2017

Essays in Labor, Public and Health Economics

Zaresani, Arezou

UNIVERSITY OF CALGARY

Essays in Labor, Public and Health Economics

by

Arezou Zaresani

A THESIS

SUBMITTED TO THE FACULTY OF GRADUATE STUDIES

IN PARTIAL FULFILLMENT OF THE REQUIREMENTS FOR THE

DEGREE OF DOCTOR OF PHILOSOPHY

GRADUATE PROGRAM IN ECONOMICS

CALGARY, ALBERTA

August, 2017

Abstract

This thesis consists of three chapters. In the first chapter, I investigate how important are adjustment costs for individuals when they face incentives to work induced by a policy change. I provide the first estimate of heterogeneous adjustment costs by exploiting a unique policy change that induces large incentives to work. The policy change dramatically decreased marginal tax rates on earnings in a non-linear tax schedule on earnings in a disability insurance program in Canada. Individuals continue to bunch at the location of a kink even when the kink no longer exists, suggesting that they face adjustment costs when changing their labor supply. I use the amount of bunching at the kinks before and after the policy change to estimate the size of adjustment costs that vary by individuals' ability to work. The estimated adjustment costs are higher for individuals with lower ability; varying from zero to 8 percent of their potential earnings. The estimated elasticity of earnings with respect to tax rates – accounting for heterogeneous adjustment costs – is 0.2 which is double the size of the one estimated with no adjustment costs. The policy change also decreased the marginal tax rates far away from the kinks. I then evaluate the overall effects of the policy change on the labor supply using a Difference-in-Differences design. I find that some individuals work more and some others start working in response to the large induced incentives to work. Accounting for the adjustment costs then might explain the disparate findings on the effects of increase in incentives to work on labor supply in disability insurance programs. My findings therefore have important implications for designing policies and targeting heterogeneous groups to increase labor supply in disability insurance programs.

In the second chapter, I describe statistical determinants of Labor Force Participation (LFP) of adults with Autism Spectrum Disorder (ASD) and investigate what might explain their lower LFP than those with the other developmental, neuro-cognitive and physical disabilities. The estimated Average Marginal Effect of completing high school on probability of LFP from Probit models is the highest for those with ASD among all the other comparison

groups of those living with the other disabilities. The estimated effects are higher for younger adults than that for the older ones. These findings suggests that improving education attainments of younger individuals with ASD could comparatively be more effective in improving their LFP. Blinder-Oaxaca decompositions show that considerable portion of the lower LFP of adults with ASD than the other comparison groups is not explained by their observable characteristics, suggesting that they might be subject to stigma and discrimination more often than the others with disabilities.

In the last chapter, co-authored with Lucie Schmidt and Lindsay Tedds, we investigate whether insurance coverage of medical treatments with high out-of-pocket costs affects patients' utilization. We exploit a policy intervention that mandates coverage for In-Vitro-Fertilization (IVF) –an expensive infertility treatment with low success rates in one cycle of treatment– in private health insurance in the US. Mandated coverage varies from one cycle of treatment in some states to unlimited cycles in some others. Patients' might increase their chances of conceiving an infant by more aggressive treatments, resulting in risky and costly multiple births. We provide the first estimate of the effects on adverse outcome of aggressive treatments from number of IVF cycles covered in mandated health insurances. We use a Generalized Synthetic Control framework to estimate causal effects. Our estimated effects varies from 0.31 percentage points decrease in share of multiple births in states with only one covered cycle to more than 35 percentage points increase in states with unlimited coverage. Our estimates of effects of mandated IVF coverage on adoption –the main alternative for IVF patients with low chances of success– furthermore shows that adoption rates in states with more covered cycles is lower. These findings suggests that high out-of-pocket costs has strong behavioural responses from patients. In states with more coverage, more patients with low chance of success –who would prefer aggressive treatments– use the treatment. These patients otherwise would have adopted a child. Our findings have important implications for designing policy interventions to increase accessibility of expensive and technologically advance medical treatments while simultaneously decreasing utilization costs.

Acknowledgements

I would like to thank all who have provided me with support, advice, and encouragement throughout the process of developing this thesis. I specially thank my first advisor Herb Emery whose utmost support and patience made this thesis possible. I am grateful to my second advisor Stefan Staubli. It has been an honor to be his first PhD student. His advice and encouragement have been priceless. My other thesis committee members Pamela Campa, Arvind Magesan and Eugene Choo provided essential and wise comments. I also thank Jean-Francois Wen for all his help and support. I am grateful to Alexander Whalley for cheering me up and morally supporting me. I also thank Christoph Luelfesmann who guided me through finding my passion in the field of Economics.

Developing this thesis would not be possible without the data sources. I thank Cheryl Raddis from the Human Services of the Government of Alberta, and Charlie Victorino from Statistics Canada for all their help with accessing the data sources and their cooperation while conducting the analysis.

I thank my fellow graduate students at the Department of Economics at the University of Calgary who created such a good atmosphere in the department. I am also grateful to my other friends specially Ines and Mozhgan for patiently supporting me in all the up and downs.

I am indebted to my parents who have supported me in every way possible. My father had believes in me and, although not with us, has left fingerprints of grace on my life. My mother, all the support that she provided to me over the years is the greatest gift anyone has ever given to me. I am grateful to my younger sister Azadeh for always supporting me as an older sister.

I have no words to say how grateful I am to Miguel, my best friend and life partner, for being with me all along the way. I am so excited to start a new chapter with him in another hemisphere.

To my mom,

and the memory of my dad,

who always picked me up on time

and supported me to go on every adventure,
specially this one.

Table of Contents

List of Tables

List of Figures and Illustrations

Chapter 1

Adjustment Costs and Incentives to Work: Evidence from a Disability Insurance Program

1.1 Introduction

A common assumption in labor supply models is that individuals can costlessly adjust their labor supply; even though facing adjustment costs affects their labor supply responses to policy changes.[1] Adjustment costs are broadly described as factors that make it harder for individuals to change their labor supply such as, time and financial costs of searching for a new job, negotiating hours of work with a current employer, understanding tax systems and policy changes, needing workplace accommodations or simply emotional costs of mental stress from working more. The size of the adjustment costs is important for evaluating welfare effects of policy changes (Chetty et al., 2009). Adjustment costs can also explain the differences in estimated elasticity of earnings in micro versus macro studies (Chetty et al., 2011; Chetty, 2012; Chetty et al., 2012). There is, however, very little empirical evidence on existence and magnitude of the adjustment costs except for Gelber, Jones, Sacks and Song (2016).

In this paper, I empirically examine the interaction between adjustment costs and incentives to work and its effects on the labor supply. I exploit a unique policy change that provides large incentives to work by dramatically decreasing marginal tax rates on earnings. More specifically, I use a policy change in the Assured Income for the Severely Handicapped

[1]See for instance Chetty, Looney and Kroft, 2009; Chetty, Friedman, Olsen and Pistaferri, 2011; Chetty, 2012; Chetty, Guren, Manoli and Weber, 2012; Chetty, Friedman and Saez, 2013; Kleven and Waseem, 2013; Kleven, 2016.

(AISH), a provincial Disability Insurance (DI) program in Alberta, Canada. The earnings below the exemption threshold in AISH do not affect the DI benefits; but DI benefits are gradually deducted for the earnings accumulated above the exemption threshold. This is comparable to a non-linear tax schedule on earnings. The marginal taxes below and above the exemption threshold are respectively zero and 50%, creating a kink at the exemption threshold. The kink generates incentives to locate – bunch – right below the exemption threshold in order to avoid the high marginal tax rate above the exemption threshold. The policy change in AISH doubled the exemption threshold and increased the maximum DI benefits by 35 percent. Individuals bunch right below the exemption threshold where the marginal tax on the earnings is zero; suggesting strong behavioral responses to the induced incentives to work. The puzzling observation, however, is that individuals continue to bunch at the location of the old threshold even when the threshold is changed. This observation suggests that individuals face adjustment costs when changing their labor supply. I use the amount of bunching at the exemption threshold before and after the policy change to provide the first estimate of heterogeneous adjustment costs. I extend Gelber, Jones, Sacks and Song (2016) by allowing for heterogeneous adjustment costs that vary by individuals' ability to work, measured by their potential earnings if no taxes had been imposed on them.

The estimates using the amount of bunching around the exemption threshold provide an incomplete picture of the effects of the policy change on labor supply; since the policy change also deceased the marginal tax rate on earnings far away from the exemption threshold. Furthermore, the policy change might also have extensive margin effects, inducing some individuals start working. Examining the overall effects of increase in incentives to work on the labor supply in a DI program is however challenging. First, individuals' labor supply is endogenous since, the selection process into a DI program strongly depends on having low labor supply. Second, adjustment costs attenuate the induced incentives to work by a policy change. The policy change in AISH creates an opportunity to investigate the potential to induce greater labor supply when individuals face adjustment costs. I estimate the causal effects of the policy change on the labor supply using Difference-in-Differences (DD) design. I use DI recipients of the Ontario Disability Support Program (ODSP) – another provincial DI program in Canada – as a control group. The ODSP is an appropriate control group since its benefit scheme is similar to – but less generous than – AISH; and ODSP did not go under major policy changes during the period of my analysis.

I use administrative data on monthly earnings of DI recipients in AISH and ODSP from the Governments of Alberta and Ontario within two years of the policy change in AISH. The datasets also have information on individuals' characteristics including sex, age, marital status, family size, age entering into the DI program and the location of residence. These datasets furthermore include ICD-9 codes[2] of DI recipients' disability conditions. This allows me to investigate the effects of incentives to work on labor supply of DI recipients with non-physical disabilities. Individuals with non-physical disabilities are believed to be the marginal entrants to DI programs and therefore are expected to be responsive to incentives to work.

My empirical analysis provides three conclusions. First, there are strong behavioral responses to the incentives to work in the form of sharp bunching at the exemption threshold. However, bunching at the location of the old threshold when the threshold no longer exists, suggests that individuals face adjustment costs when changing their labor supply. Individuals with lower ability to work face higher adjustment costs, varying from zero to 8 percent of their potential earnings. The adjustment costs are estimated for a sub-sample of individuals who bunch at the exemption threshold and are relatively more flexible in changing their labor supply. The evidence on existence of adjustment costs for individuals who bunch, suggests that adjustment costs might be even larger for those who do not bunch. My estimates are therefore, a lower bound on the adjustment costs that DI recipients face when changing their labor supply.

[2]The ICD-9 is the 9^{th} revision of the International Statistical Classification of Diseases Related Health Problems, a medical classification list by the World Health Organization. It contains codes for diseases, signs and symptoms, abnormal findings, complaints, social circumstances and external causes of injury or diseases.

Second, the estimated elasticity of earnings with respect to net-of-tax ratio[3] at the exemption threshold – accounting for the adjustment costs – is 0.2 which is double the size of the one estimated with no adjustment costs. Adjustment costs therefore make significant differences in responses to the policy changes.

Third, policies that provide incentives to work in DI programs increase labor supply only if the induced incentives to work are large enough to offset the adjustment costs. My estimate of the effects of the increased incentives to work induced by the policy change in AISH is twelve percent increase in earnings, and one percentage point increase in the labor force participation rate. This finding suggests that the induced substitution effects of the policy change is relatively larger than the induced income effects[4]; and the policy change therefore might be welfare improving. The induced increase in labor force participation also provides evidence on importance of the adjustment cost on extensive margins of the labor supply. If the induced incentive to work is large enough to offset the fixed costs of the labor force participation (i.e. monetary costs like transportation, clothing and child care or non-monetary costs like emotional costs due to stress and additional responsibilities associated with work). My findings are all robust to a set of specification tests.[5]

Findings from my empirical analysis have important implications in designing policies and targeting heterogeneous groups to increase labor supply in DI programs. DI programs are among the largest social insurance programs in advanced countries.[6] These programs provide benefits to individuals with health conditions that limit the kind or amount of work they can perform. There have been concerns about governments' high expenditure on DI

[3]The net-of-tax ratio is defined as the ratio of one minus the marginal tax rates below (τ_0) and above (τ_1) a kink as $\frac{1-\tau_0}{1-\tau_1}$.

[4]In Appendix A.4, I provide suggestive evidence that the induced income effects of the policy change in AISH is negligible.

[5]I also estimate the effects of the policy change in AISH on the labor supply using Regression Discontinuity (RD) design. I use the date of the policy change as the assignment variable. Intuitively, I compare individuals labor supply right after the policy change (treatment group) to their labor supply right before the policy change (control group). My findings from RD design also support my main findings from DD design. More details on the RD design estimates are provided in Appendix A.3.

[6]In the OECD countries, the average total expenditure on DI programs accounts for 2.5 percent of the GDP (OECD, 2010).

programs. In most of DI programs benefit recipients lose all or part of their benefits if they work. Losing DI benefits is a disincentive to work. Many countries therefore have recently implemented – or are considering – policies to generate incentives to work.[7] In the new policies benefits are reduced more gradually if DI recipients work. More gradual reduction of DI benefits generates incentives to work and therefore benefit recipients work more and might eventually exit the DI program.

While policies that provide incentives to work are intended to increase the labor supply in DI programs, empirical findings on effectiveness of such policies are not conclusive. Hoynes and Moffitt (1999), Benitez-Silva, Buchinsky and Rust (2011), Weathers II and Hemmeter (2011) and Bütler, Deuchert, Lechner, Staubli and Thiemann (2015) find no effects of financial incentives to work in the U.S. and Switzerland. While Campolieti and Riddell (2012), Kostol and Mogstad (2014) and Ruh and Staubli (2016) find positive responses respectively in Canada, Noway and Austria. Beyond change in financial incentives, medical reassessment of DI recipients and trial work periods in the US. do not appear to have effects on the labor supply (Autor and Duggan, 2006). Moore (2015) finds positive effects on labor supply of those who lost their benefits after removal of drug and alcohol addictions as qualifying conditions for DI programs in the US. Borghans, Gielen and Luttme (2014) and Staubli (2011) examine the effects of terminating benefits and stricter eligibility criteria in DI programs in respectively Netherlands and Austria. They find that individuals substitute DI benefits by collecting more from other social assistance programs. Lemieux and Milligan (2008), Fortin, Lacroix and Drolet (2004) and Gruber (2000) find negative effects of providing more generous benefits on labor supply in social assistance programs in Canada. The induced incentive

[7] The US., UK., Norway and Switzerland are among the countries that recently implemented policies in their DI programs. In the UK.'s program DI recipients are allowed to keep fifty percent of their benefits for up to twelve months if they work. In Norway's program benefits are reduced by \$0.6 for every \$1 earned above a pre-set threshold (see Kostol and Mogstad (2014) for an evaluation of the program). The U.S. is currently testing a program where benefits are reduced by \$1 for every \$2 of earnings accumulated above a pre-set threshold, rather than fully suspending the benefits (see Benitez-Silva et al. (2011) for a calibrated life-cycle model to forecast the effects of the policy. See also Weathers II and Hemmeter (2011); Wittenburg et al. (2015) for evaluations of the pilot project). Switzerland tested a program which offers a conditional cash payment if DI recipients start to work or increase their earnings (see Bütler et al. (2015) for an evaluation of the program).

to work from a policy change must be large enough to offset the adjustment costs to cause an increase in the labor supply in a DI program. Better understanding of the heterogeneous adjustment costs has also important policy implications as how to target individuals for the policy changes. There might be groups of DI recipients who need more support to be able to work whereas some others would not work regardless of the provided supports and incentives to work. Accounting for adjustment costs then might explain the mixed findings on the effects of incentives to work on labor supply in DI programs.

My paper is also related to the literature on adjustment costs. The effects of search costs, hours constraint and institutional constraints on labor supply decisions are discussed in earlier work (Pencavel, 1986; Altonji and Paxson, 1988; Dickens and Lundberg, 1993; Blundell and Mccurdy, 1999; Chetty, Friedman, Olsen and Pistaferri, 2011; Tazhitdinova, 2016). Altonji and Paxson (1992) suggests that individuals face adjustment costs changing their labor supply since the change in hours of work are lumpy. Several other works also suggest that individuals face adjustment costs changing their behavior to policy changes (Chetty, Looney and Kroft, 2009; Chetty, Friedman, Olsen and Pistaferri, 2011; Chetty, 2012; Chetty, Guren, Manoli and Weber, 2012; Chetty, Friedman and Saez, 2013; Kleven and Waseem, 2013). Chetty, Friedman, Olsen and Pistaferri (2011) show that adjustment costs affect estimates of elasticity of labor supply. None of the previous works however provide an estimate of the adjustment costs. Gelber, Jones, Sacks and Song (2016) are the first to specify a model to empirically estimate fixed adjustment costs. I contribute to this literature by extending the model for estimating fixed adjustment costs by allowing for heterogeneous adjustment costs.

For the remainder of the paper, I proceed as follows. I describe the institutional background on AISH and ODSP and the data I use for my empirical analysis in Section 1.2. I present my model for estimating heterogeneous adjustment costs and elasticity of earnings in Section 1.3. In section 1.4, I present my estimates of the effects of incentives to work on labor supply using DD design. Finally, I provide conclusions and policy implications in Section 1.5.

1.2 Institutional background and data

1.2.1 Disability insurance programs in Canada

The federal and provincial DI programs in Canada are designed to provide benefits to individuals who due to a medically verifiable physical or non-physical disability are limited in the kind or amount of work they can do. Access to the federal DI programs are based on individuals' employment history or the benefits are available only for a short period of time.[8] Most of the individuals with lifelong and severe disabilities therefore would not be eligible for the federal DI programs; and the eligible individuals would need more assistance since the federal programs provide benefits only for a short period of time. Provincial DI programs provide long term benefits for those who are not eligible for the federal DI programs or need more assistance.[9] Alberta, Ontario, British Columbia and Saskatchewan are among Canadian provinces that have provincial DI programs. Each of these programs are operated under different ministries, but they all provide similar DI benefits. Amount of the benefits and the size of the programs, however, differ substantially within the provinces, with Alberta and Ontario's program are respectively the most generous and the largest ones.

[8]Federal government's benefits include Employment Insurance (EI), Sickness benefits (one must have accumulated at least 600 hours of insurable employment in the qualifying period to receive up to 15 weeks of benefits), Canada Pension Plan (CPP) and Quebec Pension Plan (QPP) disability benefits (to be eligible, one must have enough contributions to the CPP/QPP), Child Disability benefit (CDB) (a tax-free benefit for families who care for a child under 18 with a severe and prolonged disability), Special Benefits for Parents of Critically Ill Children (PCIC) (for eligible parents who take leave from work to provide care or support to their critically ill or injured child for up to 35 weeks) and Employment Insurance Compassionate Care Benefits (for those take time off work to provide care or support to a family member who is gravely ill and is at risk of dying within six months). More information on federal government's disability benefit programs: `http://www.fcac-acfc.gc.ca/Eng/forConsumers/lifeEvents/livingDisability/Pages/Federalp-Prestati.aspx`, Accessed on Feb 29, 2016.

[9]More information on provincial DI programs: `http://www.fcac-acfc.gc.ca/Eng/forConsumers/lifeEvents/livingDisability/Pages/Resource-Ressourc.aspx`, Accessed on Feb 29, 2016.

Assured Income for the Severely Handicapped program in Alberta

The Assured Income for the Severely Handicapped (AISH) is Alberta's provincial DI program with about 40 thousands benefit recipients (about 1.5 percent of Alberta's adult population at 2008).[10] About half of the benefit recipients in AISH have non-physical disabilities. The education level of more than 80 percent of the benefit recipients is high school or less and more than 90 percent of the benefit recipients do not have dependents. Eligible individuals for the program must have a disability where no remedial therapy is available to materially improve their condition. AISH provides benefits to individuals and their family whom a disability causes a substantial limit in their ability to earn a living and are in financial needs. The program aims to enable benefit recipients to live as independently as possible in their communities.[11]

Determination Process AISH is a means tested DI program where eligible individuals are entitled to a prescribed amount of assistance. Eligibility is determined based on individuals' disability, age, income and assets. Eligible individuals must be 18 years and older and live in Alberta and be a Canadian citizen or permanent resident; where a permanent disability is the main cause limiting amount or kind of the work they can do and earn a living. Total assets of an eligible benefit recipient and their partner can not be worth more than \$100 thousands.[12] Individuals cannot collect Old Age Security (OAS) pension while they are in the program; benefits are transferred to the OAS pension once individuals are el-

[10]The following information on the AISH and ODSP programs is available from Human Resources and Skill Development Canada, Social Assistance Statistical Report: 2008, available on-line at `http://publications.gc.ca/collections/collection_2011/rhdcc-hrsdc/HS25-2-2008-eng.pdf`. Accessed at December 26, 2016.

[11]Provincial government of Alberta has also other programs to provide more support to disabled individuals. Employment First, Family Support for Children with Disabilities (FSCD), Fetal Alcohol Spectrum Disorder (FASD) initiatives, Persons with Developmental Disabilities (PDD), Provincial Disability Supports Initiatives and Residential Access Modification Program (RAMP) are provided in Alberta. More information on Alberta's DI programs: `http://www.humanservices.alberta.ca/disability-services/pdd.html`, Accessed at May 26, 2016.

[12]Verification of the financial assets of the benefit recipients is based on a honor system. Each benefit recipient must declare any monetary assets (i.e. saving accounts, bonds) by submitting monthly bank statement of the banking account which their DI benefits is deposited into.

igible to collect it. A final decision on individuals' application file is made by a social worker, after receiving all the relevant medical reports from a qualified health professional. Entitled individuals receive monthly benefits and supplemental assistance (i.e. health benefits, child care and subsidized transit).[13]

Duration of the benefits Once an individual is entitled to AISH, there are two main pathways out of the program. First, a benefit recipient may die. Second, they may no longer be eligible to receive the benefits. A benefit recipient may reach the retirement age (65 years) and be eligible to receive Guaranteed Income Support (GIS) or OAS pensions. A benefit recipient may no longer meet the medical or income and asset criteria to receive the benefits. Eligibility based exits account for a very small fraction of the exits from AISH.

The policy change in AISH The AISH program allows benefit recipients to work while they receive DI benefits. The earnings below an exemption threshold in AISH do not affect the DI benefits; but DI benefits are gradually deducted for the earnings accumulated above the exemption threshold. This is comparable to a non-linear tax schedule on earnings. The marginal tax rate on earnings below the exemption threshold is zero. The earnings above the exemption threshold up to the second earnings threshold are taxes at 50%; DI benefits are deducted $1 for every $2 earnings accumulated between exemption threshold and the second threshold. Earnings above the second threshold are taxed at 100%; DI benefits are deducted $1 for every $1 earnings accumulated above the second threshold. The earnings thresholds are higher for DI recipients with dependents. Effective from April 2012, the exemption threshold doubled and the maximum monthly DI benefits increased by 35 percent.[14] This policy change is comparable to decreasing marginal taxes in a non-linear tax schedule on earnings that induces incentives to work.

[13]More information on eligibility criteria in AISH: `http://www.alberta.ca/aish-eligibility.aspx`, Accessed on Nov 8, 2016.

[14]After Alberta's 2012 provincial election, the new premier of Alberta decided to shift the ministry responsible for AISH program from Seniors (to which it is now part of the new Health ministry) to the new Human Services ministry and implement the new policy in AISH.

Panel (a) of Figure 1.1 presents the budget constraint of DI recipients in AISH with no dependents before and after the policy change. The horizontal axis denotes the monthly earnings and the vertical axis denotes the total income including DI benefits and net monthly earnings. The maximum monthly DI benefits before the policy change is $1,188; it is increased by $400 to $1,588 after the policy change (35 percent increase). The earnings exemption threshold before the policy change is $400; in the new policy it is doubled to be at $800. The second earnings threshold has been at $1,500 since July 2008.[15] Panel (b) of Figure 1.1 presents the budget constraints for DI recipients with dependents. The maximum monthly DI benefits are the same as that for individuals with no dependents. The earnings thresholds before the policy change are at $975 and $2,500; the exemption threshold increased to $1,950 in the new policy.

1.2.2 Ontario Disability Support program

The Ontario Disability Support program (ODSP) is a comparable DI program to AISH in Ontario. The ODSP provides benefits to disabled individuals in Ontario whom a disability causes a substantial limit in their ability to earn a living. The eligibility criteria and determination process in ODSP are quite similar to those in AISH; and beneficiaries receive monthly benefits and supplementary assistance (i.e. health benefits, child care and subsidized transit).[16] The ODSP also allows benefit recipients to work while receiving DI benefits; but DI benefits are reduced by $1 for every $2 earnings. This is comparable to a flat 50% tax on all earnings. The maximum monthly DI benefits in the ODSP depend on the number of dependents varying from $1,086 to $1,999. Figure 1.2 shows the budget constraint of DI recipients in the ODSP.[17]

[15] At July 2008, the second earnings threshold in AISH increased by $500 to $1,500 for DI recipients with no dependents and to $2,500 for those with dependents.

[16] More information on Ontario's DI programs: `http://www.mcss.gov.on.ca/en/mcss/programs/social/odsp/index.aspx`, Accessed on May 26, 2016.

[17] This policy has been in effect since November 2006. At September 2013, a new policy implemented in the ODSP where an exemption threshold for monthly earnings is introduced at $200. Earnings above the exemption threshold are still subject to 50% marginal tax rate. In my DD analysis in Section 1.4, I also do

1.2.3 Data and sample selection

I use administrative data on monthly earnings of DI recipients in AISH and ODSP from the Governments of Alberta and Ontario within two years of the policy change in AISH from March 2010 to April 2014. I use the data from AISH to estimate heterogeneous adjustment costs. I then combine the data from AISH and ODSP for my DD analysis. Observing monthly earnings is essential for estimating adjustment costs since the earnings thresholds are monthly based. Both datasets also have detailed longitudinal information on individuals' characteristics including sex, age, marital status, family size, age entering into the DI program and the location of residence. These datasets furthermore include ICD-9 codes of DI recipients' disability conditions. This allows me to investigate the effects of incentives to work on labor supply of DI recipients with non-physical disabilities. Individuals with non-physical disabilities are believed to be the marginal entrants to DI programs and therefore are expected to be responsive to incentives to work. My study sample then includes 18 to 64 years old individuals with non-physical disabilities within two years of the April 2012 policy change in AISH from March 2010 to April 2014. The sample sizes in AISH and ODSP are respectively 452 thousands (10 thousands individuals over four years) and 6.9 millions (150 thousands individuals over four years). These sample sizes might look quite different but they are comparable in terms of percentage of the adult population in each province (about one percent).

Table 1.1 describes the data from DI recipients with non-physical disabilities in AISH and ODSP.[18] "Before" refers to the period before the policy change in AISH from April 2010 to March 2012 and "After" refers to the period after the policy change from April 2012 to March 2014. The first panel presents the labor market statistics. The mean monthly DI benefit in the both programs are quite similar before the policy change whereas it is higher

my analysis using a shorter time horizon to isolate the effects of this policy change. My main findings do not change.

[18]The size of the AISH and ODSP programs is about one percent of the adult population in the corresponding provinces. In each program, about half of the DI recipients have non-physical disabilities.

in AISH after the policy change. The labor supply in AISH both before and after the policy change are higher than the ODSP; about half of the DI recipients in AISH have positive earnings whereas it is less than ten percent in the ODSP. The mean inflation adjusted monthly earnings are also higher in AISH than ODSP. The labor supply in AISH after the policy change are higher than that before the policy change.

The second panel of Table 1.1 shows the individual background characteristics in AISH and ODSP before and after the policy change. There are no notable changes in DI recipients' characteristics after the policy change compared to those before the policy change in AISH and neither in the ODSP. About half of the DI recipients in both programs are female. The average age of DI recipients in AISH is 39 and the age of entering to the program is 29; whereas they are slightly higher in ODSP respectively at 43 and 42 years. In the both programs most of the benefit recipients do not have dependents. About half of the DI recipients in AISH live in metropolitan areas whereas it is about 30 percent in the ODSP.[19] I break down non-physical debilitates into three broad groups of psychic (i.e. Schizophrenia and Bipolar disorder), neurological (i.e. Autism and Down Syndrome) and mental conditions (i.e. Anxiety and Depression). The psychic and mental disabilities are respectively the largest and smallest groups.

1.3 Adjustment costs and elasticity of earnings

In this section, I first provide a conceptual framework to illustrate the interaction between adjustment costs and incentives to work, and its effects on individuals' labor supply decisions. I then provide suggestive graphical evidence that DI recipients in AISH face adjustment costs when changing their labor supply. I finally present my model for estimating heterogeneous adjustment costs using the amount of bunching at the exemption threshold before and after the policy change in AISH.

[19] The metropolitan area in Alberta includes Calgary and Edmonton and in Ontario includes Toronto and Ottawa.

1.3.1 Conceptual framework

I follow Chetty et al. (2011) and assume that individuals' preferences are described by a quasi-linear utility function $u(C, z; \tau, \alpha)$, where C and z respectively indicate consumption and earnings and α denotes individuals' ability to work. τ denotes the non-linear tax on earnings with a kink at z^*; the marginal tax on the earnings below and above z^* are respectively τ_0 and τ_1 where $\tau_1 > \tau_0$. Consumption C is:

$$C = \begin{cases} b + (1 - \tau_0)z & \text{if } z \leq z^* \\ b + (1 - \tau_0)z^* + (1 - \tau_1)(z - z^*) & \text{if } z > z^* \end{cases}$$

where b denotes lump-sum benefit. Individuals in fact choose earnings z[20] to maximize their utility. Suppose a policy change decreased the marginal tax on earnings above the threshold at z^* to τ_2 from τ_1; this generates incentives to work more. Panel (a) of Figure 1.3 shows an individual whose initial earnings is z^*. If she does not face any adjustment costs when changing her earnings, after the policy change she would then increase her earnings to z'.

Suppose now that individuals face heterogeneous adjustment costs $\phi(\alpha)$ that vary by their ability to work α; a utility loss $\phi(\alpha)$ is associated with adjustment costs. Individuals with higher ability face lower utility loss changing their earnings; for instance, they might have better opportunity for finding a new job or better bargaining power negotiating their hours of work with a current employer. Individuals would change their earnings only if their utility gain is higher than the utility loss associated with the adjustment costs they face. Panel (b) of Figure 1.3 illustrates that an individual with initial earnings in the interval $(\underline{z}, \bar{z})$ would not change her earnings since the utility gain of increase in earnings z is smaller than the utility loss associated with adjustment costs $\phi(\alpha)$ where α denotes the ability of an

[20]Individuals choose hours of work h for given wage w where earnings is $z = wh$.

individual with initial earnings z^*. $\underline{z}$ and $\bar{z}$ are described as:

$$u(C, z^*; \tau; \alpha) - u(C, \underline{z}; \tau; \alpha) = \phi(\alpha) \quad \text{with } \underline{z} < z^* \tag{1.1}$$

$$u(C, z^*, \tau; \alpha) - u(C, \bar{z}; \tau; \alpha) = \phi(\alpha) \quad \text{with } \bar{z} > z^* \tag{1.2}$$

Panel (c) of Figure 1.3 illustrate a case where a decrease in marginal tax rate above the kink is accompanied by an increase in lump-sum transfer of the amount of ψ; which increases individuals' utility by ψ. This might increase the gain of the relocation for some individuals with the initial earnings in the interval $(\underline{z}, \bar{z})$ and therefore, they might increase their earnings.

A quasi-linear utility function, however, ignores the income effect induced by a policy change. In Appendix A.4 I provide suggestive evidence that the induced income effect of the policy change in AISH is negligible. This simple framework illustrates that if induced incentives to work by a policy change are large enough to offset the associated adjustment costs, then a policy change can increase the labor supply.

1.3.2 Graphical evidence

Figure 1.4 plots the distribution of monthly earnings of DI recipients in AISH with no dependents two years before and two years after the policy change. The sample includes individuals 18 years and older with no dependents who have non-physical disabilities. The higher marginal tax rate on the earnings above a kink creates strong incentives for many individuals to locate their earnings right below the kink. Excess mass at a kink is known as "bunching." There is noticeable bunching at the exemption threshold every month before the policy change. There is, however, no noticeable bunching at the second kink.[21]

Figure 1.4 also shows that bunching at the exemption threshold gradually moves away

[21] The second earning threshold increased to $1,500 from $1,000 at July 2008, three years prior to the policy change of interest at April 2012. There is also no bunching at the former kink at $1,000 (%50 and %100 marginal taxes respectively below and above the kink).

toward the new exemption threshold after the policy change, but the bunching at the old exemption threshold does not completely disappear, even two years after the policy change. Figure 1.5 plots the distribution of monthly earnings for the pooled sample two years before and two years after the policy change.[22]

Bunching at the old exemption threshold is unlikely to be driven by higher marginal utility of leisure relative to working; since bunching at the old exemption threshold gradually fades away at months following the policy change. It is also unlikely to be driven by change in individuals' preferences to work. It also is unlikely to be due to lack of information on the policy change. Since those who bunch at the exemption threshold are the first to realize the changes in their pay check. Bunching at the old exemption threshold is then a suggestive evidence that DI recipients in AISH face adjustment costs when changing their labor supply. Findings of the several recent papers also suggest that individuals face adjustment costs when changing their behaviour in response to a policy change (see for instance, Chetty, Looney and Kroft, 2009; Chetty, Friedman, Olsen and Pistaferri, 2011; Chetty, Guren, Manoli and Weber, 2012; Chetty, 2012; Chetty, Friedman and Saez, 2013; Kleven and Waseem, 2013). Utility loss associated with adjustment costs decreases the utility gain of changing labor supply and therefore some individuals might not change their labor supply.

1.3.3 Heterogenous adjustment costs and elasticity of earnings

In this section, I present my model for estimating elasticity of earnings and heterogeneous adjustment costs that vary by individuals' ability to work. Individuals' ability is measured as their potential earnings if no tax had been imposed on them. I explore the policy change in AISH and use the amount of bunching at the exemption threshold before

[22]Figure A.1 and A.2 plot the corresponding distributions of earnings for DI recipients with dependents. There is no noticeable bunching at none of the kinks before the policy change, neither at the kinks after the policy change. This could be caused by small sample size since, as shown in Table 1.1, less than ten percent of the whole sample have dependents. It also could be that DI recipients with dependents have another source of income (i.e. their partner's income) and might not be responsive to the incentives to work. For the rest of my empirical analysis on the adjustment costs, I use only DI recipients with no dependents. For evaluating the overall effects of the policy change in AISH in my DD analysis, I use both those with and with no dependents.

and after the policy change for my estimation.

Saez (2010) estimates an elasticity of earnings by exploring an assumed proportional relationship between elasticity of earnings and the amount of bunching at a kink.[23] Bunching at a kink conceptually increases by elasticity of earnings but also decreases by the size of adjustment costs. Gelber, Jones, Sacks and Song (2016) extend Saez (2010) to develop a novel framework to simultaneously estimate the elasticity of earnings and fixed adjustment costs. They explore a policy change in the Social Security Annual Earnings Test (AET) in the US. where the marginal tax rate above a kink is decreased. They assume that individuals face a fixed adjustment costs when they change their labor supply. They then use the amount of bunching at the kink before and after the policy change to estimate the elasticity of earnings with respect to net-of-tax ratio and the fixed adjustment costs.

Assuming that all individuals faces the same adjustment costs might be a fair assumption by Gelber et al. (2016), since their study sample is relatively more homogeneous (62-69 years old individuals). Allowing for heterogeneity in adjustment costs that vary by individuals' ability to work might be more plausible in the context of a DI program, specially for DI recipients with non-physical disabilities. Most of the non-physical disabilities are hard to verify and therefore those in a DI program might differ in the level of their ability to work and the adjustment costs they face when changing their labor supply. I extend Gelber et al. (2016) and estimate heterogeneous adjustment costs that vary by individuals' ability to work. Intuitively, observing more moments of bunching allows me to estimate more parameters than theirs. Better understanding of heterogeneous adjustment costs has important policy implications in designing policies to increase labor supply and targeting heterogeneous groups in DI programs. Some groups of DI recipients might be in need for more support to be able to work more while some others would not work regardless of the support provided for them.

[23] I also estimate elasticity of earnings with no adjustment costs to compare with my estimates with heterogeneous adjustment costs. More details on the model with no adjustment costs is provides in Appendix A.2.1.

Individual utility function

The utility function that has been used in most of the related literature (see for instance Saez, 2010; Chetty, Friedman, Olsen and Pistaferri, 2011; Gelber, Jones, Sacks and Song, 2016; Kleven and Waseem, 2013) is a quasi-linear, iso-elastic utility function:

$$u(C, z; \tau; \alpha) = C - \alpha^{-\frac{1}{e}} \frac{z^{(1+\frac{1}{e})}}{1+\frac{1}{e}} - \phi(\alpha)\mathbb{1}\{\text{change labor supply}\} \tag{1.3}$$

C and z are respectively represent consumption and earnings and τ denotes the non-linear tax on earnings. e denotes the elasticity of earnings with respect to net-of-tax ratio at a kink. α is a parameter of the utility function that reflects heterogeneous ability to work. $\mathbb{1}(.)$ denotes the indicator function. Individuals lose utility $\phi(\alpha)$ if they change their labor supply that varies by their ability to work α. The consumption is defined as $C = z - T(z)$ where $T(z)$ denotes the tax liability:

$$T(z) = \begin{cases} \tau_0 z & \text{if } 0 \leq z \leq z_1 \\ \tau_0 z_1 + \tau_1(z - z_1) & \text{if } z_1 < z \leq z_2 \\ \tau_0 z_1 + \tau_1(z_2 - z_1) + \tau_2(z - z_1 - z_2) & \text{if } z > z_2 \end{cases}$$

where $\tau_0 = 0, \tau_1 = 0.5$ and $\tau_2 = 1$ in AISH. For those with no dependents, the kinks before the policy change are $z_1 = \$400$, $z_2 = \$1,500$ and kinks after the policy change are $z_1 = \$800$ and $z_2 = \$1,500$.[24]

Individuals maximize their utility subject to consumption budget constraint. The corresponding first order condition implies that for an individual with ability α, the utility maximizing level of earnings and the corresponding utility with marginal tax τ on earnings

[24]The corresponding kinks for individuals with dependents are \$975 and \$2,500 before the policy change and \$1,950 and \$2,500 after the policy change.

respectively are:

$$
\begin{aligned}
z &= \alpha(1-\tau)^e \\
u(C, z; \tau; \alpha) &= \alpha\frac{(1-\tau)^{1+e}}{1+e}
\end{aligned}
\tag{1.4}
$$

Setting $\tau = 0$ results in $z = \alpha$, individuals' potential earnings with no tax on earnings then measures individuals' ability to work.

This utility function rules out the income effects, I therefore disregard the monthly DI benefits from the model.[25] This utility function also ensures that the utility gain of relocating to a kink is increasing with the distance to the kink (See Theorem (1)). I follow previous work and assume that individuals' ability to work has a smooth distribution.[26] A smooth distribution of ability implies that distribution of earnings with a flat tax τ_0 on earnings is smooth and continuous. I also assume that the heterogeneity in earnings z stems only from heterogeneity in ability α.

The model

Assume that individuals face heterogeneous adjustment costs $\phi(\alpha)$ in the form of utility loss when they change their labor supply. The associated utility loss varies by individuals' ability α. A marginal bucher at a kink at z^* with initial earnings $z > z^*$ is indifferent between staying at z –where marginal tax on earnings is higher– or enduring adjustment cost and reducing their earnings to z^*, where marginal tax on earnings is lower. In the following, z_1^* and z_2^* denote respectively the old and the new exemption thresholds. Panel (a) of Figure 1.6 shows a marginal buncher with ability $\alpha^{m_1^0}$ at the kink at z_1^*. The initial earnings of a marginal buncher – if flat tax τ_0 would have been imposed on her – is $\underline{z}_1^0$ and she is indifferent between staying at $\underline{z}_1^0$ – where marginal tax on earnings is higher – or enduring utility loss

[25] I provide suggestive evidence in Appendix A.4 that the induced income effects of the policy change in AISH is ignorable.

[26] See for instance Saez (2010); Chetty, Friedman, Olsen and Pistaferri (2011); Gelber, Jones, Sacks and Song (2016); Kleven and Waseem (2013).

$\phi(\alpha^{m_1^0})$ and decreasing her earnings to z_1^* where marginal tax on earnings is lower. The following equation (marginal buncher condition at z_1^*) implicitly defines $\underline{z}_1^0$:

$$u\left((1-\tau_0)z_1^*, z_1^*; \tau_1; \alpha^{m_1^0}\right) = u\left((1-\tau_0)z_1^* + (1-\tau_1)(\underline{z}_1^0 - z_1^*), \underline{z}_1^0; \tau_1; \alpha^{m_1^0}\right) + \phi(\alpha^{m_1^0}) \quad (1.5)$$

Suppose that individuals with initial earnings in range of $(z_1^*, z_1^* + \Delta z_1^*]$ would bunch at the kink at z_1^* if no adjustment costs is associated with changing earnings. When individuals face adjustment costs changing their earnings, Theorem (1) implies that those with initial earnings in range of $(\underline{z_1}^0, z_1^* + \Delta z_1^*]$ gain from relocating to z_1^*. This theorem imposes mild assumptions on individuals' utility function $u(.)$. A proof is presented in Appendix A.1.

Theorem 1. *Suppose utility loss $\phi > 0$ is associated with adjusting earnings when kink $z^* = (\tau_0, \tau_1)$ is introduced where $\tau_1 > \tau_0$ and $u(c, z; \tau; \alpha)$ is individuals' utility with $\dfrac{\partial u_c}{\partial \alpha} < 0$ (marginal utility of consumption decreases as ability increases). If for $z_2 > z_1$, $\dfrac{\partial(z_2 - z_1)}{\partial \alpha}$ increases at a rate that dominates $\dfrac{\partial u_c}{\partial \alpha} < 0$, then utility gain of relocation to z^* for initial earning level z_2 is higher than that at z_1.*

Suppose that $h(z)$ is the observed distribution of earnings when there is a kink at z_1^* and $h_0(z)$ is the counter-factual distribution of earnings if a flat tax τ_0 would have been imposed on all earnings. The amount of bunching at the kink at z_1^* then is the area under the counter-factual distribution of earnings in the bunching range (bunching equation):

$$B_1^0 = \int_{\underline{z}_1^0}^{z_1^* + \Delta z_1^*} h_0(\zeta) d(\zeta) \approx (z_1^* + \Delta z_1^{*0} - \underline{z}_1^0) h_0(z_1^*) \quad (1.6)$$

Figure 1.7 shows that the bunching range at the kink at z_1^* is smaller when individuals face adjustment costs. The bunching range in absence of adjustment costs would have been $i + ii + iii$ where it is $ii + iii$ if individuals face adjustment costs. Equation (1.5) and (1.6) together describe an equation of earnings elasticity e and parameters of the adjustment costs.

I construct similar marginal buncher and bunching equations for the bunching at the old and new exemption thresholds after the policy change. The policy change shifted forward the old exemption threshold at z_1^* to the new one at z_2^*. This is comparable to decreasing marginal taxes in a non-linear tax schedule on earnings. Those who bunch at the kink at z_1^* increase their earnings if their utility gain from relocation exceeds the utility loss associated with the adjustment costs. Panel (b) of Figure 1.6 shows a marginal buncher at the old exemption threshold after the policy change, with ability $\alpha^{m_1^1}$ and initial earnings $\underline{z}_1^1$ in range of $(\underline{z}_1^0, z_1^* + \Delta z_1^*]$. The marginal buncher is indifferent between continuing to bunch at the old exemption threshold at z_1^* or enduring utility loss $\phi(\alpha^{m_1^1})$ and changing her earnings to her optimal earnings $\underline{z}_1^{1\prime}$ with the new taxes. The following equation implicitly defines $\underline{z}_1^1$:

$$u\left((1-\tau_0)\underline{z}_1^{1\prime}, \underline{z}_1^{1\prime}; \tau_0; \alpha^{m_1^1}\right) = u\left((1-\tau_0)z_1^*, z_1^*; \tau_0; \alpha^{m_1^1}\right) + \phi(\alpha^{m_1^1}) \tag{1.7}$$

Under mild assumptions about the underlying utility function $u(.)$, Theorem (1) implies that individuals with higher initial earnings gain more from changing their earnings. Those with initial earnings in range of $(\underline{z}_1^0, \underline{z}_1^1]$ continue bunching at the former kink at z_1^*. Figure 1.7 shows that the amount of bunching at the former kink at z_1^* is:

$$B_1^1 = \int_{\underline{z}_1^0}^{\underline{z}_1^1} h_0(\zeta)d(\zeta) \simeq (\underline{z}_1^1 - \underline{z}_1^0)h_0(z_1^*) \tag{1.8}$$

Equation (1.7) and (1.8) together describe another equation of elasticity of earnings e and parameters of adjustment cost.

If no adjustment costs is associated with changing earnings, individuals with initial earnings in range of $(z_2^*, z_2^* + \Delta z_2^*]$ would bunch at the new exemption threshold at z_2^* after the policy change. Panel (c) of Figure 1.6 shows a marginal buncher at the kink at z_2^*. The initial earnings of a marginal buncher with ability α^{m_2} is $\underline{z}_2$ in range of $(z_2^*, z_2^* + \Delta z_2^*]$. After imposing an exemption threshold at z_1^*, a marginal buncher changes her earnings from $\underline{z}_2$ to her optimal earnings with marginal tax τ_1 at $\underline{z}_2{}'$ when the exemption threshold is increased to z_2^* from z_1^*. A marginal buncher is then indifferent between staying at $\underline{z}_2{}'$ with marginal tax τ_1 or enduring adjustment costs $\phi(\alpha^{m_2})$ and decreasing her earnings and bunch at the kink at z_2^*. The following equation implicitly defines $\underline{z}_2$:

$$u((1-\tau_0)z_2^*, z_2^*; \tau_1; \alpha^{m_2}) = u((1-\tau_0)\underline{z}_2{}', \underline{z}_2{}'; \tau_1; \alpha^{m_2}) + \phi(\alpha^{m_2}) \tag{1.9}$$

Theorem (1) implies that the gain of relocation to z_2^* is higher for those with higher initial earnings. Figure 1.7 shows that those with initial earnings in range of $(\underline{z}_2, z_2^* + \Delta z_2^*]$ would bunch at z_2^*. The amount of bunching at the kink at z_2^* then is:

$$B_2 = \int_{\underline{z}_2}^{z_2^*+\Delta z_2^*} h_0(\zeta)d\zeta \approx (z_2^* + \Delta z_2^* - \underline{z}_2)h_0(z_2^*) \tag{1.10}$$

Equation (1.9) and (1.10) together describe another equation of elasticity of earnings e and parameters of the adjustment costs. I generalize adjustment costs to include both a fixed costs element ϕ_1 and a variable costs element that vary by individuals' ability to work $\alpha\phi_2$ defined as $\phi(\alpha) = \phi_1 + \alpha\phi_2$.[27] I numerically solve the three equations obtained from each bunching moment to estimate the elasticity of earnings with respect to net-of-tax ration e and parameters of the adjustment costs ϕ_1 and ϕ_2.[28]

Measuring amount of bunching at a kink

The crucial underlying assumption for using the amount of bunching at a kink at z^* to estimate structural parameters of a utility function is that the distribution of earnings would be smooth and continuous if a flat tax would have been imposed on earnings. The marginal taxes on earnings below and above z^* are respectively τ_0 and τ_1 where $\tau_1 > \tau_0$. I follow previous work and assume that the ability of individuals is smoothly distributed. This

[27]Kleven (2016) provides a survey of recent works on bunching and suggests extending Gelber et al. (2016) with a similar generalization of adjustment costs.

[28]More details on empirical implementation of the model is provided in Appendix A.2.2.

assumption translates into a smooth distribution of earnings z with CDF $H(z)$ and PDF $h(z)$.[29] Suppose that $h(z)$ is the observed distribution of earnings with a kink at z^*. Assume also that $h_0(z)$ is the counter-factual distribution of earnings if flat tax τ_0 would have been imposed on earnings. Then using the utility function specified in (1.3):[30]

$$h(z) = \begin{cases} h_0(z) & \text{if } \alpha < \frac{z^*}{(1-\tau_0)^e}, z < z^* \\ (\frac{1-\tau_0}{1-\tau_1})^e h_0((\frac{1-\tau_0}{1-\tau_1})^e z) & \text{if } \alpha > \frac{z^*}{(1-\tau_1)^e}, z > z^* \end{cases}$$

When there is a kink at z^*, individuals with ability α in interval $[\frac{z^*}{(1-\tau_0)^e}, \frac{z^*}{(1-\tau_1)^e}]$ would bunch at a neighbourhood of z^*. The initial earnings range of bunchers at a kink at z^*, Δz^* is:

$$\Delta z^* = \left(\left(\frac{1-\tau_0}{1-\tau_1}\right)^e - 1\right) z^* \tag{1.11}$$

Amount of bunching at a kink at z^* is the difference between the observed and counter-factual distribution of earnings. I follow Chetty et al. (2011); Kleven and Waseem (2013); Gelber et al. (2016) to construct the counter-factual distribution of earnings using the observed distribution of earnings. I divide the observed monthly earnings into z_i bins with width δ and p_i is portion of individuals with earnings in range of $[z_i - \delta/2, z_i + \delta/2]$. I then fit a flexible polynomial of degree D to the observed distribution of earnings at a neighbourhood $Q = [Q^l, Q^u]$ of z^* by estimating the following regression:

$$p_i = \sum_{d=0}^{D} \beta_d (z_i - z^*)^d + \sum_{j=-l}^{l} \gamma_j \mathbb{1}\{z_i - z^* = \delta j\} + \epsilon_i \tag{1.12}$$

---

[29] Assume CDF and PDF of α are respectively $F(\alpha)$ and $f(\alpha)$. Since $H(z) = \Pr(Z < z) = \Pr(\alpha(1-\tau)^e < z) = \Pr(\alpha < \dfrac{z}{(1-\tau)^e}) = F(\dfrac{z}{(1-\tau)^e})$. Therefore, $h(z) = H'(z) = \frac{1}{(1-\tau)^e} f(\frac{z}{(1-\tau)^e})$.

[30] For $\alpha < \frac{z^*}{(1-\tau_1)^e}$ and $z < z^*$ marginal tax on earnings after introducing kink z^* is still τ_0 and therefore $h(z) = h_0(z)$. Since for $\alpha > \frac{z^*}{(1-\tau_1)^e}$ and $z > z^*$, $H(z) = \Pr(\alpha(1-\tau_1)^e < z) = \Pr(\alpha < \frac{z}{(1-\tau_1)^e}) = F(\frac{z}{(1-\tau_1)^e})$, therefore $h(z) = H'(z) = \frac{1}{(1-\tau_1)^e} f(\frac{z}{(1-\tau_1)^e})$. Since $h_0(z) = \frac{1}{(1-\tau_0)^e} f(\frac{z}{(1-\tau_0)^e})$, then $h(z) = (\frac{1-\tau_0}{1-\tau_1})^e h_0((\frac{1-\tau_0}{1-\tau_1})^e z)$.

where $\mathbb{1}(.)$ is the indicator function denoting dummies for the bunching bins around the kink. Including dummies in the regression for the bins around the kink in range $[z^* - \delta l, z^* + \delta u]$ isolates the effects of the bunching bins on the estimated counter-factual distribution of earnings. l and u indicate the number of excluded bins respectively below and above the kink. These parameters are chosen by visual inspection of the observed distribution of earnings. The counter-factual distribution of earnings is then the fitted values from (1.12) where contribution of the bunching bins around the kink is excluded and is defined as:

$$\hat{p_i} = \sum_{d=0}^{D} \hat{\beta}_d (z_i - z^*)^d$$

An initial estimate of the amount of bunching at a kink at z^* is defined as:

$$\hat{B}^0 = \delta \sum_{j=l}^{u} (p_j - \hat{p_j}) = \delta \sum_{j=l}^{u} \hat{\gamma}_j$$

However, the estimated bunching $\hat{B}^0$ overestimates the true amount of bunching at a kink since it does not account for the fact that those who bunch at a kink would have located at points to the right of the threshold if flat tax τ_0 would have been imposed. Furthermore, when a kink is shifted forward, those who bunch at the new kink have moved from points to the left of the threshold. Therefore, the area under the estimated counter-factual distribution is not equal to the area under the observed empirical distribution (called integration constraint in Chetty et al., 2011). I use a technique proposed by Chetty et al. (2011) and shift the estimated counter-factual distribution to the right of z_1^* upward and to the left of z_2^* upward until the integration constraint holds. To do this, I estimate the following equations recursively (n is iteration counter) using the observed distributions of earnings respectively before and after the policy change:

$$\begin{aligned} p_i \cdot (1 + \mathbb{1}\{i > u_1\}\frac{\hat{B_1}^{n-1}}{\sum_{q>u_1} p_q}) &= \sum_{d=0}^{D} \beta_d^n (z_i - z_1^*)^d + \sum_{j=l_1}^{u_1} \gamma_j^n \mathbb{1}\{z_i - z_1^* = \delta j\} + \epsilon_i \\ p_i \cdot (1 + \mathbb{1}\{i < l_2\}\frac{\hat{B_2}^{n-1}}{\sum_{q<l_2} p_q}) &= \sum_{d=0}^{D} \beta_d^n (z_i - z_2^*)^d + \sum_{j=l_2}^{u_2} \gamma_j^n \mathbb{1}\{z_i - z_2^* = \delta j\} + \epsilon_i \end{aligned} \tag{1.13}$$

where $\hat{B_1}^{n-1}$ and $\hat{B_2}^{n-1}$ are the estimated bunching respectively at z_1^* and z_2^*. The stopping criteria for the recursion is that the area under the estimated counter-factual distribution be equal to the area under the empirical one as $\sum_{i \in Q} p_i = \sum_{i \in Q} \hat{p}_i$. The estimated amount of bunching at a kink at z^* at step n of the recursion then is:

$$\hat{B}^n = \delta \sum_{j=l}^{u} (p_j - \hat{p}_j) = \delta \sum_{j=l}^{u} \hat{\gamma_j}^n$$

The estimated counter-factual distribution of earnings at a kink at z^* using (1.13) is $h_0(z)$ and is defined as:

$$\begin{aligned} h_0(z) &= \sum_{d=0}^{D} \hat{\beta}_d (z - z^*)^d \\ h_0(z^*) &= \hat{\beta}_0 \end{aligned} \tag{1.14}$$

where the amount of bunching at a kink at z^* which satisfies the integration constraint is:

$$\hat{B} = \delta \sum_{j=l}^{u} \hat{\gamma}_j \tag{1.15}$$

I normalize the estimated bunching $\hat{B}$ by dividing it by the counter-factual mass at z^* bin from (1.14) to obtain a comparable measure of bunching within the kinks. The normalized bunching $\hat{b}$ at the kink at z^* is defined as:

$$\hat{b} = \frac{\hat{B}}{\delta h_0(z^*)} = \frac{\hat{B}}{\delta \hat{\beta}_0} \tag{1.16}$$

1.3.4 Estimating elasticity of earnings and heterogeneous adjustment costs

Estimation assumptions

Estimating elasticity of earnings and adjustment costs using the amount of bunching at a kink relies on the assumption that if a flat tax would have been imposed on earnings – in absence of the kinks – the distribution of earnings would have been continuous and smooth. Another key parametric assumption is that the adjustment costs and elasticity of earnings are the same at all kinks, both before and after the policy change. I also assume that the induced income effects of the policy change is negligible and use a quasi-linear utility function.[31] I furthermore make one more simplifying assumption; I assume that the payroll tax on earnings of the DI recipients is zero. Annual earnings of almost all of the DI recipients falls in the lower bracket of the income tax schedule of the federal and provincial governments in Canada. Any individual who has earnings, however must pay for the Employment Insurance (EI) (about 2-5% in the lower bracket of the tax schedule). This is too small relative to the marginal tax rates below and above the exemption threshold and is unlikely to affect the estimates. However, I check robustness of my findings by including %5 payroll tax.

Inference

I estimate bootstrapped standard errors to make inference about the estimated parameters. I calculate standard errors using a parametric bootstrapping procedure described by Chetty et al. (2012). I draw 200 times with replacement from the estimated vector of errors ϵ_i from (1.13) to generate new earnings distributions. For each bootstrapped distribution

[31]In appendix A.4 I provide suggestive evidence that the induced income effects of the policy change in AISH are negligible.

then, I estimate the parameters of interest.[32] I define standard error of a parameter θ as the standard deviation of its bootstrapped distribution $S_{\hat{\theta}}$. These standard errors reflect the misspecification of the fitted polynomial to the observed distribution of earnings rather than sampling error. To test whether an estimated parameter $\hat{\theta}$ is significantly different than zero $H_0 : \theta \neq 0$, I construct test statistic $T = \frac{\hat{\theta}}{S_{\hat{\theta}}}$ for each bootstrapped distribution. The bootstrapped critical values at level α are the lower $\alpha/2$ and the upper $\alpha/2$ quantiles of the ordered bootstrapped test statistics. I then determine whether an estimate is significantly different from zero within a $100(1-\alpha)$ confidence interval if the corresponding t-statistic lies within the critical values at level α.

Estimation results

Study sample in the main estimates includes DI recipients in AISH with 18 years and older with no dependents who have non-physical disabilities, within two years of the policy change. To estimate the amount of bunching at each kink, I set the bin size $\delta = 10$ and fit a polynomial degree $D = 6$ where $l = u = 3$ bins at each sides of a kink are excluded.[33] Figure 1.9 shows the estimated normalized bunching at the exemption thresholds before and after the policy change respectively at Panel (a) Panel (b). The horizontal axis denotes the month relative to the policy change in AISH and the vertical axis denotes the estimated normalized bunching at the corresponding exemption threshold using the method described in Section 1.3.3. Estimated bunching at the old exemption threshold is quite stable before the policy change. It gradually decreases during the months proceeding the policy change but it does not completely disappear. There is no bunching at location of the new exemption threshold before the policy change but it starts to gradually increase after the policy change. Gradual change in bunching and the fact that the estimated bunching at the old exemption threshold after the policy is still significant, suggests that individuals face adjustment costs

[32] I repeat the procedure explained in detail in Appendix A.2.2 for each bootstrapped distribution of earnings to estimate the parameters of the interest.

[33] I investigate robustness of the estimated amount of bunching to the selected parameters in Table A.2.

when changing their labor supply.

The first row of Table 1.2 presents the estimated elasticity of earnings with respect to net-of-tax ratio at a kink and the heterogeneous adjustment costs that vary by individuals' ability to work. I use the data within two years of the policy change in AISH for this estimation. The estimated heterogeneous adjustment costs is $\phi = 20.69 - 0.02\alpha$ where α denotes individuals' ability to work. The estimated adjustment costs are higher for individuals with lower ability. Figure 1.10 plots the estimated adjustment costs as percentage of the potential earnings. The estimated adjustment costs vary from zero to 8 percent of the potential earnings. Table 1.2 also shows that the estimated elasticity of earnings with respect to net-of-tax ratio – accounting for heterogeneous adjustment cost – is 0.19.

Table 1.2 shows that the estimates do not change much using data within a year of the policy change nor including %5 payroll tax. This table also presents the estimated elasticity of earnings and heterogeneous adjustment costs broken down by age, gender, disability type and location of residence. The estimates are slightly higher for older, males and DI recipients living in metropolitan areas. Heterogeneity in estimated elasticity and adjustment costs within disability types however is striking. The estimates are considerably higher for those with Psychotic disabilities among the others. The estimated elasticity for DI recipients with psychotic disabilities is 0.50 and adjustment costs vary from zero to more than half of the potential earnings. The estimated elasticity for individuals with mental disabilities is 0.33 and adjustment costs vary from zero to more than one-third of their potential earnings. The estimates for DI recipients with neurological disabilities are quite similar to those estimated for the whole sample, elasticity of earnings at 0.15 and adjustment costs that vary from zero to 10 percent of the potential earnings.

Estimated elasticity of earnings with fixed adjustment costs (Gelber et al., 2016) is presented in Panel (a) of Table A.1 in Appendix A.2.[34] Estimated elasticity of earnings for the

[34]Figure A.3 plots the estimated elasticity of earnings using the model of Saez (2010), assuming that individuals face no adjustment costs changing their labor supply. The horizontal axis denotes the relative

whole sample is quite similar to the one estimated with heterogeneous adjustment costs (0.21 versus 0.19) and the fixed adjustment costs is about 4 percent of the average earnings. My estimated elasticity of earnings is similar in magnitude to Gelber et al. (2016) but estimated adjustment costs are much larger (%4 versus %0.5 of the average earnings). This table also shows the estimates broken down by age, sex, disability type and location of residence. Estimates are quite heterogeneous among disability types. Estimated elasticity of earnings and fixed adjustment costs for DI recipients with mental disabilities are the largest among the others and are respectively 0.54 and 16 percent of the average potential earnings.

Panel (b) of Table A.1 in Appendix A.2 shows the estimated elasticity of earnings assuming that individuals do not face adjustment costs changing their labor supply (Saez, 2010). The estimated elasticity for the whole sample is 0.10, about half of the one estimated with heterogeneous adjustment costs. This table also shows the estimated elasticity of earnings broken down by age, gender, disability type and living location. Estimated elasticities for all the sub-samples are smaller than those estimated with heterogeneous adjustment costs.

My estimates show that there is considerable heterogeneity in adjustment costs among DI recipients. Individuals with higher potential earnings face lower adjustment costs when changing their labor supply. It could be that individuals with higher ability have a better chance for finding a job or stronger bargaining power in negotiating their wage or hours of work with a current employer. It also could be that they might need less support and workplace accommodation to work. The estimated heterogeneous adjustment costs are larger than the fixed ones. The estimated adjustment costs might seem quite small, but accounting for adjustment costs doubles the size of the estimated elasticity of earnings. For estimating adjustment costs, I use a sample of DI recipients who bunch at an exemption threshold. These individuals are relatively more flexible in changing their labor supply than the others.

month to the policy change and the vertical axis denotes the estimated elasticity of earnings with respect to net-of-tax ratio. Estimated elasticity of earnings at new exemption threshold increases gradually as the amount of bunching increases. More details on the models with fixed adjustment costs and a model with no adjustment costs are provided in Appendix A.2.

Evidence on existence of adjustment costs even for them magnifies the importance of the adjustment costs. Short term responses to incentives to work even to large incentives might be attenuated by adjustment costs. Furthermore, effectiveness policies that aim in increasing labor supply in DI programs would depend on size of the induced incentives to work versus size of adjustment costs that DI recipients face when changing their labor supply.

1.4 Labor supply responses to incentives to work

Estimates using the amount of bunching around an exemption threshold provide an incomplete picture of the effects of the policy change in AISH on labor supply; since the policy change also deceased the marginal tax rate on earnings far away from the exemption threshold. Furthermore, the policy change might also have extensive margin effects; some individuals might start working. Examining the overall effects of increase in incentives to work on labor supply in a DI program, however, is challenging. First, individuals' labor supply is endogenous since, selection process into a DI program strongly depends on having low labor supply. Second, adjustment costs attenuate the induced incentives to work by a policy change (Chetty, 2012). The policy change in AISH creates an opportunity to investigate the potential to induce greater labor supply when individuals face adjustment costs.

1.4.1 Identification strategy

I estimate the causal effects of the policy change on the labor supply using Difference-in-Differences (DD) design. I use DI recipients of the Ontario Disability Support Program (ODSP) – another provincial DI program in Canada – as the control group. The ODSP is an appropriate control group since its benefit scheme is similar to –but less generous than– AISH; and did not go under major policy changes during the period of my study. The first difference is over time, as the incentives to work increased in the AISH program after April 2012. The second difference is across provinces; there was a policy change in the AISH program in Alberta but not in the ODSP program in Ontario. The control group should capture the counter-factual labor market trends in the absence of the policy change. I implement a DD comparison by estimating a regression of the form:

$$y_{it} = \alpha + \beta(POST_t \times AISH_{it}) + \gamma AISH_{it} + X'_{it}\delta + \lambda_t + \epsilon_{it} \tag{1.17}$$

where i and t respectively denote individuals and monthly time. y_{it} denotes abor supply outcomes of interest which includes inflation adjusted earnings and labor force participation defined as a dummy that switches on for the positive earnings. $AISH_{it}$ is a dummy variable for the treatment group, DI recipients of AISH. This variable controls for program specific trends and is equal to one for those in the AISH program and zero otherwise. $POST_t$ is another dummy variable that turns on after the policy change. I also include a vector of time fixed effects λ_t to control for the changes in macroeconomics conditions. The vector X_{it} is a set of individual characteristics to control for any observable differences that might confound the analysis (sex, age, family structure, age entered to DI program at, disability type and location of residence). ϵ_{it} captures any unobserved factors affecting individuals' labor supply such as their ability or taste for work. The coefficient of interest is β which measures the effects of the policy change on labor supply of DI recipients in AISH relative to those in ODSP overt time.

To further explore impact of the policy change in AISH over time, I generalize (1.17) by replacing $POST_t \times AISH_{it}$ with a full set of treatment and quarterly time interaction terms and estimating a regression of the form:

$$y_{it} = \alpha + \sum_{t=-8}^{t=7} \beta_t(q_t \times AISH_{it}) + \gamma AISH_{it} + X'_{it}\delta + \lambda_t + \epsilon_{it} \tag{1.18}$$

where q_t is a dummy that is one in quarter t relative to the policy change and zero otherwise. The pre-policy change interaction terms provide pretreatment specification tests. The identification assumption is that there are no unobserved program specific change that first, are correlated with the policy change and second, are correlated with program specific changes in the outcome variable.

1.4.2 Results

Descriptive evidence

To graphically asses impact of the policy change in AISH on the labor supply, I plot trends in the inflation adjusted earnings and labor force participation among DI recipients in AISH and the ODSP in Figure 1.11, within two years of the policy change in AISH from April 2010 to March 2014. Panel (a) shows that earnings in the ODSP are fairly constant before and after the policy change. In the months following the policy change earnings of DI recipients of AISH however gradually begin to rise. Gradual increase in earnings provides an evidence that DI recipients face adjustment costs when changing their labor supply in intensive margins. A similar pattern also is observed in extensive margin of the labor supply –measured by labor force participation– in Panel (b). Labor force participation rates in the ODSP are quite low and fairly constant both before and after the policy change in AISH where participation rate slightly increases in AISH after the policy change. The policy change in AISH came to effect in April 2012, but it was publicly announced two months in advance in February 2012. Since individuals had little time to adjust their earnings or start to work, there is no observable evidence of anticipation effect in earnings neither in labor force participation.

Results

I present my main findings from estimating (1.17) in Table 1.3. The dependent variables are monthly inflation adjusted earnings and labor force participation (a dummy that switches on for positive earnings and zero otherwise). The effect of the policy change on earnings measures the intensive margins where the effect on labor force participation measures the extensive margins. The pre-period in the base specification is April 2010 to March 2012 and the post-period is April 2012 to March 2014.

The first column of Table 1.3 shows my main estimate of the effects on earnings from the policy change in AISH. The estimated effect is 12 percent increase in mean earnings of AISH benefit recipients ($30 per month). The second column shows the estimated effect after controlling for individual characteristics including sex, age, age entered to DI program at, family status, disability type and location of residence. The estimated effect is quite similar to the main estimate in the first column. The fourth and fifth columns of Table 1.3 show the estimated effects on extensive margins where the latter column shows the estimated effect after controlling for individual characteristics. The estimated effects are quite small at about one percentage point increase in the labor force participation in AISH.

Estimates using the full sample within the two years of the policy change might be contaminated since there has been a policy change in the ODSP at September 2013. The policy change implemented a monthly exemption threshold at $200 where the marginal tax on the earnings accumulated above the threshold is %50. The expected effects of this policy change is increase in the labor supply of DI recipients in the ODSP which might bias my estimates downward.[35] To account for possible contamination, I estimate the effects of the policy change using a shorter panel within one year and half of the policy change where the pre-period is November 2010-March 2012 and post-period is April 2012-September 2013. The estimated effects of the policy change using the shorter panel are presented in the third and sixth columns of Table 1.3 respectively on earnings and extensive margins. The estimated effects do not change much.

[35] The estimates might also be contaminated by November 2008 policy change in AISH where the second kink is shifted forward to $1,500 from $1,000 for those with no dependents and to $2,500 from $2,000 for those with dependents.

Estimates presented in Table 1.3 would be biased if the treatment and control groups have different labor supply trends before the policy change. I plot the estimated coefficients of the interaction terms in (1.18) in Figure 1.12. Panel (a) shows the effects on earnings and Panel (b) shows them for the extensive margins effects within two years of the policy change in AISH. Each point on the solid line indicates the estimated coefficient of the interaction between a dummy for quarter relative to the policy change and treatment variable $AISH$. The gray shades represent the corresponding 95 percent confidence intervals. In both panels, estimated coefficients vary closely around zero before the policy change. But the estimated coefficients for extensive margins in the early two quarters are slightly far from zero. This could be due to the delayed responses to the November 2008 policy change in AISH where the second kink was shifted forward. When facing an increase in work incentives, it might take longer for individual to find a job than increasing their hours of work if they are already employed. The estimated extensive margins effect using the shorter panel excluding the contaminated periods are almost the same as the one using the full sample as shown in Table 1.3. Estimated coefficients are significantly positive and gradually increase in quarters following the policy change.

I present estimated effects on labor supply for different sub-samples within two years of the policy change in Table 1.4. It is instructive to investigate effects of the policy change on those with no dependents and those with dependents separately since the earnings thresholds for those with dependents are higher than those for individuals with no dependents. Estimated effects from (1.17) are shown in the first panel of Table 1.4. The estimated increase in earnings and labor force participation of those with dependents is higher. The earnings and labor force participation of those with dependents increased respectively by 17.88 percent and 4.31 percentage points compared to the corresponding 12.77 percent and 0.62 percentage points increase for those with no dependents. There are also sizeable differences in the estimated effects of the policy change within the age groups. The second panel shows that the increase in earnings of younger individuals (18 to 34 years) is more than twice the size of that for the middle aged group (35 to 49 years) at 23 percent compared to 10 percent. The estimated effect on earnings of older individuals (50 years and older) is quite small decrease in earnings (about 2 percent). The extensive margin effect on older individuals is, however, relatively sizeable at 4.07 percentage points decrease compared to 4.21 percentage points increase for the younger ones and 0.79 percentage points decrease for the middle aged group. Estimated effects for males and females are almost the same in extensive margins but increase in earnings for males is slightly higher at 14 percent compared to 11 percent for females.

Individuals' health condition plays an important role in determination process for DI benefits. Panel (D) of Table 1.4 shows the estimated effects of the policy change broken down by types of disabilities. I divide individuals to three sub-groups based on the ICD-9 codes. Increases in earnings of those with psychotic and neurological disabilities are quite similar and relatively higher than that for individuals with mental disabilities at 15 and 12 compared to 7 percent. Extensive margin response of individuals with Psychotic disabilities is more pronounced than the others at 1.46 percentage point increase compared to 0.07 and 0.05 percentage point reductions, not even significant at conventional levels. The last panel shows the estimates broken down by the location of residence; metropolitan and non-metropolitan area. Increase in earnings in both locations are almost the same at about 13 percent. Extensive margin effects however for those who live in metropolitan areas is 1.83 percentage point increase in labor force participation, but there is no significant effect on those residing in non-metropolitan areas. This finding might be caused by more employment opportunities in metropolitan areas compared to non-metropolitan areas.

Individuals who did not work before the policy change are unlikely to be affected by the induced substitution effects of the policy change since their budget constraints before and after the policy change are parallel as shown in Figure 1.1. In Appendix A.4, I provide suggestive evidence that the induced income effects of the policy change in AISH are negligible. One plausible explanation for why they might start working after the policy change –while they are receiving more benefits– is that they might have been facing adjustment costs and the extra benefits covers up adjustment costs they might face.

Gelber et al. (2016) in a setting where individuals are not compensated for adjustment costs that they might be facing, show that existence of Annual Earnings Test (AET) in the US. results into lower employment rate among the affected older individuals. My findings highlights the role of adjustment costs in extensive margin responses to work incentives. The overall increase in labor supply in AISH from the policy change highlights the interaction between induced incentives to work and adjustment costs. Incentives to work must be large enough to offset the adjustment costs to increase the labor supply in a DI program.

Robustness analysis

To analysis robustness of my findings from DD design, I further estimate the effects from the policy change in AISH on labor supply of benefit recipients in a Regression Discontinuity Design (RDD). I exploit the policy change in AISH at April 2012 (cut-off date) using the date as assignment variable. Intuitively, I compare labor supply right after the policy change (treatment group) to that right before the policy change (control group). This approach sheds light on concern that the control and treatment group in my DD analysis might be quite different. I provide more details on my RDD estimates in Appendix A.3. Figure A.4 shows the discontinuity in inflation adjusted monthly earnings and labor force participation at the date of the policy change in AISH within one year window. Table A.3 presents the estimated effects from the RDD framework within a six months window. The estimated effect is 9 percent increase in earnings and no significant effect on extensive margins. These estimates are smaller that those from DD design using the data within two years of the policy change. There might be delayed responses to the policy change since individuals face adjustment costs. Figure A.5 plots the estimated effects using different bandwidths.

This figure shows that the estimated effects are quite robust to the selected bandwidths. There are concerns that the estimated effects from RDD design however might be contaminated by the seasonality effects. To shed light on this concern, I estimated the effects from placebo policy changes in Table A.4. All the estimates are either negative or insignificant. This finding suggest that either there is no seasonality effect or if there is, it causes decrease in labor supply. In either case, it is unlikely that my findings presented in Table A.3 be contaminated by seasonality effects and at least my estimates would be a lower bond on the true effects of the policy change in AISH on earnings and extensive margins.

1.4.3 Elasticity of labor force non-participation

My estimates show that the policy change in AISH caused increase in labor supply both in extensive and intensive margins. In this section, I adopt the approach of Kostol and Mogstad (2014) to the policy change in AISH to estimate the implied elasticity of labor force non-participation to Participation Tax Rate (PTR). Kostol and Mogstad estimate elasticity of labor force non-participation to PTR form work incentives induced by a policy change in a Norwegian DI program where the marginal taxes on earnings above a threshold is decreased. The elasticity ϵ is defined as:

$$\epsilon = \frac{\Delta(1 - LFP)/(1 - LFP_{before})}{\Delta PTR/PTR_{before}} \tag{1.19}$$

where $1 - LFP$ denotes labor force non-participation. LFP is defined as below:

$$LFP = \begin{cases} 0 & \text{if } earnings \leq z_1^* \\ 1 & \text{if } earnings > z_1^* \end{cases} \tag{1.20}$$

where z_1^* denotes the exemption threshold before the policy change ($400 for those with no dependents and $975 for those with dependents). There is no marginal tax on earnings below z_1^* but the marginal tax on earnings above z_1^* is 50%. That is, earnings below z_1^* are fully exempted but benefits phase out at a rate of \$1 for every \$2 earnings accumulated above z_1^*. The PTR captures the behavioural responses and I define it as follows for earnings level w respectively before and after the policy change where the exemption threshold at z_1^* is shifted forward to z_2^*:

$$PTR_w^{before} = \begin{cases} 0 & \text{if } w \leq z_1^* \\ 1 - \frac{I_0^{before} - I_w}{w} & \text{if } w > z_1^* \end{cases} \tag{1.21}$$

$$PTR_w^{after} = \begin{cases} 0 & \text{if } w \leq z_2^* \\ 1 - \frac{I_0^{after} - I_w}{w} & \text{if } w > z_2^* \end{cases}$$

I_0^{before} and I_0^{after} denote the mean total income (net earnings and benefits) of individuals who do not participate in the labor force respectively before and after the policy change. I_w is the total income with earnings w. ΔPTR denotes changes in PTR before and after the policy change.

I use aggregated data to empirically implement this model. To aggregate the data, I divide observed monthly earnings into $[w - \delta/2, w + \delta/2]$ bins with width δ (I set $\delta = \$10$). ΔPTR is the mean of differences in PTR in each bin weighted by p_w^{before}, the portion of the individuals in each bin before the policy change:

$$\Delta PTR = \mathbb{E}_w[(PTR_w^{after} - PTR_w^{after})p_w^{before}] \tag{1.22}$$

This specification for estimating elasticity of non-participation respect to PTR ignores the income effects, but the estimated elasticity could be interpreted as effect of the policy change. In Appendix A.4, I provide suggestive evidences that there income effects of the policy change on labor supply is negligible.

Descriptive evidences and results

Figure 1.13 plots PRT by earnings before and after the policy change for individuals with no dependents in Panel (a) and for those with dependents in Panel (b). PTR is zero for exempted earnings but it increases afterwards. For any earnings levels, PTR is lower after the policy change than that before the policy change. Figure 1.13 also plots smoothed density of earnings before and after the policy change. The figure suggests that lower PTR is associated with higher density of earnings.

Table 1.5 presents the estimated elasticity of labor force non-participation respect to PTR using the aggregate data two years before and two years after the policy change in AISH. Standard errors are estimated using a non-parametric bootstrap. I obtain 200 samples of the observed earnings with replacement. For each bootstrapped sample, I then estimate the elasticities. Standard error of a parameter is the standard deviation of its bootstrapped distribution. The first column shows the estimates for individuals with no dependents. The estimated elasticity of non-participation respect to PTR is 0.114; ten percent reduction in PTR decreases labor force non-participation by 11.4 percent. The second column shows the estimates for individuals with dependents. The estimated elasticity is 0.033, a ten percent decrease in PTR decreases labor force non-participation by 3.3 percent. This estimate is quite smaller than that for individuals with no dependents. My estimates are in line with estimates of Kostol and Mogstad (2014) where their estimates are about 0.119 to 0.186.

1.4.4 Fiscal effects

In this section, I discuss fiscal effects of the policy change in AISH for the government and the related policy implications. Table 1.6 shows fiscal effects of the policy change in AISH in years before and after the policy change. Tax revenue includes all the taxed DI benefits in addition to a 5% payroll tax on earnings. All the Monterey values are inflation adjusted based on 2012 dollar. There is an increase in DI benefits reflecting the new entries into the program. The substantial increase in expenses in DI benefits in the years after the policy change relative to those before the policy change is associated with the increase in the maximum DI benefits after the policy change. The annual cost of increase in maximum DI benefits is about fifty million dollars. Expense of DI benefits is offset by the tax revenue. Tax revenues in the years after the policy change do not fall much, despite the higher exemption thresholds. In fact, tax revenue two years after the policy change is about one million dollar higher than that one year after the policy change. This finding suggest that the policy change has resulted in a significant increase in DI recipients' earnings. The estimated effect suggests that the DI recipients who increased their labor supply, worked an additional 3 to 5 hours per month.

1.5 Policy implications and conclusions

Do disabled individuals face adjustment costs when changing their labor supply in response to a policy change? Many countries have recently implemented – or are considering – policies to increase work incentives in their DI programs. While these policies provide work incentives, findings on their effectiveness are mixed. In this paper, I provide evidence that the mixed findings on the effects of work incentives induced by a policy changes on labor supply in DI programs could be explained by adjustment costs.

I use a policy change in a Canadian DI program and estimate the size of heterogeneous adjustment costs that vary by individuals' ability to work. I use change in bunching at the earnings exemption threshold induced by the policy change for my empirical analysis. I extend the model for estimating elasticity of earnings and fixed adjustment costs by allowing for heterogeneous adjustment costs that vary by individuals ability to work. Individuals' ability to work is measured as their potential earnings – earnings if no tax would have been imposed on them. The estimated adjustment costs are higher for individuals with lower ability; varying from zero to 8 percent of their potential earnings. The estimated elasticity of earnings with respect to tax rates – accounting for heterogeneous adjustment costs – is 0.2

which is double the size of the one estimated with no adjustment costs. The overall effect of the policy change on labor supply estimated using a DD design is 12 percent increase in average earnings and one percentage point increase in labor force participation. The overall increase in labor supply in AISH from the policy change highlights the interaction between induced incentives to work and adjustment costs. Incentives to work must be large enough to offset the adjustment costs to increase the labor supply in a DI program. The adjustment costs are estimated for a sub-sample of individuals who bunch at the exemption threshold and are relatively more flexible in changing their labor supply. The evidence on existence of adjustment costs for the bunchers suggests that the adjustment costs might be even larger and my estimates are a lower bound on adjustment costs that DI recipients face when changing their labor supply.

My paper has two main caveats. First, I estimate a fixed elasticity of earnings while allowing the adjustment costs to vary by individuals' ability. I also use a static framework where it seems labor supply decisions are dynamic. For my future research, I will extend the model to a dynamic model with heterogeneous elasticity and adjustment costs. Potentially, the observed mass above the second threshold in the program could be used as another moment of bunching to estimate more parameters. Second, adjustment costs in my model is like a black box where not much is known about its nature. It could be related to supply side or demand side of the labor force (i.e. firms). Better understanding of its nature is required to implement policy interventions to increase labor supply in DI programs. This would need data sources on both sides of the market.

1.6 Tables

Table 1.1: Summary statistics

	AISH		ODSP	
	Before	After	Before	After
Labor market statistics				
Positive earnings (%)	48.1	48.4	9.9	9.4
Mean monthly earnings (2012$)	255	285	50	55
	(420)	(470)	(235)	(245)
Mean monthly net benefits (2012$)	1,160	1,530	1,020	1,015
	(120)	(150)	(470)	(460)
Number of new DI awards	1,215	636	8,440	9,965
Background characteristics				
Male (%)	55.3	55.4	53.4	53.9
Mean age (years)	38.5	39.8	43.0	42.9
	(12.5)	(12.8)	(12.6)	(12.9)
Mean age DI awarded at	28.8	29.1	33.2	33.1
	(11.1)	(11.4)	(11.8)	(11.9)
Has no dependent	91.3	90.8	82.1	82.2
Type of disability				
-Psychotic (%)	42.1	42.1	42.6	43.5
-Neurological (%)	50.1	51.0	36.3	36.4
-Mental (%)	7.3	6.9	21.1	20.2
Live in metropolitan area (%)	49.5	48.9	29.1	29.0
Mean number of individuals	8,940	9,890	142,970	160,775
Total number of observations	214,595	237,285	3,431,300	3,385,615

Notes: This table provides summary statistics of AISH and ODSP data. “Before” refers to the period before the policy change in AISH from April 2010 to March 2012 and “After” denotes the period after the policy change from April 2012 to March 2014. Mean earnings and benefits are adjusted for inflation and are rounded to the closest five according to the confidentiality guidelines of the Statistics Canada. Standard deviation of the continuous variables are in the pantheists.

Table 1.2: Estimted elasticity of earnings and heterogeneous adjustment costs

	Bunching at kink at \$400 before policy change	Earnings response at kink at \$400 before policy change	Bunching at \$400 after policy change	Bunching at kink at\$800 after policy change	Earnings response at \$800 kink after policy change	Elasticity	Adjustment costs	
	b_1^0	Δz_1^*	b_1^1	b_2	Δz_2^*	e	ϕ_1	ϕ_2
A. Full sample								
Within two years	2.920***	56.900***	1.950***	1.880***	113.800***	0.192***	20.693***	-0.024***
	(0.227)	(5.250)	(0.107)	(0.389)	(10.501)	(0.017)	(1.197)	(0.002)
Within one year and half	2.790***	63.41***	2.120***	1.820***	126.83***	0.180***	20.247***	-0.023***
	(0.202)	(11.17)	(0.124)	(0.423)	(22.34)	(0.014)	(0.989)	(0.002)
Adding 5%	2.920***	54.623***	1.950***	1.880***	109.245***	0.171***	19.526***	-0.022***
payroll taxes	(0.227)	(4.951)	(0.107)	(0.389)	(9.903)	(0.015)	(1.086)	(0.001)
B. Age								
18-34	2.660***	53.078***	1.630***	2.580***	106.156***	0.180***	19.658***	-0.023***
	(0.175)	(5.175)	(0.101)	(0.377)	(10.349)	(0.016)	(1.767)	(0.003)
35-49	2.680***	54.179***	1.550***	2.820***	108.357***	0.183***	20.033***	-0.024***
	(0.217)	(8.897)	(0.122)	(0.653)	(17.793)	(0.028)	(4.537)	(0.070)
> 50	3.600***	67.821***	2.770***	-0.320	135.642***	0.226***	24.406*	-0.026
	(0.705)	(19.226)	(0.239)	(0.264)	(38.452)	(0.059)	(10.190)	(0.118)
C. Gender								
Male	3.510***	69.146***	2.160***	1.040***	138.292***	0.230***	23.656***	-0.026***
	(0.377)	(16.177)	(0.146)	(0.334)	(32.353)	(0.050)	(7.612)	(0.006)
Female	2.210***	43.040***	1.680***	3.280***	86.079***	0.147***	18.746***	-0.024
	(0.144)	(5.816)	(0.087)	(0.490)	(11.632)	(0.019)	(2.736)	(0.119)
D. Disability type								
Psychotic	4.630	165.685***	1.620***	1.930***	331.369***	0.500***	62.727	-0.057
	(3.771)	(57.228)	(0.147)	(0.347)	(114.457)	(0.146)	(54.727)	(0.171)
Neurological	2.330***	43.836***	2.050***	1.770***	87.673***	0.150***	18.132***	-0.021***
	(0.159)	(3.076)	(0.112)	(0.447)	(6.152)	(0.010)	(0.925)	(0.002)
Mental	4.300***	102.772**	2.100***	2.770***	205.544**	0.330**	36.510**	-0.038
	(0.630)	(46.754)	(0.174)	(0.350)	(93.507)	(0.134)	(21.761)	(0.070)
E. Living location								
Metropolitan area	4.290***	81.041	3.180***	3.360***	162.081***	0.266***	30.339***	-0.034
	(0.962)	(24.731)	(0.216)	(49.461)	(0.457)	(0.074)	(10.022)	(0.118)
Other	1.650***	34.145**	0.880***	0.420***	68.289**	0.118**	11.713*	-0.014**
	(0.136)	(15.007)	(0.103)	(0.334)	(30.014)	(0.050)	(8.054)	(0.007)

Note: This table presents the estimated elasticity of earnings with respect to net-of-tax ratios assuming that utility loss is associated with adjusting earnings that varies by individuals ability from the model specified in Section 1.3.3. The bootstrapped standard errors are in the parenthesis.

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.3: Estimated effect of policy change on earnings and extensive margins

	Earnings ($)			Extensive margin (%)		
	(1)	(2)	(3)	(4)	(5)	(6)
AISH × Post	29.98***	31.02***	29.87***	0.79***	0.79***	0.78***
	(1.34)	(1.34)	(1.53)	(0.15)	(0.15)	(0.17)
AISH	202.09***	197.89***	195.57***	38.22***	38.16***	37.66***
	(0.92)	(0.92)	(1.05)	(0.11)	(0.11)	(0.12)
Sample	Full	Full	Short	Full	Full	Short
Individual co-variates	No	Yes	Yes	No	Yes	Yes
Mean in AISH before policy change	252.47	250.18	250.89	48.12	48.12	47.60
	(420.40)	(420.65)	(421.03)			
R-Sq.	0.04	0.04	0.04	0.08	0.10	0.10
Num. of. Obs.	7,741,795	7,741,795	5,810,529	7,741,795	7,741,795	5,810,529

Notes: This table shows the estimated effects of the policy change in AISH from a Difference-in-Difference framework using (1.17). The full sample includes individuals with non-physical disabilities within two years of the policy change (April 2010-March 2014). The short panel covers a period of one year and half within the policy change (October 2010-September 2013). Included individual co-variates are sex, age, age DI awarded at, family structure, disability type and living location. The earnings are CPI adjusted. The dependent variable for the extensive margins is a dummy that switches on for positive earnings. Robust standard errors are in the parenthesis.
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.4: Heterogeneity in effect of policy change on earnings and extensive margins

	Earnings ($)		Extensive margin (%)		
	AISH × Post	Mean	AISH × Post	Mean	Num. of. Obs.
A. Family structure					
No dependent	31.81***	249.06	0.62***	49.87	6,400,493
	(1.37)	(404.04)	(0.16)		
With dependent(s)	42.39***	237.11	4.31***	29.76	1,341,302
	(5.37)	(498.67)	(0.47)		
B. Age					
18-34	57.29***	249.38	4.21***	45.27	2,323,720
	(2.19)	(425.70)	(0.23)		
35-49	25.82***	262.85	-0.79***	50.80	2,660,571
	(2.39)	(420.75)	(0.26)		
> 50	-4.11*	224.29	-4.07***	49.63	2,757,504
	(2.33)	(375.49)	(0.30)		
C. Gender					
Male	37.79***	263.09	0.80***	49.02	4,162,168
	(1.88)	(428.66)	(0.20)		
Female	24.82***	229.36	0.79***	47.00	3,579,627
	(1.89)	(392.29)	(0.22)		
D. Type of disability					
Psychotic	32.65***	216.60	1.46***	39.22	3,329,884
	(2.02)	(403.23)	(0.23)		
Neurological	32.28***	272.41	-0.07	55.40	2,878,196
	(1.91)	(418.40)	(0.21)		
Mental	19.72***	260.00	-0.50	48.86	1,533,715
	(5.03)	(420.88)	(0.56)		
E. Living location					
Metropolitan area	34.34***	261.63	1.83***	46.82	2,338,947
	(1.97)	(428.07)	(0.21)		
Other	31.40***	234.69	-0.18	49.39	5,402,848
	(1.81)	(397.81)	(0.21)		

Notes: This table shows the estimated effects of the policy change in AISH from a Difference-in-Difference framework using (1.17) broken down by different sub samples. The sample includes individuals with non-physical disabilities within two years of the policy change (April 2010-March 2014). Include individual co-variates are sex, age, age DI awarded at, family structure, disability type and living location. The earnings are CPI adjusted. The dependent variable for the extensive margins is a dummy that switches on for positive earnings. Robust standard errors are in the parenthesis.
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.5: Estimated elasticity of non-participation respect to Participation Tax Rate (PTR) in AISH

	No dependent	With dependent(s)
$\Delta(1-LFP)$	-0.035	-0.030
	(0.001)	(0.003)
$(1-LFP_{before})$	0.747	0.879
	(0.001)	(0.002)
ΔPTR	-0.190	-0.204
	(0.001)	(0.002)
PTR_{before}	0.480	0.205
	(0.007)	(0.004)
$Elasticity(\epsilon)$	0.114^{***}	0.033^{***}
	(0.004)	(0.003)
Num. of Obs.	411,373	40,507

Note: This table shows the estimated elasticity of labor force non-participation respect to Participation Tax Rate (PTR) defined in (1.21) in Section 1.4.3. The sample includes individuals with non-physical debilitates within two years of the policy change in AISH. The bootstrapped standard deviations are in the parenthesis.
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 1.6: Annual fiscal effects of the policy change in AISH

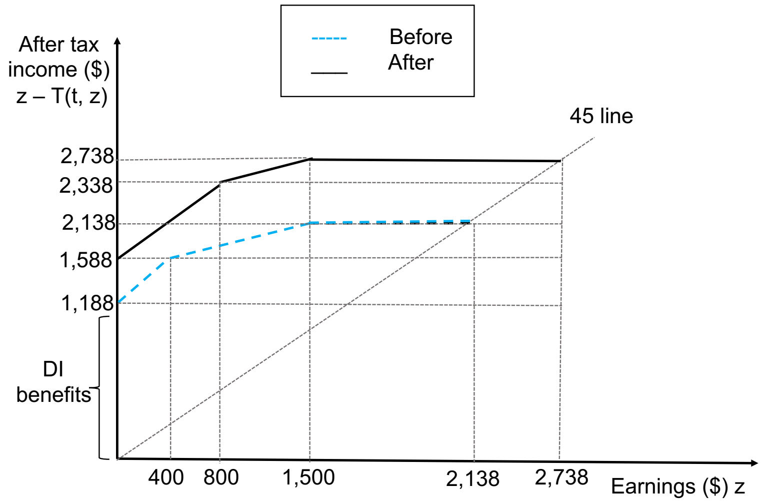
	Before			After	
	2009	2010	2011	2012	2013
DI benefits (million$)	118.3	126.9	132.6	184.8	187.5
Tax revenue (million$)	7.1	7.1	8.0	6.1	6.9
Net expenses (million$)	111.2	119.8	124.6	178.7	180.6

Note: This table shows the annual fiscal effects of the policy change in AISH. Each fiscal year is April-March. The tax revenue includes the claw backed benefits and a 5% payroll tax on the earnings. All monetary values are in 2012$.

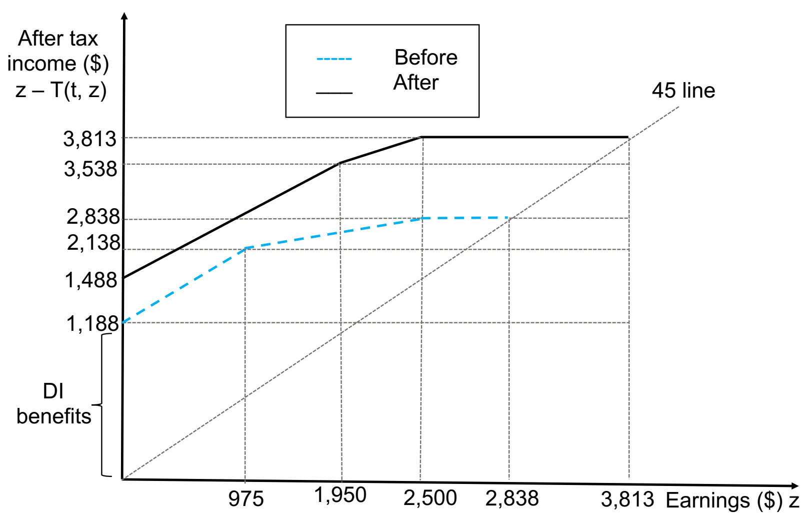
1.7 Figures

Figure 1.1: Budget constraints of benefit recipients of AISH

(a) No dependents

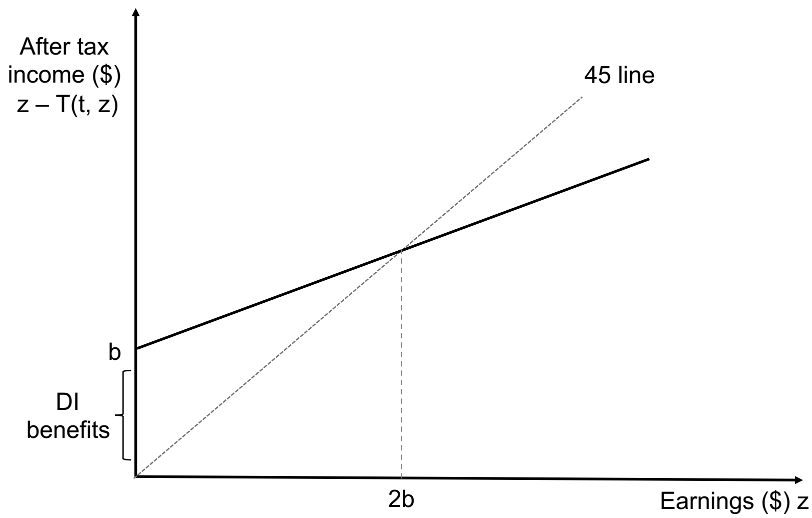


(b) With dependents



Note: This figure shows the budget constraint of DI recipients of AISH before and after the policy change. Panel (a) shows the budget constraints for those with no dependents and panel (b) shows them for those with dependents. Horizontal axes represents earnings and vertical axes is total income (DI benefits and net earnings). Earnings above the exemption threshold up to the second threshold reduce DI benefits at a rate of $1 for every $2. DI recipients lose DI benefits $1 for every $1 of earnings above the second threshold.

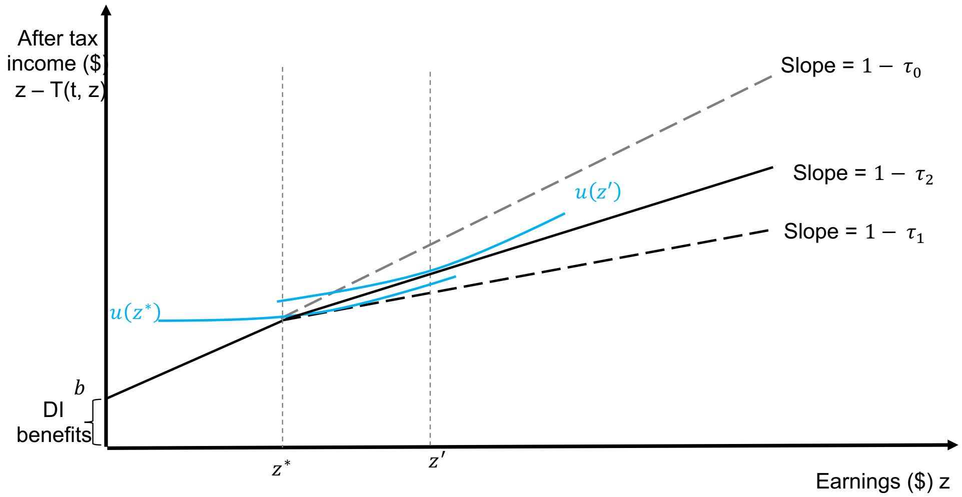
Figure 1.2: Budget constraint of benefit recipinets of ODSP



Note: This figure shows the budget constraint of DI recipients of ODSP. Horizontal axes represents monthly earnings and the vertical axes represents the total monthly income (DI benefits and net earnings). b denotes the monthly DI benefits that depends on the family size that vary from $1,086 to $1,999. DI recipients lose $1 for every $2 of earnings.

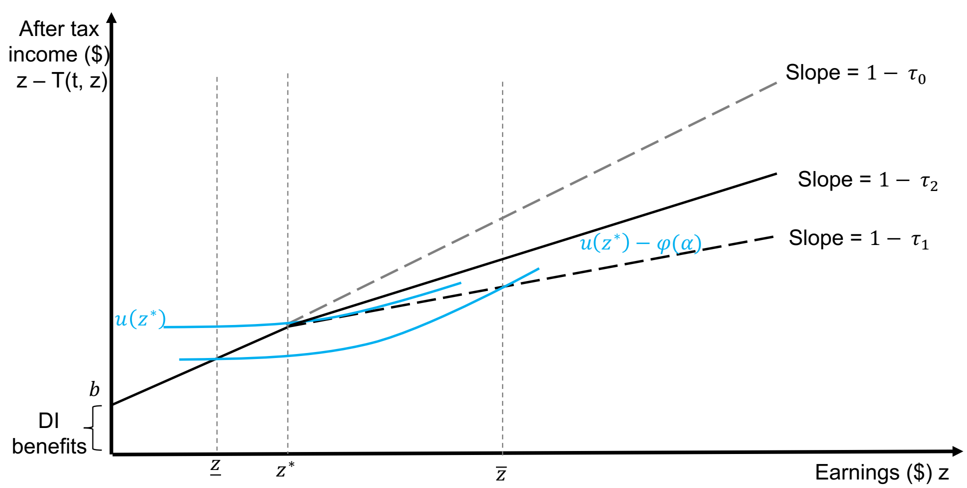
Figure 1.3: Earnings responses and adjustment costs

(a) No adjustment costs



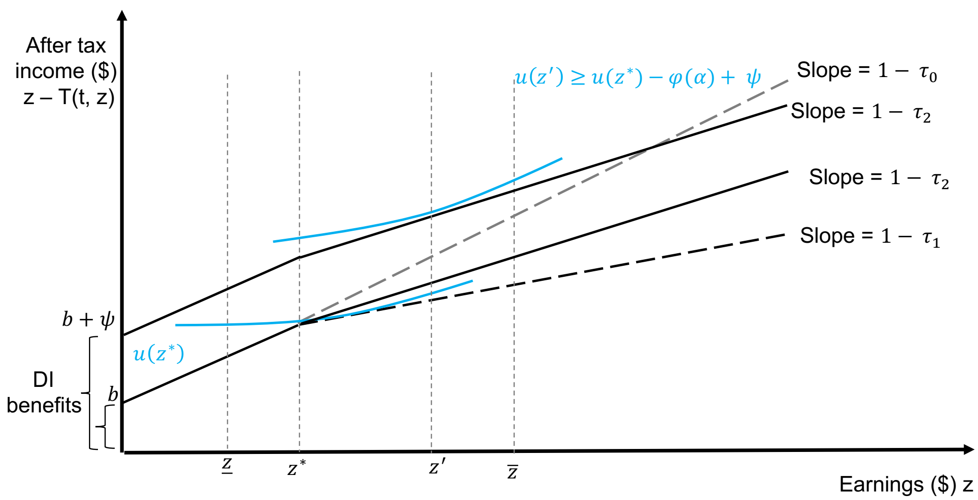
Note: This figure illustrates change in earnings around a kink at z^* where marginal tax rate below and above the kink are respectively τ_0 and τ_1 where $\tau_0 < \tau_1$. Assume that individuals do not face any adjustment costs when they change their earnings. When marginal tax rate above the kink is decreased to τ_2 where $\tau_2 < \tau_1$, then an individual with initial earnings z^* would increase their earnings to z' to get higher utility.

(b) With adjustment costs



Note: This figure illustrates change in earnings around a kink at z^* where marginal tax rate below and above the kink are respectively τ_0 and τ_1 where $\tau_0 < \tau_1$. Assume that individuals face adjustment cost $\phi(\alpha) > 0$ that varies by individuals' ability α. When marginal tax rate above the kink is decreased to τ_2, then individuals with earnings in range of $[\underline{z}, \bar{z}]$ will not change their earnings, since the their utility gain is not as large as adjustment costs they face. $\underline{z}$ and $\bar{z}$ are defined in Equation (1.1).

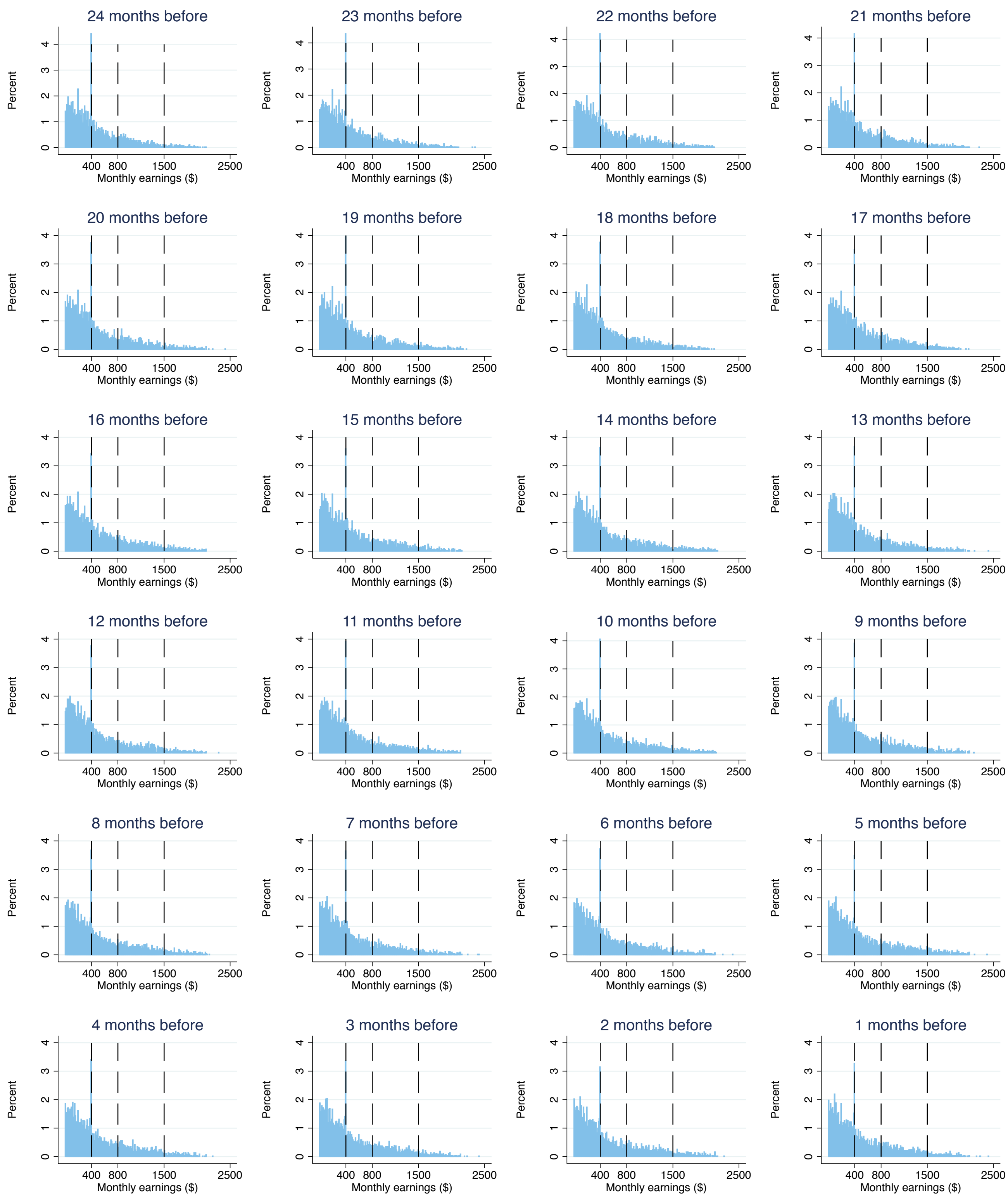
(c) With adjustment costs and lump-sum transfer



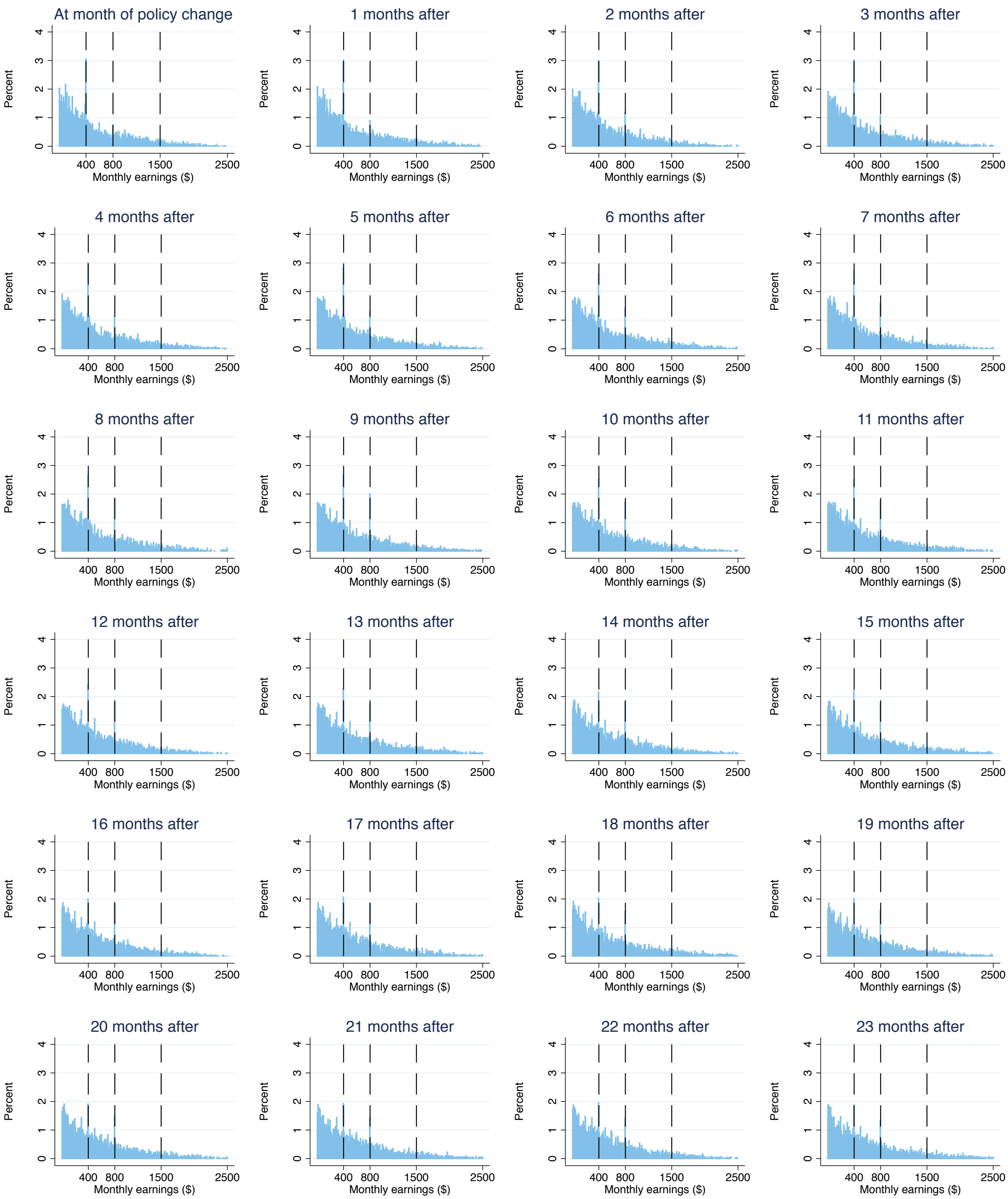
Note: This figure illustrates change in earnings around a kink at z^* where marginal tax rate below and above the kink are respectively τ_0 and τ_1 where $\tau_0 < \tau_1$. Assume that individuals face adjustment cost $\phi(\alpha) > 0$ that varies by individuals' ability α. Suppose that marginal tax rate above the kink is decreased to τ_2 and individuals receive a lump-sum transfer of amount $\psi > 0$. Then individuals with earnings in range of $[\underline{z}, \bar{z}]$ might change their earnings if their utility gain is larger than adjustment costs net of the lump-sum transfer they receive. $\underline{z}$ and $\bar{z}$ are defined in Equation (1.1).

Figure 1.4: Distribution of monthly earnings of DI recipients in AISH with no dependents by relative month to the policy change

(a) Before policy change



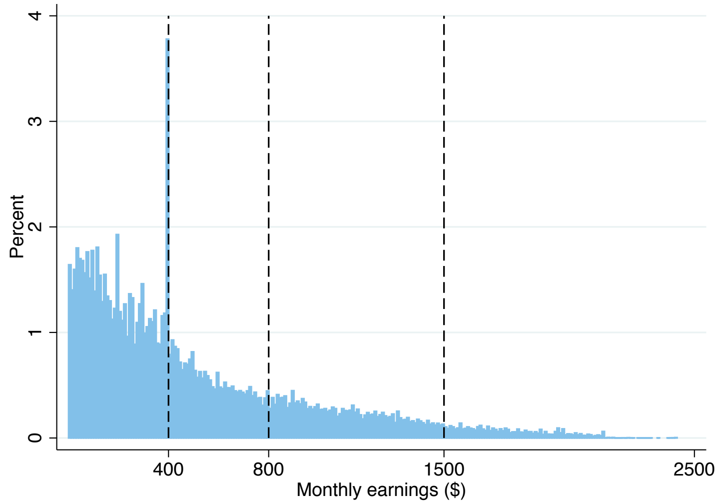
(b) After policy change



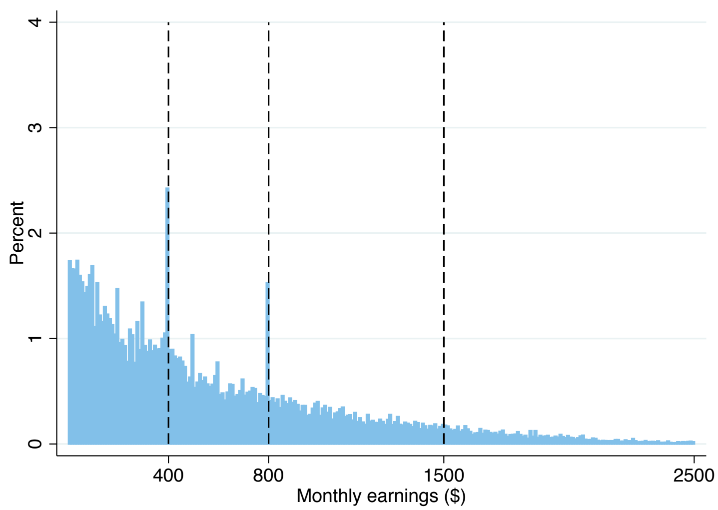
Note: This figure plots the distribution of monthly earnings of DI recipients in AISH within $10 bins. The sample includes individuals 18-64 years old with no dependents who have non-physical disabilities. Panel (a) and Panel (b) show the distributions respectively two years before and two years after the policy change. There is bunching at the exemption threshold every month before the policy change. The bunching moves away to the new exemption threshold after the policy change but still some individuals continue bunching at the old exemption threshold. There is no noticeable bunching at the second kink before, neither after the policy change.

Figure 1.5: Distribution of monthly earnings of DI recipinets in AISH with no dependents

(a) Before policy change



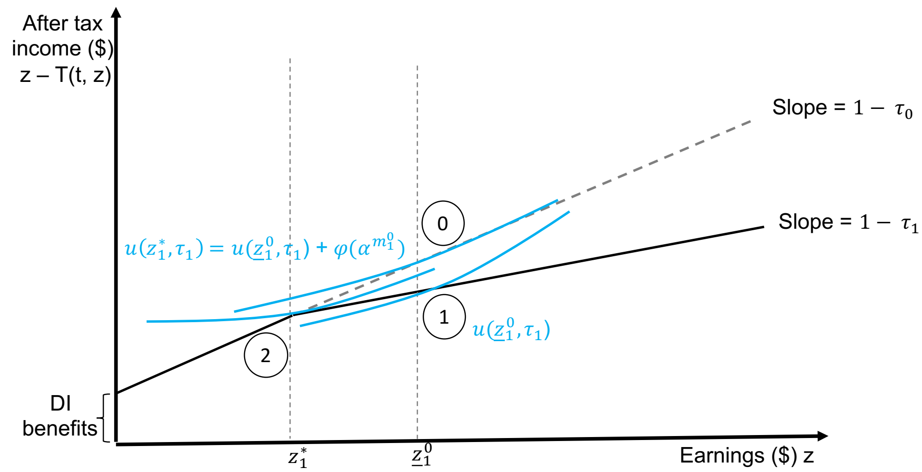
(b) After policy change



Note: This figure shows the distribution of monthly earnings of DI recipients in AISH within $10 bins. The sample includes individuals 18-64 years old with no dependents who have non-physical disabilities. Panel (a) and (b) show the distribution of earnings for the pooled sample respectively two years before and after the policy change. There is a noticeable bunching at the exemption threshold before the policy change. The bunching moves away to the new exemption threshold after the policy change. Some individuals however continue bunching at the old exemption threshold after the policy change. There is no visually noticeable bunching at the second kink before, neither after the policy change.

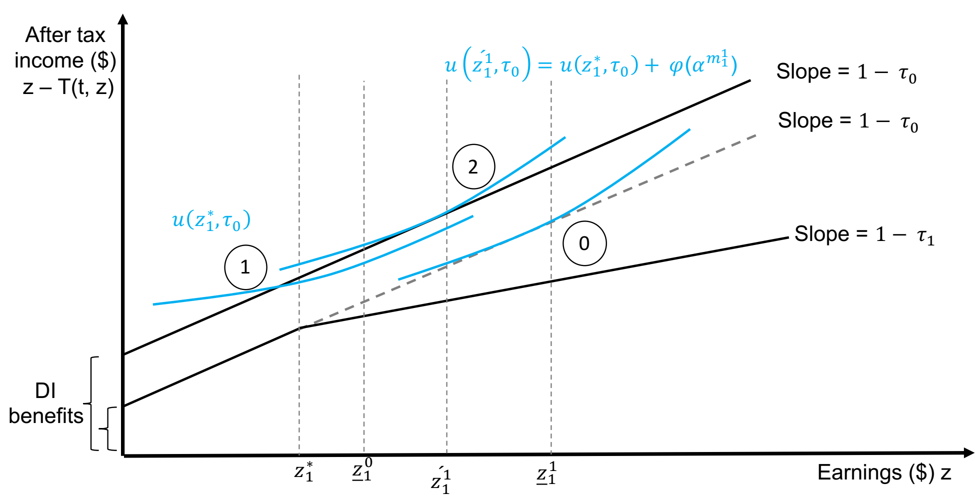
Figure 1.6: Change in bunching at an exemption threshould induced by a policy change

(a) Introducing an exemption threshould



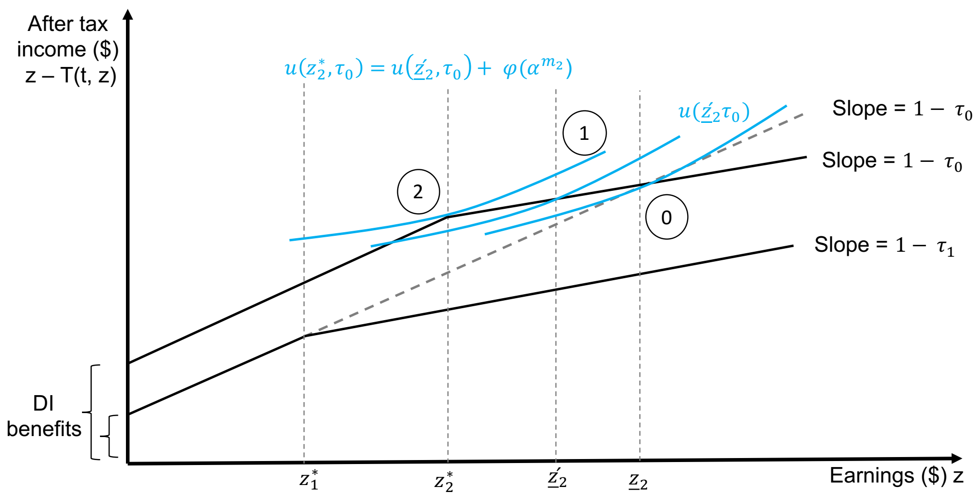
Note: This figure illustrates the earnings adjustment of a marginal buncher at the exemption threshold at z_1^* with ability $\alpha^{m_1^0}$ and initial earnings $\underline{z}_1^0$ when utility loss $\phi(\alpha^{m_1^0})$ is associated with changing labor supply. A marginal buncher is indifferent between staying at $\underline{z}_1^0$ with marginal tax τ_1 or enduring utility loss $\phi(\alpha^{m_1^0})$ and moving to z_1^* with marginal tax τ_0.

(b) Increase in exemption threshold bunching at the old exemption threshould



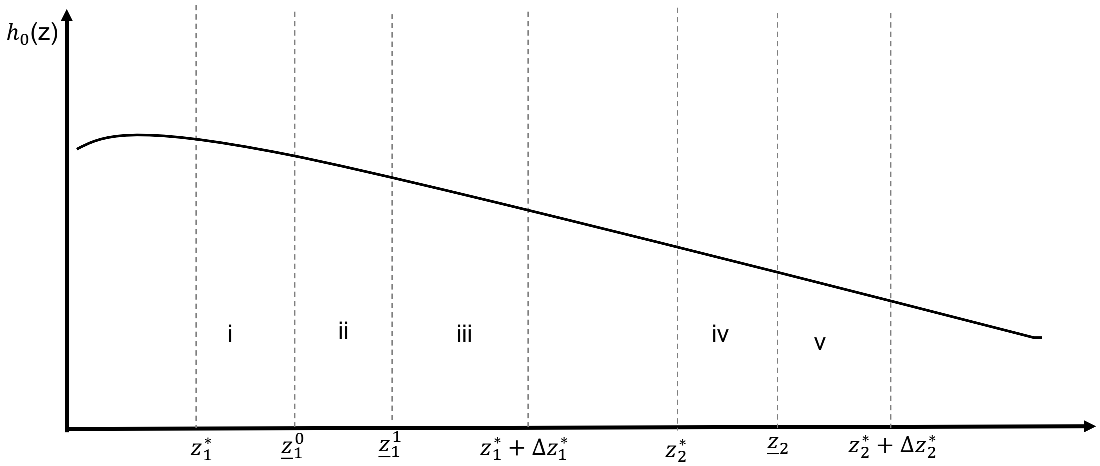
Note: This figure illustrate the earnings adjustment of a marginal buncher at the old exemption threshold at z_1^* after the policy change with ability $\alpha^{m_1^1}$ and initial earnings $\underline{z}_1^1$ when utility loss $\phi(\alpha^{m_1^1})$ is associated with changing the labor supply. After introducing an exemption threshold at z_1^*, she bunches at the exemption threshold. When the exemption threshold is increased, a marginal buncher at the old exemption threshold is indifferent between staying at z_1^* with marginal tax τ_0 or enduring utility loss $\phi(\alpha^{m_1^1})$ and changing her earnings to the optimal one at $z_1^{1\prime}$.

(c) Increase in exemption threshould: bunching at the new exemption threshold



Note: This figure illustrates the earnings adjustment of a marginal buncher at the new exemption threshold at z_2^* with ability α^{m_2} and initial earnings $\underline{z}_2$ when utility loss $\phi(\alpha^{m_2})$ is associated with changing labor supply. After introducing an exemption threshold at z_1^*, she decreases her earnings to $\underline{z}_2'$. When the exemption threshold is increased to z_2^*, a marginal buncher is indifferent between staying at $\underline{z}_2'$ with marginal tax τ_1 and enduring utility loss $\phi(\alpha^{m_2})$ and bunching at z_2^*.

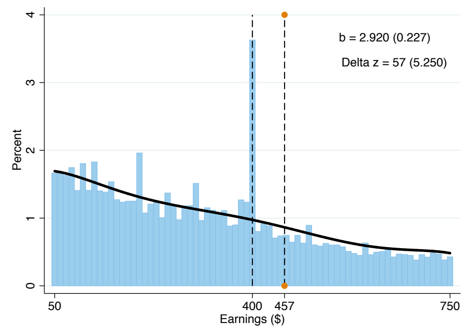
Figure 1.7: Counter-factual earnings with a flat tax



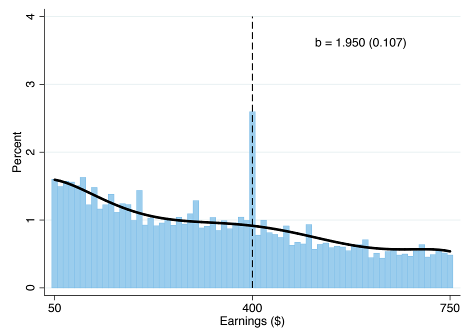
Note: This figure plots the counter-factual distribution of earnings, the distribution if a flat tax τ_0 is imposed on earnings. The amount of bunching at the exemption threshold before the policy change at z_1^* is the area $i + ii + iii$ if individuals face no adjustment costs changing their labor supply. The amount of bunching is however is smaller if individuals face adjustment costs where it is the area $ii + iii$. The area i is the amount of bunching at the old exemption threshold after the policy change. Similarly, the area $iv + v$ and v are the amount of bunching at the new exemption threshold at z_2^* respectively with and without adjustment costs.

Figure 1.8: Fitted polynomials to distribution of earnings at exemption threshoulds

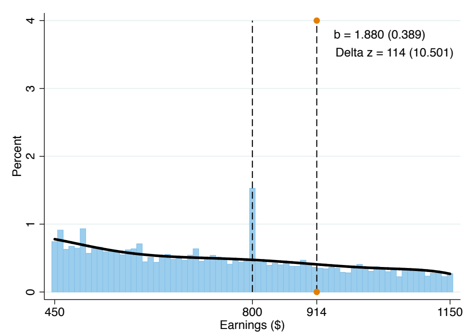
(a) At the exemption threshould before the policy change



(b) At the old exemption threshould after the policy change



(c) At the new exemption threshould after the policy change

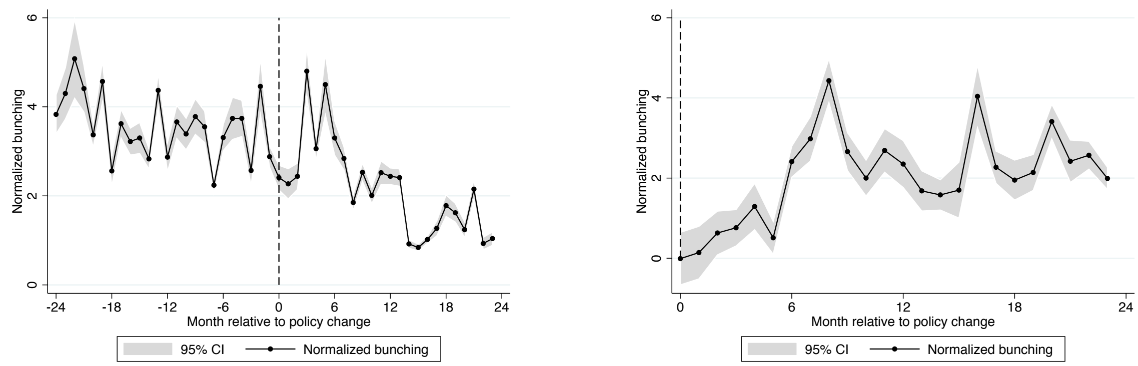


Note: This figure shows the fitted flexible polynomials to the observed distribution of earnings of DI recipients of AISH around the exemption thresholds. The corresponding regression is specified in (1.13). The estimation parameters are $D = 6$, $\delta = 10$ and $l = u = 3$. The sample includes 18-64 years old individuals with no dependents who have non-physical disabilities within two years of the policy change in AISH. Panel (a) and (b) show the fitted polynomials at the old exemption threshold at \$400 respectively before and after the policy change. Panel (c) shows the fitted polynomial at the new exemption threshold at \$800 after the policy change. The amount of normalized bunchings are estimated using (1.16).

Figure 1.9: Normalized bunching at the exemption threshould

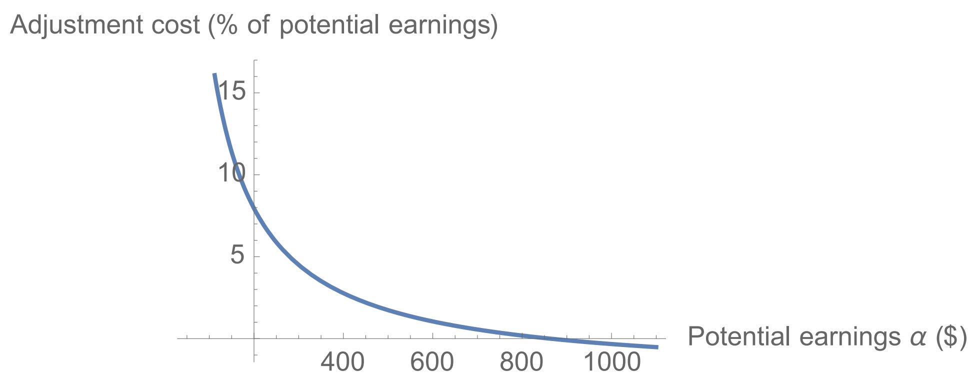
(a) At the old exemption threshould

(b) At the new exemption threshould



Note: This figure shows the amount of normalized bunching at the exemption thresholds estimated using the method presented in Section 1.3.3. The sample includes 18-64 years old DI recipients with no dependents who have non-physical disabilities. The parameters used for the estimation are $\delta = 10$, $D = 6$ and $l = u = 3$. Bunching at the old exemption threshold decreases after the policy change but it does not disappear completely. Bunching at the new exemption threshold gradually increases after the policy change. The 95% Confidence Intervals (CI) using bootstrapped standard errors are shown in gray shades.

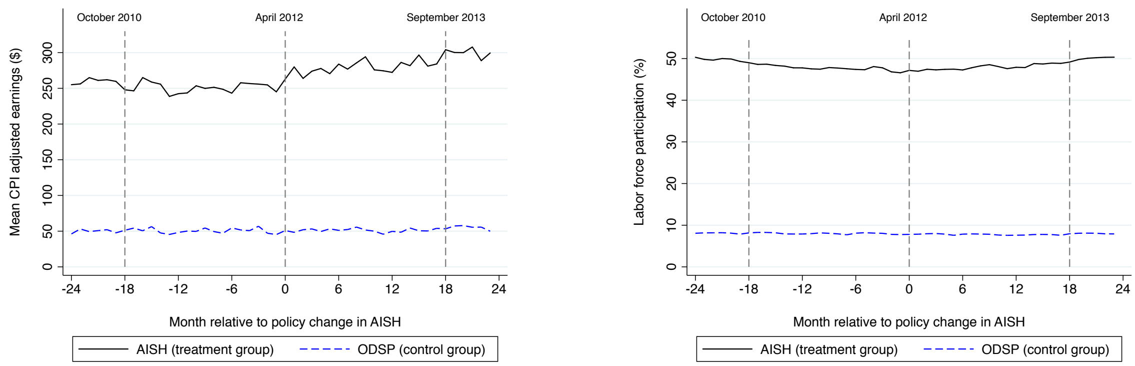
Figure 1.10: The estimated heterogeneous adjustment costs



Note: This figures plots the estimated heterogeneous adjustment costs as percentage of the potential earnings using the model specified in Section 1.3.3. The estimated heterogenous adjustment costs as shown in Table 1.2 is $\phi = \frac{20.69}{\alpha} - 0.024$ where α denotes individuals' potential earnings (ability). The sample includes 18-64 years old DI recipients in AISH with no dependents who have non-physical disabilities, within two years of the policy change in AISH.

Figure 1.11: Trends in earnings and labor force participation before and after April 2012 policy change in AISH

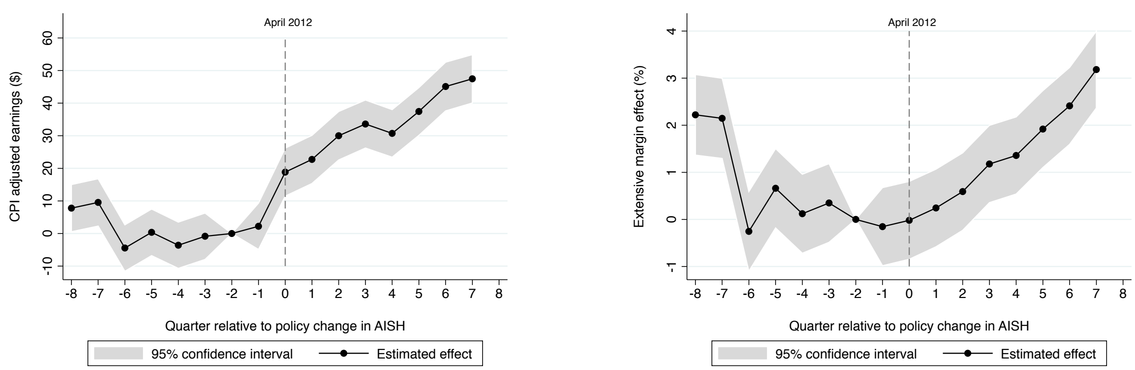
(a) Earnings (b) Labor force participation



Notes: This figure plots the mean monthly earnings and labor force participation rate in the AISH and ODSP respectively in Panel (a) and Panel (b). Labor force participation is defined as a dummy that turns on for positive earnings. The sample includes those with non-physical debilitates. Horizontal axis shows the month relative to the policy change in AISH at April 2012.

Figure 1.12: Coefficients of the interaction quarter×AISH in Equation (1.18)

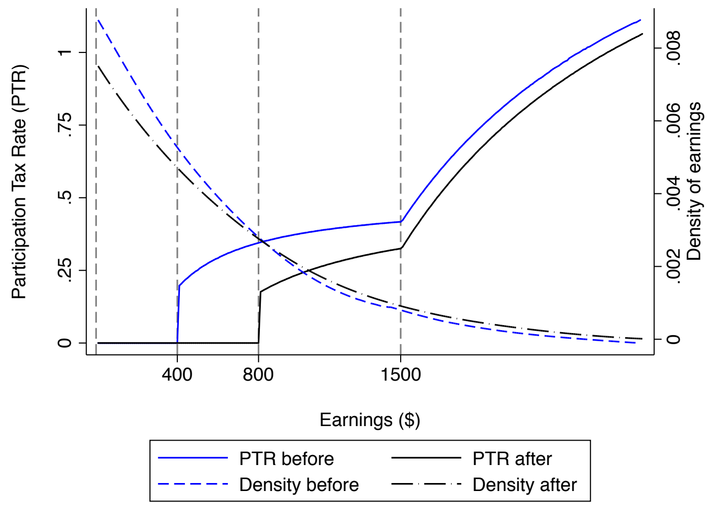
(a) Earnings (b) Extensive margins effects



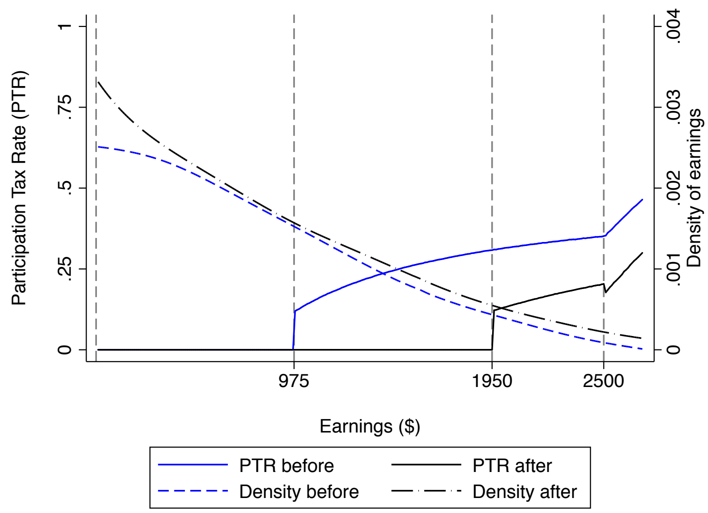
Notes: This figure plots the estimated time trends (β_t) from (1.18). For extensive margins effects the dependent variable is a dummy which switches on for the positive earnings. The individual characteristics sex, age, age DI awarded at, disability conditions, dummies for whether they live in a metropolitan area and dummies whether they have dependents are included in the model. The sample includes those with non-physical disabilities within two years of the policy change in AISH. The gray area indicated the 95% confidence intervals.

Figure 1.13: Participation Tax Rate (PTR) and smoothed density of earnings

(a) No dependents



(b) With dependents



Note: This figure shows the Participation Tax Rate (PTR) by earnings levels, before and after the policy change in AISH. It also plots the smoothed density of earnings before and after the policy change. Panel (a) and Panel (b) correspond to those respectively with no dependents and with dependents.

Chapter 2

Labor Force Participation of Adults with Autism Spectrum Disorder

More than 21.7 million individuals had Autism Spectrum Disorder (ASD) as of 2013 (Vos et al., 2015) where approximately 515 thousands of them lived in Canada[1]. The estimated worldwide prevalence of ASD for all age groups is about one percent.[2] One in 68 children has been identified with ASD in the US. at 2014. This prevalence is 120% higher than the estimates for 2002 and 2000 (1 in 150). Lifespan costs of an individual with ASD is considerable and is about $1.4 million in the US. and UK. (Dudley and Emery, 2014; Buescher et al., 2014). The associate life-span costs would be much higher if an individual has intellectual challenges in addition to ASD[3]. One-third of the lifespan costs is accounted by the lost adult employment. The remainder of the cost is accounted for by the service use which includes special education and medical services. Despite improvements in intervention, education and employment programs for individuals with ASD, their labor force outcome is much lower than the those for the other disabilities.[4] Only half of adults with ASD in the US. have ever worked for pay where one-fifth of them are in sheltered employment[5] (Roux

[1]For more information see: `http://www.autismsocietycanada.ca/asd-research/general#prevalence`.

[2]The prevalence of ASD is defined as the proportion of the population has ASD at a certain point in time.

[3]The estimate of Buescher et al. (2014) of the lifespan cost of an individual with an ASD condition who also has intellectual disabilities is $2.4 million in the Unites States and $2.2 million in the United Kingdom. The estimates of Knapp et al. (2007) for the UK. are much higher; $2.9 million for higher functioning individuals and $4.7 million for the lower functioning individuals. However, the universal finding is that the considerable portion of the lifespan cost of having ASD is accounted by the lost adult employment.

[4]See two recent systematic review of the adult outcome studies over years 1976 to 2011; Howlin and Moss (2012) and Henninger and Taylor (2013).

[5]Sheltered employment refers to the service provisions wherein people with disabilities are assisted with obtaining and maintaining employment mainly through job coach and person-centred approaches. In some sheltered employment programs wages and benefits are paid by an employer in a competitive workplace

et al., 2013). Lower labor outcome of adults with ASD could be attributed in part to their individual characteristics; specifically deficit in higher order social and cognitive skills which are important for success in the labor force. Lower labor outcomes also could be in part be attributed to lower returns to their individual characteristics. They might face greater unobserved barriers such as discrimination and stigma related to their behavioural issues (Baldwin and Johnson, 2000; Johnson and Baldwin, 1993; Thomason et al., 1998; Baldwin and Johnson, 1995, 1994). With rise in childhood prevalence of ASD and considerable life-span costs associated with lost adult employment, better understanding of determinants of adults Labor Force Participation (LFP) is critical. There is however not much empirical evidence on labor force outcomes of adults with ASD.

In this paper, I describe statistical determinants of LFP of adults with ASD and investigate what might explain their lower LFP than those with the other developmental, neuro-cognitive and physical disabilities. First, I estimate Probit models of LFP for each disability group. Second, I use a Blinder-Oaxaca decomposition framework and decompose the lower LFP of adults with ASD than each comparison group to two parts; one part explained by differences in their observable individual characteristics and an unexplained part, reflecting the effect of behavioural issues, discrimination and stigma.

I use Statistics Canada's 2012 Canadian Survey on Disability (CSD) for my empirical analysis. I further investigate robustness of my finding using the 2006 Participation Activity and Limitation Survey (PALS). The CSD and PALS are both post-surveys of Canadian Censuses. Individuals based on their responses to the disability screening questions in corresponding Census are selected to be surveyed for these data sets. These data sets include information on individuals' demographic characteristics, information on their disability and labor force outcomes.

My estimate of prevalence of ASD in adult population in Canada is one in 700. Only one-fifth of working age adults with ASD participate in labor force. This rate is the low-

where in some others wages and benefits are paid by a disability insurance program.

est among all the other comparison groups including those with the other developmental, neuro-cognitive and physical disabilities. My findings from Probit models show that Average Marginal Effect (AME) of completing high school on probability of LFP is the highest for adults with ASD among all the other comparison groups. Furthermore, receiving government transfers is not a disincentive for LFP of adults with ASD, unlike the other comparison groups. My analysis also suggests heterogeneous effects of severity of ASD on LFP depending on individual's age and education attainments. Findings from my Blinder-Oaxaca decomposition show that a large portion of the lower LFP of adults with ASD than the other comparison groups is due to lower returns to their individual characteristics (i.e. education attainment). This finding suggests that adults with ASD might be subject to discrimination and social stigma more often than the other disability groups.

My findings have important policy implications for designing policies to increase LFP of adults with ASD and targeting heterogeneous groups. Higher returns of education on probability of LFP for younger adults with ASD suggests that policies focusing on improving education attainments of younger individuals with ASD could be comparatively more effective for improving their adulthood labor force outcomes. Awareness of and monitoring this vulnerable population is critical as they may be in need of additional services and support for successful transition into employment. These findings, however should be interpreted cautiously since they might be biased, caused by endogenity and self-report errors.

The ASD is a family of developmental conditions of impaired language and social developments, repetitive behaviours and restricted interests (Abrahams and Geschwind, 2008). The causes and cures of ASD are unknown but suspected possibilities include genetic influences, pre- and post-natal development, environmental factors and immune deficiencies. Prevalence of ASD has been rising since Kanner (1943) first described an Autism condition. It was estimated that one in 2,500 children had ASD forty years ago. The 2014 report of the Center for Disease Control and Prevention (CDC) on the prevalence of ASD in the US.

estimates that one in 68 school age children are affected by ASD. This estimated prevalence is 30% higher than the estimate for 2008 (1 in 88); 60% higher than the estimate for 2006 (1 in 110) and 120% higher than the estimates for 2002 and 2000 (1 in 150). Being diagnosed with ASD and growing adult does not necessarily preclude an individual from fully participating in society and the labor force. Manifestation of ASD is a spectrum and the symptoms can occur in any combination. It can range from severe disabilities; silent individuals who are locked into disruptive repetitive behaviours to high functioning individuals who may have active but distinctly odd social approaches, narrowly focused interests and verbose, pedantic communications (Hendricks, 1994, 2010). Some of the adults with ASD especially those with Asperger Syndrome[6], could be enormously talented (Hendricks, 1994). One of the very consistent findings from many long term ASD studies is that severity of ASD decreases when they grow older[7]. That is, repetitive and stereotyped behaviours in the older adults with ASD are improved to fit in more within the society. Effective policy interventions could provide a smoother transition for adults with ASD into labor market.

The rest of this paper is organized as follows. Section 2.1 describes the data and variables used. Section 2.2 presents the empirical analysis. Section 2.3 concludes and draws policy implications.

2.1 Data

I use Statistics Canada's master file of the 2012 Canadian Survey on Disability (CSD) to investigate LFP of adults with ASD. The CSD is a post-surveys of the 2011 National Household Survey (NHS), the Canadian Census. Individuals based on their responses to the filter questions in the NHS are selected to get included into the 2012 CSD.[8] Main purpose of

[6]Asperger Syndrome is an ASD condition that is characterized by significant difficulties in the social interaction and non-verbal communication. It differs from the other ASD by its relative preservation of linguistic and cognitive development. [Source: `http://en.wikipedia.org/wiki/AspergerSyndrome`].

[7]See for instance; Kanner 1973; Howlin et al. 2004; Shattuck et al. 2007; Esbensen et al. 2009; Farley et al. 2009.

[8]More information on sample design of the 2012 CSD is provided in Appendix B.3.

the CSD is to provide information about Canadian adults whose daily activities are limited because of a health related condition. This information is used to plan and evaluate services, programs and policies for adults with disabilities to help enable their full participation in the society.

The CSD's questionnaire include questions asking respondents' about their primary and secondary disabilities and the extend to which it limits their everyday activities. Responses are then combined to create variables indicating individuals' disability type and its severity. Severity of a disability is measured by an index constructed based on respondents' answers to survey questions. Points are given according to reported intensity and frequency of the activity limitations. A disability severity scale is then defined as mild, moderate, severe and very severe. I have grouped individuals with mild and moderate disabilities as less severe and those with severe and very severe disabilities as more severe ones. ICD-10 codes are used to classify disabilities. I use these codes to identify individuals with ASD.[9] I define ASD group as individuals who have reported "childhood Autism" or "Asperger syndrome" as their primary or secondary disabilities.[10] The CSD survey also includes indicator variables for the developmental, learning, memory and psychological disabilities. I group individuals with at least one of these disabilities, excluding those with ASD, as those with neuro-cognitive disabilities. I group respondents who have answered "yes" to the question asking *"Has a doctor, psychologist or other health care professional ever said that you had a developmental disability or disorder? This may include Down syndrome, autism, Asperger Syndrome or mental impairment due to lack of oxygen at birth, etc."* –excluding those with ASD– as those with developmental disabilities. The CSD also includes indicator variables for several other disabilities including hearing, seeing, mobility, agility, pain and communication. I group

[9]The ICD-10 is the 10th revision of the International Classification of Diseases, a medical classification list by the World Health Organization (WHO). It contains codes for diseases, signs and symptoms, abnormal findings, complaints, social circumstances and external causes of injury or diseases.

[10]The ICD-10 codes corresponding to"childhood autism" and "Asperger syndrome" are respectively "F84.0" and "F84.5". I have performed my analysis including the individuals with the"Atypical Autism" (F84.1), "Rett Syndrome" (F84.2) and the "Pervasive Developmental Disorder" (F84.9) in my study sample. However, these conditions are quite rare and including them in the study sample does not affect the findings.

individuals with at least one of these disabilities –excluding those with other developmental, neuro-cognitive and ASD– as those with physical disabilities. I use developmental, neuro-cognitive and physical disabilities as comparison groups for those with ASD to analysis LFP of adults with ASD.

The outcome of interest is LFP. The CSD have a variable indicating individuals' labor force status including, employed, unemployed or out of labor force. I use this variable to construct a LFP indicator which takes the value of one for those participating in labor force (employed or unemployed) and zero otherwise. I also use following set of explanatory variables; sex (male, female), age groups (15-19, 20-24, 25-34 and 35-64 years), marital status (married/common law relationship, single/separated/widowed), education attainments (less than high school, high school and more), severity of disability (less severe and more severe), province of residence and thousands of government transfers. My study sample includes 15-64 years old individuals with ASD, developmental, neuro-cognitive or physical disabilities. Eliminating all observations with missing values for at least one of the variables of interest results in 2.4 million observations where about 30 thousands of them have ASD.[11]

Descriptive statistics of all variables of interest from the CSD are presented in Table 2.1. Panel (a) indicates that males are affected with ASD more frequently than are females with an average male-to-female ratio of 5:1. For the other developmental, neuro-cognitive and physical disabilities the male-to-female ratio is about 1:1. These findings are consistent with findings from ASD clinical studies (for instance see; Volkmar et al. 2005). ASD is a lifelong condition where most physical disabilities occur later in life. More than half of the working age individuals with ASD are 15-24 years old where only five and ten percent for those with physical and neuro-cognitive disabilities are in this age group. A relatively smaller portion of individuals with ASD have severe disabilities (46%) compared to those with the other developmental and neuro-cognitive disabilities (respectively 68% and 70%).

[11]All the statistics reported from CSD are weighted, according to guidelines of Statistic Canada.

Panel (a) also shows that those with ASD have lower education attainments than all the other comparison groups. More than half of them have never completed high school. where, more than one-quarter of those with developmental disabilities have completed high school. A larger portion of those with physical and neuro-cognitive disabilities have completed high school (about three-fourth). Almost all of those with ASD are single where about one-fifth of those with developmental disabilities are married or in a common law relationship. A higher portion of adults with self-reported disabilities reside in Ontario.[12]

Prevalence of a disability is defined as portion of individuals from whole population who have that disability in a specific year. I use the total number of population from the 2011 NHS. Estimated prevalence of ASD, developmental, neuro-cognitive and physical disabilities from the CSD across the age groups are presented in Panel (b) of Table 2.1. Estimated prevalence of ASD for 15-64 years old population in Canada is one in 771 in 2011. There are not much population based estimate of prevalence of ASD in adult population except for Brugha et al. (2012) who provide the first epidemiological prevalence of ASD in adult population (18 years old and over) in the UK. Their estimated prevalence is one in 50 for males and one in 333 for for adult females. Their estimates are higher than mine. My estimated prevalences are closer to prevalence for school age children thirty years ago which it estimated to be one in 2500. Datasets with self-reported ASD however under-represent the actual population with ASD. Therefore, the actual prevalence might be much higher than my estimates. Although it is problematic to compare prevalence of ASD over the last decades, estimated prevalence of ASD for adult population closely matches the estimated prevalence for the school age children in 1990s which is estimated to be one in thousand.[13] Comparing prevalences of ASD over the past decades is problematic mainly because the criteria for ASD used to be more restrictive. Diagnostic criteria for ASD has changed with

[12]The reported provincial shares of the self-reported disability disabilities in Table 2.1 are not in proportions to the provincial population. Therefore, higher portions are not necessarily equivalent to higher prevalences.

[13]See: `http://www.autismsciencefoundation.org/what-is-autism/how-common-is-autism`

each revision of the Diagnostic and Statistical Manual (DSM) where it used to be more restrictive. For instance, DSM used to not recognize Asperger syndrome as ASD where recently it is. Estimated prevalence for 15-64 years old individuals with of developmental, neuro-cognitive and physical disabilities are one in respectively 21, 162 and 20.

Panel (b) also shows that estimated prevalence of ASD for adult population is lower than that for younger one. This could be because most of older individuals with ASD may never get diagnosed having ASD or diagnosed as having other conditions. A sample of individuals with the self-reported ASD then under-represents the population with ASD for several reasons. First, most of the adults, especially the older ones may never diagnosed having ASD. Second, diagnosed adults with ASD may never report their disabilities for reasons such as social stigma. Third, those with very severe ASD residing in care facilities are not included in the CSD. Therefore prevalence of ASD in adult population might be higher than my estimates.

Panel (c) of Table 2.1 presents labor force statistics. LFP rate is defined as portion of adult population who are employed or unemployed. Adults with ASD have the lowest LFP, employment and the highest unemployment rates among all the other comparison groups. Less than one-fourth of them participate in labor force, more than one-third are unemployed and less than one-fifth of them are employed. Adults with physical disabilities have much better labor outcomes where more than three-fourths of them participate in the labor force, more than half of them are employed and less than one-tenth of them are unemployed. Average annual employment income of adults with ASD is $1,500 (2012 CAD) with an average of seven hours of work per week. These are the lowest among all the other comparison groups. Most of adults with disabilities who participate in labor market are in sales and service occupations.

2.2 Empirical Analysis

I describe statistical determinants of LFP of adults with ASD and investigate what might explain their lower LFP than those with developmental, neuro-cognitive and physical disabilities. I estimate Average Marginal Effect (AME) observable individual characteristics on probability of LFP from Probit models. I then use Blinder-Oaxaca decomposition to investigate the extent to which lower LFP of adults with ASD than the other comparison groups could be attributed to differences in their observable individual characteristics.

2.2.1 Probit Model

I assume an individual decides to participate in the labor force if she receives higher utility from participation than not participation. Individuals' obtained utility is not directly observable, but their labor participation decision is. The dependent variable LFP_i which indicates LFP decision of individual i is equal to one if she decides to participate in the labor force (i.e. employed or unemployed) and zero otherwise. That is:

$$LFP_i = \begin{cases} 1 & U_{i1} \geq U_{i0} \\ 0 & \text{otherwise} \end{cases}$$

where U_{i1} and U_{i0} denote utility of individual i when respectively participating and not participating in labor force. I assume utility of individual i is U_i which is specified as:

$$\begin{aligned} U_i = \beta_0 + \beta_1 Sex_i + \beta_2 Age_i + \beta_3 MaritalStatus_i + \beta_4 Severity_i \\ + \beta_5 Education_i + \beta_6 GovernmentTransfer_i + \epsilon_i \end{aligned}$$

where sets of dummy variables for sex (male acting as reference group), age groups (15-19, 25-34 and 35-64 with 20-24 years acting as reference age group), marital status (married/common law relationship, single/separated/widowed with the latter group acting as reference),[14] educational attainment (completed high school with those who have not acting as reference group), severity of disability (less severe acting as reference level and more severe), province of residence (Ontario acting as reference province).[15] $GovernmentTransfer_i$ is thousands of dollars of total annual government transfers. [16] I use this variable as a proxy for non-labor income. ϵ_i is error term which captures any unobserved factors affecting individuals' LFP decision such as their ability or taste for work. I assume that distribution of ϵ_i is normal and therefore I can use a Probit model to estimate AME of individual characteristics on probability of LFP. Conditional probability of LFP is specified as:

$$\begin{aligned}\mathbb{P}[LFP_i = 1|X_i] = \Phi(\beta X_i) = \Phi\{\beta_0 + \beta_1 Sex_i + \beta_2 Age_i + \beta_3 MaritalStatus_i + \beta_4 Severity_i \\ + \beta_5 Education_i + \beta_6 GovernmentTransfer_i\}\end{aligned} \tag{2.1}$$

X_i is a vector of all observable characteristics of individual i on the right side of (2.1). β is a vector of parameters from the model. $\Phi(\cdot)$ is the Cumulative Distribution Function of Normal distribution. I use Maximum Likelihood Estimation method to estimate β. The likelihood function depends only on $\frac{\beta}{\delta}$ where δ denotes standard error of ϵ. Standard error of an error term is not identified unless assuming $\delta = 1$. Since a constant term is included in the model, without loss of generality I assume $E(\epsilon) = 0$. Assuming that (2.1) is correctly specified, estimated coefficients are consistent. I estimate (2.1) separately for those with ASD and each comparison group.

Marginal effect of change in average individual characteristics x with continuous values (i.e. amount of the government transfers) on conditional probability of LFP is $\frac{\partial \mathbb{P}[LFP=1|X]}{\partial x} = \Phi'(X'\beta)\beta_x$, where β_x is the estimated coefficient of characteristics x (2.1). For characteristics with discrete values (i.e. education attainments) marginal effect is calculated as

[14]There is no variation in marital status of adults with ASD since almost none of them are married or in a common law relationship are included as controls. I therefore exclude marital status variable from analysis of adults with ASD. I have included marital status variable in the analyses of the other groups

[15]Yukon, Nunavut and Northwest territories are excluded from the study sample.

[16]More information on Canadian disability benefit programs is provides in Appendix B.2.

changes from the base level (i.e. never completed high school). Since calculated marginal effect depends on individual characteristics X, I calculate Average Marginal Effect (AME) as $N^{-1}\sum_i(\Phi(X_i'\widehat{\beta})\widehat{\beta}_x)$ where N denotes the sample size and $\widehat{\beta}$ is the estimated coefficients from Probit model.

Results from Probit Model

Estimated AME from Probit model specified in (2.1) are presented in Table 2.2. Panel (a) presents estimates for adults with ASD and Panel (b), (c) and (d) show the estimates respectively for adults with developmental, neuro-cognitive and physical disabilities. The first column in each table shows estimated AME from base model specified in (2.1) where sex, age, marital status, severity of disability, education and thousands of annual government transfers are included as controls.[17] Predicted average probability of LFP for adults with ASD is 0.19. After controlling for observable individual characteristics estimated probability of LFP is still remarkably smaller than those of the other comparison groups which is 0.42, 0.57 and 0.72 respectively for adults with developmental, neuro-cognitive and physical disabilities.

Estimated AME on sex variable for adults with ASD and those with developmental and neuro-cognitive disabilities is positive. This finding suggests that women with these disabilities have higher probability of LFP than the corresponding men. A noticeable finding is that the negative association between severity of a disability and LFP is less restrictive for those with ASD than it is for the other comparison groups. Adults with more severe ASD have eight percentage points lower probability of LFP than those with less severe conditions. The corresponding estimated effects for those with the other neuro-cognitive and physical disabilities are respectively 23 and 18 percentage points.

Estimated AME of education on LFP of adults with ASD is 25 percentage points increase in probability of LFP. This is the highest among all the other comparison groups. Estimated

[17] There is no variation in marital status of adults with ASD as almost all of them are single. This variable is excluded from analysis of this group.

effects for those with the other developmental, neuro-cognitive and physical disabilities are respectively 15, 19 and 12 percentage points. Receiving government transfers does not seem to be a disincentive for LFP of adults with ASD where receiving an extra \$1,000 of government transfers is associated with one to two percentage points decrease in probability of LFP of the other comparison groups. Estimates including province of residence are presented in the second column of each table where estimates do not change much.

In the model specified in (2.1), I have implicitly assumed that effect of disability on LFP is constant across age groups and education levels. Impact of severity of a disability in fact could be more or less pronounced depending on age or education level of affected individuals. It is important to understand these heterogeneous effects to frame policies to reduce them. Including severity×age and the severity×education interaction terms could partly capture such interrelationships. Estimated effects from the model including these interaction terms are reported in column (3) to column (5) of each panel of Table 2.2. The last column of each table shows estimated effects from the fully specified model where both interaction terms are included. Predicted average probability of LFP of adults with ASD from the fully specified model is lower than that of the base model reported in the first column. This finding implies that severity of ASD has heterogeneous effects on probability of LFP depending on their age and educational attainments. Negative association between the LFP and severity of ASD is still smaller than those for the other comparison groups after including the interaction terms in the model. Estimated AME of education is still the highest for adults with ASD among all the other comparison groups. Receiving government transfers –unlike the other comparison groups– does not seem to be a disincentive for LFP of adults with ASD. This estimated effect should be interpreted cautiously since is biased. Receiving government transfer is endogenous, since those receiving government transfers have lower probability of LFP.

Understanding statistical determinants of LFP of adults with ASD is important for placing effective policies to enhance their LFP, but it is not an easy task. The first problem is the omitted variable issue where individuals may differ in many aspects other than the observable individual characteristics. For instance, behavioural challenges of adults with ASD influences their LFP but it is hard or even impossible to disentangle these effects. The second one is measurement error in self-reported surveys which induces endogeneity issue and therefore estimated parameters would be biased. For instance, some respondents might use presence of a disability as a basis for not participating in the labor force. Those who do not participate in the labor force might be more likely to report a disability than the others with a similar condition who do participate in the labor force. Being a disability benefit or government transfers recipient might also affect respondents' self-report of a disability. Another issue is the potential simultaneity in relationship between severity of ASD and LFP. Health production theory suggests that employment, income and health are determined simultaneously (Grossman, 1972). On one hand, severity of ASD can be influenced by the attributes related to the LFP such as social support and enhanced self-esteem. On the other hand, severity of an ASD condition influences probability of LFP. Overlooking any of these issues results in biased estimates and findings should be interpreted carefully.

2.2.2 Blinder-Oaxaca Decomposition

I use an extension of Blinder-Oaxaca decomposition to Probit models (Blinder, 1973; Oaxaca, 1973; Yun, 2004) to investigate lower LFP of adults with ASD than those with the other developmental, neuro-cognitive and physical disabilities. Blinder-Oaxaca decomposition of average difference in probability of LFP between two groups is an algebraic manipulation of Probit model specified in (2.1). It divides the difference in average probability of LFP of adults with ASD and a comparison group to a component explained by observable individual characteristics which is called the endowment effect (E) and a part which is not explained. Unexplained component includes coefficient effect (C) and interaction effect

(I). Coefficient effect is due to the estimated coefficients and interaction effect accounts for simultaneous endowment and coefficient effects. That is:

$$\overline{LFP}_G - \overline{LFP}_{ASD} = E + C + I \tag{2.2}$$

where $\overline{LFP}_{ASD}$ and $\overline{LFP}_G$ are the average probability of LFP of respectively adults with ASD and a comparison group G. More specifically:

$$E = \overline{\Phi(X_G\widehat{\beta}_{ASD})} - \overline{\Phi(X_{ASD}\widehat{\beta}_{ASD})} \tag{2.3}$$

$$C = \overline{\Phi(X_{ASD}\widehat{\beta}_G)} - \overline{\Phi(X_{ASD}\widehat{\beta}_{ASD})} \tag{2.4}$$

$$I = \overline{\Phi(X_G\widehat{\beta}_G)} - \overline{\Phi(X_G\widehat{\beta}_{ASD})} + \overline{\Phi(X_{ASD}\widehat{\beta}_{ASD})} - \overline{\Phi(X_{ASD}\widehat{\beta}_G)} \tag{2.5}$$

where overbar represents sample's average. $\widehat{\beta}$ is estimated coefficients from Probit model for the corresponding group. Endowment effect intuitively reflects a hypothetical increase in the probability of LFP of adults with ASD if their observable characteristics were the same as those of the corresponding comparison group. Coefficient effect quantifies increase in probability of LFP of those with ASd if returns to their individual characteristics would have been the same as those of the corresponding comparison group.

Results from Blinder-Oaxaca decomposition

I use the fully specified Probit model where the sets of dummies for severity×age and severity×education and province of residence are included in model specified in (2.1) to perform Blinder-Oaxaca decompositions[18]. All of these variables –except to annual government transfers– are categorical. Decomposition estimates for categorical variables depends on choice of the base category. To avoid this issue, I compute decompositions based on the deviation from grand average called the normalized effect (Yun, 2005). Panel (a) of

[18]Since there is no variation in the marital status of adults with ASD and almost all of them are single, this variable is excluded from the decomposition analysis.

Table 2.3 presents results from decomposing the lower LFP of adults with ASD than the other comparison groups. Large portions of their lower LFP than those with the other developmental and neuro-cognitive disabilities are due to their observable characteristics. If adults with ASD had the same characteristics as those with the other developmental and neuro-cognitive disabilities, their LFP would have been respectively 11 and 21 percentage points higher. A considerable portion of their lower LFP is yet due to lower returns to their characteristics. This could be due to behavioural issues of those with ASD which makes their LFP more challenging than those with the other developmental and neuro-cognitive disabilities. Findings from decomposing lower LFP of adults with ASD than those with physical disabilities is noticeable. Only a small portion of the difference is explained by observable characteristics and an outstanding portion is unexplained and is due to lower returns to their observable characteristics. If returns to observable characteristics of those with ASD would have been the same as that of those with the physical disabilities, their LFP would have been 25 percentage points higher. Panel (b) of Table 2.3 shows results from decomposing lower LFP of adults with the other developmental and neuro-cognitive disabilities than those with the physical disabilities. A remarkably larger portions of their lower LFP is explained by their observable characteristics, compared to that of adults with ASD. Unexplained portion of the lower LFP of adults with ASD could be due to other unobserved characteristics such as behavioural issues and intellectual deficit which makes their LFP more challenging (Ameri et al., 2015). Findings from my decomposition analysis also suggest that adults with ASD might be subject to discrimination and stigma more often than the other comparison groups.

2.2.3 Robustness Analysis

I also use Statistics Canada's master file of the 2006 Participation Activity and Limitation Survey (PALS) to further investigate robustness of my findings from the 2012 CSD. PALS is a post-survey of 2006 Canadian Survey. Individuals based on their responses to the filter questions in Census are selected to get included into the 2006 PALS. It includes information on individuals' disability type and extent to which it limits their every day life. All the variables in PALS are defined the same as those in the CSD. My study sample includes 15-64 years old individuals with ASD, developmental, neuro-cognitive and physical disabilities.

Summary statistics from the 2006 PALS are presented in Table B.1. Panel (a) presents the demographic statistics. Panel (b) presents the estimated prevalence of disabilities across age groups. Estimated prevalence of ASD in 15-64 years population is one in 1808 at 2006 (compare to one in 771 from 2012 CSD). Labor force statistics from the 2006 PALS presented in Panel (c) of Table B.1 are similar to those from the 2012 CSD presented in Table 2.1. Adults with ASD have the lowest LFP among all the other comparison groups where less than one-fourth of them participate in the labor force. They also have the lowest annual employment income among all the other comparison groups where a remarkable portion of them have zero employment income and are government transfer recipients.

Estimated AME from Probit model specified in (2.1) using the 2006 PALS are presented in Table B.2. Predicted probability of LFP for adults with ASD is 0.32. This is the lowest among all the other comparison groups where it is 0.51, 0.66 and 0.68 for those respectively with developmental, neuro-cognitive and physical disabilities. Estimated AME of education on probability of LFP is the highest for adults with ASD. Negative association between severity of a disability and probability of LFP is less pronounced for those with ASD than the other comparison groups. Estimates after including severity$\times$age and severity$\times$education interaction terms are presented in the last column of each table. These interaction terms partly capture heterogeneous effects of severity of a disability on LFP of individuals with different age and education. After including the interaction terms, estimated AME of completing high school is still the highest for adults with ASD. Receiving government transfers does not seem to be a disincentive for LFP of adults with ASD while receiving an extra $1,000 of government transfers decreases LFP of the other comparison groups by about two percentage points. The overall findings from the PALS are similar to those from the CSD.

To further investigate robustness of my findings from Blinder-Oaxaca decomposition using the CSD, I also decompose lower probability of LFP of adults with ASD than the other comparison groups using PALS. I use the fully specified Probit model for my decomposition analysis. These results are presented in Table B.3. A striking finding is that almost all of the lower LFP of adults with ASD than those with the other physical disabilities is due to lower returns to their individual characteristics. While a larger portion of lower LFP of adults with developmental and neuro-cognitive conditions than those with physical condition is explained by their observable characteristics. These findings suggest that adults with ASD might be subject to social stigma and discrimination more often than the others.

A robust finding from empirical investigation using the CSD and PALS is that return of education on LFP is higher for adults with ASD among all the other comparison groups. Receiving government transfers –unlike the others– s not a disincentive for their LFP.

2.3 Policy Implications and Conclusions

Prevalence of ASD is much higher than forty years ago. One in 68 children diagnosed with ASD in 2014 where it was one in 2,500 forty years ago. A large portion of the life time cost of an individual with ASD is accounted by the lost adult employment where adults with ASD have much lower LFP compared to those with the other disabilities. My findings show that average LFP rate of adults with ASD is about 20% which is the lowest among those with the other developmental, neuro-cognitive and physical disabilities. From a policy perspective it is of interest to understand determinants of such low LFP and to know what could be done to improve it. Furthermore, it is of interest to understand whether the lower LFP of adults with ASD is due to their observable characteristics (i.e education) or lower returns to their characteristics. Having evidence on the source of lower LFP of adults with ASD is helpful to effectively invest the available funding for improving their labor outcomes by enabling policy makers to address specific target population. [19] It also would help evaluating heterogeneous effects of LFP promoting programs for groups with different age and severity of condition.

I use two surveys from Statistic Canada; the 2012 CSD and the 2006 PALS to describe statistical determinants of LFP of adults with ASD. Self-reported disabilities are coded using ICD-10 codes in these surveys. This allows me to identify adults with ASD and investigate their LFP. I estimate Probit models of LFP of adults with self reported ASD and those with developmental, neuro-cognitive and physical disabilities. I also perform Blinder-Oaxaca decompositions to investigate how much of the lower LFP of adults with ASD than the other comparison groups is due to their observable characteristics versus lower returns to their individual characteristics. My findings from estimating Probit models of LFP indicate that the AME of completing high school is the largest for adults with ASD than all the other comparison groups. Receiving government transfers does not seem to be a disincentive for LFP of adults with ASD, unlike the others. My results also indicate that severity of ASD is less restrictive than that for the other comparison groups. Furthermore, severity of ASD has heterogeneous effects on LFP depending on individuals age and educational attainments. Findings from my decomposition analysis indicate that larger portions of lower LFP of adults with ASD than those with the other physical disabilities is due to the lower returns to their characteristics where only a small portion of the lower LFP is due to lower observable characteristics of adults with ASD. Although, comparatively larger portions of lower LFP of adults with ASD than those with the other developmental and neuro-cognitive disabilities are due to observable characteristics, yet a considerable portion of the lower LFP is due to the lower returns to observable characteristics of adults with ASD. These findings imply that adults with ASD might face unobservable barriers to participate in the labor force and they might be subjects of discrimination and social stigma more often than those with the other developmental and neuro-cognitive disabilities.

[19] For instance, the $15 million budget of the Economic Action Plan 2014 of the Canadian federal government. For more information see: `http://www.budget.gc.ca/2014/docs/plan/ch3-1-eng.html`

Heterogeneity of the policy instruments

Policy interventions for improving observable characteristics of individuals with ASD could be effective in increasing their LFP although, considerable portion of their lower LFP than those with developmental, neuro-cognitive and physical disabilities is due to lower returns to their observable characteristics. My empirical analyses indicate that returns from completing high school on LFP is the highest for adults with ASD among all the other comparison groups. This finding suggests improving education attainments of individuals with ASD could be comparatively more effective in increasing their labor supply.

My findings, however, suggest heterogeneous effects of education on LFP depending on individual's age and severity of their disability. From a policy perspective it is of interest to know which groups should be targeted for policy intervention. To further investigate the heterogeneous effects of education attainment and government transfers on LFP, I compute fitted AMEs of completing high school and government transfers for adults with ASD and all the other comparison groups from estimated Probit model specified in (2.1). Fitted AME from the 2012 CSD for different severity of disabilities and age groups are presented in Figure 2.1. Panel (a) and Panel (b) present fitted AMEs of government transfers on LFP respectively across the age and the severity of the disability groups. Similarly, Panel (c) and Panel (d) present the corresponding fitted AME of completing high school. Fitted AME of receiving government transfer and completing high school is the highest for adults with ASD with any age and the severity of disability levels among all the other comparison groups. But completing high school has greater effects on the LFP of the younger adults with ASD and those with less severe conditions. Where, receiving government transfers is less disincentive for LFP of the older adults with ASD and its effects is quite the same for those with different severity levels. Fitted AME presented in Figure B.1 from the 2006 PALS show similar findings suggesting that improving education attainments of younger individuals with ASD is comparatively more effective for increasing their labor supply. Moreover, increasing government transfers for older adults with ASD does not seem to be a disincentive for their LFP while it would improve their quality of life.

Although my analysis provides new insight into labor supply of adults with ASD but it is limited. The estimated marginal returns of individual characteristics on probability of LFP are biased and should be interpreted cautiously.

Bibliography

Abadie, Alberto, “Semiparametric Difference-in-Differences Estimators,” *Review of Economic Studies*, 2005, *72* (1), 1–19.

_ , Alexis Diamond, and Jens Hainmueller, “Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California’s Tobacco Control Program,” *Journal of the American Statistical Association*, jun 2010, *105* (490), 493–505.

_ , _ , and _ , “Comparative Politics and the Synthetic Control Method,” *American Journal of Political Science*, 2015, *59* (2), 495–510.

Abrahams, BS and DH Geschwind, “Advances in autism genetics: on the threshold of a new neurobiology,” *Nature Reviews Genetics*, 2008.

Abramowitz, Joelle, “Assisted Reproductive Technology and Women’s Marriage and Birth Timing: Duration and Competing Risks Analyses,” 2012.

_ , “Turning back the ticking clock: The effect of increased affordability of assisted reproductive technology on women’s marriage timing,” *Journal of Population Economics*, 2014, *27* (2), 603–633.

Altonji, Joseph G. and Christina H Paxson, “Labor Supply , Hours Constraints , and Job Mobility,” *The Journal of Human Resources*, 1992, *27* (2), 256–278.

_ and Cristina H. Paxson, “Labor Supply Preferences, Hours Constraints, and Hours-Wage Trade-offs,” *Journal of Labor Economics*, 1988, *6* (2), 254–276.

Ameri, Mason, Lisa Schur, Meera Adya, Scott Bentley, Patrick McKay, and Douglas Kruse, “The disability employment puzzles: a field experiment on employer hiring behavior,” *NBER*, 2015, pp. 1–70.

Autor, David H. and Mark G. Duggan, “The growth in the Social Security Disability rolls: A Fiscal Crisis Unfolding,” *The Journal of Economic Perspectives*, 2006, *20* (3), 71–96.

Bai, Jushan, “Panel Data Models With Interactive Fixed Effects,” *Econometrica*, 2009, *77* (4), 1229–1279.

Baker, Michael, Jonathan Gruber, and Kevin Milligan, “Non-cognitive deficits and young adult outcomes: The long-run impacts of a universal child care program,” *NBER*, 2015, pp. 1–48.

Baldwin, Marjorie and William G Johnson, “Labor Market Discrimination against Men with Disabilities,” *Journal of Human Resources*, 1994, *29* (1), 1–19.

Baldwin, Marjorie L. and William G Johnson, "Labor Market Discrimination against Women with Disabilities," *Industrial Relations*, 1995, *34* (4), 555–577.

_ **and William G. Johnson**, "Labor Market Discrimination against Men with Disabilities in the Year of the ADA," *Southern Economic Journal*, 2000, *66* (3), 548–566.

Benitez-Silva, Hugo, Moshe Buchinsky, and John Rust, "Induced Entry Effects of a $1 for $2 Offset in SSDI Benefits," 2011.

Bitler, Marianne and Lucie Schmidt, "Health disparities and infertility: impacts of state-level insurance mandates," *Fertility and Sterility*, 2006, *85* (4), 858–865.

Bitler, Marianne P and Lucie Schmidt, "Utilization of infertility treatments: the effects of insurance mandates," *Demography*, feb 2012, *49* (1), 125–49.

Blinder, Alan S., "Wage Discrimination : Reduced Form and Structural Estimates," *Journal of Human resources*, 1973, *8* (4), 436–455.

Blundell, Richard and Thomas Mccurdy, "Labor supply: A review of alternative approaches," in Orley Ashenfelter and David Card, eds., *Handbook of labor economics*, 1999, chapter 27, pp. 1559–1695.

Borghans, Lex, Anne C. Gielen, and Erzo F.P. Luttme, "Social Support Substitution and the Earnings Rebound : Evidence From A Regression Discontinuity in Disability Insurance Reform," *American Economic Journal: Economic Policy*, 2014, *6* (4), 34–70.

Brugha, T, SA Cooper, S McManus, S Purdon, J Smith, FJ Scott, N Spiers, and F Tyrer, "Estimating the Prevalence of Autism Spectrum Conditions in Adults : Extending the 2007 Adult Psychiatric Morbidity Survey," Technical Report, The Health and Social Care Information Centre 2012.

Buckles, Kasey, "Understanding the Returns to Delayed Childbearing for Working Women," *American Economic Review*, 2008, *98* (2), 403–407.

_ , "Infertility insurance mandates and multiple births," *Health economics*, 2013, *789* (22), 775–789.

Buescher, Ariane V S, Zuleyha Cidav, Martin Knapp, and David S Mandell, "Costs of Autism Spectrum Disorders in the United Kingdom and the United States.," *JAMA pediatrics*, jun 2014, *19104*, 1–8.

Bütler, Monika, Eva Deuchert, Michael Lechner, Stefan Staubli, and Petra Thiemann, "Financial work incentives for disability benefit recipients : Lessons from a randomized field experiment," *IZA Journal of Labor Policy*, 2015, *4* (18).

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik, "Robust nonparametric confidence intervals for regression-discontinuity designs," *Econometrica*, 2014, *82* (6), 2295–2326.

Campolieti, Michele and Chris Riddell, "Disability policy and the labor market: Evidence from a natural experiment in Canada, 1998-2006," *Journal of Public Economics*, apr 2012, *96* (3-4), 306–316.

Center for Disease Control and Prevention (CDC), "Community Report on Autism," Technical Report 2014.

Chandra, Anjani, Gladys M Martinez, William D Mosher, Joyce C Abma, and Jo Jones, "Fertility, family planning, and reproductive health of U.S. women: data from the 2002 National Survey of Family Growth.," *Vital and health statistics. Series 23, Data from the National Survey of Family Growth*, 2005, (25), 1–160.

Chetty, Raj, "Bounds on Elasticities With Optimization Frictions: A Synthesis of Micro and Macro Evidence on Labor Supply," *Econometrica*, 2012, *80* (3), 969–1018.

_ , Adam Guren, Day Manoli, and Andrea Weber, "Does Indivisible Labor Explain the Difference Between Micro and Macro Elasticities? A Meta-Analysis of Extensive Margin Elasticities," *NBER Macroeconomics Annual 2012*, 2012, *27* (May), 1–56.

_ , Adam Looney, and Kory Kroft, "Salience and Taxation : Theory and Evidence," *American Economic Review*, 2009, *99* (4), 1145–1177.

_ , John N Friedman, and Emmanuel Saez, "Using Differences in Knowledge Across Neighborhoods to Uncover the Impacts of the EITC on Earnings," *The American Economic Review*, 2013, *103* (7), 2683–2721.

_ , John N. Friedman, Tore Olsen, and Luigi Pistaferri, "Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records," *The Quarterly Journal of Economics*, 2011, *126* (2), 749–804.

Dickens, William T. and Shelly J. Lundberg, "Hours Restrictions and Labor Supply," *International Economic Review*, 1993, *34* (1), 169–192.

Dudley, Carolyn and J.C. Herbert Emery, "The Value of Caregiver Time: Costs of Support and Care for Individuals Living With Autism Spectrum Disorder," *SPP Research Papere*, 2014, *7* (1).

Einav, Liran, Amy Finkelstein, and Heidi Williams, "Paying on the margin for medical care: Evidence from breast cancer treatments," *American Economic Journal: Economic Policy*, 2016, *8* (1), 52–79.

Esbensen, Anna J, Marsha Mailick Seltzer, Kristen S L Lam, and James W Bodfish, "Age-related differences in restricted repetitive behaviors in autism spectrum disorders.," *Journal of autism and developmental disorders*, jan 2009, *39* (1), 57–66.

Farley, Megan A, William M McMahon, Eric Fombonne, William R Jenson, Judith Miller, Michael Gardner, Heidi Block, Carmen B Pingree, Edward R Ritvo, Riva Arielle Ritvo, and Hilary Coon, "Twenty-year outcome for individuals with autism and average or near-average cognitive abilities.," *Autism research : official journal of the International Society for Autism Research*, apr 2009, *2* (2), 109–18.

Fortin, Bernard, Guy Lacroix, and Simon Drolet, “Welfare benefits and the duration of welfare spells: Evidence from a natural experiment in Canada,” *Journal of Public Economics*, 2004, *88* (7-8), 1495–1520.

Gelber, Alexander, Damon Jones, Daniel W. Sacks, and Jae Song, “Estimating extensive margin responses on Kinked Budget Sets: Evidence from the Earnings Test,” 2016.

Grossman, Michael, “On the Concept of Health Capital and the Demand for Health,” *Journal of Political Economy*, 1972, *80* (2), 223.

Gruber, Jonathan, “The incidence of mandated maternity benefits.,” *American Economic Review*, 1994, *84* (3), 622–641.

_ , “Disability Insurance Benefits and Labor Supply,” *Journal of Political Economy*, 2000, *108* (6), 1162–1183.

Gumus, Gulcin and Jungmin Lee, “Alternative Paths To Parenthood: Ivf or Child Adoption?,” *Economic Inquiry*, 2012, *50* (3), 802–820.

Hamilton, Barton H and Brian Mcmanus, “A Model of an Assisted Reproductive Techonology (ART) Market,” 2003.

Hamilton, Barton H. and Brian Mcmanus, “The effects of insurance mandates on choices and outcomes in infertility treatment markets,” *Health economics*, 2012, *19*, 994–1016.

Hamilton, Barton H, Brian Mcmanus, Juan Pantano, and Emily Jungheim, “Healthcare Access , Costs , and Treatment Dynamics : Evidence from In Vitro Fertilization,” 2016.

Hendricks, Dawn, *American Psychiatric Association, Diagnostic and statistical manual of mental disorders*, 4 ed. 1994.

_ , “Employment and adults with autism spectrum disorders: Challenges and strategies for success,” *Journal of Vocational Rehabilitation*, 2010, *32*, 125–134.

Henne, Melinda B. and M. Kate Bundorf, “Insurance mandates and trends in infertility treatments,” *Fertility and Sterility*, 2008, *89* (1), 66–73.

Henninger, Natalie A and Julie Lounds Taylor, “Outcomes in adults with autism spectrum disorders: a historical perspective.,” *Autism : the international journal of research and practice*, jan 2013, *17* (1), 103–16.

Howlin, Patricia and Philippa Moss, “Adults with autism Spectrum disorders,” *Canadian Journal of Psychiatry*, 2012, *57* (5).

_ , Susan Goode, Jane Hutton, and Michael Rutter, “Adult outcome for children with autism.,” *Journal of child psychology and psychiatry, and allied disciplines*, feb 2004, *45* (2), 212–29.

Hoynes, Hillary W. and Robert Moffitt, "Tax Rates and Work Incentives in the Social Security Disability Insurance Program: Current Law and Alternative Reforms," *National Tax Journal*, 1999, *52* (4), 623–654.

Jain, Tarun, Bernard L. Harlow, and Mark D. Hornstein, "Insurance Coverage and Outcomes of In Vitro Fertilization," *New England Journal of Medicine*, 2002, *347* (9), 10–17.

Johnson, William G and Marjorie Baldwin, "The Americans with Disabilities Act: Will It Make a Difference?," *Policy Studies Journal*, 1993, *21* (4), 775–788.

Kanner, Leo, "Autistic Disturbances of Affective Contact," *Nervous Child*, 1943, *2*, 217–250.

_ , *Childhood Psychosis: Initial Studies and New Insights*, New York, New York, USA: John Wiley \& Sons, 1973.

Kissin, Dmitry M., Sheree L. Boulet, and Denise J. Jamieson, "Fertility Treatments in the United States," *Obstetrics & Gynecology*, 2016, *128* (2), 387–390.

Kleven, Henrik Jacobsen, "Bunching," *Annual Review of Economics*, 2016, *8* (1), 435–464.

_ **and Mazhar Waseem**, "Using Notches to Uncover Optimization Frictions and Structural Elasticities: Theory and Evidence from Pakistan," *The Quarterly Journal of Economics*, 2013, *128* (2), 669–723.

Knapp, Martin, Renee Rmeo, and Jennifer Beecham, "The Economic Consequences of Autism in the UK," Technical Report, Foundation for People with Learning Disabilites 2007.

Kostol, Andreas Ravndal and Magne Mogstad, "How Financial Incentives Induce Disability Insurance Recipients to Return to Work," *American Economic Review*, 2014, *104* (2), 624–655.

Kroeger, Sarah and Giulia La Mattina, "Assisted Reproductive Technology and Women ' s Choice to Pursue Professional Careers," *Journal of Population Economics*, 2017.

Kulkarni, Aniket D., Denise J. Jamieson, Howard W. Jones, Dmitry M. Kissin, Maria F. Gallo, Maurizio Macaluso, and Eli Y. Adashi, "Fertility Treatments and Multiple Births in the United States," *New England Journal of Medicine*, 2013, *369* (23), 2218–2225.

Lemieux, Thomas and Kevin Milligan, "Incentive effects of social assistance: A regression discontinuity approach," *Journal of Econometrics*, 2008, *142* (2), 807–828.

Lemos, Elkin V., Dongmu Zhang, Bradley J. Van Voorhis, and X. Henry Hu, "Healthcare expenses associated with multiple vs singleton pregnancies in the United States," *American Journal of Obstetrics and Gynecology*, 2013, *209* (6), 586–586.

Marie, Olivier and Judit Vall Castello, "Measuring the (income) effect of disability insurance generosity on labour market participation," *Journal of Public Economics*, feb 2012, *96* (1-2), 198–210.

Martin, Joyce A. and Melissa M. Park, "Trends in twin and triplet births: 1980-97," *National vital statistics reports*, 1999, *47* (24).

Moore, Timothy J., "The Employment Effects of Terminating Disability Benefits," *Journal of Public Economics*, 2015, *124*, 30–43.

Oaxaca, Ronald, "Male-Female wage differentials in urban labor markets," *International economic review*, 1973, *14* (3), 693–709.

OECD, "Sickness, Disability and Work: Breaking the Barriers. OECD Publishing," Technical Report 2010.

Pencavel, John, "Labor supply of men: A survey," in Orley Ashenfelter and David Card, eds., *Handbook of Labor Economics*, Vol. 1 1986, chapter 1, pp. 3–102.

Reynolds, Meredith A, Laura a Schieve, Gary Jeng, and Herbert B Peterson, "Does insurance coverage decrease the risk for multiple births associated with assisted reproductive technology?," *Fertility and Sterility*, 2003, *80* (1), 16–23.

Roux, Anne M, Paul T Shattuck, Benjamin P Cooper, Kristy A Anderson, and Sarah C Narendorf, "Postsecondary Employment Experiences Among Young Adults With an Autism Spectrum Disorder Clinical Guidance," *Journal of the American Academy of Child & Adolescent Psychiatry*, 2013, *52* (9), 931–939.

Ruh, Philippe and Stefan Staubli, "Financial Incentives and Earnings of Disability Insurance Recipients: Evidence from a Notch Design," 2016.

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

Schmidt, Lucie, "Infertility Insurance Mandates and Fertility," *The American Economic Review, Papers and Proceedings*, 2005, *95* (2), 204–208.

_ , "Effects of infertility insurance mandates on fertility," *Journal of health economics*, may 2007, *26* (3), 431–46.

Shattuck, Paul T, Marsha Mailick Seltzer, Jan S Greenberg, Gael I Orsmond, Daniel Bolt, Sheilah Kring, Julie Lounds, and Catherine Lord, "Change in autism symptoms and maladaptive behaviors in adolescents and adults with an autism spectrum disorder.," *Journal of autism and developmental disorders*, oct 2007, *37* (9), 1735–47.

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

Tazhitdinova, Alisa, "Adjust Me If I Can't: The Effect of Firm Incentives on Labor Supply Responses to Taxes," 2016.

Thomason, Terry, John F. Burton, and Douglas Hyatt, "New Approaches to Disability in the Workplace," *Labor Law Journal*, 1998, *49* (7), 1175.

Volkmar, F, R Paul, A Klin, and D Cohen, "Epidemiological studies of pervasive developmental disorders," in "Handbook of Autism and Pervasive Developmental Disorders" 2005, pp. 42–69.

Vos, Theo, Ryan M Barber, Brad Bell, and Amelia Bertozzi-Villa, "Global, regional, and national incidence, prevalence, and years lived with disability for 301 acute and chronic diseases and injuries in 188 countries, 1990-2013: a systematic analysis for the Global Burden of Disease Study 2013," *The Lancet*, 2015, *386* (9995), 743–800.

Weathers II, Robert R and Jeffrey Hemmeter, "The Impact of Changing Financial Work Incentives on the Earnings of Social Security Disability Insurance (SSDI) Beneficiaries," *Public Policy Analysis and Management*, 2011, *30* (4), 708–728.

Wittenburg, David, David R Mann, David Stapleton, Daniel Gubits, David Judkins, Andrew McGuirk, and Others, "BOND Implementation and Evaluation: Third-Year Snapshot of Earnings and Benefit Impacts for Stage 1," *Social Security Administration*, 2015.

Xu, Yiqing, "Generalized synthetic control method: causal inference with interactive fixed effects models," *Political Analysis*, 2017, *25*, 57–76.

Yun, Myeong-Su, "Decomposing differences in the first moment," *Economics Letters*, feb 2004, *82* (2), 275–280.

_ , "a Simple Solution To the Identification Problem in Detailed Wage Decompositions," *Economic Inquiry*, oct 2005, *43* (4), 766–772.

2.4 Tables

Table 2.1: Summary statistics

(a) Demographics

	Physical disabilities	Neuro-Cognitive disabilities	Developmental disabilities	ASD
Sex (%):				
- Male	46.81	45.69	61.74	83.89
- Female	53.19	54.31	38.26	16.11
Age (%):				
- 15-19 years	1.52	5.51	20.86	34.70
- 19-24 years	2.52	5.82	16.89	27.91
- 25-34 years	8.25	12.15	12.77	11.03
- 35-64 years	87.71	76.52	49.47	26.36
Marital Status (%):				
- Single/Divorced/Widowed	31.91	56.31	79.46	100
- Married/Common law	68.05	43.69	20.54	0
Education (%):				
- Less than high school (15-64 yrs)	22.49	31.39	60.45	66.75
- High school graduate (15-64 yrs)	77.51	68.61	39.55	33.25
- High school graduate (18-64 yrs)	78.25	70.70	44.11	39.03
Province of Residence(%):				
- Newfoundland and Labrador	1.95	1.46	2.27	0.77
- Prince Edward Island	0.56	0.43	0.42	0.47
- Nova Scotia	4.12	3.53	2.60	1.75
- New Brunswick	2.59	2.78	3.28	2.29
- Quebec	16.50	14.36	14.81	11.43
- Ontario	42.19	46.81	43.94	55.92
- Manitoba	4.01	3.48	4.43	3.16
- Saskatchewan	3.49	2.44	2.32	1.98
- Alberta	10.61	10.08	11.37	6.09
- British Columbia	14.00	14.64	14.57	16.14
Severity of disability (%):				
- Less Severe	67.54	30.75	32.43	53.77
- More Severe	32.46	69.25	67.57	46.23
Number of Obs.	1,175,200	1,082,920	141,550	29,740

(b) Prevalence of disabilities

	Physical disabilities	Neuro-Cognitive disabilities	Developmental disabilities	ASD
15-24 yrs	1.08% (1 in 92)	2.81% (1 in 36)	1.22% (1 in 82)	0.43% (1 in 234)
25-34 yrs	2.24% (1 in 45)	3.04% (1 in 33)	0.42% (1 in 240)	0.08% (1 in 1320)
35-64 yrs	7.25% (1 in 14)	5.82% (1 in 17)	0.49% (1 in 203)	0.06% (1 in 1815)
Total	5.13% (1 in 20)	4.71% (1 in 21)	0.62% (1 in 162)	0.13% (1 in 771)

(c) Labour force statistics

	Physical disabilities	Neuro-Cognitive disabilities	Developmental disabilities	ASD
Labour Force Participation (%)	76.33	50.30	34.44	21.58
- Employment (%)	56.18	33.55	24.14	14.43
- Unemployment (%)	6.98	16.53	23.09	32.12
Occupation(%):				
- Management/Business/Finance	24.53	23.44	10.06	NA.
- Science/Health/Education/Art/Sport	28.00	28.77	11.07	NA.
- Sale/service	25.10	26.45	45.24	NA.
- Manufacturing/utility	5.59	4.70	6.10	NA.
Employment income:				
- Average annual employment income ($)	26,320	13,691	7,249	1,423
- Zero annual employment income (%)	35.04	50.37	70.25	82.95
- Average weekly paid hours (hrs)	20	12	9	7
Government transfer:				
- Average annual government transfer($)	5,237	7,124	7,604	9,934
- Low income after tax (%)	11.63	23.72	23.23	10.69
Number of Obs.	1,175,200	1,082,920	141,550	29,740

Note: This table presents summary statistics from 2012 Canadian Survey on Disability (CSD). Study sample includes 15-64 years old individuals who have reported having ASD, developmental, neuro-cognitive and physical disabilities. Survey weights generating estimated frequencies in the target population are used in construction of this table, in accordance with Statistics Canada guidelines. Panel (a) presents demographic statistics. Panel (b) presents estimated prevalence of each disability across the age groups. Panel (c) presents labor force statistics. Statistics are flagged as NA. when corresponding sample size is too small to be reported, following Statistics Canada's guidelines.

Table 2.2: Estimated Probit model across disability groups

(a) ASD

	(1)	(2)	(3)	(4)	(5)
Sex: Female	0.098***	0.099***	0.093***	0.097***	0.098***
	(0.006)	(0.007)	(0.006)	(0.006)	(0.006)
Age: 15-19 yrs	0.064***	0.058***	0.109***	0.103***	0.099***
	(0.007)	(0.007)	(0.009)	(0.009)	(0.009)
25-34 yrs	-0.122***	-0.129***	-0.134***	-0.135***	-0.139***
	(0.005)	(0.005)	(0.007)	(0.006)	(0.006)
35-64 yrs	-0.031*	-0.023	-0.037***	-0.028***	-0.030***
	(0.014)	(0.013)	(0.012)	(0.012)	(0.012)
Severity: More Severe	-0.083***	-0.082***	-0.067***	-0.063***	-0.065***
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
Education: $\geq$ High school	0.259***	0.267***	0.224***	0.227***	0.228***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
Thousands of annual government transfers	-0.008***	-0.007***	-0.001*	-0.001*	-0.001**
	(0.001)	(0.000)	(0.001)	(0.001)	(0.001)
Probability of participation for Reference group‡	0.194***	0.148***	0.186***	0.153***	0.171***
	(0.031)	(0.034)	(0.034)	(0.036)	(0.042)
Province of residence	No	Yes	No	Yes	Yes
Age $\times$ severity	No	No	Yes	Yes	Yes
Education $\times$ severity	No	No	No	No	Yes
Number of obs.	29,740	29,600	29,740	29,600	29,600
Pseudo R2	0.1957	0.2179	0.2279	0.2471	0.2475

(b) Developmental disabilities

	(1)	(2)	(3)	(4)	(5)
Sex: Female	0.121***	0.123***	0.111***	0.113***	0.111***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age: 15-19 yrs	-0.100***	-0.102***	-0.109***	-0.113***	-0.113***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
25-34 yrs	0.044***	0.038***	0.042***	0.032***	0.032***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
35-64 yrs	-0.010**	-0.016***	-0.012***	-0.021***	-0.022***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Marital status: Married/Common law	0.159***	0.153***	0.133***	0.132***	0.133***
	(0.003)	(0.004)	(0.003)	(0.004)	(0.004)
Severity: More Severe	-0.095***	-0.089***	-0.076***	-0.069***	-0.067***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Education: $\geq$ High school	0.152***	0.152***	0.152***	0.150***	0.150***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Thousands of annual government transfers	-0.023***	-0.022***	-0.022***	-0.022***	-0.022***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Probability of LFP for Reference group‡	0.422***	0.436***	0.469***	0.466***	0.443***
	(0.013)	(0.014)	(0.018)	(0.019)	(0.021)
Province of residence	No	Yes	No	Yes	Yes
Age $\times$ severity	No	No	Yes	Yes	Yes
Education $\times$ severity	No	No	No	No	Yes
Number of obs.	141,550	141,550	141,550	141,550	141,550
Pseudo R2	0.1862	0.1979	0.1949	0.2064	0.2067

(c) Neuro-cognitive disabilities

	(1)	(2)	(3)	(4)	(5)
Sex: Female	-0.013***	-0.014***	-0.015***	-0.016***	-0.012***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age: 15-19 yrs	-0.048***	-0.048***	-0.047***	-0.048***	-0.051***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
25-34 yrs	0.012***	0.011***	0.025***	0.025***	0.016***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
35-64 yrs	-0.059***	-0.059***	-0.063***	-0.063***	-0.068***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Marital status: Married/Common law	0.013***	0.013***	0.013***	0.013***	0.014***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Severity: More Severe	-0.231***	-0.230***	-0.237***	-0.236***	-0.237***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Education: $\geq$ High school	0.193***	0.192***	0.192***	0.190***	0.186***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Total annual government transfers/1000	-0.013***	-0.012***	-0.012***	-0.012***	-0.013***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Probability of LFP for Reference group‡	0.566***	0.569***	0.549***	0.553***	0.621***
	(0.006)	(0.006)	(0.008)	(0.008)	(0.009)
Province of residence	No	Yes	No	Yes	Yes
Age $\times$ severity	No	No	Yes	Yes	Yes
Education $\times$ severity	No	No	No	No	Yes
Number of obs.	1,082,920	1,082,920	1,082,920	1,082,920	1,082,920
Pseudo R2	0.1164	0.1185	0.1176	0.1197	0.1218

(d) Physical disabilities

	(1)	(2)	(3)	(4)	(5)
Sex: Female	-0.072***	-0.071***	-0.071***	-0.071***	-0.071***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age: 15-19 yrs	-0.047***	-0.042***	-0.054***	-0.050***	-0.046***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
25-34 yrs	0.062***	0.063***	0.099***	0.100***	0.102***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
35-64 yrs	-0.015***	-0.014***	0.003***	0.003***	0.006***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Marital status: Married/Common law	-0.017***	-0.017***	-0.019***	-0.019***	-0.019***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Severity: More Severe	-0.187***	-0.184***	-0.187***	-0.184***	-0.184***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Education: $\geq$ High school	0.119***	0.118***	0.120***	0.119***	0.120***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Thousands of annual government transfers	-0.015***	-0.014***	-0.015***	-0.014***	-0.014***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Probability of LFP for Reference group‡	0.718***	0.723***	0.753***	0.754***	0.752***
	(0.008)	(0.008)	(0.009)	(0.009)	(0.010)
Province of residence	No	Yes	No	Yes	Yes
Age × severity	No	No	Yes	Yes	Yes
Education × severity	No	No	No	No	Yes
Number of obs.	1,174,730	1,174,730	1,174,730	1,174,730	1,174,730
Pseudo R2	0.0938	0.0985	0.0960	0.1007	0.1008

Note: This table presents the estimated Average Marginal Effects (AME) of individual characteristics on probability of Labor Force Participation (LFP) estimated using (2.1) across the disability groups. Study sample includes 15-64 years old individuals from 2012 Canadian Survey on Disability (CSD) with ASD, Developmental, Neuro-cognitive and Physical disabilities. The dependent variable is a dummy that turns on for those participating in labor force. Survey weights generating estimated frequencies in the target population are used in all the estimates. Panel (a), (b), (c) and (d) present the estimated effects respectively for those with ASD, Developmental, Neuro-conitive and Physical disabilities. Robust standard errors are presented in parenthesis.

‡ Reference group for each disability group includes 15-19 years old single males with less severe disabilities who reside in Ontario and have never finished high school.
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 2.3: Blinder-Oaxaca decompositions

(a) ASD versus Developmental, Neuro-cognitive and Physiacal disabilites

	ASD					
	Developmental disabilities		**Neuro-Cognitive disabilities**		**Physical disabilities**	
	Coefficient	in % of $\widehat{\Delta}$	Coefficient	in % of $\widehat{\Delta}$	Coefficient	in % of $\widehat{\Delta}$
$lfp_{ComparisonGroup}$	0.3394***		0.4033***		0.6057***	
	(0.0014)		(0.0005)		(0.0004)	
lfp_{ASD}	0.2139***		0.2139***		0.2139***	
	(0.0024)		(0.0024)		(0.0024)	
$\widehat{\Delta}$	0.1256***		0.1895***		0.3918***	
	(0.0028)		(0.0025)		(0.0024)	
Endowment Effect (E)	0.1125***	89	0.2110***	111	0.0658***	17
	(0.0076)		(0.0110)		(0.0114)	
Coefficient Effect (C)	0.0436***	35	0.1578***	83	0.2546***	65
	(0.0027)		(0.0024)		(0.0037)	
Interaction Effect (I)	-0.0305***	-24	-0.1794***	-94	0.0714***	18
	(0.0077)		(0.0110)		(0.0118)	

(b) Developmental and Neuro-cognitive disabilites versus Physical disabilites

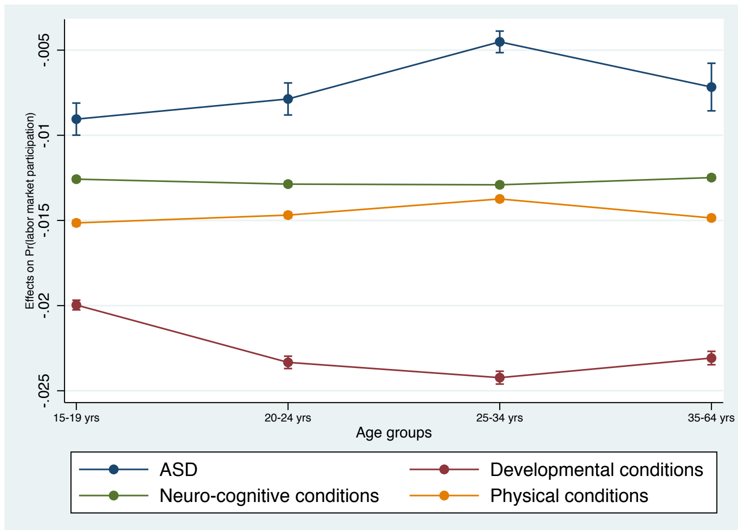
	Physical disabilities			
	Developmental disabilities		**Neuro-Cognitive disabilities**	
	Coefficient	in % of $\widehat{\Delta}$	Coefficient	in % of $\widehat{\Delta}$
$lfp_{Physical}$	0.6057***		0.6057***	
	(0.0004)		(0.0004)	
$lfp_{ComparisonGroup}$	0.3149***		0.4033***	
	(0.0012)		(0.0005)	
$\widehat{\Delta}$	0.2908***		0.2023***	
	(0.0013)		(0.0006)	
Endowment Effect (E)	0.1657***	57	0.1320***	65
	(0.0025)		(0.0005)	
Coefficient Effect (C)	0.1455***	50	0.0995***	49
	(0.0021)		(0.0008)	
Interaction Effect (I)	-0.0204***	-7	-0.0292***	-14
	(0.0030)		(0.0007)	

Note: This table presents Blinder-Oaxaca decompositions of differences in Labor Force Participation (LFP) between two groups, estimated using the model presented in Section 2.2.2. Study sample includes 15-64 years old individuals from 2012 Canadian Survey on Disability (CSD) who have reported having ASD, Developmental, Neuro-cognitive and Physical disabilities. Survey weights generating estimated frequencies in the target population are used in all the estimates. Panel (a) presents decomposing lower LFP of those with ASD than those with Developmental and Neuro-cognitive disabilities. Panel (b) presents decomposing lower LFP of those with Developmental and Neuro-cognitive disabilities than those with physical disabilities.
$*p < 0.10, **p < 0.05, ***p < 0.01$

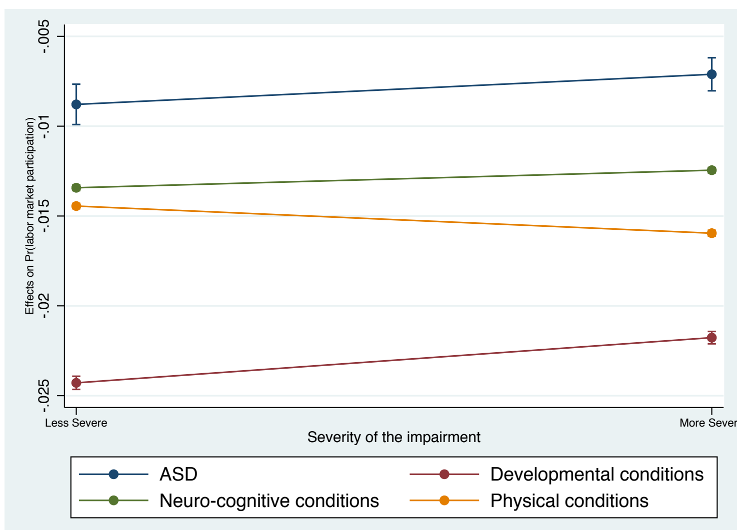
2.5 Figures

Figure 2.1: Fitted Average Marginal Effects of individual charactersitics on probability of Labor Force Participation by type of disability

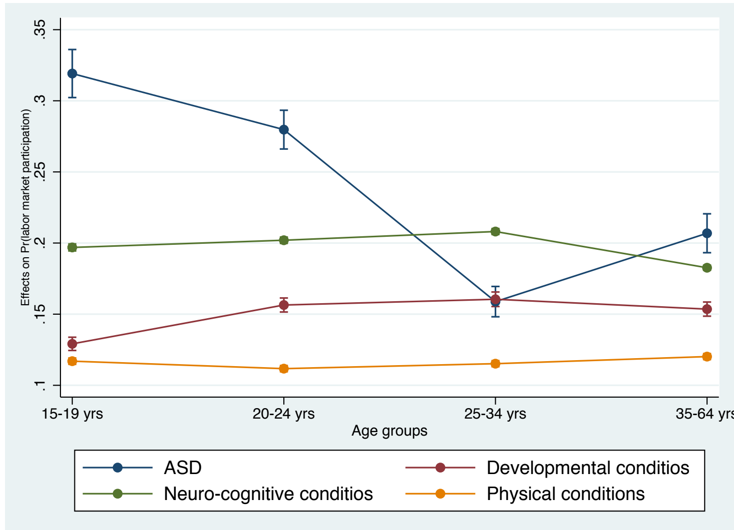
(a) Effects of government transfers across age groups



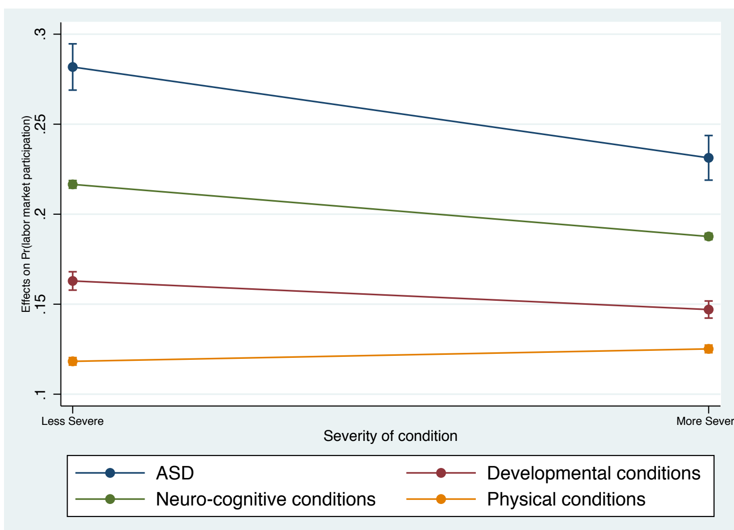
(b) Efefcts of government transfers across severity of diability



(c) Effects of completing high school across age groups



(d) Effects of completing high school across severity of disabilites



Note: This figure plots fitted Average Marginal Effects (AME) of individual characteristics on probability of Labor Force Participation (LFP) across disability types estimated from Probit model specified in (2.1). Study sample includes 15-64 years old individuals from 2012 Canadian Survey on Disability (CSD) who have reported having ASD, Developmental, Neuro-cognitive and Physical disabilities. Survey weights generating estimated frequencies in the target population are used in all the estimates. Panel (a) and (b) plot AME of government transfers respectively across age groups and severity of disability. Panel (c) and (d) plot AME of completing high school respectively across age groups and severity of disabilities.

Chapter 3

Utilization with High Out-Of-Pocket Costs: Evidence from In-Vitro-Fertilization Treatment

3.1 Introduction

Out-of-pocket costs of medical procedures requiring more than one treatment to achieve the desired outcome, is an important factor for many patients to consider before utilization. Patients with lower probability of success, however, might improve their chances by more aggressive treatments. An aggressive treatment increases incidence of adverse outcomes that impose excess burden on health care system. Recently that more expensive and medically advance treatments –such as cancer treatments– are available, there are debates on policy interventions to increase accessibility of these treatments in health insurances. Understanding patients' behavioural responses to increased access to such treatments is important for better design of such interventions. Estimating causal effects however is not straight forward. The main issue here is that patients who use these treatments are a highly selective group among those need the treatment. To overcome the selection issue, a natural experiment is needed which rarely is found in practice.

In this paper, we empirically investigate patients' behavioural responses to increased accessibility of an expensive medical treatment that more than one treatment is required to achieve desired outcome. More specifically, we empirically examine how mandated coverage for In-Vitro-Fertilization (IVF) –an expensive infertility treatment– in private health insurance plans affects patients' utilization behaviour. Health insurance increases accessibility of medical treatments by decreasing out-of-pocket costs while simultaneously decreasing utilization costs. IVF consists of fertilizing an egg with sperm in a lab and implanting resulted embryos in women's womb. We exploit a policy intervention enacted between 1987 and 2005 in fifteen states in the US. that mandated covering IVF treatment in their private health insurance plans. This policy intervention however is quite heterogeneous across states. While some states mandate their private health insurance only to offer plans with IVF coverage –with no obligation to purchase– others mandated coverage of one cycle of treatment in life time, while some others mandated covering unlimited cycles. Despite technological advances, IVF is still an expensive and quite risky and complex treatment.[1] More implanted embryos increases chances of conceiving an infant, but also increases probability of risky and costly multiple births.[2] Patients decide about the number of implanted embryos by considering their probability of success –fertility depends on age and family records– and associated out-of-pocket costs. We develop a conceptual framework to shed light on a mechanism through which high out-of-pocket cost of IVF treatment affects incidence of multiple birth. We then use state-time variation in mandated coverage of IVF in a Generalized Synthetic Control (GSC) framework to empirically quantify causal effects from the number of covered cycles on incidence of multiple birth.[3] We use states that have never legislated policies to cover IVF treatment in their private health insurance plans as the control group. To furthermore investigate a channel through which this policy intervention affects incidence of adverse outcome, we also investigate how increased access to IVF treatment affects adoption rates –an alternative to IVF– in a Difference-in-Difference-in-Differences (DDD) framework. We

[1]The American Society of Reproductive Medicine (ASRM) lists average price of an IVF cycle in the U.S. to be $14,500 (2014 US. dollars). [Source: `http://www.resolve.org/family-building-options/making-treatment-affordable/the-costs-of-infertility-treatment.html`, Accessed on July 9, 2017.] Estimated average cost including medication, pre-cycle procedures for a 35 year old woman are much higher at $19,000 to $20,000 (2014 US. dollars). [Source: `http://ivfcostcalculator.com/`, Accessed on July 10, 2017]. Success rate of one cycle of IVF treatment is quite low at twenty percent. [Source: `https://www.cdc.gov/art/artdata/index.html`, Accessed on July 16, 2017.]

[2]Multiple births are associated with greater risks to both the mother and infants, including low birth weight and prematurity (Martin and Park, 1999; Reynolds et al., 2003). Average cost of a singleton birth was $27,000 in 2012, while twin and triplet births cost $115,000 and $435,000 (Lemos et al., 2013).

[3]We potentially could use the state-time variation in mandated IVF coverage in a Difference-in-Differences (DD) framework. The estimated effects however, would not be interpreted as causal effect if the "parallel trend" assumption is violated. We however provide estimated effects from a DD framework in Appendix C.2.

exploit variation in state-time and the age of mothers for this estimation.[4]

Infertility is defined as not being able to conceive an infant after one year (or longer) of trying or carrying a pregnancy to term. More than eighteen percent of women of child bearing age in the US. have reported struggling with fertility in 2016.[5] Utilization of IVF has grown four-fold over the last few decades, contributing to one percent of all births, but more than half of multiple births in the US. Despite all the technological progress, success rate of one cycle of IVF treatment is as low as twenty percent.[6] Most of the patients therefore need more than one cycle of treatment to successfully conceive an infant. Pecuniary costs of one cycle of treatment is as high as 46% of average annual disposable income of a family in the US. (Kissin et al., 2016). Health insurers however do not cover cost of medication and pre-cycle procedures. These all add up to a high out-of-pocket costs of the treatment (both pecuniary and non-pecuniary costs such as emotional and discomfort of pre-cycle procedure of failed cycles). More aggressive treatments –implanting more embryos– increases chances of conceiving an infant, but also increases probability of multiple births. The best outcome of a cycle of IVF treatment both in terms of the infants' and mothers' health and associated costs is a single pregnancy and birth. Professional IVF service providers advise their patients on the number of embryos to implant. Patients however make the final decision by considering their probability of success –fertility depends on age and family records– and associated out-of-pocket costs. Increase in number of treatments covered in a health insurance plan would have two competing effects. First, more patients might avoid adverse outcomes by choosing less aggressive treatments, resulting into decrease in incidence of multiple births. Second, more patients with low probability of success –who need higher number of aggressive treatments– might use the treatment despite the high out-of-pocket costs. These patients

[4] Adoption rates are only available from 1994 to 2014. Since the number of pretreatment periods are too small we can not use a GSC framework.

[5] For more information, see `https://www.cdc.gov/reproductivehealth/infertility/index.htm`. Accessed on July 14, 2017.

[6] For more information, see Center for Disease Control (CDC) 2015 Fertility Clinic Success Rates Report: `https://www.cdc.gov/art/artdata/index.html`, Accessed on July 16, 2017.

improve their chance of conceiving an infant by more aggressive treatments, resulting to increase in incidence of multiple births. Overall effect of the increased accessibility of IVF treatment on the incidence of multiple births is then ambiguous. This sheds doubt to effectiveness of insurance coverage of IVF treatment for decreasing its utilization costs.

We use data from the National Center for Health Statistics' Natality Detail Files for our empirical analysis. This data includes records of all infants born in 51 states in the US. from 1975 to 2014. Data files include information on a mothers' state of residence, age, education, race and marital status. It includes an infants' year of birth, birth weight, race, sex, plurality (singleton, twine, triplet or higher) and order of birth (first, second or higher birth). Furthermore, some records include the fathers' race. For our GSC analysis we aggregate the data into state-year cells. We use share of multiple births and number of infants per thousand birth in each state-year cell as measures of incidence of multiple birth.[7] We also combine the data from Adoption and Foster Care Analysis and Reporting System (AFCARS) with Natality Detail Files to analysis how mandated IVF coverage affects adoption. We aggregate the combined data into state-year-age cells. We define adoption rate as ratio of number of adopted children to total number of infants born in each state-year cell.

Our empirical analysis has two main conclusions. First, there are strong behavioural responses to increase in accessibility of IVF treatment. Estimated causal effects on the share of multiple births from mandated coverage of IVF varies from 0.31 percent decrease in states with one covered cycle to 35 percent increase in states with unlimited coverage. Second, increase in accessibility of IVF treatment –via mandated health insurance coverage– also affects market for alternative for receiving infertility treatment, adopting a child. Our estimates from a DDD framework shows that in states with mandated coverage for IVF

[7]We follow Buckles (2013) to use share of multiple births and number of infants per thousand births as measures of incidence of multiple birth. There is one infant in a singleton birth and, two infants in a twin birth and so on.

treatment, more older women relative to younger ones choose using IVF treatment rather than adopting a child. The estimated effects are larger in states with more covered cycles of IVF. This finding sheds light on a possible mechanism through which increased accessibility of IVF treatment might lead to increase in incidence of multiple birth which is consistent with prediction of our conceptual framework presented in Section 3.3. In states with more generous IVF coverage, more patients who need more aggressive treatment –older women compensate their low fertility with implanting more embryos– use IVF treatment.

Findings from our empirical analysis have important policy implications for designing policy interventions that aim in increasing accessibility of expensive and technologically advance treatments. There are debates on policy interventions to increase accessibility of such treatments while decreasing utilization costs.[8] Our findings from a policy intervention for IVF treatment suggests that there are strong behavioural responses to generosity of insurance coverage of such medical procedures. More generous coverage together with high out-of-pocket costs gives incentives to patients with low chance of success to use the treatment. These patients would prefer more aggressive treatments, resulting to an adverse outcome. Increase in incidence of adverse outcome imposes burden on health care system both in terms of utilization and costs associated with adverse outcome. Hamilton et al. (2016) and Einav et al. (2016) suggest regulations in from of limiting intensity of treatment –number of implanted embryos for IVF treatment– or imposing top-up price for more intense treatments, implanting additional embryos.

While it is expected increased access to expensive medical treatments to decrease aggressive treatments, there is not much empirical evidence on patients' behavioural responses to generosity of access. Descriptive studies, using clinic level data, find that treated patients with health insurance plans covering IVF treatment prefer receiving less aggressive

[8]Canada currently considers including IVF treatment as part of their public health care. Manitoba offers a Fertility Treatment Tax Credit where 40% of treatment fees can be claimed to a maximum credit of $8,000. Source: https://www.gov.mb.ca/finance/tao/fttc_faq.html#question4, Accessed on July 9, 2017.

treatments, compared to those with no insurance coverage (Hamilton and Mcmanus, 2012; Reynolds et al., 2003; Jain et al., 2002; Henne and Bundorf, 2008). Fewer implanted embryos decreases incidence of multiple birth within patients with IVF insurance coverage than those with no coverage. Hamilton et al. (2016) estimate a structural model on patients' choice within and across infertility treatments using data from an infertility clinic in the US. Their policy simulations show that mandated coverage or restricting number of implanted embryos can improve access or costs, but not both. We contribute to this literature by estimating causal effects on adverse outcome of aggressive IVF treatment –incidence of multiple birth– from generosity of mandated IVF coverage. For our estimation, we use data from all births from 1975-2014 in all states in the US. We also shed light on a possible mechanism through which mandated coverage might affect adverse outcome of treatment, by estimating effects of mandated coverage on adoption market as an alternative to infertility treatment.

Our paper is also related to the literature investigating effects of mandated IVF coverage on a variety of outcomes including infertility service, fertility, age at first birth, time of marriage, women's choice to pursue professional careers and allocation of labor supply over life cycle (Bitler and Schmidt, 2006, 2012; Schmidt, 2007, 2005; Buckles, 2008; Kroeger and Mattina, 2017; Abramowitz, 2014, 2012). Most of these studies use either state-year or state-year-age variations in mandated IVF coverage in respectively DD and DDD frameworks. Buckles (2013) uses Natality Detail Files in a DD model and finds that mandated IVF coverage had a small and statistically insignificant impact on incidence multiple birth. Our estimates from GSC model –after taking care of issues with parallel trend assumption– are relatively large and statistically significant. We contribute to this literature in two ways. First, we estimate causal effects from the levels of mandated coverage for IVF treatment on incidence of multiple births using more recent data files from 1974 to 2014. Second, we provide more accurate –less biased– estimate using a GSC framework. This framework ensures that the estimated effects could be interpreted as causal effects.

For the remainder of the paper, we proceed as follows. We describe institution background and the data we use in our empirical analysis in Section 3.2. We present a conceptual framework in Section 3.3 to show how more generous IVF coverage in private health insurance plans affects patients' utilization behaviour. We present our empirical analysis using a GSC model in Section 3.4. We investigate how policy interventions in IVF market affects adoption as an alternative in Section 3.5. Finally we provide conclusion and policy implications in Section 3.6.

3.2 Background and data

3.2.1 What is In-Vitro-Fertilization (IVF) treatment?

Infertility is a disease that results in abnormal functioning of males' or females' reproductive system.[9] Infertility is described as inability to conceive as well as being unable to carry a pregnancy to full term. In-Vitro-Fertilization (IVF) is a type of Assisted Reproductive Technology (ART) used for infertility treatment and gestational surrogacy. IVF is the process of fertilization by extracting eggs, retrieving a sperm sample, and then manually combining an egg and sperm in vitro ("in glass"). The fertilized egg(s) –called embryo(s)– are then implanted in the women's womb. The first infant conceived using an IVF treatment was born in 1978 in the UK.

The infertility treatment process begins with medical tests and advice from a patients' physician on how to conceive an infant with minimum medical intervention. The next step is usually taking infertility drugs to stimulate egg production. If these simple and relatively inexpensive treatment methods are not successful then an ART such as IVF procedure might be recommended.[10]

[9] The American Society for Reproductive Medicine (ASRM) and the American College of Obstetricians and Gynecologists (ACOG) and the World Health Organization (WHO) recognize infertility as a disease.

[10] IVF is the most dominant type of ART used in the US. Other forms of ART include Gamete Intrafallopian Transfer (GIFT) and Zygote Intrafallopian Transfer (ZIFT). IVF is mostly attempted if these less invasive or expensive options have failed or are unlikely to work. The use of these alternatives to IVF peaked

A cycle of IVF treatment consists of three main steps. First, the woman starts taking drugs that stimulate her egg production. Patients have to pay for these drugs, even if their health insurance plan covers IVF treatment. During this period, the woman visits the fertility clinic frequently to monitor egg development. If the ovarian response is deemed to be sufficient, the physician and patient move onto the next step of the treatment.

In the second step, the woman undergoes a surgical process to retrieve some eggs for insemination in the laboratory. The resulted embryos are then cultured in the laboratory for 2 to 6 days as the cells begin to divide.

The third and the most important step of a cycle of IVF treatment is that patients decide how many embryos to implant. Two important factors affecting this decision are patients' fertility (i.e. embryo quality which mostly depends on women's age) and costs of the treatment. Out-of-pocket costs for one cycle of IVF treatment even with insurance coverages are relatively high, since insurance do not cover medication and pre-cycle procedures. The cost of one cycle of IVF cycle for a 35 years old woman is about $14,500 (2014 US. dollar) where average cost including medication, pre-cycle procedure is about $19,000 to $20,000 (2014 US. dollars).[11] Despite technological advances, success rates of a cycle of IVF treatment is as low as twenty percent.[12] Most of the patients need more treatments to successfully conceive an infant. If more embryos are implanted, probability of conceiving an infant increases, but also raises the incidence of multiple pregnancy and multiple births. Multiple pregnancies and resulted multiple births are risky for both mothers and the infants. Multiple pregnancies are associated with higher miscarriage rates and lower birth weights (Martin and Park, 1999; Reynolds et al., 2003). Professional IVF service providers consult their patients about the number of embryos to implant based on the graded quality of their embryos. Older patients –who have lower fertility– are more likely to have lower graded embryos and

around 1990, and they are now almost completely absent from ART market.

[11]For more information see the website of The American Society of Reproductive Medicine (ASRM): http://www.resolve.org/family-building-options/making-treatment-affordable/the-costs-of-infertility-treatment.html. [Accessed on July 9, 2017]. Also http://ivfcostcalculator.com provides more detailed information on associated costs of IVF treatment.

[12]Source: https://www.cdc.gov/art/artdata/index.html, Accessed at 16 July, 2017.

therefore lower chance of conceiving an infant. Patients with lower fertility then might decide to implant more embryos to increase their chance of success. Patients' fertility and the number of the IVF cycles covered by their health insurance then are two important factors that affect patients' decision on the number of embryos to implant. The goal here is to balance the effects of implanting fewer embryos –lower incidence of multiple births– and high pregnancy and birth rate. More than one-third of twins and more than three-quarters of triplets and higher order multiples in the US. in 2011 resulted from conception assisted by infertility treatments specially IVF (Kulkarni et al., 2013). Incidence of multiple births is then a central issue in the social benefits and costs of IVF. A healthy singleton pregnancy and birth is the best possible outcome of an IVF treatment.

3.2.2 Institutional background

High out-of-pocket costs of IVF treatment have led policy makers to consider interventions to improve access to this treatment. State level mandated IVF coverage in private health insurance in the US. aims at increasing accessibility of IVF by decreasing out-of-pocket costs of the treatment. Between 1978-2014, fifteen states in the US. have passed legislation either requiring private health insurance plans to cover IVF (mandated to cover) or offer plans that covers IVF (mandated to offer). The number of covered cycles in mandated states varies by state. Arkansas (1987)[13] and Hawaii (1989) mandate coverage for only cycle of IVF while Connecticut (2005) mandates covering two cycles. Insurers in Rhode Island (1989) and Maryland (2000) are mandated to cover three cycles. Illinois (1991) and New Jersey (2001) mandate covering up to four cycles and insured patients in Massachusetts (1987) can use as many as cycles they need. More detail on the mandated IVF coverage in the US. is provided in Table 3.1. States that mandated offering plans with IVF are Montana, Texas, California, New York, Ohio, West Virginia and Louisiana. In these states insurers are only mandated to offer plans with IVF coverage, but with no obligation to purchase for

[13] The year in parenthesis presents the year policy intervention to cover IVF in private health insurance plans is enacted in corresponding state.

buyers. The other 36 states do not have any policy intervention to improve access to IVF treatment (never mandated).

3.2.3 Data

We use data from the National Center for Heath Statistics' Natality Detail Files for our empirical analysis. This data is based on birth certificate information. The data includes records of all live births in 51 states in the US. from 1975 to 2014. The data files include information on mothers' age, education (less than high school, high school graduate, 1-2 years of college and 3 or more years of college), race (white, black, American Indian or Alaskan Native or Asian or Pacific Islander), marital status (married or not) and state of residence.[14] The data also includes information of a fathers' race. An infants' year of birth, sex, birth order, plurality and birth weights are also recorded in the data files.

There is one record for each infant in the data file, meaning that for instance, there are three records for a triplet birth. The number of infants then over-represents the incidence of multiple births. To deal with this issue, we construct weights for each record as the reverse of the plurality of the birth (i.e. the weight of each infant in a triplet birth is set to be 1/3). We use these weights throughout our empirical analysis to convert the unit of analysis from infant to birth.[15]

We aggregate the data into state-year cells. We use two outcome variables to measure incidence of multiple births. First, share of multiple births in a state-year cell. Multiple birth is defined as all births that are not singleton. Second, number of infants per thousand births. Information of a mothers' education and race, marital status and a fathers' race however is not recorded in all years. We impute missing values by setting them as the average of the corresponding variable in the year before and after.

[14]Public use files up to year 2004 includes mothers' state of resident. We use the restricted files for 2005 to 2014 which includes mothers' state of residence.

[15]Few states prior to 1985 report only half of their births. We double each record in such cases.

Table 3.2 presents summary statistic from all births in 51 states in the US. from 1975 to 2014. We divide the study period to two periods 1975-1994 and 1995-2014. We provide statistics separately for never mandated, mandated to offer and mandated to cover states. After excluding observations with missing values, the total number of births is about 150 millions. Mothers in more recent years are on average older and more educated and less likely to be married. Increase in returns to education and technological advances in infertility treatments in recent decades provides incentives for women to invest more in their education and professional carriers and postpone having a family and child bearing. Incidence of multiple births is also higher in more recent years. This could be because of technological advances that makes infertility treatments more affordable and accessible. Incidence of multiple births in states with mandated coverage is higher than both mandated to offer and never mandated states.

3.3 Conceptual framework

In this section, we build up on Hamilton and Mcmanus (2003) to provide a simple conceptual framework to show how generosity of insurance plans for covering IVF treatment might affect incidence of adverse outcome, multiple birth.

There are many factors that together affect a patient's choice for acquiring IVF treatment within alternatives for having a family (i.e. adopting an infant) as well as the intensity of their treatment if they decide to use IVF. Since IVF is a relatively expensive treatment, for many patients associated costs of the treatment is known to be the most important factor to consider. Mandated coverage of IVF treatment in private health insurance plans in some states in the US. is a policy intervention that aims to decease the associated pecuniary costs of the treatment and therefore increase its accessibility for more patients. A health insurance plan that covers more IVF cycles, also provides incentives for patients to implant fewer embryos, since they can try it again if the current cycle fails. Lower number of implanted embryos however decrease probability of conceiving an infant, as well as incidence of multiple births. It is expected then states with more covered cycles have lower incidence of multiple birth.

Patients however, have to pay hundreds of dollars out-of-pocket for the required medication and pre-cycle procedures even if their insurance plan covers IVF treatment. It also requires lots of time and emotional investment from patients. Patients should start taking medications regularly a while before the actual treatment starts. The whole process could be quite stressful for lost of patients, since there is always a good chance of failure of the treatment. Patients also worry about the outcome of their treatment, since there are lots of uncertainty about the health of the conceived infant. Patients endure non-pecuniary costs before, during and after the treatment. High out-of-pocket costs (both pecuniary and non-pecuniary) associated with IVF treatment therefore might affect the pool of patients who decide undergo the treatment in two important ways. First, in states with lower number of covered cycles, low fertile patients –who might need more cycles of treatment to successfully conceive an infant– might decide not to use the treatment at all. Second, in states with higher number of covered IVF cycles, more low fertile patients use the treatment. Low fertile patients prefer receiving more intensive treatment to increase their chances of success. Then more generous insurance coverage of IVF treatment might not necessarily result in lower incidence of multiple births.

Assume that patients get utility from consumption (c) and having infants (b). Each patient is endorsed with fixed income I. Patients can choose between natural conception N, receiving IVF treatment and adopting an infant A to make decision d to maximize their utility:

$$\max_{d \in \{N, IVF, A\}} U(c, b) = c + v_d(b) \tag{3.1}$$

where $v_d(.)$ denotes utility associated with choice d. b denotes the number of infants resulted from choice d. We assume that patients prefer to have at least one infant and they prefer a singleton birth. We therefore assume that $v_d(.) > 0$ and $v'_d(.) < 0$ for all choice of d. All patients prefer to have their own biological infant with a natural conception where $v_N(.) > v_{IVF}(.)$ and $v_N(.) > v_A(.)$.

Patients' consumption is their income net of cost of their choice d as $c = I - p_d$ where p_d is the cost associated with decision d. The cost of natural conception p_N is set to be zero. The cost of adoption is also set fixed at $p_A = \alpha$. Costs associated with IVF treatment however, consist of two parts. First, pecuniary costs of the treatment that consists of the portion covered by insurance (y) and an out-of-pocket cost (x). Second, non-pecuniary costs associated with receiving IVF treatment (ψ) where $p_{IVF} = y + x + \psi$.

More implanted embryos increases probability of conceiving an infant, as well as chances of higher order pregnancies and possibly births. We therefore assume that the number of infants resulted from an IVF treatment is $b = k\kappa$, where k is the number of implanted embryos and κ is a fixed parameter denoting probability of a natural multiple conception. Low fertile patients can increase their chances of pregnancy by implanting more embryos. We also assume that the number of implanted embryos k is a function of couples' fertility f and the max number of IVF cycles covered by their health insurance $\overline{r}$ as $k = g(f, \overline{r})$ where $g'_f(.) < 0$ and $g'_r(.) < 0$. For simplicity and without loosing generality lets assume $k = \dfrac{1}{f\overline{r}}$.

Probability of conceiving an infant in an IVF cycle is $\phi(f, k)$. We assume that patients' fertility is $f \in [\overline{F}, \underline{F}]$ and $k \in [1, \overline{k}]$ is the number of implanted embryos. $\overline{F}$ and $\underline{F}$ denote fertility of patients with respectively very low and high chances of natural conception. $\overline{k}$ denotes the maximum possible number of implanted embryos suggested by professional IVF provider. We also assume that $\phi_f(.) > 0$, $\phi_k(.) > 0$. Probability of conceiving an infant in $r \in [1, \overline{r}]$ trial of IVF treatment is then $\Phi(f, k, r) = r\phi(f, k)$. For sake of simplicity and without loss of generality we assume $\phi(f, k) = \gamma f k$. γ is the probability of a natural conception.

Patients' fertility f and their insurance coverage for IVF treatment $\overline{r}$ are the only sources of heterogeneity in our model. Figure 3.1 shows patients' decision by their fertility f. Patients with $f \in (\frac{1}{\gamma}, \overline{F}]$ are more likely to naturally conceive an infant. Patients with $f \in [\frac{1}{\gamma \bar{r} \bar{k}}, \frac{1}{\gamma}]$ would decide to use IVF. When the number of covered IVf cycles $\bar{r}$ increases, more patients with low fertility would decide using IVF over an alternative such as adopting a child. These patients would increase their chance of conceiving an infant by implanting more embryos which might therefore result in increase in incidence of multiple births. Patients with $f \in [\underline{F}, \frac{1}{\gamma \bar{r} \bar{k}})$ would decide to adopt an infant.

Patients' fertility and the their preference for enduring non-pecuniary costs associated with IVF treatment are private information. Overall effects of the number of covered IVF cycles on incidence of adverse outcome is then ambiguous and an empirical analysis is required.

3.4 Empirical Analysis

3.4.1 Descriptive evidence

Panel (a) of Figure 3.2 plots share of multiple birth across the states from 1975 to 2014. There is a trend of increase in the share of multiple birth across all the states. Share of multiple birth in states with mandated IVF coverage is relatively higher than the others. Never mandated states and states with mandated to offer IVF treatment have quite similar trends and have the lowest share of multiple birth among the others. Panel (b) plots trends in share of multiple birth across the states with mandated IVF coverage before and after mandates were enacted. Share of multiple birth in all mandated states starts from one percent in years prior to mandated IVF coverage and then gradually increases in the proceeding years. Panel (c) plots the aggregated share of multiple births by the number of IVF cycles mandated to cover. Increase in share of multiple birth in states with more covered cycles is higher in years proceeding the mandated coverage is enacted. Figure 3.3 plots the number of infants per thousand birth across states over time. Trends in number of infants per thousand births are similar to those from share of multiple birth presented in Figure 3.2. These figures suggest that mandated coverage for IVF treatment is associated with increase in incidence of multiple births. Furthermore, association is heterogeneous across the states where it is stronger in states with more covered cycles.

3.4.2 Identification strategy

Estimating causal effects on patients' behavioural responses –measured by incidence of adverse outcome– from increased access to a treatment with high out-of-pocket costs is not straight forward. The main issue here is that patients who use these treatments are a highly selected group among those who need the treatment. We use mandated coverage for IVF treatment in private health insurance plans as an exogenous source of variation in accessibility of the treatment to estimate causal effects.

States in the US. enacted policies to mandate covering IVF treatment in their private health insurance plans at various times. We could in principal use time and state level variation in the number of covered IVF cycles to estimate effects on incidence of multiple birth from the number of covered cycles, using a Difference-in-Differences (DD) framework. The estimated effects would not be interpreted as causal effect if the "parallel trend" assumption is violated. The parallel trend assumption implies that in absence of the treatment (mandated coverage of IV in private health insurance plans), the average outcome (incidence of multiple birth) in treated and control groups (never mandated states) would have followed parallel paths over time. Although this assumption is not directly and empirically testable, it is implausible if the average outcomes in both treated and control groups in pre-treatment does not follow a parallel path.

If pre-treatment characteristics that are believed to be associated with dynamics of the outcome variable are unbalanced between the treated and the untreated groups, then the parallel trend assumption is less likely to hold, for instance, when states mandating IVF coverage are influenced by state-transitory shocks. These unobserved time-varying confounders result in failure of the parallel trend assumption.[16]

Figure 3.2 and Figure 3.3 suggest that the parallel trend assumption might be violated when comparing mandated states with never mandated ones. We therefore use a Generalized Synthetic Control (GSC) framework developed by Xu (2017) to estimate causal effects on the outcome variables of the number of covered IVF cycle. GSC provides a framework to estimate the treatment effect on each treated state when the parallel trend assumption is less likely to hold.[17] We estimate a model of the form:

$$y_{it} = \delta_{it} D_{it} + X'_{it}\beta + \lambda'_i f_t + \epsilon_{it} \tag{3.2}$$

where i and t respectively denote state and time. y_{it} denotes the outcome variable in state i at year t. We use two variables to measure incidence of multiple birth. First, share of multiple birth (twin, triple or higher order) of all live births in each state-year. Second, number of infants per thousand births.

D_{it} is a dummy variable that turns on for treated state i in years following the mandated coverage enacted at time T_{i0}. A mandated coverage of IVF treatment, however, might not affect incidence of multiple birth in the year it is enacted, but can have effects with a two years lag.[18] This is to account for two factors: first, infertility treatments often do not result in an immediate conception and second, even if a conception occurs immediately, there is still a necessary 9-month waiting period before those new conceptions can affect incidence of

[16] There are two main approaches to deal with this issue. The first approach uses a matching method to condition on pre-treatment observable characteristics (Abadie, 2005; Abadie et al., 2010, 2015). This would help balancing the effects of time-varying confounders between the treatment and control groups. The second approach to deal with unobserved time-varying confounders is to model them explicitly. Bai (2009) proposes an interactive fixed effect model which includes state specific intercepts (factor loading) interacted with time varying coefficients (latent factors).

[17] The GSC links the matching and interactive fixed effect methods and brings together the synthetic control and interactive fixed effect models where the Difference-in-Differences model is a special case.

[18] We follow (Schmidt, 2007) to use a two years delay.

multiple birth. This dummy is equal to one for the mandated states two years after enacting mandated coverage of IVF treatment and zero otherwise.

The vector X_{it} is a set of time variant state characteristics to control for any observable differences that might confound the analysis (mothers' age, education, marital status, race and fathers' race and infants sex, birth weight and order of birth). λ_i is a $(r \times 1)$ vector of state specific intercepts (factor loading). f_t is a $r \times 1$ vector of time varying coefficients (latent factors) which captures unobserved common factors. r is the estimated number of confounding factors. The factor component of the model $\lambda_i' f_t$ covers a wide range of unobserved heterogeneity where the conventional fixed effects model is a special case.[19] $\lambda_i' f_t$ absorbs all unobserved confounders that can be decomposed into a state-year multiplicative form, i.e $U_{it} = a_i \times b_t$. It however, does not capture unobserved confounders that are independent across states. ϵ_{it} captures any remaining unobserved components that affects incidence of multiple birth.

The coefficients of interest are δ_{it}. The Average Treatment Effect on Treated (ATT) at time $t > T_{i0}$ is then $\widehat{ATT}_t = \dfrac{1}{|Treated|} \sum_{i \in Treated} \delta_{it}$. $Treated$ denotes treated states.

GSC estimates state level treatment effects on each treated state semi-parametrically. More specifically, treated counter-factual are imputed from a linear interactive fixed effect model. The number of interactive factors r, factor loadings λ_i and latent factors f_t are chosen within a cross-validation procedure which relies on the control group information and information from the treatment group in pre-treatment periods.[20] It then imputes treated counter-factuals based on the estimated factors and factor loadings, in spirit of the weighing scheme of the original synthetic control method (Abadie et al., 2010).[21] More details on

[19]For instance, for $r = 2$ if we set $f_{it} = 1$, $\lambda_{i2} = 1$, $\lambda_{i1} = \alpha_i$ and $f_{2t} = \tau_t$ then $\lambda_i' f_t = \alpha_i + \tau_t$. In this case, the mode is reduced to a model with state and time fixed effects model.

[20]The GSC first estimates an interactive fixed effect model (Bai, 2009), using only the control states data, to get the number of latent factors r. It then estimates factor loadings for each treated state λ_i by linearly projecting pre-treatment treated outcomes onto the space spanned by these factors.

[21]The original Synthetic Control method proposed by Abadie et al. (2010, 2015), matches both pre-treatment observable characteristics and outcome between a treated state and control states and constructs a "synthetic control" unit. More specifically, the synthetic control unit is a weighted combination of the

estimation strategy of a GSC model is provided in Appendix C.1.

The GSC framework has several advantage to the original synthetic control developed by Abadie et al. (2010). First, it allows for more than one treated state with variable treatment periods. Second, making inference on the estimated effects is more reliable since it provides estimates of the standard errors and confidence intervals.[22] Third, it provides a data driven procedure to select the right number of factors in an interacted fixed effect model and reduces risk of over fitting. This approach furthermore enables us to takes advantage of the long pre-treatment panel to decrease the bias of the estimated effects.

3.4.3 Results

Table 3.3 presents the estimated ATT on incidence of multiple birth of the mandated coverage of IVF treatment in private health insurance plans from Equation (3.2). The sample includes all the births from 1975 to 2014 in the US. In all the estimates, the control group includes all 36 never mandated states. The data is aggregated into state-year cells. All the estimates include state and year fixed effects. When the estimated number of unobserved factors r is zero, the model is reduced to the original Synthetic Control. Standard errors are produced by non-parametric bootstrap of 2,000 times. Panel (a) of Table 3.3 presents the estimated effects on share of multiple birth and Panel (b) shows the estimated effects on number of infants per thousand births. Estimated ATT for all mothers is presented in Block A of each table. The first column shows the overall effects of mandated coverage of IVF. Mandated IVF coverage on average causes 0.11 percentage point increase in share of multiple births in states with mandated coverage (from mean 1.20%) relative to never

control groups. The weights associated with the best pre-treatment match are chosen. The treatment effect then is the difference between the treatment and synthetic control groups at post-treatment period. To make an inference, it then compares the estimated effect with the effects estimated from placebo test where the treatment is randomly assigned to the states in the control group. The drawback of the original synthetic control is that first, it only applies to the case of one treated state and second, making inference about the estimated effect is not that straight forward.

[22] GSC estimator uses a parametric bootstrap procedure via re-sampling the residuals to obtain the standard errors of the estimated coefficients. For more details on the bootstrap procedure, see Xu (2017).

mandated states. Mandated coverage on average causes 1.69 more infants per thousand births (from 1,012 infant per thousand births) in mandated states. Figure 3.4 plots the treated average and estimated average for the treated states and the estimated ATT on share of multiple births in Panel (a) and on number of infants per thousand births in Panel (b). Estimated effects after controlling for demographic characteristics including mothers' age, education, race, marital status, fathers' race and infants sex, birth weight and order of birth are presented in the second columns. Estimated effects drop down to 0.03 percentage point on share on multiple births and -1.00 on the number of infants per thousand births. These estimates however are less significant at the conventional levels.

Columns three to twelve in each table present ATT from mandated coverage by the number of covered cycles (1 to +5 covered cycles). Estimated effects are the highest for states with +5 covered cycles. It is 0.45 percentage points increase in share of multiple births and 4.79 more infants per thousand births and significant. The estimated effects are the lowest in states with one covered cycle, respectively -0.09 and -0.30 and not significant. Estimated effects for states with three and four covered cycles IVF are quite small and not significant at conventional levels. Figure 3.5 to Figure 3.9 plot the treated average and estimated average for the treated states and the estimated ATT respectively for states with one cycle of mandated coverage up to +5 mandated cycles. Estimated effects after controlling for demographic characteristics are relatively smaller but still main findings hold.

Certain demographics of women are more likely to use IVF treatment, and therefore are more likely to be affected by its mandated coverage in their private health insurance plans. It is known that women's age is an important factor in determining their fertility problems, where infertility increases by women's age. Panel (a) of Figure 3.10 plots share of multiple births by mothers age from 1975 to 2015. Panel (b) plots the trends separately for 1975-1994 and 1995-2014. Incidence of multiple birth increases by mothers' age where it is higher in recent decades. The age of 35 is known to be a turning point in women's fertility.[23] Recent increase in returns to education gives incentives to more women to postpone childbearing to acquire more education and skills to advance their professional career. Highly educated women furthermore are more likely to have jobs that provide health insurance plans with IVF coverage. It is then expected that women 35 years and older are more likely than younger women to use IVF treatment. The number of covered cycles is then more likely to affect their decision to use an IVF treatment at all as well as the intensity of their treatment if they decide to use (since older women are naturally less fertile).

Estimated effects of mandated IVF coverage for mothers 35 years and older are presented in Block B of Table 3.3. A noticeable finding is that all the estimated effects are larger in magnitude and more significant than the estimates for all mothers presented in Block A. As shown in the first column, mandated coverage causes an average 0.24 percentage point increase in share of multiple births (from mean 1.64%) and 2.41 more infants per thousand births (from mean 1,017 infants per thousand births) in mandated states relative to never mandated states. Estimated ATT from mandated coverage of IVF on incidence of multiple births increases with the number of covered cycles. Estimated effect in states with one covered cycle is 0.40 decrease in share of multiple births (from mean 1.39%) and 4.79 infants less per thousand births (from mean 1,014.21). Estimated effects in states with +5 covered cycles is 0.88 percentage points increase in share of multiple births (from mean 1.27%) and 9.42 more infants per thousand births (from mean 1,012.40). These estimates are all significant in conventional levels. Estimated effects after controlling for demographic characteristics do not change much (in most cases they tend to be even larger) and are still significant.

Married women struggling with fertility are believed to seek for infertility treatment and

[23]The Patients' Fact Sheet on the Prediction of Fertility Potential in Older Female Patients from The American Society for Reproductive Medicine states that "Approximately one-third of couples in which the female partner is age 35 or older will have problems with fertility. It is estimated that two-thirds of women will not be able to get pregnant spontaneously by the age of 40." Source: `http://www.aia-zavos.com/HomeSemenAnalysis.com/Older_Female-Fact.pdf`, Accessed on July 1, 2017.

specially IVF more often than unmarried women. Women with college and higher degree also for reasons mentioned above are more likely to use IVF treatment. Panel C and D of Table 3.3 present the estimates respectively for married and mothers with college or higher degree. Estimated effects are relatively small and they tend to be even smaller after controlling for demographic characteristics. Estimated effects however follow the same trend; mandated coverage has smaller effects on incidence of multiple birth in states with lower number of covered cycles and higher in states with more covered cycles.

Effects of mandated IVF coverage on incidence of multiple birth might vary also by mothers' race. First, there are differences in timing of childbearing by mothers' age. Second, even though white women are less likely to struggle with fertility, but they are more likely to seek for infertility treatments (Bitler and Schmidt, 2006). Block E of Table 3.3 presents estimates for white mothers. Estimated effects however are not much different than those for all mothers. Estimated effects do not change much after controlling for demographic characteristics. Estimated effects from the number of covered cycles follows the same trend as those for the others.

Our estimates from GSC framework show that mandated coverage of IVF treatment in private health insurance plans causes increase in incidence of multiple births. Estimated effects are the highest in states with +5 covered cycles and the lowest in states with only one covered cycle. This finding also holds when we estimate effects for demographic groups of mothers who are more likely to use IVF treatment and therefore are more likely to be affected by increase accessibility of IVF treatment. These findings are all consistent with predictions of our conceptual framework presented in Section 3.3.[24]

[24]Estimated effects of mandated IVF coverage from a Difference-in-Differences (DD) framework using variation overs state and time are presented in Appendix C.2. Our study sample includes all the births from 1975-2014. The data is aggregated in state-year cells. Table C.1 and Table C.2 present estimated effects respectively on share of multiple births and number of infants per thousand births. Panel (a) in each table presents estimated effects from different number of covered cycles. Panel (b) in each table presents overall estimated effects of mandated IVF coverage on incidence of multiple birth. Similar to our GSC estimations, we also estimate effects on demographics that are more likely to use IVF treatment and therefore are more likely to be affected by mandated coverage. Since parallel trend assumption is less likely to hold –as plotted in Panel (c) of Figure 3.2 and Figure 3.3– these estimated effects are biased and might not be interpreted

Robustness analysis

To further check robustness of our findings from GSC framework, we also estimate effects of mandated coverage of IVF on incidence of multiple birth in a Difference-in-Differences-in-Differences (DDD) framework. Our findings from the GSC framework suggest that 35 years and older women have the strongest behavioural responses to the increased accessibility of IVF treatment. In our DDD analysis we further refine the treatment group by mothers' age. We use variation in mandated coverage over state, year and mothers' age (below and above 35 years old). The treatment group includes states with mandated coverage of IVF in their private health insurance plans. The control group includes 36 never mandated states, We estimate the following equation:[25]

$$
\begin{aligned}
y_{ita} =& \alpha_0 + \alpha_1 D_{ita} + X'_{ita}\alpha_2 + \alpha_3(Plus35_a \times Mandated_{it}) \\
&+ \alpha_4(Plus35_a \times year_t) + \alpha_5 Mandated_{it} + \alpha_i + \alpha_t + \alpha_a + \eta_{ita}
\end{aligned}
\tag{3.3}
$$

where i, t and a denote respectively state, year and mothers' age. Our study sample includes all the births in the US. from 1975 to 2014. We aggregate the data into state-year-age cells. y_{ita} denotes the outcome variable at state i, year t and age a cell. Similar to our GSC analysis, we use share of multiple births and number of infants per thousand births as measures of incidence of multiple births. For reasons mentioned earlier, mandated coverage of IVF treatment might not affect incidence of multiple birth in the year it is enacted, but can have effects with a two year lag. D_{ita} is dummy that turns on for treated states two years after the mandated coverage and mothers older than 35 years. $Plus35_a$ is a dummy that turns on for cells with mothers aged over 35 years. $Mandated_{it}$ is another dummy that switches on two years after the mandated IVF coverage is enacted. The vector X_{ia} includes a set of demographic characteristics including mothers' education, race, marital

as causal effects. Magnitude of the estimates from DD model of course are different than those from GSC (smaller in some cases and larger in some other cases), but still the general finding from GSC model holds.

[25]This approach follows Schmidt (2007, 2005); Gruber (1994).

status, fathers' race and infants sex, birth weight and order of birth. α_i, α_t and α_a capture respectively state, time and age fixed effects. η_{ita} captures any remaining unobserved factors affecting incidence of multiple births. The coefficient of interest is α_1 which captures effect of mandates coverage for IVF on 35 years and older in mandated states relative to the younger mothers.

We also estimate a DDD model using individual level data which potentially would be more precise than the estimates using the aggregated data. We estimate the following equation:

$$\begin{aligned} y_{jita} =& \alpha_0 + \alpha_1 D_{jita} + X'_{jita}\alpha_2 + \alpha_3(Plus35_{jita} \times Mandated_{it}) \\ &+ \alpha_4(Plus35_{jita} \times year_t) + \alpha_5 Mandated_{it} + \alpha_i + \alpha_t + \alpha_a + \eta_{jita} \end{aligned} \tag{3.4}$$

where j denotes individuals and rest of the notations are the same as those in Equation (3.3). y_{ijta} is the dependent variable which is a dummy turning on for multiple births. D_{jita} is a dummy that turns on for mothers older than 35 in mandated states two years after mandated coverage is enacted. The coefficient of interest is α_1 which measures the effects of mandated IVF coverage on probability of a multiple birth.

The DDD estimate starts with the time change in mean incidence of multiple birth for older mothers in states with mandated IVF coverage. It then nets out the change in means for older mothers in the control states and the change in means for the younger mothers in mandated states. The hope is that this controls for two kinds of potentially confounding trends. First, changes in incidence of multiple birth of older women across states (that would have nothing to do with mandated IVF coverage). Second, changes in incidence of multiple birth for all mothers living in mandated states, possibly due to other state policies or state-specific changes in the economy that affect affect women's fertility decisions.

Table 3.4 and Table 3.5 present estimated effects of the mandated IVF coverage using data aggregated by state-year-age respectively on share of multiple births and number of infants per thousand births. Table 3.6 presents estimated effects on probability of accruing multiple birth using individual level data. Panel (a) in each table presents estimated effects from the number of covered cycles in mandated states and Panel (b) presents overall effects of mandated coverage on incidence of multiple births. All standard error are clustered in state level.

The are three main findings from our DDD analysis. First, estimated effects from DDD model are in general larger than the corresponding estimates from GSC model. This might be because in DDD model we compare older mothers to younger mothers in mandated states while in GSC model we compare mothers in mandated states to mothers in never mandated states. Second, estimated effects using individual level data are smaller than those using the aggregate data. Third, findings from our DDD model are along the line with findings from our GSC analysis. Estimated effects of mandated IVF coverage on incidence of multiple birth are higher is states with more covered IVF cycles and lower in states with less covered cycles.

3.5 Mandated IVF coverage and child adoption

Individuals who are not able to conceive an infant have two alternative pathways: using an ART treatment such as IVF or adopting a child. Either of these choices have pros and cons. Despite the technological advances IVF treatment is still risky and expensive. Adopting a child is quite expensive and can be a very long process. Furthermore, lots of individuals might prefer to have their own biological child. More than half of the individuals who received infertility treatment also considered adoption (Chandra et al., 2005). Gumus and Lee (2012) show that one-third of individuals who consider adoption, also have sought IVF treatment.

The extent to which individuals consider IVF treatment and adoption as alternatives however is an empirical question. Gumus and Lee (2012) investigate effects of child adoption on utilization of IVF. They show that 10% increase in adoptions leads to a 1.3%-1.5% decrease in the number of IVF cycles performed. On the other hand, policy interventions such as mandated coverage of IVF treatment in private health insurance plans, increase accessibility of the treatment to more patients by decreasing out-of-pocket costs. In our conceptual framework presented in Section 3.3, individuals based on their fertility level and the number of covered IVF cycles in their health insurance plan choose between using IVF treatment or adopting a child. More covered cycles gives incentives to more less fertile patients to choose IVF treatment rather than adopting a child. We therefore might expect that adoption rates be lower in states with higher number of covered IVF cycles. In this section, we provide suggestive empirical evidence supporting this prediction of our conceptual framework.

We use data from Adoption and Foster Case Analysis and Reporting System (AFCARS) from 1994 to 2014 to investigate how mandated coverage of IVF treatment affects adoption rates.[26] These data files include information on foster and adopted children as well as their adoptive and foster parents. We use only adopted children for our analysis. The data has information on adopted child's age, race and sex. It also has information on adoptive parents' age and race. It further more has information on the year and state that the adoption is finalized. We measure adoption rate as ratio of total number of adopted children to total number of infants born in a state-year cell. We use total number of infants from the Natality Detail files.

Table 3.7 presents summary statistics from the AFCARS from 1994 to 2014. We present summary statistics separately from 1994 to 2003 and 2004 to 2014. Panel (a) of Figure 3.11

[26]The AFCARS is a federally mandated data collection system intended to provide case specific information on all children covered by the protections of Title IV-B/E of the Social Security Act (Section 427). States are required to collect data on all children in foster care for whom the State child welfare agencies have responsibility for placement, care or supervision and on children who are adopted under the auspices of the State's public child welfare agency. The AFCARS data files are given annually to the National Data Archive on Child Abuse and Neglect (NDACAN) for distribution to the research community by the Children's Bureau.

plots adoption rates by number of covered IVF cycles in private health insurances. Adoption rates are higher in never mandated states and relatively higher in mandated to offer states. Panel (b) provides a close up on mandated to cover states. This figure suggests that adoption rates are relatively lower in states with higher number of covered IVF cycles.

From 1995 to 2014 that adoption data is available, three states including Connecticut (2005, 2 cycles), Maryland (2000, 3 cycles) and New Jersey (2001, 4 cycles) have enacted policies to mandate covering IVF in their private health insurance plans. We potentially could use state-time variation in mandated IVF coverage to estimate effects of the number of covered IVF cycles on adoption rates in a DD framework. Figure 3.11 however suggest that the identification assumption –parallel trend assumption– is less likely to hold. But also, since the number of pre-treatment periods are too small we can not use a GSC framework. We therefore use state-year-age variation and estimate the effects using a DDD framework. We aggregate data into state-year-age cells and estimate the model specified in Equation (3.3). Dependent variable is natural logarithm of adoption rate defined as the ratio of total number of adoption to total number of births in each state-year-age cell.

Table 3.8 shows the estimated effects from a DDD model specified in Equation (3.3). Estimated effects are consistent with prediction of our conceptual framework. More generous insurance plans are associated with lower adoption rates. These estimates however might be biased and therefore should be interpreted cautiously. The higher the number of covered IVF cycles is, more patients with lower fertility choose IVF treatment over adoption.

3.6 Conclusion and policy implications

What are behavioural responses to the number of covered treatments of a medical procedure –an expensive one that more than one treatment is required– in health insurance plans? Expensive treatments such as IVF might not be covered by many private health insurance plans. Mandated coverage of expensive treatments in private health insurance plans is a policy intervention that aims to increase accessibility of these expensive and technologically advanced treatments. However, this intervention might effect patients utilization behaviour and impose more burden on health care system both in terms of cost associated with utilization of the service and costs associated with adverse outcome of treatment. These kind of policy interventions however are rarely found in practice and therefore there are not much empirical evidence on effects of increases accessibility of expensive treatments on patients' utilization behaviour.

We exploit a policy intervention that mandated covering In-Vitro-Fertilization (IVF) –an expensive infertility treatment– in private health insurance plans in fifteen states in the US. We provide the first estimate of effects on incidence of multiple birth– adverse outcome of an aggressive treatment– from the number of IVF cycles covered in mandated health insurance plans. We use Natality Detail Files from 1975-2014 to estimate causal effects using a Generalized Synthetic Control (GSC) framework. Our finding shows that incidence of multiple birth in states with more covered cycles is higher. We also estimate the effects of increased accessibility of IVF on adoption market as a main alternative to IVF for patients with low chance of success. We use variation in state, year and mothers' age in a Difference-in-Difference-in-Differences (DDD) framework. Our findings shows that more older women (low fertile patients who need more intensive treatments) in states with more covered cycles use IVF treatment rather than adopting a child. This finding is suggestive on a channel through which mandated coverage of IVF affects incidence of multiple births. High out-of-pocket costs associated with IVF treatment might affect pool of patients who use the treatment.

Our findings have important policy implications for improving accessibility of expensive and technologically advanced medical treatments. Increased access to these treatments without regulating intensity of the treatment imposes burden on public health care both in terms of costs associated with treatment utilization and induced costly and complicated adverse outcomes (multiple birth in IVF treatment). As suggested by Hamilton et al. (2016) and Einav et al. (2016), regulation could be in from of limiting intensity of treatment (number of implanted embryos in a cycle of IVF treatment) and imposing top-up price for intense treatments (implanting additional embryos in a cycle of IVF treatment).

3.7 Tables

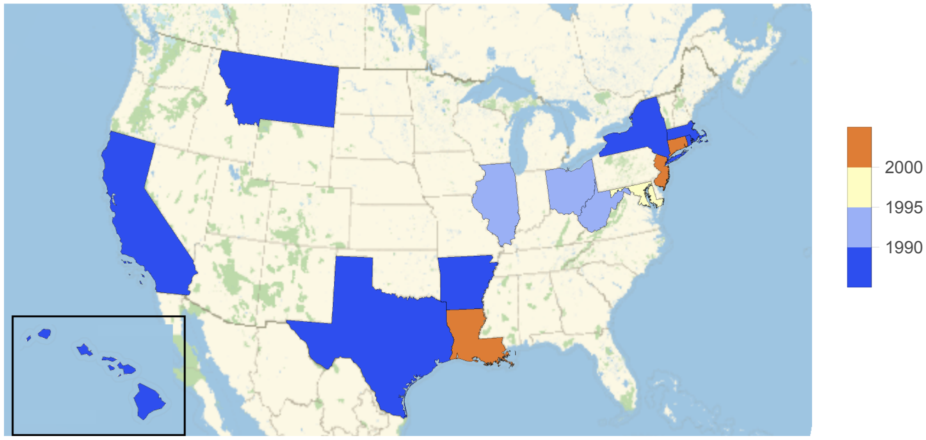
Table 3.1: In-Vitro-Fertilization (IVF) coverage in employer provided health insurances

(a) Number of covered IVF cycles

State	**Year mandate enacted**	**IVF coverage**	
		Mandated to offer	Mandated to cover
Arkansas[27]	1987		1 cycle
Massachusetts[28]	1987		No limit
Montana	1987	x	
Texas	1987	x	
California	1989	x	
Hawaii	1989		1 cycle
Rhode Island[29]	1989		3 cycles
New York[30]	1990	x	
Illinois[31]	1991		4 cycles
Ohio	1991	x	
West Virginia[32]	1995	x	
Maryland[33]	2000		3 cycles
Louisiana	2001	x	
New Jersey	2001		4 cycles
Connecticut[34]	2005		2 cycles

Note: This table presents the level of IVF coverage in employer provided health insurance in states in the US. which have passed legislations regarding covering IVF treatment in their employer provided health insurances. In states flagged with "mandated to cover", insurance companies are mandated to offer insurance plans that covers IVF treatment and, employers may choose not to purchase these plans. In states flagged with "mandated to cover", employers are mandated to purchase health plans with IVF coverage. In the rest of the states, there is no legislation regarding IVF coverage in the health insurances. We use these states as a control group in our empirical analysis. Source: RESOLVE: The National Infertility Association `http://www.resolve.org/family-building-options/insurance_coverage/state-coverage.html` [Accessed on June 15, 2017].

(b) Time of mandated IVF coverage



[27]Lifetime maximum of $15,000 for coverage.

[28]The law does not limit the number of IVF cycles and does not have a dollar lifetime cap. Insurers however, may set limits based on their clinical guidelines and patients' medical histories.

[29]Insurers that cover pregnancy benefits, must provide coverage for medically necessary expenses of diagnosis and treatment of infertility. The law imposes a $100,000 cap on treatment. The insurer however, may impose up to a 20% co-payment. This would then cover about 3 IVF cycles.

[30]Laws has been passed at 2002 to strengthen the employer provided health insurances in New York, but still it does not cover IVF treatment.

[31]Each patient is covered for up to 4 egg retrievals. However, if a live birth occurs, two additional egg retrievals will be covered, for a lifetime maximum of six retrievals.

[32]West Virginia is the first state in the US. who mandated to offer IVF treatment in private health insurances. They however, updated their regulation in 1995 to exclude IVF from coverage.

[33]Private health insurance were mandated to offer IVF treatment since 1985. The regulation updated in 2000 to cover IVF treatment. Individual and group insurance policies that provide pregnancy-related benefits must cover the cost of 3 cycles per live birth, with a lifetime maximum of $100,000.

[34]Since 1989, health insurance providers in Connecticut were mandated to offer IVF coverage.

Table 3.2: Summary statistics form Natality Detail Files

	1975-1994			*1995-2014*		
	Never mandated	Mandated to offer	Mandated to cover	Never mandated	Mandated to coffer	Mandated to cover
Multiple birth (%)	1.08	1.07	1.14	1.59	1.56	1.92
	(10.32)	(10.27)	(10.60)	(12.51)	(12.41)	(13.71)
Twin birth (%)	1.06	1.05	1.11	1.54	1.51	1.85
	(10.23)	(10.18)	(10.49)	(12.32)	(12.41)	(13.46)
Triplet or higher birth (%)	0.02	0.02	0.02	0.05	0.05	0.07
	(1.36)	(1.34)	(1.58)	(2.21)	(2.23)	(2.66)
Number of infants per thousand births	1,010.77	1,010.81	1,011.15	1,016.24	1,016.18	1,019.09
	(1.45)	(1.26)	(1.76)	(2.10)	(2.10)	(3.63)
Mean mother age	25.45	25.84	26.31	27.09	27.48	28.40
	(5.49)	(5.65)	(5.62)	(6.01)	(6.23)	(6.18)
Mothers older than 35 years (%)	6.16	7.52	8.01	12.31	14.58	17.31
	(24.5)	(26.38)	(27.14)	(32.86)	(35.29)	(37.83)
Married mothers (%)	76.70	74.04	74.74	63.21	62.33	64.91
	(42.27)	(43.84)	(43.45)	(48.22)	(48.46)	(47.72)
Mothers with some college or higher degree (%)	38.79	59.47	42.53	63.64	55.50	76.02
	(48.73)	(49.09)	(49.43)	(48.22)	(49.70)	(42.76)
White mothers (%)	80.82	80.54	76.76	78.41	78.90	73.24
	(39.37)	(39.59)	(42.24)	(41.14)	(40.80)	(44.27)
First born infants (%)	34.66	36.22	34.91	32.82	33.55	31.77
	(47.59)	(48.06)	(47.67)	(46.95)	(47.21)	(46.56)
Mean birth weight of infants (KG)	3.36	3.35	3.35	3.31	3.31	3.32
	(0.66)	(0.64)	(0.65)	(0.63)	(0.60)	(0.64)
Number of births	37,086,355	25,581,835	10,783,230	41,149,716	27,553,999	10,766,541

Note: This table presents the summary statistics from data from the National Center for Heath Statistics' Natality Detail Files from 1975 to 2014. The data includes infant records from all births in 51 states of the US. Montana, Texas, California, New York, Ohio, West Virginia and Louisiana legislated policies to mandate offering IVF treatment in their private health insurances. Arkansas, Hawaii, Connecticut, Rhode Island, Maryland, Illinois and New Jersey and Massachusetts enacted policies to mandated covering IVF treatments. The rest of the states have never enacted policies to cover or offer IVF treatment in private health insurances. The weights constructed as described in Section 3.2.3 are used to calculate statistics in this table. Standard deviations are presented in parenthesis.

Table 3.3: Estimated Average Treatment effect on Treated (ATT) from Generalized Synthetic Control (GSC) model

(a) Share of multiple births (%)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. All mothers	0.11**	0.03*	-0.09	-0.03	0.17*	0.11	0.07	0.03	0.08	0.09*	0.45***	0.36***
	(0.06)	(0.09)	(0.07)	(0.10)	(0.09)	(0.12)	(0.11)	(0.09)	(0.10)	(0.13)	(0.09)	(0.09)
Mean	1.20	1.20	0.96	0.96	1.46	1.46	1.16	1.16	1.27	1.27	1.04	1.04
	(0.35)	(0.35)	(0.11)	(0.11)	(0.47)	(0.47)	(0.26)	(0.26)	(0.33)	(0.33)	(0.08)	(0.08)
Unobserved factors (r)	1	5	0	0	2	5	1	1	1	5	0	0
B. +35 years mothers	0.24**	0.24**	-0.40**	-0.30	0.45**	0.39*	0.71**	1.19***	0.41**	0.34*	0.88***	0.94***
	(0.11)	(0.13)	(0.19)	(0.21)	(0.23)	(0.23)	(0.24)	(0.34)	(0.19)	(0.23)	(0.26)	(0.27)
Mean	1.64	1.64	1.39	1.39	2.09	2.09	1.57	1.57	1.67	1.67	1.27	1.27
	(0.64)	(0.64)	(0.37)	(0.37)	(0.87)	(0.87)	(0.51)	(0.51)	(0.62)	(0.62)	(0.14)	(0.14)
Unobserved factors (r)	3	1	1	0	2	2	3	4	2	2	1	0
c. +College mothers	0.14	0.25***	-0.16*	-0.06	-0.06	0.17	0.23	0.17	0.06	0.14	0.49***	0.59**
	(0.08)	(0.08)	(0.09)	(0.13)	(0.16)	(0.15)	(0.13)	(0.12)	(0.16)	(0.15)	(0.11)	(0.22)
Mean	1.30	1.30	1.02	1.02	1.60	1.60	1.25	1.25	1.39	1.39	1.11	1.11
	(0.42)	(0.42)	(0.16)	(0.16)	(0.53)	(0.53)	(0.32)	(0.32)	(0.44)	(0.44)	(0.09)	(0.09)
Unobserved factors (r)	3	2	0	0	3	2	1	1	5	5	0	4
D. Married mothers	0.11**	0.23**	-0.11	-0.06	0.19*	0.36*	0.22*	0.23***	0.19*	0.18	0.63***	0.49**
	(0.10)	(0.12)	(0.09)	(0.12)	(0.15)	(0.19)	(0.15)	(0.08)	(0.16)	(0.19)	(0.13)	(0.20)
Mean	1.31	1.31	0.97	0.97	1.66	1.66	1.26	1.26	1.36	1.36	1.08	1.08
	(0.46)	(0.46)	(0.12)	(0.12)	(0.62)	(0.62)	(0.33)	(0.33)	(0.45)	(0.45)	(0.08)	(0.08)
Unobserved factors (r)	5	5	0	0	5	5	1	0	5	5	0	3
E. White mothers	0.14**	0.15**	-0.07	-0.06	0.15	0.17	0.09	0.08	0.17	0.13	0.51***	0.36***
	(0.06)	(0.07)	(0.09)	(0.17)	(0.14)	(0.12)	(0.13)	(0.12)	(0.11)	(0.14)	(0.11)	(0.14)
Mean	1.21	1.21	0.99	0.99	1.48	1.48	1.16	1.16	1.26	1.26	1.02	1.02
	(0.38)	(0.38)	(0.15)	(0.15)	(0.52)	(0.52)	(0.30)	(0.30)	(0.39)	(0.39)	(0.08)	(0.08)
Unobserved factors (r)	2	3	0	0	2	5	2	3	2	5	0	0
Treatment	Mandated	Mandated	1 cycle	1 cycle	2 cycles	2 cycles	3 cycles	3 cycles	4 cycles	4 cycles	+5 cycles	+5 cycles
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Treated states	8	8	2	2	1	1	2	2	2	2	1	1
Control states	36	36	36	36	36	36	36	36	36	36	36	36
Number of cells	1,760	1,760	1,520	1,520	1,480	1,480	1,520	1,520	1,520	1,520	1,480	1,480

(b) Number of infants per thousand births

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. All mothers	1.69**	-1.00	-0.30	1.86*	2.05*	2.05*	0.68	0.13	0.89	1.83*	4.79***	3.86***
	(0.66)	(0.81)	(0.71)	(1.14)	(0.97)	(1.25)	(1.17)	(1.13)	(1.12)	(1.29)	(0.92)	(0.96)
Mean	1,012.35	1,012.35	1,009.70	1,009.70	1,015.13	1,015.13	1,011.93	1,011.93	1,013.07	1,013.07	1,010.55	1,010.55
	(3.78)	(3.78)	(1.12)	(1.12)	(5.07)	(5.07)	(2.87)	(2.87)	(3.72)	(3.72)	(0.84)	(0.84)
Unobserved factors (r)	1	4	0	0	2	4	1	2	1	4	0	0
B. +35 years mothers	2.41**	2.77**	-4.79*	-3.34	4.93**	4.25**	5.86**	11.81***	4.08**	3.44*	9.42***	10.10***
	(1.11)	(1.43)	(2.07)	(2.25)	(2.27)	(2.34)	(2.38)	(3.56)	(2.02)	(2.31)	(2.75)	(2.98)
Mean	1,017.02	1,017.02	1,014.21	1,014.21	1,021.88	1,021.88	1,016.31	1,016.31	1,017.40	1,017.40	1,012.90	1,012.90
	(6.95)	(6.95)	(3.71)	(3.71)	(9.41)	(9.41)	(5.59)	(5.59)	(6.95)	(6.95)	(1.48)	(1.48)
Unobserved factors (r)	1	1	1	0	2	1	3	4	2	2	1	0
C. +College mothers	0.78	2.58**	-1.77*	-0.63	-1.12	0.76	2.37	1.91	0.32	0.84	5.15***	6.19**
	(0.78)	(0.85)	(0.93)	(1.37)	(1.68)	(1.57)	(1.45)	(1.33)	(1.97)	(1.81)	(1.31)	(2.84)
Mean	1,013.48	1,013.48	1,010.39	1,010.39	1,016.61	1,016.61	1,012.89	1,012.89	1,014.51	1,014.51	1,011.32	1,011.32
	(4.60)	(4.60)	(1.65)	(1.65)	(5.73)	(5.73)	(3.58)	(3.58)	(4.93)	(4.93)	(0.92)	(0.92)
Unobserved factors (r)	1	2	0	0	5	1	1	1	5	5	0	4
D. Married mothers	2.38**	1.94*	-1.28	-0.54	2.83*	2.97**	2.14*	1.39	1.73	1.26	6.70***	5.06***
	(0.92)	(1.30)	(1.02)	(1.31)	(1.48)	(1.80)	(1.54)	(1.41)	(1.66)	(2.02)	(1.40)	(1.20)
Mean	1,013.56	1,013.56	1,009.90	1,009.90	1,017.35	1,017.35	1,013.01	1,013.01	1,014.22	1,014.22	1,010.98	1,010.98
	(5.06)	(5.06)	(1.25)	(1.25)	(6.62)	(6.62)	(3.69)	(3.69)	(5.02)	(5.02)	(0.89)	(0.89)
Unobserved factors (r)	2	5	0	0	3	5	1	2	5	5	0	0
E. White mothers	1.30**	1.45**	-0.78	-0.51	1.51	1.73*	0.52	0.86	1.71	1.31	5.40***	3.84***
	(0.73)	(0.74)	(0.92)	(1.86)	(1.33)	(1.31)	(1.36)	(1.29)	(1.29)	(1.58)	(1.16)	(1.45)
Mean	1,012.45	1,012.45	1,010.03	1,010.03	1,015.42	1,015.42	1,011.91	1,011.91	1,013.10	1,013.10	1,010.39	1,010.39
	(4.21)	(4.21)	(1.54)	(1.54)	(5.57)	(5.57)	(3.30)	(3.30)	(4.37)	(4.37)	(0.82)	(0.82)
Unobserved factors (r)	2	2	0	0	2	2	2	3	2	5	0	0
Treatment	Mandated	Mandated	1 cycle	1 cycle	2 cycles	2 cycles	3 cycles	3 cycles	4 cycles	4 cycles	+5 cycles	+5 cycles
Covariates	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Treated states	8	8	2	2	1	1	2	2	2	2	1	1
Control states	36	36	36	36	36	36	36	36	36	36	36	36
Number of cells	1,760	1,760	1,520	1,520	1,480	1,480	1,520	1,520	1,520	1,520	1,480	1,480

Note: This table presents the estimated Average Treatment effect on Treated (ATT) on incidence of multiple birth from Generalized Synthetic Control (GSC) model specified in Equation (3.2). The main sample includes all the births in the US. from 1978-2014, aggregated into state-year cells. The control group for each model includes the states who never been mandated to cover IVF treatment in their private health insurances. The included demographic characteristics are mothers' age, education, race, marital status, fathers' race and infants sex, birth weight and order of birth. Bootstrapped standard errors are presented in parenthesis (2,000 draws). Panel (a) presents the estimated effects on share of multiple births and Panel (b) shows the estimated effects on number of infants per thousand births.

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 3.4: Estimated effects of mandated coverage of IVF on share of multiple births from a Difference-in-Difference-in-Differences model using aggregated data

(a) Number of covered cycles in mandated states versus never mandated states

	All mothers		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 cycle * Plus 35 * Post mandate	-1.41	-0.80	-1.88***	-1.50***	-1.28	-0.90	-1.49*	-0.70
	(0.78)	(0.76)	(0.52)	(0.41)	(0.94)	(0.94)	(0.67)	(0.67)
1 cycle * Post mandate	-0.04	0.40	0.02	0.45	-0.10	0.26	-0.04	-0.04
	(0.04)	(0.44)	(0.07)	(0.35)	(0.10)	(0.48)	(0.05)	(0.09)
2 cycles * Plus 35 * Post mandate	0.23	1.49***	0.02	0.52*	-0.10	1.12***	0.16	1.25***
	(0.18)	(0.21)	(0.21)	(0.21)	(0.18)	(0.23)	(0.19)	(0.27)
2 cycles * Post mandate	0.12***	0.34*	0.05**	0.64***	0.15***	0.32	0.13***	0.16
	(0.01)	(0.15)	(0.02)	(0.11)	(0.02)	(0.16)	(0.01)	(0.14)
3 cycles * Plus 35 * Post mandate	-0.06	0.39*	-0.56*	-0.49	0.06	0.29	-0.17	0.02
	(0.18)	(0.17)	(0.27)	(0.28)	(0.20)	(0.16)	(0.17)	(0.16)
3 cycles * Post mandate	0.13***	0.37***	0.18**	0.53***	0.32**	0.36**	0.13**	0.14
	(0.01)	(0.09)	(0.05)	(0.08)	(0.12)	(0.13)	(0.04)	(0.10)
4 cycles * Plus 35 * Post mandate	1.58***	1.81**	1.67***	1.71***	1.55***	1.80***	1.82***	1.89***
	(0.34)	(0.60)	(0.21)	(0.24)	(0.36)	(0.51)	(0.34)	(0.52)
4 cycles * Post mandate	0.25***	0.22	0.22***	0.38	0.36**	0.30	0.27***	0.15
	(0.06)	(0.15)	(0.04)	(0.26)	(0.12)	(0.22)	(0.07)	(0.10)
+5 cycles * Plus 35 * Post mandate	2.61***	2.25***	2.67***	1.40***	3.18***	2.42***	2.87***	2.10***
	(0.18)	(0.17)	(0.22)	(0.30)	(0.18)	(0.18)	(0.20)	(0.18)
+5 cycles * Post mandate	0.13***	0.88***	0.13***	1.72***	0.19***	0.79***	0.14***	0.64***
	(0.01)	(0.14)	(0.02)	(0.33)	(0.02)	(0.10)	(0.01)	(0.07)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Number of cells	59,637	59,637	53,935	53,935	54,752	54,752	59,183	59,183

(b) Mandated to cover states versus never mandated states

	All mothers		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mandated cover * Plus 35 * Post mandate	0.16	0.66	-0.01	0.04	0.21	0.57	0.26	0.52
	(0.61)	(0.47)	(0.64)	(0.48)	(0.67)	(0.51)	(0.64)	(0.48)
Mandated cover * Post mandate	0.23*	0.47**	0.23*	0.71***	0.33*	0.46**	0.23*	0.27*
	(0.10)	(0.16)	(0.09)	(0.18)	(0.13)	(0.17)	(0.11)	(0.13)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Number of cells	59,637	59,637	53,935	53,935	54,752	54,752	59,183	59,183

Note: This table presents the estimated average effects of mandated coverage of IVF in private health insurances on share of multiple birth from Difference-in-Difference-in-Differences (DDD) model specified in Equation (3.3). The main sample includes all the births in the US. from 1978-2014, aggregated by state-year-age. The control group for each model includes the states who never been mandated to cover IVF treatment in their private health insurances. The included demographic characteristics are mothers' age, education, race, marital status, fathers' race and infants sex, birth weight and order of birth. Standard errors presented in parenthesis are clustered in state level. Panel (a) presents the estimated effects from the number of covered cycles and Panel (b) shows the overall effects of mandated coverage.

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 3.5: Estimated effects of mandated coverage of IVF on number of infants per thousand births from a Difference-in-Differences-in-Differences model using aggregated data

(a) Number of covered cycles in mandated states versus never mandated states

	All mothers		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 cycle * Plus 35 * Post mandate	-15.26*	-8.83	-20.19***	-16.37***	-14.05	-10.08	-16.45*	-8.29
	(7.40)	(7.24)	(4.99)	(3.77)	(9.05)	(8.91)	(6.12)	(5.77)
1 cycle * Post mandate	-0.44	4.24	0.12	4.73	-1.11	2.90	-0.43	-0.47
	(0.41)	(4.83)	(0.70)	(3.91)	(0.94)	(5.48)	(0.52)	(1.10)
2 cycles * Plus 35 * Post mandate	0.60	13.69***	-0.23	4.89*	-2.91	9.76***	0.83	11.91***
	(1.81)	(2.26)	(2.18)	(2.28)	(1.91)	(2.38)	(1.98)	(2.83)
2 cycles * Post mandate	1.20***	3.52*	0.45*	6.64***	1.40***	3.11	1.31***	1.62
	(0.10)	(1.56)	(0.18)	(1.24)	(0.19)	(1.75)	(0.12)	(1.43)
3 cycles * Plus 35 * Post mandate	-1.31	3.39	-5.58	-4.89	0.10	2.39	-2.10	-0.36
	(2.47)	(2.42)	(3.69)	(3.89)	(1.77)	(2.12)	(2.17)	(1.94)
3 cycles * Post mandate	1.50***	3.95***	1.94***	5.58***	3.30**	3.74**	1.53**	1.67
	(0.15)	(0.92)	(0.48)	(0.80)	(1.12)	(1.19)	(0.44)	(1.11)
4 cycles * Plus 35 * Post mandate	15.85***	18.19**	16.74***	17.06***	16.17***	18.69***	18.59***	19.09***
	(3.07)	(5.65)	(1.92)	(1.89)	(3.66)	(5.10)	(3.10)	(4.85)
4 cycles * Post mandate	2.58***	2.28	2.22***	3.84	3.66**	2.93	2.85***	1.53
	(0.59)	(1.60)	(0.36)	(2.72)	(1.14)	(2.26)	(0.70)	(1.07)
+5 cycles * Plus 35 * Post mandate	25.83***	21.89***	26.48***	12.44***	31.97***	23.57***	28.22***	19.49***
	(1.94)	(1.80)	(2.33)	(3.44)	(1.98)	(2.04)	(2.07)	(1.98)
+5 cycles * Post mandate	1.29***	9.51***	1.34***	18.78***	1.99***	8.51***	1.47***	7.16***
	(0.12)	(1.49)	(0.21)	(3.64)	(0.17)	(1.08)	(0.12)	(0.73)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Number of cells	59,637	59,637	53,935	53,935	54,752	54,752	59,183	59,183

(b) Mandated to cover states versus never mandated states

	All mothers		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mandated cover * Plus 35 * Post mandate	0.77	6.04	-0.46	-0.09	1.51	5.16	1.98	4.57
	(6.33)	(4.77)	(6.48)	(4.87)	(7.04)	(5.28)	(6.57)	(4.85)
Mandated cover * Post mandate	2.42*	5.02**	2.35**	7.40***	3.39*	4.83*	2.43*	2.85*
	(1.03)	(1.74)	(0.86)	(1.93)	(1.30)	(1.84)	(1.06)	(1.39)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Number of cells	59,637	59,637	53,935	53,935	54,752	54,752	59,183	59,183

Note: This table presents the estimated average effects of mandated coverage of IVF in private health insurances on number of infants per thousand births from Difference-in-Difference-in-Differences (DDD) model specified in Equation (3.3). The main sample includes all the births in the US. from 1978-2014, aggregated by state-year-age. The control group for each model includes the states who never been mandated to cover IVF treatment in their private health insurances. The included demographic characteristics are mothers' age, education, race, marital status, fathers' race and infants sex, birth weight and order of birth. Standard errors presented in parenthesis are clustered in state level. Panel (a) presents the estimated effects from the number of covered cycles and Panel (b) shows the overall effects of mandated coverage.

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 3.6: Estimated effects of mandated coverage of IVF on share of multiple births from a Difference-in-Differences-in-Differences model using individual data

(a) Number of covered cycles in mandated states versus never mandated states

	All mothers		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 cycle * Plus 35 * Post mandate	-0.33	-0.30	-0.39	-0.31	-0.31	-0.29	-0.53**	-0.36
	(0.23)	(0.19)	(0.19)	(0.16)	(0.25)	(0.21)	(0.18)	(0.23)
1 cycle * Post mandate	-0.08**	-0.04	-0.13***	-0.02	-0.09***	-0.07	-0.06***	-0.13*
	(0.02)	(0.11)	(0.02)	(0.09)	(0.01)	(0.11)	(0.01)	(0.05)
2 cycles * Plus 35 * Post mandate	0.28***	1.23***	0.17***	1.11***	0.26***	0.96***	0.27***	1.42***
	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
2 cycles * Post mandate	0.16***	1.01***	0.04**	0.91***	0.21***	1.17***	0.16***	0.97***
	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)
3 cycles * Plus 35 * Post mandate	0.18***	0.25***	0.12***	0.18***	0.17***	0.23***	0.20***	0.26***
	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.06)
3 cycles * Post mandate	0.09***	0.06	0.12***	0.09**	0.12***	0.10*	0.07*	0.04
	(0.01)	(0.03)	(0.01)	(0.03)	(0.03)	(0.05)	(0.03)	(0.07)
4 cycles * Plus 35 * Post mandate	0.20***	0.06*	0.13***	0.08	0.15***	0.03	0.16**	0.00
	(0.06)	(0.03)	(0.03)	(0.05)	(0.04)	(0.03)	(0.05)	(0.03)
4 cycles * Post mandate	0.12	0.11	0.12***	0.19	0.25*	0.17*	0.15	0.10
	(0.07)	(0.06)	(0.03)	(0.10)	(0.10)	(0.07)	(0.08)	(0.06)
+5 cycles * Plus 35 * Post mandate	0.66***	0.58***	0.62***	0.52***	0.61***	0.55***	0.68***	0.55***
	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
+5 cycles * Post mandate	0.22***	0.37***	0.23***	0.45***	0.32***	0.47***	0.26***	0.40***
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Number of births	99,007,886	64,117,539	53,315,784	31,893,355	64,537,316	52,625,358	77,807,449	53,665,021

(b) Mandated to cover states versus never mandated states

	All mothers		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mandated cover * Plus 35 * Post mandate	0.22*	0.17	0.16	0.16	0.18*	0.16	0.20	0.15
	(0.10)	(0.11)	(0.09)	(0.09)	(0.09)	(0.11)	(0.10)	(0.11)
Mandated cover * Post mandate	0.11*	0.14	0.11***	0.21**	0.21***	0.19*	0.14**	0.14
	(0.04)	(0.07)	(0.03)	(0.08)	(0.06)	(0.08)	(0.05)	(0.08)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Individual characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Observations	99,007,886	64,117,539	53,315,784	31,893,355	64,537,316	52,625,358	77,807,449	53,665,021

Note: This table presents the estimated average effects of mandated coverage of IVF in private health insurances on probability of multiple birth from Difference-in-Difference-in-Differences (DDD) model specified in Equation (3.4). The main sample includes all the births in the US. from 1978-2014. The control group for each model includes the states who never been mandated to cover IVF treatment in their private health insurances. The included demographic characteristics are mothers' age, education, race, marital status, fathers' race and infants sex, birth weight and order of birth. Standard errors presented in parenthesis are clustered in state level. Panel (a) presents the estimated effects from the number of covered cycles and Panel (b) shows the overall effects of mandated coverage.

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table 3.7: Summary statistics from National Data Archive on Child Abuse and Neglect (NDACAN)

	1994-2003			*2004-2014*		
	Never mandated	Mandated to offer	Mandated to cover	Never mandated	Mandated to coffer	Mandated to cover
Adoption rate (%)	1.04	1.04	1.16	1.50	1.47	1.29
	(0.76)	(0.69)	(0.77)	(0.67)	(0.77)	(0.48)
Number of adopted children	164,482	119,329	65,098	330,393	182,617	63,677
Number of infants	20,146,303	13,940,436	5,635,398	23,630,404	15,482,855	5,923,373
Mean age of adopted children	6.66	6.53	6.80	5.97	5.79	5.73
	(4.33)	(4.22)	(3.92)	(4.38)	(4.34)	(4.19)
White adopted children (%)	50.33	36.02	27.23	51.91	30.04	37.87
	(50.00)	(48.01)	(44.51)	(49.96)	(45.84)	(48.51)
Adopted boys (%)	49.71	50.30	49.97	50.65	50.98	51.06
	(50.00)	(50.00)	(50.00)	(49.00)	(50.00)	(49.99)
Mean age of adoptive mothers	42.07	44.38	43.69	42.16	43.18	42.66
	(7.01)	(6.40)	(6.66)	(7.06)	(6.84)	(6.91)
Adoptive mothers above 35 years old (%)	82.72	89.21	87.90	82.39	85.49	84.68
	(37.80)	(31.03)	(32.61)	(38.09)	(35.22)	(36.01)
White adoptive mothers (%)	57.95	37.68	33.27	67.00	44.17	50.01
	(49.36)	(48.46)	(47.12)	(47.02)	(49.66)	(50.00)
Mean age of adoptive fathers	43.90	45.70	46.01	44.12	44.87	45.03
	(6.58)	(5.61)	(5.46)	(6.49)	(6.13)	(6.05)
White adoptive father (%)	51.10	32.09	27.25	57.40	37.42	41.41
	(49.99)	(46.68)	(44.52)	(49.45)	(48.39)	(49.26)

Note: This table presents summary statistics from National Data Archive on Child Abuse and Neglect (NDACAN). Montana, Texas, California, New York, Ohio, West Virginia and Louisiana legislated policies to mandate offering IVF treatment in their private health insurances. Arkansas, Hawaii, Connecticut, Rhode Island, Maryland, Illinois and New Jersey and Massachusetts enacted policies to mandated covering IVF treatments. The rest of the states have never enacted policies to cover or offer IVF treatment in private health insurances. Adoption rate is defined as ratio of total number of adopted children to total number of births. Standard deviations are presented in parenthesis.

Table 3.8: Adoption and mandated IVF coverage in private health insurances

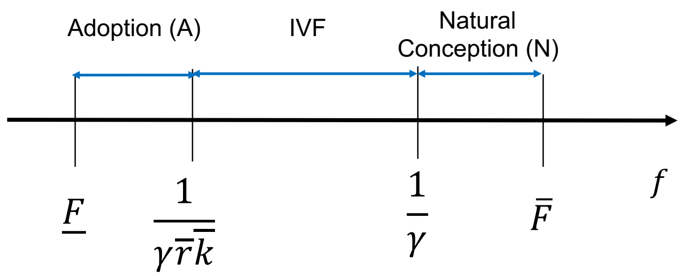
	(1)	(2)
2 cycles * Plus 35 * Post mandate	0.14***	0.15***
	(0.03)	(0.04)
3 cycles * Plus 35 * Post mandate	-0.07	-0.16**
	(0.05)	(0.05)
4 cycles * Plus 35 * Post mandate	-0.80***	-0.81***
	(0.05)	(0.06)
Time fixed effects	Yes	Yes
State fixed effects	Yes	Yes
Age fixed effects	Yes	Yes
Demographic characteristics	No	Yes
Observations	18,767	18,485

Note: This table presents the estimated effects of the mandated coverage of IV on adoption rates from a Difference-in-Difference-in-model specified in Equation 3.3. Dependent variable is natural logarithm of ratio of total number of adoption to total number of births in each state-year-age cell. Demographic characteristics include adoptive mother, father and child's age, race and family structure. Standard errors in parenthesis are clustered in state level.

$*p < 0.10, **p < 0.05, ***p < 0.01$

3.8 Figures

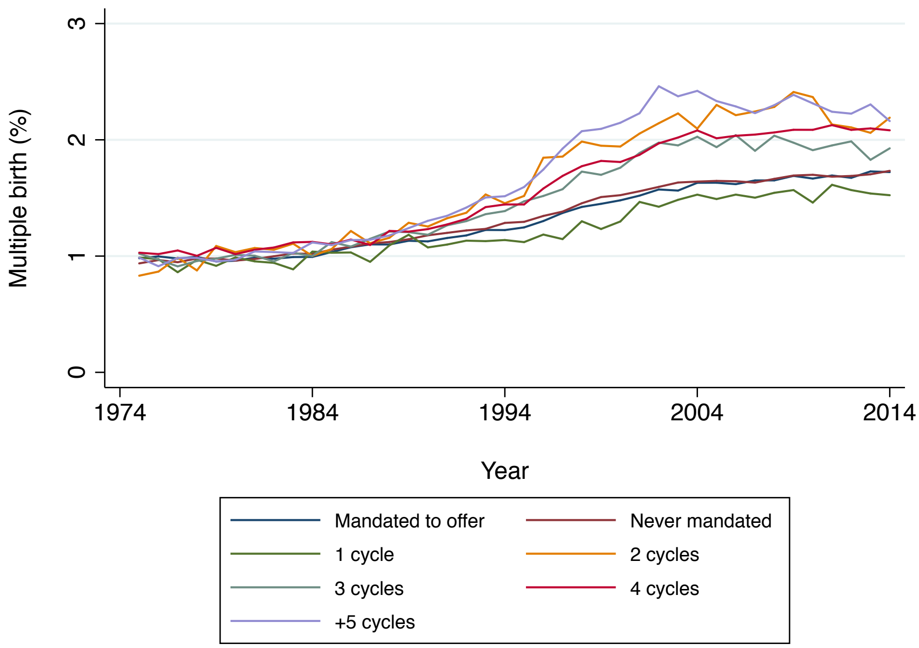
Figure 3.1: Patients' decision by their fertility



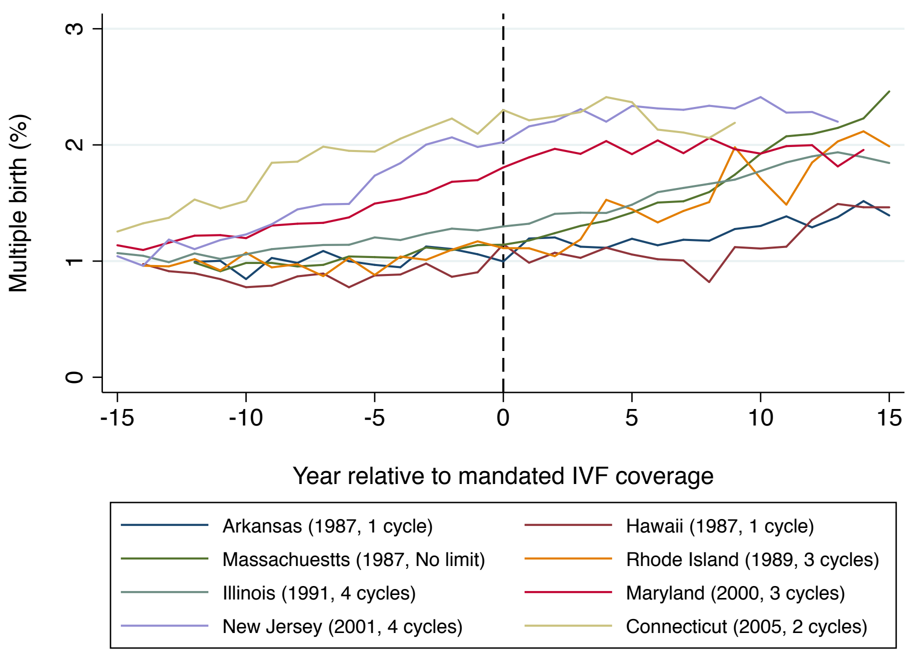
Note: This figure presents patients' choice for adopting an infant, IVF treatment and natural conception by their fertility (f) and the number of IVF cycles covered in their heath insurance plan ($\bar{r}$). $\bar{F}$ and $\underline{F}$ respectively denote the upper and lower limits of natural fertility. γ denotes the probability conceiving an infant naturally. $\hat{k}$ denotes the maximum number of embryos that can be implanted.

Figure 3.2: Incidence of multiple birth

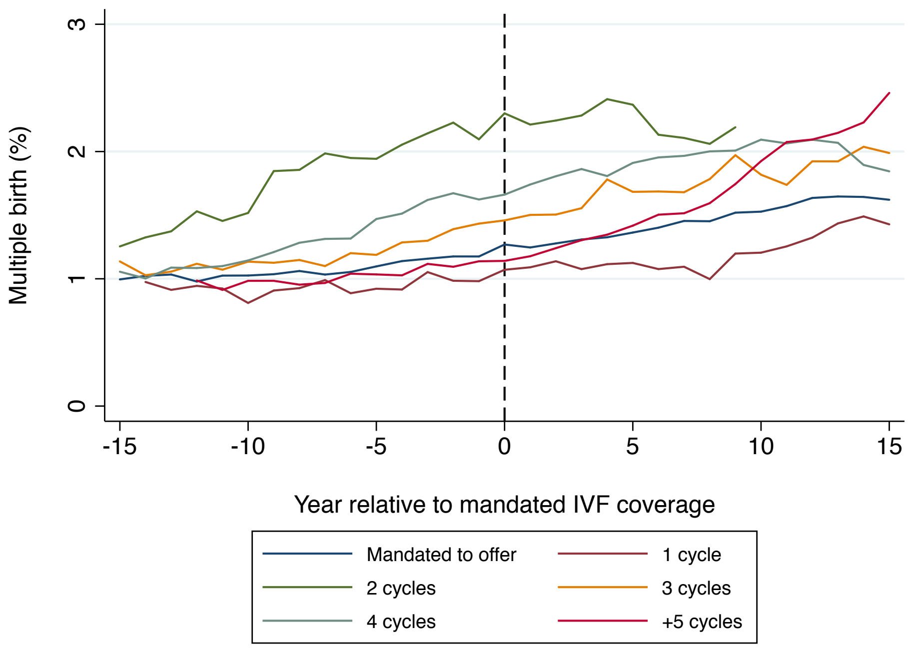
(a) Ever mandated and never mandated states



(b) States with mandated cover



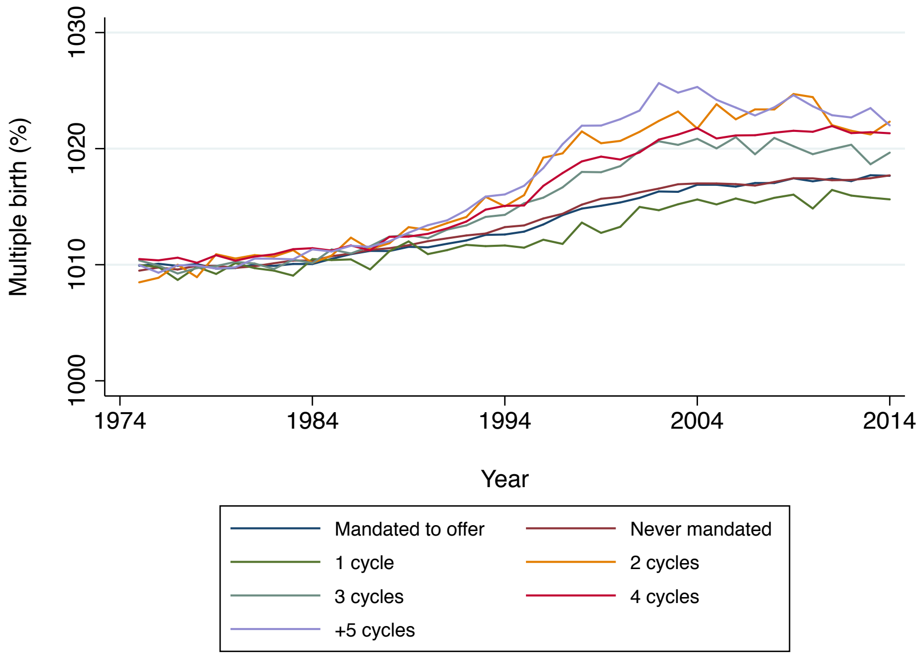
(c) Number of covered IVF cycles



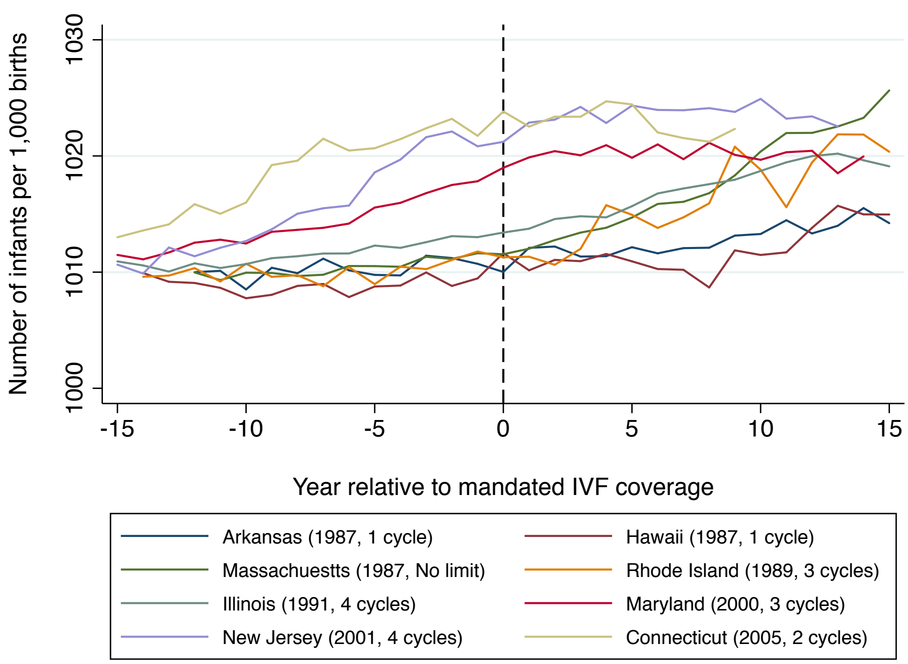
Note: The sample includes all the births from the National Vital statistics from 1974-2014. The outcome variable is portion of multiple births (twin. triplet or higher) out of whole births. Panel (a) plots the outcome variable in never mandated and ever mandated (mandated to cover or offer) states over the period of analysis. Panel (b) plots the outcome in he strongly mandated states. Panel (c) plots the outcome by the number of covered IVF cycles.

Figure 3.3: Number of infants per thousand births

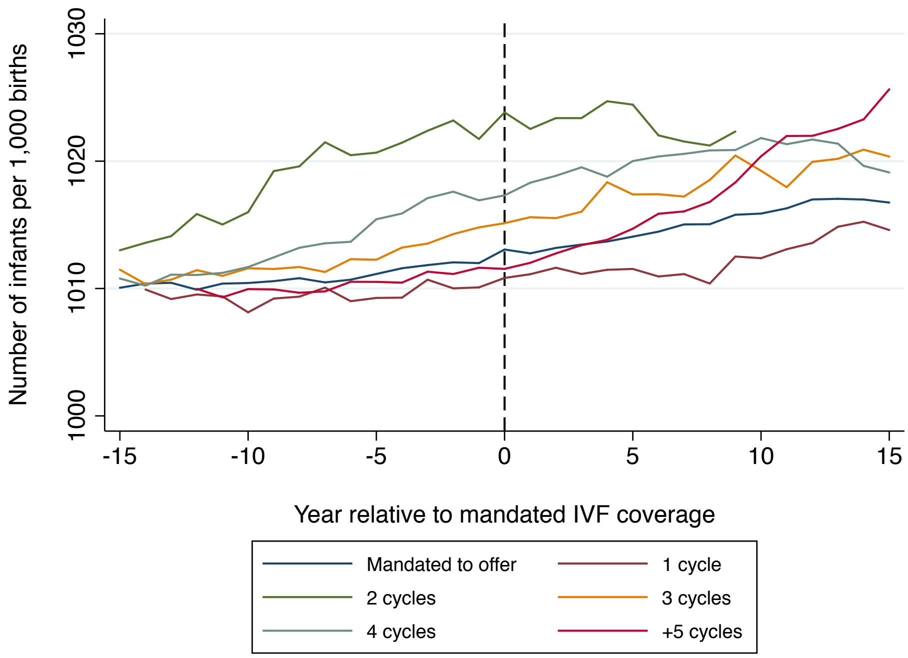
(a) Ever mandated and never mandated states



(b) States with mandated cover



(c) Number of covered IVF cycles

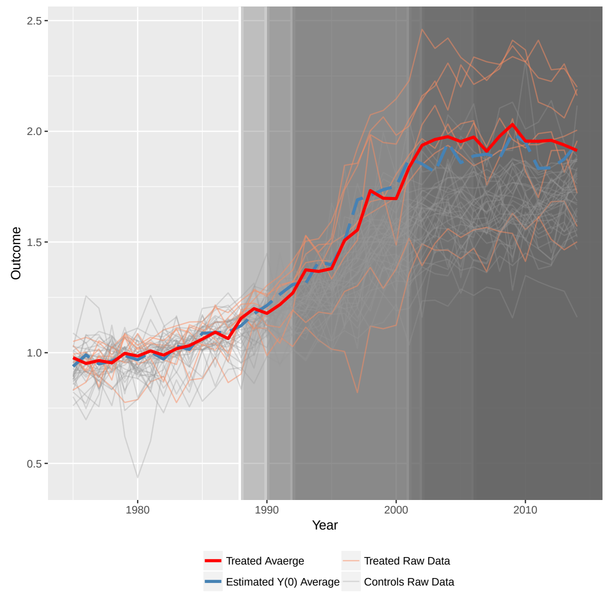


Note: The sample includes all the births from the National Vital statistics from 1974-2014. The outcome variable is the number of infants in thousand births. Panel (a) plots the outcome variable in never mandated and ever mandated (mandated to cover or offer) states over the period of analysis. Panel (b) plots the outcome in he strongly mandated states. Panel (c) plots the outcome by the number of covered IVF cycles.

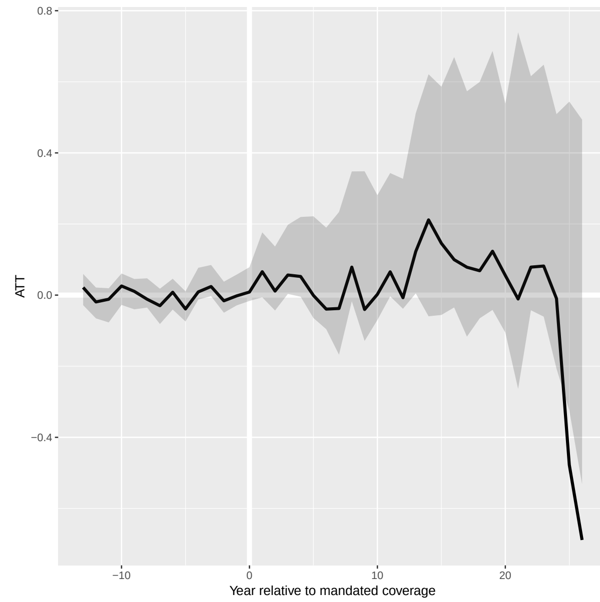
Figure 3.4: Effects of mandated IVF coverage in all mandated states

(a) Share of multiple births (%)

(1) Treated average and estimated average for treated states



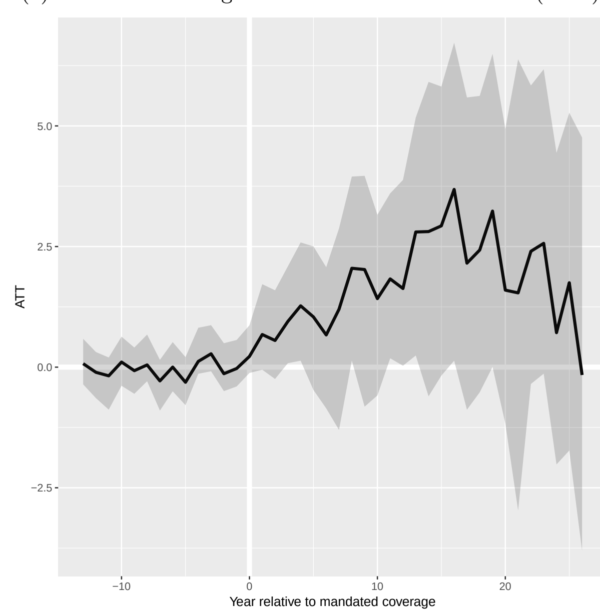
(2) Estimated Average Treatment effect on Treated (ATT)



(b) Number of infants per thousand births

(1) Treated average and estimated average for treated states

(2) Estimated Average Treatment effect on Treated (ATT)

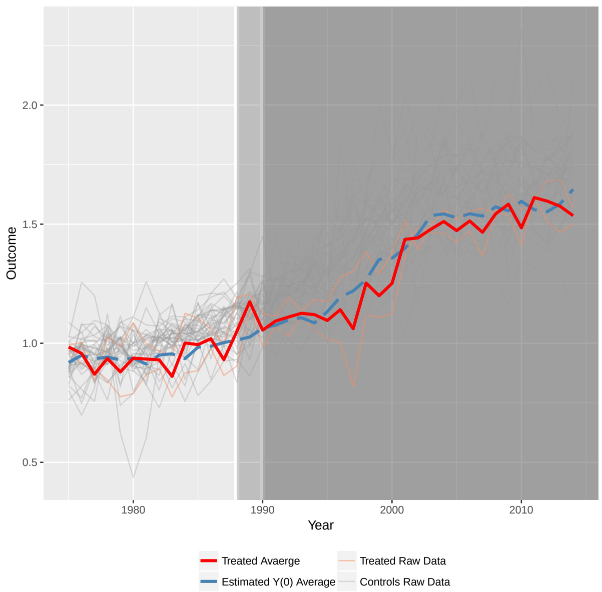


Note: This figure plots the estimated counter-factual outcome $Y(0)$ and the Average Treatment effect on Treatment (ATT) using the Generalized Synthetic Control model specified in Equation (3.2). The sample includes all the births in the US. from 1975-2014 from the National Vital Statistics, aggregated by state-year. The treatment group includes states with mandated IVF coverage in their employer provided health insurances (Arkansas (1987, 1 cycle), Massachusetts (1987, +5 cycles), Hawaii (1989, 1 cycle), Rhode Island (1989, 3 cycles), Illinois (1991, 4 cycles), Maryland (2000, 3 cycles), New Jersey (2001, 4 cycles) and Connecticut (2005, 2 cycles)) and, control group includes all the states who have never mandated covering IVF. The included covariates in the model are mothers' age, education and mother and fathers' race and baby's birth weight. Panel (a) and Panel (b) plot the estimates respectively for the incidence of multiple birth and the number of infants per thousand births. The %95 confidence intervals for the estimated ATT are shown by the gray shade.

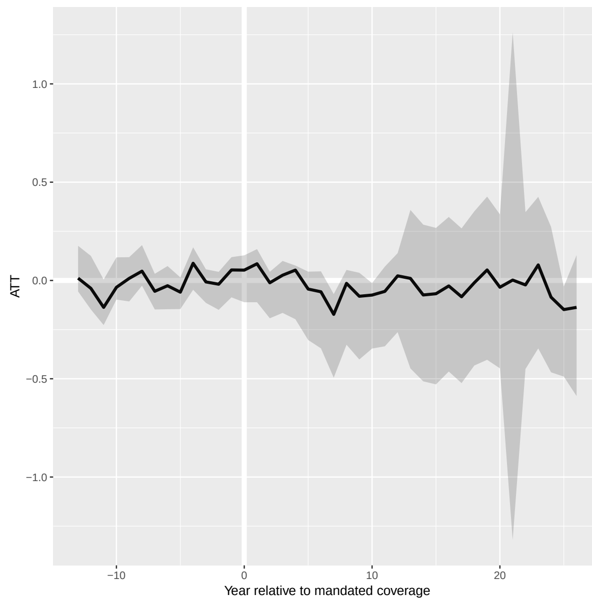
Figure 3.5: Effects of mandated IVF coverage in states with one cycle of mandated coverage

(a) Share of multiple births (%)

(1) Treated average and estimated average for treated states

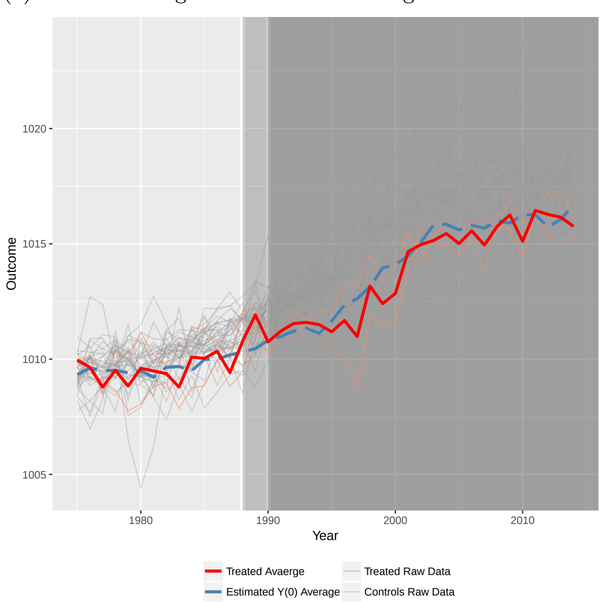


(2) Estimated Average Treatment effect on Treated (ATT)

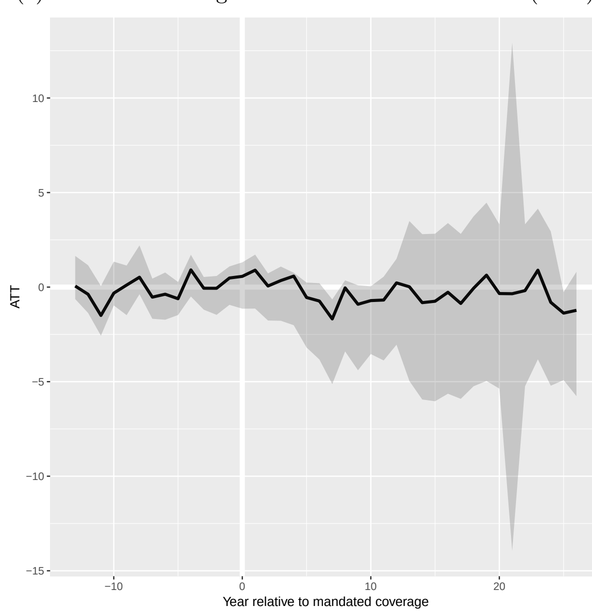


(b) Number of infants per thousand births

(1) Treated average and estimated average for treated states



(2) Estimated Average Treatment effect on Treated (ATT)

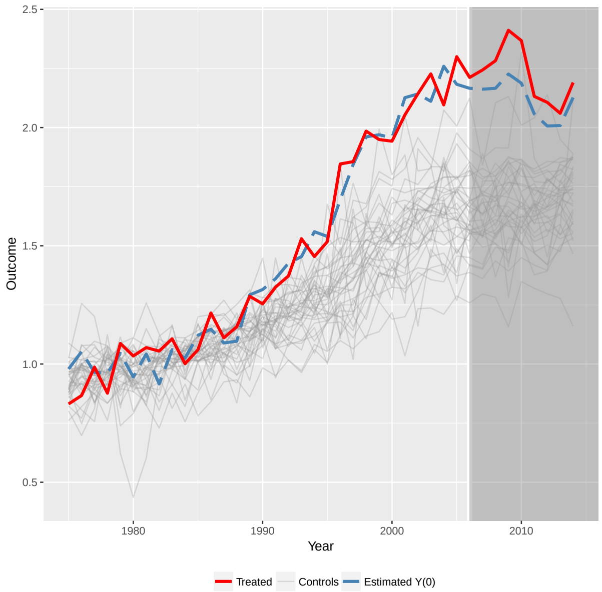


Note: This figure plots the estimated counter-factual outcome $Y(0)$ and the Average Treatment effect on Treatment (ATT) using the Generalized Synthetic Control model specified in Equation (3.2). The sample includes all the births in the US. from 1975-2014 from the National Vital Statistics, aggregated by state-year. The treatment group includes states one cycle of mandated IVF coverage in their employer provided health insurances (Arkansas (1987) and Hawaii (1987)) and, control group includes all the states who have never mandated covering IVF. The included covariates in the model are mothers' age, education and mother and fathers' race and birth weight. Panel (a) and Panel (b) plot the estimates respectively for the incidence of multiple birth and the number of infants per thousand births. The %95 confidence intervals for the estimated ATT are shown by the gray shade.

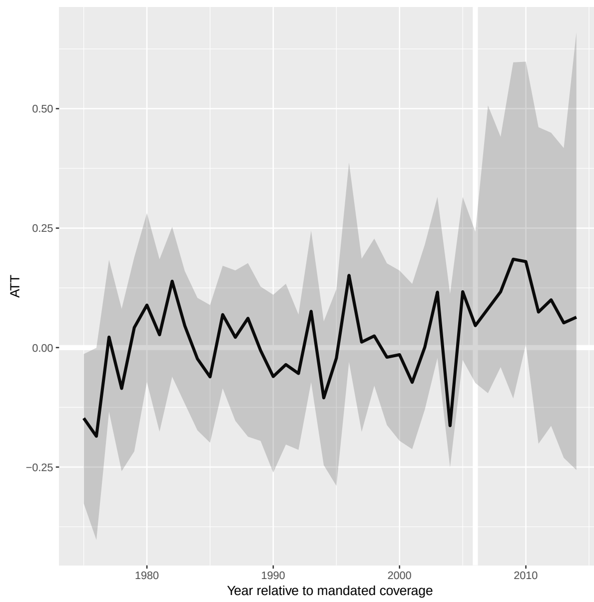
Figure 3.6: Effects of mandated IVF coverage in states with 2 cycles of mandated coverage

(a) Share of multiple births (%)

(1) Treated average and estimated average for treated states



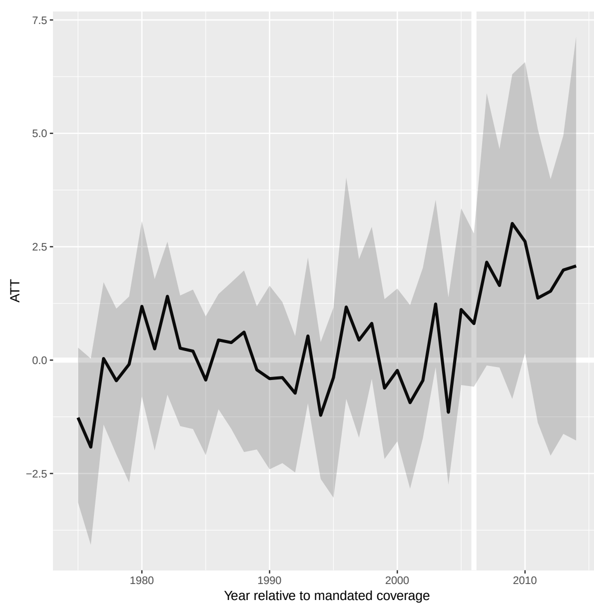
(2) Estimated Average Treatment effect on Treated (ATT)



(b) Number of infants per thousand births

(1) Treated average and estimated average for treated states

(2) Estimated Average Treatment effect on Treated (ATT)

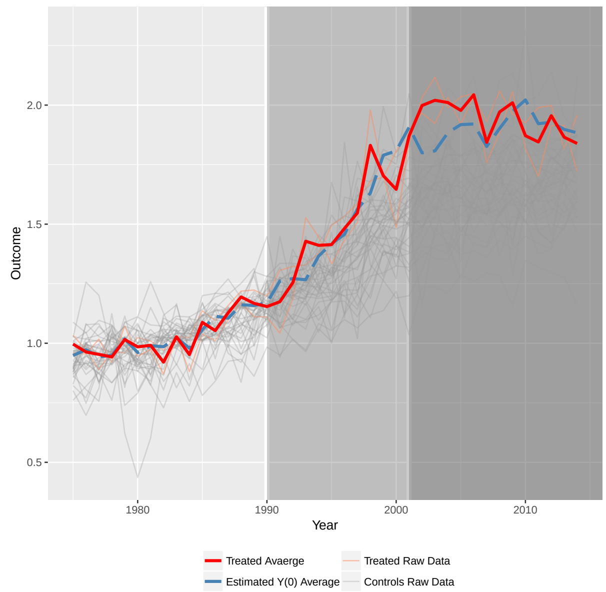


Note: This figure plots the estimated counter-factual outcome $Y(0)$ and the Average Treatment effect on Treatment (ATT) using the Generalized Synthetic Control model specified in Equation (3.2). The sample includes all the births in the US. from 1975-2014 from the National Vital Statistics, aggregated by state-year. The treatment group includes states with 2 cycles of mandated IVF coverage in their employer provided health insurances (Connecticut (2005)) and, control group includes all the states who have never mandated covering IVF. The included covariates in the model are mothers' age, education and mother and fathers' race and birth weight. Panel (a) and Panel (b) plot the estimates respectively for the incidence of multiple birth and the number of infants per thousand births. The %95 confidence intervals for the estimated ATT are shown by the gray shades.

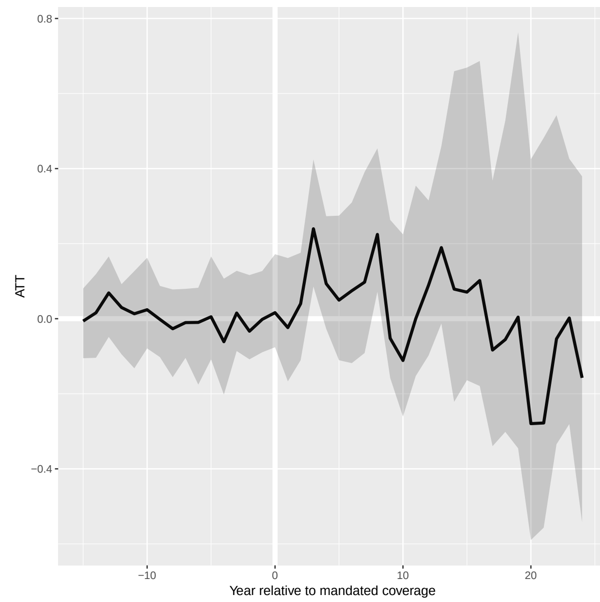
Figure 3.7: Effects of mandated IVF coverage in states with 3 cycles of mandated coverage

(a) Share of multiple births (%)

(1) Treated average and estimated average for treated states

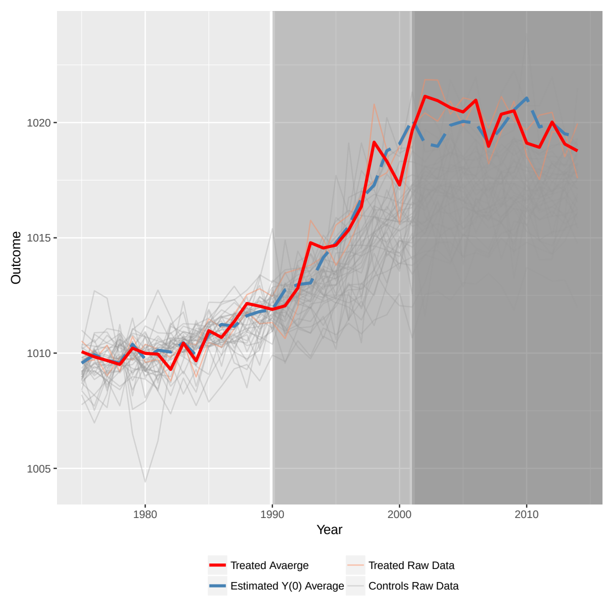


(2) Estimated Average Treatment effect on Treated (ATT)

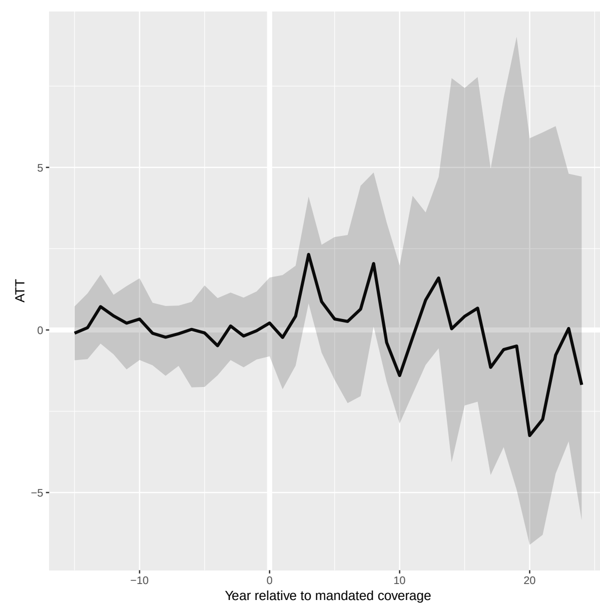


(b) Number of infants per thousand births

(1) Treated average and estimated average for treated states



(2) Estimated Average Treatment effect on Treated (ATT)

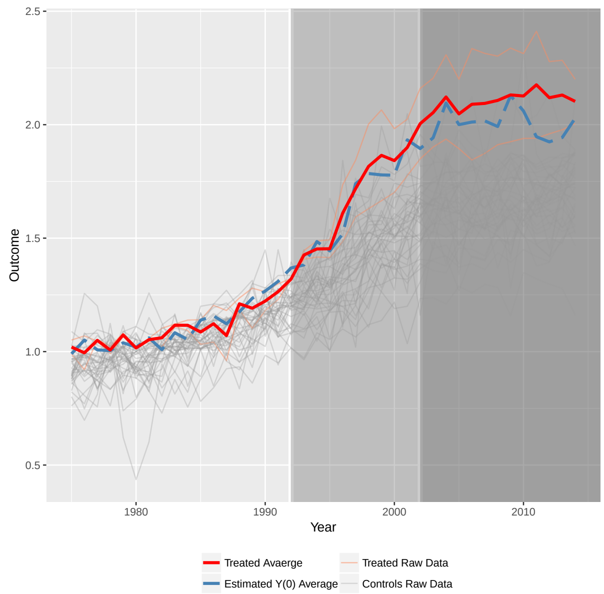


Note: This figure plots the estimated counter-factual outcome $Y(0)$ and the Average Treatment effect on Treatment (ATT) using the Generalized Synthetic Control model specified in Equation (3.2). The sample includes all the births in the US. from 1975-2014 from the National Vital Statistics, aggregated by state-year. The treatment group includes states with 3 cycles of mandated IVF coverage in their employer provided health insurances (Rhode Island (1989) and Maryland (2000)) and, control group includes all the states who have never mandated covering IVF. The included covariates in the model are mothers' age, education and mother and fathers' race and birth weight. Panel (a) and Panel (b) plot the estimates respectively for the incidence of multiple birth and the number of infants per thousand births. The %95 confidence intervals for the estimated ATT are shown by gray shades.

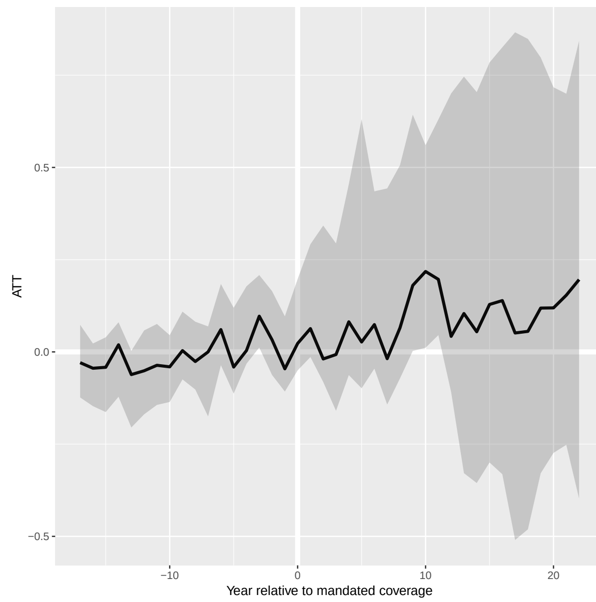
Figure 3.8: Effects of mandated IVF coverage in states with 4 cycles of mandated coverage

(a) Share of multiple births (%)

(1) Treated average and estimated average for treated states

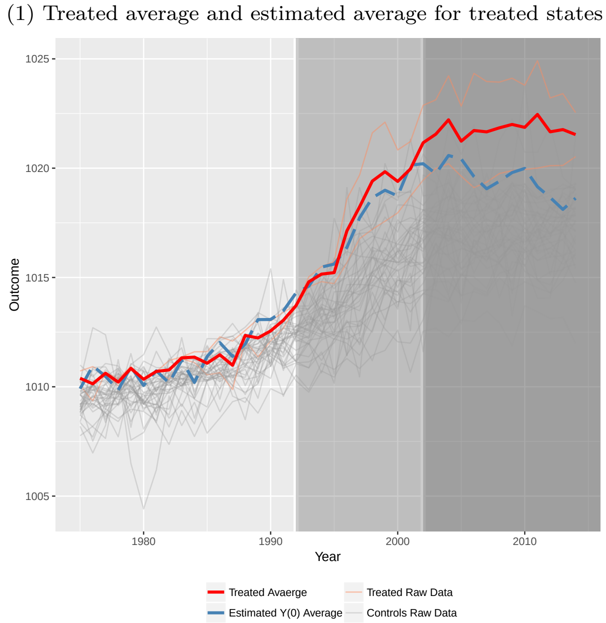


(2) Estimated Average Treatment effect on Treated (ATT)

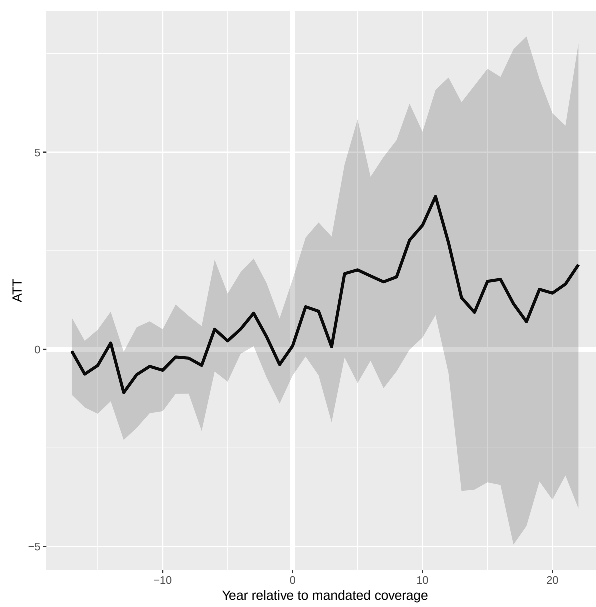


(b) Number of infants per thousand births

(1) Treated average and estimated average for treated states



(2) Estimated Average Treatment effect on Treated (ATT)



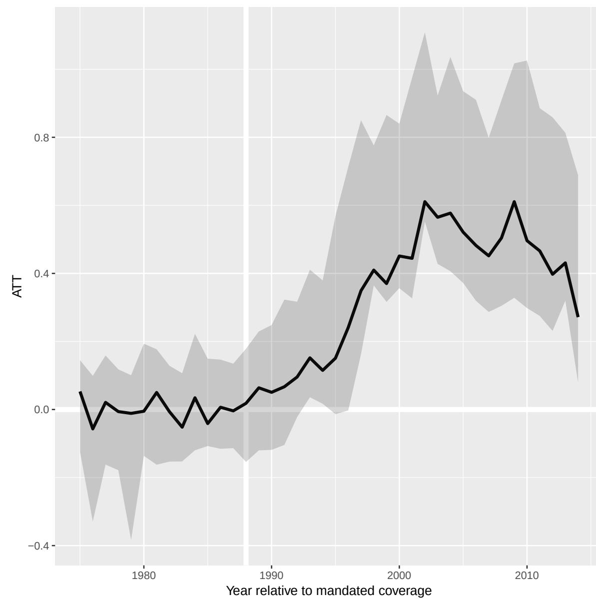
Note: This figure plots the estimated counter-factual outcome $Y(0)$ and the Average Treatment effect on Treatment (ATT) using the Generalized Synthetic Control model specified in Equation (3.2). The sample includes all the births in the US. from 1975-2014 from the National Vital Statistics, aggregated by state-year. The treatment group includes states 4 cycles of mandated IVF coverage in their employer provided health insurances (Illinois (1991) and New Jersey (2001)) and, control group includes all the states who have never mandated covering IVF. The included covariates in the model are mothers' age, education and mother and fathers' race and birth weight. Panel (a) and Panel (b) plot the estimates respectively for the incidence of multiple birth and the number of infants per thousand births. The %95 confidence intervals for the estimated ATT are shown by the gray shade.

Figure 3.9: Effects of mandated IVF coverage in states with +5 cycles of mandated coverage

(a) Share of multiple births (%)

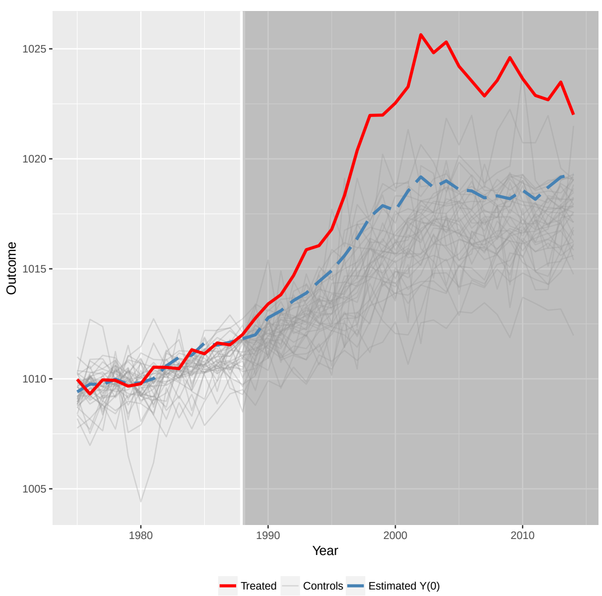
(1) Treated average and estimated average for treated states

(2) Estimated Average Treatment effect on Treated (ATT)

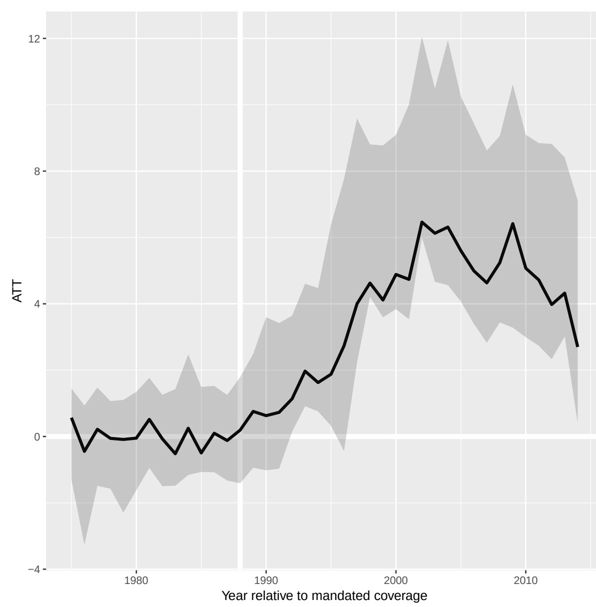


(b) Number of infants per thousand births

(1) Treated average and estimated average for treated states



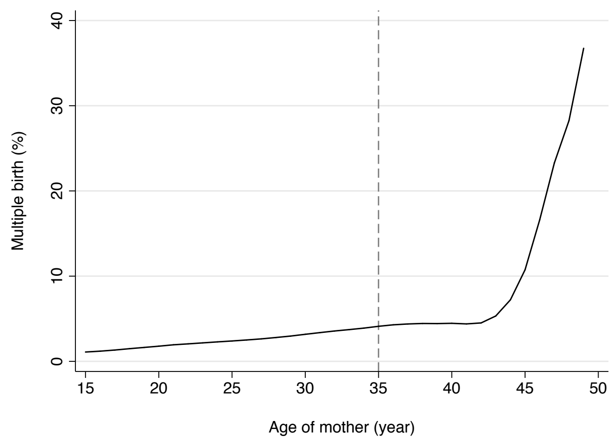
(2) Estimated Average Treatment effect on Treated (ATT)



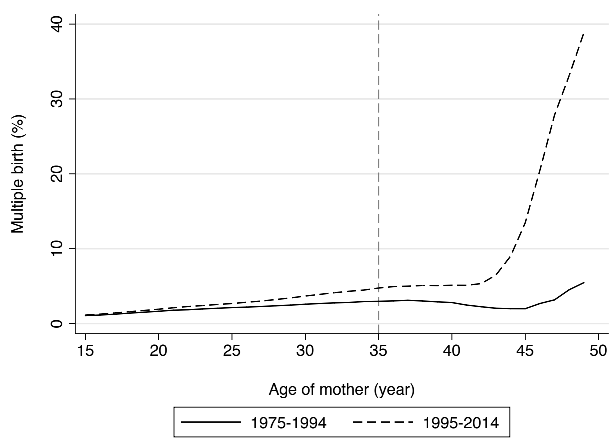
Note: This figure plots the estimated counter-factual outcome $Y(0)$ and the Average Treatment effect on Treatment (ATT) using the Generalized Synthetic Control model specified in Equation (3.2). The sample includes all the births in the US. from 1975-2014 from the National Vital Statistics, aggregated by state-year. The treatment group includes states +5 cycle of mandated IVF coverage in their employer provided health insurances (Massachusetts (1987)) and, control group includes all the states who have never mandated covering IVF. The included covariates in the model are mothers' age, education and mother and fathers' race and birth weight. Panel (a) and Panel (b) plot the estimates respectively for the incidence of multiple birth and the number of infants per thousand births. The %95 confidence intervals for the estimated ATT are shown by the gray shade.

Figure 3.10: Share of multiple births by age of mothers

(a) 1975-2014



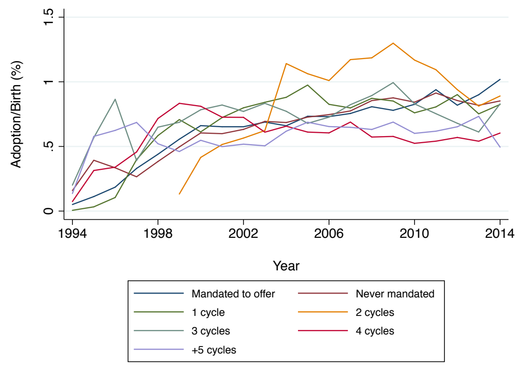
(b) 1975-1994 and 1995-2014



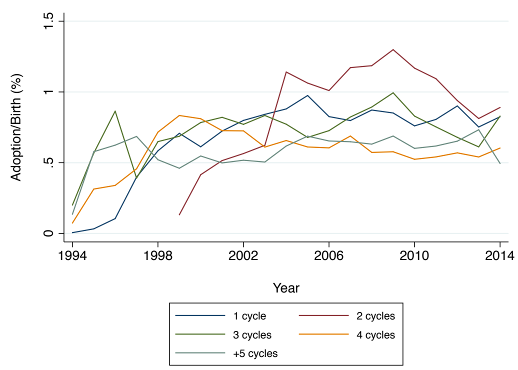
Note: This figure plots trends in share of multiple births by mothers' age. Panel (a) plots the trend from 1975-2014. Panel (b) plots the trends separately from 1975-1994 and 1995-2014. Incidence of multiple birth is higher for older mothers and it is increasing in recent decade.

Figure 3.11: Number of adopted children to number of births by number of covered IVF cycles

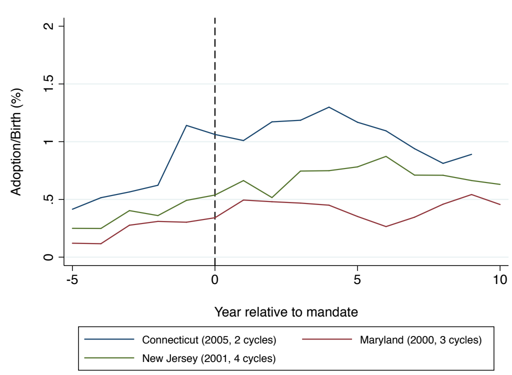
(a) All states



(b) States with mandated IVF coverage



(c) Before and after mandated IVF coverage



Note: This figure plots the ratio of number of adopted children to number of live births from 1994 to 2014 by the number of IVF cycles covered in states' private health insurances. The adoption information are from Adoption and Foster Care Analysis and Reporting System (AFCARS). The total number of births are from Natality Detail Files.

Bibliography

Abadie, Alberto, "Semiparametric Difference-in-Differences Estimators," *Review of Economic Studies*, 2005, *72* (1), 1–19.

_ **, Alexis Diamond, and Jens Hainmueller**, "Synthetic Control Methods for Comparative Case Studies: Estimating the Effect of California's Tobacco Control Program," *Journal of the American Statistical Association*, jun 2010, *105* (490), 493–505.

_ **,** _ **, and** _ , "Comparative Politics and the Synthetic Control Method," *American Journal of Political Science*, 2015, *59* (2), 495–510.

Abrahams, BS and DH Geschwind, "Advances in autism genetics: on the threshold of a new neurobiology," *Nature Reviews Genetics*, 2008.

Abramowitz, Joelle, "Assisted Reproductive Technology and Women's Marriage and Birth Timing: Duration and Competing Risks Analyses," 2012.

_ , "Turning back the ticking clock: The effect of increased affordability of assisted reproductive technology on women's marriage timing," *Journal of Population Economics*, 2014, *27* (2), 603–633.

Altonji, Joseph G. and Christina H Paxson, "Labor Supply , Hours Constraints , and Job Mobility," *The Journal of Human Resources*, 1992, *27* (2), 256–278.

_ **and Cristina H. Paxson**, "Labor Supply Preferences, Hours Constraints, and Hours-Wage Trade-offs," *Journal of Labor Economics*, 1988, *6* (2), 254–276.

Ameri, Mason, Lisa Schur, Meera Adya, Scott Bentley, Patrick McKay, and Douglas Kruse, "The disability employment puzzles: a field experiment on employer hiring behavior," *NBER*, 2015, pp. 1–70.

Autor, David H. and Mark G. Duggan, "The growth in the Social Security Disability rolls: A Fiscal Crisis Unfolding," *The Journal of Economic Perspectives*, 2006, *20* (3), 71–96.

Bai, Jushan, "Panel Data Models With Interactive Fixed Effects," *Econometrica*, 2009, *77* (4), 1229–1279.

Baker, Michael, Jonathan Gruber, and Kevin Milligan, "Non-cognitive deficits and young adult outcomes: The long-run impacts of a universal child care program," *NBER*, 2015, pp. 1–48.

Baldwin, Marjorie and William G Johnson, "Labor Market Discrimination against Men with Disabilities," *Journal of Human Resources*, 1994, *29* (1), 1–19.

Baldwin, Marjorie L. and William G Johnson, "Labor Market Discrimination against Women with Disabilities," *Industrial Relations*, 1995, *34* (4), 555–577.

_ **and William G. Johnson**, "Labor Market Discrimination against Men with Disabilities in the Year of the ADA," *Southern Economic Journal*, 2000, *66* (3), 548–566.

Benitez-Silva, Hugo, Moshe Buchinsky, and John Rust, "Induced Entry Effects of a $1 for $2 Offset in SSDI Benefits," 2011.

Bitler, Marianne and Lucie Schmidt, "Health disparities and infertility: impacts of state-level insurance mandates," *Fertility and Sterility*, 2006, *85* (4), 858–865.

Bitler, Marianne P and Lucie Schmidt, "Utilization of infertility treatments: the effects of insurance mandates," *Demography*, feb 2012, *49* (1), 125–49.

Blinder, Alan S., "Wage Discrimination : Reduced Form and Structural Estimates," *Journal of Human resources*, 1973, *8* (4), 436–455.

Blundell, Richard and Thomas Mccurdy, "Labor supply: A review of alternative approaches," in Orley Ashenfelter and David Card, eds., *Handbook of labor economics*, 1999, chapter 27, pp. 1559–1695.

Borghans, Lex, Anne C. Gielen, and Erzo F.P. Luttme, "Social Support Substitution and the Earnings Rebound : Evidence From A Regression Discontinuity in Disability Insurance Reform," *American Economic Journal: Economic Policy*, 2014, *6* (4), 34–70.

Brugha, T, SA Cooper, S McManus, S Purdon, J Smith, FJ Scott, N Spiers, and F Tyrer, "Estimating the Prevalence of Autism Spectrum Conditions in Adults : Extending the 2007 Adult Psychiatric Morbidity Survey," Technical Report, The Health and Social Care Information Centre 2012.

Buckles, Kasey, "Understanding the Returns to Delayed Childbearing for Working Women," *American Economic Review*, 2008, *98* (2), 403–407.

_ , "Infertility insurance mandates and multiple births," *Health economics*, 2013, *789* (22), 775–789.

Buescher, Ariane V S, Zuleyha Cidav, Martin Knapp, and David S Mandell, "Costs of Autism Spectrum Disorders in the United Kingdom and the United States.," *JAMA pediatrics*, jun 2014, *19104*, 1–8.

Bütler, Monika, Eva Deuchert, Michael Lechner, Stefan Staubli, and Petra Thiemann, "Financial work incentives for disability benefit recipients : Lessons from a randomized field experiment," *IZA Journal of Labor Policy*, 2015, *4* (18).

Calonico, Sebastian, Matias D. Cattaneo, and Rocio Titiunik, "Robust nonparametric confidence intervals for regression-discontinuity designs," *Econometrica*, 2014, *82* (6), 2295–2326.

Campolieti, Michele and Chris Riddell, "Disability policy and the labor market: Evidence from a natural experiment in Canada, 1998-2006," *Journal of Public Economics*, apr 2012, *96* (3-4), 306–316.

Center for Disease Control and Prevention (CDC), "Community Report on Autism," Technical Report 2014.

Chandra, Anjani, Gladys M Martinez, William D Mosher, Joyce C Abma, and Jo Jones, "Fertility, family planning, and reproductive health of U.S. women: data from the 2002 National Survey of Family Growth.," *Vital and health statistics. Series 23, Data from the National Survey of Family Growth*, 2005, (25), 1–160.

Chetty, Raj, "Bounds on Elasticities With Optimization Frictions: A Synthesis of Micro and Macro Evidence on Labor Supply," *Econometrica*, 2012, *80* (3), 969–1018.

_ , Adam Guren, Day Manoli, and Andrea Weber, "Does Indivisible Labor Explain the Difference Between Micro and Macro Elasticities? A Meta-Analysis of Extensive Margin Elasticities," *NBER Macroeconomics Annual 2012*, 2012, *27* (May), 1–56.

_ , Adam Looney, and Kory Kroft, "Salience and Taxation : Theory and Evidence," *American Economic Review*, 2009, *99* (4), 1145–1177.

_ , John N Friedman, and Emmanuel Saez, "Using Differences in Knowledge Across Neighborhoods to Uncover the Impacts of the EITC on Earnings," *The American Economic Review*, 2013, *103* (7), 2683–2721.

_ , John N. Friedman, Tore Olsen, and Luigi Pistaferri, "Adjustment Costs, Firm Responses, and Micro vs. Macro Labor Supply Elasticities: Evidence from Danish Tax Records," *The Quarterly Journal of Economics*, 2011, *126* (2), 749–804.

Dickens, William T. and Shelly J. Lundberg, "Hours Restrictions and Labor Supply," *International Economic Review*, 1993, *34* (1), 169–192.

Dudley, Carolyn and J.C. Herbert Emery, "The Value of Caregiver Time: Costs of Support and Care for Individuals Living With Autism Spectrum Disorder," *SPP Research Papere*, 2014, *7* (1).

Einav, Liran, Amy Finkelstein, and Heidi Williams, "Paying on the margin for medical care: Evidence from breast cancer treatments," *American Economic Journal: Economic Policy*, 2016, *8* (1), 52–79.

Esbensen, Anna J, Marsha Mailick Seltzer, Kristen S L Lam, and James W Bodfish, "Age-related differences in restricted repetitive behaviors in autism spectrum disorders.," *Journal of autism and developmental disorders*, jan 2009, *39* (1), 57–66.

Farley, Megan A, William M McMahon, Eric Fombonne, William R Jenson, Judith Miller, Michael Gardner, Heidi Block, Carmen B Pingree, Edward R Ritvo, Riva Arielle Ritvo, and Hilary Coon, "Twenty-year outcome for individuals with autism and average or near-average cognitive abilities.," *Autism research : official journal of the International Society for Autism Research*, apr 2009, *2* (2), 109–18.

Fortin, Bernard, Guy Lacroix, and Simon Drolet, "Welfare benefits and the duration of welfare spells: Evidence from a natural experiment in Canada," *Journal of Public Economics*, 2004, *88* (7-8), 1495–1520.

Gelber, Alexander, Damon Jones, Daniel W. Sacks, and Jae Song, "Estimating extensive margin responses on Kinked Budget Sets: Evidence from the Earnings Test," 2016.

Grossman, Michael, "On the Concept of Health Capital and the Demand for Health," *Journal of Political Economy*, 1972, *80* (2), 223.

Gruber, Jonathan, "The incidence of mandated maternity benefits.," *American Economic Review*, 1994, *84* (3), 622–641.

— , "Disability Insurance Benefits and Labor Supply," *Journal of Political Economy*, 2000, *108* (6), 1162–1183.

Gumus, Gulcin and Jungmin Lee, "Alternative Paths To Parenthood: Ivf or Child Adoption?," *Economic Inquiry*, 2012, *50* (3), 802–820.

Hamilton, Barton H and Brian Mcmanus, "A Model of an Assisted Reproductive Techonology (ART) Market," 2003.

Hamilton, Barton H. and Brian Mcmanus, "The effects of insurance mandates on choices and outcomes in infertility treatment markets," *Health economics*, 2012, *19*, 994–1016.

Hamilton, Barton H, Brian Mcmanus, Juan Pantano, and Emily Jungheim, "Healthcare Access , Costs , and Treatment Dynamics : Evidence from In Vitro Fertilization," 2016.

Hendricks, Dawn, *American Psychiatric Association, Diagnostic and statistical manual of mental disorders*, 4 ed. 1994.

— , "Employment and adults with autism spectrum disorders: Challenges and strategies for success," *Journal of Vocational Rehabilitation*, 2010, *32*, 125–134.

Henne, Melinda B. and M. Kate Bundorf, "Insurance mandates and trends in infertility treatments," *Fertility and Sterility*, 2008, *89* (1), 66–73.

Henninger, Natalie A and Julie Lounds Taylor, "Outcomes in adults with autism spectrum disorders: a historical perspective.," *Autism : the international journal of research and practice*, jan 2013, *17* (1), 103–16.

Howlin, Patricia and Philippa Moss, "Adults with autism Spectrum disorders," *Canadian Journal of Psychiatry*, 2012, *57* (5).

_ **, Susan Goode, Jane Hutton, and Michael Rutter**, "Adult outcome for children with autism.," *Journal of child psychology and psychiatry, and allied disciplines*, feb 2004, *45* (2), 212–29.

Hoynes, Hillary W. and Robert Moffitt, "Tax Rates and Work Incentives in the Social Security Disability Insurance Program: Current Law and Alternative Reforms," *National Tax Journal*, 1999, *52* (4), 623–654.

Jain, Tarun, Bernard L. Harlow, and Mark D. Hornstein, "Insurance Coverage and Outcomes of In Vitro Fertilization," *New England Journal of Medicine*, 2002, *347* (9), 10–17.

Johnson, William G and Marjorie Baldwin, "The Americans with Disabilities Act: Will It Make a Difference?," *Policy Studies Journal*, 1993, *21* (4), 775–788.

Kanner, Leo, "Autistic Disturbances of Affective Contact," *Nervous Child*, 1943, *2*, 217–250.

_ , *Childhood Psychosis: Initial Studies and New Insights*, New York, New York, USA: John Wiley \& Sons, 1973.

Kissin, Dmitry M., Sheree L. Boulet, and Denise J. Jamieson, "Fertility Treatments in the United States," *Obstetrics & Gynecology*, 2016, *128* (2), 387–390.

Kleven, Henrik Jacobsen, "Bunching," *Annual Review of Economics*, 2016, *8* (1), 435–464.

_ **and Mazhar Waseem**, "Using Notches to Uncover Optimization Frictions and Structural Elasticities: Theory and Evidence from Pakistan," *The Quarterly Journal of Economics*, 2013, *128* (2), 669–723.

Knapp, Martin, Renee Rmeo, and Jennifer Beecham, "The Economic Consequences of Autism in the UK," Technical Report, Foundation for People with Learning Disabilites 2007.

Kostol, Andreas Ravndal and Magne Mogstad, "How Financial Incentives Induce Disability Insurance Recipients to Return to Work," *American Economic Review*, 2014, *104* (2), 624–655.

Kroeger, Sarah and Giulia La Mattina, "Assisted Reproductive Technology and Women ' s Choice to Pursue Professional Careers," *Journal of Population Economics*, 2017.

Kulkarni, Aniket D., Denise J. Jamieson, Howard W. Jones, Dmitry M. Kissin, Maria F. Gallo, Maurizio Macaluso, and Eli Y. Adashi, "Fertility Treatments and Multiple Births in the United States," *New England Journal of Medicine*, 2013, *369* (23), 2218–2225.

Lemieux, Thomas and Kevin Milligan, "Incentive effects of social assistance: A regression discontinuity approach," *Journal of Econometrics*, 2008, *142* (2), 807–828.

Lemos, Elkin V., Dongmu Zhang, Bradley J. Van Voorhis, and X. Henry Hu, "Healthcare expenses associated with multiple vs singleton pregnancies in the United States," *American Journal of Obstetrics and Gynecology*, 2013, *209* (6), 586–586.

Marie, Olivier and Judit Vall Castello, "Measuring the (income) effect of disability insurance generosity on labour market participation," *Journal of Public Economics*, feb 2012, *96* (1-2), 198–210.

Martin, Joyce A. and Melissa M. Park, "Trends in twin and triplet births: 1980-97," *National vital statistics reports*, 1999, *47* (24).

Moore, Timothy J., "The Employment Effects of Terminating Disability Benefits," *Journal of Public Economics*, 2015, *124*, 30–43.

Oaxaca, Ronald, "Male-Female wage differentials in urban labor markets," *International economic review*, 1973, *14* (3), 693–709.

OECD, "Sickness, Disability and Work: Breaking the Barriers. OECD Publishing," Technical Report 2010.

Pencavel, John, "Labor supply of men: A survey," in Orley Ashenfelter and David Card, eds., *Handbook of Labor Economics*, Vol. 1 1986, chapter 1, pp. 3–102.

Reynolds, Meredith A, Laura a Schieve, Gary Jeng, and Herbert B Peterson, "Does insurance coverage decrease the risk for multiple births associated with assisted reproductive technology?," *Fertility and Sterility*, 2003, *80* (1), 16–23.

Roux, Anne M, Paul T Shattuck, Benjamin P Cooper, Kristy A Anderson, and Sarah C Narendorf, "Postsecondary Employment Experiences Among Young Adults With an Autism Spectrum Disorder Clinical Guidance," *Journal of the American Academy of Child & Adolescent Psychiatry*, 2013, *52* (9), 931–939.

Ruh, Philippe and Stefan Staubli, "Financial Incentives and Earnings of Disability Insurance Recipients: Evidence from a Notch Design," 2016.

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

Schmidt, Lucie, "Infertility Insurance Mandates and Fertility," *The American Economic Review, Papers and Proceedings*, 2005, *95* (2), 204–208.

_ , "Effects of infertility insurance mandates on fertility," *Journal of health economics*, may 2007, *26* (3), 431–46.

Shattuck, Paul T, Marsha Mailick Seltzer, Jan S Greenberg, Gael I Orsmond, Daniel Bolt, Sheilah Kring, Julie Lounds, and Catherine Lord, "Change in autism symptoms and maladaptive behaviors in adolescents and adults with an autism spectrum disorder.," *Journal of autism and developmental disorders*, oct 2007, *37* (9), 1735–47.

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

Tazhitdinova, Alisa, "Adjust Me If I Can't: The Effect of Firm Incentives on Labor Supply Responses to Taxes," 2016.

Thomason, Terry, John F. Burton, and Douglas Hyatt, "New Approaches to Disability in the Workplace," *Labor Law Journal*, 1998, *49* (7), 1175.

Volkmar, F, R Paul, A Klin, and D Cohen, "Epidemiological studies of pervasive developmental disorders," in "Handbook of Autism and Pervasive Developmental Disorders" 2005, pp. 42–69.

Vos, Theo, Ryan M Barber, Brad Bell, and Amelia Bertozzi-Villa, "Global, regional, and national incidence, prevalence, and years lived with disability for 301 acute and chronic diseases and injuries in 188 countries, 1990-2013: a systematic analysis for the Global Burden of Disease Study 2013," *The Lancet*, 2015, *386* (9995), 743–800.

Weathers II, Robert R and Jeffrey Hemmeter, "The Impact of Changing Financial Work Incentives on the Earnings of Social Security Disability Insurance (SSDI) Beneficiaries," *Public Policy Analysis and Management*, 2011, *30* (4), 708–728.

Wittenburg, David, David R Mann, David Stapleton, Daniel Gubits, David Judkins, Andrew McGuirk, and Others, "BOND Implementation and Evaluation: Third-Year Snapshot of Earnings and Benefit Impacts for Stage 1," *Social Security Administration*, 2015.

Xu, Yiqing, "Generalized synthetic control method: causal inference with interactive fixed effects models," *Political Analysis*, 2017, *25*, 57–76.

Yun, Myeong-Su, "Decomposing differences in the first moment," *Economics Letters*, feb 2004, *82* (2), 275–280.

_ , "a Simple Solution To the Identification Problem in Detailed Wage Decompositions," *Economic Inquiry*, oct 2005, *43* (4), 766–772.

Appendix A

Appendix to Chapter 1

A.1 Proof of Theorem (1)

Theorem 1: Suppose utility loss $\phi > 0$ is associated with adjusting earnings when kink $z^* = (\tau_0, \tau_1)$ is introduced where $\tau_1 > \tau_0$ and $u(c, z; \tau; \alpha)$ is individual's utility with $\frac{\partial u_c}{\partial \alpha} < 0$ (marginal utility of consumption decreases as ability increases). If for $z_2 > z_1$, $\frac{\partial (z_2 - z_1)}{\partial \alpha}$ increases at a rate that dominates $\frac{\partial u_c}{\partial \alpha} < 0$, then utility gain of relocation for initial earning level z_2 is higher than that at z_1.

Proof. The utility gain from relocating to kink z^* from z_k for $k \in \{1, 2\}$ is $\Delta u_k = u((1 - \tau_0)z^*, z^*; \alpha) - u((1 - \tau_0)z^* + (1 - \tau_1)(z_k - z^*), z_k; \tau_0; \alpha)$. Differences in utility gains from relocating to z^* is:

$$\begin{aligned}\Delta u &= \Delta u_2 - \Delta u_1 \\ &= u((1 - \tau_0)z^* + (1 - \tau_1)(z_2 - z^*), z_2; \tau_1; \alpha) \\ &- u((1 - \tau_0)z^* + (1 - \tau_1)(z_1 - z^*), z_1; \tau_1; \alpha)\end{aligned}$$

Using a first order approximation:

$$\begin{aligned}\Delta u &\simeq [(1 - \tau_1)u_c + u_z]z_2 - [(1 - \tau_1)u_c + u_z]z_1 \\ &\simeq (z_2 - z_1)[(1 - \tau_1)u_c + u_z]\end{aligned}$$

The differences in the gain of relocation to a kink at z^* from $z_2 > z_1$ depends on the marginal utility of consumption u_c and working u_z. Therefore changes in the differences of relocation to a kink by ability is:

$$\frac{\partial \Delta u}{\partial \alpha} = (z_2 - z_1)\left((1-\tau_1)\frac{\partial u_c}{\partial \alpha} + \frac{\partial u_z}{\partial \alpha}\right)$$

Since marginal utility of consumption decreases as ability increases ($\frac{\partial u_c}{\partial \alpha} < 0$), then $\frac{\partial \Delta u}{\partial \alpha} > 0$ only if $\frac{\partial u_z}{\partial \alpha}$ increases at a rate that dominates. □

Assuming that $\frac{\partial u_z}{\partial \alpha} > 0$ dominates $\frac{\partial u_c}{\partial \alpha} < 0$, then this theorem implies that gain of relocation to a kink is higher for those with higher initial earnings (ability).

A.2 Adjustment costs

A.2.1 The model with no adjustment costs

The proceeding model for estimating elasticity of earnings using the amount of bunching at a kink is well known and described in Saez (2010). Assume that initially flat tax τ_0 is imposed on earnings. Suppose now that a higher marginal tax τ_1 on earnings above z^* is imposed, introducing a kink in the tax schedule at z^*. With no adjustment costs for changing labor supply, individuals choose their utility maximizing earnings. A smooth distribution of individuals' ability to work implies that those who would locate in the range $(z^*, z^* + \Delta z^*]$ in absence of the kink would bunch in a neighbourhood of z^*. Δz^* is the earnings response range to the kink at z^*.

Suppose that $h(z)$ is the observed distribution of earnings – with a kink at z^* – and $h_0(z)$ is the counter-factual distribution of earnings, if flat tax τ_0 would have been imposed on earnings. The amount of bunching at the kink at z^* is then the area under the counter-factual density of earnings within the bunching interval and is defined as:

$$B = \int_{z^*}^{z^*+\Delta z^*} h_0(\zeta) d\zeta \simeq \Delta z^* h_0(z^*) \tag{A.1}$$

The elasticity of earnings with respect to net-of-tax ratio at kink $z^* = (\tau_0, \tau_1)$ as specified by Saez (2010) is:

$$e = \frac{\Delta z^*/z^*}{(\tau_1 - \tau_0)/(1-\tau_0)} \tag{A.2}$$

where $\Delta z^* = B/h_0(z^*)$.

A.2.2 Empirical implementation

The model with no adjustment costs: I use the observed distributions of earnings before the policy change to estimate the counter-factual distribution of earnings at the kink at z_1^* by estimating the regressions specified in (1.13). I then estimate the normalized bunching at the kinks from (1.15). I back up Δz_1^* from (A.1) by feeding in the estimated B and $h_0(z_1^*)$. Substituting Δz^* in (A.2) results into the elasticity of earnings with respect to net-of-tax at the kink at z_1^* defined as:

$$\hat{e} = \frac{\ln(1 + \frac{\delta \hat{b}_k}{z_k^*})}{\ln(\frac{1-\tau_0}{1-\tau_1})} \tag{A.3}$$

The model with fixed adjustment costs: Assume that individuals with initial earnings in the range $(\underline{z}_1^0, z_1^* + \Delta z_1^*]$ would bunch at the kink at $z_1^* = (\tau_0, \tau_1)$. $\underline{z}_1^0$ is the utility maximizing earnings level of a marginal buncher at the kink at z_1^* with ability $\alpha^{m_1^0}$, if flat tax τ_0 would have been imposed on the earnings. A marginal buncher at the kink at z_1^* is indifferent between staying at $\underline{z}_1^0$ where marginal taxes on the earnings are higher or enduring utility loss $\phi > 0$ and bunching at the kink at z_1^*. Using the utility function specified in (1.3) and the utility maximizing level of earnings from (1.4):

$$\alpha^{m_1^0} = \frac{\underline{z}_1^0}{(1-\tau_0)^e}$$

Feeding this in (1.5) using the specified utility function in (1.3) results in an equation which implicitly defines $\underline{z}_1^0$ as a function of the elasticity of earnings e and utility loss ϕ associated with adjusting earnings:

$$(1-\tau_1)(\underline{z}_1^0 - z_1^*) - \frac{1-\tau_0}{1+\frac{1}{e}}\left(\underline{z}_1^0 - {z_1^*}^{1+\frac{1}{e}}\underline{z}_1^{0-\frac{1}{e}}\right) + \phi = 0 \tag{A.4}$$

I use the observed distributions of earnings at a neighbourhood of the kink at z_1^* before the policy and estimate the first regression specified in (1.13). Panel (a) of Figure 1.8 plots the fitted degree six polynomial where three bins around the kink are excluded ($D = 6, l = u = 3$). I compute b_1^0, the normalized bunching at z_1^* before the policy change from (1.16) using the fitted polynomial. Feeding Δz_1^* from (1.11) and the estimated b_1^0 in (1.6) results in:

$$\underline{z}_1^0 = \left(\frac{1-\tau_0}{1-\tau_1}\right)^e z_1^* - \delta\hat{b_1^0} \tag{A.5}$$

Together (A.4) and (A.5) describe an equation of e and ϕ.

I then use the residual bunching at the former kink at z_1^* to construct another equation of e and ϕ and together estimate the parameters of interest. Assume that individuals with initial earnings in the range $(\underline{z}_1^0, \underline{z}_1^1]$ would bunch at the former kink at z_1^*. $\underline{z}_1^1$ is the initial earnings of a marginal buncher at z_1^* with ability $\alpha^{m_1^1}$. A marginal buncher is indifferent between bunching at z_1^* or enduring utility loss ϕ and relocating to their utility maximizing level of earnings at $\underline{z}_1^{1\prime}$. Similar to the case before the policy change:

$$\alpha^{m_1^1} = \frac{\underline{z}_1^1}{(1-\tau_0)^e}$$

Feeding this into (1.7) using the utility function specified in (1.3) results into:

$$(1-\tau_0)\left(z_1^* - \frac{1}{1+\frac{1}{e}}\underline{z}_1^{1-\frac{1}{e}}{z_1^*}^{1+\frac{1}{e}} - \frac{\underline{z}_1^1}{1+e}\right) + \phi = 0 \tag{A.6}$$

I use the observed distribution of earnings at a neighbourhood of z_1^* after the policy change to estimate the first regression specified in (1.13). Panel (b) of Figure 1.8 shows the fitted polynomial with parameters set as $l = u = 3$ and $D = 6$. Feeding b_1^1, the estimated normalized bunching at the former kink at z_1^* using (1.16), into (1.8) results into:

$$\underline{z}_1^1 = \underline{z}_1^0 + \delta \hat{b}_1^1 \tag{A.7}$$

Together (A.6) and (A.7) describe another equation of e and ϕ. This together with (A.4) and (A.5) estimate the elasticity of earnings e and the fixed adjustment costs ϕ.

The model with heterogeneous adjustment costs: Suppose that after the policy change, individuals with initial earnings in the range $(\underline{z}_2, z_2^* + \Delta z_2^*]$ would bunch at the kink at $z_2^* = (\tau_0, \tau_1)$ where $\underline{z}_2 > z_2^*$. $\underline{z}_2$ is the initial earnings of a marginal buncher at z_2^* with ability α^{m_2} who after the policy change is indifferent between staying at their optimal level of earnings before the policy change at $\underline{z}_2'$ or enduring utility loss ϕ and bunching at z_2^*. Since $\underline{z}_2$ is the utility maximizing earnings of a marginal buncher when flat tax τ_0 is imposed, then using the utility function specified in (1.3) and the utility maximizing level of earnings from (1.4):

$$\alpha^{m_2} = \frac{\underline{z}_2}{(1-\tau_1)^e}$$

Feeding this into (1.9) using the utility function specified in (1.3) results into an equation which implicitly defines $\underline{z}_2$ as a function of elasticity of earnings e and the utility loss ϕ associated with adjusting earnings:

$$(1-\tau_1)\left(\frac{\underline{z}_2}{1+e}\left(\frac{1-\tau_1}{1-\tau_0}\right)^e - z_2^*\right) + \frac{1-\tau_0}{1+\frac{1}{e}}\left(\underline{z}_2^{-\frac{1}{e}} z_2^{*1+\frac{1}{e}}\right) + \phi = 0 \tag{A.8}$$

I use the observed distribution of earnings at a neighbourhood of the kink at z_2^* after the policy change and estimate the second regression specified in (1.13). Panel (c) of Figure 1.8 shows the fitted polynomial with parameters set as $l = u = 3$ and $D = 6$. Feeding $\hat{b}_2$, the estimated normalized bunching at the kink at z_2^* after the policy change from (1.16), and

Δz_2^* from (1.11) into (1.10) results in:

$$\underline{z}_2 = \left(\frac{1-\tau_0}{1-\tau_1}\right)^e z_2^* - \delta\hat{b_2} \tag{A.9}$$

Together (A.8) and (A.9) define another equation of e and ϕ. I assume a linear adjustment costs as $\phi = \phi_1 + \alpha\phi_2$ that vary by individuals' ability α. I then numerically solve the three equations specified earlier simultaneously to estimate e, ϕ_1 and ϕ_2.

A.2.3 Tables

Table A.1: Estimated elasticity of earnings and adjustment costs

(a) Fixed adjustment costs (Gelber et al., 2016)

	Bunching at kink at \$400 before policy change b_1^0	Earnings response at kink at \$400 before policy change Δz_1^{*0}	Bunching at \$400 after policy change b_1^1	Elasticity e	Adjustment costs ϕ
A. Full sample					
Within two years	2.920***	62.605***	1.950***	0.210***	11.933***
	(0.227)	(6.028)	(0.107)	(0.019)	(0.972)
Within one year and half	2.790***	58.975***	2.120***	0.198***	11.733***
	(0.202)	(5.009)	(0.124)	(0.016)	(0.744)
Adding 5%	2.920***	59.481***	1.950***	0.186***	11.119***
payroll taxes	(0.227)	(5.373)	(0.107)	(0.016)	(0.777)
B. Age					
18-34	2.660***	57.295	1.630***	0.193***	10.642***
	(0.175)	(9.160)	(0.101)	(0.029)	(2.202)
35-49	2.680***	58.203***	1.550***	0.196***	10.657***
	(0.217)	(13.112)	(0.122)	(0.041)	(3.142)
> 50	3.600***	77.854***	2.770***	0.257***	15.639***
	(0.7.5)	(18.100)	(0.239)	(0.055)	(4.288)
C. Gender					
Male	3.510***	77.040***	2.160***	0.254***	14.410***
	(0.377)	(18.436)	(0.146)	(0.056)	(4.450)
Female	2.210***	46.063***	1.680***	0.157***	9.139***
	(0.144)	(3.371)	(0.087)	(0.011)	(0.470)
D. Disability type					
Psychotic	4.630	53.160	1.620***	0.182	3.317
	(3.771)	(35.160)	(0.147)	(0.112)	(14.756)
Neurological	2.330***	48.441***	2.050***	0.165***	10.224***
	(0.159)	(3.443)	(0.112)	(0.011)	(0.496)
Mental	4.300***	184.393***	2.100***	0.547***	39.403***
	(0.630)	(49.252)	(0.174)	(0.122)	(11.420)
E. Living location					
Metropolitan area	4.290***	95.123***	3.180***	0.308***	18.954***
	(0.962)	(18.123)	(0.216)	(0.053)	(3.242)
Other	1.650***	32.933***	0.880***	0.114***	5.647***
	(0.136)	(4.176)	(0.103)	(0.014)	(1.350)

Note: This table presents the estimated elasticity of earnings with respect to net-of-tax ratios assuming that fixed loss is associated with adjusting earnings, using the model specified in (Gelber et al., 2016). The bootstrapped standard errors are in the parenthesis.
$*p < 0.10, **p < 0.05, ***p < 0.01$

(b) No adjustment costs (Saez, 2010)

	Bunching b	Earnings response Δz^*	Elasticity e
A. Full sample			
Within two years	2.920***	29.000***	0.100***
	(0.227)	(2.274)	(0.008)
Within one year and half	2.790***	28.000***	0.100***
	(0.202)	(2.019)	(0.007)
Adding 5%	2.920***	29.000***	0.090***
payroll taxes	(0.227)	(2.274)	(0.007)
B. Age			
18-34	2.660***	27.000***	0.090***
	(0.175)	(1.748)	(0.006)
35-49	2.680***	27.000***	0.090***
	(0.217)	(2.171)	(0.007)
> 50	3.600***	36.000***	0.120***
	(0.705)	(7.048)	(0.023)
C. Gender			
Male	3.510***	35.000***	0.120***
	(0.377)	(3.770)	(0.013)
Female	2.210***	22.000***	0.080***
	(0.144)	(1.439)	(0.005)
D. Disability type			
Psychotic	34.630	46.000	0.16
	(3.771)	(36.708)	(0.241)
Neurological	2.330***	23.000***	0.080***
	(0.159)	(1.593)	(0.005)
Mental	4.300***	43.000***	0.150***
	(0.630)	(6.300)	(0.021)
E. Living location			
Metropolitan area	4.290***	43.000***	0.110***
	(0.962)	(9.616)	(0.007)
Other	1.650***	16.000***	0.060***
	(0.136)	(1.361)	(0.005)

Note: This table presents the estimated elasticity of earnings with respect to net-of-tax ratios assuming that no utility loss associated with adjusting earnings using the model specified in Section A.2.1. The bootstrapped standard errors are in the parenthesis.
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.2: Robustness of the estimted amount of bunching at the kinks with respect to the selected parameters

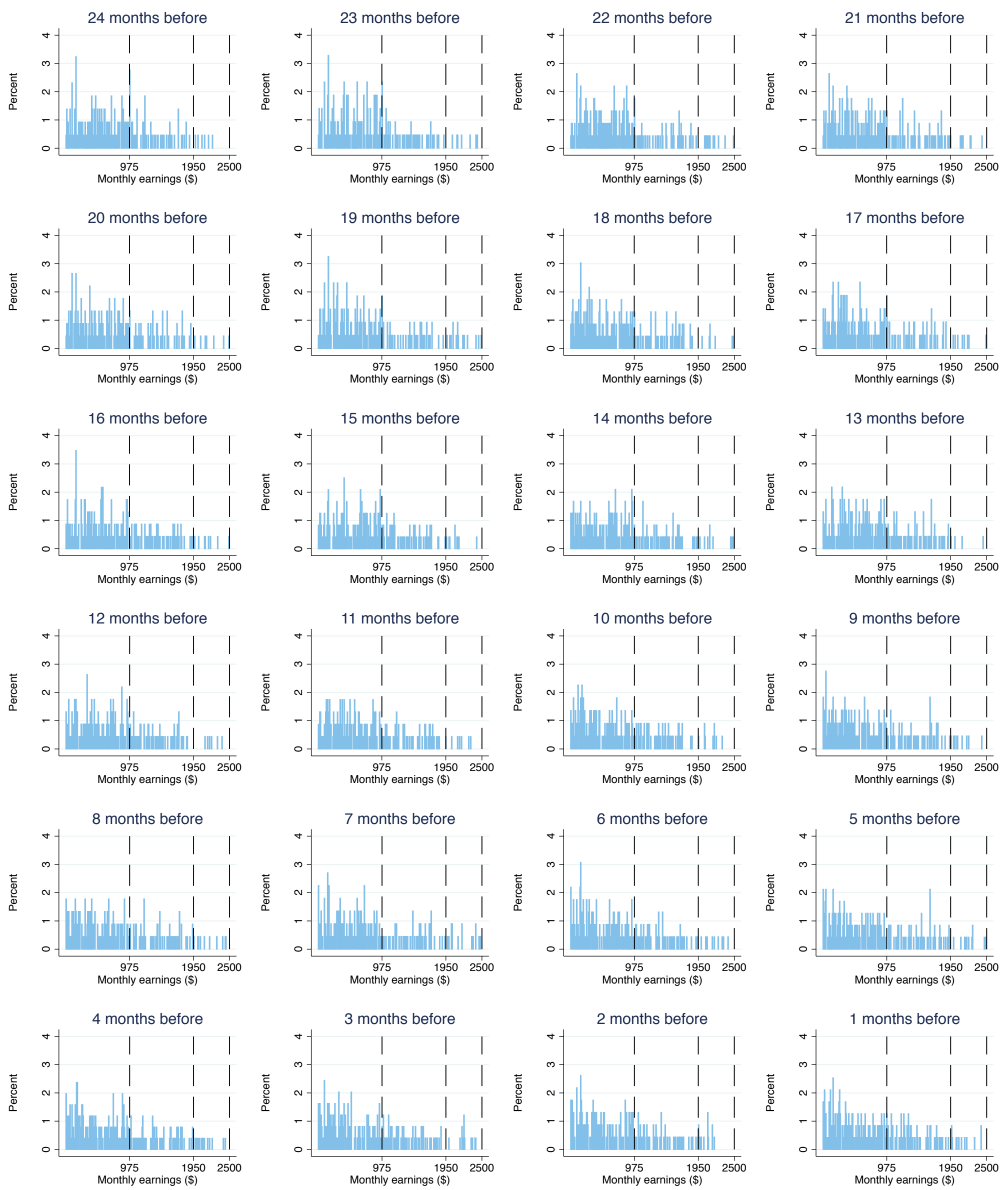
Bin size ($) δ	Degree of polynomial D	Number of excluded bins at each side l	Bunching at kink at $400 before policy change b_1^0	Bunching at kink at $400 after policy change b_1^1	Bunching at kink at $800 after policy change b_2
			Panel A: Baseline estimate		
10	6	3	2.920***	1.950***	1.880***
			(0.227)	(0.107)	(0.389)
			Panel B: Robustness to bin size		
5	6	6	3.460***	1.430***	0.730***
			(0.353)	(0.172)	(0.197)
15	6	2	1.020***	0.640***	0.310***
			(0.065)	(0.059)	(0.073)
			Panel C: Robustness to degree		
10	5	3	2.030***	1.400***	0.650*
			(0.131)	(0.113)	(0.408)
10	7	3	1.650***	0.880***	0.420*
			(0.115)	(0.092)	(0.327)
			Panel D: Robustness to excluded bins		
10	6	2	1.860***	1.170***	0.750***
			(0.126)	(0.108)	(0.304)
10	6	4	0.760***	0.710***	-0.060
			(0.086)	(0.098)	(0.214)

Note: This table presents the estimated amount of normalized bunching at the kinks with respect to the selected parameters using (1.13), (1.15) and (1.16). The selected parameters include bin size, degree of the fitted polynomial and the number of excludes bins around a kink. Since changing the bin size also changes the number of excluded bins, therefore number of the excluded bins are changes accordingly. The bootstrapped standard errors are in the parenthesis.
$*p < 0.10, **p < 0.05, ***p < 0.01$

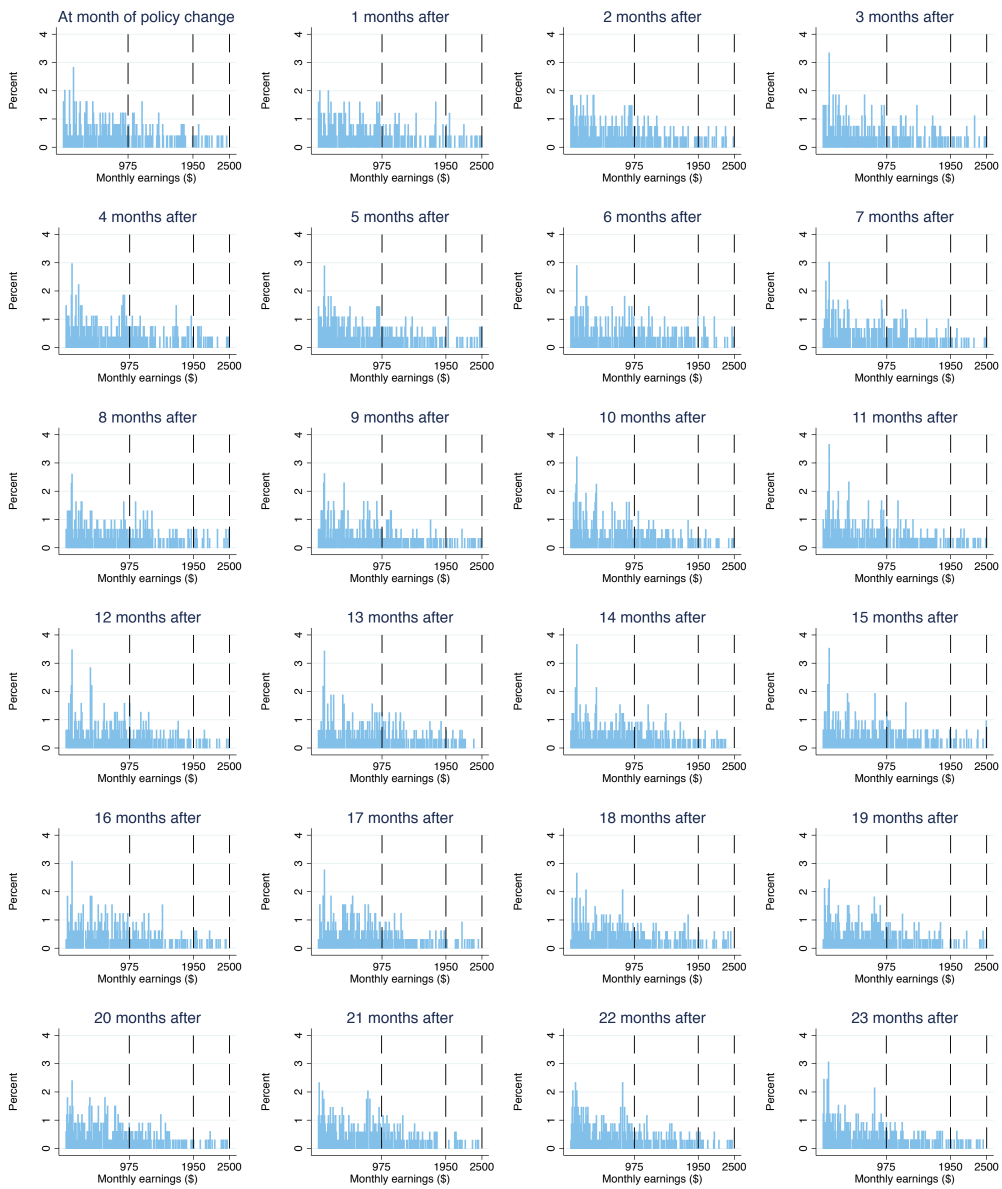
A.2.4 Figures

Figure A.1: Distribution of monthly earnings of AISH benefit recipients with dependents by relative month to the policy change

(a) Before policy change



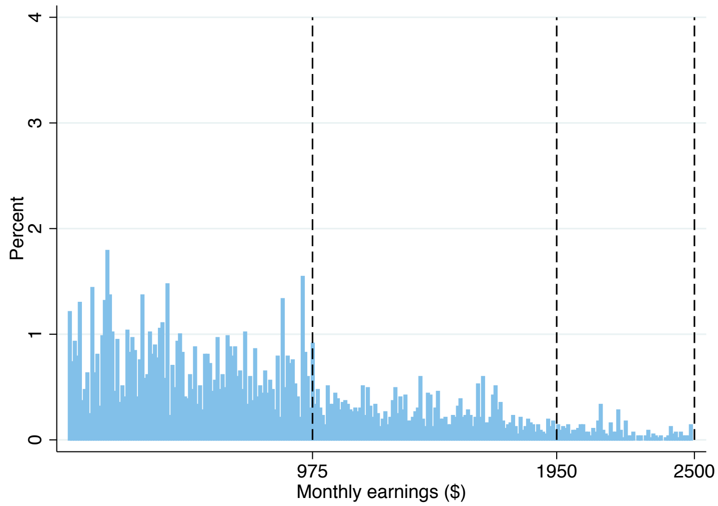
(b) After policy change



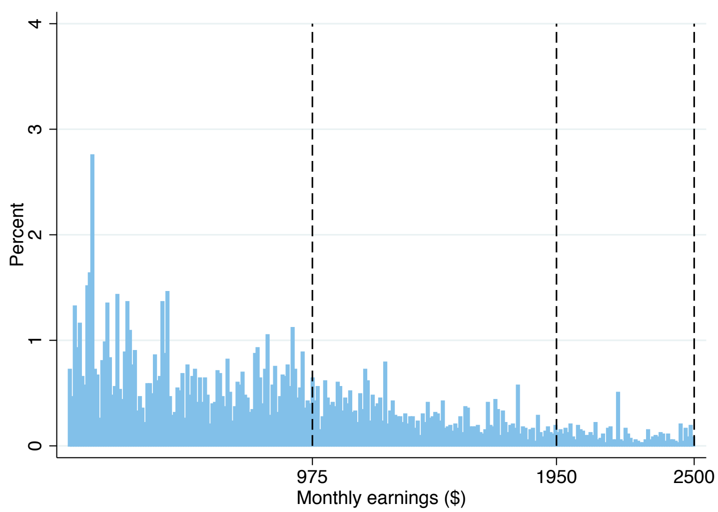
Note: This figure plots the distribution of monthly earnings of DI recipients in AISH within $10 bins. The sample includes individuals 18-64 years old with dependents who have non-physical disabilities. Panel (a) and Panel (b) show the distributions respectively two years before and two years after the policy change. There is no noticeable bunching at any of the kinks before, neither after the policy change.

Figure A.2: Distribution of monthly earnings of DI recipinets in AISH with dependents

(a) Before policy change



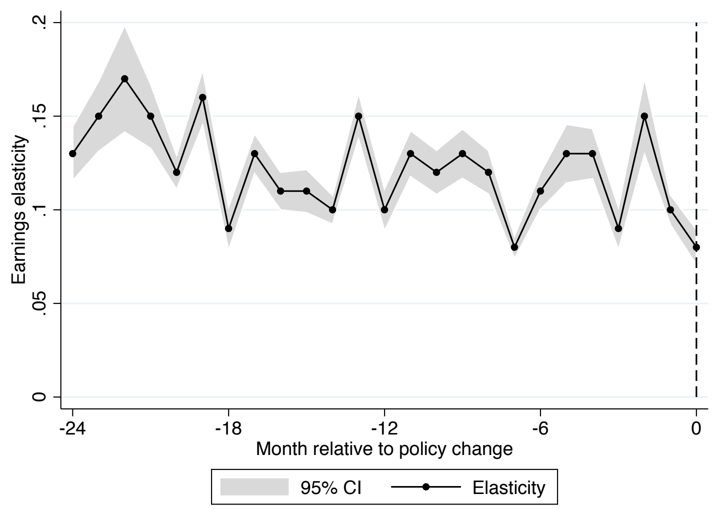
(b) After policy change



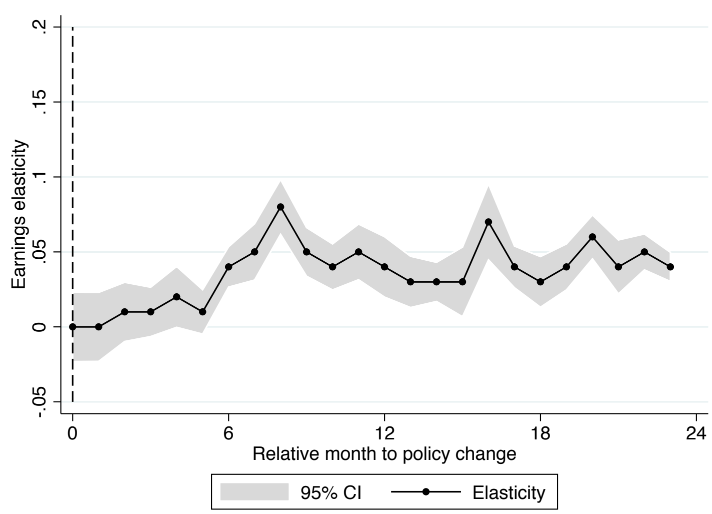
Note: This figure shows the distribution of monthly earnings of DI recipients in AISH within $10 bins. The sample includes individuals 18-64 years old with dependents who have non-physical disabilities. Panel (a) and (b) show the distribution of earnings for the pooled sample respectively two years before and after the policy change. There is no noticeable bunching at any of the kinks before, neither after the policy change.

Figure A.3: Estimated elasticity of earnings with no adjustment costs (Saez, 2010)

(a) At the old exemption threshould



(b) At the new exemption threshould



Note: This figure shows the estimated elasticity of earnings with respect to net-of-tax ratios at the exemption thresholds before and after the policy change using (Saez, 2010) method described in Appendix A.2.1. The sample includes 18-64 years old DI recipients with no dependents who have non-physical disabilities. The parameters used for the estimation are $\delta = 10$, $D = 6$ and $l = u = 3$. The estimated elasticity at the old exemption threshold gradually decreases while it increases at the new exemption threshold. The 95% Confidence Intervals (CI) using bootstrapped standard errors are shown in gray shades.

A.3 Regression Discontinuity Design

I exploit the policy change in AISH at April 2012 (cut-off date) in Regression Discontinuity (RD) design framework using date as assignment variable. Intuitively, I compare labor supply outcomes right after the policy change (treatment group) to those right before the policy change (control group). The general regression model in a RD design is:

$$y_{im} = f(m) + \rho D_{im} + \epsilon_{im} \tag{A.10}$$

where y_{im} is the outcome variable of individual i at month m and ϵ_{im} is the error term. D_{im} is a treatment dummy that captures the effects of policy change at the cut-off date c at April 2012 and defined as:

$$D_{im} = \begin{cases} 1 & \text{if } m \geq c \\ 0 & \text{if } m < c \end{cases} \tag{A.11}$$

This is a sharp RD design since the treatment variable is a deterministic function of the running variable m. The coefficient of interest is ρ which is the effect of the policy change on the outcome variable. The identification assumption here is that $f(m)$ is a smooth function. Under this assumption, the estimated ρ from the discontinuity in the empirical regression function at the point where the treatment variable switches on is the effect of the policy change on the outcome variable. In my main specification, I assume that $f(m)$ is a linear function and estimate local linear RD design regression which uses separate regressions on each side of the cut-off month c. The corresponding regression model to the left (control group) and right (treatment group) side of the cut-off are respectively:

$$y_{im} = \alpha_l + f_l(c - m) + \epsilon^l_{im} \text{ if } m < c \tag{A.12}$$

$$y_{im} = \alpha_r + f_r(m - c) + \epsilon^r_{im} \text{ if } m \geq c$$

where f_l and f_r are two smooth functions. The RD estimate of the effect of the policy change on the outcome is:

$$\widehat{\alpha}^{RD} = \widehat{\alpha_r} - \widehat{\alpha_l} \tag{A.13}$$

I use the method introduced by Calonico et al. (2014) to make inference about the impact of the policy change where they non parametrically construct robust confidence intervals for the average treatment effects at the cut-off point. In my main specification I use local linear regressions with triangular kernel density and six months of bandwidth on each side of the cut-off date.

Validity of this RD design relies on the assumption that benefit recipients just before and after the policy change are identical on average except getting exposed to the more generous benefits after the policy change. This assumption implies that change in the benefits is the only source of discontinuity in the outcome variable around the date of the policy change. There are however reasons to believe that this assumption might be violated. If more generous benefits induces more entries to the program then the benefit recipients just before and after the policy change might not be identical on average. Furthermore, since the new policy allows benefit recipients to work more and still be able to collect higher monthly allowance, it might also cause changes in the characteristics of the marginal entrants to the program where the new entrants might be healthier and be able to work more. The change in the benefits has been in effect since April 2012, but it was announced two months in advance in February 2012. To resolve this concern, I follow a similar approach to that of Marie and Vall Castello (2012) and exclude those who have entered into the program after announcing the policy change.

A.3.1 Graphical evidences

An appealing feature of RD design is that the impact of the policy change can be illustrated graphically. Figure A.4 plots mean monthly CPI adjusted earnings and labor force participation for a sample of individuals with non-physical disabilities. Those who have been awarded DI after February 2012 (the date policy change was announced) are exclude. This exclusion ensures that the individuals right before the cut-off date are on average identical to those right after, since exit rates of the program is almost zero. I plot the mean monthly earnings and the estimated monthly mean using local linear regressions in each side of the cut-off date. The scale of the y-axis is set equal to ± 0.25 standard deviation of earning. Scaling y-axis makes comparing the jumps at the cut-off date across the figures easier. This figure shows a discontinuity in earnings around the date of policy change but not much for labor force participation. This suggests that policy change in AISH has impacted earnings of the benefit recipients.

A.3.2 RD design estimates

Base estimates

Table A.3 shows the estimated impact of the policy change in AISH on earnings and extensive margins in a local linear RD design framework specified in model (A.12). I use a six months window around the cut-off date. The study sample includes individuals with non-physical disabilities. To ensure validity of the RD design, I exclude those who have been awarded after announcing the new policy at February 2012. Robust standard errors are estimated using the method of (Calonico et al., 2014) which are clustered in individual level. The estimated impact of the policy change within six months of the policy change is 8.9 percentage point increase in earnings and about one percentage point increase in extensive margins but it is not significant in conventional levels. Including individual characteristic sex, age, family structure, age DI awarded at, disability type and living location dose not change the estimated effects.

The estimated impact of the policy change using band width varying from 3 months up to 12 months around the cut-off date are plotted in Figure A.5. Panel (a) plots the estimates for earnings and Panel (b) plots the estimates of extensive margins. The 95% confidence intervals are shown in gray shades. These figures show the estimated effects of the policy change in AISH earnings and extensive margins are quite robust to the selected band width.

Seasonality effects

My estimates of the effects of the policy change in AISH at April 2012 show increase in earnings and labor force participation of benefit recipients. There is a concern than the seasonality of labor supply and demand might be the driving force in changes in labor supply. To further shed light on this concern, I estimate RD design models using placebo policy changes.

Figure A.6 shows discontinuity in earnings and labor force participation at placebo policy changes at April 2010 (two years before the actual policy change) in Panel (a), at April 2011 (one year before) in Panel (b) and at April 2013 (one year after) in Panel (c). The sample includes individuals with non-physical disabilities within a year of the corresponding placebo policy change. Scale of the y-axis is set to ± 0.25 standard deviation of the corresponding variable for the corresponding sample. This figure suggests that there is not a discontinuity in earnings neither labor force participation around the date of the corresponding placebo policy change. Table A.4 presents the estimated effects of the placebo policy changes on earnings and extensive margins from (A.12) using a six months window. All the estimates are negative and insignificant. This finding suggest that either there is no seasonality effect or if there is causes decrease in labor supply. In either case, it is unlikely that my findings presented in Table A.3 be contaminated by seasonality effects and at least my estimates would be a lower bond on the true effect of the policy change in AISH on earnings and extensive margins.

A.3.3 Tables

Table A.3: Estimated effect of policy change on earnings and extensive margins

	Earnings ($)		Extensive margin (%)	
	(1)	(2)	(3)	(4)
Robust Estimated effect	22.52*** (6.88)	22.54*** (6.86)	0.99 (0.77)	1.06 (0.76)
Mean in AISH before policy change	252.69 (427.04)	252.69 (427.04)	47.41	47.41
Individual co-variates	No	Yes	No	Yes
Num. of Obs.	112,768	112,768	112,768	112,768

Note: This table shows the estimated impact of the policy change in AISH on CPI adjusted earnings and extensive margins using local linear Regression Discontinuity (RD) design specified in (A.12). The study sample includes individuals with non-physical disabilities. Those who have been awarded DI after announcing the policy change at February 2012 are excludes. I use triangular kernel and a six month window around the cut-off date at April 2012. The included covariates consists of sex, age, age DI awarded at, disability type, live in metropolitan. Robust standard errors of the estimated coefficients in the parenthesis are clustered in individual level.
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table A.4: Estimated effects from placebo policy changes

	April 2010		April 2011		April 2013	
	Earnings ($)	Extensive margin (%)	Earnings ($)	Extensive margin (%)	Earnings ($)	Extensive margin (%)
Robust	-8.06	-0.08	-2.84	-0.20	-0.85	0.02
Estimated effect	(6.51)	(0.78)	(6.22)	(0.75)	(6.65)	(0.72)
Mean in AISH	271.95	52.08	249.92	47.82	281.83	47.92
before policy change	(422.86)		(415.43)		(472.67)	
Num. of Obs.	99,575	99,575	107,476	107,476	118,886	118,886

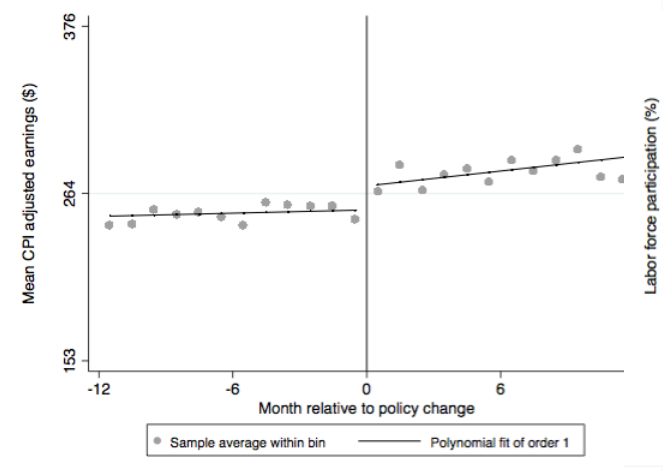
Note: This table shows the estimated effects on earnings and extensive margins from placebo policy changes in April 2010, April 2011 and April 2013 using local linear Regression Discontinuity (RD) design specified in (A.12). The study sample includes individuals with non-physical disabilities. I use triangular kernel and a six month window around the cut-off date at each placebo policy change. All estimates include individuals covariates sex, age, age DI awarded at, disability type, live in metropolitan. Robust standard errors of the estimated coefficients in the parenthesis are clustered in individual level. All the estimated coefficients are insignificant suggesting that there is no seasonality effects.

$*p < 0.10, **p < 0.05, ***p < 0.01$

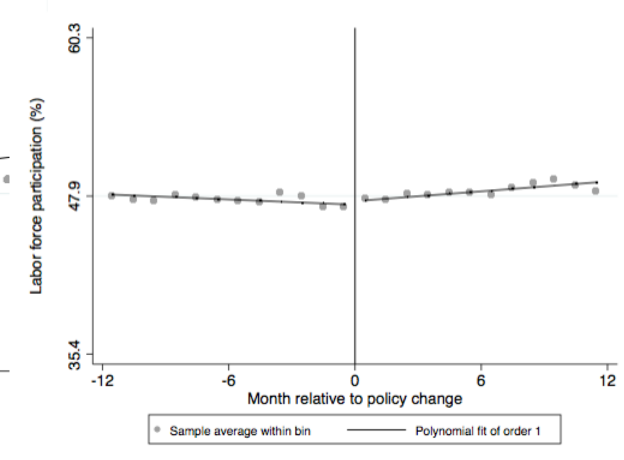
A.3.4 Figures

Figure A.4: Discontinuty in earnigs and labor force participation in AISH

(a) Earnings



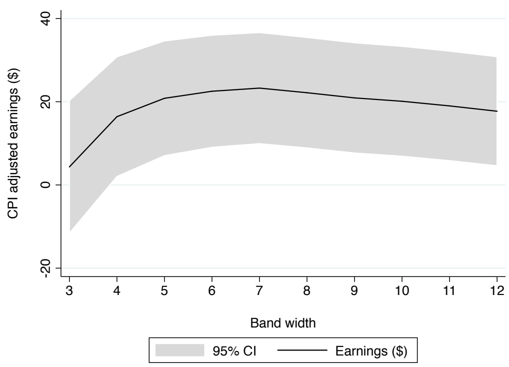
(b) Labor force participation



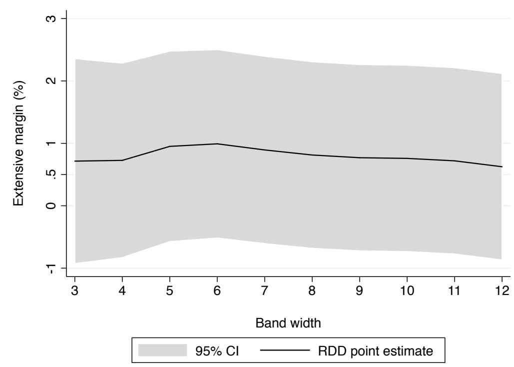
Note: This figure shows the mean CPI adjusted earnings and labor force participation by month within one year of the policy change respectively in Panel (a) and (b). The sample includes individuals with non-physical disabilities within a year of the policy change in AISH. Those who have been awarded DI after February 2012 are excluded. Scale of the y-axis is ±0.25 standard deviation of the corresponding variable.

Figure A.5: Local linear RD estimates of the policy change with different band widths

(a) Earnings



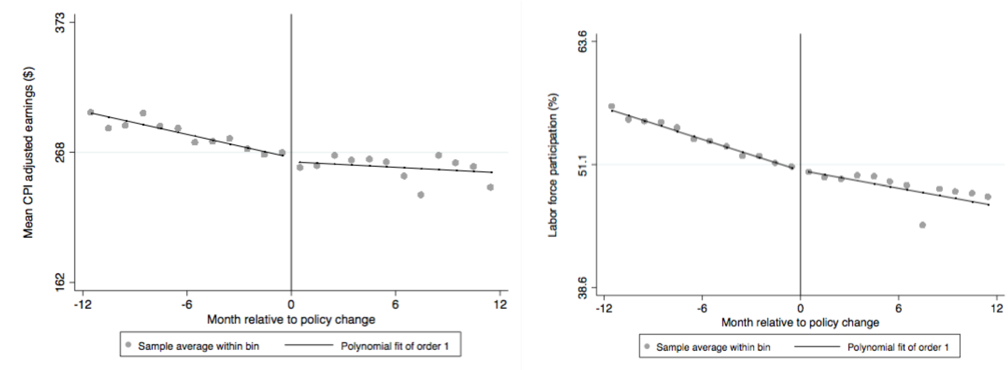
(b) Extensive margins



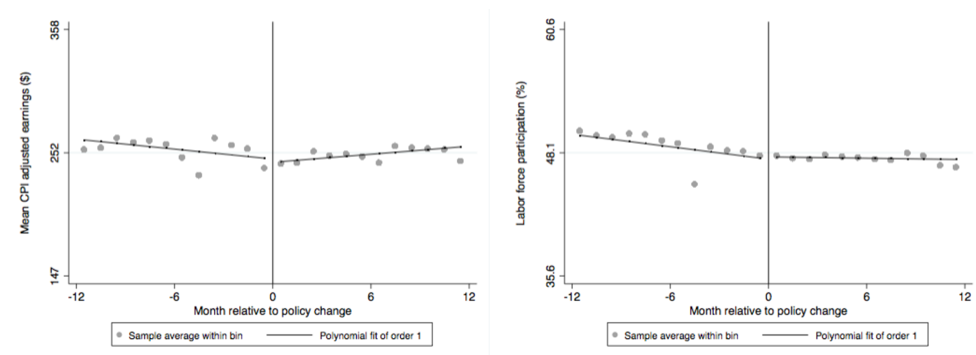
Note: This figure shows the local linear RD design estimates using different band widths ranging from 3 to 12 months specified in (A.12) using a triangular kernel. Panel (a) shows the estimated effects on earnings and Panel (b) shows them for extensive margins. The sample includes benefit recipients with non-physical disabilities. Those who have been awarded after announcing the policy change in February 2012 are excluded. The gray shade indicates the %95 Confidence Intervals (CI) which are calculated using (Calonico et al., 2014) method. The estimated effects are quite robust to the selected band width.

Figure A.6: Discontinuity in earnings and labor force participation from placebo policy changes

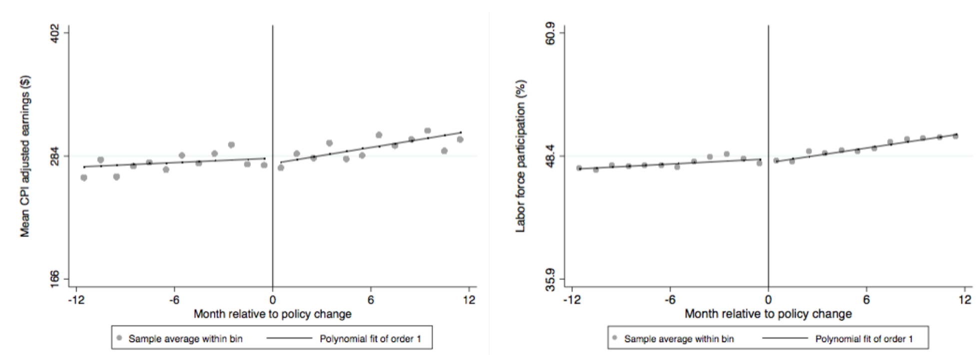
(a) placebo policy change at April 2010



(b) placebo policy change at April 2011



(c) placebo policy change at April 2013



Note: This figure plots mean CPI adjusted earnings and labor force participation around placebo policy changes at April 2010 at Panel (a), at April 2011 at Panel (b) and at April 2013 at Panel (c). In each panel, the sample includes those with non-physical disabilities within a year of the corresponding placebo policy changes. Scale of the y-axis is Âś0.25 standard deviation of the corresponding variable. These figures show that there is no clear change in earnings neither in labor force participation from placebo policy changes.

A.4 Income effect of policy change in AISH

The April 2012 policy change in AISH consists of two pieces; first, doubling earnings exemption threshold; second, 35% increase in maximum monthly DI benefits. While this policy change might induce both income and substitution effects, I assume that the induced income effect are negligible and I use a quasi-linear utility function specified in (1.3) for estimating elasticity of earnings and heterogeneous adjustment costs. In this section I provide suggestive evidence that the induced income effect of the policy change are negligible and this is a plausible assumption.

Panel (a) of Figure 1.1 shows the budget constraints of DI recipients in AISH with no dependents. Theoretically, individuals with monthly earnings between zero and \$400 and those with monthly earnings above \$800 before the policy change are only exposed to income effect (pieces with parallel budget constraints). Similarly Panel (b) shows that those with monthly earnings between zero and \$950 and above \$1,950 before the policy change are only exposed to income effect. I use sample of individuals who are expected to be exposed only to income effect, to estimate induced income effect of the policy change in AISH in Difference-in-Difference (DD) framework using corresponding sub samples of benefit recipients of ODSP as control group. My estimates of elasticity of earnings and adjustment costs presented in Table 1.2 suggest that those with earnings within \$100 of the thresholds would respond to the policy change. I restrict my samples to within \$100 of each threshold to make sure that my finding are not contaminated by any other confounding factor.

A.4.1 Descriptive evidences and findings

Figure A.7 plots the trends in mean CPI adjusted earnings of AISH and ODSP benefit recipients for different samples that are exposed to income effect. Panel (a) and (b) show the trends for samples of individuals with no dependents whose monthly earnings is in the range $(0, \$300]$ respectively six months and one year prior to the policy change. Panel (c) and (d) show the trends for samples of individuals with no dependents whose monthly earnings in above $900 respectively six months and one year prior to the policy change. Finally, Panel (e) shows the trends for those with dependents whose earnings six months prior to the policy change is in the range $(0, \$850]$. The sub sample of individuals with family whose earnings one year prior to the policy change is in the range $(0, \$850]$ is quite small. These figures all visually suggest that for each sub sample trends in earnings in AISH is quite similar to that in ODSP prior to the policy change.

Table A.5 presents the estimated effects of the policy change for each sub sample described above using the corresponding sub sample from ODSP as control group using (1.17). Most of the estimated effects are negative and insignificant. The estimated positive effects are either insignificant or very small. Each of these sub samples are more likely to be effected by income effect induced by the policy change and are less likely to be effected by the induced substitution effects of the policy change. Therefore, the estimated effects provide suggestive evidence on the induced income effect of the policy change.

A.4.2 Tables

Table A.5: Estimated income effect of the policy change

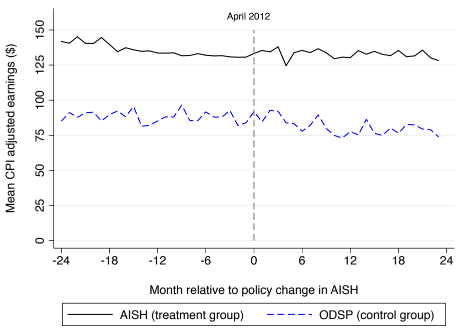
	No dependent				With dependent(s)
	(1)	(2)	(3)	(4)	(5)
AISH × Post	-1.61	4.74***	-4.99	18.97	-4.76
	(1.23)	(1.22)	(12.48)	(10.40)	(11.12)
AISH	44.66***	37.36***	-133.79***	-81.01***	2.21
	(0.81)	(0.83)	(8.23)	(7.19)	(6.67)
Sample	$0 < earnings \leq 300$	$0 < earnings \leq 300$	$earnings \geq 900$	$earnings \geq 900$	$0 < earnings \leq 850$
	12 months	6 months	12 months	6 months	6 months
Individual co-variates	Yes	Yes	Yes	Yes	Yes
Mean in AISH	138.76	135.59	1,248.98	1,140.49	307.25
before policy change	(103.65)	(118.55)	(421.28)	(492.57)	(348.25)
R-Sq.	0.06	0.04	0.07	0.07	0.01
Num. of. Obs.	213,642	268,394	29,361	52,104	55,667

Notes: This table shows the estimated effects from Difference-in-Difference framework using (1.17) for samples of individuals who are likely to get exposed only to income effect of the policy change in AISH. The sample in each columns includes those whose earnings x months before the policy change always have been $y_1 < earnings \leq y_2$. Each sample covers two years within the policy change. Included individual co-variates are sex, age, age DI awarded at, disability type and living location. Robust standard deviations are in the parenthesis.
$*p < 0.10, **p < 0.05, ***p < 0.01$

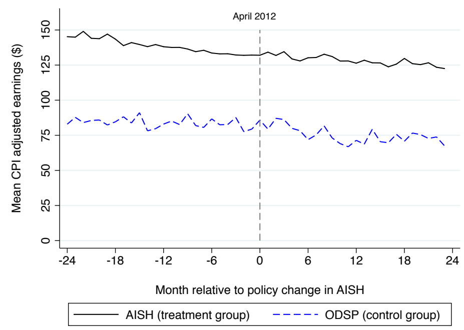
A.4.3 Figures

Figure A.7: Trends in earnings before and after April 2012 policy change in AISH for those facing only income effect

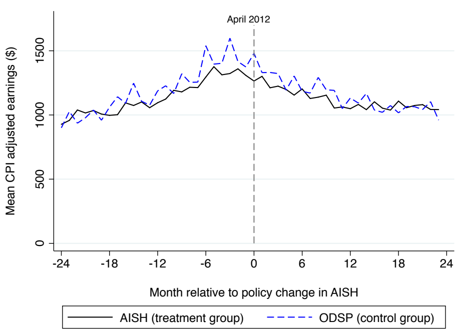
(a) No dependents and earnings in range $(0, \$300]$ six months before the policy change



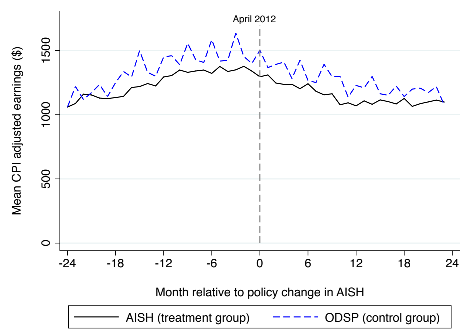
(b) No dependents and earnings in range $(0, \$300]$ one year before the policy change



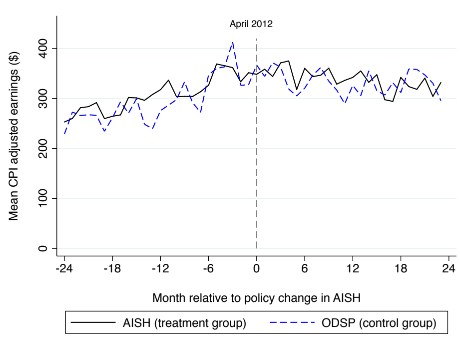
(c) No dependents and earnings over \$900 six months before the policy change



(d) No dependents and earnings over \$900 one year before the policy change



(e) With dependents and earnings in the range $(0, \$850]$ six months before the policy change



Note: This figure shows trends in earnings before and after the policy change in AISH at April 2012 for AISH and ODSP benefit recipients who are only exposed to income effect of the policy change. The sample includes those with non-physical disabilities within two years of the policy change. The sample is further specified in the title of each panel.

Appendix B

Appendix to Chapter 2

B.1 Analysis using 2006 PALS

B.1.1 Tables

Table B.1: Summary statistics

(a) Demographics

	Physical disabilities	Neuro-Cognitive disabilities	Developmental disabilities	ASD
Sex (%):				
- Male	46.51	46.72	57.99	82.83
- Female	53.49	53.28	42.01	17.17
Age (%):				
- 15-19 years	1.86	6.58	17.33	40.75
- 20-25 years	2.35	6.43	12.06	17.50
- 25-34 years	7.63	13.00	16.33	7.75
- 35-64 years	88.16	74.00	54.28	34.00
Marital status (%):				
- Single/Divorced/Widowed	33.70	54.90	77.70	100
- Married/Common law	66.29	45.05	21.94	0
Education (%):				
- Less than high school (16-64 yrs)	22.94	34.90	65.84	76.92
- High school graduate (16-64 yrs)	77.06	65.10	34.16	23.08
- High school graduate (18-64 yrs)	78.41	68.30	38.59	34.37
Province of residence‡ (%):				
- Newfoundland & Labrador & Prince Edward Island	2.61	1.94	2.58	2.42
- Nova Scotia	4.33	4.02	3.85	2.50
- New Brunswick	2.99	2.78	2.87	2.42
- Quebec	17.09	15.65	19.40	27.25
- Ontario	41.03	45.03	39.19	39.25
- Manitoba	3.90	3.17	3.49	4.92
- Saskatchewan	3.09	2.72	4.06	2.42
- Alberta	11.26	8.95	9.23	9.33
- British Columbia	13.70	15.73	15.34	9.50
Severity of condition (%):				
- Less Severe	73.64	35.34	26.34	43.42
- More Severe	26.36	64.66	73.66	56.58
Number of Obs.	1,475,970	895,830	129,400	12,000

(b) Prevalence of disabilities

	Physical disabilities	**Neuro-Cognitive disabilities**	**Developmental disabilities**	**ASD**
15-24 yrs	1.48% (1 in 68)	2.76% (1 in 36)	1.16% (1 in 86)	0.17% (1 in 604)
25-34 yrs	2.81% (1 in 35)	2.91% (1 in 34)	0.95% (1 in 105)	0.02% (1 in 4307)
35-64 yrs	9.66% (1 in 10)	4.92% (1 in 20)	0.52% (1 in 192)	0.03% (1 in 3302)
Total	6.80% (1 in 15)	4.13% (1 in 24)	0.60% (1 in 168)	0.06% (1 in 1808)

(c) Labour force statistics

	Physical disabilities	**Neuro-Cognitive disabilities**	**Developmental disabilities**	**ASD**
Labour Force Participation (%)	72.22	49.03	33.64	23.09
- Employment (%)	50.66	30.09	23.73	13.67
- Unemployment(%)	7.10	15.76	16.25	33.06
Occupation (%)				
- Management/Business/Finance	14.95	15.40	18.16	n/a‡
- Sci/Educ/Art/Health/Sport	27.39	22.99	12.52	n/a‡
- Sale/service	31.00	34.94	33.16	n/a‡
- Manufacturing/utility	21.22	15.55	16.20	n/a‡
Employment income:				
- Average annual employment income ($)	22,305	10,711	5,940	3,289
- Zero annual employment income (%)	33.07	50.62	68.01	82.08
- Average weekly paid hours (hrs)	20	11	7	3
Government transfer:				
- Average annual transfer ($)	3,963	6,114	5,661	5,104
- Low income after tax (%)	15.35	28.86	26.87	13.67
Number of Obs.	1,475,970	895,830	129,400	12,000

Note: This table presents summary statistics from 2006 Participation Activity and Limitation Survey (PALS). Study sample includes 15-64 years old individuals who have reported having ASD, developmental, neuro-cognitive and physical disabilities. Survey weights generating estimated frequencies in the target population are used in construction of this table, in accordance with Statistics Canada guidelines. Panel (a) presents demographic statistics. Panel (b) presents estimated prevalence of each disability across the age groups. Panel (c) presents labor force statistics.

Table B.2: Estimated Probit model across disability groups

(a) ASD

	(1)	(2)	(3)	(4)	(5)
Sex: Female	-0.113***	-0.154***	-0.197***	-0.210***	-0.216***
	(0.007)	(0.006)	(0.006)	(0.006)	(0.006)
Age: 15-19 yrs	0.081***	0.119***	0.108	0.120**	0.134***
	(0.012)	(0.012)	(0.018)	(0.017)	(0.017)
25-34 yrs	0.273***	0.279***	0.402***	0.356***	0.391***
	(0.019)	(0.017)	(0.020)	(0.017)	(0.018)
35-64 yrs	-0.050***	-0.063***	-0.0624**	-0.059**	-0.057*
	(0.011)	(0.010)	(0.022)	(0.019)	(0.023)
Severity: More Severe	-0.254***	-0.237***	-0.274***	-0.227***	-0.230*
	(0.007)	(0.007)	(0.010)	(0.009)	(0.010)
Education: $\geq$ High school	0.273***	0.232***	0.354***	0.282***	0.289***
	(0.011)	(0.011)	(0.015)	(0.015)	(0.012)
Thousands of annual government transfers	-0.011***	-0.002*	-0.011***	-0.005***	0.001
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)
Probability of participation for Reference group‡	0.324***	0.113***	0.305***	0.135***	0.037***
	(0.058)	(0.073)	(0.102)	(0.105)	(0.146)
Province of residence	No	Yes	No	Yes	Yes
Age $\times$ severity	No	No	Yes	Yes	Yes
Education $\times$ severity	No	No	No	No	Yes
Number of obs.	12,000	11,710	10,830	10,610	10,610
Pseudo R2	0.2314	0.2836	0.2330	0.2829	0.3270

(b) Developmental disabilities

	(1)	(2)	(3)	(4)	(5)
Sex: Female	-0.026***	-0.022***	-0.021***	-0.018***	-0.017***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Age: 15-19 yrs	-0.189***	-0.190***	-0.182***	-0.183***	-0.182***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
25-34 yrs	0.119***	0.119***	0.127	0.127***	0.127***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
35-64 yrs	-0.017***	-0.023***	-0.012***	-0.015***	-0.015***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Marital status: Married/Common law	-0.017***	0.018***	-0.019***	0.013***	0.013***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Severity: More Severe	-0.108***	-0.102***	-0.098***	-0.091***	-0.090***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Education: $\geq$ High school	0.106***	0.096***	0.103***	0.094***	0.094***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Thousands of annual government transfers	-0.020***	-0.020***	-0.020***	-0.020***	-0.020***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Probability of participation for Reference group‡	0.512 *	0.410 ***	0.614 ***	0.523 **	0.516***
	(0.015)	(0.016)	(0.020)	(0.021)	(0.023)
Province of residence	No	Yes	No	Yes	Yes
Age $\times$ severity	No	No	Yes	Yes	Yes
Education $\times$ severity	No	No	No	No	Yes
Number of obs.	128,930	128,930	128,930	128,930	128,930
Pseudo R2	0.0934	0.1254	0.1021	0.1336	0.1336

(c) Neuro-cognitive disabilities

	(1)	(2)	(3)	(4)	(5)
Sex: Female	-0.063***	-0.064***	-0.062***	-0.063***	-0.062***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age: 15-19 yrs	-0.126***	-0.125***	-0.115***	-0.115***	-0.108***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
25-34 yrs	0.017***	0.020***	0.043***	0.046***	0.046***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
35-64 yrs	-0.070***	-0.067***	-0.056***	-0.053***	-0.053***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Marital status: Married/Common law	0.008***	0.003**	0.009***	0.003**	0.004***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Severity: More Severe	-0.180***	-0.179***	-0.182***	-0.181***	-0.178***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Education: $\geq$ High school	0.093***	0.087***	0.093***	0.087***	0.093***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Thousands of annual government transfers	-0.023***	-0.022***	-0.023***	-0.022***	-0.022***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Probability of participation for Reference group‡	0.661 ***	0.635 ***	0.690 ***	0.666 ***	0.618 ***
	(0.006)	(0.006)	(0.008)	(0.008)	(0.009)
Province of residence	No	Yes	No	Yes	Yes
Age $\times$ severity	No	No	Yes	Yes	Yes
Education $\times$ severity	No	No	No	No	Yes
Number of obs.	895,350	895,350	895,350	895,350	895,350
Pseudo R2	0.1417	0.1545	0.1429	0.1559	0.1564

(d) Physical disabilities

	(1)	(2)	(3)	(4)	(5)
Sex: Female	-0.081***	-0.083***	-0.083***	-0.084***	-0.085***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Age: 15-19 yrs	-0.113***	-0.114***	-0.133***	-0.136***	-0.139***
	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)
25-34 yrs	0.058***	0.060***	0.024***	0.023***	0.022***
	(0.003)	(0.003)	(0.004)	(0.003)	(0.003)
35-64 yrs	-0.048***	-0.046***	-0.073***	-0.073***	-0.073***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Marital status: Married/Common law	-0.018***	-0.020***	-0.017***	-0.020***	-0.019***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
severity: More Severe	-0.240***	-0.238***	-0.239***	-0.237***	-0.234***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Education: $\geq$ High school	0.149***	0.144***	0.149***	0.145***	0.140***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Thousands of annual government transfers	-0.017***	-0.017***	-0.017***	-0.017***	-0.017***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Probability of participation for Reference group‡	0.680 ***	0.685 ***	0.670 ***	0.675 ***	0.694 ***
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Province of residence	No	Yes	No	Yes	Yes
Age $\times$ severity	No	No	Yes	Yes	Yes
Education $\times$ severity	No	No	No	No	Yes
Number of obs.	1,475,860	1,475,860	1,475,860	1,475,860	1,475,860
Pseudo R2	0.1177	0.1221	0.1181	0.1226	0.1232

Note: This table presents the estimated Average Marginal Effects (AME) of individual characteristics on probability of Labor Force Participation (LFP) estimated using (2.1) across the disability groups. Study sample includes 15-64 years old individuals from 2006 Participation Activity and Limitation Survey (PALS) with ASD, Developmental, Neuro-cognitive and Physical disabilities. The dependent variable is a dummy that turns on for those participating in labor force. Survey weights generating estimated frequencies in the target population are used in all the estimates. Panel (a), (b), (c) and (d) present the estimated effects respectively for those with ASD, Developmental, Neuro-conitive and Physical disabilities. Robust standard errors are presented in parenthesis.

‡ Reference group for each disability group includes 15-19 years old single males with less severe disabilities who reside in Ontario and have never finished high school.
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table B.3: Blinder-Oaxaca decompositions

(a) ASD versus Developmental, Neuro-cognitive and Physiacal disabilites

	ASD					
	Developmental disabilities		**Neuro-Cognitive disabilities**		**Physical disabilities**	
	Coefficient	in % of $\widehat{\Delta}$	Coefficient	in % of $\widehat{\Delta}$	Coefficient	in % of $\widehat{\Delta}$
$lfp_{ComparisonGroup}$	0.2949*** (0.0013)		0.3581*** (0.0005)		0.5472*** (0.0004)	
lfp_{ASD}	0.2266*** (0.0042)		0.2266*** (0.0042)		0.2266*** (0.0042)	
$\widehat{\Delta}$	0.0683*** (0.0044)		0.1315*** (0.0042)		0.3206*** (0.0042)	
Endowment Effect (E)	-0.1125*** (0.0038)	-164	-0.0792*** (0.0047)	-60	0.0020*** (0.0088)	1
Coefficient Effect (C)	0.0163*** (0.0043)	24	0.1209*** (0.0041)	91	0.1972*** (0.0055)	61
Interaction Effect (I)	0.1645*** (0.0037)	240	0.0897*** (0.0046)	69	0.1215*** (0.0095)	38

(b) Developmental and Neuro-cognitive disabilites versus Physical disabilites

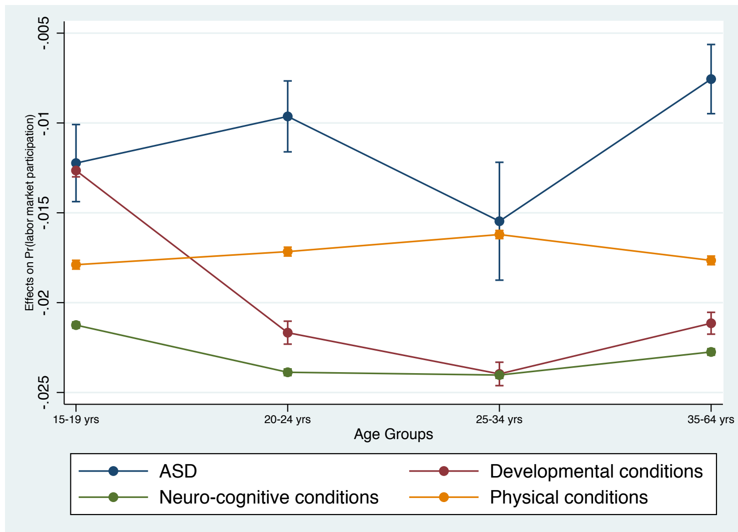
	Physical disabilities			
	Developmental disabilities		**Neuro-Cognitive disabilities**	
	Coefficient	in % of $\widehat{\Delta}$	Coefficient	in % of $\widehat{\Delta}$
$lfp_{Physical}$	0.5472*** (0.0004)		0.5472*** (0.0004)	
$lfp_{ComparisonGroup}$	0.2831*** (0.0012)		0.3581*** (0.0005)	
$\widehat{\Delta}$	0.2641*** (0.0013)		0.1891*** (0.0007)	
Endowment Effect (E)	0.1259*** (0.0025)	48	0.1207*** (0.0006)	64
Coefficient Effect (C)	0.0785*** (0.0021)	30	0.0568*** (0.0008)	30
Interaction Effect (I)	0.0596*** (0.0029)	22	0.0116*** (0.0008)	6

Note: This table presents Blinder-Oaxaca decompositions of differences in Labor Force Participation (LFP) between two groups, estimated using the model presented in Section 2.2.2. Study sample includes 15-64 years old individuals from 2006 Participation Activity and Limitation Survey (PALS) who have reported having ASD, Developmental, Neuro-cognitive and Physical disabilities. Survey weights generating estimated frequencies in the target population are used in all the estimates. Panel (a) presents decomposing lower LFP of those with ASD than those with Developmental and Neuro-cognitive disabilities. Panel (b) presents decomposing lower LFP of those with Developmental and Neuro-cognitive disabilities than those with physical disabilities.
$*p < 0.10, **p < 0.05, ***p < 0.01$

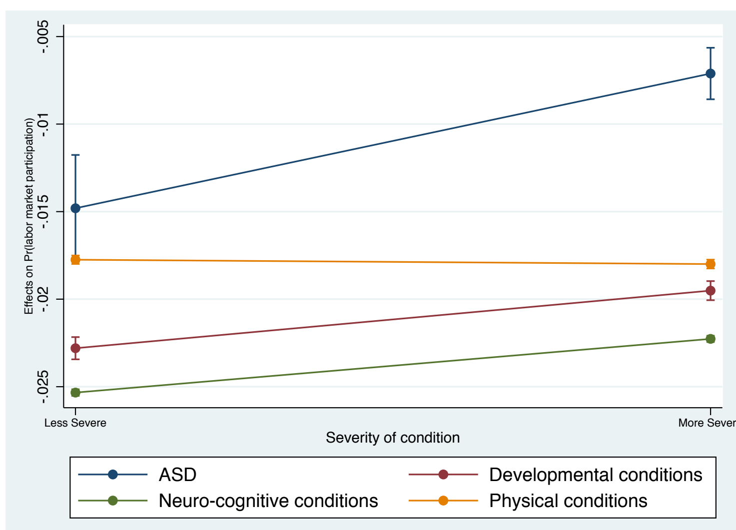
B.1.2 Figures

Figure B.1: Fitted Average Marginal Effects of individual charactersitics on probability of Labor Force Participation by type of disability

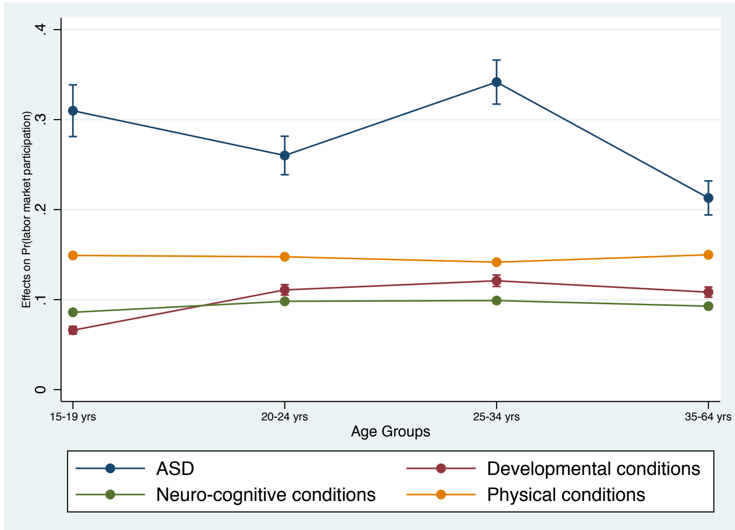
(a) Effects of government transfers across age groups



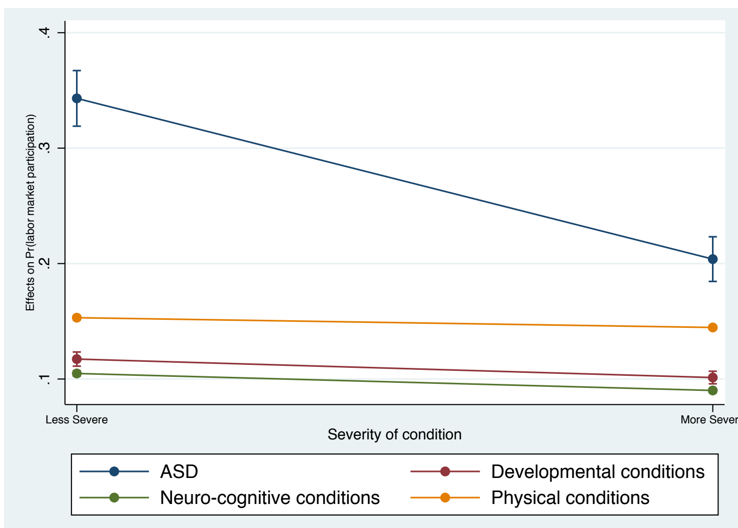
(b) Efefcts of government transfers across severity of diability



(c) Effects of completing high school across age groups



(d) Effects of completing high school across severity of disabilites



Note: This figure plots fitted Average Marginal Effects (AME) of individual characteristics on probability of Labor Force Participation (LFP) across disability types estimated from Probit model specified in (2.1). Study sample includes 15-64 years old individuals from 2006 Participation Activity and Limitation Survey (PALS) who have reported having ASD, Developmental, Neuro-cognitive and Physical disabilities. Survey weights generating estimated frequencies in the target population are used in all the estimates. Panel (a) and (b) plot AME of government transfers respectively across age groups and severity of disability. Panel (c) and (d) plot AME of completing high school respectively across age groups and severity of disabilities.

B.2 Institutional background on disability benefit programs in Canada

Federal and provincial disability benefit programs in Canada are designed to provide partial earning replacement to individuals who a medically determinable physical or non-physical disability limits kind or amount of paid work they can do. Federal government's benefits include Employment Insurance (EI), Sickness benefits (one must have accumulated at least 600 hours of insurable employment in the qualifying period to receive up to 15 weeks of benefits), Canada Pension Plan (CPP) and Quebec Pension Plan (QPP) disability benefits (to be eligible, one must have enough contributions to the CPP/QPP), Child Disability benefit (CDB) (a tax-free benefit for families who care for a child under 18 with a severe and prolonged disability), Special Benefits for Parents of Critically Ill Children (PCIC) (for eligible parents who take leave from work to provide care or support to their critically ill or injured child for up to 35 weeks) and Employment Insurance Compassionate Care Benefits (for those take time off work to provide care or support to a family member who is gravely ill and is at risk of dying within six months).[1] Access to federal DB program are based on employment history or benefits are available only for a short period of time. Individuals with lifelong and severe disabilities therefore would not be eligible to receive these benefits or even if they are eligible, since these programs are short term, they would need more assistance. Canadian provinces including Alberta, Ontario, British Columbia and Saskatchewan provide social assistance to disabled individuals who are not eligible for the federal DB program.[2] Provincial programs are operated under different ministries in each province but they all provide income support and supplementary benefits to their beneficiary. The amount of the benefits and size of the programs differ however substantially within the provinces, Alberta's

[1]More information on federal government's disability benefit programs: `http://www.fcac-acfc.gc.ca/Eng/forConsumers/lifeEvents/livingDisability/Pages/Federalp-Prestati.aspx`, Accessed on Feb 29, 2016.

[2]More information on provincial disability benefit programs: `http://www.fcac-acfc.gc.ca/Eng/forConsumers/lifeEvents/livingDisability/Pages/Resource-Ressourc.aspx`, Accessed on Feb 29, 2016.

program is the most generous one and Ontario's is the largest one.

B.3 Sample design and variable definitions

Sample Design in the 2006 PALS and the 2012 CSD

A two-phase stratified design is used for identifying and selecting individuals with disabilities in the PALS and CSD. The first phase in PALS consists of the systematic distribution of the census long form to approximately every fifth household, which contains two disability filter questions: 1) *Do you have any difficulty hearing, seeing, communicating, walking, climbing stairs, bending, learning, or doing any similar activities?* and 2) *Does a physical disability or mental disability or health problem reduce the amount or the kind of activity you can do at home, at work or at school or in other activities?* Second phase strata is based on the characteristics defining the strata: province/territory, age group, severity of disability according to the census (defined by response categories "often" and "sometimes") and probability of selection in the first phase. Then, individuals are selected from those who responded "yes" to at least one of the two disability filter questions, based on the strata. The CSD uses a similar sampling process. A two-phase design is used for identifying and selecting individuals from the the National Household Survey (NHS) in the CSD. The filtering questions are the same, however the definition of disabilities are slightly different.

As the PALS and CSD are surveys based on a probability sampling plan, each person selected for the survey represents themselves as well as certain number of other persons in the target population who are not part of the sample. Therefore, the weight variable in these datasets, gives the number of persons represented by each record. The weights of the individuals have been calculated based on the probability of selection and have been adjusted so that the these samples are representative of the population of interest. Because of those adjustments and because certain individuals had unequal probabilities of selection, the weights might vary significantly from one person to another. The weight must therefore be used for all estimates and analyses that are based on these datasets, otherwise the results will be biased.

Table B.4: Variable definition for dependent and independent variables

	Definition
Dependent variable	
Labour Market Participation	= 1 if participating in the labour market, =0 otherwise
Age	
Age 15-19 years	= 1 if aged 15-19 years, = 0 otherwise
Age 20-24 years	= 1 if aged 20-24 years, = 0 otherwise
Age 25-34 years	= 1 if aged 25-34 years, = 0 otherwise
Age 35-64 years	= 1 if aged 35-64 years, = 0 otherwise
Sex	
Male	= 1 if is a male
Female	= 1 if is a female
Marital status	
Single or divorced	= 1 if is single or divorced
Married or common law	= 1 if is married or in a common law relationship
Severity of condition	
Less severe	= 1 if condition is less severe, = 0 otherwise
More severe	= 1 if condition is more severe, = 0 otherwise
Educational	
Less than High School	= 1 if highest level of education is less than high school, = 0 otherwise
High School	= 1 if respondent is graduated from high school, = 0 otherwise
Total annual government transfer	
Total annual government transfer/1000	= thousands of total annual government transfers , = 0 otherwise
Province of residence	
Newfoundland and Labrador	= 1 if resides in Newfoundland and Labrador, = 0 otherwise
Prince Edward Island	= 1 if resides in Prince Edward Island, = 0 otherwise
Nova Scotia	= 1 if resides in Nova Scotia, = 0 otherwise
New Brunswick	= 1 if resides in New Brunswick, = 0 otherwise
Quebec	= 1 if resides in Quebec, = 0 otherwise
Ontario	= 1 if resides in Ontario, = 0 otherwise
Manitoba	= 1 if resides in Manitoba, = 0 otherwise
Saskatchewan	= 1 if resides in Saskatchewan, = 0 otherwise
Alberta	= 1 if resides in Alberta, = 0 otherwise
British Columbia	= 1 if resides in British Columbia, = 0 otherwise

Appendix C

Appendix to Chapter 3

C.1 Estimation procedure of a Generalized Synthetic Control model

Xu (2017) provides a procedure for estimating a Generalized Synthetic Control (GSC) model specified in Equation (3.2) as:

$$y_{it} = \delta_{it} D_{it} + X'_{it}\beta + \lambda'_i f_t + \epsilon_{it} \tag{C.1}$$

The procedure consists of three main steps. First step includes estimating an interactive fixed effect model using the data only from the control group (i.e. setting $D_{it} = 0$ in Equation (C.1)). The control group consists of states that never mandated IVF coverage in their private health insurances. Assume that $F = [f_1, f_2, ..., f_T]$ and $\Lambda_{control} = [\lambda_1, \lambda_2, ..., \lambda_{control}]$ where *control* denotes the number of states in control group and T denotes the time periods in the analysis. r is the number of factors (f_t and λ_i are r vectors). To identify β, F and $\Lambda_{control}$ however more constraints are required. Two constraints therefore are imposed. First, all factors are normalized, $\frac{\hat{F}'\hat{F}}{|T|} = I_r$, where I_r denotes the identity matrix and $|T|$ is the total number of time periods in the analysis. Second, loadings are orthogonal to each other, $\hat{\Lambda}'_{control}\hat{\Lambda}_{control} = \text{diagonal}$. To obtain estimated $\hat{\beta}$, $\hat{F}$ and $\hat{\Lambda}_{control}$ then:

$$(\hat{\beta}, \hat{F}, \hat{\Lambda}_{control}) = \underset{\hat{\beta}, \hat{F}, \hat{\Lambda}_{control}}{\arg\max} \sum_{i \in control} (Y_i - X_i\hat{\beta} - \hat{F}\hat{\lambda}_i)'(Y_i - X_i\hat{\beta} - \hat{F}\hat{\lambda}_i) \tag{C.2}$$

$$\text{s.t. } \frac{\hat{F}'\hat{F}}{|T|} = I_r \text{ and } \hat{\Lambda}'_{control}\hat{\Lambda}_{control} = \text{diagonal}$$

The number of factors r however is unknown and is estimated through a cross validation process that minimizes the prediction error of the model. Estimation process starts with a given r to obtain the corresponding $\hat{\beta}$, $\hat{F}$ and $\hat{\Lambda}_{control}$. For each pre-treatment period $s \in \{1, 2, ..., T_0\}$, hold back data of all treated states at time s. Then run an OLS regression using the rest of the pre-treatment data to obtain factor loadings for each treated unit i, $\hat{\lambda}_{i,-s}$. Then predict the treated outcome at time s as $\hat{y}_{is}(0) = X'_{is}\hat{\beta} + \hat{\lambda}_{i,-s}\hat{f}_s$.[1]

Define the prediction error as $e_{is} = y_{is}(0) - \hat{y}_{is}(0)$. Mean Square Prediction Error (MSPE) for given r is defined as:

$$MSPE(r) = \sum_{s=1}^{T_0} \sum_{i \in T} \frac{e_{is}^2}{T_0} \tag{C.3}$$

where T_0 denotes the number of pre-treatment periods. This process is repeated for different values of r (we try $r \in \{1, 2, ..., 5\}$). Then r^* corresponding to the smallest prediction error is chosen.

The factor loadings for the treated states are estimated in the second step. This is done by minimizing the mean square error of the predicted treated outcome in pretreatment periods:

$$\hat{\lambda}_i = \arg\max_{\hat{\lambda}_i} (Y_i^0 - X_i^0\hat{\beta} - \hat{F}^0\hat{\lambda}_i)'(Y_i^0 - X_i^0\hat{\beta} - \hat{F}^0\hat{\lambda}_i) \tag{C.4}$$

where ”0” superscripts denote the pre-treatment time periods and $\hat{\beta}$ and $\hat{F}^0$ are estimated from the first step.

The third step finally estimates the treated counter-factual based on $\hat{\beta}$, $\hat{F}$ and $\hat{\lambda}_i$. That is:

$$\hat{y}_{it}(0) = X'_{it}\hat{\beta} + \hat{\lambda}'_i\hat{f}_i \quad \text{for } i \in Treated,\, t > T_0 \tag{C.5}$$

[1] This is a notation from potential outcome framework for casual inference where $y_{it}(0)$ and $y_{it}(1)$ are the potential outcome for state i at time t when respectively $D_{it} = 0$ and $D_{it} = 1$.

The estimated Average Treatment effect on Treated at time t, ATT_t then is:

$$A\hat{T}T_t = \frac{1}{|Treated|} \sum_{i \in Treated} [y_{it}(1) - \hat{y}_{it}(0)] \quad \text{for } t > T_0 \tag{C.6}$$

C.2 Difference-in-Differences model

To further check robustness of our findings from GSC framework, we also estimate effects of mandated coverage of IVF on incidence of multiple birth using a Difference-in-Differences (DD) framework. Similar to our GSC model, we exploit variation in state and time of enacted mandated coverage. Treatment group includes states with mandated coverage of IVF in their private health insurances. Control group includes 36 never mandated states. We estimate a model as below:

$$y_{it} = \alpha_0 + \alpha_1 D_{it} + X_i'\alpha_2 + \alpha_i + \alpha_t + \eta_{it} \tag{C.7}$$

where i denotes states and t denotes time. y_{it} denotes the outcome variable including share of multiple births and number of infants per thousand births. D_{it} is a dummy variable that turns one for treated states at years after the mandated coverage of IVF in private health insurances. α_i and α_t are respectively state and time fixed effects. The vector X_i is a set of time invariant demographic characteristics to control for any observable differences that might confound the analysis (mothers' age, education, race, marital status, fathers' race and infants sex, birth weight and order of birth). η_{it} captures any unobserved factors affecting incidence of multiple births. The coefficient of interest is α_1 which measures the effects of policies mandating covering IVF in private health insurance in mandated states relative to states that never legislated such policies.

We also estimated a DD model using individual level data. We estimate the following equation:

$$y_{jit} = \alpha_0 + \alpha_1 D_{jit} + X_{ji}'\alpha_2 + \alpha_i + \alpha_t + \eta_{jit} \tag{C.8}$$

where j denotes individuals and the rest of notations are the same as those in Equation (C.7). y_{jita} denotes the dependent variable which is a dummy that turns on for multiple births and zero otherwise. D_{jita} is a dummy that turns on for mothers older than 35 in mandated states two years after mandated coverage is enacted. The coefficient of interest is α_1 which measures the effects of mandated IVF coverage on probability of a multiple birth.

Our study sample includes all births from 1975 to 2014, aggregated in state-year cells. Table C.1 and Table C.2 present the estimated effects of mandated IVF coverage from DD model specified in Equation (C.7) respectively on share of multiple births and number of infants per thousand births. Table C.3 also presents the estimated effects from DD model specified in Equation (C.8) using individual data. Panel (a) in each table presents the estimated effects from different number of covered cycles and Panel (b) presents the overall estimated effects from mandated coverage. Similar to our GSC estimations, we also estimate the effects for different demographic groups that are more likely to use IVF treatment and therefore are more likely to be affected by mandated coverage policies.

Identification assumption in a DD model is that treatment and control groups follow a parallel trends in periods prior to mandated IVF coverage. As plotted in Panel (c) of Figure 3.2 and Figure 3.3, it is unlikely that parallel trend assumption holds and therefore the estimated effects might not interpreted as causal effects. The overall findings from DD analysis however are along with those from our GSC presented in Table 3.3. The estimated effects of mandated coverage of IVF is the lowest for states with less generous plans and the highest for more generous ones.

C.2.1 Tables

Table C.1: Estimated effects of mandated coverage of IVF on share of multiple births from a Difference-in-Differences model using aggregated data

(a) Number of covered cycles in mandated states versus never mandated states

	All mothers		*+35 mothers*		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1 cycle * Post mandate	-0.11***	-0.06	-0.30	-0.39**	-0.18***	-0.14*	-0.13	-0.06*	-0.11*	-0.11*
	(0.03)	(0.04)	(0.23)	(0.14)	(0.02)	(0.06)	(0.06)	(0.03)	(0.05)	(0.05)
2 cycles * Post mandate	0.31***	0.26***	0.68***	0.56***	0.12***	0.14***	0.41***	0.28***	0.31***	0.21***
	(0.01)	(0.03)	(0.03)	(0.07)	(0.02)	(0.04)	(0.02)	(0.05)	(0.02)	(0.04)
3 cycles * Post mandate	0.20***	0.10**	0.38***	0.36***	0.18***	0.14***	0.31***	0.14**	0.20***	0.09**
	(0.02)	(0.03)	(0.04)	(0.08)	(0.02)	(0.03)	(0.07)	(0.04)	(0.04)	(0.03)
4 cycles * Post mandate	0.26*	0.20*	0.54**	0.47**	0.21***	0.18***	0.42**	0.30*	0.29*	0.22
	(0.12)	(0.09)	(0.20)	(0.16)	(0.05)	(0.04)	(0.14)	(0.12)	(0.14)	(0.12)
+5 cycles * Post mandate	0.47***	0.27***	0.93***	0.88***	0.47***	0.41***	0.61***	0.37***	0.53***	0.29***
	(0.02)	(0.04)	(0.04)	(0.08)	(0.02)	(0.05)	(0.02)	(0.06)	(0.02)	(0.04)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of cells	1,,760	1760	1,760	1,760	1,760	1,760	1,628	1,628	1,760	1,760

(b) Mandated to cover states versus never mandated states

	All mothers		*+35 mothers*		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mandate to cover * Post mandate	0.18*	0.12**	0.34*	0.27	0.13	0.09	0.28**	0.16***	0.20*	0.11*
	(0.07)	(0.04)	(0.17)	(0.14)	(0.08)	(0.05)	(0.10)	(0.04)	(0.08)	(0.05)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of cells	1,760	1,760	1,760	1,760	1,760	1,760	1,628	1,628	1,760	1,760

Note: This table presents the estimated average effects of mandated coverage of IVF in private health insurances on share of multiple birth from Difference-in-Differences (DD) model specified in Equation (C.7). The main sample includes all the births in the US. from 1975-2014, aggregated by state-year. The control group for each model includes the states who never been mandated to cover IVF treatment in their private health insurances. The included demographic characteristics are mothers' age, education, race, marital status, fathers' race and infants sex, birth weight and order of birth. Standard errors presented in parenthesis are clustered in state level. Panel (a) presents the estimated effects from the number of covered cycles and Panel (b) shows the overall effects of mandated coverage.

$*p < 0.10, **p < 0.05, ***p < 0.01$

Table C.2: Estimated effects of mandated coverage of IVF on number of infants per thousand births from a Difference-in-Differences model using aggregated data

(a) Number of covered cycles in mandated states versus never mandated states

	All mothers		*+35 mothers*		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
1 cycle * Post mandate	-1.25***	-0.61	-3.28	-4.15**	-1.98***	-1.47*	-1.45*	-0.62*	-1.22*	-1.12*
	(0.30)	(0.36)	(2.34)	(1.46)	(0.24)	(0.60)	(0.70)	(0.30)	(0.52)	(0.49)
2 cycles * Post mandate	3.16***	2.69***	6.86***	5.90***	1.14***	1.48***	4.14***	2.91***	3.11***	2.17***
	(0.13)	(0.34)	(0.32)	(0.75)	(0.22)	(0.41)	(0.20)	(0.54)	(0.19)	(0.46)
3 cycles * Post mandate	2.06***	1.04**	3.94***	3.90***	1.90***	1.56***	3.25***	1.46**	2.08***	0.93*
	(0.23)	(0.30)	(0.40)	(0.82)	(0.22)	(0.32)	(0.81)	(0.48)	(0.48)	(0.38)
4 cycles * Post mandate	2.73*	2.19*	5.59**	5.05**	2.21***	1.89***	4.35**	3.16*	3.06*	2.35
	(1.20)	(0.89)	(1.90)	(1.48)	(0.39)	(0.39)	(1.38)	(1.18)	(1.34)	(1.21)
+5 cycles * Post mandate	4.92***	2.89***	9.80***	9.51***	4.95***	4.38***	6.46***	4.00***	5.58***	3.05***
	(0.19)	(0.47)	(0.40)	(0.74)	(0.23)	(0.58)	(0.26)	(0.68)	(0.25)	(0.42)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of cells	1,760	1,760	1,760	1,760	1,760	1,760	1,628	1,628	1,760	1,760

(b) Mandated to cover states versus never mandated states

	All mothers		*+35 mothers*		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Mandate to cover * Post mandate	1.83*	1.25**	3.49*	2.87	1.28	1.02	2.84**	1.72***	2.01*	1.15*
	(0.78)	(0.37)	(1.73)	(1.53)	(0.82)	(0.59)	(1.02)	(0.47)	(0.87)	(0.53)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Demographic characteristics	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Number of cells	1,760	1,760	1,760	1,760	1,760	1,760	1,628	1,628	1,760	1,760

Note: This table presents the estimated average effects of mandated coverage of IVF in private health insurances on number of infants per thousand births from Difference-in-Differences (DD) model specified in Equation (C.7). The main sample includes all the births in the US. from 1975-2014, aggregated by state-year. The control group for each model includes the states who never been mandated to cover IVF treatment in their private health insurances. The included demographic characteristics are mothers' age, education, race, marital status, fathers' race and infants sex, birth weight and order of birth. Standard errors presented in parenthesis are clustered in state level. Panel (a) presents the estimated effects from the number of covered cycles and Panel (b) shows the overall effects of mandated coverage.
$*p < 0.10, **p < 0.05, ***p < 0.01$

Table C.3: Estimated effects of mandated coverage of IVF on share of multiple births from a Difference-in-Differences model using individual data

(a) Number of covered cycles in mandated states versus never mandated states

	All mothers		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 cycle * Post mandate	-0.13***	-0.06	-0.21***	-0.05	-0.16**	-0.10	-0.15***	-0.16*
	(0.03)	(0.12)	(0.03)	(0.12)	(0.06)	(0.13)	(0.03)	(0.07)
2 cycles * Post mandate	0.30***	1.62***	0.08***	1.49***	0.38***	1.72***	0.31***	1.68***
	(0.01)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)
3 cycles * Post mandate	0.18***	0.13***	0.17***	0.16***	0.24***	0.19***	0.17***	0.11*
	(0.02)	(0.03)	(0.02)	(0.03)	(0.04)	(0.04)	(0.03)	(0.05)
4 cycles * Post mandate	0.21	0.14	0.17**	0.23*	0.35**	0.21**	0.24*	0.13
	(0.10)	(0.07)	(0.05)	(0.10)	(0.13)	(0.08)	(0.12)	(0.07)
+5 cycles * Post mandate	0.44***	0.50***	0.46***	0.59***	0.58***	0.60***	0.51***	0.52***
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Individual characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Observations	99,007,886	64,117,539	53,315,784	31,893,355	64,537,316	52,625,358	77,807,449	53,665,021

(b) Mandated to cover states versus never mandated states

	All mothers		*+College mothers*		*Married mothers*		*White mothers*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mandated cover * Post mandate	0.20**	0.19*	0.16**	0.27**	0.32***	0.26*	0.25**	0.19
	(0.07)	(0.09)	(0.05)	(0.10)	(0.08)	(0.10)	(0.08)	(0.10)
Time fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age fixed effects	No	Yes	No	Yes	No	Yes	No	Yes
Individual characteristics	No	Yes	No	Yes	No	Yes	No	Yes
Number of births	99,007,886	64,117,539	53,315,784	31,893,355	64,537,316	52,625,358	77,807,449	53,665,021

Note: This table presents the estimated average effects of mandated coverage of IVF in private health insurances on probability of multiple birth from Difference-in-Differences (DD) model specified in Equation (C.7). The main sample includes all the births in the US. from 1975-2014. The control group for each model includes the states who never been mandated to cover IVF treatment in their private health insurances. The included demographic characteristics are mothers' age, education, race, marital status, fathers' race and infants sex, birth weight and order of birth. Standard errors presented in parenthesis are clustered in state level. Panel (a) presents the estimated effects from the number of covered cycles and Panel (b) shows the overall effects of mandated coverage.

$*p < 0.10, **p < 0.05, ***p < 0.01$