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## Disability Benefits, Consumption Insurance, and Household Labor Supply

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# WORKING PAPER

## Disability benefits, consumption insurance, and household labor supply

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# Disability Benefits, Consumption Insurance, and Household Labor Supply<sup>\*</sup>

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**Abstract:** While a mature literature finds that Disability Insurance (DI) receipt discourages work, the welfare implications of these findings depend on two rarely studied economic quantities: the full cost of DI allowances to taxpayers, summing over DI transfer payments, benefit substitution to or from other transfer programs, and induced changes in tax receipts; and the value that individuals and families place on receiving benefits in the event of disability. We comprehensively assess these missing margins in the context of Norway's DI system, drawing on two strengths of the Norwegian environment. First, Norwegian register data allow us to characterize the household impacts and fiscal costs of disability receipt by linking employment, taxation, benefits receipt, and assets at the person and household level. Second, random assignment of DI applicants to Norwegian judges who differ systematically in their leniency allows us to recover the causal effects of DI allowance on individuals at the margin of program entry. Accounting for the total effect of DI allowances on both household labor supply and net payments across all public transfer programs substantially alters our picture of the consumption benefits and fiscal costs of disability receipt. While DI denial causes a significant drop in household income and consumption on average, it has little impact on income or consumption of married applicants; spousal earnings and benefit substitution entirely offset the loss in DI benefit payments. To develop the welfare implications of these findings, we estimate a dynamic model of household behavior that translates employment, reapplication and savings decisions into revealed preferences for leisure and consumption. We find that household valuation of receipt of DI benefits is considerably greater for single and unmarried individuals than for married couples because spousal labor supply substantially buffers household income and consumption in the event of DI denial.

**Keywords:** disability insurance; consumption insurance; household labor supply; added worker

**JEL codes:** I38, J62, H53

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# 1 Introduction

Over the past 50 years, disability insurance (DI) rolls have risen dramatically in many OECD countries. In the U.S., SSDI benefits receipt has risen from less than 1 percent to nearly 5 percent of the non-elderly adult population. In many European countries, the increases are even more striking, from 1 percent to 7 percent in the U.K and from 2 percent to almost 10 percent in Norway. These increases have made DI one of the largest transfer programs in most OECD countries. In the U.S., for example, outlays for DI exceed those for food stamps, traditional cash welfare, or the EITC.<sup>1</sup> For families without small children, DI is often the primary cash benefit available after unemployment benefits run out and it has therefore become an increasingly important component of the social safety net.

To limit DI program growth, several countries have significantly tightened disability screening criteria, and many others are considering similar policies.<sup>2</sup> These enhanced gatekeeping policies can reduce the fiscal burden of disability insurance, both by lowering the DI caseload and, if rejected applicants return to work, by increasing tax revenue. At the same time, stricter screening may result in net welfare losses for individuals and families that value public disability insurance at more than its fiscal cost.<sup>3</sup> Assessing this tradeoff requires a comparison of the public costs and private benefits of DI awards for applicants at the margin of allowance versus denial, since it is their outcomes that would be changed by shifts in screening stringency. To implement this comparison, we need data on two economic quantities that are rarely measured: the economic value that individuals and families place on disability insurance; and the full cost of DI allowances to taxpayers, summing over DI transfer payments, benefit substitution to or from other transfer programs, and induced changes in tax receipts. Credibly estimating these quantities is typically hindered both by a lack of comprehensive linked data measuring the many outcomes, and by the difficulty of distinguishing the causal effects of DI receipts from the many unobserved factors that simultaneously determine disability status, earnings, tax payments and transfer receipts, and consumption.

This paper tries to address both the measurement and the identification challenge in the context of Norway's DI system, offering empirical evidence on the fiscal costs, income and consumption gains, and welfare implications of DI receipt. Our work draws on two strengths of the Norwegian environment. First, Norwegian register data allow us to characterize the household impacts and fiscal costs of disability receipt by linking employment, taxation, benefits receipt, and assets at the

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<sup>1</sup>In 2011 the U.S. paid out \$129 billion to 10.6 million disabled workers and their families, with an additional \$33 billion worth of disability benefits from the SSI program for poor Americans and \$90 billion in Medicaid for disabled workers (OASDI Trustees Report, 2012). In 2009, DI payments constituted 1.8 percent of GDP in the U.S. and 2.3 percent of GDP across the European OECD-countries (OECD, 2010).

<sup>2</sup>For example, the U.S. tightened the criteria for new disability awards in the late 1970s and introduced an aggressive program of continuing disability reviews in 1980; however, Congress responded by halting the reviews and, in 1984, liberalizing the program's screening criteria along several dimensions. Another example is the Netherlands; in 1994, the eligibility criteria were tightened and the growth in DI rolls reversed.

<sup>3</sup>In the U.S., all private disability insurance is provided through employer-based group policies. These policies 'wrap-around' the public SSDI system, so that most of the wage insurance risk and all of the medical cost risk is ultimately borne by the public program (Autor *et al.*, 2014b). There is not a strong standalone private market in disability insurance, likely because of adverse selection. In the Norwegian setting that we study, private disability insurance is rare.

person and household level. Our measure of fiscal costs includes virtually all forms of government cash transfers and revenues from (direct) taxes, accounting for changes in labor supply and substitution to other transfer programs. Our measures of household impacts of DI receipt include net government transfer payments from all sources, employment and earnings of DI applicants (both allowed and denied) and their spouses, as well as households' total income and measures of their consumption expenditure. Second, we address the threats to identification by exploiting the random assignment of DI applicants to Norwegian judges who differ systematically in their leniency. This approach recovers the causal effects of DI allowance on individuals at the margin of program entry. As a measure of judge leniency, we use the average allowance rate in all other cases a judge has handled. This leniency measure is highly predictive of judicial rulings in incumbent cases but uncorrelated with case characteristics.

Our first set of analyses estimating the causal effects of DI receipt on earnings, total income, consumption expenditure, and fiscal costs yields three main findings. First, granting DI benefits to applicants on the margin of program entry induces an fall in annual earnings of approximately \$6,800, which is about 40 percent of the annual DI transfer benefit awarded. Second, DI allowances raise average household income and consumption expenditure by 16 and 18 percent, implying that DI receipt provides partial consumption smoothing across states of nature for a given individual. Third, DI allowances have starkly different impacts on applicants according to marital status. Among single and unmarried applicants, DI awards have large direct impacts on household income and consumption expenditure. Conversely, DI allowances do not increase the household income or the consumption of married applicants on average. The reason is that spousal labor supply and benefit substitution entirely offset the gain in DI benefit payments. Despite leaving household income and consumption expenditure of married applicants largely unaffected, DI awards induce considerable external costs on other taxpayers through higher transfers and reduced payroll tax revenues. Taken together, our findings show that accounting for the total effect of DI allowances on both household labor supply and net payments across all public transfer programs substantially alters the conclusions about the consumption benefits and fiscal costs of disability receipt.

To explore the welfare implications of the estimated causal effects of DI receipt, we use a dynamic model of household behavior with heterogeneous, forward-looking individuals. The model translates employment, savings and reapplication decisions of applicants and their spouses into revealed preferences for leisure and consumption. Brought to the data, the model matches well the causal estimates of the impact of DI allowances on employment and total household income obtained non-structurally, and moreover, provides plausible parameter estimates for labor supply elasticities. We use the estimated model to compute the welfare benefits of DI receipt—by which we mean the cash equivalent value of receiving a DI allowance—and to perform counterfactual analyses that allows us to infer the extent to which the welfare loss stemming from DI denial is buffered by household labor supply, savings, and the possibility of reapplying for DI. Among married couples, there is a relatively small positive welfare benefit of DI receipt, reflecting increased leisure of applicants and their spouses without any substantial change in household income or consumption.

By comparison, the welfare gains among single and unmarried applicants are large, and almost entirely due to increased disposable income. Thus, household valuation of receipt of DI benefits is considerably greater for single and unmarried individuals than for married couples.

Our paper contributes to a growing literature on the causes and consequences of the growth in DI rolls (for a review, see [Autor & Duggan, 2006](#); [Autor, 2011](#); [Liebman, 2015](#)). While the core of this literature focuses on the impacts of disability benefits on the employment and earnings effects of DI allowance, little is known about either the fiscal costs or the household level effects on labor supply and consumption.<sup>4</sup> [Meyer & Mok \(2013\)](#) and [Kostol & Mogstad \(2015\)](#) offer to our knowledge the only prior study that documents changes in income and consumption that follow changes in health and disability. Our identification strategy, which uses judge assignments to isolate quasi-experimental variation in disability allowances, builds on three recent studies using U.S. data to estimate labor supply impacts of DI receipt. Exploiting variation in DI allowances stemming from differences in disability examiner leniency, [Maestas \*et al.\* \(2013\)](#) and [Autor \*et al.\* \(2014a\)](#) find that DI receipt substantially reduces earnings and employment of applicants. [French & Song \(2013\)](#) pursue a similar strategy—using variation in the leniency of appeal judges rather than initial examiners—and find comparable labor supply effects of DI receipt among appellants.

Our study makes two contributions to this active literature. It combines quasi-experimental variation in judicial disability determinations with extensive register data on disability applicants and household members to provide novel evidence on the income gains, consumption benefits and fiscal costs of DI receipt.<sup>5</sup> Second, by integrating causal impact estimates along multiple dimension, the subsequent structural model estimation offers a welfare assessment of these findings. Our structural model mirrors the life-cycle model used by [Low & Pistaferri \(2015\)](#) to analyze the insurance value and incentive costs of DI benefits. We deviate from Low and Pistaferri in two important exceptions. While Low and Pistaferri model individual behavior, and hence do not consider insurance from spousal labor supply, we model household behavior, which is important given our finding of a strong spousal labor supply response. Accordingly, we estimate a life-cycle model with two earners making consumption and labor supply decisions. Distinct from Low and Pistaferri, we do not model the pre-application behavior of households, largely because we do not have health information for people who do not apply for DI. Our goal is therefore limited to understanding the post-application labor supply, savings, and reapplication decisions of applicants and their spouses, taking as given their characteristics and economic circumstances at the time of application. As a consequence, the results from our counterfactuals do not take into account potential changes in the number and composition of applicants.

Our paper also advances our understanding of how households respond to shocks to income.<sup>6</sup>

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<sup>4</sup>This literature includes [Parsons \(1980\)](#), [Bound \(1989\)](#), [Gruber \(2000\)](#), [Chen & van der Klaauw \(2008\)](#), and [Kostol & Mogstad \(2014\)](#) as well as the methodologically related papers on DI discussed immediately below. See also [Autor & Duggan \(2003\)](#) and [Borghans \*et al.\* \(2014\)](#) for empirical evidence on the interaction between disability insurance and other transfer programs in the U.S. and Netherlands.

<sup>5</sup>Our analysis uses the same identification strategy as [Dahl \*et al.\* \(2014\)](#) though applied to a distinct question and set of outcomes.

<sup>6</sup>The literature is reviewed by [Blundell \*et al.\* \(2008\)](#), [Meghir & Pistaferri \(2011\)](#) and [Blundell \*et al.\* \(2016b\)](#).

Most work in this literature assumes exogenous labor supply, focuses on a single earner, or imposes restrictions on the nature and type of insurance available to families. A notable exception is [Blundell \*et al.\* \(2016b\)](#), who estimate a life cycle model with two earners making consumption and labor supply decisions.<sup>7</sup> Consistent with our findings, Blundell *et al.* find an important role for consumption insurance through household labor supply, while self-insurance through savings and borrowing matter less. In line with these results, [Persson \(2015\)](#) finds that husbands increase their labor supply to offset household income losses following the elimination of survivors insurance for their wives, and [Fadlon & Nielsen \(2015\)](#) find that wives offset income losses following the death of a spouse through increased labor supply.

A related literature tests for the added worker effect, i.e., an increase in spousal labor supply induced by negative income shocks to the other spouse ([Lundberg, 1985](#)). [Cullen & Gruber \(2000\)](#) review this literature and highlight the difficulty in drawing credible inferences from observational data. One challenge is to locate a plausibly exogenous shock to the income of one spouse that does not directly affect the labor supply of the other spouse, thus overcoming the problem of reflection or simultaneity. Another difficulty is to control for correlated unobserved spousal heterogeneity in earnings capacity, health, and the taste for work, all of which might bias estimates of an added worker effect. A third challenge is to avoid or model correlated shocks across spouses. If, for example, a general economic downturn causes a negative income shock to a primary earner, his or her spouse’s market wage will likely fall concurrently, thus biasing downward the estimated added worker effect. Our research design resolves these challenges by identifying a plausibly exogenous income shock (DI allowance) that directly affects only one member of the household (the DI applicant), thereby providing a strongly test of the added-worker effect.

The remainder of the paper proceeds as follows: Section 2 reviews the key features regarding the DI program in Norway, compares the system with the U.S. system, and describes the research design. In Section 3, we describe the data and sample restrictions. Section 4 assesses the relevance and validity of our instrument. Section 5 presents our findings on how the applicants respond to being denied versus allowed DI, and discusses the estimates of spousal responses to the allowance decision. Section 6 presents our findings on the fiscal costs and consumption benefits of disability receipt. Section 7 describes the structural model of household labor supply and explores welfare implication of disability receipt. The final section offers some concluding remarks.

## 2 Background

Following an institutional and statistical description of the Norwegian DI program, we document how the DI system generates quasi-random disability allowances for a subset of DI appellants (i.e., applicants who appeal their initial denial) and explain how our research design uses this variation

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<sup>7</sup>A complementary exception is [Finkelstein \*et al.\* \(2015\)](#), who directly estimate the insurance value of Medicaid in-kind public health plan benefits using variation from a randomized controlled trial. Distinct from our focus, their work (a) abstracts from labor supply considerations since labor supply appears unaffected by Medicaid provision in their setting ([Baicker \*et al.\*, 2014](#)); and (b) estimates both the transfer and ex ante insurance values of public benefits provision, whereas we estimate only the first component.



to estimate the economic consequences of DI allowances.

### **The Norwegian DI program**

In Norway, DI benefits are designed to provide partial earnings replacements to all workers under the full retirement age who are unable to engage in substantial gainful activity because of a medically determined physical or mental impairment that has lasted for at least a year. The DI program is part of the broader Social Security System and is financed through employer- and employee-paid taxes. The level of DI benefits received is determined using a formula based on an individual's earnings history. The benefits schedule is progressive, so that low-wage workers replace a larger fraction of their earnings with DI benefits than do high-wage workers.

The disability determination process involves multiple steps, diagrammed in Figure 1. The first step is the submission of an initial application to the Social Security Administration office for the Disability Determination Stage (DDS) review. If the applicant meets the non-medical criteria, such as age and prior employment requirements, disability examiners and medical staff assess written medical evidence regarding the applicant's ability to perform work-related activities. Examiners take into account the applicant's health status, age, education, work experience and the transferability of her skills. If the disability examiner concludes that the applicant cannot be expected to engage in any substantial gainful activity, a disability award is made. Approximately 75 percent of applicants are awarded benefits at the DDS review. About 30 percent of beneficiaries receive partial awards. Cases that are more difficult to assess—typically claims of mental illness and lower back pain—are often denied at this step.

Applicants who are denied at the DDS review may appeal within two months to the Court of Appeals, and about 25 percent of denied applicants do so. Appellants are assigned to Administrative Law Judges (ALJs), who either allow, deny, or remand (i.e., return to the DDS for reevaluation) their cases.<sup>8</sup> ALJs are required to apply the same criteria used in the initial determination process, but applicants may present new supporting information in writing. Approximately 15 percent of all appealed claims are allowed at the ALJ level. If the appeal is denied, the applicant can choose to start a new DI case by reapplying to the DDS Review stage.

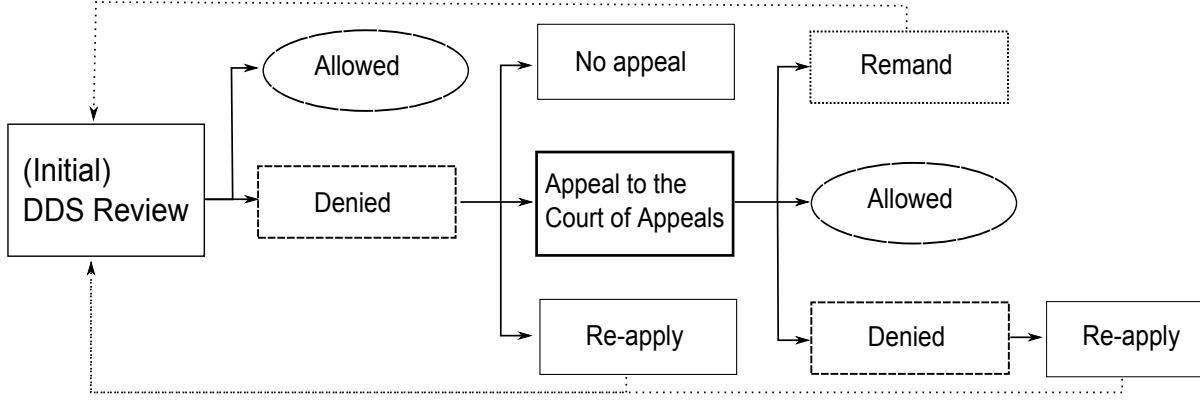
### **Assignment of DI cases to judges**

All Norwegian disability appeals are heard in Oslo. Prior to 1997, there was only one hearing department; subsequently, there were four equally sized departments, all housed in the same building, and with no specialization across the four departments. Within each department, the assignment of cases to Administrative Law Judge is performed by a department head who does not have knowledge of the content of cases. As stipulated in the rules set forth for the Administrative Law Court, case

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<sup>8</sup>Average processing time at the DDS stage is six months, and average processing time at the appeal stage is four months. Remands are uncommon, accounting for only five percent of appeal outcomes. In our baseline analysis, we code remanded cases as rejected. As a robustness check, we recoded remanded cases as allowed or denied based on their eventual outcome after they were reconsidered by the DDS case worker. Results are comparable in either case.

Figure 1: DI Application and Appeals Process



Notes: This figure summarizes the description of the application and appeal process in the Norwegian DI system.

assignment should be done “by the drawing of lots.” In practice, cases are assigned on a rotating basis depending on the date they are received and the alphabetical ordering of a judge’s last name.<sup>9</sup>

Unusual among national disability systems, Norwegian judges are not specialized according to cases characteristic (medical condition, geographic region, or other attributes), and there is never any personal contact between judges and appellants (all evidence is presented in writing). Appellants have no option to seek a different judge from the one to whom they are assigned.

### Verifying random assignment

Table 1 verifies that the hearing office assignment mechanism generates a distribution of cases across judges that is consistent with random assignment. There are 75 judges in our sample who have handled, on average, 375 cases each. We measure judge leniency as the average allowance rate in all other cases a judge has handled (including the judge’s past and future cases that may fall entirely outside of our estimation sample). To purge any differences over time or across departments in the characteristics of appellants or the overall leniency rate of the DI system, we always control for fully interacted year by department dummies (the level at which randomization occurs.)

The first column of Table 1 uses a linear probability model to test whether appellants’ (pre-determined) characteristics and economic conditions are predictive of case outcomes. As expected, demographic, economic and health variables are highly predictive of whether an appealed case is allowed. Column 3 assesses whether these same case characteristics are predictive of the leniency of the judges to which cases are assigned. Reassuringly, we find no relationship. Jointly, these 21 variables explain about 0.1 percent of the variation in the judge leniency measure (joint p-value of 0.72), and none is statistically significant at the 10 percent level.

<sup>9</sup>We verified these rules with the current Head of the Administrative Law Court, Knut Brofoss. The rules are explained in “Veileder for Saksbehandlingen i Trygderetten” (Guidelines for Processing Cases in the Court of Appeals). We have also presented our work at internal seminars with the current set of judges and department heads to make sure that we have understood how the cases are handled and assessed.

Table 1: Testing for Random Assignment of Cases to Judges

	(1)	(2)	(3)	(4)
Dependent Variable:	Case Allowed		Judge Leniency	
	coef.	s.e.	coef.	s.e.
<b>A. Pre-determined characteristics</b>				
Age	0.0044***	(0.0003)	0.0001	(0.0001)
Number of persons in household	-0.0143***	(0.0021)	-0.0003	(0.0003)
Female	0.0193***	(0.0056)	0.0008	(0.0012)
Married	0.0146**	(0.0066)	0.0005	(0.0012)
Foreign born	-0.0446***	(0.0086)	-0.0003	(0.0015)
Less than high school degree	-0.0231***	(0.0061)	-0.0005	(0.0008)
High school degree	0.0195***	(0.0061)	0.0001	(0.0007)
Any college	0.0119	(0.0116)	0.0010	(0.0014)
Children below age 18	-0.0601***	(0.0058)	-0.0009	(0.0010)
Musculoskeletal disorders	-0.0171***	(0.0059)	0.0005	(0.0017)
Mental disorders	0.0088	(0.0075)	-0.0003	(0.0024)
Circulatory system	0.0235	(0.0158)	0.0000	(0.0023)
Respiratory system	-0.0196	(0.0151)	-0.0021	(0.0021)
Neurological system	0.0459**	(0.0206)	0.0011	(0.0021)
Endocrine diseases	0.0418***	(0.0174)	-0.0029	(0.0031)
<b>B. Pre-determined economic variables</b>				
Average indexed earnings (\$1,000)	0.0009***	(0.0002)	0.0000	(0.0000)
Total transfers (\$1,000)	-0.0004	(0.0003)	0.0001	(0.0001)
Liquid assets (\$1,000, per capita)	0.0004**	(0.0002)	0.0000	(0.0001)
Total gross wealth (\$1,000, per capita)	0.0001***	(0.0000)	0.0000	(0.0000)
Total liabilities (\$1,000, per capita)	0.0001	(0.0001)	0.0000	(0.0000)
Disposable income ( \$1,000, per capita)	0.0006*	(0.0004)	0.0000	(0.0002)
F-statistic for joint significance	24.36		0.78	
[p-value]	[.001]		[.72]	
Observations	14,092		14,092	

\*\*\*p<.01, \*\*p<.05, \*p<.10. Standard errors (in parentheses) are clustered at the judge level.

*Notes:* This table reports an F-test of whether the hearing office complied with the random allocation procedure described in Section 2. The baseline estimation sample consists of individuals who appeal an initially denied DI claim during the period 1994-2005 (see Section 3 for further details). There are 75 unique judges. Columns report OLS regressions of appellant characteristics on (column 1) a dummy variable for whether the case was allowed; and (column 3) our measure of judge leniency. F-statistics are obtained from OLS estimation on the combined set of appellant characteristics. Each regression controls for fully interacted year of appeal and department dummies. Characteristics of appellants are measured prior to appeal. Variable definitions are as follows: children is equal to 1 if appellant has children under age 18 and 0 otherwise; any college is equal to one if a person has some college or has a college degree; body system codes are based on ICD-10 diagnostic codes. Pre-determined economic variables are measured one year before appeal, and average indexed earnings is mean earnings for the ten years prior to appeal. Assets, wealth, liability and disposable income are measured at the household level and normalized by the number of household members. Nominal values are deflated to 2005 and represented in US dollars using the average exchange rate NOK/\$ = 6.

A natural question is why some judges are more lenient than others. We have few detailed characteristics of judges to help illuminate this question, but we do know the number of cases that each judge has handled. We find that experienced judges appear to be slightly less lenient, but experience accounts for only a small fraction of the total variation in allowance rates across judges

(see Appendix Figure A.1).<sup>10</sup>

### Instrument and 2SLS model

We use variation in DI allowance generated from the random assignment of appeal judges as an instrument to estimate the economic consequences of disability receipt. Because some judges are systematically more lenient than others, as we document below, random assignment of appellants to judges rise to exogenous variation in the probability an individual is allowed DI in the appeals process.

Our baseline instrumental variables (IV) model can be described by the following two-equation system:

$$A_i = \gamma Z_{j(i)} + X_i' \delta + \varepsilon_i \quad (1)$$

$$Y_{it} = \beta_t A_i + X_i' \theta_t + \eta_{it}. \quad (2)$$

Here,  $A_i$  is an indicator variable equal to 1 if appellant  $i$  is allowed DI at the appeal, and  $Z_{j(i)}$  is the leniency measure for judge  $j$  to which appellant  $i$  is assigned. The vector  $X_i$  contains relevant control variables, including a full set of year-of-appeal by department dummies. In the second stage equation,  $Y_{it}$  is a dependent variable of interest that is measured for appellant  $i$  at some point  $t$  after the allowance decision (e.g. earnings three years after the decision).

The target of our estimation is the average of  $\beta_t$  among individuals who are allowed DI at the appeal because they were assigned to a lenient judge. To estimate this Local Average Treatment Effect, our baseline specification uses 2SLS with first and second stage equations given by (1) and (2). The endogenous variable in our estimation is an indicator for whether an appellant was allowed DI *at appeal*, rather than whether or not the appellant is currently receiving DI when outcome  $Y_{it}$  is observed. This specification alleviates concerns about the exclusion restriction: 2SLS estimates of  $\beta_t$  capture the causal effects of the *initial* judicial disability determination, which may operate through a number of channels, including participation in DI, subsequent reapplications to the DI program following denial, or other behavioral changes resulting from the initial outcome at appeal. We can also estimate the reduced form effect of judge leniency on appellant outcomes by directly regressing  $Y$  on  $Z$  and  $X$ .

## 3 Data and Background

### 3.1 Data and Sample Restrictions

Our analysis draws on multiple administrative data sources that are linked by unique person-level identifiers. Information on DI benefits comes from social security registers that contain complete records for all individuals who entered the DI program during the period 1967-2010. These data

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<sup>10</sup> Analyzing the underlying sources of the inter-judge differences in leniency remains outside the scope (and reach) of our paper. What is critical for our analysis is that appellants are randomly assigned to judges (as our data confirm), that some judges are systematically more lenient than others (as documented in Section 4.1), and that cases allowed by a strict judge would also be allowed by a lenient one (consistent with the tests in Section 4.2).

include information on the individual’s work history and medical diagnosis, the month when DI was awarded or denied, and the level of DI benefits received. These person-level records are linked to hearing office records on all DI appeals during 1989 through 2011, including dates of appeal and decision, outcomes for each appeal, and unique identifiers for both judges and appellants.

To capture complete information on DI applicants’ earnings, income and assets, we merge the social security data with longitudinal administrative registers provided by Statistics Norway covering every Norwegian resident from 1967 to 2010. These register data enumerate individual demographic information (including sex, age, and education) and, since 1993, all sources of annual income, including earnings, self-employment income, capital income, and cash transfers, as well as most types of assets holdings and liabilities, such as real estate, financial portfolio, and debt. Income data are reported in annual amounts, while the values of assets holdings and liabilities are measured as of the last day of each year.

The Norwegian data have several advantages over register data collected by many other countries. Because most components of income and wealth are third-party reported (e.g. by employers, banks and financial intermediaries), the coverage and reliability are rated as exceptional by international quality assessments (see e.g. [Atkinson et al. 1995](#)). Because in Norway, most register data are a matter of public record, there is no attrition from the original sample due to non-response or non-consent. The income and wealth data pertain to all Norwegian residents, and are therefore not limited to those employed in jobs covered by social security, individuals who respond to wealth surveys, or households that file estate tax returns. Measures of incomes and wealth are recorded without any top or bottom coding.<sup>11</sup> Finally, unique identifiers allow us to match spouses to one another and parents to children, thereby constructing measures of per capita household income and consumption.

A key challenge in estimating the consumption benefits of disability insurance is the lack of reliable longitudinal data on consumption expenditures. One approach to measuring expenditures is to use survey data, but expenditure surveys typical have small sample sizes and face significant measurement issues (see [Pistaferri, 2015](#) for a discussion). A second option is to create measures of consumption from the accounting identity that total consumption expenditure is equal to income plus capital gains minus the change in wealth over the period. [Browning & Leth-Petersen \(2003\)](#) shows how one can construct such measures of consumption from longitudinal data on income and assets. [Eika et al. \(2017\)](#) perform a similar exercise using highly detailed Norwegian data on income and assets. Their analysis shows that the measures of consumption derived from income and assets conform well to those reported in family expenditure surveys and to the aggregates from national accounts. We use their measures here, and refer to [Eika et al. \(2017\)](#) for more details.

Our empirical analysis considers individuals who appeal an initially denied DI claim.<sup>12</sup> To observe individuals for at least four years after the appeal decision, our estimation sample consists

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<sup>11</sup>Some individuals are reported with negative income components (e.g. negative cash transfers). In these cases, we truncate the income components at zero. We also top-code a handful of observations with extremely large income components. The results do not change appreciably if we retain these outliers.

<sup>12</sup>Some individuals have several denied DI claims over the period we consider. In such cases, we restrict our sample to the individual’s first denied DI claim.

of individuals whose appeal decision was made during the period 1994-2005. To reduce sampling variation in the instrumental variable, we follow [Maestas \*et al.\* \(2013\)](#) and [French & Song \(2013\)](#) in excluding observations for which the assigned appeal judge has handled fewer than 10 cases during the 1989 through 2011 period.<sup>13</sup> To circumvent the issue of older appellants substituting between DI and early retirement, we also exclude appellants who are above age 62 at the time of appeal.

In Table 2, we document characteristics of the sample of individuals who apply for DI and the subsample who appeal an initially denied DI claim (our baseline sample). Relative to the full sample of initial applicants, those who appeal are more likely to be female, are less educated, are more likely to be foreign born, and have lower prior earnings and assets. Sixty-three percent of applicants claim mental or musculoskeletal disorders, whereas this figure is 70 percent for appellants.

### 3.2 Institutional Background

There are a number of similarities and some key differences between the DI systems in the U.S. and in Norway (see [Autor & Duggan, 2006](#); [Kostol & Mogstad, 2014](#)). In both countries, DI is one of the largest transfer programs. However, the prevalence of receipt of DI benefits is lower in the U.S. than in Norway, as shown in Figure 2, while the time trends are similar.<sup>14</sup> From 1961 to 2012, DI prevalence increased from 2.2 to 9.7 percent in Norway and from 0.8 to 5.0 percent in the U.S. While Norway’s prevalence has leveled off at about 10 percent in recent years, the U.S. DI rate continued to rise steeply through 2013, after which time growth peaked and then reversed (Social Security Advisory Board, 2015).<sup>15</sup>

In both countries, the expansion of the DI rolls in recent decades appears to be driven in significant part by changes in disability screening criteria, which led to a steep rise in the share of DI recipients suffering from difficult-to-verify disorders such as mental illness and musculoskeletal disease.<sup>16</sup> Because mental illness and musculoskeletal disease have low associated mortality rates—and moreover, because mental illness typically has an early onset—DI recipients with such diagnoses

<sup>13</sup>Including these judges does not change the estimates appreciably, and neither does excluding judges who handle fewer than 50 cases.

<sup>14</sup>The cross-country difference in DI coverage is unlikely to explain the entire discrepancy in the incidence of DI: although virtually all non-elderly adults are covered in Norway, more than 80 percent of all non-elderly adults are covered in the U.S. The remaining difference could be a function of underlying differences in screening stringency, the generosity of the programs or the frequency with which people apply for disability benefits. [Milligan & Wise \(2011\)](#) argue that differences in health are unlikely to explain much of the observed differences in DI rates across developed countries.

<sup>15</sup>The U.S. Supplemental Security Income program (SSI) also provides disability benefits to adults and children with work-limiting disabilities. The SSI program has the same medical eligibility criteria as the DI program, but distinct from DI, it is a need-based program not an insurance program; hence, SSI benefits are not a function of prior earnings and eligibility depends on asset as well as income tests. Most adults who receive SSI have limited work histories (or, in the case of children, no work history), and hence are not insured by DI. In 2015, approximately 1.3 of 9.4 million DI recipients received both DI and SSI concurrently due to poverty. An additional 3.5 million adults received SSI alone (see [https://www.ssa.gov/policy/docs/statcomps/di\\_asr/2015/sect05.html](https://www.ssa.gov/policy/docs/statcomps/di_asr/2015/sect05.html)). DI and SSI therefore jointly provide disability benefits to a larger share of U.S. adults than does DI alone, but the U.S. DI program is more comparable to the Norwegian DI program than is the U.S. SSI program since SSI primarily provides benefits to adults with little work history.

<sup>16</sup>See [Autor & Duggan \(2006\)](#) and [Liebman \(2015\)](#) for discussions of this phenomenon. In the U.S., the 1984 congressional reforms shifted the focus of screening from medical to functional criteria. In Norway, the medical eligibility criteria were relaxed earlier and more gradually.

tend to participate in the program for relatively long periods. DI exit rates in both countries have decreased in the last few decades, with progressively fewer DI recipients reaching retirement age or dying in a given year (see Appendix Figures A.2 and A.3). In addition, the aging of the Baby Boom cohorts into their peak (near-elderly) disability age brackets has contributed substantially to the expansion of the U.S. DI rolls since the mid-1990s (Liebman, 2015)

Table 2: Descriptive Statistics of Applicants and Appellants

	DI applicants		DI appellants		Test of equal means
	Mean	Std. Dev.	Mean	Std. Dev.	t-stat
<b>A. Pre-determined characteristics</b>					
Age (at the time of decision)	48.55	[9.98]	46.61	[9.30]	-25.17
Number of persons in household	2.37	[1.17]	2.79	[1.30]	39.28
Female	0.56	[0.50]	0.63	[0.48]	17.5
Married	0.57	[0.50]	0.57	[0.49]	0.73
Foreign born	0.08	[0.27]	0.18	[0.38]	32.81
Less than high school degree	0.43	[0.50]	0.50	[0.50]	16.97
High school degree	0.42	[0.49]	0.39	[0.49]	-8.17
Any college	0.13	[0.34]	0.11	[0.31]	-7.64
Children below age 18	0.3	[0.46]	0.58	[0.49]	66.48
Musculoskeletal disorders	0.37	[0.48]	0.44	[0.50]	17.67
Mental disorders	0.26	[0.44]	0.26	[0.44]	1.42
Circulatory system	0.08	[0.27]	0.04	[0.19]	-27.59
Respiratory system	0.03	[0.17]	0.03	[0.16]	-4.12
Neurological system	0.06	[0.23]	0.04	[0.19]	-12.3
Endocrine diseases	0.02	[0.14]	0.04	[0.20]	14.05
<b>B. Pre-determined economic variables</b>					
Average indexed earnings (\$1,000)	32.76	[23.66]	25.81	[21.25]	-39.3
Total transfers (\$1,000)	14.81	[14.90]	15.78	[14.06]	8.21
Liquid assets (\$1,000, per capita)	23.85	[43.85]	9.63	[21.29]	-72.06
Total gross wealth (\$1,000, per capita)	173.13	[212.10]	91.81	[105.93]	-83.76
Total liabilities (\$1,000, per capita)	54.72	[67.25]	38.43	[49.21]	-37.97
Disposable income ( \$1,000, per capita)	26.54	[14.88]	24.08	[13.11]	-22.14
DI allowed	0.79	[0.41]	0.13	[0.33]	
Observations	240,900		14,092		

Standard deviations [in square brackets]

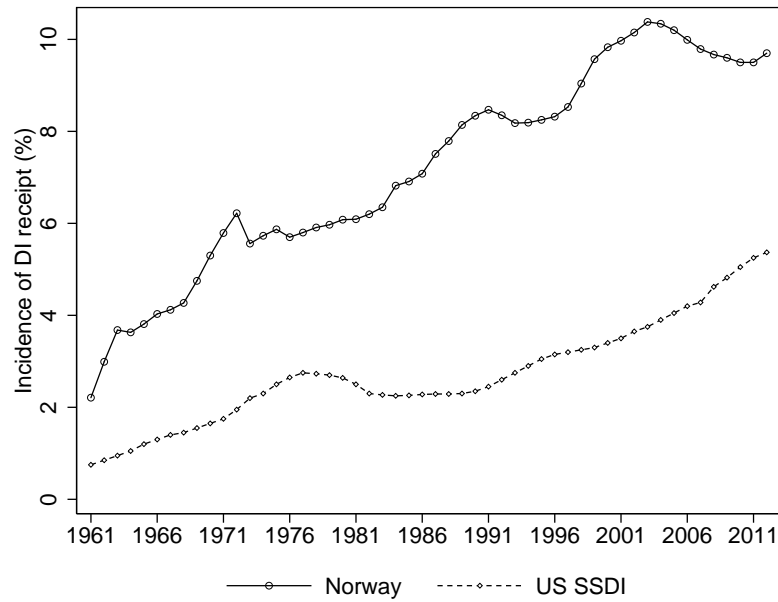
*Notes:* This table reports descriptive statistics for applicants and appellants. The applicant sample consists of all claims made during the period 1992-2003 by individuals who are at most 61 years of age. The appellant sample consists of the subset of applicants who filed an appeal during the period 1994-2005 (see Section 3 for further details). All characteristics are measured the year before application/appeal unless otherwise stated. The final column reports t-statistics of the test of equality between characteristics of applicants and appellants. Variable definitions are as in Table 1.

There are also noteworthy differences between the U.S. and Norwegian DI programs. One difference is their income replacement rates. Kostol & Mogstad, 2014 compute the replacement rate for a typical Norwegian applicant in according to the SSDI rules and the Norwegian program.



For the worker they consider, the pre-tax income replacement rate would be 31 percent in the U.S. program and 58 percent in the Norwegian program. These calculations disregard income taxation, dependent benefits, and health insurance, however. Both countries' DI programs provide dependent benefits. In addition, DI recipients in the U.S. receive health insurance coverage through the federal Medicare program, which is a substantial in-kind benefit.<sup>17</sup> In Norway, all citizens are eligible for health insurance through the Social Insurance System. Another difference is that the appeal process plays a more important role in the U.S. than in Norway. While 48 percent of initially rejected applicants appeal in the U.S. (French & Song, 2013), only 25 percent of those rejected in Norway appeal. Success rates at appeal are also considerably higher in the U.S. than Norway.

Figure 2: Trends in DI Receipt in Norway and the U.S.



Notes: This figure displays trends in DI receipt in Norway and the U.S. (see Section 2).. U.S. trends are based on Autor & Duggan (2006) for 1957-2005 and SSA Office of the Chief Actuary for 2006-2012. Norwegian trends are based on SSA Statistical Supplements. Incidence of DI receipt defined as the percent of the relevant adult population receiving DI benefits (age 18-67 in Norway; age 25-64 in the US).

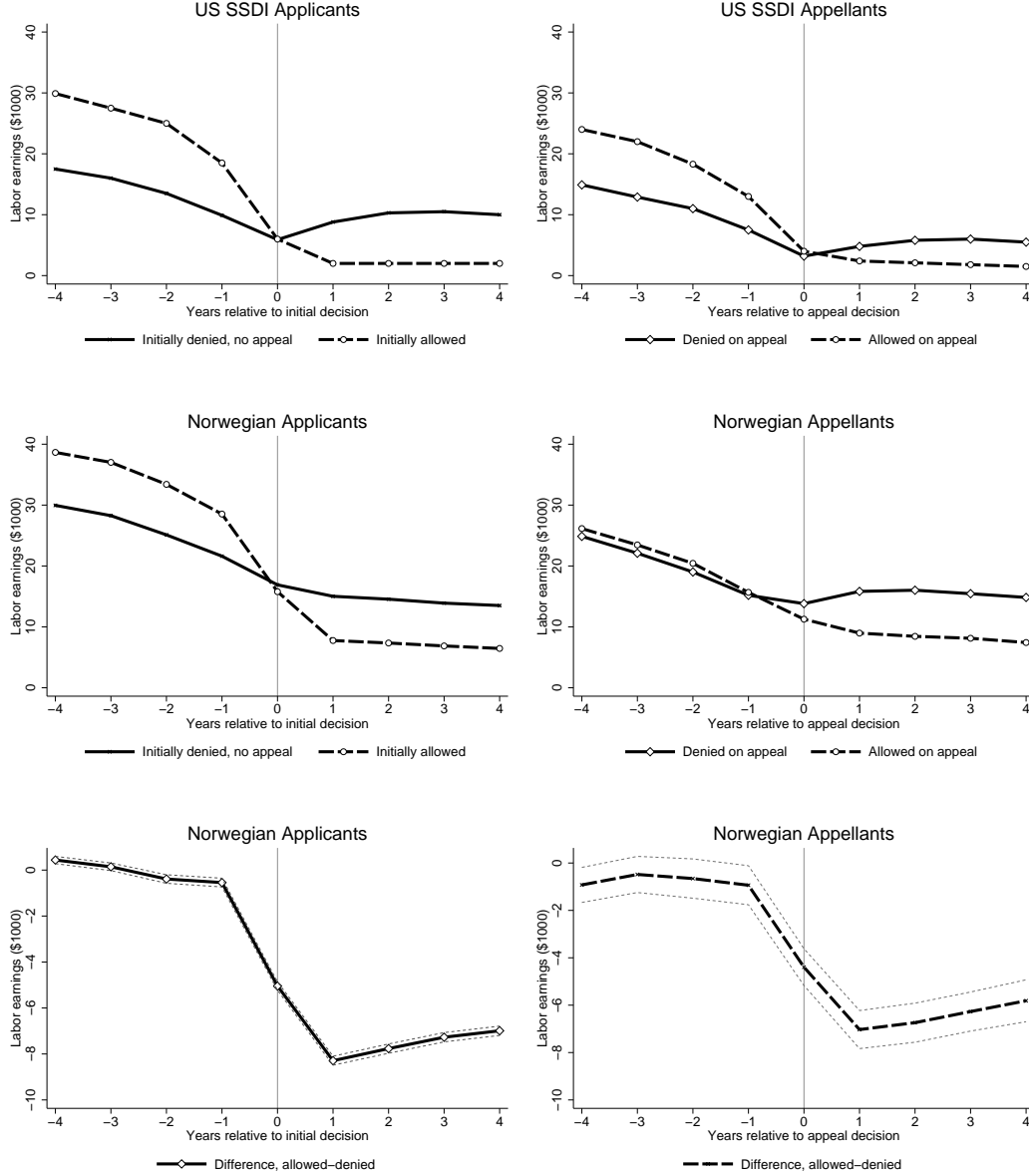
Despite these differences in prevalence, financial structure, and appeals behaviors, there are important similarities between the applicant, appellant and participant populations across the two countries. Almost 60 percent of DI recipients in both countries suffer from difficult-to-verify mental and musculoskeletal disorders (see Appendix Table A.1). And in both countries, appellants are younger, have lower prior earnings, and are more likely to suffer from difficult-to-verify disorders than are average DI applicants (see Appendix Table A.2). As a further comparison among the two programs, Figure 3 uses Norwegian and U.S. data (the latter from Maestas *et al.* 2013) to plot earnings trajectories of DI applicants and appellants in Norway and the U.S., before and after their DI determinations. We focus on years  $t - 4$  through years  $t + 4$  surrounding the initial DI

<sup>17</sup>Autor & Duggan (2006) estimate that Medicare benefits account for approximately 40 percent of the present value of an SSDI award.



determination (lefthand panel) and the year of the initial appeal decision (righthand panel).

Figure 3: **Earnings Trajectories of Allowed and Denied DI Applicants and Appellants**



*Notes:* The top four figures display changes in the levels of earnings for allowed (dashed line) and denied (solid line) DI applicants (left) and for DI appellants (right) in the nine years surrounding the initial DI determination and the initial outcome at appeal in the U.S. (top panel, sourced from [Maestas et al. 2013](#)), and for Norway (middle panel). For the Norwegian data, the applicant sample consists of all claims made during the period 1998-2003 by individuals who are at most 61 years of age. The appellant sample filed an appeal during the period 1998-2005 (see Section 3 for further details). The figures in the bottom panel plot the difference between denied and allowed applicants (left) and appellant labor earnings (right) over the same period, controlling flexibly for observable characteristics and lagged dependent variables (up to the year of the initial decision, after which they are fixed as the mean over the years prior to decision). The dashed lines in the bottom panel represent 90% confidence intervals, where each yearly difference is estimated separately with flexible controls for individual characteristics comprising application year dummies, dummy variables for county of residence, age at appeal, household size, gender, foreign born, marital status, children below age 18, educational attainment, and number of medical diagnoses, as well as polynomials of lagged averages of earnings and disposable income (not including observations after the decision). Nominal values are deflated to 2005 and represented in US dollars using the average exchange rate NOK/\$ = 6.

The patterns are quite similar across the two countries. DI applicants who are allowed at the initial determination have on average substantially higher prior earnings than those who are denied. This pattern likely reflects the fact that workers with high prior earnings who seek DI benefits typically face severe impairments that necessitate a sudden cessation of employment; conversely, applicants with low prior earnings may in part be compelled to seek DI due to a lack of employment opportunities rather than by severe health shocks per se. Similarly, earnings diverge immediately between allowed and denied appellants immediately following the appeal decision in both countries, and this gap is not closed over the subsequent four post-decision years.

## 4 Assessing the Instrument

We begin our presentation of results by providing evidence on the relevance and validity of the instrument.

### 4.1 Instrument Relevance

Figure 4 provides a graphical representation of the first stage of our IV model. In the background of this figure is a histogram for the density of judge leniency (controlling for fully interacted year and department dummies). The measure of judge leniency is the average judge allowance rate in all other cases a judge has handled (including the judge’s past and future cases that may fall entirely outside of our estimation sample). The mean of the leniency variable is 0.15 with a standard deviation of 0.05. The histogram reveals a wide spread in judge leniency, with a judge at the 90th percentile allowing approximately 18 percent of cases as compared to approximately 8 percent for a judge at the 10th percentile.

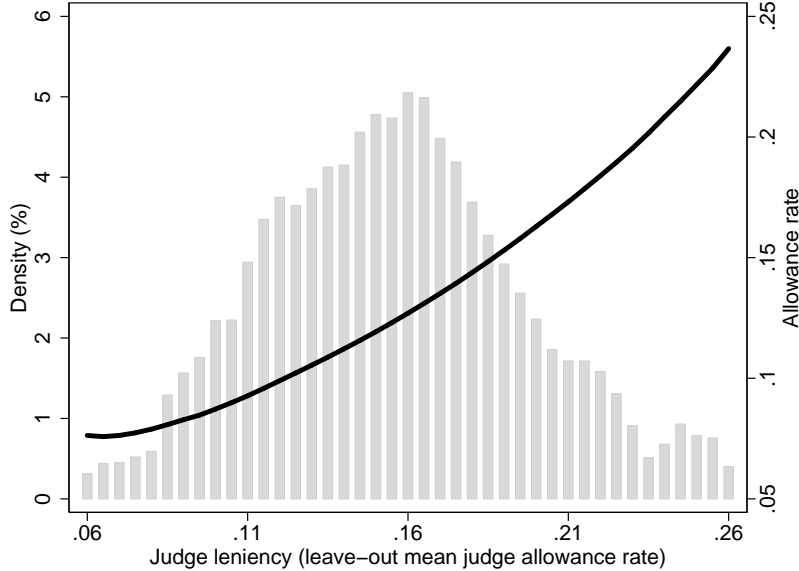
The solid line plotted in the figure’s foreground depicts the relationship between judge leniency and the appellant’s allowance rate (controlling for fully interacted year and department dummies). The graph is a flexible analog to the first stage equation (1), where we plot a local linear regression of individual allowance outcomes against judge leniency. The individual allowance rate is monotonically increasing in our leniency measure, and is close to linear. A 10 percentage point increase in the judge’s allowance rate in other cases is associated with an approximately 8 percentage point increase in the probability that an individual appellant’s case is allowed.

Table 3 presents estimates of our first equation for the relationship between judge leniency and DI allowance rates at appeal (1). We include fully interacted year and department dummies in Panel A but otherwise include no other controls. In each column, we regress a dummy variable for whether an individual is allowed DI at her first appeal on the judge leniency measure. The four columns of the table correspond to years one through four following appeal. These columns are identical except for the very modest impact of sample attrition (less than three percent over four years) stemming from death or emigration of appellants.<sup>18</sup> The point estimate of approximately

<sup>18</sup>Column 1 of Appendix Table A.5 documents that the instrument does not affect the probability that an appellant either dies or emigrates during the outcome period.

0.82 is essentially identical across columns, indicating that attrition exerts a negligible impact on the first stage relationship. All else equal, assignment to a judge with a 10 percentage point higher overall allowance rate increases the probability of receiving an allowance by 8.2 percentage points.

Figure 4: **Effect of Judge Leniency on DI Allowance**



*Notes:* This figure displays the effect of judge leniency on DI allowance, conditional on fully interacted year and department dummies. Baseline estimation sample consists of individuals who appeal an initially denied DI claim during the period 1994-2005 (see Section 3 for further details). There are 75 unique judges. The solid line plots a local linear regression of allowances on judge leniency. The histogram of judge leniency is shown in the background of both figures (top and bottom 1 percent excluded from the graph).

## 4.2 Instrument Validity

In order for judge leniency to be a valid instrument, appellants' assignment to judges must be uncorrelated with case characteristics. Table 1 provided strong empirical support for the claim that the DI system in Norway randomly assigns appeal judges within each department and year. Panel B of Table 3 provides a second confirmation of this fact: adding controls for appellant characteristics to the regression model has almost no effect on the point estimates, consistent with the fact that applicants are randomly assigned to judges.

Random assignment of cases to judges is sufficient for a causal interpretation of the reduced form impact of judge assignments on subsequent outcomes. To interpret the IV estimates as identifying the causal effect of DI allowances on appellant outcomes requires two further assumptions. The first is that judge leniency affects appellant outcomes of interest only through its impact on the appellant's allowance decision. This exclusion restriction appears particularly likely to hold in Norway, where all appeals are presented in writing, individuals (and their families) observe only judges' allowance or denial decisions, and there is no personal contact between judges and appellants. One

Table 3: **First Stage: Judge Leniency and DI Allowance**

	Years after decision			
	1	2	3	4
<b>Panel A. No covariates</b>				
Judge leniency	0.818*** (0.082)	0.819*** (0.084)	0.821*** (0.083)	0.828*** (0.083)
<b>Panel B. With individual covariates</b>				
Judge leniency	0.793*** (0.078)	0.792*** (0.081)	0.794*** (0.080)	0.800*** (0.080)
<b>Panel C. With judge characteristics</b>				
Judge leniency	0.808*** (0.074)	0.811*** (0.075)	0.815*** (0.074)	0.822*** (0.075)
Dependent mean	0.13	0.13	0.13	0.13
Observations	13,972	13,842	13,709	13,607

\*\*\*p<.01, \*\*p<.05, \*p<.10. Standard errors (in parentheses) are clustered at the judge level.

*Note:* This table reports the first stage coefficients of equation 1. The baseline estimation sample consists of individuals who appeal an initially denied DI claim during the period 1994-2005 (see Section 3 for further details). There are 75 unique judges. In Panel A, DI allowance is regressed on judge leniency and fully interacted year of appeal and department dummies. Panel B includes flexible controls for individual characteristics: fully interacted year and department dummies, and dummy variables for month of appeal, county of residence, age at appeal, household size, gender, foreign born, marital status, children below age 18, education, and number of medical diagnoses. Panel C also controls for judge leave-out-mean processing time.

potential violation of the exclusion restriction could occur if appeals processing times differed systematically with judge leniency and, moreover, exerted an independent effect on appellant outcomes (as explored in [Autor et al. 2014a](#)). To test this possibility, we calculated each judge’s average processing time based on the residual average processing time in his or her other cases. Panel C of Table 3 shows that the first stage estimates do not change appreciably when controlling for judge processing time.

The second assumption needed for a causal interpretation of the IV estimates is the monotonicity of the judge leniency instrument.<sup>19</sup> Monotonicity requires that, for each appellant, the probability of being allowed at appeal would be at least as high if assigned to a strict judge (low value of  $Z$ ) as if assigned to a lenient judge (high value of  $Z$ ). Since no individual can be assigned to two different judges at the same point in time, it is impossible to verify this assumption. There are, however, some testable implications which would allow us to reject the assumptions. The first testable implication we consider is that the first stage estimates should be non-negative for any subpopulation. If this were not the case, we would infer that the judges whom we estimate to be more lenient on average are stricter towards one subset of cases. When separately estimating the first stages based on the (pre-determined) observable characteristics of the individual, we find, reassuringly, that the estimates

<sup>19</sup>If the treatment effect of the disability determination were constant among appellants, the monotonicity assumption would be unnecessary. We do not find the constant treatment effects assumption plausible in this setting.

are consistently positive and sizable, in line with the monotonicity assumption (see Appendix Table A.3).

As a second check on this threat to validity, we directly examine whether judges who are stricter towards one subset of appellants (e.g., young appellants, those with mental disorders) are also relatively strict towards the complementary group of appellants (e.g., older appellants, those without mental disorders). We perform this test by again partitioning the data into the subpopulations that were used in the prior test, but in this case, we recalculate the leniency instrument for each subpopulation to be the judge’s leniency for cases outside of the subpopulation. For example, when assessing the effect of judge leniency on allowances for male appellants, we calculate judge leniency using only decisions in cases with female appellants. Column (2) of Appendix Table A.3 reports these results. All estimates using this redefined instrument are positive and statistically significant, consistent with the maintained assumption that leniency is a judge-specific attribute that characterizes judges’ decision-making across the panoply of cases that they are assigned.

## 5 Causal Effects of DI Allowances on Benefits Receipt, Employment and Earnings, and Total Transfer Income

### 5.1 Labor Earnings and DI Benefits

Table 4 reports 2SLS estimates of equations (1) and (2) with DI participation, DI benefits payments, and labor earnings as dependent variables in the second stage. As in Table 3, we separately estimate the effects of the initial appeal decision on outcomes for each of the four subsequent years. All specifications control for observable case characteristics and include a full set of year by department dummies.

The first two panels consider the impact of being allowed at the appeal on DI participation and benefits payments. Column 1 of panel A reports a 2SLS point estimate of 0.99, indicating that allowances induced by judge leniency increase DI receipt almost one-for-one in the first year following appeal.<sup>20</sup> Over the the first four years following appeal, the causal effect of being allowed at the appeal on subsequent DI receipt falls by approximately half, from 0.99 to 0.47, reflecting the fact that a substantial fraction of appellants who are initially denied DI benefits reapply and are ultimately allowed.<sup>21</sup> Panel B displays analogous estimates for DI benefit payments. Receiving a DI allowance at appeal leads to a large increase in benefit payments relative to the alternative outcome, with this increment equal to \$16,240 in the first year. This contrast declines over time due to successful DI reapplications, reaching \$8,167 in year four.

Panel C of Table 4 displays analogous estimates for annual labor earnings of DI appellants. DI allowances have sizable negative causal effects on labor earnings. Receiving a DI allowance on appeal reduces annual earnings by approximately \$6,800 in the first year after appeal, equal to

<sup>20</sup>Note that  $0.989 = 0.784/0.793$ , where 0.793 is the corresponding first stage coefficient from Table 3, panel B column 1.

<sup>21</sup>Although this pattern could also be consistent with successful appellants exiting the DI program in years two through four, such exits rarely occur.

approximately forty percent of the annual DI transfer benefit received. Distinct from the steeply declining causal effect of an initial allowance on DI participation and DI transfers, we find that the causal effect of an initial DI allowance on appellant earnings declines only modestly over the four years following the initial appeal outcome.

Table 4: **Effect of DI Allowance on Labor Earnings and DI Benefits of the Appellant**

	Years after decision			
	1	2	3	4
<b>Panel A. DI participation</b>				
Allowed DI	0.989*** (0.071)	0.727*** (0.102)	0.646*** (0.098)	0.470*** (0.084)
Dependent mean	0.305	0.432	0.519	0.577
<b>Panel B. DI benefits (\$1,000)</b>				
Allowed DI	16.240*** (1.539)	12.596*** (1.696)	10.203*** (1.660)	8.167*** (1.567)
Dependent mean	5.708	8.377	10.277	11.502
<b>Panel C. Earnings (\$1,000)</b>				
Allowed DI	-6.791** (2.765)	-5.946** (2.877)	-5.577* (2.952)	-5.660** (2.706)
Dependent mean	14.240	14.282	13.802	13.245
<b>Panel D. Total transfers (\$1,000)</b>				
Allowed DI	10.188*** (2.736)	8.807*** (2.749)	8.148*** (2.433)	6.429** (2.683)
Dependent mean	19.567	20.072	20.54	21.053
<b>Panel E. Non-DI transfers (\$1,000)</b>				
Allowed DI	-6.308* (3.273)	-3.744 (2.656)	-1.884 (2.062)	-1.611 (2.525)
Dependent mean	14.009	11.839	10.398	9.666
Observations	13,972	13,842	13,709	13,607

\*\*\*p<.01, \*\*p<.05, \*p<.10. Standard errors (in parentheses) are clustered at the judge level.

*Note:* This table reports instrumental variables estimates of the causal effect of receiving a DI allowance at the appeal stage on DI participation (Panel A), annual DI benefits (Panel B), and annual labor earnings (Panel C), annual total transfers inclusive of DI benefits (Panel D), and annual transfers excluding DI benefits (Panel E). The baseline sample consists of individuals who appeal an initially denied DI claim during the period 1994-2005 (see Section 3 for further details). There are 75 unique judges. All regressions include fully interacted year and department dummies, dummy variables for month of appeal, county of residence, age at appeal, household size, gender, foreign born, marital status, children below age 18, education, and a number of medical diagnoses. All control variables are measured prior to appeal.

The estimates in Table 4 can be interpreted as local average treatment effects (LATE) for appellants whose DI decisions are affected by the instrument (i.e., the compliers), meaning they could have received a different allowance decision had their case been assigned to a different judge. As shown in Imbens & Rubin (1997), we can decompose these LATEs to draw inference about what

compliers would have received in DI benefits and earned in labor income if denied or, alternatively, if allowed at appeal. These *potential outcomes* for compliers may be recovered by combining (i) the shares of never-takers and compliers to the instrument with (ii) the average *observed outcomes* of individuals who were not allowed with the most lenient or strictest judges (that is, those facing the highest and lowest values of the instrument).<sup>22</sup>

In Appendix Figure A.4, we implement these calculations to decompose the LATE into the potential outcome of appellant compliers if denied or, alternatively, if allowed. Relative to the regression estimates in Table 4, the figure plots *levels* of potential outcomes rather than simply depicting the LATE contrast between potential outcomes in the two states. Although many denied appellants reapply for, and eventually receive, DI benefits (Table 4), we find that labor earnings of compliers who are denied at appeal change little following denial. In contrast, labor earnings of compliers allowed at initial appeal fall steeply, particularly in the year of allowance and the year immediately thereafter. This pattern suggests that among the population of denied compliers, a small but non-negligible subset persists in employment following denial, while a larger group works minimally and pursues further appeals.

## 5.2 Benefit Substitution

As in many European countries, DI is one of several transfer programs available to Norwegians, and those whose DI claims are denied may potentially substitute towards these other programs.<sup>23</sup> Conversely, DI beneficiaries may also seek other transfer benefits.

Panels D and E of Table 4 reports 2SLS estimates of the causal effect of an initial DI allowance on total transfers (DI benefits plus all other cash transfers) and cash transfers excluding DI benefits. These estimates point to the importance of accounting for benefit substitution when considering the impacts of disability allowances on household incomes and public expenditure: the net impact of a DI allowance on total transfers received is about 20 to 40 smaller than its gross impact, with the largest discrepancies in the first two years following the initial appeal decision.

In Appendix Figure A.5, we decompose the LATE estimates for benefit receipt into potential outcomes for compliers when allowed and when denied. When compliers are awarded DI benefits, we see a sizable fall in their payments from non-DI transfer programs, indicating benefit substitution. Non-DI transfer payments change little in the year following appeal when compliers are denied DI, however. As many compliers who were denied at initial appeal successfully reapply for DI, their DI payments rise and non-DI transfers fall. These results reveal that DI and non-DI transfer programs serve as substitutes; hence, the net impact of a DI allowance or denial on appellant transfer payments is smaller than its gross impact. We next explore whether household labor supply provides an additional margin through which appellants and their spouses may buffer household income in

<sup>22</sup>Imbens & Rubin (1997) show how to derive the potential outcomes of compliers with and without treatment in settings with a binary instrument. Dahl *et al.* (2014) extend this to settings with multi-valued or continuous instruments. We follow the procedure of Dahl *et al.* (2014).

<sup>23</sup>The key transfers programs other than DI benefits are: social assistance (i.e., traditional welfare benefits), housing benefits, and vocational rehabilitation benefits.

the event of DI denial.

### 5.3 Spousal Responses

Table 5 extends our inquiry to consider spousal responses to DI allowance. We focus exclusively on married households in this analysis since our data do not allow us to determine whether non-married appellants are single or cohabiting.<sup>24</sup>

Panels A and B consider the impact of allowances versus denials at appeal on potential compensatory behaviors among appellants' spouses. The 2SLS estimates show that even with the extensive social insurance provided by the Norwegian government, the labor supply of appellants' spouses responds strongly to the outcomes of disability determinations. If an appellant is allowed instead of denied DI, spousal earnings fall by approximately \$5,000 in the first year after appeal, and by a further \$11,000 to \$12,000 in years two through four following denial (panel A). Panel B reveals that the income gains from the adjustments in spousal earnings are partly offset by the transfer system; up to 50 percent of the change in spousal labor earnings induced by a DI allowance or denial is effectively buffered by a countervailing change in transfer payments to the spouse.<sup>25</sup>

The estimated effects on spousal earnings are consistent with the possibility that either spouses reduce their labor supply if appellants are allowed, or that spouses increase their labor supply if appellants are denied (or potentially both). The latter would suggest that DI denials induce an added worker effect; the loss in worker earnings (due to disability) absent an offsetting gain in DI income spurs spouses to increase their labor supply. The former possibility would be consistent with DI allowance inducing a decline in labor supply among spouses due to leisure complementarities. We explore which of these interpretations is supported by the data by decomposing the causal effects estimates in Table 5 into potential outcomes of spouses of complier appellants if denied or, alternatively, if allowed at appeal. This decomposition, found in Appendix Figure A.6, indicates that the behavioral response found in Table 5 stems almost entirely from spousal responses to DI denials: spouses of denied appellants strongly increase earnings in the years following denial; conversely, spouses of allowed appellants exhibit little earnings adjustment.

By implication, DI denial induces a powerful added-worker effect among spouses. This result is somewhat surprising at first blush since households do not *lose* income when DI appeals are denied, they simply fail to gain it. However, recall from Figure 3 (center right panel) that average labor income of DI appellants declines by approximately 40 percent—from about \$25,000 to about \$15,000—over the four years prior to appeal, while close to 80 percent of applicants are awarded DI

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<sup>24</sup>The Norwegian census data allow us to observe cohabitation though unfortunately our analytic data do not. This Census data show that 59% of DI participants (applicants are not identified in the Census data) are married, 32% are single non-cohabitants, and only 9% are cohabitants. We test whether judge leniency causes endogenous selection into or out of marital status in Appendix Table A.5. Columns 2, 3 and 4 find no evidence that judge leniency affects the likelihood of a change in marital status (overall, from unmarried to married, or from married to unmarried).

<sup>25</sup>In Appendix Table A.4, we run the analysis of appellant labor income and total transfer payments from Table 4 separately for married appellants versus single and unmarried appellants. While the thinner sample size available for these estimations reduces precision, the point estimates suggest that DI allowances generate somewhat smaller reductions in labor earnings, as well as smaller increases in total (individual) transfer payments, among married appellants as compared to single and unmarried appellants.



benefits at their initial determination (Table 2). It thus appears plausible that, from the perspective of DI appellants and their spouses, the denial of benefits at the appeal stage constitutes a substantial adverse shock to expected permanent income, potentially spurring an added-worker response.

Table 5: **Effect of DI Allowance on Spousal Earnings and Transfer Payments**

	Years after decision			
	1	2	3	4
<b>Panel A.</b>	<b>Spouses' labor earnings (\$1,000)</b>			
Allowed DI	-4.856 (8.102)	-17.009** (8.552)	-16.096** (7.828)	-16.794** (8.039)
Dependent mean	40.965	39.565	38.777	37.487
<b>Panel B.</b>	<b>Spouses' total transfers (\$1,000)</b>			
Allowed DI	-0.027 (3.334)	5.823 (3.683)	5.957 (4.152)	8.020* (4.614)
Dependent mean	11.196	11.938	12.622	13.349
Observations	7,844	7,740	7,648	7,548

\*\*\*p<.01, \*\*p<.05, \*p<.10. Standard errors (in parentheses) are clustered at the judge level.

*Note:* This table reports the impact of DI allowance on earnings and total transfers of married appellants (panels A and B) and their spouses (panels C and D). Baseline estimation sample consists of married DI applicants who appeal an initially denied DI claim during the period 1994-2005 (see Section 3 for further details). There are 75 unique judges. All regressions include fully interacted year and department dummies, dummy variables for month of appeal, county of residence, age at appeal, household size, gender, foreign born, marital status, children below age 18, education, and number of medical diagnoses. All control variables are measured prior to appeal.

To drill down further on this finding, we have also explored the heterogeneity of the added-worker effect among households according to the spouse's sex, education, and prior earnings. While we find suggestive evidence that the labor supply of female spouses is more responsive to DI denial than is the labor supply of male spouses, these contrasts are not typically significant at conventional levels due to limited statistical power. Appellants may also differ in their own labor supply responses or benefit substitution following the DI determination. To examine heterogeneity in responses according to observable characteristics, we have also performed 2SLS estimation of equations (1) and (2) for each of the subsamples reported in Appendix Table A.3, where we split appellants according to diagnosis, gender, age, education, and the size of the household. These subsample estimates are insufficiently precise to draw clear inferences.<sup>26</sup>

## 6 Income Gains, Consumption Benefits and Fiscal Costs

In this section, we estimate the income and consumption gains that appellants obtain from DI allowances, and compare these gains with the fiscal costs that other taxpayers bear.

<sup>26</sup>The results are available from the authors.

Table 6: Effect of DI Allowance on Household Income, Fiscal Costs and Consumption

	Years after decision				Average
	1	2	3	4	0-4
<b>Panel A.</b>	<b>Full sample</b>				
	<b>A.1: Household income (\$1,000)</b>				
Allowed DI	1.282 (1.998)	5.578** (2.249)	2.671 (2.127)	3.198 (2.008)	3.208* (1.649)
p-value for $H_0 : \beta_t = 0$	0.5212	0.0131	0.2091	0.1113	0.0518
p-value for $H_0 : \beta_t = \Delta \text{DI benefit}_{t1}$	0.0000	0.0308	0.0003	0.0003	0.0000
Dependent mean	26.248	26.773	27.144	27.651	26.541
	<b>A.2: Fiscal costs (\$1,000)</b>				
Allowed DI	3.627 (2.286)	8.914*** (2.057)	8.525*** (2.213)	7.010*** (2.395)	6.859*** (1.756)
p-value for $H_0 : \beta_t = 0$	0.1126	0.0000	0.0001	0.0034	0.0001
p-value for $H_0 : \beta_t = \Delta \text{HH Income}_t$	0.3049	0.1049	0.0082	0.1115	0.0376
Dependent mean	7.017	7.73	8.283	9.036	7.671
Observations	13,972	13,842	13,709	13,607	14,092
<b>Panel B.</b>	<b>Restricted sample</b>				
	<b>B.1: Household income (\$1,000)</b>				
Allowed DI	2.764 (2.293)	5.184** (2.063)	2.352 (2.693)	4.951** (2.386)	4.066** (2.032)
p-value for $H_0 : \beta_t = 0$	0.228	0.012	0.3825	0.038	0.0453
p-value for $H_0 : \beta_t = \Delta \text{DI benefit}_{t1}$	0.0016	0.0197	0.0045	0.0345	0.0035
Dependent mean	25.318	25.86	26.222	26.768	25.634
	<b>B.2: Household consumption (\$1,000)</b>				
Allowed DI	2.484 (5.125)	5.313* (2.730)	1.896 (3.803)	4.728 (3.967)	4.705* (2.831)
p-value for $H_0 : \beta_t = 0$	0.6278	0.0517	0.6181	0.2333	0.0965
p-value for $H_0 : \beta_t = \Delta \text{HH Income}_t$	0.9565	0.9623	0.9044	0.9552	0.8214
Dependent mean	26.000	26.859	27.698	28.325	26.543
Observations	10,827	10,772	10,655	10,523	10,945

\*\*\*p<.01, \*\*p<.05, \*p<.10. Standard errors (in parentheses) are clustered at the judge level.

*Notes:* This table reports the impact of DI allowance on household disposable income and fiscal costs for the baseline sample (panel A) and household disposable income and consumption for the restricted sample (Panel B). All outcomes are reported per capital in \$1,000. Baseline estimation sample consists of DI applicants who appeal an initially denied DI claim during the period 1994-2005 (see Section 3 for further details). The restricted sample excludes households with housing transactions and large financial transactions. There are 75 unique judges. All regressions include fully interacted year and department dummies, dummy variables for month of appeal, county of residence, age at appeal, household size, gender, foreign born, marital status, children below age 18, education, and number of medical diagnoses. All control variables are measured prior to appeal. P-values for  $H_0 : \beta_t = \Delta \text{DI benefit}_{t1}$  in panels A.1 and B.1 correspond to tests of whether the effect of a successful DI appeal (in year  $t_0$ ) on initial DI income (in year  $t_1$ ) is equal to the effect of the successful appeal after on HH consumption in outcome years  $t \in \{1, 2, 3, 4\}$ . P-values for  $H_0 : \beta_t = \Delta \text{HH Income}_t$  in panels A.2 and B.2 correspond to tests of whether the effects of a successful DI appeal in year  $t_0$  on household income and household consumption are equivalent in outcome years  $t \in \{1, 2, 3, 4\}$ .

## 6.1 Impacts of DI Allowances on Income, Consumption, and Fiscal Cost

Table 6 reports 2SLS estimates of the impact of allowances versus denials at appeal on (per household member) disposable income and consumption as well as fiscal costs, inclusive of DI transfer payments, benefit substitution to or from other transfer programs, and induced changes in tax receipts. Panel A uses the full sample of appellants, while Panel B restricts the sample for whom we have detailed measures of household consumption expenditure.<sup>27</sup>

Despite both countervailing behavioral responses and countervailing transfer program interactions documented above, DI allowances yield meaningful income gains among individuals (and their families) at the margin of program entry. Specifically, panel A.1 of Table 6 indicates that DI allowances raise income available for consumption (per household member) by approximately \$3,200 per annum on average. This effect is statistically significant at approximately the five percent level when pooling outcomes over years zero through four following appeal. At the same time, we readily reject the null that the causal effect of a DI allowance on household income in each of the four post-appeal years is as large as its effect on initial DI benefits payments, highlighting how accounting for the total effect of DI allowances on household labor supply and net payments across all public transfer programs alters our picture of the income gains of disability receipt.<sup>28</sup>

Since DI allowances significantly increase disposable income among appellants while reducing household labor supply and therefore tax revenue, we can infer that DI allowances have positive net fiscal costs. Panel A.2 of Table 6 provides a direct accounting of these costs by summing the impact of DI allowances on DI transfer payments, benefit substitution to or from other transfer programs, and induced changes in tax receipts.<sup>29</sup> To facilitate comparison, we divide the fiscal costs for each DI allowance by the size of the appellant’s household so that both income gains and fiscal costs are scaled on a per household member basis. Our point estimates suggest that DI allowances granted on appeal increases net government spending (per household member) by nearly \$7,000 per annum, on average. A comparison of the point estimates in Panels A.1 and A.2. suggests that DI allowance raise household income by less than 50 cents per dollar of net government expenditure. As shown in the final column of panel A.2, we reject the hypothesis that the rise in disposable income per household member is as large as the increase in fiscal costs per household member over the pooled four-year outcome period.<sup>30</sup>

<sup>27</sup>There is no evidence of a significant effect of judge leniency on the likelihood of being excluded from the full sample, as shown in the fourth column of A.5, indicating that our estimates based on the restricted sample are unlikely to be biased by endogenous compositional changes. For details on the measurement of consumption expenditure, see Eika *et al.* (2017).

<sup>28</sup>Specifically, the row labeled  $H_0 : \beta_t = \Delta \text{DI benefit}_{t1}$  reports p-values for the null hypothesis that the the causal effect of a successful DI appeal on household consumption in outcome years  $t \in \{1, 2, 3, 4\}$  is equal to its immediate effect on DI benefits payments (panel B, column 1 of Table 4). We report two-tailed tests of equality. (P-values for one-tailed tests of  $\Delta DI > \Delta HH$  consumption are equal to one-half the p-values for two-tailed tests.)

<sup>29</sup>Fiscal costs are equal to transfer income minus taxes, while household income is equal to transfer income minus taxes plus labor income and other market income (e.g. capital income). Since capital income plays a small role in the DI appellant sample, most of the offset is due to labor supply impacts.

<sup>30</sup>This test is reported in the bottom row of the panel and denoted p-value for  $H_0 : \beta_t = \Delta HH \text{ Income}_t$ , indicating that we are comparing the net fiscal cost to the induced rise in household income in the contemporaneous year (or, in the final column, for the pooled four-year period).

Panels B.1 and B.2 present 2SLS estimates of the effects of DI allowance on disposable income and consumption expenditure per household member for the restricted sample. We estimate that DI allowances increase both disposable income and household consumption by roughly 16-18 percent relative to their sample means, with both effects (on average outcomes over years zero through four) significantly different from zero at the 10 percent. Notably, the point estimates for household income are broadly similar to those for consumption, suggesting that DI allowances have relatively little impact on household savings in the sample of appellants. Consistent with this observation, our data do not reject the null hypothesis that the consumption gains induced by DI allowances are equal on average to the income gains.

## 6.2 Heterogeneity in Impacts of DI Allowances by Marital Status

Given the evidence of strong spousal labor supply responses, we expect the economic consequences of DI determinations to differ by household structure, and in particular between married and non-married (i.e., single or cohabiting) households. Here, we explore the impacts of DI allowances on (per household member) income, consumption, and fiscal costs separately for married and non-married appellants in Tables 7 and 8. We find sharp contrasts between these two groups.<sup>31</sup>

Focusing first on non-married appellants, Table 7 documents that DI allowances generate large positive impacts on disposable income and consumption for this subpopulation, which comprises just over 40 percent of all appellants (Table 2). The estimates in panel A indicate that a DI allowance raises the household incomes of unmarried appellants by approximately \$6,600 per annum in years zero through four following the award, and it generates net fiscal expenditures of approximately \$12,300 per annum.<sup>32</sup> Thus, among non-married appellants, about 55 cents of each dollar of public expenditure induced by a successful appeal accrues to household incomes, though available precision does reject the hypothesis that the effects on household incomes and fiscal expenditures are equivalent. Panel B focuses on the subset of non-married appellants for whom we have detailed consumption data. DI awards increase both income and consumption in this subpopulation, raising them by approximately \$9,500 and \$10,400 respectively. These are very large increments to both outcomes, equivalent to 35 to 40 percent of their baseline values; P-values reported in the bottom of indicate that we cannot reject the hypothesis that household incomes and household consumption rise one-for-one.

Table 8 reports analogous estimates for married appellants. Accounting for the effect of DI allowances on household labor supply and net payments across all public transfer programs substantially alters our picture of the income and consumption gains disability receipt for married

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<sup>31</sup>While many of the year-by-year estimates in Tables 7 and 8 are too imprecise to draw precise inferences (e.g. about changes in effect sizes over time), we can reject equality in the average effects of household income (per household member) on singles and unmarried versus married at the 1 percent level.

<sup>32</sup>As above, we divide impacts on fiscal costs by household size so that both income gains and fiscal costs are scaled on a per household member basis.

Table 7: Effect of DI Allowance on Household Income, Fiscal Costs and Consumption for Non-married Appellants

	Years after decision				Average
	1	2	3	4	0-4
<b>Panel A.</b>	<b>Full sample</b>				
	<b>A.1: Household income (\$1,000)</b>				
Allowed DI	4.637 (3.380)	8.669*** (3.130)	7.710** (3.604)	6.296 (4.127)	6.577** (2.803)
p-value for $H_0 : \beta_t = 0$	0.1701	0.0056	0.0324	0.1271	0.019
p-value for $H_0 : \beta_t = \Delta \text{DI benefit}_{t1}$	0.0000	0.0000	0.0001	0.0002	0.0000
Dependent mean	25.346	25.745	26.164	26.731	25.549
	<b>A.2: Fiscal costs (\$1,000)</b>				
Allowed DI	9.027* (5.162)	13.168*** (3.354)	13.912*** (4.259)	9.255** (4.228)	12.352*** (4.045)
p-value for $H_0 : \beta_t = 0$	0.0803	0.0001	0.0011	0.0286	0.0023
p-value for $H_0 : \beta_t = \Delta \text{HH Income}_t$	0.395	0.1799	0.1453	0.4841	0.1534
Dependent mean	12.893	13.302	13.66	14.137	13.108
Observations	6,128	6,102	6,061	6,059	6,147
<b>Panel B.</b>	<b>Restricted sample with data on consumption expenditure</b>				
	<b>B.1: Household income (\$1,000)</b>				
Allowed DI	6.924** (3.330)	9.451*** (3.052)	7.450* (4.376)	10.438** (4.394)	9.443*** (3.160)
p-value for $H_0 : \beta_t = 0$	0.0376	0.002	0.0887	0.0175	0.0028
p-value for $H_0 : \beta_t = \Delta \text{DI benefit}_{t1}$	0.0000	0.0001	0.0017	0.0142	0.0002
Dependent mean	24.669	25.214	25.695	26.203	24.979
	<b>B.2: Household consumption (\$1,000)</b>				
Allowed DI	6.716 (6.664)	9.793** (4.203)	9.164 (7.348)	13.954** (6.426)	10.366** (5.151)
p-value for $H_0 : \beta_t = 0$	0.3136	0.0198	0.2124	0.0299	0.0442
p-value for $H_0 : \beta_t = \Delta \text{HH Income}_t$	0.9751	0.9351	0.8156	0.5843	0.8578
Dependent mean	25.907	26.178	27.76	28.099	26.135
Observations	4,979	4,946	4,891	4,880	5,001

\*\*\*p<.01, \*\*p<.05, \*p<.10. Standard errors (in parentheses) are clustered at the judge level.

Notes: This table reports the impact of DI allowance on household disposable income and fiscal costs for the full set of unmarried appellants (panel A) and the subset for whom we have detailed consumption data (panel B). All outcomes are reported per capital in \$1,000. Baseline estimation sample consists of unmarried DI applicants who appeal an initially denied DI claim during the period 1994-2005 (see Section 3 for further details). The restricted sample excludes households with housing transactions and large financial transactions. There are 75 unique judges. All regressions include fully interacted year and department dummies, dummy variables for month of appeal, county of residence, age at appeal, household size, gender, foreign born, marital status, children below age 18, education, and number of medical diagnoses. All control variables are measured prior to appeal. P-values for  $H_0 : \beta_t = \Delta \text{DI benefit}_{t1}$  in panels A.1 and B.1 correspond to tests of whether the effect of a successful DI appeal (in year  $t0$ ) on initial DI income (in year  $t1$ ) is equal to the causal effect of the successful appeal on HH income in outcome years  $t \in \{1, 2, 3, 4\}$ . P-values for  $H_0 : \beta_t = \Delta \text{HH Income}_t$  in panels A.2 and B.2 correspond to tests of whether the causal effects of a successful DI appeal in year  $t0$  on fiscal costs (A.2) and household consumption (B.2) in outcome years  $t \in \{1, 2, 3, 4\}$  are equal to the corresponding effect on household income.

Table 8: **Effect of DI Allowance on Household Income, Fiscal Costs and Consumption for Married Appellants**

	Years after decision				Average
	1	2	3	4	0-4
<b>Panel A.</b>	<b>Full sample</b>				
	<b>A.1: Household income (\$1,000)</b>				
Allowed DI	-1.705 (2.361)	2.007 (2.858)	-2.031 (2.544)	-0.710 (2.882)	-1.230 (1.918)
p-value for $H_0 : \beta_t = 0$	0.4702	0.4824	0.4246	0.8054	0.5214
p-value for $H_0 : \beta_t = \Delta \text{DI benefit}_{t1}$	0.0248	0.5785	0.027	0.1352	0.0119
Dependent mean	26.953	27.582	27.92	28.389	27.215
	<b>A.2: Fiscal costs (\$1,000)</b>				
Allowed DI	1.053 (2.890)	6.002* (3.358)	5.826* (3.300)	6.420** (2.836)	3.975* (2.365)
p-value for $H_0 : \beta_t = 0$	0.7157	0.0739	0.0774	0.0236	0.0928
p-value for $H_0 : \beta_t = \Delta \text{HH Income}_t$	0.3401	0.2342	0.0173	0.0119	0.0277
Dependent mean	2.426	3.337	4.022	4.941	3.256
Observations	7,844	7,740	7,648	7,548	7,945
<b>Panel B.</b>	<b>Restricted sample</b>				
	<b>B.1: Household income (\$1,000)</b>				
Allowed DI	-1.300 (2.733)	0.078 (2.781)	-2.265 (2.920)	-2.606 (3.086)	-2.119 (2.073)
p-value for $H_0 : \beta_t = 0$	0.6343	0.9777	0.4379	0.3984	0.3068
p-value for $H_0 : \beta_t = \Delta \text{DI benefit}_{t1}$	0.2038	0.4512	0.1285	0.1215	0.0384
Dependent mean	25.872	26.408	26.669	27.257	26.055
	<b>B.2: Household consumption (\$1,000)</b>				
Allowed DI	-0.548 (6.552)	0.215 (4.107)	-5.212 (4.987)	-5.771 (4.416)	-0.165 (3.229)
p-value for $H_0 : \beta_t = 0$	0.9333	0.9582	0.296	0.1913	0.9593
p-value for $H_0 : \beta_t = \Delta \text{HH Income}_t$	0.9086	0.9733	0.5546	0.4736	0.5451
Dependent mean	26.079	27.438	27.645	28.520	26.680
Observations	5,848	5,826	5,764	5,643	5,944

\*\*\*p<.01, \*\*p<.05, \*p<.10. Standard errors (in parentheses) are clustered at the judge level.

*Note:* This table reports the impact of DI allowance on household disposable income and fiscal costs among the full set of married appellants (panel A) and the subset for whom we have detailed consumption data (panel B). Outcomes are reported per capital in \$1,000. Baseline estimation sample consists of married DI applicants who appeal an initially denied DI claim during the period 1994-2005 (see Section 3 for further details). The restricted sample excludes households with housing transactions and large financial transactions. There are 75 unique judges. All regressions include fully interacted year and department dummies, dummy variables for month of appeal, county of residence, age at appeal, household size, gender, foreign born, marital status, children below age 18, education, and a number of medical diagnoses. All control variables are measured prior to appeal. P-values for  $H_0 : \beta_t = \Delta \text{DI benefit}_{t1}$  in panels A.1 and B.1 correspond to tests of whether the effect of a successful DI appeal (in year  $t_0$ ) on initial DI income (in year  $t_1$ ) is equal to the effect of the successful appeal on HH income in outcome years  $t \in \{1, 2, 3, 4\}$ . P-values for  $H_0 : \beta_t = \Delta \text{HH Income}_t$  in panels A.2 and B.2 correspond to tests of whether the causal effects of a successful DI appeal in year  $t_0$  on fiscal costs (A.2) and household consumption (B.2) in outcome years  $t \in \{1, 2, 3, 4\}$  are equal to the corresponding effect on household income.

beneficiaries. As shown in Table 8, DI allowances do not increase the household income or consumption of married applicants on average; instead, household labor supply and benefit substitution are estimated to fully offset the DI benefit payments.<sup>33</sup> Nevertheless, DI allowances made to married appellants incur meaningful fiscal costs through increased cash transfers and reduced payroll tax revenues. We estimate that each DI allowance to a married appellant generates a fiscal burden of approximately \$4,000 annually in the four years following appeal.

## 7 Deriving Welfare Implications using a Structural Model

We now apply the data and findings above to estimate a dynamic model of household behavior that translates employment, savings and reapplication decisions of appellants and their spouses into revealed preferences for leisure and consumption. We use this estimated model to compute the welfare benefits of DI receipt—by which we mean the cash equivalent value of DI allowance at appeal—and to perform counterfactual simulations that allow us to infer the extent to which spousal labor supply and other mechanisms attenuate the loss in household welfare from DI denial at appeal. Our goal is limited to understanding the post-appeal labor supply, savings, and DI reapplication decisions of appellants and their spouses, taking as given their characteristics and economic circumstances at the time of appeal (such as savings, disability severity, education, and past labor market experience). As a consequence, our model cannot speak to the full insurance value of the DI system to workers prior to disability onset; only the cash equivalent value of DI allowances at the time of appeal.

### 7.1 Description of model and estimation procedure

#### Preference specification

We consider a unitary model of the household with non-separable preferences between consumption and leisure.<sup>34</sup> The household utility function depends on consumption (per household member)  $C$ , indicators for employment  $P_A \in \{0, 1\}$  and reapplication  $R \in \{0, 1\}$  for the appellant, and an employment indicator  $P_S \in \{0, 1\}$  for the spouse (if present).<sup>35</sup> As in [Low & Pistaferri \(2015\)](#), we allow for preference heterogeneity according to disability severity. Following their paper, we construct an index of disability severity with three levels:  $H = 0$  indicates low-severity,  $H = 1$  indicates mid-severity, and  $H = 2$  indicates high-severity. To construct this index, we estimate the probability of being initially allowed at appeal as a function of the diagnosis codes. We then assign

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<sup>33</sup>In fact, DI allowances appear to weakly lower household income and consumption, plausibly reflecting discrete choices in labor supply by denied appellants' spouses (e.g., due to fixed costs associated with working).

<sup>34</sup>This flexible specification of preferences accommodates non-market production and work related expenses (see e.g. [Aguiar & Hurst, 2013](#)). For evidence on non-separability between labor supply and consumption, we refer to [Browning & Meghir \(1991\)](#) and [Blundell \*et al.\* \(1994\)](#).

<sup>35</sup>As in [Maestas \*et al.\* \(2013\)](#) and [Kostol & Mogstad \(2014\)](#), employment is an indicator variable that is equal to one if annual earnings exceed the annual substantial gainful activity threshold, set annually by the Norwegian Social Security Administration (at approximately USD 10,000 per year). We are unable to measure labor supply at the intensive margin because we lack reliable data on working hours.



appellants to three groups of equal size based on the predicted probabilities.

We follow [Low & Pistaferri \(2015\)](#) in the parametric specification of preferences. At time  $t$ , the instantaneous utility function of unmarried households is:

$$U_1(C_t, P_{A,t}, R_t; H) = \frac{1}{1 - \mu_1} (C_t \exp \{-P_{A,t} \phi_{1,A,H}\} - R_t \exp \omega_1)^{1 - \mu_1}, \quad (3)$$

where  $\phi_{1,A,H}$ ,  $\mu_1$ , and  $\omega_1$  are the utility parameters. The bracketed expression reflects how the marginal utility of income changes with working; it is normalized to zero if the appellant is not working. For married couples, we use a similar parametric specification of preferences:

$$U_2(C_t, P_{A,t}, P_{S,t}, R_t; H) = \frac{1}{1 - \mu_2} (C_t \exp \{-P_{A,t} \phi_{2,A,H} - P_{S,t} \phi_{2,S}\} - R_t \exp \omega_2)^{1 - \mu_2}, \quad (4)$$

where  $\phi_{2,A,H}$ ,  $\phi_{2,S}$ ,  $\mu_2$ , and  $\omega_2$  are the utility parameters, and the bracketed expression is normalized to zero if both spouses are not working.

## Earnings Process

Like [Low & Pistaferri \(2015\)](#), we specify the earnings process of appellants to depend on disability severity  $H$  and other observable characteristics  $Q$ . The vector  $Q$  includes a constant, indicators for high school drop out, high school completion, and college completion; and an indicator for young age, where we follow [Low & Pistaferri \(2015\)](#) in defining young age as less than 45 years old (at time of appeal). The observable characteristics are measured prior to the appeal decision, capturing heterogeneity in experience, skills, and abilities that may affect potential earnings. In addition, we include a fixed effect  $f_A$  in earnings estimated from pre-application earnings data, in an attempt to allow for heterogeneity in latent ability as measured prior to the application.

We specify the annual earnings process of the appellant to be:

$$\log W_{A,t} = Q' \kappa_{M,A} + \sum_{j=0}^2 \psi_{M,A,j} H_j + \tau_{A,t} + a_{M,A}(f_A), \quad (5)$$

where  $M = 1$  denotes single households and  $M = 2$  denotes married households,  $a_{M,A}$  is a (third order) polynomial in the pre-application fixed effect,  $H_j = \mathbf{1}\{H = j\}$  is an indicator for disability severity  $j = \{0, 1, 2\}$ , and the stochastic component  $\tau_{A,t}$  is specified as a random walk:

$$\tau_{A,t} = \tau_{A,t-1} + \nu_{A,t}, \quad \nu_{A,t} \sim \mathcal{N}(0, \sigma_{M,A}^2).$$

Similarly, the annual earnings process of the spouse (if present) is specified as:



$$\log W_{S,t} = Q' \kappa_S + \sum_{j=0}^2 \psi_{S,j} H_j + \tau_{S,t} + a_S(f_S), \quad (6)$$

where:

$$\tau_{S,t} = \tau_{S,t-1} + \nu_{S,t}, \quad \nu_{S,t} \sim \mathcal{N}(0, \sigma_S^2).$$

As discussed in [Low & Pistaferri \(2015\)](#), a potential concern with the earnings processes is that earnings are not observed for those who do not work and the decision to work depends on the earnings offer. While the observable characteristics and the pre-application fixed effects may help address this concern over selection bias, we also perform a robustness check. Following [Low & Pistaferri \(2015\)](#), we perform a selection correction of the earnings processes. This is done by first estimating a probit regression of employment on  $H$ ,  $Q$ , and  $a_{M,A}$ . Under the assumption that the error terms of the employment equations and the earnings processes are jointly normal, we can then selection correct the earnings processes by including the inverse Mills ratios in the estimation of the earnings processes. Appendix Table [A.6](#) and [A.8](#) present parameter estimates of the earnings processes and the corresponding labor supply elasticities with and without the selection correction. As shown in Appendix Table [A.9](#), neither the estimated cash equivalent value of DI allowance nor the results from the counterfactuals are materially affected by inclusion of the selection terms in the earnings processes.

## Disposable Income

As in [Heathcote \*et al.\* \(2014\)](#) and [Blundell \*et al.\* \(2016b\)](#), we approximate the tax-transfer system by specifying flexible functions mapping household earnings into disposable household income (earnings plus transfers minus taxes). Below, we show that the chosen functions approximate well the effective tax rates implicit in the complex Norwegian tax-transfer system.

For unmarried households that supply labor, we use the following specification of the relationship between disposable income  $I$  and appellant earnings  $E_{A,t}$  in year  $t$ :

$$I_t = (1 - \Lambda_{1,D_t,K,t}) (E_{A,t})^{(1-\Psi_{1,D_t,K,t})} \quad (7)$$

where  $D \in \{0, 1\}$  denotes current DI receipt and  $K \in \{0, 1\}$  denotes the presence of a dependent in the household. Similarly, the specification for married households that supply labor is :

$$I_t = (1 - \Lambda_{2,D_t,K,t}) (E_{A,t} + E_{S,t})^{(1-\Psi_{2,D_t,K,t})} \quad (8)$$

where  $E_{S,t}$  denotes annual spousal earnings in year  $t$ . For households that do not supply labor, disposable income is only a function of transfer payments as captured by the specification:

$$I_t = \Phi_{M,D_t,K,t} \quad (9)$$

The parameters  $\Lambda$ ,  $\Psi$  and  $\Phi$  are allowed to vary over time  $t$  and by marital status  $M$ , DI receipt

$D_t$ , and presence of dependents  $K$ . In a proportional tax-transfer system,  $\Psi = 0$  and  $\Lambda$  is the proportional effective tax rate. By contrast, if  $0 < \Psi < 1$ , then the marginal effective tax rate is increasing in earnings.

### Process for Approval of Reapplication

Like [Low & Pistaferri \(2015\)](#), we model DI approval upon reapplication for single and unmarried households as:

$$D_{t+1} = D_t + R_t (1 - D_t) \pi_{M,H,t}. \quad (10)$$

where  $\pi_{M,H,t} \in [0, 1]$  is the probability of DI approval upon reapplication ( $R = 1$ ), which we allow to vary with time  $t$ , marital status  $M$ , and disability severity  $H$ .

### The Household's Problem

Letting  $S_t$  denote savings,  $\zeta$  denote the intertemporal discount factor, and  $O_1$  denote household heterogeneity, the dynamic optimization problem of unmarried households is:

$$V_{1,t}(D_t, \tau_{A,t}, S_t; O_1) = \max_{C_t, P_{A,t}, R_t, S_{t+1}} U_1(C_t, P_{A,t}, R_t; H) + \zeta \mathbb{E} V_{1,t+1}(\cdot, \cdot, S_{t+1}; O_1),$$

where  $O_1 = (H, K, Q, f_A)$ . The expectation is taken jointly across the distribution of  $D_{t+1}$  and  $\tau_{A,t+1}$ , and the choices are subject to the exogenous earnings process, the exogenous DI approval process, the borrowing constraint  $S_{t+1} \geq 0$ , the tax-transfer system, and the intertemporal budget constraint:

$$S_{t+1} = (1 + r)(I_t + S_t - C_t) \quad (11)$$

where  $r$  is the real interest rate. To close the model, we follow [Low & Pistaferri \(2015\)](#) in assuming exogenous and fully anticipated retirement at age 62. Retirement is characterized by consuming out of savings and retirement benefits  $b_1$ . At the end of retirement, death occurs exogenously and is fully anticipated. The terminal condition of zero savings must be satisfied upon death.

The dynamic optimization problem of married households is analogously,

$$V_{2,t}(D_t, \tau_{A,t}, \tau_{S,t}, S_t; O_2) = \max_{C_t, P_{A,t}, P_{S,t}, R_t, S_{t+1}} U_2(C_t, P_{A,t}, P_{S,t}, R_t; H) + \zeta \mathbb{E} V_{2,t+1}(\cdot, \cdot, \cdot, S_{t+1}; O_2),$$

where  $O_2 = (H, K, Q, f_A, f_S)$ . The expectation is taken jointly across the distribution of  $D_{t+1}$ ,  $\tau_{A,t+1}$ , and  $\tau_{S,t+1}$ , and the choices are subject to the exogenous earnings process of each spouse, the exogenous DI approval process, the borrowing constraint, the tax-transfer system, and the intertemporal budget constraint. The model for married households is closed the same way as for single and unmarried households, with retirement benefits denoted by  $b_2$ .

## Estimation and identification

To take the model to the data, we adopt a three-step procedure. In the first step, we estimate the parameters of the earnings process similarly to [Low & Pistaferri \(2015\)](#), as described above. In the second step, certain model parameters are set externally. We follow [Low & Pistaferri \(2015\)](#) in fixing the relative risk aversion coefficient to  $\mu_M = 1.5$ , the intertemporal discount rate to  $\zeta = 0.9756$ , and the interest rate to  $r = 0.016$ . We specify annual retirement benefits as the minimum pension benefits provided under Norwegian law.

In the third step, we use the method of simulated moments to jointly estimate all remaining parameters, conditional on the estimated earnings parameters from the first step and the externally set parameters from the second step. For any given candidate parameters, we simulate the model recursively beginning with the terminal condition and ending with decisions made in the period after initial DI allowance.<sup>36</sup> The output of the model simulation is the set of optimal choices that would be made by the household given the candidate parameters. We choose the optimal parameters as those candidate parameters that minimize a weighted distance metric between observed data moments (discussed below) and corresponding moments simulated from the model.

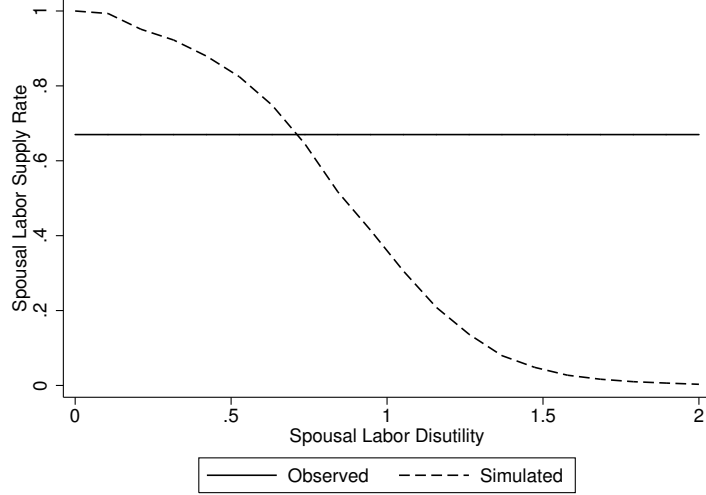
All estimated model parameters except for the disutility parameters are identified directly from sample data moments given the distributional assumptions about the error terms. The parameter  $\pi_{M,H,t}$  in Equation (10) is the DI approval rate upon reapplication for households with marital status  $M$  and disability severity  $H$  at time  $t$ , identified from the observed DI approval rate in the sample conditional on  $(M, H, t)$ . Equation (8) can be expressed equivalently as the linear regression of log disposable income on log earnings of the household, where the terms  $\log(1 - \Lambda_{M,D_t,K,t})$  and  $1 - \Psi_{M,D_t,K,t}$  are the intercept and slope, respectively. Conditional on  $(M, D_t, K, t)$ , the parameters are identified from the mean of log disposable income and the mean of the expectation of log disposable income conditional on log earnings among households in which neither the appellant or the spouse (if present) is working. Analogously,  $\Phi_{M,D_t,K,t}$  is identified as mean transfers to unemployed households, conditional on  $(M, D_t, K, t)$ . Although the tax-transfer parameters could be estimated in a first step like the earnings process, we estimate these parameters simultaneously with the other model parameters in order to improve efficiency.

While the mapping between model parameters and sample moments is less direct for the disutility parameters, there are data moments that intuitively provide identifying information. For instance,

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<sup>36</sup>Note that the value function does not have a closed form solution. To solve for the value function, we discretize the state-space by forming a grid in each of savings, appellant potential earnings state, and spousal potential earnings state (if a spouse is present). Each grid is formed using equally-spaced quantiles from the observed marginal distributions of savings and earnings, so that more grid points are positioned around more dense regions. To compute the continuation value, bivariate Gaussian quadrature is used to integrate across the joint distribution of earnings shocks for the appellant and spouse (if present). Cubic spline interpolation is fit to map the discretized value and policy functions into continuous value and policy functions. The value function and cubic spline are fit separately for each discrete type in the state-space of the model. Finally, the cubic splines are applied to the full sample in each observed time period to simulate the optimal choices of each household as a function of their discrete and continuous state-space values.

Figure 5: **Identification: Using Single-crossing to Pin Down Utility Parameters**



*Notes:* The x-axis is the parameter representing the disutility of labor for spouses in married households, which we vary while fixing all other parameters to their estimated values. The y-axis is the average labor supply of spouses. The solid line indicates the observed value in the data, while the dashed line indicates the value simulated from the estimated model. The spousal disutility of labor is pinned down as the point on the x-axis that corresponds to the crossing of the solid and dashed lines.

consider identifying the disutility of spousal labor,  $\phi_{2,S}$ . As the disutility of labor increases, the labor supply rate of spouses monotonically decreases in the model. The disutility of spousal labor is pinned down as the parameter value that equates the simulated to the observed spousal labor supply rate, which is unique due to this monotonicity, as illustrated in Figure 5. This is a form of local identification: Given all other parameters, this parameter is uniquely determined by a sample moment due to the monotonic relationship. The other disutility parameters also have corresponding moments that are monotonic in the simulation. Appendix Figure A.7 demonstrates that each disutility parameter has a unique local solution determined by a distinct sample moment.

We estimate the model separately for married couples (47 parameters) and unmarried individuals (46 parameters). To pin down these parameters, we match 57 moments (47 raw data moments and 10 IV estimates) for married couples and 52 moments (46 raw data moments and six IV estimates) for unmarried individuals. We choose two sets of moments to match. The first set consists of raw data moments, chosen based on the identification arguments above. These moments are mean log disposable income and expected log disposable income conditional on log earnings among households that supply labor, mean disposable income among households that do not supply labor, and employment rates and reapplication rates among those not receiving DI. Each of these moments is matched conditional on observable types over which the parameters vary in order to pin down all of the type-specific model parameters. The second set of moments is the IV results for consumption, disposable income, and earnings among appellants and spouses, included to discipline the model to recover our estimates of the causal effects of DI allowance.<sup>37</sup>

<sup>37</sup>We equally weight the two sets of moments. Within each set, we use the diagonal weighting matrix to form the objective function, which is equivalent to weighting each deviation between an observed and simulated moment by the

Table 9: **Fit of Instrumental Variables Estimates: Income and Consumption**

	Income (per capita)	Consumption (per capita)
<b>Panel A.</b>	<b>Married</b>	
IV estimate of effect of DI participation	-2.119	-0.165
Simulated effect of DI participation	-1.610	-2.631
<b>Panel B.</b>	<b>Single and Unmarried</b>	
IV estimate of effect of DI participation	9.443	10.366
Simulated effect of DI participation	9.815	8.894

*Notes:* This table compares the IV estimates of initial DI allowance on consumption and disposable income to the model simulated effects of these variables. All units are in \$1,000 USD.

## 7.2 Empirical results

### Parameter estimates and model fit

The externally set parameters and estimated utility parameters are presented in Panels A and B of Appendix Table A.7, respectively, whereas the estimated parameters from the earnings processes are reported in Appendix Table A.6. The parameters are precisely estimated. As anticipated and in agreement with results by Low & Pistaferri (2015), we find that the disutility of labor for appellants is strictly increasing in the severity of disability, for married as well as single and unmarried households.

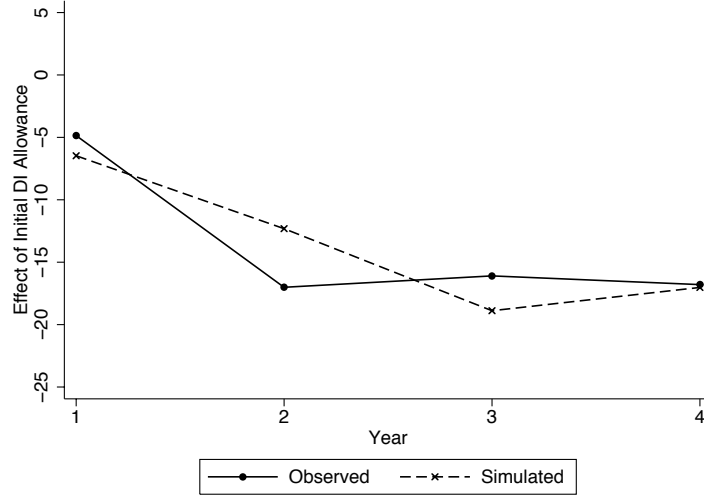
To interpret the magnitude of the utility parameters, we use the fitted models to simulate how employment rates of appellants and spouses change with a permanent one percent increase in disposable income from working, obtaining (Marshallian) labor supply elasticities (see Appendix Table A.8). Because few appellants on DI are working, we focus on employment responses in the non-DI state. We obtain plausible labor supply elasticities. As expected, the cross-wage elasticities are always negative and the own-wage elasticities are always positive. The magnitude of employment responses to a permanent wage shock are comparable to the labor supply elasticities reported by Blundell *et al.* (2016a).

Table 9 compares the IV estimates of the causal effects of DI allowance on disposable income and consumption (repeated from Tables 7 and 8) to the effects simulated from the model.<sup>38</sup> The

inverse of the standard deviation of the observed moment. This is the form of the objective function in Equation (13) of Blundell *et al.* (2016a) and is motivated by the finding of Altonji & Segal (1996) that the asymptotically efficient weighting matrix has poor small-sample properties. We use a particle swarm numerical optimization algorithm to solve for the globally optimal parameters, and validate the optimum locally using the BFGS algorithm. We use the block bootstrap to perform inference.

<sup>38</sup>The complete set of moment fits—targets, weights, and simulated values—is omitted for brevity and is available

Figure 6: **Fit of Instrumental Variables Estimates: Spousal Earnings Dynamics**



*Notes:* This figure compares the IV estimates of the effects of DI allowance on the earnings of spouses to those simulated from the estimated model. All units are in \$1,000 USD.

model is relatively successful in replicating both consumption and income effects of DI allowance. Figure 6 compares the IV estimates of the effect of DI allowance on spousal labor earnings over time in the data versus the simulation. Importantly, the model is able to replicate the time trend in the effects of DI participation on spousal employment. When we consider the other targeted moments (which vary much less over time), the model also performs well. Appendix Figure A.8 summarizes how the models fit all 109 moments (including the IV estimates and the raw data moments). Since the variables behind the moments are measured in different units (e.g. income versus employment), we divide the difference between an actual and a simulated moment by the standard deviation of the respective variable. The distribution is centered around zero, and only for a small fraction of moments do we observe differences that exceed one standard deviation.

Lastly, Figure 7 shows that we approximate well the average effective tax rates implicit in the tax-transfer system. The similar widths of the circles in this figure illustrate that the density of observed earnings is well-replicated by the model, even though the distributions of earnings and average effective tax rates are not directly targeted in the model estimation.

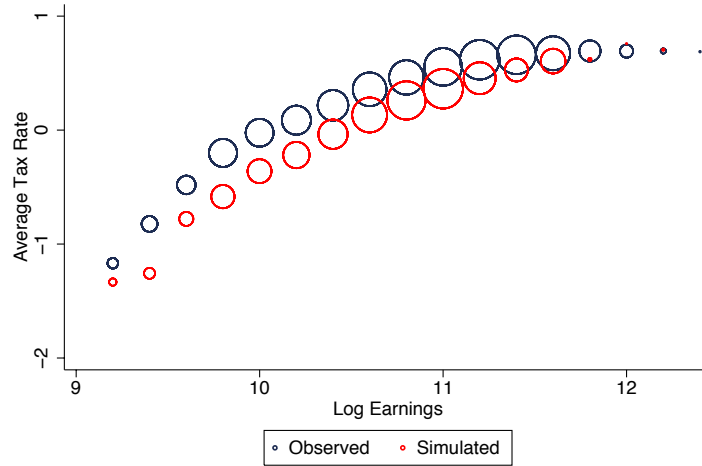
### Household valuation of DI receipt

Building on the good fit of the structural estimates to the IV estimates of the effects of DI allowance on earnings and total household income and consumption—and the plausible labor supply elasticities implied by the model—we now apply the estimated structural model to explore households’ willingness to pay for initial DI allowance, by which we mean the yearly disposable income that appellants would be willing to give up to be allowed DI at appeal.

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from the authors.

Figure 7: **Fit of Average Tax Rates Simulated from the Model**



*Notes:* This figure compares the simulated to the observed average tax rates across the distribution of earnings. The circle width represents the relative density of observed and simulated earnings, respectively.

The results are shown in Table 10. Panel A presents the average willingness to pay for initial DI allowance and compares it to the average income and fiscal cost effects presented above. We find that non-married households have statistically significant and relatively high average willingness to pay for DI (\$11,316 yearly, per household member). By comparison, the average willingness to pay for DI is relatively low for married households (\$2,300 yearly, per household member). Comparing the willingness to pay to the effect of DI allowance on disposable income, our results indicate that unmarried households primarily value DI allowance because of the associated income gains whereas married households primarily benefit from working less. Comparing the willingness to pay to the effect of DI allowance on fiscal costs suggests that, on average, each net dollar in public expenditure induced by a DI allowance raises the welfare of single and unmarried awardees by nearly 92 cents. In contrast, for married households, the fiscal costs of DI allowance are nearly twice as large as the welfare gains to awardees. Panel B displays the variation in willingness to pay across households in DI valuation. The difference between unmarried households and married households is \$3,166 at the 25th percentile, \$6,274 at the median, and \$12,563 at the 75th percentile.

Taken together, the results in Table 10 suggest that valuation of DI receipt is substantially lower among married than unmarried households. Of course, these estimates do not account for the ex ante insurance value of DI, and hence may understate total household valuation of the DI *system*. In interpreting our results, it is therefore important to emphasize that they do not preclude the possibility that valuation of the DI system exceeds its costs among both unmarried and married households.

Table 10: **Estimated Household Valuation of DI Receipt**

	Married	Single and Unmarried
<b>Panel A.</b>	<b>Average Willingness to Pay (\$1,000)</b>	
Average <i>WTP</i> :	2.300	11.316
p-value for $H_0 : WTP = 0$	0.700	0.000
Average <i>WTP</i> net of income effect:	4.419	1.873
Average <i>WTP</i> per dollar of fiscal costs:	0.579	0.916
<b>Panel B.</b>	<b>Distribution of Willingness to Pay</b>	
Percentiles		
25th:	1.161	4.327
50th:	2.216	8.490
75th:	3.031	15.594

*Note:* This table shows estimates of the average welfare benefit (\$1,000, per household member, annuitized over the four years after initial DI allowance) of DI receipt for married people and single and unmarried individuals. In the row titled, “Net of income effect,” we subtract the average effect of DI on disposable income from the average willingness to pay. In the row titled, “Per dollar of fiscal costs,” we divide the average willingness to pay by the average effect of DI on fiscal costs. The hypothesis test  $H_0 : WTP = 0$  correspond to testing whether the willingness to pay for DI receipt is equal to zero. P-values are based on reestimating the model on 20 block bootstrap replicates of the data (where the block corresponds to the individual).

### Quantifying the importance of spousal labor and other insurance mechanisms

Lastly, we perform counterfactual analyses that help us infer the extent to which spousal labor supply, savings, and reapplication buffer the household welfare consequences of DI denial versus allowance at appeal. Results from these counterfactual exercises are presented in Table 11.<sup>39</sup>

In the first exercise (reported in Panel A), we set each spouse’s labor supply to be equal to his or her labor supply in the year prior to the appeal decision. In effect, the spouse is prevented from adjusting labor supply in response to whether the appellant is initially allowed or denied DI. Notably, eliminating the option for a spousal labor supply response substantially increases the willingness of married households to pay for DI. Indeed, this restriction eliminates a considerable part of the differences in the willingness to pay of married households versus unmarried households. This underscores the importance of spousal labor supply in allowing households to buffer the welfare

<sup>39</sup>In the welfare analysis, we perform inference by evaluating willingness to pay by reestimating the model and corresponding estimated parameters with and without imposing the counterfactual restrictions on 20 block bootstrap replicates of the data.



Table 11: Counterfactual analyses

	Married	Single and Unmarried
<b>Panel A.</b>	<b>Constraining Spousal Labor Supply</b>	
Willingness to pay (\$1,000)	9.852	
p-value for $H_0 : WTP = 0$	0.000	
p-value for $H_0 :$ $WTP(Baseline) = WTP(Counterfactual)$	0.000	
<b>Panel B.</b>	<b>No Initial Savings Available</b>	
Willingness to pay (\$1,000)	3.319	13.740
p-value for $H_0 : WTP = 0$	0.550	0.000
p-value for $H_0 :$ $WTP(Baseline) = WTP(Counterfactual)$	0.000	0.000
<b>Panel C.</b>	<b>No Reapplication Available</b>	
Willingness to pay (\$1,000)	15.506	19.490
p-value for $H_0 : WTP = 0$	0.000	0.050
p-value for $H_0 :$ $WTP(Baseline) = WTP(Counterfactual)$	0.000	0.200

*Note:* This table shows estimates of the average welfare benefit (\$1,000, per household member, annuitized over the four years after initial DI allowance) of DI allowance at appeal for married households and single and unmarried households. In the rows titled “Unconstrained”, we use the estimated model to compute the welfare benefit of DI receipt. In the row titled, “Constrained spousal labor,” we compute the willingness to pay for DI receipt while constraining the spousal labor supply to the observed labor supply during the year before DI allowance is announced. In the rows titled “No reapplication,” we compute the willingness to pay for DI receipt while constraining denied appellants from reapplying for benefits by setting the probability of transitioning into DI equal to zero. The hypotheses tests  $H_0 : WTP(Baseline) = WTP(Counterfactual)$  correspond to testing whether the average willingness to pay in the baseline (unconstrained) model equals the average willingness to pay in the counterfactual. P-values are based on reestimating the model on 20 block bootstrap replicates of the data (where the block corresponds to the individual).

loss stemming from DI denial.

In the next counterfactual exercise (reported in Panel B), we set the savings of each household equal to zero at the time of the appeal decision. As DI appellant households tend to have little savings, the willingness to pay for DI receipt is only affected modestly. By contrast, the possibility of reapplying has important implications for households’ valuations of initial DI allowance, as shown in the third counterfactual exercise (reported in Panel C) where we impose the constraint that denied appellants cannot reapply for DI benefits. Formally, we set the probability of being allowed DI upon reapplication equal to zero, so appellants who are denied at the appeal will never reapply. By comparing appellants’ willingness to pay in the constrained and unconstrained settings, we find that households would be willing to pay far more for an *initial* allowance on appeal if reapplication were precluded following the denial of that appeal. This underscores the importance of taking likelihood

of reapplying, and eventually getting on DI, into account when evaluating the DI system.

## 8 Conclusion

While a mature literature finds that DI receipt discourages work, the welfare implications of these findings depend on two rarely studied economic quantities: the value that individuals and families place on receipt of disability benefits; and the full cost of DI allowances to taxpayers, summing over DI transfer payments, benefit substitution to or from other transfer programs, and induced changes in tax receipts. We assessed these missing margins in the context of Norway’s DI system, drawing on two strengths of the Norwegian environment. First, Norwegian register data allowed us to characterize the household impacts and fiscal costs of disability receipt by linking employment, taxation, benefits receipt, and assets at the person and household level. Second, random assignment of DI applicants to Norwegian judges who differ systematically in their leniency allowed us to recover the causal effects of DI allowance on individuals at the margin of program entry.

Accounting for the total effect of DI allowances on both household labor supply and net payments across all public transfer programs substantially alters our picture of the consumption benefits and fiscal costs of disability receipt. While DI denial causes a significant drop in household income and consumption on average, it has little impact on income or consumption of married appellants; spousal earnings and benefit substitution entirely offset the loss in DI benefit payments. To develop the welfare implications of these findings, we estimate a dynamic model of household behavior that translates employment, reapplication and savings decisions into revealed preferences for leisure and consumption. We used the estimated model to compute the welfare benefits of DI receipt, and to perform counterfactual exercises that help us infer the extent to which spousal labor supply, savings, and reapplication attenuate the welfare loss from DI denial at appeal.

When considering the interpretation and generality of our study, we emphasize three caveats. First, our structural model permits us to estimate the economic value of the transfer component of DI benefits—that is, the cash equivalent value of a DI award—but does not encompass the ex ante insurance value of the DI system for potential applicants. Since this insurance value is doubtless positive and potentially large, our estimates should not be interpreted as a full accounting of the welfare value of the DI system.

Second, in considering the implications of our findings for the U.S. disability system, it is worth noting that SSDI features a lower income replacement rate than the Norwegian system, and hence allowances and denials would be expected to induce less pronounced responses of spousal labor supply. Conversely, the cash and in-kind transfers available to non-SSDI households in the U.S. are surely less comprehensive than in Norway, so the marginal impact of a DI allowance on individual and household consumption may be as large or larger for U.S. households, despite the lesser generosity of the U.S. program.

Third, we emphasize that the estimates we obtain from quasi-experimental variation in judicial disability determinations correspond to the average effect of DI allowance for individuals who could

potentially have received a different allowance decision in the appeal process had their case been assigned to a different judge. Since the work capacity of individuals at the margin of program entry is likely to differ from that of inframarginal individuals, we are cautious in extrapolating the causal estimates obtained here to the broader population at large as well as other programmatic settings. Nevertheless, the economic consequences of DI receipt for marginal DI claimants are relevant for policy. In both Norway and the U.S., the rise in DI rolls in recent decades is driven in significant part by de jure or de facto changes in the screening criteria applied to claimants reporting difficult-to-verify disorders, such as back pain or mental disorders (Autor & Duggan, 2006; Kostol & Mogstad, 2014). Logically, reforms aimed at altering DI screening criteria are likely to have the largest impacts on applicants on the margin of program entry, a substantial share of whom are applicants with difficult-to-verify disorders. These observations suggest that while the estimates provided by this paper are not directly generalizable to the full DI population, they are likely to be informative for policymaking.

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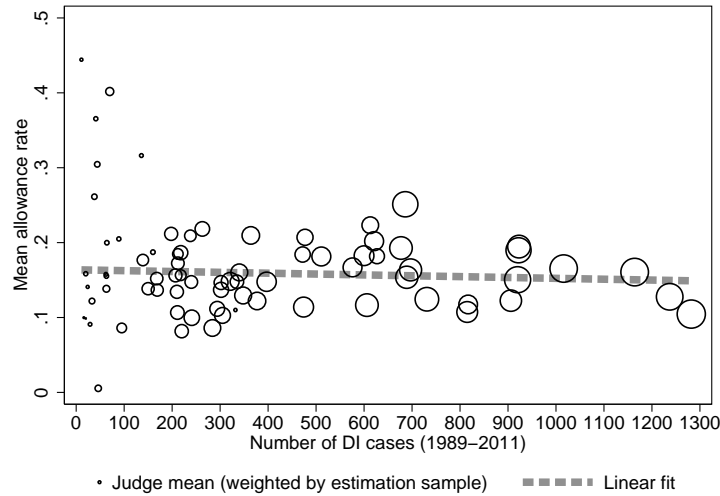
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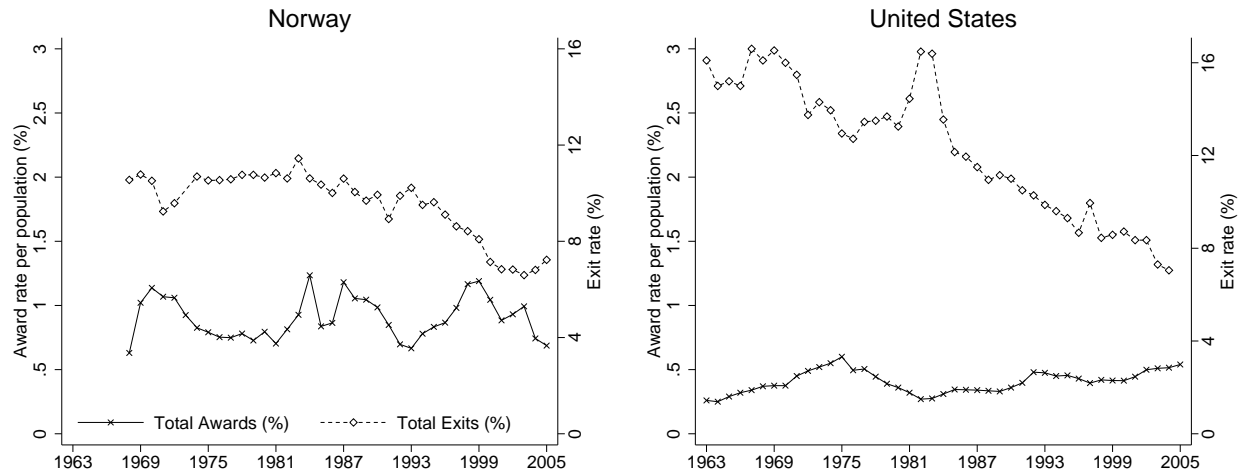
## Appendix A. Additional Tables and Figures

Figure A.1: Judge Leniency versus Number of Cases Handled



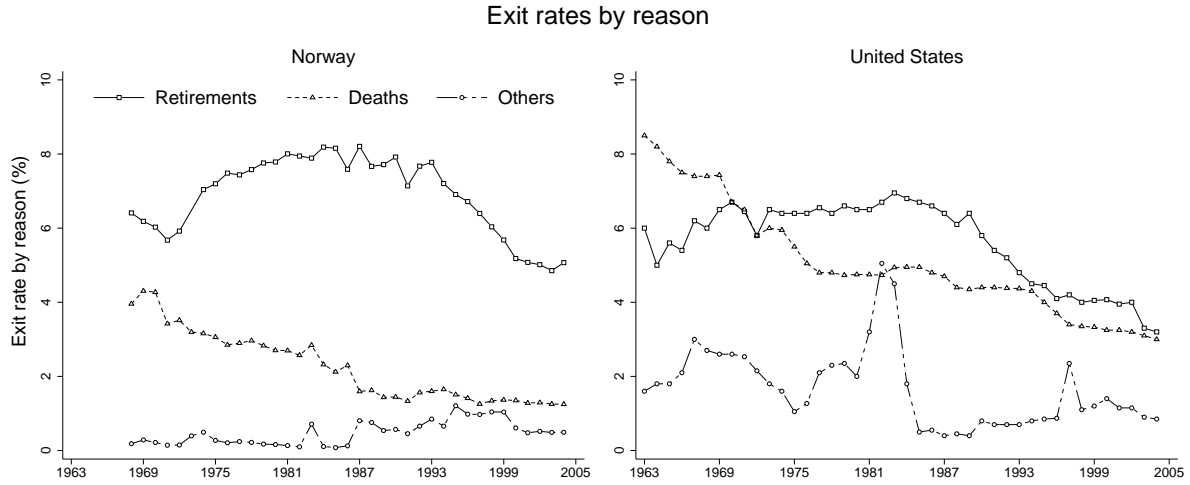
*Notes:* The figure plots judge allowance rate against the total number of cases handled by each judge. There are 75 unique judges, and on average, each judge has handled a total of 325 cases. Allowance rates are normalized by subtracting off year  $\times$  department deviations from the overall mean. Cases are restricted to claimants appealing their first denied case during the period 1994–2005. Dot size is proportional to the number of cases a judge handles in the estimation sample (which is weakly smaller than the number of cases they have ever handled, as plotted on the x-axis).

Figure A.2: DI Awards and DI Exits in Norway and the U.S.



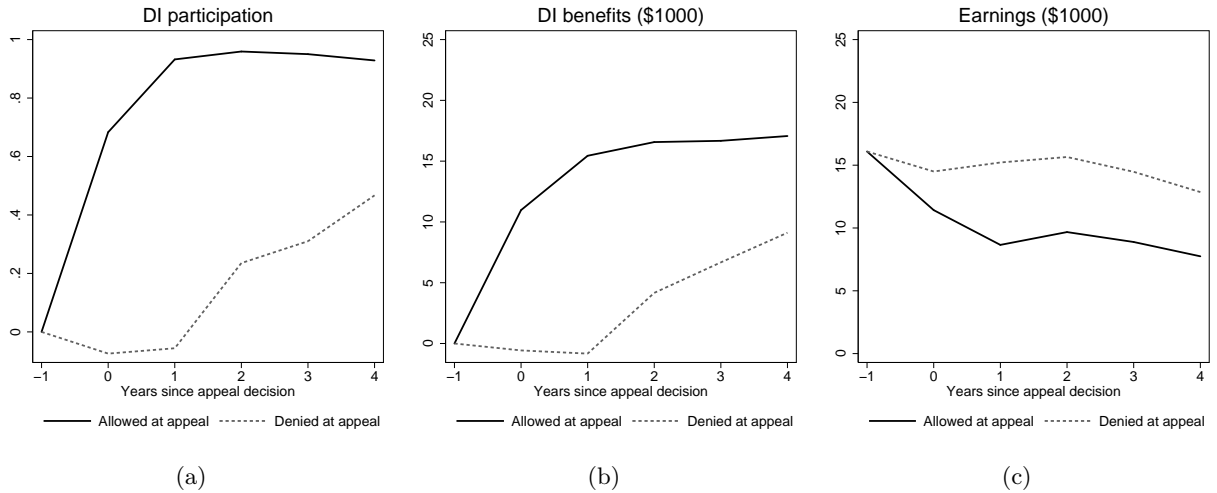
*Notes:* The U.S. trends are based on [Autor & Duggan \(2006\)](#), while the Norwegian trends are collected from various issues of the SSA Supplement. The graphs show award rates in the insured population and exit rates from the DI program in both countries.

Figure A.3: DI Exits by Reason in Norway and the United States



Notes: The U.S. trends are based on [Autor & Duggan \(2006\)](#), while the Norwegian trends are collected from various issues of the SSA Supplement. The graphs show exit rates because of death, retirement or other reasons (including eligibility-based exits).

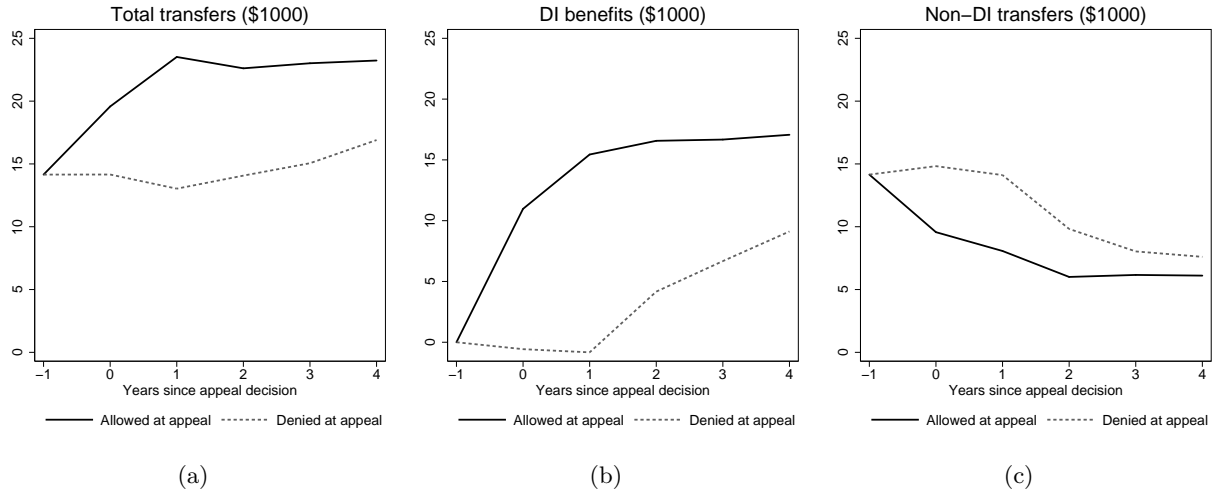
Figure A.4: Potential Outcomes: Labor Earnings and DI benefits



Notes: These figures display the decomposition of our LATE estimates into potential outcomes for allowed and denied complier appellants (see [Dahl et al. 2014](#) for details).

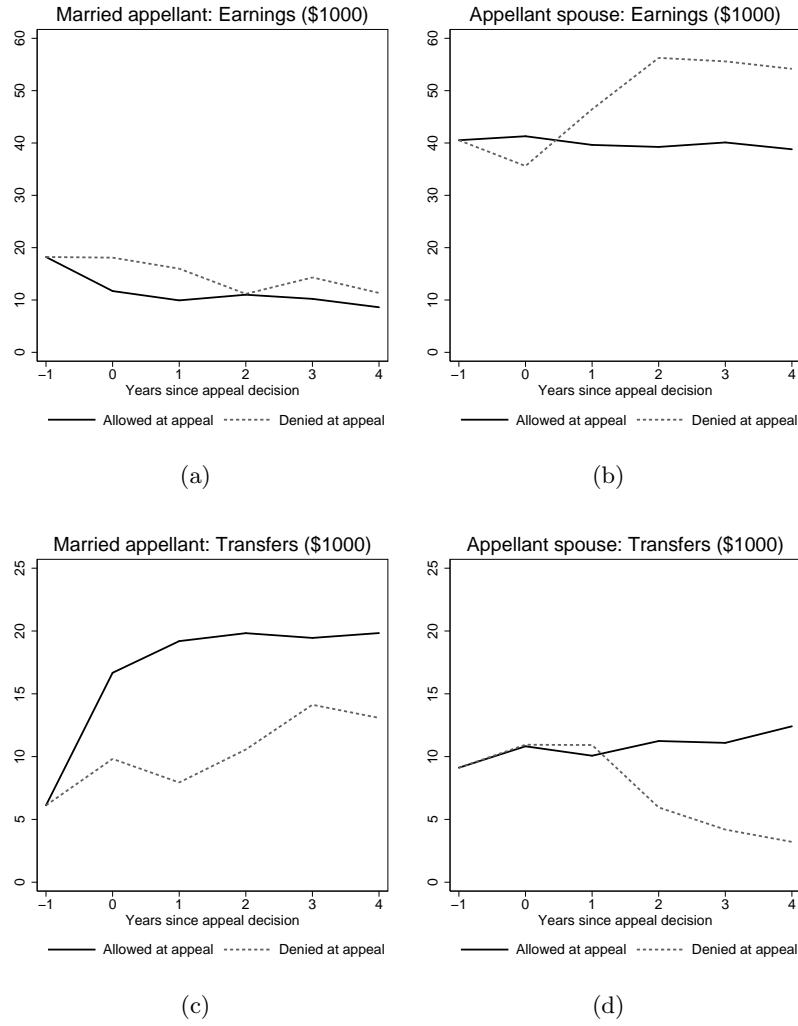


Figure A.5: **Potential Outcomes: Benefit Substitution**



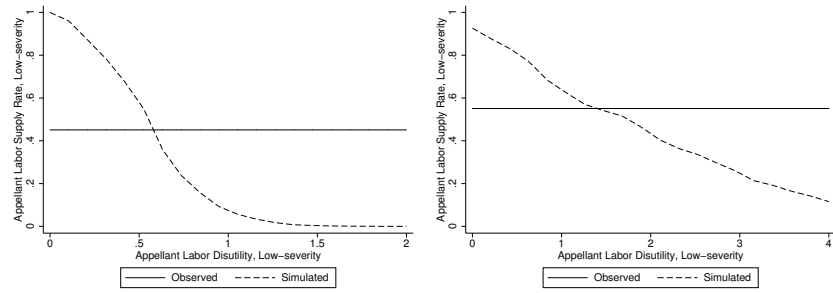
Notes: These figures display the decomposition of our LATE estimates into potential outcomes for allowed and denied complier appellants (see [Dahl \*et al.\* 2014](#) for details).

Figure A.6: **Potential Outcomes: Married appellants and Spouses**

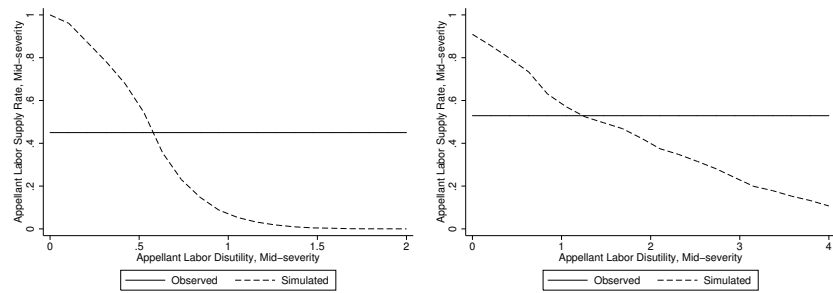


Notes: These figures display the decomposition of our LATE estimates into potential outcomes for allowed and denied complier appellants (see [Dahl \*et al.\* \(2014\)](#) for details).

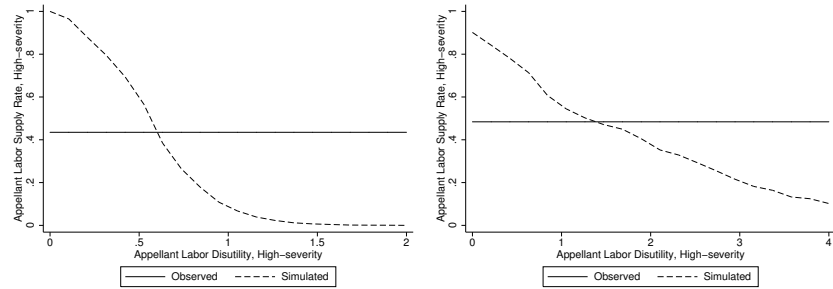
Figure A.7: Local Identification of Disutility Parameters



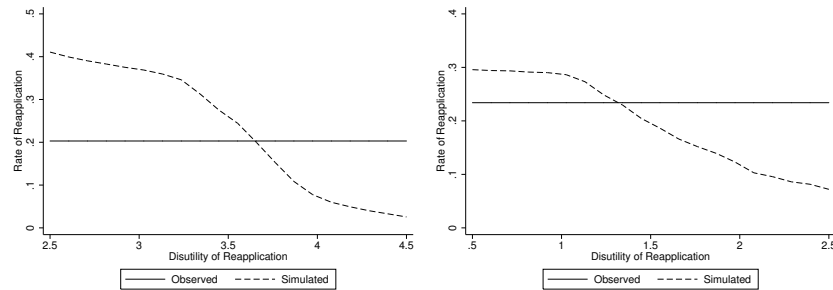
(a) Married: Labor disutility, low-severity (b) Unmarried: Labor disutility, low-severity



(c) Married: Labor disutility, mid-severity (d) Unmarried: Labor disutility, mid-severity



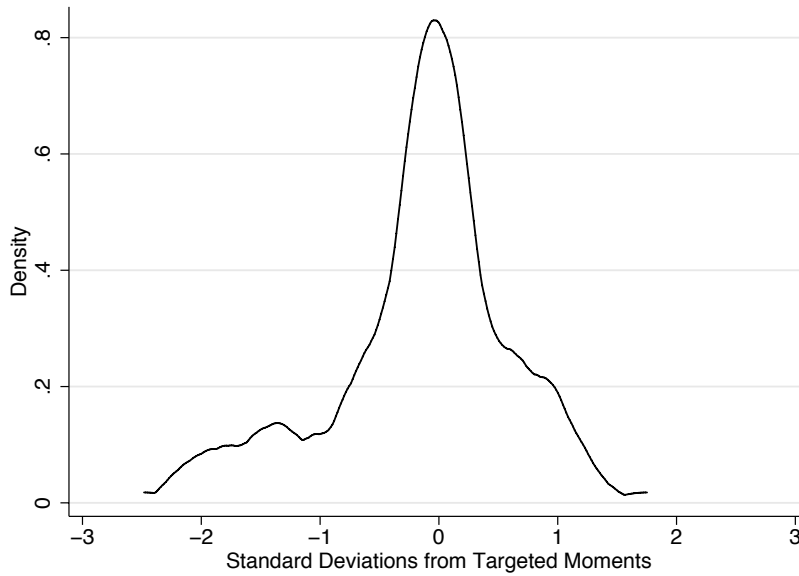
(e) Married: Labor disutility, high-severity (f) Unmarried: Labor disutility, high-severity



(g) Married: Reapplication disutility (h) Unmarried: Reapplication disutility

*Notes:* These figures illustrate local identification of the six labor disutility parameters for appellants and two disutility parameters associated with reapplication. In each figure, the x-axis is the parameter representing the disutility. The y-axis is the corresponding moment, which is the average labor supply rate by severity type for labor disutility parameters and the average reapplication rate for reapplication disutility parameters. The solid line indicates the observed value in the data, while the dashed line indicates the value simulated from the estimated model, holding all other parameters fixed to their estimated values. The disutility is pinned down as the point on the x-axis that corresponds to the crossing of the solid and dashed lines.

Figure A.8: **Distribution of Standardized Deviations from Targeted Moments**



*Notes:* This figure displays the distribution of standardized deviations of each simulated moment from its corresponding observed moment for the full set of targeted moments in the method of simulated moments model estimation.

Table A.1: **Characteristics of DI recipients in Norway and the U.S.**

Characteristic	Norway DI Recipients	U.S. SSDI Recipients
Difficult to verify disorder	59.2 %	57.3 %
Age (at decision on initial application)	52.2	49.1
Prior earnings relative to the median	71.0 %	69.9 %

*Notes:* The U.S. numbers are from [Maestas et al. \(2013\)](#), and the Norwegian numbers are drawn from the sample of DI applicants during the years 2000-2003. Difficult to verify disorders include musculoskeletal and mental diagnoses. Prior earnings are measured in years three through five prior to application or appeal.

Table A.2: **Characteristics of DI Applicants and Appellants in Norway and the U.S.**

Characteristic	Norway		U.S.	
	Applicants	Appellants	Applicants	Appellants
Difficult to verify disorder	60.9%	69.7%	58.5%	62.2%
Age (at decision on initial application)	51.1	47.1	47.1	46.1
Prior earnings relative to the median	66.5%	50.4%	60.5%	56.3%

*Notes:* This table reports the key characteristics of DI applicants and appellants discussed in Section 2. The U.S. numbers are from [Maestas et al. \(2013\)](#), and the Norwegian numbers are drawn from the sample of DI applicants during the years 2000-2003. Difficult to verify disorder comprise musculoskeletal and mental diagnoses. Prior earnings are measured during years three through five prior to application or appeal.

Table A.3: Sub-Sample First Stage Estimates

Dependent variable	Baseline instrument		Reverse-sample instrument	
	(1)		(2)	
	<i>Pr(Allow)</i>		<i>Pr(Allow)</i>	
Younger appellants (age $\leq 48$ )	0.777 (0.077)	Dep. mean: 0.093 N: 7,458	0.613 (.082)	Dep. mean: 0.093 N: 7,392
Older appellants (age $>48$ )	0.838 (0.106)	Dep. mean: 0.165 N: 6,634	0.838 (.124)	Dep. mean: 0.165 N: 6,563
Small households ( $N \leq 3$ )	0.921 (0.091)	Dep. mean: 0.140 N: 9,532	0.710 (.168)	Dep. mean: 0.139 N: 9,329
Large households ( $N > 3$ )	0.589 (0.097)	Dep. mean: 0.100 N: 4,560	0.474 (.090)	Dep. mean: 0.099 N: 4,522
Female appellants	0.837 (0.083)	Dep. mean: 0.134 N: 8,851	0.606 (.078)	Dep. mean: 0.133 N: 8,700
Male appellants	0.774 (0.115)	Dep. mean: 0.115 N: 5,241	0.668 (.139)	Dep. mean: 0.115 N: 5,184
Married appellants	0.83 (.095)	Dep. mean: 0.133 N: 8,061	0.666 (.096)	Dep. mean: 0.133 N: 7,950
Unmarried and single appellants	0.775 (0.097)	Dep. mean: 0.119 N: 6,031	0.685 (.091)	Dep. mean: 0.118 N: 5,978
Foreign born	0.425 (.155)	Dep. mean: 0.091 N: 2,534	0.373 (.141)	Dep. mean: 0.090 N: 2,509
Less than high school degree	0.899 (0.089)	Dep. mean: 0.116 N: 7,097	0.778 (.115)	Dep. mean: 0.116 N: 7,044
At least a high school degree	0.725 (.092)	Dep. mean: 0.139 N: 6,995	0.547 (.100)	Dep. mean: 0.137 N: 6,897
At least one child below age 18	0.727 (0.062)	Dep. mean: 0.102 N: 8,140	0.495 (.079)	Dep. mean: 0.101 N: 8,029
No children below age 18	0.927 (0.105)	Dep. mean: 0.162 N: 5,952	0.976 (.127)	Dep. mean: 0.161 N: 5,888
Musculoskeletal disorders	0.823 (0.112)	Dep. mean: 0.118 N: 6,149	0.732 (.119)	Dep. mean: 0.118 N: 6,102
Mental disorders	0.810 (0.120)	Dep. mean: 0.134 N: 3,666	0.605 (.125)	Dep. mean: 0.133 N: 3,624
Circulatory system	0.754 (0.367)	Dep. mean: 0.150 N: 512	0.829 (.347)	Dep. mean: 0.150 N: 510

\*\*\*p<.01, \*\*p<.05, \*p<.10. Standard errors (in parentheses) are clustered at the judge level.

*Notes:* This table reports heterogeneity in first stage estimates using the baseline instrument (1) and the reverse-sample instrument (2). The first stage specification in (1) corresponds to Panel B in Table 3. The reverse-sample instrument (2) is constructed by calculating judge leniency using all cases *except* for those in the specified subsample (e.g., judge leniency for the subsample of older applicants is constructed using judges' decisions for younger applicants). We exclude appellants whose judges handled fewer than ten cases in the reverse sample.

Table A.4: **Effect of DI Allowance on Earnings and Transfer Payments Among Married and Unmarried**

	Years after decision			
	1	2	3	4
<b>Panel A.</b>	<b>Married appellant labor earnings (\$1,000)</b>			
Allowed DI	-5.042 (3.461)	-0.444 (4.068)	-4.426 (3.993)	-3.912 (3.625)
Dependent mean	14.991	14.784	14.168	13.535
<b>Panel B.</b>	<b>Married appellant total transfers (\$1,000)</b>			
Allowed DI	9.110** (4.000)	6.499 (4.423)	5.008 (3.703)	5.395 (3.628)
Dependent mean	16.621	17.356	17.919	18.508
Observations	7,844	7,740	7,648	7,548
<b>Panel C.</b>	<b>Unmarried appellant labor earnings (\$1,000)</b>			
Allowed DI	-5.099 (7.402)	-10.939 (6.932)	-4.589 (7.018)	-6.475 (5.686)
Dependent mean	13.279	13.646	13.34	12.883
<b>Panel D.</b>	<b>Unmarried appellant total transfers (\$1,000)</b>			
Allowed DI	15.811*** (5.054)	14.466*** (4.131)	17.152*** (4.497)	10.714*** (4.084)
Dependent mean	23.336	23.518	23.848	24.224
Observations	6,128	6,102	6,061	6,059

\*\*\*p<.01, \*\*p<.05, \*p<.10. Standard errors (in parentheses) are clustered at the judge level.

*Note:* This table reports the impact of DI allowance on earnings and total transfers among married (Panel A and B) and unmarried appellants (Panel C and D). Baseline estimation sample consists of unmarried DI applicants who appeal an initially denied DI claim during the period 1994-2005 (see Section 3 for further details). There are 75 unique judges. All regressions include fully interacted year and department dummies, dummy variables for month of appeal, county of residence, age at appeal, household size, gender, foreign born, marital status, children below age 18, education, and number of medical diagnoses. All control variables are measured prior to appeal.

Table A.5: Specification Checks

Dependent variable	Died or migrated	Change in marital status			In restricted sample
		Overall	Initially Unmarried	Initially Married	
Judge leniency	0.017 (0.048)	-0.045 (0.058)	-0.030 (0.071)	-0.023 (0.074)	-0.015 (0.020)
Dependent mean	0.092	0.141	0.109	0.166	0.981
Observations	14,359	14,092	6,031	8,061	14,092

\*\*\*p<.01, \*\*p<.05, \*p<.10. Standard errors (in parentheses) are clustered at the judge level.

*Note:* This table reports the impact of judge leniency on the probability of death or migration, the probability of a change in marital status, or membership in the restricted sample. Baseline estimation sample consists of individuals who appeal an initially denied DI claim during the period 1994-2005 (see Section 3 for further details). The second to fourth columns exclude those who die or migrate during the year of the appeal. The second column tests whether DI allowance affects the likelihood of a change in marital status (married to non-married or vice versa) for the baseline sample, and the third and fourth columns test whether DI allowance affected marriage entry and exit rates respectively. There are 75 unique judges. All regressions mirror the reduced form specification of Table 4.

Table A.6: Model Parameters: Log Earnings Regressions

	Main Estimation			Robustness to Control Function		
	Married		Single and Unmarried	Married		Single and Unmarried
	Appellant	Spouse	Appellant	Appellant	Spouse	Appellant
Mid-severity of Disability	-0.015* (0.008)	-0.000 (0.005)	-0.021*** (0.002)	-0.012 (0.011)	0.002 (0.006)	-0.021*** (0.005)
High-severity of Disability	-0.031*** (0.008)	-0.032*** (0.005)	-0.041*** (0.002)	-0.045*** (0.012)	-0.033*** (0.006)	-0.027*** (0.010)
High School	0.096*** (0.007)	0.072*** (0.005)	0.145*** (0.002)	0.180*** (0.040)	0.086*** (0.008)	0.100*** (0.030)
Some College	0.249*** (0.010)	0.144*** (0.007)	0.251*** (0.002)	0.399*** (0.071)	0.167*** (0.013)	0.178*** (0.048)
Old Age	-0.064*** (0.007)	-0.066*** (0.005)	0.117*** (0.002)	-0.193*** (0.061)	-0.105*** (0.019)	0.176*** (0.038)
Pre-App. Earnings	0.847*** (0.014)	0.947*** (0.009)	1.003*** (0.003)	0.991*** (0.069)	0.993*** (0.023)	0.934*** (0.046)
Pre-App. Earnings, Squared	0.112*** (0.018)	0.056*** (0.009)	-0.017*** (0.004)	-0.202 (0.147)	0.045*** (0.011)	0.027 (0.030)
Pre-App. Earnings, Cubed	0.016 (0.029)	0.068*** (0.013)	-0.029*** (0.006)	0.046 (0.038)	0.085*** (0.015)	-0.004 (0.023)
Constant	10.133*** (0.009)	10.773*** (0.006)	10.186*** (0.002)	9.790*** (0.158)	10.731*** (0.020)	10.375*** (0.122)
Inverse Mills				0.547** (0.251)	0.152** (0.069)	-0.161 (0.303)

*Notes:* This table presents estimated parameters from the post-application log earnings regression described in the text. “Pre-app. Earnings” refers to the average earnings constructed by forming the individual-specific average log earnings over the 10 years prior to application and residualizing on the other pre-application covariates.

Table A.7: **Model Parameters: Calibrations and Labor Disutility Estimates**

	Married	Single and Unmarried
<b>Panel A.</b>	<b>Externally set parameters</b>	
Interest rate:	0.016	0.016
Discount rate:	0.976	0.976
Coefficient of relative risk aversion:	1.5	1.5
<b>Panel B.</b>	<b>Estimated disutility parameters</b>	
Labor Disutility		
Low-severity:	0.702 (0.014)	0.485 (0.010)
Mid-severity:	0.781 (0.033)	0.877 (0.011)
High-severity:	0.829 (0.007)	1.076 (0.013)
Spouse:	0.792 (0.008)	
Reapplication Disutility:	1.392 (0.008)	2.612 (0.020)

*Notes:* This table summarizes the calibrated model parameters described in the text as well as the estimated model parameters representing disutility. Inference is based on reestimating the model on 20 block bootstrap replicates of the data (where the block corresponds to the individual).

Table A.8: **Simulated Labor Supply Elasticities**

	Main Estimation		Robustness to Control Function	
	Own-wage	Cross-wage	Own-wage	Cross-wage
Single Appellant:	0.201		0.184	
Married Appellant:	0.349	-0.331	0.324	-0.299
Spouse:	0.358	-0.345	0.309	-0.319

*Notes:* This table compares labor supply elasticities for married households and single and unmarried households by severity of disability. Because few appellants allowed DI are working, we consider in the immediately following year the subsample initially denied DI. These are Marshallian elasticities, that is, labor supply responses to permanent wage shocks. We compute the elasticity using the finite-difference evaluated at a one standard deviation permanent shock to the log wage.



Table A.9: Robustness of Willingness to Pay

	Married			Single and Unmarried		
	Control function: With	Test of equality: Without P-value		Control function: With	Test of equality: Without P-value	
<b>Panel A.</b>	<b>Baseline: Average Willingness to Pay</b>					
Average	2.327	2.300	0.750	10.816	11.316	0.999
<b>Panel B.</b>	<b>Counterfactuals: Average Willingness to Pay</b>					
Constraining Spousal Labor Supply:	10.036	9.852	0.100			
No Initial Savings Available:	3.349	3.319	0.750	13.247	13.740	0.999
No Reapplication Available:	15.589	15.506	0.200	19.085	19.490	0.999

*Note:* This table shows estimates of the average welfare benefit (\$1,000, per household member, annuitized over the four years after initial DI allowance) of DI allowance at appeal for married households and single and unmarried households. In the rows titled “Unconstrained”, we use the estimated model to compute the welfare benefit of DI receipt. In the row titled, “Constrained spousal labor,” we compute the willingness to pay for DI receipt while constraining the spousal labor supply to the observed labor supply during the year before DI allowance is announced. In the rows titled “No reapplication,” we compute the willingness to pay for DI receipt while constraining denied appellants from reapplying for benefits by setting the probability of transitioning into DI equal to zero. The hypotheses tests correspond to testing equality in the average willingness with and without correcting for selectivity bias in the estimation of the earnings processes. P-values are based on reestimating the model on 20 block bootstrap replicates of the data (where the block corresponds to the individual).