Student Aid Design, Academic Achievement, and Labor Market Behavior: Grants or Loans?

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Abstract: How does financial aid design affect student behavior and achievement? We first estimate the impact of a study-aid reform that caused students to work more and accumulate less debt. To quantify mechanisms and perform counterfactuals, we estimate a dynamic model of college enrollment, work, and loan take-up decisions. We find that grants have stronger effects on student behavior, student debt, and human capital when alternative income and borrowing opportunities are costlier. Loan repayment conditions affect dropout rates 3-4 times more when grants are 5% of aid compared to 50%. Individuals with lower endowments find income-contingent loans more beneficial and their human capital is more sensitive to aid policies. Financial aid design can close the initial endowment gap in dropout rates, but it only reduces the gap in 4-year college degrees by 12% as most of the marginal graduates attain shorter degree programs with low labor market return.

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1 Introduction

Access to financial aid increases college enrollment, persistence, graduation, and economic success over the life-cycle.[1] Financial aid is provided through grants and loans.[2] Grants lower the private college cost and increase the incentive to enroll, but are publicly more costly. In contrast, loans shift the cost of college inter-temporally. Different loan repayment plans entail different inter-temporal trade-offs, while different levels of loan subsidies entail different private-public division of costs. Aid packages typically include three components: a combination of grants and loans, all student loans have a repayment plan and an implicit interest subsidy, and many aid packages also include work-study aid that may be means-tested. In this paper, we provide the first empirical estimates of how these three types of study aid policy instruments interact in determining student debt and human capital. This is crucial for allocating scarce student aid, designing optimal human capital policies that balance incentives and insurance, and go beyond *whether* a policy works to understand *how*, *when*, and for *whom* it works.

We develop and estimate a dynamic discrete choice model of joint education, work, and loan take-up decisions of college students. Individuals are heterogeneous on four-dimensional initial endowments: general ability, quantitative skills, labor market experience, and unobserved (motivation) type. Initial heterogeneity, education, work, and loan take-up choices determine human capital accumulation during college—both in terms of labor market experience and education as measured by college course credits and degrees. The stock of human capital at college exit determines earnings capacity over the remaining life-cycle. Students stay enrolled as long as their expected degree premium exceeds their opportunity cost of staying enrolled, and they derive consumption from college enrollment and three sources of income: grants, loans, and labor income. We fully specify the study aid rules and how they affect budget sets and choices.

We estimate the model using Swedish administrative panel data *and* quasi-experimental variation from a reform of the Swedish study aid rules. The reform changed all three types of policy instruments: First, the grant share of total aid

[1] This is consistently found in the literature. Deming and Dynarski (2010) and Dynarski and Scott-Clayton (2013) provide reviews, and Nguyen et al. (2019) also provide a meta-analysis of 43 studies yielding 75 effect size estimates. They collectively conclude that reducing college costs by $1,000 increases the likelihood of enrollment by about 4 percentage points (pp) and improves college student persistence and attainment by 1.5-2pp.

[2] Throughout the paper, grants refer to any non-repayable monetary transfer to students conditional on college enrollment; e.g. any scholarship or negative tuition costs.

increased.[3] Thus reduced the direct college cost. Second, the loan repayment went from an income-contingent (IC) to an annuity-based plan. Instead of conditioning repayment on *individual* income, the annuity repayment plan imitates *average* wage growth such that the cost of borrowing remains similar for the average earner. The reform changed the inter-temporal trade-off such that borrowing became more costly for low expected earners and less costly for high expected earners. Third, the means-testing loosened—effectively reducing the implicit income tax for students. After the reform, students funded more of their college cost through labor income and less through debt—this shift was largest for low expected earners.[4] The fraction of students working during the academic year increased by 4pp. The fraction of low expected earners not taking up student loans increased by 1pp, and the fraction of high expected earners taking up the full loan increased by 2pp. We use the estimated model to disentangle the effects of each policy instrument and the mechanisms by which they affect student debt and human capital accumulation during college.

Our counterfactual policy simulations provide a range of novel insights. We show that the policy instruments work on different margins, different subgroups of students, and have different distributional effects. Not one policy instrument achieves all social objectives. At the two extremes in terms of private benefit versus public cost are "all grants" and "tough loans" policies: Providing all aid as a grant maximizes human capital, but it is a costly policy that exacerbates inequality. "All grants" policies benefit students with high initial endowments more than those with low endowments *and* more aid is distributed to inframarginal students with high initial endowments.[5] Conversely, making grants scarce and borrowing costly—by front-loading loan repayments—minimizes human capital, student debt, and public cost. "Tough loans" policies reduce inequality, however, by reducing student debt and human capital more for those with high initial endowments. Effective and well-designed aid policy has to recognize these trade-offs and the fact that a singular focus on one social objective and one policy instrument in isolation may have unintended consequences.

[3] We refer to the amount of grant relative to total aid (grant plus loan) as the *grant share*. Increasing the grant share is equivalent to reducing the borrowing limit in our model, since total aid remains constant.

[4] This is consistent with Avdic and Gartell (2015) who find that this reform implied that low socioeconomic status (SES) students work more and reduce their academic achievement relative to high SES students.

[5] This is another case of the "Matthew effect"—those who are more advantaged are given more. Heckman and Landersø (2021) highlight its importance for social immobility in Denmark that also has universal provision of most public services and has more generous study grants than Sweden.

The key new insight in this paper is that interactions between policy instruments are important to consider. Increasing grants when the available loan is subsidized reduces debt at graduation without reducing graduation rates. For our baseline income-contingent repayment (ICR) plan (4% proportional income tax and subsidized 3% interest rate), we find that doubling the grant share (such that 50% of total aid is a grant) decreases debt at exit by 31% without altering graduation rates significantly. Loan repayment plans matter, however, since increasing grants when the cost of borrowing is higher has sizable negative effects on human capital accumulation. Comparing an environment where the grant share is 5% of total aid to an environment where it is 50%, we find that dropout rates are 3-4 times more responsive to changing the loan repayment conditions and subsidized annuity-based loans can be up to three times more effective in changing graduation rates than subsidized IC loans. Repayment plans that front-load debt repayment—by increasing income-contingency or shortening the loan repayment period—reduce student debt and allow the government to recuperate college costs earlier. However, this comes at the cost of lower human capital accumulation during college as students fund their college cost more by working and less by taking-up loans. It is not surprising that students take up less debt when it becomes more costly, but students tend to compensate by working more such that they accumulate less academic capital and consequently have a lower earnings capacity at college exit. Proponents of ICR plans typically focus on insurance for those who get bad labor market draws after college exit, our results suggest that their adverse effects on human capital accumulation should also be considered.[6] These adverse effects on human capital are not present with the annuity-based plan that imitates the average, rather than individual, wage growth by automatically increasing installments by 2% each year such that borrowers pay less in the early years of their careers.

This paper unites and extends two traditionally separate literatures on student aid: the applied literature that uses microeconometric methods to estimate causal effects of student aid on human capital and the more structural literature that analyses the life-cycle effects of student debt or budget constraints. Earlier studies have either focused on short-term effects, on long-term effects, on a subset of the

[6] See e.g. the recent review by Barr et al. (2018) and the references therein. ICR plans for student loans were introduced in the US in 2008. Australia, New Zealand, and the UK have also provided IC student loans since 1989, 1992, and 1998, respectively. The concept of IC loans to finance investments in education dates back to as least Friedman and Kuznets (1945) and Friedman (1955). Lergetporer and Woessmann (2019) find that voters are more likely to favor deferred income-contingent tuition and IC student loans, which suggests that these are considered more fair.

budget set, or on a subset of the study aid policies we analyze in this paper. These are all important aspects, but studying them in isolation ignores crucial policy interactions and mechanisms through which they affect human capital and student debt. We provide an empirical framework to analyze both the short- and long-term effects of study aid policies.

First and foremost, this paper contributes to the large (quasi-)experimental literature that has convincingly shown that grants can increase short-term education outcomes: college enrollment, persistence, performance, and graduation.[7] Two contemporaneous papers show that grants also increase post-college earnings using discontinuities in grant eligibility rules in California (Bettinger et al., 2019) and Texas (Denning et al., 2019). Much less is known about the effects of student loans on human capital. Solis (2017) exploits a discontinuity in eligibility rules in Chile to show how loan access can close the socioeconomic gap in college enrollment, while Card and Solis (2020) show that some of this effect persists through degree completion. Marx and Turner (2019) show that randomly "nudging" students at a US community college with loan offers increases student borrowing, persistence, and performance during the first enrollment year. Black et al. (2020) find that expansions in federal loan limits increase student debt, degree completion, and post-college earnings.[8] We provide a deeper understanding

[7]This has been shown in various populations, environments, and grant types. Dynarski (2003); Bettinger (2004, 2015); Castleman and Long (2016); Goldrick-Rab et al. (2016); Clotfelter et al. (2018) consistently estimate the causal effect of need-based grant programs across several US States, France (Fack and Grenet, 2015), and the UK (Dearden et al., 2014). Dynarski (2008); Goodman (2008); Cohodes and Goodman (2014) find gains in college enrollment and completion for students receiving merit scholarships in Arkansas, Georgia, and Massachusetts. Scott-Clayton (2011); Angrist et al. (2015, 2016); Dynarski et al. (2018); Page et al. (2019); Andrews et al. (2020) find merit aid targeted to low-income students in various US states increases each of these outcomes. Angrist et al. (2009); Garibaldi et al. (2012); Gunnes et al. (2013); Angrist et al. (2014); Barrow et al. (2014); Welch (2014); Carruthers and Özek (2016) find increases in each of these outcomes when financial aid incentives are tied to student success in Canada, Italy, Norway, New Orleans, and Tennessee. DesJardins and McCall (2010) also study the relationship between financial aid packages and college student enrollment behavior and graduation rates at a large US research university.

[8]Another strand of the empirical literature focuses on the long-term impact of student debt and repayment plans. Lochner and Monge-Naranjo (2016) review evidence on rising student debt levels and default rates in the US. Student debt can distort career choices by moving graduates away from low-paid, but potentially high social value, careers in public interest law (Field, 2009; Sieg and Wang, 2018) or as teachers (Rothstein and Rouse, 2011). Luo and Mongey (2019) find that higher student debt leads college graduates to accept jobs with higher wages and lower job satisfaction. Their quantitative model also illustrates that ICR plans are more beneficial when this job amenity-wage trade-off is considered. Using data from the UK Dearden et al. (2008) find that ICR plans reduce the life-time cost of higher education for those with low-income parents, while Migali (2012) finds a higher willingness to switch to ICR plans for those with low-income parents. This literature is complementary as it focuses on behavioral responses to student debt and repayment plans after college exit, while we show how student debt *and* human capital are

of the magnitude of these effects, the mechanisms through which they arise, and the root of why particular policy instruments are more or less effective for particular populations or in particular policy environments. For example, our results are consistent with loan access having a strong impact on college outcomes when other financial aid opportunities (e.g. grants) are scarce (Solis, 2017), while grants can simply crowd out loan take-up when subsidized loans are available (Marx and Turner, 2015; Chapman, 2015).

Estimating causal effects of financial aid is challenging because of complex eligibility rules, multiplicity of programs, and non-observability of all aid components in most contexts.[9] To overcome these challenges, this literature has convincingly established causality by focusing on one policy instrument at the time. The most convincing estimates rely on randomized experiments or regression discontinuity designs that identify causal effects for a specific population; e.g. around a cut-off in aid eligibility rules. We show that the size of financial aid effects depends strongly on who is affected by it, the *population*, and on alternative funding opportunities, the *environment*. First, the estimates depend on the population affected by the policy. We show that the behavioral and human capital responses differ by initial heterogeneity, since those with lower abilities, skills, and motivation generally are more adversely affected by increases in the college cost.[10] Second, the important policy interactions imply that analyzing one aid component in isolation can severely underestimate aid effects in an environment with less costly alternative funding opportunities—especially for a more disadvantaged population. These two elements have been highlighted as potential threats to the scalability of experimental estimates (Al-Ubaydli et al., 2017, 2020). We are the first to quantify their importance when evaluating financial aid policies.

Our main contribution to the more structural literature is twofold: we bring bigger and better data *and* quasi-experimental variation. There are several advantages of studying financial aid policies in Sweden. The university and study aid systems are centralized, universal, and uniform. Moreover, the administrative panel data enables us to observe and merge study aid, college preparedness, college achievement, labor market experience, and earnings at the individual level. This means that we both observe the rules determining selection into college and aid eligibility as well as the key variables that individuals are screened on. We

determined by repayment plans and their interaction with other study aid policy instruments.

[9] Deming and Dynarski (2010) survey the literature and outline the main challenges to estimating causal effects of aid policies in the US context.

[10] Angrist et al. (2015) make a related point regarding the grant effect sizes in this literature, since differences may arise because of differences in the identifying assumptions.

are therefore able to fill the gaps in the literature in terms of data availability and observing actual aid opportunities.[11]

We build on a large literature estimating dynamic discrete choice models.[12] The detailed panel data and uniform study aid rules allow us to include key model features to provide accurate policy predictions: First, we allow for four dimensions of heterogeneity in initial conditions.[13] Each dimension of heterogeneity has different implications for choices and policy responses. Second, we model academic skill acquisition at the course credit level—allowing for self-productivity of skills (Cunha and Heckman, 2007, 2008; Cunha et al., 2010). We distinguish between short and long college degrees. We estimate significant degrees premiums (especially for long degrees), but course credits increase earnings capacity above and beyond fulfilling degree requirements. Third, we model the intra-temporal trade-offs in human capital investments in much more detail. Not only do we allow students to self-finance consumption during college by working, but working

[11]Most of the literature uses US data, where it has neither been possible to get as accurate measures of individual student aid opportunities nor to merge such detailed data at the individual level. Kane (2006), Lochner and Monge-Naranjo (2011), Brown et al. (2012), Avery and Turner (2012), and Dynarski and Scott-Clayton (2013) provide a more detailed description of the plethora of student aid programs at the college, state, and federal level in the US. Brown et al. (2015) and Chakrabarti et al. (2017) utilize the FRBNY Consumer Credit Panel (CCP) to get measures of total student debt, but it does not distinguish between federal and private student debt. Consequently repayment plans are unknown and this data has not been merged with individual level labor market data. Chakrabarti et al. (2017) merge credit records for a random sample of individuals from the FRBNY CCP with 4-year college enrollment and attainment records from the National Student Clearinghouse (NSC), but the data on tuition is at the college level (IPEDS). This is not innocuous as Fillmore (2016) uses data from the Free Application for Federal Student Aid (FAFSA) to show that colleges price discriminate such that the actual college cost (sticker tuition fee minus college discounts through grants) depends on college preferences and is generally lower for individuals with higher ability and lower parental income.

[12]See e.g. Keane et al. (2011) for a recent review. We extend the model of Joensen (2013a,b) that incorporates student grants into a simplified version of the Eckstein and Wolpin (1999) model. Most importantly, we incorporate student loan take-up choices, endogenous student debt accumulation, and repayment plans into the model. The two most closely related papers in this literature are Sauer (2004) and Johnson (2013). Our results are also relevant to the large literature on the importance of credit constraints; see e.g. Keane and Wolpin (2001); Carneiro and Heckman (2002); Cameron and Taber (2004); Stinebrickner and Stinebrickner (2008); Nielsen et al. (2010); Lochner and Monge-Naranjo (2011, 2012); Heckman and Mosso (2014); Hai and Heckman (2017) for recent reviews and contributions. Our results are also relevant to the quantitative macro literature that incorporates some choices during college and student loans into a life-cycle framework. These papers focus on other aspects like default rates (Ionescu, 2009), liquidation and reorganization of student loans (Ionescu, 2011), and loan forgiveness (Chatterjee and Ionescu, 2012). Abbott et al. (2018) study the equilibrium effects of financial aid policies in an overlapping-generations life cycle model with education, labor supply, and saving decisions. They focus on how parental transfers can crowd-out public aid.

[13]See e.g. Keane and Wolpin (1997); Cunha et al. (2005); Huggett et al. (2011); Navarro and Zhou (2017) for evidence that the bulk of the variation in income over the life-cycle is determined by the stock of human capital prior to labor market entry.

also directly affects college achievement. We find that the timing of work matters: Working during the summer months increases college achievement, while working during the semesters lowers college achievement. We show that ignoring student income may overestimate the opportunity cost of college and bias estimated aid effects as it is not random which students work and when they work.[14] Fourth, we depart substantially from prior literature in the way we model budget constraints: we observe and model student debt accumulation directly by imposing the institutional constraints for borrowing limits.[15] Importantly, we also have exogenous variation in borrowing limits as the reform increased the grant share. The idea of combining a quasi-experiment with a structural model has been strongly advocated,[16] but has not previously been implemented in an analysis of the impact of student loans. The advantages of this empirical strategy are twofold: First, the dynamic model accounts for self-selection of student enrollment, work, and loan take-up choices—based on both observed and unobserved heterogeneity—and dynamic selection in terms of who drops out and graduates when. Second, the reform changed both intra- and inter-temporal trade-offs in budget sets. This exogenous variation in enrollment, work, and loan take-up choices of students is independent of individual characteristics. It works as an exclusion restriction as budget constraints only affect outcomes (course credits, degrees, experience, and earnings) through work-loan choices. Bringing these two together, the *ex-post* evaluation of

[14] Sauer (2004) also allows accumulated work experience during Law School to affect wage offers, the probability of Law review after the first year of Law school, and the probability of graduating in the top 20% of class. Since his sample is drawn from the alumni survey of University of Michigan Law School (UMLS) graduates, there is no college enrollment and dropout margin. Joensen (2013a) finds a non-linear effect of work hours on course credit accumulation. Keane and Wolpin (2001); Johnson (2013); Hai and Heckman (2017); Hendricks and Leukhina (2017, 2018); Abbott et al. (2018) also allow student income to be a source of consumption and take time away from college investments, but working does not directly affect college performance. Garriga and Keightley (2013) make the related point that omitting the labor supply margin of college students may lead to overestimating the effects of tuition subsidies. None of these papers utilize data on the timing of work during the year, which we show is key.

[15] One notable exception is Sauer (2004) who also directly models debt accumulation during Law school. He does not observe grants separate from loans and does not have information on debt repayment, thus assumes a repayment plan with equal yearly installments over a 10-year repayment period and a 5.3% yearly interest rate. Absent data on grants and loans, the state-of-the-art in the literature is to approximate the rules of an archetypical federal loan program and include assets (Johnson, 2013) or net wealth (Hai and Heckman, 2017) as a state variable. Johnson (2013) incorporates student loans into the Keane and Wolpin (2001) dynamic model with private borrowing limits by approximating the Federal Family Loan Program (FFEL) loan program rules, tuition differences across states as initial conditions, and proxies need- and merit-based grants as functions of parental income and AFQT scores. Hai and Heckman (2017) incorporate endogenous borrowing limits, and predict that natural borrowing limits are lower for individuals with lower human capital and higher psychic costs of working.

[16] See e.g. Card and Hyslop (2005); Todd and Wolpin (2006); Heckman (2010); Keane et al. (2011); Attanasio et al. (2012); Blundell and Shephard (2012); Blundell et al. (2016).

total reform effects that we observe in our data provides a benchmark validation of the *ex-ante* counterfactual policy simulations based on our model.

There are many potential threats to internal and external validity. When assessing the credibility and generalizability of our model predictions, we need to reflect on how key variables are affected by constraints, preferences, and expectations. We account for multidimensional heterogeneity and carefully model the causes and consequences of key variables, but these variables may also vary by other dimensions of heterogeneity that are not included in our model. A series of model validation for various sub-samples corroborates that addition dimensions of heterogeneity are not causing systematic bias in our predictions. For each sub-sample, we show that we accurately predict dropout and degree completion as well as student debt accumulation over time since initial enrollment. First, we do not directly model parental transfers and other constraints, preferences, and expectations that vary by parental background.[17] Despite generous financial aid policies, we still see large differences by parental education, parental income, and co-residence with parents. We largely capture these differences as driven by differences in initial endowments. Second, constraints vary considerably by location. Despite zero tuition and uniform study aid rules, there is a lot of variation in actual college costs because cost of living varies across cities. We observe and predict that students in cities where the fraction of living costs covered by study aid is lower work more and take on more debt. Third, preferences and expectations may also vary by gender and field of study. We observe and predict earlier degree completion in higher-paying fields of study. Having initial heterogeneity in quantitative skills is key to capturing this difference. Fourth, the reform was implemented in 2001 and the first cohort in our estimation sample enrolls in college in 1994. Many aggregate factors and other policies change during this time period. We hold out two post-reform cohorts that enroll in college in 2003-2004 from our estimation sample to verify that such factors and general equilibrium effects are not of great concern. Overall, this series of model validation for different subgroups and time periods makes us confident in external validity for different populations and environments. At the very least, our results illuminate the importance of policy interactions in study aid design in an environment where universal aid goes beyond free tuition, application is almost automatic upon enrollment, loan repayments are adjusted

[17] The 2009 HSV Eurostudent Survey reveals that grants, loans, and own labor income are the three most important income sources, while transfers from parents or partners amount to less than 7% of the total budget for the average college student in Sweden. Much of the related literature focuses on the role of parental transfers (Keane and Wolpin, 2001; Sauer, 2004; Brown et al., 2012; Mattana, 2013; Johnson, 2013; Abbott et al., 2018)

swiftly according to post-college income, and information frictions are minimal.[18]

The rest of the paper is organized as follows. Section 2 describes the data and the institutional background. Section 3 describes the Swedish study aid system and the reform. Section 4 outlines the dynamic model and its estimation. Section 5 discusses counterfactual policy simulations. Finally, section 6 concludes.

2 Data and Institutional Background

We use register-based individual panel data of the Swedish population. The data includes complete labor market and education event histories, and a range of demographic variables.[19]

Our sample selection consists of two steps. First, we include all high school graduates who enrolled in college between 1994 and 2004 and are younger than 23 years old by the end of their initial enrollment year. Our data covers the period from 1994 to 2009 and we observe students from university enrollment onwards. Thus we observe them for up to 16 years. Figure 1 shows the structure of our sample. The reform was implemented in the fall semester in 2001. Our estimation sample thus includes two pre-reform cohorts (1994-95), two post-reform (2001-2002), and five cohorts that make decisions in both study aid systems (1996-2000) and started making decisions in the post-reform system in different enrollment years. We hold out two post-reform cohorts who enroll in college in 2003-2004 from our estimation sample in order to conduct out-of-sample model validation.[20] Our college enrollment and hold-out samples include 228,262 and 70,457 individuals, respectively. Second, to account for the initial enrollment choice in the estimation we also include all high school graduates from the 1992-2002 cohorts who are not older than 20 at high school graduation and have not enrolled in college by

[18] Such an environment is often alluded to by reformers of the complex US study aid system. Dynarski and Scott-Clayton (2006) and Dynarski et al. (2013) argue that the complexity in the aid system undermines its efficacy. Bettinger et al. (2012) randomly assigned families to a simplified FAFSA aid application process, which increased college attendance as much as offering an applicant additional thousands of dollars in grant aid. Mueller and Yannelis (2019) show substantial reductions of student loan delinquency and default when randomly reducing the cost of switching to an ICR plan. The experience with the Swedish pre-reform ICR plans could also have foreshadowed the US experience with federal ICR plans that since 2008 have become increasingly popular for individuals with high debt and low earnings (Karamcheva et al., 2020) and the extent of loan forgiveness was also similar to loan forgiveness in the US (Catherine and Yannelis, 2020).

[19] Detailed information about the variables and sample selection, as well as descriptive statistics can be found in Appendix A.

[20] See e.g. Todd and Wolpin (2006), Keane and Wolpin (2007), and Schorfheide and Wolpin (2012) for evidence on the usefulness of a non-random holdout sample for model validation.

Figure 1: Sample Selection, College Cohorts

Entry Cohort																	
2004	t=											1	2	3	4	5	6
2003	t=										1	2	3	4	5	6	7
2002	t=									1	2	3	4	5	6	7	8
2001	t=								1	2	3	4	5	6	7	8	9
2000	t=							1	2	3	4	5	6	7	8	9	10
1999	t=						1	2	3	4	5	6	7	8	9	10	11
1998	t=					1	2	3	4	5	6	7	8	9	10	11	12
1997	t=				1	2	3	4	5	6	7	8	9	10	11	12	13
1996	t=			1	2	3	4	5	6	7	8	9	10	11	12	13	14
1995	t=		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
1994	t=	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
	Year=	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009

The Figure illustrates the structure of the data. The data covers the years between 1994 and 2009. We follow students from the year they enroll in college until 2009. Hence, we follow the 1994 enrollment cohort for 16 years and the 2004 enrollment cohort for 6 years. The red vertical line marks the 2001 reform. Two cohorts (1994-1995) were mostly not impacted by the reform, five cohorts (1996-2000) were mostly still enrolled when the reform took place and for cohorts (2001-2004) were only exposed to the post-reform system. The 2003 and 2004 cohorts constitute our hold-out sample for model validation.

the time they are 23 years old. In total, our estimation sample includes 769,215 individuals and 9,211,472 yearly observations.

Sweden is one of the European countries with the highest share of college graduates and expenditure per student.[21] Higher education is tuition-free for all students and is largely financed by the central government. 26% of the sizable total public expenditure on higher education is targeted to grants and loans for students.

After 9th grade—the end of compulsory schooling—most students enroll in an academic or vocational high school track. The two most common academic tracks are social sciences and natural sciences, where the latter has a stronger focus on Science, Technology, Engineering, and Mathematics (STEM). A high school degree is a prerequisite to enroll in higher education. In our data, we observe the high school track and Grade Point Average (GPA) at high school graduation. Table *A.2* in Appendix A shows that high school graduates enrolling in college have a higher than average GPA and are more likely to have (STEM) high school track: 20% are in the top decile of their high school cohort and 32% graduated from the STEM high school track, compared to 2% and 5%, respectively, of those who do not enroll in college. After high school graduation, most take a sabbatical

[21] The annual public expenditure per student in tertiary education is about 14,000 EUR per year - almost double the EU average of 8,000 EUR per year (EACEA, 2009)

period before further studies. At initial college enrollment, individuals are around 20 years old, having taken on average one gap year after high school graduation. At university enrollment, students choose a program characterized by a length and a field of study. The number of European Credit Transfer and Accumulation System (ECTS) course credits required for graduation determine the length of the program. One full year of university education is equivalent to 60 ECTS.

We focus on college students who enrolled between 1994 and 2004, hence before the Bologna process was implemented in Sweden in 2007. In this period, university programs required a minimum of 2 years and a maximum of 5 years. In the data, we observe the length and field of all college enrollment spells and acquired ECTS at the course level. Because 2-year and 5-year programs were rare, we group together the 2- and 3-year programs and the 4- and 5-year programs. We label these *short* and *long* programs, respectively. Table *A.2* in Appendix A shows that of all enrolled students, 42% drop out, 28% graduate with a short degree, and 30% with a long degree. Dropouts and graduates with a short degree are not substantially different on observables, but those with a long degree are positively selected on GPA and STEM high school track. Students accumulate around half of mandated course credits each enrollment year, and those eventually acquiring a longer college degree also tend to be more productive. Students tend to exit college with more than the required course credits. This could reflect switching between fields, a high consumption value of college attendance, or simply a high return to courses.

The Swedish study aid system has been in place since 1919. Study aid is universal and administered by a central study aid authority. Applying for aid is fast and simple—the rules are uniform, transparent, and have not been fundamentally changed since 1965. Upon enrollment in higher education, all students have access to a maximum of 240 weeks (12 semesters) of study aid. Eligibility depends only on student income and, from the second year, on merit: students must complete 75% of the required course credits to maintain eligibility for the following year. The total aid amount is available to students in part as a grant and in part as a subsidized loan to be repaid after college exit. After receiving the grant, students can decide how many weeks of loan to take up each semester—from zero up to a maximum of 20 weeks per semester. In 2001, the year when the reform was implemented, maximum weekly aid was 1,605 SEK, which amounts to 64,232 SEK per year. The debt is interest-free while the student is enrolled, and starts accruing interest after exit. The interest rate on the debt is set by the government to be 70% of the average cost of government borrowing over the past three years. In the data, we observe the total amount of study aid received each year. We assume that students always take the full grant amount before taking up any loan, and calculate the loan amount of study aid as the residual after subtracting the grant amount that the student is eligible for in the semester.[22]

Beyond grants and loans, students can earn income by working. In the data, we observe labor market histories including official employment status, employment spells, and total yearly earnings. Table *A.2* in Appendix A shows that 85% of students are employed: 41% work during the academic year and 44% work only during the summer months.

Another potential source of income for the students is parental transfers. While we do not observe parental transfers in the register data, the 2009 HSV Eurostudent Survey shows that transfers from parents or partners amount to less than 7% of income for the average college student in Sweden.[23] It is possible that the generous aid system has reduced the importance of parental contributions to the consumption of college students, and without the study aid system in place, parental transfers would amount to a higher share of student's income sources.[24] In addition, co-residence with parents could be interpreted as an implicit rent subsidy from the parent to the child. However, students leave the parental home early in Sweden: in our population the median age at which students leave their parental home is 21, and the average is 22.5. The share of students living with their parents is below 20%.[25] In Section 4.4 we corroborate that the importance of parents during college is captured by their importance on initial heterogenity in endowments at college entry.

Putting financing of college costs in Sweden into an international context, most

[22]In our data, more than 98 percent of eligible students receive the grant. Thus in the model we assume that all eligible students receive the grant.

[23]A random sample of 5,000 college students were surveyed in 2009 and 2,473 students responded. The data was gathered by Statistics Sweden (SCB) on behalf of Högskoleverket (HSV). 63% of respondents are women, median age is 24, and 40% of the students are 22-24 years old. The average respondent reports that 60% of their total income is study aid, more than 15% own labor income, around 15% savings, and less than 7% transfers from parents or partners.

[24]In fact, the main motivation for study aid in Sweden was to ensure equality of opportunity through equal access to financial resources during college for each individual, independent of their family background. In the US context, Abbott et al. (2018) argue that crowding out of parental transfers is a non-negligible factor that attenuates the effect of study aid policy. Their simulations indicate that every additional dollar of government grants crowds out 25-50 cents of inter-vivos transfers from parents. In our policy simulations we keep total study aid fixed, which should minimize the importance of crowding out of parental transfers when comparing aid policies.

[25]The actual share is almost surely lower: The data records co-residence as having the same residential address as the parents. Many students study a semester or two abroad, during which they typically move their Swedish residential address back to their parents' address.

European public Universities charge close to no tuition—ranging from zero tuition in Nordic and most Central and Eastern European countries to an average of 2,400 USD in the Netherlands (OECD, 2018). The one exception in Europe is the UK, with tuition up to 12,000 USD per year. Outside of Europe, Turkey charges zero tuition, while countries like Chile and the US charge substantially larger amounts as average tuition at US public colleges is 8,000 USD. Most countries have some form of financial aid, from near universal systems such as in Sweden, Denmark, Norway, Finland, and Turkey, to limited means-tested scholarships such as in Italy and Spain. Most countries provide study aid to cover not only tuition costs, but the full cost of college. Study aid amounts and coverage in Sweden are similar to those in other Nordic countries, although grant shares differ. The UK government provides Maintenance grants and loans to cover living costs in addition to the aid covering tuition costs. Similarly, 26.4% of US college students have a net college cost of zero (i.e. tuition and fees minus all grants), 44.4% have a net cost of no more than USD 1,110, and only 18.3% are actually facing a net cost of USD 8,700 or more.[26] Additionally, the US Federal Government offers students access to borrow (e.g. Stafford loans) up to the total cost of college—including tuition, room, board, books, and other expenses directly related to college—less any other financial aid received in the form of grants. Average student debt is also comparable. According to OECD (2018), the average amount of debt from public loans is USD 24,900 in the US, and around USD 20,000 in Sweden.

3 The 2001 Reform of the Study Aid System

In 2001 a comprehensive reform affected three major aspects of the study aid scheme: means-testing and income requirements, grant share, and loan repayment plan. These aspects of change are detailed in the following three subsections.[27]

[26]Source: U.S. Department of Education, National Center for Education Statistics (NCES), 2015-16 National Postsecondary Student Aid Study (NPSAS:16).

[27]CSNFS (2001) contains additional details (in Swedish) on the reform. Eligibility requirements were also changed along various dimensions: Part-time enrollment choices were expanded, the ex-post ECTS requirements for the first year of higher education were relaxed, and it became easier to regain eligibility after losing it for one or more semesters. We do not model these aspects, since very few students appear to enroll part-time and we cannot detect that ECTS- and time-eligibility requirements have been enforced differently before and after the reform in the data.

3.1 Student Income Thresholds and Means-Testing

Students are means-tested every semester: They receive the maximum aid amount if their income is lower than the *maximum income threshold*, $\overline{Y}$. If their income is above the threshold, the available aid amount decreases proportionally in discrete steps of one week of aid for every increase of income equivalent to 5% of the threshold. The threshold is calculated every year as a proportion of an inflation adjusted base amount, the *prisbasbelopp*.

After the reform, the threshold increased from 0.75 times the *prisbasbelopp*, i.e. $\overline{Y}_{pre} = 27,675$ SEK, in the spring semester of 2001 to 1.25 times the *prisbasbelopp*, i.e. $\overline{Y}_{post} = 46,125$ SEK, in the fall semester. This is an increase by 67% in non-taxable student income. This also implied to a reduction of the labor income tax: the total income of students—composed of aid and labor income—is taxed less after the reform as the implicit tax on total student income above the threshold went from $\tau_{pre} = 1.189$ to $\tau_{post} = .786$. As a consequence, the maximum income above which students do not receive any aid also increased by 67%: from 55,350 SEK to 92,250 SEK.[28]

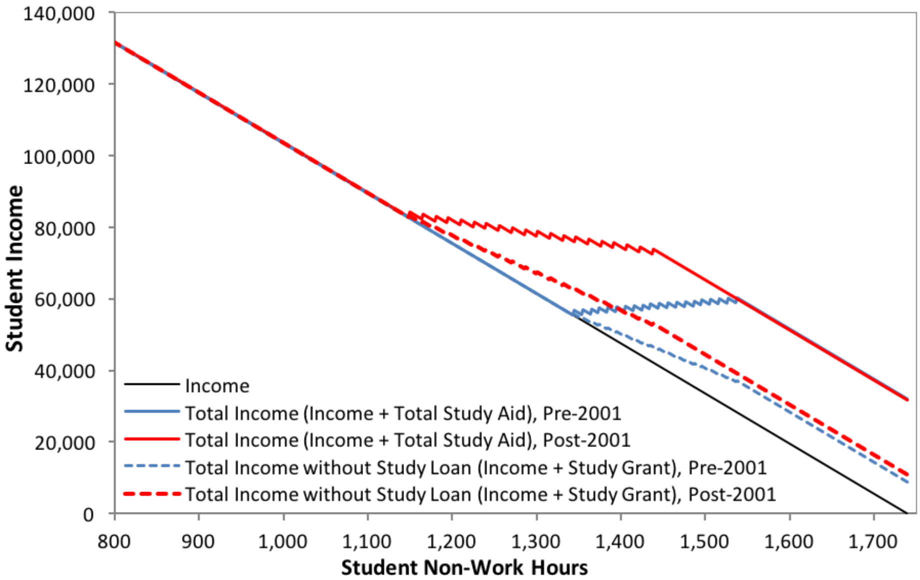
The immediate impact of the income threshold increase is that more students are eligible for the maximum aid amount. Thus fewer students face the implicit tax rate. The decrease in the tax rate further lowers work disincentives. Figure 2 illustrates the budget constraint of the students, with total income (consumption) on the y-axis and non-work hours (leisure) on the x-axis. The change in means-testing rules changes work incentives: There is no effect on the extensive margin of labor supply and no effect on students earning less than the pre-reform threshold. Students working many hours, who were far from being eligible for aid before the reform, may now work fewer hours and receive the same utility. Students working an intermediate amount of hours may either work more to take advantage of the lower tax rate, or work less and receive at least as much income. The overall effect of the means-testing on the intensive margin is thus an empirical question as it depends on the relative strengths of the income and substitution effects on student labor supply.

3.2 Grant Share

After the reform, the total amount of aid available to each student was left unchanged, but the grant share was made more generous: from $b_{pre} = 27.8\%$ to $b_{post} = 34.5\%$ of total aid. The increase in the grant share directly loosens the

[28] Further details on the *prisbasbelopp* for the relevant years are in Table *A.1* in Appendix A.

Figure 2: Total Student Income, Before and After the Reform



Total student income— including the maximum student aid and grant amount—as function of yearly hours not worked (leisure) before and after the reform. Constructed assuming a non-working student has 1739 leisure hours a year and a working student has an hourly wage of 140 SEK. All amounts are per semester in real 2000 SEK. The exchange rate on December 31, 2000 was 9.3955 SEK/USD and 8.8263 SEK/EUR. Figure *A.1* in Appendix A shows student aid as a function of student income before and after the reform.

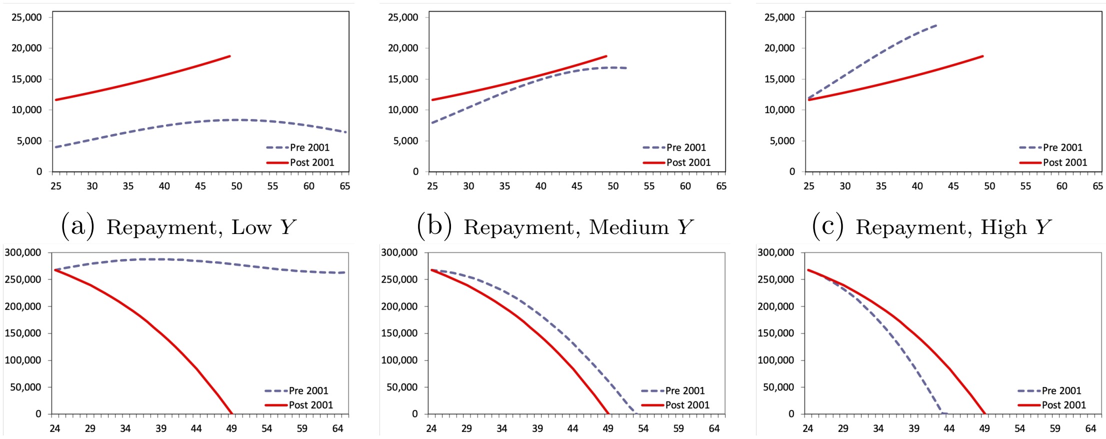
student budget constraint. Fewer students may take up the loan if the higher grant amount provides sufficient credit. This increase may also affect student labor supply, as illustrated in Figure 2. A student working h hours could get a higher total income post-reform or the same total income as before, but working fewer hours. Depending on income and substitution effects, students may simply work the same amount of hours at a higher total income or work less at the same income. Therefore, the increased grant share affects both the intensive and the extensive margin of student labor supply.

3.3 Loan Repayment Plans

The loan repayment plan was changed from an income-contingent plan (*studielån*) to an annuity-based one (*annuitetslån*). Before the reform, and since 1989, the installments consisted of 4% of the labor income earned 2 years prior, with a minimum installment of 1,320 SEK. The debt is written off in case of turning 65 years old, death, or long-term sickness.

After the reform, the installments became a 25-year annuity that keeps some

Figure 3: Expected repayment plan and Evolution of Debt.



(a) Repayment, Low Y (b) Repayment, Medium Y (c) Repayment, High Y

(d) Debt Evolution, Low Y (e) Debt Evolution, Medium Y (f) Debt Evolution, High Y

Expected repayment plan and evolution of debt for a student who exits college at age 24 with 240 weeks of outstanding student debt (2001 amounts), and with starting yearly earnings equal to: "Low", (a) and (d), the average in our sample minus one standard deviation ($Y_{24} = 99,426$ SEK); "Medium", (b) and (e), the average in our sample ($Y_{24} = 198,981$ SEK); and "High", (c) and (f), the average in our sample plus one standard deviation ($Y_{24} = 298,536$ SEK).
The student exits college at age 24 to work full-time, at age 65 any remaining debt is forgiven. The interest rate on the loan is set at 2.5%. Income equation: $\ln(Y_a) = \ln(Y_{24}) + 0.06H_a - 0.0012H_a^2$, where Y_a is earnings and H_a experience at age a. Pre-reform figures are displayed with dashed lines and post reform figures with solid lines. All amounts are in real 2000 SEK. The exchange rate on December 31, was 9.3955 SEK/USD and 8.8263 SEK/EUR.

income contingency though the possibility to apply for a reduced installment. The requirements for the reduced installment are either a negative income shock or receiving unemployment or disability benefits. The reduced installment consists of 5% of current income—after which the 25 years annuity repayment plan is recalculated.

The loan repayment plan directly affects the expected future value of working, since the repayment depends on future income, the level of student debt, and consequently on loan take-up choices. The reform meant that this inter-temporal trade-off became stronger for low earners and weaker for high earners. Figure 3 illustrates the expected repayment plans under the two plans, for a simulated student with the maximum possible pre-2001 student debt and different income at labor market entry. The figure shows that the reform implied higher installments for the low earners and lower installments for the high earners. For an entry income of 198,981 SEK per year—the average income in our data and slightly higher than the average entry income of a college dropout—the two repayment plans are not very different: the yearly installments are slightly higher and the student fully repays the debt five years earlier after the reform. We calculate the present value of lifetime income as the present value of the repayment installments due by the student from college exit until the debt is fully repaid, or alternatively, until age 65. We find that on average the reform increased the cost of borrowing by 10,924 SEK (5% of entry income). For an entry income equal to the average minus one standard deviation, however, the pre-reform repayment plan consisted of much lower installments such that the student debt would never be repaid, as shown in Figures 3a and 3d. The present value of lifetime income of this low earner was reduced by 91,552 SEK (92% of entry income). The opposite is true for an entry income equal to the average plus one standard deviation. This student repaid the debt faster in the pre-reform regime. The present value increased by 11,641 SEK (4% of entry income) for this high earner. Thus, the reform changed the incentives for human capital and debt accumulation in opposite directions for individuals with high and low earnings capacity.

3.4 Impact of the 2001 Reform

Did the reform have an impact on student choices and budget sets? In this section, we investigate the exogenous variation generated by the reform that we exploit to estimate our structural model and benchmark the ex-ante counterfactual policy evaluation. To illustrate this variation, we estimate a series of reduced form regressions of the form:

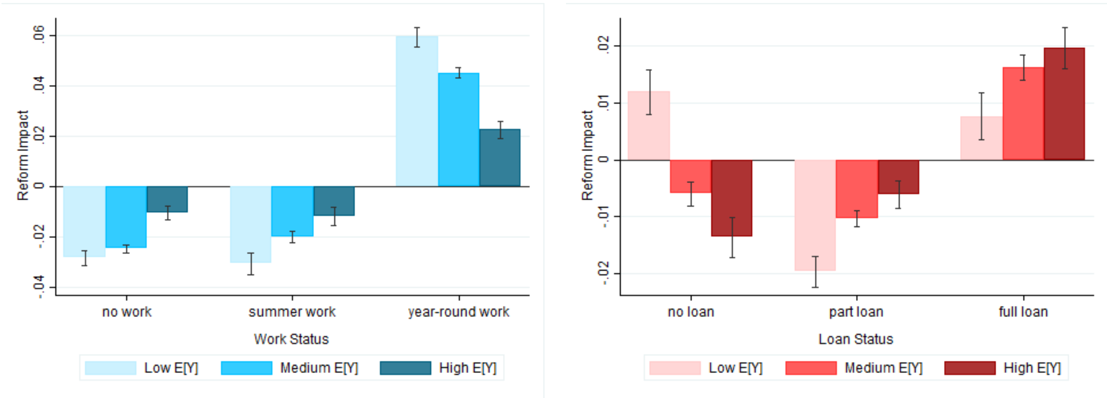
$$\Upsilon_{it} = \omega_0 + \omega_1 Post_{it} + \omega_2 X_{0i} + \varepsilon_{it} \tag{1}$$

where $X_{0i} = (A_i, K_i, H_{0i}, S_{0i}, m_i)$ is a vector of initial conditions, A is an indicator for having a high school GPA in the top decile, K is an indicator academic STEM high school degree, H_0 is labor market experience at college enrollment, S_0 is a vector of indicators for the level of the college program of initial enrollment, and m is the unobserved type indicator.[29] Subscript i denotes individuals and subscript t denotes time since initial college enrollment. $Post$ is an indicator for the observation being in the post reform period.

We are interested in how potential outcomes Υ^p_{it} vary with counterfactual study aid policies $p \in \mathcal{P}$, where $\mathcal{P}$ is the set of feasible policies. Let p' be the sub-vector of p containing the four policy parameters—related to the grant share, loan repayments, and means-testing—that the reform changed. $Post = 0$ indicates

[29] See Section 4.3 and Appendix C for the details of how we classify individual types using the group fixed effects approach proposed by **?**.

Figure 4: Impact of the Reform, Student Work and Loan Choices



(a) Student Work Choices

(b) Student Loan Choices

Reduced form estimates of the total ex-post effect of the reform on (a) student work choices (no work, summer work only, year-round work) and (b) student loan take-up choices (no loan, part loan, full loan) during the first six years after initial enrollment. Bars display before-after point estimates, $\widehat{\omega}_1$, and capped spikes display the 95% confidence intervals. All estimates presented separately by expected income: more than one standard deviation below the average (Low $\mathbb{E}[Y]$), more than one standard deviation above the average (High $\mathbb{E}[Y]$), in between (Medium $\mathbb{E}[Y]$). Figure *B.1* in Appendix B shows the total effect of the reform on student budget sets and Figure *B.2* shows how the reform shifted the earnings distribution.

that choices are made with the pre-reform policy $p'_{pre} = (b_{pre}, \widehat{I}_{pre}, \overline{Y}_{pre}, \tau_{pre})$ and $Post = 1$ indicates that choices were made with the post-reform policy $p'_{post} = (b_{post}, \widehat{I}_{post}, \overline{Y}_{post}, \tau_{post})$. ω_1 therefore captures the total ex-post reform effect we observe in the data when we change the study aid policy instruments from p'_{pre} to p'_{post} while keeping all other policy instruments in p fixed—including total aid.

We estimate the parameters in (1) by OLS for the pooled sample of those who enroll in college. We focus on the first six years of enrollment $0 \leq t \leq 6$ when all students are eligible for the universal aid and for which we observe everyone in the enrollment cohorts 1994-2002. Figure 4 presents the total ex-post reform effect for the choices of primary interest, Υ_{it}: work choices in Figure 4a and loan take-up choices in Figure 4b. Estimates are presented separately by whether expected income is more than one standard deviation below the average (Low $\mathbb{E}[Y]$), more than one standard deviation above the average (High $\mathbb{E}[Y]$), or in between (Medium $\mathbb{E}[Y]$). Expected income is calculated as the predicted income from a fully saturated linear regression of log-income on the initial conditions in X_0 for those working full-time while not enrolled in college.

Figure 4a shows the total reform effect on the work choices we include in the model: not working, working only during the summer, and working also during the academic year. Students increased their work both at the extensive and the intensive margin: The reform reduced the likelihood of not working by 2pp and increased the likelihood of year-round work by 4pp. Students with high expected income respond less in terms of substituting from no work to year-round work.

Figure 4b shows the reform impact on loan take-up by expected income. The loan take-up choices are: taking up only the grant and no loan, taking up half of the available loan, or taking up the full available loan. On average, the reform reduced the share of students not taking up the loan or only taking up part of the loan, and increased the share of students taking up the full loan by 1.7pp. However, these responses are very heterogeneous by expected income. Students with low expected income shifted from taking up partial loan to not taking up any loan, while students with high expected income increased loan take-up across the board. High expected earners became 1.4pp less likely not to take up any loan during the year, and 2pp more likely to take up the full loan. On the other hand, low expected earners became 1.2pp more likely not to take up any loan during the year. This shift is as expected from Figure 3, that shows how low expected earners have a higher borrowing cost after the reform, while high expected earners have a lower borrowing cost.

Overall, the reform implied that students funded more of their college costs by working and less by borrowing. This substitution from debt financing to labor income financing was stronger for those with low expected income.[30] The reform of the study aid scheme increased student employment and income, while it compressed the student debt distribution and caused high expected earners to become more likely to take up the student loan, and low expected earners to become less likely to take up the student loan.

4 The Model

In this section we set up the dynamic discrete choice model of joint education, work, and student loan take-up decisions. Choices are made at the individual level, but we suppress individual subscripts for most of this section for ease of exposition. Additional details on the functional forms are in Appendix C.

[30]This can be seen directly in Figure *B.1* in Appendix B that shows the reform impact on each budget component: income, grants and loans.

4.1 Initial Conditions and Unobserved Heterogeneity

At $t = 0$, high school graduates are characterized by the initial conditions A, K, and H_0. A is an indicator for having a high school GPA in the top decile and K is an indicator for graduating from the academic STEM high school track.[31] H_0 denotes labor market experience at college enrollment.

We account for initial heterogeneity beyond what is captured by A, K, and H_0 by introducing an additional state m that is unobserved to us and persistent over time. This unobserved state allows us to relax the i.i.d assumption on the unobservable shocks, both allowing for flexible correlation of the errors across the various alternatives and over time, and accounting for unobserved heterogeneity and dynamic selection. We assume that there are $m = 1, ..., M$ discrete types of individuals who differ in their preferences, academic productivity, and labor market productivity. Each type comprises a fixed proportion of the population and is persistent over time. To reduce the number of parameters and avoid identification issues, we only allow for first-order heterogeneity effects. The unobserved state enters linearly in college-work-loan preferences, in the earnings equation, and in the course credits production function. This way, we allow for self selection into work based on type, and for dependence between choices and income.[32]

4.2 Individual Choices

We model choices from time of initial college enrollment to exit. Let t denote years since initial college enrollment. At $t = 0$, individuals decide whether to enroll in college and, if so, the length of the program: either short or long, $S_0 \in \{0, 3, 4\}$. After this initial choice, the college students face an optimal stopping problem with finite horizon. From $t = 1$ onwards, students can choose to exit college and work full-time which is an absorbing state. By $t = 11$ everyone is in the labor force.

Every period after enrollment, students make three simultaneous choices. First, whether to stay enrolled $s_t \in \{0, 1\}$. Second, whether and how much to work while studying, either only during the summer or also during the academic year, $h_t \in \left\{0, \frac{1}{2}, 1\right\}$. Finally, how much of the student loan they are eligible for to take up: none, half, or all of it $\ell_t \in \{0, \frac{1}{2}, 1\}$. Therefore, students face ten mutually

[31] GPA measures multiple dimensions of ability—both cognitive and non-cognitive (Borghans et al., 2016)—and we include it as a determinant of college and labor market success as it is the only general ability measure we have available in the data.

[32] This approach is common in the literature; see e.g. Eckstein and Wolpin (1999) or Keane et al. (2011) for a discussion.

exclusive and exhaustive choices denoted by index $j \in \{0, \dots 9\}$.

Let d_t^j be an indicator equal to one if alternative j is chosen and zero otherwise. Students choose $\{d_t^*\}_{t=1}^T$, a set of decision rules for every possible realization of the observed and unobserved variables each period, such that:

$$d_t^* = arg \max_j \mathbb{E}\left[\sum_{\tau=t}^{T} \beta^{\tau-t} \sum_{j=0}^{9} \left[d_\tau^j U_\tau^j(X_\tau, m, \epsilon_\tau)\right]\right]. \quad (2)$$

where $X_t = (A, K, S_0, D_t, G_t, E_t, H_t, h_{t-1}, \ell_{t-1})$ is the vector of observed state variables, m is unobserved individual heterogeneity, ϵ_t is the vector of alternative specific preference shocks, and β is the rate at which students discount the future.

Choices determine next period's state and are determined by the current state X_t. X_t includes high school GPA and track (A, K), initial program choice S_0, the stock of student debt D_t, acquired course credits G_t, highest acquired degree E_t, and labor market experience H_t. We also keep track of last period's choices of work and student loan take-up, denoted by h_{t-1} and ℓ_{t-1}.

Individuals face uncertainty about how much income they will earn and how much academic capital they will acquire, in terms of both course credits acquired each year and graduation.

4.2.1 Labor Market

Every period, individuals decide how much to work, $h_t \in \left\{0, \frac{1}{2}, 1\right\}$. Working increases the stock of labor market experience according to the law of motion $H_t = H_{t-1} + h_{t-1}$.

Conditional on working, earnings Y depends on the choice h_t, the worker's unobserved type, previous labor market experience, academic achievement, and an idiosyncratic labor market productivity shock υ_t^y normally distributed with mean zero and variance σ_y^2. α is a vector of parameters of the earnings equation.

$$\log(Y_t) = Y(S_0, G_t, E_t, H_t, m, h_t|\alpha) + \upsilon_t^y. \quad (3)$$

In particular, we are able to separate the pecuniary importance of degrees from that of credits by letting earnings depend both on highest acquired degree and cumulated course credits. This allows for nonlinearities in the return to education: individuals who completed a degree receive higher earnings than individuals who completed the course credits necessary to obtain the degree, but did not actually graduate. These nonlinearities are also known as *sheepskin* effects.[33]

4.2.2 Academic Environment

We denote the stock of course credits by G_t and course credits accumulated from t to $t+1$ by g_t. Course credits follow the law of motion: $G_{t+1} = G_t + s_t g_t$. Course credit production is probabilistic in the sense that students are not sure how many courses they will pass during the academic year. We normalize a completed year of college to $g_t = 6$ course credits, equivalent to actual ECTS production being $g_t * 10$. We further discretize it into eight values: $g_t \in \{0, 1, 2, 3, 4, 5, 6, 7\}$, where $g_t = 7$ captures all credit production above 60 ECTS.

Production of academic course credits depends on whether the high school GPA was in the top decile A, high school STEM track K, and the type m. Course credits also depend on whether the student has already acquired a degree E_t (and is simply continuing to accumulate credits), as well as on the stock of course credits, G_t, capturing self-productivity of academic skills. Finally, course credits depend on time since initial enrollment t, on the student's current choices of loan take-up and work, and on i.i.d. logistically distributed unobservable terms υ_t^{gs} for each level of enrollment $s \in 3, 4$. The continuous latent variable g_t^* reflects the academic knowledge acquired during the year:

$$g_t^* = g_t(A, K, S_0, G_t, E_t, d_t, m|\gamma) + \upsilon_t^{gs} \tag{4}$$

where γ is a vector of parameters.

Because it is common in our population to graduate with more credits than necessary and because there are cases in which graduation is achieved with fewer credits,[34] we model highest acquired degree as a stochastic function of accumulated course credits. Hence, graduation $E_t \in \{0, 1\}$ depends on the level of enrollment, time since initial enrollment, cumulated credits, and an i.i.d. logistically distributed shock υ_t^e. η is a vector of parameters.

$$\begin{aligned} E_{t+1} &= E_t + s_t \mathbf{1}\left[e_t^* > 0 | E_t = 0\right] \\ e_t^* &= e_t(G_t|\eta) + \upsilon_t^{es}. \end{aligned} \tag{5}$$

[33]See e.g. Heckman et al. (2006) for a thorough review of non-linearities in the return to education and other specification issues of the earnings equation.

[34]This is likely due to missing credits either because of misreporting from the University or the student being abroad for a period; e.g. in an exchange program.

4.2.3 Dynamic Problem of a College Student

By the Bellman principle of optimality and given the discrete nature of the choices in the model, we can rewrite the dynamic problem of a college student as:

$$V_t(X_t, m, \epsilon_t) = \max_j \left\{ V_t^j \left(X_t, m, \epsilon_t\right) \right\}, \tag{6}$$

where $V_t^j(X_t, m, \epsilon_t)$ denotes the alternative specific value function:

$$V_t^j(X_t, m, \epsilon_t) = \mathbb{E}\left[U_t^j\left(X_t, m, \epsilon_t\right) + \beta V_{t+1}\left(X_{t+1}, m, \epsilon_{t+1} \mid X_t, m, \epsilon_t, d_t^j = 1\right)\right]. \tag{7}$$

We assume the utility of the individual to be additively separable in the observable state X_t and ϵ_t.[35] Hence, the instantaneous utility of students is given by:

$$U_t^j(X_t, m, \epsilon_t^j) = u\left(C_t^j(X_t, m)\right) + n_t^j(X_t, m|\nu) + \epsilon_t^j; \tag{8}$$

where utility from consumption is CIES with constant intertemporal elasticity of substitution parameter λ. The consumption value of college attendance n_t^j can be thought of as the value of learning, less the psychological effort cost of studying. ν is a vector of parameters. We set it equal to zero for non students, $n_t^0 = 0$. The alternative specific preference shocks, ϵ_t^j, capture the fact that new information about alternative specific tastes is revealed to students each period. We assume they are i.i.d. Type I Extreme Value (EV) distributed.

Consumption while in college is determined by the following budget set:

$$C_t^j = Y_t h_t + \widehat{b}^j(X_t, m) + \ell_t \widehat{\ell}^j(X_t, m), \tag{9}$$

where Y_t is student earnings, $\widehat{b}^j$ is the grant amount, and $\widehat{\ell}^j$ is the loan amount the student is eligible for and takes up. Let $\overline{B}$ denote the maximum base aid amount. The maximum aid amount, $\widehat{B}_t$, the student is eligible for is given by the following rule:

$$\widehat{B}(X_t, m) = \left[\overline{B} - \tau_B(Y_t - \overline{Y})\mathbf{1}_{\{Y_t \geq \overline{Y}\}}\right]. \tag{10}$$

where the grant amount is given by $\widehat{b}(X_t, m) = b\widehat{B}(X_t, m)$, and the maximum base grant amount is $\bar{b} = b\overline{B}$. Similarly, the loan amount students are eligible for is

[35] This assumption is crucial for the Conditional Choice Probability (CCP) estimation method we apply. Keane et al. (2011) and Arcidiacono and Ellickson (2011) provide thorough discussions of this assumption.

given by $\widehat{\ell}(X_t, m) = (1-b)\widehat{B}(X_t, m)$, and the law of motion of student debt is:

$$D_{t+1} = D_t + \ell_t \widehat{\ell}(X_t, m). \tag{11}$$

The reform increased both the grant share, b, and loosened means-testing by increasing the maximum student income threshold, $\overline{Y}$, and decreasing the implicit tax rate, τ_B.

Once individuals exit college, working full-time is assumed to be an absorbing state. Therefore, expected utility after college exit only depends on the choices made during college enrollment and on initial conditions, as summarized by the state at exit. For them, consumption is equal to labor income minus repayment of outstanding debt and is given by:

$$C_t^0 = Y_t^0(X_t, m) - \widehat{I}(X_t, m), \tag{12}$$

The budget constraint in (9) and (12) is static in the sense that we do not model savings.[36] Budget constraints are, however, inter-temporally linked through student debt accumulation and loan repayment post-college exit, $\widehat{I}$. These installments also changed with the reform—from IC to annuity.

Fully embedding the study aid rules and the reform thus provides us with exogenous variation in enrollment, work, and loan take-up choices of students that is independent of individual characteristics. We model the reform as a surprise to the students, and they expect the new rules to be in place for their remaining college enrollment spell.

4.2.4 Enrollment Decision

Finally, we characterize the initial enrollment decision. At $t = 0$, high school graduates decide whether or not to enroll in college and what length of program to attend if they enroll. This choice is denoted by $S_0 \in \{0, 3, 4\}$, where 0 denotes no enrollment in college, and 3 and 4 denote the length of the program conditional on enrollment. The choice is taken by maximizing discounted expected utility from enrollment:

$$W_0(X_0, m, \epsilon) = \max_{s \in S_0} \left\{ \mathbb{E}\left[s(X_0, m|\zeta) + \epsilon_0^s + V_0^s(X_0, m, \epsilon_0) \right] \right\}. \tag{13}$$

[36] We neither have data on consumption, wealth, nor assets.

The non-pecuniary utility from enrollment depends on the type m, observed initial conditions $X_0 = (A, K, H_0)$, and alternative specific Type I EV preference shocks, ϵ_0^s. ζ is a vector of parameters. The initial conditions— high school GPA, academic high school STEM track, and labor market experience—encompass the most important factors that college admission is conditioned on.[37]

4.3 Estimation

We estimate the parameters of the law of motions of the course credit production function (γ) and the graduation probability (η), the parameters of the earnings equation (α), and of the utility function (ν and ζ), including the intertemporal elasticity of substitution (λ), using a maximum-likelihood based estimation method.[38]

Specifically, we use the *two-step grouped fixed-effects* estimation method proposed by **?**. This method consists of a first step where individuals are classified into types using the *k-means* clustering algorithm and a set of individual-specific moments. In a second step, we estimate the model given this classification. **?** show how this sequential estimator can greatly improve computational tractability, especially in applications—such as this one—where the state space is very large and the dimension of the unobserved heterogeneity is much smaller than the number of observations.

The k-means classification groups together individuals who are similar in their unobserved traits by minimizing the within-group distance from the group average of the individual-specific moments. Therefore, it strongly relies on a correct choice of the moments used in the clustering. In our application, this choice is driven by the model: we use the residuals of five reduced form equations that capture the main mechanisms of our model. We regress the outcomes of the model—log yearly income, cumulated course credits, log student debt, and graduation either in 3-year or in 4-year programs—on the relevant subset of the state space of our model, and on year dummies. We then take the individual average residuals from these equations as input for the k-means algorithm.

In Appendix C we present descriptive statistics on the two types. Table *C.2* shows choices, outcomes, and initial characteristics by type. The two types differ in

[37]The main omitted variable is the Swedish SAT (SweSAT) score, which is highly correlated with high school GPA. Öckert (2010) provides a detailed description of the college admission process, while Bjorklund et al. (2006) provide a thorough description of education in Sweden during this period.

[38]Details on the estimation method and results are in Appendix C.

unobserved effort, ambition, or motivation. Type 2 individuals are lower achievers: they have lower high school GPA, fewer of them attended the STEM high school track, and they are less likely to enroll in college. However, type 2 individuals who enroll in college have higher high school GPA and more of them attended the STEM high school track. Nevertheless, they are less productive in college as they are more likely to enroll in shorter programs and produce fewer course credits per enrollment year. Type 2 individuals are also less likely to be employed and have a lower labor income—both unconditionally and after college exit. Table *C.3* shows that all the included and excluded initial conditions explain about 11% of the total variation in the type classification.[39] It also reveals that type 2 individuals are more likely to live with their parents and have less resourceful parents.

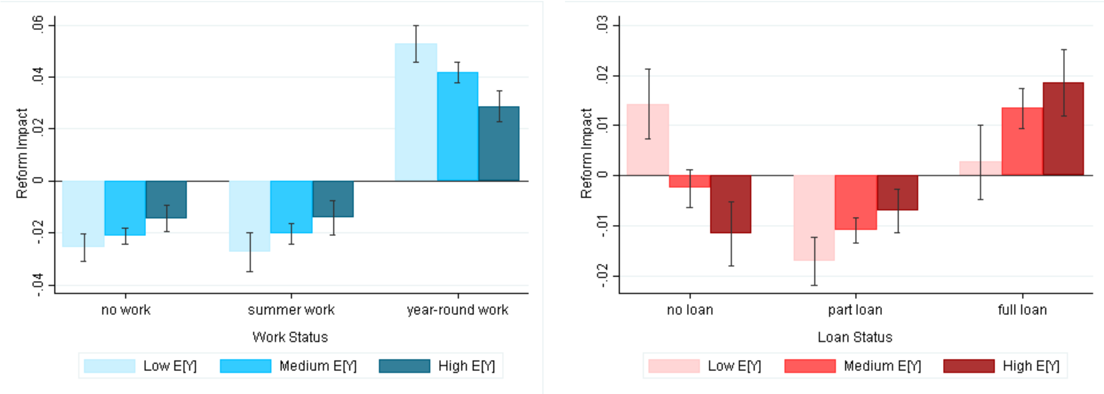
Table *C.6* in Appendix C presents all the parameter estimates. They reveal a positive effect of both ability and cumulated course credits on course credit production. This captures the overall self-productivity of academic skills as already accumulated academic knowledge enhances future academic learning. The effect of working on course credit production is instead ambiguous: Working full-time while studying has a negative effect, increasing the probability of earning zero credit by 3.9pp (19%) and decreasing the probability of earning six credits by 1.8pp (15%) for short programs. Only working during the summer has a positive effect, decreasing the probability of earning zero credit by 2.9pp (20%) and increasing the probability of earning six credits by 2pp (14%) for long programs. This result highlights the fact that the timing of work during the year is crucial and in line with Joensen (2013a) who finds that the effect of hours worked on academic productivity follows an inverse-U shape, with nine or fewer work hours being beneficial and more than 18 work hours during the week being detrimental. Our finding that loan take-up also increases course credit production is in line with Marx and Turner (2019) who find that random loan offers to community college students increases loan take-up, GPA, completed course credits, and the likelihood of 4-year college transfer and Black et al. (2020) who find that extending loan limits increased student debt and degree completion.

4.4 Model Validation

We validate the model to increase the credibility of our counterfactual policy simulations. First, we assess whether the model is able to predict the total effect

[39]F-test statistics of the null hypothesis that excluded initial conditions are not jointly significantly related to outcomes show that the types explain variation in outcomes that is largely orthogonal to excluded initial conditions (Figure *C.4*).

Figure 5: Impact of the Reform, Predicted Student Work and Loan Choices



(a) Student Work Choices, Predicted

(b) Student Loan Choices, Predicted

The Figure displays reduced form estimates of the total effect of the reform on predicted student work and loan choices during the first six years after initial enrollment. It replicates (a) Figure 4a and (b) Figure 4b in Section 3.4 using model predictions rather than actual data.

of the reform on student behavior. Second, we assess in-sample and out-of-sample model fit for the full sample and for various sub-groups that presumably are facing different constraints, have different preferences, and different expectations about the future. The model is not tailored to fit these differences, thus they are a challenging test of whether the model captures the reasons why individuals with different preferences or facing different constraints make different choices and have different outcomes.[40]

A crucial validation of our model is that it predicts the total effect of the reform on student choices. We assess this by replicating Figure **??** using the model predictions rather than data. Figure 5 shows that we accurately predict the total impact of the reform on student work choices and loan take-up choices. The reduced form estimates using the model predictions are not statistically significantly different from the ones using the actual data for any of the six work-loan choices. The reform is one particular aid policy counterfactual that changes the policy instruments from p'_{pre} to p'_{post}. The fact that the model reproduces this counterfactual that we observe in the data is a valuable benchmark and increases confidence in the counterfactual policy simulations that further change each of these policy instruments.

Figure *D.1* show highest acquired degree and student debt by degree over time since initial enrollment—both data and model predictions. We accurately

[40] We highlight the most important patterns in this section, and refer to Appendix D for additional figures.

predict the total level of graduates and non-graduates, as well as student debt by degree. The fit is similarly good by ability and skill. This is key as ability and skill constitute the most important aspects of initial heterogeneity at college enrollment. High school graduates from a STEM high school track with a GPA in the top decile are 50pp more likely to acquire a long college degree and 30pp less likely to drop out. Our model predicts these large differences in graduation rates *and* the timing of college exit.

Our model has four dimensions of initial heterogeneity: ability, skill, experience, and (unobserved) motivation type. However, dimensions of heterogeneity that we do not model could affect human capital accumulation and student debt. We can link our predictions to various individual and family characteristics to assess how our model fits conditional on them. Our model captures differences in highest acquired degree and student debt over time across sub-groups defined by: parental income and education, co-residence with parents, field of study, gender, and a cost of living index (CLI) across Swedish cities.[41] The Figures in Appendix D corroborate that we model enough initial heterogeneity—particularly through A and K—to capture the differences in choices and outcomes across these extra dimensions of heterogeneity.

Parental background is an important determinant of education and labor market outcomes.[42] We find large differences in college graduation and dropout by parental education and income. Figure *D.2* shows that around 70% of high school graduates whose parents have a postgraduate degree complete college education, compared to only 45% of those whose parents have at most a high school degree. Our model fits this fact through the common forces that operate equally for everyone with low endowments, because children of highly educated parents are more likely to have a STEM high school degree and a high GPA. This suggests that families play an essential role in shaping endowments at high school graduation, but the challenges faced in college are a first-order consequence of low endowments at college entry rather than distinctive challenges faced by students from more disadvantaged families during college.

[41] We under-predict graduation from short degrees in Education, and Health Sciences, Health and Social Care that train pedagogues and elementary school teachers, and health professionals such as nurses, midwives, and radiology technicians, respectively. Certification is required for nurses and teachers/pedagogues in order to work, thus sheepskin effects in these fields are presumably stronger than for most other degrees. On the other hand, we under-predict dropout from Humanities and Arts, which include short degrees in music, language, communications, and history, for which certification is not necessary. Moreover, this group also includes 20% of enrolled students that take courses without enrolling in a program.

[42] See e.g. Heckman and Mosso (2014) for a recent comprehensive review of this vast literature.

To assess the out-of-sample model fit along the temporal dimension, we hold out a sample of students that enroll in college shorts after the reform, and 10-11 years after the first cohort in our estimation sample. Figure *D.10* shows that our model is almost spot on in predicting degrees and the timing of college exit. The model slightly under-predicts the level of student debt, but accurately predicts the relative differences in student debt by degree. This is a challenging test of the model, since many aggregate factors and other policies change during this decade. This test rules out that general equilibrium effects, changes in initial conditions, or other temporal factors are a major concern for the model predictions.

5 Policy Simulations

In this section, we use the model to construct counterfactuals to assess the ex-ante effects of study aid policies.[43] We perform counterfactual policy simulations by changing three sets of policy parameters: repayment plans, grant share, and means-testing. Finally, we investigate policy interactions. We simulate how changing each policy instrument would affect student behavior, human capital and debt accumulation. Tables 1, 2, and 3 show the simulated impact of these study aid policies. The first column in each table shows the baseline choices, outcomes, and policy parameters; i.e. a simulation using the 2001 pre-reform study aid system. All policy simulations start from the same distribution of student initial characteristics in terms of ability, skill, and experience while the interest rate and the aid amounts are fixed at the 2001 value. To provide a cost-benefit analysis of each policy, we also show the impact on the total public aid costs, discounted future utility, and income at $t = 11$ as a summary of the labor market value of human capital once all individuals have exited university (or "earnings capacity").

5.1 Repayment Plans

Table 1 shows the effects of changing the loan repayment plan. First, we explore different income-contingent plans, where the installments are 5% and 10% of income, respectively. Second, we implement the post-reform 25-year annuity and a shortened 15-year annuity. The annuity plans we analyze still have an implicit insurance component, since repayments can be postponed when income is below a floor. However, because the insurance component is not directly tied to actual

[43] We refer to Appendix E for details on the simulation algorithm and additional policy simulations by observed and unobserved heterogeneity.

Table 1: Policy Simulations, Repayment Plans

	Baseline	Income Contingent IC5	Income Contingent IC10	Annuity 25 years	Annuity 15 years
Enrollment:					
Short Program	*0.2539*	-0.0003	-0.0011	0.0022	-0.0112
Long Program	*0.1681*	0.0000	-0.0001	0.0003	-0.0012
Student Choices:					
Year-round Work	*0.2775*	0.0022	0.0074	0.0081	0.0278
Summer Work	*0.5139*	-0.0017	-0.0056	-0.0059	-0.0211
Full Loan	*0.7266*	-0.0011	-0.0034	-0.0036	-0.0130
Partial Loan	*0.1035*	0.0005	0.0016	0.0014	0.0063
Academic Outcome:					
Dropout	*0.3959*	0.0019	0.0067	0.0084	0.0220
Short Degree	*0.3443*	-0.0017	-0.0058	-0.0053	-0.0229
Long Degree	*0.2598*	-0.0002	-0.0010	-0.0031	0.0010
Weeks to Dropout	*202*	-0.3115	-1.0950	-1.2206	-3.6413
Weeks to Short Degree	*227*	-0.3013	-0.9995	-1.1280	-3.2562
Weeks to Long Degree	*279*	-0.0278	-0.1507	-0.1952	-0.3472
Debt at Exit:	*155,741*	-642	-2,152	-2,453	-7,410
Dropout	*134,615*	-492	-1,686	-1,753	-6,255
Short Degree	*153,705*	-663	-2,139	-2,385	-7,146
Long Degree	*191,069*	-675	-2,241	-2,552	-8,095
Income at t=11	*253,723*	-88	-356	-174	-1,767
Discounted Utility:					
Ex ante, at t=0 (%)	-	0.0000	0.0000	0.0007	-0.0012
Ex post, at t=11 (%)	-	-0.1130	-0.2765	-0.1129	-1.3149
Cost:					
per Student	*124,068*	-6,144	-14,571	-4,121	-53,769
Total (%)	-	-5.3664	-12.8680	-3.5318	-47.2644
Means Testing:	0.75	0.75	0.75	0.75	0.75
Grant Share:	27.8%	27.8%	27.8%	27.8%	27.8%
Loan Repayment:	IC4	IC5	IC10	25-year	15-year

Differences from the baseline student choices and outcomes, percentage differences from the baseline for total cost and discounted utility. The baseline is the pre-reform year 2001, the values for the baseline are in levels. The maximum aid amount is 64,232 SEK. All amounts are in real 2000 SEK (the exchange rate on 12/31/2000 was 9.3955 SEK/USD and 8.8263 SEK/EUR). All other policy instruments are kept at the baseline level. Means-testing shown as percentage of the *prisbasbelopp*.

income realizations in the annuity plan, the shift from the income-contingent to the annuity plan is disproportionately more costly for the academically marginal students as they become more likely to drop out when borrowing becomes more costly for them.

We find that all these counterfactual repayment plans reduce student debt at exit, income at exit, discounted utility, and study aid cost to the government. Income-contingency of repayments exhibits and elasticity of -0.41 for debt at exit and -0.12 for income, but is marginally decreasing. This means that increasing the implicit proportional tax rate on post-college income by 1 percentage point (from 4% to 5%) has around 30% of the impact of increasing it by 6pp (from 4% to 10%), which reduces debt at exit by 1.38% and earnings capacity at $t = 11$

by 0.36%. Although it is not surprising that students take up less debt when it becomes more costly, students tend to compensate by working more, such that they accumulate less academic capital and consequently have a lower earnings capacity. Further increases in income-contingency have little impact on outcomes because of the decreasing marginal responses when the implicit tax rate increases beyond 10%. Proponents of ICR plans typically focus on the insurance aspect for those who get bad labor market draws after college exit, our results suggest that the choice of repayment rate and potential adverse effects on human capital accumulation should also be considered.

We find that moving from the pre-reform 4% ICR plan (IC4) to the post-reform 25-year annuity has an average impact similar to moving to the IC10 plan. However, the distributional effects are very different. Tables *E.5* and *E.6* in Appendix E show the effects of changing the loan repayment plan by observed and unobserved type, respectively. Students with low high school GPA and non-STEM high school degrees, $(A, K) = (0, 0)$, are closer to the dropout margin, therefore their academic capital responds more to a change to the annuity repayment plan, while their labor supply responds less than that of students with high GPA and STEM high school degrees, $(A, K) = (1, 1)$. These responses are more pronounced when moving to the shorter 15-year annuity plan as the dropout rate increases by 2.2pp and timing of drop out decreases by 3.6 weeks. Moreover, 2.8pp more students work year-round and fund education through labor income rather than the loan, and 1.3pp fewer students take up the full loan, so that student debt at exit decreases by around 7,410 SEK. All types of students dropout or graduate sooner, with less debt, and lower earnings capacity. The responses on dropout and graduation rates are concentrated to shorter short degrees and driven by the strong responses by the less prepared students with $(A, K) = (0, 0)$ and the less-motivated type, $m = 2$.

Thus, front-loading repayment plans allows the policy maker to lower aid costs and recuperate costs faster, but at the cost of increased dropout rates, particularly for academically marginal students.

5.2 Grant Share

Table 2 shows the impact of implementing policies that only change the grant share of total study aid. We show simulations with the grant share ranging from 5% to 95% of total aid.

Increasing the grant share has a large impact on student debt accumulation,

Table 2: Policy Simulations, Grant Share

	baseline	**Grant Share** 5%	**35%**	**50%**	**65%**	**95%**
Enrollment:						
Short Program	*0.2539*	-0.0042	0.0013	0.0041	0.0070	0.0137
Long Program	*0.1681*	-0.0007	0.0003	0.0009	0.0014	0.0026
Student Choices:						
Year-round Work	*0.2775*	0.0083	-0.0033	-0.0107	-0.0190	-0.0382
Summer Work	*0.5139*	-0.0047	0.0021	0.0066	0.0119	0.0241
Full Loan	*0.7266*	0.0123	-0.0036	-0.0110	-0.0187	-0.0334
Partial Loan	*0.1035*	-0.0027	0.0005	0.0017	0.0032	0.0049
Academic Outcome:						
Dropout	*0.3959*	0.0023	-0.0014	-0.0045	-0.0091	-0.0214
Short Degree	*0.3443*	-0.0043	0.0018	0.0059	0.0112	0.0250
Long Degree	*0.2598*	0.0020	-0.0004	-0.0014	-0.0022	-0.0036
Weeks to Dropout	*202*	-0.3095	0.2611	0.8756	1.5220	3.4475
Weeks to Short Degree	*227*	-0.9947	0.4354	1.5002	2.6735	5.9397
Weeks to Long Degree	*279*	-0.1117	-0.0084	0.0343	0.2065	0.6838
Debt at Exit:	*155,741*	51,088	-15,772	-48,336	-80,705	-145,004
Dropout	*134,615*	43,824	-13,592	-41,661	-69,637	-125,321
Short Degree	*153,705*	51,003	-15,713	-48,069	-80,098	-143,211
Long Degree	*191,069*	62,273	-19,254	-59,068	-98,742	-177,841
Income at t=11	*253,723*	-382	151	472	834	1,799
Discounted Utility:						
Ex ante, at t=0 (%)	-	-0.0027	0.0009	0.0030	0.0054	0.0110
Ex post, at t=11 (%)	-	-0.3148	0.1397	0.4765	0.8720	1.8293
Cost:						
per Student	*124,068*	-20,586	8,679	29,988	54,835	112,178
Total (%)	-	-17.0600	7.2461	25.4537	46.6674	96.7023
Means Testing:	0.75	0.75	0.75	0.75	0.75	0.75
Grant Share:	27.8%	5.0%	35.0%	50.0%	65.0%	95.0%
Loan Repayment:	IC4	IC4	IC4	IC4	IC4	IC4

Differences from the baseline student choices and outcomes, percentage differences from the baseline for total cost and discounted utility. The baseline is the pre-reform year 2001, the values for the baseline are in levels. The maximum aid amount is 64,232 SEK. All amounts are in real 2000 SEK (the exchange rate on 12/31/2000 was 9.3955 SEK/USD and 8.8263 SEK/EUR). All other policy instruments are kept at the baseline level. Means-testing shown as percentage of the *prisbasbelopp*.

while the effects on education attainment are modest. This means that policy makers can decide whether to privately (by the individual student) or publicly (by the government) fund college education— without altering education outcomes much. Increasing the grant share implies that fewer students drop out and more acquire a short degree, but students stay enrolled longer. Increasing the grant share also means less loan take-up, less year-round work, and more summer work. Students' discounted utility is still higher when the grant share is higher, because of their much lower student debt. For example, the increase in the grant share by 30pp (from 35% to 65%) has no significant impact on earnings capacity, but reduces debt by 46% (SEK 65,000) at an additional 35% (SEK 46,000) public aid cost per student. Thus the 0.9pp increase in short degrees draws from a 1.8pp reduction in long degrees and a 7.6pp reduction in dropouts has essentially no impact on the overall value of human capital as students work less during their extended enrollment time.

To better understand the mechanisms underlying these policy responses, we examine differences by initial conditions. Tables *E.7* and *E.8* show the impact of changing the grant share by observed and unobserved type, respectively. The increase in short graduation rates and the decrease in dropout rates is driven by a strong response by students with low high school GPA and non-STEM high school degrees, $(A, K) = (0, 0)$, and by students with low motivation, $m = 2$. These academically marginal students are generally less responsive in their student work behavior, but more responsive in their loan take-up behavior. While there is no effect on graduation rates for students with high GPA and STEM high school degrees, $(A, K) = (1, 1)$, and for the highly motivated type, $m = 1$, they stay enrolled longer—particularly those who eventually drop out or get a shorter degree. This means that the government can move academically marginal students from dropping out to acquiring short college degrees by providing relatively more grant funding and bearing a much higher funding burden.

5.3 Means-Testing

Table 3 reveals the effects of changing the means-testing, which directly changes the student work disincentive. We change the percentage of the *prisbasbelopp* that determines the semiannual income threshold and the implicit student income tax. The first three simulations increase the work disincentive by reducing the threshold from 0.75 to 0.50, 0.25, and 0.0001 times the *prisbasbelopp*—the last one essentially means that students do not receive aid if they work. The last three simulations reduce the work disincentive by increasing the threshold for allowable earnings from 0.75 to 1.00, 1.25, and 2 times the *prisbasbelopp*.[44]

The labor supply response is as expected: the looser the means-testing, the more students work year-round. When the work disincentive generated by the means-testing is above 0.75, students work more while still taking up the subsidized loan. Loosening means-testing has a small negative impact on human capital and discounted utility, despite the increased debt. On the contrary, stricter means-testing can have large impacts. The strictest means-testing that essentially eliminates the possibility to work and receive aid at the same time—by imposing

[44]Loosening the means-testing further to 5 times the *prisbasbelopp* essentially does not alter student behavior— suggesting that student labor supply is not constrained by the means-testing once it reaches 2 times the *prisbasbelopp*.

Table 3: Policy Simulations, Means-Testing

	baseline	Means Testing					
		0	**0.25**	**0.5**	**1**	**1.25**	**2**
Enrollment:							
Short Program	*0.2539*	-0.0198	-0.0083	0.0005	-0.0003	-0.0006	-0.0008
Long Program	*0.1681*	-0.0016	-0.0009	0.0000	0.0000	0.0000	-0.0001
Student Choices:							
Year-round Work	*0.2775*	0.0301	0.0229	0.0003	0.0127	0.0291	0.0561
Summer Work	*0.5139*	-0.0798	-0.0533	-0.0024	-0.0089	-0.0211	-0.0412
Full Loan	*0.7266*	-0.0072	-0.0044	-0.0005	0.0024	0.0055	0.0102
Partial Loan	*0.1035*	0.0053	0.0034	0.0001	-0.0002	-0.0003	-0.0010
Academic Outcome:							
Dropout	*0.3959*	0.0279	0.0218	0.0001	0.0063	0.0097	0.0071
Short Degree	*0.3443*	-0.0337	-0.0210	0.0007	-0.0054	-0.0083	-0.0068
Long Degree	*0.2598*	0.0058	-0.0008	-0.0008	-0.0009	-0.0014	-0.0003
Weeks to Dropout	*202*	-4.2569	-3.1362	0.0654	-0.8017	-0.9877	0.0359
Weeks to Short Degree	*227*	-3.6538	-3.3165	-0.5460	-0.5548	-1.1590	-0.7603
Weeks to Long Degree	*279*	-0.4263	-0.3665	-0.2292	0.0888	0.2649	1.1192
Debt at Exit:	*155,741*	-108,819	-48,430	-15,381	7,433	12,556	22,794
Dropout	*134,615*	-95,813	-42,724	-13,335	6,532	11,250	20,339
Short Degree	*153,705*	-105,385	-44,467	-13,830	6,560	11,163	20,701
Long Degree	*191,069*	-132,679	-61,001	-20,506	10,624	17,418	30,077
Income at t=11	*253,723*	-2,539	-1,543	-4	-205	-429	-396
Discounted Utility:							
Ex ante, at t=0 (%)	-	-0.0014	-0.0065	-0.0003	0.0002	0.0000	-0.0001
Ex post, at t=11 (%)	-	0.1250	-0.0975	0.0970	-0.1121	-0.1958	-0.2438
Cost:							
per Student	*124,068*	-88,465	-41,875	-14,028	6,936	11,750	21,740
Total (%)	-	-73.5923	-36.2130	-10.5645	4.5058	7.9606	15.1878
Means Testing:	0.75	0.0001	0.25	0.5	1	1.25	2
Grant Share:	27.8%	27.8%	27.8%	27.8%	27.8%	27.8%	27.8%
Loan Repayment:	IC4	IC4	IC4	IC4	IC4	IC4	IC4

Differences from the baseline student choices and outcomes, percentage differences from the baseline for total cost and discounted utility. The baseline is the pre-reform year 2001, the values for the baseline are in levels. The maximum aid amount is 64,232 SEK. All amounts are in real 2000 SEK (the exchange rate on 12/31/2000 was 9.3955 SEK/USD and 8.8263 SEK/EUR). All other policy instruments are kept at the baseline level. Means-testing shown as percentage of the *prisbasbelopp*.

a threshold very close to zero—decreases college enrollment by 2.1pp, increases dropout rates by 2.8pp, decreases short graduation rates by 3.4pp, and increases long graduation rates by 0.6pp. Students also work less: while year-round work increases by 3pp, summer work decreases by 8pp. Working during the summer has a large cost in terms of zero student aid and lower yearly income, so students prefer to either work year-round or not work at all. Thus, not allowing any amount of work when receiving study aid would dramatically increase dropout rates and increase inequality.

Overall, stricter means-testing reduces both student debt and the direct cost of study aid for the government. The policy simulations in Table 3 show that an intermediate amount of means-testing (between 0.5 and 0.75) on student income balances the incentives to work and accumulate academic capital as graduation rates and overall human capital is the highest when the means-testing is close to the pre-reform level.[45] These policy responses are largest for those who acquire more academic capital, which means that income inequality is lowest at this intermediate level of means-testing.

5.4 Policy Interactions

In this section, we highlight some important policy interactions. We analyze interactions between repayment plans and the grant share, and between repayment plans and means-testing.

5.4.1 Repayment Plans and Grant Share

Figure 6 displays the change in choices and outcomes relative to the baseline IC4 repayment plan with a 3.1% interest rate shown in Table 2. We evaluate six counterfactual repayment plans: a 15-year annuity, a 15-year annuity with a higher interest rate of 5%, a 25-year annuity, a 25-year annuity with a higher interest rate of 5%, a IC10 repayment plan, and a IC10 repayment plan with a higher interest rate of 5%. We refer to the repayment plans with the baseline 3.1% interest rate as *subsidized* and those with the higher 5% interest rate as *unsubsidized*.

If there were no policy interactions between the grant share and repayment plans, then all lines in Figure 6 would be horizontal. This is clearly not the case. The figure shows that differences in policy responses are generally higher when the grant share is lower. For example, Figure 6(c) shows that when the grant share is 50%, dropout rates are 0.3pp higher with a IC10 repayment plan than with a IC4 plan; however, when the grant share is 5%, they are 1.2pp higher. Similarly, when the grant share is 50%, dropout rates are 1.2pp higher with a 15-year annuity plan than with a IC4 plan; however, when the grant share is 5%, they are 3.3pp higher. Thus, dropout rates are 3-4 times more responsive to changing the repayment plan when the grant share is 5% rather than 50% of total available study aid. Most of the additional dropouts would alternatively receive short college degrees (Figure ??). Students shorten college enrollment spells (Figures 6(f), (g), and (h)), switch from summer work only to year-round work (Figures 6(i) and (j)), and from full loan take-up to no or partial loan take-up (Figures 6(k) and (l)).

[45] This intermediate amount of means-testing is slightly lower and closer to 0.5 for those with low high school GPA and non-STEM high school degrees, $(A, K) = (0, 0)$, as shown in Table *E.9*.

Consequently, student debt at exit is also lower—5000 SEK lower with a IC10 plan and 15,000 SEK lower with a 15-year annuity plan when the grant share is 5% (Figure 6(m)).

The responses are heterogeneous by initial ability and skill (Figure *E.1* and Figure *E.2*) and by unobserved type (Figure *E.3* and Figure *E.4*). Policy interactions are important for all four subgroups, but the magnitudes differ. Dropout rates, short graduation rates, and loan take-up choices are more responsive for those with low ability, skill, and motivation. Long graduation rates and work behavior are more responsive for those with high ability, skill, and motivation.

Subsidizing Student Loans Subsidizing student loans more—by lowering interest rates—makes students fund more of their college costs through debt (Table *E.4* in Appendix E). Changing the interest rate does not affect student outcomes and choices much when the repayment is income-contingent because the differential human capital accumulation incentives by earnings capacity pull in opposite directions. When the repayment plan is annuity-based, however, a 2pp lower interest rate implies a decrease in the dropout rate of 0.6pp, an increase in the short graduation rate of 0.7 percentage point, and a decrease in the long graduation rate of 0.2pp at the baseline grant share. The public cost of increasing the subsidy by reducing the interest rate by 2pp is 7-8,000 SEK per student for income-contingent loans and 19-21,000 SEK per student for annuity-based loans. This means that providing a subsidized annuity-based plan is worthwhile for the government if increasing graduation rates by 1 percentage point is socially worth more than 19-21,000 SEK per student; i.e. about 30% of the total cost of study aid.

Figure 6 corroborates that all these effects are even more pronounced when the grant share is lower. For example, when the grant share is 5% dropout rates are 0.36pp higher with an unsubsidized rather than a subsidized IC10 plan, while they are only 0.13pp higher when the grant share is 50%. Similarly, when the grant share is 5% dropout rates are 0.66pp higher with an unsubsidized rather than a subsidized 15-year annuity plan, while they are 0.40pp higher when the grant share is 50%. This implies that subsidizing annuity-based loans can be 3 times more effective in changing academic capital than subsidizing income-contingent loans.

Finally, Figures 6(n) and (o) provide an estimate of how costly each policy is. If we compare the situation with equal amounts of grants and loans available (50% grant share) to a situation with few available grants (5% grant share). Figure 6(n) shows that the discounted public cost per student is 14,000 SEK lower with an unsubsidized rather than a subsidized IC10 plan and 4,000 SEK lower with an unsubsidized rather than a subsidized 15-year annuity plan when the grant share is 50%, while it is 24,000 SEK lower with an unsubsidized rather than a subsidized IC10 plan and 12,000 SEK lower with an unsubsidized rather than a subsidized 15-year annuity plan when the grant share is 5%. Overall, this indicates that it is much more cost-effective to subsidize annuity-based loans rather than income contingent loans.

In summary, student responses to an increase in grant generosity is stronger when the alternative borrowing opportunity is more costly. Increasing grants effectively increases academic achievement when the alternative is an unsubsidized loan with a short repayment schedule; i.e. high repayment installments. However, the same increase in grants does not have a sizable effect on academic achievement when the alternative is a subsidized loan. This means that a subsidized loan is a good substitute for grants, ensuring both a similar level of academic achievement and a much lower public cost.

5.4.2 Repayment Plans and Means-Testing

Figure 7 investigates how the impact of changing the repayment plan interacts with means-testing. The figures display the change in choices and outcomes relative to the baseline IC4 repayment plan with a 3.1% interest rate shown in Table 3. The horizontal axis displays the strength of the means-testing: When means-testing approaches zero, students can either work or be eligible for financial aid, while the trade-off between student income and aid eligibility becomes weaker as means-testing approaches 2. Figure 7 confirms what we show in Section 5.3: Work incentives strongly change around the baseline means-testing of 0.75. On the one hand, when means-testing is below 0.75, there are strong policy interaction effects on academic capital accumulation. Tougher repayment plans shift the financing of college costs from debt to labor income. This implies higher dropout rates and fewer short degrees. It also implies a small increase in long degrees for the two toughest 15-year annuity plans for students with low ability and skills, as their work margin is less responsive. On the other hand, when means-testing is loose (i.e. above 1), there are no strong policy interaction effects on student debt and human capital accumulation—even if work and loan take-up choices change. As in section 5.4.1, Figure 7 shows that the IC10 and the 25-year annuity plans have very similar effects for the baseline subsidy, while increasing the subsidy on the loans has stronger effects if the loan has an annuity repayment plan.

In summary, the policy interaction effects on academic capital accumulation become stronger when it becomes more costly for students to work and maintain aid eligibility. When this trade-off is stronger and borrowing becomes more costly, students switch from borrowing to working year-round and become more likely to drop out of college. Student responses to a change in how much they are allowed to work without affecting their aid is stronger when the alternative funding opportunity is more costly.

5.5 Social Objectives: Not One Policy fits all Objectives

There are several declared social objectives of aid policies. In this section, we discuss which policies most effectively meet each goal. We also discuss the equity of each of these policies, since there are multiple trade-offs to consider.

Assume we simply want to maximize college enrollment, minimize dropout rates, maximize short degrees, minimize debt, maximize human capital (summarized by earnings capacity at $t = 11$ when everyone has exited college and is working full-time) and discounted utility after college exit. This can be achieved by providing all aid as grants, but this is also the policy with the highest public cost. The reason for this increase in public cost is that this "all grants" policy minimizes student year-round work and full loan take-up such that minimal cost is born privately and maximal cost is born publicly. For example, Table 2 shows that with a grant share of 95% we increase earnings capacity by 0.7% and discounted utility by 1.8%, but the total public cost increases by 96.7% compared to the baseline. That is, a discounted public cost percentage change of 53.7 per private util percentage change. This "all grants" policy is also regressive and increases inequality as it benefits students with high initial endowments more than those with low endowments *and* more of the public cost increase is going towards inframarginal students with high initial endowments.[46]

Conversely, assume we simply want to minimize public cost. This is achieved by making alternative income and borrowing opportunities more costly. In the set of policies we evaluate, this is exemplified by (i) the strictest means-testing for which the trade-off between working and receiving aid is so strong that students are not eligible for aid if they work and (ii) the unsubsidized 15y annuity loan for which repayments are the most front-loaded and independent of earnings capacity.

[46]For example, Tables *E.7* and *E.8* with a grant share of 95% show that the increase in discounted utility after college exit is 3.1% for the high motivation type, 2.7% for those with high ability and skill, 1.6% for those with high ability and skill, and 1.4% for the low motivation type.

It is worth to note that the 15y annuity plan also maximizes long degree acquisition for a given grant share, but it does so by minimizing enrollment rates and short degree acquisition while maximizing dropout rates. These effects are particularly strong for the low motivation type. Short program enrollment drops the most for those with low motivation, ability, and skill. Long program enrollment drops the most for those with low motivation, but high ability and skill. Overall, uniformly front-loading repayment the most for everyone minimizes human capital, debt, and discounted utility. This effect is strongest when (a) the grant share approaches zero and all aid is provided as a loan and (b) the work-aid trade-off is at its optimal level around 0.75. At a 5% grant share, the total public cost of the unsubsidized 15y annuity is SEK 2.5 billions lower than the IC4 plan, but discounted utility is also 2.5 lower as both debt at exit is almost SEK $20,000$ lower and earnings capacity 11 years after initial enrollment is SEK $3,000$ lower. This "tough loan" policy is progressive and reduces inequality as it is less unfavorable for students with low initial endowments than those with high endowments.[47]

This illustrates how a singular focus on one social objective may have unintended consequences with respect to other potentially desirable social objectives. How can we change implicit aid incentives though other policy instruments in order to increase overall human capital and balance the public aid cost?

First, we compare how we can achieve a similar effect on human capital by only changing one of the policy instruments at the time. Moving from the baseline policy with the IC4 repayment, a 27.8% grant share, and a means-testing of 0.75 we get a similarly small reduction in earnings capacity at $t = 11$ by (i) changing the repayment plan to IC10, (ii) reducing the grant share to 5% such that most aid is provided as a subsidized loan, or (iii) loosening the means-testing to 2. The first two policies have a similar public cost, but the last one is more costly. As a matter of fact, the policies that loosen means-testing above 0.75 are always strictly dominated as they are a costlier way to increase human capital overall. If we compare the two policies with similar public cost reductions, then (i) increasing income-contingency in the repayment plan—from a 4% to a 10% proportional income tax after college—lowers debt as it becomes more costly, but it comes at the cost of higher dropout rates and lower graduation rates. Conversely, (ii) lowers the grant share by almost 23pp and implies higher debt, but also higher graduation rates and lower dropout rates. If there are positive externalities of more education that are not captured in individual income, then it would be socially desirable to choose policy (ii) with less generous grants and a subsidized

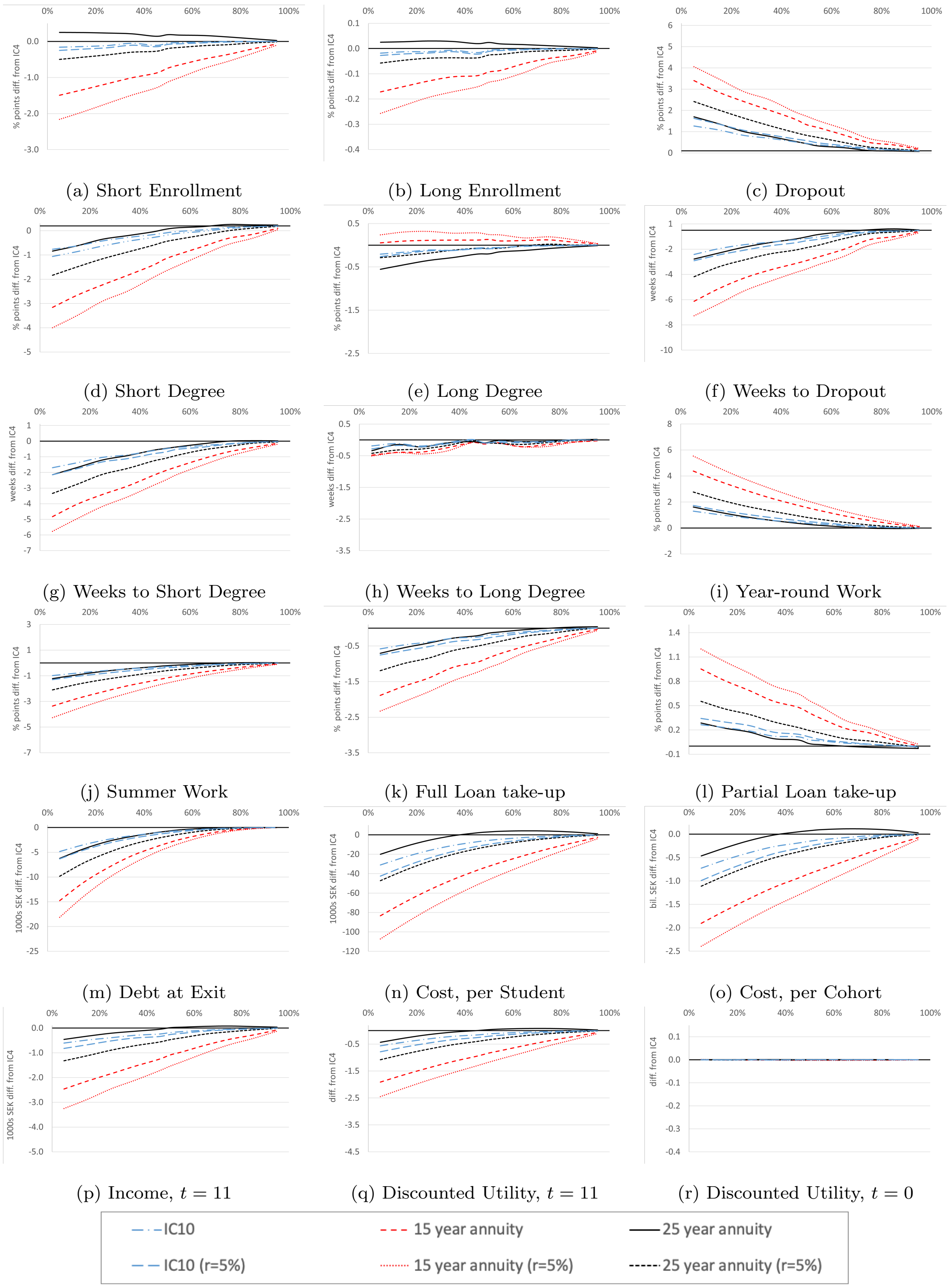
[47]Figures *E.1-E.4* (m), (p), and (q) show these numbers by initial endowments.

IC4 loan. However, this may have unintended consequences for inequality as Tables *E.5* and *E.6* reveal that (i) reduces income inequality, while Tables *E.7* and *E.8* reveal that (ii) increases income inequality.[48] This illustrates the trade-offs between front-loading income-contingent repayments and providing relatively generous grants *versus* keeping the income-contingency weak and providing less generous grants, while holding overall earnings capacity and public cost fixed. Which policy is preferred depends on social preferences over equity and efficiency.

Finally, we ask whether we can do even better with an annuity based loan that conditions on typical wage growth rather than individual income? The answer depends on the grant share. For example, if we compare the post- and pre-reform loans. The IC4 is preferable at lower grant shares, while the 25y annuity is preferable when the grant share is higher than $40-50\%$ and most of aid is provided as a grant. The reason for this non-linear effect is that the 25y annuity has larger enrollment effects. This implies that it also has larger dropout rates, since the marginally enrolled students are more likely to drop out than the inframarginal ones. Students stay enrolled shorter, work more as the cost of borrowing changes differentially for those with high and low endowments. All these effects are largest at low grant share.

[48]These policy conclusions are largely in line with reduced form estimates of the UK reforms following the Higher Education Act 2004 that replaced zero tuition fees with high tuition fees combined with generous means-tested financial aid to also cover living costs (Murphy et al., 2019; Azmat and Simion, 2021). In our model, this reform is similar to lowering the grant share and offering IC loans—although means-testing in the UK is on parental income rather than student income.

Figure 6: Repayment Plans by Grant Share.



(a) Short Enrollment (b) Long Enrollment (c) Dropout

(d) Short Degree (e) Long Degree (f) Weeks to Dropout

(g) Weeks to Short Degree (h) Weeks to Long Degree (i) Year-round Work

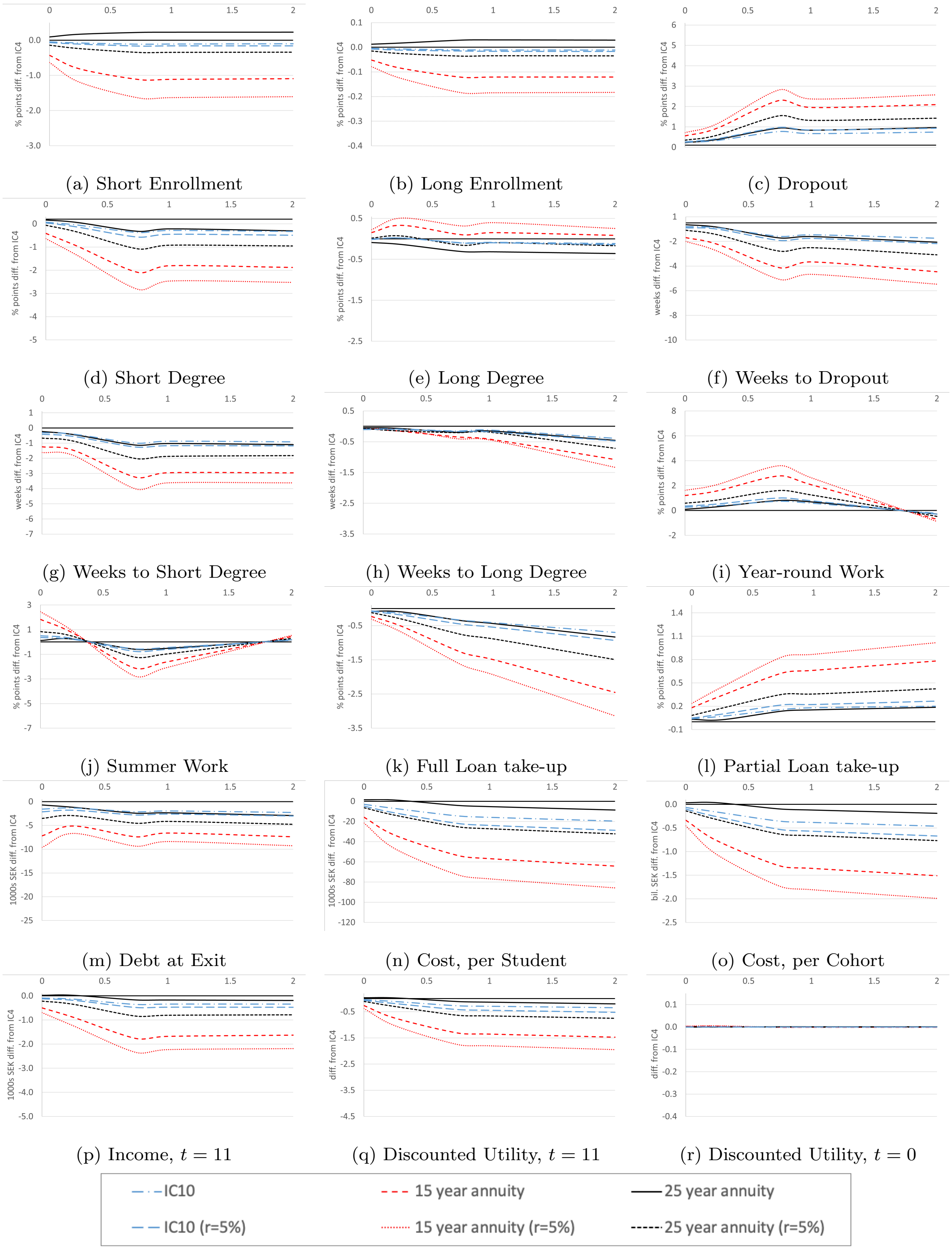
(j) Summer Work (k) Full Loan take-up (l) Partial Loan take-up

(m) Debt at Exit (n) Cost, per Student (o) Cost, per Cohort

(p) Income, $t = 11$ (q) Discounted Utility, $t = 11$ (r) Discounted Utility, $t = 0$

The policy impact of changing the repayment plan relative to the baseline IC4 repayment plan with a 3.1% interest rate (y-axis) as a function of the grant share (x-axis). We evaluate six counterfactual repayment plans: a 15-year annuity, a 15-year annuity with a higher interest rate of 5%, a 25-year annuity, a 25-year annuity with a higher interest rate of 5%, a IC10 repayment plan, and a IC10 repayment plan with a higher interest rate of 5%.

Figure 7: Repayment Plans by Means Testing.



(a) Short Enrollment (b) Long Enrollment (c) Dropout

(d) Short Degree (e) Long Degree (f) Weeks to Dropout

(g) Weeks to Short Degree (h) Weeks to Long Degree (i) Year-round Work

(j) Summer Work (k) Full Loan take-up (l) Partial Loan take-up

(m) Debt at Exit (n) Cost, per Student (o) Cost, per Cohort

(p) Income, $t = 11$ (q) Discounted Utility, $t = 11$ (r) Discounted Utility, $t = 0$

The policy impact of changing the repayment plan relative to the baseline IC4 repayment plan with a 3.1% interest rate (y-axis) as a function of means-testing (x-axis). We evaluate six counterfactual repayment plans: a 15-year annuity, a 15-year annuity with a higher interest rate of 5%, a 25-year annuity, a 25-year annuity with a higher interest rate of 5%, a IC10 repayment plan, and a IC10 repayment plan with a higher interest rate of 5%.

6 Conclusion

The design of financial aid to students affects the incentives to study and work during college, and the budget set both during and after college. In this paper, we study the behavioral and economic effects of study aid policies. We model students' choices of enrollment, work, and student loan take-up in a structural dynamic model with observed and unobserved heterogeneity. Importantly, we model both students' human capital accumulation and actual funding opportunities much more accurately than in the previous literature *and* relate them to behavior during college enrollment and longer term labor market outcomes. This enables us to quantify how the impact of aid on student behavior depends on the amount of grant available, the type of loan offered, and eligibility requirements.

We find that it is pivotal to have good estimates of how many students are at the relevant margins of choice, how strongly they respond to financial incentives, and how their outcomes are affected. We also find that interactions between policy instruments are important to consider; in general, student responses to changes in study aid design are stronger when the alternative funding opportunities are more costly. This suggests that existing estimates in the literature that only focus on one policy component and fail to account for alternative funding channels may— even if unbiased—be specific to the particular population or environment studied. The general point that both the population and the environment matter for estimating effects of a policy or program could carry over to other situations; e.g. when estimating the effects of social security programs.

We highlight that it is important to consider both short- and long-term effects of study aid policies. These policies affect both student debt and human capital accumulation during college, thus we should be cautious with policy conclusions based on models that treat human capital and debt at college exit as exogenously given.

Our results are a step towards a better understanding of the mechanisms through which student aid policies drive student debt and human capital accumulation. More can be done starting from the empirical framework in this paper; e.g. investigating the role of parental transfers, the dynamics of major choice, and heterogeneity along various skill dimensions and initial conditions. There could also be some fruitful extensions to study the effects of study aid design on post-college financial and labor market choices over the life-cycle such that the "micro" and "macro" literature became even more integrated. Finally, our empirical results are relevant for the design of optimal tax and human capital policies that balance incentives and insurance. The potential benefits of IC loans have been analyzed theoretically (Stantcheva, 2015; Findeisen and Sachs, 2016), but the trade-offs in human capital investments—both in terms of education and experience—are yet to be taken to the data. Blundell et al. (2016) take a step in this direction in their analysis of the impacts of welfare policy on female labor supply and human capital accumulation. Our model can also be used to analyze welfare policies and credit access more generally, since the means-testing embodies similar incentives to invest in human capital as an earned income tax credit (EITC) and the question of IC versus annuity loans is applicable to many situations with risky investments; e.g. starting or expanding a business.

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