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The Labor Market Effects of Disability Benefit Loss

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ABSTRACT
Disability benefits are costly and tend to reduce labor supply. While costs can be reduced by careful targeting, correcting past 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, 57% were employed in the primary labor market and 38% had neither employment nor benefit income. The consequences of leaving disability insurance sharply differed by pre-reform employment status. 62% of those without pre-reform employment did not work after exiting disability insurance, whereas this ratio was only 14% for those who had some employment in the pre-reform year. 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.

JEL codes: H53, H55, J08, J14
Keywords: disability benefit, reactivation, employment policy

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A rokkantsági ellátások megvonásának munkapiaci hatásai

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ÖSSZEFOGLALÓ

A rokkantsági ellátások költségesek és jellemzően csökkentik a munkakínálatot. Bár a költségek gondos célzással csökkenthetők, a korábbi jogosultsági szabályok vagy értékelési eljárások korrekciója jóléti költségekkel járhat. Kutatásunkban megvizsgáljuk a magyarországi rokkantsági ellátások 2012-es reformját, amelynek keretében felülvizsgálták az ellátottak jelentős részének egészségi állapotát és munkaképességét. Felhasználva, hogy a felülvizsgálatra kötelezettek körét az életkor és az egészségkárosodás mértéke határozta meg, megbecsüljük az ellátások megvonásának és csökkentésének munkapiaci hatásait. Eredményeink szerint a rokkantsági biztosítást a felülvizsgálat miatt elhagyni kényszerülők 57%-a az elsődleges munkaerőpiacon dolgozott, átlagosan 38%-uknak azonban sem foglalkoztatásból, sem a rokkantsági ellátásból nem volt jövedelme a reformot követő négy évben. A rokkantsági ellátásokból való kilépés következményei nagyban különböztek a reform előtti foglalkoztatási státusz szerint. A reform előtti munkaviszonnyal nem rendelkezők 62%-a rokkantsági biztosításból való kilépés után sem tudott elhelyezkedni, míg ez az arány csak 14% volt azoknál, akiknek a reform előtti évben volt valamilyen munkaviszonyuk. A felülvizsgálat az érintettek aktiválásában szerény eredményt ért el, ami erősen függött a reform előtti foglalkoztatási státusztól. A rokkantsági ellátásban részesülők munkapiaci integrációja akkor lehet sikeres, ha a pénzügyi ösztönzők mellé a foglalkoztatási esélyeket javító támogató munkapiaci programok is társulnak.

JEL: H53, H55, J08, J14
Kulcsszavak: rokkantsági biztosítás, felülvizsgálat, foglalkoztatás

The Labor Market Effects of Disability Benefit Loss*

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Abstract

Disability benefits are costly and tend to reduce labor supply. While costs can be reduced by careful targeting, correcting past 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 57 % were employed in the primary labor market and 38% had neither employment nor benefit income in the post-reform period. The consequences of leaving disability insurance sharply differed by pre-reform employment status. 62% of those without pre-reform employment did not work after exiting disability insurance, whereas this ratio was only 14% for those who had some employment in the pre-reform year. 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 Codes: H55, J14

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1 Introduction

The rise in disability rolls over the last decades in developed countries (OECD, 2010) combined with low levels of employment among beneficiaries have prompted policy makers to examine how the design of disability insurance (DI) can facilitate the labor market reintegration of beneficiaries. 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 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 successfully return beneficiaries to the labor market. Moreover, if they include the removal of benefits, they can harm beneficiary welfare. If, on the other hand, poorly designed incentives, such as too-low earnings limits (Krekó, Prinz and Weber, 2023) are behind low levels of reintegration, 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 active support to return to work. There are several potential reasons for poor labor market (re)integration, including human capital depreciation (Edin and Gustavsson, 2008) and stigma (Eriksson and Rooth, 2014; Fernández-Blanco and Preugschat, 2018).

In this paper, we study a unique large-scale reassessment policy 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 thousand beneficiaries (9% of the reassessed beneficiaries and 5% of all beneficiaries) lost their benefits while about 12 thousand 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 vs 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 exited 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 work increased by 0.1 percentage point. Roughly one-third of exiting beneficiaries were not employed: the probability of having no income from DI or employment increased by 0.5 percentage point.

The post-reform labor market outcomes differ greatly according to the 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 treated beneficiaries were employed in 2011, half of those who lost their benefits came from this group. 80% of them were still employed post-reform, while 14% had no job or benefits. The other half of recipients who lost their benefits fared worse in the labor market: only 32% were employed post-reform, while 62% 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. This suggests that policy makers may want to consider interventions that would bring beneficiaries back to the labor market earlier or would incentivize more work while receiving benefits.

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 reassessment initiatives in the Netherlands. They find an increase in mortality among low-income women whose benefits were reduced following the 1993 reform. They also 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. We contribute to this literature by pointing out the importance of employment during DI on post-DI labor market outcomes. Our rich database also allows us to analyze the impact of eligibility withdrawals on employment prospects and job quality. 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 tried to address 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

The Hungarian DI system in the 1990s had lenient eligibility rules and relatively high benefit levels (Scharle, 2008). The deep recession following the economic transition from socialism to market economy and the accompanying increase in the unemployment rate led to the expansion of benefit programs, including DI and early retirement, to ease the pressure on the labor market. 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 disability (Scharle, 2008). The 2008 reform consisted of three key elements. First, a new complex assessment system was introduced, which put more emphasis on remaining working capacity, rehabilitation and skill development. The second element was the introduction of a new type of benefit, the rehabilitation benefit, which was granted for up to three years only and thus helped to reduce the take-up of permanent disability benefits. Those with health damage of at least 50% and assessed as rehabilitable were eligible for this benefit, along with employment rehabilitation services. After the reform, rehabilitation services became more widely available (Adamecz-Völgyi, Lévay, Bördős and Scharle, 2018).

In this paper we study a 2012 reform which tightened eligibility and reduced benefit levels (Nagy, 2015; Kovács, 2019). As a consequence of the two reforms the share of beneficiaries decreased to 4% of the active population and the costs of DI benefits decreased below 1% of GDP by 2017, one of the lowest values in Europe. Importantly, the 2012 legislation aimed not only to curb the inflow into disability benefits by tightening eligibility but also to reactivate beneficiaries with some remaining working capacity by reviewing their status. The objective was to rationalize the DI system, which was assessed to remain unsustainable and overly generous even after the 2008 reform, and was still believed to contribute to the low activity rate in Hungary. 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

In this paper we study a 2012 policy under which approximately 200 thousand DI recipients below age 57 had to undergo a health review based on new, stricter rules. The policy applied to DI recipients with partial disability, whose assessed health impairment was below 80% (Table 1). Two disability benefit programs were affected: Category III Disability Pension received by beneficiaries with health damage of 50% to 79% and Regular Social Assistance received by beneficiaries with health damage above 40%. Exempt from the review were recipients of Category I and Category II Disability Pensions, which applied to individuals with at least 80% health damage, all beneficiaries within 5 years of the retirement age, as well as recipients of the Transitory Allowance, a fixed-term benefit targeted at moderately disabled individuals. The 80% threshold refers to the level of health damage assigned to beneficiaries in the pre-reform assessment system, when receiving disability status. The percentage value of health impairments is based on a complex assessment process carried out by a team of physicians and rehabilitation experts. Individuals whose health impairment was classified higher than 40% during the post-reform review retained eligibility to benefits. Beneficiaries had to declare by March 2012 whether they wished to undergo the review. If they failed to make a declaration or they did not request the review, they lost their entitlement by May 2012.

About 18 thousand 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 thousand in January 2012 to 355 thousand in January 2017 (Hungarian Central Statistical Office, 2022), due to a large drop in inflows. This large drop in the number of beneficiaries 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 thousand beneficiaries decreased in inflation-adjusted terms. Mainly due to capacity constraints, the reviews lasted several years, until 2016.

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. At the same time, recipients

over 62 years of age were reclassified as old-age pensioners.

Although the comprehensive reevaluation of a broad segment 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, beneficiaries in Hungary who suffered a partial or complete loss of their benefit received little support for returning to the labour market. The capacity of rehabilitation services at the time was very limited and intensive, personalised services were only provided by a handful of small NGOs, operating mainly in urban centres (Krekó and Scharle, 2020). [1]

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.[2] 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. 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

[1] In contrast, the Netherlands 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).

[2] We exclude 2017 from our analysis because DI status is not completely observed in that year.

the maximum possible earnings.[3] We analyze public work as a separate outcome.[4] Based 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.[5] 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 reform on DI recipients subject to the obligatory 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

[3]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. 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.

[4]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 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). Between 2011 and 2015, both the net and gross basic public work wage ranged between 70-80% of the statutory minimum wage.

[5]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).

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 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. 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.[6] 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 (24.2% vs 24.8%) 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 475 euros vs 455 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 19.3% for the control group. They also work in occupations with similar skill levels: 33.0% of the treatment group and 35.2% work in skilled occupations, while 18.3% and 17.6%, respectively work in unskilled ones.

Figure 1 plots 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

[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.

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 of the divergence occurs in May 2012, which suggests that although the review process lasted until 2016, most beneficiaries were affected early on. The uptick in June 2012 relative to May 2012 is caused by the reassignment to the new types of benefits.[7]

4.2 Difference-in-Differences

To study the "reduced form" impact of the reassessment on labor market outcomes, 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 at each monthly date relative to December 2011.

In all of our 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.

[7] 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.

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 assumption. First, Table 2 shows that the control and treatment groups are quite 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, $\mathbb{1}[AGE_i = 56]$ 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 percent point higher probability of losing their benefits. The exogeneity assumption requires that being subject to reassessment does not affect labor market outcomes directly. 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 the reassessment process itself impacted labor market outcomes independent of benefit removal, for example by causing stress or uncertainty about future benefit receipt, our estimates could be biased.

5 Results

5.1 Main Results

We start by reporting our estimates of the overall impact of the reassessment 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.

Panel (a) of Figure 3 shows our year-by-year instrumental variables estimates of the effect of DI exit on labor market outcomes 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. Panel A of Table 4 reports the instrumental variables estimates pooled over the post-reform period.

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. 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 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 57% 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 became inactive but were able to find employment later on. Column (5) of Panel A of Table 3 shows that the overall increase in inactivity is 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 inactivity from 60% in 2012 to about 20% in 2015, with a post-reassessment average of 37.5% 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 became inactive without 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 now turn to.

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 inactivity.[8]

The placebo results presented in Figure 5 indicate that for a placebo reform in year 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.

Overall, 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.

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

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

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.

These results reveal that the overall decrease in DI receipt is 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 decreases by 4.8 percentage points (column 2), employment without receiving benefits increases by 2.2 percentage points (column 3), while the probabilities of participating in public works (column 4), or becoming inactive without benefits (column 5) increase by smaller magnitudes. Within this group, there is also a statistically insignificant 1.7 percentage points increase in the probability of receiving benefits without employment (column 1). Among the group of beneficiaries that was not working in 2011, the patterns are different: 40% of those who lose benefit eligibility end up employed on the primary labor market after the reform, 10% participated in puplic works, while the other half ends up without employment or benefits. 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, 31.9% end up working after losing benefits, while 62.3% are not working but also not receiving benefits. At the same time, as Panel C shows among those with some pre-reform employment 80% are working and only 14.3% 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 is concentrated in the group of relatively healthy individuals, which is consistent with healthier individuals being more likely to lose their benefits. 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.

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 that as Appendix Table A3 shows the impact of benefit loss on participation in the public works program is important 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 categorize individuals with at least 10 years on DI as "long DI". 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.

The heterogeneity results are in line with the results or a logit regression model estimating the determinants of DI benefit loss. The results reported in Appendix Table A4 show that employment in 2011 and health status (proxied by drug spending in 2011) are the two key determinants of losing DI status.

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 lower decreased wages (Nekoei and Weber, 2017). The risk of human capital depreciation and stigma effect further indicate a decline in the quality of jobs even in case of successful job placement.

To investigate the quality of jobs where individuals who leave DI due to the reform arrive at, we re-estimate equation (4) with employment at specific job qualities as dependent variables. We estimate the effect of DI exit on the following four quality-specific employment

outcome variables: (1) employment with 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 goes to 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 whether people who found employment after leaving DI as a consequence of the reform, arrived at worse quality jobs than the typical pre-DI job.

Figure 6 shows our results. Panel (a) shows that relative to a pre-DI mean of 77%, on average 71% of the employment effect came from jobs paying above the minimum wage. 52% 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 50% 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 the treated individuals when the policy came into effect. We do not see such 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.

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 counselling and rehabilitation services to restore employability.

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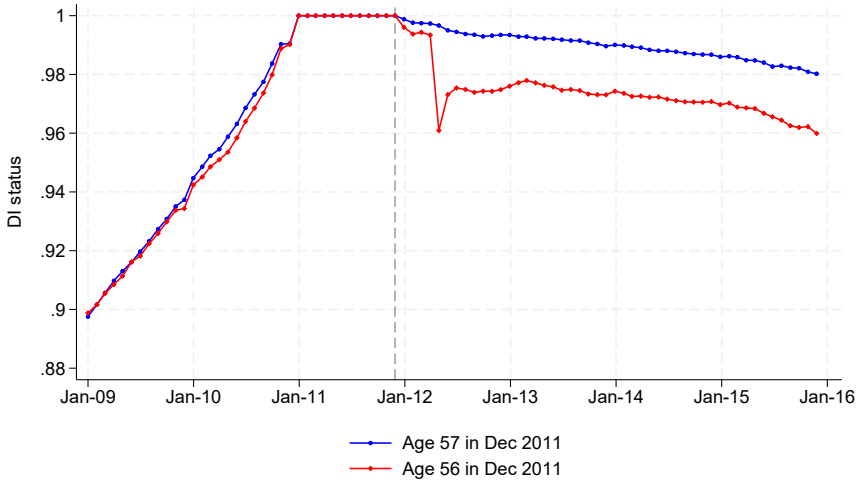
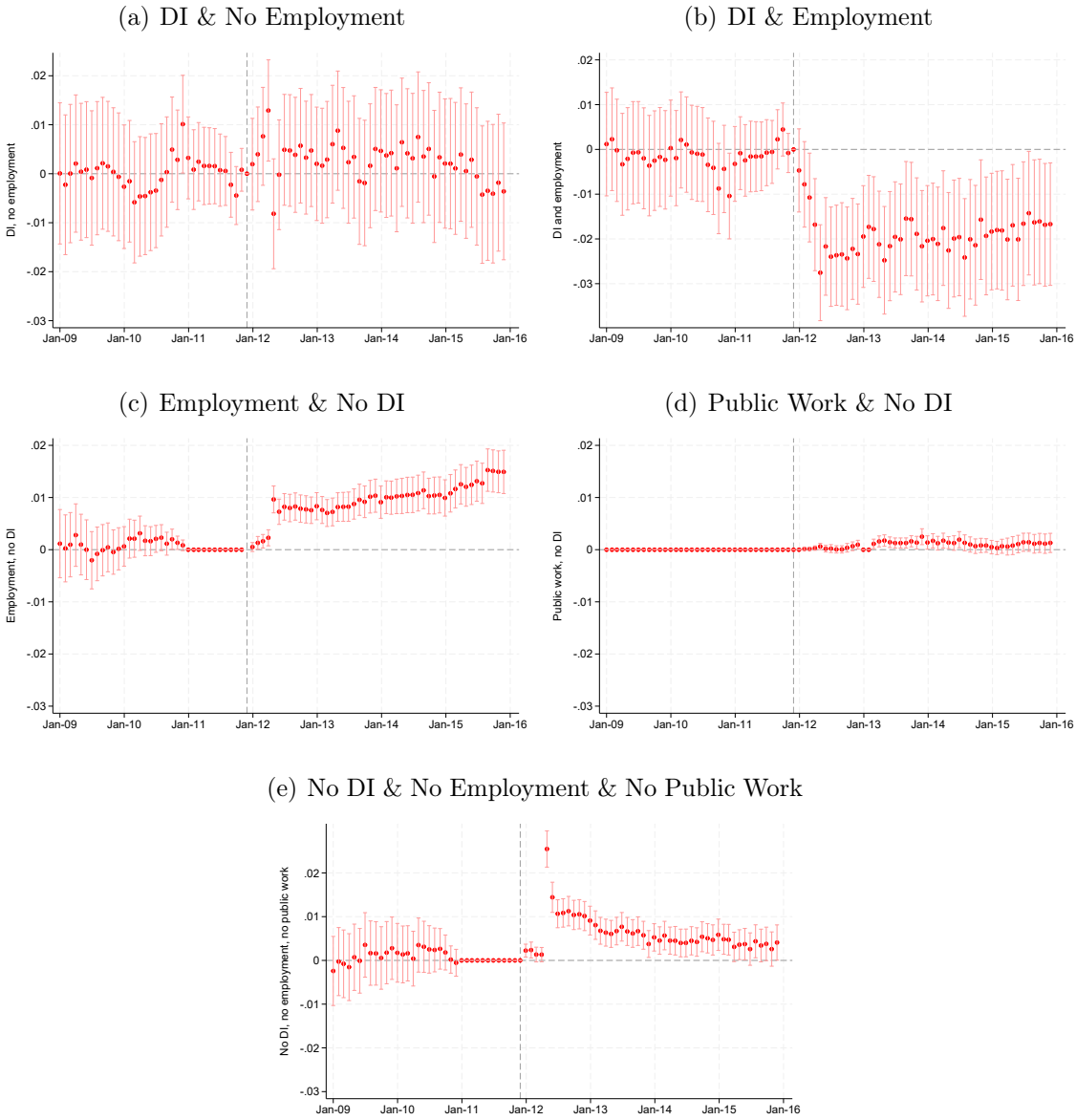
Figures and Tables

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.

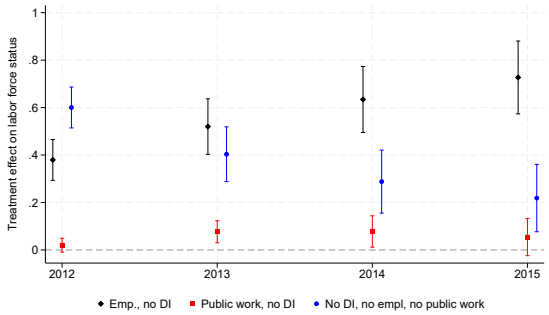
Figure 2: Effect of the Reform Over Time



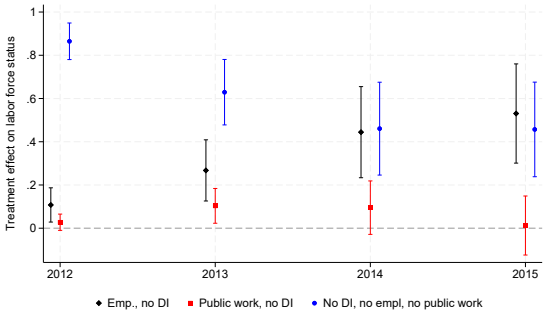
Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers below the age cut-off relative to control workers above the age cut-off. Figure displays the estimated β_t coefficients from equation (2) with 95% confidence intervals over 2009-2015, with December 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

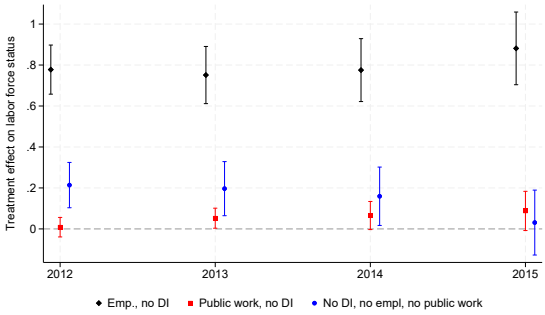
(a) All Individuals



(b) Individuals With No Employment in 2011

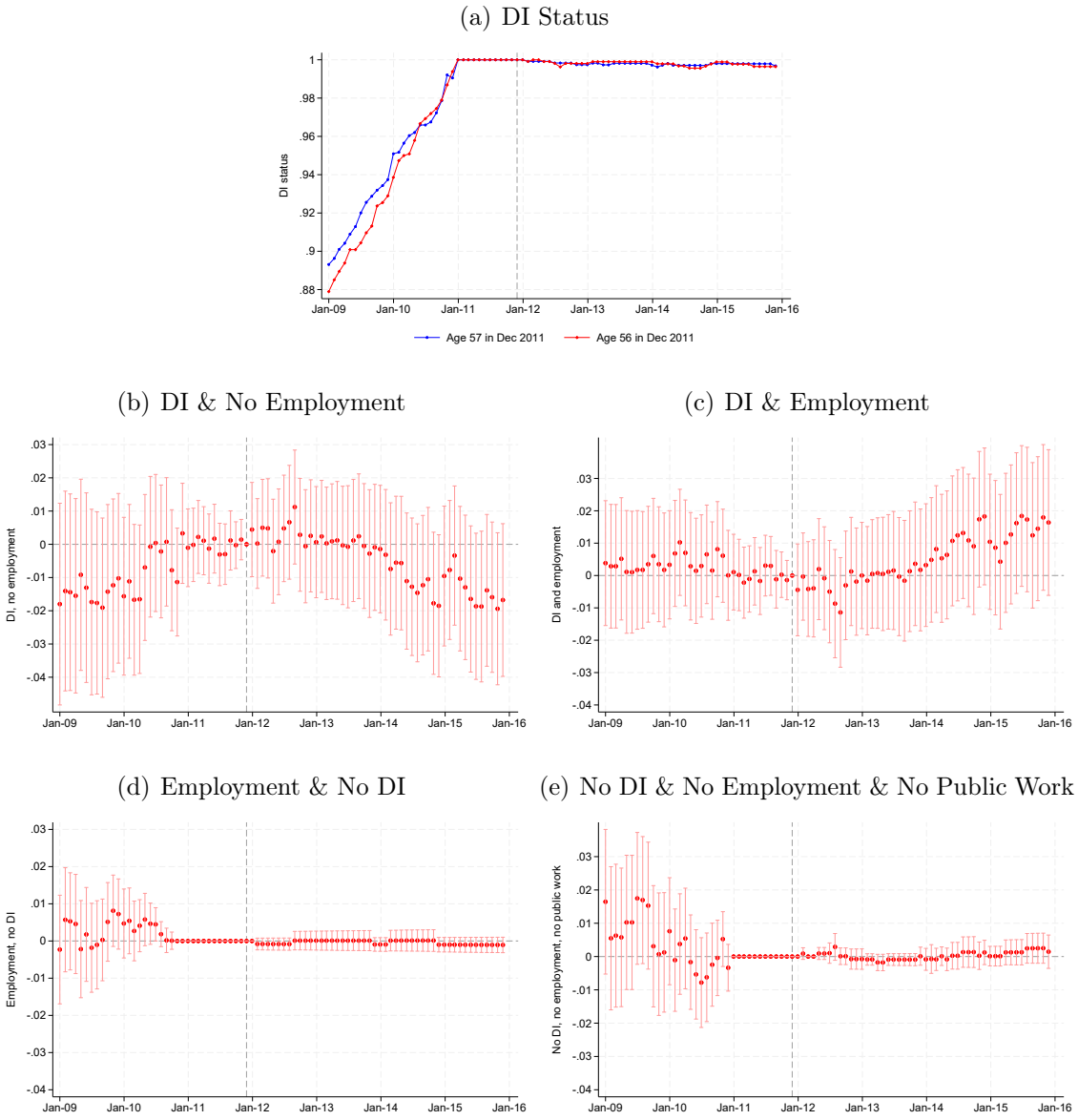


(c) Individuals With Some Employment in 2011



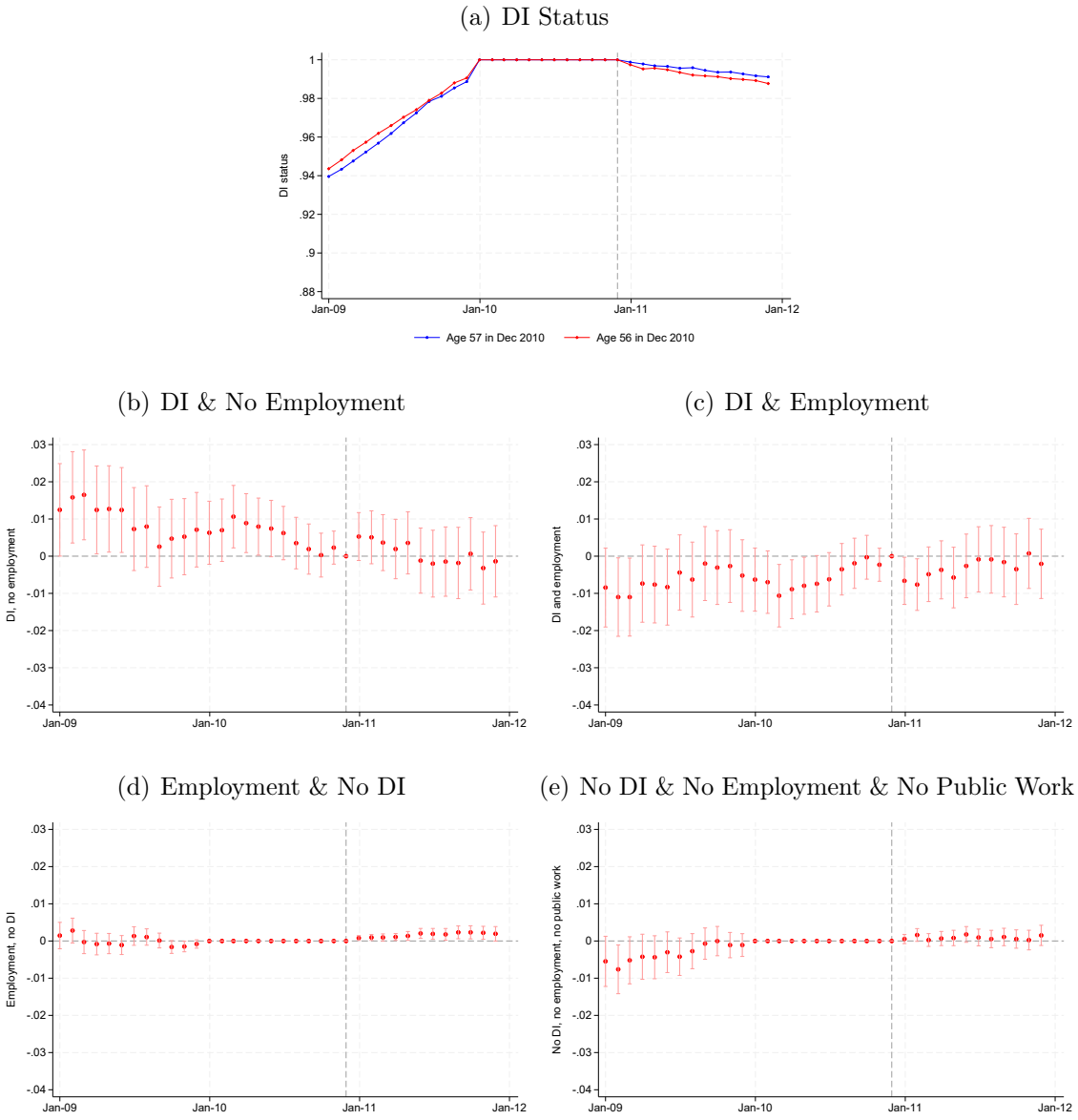
Note: Figure shows our estimates of the impact of losing DI benefits on the outcomes of affected workers. Figure displays the estimated β^{IV} coefficient from equation (4) with 95% confidence intervals estimated separately for each year 2012-2015 and by 2011 employment status. Sample is restricted to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



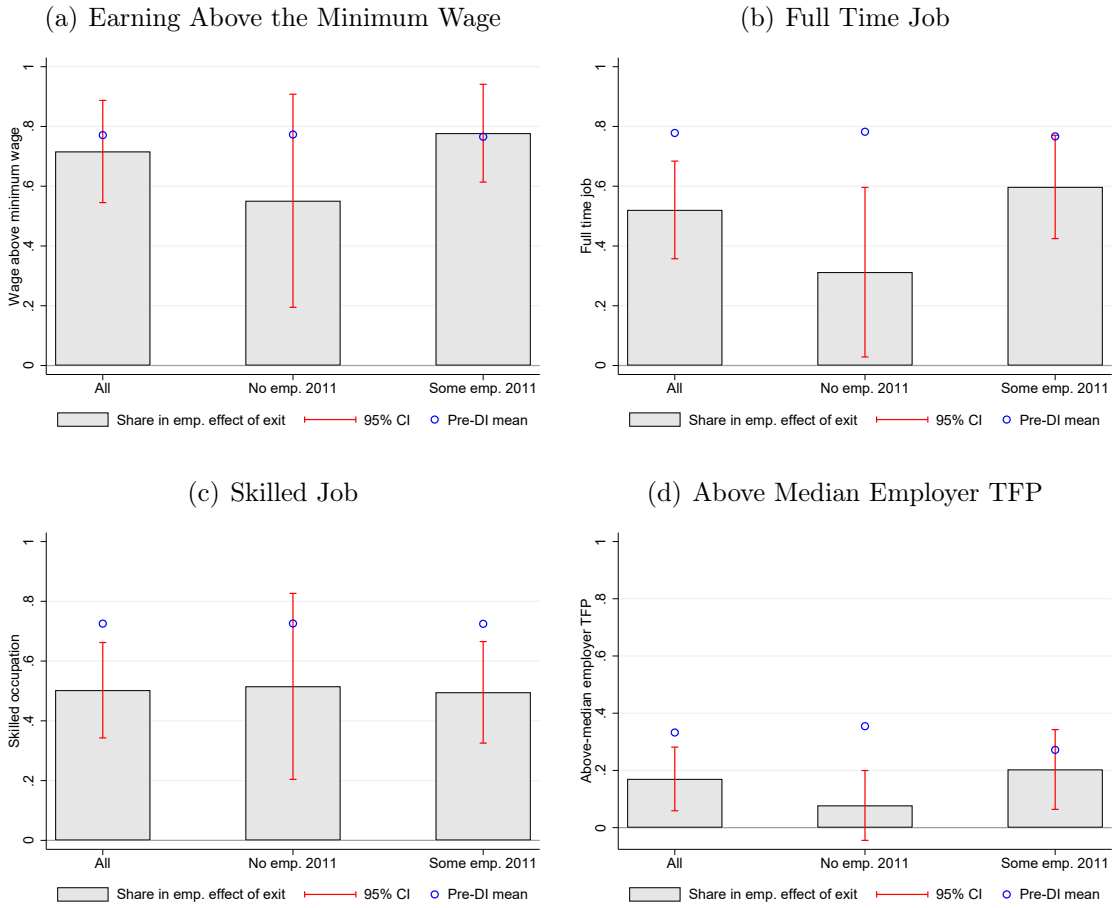
Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers below the age cut-off relative to control workers above the age cut-off for the placebo group of individuals in unaffected DI categories. Panel (a) shows the share of individuals receiving DI 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	0.248	0.242
Mean length of DI status in Dec 2011 (years)	11.1	11.0
Mean drug spending in 2011 (euros)	475	455
Micro-region level unemployment rate in 2011	0.200	0.193
Pre-reform occupation		
Skilled	0.330	0.352
Unskilled	0.183	0.176
Missing	0.487	0.471
Number of individuals	6,611	7,545

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	-0.003	-0.007	0.004***	0.001	0.005**
	(0.005)	(0.005)	(0.001)	(0.0004)	(0.002)
Treated × some emp. in 2011	0.017	-0.048***	0.022***	0.002**	0.007**
	(0.013)	(0.013)	(0.005)	(0.0009)	(0.003)
Observations	1,151,939	1,151,939	1,151,939	1,151,939	1,151,939
Individuals	14,156	14,156	14,156	14,156	14,156

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

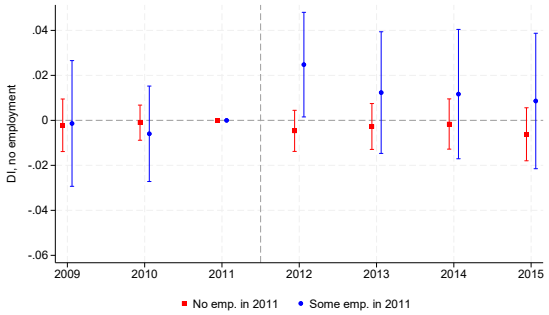
	Employment & no DI	Public work & no DI	No DI & no employment & no public work
	(1)	(2)	(3)
Panel A: All individuals			
Exit	0.567***	0.057***	0.375***
	(0.052)	(0.022)	(0.048)
Observations	642,323	642,323	642,323
Individuals	14,110	14,110	14,110
Panel B: Individuals with no employment in 2011			
Exit	0.319***	0.058	0.623***
	(0.067)	(0.035)	(0.066)
Observations	480,600	480,600	480,600
Individuals	10,644	10,644	10,644
Panel C: Individuals with some employment in 2011			
Exit	0.799***	0.057**	0.143***
	(0.063)	(0.026)	(0.056)
Observations	161,723	161,723	161,723
Individuals	3,466	3,466	3,466

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.

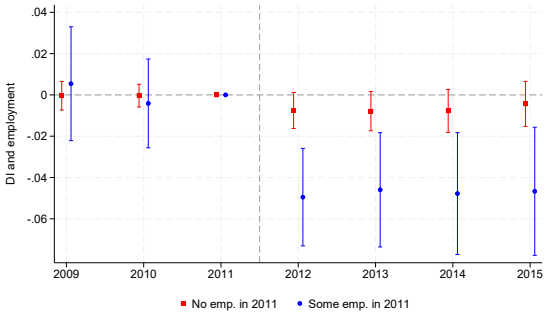
Appendix: Additional Figures and Tables

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

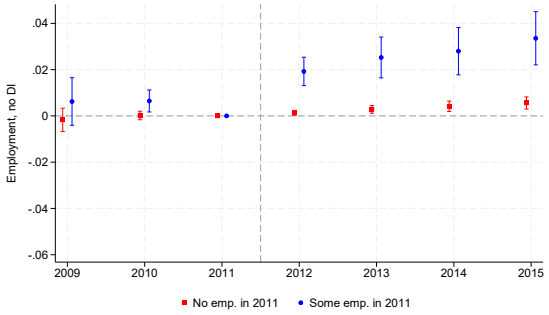
(a) DI & No Employment



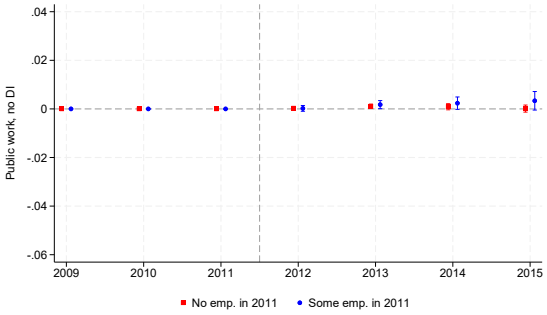
(b) DI & Employment



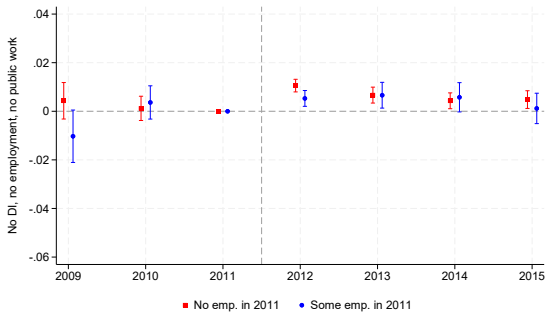
(c) Employment & No DI



(d) Public Work & No DI



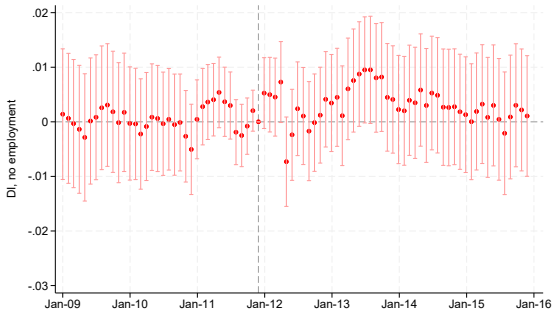
(e) No DI & No Employment & No Public Work



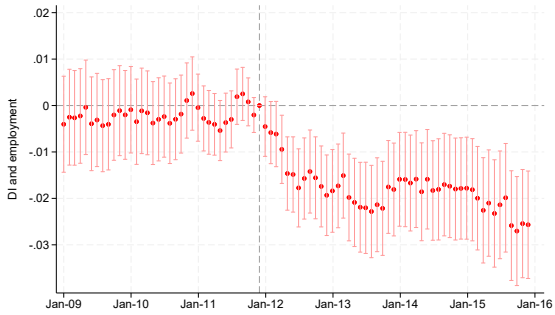
Note: Figure displays the β_t coefficient estimates of a yearly version of equation (2) interacted with employment in 2011, showing the treatment effects over 2009-2015, with 2011 as reference year. 95% confidence intervals are displayed. Sample is restricted to men who received DI throughout 2011, and 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

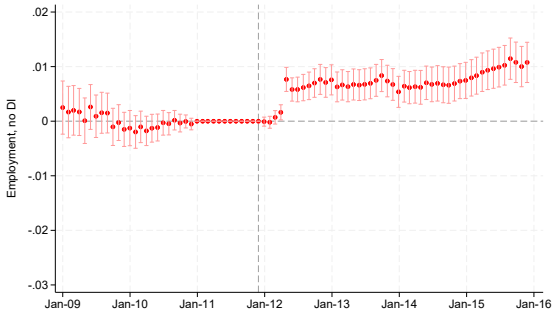
(a) DI & No Employment



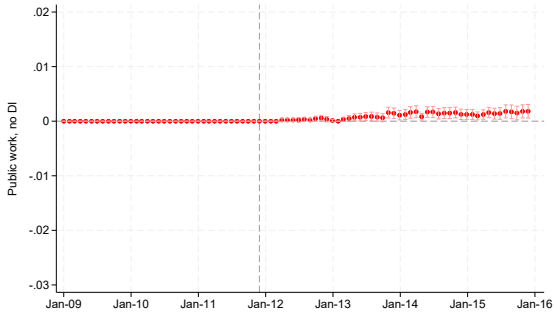
(b) DI & Employment



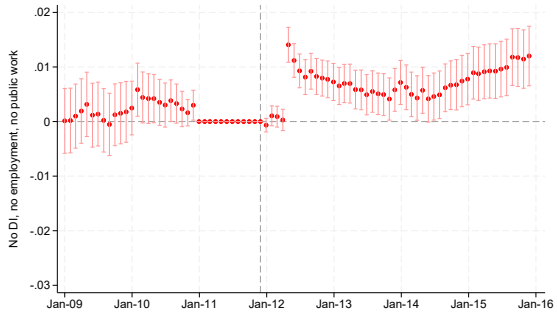
(c) Employment & No DI



(d) Public Work & No DI

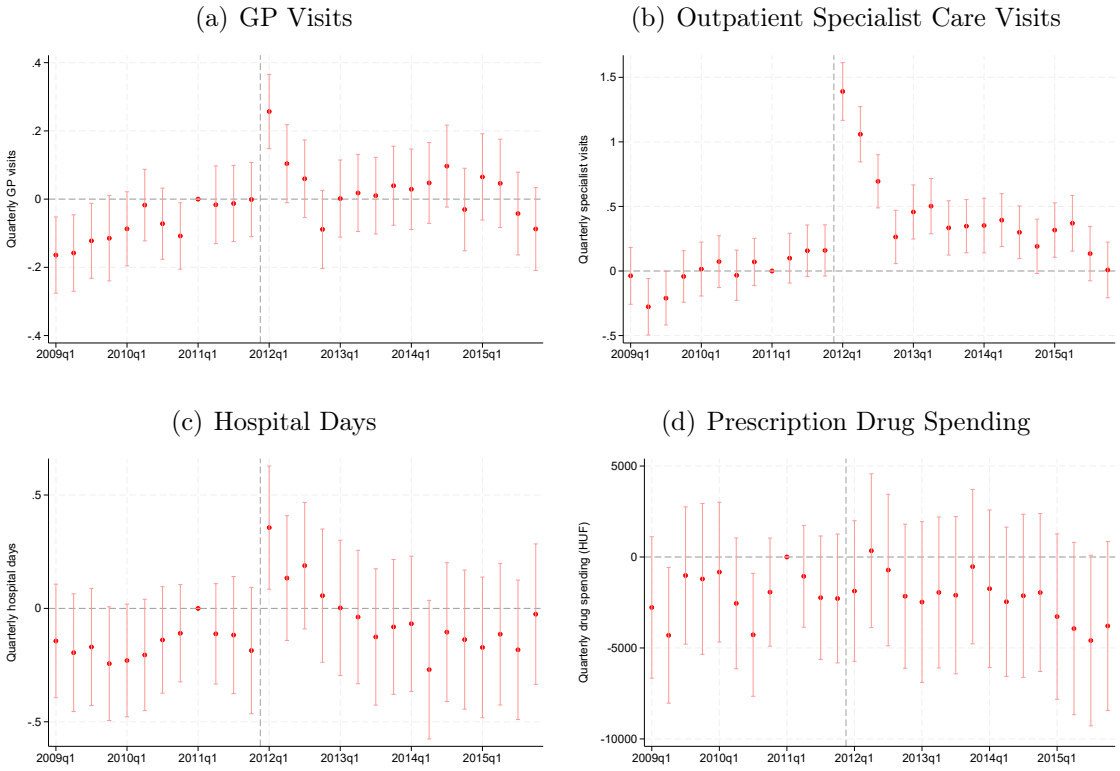


(e) No DI & No Employment & No Public Work



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**	0.010***
	(0.007)	(0.007)	(0.002)	(0.001)	(0.003)
Treated × High drug spending	-0.001	-0.004	0.004*	0.000	0.001
	(0.008)	(0.007)	(0.002)	(0.000)	(0.002)
Panel C: By pre-reform occupation					
Treated × Skilled	0.009	-0.025***	0.013***	0.001	0.002
	(0.009)	(0.009)	(0.004)	(0.0005)	(0.004)
Treated × Unskilled	0.008	-0.030**	0.012***	0.002*	0.008
	(0.013)	(0.012)	(0.004)	(0.001)	(0.005)
Treated × Missing	-0.002	-0.009	0.004***	0.001	0.006**
	(0.007)	(0.007)	(0.002)	(0.001)	(0.002)
Panel D: By length of DI status in Dec 2011					
Treated × Long DI	0.002	-0.018***	0.009***	0.001	0.006***
	(0.007)	(0.007)	(0.002)	(0.001)	(0.002)
Treated × Short DI	0.004	-0.017**	0.007***	0.001**	0.004
	(0.008)	(0.007)	(0.003)	(0.001)	(0.003)
Panel E: By unemployment rate in 2011					
Treated × High unemployment	0.009	-0.022***	0.008***	0.001*	0.004*
	(0.007)	(0.007)	(0.002)	(0.001)	(0.002)
Treated × Low unemployment	-0.003	-0.013*	0.010***	0.001	0.006**
	(0.008)	(0.007)	(0.002)	(0.0004)	(0.003)
Observations	1,151,939	1,151,939	1,151,939	1,151,939	1,151,939
Individuals	14,156	14,156	14,156	14,156	14,156

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 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, 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. 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	0.567***	0.057***	0.375***
	(0.052)	(0.022)	(0.048)
Observations	642,323	642,323	642,323
Individuals	14,110	14,110	14,110
Panel B: By drug spending in 2011			
Exit, low drug spending	0.550***	0.073***	0.378***
	(0.063)	(0.027)	(0.058)
Observations	322,266	322,266	322,266
Individuals	7,060	7,060	7,060
Exit, high drug spending	0.690***	0.018	0.373***
	(0.090)	(0.030)	(0.084)
Observations	320,057	320,057	320,057
Individuals	7,050	7,050	7,050
Panel C: By pre-reform occupation			
Exit, skilled	0.665***	0.036	0.299***
	(0.069)	(0.022)	(0.067)
Observations	221,578	221,578	221,578
Individuals	4,832	4,832	4,832
Exit, unskilled	0.639***	0.084**	0.277***
	(0.103)	(0.042)	(0.094)
Observations	115,450	115,450	115,450
Individuals	2,531	2,531	2,531
Exit, missing occupation	0.396***	0.064	0.540***
	(0.093)	(0.051)	(0.088)
Observations	305,295	305,295	305,295
Individuals	6,747	6,747	6,747
Panel D: By length of DI status in Dec 2011			
Exit, long DI	0.618***	0.041	0.342***
	(0.073)	(0.032)	(0.066)
Observations	325,267	325,267	325,267
Individuals	7,139	7,139	7,139
Exit, short DI	0.516***	0.074**	0.410***
	(0.077)	(0.030)	(0.072)
Observations	317,056	317,056	317,056
Individuals	6,971	6,971	6,971
Panel E: By unemployment rate in 2011			
Exit, high unemployment	0.540***	0.079**	0.381***
	(0.077)	(0.038)	(0.069)
Observations	342,671	342,671	342,671
Individuals	7,493	7,493	7,493
Exit, low unemployment	0.597***	0.034*	0.369***
	(0.071)	(0.020)	(0.068)
Observations	299,652	299,652	299,652
Individuals	6,617	6,617	6,617

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.

Appendix Table A4: Logit Model of DI Benefit Loss

	DI exit	
	Logit coefficient	Average marginal effect
Some employment in 2011	0.967***	0.025***
	(0.152)	(0.004)
High drug spending in 2011	-1.031***	-0.026***
	(0.141)	(0.004)
Pre-reform occupation (ref.: skilled)		
Unskilled	-0.190	-0.005
	(0.174)	(0.004)
Missing	-0.131	-0.003
	(0.174)	(0.005)
Long DI in Dec 2011	-0.192	-0.005
	(0.139)	(0.004)
High unemployment rate in 2011	-0.089	-0.002
	(0.129)	(0.003)
Number of observations	300,213	
Number of individuals	6,588	

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 is 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	0.466***	0.064***	0.470***
	(0.060)	(0.017)	(0.062)
Observations	889,355	889,355	889,355
Individuals	18,928	18,928	18,928
Panel B: Individuals with no employment in 2011			
Exit	0.167***	0.083***	0.750***
	(0.045)	(0.025)	(0.049)
Observations	651,562	651,562	651,562
Individuals	13,914	13,914	13,914
Panel C: Individuals with some employment in 2011			
Exit	0.842***	0.047**	0.112
	(0.157)	(0.023)	(0.163)
Observations	237,793	237,793	237,793
Individuals	5,014	5,014	5,014

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.