	-0.53***	-0-25	-0.43**	-0.008	-0.005	-0.010
	(0.52)	(0.67)	(0.54)	(0.06)	(0.08)	(0.06)	(0.16)	(0.20)	(0.16)	(0.008)	(0.010)	(0.008)
+4	-1.76**	-1.36	-1.65**	0.07	1.14	0.08	-0.58***	-0.42	-0.53***	-0.031**	-0.033	-0.028
	(0.56)	(0.67)	(0.55)	(0.07)	(0.09)	(0.07)	(0.16)	(0.19)	(0.15)	(0.011)	(0.015)	(0.012)
Avg. Post	-0.56*	-0.32	-0.54	0.01	-0.04	0.01	-0.29***	-0.23	-0.28***	-0.017**	-0.015*	-0.017**
	(0.34)	(0.41)	(0.35)	(0.04)	(0.06)	(0.04)	(0.09)	(0.12)	(0.10)	(0.008)	(0.009)	(0.008)

Notes: *** p < 0.01, ** p < 0.05, * p < 0.1. (Robust) Standard errors are presented in parenthesis. Mean Pre shows the average outcome over all cohorts and semesters before treatment in the control group. Never-treated students are the control group. In this table, only the treatment group changes over the three specifications (this also means that the Mean Pre does not change over the three different specifications). The model includes covariates on sex, migration background, age at matriculation, type of high school degree, high school GPA, the average dropout probability of the study program, and a variable that indicates the abolition of COVID-19 restrictions. The number of observations changes over the specifications since I remove students who graduated in a high school district further away than 20 km (40 km) to the university city.