For whom the constraint binds?
Behavioural Responses to the Removal of Income Limits for Study Aid in Sweden under COVID-19

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Abstract: Sweden provides generous financial support for students in higher education, and disintentivises excessive part-time work by reducing support received if income exceeds a certain threshold. This threshold was temporarily removed during the COVID-19 pandemic, with the intent to support students' finances through the crisis. I provide the first examination of the effects of this reform using individual-level administrative data on the entire Swedish student population, and combine three research designs: bunching, simulated instrumental variables, and differences-in-differences. Consistent with theoretical predictions, I observe significant behavioural responses. Students increase their incomes by 10-12% on average, driven especially by high-earners and partially by individuals switching between student and non-student status during the pandemic. These results help inform the design of study aid systems balancing academic achievement with part-time work, as well as policies aiming to support students through economic shocks.

Note on writing sample

This is a 4500-word extract of my Extended Essay, submitted as part of my MSc Economics at the London School of Economics in May 2024, and which received the grade 90% (best in year). The original submitted version comprised 6537 words and can be found in full here, containing a more rigorous explanation of the framework and a deeper analysis of the results. I am currently working with my supervisor (David Seim) to extend this into a full research paper for publication.

Contents

1 Introduction

I analyse a reform to Sweden's system of financial support for students, which provides generous and near-universal grants and loans to students in free-of-charge higher education. A significant share of Swedish students work part-time to support their studies, but those receiving support are incentivised to not excessively work by a semiannual income threshold, the *fribelopp*. In 2020, the fribelopp was removed in order to support students through the COVID-19 pandemic, such that students were no longer limited in the amount they could work. Standard economic theory predicts that students would respond to this by increasing their earnings, but whether this actually occurred is crucial in assessing whether the current system and the 2020 reform fulfill their objectives. If the fribelopp is not a strongly binding constraint on students' choices, then removing it is unlikely to provide an effective buffer to finances in times of need.

I provide the first analysis of this reform using individual-level data, and attempt to causally estimate its effects on students using a combination of three research designs: firstly, I apply a standard bunching analysis of the threshold to investigate the presence of a behavioural response (Chetty et al. (2011)); secondly, I adapt a Simulated Instrumental Variables (SIV) approach common to the analysis of tax reforms (Gruber and Saez (2002)), to attempt to more precisely quantify these behavioural responses; thirdly, I utilise comparable non-students as a control group, and estimate Differences-in-Differences (DID) and Changes-in-Changes (CIC) models to better disentangle the causal effects of the reform from those of COVID-19 and investigate heterogeneity in the behavioural response along the income distribution (Athey and G. Imbens (2006)).

Overall, I find strong evidence of a large positive behavioural response to the 2020 reform. I observe substantial bunching at the fribelopp which disappears entirely after the threshold is slashed. My SIV analysis suggests that this strong positive response is to some degree driven by individuals who became or stopped being students after the reform was enacted, with the sign of the estimated effect switching when these individuals are excluded. Finally, I conclude from my DID that the reform increased students' average income by 2,700-3,700 SEK (+10-12%), and from my CIC results that the effect on top earners was larger, at 15,000 SEK (+7.5%).

2 Literature Overview

I contribute to two strands of literature: firstly, to the estimation of behavioural responses to taxation, here in the specific context of students in crisis times; secondly, to work examining how the design of financial support systems for students affect the incentives to study and work.

Many countries around the world have developed public systems of financial support for students to facilitate access to higher education. However, such systems are costly: beyond the costs of providing the education itself, students must also be able to afford living expenses for several years. It may be fiscally unrealistic for governments to cover these expenses entirely or undesirable if the gains from such a policy are perceived to be too regressive. Publicly funded support schemes generally only cover part of students' expenses, with the remaining amount intended to be covered by loans, family transfers, or students' own labour income.

Working while studying provides several potential benefits for students, in addition to the direct benefits of increased disposable income: experience can facilitate the post-graduation transition into the labour market, increase future earnings, and improve the quality of job matches (Joensen (2009); Joensen and Mattana (2022)). Existing research suggests significant short-term gains to earnings which attenuate in later years (Häkkinen (2006); Joensen and Mattana (2022)). However, working too much can also adversely affect academic achievement: it is commonly found that students working more than roughly 15-20 hours a week while simultaneously engaged in full-time studies face longer times to degree completion, lower grades, and higher drop-out rates (Montmarquette, Viennot-Briot, and Dagenais (2007); Darolia (2014); Avdic and Gartell (2015); Hovdhaugen (2015); Staal (2021)). These negative effects may also disproportionately affect and harm more disadvantaged students, preventing them from fully benefiting from the returns to education (Darolia (2014); Avdic and Gartell (2015); Gensowski et al. (2020)). The design of a financial support system where students work to support themselves must incentivise academic achievement without compromising the economic stability required.

These considerations highlight that different sources of support are not fungible: working is time-consuming and hinders academic achievement, but may improve labour market outcomes; loans do not necessitate as significant a time investment, but can lead to large and durable debt post-graduation (Rösth and Stolt (2020); Petersen (2022); Steenbjerg Kristensen and Quitzau (2023)); grants are most generous towards students, but also the most fiscally expensive option.

3 Background

3.1 Institutional Background

The Swedish Central Study Support Board ("Centrala studiestödsnämnden", CSN) provides generous and near-universal financial support for students in higher education for a period equivalent to 6 years, primarily in the form of non-repayable subsidies and repayable loans. However, the support students are eligible for is reduced if their total income in a given half year is above a certain threshold, the *"fribelopp"* (free amount). For every 1 SEK above this threshold, the support is reduced by 0.61 SEK. This can be interpreted as a de-facto marginal tax rate of 61%, creating a significant convex kink in students' budget set, as shown in Figure 1.

Students themselves choose the amount of support they receive throughout their studies, with several margins of adjustment. For instance, students choose whether to take out a loan in addition to the subsidy, and choose the number of weeks per year where support is received. CSN explicitly advises students who are uncertain about their future income to choose fewer support weeks and/or a lower support intensity, in order to have a more generous fribelopp and avoid accidentally exceeding it. Students are also encouraged to regularly update their predicted income throughout the year.

Following the declaration of a national emergency in March 2020 during the COVID-19 pandemic, the fribelopp was abolished, such that students' income would no longer limit the amount of financial support they were eligible for. The objective of this policy was twofold: firstly, to avoid punishing students pushed into work by the pandemic, whether due to necessity or professional responsibilities; secondly, to avoid punishing workers laid off as a result of the pandemic wishing to take up studies. The policy ended up lasting until June 2022, such that the fribelopp was slashed for both half years of 2020 and 2021, and the first half year of 2022.

3.2 Data

My analysis is based on administrative data covering the entire Swedish population, spanning the period 2002-2021. Using the population-wide LISA dataset (*LISA* 2024), I observe students' half-yearly income, total financial support, and several other socioeconomic characteristics. I impute students' fribelopp using a highly precise matching algorithm: 99% of my matches are within 5% of the observed amount, and 60% are within 1%, equivalent to 10 SEK.

I observe 3,079,597 student-year pairs and 7,632,471 non-student-year pairs over the main analysis period 2017-2021[1]. Descriptive statistics are presented in Tables 1 and 2. One noteworthy feature is that students earn 56% more in the summer half-year on average, while non-students' incomes are evenly spread, highlighting prevalent part-time work in the summer.

	Full Sample			Restricted, 2019-2021		
	mean	min	max	mean	min	max
Age	21.15	15.00	35.00	20.93	18.00	25.00
High School (Share)	0.86	0.00	1.00	1.00	1.00	1.00
University (Share)	0.49	0.00	1.00	0.58	0.00	1.00
Half-Year 1 Inc (1000 SEK)	30.38	0.00	3803.27	34.29	0.00	299.97
Half-Year 2 Inc (1000 SEK)	47.90	0.00	3778.35	62.92	0.00	299.97
Studiemedel (1000 SEK)	43.51	0.10	873.30	55.57	0.10	200.00
Years with studiemedel	4.92	1.00	12.00	5.53	1.00	10.00
Total years with studiemedel	6.69	1.00	12.00	6.44	1.00	12.00
Observations	3079597			883662		

Table 1: Descriptive Statistics, Students

	Full Sample			Restricted, 2019-2021		
	mean	min	max	mean	min	max
Age	28.00	15.00	35.00	22.32	18.00	25.00
High School (Share)	0.91	0.00	1.00	1.00	1.00	1.00
University (Share)	0.42	0.00	1.00	0.22	0.00	1.00
Half-Year 1 Inc (1000 SEK)	131.82	0.00	13763.95	118.30	0.00	499.96
Half-Year 2 Inc (1000 SEK)	134.57	0.00	31042.92	126.11	0.00	499.97
Studiemedel (1000 SEK)	0.00	0.00	0.00	0.00	0.00	0.00
Years with studiemedel	7.96	1.00	12.00	6.56	1.00	10.00
Total years with studiemedel	9.62	1.00	12.00	7.59	1.00	12.00
Observations	7632471			1078996		

Table 2: Descriptive Statistics, Non-Students

[1]I focus my analysis on individuals aged 16-25. I drop individuals whom I observe as receiving financial support for more than 10 years. I also drop individuals earning more than 300,000 SEK in a given half year. My restricted sample thus covers 883,662 students and 1,078,996 non-students. For most of my analysis, I further drop individuals who switch between student and non-student status in 2020 or 2021, to avoid bias from compositional change. This leaves 419,977 students and 1,078,996 non-students across 2017-2021 after all sample restrictions

4 Methodology

4.1 Bunching

The reduction in support received resulting from students' incomes exceeding the fribelopp can be interpreted as a de-facto marginal income tax. When students exceed the fribelopp, their MNOTR jumps by +0.61 p.p.. This creates a significant convex kink in their budget constraint, as illustrated in Figure 1. Following standard arguments, we would expect significant bunching at the fribelopp (point A) if students optimally choose their income and non-working hours (Saez, 2010; Chetty et al., 2011). The 2020 reform eliminated the kink in the budget constraint. Utility-maximising students who previously located at the kink should then relocate to point B, with lower non-work time and higher income.

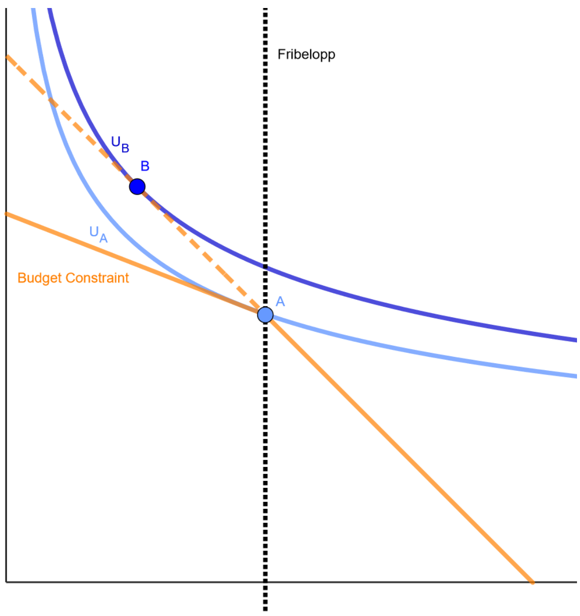


Figure 1: Kinked Budget Set

This provides us with two theoretical predictions as to what we should observe in the data. We expect significant bunching at the fribelopp, but only prior to 2020. Following 2020, we expect individuals who previously located near the threshold to increase their income. I investigate both of these hypotheses in Section 5.

To formalise these hypotheses, assume students maximise a quasi-linear utility function, subject to a potentially non-linear budget set (H. Kleven, 2016):

$$\max_{z_{it}} \quad c_{it} - \frac{n_{it}}{1+\frac{1}{\varepsilon}} \left(\frac{z_{it}}{n_{it}} \right)^{1+\frac{1}{\varepsilon}} \quad \text{s.t.} \;\; c_{it} \leq z_{it} - T_t(z_{it}, n_{it}) \tag{1}$$

Where T_t is the tax levied on an individual with income z_{it} and ability n_{it}, and $\tau_{it} = 1 - T_t'(z_{it}, x_{it})$ is the individual marginal net-of-tax rate. The degree of bunching is informative of the elasticity of taxable income (ETI) near the threshold, defined as: $\varepsilon = \frac{dz}{d(1-\tau)} \frac{1-\tau}{z}$.

First order conditions dictate that $z_{it} = n_{it}\tau_{it}^{\varepsilon}$. In the presence of the kink, optimal earnings satisfy $z^* = (n^* + \Delta n^*)(\tau - \Delta t)^{\varepsilon}$. In the counterfactual absence of the kink, optimal earnings satisfy $z^* + \Delta z^* = (n^* + \Delta n^*)\tau^{\varepsilon}$. Based on this, the predicted excess mass at the threshold is $B = \int_{z^*}^{z^*+\Delta z} h(z)\, dz \approx \Delta z^* h(z^*)$, and the ETI can be calculated by estimating:

$$\varepsilon \approx \frac{B}{h_0(z^*)z^*} \frac{1-\tau}{\Delta t} \tag{2}$$

Where $h_0(z^*)$ is the density of income, $1 - \tau$ is the MNOTR, and Δt is the change in the MNOTR, all of which are observed at the threshold. B is estimated by fitting a smooth high-order polynomial to the income distribution and calculating the excess mass.

Equivalently, as shown by Saez (2010), the ETI can be estimated by:

$$\varepsilon = \frac{\ln(z^* + dz^*) - \ln(z^*)}{\ln(1 - t_1) - \ln(1 - t_2)} \tag{3}$$

Where t_1 is the pre-kink MTR and t_2 is the post-kink MTR.

The amount of bunching and the ETI are positively correlated: if individuals are able and willing to precisely control their income near the threshold, they will be highly responsive to changes in the MNOTR, and will choose to bunch just below it. It is difficult to predict whether this is the case ex ante. Many students work only during the summer and/or part-time only during the year, positions in which employment and working hours are relatively flexible (Joensen (2009); Søgaard (2019); Joensen and Mattana (2021); Joensen and Mattana (2022)), suggesting elastic earnings and significant bunching. However, if the income necessary to sustain students' finances is close to the threshold, earnings would be inelastic for those who are able to stay in education, and we would observe insignificant bunching.

4.2 Simulated Instrumental Variables

Behavioural responses to this unexpected reform can also be exploited to estimate ε. The FOC for the utility maximisation problem presented in Equation 1 can be rewritten in log differences to yield a reduced-form formula containing the ETI, as seen in Equation 4.

However, a naive estimation of this will not estimate a causal effect: τ_{it} is determined by z_{it}, which is endogenously chosen by the individual in each period. One common solution (Gruber and Saez (2002); Auten and Carroll (1999); Henrik Jacobsen Kleven and E. A. Schultz (2014)) is to utilise $\Delta \ln \tau_{it}^p$, the period t change in NOTR *predicted* based on information available in period $t-k$, as a (simulated) instrument for the realised change, $\Delta \ln \tau_{it}$. When performing 2SLS estimation, equation 4 is the second stage, and the first stage is presented in Equation 5:

$$\Delta \ln z_{it} = \varepsilon \Delta \ln \tau_{it} + \Delta \ln n_{it} + u_{it} \quad (4)$$

$$\Delta \ln \tau_{it} = \pi \Delta \ln \tau_{it}^p + v_{it} \quad (5)$$

The identification assumptions necessary for this strategy to produce an unbiased estimate of ε is that predicted changes to the marginal NOTR are uncorrelated with changes to potential income n_{it} (Angrist, G. W. Imbens, and Rubin (1996); Weber (2014)). This is likely to generally fail in the setting of tax reforms for several reasons. Firstly, tax reforms often target the progressivity of the tax schedule, which implies that changes to an individual's MNOTR will generally be correlated with their pre-reform income. Secondly, income growth tends to be mean-reverting: individuals facing large temporary income shocks just before the reform are more likely to experience weaker subsequent income growth during the reform, but not as a result of it. (Gruber and Saez (2002); H. Kleven and E. Schultz (2014); Kopczuk (2005)). Thirdly, the secular income trend may differ between individuals who are less and more affected by the reform (Feldstein, 1993). Compositional change is also an issue in my setting: pandemic-related measures such as lockdowns may have caused workers to selectively reenter education, and/or caused students to preemptively enter the labour market[2].

I partially address these concerns by using 2021 rather than 2020 as my post-treatment period, in the hope that any heterogeneous effects of COVID-19 had stabilised by then. I drop "switchers", i.e., individuals who switch between student and non-student status[3] after 2019.

[2]Facilitating these transitions is in fact one of CSN's stated justifications for slashing the fribelopp.
[3]One motivation for this reform was precisely to facilitate these transitions during COVID-19.

4.3 Differences-in-Differences

To better disentangle the effect of removing the fribelopp from the effect of the COVID-19 shock, I employ comparable non-students as a control group in a classical DID setup.

An individual i belongs to group $G_i \in \{0,1\}$, where group 1 is the treatment group, and is observed in periods $T_i \in \{0,1\}$, where the treatment occurs in period 1. Denoting an individual's potential outcomes by $Y_{i,gt}$ and treatment with an indicator D_{it}, the realised outcome for individual i in period t is $Y_{it} = D_{it}Y_{i,1t} + (1 - D_{it})Y_{i,0t}$. The average treatment effect on the treated (ATT) is estimated by δ in:

$$Y_{it} = \gamma Post_{it} + \phi Treat_{it} + \delta Post_{it} \times Treat_{it} + \beta X_{it} + u_{it} \tag{6}$$

Where X_{it} is a vector of controls. δ identifies of the ATT under the usual parallel trends assumption, $E[Y_{i11} - Y_{i00}|D_{it}] = E[Y_{i11} - Y_{i00}]$, requiring that the difference in unobserved earnings potential between students and non-students be stable over time.

Visual results are presented in Figure 5, and estimation results in Table 3.

4.4 Changes in Changes

The DID estimator only identify average effects, but high-earning students are likely systematically different from low-earning individuals in both the treatment and control groups in terms of preferences and constraints. This suggests a high likelihood of heterogeneity in responses to the reform along the income distribution. To investigate this, I further estimate a Quantile Changes-in-Changes (QCIC) model in the style of Athey and G. Imbens (2006).

The logic is similar to the traditional DID estimator, with the key identification assumption being that $u_i \perp T|G$. This is effectively a stronger parallel trend assumption, stating that any difference between treated and control group is stable over time for a given income level.

Define $F_{Y_{gt}}(y) \equiv P(Y_{igt} < y|G_i = g, T_i = t)$, the cumulative distribution of Y, conditional on (G,T)[4]. The QCIC estimator of the quantile treatment effect (QTE) for a given quantile q is:

$$\tau_q^{CIC} \equiv F^{-1}_{Y_{11}}(q) - F^{-1}_{Y_{01}}\{F_{Y_{00}}[F^{-1}_{Y_{10}}(q)]\} \tag{7}$$

The key CIC results are presented in Figure 6 and Table 4.

[4]This is invertible under the assumption that Y_{gt} is strictly increasing in u_{gt}

5 Empirical Analysis

5.1 Bunching

Bunching thresholds are individual-specific, determined by the combination of support weeks and support intensity. I recenter all individuals' income around their estimated threshold to observe the degree to which there is bunching at the fribelopp in aggregate. The visual results are presented for each half year of 2019-2021 in Figure 2.

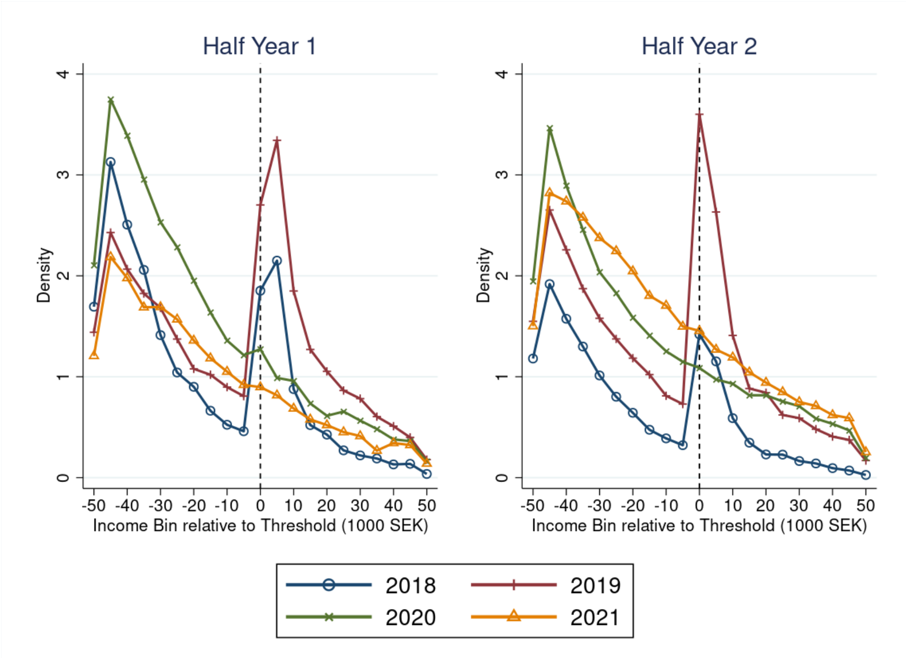


Figure 2: Aggregate Bunching at the Fribelopp

In the pre-reform years 2018 and 2019, there is clear evidence of bunching located precisely at and just beyond the the recentered fribelopp, indicating that the fribelopp acts as a salient threshold in students' decision-making. The estimated excess masses in 2019 when considering the restricted sample are of 5.063 and 4.678 in the first and second half year, respectively, implying an ETI in the range of 0.05 to 0.11. This is a relatively low ETI (although within range of my other findings), a surprising finding in a context where most students work only during the summer and/or part-time only during the year, positions in which employment and working hours are relatively flexible (Joensen (2009); Søgaard (2019); Joensen and Mattana (2021); Joensen and Mattana (2022)). This indicates that although the fribelopp is a salient threshold in students' work allocation, earnings near the threshold are relatively inelastic.

Post-reform, in 2020-2021, the distribution is smooth and no excess mass is detected: in each half year, the estimated excess mass is of roughy 0.10. This is exactly consistent with the predicted effects of removing the fribelopp, and indicates that students correctly integrated the effects of the 2020 reform into their labour supply decisions, consistent with the quick adaptations to reform observed by Søgaard (2019) for students facing a similar system in Denmark. This also suggests that CSN's communication was effective in conveying the new temporary rules.

Considering the amplitude of the kink in the budget constraint created by a fall in the MNOTR of 61 p.p., it is surprising that I observe students bunching on the *right* side of the threshold. This is likely partially caused by an imprecise estimation of the fribelopp: it may be that the excess mass is more smoothly distributed on either side of the threshold.

Another possible explanation is the presence of optimisation frictions impeding optimal decision-making such as misperception, adjustment costs, or imprecise control of income. Optimisation frictions have been found to significantly impact behaviour in response to tax schedules, as studied by Chetty et al. (2011), Chetty (2012) or Henrik J. Kleven and Waseem (2013). This would corroborate the findings of Søgaard (2019), who finds that inattention to total earnings lead to a utility cost equivalent to 2% of income among Danish students, who face a similar income threshold affecting eligibility for financial support.

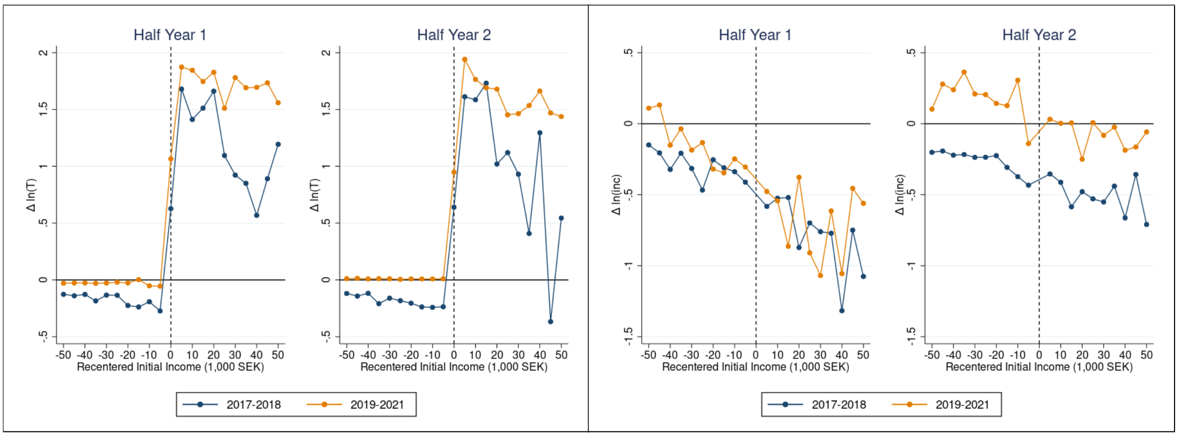
A third possible explanation is that individuals willingly exceed their income threshold in response to imprecise monitoring and enforcement by CSN. CSN verifies whether students exceeded their pre-reported income in a given year using tax data, two years after the fact. However, they claim to open an investigation only if the total *yearly* income exceeds its total reported amount, and assume until otherwise specified that yearly income is uniformly distributed throughout the year. If this information is correct and informs the implementation of enforcement, it would theoretically be possible for a student to exceed their fribelopp in a given *half-year*, but avoid repayment if their total yearly income does not exceed the total amount they reported. As a proper analysis would necessitate knowing the exact fribelopp and monthly incomes over more years, I do not further investigate this possibility.

Having established the qualitative salience of the fribelopp in determining students' labour supply decisions, I now attempt to estimate the effects of removing this income threshold.

5.2 Simulated Instrumental Variables

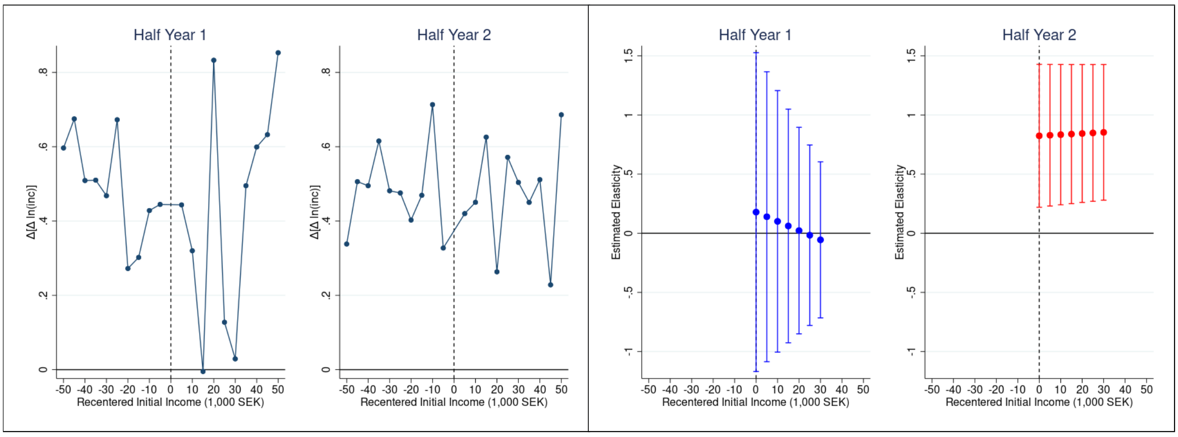
Throughout this section, let B_{it} be a vector with a dummy b_{ith} for each 5,000 SEK income bin h, ; D_{it} is a dummy denoting treatment, i.e., whether the reform has been enacted at time t. Validation is performed using a pre-reform period (2017-2018)[5] as placebo for the actual treatment period (2019-2021). The main results presented here are on the full sample, with results for the restricted sample presented in Appendix ??. I present results including switchers in Figure 3, without switchers in Figure 4.

Figures 3a and 4a display the average change in the MNOTR as a function of the initial income in each period.



(a) Change in the MNOTR

(b) Income Trend Differential



(c) Change in the Income Trend Differential

(d) Estimated Elasticity

Figure 3: Behavioural Responses in 2017-2018 and 2019-2021, With switchers

In each half year, individuals exceeding their fribelopp see a substantial increase in their MNOTR, to a stronger degree during the reform period. As income beyond the threshold increases, the change in the MNOTR moves closer to 0 once again. This is consistent with

[5] I use 2017-2018 rather than 2019 to avoid any issues arising from comparing years in which my measures of income are based on different data

three expected patterns: 1) Exceeding the fribelopp indeed leads to a significant change in the MNOTR, indicating that individuals do respond to it; 2) Mean-reversion: individuals who exceed the threshold and face a low MNOTR in the pre-reform period subsequently reduce their income and face a large growth in the MNOTR; 3) The 2020 reform affected individuals' choices: those who exceeded the threshold saw their MNOTR increase by more following the reform than in the pre-reform period. The only substantial qualitative difference across half years and with/without switchers is that the difference between the two periods as well as the decrease in the change in MNOTR are less clear when considering the first half-year in a sample excluding switchers.

Figures 3b and 4b display the average income growth as a function of initial income, for each period. This is estimated by running the following model separately for each period:

$$\Delta \ln z_{it} = \beta_{0t} + \beta_{1t} B^{inc}_{it-1} + \varepsilon_{it} \tag{8}$$

Income growth is decreasing in initial income, consistent with mean-reversion being an important aspect of the setting studied. When considering the sample including switchers, income growth in the first half year is broadly identical in the first half year, but higher across the entire distribution in the second half year. The same is true when excluding switchers, but only for the second half year. This indicates that COVID and/or the reform positively impacted income growth only in the second half year, and had little effect on the first half year. It also appears that this shock affected the entire distribution, as both control and treated individuals have higher income growth overall in the reform period than in the non-reform period.

Figures 3c and 4c display the change in the income trend differential as a function of initial income, estimated by running the following model separately for each period:

$$\Delta \ln z_{it} = \delta_0 + \delta_1 B^{inc}_{it-1} + \delta_2 D^{reform}_{it} + \delta_3 B^{inc}_{it-1} \times D^{reform}_{it} + \varepsilon_{it} \tag{9}$$

Validation of the parallel trends assumption hinges on this curve being close to 0 in the validation region. It is difficult to validate this assumption on account of both the large volatility in the change in income trend differential as well as it being significantly above 0 across the distribution, rather than only in the identification region. However, this does not necessarily indicate that the reform itself cannot be used for identification. The volatility can partially be explained by the fact that income growth in the pre-reform period is mismeasured. As a result of this, I interpret my elasticity estimates as proxies for the behavioural response to the combination of the COVID shock and the removal of the fribelopp, rather than as a true elasticity.

Finally, Figures 3d and 4d display identified income elasticities as a response to the reform across the income distribution estimated via 2SLS at different points of the distribution. For each 5000SEK bin in the identification region $h \in (0, 5000, 10000, 15000, 20000, 25000, 30000)$, the specification is:

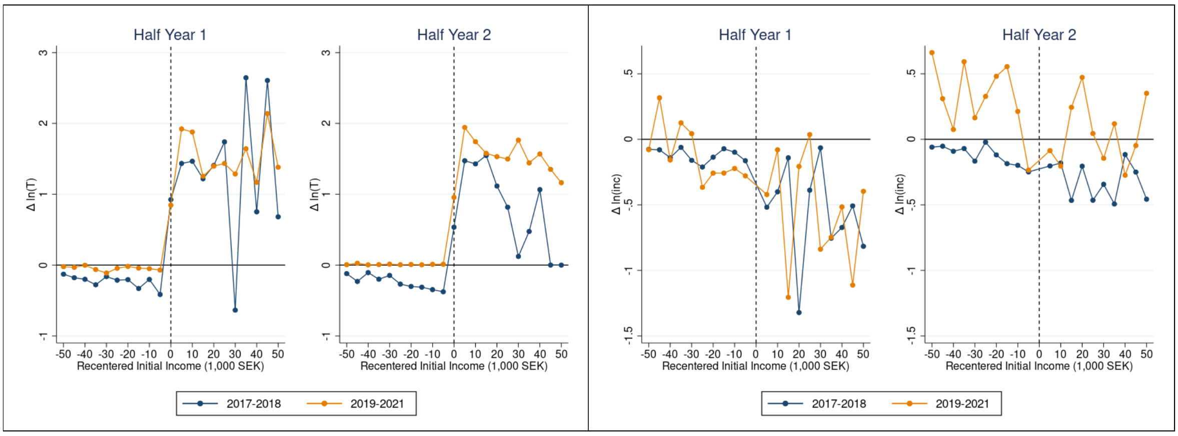
$$\Delta \ln z_{it} = \gamma_0 + \gamma_1 B_{it-1} + \gamma_2 D_{it}^{post} + \gamma_3 \Delta \ln \tau_{it} + \gamma_4 [\Delta \ln \tau_{it} \times (z_{it-1} - h)] + \varepsilon_{it} \quad (10)$$

To allow both the level and slope of the income elasticity to vary with income bin, I include the MNOTR by itself, $\Delta \ln \tau_{it}$, as well as interacted with initial income relative to the bin, $\Delta \ln \tau_{it}(z_{it-1} - h)$. Both are endogenous variables, and I instrument for them with the corresponding pre-reform predicted values $\Delta \ln \tau_{it}^p, \Delta \ln \tau_{it}^p(z_{it-1} - h)$. Due to sample size constraints, I assign identical weights to all treated and control individuals for my estimation. Results based on a local linear 2SLS estimation in the style of Jakobsen and Søgaard (2022) are shown in Appendix **??**, although the relatively small sample size makes these estimates highly unstable and unreliable.

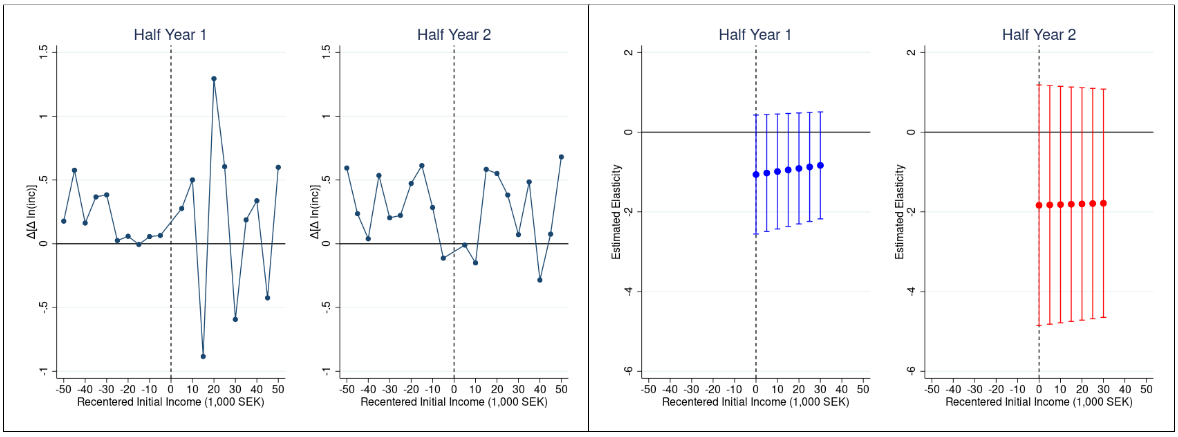
Overall, nearly all elasticity estimates are insignificant due to large standard errors. However, the fact that the point estimates are in each case stable, clearly signed, and with a clear pattern across income bins and specifications suggests that this insignificance may be driven by a small sample size rather than the absence of an effect.

When switchers are included (Figure 3d), the elasticity point estimates are positive, and significantly so for the second half-year at a stable and relatively large level of 0.84. For the first half year, the elasticity point estimate is small and positive (0.17) close to the threshold, consistently decreasing with income, and barely negative (-0.05) at 30.000 SEK above the threshold. Overall, these results indicate that students earning above their fribelopp prior to 2020 responded to the shock by increasing excess earnings in the second half year at least, consistent with the expected positive response to an increase in the MNOTR on this income.

Contrary to the previous validation steps, however, the results significantly change when



(a) Change in the MNOTR **(b)** Income Trend Differential



(c) Change in the Income Trend Differential **(d)** Estimated Elasticity

Figure 4: Behavioural Responses in 2017-2018 and 2019-2021, No switchers

switchers are dropped from consideration (Figure 4d). The elasticity point estimates are now consistently negative, of roughly -0.9 in the first half year and -1.8 in the second half year. This suggests that the positive effect on income growth for high earners is largely driven by individuals who became students after the 2020 shocks, and that the effect on pre-existing students was at best insignificant, or possibly negative.

5.3 Differences-in-Differences

Pre-trends for students and non-students are shown in Figure 5. Keeping in mind that the jump in 2019 is due to a change in measurement rather than a shock or policy change, the parallel trends assumption seems to hold prior to 2020. I estimate a simple DID model, controlling for age and its square, whether the individual has any children, any young children, how many years they have received studiemedel so far, and their highest completed educational level. The ATT estimates are presented in Table 3. Results are presented for both half-years individually, for treatment periods of both 2019-20 and 2019-21.

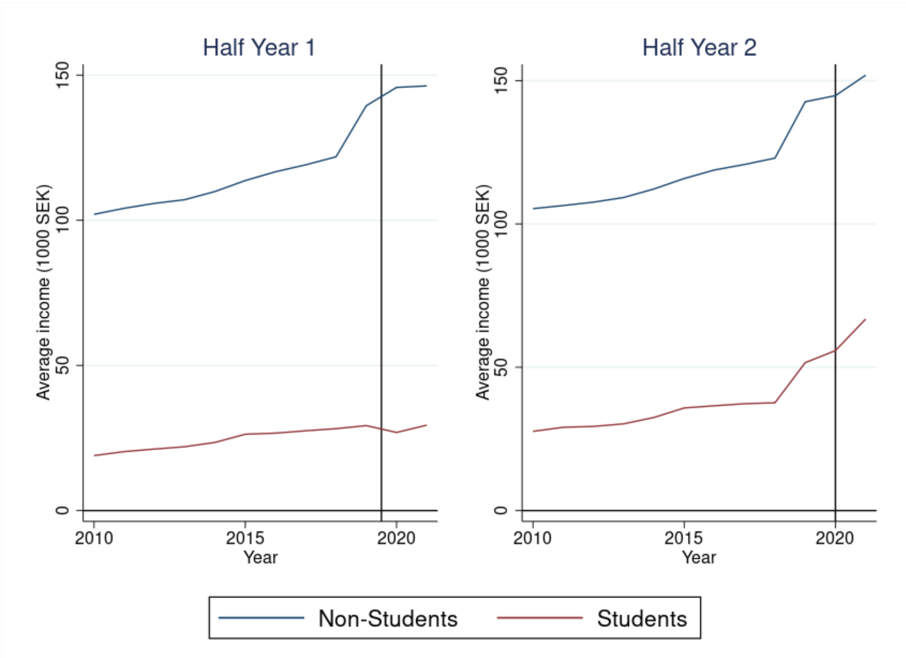


Figure 5: Pre-Trends in Mean Income for Students and Non-Students

Firstly, we can see that the 2020 shocks as a whole negatively and significantly impacted average incomes across students and non-students. Relative to 2019, average incomes fell by 3,600 SEK and 5,800 SEK in the first and second half years of 2020. It is likely that the larger effect in the second half year is to some degree driven by the fact that lockdowns in Sweden occurred in March 2020, once a third of the first half-year had already passed. Additionally, the fact that the average effect of being in the treated group on income is consistent when considering either 2019-2020 or 2019-2021 provides some reassurance that the COVID shock similarly affected the treatment and control group, at least on average.

Relative to 2019, mean incomes had fallen by 4,260 in the first half year of 2021, but had increased by 1,470 in the second half year. This suggests that the negative effects of COVID-19 may have somewhat dissipated by the summer of 2021. This is also consistent with Swedish lockdowns gradually being lifted starting in June 2021, having remained largely in effect since March 2020 (*COVID-19 Data Explorer* 2024).

	2019-2020		2019-2021	
	Inc 1	Inc 2	Inc 1	Inc 2
Post	-3.603***	-5.833***	-4.260***	1.468***
	(0.146)	(0.145)	(0.169)	(0.167)
Treat	-89.12***	-65.43***	-89.23***	-64.94***
	(0.211)	(0.226)	(0.203)	(0.218)
Treat x Post	3.145***	1.430***	2.751***	3.667***
	(0.182)	(0.208)	(0.206)	(0.235)
N	1009780	1009780	1015324	1015324

Individual-clustered standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: DID Key Results

The baseline DID estimates present clear evidence that the 2020 reform led to an increase in income for students. The coefficients on the interaction variable are significantly positive and large, particularly when considering the more credible treatment period 2019-21. Overall, this suggests the reform increased incomes by roughly 2,700 SEK in the first half year, and by 3,700 SEK in the second half year. This corresponds to an increase of approximately 12% and 10% over the 2019 averages, respectively, a quantitatively large effect on student finances. When switchers are included, the DID estimates are slightly smaller (although not significantly so), particularly when considering the effects in 2021. This is likely due to the implied change in the composition of both the treatment and control groups.

5.4 Changes-in-Changes

The key CIC results are presented in Figure 6 and Table 4, where the absence of estimates for the first two deciles is due to a small number of observations in the control group for those deciles when using the restricted sample.

The results indicate a large and significantly positive effect of the reform on student earnings, increasing in the initial earnings decile.

When considering the 2019-2020 period, QTEs range from insignificant for the bottom 4 deciles to an increase of 8,771 SEK and 1,843 SEK for the top decile in the first and second half years, respectively. This corresponds to increases of 3% and 0.3% over the initial values. Looking at the more credible treatment period 2019-2021, the effects are larger in both absolute and relative magnitude at the high end of the distribution in both half years: 15,300 SEK and 14,500 SEK, respectively, corresponding to increases of 8.10% and 7.61% over the initial values. I am also able to reject at the 5% level the hypothesis that the QTE is constant across the distribution, showing that the positive correlation between QTE and initial income is significant.

The larger effects in 2021 are consistent with the previous findings, suggesting that the negative effects of COVID-19 had attenuated by 2021, and/or that students were better able to respond to the reform by increasing their earnings at that point. The significanly positive correlation between QTE and income corresponds also provides strong evidence of heterogeneity in responses along the income distribution, consistent with theoretical predictions: students with the initially highest income, i.e., those with the most binding constraint, had the most incentive to adjust their behaviour as a result of the reform.

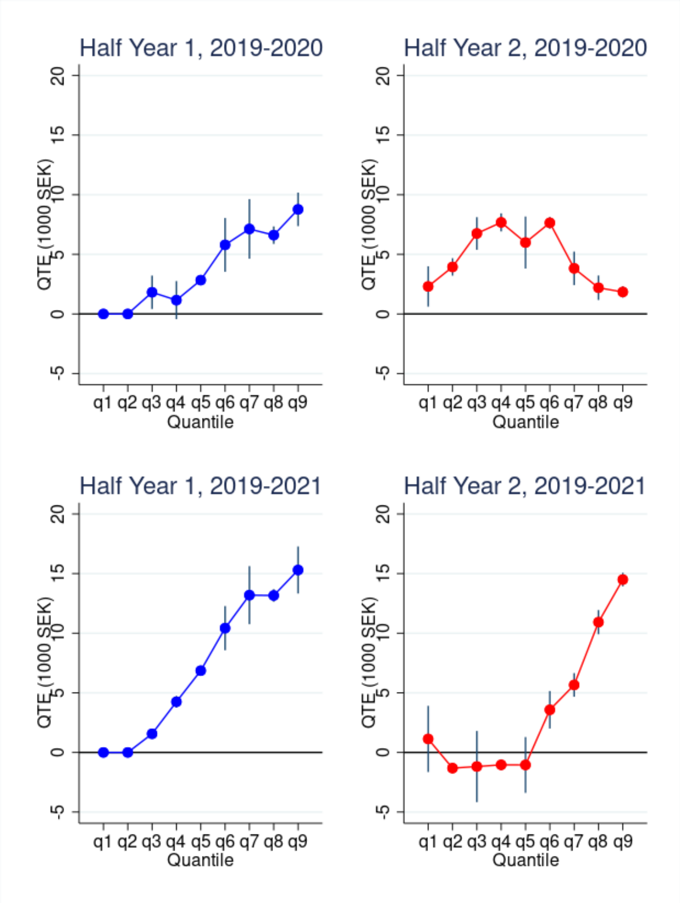


Figure 6: Estimated Quantile Treatment Effects

	2019-2020		2019-2021	
Quantile	Inc 1	Inc 2	Inc 1	Inc 2
q1	0	2.305**	0	1.130
	(.)	(0.863)	(.)	(1.416)
q2	0	3.943***	0	-1.319***
	(.)	(0.373)	(.)	(0.214)
q3	1.817*	6.750***	1.564***	-1.181
	(0.716)	(0.697)	(0.0280)	(1.524)
q4	1.158	7.680***	4.247***	-1.039***
	(0.815)	(0.386)	(0.253)	(0.235)
q5	2.837***	5.990***	6.860***	-1.049
	(0.192)	(1.113)	(0.171)	(1.196)
q6	5.794***	7.644***	10.42***	3.577***
	(1.150)	(0.252)	(0.945)	(0.802)
q7	7.130***	3.824***	13.19***	5.659***
	(1.275)	(0.718)	(1.241)	(0.505)
q8	6.611***	2.197***	13.17***	10.93***
	(0.374)	(0.523)	(0.270)	(0.517)
q9	8.771***	1.843***	15.30***	14.50***
	(0.717)	(0.245)	(1.006)	(0.291)

Individual-Clustered Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Estimated Quantile Treatment Effects

6 Discussion

The accuracy and precision of my analysis remain limited due to the perfect correlation of the fribelopp reform and the COVID-19 shock. However, the consistency and stability of my estimates across research designs and specifications suggest that my results manage to correctly, if imprecisely, identify real responses to the 2020 reform. The presence of significant excess mass at the fribelopp only when it actually impacted students highlights its salience, and indicates that these relatively high-earning students are expected to be highly responsive to reforms to the system's incentives and constraints. This is corroborated by my observations of significant positive behavioural responses, even in the face of the severe negative economic shock posed by COVID-19. These results are buttressed by the larger effects observed for high earners, in line with theoretical predictions as to the heterogeneous effect of removing a kink in the tax schedule.

Some tentative conclusions can be drawn for the functioning of the system. Firstly, in normal times, very few students overall are working enough to approach the fribelopp. This indicates that the fribelopp does not constitute a binding constraint for the majority of students, although it may disproportionately impact ex ante disadvantaged students who are forced to work more to be able to study. Secondly, the removal of the fribelopp had significant positive effects on earnings across the income distribution, particularly for high earners for whom the constraint was previously binding. All else equal, this indicates that the reform was succesful in its role as a targeted support measure in response to COVID-19.

There are two main ways in which my analysis could be improved. Firstly, studying responses to the reintroduction of the fribelopp in June 2022 is likely to yield more convincingly unbiased estimates. This would also provide an interesting exercise in comparative statics: this later reform should lead to a return to the pre-COVID trend, if COVID had no lasting impact on the student population's composition, or its labour preferences and constraints. Secondly, I have focused on the intensive margin of earnings, but my findings indicate that affected students are more likely to adjust behaviour along two extensive margins which may be more pertinent to examine: the choice to work, and the choice to be a student. Focusing only on the intensive margin necessarily discards the effects on individuals pushed out of or into education as a result of the pandemic. However, existing methods commonly used in public economics are ill-suited to study this angle, leaving room for future improvements to econometric evaluations.

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