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The employment effects of disability benefit reassessment☆

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HIGHLIGHTS

- We study a disability insurance reassessment reform in Hungary using variation generated by a birthday cutoff.
- In line with a perceived threat of benefit loss, recipients reduced work even before cuts, despite no binding earnings limit.
- Benefit removal increased employment gradually, but many experienced periods without work or benefits.
- Overall employment and earnings impacts were negative in the short term and near zero in the medium term.
- Reassessments affect work not only through realized benefit loss but also through responses to perceived eligibility risk.

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ABSTRACT

We study the consequences of a large-scale reassessment of the health and working capacity of disability insurance beneficiaries in Hungary. Leveraging birthday and health cutoffs in the reassessment, we estimate employment responses to the threat of disability benefit loss and the actual termination or reduction of benefits. We show that due to the threat of benefit removal, many beneficiaries stopped working after the reassessment was announced but before it took place. Another group of beneficiaries lost their benefits and increased their labor supply significantly. Overall, the reassessment had a negative short-term and neutral medium-term impact on employment and earnings. Our results suggest that unintended responses to the threat of benefit loss operate as a substitution effect that can reduce labor supply even without binding formal earnings limits, and thus should be considered in the design of reassessments.

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1. Introduction

The rise in disability benefit rolls in developed countries during the 1990s (OECD, 2010) and low employment levels among beneficiaries have prompted policymakers to examine how disability insurance (DI) program design can facilitate labor market reintegration. Among other tools, proposals often include improving financial incentives for work and better identification of remaining working capacity (Autor and Duggan, 2010; Burkhauser and Daly, 2011; Maestas, 2019).

DI programs typically use medical reviews to identify remaining working capacity and determine initial eligibility. Beyond the initial screening, many countries also have periodic reassessments like the Continuing Disability Review (CDR) in the United States. A few countries have implemented broad reassessments of existing beneficiaries during comprehensive reforms. For example, such reassessments were carried out in the Netherlands (1994–2001 and 2004–2008), the United Kingdom (starting in 2013), and Australia (2014–2015). Reassessments serve to identify individuals who would be able to return to work or work more if they are already working and whose benefits should therefore be cut or terminated. While they can increase the efficiency and targeting of DI programs, they have welfare costs for beneficiaries. They may also have unintended consequences: beneficiaries may work less or stop working altogether if they believe that working while receiving benefits, even if earning below formal earnings limits, would put their benefits in jeopardy during a reassessment.

In this paper, we study the impact of a large-scale reassessment reform on labor supply in a context where benefit receipt and employment are commonly combined. Starting in 2012, Hungarian DI beneficiaries born in 1955 or after with health impairments below 80% had to undergo a reassessment to remain eligible for benefits. As a result, about 18,000 beneficiaries (9% of the reassessed beneficiaries and 5% of all beneficiaries) stopped receiving benefits while about 12,000 beneficiaries (6% of the reassessed beneficiaries and 4% of all beneficiaries) had their benefits reduced by the end of 2015. We study the labor market consequences of the reassessment by leveraging these birthday and health cutoffs. Due to a 2011 policy that made it easier for women to retire early, we focus on men in our main analysis, but show that effects were similar for women.

The reform was announced on November 20, 2011 by the government, was adopted by Parliament on December 20 and took effect on December 31. It set a deadline of March 31, 2012 for beneficiaries to declare their intention to undergo reassessment. Beneficiaries who did not declare their participation in the reassessment by this deadline had their benefits terminated on May 1, 2012 when reassessments of participating beneficiaries started. The precise timeline is important for two distinct reasons. First, the short adoption timeline means that conventional anticipation effects are unlikely and indeed we detect no pre-trends when comparing beneficiaries born just before and after the birthday cutoff for reassessment. Second, this timeline allows us to detect changes in employment during the first four months of 2012, the period when the reform had already been adopted but the reassessments had not yet started.

To estimate the impact of the reassessment policy, we examine the labor market outcomes of beneficiaries with health impairments below 80% born in 1955, just after the birthday cutoff relative to those born in 1954, just before the birthday cutoff. We show that the reassessment policy had two countervailing effects on employment. On the one hand, the announcement of reassessments was followed by a decline in employment among affected benefit recipients starting in January 2012 before any reassessments actually took place. We interpret this as a response to the perceived threat of benefit loss—a type of substitution effect. Some individuals withdrew from employment (or decided not to enter employment) to improve their chances of receiving a more favorable reassessment outcome. On the other hand, there was a group of beneficiaries who lost their benefits and consequently entered employment or increased their earnings if already working.

To isolate the countervailing effects of the reassessment without relying on endogenous selection into benefit loss, we predict each beneficiary's probability of disability benefit loss using pre-reform characteristics and analyze employment responses by predicted risk. We focus on exits from DI rather than benefit reductions because exits are well-defined, homogeneous outcomes that do not rely on ad hoc cutoff values. This also simplifies the interpretation of DI status and employment results relative to analyzing exits and benefit reductions jointly. We classify beneficiaries into three groups: low, medium, and high predicted probability of benefit loss. Medium-probability beneficiaries reduced employment the most after the reassessment was announced but before it took effect, consistent with this group facing the strongest perceived tradeoff between continued employment and benefit retention. In contrast, low-probability beneficiaries may have expected to retain benefits regardless of employment status, while high-probability beneficiaries may have perceived benefit loss as inevitable whether or not they remained employed. Consistent with this interpretation, among the medium-probability group we observe a decline in benefit receipt with concurrent employment and an increase in benefit receipt without employment, on average over four years after the reassessment reform. In the high-probability group, employment rises gradually; however, a substantial share of this group does not find employment immediately, leading to an increase in the fraction of individuals with neither benefits nor employment.

Because the group of beneficiaries who stopped working and kept their benefits was larger than the group for whom the loss of benefits had a gradually increasing positive employment effect, the overall employment effect was initially negative. The decrease in overall employment was the largest during the first post-reform year, before gradually dissipating as some of those recipients who stopped working to keep benefits restarted working and as the positive employment effect for beneficiaries who lost benefits became stronger. It took more than a year for most of the positive employment effect to take hold, suggesting that beneficiaries who lost their benefit needed time to find jobs. However, even with this increase, the overall effect remained close to zero by the end of the four-year period.

Our findings have important implications for the design and impact of DI reassessments. While reassessments of broad populations are rare, periodic individual-level reassessments are more common. In some contexts, the belief that too-high earnings can lead to benefit loss even if income remains below the earnings limit could cause beneficiaries to limit their labor supply. Thus the negative labor supply effects documented here should be taken into account by policymakers and balanced against cost savings and the potential positive labor supply effects of benefit reduction or termination.

Our work most directly contributes to the literature on the consequences of broad reassessments of existing DI beneficiaries in the Netherlands (Borghans et al., 2014; García-Gómez and Gielen, 2018; Garcia-Mandicó et al., 2020) and Australia (Badji et al., 2023). This literature has found that beneficiaries have substantial residual earnings capacity and removal from benefits leads to increased labor supply. At the same time, some benefits are substituted by other programs (Borghans et al., 2014) and there can be unintended health impacts (Badji et al., 2023). Other studies of smaller scale DI removals (e.g., Moore, 2015) and the U.S. CDR system (e.g., Hemmeter and Bailey, 2016; Anderson et al., 2025) have found more limited employment impacts. The results of this paper suggest that while some recipients had remaining working capacity that allowed them to find employment following DI benefit loss, other beneficiaries reduced their earnings to avoid losing their benefits even if they were earning well below the legal earnings limit. A similar mechanism is discussed by Deshpande (2016a) in the context of Supplemental Security Income (SSI) in the U.S. The author argues that SSI may have implicit incentive effects if recipients believe that human capital investment or working increases their chance of losing their benefits during medical reviews.

More broadly, we contribute to the literature on the work disincentives of DI programs (Bound, 1989; Gruber, 2000; Chen and van der Klaauw, 2008; Maestas et al., 2013; French and Song, 2014; Mullen and Staubli, 2016; Gelber et al., 2017). Using various quasi-experimental approaches, these papers find that disability insurance receipt substantially reduces labor supply. Our main contribution to this literature, along with other papers studying reassessment reforms, is the examination of the consequences of benefit removal among existing beneficiaries. The response to newly receiving vs losing benefits could be asymmetric for several reasons, including the costs of searching for a new job when losing benefits or the loss of working capacity among longtime beneficiaries. We find that some, though far from all, of these beneficiaries can return to work after a benefit loss, though finding employment takes considerable time. However, because other beneficiaries stop working to retain their benefits, the overall employment effect is negative in the short term and neutral in the medium term.

Finally, this work also speaks to the academic and policy literature that considers the fiscal sustainability of DI programs (e.g., Autor and Duggan, 2006, 2007; Autor, 2011; Liebman, 2015). We show that reassessments can reduce DI rolls as many recipients are able to find work. However, policymakers need to be aware of unintended employment effects and potential negative welfare impacts for beneficiaries whose benefits are removed or reduced but who are unable to find employment.

2. Background

2.1. Disability insurance in Hungary

In 1990, the Hungarian DI system was characterized by lenient eligibility rules and relatively high benefit levels (Scharle, 2008). The deep recession following the transition from socialism to a market economy rapidly increased unemployment in the early 1990s and policymakers allowed DI and early retirement programs to expand to ease social and political tensions (Vanhuysse, 2004). The number of DI beneficiaries doubled between 1990 and 2003, reaching 12% of the working-age population, the highest rate among OECD countries (OECD, 2016).

Following cautious and largely ineffective attempts to tighten the eligibility criteria in the late 1990s, a 2008 reform aimed to curb the inflow into the system by prioritizing rehabilitation and encouraging labor market integration (Scharle, 2008). While the employment effect of the expanded rehabilitation programs was positive, their take-up, as well as the impact of the reform on DI spending fell below expectations.

The focus of this paper is a 2012 reform which tightened eligibility and reduced benefit levels not only for new claimants but also for existing beneficiaries (Nagy, 2015; Kovács, 2019; Hornok, 2025). The aim was to curb inflow and reactivate beneficiaries with some remaining working capacity in order to improve the sustainability of the DI system. As a consequence of the two subsequent reforms, as well as favorable demographic and economic trends, the share of beneficiaries decreased to 4% of the active population and the cost of DI benefits decreased to below 1% of GDP by 2017, one of the lowest values in Europe.

2.2. Details of the 2012 reform

Before the 2012 reform, the frequency of health reassessments was irregular, prescribed on an individual basis during the initial assessment and depended mainly on the likelihood of recovery. Most beneficiaries were never reassessed after initial benefit receipt. The 2012 reform obliged approximately 200,000 DI recipients to undergo a health review based on new, stricter rules of entitlement. The obligation applied to DI recipients born in 1955 or later, whose health impairment, as measured by the pre-reform system, was above 40% (the minimum for benefit eligibility) and below 80%. Those born before 1955 or with at least 80% health impairment were exempt from reassessment and thus unaffected.

The reform was announced by the government on November 20, 2011, adopted by Parliament on December 20, and took effect on December 31. Beneficiaries affected by the reform had to declare by March 31, 2012 whether they wished to undergo the health reassessment. If they failed to do so, their benefit entitlement was terminated on May 1, 2012 (International Labour Organization, 2011). Otherwise, their health status and employability were reevaluated according to the post-reform rules in a complex assessment procedure.

The post-2012 assessment procedure considered both health status and employment prospects, including the potential for vocational rehabilitation. The reassessments were carried out by a team of at least two physicians (one serving as chair), an employment rehabilitation expert, and a social welfare expert. The physicians assessed health status, and individuals with impairments above 40% remained eligible for benefits. For those judged moderately disabled, the experts evaluated employment and rehabilitation prospects, taking into account education, work experience, training options, and local labor market conditions.

Mainly due to administrative capacity constraints, the reassessment process was not fully completed until 2016, although our data suggest that most benefit removals occurred in 2012. During this period, about 18,000 beneficiaries (9% of those reassessed and 5% of all beneficiaries) permanently exited DI, and the benefits of an additional 12,000 beneficiaries (6% of those reassessed and 4% of all beneficiaries) decreased in inflation-adjusted terms. Throughout the entire period covered by our analysis, the level of DI benefits was determined by pre-disability earnings and the assessment outcome, with lower health impairment associated with lower benefits, ceteris paribus. The average monthly DI benefit among recipients aged 56–57 was $320–330 in 2011 (Table 1), compared with a monthly minimum wage of $388.

While our analysis focuses on individuals who were already receiving benefits prior to the reform, the total number of DI recipients decreased by more—from 473,000 in January 2012 to 355,000 in January 2017 (Hungarian Central Statistical Office, 2022). This decline was due to several factors: (i) a large drop in inflows, which began in the early 2000s and gained new momentum after 2012, suggesting that although formal eligibility conditions did not change, the assessment process became more stringent; (ii) demographic change, as the size of cohorts in their 50s decreased during this period; (iii) exits from DI through retirement or death of a large cohort of beneficiaries who entered during the 1990s; and (iv) economic recovery.

Table 1
Summary statistics.

	Born in 1954 (Control)	Born in 1955 (Treatment)	p-value of equality test
Months of employment in 2011	2.1	2.2	0.336
Months of employment in 2009–2011	6.4	6.6	0.313
Months of public work in 2011	0.01	0.01	0.756
Monthly DI benefit in 2011 (USD)	332	319	0.000
Monthly earnings in 2011 (USD)	39	38	0.822
Monthly total income in 2011 (USD)	371	358	0.000
Length of DI status in Dec 2011 (years)	11.2	11.4	0.068
Health impairment < 50% in Dec 2011	0.17	0.18	0.153
Drug spending in 2011 (USD)	727	755	0.480
Primary care visits in 2011	12.5	12.9	0.014
Outpatient specialist visits in 2011	12.7	13.2	0.040
Hospital days in 2011	6.2	6.7	0.132
Micro-region level unemployment rate in 2011	0.19	0.20	0.000
Pre-reform occupation			
Skilled	0.35	0.33	0.003
Unskilled	0.17	0.18	0.153
Missing	0.48	0.50	0.090
Number of individuals	7281	6364	

Note: Table shows summary statistics for the control and treatment groups. The sample is restricted to men with a health impairment below 80% and aged 56 or 57 as of December 2011, who received DI throughout 2011. The treatment group includes individuals born in 1955 (aged 56 in December 2011) and the control group includes individuals born in 1954 (aged 57 in December 2011). Occupation classification is based on the last observed pre-reform employment.

2.3. Employment while receiving DI benefit

During the period covered by our analysis, employment was permitted while receiving DI benefits, subject to earnings limits beyond which benefits were terminated. These earnings limits varied by disability level and across years (see Appendix Table A.1). In the pre-reform period, DI recipients covered by our study faced individually determined earnings limits. For those with 50% to 79% health impairment (75% of our analysis sample of DI beneficiaries born in 1954 or 1955), the limit was set at 90% of pre-disability earnings in 2009 and at 200% of the benefit amount in 2010-2011.[1] Beneficiaries with less than 50% health impairment (25% of our analysis sample) were allowed to accumulate earnings up to 80% of their pre-disability earnings.[2] After the 2012 reform, the earnings limit was standardized at 150% of the monthly minimum wage and applied to beneficiaries who underwent reassessment and remained eligible for DI benefits.

These earnings limits were significantly higher than those in the U.S. both before and after the reform. In the U.S. the Substantial Gainful Activity (SGA) limit is $1620 per month for non-blind individuals in 2025. The federal minimum wage is $1257 per month (assuming a 40-hour workweek), while the national average monthly minimum wage is around $1600. Median weekly earnings for full-time workers were $1196 in Q2-2025 (Bureau of Labor Statistics, 2025), corresponding to $5143 per month. Thus, the U.S. SGA limit is around the average state minimum wage and around 31% of median full-time earnings. By contrast, in Hungary, the earnings limits for the average beneficiary were 192% of the minimum wage and 98% of median earnings before the reform and 150% of the minimum wage and 85% of median earnings after the reform.

We argue that these earnings limits were not formally binding for most beneficiaries in our sample and were therefore unlikely to have played a major role in work decisions. Appendix Fig. A.1 displays earnings relative to the applicable limits for beneficiaries born in 1954 or 1955 under three scenarios: (a) pre-reform earnings against pre-reform limits, (b) pre-reform earnings against the post-reform limits, and (c) post-reform earnings against the post-reform limits. Panel (a) confirms that only a small share of beneficiaries earned just below or even close to the limit before the reform. For instance, 84% of beneficiaries earned less than 75% of the earnings limit in 2011.[3] Panels (b) and (c) show that the post-reform limit remained high relative to beneficiaries' earnings. The vast majority earned, in 2011, well below a hypothetical earnings limit – defined as 150% of the 2011 minimum wage – that would have applied had the post-reform rule been in effect at that time (panel (b)). Hence, the reform did not appear to materially tighten the effective earnings constraint. Earnings in the post-reform years (panel (c)) also remained well below the limit, with 80% of beneficiaries earning less than 75% of it.

An alternative explanation could be that beneficiaries do restrict their labor supply but setting work hours to meet the threshold or be close to it is difficult and beneficiaries give themselves a buffer from the earnings limit when deciding how much to work. 47% of beneficiaries work part-time and Appendix Fig. A.2 displays the earnings distributions for these beneficiaries. These beneficiaries are even further away from the threshold: 92% of them earn less than 75% and 65% earn less than 50% of the threshold in the post-reform period. Thus the low share of beneficiaries near the earnings limit is unlikely to be driven by the difficulty of setting hours close to the threshold. Instead, it appears that most beneficiaries work in jobs that pay much less than the earnings threshold.

While formal earnings limits do not appear to have been binding for most beneficiaries, beneficiaries may have a perception that working would decrease their chances of retaining benefits under reassessment. In fact, our results below suggest that this was likely the case during the 2012 reform, as employment decreased among previously employed beneficiaries after the reassessment was announced, but before it took place, and before any benefits were lost.

3. Data

Our analysis is based on an individual-level linked employer-employee administrative panel database, covering a randomly selected half of the population of Hungary in 2003, followed until 2017. The database consists of linked datasets at the monthly frequency of the pension, tax and health care authorities and contains detailed individual-level information on employment and earnings histories, healthcare use, and receipt of pension, disability, unemployment, and other benefits, along with firm-level indicators.[4]

Despite its rich administrative detail, the data have certain limitations relevant to our analysis. First, although coverage begins in 2003, the employment status of DI recipients is observed only from April 2007. Second, the reassessment procedure itself is not recorded: although we observe changes in benefit amounts and terminations, the underlying causes are not identifiable, preventing direct attribution of these changes to the reform. Finally, the data lack information on the underlying health conditions for benefit receipt. Based on the 2011 census (Appendix Table A.2), the majority of DI recipients suffer from long-lasting diseases and mobility impairment is the most prevalent form of disability.

When estimating the effects of the reform, we analyze the following monthly indicators of employment and DI status. DI status is a binary variable that takes the value one if the individual is receiving DI benefits in a given month and zero otherwise.[5] The binary variable for employment status equals one if the individual is employed on the 15th of the given month and zero otherwise. Our employment definition includes paid and self-employment as well as participation in the public work program, an important active labor market policy at the time. Since in Hungary many DI recipients also work, in addition to analyzing the impact of the reform on overall employment and DI receipt, we examine possible combinations of the two outcomes. We generate four mutually exclusive and exhaustive binary outcome variables: (1) DI benefit receipt without concurrent employment (DI & no employment); (2) DI benefit receipt with concurrent employment (DI & employment); (3) employment without concurrent DI receipt (employment & no DI); and (4) no income from either DI benefits or employment (no DI & no employment).

We also examine three monthly measures of income, all deflated to 2011 and measured in US dollars: total income, DI benefit income, and earnings from employment. Total income is the sum of DI benefits and earnings from employment.

[1] Pre-disability earnings are defined as the average of valorized earnings from the entire pre-disability period. Based on the database described in Section 3, and owing to data limitations, we approximate pre-disability earnings using earnings from the 12 months preceding benefit entry.

[2] This limit applied to beneficiaries with less than 50% health impairment who entered DI no later than December 31, 2007. Those entering on or after January 1, 2008 faced a more stringent earnings limit (Krekó et al., 2024). We exclude this group – comprising about 4% of beneficiaries born in 1954 or 1955 – from our sample to focus on beneficiaries without binding earnings limits.

[3] Some observations appear above the limit because benefits were not terminated immediately once earnings exceeded the threshold.

[4] For a comprehensive overview of the administrative data, see Sebők (2019).

[5] Since short gaps in DI eligibility may occur for administrative reasons, we smooth the DI status variable as follows. If an individual does not receive DI for at most 3 months, we fill in such gaps in DI receipt if the following two conditions hold: (1) received DI both before and after, (2) receives an extra one-off DI benefit payment after the DI gap which amounts on a monthly basis to at least half of the regular DI benefit payment before the gap.

4. Empirical framework

4.1. Control and treatment groups

To identify the effect of the reform on affected beneficiaries, i.e., those with health impairments below 80% born in 1955 or later, we exploit the birthday cutoff of the policy. We focus on a narrow window around the cutoff, assuming that outcomes for individuals born just before and after it would have evolved similarly in the absence of the reform.

Our sample contains beneficiaries with health impairments below 80% born in 1954 or 1955. Those born in 1955 constitute the treatment group, while those born in 1954 form the control group. For consistency, we restrict the sample to individuals receiving DI throughout 2011 and alive in January 2012; those who died after January 2012 are included until their last year of life. Appendix Fig. A.8 shows that the reform had no effect on mortality over our observation period.

The above restrictions produce a sample composed of 57% women and 43% men. In our baseline estimation, we focus on men and consider the post-reform period up to 2015, the year when the control group reaches the statutory retirement age.[6] Our focus on men is motivated by the "Women 40" policy which since 2011 gives an early retirement option to women with 40 years of work credits, regardless of age. This policy could affect the control and treatment age groups differently, potentially confounding our results for women. However, in a robustness analysis we find similar effects for women.

We provide summary statistics for the treatment and control groups in Table 1. These statistics indicate that the employment history and health status of the two groups are similar—even for the variables for which there are statistically significant differences, the magnitudes of the differences are very small.

Panel (a) of Fig. 1 shows the evolution of the share of individuals receiving DI benefits in our sample, separately for the treatment and control groups. Although the sample is restricted to individuals receiving benefits throughout 2011, no restrictions are imposed on DI status before or after 2011. The figure shows that in 2009 and 2010, the DI status of the two groups evolved very similarly, which suggests that they are likely to be comparable and that absent the reform their status would have evolved similarly. Following the reform, the control and treatment groups diverge: over the next four years, 2% of the control group but 4% of the treatment group are removed from benefits. The bulk of the divergence occurs in May 2012, even though the review process lasted until 2016. Importantly, exit from DI was not always permanent: some individuals later re-entered the program.

4.2. Estimation strategy

To study the impact of the reassessment on the labor market outcomes of the reassessed population, we estimate the following equation:

$$Y_{it} = \sum_{\substack{T=Jan2009 \\ T\neq Dec2011}}^{Dec2015} \beta_T \mathbb{1}[Date_t = T]\mathbb{1}[B_i = 1955] + \gamma \mathbb{1}[B_i = 1955] + \mu_t + \varepsilon_{it} \quad (1)$$

where i indexes individuals, t indexes months, $\mathbb{1}[Date_t = T]$ is an indicator for month T, $\mathbb{1}[B_i = 1955]$ is an indicator for the treatment group of individuals born in 1955, and the μ_t are month fixed effects. Our parameters of interest are the β_T, which capture the differential change in labor market outcomes for treated individuals relative to control individuals in each month compared to December 2011.

For our estimates to capture the causal impact of being subject to the reassessment on the labor market outcomes, the control group must represent a valid counterfactual for the evolution of the treatment group's outcomes. Specifically, we assume that, in the absence of the reassessment, the two groups' labor market outcomes would have followed similar trends. We present several pieces of evidence consistent with this assumption. First, Table 1 shows that the control and treatment groups are similar across various measures of health and employment. Second, Fig. 1 indicates that, prior to the reassessment, the disability status of the control and treatment groups evolved very similarly, suggesting that, absent the reassessment, their outcomes would have followed similar paths. Third, month-specific estimates of the difference in labor market outcomes between the control and treatment groups, presented in Fig. 2, show that all outcomes moved together prior to the reform, further supporting the control group as a valid counterfactual. Fourth, using a placebo approach, we compare the labor market outcomes of disabled individuals in the same age groups who were unaffected by the reform as they had health impairments of at least 80%. There is no evidence of differential changes by age in this group, suggesting that our main results indeed identify the impact of the reassessment on the affected group. Fifth, we also present results for a 2011 placebo reform and find no evidence of differential changes by age in labor market outcomes, in line with our main results being driven by the 2012 reform.

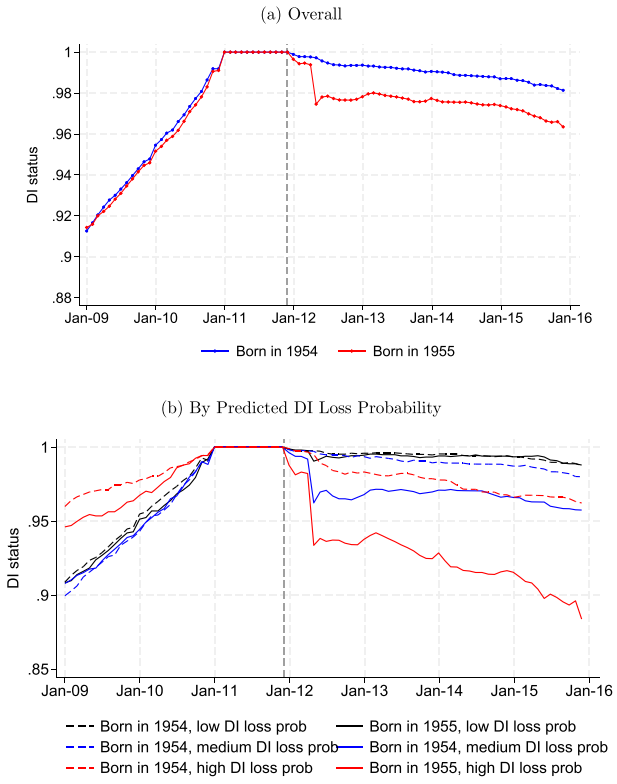


Fig. 1. DI Status. *Note:* Figure shows the share of individuals receiving DI benefits. Panel (a) displays the share of individuals receiving DI benefits separately for the control group of men born in 1954, aged 57 in December 2011 (in blue) and the treatment group of men born in 1955, aged 56 in December 2011 (in red). Panel (b) illustrates the share of individuals receiving DI benefits by predicted DI loss probability. The dashed lines represent the control group of men born in 1954, aged 57 in December 2011. The solid lines represent the treatment group of men born in 1955, aged 56 in December 2011. The sample is split by the predicted probability of DI loss. The sample is restricted to men with a health impairment below 80% as of December 2011 who received DI throughout 2011.

5. Results

5.1. Employment and benefit receipt

We start by presenting our estimates for employment and beneficiary status outcomes. Fig. 2 shows the month-by-month differences between

[6] The retirement age for individuals born before 1952 was 62. Starting with the 1952 cohort the statutory retirement age increased by six months for each successive cohort.

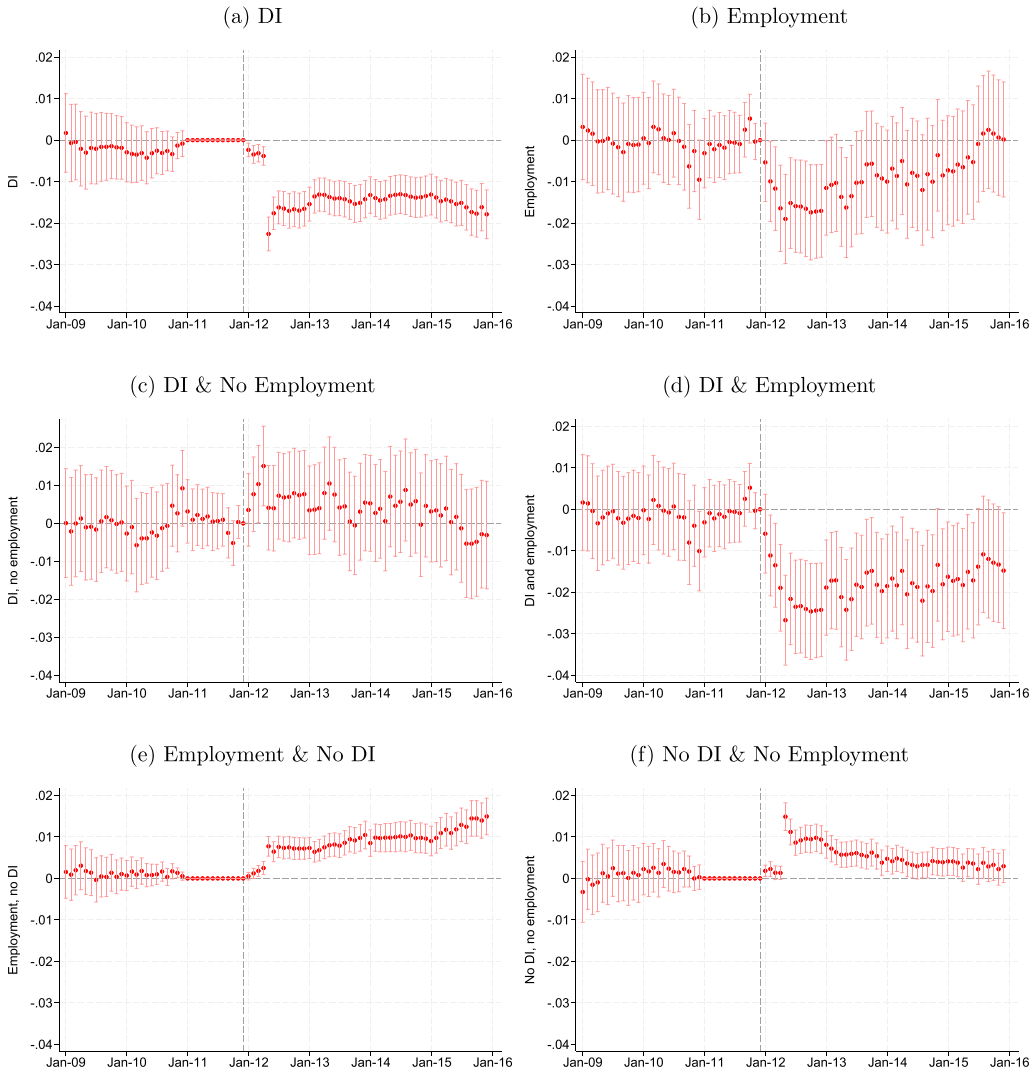


Fig. 2. Impact of Benefit Reassessment: Disability Insurance Receipt and Employment. *Note:* Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated beneficiaries born after the birthday cutoff relative to control beneficiaries born before the birthday cutoff. It displays the estimated β_T coefficients from Eq. (1) with 95% confidence intervals over 2009-2015, with December 2011 as the reference month. The sample is restricted to men with a health impairment below 80% as of December 2011 who received DI throughout 2011. The treatment group includes individuals born in 1955 (aged 56 in December 2011) and the control group includes individuals born in 1954 (aged 57 in December 2011).

control and treatment individuals for each outcome, based on estimating Eq. (1). Estimates through the end of 2011 indicate no significant differences between the groups, consistent with the absence of an anticipation effect.

In line with the reform timeline, panel (a) shows minimal changes in DI receipt up to May 2012, followed by a drop of around 2 percentage points in the treated relative to the control group.[7] Beneficiaries who did not opt for reassessment lost their benefits on May 1, when reassessments began. While we do not directly observe reassessments, the precise timing of this large drop strongly suggests it reflects the removal of these beneficiaries from the program.

Panel (b) shows the evolution of employment, which exhibits a different pattern. Employment among treated beneficiaries began to decline relative to the control group in January 2012 – before the March deadline for declaring participation in the reassessment, the May start of reassessments, and the subsequent removal from benefits. By May 2012, employment of treated beneficiaries was 1.9 percentage points lower than that of control beneficiaries (the employment rate of the treatment group was 18% in December 2011). This pattern suggests that beneficiaries responded to the reform by withdrawing from (or not entering) employment, likely because they perceived that being employed could reduce their chances of retaining benefits during the reassessment. After reassessments began, the negative employment differential between treated and control beneficiaries gradually narrowed, though it took roughly four years to close completely, and the overall employment effect did not turn positive.

Panels (c), (d), (e), and (f) show results for the joint outcomes of DI receipt and employment. In line with panels (a) and (b), panels (c) and (d) confirm that employment among treated DI recipients began to decline relative to control recipients in January 2012, before the reassessments took place. Panel (c) shows that until May 2012, the number of beneficiaries not working increased, while panel (d) shows the complement – a decline in the number of beneficiaries who were employed. Then, in May 2012, approximately 2 percent of treated beneficiaries were removed from the program.

[7] We observe a drop of 2.3% in May 2012, which reduces to 1.8% by June 2012, likely due to transitions between benefit categories.

Fig. 3. Impact of Benefit Reassessment: Income. *Note:* Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated beneficiaries born after the birthday cutoff relative to control beneficiaries born before the birthday cutoff. It displays the estimated β_T coefficients from Eq. (1) with 95% confidence intervals over 2009-2015, with December 2011 as the reference month. The sample is restricted to men with a health impairment below 80% as of December 2011 who received DI throughout 2011. The treatment group includes individuals born in 1955 (aged 56 in December 2011) and the control group includes individuals born in 1954 (aged 57 in December 2011). The income indicators are deflated to 2011.

Panel (e) shows that the share of individuals who lost their benefits and entered employment increased by 0.8 percentage points in May 2012, and continued to increase gradually to 1.5 percentage points by the end of the observation period. Panel (f) indicates that the share of former beneficiaries who lost their benefits without immediately taking up employment jumped in May 2012 and then gradually declined. Together, these panels suggest that while some beneficiaries withdrew from employment to improve their chances of retaining benefits, others lost their benefits and subsequently became more likely to enter employment. The gradual increase in employment, combined with the initial spike in the number of non-employed former beneficiaries without benefits, implies that for many of those affected, re-entering employment took several years.

While women are excluded from the baseline estimation due to concerns that the women-only early retirement option could have led to diverging outcome trends between the (older) control and (younger) treatment groups, the results for women, reported in Appendix Fig. A.3, show effects similar to those for men.

To probe the validity of our results, Appendix Figs. A.4 and Fig. A.5 present two sets of placebo estimations for the employment and beneficiary status outcomes. Fig. A.4 shows placebo regression results for individuals with a health impairment of at least 80% as of December 2011, and thus unaffected by the reform. The figure shows no statistically significant post-reform differences between the outcomes of the two age groups. The patterns indicate that among beneficiaries above the health impairment cutoff, the reform had no impact on the probability of benefit receipt or employment.

The placebo results presented in Fig. A.5 indicate that for a placebo reform in 2011, there were no major pre-reform differences between the placebo treatment and control groups. Panel (e) shows a very small, albeit statistically significant, increase in employment among the placebo treatment group – about one-tenth of the magnitude of our main effects estimated for the actual reform year in Fig. 2. No post-reform differences are observed for other outcomes.

Overall, these placebo analyses suggest that our main findings are driven by the 2012 reassessment reform itself, rather than by spurious differences between the control and treatment groups or by other contemporaneous events affecting them differently.

5.2. Income

We next estimate the effect of the reform on income from benefits and employment, using the full sample without restricting to employed individuals. Panel (a) of Fig. 3 shows that total income, i.e., the sum of DI benefits and labor earnings, began declining in early 2012 and remained lower among treated individuals throughout the period observed. This overall decline is a combination of a fall in benefit income starting in May 2012 (panel (b)), consistent with the drop in benefit receipt shown in Fig. 2, as individuals were removed from the program, and a decline in earnings from work beginning in January 2012 (panel (c)), consistent with the fall in employment. During this period, no other major benefit programs were available to offset the loss of DI benefit income (Scharle and Szikra, 2015). On average, monthly income decreased by $6-7, or around 2%, with lower DI benefits accounting for three-quarters of the drop and reduced earnings for the remainder. Overall, these results suggest that, unlike the findings of Borghans et al. (2014) and Deshpande (2016b), individuals who lost part of their DI benefits due to the reform were unable to compensate through higher earnings or alternative benefits.

We report placebo results for income in Appendix Figs. A.6 and Fig. A.7. Fig. A.6 shows no significant change in income among beneficiaries with a health impairment of at least 80%, although the pre-reform estimates in this placebo group are noisy. Fig. A.7 indicates that a placebo reform in January 2011 had no impact on income.

5.3. Results by predicted benefit loss

To better understand the impact of the threat of benefit removal, we predict each beneficiary's probability of disability benefit loss using

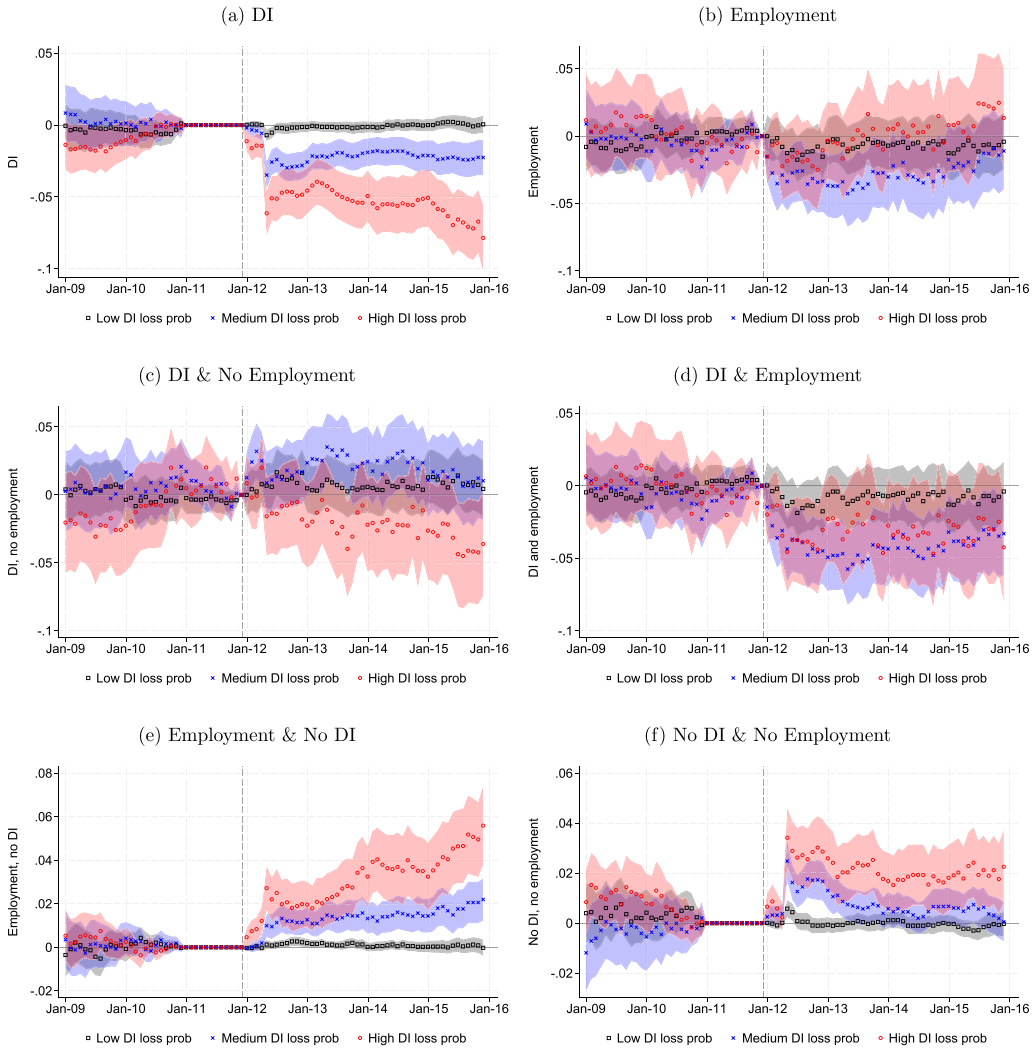


Fig. 4. Impact of Benefit Reassessment on DI Status and Employment by Predicted DI Loss Groups. *Note:* Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated beneficiaries born after the birthday cutoff relative to control beneficiaries born before the birthday cutoff. It displays the estimated β_T coefficients from equation (1) with 95% confidence intervals over 2009-2015, with December 2011 as the reference month. The sample is restricted to men with a health impairment below 80% as of December 2011 who received DI throughout 2011. The treatment group includes individuals born in 1955 (aged 56 in December 2011) and the control group includes individuals born in 1954 (aged 57 in December 2011). Both the treatment and control groups are split by predicted DI loss probability.

pre-reform characteristics and analyze employment responses by predicted risk. We focus on exits from DI rather than benefit reductions because DI exits constitute well-defined and homogeneous outcomes that do not depend on ad hoc cutoff values used to define benefit reductions. This focus also simplifies the interpretation of the DI status and employment results, as it avoids the additional complexity that would arise if exits and benefit reductions were analyzed jointly.

To predict benefit loss, we estimate a logit model on the sample of the treatment group, with a binary outcome for exiting DI anytime between January 2012 and December 2015. The model includes the following predictors: a binary indicator for prescription drug spending in 2011 above the sample median; binary indicators for any prescription drug spending in 2011 by first-level ATC (Anatomical Therapeutic Chemical) categories; a binary indicator for non-zero hospital days in 2011; a binary indicator for health impairment below 50%, based on the DI category in December 2011; occupation category in the last pre-reform job (skilled / unskilled / missing); a binary indicator for DI duration in December 2011 above the median of 10 years; and micro-region fixed effects (Hungary is divided into 174 micro regions, plus 23 districts of the capital city). We report the logit model coefficients and standard errors in Table A.3.

Based on the estimated logit model, we predict DI loss probabilities for both the treatment and control groups and classify individuals into three categories: (1) low predicted DI loss probability, including the lowest five deciles of predicted probabilities; (2) medium predicted DI loss probability, including deciles 6 to 8; and (3) high predicted DI loss probability, including the top two deciles. Table A.4 reports the mean, minimum, and maximum predicted probabilities within each category.

We depict the share of individuals receiving DI benefits in panel (b) of Fig. 1, separately for the control and treatment groups, with both groups divided into the three predicted DI loss categories. While the share receiving benefits clearly decreases with higher predicted DI loss, even in the high loss treatment group, nearly 90% of individuals still receive benefits at the end of 2015.

Fig. 4 shows the effects of the benefit reassessment on DI status and employment by predicted probability of DI loss. Panel (a) illustrates that the reassessment led to a sharp and persistent decline in DI receipt among beneficiaries with a high predicted probability of benefit loss,

reaching 7.8 percentage points after four years. In contrast, DI receipt among low-probability beneficiaries remains largely unchanged, while the medium-probability group experiences an intermediate decline of 2.3 percentage points over the same period.

Panel (b) reports employment responses. Consistent with a threat effect, employment falls most sharply for medium-probability beneficiaries following the announcement of the reassessment, declining by up to 4.3 percentage points (23.2 percent) before gradually recovering. By contrast, employment among low-probability beneficiaries remains relatively stable throughout the period, while employment among high-probability beneficiaries increases only slowly and with a delay, reaching 2.5 percentage points after four years. This divergence suggests that labor supply adjustments were largest for individuals whose benefit status was perceived as contingent on their employment behavior.

Panels (c) and (d) decompose these responses by joint benefit–employment states. Panel (d) shows that, among medium-probability beneficiaries, the likelihood of combining DI receipt with employment declines sharply by up to 5.7 percentage points, indicating a deliberate withdrawal from work while remaining on benefits. Correspondingly, panel (c) shows an increase of up to 3.5 percentage points in the probability of receiving DI without concurrent employment for this group. These shifts indicate labor supply reductions aimed at preserving benefit eligibility as the perceived likelihood of losing benefits due to the ability to work increases, concentrated among those whose ability to keep benefits was the most uncertain.

Panels (e) and (f) document outcomes including DI exit. Panel (e) shows that employment without DI rises most strongly among high-probability beneficiaries, increasing by 5.6 percentage points by the end of the fourth year after the reform, consistent with benefit loss followed by gradual labor market reentry. However, panel (f) reveals that a substantial share of this group initially transitions into a state with neither DI receipt nor employment, with the probability of being in this state increasing by up to 3.4 percentage points shortly after reassessment. This pattern suggests that benefit loss among high-probability beneficiaries does not translate immediately into employment and instead generates a period of non-employment without income support.

Taken together, the six panels highlight a clear non-monotonic employment response to reassessment risk. Beneficiaries facing an intermediate probability of benefit loss respond most strongly by reducing employment to preserve eligibility, while those facing near-certain benefit loss and those facing little risk do not reduce their labor supply. This suggests that the reassessment affected labor supply not only through realized benefit removal, but also through anticipatory responses driven by perceived eligibility risk, a type of substitution effect.

5.4. Estimating the substitution effect of perceived removal risk

The substitution effect in disability insurance arises because beneficiaries typically lose all or part of their benefits if they earn above a certain level. In other words, beneficiaries face an implicit tax on earnings (Autor and Duggan, 2007). By conditioning transfers on earnings, disability insurance distorts the relative price of leisure and labor, making work less attractive at the margin and generating deadweight loss (Autor and Duggan, 2007; Chetty, 2008). As a result, some individuals may choose not to work even if they would otherwise supply positive labor at the income level provided by the benefit. Distinguishing the substitution effect from the income effect, which reflects reduced labor supply due to higher non-wage income, is central in the literature on the labor market effects of disability and unemployment insurance. However, separating the two empirically is challenging (Autor and Duggan, 2007; Chetty, 2008; Deshpande, 2016b; Deuchert and Eugster, 2019). Identifying the magnitude of the substitution effect is particularly policy-relevant, as it reveals the extent to which disability insurance distorts work incentives and for which groups these distortions are most pronounced.

In the context of reassessments, the substitution effect can operate through either an increased probability of benefit loss when working or the *perceived* risk that labor market activity signals work capacity. In our setting, it appears that after the reform beneficiaries perceived a higher probability of benefit loss. This anticipated risk led some individuals to exit employment (if previously employed) or to refrain from entering employment (if previously non-employed).

For the three groups defined in Section 5.3, based on their predicted probability of DI loss, we estimate the average effect of the reassessment reform on employment during January–April 2012—that is, after the reform took effect but before reassessments started. Specifically, we estimate a modified version of Eq. (1) by restricting the sample to the period from January 2009 through April 2012 and replacing the monthly date indicators with a single binary variable equal to one for January–April 2012 and zero for the 2009–2011 period. The model is estimated separately for each predicted DI loss group.

Comparing the low- and medium-predicted loss groups indicates that a 4.6 percentage point increase in predicted DI loss probability (from 2.5 to 7.1 percent) corresponds to a 1.8 percentage point larger negative employment effect. This employment gap persists over 2012–2016. We focus on the short-term (four-month) results, rather than the longer-term (four-year) outcomes, when analyzing the substitution effect because after May 2012 the reassessments started and some beneficiaries experienced DI loss, which affected their employment propensities via the income effect.

Importantly, the estimated substitution effect is smaller when comparing the low- and high-risk groups. In this case, a 16.2 percentage point increase in predicted DI loss probability (from 2.5 to 18.7 percent) corresponds to only a 1.0 percentage point larger negative employment effect. A potential explanation is that individuals in the high-risk group may perceive that they would lose their benefits regardless of how much they work, and therefore do not react to the threat of benefit loss.

Overall, these results indicate that the substitution effect is particularly important among those who perceive that their DI status strongly depends on their employment. Within this group, a 4.6 percentage point increase in the predicted probability of DI loss reduces employment by 1.8 percentage points. Expressed as a semi-elasticity, this is approximately -0.39 percentage points of employment loss for each 1 percentage point increase in DI-loss risk. The response is non-monotonic across risk: the implied slope is much smaller when comparing low- to high-risk beneficiaries, consistent with the idea that substitution incentives are strongest for beneficiaries who view employment as a signal affecting eligibility, and weaker for those who anticipate near-certain loss regardless of their behavior.

Only a limited number of studies explicitly disentangle substitution and income effects in social insurance. Moreover, most of these focus on estimating income effects in settings where incentives to remain non-employed are largely absent (Autor and Duggan, 2007 and Gelber et al., 2017), and only a few quantify substitution effects. Chetty (2008) decomposes responses to unemployment insurance into liquidity and substitution components, finding sizable substitution elasticities (0.2–0.3) once income effects are netted out. In the disability context, Deuchert and Eugster (2019) exploit a Swiss reform to separately identify income and substitution effects and show that income effects account for most of the response, while substitution-driven employment changes – arising from incentive changes largely independent of benefit levels – are relatively small, ranging between 0 and -3.7 percentage points. Compared to these estimates, our results suggest that a moderate increase in perceived benefit-loss probability (4.6 percentage points) reduces employment by 1.8 percentage points even without realized income changes, indicating that substitution effects driven purely by eligibility risk can be quantitatively important.

6. Conclusion

This paper shows that disability benefit reassessments affect labor supply through two countervailing channels. The first channel—the threat effect—appeared before reassessments started, when no beneficiaries had yet been removed from DI. The pattern is consistent with beneficiaries reducing or stopping work to avoid appearing employable during reassessment. This response occurred in a setting where employment while receiving disability benefits was common and earnings limits were generous, suggesting that perceived reassessment risk rather than binding earnings constraints drove the initial decline in employment.

The second channel reflects the consequences of realized benefit loss. After reassessments began, employment increased gradually as some beneficiaries re-entered the labor market. This adjustment was slow, however, and many individuals who lost benefits took considerable time to find work. This led to a temporary increase in the share of individuals with neither DI receipt nor employment. Although this share declined over time as employment rose, it did not return to zero.

Reassessments therefore influence labor supply not only through realized benefit loss but also through anticipatory behavioral responses to perceived eligibility risk. The overall welfare implications depend on how policymakers weigh these opposing effects. In our setting, reassessments eventually reduced DI rolls and increased employment among some beneficiaries with remaining work capacity, but they also temporarily reduced employment among individuals who would otherwise have continued working while receiving benefits. The timing of these responses is important: the threat effect emerges quickly following the reform announcement, whereas employment gains from benefit loss accumulate gradually over several years.

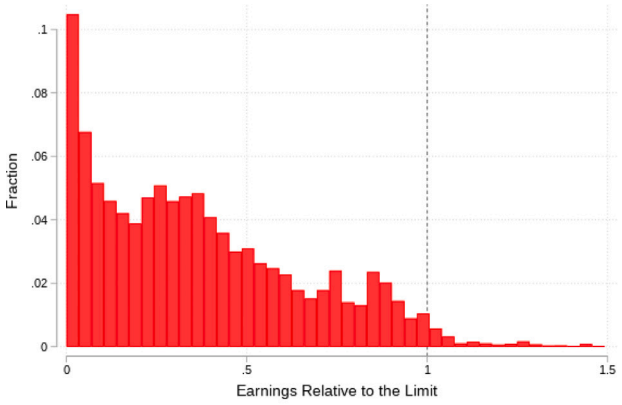
More broadly, our findings highlight that policies designed to screen disability insurance recipients can shape behavior well before eligibility decisions are made. The net employment effects of reassessments are therefore likely to depend on institutional features such as baseline work incentives while on DI, the strictness and salience of earnings limits, how reassessments are communicated and implemented, and the availability of reemployment support for beneficiaries who lose benefits.

Declaration of competing interest

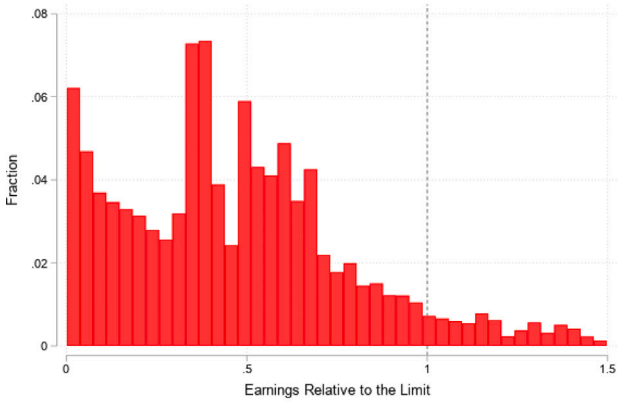
The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Additional figures and tables

(a) Pre-Reform Earnings Against Pre-Reform Limits



(b) Pre-Reform Earnings Against the Post-Reform Limits



(c) Post-Reform Earnings Against the Post-Reform Limits

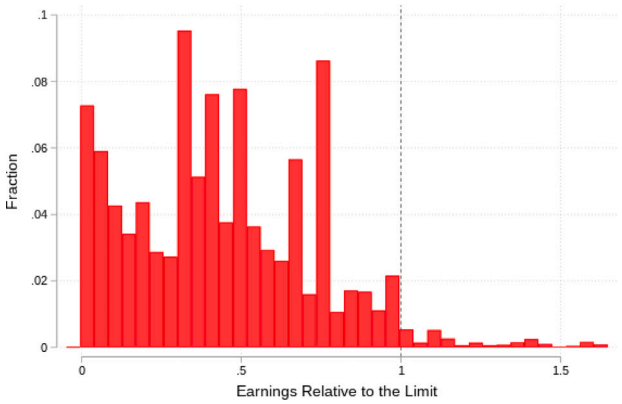
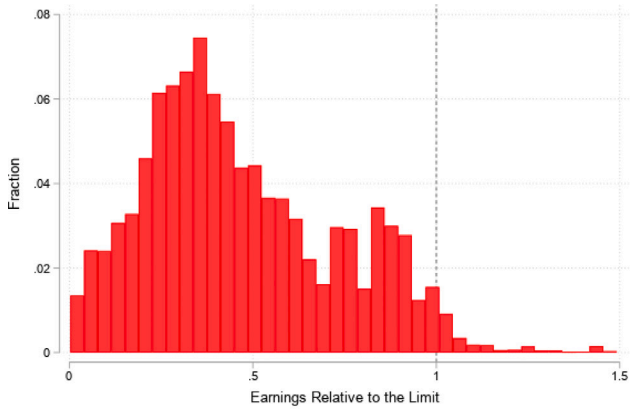
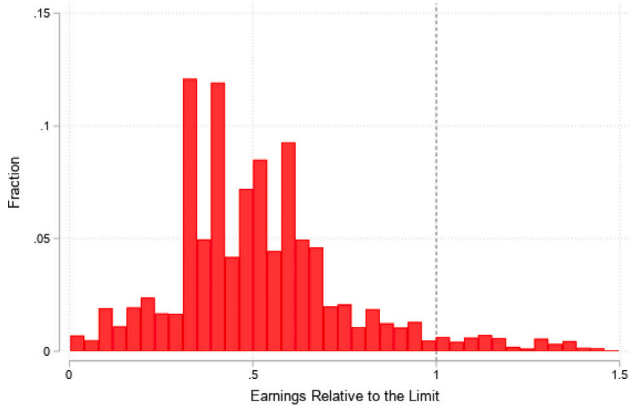


Fig. A.1. Earnings Distribution of DI Beneficiaries. *Notes:* Figure shows the distribution of earnings relative to the individual earnings limit for members of the treatment group (men born in 1955 and aged 56 in 2011) and control group (men born in 1954 and aged 57 in 2011). Panel (a) displays earnings as an average of the last 6 months for DI recipients with 50%–79% health impairment, and an average of the last 4 months for DI recipients with 40%–49% health impairment entering before 2008. To calculate the individually determined earnings limits, we estimate the valorized pre-disability earnings using earnings within the 12 months preceding benefit entry. Panel (b) shows the 2011 earnings distribution relative to a hypothetical earnings limit (set at 150% of the 2011 minimum wage) that would have applied had the post-reform earnings limit rule been in effect in 2011. Earnings observations correspond to average earnings over the preceding three months. Panel (c) shows the distribution of earnings relative to the post-reform earnings limit for those who already underwent revision and were classified as eligible for DI. Earnings observations represent average earnings for 2012–2013, and the minimum earnings value over the past three months for 2014–2015. These reference windows are selected according to the details of DI regulations.

(a) Pre-Reform Earnings Against Pre-Reform Limits



(b) Pre-Reform Earnings Against the Post-Reform Limits



(c) Post-Reform Earnings Against the Post-Reform Limits

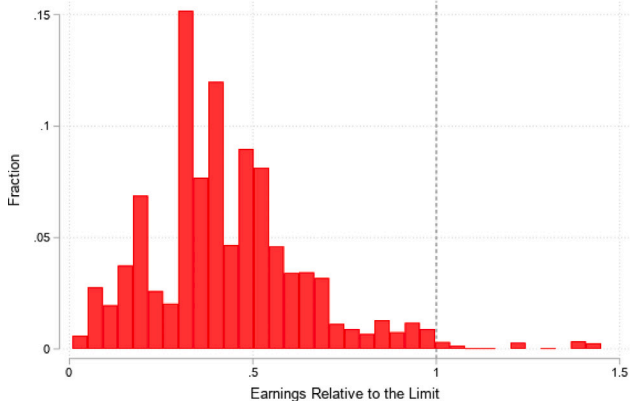


Fig. A.2. Earnings Distribution of DI Beneficiaries Who Work Part-Time. *Notes:* Figure shows the distribution of earnings relative to the individual earnings limit for members of the treatment group (men born in 1955 and aged 56 in 2011) and control group (men born in 1954 and aged 57 in 2011) who work part-time. Panel (a) displays earnings as an average of the last 6 months for DI recipients with 50%–79% health impairment, and an average of the last 4 months for DI recipients with 40%–49% health impairment entering before 2008. To calculate the individually determined earnings limits, we estimate the valorized pre-disability earnings using earnings within the 12 months preceding benefit entry. Panel (b) shows the 2011 earnings distribution relative to a hypothetical earnings limit (set at 150% of the 2011 minimum wage) that would have applied had the post-reform earnings limit rule been in effect in 2011. Earnings observations correspond to average earnings over the preceding three months. Panel (c) shows the distribution of earnings relative to the post-reform earnings limit for those who already underwent revision and were classified as eligible for DI. Earnings observations represent average earnings for 2012–2013, and the minimum earnings value over the past three months for 2014–2015. These reference windows are selected according to the details of DI regulations.

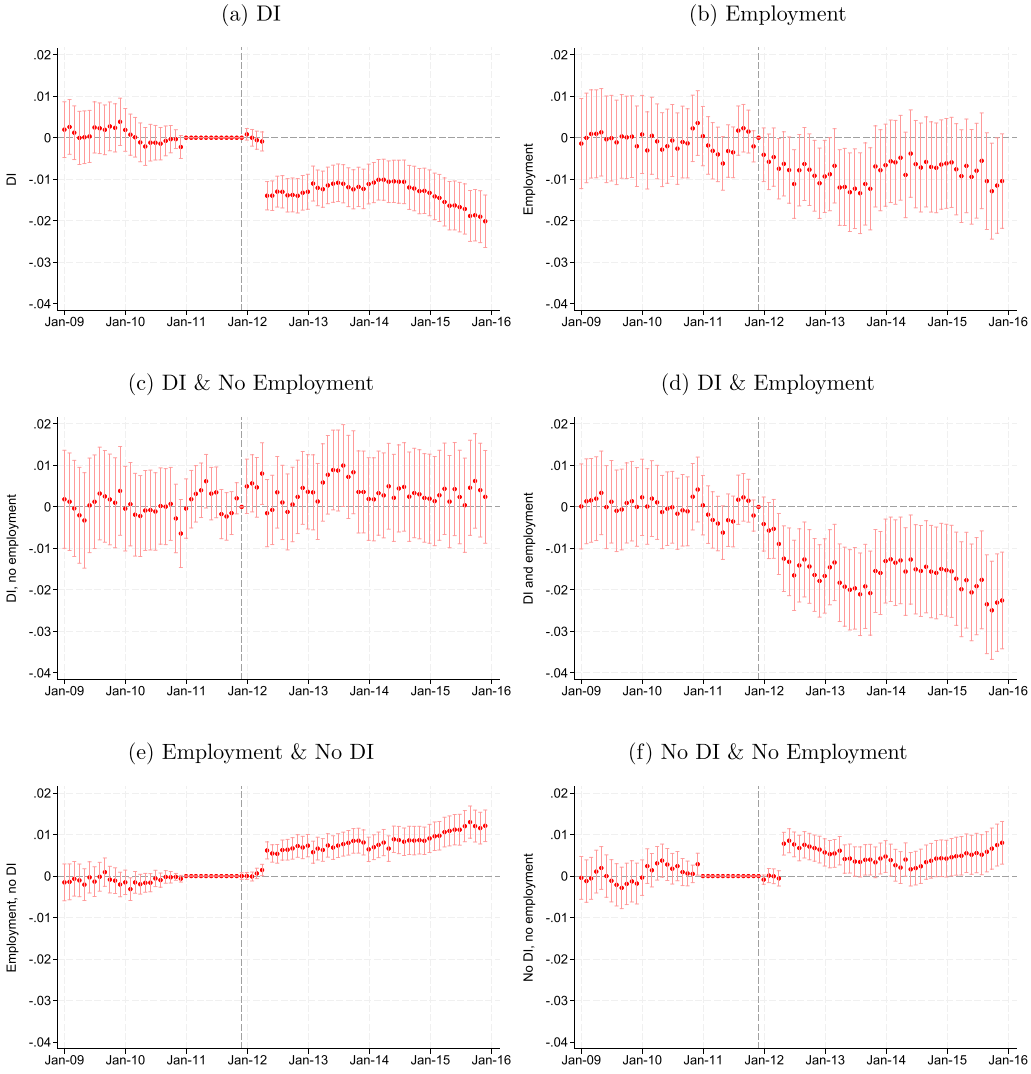


Fig. A.3. Effect of the Reform Over Time—Women. *Note:* Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated beneficiaries born after the birthday cutoff relative to control beneficiaries born before the birthday cutoff. It displays the estimated β_T coefficients from Eq. (1) with 95% confidence intervals over 2009-2015, with December 2011 as the reference month. The sample is restricted to women with a health impairment below 80% as of December 2011 who received DI throughout 2011. The treatment group includes individuals born in 1955 (aged 56 in December 2011) and the control group includes individuals born in 1954 (aged 57 in December 2011).

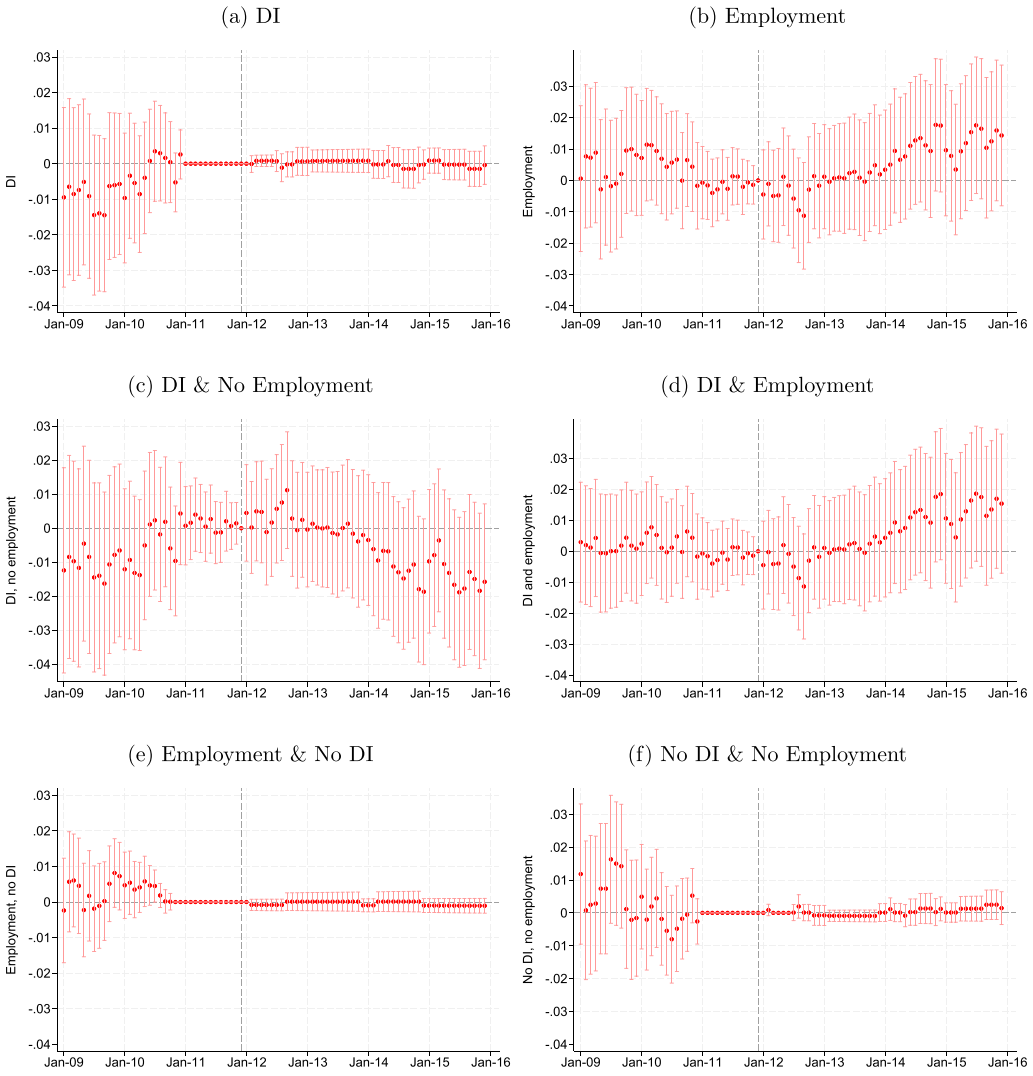


Fig. A.4. Placebo Analysis—Effect of the Reform Over Time for Beneficiaries with Health Impairments of at Least 80%. *Note:* Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated beneficiaries born after the birthday cutoff relative to control beneficiaries born before the birthday cutoff for the placebo group of individuals with health impairments of at least 80%. It displays the estimated β_T coefficients from Eq. (1) with 95% confidence intervals over 2009-2015, with December 2011 as the reference month. The sample is restricted to men with health impairments of at least 80% as of December 2011 who received DI throughout 2011. The treatment group includes individuals born in 1955 (aged 56 in December 2011) and the control group includes individuals born in 1954 (aged 57 in December 2011).

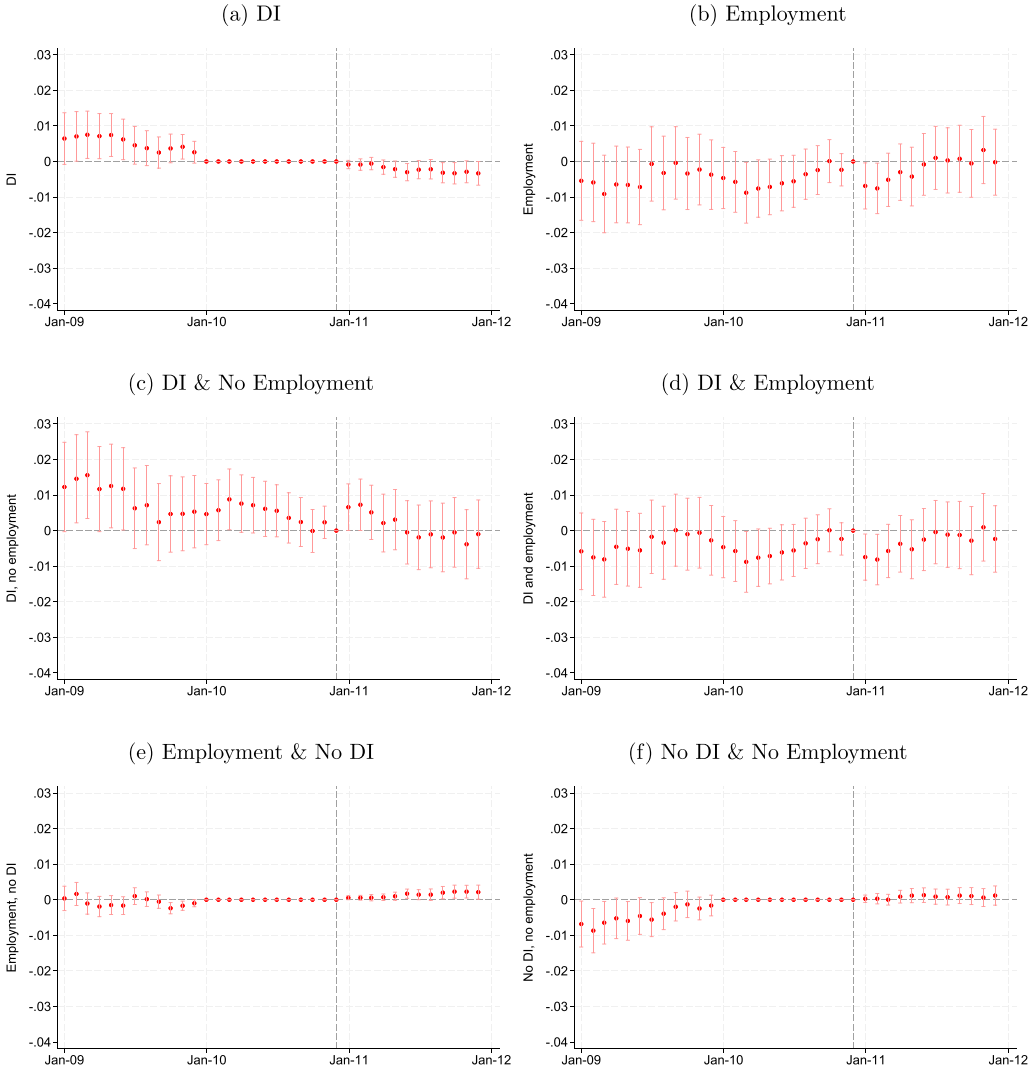


Fig. A.5. Placebo Analysis—Effect of a 2011 Placebo Reform Over Time. *Note:* Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated beneficiaries born after the birthday cutoff relative to control beneficiaries born before the birthday cutoff for a placebo reform in 2011. It displays the estimated β_T coefficients from Eq. (1) with 95% confidence intervals over 2009-2012, with December 2010 as the reference month. The sample is restricted to men with a health impairment below 80% as of December 2010 who received DI throughout 2010. The treatment group includes individuals born in 1955 (aged 56 in December 2011) and the control group includes individuals born in 1954 (aged 57 in December 2011).

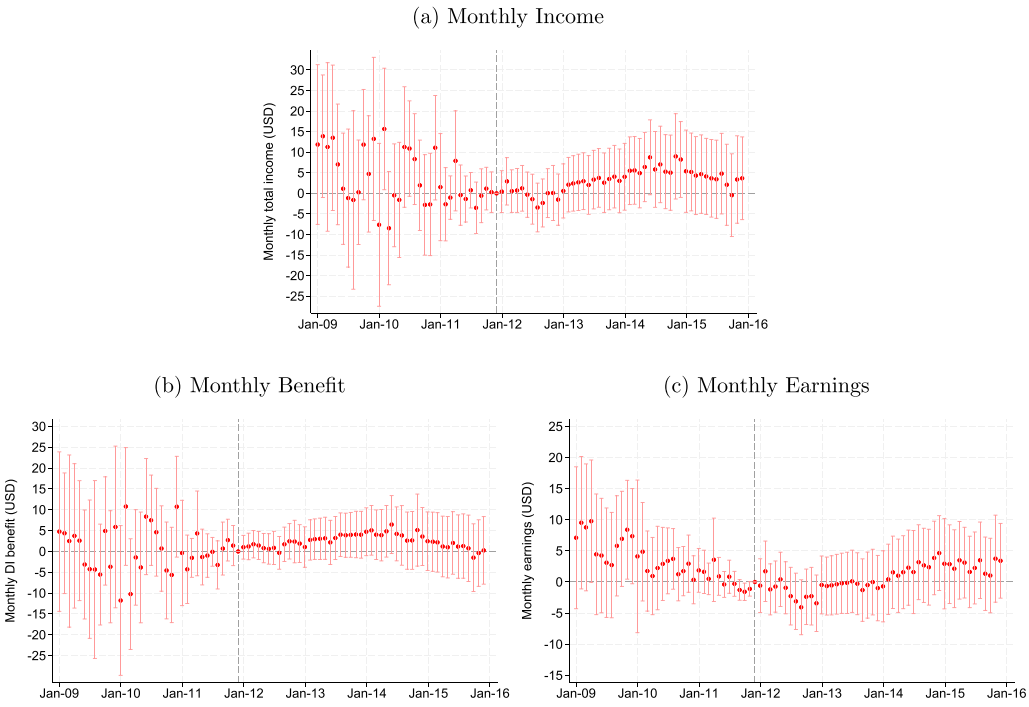


Fig. A.6. Placebo Analysis—Effect of the Reform Over Time on Income for Beneficiaries with Health Impairments of at Least 80%. *Note:* Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated beneficiaries born after the birthday cutoff relative to control beneficiaries born before the birthday cutoff for the placebo group of individuals with health impairments of at least 80%. It displays the estimated β_T coefficients from Eq. (1) with 95% confidence intervals over 2009-2015, with December 2011 as the reference month. The sample is restricted to men with health impairments of at least 80% as of December 2011 who received DI throughout 2011. The treatment group includes individuals born in 1955 (aged 56 in December 2011) and the control group includes individuals born in 1954 (aged 57 in December 2011). The income indicators are deflated to 2011.

Fig. A.7. Placebo Analysis—Effect of a 2011 Placebo Reform Over Time on Income. *Note:* Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated beneficiaries born after the birthday cutoff relative to control beneficiaries born before the birthday cutoff for a placebo reform in 2011. It displays the estimated β_T coefficients from Eq. (1) with 95% confidence intervals over 2009-2012, with December 2010 as the reference month. The sample is restricted to men with a health impairment below 80% as of December 2010 who received DI throughout 2010. The treatment group includes individuals born in 1955 (aged 56 in December 2011) and the control group includes individuals born in 1954 (aged 57 in December 2011). The income indicators are deflated to 2011.

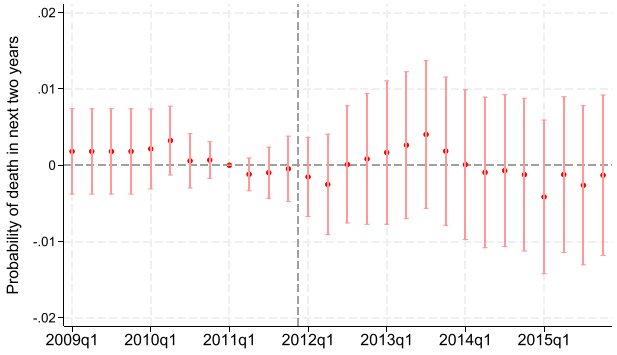


Fig. A.8. Effect of the Reform Over Time—Mortality. *Note:* Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated beneficiaries born after the birthday cutoff relative to control beneficiaries born before the birthday cutoff. It displays the estimated β_T coefficients from Eq. (1) with 95% confidence intervals over 2009–2015, with the first quarter of 2011 as the reference quarter. The sample is restricted to men with a health impairment below 80% as of December 2011 who received DI throughout 2011. The treatment group includes individuals born in 1955 (aged 56 in December 2011) and the control group includes individuals born in 1954 (aged 57 in December 2011). The average rate of two-year mortality in the control group is 0.041.

Table A.1
Health Revision obligation cutoffs and earnings restrictions.

(a) Revision cutoffs			
		Born in 1955 or after (below age 57 at end of 2011)	Born in 1954 or before (age 57 and above at end of 2011)
Health impairment ≥80%		No health revision	No health revision
Health impairment <80%		**Health revision**	No health revision
(b) Earnings restrictions			
	2009	2010–2011	From 2012
Health impairment 40–49%	80% of pre-disability wage	80% of pre-disability wage	150% of minimum wage
Health impairment 50–79%	90% of pre-disability wage	200% of DI benefit	150% of minimum wage

Note: Table shows the age and health cutoffs for revision and earnings restrictions. Panel (a) shows the revision cutoffs by health impairment and birth date. Panel (b) shows the earnings limits for different DI benefit categories. 25% of individuals in our analysis sample (i.e., DI beneficiaries aged 56–57 in December 2011) had a health impairment between 40% and 49% (and entered DI before 2008), while 75% had a health impairment between 50% and 79%.

Table A.2
Health conditions of disability benefit recipients (2011 Census).

Impairment or long-lasting disease	
Neither impairment nor long-lasting disease	10.13%
Both impairment and long-lasting disease	19.93%
Impairment	8.23%
Long-lasting disease	39.73%
No response	21.98%
Type of impairment	
Mobility impairment	16.19%
Autism	0.03%
Mental deficiency	0.88%
Mental injury (psychic injury)	2.91%
Speech handicap	0.25%
Speech deficiency	0.18%
Hard of seeing	2.04%
Blind	0.41%
Hard of hearing	0.83%
Deaf	0.29%
Deaf and blind	0.08%
Serious deficiency of internal organs	2.02%
Other disability	0.02%
Not relevant or no response	73.87%

Note: Authors' calculations based on the 2011 Census of Hungary. We restrict the data to individuals receiving disability benefits (N = 409,846).

Table A.3
Logit model of DI exit.

	DI exit	
	Coefficient	Standard error
Above median drug spending	−0.537***	0.121
Drug use: alimentary tract and metabolism	−0.185*	0.112
Drug use: blood and blood forming organs	−0.247*	0.130
Drug use: cardiovascular system	−0.011	0.082
Drug use: dermatologicals	0.029	0.190
Drug use: genito-urinary system	−0.092	0.311
Drug use: systemic hormonal preparations	0.027	0.257
Drug use: antiinfectives	0.127	0.108
Drug use: antineoplastic and immunomodulating agents	−0.239	0.542
Drug use: musculo-skeletal system	−0.007	0.116
Drug use: nervous system	0.056	0.114
Drug use: antiparasitic products, insecticides and repellents	−0.171	0.586
Drug use: respiratory system	0.196	0.169
Drug use: sensory organs	−0.259	0.230
Non-zero days in hospital	−0.226*	0.128
Below 50% health impairment	1.277***	0.120
Above 10 years of DI	−0.319**	0.127
Last occupation		
Unskilled	−0.147	0.150
Missing	−0.261*	0.139
Number of individuals	5413	

Note: Table shows logit regression coefficients of DI exit on the treatment sample. The model also includes micro-region effects which we do not report in the table. Standard errors are clustered at the individual level, *** p<0.01, ** p<0.05, * p<0.1.

Table A.4
Predicted probabilities of DI loss.

	Predicted DI loss probability					
	Control (born in 1954)			Treatment (born in 1955)		
DI loss probability	Mean	Min	Max	Mean	Min	Max
Low	0.026	0.003	0.046	0.025	0.003	0.046
Medium	0.070	0.046	0.108	0.071	0.046	0.108
High	0.186	0.108	0.537	0.187	0.108	0.545

Note: Table shows descriptive statistics of the predicted probabilities of exiting DI anytime between January 2012 and December 2015.

Data availability

The data that has been used is confidential.

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