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Take-up and labour supply responses to disability insurance earnings limits

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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 develop a simple framework to evaluate this trade-off. We use a reduction in the earnings limit in Hungary to examine screening and labor supply responses. We find that the policy changed selection into the program modestly but reduced labor supply significantly. Viewed through the lens of our model, these findings suggest that the earnings threshold should be higher.

The share of working-age adults receiving long-term disability insurance (DI) benefits has increased rapidly over the last few decades and DI programs currently 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. 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 as soon as a beneficiary earns above the SGA ($1,350 per month or about $100 above the federal minimum wage for non-blind applicants in 2022), she loses all benefits. The SGA thus creates a notch in the benefit schedule 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).

The benefit design based on a cash-cliff essentially assumes that if an applicant can earn more than the SGA 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 adopted in other countries avoid a notch in the benefit schedule by introducing a gradual phaseout of benefits above an earnings threshold. But even 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, 2021).

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. In this paper, we develop a framework to understand this trade-off in the context of a "cash cliff" design (where beneficiaries lose all benefits if they earn above the SGA).

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: conditional on receiving benefits, 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. At the optimal earnings limit, the marginal selection effect and the marginal labor supply effect of moving the earnings limit will balance each other out. Therefore, to characterize the welfare impact of changing the earnings limit these two effects should be estimated.

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 the moderately disabled was reduced from 80% of the individual's last wage before entering disability to 80% of the 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, and geographical location. We do not have detailed measures of health status, but show that there was no difference between old and new entrants in sick leave use prior to entering disability. 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.

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Though exit from DI benefits is not common even among the moderately disabled beneficiaries studied in this paper (3% of beneficiaries exit the program each year), a further margin that could have been potentially affected by the earnings limit is benefit persistence. In particular, one impact of the screening mechanism from lowering the earnings limit could be that beneficiaries stay on the program for a shorter period. We find no evidence that new entrants were more likely to exit the program than old entrants.

At the same time, we find that individuals who entered the program after the earnings limit was reduced had meaningfully lower 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 18% 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. Placebo analyses comparing entrants in other years also support the conclusion that the decreased earnings limit drives the reduced labor supply of beneficiaries.

To examine the impact of the lowered earnings limit on beneficiary health, we consider mortality, an imperfect proxy for health. Our results suggest no change in mortality which means that the primary effect of the reduction of the earnings limit on beneficiaries was through reduced work.

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; Schimmel, Stapleton and Song, 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. Some papers find that DI beneficiaries are responsive to the financial incentives induced by earnings limits. Others suggest little response to easing the earnings limit. 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 (e.g., Greenberg et al., 2018) are less likely to be able 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. Fourth, we develop a simple model that clarifies the role of the two key effects of changing the earnings limit: its impact on selection and on labor

supply.

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, Mullen and Strand, 2013; Low and Pistaferri, 2015; Mullen and Staubli, 2016). 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, Mullen and Strand (2013) find that for applicants on the margin of program entry, employment would be on average a third higher if they didn't receive benefits. Earnings limits and other features, such as benefit generosity and selection criteria jointly determine the effects of DI programs. Our contribution to this literature is examining 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. This study can shed further light on the welfare effects of changing earnings limits among moderately disabled workers.

1   Conceptual Framework

In this section, we propose a simple conceptual framework to capture the key trade-offs related to the setting of earnings limits in disability insurance programs. We focus on the case of notches, earnings limits above which beneficiaries lose their benefits completely.[1]

Individuals are characterized by their productivity types $\theta \in [0,1]$. Types are distributed according to CDF $F(\theta)$ (PDF $f(\theta)$). Individuals work h hours and have after-tax income $y = h\theta - \tau(h\theta)$ where τ is the income tax rate. Type θ is unobserved and therefore the government cannot use it to condition benefits. Disability benefits are B and there is an income threshold $\bar{y}$ for receiving benefits. Individuals have utility

$$V_\theta^{NB} = u\left(y_\theta^{NB}\right) - v(h_\theta^{NB}) \tag{1}$$

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[1]Our notation follows the framework in Finkelstein and Notowidigdo (2019) who study the take-up of welfare programs in the presence of potential behavioral biases. See also Nichols and Zeckhauser (1982), Kleven and Kopczuk (2011), and Anders and Rafkin (2021) for more general models of welfare eligibility and take-up.

if not receiving benefits, and utility

$$V_\theta^B = u\left(y_\theta^B + B\right) - v(h_\theta^B) \tag{2}$$

if receiving benefits where $v(h)$ is the disutility of work. Individuals decide to participate in the disability program if the value of participation is higher than the value of non-participation: $V_\theta^B \geq V_\theta^{NB}$. This decision rule determines in turn a cutoff type $\bar{\theta}$, who is indifferent between participating and not participating. Here we consider the program entry decision in a static framework and disregard any dynamics of repeated entry and exit. In our empirical analysis we show that program participation is highly persistent and even the reform-driven change in the earnings threshold did not lead to increased exits.

Social welfare is

$$W = \underbrace{\int_0^{\bar{\theta}} u\left(y_\theta^B + B\right) - v(h_\theta^B)d\theta}_{\text{Receiving Benefit}} + \underbrace{\int_{\bar{\theta}}^1 u\left(y_\theta^{NB}\right) - v(h_\theta^{NB})d\theta}_{\text{Not Receiving Benefit}} - \underbrace{\bar{\theta}B}_{\text{Program Cost}} \tag{3}$$

$$+ \underbrace{\int_0^{\bar{\theta}} G_\theta^B d\theta}_{\substack{\text{Fiscal Externality,} \\ \text{Benefit}}} + \underbrace{\int_{\bar{\theta}}^1 G_\theta^{NB} d\theta}_{\substack{\text{Fiscal Externality,} \\ \text{No Benefit}}} \tag{4}$$

where $\bar{\theta}$ is the highest productivity type worker who receives the benefit. The standard fiscal externality is the tax revenue the government realizes on a type θ worker: $G_\theta^B = \tau\left(h^B\theta\right)$ and $G_\theta^{NB} = \tau\left(h^{NB}\theta\right)$. However, other types of fiscal externalities can also fit into this framework. For example, time out of the labor force while receiving benefits may reduce working capacity (Autor, Maestas, Mullen and Strand, 2015; Garcia-Mandicó, García-Gómez, Gielen and O'Donnell, 2020; Bíró et al., 2022) which imposes an additional negative fiscal externality.

The government can vary the earnings limit $\bar{y}$ leaving B fixed. This has an impact on what types of workers choose to receive benefits, as the cutoff type $\bar{\theta}$ changes. This in turn impacts total program costs (how many workers receive benefits) and the fiscal externality of the program. If the government lowers (increases) $\bar{y}$ the set of workers opting for benefits shrinks (widens). The labor supply of some workers receiving benefits also decreases because some workers will lower their labor supply in order to remain eligible for benefits. At the same time, the labor supply of some workers not receiving benefits may increase as they can work more once they do not need to meet the earnings limit anymore. In particular, a

marginal change in $\bar{y}$ has the following effect on social welfare:

$$\frac{dW}{d\bar{y}} = \underbrace{\frac{d\bar{\theta}}{d\bar{y}}\left(G_{\bar{\theta}}^B - G_{\bar{\theta}}^{NB} - B\right)}_{\text{Change in Selection}} + \underbrace{\int_0^{\bar{\theta}} \frac{dG_\theta^B}{d\bar{y}}d\theta + \int_{\bar{\theta}}^1 \frac{dG_\theta^{NB}}{d\bar{y}}d\theta}_{\text{Change in the Fiscal Externality}}. \tag{5}$$

The welfare impact consists of two parts. First, the change in selection into benefit take up ($\frac{d\bar{\theta}}{d\bar{y}}$) has an impact through the program cost (B) and the fiscal externality ($G_{\bar{\theta}}^B - G_{\bar{\theta}}^{NB}$). Second, among beneficiaries (types 0 to $\bar{\theta}$) and potentially non-beneficiaries (types $\bar{\theta}$ to 1), the fiscal externality can change too ($\frac{dG_\theta^B}{d\bar{y}}$ and $\frac{dG_\theta^{NB}}{d\bar{y}}$ respectively). In the standard case, the fiscal externality is the tax revenue the government realizes and it changes because beneficiaries may adjust their labor supply to remain eligible for benefits. Note that assuming that individuals were already optimizing, applying the envelope theorem, there is no welfare effect through individuals' utilities. Welfare is maximized with respect to the earnings limit if the derivative $\frac{dW}{d\bar{y}}$ is zero. This is realized when the selection and labor supply responses balance each other out.

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 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 RSA 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. Their 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. 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: 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, the inflow into all types disability programs in Hungary had been continuously declining since the early 2000s (Appendix Figure A1). 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.

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 (Sebők, 2019). The data is based on a random 50% sample (for privacy

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[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.

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.

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

To study how the reform lowering the earnings limit 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").[3] We follow these two groups of beneficiaries for 4 years (48 months) before and 3 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 good sense of the overall selection effect due to the reform.

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, which we do in regression and reweighting analyses. A general concern with our identification strategy is the role of aggregate labor market trends and, in particular, the onset of the recession in 2008/2009. To make sure that aggregate trends are not driving our results, we offer two strategies. First, we perform Placebo analyses for a set of alternative reform years. Second, we check for heterogeneous effects comparing individuals who should be differently affected by the change in the earnings limit based on how close their pre-disability earnings were to the minimum wage.

4 Results

Selection and Benefit Take-Up Following our conceptual framework in Section 1, we start by analyzing how selection into regular social assistance (RSA) receipt changes with the reform. The framework predicts two sources of selection effects due to the change in the earnings limit. First, the lower cutoff type $\bar{\theta}$ should lead to a drop in program take-up. Second, due to the change in the average type who takes up benefits, the composition of

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[3]Program entry is defined based on the original date of application for RSA. Benefits are dated back to the application date.

observable characteristics of beneficiaries might change. Appendix Figure A1 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.

Next, 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. Some incentives were also embedded in the design of the policy to restrict labor supply immediately prior to program entry in order to be eligible for benefits. 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.

Figure 1 provides more detail on the evolution of labor market outcomes, 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 among old and new entrants 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 conditional on working earned 8% less on average pre-entry than old entrants. Column (5) of Table 2 shows mean differences in outcomes between old and new entrants from year four to year one before RSA entry. Again, we leave out the last year before entry, because of the sharp decline in work during that year in order to be eligible for benefits. Although we do not have detailed measures of health status, we show in Appendix Figure A2 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 the time benefit recipients remain in the program. Especially, if there is uncertainty about changes in eligibility regulations due to the reform, we might see that new entrants are more likely to leave the program after they learn about the restrictions from the new earnings limit. Panel (a) of Figure 2 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. There is a some divergence in the third year: after three years 90% of old entrants and 87% of new entrants receive benefits. This result suggests that lowering the earnings threshold did not lead to significantly more beneficiaries exiting the program and returning to work without receiving benefits.

These findings suggest that lowering the earnings limit has a moderate impact on who selects into taking up benefits. Next, we turn to labor supply responses to the reform conditional on program entry.

Labor Supply After Program Entry Our conceptual framework in Section 1 predicts that some DI benefit recipients will restrict their labor supply in order to remain eligible for benefits. Figure 1 and column (6) of Table 2 show how labor market outcomes of beneficiaries change with the reform. We focus on average outcomes in year two and three after entry into RSA and report mean differences between old and new entrants in Table 2.

Panel (a) of Figure 1 shows that right after entering RSA, just under 20% of both old and new entrants work. While the employment rate increases more quickly for old entrants to about 30% during the first year, it reaches the same level for new entrants by the third year of benefit receipt. There are also substantial differences in labor supply on the intensive margin. Panel (b) shows that, conditional on working, new entrants work on average 29 hours a week during the second and third year of benefit receipts, 6% less than old entrants. Panel (c) suggests that, conditional on working, new entrants earn on average 78% of the monthly minimum wage, 18% less than old entrants. Since the earnings limit was set as 80% of the monthly minimum wage for new entrants, we examine what share of old and new entrants earn above this threshold in Panel (d). Because the earnings limit applied to the average of four consecutive months' earnings, some new entrants can still earn above this limit in some months, and about 33% do in a given month vs 59% among old entrants.

Appendix Figure A3 provides further evidence that beneficiaries are indeed responding to the new earnings limit at 80% of the minimum wage by setting their earnings exactly at the threshold. Panel (a) shows the distribution of monthly wages among old entrants. It suggests no bunching at the earnings limit. Panel (b) shows the same distribution among new entrants. 5% of new entrants earn within the HUF 5,000 ($15) bin just below the threshold.

Heterogeneity By Pre-Disability Earnings In Figure 3 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 would have been below the new limit even absent the reform. Panel (a) shows earnings relative to

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the minimum wage for RSA beneficiaries who earned below the minimum wage three years before taking up RSA benefits. Among this lower-earning group, the decrease in the earnings limit is less likely to be binding. We find that the small earnings gap of 6% between old and new entrants pre RSA entry persists post entry at about 9%. Panel (b) shows the same results for beneficiaries who earned above the minimum wage three years before taking up RSA benefits. For them, there is a sharp increase in the earnings gap between old and new entrants from 4% pre entry to 21% after taking up benefits. This confirms our prediction that workers with higher earnings potential reduce their labor supply in order to remain eligible for the disability benefit.

Mortality Panel (b) of Figure 2 examines the mortality of beneficiaries. It shows that over a three-year horizon after program entry, old and new beneficiaries have the same cumulative mortality (2%). While this is an imperfect measure health, this result does suggest that the change in the earnings limit primarily impacted beneficiaries through changes in labor supply rather than through worsening health. Over a three year time horizon lower income does not worsen beneficiary health so much that it results in higher rates of mortality.

Controlling for Selection on Observables Our conceptual framework predicts that lowering the earnings threshold has an impact on who selects into taking up benefits. In particular we expect a lower earnings threshold to result in lower-productivity or less-employable individuals to select into the program on average. Our empirical findings above suggest that the selection effect is relatively small. To confirm that selection on observed characteristics is indeed not driving the gaps in post entry outcomes, we present results that use propensity score reweighting in Figure A4. Specifically, we reweight our sample using the inverse of the propensity score of selection into the reform treatment (new entrants). The propensity score is based on a logit model, where the dependent variable is an indicator for being a new entrant, and the right hand side variables include age, gender, work status and wage relative to the minimum wage 12, 24, and 36 months before taking up benefits. The patterns in reweighted measures of labor supply after entering RSA shown in Figure A4 are virtually identical to those in Figure 1. 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.

Placebo Analyses To address the potential concern that economic changes over time confound our results, we present placebo analyses where we define placebo reform dates in non-reform years (January 1, 2006/2007/2009) and "placebo old entrants" and "placebo new

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entrants" as beneficiaries who take up RSA in the years before and after each of the placebo reform dates. For each of the placebo reform years, Panels (a) to (c) in Appendix Figures A5 and A6 replicate hours worked and earnings outcomes from Panels (b) and (c) in Figure 1. These figures confirm that the difference in hours worked either before or after taking up benefits is close to zero between placebo old and new entrants. The placebo results for earnings relative to the minimum wage are slightly noisier but they do not indicate any systematic selection or labor supply effects in non-reform years either. Mean differences in all outcome variables in the pre- and post-reform periods around each of the placebo reform dates are shown in Columns (1)-(4) and (7)-(8) of Table 2. Some outcomes in placebo reform years show statistically significant differences. But they tend to be significantly smaller in magnitude, while large and systematic differences in selection and labor supply appear only for the real reform in 2008 in columns (5) and (6).

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 sufficient remaining working capacity. In this paper, we studied take-up and labor supply responses to changing earnings limits. We showed conceptually that with the choice of the earnings limit, policy makers must trade off selection and labor supply effects.

Empirically, we exploit a reform that lowers the earnings limit in a disability insurance program for the moderately disabled in Hungary. As the lower earnings limit applied to all disability entrants as of January 1, 2008 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. This result is consistent with a system in which DI benefits also act as insurance against labor market income risks. Second, we found that the change in the composition of beneficiaries in response to the policy was small as individuals with characteristics related to slightly lower work capacity selected into the program after the reform date. Third, we show that intensive margin labor supply among beneficiaries entering after the reform date decreased significantly 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.

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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. Viewed through the lens of our model, 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.
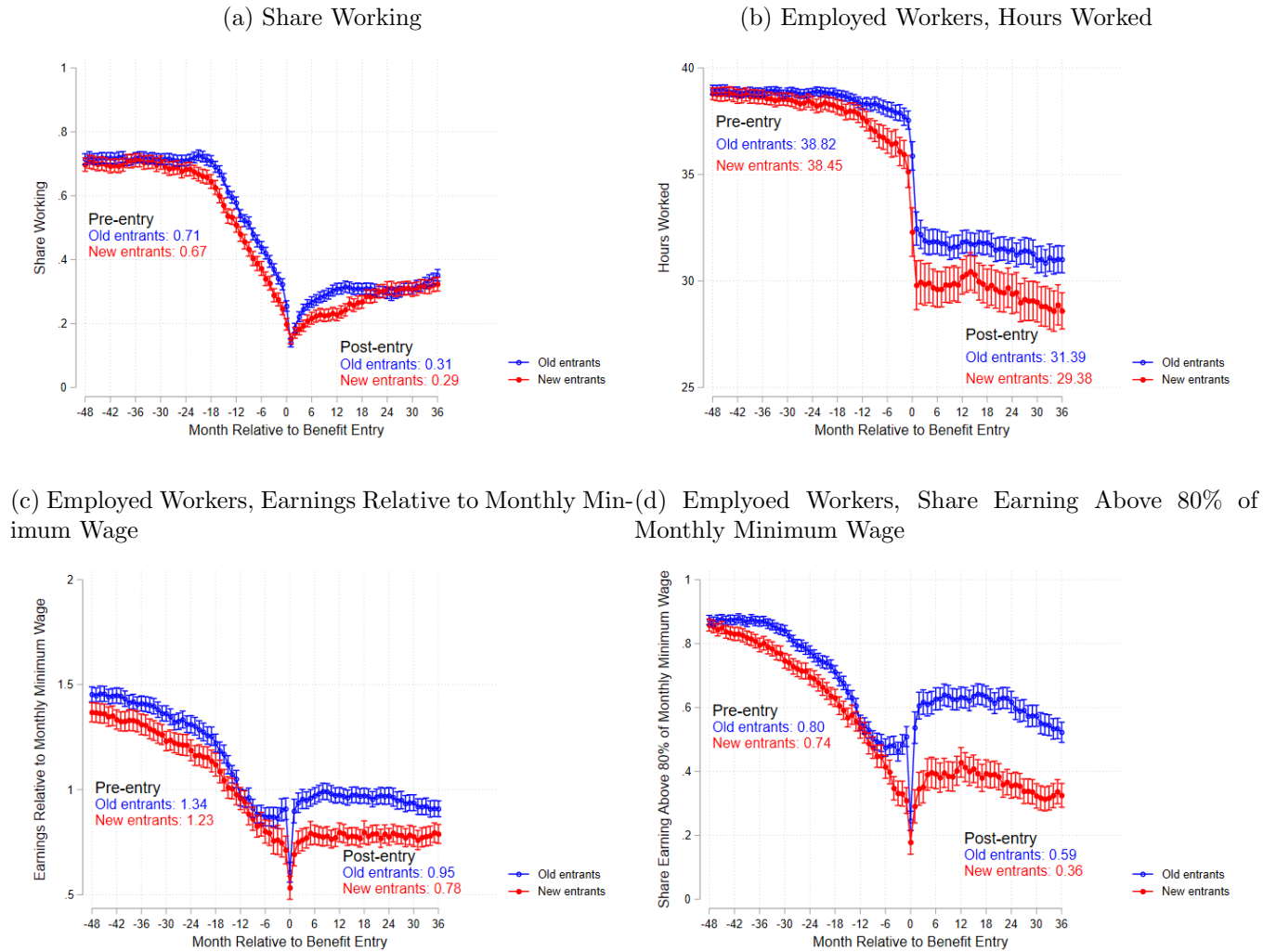
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Figure 1: Labor Market Outcomes of Regular Social Assistance Entrants

(a) Share Working

(b) Employed Workers, Hours Worked

(c) Employed Workers, Earnings Relative to Monthly Min-imum Wage

(d) Employed Workers, Share Earning Above 80% of Monthly Minimum Wage

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.

Figure 2: Other Outcomes

(a) Benefit Persistence
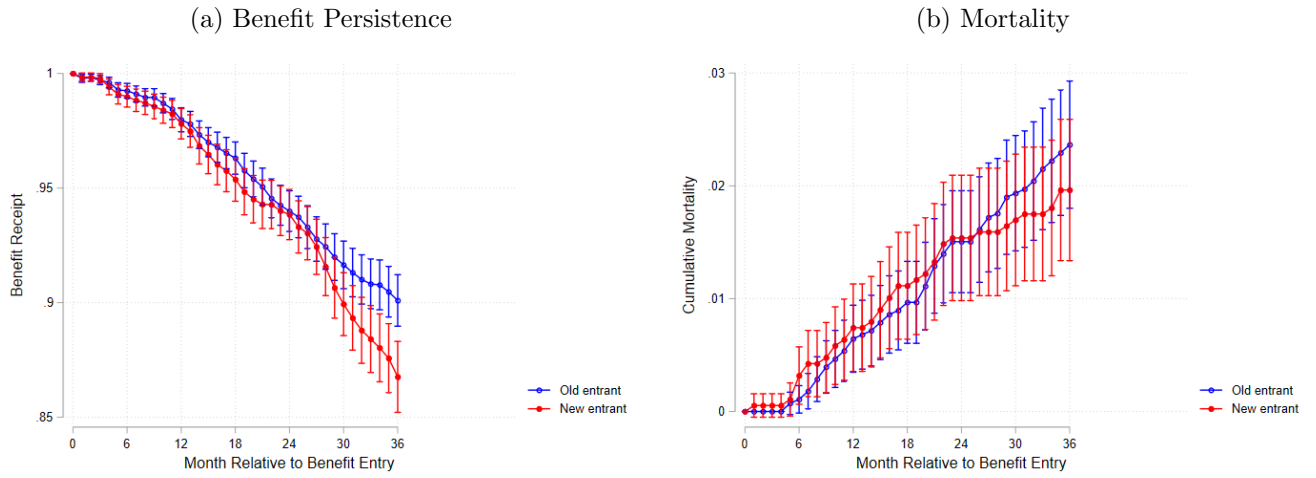
(b) Mortality

Notes: Figure shows benefit persistence in Panel (a), and cumulative mortality 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).

Figure 3: Heterogeneity By Pre-Disability Earnings

(a) Pre-Disability Earnings Below Monthly Minimum Wage

(b) Pre-Disability Earnings Above Monthly Minimum Wage
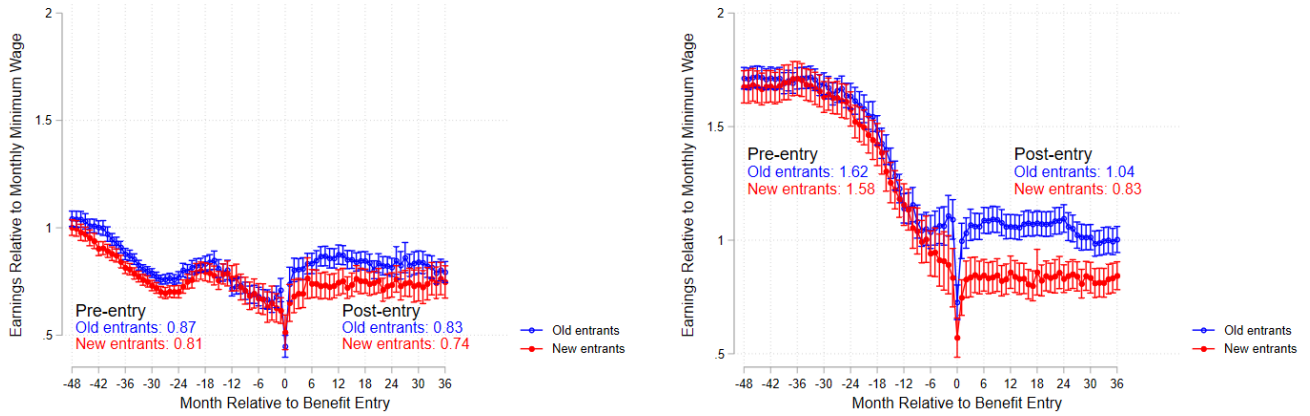
Notes: Figure shows wages relative to the minimum wage for individuals who enter Regular Social Assistance (RSA) the year before the reform, between January 1 and December 31, 2007 ("old entrants" in blue) and those who enter the year after the reform, between January 1 and December 31, 2008 ("new entrants" 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.

Table 1: Regular Social Assistance Entrants Before and After the Reform

	New Entrants	Old Entrants	p-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.

Table 2: Labor Market Outcomes of Regular Social Assistance Entrants

	Working							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	2006		2007		2008		2009	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Difference	0.00213	-0.00984	0.0340***	0.0259***	-0.0314***	-0.0179	0.0431***	0.0740***
	(0.00757)	(0.00804)	(0.00838)	(0.00964)	(0.00997)	(0.0115)	(0.0105)	(0.0128)
Old Entrants	0.666	0.296	0.672	0.286	0.706	0.312	0.675	0.294
Observations	196,876	227,807	229,240	170,177	168,066	110,397	136,295	89,632

	Hours Worked (Conditional on Working)							
	2006		2007		2008		2009	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Difference	0.0332	-0.0998	0.0786	-0.425	-0.360**	-2.056***	-0.0323	-0.490
	(0.105)	(0.253)	(0.107)	(0.294)	(0.148)	(0.423)	(0.167)	(0.477)
Old Entrants	38.68	31.91	38.75	31.81	38.82	31.39	38.46	29.33
Observations	125,053	61,046	149,845	46,777	107,070	30,034	82,464	25,147

	Earnings Relative to Monthly Minimum Wage (Conditional on Working)							
	2006		2007		2008		2009	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Difference	0.0707***	0.0420**	0.0480**	-0.0435**	-0.101***	-0.167***	0.00897	-0.00916
	(0.0166)	(0.0172)	(0.0194)	(0.0209)	(0.0245)	(0.0237)	(0.0251)	(0.0223)
Old Entrants	1.201	0.95	1.292	0.992	1.34	0.949	1.239	0.781
Observations	132,503	66,856	158,641	50,726	117,566	33,973	95,801	29,986

	Earnings Above 80% of the Monthly Minimum Wage (Conditional on Working)							
	2006		2007		2008		2009	
	Pre	Post	Pre	Post	Pre	Post	Pre	Post
Difference	0.0292***	0.0215	0.0287***	-0.0372**	-0.0618***	-0.237***	0.0178*	-0.0240
	(0.00760)	(0.0138)	(0.00715)	(0.0163)	(0.00911)	(0.0195)	(0.0103)	(0.0201)
Old Entrants	0.731	0.609	0.777	0.631	0.806	0.594	0.744	0.357
Observations	132,503	66,856	158,641	50,726	117,566	33,973	95,801	29,986

Standard errors (in parentheses) are clustered at individual level ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

Notes: Table shows labor market outcomes for individuals who enter Regular Social Assistance (RSA) the year before and after the reform (2008 in columns (5) and (6)) and Placebo reforms (2006, 2007, and 2009 in columns (1), (2), (3), (4), (7), and (8)). In each column, the third row reports the mean for "old entrants". "Old entrants" are individuals who enter RSA between January 1 and December 31 of the year before the (Placebo) reform. The first row reports the difference between "old entrants" and "new entrants". "New entrants" are individuals who enter RSA between January 1 and December 31 of the year of the (Placebo) reform. The pre-entry columns are defined over the period between four years to one year (months -48 to -13) before entering RSA and the post-entry columns are defined over the period between one and three years (months 13 to 36) after entering RSA.

Online Appendix

Appendix Figure A1: Number of Regular Social Assistance Entrants
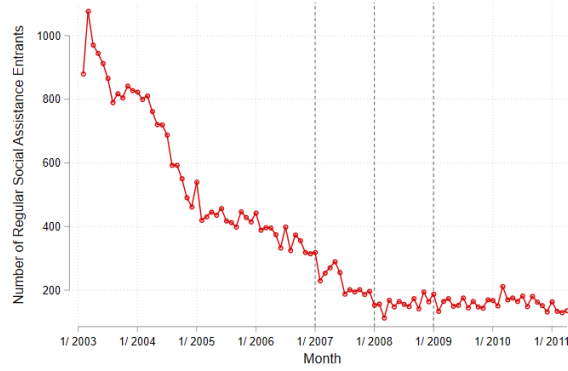
Notes: Figure shows the number of beneficiaries entering Regular Social Assistance (RSA) by month between January 2011 and April 2011. The vertical lines mark our main sample period. Entrants between January and December 2007 are considered "old entrants" and entrants between January and December 2008 are considered "new entrants" in our analysis.
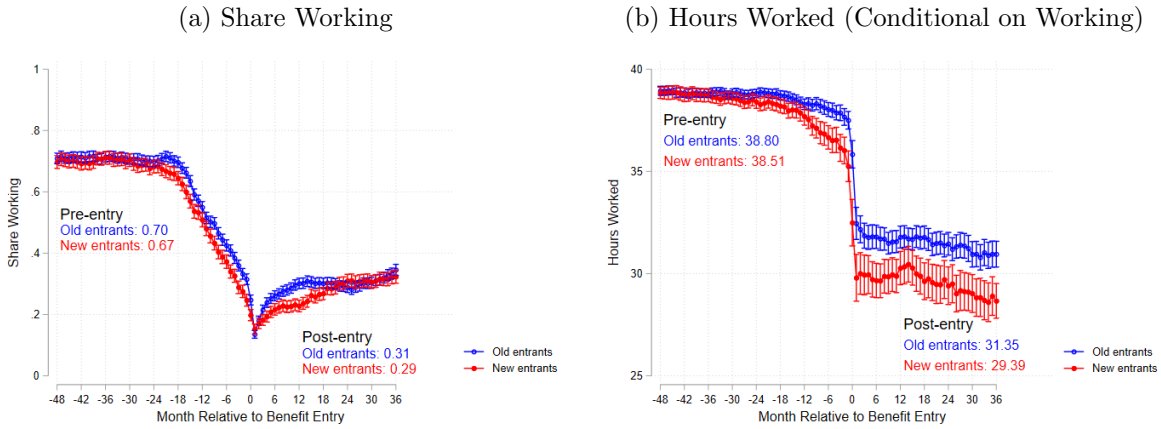
Appendix Figure A2: Sick Leave Use
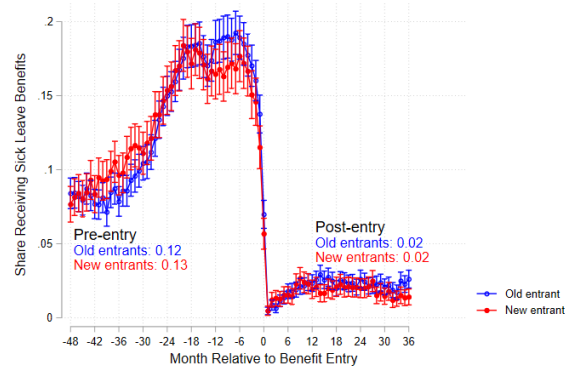
Notes: Figure shows sick leave use for individuals who enter Regular Social Assistance (RSA) the year before the reform, between January 1 and December 31, 2007 ("old entrants" in blue) and those who enter the year after the reform, between January 1 and December 31, 2008 ("new entrants" in red). The 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.

Appendix Figure A3: Wage Distribution in 2009 (Conditional on Working)

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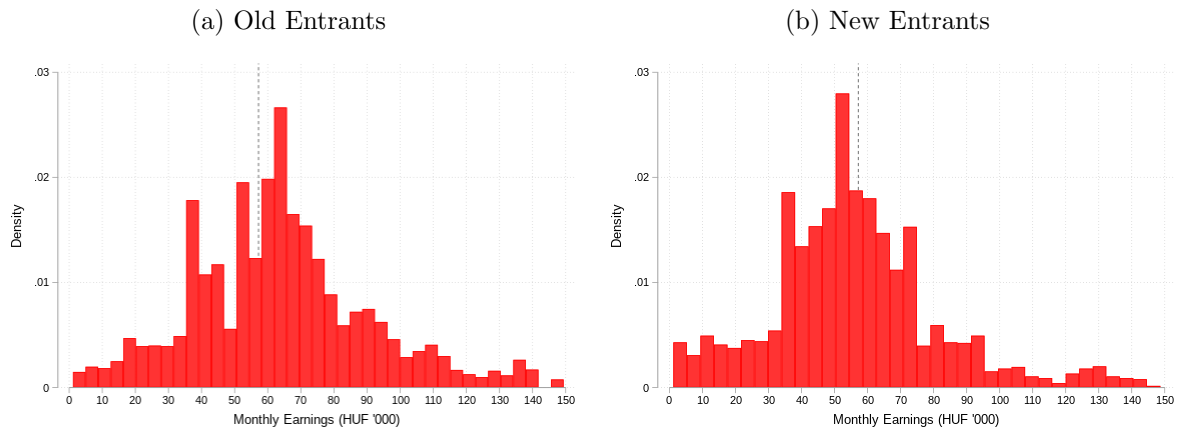
Notes: Figure shows the distribution of monthly wages observed in 2009. Panel (a) shows wages for individuals who enter Regular Social Assistance (RSA) the year before the reform, between January 1 and December, 2007 ("old entrants") and panel (b) shows wages for individuals who enter the year after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

Appendix Figure A4: Reweighting

(a) Share Working

(b) Hours Worked (Conditional on Working)

(c) Wage Relative to Minimum Wage (Conditional on Working)

### (d) Share Earning Above 80% of Minimum Wage (Conditional on Working)
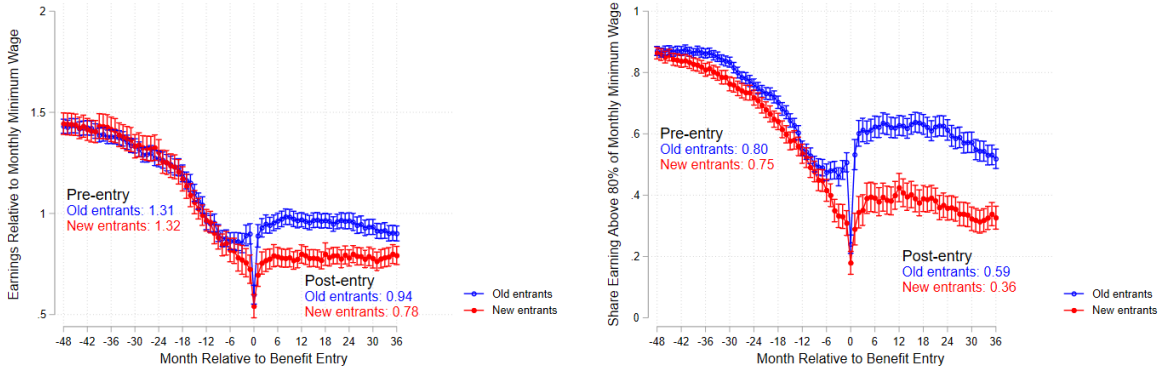
Notes: Figure shows difference-in-differences even study estimates of labor market outcomes relative to Regular Social Assistance (RSA) entry, comparing individuals who enter the year before the reform, between January 1 and December 31, 2007 ("old-entrants") and those who enter the year after the reform, between January 1 and December 31, 2008 ("new-entrants"). The 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.

Appendix Figure A5: Placebo Analyses: Hours Worked

(a) 2006

(b) 2007

(c) 2009
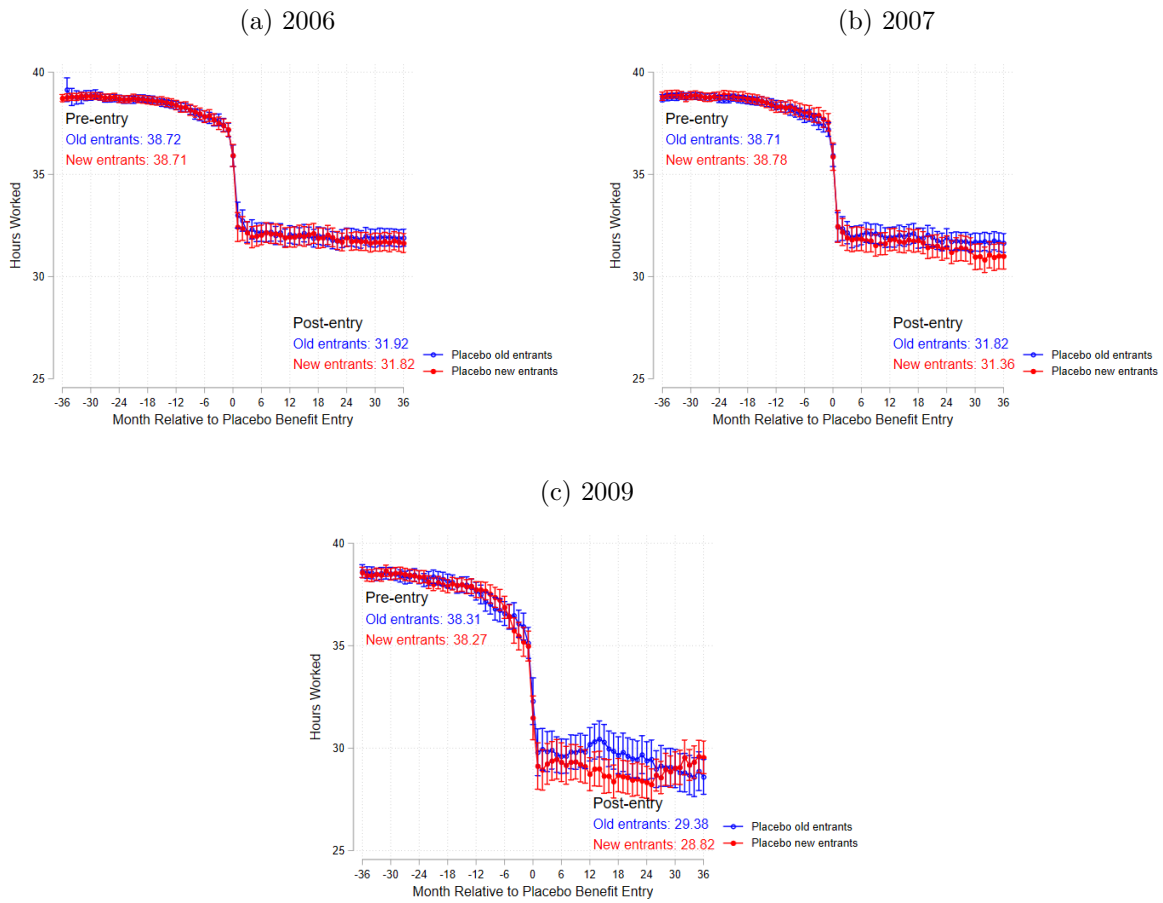
Notes: Figure shows hours worked for individuals who enter Regular Social Assistance (RSA) the year before and after three Placebo reforms in 2006, 2007 and 2009. In each of the panels "old entrants" in blue are individuals who enter RSA between January 1 and December 31 of the year before the Placebo reform year (2005, 2006, and 2008) and "new entrants" in red are individuals who enter RSA between January 1 and December 31 of the Placebo reform year (2006, 2007, and 2009). The pre-entry labels show the mean of hours worked 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 hours worked during the period between one and three years (months 13 to 36) after entering RSA.

Appendix Figure A6: Placebo Analyses: Earnings Relative to Monthly Minimum Wage

(a) 2006

(b) 2007

(c) 2009
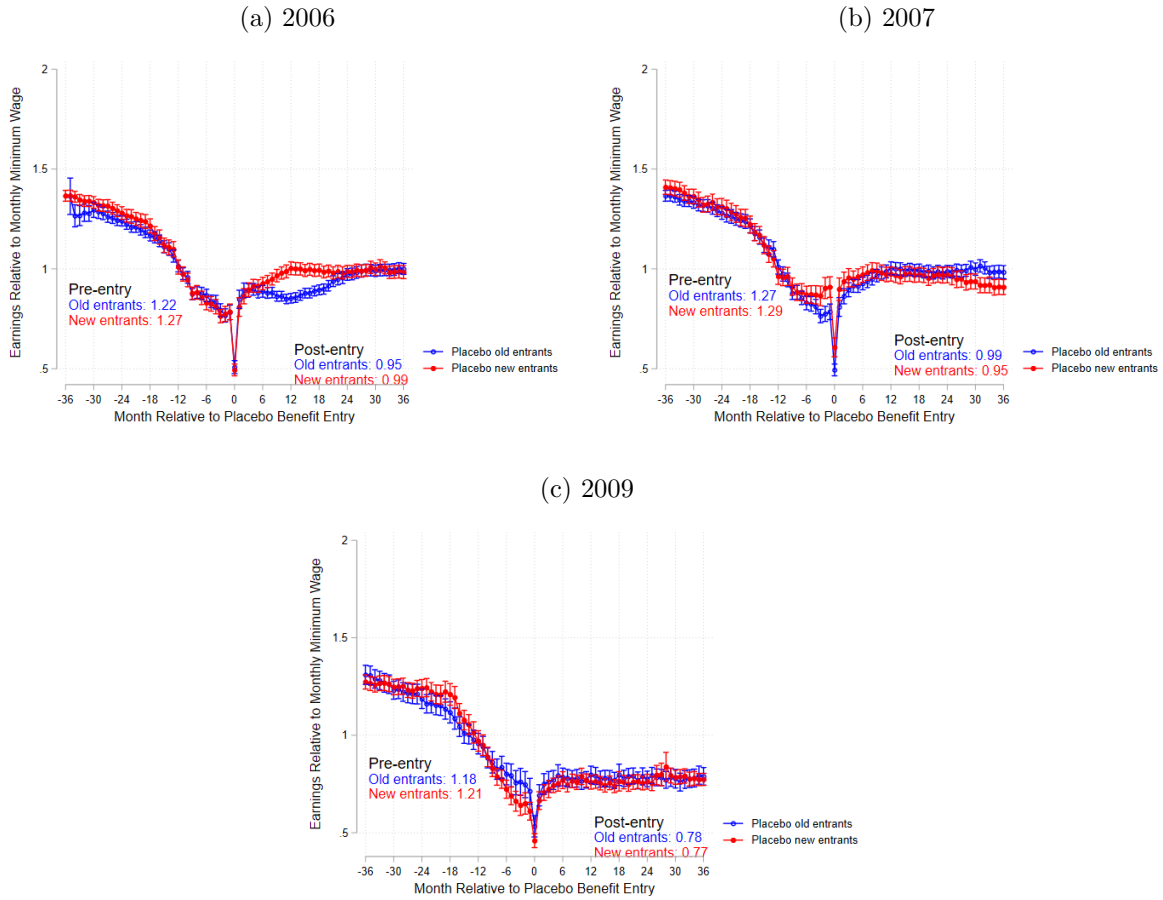
Notes: Figure shows earnings relative to the monthly minimum wage for individuals who enter Regular Social Assistance (RSA) the year before and after three Placebo reforms in 2006, 2007 and 2009. In each of the panels "old entrants" in blue are individuals who enter RSA between January 1 and December 31 of the year before the Placebo reform year (2005, 2006, and 2008) and "new entrants" in red are individuals who enter RSA between January 1 and December 31 of the Placebo reform year (2006, 2007, and 2009). The pre-entry labels show the mean of earnings relative to the monthly minimum wage 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 earnings relative to the monthly minimum wage during the period between one and three years (months 13 to 36) after entering RSA.