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IZA DP No. 17298

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ABSTRACT

Income Effects of Disability Benefits*

We provide novel evidence about the incentive and welfare effects of an increase in the generosity of disability benefits. Importantly, a unique policy variation in Germany allows us to isolate the income effect of a change in benefit generosity. We leverage this quasi-experimental policy variation using an RD design to estimate the effect of increasing disability benefits on employment, earnings, labor market transitions, and mortality outcomes using administrative data on the universe of new disability benefit recipients. Contrary to previous literature, our analysis reveals no significant impact on the employment and earnings of DI recipients due to the increased benefits. However, we find a sizable effect of the probability of returning to the labor market. We find no effects on recipient mortality six years after benefit award, but estimates imply a notable reduction in poverty risk, highlighting meaningful welfare implications of increased generosity.

JEL Classification: H55, I12, J22, J26

Keywords: disability insurance, pension reform, wealth effect, labor supply, mortality, RDD

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1 Introduction

Disability insurance (DI) programs are a key element of social security systems around the world. The programs provide insurance if workers' ability to earn income is permanently reduced or lost due to health problems. Despite providing insurance and social protection, disability benefit programs are criticized for inducing inefficiency in the labor market. The inefficiencies can arise when financial incentives rather than health conditions determine the take-up of disability benefits (Low and Pistaferri, 2015).

The design of DI programs is mainly determined by two factors: the criteria for eligibility for DI and the generosity of DI. Since the incentive effects of these two margins strongly differ (Haller et al., 2024), it is necessary to separately analyze the implication of changes in the eligibility and the benefit generosity instead of estimating the combined effect of the DI system. Moreover, take-up or substitution effects and income effects induced by the increase of DI benefits might lead to different labor market effects (Autor and Duggan, 2007). In fact, often it is assumed that income effects are small and can be ignored for welfare analyses.[1] In general, previous studies can not separate the different margins and quantify the overall employment effects of DI programs. Consequently, the evidence on the labor market effect of the income effect is scarce (Gelber et al., 2017). The same is true for the implications on welfare, social protection, or well-being.

In this paper, we use administrative data from the German pension insurance (DRV), which covers the universe of DI recipients, to provide novel evidence about the incentive and welfare effects of an increase in the generosity of disability benefits. Importantly, the unique policy variation in Germany allows us to isolate the income effect of a change in benefit generosity. On July 1, 2014, the German government considerably increased benefits for new recipients of disability benefits. On average, pension benefits for eligible individuals increased by around 5%, which corresponds to an increase of €540 per year for the average DI recipient in 2014. The size of the reform is comparable to the value of two additional years of contributions in the pension system, which DI is a part of in Germany. Due to the announcement and the timing of the reform, we can rule out that take-up behavior affects the number and the composition of DI recipients in our sample. Therefore, we can identify the income effect induced by an increase in benefits.

To quantify the incentive effects, we study the employment behavior and earnings of DI recipients and the probability of returning to the labor market. Similar to Black

[1] For a discussion in the context of the pension system, see Giupponi (2019) and for DI Gelber et al. (2017)

et al. (2018), García-Gómez and Gielen (2018), and Gelber et al. (2023), we focus on the effect on mortality to speak to the welfare implications of DI. In addition, we document the consequences for social protection by quantifying the effect of the reform on poverty risk.

We use the policy variation in a Regression Discontinuity Design (RDD) to isolate the causal effects of the increase in benefits. First, we analyze the incentive effects and estimate how the increase in DI benefits affects the employment and earnings of DI recipients. Since DI benefits are usually granted for a temporary period and need to be extended regularly, we also evaluate the probability of returning to the labor market. We next turn to the first measure of welfare and study the effect on mortality of DI recipients. Finally, we leverage the results from the RDD analysis and simulate the effect of the DI reform on poverty rates of DI recipients. For the last step of the analysis, we use additional data from the German Socio-Economic Panel (SOEP) study, which includes detailed information on all relevant income sources at the household level. To understand the distributional implication of the DI reform on labor market outcomes and mortality we study heterogeneous effects along relevant dimensions. We separately estimate effects by treatment intensity, previous occupation, and by the primary diagnoses of the DI recipients.

To establish causality of the income effect of an increase in DI benefits, we provide empirical evidence that rules out take-up effects and manipulation of the entry date. We show that neither the number of DI recipients nor their composition changes at the cut-off date. These results are consistent with the institutions and the timing of the reform. While the reform, in principle, induces incentives to postpone entry into DI to benefit from the increase in generosity, there usually is a six-month waiting period after the application date. Given that the reform was only announced shortly before the implementation, individuals were already in the waiting period and could not manipulate the starting date.

Based on the empirical analysis, we document meaningful incentive and welfare effects of the increase in DI benefits in two important dimensions. While there are no significant changes in employment, earnings, or mortality while being a DI recipient, we find a large and significant reduction in the transition from DI back to the labor market in the long run, i.e., more than four years after entering DI. Moreover, the increase in DI benefits leads to a meaningful reduction in poverty rates.

In more detail, we show that the increase in the generosity of DI benefits does not affect the employment and earnings of DI recipients in regular and marginal employment. The

large number of observations allows us to estimate small confidence bands such that we can rule out meaningful responses. This is still true when zooming into heterogeneous groups. Overall, across groups, we find a consistent pattern of non-significant effects with point estimates close to zero. The only exception is a small negative employment effect for the group with the greatest treatment intensity. However, the picture changes when we focus on the probability of returning to the labor market. In line with the institutions, in the first three years after receiving DI benefits, transition rates do not significantly change. However, after four years, when DI recipients need to apply for an extension of benefits, we find a significant and persistent effect on labor market transitions. The probability of returning to the labor market decreases by 0.7 percentage points. Relative to the pre-reform mean of about 4%, this is a reduction of more than 18%. After five years, the effect exceeds even 20%. We find that this result is mainly driven by female DI recipients. Furthermore, the response seems to be concentrated in recipients with physical DI diagnoses, while those with mental diagnoses do not react to higher generosity.

The analysis also shows a very clear picture for mortality. Despite the high mortality risk of DI recipients in combination with the increase in the generosity of benefits, mortality rates do not change. This holds for short-run effects, i.e. after one year, and for longer-run effects after 6 years. Again, we do not find significant differences for subgroups. Independent of the health diagnosis, age or gender, mortality rates do not change when the generosity of DI benefits increases.

In contrast, we find sizable effects on old age poverty. Since we can rule out meaningful behavioral responses on employment and earnings in the short run, we can derive these results focusing on the mechanical effect of the reform. In more detail, we simulate the effect of an increase in the DI benefits for the different groups and show that the risk of poverty is reduced by 5%.

Following the intuition of the optimal design framework for DI benefits developed in Haller et al. (2024) our results allow to draw a more general conclusion. The increase in DI benefits does not induce meaningful behavioral responses for employment and earnings; rather it affects labor transition rates only in the long run. The transition effects are large but concentrated on a small share of DI recipients. In contrast, the reform increases the disposable net household income of all DI recipients. Thus, our results support the findings of Haller et al. (2024), who show that stricter DI eligibility rules dominate reduced DI benefits as a policy tool for rolling back the DI program.

Our paper contributes to different strands of literature that are concerned with the

optimal design and impact of DI systems on welfare. First, our paper contributes to the literature on the incentive and employment effects of disability insurance (Blundell et al., 2016). Much of this literature analyzes the connection between benefit receipt and participation in the labor market using data on, for instance, rejected applicants to assess the remaining work potential of marginal DI recipients (Bound, 1989; Bound, 1991; Chen and Van der Klaauw, 2008; Maestas et al., 2013; French and Song, 2014; Autor et al., 2016). Studies demonstrate that the effect of benefit levels may interact with other factors like stringency of the application process (Staubli, 2011; Autor and Duggan, 2003; Campolieti, 2004; Karlström et al., 2008; Hanel, 2012; Garcia-Mandicó et al., 2020), the state of the labor market (Autor and Duggan, 2003; Black et al., 2002; Von Wachter et al., 2011), other welfare programs (Borghans et al., 2014; Low and Pistaferri, 2015), and private insurance markets (Seitz, 2021; Seibold et al., 2022; Fischer et al., 2023).

Autor and Duggan (2007) argue that there is an important distinction to be made between substitution and income effects of benefits when evaluating the generosity of DI programs. A growing number of studies evaluate substitution effects in the context of earnings thresholds (Weathers and Hemmeter, 2011; Kostøl and Mogstad, 2014; Benıtez-Silva et al., 2006; Koning and Sonsbeek, 2017; Vall Castelló, 2017; Ruh and Staubli, 2019; Kostøl et al., 2019; Krekó et al., 2023), but the evidence on income effects is rather scarce.[2]

Since our research setting only uses variation in the level of DI benefits, we can isolate pure income effects. Indeed, we are comparing recipients who only differ slightly in their entry date into DI. Since the reform only affected benefits, other components of the DI system, like eligibility requirements or substantial gainful activity thresholds for recipients, remain constant throughout the time period we observe. In this context, our study is most similar to Gelber et al. (2017), who use variation in benefit levels combined with a regression kink design to isolate income effects of the U.S. Social Security Disability Insurance (SSDI) program. Other examples of work on income effects include Autor and Duggan (2007), Marie and Castello (2012), and Deuchert and Eugster (2019). These studies generally find that income effects matter for the labor response to benefits. In Section 5, we discuss in detail why our results differ from previous papers. The central reason is differences in the institutional settings, including the role of other

[2]Several studies emphasize the importance of income or wealth effects on employment in the context of old age pensions, e.g., Fetter and Lockwood (2018) Gelber et al. (2016), Giupponi (2019), Ye (2021) Artmann et al. (2023), and Becker et al. (2023a).

transfer programs, such as relatively generous unemployment insurance, means-tested transfers, and different selection patterns into DI.

Secondly, our paper adds to the small literature studying the effect of the DI system on mortality. In the context of SSDI, Gelber et al. (2023) find that increased payments are linked to lower mortality rates. More precisely, using the variation in the US DI schedule, they estimate that a $1,000 annual increase in DI payments reduces the annual mortality rate for lower-income beneficiaries by 0.18 to 0.35 p.p. Black et al. (2018) focus on the effect of eligibility on mortality. Using random assignment of judges as instrumental variables, they find that marginal denial of benefits increases mortality within the first ten years, suggesting work is beneficial for health. However, inframarginal recipients experience reduced mortality, indicating that current disability thresholds are well-calibrated for maximizing longevity in DI applicants. García-Gómez and Gielen (2018) examine mortality effects of stricter DI eligibility and reduced generosity in the Netherlands. They document that stricter eligibility criteria and reduced generosity significantly increase mortality of women with low earnings. For Hungary, Krekó et al. (2023) find no evidence of the reduction of earnings limits on mortality.[3]

The paper proceeds as follows. Section 2 describes the disability insurance system in Germany and the changes introduced with the reform of 2014. In Section 3, we present the data and sample selection. In Section 4, we describe our estimation method and show evidence for the validity of our empirical approach. In Section 5, we present and discuss the results. Section 6 concludes.

2 Institutional Background

2.1 Disability Insurance System in Germany

Public disability insurance is a part of the general pay-as-you-go pension system in Germany, which insures the vast majority of the working-age population ($\sim 90\%$).[4] For most workers, public pension insurance is mandatory and contributions are deducted from workers' monthly paychecks alongside other social security contributions.

The application for disability insurance benefits is filed with the German pension insurance. Eligibility for benefits depends on formal requirements and the health status of

[3] Malavasi and Ye (2024) focus on the mortality effect of old age pensions in Germany. They estimate the effect of additional pension income for low-wage workers on mortality and find that additional pension income reduces mortality for this group.

[4] The main exceptions to this are civil servants and the self-employed.

the applicant. Only around half of all applications are successful. The formal requirements consist of three main criteria. First, individuals have to be below the statutory retirement age to qualify for disability insurance. Active recipients' benefits are turned into an old-age pension of the same amount when they reach the retirement age. Second, applicants have to have been insured in the public pension system for at least five years. Third, they have to have paid contributions for at least three years in the past five years to qualify for benefits.[5] After checking that the formal requirements are fulfilled, the pension insurance assesses the work impairment of the applicant via the information provided by the applicant as well as a medical officer. Generally, medical officers are instructed to exhaust other options such as rehabilitation measures or alternative occupations before benefits are authorized.[6] Successful applicants may receive partial or full disability benefits depending on the severity of their impairment. Individuals who are not capable of working more than three hours per day receive full benefits while individuals who can work up to six hours per day receive only partial benefits.[7] By default, disability benefits are granted only for a temporary amount of time and must be extended regularly for continued payment. Permanent benefits are only granted to individuals for whom the pension insurance determines that there is no potential to return to the labor market.

Disability benefits are computed on a similar basis as old-age pension benefits. Benefits depend on lifetime earnings, contribution times, and policy parameters. Lifetime earnings are measured in "pension credits" (*Entgeltpunkte*) that form the basis to compute pension entitlements once a worker claims old age retirement or disability benefits. A worker i's earned pension credits EPC_i are computed as follows:

$$EPC_i = \sum_{t=\underline{t}_i}^{\tau} \min \left\{ \frac{y_{it}}{\bar{y}_t}, \frac{y_t^*}{\bar{y}_t} \right\}, \tag{1}$$

where $t = \underline{t}_i$ is the year where the individual first registers with the German pension insurance (usually age 15) and τ denotes the year they start receiving benefits. Worker

[5] There are some exceptions to this criterion, for example, young applicants may still fulfill the formal requirements if they have not paid three years of contributions if they were in education before the onset of their impairment.

[6] Additionally, there is a special regulation for applicants born before 1961, who have access to occupation-specific disability insurance. These entitlements are part of the old DI system in Germany that has been phased out for younger cohorts (Fischer et al., 2023).

[7] Recipients of partial benefits are entitled to full benefits if it is not possible to find suitable employment on the labor market.

i's earnings in year t are denoted by y_{it} and are divided by the average earnings $\bar{y}_t$ of all insured workers who paid contributions that year. The maximum amount of pension credits that individuals can earn per year is capped by an upper ceiling that is given by the ratio of the contribution assessment ceiling y_t^* and the average earnings $\bar{y}_t$. Additional earnings are not counted toward entitlements.

Claiming disability benefits is considered a form of early retirement. Acknowledging that workers forgo contributions if they exit the labor market early due to a health related work limitation, the German pension insurance grants supplementary credits for recipients of disability pensions for these forgone working years. Specifically, workers total pension credits denoted by TPC_i are computed as

$$TPC_i = EPC_i + (g(t,a) - \tau) \times \overline{EPC}_i \tag{2}$$

where the first term gives the pension credits individual i has earned so far while the second term is the annual average of currently held credits $\overline{EPC}_i$ multiplied by the supplementary time granted by the pension insurance. $g(t,a)$ denotes the year until which supplementary time is granted. It depends on the individual's age a and the policy environment at time t.

The level of disability benefits is computed based on these credits as[8]

$$B_{i,t} = TPC_i \times v_t \times \max\left\{1 - 0.036(q(t,a) - \tau), 0.892\right\}, \tag{3}$$

where v_t denotes the value of a pension point in year t and the last term denotes the deduction levied for early retirement. For the period relevant to our analysis, the penalty caps at a maximum of 10.8%.[9]

DI benefits provide a substantial replacement to earnings from employment or other benefit sources for recipients. While many switch to DI benefits directly from employment, a large share receives other types of benefits before becoming a DI recipient. Another common pathway into DI goes through a period of sickness leave that bridges the gap between working and the award of benefits. Many recipients furthermore apply

[8] This is a simplified version of the formula, as the pension insurance distinguishes between pension credits earned in the former East and former West of Germany. This distinction is left out for simplicity in the illustration here.

[9] Before 2012, individuals were allowed to claim disability pension without deductions from the age of 63 onwards. Since then, the threshold has gradually increased to 65 years by 2024. At the same time, the statutory retirement age steadily increases from 65 by one month each year until it reaches 67 in 2030. Finally, there is a regulation that allows for early retirement without a penalty for individuals who have paid contributions for at least 45 years.

for DI benefits while receiving unemployment benefits or social assistance.[10] Depending on the previous earnings and entitlements from other programs, the replacement rate of DI benefits may vary. Before the 2014 reform the median replacement rate of DI benefits compared to the year before recipients claim benefits ranges between around 50% for earnings from employment to 55% for benefits from unemployment insurance or sickness benefits.

Overall, DI income in Germany is fairly low, and a relatively high share (34% pre-reform) of DI recipients in Germany is considered at risk of poverty. Depending on the household context, recipients with low benefits may also be eligible for means-tested social assistance. Before the introduction of the 2014 reform, this was the case for around 14% of all DI recipients (Becker et al., 2023b).

2.2 The 2014 Reform to DI Benefits

During the 1990s and 2000s, Germany reduced the generosity of the system, changed the assessment system, and tightened entrance criteria (Burkhauser et al., 2016; Fischer et al., 2023; Seibold et al., 2022). These reforms resulted in a decline in disability benefit entries and also led to a reduction in benefit levels alongside a steep increase in the poverty risk rate and take-up of welfare (Geyer, 2021). Thus, Germany, unlike other countries, did not experience growth in its disability insurance program over a long period.

In 2014, the German government changed policy and increased disability pension benefits for new recipients. The reform had two goals: First, to reduce the poverty risk of new DI claimants and, second, to adjust the benefit calculation to higher retirement ages.

The reform-induced change in benefit computation is illustrated in Figure 1. The increase in benefits was mainly implemented through an increase of the supplementary time from age 60 to age 62. Additionally, the reform changed the way the time before the claim of benefits is accounted for in the computation of entitlements. Before the reform, the supplementary time was just weighted by the average pension credits earned before entry into disability pension. Since the reform, the four years before entry were left out of this computation if doing so is advantageous for the recipient. This change was implemented to compensate for missing contributions due to disability-induced low

[10] While labor supply before a DI award is not restricted by any institutional rules, a long application process and coverage by sickness benefits or unemployment insurance result in strong reductions in employment shares of DI recipients even before benefit award.

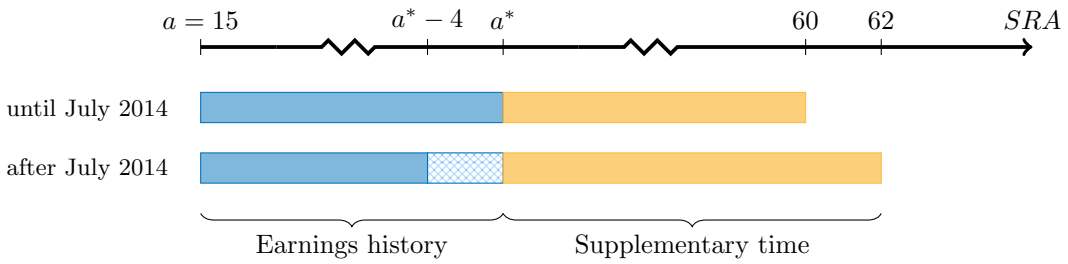


Figure 1: Illustration of Reform to Disability Benefit System

Note: The figure shows the change in benefit computation after the reform on July 1st, 2014. Individuals receive an account with the German pension insurance at age $a = 15$ which records contributions for their entire earnings biography and can claim disability benefits with the pension insurance until they reach the statutory retirement age SRA (in 2014 the statutory retirement age in Germany was 65 years and 3 months). When an individual claims disability benefits at age a^*, their benefits are computed based on their earnings history (blue area) and supplementary time (orange area) that is granted to substitute for forgone contributions after the start of the pension. Until the reform, the supplementary time was granted until age 60. The 2014 reform increased this time to age 62. The reform additionally changed the way the four years before the start of the pension (shaded blue area) enter the computation. For individuals that started receiving benefits after the reform, these four years are omitted from the computation of entitlements if doing so results in higher benefits.

labor market attachment in the years leading up to individuals' DI claims.[11]

The reform was implemented fairly quickly, with a first draft released roughly five months before it went into effect. It passed on June 23, 2014, as part of a larger pension package[12] and went into effect a week later on July 1, 2014.

2.3 Incentive Structure of the DI Reform

For the majority of recipients, DI benefits provide the main source of individual income. Thus, given the relatively low level of DI benefits, DI recipients generally have a high incentive to engage in employment to supplement low benefit payments.

In general, DI benefits can change labor market behavior via income and substitution effects (Autor and Duggan, 2006). Higher benefits induce an income effect, which reduces the incentives to be employed while receiving DI and increases the incentives to re-apply for DI benefits and reduced transitions back to the labor market. The substitution

[11] The effect of the second component on average benefit increases is rather small, as we illustrate in Section A.1 of the Appendix, meaning the majority of the bonus comes from the additional two years of supplementary time.

[12] The package consisted of three main components: the increase in benefits for new DI recipients, an increase in overall pension benefits for mothers (Becker et al., 2023a), and a new early retirement regulation for workers with a long insurance history. These additional reform components do not affect our identification. For clarity, we discuss them in detail in Section A.2 of the Appendix.

effect is more complex, potentially affecting several margins. First, earnings are subject to substantial gainful activity thresholds. For those receiving full disability benefits, the annual earning threshold lies at 6,300 Euros for the time period assessed in our study. Any amount earned above this limit will be subject to a 40% deduction from the disability benefit. Secondly, in a broader sense, employment may be limited by strategic behavior related to eligibility concerns. While the German system does not condition eligibility on employment or earnings before the award of benefits, recipients may lose their status if they work too much after they start receiving benefits. Generally, recipients must notify the pension insurance of any gainful employment they engage in. Doing so may lead to a reassessment of benefit eligibility by the pension insurance. These regulations create a strong incentive for DI recipients to reduce or abstain from labor supply.

Importantly, substitution effects cannot easily be disentangled from income effects as the incentive structures are attached to benefit payments (Autor and Duggan, 2003; Autor et al., 2016; Gelber et al., 2017). The 2014 reform to the German DI system, however, offers a unique opportunity to isolate income effects in this broader incentive structure common to DI systems (Böheim and Leoni, 2018). The reform only affected the level of benefits for new recipients while the remaining incentive structure of the system remained the same, independent of whether recipients entered the system before or after the reform. In addition, as documented in detail in Section 4, individuals could not manipulate the starting date of DI receipt due to the short announcement of the reform and a mandatory 6 month waiting period.

3 Data

We use administrative data provided by the German pension insurance on the universe of new DI recipients. Our sample combines multiple data sources from the pension insurance that we can merge using a unique identifier. The main data set used in our analysis contains information on all new DI recipients collected in the year they become recipients (EM-*Rentenzugang, RTZN*). The data includes demographic characteristics and a rich set of information from the insurance accounts of recipients, as well as details about the benefits they receive. We supplement this data with information on mortality and employment outcomes using two further data sources. Information on mortality is retrieved from an annual data set containing the universe of pension losses (*Rentenwegfall, RTWF*), which records the end date and reason for termination of a pension. We additionally supplement the data using annual information on all active insurance ac-

counts[13] in Germany (*Aktive Versichertenkonten, AKVS*). The AKVS data is recorded at the end of each calendar year and allows us to observe employment, unemployment, receipt of other social security benefits, and earnings from these different activities on an annual level. We can merge these data sources using a unique identifier to track mortality, employment, and earnings for all individuals who became benefit recipients in the years leading up to and following the reform in 2014. We describe our data preparation process in Section A.1 of the Appendix.

Our data consists of individuals who started receiving benefits between January 2012 and December 2016. Thus, we observe a period of 30 months before and after the introduction of the reform. Over the full period, 856,286 individuals started receiving some type of disability benefit. We restrict our data in multiple ways to define the relevant sample for the analysis. In the empirical analysis, we perform robustness checks to ensure that the validity of our design and results are not sensitive to sample selection.

The most important sample selection is that we focus only on temporary benefit recipients. These constitute the default case in the German DI system and make up more than half of newly awarded benefits. Permanent benefits are only granted to individuals for whom the pension insurance determines there is no potential to return to the labor market, thus functioning, in many cases, as a route to early retirement. Aside from temporary benefit recipients being more relevant in the context of continued labor supply, we also focus on them in the interest of identification: temporary benefits have a six-month waiting period between eligibility and the start date of benefit payments, which made it impossible for recipients to select into the post-reform group, thus allowing for a clean research design. In the empirical analysis, we still provide robustness checks, including recipients of permanent benefits, and, as expected, we show that the results do not change.

We apply some further standard restrictions to the sample. Firstly, we exclude individuals with additional entitlements from other pension schemes to ensure the tractability of the increases induced by the reform. In particular, this excludes individuals with miners' insurance, individuals with special entitlements after employment in workshops for people with disabilities, recipients who receive occupational disability benefits as part of entitlements from old policy regimes, individuals with pension entitlements from countries other than Germany, and individuals without German citizenship, and partial benefit recipients[14] amounting to around 20% of the sample. We also exclude recipients

[13] This includes DI recipients with any insurance-relevant activity in a year. Pension recipients without such activity are considered "passive" and do not show up in the AKVS data.

[14] We eliminate partial recipients because they only make up a very small share of the remaining ben-

above age 60 as they are not affected by the reform.[15]

Secondly, we exclude recipients with very long processing times in the benefit-granting process. In particular, we exclude recipients whose date for determining benefit entitlements lies more than two years before their start date (2.8%), recipients with a distance between the application date and start date of more than one year (1.2%), and recipients who had to wait for their acceptance notice for more than one year after the start date of their pension (16%). We apply these restrictions to exclude complicated cases that may have involved lawsuits and to make sure recipients are actually receiving disability benefits at the point in time we observe their employment and mortality outcomes. Our final sample consists of 254,094 recipients for the start years 2012-2016, including 51,044 entries from the reform year 2014. In the data cleaning process, we standardize all benefits to their respective value in 2014 to make entry cohorts comparable over time.

4 Research Design

We use a regression discontinuity design for our analysis where the assignment variable is given by the start date of the disability pension. Our setting allows for a sharp regression discontinuity design as the increase in benefits is a deterministic function of the start date: the additional supplementary credits are automatically added by the pension insurance if the start date of benefits is July 1, 2014, or later. Accordingly, our main specification estimates the discontinuity in the conditional expectation of our outcomes of interests at the reform cutoff date. In particular, we specify a local polynomial regression model of the form

$$Y_i = \alpha + \beta D_i + f_1(X_i - c) + D_i f_2(X_i - c) + \epsilon_i \tag{4}$$

where $D_i = \mathbb{1}\{X \geq c\}$ is a dummy for the benefit start date of recipient i lying after the policy reform. The coefficient β is the parameter of interest and captures the change in outcome Y_i at the cutoff point c. The assignment variable X_i is given by the start date of the pension in monthly bins with a cutoff positioned at the reform month, July 2014. We allow for different slopes in our model before and after the cutoff with f_1 and f_2 denoting unknown functional forms.

To implement the regression discontinuity design, we need to select an appropriate

efit recipients. Furthermore, they receive only a very low treatment intensity since the increase is proportional to the benefit amount, preventing meaningful analysis.

[15] The percentages are recorded in the order in which we apply the restrictions to the data.

bandwidth and to specify f_1 and f_2 for the functional form of the model. Our main specification is a local linear regression with a triangular kernel such that $f_k = \gamma_k(X_i - c)$ for $k = 1, 2$. We select bandwidths according to the optimal bandwidth selection procedure outlined in Calonico et al. (2014). For robustness we present our estimates using various other selections of bandwidth.

We focus on the following outcome variables: employment, earnings, labor market transitions, and mortality in the years after DI award. Employment, earnings, and labor market transitions are recorded by the pension insurance at the end of each calendar year. Annual mortality is measured on a 12-month basis as we can observe the month and year of death of individuals in our data. Importantly, we analyze outcomes relative to the year individuals started receiving benefits.[16]

4.1 First Stage

Figure 2 shows the first stage of our research design, the increase in average monthly benefits around the time of the reform. Benefits are reported in pre-tax terms and before social security contributions. We standardize pension entitlements to the year 2014 to make benefits comparable over multiple entry years.[17] At the time of the reform DI recipients received per month on average €765. We estimate a significant increase in average benefits of around €45 Euros per month or €540 per year at the reform cutoff. In relative terms, this is an increase of more than 5%.[18]

The point estimates hide an important aspect of how the change was implemented. Since the increase in benefits results from an extension in supplementary time, the absolute benefit bonus depends on pension wealth accumulated before becoming a DI recipient. Figure 3 displays the overall increase in disability benefits for recipients in our sample by benefit percentile. While recipients in the lowest decile of our data receive a

[16] In many cases, the actual starting time coincides with the official start date of the pension, which we call "entry into DI benefits." However, a large share of individuals (70%) are accepted for DI benefits after the official start date of their pension due to delays in the approval procedure or (legal) disputes about DI claims. Around 26% start receiving benefits more than half a year after the start date of their pension. As mentioned in Section 3, we exclude individuals who wait more than a year for their acceptance. In these cases, recipients receive their benefits retrospectively as a lump sum payment. To make sure that we observe outcomes at a point in time where individuals are actively receiving benefits we thus report annual outcomes relative to the point in time when benefit payments started. We refer to this point in time as the "start date of benefits".

[17] The DRV increases pensions in dependence of wage growth every year, resulting in an upward trend in average benefits at time of award over time. We correct for this trend by standardizing all pension to their Euro value of 2014. Further information is provided in Section A.1 of the Appendix

[18] The average increase in benefits that we estimate is in line with results from previous studies such as Krickl and Kruse, 2019.

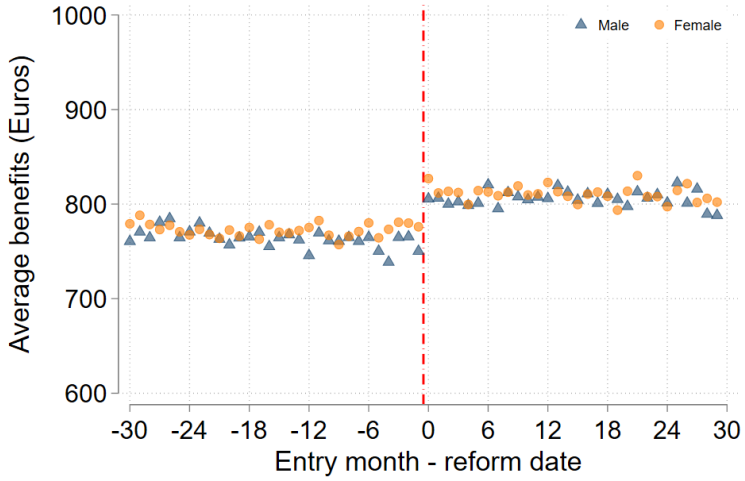


Figure 2: Average Monthly DI Benefits as a Function of Benefit Start Date Around the Reform

Note: The graph shows the average monthly benefits of DI recipients for two years before and after the reform. Benefits are reported in pre-tax terms and before social security contributions. Benefits are standardized to the pension credit values of 2014. The point estimate is 47.93 for the full sample, with a standard error of 6.89 (BW: 5.18). This constitutes a 6.2 % increase in benefits relative to the control mean of 764.93 Euros per month.

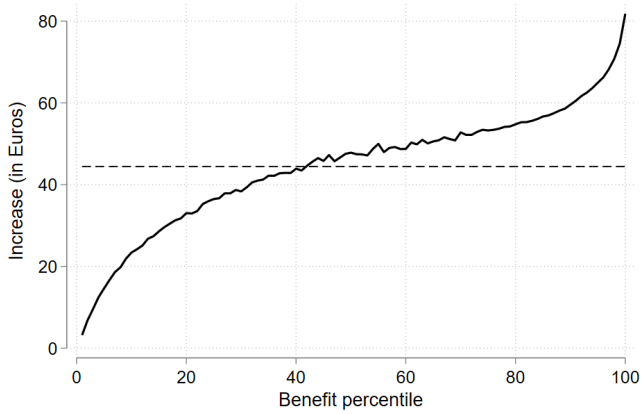


Figure 3: Reform Bonus by Benefit Percentile

Note: The figure shows the increase in DI benefits by pre-reform pension wealth percentile. Since the reform resulted in an a percentage increase of 5-6% relative to existing entitlements, the absolute Euro increase per month increases by percentile rank and ranges between almost no increase to 80 Euros per month. The dashed line marks the sample average increase in benefits. Benefits are standardized to pension values in the year 2014.

bonus of less than €20 per month, recipients in the highest decile receive around €70 more benefits. In our analysis, we pay attention to this by performing heterogeneity analyses that distinguish recipients by pre-reform pension entitlements.

4.2 Validity Checks

Our identification strategy relies on the assumption that individuals' potential outcomes are continuous throughout the reform cutoff. This implies that individuals cannot manipulate the start of their pensions. Before we turn to the discussion of the empirical results, we present various balancing checks and document that both the number of applicants and the composition of the applicants does not differ significantly at the cut-off date. We show the validity checks for the main estimation sample and also document that the checks hold for the full sample before implementing the data restriction (see Section 3). Thus, we can rule out that selection effects or postponement of entry pose a threat to our design to identify the income effect of an increase in benefit generosity.

Figure 4 shows that the sample density for both men and women is smooth around the introduction of the reform and that there is no bunching around the cutoff. These results indicate no selection into the post-reform group. In the Appendix (Table 6), we present the corresponding estimates from our RD design with the number of new recipients as the outcome variable. We show the results for our main estimation sample (row 1) alongside the subsamples of women (row 2) and men (row 3) in our data. In the last row of the table, we show the continuity of the density also holds for the full sample before we apply the sample restrictions described in Section 3.[19] Our specification remains continuous throughout the cutoff, independent of sample restriction decisions. Irrespective of functional form specifications, we find no statistically significant effect of the relevant point estimates, indicating that our sample size remains smooth throughout the reform.

To gain further confidence in the absence of selection, we conduct an additional analysis using institutional knowledge about the DI award process and data on the application dates of recipients. In short, we check whether there is a discontinuity in the time between the application and the entry into disability benefits. Successful manipulation of the benefit entry date would create a discontinuity in the time between the application

[19] Compared to the full sample, our main estimation sample shows a decline in density over time. The reason for this reduction in observation is the relative increase in DI recipients with pension entitlements from other countries, which we exclude for the sake of tractability in our analysis. Since this change is small and smooth over time, the exclusion of these recipients does not affect the validity of our RD design.

Figure 4: Smoothness of Sample Density Around Reform Cutoff

Note: The figure shows the density of observations in monthly bins around the reform date by gender. The number of observations appears continuous throughout the reform. Table 6 in the appendix reports the point estimates from the RDD regression for polynomials up to order three. None of the estimates are statistically significant, indicating that our sample size remains smooth throughout the reform.

and the entry date into disability benefits. In Section A.3 of the Appendix, we show the corresponding regression results, which further document that there is no selection in the timing of DI claims.

Next, we show that the composition of DI recipients does not change at the cutoff date. We check for qualitative differences between the pre-reform and post-reform samples by assessing the continuity of predetermined variables throughout the reform. Figure 5 shows the distribution of relevant covariates by gender: age at the start of pension, fraction whose primary diagnosis is a mental disorder, disease of the circulatory system or cancer diagnoses, last occupation before entry DI, and fraction of individuals who are receiving DI as a result of an application for a rehabilitation measure. The distribution of these covariates appears continuous through the reform threshold.

Table 1 reports the corresponding estimated discontinuity of these variables and further covariates at the reform date. Similar to Gelber et al. (2017), we perform an exercise that estimates the discontinuity in these covariates at different bandwidths and report the share of estimates that are statistically significant.

In Panel A of Table 1, we show the results for demographic characteristics available in the administrative data, including age at the start of benefits and gender. Both variables are continuous throughout the cutoff. Panel B focuses on the share of recipients by

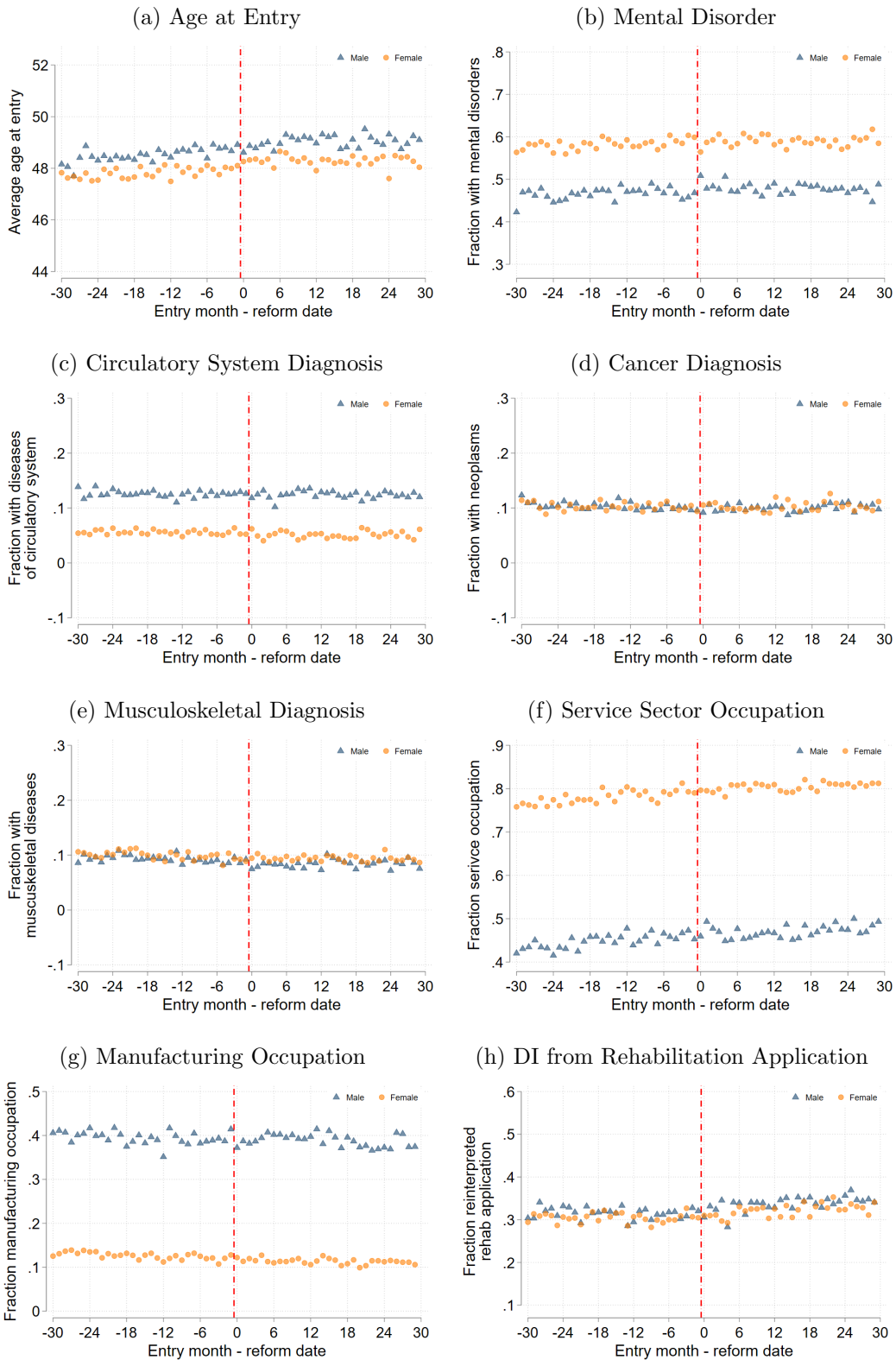


Figure 5: Continuity of Covariates Through the Reform

Note: The figure shows the distribution of predetermined covariance of the DI recipients in our sample who started their benefits between 2012 and 2016. Data is reported in monthly bins as a function of the distance from the reform date. Axis limits are set to a quarter standard deviation of each respective variable.

Table 1: Continuity of Covariates Throughout Cutoff

	Coefficient (SE)	Control Mean [Bandwidth]	Percent significant at 5-% level
Panel A. Demographic information			
Age	0.033	48.8	0.0
	(0.136)	[8.4]	
Female	0.004	0.53	0.0
	(0.01)	[6.24]	
Panel B. DI information			
Mental diagnosis	0.004	0.53	0.0
	(0.009)	[7.39]	
Circulatory diagnosis	-0.004	0.09	0.0
	(0.004)	[11.1]	
Neoplasm/Cancers	0.007	0.1	0.0
	(0.005)	[7.6]	
Musculoskeletal diagnosis	-0.002	0.09	0.0
	(0.004)	[10.75]	
Nervous system	0.001	0.06	0.0
	(0.004)	[7.61]	
Other diagnosis	-0.006	0.13	0.0
	(0.005)	[9.08]	
Panel C. Work history			
Full contribution times	2.858	261.93	33.0
	(2.115)	[7.18]	
Reduced contribution times	0.604	44.57	6.0
	(0.542)	[6.23]	
Service occupation	0.008	0.63	0.0
	(0.009)	[6.99]	
Manufacturing occupation	-0.017*	0.25	94.0
	(0.007)	[7.7]	
Technical occupation	0.005	0.03	11.0
	(0.003)	[8.27]	
Other occupation	0.007	0.09	0.0
	(0.006)	[5.33]	
DI benefits w/o reform bonus	10.836	761.29	11.0
	(6.758)	[5.1]	
Panel D. Rehabilitaion history			
Labor market rehabilitation	0.004	0.03	0.0
	(0.003)	[7.5]	
Medical rehabilitation	0.006	0.5	6.0
	(0.009)	[7.47]	
DI from rehabilitation application	-0.004	0.31	0.0
	(0.008)	[7.85]	
No consideration of employability	-0.002	0.84	0.0
	(0.007)	[6.26]	

Note: The table shows the RDD estimates for individual characteristics of new benefit recipients throughout the reform. The first column shows the point estimate and standard error (in parentheses). The second column documents the control mean and optimal bandwidth selected according to Calonico et al. (2014) for a maximum time frame of 30 months before and after the reform (in square brackets). The last column shows the share of estimates that are significant at the 5% level for a choice of 15 different bandwidths (from 3 to 18 months). The number of observations in our estimation sample is 29,664 at three months around the cutoff and 186,682 at 18 months around the cutoff. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

primary disability diagnosis. The most common diagnoses are mental disorders, which account for 55% of primary diagnoses. The share of mental diagnoses is higher in women (60%) than in men (48%). The second most common diagnoses are diseases of the circulatory system, neoplasms, and musculoskeletal conditions, which each making up around 10% of our sample. Importantly, the distribution of primary diagnoses remains continuous through the reform cutoff in our sample. The data also include information about working history (Panel C). In general, we find that the measures of working history do not differ at the cut-off. The only exception is the share of recipients holding a manufacturing occupation. However, the effect is fairly small and is driven by a strong difference between the months June and July, which does not carry over to other months around the reform date.[20] Aside from information about diagnoses, we also have access to information about the rehabilitation history of recipients (Panel D of Table 1). Rehabilitation measures are an important component of the German disability system, as applicants are usually required to participate in rehabilitation measures before they are considered for DI benefits. In turn, around 50% of recipients in our sample have participated in rehabilitation measures before the start of their disability benefits, and around 31% receive DI benefits instead or after the completion of a rehabilitation measure. For some individuals, pension insurance additionally checks employability before deciding whether to award full or partial disability benefits. However, for the majority of our sample (84%), only the health status is assessed. Again, we find no significant difference for these variables.

Overall, the absence of bunching around the reform threshold and the continuity of all predetermined variables except one give us confidence in the validity of the regression discontinuity design.

5 Results

In this section we discuss the estimation results. First, we focus on the labor market effects and show how an increase in the generosity of DI affects employment, earnings, and labor market transitions. Then, we turn to the results on mortality. In the final part of this section, we use the results of the RDD analysis to simulate the effect of the DI reform on household income and poverty risk.

[20] We run donut regressions to check for this and find no significant jump for any of the bandwidths once we exclude the months of June and July 2014.

5.1 Employment and Earnings

We distinguish between two types of employment - insured employment and marginal employment. Overall 25% of DI recipients are still employed. This underlines the importance of studying potential employment responses induced by a change in the generosity of benefits. About 21% of DI recipients work in marginal employment. Earning thresholds for marginal employment correspond directly to the earnings threshold for full DI benefits. Since these types of jobs only comprise a lower number of working hours each month and are largely exempted from social security payments, they are an attractive form of employment for DI recipients. The fraction in insured employment is considerably lower (about 5%).

In Figure 6, we provide graphical evidence of how the increase in disability benefits affects employment and related labor earnings. Specifically, we show the employment and earnings of DI recipients in the four years after the DI award, separately for men and women in insured and marginal employment. We compare the outcomes for DI recipients who entered DI in the period 30 months before and 30 months after the introduction of the reform on July 1, 2014. For all outcomes, the graphs do not indicate a discontinuity at the cut-off date. The employment shares and earnings have a very flat profile over the whole period. Thus, the graphical analysis suggests that the considerable increase in DI benefits does not affect the employment or earnings of DI recipients.

In Table 2, we show the corresponding effects from the regression specification outlined in Section 4 to test the effect of the reform on labor market outcomes more formally. In the first panel, we focus on the employment outcomes for the full population and separately by gender. In all specifications, the point estimates of the reform effects are very small and not statistically significant. Given the large sample size, standard errors are also small, so we can rule out that the insignificant effects are explained by low statistical power. Results for earnings are similar. Specifically, relative to the pre-reform, the point estimates in the two employment states are very small and never statistically significant. In combination with the findings of no employment effects, this suggests that the reform does not lead to changes in working hours or wages.[21]

We provide several robustness checks for our findings. In the main specification, we present the estimates for the optimal bandwidth following Calonico et al. (2014). In Figure 23 in the Appendix, we show that our results are robust to changes in the bandwidth. Irrespective of the bandwidth choice the employment and earnings effects

[21] Note again, the data does not include information about the number of working hours, so we cannot directly test for changes on the intensive margin.

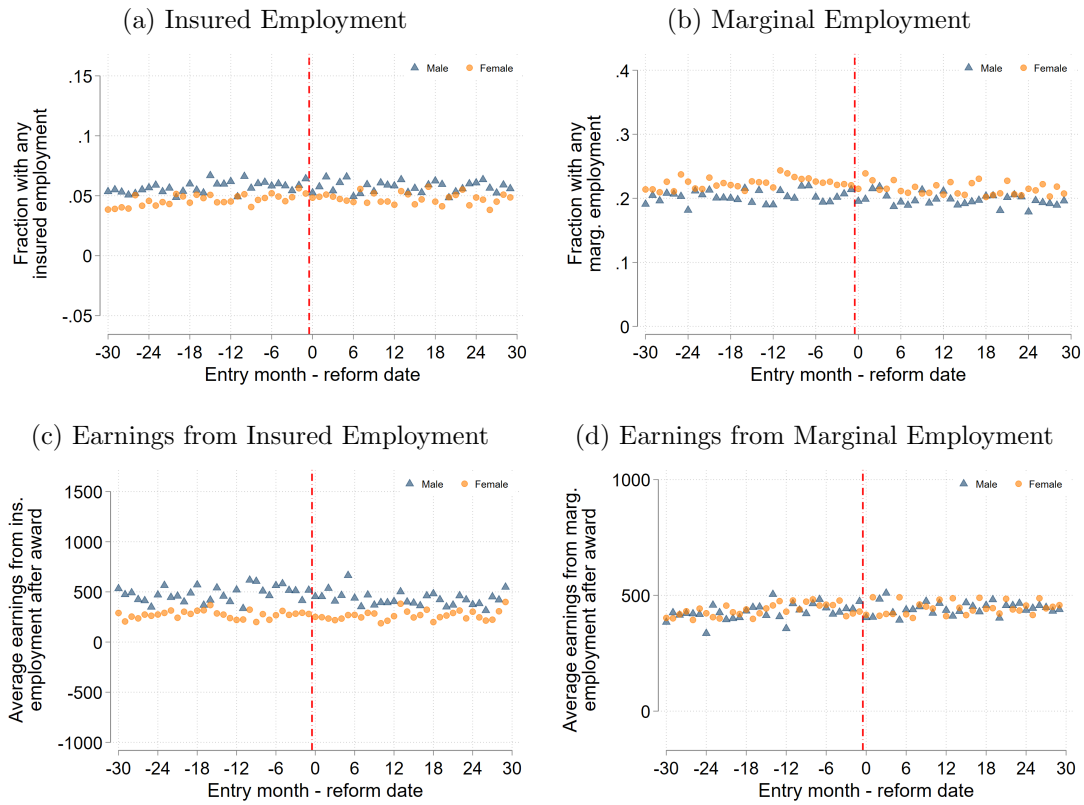


Figure 6: Employment and Earnings in the Four Years After Award of Benefits

Note: The figure shows the fraction of recipients in different employment types and average annual earnings within the four years after DI award. Data is reported in monthly bins as a function of the distance from the reform date. Axis limits are set to a quarter standard deviation of each respective variable.

are small and insignificant. We additionally inspect employment outcomes on an annual level (Table 7 and Table 8 in the Appendix) to check if our aggregated outcomes over the span of four years omit any potential dynamic effects. Lastly, we estimate the effects for the sample of permanent DI recipients, which we excluded from the main estimation sample. Permanent DI recipients are different from temporary recipients in two ways that may be relevant to our results. First, they are likely in worse health than our temporary sample, as the pension insurance only grants permanent benefits if they see no potential return to the labor market. In turn, working may have higher disutility for the average permanent DI recipient. On the other hand, permanent recipients do not need to apply for an extension of benefits and thus may feel more comfortable engaging in

Table 2: Effect of DI Benefit Increase on Employment

	All		Female		Male	
Outcome: Employment	(1)	(2)	(1)	(2)	(1)	(2)
Marginal Employment	0.004	0.004	0.008	0.011	-0.001	-0.003
	(0.0064)	(0.0065)	(0.009)	(0.0091)	(0.0086)	(0.0092)
	[9.9]	[9.3]	[9.5]	[9.1]	[11.3]	[9.7]
	0.21	*0.21*	*0.22*	*0.22*	*0.20*	*0.20*
Insured Employment	-0.003	-0.003	-0.004	-0.003	-0.002	-0.003
	(0.0033)	(0.0033)	(0.0043)	(0.0043)	(0.0049)	(0.0049)
	[11.0]	[11.0]	[11.3]	[11.1]	[11.6]	[11.6]
	0.05	*0.05*	*0.05*	*0.05*	*0.06*	*0.06*
Earnings Marginal Emp.	-1.334	-2.54	23.408	25.864	-23.953	-30.007
	(16.5261)	(16.5286)	(24.3615)	(24.3516)	(25.5136)	(27.5379)
	[11.2]	[11.0]	[9.3]	[9.2]	[10.7]	[9.0]
	432.38	*432.38*	*435.69*	*435.69*	*428.58*	*428.58*
Earnings Insured Emp.	-35.12	-33.208	-55.431	-51.167	-19.808	-18.272
	(45.7975)	(45.7035)	(38.9278)	(38.9497)	(83.6491)	(83.6578)
	[8.0]	[7.9]	[11.2]	[11.0]	[7.9]	[7.8]
	369.55	*369.55*	*273.32*	*273.32*	*479.7*	*479.7*
Controls	No	Yes	No	Yes	No	Yes

Note: Table shows the regression discontinuity estimates for employment and earnings in the four years after benefit award. The estimated model uses the monthly start date of benefits as the running variable and the reform date as the cutoff. The estimates for our baseline specification are shown in columns labeled (1). We additionally show the estimates controlling for the variables shown in Figure 5 in column (2). Bandwidths are selected according to Calonico et al. (2014) and are shown in square brackets. Standard errors are shown in parentheses. The control means are printed in italics. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

employment.[22] For individuals in our data, employment rates for permanent recipients are fairly similar to those with temporary benefits, with around 17% working in marginal employment after the DI award. Similar to our main specification, the results (Table 9 in the Appendix) indicate no effects of increased benefits on the employment behavior of permanent DI recipients. For both women and men, we find effect sizes close to zero that are statistically insignificant.

So far, we have only considered the average effect of the reform by gender. To identify potential heterogeneous effects, we split the sample across several dimensions. We estimate the effect for different age groups and by diagnoses of the DI recipients. Specif-

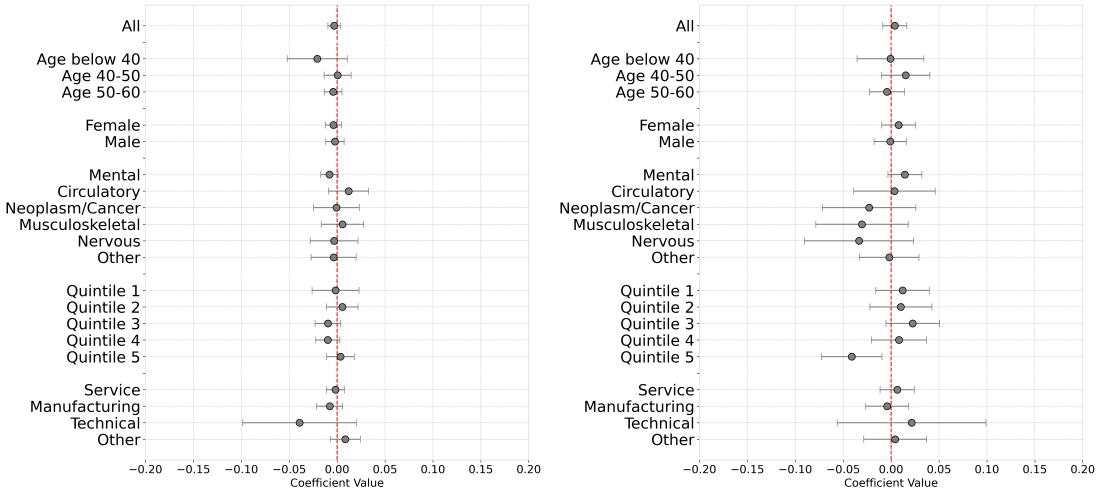
[22] They may still lose their benefits if the pension insurance determines that their health condition has improved.

ically, the data allow us to distinguish between mental disorders, disorders of circulatory systems, cancer, nervous system disorders, and other diseases. Depending on the diagnosis and age, it might be easier to respond to changes in financial incentives. Similarly, we differentiate by the last occupation before entering DI. Finally, we estimate effects by quintiles of pension wealth before entering DI. As discussed above, the treatment intensity of the reform increases with pension wealth.

The heterogeneous effects for insured and marginal employment are presented in Figure 7. The picture is very clear. While the point estimates slightly differ, in general we find no significant effect of the reform for any subgroup. The only exception is the effect on marginal employment for DI recipients in the highest quintile, which has the highest treatment intensity.[23] In line with the prediction of a negative income effect, we find a reduction in marginal employment in response to the increase in DI benefits for this group. According to the point estimate, DI recipients in this group reduce marginal employment by 0.05 p.p. In Figure 18 in the Appendix, we show that this negative effect is robust to the choice of bandwidth.

The empirical evidence that the increase in DI benefits has no effect on employment and earnings, except for a small employment reduction for the group with the highest treatment intensity, is remarkable. First, similar to other countries, a sizable fraction (about 25%) of DI recipients is still employed and potentially could respond to the changes in incentives. Second, in contrast to our findings, the previous literature often documents large labor market effects of DI reforms, which are linked to moral hazards and efficiency concerns regarding DI benefits (Chen and Van der Klaauw, 2008; Low and Pistaferri, 2015; Autor et al., 2016). This difference can be partly explained by differences in the policy environment. Whereas previous studies have, in general, analyzed labor market effects of program eligibility or related to substitution effects, we can identify changes induced by a change in the income effect. However, our results also differ from Gelber et al. (2017), who find significant responses related to an income effect in the context of DI in the US. The differences might be due to the institutional setting in the two countries and the different selection patterns into DI. For instance, the role of unemployment as an alternative pathway into retirement plays an important role in Germany (Geyer and Welteke, 2019) and other continental European countries. Moreover, the income effect in Gelber et al. (2017) is identified from variation at the upper bend point in the DI schedule, whereas in our context, the DI reform affects DI benefits of all recipients.

[23] The benefit increase in the highest quintile lies between €700 and €1000 annually.



(a) Insured Employment (b) Marginal Employment

Figure 7: Heterogeneity in Employment

Note: The figure shows the point estimates and confidence bands for employment outcomes across subsamples in our data. We split the data by age, gender, primary diagnosis, benefit quintile, and last occupation.

5.2 Return to the Labor Market

As an additional measure of incentive effect, we focus on the probability of returning to the labor market instead of remaining in DI. All individuals in our sample receive DI benefits on a temporary basis. Once benefits expire, recipients have to apply for an extension up to three times before benefits are awarded permanently. If this extension is not granted, individuals will need to go back to the labor market, either to employment or unemployment. Regulation requires temporary benefits to be paid for a maximum of three years, meaning that recipients receiving benefits for four years or more will have to have successfully applied for an extension.[24] The higher DI benefits increase the incentives to invest in the application for an extension of DI receipt and reduce the financial necessity to return to the labor market.

In general, retention of benefits for DI recipients is high, and transitions back to the labor market are rare (Drahs et al., 2022). In Figure 8, we show the probability of a transition to the labor market four years after the initial benefit receipt conditional on survival before and after the cut-off date of the benefits receipt. Overall, around

[24] For some of the older individuals in our sample, DI benefits will have been converted into old age benefits at age 63. However, since we only consider individuals below age 60 at the initial award of benefits, this is the case for a negligible fraction of our sample.

5% of recipients in our sample make a labor market transition conditional on survival. The rates are slightly lower for women than for men. The figure also provides suggestive evidence that after the reform, retention rates were slightly lower than before, specifically for women. This evidence is supported in the corresponding regression results, which we present for different years after receiving DI (Table 3). We find a persistent, significant negative effect on labor market transitions in years 4 and 5. After five years, individuals who entered DI with higher benefits are around 1 percentage point less likely to return to the labor market than those who entered DI before the reform. Relative to the low pre-reform means, this is a sizable reduction of about 20% for the full sample and, for women, even 25%. The significant effects are driven by women in our sample. We find no effects for men. The point estimates for men are smaller and only significant in the first year after the award, which, however, does not hold for smaller bandwidths.

Figure 25 in the Appendix shows that the results are robust to the choice of bandwidth. While we find significant effects for some bandwidths in some years shortly after becoming a recipient, the effect materializes only in the fourth year after the benefit award and remains significant for all choices of bandwidth thereafter. As mentioned above, benefits for temporary DI recipients are awarded for a maximum of three years. By the fourth year, this means everyone in our sample will have had to apply for a benefit extension with the pension insurance. Thus, labor market transitions are likely to be observed after the third year. The heterogeneity analysis furthermore reveals that results are driven by sub-samples of individuals with circulatory, musculoskeletal, and nervous system disorders. Interestingly, these diseases are slightly more likely to occur in men in our sample. This suggests that the effect we observe for women is likely not driven by gender differences in primary diagnoses (Figure 9).

As only a small portion of individuals in our sample makes a transition to the labor market, it is not possible to further quantify if individuals enter employment or another labor market status. In Figure 16 of the Appendix, we descriptively show the employment status of recipients after their return to the labor market independent of the DI reform. Most individuals return to insured employment or unemployment. A smaller portion works in marginal employment and receives sickness benefits or other transfers.

In summary, the increase in DI benefits induces no meaningful negative incentive effects in the short run but only in the long run. DI recipients with higher benefits do not reduce employment or earnings. Importantly, this holds for all subgroups and is independent of the DI diagnosis. We only find a small effect of recipients with the highest treatment intensity. In the longer run, i.e., four years after receiving DI benefits,

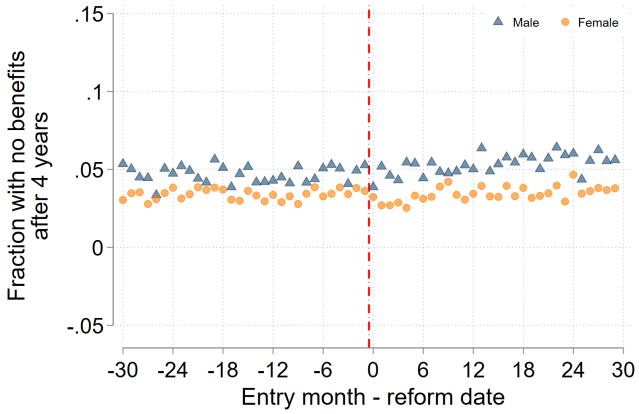


Figure 8: Fraction with No Benefits after 4 Years

Note: The figure shows the fraction of individuals without benefits 4 years after the initial award.

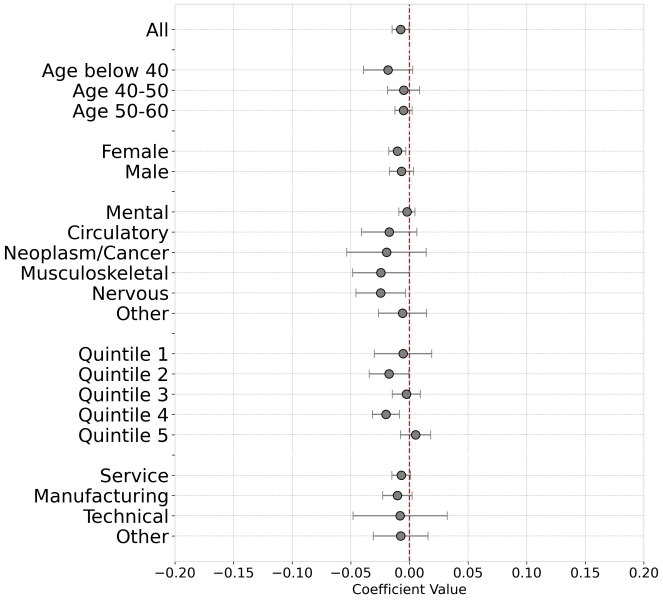


Figure 9: Heterogeneity in Return to the Labor Market

Note: The figure shows point estimates and confidence bands for the fraction of individuals without benefits 4 years after initial award across subsamples in our data.

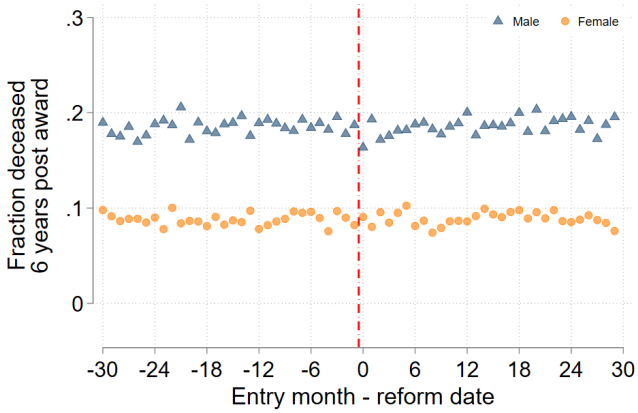


Figure 10: Fraction Deceased after 6 years

Note: The figure shows the fraction of individuals that have died 6 years post DI award.

we find a small but significant and persistent effect on the probability of returning to the labor market.

5.3 Mortality

In the following, we turn to the potential welfare effects of an increase in DI benefits. Similar to Black et al. (2018), García-Gómez and Gielen (2018), and Gelber et al. (2023) we interpret mortality as a welfare outcome and document if and how the reform has affected mortality rates.

Mortality is a central and relevant outcome that describes the welfare of DI recipients. A substantial share of DI recipients dies in the years following the award of benefits. Figure 17 in the Appendix shows the mortality rates of men and women in years after individuals start receiving benefits. On average, around 13% of recipients die within six years after initial DI receipt, with mortality being around twice as high for men than for women. These differences may largely be attributed to the differences in primary diagnoses between men and women. Female recipients are more likely to have a mental primary diagnosis, which has the lowest mortality rate. The highest mortality rate in our sample is observed for recipients with a cancer/neoplasm diagnosis. Around 40% of these recipients die within three years following the award of benefits, and after six years, only 55% remain alive. Individuals with mental disorders or musculoskeletal diseases have the lowest mortality rate (10% after 6 years) among all recipients we observe.

To study the effects of the benefit increase on mortality, we track the fraction of

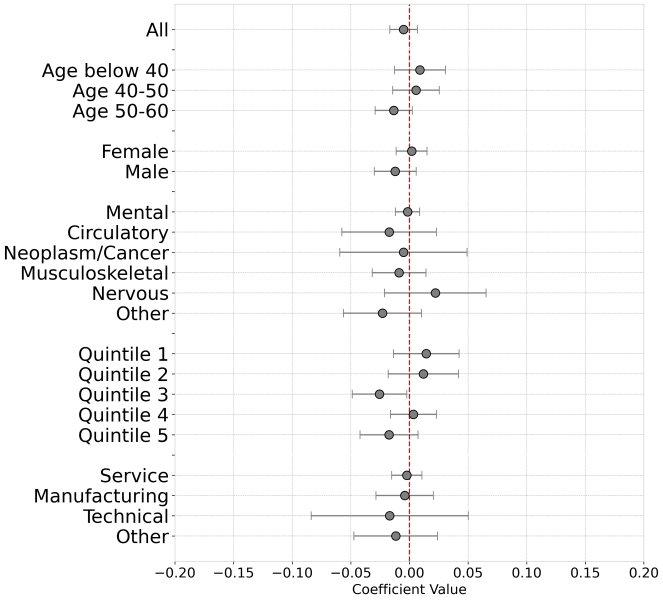


Figure 11: Heterogeneity in Mortality

Note: The figure shows point estimates and confidence bands across subsamples in our data.

individuals that die within the six years after they start receiving benefits and estimate the discontinuity at the reform cutoff using the specification outlined in Section 4. Figure 10 shows the graphical evidence for mortality up to six years after DI award. The distribution remains smooth throughout the reform cutoff, indicating that the increased benefits did not affect mortality. Before and after the reform, mortality rates for men are about 20% and for women 10%.

In Table 4, we provide evidence for an empirical test and show the results of the regression estimates for the fraction that dies after three and after six years, respectively. The point estimates are very close to zero and not statistically significant for any of the specifications we estimate, indicating that the increase in benefits induced by the DI reform did not affect mortality. As a robustness check, we estimate the effect on mortality for different choices of bandwidth. The estimates for each year after the DI award are presented in Figure 24 in the Appendix. The robustness check supports the results of our main specification: irrespective of the bandwidth choice, we find no significant effect on mortality.

We additionally estimate the effect for different subsamples to detect potential effect heterogeneity (Figure 11). We again split the sample by age, diagnoses, DI benefit quintile, and previous occupation. Across all of these stratifications, we estimate effect sizes

very close to zero that are not significant at the five percent level. Examining mortality by diagnosis is especially relevant in the context of DI due to the high discrepancy in baseline mortality rates in these subsamples. Still, we find estimates that are very close to zero for the group of recipients with mental diagnoses and musculoskeletal problems that have the lowest baseline mortality rate but also for individuals with cancer diagnoses, which have the highest mortality rate of all recipients. The zero effect for mental diagnoses is quite precisely estimated with small confidence bands, reflecting the relatively large sample size for this group of recipients. The standard errors for all other groups are larger due to smaller sample sizes. For individuals with a disease of the circulatory system or other diagnoses, we find a slight decrease in mortality of around 2 p.p., translating to a reduction of around 11%. However, the effects are not statistically significant.

Despite the meaningful increase in DI benefits and the high mortality risk of DI recipients, we can reject a significant effect on mortality. As discussed in the introduction, the scarce empirical evidence about the relationship between mortality and DI benefits is mixed and depends on the population. One important explanation for a positive effect of higher DI benefits in the US context is better access to medical services with higher DI benefits; see Gelber et al. (2023) or Black et al. (2018). This channel should not be relevant in a country with universal medical care with high standards and no or very low out-of-pocket costs. In Germany, DI recipients are fully covered by the public health care system, thus quality and access does not vary with the benefit level. Interestingly, Malavasi and Ye (2024) find significant mortality effects for old age pensioners with low pension entitlements in the Germany. However, the populations at study are clearly different. While Malavasi and Ye (2024) focus on individuals with low pensions and no entitlement for DI, we study mortality effects for individuals with low entitlements and severe health conditions. Combining the results of these two studies suggests that higher pension benefits can reduce mortality for individuals with low entitlements without severe health conditions but not for individuals with severe health conditions.

Table 3: Return to Labor Market

	All		Female		Male	
	(1)	(2)	(1)	(2)	(1)	(2)
Not receiving benefits after ...						
1 Year	0.002	0.002	0.002	0.002	-0.004*	-0.004*
	(0.0019)	(0.0019)	(0.0018)	(0.0018)	(0.002)	(0.002)
	[4.1]	[4.2]	[7.1]	[7.2]	[9.3]	[9.6]
	0.01	*0.01*	*0.01*	*0.01*	*0.01*	*0.01*
2 Years	-0.0	-0.0	-0.002	-0.002	-0.005	-0.005
	(0.003)	(0.0031)	(0.0032)	(0.0031)	(0.0032)	(0.0032)
	[4.9]	[4.8]	[6.4]	[6.7]	[11.2]	[11.4]
	0.02	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*
3 Years	-0.003	-0.002	-0.003	-0.003	-0.003	-0.003
	(0.0036)	(0.0036)	(0.0042)	(0.0041)	(0.0047)	(0.0047)
	[6.0]	[5.9]	[7.4]	[7.6]	[8.7]	[8.5]
	0.03	*0.03*	*0.03*	*0.03*	*0.04*	*0.04*
4 Years	-0.007*	-0.007	-0.01**	-0.01**	-0.007	-0.006
	(0.0037)	(0.0036)	(0.0037)	(0.0037)	(0.0052)	(0.0051)
	[7.5]	[7.4]	[10.9]	[10.6]	[9.4]	[9.3]
	0.04	*0.04*	*0.03*	*0.03*	*0.05*	*0.05*
5 Years	-0.01*	-0.01*	-0.012**	-0.012**	-0.012	-0.011
	(0.0041)	(0.0041)	(0.0039)	(0.0039)	(0.0064)	(0.0063)
	[6.8]	[6.7]	[11.5]	[11.2]	[7.3]	[7.4]
	0.05	*0.05*	*0.04*	*0.04*	*0.05*	*0.05*

Note: Table shows the regression discontinuity estimates for the fraction of individuals that are still receiving benefits up to 5 years of the initial benefit award. Bandwidths are selected according to Calonico et al. (2014) and shown in square brackets. Standard errors are shown in parentheses. Control means are printed in italics. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

Table 4: Effect of DI Benefit Increase on Mortality

Outcome:	All		Female		Male	
Fraction deceased ...	(1)	(2)	(1)	(2)	(1)	(2)
after 3 years	0.003	0.001	0.003	-0.001	-0.004	-0.001
	(0.0057)	(0.0051)	(0.0055)	(0.005)	(0.0078)	(0.0071)
	[5.4]	[5.7]	[7.4]	[7.6]	[7.9]	[8.3]
	0.08	*0.08*	*0.05*	*0.05*	*0.11*	*0.11*
N	42,641	51,041	31,134	35,724	32,275	32,275
after 6 years	-0.006	-0.007	0.002	-0.003	-0.012	-0.006
	(0.0066)	(0.0062)	(0.0066)	(0.006)	(0.0088)	(0.0074)
	[6.1]	[5.8]	[7.9]	[7.8]	[9.4]	[11.3]
	0.13	*0.13*	*0.09*	*0.09*	*0.19*	*0.19*
Controls	No	Yes	No	Yes	No	Yes
N	51,041	51,041	35,724	35,724	36,250	44,582

Note: Table shows the regression discontinuity estimates for the fraction of recipients that die in years after benefit award. The estimated model uses the monthly start date of benefits as the running variable and the reform date as the cutoff. We estimate the model without and with the control variables shown in Figure 5 and report the results for each subsample in columns (1) and (2) respectively. Bandwidths are selected according to Calonico et al. (2014) and are shown in square brackets. Standard errors are shown in parentheses. Control means are printed in italics. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

5.4 Poverty Risk

For the second welfare outcome we study the distributional effects of the increase in the DI benefits and focus specifically on the effect on poverty risks. Since the administrative data of the pension insurance do not contain information about the household composition or other sources of income, we cannot rely on the same data and the same identification strategy for this analysis. Instead, we leverage the results from the RDD analysis to conduct a simulation exercise of the effects of the DI reform on household income and poverty of DI benefit recipients.

We document that the increase in DI benefits did not significantly change the employment or earnings of DI recipients in the short run. The effect on labor market transitions is significant and sizable, but only present in the medium or long run; i.e., four years after the initial DI award. Moreover, mortality effects are not significant. Given that we do not find meaningful behavioral reactions to the reform in the short run, we use a static tax-and-transfer simulation to derive effects on income and poverty. For the simulation, we use data from the Socio-Economic Panel (SOEP) study, a household survey with rich information on household characteristics and income.[25]

We simulate the baseline scenario using pre-reform data from the year 2013. The data allow us to identify people who receive a disability pension before reaching the official retirement age. Due to sample size restrictions, we use the full population of disability pensioners in 2013. There are about 1.8 million recipients of disability pensions younger than 65 in the survey, which corresponds almost exactly to the total number of recipients in the administrative data (1.7 million).[26] To simulate disposable net income on the household level, we use the tax and transfer simulation model, which is described in Steiner et al. (2012). The model includes a detailed depiction of the German tax-and-transfer system including taxes, social security contributions, mean-tested transfers, and housing benefits.[27]

In general, the poverty risk among disability benefit recipients is about 30%, far above

[25] SOEP (2023) is a longitudinal representative survey of households and individuals living in Germany. In the 2020s, about 30,000 adults are interviewed living in about 20,000 households. For more information, see Goebel et al. (2019).

[26] In SOEP, we identify 536 individuals with disability pensions younger than 65 and apply the corresponding weighting factors to represent the 1.8 million recipients of disability pensions.

[27] We account for the relevant rules to derive gross pension income. First, pensioners pay contributions for long-term care insurance (in 2013: 2.05% and an additional 0.25% for people without children) and sickness insurance (8.2%). Second, a part of the pensions are subject to income taxation. Finally, pension income is included in the means test when calculating social assistance benefits and housing benefits.

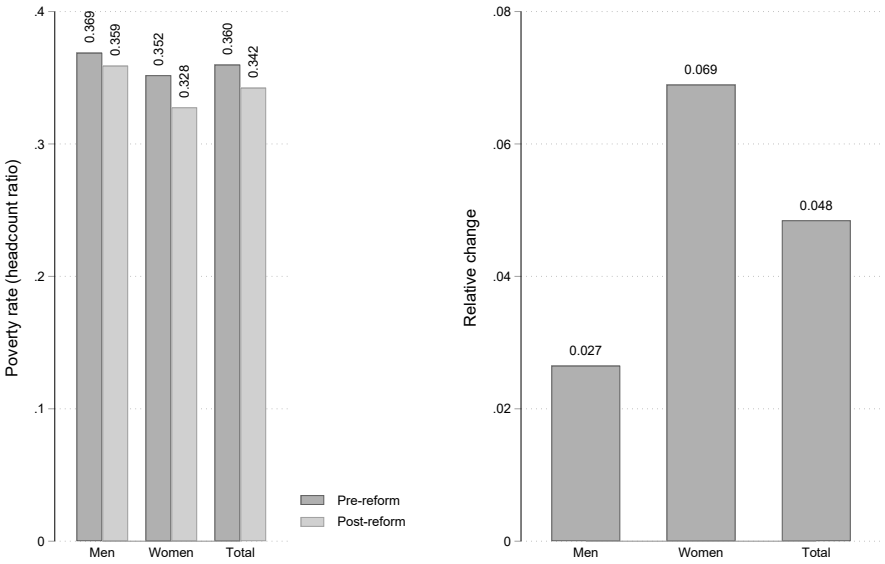


Figure 12: Poverty Risk Rate Before and After Reform

the population average, which is roughly 16%.[28] Moreover, 15% of disability pensioners receive means-tested social assistance transfers. Among old-age pensioners, the fraction is much lower at about 3%.[29]

In Figure 12, we show the pre- and post-reform poverty risk rate. The average pre-reform poverty risk rate is at 36%. Men have a slightly higher poverty risk: about 37% compared to 35% for women. The reform reduces the average poverty risk by nearly two percentage points or about 5%. The reduction in poverty risk differs between men and women. For women, the poverty risk decreases by 2.4 percentage points, or nearly 7%, while for men, the poverty risk is reduced by one percentage point, or 2.7%.[30]

[28] Individuals with poverty risk have a household income below 60% of the median income.

[29] See e.g. Märtin and Zollmann (2013), Märtin (2017), Geyer (2021), and Becker et al. (2023b). Official statistics only report rates for disability pensioners with permanent disability. This group has access to a different social assistance scheme ("Grundsicherung wegen Erwerbsminderung") than people with temporary benefits ("Hilfe zum Lebensunterhalt"). However, the level of benefits and the means tests are very similar.

[30] About 18% of the population does not benefit from the increase in benefits due to means testing. For this group, the change in pension income corresponds to a reduction in other public transfers. The largest part of our sample sees an increase in household income between 30 Euros and 40 Euros; it is for only very few that the increase exceeds 80 Euros per month.

6 Conclusion

In this paper, we analyze the importance of income effects for incentives and welfare in the context of DI benefits. Specifically, we document the impact of a policy change in Germany from 2014 that substantially increased benefit levels for new disability insurance recipients without changing eligibility criteria. Due to the institutional setting and the timing of the reform the number and composition of DI recipients did not change. Thus, we can rule out that results are affected by take-up behavior or substitution effects.

For the analysis, we use administrative data from the German pension insurance, which covers the universe of DI recipients. Since the administrative data does not include household information, we supplement our analysis with data from the German Socio-Economic Panel (SOEP) to simulate the effects on poverty.

Using a RDD we document that the increase in generosity of DI benefits does not affect the employment and earnings in regular and marginal employment of the DI recipients. About 25% of DI recipients are employed before and after the DI reform. Across heterogeneous groups, we find a consistent pattern of non-significant effects with point estimates close to zero. The only exception is a small negative employment effect for the group with the highest treatment intensity. However, in the longer run, i.e., four years after receiving DI benefits, we find sizable and significant effects on the probability of returning to the labor market. While the effects are large in relative terms, they only affect a small share of DI recipients.

The analysis also shows a very clear picture for mortality. Despite the meaningful increase in the generosity of benefits, mortality rates do not change. This holds for short run effects, i.e. after one year, and for long run effects after 6 years. Again, we do not find significant differences for subgroups. Independent of the health diagnosis, age or gender, mortality rates do not change when the generosity of DI benefits increases.

In contrast, we find sizable effects on poverty risk. Since we can rule out meaningful behavioral responses in the short run, we can derive these results focusing on the mechanical effect of the reform. Specifically, we simulate the effect of an increase in the DI benefits for the different groups and show that the risk of old-age poverty is reduced by 5%.

Following the intuition of the optimal design framework for DI benefits developed in Haller et al. (2024) our results provide support for the DI reform in terms of efficiency objectives. The increase in DI benefits does not induce meaningful behavioral responses for employment and earnings, while it only affects transition rates in the long run. The

transition effects are sizable but only for a small share of DI recipients. In contrast, the reform increases the disposable net household income of all DI recipients. Thus, our results support the conclusion of Haller et al. (2024), who show that stricter DI eligibility rules dominate reduced DI benefits as a policy tool for rolling back the DI program while keeping welfare concerns in mind.

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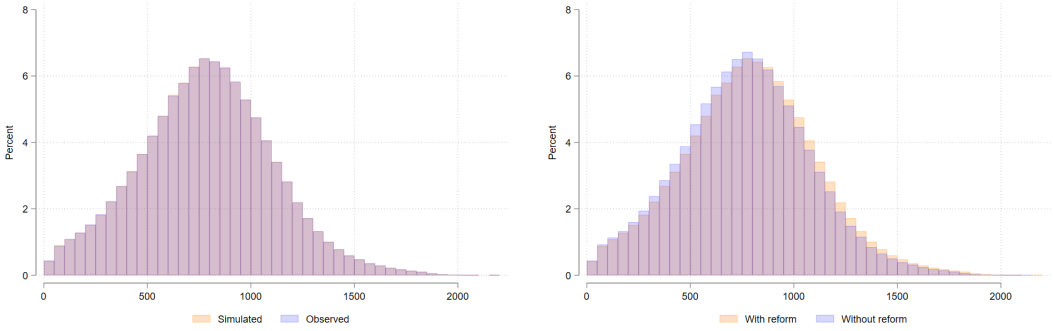


(a) Observed benefits compared to own calculations

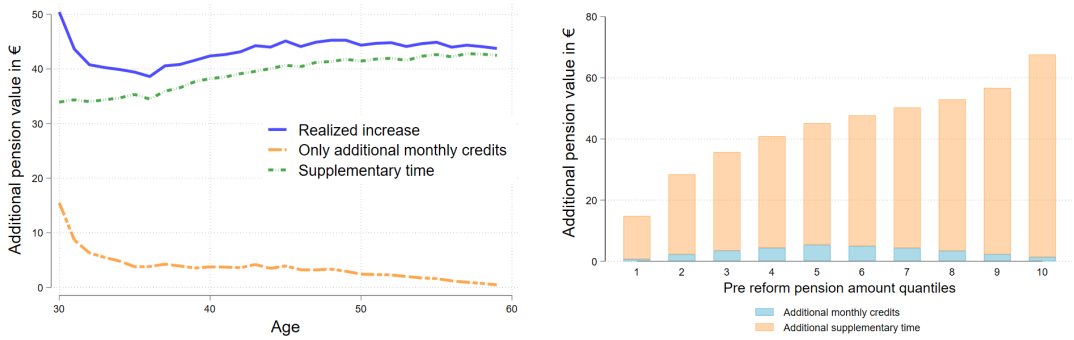
(b) Benefit distribution with and without reform

Appendix

A.1 Data

We clean our employment and earnings data as follows. Activities in the labor market are recorded in numbers of days. We distinguish between regular insured employment and marginal employment in our analysis. Marginal employment is a special type of employment contract in Germany that enables tax-free employment with monthly earnings up to 450 Euros (520 Euros since 2021) without mandatory social security contributions. This type of employment is especially attractive for disability benefit recipients since substantial gainful activity thresholds correspond to marginal employment thresholds. For our employment outcomes, we create dummy variables that are equal to one if an individual worked in an employment state within the four years after they have been accepted to disability insurance benefits. We also track employment on an annual level. For earnings, we also distinguish between marginal and regular insured employment. For marginal employment, the AKVS only contains contributions paid by the employer and (optional) contributions paid by the worker. We use the contributions to reconstruct earnings from the data. The resulting distributions of marginal earnings are depicted in Figure 21 in the Appendix. Earnings from insured employment are recorded in Euros. The earnings are right censored based on the yearly contribution assessment limit of the DRV (*Beitragsmessungsgrenze*).

We additionally standardize and recompute the pension entitlements of recipients in our sample based on information provided in the data. While the data provides us the Euro value of benefits awarded to recipients at the start of their DI spell, it makes sense to recompute benefits ourselves for multiple reasons. First, as all pensions in Germany



(a) Reform induced increase in pension wealth by age

(b) Pension wealth increase across pre-reform percentiles.

Figure 14: Increase in Pension Benefits

Note: Figure 14a shows the average increase in pension wealth compared to the pre-reform entitlements (blue line). We furthermore show how much of the increase can be attributed to additional supplementary time (green line) and the exclusion of the last 4 years (orange line) of employment history. Figure 14b shows the absolute benefit increase by pre-reform benefit decile.

are increased annually in response to wage growth, as benefits are recorded at the time of entry into benefits in our data, raw pension entitlements will increase over time. We correct this fact by standardizing all benefits to the value of a pension credits in the year 2014. Second, as the DI reform collides with a reform to mother's pensions, we observe an additional jump in benefits for mothers in the raw data that does not correspond to the actual benefit levels of our control and treatment groups. We correct for this by adding the value of one pension point per child born before 1992 to the benefit entitlements of mothers who started receiving benefits before the cutoff date of July 2014. This procedure follows the way the DRV implemented the reform for existing pension recipients in 2014 (Dünn and Stosberg, 2014). Lastly, we recompute benefits from scratch based on information on average pension credits per month and the age at award of DI benefits. We do so to precisely track the benefit increase for individuals in our sample and to construct income quintiles based on pre-reform entitlements. Figure 13a shows that this procedure allows us to perfectly compute entitlements from the insurance data.

Figure 13b shows the distribution of benefits with and without the reform bonus. Since the reform implemented a percentage increase in benefits, the post-reform distribution is shifted to the right.

Figure 3 in the main text displays the distribution of the increase in disability benefits

for recipients in our sample by hypothetical pre-reform pension benefit percentiles. The reform generated an approximate €45 benefit increase.[31] Since the reform increased the supplementary time (see Section 2 the treatment intensity strongly increases with the level of benefits. Those in the first decile received an approximate €10 increase, while individuals in the highest decile received a monthly benefit increase of around €70.

Figure 14a shows the distribution by age. Overall, the age pattern is relatively flat. However, there is some higher average increases amongst young disability benefit recipients. These age-related disparities in reform-driven benefit growth stem from the reform's twofold nature: illustrated by the green line, those under 60 benefited similarly from added supplementary time. Meanwhile, depicted by the orange line, younger individuals gained more from the adjusted valuation of supplementary time due to excluding a larger portion of their earnings history from the valuation compared to older individuals, as four years represents a larger share of their work history.

A.2 The 2014 Pension Reform Package

The increase in disability benefits we assess in this study was implemented as part of a larger reform package. We can rule out that the other two reform components interact with the DI reform and generate potential spillovers. Aside from the DI reform, the package included two major reform components. First, the package introduced an early route to old-age retirement. This change allowed individuals with 45 years of contributions to retire without deductions on their pensions at the age of 63 (see Dolls and Krolage (2023)). This additional route to early retirement is, however, not accessible for the population we consider in our sample. We only focus on individuals below the age of 60 (since older recipients did not receive the (full) additional supplementary credits). These individuals are not eligible for early old-age retirement. Furthermore, the new incentive for early retirement is only relevant for a very small share of individuals in our sample as it is conditional on having long contribution times. DI recipients exhibit below-average contribution histories and mostly cannot expect to qualify for these early retirement benefits.[32] The second relevant reform element in the package was a change to caregiver's pensions (Becker et al., 2023a; Artmann et al., 2023). This reform granted additional pension credits to parents of children born before 1992. This reform component affects almost exclusively women, as less than 1% of male disability

[31] The average increase in benefits that we find is in line with results from previous studies like Krickl and Kruse, 2019.

[32] For instance, only 16% of 59 year old's in our sample have enough relevant waiting time credited to their account to qualify for penalty-free early retirement if they continued to work until age 63.

benefit recipients have any contribution times for childcare in their account. Since the increase in caregiver credits changed benefits for existing as well as new DI recipients, the additional caregiver credits did not create any incentives to delay benefit claims for DI.

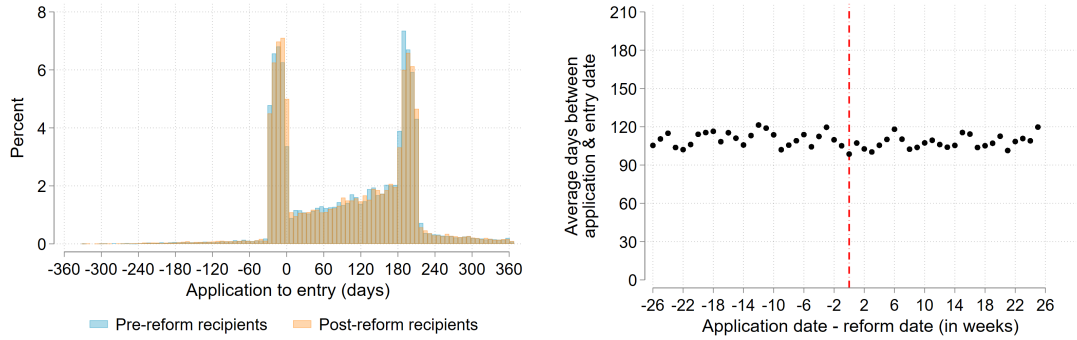
A.3 Additional Validity Check

This section presents additional manipulation checks we conduct in order to rule out selection into treatment in the context of the 2014 DI reform. As described in Section 2, the reform was passed rather quickly with less than half a year of time between the presentation of the first draft and the reform going into effect. The DI application process usually takes a considerable amount of time, making it manipulation around the reform threshold essentially impossible in our setting. As our data contains information on essential dates in the application process, we can conduct further validity checks using the data on applicants and institutional knowledge.

Three key dates are available in the dataset: the application date, the entry date, and date of the acceptance notice. Among these, the only date over which we expect individuals to have full control is the application date. The entry date of a pension is contingent on both the onset of the disability and the application date, as well as whether the pension is granted on a temporary or permanent basis. The general rule for pension applications with the DRV states that individuals must apply within three months after they become eligible for a pension to start receiving benefits right away (§ 99 SGB VI). If this deadline is missed, the pension starts in the month of application. This rule translates to permanent disability pensions but not temporary benefits. In the case of a temporary disability benefits, there is a waiting period so that benefits typically start in the seventh month after the individual becomes eligible (§ 101 SGB VI).

Notably, at the time of application, individuals do not know whether they will be granted a temporary or permanent pension if their application is successful. This circumstance provides only a limited time for selection during the relatively short period between January 2014 and July 2014, during which the reform was discussed but not yet passed. Applicants in this interval who are granted a temporary pension automatically belong to the post-reform group due to the seven-month waiting rule. We can check empirically for potential postponement by evaluating the average distance between application and entry date of recipients around the reform.

Figure 15a shows that the distribution of this variable is similar between the pre- and post-reform groups. We furthermore check for a discontinuity around the reform



(a) Distribution of distance between application and entry date

(b) Average distance between entry and application by application date

Figure 15: Distance between Application Date and Entry Date

Note: Figure 15a shows the distribution of the time distance measured in days between application date and start date of disability benefits for individuals who started receiving benefits in 2014. We plot the distribution in weekly bins and by treatment status. Figure 15b shows the average distance between application and entry date of a disability pension by application date in weekly bins for the reform year 2014.

date using the RDD specification described in Section 4. Importantly, and in contrast to our main specification, we use the application date instead of the entry date as the assignment variable in this specification, since we want to check for potential changes in application behavior. If recipients were to manipulate their start date by postponing their application until after the waiting period has passed, we would observe a downward discontinuity in the average distance between application and entry date. The reasoning behind this prediction is that the pension in this scenario would start in the same month that the application was issued instead of up to seven months later. Table 5 and Figure 15b show that the distance between application and entry date remains continuous throughout the reform date giving us further confidence in the validity of our design.

Table 5: Estimates for Manipulation of Entry Date

	All	Female	Male
Distance application to entry (in days)	-7.393	-9.352	-6.736
	(4.471)	(5.120)	(5.695)
	[7.4]	[10.9]	[9.1]
N	58,556	30,787	27,769
N effective	16,869	13,204	10,089

Note: The table shows the regression discontinuity estimates for the distance between entry and application date. The estimated model uses weekly application date as the running variable and the reform date as the cutoff. Bandwidths are selected according to Calonico et al. (2014) and are shown in square brackets. Standard errors are shown in parentheses. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

A.4 Result Appendix

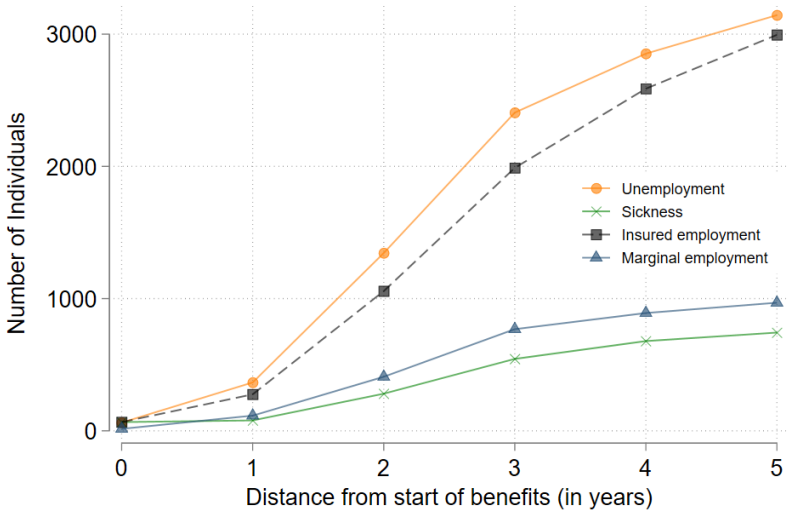


Figure 16: Employment Status after Return to the Labor Market

Note: The figure shows employment status of pre-reform recipients after they return to the labor market.

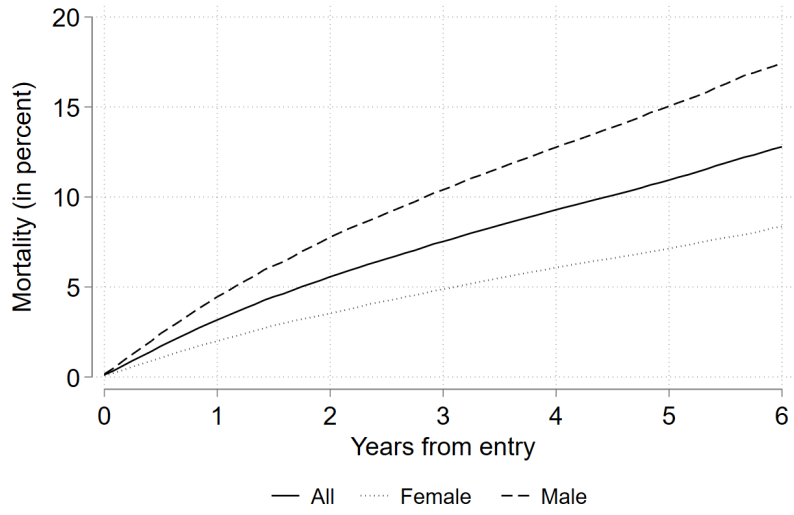


Figure 17: Fraction Deceased Over Time by Gender

The figure shows the fraction of pre-reform DI recipients that die in the six years after the award of benefits by gender of the recipient.

Table 6: Continuity of Density throughout the Reform Cutoff

Sample density at cutoff			
	(1)	(2)	(3)
Sample: All			
β	110.350	115.344	41.143
	(275.391)	(386.333)	(607.557)
Bandwidth	12.2	15.3	11.8
Control Mean		5,207	
	(1)	(2)	(3)
Sample: Female			
β	91.359	105.970	223.942
	(103.674)	(144.783)	(222.625)
Bandwidth	10	13	10
Control Mean		2,331	
	(1)	(2)	(3)
Sample: Male			
β	62.994	87.599	151.576
	(148.478)	(180.915)	(271.239)
Bandwidth	10	15	13
Control Mean		2,097	
Sample: No sampling restrictions			
β	1336.613	1457.246	1415.230
	(703.098)	(1113.263)	(1714.878)
Bandwidth	12.1	13.2	11.3
Control Mean		14,253	
N	60	60	60
Order polynomial	1	2	3

Note: Table shows the regression discontinuity estimates for the density of observations through the reform cutoff for polynomials up to order three. Bandwidths are selected according to Calonico et al. (2014). Standard errors are shown in parentheses. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 7: Effect of DI Benefit Increase on Annual Employment

Outcome:	All		Female		Male	
Employment after ...	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: Insured Employment						
1 Year	-0.002	-0.001	0.001	0.002	-0.005	-0.005
	(0.0038)	(0.0038)	(0.0041)	(0.0041)	(0.0048)	(0.0048)
	[6.1]	[6.1]	[8.1]	[8.0]	[9.7]	[9.6]
	0.04	*0.04*	*0.03*	*0.03*	*0.04*	*0.04*
2 Years	-0.001	-0.001	-0.002	-0.001	-0.002	-0.002
	(0.0041)	(0.0041)	(0.0042)	(0.0042)	(0.0054)	(0.0053)
	[5.6]	[5.6]	[8.1]	[8.1]	[8.2]	[8.2]
	0.04	*0.04*	*0.03*	*0.03*	*0.05*	*0.05*
3 Years	-0.005	-0.005	-0.008*	-0.007*	-0.007	-0.007
	(0.0039)	(0.0039)	(0.0034)	(0.0034)	(0.0053)	(0.0052)
	[6.3]	[6.4]	[12.4]	[12.3]	[9.0]	[9.0]
	0.04	*0.04*	*0.03*	*0.03*	*0.05*	*0.05*
4 Years	-0.002	-0.001	-0.003	-0.002	-0.006	-0.007
	(0.0042)	(0.0042)	(0.0042)	(0.0041)	(0.0054)	(0.0053)
	[5.7]	[5.7]	[8.6]	[8.8]	[9.5]	[9.5]
	0.04	*0.04*	*0.03*	*0.03*	*0.05*	*0.05*
Panel B: Marginal Employment						
1 Year	-0.004	-0.006	0.001	0.002	-0.002	-0.004
	(0.0064)	(0.0067)	(0.0078)	(0.008)	(0.0079)	(0.0079)
	[7.2]	[6.6]	[9.0]	[8.5]	[9.8]	[9.6]
	0.14	*0.14*	*0.15*	*0.15*	*0.14*	*0.14*
2 Years	0.005	0.005	0.01	0.012	-0.001	-0.001
	(0.0057)	(0.0061)	(0.0078)	(0.0078)	(0.0073)	(0.0076)
	[9.6]	[8.3]	[9.2]	[9.1]	[12.7]	[11.5]
	0.15	*0.15*	*0.15*	*0.15*	*0.15*	*0.15*
3 Years	0.0	-0.001	0.007	0.008	-0.008	-0.009
	(0.0058)	(0.0062)	(0.0081)	(0.008)	(0.0082)	(0.0084)
	[9.6]	[8.4]	[9.3]	[9.3]	[10.5]	[9.9]
	0.15	*0.15*	*0.16*	*0.16*	*0.15*	*0.15*
4 Years	0.007	0.007	0.021**	0.022**	-0.01	-0.011
	(0.0061)	(0.0058)	(0.008)	(0.0081)	(0.0086)	(0.0082)
	[9.2]	[10.1]	[9.7]	[9.2]	[10.3]	[11.2]
	0.15	*0.15*	*0.15*	*0.15*	*0.15*	*0.15*

Note: Table shows the regression discontinuity estimates for annual employment in the four years after benefit award. The estimated model uses the monthly start date of benefits as the running variable and the reform date as the cutoff. The estimates for our baseline specification are shown in columns labeled (1). We additionally show the estimates controlling for the variables shown in Figure 5 in column (2). Bandwidths are selected according to Calonico et al. (2014) and are shown in square brackets. Standard errors are shown in parentheses. The control means are printed in italics. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 8: Effect of DI Benefit Increase on Annual Earnings

Outcome:	All		Female		Male	
Earnings after ...	(1)	(2)	(1)	(2)	(1)	(2)
Panel A: Insured Employment						
1 Year	-22.967	-21.888	-62.069	-57.188	22.011	20.808
	(46.9089)	(46.8422)	(47.2515)	(48.1959)	(82.1529)	(82.5771)
	[8.0]	[7.9]	[8.8]	[8.5]	[8.2]	[8.0]
	295.42	*295.42*	*219.03*	*219.03*	*382.88*	*382.88*
2 Years	2.075	4.075	-0.777	2.448	-40.232	-40.627
	(55.9473)	(55.7407)	(52.2036)	(52.0824)	(95.4036)	(94.968)
	[6.2]	[6.2]	[8.5]	[8.4]	[6.7]	[6.7]
	343.92	*343.92*	*261.96*	*261.96*	*440.05*	*440.05*
3 Years	-15.814	-15.221	-59.127	-60.537	36.347	38.252
	(42.8945)	(43.3524)	(50.5397)	(49.0331)	(82.0698)	(81.8156)
	[9.0]	[8.8]	[8.7]	[9.1]	[7.4]	[7.4]
	330.7	*330.7*	*244.55*	*244.55*	*433.69*	*433.69*
4 Years	-82.479	-81.844	-77.68	-78.651	-85.834	-85.182
	(51.8359)	(52.0261)	(44.7514)	(45.818)	(94.2548)	(93.836)
	[6.7]	[6.7]	[11.3]	[10.8]	[6.3]	[6.3]
	306.23	*306.23*	*233.96*	*233.96*	*394.33*	*394.33*
Panel B: Marginal Employment						
1 Year	-7.452	-9.524	-3.92	-3.279	-10.258	-14.86
	(18.4128)	(19.4389)	(24.091)	(23.9891)	(26.0454)	(26.9876)
	[9.5]	[8.5]	[9.8]	[9.8]	[10.5]	[9.6]
	371.82	*371.82*	*376.3*	*376.3*	*366.7*	*366.7*
2 Year	2.016	-0.08	24.206	28.675	-12.018	-14.563
	(22.7013)	(22.9832)	(27.1085)	(27.8728)	(30.2355)	(30.3886)
	[7.4]	[7.1]	[9.0]	[8.6]	[9.4]	[9.1]
	432.18	*432.18*	*431.12*	*431.12*	*433.43*	*433.43*
3 Years	12.099	5.89	47.871	50.373	-28.503	-34.835
	(22.4813)	(24.0096)	(29.2807)	(29.3333)	(29.9608)	(30.99)
	[8.7]	[7.7]	[9.0]	[8.9]	[11.4]	[10.5]
	467.2	*467.2*	*465.2*	*465.2*	*469.58*	*469.58*
4 Years	8.317	9.456	50.534	53.949	-41.785	-42.204
	(23.807)	(21.1879)	(27.9639)	(28.5643)	(34.6215)	(31.6085)
	[8.9]	[11.1]	[11.2]	[10.7]	[9.8]	[11.5]
	493.27	*493.27*	*488.75*	*488.75*	*498.79*	*498.79*

Note: Table shows the regression discontinuity estimates for annual earnings in the four years after benefit award. The estimated model uses the monthly start date of benefits as the running variable and the reform date as the cutoff. The estimates for our baseline specification are shown in columns labeled (1). We additionally show the estimates controlling for the variables shown in Figure 5 in column (2). Bandwidths are selected according to Calonico et al. (2014) and are shown in square brackets. Standard errors are shown in parentheses. The control means are printed in italics. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

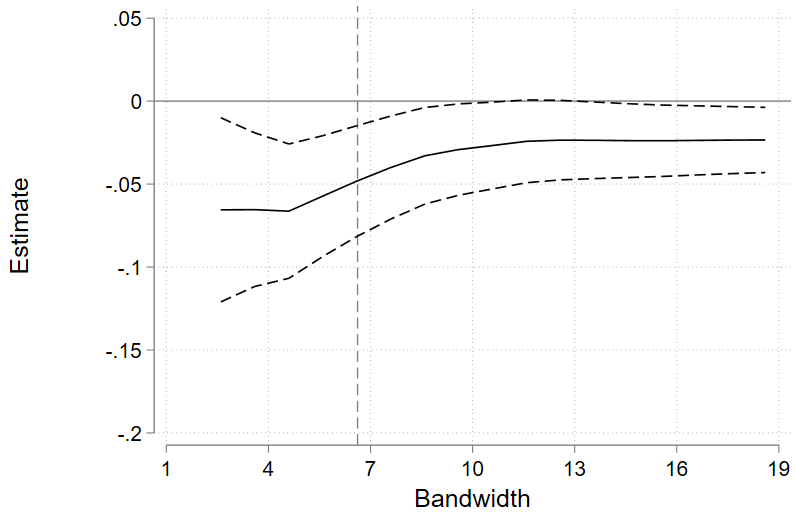
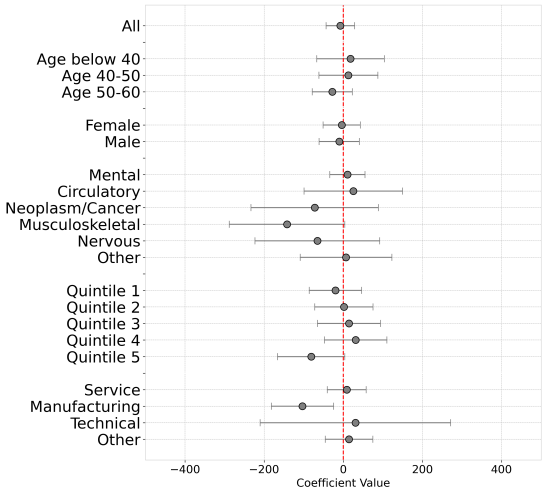
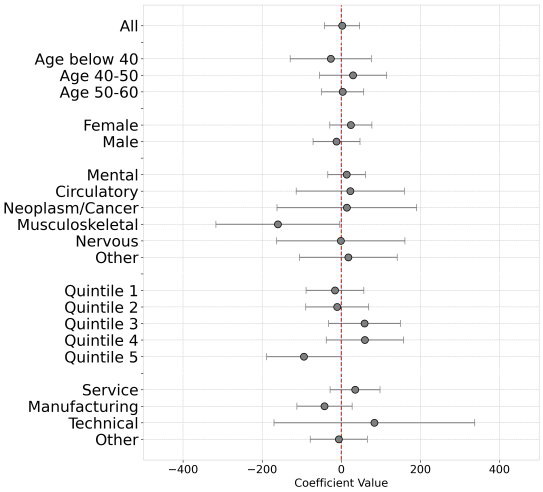


Figure 18: Bandwidth sensitivity of Marginal Employment Estimates for High Benefit Recipients

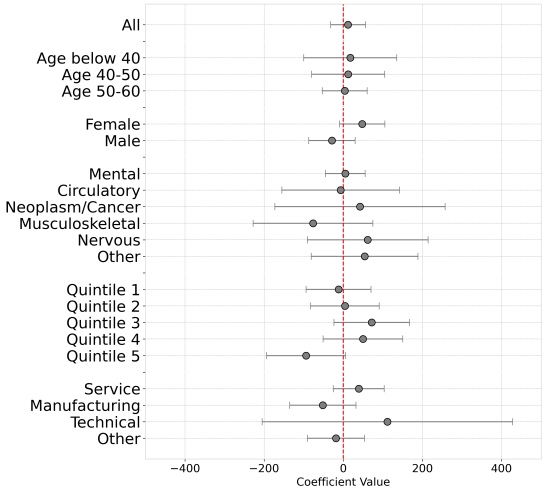
Note: Figure shows bandwidth sensitivity of the estimated effect for the subsample of high benefit recipients (5th quintile).



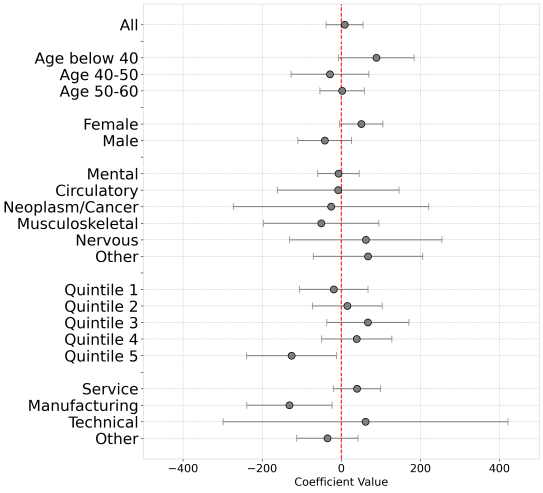
(a) Year 1



(b) Year 2

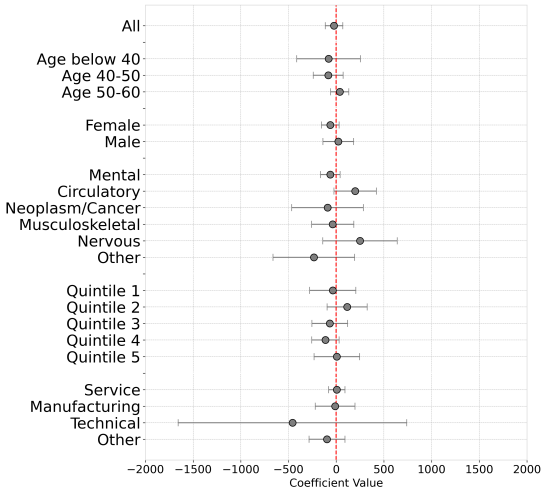


(c) Year 3

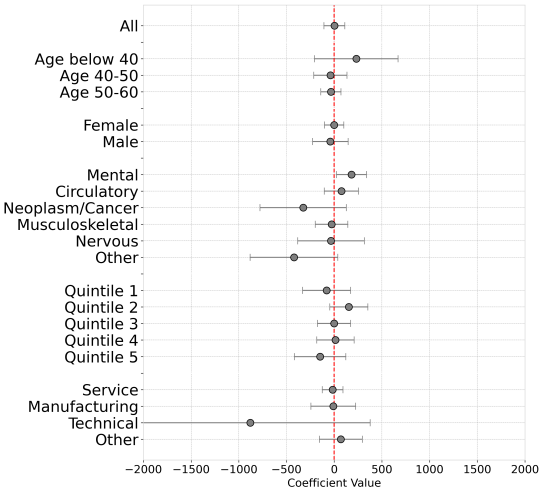


(d) Year 4

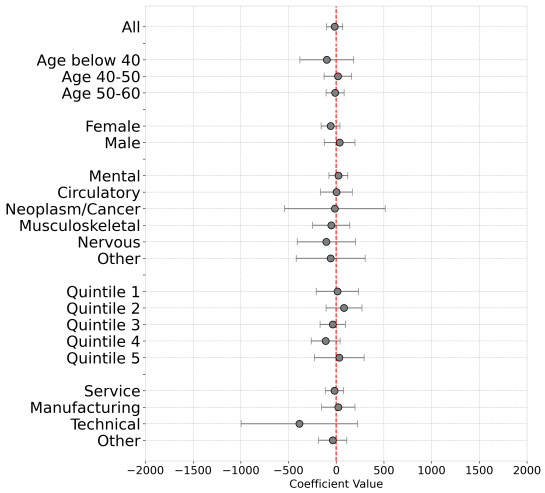
Figure 19: Heterogeneity in Earnings from Marginal Employment



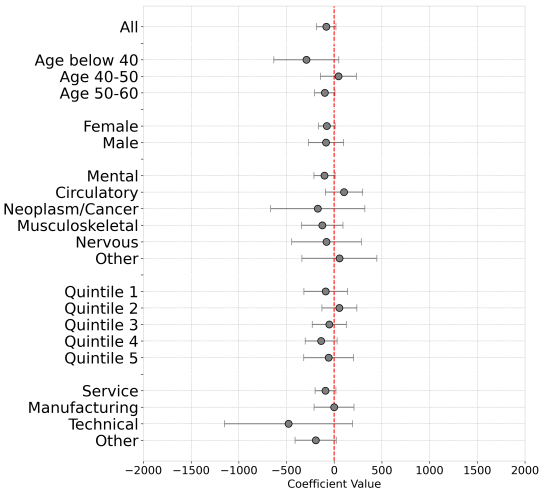
(a) Year 1



(b) Year 2



(c) Year 3



(d) Year 4

Figure 20: Heterogeneity in Earnings from Insured Employment

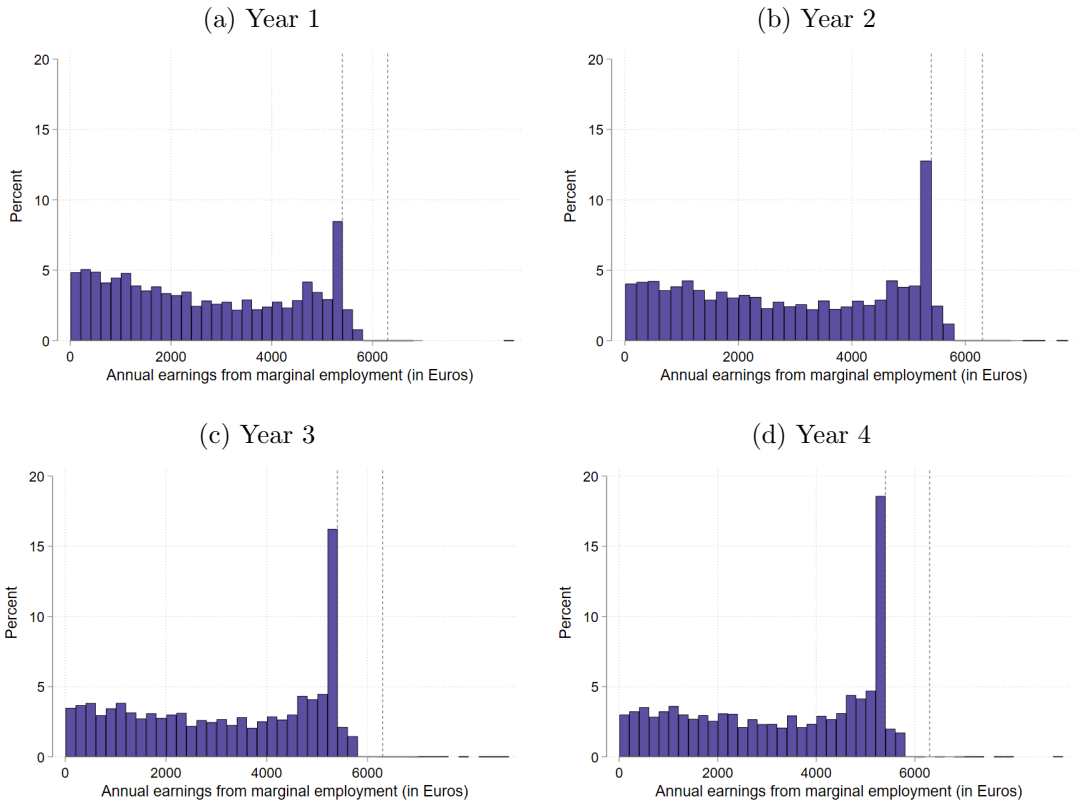


Figure 21: Distribution of Earnings from Marginal Employment

Note: The figure shows annual earnings from marginal employment in the four years following the award of DI benefits. Earnings are shown in bins of € 50. The dashed line to the left marks the regular earning limit for marginal employment of € 5400. The dashed line to the right marks the limit of € 6300, which corresponds to the earning limit for full disability benefit recipients as well as the hard limit for marginal employment, including expectations for exceeding the monthly earnings limit of € 450 on an irregular basis.

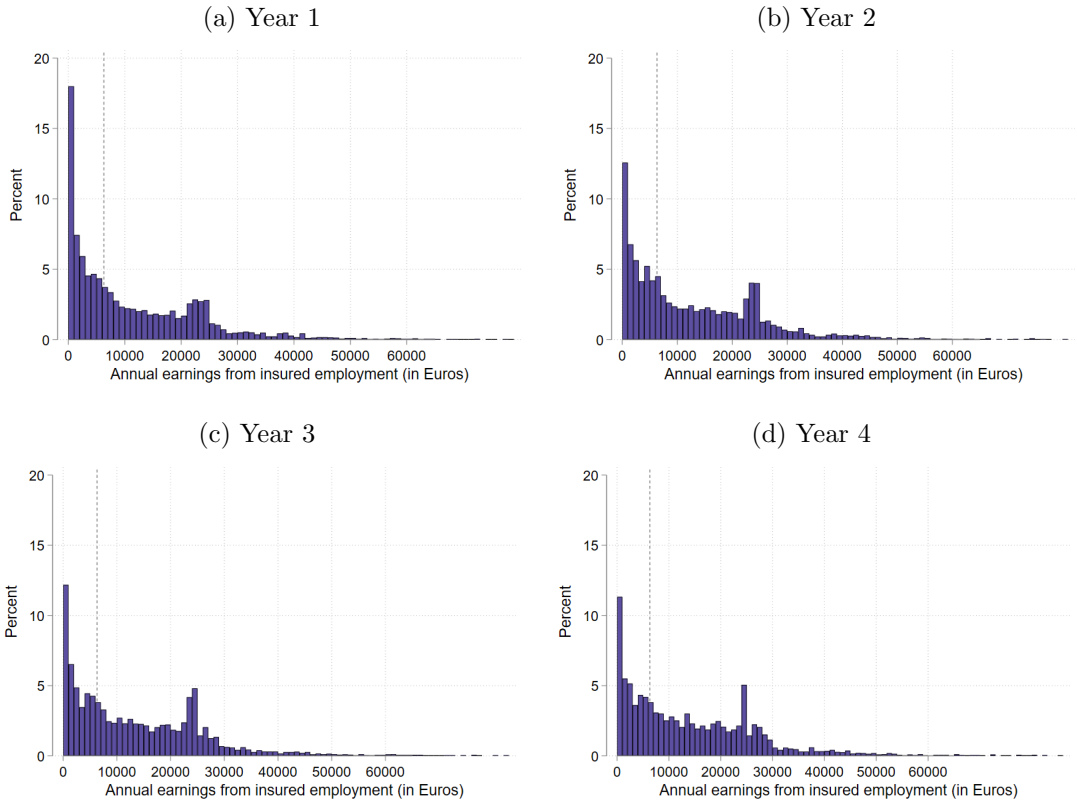


Figure 22: Distribution of Earnings from Insured Employment

Note: The figure shows annual earnings from insured employment in the four years following the start of DI benefits. Earnings are shown in bins of €50. The dashed line to the right marks the limit of €6300, which corresponds to the earning limit for full disability benefit recipients.

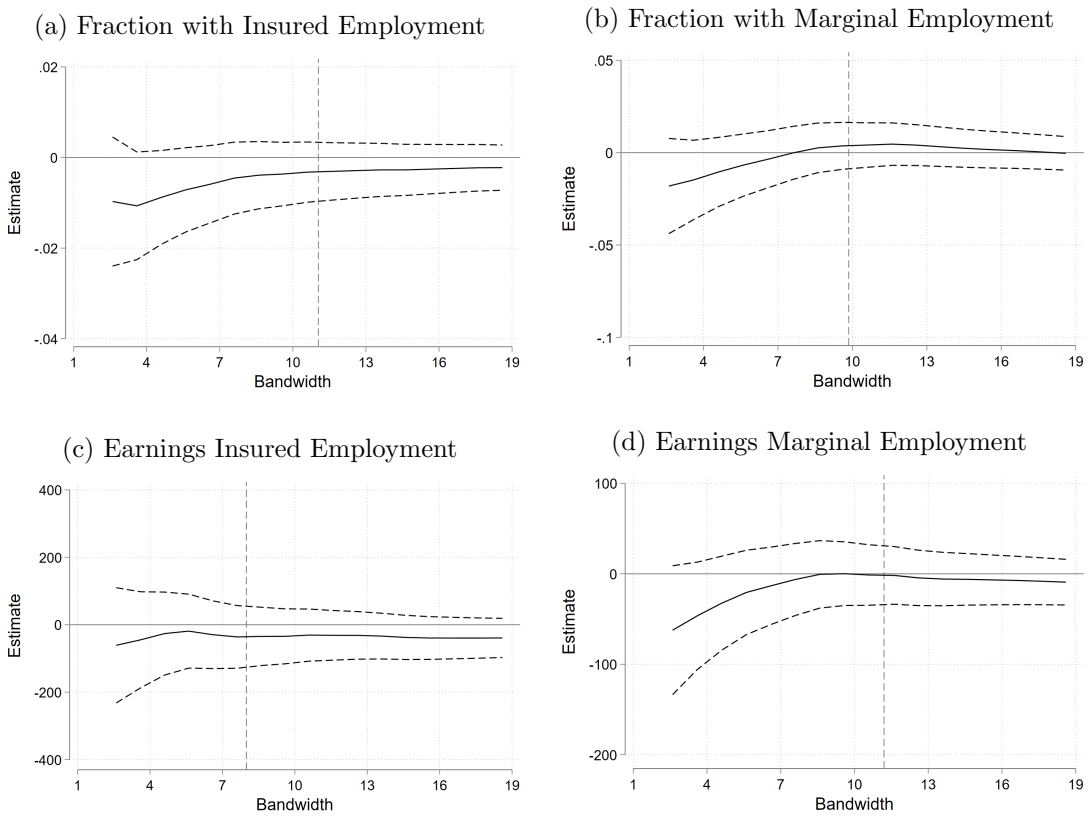
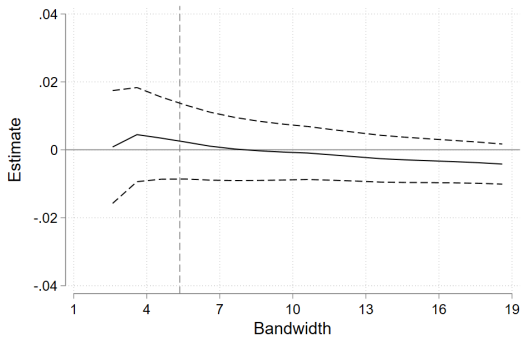


Figure 23: Bandwidth Sensitivity of Employment Estimates

Note: The figure shows the estimates for employment outcomes in the four years after individuals start receiving benefits. We plot the estimates for several choices of bandwidth, indicated on the horizontal axis. The solid line indicates the point estimates, the dashed lines indicate 95% confidence intervals. The vertical dashed line marks the point estimate at the optimal bandwidth.

(a) Fraction Deceased after 3 Years



(b) Fraction Deceased after 6 Years

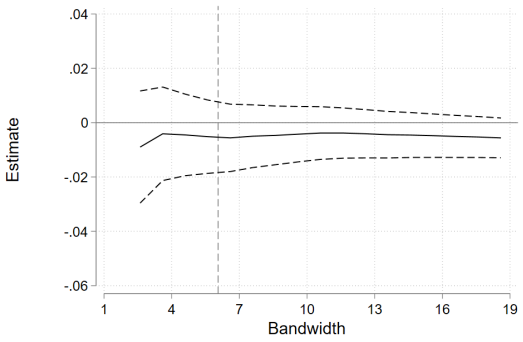
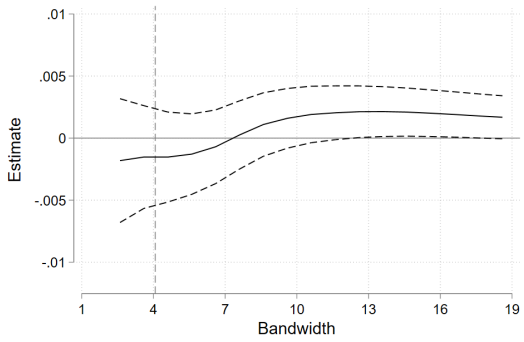


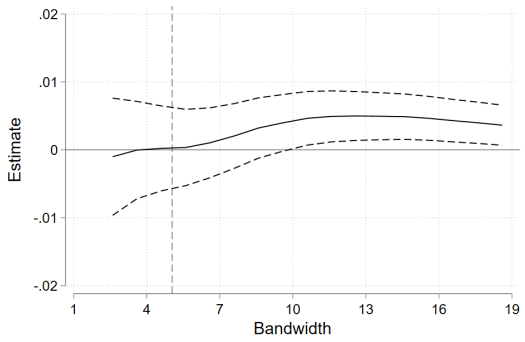
Figure 24: Bandwidth Sensitivity of Mortality Estimates

Note: The figure shows the estimates for recipient mortality in the six years after individuals start receiving benefits. We plot the estimates for several choices of bandwidth, indicated on the horizontal axis. The solid line indicates the point estimates, 95% confidence intervals are indicated by the dashed lines. The vertical dashed line marks the point estimate at the optimal bandwidth.

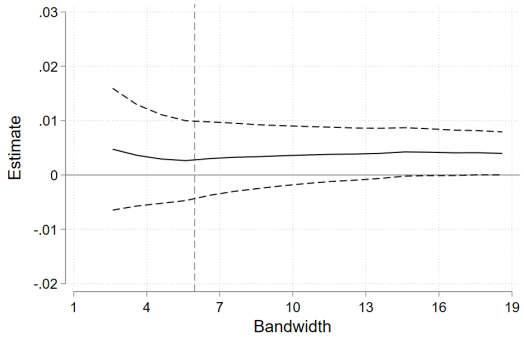
(a) Fraction Retaining Benefits after 1 Year



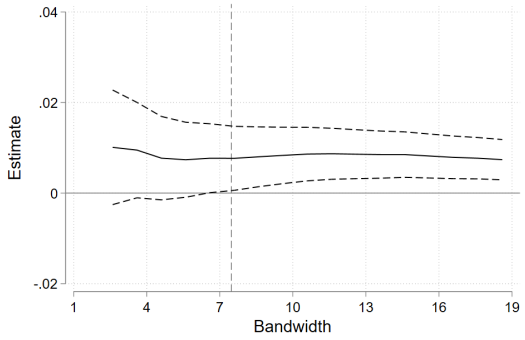
(b) Fraction Retaining Benefits after 2 Years



(c) Fraction Retaining Benefits after 3 Years



(d) Fraction Retaining Benefits after 4 Years



(e) Fraction Retaining Benefits after 5 Years

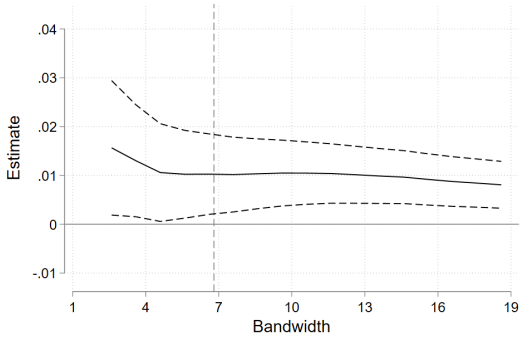


Figure 25: Bandwidth Sensitivity of Benefit Retention Estimates

Note: The figure shows the estimates for benefit retention in the five years after individuals start receiving benefits. We plot the estimates for several choices of bandwidth, indicated on the horizontal axis. The solid line indicates the point estimates, 95% confidence intervals are indicated by the dashed lines. The vertical dashed line marks the point estimate at the optimal bandwidth.

Table 9: Effect of DI Benefit Increase on Permanent DI Recipients

Outcome:	All		Female		Male	
	(1)	(2)	(1)	(2)	(1)	(2)
Marginal Employment	-0.001	-0.002	-0.007	-0.008	0.006	0.005
	(0.0083)	(0.0083)	(0.011)	(0.0109)	(0.0117)	(0.0116)
	[10.2]	[9.9]	[11.7]	[11.8]	[10.2]	[10.0]
	0.17	*0.17*	*0.17*	*0.17*	*0.17*	*0.17*
	37,812	37,812	22,157	22,157	19,504	19,504
Regular Employment	-0.004	-0.003	0.002	0.002	-0.008	-0.007
	(0.0049)	(0.0047)	(0.0057)	(0.0054)	(0.0072)	(0.0069)
	[6.8]	[7.2]	[8.3]	[9.0]	[7.1]	[7.6]
	0.03	*0.03*	*0.02*	*0.02*	*0.03*	*0.03*
N	26,189	26,189	14,526	16,397	13,600	15,582
Benefit Retention after 5 Years	0.0002	0.0002	-0.0018	-0.0018	0.0031	0.0029
	(0.001)	(0.001)	(0.0012)	(0.0012)	(0.0018)	(0.0018)
	[10.5]	[10.5]	[9.3]	[9.3]	[8.6]	[8.6]
	0.998	*0.998*	*0.999*	*0.999*	*0.998*	*0.998*
N	31,352	31,352	13,982	13,982	14,126	15,126
Mortality after 6 Years	-0.0067	0.0006	-0.0042	0.0017	-0.0093	0.0012
	(0.0093)	(0.0086)	(0.0126)	(0.0112)	(0.0133)	(0.0119)
	[10.9]	[8.9]	[11.8]	[9.1]	[10.8]	[10.2]
	0.34	*0.34*	*0.29*	*0.29*	*0.38*	*0.38*
N	50,170	40,785	26,016	19,263	26,438	23,953
Controls	No	Yes	No	Yes	No	Yes

Note: Table shows the regression discontinuity estimates for our main outcomes for permanent DI recipients. Since almost no permanent recipients get off DI benefits, we show the effect of the benefit increase on remaining a recipient (the inverse of the outcome we show in our main estimates for returns to the labor market). The estimated model uses the monthly start date of benefits as the running variable and the reform date as the cutoff. We estimate the model without and with the control variables shown in Figure 5 and report the results for each subsample in columns (1) and (2), respectively. Bandwidths are selected according to Calonico et al. (2014) and are shown in square brackets. Standard errors are shown in parentheses. Control means are printed in italics. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$