



Take-up and labor supply responses to disability insurance earnings limits[☆]

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ABSTRACT

In most disability insurance programs beneficiaries lose some or all of their benefits if they earn above an earnings threshold. While intended to screen out applicants with high remaining working capacity, earnings limits can also distort the labor supply of beneficiaries. We use a reduction in the earnings limit in Hungary to evaluate this trade-off and examine screening and labor supply responses. We find that the policy changed selection into the program modestly but reduced labor supply on the intensive margin significantly. These findings suggest that the earnings threshold should be higher.

1. Introduction

The share of working-age adults receiving long-term disability insurance (DI) benefits has increased rapidly over the last few decades and DI programs account for over 10% of social spending in OECD countries (OECD, 2010). The trend in disability rolls raises concerns about the fiscal sustainability of DI programs and has prompted policy makers to examine program designs that encourage potential beneficiaries to remain employed and those already receiving benefits to return to work (Autor and Duggan, 2010; Autor, 2011; Burkhauser and Daly, 2011; Liebman, 2015).

One way that policy makers try to limit DI take-up and incentivize work is setting earnings limits: if a beneficiary earns above a certain

level, she loses part or all of her benefits. The rationale behind earnings limits is the presence of asymmetric information: the government cannot observe applicants' true health status or work capacity, so it must rely on a screening mechanism.¹ The screening mechanism is supposed to ensure that only workers who are unable to earn above a certain level will apply for benefits, while potential applicants with higher working capacity will find it advantageous to forego benefits and remain employed instead. In the United States, the earnings limit applicable to beneficiaries in the Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI) programs is called Substantial Gainful Activity (SGA). It is designed as a "cash-cliff", which means that if a beneficiary earns even \$1 above the SGA, she loses all benefits. The SGA thus creates a notch in the benefit schedule

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¹ Other screening mechanisms include medical screening (de Jong et al., 2011; Liebert, 2019; Godard et al., 2024), benefit amounts (Mullen and Staubli, 2016), hassle costs (Deshpande and Li, 2019), and wait times (Kearney et al., 2021).

such that a higher total income (wages plus benefits) can be obtained by working less and keeping earnings below the SGA (Maestas and Yin, 2008; Weathers and Hemmeter, 2011). In 2024, the SGA is \$1,550 per month for non-blind applicants. Someone working full-time at the federal minimum wage would make approximately \$1,260. In the 30 states with minimum wages set above the federal level, a full-time minimum-wage worker would make considerably more than the SGA.

The benefit design based on a cash-cliff assumes that if an applicant can earn more than the limit in the labor market they do not need to receive any DI benefits. Policy makers and researchers have recognized that earnings limits like the SGA create strong work disincentives and have potentially negative welfare impacts. Alternative policy approaches avoid a notch in the benefit schedule by introducing a gradual phaseout of benefits above an earnings threshold. But under these policy designs the implicit tax rate may still inefficiently distort labor supply (Kostøl and Mogstad, 2014; Deuchert and Eugster, 2019; Ruh and Staubli, 2019; Zaresani and Olivo-Villabrille, 2022).

The usefulness of earnings limits as a screening mechanism and their distortionary effect on labor supply decisions create a trade-off for policy makers. When setting earnings limits, they need to take into account the impact on who takes up benefits and how much beneficiaries work. When the government increases the earnings limit, the set of workers who apply for benefits widens: receiving benefits while working becomes appealing to higher-productivity workers. On the flipside, a decrease in the earnings limit means that the set of workers applying for benefits shrinks as only less-productive workers will prefer benefit receipt and limited work. At the same time, another effect is at play: conditional on receiving benefits, a higher earnings limit means that beneficiaries who can work, will increase their labor supply as they still remain eligible for benefits. And a lower earnings limit has the opposite effect: some beneficiaries will set their labor supply lower in order to remain eligible for benefits. We call these two effects of changing the earnings limit the selection and labor supply effects.

To empirically estimate the selection and labor supply effects of changing the earnings limit, we study a policy reform in Hungary that reduced the earnings limit for some beneficiaries but not others, while leaving benefit amounts unchanged. In 2008, the cash-cliff style earnings limit in Hungary's Regular Social Assistance (RSA) program for moderately disabled individuals was reduced from 80% of the individual's last wage before entering disability to 80% of the monthly minimum wage for new entrants, while it remained the same for beneficiaries who were already approved. We exploit this policy change to understand how selection into the program and labor supply once in the program changed. To this end, we compare the evolution of various extensive and intensive margin measures of labor supply relative to the start of benefit receipt among beneficiaries who enter before ("old entrants") and after ("new entrants") the reform.

We find that the decrease in the earnings limit had a small impact on selection into the program. First, we do not find evidence of decreased program entry rates. Second, consistent with the screening mechanism, we show that individuals who entered the program after the reform had worse pre-entry labor market outcomes than beneficiaries who entered earlier. New entrants were 3 percentage points (4%) less likely to work and earned 8% less on average (conditional on working) pre-entry than old entrants. Old and new entrants were similar on a variety of other dimensions, such as age, occupation, geographical location, and sick leave use prior to entering disability. As a summary measure of the change in selection on observable characteristics, we show that the distribution of predicted program entry, based on a rich set of individual characteristics, is almost identical across years. This suggests that RSA entrants remain similar in terms of observable characteristics over time. Examining benefit persistence, we find no evidence that new entrants were more likely to exit the program than old entrants. Overall, the moderate selection effects are consistent with a world where the earnings limit and the benefit level were already sufficiently

low to deter most potential entrants who were well-positioned to find higher-paying jobs in the labor market.

At the same time, we find that individuals who entered the program after the earnings limit was reduced had meaningfully lower intensive margin measures of labor supply post-entry. New entrants were as likely to be employed as old entrants, but conditional on being employed, they worked less. On average, new entrants worked 7% fewer hours, and had 16% lower earnings (conditional on working) after taking up benefits. This result is driven by beneficiaries with higher pre-disability earnings, who were most affected by the change in the earnings limit.

Since we study a change in the DI earnings limit in 2008, it is important to rule out the role of the recession in explaining our results. We address this concern in several ways. We start by showing that the overall labor market impacts of the recession were not really present in Hungary until 2009 when the unemployment rate started rising rapidly. This means that inflow into RSA should not yet have been affected by the economic downturn in 2008. But labor market outcomes in the years after entry might be differentially affected for old and new entrants. Our first strategy to confront this concern involves comparisons across regions that were more and less severely hit by the recession and showing outcomes relative to their national or regional average to account for overall trends. Second, we perform placebo analyses based on entrants around non-reform cutoff dates. Third, we compare outcomes of old and new entrants into the accident insurance program, which did not see a change in the earnings limit. This set of robustness checks confirms that the change in labor market outcomes of new entrants is due to the change in the earnings limit rather than the change in the economic environment.

Our work contributes to three strands of the literature. We most directly contribute to the literature on earnings limits in disability insurance (e.g., Maestas and Yin, 2008; Maestas et al., 2010; Schimmel et al., 2011; Weathers and Hemmeter, 2011; Kostøl and Mogstad, 2014; Greenberg et al., 2018; Deuchert and Eugster, 2019; Ruh and Staubli, 2019; Zaresani, 2020). This literature finds mixed evidence on labor supply responses to earnings limits. A number of papers, including Zaresani and Olivo-Villabrille (2022) in Canada, Kostøl and Mogstad (2014) in Norway, and Ruh and Staubli (2019) in Austria find that DI beneficiaries are responsive to the financial incentives induced by earnings limits and easing the program's implicit high marginal tax rate would increase labor supply of beneficiaries. Others suggest little response to earnings limits. For example, Schimmel et al. (2011) and Gelber et al. (2017) find that only a minor portion of disability beneficiaries earn near the SGA limit in the U.S., suggesting that it is not a binding constraint on labor supply. Similarly, studies by Weathers and Hemmeter (2011) and Greenberg et al. (2018) find no evidence that a benefit offset policy increases employment rate or average earnings. In addition to describing possible research designs for studying a benefit offset policy instead of a cash cliff design, Maestas et al. (2010) provide a conceptual framework that captures the implications of moving from a cash cliff to a benefit offset policy for induced entry, an important consideration in our paper. We contribute to this literature in several ways. First, unlike much prior work focusing on existing DI beneficiaries, we are able to study both who takes up DI benefits (the selection effect) and how beneficiaries behave when they start receiving benefits (the labor supply effect). Second, we focus on moderately disabled individuals who have relatively high employment rates after entering the program and should thus be responsive to the change in the earnings limit. More severely disabled and longer-term beneficiaries considered in some previous work are more likely to be unable to significantly change their labor supply. Third, to the best of our knowledge, this is the first paper to study a decrease in the earnings limit, which is important as the responses to positive and negative changes in the earnings limit are not necessarily symmetric. Our results suggest that a low earnings limit impedes moderately disabled individuals from fully utilizing their remaining work capacity.

More broadly, this work contributes to the literature on the work disincentives of DI programs and the literature that studies the labor supply impacts of DI receipt (e.g., Bound, 1989; Gruber, 2000; Chen and van der Klaauw, 2008; Maestas et al., 2013; Low and Pistaferri, 2015; Mullen and Staubli, 2016; Haller et al., 2024). This literature has focused on understanding the effects of disability programs on labor supply taking into account all features of the programs as implemented. It broadly finds that DI receipt discourages work. For example, Maestas et al. (2013) find that for applicants on the margin of program entry, employment would be on average a third higher if they did not receive benefits. Earnings limits and other features (e.g., benefit generosity) jointly determine the effects of DI programs. We examine one feature of disability programs that policy makers can use to influence the incentive effects of DI programs.

Finally, this work also speaks to the academic and policy literature that has tried to address the fiscal sustainability of DI programs, partly by suggesting that work disincentives in these programs should be decreased (e.g., Autor and Duggan, 2006, 2007; Autor, 2011; Bipartisan Policy Center, 2015; Liebman, 2015). For example, several policy proposals in the U.S. included moving from a “cash cliff” to a gradual phase-out. We show that among moderately disabled workers a higher earnings limit can increase labor supply while only moderately changing selection into the program.

The rest of this paper proceeds as follows. Section 2 provides background on the Regular Social Allowance program and the 2008 reform. Section 3 describes our administrative data and empirical approach. We present our results in Section 4. Section 5 concludes.

2. Background

Preceding the 2008 reform, Hungary had the highest disability benefit receipt rate in the OECD at 12%, over twice the OECD average (OECD, 2009). Unlike the U.S. system, but similar to other European countries, Hungary’s disability insurance programs are tiered based on the severity of impairment beneficiaries have. The Regular Social Allowance (RSA) program, the focus of this paper, was available to individuals with sufficient work histories and with an at least 40% health impairment who could not work in their pre-disability job or any other job commensurate with their level of education without rehabilitation or further education. Health impairments are assessed by a panel of physicians and rehabilitation experts. The most common qualifying diagnoses for RSA recipients were musculoskeletal diseases. Different programs were available to more severely disabled individuals (disability pension), as well as those close to the retirement age when becoming disabled (temporary allowance), those who became disabled before age 25, and blind individuals (disability allowance). The benefit level of the RSA was low compared to the disability pension: 36 to 38% of the statutory minimum wage throughout the years of our analysis.

RSA recipients are allowed to work up to an earnings limit and at the time of the reform about 26% did work. As a comparison, only 12% of beneficiaries with more severe disabilities (disability pensioners) were employed in 2007. Until December 2007, the earnings limit was linked to the previous earnings of the applicants. A person with an at least 40% of health impairment was allowed to apply for RSA if her average earnings over four consecutive months did not exceed 80% of her pre-disability earnings.² The same rule applied to benefit continuation: beneficiaries whose average earnings over four consecutive months exceeded 80% of their pre-disability earnings were removed from the program. Starting January 2008, the earnings limit

² The calculation of pre-disability earnings is complex. It takes into account earnings during several years before applying for benefits. Previous earnings are adjusted for economy-wide changes in average earnings. Because of this complex calculation and the four-month rule, earnings can exceed the earnings limit in some months without removal from the program.

was lowered: irrespective of prior earnings, new entrants were only allowed to earn up to 80% of the monthly minimum wage while receiving benefits. This effectively meant that new entrants could only work part-time. The decision about the new earnings threshold was made at the end of 2007. The first internal proposal was written in November 2007 and passed on December 23, 2007, becoming effective on January 1, 2008. Hence the legislation was unexpected, making anticipatory effects unlikely. The earnings limit remained unchanged for those already approved for benefits.

To understand the bite of the reform it is useful to consider the distribution of earnings among RSA beneficiaries prior to taking up benefits. Appendix Figure A1 shows the distribution of the average monthly earnings three years before taking up benefits among old entrants in Panel (a) and new entrants in Panel (b). Average pre-disability earnings were 126% of the monthly minimum wage. 60% earned more than the monthly minimum wage and among those who earned more, the average pre-disability earnings were 169% of the monthly minimum wage. Another useful benchmark is the distribution of the earnings of RSA recipients. Fig. 1 shows earnings conditional on working in 2009. The earnings of 39% of old entrants was above the monthly minimum wage with average earnings of 95% of the minimum wage conditional working. About two-thirds of old entrants earned above 80% of the minimum wage, which assuming no change in the distribution of entrants would have meant that about two-thirds of new entrants would have been affected by the reform. (Note that beneficiaries can earn above the limit in some months without losing benefits.) This suggests that the policy change affected a substantial share of potential beneficiaries. Because the earnings limit was 80% of pre-disability earnings before the reform, the change in the earnings limit varied across beneficiaries. This created additional variation across beneficiaries in the bite of the policy which we exploit in our empirical analysis below: those with higher potential earnings were more impacted by the reform than those with pre-disability earnings close to the monthly minimum wage.

Due to declining benefit generosity and increased stringency of health requirements (Duman and Scharle, 2011), the inflow into all types disability programs in Hungary had been continuously declining since the early 2000s. This has been also the case for the RSA program where the monthly inflow declined by a factor of five between 2003 and 2007. This downward trend came to an end in 2008 when the inflow stabilized (Appendix Figure A2).

3. Data and empirical framework

We use administrative panel data that brings together information on earnings, occupations, benefit receipt, healthcare spending, and other domains for half of the Hungarian population over years 2003–2017. The data is based on a random 50% sample (for privacy reasons) of the population aged 5–74 in 2003 who are followed until 2017. Since our focus is on the working age population, we restrict the sample to individuals aged 20–60 in 2007.³

In addition to employment status, wages, and working hours, we can observe disability benefit take-up (regular social assistance, disability pension, and other types of disability benefits), unemployment insurance, and other social program (e.g., maternity leave) participation. We use monthly data, which allows us to precisely identify the timing of benefit take-up. The annual microregion level unemployment data are from the T-STAR database.

To study how the reform impacted the selection of beneficiaries into the RSA program and their labor supply conditional on participation, we compare beneficiaries who enter in 2007, the year before the reform (“old entrants”) and beneficiaries who enter in 2008, the year after the reform (“new entrants”). In our analysis, program entry is determined

³ For a detailed introduction to the data, see Sebők (2019).

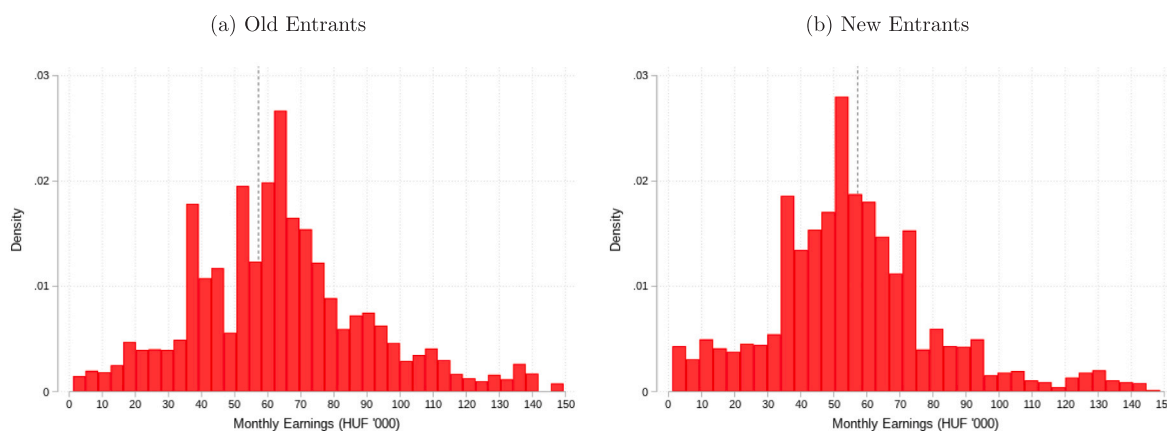


Fig. 1. Wage distribution in 2009 (Conditional on Working).

Notes: Figure shows the distribution of monthly wages observed in 2009. Panel (a) shows wages for individuals who enter Regular Social Assistance (RSA) the year before the reform, between January 1 and December 31, 2007 (“old entrants”) and panel (b) shows wages for individuals who enter the year after the reform, between January 1 and December 31, 2008 (“new entrants”). The dashed line shows the 80% of the minimum wage.

by the original date of application, which means that “old entrants” are those who applied before the rules were changed and “new entrants” are those applied after. The assessment process takes a few months and is similar before and after the reform: the average time from application to entry is 3.7 months for old entrants and 3.4 months for new entrants. The eligibility for the benefit commences on the date of application and the benefits are dated back to the application date. Appendix Figure A3 shows the share of beneficiaries already receiving cash payments relative to the date of application. We follow these two groups of beneficiaries for four years (48 months) before and three years (36 months) after they enter disability insurance.

We start our empirical analysis by comparing selection into the RSA program between old and new entrants. In particular, we compare program inflow, observed characteristics of entrants and their labor market outcomes in the years before entry. This should give us a sense of the overall selection effect due to the reform. To systematically examine possible compositional changes, we also estimate the change in the distribution of the predicted propensity to take up benefits. To calculate propensity scores, we estimate a logit regression of program entry on indicators for age categories and gender, as well as employment and earnings history preceding program entry on pre-reform data from 2005.

Next, we compare labor market outcomes of old and new entrants after disability entry. To interpret these differences as labor supply effects of the change in the earnings limit we have to control for the selection effects. We apply propensity score reweighting to control for the compositional differences between “old entrants” and “new entrants”. Specifically, we estimate the propensity score of RSA entry in 2008 versus 2007 in a logit model controlling for gender, age category, microregion, best job in the pre-disability period (based on broad occupational categories), the number of months spent in employment, the number of months with at least one day on sick leave, and cumulative earnings relative to the minimum wage in the second and third year (months -36 to -13) prior to entering the RSA. We then construct inverse probability weights based on the predicted propensity score. An important limitation of this approach is that propensity score reweighting adjusts for shifts in observable characteristics, while ideally we would want to adjust for shifts in unobserved counterfactual post-entry earnings.

A potential concern about our identification strategy is the role of aggregate labor market trends and, in particular, the onset of the Great Recession in 2008/2009. We apply the following strategies to confront this concern. First, we note that the main impact of the recession on the Hungarian labor market occurred in 2009. This is reflected in the unemployment rate, which only started to increase dramatically

at the end of 2008 (Appendix Figure A4). The inflow into RSA in the treatment and control groups should thus not be affected by the recession.⁴ But one may still be concerned that labor market outcomes after entry into RSA are affected by the economic downturn, which would imply different time patterns for old and new entrants. Our second strategy exploits large regional variation in the increase of the unemployment rate during the recession. This variation allows us to test whether the magnitude of the economic shock is related to inflow or selection into RSA, or to labor market outcomes after RSA entry. We find that there is no meaningful association between the size of the local economic shock and selection into RSA or labor market outcomes after program entry (Appendix Figure A5). Finally, we also examine outcomes of entrants around placebo reform years and entrants into the Work Accident Allowance, another disability program which was not subject to a change in the earnings limit. All of these analyses suggest that the effects of the recession are unlikely to confound our estimates of the impact of the decrease in the RSA earnings limit on selection into the program and labor supply once in the program.

4. Results

4.1. Selection and benefit take-up

We start by analyzing how selection into regular social assistance (RSA) receipt changes with the reform. We expect that the lower earnings limit would lead to a drop in program take-up and change the average type who takes up benefits. That is, the composition of observable characteristics of beneficiaries might change. Appendix Figure A2 plotting the monthly inflow into the RSA program does not provide evidence of a drop in program entry after the reform date in January 2008. In fact, the figure shows that in 2008 program inflow stabilizes after a long period of decline. The overall downward trend and the stabilization in take-up in 2008 are unlikely to be driven by the decrease in the earnings limit. This change was only announced in November 2007, making it unlikely that it affected the negative trend that started in the early 2000s. We would expect the reduction in the earnings limit to *decrease* the number of applicants as it becomes beneficial for fewer individuals to receive benefits if they also have to restrict their earnings at a lower level than earlier. This would suggest that the reform is unlikely to drive the trend change. However, as we

⁴ Prior work has suggested that recessions push more individuals into DI (Autor and Duggan, 2003; Maestas et al., 2015). We find little evidence of such an effect.

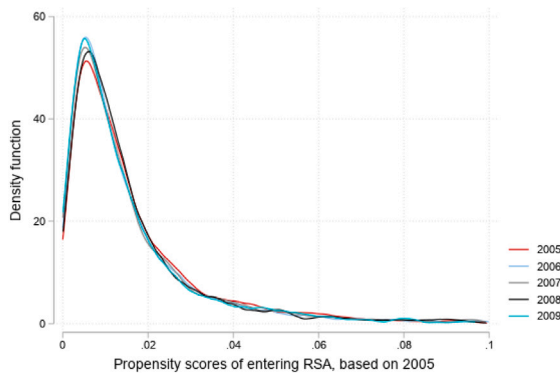


Fig. 2. Kernel density of propensity scores of entering RSA.
Notes: Figure shows the kernel densities of the propensity score for entering Regular Social Assistance (RSA). The propensity scores are estimated using a logit model on 2005 data. We estimate a logit model, regressing an indicator for entering RSA in month t on indicators for gender, age category (below 31, 31–40, 41–50, above 50 years), microregion, best job in the pre-disability period (based on broad occupational categories), number of months spent in employment, number of months with at least one day on sick leave, and cumulative earnings relative to the minimum wage. The sample consists of individuals aged 20–60 years who had not received any disability benefits in month $t - 1$.

discuss below, there is evidence of a small change in the composition of individuals who take up benefits. This small change in selection would be consistent with a small negative effect on the number of individuals taking up benefits, with this latter effect being masked by an overall trend shift and the stabilization in take-up that happens independently of the reform that we study.

The kernel density functions of the propensity scores for program entry, predicted for each year using the 2005, pre-reform relationship between observable characteristics, labor market history, and benefit take up, are almost identical (Fig. 2). This also indicates that RSA entrants remain relatively similar in terms of observable characteristics over time. Aggregate data from the annual reports of the National Rehabilitation and Social Office show that the ratio of accepted/rejected applications for all DI programs was similar in 2007 and 2008 (around 55%), suggesting that the stringency of the assessment process remained unchanged.

To understand possible differences in particular attributes, we compare observed characteristics of “old entrants” (beneficiaries who enter RSA during 2007, the year before the reform) and “new entrants” (beneficiaries who enter RSA during 2008, the year after the reform) in Table 1. We focus on differences in entrant characteristics three years before benefit take-up, because earnings decline rapidly in the year before entry due to deteriorating health and some restrictions embedded in the design of eligibility criteria further reduce labor supply immediately prior to program entry. It appears that the new and old entrants are similar along many dimensions, including age, occupation, and geographic location. But three characteristics show statistically significant differences: gender, employment and average wage. New entrants are 3 percentage points more likely to be male, 2 percentage points (3%) less likely to have been working three years prior to entry and earned 8% less on average (conditional on working). In particular, differences in labor supply prior to program entry deserve some attention to understand selection into the program.

Appendix Figure A6 provides more detail on the evolution of labor market outcomes by year of entry, including share working and conditional on working, hours worked, earnings relative to the monthly minimum wage, and share with earnings above 80% of the monthly minimum wage from four years before benefit take up to three years after. Looking at the period before entering RSA, the results suggest that old entrants were slightly more attached to the labor market. New entrants were 3 percentage points (4%) less likely to work and

Table 1
 Regular social assistance entrants before and after the reform.

	New Entrants	Old Entrants	<i>p</i> -value
Gender			
Male	0.414	0.383	0.034
Age			
35–44 years	0.186	0.195	0.429
45–55 years	0.734	0.741	0.597
Best job before disability			
Managers	0.050	0.040	0.113
Professionals	0.019	0.020	0.718
Technicians	0.082	0.087	0.584
Office and customer service	0.038	0.051	0.042
Commercial and services	0.166	0.157	0.428
Agriculture	0.038	0.045	0.232
Industry and construction	0.277	0.267	0.504
Machine operators and drivers	0.114	0.114	0.942
Elementary occupations	0.215	0.216	0.992
Region			
Budapest	0.064	0.056	0.293
Central Hungary	0.073	0.084	0.201
Central Transdanubia	0.099	0.099	0.999
Western Transdanubia	0.060	0.061	0.907
Southern Transdanubia	0.158	0.148	0.388
Northern Hungary	0.128	0.141	0.208
Northern Great Plain	0.209	0.205	0.761
Southern Great Plain	0.205	0.200	0.711
Working 3 years earlier	0.697	0.715	0.053
Earnings relative to monthly minimum wage 3 years earlier (Conditional on Working)	1.254	1.364	0.001
Number of observations	1,885	2,791	

Notes: Table shows summary statistics for individuals who enter Regular Social Assistance (RSA) the year before the reform, between January 1 and December 31, 2007 (“old entrants”) and those who enter the year after the reform, between January 1 and December 31, 2008 (“new entrants”). The table contains disability beneficiaries aged 20–60 years. Occupation categories refer to the Hungarian standard classification of occupations (HCSO-08/FEOR-08). The number of persons displays observations in the database that includes about half of the disabled population.

conditional on working earned 8% less on average pre-entry than old entrants. Column (3) of Appendix Table A1 also shows mean differences in outcomes between old and new entrants from year four to year one before RSA entry. While the figures show all months before and after benefit take up for the sake of completeness, the numbers displayed in Appendix Figure A6 (as well as other figures) and in Appendix Table A1 leave out the last year before and after benefit entry in order to abstract from short-term adjustments related to worsening health and benefit application before take-up and possible short-term adjustments after take-up. As a proxy for health status, we show in Panel (a) of Fig. 3 that there is no difference between old and new entrants in sick leave use before taking up disability benefits.

Selection could also be driven by benefit persistence. Especially, if there is uncertainty about changes in eligibility rules after the reform, new entrants might be more likely to leave the program after they learn about the restrictions from the new earnings limit. Panel (b) of Fig. 3 suggests that program participation is as persistent for new entrants as for old entrants, especially in the first two years after entry. Approximately 94% of initial beneficiaries still receive RSA two years after program entry in both groups followed by a slight divergence in the third year. This result suggests that the lower earnings threshold did not lead to significantly increased program exits.

These findings suggest that the earnings limit had a moderate impact on selection and benefit take-up. Next, we turn to labor supply responses conditional on program entry.

4.2. Labor supply after program entry

As the earnings limit is decreased, some DI benefit recipients may restrict their labor supply to remain eligible for benefits. At the same time, lowering the earnings threshold has an impact on who selects

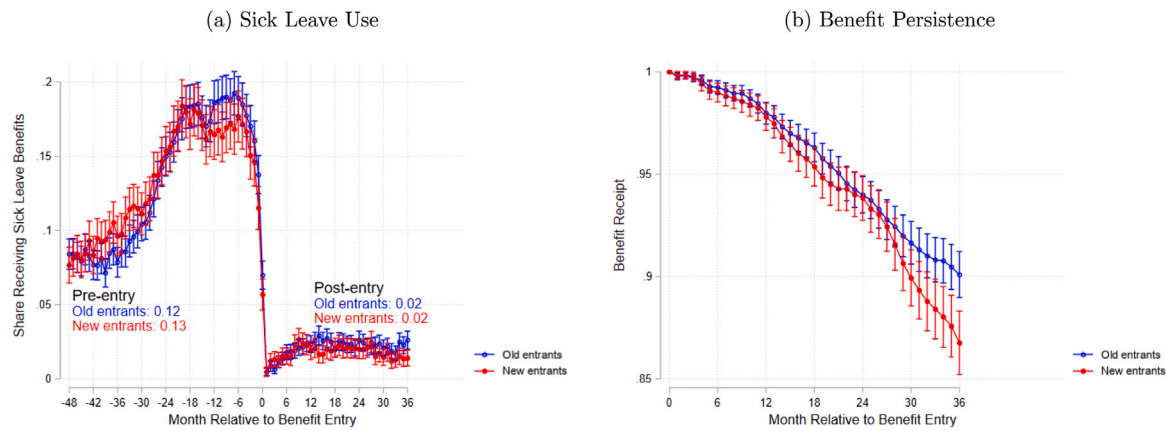


Fig. 3. Health and benefit persistence.

Notes: Figure shows sick leave use in Panel (a) and benefit persistence in Panel (b) for individuals who enter Regular Social Assistance (RSA) the year before the reform, between January 1 and December 31, 2007 (“old entrants” in blue) and those who enter the year after the reform, between January 1 and December 31, 2008 (“new entrants” in red). In Panel (a), the pre-entry labels show the mean of each outcome during the period between four years to one year (months -48 to -13) before entering RSA and the post-entry labels show the mean of each outcome during the period between one and three years (months 13 to 36) after entering RSA. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

into taking up benefits. In particular, we expect a lower earnings limit to result in increased selection toward lower-productivity or less-employable individuals. Our empirical findings above suggest that the selection effect is relatively small. Nevertheless we apply propensity score reweighting as explained in Section 3. Fig. 4 displays reweighted monthly sample means of the outcome variables around benefit entry for “old entrants” (in blue) and “new entrants” (in red) to show how labor market outcomes of beneficiaries change with the reform. In this figure, we also report average outcomes in the pre-entry and post-entry periods of old and new entrants. When computing these averages we leave out the last year before entry because of the sharp decline in work during the last pre-entry year as beneficiary health declines and also due to some incentives to reduce work to qualify for benefits and the first year after entry to examine longer-term differences beyond the first post-entry year where short-term adjustment responses may play a role in determining labor supply. Importantly, propensity score reweighting only adjusts for changes in composition related to observable characteristics and not for potential unobservable compositional changes. Furthermore, even after the reweighting some differences remain for some outcomes. Differences during the year before and after DI take-up do not factor into our estimates of selection and labor supply effects. To the extent that reweighting does not adjust for differences from selection in the time periods that enter our estimates of labor supply effects, our estimates could be somewhat biased. In particular, since the composition shifts towards beneficiaries with lower potential labor supply we could overestimate the negative labor supply effect of the lower earnings limit.

Panel (a) of Fig. 4 shows that less than 20% of entrants work right after entering RSA. Subsequently, the employment rate increases a bit more quickly for old entrants to about 30% during the first year, but it equalizes again by the second year of benefit receipt and the share of employment does not significantly differ thereafter. Differences in intensive margin labor supply are more substantial: while earnings and working hours of old entrants and new entrants move closely together in the pre-disability period, these outcomes diverge after program entry. Panel (b) shows that, conditional on working, new entrants work on average 29 h per week during the second and third year of benefit receipt, compared to the 31 h per week of old entrants. New entrants earn on average 80%, whereas old entrants earn 95% of the monthly minimum wage in the post-entry period, while the pre-entry difference is negligible (Panel (c)). There is also a significant gap in the share of beneficiaries with earnings above 80% of the monthly minimum wage (Panel (d)): while 61% of old entrants earn above 80% of the minimum wage in the post-entry period, only 38% of new entrants do.

Because the earnings limit applied to the average of earnings over six consecutive months, some new entrants can still earn above this limit in some months.

The patterns of the evolution of labor supply measures after RSA displayed in Fig. 4 measured on a reweighted sample to adjust for selection are very similar to those measured on the unweighted sample in Appendix Figure A6. This suggests that observable differences between old and new entrants prior to taking up benefits (i.e. the change in selection) do not explain the decrease in labor supply after taking up benefits.

To further demonstrate the labor supply impact of the reform, we display differences between old entrants and new entrants for all outcome variables separately for the pre- and post-entry periods around the 2008 reform and two placebo reform dates, 2007 and 2009 in Fig. 5 and Table A1. These are estimated from a simple regression of the outcome variable on an indicator for being a new entrant, weighting by the inverse of the propensity score for program entry to adjust for potential selection. In these regressions, estimated separately for the pre-entry and the post-entry periods, we leave out the last year before and the first year after entry, to abstract away from the decline in work just before benefit take-up and short-term adjustment after take-up. The middle panels suggest that for the actual reform (2008) period, pre-entry differences between old and new entrants are small. In contrast, post-entry intensive margin labor supply measures are significantly lower among new entrants, in line with the negative impact of the lower earnings limit on labor supply. In comparison to old entrants, new entrants work 2.4 h (7%) less, their earnings relative to the minimum wage are 15 percentage points (16%) lower and they are 23 percentage points less likely to have earnings above 80% of the minimum wage after RSA entry.

To further support our results, we define placebo reform dates in non-reform years (2007 and 2009) and “placebo old entrants” and “placebo new entrants” taking up RSA in the years around the placebo reform dates. The left and right panels suggest that around placebo reforms (2007 and 2009), the difference between the outcomes of old entrants and new entrants is small both in the pre-entry and the post-entry period. While some outcomes in placebo reform years show differences, they are much smaller in magnitude than those in the actual reform period. These results confirm that the main differences in labor supply outcomes can be explained by the change in the earnings limit rather than by time trends or other confounding factors.

Fig. 1 shows the post entry earnings distributions in 2009 of old and new entrants. The histograms provide further evidence that beneficiaries are indeed responding to the new earnings limit at 80% of

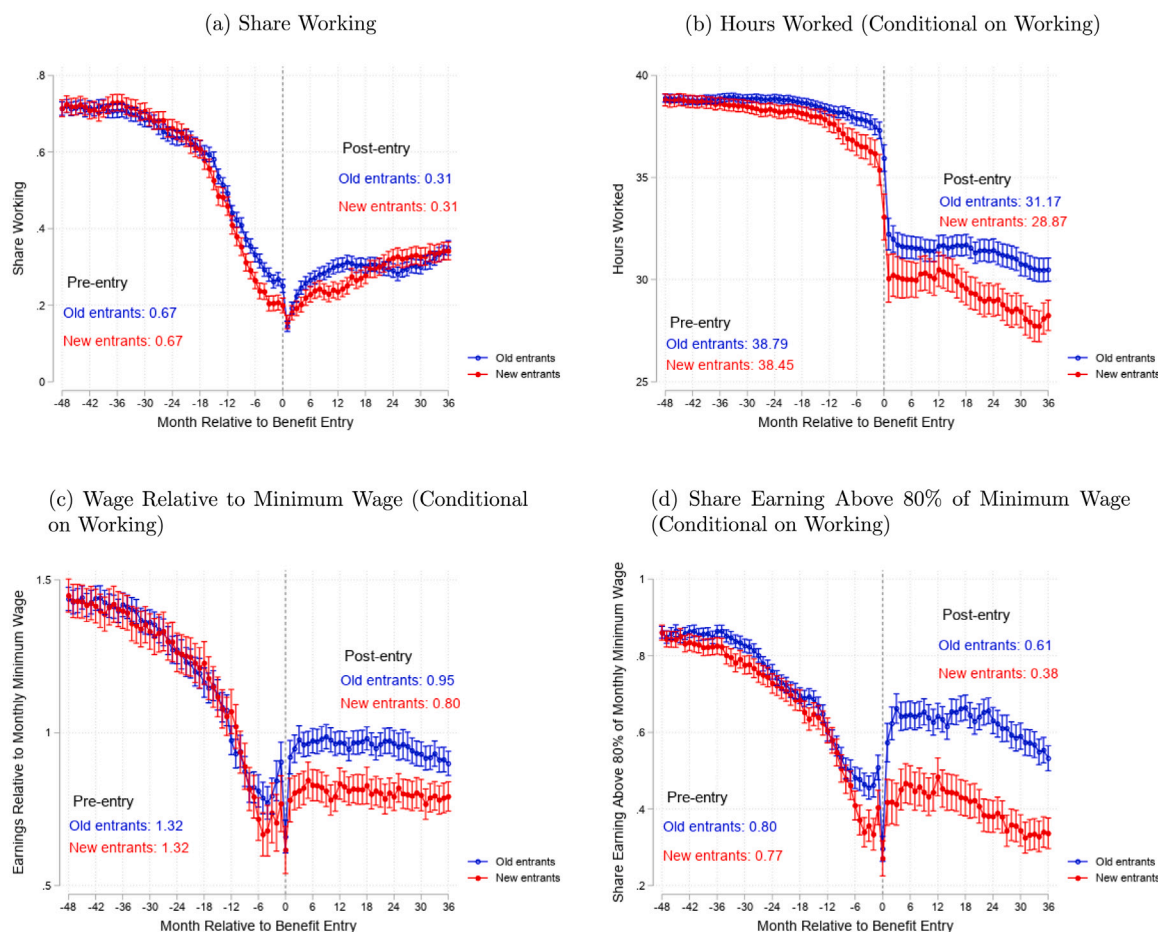


Fig. 4. Labor market outcomes of Regular Social Assistance entrants.

Notes: Figure shows labor market outcomes for individuals who enter Regular Social Assistance (RSA) the year before the reform, between January 1 and December 31, 2007 (“old entrants” in blue) and those who enter the year after the reform, between January 1 and December 31, 2008 (“new entrants” in red). The pre-entry labels show the mean of each outcome during the period between four years to one year (months -48 to -13) before entering RSA and the post-entry labels show the mean of each outcome during the period between one and three years (months 13 to 36) after entering RSA. The sample is reweighted by the inverse of the propensity score of selection into the “treatment” group (i.e., being a new entrant). For details on the specification of the propensity score used for reweighting see Section 3. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

the minimum wage by setting their earnings exactly at the threshold. While the distribution of monthly wages among old entrants is smooth through the threshold (Panel (a)), there is visible bunching among new entrants, as 5% of them earn within HUF 5,000 (\$15) of the earnings limit (Panel (b)).

In Fig. 6 we examine heterogeneity by reform exposure, comparing beneficiaries for whom the decrease in the earnings limit was likely binding and those for whom it was likely not binding, because their earnings were too low to be affected by the new limit. Panel (a) shows earnings relative to the minimum wage for RSA beneficiaries who earned below the minimum wage three years before taking up RSA benefits. Among this lower-earning group we find that the small pre-RSA-entry earnings gap of 6 percentage points between old and new entrants persists post-entry at about 9 percentage points. Panel (b) shows the same comparison for beneficiaries who earned above the minimum wage three years before taking up RSA benefits. For this group, there is a sharp increase in the earnings gap relative to the minimum wage between old and new entrants from 4 percentage points pre-entry to 21 percentage points after taking up benefits.

To provide further detail on heterogeneity by reform exposure, in Appendix Figure A7 we divide RSA beneficiaries into five quintiles by pre-disability earnings. The figure shows the difference between the earnings of new and old entrants separately in the pre-RSA-entry (in green) and post-RSA-entry (in yellow) periods as a share of the minimum wage. The figure suggests that, in line with our results above

suggesting that selection effects are small, differences between old and new entrants are small before taking up benefits. On the other hand, differences in the post-entry period are substantial and, as expected, grow with pre-disability income. New entrants in the lowest quintile of pre-disability earnings earn 6% of the minimum wage less than old entrants, growing to 11%, 15%, and 18% in the second, third, and fourth quintiles, respectively. Among the beneficiaries in the highest-earning quintile, new entrants make 25% less than old entrants.

These heterogeneity results confirm our prediction that workers with higher earnings potential reduce their labor supply in order to remain eligible for the disability benefit. It is also in line with our main results being driven by the change in the earnings limit rather than by other factors, such as the change in the macroeconomic environment.

4.3. Robustness

A potential threat to the interpretation of our results would be a differential impact of the recession on different cohorts of entrants. The placebo analysis and the heterogeneity results above suggest that labor supply changes arose in the reform year and among the affected group of beneficiaries, suggesting that they were driven by the decrease in the earnings limit, rather than by the recession. In addition, we present several pieces of evidence which suggest that the recession is unlikely to be the main driver of our results.

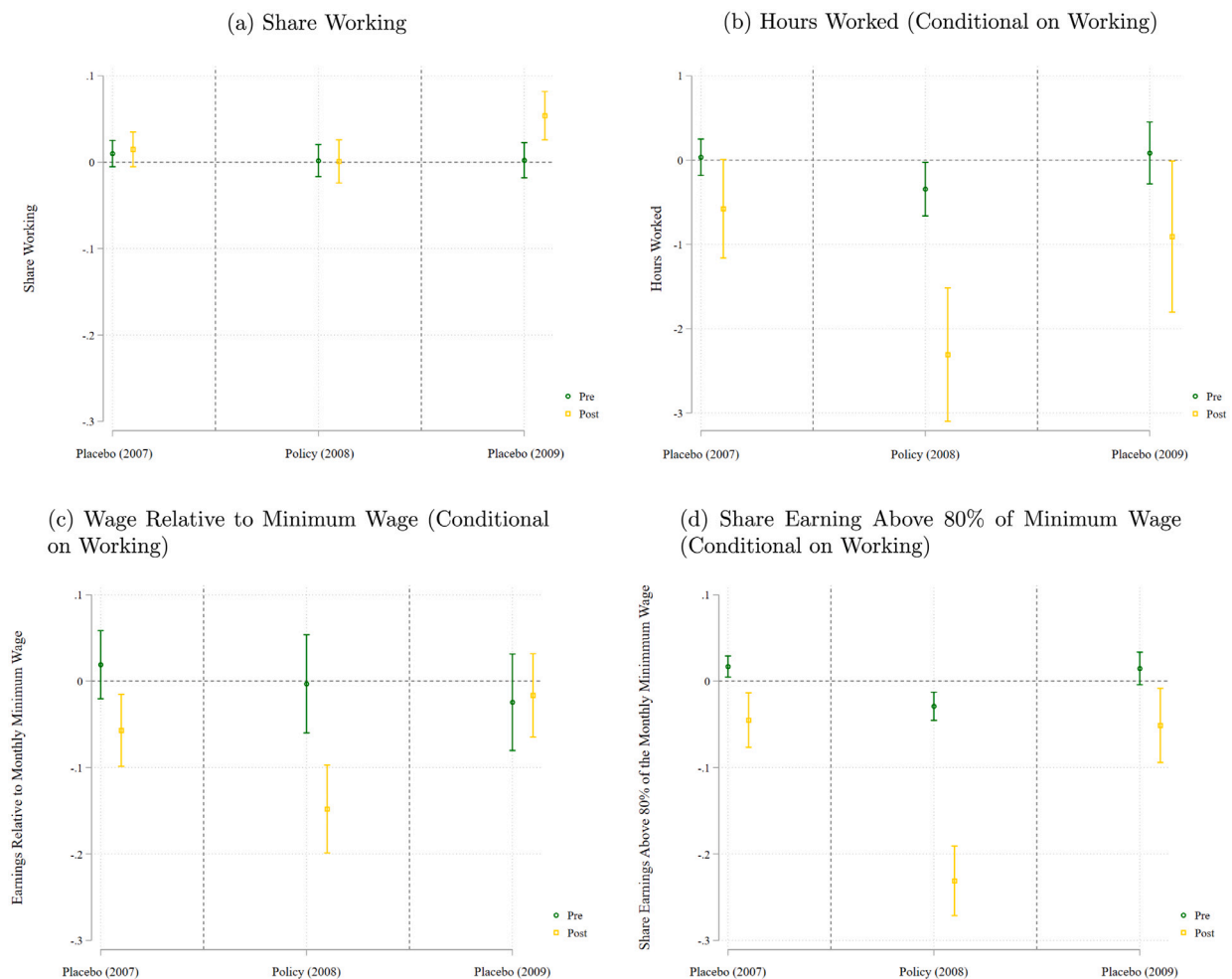


Fig. 5. Differences in labor market outcomes between old entrants and new entrants in the reform (2008) period and placebo reforms (2007 and 2009). **Notes:** Figure shows differences in labor market outcomes between individuals who enter Regular Social Assistance (RSA) the year before and after the reform (2008) and placebo reforms (2007 and 2009). The dots show the differences between “new entrants” and “old entrants” in the pre-entry (in green) and post-entry (in yellow) periods, respectively. The differences are estimated from a simple regression of the outcome variable on an indicator for being a new entrant, estimated separately for the pre-entry and post-entry periods and weighted by the inverse of the propensity score for program entry to adjust for potential selection. “New entrants” are individuals who enter RSA between January 1 and December 31 of the year of the (placebo) reform. “Old entrants” are individuals who enter RSA between January 1 and December 31 of the year before the (placebo) reform. The pre-entry period is defined as years four to one year (months -48 to -13) before entering RSA and the post-entry period is defined as years one to three (months 13 to 36) after entering RSA. Standard errors are clustered at the individual level. For details on the specification of the propensity score used for reweighting see Section 3. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

In Hungary, the unemployment rate only started increasing rapidly in 2009 (Appendix Figure A4). We exploit strong regional variation in this increase to test for responses in RSA entry and labor supply of entrants to macroeconomic conditions. Appendix Figure A5 plots changes in the unemployment rate at the beginning of the recession relative to changes RSA inflow rates across microregions. The absence of a clear relationship between the two variables indicates that the severity of the recession did not lead to a change in RSA inflow.

To absorb macroeconomic fluctuations in outcomes, we compare labor market outcomes of new and old entrants relative to their national or microregion counterparts. Appendix Figure A8 shows labor market outcomes before and after RSA entry relative to their local/national average in the given month. The dynamics of relative labor market outcomes in this figure are similar to absolute outcomes in Appendix Figure A6.

Next, we present results for two different subgroups: RSA entrants living in microregions with low (below-median) versus high (above-median) increase in the unemployment rate. Appendix Table A2 shows that reform responses are very similar for areas more and less impacted by the recession. Taken together, these results suggest that the recession is unlikely to drive our main results.

In addition to the placebo analysis discussed above (Fig. 5 and Table A1), in a second placebo analysis, we examine an alternative health related benefit, the Work Accident Allowance (WAA), which was not affected by the reform. This program is available to individuals who suffer health impairments of more than 13% resulting from workplace accidents or occupational diseases. Importantly, the accident allowance has no earnings limit, reflecting that the health impairment does not necessarily imply a loss in working capacity. Otherwise, the award procedure for WAA is similar to RSA and other disability benefits. Labor market outcomes for WAA entrants in the reform years are shown in Appendix Figures A11. Similar to the other placebo analyses, these figures show no difference in any outcomes between the two groups.

4.4. Fiscal impacts of the reform

Our estimates of the impact of the reform can be used to calculate the approximate fiscal costs of the reform. To estimate the costs of the labor supply effect of the reform we can use our estimate of the reduction in earnings which is 15% of the minimum wage among beneficiaries who worked. In 2008, the monthly minimum wage was approximately \$402 (at the contemporaneous exchange rate), which

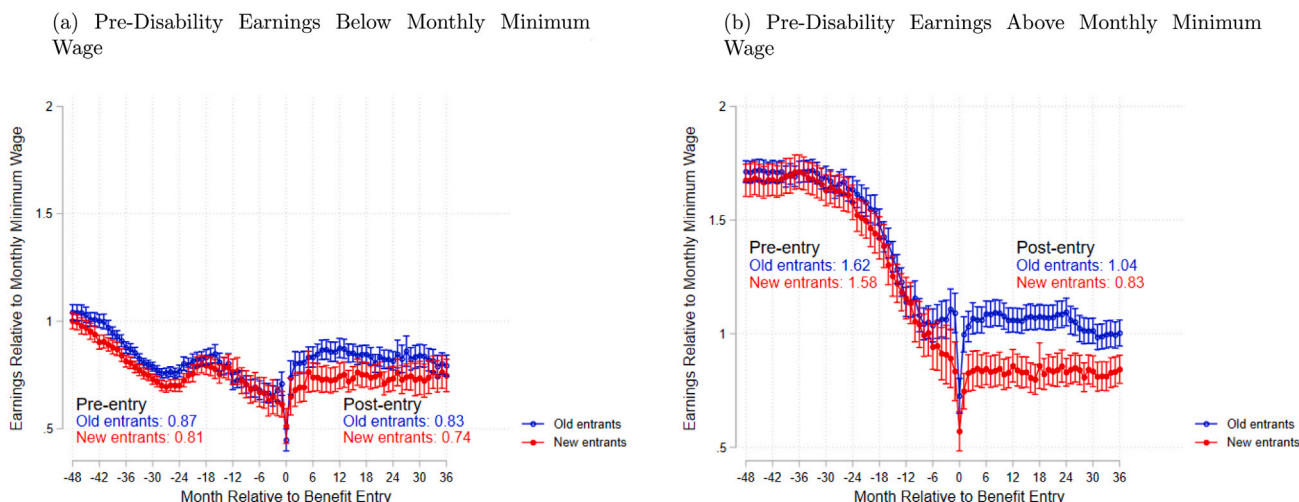


Fig. 6. Heterogeneity by pre-disability earnings.

Notes: Figure shows earnings relative to the minimum wage for individuals who enter Regular Social Assistance (RSA) the year before the reform, between January 1 and December 31, 2007 (“old entrants” in blue) and those who enter the year after the reform, between January 1 and December 31, 2008 (“new entrants” in red). Panel (a) shows individuals whose average pre-disability wage (three years before entering RSA) was below the minimum wage. Panel (b) shows individuals whose average pre-disability wage (three years before entering RSA) was higher than the minimum wage. The pre-entry labels show the mean of each outcome during the period between four years to one year (months –48 to –13) before entering RSA and the post-entry labels show the mean of each outcome during the period between one and three years (months 13 to 36) after entering RSA. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

would imply that the labor supply cost of the reform would have been a substantial \$60.25 per month or \$723 per year for each beneficiary that worked. Due to lower earnings, direct and indirect tax revenues decreased. Since the tax wedge was 35.7% on low wage earners, of which the employer contribution is 29% (OECD, 2022), the direct tax revenue loss would have been $\$723 \times 1.29 \times 0.357 = \333 . Assuming a marginal propensity to consume of 90% (which is realistic among low-earning, liquidity-constrained individuals) and using the effective standard 2008 VAT rate of 20%, the indirect tax revenue would have been $(\$723 \times 1.29 - \$333) \times 0.9 \times 0.2 = \108 . That is the total revenue loss would have been $\$333 + \$108 = \$441$ per working beneficiary. 31% of recipients worked both before and after the change in the earnings limit. Since there were new 1,885 beneficiaries in 2008, the total cost in lost tax revenues for them would have been $1,885 \times 0.31 \times \$441 = \$257,698$, which equals 7% of the pre-reform benefit costs.

We also need to make assumptions about savings from the screening effect of the reform. Taking the average pre-entry earnings (1.32 times the minimum wage), and assuming a 20% reduction relative to pre-entry earnings due to health deterioration, the average screened out applicant would have earned $\$402 \times 1.32 \times 0.8 = \424 . Taking into account the 70% pre-entry employment rate, the corresponding direct ($\$424 \times 1.29 \times 0.357 = \195) and indirect ($(\$424 - \$195) \times 0.9 \times 0.2 = \44) tax revenue, the overall loss of tax revenue would have equaled $0.7 \times (\$195 + \$44) = \$239$ per month or \$2,011 per year for individuals screened out of the program. The average benefit throughout the year was \$157 per month or \$1,880 per year. Thus we estimate that for each screened out beneficiary, the government saved \$3,891 per year. We find very small screening effects from the reform in terms of the type of applicants and the exact number of screened out beneficiaries is hard to identify due the overall negative trend in RSA take-up. Nevertheless, it is possible to calculate the “break even” number of beneficiaries N that would have needed to be screened out to make the reform budget neutral: given the assumed cost and savings, this would have been 3.5% of 2007 applicants. We can also assume that the highest earners are screened out of the program: if the percentage of earners screened out is represented by “x%”, for those who are screened out, their earnings of are calculated based on the average of the top x% of earners in the old entrants’ earnings distribution. Under this assumption, the “break even” share of beneficiaries that would have needed to be screened out to make the reform budget neutral

is 1.5%. If the screening effect was smaller, the reform would have had a net negative fiscal impact, while above this threshold it would have represented net savings to the budget. Note that because earnings limits constrain beneficiaries who are unable to fully optimize their labor supply, there could have been additional welfare costs that are not accounted for in this calculation.

5. Discussion

Disability insurance earnings limits can serve as screening mechanisms, ensuring that disability benefits go to those who truly cannot work. At the same time, they may distort labor supply among workers with significant remaining working capacity. Thus when changing the earnings limit, policy makers must trade off selection and labor supply effects.

In this paper, we studied take-up and labor supply responses to changing earnings limits exploiting a reform that lowered the earnings limit in a disability insurance program for the moderately disabled in Hungary. As the lower earnings limit applied to all new disability entrants but remained unchanged for those already receiving benefits, we compared outcomes of entrants in the year before and after this cutoff date to evaluate the reform effects. Our empirical analysis provided three main findings. First, we documented that program entry and persistence in the program were not affected by the change in the earnings limit. Second, we found that the change in the composition of beneficiaries in response to the policy was small as individuals with slightly lower work capacity selected into the program after the reform date. Third, we showed that intensive margin labor supply among beneficiaries entering after the reform date decreased significantly relative to beneficiaries entering before, leading to fewer hours of work and lower earnings. In particular, the reform resulted in a sharp reduction of labor supply among previously higher-earning beneficiaries, who presumably have higher capacity to generate labor income and whose labor supply responded to fulfill the stricter benefit eligibility requirement.

Overall, our results suggest that decreasing the earnings limit only led to a moderate improvement in screening efficiency. This evidence is consistent with a scenario where the earnings limit and benefit level before the reform were already sufficiently low to deter potential entrants who are well-positioned to find higher-paying jobs in the labor

market. At the same time, the reform substantially distorted the labor supply of program participants. The empirical findings suggest that the overall impact of the reform on efficiency and welfare was negative. The reform failed to yield sizable cost savings from benefit expenditures for the government, but left moderately disabled individuals with lower earnings, resulting in lower tax revenues in turn. At the given benefit level, a higher earnings limit would therefore be optimal.

CRedit authorship contribution statement

Judit Krekó: Conceptualization, Data curation, Formal analysis, Funding acquisition, Methodology, Project administration, Visualization, Writing – original draft, Writing – review & editing. **Dániel Prinz:** Conceptualization, Investigation, Methodology, Writing – original draft, Writing – review & editing, Formal analysis, Funding acquisition. **Andrea Weber:** Conceptualization, Investigation, Methodology, Writing – original draft, Writing – review & editing.

Data availability

Our dataset contains confidential administrative data and is not publicly available, but upon request, the data host (KRTK Databank) provides access to the dataset for review or replication purposes.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.labeco.2024.102583>.

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