DISCUSSION PAPER SERIES

IZA DP No. 9549

# Disability Benefit Generosity and Labor Force Withdrawal

Kathleen J. Mullen
Stefan Staubli

December 2015

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

# Disability Benefit Generosity and Labor Force Withdrawal

**Kathleen J. Mullen**
*RAND and IZA*

**Stefan Staubli**
*University of Calgary, RAND and IZA*

Discussion Paper No. 9549
December 2015

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 9549
December 2015

# ABSTRACT

## Disability Benefit Generosity and Labor Force Withdrawal*

A key component for estimating the optimal size and structure of disability insurance (DI) programs is the elasticity of DI claiming with respect to benefit generosity. Yet, in many countries, including the United States, all workers face identical benefit schedules, which are a function of one's labor market history, making it difficult to separate the effect of the benefit level from the effect of unobserved preferences for work on individuals' claiming decisions. To circumvent this problem, we exploit exogenous variation in DI benefits in Austria arising from several reforms to its DI and old age pension system in the 1990s and 2000s. We use comprehensive administrative social security records data on the universe of Austrian workers to compute benefit levels under six different regimes, allowing us to identify and precisely estimate the elasticity of DI claiming with respect to benefit generosity. We find that, over this time period, a one percent increase in potential DI benefits was associated with a 1.2 percent increase in DI claiming.

JEL Classification: H55, J14, J22

Keywords: disability insurance, benefit generosity, labor force withdrawal, claiming elasticity

Corresponding author:

Stefan Staubli
University of Calgary
Department of Economics
2500 University Dr. N.W.
Calgary, AB, T2N 1N4
Canada
E-mail: sstaubli@ucalgary.ca

* We thank Arie Kapteyn, Nicole Maestas, Philippe Ruh, Josef Zweimüller, participants of the 2015 NBER Summer Institute Aging Workshop and seminar participants at RAND and the University of Manitoba for helpful comments and suggestions, and Sarah Kups for excellent research assistance. This paper was supported by the National Institute on Aging through grant numbers R21AG037891 and R01AG046290. Its contents are solely the responsibility of the authors and do not represent the official views of the National Institute on Aging.

## 1. Introduction

Leaving the work force before the statutory retirement age has become the norm in many industrialized countries. Since the 1960s the labor force participation rate of males aged 55-64 in the OECD countries has decreased from 80 to 65 percent despite considerable improvements in aggregate health.[1] At the same time, disability insurance participation has grown substantially, especially among older workers who are not yet eligible for public pensions. The negative effects of this trend on economic growth and public expenditure are exacerbated by falling fertility rates and significant increases in average life expectancy. While retirement incentives arising explicitly from public old age pension systems (i.e., Social Security) have been well explored (see, e.g., Burtless 1986, Gustman and Steinmeier 1986, Stock and Wise 1990, Berkovec and Stern 1991, Blau 1994, Phelan and Rust 1997, Gruber and Wise 2004, French 2005), the impact of financial incentives arising from alternative retirement channels, most notably disability insurance (DI), still lacks a careful analysis. Although recent studies (e.g., Maestas, Mullen and Strand, 2013) have demonstrated convincingly that substantial work capacity exists among marginal DI program entrants, credible estimates of the effect of DI benefit levels on individuals' labor force participation and claiming decisions have remained elusive due to lack of exogenous variation in DI benefits, which in most countries are determined by a single function of past earnings and work experience.

In this paper we employ a novel identification strategy that exploits a series of reforms to the Austrian old age and disability insurance public pension system between 1987 and 2010. We make use of individual administrative data containing detailed information regarding earnings and employment histories and pension claiming, covering all of Austria in the period 1972 to

[1] The labor force participation rate of males aged 55-64 in OECD countries reached a minimum of 59 percent in the late 1990s and has increased since then to 65 percent.

2010. The combination of detailed administrative data and policy variation from a series of national pension reforms presents us with a unique opportunity to learn about individuals' labor force withdrawal decisions in ways that are impossible in the U.S. and other settings. Given the similarity in characteristics of the social security system and the aging population structure, and because many of the Austrian reforms mimic policy changes that are currently debated by policymakers around the world, studying their effects can yield important insights to guide social security and disability insurance reforms in other industrialized countries, including the U.S.

We find that generally Austrians in this period are sensitive to the level of DI benefits in determining their labor force participation. Over our sample period, 1.6 percent of labor force participants ages 35-59 withdraw from the labor force to claim DI benefits each year. We find that the DI inflow rate increases by 0.02 percentage points, or 1.2 percent, for every 1 percent increase in DI benefit levels over our entire sample period, 1987-2010. That is, we estimate an elasticity of DI claiming with respect to DI benefit generosity of 1.2. Because Austrian DI applicants are not required to quit their jobs or refrain from receiving unemployment insurance benefits during the disability determination period (unlike in some other systems, e.g., the U.S.), this is equivalent to estimating the elasticity of labor force withdrawal with respect to DI benefit generosity.[2]

We estimate a smaller elasticity of 0.7 in the later years of the sample, 2004-2010, which experienced the lowest replacement rates over our observation period. During the same time period, we estimate that DI *application rates* increased by 1.6 percent per 1 percent increase in DI benefits, more than twice the percent increase in new DI beneficiaries. This implies that the allowance rate for the marginal DI applicant induced by a 1 percent increase in DI benefits is less

[2] In the U.S., where withdrawing from the labor market to apply for DI benefits can adversely affect subsequent reemployment, the effect of DI benefits on labor force withdrawal is likely larger than the effect of benefits on DI claiming (see, e.g., Autor et al., 2015).

than half the overall DI allowance rate. We are unaware of any studies that examine the role of application screening in mediating the effect of DI benefit levels on new DI beneficiaries.[3]

There exist a wide range of estimates of the elasticities of DI application, award and labor force non-participation rates, respectively, with respect to DI benefit levels. Bound and Burkhauser (1999) reviewed the literature and found estimates in the range of 0.2-1.3 (applications), 0.3-0.4 (awards) and 0.2-1 (nonparticipation). Virtually all of these studies use observational data on older workers (ages 45 and older) in the U.S. in the 1960s and 1970s.[4] Of these, Kreider's (1999) study using data from the 1978 Survey of Disability and Work is generally considered the most credible (e.g., see Bound et al., 2004). Kreider estimates a structural model of SSDI applications, awards and lifetime income inflows, and simulates the effect of a 10 percent increase in DI benefit levels, finding an estimated 8.6 percent decline in DI applications, for an elasticity of DI applications with respect to benefit levels of just under 0.9.[5] It is not clear how many of these induced applications would be allowed, but applying our estimate of half the average allowance rate this translates to an elasticity of DI claiming with respect to benefit generosity of 0.43—about three times lower than our estimated elasticity.

An exception to the previous studies using observational data in the U.S. is a study by Gruber (2000) that exploits quasi-experimental variation in DI benefit levels arising from a 1987 Canadian reform in all provinces except Quebec.[6] Using a difference-in-differences framework, Gruber estimates that the 36 percent increase in DI benefit levels was associated with an 11.5

[3] Maestas, Mullen and Strand (2015) examine the effect of local unemployment rates on DI applications and initial allowances during the Great Recession; they find that virtually all of applications induced by the Great Recession were denied at the initial level, although it is not clear how the additional applications fared in the appeal process.
[4] Haveman and Wolfe (1984) attempt to instrument the DI replacement rate, recognizing it is endogenous, but they have difficulty finding credible exclusion restrictions.
[5] Intriguingly, Kreider finds that when time (i.e., the waiting period) is eliminated from the model, the estimated decline in applications is 1.3 times as large, or 11.1 percent.
[6] Campioleti (2004) examines the effect of a smaller ($50) increase in DI benefits in Quebec compared to other provinces in 1973. His estimates of the effect of the reform on nonemployment are very small but extremely imprecise, with standard errors on the order of five times larger than the estimated effects.

percent increase in the nonemployment rate among men ages 45-59 in the first two years after the reform. These estimates imply a *short run* elasticity of nonemployment with respect to DI benefit levels of around 0.3. To obtain the long run elasticity of nonemployment (nonparticipation) we need to scale this estimate by the inverse of the fraction of the stock of DI participants who are new entrants, approximately 20 percent in Austria. Thus, Gruber's estimate implies a long run elasticity of nonparticipation with respect to benefits of around 1.5, slightly higher than our estimate of 1.2.

Our large sample also allows us to examine heterogeneity in responsiveness to benefit generosity on several different margins. We find that the elasticity of DI claiming with respect to benefit generosity is highest for prime-age workers (ages 45-49) and increases with skill level and prior earnings, consistent with the idea that very young or very old, low skilled and poorer workers tend to have fewer labor market opportunities and thus less discretion in whether to apply for DI benefits vs. continuing to work. Notably, we find that individuals experiencing a current involuntary unemployment spell are much more responsive to DI benefit levels than employed individuals. This is consistent with Autor and Duggan's (2003) finding that "conditional applicants," or those applicants who apply for DI benefits only after losing their job, are more responsive to DI incentives than non-conditional applicants in the U.S.

The rest of the paper is structured as follows. In Section 2, we describe the data and institutional background on the Austrian DI system including a detailed description of the policy reforms enacted between 1987 and 2010. Section 3 describes our empirical strategy for isolating the effect of DI benefit level on labor force withdrawal, separating out the effect of unobserved preferences for work that are correlated with both DI benefits. Section 4 presents the results of our estimation models. Finally, Section 5 concludes.

## 2. Data and Institutional Background

### 2.1. The Austrian Social Security Database

We use administrative data from the Austrian Social Security Database (ASSD), provided by Hauptverband der Österreichischen Sozialversicherungsträger (the Austrian Social Security Administration). The ASSD covers the universe of workers in Austria and contains detailed information on the labor market and earnings histories of individuals beginning in 1972. Specifically, all employment, unemployment, disability, sick leave, and retirement spells are recorded.[7] Summaries of spells before 1972 (e.g., number of years employed, unemployed, etc.) are available for individuals who have claimed a public pension by the end of 2008. At the individual level, the ASSD contains information on gender, age, work experience, job tenure, and blue- or white-collar status.[8] The ASSD also contains some firm-specific information such as geographic region and industry affiliation. See Zweimüller et al. (2009) for a detailed description of the data. Although the ASSD does not contain information on DI applications (only claiming), we are able to merge to administrative application records starting in 2004.

A key feature of the data set is that it provides all of the information necessary to compute individuals' hypothetical disability insurance (DI) benefits at any given point in time and, with some minor assumptions, individuals' hypothetical DI benefits under *different* policy regimes, which is critical to our identification strategy (see Section 3). We verify the accuracy of our calculations by comparing our predicted DI benefit levels with actual DI benefit payments for the subsample of beneficiaries who received benefits in 2001 or who began receiving benefits after 2001, using matched data obtained from the Austrian Social Security Administration. Figure 1 plots mean matched DI benefits against mean predicted DI benefits (in 1,000-Euro

---

[7] Individuals do not have to be receiving unemployment insurance benefits to be registered unemployed. Individuals who have exhausted their UI benefits may still register as unemployed and receive General Assistance.
[8] Unfortunately, it is not possible to link individuals' and their spouses' earnings records.

bins), pooling all years together. Figure A1 in the appendix presents matched vs. predicted DI benefits by claiming year. In all years actual benefits track our predicted benefits very closely.

Our main sample includes all male private sectors workers ages 35-59 during the time period 1987-2010 who have not already claimed a disability pension. Although we can confidently say that we are able to calculate potential DI benefits for men, unfortunately this is not the case for women, since maternity leave spells are known to be observed with error in the ASSD data.[9] Therefore, we focus our analysis on men only. We chose 1987 as the beginning of our sample period in order to observe at least 180 months (15 years) of earnings for everyone in our sample. We chose age 59 as the upper age limit because the early retirement age (ERA) in Austria is age 60 for men and those who satisfy the eligibility requirements for early retirement are not eligible for a disability pension. We limit the sample to private sector employees, who are covered by the same pension system and hence face the same eligibility restrictions and financial incentives. That is, we exclude self-employed and civil service workers who are covered by a different pension system.[10] Our sample covers more than three quarters of all active labor market participants in Austria.

We construct the sample by defining a reference date, January 1, and obtaining individuals' information on factors that might affect DI application (i.e., previous earnings, and demographic characteristics such as employed or registered unemployed) as of that date for each year they are in the labor market. The main outcome of interest is whether they were observed to

[9] Women also have different early and full retirement ages (55 and 60, respectively, for most of the sample period), although these are shifting to become equal to those of men by 2033.
[10] One potential concern is that different policy regimes may induce individuals to switch from the private sector to self-employment or the public sector or vice versa. However, transitions between sectors are rare—generally less than 0.2-0.4 percent per year. Furthermore, we find no evidence of discontinuous changes in switching rates around any of the pension reforms, suggesting that such strategic responses are not very common.

receive disability insurance benefits within a year (i.e., by December 31).[11] For a subset of years (2004-2010) we are able to observe applications and rejections in addition to claiming.

Table 1 presents descriptive statistics, overall and grouped by observation period roughly corresponding to the different policy regimes (see Section 2.3 for more details). During the twenty-three year sample period between 1987 and 2010, we observe just over 1.5 million individuals, each for an average of 10.5 years between ages 35 and 59. At any given point in time, the average individual in our sample is 45 years old. Approximately 10 percent of private sector labor market participants are unemployed on any given January 1 and 54 percent are blue collar workers. Thirteen percent claimed sick leave at some point in the last two years and the average sick leave spell among claimants was 51 days (beyond the first 6-12 weeks, which are covered by the individuals' health insurance plan and not recorded in the ASSD). The average worker was employed 13 out of the last 15 years.

On average workers in our sample have accumulated 26 *insurance years*, which include both contribution years (i.e., periods of employment, including sick leave) and non-contributory periods of labor force participation (e.g., unemployment) over their working lives. The average number of insurance years accumulated declined over the sample period, from 27.6 in 1987 to 25.1 in 2010. The average annual earnings in the year prior to observation is 33,364 Euros (in 2010 Euros), or $38,035 (1 Euro=$1.14), and increasing in real terms over the sample period.[12] Since wages in Austria are generally monotonically increasing over time, last year's wage generally exceeds average previous earnings calculated over the best 180 months of one's career. We calculated individuals' hypothetical DI benefits if they were to claim disability in that year;

[11] Disability insurance spells are back-dated in the ASSD to the date the claim was filed, so an individual who applied for disability benefits late in the calendar year and was awarded benefits in the next calendar year is observed to "receive" disability benefits in the original calendar year.

[12] Following Austrian pension system rules, we calculate monthly earnings in all months in which we observe positive earnings and then take the average.

the average disability pension would be 17,845 Euros, or about 57 percent of the average wage over the best 15 years. The inflow rate into DI from employment is 0.9 percent on average throughout our sample period, starting at 1.35 percent in the mid-80s to mid-90s and declining by more than 75 percent to 0.33 percent by the late-2000s. The inflow rate into DI from unemployment is considerably larger, 4.7 percent on average over the sample period, and correlates positively with national unemployment levels.

To examine DI inflow rates among unemployed individuals, we constructed a second sample of all involuntary UI spells occurring between 1987 and 2010. We identify involuntary spells by limiting the sample to UI claims occurring within 28 days of the end date of the prior employment spell since voluntary quitters must wait at least 28 days to start drawing UI benefits. The last column of Table 1 presents descriptive statistics for the UI sample. Compared to the overall population, the average individual experiencing a UI spell in our sample period is much more likely to be a blue collar worker (80.5%), much more likely to have drawn from sick leave in the past two years (40.3%), and has on average two fewer years of work experience and three and half fewer insurance years. His prior year's earnings are about 30% lower than the average over the entire population and his replacement rate (defined as the ratio of DI benefit to the average wage over best 180 months) is about 5% lower. The DI inflow rate from unemployment spells is 5.25 percent.[13]

### 2.2. The Austrian Disability Insurance Program

Like the United States and other countries, the Austrian disability insurance (DI) program is part of a larger Old Age, Survivors and Disability Insurance system that is financed by a payroll tax on earned income and provides pension benefits to qualifying workers who have

[13] For the UI spell sample, we measure DI receipt from within one year of the UI spell end date.

accumulated enough labor market experience (insurance years). Generally, a disabled worker in Austria must have accumulated at least 5 insurance years within the last 10 years to be eligible for DI benefits; the insurance years requirement increases month for month after age 50 up to a maximum of 15 insurance years.[14] The insurance years requirement does not apply if the disability is job-related; for each occupation there exists an explicit list of qualifying impairments. To apply for benefits, individuals must submit an application to the local DI office. Employees at the DI office first check whether applicants meet the insurance years criteria. In a second stage, a team of disability examiners and physicians assesses the medical severity of the disability and the applicant's ability to work. Disability pensions are awarded to individuals whose earnings capacity, due to a physical or mental health impairment, has been reduced to less than half of the earnings capacity of a healthy person with comparable education in any "reasonable" occupation the individual could be expected to hold.[15] The standard is relaxed at age 57 by changing the comparison from a healthy worker performing *any type of work* in the economy to a healthy worker *in a similar occupation*.[16]

Unlike DI applicants in the U.S., DI applicants in Austria are not required to reduce their earnings below a given threshold (or separate from their employers) in order to apply for benefits.[17] Like many European countries, employed individuals may draw sickness benefits from their employers—up to 52 weeks (assuming they have worked at least 6 months in the

---

[14] Note the work history requirement in the U.S. is similar to that in Austria: 20 covered quarters (or 5 years) earned in the last 10 years, with fewer credits required before age 31 and more required after age 42 up to a maximum of 40 quarters (or 10 years).

[15] Practically, eligibility standards are less strict for white collar workers, whose set of reasonable occupations is more limited.

[16] Prior to 1997, the age at which disability screening is relaxed was 55 for both men and women (see Staubli, 2011). In 1997 (2000), the age was raised to 57 for men (women). Practically, access to disability insurance in the U.S. is also relaxed at older ages (50 and 55, depending on impairment severity) for workers with limited education and skill level (see Chen and van der Klaauw, 2008).

[17] However, it is the case that DI beneficiaries earning more than 380 Euros per month (approx. $484) would lose up to 50 percent of their benefits, depending on their earnings. (In the U.S. DI beneficiaries earning more than approx. $1,000 per month would lose their entire benefit.)

previous 12 months before onset) —while applying for DI (Social Security Administration, 2012). In addition, individuals who are or become disabled while unemployed can apply for DI benefits while continuing to receive unemployment insurance (UI) benefits. Applying for DI benefits while unemployed does not stop the clock on individuals' maximum UI benefit durations but it does suspend the requirement that individuals must actively search for work. The acceptance rate for initial DI applications in 2010 was just over 40 percent according to official statistics; about 60 percent of rejected applicants appeal, of whom 20 percent are ultimately awarded benefits, implying an ultimate award rate of 47 percent. In our sample, the ultimate allowance rate is slightly higher (55 percent) since we exclude younger workers below age 35. We are not aware of any changes to DI screening policy over our time period with the exception of a change in the age of relaxed screening discussed above.[18] Once receiving DI benefits, very few claimants (fewer than 4 percent) ever leave the DI rolls because of medical improvements and return to work.

The formula for computing DI benefits is the same one used to compute old age (Social Security) pensions. Generally it consists of a *pension coefficient* (PC), which varies by age and insurance years, multiplied by an *assessment basis* (AB), which is average indexed capped earnings over a given period of time (e.g., the last 120 months (10 years) in 1987 at the beginning of our observation period). Younger DI beneficiaries with limited work experience are eligible for a "special increment" to supplement their accumulated insurance years up to a certain amount to compensate for their limited ability to establish long work histories. For example, in 1987, individuals younger than age 50 who had accumulated less than 26.3 insurance years could

[18] Unfortunately we cannot directly test this since any observed changes to award rates over time are partially due to changes in the applicant pool induced by the reforms.

add one additional "insurance year" for each year less than age 50 up to a maximum of 26.3 total insurance years (i.e., up to a maximum pension coefficient of 50).

The rules governing eligibility and calculation of benefits have been publicly available at www.sozdok.at since 2001 and are also published (with examples) in a series of books (see Marek (1987-2001; 2002-2005; 2006-2010) for the years covering our sample period).

## 2.3. Policy Reforms

To identify the effect of benefit levels on DI pension claiming, we exploit exogenous variation in DI benefits between 1987 and 2010 stemming from a number of changes to the DI benefit formula in 1988, 1993, 1996, 2000 and 2004. Starting in 1988, Austria enacted a series of reforms designed to decrease pension levels and introduce bonuses (penalties) for delayed (early) claiming.[19] However, not all reforms were detrimental to all potential DI recipients; as a result, we observe both increases and decreases in DI pension levels during our sample period that are independent of changes in work histories and preferences over time. The reforms were immediately implemented (or in some cases phased in) without grandfathering any cohorts into the previous regime.[20]

Table 2 summarizes the rules used to compute DI pensions between 1987 and 2010. The first two columns describe changes to the assessment basis (AB) and pension coefficient (PC) formulas respectively and the third column describes other important changes such as the age of relaxed screening for DI benefits or changes to the old age (OA) pension program, which could affect DI participation for older individuals on the margin of claiming DI and waiting until

[19] While initially the reforms aimed to encourage delayed retirement/claiming of old age pensions, they also affected DI pensions since the programs were connected through the shared benefit formula. Later reforms broke apart this relationship and added penalties specific to claiming DI pensions.

[20] With the exception of the 1993 and 1996 reforms, which were implemented in July and September respectively, the new DI benefit rules went into effect on January 1. Current beneficiaries were not affected by the reforms.

eligible for (early) retirement benefits.[21] We are not aware of any changes in screening policy around the time of the reforms other than a change in policy affecting older workers ages 55-57 occurring in 1997 (discussed below). Figure 2 illustrates the PC formula as a function of insurance years (IY) for individuals aged 47, 55 and 59, respectively, under the different pension regimes, including any penalties for early claiming applied in later years (see Table 2).[22] Note the AB does not depend on age or IY but varies primarily by the number of months over which earnings are averaged.

Figures 3A and 3B illustrate the effect of the reforms on the average AB and PC, respectively, by age group and observation year for three different fixed cohorts of individuals (those observed in 1990, 2000 and 2010). For example, panel A tracks on the y-axis the AB (PC) calculated under different pension rules (x-axis) for a fixed cohort of individuals observed in 1990. The increase in AB across panels is due to real earnings growth over time. Finally, Figure 4 combines the effects of the reforms on the individual components of the pension formulas and shows the distribution of year-to-year changes in the overall hypothetical DI pension immediately following each reform for a fixed (1997) cohort. Below we discuss the main features of each reform.

Initially, in 1987, an individual's DI pension was based on an AB of the last 120 months (10 years) of earnings and a PC increasing by 1.9 percentage points for each insurance year up to 30, and by 1.5 percentage points thereafter, up to a maximum PC of 80. A special increment was added to IY for DI beneficiaries under age 50 with insufficient IY (see Section 2.2). The solid

[21] Disability benefits are subject to regular income taxes and over the course of our study period the income tax system was changed in 1989, 1994, 2000, 2005, and 2009. These reforms generally lowered the income tax liability by cutting tax rates and increasing deductions. We estimate a version of the model using after-tax benefits to account for these changes and find that it lowers the estimated elasticity (see Section 4).

[22] Since the changes to the PC under the 2000 and 2004 reforms were phased in, we illustrate the PC formulas in 2003 and 2010, at the end of each regime.

line in Figure 2A illustrates the effect of the special increment on PC for an individual at age 47. Individuals with fewer than 26.3 (=50/1.9) IY received three “bonus” IY in the calculation of their PC up to a maximum PC of 50.

The 1988 reform gradually increased the length of the AB by 12 months each year between 1988 and 1992, to the last 180 months (15 years) of earnings in 1992, while holding the pension coefficient formula fixed. As Figure 3A shows, because earnings are generally monotonically increasing in Austria, these changes had the effect of *decreasing* the average benefit level for all age groups and cohorts. As can be seen in Figure 4, the 1988-1992 reform to the AB had a detrimental effect on DI benefits for about 80 percent of potential claimants, a negligible effect for 10 percent of potential claimants, and a slightly positive effect for the remaining potential claimants.

The 1993 reform, in contrast, made DI benefits more generous by increasing the age before which the special increment applied, from 50 to 56, and at the same time increasing the maximum supplemented PC from 50 to 60.[23] For many relatively younger individuals the effective minimum PC set by the “special increment” rules was binding both before and after the 1993 change; as a result the reform increased DI pensions by 20 percent almost exactly for almost one quarter of the sample (see Figure 4). Figure 3B illustrates the effect of the reforms on the mean PC by age group and observation year. As can be seen in the figure, the 1993 reform leads to a large increase in the PC for all age groups, with the largest increase coming from the youngest individuals. The 1993 reform also affected the assessment basis by changing it from the *last* 180 months of earnings to the *best* 180 months of earnings, which could only increase benefit levels. As a result, no one was adversely affected by the 1993 reform (Figure 4).

[23] For example, a 47-year old could now receive nine “bonus” IY (from three) up to a maximum PC of 60 (from 50) (see Figure 2A). A 55-year old could now receive one bonus IY up to a maximum PC of 60, increasing PCs for all those with fewer than 32 (=(60-1.9*30)/1.5+30) IY (see Figure 2B).

The 1996 reform reduced the return to IY before 30 IY, from 1.9 percent to 1.83 percent, and increased the return to IY thereafter, from 1.5 percent to 1.675 percent. Combined with the introduction of a penalty for "early" claiming (before age 60) for those individuals with 33 to 40 IY (see Table 2), this had the effect of decreasing pensions for about 40 percent of potential claimants (see Figure 4).[24] It did not affect pensions for those at the maximum PC under the special increment rules (see Figures 2A and 2B) or for older individuals with very high insurance years (see Figures 2B and 2C). Notably, the 1996 reform also changed the age of relaxed screening for DI benefits from 55 to 57 for men, which led to large reductions in DI claimants among older men independent of the reform's effect on benefit levels (Staubli, 2011).

The 2000 reform broke apart the connection between the DI and OA pension formulas, introduced a plateau in the PC at 60 and further (gradually) reduced the return to IY before the plateau and increased the return to IY after the plateau. (See Table 2, and Figures 2B and 2C.) Similar to the 1996 reform, this had the effect of decreasing DI benefits for any potential claimants whose PC was not at the maximum PC under the special increment rules. The reform also increase the early retirement age from 60 to 61.5 for men (Staubli and Zweimüller, 2013).

Finally, the 2004 reform reversed some of the changes of the 2000 reform. The reform reunified the DI and old age pension formulas and linearized the (gradually decreasing) return to IY (in line with the OA pension formula since 2000). It phased in a new age of special increment from age 56 to age 60. However, at the same time it increased the penalty for early claiming and raised the early retirement age from 61.5 to 62 for men. In addition, the reform gradually increased the length of the AB from 180 months to 480 months by 2028, thereby decreasing the generosity of benefits. As a result, the reform resulted in a large scale curtailment of benefits

[24] The reform reduced pensions by exactly 3.683 (=(1.9-1.83)/1.9) percent for a small group of potential claimants with fewer than 30 insurance years who were not eligible for the special increment.

(especially for old age pension claimants) which was met with intense public criticism. Responding to the backlash, the Austrian government passed legislation in 2005 that reduced the maximum penalty for early retirement from 15 percent to 5-6.5 percent of the projected pension from the 2000 reform (with higher penalties phased in over time) and enlarged the group of people who were not affected by the raise in the early retirement age. Thus, the effect of the 2004/05 reform was to increase pension levels for some (mostly workers with very low IY) and decrease pension levels for almost everyone else (see Figure 4).

Figure 5 gives us a first look at how the reforms affected DI inflow rates (panel B) as DI replacement rates (panel A) rose and fell over the same time period. While we see some evidence that changes in DI inflows generally track changes in replacement rates the time series are generally noisy and seem to indicate that other factors may have largely influenced overall patterns in DI inflows over time. In the next section we discuss our strategy to isolate changes in DI inflows stemming from changes in benefit levels due to the policy reforms vs. changes in other factors that may have influenced DI receipt over time.

## 3. Empirical Strategy

To estimate the elasticity of labor force withdrawal with respect to benefit generosity, we are interested in estimating regressions of the form:

$$y_{it} = X_{it}\beta + \gamma b_t(Z_{it}) + \phi_t + \varepsilon_{it}\,, \qquad (1)$$

where $y_{it}$ is a labor supply outcome such as DI claiming for individual *i* observed at time *t* who has not already claimed DI benefits, $X_{it}$ is a vector of demographic and labor market characteristics (e.g., socioeconomic status, experience, earnings in the previous year), $b_t(Z_{it})$ are (lagged) log potential DI benefits which are a function (potentially varying with *t*) of a subset of labor market characteristics $Z_{it} \in X_{it}$ (e.g., age, insurance years, and assessment basis), $\phi_t$ are year fixed effects, and $\varepsilon_{it}$ are any unobserved factors affecting DI claiming such as tastes for work. The parameter of interest is $\gamma$, the effect of log DI benefits on DI claiming, which, when scaled by the DI inflow rate, directly gives the labor supply elasticity.

A problem may arise if the benefit formula *b* does not vary across individuals. In this case, if *b* is a linear function of $Z_{it}$ then, even if all of the components of $Z_{it}$ are orthogonal to unobserved tastes $\varepsilon_{it}$, we cannot separately identify the effect of the benefit level from the effects of the individual components of $Z_{it}$. For example, if benefits are a linear function of past wages, then benefits are perfectly collinear with past wages and we cannot independently vary them; thus we cannot identify $\gamma$.

If, on the other hand, *b* is a *non*-linear function of $Z_{it}$ and $E[\varepsilon_{it} \mid Z_{it}] = 0$ then we can independently vary *b* and $Z_{it}$ and identify both $\gamma$ and $\beta$. Even if $E[\varepsilon_{it} \mid Z_{it}] = kZ_{it}$ (where $Z_{it}$ may contain polynomials or other transformations of one or more of its elements) we can identify $\gamma$, if not $\beta$, in the case of a single benefit formula that applies to all individuals. In this case, $Z_{it}$ serves as a *control function* such that, conditional on $Z_{it}$, *b* is orthogonal to $\varepsilon_{it}$ and the estimated coefficient on *b* obtained by OLS regression of equation (1) is unbiased as long as the components of $Z_{it}$ are correctly specified. The problem with this identification strategy is that it relies heavily on functional form. For example, if benefits are a nonlinear function of past wages where the replacement rate falls as wage rises (as is the case in most countries), a problem could arise if preferences for work also vary with earnings history in a concave fashion that cannot be captured by a polynomial function or other transformation of earnings that could serve as a control function.[25]

A way around this impasse occurs if another policy regime (i.e., different benefit *formula*) were observed. Then observing individuals' behavior under the different policy regimes would enable one to break apart even a nonlinear relationship between benefits and unobserved factors. Intuitively, one can use the second policy regime to implement a "difference in differences" (DID) type estimation strategy, where identification is obtained by relating individuals' differential responses to the reform to their differential exposure in terms of change in benefits. To do this, we implement a control function approach that calculates hypothetical benefits under each policy regime for each individual and includes them as controls in the regression:

$$y_{it} = X_{it}\beta + \gamma b_t(Z_{it}) + \sum_{r=2}^{R} \kappa_r b_r(Z_{it}) + \phi_t + \upsilon_{it}\,, \tag{2}$$

where $b_r(Z_{it})$ represents hypothetical benefits under policy regime $r$=2,…,$R$, where $R$ is the total number of policy regimes observed. Conditioning on the hypothetical benefit variables ensures the *actual* benefit is uncorrelated with unobservable factors and an OLS regression of equation (2) gives an unbiased estimate of the effect of DI benefit levels on labor force withdrawal. This approach has been used by Nielsen et al. (2010) to study the response of college enrollment to changes in student aid arising from a Danish reform and by Fevang et al. (forthcoming) to study the effect of temporary disability insurance (TDI) benefits on the duration and outcome of TDI spells using policy variation in Norway.

---

[25] This approach is identified based on residual variation in benefit levels after netting out the effect of $Z_{it}$ on *b*. In our case regressing benefits on age, insurance years and assessment basis produces an $R^2$ of 0.903. If the $R^2$ is too high, and hence the residual variation too low, then this will inflate the standard error of the coefficient on log benefits in equation (1).

A simple example illustrates the intuition of our approach.[26] Let $Z_{it}$ be a scalar binary variable that takes on one of two values, $Z_{LOW}$ or $Z_{HIGH}$. Then benefits under policy regime *r* take on two values, $b_r^{LOW}$ and $b_r^{HIGH}$ corresponding to the respective values of $Z_{it}$. Assume that there are two policy regimes *r*=1,2. Then, suppressing conditioning on *X*,

$$E[y_{i1} \mid Z_d] = \gamma b_1^d + \kappa b_2^d \text{, and} \tag{3}$$

$$E[y_{i2} \mid Z_d] = \left(\gamma + \kappa\right) b_2^d + \phi , \tag{4}$$

where *d=LOW,HIGH*. Then the difference between pre- and post-reform outcomes for an individual with $Z_{it} = Z_d$ is:

$$E[y_{i1} \mid Z_d] - E[y_{i2} \mid Z_d] = \gamma\left(b_1^d - b_2^d\right) - \phi . \tag{5}$$

Subtracting this difference for *d=HIGH* vs. *d=LOW* and rearranging yields the following:

$$\gamma = \frac{\left[E[y_{i1} \mid Z_{HIGH}] - E[y_{i2} \mid Z_{HIGH}]\right] - \left[\left[E[y_{i1} \mid Z_{LOW}] - E[y_{i2} \mid Z_{LOW}]\right]\right]}{\left(b_1^{HIGH} - b_2^{HIGH}\right) - \left(b_1^{LOW} - b_2^{LOW}\right)} . \tag{6}$$

Thus, $\gamma$ is identified by the ratio of the average difference in differences of the outcome *y* to the difference in differences of the benefit *b*. In a regular DID framework in which a treatment group is compared to a control group, the difference in benefits for the control group (say those with $Z = Z_{LOW}$) would be zero and (6) gives the DID estimate scaled by the "first stage" estimate of the effect of the reform on benefit levels for the treatment group. In the example above, even though both groups are treated by the reform, the fact that they are treated *differentially* allows us to identify $\gamma$. In the case of the disability insurance reforms observed in this paper, every cluster of individuals with a unique set of characteristics $Z_{it}$ that affect disability benefits (i.e., age, insurance years and average prior earnings) serves as a comparison group, so our estimate is based on approximately 2,334 differential responses to potential benefits under six different

[26] This example was inspired by Nielsen et al. (2010).

policy regimes with 19 different benefit formulas (due to the phased-in nature of many of the reforms).[27]

To test the appropriateness of our identification strategy, we estimate a series of placebo regressions in which we randomly assign individuals' potential benefits from a different year, with and without hypothetical benefits in the control variables. If conditioning on hypothetical DI benefits indeed isolates the policy-induced variation in DI benefits, then we expect the coefficient on randomized benefits in this regression to be insignificant and close to zero when hypothetical benefits are included as controls but not necessarily otherwise. Table A1 presents the results of these placebo regressions. The first column presents estimates with base controls, which include age dummies, two-year insurance year group dummies and assessment basis. This is equivalent to estimating a placebo version of equation (1) with $X_{it} = Z_{it}$, so that all control variables are variables that affect the DI benefit level directly. The second column presents estimates with saturated controls, that is, including in addition to the base controls a number of variables capturing job tenure, prior sick leave, maximum UI benefit duration, industry, region (of the firm), blue collar status, Austrian national status and 4$^{th}$ order polynomials in last year's wage and average wage in the best 180 months ($Z_{it} \in X_{it}$). The last column presents estimates adding controls for hypothetical benefits. We find a positive and statistically significant effect of *random* DI benefits on DI claiming in the models without controls for hypothetical benefits; however, once we control for hypothetical benefits, we estimate a very small, precise and statistically insignificant effect of random DI benefits on DI claiming.

An implicit assumption of our preferred methodology is that individuals with the same $Z_{it}$, and hence the same potential benefits, at different points in time are comparable to one

[27]We cluster observations by the individual's age in years, number of insurance years (twenty-three two-year categories) and assessment basis (five quintiles).

another. That is, we assume someone with the same age, insurance years and average past earnings has the same unobservable factors affecting DI claiming (e.g., preferences for work) in 1987 as they do in 2010. There is reason to be skeptical of this assumption. For example, earlier we noted that real earnings increased and insurance years decreased over our 23-year time period (see Table 1). These changes could be driven by changes in another factor (e.g., education) that also affects unobservable tastes or they could reflect changes in unobservable tastes over time. Furthermore, as Figure 6 (Panel A) illustrates, the distribution of potential benefits calculated under any given year's rules (1997 in the figure) is observed to shift upward with each later cohort, so that an individual in the 1990s who is in the upper tail of the potential benefits distribution would find himself compared to an individual in the 2000s who is closer to the middle of the same base year's potential benefits distribution.

One way to indirectly test the plausibility of this assumption is to see whether there are changes in *observable* characteristics over time conditional on potential benefits in a given base year (1997). To do this we ran a series of regressions of various observable characteristics (years of tenure, number of sick days in the last two years, etc.) on indicators for quartile of 1997 benefits, year dummies and the interaction of benefit quartile and year dummies. Figure 7 plots the estimated coefficients on the interaction terms with 95 percent confidence intervals. As can be seen in the figure, the interaction terms are all closely centered around zero; that is, consistent with our identifying assumption, observable characteristics tend to evolve in parallel over time conditional on potential benefits.

To examine the sensitivity of our results to this assumption, we also perform two robustness checks. First, we reweight our observations so the proportion of the sample with a given age, level of insurance years and average earnings is constant over time and re-estimate the model on the reweighted sample. In a second robustness check, we control for percentile-ranked hypothetical benefits instead of actual hypothetical benefits (see Panel B of Figure 6). In this specification, we compare someone in the 1990s in the 90th percentile of the potential benefits distribution with all other individuals in the same *percentile* of the potential benefits distribution in any given year. The results of these robustness checks are reported in Section 4.1.

## 4. Results

We apply each approach discussed above in turn to estimate the effect of DI benefit generosity on DI claiming (i.e., separate regressions by year, and pooled regression without and with hypothetical benefits as controls). Since we analyze decisions over the course of a calendar year, yet two of the changes (1993 and 1996) were implemented at a point other than January 1, we measure benefits as the weighted average of benefits over the year in those cases. (For example, since the 1996 reform was implemented in September, we calculate benefits in 1996 as two-thirds of the 1995 benefit plus one-third of the 1996 benefit.) Similarly, we include a variable indicating eligibility for relaxed screening that weights eligibility over the year (e.g., an individual who turns 57 in September is eligible for relaxed screening one-third of the year). Finally, we assume a one-year lag to learn the new DI benefit level after a policy reform. We also estimate the model using current-year benefits as a robustness check. To account for cross-sectional correlation within groups and serial correlation across time, we cluster standard errors by cells according to an individual's age in years, number of insurance years (twenty-three two-year categories), and assessment basis (five quintiles) because these three variables determine the policy-induced variation in benefits.[28] The total number of clusters is 2,334.

[28] We experimented with different levels of clustering including clustering on individual, Z*year and year alone. We find that statistical inference is not sensitive to the level of clustering.

### 4.1. The Effect of Disability Insurance Benefit Generosity on Claiming

In this section we present estimates of the effect of disability insurance (DI) benefit levels on DI pension receipt in Austria. First, we estimate separate regressions of equation (1) for each year 1987-2010. Next, we pool individuals from all years to estimate the same regression with and without hypothetical benefits calculated under each of the policy regimes as a control function. We estimate several robustness checks to examine the sensitivity of our estimates to different samples and specifications. In the next section, we examine heterogeneity in the estimated labor supply elasticities for different observable characteristics such as age, blue vs. white collar status, lifetime earnings and employment status. Finally, we present estimates of the effect of DI benefit levels on applications and rejections for 2004-2010.

Table 3 presents cross sectional estimates separately for each year between 1987 and 2010. The first set of estimates presents models with base controls ( $X_{it} = Z_{it}$ ) and the second set of estimates presents models with saturated controls ( $Z_{it} \in X_{it}$ ), defined above. As Table 3 shows, the estimated effects of benefit levels on DI claiming in Austria are generally between 0.01 and 0.03, implying that a one percent increase in benefits is associated with a 0.01-0.03 percentage point increase in DI inflow. The estimates are not very sensitive to whether the base or saturated controls are included; sometimes the estimates are smaller with saturated controls, sometimes larger. Since DI inflow rates are on average 0.016 over this time period, the estimates imply a labor supply elasticity with respect to DI benefit generosity in the range of 0.6 to 2.

Table 4 presents estimates of the DI benefit elasticity pooling individuals over the entire sample period. Columns 1 and 2 present pooled versions of the models presented in Table 3, respectively, and column 3 presents estimates adding hypothetical benefits calculated under each of the policy regimes in order to control flexibly for nonlinear variation in unobservable tastes for work that may also vary across cohorts. As expected, the estimates without the hypothetical benefits center around 0.02, roughly the center of the year-by-year estimates. Including hypothetical benefits to the pooled model does not change the estimate substantially. In this case we fail to reject the hypothesis that the coefficients on the hypothetical benefits are jointly zero (p=0.141). Scaling the estimated coefficient on log benefits by the DI inflow rate, we estimate an elasticity of DI claiming with respect to benefit generosity of 1.2. (See Table 4 for the implied elasticity and standard error, estimated via delta method.) That is, we estimate for every one percent increase in DI benefit levels, an additional 1.2 percent of labor force participants will withdraw from the labor market and become a DI beneficiary.

Table 5 presents several robustness checks to examine the sensitivity of our estimates to different subsamples and specifications. First, we add into our sample those individuals ages 60-64 (below the full retirement age) who are not eligible for early retirement because they lack sufficient insurance years. Including these individuals does not have a significant impact on the estimated elasticity. Even if we exclude those who may qualify for old age retirement benefits, it may be the case that forward-looking individuals are influenced by changes in the old age pension system coinciding with the DI reforms. Therefore we estimate a version of the model where we include one's potential old age pension in the controls. As can be seen in Table 5, this also has a negligible impact on the estimated elasticity.

As discussed in Section 2, the insurance years restriction on DI benefits does not apply if the disability is job-related. In our baseline specification, we included all labor market participants under age 60, but we also estimate a version of the model where we exclude those with insufficient insurance years to qualify for DI benefits for non-job-related impairments. We estimate a larger labor supply elasticity with respect to DI benefits close to 2 for this restricted group, which is perhaps not surprising given that individuals who qualify based on job-related disabilities by construction have a job and are therefore more likely to be able to retain their attachment to the labor force and use discretion when applying for DI benefits.

Also as discussed in Section 2, DI benefits are taxed and there were several changes to the tax system during our time period, some of which coincide with the DI reforms. We estimated a version of the model using after-tax benefits and found a slightly smaller elasticity of one—not surprising if potential DI claimants have a harder time calculating after-tax than pre-tax benefit levels. We also estimated a version of the model on current log benefits instead of lagged benefits and again found a smaller response to changes in current benefits, which likely reflects a lag in learning the details of the reforms. Note in these last two cases including the control function of hypothetical benefits to isolate the policy-induced (and therefore exogenous) variation in benefits is vital to estimating an unbiased elasticity of claiming with respect to benefits; the hypothetical benefit control variables are jointly statistically significant ($p<0.05$).

We explore the sensitivity of our estimates to model specification by estimating a discrete time Cox proportional hazard model using logistic regression. An advantage of the proportional hazard specification is that the coefficient on log benefits gives the elasticity directly. When we estimate the model using a proportional hazard specification, we estimate an elasticity of 1.4, slightly higher than our baseline estimate of 1.2 but not statistically different.

Finally, as discussed in Section 3, we explore the sensitivity of our estimates to the identifying assumption that individuals with the same age, insurance years and average past earnings are comparable to one another over time. First, we reweight the sample to hold the distribution of age, insurance years and earnings fixed at their 1997 levels throughout the 1987-2010 time period. Reweighting the sample has little impact on the estimated elasticity (1.20 vs.

1.23). Next, we control for *percentile-ranked* hypothetical benefits in the control function. In this specification we are leveraging changes in DI claiming behavior across individuals in the same quantile of the benefits distribution over time. The estimated elasticity is slightly higher, 1.35; note that the percentile-ranked hypothetical benefits are jointly highly statistically significant ($p<0.001$).

### 4.2. Heterogeneity in the Effect of DI Benefit Generosity on Claiming

To examine heterogeneity in labor supply responses, Table 6 presents estimates of our preferred specification (with hypothetical DI benefits as controls) separately by different groups. Some interesting patterns emerge. Most notably, despite the fact that DI inflow rates increase sharply with age, especially at ages 50 and 55, the elasticity of DI claiming with respect to benefit generosity is actually highest for prime-age workers (ages 45-49). (The estimated effects for younger workers are small and imprecise.) This contradicts conventional wisdom that older individuals tend to use the DI system as an alternative pathway to early retirement whereas prime-age individuals are unlikely to exit the labor force unless they are facing very serious health conditions. We also find that *white* collar workers are more sensitive to changes in DI benefit levels than blue collar workers and *richer* individuals (in terms of lifetime earnings) are more sensitive than poorer individuals; in both cases the former group is likely to have more labor market opportunities than the latter group. Moreover, eligibility criteria for white collar workers in Austria are generally less strict than that for blue collar workers (see Section 2.2).

Economic theory suggests the elasticity of DI claiming with respect to DI benefit level should be increasing in the benefit level. We investigate this hypothesis next by dividing individuals into quartiles based on (1997) potential benefits and regressing DI inflow on DI benefits within each quartile. We find that, indeed, the estimated elasticity is increasing in the benefit level, more than five-fold between the bottom and top quartiles of DI benefits, from 0.35 to 2.0.[29]

Finally, the estimated elasticity for DI claiming is three times higher for individuals currently experiencing an involuntary unemployment spell compared with the overall labor force (largely made up of employed workers). We find that a one percent increase in DI benefits leads to a nearly 0.2 percentage point increase in DI claiming among this group. Since 5.25 percent of UI spells transition to DI, this amounts to a nearly 3.8 percent increase in DI inflow rates from UI. Thus, laid off workers are much more influenced by financial incentives than employed workers when making decisions about withdrawing from remaining in the labor force, although this may be specific to Austria's institutional environment which allows unemployed workers to simultaneously search for work and seek disability benefits.

### 4.3. The Effect of DI Benefit Generosity on Applications and Rejections

Finally, we investigate the role of the application screening process by examining the effect of DI benefits on *applications* and initial rejections, compared with DI inflow, for the years in which we are able to match application records, 2004-2010. Although the elasticity of DI claiming with respect to benefits is important for fiscal solvency, the elasticity of DI applications with respect to benefits is presumably a better measure of the level of moral hazard occurring in the DI system. The ratio of the two elasticities is also informative about the effectiveness of the application screening process, since an effective screening process should be more likely to reject applicants induced by an increase in DI benefits, who are on the margin of applying for DI and continuing to remain in the labor force.

[29] The average DI benefits in each quartile are: 1) 10,217; 2) 15,502; 3) 19,718; and 4) 25,944.

Table 7 presents the results of these analyses. The first two columns present estimates of the effect of log lagged DI benefits on DI applications and initial rejections, respectively, and the final column presents estimates of the effect of log lagged DI benefit on DI inflow restricted to the years 2004-2010 using our preferred specification controlling for potential DI benefits in all years. Note that we estimate a slightly lower claiming elasticity, 0.7, over this time period compared with the entire 1987-2010 sample period.[30] This is perhaps not surprising since the 2004-2010 period experienced the lowest replacement rates observed over our sample period. We also observe a slightly lower percentage of potential claimants, 1.2 percent, receiving DI benefits in any given year in the 2004-2010 period compared with the overall sample period.

In 2004-2010 approximately 2.2 percent of potential claimants applied for benefits in any given year; 62 percent (=1.4/2.2) of these applications were rejected initially, although 55 percent (=1.2/2.2) of applicants eventually received benefits. We find that DI benefit levels have a strong influence on the probability of filing a DI application. A one percent increase in DI benefits increases the application rate by 0.034 percentage points, or 1.6 percent, and the initial rejection rate by 0.021 percentage points (also 1.6 percent). Interestingly, this implies the rejection rate for the *marginal* applicant, induced to apply for DI benefits by a 1 percent increase in the benefit level, is 62 percent (=0.021/0.034), the same as the rejection rate for the *average* applicant in this time period. However, the appeals process appears to dampen considerably the flow of new DI beneficiaries. We find that the ultimate award rate for the marginal induced DI applicant is 25 percent (=0.0085/0.034)—approximately half the ultimate award rate for the average applicant in this time period.

[30] This estimate is much lower than the average implied elasticity from Table 3 because it includes potential benefits as controls, which are highly statistically significant (p<0.001) in the 2004-2010 period.

## 5. Discussion and Conclusion

While disability insurance (DI) programs are generally intended to insure against the risk of acquiring a career-ending health impairment, often individuals who would qualify for DI choose to work, at least for a time, instead. Understanding what drives these decisions is important for designing effective reforms, which are necessary to guarantee the financial stability of public pension systems. If individuals are relatively elastic in their labor supply responses to DI benefit levels, then cutting DI benefits will induce many potential DI entrants to remain in the labor force and significantly decrease program participation. On the other hand, if individuals are relatively inelastic, then cutting DI benefits is only likely to harm disabled workers who would not otherwise choose to work or try to find work after the onset of a disabling health condition.

Using detailed administrative data and policy variation in DI benefits stemming from a series of national pension reforms in Austria between 1987 and 2010, we find convincing evidence that the labor supply of Austrian workers is responsive to DI benefit levels. We find that the DI claiming rate increased by 1.2 percent per 1 percent increase in DI benefits over the entire sample period, and by 0.7 percent per 1 percent increase in DI benefits between 2004 and 2010, when average replacement rates were at their lowest. Our findings are fairly robust to sample selection and model specification. The elasticity of DI claiming with respect to benefit generosity is highest for prime-age workers (ages 45-49) and increases with skill level and prior earnings, consistent with the idea that very young or very old, low skilled and poorer workers tend to have fewer labor market opportunities and thus less discretion in whether to apply for DI benefits vs. continuing to work. We also find that (involuntarily) unemployed individuals are extremely responsive to benefit levels.

Note that the application screening process—particularly at the appeals level—dampens but does not eliminate the responsiveness of DI claiming to benefit levels. We find the elasticity of DI applications with respect to benefit generosity is more than twice as high as the elasticity of DI claiming in 2004-2010. This implies the marginal applicant induced to apply for DI benefits due to a 1 percent increase in benefit levels is half as likely to be allowed as the average applicant. Thus, it is important to account for a lower allowance rate among marginal applicants when linking estimates of the elasticity of DI applications to changes in the number of DI beneficiaries.

One caveat in interpreting our estimates as applicable to other settings is that under Austria's rules DI applicants do not have to withdraw from the labor market in order to *apply* for benefits but only do so if they are *awarded* benefits. It is not clear how this difference would affect the magnitude of the elasticity, since on the one hand the lower cost of application may encourage more individuals to try their luck but on the other hand rejected applicants do not face significant barriers to work since they do not have to re-enter the labor market after a potentially lengthy absence. Additionally, Austria's universal health insurance system and strong public safety net may also encourage applications to a greater extent than in other countries such as the U.S. (although recent changes in the availability of health insurance outside the employment relationship and DI participation in the U.S. as a result of the Affordable Care Act may make the two countries more comparable in recent years).

## References


Autor, David, and Mark Duggan (2003). "The Rise in the Disability Rolls and the Decline in Unemployment." *Quarterly Journal of Economics*, 118(1), pp. 157-206.

Autor, David, Nicole Maestas, Kathleen J. Mullen and Alexander Strand (2015). "Does Delay Cause Decay? The Effect of Administrative Decision Time on the Labor Force Participation and Earnings of Disability Applicants." NBER Working Paper 20840.

Berkovec, James and Steven Stern (1991): Job Exit Behavior of Older Men, *Econometrica*, Vol. 59, No. 1, pp. 189-210.

Bound, J. and Burkhauser R. (1999). "Economic analysis of transfer programs targeted on people with disabilities." *Handbook of Labor Economics*, vol. 3, ch. 51.

Bound, J., J. Cullen, A. Nichols and L. Schmidt (2004). "The welfare implications of increasing disability insurance generosity." *Journal of Public Economics*, Vol. 88, pp. 2487-2514.

Blau, David M. (1994): Labor Force Dynamics of Older Men, *Econometrica*, Vol. 62, No. 1, pp. 117-156.

Burtless, Gary (1986): Social Security, Unanticipated Benefit Increases, and the Timing of Retirement, *The Review of Economic Studies*, Vol. 53, No. 5, pp. 781-805.

Campioleti, M. (2004) "Disability Insurance Benefits and Labor Supply: Some Additional Evidence." *Journal of Labor Economics*, Vol. 22, No. 4 (October 2004), pp. 863-889

Chen, Susan and Wilbert van der Klaauw (2007): The Work Disincentive Effects of the Disability Insurance Program in the 1990s, *Journal of Econometrics*, Vol. 142, No. 2, pp. 757-784.

Fevang, E., I. Hardoy and K. Roed (forthcoming). "Temporary Disability and Economic Incentives" *Economic Journal*, forthcoming.

French, Eric (2005): The Effects of Health, Wealth, and Wages on Labour Supply and Retirement Behaviour, *Review of Economic Studies*, Vol. 72, pp. 395-427.

Gruber, Jonathan. 2000. "Disability insurance benefits and labor supply." *Journal of Political Economy* 108, no. 6:1162–83.

Gruber, Jonathan and David A. Wise (2004): *Social Security Programs and Retirement Around the World: Micro Estimation*, University of Chicago Press.

Gustman, Alan L. and Thomas L. Steinmeier (1986): A Structural Model of Retirement, *Econometrica*, Vol. 54, No. 3, pp. 555-584.

Kreider, B. (1999). "Social Security Disability Insurance: Applications, awards and lifetime income flows." *Journal of Labor Economics*, Vol. 17, No. 4, pp. 784-827.

Marek, E. (1987-2001). *Pensionsberechnung Leicht Gemacht*. Weiss Verlag.

Marek, E. (2002-2005). *ASVG-Pensionen ab dem Jahr*. Weiss Verlag.

Marek, E. (2006-2010). *Die Pensionen ab dem Jahr*. Weiss Verlag.

Maestas, Nicole, Kathleen J. Mullen and Alexander Strand (2013). “Does Disability Insurance Receipt Discourage Work? Using Examiner Assignment to Estimate Causal Effects of SSDI Receipt.” *American Economic Review*, 103(5), pp. 1797-1829.

Maestas, Nicole, Kathleen J. Mullen and Alexander Strand (2015). “Disability Insurance and the Great Recession.” *American Economic Review Papers & Proceedings*, 105(5), pp. 177-182.

Nielsen, Helena Skyt, Torben Sorensen and Christopher Taber (2010). “Estimating the Effect of Student Aid on College Enrollment: Evidence from a Government Grant Policy Reform.” *American Economic Journal: Economic Policy*, Vol. 2, pp. 185-215.

OECD (2005). *Ageing and Employment Policies: Austria*. ISBN-92-64-01008-4

Phelan, Christopher and John Rust (1997): How Social Security and Medicare Affect Retirement Behavior In a World of Incomplete Markets, *Econometrica*, Vol. 65, No. 4, pp. 781-831.

Social Security Administration (2012). *Social Security Programs Throughout the World: Europe, 2012*. Downloaded on 11/25/13 from http://www.ssa.gov/policy/docs/progdesc/ssptw/2012-2013/europe/index.html.

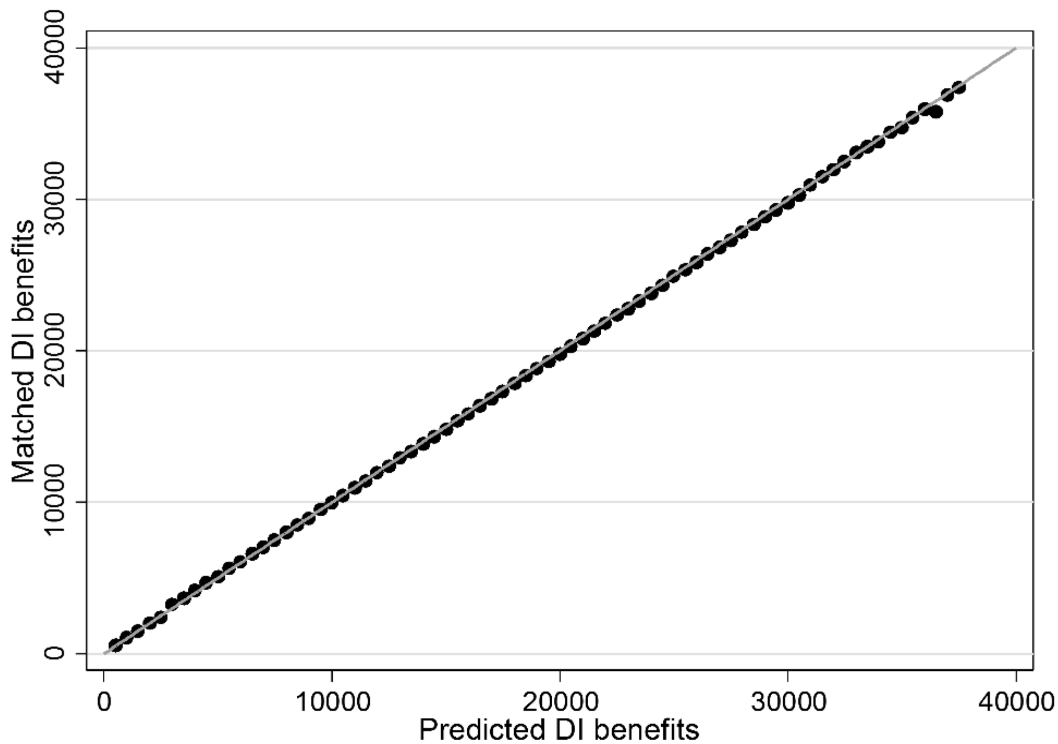
Staubli, Stefan (2011). “The Impact of Stricter Criteria for Disability Insurance on Labor Force Participation.” *Journal of Public Economics*, 95 (9-10), 1223–1235, 2011.

Staubli, Stefan and Josef Zweimüller (2013). “Does Raising the Early Retirement Age Increase Employment of Older Workers?” *Journal of Public Economics*, 108, 17–32, 2013.

Stock, John and David Wise (1990): Pensions, the Option Value of Work, and Retirement, *Econometrica*, Vol. 58, No. 5, pp. 1151-1180.

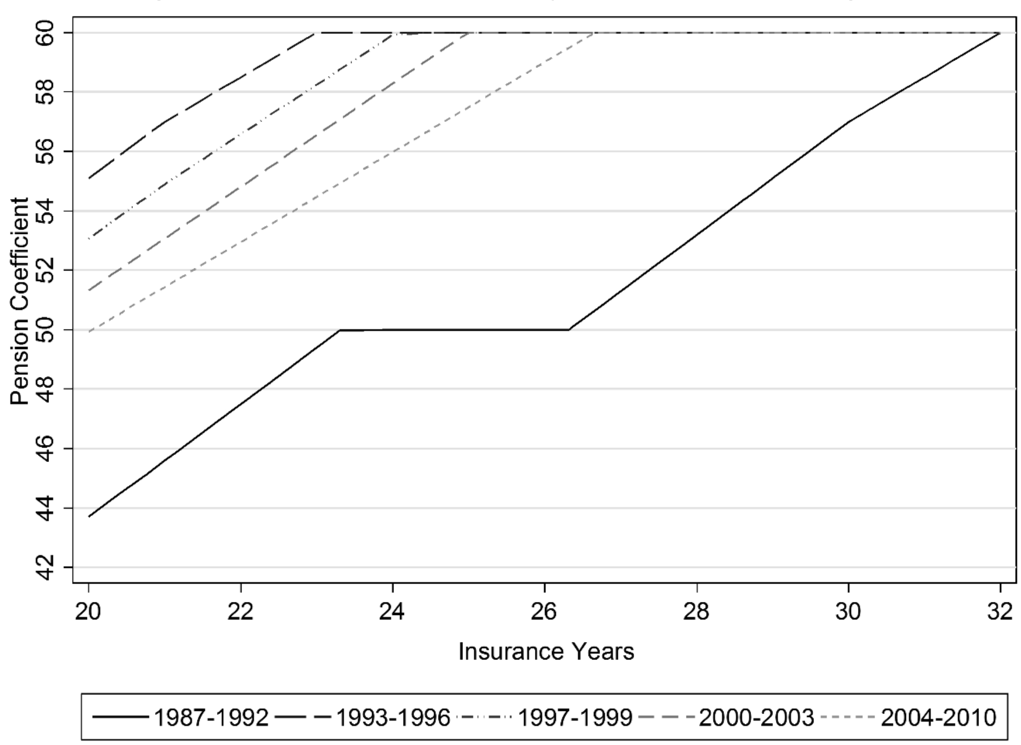
Zweimüller, Josef, Rudolf Winter-Ebmer, Rafael Lalive, Andreas Kuhn, Jean-Phillippe Wuellrich, Oliver Ruf and Simon Büchi (2009). “Austrian Social Security Database.” Manuscript available at http://ssrn.com/abstract=1399350.

Figure 1: Imputed and Matched DI benefits, All Years Pooled



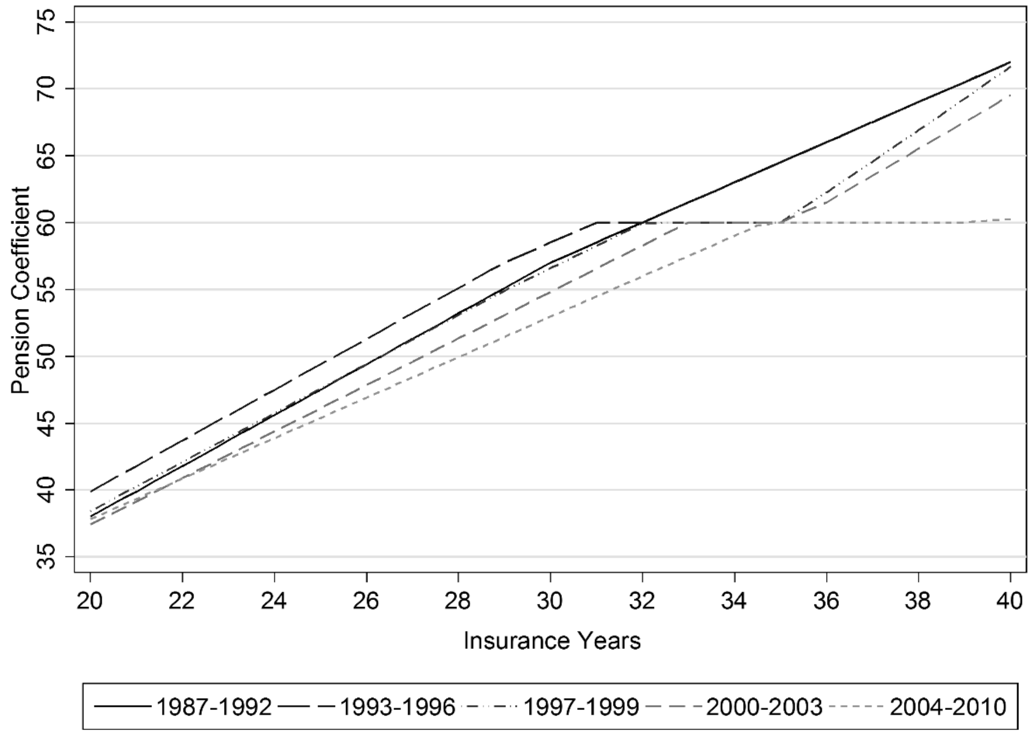

Source: Own calculations, based on Austrian Social Security and Pension Claims Data.

Figure 2A: Pension Coefficient by Insurance Years at Age 47



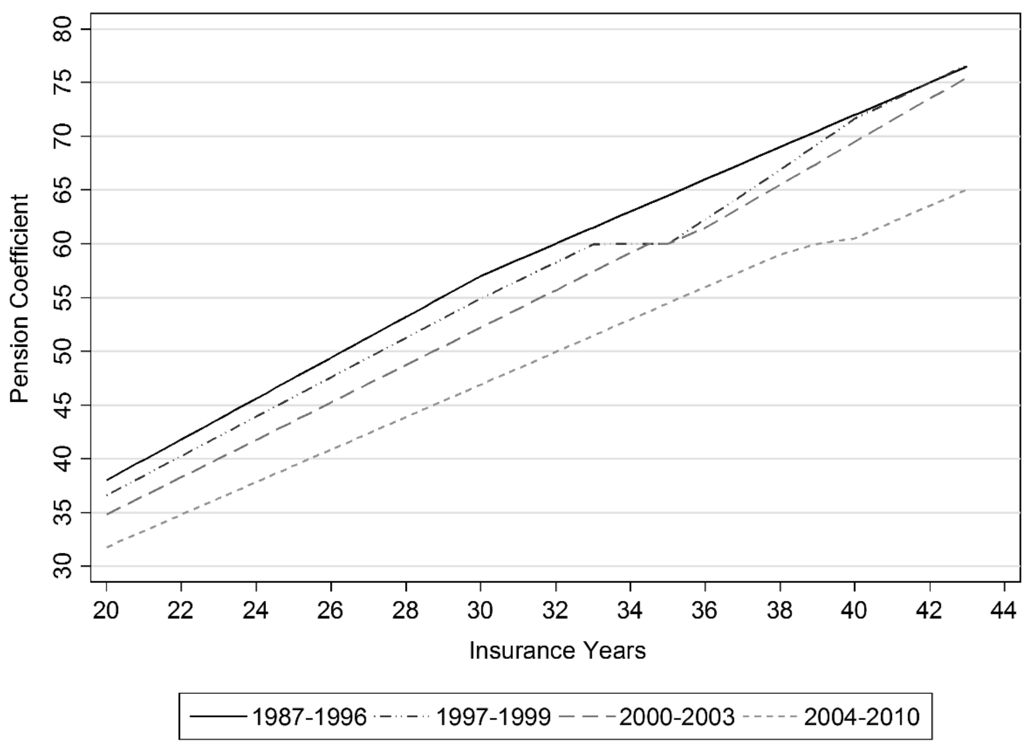

Source: Own calculations.

Figure 2B: Pension Coefficient by Insurance Years at Age 55



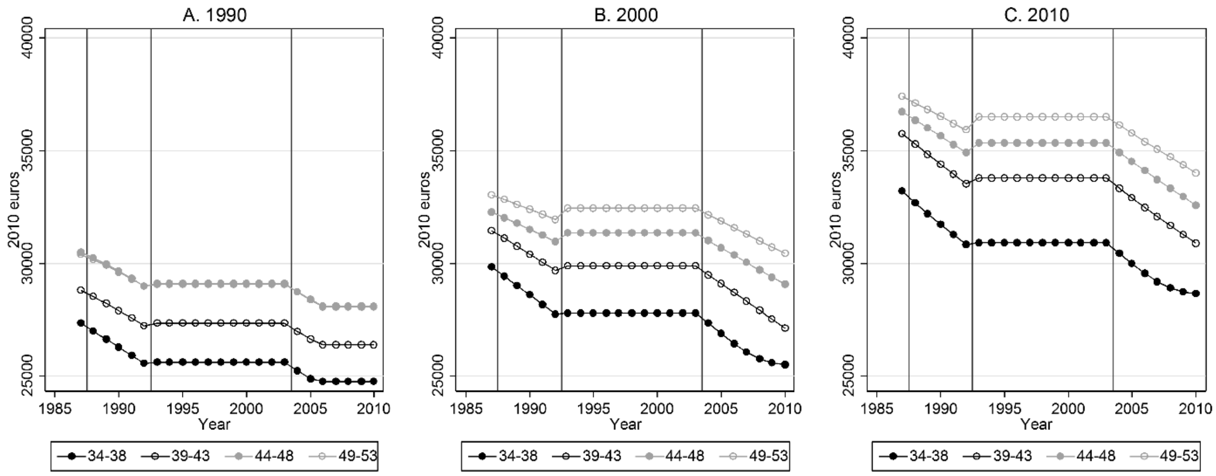

Source: Own calculations.

Figure 2C: Pension Coefficient by Insurance Years at Age 59



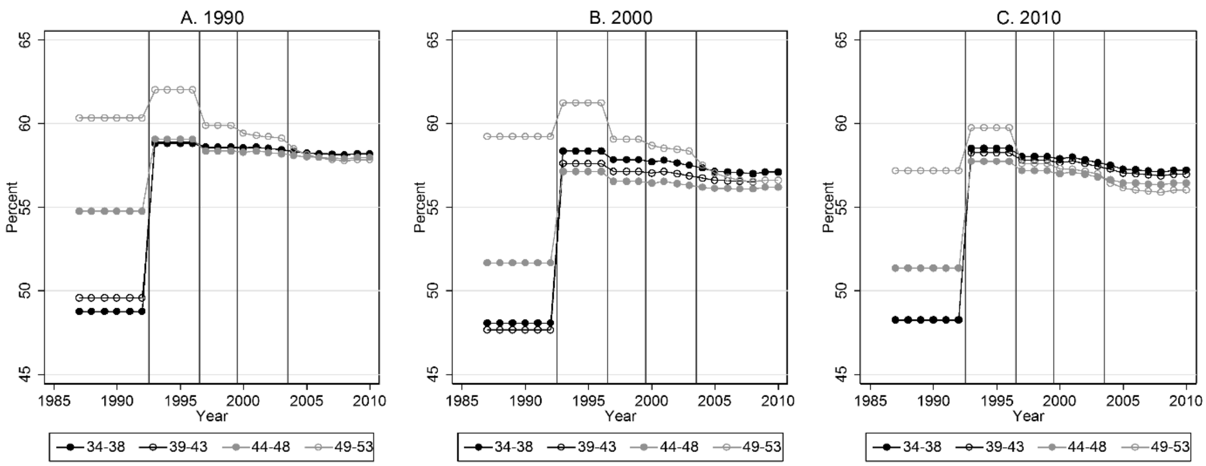

Source: Own calculations.

Figure 3A: Assessment Basis over Time by Age Group and Observation Year



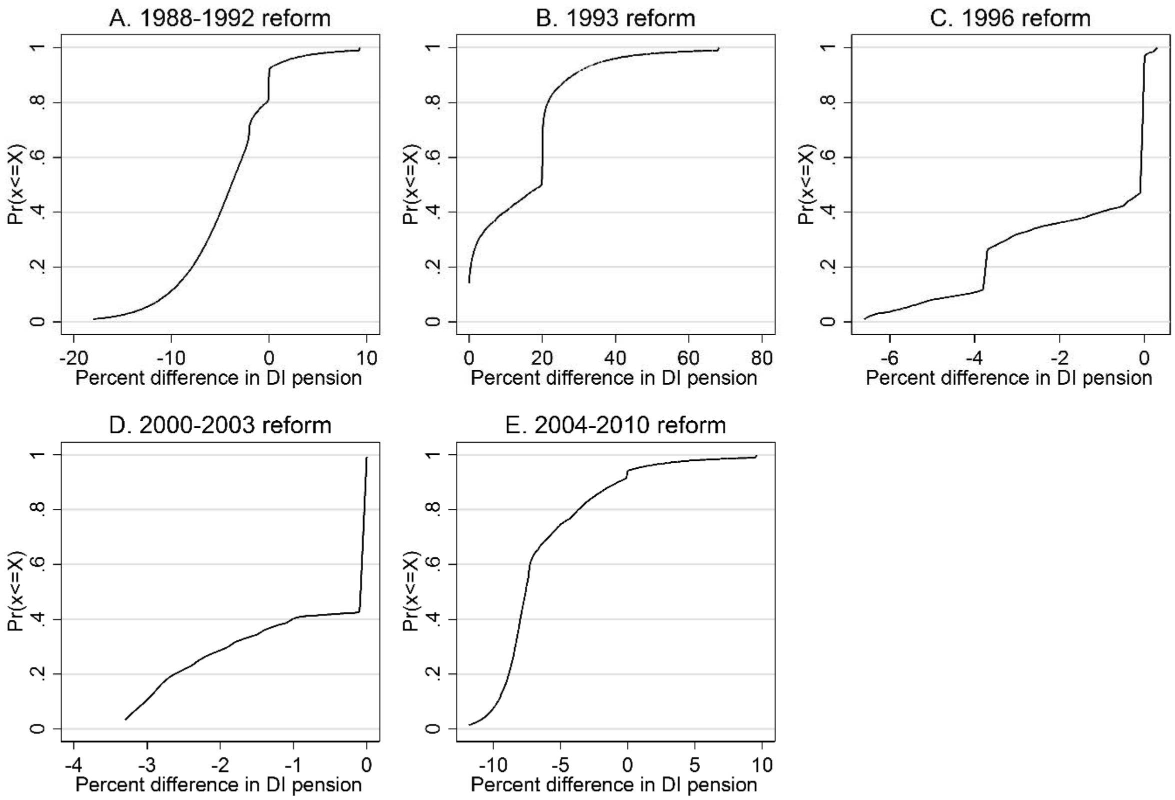

Source: Own calculations, based on Austrian Social Security Data.

Figure 3B: Pension Coefficient over Time by Age Group and Observation Year



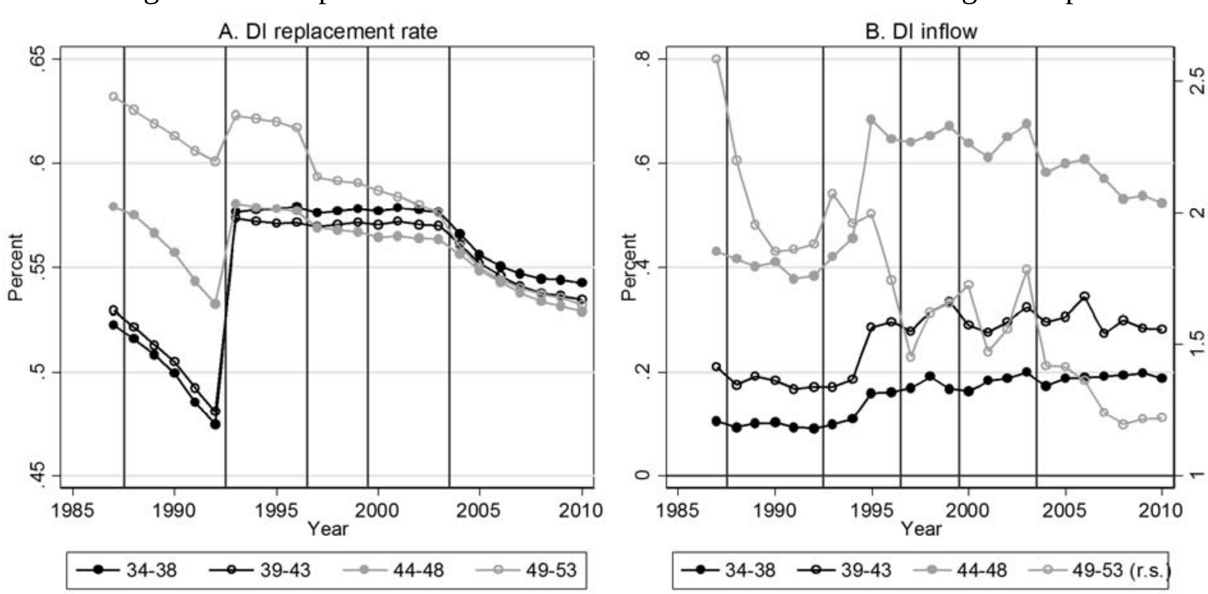

Source: Own calculations, based on Austrian Social Security Data.

Figure 4: Cumulative Distribution Functions of % Change in DI Pension



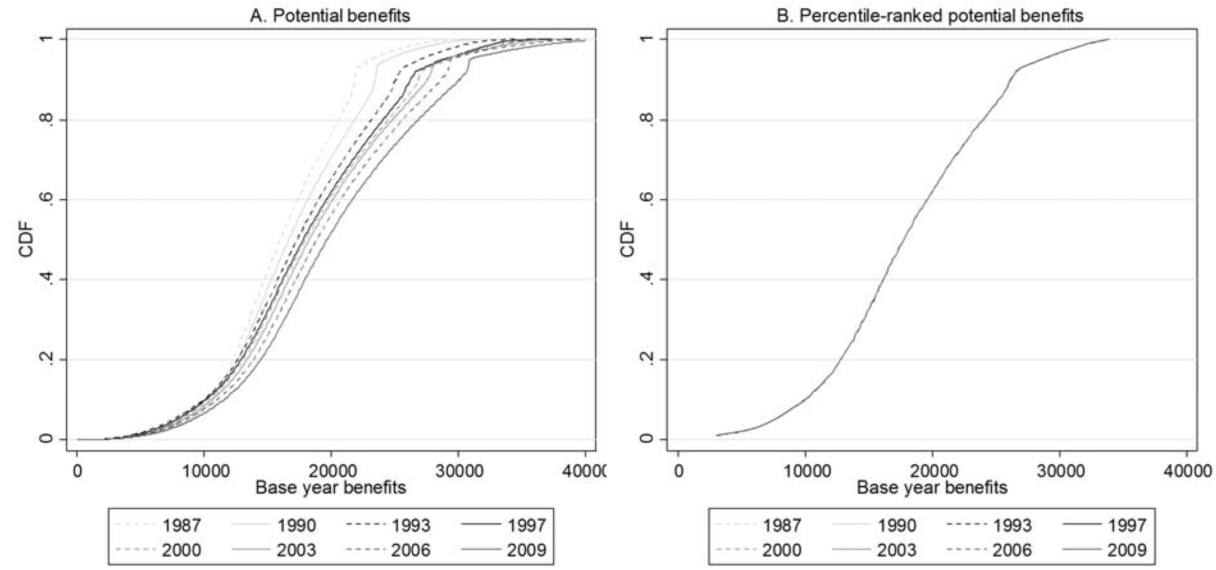

Note: Calculations based on 1997 cohort.

Figure 5: DI Replacement Rate and DI Inflow Rate in Different Age Groups



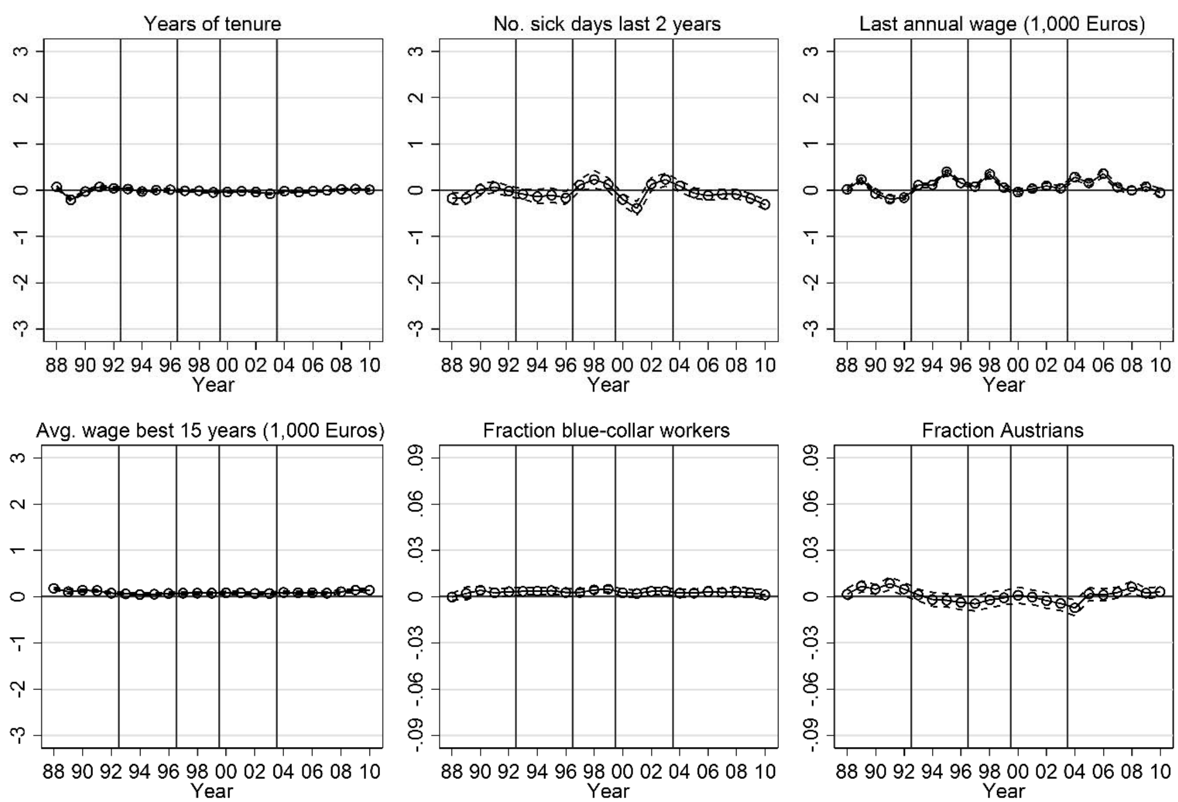

Source: Own calculations, based on Austrian Social Security Data.

Figure 6: Percentile Ranking of Potential Benefits




Note: Percentile-ranking based on 1997 potential DI benefits.

Figure 7: Change in Covariates over Time




Note: See section 3 for details.

Table 1. Summary Statistics, Men Ages 35-59

| | All | 1987-1992 | 1993-1996 | 1997-1999 | 2000-2003 | 2004-2010 | UI Spells |
|---|---|---|---|---|---|---|---|
| Age | 45.3 | 45.8 | 45.5 | 45.3 | 44.9 | 45.1 | 45 |
| % unemployed | 10.3 | 9.5 | 11.7 | 12.0 | 10.3 | 9.5 | n/a |
| % blue collar | 53.9 | 56.1 | 54.5 | 53.6 | 53.6 | 52.4 | 80.5 |
| % positive sick leave days last 2 years | 13.3 | 12.7 | 14.3 | 13.9 | 12.7 | 13.2 | 40.3 |
| Sick leave days past 2 years (if > 0) | 51.3 | 47.3 | 53.1 | 56.7 | 62.2 | 45.2 | 57.6 |
| Experience last 15 years | 13.1 | 13.5 | 13.0 | 12.9 | 12.9 | 13.1 | 10.9 |
| Insurance years | 26.0 | 27.6 | 26.5 | 26.1 | 25.3 | 25.1 | 22.6 |
| Last wage | 33,364 | 30,310 | 31,516 | 32,505 | 33,741 | 36,398 | 23,322 |
| Avg. wage best 15 years | 30,799 | 27,769 | 29,370 | 30,370 | 31,277 | 33,398 | 25,919 |
| Disability benefits | 17,845 | 16,015 | 17,866 | 18,235 | 18,504 | 18,547 | 14,202 |
| Replacement rate | 0.57 | 0.57 | 0.60 | 0.59 | 0.58 | 0.55 | 0.54 |
| DI infow from employment (in %) | 0.90 | 1.35 | 1.35 | 1.06 | 0.84 | 0.33 | n/a |
| DI inflow from unemployment (in %) | 4.68 | 3.69 | 5.21 | 4.82 | 5.26 | 4.63 | 5.25 |
| No. of observations | 16,199,158 | 3,507,613 | 2,550,646 | 2,026,008 | 2,806,316 | 5,308,575 | 1,226,228 |
| No. of individuals | 1,548,093 | 762,470 | 758,828 | 764,959 | 845,650 | 990,452 | 589,855 |

Note: All monetary amounts are in 2010 Euros.

Table 2. Summary of Austrian Disability Pension Regimes

| Regime | Assessment Basis | Pension Coefficient (%) | Other |
|---|---|---|---|
| Prior to 1988 | Last 120 months of earnings | Baseline PC: 1.9*min(IY,30) +1.5*max(IY-30,0) up to maximum PC of 80 | Relaxed screening at age 55 |
| | | "Special increment": If PC<50 and AGE<50, then add (50-AGE) to IY up to maximum PC of 50 | ERA: 60/55 for men/women FRA: 65/60 for men/women |
| 1988-1992 | Phased in change to last 180 months of earnings | No change | |
| 1993-1996 | Changed from last 180 months to best 180 months of earnings | Changed special increment: If PC<60 and AGE<56, then add (56-AGE) to IY up to maximum PC of 60 | Introduced bonus for claiming DI/OA pension after ERA: ~2.1 % per year |
| 1997-1999 | No change | Changed baseline PC: 1.83*min(IY,30) +1.675*max(IY-30,0) up to maximum PC of 80 | Changed age of relaxed screening to 57 for men |
| | | Introduced penalty (bonus) if claim before (after) ERA | |
| 2000-2003 | No change | Phased in new baseline PC: min(60,1.8-.02*(YR-2000)*min(IY,35))+ 2*max(IY-35,0) up to maximum of 80 | Changed age of relaxed screening to 57 for women |
| | | | Split DI and OA formulas |
| | | | Phased in increase in ERA to 61.5/56.5 for men/women |
| 2004-2010 | Phased in change to best 480 months of earnings (-2028) | Starting in 2005 phased in new baseline PC: max(1.96-.04*(YR-2004),1.78)*IY | Returned to same formula for OA and DI pensions in 2005 |
| | | Phased in new age of special increment to 60 | Phased in increase in ERA to 62/60 for men/women (-2017) |
| | | Increased penalty for early claiming; backlash limited loss to 5-6.5% of projected 2004 pension under prior regime | |

Notes: PC="pension coefficient," IY="insurance years," ERA="early retirement age," FRA="full retirement age," DI="disability insurance," OA="old age."

Table 3. Cross-Sectional Regressions by Year, Men, 1987-2010

| | | | | Base controls | | | Saturated controls | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | No. obs. | No. clusters | DI inflow rate | Coeff. on log benefits | Std. error | $R^2$ | Coeff. on log ben. | Std. error | $R^2$ |
| 1987 | 575,268 | 2,242 | 0.0218 | 0.0231*** | (0.0022) | 0.057 | 0.0153*** | (0.0021) | 0.076 |
| 1988 | 573,481 | 2,238 | 0.0179 | 0.0186*** | (0.0020) | 0.046 | 0.0138*** | (0.0019) | 0.064 |
| 1989 | 573,739 | 2,239 | 0.0171 | 0.0205*** | (0.0019) | 0.045 | 0.0147*** | (0.0018) | 0.065 |
| 1990 | 581,386 | 2,249 | 0.0174 | 0.0186*** | (0.0020) | 0.048 | 0.0113*** | (0.0019) | 0.070 |
| 1991 | 595,221 | 2,246 | 0.0166 | 0.0160*** | (0.0015) | 0.046 | 0.0098*** | (0.0013) | 0.069 |
| 1992 | 608,518 | 2,249 | 0.0175 | 0.0182*** | (0.0015) | 0.052 | 0.0108*** | (0.0013) | 0.073 |
| 1993 | 621,477 | 2,239 | 0.0182 | 0.0188*** | (0.0016) | 0.052 | 0.0119*** | (0.0014) | 0.074 |
| 1994 | 632,256 | 2,239 | 0.0196 | 0.0242*** | (0.0019) | 0.058 | 0.0155*** | (0.0015) | 0.083 |
| 1995 | 643,841 | 2,221 | 0.0232 | 0.0290*** | (0.0024) | 0.067 | 0.0177*** | (0.0019) | 0.093 |
| 1996 | 653,072 | 2,228 | 0.0227 | 0.0395*** | (0.0027) | 0.068 | 0.0261*** | (0.0022) | 0.092 |
| 1997 | 661,705 | 2,214 | 0.0158 | 0.0330*** | (0.0022) | 0.045 | 0.0238*** | (0.0021) | 0.064 |
| 1998 | 677,644 | 2,220 | 0.0178 | 0.0383*** | (0.0030) | 0.055 | 0.0286*** | (0.0027) | 0.079 |
| 1999 | 686,659 | 2,204 | 0.0198 | 0.0484*** | (0.0039) | 0.075 | 0.0370*** | (0.0035) | 0.102 |
| 2000 | 692,726 | 2,208 | 0.0221 | 0.0591*** | (0.0042) | 0.088 | 0.0471*** | (0.0038) | 0.118 |
| 2001 | 695,774 | 2,194 | 0.0125 | 0.0252*** | (0.0020) | 0.032 | 0.0198*** | (0.0019) | 0.056 |
| 2002 | 704,258 | 2,200 | 0.0131 | 0.0288*** | (0.0023) | 0.036 | 0.0230*** | (0.0023) | 0.064 |
| 2003 | 713,558 | 2,197 | 0.0158 | 0.0385*** | (0.0029) | 0.050 | 0.0334*** | (0.0030) | 0.084 |
| 2004 | 717,333 | 2,200 | 0.0125 | 0.0311*** | (0.0025) | 0.042 | 0.0295*** | (0.0025) | 0.070 |
| 2005 | 733,995 | 2,192 | 0.0122 | 0.0251*** | (0.0023) | 0.038 | 0.0235*** | (0.0023) | 0.078 |
| 2006 | 749,393 | 2,193 | 0.0125 | 0.0246*** | (0.0027) | 0.038 | 0.0267*** | (0.0027) | 0.079 |
| 2007 | 761,833 | 2,191 | 0.0118 | 0.0230*** | (0.0026) | 0.036 | 0.0283*** | (0.0026) | 0.076 |
| 2008 | 775,437 | 2,182 | 0.0116 | 0.0251*** | (0.0028) | 0.034 | 0.0335*** | (0.0030) | 0.073 |
| 2009 | 782,355 | 2,160 | 0.0116 | 0.0234*** | (0.0029) | 0.032 | 0.0314*** | (0.0034) | 0.069 |
| 2010 | 788,229 | 2,151 | 0.0113 | 0.0189*** | (0.0028) | 0.030 | 0.0255*** | (0.0033) | 0.065 |

Note: Base controls include age dummies, 2-year insurance year group dummies and assessment basis. Saturated controls include base controls plus dummies for tenure (in 2-year groups), any sick leave, more than 5 days of sick leave or more than 20 days of sick leave in last two years, UI benefit duration, industry, region (of firm), blue collar status and Austrian national status, and 4th order polynomials in last year's wage and average wage best 15 years. Standard errors in parentheses are clustered by cells according to an individual’s age in years, number of insurance years, and assessment basis, *** p<0.01, ** p<0.05, * p<0.1.

Table 4. Pooled Regressions, 1987-2010

| | Base controls | Saturated controls | Full model |
|---|---|---|---|
| Log benefit | 0.0309*** | 0.0235*** | 0.0197*** |
| | (0.0020) | (0.0014) | (0.0023) |
| Hypothetical log benefit 1987 | | | 0.0545*** |
| | | | (0.0046) |
| Hypothetical log benefit 1988 | | | -0.0432*** |
| | | | (0.0075) |
| Hypothetical log benefit 1989 | | | 0.0041 |
| | | | (0.0090) |
| Hypothetical log benefit 1990 | | | -0.004 |
| | | | (0.0094) |
| Hypothetical log benefit 1991 | | | 0.0652*** |
| | | | (0.0124) |
| Hypothetical log benefit 1992 | | | -0.0884*** |
| | | | (0.0097) |
| Hypothetical log benefit 1993-1996 | | | -0.0064 |
| | | | (0.0171) |
| Hypothetical log benefit 1997-1999 | | | 0.428*** |
| | | | (0.109) |
| Hypothetical log benefit 2000 | | | -0.434*** |
| | | | (0.107) |
| Hypothetical log benefit 2001 | | | -0.348** |
| | | | (0.143) |
| Hypothetical log benefit 2002 | | | 0.342 |
| | | | (0.223) |
| Hypothetical log benefit 2003 | | | -0.0367 |
| | | | (0.121) |
| Hypothetical log benefit 2004 | | | 0.120*** |
| | | | (0.0372) |
| Hypothetical log benefit 2005 | | | -0.0437 |
| | | | (0.0362) |
| Hypothetical log benefit 2006 | | | 0.0242 |
| | | | (0.0288) |
| Hypothetical log benefit 2007 | | | -0.0729*** |
| | | | (0.0170) |
| Hypothetical log benefit 2008 | | | 0.0690* |
| | | | (0.0416) |
| Hypothetical log benefit 2009 | | | -0.104 |
| | | | (0.0645) |
| Hypothetical log benefit 2010 | | | 0.0810** |
| | | | (0.0366) |
| F-test | | | 2.167 |
| Prob > F | | | 0.141 |
| Observations | 16,199,158 | 16,199,158 | 16,199,158 |
| No. clusters | 2,334 | 2,334 | 2,334 |
| R-squared | 0.046 | 0.073 | 0.073 |
| DI inflow rate | 0.0161 | 0.0161 | 0.0161 |
| Implied elasticity | 1.920*** | 1.459*** | 1.227*** |
| | (0.124) | (0.090) | (0.143) |

Note: Standard errors in parentheses are clustered by cells according to an individual’s age in years, number of insurance years, and assessment basis, *** p<0.01, ** p<0.05, * p<0.1.

Table 5. Robustness Checks

| | Coeff. on log benefits | Std. error | Implied elasticity | Std. error | No. obs. | No. clusters | F-test | Prob > F | $R^2$ |
|---|---|---|---|---|---|---|---|---|---|
| Baseline specification | 0.0197*** | (0.0023) | 1.227*** | (0.143) | 16,199,158 | 2,334 | 2.167 | 0.141 | 0.073 |
| Ages 35-64 | 0.0204*** | (0.0023) | 1.252*** | (0.141) | 1,6287,755 | 2,784 | 0.411 | 0.522 | 0.073 |
| Include old-age pension | 0.0197*** | (0.0023) | 1.224*** | (0.143) | 16,199,158 | 2334 | 2.504 | 0.114 | 0.073 |
| Restrict insurance years | 0.0327*** | (0.0034) | 1.982*** | (0.208) | 15,555,955 | 1,709 | 3.10 | 0.079 | 0.074 |
| After-tax benefits | 0.0159*** | (0.0022) | 0.985*** | (0.139) | 16,199,158 | 2,334 | 4.862 | 0.028 | 0.073 |
| Current benefits | 0.0131*** | (0.0022) | 0.813*** | (0.134) | 16,199,158 | 2,334 | 13.46 | <0.001 | 0.073 |
| Cox proportional hazard | 1.402*** | (0.131) | 1.402*** | (0.131) | 16,199,158 | 2,334 | 0.184 | 0.668 | n/a |
| Propensity score reweighting | 0.0180*** | (0.0023) | 1.120*** | (0.145) | 16,199,158 | 2,334 | 5.132 | 0.024 | 0.076 |
| Quantile control function | 0.0217*** | (0.0018) | 1.351*** | (0.110) | 16,199,158 | 2,334 | 36.60 | <0.001 | 0.075 |

Note: All regressions include saturated controls (see text for detailed description). Standard errors in parentheses are clustered by cells according to an individual’s age in years, number of insurance years, and assessment basis, *** p<0.01, ** p<0.05, * p<0.1.

Table 6. Heterogeneity

| | DI inflow rate | Coeff. on log benefits | Std. error | Implied elasticity | Std. error | No. obs. | No. clusters | F-test | Prob > F | $R^2$ |
|---|---|---|---|---|---|---|---|---|---|---|
| Age 35-39 | 0.0017 | 0.0020 | (0.0014) | 1.037 | (0.844) | 4,149,741 | 330 | 3.619 | 0.058 | 0.015 |
| Age 40-44 | 0.0030 | 0.0004 | (0.0012) | 0.145 | (0.384) | 3,779,899 | 398 | 1.574 | 0.210 | 0.022 |
| Age 45-49 | 0.0068 | 0.0064*** | (0.0014) | 0.941*** | (0.202) | 3,419,217 | 470 | 0.0861 | 0.769 | 0.032 |
| Age 50-54 | 0.0200 | 0.0141*** | (0.0022) | 0.563*** | (0.0863) | 2,893,098 | 535 | 12.23 | 0.001 | 0.068 |
| Age 55-59 | 0.0748 | 0.0141* | (0.0075) | 0.189* | (0.100) | 1,957,203 | 601 | 3.113 | 0.078 | 0.086 |
| White collar | 0.0092 | 0.0149*** | (0.0018) | 1.623*** | (0.195) | 7,466,445 | 2,283 | 8.981 | 0.003 | 0.057 |
| Blue collar | 0.0220 | 0.0216*** | (0.0034) | 0.983*** | (0.153) | 8,732,713 | 2,322 | 1.549 | 0.213 | 0.087 |
| Lifetime earnings Q1 | 0.0193 | 0.0103*** | (0.0031) | 0.532*** | (0.161) | 4,050,178 | 995 | 24.21 | <0.001 | 0.075 |
| Lifetime earnings Q2 | 0.0184 | 0.0159*** | (0.0046) | 0.861*** | (0.249) | 4,049,643 | 1,340 | 5.458 | 0.020 | 0.090 |
| Lifetime earnings Q3 | 0.0167 | 0.0306*** | (0.0053) | 1.835*** | (0.315) | 4,049,789 | 1,495 | 3.352 | 0.067 | 0.087 |
| Lifetime earnings Q4 | 0.0100 | 0.0172*** | (0.0028) | 1.714*** | (0.279) | 4,049,548 | 972 | 3.369 | 0.067 | 0.060 |
| DI benefit Q1 | 0.0160 | 0.00567*** | (0.0018) | 0.354*** | (0.110) | 4,050,002 | 1,618 | 85.42 | <0.001 | 0.065 |
| DI benefit Q2 | 0.0140 | 0.0199*** | (0.0059) | 1.425*** | (0.422) | 4,049,719 | 1,132 | 19.55 | <0.001 | 0.086 |
| DI benefit Q3 | 0.0168 | 0.0272*** | (0.0063) | 1.618*** | (0.372) | 4,049,722 | 952 | 30.96 | <0.001 | 0.096 |
| DI benefit Q4 | 0.0175 | 0.0354*** | (0.0053) | 2.019*** | (0.301) | 4,049,715 | 557 | 0.068 | 0.794 | 0.084 |
| UI: DI entry | 0.0525 | 0.198*** | (0.0177) | 3.775*** | (0.338) | 1,226,228 | 2,276 | 70.15 | <0.001 | 0.164 |

Note: See text for definitions of base and saturated controls. Standard errors in parentheses are clustered by cells according to an individual's age in years, number of insurance years, and assessment basis, *** p<0.01, ** p<0.05, * p<0.1.

Table 7. Applications and Rejections, 2004-2010

| | Applications | Rejections | DI Inflow |
|---|---|---|---|
| Log benefit | 0.0342*** | 0.0214*** | 0.0085** |
| | (0.0073) | (0.0057) | (0.0038) |
| F-test | 1.950 | 0.528 | 27.59 |
| Prob > F | 0.163 | 0.467 | <0.001 |
| Observations | 5,308,575 | 5,308,575 | 5,308,575 |
| No. clusters | 2,226 | 2,226 | 2,226 |
| R-squared | 0.143 | 0.111 | 0.072 |
| Avg. dependent variable | 0.0218 | 0.0135 | 0.0119 |
| Implied elasticity | 1.572*** | 1.590*** | 0.713** |
| | (0.334) | (0.419) | (0.317) |

Note: All regressions include saturated controls (see text for detailed description). Standard errors in parentheses are clustered by cells according to an individual's age in years, number of insurance years, and assessment basis, *** p<0.01, ** p<0.05, * p<0.1.

Figure A1: Imputed and matched DI benefits, separate years

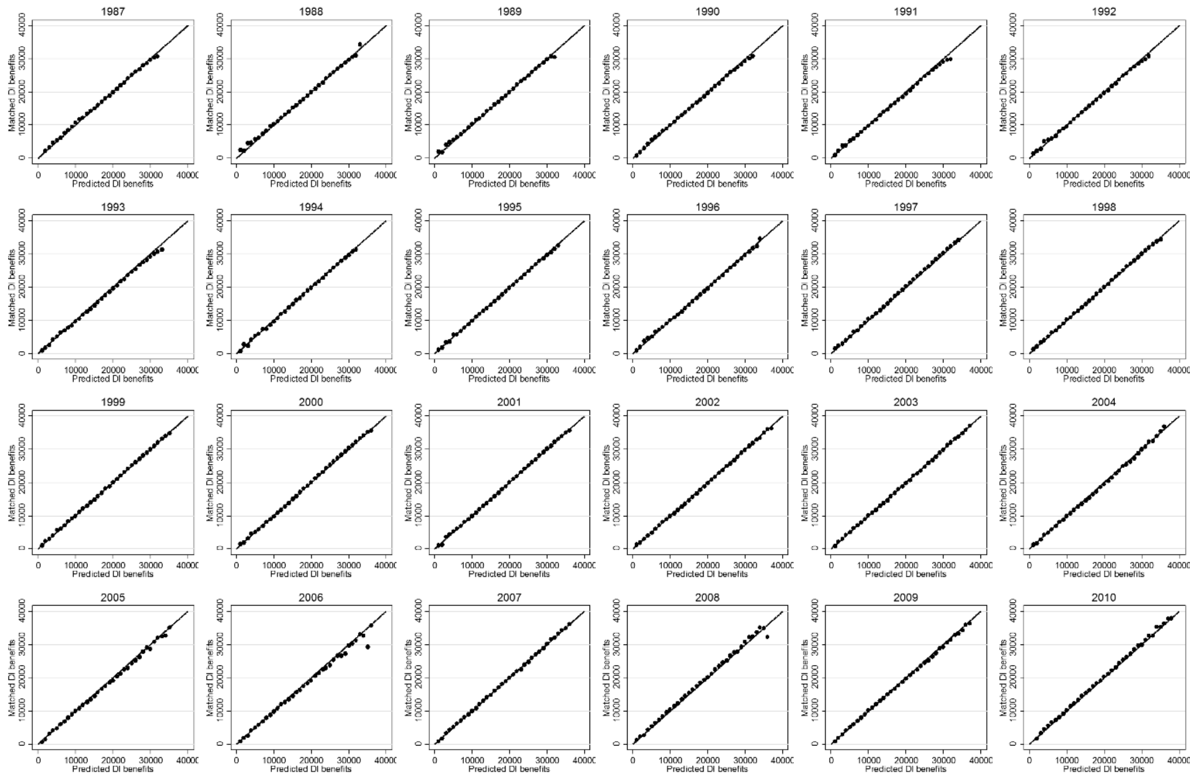


Table A1. Placebo Regressions

| | Base controls | Saturated controls | Full model |
|---|---|---|---|
| Log benefit | 0.0187*** | 0.0173*** | -4.54e-05 |
| | (0.00118) | (0.00113) | (0.000257) |
| Hypothetical log benefit 1987 | | | 0.0445*** |
| | | | (0.00398) |
| Hypothetical log benefit 1988 | | | -0.0329*** |
| | | | (0.00738) |
| Hypothetical log benefit 1989 | | | 0.00565 |
| | | | (0.00902) |
| Hypothetical log benefit 1990 | | | 0.00207 |
| | | | (0.00939) |
| Hypothetical log benefit 1991 | | | 0.0585*** |
| | | | (0.0120) |
| Hypothetical log benefit 1992 | | | -0.0833*** |
| | | | (0.00935) |
| Hypothetical log benefit 1993-1996 | | | 0.00312 |
| | | | (0.0180) |
| Hypothetical log benefit 1997-1999 | | | 0.341*** |
| | | | (0.113) |
| Hypothetical log benefit 2000 | | | -0.356*** |
| | | | (0.111) |
| Hypothetical log benefit 2001 | | | -0.253* |
| | | | (0.149) |
| Hypothetical log benefit 2002 | | | 0.133 |
| | | | (0.234) |
| Hypothetical log benefit 2003 | | | 0.123 |
| | | | (0.126) |
| Hypothetical log benefit 2004 | | | 0.0823** |
| | | | (0.0413) |
| Hypothetical log benefit 2005 | | | -0.0273 |
| | | | (0.0463) |
| Hypothetical log benefit 2006 | | | 0.0383 |
| | | | (0.0394) |
| Hypothetical log benefit 2007 | | | -0.0876*** |
| | | | (0.0224) |
| Hypothetical log benefit 2008 | | | 0.0368 |
| | | | (0.0448) |
| Hypothetical log benefit 2009 | | | 0.0686 |
| | | | (0.0870) |
| Hypothetical log benefit 2010 | | | -0.0682 |
| | | | (0.0592) |
| F-test | | | 99.53 |
| Prob > F | | | <0.001 |
| Observations | 16,199,158 | 16,199,158 | 16,199,158 |
| No. clusters | 2,334 | 2,334 | 2,334 |
| R-squared | 0.045 | 0.074 | 0.074 |
| DI inflow rate | 0.0161 | 0.0161 | 0.0161 |
| Implied elasticity | 1.162*** | 1.076*** | -0.00282 |
| | 0.0735 | 0.0701 | 0.0159 |

Note: Standard errors in parentheses are clustered by cells according to an individual's age in years, number of insurance years, and assessment basis, *** p<0.01, ** p<0.05, * p<0.1.