

# **CHILDCARE REFORM: EFFECTS ON EARNINGS AND EMPLOYMENT AMONG NATIVE SWEDISH AND IMMIGRANT MOTHERS**

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**Running head:** Childcare Reform: Effects on Earnings and LFP

## **ABSTRACT**

This paper studies earnings and labor force participation of native Swedes and recent immigrants in Sweden in response to the childcare reforms of 2001 and 2002 using a difference-in-differences approach and register-based data for the period of 1995-2009. Immigrant and native Swedish mothers are distinguished in order to study if increased accessibility to childcare might be particularly beneficial for groups facing obstacles in entering the labor market. The results show that the reforms had a positive effect on earnings and labor force participation among native mothers with preschool children. The group of immigrant mothers studied did not experience any gain in labor market outcomes as a response to the reform.

**Keywords:** Childcare reform; Immigrant; Labor force participation; Earnings; Family policy; Difference-in-difference

**JEL Classification:** D14; J13; J15; J16

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## 1. INTRODUCTION

A large body of literature studying the integration processes of immigrants and their earnings development has grown following pioneering studies such as Chiswick (1978) and Borjas (1985, 1989). The early results presented by Chiswick (1978) – that immigrants start from low earnings and then show more rapid earnings growth compared to natives – are challenged in many subsequent papers (e.g. Borjas, 1985; Borjas, 1995). Borjas (1995) has shown that later cohorts of US immigrants had lower initial wages and experienced slower growth rates in earnings meaning that earlier cohorts of immigrants experienced faster economic integration. Slow integration processes are generally explained by educational differences and language and cultural constraints, but it is also unavoidable that discrimination in its different shapes pervades most questions concerning immigrants and the labor market.

Sweden is a country with high immigration represented by labor immigrants from Nordic countries and the EU15 as well as refugees from Asia and Africa, whose share has been growing lately (Schröder, 2007). Several studies have indicated adverse labor market outcomes for immigrants in Sweden compared to natives, including higher unemployment risks (Arai & Wilhelmsson, 2004) and lower earnings (Hammarstedt, 2003; Nekby, 2002). Integration processes also seem to be slow (Nekby, 2002). A few studies indicate the presence of discrimination in the Swedish labor market (e.g. Carlsson & Rooth, 2006).

Sweden is known for its high level of gender convergence in employment and education. This is, however, only applicable to native Swedes, and immigrants exhibit both lower female labor force participation (LFP) as well as greater gaps in earnings between men and women (Schröder, 2007). In this context, an important question to ask is how welfare systems seeking to provide equal opportunities for everyone affect labor market conditions for groups facing certain obstacles to entering the labor market.

One contribution to the Swedish family policy is the childcare reform that was enacted in 2001-2002. The main aim of the reform was to increase childcare participation among groups of families that traditionally had little access to childcare with the goal of improving opportunities to entering the labor market or educational programs. The reforms consisted of three components (Swedish National Agency for Education, 2007). First, a single national childcare fee system was introduced that restricted childcare costs to 1-3 percent of the family income. Second, the reforms introduced 15 hours of childcare per week for children whose parents were unemployed. Third, the reforms introduced a universal preschool system for children of 4-5 years of age.

The purpose of this paper is to study the development of earnings and labor force participation in relation to the reforms enacted in 2001-2002 by focusing on differences between immigrant and native Swedish families. This aspect of the reforms has not been considered in previous studies. Because the unemployment rate is higher in many immigrant groups compared to Swedes,

the potential for positive effects on earnings and LFP might be greater in the group of immigrant mothers. However, it is not necessarily the case that the reforms have reached these groups because of cultural dissimilarities and differences in customs. Therefore, the outcome of the paper is related to a discussion on how well social policy (childcare reform in this case) can contribute to the integration of immigrants. Improvement of social integration and further equalization of family and labor rights is one of the most important political issues in Sweden.

There is a fairly large number of papers concerned with evaluating policy changes in childcare. Blau and Currie (2006) contains a review of the early literature focusing mainly on estimating price elasticities of the demand for childcare. Belinski and Galiani (2007), Baker et al. (2008), Cascio (2009), Havnes and Mogstad (2011), and Cascio and Schanzenbach (2013) are examples of a more recent literature that exploits exogenous variation in various dimensions in order to pinpoint the effects of child care reforms. It is fair to say that the results from previous studies vary substantially; from finding no effects on labor market outcomes to relatively large effects. Havnes and Mogstad studied the expansion of subsidized childcare in Norway in the 1970's, and found small effects on employment of mothers with preschool children. One interpretation of their results is that subsidized childcare mainly crowded out informal care. Baker et al. (2008) studied the introduction of subsidized universally accessible childcare in Quebec. They found that enrollment rates, as well as employment, increased substantially. A minor part of the increase in enrollment was due to crowding out of informal care. Similar results were obtained by Berlinski and Galiani (2007) who studied an expansion of preschool facilities in Argentina.

A few studies have assessed different effects of the Swedish childcare reform of 2001-2002. Wikström (2007) analyzed the effects of changes in the childcare fee system on hours of care and participation in the system and found that participation, as well as the number of hours of care, became significantly more equally distributed after the reforms were enacted. Mainly, the increase in participation and hours were observed in the bottom of the distributions, which is consistent with the hypothesis that the accessibility part of the reform is driving the results. Hanes et al. (2009) studied how the fee changes affected the municipalities' decisions in terms of taxation and public expenditures and found that municipalities' pre-reform fee systems affected post-reform taxes and expenditures; childcare demand increased in municipalities using time rates, but income-dependent fees did not affect demand. Lundin et al. (2008) studied if the fee reductions implied by the reform had effects on employment among families with children 1-9 years of age. They found only small effects, and concluded that the economic effects were negligible. Finally, Mörk et al. (2013) studied how fee reductions affected fertility and found that the childcare reform accelerated first-births, but that the timing of the second birth was delayed. This

latter finding is consistent with the idea that families already having one child may increase spacing between births in order to be able to capture the fee reductions in full.

The empirical analysis in this paper is based on repeated cross-sectional data for the period 1995-2009 consisting of earnings, LFP, and other family characteristics for Swedish-born and immigrant families. We use a difference-in-difference approach to study labor market outcomes of women originating from Sweden and Iran/Iraq. The latter group is selected firstly because it is one of the largest groups among immigrants by country of origin with a relatively permanent immigration flow during the last decades. Secondly, immigrants from these countries are to a large extent refugees (in contrast to immigrants from the EU and other western countries), and the labor market outcomes of this group are known to be poor and therefore particularly interesting to study from a welfare perspective. In a first part of our analysis, we establish that the reform had positive effects on earnings and LFP among native mothers with children 2-5 years old (preschool children). Then in a second part, the differential effects of the group of immigrants are analyzed. Our results show that the reform package did not improve the labor market outcomes in the group of immigrant mothers with preschool children.

The rest of the paper is organized as follows. The next section presents the research strategy, including a description of the reform package as well as the periods of comparison, the hypotheses, and the empirical model. Section 3 presents the data and empirical results, and section 4 concludes the paper.

## **2. RESEARCH STRATEGY**

### *2.1. Policy Reform*

In Sweden, the provision of childcare (also known as preschool care) for children aged 1 to 5 years is delegated to the municipalities. Municipalities decide on the amount of spending on childcare as well as the user fees, and the central government finances part of the expenditures through intergovernmental grants. The participation in formal preschool care in Sweden is relatively high in an international perspective. According to surveys in 1999 and 2002, 73% of children aged 1-5 years participated in preschool in 1999 and 82% participated in 2002 (Wikström, 2007).

During the 1990s, it was noticed that the level of user fees had increased in many municipalities and that the variation in fees between municipalities had increased as well. In 1999, a government working committee was appointed to propose changes in preschool financing. The committee noted that there were groups of families that did not have access to public childcare and that some families did not participate because of the high user fees. In particular, they noted that a large portion of the children without access to public childcare were

members of immigrant families. Based on their findings, the working committee proposed changes in financing and accessibility to public childcare and after-school care. In May of 2000, the government proposed a reform package consisting of three major changes.

(1) A uniform user fee system was introduced with a single payment per child depending on family earnings up to a maximum level, effectively introducing a price cap on childcare. On average, the fees were reduced to about half of the pre-reform levels.

(2) An increased accessibility for children of unemployed parents and those on parental leave was provided by setting the minimum requirement for childcare at three hours per day.

(3) A universally available preschool childcare system was introduced for children aged 4–5 years. This was voluntary on the part of the family and amounted to at least 525 hours per year free of charge.

The reform package was enacted by parliament in September 2000 and was implemented in the years 2001 to 2003 (Swedish National Agency for Education, 2007). Because the task of providing childcare is primarily a responsibility for the municipalities, the different parts of the reform were treated in different ways in terms of financing and regulation. The accessibility part and the universal preschool were regulated by law, but the maximum fee part was voluntary on the part of the municipalities. Those municipalities that accepted the proposal were compensated with a grant from the central government. In 2002, all municipalities except two accepted the maximum fee, and by 2003, all municipalities had accepted the proposal. The different parts of the reform package were introduced over a time span of one and a half years. On July 1, 2001, children of unemployed parents were given the right to participate in preschool. On January 1, 2002, children of parents on parental leave earned the same right, and the fee changes were implemented. Finally, on January 1, 2003, the universal preschool reform was implemented (Swedish National Agency for Education, 2007)

## *2.2. Periods of Comparison*

This paper aims to compare the development of earnings and LFP between families with preschool children and other children and between immigrant and native families during the periods before and after the childcare reform package was enacted. One important question concerns the dates to include in the comparison. Because the reform was implemented in stages between July 2001 and January 2003, there is no single date that can mark the start of the reform. The contents of the reforms were generally known in the autumn of 2000. This means that the years 2001 and 2002 could be considered pre-reform years or post-reform years depending on which part of reform is being studied. For the year 2002, only the universal preschool had not yet been implemented. According to the follow-up study by The Swedish National Agency for

Education, the universal preschool had only minor effects on participation rates because most 4-5-year-olds were already participating prior to 2003. However, because the universal preschool is free of charge, parents of these children experienced a fee reduction compared to the year 2002. The main assumption in this study is to treat the period prior to 2001 as the pre-reform period and the period from 2002 and onwards as the post-reform period, and to consider 2001 as the “reform year” and leave it out of the analysis. The data included in the analysis concerns the years 1995-2000 and 2002-2009.

### *2.3. Hypotheses*

As mentioned in the Introduction, the visible effect (at least initially) of the reform package was to equalize participation and the number of hours of childcare among families (Wikström, 2007). In particular, participation and hours of attendance increased at the lower end of the earnings distribution meaning that the groups that did not have access to childcare prior to the reforms were the ones affected the most. Refugee immigrants, especially women, have the lowest employment rate (Bennich-Björkman, 2002; Bratsberg et al., 2007; Hammarstedt, 2003; Malm, 2005; Vikman, 2013) meaning that they, as a group, would be the most likely to increase LFP and job search if given access to public childcare. However, the effects of the reform package are not necessarily connected to specific immigrant and gender groups for several reasons.

First, the reform package implied income effects as well as substitution effects on time use and labor supply because user fees were reduced for most families with preschool children both on average and at the margin.<sup>1</sup> The way the maximum fee system was constructed implied that the reduction in fees were on average the largest for families with many preschool children and high family earnings. The marginal effects, however, differed significantly depending upon what kind of system a municipality had in place prior to reform. The most common system was to charge users in relation to family income, and the percentage of income pre-reform generally exceeded the levels of the maximum fee after the reform. Consequently, the “price” of childcare fell after the introduction of the maximum fee. The second most common system was to charge users in relation to hours of care (Swedish National Agency for Education, 2007). Under normal circumstances, a reduction in user fees is expected to diminish labor supply, especially female labor supply, because of the income effect and to increase it because of the substitution effect. Therefore, earnings growth due to increased participation in childcare and the labor force might or might not be counteracted by labor supply responses at the intensive margin. It should be noted that a previous study (Lundin et al., 2008) on the

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<sup>1</sup> From here and onwards preschool children are considered to be children in the ages 2-5.

effects of the price reductions associated with the reform package found no, or very small, effects on employment.

Second, childcare decisions are known to differ among ethnic groups (see e.g. Brayfield & Hofferth, 1995; Joesch, 1998). Chiswick (1988) argues that parental investment in children might be influenced by the ethnic group to which they belong, and if this is the case then the demand for formal childcare might also be influenced by ethnicity. Chiswick & DebBurman (2006) have studied preschool enrollment among different immigrant groups in the US and found that enrollment in formal childcare varies with country of origin, and some groups had higher participation rates than natives while others had lower participation rates. One explanation for this is the language barrier. Children of Spanish-speaking immigrants had lower rates of enrollment than natives, while immigrants of English-speaking origins had higher enrollment. Children in families with one English-speaking parent also had higher enrollment than children in Spanish-speaking families.

Third, a potential obstacle to observing increased labor supply and earnings is related to cultural differences, especially the roles of women in household work and child rearing. In many of the immigrant groups, the traditional division of labor applies and this means that women mainly take care of the children and the household work (Nekby, 2002; Rosholm & Vejlin, 2010; Vikman, 2013). This can negatively affect the demand for formal childcare among immigrants once offered. At the same time, networks exist that may help with childcare through informal channels, meaning that formal care can crowd out informal care. Put in the Swedish context, the US studies do not necessarily provide a proper explanation for the differences between natives' and immigrants' childcare decisions because the latter differ between the US and Sweden in terms of the reason for immigration and culture. In a study of public childcare in Sweden, Holmlund (2009) found that having a non-native father decreases the demand for hours of childcare by approximately 11% compared to having a native Swedish father and having a non-native mother increases the demand by 4%. These effects appear large when contrasted with the comparison in the number of childcare hours published by the National Agency for Education in 2007. On average, the number of hours of care was slightly lower for children of non-native parents. Unfortunately, there is no information available regarding different immigrants groups' participation in preschool in the study.

One should also note that the division of labor among recent immigrants might be different from the traditional division of responsibilities because of investments in country-specific human capital. According to the family investment hypothesis (Long, 1980), immigrant women might work more than native (Swedish) women in order to support the human capital investment of the male partner. Swedish data, however, do not support the family investment hypothesis (Rashid, 2004).

One issue that might be important is the timing of responses to the reform package. Increased opportunities and incentives for LFP probably have a direct effect on labor market performance. However, because many immigrant families are weakly connected to the Swedish language and culture, the response to better childcare opportunities might take a considerable amount of time.

The main hypothesis in our work is that the childcare reform package affected the earnings and LFP of mothers of preschool children due to increased opportunities to find a job and to be at least part-time employed. Because immigrants, refugee immigrants in particular, have lower earnings and employment rates, one would expect the increase in these groups to be larger than for native Swedes. However, because of the language barrier and possible cultural differences, it is not necessarily the case that enrollment in preschool has improved among the immigrant groups, and the response to the reforms might be slow because of the factors discussed above. If there are barriers to labor market entry and participation in childcare among the immigrant groups, then the changes in legislation might not have had any significant effects on these groups, meaning that the impact of childcare reform on the immigrant groups is an entirely open question.

#### *2.4. Estimation and Identification*

Earnings and LFP changes should only concern families with preschool children, because the reform package affects only those who have preschool children. Therefore, we expect to find larger increases among families with children of 2-5 years old than among others. Evaluation of the reform is based on a difference-in-differences approach (DD) (see e.g. Ashenfelter & Card, 1985). A standard DD-model can be expressed as follows. Let  $i$  index the individual unit, and  $t$  time, and denote by  $P_t$  and  $C_i$  indicator variables taking the value one for the post-reform period and observations of women with preschool children, respectively. If we denote by  $y$  the variable of interest (log-earnings or LFP), the equation to be estimated is

$$y_{it} = \alpha_0 + \alpha_1 P_t + \alpha_2 C_i + \alpha_3 P_t C_i + z_{it} + \varepsilon_{it} \quad (1)$$

where  $z_{it}$  includes observed individual characteristics (to be discussed below) with associated parameters and  $\varepsilon_{it}$  is the error term. Of primary interest in Eq. (1) is the parameter  $\alpha_3$  that measures the treatment effect. We use the following identification strategy. Mothers with children aged 2-5 are considered the treated group. We do not include mothers with children below the age of two, since only a minor fraction of 1-year-old children participate in preschool activities. Furthermore, mothers of 1-year-old children are often on parental leave, and since the parental insurance system has been changed during the period we consider, including mothers of children at the age of one among the treated



might lead to biased estimates. As a comparison group, one might have included all mothers of children above the age of five. However, having teenagers is considerably different from having small children when it comes to care taking. Therefore, we define the control group as mothers with children close to the age of preschool children. Children at the age of six participate in preschool class (förskoleklass), which is a blend of preschool and primary school, and some 6-year-old children might have continued at preschool at the age of six. Therefore, we define the group of controls as mothers of children 7-10 that have no children in the age span 2-5.

Eq. (1) is estimated on native Swedish mothers. In order to study differences between native and immigrant mothers, we continue as follows. Let  $M_i$  denote an indicator variable taking the value one if the individual is identified as immigrant and zero otherwise. Interacting the migrant dummy variable with the indicator variables previously defined, one can write the equation to be estimated as

$$y_{it} = \alpha_0 + \alpha_1 P_t + \alpha_2 C_i + \alpha_3 P_t C_i + z_{it} + \beta_0 M_i + \beta_1 P_t M_i + \beta_2 C_i M_i + \beta_3 P_t C_i M_i + z_{it} M_i + \varepsilon_{it} \quad (2)$$

The parameters associated with the M-interactions in Eq. (2) measure deviations in effects between immigrant mothers and native mothers. It is assumed that individual characteristics affect differently the groups of native Swedish and immigrant mothers. Of particular interest is the parameter  $\beta_3$ , which measures the deviation in the treatment effect from that of Swedish mothers, and can be used directly to test if both groups have a common treatment effect. The effect of the reform on immigrant mothers of preschool children is to be compared with other immigrant women and called “DD, immigrant women”:

$$\frac{\partial^2 y_{it}}{\partial P_t \partial C_i} = \alpha_3 + \beta_3 M_i. \quad (3)$$

We impose a number of control variables in Eqs. (1) and (2). Observed individual characteristics include the level of education, age, partner’s education and age, the number of children in different age groups, and for migrants the number of years since immigration. Average earnings increase with time (and LFP may also fluctuate over time), which is controlled for by including time fixed effects. The reform could have had larger effects in municipalities with relatively high costs of childcare before the national equalisation (since fees were generally higher in those municipalities). To account for this, municipality fixed effects are included in both Eqs. (1) and (2).

One concern in this paper is differences in the timing of approval of the reform by the municipalities, as well as reform uncertainty that may lead to faulty definitions of the pre- and post-reform periods. As discussed in section 2.2, the baseline alternative is to consider the reform year to be 2001 (so that 1995-2000 is the pre-reform period and 2002 and onwards the post-reform period). We complement the baseline with robustness checks by using three alternative definitions of the reform years; 2001-2002, 2001-2003, and 2000-2003 respectively. To further check the robustness of our main results, we conduct a placebo test in which we consider the year 1998 as the reform year and compare the labor outcomes in 1997 and 1999. This is conducted by comparison of employment and earnings in 1997 and 1999. Additional robustness checks are performed by estimating the parameters of interest on sub-samples of age groups 20-35, 24-39, 30-49, separate estimates for different levels of educational attainment, the presence of older children in the household and different income groups (sub-samples with mother earnings below the average and below the first quartile of the earnings distribution).

The earnings equation (Mincer & Polachek, 1974) is estimated by OLS, and LFP is estimated with the same set of explanatory variables by a linear probability model. The linearity assumption allows the mean differences to be determined simply by comparing the parameters for each variable of interest (Aia & Norton, 2003). Standard errors are clustered at the municipality level.

### 3. EMPIRICAL ANALYSIS

#### 3.1. Data

The study is based on individual level register data collected by Statistics Sweden and compiled into the Swedish Longitudinal Integration Database for Health Insurance and Labour Market Studies (Longitudinell Integrationsdatabas för Sjukförsäkrings- och Arbetsmarknads-studier, LISA).<sup>2</sup> The data cover the period of 1995-2009. From the database, couples with both spouses born in the same country are selected. We classify immigrants by geographical origin, and consider a group of recent immigrants originated from Iran and Iraq. There are two reasons why we choose migrant families from these countries. First, we want to select migrants for which the integration process is considered to be relatively slow in terms of labor market performance. Although we cannot observe the actual cause for moving to Sweden, it is known that this group to a large extent consists of refugee immigrants, meaning that they did not move primarily for labor market reasons. So, members of this group were likely not to have had access to public childcare prior to the reform. Second, families originating from Iran and Iraq forms a relatively large group in comparison to other countries from which refugee immigrants originate. This means that the

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<sup>2</sup> The data are obtained through the Demographic Data Base, Umeå University.

selected group of married immigrant women with children is relatively large and consists primarily of refugee immigrants. The sample is constructed by selecting a random 10% of native couples having a female in the age range of 16-49 years and all couples from the immigrant group with a female partner in the same age range. We then select women with children in the age range 2-5 and women with children 7-10 that did not have a preschool child. The total number of women selected is 48,191, out of which 40,121 are native Swedish and 8,070 are immigrant mothers. There are 88,715 observations in the pooled data; 74,726 native Swedish and 13,989 immigrant observations, meaning that each mother is observed on average 1.8 times.

The main variables that we focus on are LFP and earnings. Because the register data do not include a precise definition of LFP, a convention used in this paper is to consider those who have at least some earnings from employment or self-employment as participants in the labor force. A list of variables included in the analysis is presented in Table A1. Register data contain information on individual incomes, structured by source of income, such as income from employment and self-employment, parental and sick leave allowance, unemployment and job training allowance, and pension. Here earnings are defined as the logarithm of income from employment and self-employment.<sup>3</sup> Earnings are discounted by consumer price index (CPI), and participation in the labor force is coded as an indicator variable taking a value of one for observations where non-zero earnings are observed. The set of explanatory variables is primarily based on variables found to be important in similar papers studying differences in earnings and employment rates for migrants and natives (e.g. Barth et al., 2004; Blanchflower & Oswald, 1994; Borjas, 1987; Borjas, 1995; Card, 1995; Kahanec et al., 2011). In particular, we include age and age squared, educational attainment, the number of children at various ages, the partner's age and education, and the number of years since immigration.

### 3.2. Descriptive statistics

Table 1 presents descriptive statistics prior to the reform (years 1995-2000) for mothers, and their spouses, with children of age 2-5 (column 1). In columns (3) and (5), the differences between the treated and control group are compared for the pre- and post-reform data with standard errors in means presented in columns (4) and (6). Swedish and immigrant women are both included in Panel A while descriptive statistics for Swedish and immigrant women are presented separately in Panels B and C respectively.

Table 1 around here

Figure 1 around here

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<sup>3</sup> We include zero-earnings in the analysis. Since the logarithm of zero is not defined, the value 1 is imposed on zero-earnings prior to transformation.

Panels B and C in Table 1 show that the most noticeable differences between Swedish and immigrant women prior to reform appear in earnings and LFP. Earnings are dramatically lower in the immigrant group and LFP for Swedish women is 86% while the corresponding figure for immigrant women is only 15%. The large differences between natives and immigrants in earnings and LFP are also illustrated in Figure 1 where earnings and LFP developments are shown for the whole period for Swedish born women and immigrant women with preschool children. Figure 1 also reveals a clear increase in earnings and LFP during the period for immigrant women, although there is a downturn in the end of the period.

For the full sample, Panel A in Table 1 reveals that the earnings difference between mothers with preschool children and the comparisons is lower post-reform than pre-reform, but there is no corresponding change in LFP. However, Panel B shows an increase in earnings and LFP of Swedish mothers relative to the comparison group post-reform, while it is evident from Panel C that earnings and LFP decreased among preschool mothers relative to the comparison. Thus, the descriptives suggest that the reform had positive effects on labor outcomes among native mothers, while there were negative effects in the group of migrants.

Turning to the explanatory variables, there are some differences in educational attainment between the groups of origin. The proportion of mothers with secondary education is lower and tertiary education higher for immigrant mothers and their spouses than native Swedes prior to reform. While the differences in educational attainment among Swedish mothers, with and without pre-school children, are almost unchanged after the reform, we see that immigrant mothers with pre-school children are less educated after the reform compared to the control group. One important observation in the immigrant group, concerns country experience (the number of years since migration, YSM). The country experience of mothers with pre-school children decreased relative to that of mothers without pre-school children. Since country experience is usually found to be an essential determinant of earnings and employment among immigrants, controlling for YSM in the empirical analysis is likely to be important.

### *3.3. Regression results – native mothers*

Below we present the results from the regression analysis that includes native Swedish families in which the female partner is 16-49 years of age with children in the selected age groups. In Table 2, the main effects are presented. As mentioned in section 2, the reforms were implemented during a period of one and a half year. We consider the period after 2001 as the post-reform period and the period prior to 2001 as the pre-reform period. A full set of results is reported in column 1 of Tables A6 and A7 in the Appendix. The rows of Table 2

represent different specifications with and without time fixed effects, municipality fixed effects and background controls.

Table 2 around here

The main results show that there are positive and statistically significant post-reform differences between the treatment and comparison group. The estimated treatment effect of earnings (Panel A) varies between 0.09 and 0.16. In Panel B, the effect concerning LFP varies between 0.015 and 0.023. All the effects reported in Table 2 are statistically significant. The preferred estimates, including time- and municipality fixed effects as well as background controls, imply that the estimated differences between the treated and control group is 17.8% (earnings) and 2.9% (LFP) (corresponding to the values 0.164 and 0.023 in column 1 of Table 2).<sup>4</sup> Thus, the results suggest that the childcare reform had substantial effects on natives.

As mentioned previously, a number of robustness checks are made to confirm the results. Although we consider 2001 to be the most appropriate break point for reform, other divisions are considered as well. First, the reform package became known in 2000, but the initial discussions of reform were conducted already in 1999. Choosing 2000 as the final pre-reform year may therefore be narrow since expectations of reform might already have been present at that time. Second, the general preschool of 4-5-year old children was enacted in January 2003. If this part of the reform affected earnings and LFP, then our reform year would not capture fully the reform effects. In order to see how sensitive the results are to changes in the reform period, we re-estimate Eq. (1) using slightly different definitions of pre- and post-reform periods by using 2000-2003, 2001-2003, and 2001-2002 as reform years. The results, presented in Tables A2 and A3 in the Appendix, reveal that the results are robust to alternative definitions of the reform period. The estimated treatment effect varies between 0.096 and 0.190 for earnings, and between 0.017 and 0.027 for LFP. This corresponds to a percentage increase in earnings of native Swedish mothers of preschool children of 10 to 21% and LFP growth of 2.1 to 3.4%.

To further strengthen our conclusions we also define a placebo reform by assuming that the reform was conducted in 1998. The year 1997 is considered the pre-reform year, and 1999 the post-reform year. The results are presented at the bottom of Tables A2 and A3. As can be seen, the estimated “treatment” is close to zero and statistically insignificant. This exercise shows that there does

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<sup>4</sup> The relative increase in earnings is equal to exponent of the parameter of interest minus one, and the relative increase in employment is equal to the ratio of the parameter of interest and the average level of LFP among women with preschool children before the reform.

not appear to be any important differences in trend between the groups of interest prior to reform.

Given that previous studies have found i) small effects on employment of the fee changes associated with the reform, and ii) that the largest changes in the number of hours of childcare was for those families that did not have access to preschool prior to reform, a natural next step is to ask if the results are robust in the sense that treatment effects are positive for different subsamples. To study this, estimations are firstly restricted by age (20-35, 24-39, 30-49). The idea here is that families that are the least connected to the labor market (and therefore the least likely to have access to childcare prior to reform) are the youngest, one would therefore expect that if some age-subgroup would drive the results it is among families with young mothers. Second, we also restrict the samples by level of education, since unemployment decreases with the level of education and the unemployed had less access to childcare prior to reform. Third, a subsample excluding observations where women in the treatment group having preschool children and children 7-10 years old is considered to distinguish more closely the treated and the comparisons. The final way of restricting data is to consider only observations in the lower half of the income distribution to see if the estimated treatment effect becomes stronger. Table 3 presents the results for the different subsamples.

Table 3 around here

Restricting the sample by age shows some, albeit small, variation in the estimated treatment parameters. In all three subsamples, treatment effects are positive and statistically significant, and the estimated treatment effects are well in line with the baseline; larger for the group of youngest mothers 20-35 years old. The treatment parameter is also significantly determined when we select women with tertiary or secondary level of education. The treatment parameter is significantly larger in size than the baseline when mothers having both preschool and school children (2-5 and 7-10 years old) are selected. Excluding women with earnings above the mean gives still positive effect on the treatment parameters. However, when restricting earnings to the bottom 25%, the estimates became insignificant. From this, we conclude that the treatment effect is stable independent of the assumed reform period and subsample.

### *3.4. Immigrant mothers*

Next, we study the effects of the reform when immigrant mothers are included on the basis of the estimates of Eq. (2). Table 4 presents the results for earnings and LFP when the reform period is assumed to be the year 2001. For the immigrant group, we study two effects. First, we are interested in the differential effect distinguishing the reform effect of immigrant mothers of

preschool children with native Swedish mothers in the group of treated (measured by the parameter  $\beta_3$  in Eq. (2)). The differential effects are presented in column 3. Second, a comparison is made within the group of immigrant mothers – between mothers with preschool children and the control by summing the coefficient associated native mothers and the differential effect (see Eq. (3)). The estimates are displayed in column 5 (DD, immigrant women).

As before, three versions of Eq. (2) are estimated; with and without background controls, time- and municipality fixed effects. At the bottom of each panel of the table, the baseline (full specification) is presented. The estimated treatment parameter for native Swedish mothers remains stable in comparison to the estimates of Eq. (1) presented in Table 2. The estimate of the differential effect for immigrant mothers is negative and statistically significant, implying that the reform effect is smaller for immigrant mothers. The size of this effect turns out to depend on the inclusion of background controls; when the background controls are introduced the effect drops (in absolute value) by approximately one half. This indicates that the background controls have different effects compared to the sample of natives. Further analysis shows that it is important to control for country experience (YSM), since the sample of immigrant mothers is unbalanced with respect to integration. When calculating the DD-effect on immigrants, the parameters become small in absolute value and statistically insignificant once we condition on background characteristics separately for the group of immigrants. Our baseline specification thus suggests that the reform did not affect earnings and LFP for immigrants from the countries under study.

Table 4 around here

As for the Swedish mothers, we also conduct robustness checks when including immigrant mothers. Tables A4 and A5 compare different assumptions of the reform period. The treatment parameter remains stable for native Swedish mothers. The DD-parameter for the immigrant group (see column 5) is not statistically significant when controlling for background characteristics and time and municipality fixed effects. Thus, the overall conclusion from this exercise is that the results are not sensitive to the exact definition of the reform period. Again, we conclude that the placebo test yields statistically insignificant results. As can be seen, the estimated effects are close to zero. The full set of results for baseline estimates with included individual characteristics is reported in Tables A6 and A7 in the Appendix (column 2), and for placebo the results are presented in column 4.

Table 5 exhibits the estimates on different sub-samples of women as described above. The results regarding Swedish mothers are stable independent of the subsample, while the results regarding immigrant mothers vary somewhat depending on subsample. Of particular importance is that the DD-effects for

immigrant mothers are not statistically significant, meaning that we cannot find any significant effects of the childcare reform on immigrant mothers with preschool children. Thus, we conclude that the results from estimation on subsamples are well in line with those of the baseline specification.

Table 5 around here

#### 4. CONCLUDING REMARKS

The main purpose of the childcare reform package introduced in the early 2000s was to increase accessibility and to even out the differences in the cost of public childcare among the municipalities. Because of the differences in employment and earnings between native Swedes and refugee immigrants, the reform has the potential to improve the labor market situation in favor of the immigrant groups.

This paper is a first attempt to address how changes in family policy and accessibility to the preschool childcare system in particular, affected labor market outcomes of groups with relatively little attachment to the labor market. Of particular interest to us is if the reforms increase the LFP of recent immigrants and lead to changes in their earnings. We study both the development of earnings and LFP for mothers of preschool-aged children as a response to the reforms. As a comparison group, we choose women with children in the ages 7-10 that do not have preschool children. The post-reform outcomes of immigrant women from Iran and Iraq are estimated separately and the results are compared to the group of native Swedish mothers.

A consistent result in the study is that native Swedish women in the treatment group increased their earnings and LFP compared to the comparison group. This result holds for all of the specifications considered. Comparing the quantitative results with those in previous studies, our results appear to lie in the interior of the range of what other studies have presented. According to Baker et al. (2008), the introduction of universal childcare in Quebec resulted in an employment increase of approximately 8 percentage points among mothers with preschool children. The study of the expansion of childcare in Argentina (Berlinski & Galiani, 2007) found that employment increased by as much as 12-13 percentage points. Our preferred estimate of 2.3 percentage points is considerably lower. One should, however, keep in mind that the enrollment rate in the Swedish formal preschool was very high by international standards at the time the reform was enacted, and that the employment rate was high as well. The quantitative results are large compared to previous Nordic studies of childcare reform (Lundin et al., 2008; Havnes & Mogstad, 2011). The paper by Lundin et al. is the closest to ours, since they study one part of the Swedish reform package; the fee changes implied by the reform. Their results show that the labor supply responses were small because of the fee changes. In this study,



we take a broader perspective, where all the different parts of the reform are included, and in particular, the study includes the accessibility parts of the reform package. A second difference is that we allow for a longer follow-up period. Their study uses data for 2003 as the post-reform year, which may be too soon after reform considering that the increase in childcare participation required investment in new capacity.

The estimated earnings effects appear to be large, and are of similar size also for different subsamples, which we interpret as evidence in favor of the hypothesis that the reform package had effect on the intensive margin as well as the extensive margin. Unfortunately, there are no comparable studies on earnings, meaning that we cannot assess whether the point estimates obtained are commonly observed. One obvious objection to our results is that there might have been some other policy change influencing the results. During this period, there was a minor change in the parental leave insurance, in which an additional month of parental insurance was introduced for children born from 2002 and on. However, changes in the parental insurance system mainly affected children under the age of two, and since we excluded them from the analysis, it is unlikely that the estimated effects come from that policy change.

The results presented in this paper give no evidence that immigrant mothers' earnings and LFP were affected by the reform package. The estimated treatment effect among immigrant mothers is close to zero. Why is it that the earnings and LFP of the immigrant group did not respond to reform? We can think of at least three reasons for this. First, it is well known that refugee immigrants in Sweden have difficulties on the labor market for various reasons. The major problem may then not be access to childcare, but that the likelihood of obtaining employment is the real problem. Second, publically provided childcare may crowd out informal care. One possibility is that the group under study had reasonably good access to informal care, and that the reform did not increase their opportunities. Finally, as we point out in the paper there may be obstacles for participating connected to culture and language making it difficult to benefit from the reform the way one could expect.

Unfortunately, there is to our knowledge no reliable information on how different immigrant groups participated in the childcare system before and after the reform dates. The only information available concerns immigrants in general. Detailed information would be very useful because participation in childcare is a necessary condition in order for childcare policy to have any effect on labor market outcomes. As mentioned in the paper, studies from the US are not necessarily representative to European conditions where a large proportion of migrants are refugee immigrants. One suggestion for future work, therefore, is to study in more detail how different groups participate in public childcare.

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## APPENDIX

**Table A1.** List of variables

Earnings	Annual earnings from employment and self-employment, $\times 100$ SEK. Log earnings deflated by CPI. Zero earnings are replaced with the value 1 before performing the log operation.
Labor force participation (LFP)	1 – if a person had income from employment or self-employment, 0 – no earnings.
Male/Female age	
Education	1 – Compulsory; schooling 9 years or less 2 – Secondary; secondary and post-secondary education less than two years 3 – Tertiary; post-secondary for two years or longer or postgraduate education
Number of children (2–3 years)	
Number of children (4–5 years)	
Number of children (6–18 years)	
Years since migration	
Ethnic groups:	1 – native Swedes, 2 – Iran/Iraq

**Table A2.** Regression results based on alternative definitions of the reform period. Native Swedish women. Earnings

DD earnings		Mean	Controls	Time FE	MU FE	N
Estimate	SE	(3)	(4)	(5)	(6)	(7)
(1)	(2)					
Reform period 2000-2003						
0.180***	0.036	6.539	No	No	No	57,369
0.137***	0.038	6.539	No	No	Yes	57,369
0.112**	0.037	6.539	No	Yes	No	57,369
0.122**	0.038	6.539	No	Yes	Yes	57,369
0.096*	0.037	6.539	Yes	Yes	Yes	57,369
Reform period 2001-2003						
0.153***	0.036	6.539	No	No	No	62,971
0.129***	0.036	6.539	No	No	Yes	62,971
0.135***	0.036	6.539	No	Yes	No	62,971
0.111**	0.036	6.539	No	Yes	Yes	62,971
0.190***	0.035	6.539	Yes	Yes	Yes	62,971
Reform period 2001-2002						
0.141***	0.034	6.505	No	No	No	68,844
0.118***	0.034	6.505	No	No	Yes	68,844
0.121***	0.034	6.505	No	Yes	No	68,844
0.097**	0.034	6.505	No	Yes	Yes	68,844
0.170***	0.033	6.505	Yes	Yes	Yes	68,844
Placebo 1998						
0.028	0.083	5.506	No	No	No	12,547
0.008	0.083	5.506	No	No	Yes	12,547
-0.038	0.081	5.506	Yes	No	Yes	12,547

Note: \*\*\*  $p < 0.001$ , \*\*  $p < 0.01$ , \*  $p < 0.05$ . Standard errors (SE) are clustered at the municipality level. Columns (4), (5), and (6) indicate if background characteristics, time fixed effects (Time FE), and municipality fixed effects (MU FE), are included. Mean refers to the average of log-earnings in the treated group post-reform. The placebo test is conducted by treating the year 1997 as the pre-reform year and 1999 as the post-reform year.

**Table A3.** Regression results based on alternative definitions of the reform period. Native Swedish women. LFP

DD LFP		Mean	Controls	Time FE	MU FE	N
Estimate	SE					
(1)	(2)	(3)	(4)	(5)	(6)	(7)
Reform period 2000-2003						
0.021***	0.005	0.931	No	No	No	57,369
0.020***	0.005	0.931	No	No	Yes	57,369
0.020***	0.005	0.931	No	Yes	No	57,369
0.018***	0.005	0.931	No	Yes	Yes	57,369
0.027***	0.005	0.931	Yes	Yes	Yes	57,369
Reform period 2001-2003						
0.021***	0.005	0.931	No	No	No	62,971
0.020***	0.005	0.931	No	No	Yes	62,971
0.019***	0.005	0.931	No	Yes	No	62,971
0.018***	0.005	0.931	No	Yes	Yes	62,971
0.027***	0.005	0.931	Yes	Yes	Yes	62,971
Reform period 2001-2002						
0.020***	0.005	0.930	No	No	No	68,844
0.019***	0.005	0.930	No	No	Yes	68,844
0.018***	0.005	0.930	No	Yes	No	68,844
0.017***	0.005	0.930	No	Yes	Yes	68,844
0.025***	0.005	0.930	Yes	Yes	Yes	68,844
Placebo 1998						
0.007	0.012	0.884	No	No	No	12,547
0.005	0.012	0.884	No	No	Yes	12,547
-0.001	0.012	0.884	Yes	No	Yes	12,547

Note: \*\*\*  $p < 0.001$ , \*\*  $p < 0.01$ , \*  $p < 0.05$ . Standard errors (SE) are clustered at the municipality level. Columns (4), (5), and (6) indicate if background characteristics, time fixed effects (Time FE), and municipality fixed effects (MU FE), are included. Mean refers to the average of log-earnings in the treated group post-reform. The placebo test is conducted by treating the year 1997 as the pre-reform year and 1999 as the post-reform year.

**Table A4.** Regression results based on alternative definitions of the reform period; Swedish and Immigrant mothers; Earnings

DD, native Swedish women		Immigrants, differential effect		DD, immigrant women		Mean	Controls	Time FE	MU FE	N
Estimate	SE	Estimate	SE	Estimate	SE					
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Reform period 2000-2003										
0.137***	0.038	-0.418***	0.105	-0.281**	0.098	5.638	No	No	No	68,239
0.109**	0.037	-0.379***	0.104	-0.271**	0.097	5.638	No	No	Yes	68,239
0.123**	0.038	-0.396***	0.104	-0.273**	0.097	5.638	No	Yes	No	68,239
0.093*	0.037	-0.358***	0.104	-0.264**	0.097	5.638	No	Yes	Yes	68,239
0.093*	0.037	-0.358***	0.104	-0.071	0.088	5.638	Yes	Yes	Yes	68,239
Reform period 2001-2003										
0.153***	0.036	-0.435***	0.099	-0.282***	0.092	5.638	No	No	No	74,821
0.127***	0.036	-0.400***	0.098	-0.273**	0.092	5.638	No	No	Yes	74,821
0.135***	0.036	-0.410***	0.099	-0.274**	0.092	5.638	No	Yes	No	74,821
0.109**	0.035	-0.376***	0.098	-0.267**	0.092	5.638	No	Yes	Yes	74,821
0.188***	0.035	-0.249**	0.09	-0.061	0.084	5.638	Yes	Yes	Yes	74,788
Reform period 2001-2002										
0.141***	0.034	-0.427***	0.096	-0.286***	0.089	5.64	No	No	No	81,752
0.116***	0.034	-0.391***	0.095	-0.275**	0.089	5.64	No	No	Yes	81,752
0.121***	0.034	-0.399***	0.095	-0.278**	0.089	5.64	No	Yes	No	81,752
0.096**	0.034	-0.364***	0.095	-0.269**	0.089	5.64	No	Yes	Yes	81,752
0.170***	0.033	-0.240**	0.088	-0.070	0.081	5.64	Yes	Yes	Yes	81,718
Placebo 1998										
0.028	0.083	-0.011	0.207	0.017	0.190	4.774	No	No	No	14,181
0.006	0.083	-0.012	0.209	-0.006	0.191	4.774	No	No	Yes	14,181
-0.041	0.081	0.037	0.206	-0.004	0.189	4.774	Yes	No	Yes	14,181

Note: \*\*\*  $p < 0.001$ , \*\*  $p < 0.01$ , \*  $p < 0.05$ . Standard errors (SE) are clustered at the municipality level. Columns (8), (9), and (10) indicate if background characteristics, time fixed effects (Time FE), and municipality fixed effects (MU FE), are included. Mean refers to the average of log-earnings in the treated group post-reform. The placebo test is conducted by treating the year 1997 as the pre-reform year and 1999 as the post-reform year.

**Table A5.** Regression results based on alternative definitions of the reform period; Swedish and Immigrant mothers; LFP

DD, native Swedish women		Immigrants, differential effect		DD, immigrant women		Mean	Con trols	Time FE	MU FE	N
Estimate	SE	Estimate	SE	Estimate	SE					
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Reform period 2000-2003										
0.021***	0.005	-0.067***	0.018	-0.046**	0.017	0.81	No	No	No	68,239
0.019***	0.005	-0.064***	0.018	-0.045**	0.017	0.81	No	No	Yes	68,239
0.020***	0.005	-0.064***	0.018	-0.044**	0.017	0.81	No	Yes	No	68,239
0.017***	0.005	-0.061***	0.018	-0.044**	0.017	0.81	No	Yes	Yes	68,239
0.027***	0.005	-0.037*	0.016	-0.010	0.015	0.81	Yes	Yes	Yes	68,239
Reform period 2001-2003										
0.021***	0.005	-0.064***	0.017	-0.043**	0.016	0.81	No	No	No	74,821
0.019***	0.005	0.062***	-0.016	0.043**	0.016	0.81	No	No	Yes	74,821
0.019***	0.005	-0.061***	0.017	-0.042**	0.016	0.81	No	Yes	No	74,821
0.017***	0.005	-0.059***	0.016	-0.042**	0.016	0.81	No	Yes	Yes	74,821
0.017***	0.005	-0.032*	0.015	-0.006	0.014	0.81	Yes	Yes	Yes	74,788
Reform period 2001-2002										
0.020***	0.005	-0.063***	0.016	-0.042**	0.015	0.813	No	No	No	81,752
0.018***	0.005	-0.060***	0.016	-0.042**	0.015	0.813	No	No	Yes	81,752
0.018***	0.005	-0.060***	0.016	-0.041**	0.015	0.813	No	Yes	No	81,752
0.016***	0.005	-0.057***	0.016	-0.041**	0.015	0.813	No	Yes	Yes	81,752
0.025***	0.005	-0.031*	0.015	-0.006	0.014	0.813	Yes	Yes	Yes	81,718
Placebo 1998										
0.007	0.012	-0.006	0.036	0.001	0.034	0.804	No	No	No	14,181
0.005	0.012	-0.005	0.036	-0.001	0.034	0.804	No	No	Yes	14,181
-0.001	0.012	0.000	0.036	-0.001	0.034	0.804	Yes	No	Yes	14,181

Note: \*\*\*  $p < 0.001$ , \*\*  $p < 0.01$ , \*  $p < 0.05$ . Standard errors (SE) are clustered at the municipality level. Columns (8), (9), and (10) indicate if background characteristics, time fixed effects (Time FE), and municipality fixed effects (MU FE), are included. Mean refers to the average of log-earnings in the treated group post-reform. The placebo test is conducted by treating the year 1997 as the pre-reform year and 1999 as the post-reform year.

**Table A6.** Full regression results of earnings corresponding to Equations (1) and (2), and placebo reform

Variable	Reform period 2001		Placebo reform	
	Eq.1	Eq.2	Eq.1	Eq.2
	(1)	(2)	(3)	(4)
Post-reform effect (P)	-	-	0.176** (0.059)	0.179** (0.059)
Families with children 2–5 years old (C)	0.177*** (0.053)	0.168*** (0.053)	0.337** (0.13)	0.329* (0.13)
DD, native Swedish women (PC)	<b>0.164***</b> (0.032)	<b>0.163***</b> (0.032)	<b>-0.038</b> (0.081)	<b>-0.041</b> (0.081)
Immigrants' group effect (M)		-1.992** (0.735)		-4.023* (1.694)
Immigrants' group effect post-reform (PM)		0.101 (0.054)		0.034 (0.133)
Immigrants' group effect. Families with children 2–5 years old (CM)		-0.078 (0.131)		-0.168 (0.275)
Immigrants' group effect. Post-reform. Families with children 2–5 years old (PCM)		<b>-0.215</b> (0.085)		<b>0.037</b> (0.206)
<i>Female partner</i>				
Age (years)	0.289*** (0.023)	0.289*** (0.023)	0.270*** (0.056)	0.271*** (0.056)
Age squared	-0.384 (0.034)	-0.383 (0.034)	-0.357 (0.087)	-0.356 (0.087)
Education Compulsory			Reference	
Education Secondary	0.923*** (0.044)	0.921*** (0.044)	0.909*** (0.093)	0.909*** (0.093)
Education Tertiary	1.561*** (0.045)	1.564*** (0.045)	1.419*** (0.097)	1.421*** (0.097)
<i>Male partner</i>				
Age (years)	0.051*** (0.015)	0.051*** (0.015)	0.057 (0.039)	0.057 (0.039)
Age/10 squared	-0.074*** (0.021)	-0.074*** (0.021)	-0.081 (0.053)	-0.082 (0.053)
Education Compulsory			Reference	
Education Secondary	0.097** (0.031)	0.096** (0.031)	0.057 (0.07)	0.055 (0.07)
Education Tertiary	0.207*** (0.033)	0.211*** (0.033)	0.202** (0.076)	0.199** (0.076)
<i>Number of children</i>				
2–3 years	-0.792*** (0.046)	-0.786*** (0.046)	-0.921*** (0.114)	-0.918*** (0.113)
4–5 years	-0.318*** (0.045)	-0.315*** (0.045)	-0.393*** (0.113)	-0.387*** (0.113)



**Table A6 continued**

	(1)	(2)	(3)	(4)
6–18 years	-0.239*** (0.013)	-0.245*** (0.013)	-0.289*** (0.033)	-0.293*** (0.033)
<b>Immigrants</b>				
<i>Female partner</i>				
Age (years)		0.177*** (0.036)		0.259** (0.087)
Age/10 squared		-0.278*** (0.058)		-0.395** (0.150)
Education Compulsory		Reference		
Education Secondary		0.304*** (0.054)		0.185 (0.130)
Education Tertiary		0.798*** (0.084)		0.700 (0.214)
<i>Male partner</i>				
Age (years)		-0.042 (0.030)		0.007 (0.066)
Age squared		0.059 (0.040)		-0.043 (0.102)
Education Compulsory		Reference		
Education Secondary		0.155*** (0.056)		0.245 (0.130)
Education Tertiary		0.358*** (0.082)		0.211 (0.200)
<i>Number of children</i>				
2–3 years		-0.526*** (0.100)		-0.635*** (0.187)
4–5 years		-0.254** (0.095)		-0.423* (0.181)
6–18 years		-0.121*** (0.024)		-0.041 (0.065)
YSM		0.290*** (0.006)		0.319*** (0.025)
Constant	-1.191** (0.37)	0.827 (0.961)	-1.069 (0.902)	3.02 (2.274)
N	74,726	88,681	12,547	14,181
R <sup>2</sup>	0.114	0.397	0.112	0.362

Note: \*\*\*  $p < 0.001$ , \*\*  $p < 0.01$ , \*  $p < 0.05$ . Standard errors are clustered at the municipality level and presented within parentheses. Column (1) reports the baseline estimates for the Swedish group only. Column (2) reports the baseline estimates for the Swedish and immigrant groups. Columns (1) and (2) include a full set of time fixed effects. The placebo test is conducted by comparing 1997 (pre-reform year) and 1999 (post-reform year).

**Table A7.** Full regression results of LFP corresponding to Equations (1) and (2), and placebo reform

	Reform period 2001		Placebo reform	
	Eq.1	Eq.2	Eq.1	Eq.2
	(1)	(2)	(3)	(4)
Post-reform effect (P)	-	-	0.019*	0.019*
			(0.008)	(0.008)
Families with children 2–5 years old (C)	0.035***	0.034***	0.058**	0.056**
	(0.008)	(0.008)	(0.020)	(0.020)
DD, native Swedish women (PC)	<b>0.023***</b>	<b>0.023***</b>	<b>-0.001</b>	<b>-0.001</b>
	(0.004)	(0.004)	(0.012)	(0.012)
Immigrants' group effect (M)		-0.666***		-1.000***
		(0.119)		(0.295)
Immigrants' group effect post-reform (PM)		0.041***		0.008
		(0.009)		(0.023)
Immigrants' group effect. Families with children 2–5 years old (CM)		-0.019		-0.032
		(0.022)		(0.049)
Immigrants' group effect. Post-reform. Families with children 2–5 years old (PCM)		<b>-0.027</b>		<b>0.000</b>
		(0.014)		(0.036)
<i>Female partner</i>				
Age (years)	-0.032***	0.031***	0.032***	0.032***
	(0.003)	(0.003)	(0.009)	(0.009)
Age squared	-0.044	-0.044	-0.045	-0.045
	(0.005)	(0.005)	(0.013)	(0.013)
Education Compulsory			Reference	
Education Secondary	0.140***	0.140***	0.134***	0.134***
	(0.007)	(0.007)	(0.015)	(0.015)
Education Tertiary	0.194***	0.195***	0.186***	0.186***
	(0.007)	(0.007)	(0.015)	(0.015)
<i>Male partner</i>				
Age (years)	0.006*	0.006*	0.005	0.005
	(0.002)	(0.002)	(0.006)	(0.006)
Age/10 squared	-0.009**	-0.008***	-0.008	-0.009
	(0.003)	(0.003)	(0.008)	(0.008)
Education Compulsory			Reference	
Education Secondary	0.012**	0.012**	0.011	0.011
	(0.005)	(0.005)	(0.011)	(0.011)
Education Tertiary	0.012**	0.012**	0.018	0.017
	(0.005)	(0.005)	(0.011)	(0.011)
<i>Number of children</i>				
2–3 years	-0.093***	-0.093***	-0.114***	-0.113***
	(0.007)	(0.007)	(0.018)	(0.018)

Table A7 continued

	(1)	(2)	(3)	(4)
4–5 years	-0.044 (0.007)	-0.044*** (0.007)	-0.056** (0.018)	-0.055** (0.018)
6–18 years	-0.022 (0.002)	-0.022*** (0.002)	-0.029*** (0.005)	-0.03*** (0.005)
<b>Immigrants</b>				
<i>Female partner</i>				
Age (years)		0.034*** (0.006)		0.047** (0.016)
Age/10 squared		-0.054*** (0.010)		-0.072** (0.027)
Education Compulsory			Reference	
Education Secondary		0.059*** (0.009)		0.033 (0.024)
Education Tertiary		0.117*** (0.014)		0.118*** (0.038)
<i>Male partner</i>				
Age (years)		-0.007 (0.005)		0.002 (0.012)
Age squared		0.010 (0.006)		-0.009 (0.018)
Education Compulsory			Reference	
Education Secondary		0.025** (0.010)		0.046 (0.024)
Education Tertiary		0.053*** (0.014)		0.026 (0.036)
<i>Number of children</i>				
2–3 years		-0.091*** (0.017)		-0.107** (0.036)
4–5 years		-0.047*** (0.016)		-0.076* (0.036)
6–18 years		-0.020*** (0.004)		-0.006 (0.011)
YSM		0.050*** (0.001)		0.058*** (0.004)
Constant	0.149** (0.054)	0.824*** (0.150)	0.144 (0.135)	1.157** (0.373)
N	74,726	88,681	12,547	14,181
R <sup>2</sup>	0.058	0.410	0.077	0.383

Note: \*\*\*  $p < 0.001$ , \*\*  $p < 0.01$ , \*  $p < 0.05$ . Standard errors are clustered at the municipality level and presented within parentheses. Column (1) reports the baseline estimates for the Swedish group only. Column (2) reports the baseline estimates for the Swedish and immigrant groups. Columns (1) and (2) include a full set of time fixed effects. The placebo test is conducted by comparing 1997 (pre-reform year) and 1999 (post-reform year).

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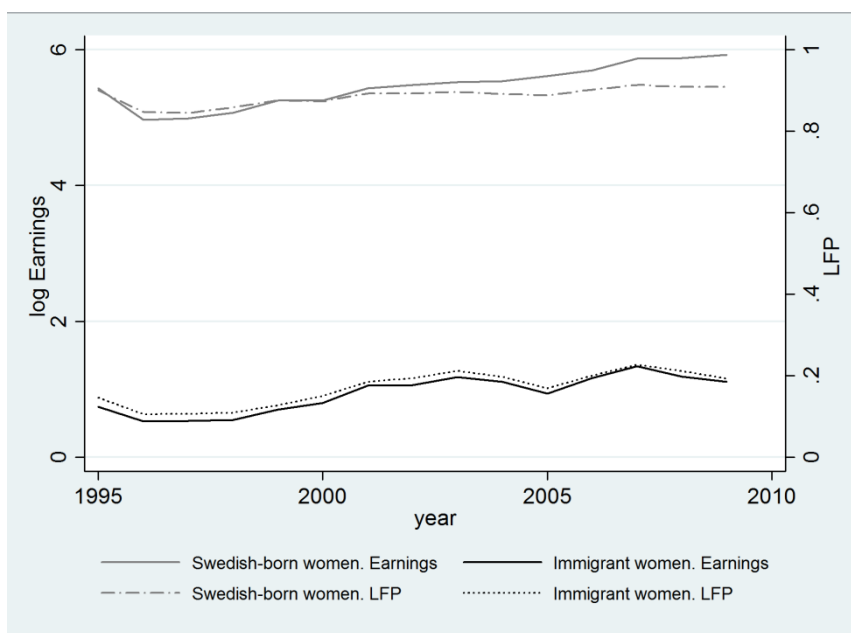
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## FIGURES



*Fig. 1* Log-earnings and LFP of women with children of 2-5 years old by origin.

## TABLES

**Table 1.** Descriptive statistics

Panel A		Swedish and immigrant mothers					
Variables	Before the reform		Diff. in means				
	Mothers of preschool children		With-Without preschool children				
	Mean	SE	Before Diff.	SE	After Diff	SE	
	(1)	(2)	(3)	(4)	(5)	(6)	
Female log-earnings	4.801	2.616	-0.049	0.028	0.002	0.025	
Female LFP	0.809	0.393	0.020	0.004	0.020	0.004	
Female age (years)	31.017	4.281	0.120	0.049	-0.142	0.042	
Female educational attainment							
Secondary	0.551	0.497	0.039	0.005	0.001	0.004	
Tertiary	0.365	0.481	-0.029	0.003	-0.002	0.004	
Male age (years)	33.635	5.082	-0.200	0.058	-0.268	0.048	
Male educational attainment							
Secondary	0.536	0.499	0.032	0.005	0.012	0.004	
Tertiary	0.343	0.475	-0.029	0.002	-0.005	0.004	
Children 2-3 years old	0.649	0.515	0.649	0.004	0.785	0.003	
Children 4-5 years old	0.430	0.529	0.430	0.004	0.294	0.003	
Children 7-18 years old	0.268	0.655	-0.206	0.008	-0.280	0.007	
		Mothers of 2-5 years children	Mothers without preschool children				
		Before the reform	Before the reform	After the reform			
Obs.		19,432	24,444	17,568	27,271		
Panel B		Swedish mothers					
Variables	Before the reform		Diff. in means				
	Mothers of preschool children		With-Without preschool children				
	Mean	SE	Before Diff.	SE	After Diff	SE	
	(1)	(2)	(3)	(4)	(5)	(6)	
Female log-earnings	5.124	2.382	-0.436	0.025	-0.301	0.021	
Female LFP	0.863	0.344	-0.039	0.004	-0.020	0.003	
Female age (years)	31.053	4.245	-0.049	0.051	-0.338	0.043	
Female educational attainment							
Secondary	0.560	0.496	0.037	0.003	0.003	0.005	
Tertiary	0.362	0.481	-0.033	0.005	-0.005	0.005	
Male age (years)	33.438	5.036	-0.068	0.060	-0.259	0.051	
Male educational attainment							
Secondary	0.546	0.498	0.026	0.006	0.004	0.005	
Tertiary	0.333	0.471	-0.025	0.005	0.000	0.005	
Children 2-3 years old	0.648	0.514	0.648	0.004	0.800	0.003	
Children 4-5 years old	0.431	0.529	0.431	0.004	0.276	0.003	
Children 7-18 years old	0.259	0.637	-0.212	0.008	-0.275	0.007	



Table 1 continued

	Mothers of 2-5 years children		Mothers without preschool children			
	Before the reform	After the reform	Before the reform		After the reform	
Obs.	17,981	20,759	14,890		21,096	
Panel C	Immigrant mothers					
Variables	Before the reform		Diff. in means			
	Mothers of preschool children		With-Without preschool children			
	Mean	SE	Before Diff.	SE	After Diff	SE
	(1)	(2)	(3)	(4)	(5)	(6)
Female log-earnings	0.788	1.985	-0.109	0.067	-0.377	0.057
Female LFP	0.147	0.354	-0.020	0.012	-0.060	0.009
Female age (years)	30.572	4.691	0.815	0.172	0.128	0.120
Female educational attainment						
Secondary	0.435	0.496	-0.009	0.016	-0.027	0.010
Tertiary	0.402	0.491	0.010	0.016	-0.025	0.010
Male age (years)	36.077	5.018	0.413	0.181	0.439	0.131
Male educational attainment						
Secondary	0.410	0.492	-0.001	0.016	-0.005	0.010
Tertiary	0.462	0.499	0.017	0.016	-0.009	0.010
Children 2-3 years old	0.666	0.521	0.666	0.010	0.701	0.006
Children 4-5 years old	0.416	0.532	0.416	0.010	0.398	0.004
Children 7-18 years old	0.380	0.840	-0.114	0.029	-0.218	0.020
YSM	4.323	2.242	0.764	0.087	0.178	0.090
	Mothers of 2-5 years children		Mothers without preschool children			
	Before the reform	After the reform	Before the reform		After the reform	
Obs.	1,451	3,685	2,678		6,175	

Note: Columns (1) and (2) show mean values and standard errors (SE) for mothers with children 2-5 years in the pre-reform period. Columns (3)-(6) show mean differences and standard errors of differences between mothers with and without preschool children in the pre- and post-reform period respectively.

**Table 2.** Regression results; Swedish-born women; Earnings and LFP

DD		Mean	Controls	Time FE	MU FE	N
Estimate	SE	(3)	(4)	(5)	(6)	(7)
(1)	(2)					
<b>Earnings</b>						
0.135***	0.033	6.478	No	No	No	74,726
0.113***	0.033	6.478	No	No	Yes	74,726
0.092**	0.033	6.478	No	Yes	No	74,726
0.114***	0.033	6.478	No	Yes	Yes	74,726
<b>0.164***</b>	<b>0.032</b>	<b>6.478</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>74,726</b>
<b>LFP</b>						
0.019***	0.005	0.93	No	No	No	74,726
0.017***	0.005	0.93	No	No	Yes	74,726
0.015***	0.005	0.93	No	Yes	No	74,726
0.017***	0.005	0.93	No	Yes	Yes	74,726
<b>0.023***</b>	<b>0.005</b>	<b>0.93</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>74,726</b>

Note: \*\*\*  $p < 0.001$ , \*\*  $p < 0.01$ , \*  $p < 0.05$ . Standard errors (SE) are clustered at the municipality level. Column (4), (5), and (6) indicate if background characteristics, time fixed effects (Time FE), and municipality fixed effects (MU FE), are included. Mean refers to the average of log-earnings in the treated group post-reform. The reform period is 2001.

**Table 3.** Regression results based on subsamples; Swedish-born women; Earnings and LFP

Panel A		DD, Swedish-born women earnings		
	Estimate	SE	Mean	N
(1)	(2)	(3)	(4)	(5)
<b>Baseline</b>	<b>0.164***</b>	<b>0.032</b>	<b>6.478</b>	<b>74,726</b>
Age 30-49	0.146***	0.039	5.776	52,270
Age 24-39	0.168***	0.034	5.655	66,920
Age 20-35	0.186***	0.036	5.806	58,394
Tertiary education	0.149***	0.043	6.314	35,412
Secondary education	0.146**	0.05	5.348	34,858
Children 2-5 and 7-10 yrs old	0.230**	0.081	5.493	13,206
Earnings less than the average	0.172**	0.059	3.106	24,690
Earnings less than 0.5 of the average	0.113	0.063	2.134	17,451
Panel B		DD, Swedish-born women LFP		
	Estimate	SE	Mean	N
(1)	(2)	(3)	(4)	(5)
<b>Baseline</b>	<b>0.023***</b>	<b>0.005</b>	<b>0.93</b>	<b>74,726</b>
Age 30-49	0.019***	0.005	0.909	52,270
Age 24-39	0.022***	0.005	0.903	66,920
Age 20-35	0.029***	0.005	0.912	58,394
Tertiary education	0.018***	0.005	0.949	35,412
Secondary education	0.018*	0.007	0.880	34,858
Children 2-5 and 7-10 yrs old	0.024*	0.012	0.885	13,206
Earnings less than the average	0.029*	0.012	0.691	24,690
Earnings less than 0.5 of the average	0.026	0.015	0.560	17,451

Note: \*\*\*  $p < 0.001$ , \*\*  $p < 0.01$ , \*  $p < 0.05$ . Standard errors (SE) are clustered at the municipality level. Controls: Yes. Time FE: Yes. Municipality FE: Yes. Mean refers to the treated group in the post-reform period. The reform period is 2001.

**Table 4.** Regression results; Swedish-born and Immigrant women; Earnings and LFP

DD, Swedish-born women		Immigrants, differential effect		DD, immigrant women		Mean	Controls	Time FE	MU FE	N
Estimate	SE	Estimate	SE	Estimate	SE					
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Earnings										
0.135***	0.033	-0.403***	0.093	-0.268**	0.087	5.643	No	No	No	88,715
0.111***	0.033	-0.366***	0.092	-0.256**	0.086	5.643	No	No	Yes	88,715
0.089**	0.033	-0.337***	0.092	-0.247**	0.086	5.643	No	Yes	No	88,715
0.114***	0.033	-0.372***	0.093	-0.258**	0.087	5.643	No	Yes	Yes	88,715
<b>0.163***</b>	<b>0.032</b>	<b>-0.215*</b>	<b>0.085</b>	<b>-0.052</b>	<b>0.079</b>	<b>5.643</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>88,681</b>
Employment										
0.019***	0.005	-0.059***	0.016	-0.040**	0.015	0.816	No	No	No	88,715
0.016***	0.005	-0.056***	0.016	-0.039**	0.015	0.816	No	No	Yes	88,715
0.014**	0.005	-0.053***	0.016	-0.038**	0.015	0.816	No	Yes	No	88,715
0.017***	0.005	-0.056***	0.016	-0.039***	0.015	0.816	No	Yes	Yes	88,715
<b>0.023***</b>	<b>0.005</b>	<b>-0.027</b>	<b>0.014</b>	<b>-0.004</b>	<b>0.014</b>	<b>0.816</b>	<b>Yes</b>	<b>Yes</b>	<b>Yes</b>	<b>88,681</b>

Note: \*\*\*  $p < 0.001$ , \*\*  $p < 0.01$ , \*  $p < 0.05$ . Standard errors (SE) are clustered at the municipality level. Columns (8), (9), and (10) indicate if background characteristics, time fixed effects, and municipality fixed effects, are included. Mean refers to the average of log-earnings in the treated group post-reform. The reform period is 2001.

**Table 5.** Regression results based on subsamples; Swedish-born and Immigrant women; Earnings and LFP

Panel A. Earnings	DD, Swedish-born women		Immigrants, differential effect		DD, immigrant women		Mean	N
	Estimate	SE	Estimate	SE	Estimate	SE		
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<b>Baseline</b>	<b>0.163***</b>	<b>0.032</b>	<b>-0.215*</b>	<b>0.085</b>	<b>-0.052</b>	<b>0.079</b>	<b>5.643</b>	<b>88,681</b>
Age 30-49	0.147***	0.039	-0.198	0.120	-0.052	0.114	5.341	60,129
Age 24-39	0.168***	0.034	-0.203*	0.096	-0.034	0.090	5.179	77,761
Age 20-35	0.186***	0.036	-0.236*	0.093	-0.050	0.086	4.990	69,122
Tertiary	0.149***	0.043	-0.240	0.135	-0.091	0.128	5.562	41,622
Secondary education	0.147**	0.05	-0.129	0.134	0.018	0.125	4.744	40,259
Children 2-5 and 7-10 yrs old	0.236**	0.009	-0.316	0.059	-0.080	0.166	4.520	26,202
Earnings less than the average	0.176**	0.058	-0.118	0.077	0.058	0.051	2.098	36,551
Earnings less than 0.5 of the average	0.119	0.063	-0.075	0.074	0.044	0.039	1.287	28,836
Panel B. LFP								
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<b>Baseline</b>	<b>0.023***</b>	<b>0.004</b>	<b>-0.027</b>	<b>0.014</b>	<b>-0.004</b>	<b>0.014</b>	<b>0.816</b>	<b>88,681</b>
Age 30-49	0.019***	0.005	-0.021	0.02	-0.002	0.019	0.828	60,129
Age 24-39	0.022***	0.005	-0.023	0.016	-0.001	0.016	0.813	77,761
Age 20-35	0.029***	0.005	-0.031	0.016	-0.002	0.015	0.789	69,122
Tertiary education	0.018***	0.005	-0.033	0.022	-0.014	0.022	0.841	41,622
Secondary education	0.018*	0.007	-0.001	0.023	0.017	0.022	0.785	40,259
Children 2-5 and 7-10 yrs old	0.025*	0.012	-0.043	0.031	-0.018	0.029	0.734	26,202
Earnings less than the average	0.030	0.012	-0.020	0.016	0.010	0.011	0.470	36,551
Earnings less than 0.5 of the average	0.027	0.015	-0.020	0.018	0.007	0.010	0.340	28,836

Note: \*\*\* p < 0.001, \*\* p < 0.01, \* p < 0.05. Standard errors (SE) are clustered at the municipality level. Controls: Yes. Time FE: Yes. Municipality FE: Yes. Mean refers to the treated group in the post-reform period. Different effects of individual characteristics on labour outcomes of immigrant and Swedish-born women are assumed. The reform period is 2001.