The Labor Market Effects of Disability Benefit Loss*

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Abstract

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 while leaving work incentives unchanged. Leveraging birthday and health cutoffs in the reassessment, we estimate employment responses to termination or reduction of benefits driven by income effects. We find that among those who exited disability insurance due to the reform, 60% were employed in the primary labor market, 3% participated in public works and 37% were out of work without benefits in the post-reform period. The consequences of exiting disability insurance sharply differed by pre-reform employment status. 80% of beneficiaries who had some employment in the pre-reform year worked in the primary labor market, compared to only 38% of those without pre-reform employment.

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1 Introduction

The rise in disability benefit 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, strong income effects, 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, benefit cuts, financial incentives or periodic reassessments are unlikely to have much success in reintegrating beneficiaries into the labor market. Moreover, if they include the termination or reduction of benefits, they can harm beneficiary welfare. By contrast, if poorly designed incentives, such as overly strict earnings limits (Krekó, Prinz and Weber, 2023) are the main cause, governments can improve the efficiency of DI programs by correcting these incentives. The impact of supply-side financial incentives however may depend on the broader policy context, and especially on the availability of 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 return to work when their benefits are terminated or reduced. Starting in 2012, Hungarian DI beneficiaries born in 1955 or after (who were under 57 years of age in December 2011) 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) exited DI 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 termination or reduction by leveraging these birthday and health cutoffs in reassessments and focusing on a narrow cohort around the birthday cutoff. As the reform impacted recipients who were allowed to work while also receiving benefits and the associated earnings limits were high and in nearly all cases non-binding, we interpret our estimates of beneficiary responses to benefit termination or reduction as capturing the income effect of benefit receipt. Due to a 2011 policy that made it easier for women to retire early, we focus on men in our main analysis, but show that effects were similar for women.

Comparing beneficiaries born just after and before the birthday cutoff, we find that among affected beneficiaries the probability of disability insurance receipt decreased by 1.3 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 and employment without the concurrent receipt of DI benefits increased by 0.8 percentage point. Roughly one-third of excluded beneficiaries were not employed and the probability of having no income from either DI or employment increased by 0.5 percentage point.

Unlike in the United States but similarly to other European countries, where partial benefits are available, a meaningful share of DI beneficiaries are employed while receiving benefits and post-reform labor market outcomes differ greatly by pre-reform employment status. Individuals who were working in 2011 were more likely to have their benefits terminated as a result of the review. While only a quarter of the reassessed beneficiaries were employed in 2011, half of those who exited DI came from this group. 80% of them were still employed in the primary labor market post-reform, 5% participated in public works, while 15% had no job or benefits. The other half of recipients who exited DI (with no work recorded in 2011) fared worse in the labor market: only 38% were employed post-reform, 1% participated in public works, while 61% had no job or benefits. We also document a deterioration of job quality of former beneficiaries relative to their pre-DI employment. This deterioration is more striking among those who were not employed just before the reassessment reform. Our results suggest that the consequences of DI benefit termination depend crucially on whether a beneficiary is employed while receiving benefits. Those who held jobs while on benefits had a high probability of remaining employed after losing their benefits, while those who did not work were likely to remain out of work and without benefits.

The existing literature on the labor supply effects of disability benefits focuses mainly on full benefits and new or recent claimants. One strand of papers compares the labor supply of accepted and rejected DI applicants (e.g., Bound, 1989; Chen and van der Klaauw, 2008; von Wachter, Song and Manchester, 2011; Maestas, Mullen and Strand, 2013; French and Song, 2014; Autor, Kostøl, Mogstad and Setzler, 2019), while others exploit variation in the generosity of benefits (e.g., Gruber, 2000; Marie and Vall Castello, 2012; Mullen and Staubli, 2016; Kantarci, van Sonsbeek and Zhang, 2023) or change in the eligibility criteria for new claimants (Gruber and Kubik, 1997; Autor and Duggan, 2003; Karlström, Palme and Svensson, 2008; Staubli, 2011; Autor, Duggan, Greenberg and Lyle, 2016). Closer to our approach, a few papers use reforms that involved the reassessment of existing disability benefit recipients to estimate the impact of full or partial withdrawal of their benefits (Borghans, Gielen and Luttmer, 2014; Moore, 2015; Deshpande, 2016*a*; Deuchert and Eugster, 2019;

Garcia-Mandicó, García-Gómez, Gielen and O'Donnell, 2020).[1]

Our paper makes two main contributions. First, our estimates are driven by the income effect of DI, while the substitution effect is arguably negligible. The substitution effect arises in systems where benefits are withdrawn upon return to work, i.e., returning to work means sacrificing benefits (Autor and Duggan, 2007). The income effect arises in all welfare programs: cash benefits allow participants to spend less time working and increase leisure. The income effect is difficult to isolate when the substitution effect is also at play (see Gelber, Moore and Strand, 2017 and Deuchert and Eugster, 2019 for recent attempts), but it is possible in our case as in our context DI benefits can be combined with work, and earnings limits are not binding for most recipients.

Measuring the income effect is important as it may help explain the poor outcomes of policy reforms that focus only on eliminating the substitution effect by lowering the implicit tax on returning to work. The few existing papers that estimate pure income effects indeed find that these are substantial, however, existing estimates are based on expansions of DI eligibility or generosity (see Autor and Duggan, 2007 and Autor, Duggan, Greenberg and Lyle, 2016 for disabled veterans of military service, and Marie and Vall Castello, 2012 for low-skilled DI recipients near retirement age). We contribute to this literature by examining income effects in the reverse scenario, i.e., when benefit entitlements or levels are reduced. This is potentially important as the labor supply response to benefit changes is not necessarily symmetrical, and recent policy initiatives sought to tighten access to benefits rather than expand it. Our results highlight that despite the loss of DI benefits, which imply incentives for employment through the income effect, many affected individuals could not find employment and were left without any income. Furthermore, our results also suggest that even if formally the DI reassessment does not create any substitution effects, it is possible that individuals who fear that their employment status could increase the likelihood of benefit loss at reassessment may quit employment preemptively, leading to adverse employment effects.

Second, we contribute to exploring heterogeneity in the labor supply effects of DI. The modest effects we find for men aged 55 to 60 are smaller than most earlier estimates that focused on younger age groups. This is in line with earlier literature finding that labor supply effects tend to be smaller for older age groups (von Wachter, Song and Manchester, 2011; Moore, 2015; Kantarci, van Sonsbeek and Zhang, 2023). Using rich administrative data we also show that the labor supply response varies by labor market status preceding the termination or reduction of DI benefits. Those who had been out of work and had been receiving DI for over 12 months were unlikely to return to work even when facing a significant

[1] See Appendix Table A1 for a detailed overview of the related literature.

drop in income. This may be partly due to variation in ability to work or work opportunities at the time of accessing the benefit, where those with better initial conditions were more likely to work during benefit receipt and more protected from losing work ability. In part, it may have also been caused by preferences for leisure or distance from the labor market increasing with benefit duration among those out of work. We find that heterogeneity in the effects of DI benefit termination or reduction is weaker by pre-reform health than by pre-reform employment status, which suggests that labor market attachment is the key driver of reintegration after benefit loss.

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 even 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 development. 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 its social costs and its actual effectiveness in reactivating long-time beneficiaries.

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.[2] The obligation applied to all DI recipients born in 1955 or after (who were under 57 years of age in December 2011) with a partial disability, whose health impairment was below 80%, as determined by the pre-reform assessment system (Table 1). Two partial 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).[3]

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 had their benefit entitlement terminated by May 2012. Otherwise, their health status and degree of employability were reevaluated 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.

[2]Before the 2012 reform, the frequency of health reassessments was irregular, prescribed on an individual basis during the initial assessment and depended mainly on the likelihood of recovery. Most beneficiaries were never reassessed after initial benefit receipt.

[3]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.

Throughout the entire period covered by our analysis, DI benefit levels were determined on the basis of prior earnings and the severity of health damage. People with partial health damage were eligible for DI benefits, but lower health damage implied lower benefits, ceteris paribus. As we report in the table of descriptive statistics (Table 3), the average amount of monthly DI benefits of beneficiaries aged 56-57 was around $300 in 2011—for comparison, the monthly minimum wage was $388 in 2011.

About 18,000 beneficiaries (9% of the reassessed beneficiaries and 5% of all beneficiaries) who underwent the review permanently exited DI. 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.[4]

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. 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 Dutch reform of 2004 involved the reassessment of the majority of DI recipients aged below 44 applying more stringent criteria. Garcia-Mandicó, García-Gómez, Gielen and O'Donnell (2020) estimate that the reassessment made 17 percent of beneficiaries exit 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 by a handful of small NGOs, operating mainly in urban centers (Krekó and

[4] 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.

Scharle, 2020).[5]

2.3 Employment While Receiving DI Benefit

In Hungary, employment is allowed while receiving partial DI benefits, with some restrictions on the maximum possible earnings, above which benefits are terminated. These earnings restrictions varied across benefit types and years during the observation period (see Table 2 for more detail). In the pre-reform period, DI recipients covered by our study faced two different earnings limits. First, for Category III Disability Pensioners (75% of our analysis sample of DI beneficiaries aged 56 or 57 at the end of 2011), the earnings limit was set on an individual basis and was equal to 90% of the pre-disability wage in 2009, and then set at 200% of the benefit in 2010-2011. Second, the recipients of the Regular Social Assistance were allowed to accumulate earnings up to 80% of their previous earnings if they entered the benefit before December 31, 2007.[6,7] This group constitutes 25% of our analysis sample of DI beneficiaries aged 56 or 57 at the end of 2011.

These earnings limits were not binding for most beneficiaries in our sample, and thus were unlikely to affect the decision to work. Figure 1 displays earnings relative to the individual-specific limit before the reform in 2011 for the entire sample of 56 and 57 year old beneficiaries (Panel (a)) and for those who exited DI in the post-reform period between 2012 and 2015 (Panel (b)). Both panels confirm that only a small share of beneficiaries earned just below or even close to the limit: 94% of all beneficiaries (92% of those who exited DI) earned below 90% of the limit before the reform, while 70% (66% of those who exited DI) earned less than half the limit.[8] After the 2012 reform, the earnings limit was set to 150% of the monthly minimum wage for those who underwent DI revision and found to be eligible to Disability Allowance.[9] Panel (c) of Figure 1 indicates that this earnings limit was also not

[5]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).

[6]Pre-disability earnings is defined as the average of valorized earnings from the entire pre-disability period. Due to sample limitations, we use earnings from the 12 months preceding the benefit entry to estimate pre-disability earnings.

[7]Regular Social Assistance beneficiaries who took up benefits after January 1, 2008 faced a more stringent earnings limit (Krekó, Prinz and Weber, 2023), and so we exclude this group, 4% of beneficiaries aged 56 or 57 at the end of 2011, from the sample in order to focus on beneficiaries without binding earnings limits.

[8]Some observations appear above the limit because it was possible to earn more than the limit in some months before the authorities would have terminated benefits.

[9]Until the date of the review the pre-reform limits remained in place. Those who were found to be eligible to Rehabilitation Allowance were allowed to work maximum 20 hours per week in 2014 and 2015, however, only 1 percent of the revised population aged 56 in 2011 (who are in the focus of our analysis) received

binding: 93% of beneficiaries earned below 90% of the limit, while 60% earned less than half of the limit. Overall, given the high individual-specific earnings limits which do not appear to have been binding, the impacts of the 2012 DI reform were likely driven by income effects, without sizable substitution effects.

3 Data

Our analysis is based on an individual-level linked employer-employee administrative panel database, covering a randomly selected half of the population of Hungary in 2003, who are then followed up until 2017. The database consists of linked datasets at the monthly frequency of the pension, tax and health care authorities and contains detailed individual-level information on employment and earnings 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 A2), the majority of DI recipients suffer from long-lasting diseases. 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 receiving DI in a given month and zero otherwise.[10] The binary variable for employment status equals one if the individual is employed on the 15^{th} of the given month and zero otherwise. Our employment definition includes paid and self-employment as well as participation in public works, unless stated otherwise.[11] Since in Hungary many DI recipients

Rehabilitation Allowance after the revision over 2012-2015, and 6% of those revised individuals aged 56 in 2011 who experienced a DI benefit termination or reduction (making up 5.2% of the revised population) received Rehabilitation Allowance after the revision over 2012-2015.

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

[11] 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). 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

also work, in addition to analyzing the impact of the reform on overall employment and DI receipt, we examine possible combinations of the two outcomes. We generate four mutually exclusive and exhaustive binary outcome variables: (1) DI benefit receipt without concurrent employment (DI & no employment); (2) DI benefit receipt with concurrent employment (DI & employment); (3) employment without concurrent DI receipt (employment & no DI); and (4) no income from either DI benefits or employment (no DI & no employment).

We consider four quarterly indicators of healthcare use: primary care provider visits, outpatient specialist care visits, hospital days, and spending on prescription drugs.[12] Indicators of healthcare use are included in our data from 2009. In addition, 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 for hours worked. We define a binary indicator of full-time work, which equals one if the weekly hours of work exceed 39. We generate a binary indicator of working in a skilled job, which includes all occupations except for elementary occupations (defined as code 9 in ISCO, the International Standard Classification of Occupations). 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, and define a binary indicator of above-median employer TFP.[13]

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%, born in 1955 or after (i.e., aged under 57 in December 2011). A limitation of our data is that we do not have information on the reassessment procedure itself; we observe the loss of benefits, but not its cause. Consequently, it is not possible to isolate those who exited DI as a result of the reform from those who would have exited even in the absence of the reform. For this reason, to identify the impact of the reform, we focus on a narrow group around the birthday cutoff of the policy, assuming that outcomes of individuals in this

statutory minimum wage.

[12]Health insurance coverage is universal and there are no charges for any inpatient or outpatient services. There is cost sharing for prescription drugs and we consider total spending, including the components paid out-of-pocket and by social security.

[13]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).

narrow group born before and after the cutoff 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 cutoff) in December 2011 make up the treatment group, while those who were 57 (just above the cutoff) make up the control group. In other words, individuals in the treatment group were born in 1955, individuals in the control group were born in 1954. Further, we restrict the sample to individuals claiming DI throughout 2011 who were alive in January 2012. Those who died after January 2012 are included in the sample until the last year they were alive. We provide evidence that the DI reform did not have an effect on mortality over our observation period. These restrictions produce a sample composed of 57% women and 43% men. In our baseline estimation, we focus on men and consider the post-reform period up to 2015, the year when the control group reaches the statutory retirement age.[14] Our focus on men is motivated by the "Women 40" policy which since 2011 gives an early retirement option to women with 40 years of work credits, regardless of age. This policy could affect the control and treatment age groups differently, potentially confounding our results for women. However, in a robustness analysis we find similar effects for women.

Figure 2 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 do not 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 evolved similarly. Following the reform, the control and treatment groups diverge: over the next four years, 2% of the control group but 4% of the treatment group is removed from benefits. The bulk of the divergence occurs in May 2012, which suggests that, although the review process lasted until 2016, many beneficiaries were affected early on.[15]

[14] 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.

[15] 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.

4.2 Difference-in-Differences

To study the "reduced form" impact of the reassessment on the 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 order for our estimates to capture 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 evolved similarly. We present several pieces of evidence consistent with this assumption. First, Table 3 shows that the control and treatment groups are similar on various measures of health and employment. Second, Figure 2 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 3 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.[16]

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. We define two binary indicators of benefit loss. The first one is "DI exit", which indicates if an individual stops receiving DI benefits and zero otherwise. The second one is "DI exit or DI benefit cut", which indicates if an individual stops receiving DI benefits or experiences an at least 10% reduction in their inflation-adjusted annual DI benefits, relative to the DI benefit income in 2011.

Using the DI exit indicator, 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 DI exit, 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 DI exit probability and the μ_t are month fixed effects. Our coefficient of interest is β^{IV}, which captures the impact of DI exit on labor market outcomes after the reassessment among individuals who lost their benefits due to the reform. We decompose the effect of benefit loss into three mutually exclusive outcomes: (1) employment (not in the public works program) (2) employment in the public works program (3) no employment. Each outcome equals one if the given labor market status is observed *and* an individual exits from the DI program. This specification ensures that the sum of the estimated β^{IV} parameters for the three outcomes equals one, and each β^{IV} parameter captures the fraction of individuals in each employment category among those who exited DI due to

[16] Alternatively, a regression discontinuity design (RDD) could be applied, comparing individuals close to the birthday cutoff (just below and above age 57 in December 2011). In Appendix Figure A1 we plot the employment and DI outcomes averaged over 2012-2015 by age (in monthly intervals) in December 2011. While the fitted regression lines indicate changes in the outcomes that are in line with our main results, RDD estimation results are noisy and sensitive to specification choices, including bandwidth choice and the functional form of the local regression.

the reform.

Similarly, using the DI exit or DI benefit cut indicator, the first-stage equation is

$$exit\_cut_{it} = \tilde{\gamma}\mathbb{1}[AGE_i = 56] + \tilde{\mu}_t + \tilde{\varepsilon}_{it} \tag{5}$$

where $exit\_cut_{it}$ is a binary indicator for not receiving DI benefits or receiving at least 10% lower DI benefits than in 2011. We then estimate the second-stage equation:

$$Y_{it} = \tilde{\beta}^{IV}\widehat{exit\_cut}_{it} + \tilde{\mu}_t + \tilde{\nu}_{it} \tag{6}$$

where $\widehat{exit\_cut}_{it}$ denotes the predicted probability of DI exit or DI benefit cut and the $\tilde{\mu}_t$ are month fixed effects. The $\tilde{\beta}^{IV}$ coefficient captures the impact of DI exit or DI benefit cut on labor market outcomes after the reassessment among individuals who lost at least 10% of their benefits due to the reform. We decompose the effect of DI exit or DI benefit cut to six mutually exclusive outcomes (splitting the three outcomes used in equation (4) by DI status): (1) DI & employment & no public work; (2) no DI & employment & no public work; (3) DI & public work; (4) no DI & public work; (5) DI & no employment & no public work; (6) no DI & no employment & no public work. Note that when decomposing the effect of DI exit only, the outcomes with DI status were by definition not relevant. Again, this specification ensures that the sum of the estimated $\tilde{\beta}^{IV}$ parameters for the six outcomes equals one, and each $\tilde{\beta}^{IV}$ parameter captures the fraction of individuals in a given employment category among those who lost their DI status or experienced a DI benefit cut due to the reform.

In addition to the identifying assumptions described above, the two standard IV assumptions of relevance and exogeneity need to be satisfied for our IV estimates to represent the causal impact of benefit loss on labor market outcomes. Figure 2, the first column of Table 4, and the second column of Table 7 show the relevance of the instrument. Table 4 suggests that over the four years after the reform, beneficiaries born after the birthday cutoff had 1.3 percentage point higher probability of DI exit. Table 7 indicates that over the four years after the reform, beneficiaries born after the birthday cutoff had on average \$4.9 (1.5%) lower monthly DI benefit income. The exogeneity assumption requires that being subject to reassessment affects labor market outcomes only through the DI exit or DI benefit cut channel. This assumption cannot be directly tested. Our placebo results provide suggestive evidence that being under the same age cutoff at the time of the placebo reform 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., with being subject to reassessment), then our estimates could be biased. Table 3 indicates that the treatment and the control groups are similar in terms of major observable characteristics, including pre-reform health damage and drug spending. Our assumption is that the two analyzed cohorts are similar in all aspects, apart from being subject to reassessment. A final concern is that the reassessment process itself could have impacted labor market outcomes independent of benefit loss, for example by causing stress or uncertainty about future benefit receipt. This is difficult to completely rule out, though we do provide some evidence in Figure 4 that our results are driven by individuals who exit DI, and in particular by those whose benefit termination is permanent.

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 loss on labor market outcomes. Figure 3 shows the month-by-month difference between control and treatment individuals for each of the labor market outcomes from estimating equation (2). For comparison, Figure 4 shows with black circles and confidence intervals the results from equation (2) when the treated sample is restricted to those who exited DI any time between January 2012 - December 2015, and with blue crosses and confidence intervals the results when the treated sample is restricted to those who exited DI between January 2012 - December 2012 and did not return to DI by December 2015 ("permanent exit"). These are two non-random sub-samples, as the treated sample was selected based on DI exit, thus the results shown in Figure 4 are not causal. Nevertheless, they help to illustrate the labor market and DI status of those who likely exited DI due to the re-assessment – the group of DI beneficiaries most affected by the reform.

Figure 3 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 have their benefits terminated from May. We do not observe if someone exited DI as a result of health reassessment or due to failing to declare their intention to undergo reassessment, nevertheless, Panel (a) of Figure 4 indicates that roughly 20% of those exiting in 2012-2015 exited in May 2012, suggesting that they likely belonged to the second category. While we cannot distinguish in the data the two exit types, both are consequences of the DI reassessment reform, therefore, in the following, we consider the two jointly when interpreting the results. Panel A of Table 4 reports the effect of the reform on labor market outcomes averaged over the post-reform period from estimating equation (1). The sum of the point estimates in columns (3)-(6) is zero, reflecting the mutually exclusive and exhaustive nature of the four outcome variables. Similarly, Panel A of Table 5 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 5.

Panels (a) and (b) of Figures 3 and 4 show the change in DI status and in employment status over time, the average effects are reported in columns (1) and (2) of Table 4, reflecting that as a consequence of the reassessment, DI receipt decreased by 1.3 percentage points, and the employment rate decreased by a statistically weak 0.9 percentage point at the same time. The decline in the employment rate was temporary and began immediately after the announcement of the reform, i.e., months earlier than the start of the health reassessments in May 2012.

When we restrict the treated sample to those who exited DI any time between January 2012 and December 2015 (black circles and confidence intervals in Figure 4), we see that compared to the control group, the employment rate increased by 20 percentage points by the end of 2015, and the rate of employment without receiving DI increases even more, by up to 30 percentage points in this sub-sample of the treated individuals. The increase in employment rate is nearly twice as large among those who exited DI in 2012 and did not return to DI by the end of 2015 (blue crosses and confidence intervals on Figure 4). These results indicate that the (temporary) decline in employment as a results of the reassessment reform was not driven by those who lost their DI status.

We see two main possible explanations for the early decline in the employment rate among those who remained on DI. First, beneficiaries who expected a benefit loss may have tried to find a new job with a higher salary and therefore quit their existing jobs. Second, beneficiaries who wanted to stay on DI and suspected that being employed would reduce their chances of a favorable reassessment outcome may have terminated their employment pre-emptively.[17] Looking at the group of treated beneficiaries who left employment in the first five months of 2012, we see that the overwhelming majority of them were still receiving DI in May 2012 (indicating that they had signed up for a health review) and that the majority (59%) of them did not return to work in later years. This suggests that the second

[17] Deshpande (2016*a*) mentions a similar channel: "SSI may also have implicit incentive effects if recipients believe that human capital investment or work activity increases the likelihood of removal during medical reviews."

interpretation of the temporary fall in employment is the more plausible one.

Panels (c) to (f) of Figures 3 and 4 break the overall effects down into mutually exclusive categories of DI and employment status. Focusing on the full sample, Panel (c) of Figure 3 and column (3) of panel A of Table 4 show that there is little change in the number of individuals who receive DI benefits while not working. Panel (d) of Figure 3 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 decreased to around 1.5 percentage points by the end of 2015. This suggests that most benefit terminations happened early on. Pooling over the post-reform period, column (4) of panel A of Table 4 shows that there was a 1.7 percentage point decline in the probability of receiving benefits and working at the same time.

Panel (e) of Figure 3 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 (5) of panel A of Table 4 shows that pooling over the post-reform years there was a 0.8 percentage point increase in employment without benefits. This suggests that approximately 60% of those exiting DI end up working. The year-by-year instrumental variables estimates displayed in Panel (a) of Figure 5 suggest that among individuals who exit the DI program due to the reassessment, the share of those employed not as public workers and 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 60% 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 5.

Panel (a) of Figure 5 shows that the impact of benefit termination on employment in public works is especially pronounced in 2013. Averaging over the post-reform years, Column (2) of panel A of Table 5 shows that according to our instrumental variables estimates 3% of individuals who exit DI due to the reassessment end up in the public works program during the years after the reform.

Panel (f) of Figure 3 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 benefit termination, 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 (6) of Panel A of Table 4 shows that the overall increase in the probability of having no income from DI, employment, or public works increased by 0.5 percentage point or about one-third of those exiting DI. Year-by-year instrumental variables estimates show a decline in the impact of DI exit on the share of those without income from employment, public works or benefit from 60% in 2012 to about 20% in 2015 (Figure 5, Panel (a)), with a post-reassessment average of 36.9% as displayed in column (3) of panel A of Table 5. These results are in line with Panel (f) of Figure 4, showing that about a quarter of those who exited DI in 2012 and did not return to DI later were left without employment in 2015.

Overall, our results suggest that, relative to unaffected DI recipients born just before the birthday cutoff for reassessment, affected beneficiaries born just after the birthday cutoff exited DI at substantially higher rates. Outcomes varied significantly among the exiting individuals: about 60% were employed in the primary labor market following exit, while 37% 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 6 and 7 present two sets of placebo results. Figure 6 replicates our main results presented in Figure 3 for DI categories that were not affected by the reassessment policy. Figure 7 replicates the same results but for a placebo reform in 2011.

Figure 6 shows 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.

The placebo results presented in Figure 7 indicate that for a placebo reform in 2011, there were no major pre-reform differences between the placebo treatment and control groups. Panel (e) 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 3. 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.

Effect of DI exit or benefit cut. The findings presented so far have focused on the consequences of exiting DI due to the reassessment. Next, we extend the analysis to also include benefit reductions. As reported in Appendix Table A3, over 2012-2016, 2.3% of the individuals in the treated group exited DI, while 2.9% experienced at least a 10% reduction and 1.8% experienced at least a 25% reduction in their inflation-adjusted benefit compared to 2011 without exiting DI. Individual-specific determinants of DI exit and benefit cut are shown by the results of a multinomial logit estimation in the same table. Employment in 2011 and better health status (captured by the magnitude of health damage and drug spending in 2011) are key determinants that increase the probability of DI exit. In contrast, the probability of a benefit cut increases with more severe health damage, a skilled occupation and shorter time spent on DI.[18]

We report in Table 6 the estimation results of equation (6), showing the IV estimates for the impact of DI exit or DI benefit cut happening due to the reassessment reform. Panel A of the table shows that according to our instrumental variables estimates 35% of individuals who exit DI or suffer a DI benefit reduction end up employed (excluding public work) without receiving DI benefits (column (2)), and a similar fraction end up without employment or public work but continuing receiving DI benefits (column (5)). At the same time, 22% end up without benefits or employment (column (6)). Note that the estimates reported in columns (2), (4), and (6) of Table 6 are different from the results reported in Table 5 because in Table 6 we report the average effect of *DI exit or DI benefit cut*, within which the share of DI exit (the instrumented variable in Table 5) is 44%, based on the total sample of treated individuals.

5.2 Heterogeneous Effects

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 equations (4) and (6) separately on the previously-employed and non-employed samples. Panel B of Table 4 reports the effect of the reform on labor market outcomes by pre-reform employment averaged over the post-reform period from estimating

[18]Occupation information is based on the last observed pre-reform occupation of an individual. For close to half (49%) of our sample occupation information is missing due to individuals for whom no employment history is observed. DI length is measured as the time between the individual's first DI entry and December 2011.

the modified equation (1). Appendix Figure A2 shows year-by-year estimates from estimating the modified equation (2). Panels B to E of Tables 5 and 6 display our instrumental variables estimates by pre-reform employment status.

The results reported in Panel B of Table 4 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 decreased by 2.5 percentage points (column 1), DI receipt while employed decreased by 4.7 percentage points (column 4), employment without receiving benefits increased by 2 percentage points (column 5), while the probability of remaining without income from work or benefits (column 7) increased only by 0.5 percentage point. Within this group, there was also a statistically insignificant 2.2 percentage points increase in the probability of receiving benefits without employment (column 3). In Panel C of Table 4, we further split the subsample of DI recipients with some employment in 2011 by the months of employment (1-11 vs 12 months). These results indicate the strongest negative effect of the DI reform on DI status and employment for those who were employed for 12 months in 2011.

Among the group of beneficiaries not working in 2011, the patterns are different: DI receipt decreased by 0.8 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 5 show that among individuals who exit DI 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, 37.9% end up working after benefit termination, while 60.9% are not working but also not receiving benefits. At the same time, as Panel C shows among those with some pre-reform employment 80.1% are working and only 15.0% end up with no employment or benefits.[19] Less than 5% of both groups end up in the public works program. Panels D and E of Table 5 show that while among those who were employed for 12 months in 2011 and later exited DI only 4% were left without employment or benefits, among those who were employed for 1-11 months in 2011, a much higher fraction, 31.9% were left without employment or benefits, suggesting that their labor market attachment was not strong.

Similarly, the IV estimates for the effect of DI exit or DI benefit cut also reveal important heterogeneities by pre-reform employment. As columns (5)-(6) of Panels B-E of Table 6 indicate, among those who exited DI or experienced at least a 10% DI benefit cut due to the DI reform, the probability of ending up without employment or benefits in 2012-2015 was

[19] If we split the 0.801 (s.e. 0.072) effect on employment & no DI by employment at the same firm as last observed in 2011 then the effect on employment at same firm & no DI is 0.591 (s.e. 0.086), and the effect on employment at different firm & no DI is 0.209 (s.e. 0.068), thus the majority continue working for their pre-reform employer.

3.6% and statistically insignificant if the affected individual was employed for 12 months in 2011. Similarly, the average probability of ending up without employment but remaining on DI in this group was 2.9% and statistically insignificant. On the other hand, the probability of ending up without employment but continuing receiving benefits was 46.1%, and the probability of ending up without employment or benefits was 27.5% among those who had no employment in 2011.

To gain a deeper understanding of this result, we compare affected beneficiaries who were employed in 2011 with those who were not, along observable characteristics. Table 3 reveals that beneficiaries not working in 2011 exhibit a higher probability to have a health damage above 50% and had higher drug spending in 2011. No differences were found based on the length of DI status or micro-region unemployment rate. This suggests that the level of health damage might influence pre-reform employment status, while the economic environment and benefit duration by themselves do not seem to affect employment while receiving benefits.

To further analyze the role of health status and other attributes in labor market outcomes after the reform, 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 A5 and Appendix Table A6 show these results. In both tables, we replicate our baseline results in Panel A.

In both tables, Panel B presents the results for health damage less than versus at least 50% in December 2011 (based on the category of DI benefit), and Panel C presents the results for individuals with low versus high pre-reform spending on prescription drugs, a proxy for health. Here we categorize individuals with annual spending above the 2011 sample median as high spending. Appendix Table A5 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, which is also confirmed by the results reported in the first two columns of Appendix Table A3. At the same time, the instrumental variables estimates in Appendix Table A6 suggest that the impact of DI exit on outcomes was rather similar among healthier and less healthy individuals, with employment increasing more among individuals with lower health damage but with higher drug spending. The fact that health-related heterogeneities are weaker than the heterogeneity by pre-reform employment status indicates that better employment outcomes after DI exit among those who worked before the reform relative to those who did not work are not solely driven by differences in health.

Panel D of both tables shows results by occupation groups (skilled, unskilled, or missing) based on the last observed pre-reform occupation of the individual. The results for skilled and unskilled workers are fairly similar, although the impact of DI exit on employment probability is smaller among those for whom we do not observe employment history.

Panel E of both tables displays results by the length of time spent on DI before the reform. 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 F 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 confirm that pre-reform employment status and health were the two major determinants of benefit termination. Once benefit was terminated, 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 benefits. Despite the former group exhibiting lower health damage and drug consumption on average, the observation that heterogeneity is less pronounced based on pre-reform health compared to pre-reform employment suggests that while both health status and labor market attachment matter for how individuals navigate the labor market after benefit loss, prior employment is the key driver of labor market success.

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 exit 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 without concurrent DI receipt; (2) full-time employment without concurrent DI receipt; (3) employment in a skilled job without concurrent DI receipt; and (4) employment at a firm with above median TFP without concurrent DI receipt.

We then divide the estimated quality-specific employment effects with the total estimated effect of DI exit on employment, to obtain the share of the employment effect that falls into each specific employment category. We compare this estimated share with the pre-DI share of treatment group individuals who were employed in a specific employment category (conditional on employment). We calculate the pre-DI shares for all individuals in the treatment group, and also separately for individuals with and without employment in 2011. With this approach, we provide insights on whether people who found employment after exiting DI as a consequence of the reform held worse quality jobs than their typical pre-DI jobs. Note that the pre-DI shares are based on a restricted set of treatment group individuals for whom we observe pre-DI employment, who may or may not exit DI and find employment later. Therefore, the comparison of the estimated quality-specific employment effects and the pre-DI shares can only be taken as suggestive evidence of possible deterioration in job quality.

Figure 8 shows our results. Panel (a) shows that relative to a pre-DI mean of 78%, on average 72% of the employment effect came from jobs paying above the minimum wage. 32% 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 48% of the employment effect came from skilled jobs, well below the pre-DI mean of 73%. Finally, Panel (d) shows that 16% of the employment effect came from employers with above-median TFP, less than half of the pre-DI mean of 34%. 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 when their benefits were 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 A3 indicate qualitatively similar reform effects for women as for men (Figure 3). Similarly, the IV estimates for the effect of DI exit and DI exit or DI benefit cut on labor market outcomes for women, reported in Appendix Table A7, are similar to the results for men (Tables 5 and 6).

Effects of the reform on income. We estimate the average effect of the reform on income, using the difference-in-differences specification of equation (1). We consider three outcomes, all deflated to 2011 and measured in US dollars: monthly total income, monthly DI benefit, and monthly earnings. Total income is the sum of DI benefit and earnings. Note that in the analyzed period there were no other major benefit programs with which the individuals affected by the DI reform could have replaced their DI benefit income. We report the estimation results in Table 7. On average, monthly income decreased by \$6.6 (1.8%), but the decrease was almost twice as high among those who had some employment in 2011, and even higher among those who worked for 12 months in 2011. On average, 74% of the decline in income is due to the decreasing DI benefit, the rest is explained by decreasing earnings. The estimated negative effect on earnings is in line with our main results showing a negative transitory impact of the reform on employment. Note, however, that none of the point estimates for the effect of the DI reform on earnings is significant statistically. We report the estimated effect of the reform on the three income indicators over time in Appendix Figure A4, which suggests that the average drop in income occurred in May 2012, and persisted up to the end of our observation period (December 2015), decreasing in absolute value only in the second half of 2015. Appendix Figure A5 shows that there was no such decline in income in the placebo group, i.e., individuals, who belonged to DI categories unaffected by the reform, although the pre-reform estimates in this placebo group are very noisy. Appendix Figure A6 shows no negative income effect of a placebo reform in January 2011, although we observe a slight negative trend in the DI benefit of the placebo treated vs the control groups over 2009-2011. Overall, these results suggest that unlike the evidence found by Borghans, Gielen and Luttmer (2014) and Deshpande (2016*b*), we do not find evidence that people who lost some of their DI benefits due to the reform could have compensated the loss by higher earnings or other benefits.

Effects of the reform on healthcare use and mortality. Appendix Figure A7 shows the time pattern of the impact of the reform on healthcare use and mortality. These results suggest that there was a jump in primary care provider 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 0.3 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). In Panel (e) of the figure, we report the estimated impact of the reform on two-year mortality, which also indicates that in contrast to some previous evidence on negative impact of DI payment on mortality, the reform did not have negative health effect over our observation period.[20]

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 born after a birthday cutoff and below a certain level of health impairment. As beneficiaries were allowed to work and faced a non-binding earnings limit, we interpret our estimates as capturing the income effect of DI benefit loss.

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 birthday cutoff of the reform. The majority of reassessed beneficiaries who were not employed pre-reform were left without any income after their benefit was terminated. 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.

[20] García-Gómez and Gielen (2018) find an increase in mortality among low-income women whose benefits were reduced following the 1993 reform in Netherlands, while Gelber, Moore, Pei and Strand (2023) show that higher DI payments reduce mortality in the US.

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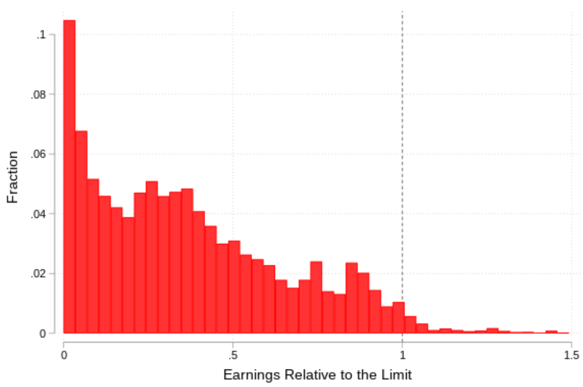
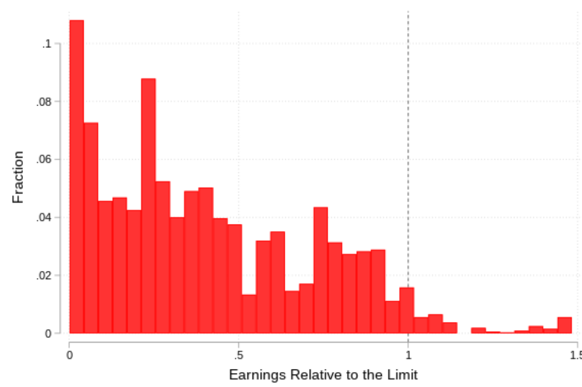
Figures and Tables

Figure 1: Earnings Distribution of DI Beneficiaries

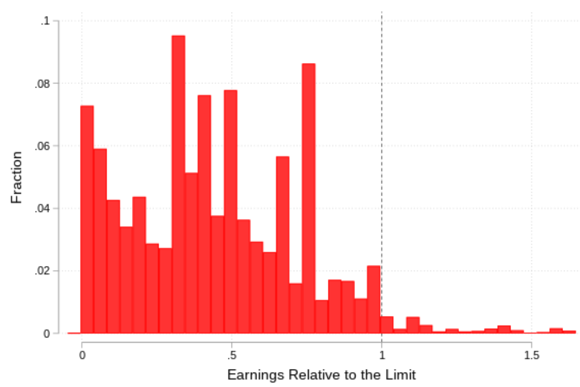
(a) 2011, Full Sample



(b) 2011, DI Exit in 2012-2015

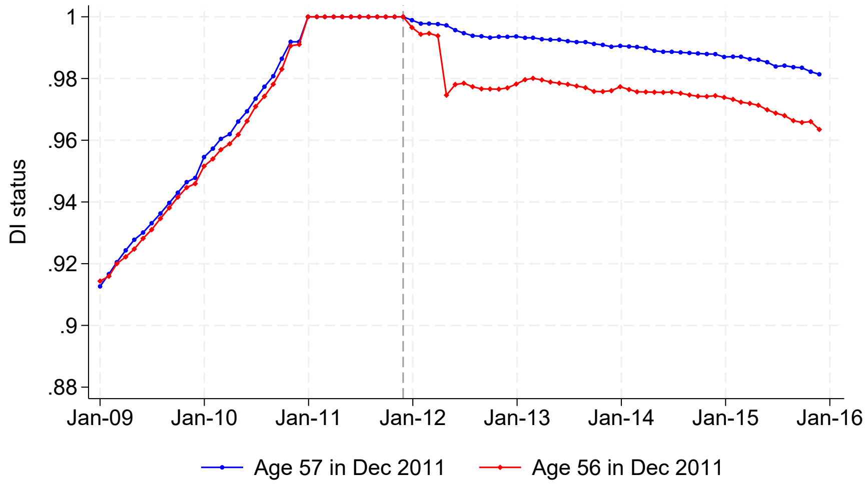


(c) 2012-2015 Average, New Earnings Limit DI After Revision



Notes: Figure shows the distribution of earnings relative to the individual earnings limit for members of the treatment group (men aged 56 in 2011) and control group (men aged 57 in 2011). Panels (a) and (b) include two groups of benefit recipients: (1) recipients of the Category III Disability Pension for whom the earnings limit is double their pension; (2) recipients of Regular Social Assistance entering between 2004-2007 for whom the valorized pre-disability earnings are estimated using earnings within the 12 months preceding benefit entry. Panels (a) and (b) display earnings as an average of the last 6 months for group (1) and an average of the last 4 months for group (2) DI recipients. Regular Social Assistance recipients entering before 2004 are excluded as their pre-disability earnings are not observed. Panel (a) includes the total sample and Panel (b) includes individuals who exited DI between January 2012 and December 2015. Panel (c) shows the distribution of earnings relative to the individual earnings limit after the reform for those who already underwent revision and were classified as eligible to Disability Allowance. The chart displays earnings as the average of the last 3 months in 2012-2013 and the minimum of the last 3 months in 2014-2015.

Figure 2: DI Status

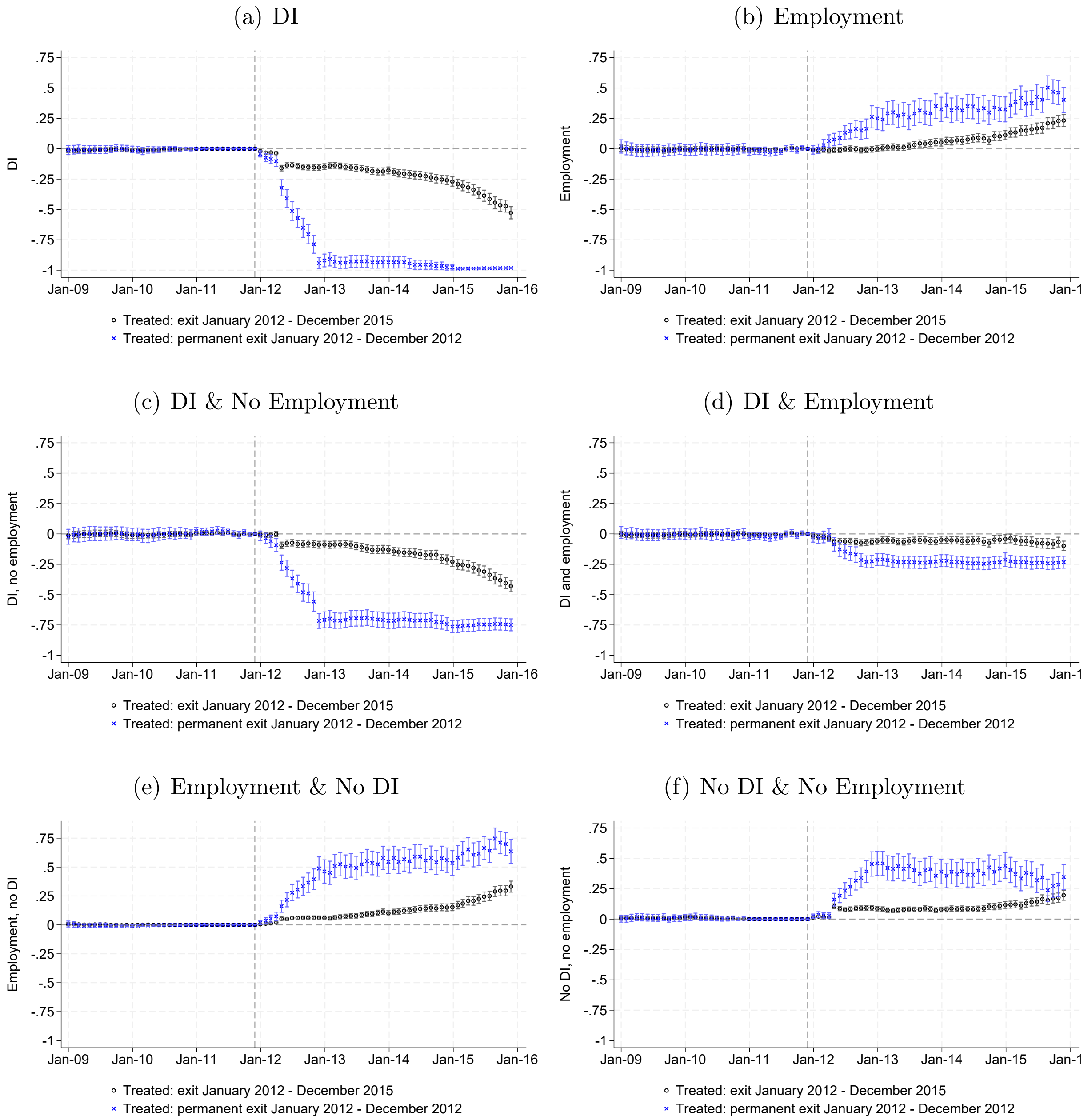


Note: Figure shows the share of individuals receiving DI benefits. The sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011.

Figure 3: Effect of the Reform Over Time

Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers born after the birthday cutoff relative to control workers born before the birthday cutoff. 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.

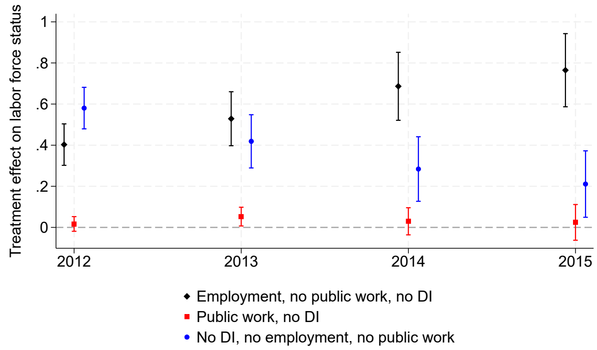
Figure 4: DI and Employment Over Time by DI Exit



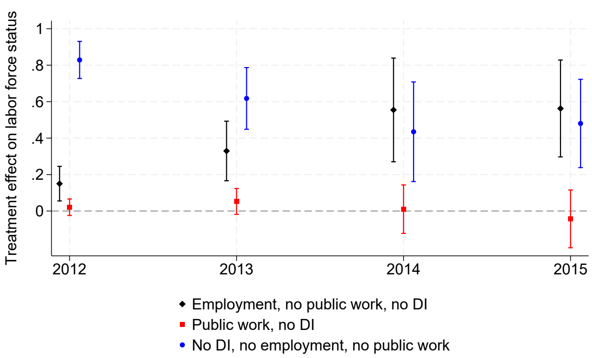
Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers born after the birthday cutoff relative to control workers born before the birthday cutoff. Figure displays the estimated β_T coefficients from equation (2) with 95% confidence intervals over 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. Black circles and confidence intervals show results when the sample of treated people is restricted to those exiting DI between January 2012 and December 2015 (still including all control observations in the sample). Blue crosses and confidence intervals show results when the sample of treated people is restricted to those exiting DI between January 2012 and December 2012 and not returning to DI by December 2015 (still including all control observations in the sample).

Figure 5: Effect of DI Exit Over Time

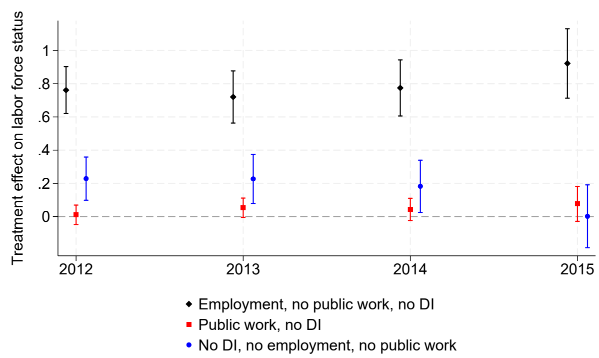
(a) All Individuals



(b) Individuals With No Employment in 2011



(c) Individuals With Some Employment in 2011

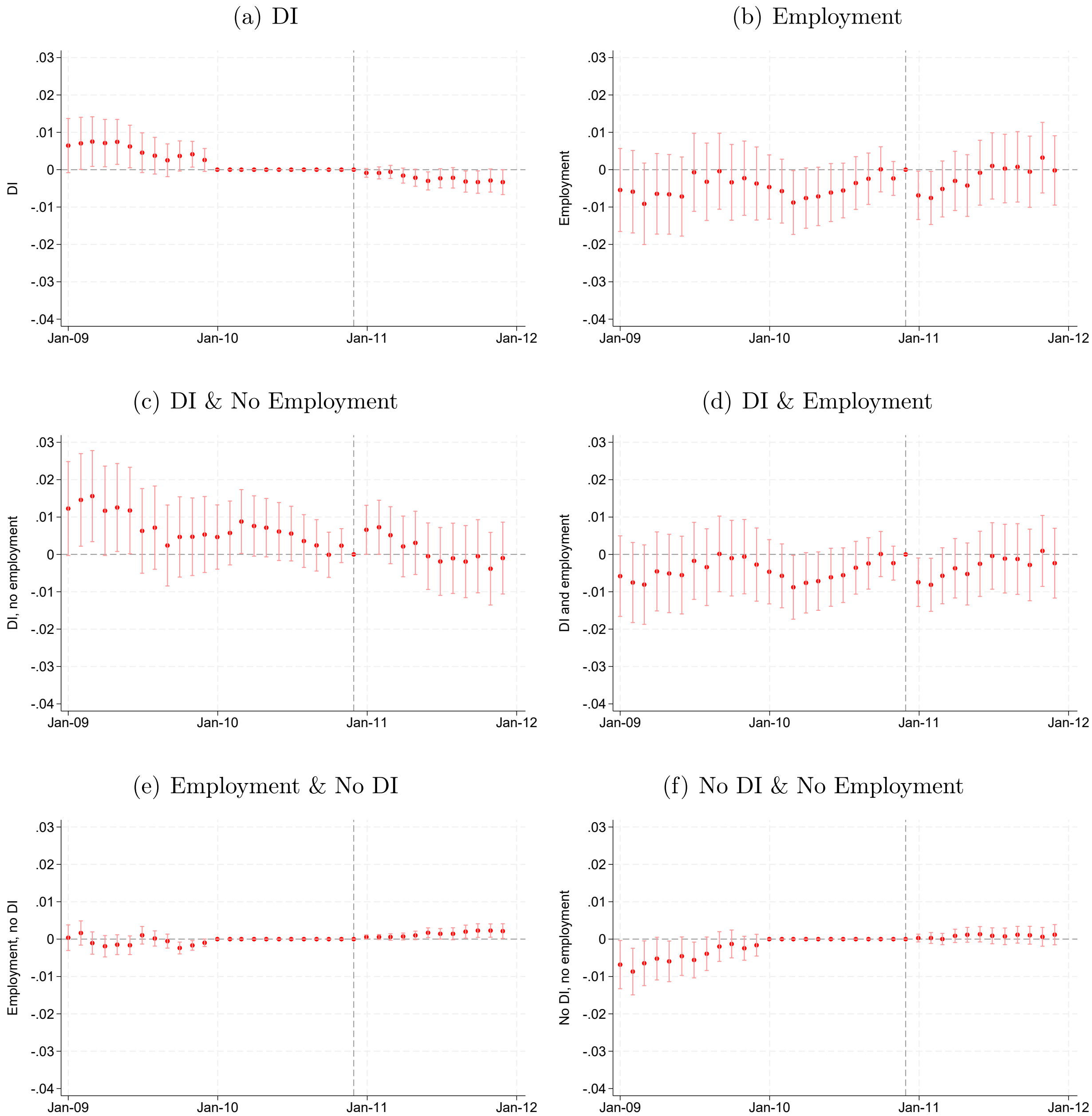


Note: Figure shows our estimates of the impact of exiting DI on the outcomes of affected workers. Figure displays the estimated β^{IV} coefficient from equation (4) with 95% confidence intervals estimated separately for each year 2012-2015 and by 2011 employment status. Sample is restricted to men who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

Figure 6: Placebo Analysis—Effect of the Reform Over Time, Unaffected DI Categories

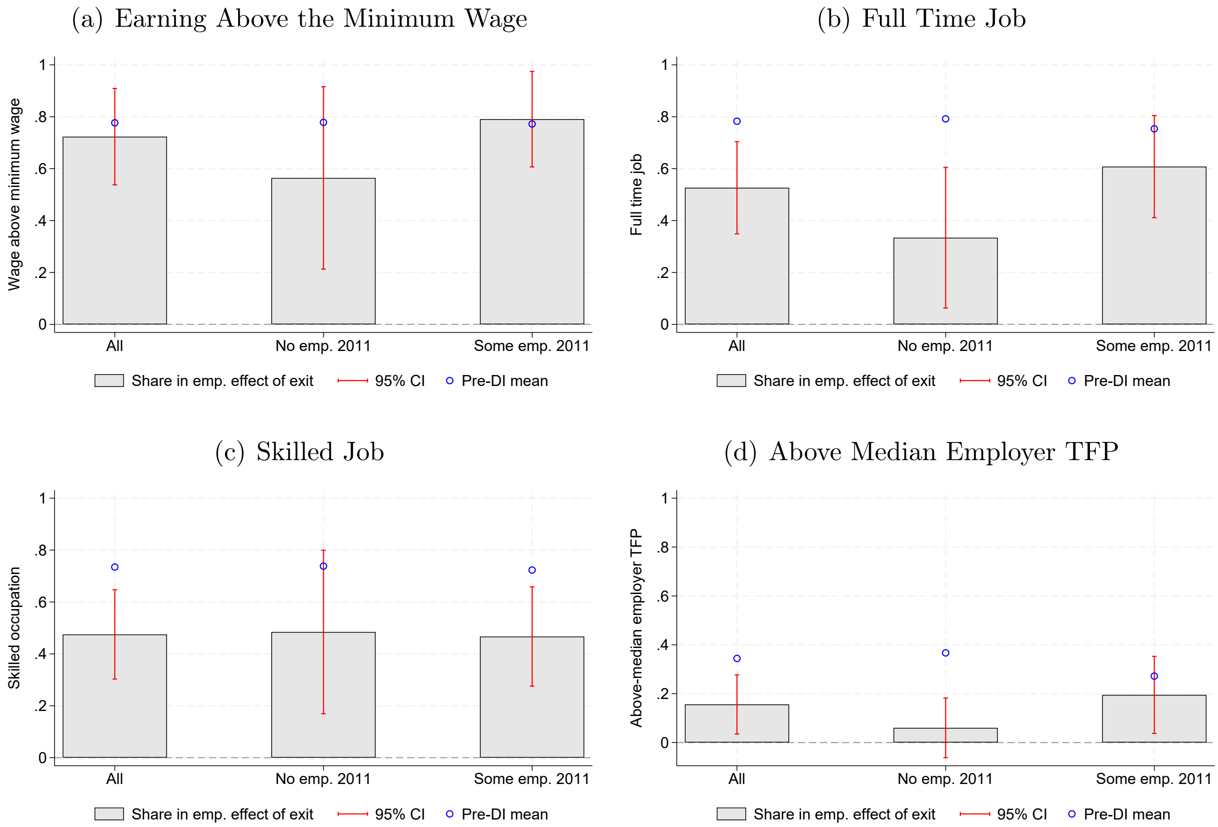
Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers born after the birthday cutoff relative to control workers born before the birthday cutoff for the placebo group of individuals in unaffected DI categories. Figure displays the estimated β_T coefficients 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 unaffected DI categories in December 2011. Treated people were aged 56 in December 2011 and control people were aged 57 in December 2011.

Figure 7: 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 born after the birthday cutoff relative to control workers born before the birthday cutoff for a placebo reform in 2011. Figure displays 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 2010 and control people were aged 57 in December 2010.

Figure 8: Effect of DI Exit—Job Quality



Note: Figure shows the share of employment effects of exiting DI by job quality. Gray bars display the β^{IV} coefficient estimates of equation (4), capturing the effect of exiting 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), for whom we observe pre-DI employment, restricting the pre-DI sample to months of employment, and calculating the pre-DI means for all individuals in the treatment group, and also separately for those who did and did not have some employment in 2011.

Table 1: Health Revision Obligation Cutoffs

		Born in 1955 or after (below age 57 at end of 2011)	Born in 1954 or before (age 57 and above at end of 2011)
Health impairment	$\geq$80%	No health revision	No health revision
	<80%	**Health revision**	No health revision

Note: Table shows the health revision cutoffs by health impairment and birthdate.

Table 2: Earnings Restrictions in Connection with DI Benefits

Before revision		After revision
Regular Social Assistance, entered before 2008 (health damage 40%-49%)	Disability Pension, category III (health damage 50%-79%)	Disability Allowance
80% of the pre-disability wage in average over 4 month	2009: 90% of the pre-disability wage in average over 6 month; 2010-2011: 200% of DI benefit but at least the minimum wage in average over 6 month	150% of the minimum wage in average over 3 month (2012-2013) or in all 3 months (2014-2015)

Notes: Table shows earnings limit of different DI benefit categories. 25% of individuals in our analysis sample (i.e., DI beneficiaries aged 56-57 in December 2011) received Regular Social Assistance (and entered DI before 2008), 75% received Disability Pension, category III.

Table 3: Descriptive Statistics

	Age at end of 2011			
	56 (Treated)		57 (Control)	
	No emp. in 2011	Some emp. in 2011	No emp. in 2011	Some emp. in 2011
	(1)	(2)	(3)	(4)
Months of employment in 2011	0	8.9	0	8.9
Months of employment in 2009-2011	1.2	23.4	1.3	23.2
Monthly DI benefit in 2011 (USD)	331	283	344	292
Monthly earnings in 2011 (USD)	0	158	0	164
Monthly total income in 2011 (USD)	331	441	344	457
Length of DI status in Dec 2011 (years)	11.3	11.4	11.1	11.2
Health damage less than 50% in Dec 2011	0.207	0.414	0.192	0.389
Drug spending in 2011 (USD)	796	234	754	244
Micro-region level unemployment rate in 2011	0.199	0.200	0.193	0.193
Pre-reform occupation				
Skilled	0.269	0.503	0.297	0.519
Unskilled	0.127	0.341	0.122	0.322
Missing	0.605	0.157	0.581	0.159
Number of individuals	4,824	1,540	5,571	1,710

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. The sample is split by having had 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. Occupation classification is based on the last observed pre-reform employment.

Table 4: Effect of the Reform—Difference-in-Differences Estimates

	Total DI	Total employment	DI & no employment	DI & employment	Employment & no DI	No DI & no employment
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Average effects						
Treated	-0.013***	-0.009*	0.004	-0.017***	0.008***	0.005**
	(0.003)	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)
Panel B: Heterogeneity by employment in 2011						
Treated × no emp. in 2011	-0.008***	-0.002	-0.002	-0.006	0.004***	0.004**
	(0.003)	(0.005)	(0.005)	(0.005)	(0.001)	(0.002)
Treated × some emp. in 2011	-0.025***	-0.026*	0.022	-0.047***	0.020***	0.005
	(0.006)	(0.013)	(0.014)	(0.014)	(0.005)	(0.003)
Panel C: Heterogeneity by employment months in 2011						
Treated × no emp. in 2011	-0.008***	-0.002	-0.002	-0.006	0.004***	0.004**
	(0.003)	(0.005)	(0.005)	(0.005)	(0.001)	(0.002)
Treated × 1-11 mo. emp. in 2011	-0.016*	-0.026	0.018	-0.034*	0.008	0.008
	(0.009)	(0.020)	(0.021)	(0.020)	(0.006)	(0.005)
Treated × 12 mo. emp. in 2011	-0.033***	-0.030	0.028*	-0.061***	0.031***	0.002
	(0.009)	(0.017)	(0.017)	(0.018)	(0.007)	(0.003)
Observations	1,111,686	1,111,686	1,111,686	1,111,686	1,111,686	1,111,686
Individuals	13,645	13,645	13,645	13,645	13,645	13,645

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 5: Effect of DI Exit—Instrumental Variables Estimates

Decomposition of the effect of DI exit	No DI & emp. & no public work	No DI & public work	No DI & no emp. & no public work
	(1)	(2)	(3)
Panel A: All individuals			
DI exit	0.599***	0.031	0.369***
	(0.060)	(0.023)	(0.056)
Observations	620,466	620,466	620,466
Individuals	13,645	13,645	13,645
Panel B: Individuals with no employment in 2011			
DI exit	0.379***	0.012	0.609***
	(0.079)	(0.037)	(0.077)
Observations	468,959	468,959	468,959
Individuals	10,395	10,395	10,395
Panel C: Individuals with some employment in 2011			
DI exit	0.801***	0.049*	0.150**
	(0.072)	(0.028)	(0.064)
Observations	151,507	151,507	151,507
Individuals	3,250	3,250	3,250
Panel D: Individuals with 1-11 mo. employment in 2011			
DI exit	0.568***	0.113*	0.319***
	(0.118)	(0.063)	(0.103)
Observations	70,968	70,968	70,968
Individuals	1,530	1,530	1,530
Panel E: Individuals with 12 mo. employment in 2011			
DI exit	0.952***	0.008	0.040
	(0.078)	(0.018)	(0.073)
Observations	80,539	80,539	80,539
Individuals	1,720	1,720	1,720

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 exiting 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. In Panels D, and E, the sample is further split by the length of employment in 2011.

Table 6: Effect of DI Exit or DI Benefit Cut—Instrumental Variables Estimates

Decomposition of the effect of DI exit or DI benefit cut	DI & emp. & no public work	No DI & emp. & no public work	DI & public work	No DI & public work	DI & no emp. & no public work	No DI & no emp. & no public work
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All individuals						
DI exit or benefit cut	0.071*	0.352***	0.002	0.018	0.339***	0.217***
	(0.041)	(0.050)	(0.004)	(0.014)	(0.062)	(0.042)
Observations	620,466	620,466	620,466	620,466	620,466	620,466
Individuals	13,645	13,645	13,645	13,645	13,645	13,645
Panel B: Individuals with no employment in 2011						
DI exit or benefit cut	0.083**	0.171***	0.004	0.005	0.461***	0.275***
	(0.039)	(0.043)	(0.006)	(0.017)	(0.082)	(0.060)
Observations	468,959	468,959	468,959	468,959	468,959	468,959
Individuals	10,395	10,395	10,395	10,395	10,395	10,395
Panel C: Individuals with some employment in 2011						
DI exit or benefit cut	0.041	0.649***	0.000	0.040*	0.148***	0.122**
	(0.091)	(0.096)	(0.000)	(0.023)	(0.054)	(0.055)
Observations	151,507	151,507	151,507	151,507	151,507	151,507
Individuals	3,250	3,250	3,250	3,250	3,250	3,250
Panel D: Individuals with 1-11 mo. employment in 2011						
DI exit or benefit cut	0.019	0.394***	0.000	0.078*	0.288***	0.221**
	(0.098)	(0.107)	(0.000)	(0.046)	(0.100)	(0.086)
Observations	70,968	70,968	70,968	70,968	70,968	70,968
Individuals	1,530	1,530	1,530	1,530	1,530	1,530
Panel E: Individuals with 12 mo. employment in 2011						
DI exit or benefit cut	0.063	0.865***	0.000	0.007	0.029	0.036
	(0.142)	(0.160)	(0.000)	(0.016)	(0.049)	(0.067)
Observations	80,539	80,539	80,539	80,539	80,539	80,539
Individuals	1,720	1,720	1,720	1,720	1,720	1,720

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 exiting DI or experiencing at least 10% DI benefit cut without 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. In Panels D, and E, the sample is further split by the length of employment in 2011.

Table 7: Effect of the Reform on Income—Difference-in-Differences Estimates

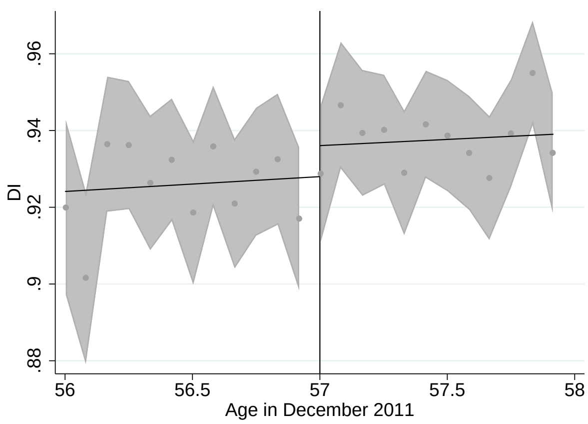
	Monthly income (USD)	Monthly DI benefit (USD)	Monthly earnings (USD)
	(1)	(2)	(3)
Panel A: Average effects			
Treated	-6.569***	-4.888***	-1.682
	(1.842)	(1.362)	(1.628)
Panel B: Heterogeneity by employment in 2011			
Treated × no emp. in 2011	-4.705**	-4.747***	0.042
	(1.844)	(1.548)	(1.522)
Treated × some emp. in 2011	-11.903**	-5.051*	-6.852
	(4.928)	(2.807)	(4.689)
Panel C: Heterogeneity by employment months in 2011			
Treated × no emp. in 2011	-4.705**	-4.747***	0.042
	(1.844)	(1.549)	(1.522)
Treated × 1-11 mo. emp. in 2011	-7.881	-2.837	-5.044
	(6.364)	(4.154)	(6.251)
Treated × 12 mo. emp. in 2011	-16.181**	-7.208*	-8.973
	(7.333)	(3.783)	(6.855)
Observations	1,111,686	1,111,686	1,111,686
Individuals	13,645	13,645	13,645

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. The income indicators are deflated to 2011.

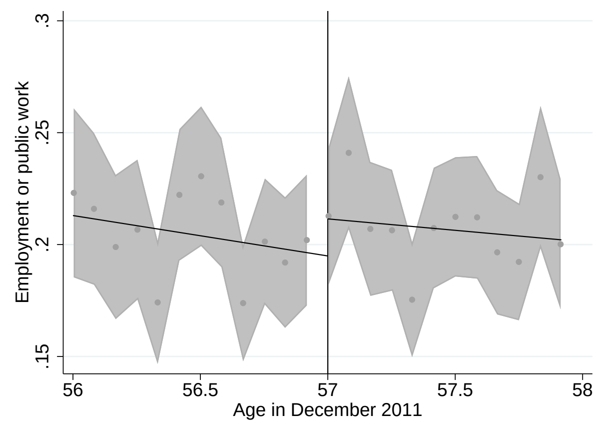
Appendix: Additional Figures and Tables

Appendix Figure A1: Regression Discontinuity Design

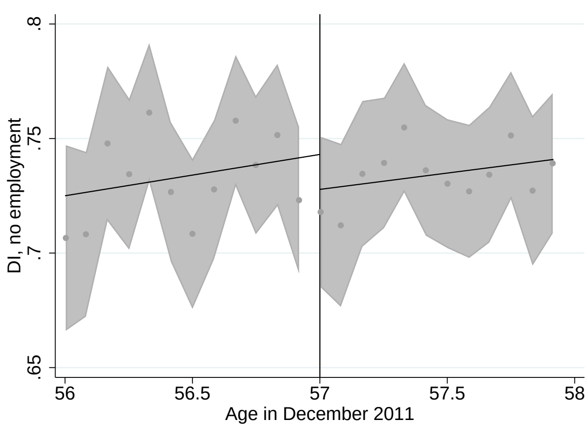
(a) DI



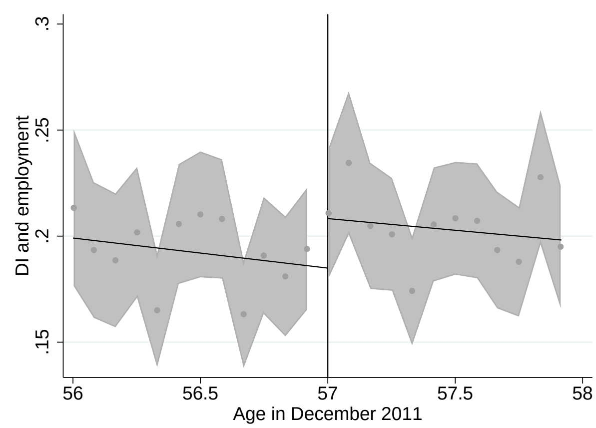
(b) Employment



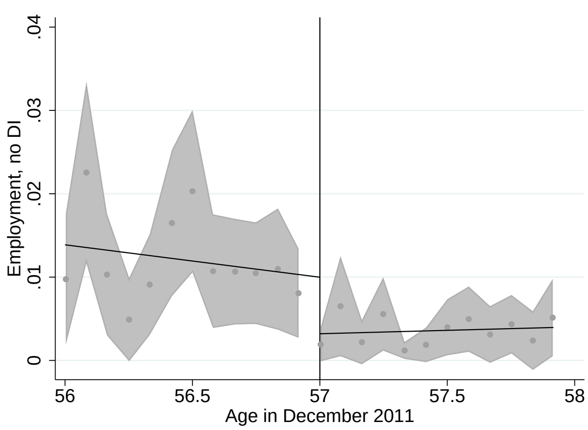
(c) DI & No Employment



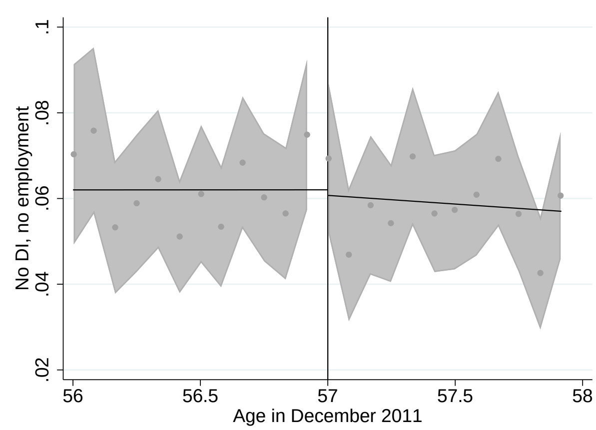
(d) DI & Employment



(e) Employment & No DI

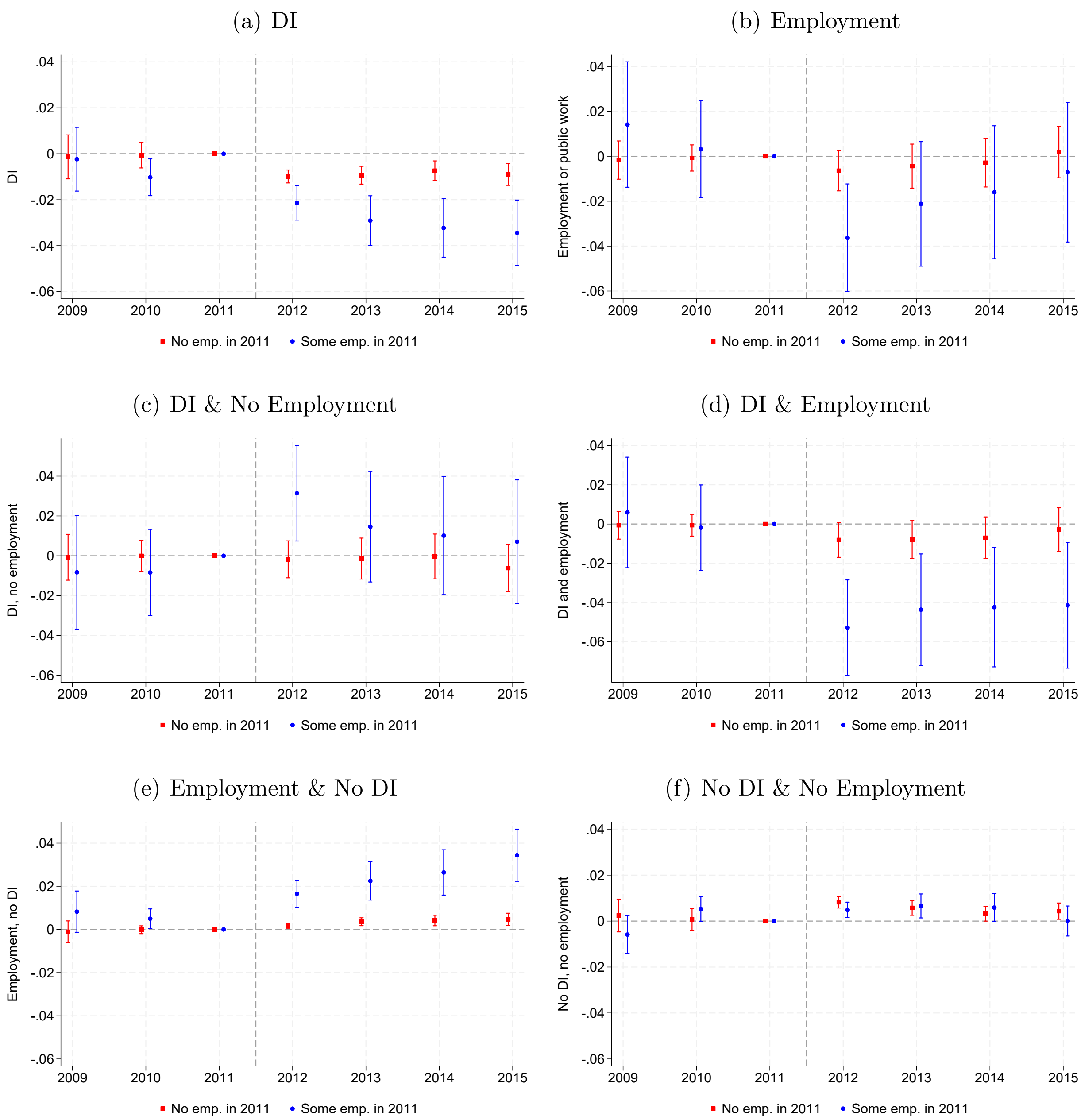


(f) No DI & No Employment



Note: Figure shows labor market status indicators averaged over 2012-2015 by age in December 2011 with 95% confidence intervals, and fitted regression lines with discontinuity at age 57. Sample is restricted to men who received DI throughout 2011, and belonged to the affected DI categories in December 2011. The figure is created using the Stata code of Calonico, Cattaneo, Farrell and Titiunik (2017).

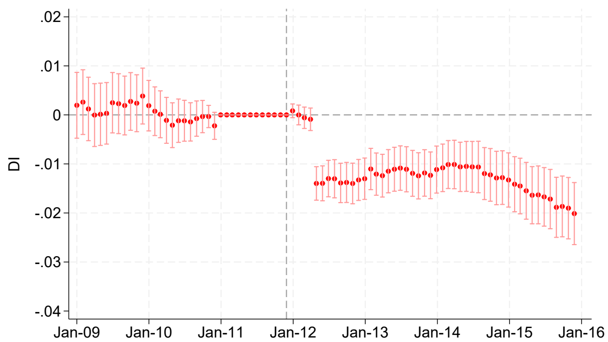
Appendix Figure A2: Effect of the Reform Over Time—Heterogeneity by Pre-Reform Employment Status



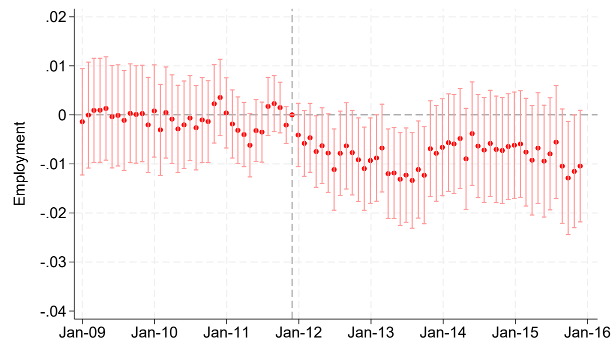
Note: Figure displays the β_T coefficient estimates of a yearly version of equation (2) interacted with employment in 2011, showing the treatment effects over 2009-2015, with 2011 as reference year. 95% confidence intervals are displayed. Sample is restricted to men who received DI throughout 2011, and 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 A3: Effect of the Reform Over Time—Women

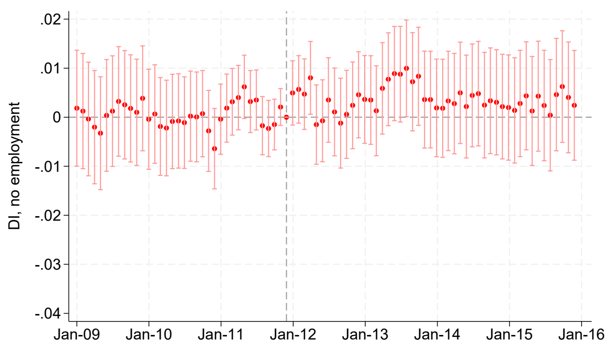
(a) DI



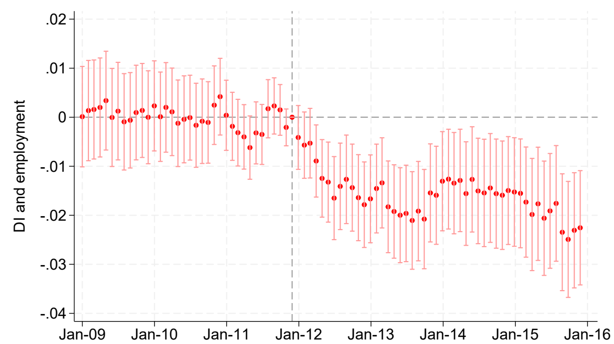
(b) Employment



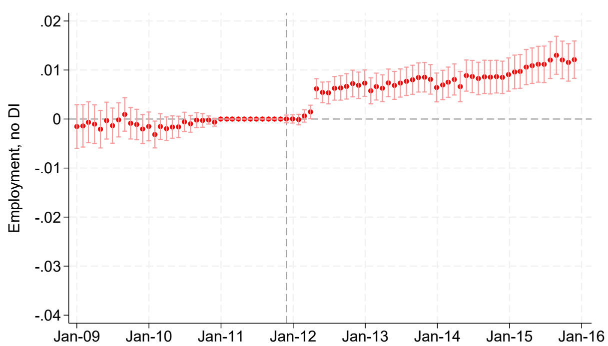
(c) DI & No Employment



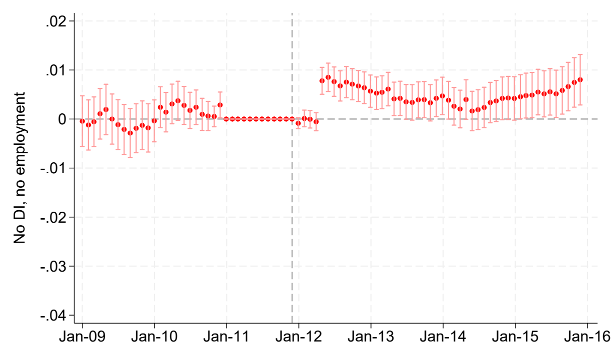
(d) DI & Employment



(e) Employment & No DI



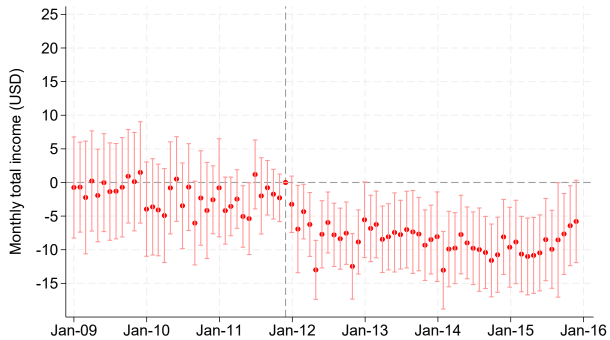
(f) No DI & No Employment



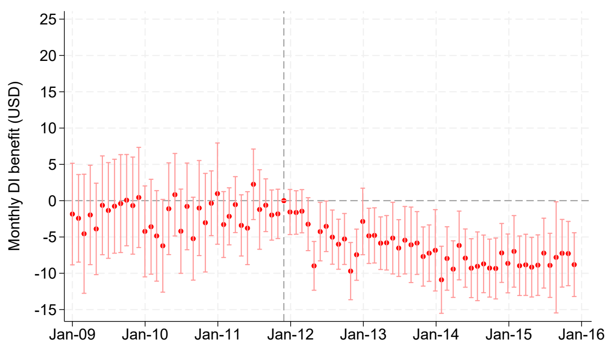
Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers born after the birthday cutoff relative to control workers born before the birthday cutoff. Figure displays the estimated β_T coefficients from equation (2) with 95% confidence intervals over 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 A4: Effect of the Reform Over Time on Income

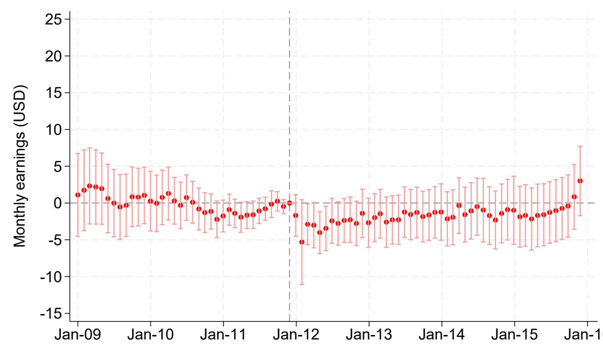
(a) Monthly Income



(b) Monthly Benefit



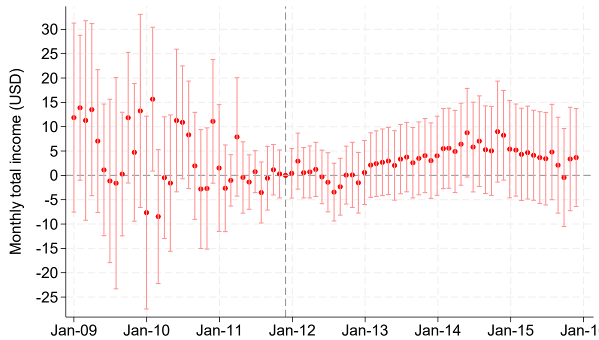
(c) Monthly Earnings



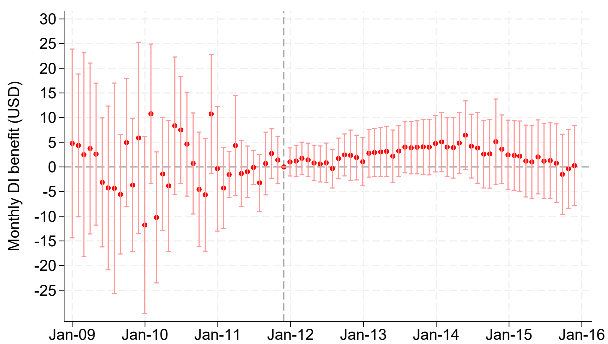
Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers born after the birthday cutoff relative to control workers born before the birthday cutoff. Figure displays the estimated β_T coefficients from equation (2) with 95% confidence intervals over 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. The income indicators are deflated to 2011.

Appendix Figure A5: Placebo Analysis—Effect of the Reform Over Time on Income, Unaffected DI Categories

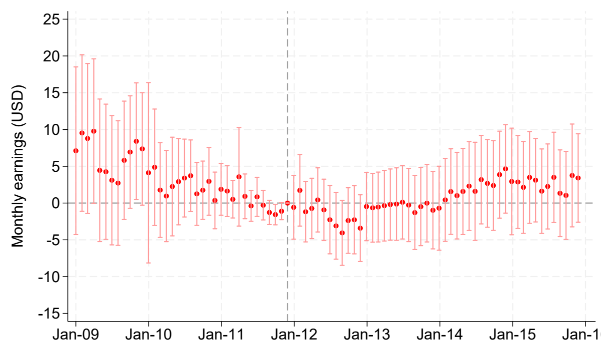
(a) Monthly Income



(b) Monthly Benefit



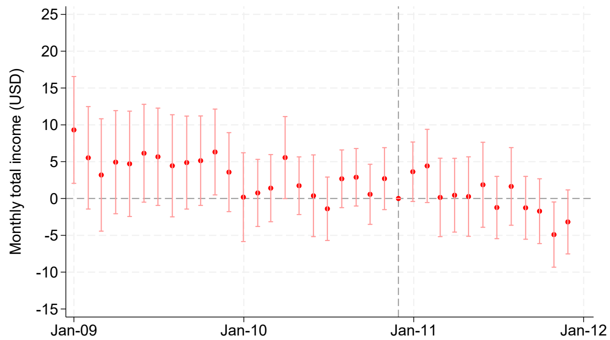
(c) Monthly Earnings



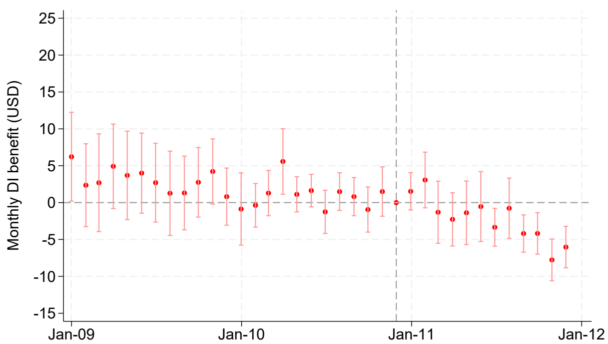
Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers born after the birthday cutoff relative to control workers born before the birthday cutoff for the placebo group of individuals in unaffected DI categories. Figure displays the estimated β_T coefficients 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 unaffected DI categories in December 2011. Treated people were aged 56 in December 2011 and control people were aged 57 in December 2011. The income indicators are deflated to 2011.

Appendix Figure A6: Placebo Analysis—Effect of Placebo Reform Over Time on Income

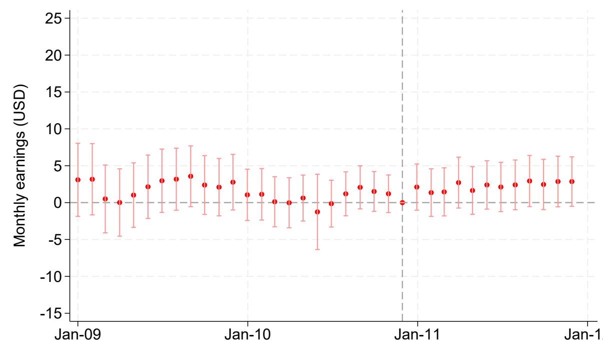
(a) Monthly Income



(b) Monthly Benefit



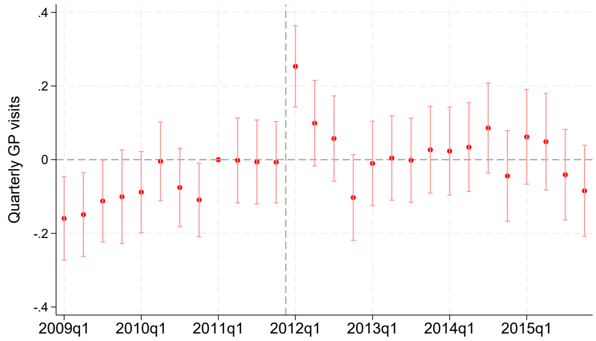
(c) Monthly Earnings



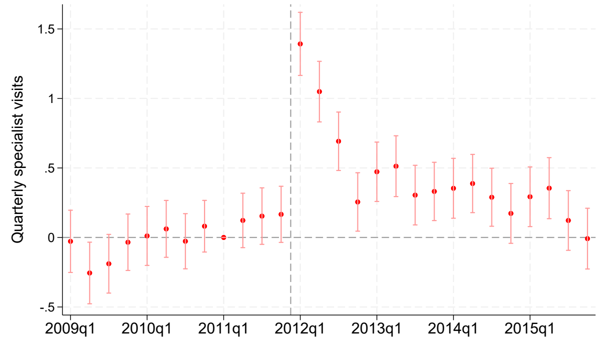
Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers born after the birthday cutoff relative to control workers born before the birthday cutoff for a placebo reform in 2011. Figure displays the estimated β_T coefficients from equation (2) with 95% 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. The income indicators are deflated to 2011.

Appendix Figure A7: Effect of the Reform—Healthcare Use and Mortality

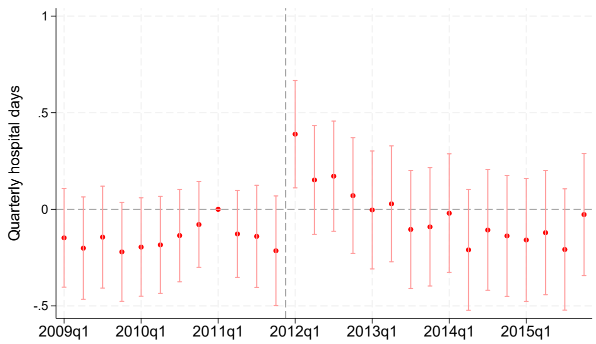
(a) Primary Care Provider Visits



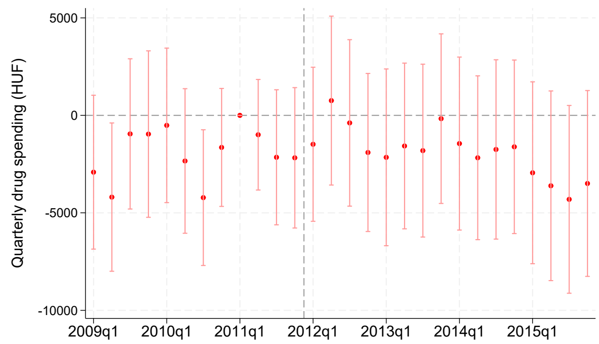
(b) Outpatient Specialist Care Visits



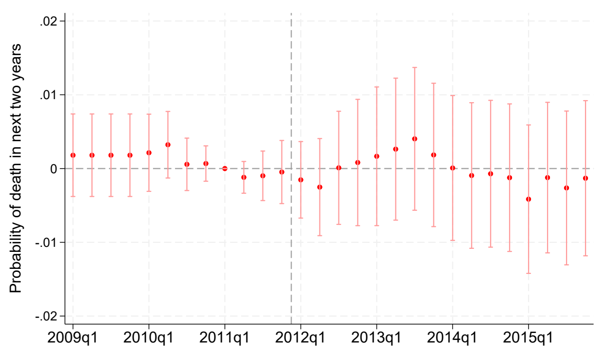
(c) Hospital Days



(d) Prescription Drug Spending



(e) Two-Year Mortality



Note: Figure shows our estimates of the impact of the reassessment policy on the outcomes of treated workers born after the birthday cutoff relative to control workers born before the birthday cutoff. 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. The average rate of two-year mortality in the control group is 0.041.

Appendix Table A1: Overview of Related Literature in Chronological Order

Study	Setting	Design	Findings
Bound (1989)	SSDI applicants, surveys from 1972 & 1978	Rejected DI applicants as comparison group	Less than 50% of DI beneficiaries would work were they not receiving DI benefits.
Gruber and Kubik (1997)	Dramatic increase in rejection rates for the SSDI program in the late 1970s.	Exploiting variations in the increase in rejection rates across US states.	Each 10% rise in denial rates led to a 2.8% fall in labor force non-participation among 45–64 year old males.
Gruber (2000)	Large increase in DI benefits in January 1987 in Canada, except for Quebec province	Difference-in-differences	Elasticity of labor force non-participation with respect to DI benefits is 0.28–0.36 among men aged 45–59.
Autor and Duggan (2003)	SSDI reduced screening stringency and a rising replacement rate between 1984 and 2001.	Instrumental variables to proxy DI benefit demand and supply conditions.	Increasing supply of DI benefits induced substantial labor force exit of low-skilled workers during 1984-1998. The DI system lowered US unemployment rate by half a percentage point since 1984.
Autor and Duggan (2007)	Isolating income effect by exploiting an unexpected 2001 policy change that extended benefits in the US Veterans Disability Compensation (VDC) program.	Difference-in-differences, using non-veterans as control group.	Labor force participation fell by more than 3 percentage points.
Chen and van der Klaauw (2008)	SSDI applicants from the 1990s, focusing on 'marginal' applicants	Bound (1989)'s comparison group approach and regression discontinuity	The labor force participation rate of DI beneficiaries would have been at most 20 percentage points higher had none received benefits; Estimate is smaller for 'marginal' applicants.
Karlström, Palme and Svensson (2008)	1997 policy change in Sweden that tightened DI eligibility criteria for applicants of 60-64 years of age.	Difference-in-differences, comparing affected ages to younger ages.	No employment effect up to 2-3 years after the reform. Social support substitution towards unemployment and sickness insurance.
Staubli (2011)	1996 policy change in Austria that tightened DI eligibility criteria for male applicants of 55-56 years of age.	Difference-in-differences, comparing affected ages to younger ages.	Drop in DI enrollment by 6-7.4 percentage points; increase in employment of 1.6-3.4 percentage points; social support substitution towards unemployment and sickness insurance.
von Wachter, Song and Manchester (2011)	SSDI applicants during the 1980s and 1990s	Difference-in-differences, comparing allowed and rejected male DI applicants.	As opposed to old rejected applicants (45-64 years), the labor force attachment of young rejected applicants (30-44 years) remains substantial (50%-60%) despite significant losses in earnings.
Marie and Vall Castello (2012)	Certain claimants of partial disability benefits in Spain are eligible to receive a 36% increase in the amount of benefits when they turn 55 years old.	Age discontinuity	Results translate into an 8% reduction in employment probability and an elasticity of DI generosity on Labor Market Participation of 0.22. As benefit eligibility is not work-contingent in Spain, the observed impacts are mainly due to an income effect.
Maestas, Mullen and Strand (2013)	SSDI applicants during 2005-2006	Exploiting random assignment to examiners with different allowance propensities	Among applicants on the margin of DI entry, employment would have been 28 percentage points higher had they not received benefit; Earnings capacity far below the per-DI earnings levels.
Borghans, Gielen and Luttmer (2014)	Dutch DI reform in 1993. Review of DI recipients under stricter eligibility criteria.	Cohort discontinuity	62% of lost DI benefits replaced with labor earnings, 31% through social support substitution.
French and Song (2014)	SSDI or SSI denials assigned to administrative law judges	Exploiting random assignment of judges to DI cases	DI benefit receipt reduces labor force participation by 26 percentage points three years after a disability determination decision.
Autor, Maestas, Mullen and Strand (2015)	SSDI applicants in 2005. Aim to identify the effect of application processing time ('delay channel'), separately from the benefit receipt effect.	IV strategy, exploiting exogenous variation in decision times induced by differences in processing speed among examiners.	Longer processing times reduce the employment and earnings of SSDI applicants for multiple years following application. Combining the decay effect with the benefit receipt effect suggests that the SSDI effect on employment is 100-140 percent larger than previous estimates have suggested for the marginal applicant.
Moore (2015)	1996 removal of drug and alcohol addictions as qualifying conditions for SSDI eligibility.	Difference-in-differences with affected beneficiaries who remained on DI as the comparison group	After the removal of disability benefits, 22% started working at levels that would have disqualified them for DI ('substantial gainful activity'). The estimate is 16% for 50-61 year olds.

Appendix Table A1: Overview of Related Literature in Chronological Order (Continued)

Study	Setting	Design	Findings
Autor, Duggan, Greenberg and Lyle (2016)	2001 policy change that extended benefit eligibility in the US Veterans Disability Compensation (VDC) program for 'boots on the ground' (BOG) veterans.	Difference-in-differences with trend break, comparing BOG and 'not on ground' veterans.	Benefits receipt reduced veterans' labor force participation by 18 percentage points, while measured income net of transfer income rose on average. Estimated income elasticity of labor force participation of -0.49 and a marginal propensity to reduce earnings per dollar of non-labor income of -0.26.
Deshpande (2016*a*)	1996 policy change in the Supplemental Security Income (SSI) age 18 reviews in the US	Cohort discontinuity	Youth who are removed from SSI recover one-third of the lost SSI income in earned income and suffer substantial long-term income loss.
Mullen and Staubli (2016)	Reforms to the Austrian DI and old age pension systems in the 1990s and 2000s.	Exogenous variation in DI benefits due to a series of reforms.	The elasticity of DI claiming with respect to DI benefit generosity is 1.2. Individuals experiencing a current involuntary unemployment spell are much more responsive than the employed.
Gelber, Moore and Strand (2017)	Discontinuous changes in the SSDI benefit formula for new entries from 2001 to 2007.	Exploiting the discontinuities with a regression kink design to estimate the income effect.	An increase in DI payments of $1 causes an average decrease in beneficiaries' earnings of $0.20. Annual employment rates decrease by 1.3 percentage points per $1,000 of DI payments. The income effect accounts for a majority of DI-induced reductions in earnings.
Autor, Kostøl, Mogstad and Setzler (2019)	DI applicants in Norway 1989-2011. Household level effects.	Exploiting the random assignment of DI applicants to judges who differ systematically in their leniency.	DI receipt induces a fall in earnings of approx. 45% of the DI benefit awarded, and raises household income and consumption by 16% and 18%. Large heterogeneity by marital status.
Deuchert and Eugster (2019)	Swiss DI reform in 2004 that aims to lower DI benefits for a group of existing DI beneficiaries by introducing a new band of partial benefit.	Difference-in-differences based on age cutoff. Principal stratification framework to provide bounds for income and substitution effect.	Modest average effect of 2.3 percentage points on employment and no effect on earnings. Only income effect is substantial: for individuals losing 25% of their DI benefits employment rose by 9-20 percentage points.
Garcia-Mandicó, García-Gómez, Gielen and O'Donnell (2020)	Dutch DI reform in 2004. Review of DI recipients under stricter eligibility criteria.	Trend-adjusted difference-in-differences	64% of lost DI benefits replaced with labor earnings.
Kantarci, van Sonsbeek and Zhang (2023)	Dutch DI reform in 2006, introducing stricter eligibility criteria and less generous benefits for new entries.	Difference-in-differences, comparing individuals who reported sick shortly before and after the reform.	The reform reduced DI receipt by 5.2 percentage points and increased labor participation and unemployment insurance receipt by 1.2 and 1.1 percentage points, respectively.

Appendix Table A2: 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 A3: Multinomial Logit Model of DI Benefit Loss

	DI exit		Benefit cut of at least 10%	
	Coefficient	Average marginal effect	Coefficient	Average marginal effect
Some employment in 2011	0.757***	0.017***	0.032	0.001
	(0.177)	(0.004)	(0.167)	(0.005)
Less than 50% health damage	1.092***	0.025***	-2.369***	-0.066***
	(0.159)	(0.004)	(0.380)	(0.011)
High drug spending in 2011	-0.763***	-0.017***	-0.237*	-0.006*
	(0.157)	(0.004)	(0.133)	(0.004)
Pre-reform occupation (ref.: skilled)				
Unskilled	-0.303	-0.007	-0.319	-0.009*
	(0.189)	(0.004)	(0.198)	(0.005)
Missing	-0.204	-0.005	-0.362**	-0.010**
	(0.196)	(0.004)	(0.165)	(0.005)
Long DI in Dec 2011	-0.190	-0.004	-0.597***	-0.016***
	(0.152)	(0.003)	(0.155)	(0.004)
High unemployment rate in 2011	-0.182	-0.004	0.328**	0.009**
	(0.147)	(0.003)	(0.132)	(0.004)
Number of observations	289,667			
Number of individuals	6,364			
Mean outcome in 2012-2015	0.023		0.029	

	DI exit		Benefit cut of at least 25%	
	Coefficient	Average marginal effect	Coefficient	Average marginal effect
Some employment in 2011	0.760***	0.017***	0.138	0.002
	(0.177)	(0.004)	(0.199)	(0.004)
Less than 50% health damage	1.104***	0.025***	-2.567***	-0.046***
	(0.159)	(0.004)	(0.519)	(0.010)
High drug spending in 2011	-0.763***	-0.017***	-0.364**	-0.006**
	(0.157)	(0.004)	(0.160)	(0.003)
Pre-reform occupation (ref.: skilled)				
Unskilled	-0.304	-0.007	-0.683***	-0.011***
	(0.189)	(0.004)	(0.261)	(0.004)
Missing	-0.202	-0.005	-0.437**	-0.008**
	(0.195)	(0.005)	(0.198)	(0.004)
Long DI in Dec 2011	-0.183	-0.004	-0.361**	-0.006*
	(0.152)	(0.003)	(0.183)	(0.003)
High unemployment rate in 2011	-0.187	-0.004	0.158	0.003
	(0.145)	(0.003)	(0.158)	(0.003)
Number of observations	289,667			
Number of individuals	6,364			
Mean outcome in 2012-2015	0.023		0.018	

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 determinants of benefit termination or reduction (coefficients 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 health damage magnitude is based on the DI category in December 2011. 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 A4: Effect of DI Benefit Loss—Instrumental Variables Estimates, Stricter Benefit Cut Definition

Decomposition of the effect of DI exit or DI benefit cut	DI & emp. & no public work	No DI & emp. & no public work	DI & public work	No DI & public work	DI & no emp. & no public work	No DI & no emp. & no public work
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All individuals						
DI exit or benefit cut	0.074*	0.430***	0.003	0.022	0.206***	0.265***
	(0.042)	(0.058)	(0.004)	(0.017)	(0.065)	(0.049)
Observations	620,466	620,466	620,466	620,466	620,466	620,466
Individuals	13,645	13,645	13,645	13,645	13,645	13,645
Panel B: Individuals with no employment in 2011						
DI exit or benefit cut	0.084**	0.224***	0.005	0.007	0.321***	0.359***
	(0.041)	(0.056)	(0.008)	(0.022)	(0.094)	(0.074)
Observations	468,959	468,959	468,959	468,959	468,959	468,959
Individuals	10,395	10,395	10,395	10,395	10,395	10,395
Panel C: Individuals with some employment in 2011						
DI exit or benefit cut	0.054	0.715***	0.000	0.044*	0.053	0.134**
	(0.085)	(0.098)	(0.000)	(0.025)	(0.044)	(0.060)
Observations	151,507	151,507	151,507	151,507	151,507	151,507
Individuals	3,250	3,250	3,250	3,250	3,250	3,250
Panel D: Individuals with 1-11 mo. employment in 2011						
DI exit or benefit cut	-0.044	0.516***	0.000	0.102*	0.138	0.289***
	(0.130)	(0.139)	(0.000)	(0.060)	(0.099)	(0.108)
Observations	70,968	70,968	70,968	70,968	70,968	70,968
Individuals	1,530	1,530	1,530	1,530	1,530	1,530
Panel E: Individuals with 12 mo. employment in 2011						
DI exit or benefit cut	0.117	0.841***	0.000	0.007	-0.001	0.035
	(0.112)	(0.129)	(0.000)	(0.016)	(0.032)	(0.065)
Observations	80,539	80,539	80,539	80,539	80,539	80,539
Individuals	1,720	1,720	1,720	1,720	1,720	1,720

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 exiting DI or experiencing at least 25% DI benefit cut without exiting 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, C, G, and H, 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 Table A5: Effect of the Reform—Difference-in-Differences Estimates, Heterogeneity

	Total DI	Total employment	DI & no employment	DI & employment	Employment & no DI	No DI & & no employment
Panel A: Average effects						
Treated	-0.013***	-0.009*	0.004	-0.017***	0.008***	0.005**
	(0.003)	(0.005)	(0.005)	(0.005)	(0.002)	(0.002)
Panel B: By health damage in 2011						
Treated × Less than 50% health damage	-0.028***	-0.001	-0.007	-0.021**	0.021***	0.008**
	(0.006)	(0.009)	(0.010)	(0.010)	(0.004)	(0.003)
Treated × At least 50% health damage	-0.006*	-0.012**	0.009	-0.015***	0.003**	0.003
	(0.003)	(0.006)	(0.006)	(0.006)	(0.002)	(0.002)
Panel C: By drug spending in 2011						
Treated × Low drug spending	-0.021***	-0.014**	0.007	-0.027***	0.013***	0.008***
	(0.004)	(0.007)	(0.007)	(0.007)	(0.002)	(0.003)
Treated × High drug spending	-0.005	-0.003	0.001	-0.006	0.003	0.002
	(0.004)	(0.007)	(0.008)	(0.007)	(0.002)	(0.002)
Panel D: By pre-reform occupation						
Treated × Skilled	-0.012**	-0.012	0.011	-0.024***	0.012***	0.001
	(0.006)	(0.009)	(0.010)	(0.009)	(0.004)	(0.004)
Treated × Unskilled	-0.018***	-0.018	0.012	-0.030**	0.012***	0.006
	(0.007)	(0.012)	(0.013)	(0.013)	(0.004)	(0.005)
Treated × Missing	-0.009***	-0.004	-0.001	-0.008	0.004**	0.005**
	(0.003)	(0.007)	(0.007)	(0.007)	(0.002)	(0.002)
Panel E: By length of DI status in Dec 2011						
Treated × Short DI	-0.009*	-0.010	0.007	-0.015**	0.006**	0.003
	(0.005)	(0.007)	(0.008)	(0.007)	(0.003)	(0.003)
Treated × Long DI	-0.015***	-0.008	0.002	-0.018***	0.010***	0.005***
	(0.003)	(0.007)	(0.007)	(0.007)	(0.002)	(0.002)
Panel F: By unemployment rate in 2011						
Treated × Low unemployment	-0.013	-0.005	-0.001	-0.013*	0.008***	0.005*
	(0.004)	(0.007)	(0.008)	(0.007)	(0.002)	(0.003)
Treated × High unemployment	-0.011***	-0.013*	0.009	-0.021***	0.008***	0.003
	(0.004)	(0.007)	(0.007)	(0.007)	(0.002)	(0.002)
Observations	1,111,686	1,111,686	1,111,686	1,111,686	1,111,686	1,111,686
Individuals	13,645	13,645	13,645	13,645	13,645	13,645

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, health damage is based on the DI category in December 2011. In Panel C, 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 D, 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 E, 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 F, 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 A6: Effect of DI Exit—Instrumental Variables Estimates, Heterogeneity

	Employment & no public work & no DI	Public work & no DI	No DI & no employment & no public work
Panel A: Average effects			
DI exit	0.599***	0.031	0.369***
	(0.060)	(0.023)	(0.056)
Observations	620,466	620,466	620,466
Individuals	13,645	13,645	13,645
Panel B: By health damage in December 2011			
DI exit, less than 50% health damage	0.676***	0.044	0.280***
	(0.096)	(0.042)	(0.086)
Observations	153,249	153,249	153,249
Individuals	3,328	3,328	3,328
DI exit, at least 50% health damage	0.526***	0.011	0.463***
	(0.082)	(0.017)	(0.078)
Observations	467,217	467,217	467,217
Individuals	10,317	10,317	10,317
Panel C: By drug spending in 2011			
DI exit, low drug spending	0.581***	0.042	0.377***
	(0.074)	(0.029)	(0.069)
Observations	310,960	310,960	310,960
Individuals	6,822	6,822	6,822
DI exit, high drug spending	0.637***	0.007	0.356***
	(0.097)	(0.027)	(0.091)
Observations	309,506	309,506	309,506
Individuals	6,823	6,823	6,823
Panel D: By pre-reform occupation			
DI exit, skilled	0.689***	0.008	0.303***
	(0.079)	(0.014)	(0.077)
Observations	211,367	211,367	211,367
Individuals	4,614	4,614	4,614
DI exit, unskilled	0.602***	0.084**	0.313***
	(0.107)	(0.042)	(0.098)
Observations	107,845	107,845	107,845
Individuals	2,368	2,368	2,368
DI exit, missing occupation	0.481***	0.016	0.503***
	(0.118)	(0.063)	(0.110)
Observations	301,254	301,254	301,254
Individuals	6,663	6,663	6,663
Panel E: By length of DI status in Dec 2011			
DI exit, at most 10 years on DI	0.515***	0.043	0.442***
	(0.094)	(0.029)	(0.089)
Observations	309,094	309,094	309,094
Individuals	6,809	6,809	6,809
DI exit, at least 10 years on DI	0.669***	0.021	0.310***
	(0.079)	(0.034)	(0.071)
Observations	311,372	311,372	311,372
Individuals	6,836	6,836	6,836
Panel F: By unemployment rate in 2011			
DI exit, low unemployment	0.638***	0.003	0.359***
	(0.083)	(0.017)	(0.089)
Observations	290,175	290,175	290,175
Individuals	6,416	6,416	6,416
DI exit, high unemployment	0.569***	0.053	0.378***
	(0.086)	(0.039)	(0.078)
Observations	330,291	330,291	330,291
Individuals	7,229	7,229	7,229

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 exiting 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, health damage is based on the DI category in December 2011. In Panel C, 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 D, 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 E, 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 F, 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 A7: Effect of DI Benefit Loss—Instrumental Variables Estimates, Women

Decomposition of the effect of DI exit	DI & emp. & no public work	No DI & emp. & no public work	DI & public work	No DI & public work	DI & no emp. & no public work	No DI & no emp. & no public work
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: All individuals						
DI exit		0.552***		0.078***		0.370***
		(0.076)		(0.022)		(0.079)
Observations		853,866		853,866		853,866
Individuals		18,185		18,185		18,185
Panel B: Individuals with no employment in 2011						
DI exit		0.171***		0.100***		0.729***
		(0.056)		(0.030)		(0.057)
Observations		631,952		631,952		631,952
Individuals		13,504		13,504		13,504
Panel C: Individuals with some employment in 2011						
DI exit		1.119***		0.053*		-0.172
		(0.262)		(0.030)		(0.272)
Observations		221,914		221,914		221,914
Individuals		4,681		4,681		4,681

Decomposition of the effect of DI exit or DI benefit cut	DI & emp. & no public work	No DI & emp. & no public work	DI & public work	No DI & public work	DI & no emp. & no public work	No DI & no emp. & no public work
Panel D: All individuals						
DI exit or benefit cut	0.112***	0.266***	-0.002	0.038***	0.408***	0.178***
	(0.039)	(0.040)	(0.002)	(0.010)	(0.053)	(0.050)
Observations	853,866	853,866	853,866	853,866	853,866	853,866
Individuals	18,185	18,185	18,185	18,185	18,185	18,185
Panel E: Individuals with no employment in 2011						
DI exit or benefit cut	0.056**	0.066***	0.000	0.039***	0.557***	0.282***
	(0.022)	(0.021)	(0.000)	(0.012)	(0.055)	(0.048)
Observations	631,952	631,952	631,952	631,952	631,952	631,952
Individuals	13,504	13,504	13,504	13,504	13,504	13,504
Panel F: Individuals with some employment in 2011						
DI exit or benefit cut	0.253***	0.776***	0.000	0.037*	0.059	-0.119
	(0.098)	(0.163)	(0.000)	(0.020)	(0.071)	(0.179)
Observations	221,914	221,914	221,914	221,914	221,914	221,914
Individuals	4,681	4,681	4,681	4,681	4,681	4,681

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 exiting DI (panels A-C) or exiting DI or experiencing at least 10% DI benefit cut without exiting DI (panels D-F), 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, C, E, and F, 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.