FINNISH CENTRE OF EXCELLENCE IN TAX SYSTEMS RESEARCH

FIT Working Paper 9

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# Discrete Labor Supply: Quasi-Experimental Evidence and Implications

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CENTRES OF EXCELLENCE IN RESEARCH

# Discrete Labor Supply:
# Quasi-Experimental Evidence and Implications

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April 19, 2023

**Abstract**

We provide quasi-experimental evidence of discrete labor supply. We utilize a novel institutional setting where a reform shifted an income notch to a higher income level, and compare earnings distributions of treated and non-treated individuals before and after the reform. We find transparent evidence of widespread changes in the earnings distribution, which is consistent with discrete but not continuous labor supply. We present a simple application for estimating changes in a distribution to detect discrete responses to local tax changes.

**Keywords:** discrete labor supply; tax elasticity; distributions

**JEL Classification Codes**: J22, H24, H21

Acknowledgments: We thank Spencer Bastani, Richard Blundell, Raj Chetty, Alexander Gelber, Jarkko Harju, Ohto Kanninen, Patrick Kline, Wojciech Kopczuk, Tomi Kyyrä, Attila Lindner, Tuomas Pekkarinen, Jukka Pirttilä, Jim Poterba, Daniel Reck, Emmanuel Saez, Matti Sarvimäki, Joel Slemrod, Marko Terviö, Mazhar Waseem, Danny Yagan and Gabriel Zucman as well as participants in numerous conferences and seminars for their helpful comments and discussions. Funding from the Academy of Finland (No. 299373, No. 303689, No. 322074, No. 346253) is gratefully acknowledged.

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# 1 Introduction

Analyzing labor supply is one of the most important topics in labor economics, public finance and macroeconomics. Textbook models often assume that labor supply can be adjusted flexibly and continuously in response to shocks or policy, but researchers have debated for quite some time whether labor supply should be modeled as continuous or discrete (see e.g. Rosen 1976, Altonji and Paxon 1988, Dickens and Lundberg 1993, van Soest 1995, Blundell *et al.* 2008, Kreiner *et al.* 2015, Beffy *et al.* 2019). Despite this discussion, there has been limited analysis on how the choice of the labor supply model affects empirical analysis. The literature analyzing earnings responses to taxes often assumes continuous labor supply underlying the analysis, even when acknowledging various types of optimization frictions (Feldstein 1999, Saez *et al.* 2012, Chetty 2012, Kleven and Waseem 2013). However, this assumption seems contrary to many situations occurring in real-life labor markets. For example, switching to another job or getting a promotion or raise can lead to a discrete change in annual earnings even conditional on participating in the labor market. These examples appear to be particularly relevant for regular wage earners who constitute the bulk of taxpayers.

This paper provides novel quasi-experimental evidence supporting the discrete labor supply model. In our empirical analysis, we utilize a reform that shifted the location of an income notch and examine the changes in the whole income distribution caused by the reform. Our main finding is that earnings increase in a wide range below the original location of the notch, which is consistent with discrete labor supply but not with any of the typical variants of continuous labor supply models used in the literature. In addition to the quasi-experimental evidence, we provide divided sample results and descriptive evidence on changes in annual earnings for the bulk of wage earners in the labor force supporting the discrete labor supply model.

We make a number of contributions to the literature. The first contribution is to provide quasi-experimental evidence supporting the discrete labor supply model often used in structural work (Rosen 1976, Altonji and Paxon 1988, Dickens and Lundberg 1993, van

Soest 1995).[1] Second, our finding has important implications for the literature studying the elasticity of taxable income (ETI) that typically assumes, explicitly or implicitly, a continuous labor supply model that possibly includes adjustment frictions (Saez *et al.* 2012, Chetty *et al.* 2011, Chetty 2012, Kleven and Waseem 2013). In addition to various types of adjustment frictions, discrete labor supply could provide a relevant explanation for the findings that income taxes typically generate only small observed changes in labor supply, especially among regular wage earners (see e.g. Martínez *et al.* 2021, Kleven and Schultz 2014, Jacobsen and Søgaard 2022). More importantly, discrete labor supply model indicates that income tax changes targeted at a specific group in the income distribution can also affect taxpayers elsewhere in the distribution. Ignoring these broader effects of tax changes can lead to a bias in estimating the ETI, and thus the welfare effects of income taxes. Identifying the broader effects in the distribution calls upon methods that can identify changes in different parts of distribution (Athey and Imbens 2006, Firpo *et al.* 2009, Firpo and Pinto 2016, Cengiz *et al.* 2019). We contribute to this literature by presenting a relatively simple application for estimating changes in the distribution caused by a local change in incentives.

Our main empirical estimates utilize a reform that changed the location of a notch in the income tax schedule. The notch is a jump in the average tax rate creating strong but local tax incentives. The institutional setting involves a monthly study subsidy for Finnish higher education students. A student loses eligibility for one month of the subsidy (approximately 500 euros) if her annual earnings exceed a predetermined gross income threshold (9260 euros before 2008), causing a sharp drop in disposable income above this notch. In 2008, the location of the income threshold was increased by about 30% to 12,070 euros, allowing students to earn more income before they lose the subsidy they are eligible for. As Finnish university students typically participate in flexible part-time labor markets during their studies, the notch and the reform create a relevant change in employment incentives for a majority of students. Students face similar labor market institutions and regulations as other workers, such as employment contract requirements

[1] However, in our analysis the more precise mechanism behind discrete labor supply remains open, such as a sparse menu of job offers or fixed job switching costs.

and minimum wage and working hours legislation. However, university students typically participate in more flexible labor markets than the majority of regular wage earners. Thus, to the extent that discreteness characterizes labor markets, it arguably affects regular wage earners more than higher education students we focus on in our analysis.

We examine the impact of the reform on the whole income distribution rather than just within a narrow window around the notch, allowing us to observe which parts of income distribution respond to the reform. This in turn is revealing of different labor supply mechanisms. To capture the causal impact of the reform in different parts of the distribution, we develop a quasi-experimental application for this purpose that resembles the methods proposed in recent literature (see e.g. Athey and Imbens 2006, Firpo *et al.* 2009, Firpo and Pinto 2016, Cengiz *et al.* 2019). We calculate the change in the relative density of the students' income distribution before and after the reform and contrast it to changes in the distribution of a control group. The control group we use consists of young wage earners who are not students themselves but similar to students in their labor market attachment and earnings. The key identification assumption is that the earnings distributions of the treatment and control groups evolve similarly over time in the absence of the reform. We evaluate this assumption and find that the distributions of both groups remained practically constant over time both before and after the reform, thus supporting our identifying assumption.

As our main empirical result, we find that the reform caused distinctive changes in the earnings distribution of students in a broad income range, especially from earnings levels below the original location of the notch. In contrast, we find no discernible changes in the income distribution of the control group, reducing the worry that the observed changes in the treatment group would be caused by, for example, local labor market shocks instead of the reform. Furthermore, our panel data evidence shows large individual-level upward jumps in earnings that were much more common for students the year following the reform compared to the period before it. The panel data evidence also shows that either individuals make large jumps in earnings or do not respond at all, but they do not make marginal adjustments in their income. Also this anatomy of the response is consistent with discrete but not continuous labor supply model. These results also indicate that the cross-sectional changes in the distribution arise because of individual-level discrete changes and not because of, for example, changes in the composition of students over time.

We contrast our empirical results with predictions from stylized models featuring either discrete or continuous labor supply. Between these two models our results are only consistent with the discrete model. In that model individuals consider choosing between distinct earnings locations in the distribution, and change their location if one of them becomes more attractive than their current location. This feature of the discrete model applies regardless of the more precise mechanism leading to jumping between locations, such as fixed costs for switching jobs, career considerations or a sparse menu of work offers available. Our key finding that many students jumped to a higher earnings level after the relocation of the income notch is thus consistent with individuals following the changes in incentives in the discrete model.

Instead, in the continuous model individuals well below the original notch are unaffected by the reform. In this model individuals only consider changes that affect their utility or budget constraint at the currently chosen optimal earnings level. A reform that takes place at a significantly higher income level affects neither of these elements. Thus, the continuous model is unable to predict that individuals located far below the notch at a linear budget segment would respond to a relocation of the notch to a higher income level. Note that including additional elements to the continuous model, such as adjustment frictions or optimization errors, do not change the basic feature that the continuous model cannot predict responses occurring far below the original notch. To further visualize the effects of the modeling choices, we present income distributions drawn from a simulation model we built for this purpose.

We find further empirical support for discrete labor supply by examining the effects of the reform among two specific subgroups of students: those who work in plausibly more discrete labor markets (public sector, or research, manufacturing and construction in the private sector) and those working in less discrete labor markets (restaurants, bars and cafes, hotels, cleaning and security services). The latter group faces arguably less discrete labor markets because they typically have more flexible working hours and are more likely subject to hourly rather than monthly wages compared to the first group. We find a significantly larger shift in the earnings distribution of students working in the more discrete labor markets, providing additional suggestive evidence in favor of the discrete model. Moreover, this finding suggests that discreteness stems from the functioning of the labor markets, a feature that is not specifically related to students. When looking at the labor force as a whole, we observe that a significant share of regular wage earners in Finland are employed in industries that we classified as more discrete (61% in our data), suggesting that discrete labor supply can induce a relevant constraint for a large share of the overall labor force.

Moreover, we find in our full population data that a slightly over half of all regular wage earners in the Finnish labor force who earn over 30,000 euros per year experience larger than 5% jumps in their earnings from year to year. About 15% experience changes greater than 20% in annual earnings. These findings are consistent with the observations of Guvenen *et al.* (2021) for the US, and highlight that the earnings generation process of regular wage earners is easier to reconcile with a discrete rather than continuous model.

Our findings have implications for the empirical analysis of taxes. Discrete labor supply could explain why many empirical estimates of the effects of income taxes on earnings, often measured as the elasticity of taxable income (ETI), are very low especially for wage earners (Martínez *et al.* 2021, Kleven and Schultz 2014, Jacobsen and Søgaard 2022, Neisser 2021). Intuitively, discreteness in labor supply could reduce the extent to which individuals are able and willing to respond to changes in incentives, reducing the observed effects of income taxes on earnings. For example, Martínez *et al.* (2021) do not find significant changes in earnings or the employment rate as a response to a very large cut in income taxes related to the Swiss income tax holiday scheme. Instead, they do find larger effects among the self-employed, who potentially can adjust their taxable income more flexibly compared to wage earners.

In addition, our evidence has implications for the widely applied bunching estimator (see Kleven 2016 for a survey). When labor markets are characterized by the discrete model, individuals are not able to very precisely bunch at kinks or notches in their budget sets, but instead the earnings distribution might be affected from a broader range around the discontinuity. This could explain the insignificant or small bunching estimates observed in many studies for wage earners (Saez 2010, Chetty *et al.* 2011, Bastani and Selin 2014). The bunching method focuses on local responses and thus ignores the widespread effects in the distribution, and would consequently underestimate the true extent of behavioral responses to income taxes when the responses are discrete.[2] To demonstrate the extent of this bias, we estimate a mobility elasticity of 0.18 for students, which is approximately 2.5 times larger than what we would get if we only use local bunching at the notch to estimate an ETI of 0.07.

This paper proceeds as follows: Section 2 presents the relevant institutions and empirical methods. Section 3 presents the main results. In Section 4 we discuss the theoretical mechanisms, further empirical support for the discrete labor supply model, and its implications for empirical analysis. Section 5 concludes.

# 2 Institutions, data and empirical methods

## 2.1 Study subsidy for university students

In Finland, all students who are enrolled in a university or polytechnic can apply for a monthly study subsidy, administered nationally by the Social Insurance Institution of Finland (hereafter SII). The subsidy is intended to enhance equal opportunities in acquiring higher education, and to provide income support for students who often have low disposable incomes. In Finland, university education is publicly provided and there are no tuition fees. A large proportion of individuals receive higher education in Finland (approximately 40% of individuals aged 25-34 have a degree), and the study subsidy program is widely used among students.

[2]Note that this criticisms is based on what is assumed as the underlying model generating the behavioral responses in the bunching model, and is different from, for example, the criticism of Blomquist *et al.* (2021).

The maximum amount of the subsidy was 461 euros per month in 2007. The default number of subsidy months per year is 9, which is provided if a student does not actively apply for a different number of subsidy months, and which a large proportion of students also receive. The eligibility for the study subsidy depends on personal annual gross income (labor income + capital income), and an academic criterion of completing a certain predefined number of credit points per academic year. Parental income or wealth do not affect eligibility nor the amount of the benefit for students not living with their parents.[3]

The discontinuity in labor supply incentives is created by an income threshold. If a student has annual gross income higher than a predetermined threshold, one month's subsidy is reclaimed by the SII. This results in an increase in the effective average tax rate, or an increase in the implied marginal tax rate of over 100%, in a region just above the threshold, creating a *notch* in the budget set of students. With 9 subsidy months the income threshold was 9260 euros in 2007. An additional month of the subsidy was reclaimed for an additional 1010 euros of income above the threshold. This implies that the schedule ultimately comprises multiple notches in an income range above the first notch. Students can deviate from the default of 9 months and alter the number of subsidy months by application, or by returning already granted subsidies by the end of March in the next calendar year. Having more study subsidy months reduces the income threshold, and vice versa.[4]

The study subsidy program was reformed in 2008. In the reform the income threshold was increased by approximately 30%. For a typical student with 9 study subsidy months, the annual income threshold increased from 9260 to 12,070 euros. In addition, the monthly study subsidy was increased slightly from 461 to 500 euros per month. Other details of the system were not changed, including the academic criterion related to the

[3] The full study subsidy includes a study grant and a housing benefit. The standard study grant was 259 euros/month and the maximum housing benefit 202 euros/month in the academic year 2006/2007. Housing benefits are granted only for rental apartments, and the housing allowance is reduced if spousal gross income exceeded 15,200 euros per year (in 2007). In addition to the study subsidy, students can apply for repayable student loans secured by central government.

[4] In 2007, the formula for the annual income threshold was the following: 505 euros per study subsidy month and 1515 euros per month without the study subsidy, plus a fixed amount of 170 euros.

required number of academic credits.[5]

Figure 1 illustrates the study subsidy schedule before and after 2008 for a student who collects the default 9 subsidy months. The figure shows that students face large local incentives not to exceed the first income threshold because of the initial threshold and multiple similar thresholds after that. Once an income threshold is exceeded, losing one month of the subsidy causes a significant drop in disposable income, and thus a dominated region right above the threshold. The figure underlines the distinctive change in incentives caused by the increase in the location of the income thresholds in 2008, highlighting that the reform encouraged students to increase their earnings above the old income threshold. Finally, Table 4 in the Appendix shows the income thresholds in numbers before and after 2008, and presents the relative loss in disposable income incurred if the income threshold was exceeded.

## 2.2 Data and descriptive statistics

Although the majority of students have access to the study subsidy and repayable student loans, most university students in Finland also work part-time during their studies within and between semesters. Therefore, the means-testing of the study subsidy creates a real constraint affecting the labor supply choices of a majority of students. In our analysis, we use panel data on all working-age individuals (15–70 years) living in Finland in 1999–2013, provided by Statistics Finland. These data include a rich set of register-based information, such as tax and social benefit registers and information on the study subsidy program. With these data, we can analyze responses to the incentives created by the program and learn how various individual characteristics affect labor supply responses.

Table 1 shows the descriptive statistics for a pooled sample of students in 1999–2013. In our analysis, we drop first-year students and students who graduate within a given year in order to avoid the effects of potential income shocks before enrollment and after graduation. However, dropping first-year students and graduates does not affect the main

[5] As with the old regime, an additional month of the subsidy is reclaimed after an additional 1310 euros of gross income above the threshold. After 2008, the formula for the threshold was the following: 660 euros per study subsidy month and 1970 euros per month when no study subsidies are collected, plus a fixed amount of 220 euros.

results in a meaningful way. Mean annual labor income among our sample of students is 8446 euros. We observe that on average 80% of students earned at least 500 euros of labor income in a year. In addition, only 55% of students received labor earnings from only one employer, indicating that students tend to work in different types of jobs during a year. These observations indicate that many students work in part-time or temporary jobs during their studies and breaks between semesters in order to increase their disposable income and/or to gain work experience while studying. In terms of sectors, 19% of students work in manufacturing (including construction), 16% in hospitality services such as hotels and restaurants, 39% in administrative and support services or in the public sector, and 25% in other sectors, including those whose sector cannot be identified in the data. In terms of study fields, 17% of students in our sample study arts and humanities, 19% business and social sciences, 34% technology or health and social services and 29% in other fields, including those whose field of study cannot be identified.

In our baseline analysis, we focus on students who received the default 9 months of study subsidy before and after 2008. For this group, the income threshold increased from 9260 to 12,020 euros. The advantage we gain by fixing the number of subsidy months is that we can isolate the effect of the change in the location of the threshold on the earnings distribution. This restriction is, however, not very selective as a large proportion of students receive 9 months of the study subsidy, partly because it is the default choice and partly because it presumably creates a good balance between subsidies and labor earnings for many students. The share of students receiving the default subsidy months is 42.4%. Students who receive the default subsidy are similar to the overall student population, except that they are on average slightly younger (22.4) and have less labor income (5633 euros) than all students. We test the robustness of our main results by including students who deviate from the default subsidy choice.

### 2.3 Estimation method

The 2008 reform that shifted the location of the notch creates a unique empirical set-up to study earnings responses to a distinctive and salient change in tax incentives. We are particularly interested in investigating whether local tax incentives, such as notches or kinks, affect income distributions in a wider income range rather than just close to the income threshold. Thus, we examine the changes in the whole income distribution at the time of the reform, which enables us to test whether labor supply is discrete or continuous, as discussed in detail in Section 4.

Recent literature often uses a bunching method to estimate responses to a local tax discontinuity by relating an excess mass in the distribution just below a notch or kink to an estimated counterfactual (see Kleven 2016 for a survey). Standard bunching method is presented in detail in Online Appendix A, and graph (a) in Figure 2 illustrates the method graphically. However, we do not apply the bunching method in our main analysis for two reasons: first, it produces downward-biased earnings response estimates if the notch affects the earnings distribution more broadly than just around the local discontinuity. Second, the surrounding density outside the bunching region cannot be used to estimate a credible counterfactual when that part of the distribution is also affected by the notch. We discuss these issues in more detail in Section 4.

Instead of using the bunching method, we develop a new method in the spirit of differences-in-differences (DiD) and changes-in-changes (CiC) methods to estimate distribution-wide responses to a reform that shifts the location of an income notch. Our method is similar to that used in Cengiz *et al.* (2019) who estimate the localized effects of minimum wages by income bins of the wage distribution, but our focus is to show more explicitly what happens to the shape of the overall distribution.

We estimate a counterfactual change in the distribution utilizing a control group, similarly as in the DiD method. We estimate to what extent the whole earnings distribution is affected by the reform relative to this counterfactual. We measure the distributions relative to the total number of students in order to account for potential changes in the number of students across years.[6]

More formally, we first measure the change in the students distribution as follows:

[6]Note that in the standard bunching method, using relative distributions or frequency distributions produces identical estimates.

$$\hat{b}(z) = \frac{\sum_{i=z_L}^{z_H} \left[ (c_j^B/N^B) - (c_j^A/N^A) \right]}{\sum_{i=z_L}^{z_H} (c_j^A/N^A)/N_j} \tag{1}$$

where $c_j$ is the count of individuals in an income bin $j$, and $z_j$ denotes the income level in bin $j$. $\sum_{i=z_L}^{z_H} (c_j^B/N^B)$ is the share of students within a fixed income range $[z_L, z_H]$ relative to the total number of students in the distribution $(N^B)$ before the 2008 reform, and $\sum_{i=z_L}^{z_H} (c_j^A/N^A)$ after the reform in the same income range. $N_j$ denotes the number of bins within $[z_L, z_H]$. In the estimation, we set the lower limit $z_L$ to zero and measure changes in the whole distribution below the old income threshold by setting $z_H$ equal to the old income threshold (9260 euros). Graph (b) in Figure 2 graphically illustrates the estimation approach.

To complete our method, we utilize a control group to take into account potential changes in the earnings distribution for reasons other than the change in the study subsidy system, such as changes in the economic environment affecting the labor markets where students work. We use young part-time workers who are not higher education students as the control group. Those in the control group are not eligible for the study subsidy and thus not subject to the income threshold or the reform, but are of the same age as the students and work essentially in the same labor markets and in similar jobs. As a result, the control and treatment groups are similar in their labor market characteristics such as labor earnings, as described in Table 2.[7]

Our method to estimate the change in the distribution caused by the reform calculates the change in the density in the treatment group between the two time periods as presented in equation (1), and subtracts from this the change in the control group over the same period:

$$\hat{b}_d(z) = \left[ \frac{\sum_{i=z_L}^{z_H} \left[ (c_j^B/N^B) - (c_j^A/N^A) \right]}{\sum_{i=z_L}^{z_H} (c_j^A/N^A)/N_j} \right]^S - \left[ \frac{\sum_{i=z_L}^{z_H} \left[ (c_j^B/N^B) - (c_j^A/N^A) \right]}{\sum_{i=z_L}^{z_H} (c_j^A/N^A)/N_j} \right]^P \tag{2}$$

[7] The control group is selected to roughly match students' job and age characteristics. Students typically work in part-time jobs and they tend to be young. Thus the control group comprises individuals who we observe to have less than 12 working months per year, and who are 19–24 years old. This age interval matches the 25–75 percentile points of the students' age distribution. Our results are not sensitive to small changes in the composition of the control group.

where superscript $S$ denotes students (treatment group) and $P$ non-student part-time workers (control group). This estimate summarizes the broader change in the earnings distribution of students caused by the reform while taking into account any other potential changes in the labor market environment of part-time workers.[8]

Our identification assumption is *not* random assignment into treatment and control groups, but that the changes in the earnings distribution of the control group reflect the changes in the treatment group in the absence of the reform. More precisely, we need to assume that the relative distributions would evolve similarly over time in the treatment and control groups in the absence of the reform. We empirically evaluate this assumption by examining the development of the distributions before and after the 2008 reform.

Our identification assumption resembles the parallel trends assumption familiar from the DiD approach. Note that as we estimate the changes in densities separately for the treatment and control groups, our approach is not as sensitive to linear functional form assumptions as the regression version of the DiD. Also, our identification assumptions are similar to those in the CiC approach (see e.g. Athey and Imbens 2006), but we estimate the overall change in the density due to the reform, in comparison to identifying changes at each quantile as in the CiC approach. Moreover, our approach is relatively straightforward to apply in practice compared to the CiC approach.

# 3 Main results

We begin by presenting the earnings distributions of the treatment and control groups over time around the reform in 2008. Figure 3 shows the labor earnings distributions of the two groups before and after the reform for those with positive earnings within an income range of 0–18,000 euros in 2006–2007 and 2008–2009, denoting the pooled pre

[8]Following the bunching literature, the standard errors for $\hat{b}_d(z)$ are calculated using a residual-based bootstrap procedure (Kleven and Waseem, 2013). First, we fit a flexible polynomial function to both the pre- and post-reform earnings distributions of students and other young part-time workers. We then generate a large number of new estimates for the distributions by randomly re-sampling the residuals from these regressions (with replacement). The standard error is defined as the standard deviation of $\hat{b}_d(z)$ based on the bootstrapped distributions.

and post-reform periods, respectively.[9]

Remarkably, the figure shows that the earnings distribution of students has a clearly different shape after the reform than before it; the earnings of students have increased over a wide income range. Especially intriguing is that the shifting of the earnings distribution to the right occurs from an earnings range far below the old location of the income threshold. Contrary to students, the earnings distribution of the control group remained practically constant between 2006–2007 and 2008–2009. This suggests that the shifting of the students' distribution cannot be explained by other contemporary changes in the local labor markets affecting the earnings development of both the control and treatment groups. We discuss this further below.

To quantify the changes in the distribution, we estimate equation (2) that produces as its outcome to what extent the density in the students' distribution responded to the reform. We estimate the change in density in the students' distribution and subtract from this the change in density in the distribution of the control group. The estimation is performed within an income range of 0–9260 euros, thus including the whole distribution below the old location of the income threshold. The estimate is large (9.809 with a standard error of *1.01*), suggesting that the magnitude of the change in the overall earnings distribution is both economically and statistically significant. This estimate is approximately three times larger than the standard bunching estimate, 2.931 (*0.875*), estimated following the methods of Kleven and Waseem (2013) within an income range just below the threshold (8100–9260 euros) before the reform.[10] In order to further characterize the general magnitude of the response, we estimate that the earnings of affected students increased on average by 550 euros when accounting for changes in whole distribution, which corresponds roughly to a 10% average annual increase in labor earnings.

In order to ensure that the above estimates represent causal responses to the reform, we provide a number of robustness checks. We first check that the distributions of the treatment and control groups evolve similarly over time in the absence of the reform,

[9]The figure includes only labor earnings and not all income to which the income threshold applies because receiving capital income is very rare among students and other young part-time workers.

[10]Bunching estimates are discussed in more detail in Online Appendix A.

as discussed in Section 2.3. As a first check to this end, Figure 4 plots students' and other young part-time workers' earnings distributions over a longer time period of four years before and after the reform of 2008. The figure shows that the change in the earnings distribution of students occurred exactly at the time of the relocation of the income threshold, indicating that any gradual shifting of the distribution does not explain the observed pattern. Furthermore, the distribution of the control group remained almost unchanged throughout this period, therefore strongly supporting our identification assumptions. In more detail, the distributions of both the treatment and control group exhibit very similar minor changes at the bottom from 2004–2005 to 2006–2007, further strengthening the case that the distributions have developed very similarly over time in the absence of the reform.

Our second robustness check concerns potential changes in the composition of students in the distribution over time. Figure 10 in the Appendix shows the distributions in 2006–2007 and 2008–2009 when we use bin-level inverse probability weighting to re-weight the distribution in the latter period to match the pre-reform characteristics of students in terms of age, field of study and industry of the firm they work for. Re-weighting does not change the outcomes in a significant manner, indicating that potential changes in the characteristics of the student population are not likely to explain the results.

Furthermore, one might be concerned that students can also respond to the reform by changing the number of study subsidy months they choose. First, Figure 11 in the Appendix shows the earnings distributions in 2006–2007 and 2008–2009 when including students with other than the default 9 study subsidy months. The figure illustrates that the broader changes in the distribution are clearly visible when including this broader group of students, implying that the choice of study subsidy months is not driving our main results. Second, we find no significant changes in the distribution of subsidy months associated with the reform. Instead, 9 months is the most typical choice with a similar fraction choosing it in all of the years around the reform, as illustrated in Figure 12 in the Appendix. This indicates that students responded to the reform by changing their earnings, but not, on average, by claiming more or less subsidies per year.

In addition, a closer examination of the students' earnings distribution in Figure 3 implies that lack of salience is not a significant factor in explaining the results. Instead, at least a fraction of students seem to be aware of the exact location of the income thresholds and are able to adjust their labor earnings in response to them, as the distribution exhibits clearly visible bunching just below the thresholds both before and after 2008. Also, the bunching response disappeared below the old threshold immediately after the reform.[11]

Next, we present panel data evidence revealing the anatomy of the responses by estimating how students starting from different parts of the base-year income distribution changed their earnings. These results highlight that many students who were located below the threshold before 2008 responded to the reform with a large increase in their earnings, while others might have not responded at all.

First, in graph (a) of Figure 5 we analyze average individual-level changes in earnings. The figure shows that average changes in individual income are very similar in the years before the reform (from 2005 to 2006 and 2006 to 2007), and that there is a visible pattern of mean reversion (on average, starting from a lower income level leads to higher income in the next year, and vice versa). The figure shows that labor income increased significantly from 2007 to 2008 compared to the years before the reform for students originally below the threshold. This pattern is visible even for students with base-year earnings around 3000–6000 euros, which is well below the old threshold. Instead, in income bins above the new threshold at 12,000 euros the changes in earnings in the reform year is not different from other years, suggesting that the rapid increase in earnings below the old threshold stems from the change in the location of the income threshold.

Second, graph (b) of Figure 5 presents the likelihood of increasing individual earnings by 50% or more relative to base-year income. We observe that large increases in earnings were significantly more likely for students below the old threshold when the threshold was increased compared to previous years. The prevalence of increases larger than 50%

[11] Additional examination of excess bunching before and after the reform reveals, as further illustrated in Figure A2 in Online Appendix A, that bunching is slightly larger before the reform than after it in relative terms. One intuitive explanation for this finding is that the local incentives not to exceed the notch are smaller after 2008, since the relative significance of losing one month's subsidy in terms of disposable income is now smaller than before 2008 when the threshold was located at a lower income level.

doubled from 5% to 10% in the income bins below the threshold at the time of the reform but remained constant between the years before 2008. In contrast, there are no significant differences in the likelihood of large earnings increases between the pre- and post-reform years in the bins above the new income threshold.

Third, graph (c) of Figure 5 shows that the likelihood of relocating to earnings levels above the old income threshold increased significantly at the time of the reform, compared to the years prior to 2008. The fact that the likelihood of being located above the threshold increased even when starting from the income bins far below the old threshold further illustrates that a notable share of students responded to the reform with a large increase in their earnings when their budget constraint was relaxed at a higher earnings level.[12] Also, the panel data evidence highlight that students do not often adjust their income marginally, but they rather either make large jumps in their earnings or do not respond to changes in incentives at all.

In summary, we find clear evidence that the 2008 reform that shifted the location of the income threshold for students induced earnings responses in a wide range in their earnings distribution, especially among those who were previously located below the old location of the income threshold. This indicates that the relaxed budget constraint induced large jumps in earnings for many students who were not directly targeted by the reform based on their pre-reform earnings. We explore the potential mechanisms explaining this result below.

# 4 Conceptual framework, mechanisms and implications

## 4.1 Labor supply models and simulations

The aim of this section is to discuss which labor supply models are and are not compatible with our empirical results presented above. The main feature we want to explain is the

[12] Furthermore, these panel data results indicate that the observed changes in the cross-sectional earnings distributions of students discussed above stem from intensive-margin responses. To further support this, we find that the share of students not working at all (earning less than 500 euros per year) did not change significantly at the time of the reform. Therefore, potential extensive-margin responses do not explain the change in the shape of the observed earnings distributions.

shifting of the income distribution from a wide income range below the notch following the upward change in the location of the notch. We highlight that a discrete labor supply model can explain these results while any version of the continuous models cannot, even when augmented with adjustment frictions or optimization errors.

We assume a standard utility function $u(c, z)$, where $c$ denotes consumption (net earnings) and $z$ gross earnings, with properties $u_c>0$ and $u_z<0$. The budget set is $c = (1-\tau)z + R$, where $(1-\tau)$ is the net-of-tax rate and $R$ is virtual income. We abstract from income effects following the earlier literature (Saez *et al.* 2012). Including income effects would not change the main results in qualitative terms but would complicate the formulas.

For simplicity we parameterize the utility function to a quasi-linear form:

$$u(c, z) = c - \frac{w^i}{1+\frac{1}{e}} \left(\frac{z}{w^i}\right)^{1+\frac{1}{e}}, \tag{3}$$

where $w^i$ is an ability (productivity) parameter of individual $i$ over which individuals are heterogeneous such that there is some underlying distribution of abilities. Thus, the utility maximization with respect to $z$ gives the optimal choice for an individual, $z^* = w^i (1-\tau)^e$, where $e$ is the earnings elasticity parameter with respect to $\tau$. In Online Appendix B, we present versions of the continuous model that include adjustment frictions and optimization errors, which we discuss more below.

Following Saez (2002), we next construct a simple version of a discrete model by including to the canonical model in equation (3) a constraint that an individual must choose her earnings level from a sparse set of available locations. Modeling discreteness in this way is silent about the reasons for why the locations are discrete, and could be made more specific by assuming, for example, fixed costs for changing the earnings level while still arriving at similar qualitative results as below.

More formally, individuals choose from a discrete set of alternative earnings locations $j = 1, ..., N$, yielding utility $u(c_{j-1}, z_{j-1})$, $u(c_j, z_j)$, $u(c_{j+1}, z_{j+1})$, but individual preferences and the underlying ability distribution are continuous. The budget set is now described as $c_j = w_j - T_j + R$, where $T_j$ is the average tax at location $j$. Considering two locations $j-1$ and $j$, individual chooses the one which yields the highest utility:

$$u(c_{j-1}, z_{j-1}) \leq u(c_j, z_j) = c_j - \frac{w^i}{1+\frac{1}{e}} \left(\frac{z_j}{w^i}\right)^{1+\frac{1}{e}} \tag{4}$$

The main conceptual difference between this model and the baseline continuous model is that individuals now consider which one of the distinct earnings levels yields the highest utility. For example, if an individual is located at $z_{j-1}$ and a tax rate cut occurs such that it applies to $z_j$ but not to $z_{j-1}$, in this model an individual could switch from $z_{j-1}$ to $z_j$. This leads to a potentially large jump in earnings depending on how far apart the two locations are. More precisely, in the discrete model the individual either responds to a tax change by switching to an alternative location, or does not respond at all. There are no marginal adjustments as in the continuous model. Moreover, in a baseline continuous model in equation (3), an individual who is located at an optimal income level $z^*$ that is not directly at a discontinuity in the budget set, would not respond to tax changes occurring at a higher income level $z^{**} > z^*$. If there is a change in incentives applying to the current location, an individual can respond by adjusting income marginally in the continuous model.

Therefore, a model including a discrete choice component can rationalize large jumps in earnings as a response to a local tax rate change compared to any continuous model. Consequently, the discrete model can explain our empirical result that the distribution shifted from a wide income range below the original location of the notch as a response to the reform that shifted the location of the notch. Moreover, the discrete model is consistent with our panel data evidence that students often made large jumps in their income also in the absence of a reform. Also, consistent with discrete but not continuous model is the panel data evidence that some students did not respond to the reform at all while others made large adjustments in their income.

Next, we further illustrate the impact of modeling choices using a simple simulation model based on the above theoretical framework. Our aim is to visualize how earnings distributions would respond to a relocation of an income notch, which resembles our empirical case, under the discrete model and alternative continuous models from the literature.

The simulation model assumes an underlying ability distribution from which each individual $i$ receives a predetermined draw $w_i$. This draw represents earnings in the absence of behavioral responses to the tax system. Our parameterized ability distribution is presented in Figure B1 in Online Appendix B.[13] In the discrete model, the available discrete earnings locations for each individual are drawn from the probability distribution presented in Figure B2 in Online Appendix B. The number of choices drawn can be altered in different specifications and the draws vary between different individuals. Therefore, even when the individual-level choices are discrete, the overall earnings distribution is smooth. We discuss the simulation model in more detail in Online Appendix B.

We assume parameters in the simulation that roughly correspond to our empirical setting given in Table 3. The marginal income tax rate is set at 22% below the notch and a high marginal tax rate of 61% is applied above the notch, constituting a simplified linear version of the actual budget set for students including multiple notches above the income threshold (see Figure 1). The size of the notch, i.e. the size of the drop in disposable income at the income threshold, is 500 euros. The notch is relocated from 9000 to 12,000 euros in the simulated reform.

Figures 6 and 7 present the income distributions drawn form the simulation model. Figure 6 presents variants of the continuous model: the baseline continuous model (panel a), continuous model supplemented with adjustment frictions (panel b) and with both adjustment frictions and optimization errors (panel c).

First, in the baseline continuous model in panel (a) we find that bunching at the income thresholds is sharp and the excess mass relocates to the new location of the notch, but we observe no wider changes in the distribution following the reform. Following Kleven and Waseem (2013), optimization frictions that simply mitigate or hamper responses to taxes can be included in the parameterized continuous model by adding to the utility function a heterogeneous friction parameter (discussed in more detail in Online

[13]The distribution is a combination of power distributions and normal distributions, which gives an approximate match for the shape of the empirical earnings distribution of students in our empirical case. Our results are not sensitive to different underlying ability distributions that roughly match the empirical income distribution of students.

Appendix B). As can be expected, adding adjustment frictions to the model in panel (b) only leads to slightly smaller bunching and slightly more individuals being located just above the notch, but no changes in earnings over a broader income range. These results further illustrate the basic feature of the continuous model that individuals change their income continuously starting from marginal adjustments as a response to marginal changes in incentives. Adding earnings shocks as optimization errors on top of adjustment frictions in the model in panel (c) of Figure 6 yields more diffuse bunching as can be expected, but again no earnings responses over a wider income range below the old income threshold, in contrast to what we observe our empirical case. In summary, all considered variants of the continuous model fail to reproduce the widespread changes in the distribution from below the original location of the notch we observed in our empirical case.

In contrast, a discrete model produces distribution-wide responses to the reform that are at least qualitatively similar to their empirical counterparts. Figure 7 illustrates the changes in earnings distributions as a response to the simulated reform assuming 30, 15, 10 or 5 available earnings choices over the distribution for each individual. Using 10–15 available earnings locations yields the closest match with our empirical case in terms of shifting of the distribution from a wider range and in the shape of local bunching around the notch. This suggests that the earnings locations are quite sparse in the version of the model that best replicates the actual empirical distributions of students: 10-15 locations in the income range of 0–25,000 euros translate into annual changes of 10–30% in earnings. The simulated distribution with the discrete model also illustrates the feature of the discrete model that individuals either respond with large changes in income or do not respond at all, but make no marginal adjustments in income, which is consistent with our panel data evidence as discussed above.

Finally, we could potentially produce an even closer match with our main empirical results by adding further features to the discrete model, such as adjustment frictions and optimization errors we considered in Figure 6 in the continuous model or uncertainty which we did not consider above. However, we leave these extension for future research, because the main point of our simulations is to simply illustrate that we need some kind of a discrete component in the model in order to even qualitatively match our empirical results.

### 4.2 Welfare loss estimation and mobility elasticity for students

Next, we estimate an elasticity from our empirical reform that is consistent with discrete labor supply model and the distribution-wide responses it entails. The elasticity concept correctly capturing discrete jumps in earnings is the mobility elasticity, not the standard marginal elasticity in the continuous model (Saez 2002, Kreiner *et al.* 2015). Following Kreiner *et al.* (2015), we can express the mobility elasticity with the following equation:

$$\zeta = \frac{dY}{d(1-m)} \frac{1-m}{Y} \tag{5}$$

where $m$ is the average tax rate difference between two distinct earnings locations $j$ and $j-1$, $m = \frac{T_j - T_{j-1}}{w_j - w_{j-1}}$, and $Y = \sum_{j=1}^{N}(z_j g_j)$ where $g_j$ is the relative mass of individuals in each earnings location. Equation (5) thus captures the change in earnings inflicted by individuals moving between different earnings locations due to changes in the average tax rate differential between these locations.

This elasticity formula captures two important features that are missing from the continuous model. First, mobility elasticity captures earnings responses over a broader income range across multiple earnings locations, denoted above by $dY$. Second, the standard measure for the change in the marginal tax rate does not capture the change in tax incentives in non-linear budget sets across distinct earnings locations, whereas the average tax change across locations ($m$) does.[14]

Next, we empirically estimate a mobility elasticity for students utilizing the 2008 reform to empirically approximate the welfare losses created by the notch. We apply equation (5) where the necessary ingredients are the estimated changes in earnings and the changes in average tax rates due to the reform.

[14]Note that the two tax rate concepts $\tau$ and $m$ coincide when the tax system is linear across the distribution.

First, to measure the changes in average tax rates in the reform, we simplify the setting by assuming that students, on average, choose from only two earnings locations: one below the old income threshold and one above it. This simplification facilitates the calculations considerably, because we do not need to estimate the size of the jump in incomes for each individual while producing a roughly correct estimate of the average changes in incentives due to the reform. Average gross earnings were 6008 euros below the old threshold in 2007 in the income range of 2000–9200 euros, and 11,821 euros above the old threshold in 2008 in the income range of 9201–18,000 euros.[15] By using the actual tax and subsidy rules before and after the 2008 reform, we find that the net earnings difference between these two locations increased from 3534 to 4807 euros due to the relocation of the income threshold, highlighting the significant impact of the reform on incentives.

Next, we define the average earnings response to the reform. Average earnings were 7116 and 7529 euros in 2007 and 2008, and thus the average real earnings within an income range of 2000–18,000 euros increased by 413 euros from 2017 to 2018.[16] To approximate the mobility elasticity, we relate the average change in log gross earnings to the log change in the difference in net incomes between the average earnings locations described above, thus using a logarithmic version of equation (5).

Using these numbers produces a mobility elasticity estimate of 0.183.[17] Therefore, even though the reform caused large and distinctive earnings responses over a wide income range in the distribution, the strong change in incentives caused by the reform implies that the elasticity estimate is nevertheless modest. Our elasticity estimate is close to what Søgaard (2019) finds for university students in Denmark (0.1) who face shifting of a kink instead of a notch in their study subsidy program, but the income range over which the responses occur are much more modest in the Danish setting. Because the notch creates much stronger incentives compared to a kink in our setup, it is not surprising that the

[15]We limit our analysis to the income range of 2000-18,000 euros as there were no observable changes in the distribution in the area below 2000 or above 18,000 euros between 2007–2008 (see Figure 3).

[16]As shown in Figure 4, there were no significant changes in the annual earnings of students in years before or after the 2008 reform, and no changes in the earnings of the control group of young non-student part-time workers.

[17]Table 5 in the Appendix presents the variables used to calculate the mobility elasticity estimate.

responses occur over a broader income range than in the Danish setting, although the average elasticities are similar. Also, our estimated elasticity is within the range of the average ETI estimates in the literature (see Saez *et al.* 2012 and Neisser 2021), but this literature does not typically assess the aggregate income changes over the distribution as in our mobility elasticity estimation.

## 4.3 Further empirical evidence on discrete responses

Next, we provide further empirical evidence supporting the discrete labor supply model and discuss the external validity of our findings. First, we divide individuals in our estimation sample into subgroups based on the industry where they work in. The two groups feature arguably different degrees of discreteness in their labor markets based on the typical job characteristics in the industry, such as employment contracts and working hours. In the group working in labor markets characterized by more discreteness, we include the whole public sector, and research, manufacturing and construction industries in the private sector. In the group facing less discreteness in labor markets we include restaurants, bars and cafes, hotels and other accommodation services, cleaning and security services, and retail sales such as supermarkets and gas stations. In this group working hours are typically more flexible, work contracts more short-term and wages are more likely to be defined on a hourly rather than monthly basis, institutionally creating less discreteness in their labor market compared to the first group.

Figure 8 shows that the extent of changes in the earnings distribution at the time of the reform are smaller for those students who work in less discrete labor markets (6.14 (*1.71*)) compared to those working in more discrete labor markets (10.94 (*1.10*)). This difference supports our assertions above that discrete labor supply is a key factor explaining our main result.

Importantly, this result suggests that discrete labor supply is not specifically related to students' behavior, but stems from how the labor markets function. The fact that labor markets may create discreteness in employment decisions seems even more important for regular wage earners compared to students. When looking at the labor force as a whole, a significant share of regular wage earners in Finland are employed in labor markets we defined as discrete above (61% of the Finnish work force). This suggests that a discrete model would be a better way to characterize labor supply than the continuous model for a large share of the regular labor force.

Additionally, we use our data on the entire labor force in Finland to elucidate whether typical individual-level changes in earnings are supported by continuous or discrete models. Figure 9 shows the distribution of one-year changes in earnings for regular wage earners in 1999–2013 in Finland. The figure includes wage earners with wage income of at least 30,000 euros in the base year. Figure shows that a large share of one-year changes in earnings are distinctively large. Overall, 44% of all changes in earnings are below 5% in the figure including zero changes. Small non-zero changes could be explained, for example, by changes stemming from centralized wage bargaining that typically results in many workers getting a small raise in the Finnish setting. However, as much as one third of all changes are above 10%, 15% are above 20%. This indicates that a significant proportion of typical annual wage adjustments occur in a manner that is more compatible with discrete changes rather than smooth and continuous adjustments, supporting again the discrete labor supply model. Guvenen *et al.* (2021) find a very similar overall pattern in individual earnings adjustments for US workers, offering further general motivation for the relevance of discrete changes in earnings for wage earners.

## 4.4 Implications on empirical analysis

Discrete labor supply has several implications for reduced-form empirical labor supply analysis. First, discrete labor supply could explain why taxable incomes of wage earners seem to respond to taxes relatively little (see e.g. Martínez *et al.* 2021, Kleven and Schultz 2014, Jacobsen and Søgaard 2022, and Neisser 2021 for a recent meta-analysis). Taxable income of wage earners consists in large part of labor earnings, and discreteness could reduce the extent to which their labor supply responds to changes in incentives. Instead, other groups of taxpayers, such as temporary workers or entrepreneurs, arguably have more flexibility in their decisions regarding income, allowing them to respond to incentives more actively. For example, Martínez *et al.* (2021) find very modest behavioral responses among regular wage earners and an order of magnitude larger responses among the self-employed to a very large cut in income taxes related to the Swiss income tax holiday scheme.

In addition, discreteness influences which methods can reveal the full extent of behavioral responses to taxes. As discuss above in Section 4.1, a fundamental feature of the discrete labor supply model is that the effects of a local tax change are scattered across the distribution, whereas in the standard continuous model the effects are local. Empirical applications that aim at inferring the elasticity of taxable income (ETI) often explicitly or implicitly assume a continuous model, and following from this, focus on analyzing local effects of tax rate changes. For example, many empirical studies utilize tax rate changes targeted at some specific income group using other individuals outside this income range as a control group (see e.g. Gruber and Saez 2002, Kleven and Schultz 2014). However, under discrete labor supply model, taxes affect individuals in other parts of the distribution, and ignoring these responses either misses part of the total response to taxes, or in the worst case, includes in the control group individuals that are actually affected by tax rate changes, creating a downward-bias in ETI estimates. Instead, in the presence of discrete labor supply, empirical methods that enable identifying changes in earnings in different parts of the distribution are preferred over local estimation methods in capturing the true welfare effects of tax and transfer policies. Our empirical approach provides one alternative for such a method, and the empirical literature includes many others (see e.g. Athey and Imbens 2006, Firpo *et al.* 2009, Firpo and Pinto 2016, Cengiz *et al.* 2019).

A specific example of a bias arising from using local estimators when the true effects are more widespread is the widely applied bunching method (Saez 2010, Kleven and Waseem 2013, Kleven 2016), which has already been criticized for various other reasons than the assumed continuous labor supply model (see e.g. Blomquist *et al.* 2021). The bunching method focuses on a narrow window around a kink or notch to estimate behavioral responses to tax incentives. However, any potential responses occurring outside of this window are ignored, producing downward-biased estimates in the presence of discrete labor supply. Also, for the same reason the surrounding density outside the bunching region cannot be used to estimate an unbiased counterfactual describing the shape of the distribution in the absence of a local discontinuity, as this part of the distribution is also affected by the widespread responses to the kink or notch.[18] More broadly, under discrete labor supply the effects of tax rate kinks can be scattered throughout the distribution, and this could provide one explanation for why numerous previous studies tend not to observe local bunching at these kinks among wage earners (see e.g. Saez 2010 and Bastani and Selin 2014).

In our empirical application, we observe distinct bunching at and just below the notch. We can use the standard bunching estimate and the reform revealing more extensive behavioral responses in the distribution to illustrate the problems arising from using the bunching method. Using the reduced-form earnings elasticity formula for income notches presented in Kleven and Waseem (2013), we obtain a local elasticity estimate of 0.065 (*0.007*) for students with 9 subsidy months (see Online Appendix A for more details). In contrast, our approximation for the mobility elasticity estimate (0.18) presented above that captures the broader behavioral responses to the reform is approximately 2.5 times larger (0.18 vs. 0.065). Alternatively, we can compare our estimate for the broader changes in the distribution to the local excess mass estimate derived using the standard bunching approach, which is more than three times larger (9.81 vs. 2.93), as shown in Figure 3.

# 5 Concluding remarks

In this paper, we provide novel reduced-form evidence of distribution-wide changes in earnings as a response to a reform that shifted the location of a notch in the income tax schedule. We find that the reform caused changes in earnings over a broad income range among Finnish university students. We then show that these patterns are consistent with

[18] Note also that in the discrete model the income range just above a notch is not necessarily dominated if individuals make sufficiently large discrete changes in their earnings when responding to tax changes.

a discrete labor supply model instead of a continuous model even when the continuous model is augmented with adjustment frictions that merely reduce responsiveness to taxes.

We argue that our results for higher education students generalize to all wage earners, because students in the Finnish context earn income in the same labor markets as regular wage earners. One could argue that because students are often part-time workers, they have more flexibility in their labor contracts and arrangements and thus less discreteness than regular wage earners who typically have full-time and permanent contracts. We empirically show that more widespread changes in earnings occur among students who work in labor markets that arguably feature more discreteness, characterized by for example monthly instead of hourly wages and longer term work contracts. We also show that regular wage earners having at least medium earnings experience often large changes in their annual earnings consistent with a discrete model.

Discrete labor supply among wage earners would suggest that when assessing the impacts of local tax incentives, it would be important to ensure that all potential responses over the income distribution are included in the analysis. Otherwise the estimates might be substantially downward biased. We present one simple application for this analysis, and the literature contains many others (see e.g. Athey and Imbens 2006). More broadly, discrete labor supply can be an important explanation for the finding that bunching at discontinuities in budget sets and earnings responses to income taxes more generally are often observed to be limited in a growing number of studies (Chetty *et al.* 2011, Martínez *et al.* 2021, Kleven and Schultz 2014, Jacobsen and Søgaard 2022).

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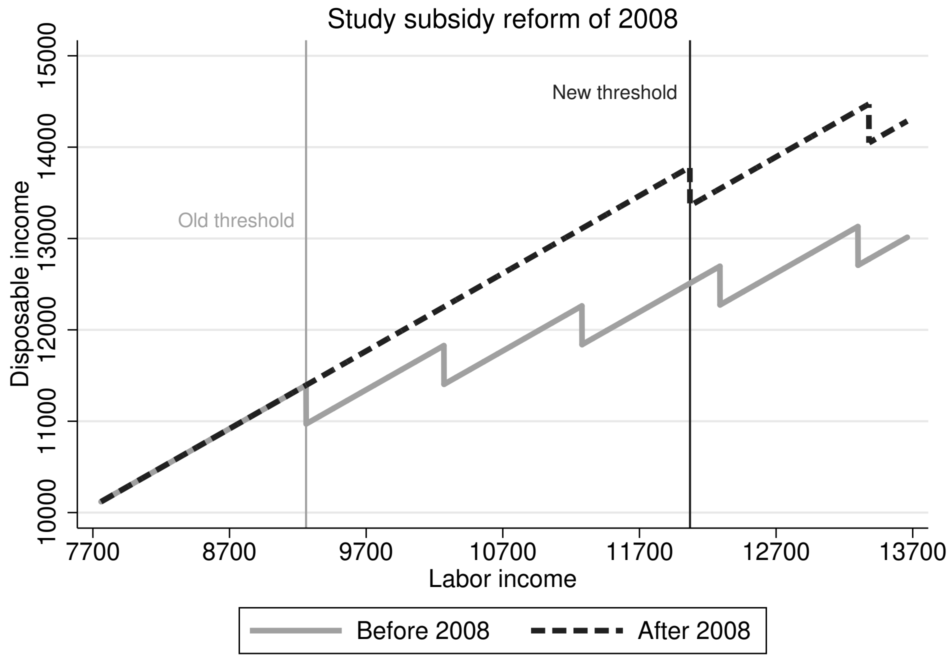
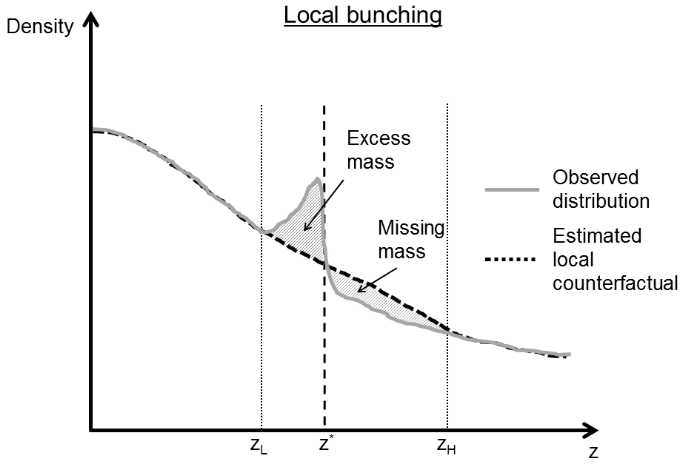
# Figures

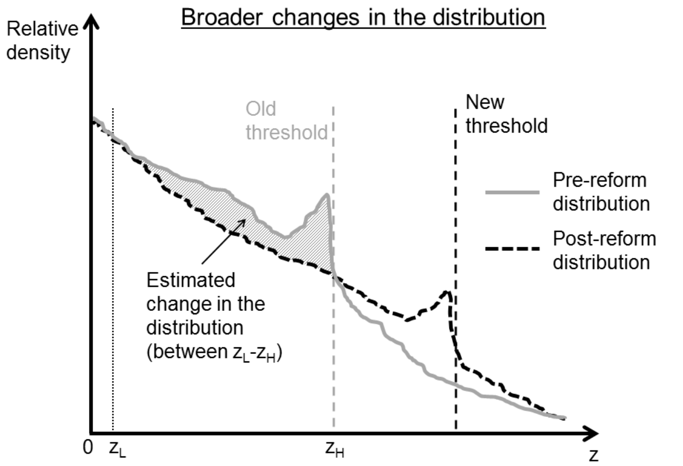



Notes: Figure presents the study subsidy schedule before (gray solid line) and after 2008 (black dashed line) for a student who collects the default 9 subsidy months. The vertical axis denotes disposable income, and horizontal axis labor income. The vertical lines denote the thresholds before (9200 euros) and after (12,070 euros) the 2008 reform. Above the income threshold one month of the study subsidy is reclaimed, resulting in a discontinuous drop in disposable income. An additional month of the subsidy is reclaimed after an additional 1010 and 1310 euros above the threshold before and after 2008, respectively. The figure illustrates the distinctive change in incentives caused by the increase in the income threshold in 2008, highlighting that the reform encouraged students to increase their earnings above the old income threshold.

Figure 1: Disposable income at different income levels for students with 9 subsidy months in 2007 and 2008



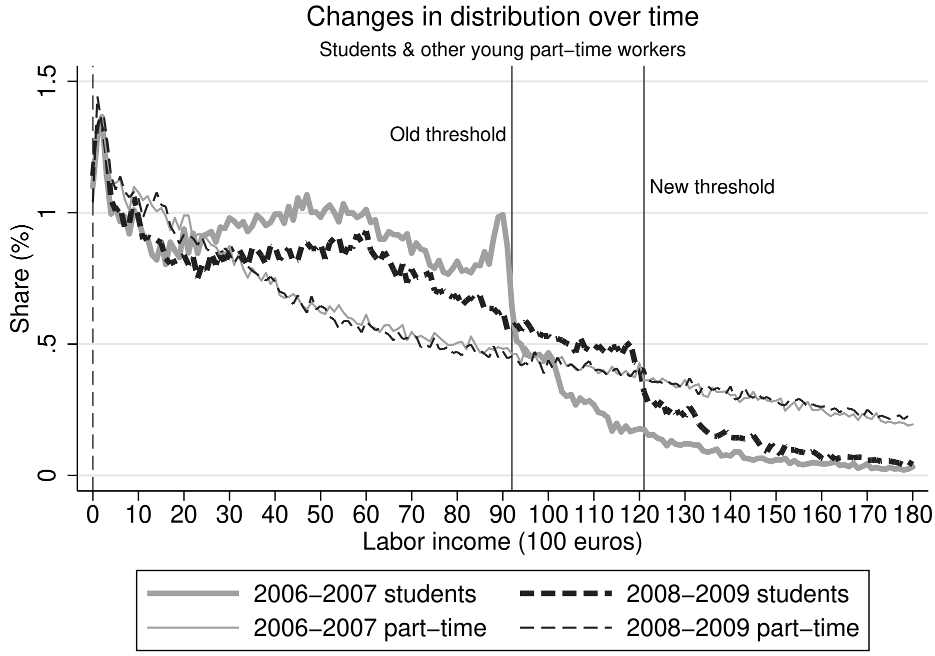

(a) Local bunching




(b) Broader changes in the distribution

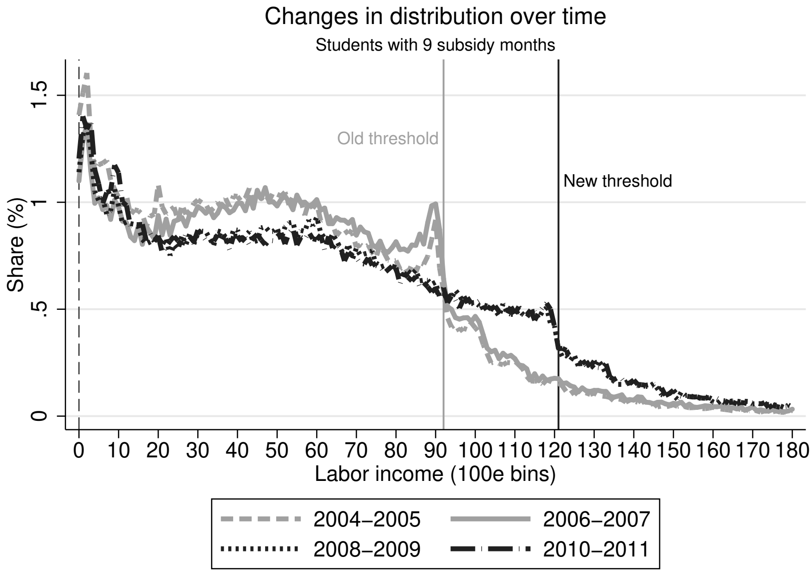
Notes: Graph (a) illustrates the excess bunching at the income threshold in a hypothetical earnings (z) distribution (gray solid line), compared to an estimated counterfactual distribution in the absence of the threshold (black dashed line). In the figure, the threshold is denoted by $z^*$, and $z_L$ and $z_H$ denote the lower and upper limits of the bunching region. The counterfactual is estimated by fitting a flexible polynomial function to the observed distribution excluding the area close to the notch between $z_L$ and $z_H$ from the regression. Excess bunching is estimated by relating the share of individuals in the bunching region $(z_L, z^*)$ to the counterfactual density. See Online Appendix A for more details on the bunching estimation.
Graph (b) illustrates broader changes in a hypothetical earnings distribution after an increase in the location of the threshold. The pre-reform distribution is marked with a gray solid line and the post-reform distribution with a black dashed line. $z_L$ and $z_H$ denote the lower and upper limits of the estimation region. Broader changes in the distribution are estimated by relating the observed relative density before the reform to the relative density after the reform in the income range between the lower and upper limits. See Section 2.3 for more details on the estimation method.

Figure 2: Estimating bunching and broader changes in the earnings distribution

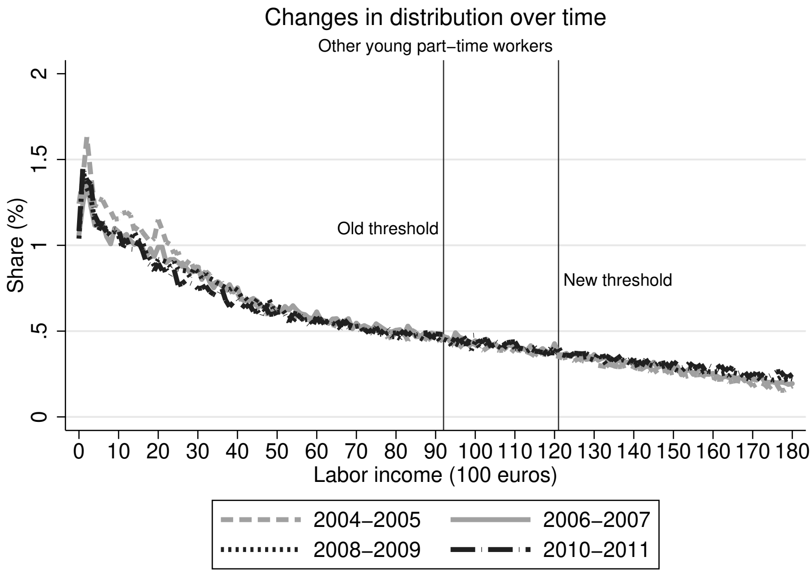



Notes: Figure presents the observed relative earnings distributions before the reform in 2006–2007 (gray solid line) and after the reform in 2008–2009 (black dashed line) within an income range of 0–18,000 euros in bins of 100 euros for students with the default 9 subsidy months in each year, and for young part-time workers who are not students (see Table 2). The first vertical line at 0 denotes the lower limit in the estimation of broader earnings changes in the distribution estimated using equation (2), and the second and third lines denote the pre and post-reform income thresholds, respectively. The figure illustrates that the earnings distribution after 2008 has a clearly different shape than before the reform, implying that the income threshold affects the shape of the whole labor income distribution, not just the region close to the notch point. The differences-in-differences estimate for broader changes in the distribution within an income range of 0–9200 euros is 9.81 (standard error 1.01). The estimate for broader changes among the student population only is 10.97 (1.85), estimated using equation (1). The bunching estimates at the threshold are 2.93 (0.88) before and 1.71 (0.88) after 2008, respectively. A lower limit of 1100 euros below the threshold is used in the bunching estimation both before and after 2008. See Online Appendix A for a more detailed analysis of bunching responses.

Figure 3: Earnings distributions of students and non-student part-time workers before and after the 2008 reform



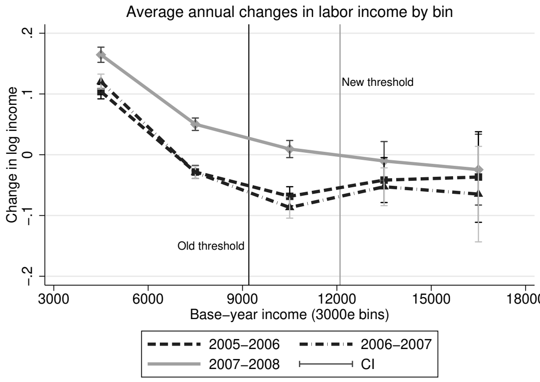

(a) Students



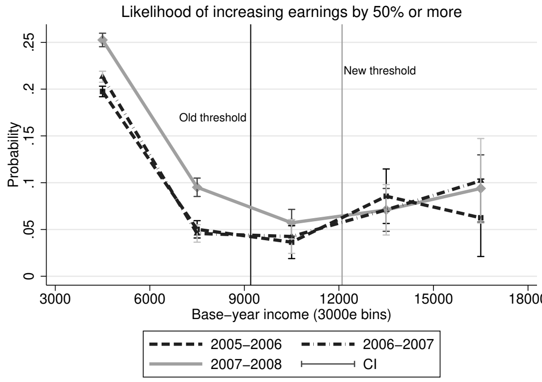

(b) Other young part-time workers

Notes: Figure presents the income distributions of students with 9 subsidy months (graph a) and other young part-time workers (graph b) in 2004–2005 (gray dashed line), 2006–2007 (gray solid line), 2008–2009 (black solid line) and 2010–2011 (black dotted line) within an income range of 0–18,000 euros in bins of 100 euros. The figure shows that the earnings responses of students occurred exactly at the time of the 2008 reform, and that the response is not caused by any gradual changes in the shape of the distribution over time. The distribution for other young part-time workers remained almost unchanged throughout the time period 2004–2011. However, there are similar minor changes at the bottom of distributions of both the treatment and control groups from 2004–2005 to 2006-2007, which further strengthens the case that the distributions develop similarly over time in the absence of the reform.

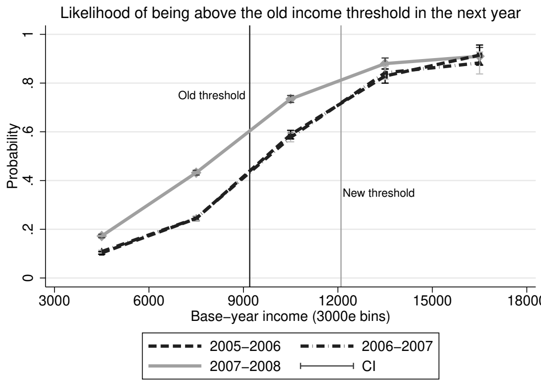
Figure 4: Income distributions of students and other young part-time workers in 2004–2011




(a) Average changes in labor income



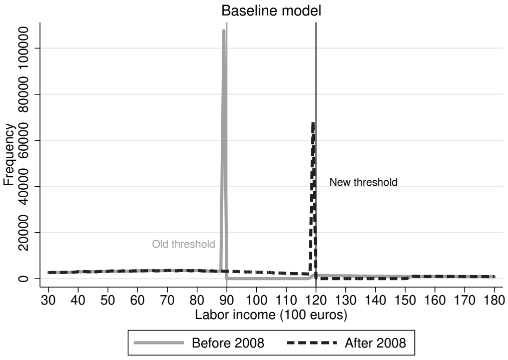

(b) More than 50% earnings increases



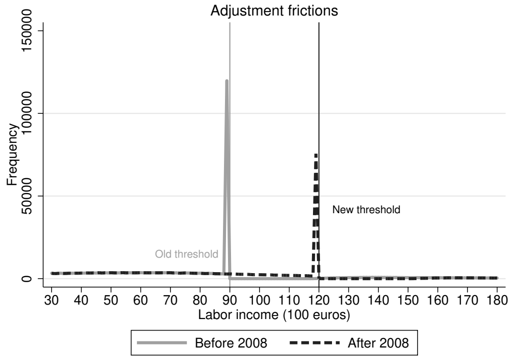

(c) Likelihood of locating above the old income threshold

Notes: Graph (a) presents the average individual-level changes in real log labor income (relative to the 2007 real price index) with 95% standard errors in base-year bins of 3000 euros for students with 9 subsidy months. Gray solid line represents the years 2007–2008, and black dashed lines the pre-reform years 2005–2006 and 2006–2007. The graph shows that earnings increases are more prevalent below the new threshold at the time of the reform compared to previous years, but there are no significant differences above the new income threshold. Graph (b) presents the average likelihood and 95% standard errors for increasing labor income by 50% or more relative to base-year income. The graph illustrates that the likelihood of large income increases is significantly higher below the old threshold at the time of the reform compared to previous years, but there are no significant changes above the old threshold between the years. Graph (c) presents the average likelihood and 95% standard errors for locating above the old income threshold in the next year. The graph shows that this likelihood increased significantly in bins below the new threshold, but there are no significant changes between the years at larger income levels. Overall, these findings support the view that students responded to the relocation of the notch with large intensive-margin earnings increases instead of marginal earnings adjustments along the whole distribution.

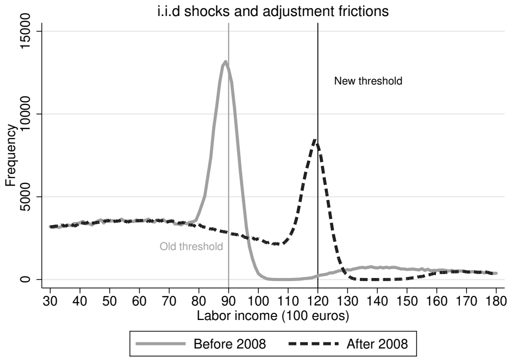
Figure 5: Panel data evidence of individual-level earnings responses




(a) Baseline continuous model



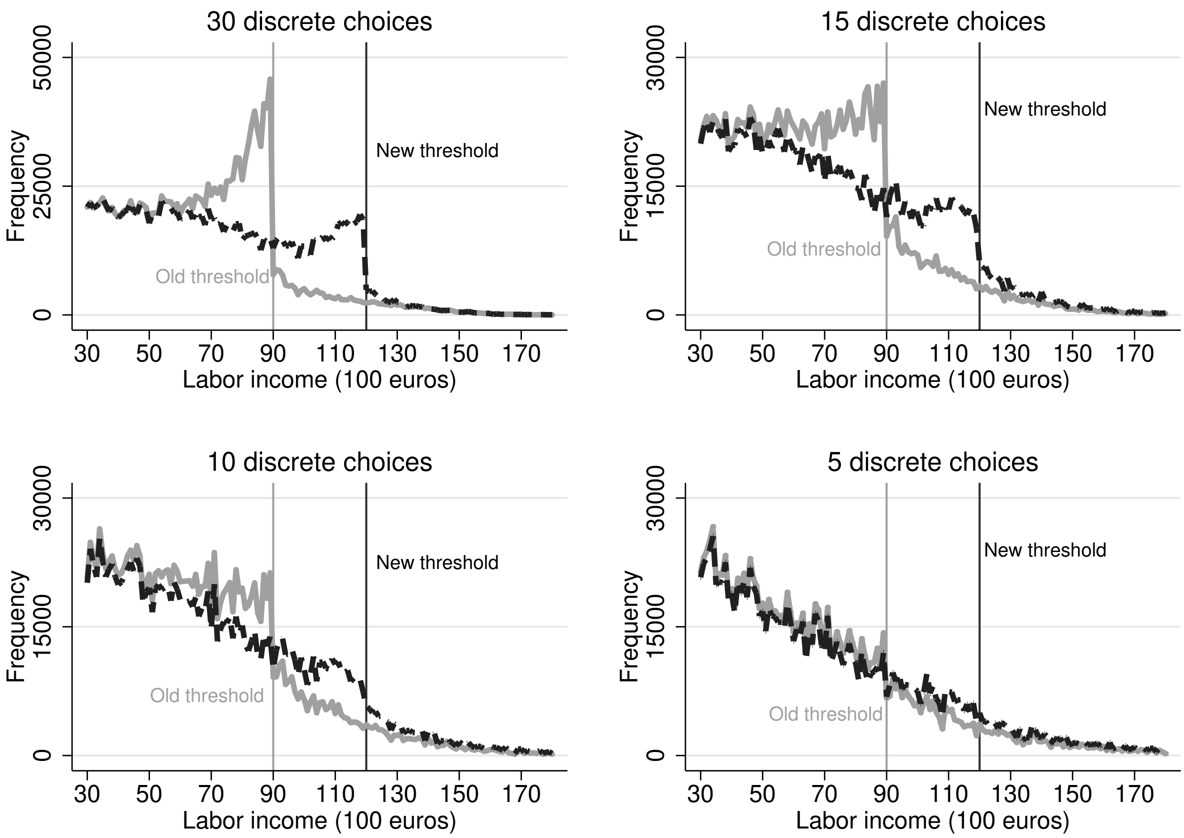

(b) Adjustment frictions




(c) Earnings shocks and adjustment frictions

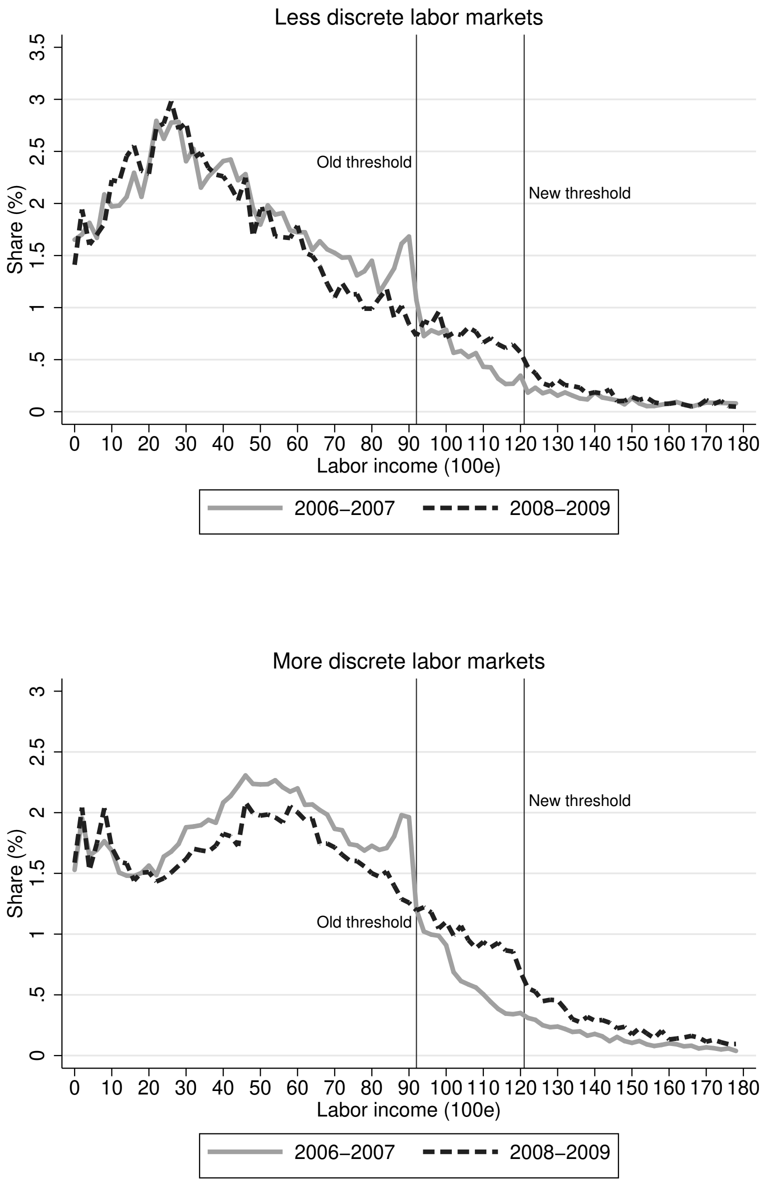
Notes: Figure presents simulated income distributions before (gray solid line) and after (black dashed line) an increase in the location of the income threshold from 9000 euros to 12,000 euros within an income range of 0–18,000 euros. The underlying $e$ parameter of 0.2 is used in the simulations. Qualitative results are not sensitive to the choice of this parameter value, except that with higher parameter values the densities above the thresholds reduce. Graph (a) presents the standard continuous-choice model. Graph (b) presents the standard model with adjustment frictions and graph (c) includes both adjustment frictions and unexpected i.i.d shocks in earnings to the standard model. We assume heterogeneous adjustment frictions represented by a uniformly distributed parameter $a$ in the unit interval. Each individual has a different and independent draw from this distribution. The earnings shocks related to optimization errors are normally distributed mean-zero income shocks with a standard deviation of 800 euros in the simulations. The simulation model is introduced in more detail in Online Appendix B. The graphs illustrate that these frictions typically discussed in the literature can induce mitigated and scattered bunching around the threshold, but they do not produce broader changes in the earnings distributions we observed in Figure 3.

Figure 6: Simulated income distributions in the baseline continuous model and with different types of adjustment frictions




Notes: Figure presents simulated earnings distributions before (gray solid line) and after (black dashed line) an increase in the location of the income threshold from 9000 euros to 12,000 euros within an income range of 0–18,000 euros using different options for an available discrete earnings choice set. The underlying $e$ parameter of 0.5 is used in the simulations. Qualitative results are not sensitive to the choice of this parameter value. Using 30 location choices produces distinctive bunching at the threshold, and limited changes in the distribution at lower income levels. In contrast, using 15 or 10 choices produces more limited bunching and more prevalent responses at lower income levels, in a qualitatively similar manner as in Figure 3. However, using only 5 available choices reduces both local responses and broader changes in the distribution, which is inconsistent with our empirical observations.

Figure 7: Simulated income distributions with different discrete choice sets




Notes: Figure presents the observed relative earnings distributions before the reform in 2006–2007 (gray solid line) and after the reform in 2008–2009 (black dashed line) within an income range of 0–18,000 euros in bins of 100 euros for students with the default 9 subsidy months in each year working in different types of jobs. Jobs are categorized using firm-level industry classification codes. Less discrete labor markets include restaurants, bars and cafes, cleaning and security services, and retail sales such as supermarkets and gas stations. More discrete labor markets include public sector, and research, manufacturing, construction and maintenance in the private sector. Using equation (1), the estimate for broader changes in the distribution within an income range of 0–9,200 euros for the less discrete group is 6.14(1.71), and for the more discrete group 10.94(1.10), illustrating that broader changes in the distribution are significantly more prevalent for the latter group compared to the first group.

Figure 8: Labor income distributions before and after 2008 for students working in less discrete and more discrete labor markets


Notes: Figure presents the distribution of one-year changes in wage income in 1999–2013 for wage earners with real wage income (in 2007 terms) of at least 30,000 euros in the base-year. The figure denotes the absolute value of the change, thus including both negative and positive changes in earnings. The figure is restricted to include all changes below 100%, excluding 0.2% of all one-year changes that are larger than that.

Figure 9: The distribution of one-year changes in earnings for regular wage earners (earning at least 30,000 euros in the base year)

# Tables

Table 1: Descriptive statistics, all students 1999–2013

| | Individual characteristics | | | |
|---|---|---|---|---|
| | Age | Female | Labor income | Labor income > 500 |
| Mean | 23.8 | .58 | 8446 | .80 |
| Median | 23 | 1 | 6306 | 1 |
| sd | 4.23 | .49 | 8197 | .40 |
| N | 2,417,517 | 2,417,517 | 2,078,538 | 2,417,517 |
| | One employer | Study subsidy months | 9 subsidy months | Years studied |
| Mean | .55 | 8.02 | .42 | 2.2 |
| Median | 1 | 9 | 0 | 2 |
| sd | .50 | 2.64 | .49 | 1.80 |
| N | 1,863,702 | 2,417,517 | 2,417,517 | 2,098,485 |
| | **Field of industry** | | | |
| | Manufacturing | Hospitality services | Admin. & Public Sector | Other/missing |
| Mean | .19 | .16 | .39 | .25 |
| sd | .40 | .37 | .49 | .43 |
| N | 2,417,517 | 2,417,517 | 2,417,517 | 2,417,517 |
| | **Field of study** | | | |
| | Arts & Humanities | Business & Soc. Science | Tech., Health & Soc. Serv. | Other/missing |
| Mean | .17 | .19 | .34 | .29 |
| sd | .38 | .39 | .47 | .42 |
| N | 2,417,517 | 2,417,517 | 2,417,517 | 2,417,517 |

Notes: Table presents the descriptive statistics for all students in 1999–2013, excluding first-year students and those who graduate within a given year. Labor income > 500 denotes the share of students with annual labor income above 500 euros. One employer denotes the share of students who we observe to work for only one employer within a year among those with information on the employer in the data. 9 subsidy months denotes the share of students with the default study subsidy choice.

Table 2: Descriptive statistics, non-student part-time workers, 1999–2013

| | Individual characteristics | | | | |
|---|---|---|---|---|---|
| | Age | Female | Labor income | Labor income > 500 | One employer |
| Mean | 21 | .56 | 8318 | .93 | .62 |
| Median | 21 | 1 | 6741 | 1 | 1 |
| sd | 1.710 | .496 | 7229 | .25 | .48 |
| N | 940,786 | 940,786 | 932,572 | 940,786 | 940,786 |
| | **Field of industry** | | | | |
| | Manufacturing | Hospitality services | Admin. & Public Sector | Other/missing | |
| Mean | .31 | .22 | .41 | .06 | |
| sd | .46 | .41 | .49 | .24 | |
| N | 940,786 | 940,786 | 940,786 | 940,786 | |

Notes: Table presents the descriptive statistics for young, non-student part-time workers used in our analysis. The group of non-student part-time workers is selected to roughly match students' job and age characteristics. The non-student group comprises individuals who we observe to have less than 12 working months per year in the data, and who are 19–24 years old. The age interval is chosen to match between the 25–75 percentile points of the students' age distribution. Labor income > 500 denotes the share of individuals with annual labor income above 500 euros. One employer denotes the share of individuals who we observe to work for only one employer within a year among those with information on the employer in the data.

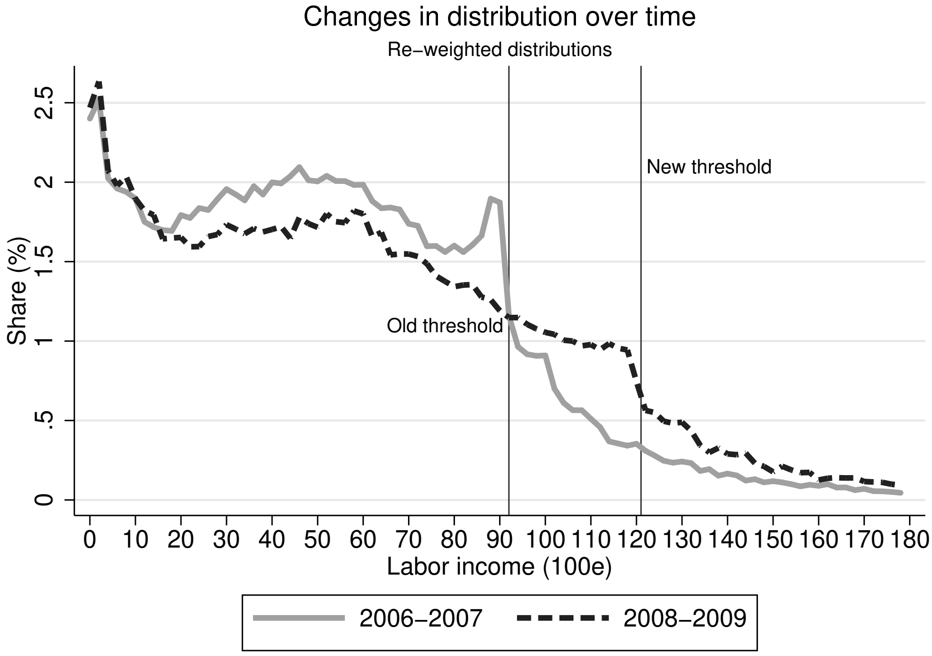
Table 3: Parameter values in the simulation model for the income threshold reform

| Parameter | Value |
|---|---|
| *Marginal tax rate ($\tau$)* | |
| Below the notch | 0.22 |
| Above the notch | 0.61 |
| Size of the notch | 500e |
| *Virtual income ($R$)* | |
| Before | 4100e |
| After | 3600e |
| *Location of the notch (income threshold)* | |
| Before | 9000e |
| After | 12,000e |

Notes: Table presents the parameter values used in the simulation model. The parameter values are selected to approximate the actual budget set faced by students under the study subsidy program (see Figure 1).

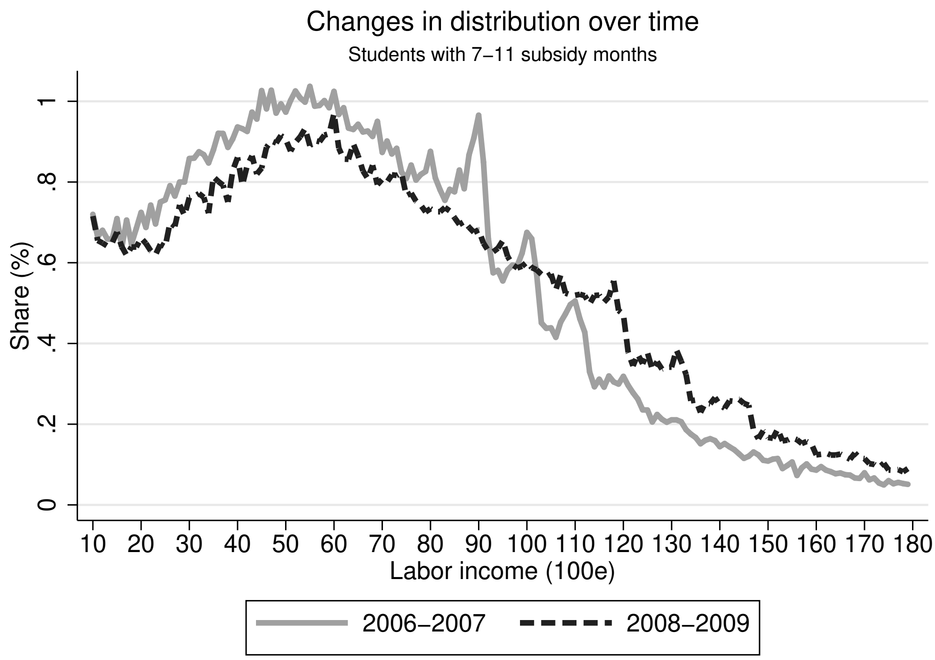
# Appendix

## Figures



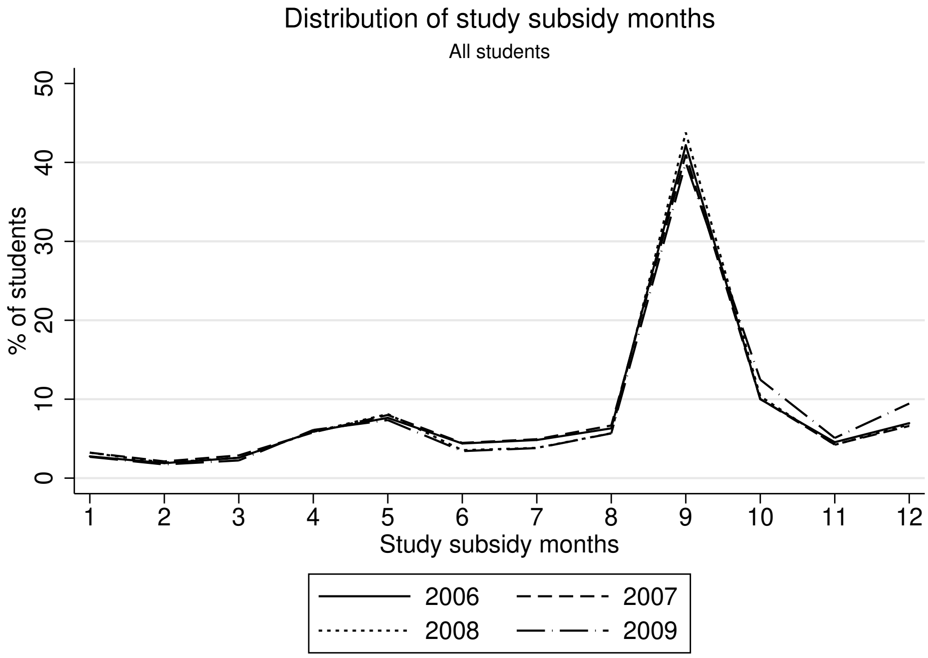

Notes: Figure presents the re-weighted relative earnings distributions before the reform in 2006–2007 (gray solid line) and after the reform in 2008–2009 (black dashed line) within an income range of 0–18,000 euros in bins of 200 euros for students with the default 9 subsidy months in each year. Bin-level inverse probability weighting is used to re-weight the annual distributions using 2006 as the base year. The re-weighting procedure utilizes four groups for both the field of industry and field of study, and three age groups based on age terciles. Using equation (1), the estimate for broader changes in the distribution within an income range of 0–9200 euros is 11.40 (1.01), which is similar to that estimated in the baseline case in Figure 3 in the main text.

Figure 10: Re-weighted income distributions in 2006–2007 and 2008–2009.




Notes: Figure presents the observed relative earnings distributions before the reform in 2006–2007 (gray solid line) and after the reform in 2008–2009 (black dashed line) within an income range of 0–18,000 euros in bins of 200 euros for students with 7–11 subsidy months. The figure shows that broader changes in the distribution are prevalent when including students who deviate from the default choice of 9 subsidy months. However, as the number of subsidy months defines the location of the income threshold, changes in the distribution are more scattered over the distribution compared to our baseline case with 9 subsidy months in Figure 3 in the main text. Also, a fraction of students who choose other than 9 subsidy months bunch at their associated income thresholds, which appear as additional spikes in the distribution. As the location of the thresholds both before and after 2008 is not constant in this population, we cannot estimate a measure for broader changes in the distribution for this population following the procedures introduced in Section 2.3.

Figure 11: Income distributions in 2006–2007 and 2008–2009, students with 7–11 subsidy months.




Notes: Figure presents the distribution of study subsidy months in 2006, 2007, 2008 and 2009. In each year, the default 9 months of the subsidy is the most common choice. There are no significant changes in the distribution over time. This indicates that students responded to the reform of 2008 by changing their earnings, but not, on average, by claiming more or less subsidies per year.

Figure 12: Distributions of study subsidy months, 2006–2009.

## Tables

Table 4: Income thresholds before and after the 2008 reform

| | **Before 2008 (academic year 2006/2007)** | | **After 2008 (academic year 2008/2009)** | |
|---|---|---|---|---|
| Study subsidy months | Income threshold | Relative income loss at the margin if the threshold is exceeded | Income threshold | Relative income loss at the margin the threshold is exceeded |
| 1 | 17,340 | 3.1% | 22,550 | 2.5% |
| 2 | 16,330 | 3.2% | 21,190 | 2.7% |
| 3 | 15,320 | 3.5% | 19,930 | 2.9% |
| 4 | 14,310 | 3.7% | 18,620 | 3.1% |
| 5 | 13,300 | 4.0% | 17,310 | 3.3% |
| 6 | 12,290 | 4.3% | 16,000 | 3.6% |
| 7 | 11,280 | 4.7% | 14,690 | 3.9% |
| 8 | 10,270 | 5.2% | 13,380 | 4.3% |
| **9** | **9260** | **5.7%** | **12,070** | **4.8%** |
| 10 | 8250 | 6.4% | 10,760 | 5.3% |
| 11 | 7240 | 7.3% | 9450 | 6.1% |
| 12 | 6230 | 8.5% | 8140 | 7.1% |

Note: Table presents the annual income thresholds in euros for different subsidy months before and after the 2008 reform. The highlighted 9 months of the subsidy is the default choice. The relative income loss from marginally exceeding the income threshold is calculated using the full study subsidy (461 euros and 500 euros before and after 2008, respectively) plus 15% interest collected by the Social Insurance Institution if the subsidy is reclaimed due to exceeding the income threshold.

Table 5: Variables used in the mobility elasticity estimation for students

| | Avg. gross earnings (2000—18,000e) | Avg. net income below notch (2000–9300e) | Avg. net income above notch (9300–18,000) | Differences in net incomes between avg. locations |
|---|---|---|---|---|
| 2007 | 7116 | 8693 | 12,173 | 3534 |
| 2008 | 7529 | 8785 | 13,592 | 4807 |
| | | Gross earnings below: 6008 | Gross earnings above: 11,821 | |

Notes: Table presents the variables used when calculating the mobility elasticity estimate for students in Section 4.2 in the main text. Mobility elasticity is measured by relating the log change in average gross earnings to the log change in the difference in net income between the two average earnings locations below and above the original notch. Net earnings are calculated using the SISU microsimulation model.

# Online Appendix

## Online Appendix A

### Bunching estimation

Behavioral responses to local discontinuous changes in the budget set, such as tax rate kinks or notches, are predominantly estimated in the recent literature using a bunching methodology (see Kleven 2016 for a summary). Intuitively, if a discontinuous jump in incentives affects earnings, we should find an excess mass of individuals located just below the threshold in the earnings distribution. The excess bunching thus captures the earnings distortions created by the threshold in the absence of optimization frictions and when earnings choices are continuous. Saez (2010) and Kleven and Waseem (2013) show that under certain restrictions and within the continuous labor supply model, the bunching estimate can be translated into an average earnings elasticity, representing a relevant parameter for the welfare analysis of taxes and income transfers.

We measure local responses to the notch caused by the income threshold following the bunching method presented in Kleven and Waseem (2013). The local counterfactual density is estimated by fitting a flexible polynomial function to the observed distribution, excluding an area around the study subsidy income threshold $z^*$ from the observed income distribution. We group students into income bins of 100 euros and then estimate a counterfactual density by excluding the region $[z_L, z_H]$ around the threshold from the regression:

$$c_j = \sum_{i=0}^{p} \beta_i (z_j)^i + \sum_{i=z_L}^{z_H} \eta_i \cdot \mathbf{1}(z_j = i) + \varepsilon_j \tag{6}$$

where $c_j$ is the count of individuals in bin $j$, and $z_j$ denotes the income level in bin $j$. The order of the polynomial is denoted by $p$. Thus the fitted values for the counterfactual density are given by $\hat{c}_j = \sum_{i=0}^{p} \beta_i (z_j)^i$. The excess bunching is then estimated by relating the actual number of students close to the threshold within $(z_L, z^*)$ to the estimated counterfactual density in the same region:

$$\hat{b}(z^*) = \frac{\sum_{i=z_L}^{z^*} (c_j - \hat{c}_j)}{\sum_{i=z_L}^{z^*} \hat{c}_j / N_j} \tag{7}$$

where $N_j$ is the number of bins within $[z_L, z^*]$.

Following Kleven and Waseem (2013), we set the lower limit of the excluded region ($z_L$) based on visual observations of the income distribution to represent the point in the distribution where the bunching behavior begins, i.e. when the density begins to increase. We determine $z_H$ such that the estimated excess mass, $\hat{b}_E(z^*) = (\sum_{i=z_L}^{z^*} c_j - \hat{c}_j)$, equals the estimated missing mass above the threshold, $\hat{b}_M(z^*) = (\sum_{i=z>z^*}^{z_H} \hat{c}_j - c_j)$, stemming from individuals who would locate above the income threshold in the absence of it and who respond to the notch by bunching below it, illustrated in Figure 2 in the main text. We apply this convergence condition by starting from a small value of $z_H$ and increasing it gradually until $\hat{b}_E(z^*) \approx \hat{b}_M(z^*)$. This convergence condition also defines the marginal buncher student with income $z^* + \triangle z$, representing the student with highest earnings in the absence of the notch who responds by locating below the income threshold.

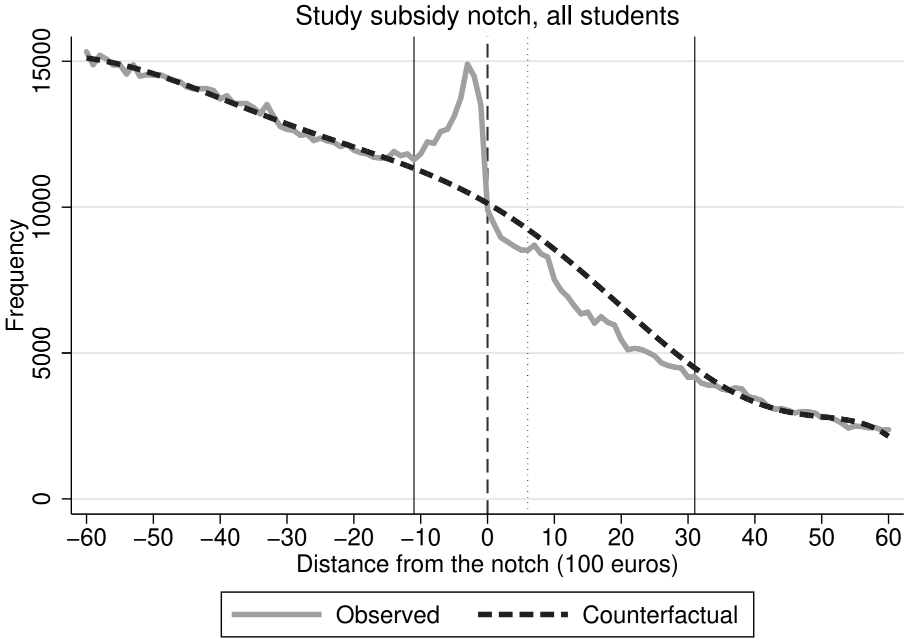
Following Kleven and Waseem (2013), we calculate standard errors by using a residual-based bootstrap procedure. We generate a large number of income distributions by randomly resampling the residuals from equation (6) with replacement, and generate a large number of new estimates of the counterfactual density based on the resampled distributions. Based on the bootstrapped counterfactual densities, we evaluate variation in the bunching estimate. The standard error is defined as the standard deviation in the distribution of the estimate.

## Bunching responses

We find clear local responses to the income threshold of the study subsidy program. Figure A1 presents the gross income distribution and the counterfactual distribution relative to the notch in bins of 100 euros in the range of +/- 6000 euros from the notch in 1999–2013. The dashed vertical line denotes the notch point above which a student loses one month of the subsidy. The solid vertical lines denote the excluded range used in the estimation of the counterfactual, which is estimated using a 7th-order polynomial function. The dash-point vertical line above the notch shows the upper limit for the dominated region just above the notch where students can increase their net income by lowering their gross income subject to the income threshold.

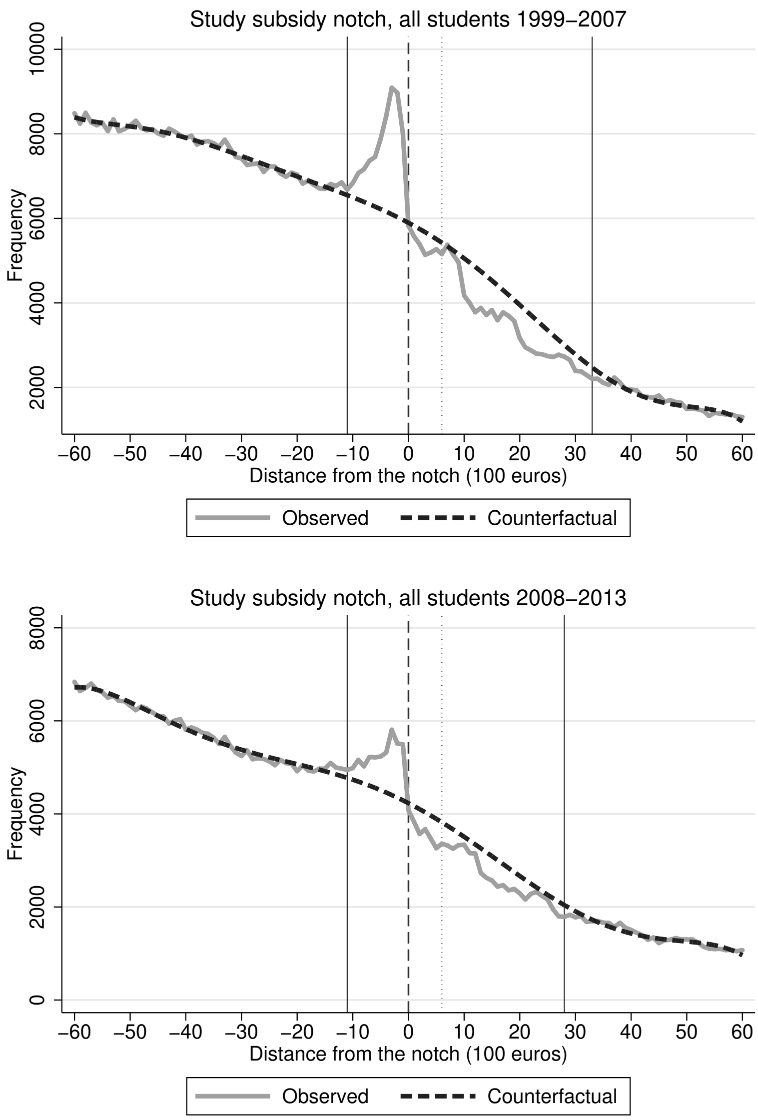
Figure A1 indicates a visually clear and statistically significant excess mass (2.19 (*0.189*)) below the income threshold for all students (standard error in parenthesis). This implies that students are both aware of the notch and respond to the strong local incentives created by it. In addition, there is clear evidence of the existence of some types of frictions. There is an economically and statistically significant mass of students, 0.915(*.027*) of the mass relative to the counterfactual, at the locally dominated region just above the notch where no students should locate in the absence of any types of frictions or constraints and when earnings choices are continuous (Kleven and Waseem 2013). Furthermore, even though the study subsidy schedule ultimately consists of multiple notches, we observe a distinctive response only to the first income threshold they face.

Figure A2 shows the bunching responses before (1999–2007) and after (2008–2013) the 2008 reform. The figure shows that excess bunching is slightly larger before (2.55 (*0.228*)) than after (1.71 (*0.882*)) the reform. One explanation for this is that the incentives not to exceed the income threshold are somewhat smaller after 2008, since the relative significance of losing one month's subsidy in terms of disposable income is now smaller than before 2008 when the threshold was at a lower income level. However, as discussed in Section 2 in the main text, this standard bunching method is not a valid measure for estimating labor supply responses to tax incentives under the discrete labor supply model, and therefore these estimates need to be interpreted as suggestive.




Notes: Figure presents the observed earnings distribution (gray solid line) and the estimated counterfactual distribution (black dashed line) around the income threshold (denoted by zero in the figure) in bins of 100 euros for all students using pooled data from 1999–2013. The first and second solid vertical lines denote the lower and upper limits of the excluded region when estimating the counterfactual distribution. The counterfactual is estimated using a seventh-order polynomial. The dotted vertical line denotes the upper limit of the region of dominated choice just above the threshold. The estimate for excess bunching at the notch is 2.19 (0.189), and the estimate for the mass at the dominate region is 0.915 (0.027).

Figure A1: Bunching at the study subsidy notch, 1999–2013




Notes: Figure presents the observed earnings distributions (gray solid line) and the estimated counterfactual distributions (black dashed line) around the income threshold (denoted by zero in the figure) in bins of 100 euros for all students before (1999–2007) and after (2008–2013) the 2008 threshold reform. The first and second solid vertical lines in the figure denote the lower and upper limits of the excluded region when estimating the counterfactual distribution. The counterfactual is estimated using a seventh-order polynomial. The dotted vertical line denotes the upper limit of the region of dominated choice just above the threshold. The estimate for excess bunching at the notch before 2008 is 2.55(0.228) and 1.71(0.882) after the reform.

Figure A2: Bunching at the study subsidy notch: Before and after the 2008 reform

### Earnings elasticity estimates

We approximate the earnings elasticity at the study subsidy notch using a similar approach as Kleven and Waseem (2013). We derive an upper-bound reduced-form earnings elasticity by relating the earnings response of a marginal buncher student at $z^H$ to the implicit change in tax liability between the notch point $z^*$ and $z^H$ (see Figure 2 in the main text). The marginal buncher represents the individual with the highest income to move to the notch point, compared to a counterfactual state in the absence of the notch. Intuitively, this approach treats the notch as a hypothetical kink which creates a jump in the implied marginal tax rate. More formally, the reduced-form earnings elasticity is calculated with a quadratic formula

$$e(z^*) \approx (z^H/z^*)^2/(\triangle t/(1-t)) \tag{8}$$

where $(1 - t)$ is the net-of-tax rate at the notch, and $\triangle t$ defines the change in the implied marginal tax rate for the marginal buncher (Kleven and Waseem 2013). We include all the income tax and benefit rules and use the SISU microsimulation model to calculate the implied marginal tax rates for the students in the estimation.

The implied earnings elasticities are 0.083 (*0.019*) for all students and 0.065 (*0.007*) for students with 9 subsidy months. Nevertheless, as discussed above, the bunching method does not capture all earnings responses when the earnings choices are discrete, and therefore these estimates do not represent the true earnings elasticity of students.

# Online Appendix B

In this Appendix we first present theoretical models formalizing optimization frictions in more detail, and discuss whether or not these models can explain our empirical findings in Section 3 in the main text. Then we introduce our simulation model that we use in Section 4.1 in the main text to illustrate that a discrete model as opposed to any variant of a continuous model qualitatively matches our empirical findings.

## Theoretical models on optimization frictions

The canonical continuous model includes a standard utility function $u(c, z)$, where $c$ denotes consumption (net earnings) and $z$ gross earnings, with properties $u_c>0$ and $u_z<0$. The budget set is $c = (1-\tau)z + R$, where $(1-\tau)$ is the net-of-tax rate and $R$ is virtual income. We abstract from income effects following the earlier literature (Saez *et al.* 2012). Including income effects would not change the main results in qualitative terms but would complicate the formulas.

For simplicity and to illustrate transparently how certain extensions modify the model, we parameterize the utility function to a quasi-linear form often used in the earlier literature:

$$u(c, z) = c - \frac{w^i}{1+\frac{1}{e}} \left(\frac{z}{w^i}\right)^{1+\frac{1}{e}}, \tag{9}$$

where $w^i$ is an ability (productivity) parameter of individual $i$ over which individuals are heterogeneous such that there is some underlying distribution of abilities. Thus, the utility maximization with respect to $z$ gives the optimal choice for an individual, $z^* = w^i (1-\tau)^e$, where $e$ is the earnings elasticity parameter with respect to $\tau$. Thus, the earnings choices are governed by innate productivity $w^i$, marginal tax rate $\tau$, and $e$ parameter.

We want to understand how this model would explain the observed changes in the income distribution following an upward shift in the location of an income threshold, such as in the reform we study. If individual's optimal choice is well below the original location of the notch, they will not respond at all to this kind of a reform in the continuous model. This is because none of the parameters determining the individual's originally chosen location has changed, including $\tau$. Therefore, this model cannot explain the responses in income levels reaching far below the original location of the notch we observed empirically. Note that if the initial location was at the notch, this model would predict those individuals shifting upwards as a response to the reform. This is because individuals would have bunched at the notch in response to the discontinuous incentives, and the reform would have removed the disincentive to be located at higher income levels.

Following Kleven and Waseem (2013), optimization frictions that simply mitigate or hamper responses to taxes can be included in the parameterized continuous model by adding to the utility function a heterogeneous friction parameter $a \in (0, 1)$. If $a$ is close to one on average, frictions are high and average individual responses to taxes would be heavily restricted, and if $a$ is zero, responses to taxes would follow the standard continuous model above. We assume that $a$ enters the parametrized utility function as follows:

$$u(c, z) = c - \frac{w^i}{1+\frac{1}{e(1-a)}} \left(\frac{z}{w^i}\right)^{1+\frac{1}{e(1-a)}}. \tag{10}$$

From the above equation it becomes clear that these types of optimization frictions merely reduce the responsiveness to taxes, but they do not alter individual responses in a more fundamental manner. In particular, this modification does not alter the above consideration of whether the modified model explains the empirically observed changes in the distribution following the reform. It continues to apply that individuals located below the original location of the notch would not consider responding to the reform because the parameters defining their earnings location remain unchanged.

We can further alter the canonical framework by adding optimization errors to the model arising from an unanticipated shock to the initially chosen income. A simple approach to including optimization errors is to consider an error parameter drawn from some distribution $r \in f(r)$ that alters the optimized income $z^*$ into $z^* - r$. If individuals have an expectation of the shock or are risk-averse, they could respond to the expectation but not to the realized shock. These kinds of frictions would typically cause only small deviations in income (depending of course on the size of the shock), but again would not induce individuals located far below the notch originally to respond to the reform.

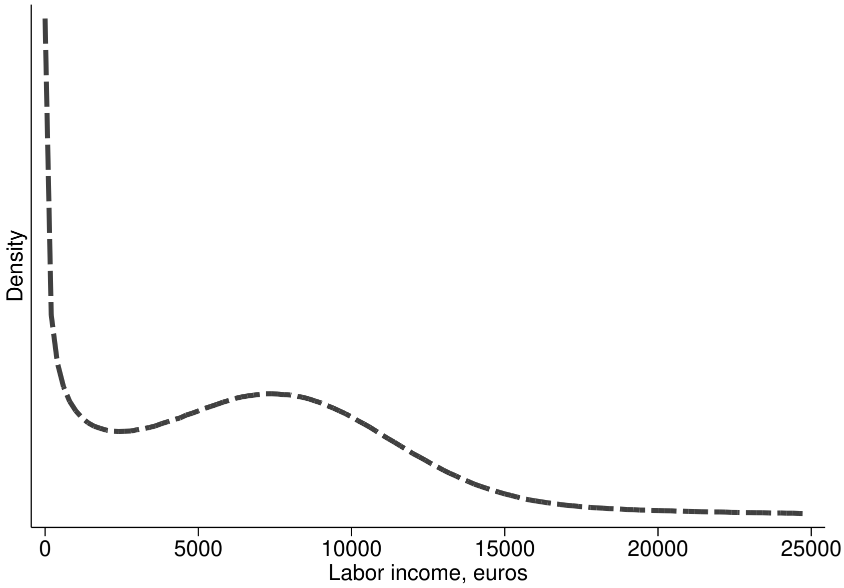
### Simulation model

We build our simulation model on the theoretical framework presented in Section 4 in the main text. The individual utility function is given in equation (9), where the $e$ parameter governs the disutility from earnings supply and would correspond to the elasticity with respect to taxes in the continuous model. The discrete model has the same utility function but adds a fixed number of discrete earnings choices to the individual decision problem as an additional constraint. The budget set for individuals arises from the tax system used in the simulations, which we discuss in the main text.

The model assumes an underlying ability distribution from which each individual $i$ receives a predetermined draw $w_i$. This draw represents earnings in the absence of responses to the tax system. Our parameterized ability distribution is presented in Figure B1. The distribution is a combination of power distributions and normal distributions, which gives an approximate match for the shape of the empirical earnings distribution of students in our empirical case. Our results are not sensitive to different underlying ability distributions that roughly match the empirical income distribution of students.

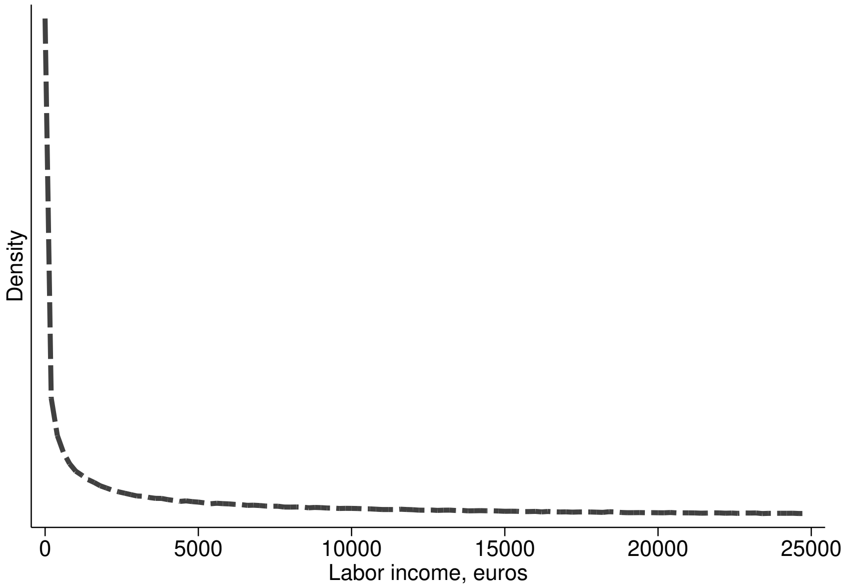
The available discrete earnings locations for each individual are drawn from the probability distribution presented in Figure B2. The number of choices drawn can be altered in different specifications and the draws vary between different individuals. Therefore, even when the individual-level choices are discrete, the overall earnings distribution is smooth.

In the simulation model including adjustment frictions, we assume heterogeneous adjustment frictions represented by a uniformly distributed parameter $a$ in the unit interval. Each individual has a different and independent draw from this distribution. The earnings shocks related to optimization errors are normally distributed mean-zero income shocks with a standard deviation of 800 euros in the simulations. Note that if we were to assume only negative income shocks we would obtain diffuse bunching only below the notch, similarly as in the empirical distribution. However, such asymmetric shocks cannot be not easily justified.




Notes: Figure presents the underlying earnings distribution used in the simulation model. The distribution is a combination of a power distribution and a normal distribution, which delivers an approximate match for the shape of the empirical earnings distribution of students in our empirical analysis. The simulation results are not sensitive to different underlying ability distributions that roughly match the empirical income distribution of students.

Figure B1: Simulated earnings distribution in the absence of taxes




Notes: Figure presents the underlying probability distribution of discrete earnings choices utilized in the discrete choice model simulations. The large mass in the probability distribution at small earnings ensures that each individual has at least one available choice that produces positive utility with positive earnings. The thick tail in the distribution ensures that there is another available choice at a higher income level, although the specific location of this choice can vary across different draws. In the simulation procedure, we iterate the model multiple times, and in each round draw new available earnings choices. The resulting earnings distribution for the full population is continuous, although one individual faces only a discrete and limited number of available choices.

Figure B2: Probability distribution of discrete earnings choices