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ABSTRACT

THE LABOR MARKET EFFECTS OF DISABILITY BENEFIT LOSS*

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Disability benefits are costly and tend to reduce labor supply. While spending can be contained by careful targeting, correcting past flaws in eligibility rules or assessment procedures may entail welfare costs. We study a major reform in Hungary that reassessed the health and working capacity of a large share of beneficiaries. Leveraging age and health cutoffs in the reassessment, we estimate employment responses to loss or reduction of benefits. We find that among those who left disability insurance due to the reform 58% were employed in the primary labor market, 6% participated in public works and 36% were out of work without benefits in the post-reform period. The consequences of leaving disability insurance sharply differed by pre-reform employment status. 81% of beneficiaries who had some employment in the pre-reform year worked, while only 33% of those without pre-reform employment did. The gains of the reform in activating beneficiaries were small and strongly driven by pre-reform employment status. This points to the importance of combining financial incentives with broader labor market programs that increase employability.

Keywords: disability insurance; benefit reduction; employment

JEL classification: H55; J14

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1 Introduction

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

Whether low levels of reintegration result from limited working capacity, other barriers to employment or poorly designed financial incentives is an important question for policy design. To the extent that limited working capacity is the reason behind low levels of reintegration, financial incentives or periodic reassessments are unlikely to have much success in reintegrating beneficiaries into the labor market. Moreover, if they include the removal of benefits, they can harm beneficiary welfare. If, on the other hand, low levels of reintegration are caused by poorly designed incentives, such as earnings limits set too low (Krekó, Prinz and Weber, 2023), governments can improve the efficiency and fiscal sustainability of DI programs by setting incentives appropriately. But even then, supply-side financial incentives might not be effective without rehabilitation and personalized support services to mitigate other potential barriers to work, such as human capital depreciation (Edin and Gustavsson, 2008), stigma (Eriksson and Rooth, 2014; Fernández-Blanco and Preugschat, 2018) or psychological distress (Diette, Goldsmith, Hamilton and Darity Jr., 2012) caused by long term unemployment.

In this paper, we study a unique large-scale reassessment reform to investigate the extent to which beneficiaries can be reintegrated when their benefits are removed or reduced. Starting in 2012, Hungarian DI beneficiaries under 57 years of age with health damage below 80% had to undergo a reassessment in order to remain eligible for benefits. As a result, about 18,000 beneficiaries (9% of the reassessed beneficiaries and 5% of all beneficiaries) lost their benefits while about 12,000 beneficiaries (6% of the reassessed beneficiaries and 4% of all beneficiaries) had their benefits reduced. We study the labor market consequences of benefit loss or reduction by leveraging age and health cutoffs in reassessments and focusing on a narrow age cohort around the cut-off age.

Comparing beneficiaries just below and just above the age cut-off, we find that among affected beneficiaries the probability of disability insurance receipt decreased by 1.5 percentage points due to the reform. About two-thirds of those who left DI were employed in the primary labor market or participated in public works in the post-reform period: without the concurrent receipt of DI benefits, employment increased by 0.9, and public works partici-

pation increased by 0.1 percentage point. Roughly one-third of excluded beneficiaries were not employed: the probability of having no income from DI or employment increased by 0.5 percentage point.

Unlike in the United States but similarly to other European countries, a meaningful share of DI beneficiaries are employed while receiving benefits and post-reform labor market outcomes differ greatly according to pre-reform employment status. Individuals who were working in 2011 (and were healthier) were more likely to lose their benefit as a result of the review. While only a quarter of the reassessed beneficiaries were employed in 2011, half of those who lost their benefits came from this group. 81% of them were still employed post-reform, 6% participated in public works, while 14% had no job or benefits. The other half of recipients who lost their benefits (with no work recorded in 2011) fared worse in the labor market: only 33% were employed post-reform, 6% participated in public works, while 61% had no job or benefits. By comparing pre- and post-reform job quality indicators among those who were employed, we also document the deterioration of job quality of former beneficiaries. Moreover, the deterioration is more striking among those who had no employment in 2011.

Our results suggest that the consequences of DI benefit removal depend crucially on whether a beneficiary is employed while receiving benefits. Those who held jobs while on benefits have a good chance of being employed after losing their benefits, while those who did not work are likely to remain out of work and without benefits. This suggests that improving the labor market attachment of beneficiaries is an important direction for policy. Moreover, our results point to the importance of combining financial incentives with broader labor market programs that increase employability after benefit loss.

Our work contributes to three strands of the literature. We most directly contribute to the literature that has examined the labor market consequences of benefit reduction or removal. Borghans, Gielen and Luttmer (2014), García-Gómez and Gielen (2018) and Garcia-Mandicó, García-Gómez, Gielen and O'Donnell (2020) study two large-scale DI re-assessment initiatives in the Netherlands. García-Gómez and Gielen (2018) and Garcia-Mandicó, García-Gómez, Gielen and O'Donnell (2020) show that following both reforms, recipients who experienced benefit reduction or removal increased their labor supply substantially, replacing almost two-thirds of lost benefits with earnings in the labor market. As an adverse effect of benefit loss, Borghans, Gielen and Luttmer (2014) find an increase in mortality among low-income women whose benefits were reduced following the 1993 reform. We contribute to this literature by pointing out the importance of employment during DI for post-DI labor market outcomes. Our rich database also allows us to analyze the impact of benefit loss on employment prospects and job quality. With this part of the analysis we contribute to the literature showing that long periods of inactivity may lead to a growing

distance from the labor market, depreciation in human capital, lower probability of work, and lower wages (Vingård, Alexanderson and Norlund, 2004; Edin and Gustavsson, 2008; Bryngelson, 2009).

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, Mullen and Strand, 2013; French and Song, 2014; Mullen and Staubli, 2016; Gelber, Moore and Strand, 2017). Using various quasi-experimental approaches, these papers find that disability insurance receipt reduces labor supply substantially. Our main contribution to this literature is the examination of the consequences of benefit reduction among individuals who were already receiving benefits for some time.

Finally, our work also speaks to the academic and policy literature that has focused on the fiscal sustainability of DI programs (e.g., Autor and Duggan, 2006, 2007; Autor, 2011; Liebman, 2015). We show that, although reassessments may be a way to reduce DI rolls, policy makers need to be aware of the potential negative impact on the welfare of beneficiaries whose benefits are removed or reduced but who are unable to find employment.

The remainder of the paper is structured as follows. Section 2 describes the institutional background and the details of the 2012 reform. Section 3 describes our data. Section 4 explains our empirical approach. Section 5 presents our results. Section 6 concludes.

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 economic transition from socialism to market economy rapidly increased unemployment in the early 1990s and policy makers allowed (or encouraged) the expansion of benefit programs such as DI and early retirement in order to ease social and political tensions (Vanhuysse, 2004). As a result, the number of DI beneficiaries doubled between 1990 and 2003 and reached over 700,000 or 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 instead of focusing solely on health impairment in the assessment of new benefit claims (Scharle, 2008). The 2008 reform consisted of three key elements. First, a new assessment system was introduced which put more emphasis on remaining working capacity and the potential for rehabilitation and skill de-

velopment. The second element was the introduction of a temporary rehabilitation benefit, which was granted for up to three years and thus helped to reduce the take-up of permanent disability benefits. New claimants with a health damage of at least 50% and assessed as rehabilitable were eligible for this benefit. Third, recipients of the temporary benefit were obliged to cooperate with the public employment service and participate in employment rehabilitation programs, which were expanded in terms of range and capacity (Adamecz-Völgyi, Lévay, Bördős and Scharle, 2018). 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). The aim was to curb inflow and to reactivate beneficiaries with some remaining working capacity in order to improve the sustainability of the DI system, which was considered overly generous even after the 2008 reform, and was believed to contribute to the low activity rate in Hungary. 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. While the 2012 reform was successful in reducing the costs of the DI system, its harshness generated debates about whether it would reactivate long-time beneficiaries or simply leave them without income.

2.2 Details of the 2012 Reform

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 all DI recipients below age 57 with a partial disability, whose health impairment was below 80%, as determined by the pre-reform assessment system (Table 1). Two disability benefit programs were affected: the Category III Disability Pension for those with a health damage of 50% to 79% and the Regular Social Assistance for those with a health damage above 40%. The reform did not apply to recipients of Category I and Category II Disability Pensions (who had at least 80% health damage).[1]

Beneficiaries affected by the reform had to declare by March 2012 whether they wished to undergo the health reassessment. If they failed to do so, they lost their benefit entitlement by May 2012. Otherwise, their health status and degree of employability were reevaluated

[1]Exemption was granted also to recipients of the Transitory Allowance, a benefit targeted at moderately disabled individuals within 5 years of the retirement age, but only 0,2% of them, 15 individuals, were younger than 57 in December 2011.

according to the post-reform rules in a complex assessment process carried out by a team of physicians and rehabilitation experts. Individuals whose health impairment was classified higher than 40% during this review retained eligibility to benefits. Mainly due to capacity constraints, it took several years to undertake all the reviews, so the process was completed only in 2016.

About 18,000 beneficiaries (9% of the reassessed beneficiaries and 5% of all beneficiaries) who underwent the review lost benefit eligibility. The total number of recipients decreased much more, from 473,000 in January 2012 to 355,000 in January 2017 (Hungarian Central Statistical Office, 2022), due to a large drop in inflows, that started from the early 2000s and gained new momentum after 2012. This drop in the number of beneficiaries after the reform suggests that while in principle the eligibility conditions (expressed as percent of health damage) did not change, the assessment process became more stringent. On top of the large drop in the number of beneficiaries, the benefits of 12,000 beneficiaries decreased in inflation-adjusted terms.[2]

The pre-reform disability benefit categories were consolidated into two benefit programs called Disability Allowance and Rehabilitation Allowance. Beneficiaries not recommended for vocational rehabilitation became eligible for the Disability Allowance while those who were deemed able to return to the labor market following rehabilitation became eligible for the Rehabilitation Allowance, which was paid for up to 3 years and set at a much lower rate than the Disability Allowance.[3] Rehabilitation Allowance recipients were required to cooperate with the rehabilitation authority and fulfill obligations set out in the employment rehabilitation plan. At the same time, recipients over 62 years of age were reclassified as old-age pensioners.

Although the comprehensive reevaluation of a large subgroup of DI recipients is uncommon, it is not without precedent: the majority of DI recipients under age 44 were reassessed under more stringent rules in the Netherlands in 2004. Garcia-Mandicó, García-Gómez, Gielen and O'Donnell (2020) estimate that the reassessment removed 17 percent of beneficiaries from the program and reduced benefit income by 20 percent, on average. However, in contrast to the Netherlands, where support for labor market reintegration was substantially expanded between 1997 and 2002, beneficiaries in Hungary who lost part or all of their benefit received little support in returning to the labor market. The capacity of rehabilitation services at the time was very limited and intensive, personalized services were only provided

[2]Other factors also contributed to the drop in DI claims: the cohorts in their 50s where shrinking in size during this period, their level of education was increasing, and the economy was recovering.

[3]Those subject to the health review were also placed on this temporary allowance until the date of their review but received their original income until the review.

by a handful of small NGOs, operating mainly in urban centers (Krekó and Scharle, 2020).[4]

3 Data

The 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, who are then followed up until 2017. The database consists of linked data sets at the monthly frequency of the pension, tax and health care authorities and contains detailed individual-level information on employment and earnings history, use of the health care system, pension and other social benefits, and firm-level indicators. Importantly, it also contains information on the type and amount of different disability benefits and old-age pensions received. Two important limitations of the data are that the employment status of DI recipients cannot be observed until April 2007 and we do not observe the health condition based on which the disability benefit is received. Based on the 2011 census (Appendix Table A1), the majority of DI recipients suffer from long-lasting diseases. Among those recipients who have an impairment, mobility impairment is the most prevalent form of disability.

When estimating the effects of the reform, we analyze the following monthly indicators of labor market and DI status. DI status is a binary variable that takes value one if the individual is DI recipient in a given month and zero otherwise.[5] The binary variable for employment status equals one if the individual is employed on the 15^{th} of the given month and zero otherwise. Employment includes self-employment but excludes public work. Importantly, employment was always allowed while receiving DI benefits, with restrictions on the maximum possible earnings.[6] We analyze public work as a separate outcome.[7] Based

[4]In contrast, the Netherlands provided access to a wide range of active labor market programs (Drøpping, Hvinden and van Oorschot, 2000) and introduced a temporary program to cushion the short-term impact of the reform on those whose benefit was reduced or terminated (and who were not eligible for unemployment benefit) by maintaining their income at its pre-reform level for a period of six months, which was later increased to twelve months (Garcia-Mandicó, García-Gómez, Gielen and O'Donnell, 2020).

[5]Since short gaps in DI eligibility may occur for administrative reasons, we have decided to 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 the monthly basis to at least half of the regular DI benefit payment before the gap.

[6]Until 2009, the earnings limit for the disability pension was determined on the basis of the valorized previous earnings using a complex calculation. From 2010, the earnings limit was changed to double of the amount of the disability pension. The recipients of the regular social assistance were allowed to accumulate earnings up to 80% of their previous earnings until 2007 and 80% of the minimum wage from 2008 (Krekó, Prinz and Weber, 2023). After the 2012 reform, the earnings limit was linked to the minimum wage for new entrants, but remained unchanged for those who acquired their benefit before the reform.

[7]The public works scheme was the dominant active labor market policy measure at the time of the DI reform in Hungary, aimed at direct job creation for the unemployed working-age population. The program, which was launched in 1996, was significantly expanded from 2011. The public works scheme had two

on these indicators, we generate five mutually exclusive and exhaustive binary outcome variables: (1) DI & no employment; (2) DI & employment; (3) employment & no DI; (4) public work & no DI; (5) no DI & no employment & no public work.

In addition, we look at four quarterly indicators of healthcare use: GP visits, outpatient specialist care visits, hospital days, and total spending (social security plus out-of-pocket spending) on prescription drugs. Indicators of healthcare use are included in our data from 2009.

We extend the analysis with job quality indicators derived from the administrative panel database. We generate a binary indicator of earning above the minimum wage, after adjusting the monthly wage to hours worked. We define a binary indicator of full-time job, which equals one if the weekly hours of work are at least 40. We generate a binary indicator of working in a skilled job, which includes all occupations except for elementary occupations, with elementary occupations corresponding to International Standard Classification of Occupations (ISCO) code 9. Finally, using the entire sample in the administrative database, we calculate the year-specific median of the total factor productivity (TFP) of firms, weighted by firm size.[8] Based on this indicator, we define a binary indicator of above-median employer TFP.

4 Empirical Framework

4.1 Control and Treatment Groups

In our empirical analysis, we estimate the impact of the 2012 reform on DI recipients subject to the compulsory health reassessment—partially disabled individuals with health impairment below 80%, who were under age 57 at the end of 2011. A limitation of our data is that we do not have information on the reassessment procedure itself; we observe exits from the DI system, but not the reason for leaving the system. Consequently, it is not possible to isolate those who lost their benefit as a results of the revision from those who would have exited DI even in the absence of the reform. For this reason, to identify the impact of the reform, we focus on a narrow age group around the age cut-off of the policy, assuming that

stated functions: to reintegrate participants into the primary labor market and to exclude people not willing to participate in public works from receiving benefits and social assistance (Molnár, Bazsalya, Bódis and Kálmán, 2019). However, the vast majority of Hungarian public workers – especially the unskilled and those in depressed areas – worked in separated public works units (Köllő, 2015) and received very low pay. Between 2011 and 2015, both the net and gross basic public work wage ranged between 70-80% of the statutory minimum wage.

[8]We calculate the value added-based TFP. When doing so, we apply the estimation procedure of Wooldridge (2009) and use the *prodest* Stata package of Rovigatti and Mollisi (2020).

outcomes of individuals in this narrow age group below and above the cut-off age would have evolved similarly in the absence of the reform.

Our sample contains DI recipients belonging to the affected benefit categories who were aged 56 or 57 in December 2011. Those who were 56 (just below the cut-off) in December 2011 make up the treatment group, while those who were 57 (just above the cut-off) make up the control group. We restrict the sample to individuals claiming DI throughout 2011 who were alive in January 2012. We restrict the control age group to age 57 at the end of 2011 to exclude individuals close to the old-age retirement age in order to improve comparability across the control and treatment age groups.[9] We focus on men below 62, the statutory retirement age for the oldest cohorts, allowing us to use data up to 2015. 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. Finally, those who died during the observed time period are included in the sample until the last year they were alive.

Summary statistics for the control and treatment groups are displayed in Table 2. The two groups are quite similar to each other on most dimensions. They have approximately the same employment rate (24.3% vs 24.9%) while receiving benefits in 2011 and each group has been receiving benefits for 11 years on average. Despite being a year younger, the 56-year-old treatment group may be slightly less healthy with average prescription drug spending of 533 euros vs 512 euros among the 57-year-old control group. Importantly for labor market outcomes, the two groups live in geographic areas with similar economic environments as evidenced by the average unemployment rate of their micro-regions of residence, 20.0% for the treatment group and 19.4% for the control group. They also work in occupations with similar skill levels: 33.1% of the treatment group and 35.3% of the control group work in skilled occupations, while 18.3% and 17.7%, respectively work in unskilled ones.

Figure 1 plots the evolution of the share of individuals receiving DI benefit in our sample separately for the treatment and the control groups. The sample is restricted to individuals who receive benefits throughout 2011, but we don't impose any restrictions on DI status before or after 2011. The figure suggests that in 2009 and 2010, the DI status of the control and treatment groups evolved very similarly, which suggests that the two groups are likely to be comparable and that absent the reform their status would have involved 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 is removed from benefits. The bulk

[9]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.

of the divergence occurs in May 2012, which suggests that although the review process lasted until 2016, most beneficiaries were affected early on.[10]

4.2 Difference-in-Differences

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

$$Y_{it} = \beta^{DiD} \mathbb{1}[Year_t \geq 2012] \mathbb{1}[AGE_i = 56] + \gamma_a \mathbb{1}[AGE_i = 56] + \mu_t + \varepsilon_{it}, \tag{1}$$

where i indexes individuals, t indexes months, $\mathbb{1}[Year_t \geq 2012]$ is an indicator for the post reform period, $\mathbb{1}[AGE_i = 56]$ is an indicator for the treatment group, and the μ_t are month fixed effects. Our coefficient of interest is β^{DiD}, the difference-in-differences estimator, which captures the differential change in labor market outcomes for treated relative to control individuals.

To explore the evolution of the reform's impact over time, we also estimate month-specific treatment effects β_t from the following equation:

$$Y_{it} = \sum_{\substack{t=Jan2009 \\ t \neq Dec2011}}^{Dec2015} \beta_t \mathbb{1}[Date_t = t] \mathbb{1}[AGE_i = 56] + \gamma_a \mathbb{1}[AGE_i = 56] + \mu_t + \varepsilon_{it}. \tag{2}$$

where i indexes individuals, t indexes months, $\mathbb{1}[Date_t = t]$ is an indicator for month t, $\mathbb{1}[AGE_i = 56]$ is an indicator for the treatment group, and the μ_t are month fixed effects. Our parameters of interest are β_t, which capture the differential change in labor market outcomes for treated relative to control individuals in each month relative to December 2011.

In our reduced-form analyses, we focus on five mutually exclusive and exhaustive binary outcome variables Y_{it}: (1) DI & no employment; (2) DI & employment; (3) employment & no DI; (4) public work & no DI; (5) no DI & no employment & no public work.

In order for our estimates to represent the causal impact of being subject to the reassessment on the labor market outcomes of the treatment group, the control group must represent a valid counterfactual for the evolution of the treatment group's labor market outcomes. In particular, we assume that absent the reassessment, the two groups' labor market outcomes would have involved similarly. We present several pieces of evidence consistent with this

[10] For comparison, younger beneficiaries subject to reassessment (aged 30-55 in December 2011) were more likely to exit DI during the same period than our treatment or control groups. Among them, benefit entitlement decreased by around 10% by the end of 2015. We focus on the age groups around the cutoff to improve comparability.

assumption. First, Table 2 shows that the control and treatment groups are similar on a number of different measures of health and employment. Second, Figure 1 suggests that prior to the reassessment the disability status of the control and treatment groups evolved very similarly, suggesting that absent the reassessment they would have moved together as well. Third, the month-specific estimates of the difference in labor market outcomes between the control and treatment group presented in Figure 2 also show that all outcomes move together in the two groups prior to the reform, which also suggests that the outcomes of the control group post-reform are a good counterfactual for the outcomes of the treatment group. Fourth, we present results using a placebo approach, comparing the labor market outcomes of disabled individuals who fall into the same age groups but were unaffected by the reform as they had health impairments over 80%. There is no evidence of differential changes by age in this unaffected group which suggests that our main results indeed identify the impact of the reassessment for 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.

4.3 Instrumental Variables Approach

To quantify the labor market impact of benefit loss, we use being subject to the reassessment as an instrument for benefit loss. The first-stage equation is

$$exit_{it} = \gamma \mathbb{1}[AGE_i = 56] + \mu_t + \varepsilon_{it} \tag{3}$$

where $exit_{it}$ is a binary indicator for not receiving DI benefits (equals one minus the DI status variable), $\mathbb{1}[AGE_i = 56]$ is an indicator for the treatment group, and the μ_t are month fixed effects. Using the first stage to estimate predicted loss of benefits, we estimate the second-stage equation:

$$Y_{it} = \beta^{IV} \widehat{exit_{it}} + \mu_t + \nu_{it} \tag{4}$$

where $\widehat{exit_{it}}$ denotes predicted benefit loss and the μ_t are month fixed effects. Our coefficient of interest is β^{IV}, which captures the impact of benefit loss on labor market outcomes after the reassessment among individuals who lost their benefits due to the reform.

We estimate the impact of benefit loss on three of the previously defined five outcome variables: (1) employment & no DI; (2) public work & no DI; (3) no DI & no employment & no public work.

In addition to the identifying assumptions described above, the two standard IV assumptions of relevance and exogeneity need to be satisfied for our estimate to represent the causal

impact of benefit loss on labor market outcomes. Figure 1 and the first two columns of Table 3 show the relevance of the instrument. Table 3 suggests that over the four years after the reform, beneficiaries under the age cut-off had an approximately 1.5 percentage point higher probability of losing their benefits. The exogeneity assumption requires that being subject to reassessment affects labor market outcomes only through the DI exit channel. This assumption cannot be directly tested. Our placebo results provide suggestive evidence that being under the same age cut-off did not affect labor market outcomes among disability recipients not subject to reassessment and in a placebo reform year. However, if for example unobservable health status varies significantly with being 56 or 57 years old at the end of 2011 (i.e., being subject to reassessment), and health status affects labor market outcomes conditional on DI loss then our estimates could be biased. Table 2 indicates that the treatment and the control groups are similar in terms of major observable characteristics, including pre-reform drug spending. Our assumption is that the two analyzed cohorts are similar in all aspects, apart from being subject to reassessment.

5 Results

5.1 Main Results

In this section, we report our difference-in-differences estimates of the overall impact of the reassessment and our instrumental variables estimates of the impact of benefit removal on labor market outcomes. Figure 2 shows the month-by-month difference between control and treatment individuals for each of the labor market outcomes from estimating equation (2). It suggests that there were no significant differences in the evolution of labor market outcomes before the 2012 reform. The outcomes of treated individuals start to diverge in 2012, with the biggest change occurring in May, in line with the reform timeline which required benefit recipients to declare by March their intention to undergo reassessment or lose benefits from May. Panel A of Table 3 reports the effect of the reform on labor market outcomes averaged over the post-reform period from estimating equation (1). The sum of the five point estimates is zero, reflecting the mutually exclusive and exhaustive nature of the five outcome variables. Similarly, Panel A of Table 4 reports the instrumental variables estimates of the effect of DI exit on labor market outcomes pooled over the post-reform period from estimating equation (4). The sum of the three point estimates is one due to the mutually exclusive and exhaustive nature of the outcome variables. Year-by-year instrumental variables estimates are shown in Panel (a) of Figure 3.

Panels (a) and (b) of Figure 2 show the change in DI status, breaking the overall effect

displayed in Figure 1 down into two categories by concurrent employment status. Panel (a) and column (1) of panel A of Table 3 show that there is little change in the number of individuals who receive DI benefits while not working. Panel (b) of Figure 2 shows that by May 2012 affected beneficiaries were about 2 percentage points less likely to be receiving benefits and working at the same time and the gap increased further to 2.5 percentage points by the end of 2015. This suggests that most benefit removals happened early on with additional exits happening gradually over the subsequent years as reassessment progressed. Pooling over the post-period, column (2) of panel A of Table 3 shows that there was a 1.8 percentage point decline in the probability of receiving benefits and working at the same time.

Panel (c) of Figure 2 suggests a concurrent jump in the number of former beneficiaries who work without receiving benefits, followed by a slow increase over the next four years. Column (3) of panel A of Table 3 shows that pooling over the post-reform years there was a 0.9 percentage point increase in employment without benefits. This suggests that approximately 60% of those removed from benefits end up working in the open labor market. The year-by-year instrumental variables estimates displayed in Panel (a) of Figure 3 suggest that among individuals who exit the DI program due to the reassessment, the share of those employed without receiving benefits increased from 40% in 2012 to over 70% in 2015. Consistent with the difference-in-differences estimates, over the post-reform years on average 58% of those who exit due to the reassessment are employed in the open labor market without receiving benefits as displayed in column (1) of panel A of Table 4.

Panels (d) and (e) of Figure 2 show the outcomes of recipients who lost their benefits but were not employed in the open labor market. Panel (d) suggests that some of those who lost benefits end up in the public works program. Over the 2012-2015 period, the average increase in public works employment is 0.1 percentage points (Column 4 of Panel A of Table 3). Panel (a) of Figure 3 shows that the impact of benefit loss on employment in public works is especially pronounced in 2013 and 2014. Averaging over the post-reform years, Column (2) of panel A of Table 4 shows that according to our instrumental variables estimates 6% of individuals who lose benefits due to the reassessment end up in the public works program during the years after the reform.

Panel (e) of Figure 2 shows an initial jump, followed by a gradual decline in the number of beneficiaries who are not employed or receiving any benefits. These results suggest that after the initial loss of benefits, some beneficiaries were able to quickly find employment (or remain employed if they were already working), while a significant share initially remained without a job but were able to find employment later on. Column (5) of Panel A of Table 3 shows that the overall increase in the probability of having no income from DI, employment, or

public works increased by 0.5 percentage points or one-third of those removed from benefits. Year-by-year instrumental variables estimates show a decline in the impact of benefit loss on the share of those without income for employment, public works or benefit from 60% in 2012 to about 20% in 2015, with a post-reassessment average of 35.7% as displayed in column (3) of panel A of Table 4.

Overall, our results suggest that relative to unaffected DI recipients just above the age cut-off for reassessment affected beneficiaries just below the cut-off lost their benefits at substantially higher rates. Outcomes varied significantly among individuals losing benefits: about 60% were employed in the primary labor market following benefit loss, while a third were left without a job or any benefits, the public works program only accommodating a small share. These results suggest the potential presence of important heterogeneity across types of beneficiaries which we turn to in Section 5.2.

Placebo analysis. In order to further probe the validity of our main results, Figures 4 and 5 present two sets of placebo results. Figure 4 replicates our main results presented in Figure 2 for DI categories that were not affected by the reassessment policy. Figure 5 replicates the same results but for a placebo reform in 2011.

Figure 4 shows DI coverage and placebo regression results for individuals who belonged to more severe and hence unaffected DI categories in December 2011. The figure shows that while the pre-reform trends deviated slightly between the placebo treatment and control groups (although none of the differences are significant at the 5% level), there were no statistically significant post-reform differences between the outcomes of the two groups. The patterns indicate that in the unaffected DI categories the reform had no impact on the probability of benefit receipt, employment, and having no income.[11]

The placebo results presented in Figure 5 indicate that for a placebo reform in 2011, there were no major pre-reform differences between the placebo treatment and control groups. Panel (d) suggests a very small, albeit statistically significant, increase in employment among the placebo treatment group relative to the placebo control group. This increase is about a tenth of the magnitude of our main effects estimated for the real reform year in Figure 2. There were no post-reform differences in other outcomes.

These placebo analyses suggest that our main results are driven by the impact of the 2012 reassessment reforms rather than by spurious differences that arise between our control and treatment groups or by other events that affect the two groups differently.

[11] Note that we do not present results for participation in the public works program because more severely disabled individuals had zero uptake.

5.2 Heterogeneity

To better understand the mechanisms underlying the broad effects of the reform documented so far, we turn to assessing the potential heterogeneous effects of the reassessment. We expect the reassessment and benefit loss to affect beneficiaries with different levels of attachment to the labor market in different ways.

We start by examining heterogeneity in outcomes by pre-reform employment. Importantly, approximately a quarter of benefit recipients were concurrently employed in 2011, the last pre-reform year. We add terms capturing the interaction of treatment status with 2011 employment status to our reduced form equations (1) and (2). We also re-estimate the instrumental variables equation (4) separately on the previously-employed and non-employed samples. Panel B of Table 3 reports the effect of the reform on labor market outcomes by pre-reform employment averaged over the post-reform period from estimating the modified equation (1). Appendix Figure A1 shows year-by-year estimates from estimating the modified equation (2). Panels B and C of Table 4 displays our instrumental variables estimates by pre-reform employment status.

The results reported in Panel B of Table 3 reveal that the overall decrease in DI receipt was driven by individuals who already had some employment while receiving DI benefits in 2011. Within this group, which makes up approximately one quarter of recipients, DI receipt while employed decreased by 4.9 percentage points (column 2), employment without receiving benefits increased by 2.3 percentage points (column 3), while the probabilities of participating in public works (column 4), or remaining without income from work or benefits (column 5) increased by smaller magnitudes. Within this group, there was also a statistically insignificant 1.7 percentage points increase in the probability of receiving benefits without employment (column 1).

Among the group of beneficiaries not working in 2011, the patterns are different: DI receipt decreased by 0.9 percentage point, and employment without receiving benefits increased by only 0.4 percentage point. The IV regression results reported in Panels B and C of Table 4 show that among individuals who lose their benefits as a consequence of the reform, labor market outcomes differ markedly by pre-reform employment. Panel B shows that among those with no pre-reform employment, 33.1% end up working after losing benefits, while 60.8% are not working but also not receiving benefits. At the same time, as Panel C shows among those with some pre-reform employment 80.8% are working and only 13.5% end up with no employment or benefits. Approximately 6% of both groups end up in the public works program.

In a similar vein, we investigate heterogeneity with respect to several other individual-

and region-specific characteristics that might moderate the impact of the reform on DI and employment outcomes. Appendix Table A2 and Appendix Table A3 show these results. In both tables, we replicate our baseline results in Panel A.

In both tables, Panel B presents the results for individuals with low versus high pre-reform spending on prescription drugs, a proxy for health. Here we define high spending as individuals whose annual spending was at least as high as the sample median in 2011. Appendix Table A2 shows that the impact of the reassessment on employment outcomes was concentrated in the group of relatively healthy individuals, which is consistent with healthier individuals being more likely to lose their benefits.[12] At the same time, the instrumental variables estimates in Appendix Table A3 suggest that the impact of benefit loss on outcomes was similar among healthier and less healthy individuals, with the exception of public work, which increases only in the healthier group.

Panel C of both tables shows results by occupation groups. We group individuals into skilled, unskilled and missing occupation categories based on the last observed pre-reform occupation. Occupation information is missing if no employment history is observed for an individual since January 2003. Close to half (48%) of our sample belong to this category. The results for skilled and unskilled workers are fairly similar. One exception is public work participation: Appendix Table A3 shows that benefit loss increases the probability of public work participation for unskilled workers but not for skilled workers.

Panel D displays results by the length of time spent on DI before the reform. DI length is measured as the time between the individual's first DI entry and December 2011. We estimate our results separately for individuals who received DI benefits for more or less than 10 years. The results are fairly consistent across groups with shorter and longer durations on benefits.

Finally, Panel E compares individuals in low- and high-unemployment areas. We distinguish between high and low unemployment groups depending on whether the unemployment rate in the individual's micro region was above or below the median in 2011. The results are similar for the two groups.

In sum, these heterogeneity results suggest that employment status while receiving benefits and health were the key determinants of being removed from benefits. Once removed from benefits, prior employment was the main driver of labor market success. Most individuals who were already employed while on benefits were able to remain employed, while most of those who were not working while on DI remained out of work while also losing their

[12] A logit regression model estimating the determinants of DI benefit loss is in line with our results on the importance of employment and health: Appendix Table A4 shows that employment in 2011 and health status (proxied by drug spending in 2011) are the two key determinants of losing DI status.

benefits. These findings point to the importance of improving labor market attachment while on benefits.

5.3 Additional Results

Job Quality. The sudden loss of income compels expelled beneficiaries to promptly search for employment. However, this rush can lead to lower-quality employment, for example, in the form of lower wages (Nekoei and Weber, 2017). The risk of human capital depreciation and a potential stigma effect can also lead to employment in lower quality jobs even in the case of successful job placement.

To investigate the quality of jobs held by individuals who leave DI due to the reform, we re-estimate equation (4) with employment at jobs with different quality attributes as dependent variables. We estimate the effect of DI exit on the following four outcome variables: (1) employment earning above the minimum wage & no DI; (2) full-time employment & no DI; (3) employment in a skilled job & no DI; (4) employment at a firm with above median TFP & no DI.

We then divide the estimated quality-specific employment effects with the total estimated effect of DI exit on employment, to obtain the share of employment effect that falls into the specific employment category. We compare this estimated share with the pre-DI share of treatment group individuals who were employed at the specific employment category (conditional on employment). With this approach, we provide insights on whether people who found employment after leaving DI as a consequence of the reform held worse quality jobs than their typical pre-DI jobs.

Figure 6 shows our results. Panel (a) shows that relative to a pre-DI mean of 77%, on average 72% of the employment effect came from jobs paying above the minimum wage. 53% of the employment effect came from full-time jobs according to panel (b), significantly lower than the pre-DI mean of 78%. Panel (c) shows that 51% of the employment effect came from skilled jobs, well below the pre-DI mean of 73%. Finally, panel (d) shows that 17% of the employment effect came from employers with above-median TFP, half of the pre-DI mean of 33%. The differences between the quality-specific employment effects and pre-DI means are more striking among those who had no employment in 2011. These results indicate that even individuals who were able to secure employment among the population whose benefit was terminated as a result of the reform experienced a deterioration in the quality of their jobs.

Results for women. We exclude women from the analysis of the impact of the reform because due to an early retirement option available for women only, the labor force outcomes

of the control and treatment group may evolve differently, as the early retirement option is more likely to be available in the (older) control group. Despite this concern, the results reported in Appendix Figure A2 indicate qualitatively similar reform effects for women as for men (Figure 2). Similarly, the IV estimates for the effect of DI exit on labor market outcomes for women, reported in Appendix Table A5, are similar to the results for men (Table 4).

Effects of the reform on healthcare use. Appendix Figure A3 shows the time pattern of the impact of the reform on healthcare use. These results suggest that there was a jump in GP visits, outpatient specialist visits, and the number of hospital days among treated individuals when the policy came into effect. This is likely explained by participation in the reassessment process. We do not see a similar jump in prescription drug spending. We also see that by 2013 (i.e., one year after the reform came into effect), the differences between the treatment and control group disappeared. We observe a small permanent increase in outpatient specialist care use – an increase by around 0.4 visit per quarter. Overall, these results suggest that the reform did not have major permanent effects on healthcare use, suggesting that the reform also did not have major health effects (assuming that health deterioration would be reflected in higher healthcare use).

6 Conclusion

This paper provides evidence on the labor market implications of a major reform that aimed to improve the targeting of disability benefit receipt by tightening eligibility conditions and reassessing benefit entitlement for a large share of beneficiaries. We identified the effects of the reform using the fact that the reassessment only applied to beneficiaries under an age cut-off and below a certain level of health impairment.

Our results suggest that while the reform decreased disability insurance receipt in the reassessed population, the resulting increase in employment was modest for those with no pre-reform employment in the age groups close to the age cut-off of the reform. The majority of reassessed beneficiaries who were not employed pre-reform were left without any income after losing their benefit. Further, those who returned to employment typically worked in lower quality jobs than pre-DI.

Overall, while the stricter disability benefit rules proved effective in reducing the number of disability recipients, the reform failed to activate those who were not employed pre-reform and thus had weaker ties to the labor market and were likely to be less employable. These results suggest that financial incentives for reactivating disability benefit recipients may

need to be combined with broader labor market policies, such as job search counseling and rehabilitation services to restore employability.

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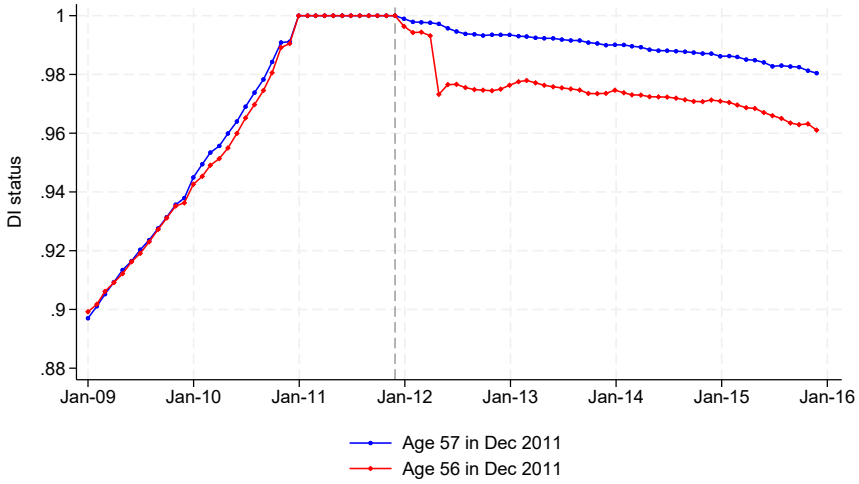
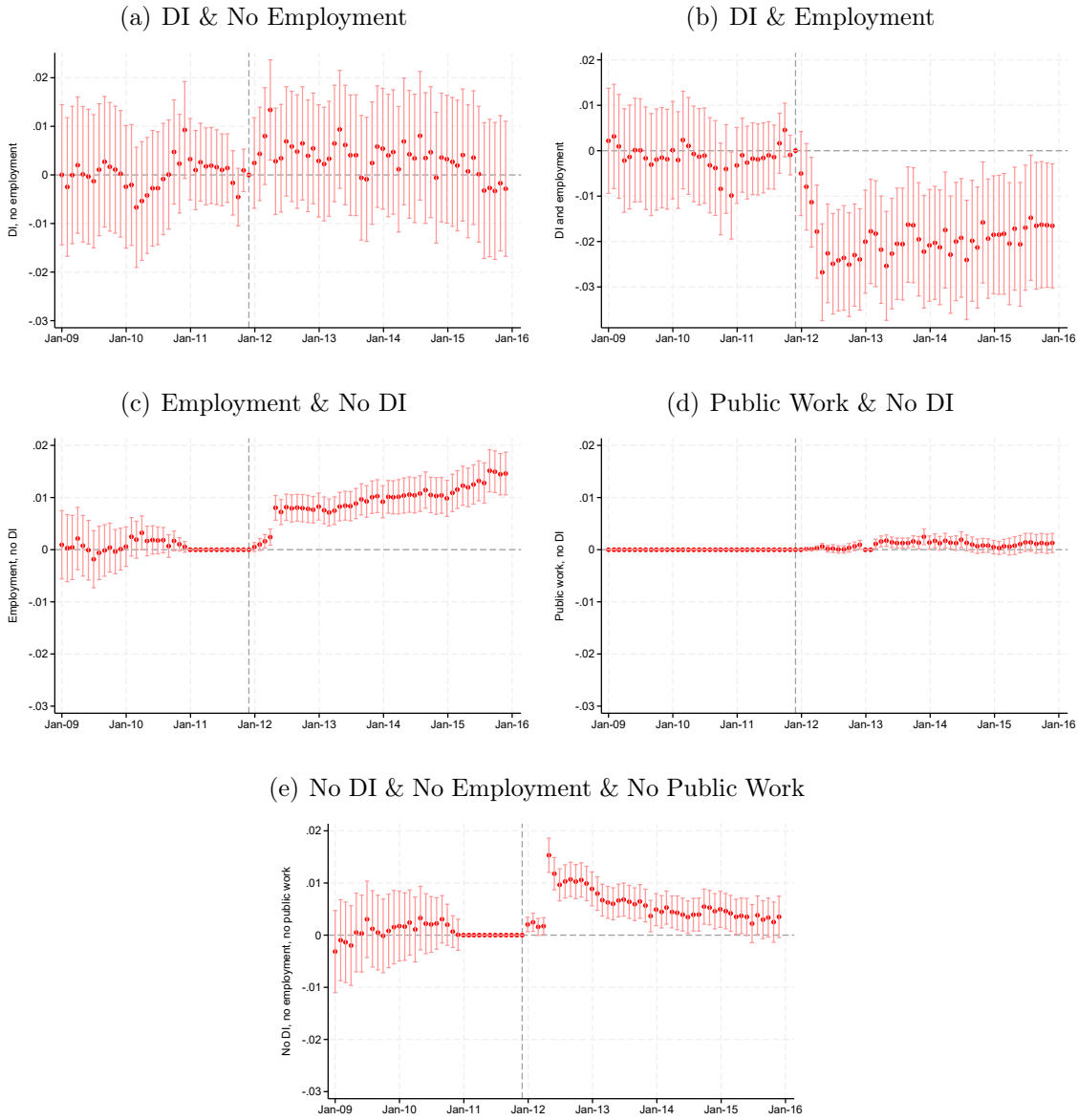
Figures and Tables

Figure 1: DI Status



Note: Figure shows the share of individuals receiving DI benefits. The sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011.

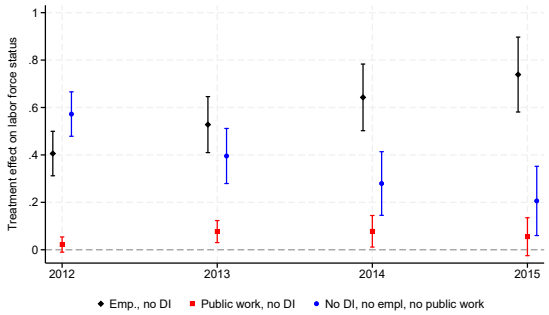
Figure 2: Effect of the Reform Over Time



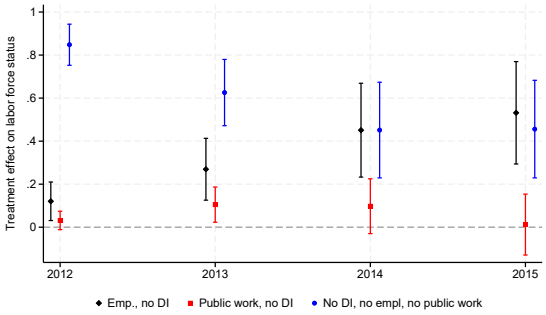
Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers below the age cut-off relative to control workers above the age cut-off. Figure displays the estimated β_t coefficients from equation (2) with 95% confidence intervals over 2009-2015, with December 2011 as the reference month. Sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011. Treated people were aged 56 in December 2011 and control people were aged 57 in December 2011.

Figure 3: Effect of DI Benefit Loss Over Time

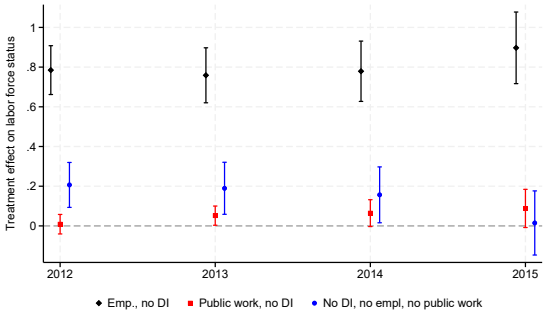
(a) All Individuals



(b) Individuals With No Employment in 2011

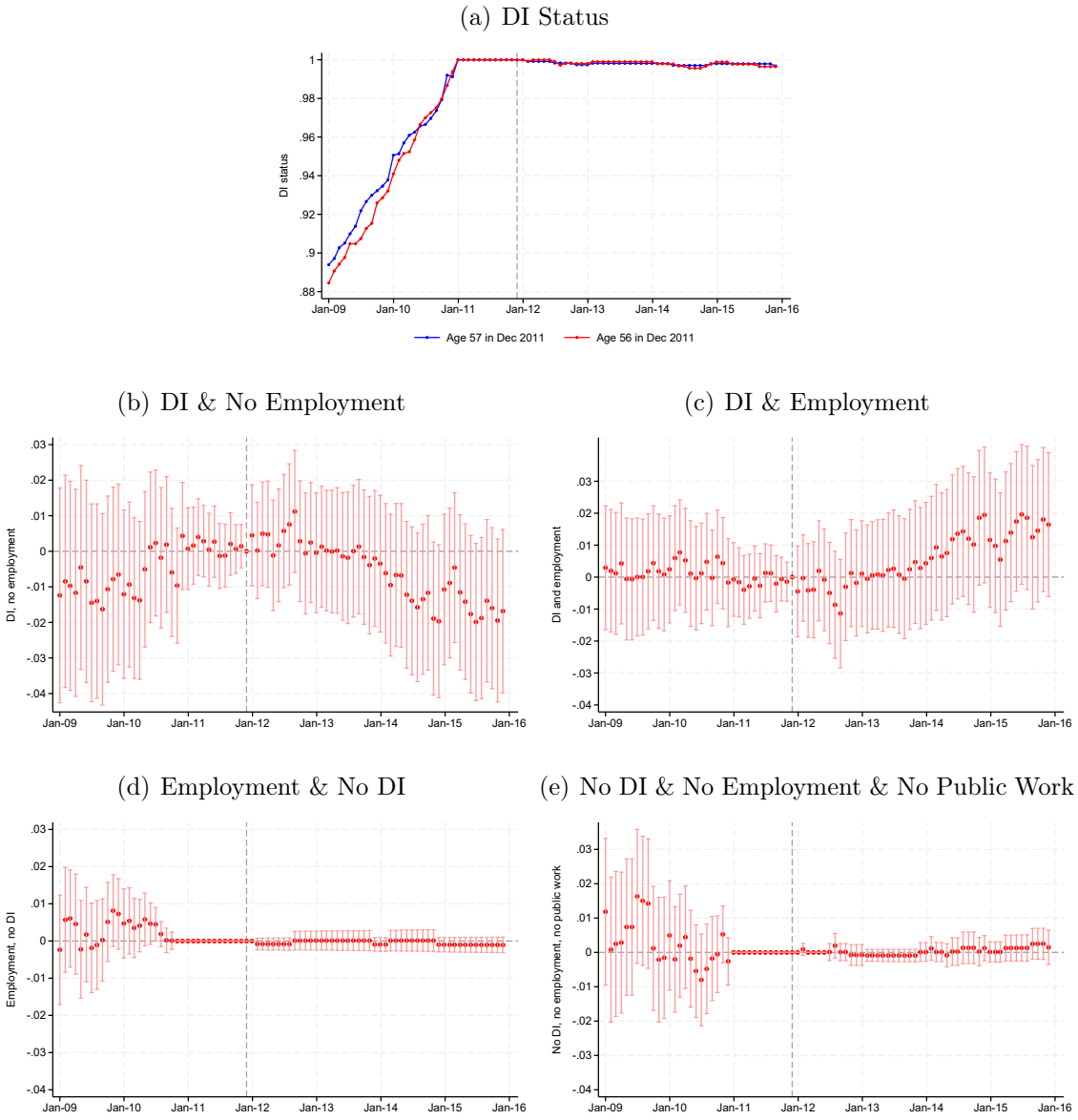


(c) Individuals With Some Employment in 2011



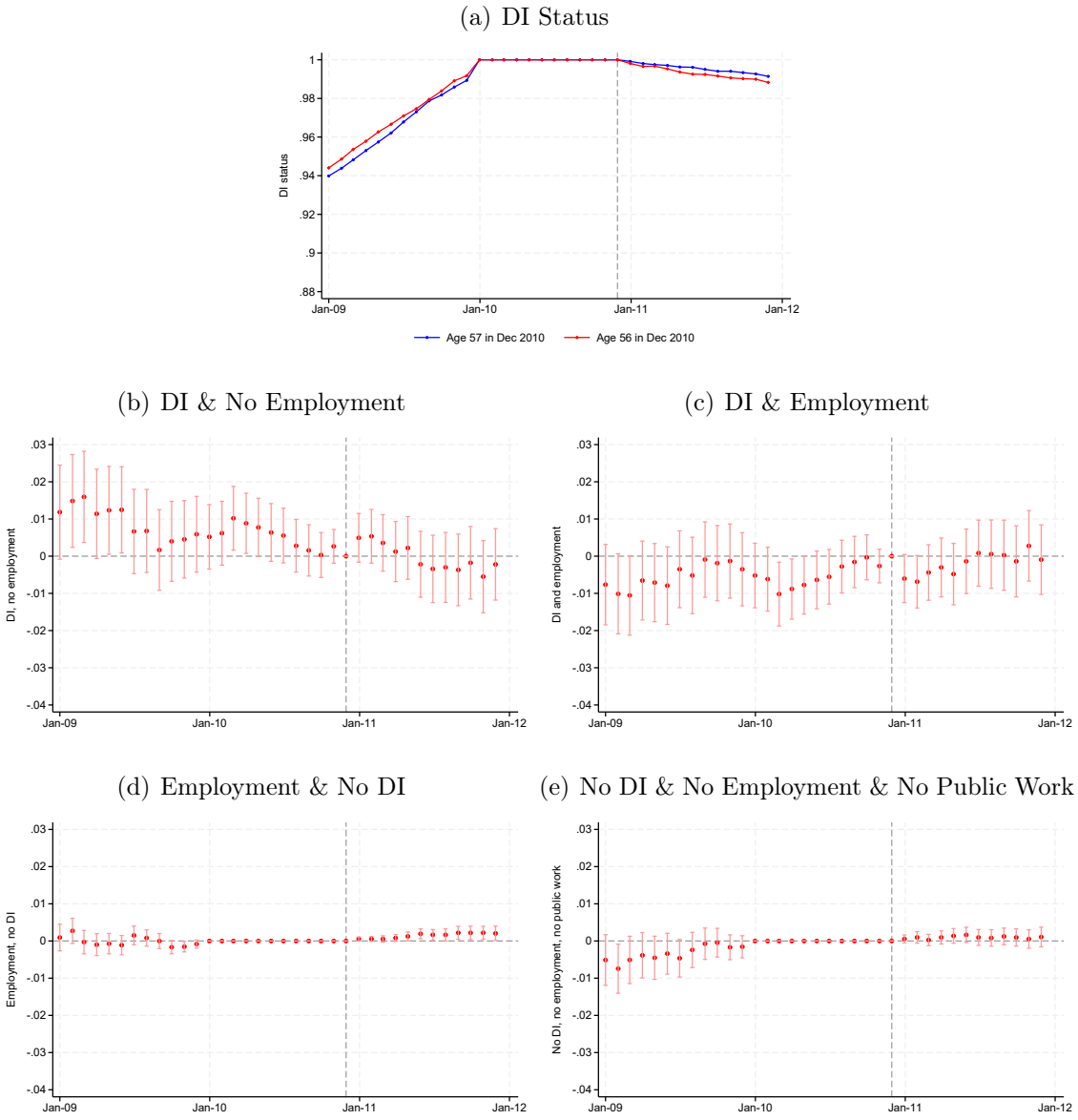
Note: Figure shows our estimates of the impact of losing DI benefits on the outcomes of affected workers. Figure displays the estimated β^{IV} coefficient from equation (4) with 95% confidence intervals estimated separately for each year 2012-2015 and by 2011 employment status. Sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011.

Figure 4: Placebo Analysis—Effect of the Reform Over Time, Unaffected DI Categories



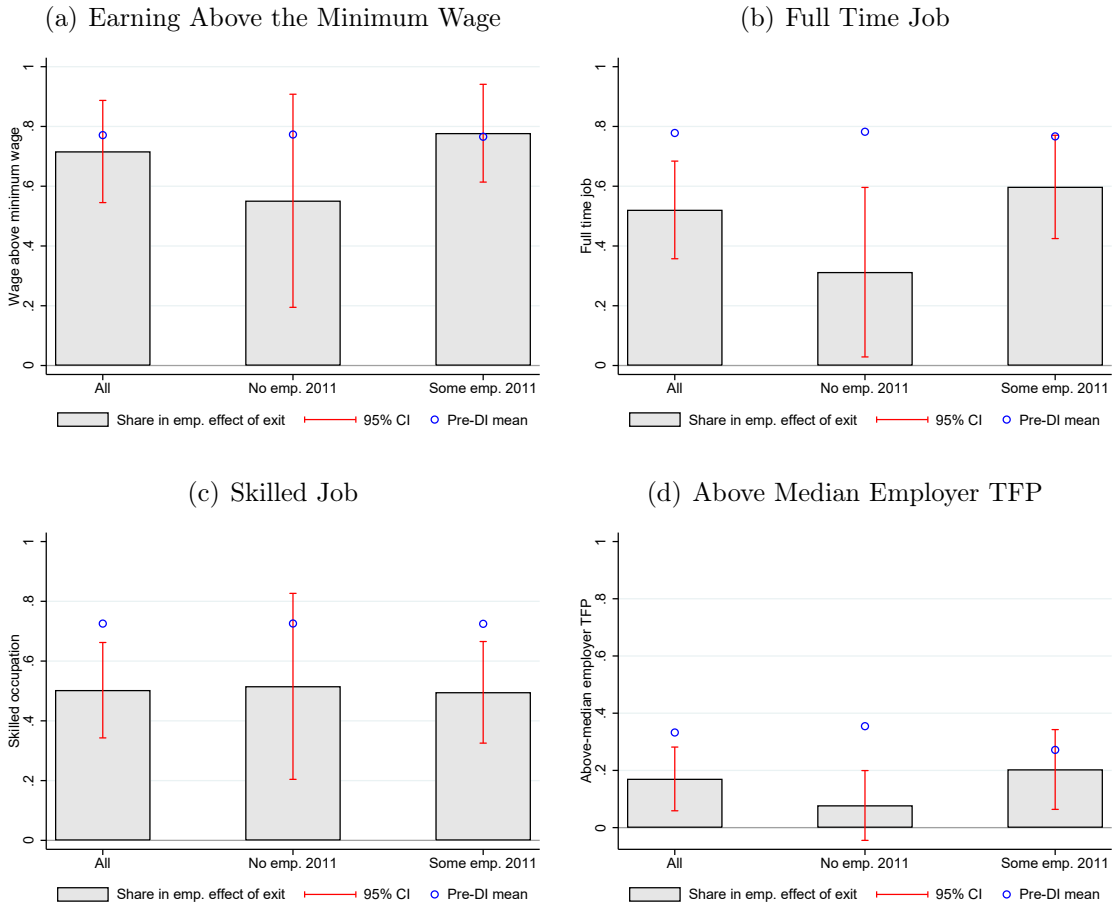
Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers below the age cut-off relative to control workers above the age cut-off for the placebo group of individuals in unaffected DI categories. Panel (a) shows the share of individuals receiving DI benefits. The sample is restricted to men who received DI throughout 2011, and belonged to the unaffected DI categories in December 2011. Panels (b) to (e) display the estimated β_t coefficients from equation (2) with 95% confidence intervals over 2009-2015, with December 2011 as the reference month. Sample is restricted to men who received DI throughout 2011, and belonged to the unaffected DI categories in December 2011. Treated people were aged 56 in December 2011 and control people were aged 57 in December 2011.

Figure 5: Placebo Analysis—Effect of Placebo Reform Over Time



Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers below the age cut-off relative to control workers above the age cut-off for a placebo reform in 2011. Panel (a) shows the share of individuals receiving DI benefits. The sample is restricted to men who received DI throughout 2011, and belonged to the unaffected DI categories in December 2010. Panels (b) to (e) display the estimated β_t coefficients from equation (2) with 95% confidence intervals over 2009-2012, with December 2010 as the reference month. Sample is restricted to men who received DI throughout 2010, and belonged to the unaffected DI categories in December 2010. Treated people were aged 56 in December 2011 and control people were aged 57 in December 2010.

Figure 6: Effect of DI Benefit Loss—Job Quality



Note: Figure shows the share of employment effects of leaving DI by job quality. Gray bars display the β^{IV} coefficient estimates of equation (4), capturing the effect of leaving DI on employment in a specific job category (job paying above the minimum wage, full time job, skilled job, employer having above median TFP), instrumented with being aged 56 versus 57 in December 2011, and divided by the IV estimated effect on overall employment. Red lines indicate 95% confidence interval. Sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011. Sample is split by having some employment in 2011, which indicator is set to one for people who had at least one month of employment, including self-employment, in 2011. Blue dots display the pre-DI mean outcome of individuals in the treatment group (age 56 in December 2011), restricting the pre-DI sample to months of employment.

Table 1: Health Revision Obligation Cut-Offs

		Age at end of 2011	
		Below 57 years	57 years and above
Health impairment	≥80%	No health revision	No health revision
	<80%	**Health revision**	No health revision

Note: Table shows the health revision cut-offs by health impairment and age.

Table 2: Descriptive Statistics

	Age at end of 2011	
	56 Treatment	57 Control
	(1)	(2)
Some employment in 2011	0.249	0.243
Mean length of DI status in Dec 2011 (years)	11.1	10.9
Mean drug spending in 2011 (euros)	533	512
Micro-region level unemployment rate in 2011	0.200	0.194
Pre-reform occupation		
Skilled	0.331	0.353
Unskilled	0.183	0.177
Missing	0.486	0.470
Number of individuals	6,638	7,554

Note: Table shows summary statistics for the control and treatment groups. Sample is restricted to men aged 56 or 57 in December 2011, who received DI throughout 2011, and belonged to the affected DI categories in December 2011. Treated people were aged 56 in December 2011 and control people were aged 57 in December 2010. Occupation classification is based on the last observed pre-reform employment.

Table 3: Effect of the Reform—Difference-in-Differences Estimates

	DI & no employment	DI & employment	Employment & no DI	Public work & no DI	No DI & no employment & no public work
	(1)	(2)	(3)	(4)	(5)
Panel A: Average effects					
Treated	0.003	-0.018***	0.009***	0.001**	0.005***
	(0.005)	(0.005)	(0.002)	(0.0004)	(0.002)
Panel B: Heterogeneity by employment in 2011					
Treated × no emp. in 2011	-0.002	-0.007	0.004***	0.001	0.004*
	(0.005)	(0.005)	(0.001)	(0.0004)	(0.002)
Treated × some emp. in 2011	0.017	-0.049***	0.023***	0.002**	0.007**
	(0.013)	(0.013)	(0.005)	(0.0009)	(0.003)
Observations	1,156,844	1,156,844	1,156,844	1,156,844	1,156,844
Individuals	14,192	14,192	14,192	14,192	14,192

Note: *** p<0.01, ** p<0.05, * p<0.1. Cluster-robust standard errors in parentheses. Table displays the β^{DiD} coefficient estimates of equation (1), showing the average treatment effect over 2012-2015. Sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011. Treated people were aged 56 in December 2011, control people were aged 57 in December 2011. In Panel B, the binary heterogeneity indicator of some employment in 2011 is set to one for people who had at least one month of employment, including self-employment, in 2011.

Table 4: Effect of DI Benefit Loss—Instrumental Variables Estimates

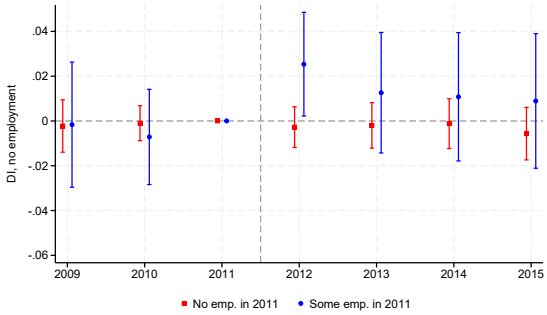
	Employment & no DI	Public work & no DI	No DI & no employment & no public work
	(1)	(2)	(3)
Panel A: All individuals			
Exit	0.584***	0.059***	0.357***
	(0.054)	(0.022)	(0.050)
Observations	645,932	645,932	645,932
Individuals	14,192	14,192	14,192
Panel B: Individuals with no employment in 2011			
Exit	0.331***	0.061	0.608***
	(0.071)	(0.037)	(0.070)
Observations	483,124	483,124	483,124
Individuals	10,702	10,702	10,702
Panel C: Individuals with some employment in 2011			
Exit	0.808***	0.057**	0.135**
	(0.064)	(0.026)	(0.056)
Observations	162,808	162,808	162,808
Individuals	3,490	3,490	3,490

Note: *** p<0.01, ** p<0.05, * p<0.1. Cluster-robust standard errors in parentheses. Table displays the β^{IV} coefficient estimates of equation (4), capturing the effect of leaving DI, instrumented with being aged 56 versus 57 in December 2011. Sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011. In Panels B and C, the sample is split by having some employment in 2011, which indicator is set to one for people who had at least one month of employment, including self-employment, in 2011.

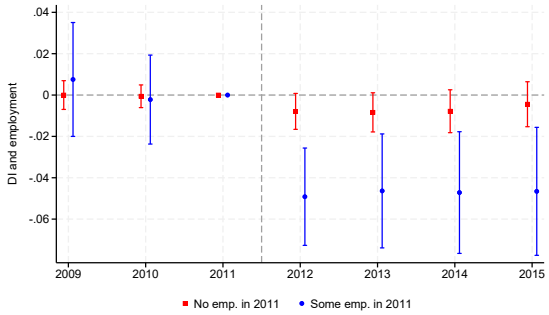
Appendix: Additional Figures and Tables

Appendix Figure A1: Effect of the Reform Over Time—Heterogeneity by Pre-Reform Employment Status

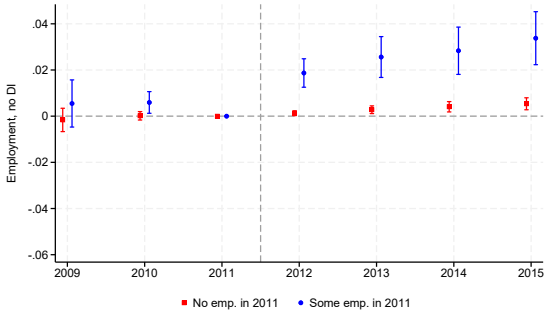
(a) DI & No Employment



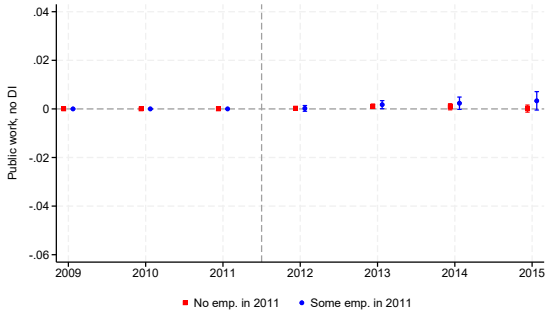
(b) DI & Employment



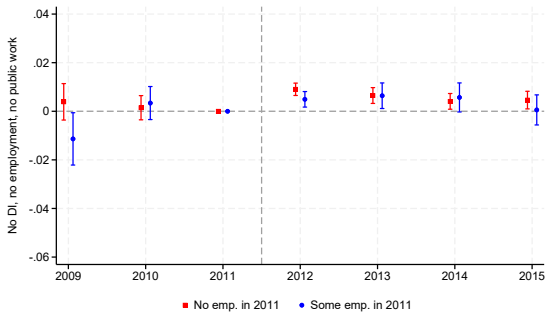
(c) Employment & No DI



(d) Public Work & No DI



(e) No DI & No Employment & No Public Work

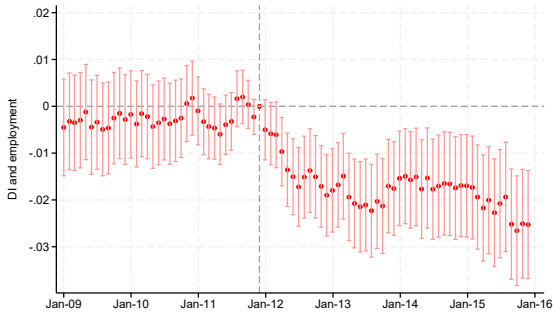


Note: Figure displays the β_t coefficient estimates of a yearly version of equation (2) interacted with employment in 2011, showing the treatment effects over 2009-2015, with 2011 as reference year. 95% confidence intervals are displayed. Sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011. Treated people were aged 56 in December 2011, control people were aged 57 in December 2011. The binary heterogeneity indicator of some employment in 2011 is set to one for people who had at least one month of employment, including self-employment, in 2011.

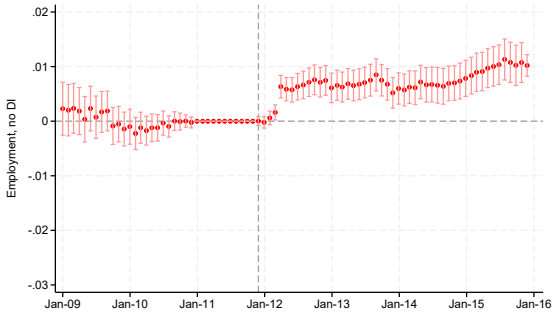
Appendix Figure A2: Effect of the Reform Over Time—Women

(a) DI & No Employment

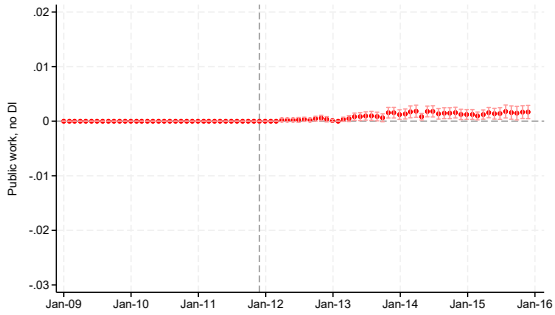
(b) DI & Employment



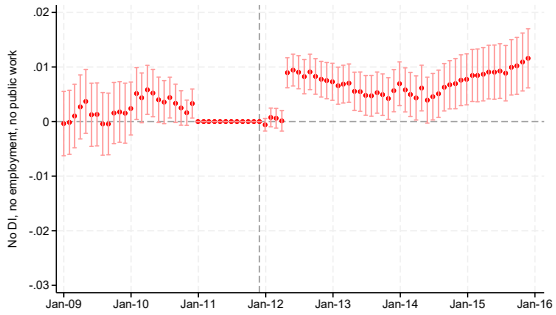
(c) Employment & No DI



(d) Public Work & No DI

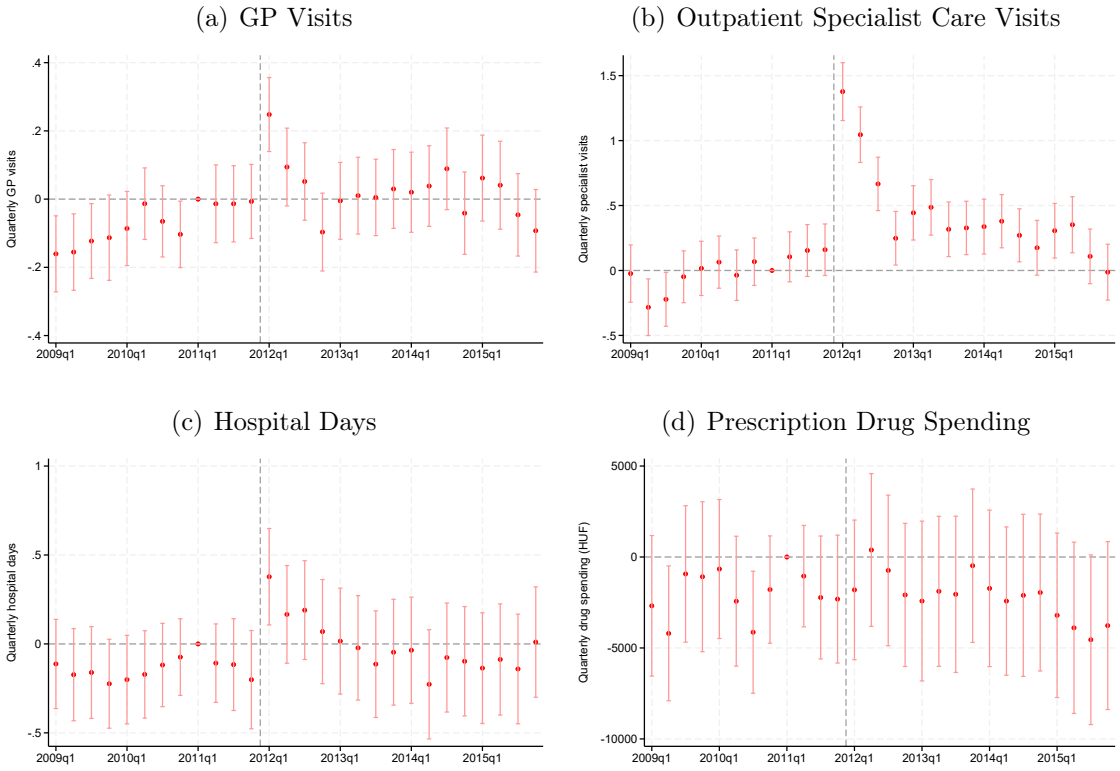


(e) No DI & No Employment & No Public Work



Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers below the age cut-off relative to control workers above the age cut-off. Figure displays the estimated β_t coefficients from equation (2) with 95% confidence intervals over 2009-2015, with December 2011 as the reference month. Sample is restricted to women who received DI throughout 2011, and belonged to the affected DI categories in December 2011. Treated people were aged 56 in December 2011 and control people were aged 57 in December 2011.

Appendix Figure A3: Effect of the Reform—Healthcare Use



Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers below the age cut-off relative to control workers above the age cut-off. Figure displays the estimated β_t coefficients from equation (2) with 95% confidence intervals over 2009-2015, with the first quarter of 2011 as the reference quarter. Sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011. Treated people were aged 56 in December 2011 and control people were aged 57 in December 2011.

Appendix Table A1: 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 people receiving disability benefits (N=409,846).

Appendix Table A2: Effect of the Reform—Difference-in-Differences Estimates, Heterogeneity

	DI & no emp.	DI & emp.	Emp. & no DI	Public work & no DI	No DI & no emp. & no public work
Panel A: Average effects					
Treated	0.003	-0.018***	0.009***	0.001**	0.005***
	(0.005)	(0.005)	(0.002)	(0.0004)	(0.002)
Panel B: By drug spending in 2011					
Treated × Low drug spending	0.006	-0.031***	0.014***	0.002**	0.009***
	(0.007)	(0.007)	(0.002)	(0.001)	(0.003)
Treated × High drug spending	-0.001	-0.005	0.004*	0.000	0.001
	(0.008)	(0.007)	(0.002)	(0.000)	(0.002)
Panel C: By pre-reform occupation					
Treated × Skilled	0.011	-0.026***	0.013***	0.001	0.002
	(0.009)	(0.009)	(0.004)	(0.0005)	(0.004)
Treated × Unskilled	0.009	-0.030**	0.012***	0.002*	0.008
	(0.013)	(0.012)	(0.004)	(0.001)	(0.005)
Treated × Missing	-0.001	-0.009	0.004***	0.001	0.005**
	(0.007)	(0.007)	(0.001)	(0.001)	(0.002)
Panel D: By length of DI status in Dec 2011					
Treated × Short DI	0.005	-0.017**	0.007***	0.001**	0.004
	(0.008)	(0.007)	(0.003)	(0.001)	(0.003)
Treated × Long DI	0.003	-0.018***	0.010***	0.001	0.005***
	(0.007)	(0.007)	(0.002)	(0.001)	(0.002)
Panel E: By unemployment rate in 2011					
Treated × Low unemployment	-0.003	-0.013*	0.010***	0.001	0.006*
	(0.008)	(0.007)	(0.003)	(0.0004)	(0.003)
Treated × High unemployment	0.010	-0.023***	0.008***	0.001*	0.004
	(0.007)	(0.007)	(0.002)	(0.001)	(0.002)
Observations	1,156,844	1,156,844	1,156,844	1,156,844	1,156,844
Individuals	14,192	14,192	14,192	14,192	14,192

Note: *** p<0.01, ** p<0.05, * p<0.1. Cluster-robust standard errors in parentheses. Table displays the β^{DiD} coefficient estimates of equation (1) extended with heterogeneity indicators, showing the average treatment effect over 2012-2015. Sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011. Treated people were aged 56 in December 2011, control people were aged 57 in December 2011. In Panel B, the binary heterogeneity indicator of low (high) drug spending in 2011 is set to one for people whose spending on medicines in 2011 is below (equal to or above) the sample median in that year. In Panel C, occupation classification is based on the last observed pre-reform employment. 34% of the individuals are skilled workers (including both white and skilled blue collars), 18% are unskilled workers. Occupation information is missing for 48% of the sample. In Panel D, at least 10 years on DI is DI length measured up to December 2011 of 10 or more years, where 10 years is the sample median DI length in December 2011. In Panel E, high (low) unemployment is unemployment rate equal to or above (below) the median unemployment rate (16.7%) at the micro-region level in 2011.

Appendix Table A3: Effect of DI Benefit Loss—Instrumental Variables Estimates, Heterogeneity

	Emp. & no DI	Public work & no DI	No DI & no emp. & no public work
Panel A: Average effects			
Exit	0.584***	0.059***	0.357***
	(0.054)	(0.022)	(0.050)
Observations	645,932	645,932	645,932
Individuals	14,192	14,192	14,192
Panel B: By drug spending in 2011			
Exit, low drug spending	0.565***	0.074***	0.361***
	(0.064)	(0.028)	(0.060)
Observations	323,796	323,796	323,796
Individuals	7,096	7,096	7,096
Exit, high drug spending	0.629***	0.018	0.353***
	(0.095)	(0.032)	(0.088)
Observations	322,136	322,136	322,136
Individuals	7,096	7,096	7,096
Panel C: By pre-reform occupation			
Exit, skilled	0.676***	0.036	0.288***
	(0.070)	(0.022)	(0.067)
Observations	222,964	222,964	222,964
Individuals	4,863	4,863	4,863
Exit, unskilled	0.654***	0.087**	0.260***
	(0.108)	(0.044)	(0.099)
Observations	116,169	116,169	116,169
Individuals	2,548	2,548	2,548
Exit, missing occupation	0.413***	0.068	0.519***
	(0.099)	(0.054)	(0.093)
Observations	306,799	306,799	306,799
Individuals	6,781	6,781	6,781
Panel D: By length of DI status in Dec 2011			
Exit, at most 10 years on DI	0.521***	0.077**	0.402***
	(0.080)	(0.031)	(0.074)
Observations	319,241	319,241	319,241
Individuals	7,023	7,023	7,023
Exit, at least 10 years on DI	0.643***	0.042	0.315***
	(0.076)	(0.032)	(0.069)
Observations	326,691	326,691	326,691
Individuals	7,169	7,169	7,169
Panel E: By unemployment rate in 2011			
Exit, low unemployment	0.616***	0.034*	0.349***
	(0.073)	(0.021)	(0.070)
Observations	301,150	301,150	301,150
Individuals	6,653	6,653	6,653
Exit, high unemployment	0.554***	0.081**	0.365***
	(0.080)	(0.039)	(0.072)
Observations	344,782	344,782	344,782
Individuals	7,539	7,539	7,539

Note: *** p<0.01, ** p<0.05, * p<0.1. Cluster-robust standard errors in parentheses. Table displays the β^{IV} coefficient estimates of equation (4), capturing the effect of leaving DI, instrumented with being aged 56 versus 57 in December 2011. Sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011. Sample is split by heterogeneity indicators. In Panel B, the binary heterogeneity indicator of low (high) drug spending in 2011 is set to one for people whose spending on medicines in 2011 is below (equal to or above) the sample median in that year. In Panel C, occupation classification is based on the last observed pre-reform employment. 34% of the individuals are skilled workers (including both white and skilled blue collars), 18% are unskilled workers. Occupation information is missing for 48% of the sample. In Panel D, at least 10 years on DI is DI length measured up to December 2011 of 10 or more years, where 10 years is the sample median DI length in December 2011. In Panel E, high (low) unemployment is unemployment rate equal to or above (below) the median unemployment rate (16.7%) at the micro-region level in 2011.

Appendix Table A4: Logit Model of DI Benefit Loss

	DI exit	
	Logit coefficient	Average marginal effect
Some employment in 2011	0.995***	0.025***
	(0.155)	(0.004)
High drug spending in 2011	-1.060***	-0.026***
	(0.146)	(0.004)
Pre-reform occupation (ref.: skilled)		
Unskilled	-0.217	-0.005
	(0.178)	(0.004)
Missing	-0.157	-0.004
	(0.178)	(0.005)
Long DI in Dec 2011	-0.178	-0.004
	(0.142)	(0.004)
High unemployment rate in 2011	-0.097	-0.002
	(0.132)	(0.003)
Number of observations	302,398	
Number of individuals	6,638	

Note: *** p<0.01, ** p<0.05, * p<0.1. Cluster-robust standard errors in parentheses. Table displays logit coefficients and average marginal effects for not receiving DI benefit (cofficients of monthly date dummies are not displayed). Sample is restricted to men aged 56 in December 2011, who belonged to the affected DI categories in December 2011. Sample years: 2012-2015. The binary indicator of high drug spending in 2011 is set to one for people whose spending on medicines in 2011 is equal to or above the sample median in that year. Occupation classification is based on the last observed pre-reform employment. Long DI is DI length measured up to December 2011 of 10 or more years, where 10 years is the sample median DI length in December 2011. High unemployment indicates unemployment rate equal to or above the median unemployment rate (16.7%) at the micro-region level in 2011.

Appendix Table A5: Effect of DI Benefit Loss—Instrumental Variables Estimates, Women

	Emp. & no DI	Public work & no DI	No DI & no emp. & no public work
Panel A: All individuals			
Exit	0.474***	0.067***	0.459***
	(0.062)	(0.018)	(0.064)
Observations	895,452	895,452	895,452
Individuals	19,060	19,060	19,060
Panel B: Individuals with no employment in 2011			
Exit	0.174***	0.086***	0.739***
	(0.047)	(0.027)	(0.052)
Observations	655,631	655,631	655,631
Individuals	14,003	14,003	14,003
Panel C: Individuals with some employment in 2011			
Exit	0.835***	0.050**	0.115
	(0.156)	(0.023)	(0.162)
Observations	239,821	239,821	239,821
Individuals	5,057	5,057	5,057

Note: *** p<0.01, ** p<0.05, * p<0.1. Cluster-robust standard errors in parentheses. Table displays the β^{IV} coefficient estimates of equation (4), capturing the effect of leaving DI, instrumented with being aged 56 versus 57 in December 2011. Sample is restricted to women who received DI throughout 2011, and belonged to the affected DI categories in December 2011. In Panels B and C, the sample is split by having some employment in 2011, which indicator is set to one for people who had at least one month of employment, including self-employment, in 2011.