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Return-to-Work Policies and Labor Supply in Disability Insurance Programs[†]

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Disability Insurance (DI) programs are among the largest social assistance programs in developed countries. These programs provide benefits to individuals with health conditions that limit the amount or type of paid work they can perform. There have been concerns about the high levels of expenditure on DI programs. DI programs also have been criticized for inducing beneficiaries to reduce their labor supply. Most countries therefore have already implemented—or are considering implementing—policies to make their beneficiaries "return to work." These programs allow DI beneficiaries to collect all or a portion of their benefit while working. Although return-to-work policies intend to increase the labor supply in DI programs, the empirical evidence on the effectiveness of such policies is mixed. For example, Hoynes and Moffitt (1999); Benitez-Silva, Buchinsky, and Rust (2011); Weathers and Hemmeter (2011); and Bütler et al. (2015) find no effects of financial incentives to work in the United States and Switzerland. However, Campolieti and Riddell (2012); Kostol and Mogstad (2014); and Ruh and Staubli (2016) find positive responses in Canada, Norway, and Austria. Zaresani (2017) suggests that the relative size of the incentives to work induced by a program versus the adjustment costs that individuals face when changing their labor supply could possibly explain these disparate findings.

In this paper, I investigate whether a large increase in incentives to work in a return-to-work setting could induce DI recipients to increase their labor supply. I explore a policy change in a DI program called "Assured Income for the Severely Handicapped" (AISH) in Canada using a regression discontinuity design (RDD).

I. Institutional Background

AISH is the provincial DI program in Alberta, Canada. The benefits include monthly DI benefits and some other supplementary benefits (i.e., health insurance and child care). Furthermore, beneficiaries can work and still collect a portion of their DI benefits. The earnings below an exemption threshold do not affect the benefits. But, DI benefits are deducted if the monthly earnings exceed a threshold. Beneficiaries who work would lose $1 of their monthly DI benefits for every $2 in monthly earnings accumulated above the first threshold (C$400 for beneficiaries with no dependent and C$975 for those with dependents). Furthermore, DI benefits are reduced $1 for every $1 earned above the second threshold (C$1,500 for beneficiaries with no dependent and C$2,500 for those with dependents). This is comparable to a 50 percent marginal tax on earnings between the first and the second threshold and 100 percent marginal tax on earnings above the second threshold. The policy change doubled the first exemption threshold and increased the DI benefits by 35 percent, effective from April 2012. The first threshold was increased from C$400 to C$800 (C$975 to C$1,950) for those with no dependent (with dependent). The monthly DI benefits were increased to C$1,588 from C$1,188 for all beneficiaries. Figure 1 illustrates the budget constraints of the beneficiaries before and after the policy change. This policy change is comparable

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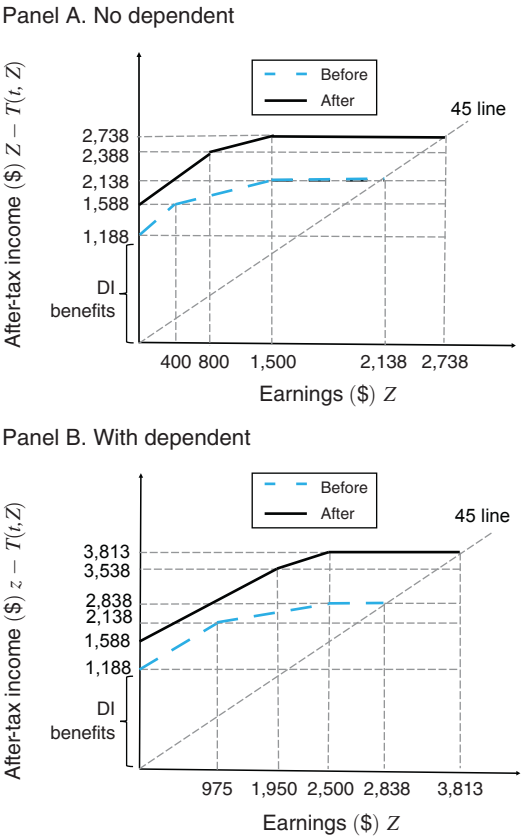

FIGURE 1. BUDGET CONSTRAINT OF BENEFICIARIES BEFORE AND AFTER THE POLICY CHANGE

to a drastic decrease in the marginal tax rate on earnings, providing much greater incentives to work for DI recipients.

II. Data

I use an administrative dataset on monthly earnings and benefits of the AISH benefit recipients, obtained from the Government of Alberta. The data also has information on beneficiaries' individual characteristics including sex, age, marital status, family size, age DI awarded, location of residence, and the ICD-9 codes indicating type of disability. I focus on beneficiaries with nonphysical disabilities, since it is believed that they are the marginal entrants to DI programs and are expected to be more responsive to the incentives to work. My study sample includes 18-to-64-year-old individuals with nonphysical disabilities. I select a time frame within two years of the policy change in AISH in March 2012: from March 2010 to April 2014.

III. Empirical Analysis

A. *Regression Discontinuity Design (RDD)*

I investigate the effect of return-to-work policies on labor supply of DI beneficiaries using a sharp discontinuity in the induced incentives to work at the month of the policy change in the AISH program. I implement a RDD by estimating a regression of the form:[1]

$$(1) \quad y_{im} = \alpha + f(m) + \rho D_m + \beta X_{it} + \epsilon_{im},$$

where y_{im} denotes the labor supply of individual i in month m. I use real monthly earnings and labor force participation (LFP) as outcome variables. The LFP is defined as a dummy that switches on when I observe positive earnings. X_{it} is a set of individual characteristics including sex, age, marital status, family size, age DI awarded, location of residence, and type of disability (psychotic, neurological, or mental). D_m is the treatment dummy that captures the effects of the policy change by switching on for months following the policy change. ϵ_{im} is the error term. $f(\cdot)$ denotes a polynomial time trend to control for time series variation in labor supply that would have occurred in absence of the policy change. The coefficient of interest is ρ which captures the effect of the policy change on the labor supply.

The intuition behind my identification strategy is straightforward. I compare the labor supply outcomes right after the policy change (treatment group) to those right before the policy change (control group). The key identification assumption is that the only reason for change in individuals' labor supply is the policy change itself. In other words, there are not other variables that change discontinuously at the same time of the policy change in AISH. I isolate the change in labor supply solely due to the policy change by controlling for trend in labor supply from other sources using $f(m)$. This approach will not be threatened if other

[1] RDD based on a time-series discontinuity is discussed by Hausman and Rapson (2017) and is similar to that used by Davis (2008) and Chen and Whalley (2012).

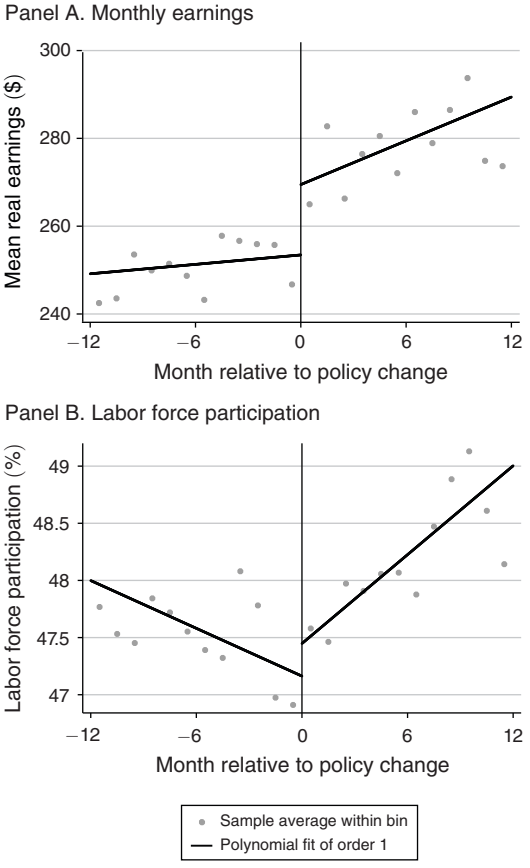

FIGURE 2. DISCONTINUITY IN LABOR SUPPLY BEFORE AND AFTER THE POLICY CHANGE

unobservable variables affecting labor supply change smoothly in the neighborhood of the policy change date (Hahn, Todd, and Van der Klaauw 2001).

However, there are reasons to believe that the identification assumption might be violated. First, new entrants to the program after the policy change might be relatively healthier than those who entered before the policy change, and these new entrants can potentially work more. This is because the new policy allows beneficiaries to work more while collecting DI benefits. Second, there might be anticipatory effects since the policy change was announced two months before it was implemented. To deal with these concerns, I follow a similar approach to that of Marie and Vall Castello (2012) and exclude those who have entered into the program after the policy change was announced in February 2012.

B. *Graphical Evidence*

Figure 2 plots the trends in labor supply outcomes; the mean real earnings[2] and the LFP within one year of the policy change. The fitted lines before and after the policy change are also illustrated. This figure suggests that first, there is a discontinuous increase in both earnings and LFP around the date of the policy change. Second, the trends in earnings and LFP both before and after the policy change are linear. I therefore use a linear time trend $f(m)$ in (1).

C. *RDD Estimates*

Table 1 presents the estimated effects on the labor supply from the increase in the incentives to work induced by the policy change. I use Calonico, Cattaneo, and Titiunik (2014) to nonparametrically construct confidence intervals around the estimated average treatment effects using (1). My main specification is a local linear model with a triangular kernel density (which imposes more weight on observations in months closer to the time of the policy change) within six months of the policy change (i.e., six months bandwidth). The estimated effect of the policy change is a statistically significant 8.9 percent increase in the real average monthly earnings and about one percentage point increase in the LFP (although not significant at conventional levels). Adding individual covariates including sex, age, marital status, family size, age DI awarded, location of residence, and disability type do not change the estimates. I also estimate the effects using bandwidths varying from 3 to 12 months. The estimates are quite robust to the selected bandwidth.

D. *Seasonality in the Labor Market*

The estimates suggest that the increase in incentives to work induced by the policy change in April 2012 caused an increase in the labor supply of the beneficiaries. However, there are concerns that the seasonality of the labor market might be the driving force. To shed light on this concern, I also estimate the effects of placebo

[2] The nominal earnings are adjusted for inflation using the CPI, with March 2012 as base.

Table 1—Estimated Effects of the Policy Change on on Earnings and Labor Force Participation

	(1)	(2)
Panel A. Monthly earnings (C$)		
Estimated effect	22.52	22.54
	(6.88)	(6.86)
Mean before policy change	252.69	252.69
	(427.04)	(427.04)
Individual co-variates	No	Yes
Observations	112,768	112,768
Panel B. Labor force participation (*percent*)		
Estimated effect	0.99	1.06
	(0.77)	(0.76)
Mean before policy change	47.41	47.41
Individual co-variates	No	Yes
Observations	112,768	112,768

Notes: Individual covariates includes sex, age, marital status, family size, age DI awarded at, location of residence, and type of disability (psychotic, neurological, or mental). The robust standard errors in the parenthesis are clustered at the individual level and are estimated using Calonico, Cattaneo, and Titiunik (2014).

Table 2—Estimated Effects from Placebo Policy Changes

	Earnings (dollar)	LFP (percent)
Panel A. Placebo policy change in April 2010		
Estimated effect	−8.06	−0.08
	(6.51)	(0.78)
Mean before policy change	271.95	52.08
	(422.86)	
Observations	99,575	99,575
Panel B. Placebo policy change in April 2011		
Estimated effect	−2.84	−0.20
	(6.22)	(0.75)
Mean before policy change	249.92	47.82
	(422.86)	
Observations	107,476	107,476
Panel C. Placebo policy change in April 2013		
Estimated effect	−0.85	0.02
	(6.65)	(0.72)
Mean before policy change	281.83	47.92
	(472.67)	
Observations	118,886	118,886

Notes: The effects are estimated using a six-month window of the corresponding placebo policy change. Individual covariates are included in all the estimates. The robust standard errors in parenthesis are clustered at the individual level and are estimated using Calonico, Cattaneo, and Titiunik (2014).

policy changes in April 2010, April 2011, and April 2013.

Table 2 presents the estimates within a six-month window of the corresponding placebo policy change. All the estimates are negative and insignificant at conventional levels. This suggests that if there is seasonality in the labor market, the estimates presented in Table 1 represent the lower bound on the effects on the labor supply from the increase in incentives to work.

IV. Conclusion

Findings on the effectiveness of return-to-work policies in DI programs are mixed. Zaresani (2017) shows that the size of the barriers that individuals face when changing their labor supply, versus the induced incentives to work by a policy change might explain the disparate findings. In this paper, I evaluate the effect of a policy change in a DI program that provides large incentives to work. My findings from an RDD model that explores the discontinuity in incentives to work induced by a policy change in the AISH program confirms the findings of Zaresani (2017). These results have important implications for designing policies to increase labor supply in DI programs.

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