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Incentive Effects of Disability Benefits*

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We provide novel evidence on the trade-off between insurance and incentives when adjusting disability insurance (DI) benefit generosity using a comprehensive measure that encompasses not only the effect on take-up but also behavioral responses of DI recipients with respect to employment and exit from DI. Based on administrative data from the German pension insurance and exogenous policy variation, we identify the relevant behavioral margins induced by a change in benefit generosity. Using a theoretical framework, we show that our comprehensive measure of incentive effects implies a fiscal multiplier of 1.83. Incorporating elasticities with respect to exit from DI increases the fiscal multiplier compared to estimates that only account for take-up elasticities. In the context of the model, we estimate that increasing benefits is welfare improving, given the insurance effects of DI benefits estimated in previous literature.

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

JEL classification: H55, I12, J22, J26

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1 Introduction

Disability insurance (DI) programs are a key component of social security systems around the world. These programs provide income protection for workers whose ability to earn is reduced or lost due to health problems. Despite their role in providing insurance and social protection, disability benefit programs are criticized for generating inefficiencies in the labor market. Such inefficiencies can arise when financial incentives, rather than health conditions, drive the take-up of disability benefits (Low & Pistaferri, 2015). The literature has primarily focused on inefficiencies related to the generosity of DI programs by quantifying the take-up effect of benefits. In this paper, we propose a more comprehensive measure of inefficiency that encompasses not only the effect on DI claims but also the behavioral responses of DI recipients after they start receiving benefits. Two key margins are central to our analysis: employment during DI receipt and return to the labor market. As documented in Kostøl and Mogstad (2014), DI recipients often retain considerable work capacity, which can be effectively activated through financial work incentives.

In the first part of the paper, we extend the theoretical framework of Haller et al. (2024), which provides a sufficient statistics framework for a welfare analysis of DI design, to analyze how a comprehensive incentive measure incorporating take-up, employment, and exit from DI, shapes the trade-off between insurance and incentives when adjusting DI benefit generosity. We demonstrate that these additional behavioral margins increase incentive costs, driven by their respective elasticities.

In the second part, we use administrative data from the German pension insurance, which cover the universe of DI recipients, to provide empirical estimates of the three behavioral effects. We present further empirical results that potentially affect the trade-off between insurance and incentives. While the data do not contain sufficient information to directly identify the insurance effect of changes DI generosity, we follow Black et al. (2018), García-Gómez and Gielen (2018), and Gelber et al. (2023) in estimating how the generosity of DI benefits affects the mortality of DI recipients. A reduction in mortality can be interpreted as a non-monetary welfare effect. At the same time, a longer life also has direct effects on fiscal costs via a longer benefit payment period.

For identification, we exploit unique policy variation in Germany that allows us to isolate the different behavioral margins induced by a change in benefit generosity. On July 1, 2014, the German government substantially increased benefits for new recipients of disability benefits. On average, DI benefits for eligible individuals rose by more than

6%, corresponding to an increase of €565 per year for the average DI recipient in 2014. The magnitude of the reform is comparable to the value of two additional years of contributions to the pension system, of which DI is a part in Germany.

We use an event study design to estimate the effect of the reform on DI take-up by computing the number of claims as a share of all active insurance accounts in Germany that potentially have access to claiming disability benefits. We use a procedure similar to Mastrobuoni (2009) and compare post-reform take-up to pre-reform trends.

To identify the behavioral effects of DI recipients, we first focus on the subgroup of recipients with temporary DI, leveraging the announcement and timing of the DI reform. In additional analyses, we also include permanent DI recipients and show that the results are very similar. By default, disability benefits are granted for a temporary period of three years with the possibility of extension. Permanent DI is only given if medical recovery is unlikely. For temporary DI, a six-month waiting period applies after the application date. Since the reform was announced shortly before its implementation, individuals in the waiting period could not manipulate their starting date. Consistent with these regulations, we empirically rule out take-up behavior affecting the number or composition of temporary DI recipients around the reform's eligibility cut-off date.

We use the policy variation in a Regression Discontinuity Design (RDD) to isolate the causal effects of the increase in benefits. We analyze the incentive effects and estimate how the increase in DI benefits affects the employment and earnings of DI recipients. We then quantify the return effect by estimating the probability of DI recipients returning to the labor market instead of extending DI. Second, we study the effect on mortality of DI recipients. To understand the distributional implications of the DI reform on employment, earnings, labor market return, and mortality, we study heterogeneous effects along relevant dimensions. We separately estimate effects by gender, age, treatment intensity, previous occupation, and by the primary diagnoses of the DI recipients.

Based on the empirical analyses, we document meaningful incentive effects of the increase in DI benefits in two important dimensions. In line with previous studies (e.g. Mullen and Staubli (2016) or Gruber (2000)), we find significant and positive take-up effects. Overall, we estimate a take-up elasticity of 0.5, which implies that an increase of DI benefits of 1% increases take-up of DI by 0.5%.

In contrast, the results show on average no significant effect on employment and earnings of DI recipients. In more detail, we document 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 negative employment effect for the group with the largest treatment intensity. For this group, we quantify an income effect of around -0.13 Euros per one Euro increase in DI benefits, which is comparable to estimates by Gelber et al., 2017 for a comparable subgroup in the US. Our results are robust to various specification checks, including variations in the bandwidth.

The exit elasticity, measuring the probability of returning to the labor market, however, is significant and of meaningful size. Consistent 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 have applied for an extension of benefits, we find a significant and persistent effect on labor market transitions. The probability remaining on benefits past the initial granting period increases by 0.08 percentage points. Relative to the pre-reform mean of about 95%, this is an increase of around 0.8%. In the fifth year after DI award, the effect is around 1.2%. This corresponds to an average elasticity of 0.17. 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 provides a clear picture regarding mortality. Despite the high mortality risk among DI recipients and the increase in benefit generosity, mortality rates remain unchanged. This result holds both in the short run (after one year) and in the long run (after six years). We likewise find no significant differences across subgroups. Regardless of health diagnosis, age, or gender, mortality rates do not respond to increased DI generosity. Similarly, we find no mortality effects among permanent DI recipients.

To better interpret these insignificant results, we provide additional descriptive evidence on the relationship between DI generosity and mortality. When controlling for relevant characteristics–including diagnosis and employment history–the OLS estimates, though likely upward biased due to omitted variables, indicate only a small effect. In contrast, other factors such as health diagnoses and demographic characteristics have a substantially stronger impact on mortality.

Following the insights of the optimal design framework for DI benefits developed in Haller et al. (2024) and extended in this paper, our results allow us to draw a more general conclusion. Our comprehensive measure of incentive effects suggests that an increase of DI benefit by one Euro leads to fiscal costs of 1.83 Euros. We further show that the additional behavioral margin of DI recipients increases the fiscal costs estimate by 5%. When only considering the take-up margin, we calculate a fiscal multiplier of 1.74. This shows that the take-up effect is the main drive of the behavioral effect. This is consistent with two empirical findings. First, the estimated take-up elasticity is larger than the extension elasticity, and second, although the relative size of the extension effect is sizable in absolute terms, it only affects a small number of DI recipients.

We can compare these fiscal effects to the insurance effects of DI benefits estimated in the previous literature. For instance, Haller and Staubli (2025) estimate an insurance effect of 2.2 for Canada and 3.4 for the US. Thus, when taking our theoretical framework and these insurance values, we can conclude that an increase in DI benefits is welfare improving, which is consistent with the findings in Haller et al. (2024) and Haller and Staubli (2025).

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, for a survey see e.g. 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 (Autor et al., 2016; Bound, 1989, 1991; Chen & Van der Klaauw, 2008; French & Song, 2014; Maestas et al., 2013). Studies demonstrate that the effect of benefit levels may interact with other factors like stringency of the application process (Autor & Duggan, 2003; Campolieti, 2004; Garcia-Mandicó et al., 2020; Hanel, 2012; Karlström et al., 2008; Staubli, 2011), the state of the labor market (Autor & Duggan, 2003; Black et al., 2002; Von Wachter et al., 2011), other welfare programs (Borghans et al., 2014; Low & Pistaferri, 2015), and private insurance markets (Fischer et al., 2023; Seibold et al., 2025; Seitz, 2025).

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 (Benıtez-Silva et al., 2006; Koning & van Sonsbeek, 2017; Kostøl et al., 2019; Kostøl & Mogstad, 2014; Krekó et al., 2023; Ruh & Staubli, 2019; Vall Castelló, 2017; Weathers & Hemmeter, 2011), but the evidence on income effects is rather scarce.[1]

[1] 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).

The reform we analyze only affects the generosity of 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. Thus, our study is 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 Vall Castelló (2012), and Deuchert and Eugster (2019). These studies generally find that income effects matter for the labor response to benefits. In Section 6, 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 transfer programs, such as relatively generous unemployment insurance, means-tested transfers in Germany, 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.[2]

The paper proceeds as follows. In the next Section, we present the theoretical framework. Section 3 describes the disability insurance system in Germany and the changes introduced with the reform of 2014. In Section 4, we present the data and sample selection. In Sections 5 and 6, we describe our estimation methods and discuss the empirical results. We discuss the implications of the results in the context of the theoretical framework (Section 7) and conclude in Section 8.

[2] 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.

2 Theoretical Framework

We build on the framework of Haller et al. (2024), who characterize optimal disability insurance policy by studying the trade-off between insurance provision and incentive effects. Unlike their approach, which considers both benefit generosity and eligibility criteria, our study focuses on an empirical context where eligibility remains fixed. We therefore examine the welfare implications of varying benefit levels under constant eligibility rules.

We extend the baseline model by incorporating additional behavioral margins to derive a more comprehensive measure of moral hazard. In addition to modeling the take-up decision, we account for the labor supply behavior of DI recipients while receiving benefits. Furthermore, we incorporate exits from the DI system back into the regular labor market. These extensions influence both individual behavior and the relationship between policy parameters, welfare, and fiscal costs. In the following, we first present the model outline and derive response thresholds that result from the incentives presented by the DI system. We then demonstrate how a benefit increase in DI affects welfare from a social planner's perspective and how behavioral adjustments influence the fiscal costs of the program.

2.1 Setup

Each individual faces a health shock $\theta \in \mathbb{R}_+$, drawn from a continuous distribution $F(\theta)$, which represents the disutility of working due to health limitations. Higher values of θ indicate worse health and greater costs of labor market participation. An individual's health status is private information, which is known to them as they make decisions about labor supply and applying to DI. Agents can either work, apply for DI, or claim social assistance. Successful DI applicants also make decisions about working as DI recipients and whether to apply for an extension of DI if they are granted benefits only temporarily.

Working individuals earn a wage w, pay taxes τ, and may receive additional income A. They receive utility $u(c^w) - \theta$ where $c^w = w - \tau + A$. Accepted DI recipients receive a benefit b and may also earn additional labor income w' up to permitted thresholds with $w' < w$ and pay taxes $\bar{\tau} < \tau$. Their utility is $u(c^{b,e}) - \alpha\theta$ with consumption $c^{b,e} = b + w' - \bar{\tau} + A$ and $0 < \alpha < 1$ if they work while receiving DI[3], and $u(c^{b,n})$ with

[3]Note that working as a DI recipient is associated with lower work disutility $\alpha\theta < \theta$ and lower labor-income $w' < w$ as DI recipients are only permitted to work in much smaller capacities (e.g. with

consumption $c^{b,n} = b + A$ if they do not work. Social assistance recipients consume $c^z = z + A$ and receive utility $u(c^z)$, where $z < b$. In contrast to DI, social assistance receipt is not subject to uncertainty and not associated with any application costs.[4] We assume u is strictly increasing and concave in consumption and that $c^z < c^{b,n} < c^{b,e} < c^w$ such that consumption from being a working recipient exceeds that of a not working recipient and DI benefits exceed the consumption floor.

2.2 DI Application & Extension

DI applications are accepted with probability $p(\theta)$. If the application is rejected, the individual continues to work or claims social assistance otherwise. Among accepted applicants, we distinguish between temporary and permanent recipients. For permanent recipients, a successful DI claim presents an absorbing state until the end of the model horizon. Temporary recipients are admitted to DI benefits on a fixed-term award, which is granted only for a limited duration. After this temporary receipt, they either return to the labor market by working or claiming social assistance, or they can apply for an extension of benefits. Successful extensions result in permanent benefit receipt. We denote the probability of being accepted as a temporary and permanent recipient $p_t(\theta)$ and $p_p(\theta)$, respectively. We assume acceptance probabilities are increasing in θ and that the total acceptance probability is defined as $p(\theta) = p_t(\theta) + p_p(\theta)$ with $p(\theta), p_p(\theta), p_t(\theta) \in [0, 1]$.

Ex ante, individuals do not know whether they will be accepted as a permanent or temporary recipient. As in Haller et al. (2024), we propose that the probability of acceptance depends on a noisy signal of θ, which the government observes $s = \theta + e(\theta)$ where θ is the true signal and $e(\theta)$ is the noise.

We denote $0 < D < 1$ the expected share of remaining work life[5] that temporary recipients spend in the DI system, with $1 - D$ denoting the remaining share of work life spent working or on social assistance. Access to DI is costly at two points in the claiming process. Firstly, all individuals who apply for DI incur an application cost $\psi(\theta) \geq 0$, independent of the outcome of the application. We assume $\psi(\theta)$ is weakly decreasing in θ, i.e., that applying for benefits does not become more costly with increasing severity of one's health condition. Secondly, a temporary recipient who would like to remain on DI beyond the initial period must again pay a continuation or extension cost $\psi(\theta)$[6] in

respect to work hours) than in absence of DI benefits.

[4]This assumption reflects that there is no health assessment when applying for social assistance.

[5]A natural endpoint for DI benefits is usually old-age retirement.

[6]For the simplicity of the exposition, we propose that application and extension incur the same cost.

order to maintain benefit receipt. These costs capture administrative frictions, effort, or disutility associated with interacting with the DI system over time. Individuals who apply for an extension are accepted with probability $t(\theta) \in [0, 1]$ with $t'(\theta) > 0$. Similar to the initial DI application, extending benefits depends on a noisy signal of health status and extension criteria. Rejected extension applicants and those who do not apply for an extension return to the labor market after duration D when benefits run out.

2.3 Response Thresholds for an Increase in DI Benefits

This section introduces four behavioral margins that respond to changes in DI generosity: the decision to apply for DI, the choice to work while receiving benefits, the decision to extend the duration of benefits, and the decision of whether to engage in market work in the absence of receiving DI benefits versus claiming social assistance. These margins jointly determine the insurance value and the fiscal cost of the program. Individuals respond to benefit generosity b given the policy environment and their individual work impairment θ. In the following, we derive the model thresholds of θ which are relevant to the policy maker in designing DI benefits. Importantly, the DI application process nests decisions about claiming social assistance, extending benefits, and working on DI, which are reflected in workers' DI claiming decisions.

Social Assistance The first response threshold is given by the option set workers face in the absence of DI, i.e., if they choose not to apply or are rejected from DI benefits. In the absence of DI, individuals engage in employment given the available consumption floor provided by social assistance. Workers are indifferent between market work and claiming social assistance at $\theta^R = u(c^w) - u(c^z)$. Let $V_O = V_O(\theta, z) = \max\{u(c^w) - \theta, u(c^z)\}$ denote the utility-maximizing value of the outside options to DI benefits.

Working on DI All successful DI applicants must decide whether to work in addition to receiving benefits. Let θ^W be the cutoff above which recipients do not work. An agent is indifferent between being a working recipient and not working if $u(c^{b,n}) = u(c^{b,e}) - \alpha\theta$ such that $\theta^W = \frac{u(c^{b,e}) - u(c^{b,n})}{\alpha}$. Let $V_{DI} = V_{DI}(\theta, b, \alpha) = \max\{u(c^{b,e}) - \alpha\theta, u(c^{b,n})\}$ denote the utility-maximizing value of the option to work while being a DI recipient.

This is in line with the institutions in the German case, as extending benefits often requires submitting proof of disability and additional health checks similar to the initial DI benefit application.

DI Extensions Successful DI applicants become either temporary (with probability $p_t(\theta)$) or permanent recipients (with probability $p_p(\theta)$). Permanent recipients stay on benefits for the remainder of their work life, but being awarded temporary benefits warrants additional choices. As mentioned above, those granted temporary benefits have to decide whether to extend benefits at cost $\psi(\theta)$ if they wish to remain on DI benefits until the end of their work life. A temporary benefit recipient is indifferent between extending benefits at θ^E such that

$$DV_{DI} + (1-D)V_O = t(\theta)V_{DI} + (1-t(\theta))(DV_{DI} + (1-D)V_O) - \psi(\theta). \tag{1}$$

i.e. when the cost of interacting with the DI system equals the expected value of continuing to receive benefits given the available outside options

$$\psi(\theta) = t(\theta)(1-D)(V_{DI} - V_O). \tag{2}$$

We denote $V_E = V_E(\theta, b, t(\theta), \psi(\theta), \alpha)$ the value of the utility maximizing extension decision.

DI Application Workers decide to apply for benefits if the expected value of doing so, given utility-maximizing extension and working decisions, exceeds the value of remaining in the labor market. Let θ^A be the disability level of the marginal DI applicant, i.e. the individual is indifferent between being in the labor market and claiming DI benefits such that

$$V_O = p_p(\theta)V_{DI} + p_t(\theta)V_E + (1 - p_p(\theta) - p_t(\theta))V_O - \psi(\theta). \tag{3}$$

2.3.1 Threshold Ranking

In the interest of the exposition, we henceforth assume $\theta^A < \theta^E < \theta^W < \theta^R$. We illustrate the setup of the model given this ranking in Figure 1. This ranking warrants some explanation, albeit some relations arise naturally from the assumptions of the model. Firstly, we assume the marginal applicant has better health than the marginal social assistance claimant ($\theta^A < \theta^R$), similar to Haller et al. (2024), who make the same assumption in their exposition.[7] Next, we assume $\theta^W < \theta^R$, which holds if the gain from full-capacity market work over social assistance benefits exceeds the gain from (limited) work while on DI, and $\theta^A < \theta^W$, i.e, that the marginal applicant is in better health and the marginal recipient who decides not to work in addition to receiving benefits. Both assumptions can be justified by the empirical observation that some but not all DI recipients work in smaller capacities (and lower earnings) than before entering DI, and the discrepancy between social assistance, average earnings before DI takeup, and earnings after claiming DI benefits. Thirdly, given $\theta^W < \theta^R$ and $u(c^{b,n}) < u(c^w)$, we will have that $\theta^E < \theta^R$ as well. This relation follows from the indifference condition on θ^E. For all $\theta^E < \theta^W$ the condition is fulfilled if $\theta^W < \theta^R$, so we only need to check scenarios of $\theta^E > \theta^W$. For this range of θ, the individual is indifferent between extending if the expected value of extending compared to social assistance is larger than the cost of applying for the extension. The expected difference will be smaller than the difference of working compared to social assistance (indifference condition for θ^R) as long as $u(c^{b,n}) < u(c^w)$. Lastly, we assume that $\theta^A < \theta^E < \theta^W$. The first inequality is fulfilled for sufficiently small ψ and high $t(\theta)$ such that there exist individuals who apply for DI but do not apply for an extension if they are granted only temporary benefits. The second inequality holds under the same conditions for low values of α and sufficiently high $u(c^w) - u(c^{b,e})$, where a large group of recipients are incentivized to work while receiving benefits.

[7] Haller et al. (2024) further discuss how heterogeneity in wages, disability severity, and benefits can lead to cases where $\theta_i^R < \theta_i^A$. Importantly, any scenario in which $\theta^R < \theta^A$ does not help assess the question at hand and is rejected by empirical data showing labor supply responses to DI design.

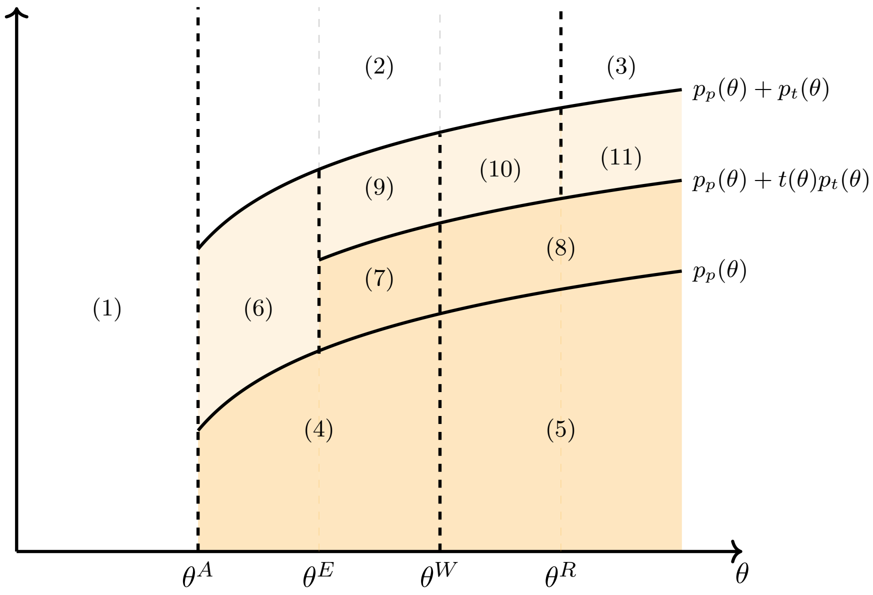


Figure 1: Model Setup

Note: This figure shows a graphical representation of the model. Individuals face different disability levels θ and decide whether to work, apply for disability benefits, or claim social assistance. DI application process is costly to the applicant and involves a noisy signal of the true disability state. Disability applications are awarded benefits permanently with probability $p_p(\theta)$ or temporarily with probability $p_t(\theta)$ or are otherwise rejected from DI. Temporary DI recipients receive benefits initially for a limited time and can apply for an extension to permanent benefits at a cost. Successful extensions are awarded permanent benefits, while rejected recipients return to the labor market. All DI recipients can choose to work while receiving benefits. The graph shows the ten different outcomes associated with individuals interacting with the DI process for given thresholds of θ for a given random distribution $F(\theta)$. Individuals with $\theta < \theta^A$ do not apply to DI (1) while rejected applicants work if $\theta < \theta^R$ (2) or claim social assistance otherwise (3). Permanent DI recipients work as long as $\theta < \theta^W$ (4) or rely solely on DI benefits otherwise (5). Among working temporary working recipients, there are those who do not extend benefits (6), those who do so successfully with probability $t(\theta)$ (7), and those whose extension attempt is rejected with probability $1 - t(\theta)$, requiring them to return to the labor market (9). Among non-working temporary recipients, everyone tries to extend benefits as long as $\theta^E < \theta$. Successful recipients remain on DI and become permanent recipients (8) while recipients with a rejected extension application return to the labor market (10) if $\theta < \theta^R$ or claim social assistance otherwise (11).

2.4 Welfare Effects of a DI Benefit Reforms

Following Haller et al. (2024) and building on Chetty (2006), we propose a utilitarian objective function subject to the government's budget constraint:

$$W(b) = V(b) + \lambda \left(G(b) - \bar{G}\right),$$

where $V(b)$ denotes aggregate utility, $G(b)$ denotes government expenditure, λ denotes the shadow value of public funds, and $\bar{G}$ is a fiscal constraint.

The value function of workers at benefit level b consists of the aggregated utility of non-applicants, rejected applicants and successful DI applicants given the acceptance probability to DI $p(\theta)$, successful extension probability $t(\theta)$, and costs of interacting with the DI system $\psi(\theta)$ (Equation 10 in Appendix A.1). The government budget depends on tax revenue τ and $\bar{\tau}$, expenditures for DI benefits b, and social assistance transfers z (Equation 11 in Appendix A.1).

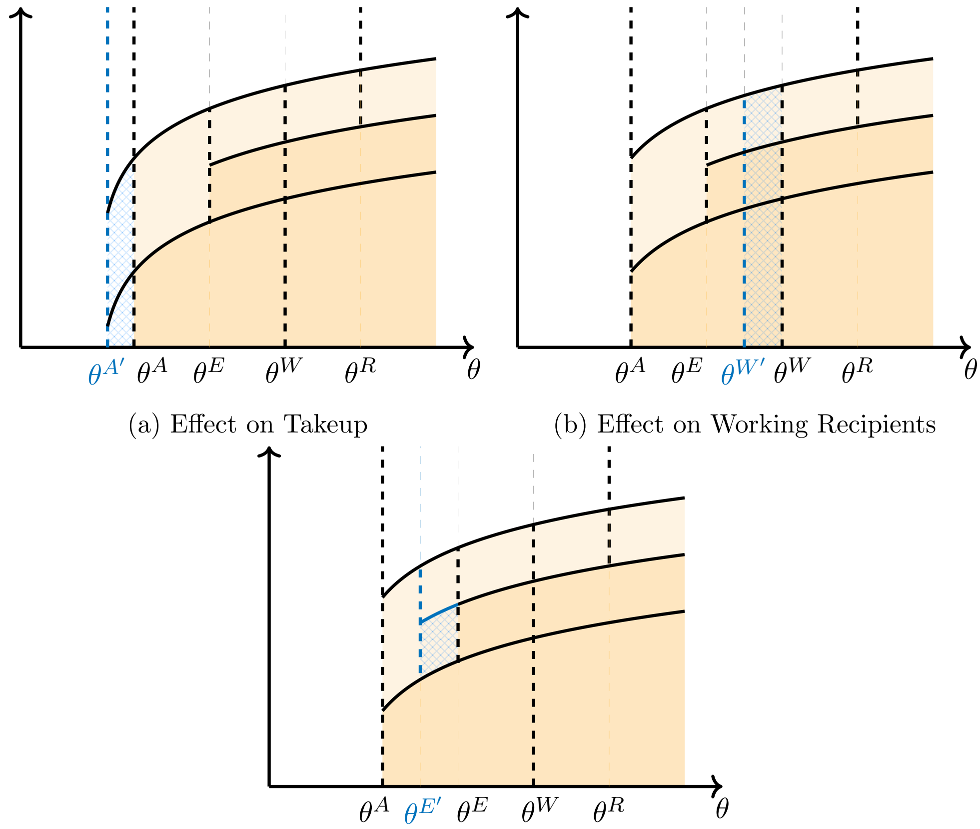
2.5 Effects of Increasing Disability Benefits

An increase in benefit generosity affects welfare through direct insurance gains and fiscal costs. The total derivative of the government's welfare function with respect to benefits b is given by

$$\frac{\partial W(b)}{\partial b} = \frac{\partial V(b)}{\partial b} + \lambda \frac{\partial G(b)}{\partial b}.$$

Higher benefits increase consumption among recipients, increasing the program's insurance value.

Simultaneously, the increase in benefits affects the fiscal costs of the DI program through two channels. It can be decomposed into a mechanical and a behavioral component. The mechanical component captures the additional costs that arise from additional benefit payments under a new benefit schedule, given the underlying recipient population. The behavioral components capture additional costs (or savings) that arise from the change in the incentive structure that agents face, leading to potential changes in DI claiming behavior, labor supply after DI entry, and agents' propensity to extend benefits from temporary to permanent payments.



(a) Effect on Takeup (b) Effect on Working Recipients

(c) Effect on Extensions

Figure 2: Graphical Analysis of Policy Changes

Note: This figure shows the graphical representation of the effects of an increase in benefits. Figure 2a shows how an increase in benefits affects incentives for disability benefit take-up. Figure 2b shows how a benefit increase may affect the share of recipients who work despite receiving benefits. Figure 2c shows the effect of increased benefits on incentives to extend benefits and become a permanent recipient.

The mechanical increase in spending for a higher benefit $b' > b$ is

$$M(b) \equiv [b' - b]\left[\int_{\theta^A}^{\infty} p_p(\theta)dF(\theta) + \int_{\theta^A}^{\theta^E} p_t(\theta)DdF(\theta) + \int_{\theta^E}^{\infty} p_t(\theta)(t(\theta) + D(1 - t(\theta))dF(\theta))\right]$$

which is equivalent to the additional benefit payments that are received by existing recipients in the absence of behavioral adjustments.

Behavioral responses amplify fiscal costs. Adding the costs from behavioral adjustments and expressing them in terms of the mechanical costs allows us to derive a fiscal multiplier of DI benefit changes. Given the mechanical and behavioral adjustments to a change in benefits, the total marginal fiscal cost change for a one-unit increase in benefits is given by:

$$1 + \frac{B_A(b) + B_W(b) + B_E(b)}{M(b)}$$

where the fiscal effect of take-up is denoted by $B_A(b)$, of workers' propensity to work while receiving benefits $B_W(b)$, and the extension rate $B_E(b)$. The total fiscal effect depends on how much each behavioral margin adjusts.

An increase in b improves welfare if:

$$\frac{\partial W(b)}{\partial b} \geq 0 \iff 1 + \frac{B_A(b) + B_W(b) + B_E(b)}{M(b)} \leq \frac{\frac{\partial V(b)}{\partial b}}{\lambda M(b)}.$$

We define the three behavioral adjustment margins of the model below. In the interest of the exposition, we derive the behavioral responses for an incremental benefit change that does not affect the established threshold order.

(i) Take-up Margin ($B_A(b)$) The change in disability application behavior is illustrated in Figure 2a. As benefits become more generous, the application threshold θ^A shifts to the left to θ^A (blue shaded area). The marginal DI applicant is indifferent to applying if the value of becoming a recipient does not exceed the application cost ψ. As benefits increase, a larger share of workers derive a positive value from applying to DI. The effect on government expenditure from this behavioral adjustment is given by

$$B_A(b) = [b' + \tau - \bar{\tau}] \left(\frac{\partial \theta^A}{\partial b} \right) \left[(Dp_t(\theta^A) + p_p(\theta^A) \right] f(\theta^A).$$

It is equivalent to the additional benefit costs b' and forgone tax payments τ of additional successful DI applicants.

(ii) Employment of Recipients ($B_W(b)$) The change in DI benefits results in an income effect for working DI recipients, illustrated in Figure 2b. Higher benefits allow recipients to hold consumption stable at lower labor supply levels. The change in benefits shifts the threshold of working DI recipients to the left from θ^W to $\theta^{W'}$, reducing the number of working DI recipients. The change in government expenditure is given by

$$B_W(b) = \bar{\tau}\left(\frac{\partial \theta^W}{\partial b}\right)\left[p_p(\theta^W) + p_t(\theta^W)(t(\theta^W) + D(1 - t(\theta^W))\right] f(\theta^W).$$

The change in tax expenditure will depend on how labor income of DI recipients is taxed.

(iii) Exit Rate ($B_R(b)$) Lastly, a change in DI benefits will change the share of temporary recipients who extend benefits to become permanent recipients. We illustrate the mechanism in Figure 2c. An increase in benefits should reduce the exit rate from DI, as higher benefits offset the cost of extending for a larger share of recipients. The change in government expenditure is given by

$$B_E(b) = [b' + \tau - \bar{\tau}]\left(\frac{\partial \theta^E}{\partial b}\right) p_t(\theta^W)t(\theta)(1 - D)f(\theta^E).$$

It increases the tax revenue lost from recipients returning to work and the additional benefits paid to DI recipients who stay on benefits permanently. Simultaneously, the government saves on social assistance payments for those fixed-term recipients who do not work if they are rejected from a benefit extension. The loss in tax revenue is partially reduced depending on the tax revenue generated from working DI recipients.

3 Institutional Background

3.1 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%).[8] 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 the applicant. Only around half of all applications are successful. The formal requirements consist of three main criteria. First, individuals must be below the statutory

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

retirement age to qualify for DI.[9] The benefits of active recipients are converted into an old-age pension of the same amount once they reach retirement age. Second, applicants must have been insured in the public pension system for at least five years. Third, they must have paid contributions for at least three years in the past five years to qualify for benefits.[10]

After verifying that the formal requirements are met, the pension insurance assesses the applicant's work impairment based on the information provided by the applicant and a medical officer. In general, medical officers are instructed to exhaust other options, such as rehabilitation measures or alternative occupations, before authorizing benefits.[11]

Successful applicants may receive partial or full disability benefits depending on the severity of their impairment. Individuals who are unable to work more than three hours per day receive full benefits, while those who can work up to six hours per day receive partial benefits.[12]

Disability benefits are generally granted for a limited period, usually up to three years from the start date, and may be extended to a permanent pension (§102(2) SGB VI). The extension process requires a new examination by the pension insurance, similar to the initial assessment. A permanent benefit is granted only if medical improvement in the reduced earning capacity is deemed unlikely. The question of probability must be assessed prognostically. In cases of uncertainty in the medical prognosis, therefore, a temporary benefit must be granted. This distinction is important for the timing of benefit payments. While permanent benefits begin with the onset of reduced earning capacity, temporary DI benefits are not paid before the beginning of the 7th calendar month following the onset of reduced earning capacity.

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:

[9] The statutory retirement age is codified in §235 of Social Code Book VI. It is being gradually raised from 65 to 67 in one- or two-month increments for cohorts born between 1947 and 1964.

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

[11] 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).

[12] Recipients of partial benefits are entitled to full benefits if suitable employment cannot be found in the labor market.

$$EPC_i = \sum_{t=\underline{t}_i}^{\tau} \min\left\{\frac{y_{it}}{\bar{y}_t}, \frac{y_t^*}{\bar{y}_t}\right\}, \tag{4}$$

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 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 $\bar{y}_t$.

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 pension insurance grants supplementary credits for recipients of disability benefits for these forgone working years. Specifically, workers total pension credits denoted by TPC_i are computed as

$$TPC_i = EPC_i + (g(t,a) - \tau) \times \overline{EPC}_i \tag{5}$$

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[13]

$$B_{i,t} = TPC_i \times v_t \times q(t,a), \tag{6}$$

where v_t denotes the value of a pension point in year t, and the last term, $q(t,a)$, represents the deduction applied for early retirement. The function $q(t,a)$ depends on the retirement age a and the policy environment prevailing in year t. Deductions are capped at 10.8%. At the time relevant for our study, the maximum deduction was applied if retirement occurred before the age of 60 years and 8 months, while no deductions were applied after reaching the age of 63 years and 8 months.[14]

[13] This is a simplified version of the formula, as the Pension Insurance distinguishes between pension credits earned in the former East and West Germany. This distinction is omitted here for simplicity.

[14] Before 2012, individuals could claim disability benefits without deductions starting at age 63. Since then, this threshold has gradually increased to 65 years by 2024. At the same time, the statutory

DI benefits provide a substantial replacement for earnings from employment or other benefit sources for recipients. While many individuals switch to DI benefits directly from employment, a large share receives other types of benefits (e.g. unemployment benefits or social assistance) 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.[15] Sickness and unemployment benefits can only be claimed for a limited duration before individuals fall into social assistance. Social assistance is usually less generous and is (in contrast to other benefit types) means-tested, such that other household income and savings are taken into account to determine benefit eligibility.

Depending on the previous earnings and entitlements from other programs, the replacement rate of DI benefits may vary. Before the 2014 reform (see below) the median replacement rate of DI benefits compared to the year before recipients claimed benefits ranged between around 50% for earnings from employment to 55% for benefits from unemployment insurance or sickness benefits. We show the distribution of the replacement rate with respect to individual income in the year before the DI claim in Figure 17 of the Appendix. For the vast majority of recipients, claiming DI benefits constitutes a cut in individual income by around 50%. 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).

3.2 The 2014 Reform to DI Benefits

During the 1990s and 2000s, Germany reduced the generosity of the system, changed the assessment system, and tightened eligibility criteria (Burkhauser et al., 2016; Fischer et al., 2023; Seibold et al., 2025). These reforms led to a decline in disability benefit entries, a reduction in benefit levels, and a sharp increase in both the poverty risk rate and welfare take-up (Geyer, 2021). Consequently, unlike other countries, Germany did not experience long-term growth in its disability insurance program.

In 2014, the German government changed policy and increased disability benefits for

retirement age has been steadily rising from 65, increasing by one or two months for cohorts born after 1947, until it reaches 67 for those born in 1964 in 2031. Finally, there is a regulation that allows individuals with at least 45 years of contributions to retire early without any deductions.

[15] 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 reductions in employment shares of DI recipients even before benefit award.

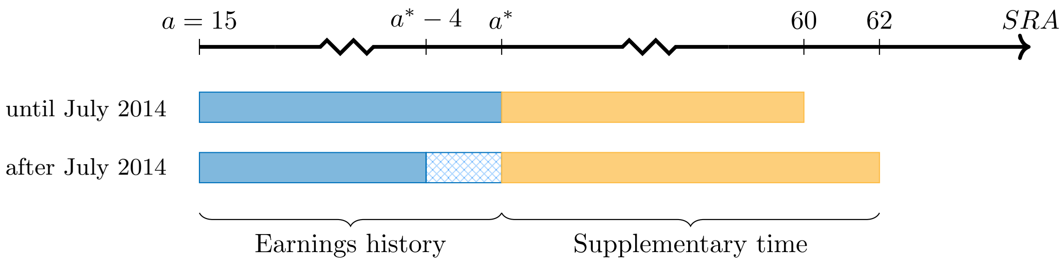


Figure 3: Illustration of Reform to Disability Benefit System

Note: The figure illustrates the change in benefit computation introduced by the reform of July 1, 2014. Individuals receive an account with the German Pension Insurance at age $a = 15$, which records contributions over their entire earnings history. They can claim disability benefits from the Pension Insurance until reaching the statutory retirement age (SRA). In 2014, the statutory retirement age in Germany was 65 years and three months. When an individual claims disability benefits at age a^*, their benefits are calculated based on their earnings history (blue area) and the supplementary time (orange area) granted to substitute for forgone contributions after the start of the pension. Prior to the reform, supplementary time was granted until age 60; the 2014 reform extended it to age 62. The reform also changed how the four years preceding the start of the pension (shaded blue area) are treated in the computation. For individuals who began receiving benefits after the reform, these four years are excluded from the entitlement calculation if doing so results in higher benefits.

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 effectively increases DI benefits for new recipients by around 6%, which is equivalent to two additional years of contributions to the pension system. The change was especially large in absolute terms for recipients with higher pension entitlements.

The reform-induced change in benefit computation is illustrated in Figure 3. The increase in benefits was primarily achieved by extending the supplementary time from age 60 to 62. Additionally, the reform altered how the period prior to claiming benefits is accounted for in entitlement calculations. Before the reform, the supplementary time was weighted by the average pension credits earned before entering disability benefits. Since the reform, the four years immediately preceding entry are excluded from this computation if doing so benefits the recipient. This change was intended to compensate for missing contributions resulting from disability-related low labor market attachment in the years leading up to an individual's DI claim.[16]

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

[16]The effect of this second component on average benefit increases is relatively small, as illustrated in Section A.2 of the Appendix, meaning that the majority of the bonus comes from the additional two years of supplementary time.

package and went into effect a week later on July 1, 2014.

3.3 Other elements of the 2014 Pension Reform Package

The increase in disability benefits was implemented as part of a broader reform package. In addition to the DI reform, the package included two major reform components. First, it introduced an early retirement option allowing individuals with 45 years of contributions to retire without deductions at the age of 63 (see Dolls and Krolage (2023)). However, this additional route to early retirement is not available to the population considered in our sample. Therefore, we can rule out that this reform component interacts with the DI reform and generates potential spillovers. 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.[17] The second relevant reform element in the package was a change to caregiver's pensions (Artmann et al., 2023; Becker et al., 2023a). This reform granted additional pension credits to parents of children born before 1992. This component primarily affects women, as fewer than 1% of male disability benefit recipients have any contribution periods for childcare in their accounts. Because the increase in caregiver credits affected both existing and new DI recipients, it does not influence our results for current DI recipients. However, when estimating the take-up effect, we account for the additional pension credits, as they also alter the generosity of DI benefits. Specifically, when estimating the elasticities, we rescale the response of women by the overall increase in DI generosity, which reflects both reform components.

4 Data

For our empirical analyses, we use administrative records from the German pension insurance. We construct two samples for our analysis of insurance claims (*full sample*) and recipient outcomes (*recipient sample*) respectively. To this end, we draw on data on the universe of German insurance accounts between the years 2012 and 2017.

Full Sample For the full sample, we combine annual information from all active insurance accounts (*Aktive Versichertenkonten, AKVS*) with the universe of disability

[17] 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. 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.

insurance claims *Rentenzugang, RTZN*). Combining these data sources using a unique identifier, we can compute the share of insured individuals which claims DI benefits in the German population. We use the data to construct a quarterly panel of German workers, including information on individual DI claims, employment status, and demographic characteristics like age and region of residence. We restrict our data to workers below age 60, as older workers did not (fully) benefit from the reform, and exclude workers below age 50 for computational tractability and since DI claims are very rare below this age. We additionally exclude workers with special miners' pension entitlements, who are covered by separate DI schemes, and individuals without German citizenship. Our quarterly panel consists of approximately 15 million individuals.

Recipient Sample For the recipient sample, we use the universe of new DI recipients from the administrative pension data. 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-*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 accounts[18] in Germany (*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.2 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

[18]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.

to ensure that the validity of our design and results is not sensitive to sample selection.

The most important sample selection is that we focus the main analysis only on temporary benefit recipients. As mentioned above, 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 and labor market return, we also focus on them in the interest of identification: as discussed above 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 provide robustness checks DI 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[19] amounting to around 20% of the sample. We again exclude recipients above age 60 as they are not affected by the reform.[20]

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 around 255,000 recipients for the start years 2012-2016, including

[19] We eliminate partial recipients because they only make up a very small share of the remaining benefit recipients. Furthermore, they receive only a very low treatment intensity since the increase is proportional to the benefit amount, preventing meaningful analysis.

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

around 51,000 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.

5 Take-up Effect

In this section, we show first descriptive evidence of DI take-up, introduce the research design to estimate the take-up effect of an increase in benefit generosity, and present the estimation results.

5.1 Descriptive Evidence

To estimate the effect of the reform on DI take-up we analyze the number of DI claims as a share of all active insurance accounts in Germany that potentially have access to claiming disability benefits. Figure 4a shows the quarterly fraction of DI claims in the full sample. Around 0.17 % of workers between ages 50 and 60 claim benefits in any given quarter. There exists a seasonality pattern with higher take-up in the second and third quarters, but there is no evidence of a clear change in time trend. In the presence of strong take-up effects of a change in the generosity of DI, we would expect to see a long-lasting uptick in DI claims after the reform, as an increase in benefits makes claiming DI more attractive. In the empirical analysis, we will account for seasonality and potential time trends to test this more formally and to quantify the take-up effect.

5.2 Research Design

As the variation in DI benefits is time-dependent based on the start date of disability benefits, we use an event study design and compare take-up before and after the reform to isolate the effect. We apply a procedure similar to Mastrobuoni (2009) where we compare post-reform take-up to pre-reform trends. In a first step (Equation 7), we de-trend the data based on the pre-reform years 2012 and 2013 using a linear trend t_q and quarterly fixed effects δ_q to control for seasonality. We additionally control for individual time-varying characteristics X_{iq} like occupation, age, citizenship, and region of residence to capture variation in the population composition over time. Thus, identification is based on the assumption that when controlling for the time trend, seasonality, and time-specific covariates, post-reform differences are explained by the increase in DI generosity. We can test this assumption by inspecting the pre-reform trend.

$$y_{iq} = \alpha + \sum_{q'=q_{1,2012}}^{q_{4,2013}} \delta_{q'} \, \mathbf{1}\{q' = q\} \; + \; \tau \, t_q \; + \; X'_{iq}\gamma \; + \; u_{iq}. \tag{7}$$

Based on this model, we predict the quarterly take-up rate for the entire time period and compute the residual DI claims ($\hat{r}_{iq}$) relative to the detrended data.

$$\hat{r}_{iq} \equiv y_{iq} - \widehat{y}_{iq} = \alpha + \sum_{q'=q_{1,2012}}^{q_{4,2017}} \beta_{q'} \, \mathbf{1}\{q' = q\} \; + \; \varepsilon_{iq}. \tag{8}$$

The take-up effects of the reform β_q are then given by the quarterly deviation in DI claims after the reform relative to the pre-reform period.[21] The quarterly effects are estimated relative to DI take-up shares in the first quarter of 2014. We select this specification to detect potential decreases in DI take-up, which could arise from applicants anticipating the benefit increase in the three months leading up to the reform.

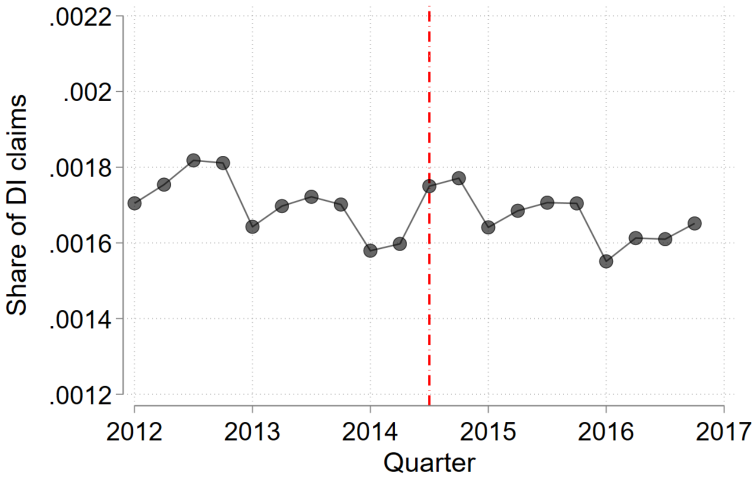
5.3 Effect on DI claims

Figure 4b shows the resulting β_q coefficient estimates in our sample. Given the large number of individuals (approximately 15 million), the confidence bands are very narrow. We find a significant increase in take-up following the reform, which suggests that the increase in benefits led to an increase in DI claims. The increase in take-up is already observable in the first quarter after the reform. This effect is driven by permanent DI recipients. For this group, the waiting period of six months does not hold, which only applies to the temporary DI recipients. Thus, they can immediately respond to the reform. In Section 6.1, we show that for temporary DI recipients there is no discontinuity at the cut-off. The take-up effect slightly decreases over time, but is still present and significant two years after the reform. Importantly, we find no significant pre-trends, which provides evidence that our specification with a linear time-trend satisfies the identification assumption.

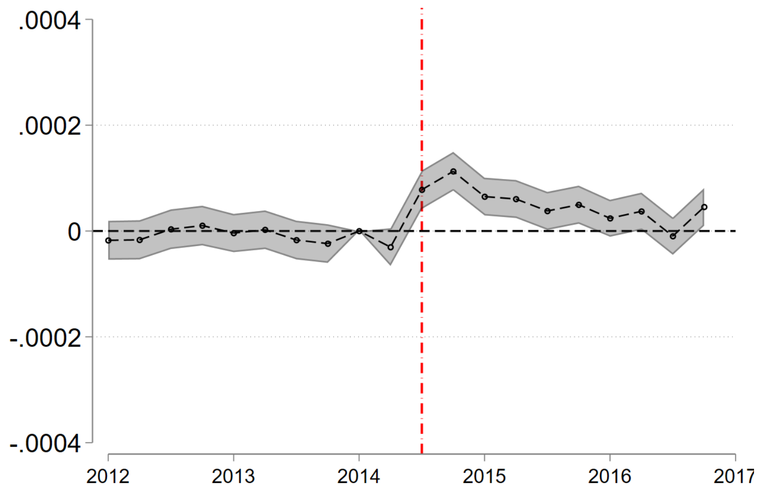
[21] The pooled specification is given by

$$r_{iq} = \alpha + \beta^{\text{post}} \, POST_{iq} \; + \; \varepsilon_{iq}$$

.



(a) Claiming Rate across all Insurance Accounts



(b) Estimated Effect of Reform on all DI Claims

Figure 4: Estimates of the quarterly Take-up Effect for ages 50 to 60

Note: The Figure shows (a) the quarterly amount of DI claims by gender as a share of all active annual insurance accounts. Figure (b) shows the quarterly deviation (given by β_q in Equation 8) in take-up relative to the quarter before the reform, controlling for demographic characteristics, occupation, and region.

To quantify the effect and to calculate the take-up elasticity, we turn to the pooled estimates, which are presented in Table 1. Overall, we find a significant positive effect of the reform on disability insurance take-up for the full sample and by gender. The estimates suggest a relative change in take-up of 2.5% for men and 6.5% for women. The stronger effect for women is consistent with the additional increase in DI generosity due to the reform of child-related pension entitlements (see Section 3 and Table 14 in the Appendix). The gender differences are smaller when calculating the resulting elasticities by relating these behavioral responses to the group-specific increase in DI benefits. Our results imply an elasticity of DI benefits on take-up for women of 0.74 and for men of 0.32. The overall elasticity is 0.53. The size of our estimated elasticity is comparable to previous studies. For example, Mullen and Staubli (2016) find for Austria an elasticity of 0.7 for the time period 2004-2010[22],Gruber (2000) estimates a short-run elasticity of 0.36 for Canada and Haller and Staubli, 2025 an elasticity of 0.58 also for Canada.

In Panels B and C, we present the estimated effect separately for temporary and permanent DI recipients. The overall change in take-up is mainly driven by permanent DI recipients (Panel B), for whom we find a significant increase in DI take-up of 7.8% relative to the pre-reform mean. The resulting average elasticity is 0.9 and is again larger for women than for men. In contrast, for temporary DI receipt, we find a much smaller effect size. For men, the effect is not significant and very close to zero.

As recipients can not select between permanent versus temporary DI receipt, the different effects for permanent and temporary DI results can be seen as a positive indication for the efficacy of the medical test and the DI application process: while an increase in benefits induces more individuals to claim benefits, this mainly applies to relatively sicker individuals who receive permanent DI benefits.

6 Effects of DI Recipients

In this section, we focus on the behavioral responses of DI recipients. First, we discuss the research design, show the results of the first stage, i.e., the effect of the reform on DI benefits, and provide validity checks for the identification strategy. Second, we present the estimation results for the labor market outcomes. We show how an increase in the generosity of DI affects employment and earnings and the transitions back to the labor market. This allows us to derive the employment elasticity and the elasticity for extending benefits. Finally, we turn to the results on mortality. As mentioned above, in the main analysis, we focus on individuals with temporary DI, leveraging the announcement and timing of a DI reform. However, we show that the effects for employment, earnings, and mortality are similar for individuals with permanent DI

[22]The effects are slightly larger (1.2) when considering the period 1987-2010.

Table 1: Effect of Increased Benefits on DI Claims

Outcome: Takeup	All	Female	Male
A. Any DI Benefit	0.000079***	0.000114***	0.000042**
	(0.000011)	(0.000018)	(0.000012)
Dep. Mean	0.00172	0.00175	0.00168
Relative Change	4.6%	6.5%	2.5%
Elasticity	0.53	0.74	0.32
B. Permanent DI Benefit	0.000057***	0.000066***	0.000047***
	(0.000007)	(0.000009)	(0.000007)
Dep. Mean	0.00073	0.00071	0.00074
Relative Change	7.8%	9.3%	6.4%
Elasticity	0.90	1.05	0.81
C. Temporary DI Benefit	0.000023*	0.000047**	-0.000006
	(0.000010)	(0.000015)	(0.000008)
Dep. Mean	0.00099	0.00104	0.00094
Relative Change	2.3%	4.5%	-0.6%
Elasticity	0.27	0.51	-0.08
Observations	114,775,508	58,453,284	56,322,224

Note: Table shows the estimation results of DI claims with respect to the increase in benefits in a window of 18 months around the reform. The computed elasticities of take-up w.r.t. the benefit change use the group-specific point estimate of the benefit change and the 95% CI of the take-up effect. Our point estimates for the change in benefits do not include ex-post corrections in mother pension entitlements to reflect the information set of applicants at the time of the reform. Standard errors are clustered at the quarter level.
Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

benefits.[23]

[23] By definition, permanent DI recipients do not make transitions back to the labor market; therefore, we do not analyze the exit margin for this group. This is supported by the data, which shows almost no exits from DI.

6.1 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 interest at the reform cutoff date. In particular, we specify a local polynomial regression model of the form

$$Y_i = \alpha + \beta D_i + f_1(X_i - c) + D_i f_2(X_i - c) + \epsilon_i \tag{9}$$

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 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.

As mentioned above, 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.[24]

[24] 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 4, 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

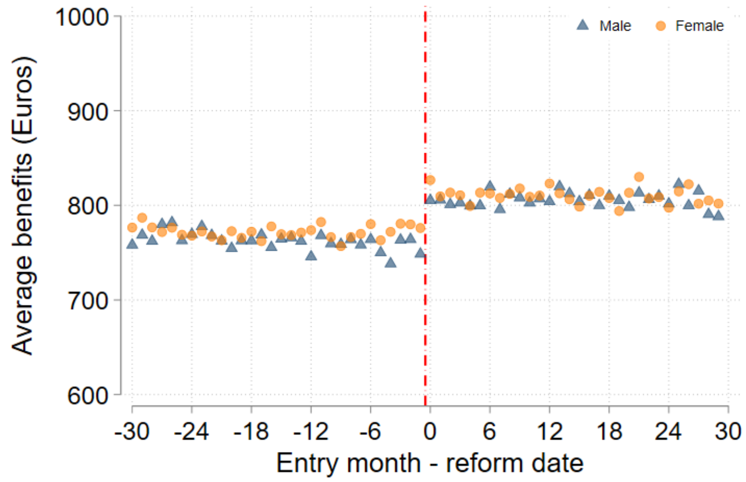


Figure 5: Average Monthly DI Benefits as a Function of Benefit Start Date Around the Reform

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 47.2 for the full sample, with a standard error of 6.2 (BW: 6.5). This constitutes a 6% increase in benefits relative to the control mean of 765 Euros per month.

First Stage: The Effect on Disability Benefits

Figure 5 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.[25] At the time of the reform DI recipients received per month on average €765. We estimate a significant increase in average benefits of around €47 Euros per month or €565 per year at the reform cutoff. In relative terms, this is an increase of more than 6%.[26]

The point estimate hides 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

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".

[25]The DRV increases pensions according to wage growth every year, resulting in an upward trend in average benefits at the time of award over time. We correct for this trend by standardizing all benefits to their Euro value of 2014. Further information is provided in Section A.2 of the Appendix.

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

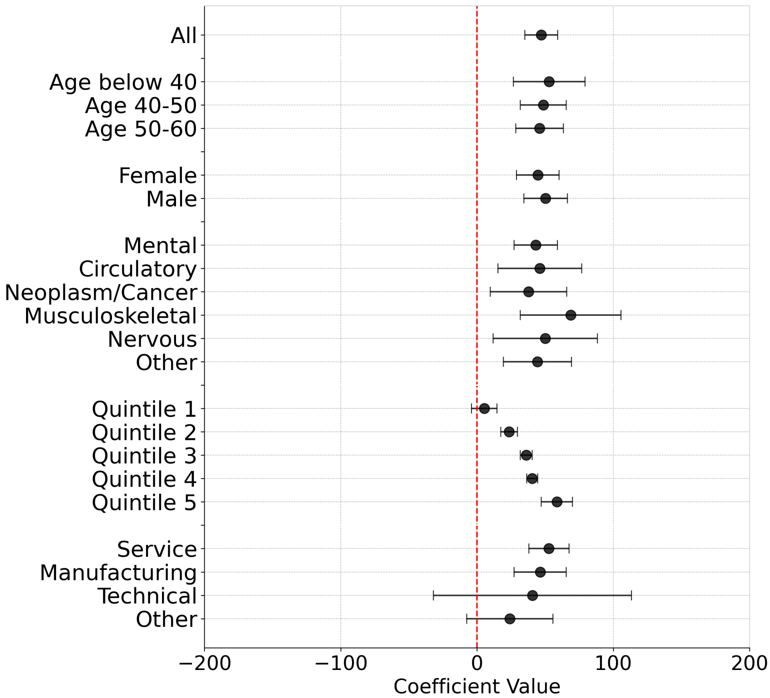


Figure 6: Benefit Increases by Subsample

Note: The figure shows the increases in DI benefits for subsamples in our data. We split the sample by age group, gender, primary diagnosis, pension wealth, and prior occupation. Benefit quintiles are constructed on pre-reform pension wealth. Benefits are standardized to 2014 pension values.

recipient. Figure 6 displays the overall increase in disability benefits for different subsamples in our data.[27] Notably, the absolute realized change in benefits differs across quintiles. This is expected, given how the increase in benefits was implemented. While recipients in the lowest quintile of our data receive a bonus of less than €20 per month, recipients in the highest quintile receive around €60 more in benefits. In our analysis, we account for this by performing heterogeneity analyses that distinguish recipients by pre-reform pension entitlements. The differences in benefit increases across other dimensions of heterogeneity are relatively smaller. We find a slightly higher increase for individuals with musculoskeletal conditions compared to other diagnoses, and slightly higher increases for individuals from service and manufacturing jobs compared to other occupations. Overall, however, the distribution of pension wealth seems to be fairly similar across recipient subsamples.

[27]Figure 23 in the Appendix furthermore shows the reform's effects on benefits across subsamples in relative terms.

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. 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. In summary, we show the validity checks for the main estimation sample and for the full sample before implementing the data restriction (see Section 4) hold. 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.

In more detail, Figure 7 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 9), 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 that the continuity of the density also holds for the full sample before we apply the sample restrictions described in Section 4.[28] 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 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 cut-

[28] 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.

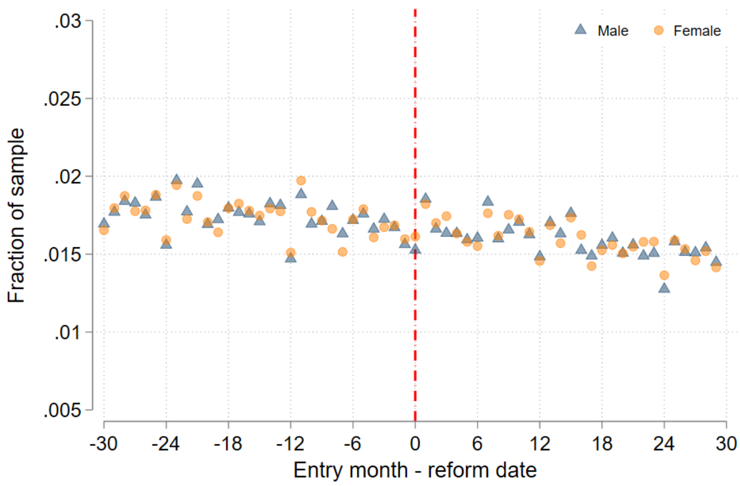


Figure 7: Smoothness of Sample Density Around Reform Cutoff

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 9 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.

off 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 8 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 2 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 2, 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 primary disability diagnosis. The most common diagnoses are mental disorders, which account for 53% 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

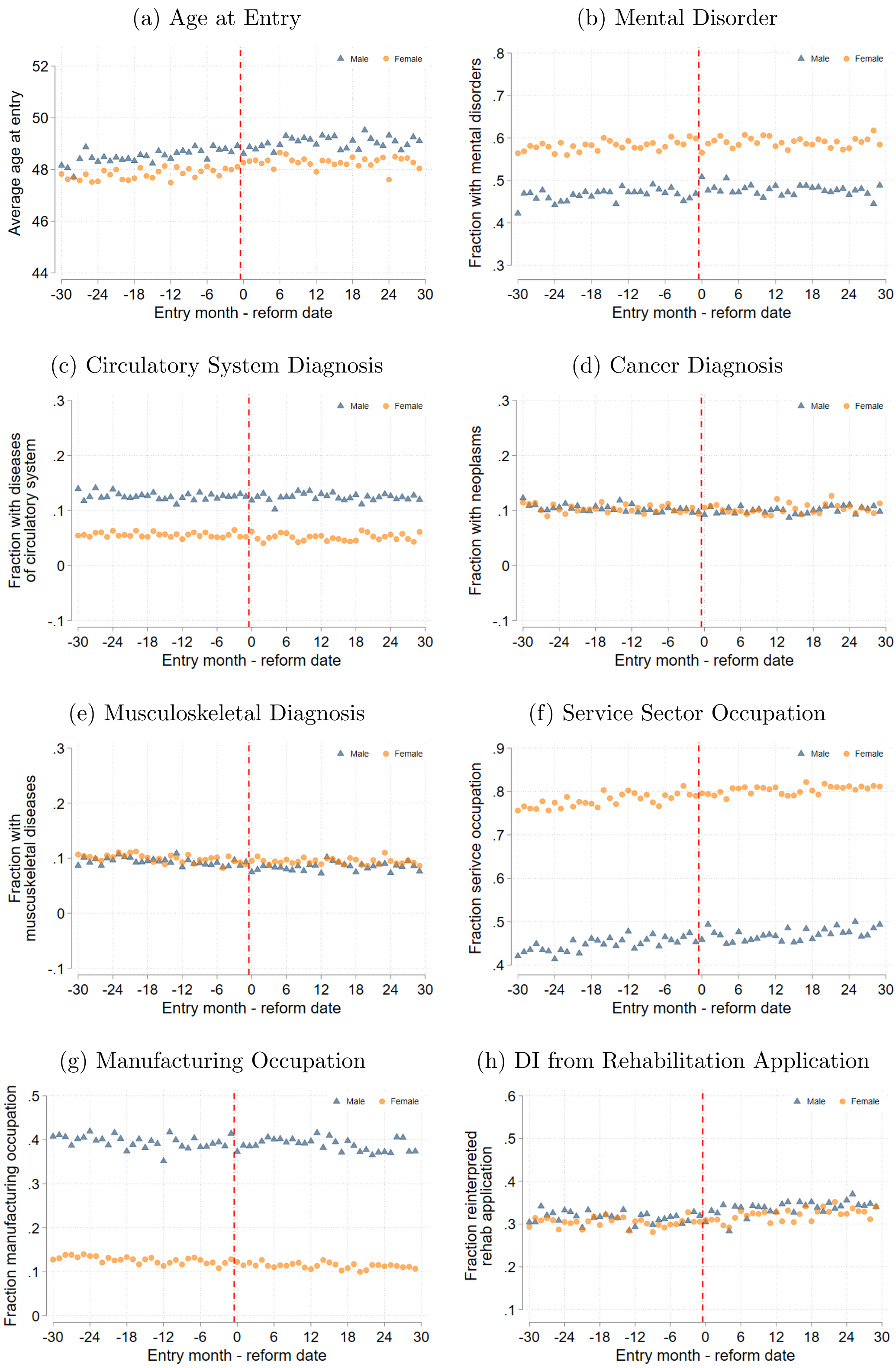


Figure 8: Continuity of Covariates Through the Reform

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 2: Continuity of Covariates Throughout Cutoff

	Coefficient (SE)	Control Mean [Bandwidth]	Percent significant at 5-% level
Panel A. Demographic information			
Age	0.053	48.77	0.0
	(0.12)	[10.55]	
Female	0.007	0.52	0.0
	(0.009)	[7.48]	
Panel B. DI information			
Mental diagnosis	0.005	0.53	0.0
	(0.008)	[9.37]	
Circulatory diagnosis	-0.005	0.09	0.0
	(0.004)	[11.88]	
Neoplasm/Cancers	0.006	0.10	0.0
	(0.005)	[9.04]	
Musculoskeletal diagnosis	-0.002	0.09	0.0
	(0.004)	[11.65]	
Nervous system	0.001	0.05	0.0
	(0.004)	[9.22]	
Other diagnosis	-0.005	0.13	0.0
	(0.005)	[10.66]	
Panel C. Work history			
Full contribution times	3.183	261.48	44.0
	(1.939)	[8.45]	
Reduced contribution times	0.407	44.55	6.0
	(0.485)	[7.65]	
Service occupation	0.005	0.63	0.0
	(0.008)	[8.37]	
Manufacturing occupation	-0.014*	0.25	56.0
	(0.007)	[9.20]	
Technical occupation	0.005*	0.02	22.0
	(0.003)	[9.05]	
Other occupation	0.005	0.09	6.0
	(0.005)	[6.27]	
DI benefits w/o reform bonus	9.810	762.42	11.0
	(6.077)	[6.20]	
Panel D. Rehabilitation history			
Labor market rehabilitation	0.004	0.03	0.0
	(0.003)	[9.24]	
Medical rehabilitation	0.009	0.50	6.0
	(0.008)	[9.33]	
DI from rehabilitation application	-0.006	0.31	0.0
	(0.007)	[9.07]	
No consideration of employability	-0.002	0.84	0.0
	(0.006)	[7.57]	

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: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

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.[29] Aside from information about diagnoses, we also have access to information about the rehabilitation history of recipients (Panel D of Table 2). 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.

6.2 Employment and Earnings

We distinguish between two types of employment - insured employment and marginal employment. Figure 18 in the Appendix shows the employment status of recipients leading up to and after the entry into DI. Employment typically drops leading up to the DI claim, while claims for sickness and unemployment benefits increase. However, overall, 25% of DI recipients are still employed many years after the DI award. This underlines the importance of studying potential employment responses induced by a change in the generosity of benefits. Around 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 exempt from social security and tax payments, they are an attractive form of employment for DI recipients. The fraction

[29] 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.

in insured employment is considerably lower (about 5%).

In Figure 9, 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.

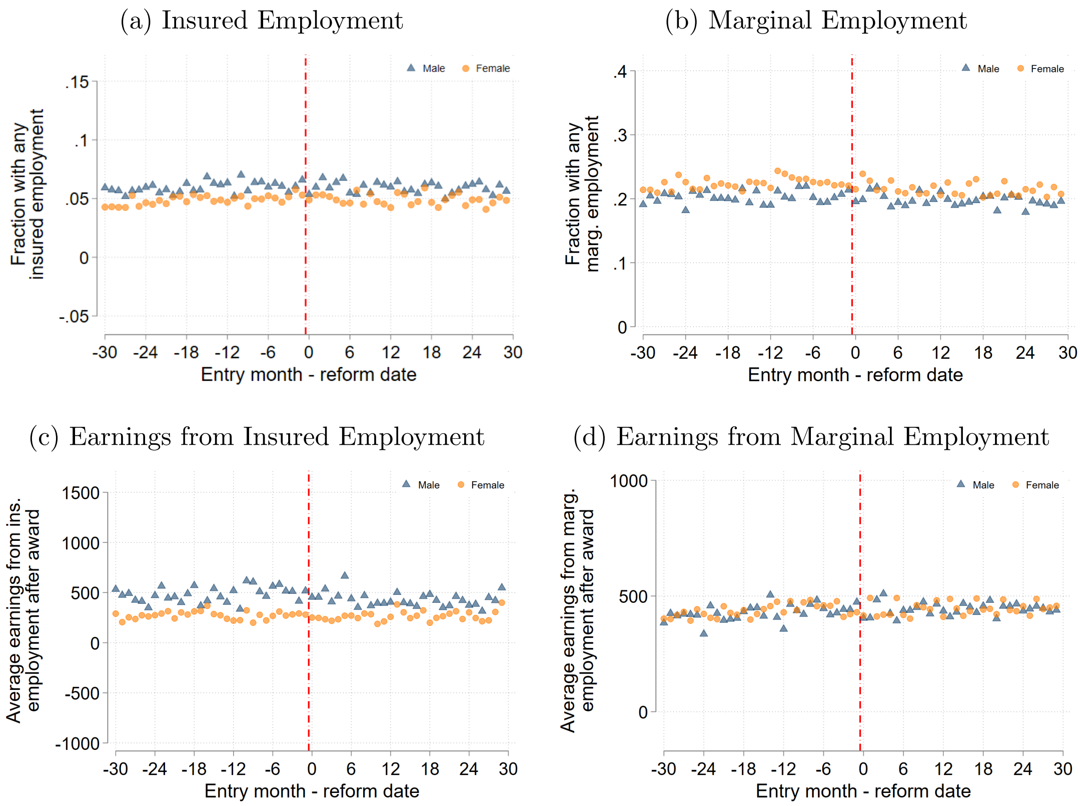


Figure 9: Employment and Earnings in the Four Years After Award of Benefits

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.

In Table 3, we show the corresponding effects from the regression specification outlined

Table 3: Effect of DI Benefit Increase on Employment

	All		Female		Male	
Outcome: Employment	(1)	(2)	(1)	(2)	(1)	(2)
Marginal Employment	0.004	0.004	0.009	0.012	-0.001	-0.003
	(0.0063)	(0.0065)	(0.0090)	(0.0091)	(0.0083)	(0.0090)
	[10.1]	[9.3]	[9.5]	[9.0]	[11.9]	[10.0]
	0.22	*0.22*	*0.22*	*0.22*	*0.20*	*0.20*
Insured Employment	-0.002	-0.002	-0.003	-0.002	-0.002	-0.002
	(0.0034)	(0.0034)	(0.0044)	(0.0044)	(0.0050)	(0.0050)
	[10.8]	[10.7]	[11.2]	[11.1]	[11.3]	[11.4]
	0.05	*0.05*	*0.05*	*0.05*	*0.06*	*0.06*
Earnings Marginal Emp.	0.463	-0.634	27.823	30.415	-23.771	-29.476
	(16.4912)	(16.6110)	(24.5030)	(24.5521)	(25.2549)	(27.1600)
	[11.1]	[10.8]	[9.1]	[8.9]	[10.7]	[9.1]
	432.72	*432.72*	*436.14*	*436.14*	*428.82*	*428.82*
Earnings Insured Emp.	-25.205	-23.239	-56.511	-52.438	4.808	8.962
	(46.7347)	(46.7896)	(40.3131)	(40.4058)	(87.5927)	(87.7425)
	[7.9]	[7.9]	[11.1]	[10.9]	[7.5]	[7.4]
	387.71	*387.71*	*288.47*	*288.47*	*500.84*	*500.84*
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 8 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. Elasticities are expressed with respect to the 95% confidence interval of the outcome of interest.
Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

in Section 6.1 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.[30]

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 27 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 are small and insignificant. We additionally inspect employment outcomes on an annual level (Table 10 and Table 11 in the Appendix) to check if our aggregated outcomes over the span of four years omit any potential dynamic effects.

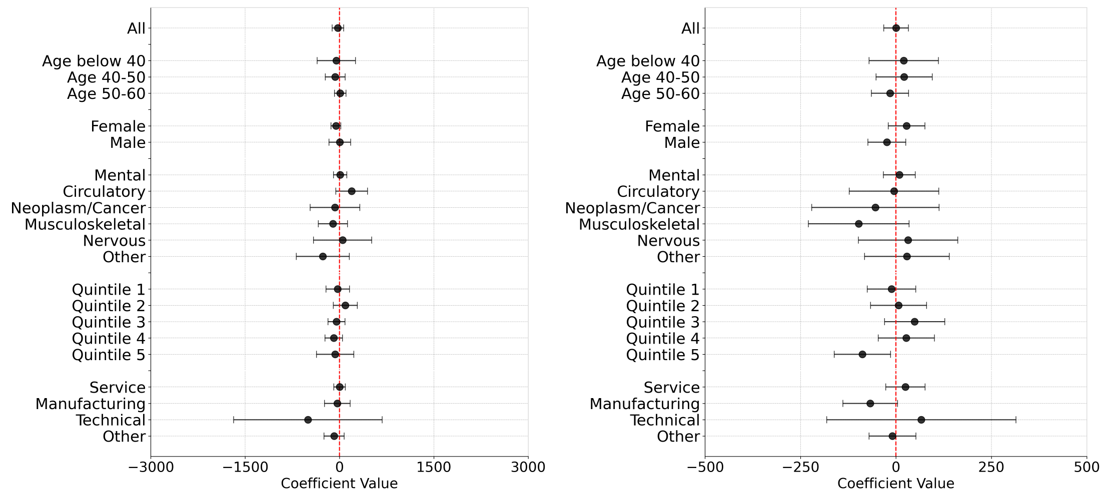
We also estimate the effects for the sample of permanent DI recipients, which we excluded from the main estimation sample since for this group selection at the cut-off is possible (see Section 3. For completeness, we also present the result for this group. As mentioned above, permanent DI recipients 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 lead to 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 employment.[31] 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 12 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. Specifically, we 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.

The heterogeneous earnings effects for insured and marginal employment are presented

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

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



(a) Insured Employment

(b) Marginal Employment

Figure 10: Heterogeneity in Earnings

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

in Figure 10. 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 earnings effect for DI recipients in marginal employment in the highest pension wealth quintile, which has the highest treatment intensity.[32] In line with the prediction of a negative income effect, we find a significant reduction in earnings from marginal employment in response to the increase in DI benefits for this group. The effect appears to be driven by the extensive margin, as we observe a corresponding decline in employment shares for this group (see Figure 24 in the Appendix). In Figure 28 in the Appendix, we show that this negative effect is robust to the choice of bandwidth. According to the point estimate, DI recipients in this group reduce earnings by about 88 Euros throughout the four years after DI award. The effect size increases over time from an average reduction of 72 Euros in the first year after DI award to a significant decrease of 122 Euros in the fourth year (see Figure 25 in the Appendix). The earnings response translates to an income effect of around -0.13 Euros per one Euro increase in DI benefits for this group. The magnitude of the effect is slightly lower but similar to the income effect of about 0.2 estimated in Gelber et al. (2017), who find significant responses in the context of DI in the US using a regression kink design. As in our context, the effect

[32]The benefit increase in the highest quintile lies on average at 703 Euros annually.

is related to individuals with relatively high DI benefits. While our effect is estimated for individuals in the highest quintile of DI benefits, Gelber et al. (2017) identify their elasticity at the upper bend point of the DI schedule which affects only DI recipients with monthly benefits of about $1,773 (the average benefits amount to $1,369, see Table 1 in Gelber et al. (2017)).[33]

The negative earnings response only affects a relatively small share of DI recipients. This explains why we estimate in our context on average an insignificant employment effect which is close to zero.

6.3 Return to the Labor Market

In the following, we use the RDD and focus on the probability of returning to the labor market instead of remaining in DI. This allows us to estimate the elasticity with respect to extending benefits. All individuals in the sample receive DI benefits on a temporary basis. Once benefits expire, recipients have to apply for an extension. If this extension is not granted, individuals will need to go back to the labor market, either to employment or unemployment. Regulation requires temporary DI 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. These rules do not change at the cut-off date and apply for all individuals in the sample. Thus, the only difference is the higher DI generosity for recipients who claimed benefits after the reform date.

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 11, 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 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 4). Consistent with the financial incentive and the institutional rules, there is no evidence for behavioral effects in the first three years (see also Figure 29 in the Appendix). However, we find a persistent, significant negative effect on labor market transitions in years four and five. After five years, individuals who entered DI with higher benefits are around 1 p.p. less likely to

[33]Since the DI schedule in Germany has a strong contributory link, there is no clear kink which we can be exploited for identification.

Table 4: Effect of DI Benefit Increase on Exit from Benefits

	All		Female		Male	
	(1)	(2)	(1)	(2)	(1)	(2)
Receiving pension benefits after ...						
1 Year	-0.001	-0.001	-0.001	-0.003	0.009	0.012
	(0.0019)	(0.0019)	(0.0021)	(0.0021)	(0.0023)	(0.0023)
	[4.7]	[4.7]	[5.8]	[5.9]	[8.0]	[8.0]
	0.99	*0.99*	*0.99*	*0.99*	*0.99*	*0.99*
2 Years	0.004	0.004	0.002	0.002	0.006	0.006
	(0.0025)	(0.0025)	(0.0035)	(0.0035)	(0.0040)	(0.0039)
	[8.4]	[8.1]	[5.9]	[6.0]	[9.1]	[9.2]
	0.98	*0.98*	*0.98*	*0.98*	*0.97*	*0.97*
3 Years	0.003	0.002	0.001	0.001	0.005	0.004
	(0.0035)	(0.0035)	(0.0046)	(0.0045)	(0.0053)	(0.0053)
	[7.0]	[7.0]	[6.8]	[6.9]	[7.6]	[7.6]
	0.96	*0.96*	*0.97*	*0.97*	*0.95*	*0.95*
4 Years	0.008*	0.008*	0.009*	0.009*	0.010	0.009
	(0.0032)	(0.0033)	(0.0037)	(0.0037)	(0.0057)	(0.0057)
	[9.8]	[9.4]	[11.5]	[11.3]	[8.5]	[8.3]
	0.95	*0.95*	*0.96*	*0.96*	*0.95*	*0.95*
5 Years	0.011**	0.010**	0.009	0.009	0.018*	0.016*
	(0.0039)	(0.0039)	(0.0047)	(0.0047)	(0.0075)	(0.0072)
	[7.8]	[7.7]	[8.2]	[8.0]	[5.7]	[5.8]
	0.95	*0.95*	*0.96*	*0.96*	*0.94*	*0.94*
Controls	No	Yes	No	Yes	No	Yes

Note: Table shows regression discontinuity estimates for the fraction of individuals receiving pension benefits up to 5 years after the initial benefit award. Bandwidths are shown in square brackets. Standard errors are in parentheses. Control means are printed in italics. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

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 16% in exits for the full sample. Given these estimates, we can calculate the elasticity of extension of DI past the initial granting period for an increase in benefits. We estimate that the 1.1 p.p. increase in recipients remaining on benefits in the last year we observe translates to an elasticity of around 0.17, meaning that a 1% increase in benefits induces 0.17% of temporary recipients to extend benefits past the initial granting period successfully.

Figure 29 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

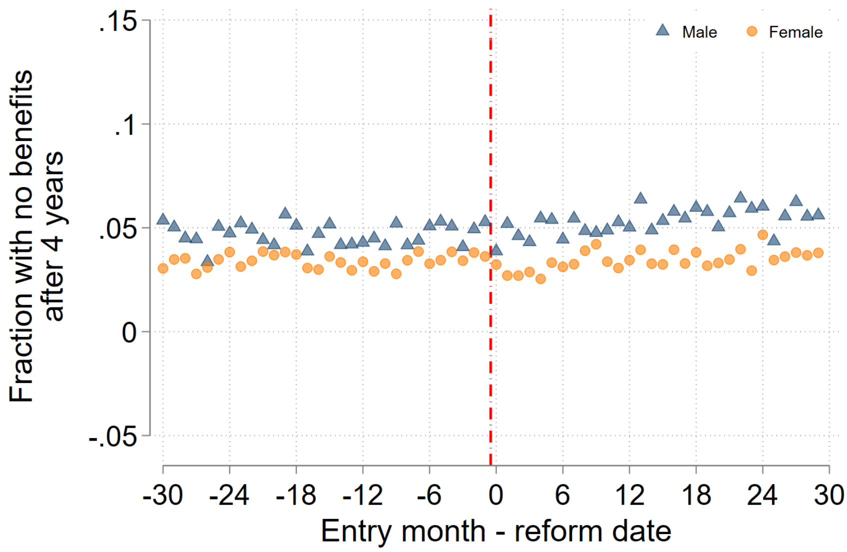


Figure 11: Fraction with No Benefits after Four Years

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

becoming a recipient, the effect materializes only in the fourth year after the benefit award and remains significant for all choices of bandwidth thereafter. The heterogeneity analysis furthermore reveals that results are driven by sub-samples of individuals with circulatory, musculoskeletal, and nervous system disorders. We furthermore find larger effect sizes for younger recipients and those in the lower two and fourth pension wealth quintiles.

As only a small share of individuals in our sample makes a transition to the labor market, it is not possible to further quantify whether individuals enter employment or another labor market status. In Figure 19 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 sizable effects in the long run when the period of the temporary benefits ends, and reapplication is required. The sizable exit effects are consistent with the findings of Kostøl and Mogstad (2014) who document in the context of Norway that

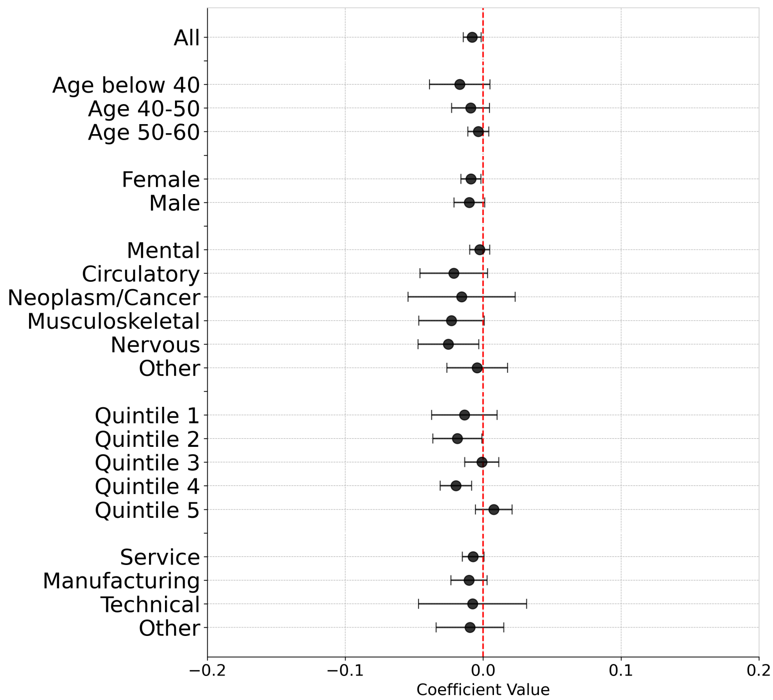


Figure 12: Heterogeneity in Return to the Labor Market

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

financial incentives encourage DI recipients to return to work.[34]

6.4 Mortality

Finally, we turn to the potential effects of an increase in DI benefits on mortality. Mortality outcomes are relevant for the evaluation of DI designs from a welfare perspective. Mortality is a strong welfare indicator as changes in mortality due to benefit generosity are likely to proxy for a larger array of health and social welfare effects, see e.g. Black et al. (2018), García-Gómez and Gielen (2018), and Gelber et al. (2023). At the same time, mortality outcomes are relevant from a fiscal perspective. Since DI is an absorbing state for most recipients, DI spells usually end at death or when benefits are replaced by old age pensions (which are equivalent to DI benefits in many systems, such as the German DI system). Thus, a reduction in mortality leads to higher fiscal costs.

A substantial share of DI recipients dies in the years following the award of benefits.

[34]Since Kostøl and Mogstad (2014) analyze the effect of a large change in the earnings threshold for DI recipients, results can be only compared qualitatively.

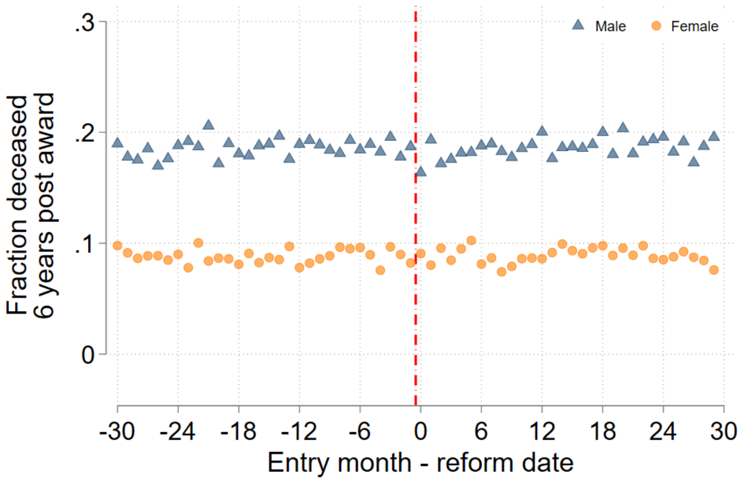


Figure 13: Fraction Deceased after 6 years

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

Figure 20 in the Appendix shows 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 as 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.

To study the effects of the benefit increase on mortality, we track the fraction of 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 6.1. Figure 13 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 5, 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

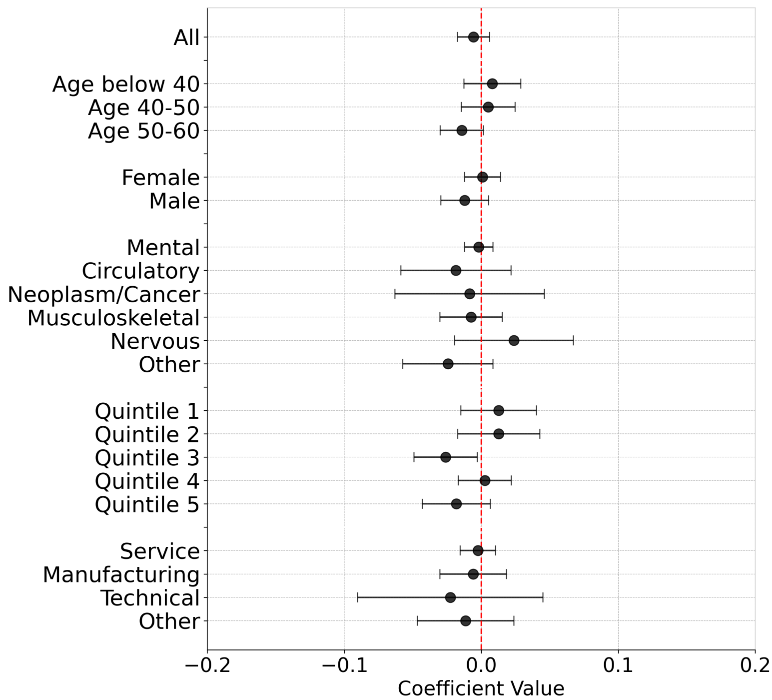


Figure 14: Heterogeneity in Mortality

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

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 30 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 14). 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 rela-

Table 5: Effect of DI Benefit Increase on Mortality

Outcome: *Fraction deceased ...*	All (1)	All (2)	Female (1)	Female (2)	Male (1)	Male (2)
After 3 years	0.001	-0.001	0.003	-0.001	-0.002	-0.001
	(0.005)	(0.0045)	(0.0054)	(0.0050)	(0.0080)	(0.0071)
	[6.7]	[7.1]	[7.6]	[7.6]	[7.5]	[8.0]
Dep. Mean	*0.08*	*0.08*	*0.05*	*0.05*	*0.11*	*0.11*
N	59,676	59,676	36,052	36,052	32,712	32,712
After 6 years	-0.006	-0.007	0.001	-0.005	-0.012	-0.006
	(0.0060)	(0.0055)	(0.0067)	(0.0063)	(0.0089)	(0.0075)
	[7.4]	[7.2]	[7.7]	[7.1]	[8.9]	[10.9]
Dep. Mean	*0.13*	*0.13*	*0.09*	*0.09*	*0.18*	*0.18*
Controls	No	Yes	No	Yes	No	Yes
N	59,676	59,676	36,052	31,402	36,731	45,185

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 8 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: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$.

tively 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. Finally, we again extend the sample and also focus on the mortality effects for permanent DI recipients. For this group, the results are very similar, as we do not find any significant effect of higher benefit generosity on DI (see bottom panel in Table 12 in Appendix).[35]

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

[35] In the data we only observe mortality for DI recipients. Therefore, we can not estimate mortality effects related to DI take-up as in Black et al., 2018.

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 (Black et al., 2018; Gelber et al., 2023). For instance, Gelber et al. (2023) note that effects are highest shortly after the award of DI before Medicare eligibility begins. This group has high out-of-pocket costs. Instead, they find lower effects for SSI recipients who have access to Medicaid, concluding that medical expenditures may be an important driver of income-related mortality effects in the US policy context.

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 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.

To provide additional empirical background for our findings of a zero effect, we supplement the results of the causal analysis with more descriptive evidence about the association between DI benefits and mortality. Table 13 in the Appendix shows the results of OLS estimates of log DI benefits on mortality for the recipients in our sample. The OLS estimates, which are likely biased due to omitted variables, range between -0.008 and -0.028, implying a six-year mortality elasticity of -0.06 to -0.22. These estimates are larger than our causal effects, consistent with a positive omitted variable bias of the OLS. Specifically, the effect of DI benefits estimated with OLS might capture unobserved individual effects, which are controlled for in the RRD. However, despite the upward bias of the mortality elasticity, effects are much lower than the causal estimates reported in Gelber et al. (2023), which range between -0.6 and -1.0 for annual mortality rate. The limited relationship between DI benefits and mortality in the German context is further corroborated by a comprehensive analysis of the relationship between mortality and other relevant factors. Figure 31 in the Appendix shows results of multivariate OLS regressions including quintiles of DI benefits, demographic characteristics, diagnoses, and labor market history of recipients. The results support the findings of our causal estimates of DI benefits on recipient mortality. While higher DI income is associated with lower mortality rates, the correlation is fairly low compared to other individual characteristics. The most important predictor of mortality in our data is a cancer diagnosis (in the primary as well as secondary diagnosis), followed by other (physical) diagnoses, age, and gender.[36]

7 Discussion

Table 6: Elasticities of Outcomes with Respect to Positive Change in Benefits

	Takeup	Work as recipient		Extension rate
	(Age 50-60)	*Marginal*	*Insured*	
Estimate	0.000079***	0.004	-0.002	0.011**
	(0.000011)	(0.0063)	(0.0034)	(0.0039)
Relative Change	4.6%	–	–	1.1%
Elasticity	0.53	0	0	0.17
CI (Elasticity)	[0.38, 0.67]	[-0.6, 1.2]	[-2.6, 1.2]	[0.05, 0.29]

Note: Table shows the coefficient estimates and resulting elasticities for the increase in benefits. Significance levels: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

In the final section, we combine the empirical evidence with the theoretical framework developed in Section 2, to discuss the insurance-efficiency trade-off for a change in benefit generosity. As discussed in Section 2, the insurance-efficiency trade-off is described in the following equation, which shows that an increase in benefits b improves welfare if the insurance effect exceeds the fiscal multiplier:

$$\frac{\partial W(b)}{\partial b} \geq 0 \iff 1 + \frac{B_T(b) + B_I(b) + B_R(b)}{M(b)} \leq \frac{\frac{\partial V(b)}{\partial b}}{\lambda M(b)}.$$

We compute the fiscal multiplier of a DI benefit increase using the elasticities we have estimated in the previous sections and which are summarized in Table 6. We compute the fiscal multiplier for a 1 percent increase in benefits for an average 2014 DI recipient in our sample. Based on the data we define the relevant parameters for an average 2014 DI recipient. First, we use the average DI benefit payment of 765 Euros per month in our

[36]The correlation between benefits and mortality is fairly robust to the choice of control variables. Omitting employment controls, specifically average pension points, increases the estimates slightly as pension points are highly correlated with DI benefit levels (see Table 13 in the Appendix).

Table 7: Fiscal Multiplier for Increase in DI Benefits

	Permanent	Temporary
Total Effect	159.42	170.53
Mechanical Effect	91.79	88.08
Behavioral Effect Takeup ($B_T(b)$)	67.62	64.88
Behavioral Effect Employment ($B_W(b)$)	0	0
Behavioral Effect Extension ($B_E(b)$)	0	17.56
Fiscal Multiplier (ignoring exit)	1.7366	
Fiscal Multiplier	1.8343	

Note: Table shows the computed mechanical and behavioral effects on fiscal costs given the estimated elasticities for a change in DI benefits. We compute the multiplier assuming 50% of recipients are assigned to temporary benefits initially, which corresponds roughly to the ratio observed in the German DI data.

sample to compute the costs related to changes in benefits. Secondly, we calculate that a recipient in the absence of DI benefits would have earned 27,871 Euros per year and paid 3487 Euros of taxes in the absence of benefits. These earnings correspond to 84% of the average income of workers subject to social security contributions in the year 2014. The average recipient in our sample has earned 0.84 pension credits throughout their career before claiming benefits. Third, we calculate a duration $D = 0.19$ for temporary recipients corresponding to the share of time an average recipient in our sample can expect to receive benefits before retirement.[37] Fourth, we use a baseline extension rate of 0.95, which corresponds to the control group extension rate in our data. Lastly, in line with the data we presume that 50% of recipients are awarded permanent benefits straight away.

Based on these parameters, we calculate the fiscal multiplier given the estimated elasticity of 0.53 with respect to take-up and 0.17 with respect to extensions and the empirical evidence that the employment effect for DI recipients is not significant. The calculated mechanical and behavioral costs of a one percent increase in benefits for permanent and temporary recipients are shown in the upper panel of Table 7. The total effect for the temporary recipients is higher since the behavioral extension effect, which does by definition not apply to the permanent recipients, increases the overall cost. In the second panel of the table, we present the corresponding fiscal multiplier. We additionally compute the fiscal multiplier of increasing DI benefits when considering only the take-

[37]The average recipient in our sample is 49 and will retire at 65.

up margin, similar to Haller et al. (2024) or Haller and Staubli (2025). We calculate a fiscal multiplier of 1.83. When ignoring the behavioral margins of DI recipients and only considering the take-up effect, the fiscal multiplier is reduced to 1.74.[38] Incorporating behavioral adjustments of DI recipients thus increases the estimated fiscal multiplier of a positive benefit adjustment in DI by around 5%. This shows that the take-up effect is the main driver of the behavioral effect on fiscal costs. This is consistent with two empirical findings. First, the estimated take-up elasticity is larger than the extension elasticity, and second, although the relative size of the extension effect is large in absolute terms, it only affects a small number of DI recipients.

Given the data limitations, we cannot directly estimate the related insurance effect of higher DI benefits.[39] However, we can compare our incentive effect to the insurance effects of DI benefits estimated in the previous literature. For instance, Haller and Staubli (2025) estimate an insurance effect of an increase in DI benefits of 2.2 for Canada and 3.4 for the US, which exceeds the incentive effect estimated in our study. Importantly, this is true for the incentive effect with and without the additional behavioral margin of DI recipients. Thus, when taking our theoretical framework and these insurance values, we can conclude that an increase in DI benefits is welfare improving which is consistent with the findings in Haller et al. (2024) and Haller and Staubli (2025). Our empirical findings of non-significant mortality effects does not change this conclusion. First, the zero effect implies that higher generosity does not provide additional non-monetary welfare for the individual since life expectancy does not change. Second, there are no additional fiscal effects that would be present if DI recipients live longer and thus would receive benefits for a longer period.

8 Conclusion

In this paper, we focus on a comprehensive measure of inefficiencies related to the generosity of DI benefits that encompasses not only the effect on DI take-up, but also the behavioral responses of DI recipients, namely employment during DI receipt and return to the labor market. To understand the implications of this broader measure of

[38] Haller et al. (2024) estimate a fiscal multiplier for DI generosity in Austria of 1.4 and Haller and Staubli (2025) a multiplier of 1.6 for Canada and 2.2 for the US.

[39] In the German social security data, we do not observe the household context which Haller et al. (2024) use to estimate bounds of the insurance effect, and there are no consumption data. Moreover, there exist no reliable estimates of DI take-up effects related to wage changes which can be used in combination with take-up effects of DI benefits to calculate the insurance effect, see Haller and Staubli (2025).

the trade-off between insurance and incentives when adjusting DI benefit generosity, we extend a theoretical framework by Haller et al. (2024) on welfare implications of DI generosity and show that these additional behavioral margins increase incentive costs, driven by their respective elasticities. Based on administrative data from the German pension insurance and exogenous policy variation, we provide novel empirical estimates of the three behavioral effects.

We document meaningful incentive effects of the increase in DI benefits in two important dimensions. We find significant and positive take-up effects which imply that an increase of DI benefits of 1% increases take-up of DI by around 0.5%. The extension elasticity, which measures the probability of returning to the labor market, is also significant and with 0.17 of meaningful size. In contrast, the results show on average no significant effect on employment and earnings of DI recipients. We combine the novel empirical evidence with the theoretical framework and show that our comprehensive measure of incentive effects leads to a fiscal multiplier of 1.83. We further show that additional behavioral margins of DI recipients are modest and increase the fiscal multiplier by only 5%. When only considering the take-up margin, we estimate a fiscal multiplier of 1.74. These efficiency effects are smaller than the insurance effects of DI benefits estimated in the previous literature. This suggests that in the context of the theoretical framework an increase in DI benefits is welfare improving despite the additional margins of incentive effects.

We also present empirical results on how the generosity of DI benefits affects mortality of DI recipients. Despite the high mortality risk among DI recipients and the increase in benefit generosity, mortality rates remain unchanged. This result holds both in the short run (after three years) and in the long run (after six years). This suggests that non-monetary effects are more important to explain mortality, which we also document in additional descriptive analysis.

This finding is also important for the interpretation of the efficiency-insurance trade-off. First, the zero effect implies that higher generosity does not provide additional non-monetary welfare for the individual since life expectancy does not change. Second, there are no additional fiscal effects which would be present if DI recipients live longer and thus would receive benefits or a longer period.

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Appendix

A.1 Model

The government objective discussed in Section 2 balances the value of disability insurance $V(b)$ given a benefit level b with the fiscal cost of the program $G(b)$. Below, we formulate the aggregate value function of the DI program given a random distribution of work impairments $F(\theta)$ and the resulting government budget for taxes τ and $\bar{\tau}$ and social assistance benefits z for thresholds of θ such that $\theta^A < \theta^E < \theta^W < \theta^R$.

Value Function

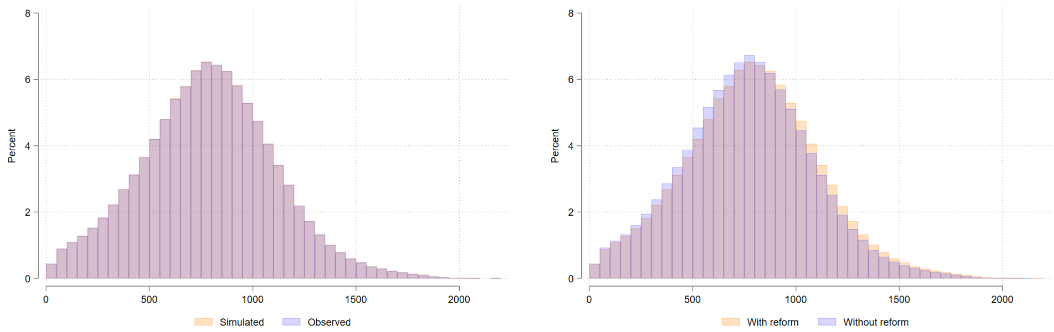
$$
\begin{aligned}
V(b) = & \underbrace{\int_0^{\theta^A} \left(u(c^w) - \theta\right) dF(\theta)}_{\text{(1) Non-applicants}} + \underbrace{\int_{\theta^A}^{\theta^R} (1 - p(\theta)) \left(u(c^w) - \theta\right) dF(\theta)}_{\text{(2) Rejected applicants who continue working}} + \underbrace{\int_{\theta^R}^{\infty} (1 - p(\theta))\, u(c^z) dF(\theta)}_{\text{(3) Rejected applicants on welfare}} \\
& + \underbrace{\int_{\theta^A}^{\theta^W} p_p(\theta) v(b^e) dF(\theta)}_{\text{(4) Permanent working DI recipients}} + \underbrace{\int_{\theta^W}^{\infty} p_p(\theta) v(b^n) dF(\theta)}_{\text{(5) Permanent non-working DI recipients}} \\
& + \underbrace{\int_{\theta^A}^{\theta^E} p_t(\theta) \left[D v(b^e) + (1 - D)(u(c^w) - \theta)\right] dF(\theta)}_{\text{(6) Temporary working non-extending recipients}} \\
& + \underbrace{\int_{\theta^A}^{\theta^E} p_t(\theta) \left[t(\theta) v(b^e) + (1 - t(\theta)(D v(b^e) + (1 - D)(u(c^w) - \theta)\right] dF(\theta)}_{\text{(7) Temporary extending working recipients}} \\
& + \underbrace{\int_{\theta^E}^{\infty} p_t(\theta) t(\theta) v(b^n) dF(\theta)}_{\text{(8) Temporary extending non-working recipients}} \\
& + \underbrace{\int_{\theta^E}^{\theta^R} p_t(\theta)(1 - t(\theta) \left[D v(b^n) + (1 - D)(u(c^w) - \theta)\right] dF(\theta)}_{\text{(9) Temporary + non-working + rejected extension + return to LM}} \\
& + \underbrace{\int_{\theta^R}^{\infty} p_t(\theta)(1 - t(\theta)) \left(D v(b^n) + (1 - D) v(b^z)\right) dF(\theta)}_{\text{(10) Temporary + non-working DI + rejected extension + return to SA}} \\
& + \underbrace{\int_{\theta^A}^{\infty} \psi(\theta) dF(\theta)}_{\text{Application cost}} + \underbrace{\int_{\theta^E}^{\infty} p_t(\theta) \psi(\theta) dF(\theta)}_{\text{Extension cost}} \qquad (10)
\end{aligned}
$$

Government Budget

$$
\begin{aligned}
G(b) = \; & \underbrace{\tau\left[F(\theta^A) + \int_{\theta_A}^{\theta_R} (1 - p(\theta))\, dF(\theta)\right]}_{\text{Tax revenue from healthy \& rejected workers}} \\
+ \; & \underbrace{\bar{\tau}\int_{\theta_A}^{\theta_W} p_p(\theta) dF(\theta)}_{\text{Tax revenue from working permanent recipients}} \quad + \quad \underbrace{\int_{\theta^A}^{\theta^E} p_t(\theta)\,(D\bar{\tau} + (1-D)\tau)\, dF(\theta)}_{\text{Tax revenue from working DI recipients who do not extend}} \\
+ \; & \underbrace{\int_{\theta^E}^{\theta^W} p_t(\theta)\,(t(\theta)\bar{\tau} + (1 - t(\theta))(D\bar{\tau} + (1-D)\tau))\, dF(\theta)}_{\text{Tax revenue from extending DI recipients}} \\
+ \; & \underbrace{\int_{\theta^W}^{\infty} p_t(\theta)\,((1 - t(\theta)(1-D)\tau)\, dF(\theta)}_{\text{Tax revenue from rejected extensions returning to work}} \\
- \; & \underbrace{b\left[\int_{\theta_A}^{\infty} p_p(\theta)\, dF(\theta) + \int_{\theta_A}^{\theta^E} p_t(\theta) D\, dF(\theta) + \int_{\theta_E}^{\infty} p_t(\theta)(t(\theta) + D(1 - t(\theta))\, dF(\theta)\right]}_{\text{DI payments to recipients}} \\
- \; & \underbrace{z\left[\int_{\theta_R}^{\infty} 1 - p(\theta) + p_t(\theta)(1 - t(\theta))(1-D) dF(\theta)\right]}_{\text{Social assistance payments for rejected applicants \& after DI expiration}}. \qquad (11)
\end{aligned}
$$

A.2 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



(a) Observed and Simulated Benefits (b) Benefit Distribution with Reform

Figure 15: Distribution of Observed and Simulated Benefits

Note: The figure shows the distribution of (standardized and corrected) DI benefits compared against our own benefit computation based on information from the insurance account of recipients.

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 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 & 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 15a shows that this procedure allows us to perfectly compute entitlements from the insurance data.

Figure 15b 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.

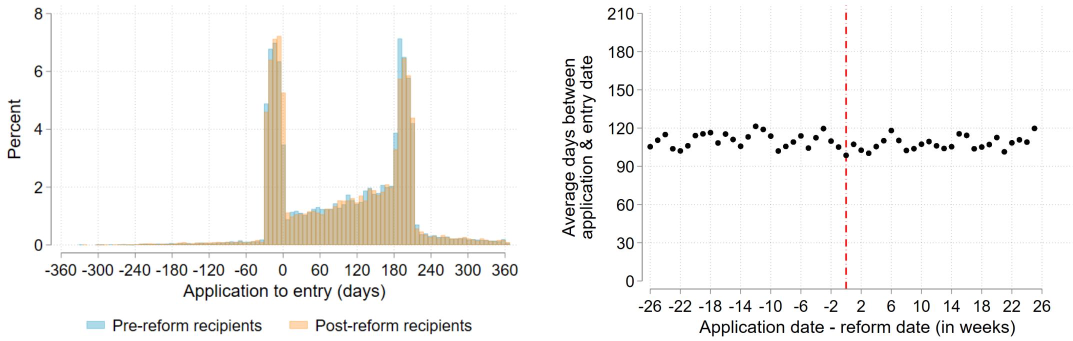
A.3 Additional Validity Check

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 3, 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 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 16a 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 16: Distance between Application Date and Entry Date

Note: Figure 16a 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 16b 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 6.1. 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 8 and Figure 16b 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 8: 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

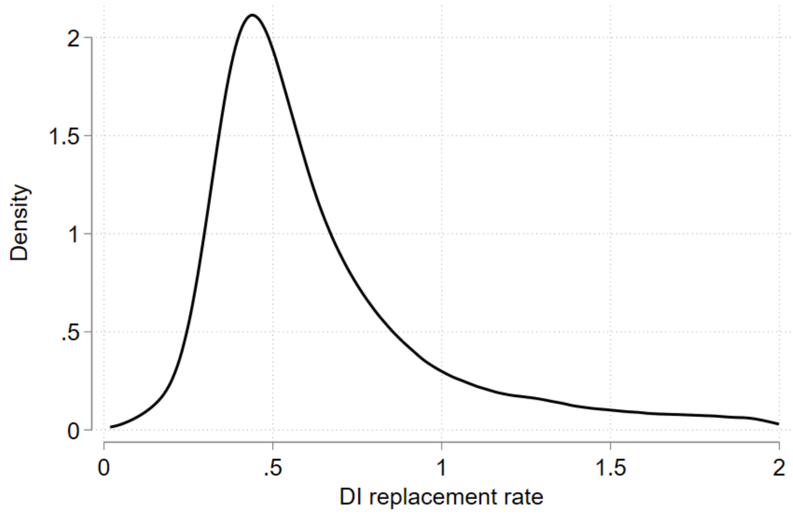


Figure 17: Replacement Rate of Benefits

Note: The figure shows the replacement rate of benefits compared to income one year before take-up. Income in the year preceding take-up can comprise a combination of employment income, unemployment benefits, and sickness benefits.

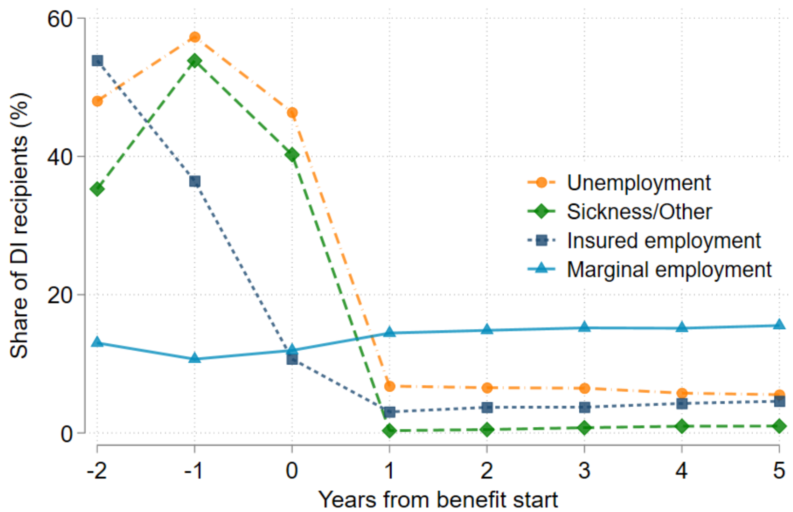


Figure 18: Employment Status of Recipients

Note: The figure shows the employment status of pre-reform recipients before and after entry into DI. Data is collected annually, and individuals can hold multiple employment states over the year, such that percentages do not sum to one.

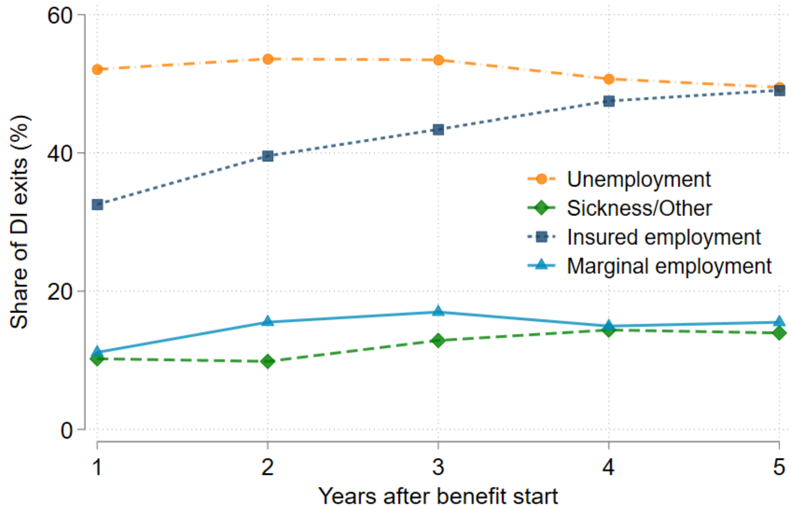


Figure 19: Employment Status after Return to the Labor Market

Note: The figure shows employment status of pre-reform recipients after they return to the labor market. Data is collected annually, and individuals can hold multiple employment states over the year, such that percentages do not sum to one.

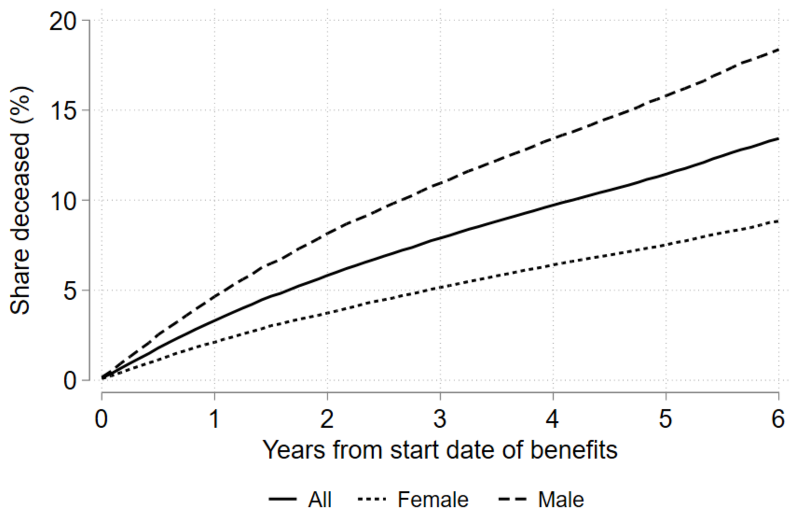


Figure 20: Fraction Deceased Over Time by Gender

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 9: 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 10: 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	-0.001	-0.001	-0.001	-0.000	-0.001	-0.001
	(0.0027)	(0.0027)	(0.0034)	(0.0035)	(0.0038)	(0.0038)
	[8.9]	[8.6]	[9.1]	[8.6]	[10.5]	[10.1]
	0.02	*0.02*	*0.02*	*0.02*	*0.03*	*0.03*
2 Years	0.000	0.001	-0.001	-0.001	0.002	0.002
	(0.0028)	(0.0028)	(0.0034)	(0.0034)	(0.0046)	(0.0046)
	[7.8]	[7.7]	[9.4]	[8.9]	[7.0]	[6.9]
	0.03	*0.03*	*0.02*	*0.02*	*0.03*	*0.03*
3 Years	-0.002	-0.002	-0.007*	-0.007*	0.000	0.000
	(0.0022)	(0.0022)	(0.0032)	(0.0032)	(0.0040)	(0.0040)
	[10.5]	[10.4]	[8.3]	[8.3]	[7.5]	[7.4]
	0.02	*0.02*	*0.02*	*0.02*	*0.03*	*0.03*
4 Years	-0.004	-0.004	-0.004	-0.003	-0.003	-0.003
	(0.0029)	(0.003)	(0.0034)	(0.0035)	(0.0043)	(0.0043)
	[6.2]	[6.0]	[7.6]	[7.4]	[7.2]	[7.3]
	0.02	*0.02*	*0.02*	*0.02*	*0.02*	*0.02*
Panel B: Marginal Employment						
1 Year	0.000	-0.001	0.003	0.004	-0.003	-0.005
	(0.0056)	(0.0057)	(0.0078)	(0.0079)	(0.0081)	(0.0083)
	[9.0]	[8.7]	[9.0]	[8.6]	[9.3]	[8.6]
	0.14	*0.14*	*0.15*	*0.15*	*0.14*	*0.14*
2 Years	0.006	0.006	0.012	0.013	0.000	-0.001
	(0.0055)	(0.0056)	(0.0078)	(0.0079)	(0.0077)	(0.0081)
	[10.2]	[9.7]	[9.1]	[8.9]	[11.4]	[9.9]
	0.15	*0.15*	*0.15*	*0.15*	*0.15*	*0.15*
3 Years	-0.000	-0.000	0.006	0.008	-0.007	-0.009
	(0.0054)	(0.0056)	(0.0081)	(0.008)	(0.0087)	(0.0089)
	[10.9]	[10.2]	[9.2]	[9.5]	[9.3]	[8.7]
	0.15	*0.15*	*0.16*	*0.16*	*0.15*	*0.15*
4 Years	0.007	0.007	0.021**	0.022**	-0.012	-0.011
	(0.0056)	(0.0055)	(0.0079)	(0.0081)	(0.0096)	(0.009)
	[10.7]	[11.0]	[9.7]	[9.3]	[8.3]	[9.2]
	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 8 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 11: Effect of DI Benefit Increase on Annual Earnings

Outcome: *Earnings after ...*	All (1)	All (2)	Female (1)	Female (2)	Male (1)	Male (2)
Panel A: Insured Employment						
1 Year	-11.215	-10.429	-59.195	-54.676	42.419	42.308
	(47.6876)	(47.6020)	(46.8582)	(47.9306)	(84.8599)	(85.0783)
	[8.0]	[8.0]	[9.4]	[8.9]	[7.9]	[7.9]
	310.40	*310.40*	*230.57*	*230.57*	*401.44*	*401.44*
2 Years	9.713	11.784	-18.838	-14.956	-19.272	-16.560
	(57.1324)	(56.9527)	(49.3053)	(50.0570)	(97.2058)	(97.3223)
	[6.2]	[6.1]	[10.1]	[9.7]	[6.7]	[6.6]
	357.91	*357.91*	*273.42*	*273.42*	*456.75*	*456.75*
3 Years	-16.261	-15.909	-68.418	-68.553	41.982	43.726
	(44.2601)	(44.5234)	(53.4154)	(52.0033)	(84.6869)	(84.2168)
	[8.8]	[8.7]	[8.5]	[8.8]	[6.9]	[6.9]
	338.61	*338.61*	*251.14*	*251.14*	*442.94*	*442.94*
4 Years	-79.265	-79.493	-73.101	-72.122	-92.734	-91.645
	(53.6389)	(53.7417)	(44.6178)	(44.9008)	(95.9432)	(95.2122)
	[6.4]	[6.3]	[11.5]	[11.2]	[6.0]	[6.0]
	312.80	*312.80*	*241.59*	*241.59*	*399.36*	*399.36*
Panel B: Marginal Employment						
1 Year	-5.407	-6.775	3.877	4.426	-13.383	-17.280
	(16.7820)	(17.3837)	(24.7341)	(24.6796)	(27.4235)	(28.0163)
	[11.1]	[10.3]	[9.4]	[9.3]	[9.5]	[8.8]
	372.93	*372.93*	*377.34*	*377.34*	*367.90*	*367.90*
2 Years	9.113	6.972	30.454	33.228	-16.618	-21.898
	(20.8165)	(21.5028)	(27.9168)	(28.1488)	(31.8986)	(32.7864)
	[8.6]	[8.0]	[8.6]	[8.4]	[8.4]	[7.8]
	432.68	*432.68*	*431.72*	*431.72*	*433.80*	*433.80*
3 Years	11.453	13.405	50.430	53.355	-29.032	-32.973
	(20.6562)	(21.7900)	(29.4189)	(29.5769)	(31.9958)	(32.7721)
	[10.2]	[9.1]	[8.9]	[8.7]	[9.8]	[9.2]
	466.98	*466.98*	*464.72*	*464.72*	*469.67*	*469.67*
4 Years	8.970	7.537	51.908	55.092	-50.616	-49.312
	(21.6786)	(20.3040)	(28.3554)	(28.5250)	(37.1466)	(35.0478)
	[10.6]	[11.9]	[10.8]	[10.6]	[8.5]	[9.3]
	492.65	*492.65*	*488.02*	*488.02*	*498.27*	*498.27*

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 8 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: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

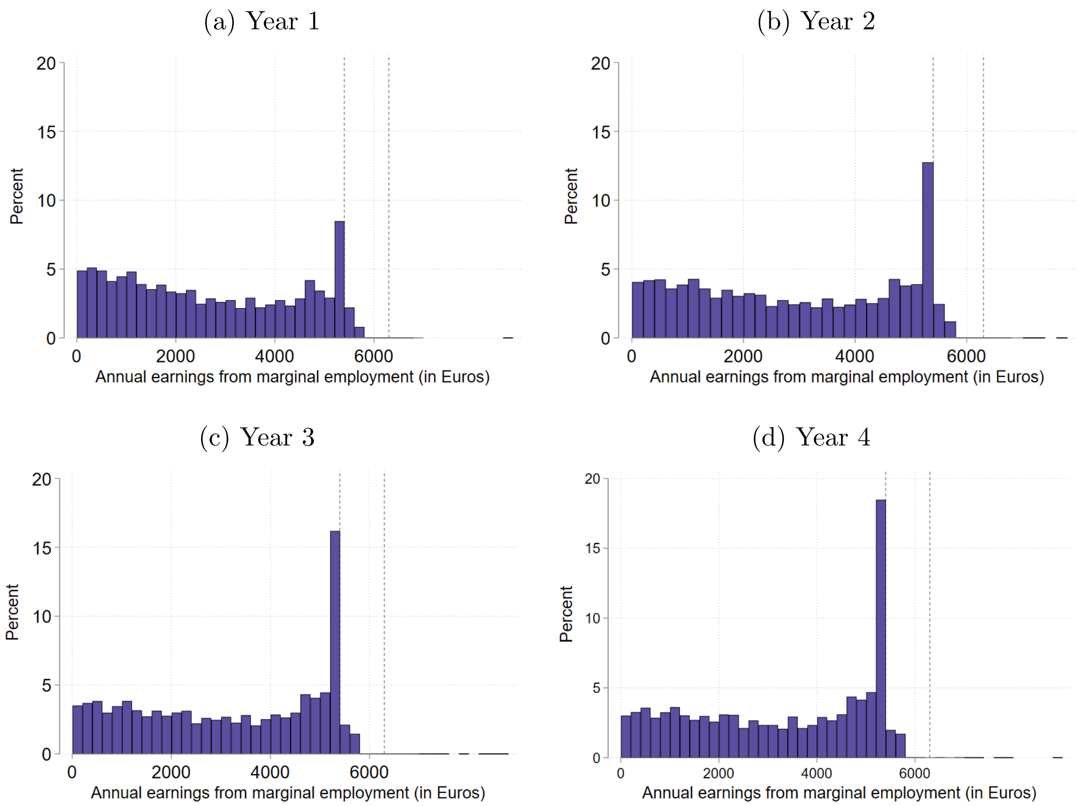


Figure 21: Distribution of Earnings from Marginal Employment

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.

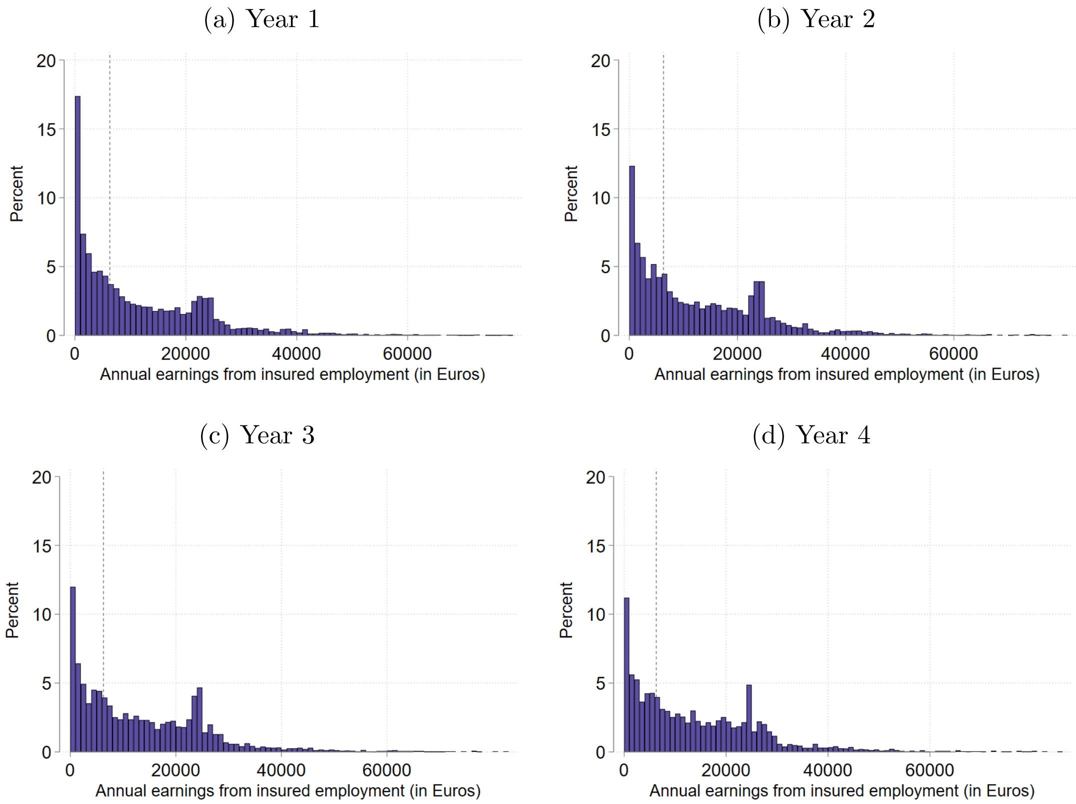


Figure 22: Distribution of Earnings from Insured Employment

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.

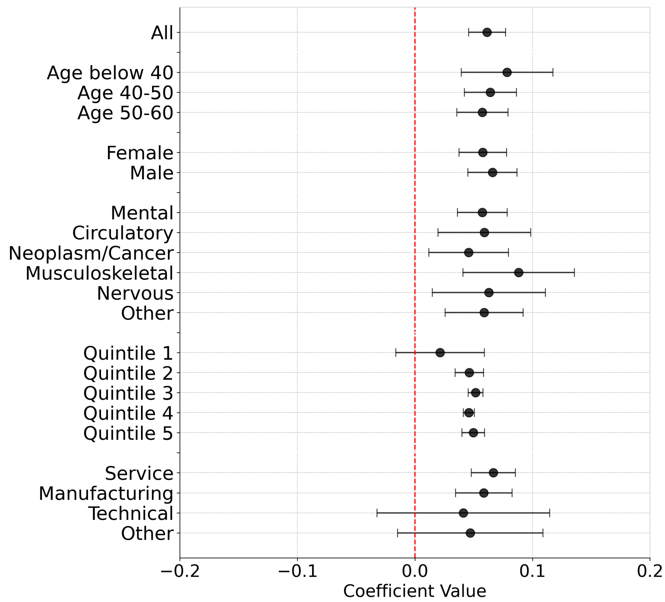
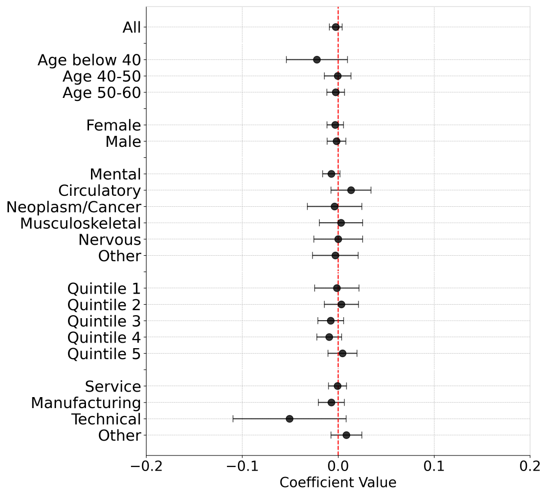
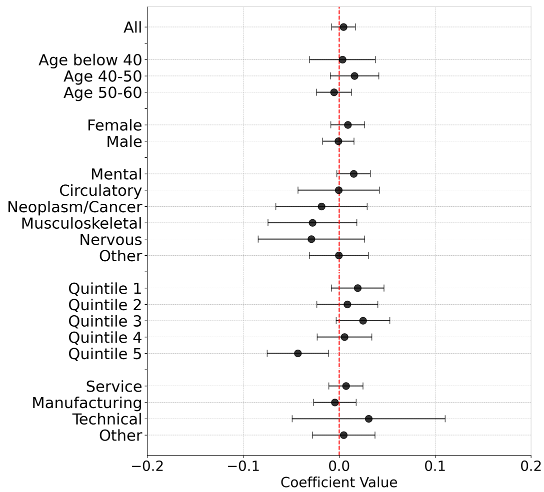


Figure 23: Relative Benefit Increase by Subsample

Note: The figure shows the relative benefit increase due to the reform by subsample in our data. Underlying pensions are standardized to pension values in the year 2014.



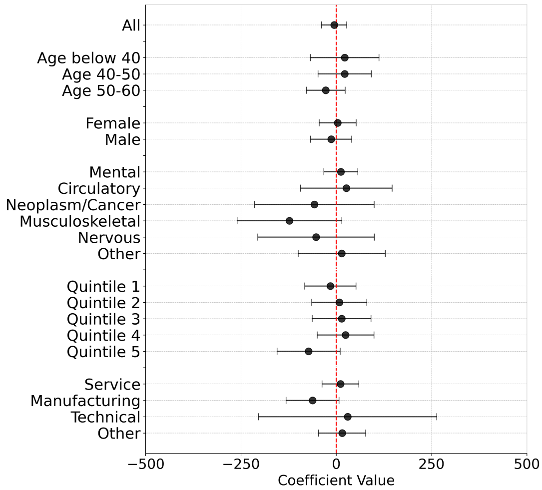
(a) Insured Employment



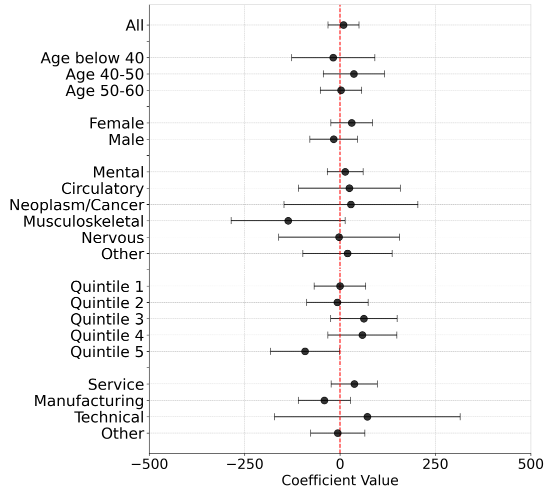
(b) Marginal Employment

Figure 24: Heterogeneity in Employment

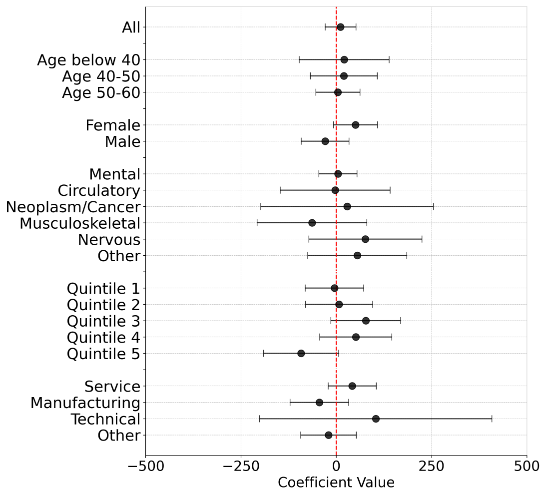
Note: The figure shows the point estimates and confidence bands for employment outcomes across subsamples in our data. We split the data by age, gender, primary diagnosis, benefit quintile, and last occupation.



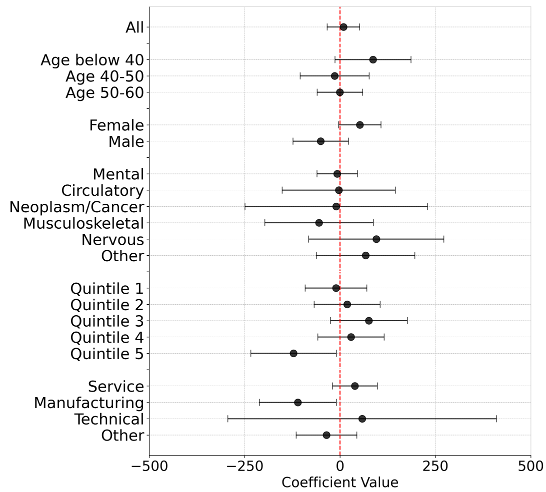
(a) Year 1



(b) Year 2

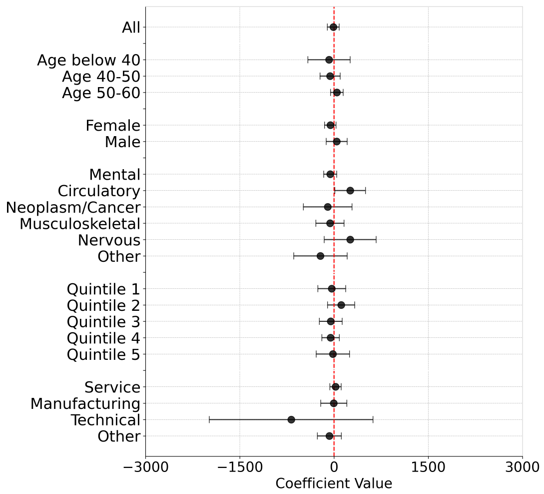


(c) Year 3

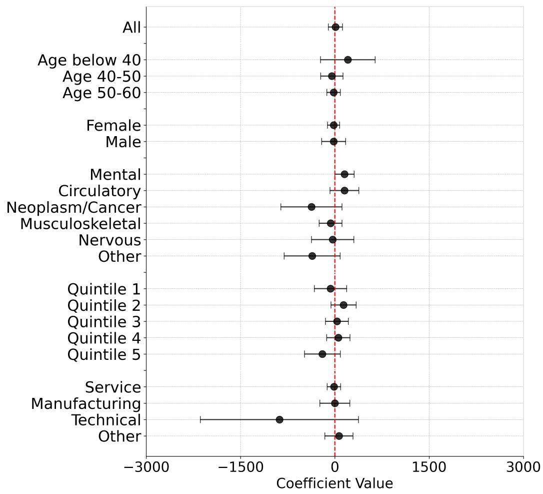


(d) Year 4

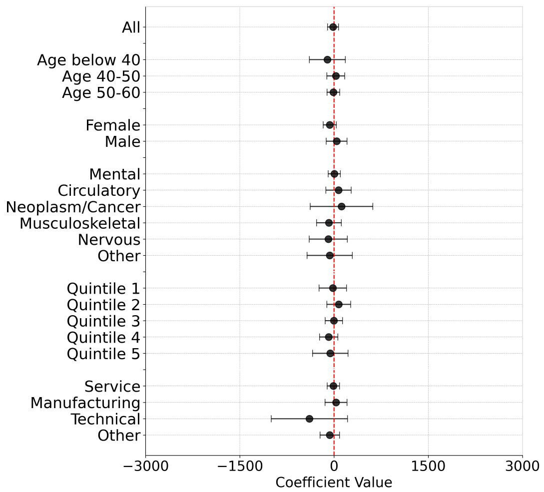
Figure 25: Heterogeneity in Earnings from Marginal Employment



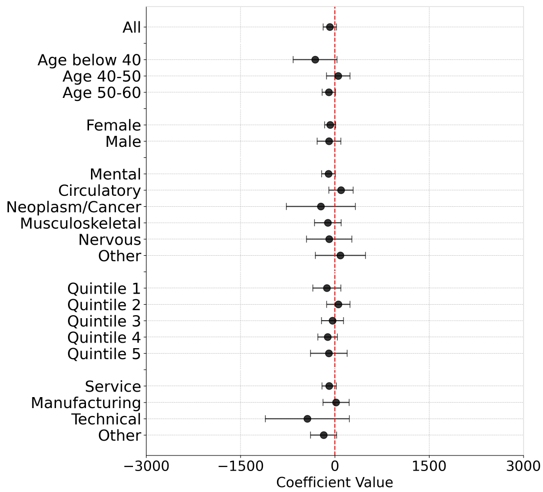
(a) Year 1



(b) Year 2



(c) Year 3



(d) Year 4

Figure 26: Heterogeneity in Earnings from Insured Employment

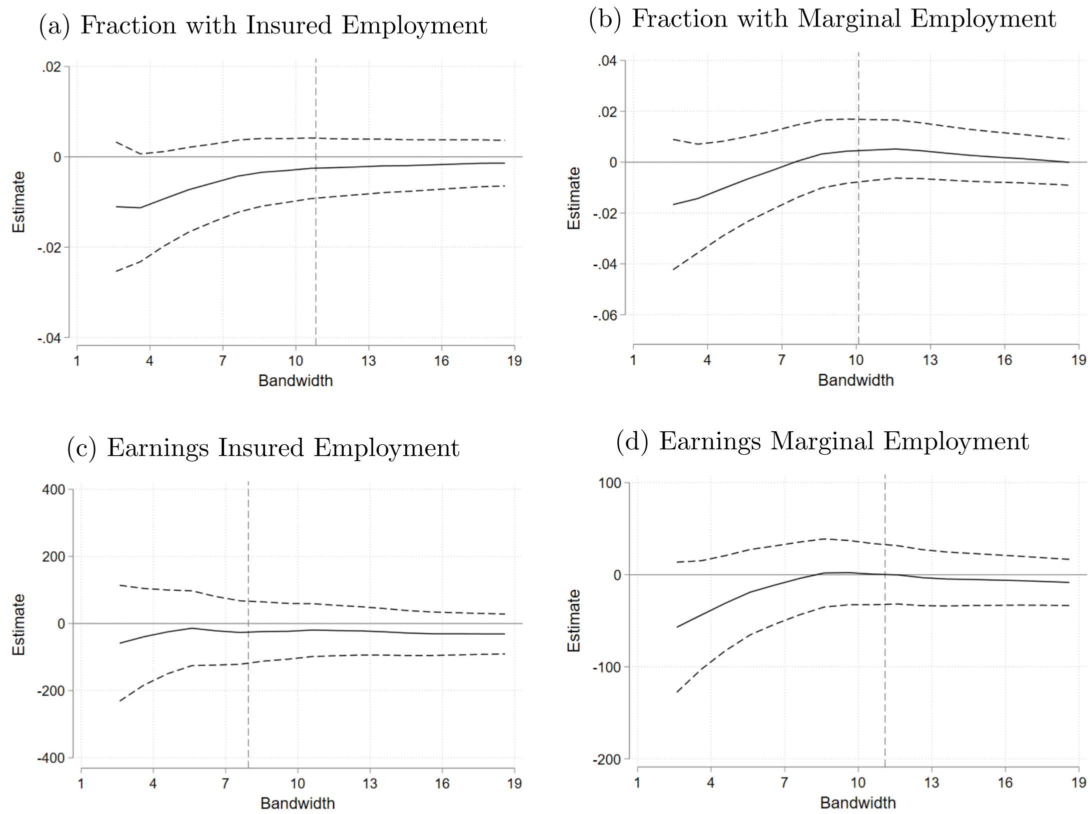


Figure 27: Bandwidth Sensitivity of Employment Estimates

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.

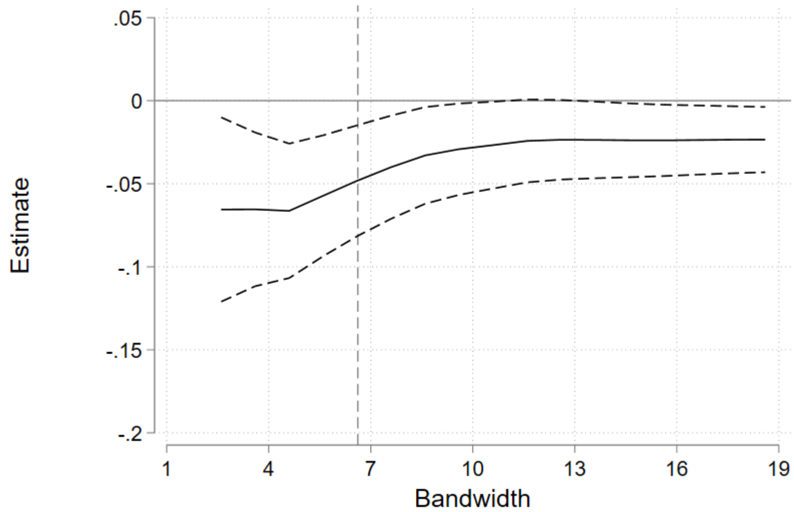


Figure 28: Bandwidth Sensitivity of Marginal Employment Estimates for High Benefit Recipients

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

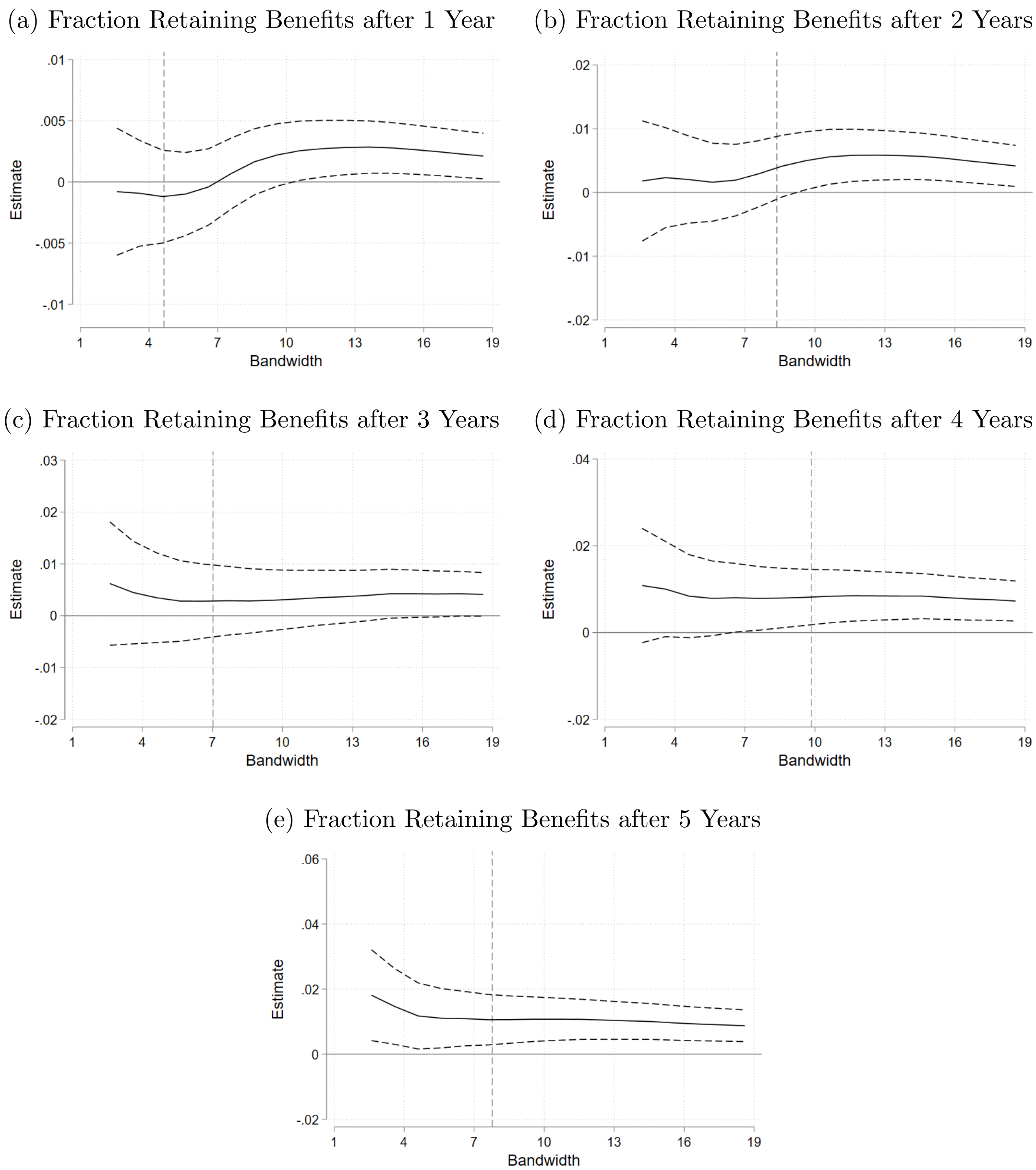


Figure 29: Bandwidth Sensitivity of Estimates for Return to the Labor Market

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.

(a) Fraction Deceased after 3 Years (b) Fraction Deceased after 6 Years

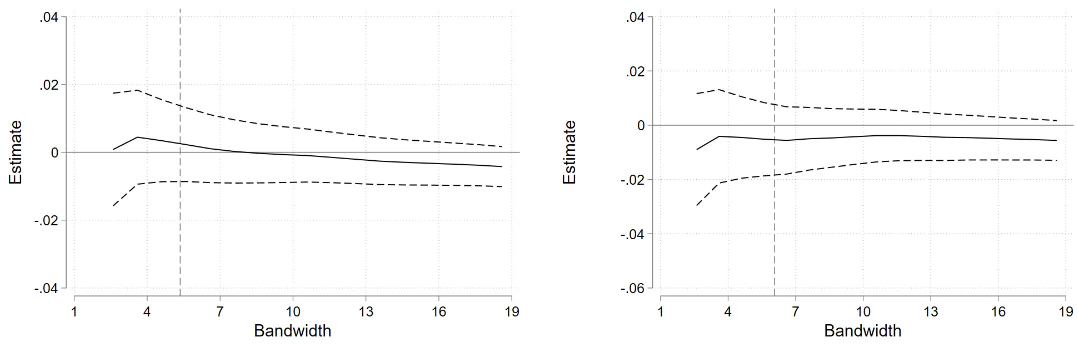


Figure 30: Bandwidth Sensitivity of Mortality Estimates

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.

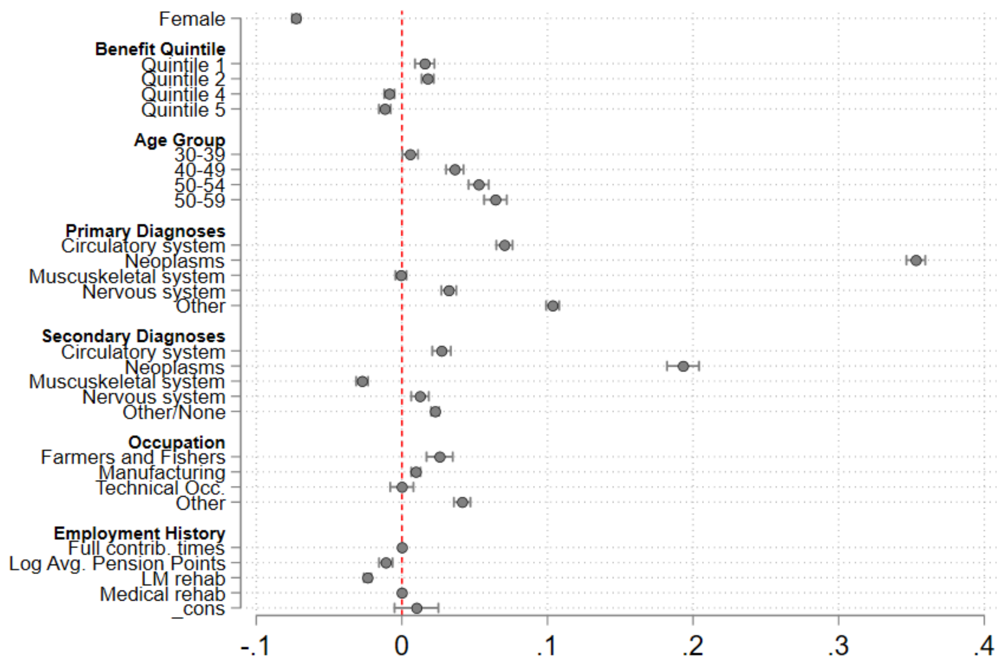


Figure 31: Predictors of Mortality

Note: The figure shows the coefficient estimates of a regression of individual characteristics on six-year mortality outcomes of recipients in our sample, including log benefits, demographic characteristics, diagnoses, and labor market history. The baseline categories are the third quintile for benefits, mental disorders for diagnoses, and service jobs for occupations.

Table 12: Effect of DI Benefit Increase on Permanent DI Recipients

Outcome:	All		Female		Male	
	(1)	(2)	(1)	(2)	(1)	(2)
Marginal Employment	-0.002	-0.004	-0.008	-0.009	0.005	0.004
	(0.0085)	(0.0085)	(0.0111)	(0.0109)	(0.0117)	(0.0116)
	[9.8]	[9.7]	[11.6]	[11.7]	[10.1]	[10.0]
	0.17	*0.17*	*0.17*	*0.17*	*0.17*	*0.17*
N	37,857	37,857	22,190	22,190	19,522	19,522
Regular Employment	-0.005	-0.004	0.002	0.002	-0.008	-0.007
	(0.0050)	(0.0048)	(0.0057)	(0.0056)	(0.0073)	(0.0070)
	[6.7]	[7.2]	[8.2]	[8.6]	[7.1]	[7.5]
	0.03	*0.03*	*0.02*	*0.02*	*0.03*	*0.03*
N	26,219	26,219	14,545	16,417	13,614	13,614
Mortality after 6 Years	-0.007	0.000	-0.005	0.001	-0.010	0.001
	(0.0093)	(0.0087)	(0.0126)	(0.0112)	(0.0132)	(0.0120)
	[11.1]	[8.9]	[11.9]	[9.2]	[10.9]	[10.0]
	0.34	*0.34*	*0.29*	*0.29*	*0.38*	*0.38*
N	50,243	40,840	26,056	19,291	26,472	23,984
Controls	No	Yes	No	Yes	No	Yes

Note: Table shows the regression discontinuity estimates for our main outcomes for permanent DI recipients, which we exclude in the main analysis. 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 8 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: $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 13: Relationship of Benefits and Six-Year Mortality

	Fraction Deceased after 6 Years			
	(1)	(2)	(3)	(4)
Log Benefits	-0.013^{***}	-0.016^{***}	-0.028^{***}	-0.008^{**}
	(0.001)	(0.001)	(0.001)	(0.004)
Dep. mean	0.13	0.13	0.13	0.13
Observations	256,953	256,953	256,953	256,953
Demographic controls	No	Yes	Yes	Yes
Diagnosis controls	No	No	Yes	Yes
Employment controls	No	No	No	Yes

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note: Table shows the results of a regression of (log) DI benefits on six-year mortality, controlling for various individual characteristics. The regression equation has the form $y_i = \alpha + \beta log(b_i) + X_i'\gamma + \varepsilon_i$ where b_i denotes benefits, X' the various controls, and y_i indicates whether a recipient has died six years after benefit award.

Table 14: Benefit Increase Ex-Ante vs. Ex-Post Reform

	All	Men	Women
Panel A: Ex-Post Benefit Increase			
RD Estimate	47.15^{***}	50.33^{***}	44.60^{***}
	(6.20)	(8.14)	(7.94)
Dep. mean	765.4	764.5	752.4
Observations	59,676	36,731	27,255
Panel B: Ex-Ante Benefit Increase			
RD Estimate	59.85^{***}	50.41^{***}	68.93^{***}
	(6.04)	(8.14)	(7.90)
Dep. mean	752.3	751.4	752.4
Observations	59,676	36,731	27,255

Note: Change in benefits in the recipient sample in Euros. Panel A shows the benefit increase from our main specification, which includes an ex-post correction of pension entitlements for the control group due to the mother pension reform. Panel B shows the raw estimate, which only includes a correction for the annual pension value (*Rentenwert*) but does not account for the mother pension, which is paid to the control group.