Student Debt and Labor Market Outcomes: Evidence from the Unlucky Generation in the Netherlands

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Job Market Paper

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This version: 5th December 2025

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Abstract: This paper investigates the causal effect of student debt on early-career labor market outcomes. I exploit a 2015 Dutch reform from a grant-reliant system to a loan-based regime, which created a sharp exogenous increase in debt for cohorts that started university from 2015. Using rich administrative data, I employ a difference-in-differences strategy that takes advantage of variation in study grant loss. I find that higher student debt did not lead to a significant reduction in university enrollment or delay in completion. However, it did cause a notable decrease in earnings and an increase in the likelihood of securing permanent employment within one year after graduation. Higher debt is associated with more part-time work during university, shorter job-searching spell, and reduced enrollment in master's programs. These findings suggest the psychological burden of debt harms human capital accumulation after undergraduate studies and the realization of their accumulated human capital, consistent with a "debt aversion" channel.[1]

[1]I am deeply grateful to my advisors, Prof. Peter Koudijs and Clemens Mueller, for their invaluable guidance and support throughout this project. I also thank Matthijs Korevaar, Eva Mulder, Daniel Karpati, Mikael Paaso and seminar participants at Erasmus University Rotterdam for their helpful comments and suggestions. This paper includes own computations based on non-public microdata of Statistics Netherlands (project 9817). All remaining errors are my own. E-mail: xiangli@ese.eur.nl

1 Introduction

Higher education is increasingly expensive in most developed economies, and student debt continues to increase in many countries. Based on the Federal Reserve Bank of New York's official reports, America's student loan debt exceeds $1.6 trillion, impacting over 40 million borrowers and holding its position as the second largest non-housing debt category for over a decade. Investing in human capital is one of the most important decisions individuals make in their life. Despite the centrality, the labor-market consequences of student debt remain surprisingly understudied. This paper provides rigorous causal evidence on the early-career labor-market effects of student debt and discusses the mechanisms behind the effects in an institutional environment characterized by generous income-contingent repayment, universal tuition subsidies, and near-zero default rates.

The effects of student debt on earnings remain ambiguous in theory . On the one hand, individuals typically begin to pay back when their earnings are relatively low. Debt holders also face higher financial costs on other borrowings, such as mortgages, which tighten their constraints. These pressures may lead student debt holders to focus on short-term financial security over long-term career growth—pushing them toward higher-paying jobs or longer working hours. On the other hand, students with debt can prioritize employment to start repaying. As a result, students tend to be less picky in the labor market and may be more likely to accept part-time jobs, as well as lower paid jobs.

Estimating the causal effects of student debt on labor market outcomes has always been challenging due to endogeneity concerns. Borrowing and attaining higher education are often determined simultaneously. Higher education is typically viewed as an investment in human capital, where students weigh the expected longer lifetime returns from improved labor market prospects against total costs. Consequently, students who anticipate strong labor market outcomes are more inclined both to pursue higher education and to take on debt, while those who are debt-averse or uncertain about future returns may be reluctant to enroll or may minimize their reliance on loans. These decisions are also shaped by heterogeneity in family wealth, financial literacy, and risk preferences. This study addresses such endogenous problems by utilizing rich administrative data and providing evidence that higher debt did not deter or defer the obtain of bachelor's degree as a first step, and sheds light on how student debt causally affects early career earnings.

I take advantage of a recent Dutch reform as a natural experiment. The new regime abolished the universal basic student grant (*basisbeurs*) and replaced it with an expanded loan facility (*sociaal leenstelsel*) for all new bachelor's students starting in September 2015. The reform generated a large, unanticipated increase in expected lifetime borrowing—approximately €12,000 on average—for post-reform cohorts, while leaving supplementary grants for low-income families intact. It is crucial for identification that the supplementary grant (*aanvullende beurs*) was not only retained, but substantially increased to largely offset the loss of the basic grants for students coming from lower-income families. Eligibility and amount depended continuously on parental gross income. The maximum grant was available to students with a gross parental income below €31,200 (the exact threshold varied slightly by year and family composition), and then the grant gradually phased out until it reached zero for a gross parental income of €46,600. This institutional design delivers clean quasi-experimental variation in treatment intensity along the distribution of parental income. Students from the poorest households (well below the threshold) saw their net borrowing increase only modestly because the boosted supplementary grant largely compensated for the loss of the basic grants. Students from lower middle-income households just above the cutoff — the widely known "unlucky" group in Dutch media — did not receive an offset supplementary grant and therefore faced the entire shock. This variation in grant loss allows me to implement a difference-in-differences strategy effectively across cohorts.

I employ comprehensive administrative register data from Statistics Netherlands (CBS) covering the full universe of Dutch students and their complete labor-market trajectories. I compare adjacent cohorts who entered the university two years before and three years after the cutoff of the budget cut by the Dutch government. There are three main challenges to this empirical design. First, the status of the reform's treatment is formally defined by the academic year of first enrollment in bachelor's higher education: students whose first registration occurs in the 2015/2016 academic year or later are fully exposed to the loan regime. However, the actual enrollment year is potentially endogenous because students near the cutoff could strategically accelerate entry, for example, by skipping a gap year, to secure the basic grant rather than face the loan system. To eliminate this source of selection, I instrument the actual enrollment status with predicted treatment based on the birth cohort. As students are mostly expected to enter universities at the age of 18, the 1994 to 1996 cohorts are the unaffected cohorts as controls, and for individuals born in 1997 and 1998, they are expected to start university from or after the 2015/2016 academic year.

Second, I address potential threats to identification stemming from unobserved secular trends across cohorts. For example, a general increase in the demand for higher education could increase enrollment rates for younger cohorts. To distinguish the policy shock from these confounding time factors, I corroborated my main results by estimating the difference-in-differences specification across multiple, varying cohort windows. By systematically restricting the sample, ranging from the entire 1994–1998 cohorts down to a narrow comparison of more immediately adjacent cohorts (for instance, 1996 vs. 1997). This approach ensures that my findings are driven by discrete policy changes rather than underlying changes in educational attainment.

A final threat to identification arises from the potential for endogenous intra-household transfers. Wealthier parents may increase financial support to compensate for the reduction in government grants, thereby neutralizing the liquidity shock for their children. This substitution effect is well-documented in the literature [Abbott et al., 2019], and such a compensatory behavior would violate the assumption of parallel trends. To mitigate this concern, I perform a robustness check designed to isolate a set of students whose parents face binding financial constraints and lack the resources to make potentially large private transfers. I re-estimate the difference-in-differences specification on progressively restricted samples, imposing an upper threshold on gross parental income that varies from €100,000 down to €50,000 in €1,000 increments in the year of enrollment into university. By systematically lowering this ceiling, I test whether the treatment effect remains in the same pattern when excluding students from better-off families, thereby relying on a more homogeneous subsample where the assumption of parallel trends is most plausible.

I begin by establishing a foundational result with respect to the investment of human capital. A central concern surrounding the transition from grants to loans is that the increased financial burden might deter risk-averse or credit-constrained students from pursuing higher education. A large body of literature has established that financial aid and borrowing constraints can significantly influence educational trajectories, and increased access to loans has been shown to improve college persistence and degree attainment [Sun and Yannelis, 2016, Solis, 2017, Marx and Turner, 2019, Denning, 2019, Black et al., 2023]. In contrast to this hypothesis, I document estimated null effects on both university enrollment and timely degree completion. The replacement of grants with loans did not alter the probability that students enroll in a research university, nor did it affect their likelihood of graduating by age 21 through 24. These results remain across the income distribution, indicating that even students from lower-income backgrounds — who theoretically faced the tightest binding constraints — did not reduce their educational attainment in response to the policy. These null findings serve as a critical stepping stone for identifying the causal effect of debt on labor market trajectories. The reform did not induce selection effects—neither scaring away marginal students nor altering the composition of the graduating cohort—I can rule out the concern that after-graduation outcomes are driven by unobserved changes in the ability or background of the graduate pool. Consequently, the cohort of students treated by the policy represents a set of human capital comparable to the control group, but differs only in their financial burdens.

Based on unaffected educational choices, this study could yield more credible results of higher debt on labor market outcomes. The analysis of labor market outcomes begins by investigating the intermediate margin of the school-to-work transition. The liquidity constraints associated with student debt may increase the urgency of entering the labor market, especially in the American setting, prompting graduates to secure employment more quickly to meet repayment obligations. To test whether this hypothesis holds in the continental European setting, I restrict the sample to students who successfully completed their bachelor's degree by the age of 24 and define the outcome as a binary indicator for being hired within six months of graduation. This specification shows that the switch to a loan-based system did not alter whether graduates entered the workforce or their chance of getting hired. The main reason for the null results should be the generous repayment suspension in the Netherlands [2].

Next, I turn to the central measure of the early-career outcome: initial earnings. I restrict the analysis to employed graduates and define the outcome variable as the natural logarithm of annual earnings in the first full tax-filing year. Using the same difference-in-difference frameworks, the starting wages of treated cohorts decreased by approximately 2.6% under the loan-based system compared to their grant-funded peers.

[2] The repayment architecture for Dutch student loans, administered by the Education Executive Agency (*Dienst Uitvoering Onderwijs*), functions as an income-contingent mechanism designed to shield borrowers from liquidity shocks post-graduation. Following a statutory two-year grace period (*aanloopfase*), amortization obligations are strictly determined by the borrower's financial capacity (*draagkracht*); consequently, unemployed graduates are assigned a monthly installment of zero, which nonetheless counts towards the maturity of the 15- or 35-year term. Although no mandatory contribution is required during periods of insolvency, borrowers retain the prerogative to execute voluntary repayments (*extra aflossing*) at any time to reduce the principal balance and mitigate interest accrual. To ensure that mandatory terms reflect real-time solvency rather than the standard two-year historical income lag, borrowers must actively utilize the reference year shift (*verlegging peiljaar*) mechanism to align their repayment schedule with their current economic status.

After documenting the decline in starting income, I move on to the underlying job characteristics. It is possible that this wage penalty reflects a compensating differential where graduates trade off immediate income for non-pecuniary amenities like security. It is also possible that graduates have biased their displacement into different sectors. I examine two less-explored margins of job traits: employment stability and sectoral allocation. First, I test whether treated graduates exhibit a stronger preference for the security of permanent contracts over temporary positions. Second, I address the prominent occupational choice hypothesis [Rothstein and Rouse, 2011] by estimating whether the financial pressure of repayment drives graduates away from the subsidized sector and into private employment. I apply the baseline difference-in-differences specification to binary indicators for contract type and employment sector to determine whether the debt shock distorted the composition of early-career jobs. Graduates in the treated cohorts are substantially more likely to secure a permanent contract within one year after entering the labor market. Moreover, there is no evidence of sectoral reallocation, as the propensity to work in the private or public sector relative to the subsidized sector remains statistically indistinguishable from zero.

Then, I discuss plausible mechanisms behind the effects. One potential explanation of the negative effect of student loans on life outcomes is "debt aversion", which is especially relevant for students from lower-income families. I provide several pieces of evidence corresponding to the negative effect that debt aversion exerts on the further accumulation and realization of human capital. I first investigate the substitution between borrowing and labor supply. Facing a reduction in grant funding, debt-averse students may seek to mitigate the liquidity shock by increasing their working hours rather than accumulating debt, or they may already have reached the upper limit of their debt. This response imposes a reallocation of effort toward immediate income generation, which necessarily crowds out the time available for academic study. Students in the more financially constrained sample increased 2.4 hours per week in working and 1.3 hours in the full sample. Although previous evidence established that this trade-off did not affect degree completion, which is the most important measure of one's education, working during school might also harm academic performance and especially skill acquisition outside the classroom. Consequently, graduates entered the labor market with weaker signals.

Second, I investigate the job search process to determine if the earnings penalty is due to suboptimal matching strategies. Theoretical models of job search predict that agents facing high fixed obligations will reduce their reservation wages and exit unemployment more quickly, potentially settling for lower-quality matches to secure immediate cash flow [Ji, 2021]. To test this channel, I examine both the extensive margins of graduates' immediate status — distinguishing between employment, entrepreneurship, and social benefits — and the intensive margins of search duration, defined as the number of days between graduation and the first employment contract. Although the policy caused only minor shifts in initial status, such as a slight increase in self-employment and a decrease in risk-taking entrepreneurship, it significantly altered the pace of entry: treated graduates spent approximately 14 fewer days searching for a job than the control group. This accelerated transition is consistent with the hypothesis that debt pressured graduates to prioritize a job over match quality, contributing to the observed decline in starting wages.

Finally, I examine whether the earnings penalty is driven by a reduction in subsequent human capital investment. The theory of debt aversion suggests that students burdened with high undergraduate debt may be reluctant to finance further education. To test this, I estimate the impact of the policy on the probability of enrolling in a master's program immediately after bachelor's completion. Treated graduates are significantly less likely to pursue advanced degrees by 4%. I re-estimate the main earnings specification controlling for a master's degree. When educational attainment is kept constant, the negative effect of treatment on earnings attenuates but does not vanish, especially for the 1997 cohort. Finally, I present results showing that the policy did not alter students' major choices. In conclusion, these findings are consistent with a debt aversion channel where people are more reluctant to accumulate additional debt when they experience more financial burdens than their peers.

Related Literature

This research is not the first study on the effect of student debt on early-career earnings. One prominent line of literature finds that higher debt increases initial income, possibly by inducing graduates to accept higher-paying but lower-amenity jobs [Rothstein and Rouse, 2011, Field, 2009, Luo and Mongey, 2019, Chapman, 2015, Black et al., 2023] or by increasing hours worked [Daniels Jr and Smythe, 2019]. In direct contrast, another body of work finds precisely zero or even negative effects on earnings [Goodman et al., 2021]. The negative relationship is consistent with a channel of "debt overhang", where debt reduces the incentive to work. Consistent with this, Di Maggio et al. [2019] exploit a random discharge of default debt and find that the reduction in debt led to an increase in income of roughly \$3,000 over three years. This research differs from previous studies by further separating the causal effect of a higher student debt on educational choices and labor market outcomes and provides more explanatory evidence on the channel of debt aversion.

This research primarily contributes to the vast literature on the real-life outcomes of student debt. Beyond the effect on earnings, a broader consensus highlights how high student debt mostly negatively impacts borrowers' life. Student debt has been shown to causally deter subsequent investment in human capital by reducing graduate school enrollment [Libassi and Turner, 2025]. Furthermore, it delays major milestones in life such as home-ownership [Cooper and Wang, 2014, Mezza et al., 2020] and marriage [Gicheva, 2016]. The "debt overhang" effect has also been robustly shown to stifle risk-taking and entrepreneurship [Krishnan and Wang, 2019, Morazzoni, 2021, Bianchi and Bobba, 2013].

Second, this paper also relates to a broader literature that studies the impact of household balance sheets on labor market outcomes. Many articles have researched that shocks of wealth, mortgage, and liquidity to individuals or households can affect labor mobility and job search strategies [Head and Lloyd-Ellis, 2012, Herkenhoff et al., 2024, Dinerstein et al., 2025], entrepreneurial activity and occupational choices [Bianchi and Bobba, 2013], and labor supply [Rossi and Trucchi, 2016, Bernstein, 2017]. In particular, exploiting the 1992 mortgage reform in Denmark, He and Maire [2023] found that providing liquidity to financially constrained homeowners can create better job matches and potentially increase earnings in the longer run. Herkenhoff et al. [2024] studies how credit constraints shape job search, labor market sorting, and aggregate productivity. The authors found that constrained workers accept lower wages and lower-quality job matches, increasing job finding rates, leading to weakening sorting efficiency and aggregate output.

Lastly, this study also contributes to the long debate on the government's intervention in the financing of higher education and the design of student loan systems. People who go to university earn more than those who do not during their life course. Due to this equity nature of human capital, higher education is more private-funded in places like the U.S. and the UK, as the transfer of money to people who earn more over the life cycle is considered more regressive [Yannelis and Tracey, 2022]. In much of continental Europe, university education is largely publicly funded. A critical aim of a higher education financing system is to ease the barriers for talented but poor students to enroll and persist in the university. Thus, there is a clear justification for government intervention in the student loan market; however, it is less clear what the best magnitude should be and how it should be administered. Ionescu and Simpson [2016] show that tuition subsidies increase aggregate welfare by increasing college investment and reducing default rates in the private market. de Silva [2023] shows that income contingent loans provide substantial insurance benefits by improving consumption smoothing; however, income contingent loans also induce a significant moral hazard. Borrowers reduce labor supply (both hours/effort and participation) and are more likely to strategically default compared to those with fixed loans. This study provides new evidence based on a Dutch context, exploring the effects of the introduction of a debt-reliant regime on student' education outcomes and the realization of the' accumulated human capital of students in the labor market using rich administrative microdata.

The remainder of this paper is organized as follows. In Section 2, I describe the education system and the policy change in 2015 in the Netherlands that underlies the empirical analysis. Section 3 provides the data sources and sample selection strategy, and Section 4 discusses the design of empirical research and the identification of challenges. Section 5 presents the main results. Section 6 discusses possible mechanisms through which student loans can affect labor market outcomes. Section 7 contains concluding remarks.

2 Institutional Background

2.1 Dutch Education System

Normally, Dutch students start primary school when they turn four years old, and primary school lasts eight years. Dutch students take the national assessment (*Doorstroomtoets*) in February of group 8, which is their final year of primary school (*basisschool*). The students will be divided into three different tracks, which differ in prerequisites and durations, based on the national exam results and the' recommendations of the teachers. pre-university education (VWO) has a duration of six years and provides access to research universities (WO). Higher General Secondary Education (HAVO) has a duration of five years and provides access to the universities of applied sciences (HBO). Pre-vocational secondary education (VMBO) has a duration of four years and can enter secondary vocational education (MBO) later. Different routes are shown in Figure 1.

Students in different tracks can transfer to another track if they are willing to and meet the requirements. Students who complete a MBO level 4 diploma (the highest level of secondary vocational training) are eligible to enter HBO directly. MBO programs take between two and four years to complete, and this MBO-to-HBO pathway is quite common. Also, students from VWO can choose to enter HBO after a six-year pre-university education instead of WO because it is more applied and offers programs not available at research universities. As a result, HBO serves as a meeting point in the Dutch system to some extent, drawing entrants from HAVO, VWO, and MBO level 4, making it both the broadest and most diverse higher-education entry channel. These VWO and MBO students can arrive at HBO years later than their peers who come straight from HAVO around the age of 17. This is why entry ages into HBO vary widely, in contrast to research universities (WO), which almost exclusively admit pre-university school (VWO) graduates and therefore have much more uniform entry ages. In addition, students in secondary vocational education (MBO) are also eligible for student finance, but the conditions differ significantly. Therefore, this research limits the sample to only research university students (WO) for better identification.

2.2 Student Finance System and the Policy Changes

Higher education in the Netherlands is predominantly publicly financed and students pay relatively low tuition fees. These fees for Dutch and EU/EEA students in publicly funded higher education are statutory and uniform across institutions, set annually by the Ministry of Education before each academic year. The fee was roughly €1,950 per year in the academic year 2014–2015, increasing slightly each year in line with inflation. To finance living during their education, all students under the age of 30 with Dutch nationality or a qualifying residence permit are eligible for student finance [3].

Since 1986, the Dutch student finance system has been regulated by the Student Finance Act (*Wet Studiefinanciering*). The original framework consisted of four pillars: a universal basic grant, a supplementary grant, loans, and in-kind benefits such as a public transport card. The universal basic grant was available to all students, regardless of parental income, and the amount of the grant depended on whether a student lived with their parents or independently. In 2014, the grant amounted to €100 per month for students living at home and €279 for those living independently. Alongside this universal entitlement, the supplementary grant targeted students from lower-income families, with eligibility determined by parental income and family composition. Both grants were

[3] Foreign students from the EEA, UK or Switzerland can also be eligible if they have been living in the Netherlands for at least five consecutive years, and fulfil other criteria. This study uses only Dutch-born students.

performance-dependent: they were converted into gifts if students completed a degree within ten years, but otherwise they would be transformed into loans. In addition to grants, students could borrow funds under favorable conditions. The maximum monthly loan increased from €980 in 2014 to €1,056 in 2015 repayable over a 15-year period subject to income-contingent rules. A further form of support was the public transport card, which allowed students to travel free or discounted on weekdays or weekends.

Applications for all components of student finance are processed digitally via the Dutch government's Education Executive Agency (*Dienst Uitvoering Onderwijs, DUO*). Students indicate which products they wish to receive, specifying the loan amount if applicable. Eligibility for the supplementary grant is determined only after application, based on parental income data retrieved from tax authorities and municipal records; additional documentation is requested only when necessary. By default, parental income from two years prior is used, though students may request reassessment if income has recently declined. The uniform procedure and near-universal acceptance of the former basic grant suggest that application costs are minimal. Information on student finances is disseminated to secondary school students through Ministry of Education communications; meanwhile, DUO actively promotes awareness of the supplementary grant through public campaigns. Thus, the take-up rate of the government's gift of studying was very high before the loan-based system.

By the early 2010s, the increasing number of students put growing pressure on higher education budgets. In October 2012, the Dutch government announced a major overhaul of the student finance system, with a plan to replace the basic grant with a loan-based model beginning in September 2014. The government committed to abolishing the basic grant for a budget cut, which amounted to around €1 billion per year. However, this timeline was postponed in summer 2013, delaying implementation due to political controversy. Finally, in May 2014, a political agreement was reached to enact the reform in September 2015, at which point all incoming students entered the new social loan system. The fact that the budget cut was passed in the parliament right before the enrollment in higher education implied that students could not have reasonably foreseen these policy shifts when they commenced their studies in secondary education.

Students who began their studies in September 2015 would be financed under a different paradigm, the Social Loan System (*het sociaal leenstelsel*) as a replacement for the universal basic grant. Under the new system, students no longer received a guaranteed monthly transfer, but instead could take out larger loans, repayable over a much longer horizon of 35 years, compared to 15 years in the old scheme. This was intended to ease the short-term repayment burden and spread costs more smoothly over the' working lives of graduates. When the universal basic grant was removed for all students, the supplementary grant remained in place for students from poor families. The government expanded the generosity and eligibility for the supplementary grant. The public transport card was also retained. Whether the student is under the more generous old system or the new system due to the budget cut in Dutch higher education was solely determined by the year they enrolled in the university, and the policy for each student remained the same throughout the period of study, meaning that once one received the basic grant before 2015, the benefit would continue until they graduated from university, which can be years later than 2015. However, if a student enrolled in the university from and after 2015, then they lost all the gifts that used to exist under the old system.

The reform fundamentally altered the financial calculus for students. Before 2015, students with low parental income could rely on a combination of basic grants, supplementary grants, and modest borrowing; after 2015, they faced a greater dependence on loans. Students from low-income families who lived independently lost approximately €140 per month in total support relative to the old system, which is the difference between the total amount of basic grants and supplementary grants in the early years and the increased amount of supplementary grants after 2015. Students from higher-income families lost the entire basic grant, previously around €280 per month. For students, the most visible consequence was a sharp increase in debt. By early 2023, the average debt of individual graduates had increased by around €11,000 compared to the pre-reform period.

The media labeled affected cohorts as the "unlucky generation" (*de pechgeneratie*), arguing that the system reduced the financial situation of young adults' and created long-term inequality in wealth accumulation. This sentiment catalyzed widespread public discontent, manifesting itself in large-scale student protests in The Hague and Amsterdam. The policy's legitimacy was further undermined by the failure of its central promise: that the fiscal savings from abolishing grants would be reinvested to tangibly improve the quality of higher education. Subsequent evaluations, including those by the Netherlands Court of Audit (*Algemene Rekenkamer*), indicated that these promised investments were often opaque or failed to yield observable improvements in the educational experience [4]. Facing

[4] Netherlands Court of Audit (*Algemene Rekenkamer*) published a report, Investments in higher education (*Investeringen in het hoger onderwijs*) in January 2018. The report noted that universities reported

sustained political pressure and growing consensus regarding the system's adverse distributional consequences, the government ultimately reversed course. The loan system was formally abolished and the universal basic grant was reintroduced starting in the 2023–2024 academic year, marking the end of this significant but short-lived policy experiment.

3 Data and Sample Selection

For this study, I compile multiple administrative datasets from the Central Bureau of Statistics of the Netherlands (*Centraal Bureau voor de Statistiek, CBS*). The administrative data set provides the above information on the entire Dutch population. For each individual, there is a unique identifier to track over time and link with their family members. I merged datasets of individual characteristics, household characteristics, incomes, parental characteristics and incomes, enrollment in different levels of education, socio-economic category, jobs, and wages. Furthermore, this study incorporates some data sets at the household level on wealth and investments. All types of debts and equities are collected at the household level; therefore, how much a student borrowed to finance education can only be observed if they constitute a single household; otherwise, the student loan can be a sum of the total amount of the unpaid loans of all members within the same household.

To construct the final sample for analysis, this study followed several steps. First, the study constructed a complete sample for all potential Dutch students who received their secondary education in the Netherlands. The education data set does not provide much information about the education that students finished abroad, so Dutch students who did not have a complete record are removed. In addition, I focused on students whose legal parents can be found in the registry so that I can observe parental income two years before the student's enrollment in higher education. By tracking the educational trajectory for each student and the parental income, I could analyze the enrollment rate in different educational tracks across different birth cohorts and family backgrounds of all students in secondary education.

Second, the study constructed a subsample of students who finished their secondary and higher education in research universities in the Netherlands. To compare student'

€860 million in investments, but concluded that only approximately one-third of this amount (around €280 million) could be definitively traced back to the agreed-upon criteria for new and additional quality investments funded by the abolished basic grant. This indicated a substantial failure to realize the policy's central promise of quality improvement.

outcomes in education and the labor market before and after the policy change, I defined cohorts of students based on their birth year and quarter. This study includes students who were born between the last quarter of 1993 and the third quarter of 1998. Students are normally expected to enter research universities at the age of 18 years, and 2015 is the first year when there was no universal basic grant. So, students are assigned to the control cohort if they were born no later than the third quarter of 1996 and to the treated cohort if they were born after the third quarter of 1996.

The education data that I used in the study were updated to the first of October in 2023, so I could observe whether higher education was completed by this date. Individual income data are updated until the tax filing year 2023. For analysis of the labor market outcomes of the graduates from the research universities, I subset the sample to students who managed to obtain their higher education degree by the first of October in 2022 so that I could observe a full tax filing year of their future incomes.

4 Empirical Strategy

4.1 Variable Definition

The most straightforward way to explore the effect of a higher debt induced by the budget cut is a difference-in-differences (DID) analysis.

Before the change, for example, in 2014, all students who started their studies in 2014 were entitled to a basic grant of up to €279 per month and a supplementary grant of up to €258 per month. After 2015, the Dutch government's budget cuts to higher education not only replaced the universal basic grant with income-contingent loans but also increased the supplementary grant for eligible students. In 2015, students with parental income less than €31200 were fully eligible for the supplementary grant. The full amount was €378, which was close to the total grant they were entitled to before the reform. For these students from low-income families, the loss of the basic grant was offset by this higher supplementary grant, and thus they were least affected by the reform and can act as the control group. Students whose parental income was less than €46600 were partially eligible. They could not get the maximum of the supplementary grant, and, meanwhile, they lost all the basic grant. Therefore, the policy change had a greater impact on these students, who were only partially eligible for the supplementary grant. The policy change had the largest impact on students who were not eligible for the supplementary grants at all, received nothing during their studies, and must borrow much more money than their previous peers. This feature of the policy change creates a variation in the intensity of the treatment.

Thus, I establish a measure of treatment intensity. The treatment intensity takes the value 0 for fully eligible students as the control group and the value 1 for fully ineligible students. For partially eligible students, the amount of supplementary grant that they were entitled to was expected to be a linear function of parental income ranging from 0 to the maximal amount, thus the treatment intensity measure is also set to be a negatively sloped linear function of the parent income ranging from the cutoff for full eligibility and the cutoff of receiving nothing and takes a value between 0 and 1.

4.2 Identification Challenges

There are three main concerns with the DiD strategy. First, the status of treatment was determined purely by the year of enrollment. Students who entered the university from 2015 lost the study grant, and students who began university before 2015 would enjoy the free gift throughout their entire study period. There could be a concern that the DiD design may be vulnerable to selection bias. For example, the announcement of the introduction of the loan-based system might stop students from taking a gap year after secondary education in order to avoid the higher private contribution if they postpone to next year. There may also be other idiosyncratic reasons that students delayed or advanced in their previous studies. To address this concern, I use the birth year to instrument the actual year of enrollment. Most Dutch students go to university at the age of 18 after 8 years in primary education and 6 years in secondary education. So, students who were born in the years 1994 to 1996 were largely expected to start university in the years 2012 to 2014, and they became the control cohorts. And students who were born in 1997 and 1998 were more likely to enroll in university in 2015 and 2016, and they could perform as the treated cohorts. Students who were born in 1996 are the last lucky cohort and serve as the reference.

Another concern of this empirical strategy is related to unobserved time factors between cohorts. For example, the total number of students that universities accept may witness an upward trend over the years. So, more recent cohorts may be more likely to enroll in the university than previous cohorts. As a consequence, it is difficult to isolate the effect of the policy change from the effect of unobserved time factors, and the results of an event-study specification may underestimate the potential negative effect of a higher debt under the new loan system. To address this concern, I estimate a basic DiD specification using multiple windows of consecutive birth cohorts. Specifically, I use cohorts that span 1 before and 1 after, 1 before and 2 after, 2 before and 1 after, 2 before and 2 after, 3 before and 1 after, and 3 before and 2 after, i.e., the full sample.

The third potential challenge to the DiD identification is that students with wealthier parents can get transfers from their parents to fill in the gap left by the government. This endogenous response has been documented both in theoretical and empirical literature. Abbott et al. [2019] find that every additional dollar of government grants crowds out 25–50 cents of parental transfers. To address this identification challenge, I implement a robustness check designed to isolate a more homogeneous and comparable set of students. Then I estimate a basic DiD specification on progressively restricted samples, each defined by a different upper threshold for parental income. Specifically, I run a bunch of regressions with an upper threshold of gross parental income from €50,000 to €100,000 by an interval of €1,000. In terms of coverage rate, €50,000 is around the 28th percentile of all gross parental income, and €100,000 is about the 70th percentile during the sample years. By lowering this income ceiling, I can test whether the estimated treatment effect is sensitive to the inclusion of students from better-off families who are more likely to receive confounding parental transfers and lessen the concern of the parallel trends assumption by using subgroups that are more plausibly similar in their financial constraints and behavioral responses.

4.3 Educational Outcomes

To better study the causal effect of student debt on labor market outcomes, I first examine whether the policy also altered students' choices on higher education, especially on attending and completing a bachelor's course at research universities.

The main specification for estimating educational outcomes is as follows

$$y_{ic} = \alpha_c + \sum_{k \neq 1996} \beta_k \cdot (1[c = k] \times I_{ic}) + \mathbf{X}'_{ic}\beta_x + \epsilon_{ic} \tag{1}$$

where y_{ic} is the outcome variable of an individual i in cohort c, I_{ic} is the measure of the above-mentioned treatment intensity. This specification includes a battery of control variables such as gender, single parent, and parents staying in institutional households or without observed income, and a cohort fixed effect α_c. ϵ_{ic} is the error term.

Using different cohort windows and subsamples defined by gross parental income, I estimated the following equation

$$y_{ic} = \alpha_c + \beta_1 I_{ic} + \beta_2 I_{ic} \cdot post_c + \mathbf{X}'_{ic}\beta_x + \epsilon_{ic} \tag{2}$$

where y_{ic} is the outcome variable of an individual i from cohort c, I_{ic} is the measure of the intensity of the treatment mentioned above. This specification also includes a battery of control variables, such as gender, single-parent status, and parents staying in institutional households or without an observed income, along with an error term ϵ_{ic}. As in the previous specification, $post_c$ is the instrumented cohort assignment by the birth year of the student instead of the individual's realized treatment situation, i.e., whether the individual has started studying at a university before the policy change in 2015. So, it takes the value 0 if students were born in the years 1994 to 1996 and became the control cohorts. And, it takes the value 1 if students born in 1997 and 1998 were expected to enroll in university in 2015 and 2016 and performed as the treated cohorts.

I first explore whether students attend research universities more or less likely before or after the budget cut. Firstly, as described in subsection 2.1, Dutch students are divided into different tracks at a relatively early age, typically around 12, after completing primary school based on teacher recommendations and standardized test results. The passage of the budget cut happened only months before the start of the new academic year, therefore it is nearly impossible for students to change tracks under the limited time until graduation. To provide empirical evidence, I estimate my main specification as in Equation 1 on the full sample of all Dutch students who belong to the 1994 to 1998 birth cohorts. I then estimate Equation 2 for the different windows of cohorts, and with the progressive subsamples based on parental income. The outcome variable is a binary indicator equal to one if the individual enrolled in a research university. A negative coefficient on the interaction term would suggest that the increase in borrowing cost discouraged university enrollment.

Next, I investigate the effect on the probability of timely degree completion, conditional on having enrolled in a research university. Financial pressure from higher debt can force students to increase their labor supply while studying, which can negatively affect academic performance or delay graduation. To test whether it is the scenario, I estimate my main specification as in Equation 1 on the full sample of all Dutch students who belong to the 1994 to 1998 cohorts, and Equation 2 for the different windows of cohorts, and with the progressive sub-samples based on parental income. I define a set of binary outcome variables equal to one if a student completed their bachelor's degree by the age of 21, 22, 23, and 24, respectively. Using these sequential cutoff points allows me to distinguish between a delay in graduation and an ultimate failure to complete the degree on time. Dutch research universities have a duration of 3 years, so 21 is the graduation age if a student starts university at the age of 18 and goes smoothly during the study. The reason for choosing 24 is that the last cohort in the research would only be observed utill the age of 25. To eliminate possible selection biases, I trim the sample for analysis of labor market outcomes to include every employee below the age of 25; therefore, they needed to finish university by the age of 24.

4.4 Labor Market Outcomes

After assessing the effect on educational outcomes, I then turn to investigate the core question: what is the causal effect of a higher debt on students' labor market outcomes?

The main specification for estimating labor market outcomes is as follows

$$y_{ict} = \alpha_{ct} + \sum_{k \neq 1996} \beta_k \cdot (1[c = k] \times I_{ic}) + \mathbf{X}'_{ic}\beta_x + \epsilon_{ict} \tag{3}$$

where y_{ict} is the outcome variable of an individual i in cohort c in year t, I_{ic} is the measure of the intensity of treatment mentioned above. This specification includes a battery of control variables such as gender, single parent, and parents staying in institutional households or without an observed income, as in the estimation of educational outcomes. I also include a cohort $\times$ year fixed effect α_{ct}, and ϵ_{ict} is the error term.

This empirical strategy can also be confounded by unobserved time factors between cohorts and family backgrounds. I also estimate a basic DiD specification using multiple windows of consecutive birth cohorts and using progressively restricted samples. The same as in the previous subsection, I use cohorts spanning 1 before and 1 after, 1 before and 2 after, 2 before and 1 after, 2 before and 2 after, 3 before and 1 after, and 3 before and 2 after, i.e. the full sample, and with an upper threshold of gross parental income from €50,000 to €100,000 by an interval of €1,000.

The specification is as follows

$$y_{ict} = \alpha_{ct} + \beta_1 I_{ic} + \beta_2 I_{ic} \cdot post_c + \mathbf{X}'_{ic}\beta_x + \epsilon_{ict} \tag{4}$$

where y_{ict} is the outcome variable of an individual i from cohort c in year t, I_{ic} is the measure of the above-mentioned treatment intensity. $post_c$ is the instrumented cohort by the birth year of the student, instead of the individual's realized treatment situation, which takes the value 0 if the students were born in the years 1994 to 1996 and became the control cohorts. And, it takes the value 1 if students born in 1997 and 1998 were expected to enroll in university in 2015 and 2016 and performed as the treated cohorts. This specification also includes a battery of control variables such as gender, single parent, and parents staying in institutional households or without an observed income, as well as a cohort $\times$ year fixed effect α_{ct}. ϵ_{ict} is the error term.

As a first step, I estimate the immediate transition from school to work. Conditional on graduation, I estimate the effect of the policy on the probability of being employed shortly after finishing university. The literature offers competing conclusions for this margin: the financial pressure of repayment may increase the urgency of finding any job, leading to a higher probability of immediate employment [Di Maggio et al., 2019]. In contrast, a higher debt burden could delay entry into the labor market for two reasons. First, graduates may prolong their job search to secure a position with a sufficiently high wage to pay the larger loan, thus rejecting jobs that would otherwise have been lower paid [Rothstein and Rouse, 2011]. Second, a large debt balance from undergraduate studies may induce students to immediately re-invest in human capital by enrolling in a Master's program, thereby improving long-term earnings potential and deferring loan repayment [Chakrabarti et al., 2023]. My analysis adds more evidence between these channels by testing the effect of the reform on the probability of being employed within one year of graduation.

My outcome variable is a dummy that equals 1 if the graduate is hired within half year upon graduation, I estimate my main specification as in Equation 3 on the full sample of all Dutch students who belong to the 1994 to 1998 cohorts and graduate timely – no later than the age of 24 – with at least a bachelor's degree, and Equation 4 for the different windows of cohorts, and with the progressive sub-samples based on parental income.

Subsequently, I analyze the effect on initial income, where I use the logarithm of the first yearly income as the outcome variable. I estimate my main specification as in Equation 3 for the full sample of all Dutch employed university graduates who belong to the 1994 to 1998 cohorts, and Equation 4 for the different windows of cohorts, and with the progressive sub-samples based on parental income.

In addition, I explore the impact on sector choice and job tenure by analyzing the type of employment contracts of graduates. The "debt overhang" effect may make graduates more risk-averse, influencing not just salary but also other job characteristics. Utilizing the CAO (collectieve arbeidsovereenkomst in Dutch, collective labor agreement [5]) provided by CBS microdata, I first test the specific channel whether students crowd in or move away from "public interest" jobs by estimating the effect on employment in the private versus the public sector, using separate binary indicators as outcome variables. Public interest jobs are generally associated with stable earnings and relatively low expected earnings growth. For this end, I estimate Equation 4 on the full sample of all employees with dummy variables of working in the public or private sector as outcome variables.

Finally, I investigate a critical but less-explored margin of early-career job quality: employment stability. Agents with lower net wealth (or higher debt) may display a stronger preference for jobs with lower variance in earnings, effectively trading off higher potential wages for the insurance provided by job security. This mechanism is consistent with Di Maggio et al. [2019], who find that debt forgiveness leads to increased labor market mobility and risk-taking; conversely, the imposition of debt should drive graduates toward the safety of permanent employment, potentially at the expense of early-career wages. The outcome variable is defined as a binary indicator for whether the graduate has a permanent contract within one year of entering the labor market. Theoretical predictions regarding the effect of debt on contract choice are ambiguous. In the European context, where permanent contracts offer substantial protection against dismissal and income volatility, a higher probability of securing such a contract may reflect a debt-induced increase in risk aversion. I will estimate Equation 4 on obtaining a permanent contract to study the effect of higher debt on the seek for job security.

5 Main Results

This section describes the main results of the paper. I first discuss the results of university enrollment and completion, then move to the participation in the labor market, and finally, I present the effects of a higher student debt on labor market outcomes, including early income and contract types.

[5] A collective labor agreement is a written agreement that sets out the terms and conditions of employment. All companies and institutions active in the Netherlands are classified in a collective labor agreement sector. Three main groups of collective labor agreement sectors are distinguished: private companies, government, and subsidized institutions.

5.1 University Enrollment and Completion

I begin the analysis by examining the effect of the shift from a grant-generous to a loan-based system on enrollment in a research university. The primary concern of the policy change was that the prospect of higher debt could discourage students, particularly those from disadvantaged backgrounds, from pursuing university education.

Figure 3 plots the estimated coefficients and 95% confidence intervals from the event study specification in Equation 1. This regression is run on the full sample of all Dutch students born between 1994 and 1998, with the 1996 birth cohort—the last cohort fully unaffected by the reform—serving as the reference group. The outcome variable is a dummy equal to 1 if an individual is enrolled in a research university after high school. The coefficients for the pre-reform cohorts (1994 and 1995) are statistically indistinguishable from zero, providing support for the parallel trends assumption. Crucially, the coefficients for the treated cohorts (1997 and 1998) are also small and statistically insignificant. This initial result shows that, at the aggregate level, the reform did not significantly alter university enrollment rates based on the universe of all Dutch students.

Table 1 presents the results corresponding to Equation 2 using different cohort windows to ensure that the findings are not sensitive to the choice of the sample period and the unobservable time trend. The outcome variable is again the dummy for university enrollment. The control group includes students born from 1994 to 1996, while the treated group includes those born in 1997 and 1998. Across all six samples, the interaction term is small and statistically insignificant. Taken together, the results provide robust evidence that the switch to a loan-based system did not have a significant causal effect on the decision to enroll in a research university. These results are consistent with those of [Bolhaar et al., 2024], who provided more detailed evidence on higher education enrollments, including students attending applied sciences universities.

The final challenge to this identification is that students from better-off families may receive private financial transfers from their parents to compensate for the lost grant, a behavior well-documented in the literature like Abbott et al. [2019]. This endogenous response would mask the true effect of the policy, as it would confound the results by introducing treatment effect heterogeneity correlated with parental income and altering the composition of students from different backgrounds. To address this, I test the sensitivity of the null finding by estimating Equation 2 on progressively restricted samples based on an upper threshold for parental income. Figure 4 plots the results. The x-axis shows the upper parental income limit used for each regression, ranging from €50,000 to €100,000 in €1,000 increments. The coefficients remain statistically insignificant and stable across the entire range of income thresholds. This shows that the null effect is not driven by high-income families buffering the policy shock and strengthens the conclusion that the reform did not deter enrollments in research universities, even for students from more financially constrained backgrounds.

Having established that the reform did not deter enrollment, I next investigate its effect on the timely completion of higher education. Previous research, such as Black et al. [2023] and Denning [2019], found that a higher loan helps students to be more financially independent and graduate faster than their more constrained counterparts. I test whether this hypothesis holds in the Dutch setting by applying the same empirical structure. The outcome variables are dummy variables equal to 1 if an individual completed their university degree by a certain age.

First, Figure 5 plots the estimated coefficients with 95% confidence intervals from Equation 1 for the entire 1994-1998 sample, with 1996 as the baseline. All four panels on the likelihood of graduation by the age from 21 to 24 years old show no evidence of pre-trends, and the coefficients for the treated cohorts (1997 and 1998) are not notably different from zero.

Second, Table 2 shows the estimates corresponding to Equation 2 for completion by the age of 21 to 24, using different cohort windows. As before, the control group consists of students born from 1994 to 1996, and the treated group includes those born in 1997 and 1998. Across all specifications, the estimates are small and statistically indistinguishable from zero.

Third, to ensure that this aggregate null result is not biased by confounding parental transfers, I replicate the robustness check using restricted samples. Figure 6 plots the estimates from Equation 2. The x-axis again shows the upper parental income threshold, ranging from €50,000 to €100,000. In all four panels for a certain age, the point estimates are stable and statistically insignificant across all income thresholds, confirming that the policy shock did not negatively impact timely completion, even for students from more constrained families.

This set of results across multiple identifications shows strong evidence that the shift to a loan-based system in the Netherlands harmed neither university enrollment nor completion, which allows me for separating the causal effect of a higher student debt on labor market outcomes, where a possible higher debt did not deter students from investing in human capital at a bachelor level.

5.2 Labor Market Outcomes

Having established that the policy did not deter university enrollment or timely completion, I next examine the causal effect of the grant-to-loan shift on graduates' labor market outcomes.

5.2.1 Labor Market Participation

The first and most immediate margin for new graduates is the decision to participate in the labor market. In theory, the financial pressure of looming repayments may increase the urgency of finding employment, pushing graduates to accept a job quickly. Among all 124 thousand research university students, 81 thousand of them successfully finished their education before the age of 24. Based on this graduate sample, I test this hypothesis using the same empirical strategy as previously.

Figure 7 plots the estimated coefficients with 95% confidence intervals based on Equation 3. The outcome variable is a dummy variable equal to 1 if an individual is hired within six months after graduation. Compared with the last lucky cohort of 1996, the coefficients for the pre-reform cohorts (1994 and 1995) and the treated cohorts (1997 and 1998) are all statistically insignificant and close to zero. Table 3 shows estimates corresponding to Equation 4 as changes in post-policy employment, testing the robustness of the finding to different cohort windows using the same outcome variable. Across all specifications in the different columns, the interaction term is small and lacks statistical significance. The coefficients remain statistically insignificant and stable across the entire range of income thresholds. This shows that the null effect is not driven by high-income families buffering the policy shock and strengthens the conclusion that the reform did not distort immediate employment decisions. Finally, Figure 8 plots a bunch of estimates with 95% confidence intervals based on Equation 4 as changes in post-policy employment compared to the pre-period on the same outcome variable. The X-axis shows the upper threshold that is used to construct the sample of a certain regression.

These three findings are coherent and provide robust evidence that the switch to a loan-based system did not have a significant causal effect on the probability of immediate participation in the labor market after timely university graduation.

5.2.2 Earnings

I next turn to the central labor market outcome: initial income. The outcome variable is the log of the first-year earnings from work. I first present the cohort results for the full sample. Figure 9 plots the estimated coefficients with 95% confidence intervals from Equation 3. This regression is run on the full sample of all Dutch students who were born between 1994 and 1998, with the baseline birth cohort of 1996 (the last cohort before the reform) serving as the reference group. The outcome variable is the log of annual earnings in the first tax filing year after graduation. The coefficients for the pre-reform cohorts (1994 and 1995) are statistically insignificant. Conditional on timely graduation and participation in the labor market, the coefficient for students in the 1997 cohort is around -2.7% and statistically significant at 95% confidence. The coefficient for cohort 1998 is also largely different from zero, though marginally insignificant with 95% confidence. The point estimates show a decline in starting income for the impacted students in general.

Table 4 shows the estimates corresponding to Equation 4 using different cohort windows. Students born between 1994 and 1996 serve as the control group, and those born in 1997 and 1998 are the treated group. Across the first five subsample regressions, where at least three cohorts are used, the DiD coefficient is consistently negative and statistically significant with a magnitude of approximately -2.7% change.

Finally, wealthier families can not only offset the grant loss with private transfers but could also help graduates land a better job via their social networks, which further challenges the results of debt on income. To address this, Figure 10 shows the estimates from Equation 4 against the upper parental income thresholds, ranging from €50,000 to €100,000. The estimates are constantly negative across the entire range of income thresholds, and converging to -2.7% as the sample size increases. The estimate becomes statistically significant nearly always after the cutoff point jumps above €80,000. This shows that the negative effect of earnings is not an artifact of high-income families buffering the policy shock and strengthens the conclusion that the reform had a negative causal effect on earnings, even for students from more financially constrained backgrounds.

5.2.3 Job Traits: Contract Type and Sector Choice

The negative effect on earnings motivates an analysis of job characteristics. First, I investigate a less-examined margin of job quality: stability. If student debt functions as a "committed expenditure" that increases the borrower's sensitivity to income shocks, more leveraged graduates may display a stronger preference for the insurance provided by permanent employment [Luo and Mongey, 2019]. Table 5 presents the results where the outcome variable is a dummy equal to 1 if the graduate obtained a permanent contract within one year of employment. In Column (1), I find a positive and statistically significant coefficient, which indicates that graduates from the treated cohorts were 2.8% more likely to secure a permanent contract. The effect remains stable and significant in Column (2) at the same magnitude for the restricted-income subsample. There could be two reasons for this that this study could not distinguish: first, people with higher debt may trade off salary for job security, and second, people with higher debt may be more motivated to obtain a permanent contract during employment. For either or both reasons, I find that people with higher debts value job security more than their peers.

Next, I test the occupational choice that is prominent in the literature by examining whether graduates altered their sorting across broad employment sectors. Table 6 presents the results for two binary outcome variables: working in the private sector and working in the public sector. The dropped category is the "subsidized sector". In columns (1) and (2), using the full sample, the coefficients for private and public sector employment are small and statistically insignificant. This result holds in Columns (3) and (4) for the restricted-income subsample, and both coefficients are not different from zero. These findings present that the policy-induced debt shock did not cause graduates to sort across sectors and that the job security findings are not driven by entering a job sector.

6 Mechanisms

The empirical results in the previous sections indicate that the rising student debt did not alter the decision of Dutch students to enroll in the university, but affected students' early earnings. This section discusses plausible mechanisms behind the effects. One potential explanation of the negative effect of student loans on life outcomes is "debt aversion", an unwillingness to borrow for education even when it might be financially rational. This is especially relevant for low-income students when making educational investment decisions [Gopalan et al., 2024]. I provide several pieces of evidence corresponding to the negative effect that debt aversion exerts on the accumulation and realization of human capital.

6.1 Work during Study

Firstly, students may cope with the loss of grants by working more during their studies. In this case, students face a fundamental resource allocation problem: they must balance their time between studying and part-time work. In the main result, the probability of graduation in time is found to be not significantly impacted by the policy change. However, possibly increasing part-time work could also introduce a significant trade-off in study time. This time constraint is found to negatively impact academic performance by Triventi [2014] and should thus act as a bad signal to the student on the labor market later.

Using additional administrative data with information on student' work hours, I compared the average hours of student' work before and after the reform. I estimate Equation 2 where the outcome variable is the average weekly working hours throughout the study period. The results are shown in Table 7. Empirical evidence establishes a material and statistically significant increase in student working hours among the cohorts subjected to the loan-based system, consistent with previous literature. In the whole sample, treatment increases working hours by approximately 1.3 hours per week, significant with 90% confidence. When restricting the sample to students from households with parental income below €100,000 — a group more likely to be financially constrained — the estimated effect increases to about 2.4 hours per week and is highly significant. These results suggest that the loan system induced a meaningful rise in student labor supply during studies, with substantially stronger responses among lower-income students, which may harm the students' performance in university without reducing the chance of graduation, and later harm their outcomes in the labor market.

6.2 Job Searching Behaviors and Duration

Second, I investigate another potential mechanism driving the main results by examining graduates' job-searching behavior. The central hypothesis is that the financial and psychological pressure of debt induces suboptimal search strategies, leading to poorer job matches. As predicted by Ji [2021], graduates with limited liquidity would be less willing to bear the uncertainty of a prolonged job search under debt pressure. This can cause an inadequate job search and finally lead graduates to accept early-arriving job offers more quickly, resulting in lower initial earnings.

To begin, I analyze the immediate status of graduates after leaving university. I estimate my Equation 4 using four distinct binary outcomes: whether the graduate is an entrepreneur, is self-employed, is a social benefit recipient, or is otherwise without income, which are several common margins rather than entry into the right labor market, though not exhaustive. The literature consistently finds that student debt acts as a significant barrier to entrepreneurship, primarily by restricting access to capital and increasing risk aversion [Krishnan and Wang, 2019, Morazzoni, 2021]. Nevertheless, here it is important to distinguish entrepreneurship and other forms of self-employment, which can represent different business activities and skill demands [Levine and Rubinstein, 2017]. I used the socio-economic category given by CBS: a self-employed person is someone who performs work for their own account or risk, primarily not as an employee of someone else, and an entrepreneur is typically someone who runs a business with the aim of making a profit, and they typically take much risk. Using the same data, the third dummy represents whether there is an increase in social benefit dependence, which may signal that the psychological strain of debt hinders an effective job search. The without-income dummy indicates whether the student experienced a period of staying without any income, which plausibly means an intense job-searching spell. The last dummy is whether the student became an employee immediately after graduation. It is slightly different from the labor participation margin that I explored in the main results, where a period of six months can occur before formal employment. I also included this indicator here as a robustness check for the main result.

The results of the personal situation immediately after graduation can be found in Table 8. Panel (1) shows that the estimates indicate that there are no substantial changes in the categories of post-graduation without income, recipient of any kind of social benefit, and directly hired, with coefficients generally close to zero and statistically insignificant. There is a small but statistically significant increase in the likelihood of becoming self-employed (0.6%), suggesting that the treated cohorts exhibit a slightly higher propensity towards self-employment relative to the control cohorts. This may be owed to the greater financial pressure during those days. The probability of being an entrepreneur decreases significantly, although the magnitude is small by 0.2%. Generally, more affected students presented higher risk aversion.

Panel (2) restricts the analysis to students from households with parental income below €100,000, and the pattern of results is broadly similar. Treatment again has a modest positive effect on self-employment — 0.5% — which is statistically significant, while the effects on entrepreneurship are also negatively significant by 0.2%. Unemployment, social benefit receipt, and direct employment remain close to zero and insignificant. In general, the results indicate that the policy had little discernible impact on the subsequent direct entry of graduates into the labor market or the social security system, in addition to a small increase in self-employment and a decrease in risk-taking entrepreneurship.

In addition, I explore the duration of the job search as an intensive margin. For this purpose, I focus on the subsample of graduates who follow a student-unemployed-employed trajectory and use the number of days they remain unemployed without income as the outcome variable. The pressure of high accumulated debt pressures graduates to shorten their job search spell, and a shorter search duration is often associated with a lower reservation wage, which in turn leads to a poorer job match and lower earnings. By estimating Equation 4 on unemployment duration in days right after graduation and before first employment, I can test whether a rushed job search can be a reason for sub-optimal realization of the student's human capital and the negative effect on income observed in my main results.

The results are shown in Table 9. Across the entire sample, the treatment reduces the duration of the job-search by approximately 13 days, a statistically significant effect. This suggests that treated graduates transition into employment significantly faster than their counterparts in the control cohorts. The magnitude is even larger for students from households with parental income below €100,000, where the estimated reduction is roughly 14.6 days and is statistically notable, which is longer than in the full sample for these more disadvantaged students. Overall, the results indicate that the policy pushed graduates to speed up job search after graduation. Additionally, the unconditional means of job search days for the full sample and the subsample with a parental gross income below €100,000 are 83 days and 87 days. Regarding absolute values, the mean and reduction of the unemployment spell appear to be longer for less wealthy students, while the estimated reduction amounts to 15.7% and 16.8% of these unconditional means, respectively, and there is a slightly larger reduction regarding the unemployment spell for the subsample. It should be noted that, for the inclusion of these individuals in the main sample, 85.27% of them got employed within six months.

Another possible intermediate step that students could take is to work temporarily as a self-employed person. Table 10 reports the estimates spent on self-employment between the completion of the university and the entry into employment in Equation 4. The sample is restricted to individuals who experienced one spell of self-employment during the post-graduation transition period in days. Column (1) uses the full sample, while Column (2) restricts to graduates from households with parental income below €100,000. In the full sample, the point estimate implies that the reform reduced the average duration of self-employment by approximately 3.7 days and in the lower-parental-income subsample by 5.8 days. The estimated coefficients are notable in size in both specifications, yet statistically indistinguishable from zero given the small sample sizes and the correspondingly low statistical power.

6.3 Graduate Studies

An additional important channel of the negative effect of student debt on life-cycle outcomes is by altering subsequent human capital investment decisions. Undergraduate debt has been found to deter enrollment in graduate and professional degree programs [Chakrabarti et al., 2023]. This could also lead to future earnings losses.

To test this mechanism, I examine whether the higher debt discouraged bachelor's graduates from continuing to a master's program shortly after their first diploma. Due to data limitations, as I can only observe the last cohort of students until the age of 25, I define my outcome variable as a binary indicator equal to one if a student started a master's program before this age. I estimate Equation 2 using the sample of university graduates to test the hypothesis that the increased debt burden had a negative effect on the propensity to enroll in postgraduate education. A finding in this direction would provide evidence that the policy not only affected early-career labor market choices, but also constrained further human capital accumulation.

The results of the likelihood of doing a master's course after the completion of the bachelor's degree are presented in Table 11. Column (1) presents results for the full sample, while Column (2) restricts the sample to students from households with gross parental income below €100,000. The coefficient of the DiD estimator is negative and statistically significant in both specifications, where the policy reform reduced the probability of master's enrollment by around 4.0%.

Figure 11 displays the coefficients of Equation 3, estimated in the full sample of research university graduates entering the labor market. The outcome variable is still the logarithm of annual earnings in the first full year of employment as an employee. The specification explicitly controls whether the individual ultimately obtained a master's degree. The post-reform impact pattern persists. The coefficients of the DiD term on the 1997 cohort decreased slightly in both magnitude and significance. and the coefficient for the cohort 1998 moved more to zero and became more statistically insignificant.

Generally, the estimated negative effect of the policy change on income is somewhat attenuated relative to Figure 9, but remains negative and does not fall entirely to zero, especially for the cohort 1997. This pattern indicates that reduced master's enrollment accounts for a small part, and not all — of the adverse impact of the loan-based financing regime on early-career earnings. The mediating effect through master's attainment could be underestimated in the current analysis. Because the earnings outcome is observed only in the first full year of employment and the sample is restricted to individuals who enter the labor market by age 25 at the latest, the full labor market return to a master's degree has likely not yet materialized for most graduates. The master's premium typically accrues over time through faster wage growth and access to higher-level positions; consequently, the portion of the policy's total effect that is channeled through lower master's completion appears smaller in these early-career data than it will prove to be over the life cycle.

6.4 Major choices

In this last subsection, I explore another possible concern that a higher expected student loan can also affect the choice of university major and thus subsequent occupations, and earning patterns. individuals invest in certain types of human capital and skills through their choice of college major [Hemelt et al., 2021], and such major choices are made based on individuals' elicited beliefs about the likelihood of entering alternative occupations and the expected earnings returns and risks associated with those choices [Arcidiacono et al., 2012, 2020, Patnaik et al., 2022]. When faced with a higher debt, students may prefer early-career earnings over higher earning growth and higher lifetime earnings; in contrast, students may avoid choosing majors with a low initial income but high potential.

The above concern of university students choosing majors differently before and after the policy change is addressed by estimating Equation 2 with the outcome variable being a dummy variable equal to 1 if an individual graduated in a specific study area. The process of getting study areas from thousands of registered course descriptions is described in detail in the appendix. The results are shown in Figure 12. Panel (1) reports results for the full sample of university enrollees, while Panel (2) restricts the analysis to students from households with gross parental income below €100,000, the subgroup for whom the financial shock was presumably most salient. Across both samples and almost all ten major fields, the point estimates are uniformly small in magnitude typically ranging between -0.5% and 0.5%. There is no evidence of statistically significant shifts in enrollment toward or away from any particular field, including those traditionally associated with higher earnings (e.g. Business, Economics, Finance & Law; Computer Science & Information Technology; Engineering) or lower earnings (e.g. Humanities, Arts, Education, Social Sciences). Only in Health, Medicine & Life Sciences, the drop in enrollment is about 2% in both samples. Although the effect remains statistically insignificant, the magnitude is considerable. A plausible explanation is that the most financially impacted students — those facing the largest increase in borrowing—became more sensitive to the debt implications of especially long programs. Although Health, Medicine & Life Sciences offer strong long-term earnings premia, the extended duration of study (often 6+ years for medical degrees and related fields) implies substantially higher cumulative debt under the new loan regime. Debt-averse students may therefore have been disproportionately deterred from entering this field despite its favorable long-term returns.

As a conclusion, these null results indicate that the loan-based financing reform did not induce students to re-sort across fields of study in response to changed financial incentives. The policy therefore appears to have operated primarily on the increase of working hours as a student, shorter job search spell, and reduced progression to master's programs rather than the margin of field switching. These findings are consistent with a debt aversion channel in which individuals are more reluctant to accumulate a bigger debt when they experience an increase in existing debts as a shock.

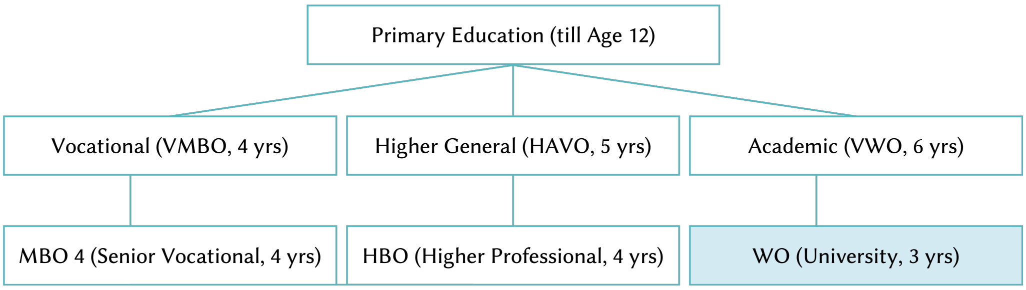
7 Conclusion

This paper studies the causal effects of student debt on labor market outcomes. Using microdata from the Dutch Central Bureau of Statistics, I apply a difference-in-differences approach to compare student debt holders who entered research universities before and after the government's budget cut on higher education, thus with lower and higher student debt afterwards. This study first establishes a foundational result on the impact of student loan reforms on human capital investment. Contrary to the concern that debt acts as a barrier to entry, I find that the replacement of universal grants with income-contingent loans was neutral with respect to university access. The policy change did not have a statistically detectable impact on the propensity to enroll in a research university, nor did it alter the likelihood of timely completion of the bachelor's degree between the ages of 21 and 24. Importantly, these null estimates are precise and robust across the parental income distribution, indicating that even students from financially constrained backgrounds did not reduce their higher educational attainment.

In contrast to the stability observed in educational attainment, the reform generated significant frictions in early-career labor market trajectories. Graduates exposed to the loan-based system experienced a statistically significant reduction in their first-year annual earnings of approximately 2.6% compared to the control group. An analysis of job characteristics reveals that this earnings penalty is not driven by a reallocation of labor across sectors; treated graduates did not crowd out of the private sector or sort differentially into public employment. Instead, the decline in earnings coincides with a substantial increase in the probability of securing a permanent contract within one year of labor market entry, suggesting that debt-burdened graduates may accept lower starting wages in exchange for greater employment security or work harder during the first year.

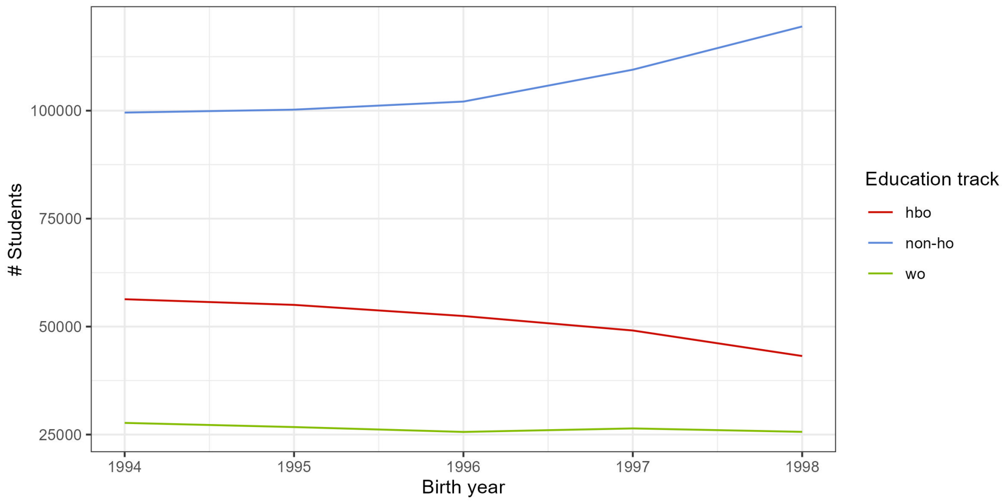
The empirical evidence points to three specific evidence that explain this earnings penalty, all consistent with a channel of debt aversion. First, during their studies, treated students—particularly those from lower-income families—significantly increased their weekly working hours, substituting study time for immediate labor supply to mitigate liquidity needs. Second, in the transition to the labor market, graduates accelerated their entry, engaging in job search spells that were approximately 14 days shorter than their peers. This rushed entry is consistent with a lower reservation wage and a prioritization of immediate cash flow over optimal matching. Third, the reform discouraged further human capital accumulation, as treated graduates were significantly less likely to enroll in Master's programs. Crucially, these distortions occurred without a corresponding shift in university majors, indicating that the earnings decline stems not from what students studied, but from how financial pressure degraded the quality of their skill acquisition and labor market transition.

Figure 1: The Dutch education system



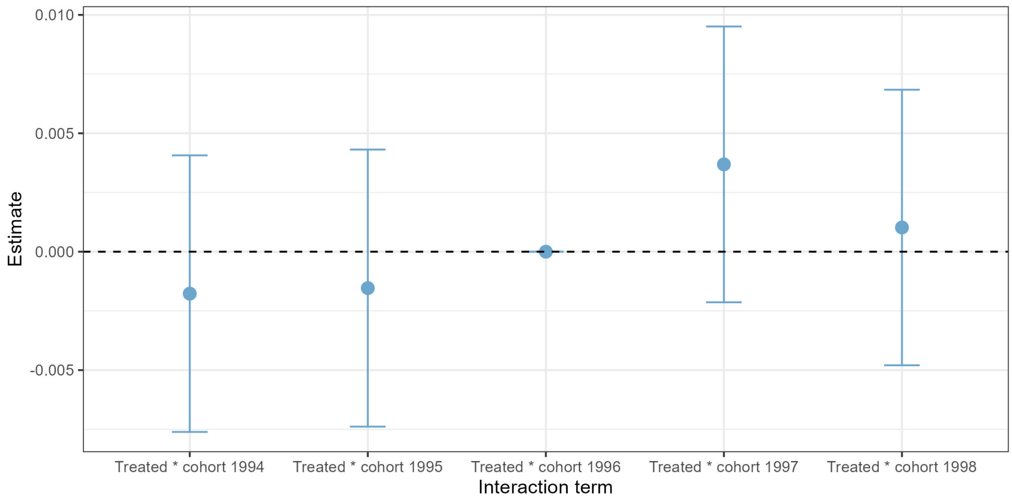
Note: This figure plots different tracks in the Dutch education system.

Figure 2: Students in Different Education Tracks



Note: This figure plots the number of individuals from each cohort taking different educational tracks. The green line indicates the enrollment into research universities which is the focus of this paper. The red line shows the trend of enrollment into universities of applied sciences and the green line shows the trend of individuals who are not taking higher education. All data come from the Dutch CBS microdata.

Figure 3: DiD Results of Research University Enrollments on the Full Sample



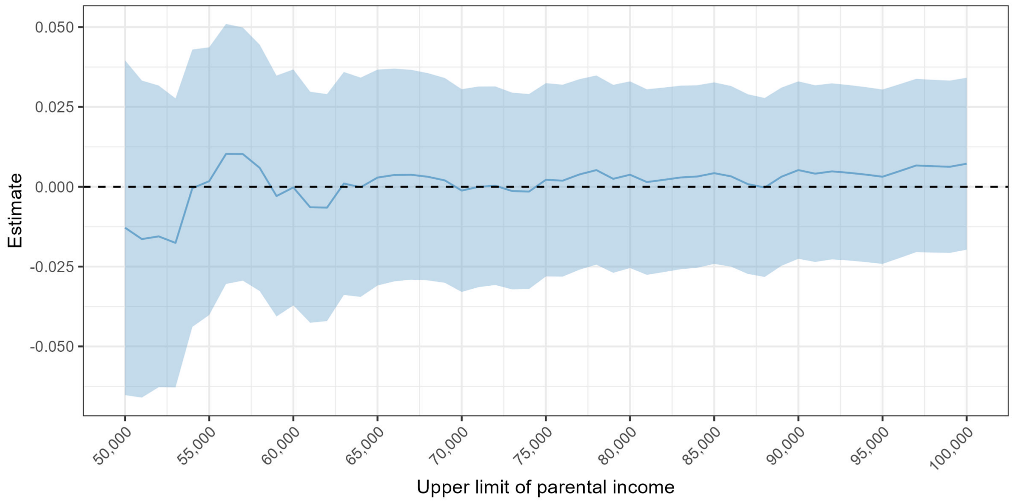
Note: This figure plots the estimated coefficients with 95% confidence intervals based on the Equation 1 using the full sample of all Dutch students who were born between 1994 and 1998. Cohort 1996 is the last cohort before the reform and serves as the reference here. The outcome variable is university enrollment, which is a dummy variable that equals 1 if an individual enrolled in a research university following high school graduation. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort is included. All data come from the Dutch CBS microdata.

Table 1: DiD Results of Research University Enrollments Using Different Cohort Windows

Sample Cohorts	1994–1998	1994–1997	1995–1998	1995–1997	1996–1998	1996–1997
Treated × Post	0.003	0.003	0.003	0.004	0.002	0.004
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Observations	919,103	730,788	735,503	547,188	553,499	365,184

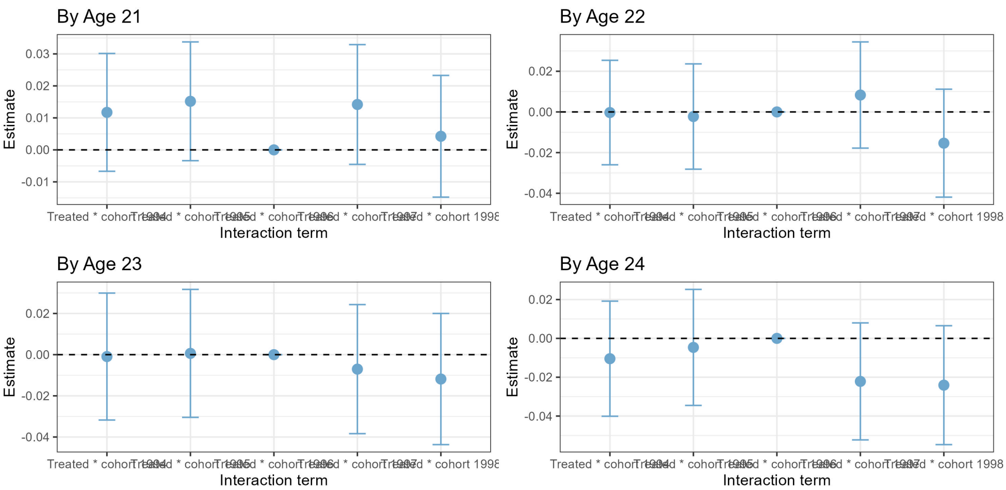
Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
This table shows estimates corresponding to Equation 2. The outcome variable is university enrollment, which is a dummy variable that equals 1 if an individual enrolled in a research university following high school graduation. Different columns contain results using different cohort windows. Students who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort is included. All data come from the Dutch CBS microdata.

Figure 4: DiD Results of Research University Enrollments Under a Limit of Parental Income



Note: This figure plots a bunch of estimates with 95% confidence intervals based on Equation 2 using a restricted sample. The outcome variable is university enrollment, which is a dummy variable that equals 1 if an individual enrolled in a research university following high school graduation. The X-axis shows the upper threshold of gross parental income of the year of expected enrollment into university, which is used to construct the sample of a certain regression. The parental limit ranges from €50,000 to €100,000 in €1,000 increments. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort is included. All data come from the Dutch CBS microdata.

Figure 5: DiD Results of Research University Completions on the Full Sample



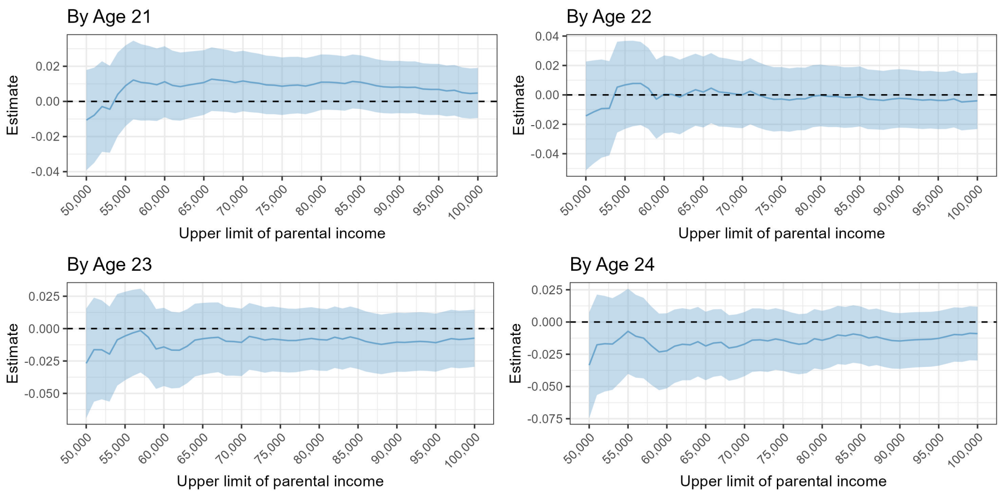
Note: This figure plots the estimated coefficients with 95% confidence intervals based on the Equation 1 using the full sample of all Dutch students who enrolled in a research university and were born between 1994 and 1998. Cohort 1996 is the last cohort before the reform and serves as the reference here. The outcome variable is the timely completion of a bachelor's degree, which is a dummy variable that equals 1 if an individual graduated from a research university by a certain age. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort is included. All data come from the Dutch CBS microdata.

Table 2: DiD Results of Research University Completion Using Different Cohort Windows

	Panel (1): Graduation by the age of 21					
Sample Cohorts	1994–1998	1994–1997	1995–1998	1995–1997	1996–1998	1996–1997
Treated × Post	0.000	0.005	0.002	0.006	0.010	0.014
	(0.006)	(0.008)	(0.007)	(0.008)	(0.008)	(0.009)
	Panel (2): Graduation by the age of 22					
Sample Cohorts	1994–1998	1994–1997	1995–1998	1995–1997	1996–1998	1996–1997
Treated × Post	−0.002	0.009	−0.001	0.010	−0.003	0.008
	(0.009)	(0.011)	(0.009)	(0.011)	(0.012)	(0.013)
	Panel (3): Graduation by the age of 23					
Sample Cohorts	1994–1998	1994–1997	1995–1998	1995–1997	1996–1998	1996–1997
Treated × Post	−0.009	−0.007	−0.009	−0.007	−0.009	−0.007
	(0.010)	(0.013)	(0.011)	(0.014)	(0.014)	(0.016)
	Panel (4): Graduation by the age of 24					
Sample Cohorts	1994–1998	1994–1997	1995–1998	1995–1997	1996–1998	1996–1997
Treated × Post	−0.015	−0.017	−0.018	−0.020	−0.016	−0.022
	(0.010)	(0.013)	(0.011)	(0.013)	(0.013)	(0.015)
Observations	123,994	99,327	98,694	74,027	73,945	49,278

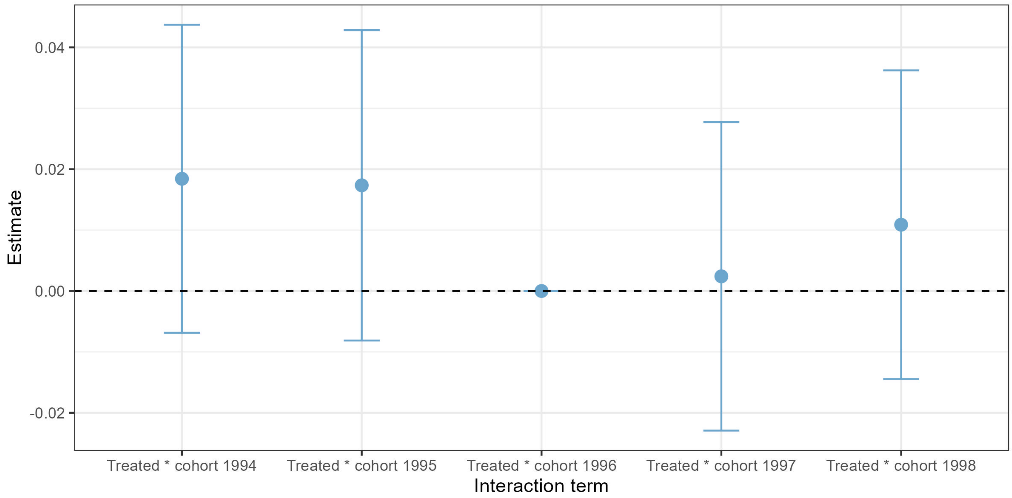
Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
This table shows estimates corresponding to Equation 2. Each panel has a different outcome variable, which is successful completion of a research university, indicated by a dummy variable that equals 1 if an individual graduated from a research university by a certain age. Different columns contain results using different cohort windows. Students who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort is included. All data come from the Dutch CBS microdata.

Figure 6: DiD Results of Research University Completion Under a Limit of Parental Income



Note: This figure plots a bunch of estimates with 95% confidence intervals based on Equation 2 using a restricted sample. The outcome variable is timely graduation from a research university, which is a dummy variable that equals 1 if an individual graduated from a research university by a certain age. The X-axis shows the upper threshold of gross parental income of the year of expected enrollment into university, which is used to construct the sample of a certain regression. The parental limit ranges from €50,000 to €100,000 in €1,000 increments. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort is included. All data come from the Dutch CBS microdata.

Figure 7: DiD Results of Labor Market Participation on the Full Sample



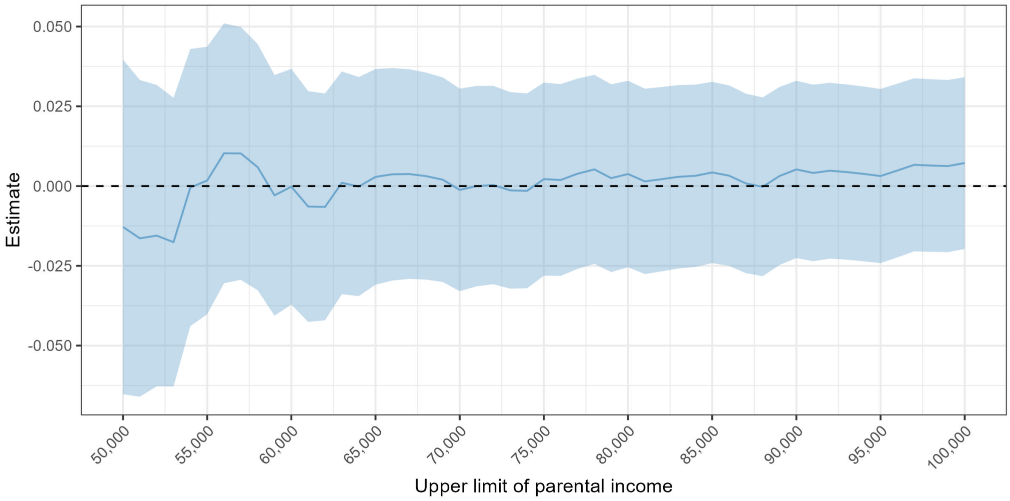
Note: This figure plots the estimated coefficients with 95% confidence intervals based on the Equation 3 using the full all the timely university graduates who were born between 1994 and 1998. Cohort 1996 is the last cohort before the reform and serves as the reference here. The outcome variable is a dummy variable that equals 1 if an individual got hired as an employee within six months after graduating from a research university by the age of 24. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Table 3: DiD Results of Labor Market Participation Using Different Cohort Windows

Sample Cohorts	1994–1998	1994–1997	1995–1998	1995–1997	1996–1998	1996–1997
Treated × Post	-0.013	-0.011	-0.008	-0.013	0.007	0.002
	(0.008)	(0.011)	(0.009)	(0.011)	(0.011)	(0.012)
Num.Obs.	81,259	64,064	65,297	48,102	49,444	32,249

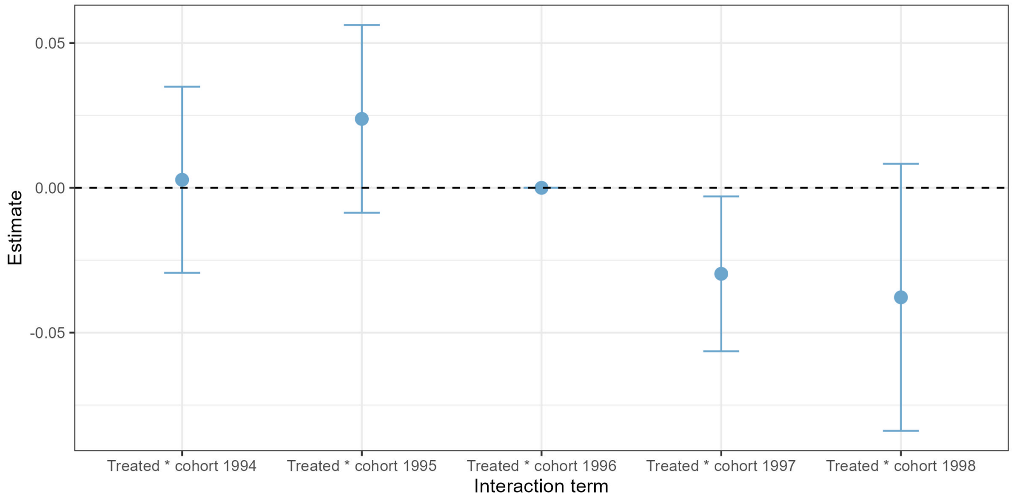
Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
This table shows estimates corresponding to Equation 4. The outcome variable is a dummy variable that equals 1 if an individual got hired as an employee within six months after graduating from a research university by a certain age. Different columns contain results using different cohort windows. Students who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Figure 8: DiD Results of Labor Market Participation Under a Limit of Parental Income



Note: This figure plots a bunch of estimates with 95% confidence intervals based on Equation 4 using a restricted sample. The outcome variable is a dummy variable that equals 1 if an individual got hired as an employee within six months after graduating from a research university by the age of 24. The X-axis shows the upper threshold of gross parental income of the year of expected enrollment into university, which is used to construct the sample of a certain regression. The parental limit ranges from €50,000 to €100,000 in €1,000 increments. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Figure 9: DiD Results of Earnings on the Full Sample



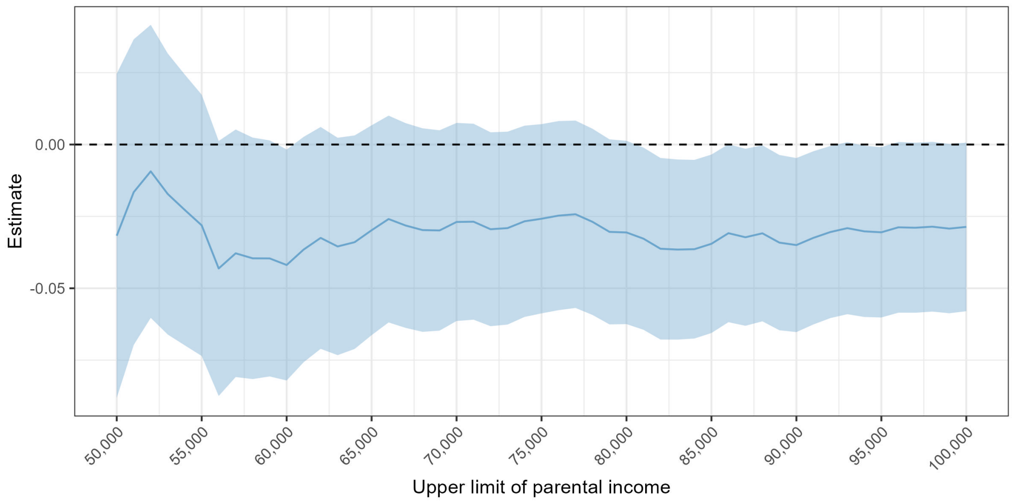
Note: This figure plots the estimated coefficients with 95% confidence intervals based on the Equation 3 using the full sample of all fresh graduates in the labor market who were born between 1994 and 1998. Cohort 1996 is the last cohort before the reform and serves as the reference here. The outcome variable is the logarithm of the annual income during the first year of employment if an individual got hired as an employee after graduating from a research university. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Table 4: DiD Results of Labor Market Participation Using Different Cohort Windows

Sample Cohorts	1994–1998	1994–1997	1995–1998	1995–1997	1996–1998	1996–1997
Treated × Post	−0.026***	−0.029***	−0.027***	−0.030**	−0.026*	-0.022
	(0.007)	(0.009)	(0.011)	(0.015)	(0.014)	(0.022)
Num.Obs.	59,940	47,181	48,677	35,918	37,263	24,504

Note: Standard errors in parentheses. * $p<0.1$, ** $p<0.05$, *** $p<0.01$.
This table shows estimates corresponding to Equation 4. The outcome variable is the logarithm of the annual income during the first year of employment if an individual got hired as an employee after graduating from a research university. Different columns contain results using different cohort windows. Students who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Figure 10: DiD Results of Earnings Under a Limit of Parental Income



Note: This figure plots a bunch of estimates with 95% confidence intervals based on Equation 4 using a restricted sample. The outcome variable is the logarithm of the annual income during the first year of employment if an individual got hired as an employee after graduating from a research university. The X-axis shows the upper threshold of gross parental income of the year of expected enrollment into university, which is used to construct the sample of a certain regression. The parental limit ranges from €50,000 to €100,000 in €1,000 increments. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Table 5: DiD Results of Permanent Contract

Sample	Full Sample	Parental income below 100k
Treated × Post	0.028**	0.028*
	(0.013)	(0.015)
Num.Obs.	59,799	26,049

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
This table shows estimates corresponding to Equation 4. The outcome variable is a dummy variable, which turns to 1 if the individual gets a permanent contract within one year after getting employed. Column (1) contains results based on the full sample of all employed graduates, and column (2) contains results based on the restricted sample of students with a gross parental income under €100,000. Students from all cohorts are included, and those who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Table 6: DiD Results of Job Sector

	Full sample		Parental income below 100k	
	Public	Private	Public	Private
Treated × Post	0.009	−0.006	0.012	−0.007
	(0.015)	(0.017)	(0.016)	(0.019)
Num.Obs.	43,730	43,730	18,642	18,642

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
This table shows estimates corresponding to Equation 4. The outcome variables are dummy variables, which turn to 1 if the individual gets an employment contract within the public sector, in columns (1) and (3), or within the private sector, columns (2) and (4). Columns (1) and (2) contain results based on the full sample of all employed graduates, and columns (3) and (4) contain results based on the restricted sample of students with a gross parental income under €100,000. Students from all cohorts are included, and those who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Table 7: DiD Results of Working Hours as a Student

	Full sample	Parental income below 100k
Treated × Post	1.312*	2.429***
	(0.684)	(0.748)
Num.Obs.	55,901	24,394

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
This table shows estimates corresponding to Equation 2. The outcome variable is the average number of weekly working hours of students during the whole study period. Column (1) contains results based on the full sample of all employed graduates, and column (2) contains results based on the restricted sample of students with a gross parental income under €100,000. Students from all cohorts are included, and those who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort is included. All data come from the Dutch CBS microdata.

Table 8: DiD Results of Life after Graduation

	Entrepreneur	Self-employed	W/o income	Social benefits	Employed
Panel (1): Full sample					
Treated × Post	-0.002^{**}	0.006^{***}	-0.006	0.000	-0.004
	(0.001)	(0.001)	(0.004)	(0.001)	(0.004)
Num.Obs.	57,476	57,476	57,476	57,476	57,476
Panel (2): Parental income below 100k					
	Entrepreneur	Self-employed	W/o income	Social benefits	Employed
Treated × Post	-0.002^{*}	0.005^{**}	-0.002	-0.001	-0.004
	(0.001)	(0.002)	(0.006)	(0.001)	(0.006)
Num.Obs.	25,390	25,390	25,390	25,390	25,390

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
This table shows estimates corresponding to Equation 4. Each column shows an outcome variable of a dummy, which turns to 1 if the individual is observed to be under that social category after graduating from university. Panel (a) contains results based on the full sample of timely graduates, and panel (b) contains results based on the restricted sample of students with a gross parental income under €100,000. University graduates from all cohorts are included, and those who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Table 9: DiD Results of Job-Searching Duration When Being Unemployed

	Full sample	Parental income below 100k
Treated × Post	-13.018^{**}	-14.598^{*}
	(1.449)	(2.540)
Num.Obs.	13,846	5,641

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
This table shows estimates corresponding to Equation 4. The outcome variable is the average days of staying unemployed without other incomes between graduation and employment. Column (1) contains results based on the full sample of all temporarily unemployed graduates, and column (2) contains results based on the restricted sample of students with a gross parental income under €100,000. Students from all cohorts are included, and those who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Table 10: DiD Results of Job-Searching Duration When Being Self-employed

	Full sample	Parental income below 100k
Treated × Post	-3.694	-5.777
	(3.519)	(6.148)
Num.Obs.	1511	610

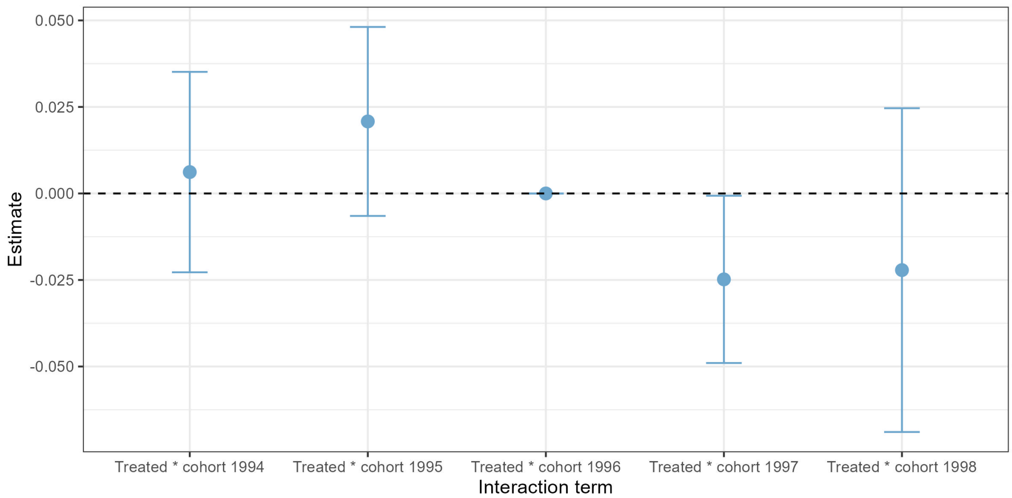
Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
This table shows estimates corresponding to Equation 4. The outcome variable is the average days of staying self-employed between graduation and employment. Column (1) contains results based on the full sample of all temporarily unemployed graduates, and column (2) contains results based on the restricted sample of students with a gross parental income under €100,000. Students from all cohorts are included, and those who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Table 11: DiD Results of Entering a Master's Course

Sample	Full sample	Parental income below 100k
Treated × Post	−0.040***	−0.042***
	(0.007)	(0.009)
Num.Obs.	59,940	26,115

Note: Standard errors in parentheses. * p<0.1, ** p<0.05, *** p<0.01.
This table shows estimates corresponding to Equation 2. The outcome variable is a dummy that turns to 1 if the student entered a master's program. Column (1) contains results based on the full sample of all graduated bachelors, and column (2) contains results based on the restricted sample of students with a gross parental income under €100,000. Students from all cohorts are included, and those who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort is included. All data come from the Dutch CBS microdata.

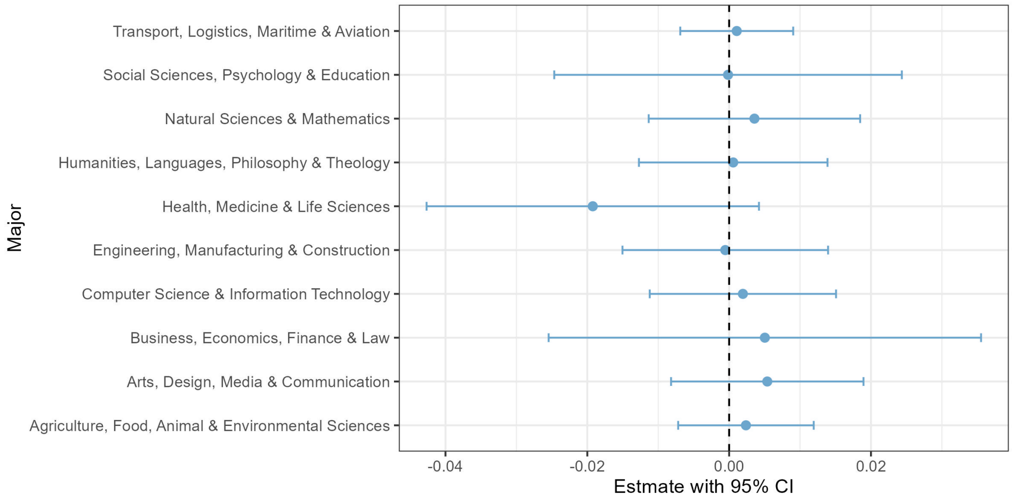
Figure 11: DiD Results of Earnings with Master's as a Control on the Full Sample



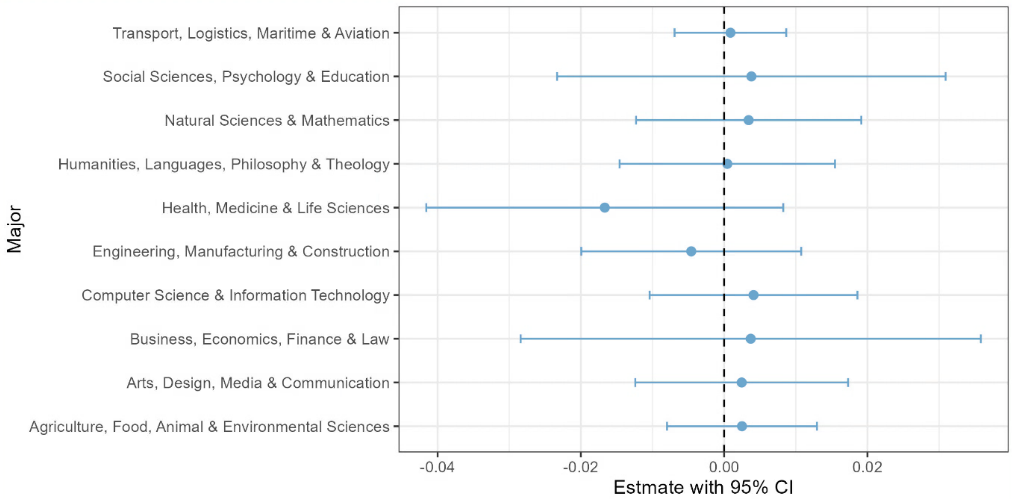
Note: This figure plots the estimated coefficients with 95% confidence intervals based on the Equation 3 using the full sample of all fresh graduates in the labor market who were born between 1994 and 1998. Cohort 1996 is the last cohort before the reform and serves as the reference here. The outcome variable is the logarithm of the annual income during the first year of employment if an individual got hired as an employee after graduating from a research university. Particularly, holding a master's degree is added as a control variable. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort × year is included. All data come from the Dutch CBS microdata.

Figure 12: DiD Results of Major Choices

Panel (1) Full sample



Panel (2) Parental income below 100k



Note: This figure plots the estimated coefficients with 95% confidence intervals based on the Equation 2. Each horizontal bar shows an outcome variable of a dummy, which turns to 1 if a student chose the corresponding major on the Y-axis during university. Panel (1) contains results based on the full sample of all enrolled students, and panel (2) contains results based on the restricted sample of students with a gross parental income under €100,000. Students from all cohorts are included, and those who were born between 1994 to 1996 are expected to be in the control group, and students who were born in 1997 and 1998 are expected to be treated by the policy change. Regression includes gender, a dummy for single parenthood, and a dummy for parents residing in institutional households, or those without observed income. A fixed effect of cohort is included. All data come from the Dutch CBS microdata.

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Appendix: Categorization of Courses

There are more than three thousand possible courses in higher education that a student has ever registered for during the sample years. A unique and non-hierarchical number is assigned, and it can, in fact, be registered as a separate course or a group of virtually identical courses. If a course expires, the course number does not expire. If a new type of training takes the course's place, it will receive a new training number. It is challenging to study whether the study area changed before and after the policy change, as the course numbers can change while the contents do not, and there can be very few students who have registered for a certain course. The primary objective here was to classify each course into a predefined, standardized taxonomy of broad academic and professional majors based on their descriptions. To standardize the unstructured course descriptions, I employed Google's 'Gemini-2.5-flash-lite Large Language Model (LLM).

First, 1000 course descriptions are randomly sampled from the original dataset and fed into Google's 'Gemini-2.5-flash-lite with the accompanying instruction asking it to come up with 10 broad categories. The resulting ten categories were used in a workflow where each course name was fed separately into the LLM along with the instruction to categorize it as one of the previously obtained broad categories. The model was then asked to repeat the categorization, as a structured output where the only permitted responses were the ten broad categories from the first step. The 10 broad study areas are

- Engineering, Manufacturing & Construction
- Computer Science & Information Technology
- Natural Sciences & Mathematics
- Health, Medicine & Life Sciences
- Social Sciences, Psychology & Education
- Humanities, Languages, Philosophy & Theology
- Business, Economics, Finance & Law
- Arts, Design, Media & Communication
- Agriculture, Food, Animal & Environmental Sciences
- Transport, Logistics, Maritime & Aviation