Andreas Myhre

Disability Benefits, Welfare Reform and Labor Supply

June 2021

Dissertation for the Ph.D degree
Department of Economics
University of Oslo

Series of dissertations submitted to the
Faculty of Social Sciences, University of Oslo
No. 858

ISSN 1564-3991

Cover: Hanne Baadsgaard Utigard.
Print production: Reprosentralen, University of Oslo.

Acknowledgements

This thesis is the final product of more than four years as a PhD researcher at the University of Oslo, and is part of the project “Information and financial incentives in disability insurance reform” funded by the Norwegian Welfare Administration. I am grateful to the Department of Economics for giving me the opportunity to complete a PhD. I also acknowledge the Norwegian Welfare Administration for financial support. Along with Statistics Norway, they have provided access to their databases which has been crucial to writing this thesis.

A special thanks goes to my co-author and co-supervisor Andreas Kostøl, without whom I probably would not be writing this thesis. In addition to giving me the opportunity to being part of the project “Information and financial incentives in disability insurance reform”, he has been a brilliant co-author, supervisor and not least a great motivator. I am also grateful to my supervisor Edwin Leuven who has always kept his door open for me and provided excellent guidance and honest feedback. Both my supervisors have contributed immensely to the final look of this thesis.

My colleagues and friends at the Department of Economics have meant a lot to me while writing this thesis, both academically and socially. A special thanks goes to my great friend and co-author Herman Kruse, who has made my stay at the University a lot more enjoyable. Your support has been outstanding. I would also like to thank friends and colleagues at Statistics Norway, where I had the pleasure to work with the data used in this thesis.

Finally, I want to give a special mention to my dad, who sadly passed away before this thesis was finished. Dad, you have always been a great inspiration for me, and I would probably not have written this thesis if I did not share the same interests as you. You will be greatly missed, but not forgotten. Thankfully, my friends and family have been supporting through this incident, as well as through the process of writing this thesis. A special thanks goes to you.

Oslo, June 2021
Andreas Myhre

Contents

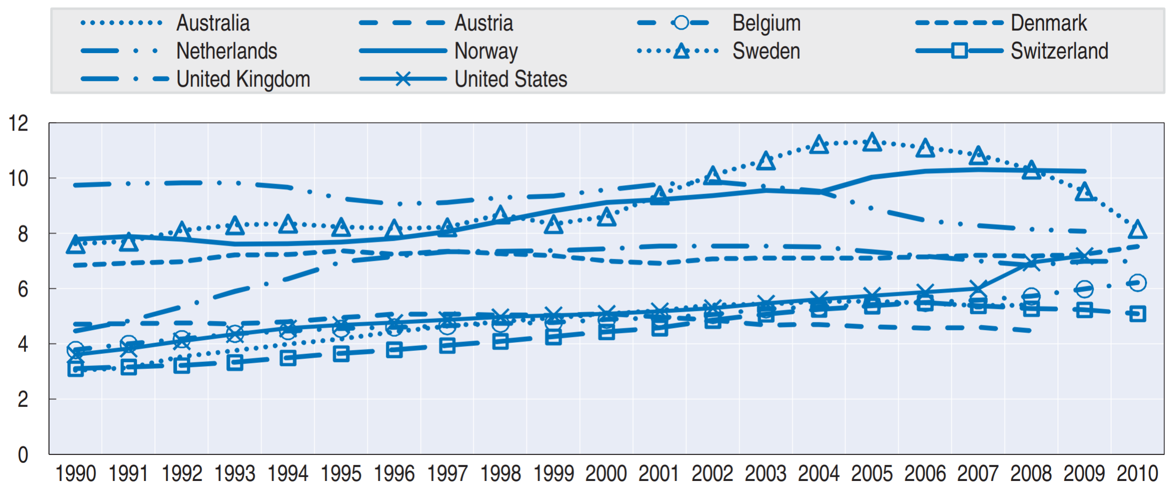
Introduction and Summary

Introduction

In most developed countries, participation rates in disability insurance (DI) programs have increased significantly during the last decades. Participation rates have increased from less than 4 percent to more than 7 percent of the working age population in the US, and a similar development has taken place in the UK (OECD, 2013). Figure 1 shows the development in participation rates between 1990 and 2010 in selected OECD countries. In Norway, the fraction of the working age population on disability rolls has grown from less than 2 percent to more than 10 percent during the last half century, and participation rates are among the highest in developed countries. Due to the long increasing rates of participation, DI programs have become one of the largest public transfer programs in developed countries. In the OECD, total spending on DI programs amounts to approximately 2.5% of GDP (OECD, 2010).

An important reason for the growth in DI rolls is the liberalization of the screening process (e.g. Autor & Duggan, 2003, 2006). This made DI more accessible to individuals with difficult-to-verify disorders, by placing less relative weight on diagnostic and medical factors and more relative weight on the ability to function in a work-like setting. This shift towards subjective factors in disability determination decisions has increased the permeability of the screening process, and fueled a debate over whether DI programs are being used as a gateway for early retirement, rather than only providing insurance against health shocks that prevent work.[1] It has also lead to a large increase in the number of younger recipients, who are more likely to be diagnosed with difficult-to-verify disorders such as mental disorders. Because of this, recipients on DI rolls today typically have lower mortality rates and are more likely to endure longer spells on the DI program. This has fueled a debate over the fiscal sustainability of DI programs.

Figure 1: Disability benefit recipients as a proportion of the working-age population between 1990-2010



Source: OECD (2013).

[1]See e.g. Autor & Duggan (2006); Wise (2012).

In light of the increased spending on DI benefits throughout the OECD, several countries are debating policy changes or large-scale reforms in order to reduce costs. While some countries have already tightened the eligibility criteria to reverse the growth in DI rolls (e.g. the US, the Netherlands, Sweden), other countries have already implemented or are debating policy alterations in order to improve incentives to work for beneficiaries of the programs (e.g. the UK, the US, Sweden, Switzerland). DI programs have long been criticized for their inherent work disincentives for the programs' beneficiaries, and in most countries, recipients have little incentives to engage in employment. Typically, beneficiaries lose parts or all of their benefits if earnings exceed a specific threshold, and in some countries, beneficiaries may even lose their DI receipt (e.g. the US). As a result, exit rates from DI to employment are typically very low and close to zero. Moreover, such "cash cliffs" have been argued to create a sharp divide between recipients who are able to work and those who are not.

In the case of Norway, the DI reform in 2015 allowed beneficiaries to keep a larger share of DI benefits if they attempted to return to work by removing the "cash cliff". As I will explain in the next section, the reform implemented a continuous reduction in DI benefits so that it would always pay to work. The reform was similar to proposed policy changes in DI programs in other countries. For example, the UK has introduced the "Ticket to work program", while in the US, the "$1 for $2 offset" has been proposed for many years, but never been implemented. Other OECD countries have also implemented or are considering implementing similar work incentives for DI recipients.

In light of the increased emphasis on DI policy, this dissertation primarily investigates economic implications of policies designed to improve incentives to work for DI recipients, and to reduce program costs. The first essay investigates how recipients respond to an earnings test following the Norwegian DI reform, and the associated policy implications of tightening or relaxing the earnings test. Understanding these responses is crucial for policymakers in terms of optimal policy design of DI systems. The second essay is closely related to the first essay, but focuses on how information about financial incentives affects labor supply responses to the Norwegian DI reform. This essay contributes to the understanding of how labor supply responses are attenuated due to lack of information, and has important implications for tax and transfer policy. The third essay investigates how provision of early retirement benefits affects enrollment onto other social security programs, and in particular the DI program. As DI programs have long been argued to being used as a gateway for early retirement, this essay sheds light on whether elderly workers exit the labor market through the DI channel.

The Norwegian DI reform offers an attractive setting to answer the research questions of the first and second essay. The first essay uses a sharp discontinuity in benefit schedules in order to quantify how recipients respond to financial incentives. By comparing recipients who were awarded DI just before the reform with recipients who were awarded DI just after the reform, the essay quantifies labor supply responses at the intensive margin as well as the extensive margin. The essay documents that recipients subject to the higher earnings exemption threshold earn significantly more compared to recipients subject to the lower threshold, and calculates earnings elasticities in order to assess how different benefit schedules affect recipients' welfare and overall program costs. Taken together, the essay provides sufficient statistics for optimal design of a DI system with continuous reduction of DI benefits, and thus guides policymakers in the design of such policies which many countries are considering implementing.

Answering the research question of the second essay is not straightforward. To understand how lack of information attenuates labor supply responses, one needs large-scale data on labor supply under different incentive schemes including measures of how incentives are perceived. The essay uses an information experiment administered by the Norwegian Labor and Welfare Administration that provided information about the incentives to work after the DI reform. The information letters were quasi-randomly assigned to recipients who were engaged in employment, and were targeted to those who were likely to earn above the annual earnings exemption threshold after the DI reform. The essay documents that the information letters had a large impact on recipients' responses to the financial incentives following the reform. Using the pre-reform incentives, the setting allows us to further investigate how lack of information attenuates responses versus other frictions that attenuate responses such as not being able to choose hours of work due to e.g. fixed work contracts. By quantifying the number of recipients that located in strictly dominated regions, where they could reduce working hours and gain more disposable income, we quantify the importance of information frictions versus other types of frictions that attenuate labor supply responses. Taken together, the findings of the essay have important implications for tax and transfer policy, and shows that welfare reforms can be more effective by an information intervention. The specific focus on the DI system speaks to a debate among academics and policymakers on the effectiveness of improving financial incentives to induce DI recipients to work.

Strict qualifying criteria in the Norwegian early retirement program provided an attractive setting to analyze spillovers onto DI programs for elderly workers in the third essay. Using data on bankruptcies in the private sector, the essay analyzes how displaced workers react to losing eligibility for retirement benefits. The early retirement qualifying criteria created a sharp discontinuity in eligibility depending on age and tenure in the firm, and allow us to compare workers who lost their job just too soon to be eligible for early retirement benefits versus workers who lost their job just being eligible. We assess re-employment rates and enrollment onto social security programs, and find no evidence of early retirement provision harming re-employment rates, but large spillovers onto the DI program in particular. This highlights the fact that DI programs may being used as an exit route to early retirement, and that policymakers must take such spillovers into account when evaluating overall costs of other social security programs.

Common for all three chapters is that they make use of two key ingredients in terms of answering the desired research questions: Rich administrative data and credible research strategies. Because the data is third-party reported (i.e. by the Norwegian Labor and Welfare Administration and employers), the administrative nature of the data reduces the extent of measurement errors, and is rated as exceptional by international quality assessments (e.g. Atkinson *et al.*, 1995). All three essays make use of quasi-experimental econometric methods that provide credible identification of the effects of interest. Taken together, this thesis sheds light on important questions in the current debate on DI programs, and provides credible information for policymakers.

The remainder of this introduction is organized as follows. First, I provide a brief overview of the Norwegian DI program and key aspects of the reform in 2015. Second, I summarize key contributions of the thesis. Third, I provide a brief summary of each essay that is part of the thesis. I begin by presenting the essay "Intensive and Extensive Margin Responses to Kinks in Disability Insurance Programs" (single authored). Next, the essay "Labor Supply Responses to Learning the Tax and Benefit Schedule" (joint with Andreas R. Kostøl) is presented. Finally, I present the essay "Early Retirement Provision for Elderly Displaced Workers (joint with Herman Kruse).

The Norwegian Disability Insurance Program

Because this thesis investigates responses in relation to the Norwegian DI system, and in particular following the reform of 2015, I provide a brief overview of the DI program in Norway and the reform. The Norwegian DI program is designed to provide partial earnings replacements to individuals below retirement age who are unable to work because of a medically determined physical or mental impairment. The program is part of the broader social security system and is financed by payroll taxes. To apply for DI benefits, individuals' ability to work must have been clarified by a primary medical doctor and appropriate vocational measures must have been completed. Individuals' ability to work must be permanently reduced by at least 50 percent, and illness or injury must be the main reason for the reduced work capacity. About 9.5 percent of the working age population in Norway received DI benefits as of 2016, with public spending of about $10 billion, or 3 percent of GDP (NAV, 2018).

The 2015 reform

In January 2015, the Norwegian DI reform was implemented. The reform was partly implemented as a reaction to the pension reform in 2011, as the benefit schedule before the pension reform provided incentives to apply for DI benefits rather than claiming early retirement benefits for individuals above the age of 62 years. The main purpose of the reform was to provide greater incentives to work for the programs' beneficiaries and simplify the benefit schedule. Table 1 highlights the most important changes to the work incentives of the reform.

Table 1: Main changes of the Norwegian DI Reform in 2015

	Pre-reform	Post-reform
Taxation of DI benefits	As pension benefits	As labor income
Earnings exemption threshold	$12,000*	$5,000**
Earnings > threshold	Reduction in disability rating	Continuous reduction in DI benefits, no reduction in disability rating
Earnings and DI benefits combined	May be less than uncapped DI benefits	Always larger than uncapped DI benefits***

* For totally disabled recipients. Partially disabled were subject to an individual earnings threshold based on past earnings pre- and post the reform. Monetary values are measured in 2015 dollars given an exchange rate of NOK/USD = 7.5.
** $8,000 until 2018 for recipients awarded DI before the reform.
*** Provided that earnings are lower than an individual threshold of approximately 80 percent of past annual earnings before disability (in present value).

An important change in the reform was changing taxation of DI benefits to the same tax schedule as labor income, where DI benefits were previously taxed as pension benefits. This was done to simplify the benefit schedule, and clarify the work incentives. To compensate for the increased taxes on DI benefits, gross benefits were increased accordingly. While the reform reduced the earnings exemption threshold, i.e. the maximum amount of earnings that recipients were allowed to earn without a reduction in DI benefits, the reform changed the manner of which DI benefits were reduced for earnings above the threshold. Before the reform, recipients' disability rating was reduced if earnings exceeded the exemption threshold, implying that DI benefits could be reduced permanently.[2] Moreover, this created

[2]Recipients had to apply to the Norwegian Labor Administration in order for their disability rating not to be permanently reduced if they attempted to engage in employment and earn above the earnings threshold.

a strictly dominated region where recipients could experience a drop in disposable income if they increased working hours. The reform removed this "cash cliff", and replaced the previous rules with a continuous reduction in DI benefits for earnings above the threshold, with a marginal tax rate on earnings of about 66 percent.[3] This change ensured that the sum of earnings and DI benefits would always be larger than DI benefits alone, and that it would always pay to work more. Recipients would also keep their initial disability rating, meaning that they could engage in employment without risk of their DI benefits being permanently reduced. After the reform, recipients could engage in full-time employment for up to 5 years and keep their DI receipt.[4]

Literature and Main Contributions

This section summarizes relevant literature and the key contributions of the thesis. The first essay complements a relatively small literature on policy implications of work incentives in DI programs. In particular, Ruh & Staubli (2019) exploit a notch in the Austrian DI program and find that abolishing the benefit offset would increase program costs. In contrast, Kostøl & Mogstad (2014) find that replacing a notch at the exemption threshold with a kink in the Norwegian DI program was likely to reduce program costs. The essay contributes to this small and inconclusive literature by assessing policy implications in a DI system where recipients are subject to kinked incentives, which is the type of policy that many countries have recently implemented or are considering implementing. The essay also complements a relatively large, yet inconclusive literature on labor supply responses to financial incentives for DI recipients. For instance, Weathers & Hemmeter (2011) and Campolieti & Riddell (2012) examine effects of increased work incentives for DI recipients in the US and Canada, respectively. Both studies document significant labor supply responses. In contrast, Schimmel *et al.* (2011) and Butler *et al.* (2015) find small and negligible effects to a policy change in the US and Switzerland. Finally, the essay also contributes to the literature on bunching at kinks in the tax and benefit schedule. The main empirical strategy implements the conceptual framework of Blomquist *et al.* (2019) and estimates bunching and an earnings elasticity using variation in kinked budget sets. In terms of understanding how standard empirical implementations of the bunching approach perform, the essay contributes by investigating how standard approaches in the literature compare to the non-parametric approach that is being used in the essay.

The main contribution of the second essay is to quantify the relative importance of information frictions versus other types of frictions that shape earnings responses. While the fact that optimization frictions are attenuating earnings responses is well understood (e.g., Chetty *et al.*, 2011 and Kleven & Waseem, 2013), less is known about the importance of information frictions. This essay bridges this particular gap in the literature. The essay is closely related to a small number of studies that experimentally control the type of information that tax filers receive (e.g., Liebman & Luttmer, 2012 and Chetty & Saez, 2013), and how mobility across areas with varying knowledge about the earned income tax credit affects tax refunds Chetty *et al.* (2013). The essay contributes to this line of research by showing that governments can shape earnings elasticities by information policy and by pinning down the relative importance of information for overall attenuation in earnings responses. Our specific focus on the DI system speaks to a debate among academics and policymakers on the effectiveness of improving finan-

[3]The implicit tax rate is equal to the benefit replacement rate.

[4]Full-time employment being defined as earning at least 80 percent of past annual earnings before disability (in present value).

cial work incentives to induce DI recipients to work part-time. While some studies find certain policy changes to be effective, e.g. Schimmel *et al.* (2011) and Butler *et al.* (2015) find negligible responses to financial incentives, suggesting that optimization frictions limit the scope for success. Our findings suggest that welfare reforms may be more successful and induce larger labor supply responses if an informational intervention accompanies the reform.

The third essay contributes to the literature by assessing economic implications of providing elderly displaced workers an option to retire early, and in particular benefit substitution behavior onto the DI program. The essay is closely linked to the literature investigating implications of providing extended unemployment benefits (e.g. Inderbitzin *et al.*, 2016; Kyyrä & Pesola, 2020), and contributes to this line of research by investigating implications of exiting the labor market entirely, and by investigating workers closer to the regular retirement age. In contrast to this literature, we do not find evidence of an employment effect. We do, however, find large spillover effects onto other social security programs consistent with the existing literature, and in particular large spillovers onto the DI program. The essay is also related to the literature examining changes to the minimum legal retirement age on labor supply and program substitution (e.g. Geyer & Welteke, 2017; Manoli & Weber, 2016; Staubli & Zweimüller, 2013 among others). Hernæs *et al.* (2016) and Johnsen *et al.* (2020) show that workers tend to decrease take-up of disability insurance benefits when workers have access to early retirement benefits in Norway, while Bratsberg *et al.* (2013) show that a large fraction of Norwegian DI claims can be attributed to job displacements. The essay complements this literature by investigating economic implications of providing early retirement benefits for displaced workers.

Summary of Essays

Essay 1: Intensive and Extensive Margin Responses to Kinks in Disability Insurance Programs

Single authored paper

The first essay investigates how disability insurance (DI) recipients respond to financial incentives to work and the associated policy implications. While many countries have already implemented or are considering implementing policies designed to improve incentives to work for DI recipients, there is limited empirical evidence on how different benefit offset policies affect beneficiaries' welfare and program costs. Knowledge about these effects is crucial for optimal design of DI policies. This paper contributes to a fairly small and inconclusive literature on these matters by using quasi-experimental variation in earnings exemption thresholds for DI recipients to estimate an earnings elasticity with respect to the implicit tax rate implied by the reduction in DI benefits for earnings above the threshold, and assesses the policy implications of changing the benefit offset.

To estimate the earnings elasticity, I use the fact that recipients awarded DI before a certain date were subject to a relaxed earnings exemption threshold of $8,000 compared to the baseline threshold of about $5,000. Using the theoretical framework of Blomquist *et al.* (2019), I implement a non-parametric bunching design and estimate an earnings elasticity by comparing the cumulative earnings distributions between recipients awarded DI before the cut-off date with recipients awarded DI after the cut-off. Next, I investigate how the relaxed earnings threshold affects labor force participation and whether such responses induce a bias in the intensive margin elasticity. Using bunching methods common in the literature (Saez, 2010; Chetty *et al.*, 2011), I compare my estimates to these methods in order to shed light on the performance of common bunching methods. Finally, I assess the policy implications of relaxing the benefit offset for DI recipients.

I find that the benefit offset induced substantial bunching around each kink, and significantly reduced recipients' earnings. The earnings responses correspond to an earnings elasticity of about 0.18. Furthermore, I find that labor force participation is higher among recipients subject to the relaxed kink. Using Monte Carlo simulations, I find that extensive margin responses induce a large bias in the estimated intensive margin elasticity, and implement an adjustment method that accounts for these responses. Moreover, I find that bunching methods that relies on fitting a flexible polynomial to the observed earnings distribution perform well in my context. Lastly, I find that relaxing the benefit offset likely will increase government expenditures.

The paper contributes to the literature in two aspects. First, my paper is primarily related to a small literature investigating policy implications of relaxing the benefit offset for DI recipients (Kostøl & Mogstad, 2014; Ruh & Staubli, 2019). In particular, I contribute to this literature by assessing a DI policy where DI recipients are subject to kinked incentives which is the type of policy that many countries have already implemented or are considering implementing. My findings are therefore particularly policy relevant. Second, my paper is closely related to the literature using bunching at kinks to identify a behavioral response (e.g. Saez, 2010; Chetty *et al.*, 2011). In contrast to this literature that estimates an implied elasticity by fitting a flexible polynomial to the counterfactual density, I contribute by using variation in budget sets to non-parametrically identify the implied elasticity.

Essay 2: Labor Supply Responses to Learning the Tax and Benefit Schedule

Written jointly with Andreas R. Kostøl, University of Arizona

In the second essay, we investigate how optimization frictions attenuate earnings responses, and in particular the role of information frictions. Understanding how individuals respond to information about financial incentives is important in order to understand the different factors that shape earnings responses. It also has important implications for the effectiveness of tax and transfer policy. While the fact that optimization frictions are attenuating earnings responses is well established, less is understood about the different factors shaping the responses. This paper aims to bridge this gap in the literature by separately quantifying the role of learning the tax and benefit schedule versus other kinds of frictions such as e.g. lumpy hours. A unique combination of kinks and notches in the tax and benefit schedule and an information policy in a Norwegian welfare reform facilitates our study.

Using bunching methods, we pin down the different factors that attenuate earnings responses. First, following Kleven & Waseem (2013), we use the presence of notches to estimate overall frictions and a structural earnings elasticity. Second, we use quasi-random assignment of an information letter targeting misperceptions about the slope and location of benefit phase-out regions to estimate how information shapes earnings responses. Following Chetty *et al.*, 2011, we estimate bunching and calculate the implied earnings elasticity for individuals who received the information letter and those who did not. By combining our estimates of the various elasticities, we pin down the relative importance of information frictions versus other types of frictions that attenuate earnings responses.

Our analysis delivers two main findings. First, we find that about half of the population do not behave as predicted by standard labor supply models and locate in strictly dominated regions. The observed earnings responses translate to an earnings elasticity of about 0.1. After we account for optimization frictions, we estimate a structural elasticity of about 0.3 suggesting that optimization frictions attenuate earnings responses by about two thirds in our context. Second, we estimate an earnings elasticity among individuals who received an information letter about the slope and location of benefit phase-out regions which is about twice as large as among the non-informed. In our context, this implies that information frictions account for at least 20-30 percent of overall frictions. Recognizing that the information letter does not fully correct for imperfect information about financial incentives, we interpret our estimates as a lower bound on the role of information in shaping earnings responses.

To further understand how information shapes earnings responses and the persistence of our findings, we employ a simple difference-in-difference design to assess various margins of earnings responses to the information letter. We find that the information letter induced earnings responses at the intensive margin as well as the extensive margin, and that individuals who received the letter worked fewer hours. Responses are also fairly persistent over time, with responses one year after receiving the letter amounting to about 75 percent of the initial response.

Our paper makes several important contributions to the existing literature. The main contribution is to quantify the relative importance of information about financial incentives versus other types of frictions. Our paper is closely related to a small number of studies that experimentally control the type of information that tax filers receive (Liebman, 2015; Chetty & Saez, 2013). We contribute to this research by showing how governments can shape earnings responses by information policy and increase effectiveness of welfare reforms. Our paper is also closely related to the literature on bunching at kinks and notches (Saez, 2010; Chetty *et al.*, 2011; Kleven & Waseem, 2013). In terms of understanding the factors that shape earnings responses, we contribute to this research by documenting a sharp increase in the number of nonoptimizers as the location of the kinks and notches change.

Essay 3: Early Retirement Provision for Elderly Displaced Workers

Written jointly with Herman Kruse, University of Oslo

In the final essay, we assess economic implications of access to an early retirement program for elderly displaced workers. Several studies show that a significant share of elderly workers drop out of the labor force entirely and increase take-up of social security benefits following a job displacement, and in particular disability benefits (e.g. Bratsberg *et al.*, 2013; Marmora & Ritter, 2015). While providing an option to retire early for these individuals might reduce re-employment rates, it might also reduce enrollment onto other social security programs. We investigate the economic implications of such an option by exploiting strict eligibility criteria in the Norwegian early retirement program, where some individuals lost eligibility for early retirement benefits because they lost their job just too soon to fulfill the eligibility criteria.

Using data on bankruptcies in private sector firms, we implement a regression discontinuity design comparing workers who lost their job *just* too soon to be eligible versus individuals who *just* fulfilled the eligibility criteria depending on age and tenure in the firm. As a firm bankruptcy is for the most part exogenous to the workers, individuals were unable to manipulate eligibility for early retirement in our setting. In contrast to theoretical predictions, we find no evidence of an employment effect. Re-employment rates and labor market earnings were practically indistinguishable between workers who were eligible for early retirement benefits and those who were not. We do, however, find evidence of substantial program substitution. Those who were ineligible for the early retirement program replaced 69 percent of their lost early retirement benefits by take-up of other social security benefits, where 51 percent comes from disability benefits, 13 percent comes from unemployment benefits and 5 percent from other social security benefits.

To further investigate the implications of our findings, we explore how eligibility for early retirement affected individuals' disposable income and net public expenditures. We find that government expenditures were slightly higher for eligible individuals, but only about $7,200 per year. On average, disposable income of eligible individuals were $3,800 higher compared to ineligible individuals. However, we show that the distribution of disposable income is slightly more dispersed for ineligible individuals, suggesting that some of these individuals did not manage to replace the lost income.

Our main contribution to the literature is to assess economic implications of providing elderly displaced workers an option to retire early. As pension systems are typically not designed to fully offset shocks affecting individual work careers, we believe that our analysis is of general interest for policy. Our paper is closely related to the literature investigating implications of providing extended unemployment benefits (e.g. Inderbitzin *et al.*, 2016; Kyyrä & Pesola, 2020). We contribute to this literature by investigating implications of exiting the labor market entirely, and by investigating workers closer to the regular retirement age.

References

ATKINSON, A., RAINWATER, L., & SMEEDING, T.M. 1995. Income Distributions in OECD countries: Evidence from the Luxembourg Income Study. *OECD Publications and Information Center*.

AUTOR, DAVID H., & DUGGAN, MARK G. 2003. The Rise in Disability Rolls and the Decline in Unemployment. *The Quarterly Journal of Economics*, **118**(1), 157–206.

AUTOR, DAVID H., & DUGGAN, MARK G. 2006. The Growth in the Social Security Disability Rolls: A Fiscal Crisis Unfolding. *Journal of Economic Perspectives*, **20**(3), 71–96.

BLOMQUIST, SÖREN, NEWEY, WHITNEY, KUMAR, ANIL, & LIANG, CHE-YUAN. 2019. On Bunching and Identification of the Taxable Income Elasticity. *NBER Working Paper 24136*.

BRATSBERG, BERNT, FEVANG, ELISABETH, & RØED, KNUT. 2013. Job loss and disability insurance. *Labour Economics*, **24**, 137–150.

BUTLER, MONIKA, LECHNER, MICHAEL, DEUCHERT, EVA, STAUBLI, STEFAN, & THIEMANN, PETRA. 2015. Financial Work Incentives for Disability Benefit Recipients: Lessons from a Randomized Experiment. *IZA Journal of Labor Policy*, **4**(18), 1–18.

CAMPOLIETI, MICHELE, & RIDDELL, CHRIS. 2012. Disability Policy and the labor market: Evidence from a natural experiment in Canada, 1998-2006. *Journal of Public Economics*, **96**, 306–316.

CHETTY, RAJ, & SAEZ, EMMANUEL. 2013. Teaching the tax code: Earnings responses to an experiment with EITC recipients. *American Economic Journal: Applied Economics*, **5**(1), 1–31.

CHETTY, RAJ, FRIEDMAN, JOHN, OLSEN, TORE, & PISTAFERRI, LUIGI. 2011. Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records. *The Quarterly Journal of Economics*, **126**(2), 749–804.

CHETTY, RAJ, FRIEDMAN, JOHN N, & SAEZ, EMMANUEL. 2013. Using Differences in Knowledge across Neighborhoods to Uncover the Impacts of the EITC on Earnings. *The American Economic Review*, **103**(7), 2683–2721.

GEYER, JOHANNES, & WELTEKE, CLARA. 2017. Closing Routes to Retirement: How Do People Respond? *DIW Berlin, German Institute for Economic Research*.

HERNÆS, ERIK, MARKUSSEN, SIMEN, PIGGOT, JOHN, & RØED, KNUT. 2016. Pension Reform and Labor Supply. *Journal of Public Economics*, **142**(October), 39–55.

INDERBITZIN, LUKAS, STAUBLI, STEFAN, & ZWEIMÜLLER, JOSEF. 2016. Extended Unemployment Benefits and Early Retirement: Program Complementarity and Program Substitution. *American Economic Journal: Economic Policy*, **8**(1), 253–288.

JOHNSEN, JULIAN VEDELER, VAAGE, KJELL, & WILLÉN, ALEXANDER. 2020. Interactions in Public Policies: Spousal Responses and Program Spillover of Welfare Reforms. *Scandinavian Working Papers in Economics, NHH*, **20**(September 23).

KLEVEN, HENRIK J., & WASEEM, MAZHAR. 2013. Using Notches to Uncover Optimization Frictions and Structural Elasticities: Theory and Evidence from Pakistan. *Quarterly Journal of Economics*, **128**, 669–723.

KOSTØL, ANDREAS RAVNDAL, & MOGSTAD, MAGNE. 2014. How Financial Incentives Induce Disability Insurance Recipients to Return to Work. *American Economic Review*, **104**(2), 624–55.

KYYRÄ, TOMI, & PESOLA, HANNA. 2020. Long-term effects of extended unemployment benefits for older workers. *Labour Economics*, **62**(101777).

LIEBMAN, JEFFREY B. 2015. Understanding the Increase in Disability Insurance Benefit Receipt in the United States. *Journal of Economic Perspectives*, **29**(2).

LIEBMAN, JEFFREY B., & LUTTMER, ERZO F.P. 2012. Would People Behave Differently If They Better Understood Social Security? Evidence From a Field Experiment. *American Economic Journal: Economic Policy*, **7(1)**(17287), 275–99.

MANOLI, DAYANAND S., & WEBER, ANDREA. 2016. The Effects of the Early Retirement Age on Retirement Decisions. *NBER Working Papers 22561*.

MARMORA, PAUL, & RITTER, MORITZ. 2015. Unemployment and the Retirement Decisions of Older Workers. *Journal of Labor Research*, **36**(3), 274–290.

NAV. 2018. Nav-ytelsene frem mot 2060 - En forenklet analyse av store penger. *NAV-rapport nr. 1-2018*.

OECD. 2010. *Sickness, Disability and Work: Breaking the Barriers. A synthesis of findings across OECD countries*. OECD Publishing, Paris.

OECD. 2013. *Mental Health and Work: Norway*. OECD Publishing, Paris.

RUH, PHILIPPE, & STAUBLI, STEFAN. 2019. Financial Incentives and Earnings of Disability Insurance Recipients: Evidence from a Notch Design. *American Economic Journal: Economic Policy*, **11**(2), 269–300.

SAEZ, EMMANUEL. 2010. Do Taxpayers Bunch at Kink Points? *American Economic Journal: Economic Policy*, **2**(3), 180–212.

SCHIMMEL, JODY, STAPELTON, DAVID C., & SONG, JAE G. 2011. How Common is 'Parking' among Social Security Disability Insurance Beneficiaries? Evidence from the 1999 Change in the Earnings Level of Substantial Gainful Activity. *Social Security Bulletin*, **71**(4), 77–92.

STAUBLI, STEFAN, & ZWEIMÜLLER, JOSEF. 2013. Does raising the early retirement age increase employment of older workers? *Journal of Public Economics*, **108**(C), 17–32.

WEATHERS, ROBERT R., & HEMMETER, JEFFREY. 2011. The Impact of Changing Financial Work Incentives on the Earnings of Social Security Disability Insurance (SSDI) Beneficiaries. *Journal of Policy Analysis and Management*, **30**(4), 708–728.

WISE, DAVID A. 2012. *Social Security Programs and Retirement around the World: Historical Trends in Mortality and Health, Employment, and Disability Insurance Participation and Reforms*. University of Chicago Press.

Chapter I

Intensive and Extensive Margin Responses to Kinks in Disability Insurance Programs

Single-authored essay

Intensive and Extensive Margin Labor Supply Responses to Kinks in Disability Insurance Programs*

Andreas S. Myhre†

This version: June 9, 2021

Abstract: While kinks are prevalent in tax and transfer systems, the fiscal revenue and behavioral responses are not fully understood. In disability insurance (DI) programs, for instance, kinks help balance the moral hazard effects from the induced entry with the provision of work incentives for recipients who regain their ability to work. Using quasi-random variation in kink points in the benefit schedule for Norwegian DI recipients, I identify intensive and extensive margin earnings responses to the implicit tax on earnings as DI benefits are phased out above the kink. To identify the intensive margin responses, I implement a non-parametric bunching design that does not require functional form assumptions or deciding an excluded region around the kink. Responses correspond to an earnings elasticity with respect to the implicit net-of-tax rate of about 0.18. Using a regression discontinuity design, I further show that the kink in the benefit schedule induces significant responses at the extensive margin. I use the estimated earnings responses to evaluate how the benefit offset affects program costs, and find that relaxing the benefit offset reduces public expenditures only if program entry is very inelastic. My findings speak to recent policy-proposals aiming to improve work incentives of DI recipients.

Keywords: labor supply, disability insurance, policy evaluation, bunching
JEL codes: H53, H55, I38, J21

*This project received financial support from the Norwegian Welfare Administration. I would also like to thank Kristoffer Berg, Andreas Kostøl, Herman Kruse and Edwin Leuven for valuable input.

†University of Oslo, Department of Economics; Statistics Norway; The Norwegian Labor Force Administration. E-mail: a.s.myhre@econ.uio.no

1 Introduction

Disability Insurance (DI) programs are among the largest public transfer programs in developed countries. In the OECD, total spending on DI programs amounts to approximately 2.5% of GDP (OECD, 2010). In an attempt to reduce fiscal costs, many countries have already implemented or are considering implementing policies designed to improve incentives to work for the programs' beneficiaries.[1] While such policies allow beneficiaries to keep a larger share of their benefits if they engage in employment, they typically involve a high implicit tax on earnings above an exemption threshold, creating a large kink in the budget constraint. If labor supply responses to the kink are large, policymakers could relax the exemption threshold or reduce the implicit tax rate to increase work effort among DI recipients. This would improve welfare among current recipients and increase revenue from income taxes. On the other hand, a more lenient policy might also increase expenditures on DI benefits and induce more potential applicants to apply for DI. Knowledge about recipients' labor supply responses in such policies is therefore crucial for optimal policy design.

The main contribution of this paper is to assess this policy trade-off by investigating intensive and extensive margin labor supply responses of DI recipients to kinked budget sets. Two key features in the Norwegian DI program allow me to do this. First, the kink in the benefit schedule is salient and large in magnitude. DI beneficiaries in Norway can earn up to approximately $4,937 per annum without losing benefits. If earnings exceed this threshold, DI benefits are reduced by approximately $2 for every $3 in earnings above the threshold. Second, a sharp discontinuity in benefit schedules provides a particularly attractive setting to analyze behavioral responses. As a transitional policy from prior work incentives, recipients awarded DI before 1st of January 2015 were subject to a relaxed exemption threshold of $8,000 until 2018.[2] This sharp discontinuity in benefit schedules therefore allows me to analyze intensive and extensive margin responses separately. I identify the intensive margin elasticity using a non-parametric bunching design, where the sharp discontinuity allows me to observe bunching at the kink for one group of individuals and at the same time observe the earnings distribution of a comparison group where the implicit tax on earnings does not change. To identify extensive margin responses, I implement a regression discontinuity design, and estimate an extensive margin elasticity with respect to participation tax rates. In combination, the intensive and extensive margin elasticities allow me to analyze how different benefit schedules affect overall public expenditures.

My main empirical strategy implements the theoretical framework of Blomquist *et al.* (2019) who show that the earnings elasticity can be identified non-parametrically by inverting the cumulative earnings distributions of two groups subject to different kinked budget sets. In my setting, identification relies on recipients with DI award on either side of the cut-off date (i.e. 1st of January 2015) being drawn from the same distribution of potential earnings, i.e. the earnings distributions of the two groups would have been comparable if they were subject to the same benefit schedule. Each point in the cumulative distributions of the two groups will then correspond to individuals with the same earnings potential. This allows me to estimate the intensive margin earnings elasticity by comparing earnings of recipients at the same point in the cumulative distributions where the marginal tax rate is different between the two

[1] One example is the "$1 for $2 offset" in the US that has been proposed for many years but never been implemented. Under this policy, DI benefits would be reduced by $1 for every $2 in earnings above about $14,000 per annum. Other examples include i.e. the "Ticket to Work" program in the UK. Switzerland tested a conditional cash program that offered DI recipients cash payments if they expanded or stared working. Sweden introduced the so-called continuous deduction program back in 2009.

[2] Before 2015, the annual exemption threshold was $12,000.

groups. An advantage of this method is that it does not require functional form assumptions or deciding an excluded region around the kink as in standard bunching applications. To shed light on these issues in the standard applications, I estimate bunching responses using common strategies in the literature and compare these with the non-parametric approach.[3]

A kink may also create responses along the extensive margin as it increases the average tax of participating in the labor force.[4] Therefore, if individuals have a fixed cost of labor force participation, it is possible that the high kink at $8,000 induces some recipients to start working. To identify this effect, I implement a regression discontinuity design which compares recipients with DI award on each side of the cut-off date, and pin down an extensive margin elasticity with respect to participation tax rates. Furthermore, it is possible that the extensive margin responses could affect the observed earnings distributions, and therefore induce a bias in the estimated intensive margin elasticity. To shed light on this matter, I perform Monte Carlo simulations generating data from a utility function with a fixed cost of labor force participation. This allows me to investigate how extensive margin responses affect the estimated elasticity in the bunching setting.

My main empirical findings can be summarized by the following conclusions. First, I find large and sharp bunching in recipients' earnings around each kink. If DI benefits were not reduced above the threshold, recipients who bunch at the baseline kink at $4,937 would have earned $944 or about 19 percent more. This response corresponds to an earnings elasticity with respect to the implicit net-of-tax rate of about 0.18. This elasticity is higher than estimates found in studies that examine bunching at kinks in the income tax schedule, but is in line with estimates found in studies that examine bunching in social security programs.[5] Second, I find that the benefit offset creates sizable responses at the extensive margin. Labor force participation is about 0.8 percentage points higher among recipients with a kink at $8,000 which is sizable considering that only about 11 percent of DI recipients participate in the labor force. The response corresponds to an elasticity of labor force nonparticipation with respect to participation taxes of about 0.11, and is in line with estimates found in other studies.[6]. Third, I find that extensive margin responses induce a large bias in the bunching elasticity in my setting. The Monte Carlo simulations reveal that the bias amounts to about 70 percent of the true elasticity. However, I show that the bias is negligible if one accounts for extensive margin responses in earnings distributions. Fourth, my findings indicate that relaxing the benefit offset increases disposable income and reduces costs for current recipients of the program. However, overall costs for the government are likely to increase if a more lenient benefit offset policy attracts more individuals to the DI program.

A caveat with this study is that it is not informative about the level of increased program inflow when recipients are allowed to keep a larger share of their benefits as they earn more. Because recipients were unable to manipulate the DI award date, and the more lenient benefit schedule was a transitional policy, I am unable to identify this effect. I do, however, calculate the size of induced entry that has to be generated by more generous benefit schedules in order to increase program costs. Based on findings in

[3]In particular, I estimate the bunching elasticity using the polynomial approach of Chetty *et al.* (2011) and the linear approximation approach suggested by Saez (2010).

[4]See e.g. Gelber *et al.* (2017b, 2020a).

[5]Saez (2010); Chetty *et al.* (2011); Bastani & Selin (2014); Paetzold (2019) find elasticities in the range of 0-0.05 among wage earners. Zaresani (2020) and Ruh & Staubli (2019) estimate structural earnings elasticities of 0.20 and 0.27 using a kink in the Canadian DI program and a notch in Austrian DI program, respectively. Gelber *et al.*, 2020b find an observed elasticity of 0.19 in the US social security program.

[6]Kostøl & Mogstad (2014) estimate an elasticity of labor force nonparticipation with respect to participation taxes of about 0.12 in the Norwegian DI program. Ruh & Staubli (2019) find an elasticity of about 0.10 in the Austrian DI program.

previous studies, I find that relaxing the benefit offset is likely to increase overall public expenditures.[7]

My paper primarily contributes to a small and inconclusive literature that assesses fiscal costs of the benefit schedule in DI programs. Kostøl & Mogstad (2014) find that replacing a notch at the exemption threshold with a kink in the Norwegian DI program was likely to reduce program costs. In contrast, Ruh & Staubli (2019) exploit a notch in the Austrian DI program, and conclude that abolishing the notch would increase program costs.[8] They find that most of the increased government revenue comes from extensive margin responses if the benefit schedule is relaxed. I contribute to this literature by assessing policy implications in a DI policy where recipients are subject to kinked incentives.

My paper is also closely related to the broader literature that examines labor supply effects of financial incentives to work for DI recipients. Zaresani (2020) estimates a structural earnings elasticity of 0.20 using kinks in the Canadian DI program. Weathers & Hemmeter (2011) examine effects of a pilot project that replaced a notch at the exemption threshold with a gradual reduction in DI benefits in the US. They find that the policy change significantly increased the number of recipients with earnings above the exemption threshold. Campolieti & Riddell (2012) find that the introduction of an exemption threshold significantly increased labor force participation of Canadian DI beneficiaries. They find no changes in program inflow or outflow. In contrast to the above studies, Schimmel *et al.* (2011) find that increasing the exemption threshold from $500 to $700 in the US only increased beneficiaries' earnings by a small amount. Butler *et al.* (2015) investigate employment effects of a conditional cash program that offered cash claims for DI recipients who expanded work in a randomized experiment. They only find small and negligible effects on employment. I contribute to this literature by identifying intensive margin and extensive margin responses separately. My paper is also related to the literature on the potential work capacity of DI recipients.[9]

Finally, my paper also relates to a large literature that studies behavioral responses to kinked incentives. In particular, Saez (2010) shows that bunching at kinks can identify a behavioral elasticity. Chetty *et al.* (2011) extend this framework to allow for adjustment costs. More recently, Blomquist *et al.* (2019) show how an elasticity can be identified from variation in budget sets. I contribute to this literature by implementing the conceptual framework of Blomquist *et al.* (2019) and estimate the bunching elasticity non-parametrically. In terms of understanding how standard empirical implementations of the bunching approach perform, I contribute by investigating how these approaches compare to the non-parametric approach. My findings indicate that common estimation approaches in the literature perform well in my context.[10] My paper is also related to several studies using bunching at kinks to identify earnings responses in social security programs (e.g. Le Barbanchon, 2016; Gelber *et al.*, 2020b; Zaresani, 2020). The bunching approach has also been used to identify responses in many other contexts.[11] Kleven (2016) provides a review of this literature.

[7] See e.g. Hoynes & Moffitt (1999); Gruber (2000); Campolieti & Riddell (2012); Mullen & Staubli (2016); Castello (2017).

[8] Ruh & Staubli (2019) estimate a structural elasticity driving the responses of about 0.27.

[9] A number of studies document that DI receipt significantly reduces earnings and labor force participation by using rejected applicants as a control group for DI recipients (e.g. Bound, 1989; Chen & van der Klaauw, 2008; Singleton, 2012; Maestas *et al.*, 2013; French & Song, 2014). There is also some evidence on the work capacity for DI recipients who have endured longer spells on DI (Borghans *et al.*, 2014; Moore, 2015).

[10] Specifically, fitting a flexible polynomial to the empirical distribution following Chetty *et al.* (2011) yields an elasticity of about 0.15-0.19 depending on the polynomial order, while the non-parametric approach yields an elasticity of about 0.18. Using the approach of Saez (2010), I estimate an elasticity of about 0.19.

[11] Some examples include Einav *et al.* (2017) who use a dynamic model to investigate responses to health insurance contracts, retirement decisions (Manoli & Weber, 2016), effects of minimum wages (Harasztosi & Lindner, 2019), transaction taxes in housing markets (Kopczuk & Munroe, 2015; Best & Kleven, 2017) and responses to speed controls (Traxler *et al.*, 2018).

The remainder of this paper is organized as follows. Section 2 describes the Norwegian DI program and the incentives to work. Section 3 describes the data and sample used in the empirical analysis. Section 4 outlines the methodology and the validity of the empirical design. Section 5 presents the main results of the estimated earnings elasticity. Section 6 analyzes the extensive margin responses. Section 7 performs an analysis of the elasticity using common bunching methods in the literature. Section 8 calculates fiscal effects of alternative policies and discusses their implications. Section 9 concludes.

2 Institutional Background

2.1 The Norwegian DI Program

The Norwegian DI program is designed to provide partial earnings replacement to working-age individuals whose work capacity is permanently reduced due to a medically verifiable physical or mental impairment. The program is part of the broader social security system and is financed by payroll taxes. Of the OECD countries, Norway has one of the highest proportions of the working age population on DI rolls. From 1961 to 2004, the percentage of the working age population on DI rolls increased consistently from 2.2 to 10.4 percent. Since 2004, the proportion decreased slightly and is around 9.5 percent as of 2016, with public spending around $10 billion, or 3 percent of GDP (NAV, 2018).

Pathways into DI and Determination Process In order to apply for DI benefits, individuals' ability to work must have been clarified by a primary medical doctor and appropriate vocational measures must have been completed. Only individuals in the working age population 18-67 years are eligible to apply. Individuals' ability to work must be permanently reduced by at least 50 percent, and illness or injury must be the main reason for the reduced work capacity.[12] In Norway, most applicants for permanent DI benefits are beneficiaries of a temporary DI program whose purpose is to evaluate and improve beneficiaries' ability to work. More than 80 percent of allowed cases for permanent DI benefits are individuals who have endured spells on this program. The temporary DI program in its current form was implemented in March 2010 when three different types of temporary DI or rehabilitation programs were replaced by the current program. As a general rule, temporary DI benefits are provided for up to 4 years for eligible individuals during the time period I consider in this paper.[13] Other pathways into DI include individuals on sickness benefits while some clear cut cases lead directly to award.

If the criteria for permanent disability are met, individuals must submit an application to the Norwegian Labor and Welfare Administration (NAV) whose disability examiners and medical doctors assess the medical evidence and verify the validity of the claim.[14] Processing an application usually takes between 4-9 months depending on the complexity of the application, workload and geographical location of the local DI office. If the disability examiner concludes that the applicant cannot engage in more than 50 percent of full-time employment because of the health impairment, and appropriate vocational measures have been completed, a disability award is made. Approximately 85 percent of applications are accepted. Of allowed cases, about 80 percent are allowed a full disability claim.[15]

[12] For individuals on the temporary DI program at the time of application, a 40 percent permanent reduction in earnings capacity is sufficient. For individuals whose disability is due to an approved occupational illness or injury, a 30 percent permanent reduction in earnings capacity is sufficient.

[13] From 2018, benefits are provided for up to 3 years as a general rule.

[14] Dahl *et al.* (2014) explain the disability determination process in detail.

[15] The remaining 20 percent of allowed DI cases are allowed a partial DI claim where disability rating depends on the perceived ability to work. I abstract from partially disabled recipients in this paper.

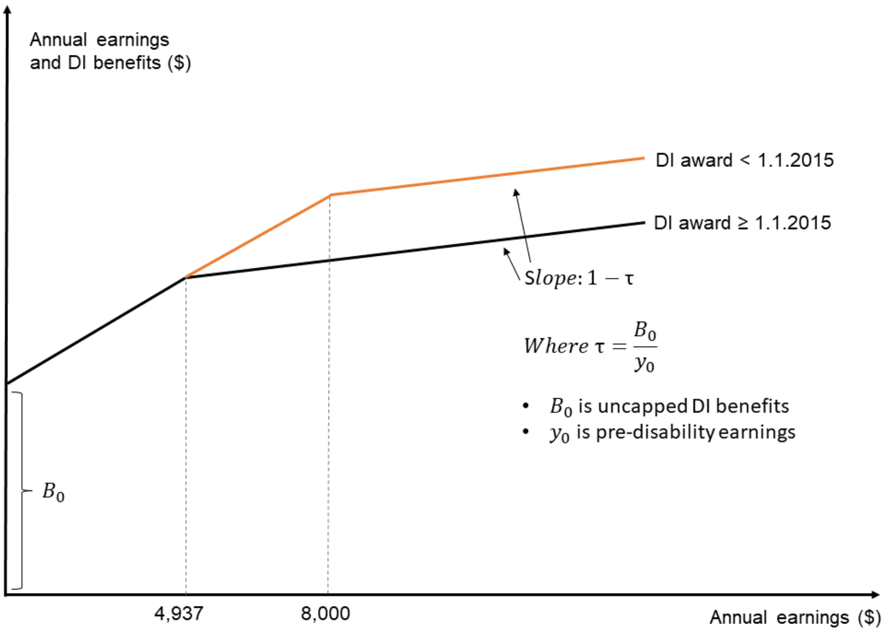
2.2 Benefit Phase-out and Levels

DI recipients deemed as totally disabled in Norway can earn up to an exemption threshold K each year without any reductions in DI benefits B.[16] If annual earnings z exceed this threshold, DI benefits are gradually reduced by a share τ for every dollar in earnings above the threshold. The relationship can be summarized as follows, where B_0 is the uncapped DI benefits before any reductions:

$$B = \begin{cases} B_0 & if \quad z \leq K \\ B_0 - \tau(z-K) & if \quad z > K \end{cases} \tag{1}$$

where the implicit tax-rate τ is equal to the replacement rate of DI benefits which is approximately 66 percent for most recipients. Specifically, $\tau = B_0/y_0$ where y_0 is pre-disability earnings adjusted for wage growth.[17] Both earnings and DI benefits are subject to regular income taxes. The general annual exemption threshold is $4,937 in 2016 dollars and is indexed annually according to the average wage growth. This is equivalent to about 4 hours of work per week for the representative DI recipient. Until 2018, a transitional policy applied to recipients awarded DI before 1st of January 2015 who were subject

Figure 1: Budget Sets



Notes: The figure shows the budget set for recipients awarded DI before and after 1.1.2015, respectively, during years 2015-2018. For illustrative purposes and with minimal loss of generality, I assume that recipients awarded DI before and after 1.1.2015 receive the same amount of (uncapped) DI benefits, and disregard dependent benefits and income taxation.

[16] Partially disabled recipients are subject to the same benefit phase-out rules as totally disabled, but with an individual exemption threshold K_i which depends on disability rating and past earnings.

[17] Pre-disability earnings are defined as each individual's earnings potential if engaged in full-time employment prior to disability onset. For recipients with little or no earnings history, pre-disability earnings are set to minimum levels. If annual earnings exceed 80 percent of pre-disability earnings, recipients are considered engaging in full-time employment and no benefits are provided for that year. DI recipients can exceed this threshold for up to five consecutive years and keep their DI receipt.

to a relaxed threshold of $8,000, or about 6 hours of work per week.[18] Figure 1 represents the budget constraints for the two groups.

Benefit Levels DI benefits replace about 66 percent of past earnings up to a maximum amount of about $49,000 per annum. If recipients have little or no earnings history, they receive a minimum amount of DI benefits of about $31,000.[19] In 2015, the definition of past earnings which calculations of DI benefits is based upon was changed. For recipients awarded DI before 1st of January 2015, benefits were based on projected retirement savings as if the individual had continued to work until the general retirement age of 67 years. The years with the lowest income, or projected income in the future were excluded so that a maximum of 20 years of earnings history were used in the calculations of DI benefits. For recipients awarded DI on 1st of January 2015 or later, DI benefits were calculated using the highest average income in three of the last five years prior to disability onset. Because DI benefits were based on more recent earnings history, this group of recipients receive slightly higher (uncapped) DI benefits on average due to real wage growth, approximately 4% more on average. Minimum benefits were the same independently of award date.

3 Data and Sample Selection

This section describes the administrative data sources, key outcome variables and the main sample for the empirical analysis.

3.1 Data Sources

In the empirical analysis, I use data from three main sources that can be linked by unique and anonymized identifiers for every resident individual. The data on DI recipients is provided by the Norwegian Labor and Welfare Administration (NAV) and contains monthly records of all DI recipients who entered the permanent DI program until 31st of December 2015. It contains information about the level of DI benefits received, disability rating, month of DI award, month of disability onset, pre-disability earnings and a rich set of demographic and socioeconomic information including gender, age and cohabitant status. The earnings data is also provided by NAV and contains monthly records of wage earnings for each employer-employee relationship during years 2015-2017. Finally, I use administrative data provided by Statistics Norway which contains individual demographic and socio-economic information such as education, number of children and date of death.

The administrative nature of the data reduces the extent of measurement errors in disability variables and employment relationships. Because individual disability status and earnings are third-party reported (i.e. by NAV and the employers), the coverage and reliability are rated as exceptional by international quality assessments (see e.g. Atkinson *et al.*, 1995). Since administrative data are a matter of public record, there is no attrition due to non-response or non-consent by individuals or firms, and individuals can only exit these data sets due to natural attrition (i.e. death or out-migration).

[18] Before 2015, the exemption threshold was approximately $12,000 and recipients were subject to a different set of work incentives. The exemption threshold at $8,000 was therefore a transitional policy from the previous work incentives until 2018. From 2019 and onward, all recipients are subject to a common threshold of $4,937.

[19] For cohabitant recipients, minimum benefits amounts to about $28,000. In addition, recipients classed as "young disabled" get an additional amount of about $5,000 if they were 26 years or younger at disability onset.

3.2 Variables

Because phase-out of DI benefits is determined at the annual level, the main outcome variable I consider is annual wage earnings.[20] As the earnings data comes from monthly records, I construct this variable by summarizing earnings from all employer-employee relationships (if more than one) for each month during the calendar year. The second key outcome variable I consider is whether recipients have any annual earnings. In Norway, employees are subject to holiday pay which is based on last year's earnings.[21] In order to distinguish between recipients who have engaged in employment and those whose earnings are based on last year's earnings, I therefore define this variable as positive earnings excluding holiday pay. The time period I consider is 2016-2017 as some recipients in the estimation sample were awarded DI in 2015, and 2017 is the last year of the data.

3.3 Estimation Sample

The main sample used in the empirical analysis consists of recipients awarded DI between 1st of April 2014 and 30th of September 2015, i.e. +/- 9 months around the cut-off date for being subject to the relaxed benefit phase-out. I restrict the sample to recipients who are deemed totally disabled by NAV due to a lack of information on the exact location of the exemption threshold among partially disabled recipients. Furthermore, I exclude recipients who turn 67 years during the calendar year due to eligibility for old-age pension beginning at age 67.

Table 1 reports summary statistics for all totally disabled DI recipients awarded DI before 2015 as well as the estimation sample of recipients with DI award on each side of the cut-off date, respectively. Compared to the average DI recipient, recipients in the estimation sample have lower earnings and fewer recipients have any labor market earnings. By construction, they have also spent fewer years on DI and are somewhat younger. Otherwise, individual characteristics are fairly similar. As for the two groups in the estimation sample, recipients awarded DI in 2015 receive about 5 percent more (uncapped) DI benefits compared to recipients awarded DI in 2014. While the two groups share fairly similar characteristics, there is in particular one notable difference. Recipients awarded DI in 2014 are far more likely to have been on a prior temporary DI program before the current temporary DI program was implemented in March 2010. As the general maximum spell on the current temporary DI program is 4 years, many recipients who were transferred from the prior programs were awarded DI in 2014. As a result, these individuals are slightly younger and have endured longer spells since disability onset before being awarded DI on average.

[20] While income from self-employment is also subject to a reduction in DI benefits, very few DI recipients have income from self-employment (less than 1 percent of recipients with some earnings).

[21] As a general rule, employees get 12 percent of last year's earnings as holiday pay in June the following year regardless of current employer-employee relationship(s).

Table 1: Summary Statistics

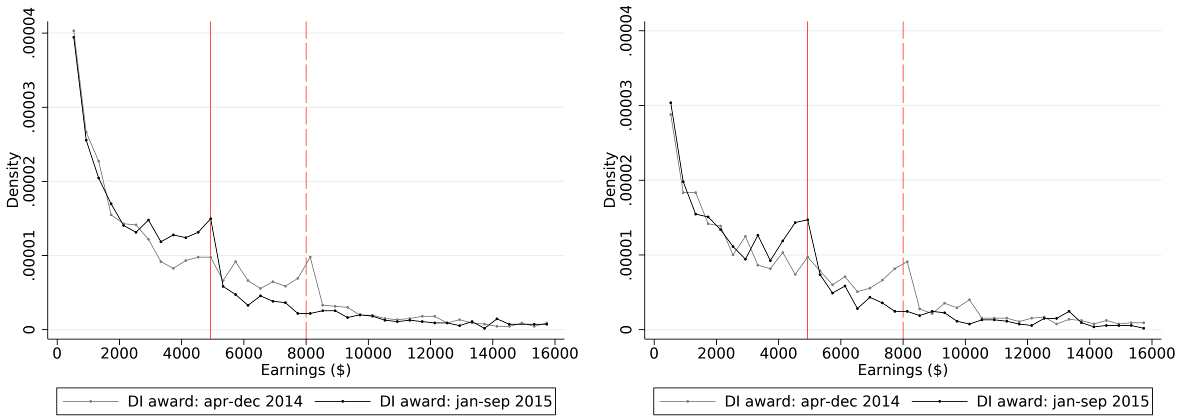
	All DI recipients		Estimation sample			
DI award:	- dec 2014		apr-dec 2014		jan-sep 2015	
Kink point:	$8,000		$8,000		$4,937	
Outcome variables:	mean	sd	mean	sd	mean	sd
Annual earnings ($)	993	(4,309)	627	(3,229)	463	(2,450)
Any annual earnings (%)	15.6		12.1		11.0	
DI Information:						
Uncapped DI benefits ($)	35,566	(6,258)	34,485	(6,570)	36,207	(8,193)
Pre-disability earnings ($)	57,334	(15,197)	59,410	(15,233)	60,224	(16,395)
Benefit replacement rate	.62	(.09)	.59	(.10)	.61	(.11)
DI award date	2003	(11)	2014	(0)	2015	(0)
Disability onset date	1999	(9)	2007	(4)	2009	(4)
Fraction from TDI program	.41		.92		.89	
Fraction from prior TDI programs	.27		.57		.30	
Individual characteristics:						
Age	52.3	(11.0)	47.6	(12.1)	48.0	(13.1)
Age at DI award	40.3	(13.8)	46.3	(12.1)	47.4	(13.1)
Years of schooling	10.7	(2.1)	10.9	(2.2)	11.0	(2.2)
Number of children	1.6	(1.4)	1.7	(1.4)	1.6	(1.4)
Fraction females	.56		.56		.54	
Fraction cohabitants	.48		.51		.51	
Number of recipients	236,568		16,620		13,697	

Notes: All samples consist of totally disabled DI recipients with DI receipt 31.12.2015. Outcome variables are measured in 2016 and 2017. Age, years of schooling, cohabitant status and number of children are measured in 2015. All other covariates are either pre-determined or constant over time. Earnings and DI benefits are measured in 2016 dollars (NOK/USD = 7.5).

Next, I examine the earnings distributions for each group in the estimation sample around the annual kinks. Figure 2 shows the raw earnings distributions for each group in 2016 and 2017 grouped into $400 bins. Specifically, the black solid line indicates the density for recipients awarded DI between January and September 2015, and the vertical red solid line indicates the kink point for these recipients at $4,937. The gray solid line indicates the density for recipients awarded DI between April and December 2014, with the vertical red dashed line indicating the kink point for this group at $8,000. I use the full sample of recipients (i.e. including recipients with zero earnings) to calculate the density for each group. Notably, there is large bunching around each kink. Otherwise, the densities appear to track each other very closely in regions outside of the two kinks. The similarities in densities below the first kink point is particularly striking, indicating that the earnings potential between the two groups is similar.

Figure 2: Earnings Distributions Around the Annual Kinks

(a) 2016 **(b)** 2017



Notes: Panel (a) and (b) show the earnings distributions in $400 bins for DI recipients awarded DI between April 1st and December 31st 2014 (gray line) and recipients awarded DI between January 1st and September 30th 2015 (black line) in 2016 and 2017. The red dashed line and the red solid line indicate the kink point in the budget constraint for each group, respectively. Earnings are measured in 2016 dollars (NOK/USD = 7.5).

4 Methodology

This section outlines the conceptual framework for my empirical strategy. In the empirical application, my goal is to estimate the earnings elasticity with respect to the implicit tax rate implied by the phase-out of DI benefits. Identification of the elasticity relies on the fact that DI recipients are subject to different budget sets depending on award date.

4.1 Theoretical Framework

My framework follows Blomquist *et al.* (2019) but focus on the kink in the consumption-leisure space for DI recipients as opposed to a kink in the income tax schedule. I assume that recipients maximize the following quasi-linear utility function:

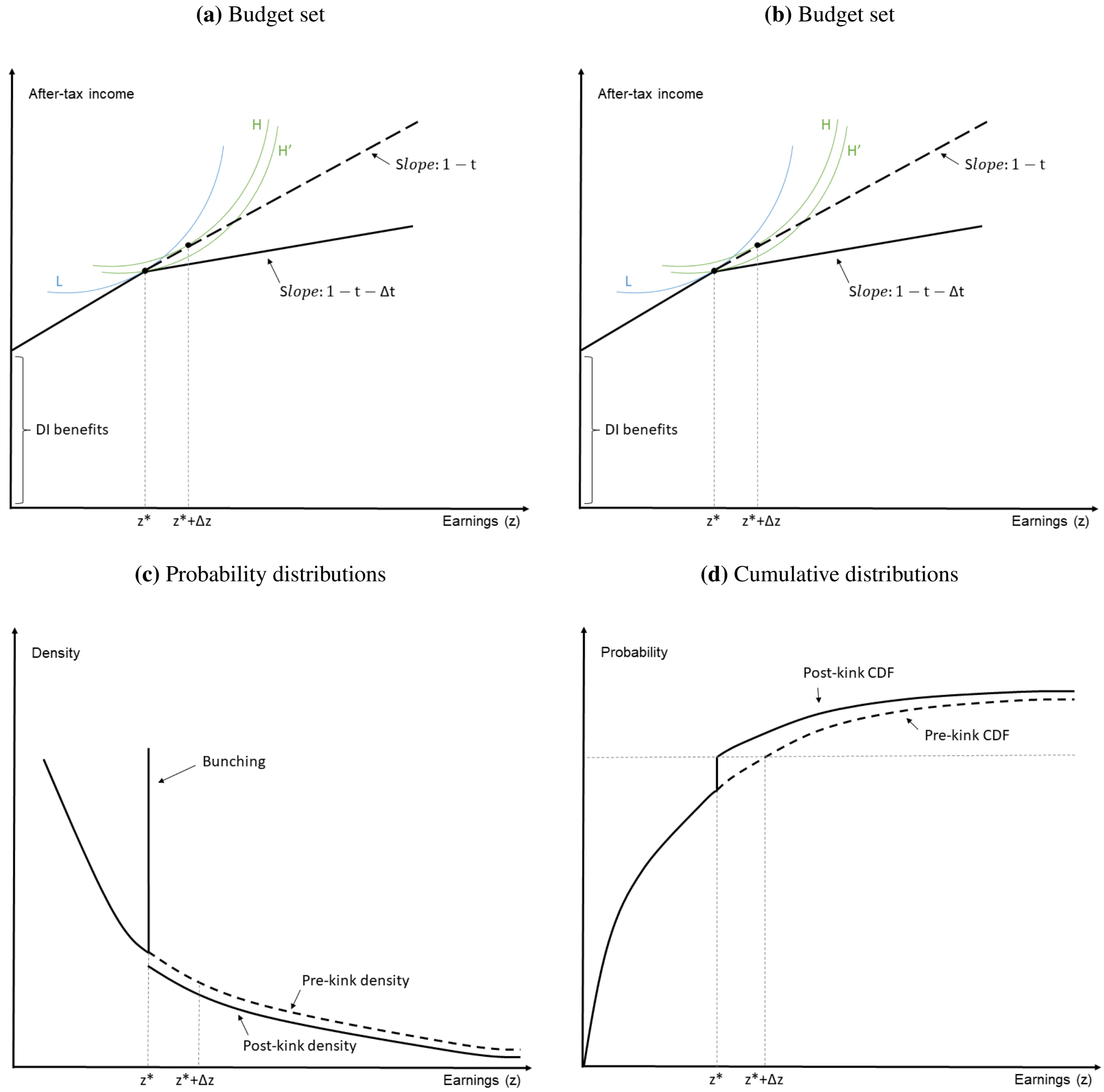
$$U(c,z) = c - \frac{n}{1+\frac{1}{e}}\left(\frac{z}{n}\right)^{1+\frac{1}{e}} \tag{2}$$

subject to the budget constraint $c = B + z - T(z;B)$ where c is consumption, B is DI benefits and z is before-tax earnings. $T(z;B)$ is the implicit tax liability, and depends on earnings and DI benefits. n is an ability parameter and e is the earnings elasticity with respect to the marginal net-of-tax rate $1-t$. A key identifying assumption is that the distribution of ability n is smooth in the population. This assumption implies that, given a linear tax system $T(z;B) = t\cdot(z+B)$, the smooth ability distribution translates into a smooth after-tax earnings distribution. Maximization of $U(c,z)$ subject to the linear budget constraint yields $z = n(1-t)^e$. Note that $z = n$ if $t = 0$, i.e. n can be interpreted as potential earnings if there were no implicit taxes on earnings. Because of the quasi-linearity assumption, the model rules out income effects of tax changes on earnings.[22]

[22] If income effects are present, the estimated elasticity will be downward biased as the income effect induces individuals to work more (assuming leisure is a normal good). Hence, the compensated elasticity (accounting only for substitution effects) will be larger than the uncompensated elasticity (accounting for income and substitution effects). However, Bastani & Selin (2014) show that even large income effects induced by a kink in the budget set have little impact on the compensated elasticity.

Now, suppose that a kink is introduced at some threshold z^*, with the tax rate increasing from t to $t+\Delta t$. The tax schedule can now be expressed as $T(z;B) = t \cdot (z+B) + \Delta t \cdot (z - z^*) \cdot 1(z \geq z^*)$. Figure 3 (a) and (b) illustrate how the slope in the budget set changes at z^*. Individual H is the individual with the highest earnings before the kink is introduced who would locate at z^* with the kink, illustrated by the slope in the indifference curve H' being exactly equal to the slope in the budget set above the kink $1-t-\triangle t$. This is the marginal bunching individual. The individual would locate at $z^* + \triangle z$ before the kink is introduced and locate at z^* with the kink in the budget set. Individual L would locate at z^* in both cases. Individuals with earnings in the interval $[z^*, z^* + \triangle z]$ before the kink is introduced would locate at z^* with the kink. This is illustrated in Figure 3 (c) which shows the probability density distributions pre- and post the introduction of the kink. The earnings distribution is smooth in the population before

Figure 3: Budget Set and Density Distributions



Notes: Panel (a) and (b) show after-tax income as a function of annual earnings with a linear tax t (dashed line) and with the tax increasing from t to $t+\triangle t$ at the kink point z^* (solid line). $z^* + \Delta z$ denotes the earnings of the marginal buncher, i.e. the individual with the highest earnings without the implicit tax on DI benefits who will locate at z^* with the implicit tax on DI benefits above z^* . Panel (c) shows the probability density distribution with and without the kink at z^*. Panel (d) shows the corresponding cumulative distributions.

the kink is introduced, while there is substantial bunching at the kink after the kink is introduced. Figure 3 (d) shows the corresponding cumulative distributions. Because the pre-kink earnings distribution is smooth in the population, the point in the pre-kink CDF at $z^* + \triangle z$ corresponds to the point in the post-kink CDF of the marginal bunching individual who is now locating at z^*. This is illustrated by the horizontal dashed line.

Next, consider two groups $j = 1,2$ drawn from the same distribution of ability n. Group 1 is subject a constant marginal tax rate t for the whole budget set, while for group 2 the marginal tax rate increases from t to $t + \Delta t$ at z^*. Let $F_1(Z)$ and $F_2(Z)$ be the corresponding cumulative distribution functions for each group, i.e. $F_i(Z) = Pr(Z_i \leq z_i)$ for $j = 1,2$. Then from theorem 3 in Blomquist *et al.* (2019), it follows that

$$e = \frac{\ln(\frac{z^*}{z^*+\Delta z})}{\ln(\frac{1-t-\Delta t}{1-t})} \tag{3}$$

where e is the earnings elasticity with respect to the net-of-tax rate t, and Δz is the earnings response of the marginal bunching individual defined as the point where $F_1(z^* + \Delta z) = F_2(z^*)$.[23] Intuitively, this corresponds to the same point in the CDF between the two groups at the kink point for group 2 where the marginal tax rate differs between the two groups. Because the distribution of ability is smooth, each point in the CDFs of the two groups corresponds to individuals with the same ability, or potential earnings.

4.2 Empirical Implementation

My approach to estimate the earnings elasticity e relies on the fact that two groups of DI recipients in the estimation sample are subject to different budget sets. In particular, the kink point for recipients awarded DI in 2015 is $z^* = \$4,937$ where the marginal tax rate on earnings increases from t_0 to t_1. Here, t_0 is the regular marginal tax rate on income and $t_1 = t_0 + \tau(1 - t_0)$, where τ is the benefit phase-out rate.[24] At the same point in the budget set, the marginal tax rate is t_0 for recipients awarded DI before 2015. Using the fact that $t_0 \neq t_1$ at z^*, I estimate the earnings elasticity e using the following formula:

$$\hat{e} = \frac{\ln(\frac{z^*}{z^*+\Delta\hat{z}})}{\ln(\frac{1-t_1}{1-t_0})} \tag{4}$$

where $\Delta\hat{z}$ is the estimated response of the marginal buncher, and is given by $\hat{F}_0(z^* + \Delta\hat{z}) = \hat{F}_1(z^*)$, where $F_i(Z)$ is the cumulative distribution function of DI recipients with treatment status $i = 0,1$ where treatment status $i = 0$ indicates recipients awarded DI before 2015 and $i = 1$ indicates recipients awarded DI in 2015 or later. Intuitively, the estimated earnings response of the marginal buncher, or the last individual with earnings at the kink $z^* = \$4,937$ in the treated group corresponds to the individual at the same point in the CDF in the non-treated group. The crucial assumption in my setting is that potential earnings, i.e. earnings without the phase-out of DI benefits for individuals with DI award on either side of the cut-off date are drawn from the same distribution, and that this distribution is smooth. Additionally, I assume that the earnings distribution of recipients awarded DI before 2015 is unaffected by the kink at \$8,000 in the interval $[0, z^* + \Delta z]$, i.e. recipients who would locate above this region without the kink would not locate in this region with the kink.

[23] A formal proof of this result is provided in Blomquist *et al.* (2019).

[24] For most recipients, the marginal tax rate on income t_0 is about 35 percent. At the kink, the marginal tax rate therefore increases from about 35 percent to about .35 + .66·(1-.35) = 78 percent.

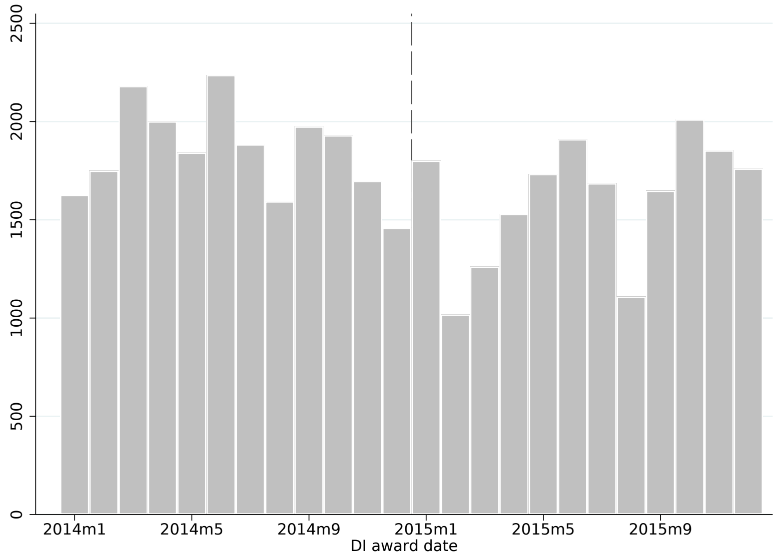
Inference To calculate the standard error of the estimated elasticity, I use bootstrap methods. First, I generate many earnings distributions by random resampling with replacement. As the estimation sample may include the same individuals more than once, I use a pairs-cluster bootstrap that accounts for clustering at the individual level, keeping all observations of each individual that I resample. Second, I re-estimate the elasticity within each sample, and define the standard error as the standard deviation of the distribution of the elasticity. In all estimations, I use 500 repetitions.

4.3 Threats to Identification

The validity of my empirical design hinges on the assumption that recipients with DI award on either side of the cut-off at 1st of January 2015 are drawn from the same distribution of potential earnings. In other words, the earnings distributions should be comparable if these recipients were subject to the same benefit phase-out policy. The validity of my design therefore requires that recipients are unable to precisely manipulate the DI award date. Crucially, there were no changes to eligibility for DI around the cut-off date. Although some institutional details were formalized as early as 2011, the policy change was announced as late as October 2014. Because processing times of applications usually take between 4-9 months, recipients were unable to gain entry before the cut-off date of January 1st, 2015. Even if potential applicants would have some influence over when to apply for DI, recipients would be unable to manipulate the award date precisely because of uncertainty in the processing time of applications. Therefore, the variation in treatment should be randomized close to the cut-off.[25]

Figure 4 shows the distribution of DI award date around the cut-off. Because I only have monthly data on DI award, the assignment variable in this context is discrete. I therefore follow Frandsen (2017) and perform a formal statistical test for bunching on either side of the cut-off. While this test rejects the null hypothesis of no bunching, the test also rejects the null in more than half of hypothetical placebo

Figure 4: Distribution of DI Award Date



Notes: The figure shows the distribution of DI award between January 2014 and December 2015. The sample consists of totally disabled recipients on DI receipt 31.12.2015 aged 18-66 years.

[25] See Lee & Lemieux (2010).

cut-off points during the time period considered in the empirical analysis.[26] This suggests a natural high variation in the number of allowed applications may explain why the test of no bunching fails in this context rather than manipulation of the DI award date.

While recipients awarded DI before the cut-off date were subject to a more lenient benefit phase-out, the policy was announced as a transitional policy which would revert to a common policy in 2019. Therefore, the gain from being subject to the more lenient benefit phase-out was limited. If one would worry about potential manipulation of the DI award date, a bigger concern is the fact that calculation of DI benefits was also changed at the cut-off date. Because DI benefits were calculated using more recent years of earnings history for awards after 1st of January 2015, most recipients would receive slightly higher levels of DI benefits if awarded DI after this date due to real wage growth. While this would require detailed information about earnings history and institutional details among recipients, I cannot rule out this possibility. In order to shed light on this concern, I calculate the DI benefits that recipients hypothetically would receive if awarded DI after the cut-off date.[27] If DI recipients were able to manipulate the award date, one would expect recipients with high potential DI benefits post the cut-off date to locate to the right of the cut-off, and vice versa for recipients with low potential DI benefits who would locate before the cut-off date. As a formal test, I run a regression of projected DI benefits (if recipients were awarded DI after the cut-off date) on a dummy which equal to 1 if the observed award date is after the cut-off date using 2 months of bandwidth on each side of the cut-off and a triangular kernel. Reassuringly, the coefficient on the dummy is highly insignificant with a p-value of 0.47, lending support to the claim that recipients were unable to manipulate the DI award date to get maximum DI benefits.

To shed further light on possible manipulation of the DI award date, I extend the above exercise to include a rich set of individual characteristics. If recipients indeed were unable to manipulate the DI award date, any pre-determined covariate should have the same distribution on each side, close to the cut-off. In Appendix Table A.1, I report coefficients and standard errors of each pre-determined covariate running the same regression as described above. While most covariates appear smooth around the cut-off and are insignificant at conventional levels, one exception is years of education which is significant at the 1% level. However, based on the large number of covariates I consider, the probability of observing changes in one covariate around the cut-off is quite large. If I perform a joint test for all covariates, I cannot reject the null of no manipulation at conventional levels of significance as reported in Appendix Table A.1. The p-value of the joint test is 0.18.[28]

Income Effects and Weighting Strategy As recipients awarded DI before the cut-off date receive a slightly lower DI benefit, this might induce this group of recipients to work more than recipients awarded DI after the cut-off date through an income effect.[29] In that case, this would shift the pre-kink CDF in Figure 3 (d) to the right and bias the elasticity upwards. Ideally, one would want to identify the

[26]The test rejects 12 out of the 22 placebo cut-off points between each pair of months of DI award between January 2014 and December 2015.

[27]Unfortunately, I am unable to calculate DI benefits using the definition before 2015 due to data limitations.

[28]Using the sample with only 1 month of bandwidth on each side yields the same conclusions for each covariate separately as well as jointly, with the p-value of the joint test being 0.13.

[29]Several studies have shown that increasing (reducing) the generosity of DI benefits reduces (increases) beneficiaries labor supply through an income effect. See e.g. Gruber, 2000; Marie & Castello, 2012; Gelber *et al.*, 2017a; Deuchert & Eugster, 2019.

income and substitution effects separately. Unfortunately, separating the two effects is not possible in this context as I only have one instrument that is the DI award date. In order to address the issue of DI benefits not being directly comparable between the two groups, I apply the weighting approach proposed by Kline (2011).[30] This approach accounts for differences in pre-determined covariates between the two groups, and in particular the level of DI benefits. Intuitively, recipients awarded DI before the cut-off with high DI benefits are assigned a higher relative weight in estimations. While the approach does not fully account for the difference in DI benefits between the two groups, it reduces the difference from about 4% to 1%. In Section 5, I show that the bias in the compensated elasticity is small if income effects induces recipients to work more due to a lower DI benefit.

Another advantage of the weighting approach is that it accounts for differences in (other) pre-determined covariates between the two samples, including years of education which was significant in the balancing tests. While my estimation sample ideally would only include observations very close to the cut-off, deciding the bandwidth, i.e. the sample of DI recipients on each side of the cut-off date is a trade-off between bias and variance. As one includes observations further away from the cut-off, differences in pre-determined covariates increase. In particular, a larger share of recipients awarded DI early in 2014 had endured spells on a prior temporary DI program before being awarded DI. These recipients were slightly younger and had endured longer spells between DI award and disability onset, and could therefore have slightly different earnings potential than other recipients if i.e. health improves or worsens over time.[31] The weighting approach assigns lower weights to these recipients as they are less likely to be awarded DI after the cut-off date. Appendix Table A.2 shows that the difference between the two groups in the estimation sample is insignificant at all conventional levels when using the weighting approach. This result holds for all covariates separately as well as jointly. The p-value of the joint test is 0.43.

As the goal of my main estimation strategy is not to identify average effects of the different incentives to work, it is not entirely clear how to decide the bandwidth in this context. In my baseline specification, I use 9 months of bandwidth on each side of the cut-off which is the optimal bandwidth suggested by Calonico *et al.* (2014).[32] A potential worry using observations further away from the cut-off is trends in earnings potential if i.e. health improves or worsens over time. Because of this, I use triangular weights in my baseline specifications. To examine the validity of my findings, I perform several robustness checks. In particular, I show that the estimated earnings elasticity is relatively robust to bandwidth selection. I also show that average effects are practically indistinguishable if I include linear or quadratic trends in the DI award date in a standard regression discontinuity design.

[30] I implement this adjustment by estimating the probability of each recipients being awarded DI after the cut-off date $P(I_i = 1|x_i)$ using a logistic regression. As the level of DI benefits may be correlated with other covariates such as age, education and pre-DI earnings, only re-weighting the level of DI benefits might induce imbalance in covariates that are correlated with DI benefits between the two samples. Therefore, I include the full set of covariates included in Table A.2 along with uncapped DI benefits as control variables. Recipients awarded DI before the cut-off date are then re-weighted using the propensity score weight $w(x_i) = \frac{1-P(I=1)}{P(I=1)} \frac{P(I_i=1|x_i)}{1-P(I_i=1|x_i)}$ where $P(I = 1)$ denotes the probability of being awarded DI after the cut-off date.

[31] As a robustness check, I exclude recipients who have endured spells on a prior DI program. As opposed to the full (unweighted) sample of DI recipients, this alternative sample is balanced in terms of pre-determined covariates as reported in Appendix Table A.1. Although less precise, the estimated elasticity is practically indistinguishable from the estimated elasticity using the full sample with the weighting approach.

[32] The optimal bandwidth is calculated using the weighted sample and a triangular kernel with no (linear) trends in the assignment variable.

Extensive Margin Responses While my baseline model does not incorporate responses at the extensive margin, it is possible that the lower kink induces some individuals to stop working altogether. Theoretically, a higher average tax on earnings will induce some individuals to stop working if individuals have a fixed cost of labor force participation. In Section 6, I document that the fraction of recipients with some earnings is lower among recipients with the kink at $4,937 compared to recipients with the kink at $8,000. I also show that the elasticity is substantially upward biased if I do not account for extensive margin responses by calibrating a model with a fixed cost of labor force participation to the empirical distribution. Intuitively, the recipients who stop working would have earned above the kink under the more lenient benefit phase-out policy. Because of this, there would be missing mass in the upper part of the earnings distribution which would shift the post-kink CDF in Figure 3 (d) to the left. If one does not account for this response, the response of the marginal bunching individual will be overstated and the estimated elasticity upward biased. In order to adjust for extensive margin responses, I follow Ruh & Staubli, 2019 and assume that the distribution of recipients who stop working is the same as the observed earnings distribution above the kink.[33] In Section 6, I show that the estimated elasticity is very close to the theoretical elasticity when incorporating this adjustment procedure in a simulation exercise.

5 Main Results

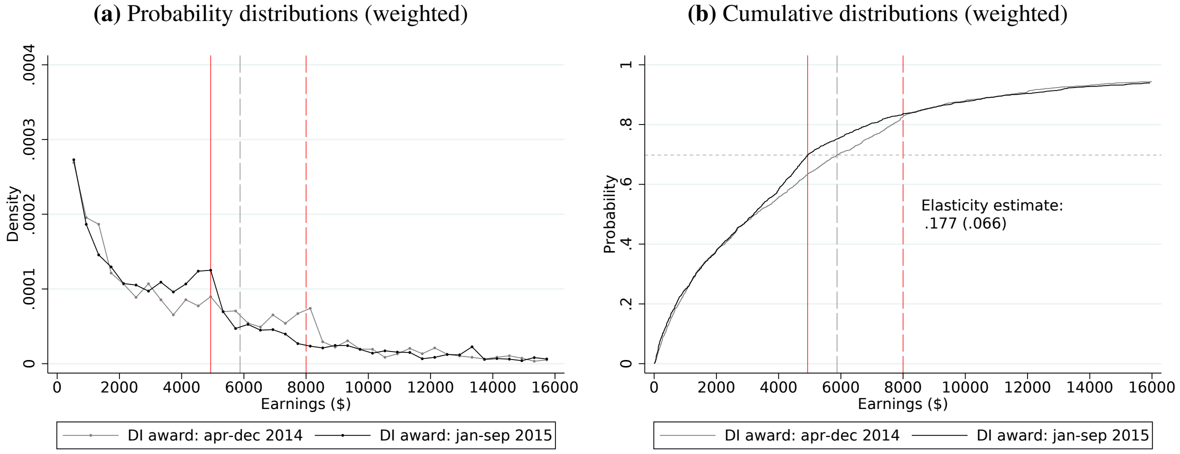
This section presents the main results and begins with a graphical representation of the estimation strategy. I then proceed by presenting the main analytical results before challenging the empirical specification in several ways.

5.1 Graphical Evidence of Behavioral Responses

I begin my analysis by providing a graphical representation of the estimation procedure. Figure 5 (a) shows the weighted earnings distributions for the pooled sample of recipients with some earnings for each group in the estimation sample grouped into $400 bins. First, recipients are weighted by a triangular weight so that recipients close to the cut-off are assigned higher relative weights. Second, recipients awarded DI before the cut-off are weighted by propensity score weights that accounts for differences in pre-determined covariates between the two groups, and in particular the level of uncapped DI benefits. Third, I incorporate the adjustment procedure for extensive margin responses outlined in Section 4 by adding recipients awarded DI after the cut-off to the right of the kink until the fraction of recipients with some earnings is the same between the two groups. Responses should therefore be interpreted as intensive margin responses to the implicit tax on earnings as DI benefits are phased out above the kink.

[33] Specifically, I add individuals to the right of the kink for recipients subject to the lower exemption threshold until the fraction of working individuals is the same for both groups.

Figure 5: Graphical Representation of Earnings Elasticity Estimation: Pooled Sample



Notes: Panel (a) shows the (weighted) pooled earnings distributions for 2016 and 2017 in $400 bins for recipients with positive earnings awarded DI between April 1st and December 31st 2014 (gray line) and recipients awarded DI between January 1st and September 30th 2015 (black line). The red dashed line and the red solid line indicate the kink point in the budget constraint for each group, respectively. Both groups are weighted by a triangular kernel weight in estimations. For recipients awarded DI in 2015, I add recipients to the right of the kink until the fraction of recipients with positive earnings is the same as for recipients awarded DI in 2014 in order to adjust for extensive margin responses. Recipients awarded DI in 2014 are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of being awarded DI in 2015 and $P(I_i=1|x_i)$ is estimated with a logit model using the covariates in Table A.2 (including uncapped DI benefits) as control variables. Panel (b) shows the corresponding cumulative distributions. The horizontal dashed line indicates the CDF at the kink for recipients awarded DI in 2015. The vertical gray dashed line indicates earnings of the marginal buncher $z^* + \triangle z$. Standard errors are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications. Earnings are measured in 2016 dollars (NOK/USD = 7.5).

Figure 5 (b) shows the corresponding cumulative earnings distributions for each group. The distributions appear to track each other very closely until about $3,000 from which the cumulative distribution for recipients with DI award after the cut-off increases more steeply due to recipients bunching around the kink at $4,937 (indicated by the red solid line). The same pattern is observed for recipients with DI award before the cut-off who bunch around the kink at $8,000 (indicated by the red dashed line). Reassuringly, the cumulative distributions appear to track each other very closely above the second kink, indicating that the distributions would have been comparable if the two groups were subject to the same benefit phase-out policy. The horizontal dashed line indicates the point in the CDF for the last individual who bunches at the kink at $4,937 for the sample of recipients awarded DI after the cut-off. The vertical dashed line indicates the earnings of the individual at the same point in the CDF for the sample of recipients with DI award before the cut-off. This is the estimated earnings of the marginal bunching individual. Then, I plug this estimate into the formula for the elasticity given by Equation 4 and estimate an earnings elasticity of about 0.18.

5.2 Earnings Elasticity Estimates

In this section, I present the main estimation results. Table 2 reports estimates of the earnings elasticity (e), the earnings response of the marginal buncher (Δz) and the average intensive margin response for the main estimation sample. I present estimates using the weighting approach that accounts for differences in pre-determined covariates between the two groups and unweighted estimates for comparison. Both specifications use triangular weights and incorporates the adjustment procedure for extensive margin responses outlined in Section 4.

Table 2: Earnings Elasticities for Pooled Sample and by Year

	Elasticity (e)		Earnings response ($) Marginal buncher ($\Delta z$)		Average response		Observations <individuals>
Full sample	.177***	.198***	944**	1,067***	157	531*	59,753
(2016-2017)	(.066)	(.059)	(392)	(357)	(308)	(307)	<30,317>
By year:							
2016	.132**	.172***	689*	914***	151	455	30,317
	(.067)	(.058)	(368)	(341)	(322)	(316)	
2017	.217***	.218***	1,186***	1,192***	163	532	29,436
	(.064)	(.063)	(401)	(392)	(396)	(410)	
Weighted	Yes	No	Yes	No	Yes	No	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Notes: The table reports intensive margin estimates of the elasticity, the earnings response of the marginal buncher defined as the earnings response at the first kink and the average response of recipients for the full sample and by each year (2016 and 2017). The sample consist of totally DI recipients awarded DI between 1st of April 2014 and 30th of September 2015. For recipients awarded DI in 2015, I add recipients to the right of the kink until the fraction of recipients with positive earnings is the same as for recipients awarded DI in 2014 in order to adjust for extensive margin responses. For the weighted estimates, recipients awarded DI in 2014 are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of being awarded DI in 2015 and $P(I_i=1|x_i)$ is estimated with a logit model using the individual characteristics in Table A.2 and uncapped DI benefits as control variables. Both groups are weighted by a triangular kernel weight in all estimations. Standard errors (in parentheses) are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications. Earnings are measured in 2016 dollars (NOK/USD = 7.5).

For the weighted pooled sample, the estimated earnings response for the marginal buncher is large and statistically significant. I estimate that without the kink, the marginal bunching individual who bunches at the kink at $4,937 would have earned $944 or about 19 percent more. This response corresponds to an earnings elasticity with respect to the implicit net-of-tax rate of about 0.18. In contrast, the average earnings response is $157 and is only 17 percent as large as the response of the marginal buncher. This estimate can be interpreted as the average intensive margin response of increasing the kink point from $4,937 to $8,000. The fact that this estimate is small and insignificant suggests that increasing the kink point only affects recipients with earnings in a narrow region. The unweighted estimates are slightly larger, but qualitatively similar to the weighted estimates. This suggests that differences in predetermined covariates, and in particular DI benefits do not change my main conclusions. Notably, the estimated earnings response for the marginal buncher and the corresponding elasticity is larger for 2017 than for 2016. While this might be explained by responses increasing over time as recipients overcome frictions such as changing hours worked and learn the tax schedule, the difference between the two years is not significant at conventional levels. The average response is almost indistinguishable between the two years.

In comparison to other studies, the estimated elasticity is significantly higher than in studies that exploit kinks in the income tax schedule. These studies typically find elasticities in the range of 0-0.05 among wage earners (Saez, 2010; Chetty *et al.*, 2011; Bastani & Selin, 2014; Paetzold, 2019). Compared to similar studies on DI beneficiaries, Ruh & Staubli (2019) and Zaresani (2020) estimate earnings elasticities of 0.27 and 0.20 in Austria and Canada, respectively. However, both these studies estimate a structural elasticity as opposed to my study. Although the kink in this setting is large and salient implying that recipients are more likely to overcome adjustment costs, my estimate might be attenuated by i.e. lumpy hours or imperfect information about the benefit phase-out. In that case, the estimate represents a lower bound of the long-run elasticity. Also, it is important to keep in mind that my estimation approach relies on local moments around the kink at $4,937. Compared to other countries, the exemption threshold in Norway is quite low.[34] The earnings elasticity might differ in other countries where the exemption threshold is higher, as this subgroup of beneficiaries have higher earnings capacity and might also be different in other dimensions. In that case, it is likely that my estimate represents a lower bound compared to recipients around the exemption threshold in other countries.

Heterogeneity To shed further light on my main findings, I explore heterogeneity in earnings responses to the implicit tax on earnings. Appendix Table A.3 reports the estimated earnings responses for different subgroups in the population. Somewhat surprisingly, I am unable to detect any statistically significant differences in effects across the different subgroups. Point estimates are slightly higher for older recipients, and slightly higher for recipients with high (uncapped) DI benefits. However, I lack statistical precision to draw any firm conclusions. I am unable to detect any notable differences in responses between genders, recipients with different levels of education and recipients with different levels of earnings prior to disability onset, with point estimates being very similar across subgroups.

5.3 Robustness Analysis

In order to verify the validity of my main results, I do a series of robustness checks reported in Table 3. The first row reports estimates using the baseline specification with triangular weights and 9 months of bandwidth on each side of the cut-off. Next, I estimate responses using rectangular weights implying that all recipients in my sample are assigned the same relative weight in the initial estimation procedure. Although slightly lower, the estimated effects are well within one standard error of the baseline specification. In the third specification, I exclude recipients who had endured spells on a prior temporary DI program as this group of recipients were more likely to be awarded DI early in 2014 (i.e. before the cut-off). This alternative sample is well balanced in terms of pre-determined covariates as reported in Appendix Table A.1. Using this alternative sample, estimates are remarkably similar as to the full sample of recipients. Next, I perform a placebo test by pretending that the cut-off date for being subject to the different phase-out policies in DI benefits was 1st of January 2014 instead of 1st of January 2015. Reassuringly, the point estimates are small and insignificant. Appendix Figure A.1 shows the probability distributions and cumulative distributions for each group in the placebo sample, respectively. The distributions of the two groups in the placebo sample appear remarkably similar. This lends some support to the assumption of earnings potential being comparable for recipients with slightly different DI award dates.

Next, I examine how the estimated earnings responses change as I deviate from the baseline bandwidth selection of 9 months. While the estimated elasticity is somewhat lower if I use only 1 month of bandwidth on each side of the cut-off, the estimated elasticity is within one standard error of my main specification. For specifications using 2 months of bandwidth or more, the estimated earnings responses appear stable and are very similar to my main specification which is reassuring. In Appendix Figure A.2 (a), I show how the estimated elasticity vary with bandwidth selection by plotting point estimates with 95 percent confidence intervals for each bandwidth between 1 and 12 months. The figure yields the same conclusion of point estimates being relatively stable to bandwidth selection.

[34] In the US and the UK, the exemption thresholds are about $14,000 and $8,000, respectively.

Table 3: Robustness Checks for Earnings Responses

	Elasticity (e)		Earnings response ($) Marginal buncher ($\Delta z$)		Earnings response ($) Average response		Observations <individuals>
Baseline	.177***	.198***	944**	1,067***	157	531*	59,753
specification	(.066)	(.059)	(392)	(357)	(308)	(307)	<30,317>
Rectangular	.141***	.177***	741***	943***	107	559**	59,753
weights	(.049)	(.037)	(278)	(218)	(241)	(245)	<30,317>
Alternative sample:	.181**	.173**	951**	907**	60	60	32,782
Not on prior TDI programs	(.073)	(.074)	(426)	(426)	(350)	(347)	<16,762>
Placebo sample:	-.026	-.043	-116	-187	115	-70	57,189
DI award 2013-2014	(.033)	(.033)	(142)	(139)	(315)	(298)	<29,046>
Alternative bandwidths:							
1 month	.117	.117	585	585	-797	-709	6,422
	(.126)	(.115)	(655)	(607)	(931)	(918)	<3,260>
2 months	.182	.182*	947	947	-151	-67	11,757
	(.112)	(.106)	(627)	(604)	(723)	(722)	<5,976>
4 months	.190**	.208***	999**	1,106**	46	277	24,972
	(.083)	(.079)	(478)	(465)	(507)	(521)	<12,673>
6 months	.172**	.181***	906**	958**	53	341	39,006
	(.071)	(.066)	(412)	(387)	(404)	(405)	<19,794>
12 months	.154***	.186***	809**	996***	140	552**	81,764
	(.056)	(.048)	(322)	(285)	(281)	(273)	<41,500>
Weighted	Yes	No	Yes	No	Yes	No	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Notes: The table presents intensive margin estimates of the elasticity, the earnings response of the marginal buncher defined as the earnings response at the first kink and the average response of recipients for the baseline specification and each alternative specification. For recipients awarded DI in 2015, I add recipients to the right of the kink until the fraction of recipients with positive earnings is the same as for recipients awarded DI in 2014 in order to adjust for extensive margin responses. For the weighted estimates, recipients awarded DI in 2014 are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of being awarded DI in 2015 and $P(I_i=1|x_i)$ is estimated with a logit model using the covariates in Table A.2 (including uncapped DI benefits) as control variables. Both groups are weighted by a triangular kernel weight in estimations. Standard errors (in parentheses) are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications. Earnings are measured in 2016 dollars (NOK/USD = 7.5).

As I am unable to impose functional form assumptions in the assignment variable (i.e. the DI award date) due to the nature of the empirical design, a potential worry using observations further away from the cut-off arises if earnings potential is correlated with DI award due to e.g. health improving or worsening over time. To further investigate the validity of my findings, I examine whether earnings are correlated with the assignment variable. Appendix Table A.4 reports regression discontinuity estimates using no trend in the assignment variable, a linear trend and a quadratic trend, respectively.[35] This is the average effect of being subject to the more lenient benefit phase-out policy (i.e. kink at $8,000 versus $4,937) in the population. I use a triangular kernel and 9 months of bandwidth on each side of the cut-off as in the main specification. Reassuringly, the point estimates (with control variables) are very similar across the different specifications. The point estimate is $117 using no functional form in the assignment

[35] The regression can be expressed as $y_{it} = \alpha + f(x_i) + \beta I_{x_i<c} + \delta X_i + \varepsilon_{it}$ where y is earnings, x is the assignment variable (i.e. the DI award date) and c is the cut-off date at 1st of January 2015. $I_{x_i<c}$ is a dummy equal to 1 if being awarded DI before the cut-off date. X is a vector of covariates and ε is the error term. $f(x)$ takes the functional form for each specification as explained in text.

variable, \$113 with linear trends and \$108 for the quadratic specification, respectively.

Finally, I investigate how income effects would affect the elasticity estimate. As recipients awarded DI in 2015 or later receive slightly higher levels of DI benefits on average due to institutional changes, this might induce recipients awarded DI before the cut-off date to work more compared to recipients awarded DI after the cut-off through an income effect. While evidence on intensive margin responses to the benefit generosity is scarce, Gelber *et al.* (2017a) estimate an income elasticity of earnings with respect to DI benefits of about 1.[36] Using the weighting approach, the difference in DI benefits between the two groups in the estimation sample is about 1 percent. Assuming an income elasticity of 1, i.e. recipients reduce earnings by 1 percent if the level of DI benefits increase by 1 percent, a back-of-the-envelope calculation yields an elasticity of .166 or about 6 percent lower than the baseline estimate.[37] This suggests that the upward bias in the elasticity due to income effects is small in this context.[38]

6 Extensive Margin Responses

Since a lower kink point increases the average tax for recipients with earnings above the kink, it is possible that the lower kink induces some recipients to stop working altogether. In this section, I estimate the magnitude of the extensive margin response in my setting. I then investigate how extensive margin responses affect the estimate of the intensive margin earnings elasticity.

6.1 Empirical Analysis

To assess the extensive margin responses of being subject to the different benefit phase-out policies, I implement a simple regression discontinuity (RD) design. Specifically, I run the following regression:

$$y_{it} = \alpha + f(x_i) + \beta I_{x_i<c} + \delta X_i + \varepsilon_{it} \tag{5}$$

where y is the outcome variable (such as a dummy for having positive earnings), x is the assignment variable (i.e. the DI award date) and c is the cut-off date at 1st of January 2015. $f(x)$ is an unknown functional form of the assignment variable and $I_{x_i<c}$ is a dummy equal to 1 if individual i is awarded DI before the cut-off date. X is a vector of covariates and ε is the error term. β is the coefficient of interest, and measures the average effect of being subject to the more lenient benefit phase-out policy (i.e. kink at \$8,000 versus \$4,937) on labor force participation in the population. The validity of my RD design hinges on recipients not being able to manipulate the assignment variable, which I outlined in Section 4.3. I use the same baseline specifications as for the main empirical strategy using a triangular kernel, 9 months of bandwidth on each side of the cut-off and no trend in the assignment variable (i.e. $f(x) = 0$). For consistency, I also incorporate the same weighting approach as outlined in Section 4.

Table 4 reports estimates of Equation 5 for the full estimation sample. Column 1 and 2 shows that the more lenient exemption threshold increased labor force participation by about 0.8 percentage points or about 7 percent compared to recipients with the kink at \$4,937. This estimate is robust to including trends in the assignment variable, yielding a point estimate of .010 for a linear trend and .009

[36] Most evidence on the effect of benefit generosity on labor supply for DI beneficiaries investigate extensive margin responses. See e.g. Gruber (2000); Marie & Castello (2012); Deuchert & Eugster (2019).

[37] If recipients awarded DI after the cut-off decrease earnings by 1 percent, earnings of the marginal bunching individual would be $.99 \cdot (z^* + \triangle z) = .99 \cdot (4937 + 944) = \$5,822$. The estimates earnings response of the marginal bunching individual would then be $5822 - 4937 = \$885$. Plugging this into Equation 4 yields an elasticity of .166.

[38] As the general level of DI benefits in Norway is higher on average than in the US, it is possible that the income effect for Norwegian DI recipients is smaller than for US recipients . In that case, the bias represents an upper bound.

for a quadratic trend, respectively. Column 3 and 4 shows that the more lenient exemption threshold significantly decreased the average tax of participating in the labor market by about 3 percentage points, or 8 percent. In order to shed light on the magnitude of this response, I follow Kostøl & Mogstad (2014) and calculate the elasticity of labor force nonparticipation with respect to the participation tax rate.[39] The results suggest an elasticity of about 0.11 which is comparable to similar studies on DI recipients.[40] Figure A.2 (b) shows how the elasticity of labor force nonparticipation vary with bandwidth selection. Although the estimate is somewhat higher if I use 1 or 2 months of bandwidth on each side of the cut-off, the estimated elasticity appears relatively robust to bandwidth selection.

Table 4: Extensive Margin Responses and Implied Elasticity of Labor Force Nonparticipation

	Labor force participation		Participation tax rate		Nonparticipation elasticity (ε)		Observations <individuals>
Full sample	.008*	.007*	-.032***	-.032***	.106*	.104*	59,753
(2016-2017)	(.004)	(.004)	(.001)	(.001)	(.058)	(.056)	<30,317>
	[.109]	[.109]	[.401]	[.400]			
Weighted	Yes	No	Yes	No	Yes	No	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Notes: The table reports estimates of labor force participation, the participation tax rate and the implied elasticity of non-participation using a regression where the outcome variable is regressed on a dummy which is equal to 1 if recipients are awarded DI in 2014 using a triangular kernel. The sample consists of totally disabled recipients awarded DI between 1st of April 2014 and 31st of September 2015. For the weighted estimates, recipients awarded DI in 2014 are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of being awarded DI in 2015 and $P(I_i = 1|x_i)$ is estimated with a logit model using the covariates in Table A.2 (including uncapped DI benefits) as control variables. Labor force participation is defined positive earnings excluding holiday pay. The participation tax rate is defined as the implied tax of participating in the labor force including income taxes and the implicit tax of DI benefits. The elasticity of labor force nonparticipation is defined as $\varepsilon = \frac{\Delta(1-LFP)/(1-LFP)}{\Delta PTR/PTR}$ where $\Delta(1-LFP) = -\Delta LFP$ is the estimated effect on labor force nonparticipation. LFP and PTR are the mean labor force participation and participation tax rate of recipients awarded DI in 2015 (in brackets). ΔPTR is the difference in participation tax rates between the different benefit phase-out policies evaluated for the earnings distribution of recipients awarded DI in 2015. Standard errors (in parentheses) are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications.

6.2 Elasticity Estimation with Extensive Margin Responses

I now investigate how extensive margin responses would affect the estimated intensive margin earnings elasticity. As shown in Section 6.1, the lower kink induces some recipients to stop working altogether because of a higher participation tax rate for earnings above the kink. Because this only affects individuals who would have earned above the kink, the density above the kink would shift downwards. This would have a knock-on effect and shift the density below the kink upwards and shift the cumulative distribution to the left. Because of this, I would overstate the response of the marginal bunching individual, and hence also the intensive margin earnings elasticity. To assess the magnitude of this bias, I do a simulation exercise following the same steps as Ruh & Staubli (2019). I base my simulations on the utility function in Equation 2 with the addition of individuals having a fixed cost of labor force participation q that is smoothly distributed across the population. Individuals choose earnings z to maximize

[39] The elasticity of labor force nonparticipation is defined as $\varepsilon = \frac{\Delta(1-LFP)/(1-LFP)}{\Delta PTR/PTR}$, where $\Delta(1-LFP) = -\Delta LFP$ is the estimated effect of labor force nonparticipation being subject to the more lenient benefit phase-out policy, LFP is the mean labor force participation for the recipients with the (low) kink at \$4,937, ΔPTR is the difference in the participation tax rate and PTR is the average participation tax rate for recipients with the (low) kink at \$4,937.

[40] Kostøl & Mogstad (2014) estimate an elasticity of labor force nonparticipation with respect to participation taxes of about 0.12. Ruh & Staubli (2019) find an elasticity of about 0.10.

the following utility function:

$$U(c,z) = c - \frac{n}{1+\frac{1}{e}}\left(\frac{z}{n}\right)^{1+\frac{1}{e}} - q \tag{6}$$

subject to the budget constraint $c = B + z - T(z;B)$. Individuals will only participate in the labor force if $q \leq u^*(c,z) - u_0$ for some $z > 0$, where u_0 denotes the utility from nonparticipation. If the tax of participating in the labor force increases, some individuals would stop working because of the fixed cost q. This allows me to investigate how taxes create extensive margin responses and how it affects the estimated intensive margin elasticity.

The simulation exercise proceeds as follows. First, I calibrate a vector of ability parameters n that best resembles the empirical ability distribution.[41] I assume that n follows a gamma distribution because this distribution most closely resembles the empirical distribution. Appendix Figure A.3 shows that the simulated probability and cumulative distributions closely resembles the empirical distributions. Second, I assign individuals to the two different tax systems considered in this paper, one with a kink at \$4,937 and the other with a kink at \$8,000. I then calculate each individual's optimal earnings z using the estimated elasticity $e = .177$ from Section 5. Third, I calibrate a vector of fixed costs q following Liebman (2002). Specifically, I draw a random fixed cost from a uniform distribution with a lower limit of zero and an upper limit equal to the difference between the individual's utility at the optimal z with no (implicit) taxes and the utility with zero earnings. These fixed costs are then divided by a scalar so that the extensive margin response is consistent with the response estimated in the empirical analysis.

Next, I estimate the intensive margin elasticity considering the three following scenarios: In the first scenario, I assume no fixed cost of labor force participation, i.e. $q = 0$. The second scenario considers individuals with $q \geq 0$ who work only if the utility from working is larger than the utility from nonparticipation. I then estimate the elasticity as in Section 5 but ignore the adjustment procedure for extensive margin responses. In the third scenario, I estimate the elasticity incorporating the adjustment procedure outlined in Section 4. Specifically, I add individuals to the right of the kink for treated individuals until the fraction of working individuals is the same as for non-treated individuals. I assume that the distribution of recipients who have stopped working because of the fixed cost of participation is the same as the observed earnings distribution above the kink.

The results from the simulation exercise for the different cases are shown in Table 5 (column 1) and the corresponding empirical estimates (column 2). With no extensive margin responses, the elasticity is precisely estimated. If I allow for a fixed cost of labor force participation, ignoring the extensive margin response induces a large bias in the estimated elasticity of 71% in this context. However, the bias is small when I incorporate the adjustment procedure explained above. As reported in the table, the adjusted elasticity estimate is slightly smaller than the theoretical elasticity, but the bias is only 2%. This suggests that the adjustment procedure works well in this context. Appendix Figure A.4 shows a graphical representation of the estimation procedure comparing the cumulative distributions for the simulation exercise and the empirical sample, respectively. From the figures, it is clear that I overestimate the response of the marginal buncher when I do not account for extensive margin responses

[41] Under the assumption that ability, or potential earnings being the same for the two groups in the estimation sample, non-treated recipients at the same point in the CDF as treated recipients should resemble the potential earnings of treated with earnings below the kink. To construct the empirical ability distribution, i.e. the earnings distribution if there were no (implicit) taxes on earnings, I assume that ability of treated is $n = F_0^{-1}(z)$ when $F_1 = F_0$ for treated with earnings $z \leq z^*$, where $F_j(z)$ is the cumulative distribution for treatment status $j = 0, 1$. For treated with earnings $z^* > z$, I use the estimated elasticity $e = .177$ to calculate the earnings response which yields ability $n = z(\frac{1-t_0}{1-t_1})^e$.

as the CDF for the treated group has shifted to the left when some individuals above the kink have stopped working.

Table 5: Simulation Exercise and Adjustment for Extensive Margin Responses

	Simulation	Empirical
Without extensive margin responses	.177 (0%)	
With extensive margin responses		
unadjusted	.303 (71%)	.315
adjusted	.173 (-2%)	.177

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Notes: The table shows intensive margin elasticity estimates with and without adjustment for extensive margin responses for the simulated and empirical earnings distributions. For the simulated earnings distributions (see text for details), I assume an elasticity of $e = .177$. In the first scenario, I assume no extensive margin responses. In the second scenario, I calibrate a fixed cost of labor force participation that resembles the estimated empirical extensive margin response. For the unadjusted estimates, I calculate the intensive margin elasticity using observations (individuals) with positive earnings only. For estimates adjusted for extensive margin responses, I add observations (individuals) to the right of the kink until the fraction of observations (individuals) with positive earnings is the same between the two samples. In parentheses, I report the bias in the estimated elasticity relative to the theoretical elasticity.

7 Elasticities using Bunching Methods

In most studies that use bunching at kinks to identify a behavioral response, the counterfactual distribution, i.e. what the distribution would have looked like without the kink, is unobserved. As pointed out by Blomquist *et al.* (2019), the amount of bunching at the kink is not informative about responses unless one is willing to impose restrictions on the counterfactual distribution. The common way to deal with this issue in the literature is to use the observed density to estimate the counterfactual density using a flexible polynomial. As a result, identification of the behavioral response depends on the assumed shape of the counterfactual distribution. However, without information on the true counterfactual density, it is not clear whether the polynomial approach provides a valid estimate of the counterfactual distribution, and therefore whether it provides a valid estimate of the behavioral responses.

To shed light on this matter, I re-estimate the earnings elasticity for the sample of recipients with the kink at $4,937 using bunching methods common in the literature. My first approach follows Chetty *et al.* (2011) and fits a flexible polynomial to the observed distribution to estimate the amount of bunching around the kink. Specifically, I group individuals into earnings bins of $400 and estimate a regression of the following form:

$$c_j = \sum_{i=0}^{p} \beta_i (z_j)^i + \sum_{k=z^L}^{z^U} \gamma_k \mathbf{1}(z_j = k) + \varepsilon_j \tag{7}$$

where c_j is the number of individuals in bin j, z_j is the earning level of bin j, z^L and z^U is the lower and upper limit of the excluded range around the kink and p is the order of the polynomial. The counterfactual density is obtained as the predicted values from Equation 7 omitting the contribution of the dummies in the excluded range, i.e. $\hat{c}_j = \sum_{i=0}^{p} \hat{\beta}_i (z_j)^i$. This density is then adjusted so that the estimated missing mass above the kink is equal to the estimated bunching mass around the kink.[42] The estimated amount of "bunching", or excess mass around the kink is then determined by the sum of the predicted values of the dummies, i.e. $\sum_{k=z^L}^{z^U} \hat{\gamma}_k \mathbf{1}(z_j = k)$. This estimate is then normalized by the estimated density

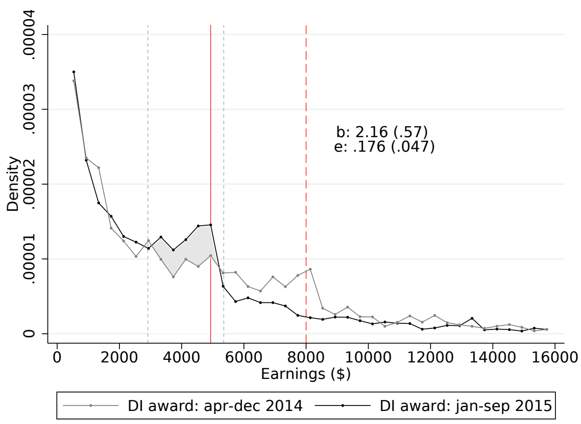
[42] This is achieved by upward shifts in the counterfactual distribution to the right of the kink, which is done in increments until the counterfactual satisfies the integration constraint.

at the kink. Multiplying this estimate with the binwidth obtains an estimate of the earnings response of the marginal buncher with the same interpretation as in my main estimation approach. Plugging this into Equation 4 yields the estimated earnings elasticity using the polynomial approach. As a comparison to the polynomial approach, I estimate the amount of "bunching" and the earnings elasticity using the same framework, but instead using the sample of recipients awarded DI before the cut-off as the counterfactual density. This should give an indication of how well the polynomial approach works in this context.

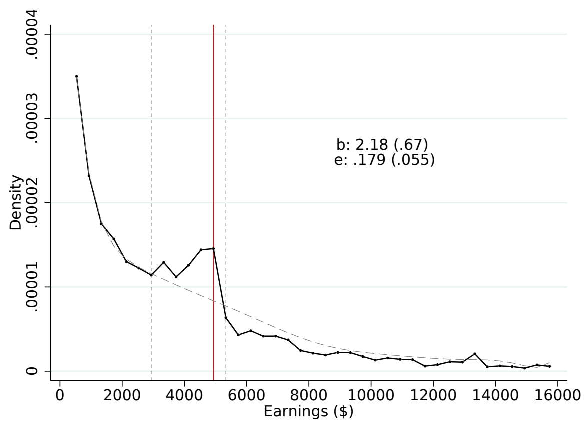
Figure 6 provides a graphical representation of the two approaches that I consider. In Panel (a), the gray area indicates the estimated amount of "bunching" using the non-parametric approach where recipients with DI award before the cut-off serve as the counterfactual density for recipients awarded DI after the cut-off. The vertical gray dashed lines indicate the lower and upper limits of the excluded region and are determined by visual inspection. The estimated amount of "bunching" (b) amounts to 2.16 and implies that the excess mass around the kink indicated by the gray area amounts to 2.16 of the counterfactual density at the kink. The estimated elasticity ($e = .176$) is almost indistinguishable from the estimated elasticity in my main empirical approach using the cumulative distributions of the two groups ($e = .177$). Panel (b) shows the estimated "bunching" and elasticity using the polynomial approach where I use the same excluded region as in the non-parametric approach. The dashed line indicates the estimated counterfactual density using a 10th degree polynomial fitted to the empirical distribution. The estimated "bunching" and elasticity ($e = .179$) are remarkably similar to the non-parametric approach suggesting that the polynomial approach provides a valid estimate of earnings responses in this context.

Figure 6: Graphical Representation of Bunching Estimates

(a) Non-parametric approach



(b) Polynomial approach



Notes: Panel (a) illustrates the bunching estimate using the non-parametric approach, where bunching is estimated using the earnings distribution of recipients awarded DI between April 1st and December 31st 2014 (gray line) as a counterfactual density for recipients awarded DI between January 1st and September 30th 2015 (black line), both distributions using \$400 bins. The vertical dashed lines indicate the bunching region where the estimated bunching is illustrated by the gray area. The red dashed line and the red solid line indicate the kink point in the budget constraint for each group. Recipients awarded DI in 2014 are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I = 1)$ denotes the probability of being awarded DI in 2015 and $P(I_i = 1|x_i)$ is estimated with a logit model using the covariates in Table A.2 (including uncapped DI benefits) as control variables. Both groups are weighted by a triangular kernel weight. Panel (b) illustrates the bunching estimate using the polynomial approach, where a 10th degree polynomial is fitted to the empirical distribution of recipients awarded DI between January 1st and September 30th 2015, illustrated by the gray dashed line. Standard errors are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications. Earnings are measured in 2016 dollars (NOK/USD = 7.5).

Table 6 reports estimates of the elasticity (e), bunching (b) and the earnings response of the marginal buncher ($\triangle z$) for each approach. Note that the polynomial approach is unaffected by the weighting approach as it only uses data on recipients to the right of the cut-off. In addition to the approaches discussed in this section, I estimate the earnings elasticity following Saez (2010) which relies on a linear approximation of the counterfactual density around the kink.[43] While this elasticity estimate is slightly higher than in the main empirical approach, it is qualitatively similar. For the polynomial approach, the estimated responses are based on a 10th degree polynomial fitted to the empirical distribution. As in most bunching applications, it is not clear how to decide the order of the polynomial in this setting. I therefore perform a robustness analysis and estimate responses using alternative orders of the polynomial fitted to the empirical distribution. Appendix Table A.5 reports estimates of "bunching" and the earnings elasticity using polynomials of order 8 - 12, and Appendix Figure A.5 provides a graphical representation.[44] The estimated elasticity range from .150 to .194 suggesting that the estimates are fairly robust to alternative specifications of the counterfactual density.

Table 6: Earnings Elasticity Estimates from Bunching Methods

	Elasticity (e)		Bunching (b)		Earnings response ($) Marginal buncher ($\Delta z$)		Observations <individuals>
CDF method	.177***	.198***	2.36**	2.67***	944**	1,067***	59,753
	(.066)	(.059)	(.98)	(.89)	(392)	(357)	<30,317>
Bunching methods:							
Counterfactual:	.176***	.184***	2.16***	2.26***	863***	902***	59,753
Awarded DI in 2014	(.047)	(.045)	(.58)	(.55)	(230)	(218)	<30,317>
Counterfactual:	.179***	.179***	2.18***	2.18***	873***	873***	26,950
Fitted polynomial	(.055)	(.055)	(.67)	(.67)	(267)	(267)	<13,697>
Saez method	.190***	.190***	2.32***	2.32***	928***	928***	26,950
	(.051)	(.051)	(.63)	(.63)	(250)	(250)	<13,697>
Weighted	Yes	No	Yes	No	Yes	No	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Notes: The table reports intensive margin estimates of the elasticity, bunching and the earnings response of the marginal buncher defined as the earnings response at the first kink for four different methods (see text for details). The sample consists of totally DI recipients awarded DI between 1st of April 2014 and 30th of September 2015. For the weighted estimates, recipients awarded DI in 2014 are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of being awarded DI in 2015 and $P(I_i=1|x_i)$ is estimated with a logit model using the covariates in Table A.2 (including uncapped DI benefits) as control variables. Both groups are weighted by a triangular kernel weight in all estimations. For the specification using a fitted polynomial, I use a 10th degree polynomial fitted to the empirical density using the sample of recipients awarded DI in 2015. Standard errors (in parentheses) are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications. Earnings are measured in 2016 dollars (NOK/USD = 7.5).

A caveat with this exercise is that even though my analysis suggests that the polynomial approach works well in this context, the same may not be true in other settings. In general, the performance of the polynomial approach will depend on choices made by the researcher, and in particular the order of the polynomial and the upper and lower limit of the excluded region. Moreover, the approach will

[43] The elasticity is derived from Equation (5) in Saez, 2010 which can be solved explicitly for the elasticity e: $B = z^*[T-1]\frac{h(z^*)_- + h(z^*)_+/T}{2}$ where $T = (\frac{1-t_0}{1-t_1})^e$. B denotes the estimated fraction of "bunching" in the population, z^* is the kink point and $h(z^*)_-$ and $h(z^*)_+$ denotes the estimated densities just below and just above the kink, respectively.

[44] Based on visual inspection, orders lower than 8 clearly underfits the empirical distribution in my setting.

depend on the size of the kink and the size of the earnings response. If bunching is less sharp, deciding the excluded region is often not clear. Additionally, one often relies on using observations further away from the kink to estimate the counterfactual density. The predicted density then may serve as a poor counterfactual density around the kink.

8 Fiscal Effects and Policy Implications

In this section, I assess fiscal effects of different benefit offset policies for the government and program's beneficiaries, and the associated policy implications. While a more lenient policy may improve welfare of recipients and increase tax revenues, it might increase expenditures on DI benefits. It could also increase program inflow as the program becomes more desirable for potential applicants. Therefore, it is not clear how such policies should be designed and how different policies affect total program costs.

To shed light on these matters, I examine implications of two alternative policies on recipients' disposable income, DI benefits paid, income taxes and net public expenditures. The alternative policies are compared to the baseline policy with the kink at \$4,937. The first policy I consider is relaxing the exemption threshold to \$8,000 which is the temporary policy for recipients awarded DI in 2014 or earlier. To calculate effects of this policy, I estimate Equation 5 for each outcome that I consider for the main estimation sample. The second policy I consider is abolishing the phase-out of DI benefits entirely. Under this policy, recipients would keep full DI benefits regardless of how much they earn. In both scenarios, recipients would still have to pay regular income taxes. To calculate effects of this policy, I decompose responses into intensive and extensive margin responses. For the intensive margin response, I estimate responses in the same way as in the main empirical approach for recipients with earnings below the kink.[45] For recipients with earnings above the kink, I calculate responses using the estimated intensive margin elasticity ($e = .177$).[46] To calculate the extensive margin response, I use the estimated elasticity of labor force nonparticipation from Section 6 and calculate the number of recipients who would start working under the alternative policy.[47] I assume that the earnings distribution of additional working recipients is the same as the observed earnings distribution above the kink. Lastly, I calculate the changes in recipients' disposable income, taxes and government expenditures based on the earnings responses.

The results from the two alternative policy changes are reported in Table 7. The two first columns show that relaxing the kink from \$4,937 to \$8,000 significantly increases recipients' disposable income with \$83 on average for the weighted approach. This effect is mainly driven by working recipients who increase labor supply, while a few recipients start working under the more lenient policy. Most recipients do not experience increased disposable income as they do not work under either policy. While estimated government expenditures on DI benefits increase with \$15 on average per recipient, this effect is offset by an increase in income taxes by \$42. Because of this, estimated net government expenditures decrease with \$27 per recipient. However, this effect is too imprecisely estimated to draw firm conclusions. Column 3 and 4 show that abolishing the phase-out of DI benefits entirely increases disposable income

[45] The earnings response can be expressed as $F_0(z + \triangle z) = F_1(z)$ where $\triangle z$ is the earnings response, F_0 is the CDF of recipients with DI award before the cut-off, and F_0 the CDF of recipients awarded DI after the cut-off.

[46] The earnings response can be expressed as $\triangle z = z(\frac{1-t_0}{1-t_1})^e - z$ where $\triangle z$ is the earnings response, z is current earnings, t_0 is the marginal tax rate below the kink and t_1 is the marginal tax rate above the kink.

[47] More specifically, the change in labor force participation is calculated as $\triangle LFP = -\varepsilon \frac{\triangle PTR}{PTR}(1 - LFP)$ where $\triangle PTR$ denotes the policy-induced reduction in participation tax rate.

by \$183 which is more than twice as much as the first alternative policy. DI benefits paid and income taxes also increase more. Although net government expenditures are estimated to be \$22 lower for each recipient than under the current policy, they are slightly higher than under the first alternative policy. Again, I lack precision to draw firm conclusions.

Table 7: Annual Fiscal Effects of Increased Incentives to Work

Outcome:	Relax kink		Abolish kink	
Disposable income (\$)	83***	100***	183***	191***
	(17)	(17)	(26)	(24)
DI benefits (\$)	15	1	50	51
	(11)	(11)	(122)	(109)
Payroll taxes (\$)	42***	52***	72***	76***
	(12)	(12)	(15)	(14)
Net expenses (\$)	-27	-51**	-22	-26
	(22)	(24)	(22)	(22)
Implied elasticity of induced entry	.07	.09	.01	.01
Weighted	Yes	No	Yes	No
Individuals	30,317		30,317	
Observations	59,753		59,753	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Notes: The table reports estimates of alternative benefit phase-out policies on annual disposable income, DI benefits, payroll taxes and net public expenditures. The first alternative policy considers relaxing the annual kink from \$4,937 to \$8,000. Column 1 and 2 report estimates using a regression where outcome variables are regressed on a dummy which is equal to 1 if recipients are awarded DI in 2014. Columns 3 and 4 report estimates of abolishing the DI phase-put policy entirely (see text for details). In all estimations, I use the full sample of totally disabled recipients awarded DI between 1st of April 2014 and 31st of September 2015 and a triangular kernel. For the weighted estimates, recipients awarded DI in 2014 are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of being awarded DI in 2015 and $P(I_i=1|x_i)$ is estimated with a logit model using the covariates in Table A.2 (including uncapped DI benefits) as control variables. Standard errors (in parentheses) are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications. All variables are measured in 2016 dollars (NOK/USD = 7.5).

It is important to keep in mind that the estimated effects are specific to my estimation sample of recipients with only 1-4 years on DI receipt. Therefore, it is possible that the alternative policies would have different implications for the full sample of DI recipients. As shown in Table 1, the full sample of recipients differ in some dimensions compared to the estimation sample. In particular, they are slightly older, have spent more years on DI and have about 50% higher earnings on average. It is therefore likely that the alternative policies would have larger impacts for the full sample of recipients. Additionally, it is possible that the long-run earnings responses, which my fiscal calculations are based upon, are higher than the observed responses if responses are attenuated by e.g. lumpy hours or imperfect information about the benefit phase-out. In that case, my estimates represent a lower bound of the true effects.

While my exercise shows that the alternative policies might decrease program costs for current recipients, these estimates ignore the possibility that a more generous DI program could induce more program entry.[48] To shed light on this matter, I calculate how elastic program inflow would have to be in order to not increase program costs. Specifically, I calculate the elasticity of induced entry as in Kostøl

[48] A more generous DI program could also lead to fewer program exits by current beneficiaries in the long run. However, this effect is likely to be small because the exit rate from DI is already very low.

& Mogstad (2014), defined as the percentage increase in the number of DI recipients relative to the percentage increase in disposable income as a DI recipient.[49] Table 7 shows that relaxing the kink yields an induced entry elasticity of about 0.07, while abolishing the disincentives to work entirely yields a very low elasticity of about 0.01. These calculations suggest that both programs are likely to increase program costs.[50]

9 Conclusion

In this paper, I have examined recipients' labor supply responses to the financial incentives induced by the benefit phase-out. Using earnings distributions in a local experiment that assigned recipients to different benefit offset policies, I provide transparent and credible identification of labor supply responses of DI beneficiaries. I find evidence of large behavioral responses around the exemption threshold suggesting that working recipients would have earned considerably more if benefits were not phased out above the threshold. I also find that recipients subject to the higher exemption threshold are more likely to participate in the labor force. My framework is also useful for understanding responses to kinked budget sets. My findings suggest that common estimation strategies in the literature that identify behavioral responses using bunching at kinks in the budget set performed well in this context.

As my study investigates recipients of the Norwegian DI program, one needs to exercise caution in applying these findings to other countries. In particular, the exemption threshold in Norway is lower than in most other countries. This difference is important as my main estimation strategy exploits recipients who locate around the threshold. Furthermore, I advise readers to exercise the usual caution in interpreting findings from a local experiment. In this context, the study considers recipients who have entered the DI program fairly recently and have a lower earnings capacity compared to recipients with longer spells on the program. Therefore, it is likely that responses are larger for the full population of DI recipients.

The estimated labor supply responses are particularly useful for guiding policymakers in how different benefit offset policies will affect recipients' disposable income and program costs. My findings indicate that relaxing the exemption threshold increases disposable income and reduces costs for current recipients of the program. A caveat with this study is that it is not informative about the level of increased program inflow when recipients are allowed to keep a larger share of their benefits as they earn more. I do, however, calculate the size of induced entry that has to be generated by more generous benefit offset policies in order to increase program costs. Based on findings in other studies, I conclude that more generous policies will likely increase public expenditures.

[49]The elasticity is defined as $\varepsilon_{entry} = \frac{E(\triangle NE)/E(B|Award=1)}{P(Award=1)\cdot E(\triangle I|Award=1)}$ where $\triangle NE$ is the change in net government expenditures and $\triangle I$ is the change in disposable income between the current policy and the alternative policy.

B is DI benefits. I assume that new entries have the same earnings distribution and DI benefits as recipients with earnings above the kink as the alternative programs gives no further incentives for entries who would earn below the kink. Furthermore, I assume a probability of award of 0.85 which is roughly the award rate in the Norwegian DI program.

[50]While the literature on induced entry of DI recipients is somewhat inconclusive, Gruber (2000) reports induced entry elasticities in the range of 0.28-0.36 in Canada. Mullen & Staubli (2016) and Hoynes & Moffitt (1999) report an elasticity of about 1.2 in Austria and the US, respectively. In contrast, Campolieti & Riddell (2012) and Castello (2017) do not find any evidence of induced entry in Canada and Spain, respectively.

References

ATKINSON, A., RAINWATER, L., & SMEEDING, T.M. 1995. Income Distributions in OECD countries: Evidence from the Luxembourg Income Study. *OECD Publications and Information Center*.

BASTANI, SPENCER, & SELIN, HÅKAN. 2014. Bunching and non-bunching at kink points of the Swedish tax schedule. *Journal of Public Economics*, **109**(Jan.), 36–49.

BEST, MICHAEL CARLOS, & KLEVEN, HENRIK JACOBSEN. 2017. Housing market responses to transaction taxes: Evidence from notches and stimulus in the UK. *The Review of Economic Studies*, **85**(1), 157–193.

BLOMQUIST, SÖREN, NEWEY, WHITNEY, KUMAR, ANIL, & LIANG, CHE-YUAN. 2019. On Bunching and Identification of the Taxable Income Elasticity. *NBER Working Paper 24136*.

BORGHANS, LEX, GIELEN, ANNE C., & LUTTMER, ERZO F. P. 2014. Social Support Substitution and the Earnings Rebound: Evidence from a Regression Discontinuity in a disability Insurance Reform. *American Economic Journal: Economic Policy*, **6**(4), 34–70.

BOUND, J. 1989. The Health and Earnings of Rejected Disability Insurance Applicants. *American Economic Review*, **79**(3), 482–503.

BUTLER, MONIKA, LECHNER, MICHAEL, DEUCHERT, EVA, STAUBLI, STEFAN, & THIEMANN, PETRA. 2015. Financial Work Incentives for Disability Benefit Recipients: Lessons from a Randomized Experiment. *IZA Journal of Labor Policy*, **4**(18), 1–18.

CALONICO, SEBASTIAN, CATTANEO, MATIAS D., & TITIUNIK, ROCIO. 2014. Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica*, **82**(6), 2295–2326.

CAMPOLIETI, MICHELE, & RIDDELL, CHRIS. 2012. Disability Policy and the labor market: Evidence from a natural experiment in Canada, 1998-2006. *Journal of Public Economics*, **96**, 306–316.

CASTELLO, JUDIT VALL. 2017. What happens to the employment of disabled individuals when all financial incentives to work are abolished? *Health Economics*, **26**(S2), 158–174.

CHEN, S., & VAN DER KLAAUW, W. 2008. The work disincentive effects of the disability insurance program in the 1990s. *Journal of Econometrics*, **142**(2), 757–784.

CHETTY, RAJ, FRIEDMAN, JOHN, OLSEN, TORE, & PISTAFERRI, LUIGI. 2011. Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records. *The Quarterly Journal of Economics*, **126**(2), 749–804.

DAHL, GORDON B., KOSTØL, ANDREAS RAVNDAL, & MOGSTAD, MAGNE. 2014. Family Welfare Cultures. *The Quarterly Journal of Economics*, **129**(4), 1711–1752.

DEUCHERT, EVA, & EUGSTER, BEATRIX. 2019. Income and substitution effects of a disability insurance reform. *Journal of Public Economics*, **170**(Feb.), 1–14.

EINAV, LIRIAN, FINKELSTEIN, AMY, & SCHRIMPF, PAUL. 2017. Bunching at the kink: Implications for spending responses to health insurance contracts. *Journal of Public Economics 146 (2017) 27-40*.

FRANDSEN, BRIGHAM R. 2017. *Party Bias in Union Representation Elections: Testing for Manipulation in the Regression Discontinuity Design when the Running Variable is Discrete*. Advances in Econometrics, vol. 38. Emerald Publishing Ltd. Chap. Regression Discontinuity Designs, pages 281–315.

FRENCH, ERIC, & SONG, JAE. 2014. The effect of disability insurance receipt on labor supply. *American Economic Journal: Economic Policy*, **6**(2), 291–337.

GELBER, ALEXANDER, MOORE, TIMOTHY J., & STRAND, ALEXANDER. 2017a. The Effect of Disability Insurance Payments on Beneficiaries Earnings. *American Economic Journal: Economic Policy*, **9**(3), 229–261.

GELBER, ALEXANDER M., JONES, DAMON, SACKS, DANIEL W., & SONG, JAE. 2017b. Using Non-Linear Budget Sets to Estimate Extensive Margin Responses: Method and Evidence from the Social Security Earnings Test. *NBER Working Papers 23362, National Bureau of Economic Research, Inc*.

GELBER, ALEXANDER M., JONES, DAMON, SACKS, DANIEL W., & SONG, JAE. 2020a. The Employment Effects of the Social Security Earnings Test. *Working Papers 2020-05, Becker Friedman Institute for Research in Economics*.

GELBER, ALEXANDER M., JONES, DAMON, & SACKS, DANIEL W. 2020b. Estimating Adjustment Frictions Using Nonlinear Budget Sets: Method and Evidence from the Earnings Test. *American Economic Journal: Applied Economics*, **12**(1), 1–31.

GRUBER, JONATHAN. 2000. Disability Insurance Benefits and Labor Supply. *Journal of Political Economy*, **108**(6), 1162–83.

HARASZTOSI, PÉTER, & LINDNER, ATTILA. 2019. Who Pays for the minimum Wage? *American Economic Review*, **109**(8), 2693–2727.

HOYNES, HILARY WILLIAMSON, & MOFFITT, ROBERT. 1999. Tax Rates and Work Incentives in the Social Security Disability Insurance Program: Current Law and Alternative Reforms. *National Tax Journal*, **52**(4), 623–54.

KLEVEN, HENRIK. 2016. Bunching. *Annual Review of Economics, Annual Reviews*, **8**(1), 435–464.

KLINE, PATRICK. 2011. Blinder-Oaxaca as a reweighting estimator. *American Economic Review: Papers and Proceedings*, **101**(3), 532–537.

KOPCZUK, WOJCIECH, & MUNROE, DAVID. 2015. Mansion Tax: The Effect of Transfer Taxes on the Residential Real Estate Market. *American Economic Journal: Economic Policy*, **7**(2), 214–257.

KOSTØL, ANDREAS RAVNDAL, & MOGSTAD, MAGNE. 2014. How Financial Incentives Induce Disability Insurance Recipients to Return to Work. *American Economic Review*, **104**(2), 624–55.

LE BARBANCHON, THOMAS. 2016. The effect of the potential duration of unemployment benefits on unemployment exits to work and match quality in France. *Labour Economics*, **42**(C), 16–29.

LEE, DAVID S., & LEMIEUX, THOMAS. 2010. Regression Discontinuity Designs in Economics. *Journal of Economic Literature*, **48**(June), 281–355.

LIEBMAN, JEFFREY B. 2002. The Optimal Design of the Earned Income Tax Credit. *Making Work Pay: The Earned Income Tax Credit and Its Impact on American Families*, **edited by Bruce D. Meyer and Douglas Holtz-Eakin**, 196–234. New York: Russel Sage Foundation.

MAESTAS, NICOLE, MULLEN, KATHLEEN J., & STRAND, ALEXANDER. 2013. Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt. *American Economic Review*, **103**(5), 1797–1829.

MANOLI, DAY, & WEBER, ANDREA. 2016. Nonparametric Evidence on the Effects of Financial Incentives on Retirement Decisions. *American Economic Journal: Economic Policy*, **8**(4), 160–182.

MARIE, OLIVIER, & CASTELLO, JUDIT VALL. 2012. Measuring the (income) effect of disability insurance generosity on labour market participation. *Journal of Public Economics*, **96**(1-2), 198–210.

MOORE, TIMOTHY J. 2015. The Employment Effects of Terminating Disability Benefits. *Journal of Public Economics*, **124**, 30–43.

MULLEN, KATHLEEN J., & STAUBLI, STEFAN. 2016. Disability Benefit Generosity and Labor Force Withdrawal. *Journal of Public Economics*, 49–63.

NAV. 2018. Nav-ytelsene frem mot 2060 - En forenklet analyse av store penger. *NAV-rapport nr. 1-2018*.

OECD. 2010. *Sickness, Disability and Work: Breaking the Barriers. A synthesis of findings across OECD countries*. OECD Publishing, Paris.

PAETZOLD, JOERG. 2019. How do taxpayers respond to a large kink? Evidence on earnings and deduction behavior from Austria. *International Tax and Public Finance*, **26**(1), 167–197.

RUH, PHILIPPE, & STAUBLI, STEFAN. 2019. Financial Incentives and Earnings of Disability Insurance Recipients: Evidence from a Notch Design. *American Economic Journal: Economic Policy*, **11**(2), 269–300.

SAEZ, EMMANUEL. 2010. Do Taxpayers Bunch at Kink Points? *American Economic Journal: Economic Policy*, **2**(3), 180–212.

SCHIMMEL, JODY, STAPELTON, DAVID C., & SONG, JAE G. 2011. How Common is 'Parking' among Social Security Disability Insurance Beneficiaries? Evidence from the 1999 Change in the Earnings Level of Substantial Gainful Activity. *Social Security Bulletin*, **71**(4), 77–92.

SINGLETON, PERRY. 2012. Earnings of rejected applicants to the Social Security Disability Insurance program. *Economics Letters*, **116**(2), 147–150.

TRAXLER, CHRISTIAN, WESTERMAIER, FRANZ G., & WOHLSCHLEGEL. 2018. Bunching on the Autobahn? Speeding responses to a 'notched' penalty scheme. *Journal of Public Economics*, **157**(Jan.), 78–94.

WEATHERS, ROBERT R., & HEMMETER, JEFFREY. 2011. The Impact of Changing Financial Work Incentives on the Earnings of Social Security Disability Insurance (SSDI) Beneficiaries. *Journal of Policy Analysis and Management*, **30**(4), 708–728.

ZARESANI, AREZOU. 2020. Adjustment Costs and Incentives to Work: Evidence from a Disability Insurance Program. *Journal of Public Economics*, **188**(104223).

Appendix: Additional Tables and Figures

Table A.1: Balancing Tests of Pre-determined Covariates

Dependent variable:	Full sample (2 months around cut-off)			Alternative sample: Not from prior TDI programs (9 months around cut-off)		
	difference	std. error	p-value	difference	std. error	p-value
Number of recipients	-86	(307)	.778	-155	(122)	.206
Characteristics:						
Age at DI award	-.67*	(.37)	.072	-.27	(.25)	.282
Fraction females	.002	(.014)	.902	-.004	(.009)	.634
Years of schooling	-.18***	(.06)	.005	-.06	(.04)	.108
Pre-disability earnings ($)	-225	(468)	.631	89	(301)	.766
Projected DI benefits ($)	-169	(234)	.471	-5	(147)	.971
Years since onset date	-.12	(.12)	.320	-.08	(.07)	.232
Fraction cohabitants	-.005	(.014)	.745	-.012	(.009)	.168
Number of children	.01	(.04)	.755	.008	(.025)	.759
Fraction from TDI program	-.013	(.009)	.125	-.016**	(.007)	.013
Fraction from prior TDI programs	-.015	(.013)	.253	-	-	-
Joint test			.176			.194
Observations		5,976			16,762	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Notes: The table reports results of a regression using a triangular kernel where each variable is regressed on a dummy which is equal to 1 if recipients are awarded DI in 2014. The full sample consist of totally disabled recipients awarded DI between 1st of November 2014 and 28th of February 2015. The alternative sample consists of recipients awarded DI between 1st of April 2014 to 30th of September 2015 and excludes recipients who received temporary DI benefits before a reform in the temporary DI program in March 2010. Projected DI benefits are defined as the level of DI benefits if recipients were awarded DI after 1st of January 2015. Years of schooling, cohabitant status and number of children are measured in 2015. All other covariates are fixed over time. Standard errors (in parentheses) are clustered at the individual level and are robust to heteroskedasticity. Monetary variables are measured in 2016 dollars (NOK/USD = 7.5).

Table A.2: Balancing tests of Pre-determined Covariates: Weighted Estimates

Dependent variable:	mean: DI award apr-dec 2014		mean: DI award jan-sep 2015	difference (std. error)				p-value	
Net uncapped DI benefits ($)	27,647	28,405	28,741	-1,095***	(68)	-337***	(69)	<.001	<.001
Characteristics:									
Age at DI award	46.68	47.34	47.02	-.34**	(.17)	.32*	(.19)	.048	.085
Fraction females	.550	.531	.531	.019***	(.007)	.000	(.007)	.004	.962
Years of schooling	10.93	11.01	11.01	-.08***	(.03)	.00	(.03)	.008	.998
Pre-disability earnings ($)	59,557	60,304	60,224	-668***	(214)	80	(254)	.002	.753
Years since DI onset date	6.89	6.12	6.12	.77***	(.06)	.00	(.06)	<.001	.948
Fraction cohabitants	.504	.502	.507	-.003	(.007)	-.005	(.007)	.626	.457
Number of children	1.65	1.60	1.62	.03	(.02)	-.02	(.02)	.119	.314
Fraction from TDI program	.913	.887	.890	.022***	(.004)	-.003	(.005)	<.001	.436
Fraction from prior TDI programs	.498	.303	.305	.193***	(.006)	-.002	(.006)	<.001	.709
Joint test (p-value)								<.001	.429
Weighted	No	Yes	No	No		Yes		No	Yes
Observations	16,620	16,620	13,697		30,317				

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Notes: The table reports weighted and unweighted means of recipients awarded DI before/after 1st of January 2015, the difference between the weighted and unweighted means and the corresponding p-values. The sample consist of totally disabled recipients awarded DI between 1st of April 2014 and 31st of September 2015. Years of schooling, cohabitant status and number of children are measured in 2015. All other covariates are constant over time. Means and the corresponding differences are weighted by a triangular kernel. For the weighted means and differences, recipients awarded DI in 2014 are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of being awarded DI in 2015 and $P(I_i=1|x_i)$ is estimated with a logit model using the covariates in Table (including uncapped DI benefits) as control variables. Standard errors (in parentheses) are clustered at the individual level and are robust to heteroskedasticity. Monetary variables are measured in 2016 dollars (NOK/USD = 7.5).

Table A.3: Subsample Analysis of Earnings Responses

	Elasticity (e)		Earnings response ($) Marginal buncher ($\Delta z$)		Earnings response ($) Average response		Observations <individuals>
Full sample	.177***	.198***	944**	1,067***	157	531*	59,753
(2016-2017)	(.066)	(.059)	(392)	(357)	(308)	(307)	<30,317>
Age							
18-49	.115*	.182***	606	999***	242	720*	27,029
	(.069)	(.064)	(392)	(383)	(407)	(422)	<13,960>
50-66	.254***	.295***	1,379***	1,634***	0	458	21,845
	(.091)	(.076)	(536)	(460)	(492)	(461)	<11,896>
Gender							
Male	.181**	.194***	946**	1,021**	153	740	26,839
	(.078)	(.074)	(446)	(431)	(488)	(527)	<13,600>
Female	.185*	.199**	1,022*	1,106**	232	335	32,914
	(.097)	(.084)	(586)	(514)	(387)	(345)	<16,717>
Education							
High	.155	.176**	804	921*	331	759	24,133
	(.098)	(.084)	(551)	(482)	(483)	(501)	<12,244>
Low	.179**	.218***	972**	1,207***	-70	348	35,620
	(.076)	(.072)	(459)	(448)	(413)	(411)	<18,073>
DI benefits							
High	.257***	.245***	1,466***	1,385***	362	624	35,165
	(.077)	(.067)	(486)	(422)	(421)	(398)	<17,912>
Low	.151	.158*	766	804	214	371	24,588
	(.096)	(.090)	(526)	(496)	(490)	(480)	<12,405>
Pre-DI earnings							
High	.189**	.184**	931*	904**	299	865*	26,429
	(.093)	(.077)	(496)	(416)	(492)	(520)	<13,482>
Low	.179**	.232***	1,034**	1,384***	-107	209	33,324
	(.080)	(.065)	(494)	(418)	(387)	(358)	<16,835>
Weighted	Yes	No	Yes	No	Yes	No	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Notes: The table presents intensive margin estimates of the elasticity, the earnings response of the marginal buncher defined as the earnings response at the first kink and the average response of recipients for the full sample and by each subgroup. The sample consist of totally DI recipients awarded DI between 1st of April 2014 and 30th of September 2015. For recipients awarded DI in 2015, I add recipients to the right of the kink until the fraction of recipients with positive earnings is the same as for recipients awarded DI in 2014 in order to adjust for extensive margin responses. For the weighted estimates, recipients awarded DI in 2014 are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of being awarded DI in 2015 and $P(I_i=1|x_i)$ is estimated with a logit model using the covariates in Table A.2 (including uncapped DI benefits) as control variables. Both groups are weighted by a triangular kernel weight in estimations. Low education is defined as not finishing high school or less. High education is defined as high school education or more. Low (uncapped) DI benefits is defined as receiving minimum benefit levels, and high DI benefits otherwise. Low (high) pre-DI earnings are defined as less than or equal to (larger than) the sample median. Standard errors (in parentheses) are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications. Earnings are measured in 2016 dollars (NOK/USD = 7.5)

Table A.4: Regression Discontinuity Estimates

Outcome:	Trends in running variable (DI award)						Observations
	None		Linear		Quadratic		<individuals>
Annual earnings ($)	117***	151***	113	117	108	112	59,753
	(39)	(39)	(85)	(86)	(85)	(85)	<30,317>
	[531]	[513]	[533]	[530]	[560]	[552]	
Controls	Yes	No	Yes	No	Yes	No	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Notes: The table reports results of regression discontinuity estimates using a triangular kernel for different specifications of the running variable: No trend (differences in means), a common linear trend and a common quadratic trend. The sample consist of totally disabled recipients awarded DI between 1st of April 2014 and 31st of September 2015. Controls include the variables in Table A.2, including (uncapped) DI benefits and the implicit tax rate on DI benefits (equal to the benefit replacement rate). Results reports the coefficient of the dummy which is equal to 1 if recipients are awarded DI in 2014. Standard errors (in parentheses) are clustered at the individual level and are robust to heteroskedasticity. Dependent means in brackets. Earnings are measured in 2016 dollars (NOK/USD = 7.5).

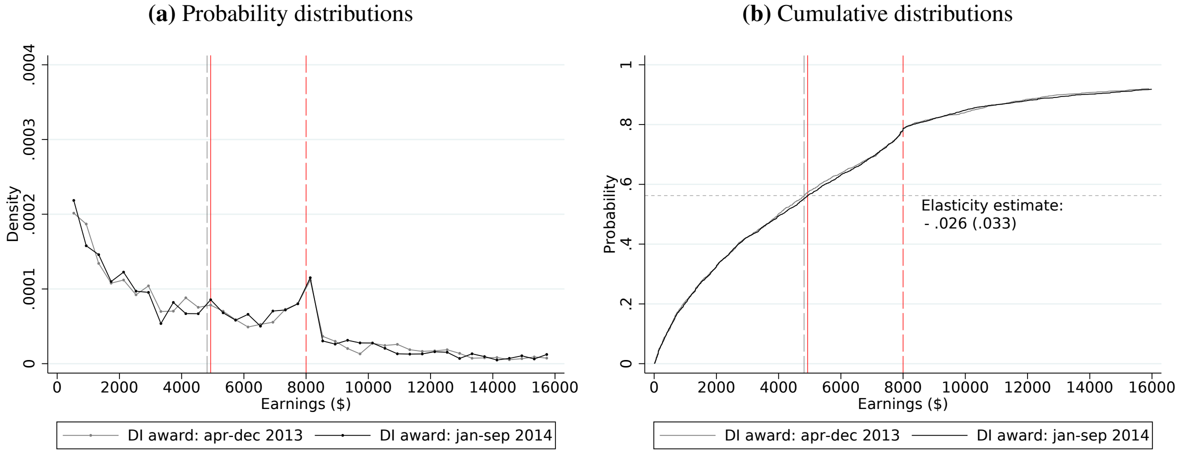
Table A.5: Parametric Bunching Estimates for Alternative Orders of Polynomial

Order of polynomial:	8	9	10	11	12	Observations <individuals>
Bunching (b)	1.83***	1.97***	2.18***	2.17***	2.37***	59,753
	(.51)	(.53)	(.67)	(.64)	(.78)	<30,317>
Elasticity (e)	.150***	.161***	.179***	.177***	.194***	
	(.042)	(.044)	(.055)	(.053)	(.064)	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

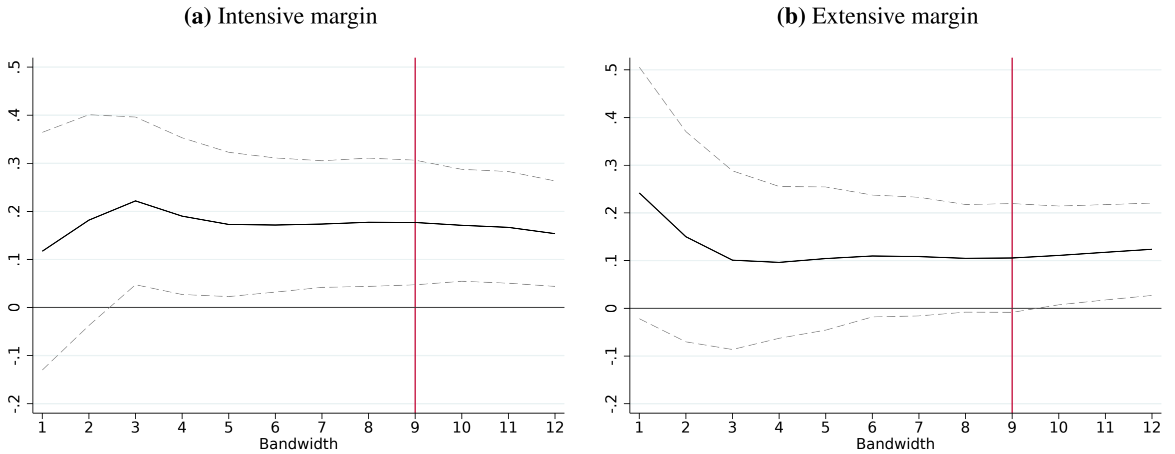
Notes: The table reports bunching and elasticity estimates from the parametric bunching method (see Section 7 for details) using alternative orders of polynomial fitted to the empirical distribution. In the baseline specification I use a 10th order polynomial. The sample consist of recipients awarded DI between January 1st and September 30th 2015 and weighted by a triangular kernel (assigning more weight to the individuals awarded DI early in the year). Standard errors are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications.

Figure A.1: Placebo Elasticity Estimates: Recipients Awarded DI 2013-2014



Notes: Panel (a) shows the (weighted) pooled earnings distributions for 2016 and 2017 in $400 bins for recipients with positive earnings awarded DI between April 1st and December 31st 2013 (gray line) and recipients awarded DI between January 1st and September 30th 2014 (black line). The red solid line indicates the kink point in the budget constraint for both groups. Both groups are weighted by a triangular kernel weight in estimations. For recipients awarded DI in 2014, I add recipients to the right of the kink until the fraction of recipients with positive earnings is the same as for recipients awarded DI in 2013 in order to adjust for extensive margin responses. Recipients awarded DI in 2013 are weighted by propensity score weights $w(x_i) = \frac{P(I_i=1|x_i)}{P(I=1)} \frac{1-P(I=1)}{1-P(I_i=1|x_i)}$ where $P(I=1)$ denotes the probability of being awarded DI in 2014 and $P(I_i = 1|x_i)$ is estimated with a logit model using the covariates in Table A.2 (including uncapped DI benefits) as control variables. Panel (b) shows the corresponding cumulative distributions. The horizontal dashed line indicates the CDF at the kink for recipients awarded DI in 2014. The vertical gray dashed line indicates earnings of the marginal buncher $z^* + \triangle z$. Standard errors are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications. Earnings are measured in 2016 dollars (NOK/USD = 7.5).

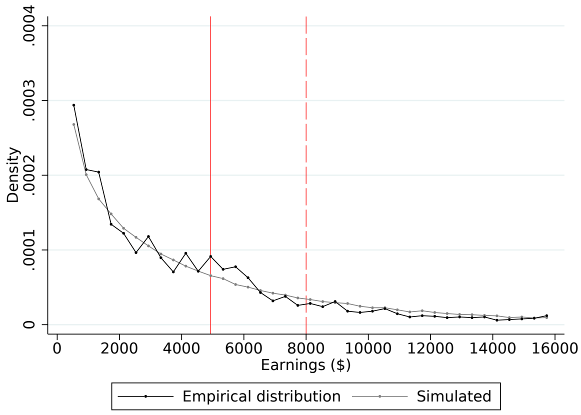
Figure A.2: Earnings Elasticity Estimates for Different Bandwidths



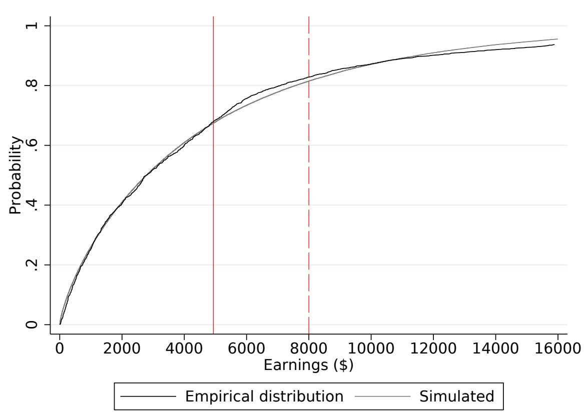
Notes: Panels (a) and (b) displays the weighted estimates for different bandwidth choices of the intensive margin elasticity (see Section 5.2 for details) and the extensive margin elasticity (see Section 6 for details), respectively. The black solid line indicates the point estimates, and the dashed lines indicate 95% confidence intervals. Standard errors are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications. The red vertical line indicates the baseline specification of 9 months bandwidth. The sample consists of totally disabled recipients awarded DI between 1st of January 2014 and 31st of December 2015.

Figure A.3: Empirical and Simulated Ability Distributions

(a) Probability distributions

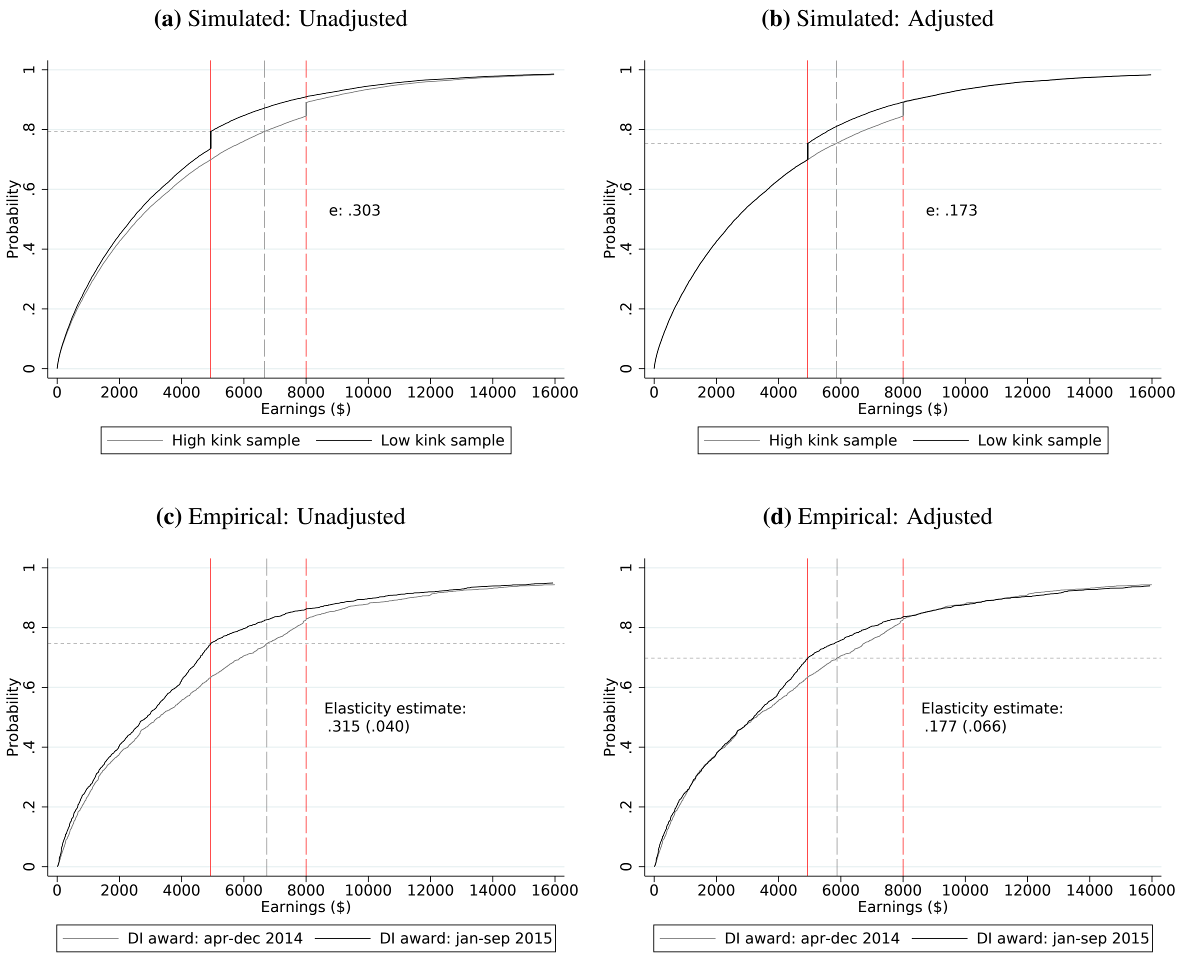


(b) Cumulative distributions



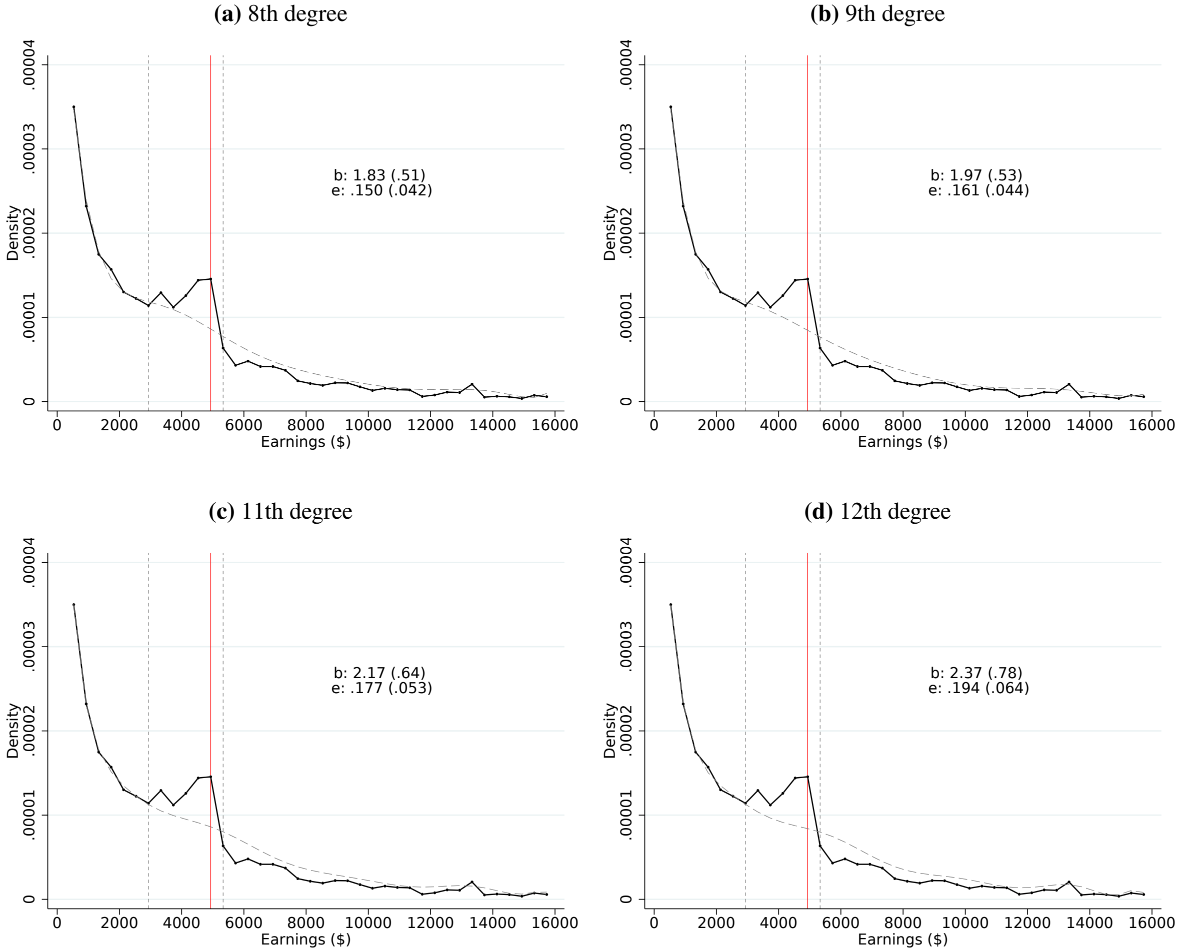
Notes: Panel (a) shows the empirical and simulated ability distributions (see Section 6 for details of how the empirical ability distribution is constructed), and Panel (b) shows the corresponding cumulative distributions. For the empirical distributions, the sample consists of recipients awarded DI between 1st of January and 30th of September 2015. The sample is weighted by a triangular kernel (putting more weight on recipients awarded DI earlier in the year). For the simulated distribution, I calibrate a gamma distribution that best resembles the empirical probability density distribution.

Figure A.4: Simulation-based Adjustment for Extensive Margin Responses



Notes: Panel (a) shows the cumulative distributions from the simulated earnings distributions without adjustment for extensive margin responses and the estimated intensive margin elasticity. The earnings distributions are constructed using the simulated ability distributions in Figure A.3, and are then split into two samples: One with a (low) kink at \$4,937 indicated by the red solid line, and the other with a (high) kink at \$8,000 indicated by the red dashed line. I then construct earnings responses assuming quasi-linear utility and an elasticity $e = .177$ and calibrate a fixed cost of labor force participation that resembles the estimated empirical extensive margin response. Only individuals with earnings larger than the fixed cost of labor force participation are used in the estimation. The horizontal dashed line indicates the CDF at the (low) kink at \$4,937. The vertical gray dashed line indicates earnings of the marginal buncher $z^* + \triangle z$. Panel (b) follows the same procedure where I add observations to the right of the kink for the sample with kink at \$4,937 until the fraction of observations participating in the labor force is the same between the two samples in order to adjust for extensive margin responses. I assume that the additional observations follow the same distribution as the earnings distribution to the right of the kink. Panel (c) shows the corresponding (weighted) cumulative distributions and the estimated intensive margin elasticity for the empirical sample, where the sample consists of recipients with positive earnings only. Panel (d) shows the corresponding (weighted) cumulative distributions and the estimated intensive margin elasticity for the empirical sample, where I add recipients to the right of the kink until the fraction of individuals with positive earnings is the same between the two samples. Standard errors are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications.

Figure A.5: Parametric Bunching Estimates for Alternative Orders of Polynomial



Notes: The figures illustrate the parametric bunching approach using alternative specifications of the polynomial fitted to the empirical distribution which is grouped into $400 bins. The vertical dashed lines indicate the excluded region, and the red solid line indicate the kink point. The sample consist of recipients awarded DI between January 1st and September 30th 2015 and weighted by a triangular kernel (assigning more weight to the individuals awarded DI early in the year). Standard errors are calculated by a pairs cluster bootstrap which accounts for clustering at the individual level using 500 replications. Earnings are measured in 2016 dollars (NOK/USD = 7.5).

Chapter II

Labor Supply Responses to Learning the Tax and Benefit Schedule

Written jointly with Andreas R. Kostøl, submitted to American Economic Review (revise and resubmit). A revised version has been accepted.

Labor Supply Responses to Learning the Tax and Benefit Schedule*

Andreas R. Kostøl[†] Andreas S. Myhre [‡]

This version: November 25, 2020. Submitted to the American Economic Review (revise and resubmit). A revised version has been accepted.

Abstract: While optimization frictions have been shown to attenuate earnings responses to financial incentives, less is understood about the individual factors shaping the response. The main contribution of this paper is to separately quantify the role of learning the tax and benefit schedule versus other kinds of frictions. A unique combination of notches in the tax and benefit schedule and an information policy in a Norwegian welfare reform facilitate our study. The presence of notches allows us to measure overall frictions. Quasi-random assignment of a letter targeting misperceptions about the slope and locations of benefit phase-out regions allows us to pin down the role of information. Our analysis delivers two main findings. First, about 50% do not behave as predicted by standard labor supply models, and optimization frictions are particularly prevalent when financial incentives change. Without adjusting for these overall frictions, estimated elasticities would be attenuated by at least 70%. Second, the observed elasticity among those who receive the information letter is at least twice as large as among the non-informed, suggesting governments can partly offset the attenuation with information policy. Our calculations suggest misperceptions of the tax and benefit schedule account for two-thirds of the attenuation in earnings responses to financial work incentives. The findings have important implications for the effectiveness of tax and transfer policy.

Keywords: labor supply, information, optimization frictions, social security, disability insurance
JEL codes: H20, H31, H55, J22, J26

1 Introduction

A well-established fact from labor economics is that optimization frictions attenuate the earnings response to changes in financial incentives (e.g., Chetty, 2012 and Kleven & Waseem, 2013). Much less is known about whether the factors originate from the demand side or from supply-side constraints in the labor market.[1] In their review of the empirical evidence, for example, Saez *et al.* (2012) argue that "taxpayers may not be aware of the minute details of the tax code, and hence might not respond to very

*This project received financial support from the Norwegian Welfare Administration and the President of ASU's Strategic Initiative Fund. We would like to thank Esteban Aucejo, Richard Blundell, Erlend Eide Bo, Sigurd Galaasen, Francois Gerard, Jonas Hjort, Hans Hvide, Damon Jones, Henrik Kleven, Patrick Kline, Camille Landais, Edwin Leuven, Andrew McCallum, Jan Nimczik, Nathan Seegert, Dan Silverman, Ola Vestad, Nicholas Vreugdenhil and Andrea Weber and participants at several seminars and workshops for useful comments and suggestions. Matthew Merkle provided excellent research assistance.

[†]Department of Economics, Arizona State University; Statistics Norway; IZA. E-mail: andreas.kostol@asu.edu

[‡]The Norwegian Welfare Administration; Statistics Norway. E-mail: a.s.myhre@econ.uio.no

[1]A long literature discusses the implications of hours constraints (see e.g., Pencavel, 1986; Altonji & Paxson, 1988; Dickens & Lundberg, 1993; and Blundell & Macurdy, 1999), and more recently the role of imperfect information (see e.g., DellaVigna, 2009 and Saez, 2010). Existing empirical evidence is limited to the effects of information on take-up of the EITC (see e.g., Chetty & Saez, 2013, Chetty *et al.*, 2013 and Bhargava & Manoli, 2015) and whether an information brochure aiming to correct misperceptions about the Social Security earnings test could affect the labor supply of old-age retirees (Liebman & Luttmer, 2012). None of these studies are able to pin down the relative importance of information to the overall attenuation of the response to incentives.

localized changes in their marginal tax rate situation". Distinguishing between these factors matters for whether governments can shape behavioral responses with policy (see, e.g., Slemrod & Kopczuk, 2002) and for the design of optimal tax and transfer policies (e.g., Farhi & Gabaix, 2020). Yet, unpacking the "black box" of optimization frictions has proven difficult due to measurement and identification challenges. To make progress, researchers need large-scale data on labor supply under different tax regimes including measures of how incentives are perceived and other adjustment costs. On top of these measurement hurdles, separating a person's ability to learn complex tax incentives or negotiate with employers from the person's underlying labor market productivity is hard. Without addressing this selection a bias of unknown sign and size will hamper any conclusions drawn from the data.

The contribution of this paper is to quantify the relative importance of information about financial incentives versus other types of frictions in shaping earnings responses. Our study overcomes the measurement and identification challenges by drawing on key advantages from the Norwegian context. The first is notches in the disability insurance (DI) system, which allows us to measure the prevalence of overall optimization frictions from dominated regions where part-time employed DI recipients are better off by working fewer or more hours. The second is an information policy targeting recipients' perceptions about a new kink in the tax and benefit schedule. The policy was implemented in June 2015 and informed recipients about the location and the slope of the kink. However, the social security administration (SSA) decided that only individuals likely to locate above the kink would receive the letter. To implement the targeted intervention, the SSA used monthly earnings records from January to May 2015 to forecast annual earnings. Our research design uses forecast errors due to fluctuations in monthly payments and electronic reporting by employers generating quasi-random variation in information letters. We use this variation to see whether the earnings elasticity is shaped with information policy by comparing bunching behavior around the kink with additional information (i.e., the treated) to a baseline information case (i.e., the non-treated).

The informational treatment was contained in a letter detailing the location and the slope of a new kink in the annual tax and benefit schedule.[2] We view the treatment as changing perceptions of marginal incentives around the DI recipient's current earnings level. Survey evidence indicated that most recipients were aware of a change in their tax and benefit schedule but did not fully understand how the benefits would be phased out with their labor earnings. If the information treatment updates perceptions toward the actual schedule and informed recipients are primarily responsive along the intensive margin, we would expect increased bunching around the new annual kink. The unique combination of the information treatment and dominated regions allows us to identify the role of information about financial incentives and overall frictions in shaping earnings responses. To pin down the relative role of information vs. other kinds of frictions, we assume the structural elasticity is policy-invariant; that is, an agent's preferences over leisure and consumption do not depend on the strength of incentives. The role of information is then pinned down by comparing the elasticity change due to the information treatment in 2015 with the structural elasticity identified from the notch in 2014.

Our main empirical findings can be summarized with three broad conclusions. First, we find that about 50 percent do not behave as predicted by standard labor supply models, and that optimization frictions are particularly prevalent when financial incentives change. Without adjusting for these over-

[2]It also encouraged DI recipients to update annual earnings expectations on a web portal with access to an application that allowed a person to simulate disposable income for different gross earnings levels.

all frictions, estimated elasticities would be attenuated by at least 70 percent. Second, we show that governments can offset part of this attenuation in behavioral response with information policy. Among the non-treated, we find the observed elasticity falls from 0.2 to 0.06 after the tax and benefit schedule change, whereas the earnings elasticity equals 0.15 among those who received the information letter. These four elasticities imply about two-thirds of the increase in attenuation from 2014 to 2015 is due to misperceptions of the tax and benefit schedule, and the remainder is due to other adjustment frictions. Third, we provide several pieces of evidence suggesting our findings reflect real labor supply responses to information about financial incentives. We find that the employment rate from July to December falls by 17 percent for the treated relative to the control group, i.e., those not informed. Using detailed information on contracted hours, and fixed and variable pay, we provide further evidence suggesting that workers adjust by either renegotiating work contracts with their current employer or working fewer hours.

Recognizing that the information letters, in practice, do not fully correct for imperfect information, we view our approach as identifying a lower bound on the role of information in shaping earnings responses. We take several additional steps to assess our research design's validity. One concern is that our conclusion may be specific to our setting, where hours constraints, for example, are less likely to constrain adjustments for part-time workers than full-time workers. We address this concern by assessing whether the response varies across industries with varying flexibility in hours worked, but do not detect significant differences in treatment effects. A natural question is whether the treatment effect persists or reflects a temporary change in behavior to a "nudge". We exploit the time-dimension in our data to assess this question and find the treatment effects in 2015 persisted in 2016. This persistence supports the view that the information treatment affects earnings primarily from learning the tax and benefit schedule and not by other short-lived behavioral biases (see, e.g., Levitt, 2020). Finally, we find that the main conclusions from our bunching analysis hold across several assumptions about the counterfactual earnings distributions, and show that the information letters' assignment is unrelated to earnings distribution and trend before the change in 2015 took place.

Our paper is closely related to a small number of studies that experimentally control the type of information tax filers receive (e.g., Liebman & Luttmer, 2012 and Chetty & Saez, 2013), and how mobility across areas with varying knowledge about the earned income tax credit affects tax refunds (Chetty *et al.*, 2013). We contribute to this line of research by showing that governments can shape earnings elasticities by information policy and by pinning down the relative importance of information for overall attenuation in earnings responses.[3] Our paper also relates to the literature on bunching at kinks and notches, beginning with Saez (2010), Chetty *et al.* (2011), and Kleven & Waseem (2013).[4] In terms of understanding the factors that shape bunching responses, we contribute by documenting a sharp increase in the number

[3] Chetty & Saez (2013) study whether tax preparers in private firms affect the bunching around the EITC kink. On average, they find no evidence of the information provided by tax preparers on earnings. Liebman & Luttmer (2012) run a field experiment among retired workers, and find that an information brochure increased labor force participation by five percent among the elderly. The remaining evidence on salience effects is limited to alcohol and consumer goods (Chetty *et al.*, 2009), commuting tolls (Finkelstein, 2009) and cigarette consumption (Goldin & Homonoff, 2013). While our evidence highlights large attenuation in the intensive margin of labor supply, our findings are also consistent with information frictions to explain the absence of extensive margin responses to expansions of the EITC (Kleven, 2019).

[4] The bunching approach has been subsequently used to study the earnings test in social insurance (Gelber *et al.*, 2019), impacts of minimum wages (Harasztosi & Lindner, 2019; Cengiz *et al.*, 2019), inter-temporal responses to mortgage contracts changes (Best *et al.*, 2019), transaction taxes in housing markets (Kopczuk & Munroe, 2015; Best & Kleven, 2017), and corporate taxation (Best *et al.*, 2015).

of nonoptimizers after the locations of kinks and notches change. This matters for the interpretation of evidence on earnings elasticities deriving from tax reforms (see, e.g., Saez *et al.*, 2012). The evidence also highlights a difference between temporal factors (i.e., adjustment costs and information friction) and more permanent factors (i.e., preferences or ability) underlying optimization frictions. Relatedly, Gelber *et al.* (2019) document attenuated responses to the elimination of the earnings test in the US. Our evidence echoes this evidence, underscoring the difference between short vs. long-run responses to financial incentives.

Our paper also relates to Kline & Tartari (2016), who study labor supply responses to welfare reform with adjustment frictions, and other empirical studies of information and complexity in labor markets. Abeler & Jäger (2015) show that agents systematically under-react to complex incentives in an experimental setting, and Rees-Jones & Taubinsky (2019) document that individuals tend to use the average tax to forecast annual incentives on hourly or weekly income. Morrison & Taubinsky (2019) test for costly attention to incentives, and find that people increase the accuracy of their assessment of incentives when the stakes increase.[5] We contribute to these studies in two ways. First, using cross-sectional variation in notches' size, we find that the fraction of nonoptimizers does not vary with incentives' strength. This evidence is consistent with adjustment costs that are zero or prohibitively high to respond, and with models where nonresponse is explained by a lack of information about the notch. Second, our evidence from the informational treatment is consistent with models where agents either underestimate the marginal tax rate and where agents are inattentive to kinks' existence.

Finally, our findings have broad and specific policy implications. Our evidence lends broad support to models in public finance where the elasticity of taxable income is subject to policy control (e.g., Slemrod, 1994, Slemrod & Kopczuk, 2002 and Farhi & Gabaix, 2020). In stark contrast to classical work on optimal tax and transfer policy, where behavioral elasticities characterize the tradeoff between redistribution gains and the incentive costs of high taxes and benefits, these models suggest the efficiency losses from redistribution can be limited while strengthening the insurance from transfer programs. Our specific focus on the DI system speaks to a debate among academics and policymakers on the effectiveness of improving financial work incentives to induce DI recipients to work part-time.[6] While some researchers find some policies to be effective, Schimmel *et al.* (2011) find minimal earnings responses to shifts in the location of a notch under the US social security DI program, suggesting optimization frictions limit the scope for success.[7] However, our findings suggest the "$1 for $2 offset" policy proposal

[5] A large empirical literature studies how information and complexity affects a broader range of economic decisions. These decisions include how attention affects savings decisions (Jones, 2010), and over-withholding of tax liabilities (Jones, 2012). Hastings & Weinstein (2008) and Jensen (2010) study how information and perceptions about the returns to schooling affect behavior, Chetty *et al.* (2009) and Finkelstein (2009) document attenuated responses to non-salient taxes in consumer spending.

[6] Over the past 50 years, DI rolls steadily increased from below 1% to over 5% of the US adult population, 1% to 7% in the UK, and 2% to almost 10% in Norway (for a review, see Autor & Duggan, 2006). This rise has led to several attempts to induce DI recipients to work part-time by improving financial work incentives; for example, the US "$1 for $2 offset" policy proposal is meant to reduce caseloads by increasing the outflow. In our setting, part-time employed DI recipients keep approximately $1 for every $3 in earnings that they accumulate above the substantial gainful activity (SGA) threshold.

[7] Weathers & Hemmeter (2011) study the effects of a $1 for $2 earnings test among DI recipients who self-select into a pilot project, but are randomly assigned to the return-to-work program and the baseline system, including a notch at the SGA level. They find a 25 percent increase in employment among a group likely to be well informed given the self-selection into the program. Kostol & Mogstad (2014) study the impact of a similar return-to-work program in Norway that was provided randomly among existing recipients around an exogenous cutoff and find that employment impacts rise from 3 to 9 percentage points over the first three years after the change. Ruh & Staubli (2019) study the Austrian DI system and estimates a structural elasticity of 0.25 using bunching around notches. Similarly, Zaresani (2020) finds evidence of substantial adjustment costs in the Canadian DI system.

– that would replace the notch by a kink – may be much more successful and induce larger labor supply responses if an informational treatment accompanies the reform.

The remainder of the paper is organized as follows. Section 2 discusses institutional setting and Section 3 describes the data we use. Section 4 motivates our empirical framework with a static model of labor supply and Section 5 performs an observational analysis of bunching and nonoptimization. Section 6 documents how the information treatment shapes earnings responses, Section 7 assesses the labor supply margins through which the information treatment's effect operates, and Section 8 concludes.

2 Policy Environment

This section describes Norwegian Disability Insurance (DI) program, its work incentives and the informational setting.

2.1 The Norwegian DI System

The Norwegian DI program is designed to provide partial earnings replacement to all workers below retirement age who cannot work because of an impairment that has lasted for at least a year. The level of DI benefits received is based on a worker's previous earnings and calculates a worker's average indexed annual earnings (**AIE**).[8] Like other European DI programs, the Norwegian system distinguishes between total disability awards and partial awards that permit beneficiaries to use their residual work capacity. Since the inception of the Norwegian DI program in 1967, beneficiaries can earn up to a substantial gainful activity (**SGA**) threshold without losing any benefits.[9]

The SGA threshold plays a key role in determining the financial work incentives.[10] While the SGA amount has been adjusted every year to account for average wage growth, the first (real) change to the SGA threshold in Norway happened in May 1997, where the SGA level increased from $6,000 to $10,000. The SGA amount was further increased by an incremental $2,000 to $12,000 in 1998. These changes are summarized in the first row of Table 1 and illustrated by the first two long-dashed lines in Figure 1. The solid black lines illustrate the tax and benefit schedules in 1996, and the dashed line represents the schedules in 1997 and 1998. The figure also illustrates the discontinuous increase in tax liability at the SGA threshold – the notch – which creates a strong incentive to keep earnings below the SGA or above the upper limit of the dominated region, represented by the two vertical short-dashed lines to the right.[11]

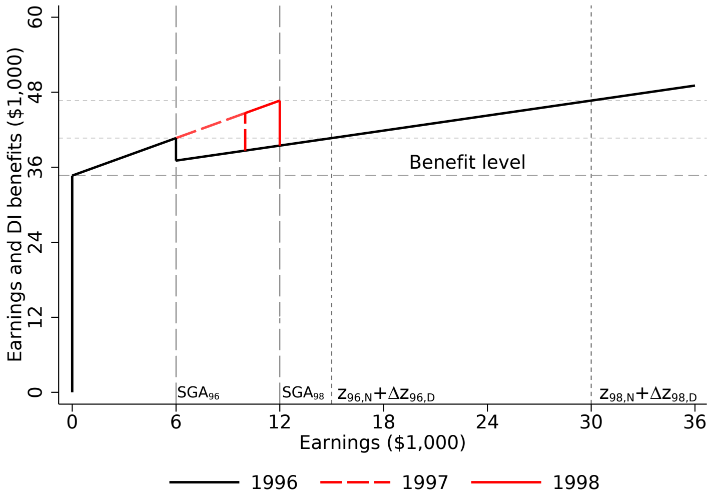
[8] Past wages are indexed to present wages using a deflator equal to the average wage growth in the economy. The years with the lowest earnings are excluded, and the proportion of income replaced falls with the level of previous earnings.

[9] Kostol & Mogstad (2014) summarize differences relating to demographics and replacement rates between Norway and the US. In terms of work incentives, SGA thresholds were similar in both countries (approximately $12,000 per annum in Norway and $13,000 per annum in the United States). There are two key differences. First, the SGA threshold is at the annual level in Norway, but at the monthly level in the US Social Security Disability Insurance (SSDI) system. Second, in contrast to the partial DI system in Norway, a person earning above the US's SGA amount is no longer eligible for benefits. A recent US policy proposal – known as the $1 for $2 benefit offset – would eliminate the notch in recipients' budget sets and replace the notch encouraging workers with residual work capacity to engage in the labor force (i.e., the Benefit Offset National Demonstration project).

[10] Earnings up to this point are subject to income taxation only and are exempt from benefit offset rules. Before 2015, the income tax rate on earnings is 3 percent higher than for DI benefits. After 2015, the income tax rates on earnings and benefits are the same. We disregard dependent benefits for the sake of simplicity and with minimal loss of generality about the expected impacts of the return-to-work program.

[11] Before the outward shift of the notch in 1997, the dominated region included earnings from the SGA threshold to $15,000,

Figure 1: Changes in the SGA Thresholds



Notes: The solid black lines represent the budget sets for recipients under 1996 rules, and the red dashed lines represent the budget sets that applied to recipients in 1997 and 1998. $z_{t,N} + \Delta z_{t,D}$ is the upper bound of the average dominated region in 1996 and 1997. The calculations are based on the sample of DI recipients with full benefits in the period 1997 to 2003. The changes are summarized and described in Table 1.

2.2 Benefit Phase-Out

A unique feature in the Norwegian DI system has been the co-existence of two sets of rules governing how benefits are phased out. At the end of 2004, the Social Security Administration (SSA) decided that recipients who had been awarded DI before January 1st of 2004 were eligible for a kinked phase-out of DI benefits (see Kostol & Mogstad (2014) for details). Ten years after the change, in December 2014, about half of all DI recipients faced a kink, while the other half faced a notch when their earnings reached the SGA level.

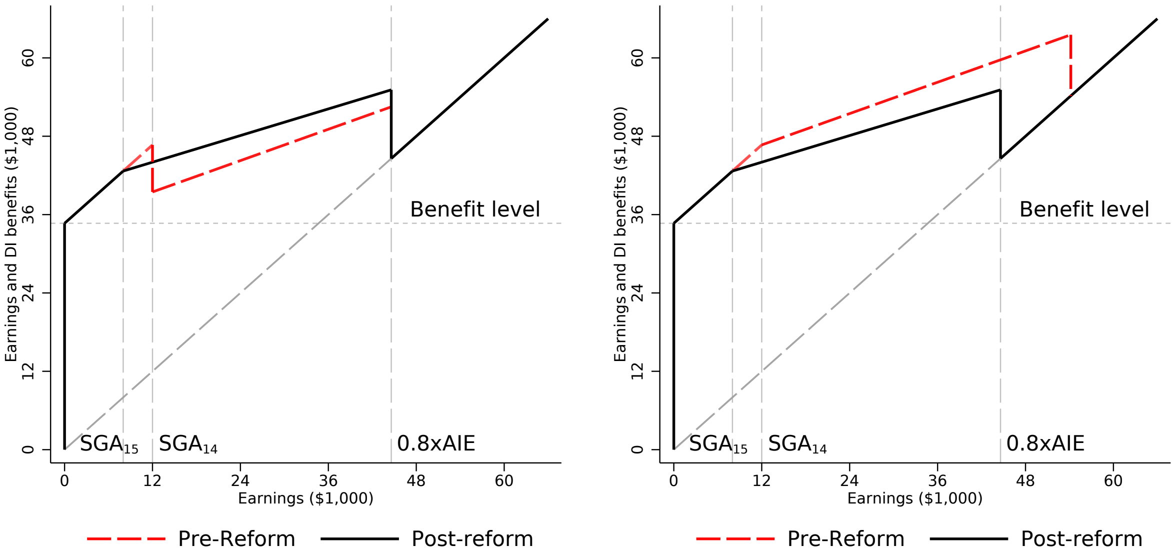
Under the notch regime, the benefit phase-out is described by b_N in the equation below. The rules create a discontinuous reduction of size T in benefits and very high marginal tax rates from working one extra hour at the notch. After the notch, there is a τ_b reduction in benefits for every dollar in earnings which equals about 60 percent for the average recipient. When earnings exceed the maximum permitted amount, equal to 80 percent of a person's previous indexed earnings capacity, $N_E = 0.8 \cdot AIE$, the person is automatically disenrolled from the program. We illustrate the notched budget constraint for the average DI recipient by the dashed line in Figure 2a. Under the kink regime, the discontinuous fall in benefits at the SGA notch is eliminated, and the maximum permitted earnings amount is increased to N_e. The dashed line in Figure 2b illustrates the kinked budget constraint and the exact benefit phase-out is described by b_K in the equation below.

$$b_N = \begin{cases} b^0 & \text{if } z \leq SGA \\ b^0 - \tau(z - SGA) - T & \text{if } SGA < z \leq N_E \\ & \text{ where } T = \frac{SGA}{AIE} b^0, \tau_b = \frac{b^0}{AIE} \\ 0 & \text{if } z > N_E \\ & \text{ where } N_E = 0.8 \times AIE \end{cases} \qquad b_K = \begin{cases} b^0 & \text{if } z \leq SGA \\ b^0 - \tau(z - SGA) & \text{if } SGA < z \leq e \\ & \text{ where } \tau_b = \frac{B^0}{AIE} \\ 0 & \text{if } z > N_e \\ & \text{ where } N_e = 0.8 \times (SGA + AIE) \end{cases}$$

denoted by the dashed line. After the shift, the dominated earnings region increased from a $9,000 range to an $18,000 range, up to an upper limit of $30,000, denoted by the rightmost dashed line. By comparison, the average earnings of DI recipients before disability onset was about $50,000.

Figure 2: Changes in the Tax and Benefit Schedule

(a) Year of DI award $\geq$ 2004 (b) Year of DI Award $<$ 2004



Notes: The solid black lines represent the budget sets for recipients under current rules and the red dashed lines represent the budget sets that applied before 2015 (see Kostol & Mogstad, 2014 for a detailed description of these two programs). To compute DI benefits and AIE, we use the average levels of all fully disabled DI recipients in January 2015. With minimal loss of generality, we disregard income taxation and dependent benefits. The changes are summarized and described in Table 1.

The last noteworthy change replaced the notch with a kink for everyone and reduced the SGA threshold from $12,000 to $8,000.[12] The change in the kink's slope and location in 2015 is illustrated by the solid lines in Figure 2a and 2b , where the marginal tax rate on earnings between SGA_{14} and SGA_{15} increased from zero to about 66 percent.[13] The new rules can be characterized by the phase-out equation b_K with two minor adjustments. The benefits level increased to compensate for the higher income tax rate. This change leads to a slightly higher phase-out rate for earnings above the new SGA level. The second was that the maximum permitted amount was reduced to N_E. Table 1 summarizes the relevant changes in the financial incentives.

Administration of the Benefit Phase-out. There are two ways in which DI benefits are phased out. Individuals can report earnings expectations directly to the SSA, which leads to immediate adjustments to benefits. The other is by ex-post adjustments. Before 2015, these ex-post adjustments were made after the DI program received information from the tax authorities. The DI program typically received this information around August in the following calendar year. After 2015, employers reported income monthly, which meant that the DI system would start phasing out benefits once the cumulative monthly earnings exceeded the annual SGA amount. With monthly reporting, this information would reach the DI system earlier and at a higher frequency.

[12]The new SGA level of $8,000 applied until 2018 for recipients awarded DI before January 1st, 2015. After 2018, and for recipients awarded DI after January 1st, 2015, the new SGA level was set to approximately $4,800.

[13]The marginal tax rate above notch increased by approximately six percentage points because gross benefits were increased to compensate for being taxed at the same rate as labor earnings. Hence, for every $1 above the SGA, benefits were offset according to the replacement ratio. Since the replacement ratio was slightly higher after 2015, the marginal tax rate for earnings above the SGA was higher.

Table 1: Timeline of Reforms

Dates:	Jan 1967	May 1997	Jan 1998	Dec 2004	Jan 2015
SGA level:	\$6,000	\$10,000	\$12,000	\$12,000	\$8,000
Incentive:	Notch	Notch	Notch	Kink replaces notch (awards < 1.1.2004)	Kink

Notes: This table summarizes the welfare-to-work reforms in the Norwegian disability insurance (DI) system from 1967 to 2015. The changes in the SGA levels are represented by Figure 1, and the notch versus kink tax and benefit schedules are presented in figures 2a and 2b. SGA amounts represented at 2015 levels are adjusted using the average wage growth in the economy. All variables are measured in 2015 dollars (NOK/\$ = 7.5).

2.3 Informational Setting

To characterize the knowledge about the new benefit phase-out in 2015, the SSA administered a survey among existing DI recipients at the end of 2014. The main finding was that most recipients knew about the changes to their tax and benefit schedule in 2015. The majority of respondents were aware that benefits would be taxed as earnings in 2015 and agreed to the statement that it will always pay to work more (i.e., kinked phase-out in 2015; see Section 2.2). However, the survey also revealed that many did not fully understand the extent to which the benefits would be phased out with their labor earnings. Only 34 (27) percent of respondents with (no) positive earnings were aware of how the changes in 2015 would affect their economic circumstances. The survey's main findings are summarized in Appendix Table A.1.

To target misperceptions about the new tax and benefit schedule, the SSA decided to issue information letters in May 2015. The information letter was sent by regular mail to a recipient's residential address and included SSA's emblem, as imprinted on standard SSA payment checks. The letter informed individuals about i) the exact location of the kink (e.g. $K = SGA_{15}$ in Figure 2a), ii) that it would always pay to work more (i.e., the elimination of the notch), and iii) encouraged individuals to report any expected change in annual earnings to the SSA. Recipients were also encouraged to use a web portal administered by the SSA. The portal included a micro-simulation tool allowing recipients to simulate the after-tax income for any level of earnings. An example of the information letter is displayed in Appendix Exhibit A.10.

However, the social security administration (SSA) decided that only individuals likely to locate above the kink by December 2015 would receive the letter.[14] To implement the information policy, the SSA used monthly earnings data reported directly from employers to project whether annual earnings would exceed the annual kink.[15] To forecast annual earnings, the SSA collected information about recipients' earnings for the first five months of the year using actual wage payments in January and February and earnings from March to May to project earnings for the remainder of the calendar year.

[14] Individuals who had informed SSA about expected annual earnings above SGA would not receive the letter. Individuals with cumulative earnings above the SGA in May 2015 were not eligible for the information letter. See the description of the administration of the benefit phase-out in Section 2.

[15] The projected annual earnings (PAE) is defined as $\hat{z}_i^{15} = \sum_{m=1}^{2} z_{im} + 3 \cdot \sum_{m=3}^{5} z_{im}$ where z_{im} is earnings for individual i in month m. Earnings in March, April, and May were used to forecast the annual earnings. Moreover, since the SSA used eleven months, they targeted individuals who would reach the annual kink by the end of November. This policy design accommodated an adjustment in the "last" month of the year, provided the information letter contained relevant information. To the extent that individuals' annual earnings were predictable, they could start re-adjusting annual earnings (by negotiating fixed hours contract, etc.) in July.

Overweighing earnings from March to May meant recipients with high earnings these months were more likely to receive the letter than individuals whose earnings were below their annual average in these months.

3 Data

This section describes the administrative data sources, our main analytical samples and key variables.

3.1 Data Sources

Our empirical analysis combines several administrative data sources linked by unique and anonymized identifiers for every resident individual and employer. The administrative nature of our data reduces the extent of measurement errors in wages and employment relationships. Because individual employment histories and most income components are third-party reported (e.g., by employers and financial intermediaries), the coverage and reliability are rated as exceptional by international quality assessments (see, e.g. Atkinson *et al.*, 1995). Since administrative data are a matter of public record, there is no attrition due to nonresponse or non-consent by individuals or firms. Individuals can only exit these data sets due to natural attrition (i.e., death or out-migration).

3.2 Variables

Our primary outcome variable is labor earnings, which we observe at monthly frequency from January 2015 and onward but only at the annual level before 2015. The main advantage of the monthly data is that sources of income are reported regularly by every employer and the SSA, and are continuously updated as new files from employers are received. The data files include information about contracted hours, hourly wages, bonuses, and other taxable benefits, and allows us to distinguish between bunching at kinks due to strategic reporting of income from self-employment and other types of behavioral responses. The monthly files from the SSA include cash payments from different types of transfer programs. The SSA data also includes records of whether a person received a letter in June 2015 that informed DI recipients about changes in the financial work incentives.

Finally, we link several characteristics of DI recipients to our estimation file, including the date of award, whether the award was for partial or full DI benefits; the age, gender, prior and current occupation, educational attainment, and household information of recipients.

3.3 Samples

We consider two samples for our empirical analysis. The first sample includes individuals deemed fully disabled by the SSA during the period from 1993 to 2003 to study shifts in the notch location in 1997 and 1998. The second sample considers individuals engaged in the labor market during the period from January to May 2015. We exclude DI recipients aged above 66 due to eligibility for old-age retirement benefits beginning at age 67. We further restrict the sample to individuals who were deemed fully disabled by the SSA due to a lack of information on the kink's exact location among the partially disabled.[16]

Table 2: Summary Statistics in 2015

Sample	Full sample of DI recipients		Main Analytical Sample, After Introduction of Kink in 2015					
			Any income		Notch in 2014		Kink in 2014	
Column:	(1)		(2)		(3)		(4)	
Earnings:	mean	sd	mean	sd	mean	sd	mean	sd
Monthly earnings Jan-May in 2015 ($)	89	472	524	1,039	516	1,173	533	883
Annual earnings in 2014 ($)	1,469	5,432	6,539	8,888	6,375	9,470	6,704	8,256
DI information:								
Age at DI award	40.29	13.78	35.79	14.68	43.19	13.94	28.33	11.21
Years on DI	12.26	11.14	13.85	11.17	4.36	2.78	23.42	7.77
Uncapped annual DI benefits ($), b^0	34,569	5,954	35,302	5,223	35,224	5,853	35,380	4,498
Annual indexed earnings ($)	55,791	18,241	56,333	17,596	59,311	20,149	53,331	13,949
Characteristics:								
Females	.56		.53		.52		.55	
Married/cohabitants	.49		.43		.48		.39	
Years of Schooling	10.35	2.81	10.21	2.75	10.46	2.79	9.95	2.69
Number of Children	1.62	1.4	1.43	1.4	1.56	1.4	1.29	1.39
Observations	229,648		39,073		19,616		19,457	

Notes: The sample consists of all DI recipients with full (100 percent) benefit at the beginning of the calendar year. The sample is further restricted to awards made before the 1st of January 2015, for individuals aged 18 to 66. The first column shows earnings and characteristics of the full sample of DI recipients, and the second column reports earnings and characteristics of individuals who had positive labor earnings in at least one month during January-May 2015. The third column shows the earnings and characteristics of recipients with positive earnings and faced a notch in 2014 (see the tax and benefit schedule in Figure 2a). The fourth column shows earnings and characteristics of recipients with positive earnings and faced a kink in 2014 (see the tax and benefit schedule in Figure 2b). Annual indexed earnings (AIE) summarize earnings history before the disability onset. All variables are measured in 2015 dollars (NOK/$ = 7.5).

Table 2 reports summary statistics for the latter sample. The first column shows the average recipient has spent nearly 12 years on the DI program and receives benefits replacing about 62 percent of his previous earnings. The second column reports summary statistics for individuals with some earnings. The average recipient earned about half of the SGA amount in 2014 and $524 per month during the first five months of 2015. The third and fourth columns further split the working DI recipients into two groups based on their tax and benefit schedule in 2014. These two columns illustrate that due to the award cutoff date, the notch sample is younger than the kink sample and has received DI benefits for a considerably shorter time. The notch sample has spent four years on the program on average, while the kink sample has spent more than 23 years on the program in 2015. This age difference is due to the award cutoff explained in Section 2.2.

A key feature of the monthly income data is that employers can update their reported monthly wages at any point until the tax return is filed in April the following year. This feature generates variation in the letter's receipt even among those eligible for the information treatment (i.e., those with annual earnings forecasts above the threshold).[17] Using the data on earnings from January to May 2015, we

[16]We also restrict to awards from before January 1, 2015 because recipients awarded benefits in 2015 or later faced a different set of work incentives.

[17]Note that we could implement a regression discontinuity (RD) design where we identify the effect of the information treatment for individuals at the threshold. However, since our focus is on how information shapes bunching behavior, we do not consider this empirical design, but instead implement a difference-in-difference (DD) estimator. Compared to the RD

reconstructed the assignment rule used by SSA in targeting the information letter. Appendix Table A.2 reports pre-determined earnings and characteristics among treated individuals and those who were eligible but did not receive the information letter.

4 Empirical Framework

This section presents a conceptual framework that motivates our empirical strategy.

4.1 A Stylized Model of Labor Supply

We follow Saez (2010) and Chetty (2012) and assume that agents maximize utility by choosing consumption (c) and pre-tax earnings (z) subject to the constraint $b + z - T(z) = c$. Higher consumption increases utility, and higher earnings generates dis-utility from effort. The tax system is linear, $T(z) = \tau_t z$, where τ_t is a proportional tax on earnings. Unearned cash income, or benefits, is represented by b. We follow the convention in public economics in assuming quasi-linear and iso-elastic flow utility $v_i(c,z) = c - \frac{a_i}{(1+1/e)}\left(\frac{z}{a_i}\right)^{1+1/e} - \psi_i$, where the parameter e is the structural elasticity of labor supply with respect to net after-tax earnings, a_i is ability and ψ_i is cost of adjusting earnings for individual i. In absence of adjustment costs, individual labor supply is given by $z_i^* = a_i(1-\tau_t)^e$.[18]

In standard labor supply models, adding a convex kink τ_b to the linear tax schedule will induce individuals with earnings initially above the (new) kink to reduce their earnings. We assume that agents optimize perfectly before the change in work incentives to focus our analysis on mistakes arising only from changes in incentives. The new tax system is perceived as $T(z) = \tau_t z_i + \theta_i \tau_b (z_i - K) \cdot 1_{z>K}$, where the parameter θ_i measures the degree to which agent i under- or overreacts to the tax τ_b at the bracket cutoff K. Labor supply can now be written as

$$z_{F,i} = \begin{cases} a_i(1-\tau_t-\theta_i\tau_b \cdot 1_{z>K})^e & \text{if } v_i(c,z_F) - v_i(c,z^*) \geq \psi_i \\ a_i(1-\tau_t)^e & \text{if } v_i(c,z_F) - v_i(c,z^*) < \psi_i. \end{cases} \tag{1}$$

This equation illustrates that both adjustment costs and information frictions attenuate the structural earnings response to the change in work incentives. Only agents with costs below the gain from adjusting, and only individuals aware of the kink respond. The attenuation in the overall earnings response is given by $\frac{\Delta z_F}{\Delta \tau_b} = (1-\alpha)\Delta z^*$, where Δz^* is the structural earnings response, and α is the fraction of agents that do not respond due to adjustment costs being greater than utility gains $\psi_i > v_i(c,z_F) - v_i(c,z^*)$, or due to incorrect perception of the tax $\theta \neq 1$. The equation also highlights the interdependence of the two classes of frictions. Increasing the fraction of individuals with the correct perception of the tax also changes the perceived gains from re-optimizing.

Suppose we would randomize information about the location and slope of the kink just after the reform and eliminate misperceptions about the kink. Assuming that a fraction of the noninformed (i.e., the control group) perceives the tax to be lower or are unaware of its introduction, i.e., $\theta < 1$, the

design, the DD approach's main advantage is that it allows us to consider mean impacts for the same population we use in our bunching analysis.

[18] We disregard heterogeneity in the elasticity for expositional clarity. In our empirical application, our goal is to estimate the average structural elasticity.

experiment would induce a stronger response to the introduction of the kink. The labor supply of an agent with ability a_i is now given by

$$z_{I,i} = \begin{cases} a_i(1-\tau_t-\tau_b\cdot 1_{z>K})^e & \text{if } v_i(c,z_I)-v_i(c,z^*)\geq \psi_i \\ a_i(1-\tau_t)^e & \text{if } v_i(c,z_I)-v_i(c,z^*)< \psi_i. \end{cases} \quad (2)$$

In such a hypothetical information experiment, the ratio of earnings elasticities equals the ratio $(1-\alpha')/(1-\alpha)<1$. If the adjustment cost was sufficiently small, so that $\alpha'\approx 0$, the information experiment would identify the structural elasticity e.

4.2 Empirical Strategy

Consider the density of earnings $h_0(z)$ and the behavioral response to introducing a kink (K). Theory predicts individuals with initial labor earnings in the range $[K,K+\Delta z]$ will bunch exactly (or near) the kink in response to the introduction of the tax τ_b.[19] Our empirical strategy employs a bunching approach to estimate excess mass and to identify attenuation in earnings responses from information (e.g., θ) versus other types of frictions (e.g., ψ). In the special case without frictions, the amount of bunching $B(e)$ at the kink depends on the structural elasticity and the size of the tax and equals the integral over the bunching segment $\int_K^{K+\Delta z} h_0(z)dz$. In a more realistic setting, Kleven (2016) shows that the amount of bunching in the presence of optimization frictions depends on both the elasticity, the perception of the tax, and the adjustment costs $B_F(e,\theta,\psi)$. Under the assumption that the baseline (counterfactual) density is constant on the bunching segment, Saez (2010) showed that bunching is proportional to the marginal response Δz^*. This assumption implies the earnings response can be inferred by dividing the total amount of bunching by the height of the density at the kink.

Strategy 1: Identifying the fraction of nonoptimizers. Our first strategy exploits regions of dominated choice that arise from notches in the tax and benefit schedule. Denoting this region $[z_N, z_N+\Delta z_D]$, where z_N is the notch's location in the tax and benefit schedule, we follow Kleven & Waseem (2013) to estimate the population's share inert to the change in disposable income from a notch. The basic idea is that any form of preference for leisure and consumption dictates that earnings below the notch are preferred to earnings above it: Reducing labor supply from just above a notch increases utility through higher leisure and consumption. Individuals who locate in the dominated region do so either because adjustment costs are higher than the gain from reoptimizing or lack of information about the notch's presence. With an estimate of the counterfactual density, $h_0(z)$, the population share inert to the notch can be calculated as the ratio $\alpha\equiv\int_{z^*}^{z^*+\Delta z_D} h(z)dz \,/ \int_{z^*}^{z^*+\Delta z_D} h_0(z)dz$. In combination with estimates of the observed earnings response, the quantity helps recover the structural earnings response $\Delta z^* = \Delta z/(1-\alpha)$.

Strategy 2: Information Experiment. Our second strategy exploits the quasi-experimental variation in information letters targeting perceptions about the tax and benefit schedule. Assuming the treatment

[19] This behavioral response can be illustrated graphically by drawing indifference curves in a budget set without a kink. The introduction of a kink only affects the utility (and indifference curves) of agents who initially (i.e., before the reform) supply labor earnings above the kink location. This argument relies on an assumption of no income effects. Bastani & Selin (2014) shows that this assumption does not materially affect the estimate of the compensated elasticity.

affects Δz_I relative to Δz_F only through the effect of changing θ, the effect of the information policy on behavioral elasticities can be inferred from the difference between bunching among the treated $B_I(e,\psi)/h_0(K)$ and bunching among non-treated $B_F(e,\theta,\psi)/h_0(K)$. Under the assumption that adjustment costs are sufficiently large, the average perception of the tax can be recovered from the ratio $B_F(e,\theta,\psi)/B_I(e,\psi)=\theta$.[20]

5 Observational Bunching Analysis

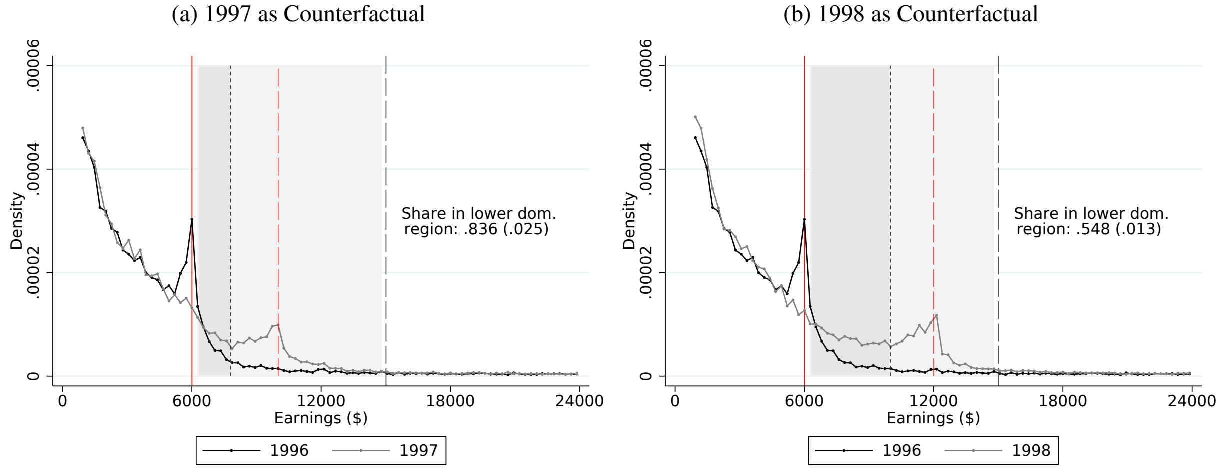
This section estimates the fraction of nonoptimizers under a notch and the earnings elasticity under a kink using bunching techniques.

5.1 Empirical Implementation

The key challenge confronting research on bunching behavior is obtaining a valid estimate of the counterfactual density, $h_0(z)$. We address this challenge in two ways. Our first approach exploits policy-induced changes in notch thresholds in 1997 and 1998 to obtain a non-parametric estimate of the density in 1996. The change in the dominated region allows us to reconstruct a counterfactual distribution of earnings in the interval ranging from \$6,000 to \$12,000 under the assumption of no other distortions to this region of the earnings distribution in 1997/1998.

Figure 3 illustrates this non-parametric approach. We use a histogram to estimate the empirical and counterfactual densities and group DI recipients into earnings bins of \$267 (2,000 NOK) based on their

Figure 3: Illustration of Non-Parametric Approach



Notes: Figure a illustrates the earnings distributions in 1996 and 1997, and Figure b illustrates the earnings distributions in 1996 and 1998. The light-gray shaded region and long-dashed line denote the dominated region in 1996. The dark shaded region and short dashed lines denote the lower part of the dominated region in which we can identify the fraction of nonoptimizers for in 1996. Using 1997 as a counterfactual for 1996, it covers 20 percent of the dominated region (left panel). Using 1998 as a counterfactual for 1996, it covers 44 percent of the dominated region (right panel). The lower dominated region share is calculated by comparing the total range frequency in 1996 over the counterfactual frequency (i.e., 1997/1998). The standard deviation in parenthesis is calculated with 500 bootstrap repetitions. The solid red (dashed) line denotes the old (new) SGA threshold. The sample consists of DI recipients with full (100 percent) benefit at the beginning of the calendar year from 1996 to 1998. Earnings are adjusted using the average wage growth and are reported in 2015 dollars (NOK/\$ = 7.5).

[20]Assuming average earnings are pre-determined and $\bar{z}_N=\bar{z}_I$. If adjustment costs are small, then the fraction of agents responding to the change in work incentives may also be affected by the information experiment. This means that earnings adjust because $\Pr[v_i(c,z_F)-v_i(c,z^*)<\psi_i]<\Pr[v_i(c,z_I)-v_i(c,z^*)<\psi_i]$ and because $a_i(1-\tau_t-\tau_b\cdot 1_{z>K})^e\neq a_i(1-\tau_t-\theta\tau_b\cdot 1_{z>K})^e$.

annual earnings. The left panel displays a (nominally adjusted) histogram of the earnings distributions in 1996 and 1997 and shows that the two densities are virtually indistinguishable for earnings below \$5,000. The right panel shows that the bin counts below \$5,000 in 1996 and 1998 are also similar. Noticeable differences in the earnings distribution slope occur around \$8,000 in 1997 and around \$10,000 in 1998, indicating that optimization frictions create bunching regions about \$2,000 below the new SGA levels. The solid vertical line and the dashed line farthest to the right illustrate that we can identify nonoptimizers from about 20 percent of the full dominated region using the 1997 data and about 44 percent of the full range if we use the 1998 data. We calculate the population share of nonoptimizers from the cumulated observed bin counts in the dominated range and divide it by number by the cumulated counterfactual bin counts in the dominated range.

Our second approach fits a flexible polynomial to the histogram of earnings to obtain an estimate of the counterfactual distribution (see, e.g., Chetty *et al.*, 2011). We follow standard practice and estimate

$$c_j = \sum_{i=0}^{p} \beta_i (z_j)^i + \sum_{k=z^L}^{z^U} \gamma_k \mathbf{1}(z_j = k) + \varepsilon_j, \tag{3}$$

where c_j is the number of recipients in income bin j, z_j is the earnings level of bin j, and z^L is determined by visual inspection. The indicator variables for each bin in the excluded range ensure that the polynomial is estimated without considering the range of earnings $[z_L, z_U]$ below and above the SGA. The procedure for deciding z^U differs for kinks and notches. For kinks, z^U is determined by visual inspection. The counterfactual density function is then adjusted so that the estimated missing mass above the kink is equal to the estimated bunching mass at the kink.[21] We cannot determine z^U by visual inspection for notches due to the thin right tail of the DI recipients' earnings distribution. Instead, we assume no extensive margin response. This assumption implies the missing mass above the SGA threshold must equal the bunching mass below the SGA threshold. We identify the upper bound by increasing it in small increments, and the equation above is re-estimated until the estimated missing mass is equal to the estimated bunching mass. Another choice is the order of the polynomials p. We leverage identification from the non-parametric approach and choose a polynomial order that minimizes the distance between the estimate from the non-parametric approach and the polynomial approach. As the non-parametric approach is limited to the lower part of the dominated range, the polynomial approach allows us to estimate the fraction of nonoptimizers in the full dominated range.

Structural Elasticity. Kleven & Waseem (2013) show that the structural elasticity e can be inferred from the observed earnings response and the fraction of nonoptimizers. This result is derived from the fact that the marginal bunching individual is indifferent between locating at the SGA threshold and some interior point. They show that it is possible to identify the elasticity e as an implicit function of the tax parameters, the SGA threshold, and the average earnings response. This function can be written as

$$\frac{1}{1+\Delta z/z_N} - \frac{1}{1+1/e}\left[\frac{1}{1+\Delta z/z_N}\right]^{1+1/e} - \frac{1}{1+e}\left[1 - \frac{\Delta t}{1-t}\right]^{1+e} = 0 \tag{4}$$

[21] This is done by first calculating the fraction of number of individuals in the bunching region over the estimated counterfactual, $\hat{c} = \sum_{k=z^L}^{z^U} \sum_{i=0}^{p} \beta_i (z_k)^i$. Then, we calculate the ratio of the actual frequency and estimated counterfactual above the bunching region. These two ratios should equal to ensure that the sum of the bins in the histogram equals one. This is achieved by upward shifts in the counterfactual distribution to the right of the kink, which is done in increments until the counterfactual satisfies the integration constraint.

where the structural response Δz^* identifies the structural elasticity e. Using the observed earnings response $\Delta z = (1-\alpha)\Delta z^*$ identifies an "observed" elasticity e_F, which is attenuated by information and adjustment frictions.

Inference. We calculate standard errors using bootstrap methods. First, we generate many earnings distributions by random resampling with replacement. We then re-estimate the parameters of interest within each sample, and define the standard error as the standard deviation of the distribution of interest. In the non-parametric approach, we may observe the same individual more than once. To account for potential serial correlation in the error terms, we perform block-bootstrap with replacement over individuals, keeping all observations of each individual that we resample. In all estimations, we use 500 repetitions.

5.2 Overall Frictions

From the histograms in Figure 3, we calculate the share of nonoptimizers in the lower dominated region in 1996 to be equal to 0.55 when we use 1998 as a counterfactual, and 0.85 where we use 1997 as the counterfactual distribution. These non-parametric estimates are highly statistically significant and are comparable to findings in Kleven & Waseem (2013), who consider income tax filers in Pakistan, and Ruh & Staubli (2019), who consider DI recipients in Austria.

One concern with the non-parametric approach is that the earnings distribution in 1997 may be prone to inertia. Some individuals who previously bunched at the $6,000 threshold may remain at this earnings level in 1997. Such inertia would invalidate the assumption of no behavioral distortions to the lower part of the earnings distribution and inflate the density used to calculate the counterfactual, leading to an upward bias in the estimated fraction of nonoptimizers. To address this concern, we re-estimate the lower part of the exact same dominated range but using the earnings distribution in 1998, where we expect less inertia if individuals adjust to changes in the notch with a lag. Reassuringly, our estimate of 0.81 is similar, and we cannot reject the null hypothesis that the estimate is equal to 0.85 using 1997 as the counterfactual density. This comparison is illustrated in Appendix Figure A.1.

We expand our assessment to the full dominated range using the polynomial approach. Figure 4a shows the fitted counterfactual and the actual earnings histogram, and Figure 4b shows that the estimate stable across different polynomial specifications (choices of p in equation 3). The circles represent point estimates, and the solid vertical lines represent two-sided 95 percent confidence intervals illustrating that the estimates are all within the confidence interval of the highest order polynomial.[22] Our baseline estimate equals 0.51. This smaller fraction of nonoptimizers for the full range than the lower dominated ranges is somewhat surprising. In theory, the share of nonoptimizers depends on the utility gain from moving to the notch point and the underlying optimization frictions. Since the utility gain of reoptimizing falls the closer a person is to the point of indifference between an interior solution and locating at the notch, we expect the fraction of nonoptimizers to increase with higher earnings. Hence, a smooth distribution of adjustment costs implies the fraction of nonoptimizers is underestimated when focussing on the lower part of the dominated range. However, under a discrete distribution of adjustment

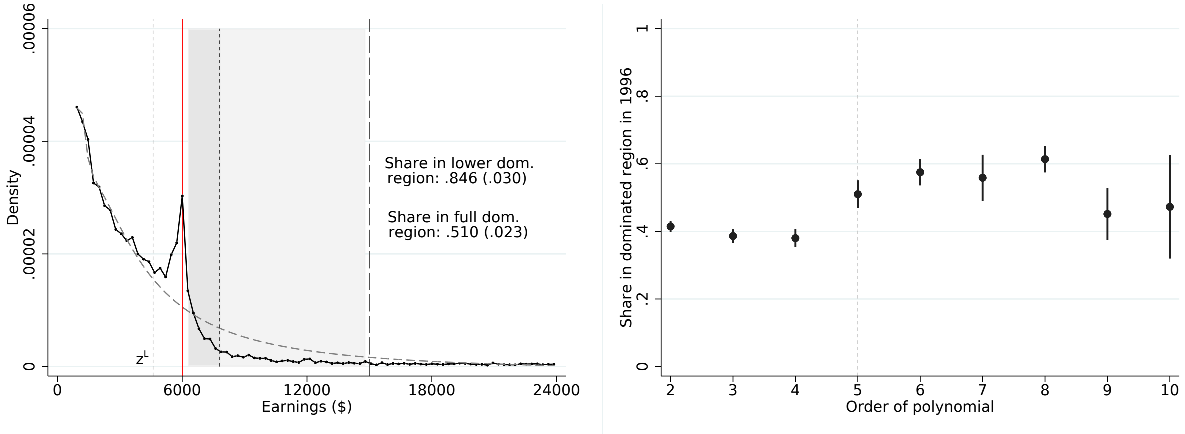
[22]The SGA-amount has not changed in real terms since 1998 but has only undergone minor nominal adjustments to account for average wage growth. We repeat the exercise for the most recent notch-year and find the share of nonoptimizers in the full range in 2014 is about 0.4, and is similar but slightly lower than the estimate from 1996. Appendix Figure A.2 illustrates these results and shows that the fraction is very stable across specifications.

costs, this relationship is weakened, and the relationship reverses if adjustment costs (i.e., unobserved heterogeneity) are negatively correlated with earnings potential.

Figure 4: Fraction of Nonoptimizers

(a) Nonoptimizers Over Full Dominated Range

(b) Nonoptimizers By Polynomial Order



Notes: Figure a illustrates the earnings distributions of DI recipients in 1996 and the fitted counterfactual density using the polynomial approach. The polynomial order is chosen to minimize the distance between the estimate of nonoptimizers from the non-parametric approach and the parametric approach for the lowest range of the dominated region (see Figure 3a). The dark shaded region and short dashed lines denote the lower part of the dominated region in which we can identify nonoptimizers using 1997 as a non-parametric estimate of the counterfactual distribution in 1996. The light-gray shaded region and long-dashed line denote the full dominated region in 1996. The red (dashed) line denotes the old (new) SGA threshold. Figure b shows the sensitivity of the estimated fraction of nonoptimizers over the full dominated range. The dashed vertical line denotes our baseline specification. The sample consists of DI recipients with full (100 percent) benefit at the beginning of 1996. Earnings are adjusted using the average wage growth, and are reported in 2015 dollars (NOK/$ = 7.5).

We exploit variation in the size of the notch across individuals to assess whether the smaller share of nonoptimizers in the upper part of the dominated region may be driven by unobserved heterogeneity. In the Norwegian DI program, lower average indexed earnings lead to higher replacement rates, and higher replacement rates create larger notches. We examine whether the size of the notch varies with the response to it by dividing our sample by the strength of the notch incentive, and re-estimate the fraction of nonoptimizers for groups of individuals with different replacement rates. We find no significant relationship between this fraction and the strength of the incentive to re-optimize. These findings are reported in Appendix Figure A.3.

To further examine the nature of optimization frictions, we take three steps. First, we study how the fraction nonoptimizers vary with the notch location changes in 1997 and 1998. We expect to observe increases in the number of nonoptimizers during 1997 and 1998, followed by a decline if people learn about the financial work incentives or overcome other adjustment costs with a time lag. We estimate the counterfactual and fraction nonoptimizers using the polynomial approach for each year from 1993 and 2003, and plot the time pattern in Figure 5a. It shows that the number of nonoptimizers increases sharply during the reform and then falls back to the 1996 level three to four years later. In the second step, we assess how the number of nonoptimizers evolves since first entering the DI program. Figure 5b shows that five years after the entry year, the fraction falls from 0.55 to below 0.4. This evidence suggests that optimization frictions can be divided into two broad classes of factors. One class appears to have a temporal nature, and is consistent with learning the tax and benefit schedule and with the idea that people overcome hours constraints (or other adjustment frictions) with a time lag. The second class is consistent with permanent traits, such as preferences or ability, giving rise to persistence in dominated behavior.[23]

Figure 5: Fraction of Nonoptimizers Over Time

Notes: Figure a illustrates repeated estimates of the fraction of nonoptimizers in the full dominated region from 1993 to 2004. The substantial gainful activity (SGA) threshold changed from \$6,000 to \$10,000 in 1997 (dashed vertical line) and to \$12,000 in 1998 (solid vertical line). Figure b illustrates repeated estimates of nonoptimizers in the full dominated region from the year of DI award to eight years after. The polynomial order ($p = 5$) is chosen to minimize the distance between the estimate of nonoptimizers from the non-parametric and the parametric approach for the lowest range of the dominated region (see Figure 4). The sample consists of DI recipients with full (100 percent) benefit at the beginning of the calendar year over the period 1993 to 2007 who face a notch in their budget set. Earnings are adjusted using the average wage growth.

In the last step, we assess the role of career concerns. If there is a continuous relationship between current and future wages, career concerns would reduce, but not eliminate, the dominated range. We follow Ruh & Staubli (2019) and group recipients in each year into four segments of earnings z. The first segment is the bunching region ($z_L \leq z \leq SGA$) and the second segment is the dominated region ($SGA < z \leq z_D$). The third and fourth segments include earnings below the bunching region and above the dominated region. Subsequent moves above the dominated earnings would represent career concerns, where agents rationally locate in the dominated region today to increase their career prospects tomorrow. By tracking individuals relative to the first year a person is observed in the dominated region, we find that nearly all nonoptimizers either return to the bunching segment or reduce their earnings to below the bunching region. Only 10 percent of the nonoptimizers remain in the dominated region after the first year, and only five percent are earning above the dominated region five years later. This evidence is presented in Appendix Figure A.4.

5.3 Bunching Elasticity

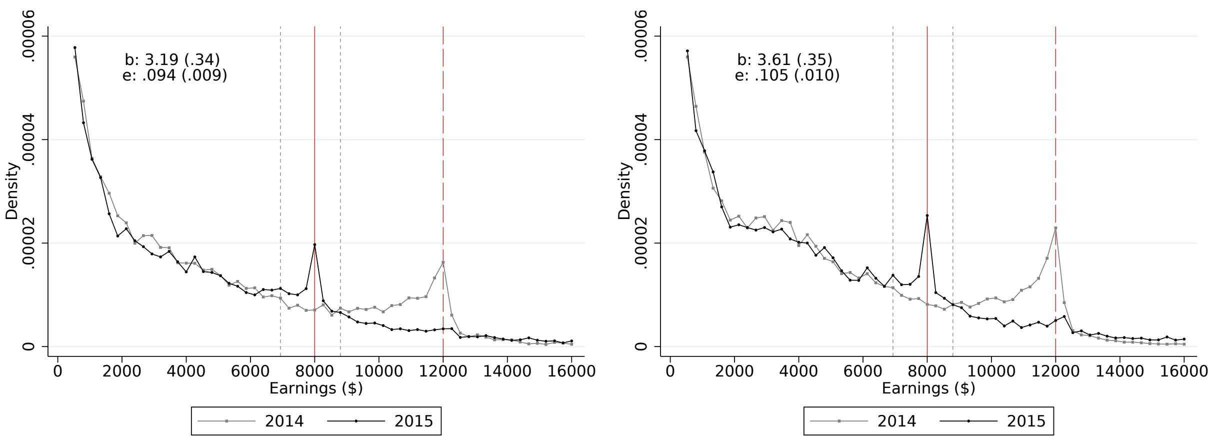
Under the assumption of no anticipation effects, we can use the non-parametric approach to identify the counterfactual density of earnings around the new kink in 2015, $\hat{h}_0(K)$.[24] We plot histograms of earnings distributions in 2014 and 2015 for the notch-sample (i.e., awards prior to January 1, 2004) in Figure 6a and for the kink-sample (i.e., awards after January 1, 2004) in Figure 6b. Earnings bins are by \$267 (2,000 NOK) increments, the solid line is the new SGA threshold at \$8,000, and the dashed line is the

[23]While beyond the scope of this paper, this latter explanation speaks to the potential undesirable distributional impacts of increasing the complexity of transfer programs to control the behavioral elasticity (see Taubinsky & Rees-Jones, 2017).

[24]The earnings response is subsequently inferred from the total amount of bunching, which is approximately equal to $\hat{h}_0(K)\Delta z$. Since earnings are grouped into bins, the earnings response is equal to the normalized excess mass, or total bunching, multiplied by the bin width $\$266 \cdot \big(B/\hat{h}_0(K)\big)$.

threshold from 2014, at \$12,000. We see that the two densities are very similar for earnings bins below \$6,000 in both samples. After this point, it is somewhat unclear where the bunching segment starts. We set the lower-bound, z_L, represented by the lower short-dashed line to approximately \$7,000. We set the upper-bound z_U to the point where the two earnings distributions intersect at just below \$9,000, represented by the upper short-dashed line. Next, we calculate the excess mass by integrating the area above the earnings distribution in 2014 and divide by the average height of the density in the bunching region. This ratio gives us a normalized bunching mass of 3.2 for the notch-sample and a slightly higher bunching estimate for the kink-sample. We can not reject the hypothesis that the two bunching estimates are the same.

Figure 6: Non-Parametric Evidence of Bunching Elasticity in 2015



(a) Notch sample: 2014 vs 2015

(b) Kink sample: 2014 vs 2015

Notes: Figure a illustrates the earnings distributions in 2014 and 2015 for DI recipients with awards from before January 1st, 2004 (i.e.,. notch sample), and Figure b illustrates the earnings distributions in 2014 and 2015 for DI recipients with awards from after January 1st, 2004 (i.e.,. kink sample). The solid line is the new SGA threshold, and the dashed line is the SGA threshold from 2014. The sample consists of DI recipients with full (100 percent) benefits during 2014 and 2015. Earnings are adjusted using the average wage growth, and are reported in 2015 dollars (NOK/\$ = 7.5).

We multiply the bunching estimate with the bin-width to get the non-parametric estimate of the observed earnings response. Our estimate is about \$850 for individuals awarded benefits after January 1st, 2004, and \$960 for individuals awarded benefits before January 1st, 2004. We then divide by the SGA threshold and multiply by a normalized tax change at the kink. The normalization divides the tax change by the average net after benefit offset earnings and provides non-parametric estimates of the kink-elasticity of around 0.1.[25] We proceed by applying the polynomial approach to the kinks in 2015 and estimate slightly smaller bunching elasticities but still quantitatively similar to the non-parametric approach. Appendix Figure A.5 illustrates the fitted counterfactual earnings distribution for the notch- and kink-samples, and Appendix Figure A.6 shows that the elasticities are remarkably stable across specifications.

Table 3 summarizes our bunching analysis. Columns 3 and 4 of Panel B reports the estimates of bunching and earnings elasticities in 2015 using the polynomial approach. The point estimates of the observed elasticities are 0.065 and 0.07 are precisely estimated with bootstrapped standard errors of 0.007. By comparison, Panel A reports estimates of bunching behavior in 2014. The first row reports the

[25]By comparison, Zaresani (2020) estimates the elasticity of earnings with respect to net-of-tax income to be 0.1 in the Canadian DI system. Bunching elasticities from notches and kinks among wage earners tend to be much smaller. Estimates reported by Chetty *et al.* (2011), Le Maire & Schjerning (2013), and Bastani & Selin (2014) range from 0-0.05.

fraction of nonoptimizers, and the second row reports our estimate of bunching at the notch. The estimate of 11.75 translates into a somewhat imprecise estimate of 0.087 for the observed notch-elasticity. Using the share of nonoptimizers to adjust the earnings response, we estimate a structural elasticity of 0.286 by applying equation 4. The estimate is precisely estimated with bootstrapped standard errors of 0.027. Assuming the structural elasticity (e) is a deep parameter, i.e., it is policy-invariant and does not change with the change in incentives from 2014 to 2015 – we can compare the amount of attenuation in observed elasticities before and after the reform. The last row reports the implied attenuation by comparing the observed elasticities with the structural elasticity. We calculate large attenuation in the observed elasticity in 2014 and conclude that an observational bunching analysis would miss 70 percent of the underlying response. The third and fourth column shows a slightly larger but comparable attenuation rate of observed elasticities after the reform.

Table 3: Bunching Elasticity in 2014 and 2015

	A. Pre-reform		B. Post-reform	
Year of DI award:	$\geq$ 2004	$<$ 2004	$\geq$ 2004	$<$ 2004
Incentive:	Notch at \$12,000	Kink at \$12,000	Kink at \$8,000	Kink at \$8,000
	(1)	(2)	(3)	(4)
Fraction of Nonoptimizers (α):	.387***			
Standard error	(.027)			
Normalized Bunching ($B/\hat{h}_0$):	11.75***	8.96***	2.12***	2.26***
Standard error	(.71)	(.51)	(.21)	(.23)
Observed Bunching Elasticity:	.087	.217***	.065***	.070***
Standard error	(.059)	(.012)	(.007)	(.007)
Structural Elasticity (e):	.286***			
Standard error	(.077)			
Attenuation rate (%):	69.6	24.1	77.3	75.5
Parametric approach	Yes	Yes	Yes	Yes
Observations	21,463	21,581	25,484	22,209

Notes: This table provides estimates of the fraction of nonoptimizers, normalized bunching, the implied observed elasticity and structural elasticity using equation 4 and adjusting for nonoptimizers. The estimates are obtained using the fitted values from the polynomial approach. The polynomial order ($p = 5$ for notch and $p = 7$ for kink) is chosen to minimize the distance between the estimate from the non-parametric and the parametric approach in 2015 (see Figure A.9). The sample consists of DI recipients with full (100 percent) benefit at the beginning of the calendar year in 2014 and 2015 (see Table 2 for details). Earnings are adjusted using the average wage growth, and are reported in 2015 dollars (NOK/$ = 7.5).

One concern with our approach is that the notch and kink elasticities may be different because notched incentives are much stronger and, therefore, more likely to overcome any form of friction. To assess this concern, we report estimates of bunching and the earnings elasticity in 2014 for the kink sample in Panel A of Table 3. The kink-elasticity in column 2 is about three times larger than the elasticity for the notch sample in column 1. At first glance, this may seem surprising. Yet, the similarity of the estimated elasticities in 2015 from Panel B of Table 3 suggests the discrepancy is not explained by differences in ability or underlying heterogeneity in the elasticity. Instead, Table 2 documents that DI recipients in the kink-sample have been in the program more than five times as long as the notch-sample. This observation suggests that with more time since entry, overall optimization frictions matter less in attenuating the kink-elasticity.

5.4 Threats to Identification.

We have identified the bunching elasticity under the assumptions of no distortions to the lower part of the earnings distribution in 2014 (below the SGA) for the non-parametric approach, and that a polynomial specification identifies the counterfactual distribution for the parametric approach. To further challenge the validity of the histogram and polynomial approach, we implement two alternative non-parametric methods that impose different assumptions.

The first approach addresses concerns about the shape of the unobserved ability distribution. Consider the bunching segment defined by the vertical lines in Figure 6. If the true slope of the counterfactual density is flatter (steeper) than implied by the line between the upper and lower part of the bunching segment, the estimated elasticity from a linear approximation will be biased upward (downward). Bertanha *et al.* (2019) show that the elasticity can be set identified by making an assumption about the absolute value of the slope of the counterfactual density in the bunching segment.[26] We implement this approach on a logarithmic transformation of earnings in 2015 and vary the maximum slope parameter to identify the bunching elasticity bounds. We plot these bounds and describe our implementation in Appendix Figure A.8. The estimated bounds are relatively sharp for a wide range of slope parameters. Importantly, our estimates from both the histogram and polynomial approach lie inside the non-parametric bounds.

The second approach assumes that the ability ranking is preserved when DI recipients move from a notch or kink at \$12,000 to a kink at \$8,000. Blomquist *et al.* (2019) show that under this assumption, the earnings response can be identified non-parametrically by inverting the cumulative distribution function (CDF). Similar to the histogram approach, it uses the earnings distribution from 2014 to estimate the counterfactual for 2015. The CDF approach differs from the histogram approach in that it identifies the observed earnings response by inverting the two CDFs to the point where the cumulative probabilities equaled the cumulative probability at the kink in 2015. We then follow the standard formula for calculating the kink-elasticity. Reassuringly, this approach delivers estimates of the elasticity ranging from 0.07 to 0.12. Throughout the rest of the paper, we are confident that we can rely on the polynomial approach to estimate both the share nonoptimizers and the bunching elasticity. We illustrate the CDFs and describe the implementation in Appendix Figure A.7.

6 How Information Frictions Shape Earnings Responses

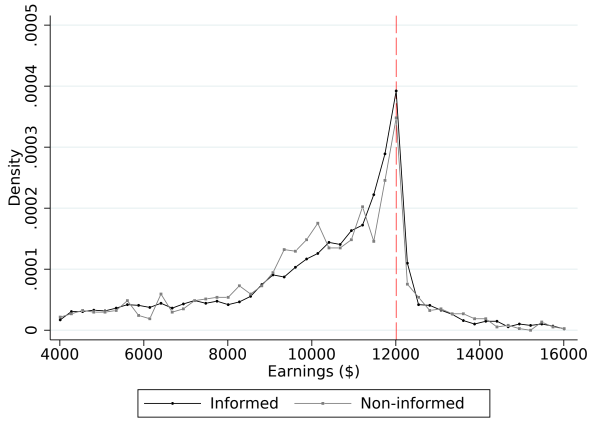
This section examines whether earnings responses can be shaped by information policy, and pins down the relative importance of information frictions versus other kinds of frictions.

[26]The intuition is that the elasticity is determined by the bunching segment's length and the excess bunching. By imposing structure on the counterfactual shape within the bunching segment, it is possible to derive a lower bound and an upper bound of the elasticity without taking a stance on whether a polynomial function approximates the slope well. The challenge is that the counterfactual distribution could, in principle, take any form within the segment. Bertanha *et al.* (2019) allow the counterfactual to be both steeper and flatter than the linear approximation, and shows that when the maximum slope parameter in absolute terms equals a line between the lower and upper part of the bunching segment, their bounding approach collapses to the trapezoid estimator of Saez (2010). Suppose the maximum slope is steeper than the trapezoid. In that case, the density within the bunching segment becomes thinner and necessitates a larger elasticity to rationalize the observed bunching (i.e., an upper bound). If the slope is flatter, the counterfactual density inside the bunching segment becomes thicker, and a smaller elasticity rationalizes the observed bunching (i.e., a lower bound). This bound can be assessed for different values of the slope parameter. When the maximum slope parameter is such that there is a zero mass point within the bunching segment, the bunching elasticity's upper bound equals infinity.

6.1 Quasi-Experimental Design

To assess the assignment of the information letters, we compare the earnings distributions of the informed and non-informed in 2014. Figure 7 illustrates that both groups display strong bunching behavior around the SGA threshold in 2014. The density of non-informed is somewhat less stable due to the smaller sample size, but otherwise tracks the earnings histogram of the informed sample reasonably well. While a Kolmogorov-Smirnov test of equality rejects the null hypothesis that raw earnings distributions are equal, we apply a weighting approach proposed by Kline (2011) that accounts for the pre-determined differences between the two groups. This approach adjusts each unit's importance to account for the fact that the two groups differ in some dimensions of our data.[27] Appendix Figure A.12 shows that the re-weighted earnings distribution tracks the pre-determined earnings distribution of the treatment group significantly better. Importantly, we are no longer able to reject the null-hypothesis that the two distributions are equal. The p-value of the Kolmogorov-Smirnov test is 0.4, and supports the assumption that conditional on the observed differences in characteristics, the assignment of the information letter was random.

Figure 7: Earnings Distributions Around the SGA Threshold in 2014: By Information Status

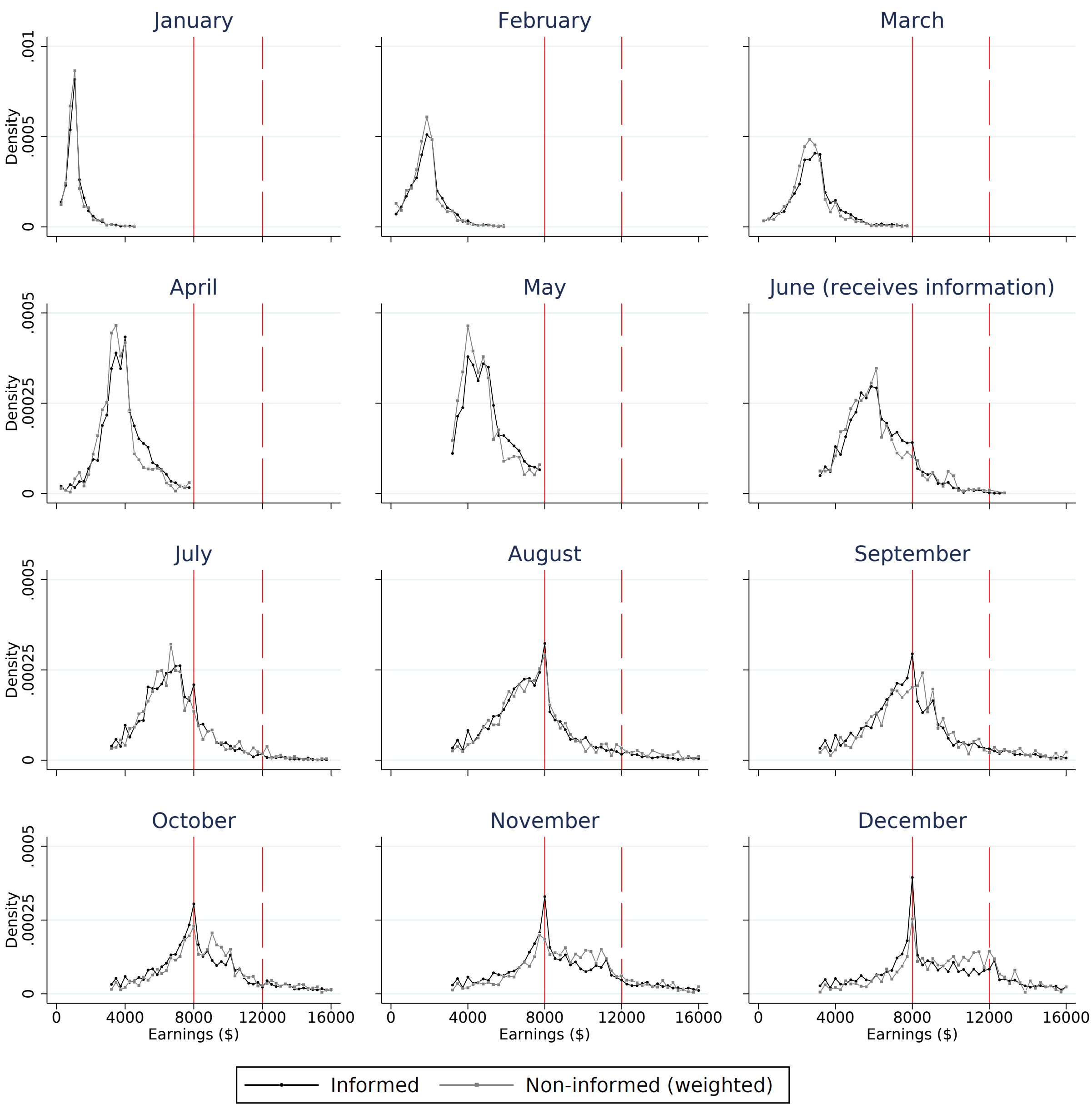


Notes: This figure shows the distribution of annual earnings in 2014 around the SGA threshold (marked by the red dashed line) in \$267 (2,000 NOK) bins by information status. The sample consists of fully disabled recipients awarded DI before the 1st of January 2015 who were eligible to receive the information letter (i.e., PAE above the cutoff). Earnings are measured in 2015 dollars (NOK/\$ = 7.5).

We take two additional steps to examine the validity of our empirical design. First, we assess the trends in average earnings of the two groups before the information event. Reassuringly, we are unable to detect any significant differences in the trend in earnings in the months leading up to the information letter. Appendix Figure A.14 illustrates the estimates. Second, we examine the distribution of cumulative earnings by each month in 2015 in Figure 8. The solid line represents the informed sample's earnings distributions, and the dashed line represents the non-informed sample. The two earnings distributions track each other well over the first five months of the year; in May, the month before the information letter was sent, none of the two groups show signs of bunching at the new kink.

[27] We implement this adjustment by estimating the probability a person is exposed to the letter with logistic regression using the characteristics listed in Appendix Table A.2. We then re-weight non-treated individuals using propensity score weights $w(x) = \frac{P(I=1|x)}{P(I=1)} \frac{1-P(I=1)}{1-P(I=1|x)}$ where $P(I=1)$ denotes the probability of receiving information treatment. To perform an adjusted test of equal earnings distributions, we have left out the earnings in 2014 and earnings from January to May of 2015 from estimating the propensity score.

Figure 8: Earnings Distributions in 2015: By Month and Information Status



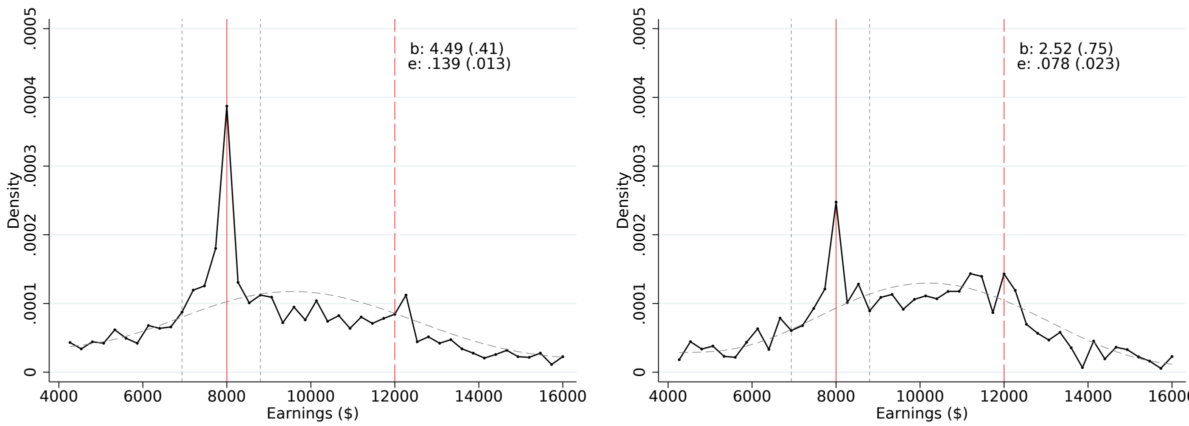
Notes: These figures show the distribution of earnings ($) for each month of 2015 in $267 (2,000 NOK) bins by information treatment status. Non-informed individuals are weighted by propensity score weights $w(x) = \frac{P(I=1|x)}{P(I=1)} \frac{1-P(I=1)}{1-P(I=1|x)}$ where $P(I = 1)$ denotes the probability of receiving information treatment. $P(I = 1|x)$ is estimated with a logit model using DI benefits, AIE, age, years on DI, gender, cohabitation status, number of children and years of schooling as control variables. The red solid line indicates the SGA threshold at $8,000, and the red dashed line indicates the SGA threshold in 2014 at approximately $12,000. The sample consists of fully disabled recipients awarded DI before the 1st of January 2015 who were eligible to receive the information treatment (PAE above the cutoff). Earnings are measured in 2015 dollars (NOK/$ = 7.5).

6.2 The Effects of the Information Letter on Bunching Behavior

Figure 8 examines the within calendar-year variation in the earnings distributions of the informed and non-informed. In August, two months after the letter was received, both earnings distributions display clear bunching at the kink but otherwise remain similar. After this point, we see that bunching among the informed grows slightly, whereas the earnings distribution among the non-informed is gradually shifting to the right. By the end of the year, the two distributions differ primarily in the region between the new kink and old SGA threshold. Next, we apply the bunching approach from Section 5 to estimate the information letter's impacts on bunching behavior.[28] By comparing bunching at the end of the year, we can infer the extent to which the information letter led to sharper bunching. Figure 9 displays the distribution of cumulative earnings in December 2015 of the informed and non-informed, where the counterfactual distribution is obtained from fitted values from the polynomial approach, and is visualized by the gray dashed lines for the informed in the left panel and the non-informed sample in the right panel. The actual earnings distribution is illustrated by the solid lines, and shows that bunching at the new kink (i.e., vertical solid line) is significantly sharper among the treated than the non-treated whose earnings are more likely to remain around the old SGA threshold (i.e., the long-dashed line to the right).

Figure 9: Earnings Distributions Around the SGA Threshold in 2015

(a) Informed (b) Non-Informed (weighted)



Notes: Figure a shows the distribution of annual earnings in 2015 around the SGA threshold (marked by the red solid line, the red dashed line indicates the SGA threshold in 2014) in \$267 (2,000 NOK) bins for the sample of recipients (PAE above the cutoff) who received the information letter from SSA in June 2015. Figure b shows the earnings distribution in 2015 for eligible individuals who did not receive the information letter in June 2015, with the sample being weighed by propensity score weights $w(x) = \frac{P(I=1|x)}{P(I=1)} \frac{1-P(I=1)}{1-P(I=1|x)}$ where $P(I = 1)$ denotes the probability of receiving the information treatment. $P(I = 1|x)$ is estimated with a logit model using DI benefits, AIE, age, years on DI, gender, cohabitation status, number of children and years of schooling as control variables. In all figures, the gray dashed line illustrates a 7th degree polynomial fitted to the empirical distribution. The excluded bunching region is indicated by the vertical gray lines. The sample consists of fully disabled recipients awarded DI before the 1st of January 2015 who were eligible to receive the information letter (PAE above the cutoff). Earnings are measured in 2015 dollars (NOK/\$ = 7.5).

We summarize our evidence on bunching and elasticities for the two groups in Table 4. The first column shows that (normalized) bunching is highly statistically significant and relatively strong, equal to about 4.5 times the height of the density and an observed elasticity equal to 0.14.[29] The third column displays the estimated bunching and observed elasticity for the non-informed sample. While the bunching estimate is statistically significant, it is only half of the size of the estimate of the informed sample. The stark difference shows that the information letter led to stronger responsiveness to changes in financial incentives. Assuming the information letter did not affect the share of individuals with utility gains larger than the adjustment cost, the average misperception, $\bar{\theta}$, equals about 0.56 with a bootstrapped standard error of 0.18. This interpretation is consistent with the average part-time working DI recipient

[28]Note that we cannot use the non-parametric approach because of the sample of eligible recipients conditions on having earnings over a certain threshold in May 2015. If we used annual earnings in 2014 as a counterfactual, we would underestimate the height of the earnings density in 2015. Moreover, we are unable to reconstruct a comparable sample of eligible in 2014 due to missing data (i.e., the monthly earnings data was introduced in 2015).

[29]We get the elasticity by multiplying the normalized bunching estimate with the bin width and the normalized change in the net after-tax earnings and then divide by the kink location.

underestimating the tax rate by one-half or that nearly half of the recipients were inert to the new location of the kink (e.g. Jones, 2012 and Gelber *et al.*, 2019).

Table 4: Bunching Behavior With and Without Information Treatment

	A. Informed		B. Non-informed	
	(1)	(2)	(3)	(4)
Normalized Bunching ($B/\hat{h}_0$):	4.49 ***	4.85***	2.52***	1.88**
Standard error	(.42)	(.43)	(.75)	(.64)
Observed Bunching Elasticity:	.139***	.150***	.078***	.058***
Standard error	(.013)	(.013)	(.023)	(.020)
Counterfactual	Informed	Both	Non-informed	Both
Parametric Approach	Yes	Yes	Yes	Yes
Reweighed	No	No	Yes	Yes
Observations:	3,642	5,163	1,521	5,163

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are calculated by bootstrap using 500 replications.

Notes: This table presents estimates of excess mass for the sample of recipients who were eligible to receive the information treatment (PAE above the cutoff), separately for treated individuals (received information letter in June 2015) in columns 1 and 2, and untreated recipients (did not receive information letter) in columns 3 ad 4. Bunching behavior is estimated using bins of \$267 (2,000 NOK) and a 7th degree polynomial for the counterfactual density (see Table 3). Columns 3-4 report estimates after non-treated recipients are re-weighted using propensity score weights $w(x) = \frac{P(I=1|x)}{P(I=1)} \frac{1-P(I=1)}{1-P(I=1|x)}$, where $P(I=1)$ denotes the probability of receiving the information letter in June 2015 (see text for details). Columns 2 and 4 use the sample of eligible for the information letter to estimate the counterfactual density.

One concern with our approach is that any bias from estimating the two different counterfactual densities would bias our conclusion. However, under the assumption of conditional random assignment of the information letter, the two groups' counterfactual density should be the same. In the second and fourth columns, we estimate bunching behavior where we use both groups to estimate the counterfactual density. This specification ensures that any bias in the first step would be identical for the two groups and cancel out. Our estimates of bunching and the observed elasticity for the informed sample, reported in columns 1 and 2, are not significantly different. Similarly, the estimates reported in columns 3 and 4 are also not significantly different from each other. While the attenuation parameter is somewhat smaller, now at 0.39, we cannot reject the null hypothesis that it is equal to our baseline estimate of 0.56.[30] Taken together, the policy-induced difference in bunching lends support to models in public finance where behavioral elasticities are subject to policy control (see, e.g., Slemrod & Kopczuk, 2002 and Farhi & Gabaix, 2020).

6.3 Unpacking Optimization Frictions

What is the relative importance of misperceptions of tax and benefit schedules in shaping earnings responses? We assess this question by unpacking overall optimization frictions using a decomposition approach. In the first step of our approach, we estimate the fraction of nonoptimizers and the structural elasticity among our sample of eligible recipients who faced a notched budget set in 2014 (see details

[30]We perform additional specification checks on the estimated elasticities by changing the polynomial order. These specification checks are reported in Appendix Figure A.11 and confirm that the observed elasticity is stable.

in Appendix Figure A.13). The first column of Table 5 shows that the eligible sample appears to be somewhat more likely to optimize than the overall sample – reflected by the lower share of nonoptimizers and the higher observed elasticity. While the estimate of the average structural elasticity is somewhat higher for this sample, we lack the precision to reject that it is equal to the estimate from Section 5. In the second and third columns, we copy the observed elasticities for the non-informed from Table 4.

In the second step, we maintain the assumption that the structural elasticity is a deep parameter and is unaffected by the reform. The first column of Panel C reports the total attenuation as the difference between the observed elasticity in 2014 and the structural elasticity. Moving to columns 2 and 3, we see that the attenuation doubles from 0.15 to about 0.3 in 2015. The next row copies in the estimated effect of the information treatment on the observed elasticity, and the third column suggests that at least 30 percent of the total attenuation in earnings elasticities can be attributed to learning the *new* tax and benefit schedule. The remaining difference between the structural and the observed elasticity is attributed to other types of frictions. In our alternative specification, where we use the separate distributions to estimate the counterfactual density the conclusion remains unchanged. Still, it is worth noting that our evidence likely represents lower bounds of the true importance of information because, in practice, the information letters do not fully correct for misperceptions of the tax and benefit schedule.

Table 5: The Relative Importance of Information versus Other Types of Frictions

	A. Pre-reform	B. Post-reform	
	(1)	(2)	(3)
Share nonoptimizers:	.288***		
Standard error	(.032)		
Observed elasticity:	.204***	.078***	.058***
Standard error	(.070)	(.023)	(.020)
Structural elasticity:	.358***		
Standard error	(.093)		
	C. Decomposition of Attenuation		
Total Attenuation	.154	.280	.300
Attenuation: Information friction		.061	.092
Attenuation: Other types of frictions		.219	.208
Attenuation rate (%)	42.8	78.2	83.8
Information, % of total attenuation		21.8	30.7
Counterfactual	Eligible Notch	Non-informed	Both
Parametric Approach	Yes	Yes	Yes
Observations:	2,381	5,163	5,163

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Standard errors (in parentheses) are calculated by bootstrap using 500 replications.

Notes: Panel A presents estimates of the fraction of nonoptimizers, observed and structural elasticity in 2014 using the sample of recipients eligible to receive the information treatment (PAE above the cutoff). Bunching behavior is estimated using bins of $267 (2,000 NOK) and a 7th degree polynomial for the counterfactual density (see Table 3). Panel B copies in estimates from Table 4. Panel C reports the decomposition of total attenuation. Column 1 reports the difference between the observed elasticity in 2014 and the structural elasticity, and the attenuation rate, which is equal to the difference divided by the structural elasticity. Columns 2 and 3 report changes in the attenuation and decompose it into the part due to the information letter and other kinds of frictions as the residual.

We can similarly infer the contribution of misperceptions of the tax and benefit schedule to the increase in attenuation. To perform this calculation, we compare the change in total attenuation from 2014 to 2015, 0.30-0.154. We then relate our estimate from the previous section, 0.092 to this difference. We find that at least two-thirds of the increase in attenuation is due to informational frictions: .092/(.300-.154) $\approx$ 2/3. If we use the separate distributions to compute the counterfactual instead, we conclude that half of the increase in attenuation is due to misperceptions of the tax and benefit schedule. These calculations suggest that a reform that improves the financial work incentives by 10 percent would induce a 0.6 percent increase in labor supply. However, if the reform were accompanied by an information intervention and absent induced entry effects, the response would increase by 150 percent and substantially increase government revenue.

7 Assessing Margins of Response

This section presents regression evidence on the effects of the information letter and decomposes the effect into intensive and extensive margins of labor supply response.

7.1 Regression Model

To assess the margins of labor supply response, we estimate a standard event study specification

$$y_{it} = d_0 + d_1 I_i + \sum_{t=2}^{T=12} \gamma_t + \sum_{t=2}^{T=12} \delta_t I_{it} + \varepsilon_{it}, \tag{5}$$

where month effects are captured by γ_t, omitting January to allow for pre-existing differences between the two groups, and y_{it} is the outcome for a person at a given month in 2015. The indicator I_{it} is equal to one in month t if the person received the information letter and zero otherwise. The parameters of interests, the treatment-on-the-treated (TOT) effects are represented by δ_t for the period after the letter was received $t \in [6,12]$. The validity of our quasi-experimental design hinges on the parallel trend assumption. This assumption would be violated if, for example, worker outcomes or labor productivity grow at different rates by treatment status. Another concern is that treated DI recipients are employed by more technologically advanced firms that provide more information about their workers' tax and benefit system. This behavior would lead to different trends in earnings, even without the information treatment.[31] Fortunately, our data allows us to assess the parallel trend assumption by inspecting the estimates of δ_t for $t \in [2,5]$.

In our baseline specification, we impose a common effect over months within a calendar year but allow the effect to differ by year to test for persistence in the effect of the information treatment. Our estimating equation is given by

$$y_{it} = d_0 + d_1 I_i + \gamma_0 Post_{i,15} + \gamma_1 Post_{i,16} + \delta_0 I_i \cdot Post_{i,15} + \delta_1 I_i \cdot Post_{i,16} + \varepsilon_{it}. \tag{6}$$

where $Post_{i,15}$ is an indicator variable equal to one if the observation is in June to December in 2015,

[31] Note that such effects would arguably be present before the reform and assessed by inspecting the earnings distribution in 2014. More information transmitted from employers to workers should lead to stronger bunching. The data do not support this concern, and we are unable to reject the null hypothesis of equal distributions with a p-value of 0.4 from Kolmogorov-Smirnov tests (see Appendix Figure A.12).

and $Post_{i,16}$ equals one if the observation in from 2016. The effect of the information treatment in 2015 is given by δ_0, and the effect of the treatment in 2016 is given by δ_1. Throughout this section, all standard errors are clustered at the individual level and are robust to heteroskedasticity.

7.2 Evidence

A virtue of the event study design is that it provides a transparent way of showing how we identify the labor supply responses. To this end, we plot the estimated coefficients from the distributed lag model in equation 5 in Appendix Figure A.14. Reassuringly, we are unable to detect statistically significant differences in monthly earnings prior to the letter. There is a sharp fall in monthly earnings after the letter is sent, consistent with the evidence from the bunching analysis in the previous section.

We next turn to estimates from our baseline specification. Panel A of Table 6 reports the estimated effect of treatment in 2015. The second column presents the baseline estimate of how the information letter impacts monthly earnings. The average treatment effect on the treated equals about \$200. This effect is highly statistically significant. It corresponds to a 25 percent decrease in monthly earnings and suggests that individuals can adjust their labor supply over the remaining six months of the year. Panel B shows the effect of the treatment persisted in 2016. The estimate is somewhat smaller than the estimated effect in 2015 but still corresponds to 18 percent of the average monthly earnings in January-May of 2015. This finding suggests that our findings do not reflect the effects of a nudge that temporarily induced treated individuals to move away from the status quo (see, e.g., Levitt, 2020). To assess whether

Table 6: Effect of Information Treatment on Labor Market Outcomes

		Effects on Labor Supply					
	Probability to Switch Firm (1)	Monthly earnings (\$) (2)	Any earnings (3)	Fixed pay (\$) (4)	Variable pay (\$) (5)	Contract hours (6)	Observations <individuals> (7)
A. Post-Treatment, 2015	-.009*	-195***	-.086***	-28***	-113***	-1.2***	61,956
	(.005)	(21)	(.010)	(11)	(16)	(.2)	<5,163>
p-value	.079	<.001	<.001	.009	<.001	<.001	
B. Post-Treatment, 2016	-.019**	-142***	-.071***	-25*	-68***	-.79**	123,912
	(.008	(22)	(.012)	(14)	(16)	(.34)	<5,163>
p-value	.020	<.001	<.001	.063	<.001	.018	
Dep. mean (Pre-Treatment)	.04	789	.690	235	430	8.76	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Standard errors (in parentheses) are clustered at the individual level and are robust to heteroskedasticity.

Notes: This table presents estimates of the impacts of the information letter on labor market outcomes during 2015. The event study is implemented using a difference in difference design, where we include a dummy for the treatment group, a dummy for the post-treament time period for 2015, a dummy for the post-treatment time period for 2016, and an interaction between the treatment indicator and the post-treatment period for each year respectively. Non-treated units are reweighed using propensity score weights $w(x) = \frac{P(I=1|x)}{P(I=1)} \frac{1-P(I=1)}{1-P(I=1|x)}$ where $P(I = 1)$ denotes the probability of receiving the information treatment, and $P(I = 1|x)$ is estimated by logit regression using the characteristics from Table 2. Fixed pay is defined by the work contract. Variable pay is defined as wages paid on an hourly basis, bonuses, overtime pay, or other payments as defined by employers. Switching employer is an indicator variable equal to one if the current employer is different than the main employer at the beginning of the calendar year. The sample consists of fully disabled recipients awarded DI before the 1st of January 2015 who were eligible to receive the information treatment. Earnings are measured in 2015 dollars (NOK/\$ = 7.5). The sample is restricted to individuals that are observed in the monthly earnings data. Dependent mean (control group) is measured before treatment.

the effects are coming from existing employment relationships or from new employers who permit fine-tuning their labor supply, the first column reports the estimated impact on the probability of switching employers. While the estimate is less precisely estimated, it suggests that adjustments in existing employment relationships primarily explain the response.

Using the detailed data on earnings and hours reported by the employer, we decompose total earnings into fixed and variable pay and contracted hours. As only one percent of our sample has any income from self-employment, the scope for reporting behavior to be an important driver of our results is very limited. Relevant margins of response are changing number of hours worked – either at the intensive margin of hours or by stopping work entirely. We begin by estimating the effect of the information letter on the probability of being observed in a given month with zero earnings. The third column of Table 6 reports the estimated impacts of the information letter on having any earnings in a given month. The point estimate shows a statistically significant decrease of 8.6 percent in 2015 and 7.1 percent in 2016. This finding suggests that the information experiment led to a 10-12 percent reduction in the extensive margin of labor supply, indicating that the extensive margin may explain half of the total effect on earnings.[32] We further decompose total earnings into fixed pay (e.g. according to the work contract) and variable pay that includes wages on an hourly basis, bonus and overtime payments.[33] The estimates reported in columns 4 and 5 show that variable pay reductions are the primary reason why earnings fall. By contrast, we find a small, but still statistically significant reduction in fixed pay. As expected, the sixth column shows the information letter lead to a reduction in the average number of hours worked. The point estimate shows a statistically significant decrease in contracted hours by 1.2 hours, equal to a 16 percent relative to the dependent mean. Taken together, our evidence supports the view that informational frictions attenuate earnings and that the effects of the information letter reflect real labor supply responses.

Finally, we address the concern that our conclusions may be specific to our setting. Hours constraints, for example, could be less binding among part-time than among full-time workers. We assess whether the response varies across industries with varying flexibility levels. Appendix Table A.3 illustrates that we are unable to detect significant differences in treatment effects, lending some support to our analysis's external validity.

8 Conclusion

A growing line of empirical research has stressed the importance of optimization frictions in attenuating earnings responses to financial incentives. While these frictions may arise from real constraints such as hours of work restrictions or the tax and benefit schedule not being fully understood, to date, evidence on the role of factors shaping the response is scant. This lack of evidence is unfortunate and limits the practical implications of optimization frictions for optimal tax and transfer policies. The main reason so little is known is that labor supply elasticities are attenuated by a variety of frictions that are unobserved to the econometrician.

This paper aimed to help fill this literature gap by assessing how information about the tax and

[32]The average earnings among those who work is $789/.690. The extensive margin effect can thus account for 0.086*$1143=$98 of the total effect on monthly earnings.

[33]Total earnings also include other variable and irregular income sources such as severance pay, summer salary, and other taxable employment benefits such as employer-paid phone.

benefit schedule shapes earnings elasticities. Two features from the Norwegian setting allowed us to make progress. The first was policy-induced changes in the existence of notches in the tax and benefit schedule. A key feature under a notch regime is that an incremental change in earnings causes discrete changes in the level of net tax liability. Exploiting the regions of dominated choice in combination with individual earnings, we found that about half of DI recipients did not behave as predicted by standard labor supply models, which led to substantial attenuation in the observed elasticity of labor supply. Our setting's second key feature was an informational quasi-experiment that targeted recipients' perceptions about the location and slope of a new kink. We found the information letter lead to a doubling in the bunching probability compared to the non-treated, indicating that policymakers have some control over behavioral elasticities. We unpacked overall optimization frictions by combining the excess bunching with and without the informational treatment and dominated behavior under the notch. We found that a substantial fraction of attenuation in the response to financial incentives can be attributed to informational frictions.

Our paper made progress in understanding the factors that shaped the earnings response and has several policy implications. The first is to show that governments can shape earnings responses with targeted information letters, and the second to decompose overall optimization frictions into information vs. other behavioral frictions. This decomposition approach may be applied and prove useful in other settings. Access to experimental variation in information (or other measurable barriers to optimal choice) and dominated choice (like a notched budget set) allows for evaluating the costs and benefits of information intervention. When policymakers exert some control over behavioral responses, as we document they do, the additional policy-levers may help reduce the efficiency losses from redistribution and even strengthen the insurance from programs such as the DI system.

References

ABELER, JOHANNES, & JÄGER, SIMON. 2015. Complex tax incentives. *American Economic Journal: Economic Policy*, **7**(3), 1–28.

ALTONJI, JOSEPH G, & PAXSON, CHRISTINA H. 1988. Labor supply preferences, hours constraints, and hours-wage trade-offs. *Journal of labor economics*, **6**(2), 254–276.

ATKINSON, A., RAINWATER, L., & SMEEDING, T.M. 1995. Income Distributions in OECD countries: Evidence from the Luxembourg Income Study. *OECD Publications and Information Center*.

AUTOR, D. H., & DUGGAN, M. G. 2006. The growth in the social security disability rolls: A fiscal crisis unfolding. *Journal of Economic Perspectives*, **20**(3), 71–96.

BASTANI, SPENCER, & SELIN, HÅKAN. 2014. Bunching and non-bunching at kink points of the Swedish tax schedule. *Journal of Public Economics*, **109**, 36–49.

BERTANHA, MARINHO, MCCALLUM, ANDREW H, & SEEGERT, NATHAN. 2019. Better bunching, nicer notching. *Nicer Notching (December 23, 2019)*.

BEST, MICHAEL, CLOYNE, JAMES, ILZETZKI, ETHAN, & KLEVEN, HENRIK JACOBSEN. 2019. Interest rates, debt and intertemporal allocation: evidence from notched mortgage contracts in the United Kingdom. *Review of Economic Studies*.

BEST, MICHAEL CARLOS, & KLEVEN, HENRIK JACOBSEN. 2017. Housing market responses to transaction taxes: Evidence from notches and stimulus in the UK. *The Review of Economic Studies*, **85**(1), 157–193.

BEST, MICHAEL CARLOS, BROCKMEYER, ANNE, KLEVEN, HENRIK JACOBSEN, SPINNEWIJN, JOHANNES, & WASEEM, MAZHAR. 2015. Production versus revenue efficiency with limited tax capacity: theory and evidence from Pakistan. *Journal of political Economy*, **123**(6), 1311–1355.

BHARGAVA, SAURABH, & MANOLI, DAYANAND. 2015. Psychological frictions and the incomplete take-up of social benefits: Evidence from an IRS field experiment. *The American Economic Review*, **105**(11), 3489–3529.

BLOMQUIST, SÃ¶REN, NEWEY, WHITNEY, KUMAR, ANIL, & LIANG, CHE-YUAN. 2019. On Bunching and Identification of the Taxable Income Elasticity.

BLUNDELL, RICHARD, & MACURDY, THOMAS. 1999. Labor Supply: A Review of Alternative Approaches. Handbook of Labor Economics, Elsevier.

CENGIZ, DORUK, DUBE, ARINDRAJIT, LINDNER, ATTILA, & ZIPPERER, BEN. 2019. *The effect of minimum wages on low-wage jobs: Evidence from the United States using a bunching estimator*. Tech. rept. National Bureau of Economic Research.

CHETTY, RAJ. 2012. Bounds on elasticities with optimization frictions: A synthesis of micro and macro evidence on labor supply. *Econometrica*, **80**(3), 969–1018.

CHETTY, RAJ, & SAEZ, EMMANUEL. 2013. Teaching the tax code: Earnings responses to an experiment with EITC recipients. *American Economic Journal: Applied Economics*, **5**(1), 1–31.

CHETTY, RAJ, LOONEY, ADAM, & KROFT, KORY. 2009. Salience and taxation: Theory and evidence. *American economic review*, **99**(4), 1145–77.

CHETTY, RAJ, FRIEDMAN, JOHN, OLSEN, TORE, & PISTAFERRI, LUIGI. 2011. Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records. *Quarterly Journal of Economics*, **126**, 749–804.

CHETTY, RAJ, FRIEDMAN, JOHN N, & SAEZ, EMMANUEL. 2013. Using Differences in Knowledge across Neighborhoods to Uncover the Impacts of the EITC on Earnings. *The American Economic Review*, **103**(7), 2683–2721.

DELLAVIGNA, STEFANO. 2009. Psychology and economics: Evidence from the field. *Journal of Economic literature*, **47**(2), 315–72.

DICKENS, WILLIAM T, & LUNDBERG, SHELLY. 1993. Hours Restrictions and Labor Supply. *International Economic Review*, **34**(1), 169–92.

FARHI, EMMANUEL, & GABAIX, XAVIER. 2020. Optimal taxation with behavioral agents. *American Economic Review*, **110**(1), 298–336.

FINKELSTEIN, AMY. 2009. E-ztax: Tax salience and tax rates. *The Quarterly Journal of Economics*, **124**(3), 969–1010.

GELBER, ALEXANDER M, JONES, DAMON, & SACKS, DANIEL W. 2019. Estimating Earnings Adjustment Frictions: Method and Evidence from the Earnings Test. *American Economic Journal: Applied Economics*.

GOLDIN, JACOB, & HOMONOFF, TATIANA. 2013. Smoke gets in your eyes: cigarette tax salience and regressivity. *American Economic Journal: Economic Policy*, **5**(1), 302–36.

HARASZTOSI, PÉTER, & LINDNER, ATTILA. 2019. Who Pays for the minimum Wage? *American Economic Review*, **109**(8), 2693–2727.

HASTINGS, JUSTINE S, & WEINSTEIN, JEFFREY M. 2008. Information, school choice, and academic achievement: Evidence from two experiments. *The Quarterly journal of economics*, **123**(4), 1373–1414.

JENSEN, ROBERT. 2010. The (perceived) returns to education and the demand for schooling. *The Quarterly Journal of Economics*, **125**(2), 515–548.

JONES, DAMON. 2010. Information, Preferences, and Public Benefit Participation: Experimental Evidence from the Advance EITC and 401 (k) Savings. *American Economic Journal: Applied Economics*, **2**(2), 147–163.

JONES, DAMON. 2012. Inertia and overwithholding: explaining the prevalence of income tax refunds. *American Economic Journal: Economic Policy*, **4**(1), 158–185.

KLEVEN, HENRIK. 2019. *The EITC and the Extensive Margin: A Reappraisal*.

KLEVEN, HENRIK J., & WASEEM, MAZHAR. 2013. Using Notches to Uncover Optimization Frictions and Structural Elasticities: Theory and Evidence from Pakistan. *Quarterly Journal of Economics*, **128**, 669–723.

KLEVEN, HENRIK JACOBSEN. 2016. Bunching. *Annual Review of Economics*, **8**, 435–464.

KLINE, PATRICK. 2011. Blinder-Oaxaca as a reweighting estimator. *American Economic Review: Papers and Proceedings*, **101**(3), 532–537.

KLINE, PATRICK, & TARTARI, MELISSA. 2016. Bounding the labor supply responses to a randomized welfare experiment: A revealed preference approach. *American Economic Review*, **106**(4), 972–1014.

KOPCZUK, WOJCIECH, & MUNROE, DAVID. 2015. Mansion tax: the effect of transfer taxes on the residential real estate market. *American economic Journal: economic policy*, **7**(2), 214–57.

KOSTOL, ANDREAS RAVNDAL, & MOGSTAD, MAGNE. 2014. How Financial Incentives Induce Disability Insurance Recipients to Return to Work. *American Economic Review*, **104**(2), 624–55.

LE MAIRE, DANIEL, & SCHJERNING, BERTEL. 2013. Tax bunching, income shifting and self-employment. *Journal of Public Economics*, **107**, 1–18.

LEVITT, STEVEN D. 2020. Heads or Tails: The Impact of a Coin Toss on Major Life Decisions and Subsequent Happiness. *The Review of Economic Studies*, 05. rdaa016.

LIEBMAN, JEFFREY B., & LUTTMER, ERZO F.P. 2012. Would People Behave Differently If They Better Understood Social Security? Evidence From a Field Experiment. *American Economic Journal: Economic Policy*, **7(1)**(17287), 275–99.

MORRISON, WILLIAM, & TAUBINSKY, DMITRY. 2019. *Rules of Thumb and Attention Elasticities: Evidence from Under-and Overreaction to Taxes*. Tech. rept. National Bureau of Economic Research.

PENCAVEL, JOHN. 1986. Labor supply of men: a survey. *Handbook of labor economics*, **1**, 3–102.

REES-JONES, ALEX, & TAUBINSKY, DMITRY. 2019. Measuring Schmeduling. *The Review of Economic Studies*.

RUH, PHILIPPE, & STAUBLI, STEFAN. 2019. Financial incentives and earnings of disability insurance recipients: Evidence from a notch design. *American Economic Journal: Economic Policy*, **11**(2), 269–300.

SAEZ, EMMANUEL. 2010. Do taxpayers bunch at kink points? *American economic Journal: economic policy*, **2**(3), 180–212.

SAEZ, EMMANUEL, SLEMROD, JOEL, & GIERTZ, SETH H. 2012. The elasticity of taxable income with respect to marginal tax rates: A critical review. *Journal of economic literature*, **50**(1), 3–50.

SCHIMMEL, JODY, STAPLETON, DAVID C, & SONG, JAE G. 2011. How Common is Parking among Social Security Disability Insurance Beneficiaries-Evidence from the 1999 Change in the Earnings Level of Substantial Gainful Activity. *Soc. Sec. Bull.*, **71**, 77.

SLEMROD, JOEL. 1994. Fixing the leak in Okun's bucket optimal tax progressivity when avoidance can be controlled. *Journal of Public Economics*, **55**(1), 41–51.

SLEMROD, JOEL, & KOPCZUK, WOJCIECH. 2002. The optimal elasticity of taxable income. *Journal of Public Economics*, **84**(1), 91 – 112.

TAUBINSKY, DMITRY, & REES-JONES, ALEX. 2017. Attention variation and welfare: theory and evidence from a tax salience experiment. *The Review of Economic Studies*, **85**(4), 2462–2496.

WEATHERS, ROBERT R, & HEMMETER, JEFFREY. 2011. The impact of changing financial work incentives on the earnings of Social Security Disability Insurance (SSDI) beneficiaries. *Journal of Policy Analysis and Management*, **30**(4), 708–728.

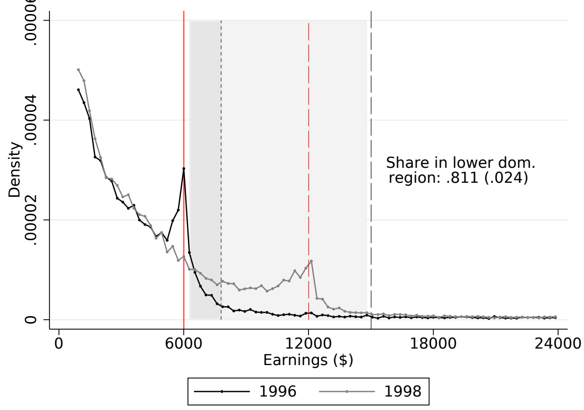
ZARESANI, AREZOU. 2020. Adjustment Costs and Incentives to Work: Evidence from a Disability Insurance Program. *Journal of Public Economics*, **188**(104223).

For Online Publication:

Appendix

A Additional Tables and Figures

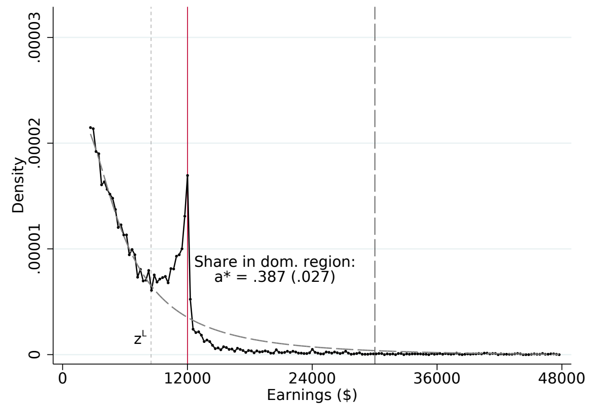
Figure A.1: Nonoptimizers in Lower Range of Dominated Region



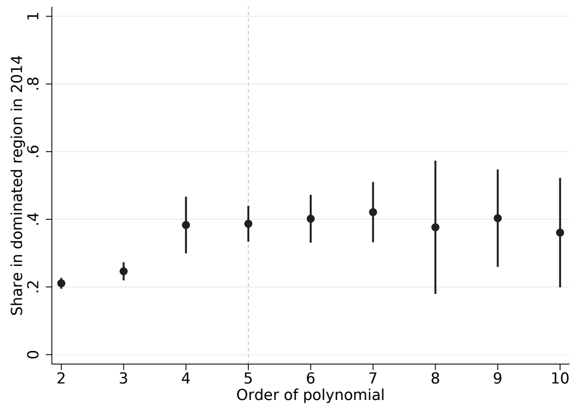
Notes: The figure illustrates the earnings distributions of DI recipients in 1996 and 1998. The dark shaded region and short dashed lines denote the lower part of the dominated region in which we can identify the fraction of nonoptimizers for in 1996. Our non-parametric approach covers 20 percent of the dominated region using 1998 as a counterfactual. The light-gray shaded region and long-dashed line denote the full dominated region in 1996. The sample consists of DI recipients with full (100 percent) benefit at the beginning of the calendar year during the period 1996 to 1998. Earnings are adjusted using the average wage growth, and are reported in 2015 dollars (NOK/$ = 7.5).

Figure A.2: Fraction of Nonoptimizers in 2014

(a) Nonoptimizers Over Full Dominated Range

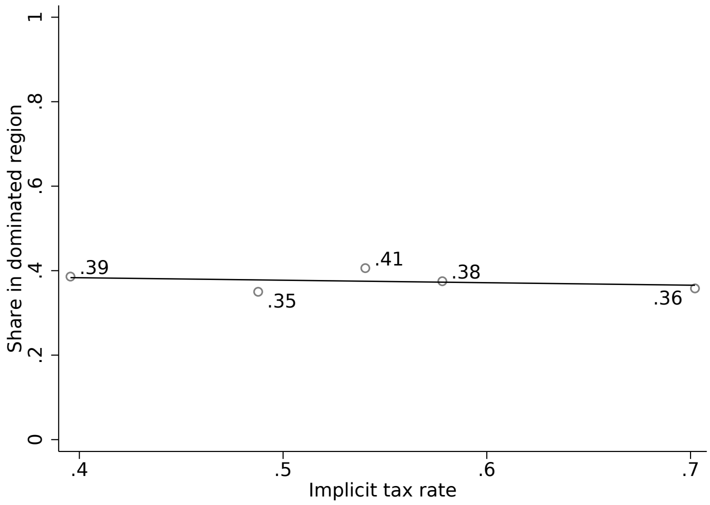


(b) Nonoptimizers By Polynomial Order



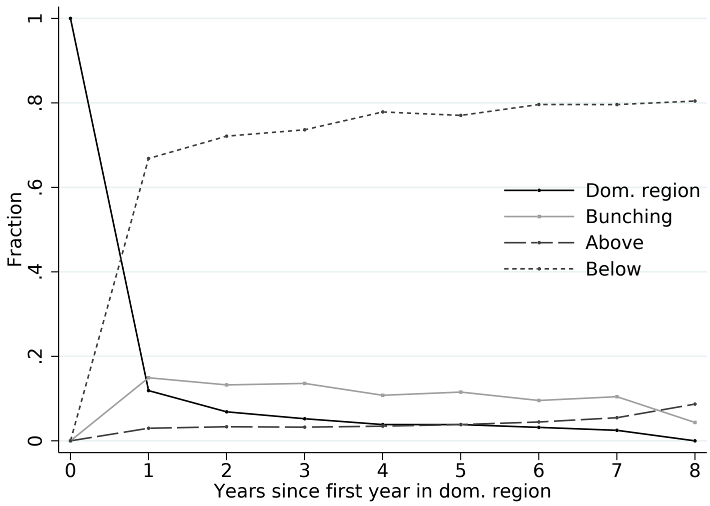
Notes: Figure a illustrates the earnings distributions of DI recipients in 2014 and the fitted counterfactual density using the polynomial approach. The polynomial order is chosen to minimize the distance between the estimate of nonoptimizers from the non-parametric approach and the parametric approach for the lowest range of the dominated region (see Figure 3a). The long-dashed line denote the full dominated region, the red line denotes the SGA threshold in 2014. The short dashed line represent the lower part of the excluded range. Figure b shows the sensitivity of the estimated fraction of nonoptimizers. The sample consists of DI recipients with full (100 percent) benefit at the beginning of 2014 and an award date from after January 1st, 2004. Earnings are adjusted using the average wage growth, and are reported in 2015 dollars (NOK/$ = 7.5).

Figure A.3: Dominated Behavior and Strength of Notch Incentives



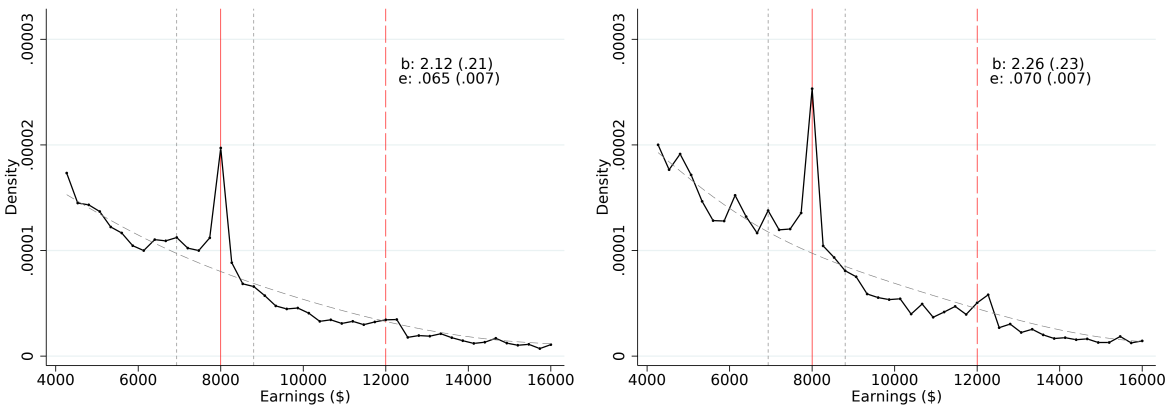
Notes: This figure plots subsample estimates of the fraction of nonoptimizers among DI recipients in 2014 using the polynomial approach (see Figure 4). The implicit tax rate is calculated using the benefit level over average indexed earnings. The sample consists of DI recipients with full (100 percent) benefit at the beginning of 2014, where the award date is after January 1st, 2004.

Figure A.4: Dynamics of Dominated Behavior Over Time



Notes: The figure illustrates the dominated behavior of individuals relative to the first year a person is observed with earnings in the dominated region. The sample consists of DI recipients with full (100 percent) benefit at the beginning of the calendar year and an award date from after January 1st, 2004.

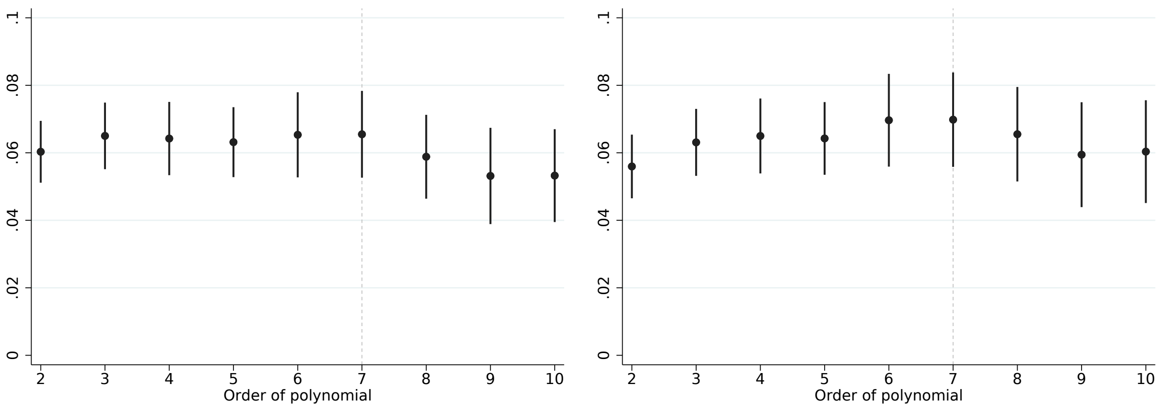
Figure A.5: Polynomial Approach: Bunching Elasticity in 2015



(a) Notch sample: Bunching Elasticity in 2015 (b) Kink sample: Bunching Elasticity in 2015

Notes: Figure a illustrates the earnings distributions in 2015 of DI recipients awarded benefits prior to January 1, 2004 and the fitted counterfactual density using the polynomial approach. Figure b illustrates the earnings distributions in 2015 of DI recipients awarded benefits after January 1, 2004 and the fitted counterfactual density using the polynomial approach. The red (dashed) line denotes the old (new) SGA threshold. The polynomial order is chosen to minimize the distance between the estimate from the non-parametric approach and the parametric approach (see Figure A.5). The sample consists of DI recipients with full (100 percent) benefit at the beginning of 2015. Earnings are adjusted using the average wage growth, and are reported in 2015 dollars (NOK/$ = 7.5).

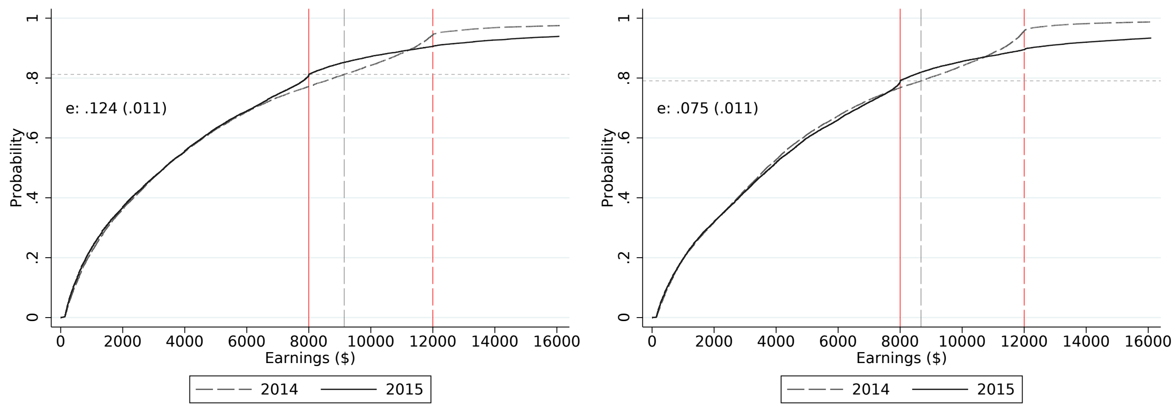
Figure A.6: Specification Checks for Polynomial Approach: Bunching Elasticity in 2015



(a) Notch sample: Sensitivity of Bunching Elasticity in 2015 (b) Kink sample: Sensitivity of Bunching Elasticity in 2015

Notes: Figure a illustrates the estimated bunching elasticity in 2015 for DI recipients awarded benefits prior to January 1, 2004. Figure b illustrates the earnings distributions in 2015 of DI recipients awarded benefits after January 1, 2004 and the fitted counterfactual density using the polynomial approach. The dots represent point estimates, and the vertical lines represent 95 percent confidence intervals. The x-axis represent different specifications for the polynomial order in equation 3. The two samples consist of DI recipients with full (100 percent) benefit at the beginning of 2015. Earnings are adjusted using the average wage growth, and are reported in 2015 dollars (NOK/$ = 7.5).

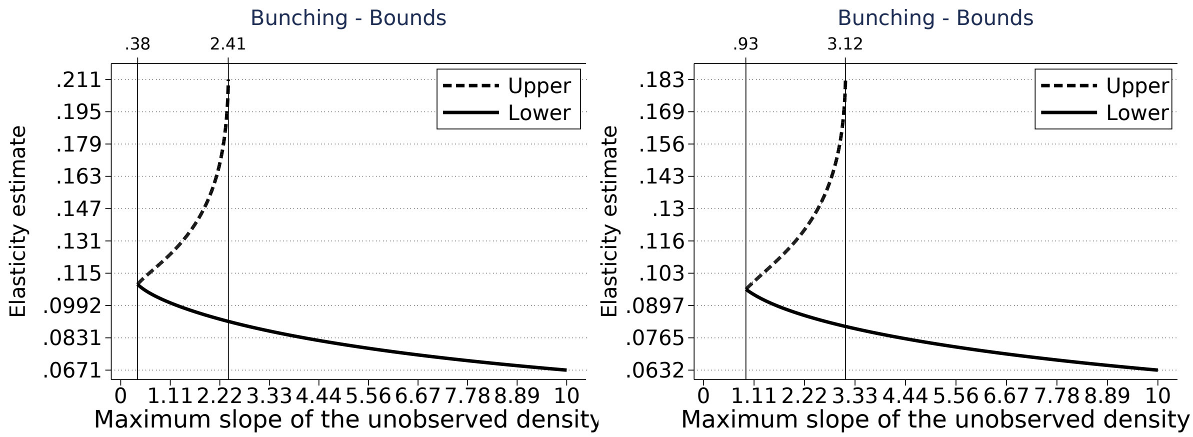
Figure A.7: CDF Approach: Bunching Elasticity in 2015



(a) Notch sample: Bunching Elasticity in 2015 (b) Kink sample: Bunching Elasticity in 2015

Notes: Figure a illustrates the cumulative earnings distributions in 2014 and 2015 for DI recipients with awards from before January 1, 2004 (i.e.,. notch sample), and Figure b illustrates the cumulative earnings distributions in 2014 and 2015 for DI recipients with awards from after January 1, 2004 (i.e.,. kink sample). The red (dashed) line denotes the old (new) SGA threshold. The gray dashed line represent the earnings response, and is inferred from the earnings level in 2014 that has the same CDF as the CDF at the SGA threshold in 2015, and subtracting off the SGA amount. The response is calculated as the difference $F_{14}^{-1}(F_{15}(\$8,000)) - \$8,000$ (see Blomquist *et al.* (2019) for a detailed description of the approach). The sample consists of DI recipients with a full (100 percent) benefit during 2014 and 2015. Earnings are adjusted using the average wage growth, and are reported in 2015 dollars (NOK/$ = 7.5).

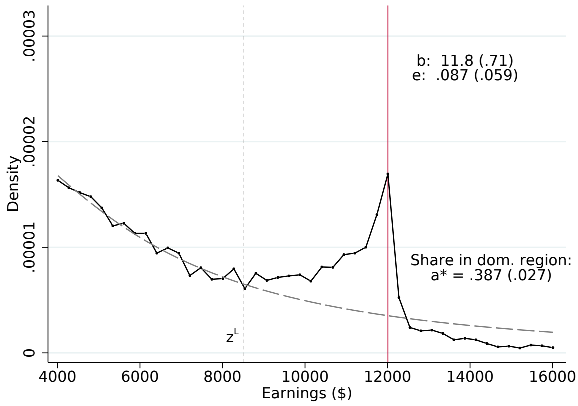
Figure A.8: Bounds on the Bunching Elasticity in 2015



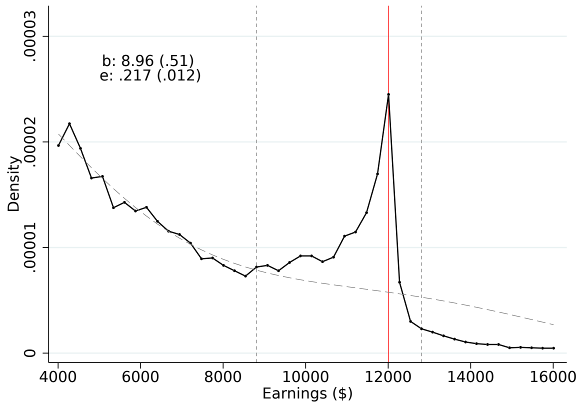
(a) Notch sample: Bunching Bounds in 2015 (b) Kink sample: Bunching Bounds in 2015

Notes: Figure a illustrates upper- and lower bounds for the bunching elasticity for DI recipients with awards from before January 1, 2004 (i.e.,. notch sample), and Figure b illustrates upper- and lower bounds for the bunching elasticity for DI recipients with awards from after January 1, 2004 (i.e.,. kink sample). Earnings is transformed by the natural logarithm, and the earnings distribution is filtered in the bunching segment (from ln(8,000) - 0.12 to ln(8,000)+0.12). We then vary the maximum slope parameter from 0 to 10 to identify the bounds (see Theorem 2 in Bertanha *et al.* (2019) for a detailed description). The vertical lines denote the minimum and maximum slope of the unobserved heterogeneity. The line to the left is the smallest slope that allows a continuous probability density function (PDF) to be consistent with both the bunching mass and observed income distribution. The right line is the maximum slope before the set of possible distributions allows for a PDF that equals zero in the bunching interval (see Figure 1 in Bertanha *et al.* (2019)). The sample consists of DI recipients with a full (100 percent) benefit during 2015.

Figure A.9: Polynomial Approach: Bunching Elasticity in 2014



(a) Notch sample: Bunching Elasticity in 2014



(b) Kink sample: Bunching Elasticity in 2014

Notes: Figure a illustrates the earnings distributions in 2014 of DI recipients awarded benefits prior to January 1, 2004 and the fitted counterfactual density using the polynomial approach. Figure b illustrates the earnings distributions in 2014 of DI recipients awarded benefits after January 1, 2004 and the fitted counterfactual density using the polynomial approach. The red line denotes the SGA threshold in 2014. The polynomial order is chosen to minimize the distance between the estimate from the non-parametric approach and the parametric approach in 2015 (see Figure A.9). The sample consists of DI recipients with full (100 percent) benefit at the beginning of 2015. Earnings are adjusted using the average wage growth, and are reported in 2015 $.

Figure A.10: Information letter

Returadresse,
NAV HAUGESUND POSTBOKS 353 5501 HAUGESUND

Dato: 11. juni 2015

NAV har mottatt opplysninger om inntekten din

Fødselsnummer: Saksreferanse:

Opplysninger fra arbeidsgiver viser at den årlige arbeidsinntekten din kan bli høyere enn inntekten vi brukte til å beregne utbetalingen av uføretrygden din.

Dersom inntekten din i år overstiger 174 926 kroner skal utbetalingen av uføretrygden reduseres. Det lønner seg likevel å jobbe, fordi inntekt og uføretrygd vil være høyere enn uføretrygd alene. Selv om vi reduserer uføretrygden din, beholder du uføregraden du har fått innvilget.

Dette kan du gjøre

Vi sender deg dette brevet slik at du så tidlig som mulig kan melde fra om eventuell ny årlig forventet inntekt.

Forventer du at inntekten din i år ikke overstiger 174 926 kroner, trenger du ikke gjøre noe.

Dersom du forventer at inntekten i år overstiger 174 926 kroner, må du melde fra om den nye inntekten din. Dette kan du gjøre under menyvalget «Uføretrygd» når du logger deg inn på nav.no. Det er viktig at du melder fra om ny årlig inntekt slik at du får riktig utbetaling av uføretrygd.

NAV Forvaltning

Postadresse: NAV HAUGESUND // POSTBOKS 353 // 5501 HAUGESUND PE_UT_04_008
Besøksadresse: ØVREGATA 126 // 5527 HAUGESUND
Tel: 55 55 33 33
nav.no

Dette kan NAV gjøre

Dersom opplysninger fra arbeidsgiver på et senere tidspunkt viser at inntekten din har oversteget 174 926 kroner, reduserer vi uføretrygden. Dette kan imidlertid være for sent til at du får riktig årlig utbetaling av uføretrygden din.

Du vil motta vedtaksbrev fra NAV dersom vi justerer uføretrygden din på grunn av endret inntekt.

Dersom du har fått for lite eller for mye utbetalt i uføretrygd, foretar vi et etteroppgjør. Dette gjør vi på høsten når likningen for året før er klar.

Har du spørsmål?

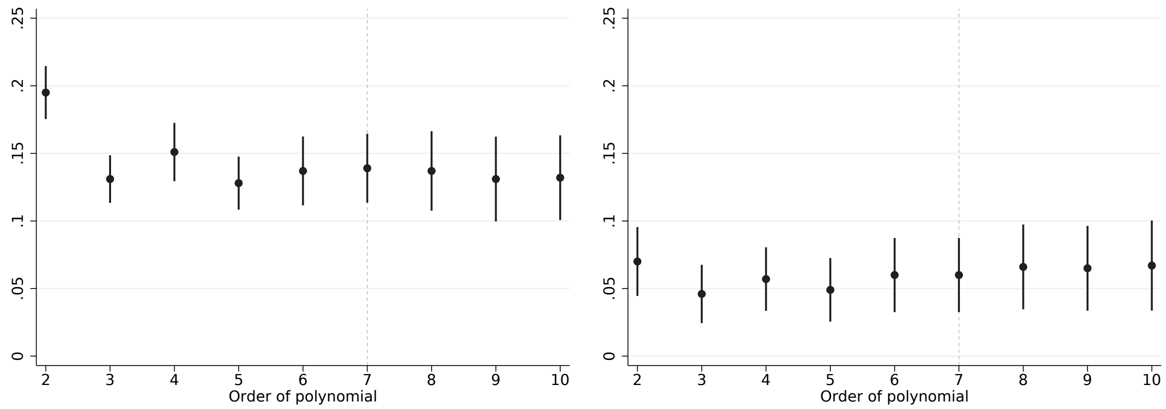
Kontakt oss gjerne på telefon 55 55 33 33. Du må alltid oppgi fødselsnummeret ditt når du tar kontakt med NAV. Da kan vi lettere gi deg rask og god hjelp.

Med vennlig hilsen
NAV Forvaltning

Side 2 av 2

Notes: This is an example of an anonymized letter (in Norwegian) for a recipient working part time. **"NAV/SSA has received information about your income".** Personal identifier (removed), Case file (removed). 1st paragraph: The information from your employer shows that the annualized labor earnings can be higher than the earnings we used to calculate your benefit level. Second paragraph: If your earnings this year exceed X kroner, your benefit payments will be reduced. It still pays to work more, as earnings and benefits are higher than benefits alone. Even if we reduce your benefits, you will keep the degree of disability you were awarded. **"This is what you can do"**. We send you this letter so you can prepare, and notify us as soon as you expect your annual income to increase. Fourth paragraph: If you expect you annual earnings to not exceed X, you do not need to do anything. Fifth paragraph: If you expect you earnings to exceed X kroner, you will have to report your new earnings. You can do this under the choice "disability benefits" when logging in to nav.no. It is important that you report your expected annual earnings to receive the correct amount of benefits. Page 2, first paragraph: **"This is what NAV can do"**. If the information provided by your employer at a later date shows that your earnings exceeded X, we will reduce your benefit payments. It may be too late to make sure the annual payment is correct. Second paragraph: Du will receive a letter from NAV if we reduce your benefits due to the changes in the level of earnings. Third paragraph: If you have received too many or too few benefits, we will make an after settlement. **"Do you have any questions?"** Contact us at phone number Y. You will have to provide your personal identifier when contacting NAV. That way we can provide good and fast assistance.

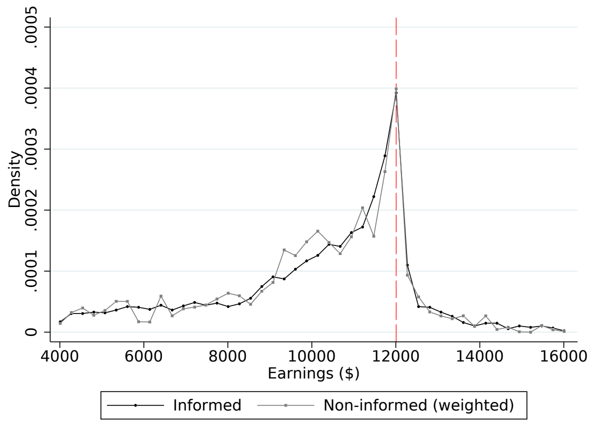
Figure A.11: Specification Checks for Polynomial Approach: Bunching Elasticity in 2015



(a) Informed: Sensitivity of Bunching Elasticity in 2015 (b) Non-informed: Sensitivity of Bunching Elasticity in 2015

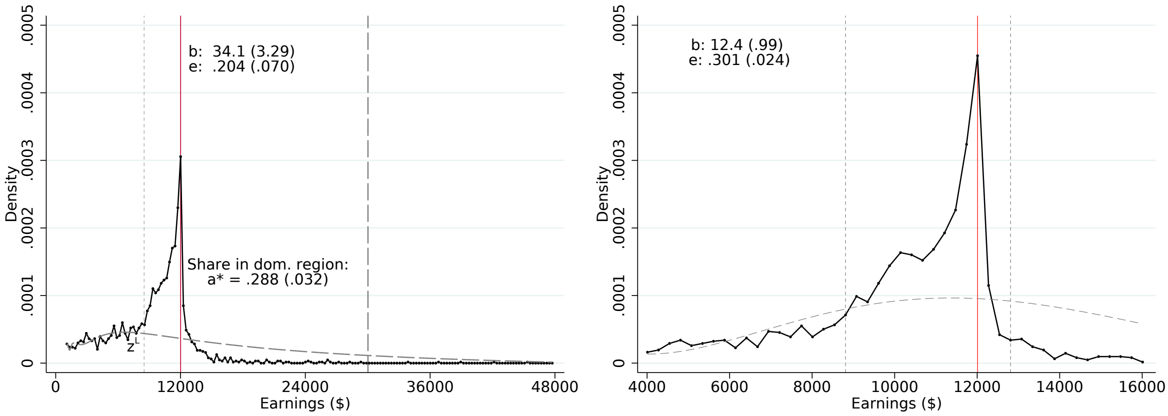
Notes: Figure a illustrates the estimated bunching elasticity in 2015 for treated DI recipients . Figure b illustrates the earnings distributions in 2015 for non-treated DI recipients and the fitted counterfactual density using the polynomial approach. The dots represent point estimates, and the vertical lines represent 95 percent confidence intervals. The x-axis represent different specifications for the polynomial order in equation 3. The two samples consist of DI recipients with full (100 percent) benefit at the beginning of 2015 who were eligible for the information treatment. Earnings are adjusted using the average wage growth, and are reported in 2015 dollars (NOK/$ = 7.5).

Figure A.12: Weighted Earnings Distributions Around the SGA Threshold in 2014: By Information Status



Notes: This figure shows the distribution of annual earnings in 2014 around the SGA threshold (marked by the red dashed line) in $267 (2,000 NOK) bins by information status, where the non-treated units is weighted by propensity score weights $w(x) = \frac{P(I=1|x)}{P(I=1)} \frac{1-P(I=1)}{1-P(I=1|x)}$ where $P(I = 1)$ denotes the probability of receiving information treatment. $P(I = 1|x)$ is estimated with a logit using DI benefits, AIE, age, years on DI, female, cohabitant, number of children and years of schooling (same as in table 2, not including earnings in 2014 and 2015) as control variables. The sample consists of fully disabled recipients awarded DI before the 1st of January 2015 who were eligible to receive the information letter (PAE above the cutoff). Earnings are measured in 2015 dollars (NOK/$ = 7.5).

Figure A.13: Polynomial Approach: Bunching Among Eligible in 2014



(a) Eligible Notch Sample: Bunching Elasticity in 2014 (b) Eligible Kink Sample: Bunching Elasticity in 2014

Notes: Figure (a) illustrates the earnings distributions in 2014 of DI recipients awarded benefits prior to January 1, 2004 and the fitted counterfactual density using the polynomial approach. Figure (b) illustrates the earnings distributions in 2014 of DI recipients awarded benefits after January 1, 2004 and the fitted counterfactual density using the polynomial approach. The red line denotes the SGA threshold in 2014. The polynomial order is chosen to minimize the distance between the estimate from the non-parametric approach and the parametric approach ($p = 5$ for notch and $p = 7$ for kink).. The sample consists of DI recipients with full (100 percent) benefit at the beginning of 2015, and who were eligible for the information treatment. Earnings are adjusted using the average wage growth, and are reported in 2015 dollars (NOK/$ = 7.5).

Figure A.14: Average Earnings By Information Status

Notes: This figure shows the regression coefficients from the distributed lag lag model that tests for significant differences between the two groups prior to the information treatment (see Section 7). The non-treated units is weighted by propensity weights $w(x) = \frac{P(I=1|x)}{P(I=1)} \frac{1-P(I=1)}{1-P(I=1|x)}$ where $P(I = 1)$ denotes the probability of receiving information treatment. $P(I = 1|x)$ is estimated with a logit using DI benefits, AIE, age, years on DI, female, cohabitant, number of children and years of schooling (same as in table 2, not including earnings in 2014 and 2015) as control variables. The sample consists of fully disabled recipients awarded DI before the 1st of January 2015 who were eligible to receive the information letter (with PAE above the cutoff). Earnings are measured in 2015 dollars (NOK/$ = 7.5).

Table A.1: Survey evidence on knowledge about the change in work incentives

	Agree (Strongly agree), %	
	Positive earnings	No earnings
Column:	(1)	(2)
Panel A:		
Agrees to statement "Cash benefit is higher in 2015 than 2014"	73 (54)	68 (48)
Panel B:		
Agrees to statement "Cash benefit is taxed as labor income"	92 (78)	86 (70)
Panel C:		
Agrees to statement "From 2015, it will always pay to work more"	79 (58)	71 (51)
Panel D:		
Understands how the change affects ***myself***	34 (13)	27 (14)
Number of respondents	784	884

Notes: This table displays results from the SSA's (NAV) survey of recipients after the information package in the fall of 2014 had been sent. The letter informed recipients about the overall goal of the reform and details of the changes in work incentives. 39 percent of the sample (78 percent with assistance) recollected receiving the information package about individual thresholds and benefit levels. Survey is made available from NAV.

Table A.2: Descriptive Statistics: By information status

Sample:	Eligible ($\hat{z} > K$)				Ineligible ($\hat{z} < K$)	
	Informed		Non-informed		Comparison group	
Column:	(1)	(2)	(3)	(4)	(5)	(6)
Earnings:	mean	sd	mean	sd	mean	sd
Earnings Jan-May in 2015 ($)	4,861	(1,219)	4,713	(1,203)	1,297	(1,038)
Annual earnings in 2014 ($)	9,679	(6,344)	9,246	(5,724)	4,442	(6,340)
DI information:						
Age at DI award	41.57	(12.46)	36.85	(12.16)	34.72	(15.18)
Years on DI	13.38	(10.01)	11.36	(9.78)	14.07	(11.53)
Uncapped annual DI benefits ($)	35,807	(6,255)	34,859	(5,351)	35,229	(4,970)
Annual indexed earnings ($)	58,200	(22,162)	56,614	(14,618)	55,833	(16,569)
Characteristics:						
Females	.53		.47		.53	
Married/Cohabitants	.57		.56		.39	
Years of Schooling	10.60	(2.32)	10.49	(2.38)	10.04	(2.83)
Number of Children	1.87	(1.29)	2.02	(1.46)	1.27	(1.38)
Observations	3,642		1,521		30,179	

Notes: Columns 1, 3, and 5 reports the means and Columns 2, 4 and 6 report standard deviations of key outcome variables and characteristics of three groups. The first group, "Informed" includes recipients whose projected earnings were above the cutoff and who received the information letter from SSA in June 2015. The second group, "Non-informed" includes recipients whose projected earnings were above the cutoff, but did not receive the information letter in June 2015 e.g. due to lags in reporting.and the group of individuals who were ineligible to receive the letter. The third group, "Comparison group" were ineligible to receive the information letter. The sample consists of fully disabled recipients awarded DI before the 1st of January 2015 and had positive earnings at some point during January-May 2015. Uncapped DI benefits are the initial DI benefits before being earnings tested. Annual indexed earnings summarizes earnings history before the disability onset. All variables are measured in 2015 dollars (NOK/$ = 7.5). The difference between the sample size in the second column of Table 2 and the total sample size of eligible and ineligible in this table is due to some individuals with high levels or earnings but who were ineligible for the letter. This group included individuals who reported a change in earnings before June 2015, and individuals who had already exceeded the limit and therefore received a letter from SSA informing them about a reduction in benefit payments.

Table A.3: Heterogeneous Effects of Information Treatment on Labor Market Outcomes

		Effects on Labor Supply					
	Probability to Switch Firm	Monthly earnings ($)	Any earnings	Fixed pay ($)	Variable pay ($)	Contract hours	Observations <individuals>
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Rigid hours	-.013	-206***	-.083***	-32**	-113***	-1.18***	28,788
	.009	30	.016	15	23	.31	<2,399>
p-value	.136	<.001	<.001	.037	<.001	<.001	
Dep. mean (Pre-Treatment)	.050	873	.807	243	485	9.03	
B. Flexible hours	-.009	-174***	-.079***	-20	-112***	-.97***	28,944
	.008	34	.015	18	26	.37	<2,412>
p-value	.270	<.001	<.001	.253	<.001	.009	
Dep. mean (Pre-Treatment)	.039	883	.727	282	473	10.51	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Standard errors (in parentheses) are clustered at the individual level and are robust to heteroskedasticity.

Notes: This table presents estimates of the impacts of the information letter on labor market outcomes during 2015. The event study is implemented using a difference in difference design, where we include a dummy for the treatment group, a dummy for each month of the year, and an interaction between the post-treatment period and the treatment indicator. Non-treated units are reweighed using propensity score weights $w(x) = \frac{P(I=1|x)}{P(I=1)} \frac{1-P(I=1)}{1-P(I=1|x)}$ where $P(I=1)$ denotes the probability of receiving the information treatment, and $P(I=1|x)$ is estimated by logit regression using the characteristics from Table 2. Fixed pay is defined by the work contract. Variable pay is defined as wages paid on an hourly basis, bonuses, overtime pay, or other payments as defined by employers. Switching employer is an indicator variable equal to one if the current employer is different than the main employer at the beginning of the calendar year. The sample consists of fully disabled recipients awarded DI before the 1st of January 2015 who were eligible to receive the information treatment. Earnings are measured in 2015 dollars (NOK/$ = 7.5). The sample is restricted to individuals that are observed in the monthly earnings data. Dependent mean (control group) is measured before treatment. Rigid vs Flexible hours is determined by whether the industry (five-digit) has more or less than the median fraction of contracts within the five most common hours categories. The estimates does not change if we instead use the three most common hours categories.

Chapter III

Early Retirement Provision for Elderly Displaced Workers

Written jointly with Herman Kruse

Early Retirement Provision for Elderly Displaced Workers*

Herman Kruse† Andreas S. Myhre ‡

This version: December 18, 2020

Abstract: This paper studies the economic effects on re-employment and program substitution behavior among elderly displaced workers who exogenously lose eligibility for their early retirement option. We use detailed Norwegian matched employer-employee data containing information on bankruptcy dates and individual-level wealth, income, pensions and social security benefits. Our empirical strategy employs a regression discontinuity design, as job displacement before a certain age cut-off results in losing eligibility for early retirement benefits between ages 62–67 years in Norway. We find that re-employment rates are indistinguishable between workers who just retain eligibility for early retirement benefits and those who just do not. Meanwhile, those who lose eligibility offset 69% of their lost benefits through take-up of other social security benefits, where 51% comes from disability insurance and 13% from unemployment insurance. Our findings are particularly policy relevant as tightening of age-limits for old-age pensions is on the agenda in several OECD countries, while current economic hardship throughout the region may lead to increased job displacement for elderly workers.

Keywords: early retirement, job displacement, labor supply, benefit substitution, social security
JEL codes: H55, I38, J14, J26, J65

*The authors would like to thank Andreas Kostøl, Edwin Leuven, Kjetil Storesletten and Simen Markussen for their invaluable comments and suggestions. Thanks are also due to Eric French and Fang Yang. We acknowledge Statistics Norway and Fellesordningen for AFP for access to their databases. The data are used in compliance with the rules given by the Norwegian Data Inspectorate.

†University of Oslo, Department of Economics; E-mail: herman.kruse@econ.uio.no. I am grateful towards the Norwegian Research Council for the financial support (The evaluation of the pension reform, grant number 248868)

‡University of Oslo, Department of Economics; Statistics Norway. E-mail: a.s.myhre@econ.uio.no

1 Introduction

Pension entitlements can be affected by interrupted labor market careers, and pension systems are typically not designed to fully offset shocks affecting individual work careers (OECD, 2015). Retirement income is tightly linked to past earnings history, and unwilling displacement from the labor market may therefore lead to a negative wealth shock in terms of lost pension entitlements. In many OECD countries, late-career job displacement may even lead to individuals losing the ability to retire early or at the same terms as full-career workers.[1] While late-career job displacement already has severe implications for individual welfare, the loss of pension entitlements adds an additional element of reduced individual welfare that may have implications for job-seeking effort or enrollment onto other social security programs.

The main contribution of this paper is to assess the economic implications of access to early retirement benefits for elderly displaced workers. In particular, we study (i) the adverse effects on re-employment rates, (ii) benefit substitution onto other social security benefits and (iii) the associated implications on policy and welfare. A sharp eligibility criterion in the Norwegian early retirement program (AFP) facilitates our study.[2] Before 2011, workers in private sector firms covered by the AFP scheme could claim early retirement benefits from the age of 62, but in order to be eligible, workers had to be employed by the firm at the date of claiming. A job displacement before an individual cut-off date therefore implied that the individual did not qualify for AFP benefits, provided between ages 62–67 years. This allows us to employ a regression discontinuity design to study causal effects of early retirement provision for various outcomes, by comparing workers who lost their job just too soon to be eligible versus workers who just retained their eligibility. To identify job displacements, we use data on bankruptcies among Norwegian private sector firms between 2001–2010 which helps avoid potential endogeneity problems of workers voluntarily leaving a firm.[3] Combined with high-quality data on matched employer-employee relationships and take-up of various social security benefits from tax registers, this allows us to study effects of early retirement eligibility on re-employment rates, earnings, and benefit substitution between ages 62–67. We focus in particular on substitution towards disability insurance (DI) and unemployment insurance (UI). Furthermore, we explore the welfare implications and the financial costs for the state.

Our main empirical findings can be summarized by the following conclusions. First, we do not find evidence of eligibility for early retirement harming re-employment rates among workers in our sample. We estimate that re-employment rates among workers who are displaced just before becoming eligible for AFP are only 2 percentage points lower than among workers who are displaced just after becoming eligible, with the point estimate being statistically insignificant. Our corresponding estimate on labor market earnings is similarly small and insignificant and suggests that early retirement eligibility decreases labor market earnings between ages 62–67 by \$5,600, or about 9 percent. Second, we find clear evidence of program substitution, and in particular increased enrollment onto the DI program among ineligible workers. The fraction of workers who are displaced just before becoming eligible for AFP and

[1]Some examples are Austria, Estonia, Hungary, Israel, Korea, Norway, the Slovak Republic, Sweden, Chile, Mexico and Germany (OECD, 2015).

[2]The early retirement program in Norway is known by its acronym AFP from Norwegian "Avtalefestet pensjon".

[3]For job displacements due to bankruptcies, an additional rule referred to as the "52-week-rule" pushed back this threshold to 61 years of age, plus the standard notice period, which may be some period from 1–6 months depending on tenure and age of the worker. This means that the relevant cut-off for workers experiencing a bankruptcy is individual-specific and may be some time between 60 years and 6 months to 60 years and 11 months. The details of this will be outlined in Section 2.

claim DI before reaching the general retirement age of 67 years is about 48 percent, compared to just 12 percent among workers who just retain AFP eligibility. The increase in DI claiming of 36 percentage points among the ineligible is highly statistically significant. We further estimate that of the $61,600 in lost AFP benefits among the ineligible, $42,500 is replaced by non-pension public transfers, where about $31,400 is increased take-up of DI benefits and $7,700 is increased take-up of UI benefits. This is equivalent to a replacement of 69 percent of the lost benefits with other non-pension social security benefits, where about half of the lost AFP benefits are replaced with DI.[4] Third, there is substantial heterogeneity in benefit substitution behavior among workers in our sample. Benefit substitution is largest for workers with low earnings, workers with low educational attainment and workers in the manufacturing industry. Fourth, we find that the increase in public expenditures of providing early retirement benefits is modest. Our point estimate suggests that the increase in overall costs amounts to $7,200 for each worker per annum, and at the 95% confidence level, our findings suggest that the increase is at most $16,400.

We emphasize that our findings should be interpreted with some caution. As the composition of workers in the private sector with access to AFP is heavily skewed towards male workers (more than 75 percent), and bankruptcies occur more commonly in the manufacturing industry (about 68 percent of all bankruptcies) where workers typically have low educational attainment, our main findings are mainly driven by workers with these characteristics. We do not find evidence of increased take-up of DI for workers in non-manufacturing professions whose bankruptcy occurred before their individual age cut-off, but we do find substitution towards UI for these workers. Moreover, we do not find distinguishable differences between workers with high educational attainment who reach the eligibility threshold and those who do not.

To investigate the welfare implications of early retirement provision for elderly displaced workers, we assess disposable income among workers in our sample who were eligible for AFP and those who were not. We do not find clear evidence of disposable income being higher among the eligible on average, with a statistically insignificant point estimate of about $3,800 per annum, or about 12 percent higher than among the ineligible. However, this exercise does not necessarily capture the full picture, and in particular whether some ineligible individuals are significantly worse off. Therefore, we extend our exercise and investigate distributional impacts in a standard Imbens & Rubin (1997) framework, and assess the distribution of disposable income depending on eligibility status. While we do find evidence of disposable income being more dispersed among the ineligible, the difference in the lower part of the distribution is small. This suggests that few individuals in our sample were significantly worse off when being ineligible for early retirement benefits, and that most ineligible individuals who were not re-employed got some type of social security benefit.

We believe our analysis is of general interest for three main reasons. First, economic hardship throughout the OECD may lead to increased job displacements and decreased labor demand, and in particular for elderly workers who are usually less attractive hires.[5] Second, many countries have implemented early retirement schemes to provide more flexible withdrawal opportunities from the labor market and to reduce enrollment onto other social security programs, but these programs have also turned out to be very costly. We are able to shed light on a particularly large shock to early retirement

[4] Regarding the very high substitution onto DI, we emphasize that on average about 23 percent claim DI at some point between ages 62–67 in the population, while among those who experience a late-career job displacement and at the same time do not reach eligibility, about 48 percent claim DI at some point between ages 62–67.

[5] See e.g. Heyma *et al.* (2014); Vigtel (2018).

entitlements, as losing eligibility for AFP leads to the loss of the entire early retirement option, or the equivalent of five years of benefits. We are also able to account for the outcomes in the entire early retirement period for our sample individuals, meaning that we can fully account for the employment effects in the period of interest and the potential program substitution. Third, many countries are debating whether parts of the social security system in general must undergo reforms to uphold fiscal sustainability. This may lead to the use of prescriptions such as eligibility tightening or benefit cuts, prescriptions to which we provide evidence to policymakers' knowledge about potential gains and harms.

Our paper is primarily related to the literature focused on the effects of extended UI for elderly workers. Closest to our paper is a few studies which have shown that extended UI benefits discourages job searching and prolongs unemployment spells, and may even bridge the gap to retirement.[6] Inderbitzin *et al.* (2016) showed that extended UI have strong effects on labor market exit through early retirement, and increased exit through the DI channel. Kyyrä & Ollikainen (2008) used a reform in Finland which increased the eligibility age for extended UI from 53 to 55 and later in Kyyrä & Pesola (2020) from 55 to 57 to study the effects on early retirement and labor supply, respectively. Kyyrä & Ollikainen (2008) documented a decrease in early retirement from the first increase in access age, while Kyyrä & Pesola (2020) documented increased employment over the remainder of the working career, and no substitution onto other programs. In contrast to the literature on extended UI, our study consists of workers very close to the general retirement age. We contribute to this literature by studying effects of having the option to retire early, and thus exiting the labor market entirely, which we argue may have fundamentally different implications than extended UI spells.

Our paper is also related to the literature on early retirement programs and changes to the minimum legal retirement age on labor supply and program substitution (e.g. Geyer & Welteke, 2017; Manoli & Weber, 2016; Staubli & Zweimüller, 2013 among others). Their common finding is that increasing the retirement age increases employment, but evidence on program substitution is mixed. Hernæs *et al.* (2016) used a recent Norwegian reform of the pension system which gave workers more flexible withdrawal opportunities, while Johnsen *et al.* (2020) used the introduction of the Norwegian early retirement program, essentially studying a reduction in the legal retirement age. Their common finding is that workers tend to decrease take-up of DI benefits in response to greater flexibility of the retirement program. Vigtel (2018) showed, on the labor demand side, that decreasing the minimum legal retirement age in Norway for a subset of workers leads to risk-averse firms becoming more willing to hire senior workers. While most of these studies are focused on the spillover effects between two programs or their employment effects, our paper broadly investigates the spillover effect onto the entire spectrum of social security programs that the elderly workers may be eligible for. In that sense, we contribute to the literature by broadening the scope of program substitution.

Another broad branch of the literature is focused on the effects of tightening policies regarding eligibility for social benefits and their effect on employment rates and program substitution. Borghans *et al.* (2014) studied how stricter criteria for access to DI in the Netherlands affected enrollment onto other social insurance programs, and found that individuals disqualifying for DI offset about 30 percent of the lost benefits in take-up of other social benefits. Similarly, Karlstrom *et al.* (2008) found that stricter eligibility criteria for DI in Sweden increased take-up of UI and sickness benefits, but that it did

[6]While this literature is often interested in the push and pull factors of UI systems for older workers into unemployment (e.g. Tuit & van Ours, 2010 and Baugelin & Remillon, 2014), we do not explore this margin in our paper.

not influence employment rates. Staubli (2011) suggests that increasing the minimum age of relaxed DI access in Austria only had a slight positive effect on employment rates, but a significant decline in DI enrollment. Our study echoes these studies regarding the importance of assessing program substitution when considering policy changes to social security programs.

Finally, our paper is related to an extensive literature on the effects of job displacement (e.g. Jacobson *et al.*, 1993; Lassus *et al.*, 2015; Marmora & Ritter, 2015; Ichino *et al.*, 2017; Huttunen *et al.*, 2018 among others). Common findings for these studies are large adverse effects on earnings and employment, both in the short and long run. Particularly relevant for our study is Bratsberg *et al.* (2013), who used data on Norwegian bankruptcies and showed that a large fraction DI claims can be attributed to job displacements. They found that non-participation in the labor market is significantly affected by exogenous changes in employment opportunities. Marmora & Ritter (2015) found that unemployment late in workers' careers affects retirement timing, and that the effect is stronger once the workers become eligible for social security benefits. Recently, Ichino *et al.* (2017) showed that old and young workers face similarly large displacement costs in terms of long-run employment, but older workers lose considerably more initially and gains later. While our study does not primarily focus on the effects of job displacement, we show how an outside option for displaced workers affects re-employment rates and enrollment onto social security programs.

The remainder of this paper is organized as follows. First, we present an overview of the Norwegian early retirement program, and briefly provide an overview of the related public transfer systems in Section 2. Then, in Section 3, we present the administrative data that we use, while in Section 4 we lay out our empirical strategy. In Section 5 we present our main results. In Section 6 we present a fuzzy RD as an extension of our main results. In Section 7 we assess the implications of our findings for policy and welfare. Finally, we conclude in Section 8.

2 Institutional setting

Our focus lies on elderly workers in private sector firms covered by the early retirement program (AFP) who experience a job displacement due to bankruptcy of the firm they work in. The institutional background information therefore includes an overview of the AFP program and the eligibility criteria including particular rules concerning firm bankruptcies. We also provide a brief overview of other social security programs that workers may be eligible for, and in particular the disability insurance (DI) program and the unemployment insurance (UI) program.

2.1 Early retirement (AFP)

The AFP program was introduced in 1988. For public sector workers, there has been full coverage since the introduction, while about half of private sector workers have been covered since the introduction, although the rate has increased somewhat over time. For private sector firms, membership is voluntary and requires a centrally negotiated collective pay agreement. For member firms, employees are enrolled regardless of their individual union memberships. The AFP is partially funded by the government, and partially funded through payments by member firms. Until November 2010, the AFP offered enrolled workers a full pension claim starting from age 62, whereas the normal retirement age through the National Insurance Scheme was 67 years.[7]

[7] When the system was first introduced, the minimum claiming age was set to 66 years, but the limit has since been reduced in four steps. The final reduction of the minimum legal claiming age happened in 1998, and all our possible claimants became

Eligibility criteria The AFP system imposed a sharp lock-in (or lock-out) mechanic. Workers had to be working in *the same firm* covered by the AFP for the last three years before claiming benefits, or in *any firm in the same sector* covered by AFP for the last five years with the last two years being in one firm.[8] Furthermore, the firm had to employ at least two workers not counting the owner of the firm. At the day of claiming benefits, the worker had to be employed by the firm, and the first possible claiming time is the beginning of the month after reaching age 62. Workers' salary had to be at least equivalent to approximately $10,000 (in 2015 dollars) in annual earnings, with this firm being the worker's main employer. Finally, claiming AFP benefits could not be combined with claiming DI benefits.

There was an exception made in the case of mass-layoffs or bankruptcy. If the work relation was terminated because of either of these events, the worker retained the AFP membership for 52 weeks after the day of the incident plus the duration of the standard notice period. The standard notice period is governed by the Norwegian Work Environment Act and is a mapping based on tenure and the worker's age, where the shortest notice period is 1 month and the longest is 6 months.[9] This means that a worker who lost the job due to bankruptcy or mass-layoff essentially could retain the membership for up to 18 months after the incident.

Benefits levels The AFP benefit level was a mapping from the old-age pension benefit that the worker would receive from the National Insurance Scheme given pension claiming at age 67. The old-age pension benefit level received at age 67 was unaffected by claiming AFP. We provide a detailed overview of how old-age benefits were calculated in Appendix B. Additionally, claimants received an "AFP top-up", which was a flat rate of about $2,300.[10] Claimants were subject to a pro-rata earnings test on continued work above a very small tolerance level, essentially implying a marginal tax-rate on continued work close to 100 percent for those who claimed AFP. Average annual benefits amounted to about $24,000 in 2001 and approximately $27,000 in 2010. The average benefit levels were significantly higher for men than for women. In 2001, the average benefit for men was $27,000 and for women $22,000 while in 2010 the average benefit for men was $31,000 and for women $24,000.

2.2 Other social security benefits

Disability insurance For those deemed to have permanent reduced earnings capacity due to illness or injury, disability insurance (DI) benefits replaces parts of the past earnings that are lost due to the reduced capacity. This benefit may be partial, depending on the residual earnings capacity. To be eligible for disability benefits, an individual must be between 18–67 years old and have been a member of the National Insurance Scheme in the last three years before becoming disabled. Illness or injury must be the main reason why the earnings capacity has been reduced, appropriate vocational rehabilitation measures must have been completed and the earnings capacity must be permanently reduced by at least 50 percent.[11] In the time period we consider in this paper, the benefit level was equivalent to the old-age

eligible *after* this year, unifying our minimum legal claiming age for AFP to 62 years of age. The structure of the AFP, including some of the rules governing eligibility, was changed in 2011, a reform that does not affect our sample as workers in our cohorts spanning from 1939–1948 were entirely covered by the old rules.

[8] For instance, switching jobs between private and public sector firms just before retirement would lead to loss of eligibility for AFP benefits, even if both the private and the public firm were covered by AFP.

[9] The exact mapping from age and tenure to the notice period is displayed in Equation (2) in Section 4.

[10] In 2015 dollars. Throughout the paper, we measure monetary values in 2015 dollars given an average exchange rate of NOK/USD = 9.

[11] Under some criteria, DI may be given even though the earnings capacity is reduced by less than 50 percent; if the worker is currently on the work assessment allowance program, 40 percent is sufficient, and if the reduced earnings capacity is due to

pension benefit, and therefore almost equivalent to AFP benefits (the difference was equivalent to the "AFP top-up" of $2,300 per annum). Individuals allowed DI were subject to an earnings test implying a marginal tax rate of about 60 percent if earnings exceeded about $10,000.[12]

Unemployment benefits To be eligible for unemployment benefits, a person must be a registered job-seeker at the Norwegian Labour and Welfare Administration. A person whose working hours have been reduced by at least half, is a genuine job-seeker, a member of the National Insurance Scheme, a legal resident, and has had at least $15,000 of income in the previous calendar year or $30,000 combined over the past three calendar years may apply for unemployment benefits. If the pre-unemployment income exceeded $20,000, the recipient may receive unemployment benefits for up to 104 weeks, while if it was lower than $20,000, the longest period is 52 weeks. A recipient of unemployment benefits is entitled to 62.4 percent of the past earnings. The past earnings are either the last 12 months before unemployment, or the annual average of the last 36 months if this exceeds the former.

Other public transfers Besides disability and unemployment insurance, workers in our sample may also be eligible for various other social security benefits. One particularly relevant program for elderly workers is sickness benefits which is intended as replacement of income loss due to short-term sickness (up to one year) for workers engaged in employment who are members of the National Insurance Scheme. A full sickness benefit fully replaces the earnings in the past year. Although less relevant for elderly individuals than for prime-age workers, workers in our sample may also be eligible for temporary DI benefits. While the temporary DI program has undergone several changes during our sample period, the program's main intention has been to provide financial support in periods when the person is ill or injured but attempt to return to work. Temporary DI was provided for up to 1–4 years during our sample period for most individuals.[13] Additionally, individuals in our sample may also be eligible for a few less relevant benefits such as social assistance and child support.

3 Data and sample selection

In our empirical analysis we use data from two main sources that can be linked by unique and anonymized identifiers for every resident individual and employer. The main data we use is provided by Statistics Norway (SSB) and contains detailed information about individual characteristics and employer-employee relationships, including exact dates of each relationship. This allows us to construct monthly data on earnings and employment for each individual and firm. The employer-employee data also contains information on firm characteristics, including 5-digit industry codes and the exact date of bankruptcy (if such a date exists). Thus, we are able to identify individuals who work in firms experiencing a bankruptcy. Our second source of data is provided by *Fellesordningen for AFP*, and includes information about exact dates on each firm's affiliation to the AFP-scheme.[14] This allows us to identify

an approved occupational illness or injury, 30 percent is sufficient.

[12]Every dollar in earnings were earnings tested if earnings exceeded this threshold. After 2005, only the earnings above the threshold were earnings tested if the individual was allowed DI in 2003 or earlier.

[13]Before 2010, temporary DI consisted of three separate programs: Rehabilitation benefits (up to 1 or 2 years), occupational rehabilitation benefits (no upper time constraint) and time-constrained DI benefits (up to 5 years). In March 2010, these programs were replaced with the Work Assessment Allowance program that provided benefits to individuals for up to 4 years as a general rule.

[14]Fellesordningen for AFP is the largest private sector organization for AFP schemes and almost the entire market.

whether individuals are eligible for AFP based on their employment relationship which is crucial for our analysis. For our main outcome variables, we use annual data on earnings and social security transfers from reported tax-records (SSB). The data we use contains years 1999–2014.

The administrative nature of our data reduces the extent of measurement errors in income variables and employment relationships. Because individual employment affiliation and income variables are third-party reported (i.e. by employers and the tax authorities), the coverage and reliability are rated as exceptional by international quality assessments (see e.g. Atkinson *et al.*, 1995). Since administrative data are a matter of public record, there is no attrition due to non-response or non-consent by individuals or firms, and individuals can only exit these data sets due to natural attrition (death or out-migration).

3.1 Sample selection

In our empirical analysis, our main estimation sample considers workers aged 59–61 years when the firm experiences a bankruptcy. The upper age restriction is set to avoid selection bias. As workers in affiliated firms are eligible for AFP benefits from the age of 62, we ensure that individuals in our estimation sample have not yet made their decision to retire early. The lower age restriction ensures that we have roughly 18 months of bandwidth on each side of the cut-off in our RD analysis. A potential worry in our setting is that firms may lay off workers before the actual bankruptcy occurs. Another worry is that workers may anticipate that their job is at risk and leave early. To avoid such selection of workers, our main estimation sample includes those workers who were employed in a firm with AFP affiliation 24 months prior to the bankruptcy date of the firm. Thus, we also pre-determine worker and firm characteristics to this initial point in time (when workers are 57–59 years of age). While our estimates appear to be very stable across different specifications of when we pre-determine work affiliation, we test alternative samples of workers with pre-determined affiliation 12 months and 1 month before the bankruptcy dates of firms as robustness checks.

Additionally, we do the following sample restrictions due to the eligibility criteria of the AFP program presented in Section 2.1. One of the requirements states that individuals must work at least 3 consecutive years in the same firm with AFP affiliation. To be eligible for AFP at the age of 62, we therefore require that individuals started their employment relationship before the month of when individuals turned 59 years. We also require that the specific employment relationship was each individual's main employer (the one with the highest earnings) if the individual had more than one employer, as only the main employment relationship was considered for eligibility. Third, we require that individuals did not participate in the DI program, as recipients of this program were ineligible for AFP benefits. Fourth, we require that firms have at least 2 employees as workers were considered ineligible if there were no other employees at the firm. Fifth, we require that individuals worked at least 20 percent of a full-time position, which translates to roughly $10,000 in annual earnings to meet the final eligibility criteria for AFP.

Even though our data contains information on registered firm bankruptcies, some of the firms may get new owners and keep a share of the workforce, leading to few or no job displacements despite the original firm being bankrupt. As we are interested in workers who in fact do experience a job displacement, we therefore follow previous studies (see e.g. Jacobson *et al.*, 1993; Rege *et al.*, 2009; Huttunen *et al.*, 2011; Basten *et al.*, 2016), imposing a restriction on the fraction of workers (including younger workers not in our estimation sample) who from the month of the bankruptcy to 12 months post-bankruptcy work in the same firm. In our baseline specification, we set our threshold to 1/3 meaning that if more than 1/3 of all workers in the bankruptcy firm (excluding "self"[15]) work in the *same firm* 12 months after the bankruptcy, it is considered a "spurious bankruptcy" and the entire firm is dropped from our initial estimation sample. We do, however, include these firms in an alternative sample as a robustness check.

As our data spans from 1999, our main estimation sample includes bankruptcies in private sector AFP-firms during January 2001–November 2010 and cohorts 1939–1948.[16] This means that for firms with a bankruptcy occurring in 2001, our workers must be employed by the firm in 1999. As our data spans to 2014 we are able to follow individuals during the entire early retirement period until they reach the standard retirement age of 67 years.

3.2 Descriptive statistics

In Table 1 we present summary statistics for individuals aged 57–59 years who work in a private sector firm. The first two columns include our main estimation sample of individuals who worked in a private sector firm with AFP affiliation 24 months before the bankruptcy. The third and fourth columns include workers in bankruptcy firms without AFP affiliation which we use as a placebo sample in the empirical analysis. The fifth and sixth columns include workers who worked in private sector firms that did not become bankrupt, which we use as a comparison sample in our analyses.

There are some noteworthy differences between our main estimation sample of workers in bankruptcy firms with AFP affiliation and the other private sector firms that do not become bankrupt, particularly for industries. The firms in our estimation sample are far more likely to be in the manufacturing sector, while the workers are more likely to be male workers and have slightly lower earnings on average. Firms are also somewhat smaller compared to the other private sector firm. Otherwise, workers share fairly similar characteristics.

[15]For instance, the workers in a firm with 10 employees which ends up bankrupt is "spurious" if $n > (10-1)/3$ works in the same firm a year after the bankruptcy, where the one subtracted is "self".

[16]We restrict our attention to bankruptcies occurring before the 2011 Norwegian pension reform for two reasons; the reform changed the rules regarding eligibility and work incentives for individuals claiming AFP benefits. While workers in our sample could in principle become eligible for AFP benefits under the new scheme following the reform, individuals in our sample had to postpone claiming after the initial claiming month when turning 62 years, and had to be re-employed in a firm with AFP affiliation to satisfy the new eligibility criteria. Only 3% of our sample claim AFP benefits under the new scheme, compared to 37% claiming before the reform.

Table 1: Summary statistics of private sector workers aged 57-59 years

	Bankruptcy samples				Comparison sample	
	Main est. sample: AFP workers		**Placebo sample: Non-AFP workers**		**All private sector workers**	
Individual characteristics:	mean	sd	mean	sd	mean	sd
Age	58.0	(.84)	58.0	(.82)	58.0	(.82)
Fraction females	.23		.29		.33	
Fraction married	.75		.72		.76	
Years of education	10.8	(1.7)	11.2	(2.2)	11.4	(2.3)
Number of children	2.0	(1.2)	2.3	(1.1)	2.2	(1.1)
Wealth ($1,000)	89	(95)	94	(107)	119	(123)
Labor market characteristics:						
Monthly earnings ($1,000)	4.1	(1.8)	3.9	(2.1)	4.9	(2.3)
Fraction full time employment	.91		.87		.87	
Tenure (years)	8.8	(8.7)	6.1	(6.8)	10.9	(8.9)
Number of employees	84	(127)	11	(13)	157	(315)
Fraction receiving sickness benefits	.11		.11		.08	
Local DI rate	.10	(.03)	.10	(.03)	.10	(.03)
Local unemployment rate	.02	(.01)	.02	(.01)	.02	(.01)
Industry (%):						
Primary sector	1.2		3.1		4.3	
Manufacturing	68.1		18.8		30.0	
Construction	10.3		14.0		8.5	
Wholesale retail and trade	13.3		37.6		25.8	
Transportation and storage	1.2		7.5		9.2	
Scientific and legal activities	1.2		5.1		6.5	
Other	4.7		14.0		15.7	
Number of firms	177		511		48,451	
Number of individuals	339		591		141,122	

Notes: Bankruptcy samples include individuals aged 57–59 years who work in a private sector firm 24 months before the firm's bankruptcy date. Comparison sample includes individuals aged 57–59 years who work in a private sector firm (excluding bankruptcies). All samples include firms with at least 2 employees, individuals not on disability insurance, cohorts 1939–1948 and years 1999–2008. Firm must be each individual's main employer (with the highest earnings if more than 1 employer). Local DI and unemployment are measured at the municipality level. Earnings and wealth are measured in 2015 dollars (NOK/USD = 9).

4 Empirical framework

This section first presents the assignment rule that creates local random variation in eligibility for early retirement (AFP). We then present the regression discontinuity design that we use to identify effects of early retirement eligibility and discuss threats to identification.

Assignment variable As our proxy for job displacements comes from bankruptcies, our assignment variable is based on the age of individual i at the time of the bankruptcy of the firm. As explained in Section 2.1, individuals are in normal cases eligible for AFP from the age of 62, but in the case of bankruptcies, workers are granted an additional 52 weeks plus the individual notice period. Hence, our assignment variable (measured in months) is defined as:

$$a_i = age_i - (61 - NP_i) \tag{1}$$

where age_i is individual i's age at the bankruptcy date and NP_i is the *notice period* (in months) of individual i, which is governed by the Norwegian Work Environment Act, according to:

$$NP_i = 1 + T_{i,5} + T_{i,10}(1 + \mathbb{I}_{i,50} + \mathbb{I}_{i,55} + \mathbb{I}_{i,60}) \tag{2}$$

where $T_{i,y}$ is a dummy equal to one if individual i has at least y years of tenure and $\mathbb{I}_{i,\bar{a}}$ is a dummy equal one if individual i is at least as old as age $\bar{a}$. The relationship implies that individuals in our sample have a notice period of 1–6 months depending on age and tenure. If a_i is positive (negative), then the firm went bankrupt sufficiently late (too early) and individual i is initially eligible (ineligible) for AFP benefits.

4.1 Regression discontinuity design

In our RD design, assignment to eligibility is a deterministic function of the assignment variable a, the age at bankruptcy including each individual's notice period as defined in Equations (1) and (2). Individuals are initially eligible for AFP if $a \geq 0$. The regression model for our reduced form RD model can be summarized by the following equations:

$$y_{it} = \alpha_l + f_l(a_i) + \delta X_{it} + \varepsilon_{it} \qquad \text{if} \quad a_i < 0 \tag{3}$$

$$y_{it} = \alpha_r + f_r(a_i) + \delta X_{it} + \varepsilon_{it} \qquad \text{if} \quad a_i \geq 0 \tag{4}$$

$$\beta = \alpha_l - \alpha_r \tag{5}$$

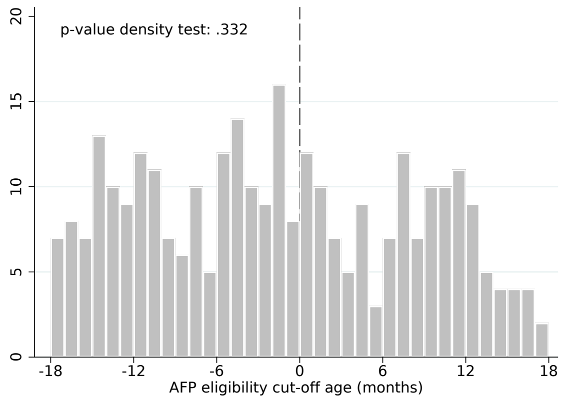
where y_{it} denotes the outcome of individual i at time t, X_{it} is a set of covariates, ε_{it} is the error term and f_l and f_r are unknown functional forms of the assignment variable on each side of the cut-off respectively. The reduced form RD estimate is given by β, the difference between the intercepts of each side of the cut-off.

In our baseline specification, we follow Lee & Lemieux (2010) and use a local linear regression with separate linear trends and a rectangular kernel density on each side of the cut-off. While we consider multiple outcome variables in our analyses, we keep our bandwidth fixed in our baseline specifications. Although different outcomes have different optimal bandwidths, we choose a bandwidth of 12 months (of age) which is in the neighborhood of the optimal bandwidth suggested by Imbens & Kalyanaraman (2012) for two of our key outcome variables AFP benefits and (total) social security benefits. We also show that our estimates are relatively stable to bandwidth selection in Section 5.4.

4.2 Threats to identification

The validity of our RD design requires that individuals are not able to precisely manipulate the assignment variable, which in our setting is their age at the bankruptcy date. As individuals cannot manipulate age, the only possible way to manipulate the assignment variable is manipulation of the bankruptcy date itself. While we consider this is highly implausible, we carry out the standard validity checks for RD designs. Figure 1 shows the distribution of the assignment variable around the cut-off. Because our assignment variable is discrete, we follow Frandsen (2017) and perform a formal statistical test for bunching on either side of the cut-off. Reassuringly, the test is unable to reject the null of no bunching.

Figure 1: Distribution of eligibility age around cut-off



Notes: The figure shows the distribution of age (in months; defined as in Equation (1)) around the individual AFP eligibility cut-off. P-value is calculated using the discrete density test of Frandsen (2017). The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date.

If individuals are unable to manipulate the assignment variable, any pre-determined covariate should have the same distribution on either side, close to the cut-off. As a formal test, we run RD regressions with our baseline specifications on worker characteristics as the dependent variable, each measured 24 months prior to the bankruptcy. The point estimates and standard errors are reported in Appendix Table A.1. We also present these results graphically in Appendix Figure A.1. Reassuringly, key covariates such as monthly earnings, tenure, and the number of employees in each firm appear smooth around the cut-off and are insignificant at all conventional levels. One exception is the local DI rate (measured at the municipality level) which is significant at the 5% level. However, based on the large number of covariates that we consider, the probability of observing changes in one covariate around the cut-off is quite large. Additionally, the correlations between the local DI rate and the outcome variables we consider are very small and close to zero. When we perform a joint test for all covariates, we cannot reject the null of no manipulation at any conventional level as reported in Appendix Table A.1.

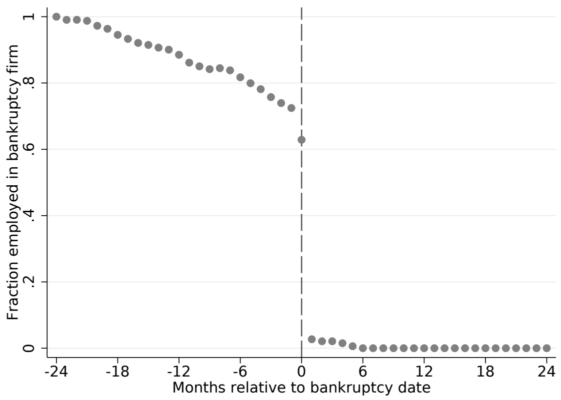
4.3 Interpretation of estimates

While a significant share of the workers are indeed displaced when their respective employer becomes bankrupt, not everyone is displaced at this point in time. As our main estimation sample consists of workers 24 months prior to the bankruptcy, some workers may be displaced or leave the firm for other reasons before the actual bankruptcy. While we impose this restriction to avoid selection, workers may still lose eligibility for AFP despite being initially eligible. Additionally, some workers may not be displaced at all as new owners may keep a share of the workforce in the event of a takeover, while other workers may become re-employed by a different employer. These individuals may become eligible for AFP at a later stage despite being initially ineligible. While we cannot perfectly distinguish between the firms that get new owners ("takeover firms") and other firms, we can investigate the overall employment rates around bankruptcy date of the initial employer.

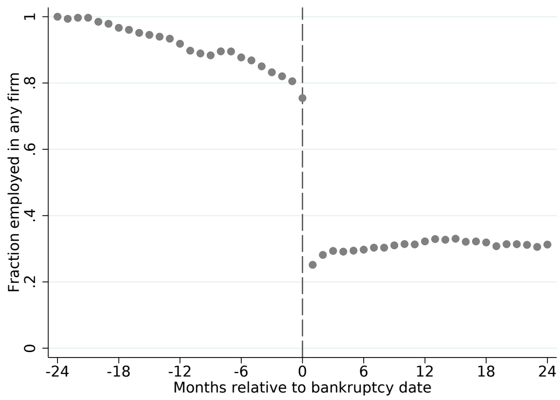
Figure 2 shows the monthly employment rates for our main estimation sample of AFP-workers around the bankruptcy of the firm. In panel 2a, we plot the fraction of workers employed by the bankruptcy firm. While everyone was employed 24 months prior to bankruptcy by construction, just over 60 percent of workers were still employed by the firm in the month of bankruptcy. This indicates that a significant share of workers either left early or that the actual lay-off occurred before the bankruptcy date. There were very few who were still employed by the firm in the months after bankruptcy. In panel 2b, we plot the fraction of workers who were employed by any firm around the bankruptcy date of the original firm. Around 80 percent of workers were still employed in the month of bankruptcy, while around 25 percent were employed in the month after bankruptcy. This suggests that a substantial share of workers were re-employed either by new owners of the bankruptcy firm or by a different firm, and may gain eligibility for AFP despite being initially ineligible.

Figure 2: Employment around bankruptcy date

(a) Fraction employed by bankruptcy firm



(b) Fraction employed by any firm



Notes: The figures show the fraction of individuals employed by bankruptcy firm (left) and any firm (right) relative to the month of bankruptcy. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date.

For perfect identification of exogenous loss (or gain) of access to AFP, we would ideally want to observe exogenous shocks to eligibility directly. However, we can only observe the age related to the day of the bankruptcy serving as an instrument for eligibility. While we are able to construct a measure of eligibility based on the various criteria, we cannot observe actual eligibility for AFP directly. It is also not clear how to define eligibility for AFP in our setting as workers who are initially ineligible may regain eligibility at a later stage if they become re-employed in a covered firm. In our main empirical approach, we therefore report reduced form estimates from the RD model outlined in Equations 3 and 4 which yields the intention-to-treat effect (ITT) of optional early retirement. These estimates can be interpreted as the effect of being initially eligible for AFP based on employment status two years prior to bankruptcy and can be considered as lower bound estimates of optional early retirement.

In an attempt to quantify the effect of optional early retirement, we use an alternative RD model where we use age at bankruptcy as an instrument for our constructed eligibility measure in a fuzzy RD approach. Under certain assumptions, this approach yields the local average treatment effect (LATE), that is the average effect of having the option to retire early for compliers in our sample.[17] While we are concerned about measurement errors in our treatment variable in particular, this approach is useful for better understanding of the effects of having the option to retire early. We report results from our fuzzy RD approach in Section 6.

5 Main results

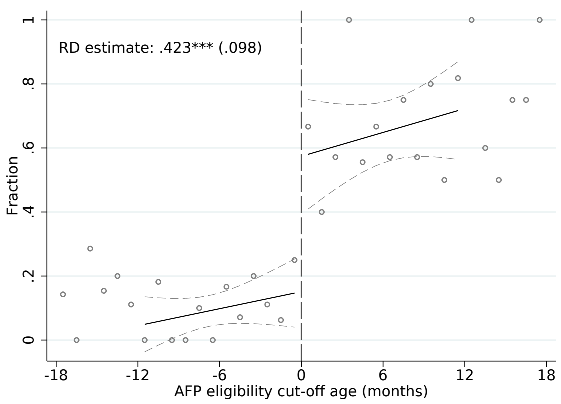
We now turn to our main results. First, we present the direct effect on take-up of AFP benefits from reaching the individual cut-off date before the bankruptcy occurs. We then turn to investigating the effects on subsequent employment, and finally explore whether the loss of eligibility for early retirement benefits induces benefit substitution toward other public transfer programs.

5.1 Direct effects on early retirement

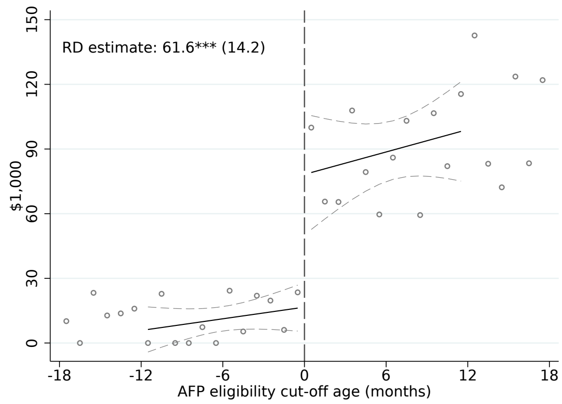
Figure 3 illustrates two measures of the magnitude of the direct treatment effect: AFP claiming (panel a), which is a dummy equal to 1 if individuals have claimed AFP benefits at some point between ages 62–67, and AFP benefits (panel b), which is the cumulative take-up of benefits between ages 62–67 (in $1,000). The left-hand side observations consist of individuals who lose their job before reaching the eligibility cut-off, and thus lose their AFP benefit from that particular firm. However, they might recover the lost benefit by extending their working career or by leaving the firm early and find a new job. Those on the right-hand side are certain to fulfill the eligibility criteria if they are still employed by the firm when the bankruptcy occurs. The closer to the cut-off, the shorter the time-period for which the individual may claim AFP. Those who are just above the cut-off have to claim AFP in the month after they turn 62 years which is the first month they can claim AFP, and the last month they are considered as engaged in employment by the bankruptcy firm.

Figure 3: Graphical evidence of AFP benefit take-up between 62–67 years of age

(a) AFP claiming



(b) AFP benefit take-up ($1,000)



Notes: The figures show the fraction of individuals with some AFP benefit take-up (a) and AFP benefit take-up in $1,000 (b) between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. AFP benefits are measured in 2015 dollars (NOK/USD = 9).

[17]In our setting, the compliers are the workers who become eligible for early retirement because their age is above the eligibility cut-off.

The figures show a visually clear discontinuity at the threshold for the two measures of the magnitude of the direct treatment. Using our RD strategy, we estimate an increase in AFP claiming of about 42 percentage points among individuals who worked in firms that experienced a bankruptcy just after reaching the individual threshold. Equally, we estimate that these individuals claim about $61,600 more total AFP benefits. The estimates suggest that our treatment had a significant impact on the displaced workers' ability to retire early with an AFP benefit.

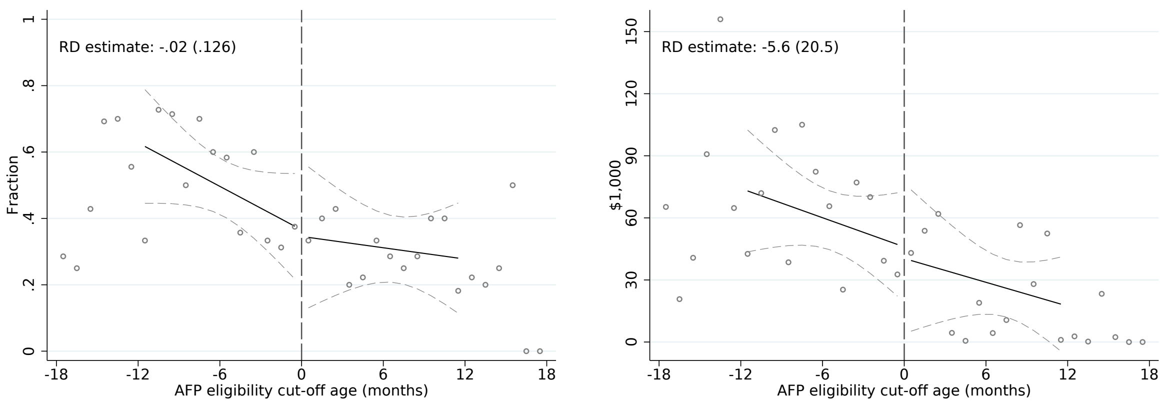
5.2 Effect on subsequent employment

We now ask whether initial AFP eligibility had an impact on re-employment rates and labor market earnings. Theoretically, those who lose eligibility should be induced to extend their working career to redeem some of the lost pension benefits at the expense of foregone leisure which becomes costlier. At the same time, individuals may have a hard time finding a new job as they are relatively close to the standard retirement age of 67 years. Local labor demand could also be an important factor.

Visually, Figure 4a shows that we are unable to detect a discontinuity around the cut-off in terms of employment at the extensive margin between ages 62–67. Similarly, Figure 4b shows that we cannot distinguish between labor market earnings for individuals on either side of the cut-off, with a negligible point estimate of $5,600 which corresponds to about $1,100 in annual earnings. We observe a downward slope in both figures, consistent with the fact that those who are further to the right are older workers at the time of the bankruptcy and thus closer to the standard retirement age.

Figure 4: Graphical evidence of employment at the extensive margin and labor market earnings between 62–67 years of age

(a) Ever employed **(b)** Labor market earnings ($1,000)



Notes: The figures show the fraction of individuals ever engaging in employment (a) and the unrestricted means for each age-bin of labor market earnings in $1,000 (b) between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level and are robust to heteroskedasticity. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date.

We report regression results for the two outcomes in Table 2. The first column reports results of our main specification without controls. In the second column, we report results where we include the pre-determined covariates in Appendix Table A.1 as control variables and year fixed-effects. The inclusion of control variables barely moves our estimates which is reassuring as the pre-determined covariates should have the same distribution on either side of the cut-off. We also report means and standard deviations of the initially ineligible workers (i.e. the workers to the left of the cut-off) and of our comparison sample of all private sector workers in columns 3 and 4, respectively. Our results indicate that workers who lose eligibility for early retirement benefits because of job displacement are either unwilling to, or possibly unable to redeem parts of the lost benefits through re-engaging in the labor market. While this may be surprising from a theoretical point of view, a possible explanation could be that workers could offset some of the lost benefits if they are eligible for other types of social security benefits such as unemployment benefits before they reach the standard retirement age. We investigate this hypothesis in the next section.

Table 2: Effect of initial AFP eligibility on employment and labor market earnings between 62–67 years of age

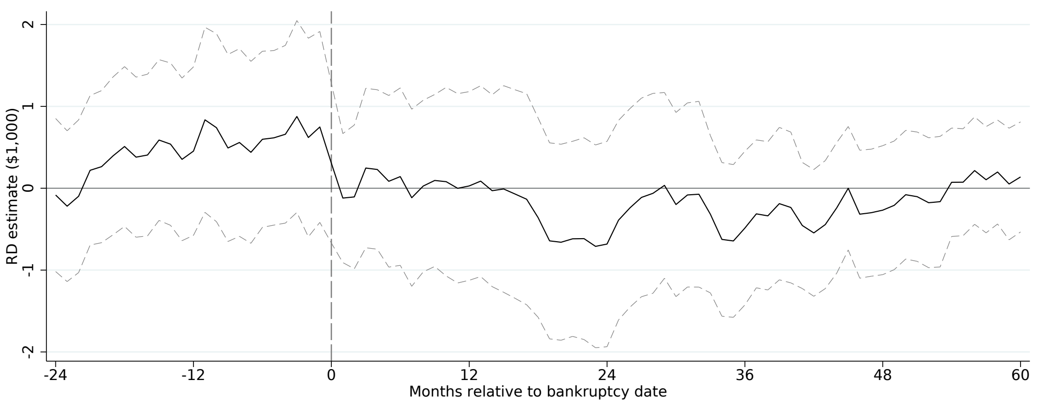
			Mean [SD]	
			Initially	All private
Outcome:	RD estimate (ITT):		ineligible	sector workers
Ever employed	-.020	-.018	.492	.808
	(.126)	(.136)		
Labor market earnings ($1,000)	-5.6	-4.1	59.5	122.0
	(20.5)	(20.3)	[89.0]	[142.4]
Controls	NO	YES		
Number of firms	127	127	82	48,451
Number of individuals	223	223	120	141,122

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome. Controls in the alternative specification include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Initially ineligible are defined as the estimation sample to the left of the cut-off. The comparison sample of all private sector workers includes individuals who were employed by a private sector firm when aged 57–59 years (excluding bankruptcies). Earnings are measured in 2015 dollars (NOK/USD = 9).

One might argue that the employment effect can be affected by the timing of when the bankruptcy occurs, perhaps due to anticipation in the pre-period and increasing job-searching effort in the post-period. Therefore, we explore whether the RD effect is stable over time relative to the bankruptcy date. This also serves partly as a robustness check of our main result. We compute separate RD point estimates for each month m in the time span $m \in (-24, 60)$ for labor market earnings. The results are presented in Figure 5.

Figure 5: Labor supply effects over time

(a) Earnings ($1,000)



Notes: The figures show separate ITT estimates of labor market earnings (in $1,000) for each month relative to bankruptcy date. The ITT effects are estimated by local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. Point estimates are represented by the black solid line, and the dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings are measured in 2015 dollars (NOK/USD = 9).

We observe that the ITT estimate on labor market earnings is very close to zero in our initial time period 24 months before bankruptcy, and then increases somewhat during the months leading up to bankruptcy. While the effect is not significant for either of these months, we observe a sharp and sizable drop in the month after the bankruptcy for which the effect remains roughly stable around zero. This might suggest that we are unable to find an effect on labor supply in the months after bankruptcy because of noise in the months prior. We therefore repeat this exercise for the sample of workers who were employed by the bankruptcy firm 12 months before and 1 month before bankruptcy as robustness checks, shown in Appendix Figure A.3. As the figures show, we are still unable to find a significant labor market earnings effect, with point estimates very stable around zero. This suggests that the additional "early leavers" in our initial estimation sample do not affect our point estimates substantially, providing further evidence of lack of labor supply responses.

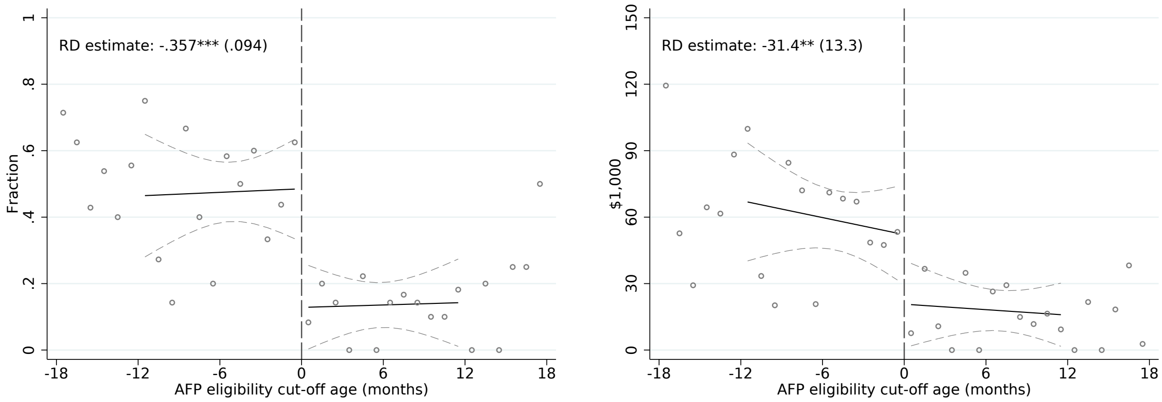
5.3 Benefit substitution

As reported in the previous section, we were unable to find any effects of lost AFP eligibility on re-employment rates. A possible explanation for this could be that workers were able to offset some of the lost benefits through take-up of other social security benefits depending on eligibility. In particular, Bratsberg *et al.* (2013) showed that a large share of DI claims in Norway could be attributed to job displacements. We therefore start our analysis by investigating benefit substitution toward DI benefits. As explained in Section 2, the DI benefit in our sample period was essentially equivalent to the AFP, meaning that given the choice of AFP or DI, all else equal, workers should in principle be financially indifferent between the two benefit programs.

Disability insurance (DI) Figure 6 shows the fraction of individuals who claim DI benefits at some point between ages 62–67 (panel 6a) and the cumulative DI benefit take-up between ages 62–67 (panel 6b) around the cut-off. From panel 6a, we observe a clear discontinuity in the likelihood of claiming DI benefits depending on initial AFP eligibility. Our reduced-form RD-estimate indicates that DI claiming is about 36 percentage points lower among individuals who worked in firms where the bankruptcy occurred just after they reached the individual age threshold. As about half of those who were just initially ineligible for AFP claim DI benefits, the effect of reaching the threshold translates to a reduction in DI claiming by about 75 percent. Panel 6b shows the corresponding effect on cumulative DI benefit take-up (in $1,000). Workers who retain eligibility for AFP claim about $31,400 less DI benefits between ages 62–67, or about half of the DI benefits that individuals who do not retain eligibility receive.

Figure 6: Graphical evidence of benefit substitution towards DI between 62–67 years of age

(**a**) Ever on DI (**b**) DI benefits ($1,000)



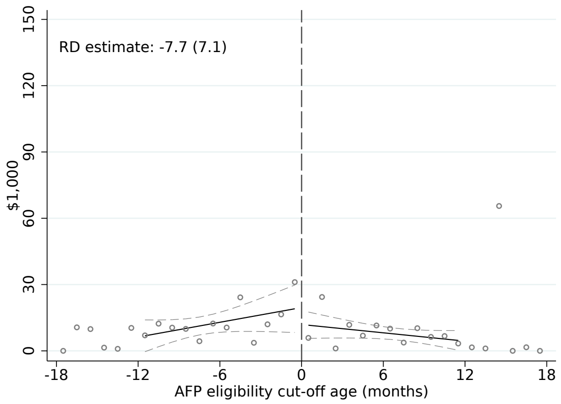
Notes: The figures show the fraction of individuals ever on DI (a) and the unrestricted means for each age-bin of cumulative DI take-up in $1,000 (b) between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Benefits are measured in 2015 dollars (NOK/USD = 9).

The RD estimate for AFP benefits in panel 3b suggested that those who reached the individual eligibility age before bankruptcy date increased their take-up of AFP benefits by about $61,600. Our results thus indicate that about half the lost benefits are replaced by DI benefits. The estimates are highly significant, and we interpret this as clear evidence of program substitution toward DI benefits.

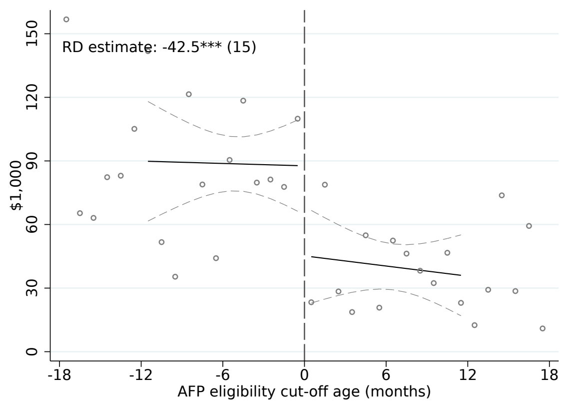
Unemployment insurance (UI) and other public transfers We now investigate whether individuals who were initially ineligible offset some of the lost AFP benefits through take-up of unemployment insurance. Additionally, we pool all public transfers (excluding AFP and old-age pensions) in order to estimate benefit substitution toward all relevant parts of the social security system. Figure 7 shows the cumulative take-up of UI benefits (panel 7a) and total public transfers (panel 7b) between ages 62–67 years (in $1,000). Although we estimate that individuals who were just initially eligible claimed less UI benefits, this effect is not significant at conventional levels. However, workers in our sample are only eligible for UI benefits for up to 2 years. As most individuals close to the cut-off are just a few months shy of turning 61 years when bankruptcy occurs, most individuals would have exhausted their UI spell before turning 62 years.[18] Panel 7b shows that initially ineligible individuals claimed significantly more non-pension public transfers. Our point estimate indicates that they claim about $42,500 more between ages 62–67, where we estimated that $31,400 is DI benefits and $7,700 is UI benefits. This suggests that a negligible $3,400 is replaced by other social security benefits.

Figure 7: Graphical evidence of unemployment insurance and total social insurance benefit take-up ($1,000) between 62–67 years of age

(a) UI benefits



(b) Total public transfers



Notes: The figures show unrestricted means for each age-bin of cumulative UI take-up and total social insurance benefit take-up in $1,000 between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Benefits are measured in 2015 dollars (NOK/USD = 9).

Table 3 reports point estimates of AFP benefits and program substitution toward social insurance benefits. While total program substitution effects are slightly lower if we include control variables, estimates are qualitatively similar. Our estimates indicate that individuals who were just initially eligible for AFP claim about half of non-pension social security benefits compared to those who were initially ineligible. While AFP benefit take-up is $61,600 higher among workers who retain eligibility, about $42,500 are replaced with other social security benefits among those who are initially ineligible, equivalent to a replacement rate of about 69 percent. Of those, about 51 percent is DI benefits and 13 percent is UI benefits. We interpret this as substantial benefit substitution, as those who are initially ineligible due to the job displacement substantially increase take-up of other social transfers.

[18]We consider program complementarity between AFP and UI highly unlikely between ages 62–67 years as eligible individuals can claim AFP from age 62.

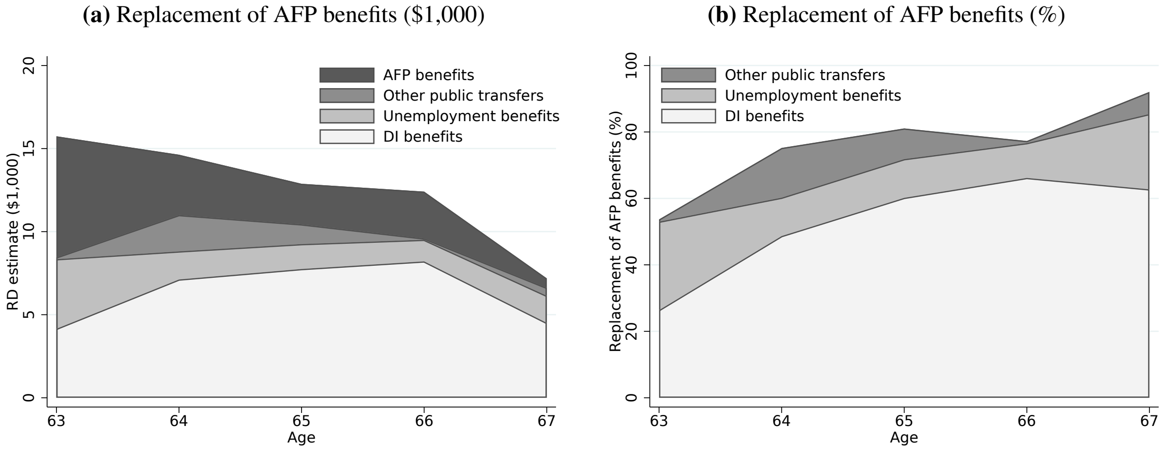
Table 3: Effect of initial AFP eligibility on cumulative social insurance benefit take-up ($1,000) between 62–67 years of age

			Mean [SD]	
Outcome:	RD estimate (ITT):		Initially ineligible	All private sector workers
AFP benefits	61.6***	57.6***	11.4	35.3
	(14.2)	(15.7)	[35.0]	[59.4]
Program substitution:				
Total public transfers	-42.5***	-36.2**	88.8	69.0
	(15.0)	(16.1)	[74.5]	[98.7]
• DI benefits	-31.4**	-24.6*	59.5	25.1
	(13.3)	(14.5)	[72.1]	[53.6]
• Unemployment benefits	-7.7	-9.5	13.2	2.7
	(7.1)	(7.2)	[24.9]	[13.8]
Controls	NO	YES		
Number of firms	127	127	82	48,451
Number of individuals	223	223	120	141,122

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome. Controls in the alternative specification include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Initially ineligible are defined as the estimation sample to the left of the cut-off. The comparison sample of all private sector workers includes individuals who were employed by a private sector firm when aged 57–59 years (excluding bankruptcies). Benefits are measured in 2015 dollars (NOK/USD = 9).

Effect for each age group To further investigate how those who become displaced just before the age cut-off redeem their lost benefits in terms of increased take-up of other public transfers, we run separate RD regressions for each age group. The point estimates are reported in Appendix Table A.2. Figure 8 illustrates the effects graphically. In panel 8a, the darkest area, spanning from zero, is the ITT estimate on AFP benefit take-up for each age, e.g. just reaching the individual threshold implies an increased take-up of AFP benefits by just over $15,000 at age 63. The three lighter stacked areas show how those who are initially ineligible redeem the lost benefits at each age, mainly due to lower AFP take-up among the oldest individuals while take-up of other social benefits is fairly stable across the age groups. We observe that take-up of DI benefits is by far the largest substitute, and that the degree of substitution is increasing in age. This is further illustrated in panel 8b, showing the effects on take-up of DI benefits, UI benefits and other public transfers relative to the effect on take-up of AFP benefits for each age. We observe that the increased replacement rate mainly is driven by increased replacement through take-up of DI benefits.

Figure 8: Graphical illustration of program substitution



Notes: Panel (a) illustrates the ITT effect for each outcome and each age (in $1,000). The total area is the ITT effect of AFP benefits, while the other shaded areas illustrate the ITT effect of each social insurance benefit. Panel (b) illustrates the same effects, but relative to of the ITT effect of AFP benefit take-up. The ITT effects are estimated by local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Benefits are measured in 2015 dollars (NOK/USD = 9).

5.4 Robustness analysis

To verify the validity of our main results, we conduct a series of robustness checks. In Table 4 we present eight alternative specifications in addition to our main specification which uses a rectangular kernel and 12 months of bandwidth on each side of the cut-off. We observe that in all our robustness checks, estimates remain fairly close to our baseline specification. Take-up of AFP benefits are positive and significant for all specifications, with the point estimates being quite stable across specifications. For total public transfers (excluding pensions) and DI benefit take-up, we observe that the point estimates are negative for all our specifications and are close in magnitude. For total public transfers, all specifications are significant at the 10% level.

The first specification in Table 4 is our baseline RD estimates of the cumulative outcomes between ages 62–67. The second row adds control variables which include the pre-determined variables we use for balancing (see Appendix Table A.1) and year fixed-effects, which we observe has little impact on our main cumulative outcomes. Next, we use separate quadratic trends on each side of the discontinuity instead of separate linear trends. We observe that estimates are less precisely estimated and a magnitude larger. In specifications (iv) and (v) we check whether a local linear specification is appropriate when we deviate from the baseline choice of bandwidth. Particularly, we report estimates reducing the bandwidth by 50 percent (from 12 to 6 months) and increasing the bandwidth by 50 percent (18 months). We observe that the point estimates are very similar to the baseline specification. In Appendix Figure A.2 we extend this exercise by plotting the RD estimates with confidence intervals for each outcome. Combining the evidence from specifications (iv) and (v) with the graphical evidence in Appendix Figure A.2, we conclude that the estimates are very stable to the choice of bandwidth when we use linear trends. This suggests that linearity is a reasonable approximation to the trends around the cut-off. In specification (vi) we use a triangular kernel (rather than rectangular kernel) which has negligible impact on our estimates. Specifications (vii) and (viii) change the pre-determination of employment status in bankruptcy firms from 24 months before bankruptcy to 12 months before and 1 month before, respectively. Reassuringly, the point estimates are quite similar to our main specification although estimates in the latter specification are less precise due to the smaller sample size. Finally, specification (ix) includes bankruptcies where at least 1/3 of (all) employees switched to the same firm which we deemed as "spurious" bankruptcies. As expected, the estimated effects are smaller in magnitude when we include these firms as a larger share of workers did not experience a job displacement but were rather collectively moved to a new firm.

Table 4: Specification checks

				Program substitution:			
		AFP benefits	Labor market earnings	Total public transfers	DI benefits	Unemployment benefits	Obs <Firms>
Column:		(1)	(2)	(3)	(4)	(5)	(6)
i:	Baseline RD estimate	61.6***	-5.6	-42.5***	-31.4**	-7.7	223
		(14.2)	(20.5)	(15.0)	(13.3)	(7.1)	<127>
ii:	With controls	57.6***	-4.1	-36.2**	-24.6*	-9.5	223
		(15.7)	(20.3)	(16.1)	(14.5)	(7.2)	<127>
iii:	Quadratic trends	73.5***	30.2	-67.0***	-40.4**	-12.6	223
		(19.2)	(34.8)	(23.0)	(20.2)	(11.8)	<127>
iv:	Bandwidth:	75.9***	19.3	-43.6**	-26.9	-9.9	115
	50% lower	(20.3)	(33.2)	(22.2)	(18.6)	(11.0)	<70>
v:	Bandwidth:	64.4***	-8.8	-42.5***	-32.3***	-9.0	305
	50% higher	(12.6)	(17.5)	(13.7)	(12.3)	(5.9)	<160>
vi:	Triangular kernel	66.3***	8.4	-52.0***	-35.0**	-9.6	223
		(14.4)	(24.5)	(16.4)	(13.8)	(8.6)	<127>
vii:	Workers 12 months	60.6***	.4	-44.9***	-34.0**	-5.7	213
	pre-bankruptcy	(14.9)	(21.7)	(17.0)	(15.1)	(7.1)	<124>
viii:	Workers 1 month	62.9***	.4	-37.0*	-27.9	-5.6	163
	pre-bankruptcy	(16.2)	(27.5)	(20.0)	(17.5)	(8.9)	<96>
ix:	With "spurious"	49.0***	-2.3	-26.5*	-18.4	-3.0	290
	bankruptcies	(13.2)	(22.5)	(14.5)	(12.2)	(6.6)	<161>

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows results of local RD regressions for each outcome (in $1,000) and each respective specification. All specifications use linear separate linear trends except specification (iii) which uses separate quadratic trends. Main specification (i) uses a rectangular kernel and 12 months of bandwidth. Controls in specification (ii) include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. Specification (vii) and (viii) includes workers who worked in bankruptcy firm 12 and 1 month respectively before the bankruptcy date (all other specifications include individuals who worked in firm 24 months before bankruptcy). Specification (ix) also includes bankruptcies where at least 1/3 of (all) employees switched to the same firm. The sample consists of individuals employed by a firm with AFP affiliation 24, 12 or 1 month(s) (depending on specification) before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

We also perform a placebo test by using private sector bankruptcy firms *without* AFP coverage in an otherwise similar setup to our baseline sample. As the "cut-off" for these workers does not involve the loss (or gain) of early retirement eligibility, our main outcomes should have the same distribution just before and just after the hypothetical cut-off. The estimated effects of our cumulative outcomes are relegated to Appendix Table A.3 and shown graphically in Appendix Figure A.5. We are unable to reject the null of no difference between workers on each side of the cut-off for any of our main outcomes. There is, as expected, a close-to-zero effect on AFP benefit take-up, as the only way for these individuals to become eligible for AFP benefits is to switch workplace to a firm with AFP coverage and acquire at least three years of tenure. While the point estimate for labor market earnings is positive, and the point estimates for public transfers and DI benefits are negative, the estimates are roughly within one standard error.

5.5 Heterogeneity

As workers in our estimation sample differ somewhat in characteristics compared to the average private sector worker, we further investigate the driving forces behind the main responses. Particularly, Table 1 revealed that workers in our sample are typically male workers in the manufacturing sector. To understand to which extent our results have external validity, we therefore explore heterogeneous effects. Workers' wages and education may also be important; workers with high wages are likely eligible for a higher AFP benefit as the benefit is linked to past earnings, which may result in loss of eligibility for AFP being a larger shock to individuals with higher wages. However, workers with high wages may also have better outside options in the labor market than workers with low wages, and may have lower search costs when unemployed.[19] We therefore expect that workers with higher pre-bankruptcy earnings have higher re-employment rates, and possibly lower program substitution rates.

To determine how the pattern of labor market adaptation and take-up of social benefits differ across worker groups, we use the same initial estimation sample and empirical strategy on subsets of workers. In Table 5 we report estimates of our main cumulative outcomes between ages 62–67 corresponding to differences in gender, pre-bankruptcy earnings, educational attainment and industry.[20]

The estimated coefficients for men indicate that they exhibit similar properties as the full estimation sample. For women, the point estimates are smaller, but also more imprecise mainly due to the small sample size. While we lack precision to provide a definitive answer to whether there are differences between genders, the estimates suggest that men are more likely to respond to the incentive to claim AFP benefits and reduce take-up of other social benefits, while women to a larger extent claim other social security benefits regardless of having the option to retire early.[21]

To explore heterogeneous effects in pre-bankruptcy earnings, we split our sample on earnings (24 months) prior to bankruptcy. As expected, compared to high earnings workers, the effect on AFP benefit take-up is smaller for workers with low earnings (smaller than or equal to the median). This difference is likely somewhat mechanical as low earnings workers have lower accrual of AFP on average. However, we observe that low earnings workers replace almost the entire loss of AFP benefits with other social security benefits, while high earnings workers replace a significantly lower share. In fact, the estimated coefficients for high earnings workers on our social security outcomes are not significantly different from zero at conventional levels. While this suggest that high earnings workers may have better outside options and respond to the labor supply incentives, we observe that the estimated coefficients on labor market earnings, although imprecise, are practically indistinguishable between the two groups.

[19] Similarly, education may be correlated with better outside options, as education is highly correlated with earnings.

[20] For the latter subgroup, we explore manufacturing specifically, as this is the by far largest subgroup of workers within the private sector AFP workers.

[21] When exploring gender differences, we would ideally also want to explore spousal spillover effects. We estimated the effect on spousal outcomes and found no effects on employment or take-up of any social security benefits for the spouse. We emphasize that this should be interpreted with caution due to our small sample size, although the point estimates are close to zero.

Table 5: Subsample analysis of labor market earnings and social insurance benefit take-up ($1,000) between 62–67 years of age

			Program substitution:			
	AFP benefits	Labor market earnings	Total public transfers	DI benefits	Unemployment benefits	Obs <Firms>
Column:	(1)	(2)	(3)	(4)	(5)	(6)
Full sample	61.6***	-5.6	-42.5***	-31.4**	-7.7	223
	(14.2)	(20.5)	(15.0)	(13.3)	(7.1)	<127>
	[11.4]	[59.6]	[88.8]	[59.5]	[13.2]	
Males	70.7***	-8.2	-44.9***	-39.1**	-3.1	173
	(14.8)	(24.8)	(17.4)	(16.2)	(5.7)	<101>
	[11.2]	[66.6]	[89.9]	[62.7]	[11.2]	
Females	29.8	-12.5	-30.0	-7.9	-17.4	50
	(37.7)	(29.3)	(32.8)	(25.3)	(20.5)	<42>
	[12.1]	[36.4]	[85.1]	[49.2]	[19.5]	
High earnings	75.0***	-3.5	-32.7	-28.9	-6.5	108
	(23.9)	(34.0)	(24.8)	(21.2)	(10.0)	<65>
	[13.8]	[74.2]	[91.4]	[57.1]	[14.4]	
Low earnings	51.1***	-4.9	-50.5**	-32.7*	-9.2	115
	(18.3)	(22.6)	(19.9)	(18.3)	(9.2)	<87>
	[9.2]	[45.8]	[86.3]	[61.8]	[12.1]	
High education	21.9	-7.5	-5.2	-5.1	-2.1	75
	(27.7)	(41.4)	(34.8)	(27.8)	(6.1)	<50>
	[18.7]	[89.7]	[68.9]	[42.6]	[7.9]	
Low education	81.8***	.8	-63.3***	-46.3***	-11.5	148
	(16.5)	(21.1)	(17.3)	(16.1)	(9.3)	<99>
	[7.4]	[42.7]	[99.9]	[69.0]	[16.1]	
Manufacturing	47.8***	18.7	-46.9**	-49.4***	4.1	149
	(16.6)	(22.5)	(19.3)	(17.1)	(6.9)	<72>
	[14.3]	[58.5]	[93.7]	[63.9]	[11.9]	
Other industries	96.6***	-53.1	-42.8*	-6.6	-29.7**	74
	(25.7)	(37.9)	(24.7)	(19.0)	(12.0)	<55>
	[6.4]	[61.4]	[80.0]	[51.6]	[15.5]	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity. Independent means of initially ineligible (the sample to the left of cut-off) in brackets.
Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in $1,000) and each subgroup. High earnings are defined as larger than median 24 months before bankruptcy date, and low earnings otherwise. High education is defined as completed high school or more, and low education otherwise. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

When we split our sample on educational attainment (high education is defined as completed high school and low education otherwise), we find a quite similar pattern as when we split our sample on earnings prior to bankruptcy, although with one notable exception; the point estimate on AFP benefits is large and highly significant for workers with low education, but rather low and insignificant for workers with high education. The point estimates on our social security outcomes are significantly larger for low-education workers and gives relatively clear evidence of responses being driven by low education workers.

In our estimation sample, around 68 percent of workers are employed in the manufacturing industry compared to 30 percent of all private sector firms. To investigate the external validity of our findings, we therefore do separate estimations for workers in the manufacturing industry and workers who were employed in other industries. We observe that point estimates on AFP benefits are smaller for workers in the manufacturing industry. However, this is not because of differences in wages; in fact, workers in the manufacturing have comparable earnings to workers in other industries prior to bankruptcy. While the point estimates of total public transfers are similar between the two subgroups, manufacturing workers replace a much larger share of the lost AFP benefits with other social security benefits compared to other workers. In fact, the point estimates suggest that manufacturing workers replace the entire lost AFP benefits with DI benefits, suggesting that workers in more physically demanding jobs are more inclined to be eligible and possibly apply for DI benefits. There is no evidence for such replacement for workers in other industries. In fact, there is clear evidence of workers in other industries replacing some of the lost AFP benefits with unemployment benefits, with a coefficient significant at the 5% level. Interestingly, the point estimate of labor market earnings is negative and relatively large for workers in non-manufacturing industries compared to manufacturing workers. While not significant at conventional levels, it may seem that the lack of a labor supply response for our main estimation sample could be driven by manufacturing workers. A possible explanation for this could be because of low local labor demand, and in particular for workers with specific occupational skills, as a relatively large share of the manufacturing firms in our sample were relatively large firms located in small towns.

6 Instrumental variable estimates

While our main findings show that being initially eligible for AFP based on employment status 24 months prior to bankruptcy affects AFP claiming and take-up of social security benefits, these findings may underestimate the true effects of being eligible for AFP as some initially eligible individuals may leave the firm early and not satisfy the eligibility criteria, and some individuals who were initially ineligible may regain eligibility if re-employed in a different firm covered by the AFP scheme. In this section, we therefore use the individual eligibility age as an instrument for AFP eligibility in an instrumental variables (IV) setup in an attempt to estimate the true effect of optional early retirement. This approach yields the local average treatment effect (LATE), that is the average effect of having the option to retire early for compliers in our sample (Imbens & Angrist, 1994). In our setting, the compliers are workers who become eligible for early retirement because their age is above the eligibility cut-off but would not have become eligible otherwise. In our alternative fuzzy RD design, the empirical model can be summarized by the following two equations:

$$E_i = \alpha_0 + \alpha_1 \mathbb{Z}_{a_i \geq 0} + f(a_i) + \delta X_{it} + \varepsilon_{it} \tag{6}$$

$$y_{it} = \beta_0 + \beta_1 E_i + f(a_i) + \delta X_{it} + \varepsilon_{it} \tag{7}$$

where E_i takes the value one if individual i is eligible for AFP and zero otherwise, X_{it} is a set of covariates and y_{it} is the outcome of interest for individual i at time t, ε_{it} is the error term and f is an unknown functional form of the assignment variable. The indicator variable $\mathbb{Z}_{a_i \geq 0}$ is the instrumental variable, taking the values:

$$\mathbb{Z}_{a_i \geq 0} = \begin{cases} 0 & \text{if} \quad a_i < 0 \\ 1 & \text{if} \quad a_i \geq 0 \end{cases} \tag{8}$$

where a_i is defined as in Equation (1), meaning that if individuals' age at the bankruptcy date is above the threshold, the instrument takes the value one, and zero otherwise. It is crucial that $\mathbb{Z}$ is uncorrelated with potential measurement error in E. While we are able to construct a fairly accurate measure for eligibility by determining who is eligible based on the criteria outlined in Section 2.1, we cannot observe eligibility directly. Because of this, it is possible that our treatment variable is measured with some errors.[22] While measurement error in the treatment variable in an IV setting creates a bias in the estimator (see e.g. Lewbel, 2007; Jiang & Ding, 2020; Yanagi, 2019), Ura (2018) and Yanagi (2019) showed that under the assumption that the instrumental variable is uncorrelated with the measurement error in the treatment variable (i.e. the probability of misclassification of treatment), the Wald estimator gives an upper bound estimate in absolute value of the true coefficient.

Additionally, it is not clear how to define the treatment in our setting as individuals' eligibility status could change depending on employment status and the various other criteria for AFP. Therefore, some non-treated or treated individuals could be partially treated. We decide to define our treatment as eligible for AFP at some point between ages 62–67 years as most partially treated individuals will regain eligibility shortly after the earliest point of withdrawal (e.g. at ages 62 or 63). In practice, this means that our estimates will serve as upper bound estimates as some individuals we define as treated will be partially treated. As potential measurement errors in our treatment variable will also contribute to overestimate the true effects, we therefore emphasize that the IV estimates should be interpreted as upper bound estimates of the effect of access to early retirement. However, we argue that the IV estimates are useful for scaling of our main findings and interpretation of the true effect of AFP eligibility on our outcomes.

A key identifying assumption for the IV to be valid is the exclusion restriction, i.e. the instrument must be conditionally independent of potential outcomes. We argue that the exclusion restriction holds in our case as just reaching a certain age in itself does not affect employment or take-up of other social security benefits, but only because age affects eligibility. As a further argument for this claim, our placebo estimates of non-AFP workers reported in Appendix Table A.3 indicate that outcomes of ineligible individuals are indeed similar around the age-threshold. Another key identifying assumption is monotonicity in responses. We consider "defiers" highly unlikely in our setting as this would imply that some individuals become eligible because age is just below the threshold but would not have become eligible otherwise. Finally, the instrument must be relevant, i.e. just reaching the individual age-threshold must affect eligibility. We verify this when summarizing our results.

The results of our fuzzy RD model are presented in Table 6. For comparison with our main estimates, we also include the ITT estimates from the reduced form RD model. We emphasize that our instrument has a high predictive power of the treatment variable. Our first-stage estimate shows that the probability of being eligible for AFP is among 70 percentage points higher among those who just reached the

[22] Out of the 199 individuals we classified as ineligible following the standard criteria, 4 individuals in our sample or around 2 percent were observed with actual take-up of AFP. Unfortunately, we are unable to provide a measure of the number of individuals we classify as eligible whose true status are in fact ineligible as we cannot distinguish these individuals from never-takers of AFP.

individual cut-off age at the firm bankruptcy date. We estimate that compliers increase AFP take-up with \$87,900 and decreases take-up of other social benefits by \$60,600, where \$44,800 of this is due to decreased take-up of DI when becoming eligible for AFP.

Table 6: IV estimates of cumulative outcomes (\$1,000)

Treatment variable:	First stage:					
Eligible for AFP	.70***	.67***				
	(.08)	(.09)			Mean [SD]	
					Initially	All private
Outcome:	IV estimate (2SLS):		Reduced form (ITT):		ineligible	sector workers
AFP benefits	87.9***	85.5***	61.6***	57.6***	11.4	35.3
	(19.5)	(21.0)	(14.2)	(15.7)	[35.0]	[59.4]
Labor market earnings	-8.0	-6.1	-5.6	-4.1	59.5	122.0
	(29.0)	(28.4)	(20.5)	(20.3)	[89.0]	[142.4]
Total public transfers	-60.6***	-53.7**	-42.5***	-36.2**	88.8	69.0
	(21.2)	(21.6)	(15.0)	(16.1)	[74.5]	[98.7]
• DI benefits	-44.8**	-36.5*	-31.4**	-24.6*	59.5	25.1
	(19.5)	(20.4)	(13.3)	(14.5)	[72.1]	[53.6]
• Unemployment benefits	-10.9	-14.0	-7.7	-9.5	13.2	2.7
	(9.7)	(9.4)	(7.1)	(7.2)	[24.9]	[13.8]
Controls	NO	YES	NO	YES		
Number of firms	127	127	127	127	82	48,451
Number of individuals	223	223	223	223	120	141,122

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows the 2SLS estimates of fuzzy RD regressions using AFP eligibility as the treatment variable, and the corresponding reduced form estimates. Both specifications use local linear regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in \$1,000). Controls in the alternative specifications include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

7 Implications

In this section, we assess the implications of our findings for policy and welfare for the displaced workers in our sample. While access to an early retirement program provides better insurance for displaced workers, it could also increase public expenditures through increased benefit payments and decreased tax revenues. However, as we have shown, decreased benefit payments of other social security benefits could offset some of the increased costs. These trade-offs are particularly important in assessing the desirability of the program.

To assess how access to early retirement affects public finances, we estimate our RD model on net public expenditures as the outcome variable, defined as net benefit payments from (all) social security benefits net of payroll taxes from earnings (including income from self-employment). As a rough measure of how access to early retirement affects workers' welfare, we consider disposable income as an outcome variable, defined as total income from social security and earnings net of taxes. Finally, we investigate savings as our third outcome variable defined as the annual change in wealth. To ease interpretation, we do estimations on an annual basis when individuals are between 62 and 67 years of age.[23]

In Table 7, we report IV estimates from our fuzzy RD model as well as ITT estimates from our main reduced form model. As we report estimates at the annual level, we consider individuals' eligibility status for AFP also at the annual level, and cluster standard errors at the individual and firm level. Our estimates indicate that access to the AFP program had only a small impact on public finances. This is not surprising given our previous findings, where we did not find evidence of an effect on labor supply, but relatively large substitution effects onto other social security programs. At the 95% confidence level, our IV estimate suggests that the annual increase in public expenditures is at most $16,400 for compliers in our sample. Our estimates also indicate that access to the AFP program had little impact on the average welfare for individuals. Due to lack of significance, we cannot conclude that access to early retirement increased average disposable income for individuals. However, we can rule out a large decrease in the average welfare for ineligible individuals. At the 95% confidence level, our IV estimate suggest that the annual effect on disposable income is at most $10,300 for compliers in our sample. We are also unable to conclude that access to early retirement had an effect on savings. Note that average savings are positive among initially ineligible. Taken together, this suggests that most ineligible individuals had some source of income.

Table 7: Annual financial costs and benefits ($1,000)

Treatment variable:	First stage:					
Eligible for AFP	.78***	.74***				
	(.08)	(.09)			Mean [SD]	
					Initially	All private
Outcome:	IV estimate (2SLS):		Reduced form (ITT):		ineligible	sector workers
Net public	7.2	7.1	5.6	5.3	14.8	10.1
expenditures	(4.7)	(4.8)	(3.6)	(3.6)	[18.9]	[30.7]
Disposable income	3.8	4.2	2.9	3.1	31.7	40.5
	(3.3)	(2.6)	(2.6)	(1.9)	[11.6]	[23.4]
Savings	1.6	1.7	1.2	1.3	.9	3.2
	(3.1)	(3.1)	(2.5)	(2.3)	[33.5]	[43.0]
Controls	NO	YES	NO	YES		
Number of firms	124	124	124	124	79	48,644
Number of individuals	216	216	216	216	116	138,644
Number of observations	1,224	1,224	1,224	1,224	667	798,228

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the individual and firm level and are robust to heteroskedasticity.
Notes: The table shows the 2SLS estimates of fuzzy RD regressions using AFP eligibility as the treatment variable, and the corresponding reduced form estimates. Both specifications use local linear regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in $1,000). Controls in the alternative specifications include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. Net public expenditures are defined as net benefit payments from all social security programs subtracting payroll taxes from earnings. Disposable income is defined as benefit payments and earnings net of taxes. Savings are defined as the change in annual wealth. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Variables are measured in 2015 dollars (NOK/USD = 9).

[23]To compare these estimates to our main cumulative outcomes, these estimates should therefore be multiplied by 6, as years between ages 62–67 include 6 calendar years.

To further investigate this claim, we follow the standard framework of Imbens & Rubin (1997) and estimate marginal distributions of disposable income under different treatment statuses for compliers. If a larger share of individuals are significantly worse off, this might be of particular interest for policy-makers. More specifically, we use eligibility age above cut-off ($\mathbb{Z}$) as an instrument for AFP eligibility (E) in a standard Imbens & Rubin (1997) framework. The marginal distributions of potential outcomes for compliers g_e where e is treatment status are defined as:

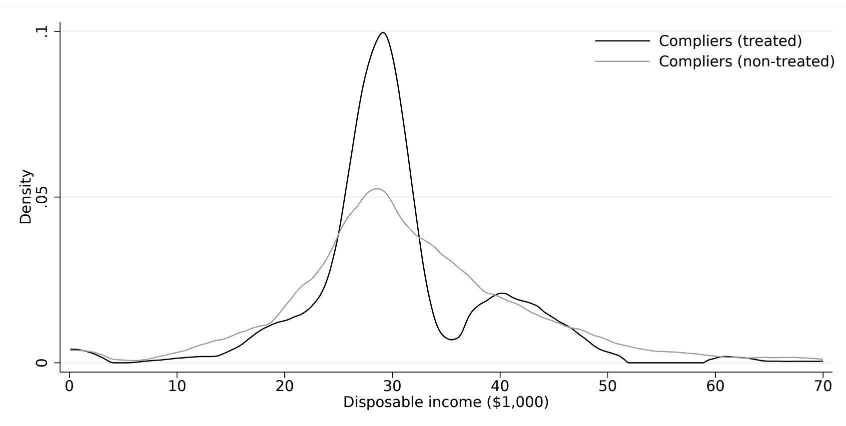
$$g_0(y) = f_{00}(y) \cdot (p_c + p_c)/p_c - f_{10}(y) \cdot p_n/p_c \tag{9}$$

$$g_1(y) = f_{11}(y) \cdot (p_c + p_c)/p_c - f_{01}(y) \cdot p_a/p_c \tag{10}$$

where f_{ze} is the distribution of disposable income for individuals with z being equal to 1 if eligibility age is above cut-off and 0 otherwise and treatment status $e = 0,1$. p_a is the proportion of "always-takers", p_n is the proportion of "never-takers" and p_c is the proportion of compliers. We estimate f using an epanechnikov kernel with optimal bandwidth.

Figure 9 shows the estimated distributions of potential disposable income for compliers in our sample, that is, the individuals who become eligible for AFP because their age is above the eligibility cut-off but would not have become eligible otherwise. Evidently, the disposable income of eligible compliers is more concentrated around the mean with a rather small dispersion. In contrast, the dispersion is higher among ineligible compliers, with a slight tendency of a fatter right-tail, meaning that a larger proportion have higher disposable income. Even though there is evidence of a slightly larger proportion of ineligible compliers having low disposable income, the difference in the lower part of the distribution is almost indistinguishable. This suggests that a very low share of ineligible individuals are significantly worse off because of failing to qualify for early retirement, with most individuals getting some source of income either through participation in the labor market or receiving some type of social security benefit.

Figure 9: Potential outcomes for compliers: Disposable income ($1,000)



Notes: The figure shows distributions of potential disposable income for compliers as defined by Imbens & Rubin (1997) (see text for details). Densities are estimated using an epanechnikov kernel with optimal bandwidth of 1.74. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Disposable income is defined as earnings and benefits excluding taxes and is measured in 2015 dollars (NOK/USD = 9).

8 Conclusion

In this paper, we have asked how the loss of eligibility for early retirement benefits among displaced workers affects re-employment rates and spillover onto other social security programs. We have used detailed register data with information on exact dates of firm bankruptcies which allowed us to causally estimate effects of individual eligibility for early retirement provision.

Using a regression discontinuity research design which compares workers where some end up "reaching the threshold" for eligibility before a firm bankruptcy while some do not, we have been unable to find that early retirement provision induces unintended adverse effects on re-employment. Furthermore, our findings suggest that the loss of early retirement eligibility induces substantial excess take-up of DI benefits among displaced workers. Tight eligibility criteria therefore may induce workers to excessively apply for other social security benefits.

We emphasize that our findings are mainly driven by male, low educated workers in the manufacturing sector. While we do not find significant effects for female workers or high educated workers, we find an offsetting effect on UI benefit take-up among workers in non-manufacturing industries, but no effect on DI for these workers. Moreover, we take several steps to ensure the validity of our findings and show that our main conclusions do not change depending on specifications of the RD design. Reassuringly, our results are not sensitive to the choice of when to pre-determine employment in the firm or the choice of bandwidth.

While access to an early retirement program provides better insurance for displaced workers, it could also increase public expenditures through increased benefit payments and decreased tax revenues. We showed that the early retirement program did not significantly increase public expenditures, as ineligible workers did not increase their labor supply but rather claimed other social security benefits. Therefore, we conclude that provision of early retirement for displaced elderly workers is desirable for policymakers, and that too tight eligibility criteria might be harmful as it induces considerable program substitution.

References

ATKINSON, A., RAINWATER, L., & SMEEDING, T.M. 1995. Income Distributions in OECD countries: Evidence from the Luxembourg Income Study. *OECD Publications and Information Center*.

BASTEN, CHRISTOPH, FAGERENG, ANDREAS, & TELLE, KJETIL. 2016. Saving and Portfolio Allocation Before and After Job Loss. *Journal of Money, Credit and Banking*, 48(2-3), 293–324.

BAUGELIN, OLIVIER, & REMILLON, DELPHINE. 2014. Unemployment insurance and management of the older workforce in a dual labor market: evidence from France. *Labour Economics*, 30(Oct.), 245–264.

BORGHANS, LEX, GIELEN, ANNE C., & LUTTMER, ERZO F. P. 2014. Social Support Substitution and the Earnings Rebound: Evidence from a Regression Discontinuity in Disability Insurance Reform. *American Economic Journal: Economic Policy*, 6(4), 34–70.

BRATSBERG, BERNT, FEVANG, ELISABETH, & RØED, KNUT. 2013. Job loss and disability insurance. *Labour Economics*, 24(Oct.), 137–150.

FRANDSEN, BRIGHAM R. 2017. *Party Bias in Union Representation Elections: Testing for Manipulation in the Regression Discontinuity Design when the Running Variable is Discrete*. Advances in Econometrics, vol. 38. Emerald Publishing Ltd. Chap. Regression Discontinuity Designs, pages 281–315.

GEYER, JOHANNES, & WELTEKE, CLARA. 2017. Closing Routes to Retirement: How Do People Respond? *IZA Disussion Papers 10681, Institute of Labor Economics (IZA)*.

HERNÆS, ERIK, MARKUSSEN, SIMEN, PIGGOT, JOHN, & RØED, KNUT. 2016. Pension Reform and Labor Supply. *Journal of Public Economics*, 142(Oct.), 39–55.

HEYMA, ARJAN, VAN DER WERFF, SIEMEN, NAUTA, AUKJE, & VAN SLOTEN, GUURTJE. 2014. What Makes Older Job-Seekers Attractive to Employers? *De Economist*, 162(4), 397–414.

HUTTUNEN, KRISTIINA, MØEN, JARLE, & SALVANES, KJELL G. 2011. How Destructive Is Creative Destruction? Effects Of Job Loss On Job Mobility, Withdrawal And Income. *Journal of the European Economic Association*, 9(5), 840–870.

HUTTUNEN, KRISTIINA, MØEN, JARLE, & SALVANES, KJELL G. 2018. Job Loss and Regional Mobility. *Journal of Labor Economics*, 36(2), 479–509.

ICHINO, ANDREA, SCHWERDT, GUIDO, WINTER-EBMER, RUDOLF, & ZWEIMÜLLER, JOSEF. 2017. Too old to work, too young to retire? *The Journal of the Economics of Ageing*, 9(C), 14–29.

IMBENS, GUIDO, & KALYANARAMAN, KARTHIK. 2012. Optimal Bandwidth Choice for the Regression Discontinuity Estimator. *The Review of Economic Studies*, 79(3), 933–959.

IMBENS, GUIDO W., & ANGRIST, J. 1994. Identification and estimation of local average treatment effects. *Econometrica*, 62, 467–475.

IMBENS, GUIDO W., & RUBIN, DONALD B. 1997. Estimating Outcome Distributions for Compliers in Instrumental Variable Models. *The Review of Economic Studies*, 64(4), 555–574.

INDERBITZIN, LUKAS, STAUBLI, STEFAN, & ZWEIMÜLLER, JOSEF. 2016. Extended Unemployment Benefits and Early Retirement: Program Complementarity and Program Substitution. *American Economic Journal: Economic Policy*, 8(1), 253–288.

JACOBSON, LOUIS S., LALONDE, ROBERT J., & SULLIVAN, DANIEL G. 1993. Earnings Losses of Displaced Workers. *American Economic Review*, 83(4), 685–709.

JIANG, ZHICHAO, & DING, PENG. 2020. Measurement errors in the binary instrumental variable model. *Biometrika*, 107(1), 238–245.

JOHNSEN, JULIAN VEDELER, VAAGE, KJELL, & WILLÉN, ALEXANDER. 2020. Interactions in Public Policies: Spousal Responses and Program Spillover of Welfare Reforms. *Scandinavian Working Papers in Economics, NHH*, 20(Sept.).

KARLSTROM, ANDERS, PALME, MÅRTEN, & SVENSSON, INGEMAR. 2008 (Apr.). *The Employment Effect of Stricter Rules for Eligibility for DI: Evidence from a Natural Experiment in Sweden*. Research Papers in Economics 2008:3. Stockholm University, Department of Economics.

KYYRÄ, TOMI, & OLLIKAINEN, VIRVE. 2008. To search or not to search? The effects of UI benefit extension for the older unemployed. *Journal of Public Economics*, 92(10-11), 2048–2070.

KYYRÄ, TOMI, & PESOLA, HANNA. 2020. Long-term effects of extended unemployment benefits for older workers. *Labour Economics*, 62(101777).

LASSUS, LORA A. PHILLIPS, LOPEZ, STEVEN, & ROSCIGNO, VINCENT J. 2015. Aging workers and the experience of job loss. *Research in Social Stratification and Mobility*, 41, 81–91.

LEE, DAVID S., & LEMIEUX, THOMAS. 2010. Regression Discontinuity Designs in Economics. *Journal of Economic Literature*, 48(June), 281–355.

LEWBEL, ARTHUR. 2007. Estimation of Average Treatment Effects With Misclassification. *Econometrica*, 75(2), 537–551.

MANOLI, DAYANAND S., & WEBER, ANDREA. 2016. The Effects of the Early Retirement Age on Retirement Decisions. *NBER Working Papers 22561*.

MARMORA, PAUL, & RITTER, MORITZ. 2015. Unemployment and the Retirement Decisions of Older Workers. *Journal of Labor Research*, 36(3), 274–290.

OECD. 2015. *Pensions at a Glance 2015: OECD and G20 Indicators*. OECD Publishing, Paris.

REGE, MARI, SKARDHAMAR, TORBJØRN, TELLE, KJETIL, & VOTRUBA, MARK. 2009 (Sept.). *The effect of plant closure on crime*. Discussion Papers 593. Statistics Norway, Research Department.

STAUBLI, STEFAN. 2011. The impact of stricter criteria for disability insurance on labor force participation. *Journal of Public Economics*, 95(9), 1223–1235.

STAUBLI, STEFAN, & ZWEIMÜLLER, JOSEF. 2013. Does raising the early retirement age increase employment of older workers? *Journal of Public Economics*, 108(C), 17–32.

TUIT, S., & VAN OURS, J.C. 2010. How changes in unemployment benefit duration affect the inflow into unemployment. *Economic Letters*, 109(2), 105–107.

URA, TAKUYA. 2018. Heterogeneous treatment effects with mismeasured endogenous treatment. *Quantitative Economics*, 9(3), 1335–1370.

VIGTEL, TROND CHRISTIAN. 2018. The retirement age and the hiring of senior workers. *Labour Economics*, 51, 247–270.

YANAGI, TAKAHIDE. 2019. Inference on local average treatment effects for misclassified treatment. *Econometric Reviews*, 38(8), 938–960.

Appendix A Additional Tables and Figures

Table A.1: Smoothness of predetermined covariates

	Main est. sample: AFP workers			Placebo sample: Non-AFP workers		
Dependent variable:	*coeff.*	*std. error*	*p-value*	*coeff.*	*std. error*	*p-value*
Female	-.093	(.109)	.395	.050	(.087)	.568
Married	.015	(.113)	.893	-.068	(.090)	.448
Years of education	-.236	(.435)	.588	.324	(.475)	.496
Tenure	-.85	(2.45)	.728	.58	(1.45)	.688
Number of employees	17	(36)	.640	-1.51	(2.89)	.600
Monthly earnings ($1,000)	-.155	(.530)	.771	.387	(.406)	.341
Manufacturing	.073	(.123)	.554	-.177**	(.084)	.036
Full time employment	.077	(.064)	.228	-.121**	(.053)	.023
Local DI rate	.014**	(.007)	.037	.005	(.005)	.307
Local unemp. rate	-.001	(.002)	.687	.000	(.002)	.871
Share senior workers	.026	(.029)	.383	.028	(.042)	.505
Wealth ($1,000)	-27	(24)	.263	.2	(20.1)	.993
Sickness benefits	-.043	(.096)	.655	.002	(.065)	.975
Joint test			.402			.228
Number of individuals (firms)	223	(127)		417	(372)	

*** significant at 1% level, ** significant at 5% level, * significant at 10% level
Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.
Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each pre-determined covariate. Each covariate is measured 24 months before bankruptcy date for each employee. Local DI rate and unemployment rates are measured at the municipality level. The share of senior workers is defined as the share of (all) coworkers above 57 years (excluding self). The main estimation sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The placebo sample consists of individuals employed by a firm without AFP affiliation 24 months before the firm's bankruptcy date, but otherwise satisfied the initial AFP eligibility criteria. Both samples include bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and wealth are measured in 2015 dollars (NOK/USD = 9).

Table A.2: Effect of initial AFP eligibility on labor market earnings and social insurance benefit take-up ($1,000) by age

			Program substitution:			
	AFP benefits	Labor market earnings	Total public transfers	DI benefits	Unemployment benefits	Obs <Firms>
Column:	(1)	(2)	(3)	(4)	(5)	(6)
Total effect	61.6***	-5.6	-42.5***	-31.4**	-7.7	223
62-67 years	(14.2)	(20.5)	(15.0)	(13.3)	(7.1)	<127>
Effect by age:						
62 years	7.2***	-2.7	-1.8	-3.7	1.5	216
	(2.0)	(8.3)	(3.7)	(2.9)	(3.2)	<124>
63 years	15.9***	-1.2	-8.4**	-4.1	-4.2**	214
	(3.2)	(7.7)	(3.5)	(3.1)	(1.9)	<124>
64 years	14.3***	-1.9	-10.8***	-7.0**	-1.7	212
	(3.3)	(5.3)	(3.3)	(3.0)	(1.5)	<123>
65 years	13.3***	1.0	-10.6***	-7.9***	-1.5	211
	(3.0)	(4.3)	(3.3)	(3.0)	(1.2)	<123>
66 years	12.4***	1.2	-9.6***	-8.2***	-1.3	208
	(3.0)	(4.3)	(3.1)	(2.8)	(1.1)	<122>
67 years	7.2***	-3.7	-6.6***	-4.5**	-1.6*	163
	(2.4)	(3.5)	(2.3)	(2.1)	(1.0)	<91>

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.

Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in $1,000). The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

Table A.3: Placebo estimates of cumulative outcomes (in $1,000): Non-AFP bankruptcies

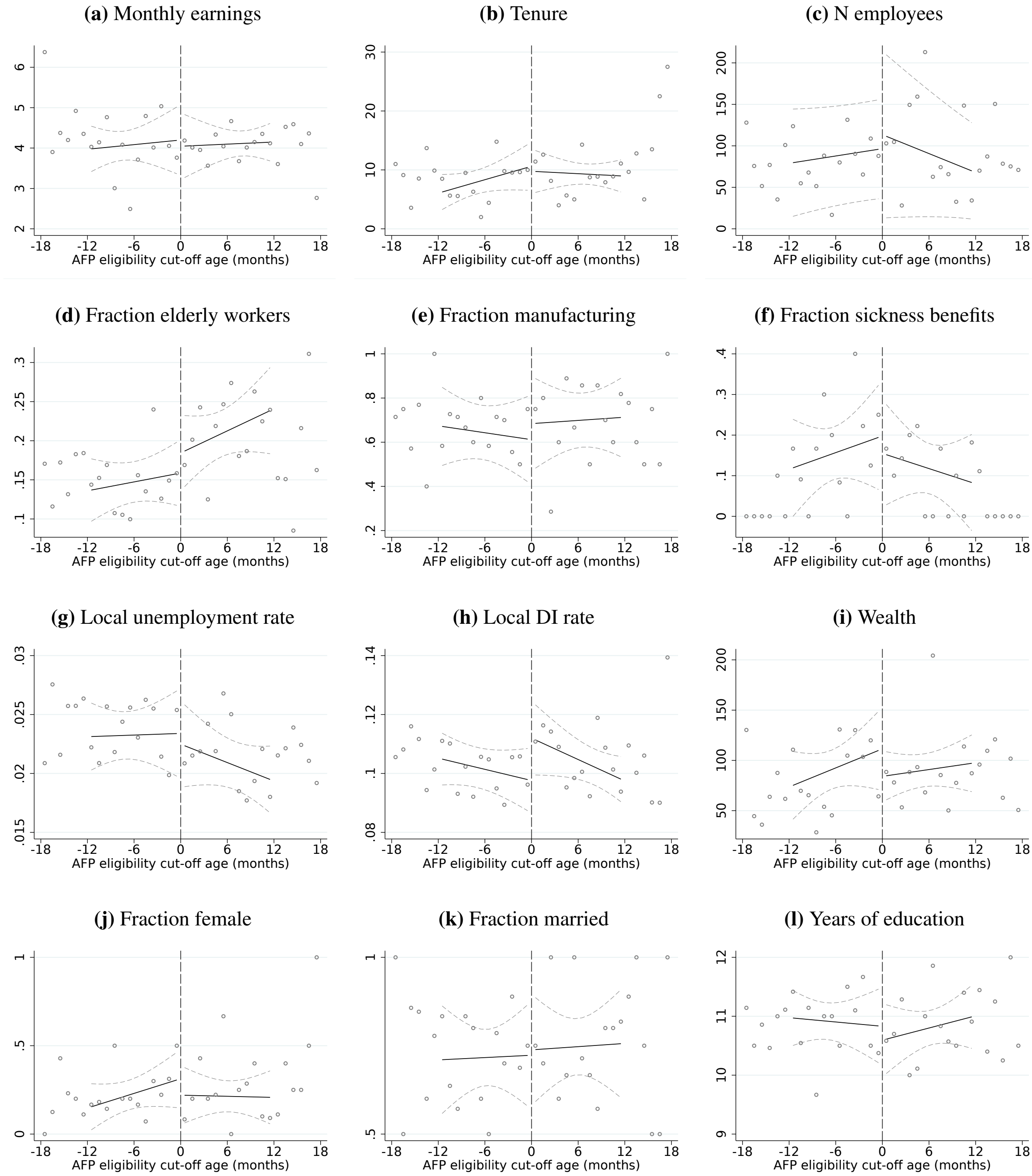
			Mean [SD]	
			Initially	All private
Outcome:	RD estimate (ITT):		ineligible	sector workers
AFP benefits	.8	1.8	4.4	35.3
	(2.1)	(2.1)	[23.5]	[59.4]
Labor market earnings	20.0	19.3	79.0	122.0
	(23.5)	(21.4)	[104.0]	[142.4]
Program substitution:				
Total public transfers	-6.1	-3.3	76.7	69.0
	(16.2)	(15.7)	[83.2]	[98.7]
• DI benefits	-15.2	-11.0	40.0	25.1
	(13.7)	(12.8)	[64.5]	[53.6]
• Unemployment benefits	3.1	5.6	10.9	2.7
	(5.8)	(6.0)	[26.8]	[13.8]
Controls	NO	YES		
Number of firms	372	372	201	48,451
Number of individuals	417	417	221	141,122

*** significant at 1% level, ** significant at 5% level, * significant at 10% level

Standard errors (in parentheses) are clustered at the firm level and are robust to heteroskedasticity.

Notes: The table shows results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off for each outcome (in $1,000). Controls in the alternative specification include the variables used for balancing tests (see Appendix Table A.1) and year fixed-effects. Placebo sample consists of individuals employed by a firm without AFP affiliation 24 months before the firm's bankruptcy date, but otherwise satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Initially ineligible are defined as the sample to the left of the cut-off. The comparison sample of all private sector workers includes individuals who were employed by a private sector firm when aged 57–59 years (excluding bankruptcies). Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

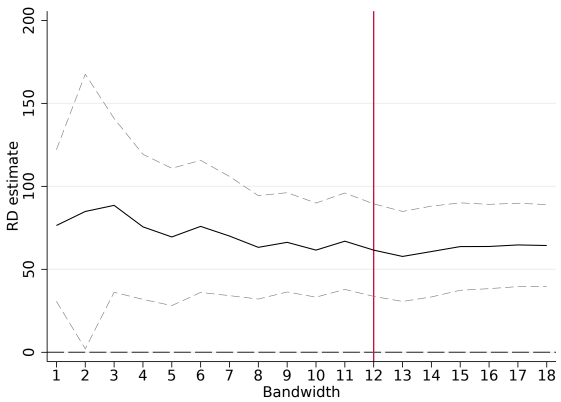
Figure A.1: Characteristics around cut-off



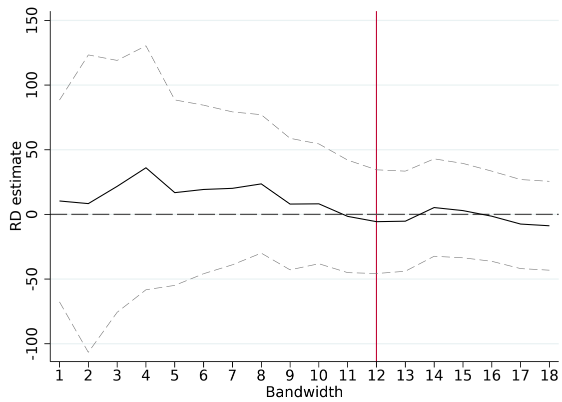
Notes: The figures show the unconditional means of each pre-determined covariate for each monthly age-bin relative to cut-off. Each covariate is measured 24 months before bankruptcy date. The black solid lines illustrate results of local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level and are robust to heteroskedasticity. Local DI rate and unemployment rates are measured at the municipality level. The share of senior workers are defined as the share of (all) coworkers above 57 years (excluding self). The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and wealth are measured in 2015 dollars (NOK/USD = 9).

Figure A.2: RD estimates and bandwidth selection: Cumulative outcomes

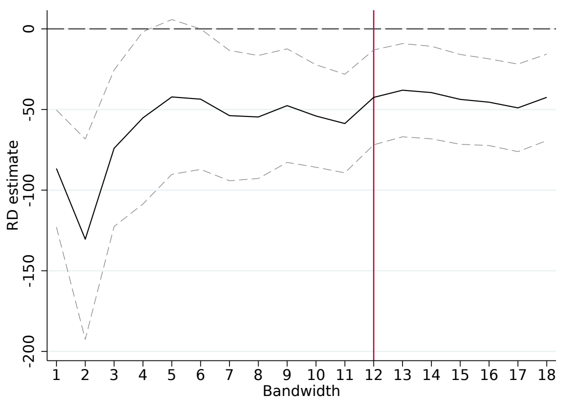
(a) AFP benefit take-up



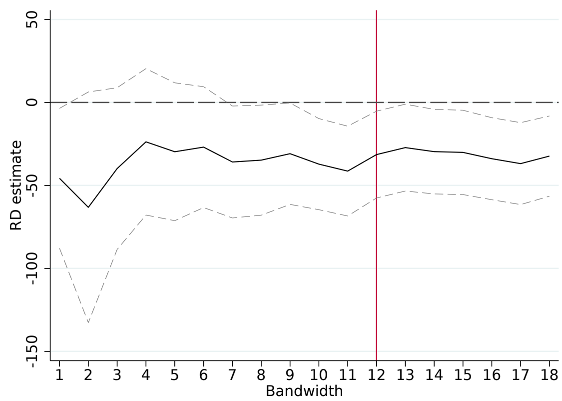
(b) Labor market earnings



(c) Public transfers



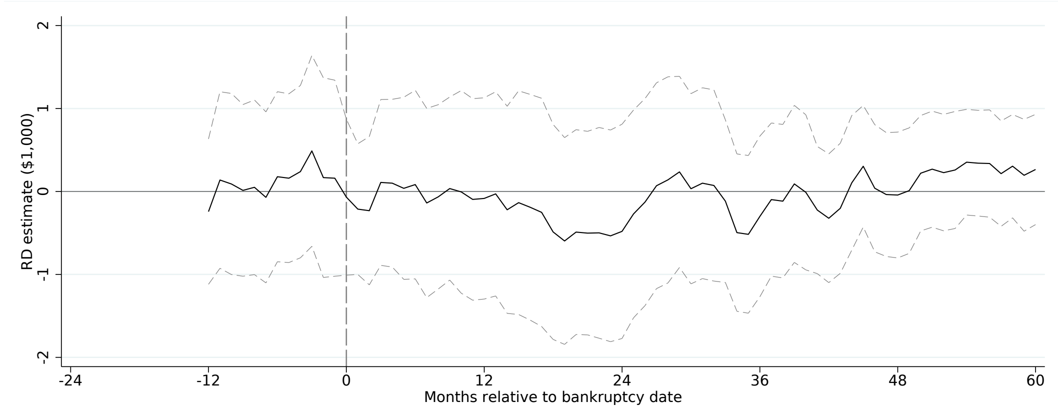
(d) DI benefits



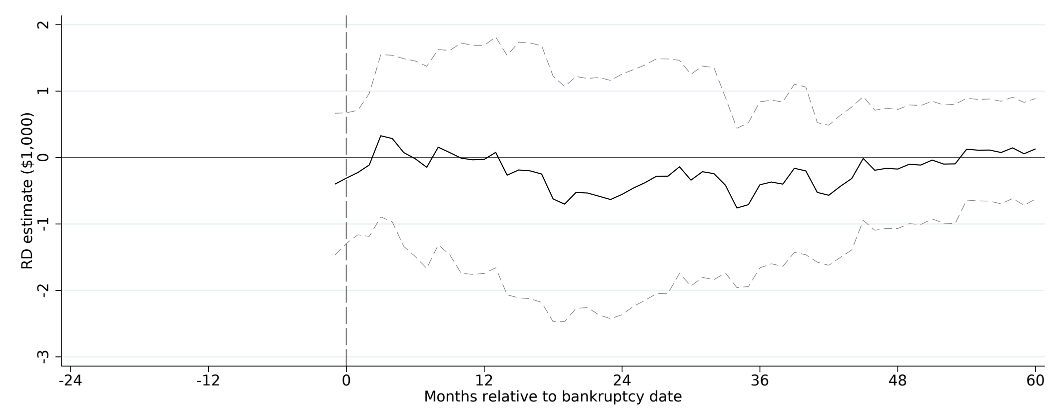
Notes: The figures illustrate the estimated ITT effect for each outcome (in $1,000) for each choice of bandwidth (indicated on the horizontal axis). The ITT effects are estimated by RD regressions using a local linear regression and a rectangular kernel on each side of the cut-off. The red vertical line represents the baseline bandwidth choice of 12 months. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level and are robust to heteroskedasticity. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

Figure A.3: Labor market earnings effects over time (in $1,000) for alternative sample of workers

(a) Workers employed 12 months before bankruptcy

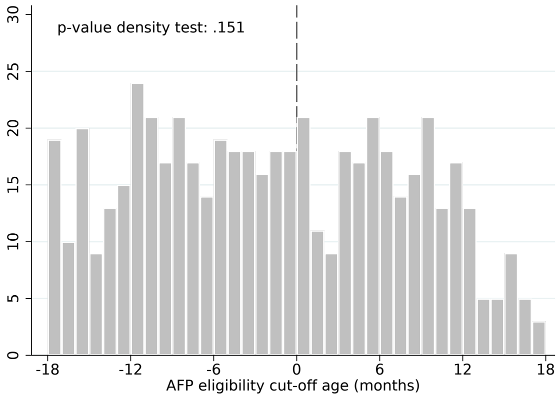


(b) Workers employed 1 month before bankruptcy



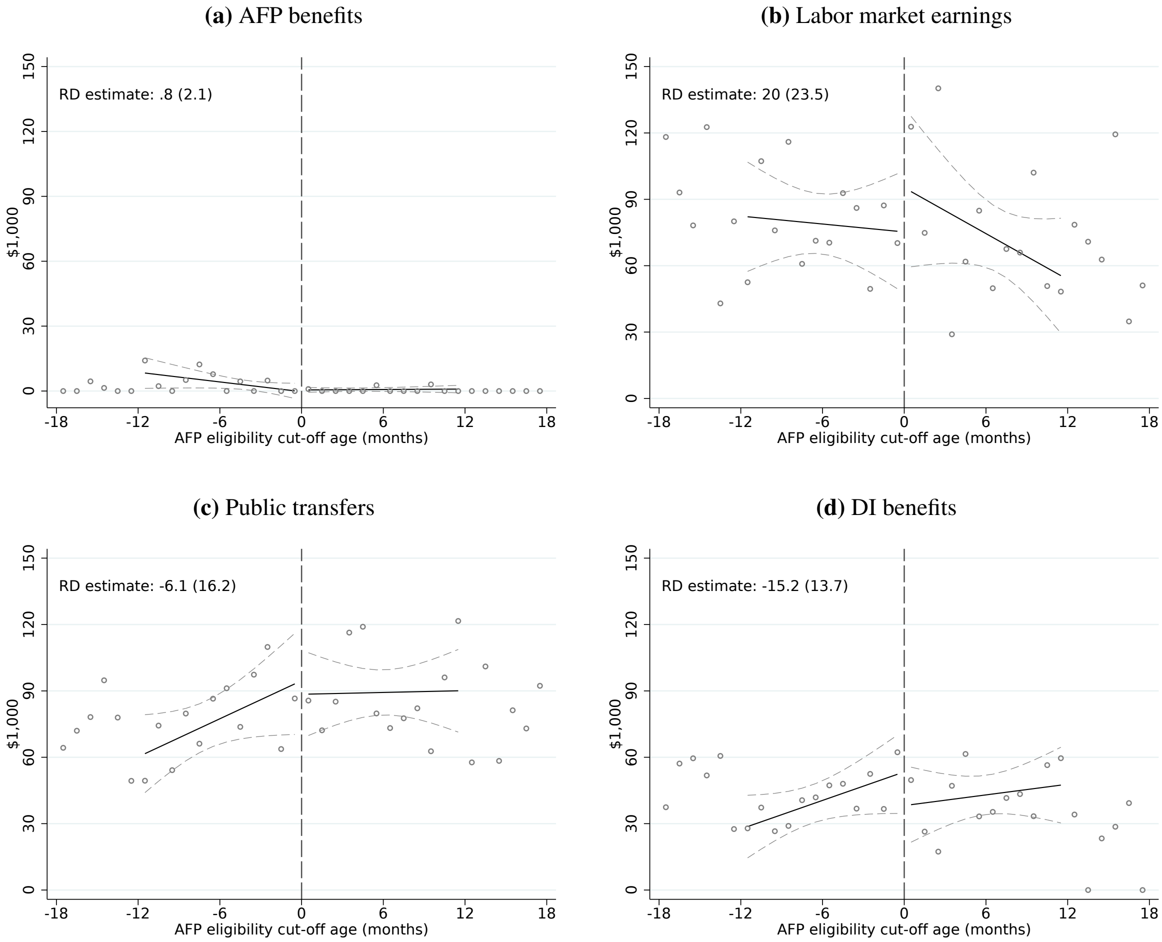
Notes: The figures show separate ITT estimates of labor market earnings (in $1,000) for each month relative to bankruptcy date for the sample of workers employed 12 months before bankruptcy (top graph) and 1 month before bankruptcy (bottom graph). The ITT effects are estimated by local linear RD regressions using a rectangular kernel and 12 months of bandwidth on each side of the cut-off. Point estimates are represented by the black solid line, and the dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. The sample consists of individuals employed by a firm with AFP affiliation 24 months before the firm's bankruptcy date who satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings are measured in 2015 dollars (NOK/USD = 9).

Figure A.4: Distribution of eligibility age around cut-off for placebo sample: Non-AFP bankruptcies



Notes: The figure shows the distribution of age (in months; defined as in equation 1) around the individual eligibility cut-off. P-value is calculated using the discrete density test of Frandsen (2017). The sample consists of individuals employed by a firm without AFP affiliation 24 months before the firm's bankruptcy date, but otherwise satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date.

Figure A.5: Graphical evidence of placebo estimates of cumulative outcomes (in $1,000): Non-AFP bankruptcies



Notes: The figures show unrestricted means for each age-bin of labor market earnings and social insurance benefit take-up in $1,000 between 62–67 years of age, and the estimated regression lines of local linear regressions with rectangular kernel densities and 12 months of bandwidth on each side of the cut-off. The dashed lines represent 95% confidence intervals. Standard errors are clustered at the firm level. Placebo sample consists of individuals employed by a firm without AFP affiliation 24 months before the firm's bankruptcy date, but otherwise satisfied the initial AFP eligibility criteria (see details in Section 3.1). The sample includes bankruptcies between 2001–2010 and workers aged 59–61 years at the firm's bankruptcy date. Earnings and benefits are measured in 2015 dollars (NOK/USD = 9).

Appendix B Old-age pension benefit calculation

In this Appendix, we outlay the details of how the old-age pension benefit levels are calculated in the Norwegian pension system. Except for the "AFP top-up" of about $2,300, the AFP benefit calculation was equivalent to this calculation. The old-age pension benefits consist of three main pillars: a guarantee pension, an income-related pension and a defined-contribution employer-provided pension plan.

Guarantee pension Individuals who had resided in Norway for at least three years between ages 16–66 were entitled to the minimum *guarantee pension*. However, the guarantee pension was *pro-rata* cut with years of residence succeeding 40 years. A full guarantee pension in 2015 was approximately $15,500, and the guarantee pension is indexed annually.[24]

Income pension The income pension was a mapping based on the 20 best years of income after the introduction of *Folketrygden* in 1967.[25] The mapping was based on a *base level* that we denote G, which is set by the government and indexed annually. In 2015, $1G$ was approximately $10,000. Essentially, accrual in a year was calculated as the income exceeding $1G$. For instance, a person earning $5G$ accrued 4 in that year. Only years where the accrual exceeded the average of the 20 best years up until that year would adjust the accrued level. The income pension on accrual was capped at $12G$ which implied, in combination with a decreasing accrual rate for income exceeding a certain threshold, that the replacement rate from the old-age pension system declined with income.[26] In the years between 1967–1991, the accrual rate of pension benefits was 45 percent of the resulting accrued number calculated as above, while in the years 1992–2011, the accrual rate was 42 percent. The average of the 20 years with the highest accrual numbers constituted the final number (*sluttpoengtallet*), which was multiplied by the accrual rate for the number of years of accrual pre-1992 and post-1992, and finally the base amount G, to determine the income pension level. As a minimum, the income pension yielded $1G$, given 40 years of residence (with similar pro-rata cut as the guarantee pension).[27]

Defined-contribution pension plan After 2006, employers had to make a mandatory minimum contribution of 2 percent of earnings of their employees to a *defined contribution* pension plan. A defined benefit scheme was allowed as an alternative, however the defined benefit plan had to be on at least the same level as the expected benefits under the defined contribution plan. Contributions were mandatory for income levels between $1G$–$12G$. Benefits were paid out as life-long annuities from claiming age.

[24] Exchange rate NOK/USD=9. There were different levels depending on marital status and the labor market status of the spouse.

[25] Folketrygden is the Norwegian law governing the social security system, known as the National Insurance Scheme. All residents are automatically member of the National Insurance Scheme.

[26] For the years 1967–1992, years with income exceeding $8G$ only gave one-third accrual for the income exceeding $8G$. For instance, a person earning $9G$ would accrue 7.33 that year ($(8-1)+1\times 0.33$). After 1992, income exceeding $6G$ would only give one third accrual. A person earning $9G$ would then get $(6-1)+3\times 0.33 = 6$.

[27] As an example, say an unmarried individual worked for 40 years, where 25 of those years were pre-1992. The person had a smooth income for all those years equal to $6G$, meaning that the average of the 20 best years gives an accrual of 5. The person claimed old-age pension in 2015, giving approximately:

$$\$10,000 + (0.45\times 5\times 25/40\times \$10,000) + (0.42\times 5\times 15/40\times \$10,000) = \$32,000$$

This benefit would be *upward adjusted* if it was lower than the minimum guarantee pension, which for 2015 was about $16,200 (at the regular level for married couples with one spouse claiming benefits and the other working or claiming DI).