Effect of the 20 Hours Early Childhood Education Reform on women's labor market outcomes in New Zealand

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December 2017

HEC MONTREAL

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Management Science
Applied Economics
Master Thesis (M. Sc.)

February 2017

Abstract

The development of comprehensive childcare policies has become an important driver for governments in the hopes of improving labour market outcomes for women. Although some accommodation for work and family are in place, mothers still encounter barriers to the labour market, often referred as the motherhood penalty. This study provides a comprehensive analysis of the impact of the 20 Hours Early Childhood Education (ECE) reform in New Zealand, and how its economic incentives indirectly affect outcomes for mothers in the labour force. Our quasi-experimental framework uses a difference-in-differences (DD) model, as well as a triple-difference model (DDD), on both two-period and multi-period specifications. Our findings support the hypothesis that reducing the price of childcare increased maternal earnings for women. The estimates show a reduction of the motherhood penalty between mothers and childless women but are statistically insignificant.

Keywords: childcare subsidy, family-friendly policies, maternal earnings, motherhood penalty, 20 Hours Early Childhood Education

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Acknowledgement

After a rigorous couple of months, I am happy to be writing my acknowledgment and thanks those who have made this possible. It has been a period of intense learning for me, not only in terms of scientific research, but also on a personal level. This master thesis could not have been realized without a great deal of guidance and support. I am using this opportunity to deeply thank everyone who supported me throughout the course of this thesis.

I would like to express my gratitude to my supervisor, Dr. Gail Pacheco, whose expertise, understanding, and patience, added considerably to my graduate experience. I appreciate her vast knowledge and skill, and her engagement through the learning process of this project. I would also like to thank my co-supervisors, Dr. Lydia Cheung and Dr. Benoit Dostie, for the assistance they provided me at all levels of the research project. I am thankful for their inspiring guidance, invaluably constructive criticism and advice during the project.

Many thanks and appreciations also go to my family and friends who have provided me with continuous encouragement.

In conclusion, this research would not have been possible without the support of the New Zealand Work Research Institute, the Economics School of the Auckland University of Technology, and the University of HEC Montreal who provided me with the necessary resources.

Thank you,

Isabelle Bouchard

Montreal, December 2017

Disclaimer

The results in this paper are not official statistics, they have been created for research purposes from the Integrated Data Infrastructure (IDI), managed by Statistics New Zealand. The opinions, findings, recommendations, and conclusions expressed in this paper are those of the authors, not Statistics NZ.

The results are based in part on tax data supplied by Inland Revenue to Statistics NZ under the Tax Administration Act 1994. This tax data must be used only for statistical purposes, and no individual information may be published or disclosed in any other form, or provided to Inland Revenue for administrative or regulatory purposes. Any person who has had access to the unit record data has certified that they have been shown, have read, and have understood section 81 of the Tax Administration Act 1994, which relates to secrecy. Any discussion of data limitations or weaknesses is in the context of using the IDI for statistical purposes, and is not related to the data's ability to support Inland Revenue's core operational requirements.

Access to the anonymised data used in this study was provided by Statistics NZ in accordance with security and confidentiality provisions of the Statistics Act 1975. Only people authorised by the Statistics Act 1975 are allowed to see data about a particular person, household, business, or organisation, and the results in this paper have been confidentialised to protect these groups from identification. Careful consideration has been given to the privacy, security, and confidentiality issues associated with using administrative and survey data in the IDI.

Further detail can be found in the Privacy impact assessment for the Integrated Data Infrastructure available from www.stats.govt.nz.

Introduction

> *"I have had the view for a long time the best investment a country can make is in its early childhood education system."*
>
> - Trevor Mallard, Minister of Education 1999-2004 (Bushouse, 2008)

Rising levels of the labour force participation rate for females in the past century have driven the most astonishing compositional changes in the labour markets of developed countries. The combination of social and political pressures has resulted in a slow but resolute evolution of social norms. These changes have been accompanied by an emerging need for women to balance career and motherhood. The institutional context in which the labour market structure is embedded strongly influences the extent to which women are able to participate in paid market work.

Although some accommodation for work and family is in place, mothers still encounter barriers to participating in the labour market, often referred as the motherhood penalty. Mothers tend to earn lower wages than women without children, but are also disadvantaged in terms of hiring, training and remuneration (Budig, Misra, & Boeckmann, 2012, 2016; Gangl & Ziefle, 2009; Gough & Noonan, 2013; Hegewisch & Gornick, 2011). Bridging that gap by developing an inclusive infrastructure to support mothers has been the objective of many countries. The understanding of the causal impact of family policies on maternal outcomes has become very important to the development of comprehensive policies as a means of reducing penalties for mothers who want to work.

Women's economic responses to motherhood undoubtedly depend on the trade-off between the costs associated to childcare and the wage earned working (Bainbridge, Meyers, & Waldfogel, 2003; Baum, 2002; Blau & Robins, 1988; Meyer & Rosenbaum, 2001). In this study, we will focus specifically on the impact of childcare policies. Childcare policies reduce the financial burden of childcare. Therefore, mothers have higher incentive to return to paid work. (Budig et al., 2016). A new growing body of empirical studies, to which our paper relates, exploits exogenous policy changes to uncover the causal effects of the childcare price on maternal outcomes. This literature provides evidence that childcare reforms have positive impacts on maternal labour market outcomes.

In New Zealand, as in many other developed countries, mothers still suffer a labour market penalty. While New Zealand has a high labour force participation rate (5th highest in the OECD), it also has a relatively low participation rate amongst women of child-bearing age (25-34)[1]. Furthermore, approximately a third of all women in the labour force are in part-time employment. One of the most cited reasons for women's limited labour participation is the cost of childcare (Baker, Gruber, & Milligan, 2008; Baum, 2002; D. Blau & Currie, 2004; Cascio, 2009; Hegewisch & Gornick, 2011; Lefebvre & Merrigan, 2008; Powell, 1997).

Emerging from a range of social, economic, political and cultural perspectives, the Early Childhood Education (ECE) sector in New Zealand has adapted to the changing needs of children, parents and communities. The recent introduction of the 20 hours ECE reform is an attempt to alleviate these pressures. In 2007, the government undertook this major reform, which resulted in an important transformation in the landscape of the ECE sector. Childcare funding has changed from a partial subsidy to a full coverage of the cost of care for up to 20 hours a week. All children aged 3 to 5 years-old nationwide were eligible regardless of their parents' status. This represents the largest expansion of public subsidies for childcare in the ECE sector.

While most studies on childcare reforms have focused on labour force participation, the earning pattern of mothers after birth has rarely been investigated, and yet reflects a major source of disparities for mothers. While the 20 hours ECE policy was not aimed at improving maternal labor outcomes directly, it did have repercussions. The question that ultimately interests us is whether the childcare reform in New Zealand has made progress toward reducing the loss in maternal earnings or the motherhood wage penalty. In this paper, we provide a comprehensive analysis of the causal impact of the 20 hours ECE reform on maternal earnings.

In order to assess the impact of the reform, we use the data from Statistics New Zealand Integrated Data Infrastructure (IDI). We estimate a difference-in-differences (DD) model that exploits the temporal variation in childcare coverage induced by the difference in birth years to identify the causal impact of the reform on the mother wage penalty. As the program was implemented simultaneously across the country, we do not have a concurrent comparison group, like untreated states or regions[2]. Instead, we use the birthdate of the child to define the treatment and control group of a mother based on their eligibility to the program. Therefore, we estimate how the childcare reform affected the earnings of mothers who benefited from the reform compared to the mothers who did not, comparing earnings before and after the birth event.

[1] OECD (2017), Labour force participation rate (indicator). doi: 10.1787/8a801325-en
[2] In the literature on DD, it is common to use untreated states or regions as a natural control group (Angrist & Pischke, 2009; Baker et al., 2008; Haeck et al., 2015; Nollenberger & Rodríguez-Planas, 2015).

We also augment the model by using a multi-period specification. The results provide evidence that the 20 hours ECE reform in 2007 led to an increase in monthly earnings of mothers of one child by approximately 2 to 2.6 percent.[3]

To gain further confidence in our identification strategy, we use a triple-differences model (DDD) addressing compositional changes over time. To do so, we add a second comparison group which is composed of women without children. These non-mothers are chosen to be contemporaneous with the previous groups of mothers mentioned. The identification assumption of this DDD is that, on average, the difference between the earnings of mothers and non-mothers would have changed similarly in the absence of the childbirth event. The effect of the reform is then evaluated on three dimensions: between eligible and non-eligible mothers, between pre and post birth event, and between mothers and non-mothers. The aim of this extra comparison group is to pick up the time-varying effects specific to the calendar date, and to allow us to draw conclusions on how the reform impacts the motherhood penalty.

The first step in estimating the DDD model is to create the groups of non-mothers. We use a matched sampling approach to construct comparable control groups, based on covariates of observable characteristics shared by mothers and non-mothers, and by matching on a comparable calendar date. Then, we perform the DDD model with two periods, followed by the multiple-period specification. The regression estimates show that the 20 hours ECE reform increased monthly earnings of eligible mothers by 33 NZD, or 1.04 percent of pre-motherhood average earnings, controlling for time-specific effects such as the global financial crisis. It is to be noticed that the policy has not eliminated the motherhood wage penalty. The coefficient of the DDD are positive, but not statistically significant. To sum up, our study supports the hypothesis that reduced prices of child care had a positive impact on the labour outcomes of mothers, as measured by their earnings.

Our contribution to the existing literature is fourfold. First, ours is one of the rare studies to look at earnings outcomes in the context of a childcare reform. Previous empirical studies on childcare policies have focused mainly on labour force participation. Therefore, we contribute by adding solid evidence to the scarce body of literature focusing on childcare policies, motherhood penalty and earnings outcomes. Second, our study is the first to look at the impact of the 20-hour ECE reform in New Zealand. Considering the magnitude of the reform, it appears important to understand its benefits. Third, another contribution of our paper is that it highlights the importance of reduced childcare costs in increasing maternal earnings, and therefore reducing income inequality for women. It is widely accepted that childcare policies help mothers balance care and work, but it is less clear how much it can reduce the motherhood penalty.

[3] Further research looking at mother of multiple children is currently in progress, and should complete these findings

The study of a very recent reform in a developed country, makes our results particularly relevant for other developed countries looking at expanding their childcare support. Fourth, our rich longitudinal data allow us to perform a more comprehensive and reliable analysis. Many studies have used cross-sectional data, which can generate compositional variations through time in the groups, and bias the estimates (D. J. Anderson, Binder, & Krause, 2002; Gangl & Ziefle, 2009). Another advantage of our data is that we have access to socioeconomic information. This allows us to apply an improved empirical design to assess the policy change, namely a multi-period assessment and a matched sampling. To date, such methods have been rarely used in policy evaluation, mostly by lack of extended and rich datasets.

The structure of the remainder of this study is as follows. Chapter One reviews the motherhood penalty and its relationships to labour market structures. Moreover, it presents a relevant body of literature examining the effects of childcare policies on maternal outcomes. The Chapter Two examines the key facts regarding the childcare sector and its historical context in New Zealand, and the implementation of the 20 hours ECE reform. Chapter Three and Four present respectively the empirical methodology, and the data. The empirical findings are the subject of Chapter Five, followed by a discussion in Chapter Six. In the concluding chapter, we identify some suggestions for future research in this area.

Chapter 1

Literature Review: Motherhood Penalty and Childcare Policies

The aim of this research is to build an integrated and more nuanced understanding of the relationship between childcare policies and the earnings consequences inflicted by motherhood. First, we present the literature on the motherhood penalty, and the effects of work–family policies on labour force outcomes. Interestingly, these two sets of literature are quite separate and distinct, and only have little overlap. As our focus is on the latter, but does cover the former too, our study contributes to the scant literature that overlaps the two.

1.1 Motherhood penalty: How having children affects women's labour force outcomes

It has now been well established by researchers that the decision for women to have children is costly in terms of their labour outcomes. This is commonly invoked as the motherhood penalty (D. J. Anderson, Binder, & Krause, 2003; Budig & England, 2001; Harkness & Waldfogel, 1999; Waldfogel, 1997, 1998). The motherhood penalty refers to systematic disadvantages encountered by working mothers in their labour outcomes relative to childless women. More specifically, mothers may suffer hiring, wage or job experience penalties resulting in a labour outcome gap between mothers and non-mothers. Having to carry out the responsibilities of childbearing, and still most of the caring of the children, women face a heavy economic cost of doing so.

The motherhood penalty has been studied extensively in many developed countries. Viitanen (2012) presents a comprehensive summary of the findings on the motherhood wage gap. Interestingly, the gap extents from 0 percent for Denmark up to 25 percent for the United Kingdom and United States, with many estimates between these two bounds (see Table 1 in Vittanen, 2012). In

New Zealand, the motherhood wage gap was evaluated at 12 percent in 2016 (Statistics New Zealand & Ministry for Women, 2016). Differences in the country-level factors, but also in the empirical strategy used in the research generate this extensive range of estimates. Both individual and policy factors shape the extent to which motherhood impacts women's outcomes. These factors have been extensively studied in recent years (D. J. Anderson et al., 2003; Budig & England, 2001; Budig et al., 2012; Gangl & Ziefle, 2009; Waldfogel, 1997).

1.1.1 Individual-level Factors

First, some of the competitive disadvantage associated with motherhood can be explained by individual-level factors. The observable heterogeneity amongst women is a strong indicator, and includes the level of education, marital status, and work experience. The literature on the motherhood penalty has also shown that individual differences amongst women partially account for the penalty through several competing mechanisms. Gangl and Ziefle (2009) described three of these mechanisms: human capital depreciation, compensating wage differentials related to mothers' choices, and other mechanisms like employer discrimination.

Human capital profoundly shapes the motherhood penalty. It is cited by many as the most important cause, explaining one third to over one half of the motherhood earnings penalty (Budig & England, 2001; Gough & Noonan, 2013; Meyers et al., 2002). Going back to Becker (1993), human capital theory explains that market wages reflect individual productivity, which is determined by the accumulation of skills through formal education and experience (Gangl & Ziefle, 2009). Leaving the workforce for any period of time likely results in human capital depreciation, and moreover, in a loss of further human capital investment. Hence, it is not surprising that mothers who interrupt their careers for childbearing reasons will be disadvantaged for accumulating a smaller stock of human capital than employed childless women. In consequence, their wage will fall; or rather fail to rise further.

The loss of experience due to employment breaks is the first factor, yet further wage losses are associated to the mothers' labour market choices and behaviour. Although mothers nowadays return back to work quickly, they still carry out a larger share of children caring duties. In seeking to accommodate both the role of worker and mother, women have shown to put aside their personal career goals in favour of their family. They are more inclined to reduce work hours, shift to occupations and industries offering more flexible or secure work conditions, change for family-friendly employers, choose part-time employment and even pass up promotions (D. J. Anderson et al., 2003; Baum, 2002; Felfe, 2012; Gangl & Ziefle, 2009). These labour market choices and behaviour illustrate how mothers trade between wages and mother-friendly jobs that pay less.

Finally, a number of other mechanisms caused by motherhood might impact the wage penalty. Differences in unobserved characteristics of mothers such as motivation on the job might explain part of the gap. Researchers have also argued that mothers face workplace discrimination from their employers in hiring, training, or remuneration decisions (Budig et al., 2012, 2016; Gangl & Ziefle, 2009; Gough & Noonan, 2013; Hegewisch & Gornick, 2011). While motherhood may have little direct effect on productivity, employers may regard their competences or commitment to the job in less favourable terms (Budig & England, 2001; Gough & Noonan, 2013; Misra, Budig, & Boeckmann, 2011).

1.1.2 Policies and Institutional Factors

Moreover, women's economic responses to motherhood depends on the institutional context in which the labour market structure is embedded. Family-friendly policies aiming at connecting families and markets can take the form of childcare, parental leave, or taxation policies. While there is no doubt amongst researchers that family-oriented policies offer greater opportunities for mothers to join the labour market, they disagree on the impact of these policies on the occupational status of mothers, and on the motherhood gap. On the one hand, many studies show that such policies help mothers to balance their family, and therefore increase employment and wages (Hegewisch & Gornick, 2011). On the other hand, some policies seem to have a detrimental effect on mothers' outcomes, especially policies that lead to extended periods out of the labour force like long periods of maternity leave (Mandel & Semyonov, 2005; Pettit & Hook, 2005). This debate has certainly helped to shape better differentiation amongst policies. Hence, policies that keep women attached to the labour market, like moderate length leave and subsidized childcare, are the most effective in diminishing the motherhood wage penalty (Budig et al., 2012, 2016; Gangl & Ziefle, 2009; Hegewisch & Gornick, 2011; Misra et al., 2011; Olivetti & Petrongolo, 2017).

1.2 Childcare Policies

In this study, we focus on the impact of *childcare policies*. These can take the form of direct and indirect subsidies, as well as publicly provided or universal childcare. The effect of children on the employment decision of mothers is strongly influenced by the need for childcare during working hours. The trade-off between costs associated to childcare and the wage earned working appears therefore as an important aspect behind that decision. As childcare policies reduce the financial burden of childcare costs, mothers experience a direct increase in earnings net of childcare and their opportunity cost of employment is reduced (Budig et al., 2016). As a result, this leads to more continuous employment. The duration of career interruption is reduced, as is the loss of human capital (P. M. Anderson & Levine, 1999; Bainbridge et al., 2003a; Baum, 2002; D. M. Blau & Robins, 1988; Meyer & Rosenbaum, 2001; Meyers et al., 2002). Also, on a more cultural aspect, childcare policies indicate the social acceptability of non-parental childcare, and ease the choice of going back to work for mothers (McLachlan, 2011; Pettit & Hook, 2005).

Looking at the differences in women's outcomes between countries can give a comprehensive overview of the impact that childcare support has on a maternal outcomes, especially since the extent and nature of childcare support vary widely across countries (Misra et al., 2011; Olivetti & Petrongolo, 2017; Pettit & Hook, 2005). Pettit and Hook (2005) analyze data from 19 countries, and find that women in countries with high levels of childcare (Belgium, Sweden, Denmark) have higher labour participation, while countries with women in countries with low childcare (Czech Republic) have lower participation. Moreover, their study disentangles this effect by the age of the children. Childcare policies that are directed at children aged 0 to 3 (4 to 6) years-old explains around one fifth (one fourth) of the variance on women's employment. Similar evidence from Hegewisch and Gornick (2011) show that in countries with poor childcare support, women are less likely to work, and more likely to have low-quality jobs, low wages and higher employment turnover. In view of the beneficial outcomes for mothers, researchers have taken interest in evaluating and measuring the impacts of such policies at a country-level. This is our next focus.

1.2.1 Non-Experimental Studies

When trying to understand the effects of childcare policies on women's labour market outcomes, the first thing to understand is how the cost of childcare influences a mother's work decision. Most of the early literature on childcare policies have focused exclusively on the individual response to cost in a non-experimental setting (D. J. Anderson et al., 2002, 2003; Baum, 2002; D. Blau & Currie, 2004; Cleveland, Gunderson, & Hyatt, 1996; Han & Waldfogel, 2001; Kesting & Fargher, 2008; Kimmel, 1998; Lundin, Mörk, & Öckert, 2008; Powell, 1997; Wrohlich, 2004). The price-elasticity found in these studies highlights how the price of childcare affects the decision of mothers to engage in work. More specifically, it shows how the percentage change of the labour participation of mothers to a one percentage increase in the price of childcare. The range of estimates in the literature is wide, extending from -0.02 (Wrohlich, 2004) to values around -0.92 (Kimmel, 1998), while lots of mid-range estimates are found between these extremes (P. M. Anderson & Levine, 1999; Baker et al., 2008; Baum, 2002; D. M. Blau & Robins, 1988; Cleveland et al., 1996; Han & Waldfogel, 2001; Powell, 1997). While these studies offer some insight on how a mother's decision is affected by the price of childcare, the wide range of results and the non-experimental design of these studies have raised some questions as to their reliability. Thus, a new wave of studies in the field has adopted a quasi-experimental design, exploring more specific policy changes or reforms, and using it as an exogenous variation in the price of childcare.

1.2.2 Quasi-Experimental Studies

This new growing body of empirical studies, to which our paper relates, applies quasi-experimental identification strategies. They exploit exogenous policy change to uncover the causal effects of childcare prices on maternal employment, and most of them use a difference-in differences (DD) approach. They provide evidence that childcare reforms are favourable to maternal labour outcomes. However, very few studies have yet analyzed the effects of childcare policies on women's earnings, and how it reduces the motherhood penalty. We still present studies analyzing the labour participation outcome because the context of the childcare reform and the application of the methodology still provide us beneficial insight for our research.

While most developed countries now have some form of childcare support, the path to such policies varies extensively across countries both in magnitude, timing and political motivation. The United States (US) has historically been considered a latecomer in terms of work-family policy development (Bainbridge et al., 2003a). As of today, childcare support offered in the US is strongly targeted toward disadvantaged groups as single or low-income mothers.

The creation of the Child Care and Development Fund (CCDF) in 1996 in the United States resulted in childcare subsidies for low-income working parents all around the country. Zanoni and Weinberger (2015) look specifically at the effects of that reform on employment status and earnings of low-income mothers in the state of Illinois. They use data for 8 quarters before and after the receipt of subsidies in 2000, and find that the childcare subsidy program increases earnings by approximately 25 percent in the first year. Yet, the effect fades out in the second year of receiving the subsidy. The authors point out that these results show a short-time effect that is mainly influenced by the variations at the extensive margin of labour supply. In fact, many mothers shifted from no earnings to some earnings.

Michalopoulos, Lundquist, and Castells (2010) also study low-income mothers in Illinois during 2005 and 2006 using a random assignment design. They sample mothers with incomes slightly above the eligibility limit for childcare subsidies, and randomly assign them to a treatment and a control groups, which determined if they receive subsidies. The aim of such a research design is to determinate if childcare subsidies should be extended above the current state's eligibility limit. The authors looked at multiple outcomes, including employment, earnings, and the enrollment in childcare services. Interestingly, the results show no significant effects of childcare subsidies on the earnings of mothers. They explained that the lack of effect could be that the sample of mothers were steadily employed both before and after they entered the study.

Cascio (2009) exploits variations across geographical areas over time in the US. She studies more specifically the provision of universal kindergarten for five-year-olds in the mid-1960s. Her DD analysis finds large effects on the labour force participation of single mothers whose youngest child was five, whereas effects for married mothers were insignificant. Gelbach (2002) also studied the provision of kindergarten for five-year-olds. He uses quarter of birth as an instrument for public kindergarten enrollment of five-year-olds. He finds that married mothers with children in public kindergarten were more likely to be working. However, the effects tend to be smaller than the ones found by Cascio. It is to be noticed that Cascio (2006) and Gelbach (2002) consider only five-year-olds, therefore it is not possible to infer the impact of policies for children at younger ages. Other interesting studies on the US, including Bainbridge (2003), Mckernan (2000), Berger (1993), Gerard (2002) and Meyer (2002), who all found that childcare policies had positive effects on the maternal labour supply.

The relatively limited childcare support found in the US contrasts with the generous support found in Canada. The "5$ per day childcare" reform in the Canadian province of Quebec has been extensively studied since its introduction in the late 1990s. Lefebvre and Merrigan (2008) use a difference-in-differences (DD) approach to evaluate the impact of this universal reform on maternal labour participation. Their treatment group is composed of mothers of preschool children in Quebec, and their control group is composed of mothers in the rest of Canada with children the same age. They find that the reform stimulated the female labour supply substantially in Quebec, with an increase of 7.6 percent. Moreover, Lefebvre and Merrigan (2005) find an increase of approximately 2,300$ CAD in the yearly earnings of eligible mothers (around 190$ per month).[4] Baker et al. (2005) analyze the same reform, and their results confirm the ones found by Lefebvre and Merrigan (2008). More studies also looked at the long-term effect of this reform, Lefebvre, Merrigan and Verstraete (2009) as well as Haeck, Lefebvre and Merrigan (2015) confirm that the positive effect on mothers′ outcomes lasts in the long-run.

Bauernschuster and Schlotter (2015) investigate the German public childcare reform introduced in 1996, which provided children from age three until school entry age with highly subsidized half-day public childcare. Using both an instrumental variable model and a DD model, Bauernschuster and Schlotter (2015) obtain positive effects of public childcare on maternal employment, more specifically mothers' employment increases by 6 percentage points.

[4] Both specification (i) and (ii) reveal effects that are close in size. The specification (i) shows a significant effect, but the null of no pre-policy trends is close to being rejected. While the specification (ii) show non-significant effect, but null is not rejected (Lefebvre and Merrigan, 2005).

The childcare subsidy reform introduced in 2004 in France is investigated by Givord and Marbot (2015). Their results show significant increases in the rate of female labour market participation and in the average annual earnings of mothers, but both effects are small. In fact, the reform increased the average earnings of mothers by around 100 euros per year, less than 1percent.[5] Givord and Marbot (2005) highlight the fact that the effects vary depending on family size, with mothers with more children benefiting more.

Scandinavian countries are among the firsts to introduce extremely low childcare prices. Wikstrom, Kotyrlo and Hanes study the 2001-2002 childcare reform in Sweden, which restricted the costs of childcare between 1 to 3 percent of the family income, and established universal preschool childcare for child aged 4 and 5 years old. The research focused on the labour force participation and earnings of both native Swedish and immigrant mothers. The analysis is based on cross-sectional data for the period 1995-2009, and two groups, mothers of children aged 2 to 5 years-old and mothers of children aged 7 to 10 years old. The DD results establish that the reform had substantial effects on earnings and employment of mothers with children aged 2 to 5 years-old, with an increase of respectively 17.8 percent and 2.9 percent. Interestingly, the reform did not improve the earnings or labour force of immigrant mothers.

The Norwegian reform, which led to a large-scale expansion of subsidized childcare for 3 to 6 year-old children in 1975, is examined by Havnes and Mogstad (2011). Interestingly, little impact on maternal employment is found. Their results reveal that the subsidy mostly crowded out informal childcare services. More recently, Hardoy and Schone (2015) investigate a reform implemented in 2003 in Norway putting a cap on the price of childcare. Their estimates determine that reduced prices of childcare have a positive impact on the labour supply of mothers, with a small effect of approximately 5 percent. It is to be noticed that Scandinavian reforms are carried out in very high maternal employment context. Therefore, it is normal to expect that further reductions in the price of childcare seems to increase employment rates less.

This literature establishes the positive impact of childcare reforms on mothers' labour market outcomes, but also highlights the importance of the context in which the reforms are carried out. Confirming expectations, researchers have discovered relatively smaller effects of childcare policies in countries with already low childcare costs or high maternal employment at baseline (e.g. Sweden), in contrast to countries where the variation generated by the reform was more important like Canada.

[5] These results were found using an average yearly earning of 12 000 euros, and restricting to only working mothers.

A direct comparison between these studies proves to be challenging, as empirical strategies and sample compositions differ in a number of ways. One potential limitation in some studies is related to the difference in pre-reform trends between the treatment and comparison groups. The estimated effect of the reform would therefore represent the difference in time trends rather than the real policy effect (Lefebvre & Merrigan, 2008). Another potential problem is that there might be other economic shocks or policy changes occurring at the same time as this policy, confounding the estimated effect (Baker et al., 2008). Finally, many studies use cross-sectional data, which might generate compositional variations through time in the groups, and biases the estimates (D. J. Anderson et al., 2002; Gangl & Ziefle, 2009).

Even though these studies were performed in different institutional contexts and environments than New Zealand, they give interesting insight on the reaction of mothers' outcomes to childcare reform. Examining this body of literature, we believe that a childcare reform should have a positive impact on mothers' earnings, and moreover, contribute to decreasing the motherhood gap. In the next chapter, we examine the evolution of childcare policies in New Zealand and the implementation of the 20 Hours ECE reform.

Chapter 2

The evolution of childcare policies in New Zealand

In the majority of developed countries, the evolution of women's participation in the workforce has paralleled the growing need for childcare systems. Triggered by the rising issue of work and family reconciliation, many governments have placed childcare support in their political agendas. During the second half of the twentieth century in New Zealand, the sector underwent dramatic transformations reflecting shifts in the political, educational and social opinions regarding the best way to support young children and their parents. Helen May (2002), a renowned researcher on ECE in New Zealand, describes the development of the sector as a shifting debate from 'social progress' in the mid-century, to an 'economic value' perspective in the 1990s, and now to the valuing of children as a citizen in the 2000s. (Everiss, Hill, & Meade, 2017). New Zealand has maintained a strong commitment to universalized ECE throughout the country. In 2007, the government undertook a major policy reform to achieve this goal, namely the 20 Hours ECE reform. It resulted in major transformations in the landscape of the ECE sector, as the funding changed from a partial subsidy to a full coverage of the cost of care for up to 20 Hours a week (Bushouse, 2008). The following chapter will introduce the ECE sector in New Zealand. We will look at how its historical and societal influences have led to the transformation of childhood care, and to the implementation of the 20 Hours ECE reform.

2.1 The ECE Sector in New Zealand

In New Zealand, ECE is available for children from birth to school entry age, which is on or near their 5^{th} birthday (Meade & Podmore, 2002). As of today, attendance levels in ECE services continues to increase for all ages, and 96.2 percent of children starting school had attended ECE (Ministry of Education, 2015b). While there were just over 2,000 children enrolled in fortynine kindergartens in 1944, today there are 198,887 children enrolled in 5,272 early childhood services (Ministry of Education, 2015a).

The sector is characterized by different types of ECE services. These can be distinguished on the nature of their ownership, their parental involvement and their structural organization (Education Review Office, 2016). In terms of ownership, the government does not provide ECE services directly (Mitchell, 2015). Instead, the provision of childcare is assured either by private or community-based providers. Also, services are grouped into two main models based on the parental involvement: teacher-led services and parent-led services. The main difference is that in teacher-led services the children's education and care are assured by paid staff with a requirement for at least 50 percent of the staff to be registered as ECE teachers (Ministry of Education, 2016). Teacher-led services include kindergartens, home-based services and education and care centres.[6] On the other hand, in parent-led services the role of educators or teacher is assured directly by the parents of the children. The centers may operate on various schedules, either session, school day or full day depending of the demand within the community. The government has the regulatory role of setting the regulations surrounding the centers, and to provide subsidies based on enrollment.

2.2 The History of Childcare Policies

ECE in New Zealand has a history over 120 years. Emerging from a range of social, economic, political and cultural perspectives, the ECE sector has adapted to the changing needs of children, parents and communities. The first regulations surrounding childcare were implemented in 1960. Since then, ECE have seen its importance rise, and significant reforms were achieved to encourage the expansion of ECE services. While lots of changes have happened through the years, the most significant transformations in this sector took place during the second half of the twentieth century (May, 2002). More interestingly, there were two major waves of policy shifts: the 1988 Before Five Report and the 2002 ECE Strategic plan. As Bushouse remarked, "both of these policy waves were important in creating the policy environment that led to the creation of the 20 Hours Free Program" (Bushouse, 2008).

During the first half of the twentieth century, women were encouraged to assume their responsibilities of mothers 'at home' (May, 2002). This ideal was reinforced by social policies and public perceptions that children were better looked after by their mother. The perceived role of women slowly changed during the 1960s and 1970s as more women entered the workforce. The

[6] Education and care centres, home-based services and kindergartens offer their services to children from birth to school starting age. However, only some kindergartens cater babies and toddlers. (Mitchell, 2015)

considerable increase in the number of parents calling for more available and affordable childcare services was the first step for the government to start acknowledging the need for regulatory responsibilities (Meade & Podmore, 2002).

The mid-80s came with a major overhaul of the childcare sector. In fact, in 1986, the government integrated all ECE services under the Ministry of Education (Meade & Podmore, 2002).[7] Under that reorganization, both childcare and education were funded by the Ministry of Education. As McLachlan notes: "this was a significant and important development, providing the financial and regulatory framework that enabled the establishment of an early childhood sector in New Zealand" (McLachlan, 2011). It also placed New Zealand as the second country in the world to integrate all its ECE services under an education administration (Mitchell, 2015). One of the first major policy waves in ECE happened in 1988 with the Before Five Report. The entire education system was reviewed by the government, and recommendations for a new administration structure and funding framework were made. The Before Five reforms created a momentum for policy changes and raised the confidence that equity issues could be solved. The government introduced bulk funding for ECE services based on a universal hourly rate of subsidy (Bushouse, 2008). Every parent was now benefiting of up to 30 hours-a-week subsidy per child, set at 2.25$ per hour for children over two. McLachland characterizes the results of these mid-80s reforms as a time of unification at a policy level for the sector (McLachlan, 2011).

The next wave of transformations happened in the early 2000s with the transition from a National-led to a Labour-led government. The new government introduced a policy of "equity funding"(May, 2002), with the goal of "closing the gap" by giving access to quality ECE to every child (Mitchell, 2015). This translated into a 10 year-strategic plan for the early childhood entitled *Pathways to the Future 2002-2012* (May, 2008). The plan had three stated main goals: increasing the participation, improving the quality, and promoting collaborative relationships between ECE, schools and families (McLachlan, 2011).

In 2004, the government announced its plan to introduce in 2007 the 20-Hour-free early childhood education for all three-and four-year-olds in community-based, and teacher-led services. This policy is considered as the most important reform in New Zealand. It marked the passage from a partial subsidy to a full coverage of ECE costs for all parents. While the reform was positively viewed by society, protests arose amongst providers regarding the limitation of the

[7] Before 1986, the administration of ECE services was divided between the Departments of Education, Social Welfare, and Māori Affairs. (Meade & Podmore, 2002)

20 Hours ECE to community-based and teacher-led providers. In the heat of the upcoming election of 2005, these exclusions were heavily challenged (Bushouse, 2009). As a result, the government was pressured to modify the eligibility to all teacher-led providers, including private providers (Bushouse, 2008). This policy marked the beginning of a new era of public funding for the ECE sector in New Zealand.

2.3 The 20 Hours ECE Reform

> *"I have had the view for a long time the best investment a country can make is in its early childhood education system."*
>
> - Trevor Mallard, Minister of Education 1999-2004 (Bushouse, 2008)

While the ECE sector has evolved a lot since the beginning of the century, the 20-Hours ECE is definitely the latest major change in ECE policies. It marks the beginning of a new period of accessibility and affordability for all families. Implemented country-wide in July 2007, the 20 Hours policy funds up to 6 hours a day, and up to 20 Hours a week per child of free ECE services (May, 2002). The funding is available to all children aged three and four who attended an ECE offering the 20 Hours ECE. This policy comes as an addition to the partial subsidy of 30 hours a week of childcare service for children from birth to school age. Mitchell (2015) notes that it is not a "universal entitlement" since parents and centers have the choice to opt into the program. In fact, service providers can choose to participate or not in the program, which leaves the government with no control over the supply. The program reimburses the compulsory fees directly to the providers on the condition that they do not charge additional fees for the free hours. However, the centers are authorized to ask the parents for optional charges or for voluntary donations (Mitchell, 2015). These charges can take different forms, as an example, in 2015 most kindergartens had an average of $5-6 per hour optional charge (Ministry of Education, 2016a).

The proportion of ECE providers initially joining the 20 Hours ECE program in July 2007 was 62 percent, and increased to 76 percent six months after the implementation (May, 2008). Comparing the years surrounding the reform highlight the positive and beneficial impacts of the reform in the ECE sector. Table 1 shows prior to the reform there were 165,254 children enrolled in ECE service, and by 2010 that number increased to 188,924, denoting a growth of 14 percent. The public spending on ECE expanded massively, passing from $522 million in 2006 to $1.157 billion in 2010. More specifically, this represents an increase from $2,885 to $5,543 per-enrollment subsidy per child, which more than doubled. Over this period, the mean hours of childcare use per week went from 16.9 to 19 hours (Education counts - 2013).

Table 1. ECE enrollments and expenditures in New Zealand for 2006-2010

	2006	2007	2008	2009	2010
Enrollments	165,254	171,1381	176,993	180,910	188,924
Expenditures (in millions dollars)	574	632	837	1,047	1,157
Subsidy per enrollment (in dollars)	2,885	3,357	3,571	4,626	5,543

Notes: The data used in Table 1 are sourced from Education Counts from the Ministry of Education (2013). The dollars are in constant 2010 dollars.

Not only did the reform make childcare more accessible, it also lightened the financial burden of childcare costs for parents. Prior the reform, White (2006) reported that the typical fees for ECE service in Auckland would range between \$275 and \$474 a week. This represents approximately 13 to 14 percent of the average household income.[8] With the introduction of the 20 Hours ECE, it is estimated that household expenditures were reduced by about 34 percent, while the affordability of ECE increased by about 37 percent (Ministry 2014a).

[8] Based on the average household income of the 2006 NZ Census

Chapter 3

Methodology

The goal of this research is to estimate the causal effect of the 20 Hours ECE reform on maternal earnings. We approach this objective by comparing the changes in earnings between mothers affected by the reform and mothers not affected by the reform based on the date of birth of the child. We follow a difference-in-differences approach, exploiting the change in eligibility to the program, by year of childbirth. We first describe our main empirical strategy, as well as the multi-period specification. We then discuss alternative specifications used to test the robustness of our findings. More specifically, we explain our DDD approach with two periods, followed by the DDD with multiple periods.

3.1 Difference-in-Differences

Our econometric approach is based on a difference-in-differences (DD) framework, which is a well-established policy evaluation approach in the labour economics literature (Angrist & Krueger, 1999; Angrist & Pischke, 2009; Meyer & Rosenbaum, 2001). By using data before and after the policy implementation, for a group affected by the change (treatment group) and a group not affected by the change (control group), we can isolate the specific effect of that policy. Most studies exploiting a quasi-experiment have applied DD to identify the effects of childcare reform (Bainbridge, Meyers, & Waldfogel, 2003b; Baker et al., 2008; Bettendorf, Jongen, & Muller, 2015; Cascio, 2009; Francesconi & Van Der Klaauw, 2004; Givord & Marbot, 2015; Haeck, Lefebvre, & Merrigan, 2015; Hardoy & Schøne, 2015; Havnes & Mogstad, 2011; Lefebvre & Merrigan, 2005; Lundin et al., 2008; Nollenberger & Rodríguez-Planas, 2015).

The 20 Hours ECE reform is a nationwide program accessible to all parents with children aged 3 to 5-years-old. The program was implemented simultaneously across the country, and consequently we do not have a concurrent comparison group, like untreated states or regions[9]. Instead, we use the birthdate of the child to define the treatment and control group of mothers based on their eligibility to the program.[10] As the ECE program was implemented in July 2007, it means that children born as of July 2003 could benefit from the program. Therefore, we employ the month and year of birth of the children to define the group of mothers affected by the policy, where mothers whose child was born after July 2003 were eligible, and the mothers who gave birth before July 2003 were not eligible. We decide to use a two-year interval period to select the samples of mothers. For our treatment group, we select mothers who gave birth between July 2004 and June 2006. The control group is composed of mothers who gave birth between July 2000 and June 2002. Using mothers as controls assure that we are comparing similar individuals. Indeed, women with children experience similar trends in their working behavior and earnings, as will be shown in the next chapter. Therefore, our identification strategy which relies on the "parallel trend" assumption, would be respected as the two groups have a similar trend in the absence of treatment (Angrist & Pischke, 2009).

The fact that the groups are determined by different birth periods causes two issues. First, mothers experience a significant decrease in their earnings around the time of birth. Therefore, the dip in earnings for both groups does not happen at the same calendar time period, and causes the trends to differ significantly between the groups and within each group. Secondly, earnings could develop differently between these two groups. Indeed, mothers in these two groups may be giving birth at different stages in their life and career, which can further confound our analysis. These two concerns would cause biased results for the effects of the reform on earnings. To overcome these challenges, we define t, the distance in months from childbirth, as the time reference instead of the calendar date. The month of childbirth is defined as t=0 for all mothers, and all our analysis will be referencing t, not calendar months. This allows us to align all mothers' income time series with childbirth as the reference point. In doing so, we can make sure that the income trend surrounding childbirth is similar across the two groups of mothers. The DD estimator of the ECE reform on mother's earnings can be defined as:

$$DD\ Estimator = E\left[Y_1 - Y_0|_{Eligible=1}\right] - E\left[Y_1 - Y_0|_{Eligible=0}\right] \tag{1}$$

Where E is the expectation operator, *Eligible* is a dichotomous dependent variable equal to 1 if the mothers gave birth between July 2004 and June 2006, and 0 if between July 2000 and June 2002. Y_1 represents the monthly earnings

[9] In the literature on DD, it is common to use untreated states or regions as a natural control group (Angrist & Pischke, 2009; Baker et al., 2008; Haeck et al., 2015; Nollenberger & Rodríguez-Planas, 2015).
[10] We do not have information directly on the participation of mothers to the program.

after birth, and Y_0 represents the earnings before birth. Equation (1) represents the difference between the change in earnings for eligible mothers and the change in earnings for the non-eligible mothers. That difference between the treatment and control group is the DD-estimate. The corresponding DD regression can be defined as:

$$Y_{ijg} = \beta_0 + \beta_1 Eligible_g + \beta_2 Post_j + \beta_3 (Eligible_g * Post_j) + \beta_4 X_{igt} + \gamma_t + \varepsilon_{ijgt} \quad (2)$$

Where *i* indexes mothers, *g* indexes group of mothers based on their eligibility, *t* indexes the month periods, *j* indexes the pre- or post-period, and X_{ijg} is a vector of controls for individual characteristics. In our specification, *X* encompasses relevant controls such as categorical variables for the mother's age, level of education and ethnicity. *Post* is a dummy variable equal to 1 for the period after the birth of the child, and 0 before. The monthly fixed effects are captured by γ_t which reflects the common trend between both the treatment and control groups over time. The parameter of interest here is β_3, which gives the average effect of the reform on the monthly earnings for eligible mothers after the birth event. Our pre-period is defined as the 21st to 12th months prior to the birth of the child, totalizing 10 months. It is to be noticed that t=-12 is chosen because a future mother would not know at t=-12 that she'll be pregnant, thus it minimizes the chance of our pre-period containing behavioral changes in anticipation of future childbirth. For our post period, we use two specifications. The first includes the 12th to 60th months after the birth event, and the second is from the 24th to 60th months after the birth event.[11]

The model (2) can be enriched by adding multiple pre- and post-treatment interaction periods, instead of only two periods (Bettendorf et al., 2015; Francesconi & Van Der Klaauw, 2004; Haeck et al., 2015). Such a model captures the gradual increase of the reform thought time. As mentioned in Section I, there has been a relatively long build-up to the reform, from when it was announced in 2004 to when it was put in place in July 2007. Due to the high mediatisation, behavior changes could have happened even before the implementation of the policy, or alternatively the response could have been delayed. For example, mothers could have looked more intensively for a job or kept their current job in the expectation of the future benefits from when her child turns 3 years-old. Also, it is important to capture the progressive enrollment in the program. Francesconi and Van der Klaauw (2004) discussed similar context of reform with long implementation[12], and proposed a multiple

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[11] We exclude the period from the 12th month before to the 12th month after birth from the analysis. In principle, that period could be included, which would increase the sample size. However, because of the high fluctuation and atypical movement of earnings during the period close to childbirth, we decide to exclude it.
[12] Their paper presents the impacts of the Working Families tax credit on lone mothers in Britain.

period model, taking into account anticipation and delayed effects. Here is the dynamic model including the DD effects for multiple months:

$$Y_{ijgt} = \propto + \theta Eligible_g + \sum_{l=m+1}^{t} \beta_t * (Eligible_g * Period_t * Post_j) + \gamma_t + \varphi X_{igt} + \varepsilon_{ijgt} \quad (3)$$

We regress earnings on a group fixed effect ($Eligible_g$), time fixed effect (γ_t), individual characteristics (X_{ijg}), and a set of treatment interaction dummies for each period after the reform. $Period_j$ is a cycle dummy variable indexing each period relative to childbirth, and *m* indexes the number of periods in the pre-period. The interaction dummies ($Eligible_g * Period_t * Post_j$) equals 1 if the mother is eligible and in the post birth period *j*. This model indexes the effect of the policy for each period. It allows the effect to be different over time, and therefore pick up the anticipation or delay responses to the policy change. We run the model using a monthly specification, as well as a 3-month average specification.

The DD model relies on the assumption that these two groups of mothers would have the same life-cycle income trends had there been no policy change. The DD approach controls for unobserved differences between mothers in different periods, as well as between mothers from treatment and control. However, this approach may not give us an unbiased estimation of the effect of that policy, if unobserved group factors are correlated with labor market trend at the time level. In our context, there could be a bias due to the difference in the time evolution of the outcome variable between control and treatment group. In other words, since our two groups are defined by different periods in time, there could be changes over time such as macroeconomic shocks that are not taken into account. For example, the global financial crisis could have affected the treatment group more than the control group. Also, this could result from the fact that the time reference is not calendar dates, but distance from childbirth. The presence of such time specific fluctuations might yield a biased DD estimate. To address the possibility of a selection bias due to time trends, we run a specification check based on a triple-differences.

3.2 Robustness Tests

To gain further confidence in our identification strategy, we use an alternative specification addressing compositional changes over time. We need to determine which level of earnings a mother would have achieved if she had not been eligible to the reform for the same specific observation period in calendar date than the mother eligible. Since this counterfactual outcome cannot be observed, we have to identify another control group of females who didn't benefit from the reform, which is comparable to our treatment group with respect to time of reference. To do so, we add non-mothers, which refers to women without children, as a second comparison group. The logic here is that if the childcare policy contributes to a change in the outcomes of mothers, that relationship should be seen only for eligible mothers, and childless women should absorb any time trend bias. By adding this additional group of comparison, it gives us a triple-differences (DDD) estimator. In fact, we exploit the variation created by the reform along three dimensions: (1) between eligible and non-eligible mothers; (2) between time periods before and after the birth; (3) between mother and non-mothers (Schøne, 2005). The aim of using this extra comparison group is to pick up the time-varying effects specific to the calendar date, and correct for the sources of bias mentioned above. This type of DDD approach has been used recently in the literature on childcare reform, especially in combination with the DD approach as an additional check for time effects (Bainbridge et al., 2003b; Cascio, 2009; Francesconi & Van Der Klaauw, 2004; Hardoy & Schøne, 2015; Havnes & Mogstad, 2011; Lefebvre & Merrigan, 2005; Nollenberger & Rodríguez-Planas, 2015).

3.3 Constructing the Non-Mothers Control Groups: Matched Sampling

The perfect counterpart for a mother would be a childless woman with the same observable characteristics, and the same unobservable characteristics. Such an ideal counterpart is almost impossible to find. In our context, forming a control group of non-mothers by random sampling would not be the optimal method, as many of the non-mothers are quite different from the mothers. Another important issue is that we are not able to identify the childbirth period, t=0, for the non-mothers. Matched sampling solves these problems by taking into account the covariates shared by mothers and non-mother, and by matching on a comparable calendar date. Therefore, the matched sampling approach allows us to construct a comparable control group based on covariates (Angrist, 1998; Rosenbaum, Ross, & Silber, 2007; Rosenbaum & Rubin, 1985; Rubin, 1973, 1979).

Matching aims to balance the distribution of covariates in the treated and control groups. Since the 1970s, work on matching methods has examined how to best choose treated and control subjects for comparison. Matching has gained popularity in many research fields and have been widely studied (Abadie & Imbens, 2006; Gu & Rosenbaum, 1993; Heckman, Ichimura, & Todd, 1997; Imbens, 2004; Meyer & Rosenbaum, 2001; Rosenbaum & Rubin, 1985; Rubin, 1973, 1979). More specifically, matched sampling is a method for selecting the control subject, usually from a larger group of potential controls. This allows us to choose control subjects that are more similar to the treated group with respect to the distribution of observed covariates. It is based on the assumption that the conditioning on attributes, X, eliminates the selective differences between treated and control. This method assumes that we have access to conditional variables sufficiently rich such that the counterfactual distribution of non-mothers would be the same as the observed distribution of non-mothers.

Finding matches for high-dimensional covariates with close or exact values was a challenge until the introduction of the Propensity score matching (PSM) method (Angrist, 1998; Rosenbaum & Rubin, 1985). The propensity score uses logistic regression to summarize all covariates into one scalar, which predict the probability of treatment. Therefore, PSM does not require close or exact matching on all variables, and is quite flexible to use. In recent literature PMS is the most popular matching approach, and has also been used in childcare policy studies (Beblo, Bendery, & Wolfz, 2009; Simonsen & Skipper, 2006). While PMS is an easy and fast method to remove selection bias based on observable characteristics, it is not always the most precise method (Stuart, 2010). Exact matching, where each variable X is matched to exactly the same value, remains in many ways the ideal method to match, but can only be applied when there is a small set of discrete variable. Rosenbaum and Rubin (1985b) highlight the fact that, if X is highly dimensional, using exact matching would only result in larger bias due to many individual not being matched.

We apply a hybrid approach to matching, where both exact matching and proximity score matches are used. Beblo, Bendery and Wolfz (2009) use exact matching on one key variable followed by PSM to match mothers to non-mothers. In a similar approach, Lundin, Mörk, and Öckert (2008) use exact matching on all discrete variables, and transform the continuous income variable into discrete intervals. The first step of this approach consists in finding all possible matches of non-mothers for each mother. A single mother is matched to all non-mothers that have the same characteristics, X, as the exact same month and year of birth, the same level of education and the same ethnicity. This step is conducted *with replacement,* so individuals from the non-mother list can be matched to more than one participant from the mother list.[13] The second step is then to calculate a proximity score for all possible matches using the monthly earnings for specific dates. These dates are determined based on the corresponding period from the 21st month to 12th month before the birth.[14] Our approach to generating this score involves calculating the average squared value of the difference between the earnings for each month (from t=-21 to -12), and for each potential matched non-mother. It is to be noted that we smooth the observed earnings using the 4-month moving average on earnings before matching. Individual income streams have many idiosyncratic shocks. Finding a twin that fits all these idiosyncratic shocks can be difficult. More importantly, these shocks are not what we want to match. We are more

[13] Using matching with replacement usually decreases bias due to the fact that control individual can be used multiple times to create a more precise match (Stuart, 2010).
[14] It is to be noted that closer than the 12th month before birth, the shape of the earning pattern might already be affected by the childbirth event, and therefore should not be taken into consideration.

interested in extrapolating the mother's income stream had she not had a child. Overfitting the data in the pre-period may hurt extrapolation in the post-period. Therefore the moving average allows us to smooth out idiosyncratic shocks. The proximity score is defined below, where M is a dummy taking the value 1 for mothers, and 0 for non-mothers.

$$Prox_i^2 = \begin{bmatrix} \left(Y_{t=-21,\ M=1} - Y_{t=-21,\ M=0}\right)^2 \\ +\left(Y_{t=-20,\ M=1} - Y_{t=-20,\ M=0}\right)^2 \\ + \cdots + \left(Y_{t=-12,\ M=1} - Y_{t=-12,\ M=0}\right)^2 \end{bmatrix} \tag{4}$$

Among the potential control candidates, the smallest proximity score defines the final control non-mother for each mother. To restrict the matches that tend to have larger difference in monthly earnings, we only keep matches in a 1,000$ interval of comparative earning. By using matched sampling, we are able to construct the non-mother groups based on the same observable characteristics as the mother groups. Because we match each mother to her similar non-mother, we are also able to match the calendar date, and therefore identify the t=0 for the non-mother. This way, we are able to generate the t variable for all non-mothers.

Two major assumptions most hold when comparisons are made between treated and control based on the similarity of their observed characteristics. The first key assumption is the Conditional Independence Assumption (CIA), also referred to confoundedness assumption, or ignorable treatment assignment (Imbens, 2004; Rosenbaum & Rubin, 1985). The CIA implies that the treatment assignment is independent of the potential outcome conditional on the covariates X as in equation 5. Following this assumption, the mothers would have had the same earnings than the non-mothers if they did not have a child. The second assumption is the common support assumption, which is tested in the results chapter. It is essentially the overlap of the comparison groups, and ensures that individuals with the same covariates values have a positive probability of being both treated and control (Heckman, Lalonde, & Smith, 1999).

$$E(Y_0 \mid M = 1, X) = E(Y_0 \mid M = 0, X) \tag{5}$$

Using the matched sampling method, we are able to match the eligible and non-eligible mothers with the most comparable non-mother, and we consequently end up with four groups: eligible mothers, non-eligible mothers, 'eligible' non-mothers and non-eligible non-mothers.[15]

[15] The eligible non-mothers are the controls for the eligible mothers, and are matched on the same calendar date than eligible mothers. Therefore, eligible non-mothers are in the *eligible* period of the reform even though they obviously did not used any childcare services.

3.4 DDD Regression

Adding a control group of non-mothers allows us to adjust for time effects and macroeconomic shocks related to calendar time as long as the aforementioned specific shocks affect mothers and non-mothers similarly. The identification assumption of this DDD is that, on average, the difference between the earnings of mothers and non-mothers would have changed similarly in the before and after period, in absence of the childbirth event. The DDD-estimator can be written as:

$$DDD\ Estimatior =$$
$$\begin{bmatrix} E\left(Y_1 - Y_0\ |_{Eligible=1,\ Mother=1}\right) \\ -E\left(Y_1 - Y_0\ |_{Eligible=0,\ Mother=1}\right) \end{bmatrix} - \begin{bmatrix} E\left(Y_1 - Y_0\ |_{Eligible=1,\ Mother=0}\right) \\ -E\left(Y_1 - Y_0\ |_{Eligible=0,\ Mother=0}\right) \end{bmatrix} (6)$$

We can see that the first square bracket represents the DD-estimate between eligible and non-eligible mothers. It is therefore the treatment group. First, $E\left(Y_1 - Y_0\ |_{Eligible=1,\ Mother=1}\right)$ measures the change in earnings before and after birth for eligible mothers. Similarly, $E\left(Y_1 - Y_0\ |_{Eligible=1,\ Mother=1}\right)$ measures change in earnings of non-eligible mothers. The second bracket presents the estimate for the DD for the corresponding groups of non-mothers, where $E\left(Y_1 - Y_0\ |_{Eligible=1,\ Mother=0}\right)$ measures the change in earnings of eligible non-mothers, and $E\left(Y_1 - Y_0\ |_{Eligible=0,\ Mother=0}\right)$ measures the change in earnings of non-eligible non-mothers. The difference between these two is the DD-estimate for the control group. Taking the difference between the two DD-estimates gives us the DDD-estimate of the effects of the childcare reform. The corresponding DDD regression estimated by OLS is expressed as

$$\begin{aligned} &Y_{ijgkt} = \propto_0 + \propto_1 Eligible_g + \propto_2 Mother_k + + \propto_3 Post_j \\ &+ \propto_4 (Eligible_g * Mother_k) + \propto_4 (Eligible_g * Post_j) + \propto_6 (Mother_k * \\ &Post_j) + \beta_1 \left(Eligible_g * Mother_k * Post_j\right) + \gamma_t + \varphi X_{igt} + \varepsilon_{ijgt} \qquad (7) \end{aligned}$$

The parameter of interest lies in β_1, which gives the interaction between $\left(Eligible_g * Mother_k * Post_j\right)$. That coefficient measures all changes in earnings for the eligible group relative the non-eligible group, for mothers relative to non-mothers, and between the periods before and after childbirth. In other words, it is the effect of the ECE reform on eligible mothers. In the absence of treatment, β_1 should be equal to zero. This assures that there is no correlation between the error term measuring unobservable individual-transitory shocks and the variables measuring the effect of the reform.

Specification (7) is enriched by adding multiple pre- and post-treatment interaction periods, as we do with the DD regression:

$$Y_{ijgkt} = \propto_0 + \propto_1 Eligible_g + \propto_2 Mother_k + \propto_3 (Eligible_g \cdot Mother_k) + \propto_4 (Eligible_g * Post_j) + \propto_5 (Mother_k * Post_j) + \sum_{l=m+1}^{t} \beta_t (Eligible_g \cdot Mother_k \cdot Period_t \cdot Post_j) + \gamma_t + \varphi X_{igt} + \varepsilon_{ijgt} \quad (8)$$

An important issue to address with both the DD and DDD models is the correct computation of the standard errors that account for both within group-period correlation across observation, and correlation within individuals across time. As mentioned by Angrist and Pischke (2009), the error term resulting from the DD approach reflects the idiosyncratic variation in potential outcomes across individual, eligible groups, and time. The trouble resides in the fact that there can be potential common variation between individual in the same group and period in time. Therefore, to account for this correlation, we correct the error term ε_{igt} by clustering it on the individual level, allowing for dependence in the error terms (Havnes & Mogstad, 2011).[16]

[16] The presence of numerous clusters in our data can cause bias because it tends to underestimate the serial correlation in random shock or the correlation between clusters (Angrist & Pischke, 2009).

Chapter 4

Data

4.1 Data Description

The data used in this study is provided by Statistics New Zealand's Integrated Data Infrastructure (IDI). The IDI is a large research database that links various administrative records in New Zealand at an individual level. The fundamental spine of the IDI consists of a small number of core datasets that are connected to many other datasets using a unique key identifier. That identifier preserves the identity of the individual, and links the same individuals throughout the system.

To achieve our research objective, we use a number of different datasets from the IDI. As a starting point, we use the birth registration dataset (DIA), which contains information about all births in New Zealand, as well as all children adopted in New Zealand. Then, we can link information on the mother's monthly earnings, which constitutes the dependent variable in this study, to the DIA dataset. The earnings data comes from the Statistics New Zealand's Inland Revenue dataset (IR). The IR data contains a comprehensive longitudinal record covering the period from April 1999 to December 2016. It records the Employer Monthly Schedule (EMS) filed by the employers with Inland Revenue. The EMS details on a monthly basis all the employees working for a specific employer, the earnings they received, and the amount of tax that is deducted from their pay. The IR data infrastructure also captures various sources of governmental non-employment income, as unemployment benefits, student allowances, pension welfare benefits, etc. It should be noted that individuals included in this database are employees who pay their income tax at the source. Therefore, working owners, self-employed, and unpaid workers are excluded.

While the IR dataset provides a rich source of information on the labour market in New Zealand, it also has its weaknesses. One significant shortcoming is that the data is collected on a monthly basis. Therefore, if an employment starts or finishes mid-month, it is impossible to know exactly how many days are worked.

The second weakness is the lack of information on the number of hours worked, which makes it difficult to distinguish changes in hourly wages from changes in hours worked. In this context, we consider that an individual is employed for any calendar month if any earnings are received that month.

In this research, we also use non-mothers as a control group. To obtain information on those non-mothers, we combine the DIA dataset and the Census 2013. The Census enables us to identify all women in NZ, and then by linking it to the DIA we exclude all women who ever gave birth.[17] Therefore, we are able to identify all the female non-parents. Finally, we link these non-mothers to the IR tax data in the same way detailed above.

Also, we include a set of socioeconomic controls for observed characteristics using a link to the Census, such as age, educational attainment, and ethnicity.[18] The access to socioeconomic data represents a valuable added value to our study, as usually administrative data is very poor in socioeconomic information. Education is a categorical variable with four values: no education, high school graduate, university graduate and postgraduate education.[19] The ethnicity control variable is based on the prioritized ethnicity, which is the first ethnicity an individual identifies himself to. Ethnicity is categorized by five dummy variables: European, Māori, Pacific, Asian and other.[20]

4.2 Study Population

To define our sample, we start with the entire population of New Zealand mothers who gave birth between July 2000 and June 2002 (control group), and between July 2004 and June 2006 (treatment group). We extract the identifier of the mothers who gave birth between these periods, and then extract the information on all the children born from these mothers. This step allows us to have a list of all mothers who gave birth during these two periods, as well as a list of all the children of these mothers through time.

From this subset of 198,822 mothers, we eliminate all mothers who gave birth to a stillborn, as well as those with missing values for the socioeconomic control variables. We restrict the analyses to mothers who gave birth to *one child* in the periods selected above, which represent 21.3 percent of the sample or 36,744 mothers.[21] From these, only 27,642 mothers ever worked. Since we

[17] Or adopt a child
[18] The marital status could have been added but due to the panel structure of the data, it could have created more bias due to the changes in marital status (declared or undeclared) through time.
[19] The educational attainment is based on the Census 2013 Dataset
[20] The population is 75% from European descent, 16% Māori, 8% Pacific, 12% Asian and 1% other. (New Zealand Treasury, 2016). The Māori the first indigenous Polynesian people to reach New Zealand, followed by the early European settlers. New Zealand has a democratic parliamentary government, and is part of the Commonwealth.
[21] The analysis of mothers of two children and more children will be the subject of a whole different project as the earnings trends are quite different.

exploit the variation in earnings before and after the reform, we only include mothers that have observations in both the pre- and post-period of childbirth. Furthermore, we restrict the sample to mothers who gave birth or adopted a child between 20 and 55 years old to eliminate extremes (Bettendorf et al., 2015; Hardoy & Schøne, 2015; Nollenberger & Rodríguez-Planas, 2015; Schøne, 2005). After these limitations, our final sample consists of 14,589 mothers.

Also, to be able to apply our sampled matching method, we restrict further the sample to mothers who worked consecutively for the period ranging from the 21st to 12th month before birth.[22] As explained in Section 2, we do this to be able to calculate a proximity score for all possible matches using the monthly earnings from the 21st month to 12th month before the birth.[23] This restriction gives us a sample of 9690 mothers for the matching and DDD regressions.

For the non-mothers, we apply a similar process. We first extract all women in NZ from the Census 2013. Then, we link it the DIA database, and only keep the childless women. After linking this sample of non-mothers to the IR tax data, we are left with 319,461 potential non-mothers who ever worked.

The earnings included in the sample are only the ones coming from wages and salaries, excluding all incomes from benefits, parental paid leave, etc. as these are provided by the government. We also sum all the earnings received by an individual for the same month.[24] Finally, we normalize all the monthly earnings to constant June 2006 dollars, using the quarterly seasonally adjusted CPI series provided by Statistics NZ.[25] Therefore, we compare equivalent earnings between mothers. Finally, we decide to eliminate some of the more extreme observations. All monthly earnings exceeding $15,000 are excluded from the sample. We notice that earnings exceeding this point are mostly irregular bonuses for high-paying jobs, and could therefore bias the estimate.

[22] We apply a rule to keep mothers who were out of the labor force for maximum of one month at a time during the period between the 21st to 12th month before birth.
[23] It is to be noted that closer than the 12th month before birth, the shape of the earning pattern might already be affected by the childbirth event, and therefore should not be taken into consideration.
[24] An individual can receive multiple earnings from difference sources during the same month. These multiple earnings are presented separately in the dataset.
[25] The Consumer Price Index (CPI) is a measure of the average change in prices over time across a set of goods and services bought by consumers.

Chapter 5

Results

We begin this chapter with the descriptive statistics of our data. Then, we present the DD analysis of changes in mean earnings over time. We improve our estimate by executing the multi-period DD model. Next, we do the specification check using the DDD approach with two periods, followed by the DDD with multiple periods. Throughout the research, we present estimates by levels of education, ethnicity and age of the mother at birth. Because of differences in constraints between these subgroups of women, the introduction of the 20 Hours policy might have had different effects on their employment decisions and outcomes.

5.1 Descriptive Statistics

Table 2 presents the descriptive statistics for our dependent variables, and the set of controls for mothers. The mothers are divided into two groups: the treatment group, consisting of mothers who gave birth between July 2000 and June 2002, and the control group, consisting of mothers who gave birth between July 2004 and June 2006. We take the mean values of all observations for the whole period. We can see that the two groups have fairly similar characteristics. Overall, the majority of mothers gave birth after 30 years of age in both groups. Also, their education level is high. About 90 percent of the mothers have at least a high-school degree, and more than a quarter have a university degree. We can notice that the mothers in the treatment group are slightly more educated. About 6 percent more mothers have a bachelor's degree in the treatment group, reflecting an improved access to education and growing trend for more educated women. The proportion of European mothers is the largest, followed by Asian. We also notice that there are more Asian in the treatment gr, passing from 8.1 percent to 13.5 percent respectively.[26]

[26] This reflects a trend in New Zealand population. Asian ethnic group was representing 6.6% of the population in the 2001 Census, and increased to 9.2% of the population in the 2006 Census. (Stats NZ, Census 2013)

Table 2. Descriptive statistics for the treatment and control groups

Variables	Definition	Mean	
		Treatment	Control
Monthly earnings	Usual monthly total earnings ($)	3187.07	2803.97
Age	Age in years	34.97	34.93
Age at birth			
20-24	Dummy variable: 1 = 20 to 24 years when given birth; 0 otherwise	10.96%	8.28%
25-29	Dummy variable: 1 = 25 to 29 years when given birth; 0 otherwise	16.97%	19.63%
30-34	Dummy variable: 1 = 30 to 34 years when given birth; 0 otherwise	32.73%	36.04%
35 & more	Dummy variable: 1 = 35 years and up when given birth; 0 otherwise	39.35%	36.05%
Education			
Lower than high school	Dummy variable: 1 = Lower secondary school qualification; 0 otherwise	7.89%	10.00%
High school graduate	Dummy variable: 1 = High school qualification (level 1-7 certificate or diploma); 0 otherwise	59.68%	63.43%
Bachelor graduate	Dummy variable: 1 = Bachelor's degree (including Honours); 0 otherwise	27.17%	21.75%
Postgraduate	Dummy variable: 1 = Postgraduate qualification; 0 otherwise	5.26%	4.82%
Ethnicity			
European	Dummy variable: 1 = European; 0 otherwise	73.48%	79.83%
Māori	Dummy variable: 1 = Māori; 0 otherwise	5.12%	5.39%
Pacific	Dummy variable: 1 = Pacific; 0 otherwise	5.43%	4.76%
Asian	Dummy variable: 1 = Asian; 0 otherwise	13.51%	8.14%
Other ethnicity	Dummy variable: 1 = Other ethnicity; 0 otherwise	2.46%	1.87%
N		6735	7854

Note: Shows the summary statistics for the mother of the treatment and control groups. All statistics are percentages except the average monthly earnings and age. The treatment group consists of mothers who gave birth between July 2000 and June 2002, and the control group of mothers who gave birth between July 2004 and June 2006.

Figure 1 shows average monthly earnings, tracing the time series evolution of the earnings from the 24th prior to birth to the 72nd month after birth for treatment and control groups. The drop of earnings happens approximately at the same period for both groups, therefore assuring a similar pattern between them. The dash lines identify the pre-period used in the analysis. It ranges from the 21st month through the 12th month prior to birth. We do not use the observation after the 12th month prior to birth, since changes in the earning pattern start happening due to the future birth event. Both the treatment and control groups exhibit a stable trend in the dashed pre-period, following by a dip and an upward recuperation period. It is also evident that eligible mothers have higher monthly earnings than the comparison group throughout the sampled period. They earn approximately $350 more per month in the pre-period, possibly due to the fact that the treatment group is slightly more educated than the control group. That gap then contracts by more than half around the time of birth, and reaches the lowest point at the 3rd month after birth. The graph clearly shows that from the 18th month the earnings of eligible mothers increases more rapidly, which eventually results in creating a bigger gap than in the pre-period. Also, the gap seems to narrow down after the 60th month. It is to be noticed that the gap is not caused by inflation since we use constant 2006 dollars. One likely cause might be the compositional differences between the two groups of mothers, perhaps the higher proportion of university graduates in the treatment group compare to the control group.

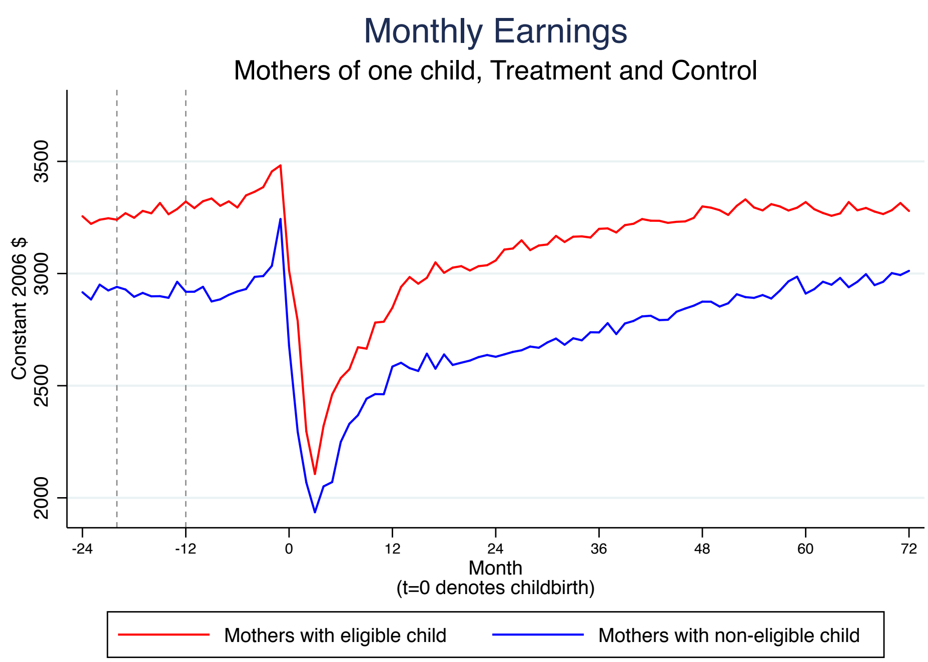

Figure 1. Monthly Earnings for the eligible and non-eligible mothers' groups by month from childbirth.
Note: Shows the evolution of the monthly earnings of mothers 24 months prior to birth to 72 months after birth for the treatment and control groups. The earnings are in constant 2006 dollars.

In the Table 3, we find the means and the quartiles for the monthly earnings of eligible and non-eligible mothers. The period before childbirth is from the 21st to 12th month prior to birth, while the post-period is composed of 6-months average from the 18th to 60th month. Again, it is evident that the treatment group has consistently higher earnings than the control group. Also, we can notice that the gap tends to increase in the post-period, indicating a possible positive effect of the childcare reform. Interestingly, we can see that percentage change in earnings with the pre-period is smaller for the treatment group, indicating a smaller decline in their earnings. Eligible mothers tend to have a smaller drop in earnings, but also a faster recovery of their earnings. In fact, they return to the same level of earnings as of the pre-period between the 48th and 54th months after birth, while non-eligible mothers recover only between the 54th and 60th months.

Table 3. Mean Earnings by 6-months average for the treatment and control groups

	Pre-Period	**Post-Period**							
	21st to 12th month	18th month	24th month	30th month	36th month	42nd month	48th month	54th month	60th month
Treatment									
Average monthly earnings	3274.37	2985.86	3033.60	3120.99	3166.40	3216.79	3245.27	3294.14	3297.16
% change from pre-period		-9.66%	-7.94%	-4.91%	-3.41%	-1.79%	-0.90%	0.60%	0.69%
Percentiles									
25th	1950.52	1304.35	1368.63	1465.67	1528.54	1570.03	1608.01	1642.53	1639.89
50th	2975.51	2564.18	2640.93	2719.65	2779.85	2824.76	2857.01	2893.97	2901.58
75th	4161.36	3949.61	4026.42	4141.88	4175.34	4230.02	4292.48	4337.98	4373.98
N	55242	24771	25941	27180	27855	28185	28278	28509	28992
Control									
Average monthly earnings	2916.81	2600.83	2616.76	2664.26	2713.71	2782.60	2831.92	2881.59	2929.75
% change from pre-period		-12.15%	-11.47%	-9.48%	-7.48%	-4.82%	-3.00%	-1.22%	0.44%
Percentiles									
25th	1523.75	988.49	1040.88	1108.05	1148.93	1171.74	1217.14	1254.98	1294.16
50th	2631.79	2146.88	2185.70	2250.73	2287.70	2365.20	2419.76	2445.86	2507.91
75th	3816.54	3510.67	3555.15	3590.17	3657.77	3735.67	3777.95	3824.89	3857.26
N	57681	28302	29943	30987	31959	32562	32967	33447	34098

Note: Shows the summary statistics of the pre-period and post-periods for the treatment and control groups. The pre-period is the average of the 21st month through the 12th month prior to birth, and the post-periods are 6-months average from the 18th to 60th month. The % change from the pre-period gives the difference in average monthly earnings compared to the pre-period average. The earnings are in constant 2006 dollars.

We also present disaggregated results for the impact of the reform on earnings by levels of education, ethnicity and age of the mother at birth. Figure A1 presents the 3-months average earnings by levels of education. A few things are worth pointing out. First, the effect of the policy is striking for the highly educated group, especially since the treatment group have lower earnings in the pre-period. Secondly, the gap that existed before birth appears to stay constant or even enlarge slightly for the university graduates and high school graduates. Figure A2 compares the patterns by ethnic groups. The graphs display the distinct trends of earnings relative to ethnicity, where European seems to have the biggest drop at birth. Interestingly, eligible Asian mothers seem to benefit the most from the policy with a huge increase after birth relative to non-eligible mothers. Finally, the graphs by age of the mother at birth are presented in Figure A3. Mothers from the middle age group, from 25 to 29 and 30 to 34 years old, seems have higher positive effects.

5.2 Difference-in-Differences Results

The econometric results for the DD approach are found below, where three specifications are estimated for two samples. The different samples are defined by the length of the post-period. In addition to our two-period model, we also run a multi-period model to check the robustness of our results.

5.2.1 Two-period Models

Table 4 presents the results of our DD regressions using two periods, and three different specifications (see Table A1 for the detailed table). We run the specifications on two alternative post-period samples. The estimates in columns (1) through (3) are based on the sample from the 12^{th} to 60^{th} month while the estimates in columns (4) through (6) are based on the sample from the 24^{th} to 60^{th} month.

Table 4. DD-estimates of monthly earnings using the two-periods model

	DD (12^{th}-60^{th} month)			DD (24^{th}-60^{th} month)		
	1	2	3	4	5	6
DD-effect	**56.09***	**35.81**	**65.49****	**63.91****	**40.54**	**85.54****
	(1.81)	*(30.03)*	*(32.16)*	*(32.22)*	*(31.24)*	*(35.44)*
Eligible	357.49***	322.21***	292.50***	357.49***	323.52***	287.14***
	(33.19)	*(30.48)*	*(33.10)*	*(33.19)*	*(30.45)*	*(33.97)*
Post	-160.98***	-456.27***	-484.58***	-117.82***	-454.27***	-492.93***
	(20.60)	*(33.09)*	*(35.51)*	*(21.38)*	*(33.10)*	*(36.77)*
Pre-period average	3274.37	3274.37	3274.37	3274.37	3274.37	3274.37
% effect	1.7%	1.09%	2.00%	1.95%	1.24%	2.61%
Individual Controls		X	X		X	X
Fixed time effects			X			X
Unemployment rate			X			X
R-squared	0.0077	0.1113	0.1113	0.0077	0.112	0.112
N	595,269	595,269	595,269	487,386	487,386	487,386

Note: This model relies on Eq.(2). The estimates in columns (1) through (3) are based on the sample from the 12^{th} to 60^{th} month, while the estimates in columns (4) through (6) are based on the sample from the 24^{th} to 60^{th} month. Column (1) and (4) presents a simple OLS model with no control. In columns (2) and (5) controls are added for individual characteristics, with respect to age, age squared, level of education, and ethnicity. Columns (3) and (6) present the specification with controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. The variable *Post* is a dummy variable: 1=period after birth, 0 otherwise. *DD*-effect is the interaction variable between *Eligible* and *Post*. The % effect represents the change in percentage between the pre-period and the post-period average earnings. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

In columns (1) and (4) we estimate a simple OLS model with no control. These naïve estimates show a strong correlation between the childcare reform and the mother's earnings. Such simple estimates cannot assure a causal interpretation since the effect might not be entirely isolated. Consequently, the two other models attempt to control for the direct effects of the childcare reform. In columns (2) and (5) we add simple controls for the individual mother's characteristics, with respect to age, age squared, level of education, and ethnicity. This model corresponds to Eq.(2) in Chapter 3. Adding these controls leads to a reduction in the estimated effect of the reform.

Finally, we include time fixed effects, as well as the quarterly unemployment rate to control for calendar time effect. The unemployment rate is matched to the actual date (month and year) for each observation at a country-level. As mentioned in the methodology chapter, our model is based on the distance from childbirth (t), and therefore it is to control for the calendar time. The model 3 is hence considered the complete specification, and is presented in columns (3) and (6). Adding the time controls increase the size of the coefficients. It suggests that the 20 Hours ECE reform raises the earnings of eligible mothers of one child by \$65.5 per month between the 12th to 60th month after birth. This represents a 2 percent growth in earnings compared to the average pre-period earnings of eligible mothers. Similarly, the treatment effect is estimated to \$85.5 per month for the post-period sample between the 24th and 60th month, which corresponds to a 2.6 percent increase in maternal earnings. The estimate is slightly higher when we use a shorter interval because the effect between the 12th to 24th month are only starting to grow, and still quite small. Therefore, adding these early months pull the estimate down. Using specification 3, both samples leads to an estimate that is significantly different from zero at 5 percent level. Related to the effect of the controls all variables present the expected signs (see Table A1). Earnings increase with age and the level of education, while it decreases for ethnicity groups other than European.

5.2.2 Multi-period model

The estimates of the growing effects of the 20 Hours ECE reform disaggregated by month are provided in Figure 2 and Table A2. We apply the complete specification, which controls for mother's characteristics, time fixed effects, and the unemployment rate. With this model, which corresponds to Eq.(3) in Chapter 3, we use the sample from the 12th to 60th month as we believe it gives a more complete representation of the intensification of the effect. The results are perfectly in line with those obtained in the previous two-period DD table. The effects of the reform begin to be entirely positive at the 19th month after birth. Also, the coefficients are all statistically significant between the 25th through 44th month, and up to the 56th with some insignificant periods in-between. The most precisely estimated effect for a mother happens when her child is 2 and 3 years old, with an average increase of \$91.9 in her monthly earnings for that specific period. The maximum effect in dollars happens at the 27th month after birth, with an increase in earnings established at $123.1. When running the 3-months average specification of this model, we find very similar conclusions, just slightly more aggregated.

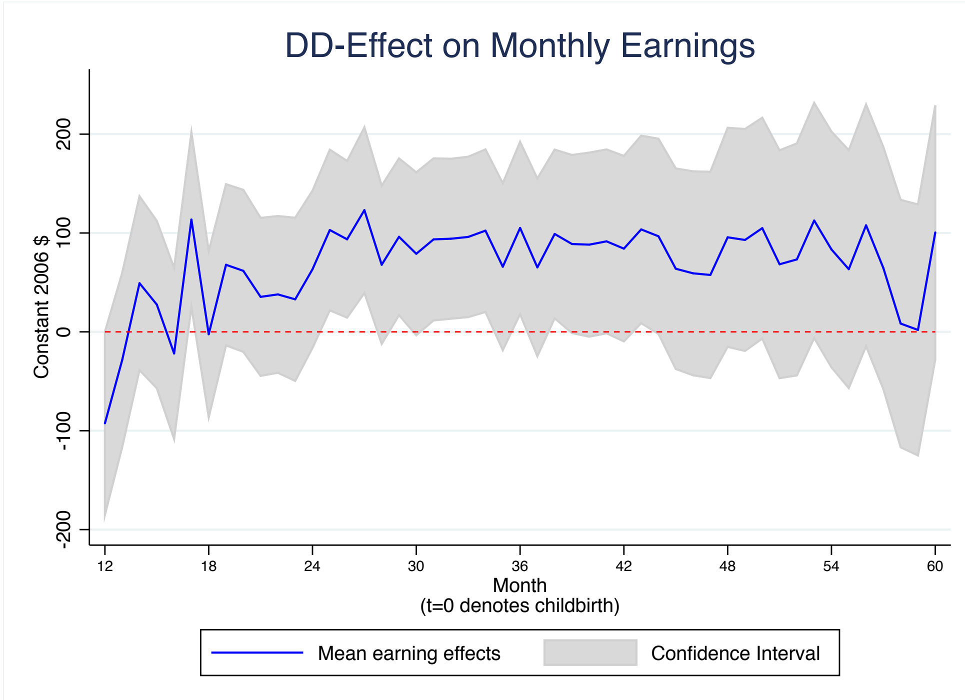

Figure 2. DD-Effect on monthly Earnings for the eligible mothers by month from childbirth.
Note: Shows the evolution of the increase in monthly earnings for eligible mothers from the 12th to 60th month after birth. The gray zone represents the 95% confidence interval. The effects in earnings are in constant 2006 dollars.

5.2.3 Subsample of mothers

The differences found in women's socioeconomic background are reflected in their labour outcomes. The analysis of the separated groups based on ethnicity, level of education and age of the mother at birth is realized using the 3-month average specification of equation (3), and are presented in the Tables A3 to A5.

The earnings effects found among mothers reflect a heterogeneous response to the reform by ethnic groups. Interestingly, Asian mothers benefited the most from the 20 Hours ECE reform. The estimators of the effect are highly positive, and statistically significant for the whole period from the 12th to 60th month, with a high average of $340.2 increase in earnings per month. For European mothers, the policy effects start being positive from the 27th month until the 57th month. The average effect during this period is $37.1 per month for eligible mothers. The Māori and Pacific ethnic groups experience a much more irregular pattern in their effects. The coefficients are high in the early 12th and 15th months after birth, followed by scattered effects altering between positive and negative coefficients. Therefore, they seem to benefit less from the policy.

Heterogeneous effects by educational attainment show some interesting patterns. All levels of education have a positive effect for the whole period from the 15th to 60th month, with the exception of mothers without education that finishes earlier (at the 51st month) and university graduates that starts later (at the 27th month). Interestingly, the effects grow larger as the level of education increases. The estimates rise by about 25 percent from high school qualification to graduate qualification, with the mean effect passing from \$59.2 to \$80.8 per month. Then, the effect grows by 75 percent from graduate to post-graduate level, to reach a mean effect of \$317.9 per month for mothers with a post-graduate degree. Possibly, the rise of these patterns is because we face some unobserved heterogeneity amongst mothers.

The age of the mother at childbirth does influence the impact of the childcare policy. In fact, the strongest effects are seen in women who gave birth between 25 and 29 years old, followed closely by the 30 to 35-year-old group. Both age groups show positive effects from the 15th month, and have significant estimates between the 27th to 60th month. The mean effect is respectively \$155.6 for the 25 to 29-year-old group, and \$118.30 for the 30 to 34-year-old group. Younger mothers, aged 20 to 24 years old, have an average effect of \$54.28 with positive period from 12th to 42nd month. The estimates are lower for older mothers, aged 35 to 50 years old, who have an average effect of \$40.10 for the positive period ranging from the 21st to 48th month.

5.3 Difference-in-Difference-in-Differences Results

At this point, it could be asked whether the earning effects found among eligible mothers is attributed to the reform itself, or whether it is the random result of specific shocks in the market, like economic fluctuations. To put our results in perspective, we present robustness checks based on the DDD approach. We therefore develop an alternative specification by adding a sample of non-mothers as a second control group. This allows us to control for time-specific variations as long as the aforementioned specific shocks affect mothers and non-mothers similarly. As explained in Chapter 3, we need to use a matched sampling method to select our sample of non-mothers. Hence, we first explain how we find a non-mother match for each mother using the control variables and earnings. Secondly, we present the results of the DDD regressions for the two-period models followed by the multi-period model.

5.3.1 Construction of the matched group of non-mothers

To use the matched sampling procedure, we need to further restrict the sample of 14,589 mothers to mothers who only worked all the months during the pre-period, therefore between the 21st and 12th month before birth. With this purpose in mind, we keep mothers with a maximum of one non-working month, and exclude mothers with a bigger gap between these months. For example, we would keep a mother who worked all months from her 21st to 12th month prior to birth even if she didn't work during the 18th month because it is a single month gap. But we would exclude this mother if she did not work during the 18th and 19th months. We then replace the single month gap with earnings equal to 0. After restricting the sample, we are left with 9,691 mothers. That sample of mothers represents 3 percent of the 319,461 potential non-mother controls.

Our conditioning set includes date of birth (month and year), level of education and ethnicity. Based on these discrete variables, we find an exact match with replacement. We also use the smoothed monthly earnings for the pre-period to find the smallest proximity score. This allows us to find the closest match.[27] The detailed matched sampling methodology is presented in Chapter 3.

We are then able to match 4,494 mothers from the eligible group and 4,878 from the non-eligible group, for a total of 9,691 pairs of matched mothers and non-mothers. We can be confident in the matching procedure since the fit of the variable is exact, and we control the fit of the earnings within a $1000 interval. In fact, only 319 pairs are outliers to this correcting, and therefore the matching procedure has been accurate for more than 95 percent of the cases. This is a high success rate compared to Beblo, Bendery and Wolfz (2009) who matched about 75 percent of their sample, and Lundin, Mörk, & Öckert (2008) with 88 percent of their sample. It is to be noted that the outliers are most likely to be women with very high levels of education, and from smaller ethnic group. Thus, in a way, the matching excludes some of the more extreme observations.

Table 5 compares the descriptive statistics for the selected mothers, and their potential and effective non-mother controls based on all observations. The left side of the table presents the observed characteristics before the matching, and the right side after the matching. It shows that the average means differ slightly between mothers and potential non-mothers in the raw data. Mostly, potential non-mothers are about 6.5 years older, 4 percent less educated, and 5 percent more likely to be European. Evidently, the matching contributes to a balancing of the samples with respect to the relevant variables. The right side of the table 5 also shows that the matching leads to strongly decreasing differences between mothers and selected non-mothers, with less than a 1 percent difference.

[27] The monthly earnings are smoothed using the 4-months moving average.

Table 5. Descriptive statistics for the treatment and control groups before and after the matched sampling

	Raw Data		Matched Data			
	All Mothers	All Potential Non-mothers	Mothers Eligible	Mothers Non-Eligible	Selected Eligible non-mothers	Selected Non-Eligible non-mothers
Mean monthly earnings	2983.85 *(3.46)*	3347.20 *(3.30)*	3239.90 *(3.67)*	2880.51 *(3.71)*	3731.55 *(3.19)*	3407.23 *(3.31)*
Age (mean)	34.95	41.44	35.00	34.94	34.95	34.95
Education						
Lower than high school	9.01%	13.12%	7.83%	9.68%	8.31%	10.28%
High school graduate	61.67%	58.55%	61.95%	64.58%	62.64%	65.13%
Bachelor graduate	24.29%	24.21%	25.51%	21.50%	24.57%	20.55%
Postgraduate	5.03%	4.12%	4.70%	4.23%	4.48%	4.04%
Ethnicity						
European	76.85%	82.00%	75.76%	81.73%	76.92%	82.36%
Māori	5.26%	4.81%	4.99%	5.10%	4.78%	5.09%
Pacific	5.08%	3.53%	5.34%	4.51%	5.10%	4.25%
Asian	10.66%	7.43%	11.80%	7.14%	11.28%	6.71%
Other ethnic.	2.15%	2.23%	2.10%	1.51%	1.91%	1.59%
N (Obs.)	951,039	50,162,334	354,180	323,967	420,354	379,422
N (Mothers)	14,589	319,461	4,878	4,494	4,878	4,494

Note: Shows the summary statistics of the raw data and the matched data for the treatment and control groups defined on the basis of eligibility and mother status. The earnings are in constant 2006 dollars.

Matched sampling is judged to be successful only if the matches overlap sufficiently in the pre-period (Heckman et al., 1999). The most straightforward way to check the common overlapping between the treatment and the control groups is the visual analysis of Figure 3. The graph shows the monthly earnings of the four groups: eligible mothers, non-eligible mothers, eligible non-mothers and non-eligible non-mothers. There is clear evidence of an overlap between the groups during the pre-period, more specifically between the 21st to 12th month identified by the grey dash lines. It is to be noticed that slightly after the 12th month before birth, there is some decline in the earnings of mothers compared to non-mothers. Interestingly, the gap between the two non-mother groups seems to grow a little over time, until around the 36th month where it starts to contract significantly. Overall, the trend in earnings for the mothers groups is similar to the one in previous Figure 1 for the DD method. The difference between the two graphs is that the upward jump before the drop in earnings becomes negligible here. This is probably due to the matching which eliminate some of the more extreme observations.

Our data allows us to accommodate t=0, the birth event, by linking the earnings of a matched non-mother to the same calendar date as the mother, and therefore identify where the t=0 would be for the non-mother. Therefore, we are able to compare mothers and non-mothers at each period t.

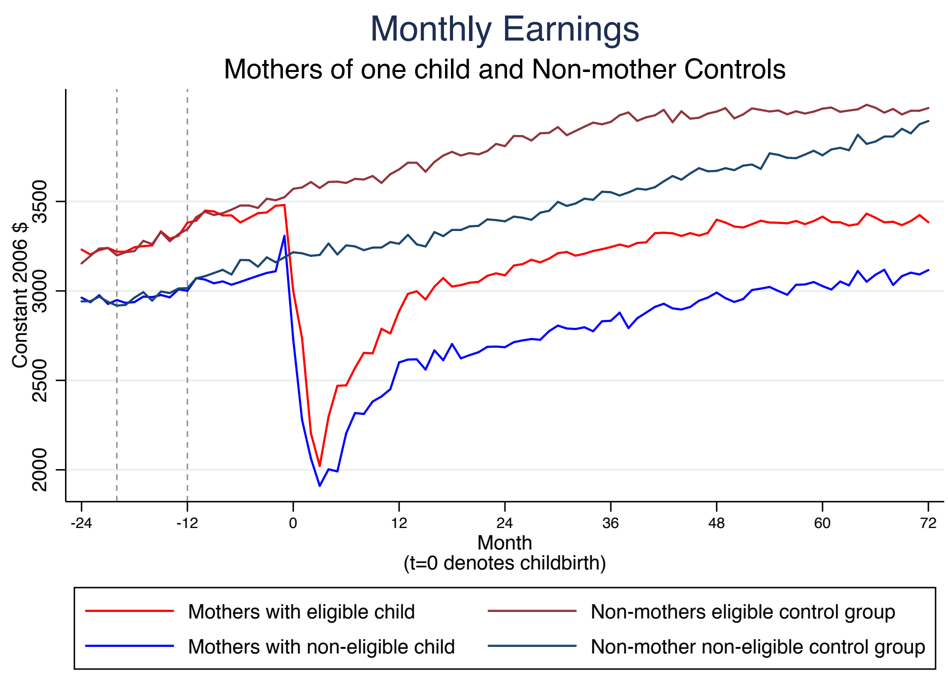

Figure 3. Monthly Earnings for mother and non-mother groups by eligibility and by month from childbirth.
Note: Shows the evolution of the monthly earnings of mothers 24 months prior to birth to 72 months after birth. The groups are based on eligibility to the program and mother status. The earnings are in constant 2006 dollars.

We also perform an additional robustness check to assess the validity of the main identifying assumption. In fact, our methodological approach lies on the common trend assumption in the treatment and comparison groups in the pre-period. A concern when applying any DD or DDD model is that the effect found may be the result of a different time trend between the treatment and control group instead of the impact of the policy. Such difference in time trends in the pre-period between the groups would definitely raise concerns about the validity of the results.

Figure 4 shows the average monthly earnings for eligible and non-eligible mothers as used in the DD model. Figure 5 shows the average monthly earnings for the four groups of eligible mothers, non-eligible mothers, eligible non-mothers and non-eligible non-mothers as used in the DDD model. These graphs suggest that the trend between the groups are very parallel, and both stable. Based on this visual analysis, we find no evidence indicating that earnings evolved differently between the treatment and control groups in the pre-period.

Furthermore, we directly test the trend over the pre-period to make sure they are parallel and stable. Specifically, we estimate the following regressions respectively for the DD and the DDD models:

$$Y_{igt} = \propto_0 + \propto_1 Eligible_{ig} + \propto_2 Trend_t + \beta_1 (Eligible_{ig} * Trend_t) + \varepsilon_{it} \quad (9)$$

$$Y_{igkt} = \propto_0 + \propto_1 Eligible_{ig} + \propto_2 \ Trend_t + \propto_3 Mother_k + \propto_4 (Eligible_g * Mother_k) + \propto_4 (Eligible_g * Trend_t) + \propto_6 (Mother_k * trend_t) + \beta_1 \big(Eligible_g * Mother_k * Trend_t \big) \quad (10)$$

The *Trend* variable is a time trend for the pre-period months, and it interacts with the treatment variables. The coefficient $\propto_2$ identifies the common trend between the treatment and control groups. β_1 is the coefficient that captures any difference in the trend between the two groups. If there were a difference in the time trend in the treatment and control groups, then the estimated effect of β_1 should be significantly different from zero. The results from these regressions for the pre-trend are presented in Table A6. Both coefficients for the interaction variable are small, and most importantly not statistically significant. Therefore, the parallel trend assumption is not rejected.

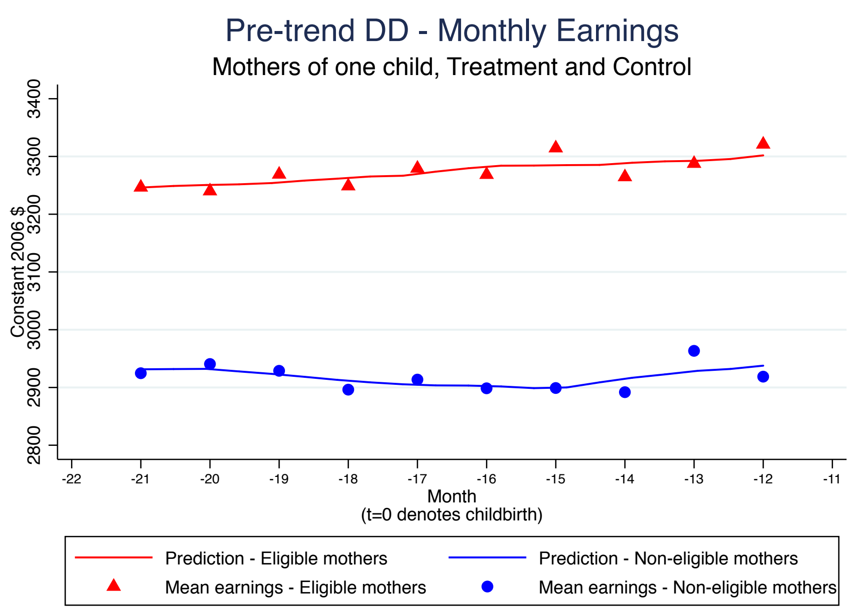

Figure 4. Monthly Earnings for the pre-trend period of the DD model
Note: Shows the evolution of the monthly earnings of mothers from 21st month through the 12th month prior to birth for the treatment and control groups. The earnings are in constant 2006 dollars.

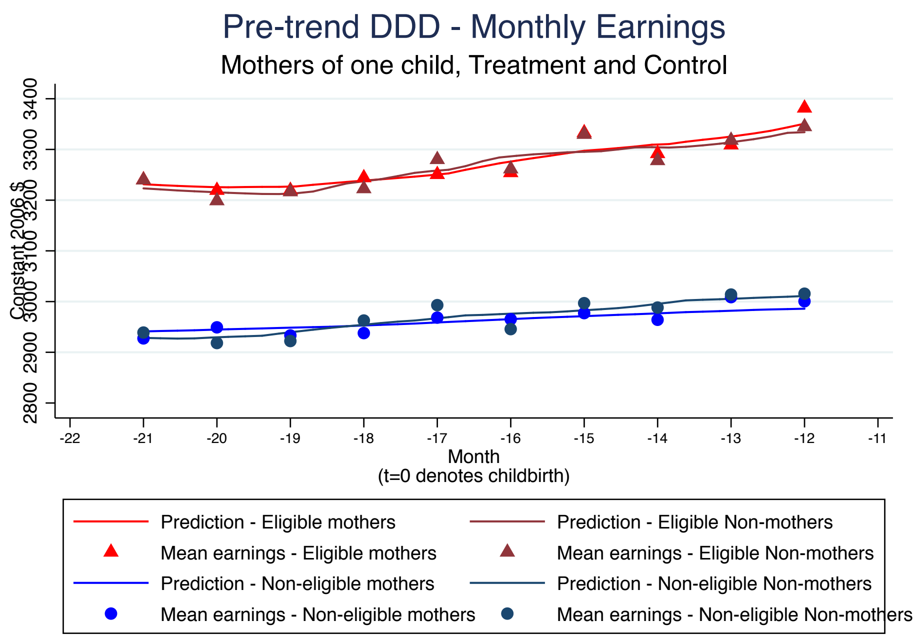

Figure 5. Monthly Earnings for the pre-trend period of the DDD model
Note: Shows the evolution of the monthly earnings of mothers from 21st month through the 12th month prior to birth. The groups are based on eligibility to the program and mother status. The earnings are in constant 2006 dollars.

Table 6 presents the means for the pre-period and the post-period for the four groups when using the 12th to 60th month sample. This corresponds to the equation (6) in Chapter 3. The first DD is given by the difference in means of the mothers, and the second DD is between the non-mothers. These two difference-in-differences give us the triple-differences, which appears considerably smaller than the DD estimate.

Table 6. DD- and DDD-estimates of monthly earnings using the pre- and post-means

	Matched data			
	Mothers		Selected Controls	
	Eligible	Non-Eligible	Eligible non-mothers	Non-Eligible non-mothers
Pre-period	3274.35	2963.68	3269.12	2970.28
	(8.22)	*(8.53)*	*(7.72)*	*(8.07)*
Post-period	3228.88	2832.40	3902.29	3532.13
	(5.40)	*(5.40)*	*(4.74)*	*(4.85)*
Difference	-45.47	-131.28	633.17	561.85
DD		85.81		71.31
DDD				**14.50**
N				909 432

Note: Shows the means of the pre-period and post-periods for the treatment and control groups based on eligibility and mother status using the matched data. The pre-period is the average of the 21st month through the 12th month prior to birth, and the post-period ranges from the 18th to 60th month. The earnings are in constant 2006 dollars.

5.3.2 Two-period Models

First, we run the same three model specifications used for the two-period DD at the beginning of this chapter. To address concerns for variable selection, we report three sets of estimates: without control, with individual controls, and with time fixed effects and time controls. Table 7 presents the results of the two-period DDD based on equation (7) (See Table A7 for the detailed table). We run the specifications on two alternative post-period samples, one from the 12^{th} to the 60^{th} month in columns (1) through (3), as well as one from the 24^{th} to the 60^{th} month in columns (4) through (6).

Table 7. DDD-estimates of monthly earnings using the two-periods model

	DDD (12^{th}-60^{th} month)			DDD (24^{th}-60^{th} month)		
	1	2	3	4	5	6
DDD-effect	**14.50**	**14.88**	**16.45**	**33.74**	**32.15**	**33.13**
	(40.69)	*(39.70)*	*(39.73)*	*(43.23)*	*(42.15)*	*(42.16)*
Eligible	298.85***	273.54***	235.66***	298.85***	273.47***	233.60***
	(31.24)	*(28.43)*	*(29.21)*	*(31.24)*	*(28.44)*	*(29.59)*
Post	561.85***	294.73***	389.55***	629.87***	345.86***	387.54***
	(16.74)	*(17.99)*	*(26.19)*	*(18.36)*	*(19.88)*	*(27.52)*
Mother	-6.60***	-0.69	-0.67	-6.60	-0.70	-0.64
	(32.47)	*(29.81)*	*(29.80)*	*(32.47)*	*(29.81)*	*(29.81)*
*Eligible*Post*	71.31***	67.51***	105.95***	55.91**	50.99**	102.65***
	(23.90)	*(23.25)*	*(23.77)*	*(26.07)*	*(25.33)*	*(27.29)*
*Mother*Post*	-693.14***	-725.12***	-730.33***	-703.78***	-728.77***	-730.80***
	(28.56)	*(27.87)*	*(27.91)*	*(30.35)*	*(29.61)*	*(29.64)*
*Eligible*Mother*	11.83	11.68	11.61	11.83	11.68	11.61
	(44.98)	*(40.84)*	*(40.83)*	*(44.98)*	*(40.84)*	*(40.84)*
Pre-treatment average	3178.62	3178.62	3178.62	3178.62	3178.62	3178.62
% effect	0.46%	0.47%	0.52%	1.06%	1.01%	1.04%
Individual Controls		X	X		X	X
Fixed time effects			X			X
Unemployment rate			X			X
R-squared	0.0313	0.1314	0.1328	0.0318	0.1324	0.133
N	909,432	909,432	909,432	734,808	734,808	734,808

Note: This model relies on Eq.(7). The estimates in columns (1) through (3) are based on the sample from the 12^{th} to 60^{th} month, while the estimates in columns (4) through (6) are based on the sample from the 24^{th} to 60^{th} month. Column (1) and (4) presents a simple OLS model with no control. In columns (2) and (5) controls are added for individual characteristics, with respect to age, age squared, level of education, and ethnicity. Columns (3) and (6) present the specification with controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. The variable *Post* is a dummy variable: 1=period after birth, 0 otherwise. The variable *Mother* is a dummy variable: 1=mother group, 0 otherwise. *DDD*-effect is the interaction variable between *Eligible*, *Mother* and *Post*. The % effect represents the change in percentage between the pre-period and the post-period average earnings. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

We find that the DDD model leads to a significant reduction in the estimates of the effect of the reform compared to the DD model. The coefficients are positive but rather small and not statistically significant, both before and after the introduction of the control variables. It is noticeable that the impact of the reform is now only about half as large as the impact we found previously with the DD approach. This could be due to the compositional differences in mothers used in the DD and the DDD. It could also be that the previous bigger effect seen in the DD was due to time-specific macroeconomic trends, which are filtered out in the DDD. In column (3), the specification including all controls for the 12^{th} to 60^{th} month period gives a treatment effect of \$16.5 per month for eligible mothers. When compared to the pre-period mean, the estimate implies a 0.52 percent increases in monthly earnings. The sample from the 24^{th} to 60^{th} month produces a larger estimate, with an increase of \$33.1 or 1.04 percent of the pre-period mean earnings. These results suggest that the 20 Hours ECE reform had a small effect on maternal earnings. Also, the DDD can be interpreted as the variation in the motherhood penalty between mothers and non-mothers resulting from the reform.

5.3.3 Multi-periods model

To get even more precision with the DDD approach, we run the multi-period specification to explore whether the effects of the reform changes over time. We use our baseline specification including controls for individual characteristics, fixed time effects, and unemployment rate for the 12^{th} to 60^{th} month sample. This corresponds the model Eq(8) in Chapter 3. The estimates for the dynamic impact of the reform are reported in Figure 6 and Table A8. From the 25^{th} month, all coefficients are positive, but not statistically significant and quite small. Interestingly, there is a clear rising trend starting from the 39^{th} month, which intensifies after the 48^{th} month. This suggests that the reform impacts a mother the most when her child is between 3 and 5 years old. This seems reasonable since it corresponds to the admissible age for the 20 Hours ECE program. In terms of dollars, the average increase in monthly earnings for the 3^{rd} year after birth is \$28.8 per month, and rise to an average increase of \$54.2 per month during the 4^{th} year after birth. When compared to the pre-period mean, these estimates imply a 0.9 percent and a 1.7 percent increases in earnings, respectively for the 3^{rd} and 4^{th} years after birth. Again, the 3-months average specification give us similar results.

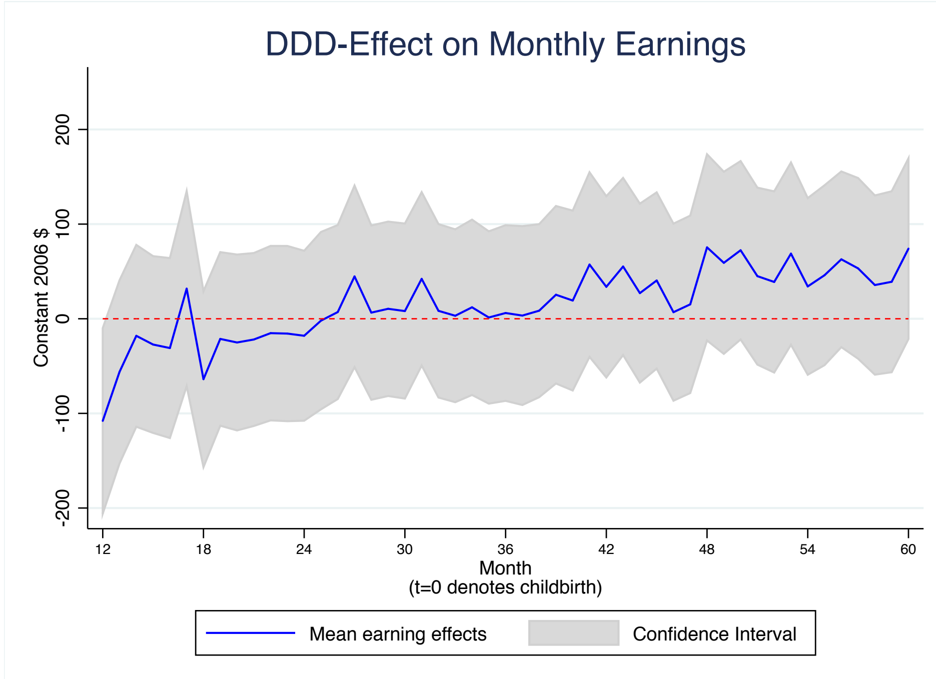

Figure 6. DDD-Effect on monthly Earnings for the eligible mothers by month from childbirth.
Note: Shows the evolution of the increase in monthly earnings for eligible mothers from the 12^{th} to 60^{th} month after birth. The gray zone represents the 95% confidence interval. The effects in earnings are in constant 2006 dollars.

5.3.4 Subsample of mothers

Turning to subsample analysis, Table A9 through Table A11 displays results from estimating Eq(8) separately by educational attainment, ethnicity, and age at birth. The regressions are estimated using the complete specification and the 3-month average. It should be noted that most coefficient estimates are not statistically significant, consistent with the previous results using the triple-differences approach. The distinctive patterns emerging from these subsamples are not only quite strong, but also differ considerably from the previous subsample results using the DD model. These patterns can be seen in the Figure A4 through A6.

When analyzing by ethnicity, we first notice that European mothers only have positive coefficients when their child is 4 years old, with a small average effect of $18.3 per month for that period. The strongest effect of the reform is experienced by Asian and other ethnicity. In fact, they have positive and very high estimates for the whole period from the 15th to 60th month after birth. Finally, the most striking estimates appear for Māori and Pacific mothers, where almost all their estimates are negative. This suggests that Māori and Pacific do not experience the benefits of the 20 Hours ECE reform. Interestingly, it is pointed out by Crossan et al. (2011) that financial literacy among Māori is lower than that of the general population of New Zealand, which might be a factor in explaining our results.

The estimates by levels of education are very interesting. Previously the DD model results were showing that mothers with higher educational attainment had significantly higher effect for the whole period. However, when using the DDD model, the positive effects are only present during the 4^{th} year after birth, with an average increase in maternal earnings of $107.9 for that period. All mothers with no education benefit from the reform starting from the 21^{st} month, with an average effect of $98.3 per month. Surprisingly, mothers with a bachelor degree are not impacted by the childcare intervention, and have mostly small and negative coefficients.

Further, the decomposition of mothers at birth by age groups gives us some insights. Younger mothers, aged 20 to 24 years old, benefit the most from the reform. Not only do they have positive effects for the whole sample period, but they also have the highest mean increase in earnings with $111.1 per month. Overall, the other age groups have mostly positive coefficients only for the 3^{rd} and 4^{th} years after birth.

Chapter 6

Discussion

While workplaces continue to penalize women who become mothers, both in terms of employment probabilities and earnings, the implementation of family-friendly policies aim to ensure adequate support for these mothers (Gornick and Meyers 2003). Up until now, the majority of quasi-experimental studies, investigating the effects of childcare reform on maternal outcomes, have analyzed the labour force participation of mothers. This study is therefore one of the rare studies analyzing the earning effects of childcare reform, and the first study to directly examine the impact of the 20 Hours ECE reform in New Zealand. Also, we improve upon recent studies in this field by using a very rich longitudinal dataset, which allows us to estimate a number of different models to assess the robustness of our results. This research estimates the impacts of the introduction of the 20 Hours ECE reform in New Zealand on mothers' earnings, and whether it has reduced the motherhood penalty. We assess monthly earnings differentials among mothers, conditional on the eligibility the program, using DD and DDD models with two- and multi-period specifications.

Our results show that the 20 Hours ECE reform had positive effects on mother incomes. When using the DD method, we find that the impact of the reform on maternal earnings is statistically significant, but modest in size. For eligible mothers, the childcare reform increases earnings by $65.5 to $85.5 per month for up to the 5th year after birth compared to their counterparts who do not benefit from the policy.[28] This represents a 2 to 2.6 percent rise in the baseline earnings of these mothers compared to non-eligible mothers. The DDD model provides some insight into the effects of the 20 Hours ECE reform on the motherhood gap, more specifically the differential between mothers and non-mothers, for both eligible and non-eligible groups. The regression estimates show that the 20 hours ECE reform increased monthly earnings of eligible mothers by 33 NZD, or 1.04 percent of pre-motherhood average earnings, controlling for motherhood wage penalty and time-specific effects such as the

[28] These are presented in constant 2006 dollars, and adjusted for inflation using the CPI index.

global financial crisis. The estimates are positive, but not statistically significant. A possible explanation of these results, as mentioned in Lefebvre and Merrigan (2005), is that the policy benefited the same women who would have worked even without the policy. In that case, the reform would have no effect on mothers' earnings, but would have increased their relative wealth. Another explanation could be our sample restriction to mothers of a single child, which only allow us to capture a partial effect of the reform.

It is worth noticing that breaking down the effect on earnings by months shows interesting results. The estimate indicates an increasing effect of the reform from the 3rd year after birth, with an intensification in the effect from the 4th year. Since the policy is addressed to children aged 3 and 4-year-old, this suggests these results are consistent with the age eligibility of the program. Conversely, it could also reflect the progressive take up of the program amongst parents as the number of providers participating in the program rose from 62 percent to 76 percent between the first and second year of implementation (May, 2008).

Results show that the reform impacted less educated mothers the most despite the fact the relative reduction in childcare costs was larger for highly educated mothers.[29] The impact on less educated mothers could be explained by the fact that they possibly have lower income, and because of that budget constraint, the policy made childcare more accessible to them. However, highly educated mothers experience the largest positive effect in terms of size, but only for the 4th year after birth. Similar patterns for highly educated mothers have been found in other studies (Haeck et al., 2015; Schlosser, 2011). Interestingly, our estimates show that most of the gains in earnings associated with the childcare reform are associated with Asian mothers, whereas we find no evidence that the reform has any benefit for mothers of Pacific and Māori ethnicity. Also, younger mothers, aged 20 to 24 years old seems to benefit the most from the childcare reform compared to other age groups.

It is important to compare our estimated effects with those found in the literature. However, it reveals to be a difficult comparison since our research question and method differ considerably from other studies. Also, as mentioned in the literature review, the studies addressing the maternal earning effects in relation to childcare policies are rare. Interestingly, we found similar results than Lefebvre and Merrigan (2005). In their study of the $5 a day reform in Quebec, they demonstrate that the earnings of mothers increase by approximately $2,300 per year after the reform.[30] Moreover, they point out that

[29] In fact, prior to the reform, families with low-income were eligible for tax-subsidies.
[30] Both specification (i) and (ii) reveal effects that are close in magnitude. The specification (i) shows a significant effect, but the hypothesis of no pre-policy trends is close to being rejected. While the specification (ii) show non-significant effect, but the hypothesis of no pre-policy trend is not rejected (Lefebvre and Merrigan, 2005).

the effects increase significantly for the 3rd and 4th years after the reform. We also found a similar upward trend, where the impact of the reform seems to be more important over time. While Lefebvre and Merrigan (2005) confirm the consistency of our results, the effects we find are much smaller. One explanation is that the $5 reform in Quebec is of a relatively bigger scope compared to the 20 Hours ECE reform, and would therefore be expected to have a larger impact on mothers. Another explanation could be that the empirical strategy we employ differed in several ways. Amongst these, we use the distance from childbirth instead of using the calendar time, and we use a more restricted sample of mothers. Another reason is that younger children were eligible to the subsidy in Quebec while only 3 to 5 years-old were eligible in New Zealand, which would explain a higher effect on maternal earnings in Quebec.

Our results are larger than the ones found in Givord and Marbot (2015). In their analysis of the French subsidy reform in 2004, they found a significant annual increase of about 100 euros for a mother of one child.[31] As mentioned by Givord and Marbot, such small effects are probably influenced by the French context, as well as the cultural background. In fact, French mothers were highly active in the labour force even before the reform, and it is culturally accepted for them to return to work just a few months after birth (Givord and Marbot, 2015).

This study provides the first empirical evidence on mother labour responses to the 2007 ECE reform in New Zealand. It also reflects how motherhood is a significant factor in influencing the labour market. Interestingly, in the early 2000's in New Zealand the participation of women aged 25-39 was quite low in comparison to women in other OECD countries.[32] However, over the last ten years there has been a significant increase in women's participation in the labour force, probably due to better policies encouraging mothers to remain in the labour market. In New Zealand, the labour force participation rate has increased for women aged 25–49 years. More specifically, from June 1994 to 2014, the participation rate of sole mothers has increased by 23 percent, compared to an increase of 7.8 percent for partnered mothers, and only 1.9 percent for women without children.[33] Such changes in the labour market responses of mothers definitely raise questions about the causality with the 20 hours ECE reform . This study will therefore gain to also examine the labor force participation and the decomposition of hours and hourly wages.

[31] The model is based on a yearly salary of 12 000 euros
[32] http://www.treasury.govt.nz
[33] http://archive.stats.govt.nz

Our research presents some potential sources of bias. The main limitation is related to our restricted sample. In fact, we constrained our sample to mothers of one child only, which is not representative of the whole population of mothers. We chose this approach as the earning patterns of mothers of multiple children require a different method. However, we started a follow-up project analyzing the effects for mothers of multiple children.[34] Indeed, Givord and Marbot (2015) point out that the effect for mothers of only one child is weak, comparable to those of mothers of two and more children. Furthermore, we restrict our sample to working mothers with earnings only. These restrictions might introduce a selection bias in our results as the observable and unobservable characteristics of working mother might differ from non-working mothers.

Despite the individual-level control variables included in our models, unobserved heterogeneity among mothers and non-mothers within and between the groups may prevent us to fully explain the effects of the reform. For instance, it has been argued that mothers might prefer to substitute their monetary advantage for flexibility or family-based advantages in their job. As argued by Gangl and Zieffle (2009), in some cases, mothers might change for more family-friendly employment after childbirth to better accommodate their family constraints. Such unobserved heterogeneity might therefore affect our results, as the earnings for these mothers might decrease.

A further concern is the possible presence of another policy or economic shock that coincides with the policy change. In fact, Baker et al. (2008) explain that other shocks occurring at the same time then the reform could confound the estimates. However, up to now, we have found no other reforms or changes that could affect our results. A final limitation that might affect our research is the lack of information on hours worked and hourly wage. It is therefore not possible to distinguish if the reform encourages mothers to work longer hours, or to go back to work earlier. These insights would be necessary to allow us to extensively understand the impact of the reform on New Zealand mothers.

[34] This project is expected to be completed in 2018.

Conclusion

In the heat of the debate about moving toward universal childcare, understanding the complex intersection of state policies, motherhood and the labour market has become very important to the development of comprehensive childcare policies. This is particularly relevant in the context of New Zealand, where there is yet no empirical evidence of the effect of economic incentives on maternal outcomes. The aim of this research is therefore to fill some of this void by undertaking the first comprehensive analysis of the 20 Hours ECE reform on mother's earnings. Our findings support the hypothesis that reducing the price of childcare has a positive impact on the labour market outcomes of mothers, as measured by their earnings.

We use a quasi-experimental method created by a reform of the New Zealand childcare system that occurred in 2007. We apply a DD regression analysis, as well as a DDD, on both two-period and multi-period specifications. Our results show that the 20 Hours ECE reform increased the earnings of mothers between 2 to 2.6 percent. Moreover, our results suggest that the new subsidized childcare increased monthly earnings of eligible mothers by 33 NZD, or 1.04 percent of pre-motherhood average earnings, controlling for motherhood wage penalty and time-specific effects. It is to be noticed that the policy has not eliminated the motherhood wage penalty. The estimated effects for the motherhood penalty are positive, but statistically insignificant. Interestingly, the intention of the policy was not aimed at increasing mother's earnings in the labour market, and such positive effects would therefore be considered a useful byproduct of the system

Some potential limitations of our study include our sample which is restricted to mothers of one child only and lack to represent the whole population of mothers. Also, even with the individual-level control variables included in our models, unobserved heterogeneity among mothers and non-mothers within and between the groups may bias our results. Finally, the lack of information on hours worked and hourly wage prevent us from distinguishing if the reform encourages mothers to work longer hours, or to go back to work earlier.

We bring many contributions to the literature on childcare policies and the motherhood penalty. First, we are the first quasi-experimental study looking at the effect of childcare reform on maternal earnings in New Zealand, and moreover, the first study on the effect of the 20 Hours ECE reform on maternal outcomes. Second, our study uses a comprehensive administrative dataset, including sociodemographic information, which is usually rare for administrative data. This data allows us to use multi-period specifications, and perform robustness checks. Finally, we study a very recent reform in a developed country, making our results particularly relevant for other developed countries with similar conditions looking at expanding their childcare support. Therefore, this research improves upon the empirical approach used in previous studies, and brings a valuable contribution to the literature.

To conclude, supporting the cost of raising children should be at the center of the political agenda of developed countries as the benefits resulting from child rearing diffuse to the whole society, as children are the future of their nation. With respect to avenues for future research, it would be worthwhile to extend this study to a broader array of outcomes like labour force participation, job and occupational segregation, enrollment, and quality of care. Finally, the outcomes of fathers could also be interesting perspective to consider empirically.

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Annexes I—Tables

Table A1. DD-regressions of monthly earnings using the two-periods model

	DD (12^{th}–60^{th} month)			DD (24^{th}–60^{th} month)		
	1	2	3	4	5	6
DD-effects	**56.09***	**35.81**	**65.49****	**63.91****	**40.54**	**85.54****
	(30.98)	*(30.03)*	*(32.16)*	*(32.22)*	*(31.24)*	*(35.44)*
Eligible	357.49***	322.21***	292.50***	357.49***	323.52***	287.14***
	(33.19)	*(30.48)*	*(33.10)*	*(33.19)*	*(30.45)*	*(33.97)*
Post	-160.98***	-456.27***	-484.58***	-117.82***	-454.27***	-492.93***
	(20.60)	*(33.09)*	*(35.51)*	*(21.38)*	*(33.10)*	*(36.77)*
Age		307.25***	307.31***		304.41***	304.49***
		(20.12)	*(20.13)*		*(19.44)*	*(19.44)*
Age^2		-3.50***	-3.50***		-3.47***	-3.47***
		(0.31)	*(0.31)*		*(0.30)*	*(0.30)*
Ethnicity						
European		Base	Base		Base	Base
Māori		36.49)	36.13		-6.70	-7.17
		(56.96)	*(56.97)*		*(57.07)*	*(57.07)*
Pacific		108.66**	108.97**		85.34*	85.80*
		(48.36)	*(48.36)*		*(48.48)*	*(48.48)*
Asian		-237.88***	-238.00***		-283.91***	-283.97***
		(50.57)	*(50.56)*		*(50.48)*	*(50.47)*
Other ethnic.		-83.44	-83.87		-102.11	-102.68
		(109.68)	*(109.68)*		*(109.03)*	*(109.03)*
Level of education						
No qualification		Base	Base		Base	Base
High School qualification		603.50***	603.67***		601.57***	601.84***
		(39.15)	*(39.15)*		*(39.01)*	*(39.01)*
Graduate qualification		1549.22***	1549.43***		1569.69***	1570.01***
		(53.98)	*(53.98)*		*(53.90)*	*(53.90)*
Post-Graduate qualification		2524.36***	2524.73***		2551.16***	2551.65***
		(118.89	*(118.88)*		*(120.43)*	*(120.42)*
Time fixed effects			X			X
Unemployment rate			X			X
Constant	2916.88***	-3970.76***	-3841.89***	2916.88***	-3913.35***	-3755.94***
	(22.91)	*(324.30)*	*(328.91)*	*(22.91)*	*(313.45)*	*(319.23)*
R-squared	0.0077	0.1113	0.1113	0.0077	0.112	0.112
N	595,269	595,269	595,269	487,386	487,386	487,386

Note: This model relies on Eq.(2). The estimates in columns (1) through (3) are based on the sample from the 12th to 60th month, while the estimates in columns (4) through (6) are based on the sample from the 24th to 60th month. Column (1) and (4) presents a simple OLS model with no control. In columns (2) and (5) controls are added for individual characteristics, with respect to age, age squared, level of education, and ethnicity. Columns (3) and (6) present the specification with controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. The variable *Post* is a dummy variable: 1=period after birth, 0 otherwise. *DD*-effect is the interaction variable between *Eligible* and *Post*. The % effect represents the change in percentage between the pre-period and the post-period average earnings. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

Table A2. DD-regressions of monthly earnings using the multi-period model

	Monthly			3-month average	
	DD-effects	***Std. Err.***		***DD-effects***	***Std. Err.***
β_{1} *(12th month)*	-92.49**	*(47.24)*	β_{1} *(12th month)*	-86.66*	*(47.38)*
β_{2} *(13th month)*	-28.43	*(44.97)*	β_{2} *(15th month)*	22.35	*(37.77)*
β_{3} *(14th month)*	49.23	*(44.85)*	β_{3} *(18th month)*	35.85	*(37.36)*
β_{4} *(15th month)*	27.58	*(43.21)*	β_{4} *(21st month)*	60.79*	*(36.11)*
β_{5} *(16th month)*	-21.87	*(44.14)*	β_{5} *(24th month)*	50.57	*(35.77)*
β_{6} *(17th month)*	113.62**	*(45.01)*	β_{6} *(27th month)*	112.61***	*(36.16)*
β_{7} *(18th month)*	-2.38	*(42.91)*	β_{7} *(30th month)*	87.16**	*(36.14)*
β_{8} *(19th month)*	67.84	*(41.60)*	β_{8} *(33rd month)*	100.96***	*(36.89)*
β_{9} *(20th month)*	61.73	*(41.80)*	β_{9} *(36th month)*	97.69**	*(38.45)*
β_{10} *(21st month)*	35.38	*(40.78)*	β_{10} *(39th month)*	91.40**	*(40.61)*
β_{11} *(22nd month)*	37.87	*(40.45)*	β_{11} *(42nd month)*	95.41**	*(42.70)*
β_{12} *(23rd month)*	32.91	*(42.12)*	β_{12} *(45th month)*	96.01**	*(46.06)*
β_{13} *(24th month)*	63.28	*(40.57)*	β_{13} *(48th month)*	79.11	*(50.16)*
β_{14} *(25th month)*	103.03**	*(41.46)*	β_{14} *(51st month)*	97.58*	*(53.63)*
β_{15} *(26th month)*	93.62**	*(40.42)*	β_{15} *(54th month)*	99.13*	*(56.54)*
β_{16} *(27th month)*	123.12***	*(42.72)*	β_{16} *(57th month)*	88.20	*(58.62)*
β_{17} *(28th month)*	67.84*	*(40.84)*	β_{17} *(60th month)*	46.81	*(61.03)*
β_{18} *(29th month)*	96.18**	*(40.44)*			
β_{19} *(30th month)*	78.98*	*(42.03)*			
β_{20} *(31st month)*	93.54**	*(41.83)*			
β_{21} *(32nd month)*	94.21**	*(41.28)*			
β_{22} *(33th month)*	96.02**	*(41.38)*			
β_{23} *(34th month)*	102.38**	*(41.95)*			
β_{24} *(35th month)*	65.88	*(43.05)*			
β_{25} *(36th month)*	105.10**	*(44.48)*			
β_{26} *(37th month)*	65.22	*(45.85)*			
β_{27} *(38th month)*	99.03**	*(43.53)*			
β_{28} *(39th month)*	88.86*	*(45.96)*			
β_{29} *(40th month)*	88.22*	*(47.49)*			
β_{30} *(41st month)*	91.55*	*(47.46)*			
β_{31} *(42nd month)*	84.19*	*(47.86)*			
β_{32} *(43rd month)*	103.63**	*(48.38)*			
β_{33} *(44th month)*	96.70*	*(50.35)*			
β_{34} *(45th month)*	63.84	*(51.75)*			
β_{35} *(46th month)*	59.23	*(52.63)*			
β_{36} *(47th month)*	57.55	*(53.24)*			
β_{37} *(48th month)*	95.64*	*(56.54)*			
β_{38} *(49th month)*	92.97	*(57.25)*			
β_{39} *(50th month)*	104.91*	*(56.99)*			
β_{40} *(51st month)*	68.38	*(58.80)*			
β_{41} *(52nd month)*	73.27	*(59.87)*			
β_{42} *(53rd month)*	112.68*	*(60.66)*			
β_{43} *(54th month)*	83.37	*(60.86)*			
β_{44} *(55th month)*	63.41	*(61.38)*			
β_{45} *(56th month)*	107.77*	*(62.36)*			
β_{46} *(57th month)*	64.53	*(62.50)*			
β_{47} *(58th month)*	8.37	*(63.83)*			
β_{48} *(59th month)*	1.95	*(64.80)*			
β_{49} *(60th month)*	100.53	*(65.58)*			
Eligible	288.50***	*(37.56)*	*Eligible*	282.55***	*(37.54)*
Age	307.32***	*(20.13)*		309.77***	*(19.39)*
Age2	-3.50***	*(0.31)*		-3.54***	*(0.30)*
Ethnicity					
European	Base				
Māori	36.02	*(56.98)*		25.28	*(56.36)*
Pacific	108.92**	*(48.36)*		95.92**	*(47.90)*
Asian	-237.93***	*(50.57)*		-251.96***	*(50.18)*
Other ethnicity	-83.91	*(109.69)*		-85.37	*(109.03)*
Level of education					
No qualification	Base				
High School	603.64***	*(39.15)*		603.71***	*(38.67)*
Graduate	1549.34***	*(53.98)*		1544.40***	*(53.41)*
Post-Graduate	2524.57***	*(118.88)*		2507.35***	*(117.53)*
Time effects	X			X	
Unemployment	X			X	
Constant	-3824.62***	*(336.31)*		-3850.75***	*(322.95)*
R-squared	0.1114			0.1122	
N	595,269			613,842	

Note: This model relies on Eq.(3). The estimates are based on the sample from the 12th to 60th month. The first model uses monthly period, and the second uses 3-month average. Both models are evaluated with controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. *DD-effect* is the interaction variable between *Eligible* and the period. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

Table A3. DD-regressions of monthly earnings using the 3-month average multi-period model by ethnicity

Ethnicity	European		Māori		Pacific		Asian		Other ethnicity	
DD-effects										
β_1 (12th month)	-213.93***	(57.23)	265.04*	(159.71)	182.33	(132.48)	171.97	(113.73)	84.66	(317.60)
β_2 (15th month)	-67.71	(45.55)	34.01	(123.07)	170.88*	(102.01)	213.30**	(97.68)	221.29	(224.31)
β_3 (18th month)	-44.49	(44.73)	-63.60	(128.62)	-29.67	(110.57)	309.42***	(98.73)	252.51	(220.95)
β_4 (21st month)	-17.23	(43.09)	-32.37	(123.02)	33.65	(109.75)	334.51***	(97.34)	155.90	(219.15)
β_5 (24th month)	-16.50	(42.69)	-101.33	(127.10)	37.68	(102.62)	330.68***	(94.66)	-122.81	(229.22)
β_6 (27th month)	39.36	(43.02)	-102.21	(119.57)	151.33	(107.84)	433.41***	(101.17)	184.63	(227.43)
β_7 (30th month)	40.62	(43.09)	-26.24	(124.56)	-35.39	(106.05)	306.14***	(103.80)	-39.75	(216.61)
β_8 (33rd month)	**54.83**	(44.24)	73.12	(127.37)	-28.51	(103.38)	294.81***	(104.44)	-140.35	(217.92)
β_9 (36th month)	29.06	(45.90)	160.02	(133.40)	25.11	(107.82)	366.94***	(108.19)	6.42	(234.42)
β_{10} (39th month)	27.73	(48.34)	48.84	(132.00)	-7.92	(117.31)	370.28***	(117.87)	138.95	(239.70)
β_{11} (42nd month)	48.45	(50.87)	-0.29	(145.80)	-136.83	(135.90)	334.47***	(120.56)	246.51	(242.63)
β_{12} (45th month)	35.33	(54.82)	32.74	(160.92)	-76.96	(130.87)	386.60***	(132.28)	141.33	(252.97)
β_{13} (48th month)	6.95	(59.68)	54.58	(177.61)	-106.50	(138.89)	425.88***	(142.90)	208.54	(293.75)
β_{14} (51st month)	42.17	(64.16)	-79.17	(182.61)	5.82	(145.02)	406.25***	(148.74)	57.75	(316.93)
β_{15} (54th month)	44.55	(67.28)	55.29	(196.94)	-11.51	(156.12)	380.20**	(161.82)	-166.62	(325.32)
β_{16} (57th month)	38.68	(70.00)	-83.01	(203.98)	9.00	(158.97)	370.78**	(165.56)	-178.59	(334.50)
β_{17} (60th month)	-9.53	(72.89)	32.03	(211.16)	-87.53	(170.67)	347.08**	(170.28)	-250.63	(367.59)
Eligible	372.74***	(44.51)	227.28*	(120.30)	71.48	(104.10)	-181.27*	(106.51)	724.51***	(238.76)
Age	310.84***	(23.30)	340.58***	(59.99)	278.92***	(44.04)	288.28***	(63.65)	292.98**	(135.97)
Age2	-3.48***	(0.36)	-4.10***	(0.94)	-3.60***	(0.67)	-3.60***	(0.93)	-3.20	(2.02)
Level of education										
No qualification										
High School	630.06***	(47.98)	510.96***	(119.11)	588.58***	(98.33)	577.75***	(110.04)	199.97	(298.50)
Graduate	1621.63***	(65.75)	1391.88***	(187.78)	942.32***	(178.94)	1281.40***	(119.31)	949.21***	(361.57)
Post-Graduate	2813.96***	(155.92)	2603.56***	(346.65)	1002.01***	(291.95)	1555.27***	(163.70)	2762.00***	(736.43)
Constant	-3956.00***	(386.90)	-4519.52***	(969.20)	-2877.35***	(753.32)	-3058.94***	(1121.64)	-3851.94***	(2339.45)
N	471135		32454		31584		65493		13176	
R-squared	0.116		0.1546		0.07		0.0785		0.1639	

Note: This model relies on Eq.(3). The estimates are based on the sample from the 12th to 60th month, and are estimated using 3-month average. The model is evaluated by subgroups of ethnicity, and includes controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. *DD*-effect is the interaction variable between *Eligible* and the period. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

Table A4. DD-regressions of monthly earnings using the 3-month average multi-period model by levels of education

Levels of education	No Qualification		High School Qualification		Graduate Qualification		Postgraduate Qualification	
DD-effects								
β_1 (12th month)	-2.18	*(95.06)*	-91.31	*(58.21)*	-91.91	*(109.54)*	-159.83	*(238.25)*
β_2 (15th month)	33.53	*(87.95)*	23.24	*(45.06)*	-29.02	*(87.12)*	190.81	*(217.38)*
β_3 (18th month)	33.56	*(82.44)*	30.01	*(44.81)*	-25.69	*(86.89)*	281.84	*(206.37)*
β_4 (21st month)	115.21	*(85.50)*	50.63	*(43.26)*	14.25	*(85.35)*	226.89	*(190.35)*
β_5 (24th month)	73.88	*(80.12)*	41.26	*(42.76)*	-20.94	*(83.90)*	408.83**	*(199.81)*
β_6 (27th month)	124.90	*(85.78)*	105.37**	*(42.66)*	51.89	*(85.87)*	391.16*	*(211.36)*
β_7 (30th month)	46.50	*(84.10)*	70.94	*(43.33)*	91.74	*(83.80)*	284.46	*(213.36)*
β_8 (33rd month)	81.10	*(83.26)*	90.59**	*(43.64)*	91.52	*(87.94)*	212.73	*(219.54)*
β_9 (36th month)	99.47	*(83.64)*	72.31	*(45.20)*	114.91	*(92.24)*	212.53	*(236.12)*
β_{10} (39th month)	82.39	*(88.67)*	58.40	*(47.87)*	89.14	*(95.66)*	364.29	*(259.83)*
β_{11} (42nd month)	68.09	*(99.780*	59.13	*(49.77)*	89.88	*(103.82)*	373.10	*(256.89)*
β_{12} (45th month)	-13.14	*(104.68)*	72.00	*(53.38)*	79.70	*(113.43)*	450.39	*(281.93)*
β_{13} (48th month)	58.56	*(109.11)*	49.15	*(57.99)*	33.09	*(122.57)*	511.16	*(317.94)*
β_{14} (51st month)	77.83	*(124.16)*	84.38	*(62.15)*	51.20	*(129.42)*	308.80	*(337.79)*
β_{15} (54th month)	-13.80	*(125.76)*	64.16	*(65.90)*	83.86	*(135.23)*	538.52	*(358.00)*
β_{16} (57th month)	-43.44	*(135.20)*	63.95	*(67.95)*	119.46	*(140.36)*	222.48	*(377.59)*
β_{17} (60th month)	-61.64	*(140.76)*	12.33	*(70.79)*	73.26	*(144.96)*	268.16	*(392.20)*
Eligible	225.11***	*(82.51)*	313.39***	*(42.17)*	310.85***	*(93.83)*	0.88	*(246.71)*
Age	185.65***	*(35.78)*	326.16***	*(21.69)*	559.95***	*(49.93)*	686.68***	*(153.92)*
Age2	-2.29***	*(0.550*	-3.95***	*(0.340*	-6.57***	*(0.76)*	-7.38***	*(2.20)*
Ethnicity								
European	Base		Base		Base		Base	
Māori	147.90	*(110.14)*	14.94	*(68.12)*	6.06	*(167.43)*	155.50	*(360.44)*
Pacific	292.56***	*(95.05)*	198.07***	*(56.59)*	-319.79*	*(165.68)*	-751.01***	*(198.51)*
Asian	109.88	*(103.09)*	-16.65	*(63.56)*	-342.10***	*(91.65)*	-1216.94***	*(199.90)*
Other ethnicity	201.91	*(264.56)*	-87.03	*(117.00)*	-291.77	*(248.03)*	258.96	*(690.63)*
Constant	-1434.02	*(617.43)*	-3274.96	*(355.22)*	-7268.67	*(830.14)*	-9507.31	*92783.45)*
N	55623		378321		149133		30765	
R-squared	0.0372		0.0438		0.0492		0.0726	

Note: This model relies on Eq.(3). The estimates are based on the sample from the 12th to 60th month, and are estimated using 3-month average. The model is evaluated by subgroups of education levels, and includes controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. *DD*-effect is the interaction variable between *Eligible* and the period. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

Table A5. DD-regressions of monthly earnings using the 3-month average multi-period model by age at birth

Age of the mother at birth	20-24		25-29		30-34		35-50	
DD-effects								
β_1 *(12th month)*	72.23	*(91.93)*	8.50	*(87.52)*	-110.38	*(80.67)*	-161.60	*(88.37)*
β_2 *(15th month)*	85.80	*(74.22)*	88.58	*(72.02)*	0.89	*(62.50)*	-28.79	*(70.98)*
β_3 *(18th month)*	47.51	*(75.49)*	59.43	*(71.21)*	73.50	*(63.32)*	-33.91	*(69.36)*
β_4 *(21st month)*	54.20	*(75.65)*	78.02	*(70.16)*	82.73	*(60.09)*	14.53	*(67.76)*
β_5 *(24th month)*	22.42	*(74.52)*	64.50	*(69.42)*	83.81	*(59.67)*	3.45	*(67.34)*
β_6 *(27th month)*	71.26	*(76.16)*	141.94**	*(71.81)*	147.93**	*(60.42)*	68.41	*(67.77)*
β_7 *(30th month)*	48.25	*(74.40)*	89.52	*(69.09)*	142.39**	*(61.00)*	41.01	*(68.09)*
β_8 *(33rd month)*	125.92*	*(75.19)*	203.04***	*(72.14)*	81.96	*(61.22)*	61.06	*(70.17)*
β_9 *(36th month)*	34.14	*(74.54)*	160.01**	*(73.55)*	124.55*	*(64.03)*	57.99	*(73.66)*
β_{10} *(39th month)*	34.93	*(81.67)*	146.90*	*(76.71)*	107.45	*(67.41)*	62.90	*(77.81)*
β_{11} *(42nd month)*	0.37	*(86.23)*	172.38**	*(79.54)*	140.16**	*(71.08)*	35.80	*(82.02)*
β_{12} *(45th month)*	-48.71	*(92.29)*	237.18***	*(85.12)*	152.06**	*(76.71)*	6.43	*(88.62)*
β_{13} *(48th month)*	-109.83	*(101.37)*	210.32**	*(94.28)*	89.16	*(81.86)*	49.61	*(97.44)*
β_{14} *(51st month)*	-48.64	*(104.51)*	259.23***	*(100.39)*	155.26*	*(87.73)*	-3.08	*(104.33)*
β_{15} *(54th month)*	-33.11	*(107.07)*	224.28**	*(102.02)*	196.95**	*(93.71)*	-22.79	*(110.18)*
β_{16} *(57th month)*	-132.62	*(113.06)*	256.44**	*(105.74)*	180.41**	*(97.73)*	-29.50	*(113.76)*
β_{17} *(60th month)*	-190.08	*(118.68)*	245.41**	*(110.83)*	133.92	*(100.38)*	-74.51	*(119.07)*
Eligible	189.01***	*(56.67)*	198.15***	*(62.84)*	237.84***	*(65.51)*	383.67***	*(72.32)*
Age	285.13**	*(115.6)2*	170.05	*(119.59)*	413.19***	*(132.87)*	381.93***	*(122.16)*
Age2	-3.95	*(2.57)*	-1.16	*(2.15)*	-4.75**	*(2.00)*	-4.84***	*(1.48)*
Ethnicity								
European	Base		Base		Base		Base	
Māori	82.84	*(84.17)*	-29.67	*(97.12)*	322.13***	*(116.28)*	-184.43	*(131.56)*
Pacific	405.68***	*(80.44)*	319.52***	*(90.90)*	127.90	*(90.48)*	-104.50	*(91.10)*
Asian	415.01***	*(117.86)*	-23.98	*(88.88)*	-147.67*	*(80.43)*	-540.56***	*(92.30)*
Other ethnicity	108.31	*(297.97)*	-347.71**	*(147.68)*	23.16	*(191.27)*	-108.99	*(198.02)*
Level of education								
No qualification	Base		Base		Base		Base	
High School	252.22***	*(67.06)*	605.82***	*(70.95)*	695.54***	*(64.95)*	666.11***	*(84.30)*
Graduate	408.18***	*(102.64)*	1243.49***	*(97.43)*	1635.28***	*(87.10)*	1845.22***	*(105.31)*
Post-Graduate	607.36	*(400.94)*	1590.27***	*(239.27)*	2343.38***	*(157.52)*	2934.32***	*(186.80)*
Constant	-2982.59**	*(1313.83)*	-1610.40	*(1682.27)*	-5861.40***	*(2237.49)*	-4968.06**	*(2494.11)*
N	59028		112704		211629		230478	
R-squared	0.0533		0.073		0.0813		0.0873	

Note: This model relies on Eq.(3). The estimates are based on the sample from the 12th to 60th month, and are estimated using 3-month average. The model is evaluated by subgroups of age at birth, and includes controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. *DD*-effect is the interaction variable between *Eligible* and the period. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

Table A6. DD- and DDD-Regression of Monthly Earnings for the pre-trend period

DD		DDD	
Eligible	209.58**	*Eligible*	198.39***
	94.27		*72.07*
Trend	-0.11	*Trend*	9.61***
	3.19		*2.11*
		Mother	-4.38
			-1.51
		*Eligible*Trend*	4.91
			3.49
		*Mother*Trend*	-0.36
			0.57
		*Eligible*Mother*	-12.51
			81.23
Trend*Eligible	**7.19**	***Trend*Eligible*Mothe***	**1.22**
	4.51		*3.96*
Constant	2919.18***		2772.80***
	67.10		*43.37*
R-squared	0.01		0.01
N	112923.00		181896.00

Note: Shows the trend test over the pre-period for the DD and the DDD models. The variable *Trend* is a time trend for the pre-period per month, and identifies common trend between the groups. The interaction variable *Trend*Eligible* captures the difference in trends between the groups. The pre-period ranges from the 21st month through the 12th month prior to birth. The earnings are in constant 2006 dollars.

Table A7. DDD-regressions of monthly earnings using the two-periods model

	DDD (12th–60th month)			DDD (24th–60th month)		
	1	3	4	6	8	9
Eligible*Mother*Post	**14.50**	**14.88**	**16.45**	**33.74**	**32.15**	**33.13**
	(40.69)	*(39.70)*	*(39.73)*	*(43.23)*	*(42.15)*	*(42.16)*
Eligible	298.85***	273.54***	235.66***	298.85***	273.47***	233.60***
	(31.24)	*(28.43)*	*(29.21)*	*(31.24)*	*(28.44)*	*(29.59)*
Post	561.85***	294.73***	389.55***	629.87***	345.86***	387.54***
	(16.74)	*(17.99)*	*(26.19)*	*(18.36)*	*(19.88)*	*(27.52)*
Mother	-6.60***	-0.69	-0.67	-6.60	-0.70	-0.64
	(32.47)	*(29.81)*	*(29.80)*	*(32.47)*	*(29.81)*	*(29.81)*
*Eligible*Post*	71.31***	67.51***	105.95***	55.91**	50.99**	102.65***
	(23.90)	*(23.25)*	*(23.77)*	*(26.07)*	*(25.33)*	*(27.29)*
*Mother*Post*	-693.14***	-725.12***	-730.33***	-703.78***	-728.77***	-730.80***
	(28.56)	*(27.87)*	*(27.91)*	*(30.35)*	*(29.61)*	*(29.64)*
*Eligible*Mother*	11.83	11.68	11.61	11.83	11.68	11.61
	(44.98)	*(40.84)*	*(40.83)*	*(44.98)*	*(40.84)*	*(40.84)*
Age		291.91***	298.22***		288.79***	292.13***
		(15.00)	*(15.15)*		*(14.67)*	*(14.77)*
Age²		-3.41***	-3.54***		-3.38***	-3.45***
		(0.23)	*(0.23)*		*(0.22)*	*(0.23)*
Ethnicity						
European		Base	Base		Base	Base
Māori		-118.41***	-122.68***		-134.29***	-136.44***
		(43.75)	*(43.78)*		*(43.86)*	*(43.88)*
Pacific		-28.08	-28.27		-42.79	-42.36
		(37.44)	*(37.43)*		*(37.71)*	*(37.70)*
Asian		-320.62***	-320.91***		-338.71***	-338.70***
		(38.32)	*(38.33)*		*(38.43)*	*(38.42)*
Other ethnicity		-344.45***	-343.83***		-339.03***	-339.00***
		(75.11)	*(75.15)*		*(75.24)*	*(75.27)*
Level of education						
No qualification		Base	Base		Base	Base
High School qualification		696.49***	697.47***		697.83***	698.42***
		(30.54)	*(30.51)*		*(30.65)*	*(30.64)*
Graduate qualification		1595.76***	1599.75***		1618.13***	1620.23***
		(40.53)	*(40.53)*		*(40.72)*	*(40.73)*
Post-Graduate qualification		2161.77***	2172.88***		2193.82***	2199.29***
		(73.89)	*(74.00)*		*(74.64)*	*(74.69)*
Time fixed effects			X			X
Unemployment rate			X			X
Constant	2970.28***	-3591.96***	-3489.20***	2970.28***	-3521.02***	-3569.56***
	(22.58)	*(245.16)*	*(248.98)*	*(22.58)*	*(239.65)*	*(240.64)*
R-squared	0.0313	0.1314	0.1328	0.0318	0.1324	0.133
N	909,432	909,432	909,432	734,808	734,808	734,808

Note: This model relies on Eq.(7). The estimates in columns (1) through (3) are based on the sample from the 12th to 60th month, while the estimates in columns (4) through (6) are based on the sample from the 24th to 60th month. Column (1) and (4) presents a simple OLS model with no control. In columns (2) and (5) controls are added for individual characteristics, with respect to age, age squared, level of education, and ethnicity. Columns (3) and (6) present the specification with controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. The variable *Post* is a dummy variable: 1=period after birth, 0 otherwise. The variable *Mother* is a dummy variable: 1=mother group, 0 otherwise. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

Table A8. DDD-regressions of monthly earnings using the multi-period model

Monthly			3-months average		
	DDD-effects	*Std. Err.*		*DDD-effects*	*Std. Err.*
β_1 *(12th month)*	-107.89**	*(49.99)*	β_1 *(12th month)*	-106.12***	*(49.99)*
β_2 *(13th month)*	-56.13	*(49.37)*	β_2 *(15th month)*	-32.16	*(43.74)*
β_3 *(14th month)*	-18.05	*(49.01)*	β_3 *(18th month)*	-19.30	*(44.37)*
β_4 *(15th month)*	-27.26	*(47.64)*	β_4 *(21st month)*	-21.06	*(43.02)*
β_5 *(16th month)*	-30.96	*(48.51)*	β_5 *(24th month)*	-14.62	*(42.91)*
β_6 *(17th month)*	31.79	*(52.56)*	β_6 *(27th month)*	18.32	*(43.60)*
β_7 *(18th month)*	-63.85	*(47.39)*	β_7 *(30th month)*	10.08	*(43.31)*
β_8 *(19th month)*	-21.31	*(46.79)*	β_8 *(33rd month)*	19.55	*(43.19)*
β_9 *(20th month)*	-25.08	*(47.41)*	β_9 *(36th month)*	8.10	*(43.36)*
β_{10} *(21st month)*	-21.93	*(46.61)*	β_{10} *(39th month)*	14.01	*(43.68)*
β_{11} *(22nd month)*	-15.21	*(47.02)*	β_{11} *(42nd month)*	38.23	*(44.55)*
β_{12} *(23rd month)*	-15.67	*(47.23)*	β_{12} *(45th month)*	42.41	*(44.13)*
β_{13} *(24th month)*	-17.97	*(45.80)*	β_{13} *(48th month)*	33.94	*(44.60)*
β_{14} *(25th month)*	-2.01	*(47.81)*	β_{14} *(51st month)*	60.26	*(44.70)*
β_{15} *(26th month)*	7.06	*(46.90)*	β_{15} *(54th month)*	48.68	*(44.97)*
β_{16} *(27th month)*	44.75	*(48.95)*	β_{16} *(57th month)*	55.38	*(44.52)*
β_{17} *(28th month)*	6.47	*(46.98)*	β_{17} *(60th month)*	50.91	*(44.97)*
β_{18} *(29th month)*	10.50	*(47.00)*			
β_{19} *(30th month)*	8.13	*(47.18)*			
β_{20} *(31st month)*	42.16	*(46.73)*			
β_{21} *(32nd month)*	8.37	*(46.74)*			
β_{22} *(33rd month)*	3.15	*(46.59)*			
β_{23} *(34th month)*	12.09	*(47.26)*			
β_{24} *(35th month)*	1.33	*(46.44)*			
β_{25} *(36th month)*	6.05	*(47.35)*			
β_{26} *(37th month)*	3.35	*(48.20)*			
β_{27} *(38th month)*	8.51	*(46.64)*			
β_{28} *(39th month)*	25.37	*(47.84)*			
β_{29} *(40th month)*	19.22	*(48.48)*			
β_{30} *(41st month)*	57.24	*(49.75)*			
β_{31} *(42nd month)*	33.73	*(48.86)*			
β_{32} *(43rd month)*	55.20	*(47.74)*			
β_{33} *(44th month)*	27.07	*(48.23)*			
β_{34} *(45th month)*	40.43	*(47.51)*			
β_{35} *(46th month)*	6.93	*(47.72)*			
β_{36} *(47th month)*	15.21	*(47.82)*			
β_{37} *(48th month)*	75.43	*(50.19)*			
β_{38} *(49th month)*	59.10	*(49.08)*			
β_{39} *(50th month)*	72.35	*(48.08)*			
β_{40} *(51st month)*	45.03	*(47.70)*			
β_{41} *(52nd month)*	38.88	*(48.84)*			
β_{42} *(53rd month)*	68.81	*(49.14)*			
β_{43} *(54th month)*	34.14	*(47.61)*			
β_{44} *(55th month)*	46.00	*(48.48)*			
β_{45} *(56th month)*	62.81	*(47.35)*			
β_{46} *(57th month)*	53.22	*(48.70)*			
β_{47} *(58th month)*	35.60	*(48.31)*			
β_{48} *(59th month)*	39.08	*(48.77)*			
β_{49} *(60th month)*	74.06	*(48.67)*			
Eligible	228.64***	*(29.35)*		224.50***	*(29.47)*
Mother	-0.66	*(29.80)*		0.04	*(29.93)*
*Eligible*post*	112.91***	*(23.92)*		118.12****	*(23.96)*
*Mother*post*	-730.10***	*(27.92)*		-730.63***	*(28.01)*
*Eligible*Mother*	11.61	*(40.83)*		10.11	*(40.84)*
Age	298.27***	*(15.15)*		299.68***	*(14.61)*
Age2	-3.54***	*(0.23)*		-3.56***	*(0.22)*
Ethnicity					
European					
Māori	-122.74***	*(43.79)*		-127.18***	*(43.25)*
Pacific	-28.30	*(37.44)*		-34.52	*(36.99)*
Asian	-320.81***	*(38.34)*		-332.56***	*(37.89)*
Other ethnicity	-343.85***	*(75.16)*		-343.58***	*(74.39)*
Level of education					
No qualification					
High School	697.50***	*(30.52)*		694.81***	*(30.15)*
Graduate	1599.78***	*(40.54)*		1589.94***	*(40.06)*
Post-Graduate	2172.94***	*(74.01)*		2157.06***	*(73.180*
Time effects	X			X	
Unemployment	X			X	
Constant	-3457.22***	*(249.46)*		-3469.15***	*(239.58)*
R-squared	0.1328			0.1334	
N	909,432			941,088	

Note: This model relies on Eq.(8). The estimates are based on the sample from the 12th to 60th month. The first model uses monthly period, and the second uses 3-month average. Both models are evaluated with controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. The variable *Post* is a dummy variable: 1=period after birth, 0 otherwise. The variable *Mother* is a dummy variable: 1=mother group, 0 otherwise. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

Table A9. DDD-regressions of monthly earnings using the 3-month average multi-period model by ethnicity

Ethnicity	European		Māori		Pacific		Asian		Other ethnicity	
DDD-effects										
β_1 (12th month)	-184.36***	(58.69)	113.14	(195.45)	-75.40	(151.18)	-42.17	(143.54)	462.55	(319.07)
β_2 (15th month)	-102.26**	(51.37)	-5.80	(156.23)	-39.80	(131.30)	105.26	(126.92)	452.73*	(267.98)
β_3 (18th month)	-73.97	(52.13)	-81.06	(158.65)	-136.02	(141.08)	99.14	(127.70)	519.02*	(270.41)
β_4 (21st month)	-81.06	(49.99)	-92.86	(156.23)	-126.06	(137.55)	152.06	(130.74)	422.57	(267.95)
β_5 (24th month)	-76.41	(49.98)	-109.67	(153.60)	-123.82	(137.11)	211.83*	(128.69)	371.00	(268.58)
β_6 (27th month)	-42.52	(50.68)	-138.04	(150.36)	-25.27	(142.22)	233.53*	(133.61)	406.05	(273.55)
β_7 (30th month)	-35.20	(50.54)	-70.52	(160.79)	-121.25	(136.20)	143.99	(129.51)	378.24	(269.68)
β_8 (33rd month)	-22.61	(50.50)	-7.33	(157.81)	-166.20	(131.63)	119.98	(129.99)	349.92	(268.44)
β_9 (36th month)	-44.95	(50.49)	9.33	(156.57)	-154.12	(135.35)	161.90	(131.94)	309.49	(279.81)
β_{10} (39th month)	-39.23	(50.97)	-27.90	(155.85)	-139.27	(134.28)	146.09	(133.25)	562.61**	(283.04)
β_{11} (42nd month)	0.38	(52.16)	-103.68	(158.58)	-123.34	(141.72)	107.90	(132.36)	613.54**	(282.78)
β_{12} (45th month)	-7.70	(51.44)	-8.57	(159.06)	-111.99	(137.49)	153.16	(133.88)	535.57*	(280.36)
β_{13} (48th month)	-16.61	(51.78)	-52.54	(168.43)	-97.83	(138.47)	193.33	(136.66)	399.65	(282.00)
β_{14} (51st month)	16.62	(52.08)	-54.55	(162.80)	-42.58	(138.15)	166.38	(135.85)	456.52	(278.74)
β_{15} (54th month)	15.34	(52.42)	16.02	(167.78)	-47.18	(140.52)	47.74	(135.56)	410.48	(273.99)
β_{16} (57th month)	20.35	(51.73)	-118.12	(164.88)	7.68	(139.00)	107.08	(137.97)	417.64	(275.67)
β_{17} (60th month)	20.69	(52.13)	-26.67	(171.82)	-69.99	(144.03)	61.77	(139.21)	407.97	(287.55)
Eligible	293.30***	(34.25)	98.96	(102.05)	-18.86	(85.93)	-46.23	(87.65)	180.62	(192.09)
Mother	1.28	(33.97)	-40.47	(108.70)	-42.48	(99.54)	84.92	(104.34)	-132.99	(195.72)
*Eligible*Post*	109.07***	(27.71)	71.71	(88.42)	98.60	(77.48)	224.17***	(74.48)	-151.27	(161.81)
*Mother*Post*	-818.78***	(31.66)	-322.90***	(102.02)	-208.48***	(95.23)	-329.70***	(98.69)	-975.79***	(191.74)
*Eligible*Mother*	10.16	(47.35)	83.85	(143.99)	63.49	(126.30)	-95.64	(127.06)	228.19	(265.22)
Age	301.66***	(17.32)	332.73***	(51.71)	312.36***	(33.10)	228.04	(48.79)	325.71	(75.86)
Age2	-3.54***	(0.27)	-4.19***	(0.82)	-4.03***	(0.50)	-2.82	(0.72)	-3.79	(1.13)
Level of education										
No qualification	Base		Base		Base		Base		Base	
High School	721.60***	(36.40)	563.64***	(94.65)	546.59***	(76.45)	819.00***	(97.41)	147.28	(193.91)
Graduate	1667.36***	(47.71)	1042.57***	(154.45)	848.60***	(146.52)	1602.68***	(102.73)	705.54***	(241.11)
Post-Graduate	2358.99***	(91.17)	2211.84***	(322.92)	1183.31***	(254.53)	1661.62***	(129.17)	1455.73***	(449.05)
Constant	-3560.39***	(283.73)	-4046.68***	(823.98)	-3242.96***	(556.74)	-2454.99***	(843.43)	-3744.02***	(1276.05)
N	743,733		46,914		45,444		88,044		16,956	
R-squared	0.1408		0.1232		0.0922		0.1073		0.1362	

Note: This model relies on Eq.(8). The estimates are based on the sample from the 12th to 60th month, and use 3-month average. The model is evaluated by subgroups of ethnicity, and includes controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. The variable *Post* is a dummy variable: 1=period after birth, 0 otherwise. The variable *Mother* is a dummy variable: 1=mother group, 0 otherwise. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

Table A10. DDD-regressions of monthly earnings using the 3-month average multi-period model by levels of education

Levels of education	No Qualification		High School Qualification		Graduate Qualification		Postgraduate Qualification	
DDD-effects								
β_1 (12th month)	-98.99	(117.46)	-54.02	(59.89)	-173.85	(119.58)	-269.97	(265.13)
β_2 (15th month)	-5.66	(110.36)	16.93	(51.51)	-104.95	(105.88)	-186.21	(250.05)
β_3 (18th month)	-2.95	(104.59)	7.27	(52.48)	-83.03	(107.41)	42.07	(250.50)
β_4 (21st month)	47.84	(108.80)	5.93	(50.77)	-69.70	(104.57)	-93.63	(240.41)
β_5 (24th month)	67.85	(102.53)	6.58	(50.85)	-72.76	(104.20)	-8.04	(240.69)
β_6 (27th month)	45.08	(111.53)	49.23	(51.22)	-50.82	(105.52)	37.20	(256.25)
β_7 (30th month)	66.30	(109.64)	43.73	(51.25)	-58.86	(104.63)	-26.39	(245.45)
β_8 (33rd month)	56.40	(107.46)	51.27	(50.46)	-37.47	(105.96)	-35.00	(249.29)
β_9 (36th month)	109.54	(106.81)	32.77	(50.64)	-49.24	(105.99)	-77.52	(254.88)
β_{10} (39th month)	63.68	(107.23)	39.23	(50.83)	-42.30	(106.98)	-3.86	(261.24)
β_{11} (42nd month)	173.97	(116.84)	35.64	(51.64)	21.90	(109.64)	-6.30	(254.66)
β_{12} (45th month)	120.86	(111.13)	50.42	(51.17)	14.29	(108.30)	-2.88	(258.85)
β_{13} (48th month)	163.63	(108.58)	41.19	(52.03)	-38.71	(108.91)	151.76	(258.93)
β_{14} (51st month)	150.56	(112.29)	81.51	(51.56)	-5.00	(110.74)	-7.39	(257.38)
β_{15} (54th month)	132.39	(109.81)	47.01	(52.11)	-0.64	(109.93)	206.49	(264.57)
β_{16} (57th month)	99.94	(108.09)	60.42	(51.62)	17.52	(109.89)	113.79	(252.32)
β_{17} (60th month)	78.41	(113.60)	58.46	(52.00)	20.25	(110.69)	73.23	(259.84)
Eligible	214.83***	(66.96)	254.22***	(34.79)	246.36***	(72.72)	34.67	(163.67)
Mother	8.71	(66.27)	-8.82	(35.44)	22.45	(76.72)	38.20	(175.64)
*Eligible*post*	91.84	(57.64)	57.43**	(27.17)	216.59***	(62.52)	207.06	(138.86)
*Mother*post*	-576.10***	(62.18)	-746.50***	(32.43)	-801.11***	(72.71)	-573.40***	(180.37)
*Eligible*mother*	-11.01	(94.27)	22.50	(47.85)	-10.90	(102.03)	-46.63	(225.36)
Age	157.24***	(28.07)	315.17***	(16.34)	492.09***	(41.96)	624.84***	(94.13)
Age2	-2.00***	(0.43)	-3.88***	(0.25)	-5.88***	(0.63)	-7.10***	(1.30)
Ethnicity								
European	Base		Base		Base		Base	
Māori	60.57	(88.99)	-81.81	(50.56)	-487.74***	(138.32)	-32.48	(332.10)
Pacific	232.00***	(74.09)	19.23	(43.64)	-513.17***	(131.12)	-323.66	(230.71)
Asian	-217.97**	(93.83)	-191.17***	(50.91)	-385.19***	(70.18)	-994.94***	(128.61)
Other ethnicity	49.74	(230.54)	-252.67***	(85.64)	-651.94***	(169.43)	-511.64	(442.81)
Constant	-608.20	(459.52)	-2934.63***	(263.45)	-5618.57***	(690.44)	-8189.58	(1705.27)
N	85,020		597,573		217,305		41,193	
R-squared	0.0585		0.07		0.0784		0.0905	

Note: This model relies on Eq.(8). The estimates are based on the sample from the 12th to 60th month, and use 3-month average. The model is evaluated by subgroups of education levels, and includes controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. The variable *Post* is a dummy variable: 1=period after birth, 0 otherwise. The variable *Mother* is a dummy variable: 1=mother group, 0 otherwise. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

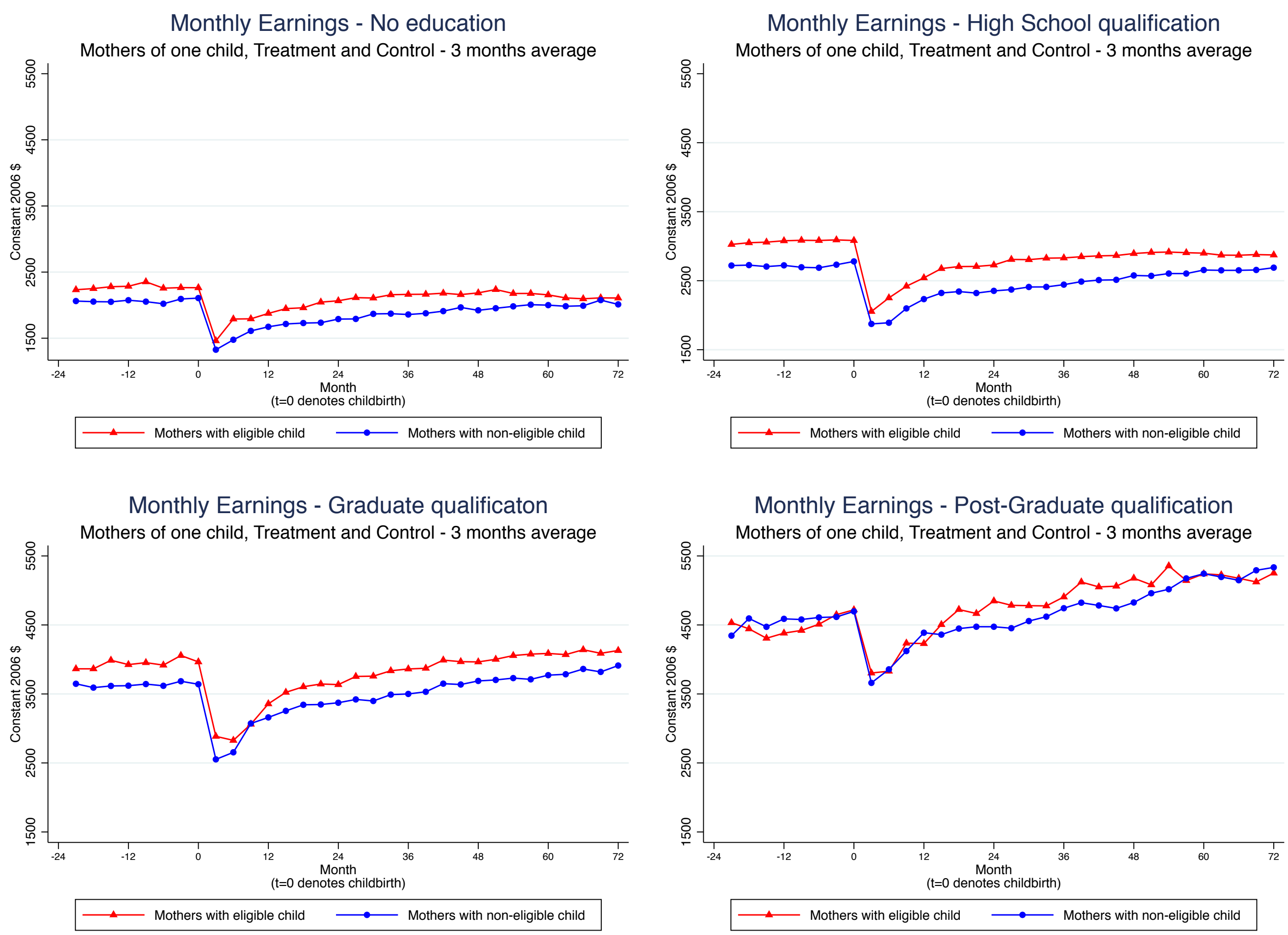
Table A11. DDD-regressions of monthly earnings using the 3-month average multi-period model by age at birth

Age of the mother at birth	20-24		25-29		30-34		35-50	
DDD-effects								
β_1 (12th month)	25.55	(115.85)	-107.44	(102.58)	-115.36	(89.19)	-129.06	(85.92)
β_2 (15th month)	78.11	(107.24)	-56.04	(90.97)	-61.99	(75.78)	-25.63	(76.17)
β_3 (18th month)	70.54	(107.89)	-59.15	(90.83)	-21.56	(77.19)	-22.03	(77.43)
β_4 (21st month)	66.30	(108.46)	-70.73	(91.02)	3.19	(74.72)	-37.92	(74.42)
β_5 (24th month)	80.22	(108.17)	-41.50	(91.12)	-13.31	(73.52)	-23.58	(75.00)
β_6 (27th month)	109.22	(110.36)	-19.70	(91.73)	52.38	(75.31)	-8.39	(75.91)
β_7 (30th month)	133.78	(109.89)	-22.85	(90.19)	54.87	(75.53)	-37.02	(75.24)
β_8 (33rd month)	234.11***	(111.26)	31.76	(92.56)	-7.58	(74.55)	-5.30	(74.83)
β_9 (36th month)	126.24	(108.37)	-30.38	(93.07)	18.70	(75.09)	-3.26	(75.23)
β_{10} (39th month)	104.87	(111.82)	-10.82	(91.97)	-5.28	(75.96)	30.25	(75.86)
β_{11} (42nd month)	109.67	(112.91)	5.14	(91.90)	37.15	(78.21)	44.56	(77.36)
β_{12} (45th month)	112.66	(114.76)	57.21	(93.63)	29.86	(76.27)	38.73	(76.54)
β_{13} (48th month)	38.40	(115.59)	41.59	(96.73)	27.33	(76.22)	42.26	(77.60)
β_{14} (51st month)	166.76	(115.17)	41.98	(94.75)	57.60	(77.04)	48.96	(77.81)
β_{15} (54th month)	160.03	(115.38)	-15.75	(94.41)	65.50	(78.35)	40.44	(77.91)
β_{16} (57th month)	128.66	(114.54)	-8.73	(94.62)	90.58	(77.03)	37.39	(77.35)
β_{17} (60th month)	143.55	(117.26)	0.57	(96.30)	59.86	(77.62)	46.07	(78.04)
Eligible	108.64***	(53.86)	174.72***	(52.67)	251.12***	(50.91)	263.69***	(54.83)
Mother	-10.75	(58.13)	-24.41	(51.60)	-10.09	(50.05)	23.54	(57.37)
*Eligible*post*	52.00	(65.31)	169.27***	(49.54)	112.59***	(41.95)	102.99**	(40.960
*Mother*post*	-793.50***	(77.77)	-620.25***	(55.29)	-751.75***	(46.61)	-756.03***	(50.54)
*Eligible*mother*	8.92	(72.45)	9.55	(72.19)	24.33	(70.62)	-3.42	(76.43)
Age	328.41***	(94.25)	176.72*	(92.13)	114.35	(95.87)	104.76	(96.17)
Age2	-4.83**	(2.11)	-1.50	(1.67)	-0.55	(1.47)	-1.43	(1.18)
Ethnicity								
European	Base		Base		Base		Base	
Māori	-94.78	(73.12)	-96.09	(78.96)	62.26	(80.21)	-350.79***	(100.18)
Pacific	137.35**	(66.80)	44.95	(68.96)	42.92	(75.37)	-130.17*	(67.97)
Asian	220.51**	(101.68)	-144.30*	(80.64)	-298.43***	(63.81)	-508.13*	(62.69)
Other ethnicity	-225.11	(166.06)	-404.92***	(155.31)	-488.39***	(136.34)	-226.60*	(123.55)
Level of education								
No qualification	Base		Base		Base		Base	
High School	287.38***	(55.43)	653.02***	(58.07)	772.30***	(52.38)	788.90***	(60.03)
Graduate	393.64***	(89.55)	1360.28***	(78.97)	1710.43***	(68.11)	1822.15***	(72.98)
Post-Graduate	-45.33	(389.74)	1470.95***	(224.58)	2149.29***	(116.82)	2449.03***	(107.59)
Constant	-3085.21***	(1066.30)	-1605.55	(1283.48)	-686.62	(1589.41)	657.75	(1934.99)
N	85,818		169,278		320,907		365,085	
R-squared	0.0533		0.1077		0.1096		0.1046	

Note: This model relies on Eq.(8). The estimates are based on the sample from the 12th to 60th month, and use 3-month average. The model is evaluated by subgroups of age at birth, and includes controls for individual characteristics, fixed time effects and unemployment rate. The variable *Eligible* is a dummy variable: 1=treatment group, 0 otherwise. The variable *Post* is a dummy variable: 1=period after birth, 0 otherwise. The variable *Mother* is a dummy variable: 1=mother group, 0 otherwise. Standard errors (SE) are clustered on the mother and robust to heteroscedasticity. N denotes the sample size. The earnings are in constant 2006 dollars. Statistical significance is denoted using asterisks: *** is p<0.01, ** is p<0.05 and * is p<0.1.

Annexes II – Figures

Figure A1. Monthly Earnings for the eligible and non-eligible mothers' groups by education levels



Note: Shows the evolution of the monthly earnings of mothers 24 months prior to birth to 72 months after birth for the treatment and control groups. The graphs are subdivided per education level. The earnings are in constant 2006 dollars, and in 3-month average.

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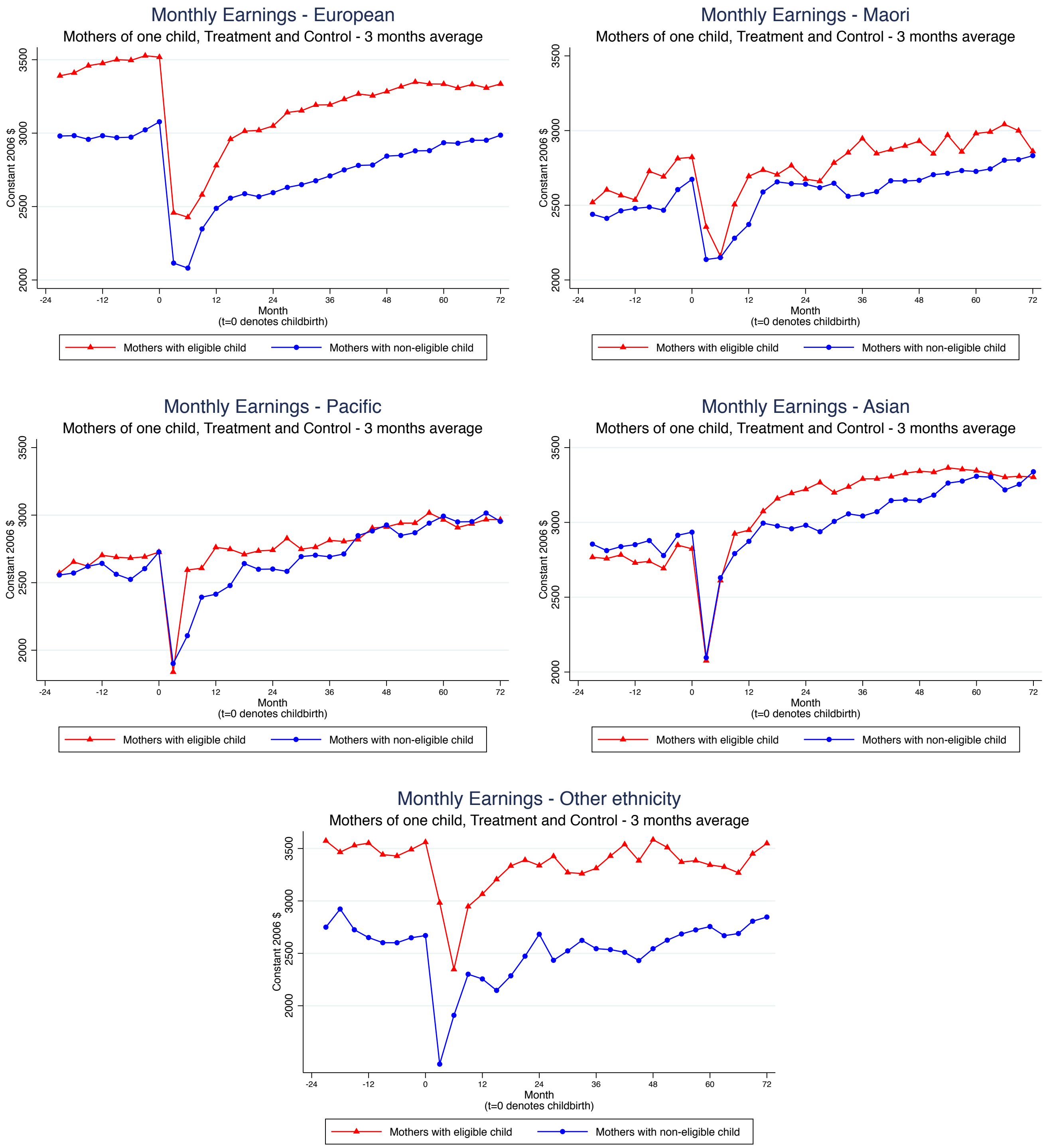
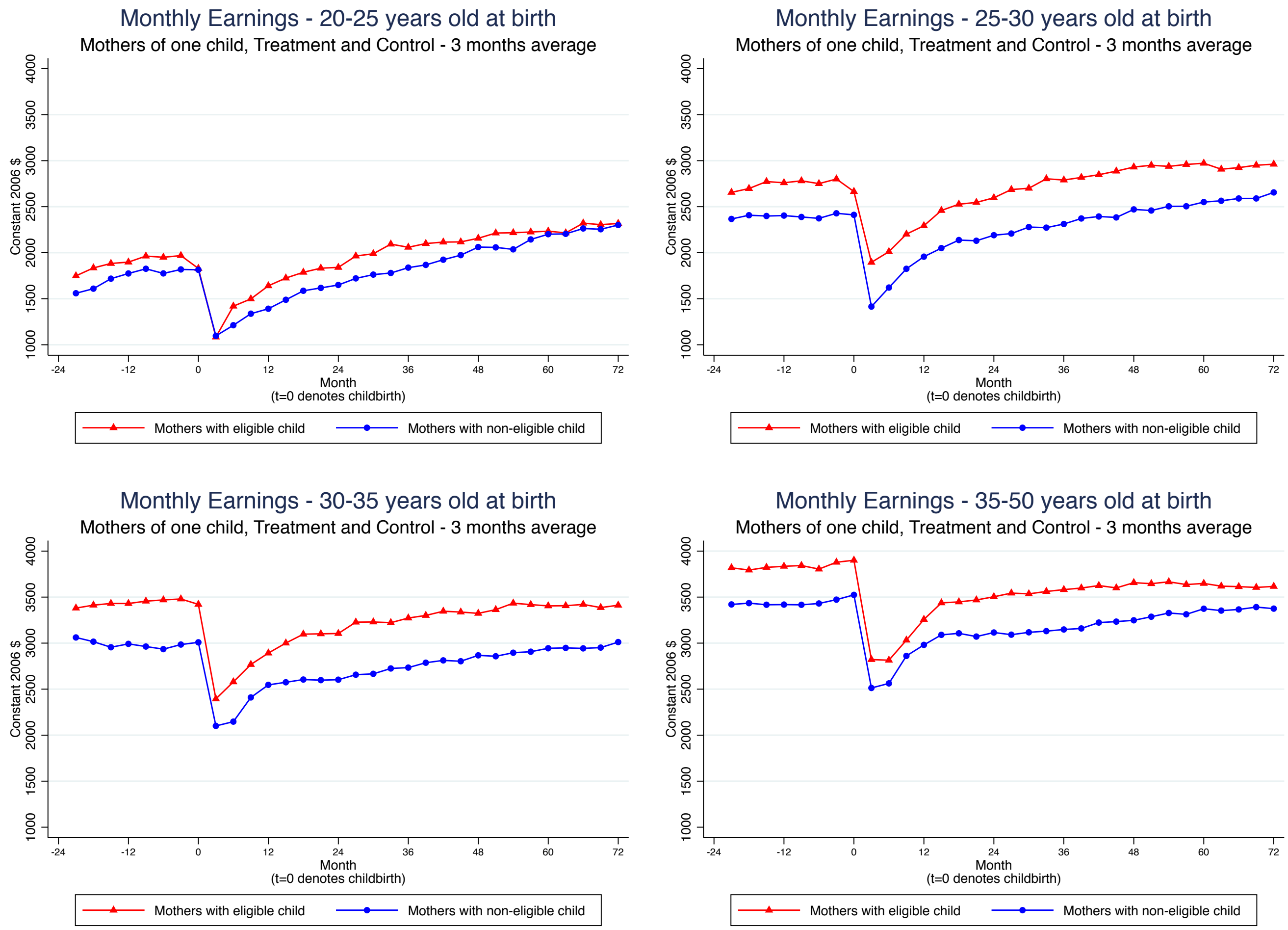
Figure A2. Monthly Earnings for the eligible and non-eligible mothers' groups by ethnicity



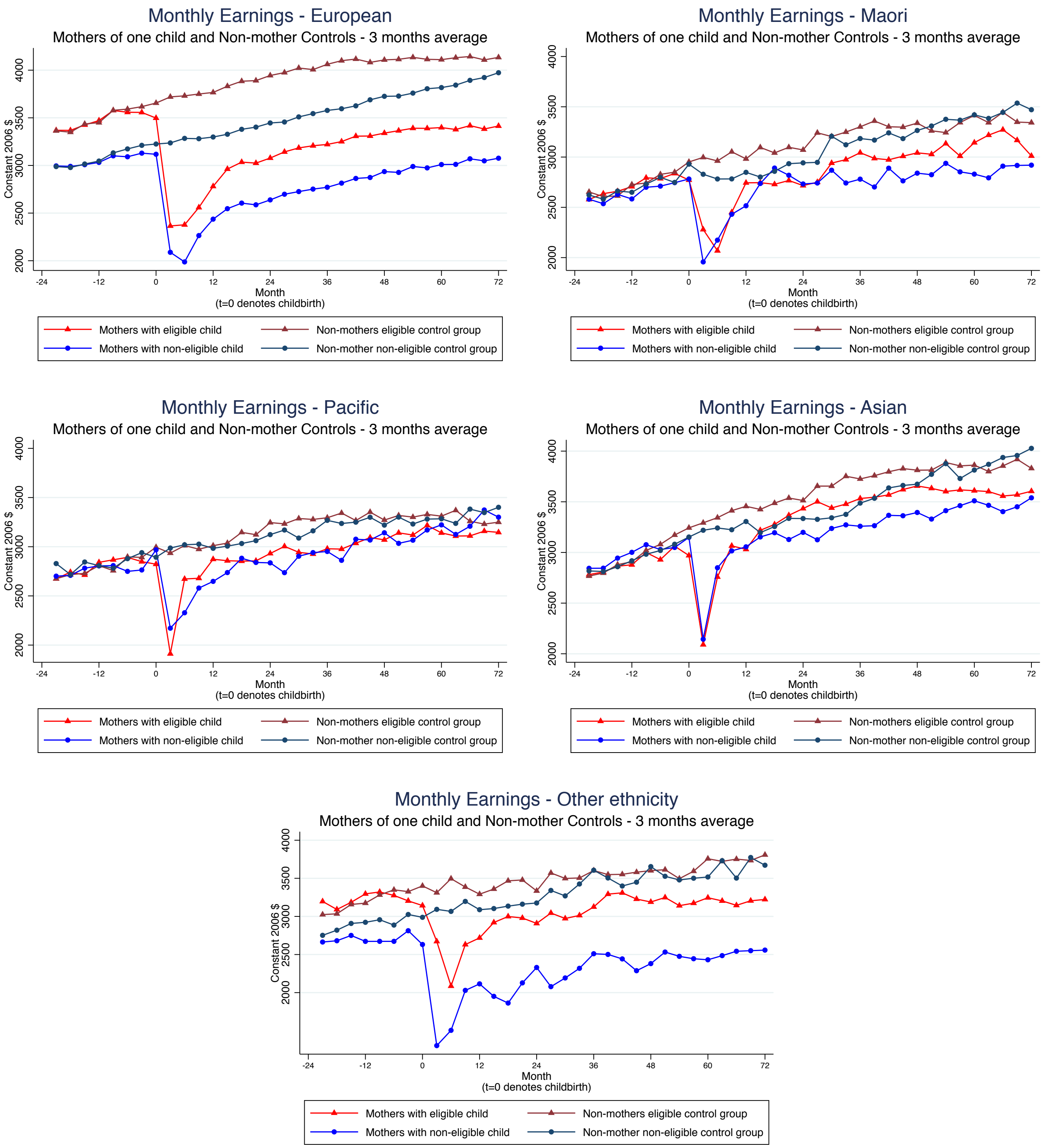
Note: Shows the evolution of the monthly earnings of mothers 24 months prior to birth to 72 months after birth for the treatment and control groups. The graphs are subdivided per ethnicity. The earnings are in constant 2006 dollars, and in 3-month average

Figure A3. Monthly Earnings for the eligible and non-eligible mothers' groups by age at birth



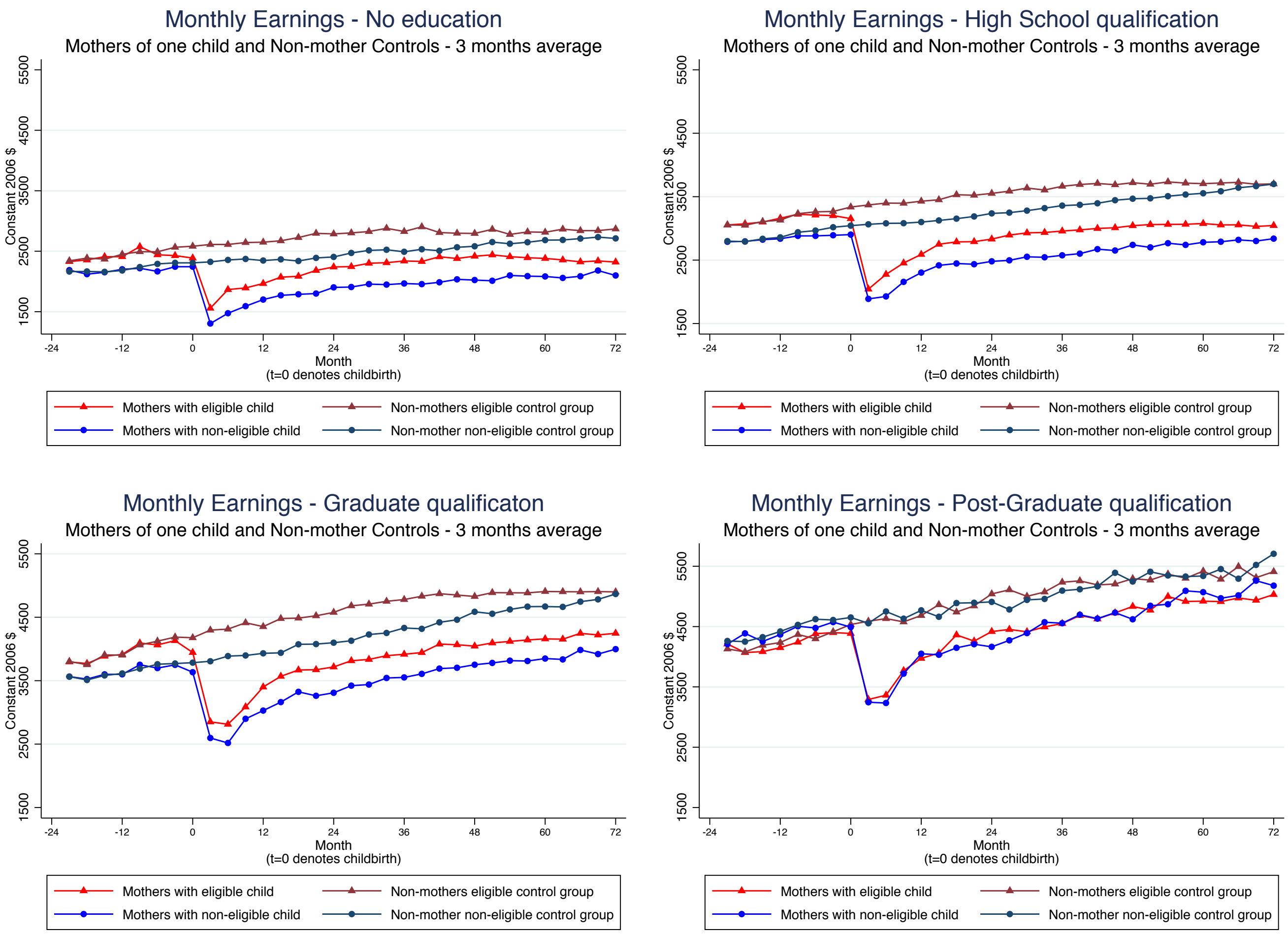
Note: Shows the evolution of the monthly earnings of mothers 24 months prior to birth to 72 months after birth for the treatment and control groups. The graphs are subdivided per age at birth groups. The earnings are in constant 2006 dollars, and in 3-month average.

Figure A4. Monthly Earnings for the mother and non-mother groups by eligibility and by ethnicity



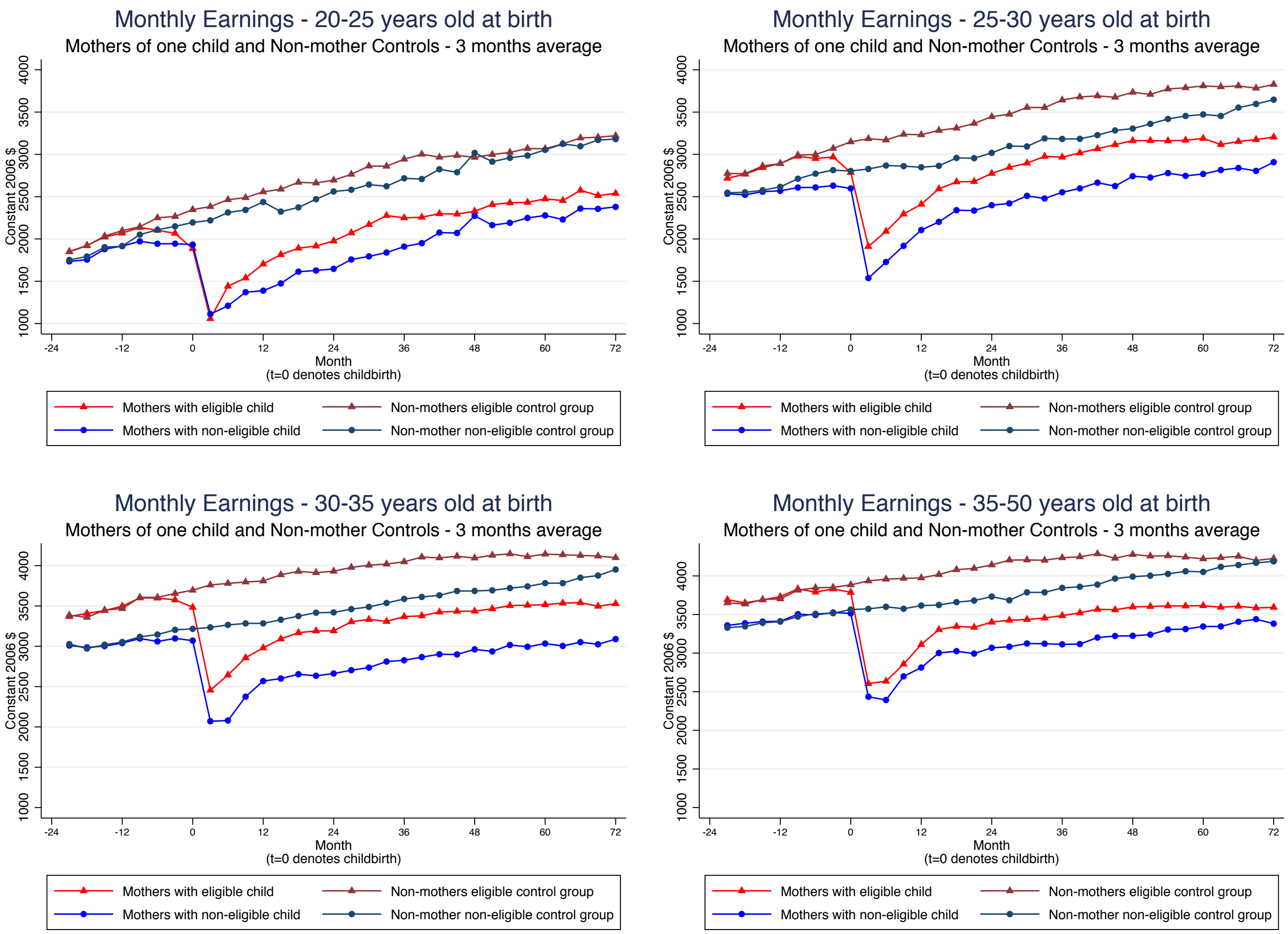
Note: Shows the evolution of the monthly earnings of mothers 24 months prior to birth to 72 months after birth. The groups are based on eligibility to the program and mother status. The graphs are subdivided per ethnicity. The earnings are in constant 2006 dollars, and in 3-month average.

Figure A5. Monthly Earnings for the mother and non-mother groups by eligibility and by education levels



Note: Shows the evolution of the monthly earnings of mothers 24 months prior to birth to 72 months after birth. The groups are based on eligibility to the program and mother status. The graphs are subdivided per level of education. The earnings are in constant 2006 dollars, and in 3-month average.

Figure A6. Monthly Earnings for the mother and non-mother groups by eligibility and by age group



Note: Shows the evolution of the monthly earnings of mothers 24 months prior to birth to 72 months after birth. The groups are based on eligibility to the program and mother status. The graphs are subdivided per age at birth groups. The earnings are in constant 2006 dollars, and in 3-month average.