

Danzer, Natalia; Halla, Martin; Schneeweis, Nicole; Zweimüller, Martina

**Conference Paper**

## Parental Leave, (In)formal Childcare and Long-term Child Outcomes

Beiträge zur Jahrestagung des Vereins für Socialpolitik 2017: Alternative Geld- und Finanzarchitekturen - Session: Applied Microeconomics II, No. E03-V1

**Provided in Cooperation with:**

Verein für Socialpolitik / German Economic Association

*Suggested Citation:* Danzer, Natalia; Halla, Martin; Schneeweis, Nicole; Zweimüller, Martina (2017) : Parental Leave, (In)formal Childcare and Long-term Child Outcomes, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2017: Alternative Geld- und Finanzarchitekturen - Session: Applied Microeconomics II, No. E03-V1, ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft, Kiel, Hamburg

This Version is available at:

<https://hdl.handle.net/10419/168239>

**Standard-Nutzungsbedingungen:**

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

**Terms of use:**

*Documents in EconStor may be saved and copied for your personal and scholarly purposes.*

*You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.*

*If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.*

# Parental Leave, (In)formal Childcare and Long-term Child Outcomes\*

Natalia Danzer<sup>a,b</sup>, Martin Halla<sup>c,b,d</sup>, Nicole Schneeweis<sup>e,b,d,f</sup>, Martina  
Zweimüller<sup>e,d</sup>

<sup>a</sup>*Ifo, Institute for Economic Research, Munich, Germany*

<sup>b</sup>*IZA, Institute for the Study of Labor, Bonn, Germany*

<sup>c</sup>*University of Innsbruck, Austria*

<sup>d</sup>*Christian Doppler Laboratory for Aging, Health and the Labor Market, Austria*

<sup>e</sup>*Johannes Kepler University Linz, Austria*

<sup>f</sup>*CEPR, Centre for Economic Policy Research, London*

Preliminary version: February 28, 2017

Do not cite without permission

## Abstract

There is a strong debate about who should provide care to young children. Governments offer two alternative types of institutions: formal childcare and parental leave. We assess the effectiveness of these two competing institutions in promoting child development by comparing how a major parental leave extension from one to two years affected Austrian children's long-term outcomes in communities with and without formal childcare facilities for under-3-year-olds. Empirical identification of treatment effects is based on a sharp birthday cutoff-based discontinuity in the eligibility for extended parental leave and geographical variation in formal childcare. We find evidence that the counterfactual mode of care is decisive. If formal childcare is available, the reform induced a replacement of formal childcare by maternal care and had zero (or negative effects) on child outcomes. Whereas if formal childcare is *not* available, informal childcare was replaced by maternal care, and the reform improved child outcomes. This heterogeneity is driven by the additional time with the mother in the second year of the child's life and not by a change in maternal income. We conclude that care provided by mothers or formal institutions is superior to informal care-arrangements.

*JEL Classification:* J13, H52, J22, J12, I38.

*Keywords:* Parental leave, formal childcare, informal childcare, child development, maternal labor supply, fertility.

---

\*Corresponding author: Martina Zweimüller, Department of Economics, Johannes Kepler University Linz, Altenbergerstr. 69, 4040 Linz, Austria; email: [martina.zweimueller@jku.at](mailto:martina.zweimueller@jku.at). For helpful discussions and comments we would like to thank Joshua Angrist, Christian Dustmann, Anna Raute, Heather Royer, Steven Stillman, Andrea Weber, Rudolf Winter-Ebmer, Josef Zweimüller, and participants of seminars at the University of Linz, the Ski and Labor Workshop 2014 in Laax (Switzerland), the Annual Meeting 2014 of the Education Economics Committee of the Verein für Socialpolitik in Munich, the University of Mannheim, the Ifo Institute, the ESPE Annual Conference 2014 in Braga (Portugal), the University of Innsbruck, the Austrian Institute of Economic Research (WIFO), the Centre for European Economic Research (ZEW), the University of Salzburg, the National Taiwan University, the Free University of Bozen-Bolzano, the Swedish Institute for Social Research (SOFI) at Stockholm University, the Early Childhood Inequality Workshop 2016 in Nuremberg, and the Labor Economics Workshop 2016 of the Austrian Chamber of Labor. The usual disclaimer applies. This research was funded by the Austrian Science Fund (FWF): National Research Network S103, The Austrian Center for Labor Economics and the Analysis of the Welfare State. We also gratefully acknowledge financial support from the Austrian Federal Ministry of Science, Research and Economic Affairs (bmwfw) and the National Foundation of Research, Technology and Development.

# 1 Introduction

There is a strong public and private debate about who should provide care to very young children (Gregg and Waldfogel, 2005). This question gained importance due to increasing female labor force participation and the absence of grandparents within the household, who have been the most important providers of informal childcare. Governments in the Western world responded to these developments by offering two alternative types of institutions: formal childcare and parental leave policies. Two strands of the economic literature analyze the impact of these institutions on children and parents. Surprisingly, they are hardly connected and do not consider that parental leave and formal childcare promote competing models of family organization. Proponents of parental leave policies implicitly assume that mothers and more recently also fathers are the best care takers. In contrast, advocates of formal childcare prefer children to spend the day in a nursery and see the parents rather participating in the labor market. In this paper, we aim to assess the effectiveness of these two competing institutions in promoting child development.

We evaluate a large and generous parental leave extension with a focus on long-term child outcomes. In Austria, paid and job-protected parental leave was extended by 12 months in 1990. Mothers of children who were born on June 30 or earlier were eligible for one year of parental leave. Mothers who gave birth on July 1 or later were entitled to take parental leave until the child's second birthday. In the years before the reform, the availability of formal childcare for under-3-year-olds (provided by so-called nurseries) varied substantially across communities. At that time, about 67 percent of the population lived in communities without nurseries. Working parents had to rely informal care-arrangements after the child's first birthday. This setting provides us with the unique opportunity to shed light on the effects of parental leave policies across varying counterfactual modes of childcare. Before the reform, children of working mothers who lived in communities with nurseries had the possibility to attend a nursery, while their counterparts in communities without nurseries were in informal care settings, e. g. care provided by grandparents. The core of the paper is to evaluate, whether the parental leave reform had different effects on child outcomes, depending on the counterfactual mode of care (nursery versus grandparents).

This parental leave reform is particularly well-suited for our purposes not only because of its scale, but also because it affected virtually all working women: eligibility was extremely high and take-up rates were almost universal (Lalive and Zweimüller, 2009). Combining various sources of administrative data, we investigate how the parental leave extension affected children's education, labor market and health-related outcomes in communities with and without nurseries. Our data also allow us to deepen and enrich our analysis by investigating the reform's effects on other family members. We examine maternal labor supply, fertility and family stability up to 17 years after childbirth. The analysis of these potential transmission channels is important to fully understand how parental leave policies affect child development.

Our research design combines a regression discontinuity design with a difference-in-differences approach. We exploit the fact that the eligibility for extended parental leave was based on a birthday cutoff date (July 1, 1990). Thus, we compare families with children born shortly before and after the cutoff date. The reform was only announced around three months before the cutoff date, thus, sorting into treatment by planning conception can be ruled out. As parents can potentially postpone the date of delivery, we exclude children born 5 days before and after the cutoff date. Additionally, we use unaffected control cohorts to difference out potential seasonal or age effects.

On average, the reform improved child health outcomes, but had zero effects on educational and labor market outcomes. These effects mask substantial heterogeneity. While we find little variation across socio-economic status and child’s sex, we observe strong heterogeneity by the availability of formal childcare. The effects on child outcomes are zero (or negative) for children in communities with nurseries and positive in communities without nurseries. Thus, positive effects are only observed for families who substituted informal care-arrangements with maternal care and not for families who substituted a nursery with maternal care. This treatment effect heterogeneity is also observed in the analysis of family outcomes. In communities with nurseries, we find an increase in completed fertility, a reduction in maternal employment in the short-run (but not in the long-run), and some positive effects on family stability. In communities without nurseries, completed fertility did not change and maternal labor supply increased permanently. Notably, we provide evidence that these changing family circumstances are not the causal drivers of the treatment effects on children. Instead, the effects are predominantly driven by the additional time with the mother in the second year of the child’s life. This indicates that care provided by mothers (or nurseries) is superior to informal care-arrangements. The prolonged duration of parental leave also led to small changes in the available household income. However, we find no evidence for the relevance of these income effects.

Our results on potential channels fully confirm and expand the analyses by Lalive and Zweimüller (2009) and Lalive, Schlosser, Steinhauer and Zweimüller (2014), who investigated the short and medium-run effects of this reform on maternal labor supply and fertility. They show that the reform caused a substantial delay in the return to work and reduced maternal labor supply in the first years after childbirth. On average, mothers increased their time at home before returning to work by about 8 months. Accordingly, daily earnings dropped in the first three years. In the medium-run, there were no significant effects on labor supply and earnings. They also document that the parental leave extension brought about a significant rise in subsequent fertility among affected mothers and altered the spacing of births (within 10 years after childbirth). As our analysis on potential channels spans a time horizon of up to 17 years, we are able to show that the fertility effects are indeed long-lasting and thus resemble a change in completed fertility (instead of mere tempo effects).

So far, only one paper investigated the effects of this reform on child outcomes. Using data from the OECD’s Programme for International Student Assessment (PISA), Danzer and Lavy

(forthcoming) do not find any significant effects of the parental leave extension on proficiency scores in mathematics, reading and science at age 15 on average. However, their subgroup analysis uncovers significantly positive intention-to-treat effects on PISA scores for children (especially boys) of highly educated mothers. In contrast, the parental leave extension had zero (or negative) effects on PISA outcomes for children of less educated mothers. As the respective waves of PISA do not contain information on parental leave take-up, maternal employment, siblings, family status or childcare attendance, they cannot empirically explore and test potential mechanisms through which the reform may have affected child outcomes. Furthermore, it is unclear as to whether effects on schooling outcomes in math, reading and science at age 15 persist over time and translate into long-run effects on human capital and labor market outcomes.

We contribute to the existing literature in several ways. *First*, we advance the literature by conducting one of the most comprehensive long-run analysis of a parental leave reform on child outcomes. This is facilitated by the clear design of the policy reform and by the unique and superior data available in Austria. We provide new and important insights by carefully discussing and assessing potential transmission mechanisms through which the reform might have affected child outcomes. Moreover, we highlight the role of the alternative mode of care and provide first evidence on how the type of alternative care shapes the effect of extended parental leave on child outcomes.

[Table 1]

Design-based papers, most of which focus on Nordic countries, have exploited unanticipated changes in paid parental leave to evaluate the importance of parental leave and early maternal employment for child development (see overview in Table 1). The large majority of these studies focuses on schooling outcomes, only two papers estimate long-run effects on adult labor market outcomes (Carneiro, Løken and Salvanes, 2015; Dustmann and Schönberg, 2012). One group of papers examines parental leave extensions within the first year after birth. The dominant finding of these studies is that parental leave in the first year has no effect on child development captured by education and labor market outcomes (see Rasmussen (2010) for Denmark, Baker and Milligan (2010, 2015) for Canada, Dahl, Løken, Mogstad and Salvanes (2016) for Norway (1987-92) and Dustmann and Schönberg (2012) for Germany). The only exception is the study by Carneiro et al. (2015) which documents significant positive effects on long-term child outcomes of the 1977 reform in Norway. Another group of studies looks at parental leave extensions in the second and third year after birth. Liu and Skans (2010) find positive effects for children of highly educated mothers in Sweden and Dustmann and Schönberg (2012) find some negative effects for Germany.

The reasons for these differences in findings are not well understood. Potential sources are institutional differences in the parental leave systems and methodological differences across

studies. The most important institutional aspects are the timing and length of leave, paid vs. unpaid leave and the level of income replacement. An important methodological flaw may result from incomplete information on actual parental leave take-up. Many existing studies are restricted to estimate intention-to-treat effects on children as they cannot match child with maternal information. Since we have perfect information on parental leave take-up, we can estimate local average treatment effects for most of our outcomes. If seasonal or age effects are important, studies may also differ in their ability to control for these effects. Another potential explanation is that treatment effects depend on the counterfactual mode of care. This explanation has been largely ignored by the previous literature.<sup>1</sup> Our findings point to the importance of the counterfactual mode of care.

*Second*, we contribute to the literature on the role of formal versus informal childcare for child development and human capital formation. In particular, we add to the scarce quasi-experimental evidence on the impact of formal childcare for children below the age of 3. **DO WE?** With the exception of two studies (Felfe and Lalive, 2014; Drange and Havnes, 2015), both of which find positive effects of very early childcare on child development, all existing studies on childcare focus on children aged 3 and above and provide mixed evidence (Bernal and Keane, 2011; Datta-Gupta and Simonsen, 2010; Havnes and Mogstad, 2011, 2015; Baker, Gruber and Milligan, 2008; Herbst, 2013; Gathmann and Sass, 2012). Better knowledge and more evidence for the impact on the very young is urgently needed, as nowadays, about one third of the under-three-year-olds in OECD countries attend formal childcare, and this upward trend seems to continue further.<sup>2</sup>

Children may benefit from participation in formal day-care centers through professional child-minders as well as through interaction with other children. Existing empirical evidence based on children *above 3 years of age* suggests that increased informal childcare (i.e., non-center based childcare by grandparents or other relatives) has negative effects on the cognitive development of children of single mothers, while an additional year of center-based childcare has no adverse effects (Bernal and Keane, 2011). Similarly, Datta-Gupta and Simonsen (2010) find no detrimental effects of participation in center-based care with highly qualified personnel in Denmark, but negative effects of home-based family day care (in private homes by one child-minder), especially for boys from less educated mothers. Using German data, Gathmann and Sass (2012) estimate negative effects of an increase in maternal care (and a corresponding decrease in formal and informal care) on cognitive and non-cognitive skills for girls. Exploiting an exogenous rise in subsidized formal childcare in Norway, which mainly crowded out informal care, Havnes and Mogstad (2011) find significant improvements in long-run educational out-

---

<sup>1</sup>A recent paper by Kline and Walters (2016) shows that the impact of Head Start on test scores differs between children with alternative modes of care. While children who would have otherwise been at home experience a short-run increase in test scores, children who would have otherwise attended other preschools are not significantly affected.

<sup>2</sup>Enrollment rates vary between 3.1 and 67 percent (Slovak Republic and Denmark, respectively). For instance, in Norway, the enrollment rate of under-three-year-olds has increased from 22 to 54.3 percent between 1995 and 2013. Source: OECD Family Database.

comes and labor market attachment. This study also reveals substantial heterogeneity in the effect of formal childcare, with positive effects for children from medium and low-income households and negative effects for children in the uppermost part (see also Havnes and Mogstad, 2015). In contrast, Baker et al. (2008) and Herbst (2013) find adverse effects on child outcomes of formal and informal care in comparison to maternal care for Canada and the USA. Regarding children *below 3 years of age*, Felfe and Lalive (2014) exploit variation in the provision of early childcare across regions and over time in Germany and find positive effects of early childcare for young children, especially boys and for children from low SES families. Drange and Havnes (2015) estimate the causal effect of early childcare before the age of two using the random starting age of early childcare created by a lottery following oversubscription in Oslo, Norway. They find positive effects on language and numeracy skills at age 7.

*Third*, we show that parental leave policies and formal childcare are important aspects of the early-childhood environment, thereby contributing to the literature that emphasizes the importance of the early-childhood environment for the production of human capital (Cunha et al., 2006). Thus, our findings add to a very dynamic policy debate (Elango et al., 2016; Rossin-Slater, 2017). Countries spend a substantial amount of money on parental leave benefits and formal childcare. Among OECD countries, public spending on maternity and parental leave cash-benefits accrued on average to 0.38 percent of national GDP or to almost 20 percent of total public spending on family policies in 2011.<sup>3</sup> In comparison, total family-related public expenditure comprised 2.24 percent of GDP on average, expenditure related to early childhood education and care amount to 0.71 percent.<sup>4</sup> Given the increasing shares of working mothers and under-three-year-olds enrolled in formal day-care centers, our analysis provides unique and timely insights into the interplay of maternal employment, parental leave policies and formal childcare.

The remainder of the paper is organized as follows. Section 2 provides details on the Austrian parental leave reform and other relevant aspects of the institutional setting. Section 3 introduces our data. Section 4 presents our research design. We define our treatment, assignment and outcome variables, lay out our estimation strategy, and spell-out the identifying assumptions. Section 5 presents our results. First, we show the effects of the parental leave reform on child outcomes in general and with regard to the available alternative mode of care. We also provide an in-depth discussion of the nature of the identified treatment effects. Then, we present evidence on various potential transmission mechanisms, such as fertility behavior, maternal labor supply and family stability. Last, we provide several robustness checks. Section 6 concludes the paper and discusses potential policy implications.

---

<sup>3</sup>Among countries with positive spending the share varies between 0.02 percent (Turkey) and 1.38 percent (Estonia). Source: OECD Social Expenditure Database for 2011.

<sup>4</sup>Total family-related expenditure in 2011 ranges from 0.02 to 4.05 percent of GDP (for Turkey and Denmark, respectively); expenditure on early childhood education and care ranges from 0 (only Turkey) to 2.01 (Denmark) percent (OECD 2016).

## 2 Institutional background

In this section, we briefly describe the parental leave system before and after the 1990 reform. For a better understanding of the Austrian institutional background, we also provide information on female labor force participation, the availability and the characteristics of formal childcare, and the use of informal care.

### 2.1 The Austrian parental leave system and its reform in 1990

We exploit exogenous variation in the duration of parental leave induced by a policy reform in the year 1990. Before the reform, mothers were eligible for parental leave up to the child's first birthday. While on parental leave the mother enjoyed job protection and received a flat-rate transfer. The reform extended the parental leave entitlement by 12 months. In particular, all eligible mothers giving birth on or after July 1, 1990, became entitled for paid and job-protected parental leave up to the child's second birthday.

The eligibility criteria for parental leave and the associated transfer payments, as well as the maternity leave regulations remained unaffected by the reform. Maternity leave, which precedes parental leave, mandates a compulsory leave period of 8 weeks before and after delivery for all working mothers. This period is extended in the case of medical complications, a multiple birth, or a Caesarean section. During maternity leave, mothers receive a transfer payment that amounts to 100 percent of the average net earnings of the preceding 13 weeks (*Wochengeld*). Furthermore, they cannot be dismissed by their employer until 4 months after delivery. To become eligible for parental leave, mothers need to be in employment (subject to compulsory social insurance contributions) for at least 52 weeks during the two years preceding the first birth. For young mothers (below 25 years if age) 20 weeks of equivalent employment during the last 52 weeks are sufficient. During the parental leave period, eligible women receive a monthly transfer payment of €352 (in 2015 values).<sup>5</sup> This corresponds to about 40 percent of net median female earnings.

The only further change (besides the leave duration), caused by the 1990 reform, concerns the specific eligibility criteria for mothers who give birth while being on parental leave. Under specified conditions mothers were allowed to transition from one parental leave spell to the next without fulfilling the work criteria (i.e., without returning to the labor market). Before the reform, an automatic parental leave renewal would apply to all mothers giving birth to another child within 15.5 months after the previous birth. Due to the rather tight time window, this automatic renewal was barely utilized. After the reform, this time frame was extended by 12

---

<sup>5</sup>Non-married mothers who do not live in the same household with the child's father and who do not receive sufficient child support from him, and married mothers whose husbands earn no or low income received about 50 percent higher assistance.



months, and enabled more women to transition from one parental leave spell to another. Since we focus on first-born children, this aspect of the reform affects our analysis only indirectly.<sup>6</sup>

Several features of the reform make it particularly suitable for our analysis, as they allow for a clean identification of causal effects. *First*, the reform was implemented with a clear cutoff date, and there were no transition rules. Hence, entitlement to the extended leave period was strictly limited to mothers giving birth on or after the cutoff date. *Second*, the parental leave extension was announced and implemented at relative short notice. It passed the Austrian parliament only in April 1990 and it was first publicly discussed in mid-November 1989, about 7.5 months before it came into effect (Lalive and Zweimüller, 2009). This precluded parents from adjusting the timing of conception in order to take advantage of the more generous parental leave regime. There is also no evidence that parents postponed the delivery date. *Third*, the reform affected the vast majority of mothers, since almost all first-time mothers were eligible and parental leave take-up among eligible mothers was almost universal. *Fourth*, the reform increased the average parental leave duration substantially.

## 2.2 Female labor force participation

In 1990, about 64 percent of all Austrian women between the age of 25 and 54 were participating in the labor market. This *overall* female labor force participation rate was low as compared to Scandinavia and Anglo-Saxon countries, comparable to Germany, and well above Southern Europe.<sup>7</sup> For the population of our primary interest, i. e., women without children, substantially higher participation rates are observed. Based on Census data from the year 1991, we calculate a labor force participation rate of 88.3 percent for women without children (between the age of 25 and 40).<sup>8</sup> Hence, the vast majority of these women should be eligible for parental leave, and should be affected by the reform. According to data from the *Austrian Birth Register*, about 90 percent of women, having their first birth in 1990, were employed (i. e., on maternity leave) at the time of birth. This figure matches very well with the share of eligible mothers that we calculate from our administrative data (*Austrian Social Security Database*) based on exact information on prior employment.

---

<sup>6</sup>Lalive and Zweimüller (2009) show that the change in the automatic renewal regulation affected the timing and spacing of second births.

<sup>7</sup>According to estimates of the *International Labour Office*, the overall female labor force participation rates in the year 1990 were 90.9 in Sweden, 87.7 in Denmark, 79.1 in Norway, 74.0 in the United States (US), 73.0 in the United Kingdom (UK), 63.3 in Germany, 63.8 in Austria, 52.4 in Italy, and 51.8 in Greece. Over time, the Austrian overall female labor force participation rate has increased. Since the early 2000s the Austrian rate has been above 80. Austria overtook the US and the UK, and is approaching to Scandinavian levels. Source: ILOSTAT Database (accessed on September 20, 2016).

<sup>8</sup>For their counterparts with one and two children the participation rates are 78.0 and 57.8 percent, respectively.

## 2.3 Formal and informal childcare

The Austrian system of formal childcare traditionally distinguishes between facilities for children below the age of 3 (nurseries, *Kinderkrippe/Krabbelstube*) and those for children above 3 (kindergarden, *Kindergarten*). While the vast majority of communities have offered a kindergarden since the 1980s, nurseries are, even in 2011, only available in about 11 percent of all communities. In the year 1990, about 82 percent of all communities had a kindergarden and about 2 percent a nursery. Table 2 summarizes the development between 1988 and 1995.

[Table 2]

The share of communities with nurseries was below 2 percent between 1988 and 1991 and increased to around 3 percent until 1995. Thus, in the years around the parental leave reform the presence of formal childcare for under-3-year-olds did not substantially change, and we find a relatively fixed geographical variation in the availability of nurseries. The existing nurseries were predominantly in more densely populated areas. Therefore, the share of the covered population (around 33 percent) is substantially larger as compared to the share of communities.

The upper panel of Figure 1 plots enrollment rates of children below age 1, children between the age of 1 and 2 and children between the age of 2 and 3 for communities *with* nurseries. We calculate these enrollment rates using data on the number of enrolled children by age and community and the number of children in the respective birth cohort and community.<sup>9</sup> Between 1988 and 1990 enrollment rates were below 0.5 percent for children below age 1, around 8 percent for 1-year-old children and around 17 percent for 2-year-old children. As expected, due to the parental leave reform, the enrollment rate of 1-year-old children decreased strongly. In 1992, only 4 percent of 1-year-old children were enrolled in formal childcare, whereas no such decrease is observed for the other two age groups. Thereafter, enrollment rates have been increasing for all age groups, which reflects a secular trend.<sup>10</sup>

[Figure 1]

Assuming that mainly children of working mothers attended formal childcare, enrollment rates for children of these mothers should actually have been higher.<sup>11</sup> We approximate en-

---

<sup>9</sup>Own calculations based on official statistics on children in center-based care (Statistics Austria, Kindertagesheimstatistik, Statcube, retrieved on November 17, 2016) and the Austrian Birth Register.

<sup>10</sup>It has to be noted that these figures cannot be directly compared with our estimation sample of children in communities with nurseries for the following reasons. These enrollment rates include all children while our sample is restricted in several ways: We include only first-born children who were born in Austria and whose mothers have worked before child birth (i.e., were eligible for parental leave). These differences may lead to a substantially higher enrollment rate in our estimation sample. The enrollment rate of Austrian born children is likely higher compared to migrant children. The decline in the labor force participation rate of mothers (from 78 percent for mothers with one child to 58 percent for mothers with two children) suggests that first-born children have a higher enrollment rate than children with siblings.

<sup>11</sup>This assumption seems to be highly plausible since one prerequisite for attending formal childcare is that both parents work at least 20 hours per week.

rollment rates for (full-time) employed mothers by using only children of (full-time) employed mothers as a denominator in our calculation of enrollment rates. More specifically, we use the (full-time) employment rate of pre-reform mothers in the second year of the child's life to adjust the denominator. Only 35 percent of these mothers were employed (21 percent were employed full-time) even in the absence of parental leave entitlement. This leads to adjusted enrollment rates of around 22 percent and around 35 percent between 1988 and 1990 for children of employed and full-time employed mothers, respectively (see lower panel of Figure 1). Thus, the PL reform potentially induced a replacement of formal childcare by maternal care for a substantially higher share of children than suggested by the unadjusted enrollment rates.

The vast majority of nurseries are public and typically operated by the respective community. In 1990, about 70 percent of enrolled children were in a public nursery. Private nurseries operate under the same regulatory environment, receive substantial subsidies, and have to fulfill pre-defined quality standards. The operators of private nurseries are non-profit associations (17.6 percent of children), private persons (11.0 percent of children) and other entities (1.4 percent). The effective average group size in nurseries was about 15 children in 1990, and there were about 2 educators per group. On average, 1.5 of these graduated from a college for nursery education (ISCED level 4B degree). About 92 percent of children stay all-day in their nursery.<sup>12</sup>

Information about fees for formal childcare institutions in the early 1990s is sparse. Own estimates based on the Austrian Microcensus from 1995 reveal that the average monthly expenditure on formal childcare for 2 year-olds was about 1,300 Austrian Schilling (€136)<sup>13</sup> per child, considering a standard care arrangement of about 4 to 6 hours per day. For children having lunch at the childcare facilities average expenditure rose to about 2,100 Austrian Schilling (€220). These costs correspond to about 10 to 17 percent of average monthly earnings of women at that time.<sup>14</sup>

The availability and structure of informal childcare is comparably harder to describe, since we have to rely on survey data. The most recent pre-reform survey data including detailed information on informal childcare is from the year 1983. In this year, the Austrian Microcensus included a special supplement on childcare. According to this data, about 63 percent of children from working mothers were in any type of informal care-arrangements during their second year of life on a weekly or daily basis. Among the informal care-arrangements, the by far most common care provider were grandparents (89 percent), followed by other relatives (10 percent). Nannies (or other forms of paid help) were very uncommon at that time. Obviously, it is hard to assess the quality of these informal care-arrangements. At least, we can note that average

---

<sup>12</sup>Own calculations based on official statistics on children in center-based care (Statistics Austria, Tagesheimstatistik, Statcube, retrieved September 9, 2016).

<sup>13</sup>All Euros are expressed in 2015 values.

<sup>14</sup>According to our estimates from the Austrian Microcensus 1995, average monthly earnings of employed women in childbearing age (aged 20 to 45) were about 12,500 Austrian Schilling (€1,304), when considering only women working 35 hours or more, this average wage was about 14,000 Austrian Schilling (€1,461).

grandparents' level of formal education, was significantly lower as compared to that of the average educator in a nursery.

To summarize, childcare options of working mothers were regionally dispersed. Hence, for children born in communities without nurseries, the parental leave extension in 1990 implied a shift from informal childcare by mostly grandparents to maternal care in the second year of life. In contrast, for children born in communities with nurseries, the 1990 reform also resulted in a substitution of formal childcare by maternal care. As it turns out, these different alternative or counterfactual modes of non-parental childcare crucially determine the effect of the parental leave extension on child outcomes.

### 3 Data

We construct our main data set by combining various administrative data sources. In this data set, we observe the universe of births with detailed information on families' socio-economic background. Most importantly, we can follow the mother and the child over time along different aspects of life. The *Austrian Social Security Database* (ASSD) provides information on the mother's eligibility for parental leave, her actual take-up, her return to work behavior, the child's labor market behavior, and any other event that is relevant for pension claims, such as military service.<sup>15</sup> The *Austrian Birth Register* enables us to closely track subsequent fertility behavior. The *Austrian Marriage Register* and the *Austrian Divorce Register* document any change in marital status. Finally, a database provided by the *Ministry of Labour, Social Affairs and Consumer Protection* includes information on current formal education (school or college attendance) and disability status. We use these data sets to generate our treatment variable, our assignment variable, and a comprehensive set of covariates. On the family level we define outcome variables such as current family size, maternal labor supply and marital status. Finally, we define a number of children's medium and long-term outcomes in the domains of labor market and health. We restrict our analysis to first-born singleton children of mothers, who were between 15 and 45 years of age at the time of birth and eligible for parental leave.<sup>16</sup>

To obtain further educational outcomes, we use PISA data from 2003 and 2006 and data from the Educational Register of the city of Linz (EducReg).<sup>17</sup> These data sets have several drawbacks as compared to our main data set. First, these data do not cover the universe of births. PISA includes a sample of about 5,000 children who are 15/16 years old at the time of testing, thus, in PISA 2006, the 1990 birth cohort was sampled and in PISA 2003, we observe the

---

<sup>15</sup>The ASSD includes administrative records to verify pension claims and is structured as a matched employer-employee data set. We observe for each individual on a daily basis where she is employed, along with her occupation, experience, and tenure. Information on earnings is provided per year and per employer. The limitations of the data are top-coded wages and the lack of information on (contracted) working hours (Zweimüller et al., 2009).

<sup>16</sup>There is no difference in the share of twins or multiple births before and after the reform cutoff date.

<sup>17</sup>Linz is the third-largest city of Austria and the capital of the state of Upper Austria. Upper Austria is one of nine federal states in Austria. It comprises about one sixth of the Austrian population and workforce.

1987 birth cohort. The EducReg includes all children residing in the city of Linz. Second, since these data do not include information on mother’s eligibility or actual parental leave take-up, we can only estimate the *intention-to-treat effect* (ITT). However, given the very high take-up rate, the difference between the ITT and the *local average treatment effect* (LATE) can be expected to be very small. Third, we cannot impose the same sample restrictions because these data sets lack information on birth order, multiple births, and the exact birth date (only month of birth is available). Fourth, the set of covariates is smaller. For instance, we do not have information on the country of birth in the EducReg.<sup>18</sup> Fifth, the PISA and education register data lack information on community of birth. Hence, the availability of formal childcare is not observed in these samples. In the analysis of PISA data we proxy for childcare availability by stratifying the sample by school location. We use the number of inhabitants in the community the school is located, and assume that communities with  $\geq 100,000$  inhabitants had a nursery in 1990 whereas communities with  $< 100,000$  had no nursery. In the education register data for the city of Linz, we have to assume that the mothers have been living in Linz at the time of birth. With more than 180,000 inhabitants, nurseries have been available in 1990.

## 4 Research design

We estimate the effect of the parental leave extension using a fuzzy regression discontinuity difference-in-differences (RD-DiD) approach. In this set-up, the treatment resembles a prolonged duration of paid and job-protected parental leave (PL) up to the child’s second birthday. The assignment into treatment depends on whether a child is born in the post-reform period (July 1, 1990) or not. To identify the treatment effect, we exploit the discontinuity in duration of PL at the reform date and compare maternal, family and child outcomes of children born shortly before and after the reform. Additionally, we use unaffected control cohorts to difference out potential seasonal or age effects. In our regressions, we include children born in 1989 (and 1987 when using PISA data) as a control cohort.

### 4.1 Treatment and assignment variable

The treatment variable is defined as the duration of PL. The maximum PL duration is 10 months before and 22 months after the reform. The assignment into treatment depends on whether a child is born in the post-reform period (on July 1, 1990 or later) or not. Panel A of Figure 2 depicts the relationship between assignment and treatment for eligible mothers. It plots, by birthdate, the average duration of PL measured in days excluding the post-birth maternity leave period. As expected, we observe a distinctive jump in the duration of PL at the cutoff. The average duration of PL jumps discontinuously from 285 to 590 days. Given

---

<sup>18</sup>To avoid any attenuation bias in our estimates due to children living abroad in the year 1990, we exclude all students with foreign language/citizenship. This is particularly important since Austria witnessed a large influx of migrants past 1993.

that the average post-birth maternity leave period is about 56 days, we can conclude that the average duration of PL has increased due to the reform from almost one year to almost two years. Panel B shows for comparison the average PL duration for the year before the reform. There is no jump around July 1 and no intra-year variation at all.

[Figure 2]

*Eligibility and take-up rate* This clean and large jump results from two facts, which make this reform particularly useful from a methodological perspective. First, the share of mothers who were eligible for PL before and after the reform is very high (about 90 percent), and the reform has not changed the eligibility criteria. Panels C and D of Figure 2 depict the share of eligible mothers pre and post reform. Second, the actual take-up of PL is almost universal in both periods and amounts to around 97 percent. In both cases, there is no discontinuous change in the respective share around the cutoff date. Panels E and F of Figure 2 refer to the take-up rate among the eligible mothers.

## 4.2 Outcome variables

*Children’s educational outcomes* First, we analyze PISA test scores in the fields of mathematics, science and reading. Children were tested at the age of about 15. Further, we check which school track the child attended in grades 5, 8 and 9. Austria has a system of early tracking. After primary school, students are allocated to two different educational tracks. Higher secondary schools (the *high track*) comprise a first stage (grades 5 to 8) and a second stage (grades 9 to 12), provide advanced education, and conclude with a university entrance exam. Lower secondary schools (the *low track*) comprise grades 5 to 8, provide basic general education and prepare students for vocational education either within an intermediate vocational school or within the dual education system. The dual education system combines apprenticeship in a firm and (vocational) education at a vocational school. In the EducReg sample (Linz), we observe school tracks in grades 5 and 8. About 42 and 39 percent are in the high track, respectively. These shares are above the national average.<sup>19</sup> In the PISA sample, which is representative for Austria, about 63 percent of students are in the high track in grade 9.<sup>20</sup> Table 3 summarizes these and all other outcome variables.

[Table 3]

---

<sup>19</sup>Data for the school year 2005/06 show that around 30 percent of all Austrian children attended the high track in grade 8. This share was higher in urban areas, 37 percent in Linz and 46 percent in Vienna (Schneeweis and Zweimüller, 2012, see Figure 3).

<sup>20</sup>If graduates from the low track want to attend college, they have to transfer to the high track after grade 8. This transition is tough in practice; especially in urban areas where the quality of the low track tends to be inferior.

*Children's labor market outcomes* The majority of the students who graduate from the low track enter the work force at around age 16, ideally via the dual education system, or as unskilled workers. We analyze children's labor market outcomes starting from the age of 17 and follow them until they are 23. To capture the fact that Austrian children in this age cohort are either productive in school and/or in the labor market, we define the outcome variable 'active'. Children are categorized as active, if they are either in education (school, apprenticeship or university), employed, in military (or alternative civilian) service, or on maternity/parental leave. Inactive children are either unemployed, only marginally employed, disabled, on sick leave or rehabilitation or in other kinds of inactive social security insurance periods. In particular, we define binary variables capturing children's activity status at the age of 17 and 23. While almost 98 percent of all children are active at the age of 17, this share drops to about 90 percent at the age of 23. We also define a variable that captures the share of active periods between the age of 17 and 23 (87 percent) and a binary variable for children who have been active in each period during this age range. This variable has a mean of about 49 percent. To further explore the type of activity, we define binary indicators for being in education (26 percent) and in employment (60 percent). Finally, we check for any treatment effects on the log of wage.

*Children's health outcomes* We use two binary outcome variables to assess children's health. The first variable assess the disability status of the child up to the age of 23. We exploit available information on the receipt of increased family allowance, which is granted for any child with a disability rating of at least 50 percent (or in the event of permanent earning incapacity). The disability can be either physical or mental. The rating is performed by the regional chief medical officer. We define a variable equal to one, if the child has never been disabled according to this definition up to 23 years of age. The vast majority of children (98.6 percent) is not disabled. The second variable indicates whether male children are fit for military service. In Austria, all male citizens are subject to compulsory military service and have to enlist and muster for different medical examinations within one year after attaining their 17th birthday. These examinations last for two days and show whether the individual is physically and mentally able to serve in the military. In our sample, 78 percent of boys are fit for military service. This percentage is in line with official statistics (74 percent in 2006, Statistik Austria (2008)).

*Mothers' outcomes* We examine mothers' labor market behavior up to 17 years after the birth of their first child. The analysis of maternal labor supply is based on two variables measuring the extensive and the intensive margin. The extensive margin is captured by binary indicators equal to one if mothers are employed  $t$  years after parity one. Since we do not observe (contracted) working hours in our data, we have to approximate full-time employment based on earnings in order to obtain a measurement for the intensive margin. We define mothers as full-time employed  $t$  years after parity one, if they earn a real daily wage of at least 75 percent of

their average pre-birth earnings (over the last two years before birth).<sup>21</sup> Ten years after parity one, about 58 percent of mothers are employed, and about 36 percent are full-time employed.

*Family size and stability* Finally, we look at family size and family stability up to 17 years after parity one. Family size is measured as the total number of live births  $t$  years after parity one. In our sample, the average number of live births is about 1.9 ten years after parity one. To assess family stability, we check whether parents are legally married  $t$  years after parity one. About 52 percent of children from these cohorts were born out-of-wedlock. Thus, potential post-birth changes in family status comprise marriage and divorce. Ten years after parity one, about 59 percent of parents are legally married.

### 4.3 Econometric model

We exploit the sharp birthday cutoff-based discontinuity in the eligibility for extended PL to estimate treatment effects on all outcomes discussed above. While the relationship between assignment and treatment is very strong, it is not fully deterministic and we have to set-up a *fuzzy regression discontinuity design*. We use the assignment into treatment as an instrumental variable (IV) for the endogenous treatment variable. The design can be translated into the following *two-stage least squares* (2SLS) setup:

$$PL_i = \alpha_1 + T_i\beta_1 + X_{i,t=0}\gamma_1 + \delta_{1y} + \theta_{1m} + v_i \quad (1)$$

$$O_i = \alpha_2 + \hat{PL}_i\beta_2 + X_{i,t=0}\gamma_2 + \delta_{2y} + \theta_{2m} + w_i \quad (2)$$

In the first-stage equation (1), the dependent variable is  $PL_i$ , the actual duration of parental leave measured in years. The assignment variable  $T_i$  is equal to one if a child is born post-reform. The vector of covariates  $X_i$  comprises information on maternal age at birth (in categories; 15-20, 21-25, ... 41-45), maternal socio-economic background (SES)<sup>22</sup>, maternal migration status, the sex of the child, and whether the child was born pre-term.

We use a two month window around the cutoff date and include all children born in June or July in 1990. To account for any unobserved characteristics that follow a seasonal pattern between children born in June and July, we pool information on unaffected control cohorts from the year 1989.<sup>23</sup> Thus, we use information on all births from June and July in 1990, and June and July in 1989. This data-structure allows us to control for birth year effects ( $\delta_{1y}$ ) and birth

---

<sup>21</sup>Note that our sample consists of mothers who gave birth to their first child. Most likely these mothers worked full-time before giving birth. Only 9.8 percent of women aged between 15 and 44 who were employed in 1990 and had no children worked below 35 hours per week (Statistik Austria, 1990). We are fully aware that not all changes in wages are due to changes in working hours but also due to job mobility, promotions and demotions.

<sup>22</sup>We form two groups based on education and pre-birth earnings. We classify mothers with compulsory school, apprenticeship training or intermediate vocational school and below median pre-birth earnings as low SES mothers. High SES mothers are those with at least higher school, apprenticeship training or intermediate vocational school and above median pre-birth earnings.

<sup>23</sup>There is some evidence for the US (Buckles and Hungerman, 2013) that children born at different times of the year are born to mothers with significantly different characteristics. There is evidence from Austria



month effects ( $\theta_{1m}$ ). This DiD component of our approach assumes that unobserved seasonality is constant across 1989 and 1990.

In the second-stage equation (2), we regress the respective outcome variable  $O_i$  on the predicted PL duration from the first stage  $\hat{PL}_i$ . This allows us to interpret  $\hat{\beta}_2$  as a LATE, that is, the causal effect of an additional year of PL due to being assigned to the new regulations. In our complementary data sets, which we use to examine educational outcomes, we do not observe the actual duration of PL. Therefore, we will estimate the ITT effect given in equation (3).

$$O_i = \alpha_3 + T_i\beta_3 + X_{i,t=0}\gamma_3 + \delta_{3y} + \theta_{3m} + e_i \quad (3)$$

Three conditions need to hold for  $\hat{\beta}_2$  to be informative about the causal effect of an additional year of PL. First, the assignment to the increased PL duration  $T_i$  must predict actual take-up  $PL_i$ . Second, families must not precisely manipulate their child’s dates of birth around the eligibility cutoff. Third, assignment must not be correlated with any outcome-determining factor. The first condition is testable. We have already shown the distinctive jump in the take-up rate at the cutoff in Panel A of Figure 2. This condition also holds in our regression framework, where we obtain a  $\hat{\beta}_1$  of 0.813, implying that the assignment increases the PL duration by 0.813 years or 297 days. The estimated coefficient is highly statistically significant with an F-statistics of about 6,600.<sup>24</sup>

The inability to precisely manipulate the assignment into treatment is the key identifying assumption behind any RDD. The public discussion about a potential reform of the PL system had started in November 1989 (Lalive and Zweimüller, 2009). Eventually, on April 5, 1990, the Austrian government enacted the reform. This timing rules out that parents adjusted their conception behavior. The only way for parents to manipulate the birthdate was to prolong the pregnancy. Mothers with a due-date sufficiently close to July 1, 1990 could try to postpone birth by a couple of days.<sup>25</sup> Figure 3 shows that the average number of births per day does not vary around the cutoff date. Thus, there is no evidence for manipulations of the birthdate. Still, to be on the safe-side, we exclude births five days before or after the cutoff date.<sup>26</sup> This so-called *doughnut sample* should be definitely free of any sorting. Depending on the outcome under consideration, we have around 9,000 observations in our sample (for more details see Table 3).

[Figure 3]

---

(Schneeweis and Zweimüller, 2014) that the birth month is important in determining education outcomes due to relative age effects in schools.

<sup>24</sup>This coefficient is very stable across sub-groups. The largest difference is observed between mothers with low and high SES, for whom we obtain coefficients of 0.838 and 0.781, respectively.

<sup>25</sup>This could apply to planned Caesarian sections. In 1995, the earliest year since which the birth register documents the birth method, about 12 percent of all births are delivered by a Caesarian section. An unknown fraction of these were *planned* Caesarian sections.

<sup>26</sup>One of the first studies to demonstrate marginal timing of births due to financial incentives is Dickert-Conlin and Chandra (1999) for the US.

Whether the assignment is correlated with any outcome-determining factor is not fully testable, but it is reassuring that none of our covariates changes discontinuously around the cutoff. This can be seen in Figure 4, which plots daily averages of all covariates and some other pre-determined variables between May and September 1990. More formally, we test for differences in means of covariates and other predetermined variables between not-assigned (child is born in June 1990) and assigned families (child is born in July 1990). Table 4 shows no quantitatively important differences (see fifth column). Very few differences are statistically significant (see sixth column), but there is no evidence for a systematic pattern. Based on this evidence, we have no reason to expect a correlation between assignment and any unobserved outcome-determining factor (included in the error term  $w_i$ ).

[Figure 4 and Table 4]

## 4.4 Type of counterfactual mode of care

Our IV estimation strategy yields the average effect of an additional year of PL for compliers relative to their own counterfactual care choices. We assume that during PL childcare is largely provided by the mother (or the father) and not by other formal or informal caregivers. This LATE is a weighted average of LATEs for two different subpopulations: (i) the LATE for children who switch from informal care to maternal care and (ii) the LATE for children who switch from formal care to maternal care.<sup>27</sup> Given the evidence from the childcare literature we suppose that the counterfactual mode of care is one of the most important sources of heterogeneity in the effect of extended PL on child outcomes.

While we cannot observe the counterfactual care choices, we take advantage of regional variation in the availability of formal childcare for under three-year-olds. In communities without nurseries, the counterfactual is unambiguously defined; working parents had to rely exclusively on informal childcare. Survey data shows that the most important provider of informal childcare were grandparents. In communities with nurseries, the counterfactual could be in principle one of several options: Working parents could either enroll their child in formal childcare, rely exclusively on informal care-arrangements or use a combination of both. We do presuppose that a large fraction of working first-time mothers (about 35 percent) would have indeed relied on formal care-arrangements during their child's second year of life in the absence of the reform. Given that enrollment in formal care was not universal, we cannot claim that formal care was the only counterfactual mode of care for all mothers. Hence, the counterfactual in communities with nurseries is less clear. Throughout our analysis we compare treatment effects

---

<sup>27</sup>In fact, there is a third subpopulation (and LATE): (iii) Children whose mothers provide maternal care in the second year of the child's life independent of PL entitlement. These children would not have experienced a change in the mode of care but an increase in family income. We further deal with this issue in Section 5.2.

across communities with and without nurseries by splitting our sample along this dimension. It turns out that this dimension is the most important source of treatment effect heterogeneity.

## 5 Results

We present our estimation results in three steps. First, we discuss the effects of extended PL on children’s medium and long-term education, labor market and health outcomes. We study how the expansion in PL has affected children, by exploring the relative importance of time with the mother versus income effects. Moreover, we show that the availability of formal childcare is responsible for the observed treatment effect heterogeneity by demonstrating the insignificance of a list of potentially confounding community characteristics. Second, we examine treatment effects on family size, maternal labor supply and family stability up to 17 years after birth. Any significant behavioral responses along these dimensions may constitute important causal channels for the effects on children. Third, while we find evidence for behavioral responses, we show that these are not important drivers of the treatment effects on children.

### 5.1 Parental leave and child outcomes

Table 5 presents estimation results for education, labor market and health outcomes of children. The outcome variables capture the medium and long-run, ranging from high track attendance in secondary school, test scores and labor market activity in the early and late teenage years to labor market and health status at age 23. The first column presents average effects (the ITT for educational outcomes and the LATE for all other outcomes). The PL extension had zero effects on education and labor-market outcomes and significantly positive effects on health. Children whose mothers have been exposed to extended PL are less likely suffering from disability (minus 1.5 percentage points) and boys are more likely to be fit for military service (plus 9.2 percentage points). The positive effect of extended PL on health, result most likely from a higher likelihood of (appropriate) early intervention. There is evidence from US studies that early identification of impairments can improve adult health outcomes (Campbell et al., 2014).

In the second and third columns, we account for potential heterogeneity with respect to the local availability of formal childcare. We show separate regressions for children, whose mothers lived in communities with and without nurseries. The estimated effects on test scores in science and reading differ significantly between communities. In communities with a childcare facility, the coefficients are negative for all three domains, though statistically significant only in reading. In contrast, we obtain positive and statistically significant effects in communities where childcare is not available. The positive coefficients on test scores amount to about one fourth of the standard deviations in these variables. Next to test scores, we investigate high track attendance in different grades. We do not find any significant effects in grade 9. For high

track attendance in grades 5 and 8 in Linz, a community where childcare has been available, negative coefficients are obtained that are large in magnitude.

Turning to labor market and health outcomes, we find a similar pattern. In communities with nurseries, the estimated coefficients are mostly negative, however, with one exception not significant at conventional levels. In communities without nurseries, we find positive significant coefficients. One more year of PL increases the likelihood that the child is active at age 17 by 1.9 percentage points, at age 23 by 3 percentage points and for the whole period by 2.7 percentage points. These estimates range between 1.9 and 3.3 percent of the overall sample means.

For age 17, we find evidence that activity is driven by education. Children of mothers who were exposed to extended PL are more likely to attend a school or apprenticeship training. For age 23, the coefficients in the regressions for specific kinds of activity (education and employment) are not significant. However, whether being in education or being employed at age 23 is a positive outcome or a negative outcome is not clear. Being in education would be a positive outcome if the child attends university but would be a sign of poor achievement if the child is still in school or attends an apprenticeship training. Being employed at that age would be a positive outcome for children who would be inactive, still in school or attending an apprenticeship training in the counterfactual situation, and a negative outcome for children who would attend university in the counterfactual situation. The wage results are also difficult to interpret because these estimations are conditional on being employed. Due to these shortcomings, we focus on our activity measures henceforth. Being active as opposed to being inactive can unambiguously be interpreted as a positive outcome.

Regarding health outcomes, our analysis shows that the average positive effects come from communities without nurseries and amount to 2.2 percentage points and 12.2 percentage points in these areas. These effects are sizeable and amount to 2.2 and 15.6 percent of the sample means.

Our results show that the average treatment effects mask substantial heterogeneity. The local availability of childcare is a crucial dimension in determining how PL shapes child outcomes. The counterfactual mode of care plays an important role. The positive treatment effects in communities where childcare is not available show that maternal care is superior to informal care. One potential channel is that children with impairments are less likely to receive (optimal) early intervention under informal care-arrangements. In communities where childcare is available, the reform had zero (or negative) effects on children. While we have less power to compare maternal care with formal care, the results suggest that both modes of care lead to comparable child outcomes.

Table A.1 in the Appendix presents sensitivity analysis for our main outcome variables. The results are robust to the exclusion of individual covariates (such as gender, maternal SES, maternal migration background, pre-mature birth and maternal age), the inclusion of maternal pre-birth occupation and wage and the inclusion of children born  $\pm 5$  days around the cutoff

date. Probit and IV-Probit regressions yield marginal effects that are similar in size to the coefficients obtained in linear regressions.

We also analyze potential heterogeneities with respect to maternal socio-economic status and child gender. Table A.2 in the Appendix shows that there is no systematic pattern with respect to maternal socio-economic status. Our estimation results by child gender provide some evidence that boys are positively affected by the PL reform. This result is in line with previous evidence for Austria (Danzer and Lavy, forthcoming).

## 5.2 Mechanisms: time with the mother versus income effects

The estimated effects capture two aspects: a change in income and a change in the time the mother spends at home with the child. Depending on the mother’s counterfactual return-to-work behavior, we can distinguish two groups of mothers. For those mothers, who would have remained at home during the second year of the child’s life even under the old regime, the treatment implies a rise in income during the second year and a prolonged job protection.<sup>28</sup> In contrast, for those mothers who change their return-to-work behavior due to the reform, the treatment effect captures most likely a decrease in income, as well as a prolonged time with the child. For these children, the mode of child care also changes. Due to missing information on their counterfactual return-to-work behavior, we cannot differentiate between these two groups of mothers, and we are not able to cleanly disentangle the effects of income and the change in the mode of care. However, we have complementary estimation strategies to uncover the dominant force of these two.

[Figure 5]

*Changes in the time with mother* Figure 5 shows maternal employment rates for pre-reform and post-reform mothers by child’s age. A completely similar pattern is observed in communities with and without nurseries. During the first year of the child’s life only 3 percent of mothers were employed, thus were not on PL. For pre-reform mothers, employment increased only to 35 percent in the second year of the child’s life, implying that 65 percent of mothers stayed at home with their child even in the absence of PL entitlement. Thus, for around 35 percent of children in our sample, the reform induced a reduction in income and prolonged maternal care, while for 65 percent of children, the reform increased income and did not change the duration of maternal care.

[Figure 6]

*Changes in income* Figure 6 shows the hypothetical change in maternal income during the second year of the child’s life. We use pre-reform mothers, calculate their net income in the

---

<sup>28</sup>The extended job protection might foster the medium-run labor market attachment of these women.

second year and subtract PL benefits. Around 50 percent of mothers had no labor income during this period. For those mothers disposable income increased by the PL benefit, on average by around €4,400. For mothers who earned income in the second year, the reform either increased or decreased disposable income, depending on the level of their earnings. Overall, about 65 percent of the sample experienced an increase in disposable income by on average €3,900 and 35 percent experienced a decline by on average €8,400.

*Disentangling the two mechanisms* In a nutshell, 35 percent of the children in our sample experienced more time with the mother and a potentially lower family income, while 65 percent experienced no change in the time with the mother but the family had a higher income. This raises the question, whether we observe treatment effects for children because they had more time with their mother, or because their family enjoyed a higher income? To shed light on the importance of time versus income effects, we identify two groups of mothers based on pre-determined, observable characteristics and propose the following strategy:

- First, we use pre-reform mothers and estimate their propensity of being employed in the second year after child birth as a function of their characteristics (such as education and pre-birth earnings).
- Second, using these characteristics, we predict the propensity to work in the second year for the full sample of mothers.
- Third, we split the sample by this propensity and test whether the treatment effects on children are driven by mothers with a high or low propensity to work in the second year of the child's life.

To predict the propensity to work, we estimate a linear probability model of employment in the second year on maternal characteristics and birth outcomes.<sup>29</sup> As expected, mothers with higher education and higher pre-birth earnings are more likely to work in the second year after childbirth. Also foreign-born mothers and mothers with a religion other than roman-catholic have a higher propensity to work. Overall, the propensity to work is positively correlated with our measure of maternal socio-economic background. The correlation coefficient ranges from 0.3 to 0.4, depending on the definition of working in the second year. Table A.3 in the Appendix shows summary statistics of maternal characteristics for women with a low and high propensity to work (full-time) in the second year after child birth.

The separate estimations for mothers, whose characteristics indicate a low and a high propensity to work (in communities with and without nurseries) are summarized in Table 6. This set of estimations provide the key result of our analysis. We learn that the positive

---

<sup>29</sup>These variables are mostly taken from the Austrian Birth Register and characterize the time before birth. We use birth outcomes (premature birth, child was born with a low birth weight), whether the mother is foreign born, maternal religion, whether the mother is married, maternal education, the mother's occupation, maternal earnings in the last two years before child birth, indicator variables for maternal age at birth ranging from 17 or younger to 35 or older and indicator variables for the province of residency (9 provinces).

effects obtained for children in communities without a nursery are driven by mothers with a high propensity to work (i.e., who would work in the second year if PL would only last one year). Strikingly, most coefficients are twice as large as the coefficients of our baseline estimates and are significantly different from the coefficients obtained for communities with nurseries. For these children, an additional year of PL increases the likelihood that the child is active at age 17 by 3.7 percentage points, at age 23 by 6.5 percentage points and for the whole period by 5.5 percentage points. The probability of being fit for military is increased by almost 18 percentage points. In contrast, the coefficients for low propensity mothers in communities without a nursery are almost zero (with one exception) and statistically not significant. This suggests that the income effect is of second order importance. In communities with nurseries, almost all coefficients are—irrespective of the mother’s propensity to work—statistically not significant. These results are robust to alternative classifications of maternal employment.<sup>30</sup>

[Table 6]

This analysis suggests that the positive effects obtained for children in communities without nurseries come from children whose mothers would have been working in the counterfactual situation. For these children, the PL reform most likely replaced informal care-arrangements by maternal care. It seems that time with the mother is the driving force behind our results on child development. Concerning income effects, we conclude that parental leave benefits during the second year are less important for child development. The positive effects for children of mothers with a low propensity to work are generally smaller and statistically less significant than our baseline results.

### 5.3 Confounding community characteristics

Our estimated treatment effects strongly differ with respect to the availability of formal child-care at the community level. This heterogeneity may be explained by the counterfactual mode of care. However, there might be other community characteristics that are crucial in shaping treatment effects of PL. Actually, communities with and without nurseries differ in many aspects, such as the level of urbanity, the age structure of the population, the preponderance of conservative values or the attachment of females to the labor market. The number of inhabitants per square kilometer is around 51 in communities with nurseries and around 2 in communities without. The share of children below the age of 15 is 0.13 in communities with

---

<sup>30</sup>In the first panel of Table A.4 in the Appendix, women are classified with respect to their propensity to work during the *whole* second year of the child’s life ( $\geq 360$  days). In the second and third panel, we distinguish between mothers according to their propensity of being employed with a wage of at least 50 percent and 75 percent of their pre-birth earnings, respectively. Across all classifications of maternal employment, we find again in communities without a nursery the largest and statistically most significant results for children from mothers with a high propensity to work. For their counterparts with a low propensity to work, we observe (again with exception of being fit for military) no significant effects. In communities with nurseries, almost all coefficients are statistically not significant.

nurseries and 0.18 in communities without. The percent of inhabitants who are roman-catholic is 69 in communities with and 91 in communities without childcare. Also the female employment rate differs by community and is 64 percent where formal childcare is available and 52 percent where formal childcare is not available.

To shed light on the importance of other, potentially confounding community characteristics, we pursue the following strategy. We create indicators for low and high values of these community characteristics based on the median of these characteristics in the pooled sample (communities with and without nurseries) and split each sample by these indicators. For each sample we obtain two subsamples, one that includes typical communities and one that includes atypical communities. Atypical communities are those in which formal childcare is not available, but other community characteristics would suggest that childcare is available (based on the correlation between the two variables in the pooled sample), and those in which formal childcare is available but other community characteristics would suggest that childcare is not available. The idea behind this exercise is that the estimated effects should not only be driven by observations from typical communities. Examples for atypical communities are communities without formal childcare, but with either a high level of urbanity, a low share of children below age 15, a low share of catholics, or a high female employment rate. It turns out that this strategy is only feasible for communities without nurseries. In the case of communities with nurseries, the number of atypical communities is too low. This asymmetry is not so problematic, since this robustness check is less crucial for communities with nurseries; for which we do not find significant effects.

Table 7 shows estimation results for typical and atypical communities without nurseries. In the first two columns we compare communities with high and low population density. We find comparable effects (for four out of five outcome variables) in typical and atypical communities, however, the effects are only statistically significant in typical communities. The second panel investigates the age structure of the population by using the share of inhabitants below the age of 15. Here, we find significant effects for atypical and typical communities. Panel three investigates whether conservative values of the population might confound our estimates. Using the share of catholics as indicator for traditional family values, we again find positive effects for typical and atypical communities. Last but not least, we focus on differences in the female employment rate. Again, positive effects are found both in communities with a low and a high female employment rate.

This exercise provides evidence for our hypothesis that the availability of formal childcare and its consequences for the counterfactual mode of care is driving the effects of PL. The availability of formal childcare and not other community characteristics are most likely responsible for the heterogeneity in treatment effects.



## 5.4 Fertility, maternal labor supply and family stability

Family policies affecting the relative costs of child-bearing by means of generous PL entitlements might alter fertility decisions. Indeed, the 1990 Austrian parental leave reform caused a rise in the number of children and a change in the spacing of births (Lalive and Zweimüller, 2009). Increased family size might reduce parental monetary and time investments into the child or affect child outcomes through adjusted maternal labor supply and family income. Moreover, extended PL might, also through its effects on fertility and maternal employment, alter specialization within the household, the bargaining power of spouses and marital stability.

Shedding some light on the effects of these potentially intermediating channels, we study the effects of the reform on family size, maternal employment and family stability. Figure 7 shows the estimated coefficients and 95% confidence intervals obtained by 2SLS regressions of years on PL on family size up to 17 years after treatment for women in communities with and without nurseries. The full regression results are given in Table A.5 in the Appendix.

[Figure 7]

The PL reform significantly increased fertility in communities where childcare is available. The coefficient on the number of children ranges from 0.09 in the third year after childbirth to 0.16 in year 17. Thus, the PL extension induced the birth of 16 additional children per 100 women within 17 years. Given a mean value of about 1.9 children per woman, the reform increased fertility by about 8 percent in these areas. In communities without childcare, we observe positive fertility effects in the short run, pointing towards a reduced spacing of births due to the reform. However, the reform had no effects on completed fertility in these communities.

Extended PL and the resulting increase in family size might induce negative effects on maternal labor supply. Figure 8 shows estimated coefficients of PL on the probability that the mother is employed (upper panel) and the probability that the mother is full-time employed (lower panel) in the years after childbirth. Tables A.6 and A.7 in the Appendix show the full estimation results.

[Figure 8]

The additional year of PL had similar negative effects on maternal labor supply in the second year after childbirth in both subsamples and amounts to 32 percentage points in communities with nurseries and 31 percentage points in communities without nurseries. These results confirm the descriptive evidence presented in section 4. After the extended PL period has expired we do not find any significant effects on maternal labor supply. Given the sizeable fertility effects of the reform, the results show that mothers in communities with nurseries are able to reconcile family life and employment relatively quickly.

These results confirm the earlier analysis by Lalive and Zweimüller (2009). Our analysis of maternal labor supply at the intensive margin, uncovers long-lasting effects on mothers in communities without nurseries. As expected, the reform reduced maternal full-time employment in the second year of the child’s life in all communities. Beyond the second year a diverging pattern emerges across communities with and without nurseries. In communities with nurseries we find no significant effects on maternal full-time employment. In contrast, we observe significant positive effects for mothers in communities without nurseries. These mothers are around 10 percentage points more likely to work in a full-time job in the long run. This result seems surprising at first glance but is plausible on second thought. Extended PL should facilitate the return to work for women. In communities without nurseries, women are probably more likely to use their right to come back to their job after two years as opposed to one year. Furthermore, the somewhat reduced spacing of births between the first and second child might reduce the overall absence from work and facilitates the return to a permanent career. Another explanation is that mothers might react to their children’s needs. Maternal labor supply increases at the intensive margin because they are able to work more in the absence of child development problems.

Results on family status are summarized in Figure 9. Tables A.8-A.10 in the Appendix give the full estimation results.

[Figure 9]

Panel A shows the effects of PL on the probability that the mother is currently married. None of the coefficients is statistically significant at conventional levels. Panels B and C show separate estimations for mothers who have and who have not been married at the time of birth.<sup>31</sup> No significant results are obtained for mothers who have been married at birth, indicating that the probability of divorce has not been influenced by the PL reform. For mothers who have not been married at birth, the reform increased the probability of getting married in communities with nurseries in the first years after birth. The coefficients are statistically significant up to 7 years after birth and fade away thereafter. This result is in line with our estimations for family size. The increased fertility in communities where nurseries are available is accompanied by an increase in marriages. In communities where nurseries are not available, no comparable effect is observed.

Overall, our analysis shows that extended PL affects the family environment in which children grow up in multiple ways. The PL reform had significant effects on family size, maternal labor supply and marriage behavior. The local availability of formal childcare seems to be a central component in shaping the impact of PL. Mothers in communities where formal childcare is available reacted to the reform with an increase in completed fertility, a short-term decrease

---

<sup>31</sup>About half of the children were born legitimate and the other was born out of wedlock. This distribution is quite comparable in the sub-samples with nurseries (51.2 and 48.8 percent) and without nurseries (46.4 and 53.6 percent).

in labor supply on the extensive margin, zero effects on long-run full-time employment and an increased propensity to get married in the medium-run. Mothers in communities without formal childcare reacted very differently to the reform. Apart from a differential spacing between births, these women did not alter fertility decisions. They show zero employment effects and an increase in their medium and long-run full-time employment. Furthermore, they did not change their marriage behavior.

## 5.5 Child outcomes revisited

We find zero (or negative) effects of extended PL for children in communities with nurseries and positive effects in communities without. What drives this heterogeneity in the effects on child outcomes? The counterfactual mode of care in the second year of a child's life might play an important role. In communities without childcare facilities, the reform most likely caused a replacement of informal care-arrangements by maternal childcare. To the extent that maternal care is superior to informal care, the shift in care arrangements might have caused the positive effects on child development. On the contrary, in communities where formal childcare is available, maternal care partly replaced institutionalized care.

As discussed above, the reform had some effects on family size, maternal employment and family stability. In communities with nurseries, the PL reform increased fertility by around 8 percent. Furthermore, their mothers reduced their short-run labor supply at the extensive margin. Thus, reduced parental monetary and time investments into the child due to a quantity-quality trade-off might explain the zero (or negative) treatment effects for children in those communities. Accordingly, the positive effects in communities without a childcare facility might stem from the positive effects of the reform on long-run maternal full-time employment, which boosts family income. These women also reduced the spacing between the first and the second child somewhat.

In Table 8 we show the sensitivity of our estimates for children's labor market and health outcomes to the inclusion of control variables for family size and maternal (full-time) employment in the second, third, fifth and tenth year after birth. Being aware of the endogeneity of these variables, we evaluate the sensitivity of our estimated treatment effects with respect to the inclusion of these controls. If the positive effects in communities without nurseries are mainly due to maternal full-time employment and income effects, the coefficients on PL should decrease in magnitude when maternal (full-time) employment is controlled for. The same applies for family size: if siblings are the main reason why the PL extension does not show any positive effects on children, controlling for family size might alter the estimated effects.

The treatment effects for children in communities with and without formal childcare are not sensitive to the inclusion of control variables for family size in the second, third, fifth and tenth year after child birth (see first panel of Table 8).<sup>32</sup> None of the coefficients in communities

---

<sup>32</sup>Allowing for non-linear effects of family size by including binary variables for one sibling, two siblings and three or more siblings neither alters the estimated coefficients.

with nurseries changes the magnitude in a positive manner or becomes statistically significant. While the standard errors are exactly the same as above, the magnitudes of the coefficients even get more negative. In communities without nurseries, the coefficients on PL do not change either.

Turning to the second panel of Table 8, we see that the estimated effects are robust to the inclusion of maternal employment in the second, third, fifth and tenth year after child birth. Reduced maternal short-term employment in communities with formal childcare does not seem to drive the treatment effects. The same is true for maternal full-time employment (see third panel). The positive effects for children in communities without nurseries do not vanish when maternal full-time employment is included in the regressions. The positive effects even slightly increase in magnitude. This indicates that increased long-run maternal full-time employment is not the driving force behind the positive effects in these areas.

Overall, family size and maternal employment do not seem to be important intermediating channels of the PL reform. The estimated treatment effects on children most likely have other origins, namely, parental leave in the second year of the child’s life per se and the replacement of pre-reform care-arrangements. Figure 10 summarizes the estimated treatment effects for children in communities with and without nurseries.

[Figure 10]

## 6 Conclusions

Our analysis has shown that accounting for the availability of formal childcare is important for the evaluation of any parental leave reform since it shapes the effects of parental leave on child outcomes and other family outcomes such as family size or maternal labor supply. Strong heterogeneities with respect to this dimension are obtained for all outcomes we consider in this study. Our results on mediating channels such as family size and maternal labor supply are in line with and extend existing studies on the Austrian PL reform (Lalive and Zweimüller, 2009; Lalive et al., 2014). Furthermore we add to the scarce literature on the effects of parental leave in the second year of the child’s life (e. g. Danzer and Lavy, forthcoming).

Concluding from our results on child outcomes, it seems that informal care-arrangements do not provide the same fruitful environment for child development as maternal care in the second year of a child’s life. Children in communities without any childcare facility are better off after the parental leave reform. In these communities, informal care-arrangements by grandparents (or other relatives) have been replaced by maternal care. Especially those children are better off, whose mothers would have been working in the counterfactual situation, indicating that time with the mother is superior to informal childcare in the second year of a child’s life. There is evidence that childcare stability (e. g., the number of different care arrangements over time,

daily stability, including predictable routines and structure) is important for child development (Morrissey, 2009).<sup>33</sup> Informal care-arrangements may provide a lower level of stability than care provided by mothers or formal institutions. Moreover, grandparents – the most important providers of informal care – are less educated on average and have more traditional views about childrearing. The positive income shock for children of mothers who would have stayed at home with the child in the second year in any case, seems to be less important.

Our results also indicate that a replacement of nurseries by maternal care has no effects on long-term child outcomes. However, this result should be interpreted with some caution since we do not know how many women in communities with formal childcare substituted a nursery with maternal care. Our results fully confirm the evidence from the literature on formal and informal childcare. This literature finds mostly negative effects of informal childcare compared to maternal care and zero or positive effects of formal childcare compared to maternal care.

## References

- Baker, Michael, Jonathan Gruber and Kevin Milligan (2008), ‘Universal Childcare, Maternal Labor Supply, and Family Well-Being’, Journal of Political Economy **116**, 709–745.
- Baker, Michael and Kevin Milligan (2010), ‘Evidence from Maternity Leave Expansions of the Impact of Maternal Care on Early Child Development’, Journal of Human Resources **45**, 1–32.
- Baker, Michael and Kevin Milligan (2015), ‘Maternity Leave and Children’s Cognitive and Behavioral Development’, Journal of Population Economics **28**, 373–391.
- Bernal, Raquel and Michael P. Keane (2011), ‘Child Care Choices and Children’s Cognitive Achievement: The Case of Single Mothers’, Journal of Labor Economics **29**, 459–512.
- Buckles, Kasey S. and Daniel M. Hungerman (2013), ‘Season of Birth and Labor Outcomes: Old Questions, New Answers’, Review of Economics and Statistics **95**, 711–724.
- Campbell, Frances, Gabriella Conti, James J. Heckman, Seong Hyeok Moon, Rodrigo Pinto, Elizabeth Pungello and Yi Pan (2014), ‘Early Childhood Investments Substantially Boost Adult Health’, Science **343**(6178), 1478–1485.
- Carneiro, Pedro, Katrine V. Løken and Kjell G. Salvanes (2015), ‘A flying start? Maternity Leave Benefits and Long Run Outcomes of Children’, Journal of Political Economy **123**, 365–412.

---

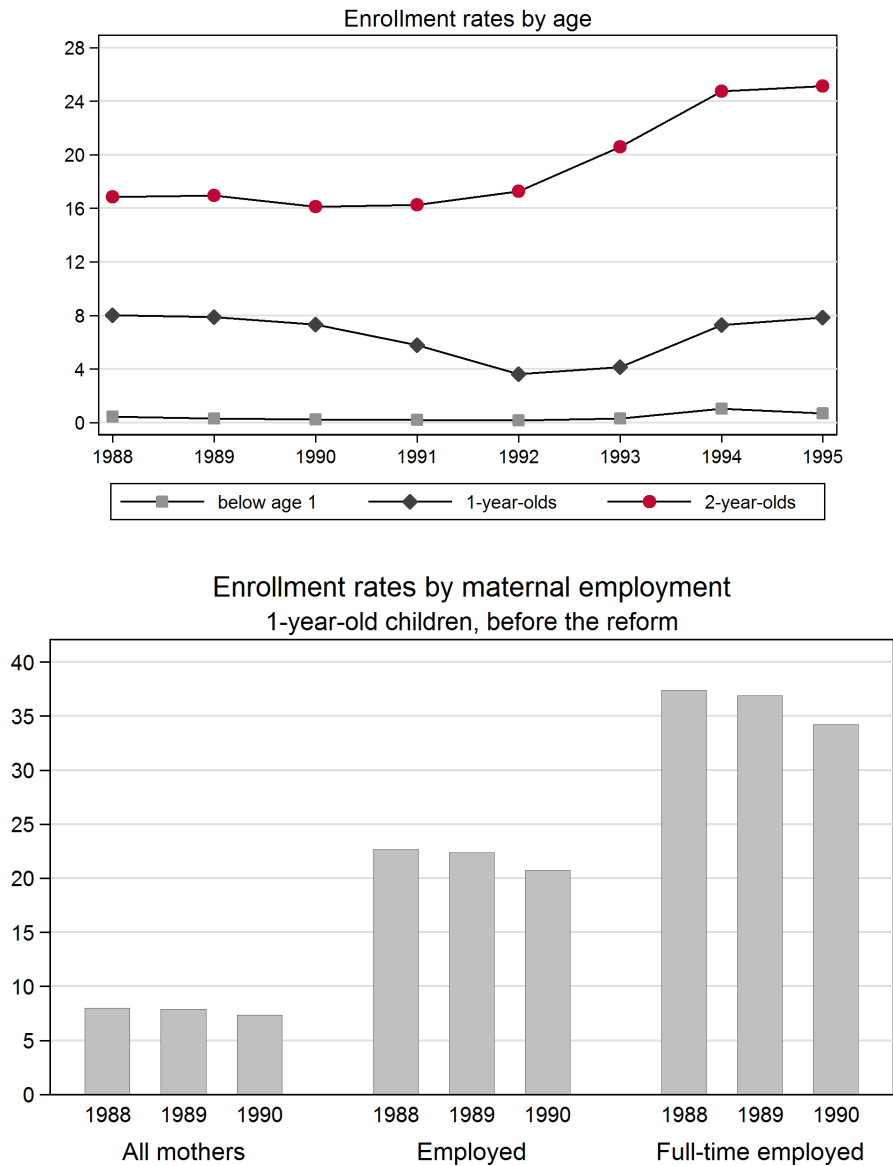
<sup>33</sup>Developmental psychology theory and research suggests that the quality of care, the extent of care, and the type of care arrangement potentially influence child outcomes (Votruba-Drzal et al., 2010).

- Cunha, Flavio, James J. Heckman, Lance Lochner and Dimitry V. Masterov (2006), Interpreting the evidence on life cycle skill formation, in E. A. Hanushek and F. Welch, eds, ‘Handbook of the Economics of Education, Vol. 1, Ch. 12’, North Holland, pp. 697–812.
- Dahl, Gordon, Katrine V. Løken, Magne Mogstad and Kari Salvanes (2016), ‘What Is the Case for Paid Maternity Leave?’, Review of Economics and Statistics **98**, 655–670.
- Danzer, Natalia and Victor Lavy (forthcoming), ‘Parental Leave and Children’s Schooling Outcomes: Quasi-Experimental Evidence from a Large Parental Leave Reform’, Economic Journal .
- Datta-Gupta, Nabanita and Marianne Simonsen (2010), ‘Non-cognitive Child Outcomes and Universal High Quality Child Care’, Journal of Public Economics **9**, 30–43.
- Dickert-Conlin, Stacy and Amitabh Chandra (1999), ‘Taxes and the Timing of Births’, Journal of Political Economy **107**, 161–177.
- Drange, Nina and Tarjei Havnes (2015), Child Care Before Age Two and the Development of Language and Numeracy: Evidence from a Lottery, Discussion Paper 8904, Institute for the Study of Labor, Bonn.
- Dustmann, Christian and Uta Schönberg (2012), ‘Expansions in Maternity Leave Coverage and Children’s Long-Term Outcomes’, American Economic Journal: Applied Economics **4**, 190–224.
- Elango, Sneha, Andrés Hojman, Jorge Luis García and James Heckman (2016), Early Childhood Education, in R. Moffitt, ed., ‘Means-Tested Transfer Programs in the United States, Volume II’, Chicago: University of Chicago Press.
- Felfe, Christina and Rafael Lalive (2014), Does early child care help or hurt children’s development?, Discussion Paper 8484, Institute for the Study of Labor, Bonn.
- Gathmann, Christina and Björn Sass (2012), Taxing Childcare: Effects on Family Labor Supply and Children, Discussion Papers 6440, Institute for the Study of Labor, Bonn.
- Gregg, Paul and Jane Waldfogel (2005), ‘Symposium on Parental Leave, Early Maternal Employment and Child Outcomes: Introduction’, Economic Journal **115**(501), F1–F6.
- Havnes, Tarjei and Magne Mogstad (2011), ‘No Child Left Behind: Subsidized Child Care and Children’s Long-Run Outcomes’, American Economic Journal: Economic Policy **3**, 97–129.
- Havnes, Tarjei and Magne Mogstad (2015), ‘Is Universal Child Care Leveling the Playing Field?’, Journal of Public Economics **127**, 100–114.

- Herbst, Chris M. (2013), ‘The Impact of Non-Parental Child Care on Child Development: Evidence from the Summer Participation’, Journal of Public Economics **105**, 86–105.
- Kline, Patrick and Christopher Walters (2016), ‘Evaluating Public Programs with Close Substitutes: The Case of Head Start’, Quarterly Journal of Economics **131**, 1795–1848.
- Lalive, Rafael, AnaIa Schlosser, Andreas Steinhauer and Josef Zweimüller (2014), ‘Parental Leave and Mothers’ Careers: The Relative Importance of Job Protection and Cash Benefits’, Review of Economic Studies **81**, 219–265.
- Lalive, Rafael and Josef Zweimüller (2009), ‘How does Parental Leave Affect Fertility and Return to Work? Evidence from Two Natural Experiments’, Quarterly Journal of Economics **124**, 1363–1402.
- Liu, Qian and Oskar Nordström Skans (2010), ‘The Duration of Paid Parental Leave and Children’s Scholastic Performance’, B. E. Journal of Economic Analysis and Policy (Contributions) **10**, Article 3.
- Morrissey, Taryn W. (2009), ‘Multiple Child-Care Arrangements and Young Children’s Behavioral Outcomes’, Child Development **80**, 59–76.
- Rasmussen, Astrid (2010), ‘Increasing the Length of Parents’ Birth-Related Leave: The Effect on Children’s Long-Term Educational Outcomes’, Labour Economics **17**, 91–100.
- Rossin-Slater, Maya (2017), Maternity and Family Leave Policy, Working Paper 23069, National Bureau of Economic Research, Cambridge.
- Schneeweis, Nicole and Martina Zweimüller (2012), ‘Girls, Girls, Girls: Gender Composition and Female School Choice’, Economics of Education Review **31**, 482–500.
- Schneeweis, Nicole and Martina Zweimüller (2014), ‘Early Tracking and the Misfortune of Being Young’, Scandinavian Journal of Economics **116**, 394–428.
- Statistik Austria (1990), ‘Mikrozensus 1990’. Statistik Austria, Vienna.
- Statistik Austria (2008), ‘Jahrbuch der Gesundheitsstatistik 2008’. Statistik Austria, Vienna.
- Votruba-Drzal, Elizabeth, Rebekah Levine Coley, Carolina Maldonado-Carre no, Christine P Li-Grining and P Lindsay Chase-Lansdale (2010), ‘Child Care and the Development of Behavior Problems among Economically Disadvantaged Children in Middle Childhood’, Child Development **81**, 1460–1474.
- Zweimüller, Josef, Rudolf Winter-Ebmer, Rafael Lalive, Andreas Kuhn, Jean-Philippe Wuellrich, Oliver Ruf and Simon Büchi (2009), The Austrian Social Security Database (ASSD), Working Paper 0901, The Austrian Center for Labor Economics and the Analysis of the Welfare State, University of Linz.

## 7 Figures (to be placed in the paper)

Figure 1: Enrollment rates in formal childcare



Notes: Own calculations based on *Kindertagesheimstatistik* (Statistics Austria, Statcube, retrieved on November 17, 2016) and the *Austrian Birth Register*. Figures refer to communities with a nursery. Enrollment rates are calculated as the ratio between the number of enrolled children by age and the number of children in the respective birth cohort. Between 1993 and 1994 the definition of age groups has changed: The age-definition is based on calendar years (January 1 to December 31) up until 1993. From 1994 onwards, the age-definition is based on school years (September 1 to August 31). Higher enrollment rates after 1994 are partly due to the fact that the age-groups consist of slightly older children. Furthermore, data problems occurred in 1993, thus, data-points for this year have to be taken with care. Enrollment rates by maternal employment are calculated by multiplying the denominator of the enrollment rate of 1-year-old children with the (full-time) employment rate of pre-reform mothers in the second year of the child's life (35 and 21 percent).



Figure 2: Parental leave duration, eligibility and take-up rate

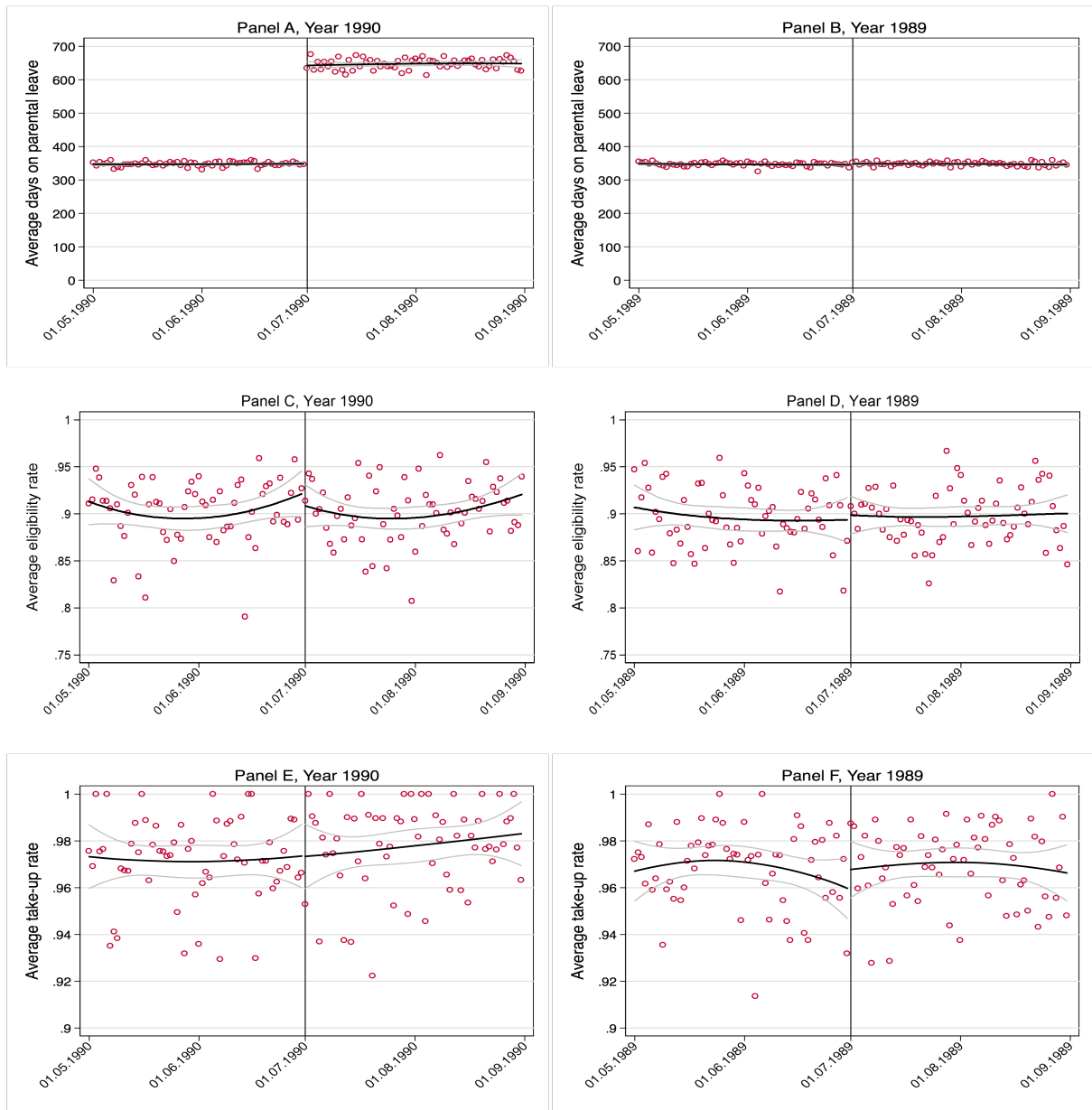


Figure 3: Density of births

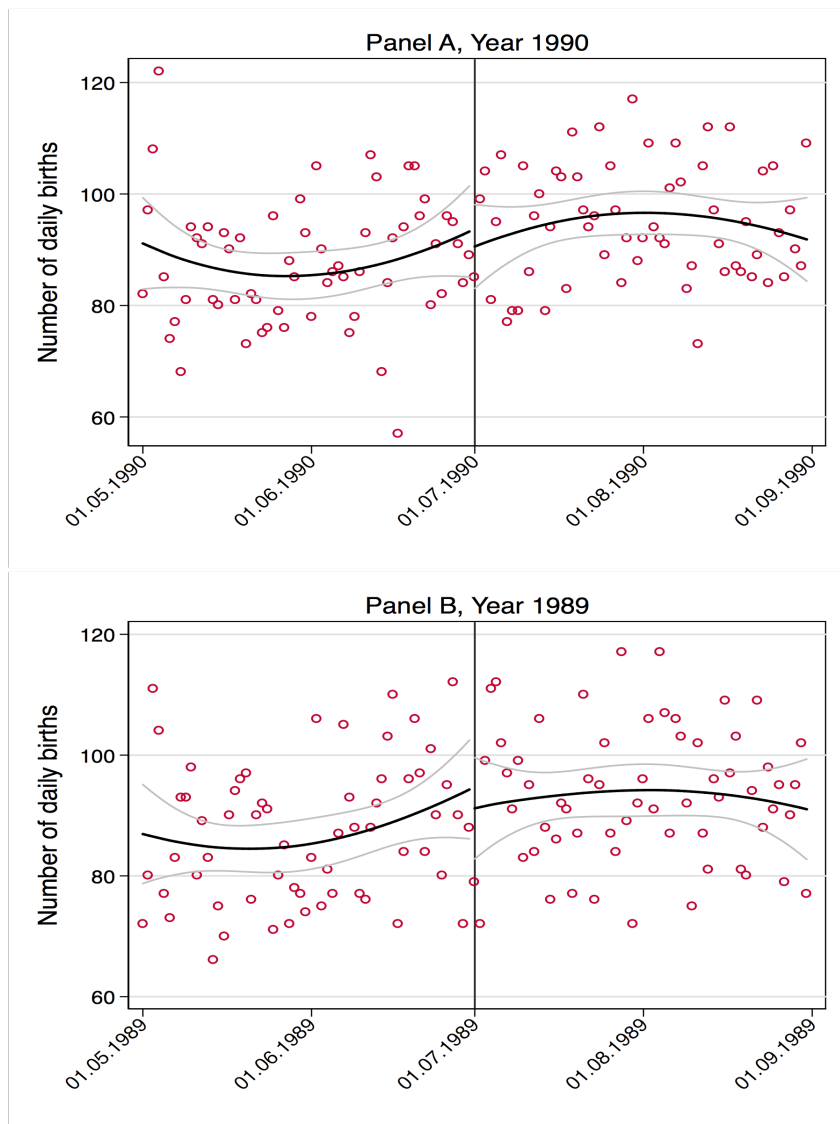


Figure 4: Daily averages of covariates and two pre-determined variables

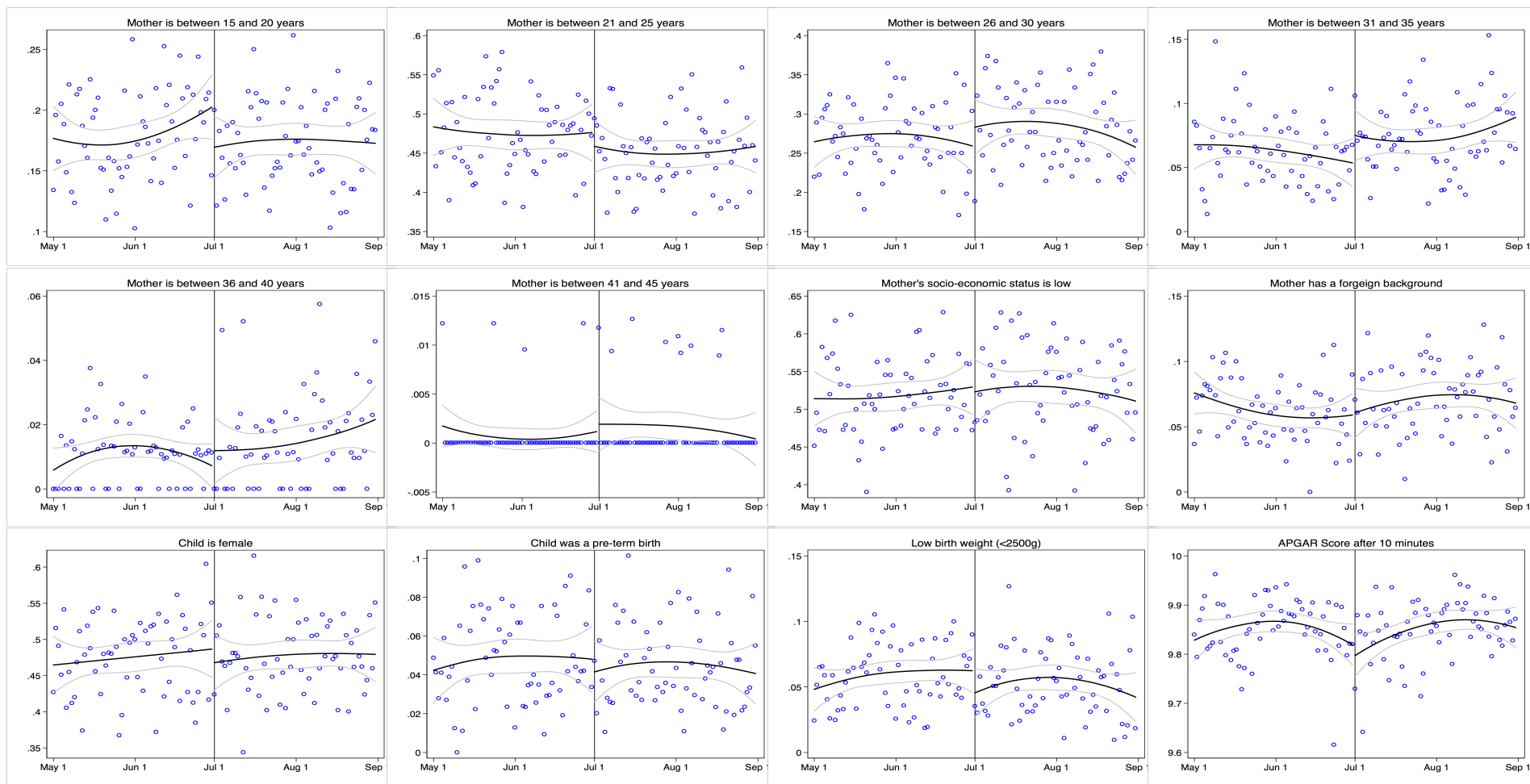


Figure 5: Return to work

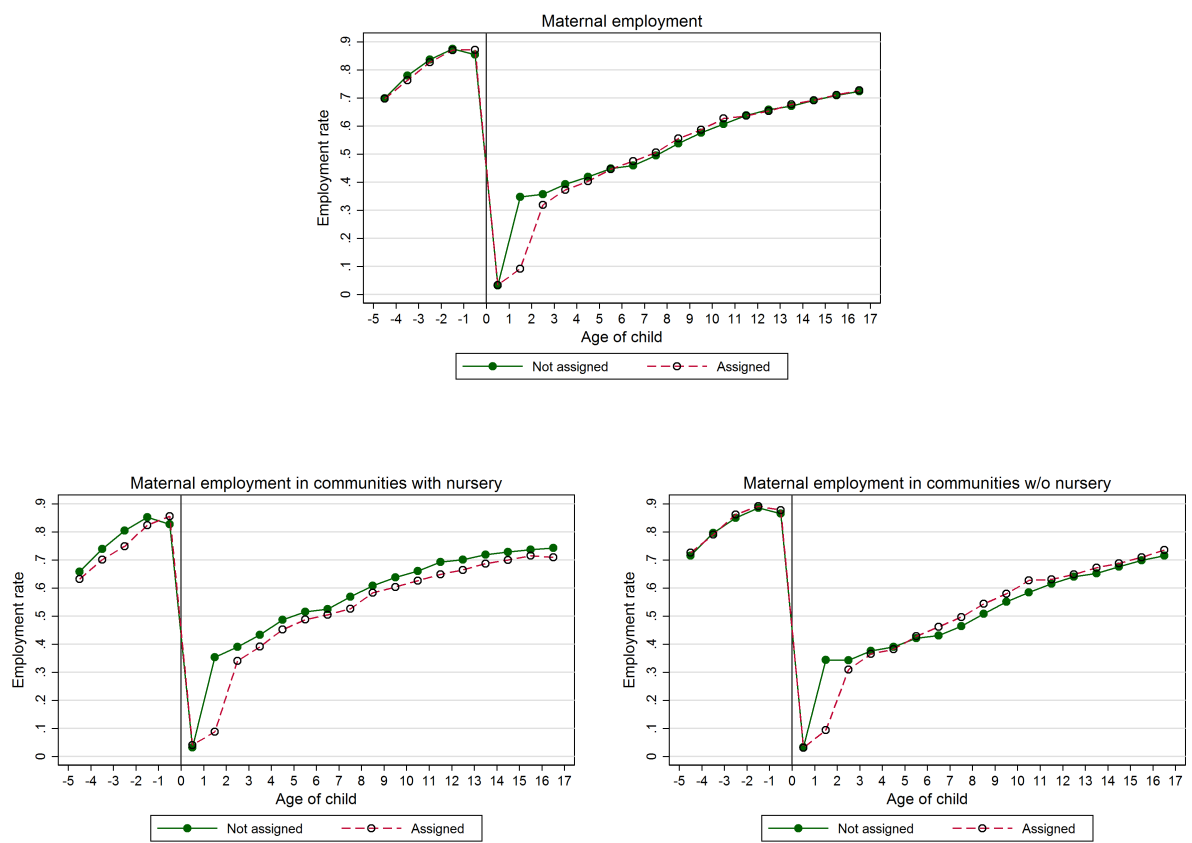
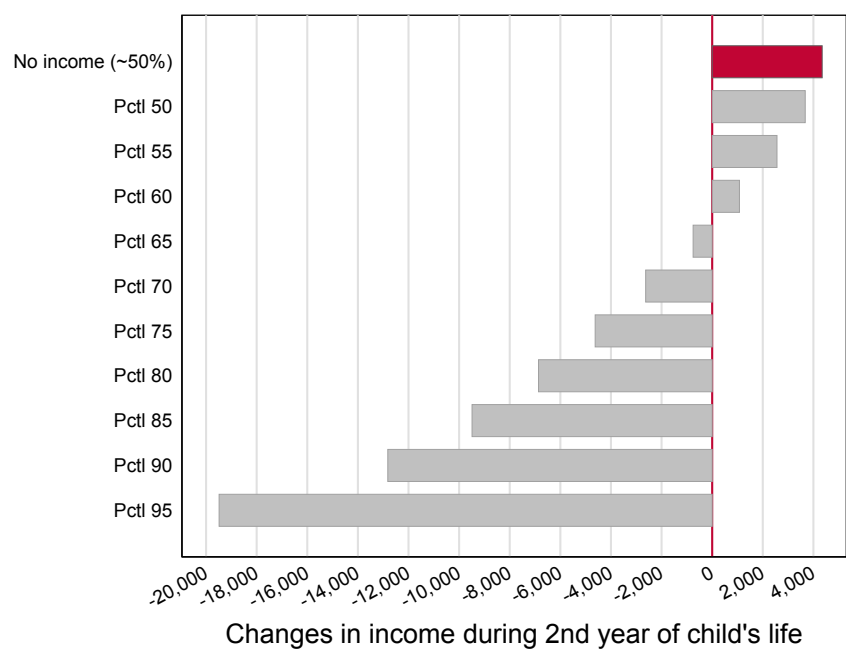
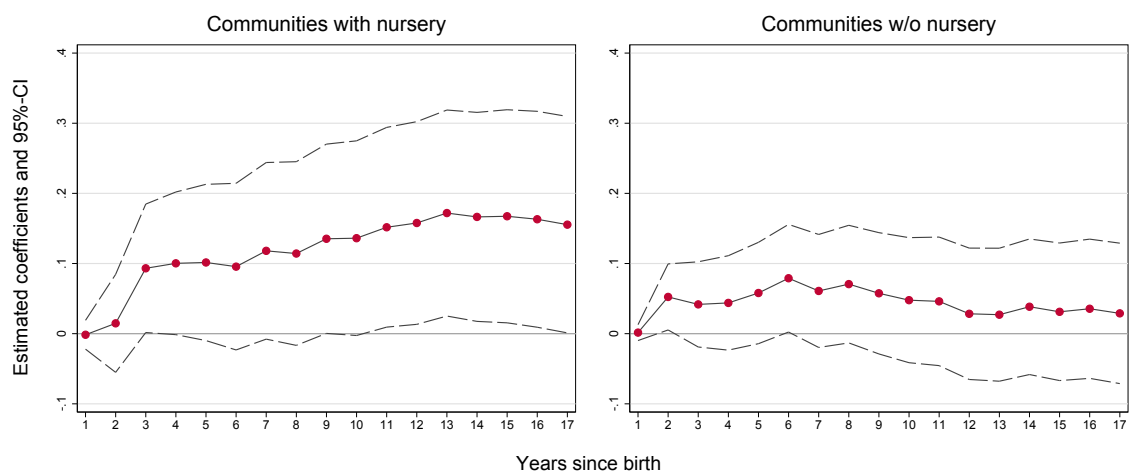


Figure 6: Income



**Figure 7: Family size up to 17 years after childbirth**



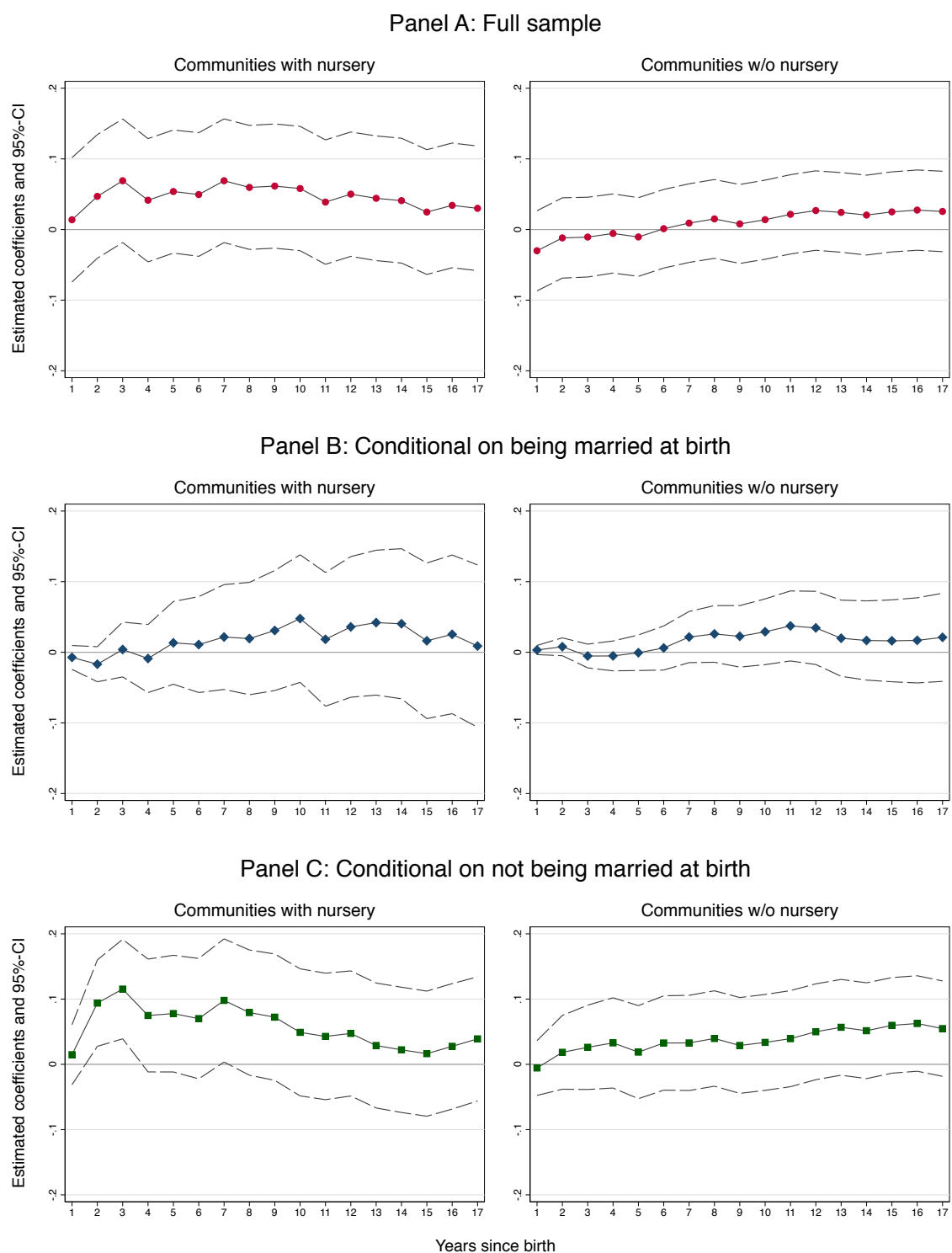
Notes: This figure shows the estimated coefficients and 95% confidence intervals from separate 2SLS regressions with years on parental leave instrumented by the assignment to the reform. Family size is measured as the number of children at the first child's birthday in each year. See Table A.5 in the Appendix for further information.

Figure 8: Maternal labor supply up to 17 years after childbirth



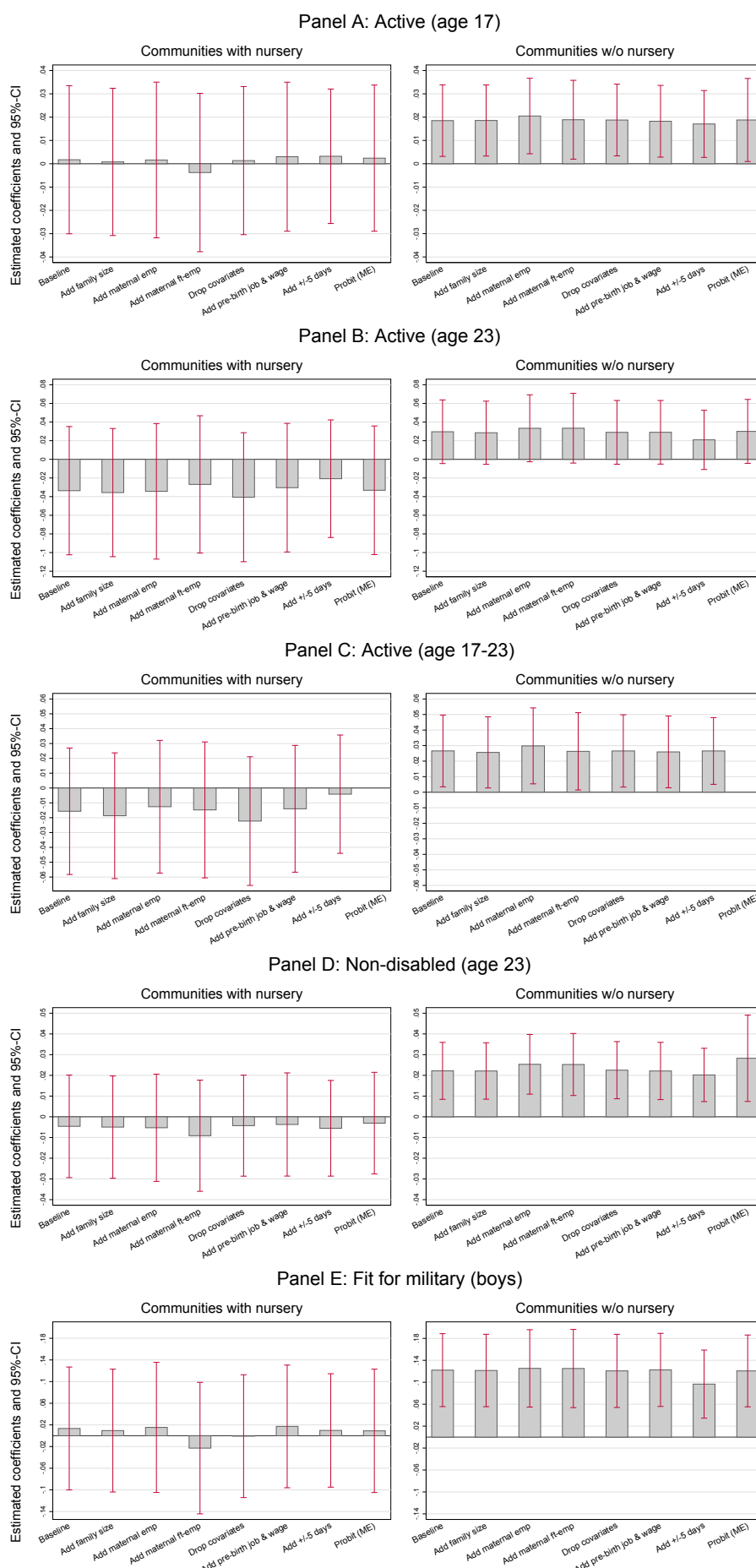
Notes: These figures show the estimated coefficients and 95% confidence intervals from separate 2SLS regressions with years on parental leave instrumented by the assignment to the reform. Maternal labor supply is measured as the probability to be employed (Panel A) and the probability to be employed full-time (Panel B) in each year of the child's life. See Tables A.6 and A.7 in the Appendix for further information.

Figure 9: Family status (currently married) up to 17 years after childbirth



Notes: These figures show the estimated coefficients and 95% confidence intervals from separate 2SLS regressions with years on parental leave instrumented by the assignment to the reform. Family status is measured as the probability to be married in each year of the child's life for the full sample of mothers (Panel A), for the sample of mothers who have been married at birth (Panel B), and for the sample of mothers who have not been married at birth (Panel C). See Tables A.8 , A.9 and A.8 in the Appendix for further information.

Figure 10: Child outcomes in communities with and w/o nursery





**8   Tables (to placed in paper)**

**Table 1: Overview: Parental leave reforms and child outcomes**

Study	Country and year of reform	Content of reform	Assessed child outcomes & data	Results	Mode of non-parental childcare
Baker and Milligan (2010)	Canada 31 December 2000	Extension of maternity leave benefits from 25 to 50 weeks. Extension of j.p. PL from 18-70 to at least 52 weeks in all regions.	Parent-reported measures (temperament, motor and social development) at age: 7 and 24 months. <i>Data:</i> survey data (NLSCY)	Small and mostly insignificant results. <i>Heterogeneity:</i> Not tested.	Mainly informal care (40% for under-2-year-olds). Formal care rare (4/6% of children younger than 1/2 year/s).
Baker and Milligan (2015)	Canada 31 December 2000	see Baker and Milligan (2010)	Cognitive development (vocabulary, numbers), parent-reported measures (eg hyperactivity) at age: 4/5 years. <i>Data:</i> survey data (NLSCY)	No significant positive effects. Small negative effects on vocabulary scores. <i>Heterogeneity:</i> Same across sub-groups (gender, parental education).	see Baker and Milligan (2010)
Carneiro, Løcken and Salvenes (2015)	Norway 1 July 1977	Introduction of paid PL for 18 weeks (100% income replacement) Extension of unpaid j.p. PL from 12 weeks to 12 months	High school dropout, college attendance, earnings at age 30, years of schooling, IQ (males age 18-19), teenage pregnancy <i>Data:</i> Administrative data	Significant positive effects: Reduced drop-out rates and increased earnings, college attendance, completed years of schooling and IQ (males) <i>Heterogeneity:</i> Differential effects by maternal education, gender, birth order, rural/urban location and distance to grandparents.	Mainly informal care. Formal childcare rare (1-2% for under-2-year-olds).
Dahl, Løcken, Mogstad and Salvenes (2016)	Norway Six PL reforms: 1 May 1987 – 1 April 1992.	6 extensions of paid PL by 2 to 4 weeks each during the first year of life (at 100% income replacement).	Compulsory exam at end of junior high school, high school dropout <i>Data:</i> Administrative data	No significant effects. <i>Heterogeneity:</i> Not tested.	Mainly informal care. (see Carneiro et al. 2015)
Danzer and Lavy (forthcoming)	Austria 1 July 1990	Extension of paid+j.p. PL from child's 1st to 2nd birthday.	Test scores in reading, math and science at age 15/16. <i>Data:</i> PISA	No significant average effects. <i>Heterogeneity:</i> Significantly positive effects for sons of highly educated mothers.	Mainly informal care. Formal childcare for under-3-year-olds rare (<3%).
Dustmann and Schönberg (2012)	Germany Three PL reforms: 1 May 1979 1 January 1986 1 January 1992	Extension of paid+j.p. PL from 2 to 6 months (flat rate; 1979), from 6 to 10 months (means-tested; 1986) Extension of unpaid j.p. PL from 18 to 36 months (1992).	Wages, educational attainment (age 28/29; 1979 reform), graduation from academic track (1986 reform), school track (age 14; 1992 reform) <i>Data:</i> Administrative data	No or extremely small effects. Expansion from 18 to 36 months slightly negative effects. <i>Heterogeneity:</i> Not tested.	Mainly informal care. Enrollment in formal care low (5% for under 18-months-olds).
Liu and Nordstrom Skans (2010)	Sweden 1 August 1988 – 1 October 1988	Gradual extension of paid PL from 12 to 15 months (by 30 days in each of 3 consecutive months 08/09/10 1988).	Test scores during last compulsory school year, compulsory school grades at age 16 <i>Data:</i> Administrative data	No significant effects. <i>Heterogeneity:</i> Positive effect for children from mothers with higher education.	Mainly formal care (40-50% of children aged 1-2). Few children in informal care.
Rasmussen (2010)	Denmark 26 March 1984	Extension of paid PL from 14 to 20 weeks	High school enrollment, GPA, reading scores at age 15/16 <i>Data:</i> Administrative data, PISA	No significant effects <i>Heterogeneity:</i> Same across sub-groups (gender, parental education).	Mainly formal day care even for very young children.

*Abbreviations:* j.p. - job-protected; NLSCY - National Longitudinal Study of Children and Youth; PL - parental leave; PISA - Programme for International Student Assessment.

**Table 2: Availability of kindergardens and nurseries**

Year	1988	1989	1990	1991	1992	1993	1994	1995
<i>Kindergarden available:</i>								
Percent of communities	78.6	81.0	81.5	85.2	84.1	85.2	85.6	86.7
Percent of total population	94.6	95.7	95.8	95.9	96.5	96.9	97.0	97.5
<i>Nursery available:</i>								
Percent of communities	1.6	1.6	1.8	1.8	2.2	2.4	2.7	2.9
Percent of total population	33.2	33.1	33.4	33.3	33.9	34.5	34.7	35.1

*Notes:* Own calculations based on data from Statistics Austria.

Table 3: Description of outcome variables

Outcome	Variable description	Data source <sup>a</sup>	N	All Mean	Communities	
					with nursery Mean	w/o nursery Mean
<b>Child:</b>						
EDUCATIONAL OUTCOMES						
Test score math	This variable captures the tests core in mathematics (age 15/16).	PISA	1,405	522	526	520
Test score science	This variable captures the tests core in science (age 15/16).	PISA	1,405	506	510	503
Test score reading	This variable captures the tests core in reading (age 15/16).	PISA	1,405	519	520	517
High track grade 5	Binary indicator equal to one if child is in the high track in grade 5 (age 10/11).	EducReg	498	-	0.422	-
High track grade 8	Binary indicator equal to one if child is in the high track in grade 8 (age 13/14).	EducReg	456	-	0.386	-
High track grade 9	Binary indicator equal to one if child is in the high track in grade 9 (age 14/15).	PISA	1,386	0.560	0.630	0.526
LABOR MARKET OUTCOMES						
Active (age 17)	Binary indicator equal to one if child is active at the age of 17. The child is considered as active if s/he is either in education (school, apprenticeship, or university), employed (excl. marginal employment <sup>b</sup> ), on maternity/parental leave or in military or alternative civilian service. Inactive children are unemployed, marginally employed, disabled, on sick leave or in rehabilitation or in other kinds of inactive social insurance periods.	ASSD/Ministry	8,692	0.980	0.972	0.984
In education (age 17)	Binary indicator equal to one if child is in education at the age of 17.	ASSD/Ministry	8,692	0.976	0.967	0.980
Active (age 23)	Binary indicator equal to one if child is active at the age of 23.	ASSD/Ministry	8,518	0.897	0.852	0.915
In education (age 23)	Binary indicator equal to one if child is in education at the age of 23.	ASSD/Ministry	8,518	0.259	0.315	0.236
Employed (age 23)	Binary indicator equal to one if child is employed at the age of 23.	ASSD/Ministry	8,518	0.604	0.503	0.645
Log wage (age 23)	This variable captures the daily log wage at the age of 23.	ASSD/Ministry	4,992	4.253	4.176	4.277
Active (age 17-23)	This variable captures the share of active spells between 17 and 23 years of age.	ASSD/Ministry	8,965	0.867	0.835	0.880
Always active (age 17-23)	Binary indicator equal to one if child is always active between 17 and 23 years of age.	ASSD/Ministry	8,965	0.494	0.443	0.516
HEALTH OUTCOMES						
Non-disabled	Binary indicator equal to one if child is <i>not</i> disabled between 17 and 23 years of age.	Ministry	8,815	0.986	0.983	0.987
Fit for military (boys)	Binary indicator equal to one if male child is fit for military.	ASSD	4,603	0.783	0.751	0.795
<b>Mother:</b>						
Employed ( $t = 10$ )	Binary indicator equal to one if the mother is employed $t$ years after parity one (measured on January 1, in each year).	ASSD	9,499	0.579	0.629	0.558
Full-time employed ( $t = 10$ )	Binary indicator equal to one if the mother is full-time employed $t$ years after parity one. We define a mothers as full-time employed, if she earns at least 75% of her pre-birth earnings.	ASSD	9,019	0.364	0.450	0.329
<b>Family:</b>						
Family size ( $t = 10$ )	This variable captures the number of own children $t$ years after parity one (measured on the child's birthday each year).	ABR	9,499	1.885	1.795	1.936
Family status ( $t = 10$ ) <sup>c</sup>	Binary indicator equal to one if the parents are married $t$ years after parity one.	AMR/ADR	9,496	0.589	0.524	0.616

Notes: <sup>a</sup>PISA = Programme for International Student Assessment, EducReg = *Educational Register* of the city of Linz, ASSD = *Austrian Social Security Database*, Ministry = Database of the *Ministry of Labour, Social Affairs and Consumer Protection*, ABR = *Austrian Birth Register*, AMR = *Austrian Marriage Register*, ADR = *Austrian Divorce Register*. <sup>b</sup>This type of employment contract is for jobs with a low number of working hours and low pay and covers only accident insurance. <sup>c</sup>In the analysis of the current family status, we exclude three observations, where parents divorced before birth.

Table 4: Testing for baseline differences between not-assigned and assigned families

	Sample of pre-reform mothers		Sample of post-reform mothers		Diff.	P-value
	Mean	N	Mean	N		
<b>Covariates:</b>						
Mother's age at party one:						
Between 15 and 20 years	0.19	2,306	0.18	2,477	0.01	0.54
Between 21 and 25 years	0.47	2,306	0.44	2,477	0.03*	0.06
Between 26 and 30 years	0.27	2,306	0.29	2,477	-0.02	0.21
Between 31 and 35 years	0.06	2,306	0.08	2,477	-0.02**	0.03
Between 36 and 40 years	0.01	2,306	0.01	2,477	-0.00	0.68
Between 41 and 45 years	0.00	2,306	0.00	2,477	-0.00	0.71
Mother's socio-economic status is high	0.47	2,306	0.47	2,477	0.01	0.58
Mother has a foreign background	0.06	2,306	0.07	2,477	-0.01*	0.09
Child is female	0.48	2,306	0.47	2,477	0.00	0.88
Child was a pre-term birth	0.04	2,306	0.05	2,477	-0.00	0.48
<b>Other pre-determined variables:</b>						
Proxies for health at birth:						
Gestation length in weeks	39.77	2,306	39.77	2,477	0.00	0.92
Birth weight in dekagram	323.14	2,306	323.34	2,477	-0.20	0.89
Lowe birth weight (<2500g)	0.06	2,306	0.06	2,477	-0.00	0.69
APGAR Scores:						
After 1 minute	8.58	2,303	8.54	2,477	0.04	0.29
After 5 minutes	9.58	2,303	9.58	2,475	-0.00	0.94
After 10 minutes	9.86	2,289	9.84	2,469	0.02	0.18
Maternity leave after birth (in days)	65.64	2,268	65.14	2,430	0.50	0.63
Mother's highest degree:						
Compulsory schooling	0.20	2,306	0.22	2,477	-0.01	0.25
Apprenticeship	0.45	2,306	0.42	2,477	0.03**	0.04
Intermediate vocational school	0.20	2,306	0.21	2,477	-0.01	0.40
Higher general or vocational school	0.09	2,306	0.09	2,477	-0.00	0.94
College degree	0.05	2,306	0.05	2,477	-0.00	0.61
Unknown	0.00	2,306	0.00	2,477	-0.00	0.30

*Notes:* This table summarizes sample means and the number of observations of the samples of not-assigned and assigned mothers, the difference in the two sample means, and the p-value resulting from a *t* test on the equality of means. Not-assigned or pre-reform mothers are those whose child is born in June 1990, while assigned or post-reform mothers' children are born in July 1990. We exclude children born  $\pm 5$  days around the cutoff. \* and \*\* indicate statistical significance at the 10-percent and 5-percent level

Table 5: Child outcomes

	All communities	Communities with nursery	Communities w/o nursery	P-value $\Delta^a$
<b>Education outcomes (ITT)<sup>b</sup></b>				
Test score math (age 15/16)	13.168 (10.880)	-11.833 (21.499)	21.712* (12.536)	0.179
Test score science (age 15/16)	11.487 (11.274)	-27.516 (20.280)	23.435* (12.821)	0.032
Test score reading (age 15/16)	9.806 (11.159)	-39.839* (20.338)	27.161** (12.636)	0.005
High track grade 9 (age 14/15)	0.022 (0.048)	0.027 (0.095)	0.012 (0.056)	0.891
High track grade 5 (age 10/11)		-0.311*** (0.087)		
High track grade 8 (age 13/14)		-0.213** (0.091)		
<b>Labor market outcomes (LATE)</b>				
Active (age 17)	0.014* (0.007)	0.002 (0.016)	0.019** (0.008)	0.351
In education (age 17)	0.013 (0.008)	-0.004 (0.018)	0.019** (0.009)	0.235
Active (age 23)	0.011 (0.016)	-0.034 (0.035)	0.030* (0.017)	0.107
In education (age 23)	-0.001 (0.022)	-0.011 (0.044)	0.002 (0.026)	0.789
Employed (age 23)	0.003 (0.025)	-0.028 (0.049)	0.017 (0.029)	0.431
Log wage (age 23)	-0.012 (0.025)	-0.032 (0.053)	-0.005 (0.027)	0.656
Active (age 17-23)	0.014 (0.011)	-0.016 (0.022)	0.027** (0.012)	0.088
Always active (age 17-23)	-0.009 (0.026)	-0.096** (0.047)	0.028 (0.031)	0.026
<b>Health outcomes (LATE)</b>				
Non-disabled (age 23)	0.015** (0.006)	-0.005 (0.013)	0.022*** (0.007)	0.064
Fit for military (boys)	0.092*** (0.029)	0.013 (0.058)	0.122*** (0.034)	0.105

*Notes:* Each coefficient represents a separate regression based on data from *PISA* and *EducReg Linz* (education outcomes) and *ASSD* and *Ministry* (labor and health outcomes). We use a sample of children born in Austria in June/July 1989/1990 (1987/1990 in PISA). For labor market and health outcomes we exclude children born  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad and birth-year and birth-month fixed-effects. Additional control variables are included for labor and health outcomes: maternal age groups and premature birth. Coefficients for education outcomes represent reduced form estimates, coefficients for labor market and health outcomes are 2SLS estimates, with years on parental leave instrumented by the assignment to the reform. Robust standard errors are shown in parentheses. Estimations for PISA education outcomes control for the survey design (school clusters, student weights). \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Prob>F(chi2) of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. <sup>b</sup>We stratify the sample by school location and assume that communities with  $\geq 100,000$  inhabitants had a nursery.

Table 6: Child outcomes by predicted maternal propensity of being employed in second year<sup>a</sup>

	Low work propensity			High work propensity		
	Communities with nursery	Communities w/o nursery	P-value $\Delta^b$	Communities with nursery	Communities w/o nursery	P-value $\Delta^b$
% in Sample	16.52	33.49		13.10	36.90	
Active (age 17)	0.015 (0.025)	0.004 (0.011)	0.687	-0.010 (0.021)	0.037*** (0.012)	0.052
Active (age 23)	0.000 (0.051)	0.003 (0.023)	0.971	-0.063 (0.049)	0.065** (0.027)	0.021
Active (age 17-23)	-0.038 (0.033)	0.004 (0.016)	0.248	0.009 (0.029)	0.055*** (0.017)	0.179
Non-disabled (age 23)	0.005 (0.021)	0.006 (0.010)	0.950	-0.013 (0.015)	0.041*** (0.010)	0.003
Fit for military (boys)	0.072 (0.082)	0.079* (0.046)	0.937	-0.044 (0.083)	0.177*** (0.051)	0.023

*Notes:* Each coefficient represents a separate 2SLS regression, with years on parental leave instrumented by the assignment to the reform based on data from *ABR*, *ASSD* and *Ministry*. We use a sample of children born in Austria in June/July 1989/1990, excluding children born  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad, maternal age groups, premature births and birth-year and birth-month fixed-effects. Robust standard errors are shown in parentheses. \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Maternal characteristics indicate a low/high propensity of being employed >0 days in second year after childbirth, low/high according to median prediction (0.52). <sup>b</sup>Prob>F(chi2) of difference in coefficients between communities with and w/o nursery based on fully interacted regressions.

Table 7: Child outcomes in typical and atypical communities w/o nursery

Community type <sup>a</sup>	Population density <sup>b</sup>		Share of children <sup>c</sup>		Share of catholics <sup>d</sup>		Female employment <sup>e</sup>	
	Low Typical	High Atypical	High Typical	Low Atypical	High Typical	Low Atypical	Low Typical	High Atypical
% in Sample	70.59	29.41	70.38	29.6	69.87	30.13	67.32	32.68
Active (age 17)	0.018** (0.009)	0.021 (0.015)	0.006 (0.008)	0.056*** (0.019)	0.016* (0.009)	0.025 (0.015)	0.017* (0.009)	0.024 (0.015)
Active (age 23)	0.039* (0.020)	0.006 (0.035)	0.037* (0.019)	0.013 (0.037)	0.034* (0.020)	0.023 (0.034)	0.039* (0.021)	0.010 (0.032)
Active (age 17-23)	0.023* (0.014)	0.036 (0.024)	0.005 (0.013)	0.086*** (0.026)	0.015 (0.014)	0.055** (0.023)	0.014 (0.014)	0.052** (0.022)
Non-disabled (age 23)	0.023*** (0.008)	0.022 (0.014)	0.015** (0.007)	0.044*** (0.017)	0.018** (0.008)	0.032** (0.014)	0.017** (0.009)	0.034*** (0.013)
Fit for military (boys)	0.132*** (0.040)	0.115* (0.065)	0.125*** (0.040)	0.124* (0.065)	0.107*** (0.040)	0.166*** (0.064)	0.122*** (0.041)	0.126** (0.061)

*Notes:* Each coefficient represents a separate 2SLS regression, with years on parental leave instrumented by the assignment to the reform based on data from *ASSD* and *Ministry*. We use a sample of children born in Austria in June/July 1989/1990, excluding children born  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad, maternal age groups, premature births and birth-year and birth-month fixed-effects. Robust standard errors are shown in parentheses. \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Communities are stratified based on the median of the respective community characteristic in the overall sample of communities with and w/o nurseries. <sup>b</sup>The mother lived in a community with a low/high number of inhabitants per square-kilometer, low/high according to median (0.946). <sup>c</sup>The mother lived in a community with a low/high share of children aged 0-15, low/high according to median (0.180). <sup>d</sup>The mother lived in a community with a low/high share of catholics, low/high according to median (0.926). <sup>e</sup>The mother lived in a community with a low/high female employment rate, low/high according to median (0.528).



Table 8: Child outcomes: adding family size and maternal (full-time) employment

	All communities	Communities <b>with</b> nursery	Communities <b>w/o</b> nursery
<b>Adding family size<sup>a</sup></b>			
Active (age 17)	0.014* (0.007)	0.001 (0.016)	0.019** (0.008)
Active (age 23)	0.010 (0.016)	−0.036 (0.035)	0.029* (0.017)
Active (age 17-23)	0.012 (0.010)	−0.019 (0.022)	0.026** (0.012)
Non-disabled (age 23)	0.014** (0.006)	−0.005 (0.013)	0.022*** (0.007)
Fit for military (boys)	0.090*** (0.029)	0.010 (0.058)	0.122*** (0.034)
<b>Adding maternal employment<sup>b</sup></b>			
Active (age 17)	0.016** (0.008)	0.002 (0.017)	0.020** (0.008)
Active (age 23)	0.014 (0.017)	−0.034 (0.037)	0.033* (0.018)
Active (age 17-23)	0.017 (0.011)	−0.013 (0.023)	0.030** (0.012)
Non-disabled (age 23)	0.017*** (0.006)	−0.005 (0.013)	0.025*** (0.007)
Fit for military (boys)	0.096*** (0.031)	0.015 (0.061)	0.125*** (0.036)
<b>Adding maternal full-time employment<sup>c</sup></b>			
Active (age 17)	0.013* (0.008)	−0.004 (0.017)	0.019** (0.009)
Active (age 23)	0.017 (0.017)	−0.027 (0.038)	0.033* (0.019)
Active (age 17-23)	0.015 (0.011)	−0.015 (0.023)	0.026** (0.013)
Non-disabled (age 23)	0.016** (0.007)	−0.009 (0.014)	0.025*** (0.008)
Fit for military (boys)	0.084*** (0.031)	−0.023 (0.062)	0.125*** (0.036)

*Notes:* Based on data from *ASSD, Ministry* and *ABR*. We use a sample of children born in Austria in June/July 1989/1990, excluding children born  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad, maternal age groups, premature births and birth-year and birth-month fixed-effects. Robust standard errors are shown in parentheses. \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Additional control variables: family size 2, 3, 5, and 10 years after birth. <sup>b</sup>Additional control variables: binary indicators for maternal employment 2, 3, 5, and 10 years after birth. <sup>c</sup>Additional control variables: binary indicators for maternal full-time employment 2, 3, 5, and 10 years after birth.

# A Appendix

## A.1 Tables

**Table A.1: Robustness of child outcomes**

	All communities	Communities <b>with</b> nursery	Communities <b>w/o</b> nursery
<b>No covariates<sup>a</sup></b>			
Test score math (age 15/16)	8.646 (11.438)	−25.090 (23.219)	19.500 (13.228)
Test score science (age 15/16)	8.540 (11.865)	−39.560* (21.371)	22.720* (13.352)
Test score reading (age 15/16)	8.180 (12.012)	−50.207** (21.786)	27.403** (13.447)
High track grade 5 (age 10/11)		−0.294*** (0.082)	
High track grade 8 (age 13/14)		−0.184** (0.084)	
Active (age 17)	0.013* (0.007)	0.001 (0.016)	0.019** (0.008)
Active (age 23)	0.008 (0.016)	−0.041 (0.035)	0.029* (0.017)
Active (age 17-23)	0.011 (0.011)	−0.022 (0.022)	0.027** (0.012)
Non-disabled (age 23)	0.015** (0.006)	−0.004 (0.012)	0.023*** (0.007)
Fit for military (boys)	0.086*** (0.029)	−0.001 (0.058)	0.121*** (0.034)
<b>Controls for maternal pre-birth job and wage<sup>b</sup></b>			
Active (age 17)	0.014* (0.007)	0.003 (0.016)	0.018** (0.008)
Active (age 23)	0.010 (0.016)	−0.030 (0.035)	0.029* (0.017)
Active (age 17-23)	0.014 (0.011)	−0.014 (0.022)	0.026** (0.012)
Non-disabled (age 23)	0.014** (0.006)	−0.004 (0.013)	0.022*** (0.007)
Fit for military (boys)	0.093*** (0.029)	0.017 (0.058)	0.123*** (0.034)
<b>Including children born <math>\pm 5</math> days around cutoff date</b>			
Active (age 17)	0.013** (0.007)	0.003 (0.015)	0.017** (0.007)
Active (age 23)	0.008 (0.015)	−0.021 (0.032)	0.021 (0.016)
Active (age 17-23)	0.017* (0.010)	−0.004 (0.020)	0.027** (0.011)
Non-disabled (age 23)	0.013** (0.006)	−0.006 (0.012)	0.020*** (0.007)
Fit for military (boys)	0.071*** (0.027)	0.010 (0.053)	0.097*** (0.032)
<b>Probit models for binary outcomes (Marginal effects)</b>			
High track grade 5 (age 10/11)		−0.291*** (0.078)	
High track grade 8 (age 13/14)		−0.184** (0.082)	
Active (age 17)	0.014* (0.008)	0.002 (0.016)	0.019** (0.009)
Active (age 23)	0.011 (0.016)	−0.033 (0.035)	0.030* (0.017)
Non-disabled (age 23)	0.017** (0.007)	−0.003 (0.013)	0.028*** (0.011)
Fit for military (boys)	0.090*** (0.029)	0.009 (0.058)	0.121*** (0.033)

*Notes:* Each coefficient represents a separate regression based on data from *PISA* and *EducReg Linz* (education outcomes) and *ASSD* and *Ministry* (labor and health outcomes). We use a sample of children born in Austria in June/July 1989/1990 (1987/1990 in *PISA*). For labor market and health outcomes we exclude children born  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad and birth-year and birth-month fixed-effects. Additional control variables are included for labor and health outcomes: maternal age groups and premature birth. Coefficients for education outcomes represent reduced form estimates, coefficients for labor market and health outcomes are 2SLS estimates, with years on parental leave instrumented by the assignment to the reform. Robust standard errors are shown in parentheses. Estimations for *PISA* education outcomes control for the survey design (school clusters, student weights). \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>No covariates included other than month and year-of-birth. <sup>b</sup>Additional controls for maternal daily real wage (mean over last 2 years before childbirth) and maternal occupation (while-collar/civil-servant, blue-collar, self-employed/farmer/help) included.

**Table A.2: Child outcomes by socio-economic status and gender**

	Maternal SES <sup>a</sup>				Gender	
	Low	High	P-value $\Delta^b$	Girls	Boys	P-value $\Delta^c$
<b>Education outcomes (ITT)</b>						
Test score math (age 15/16)	5.423 (14.942)	24.734 (16.440)	0.402	3.431 (15.659)	24.033 (15.005)	0.341
Test score science (age 15/16)	-1.749 (14.508)	30.545* (15.660)	0.115	5.341 (15.552)	18.348 (15.468)	0.543
Test score reading (age 15/16)	-6.784 (15.091)	32.436* (16.704)	0.087	-0.484 (15.394)	21.729 (15.889)	0.308
High track grade 5 (age 10/11)	-0.296** (0.118)	-0.321** (0.131)	0.887	-0.428*** (0.124)	-0.207* (0.125)	0.209
High track grade 8 (age 13/14)	-0.224* (0.121)	-0.175 (0.139)	0.791	-0.274** (0.130)	-0.164 (0.129)	0.550
<b>Labor market outcomes (LATE)</b>						
Active (age 17)	0.012 (0.010)	0.016 (0.010)	0.808	0.010 (0.010)	0.019* (0.011)	0.536
In education (age 17)	0.014 (0.012)	0.012 (0.011)	0.916	0.003 (0.012)	0.022** (0.011)	0.245
Active (age 23)	0.010 (0.022)	0.010 (0.023)	0.998	0.021 (0.023)	0.002 (0.023)	0.558
In education (age 23)	-0.019 (0.027)	0.021 (0.037)	0.382	0.028 (0.033)	-0.027 (0.030)	0.222
Employed (age 23)	0.027 (0.033)	-0.031 (0.039)	0.258	-0.018 (0.038)	0.019 (0.034)	0.474
Log wage (age 23)	0.008 (0.032)	-0.041 (0.039)	0.325	-0.061* (0.036)	0.032 (0.033)	0.057
Active (age 17-23)	0.016 (0.015)	0.010 (0.015)	0.783	0.013 (0.015)	0.015 (0.015)	0.910
<b>Health outcomes (LATE)</b>						
Non-disabled (age 23)	0.012 (0.009)	0.019** (0.008)	0.562	0.015* (0.008)	0.016* (0.010)	0.919
Fit for military (boys)	0.128*** (0.040)	0.047 (0.043)	0.170		0.092*** (0.029)	

*Notes:* Each coefficient represents a separate regression based on data from *PISA* and *EducReg Linz* (education outcomes) and *ASSD* and *Ministry* (labor and health outcomes). We use a sample of children born in Austria in June/July 1989/1990 (1987/1990 in *PISA*). For labor market and health outcomes we exclude children born  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad and birth-year and birth-month fixed-effects. Additional control variables are included for labor and health outcomes: maternal age groups and premature birth. Coefficients for education outcomes represent reduced form estimates, coefficients for labor market and health outcomes are 2SLS estimates, with years on parental leave instrumented by the assignment to the reform. Robust standard errors are shown in parentheses. Estimations for *PISA* education outcomes control for the survey design (school clusters, student weights). \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Maternal socio-economic status is based on maternal education and pre-birth earnings (low: compulsory education, apprenticeship training or intermediate vocational school plus below median pre-birth earnings, high: apprenticeship training or intermediate vocational school plus above median pre-birth earnings, at least higher secondary education). For the education outcomes in *PISA*, we use maternal education (low: less than higher secondary education; high: at least higher secondary education). For the education outcomes based on register data of *Linz*, we use the proportion of highly educated individuals (higher secondary, post-secondary, tertiary) in the school district (low/high according to sample median). <sup>b</sup>Prob>F(chi2) of difference in coefficients between children of mothers with low/high socio-economic status based on fully interacted regressions. <sup>c</sup>Prob>F(chi2) of difference in coefficients between girls and boys based on fully interacted regressions.

**Table A.3: Maternal characteristics and propensity to work in second year**

Predicted propensity	Employed <sup>a</sup>		Full-time employed <sup>b</sup>	
	Low	High	Low	High
Premature birth	0.05	0.04	0.05	0.03
Low birth weight	0.07	0.04	0.06	0.04
Married	0.30	0.94	0.58	0.66
Foreign born	0.01	0.10	0.01	0.10
Religion				
Roman-catholic	0.92	0.85	0.93	0.83
Protestant	0.04	0.05	0.04	0.05
Muslim	0.00	0.03	0.00	0.03
Other religion	0.04	0.05	0.03	0.06
Without denomination or missing	0.00	0.02	0.00	0.02
Education				
Compulsory education	0.31	0.11	0.27	0.15
Apprenticeship	0.50	0.37	0.53	0.34
Intermediate vocational school	0.14	0.27	0.14	0.26
Higher general or vocational school	0.05	0.14	0.05	0.14
Post-secondary education	0.00	0.05	0.00	0.06
University degree	0.00	0.05	0.00	0.05
Missing	0.00	0.00	0.00	0.00
Occupation				
Self-employed or farmer	0.01	0.02	0.02	0.00
White-collar or civil servant	0.44	0.81	0.45	0.80
Blue-collar	0.47	0.16	0.44	0.19
Missing	0.09	0.01	0.08	0.01
Pre-birth daily real wage	38.75	54.38	39.03	54.10
Pre-birth wage is missing	0.00	0.01	0.01	0.00
Age at birth	23.47	25.11	23.89	24.69
Number of observations	4,482	4,483	4,482	4,483

*Notes:* Mean values of maternal characteristics in the sample of mothers with a low/high predicted propensity to work (full-time) in the second year after childbirth. <sup>a</sup>Propensity of the mother of being employed >0 days in the second year of the child's life. <sup>b</sup>Propensity of the mother of being full-time employed (earn  $\geq 75\%$  of pre-birth earnings) in January following the child's first birthday.

**Table A.4: Child outcomes by predicted maternal propensity to work in second year — alternative classifications of mothers**

	Low work propensity			High work propensity		
	Communities with nursery	Communities w/o nursery	P-value $\Delta^a$	Communities with nursery	Communities w/o nursery	P-value $\Delta^a$
<b>Employed during entire second year<sup>b</sup></b>						
% in Sample	15.30	34.70		14.31	35.68	
Active (age 17)	0.027 (0.024)	0.005 (0.010)	0.400	−0.023 (0.022)	0.034*** (0.012)	0.021
Active (age 23)	0.022 (0.051)	0.027 (0.024)	0.919	−0.086* (0.049)	0.032 (0.025)	0.032
Active (age 17-23)	−0.003 (0.032)	0.022 (0.016)	0.501	−0.026 (0.030)	0.033* (0.017)	0.089
Non-disabled (age 23)	0.007 (0.020)	0.016 (0.010)	0.693	−0.014 (0.015)	0.030*** (0.010)	0.016
Fit for military (boys)	−0.024 (0.082)	0.124*** (0.047)	0.115	0.054 (0.082)	0.122** (0.048)	0.473
<b>Earns <math>\geq</math> 50% of pre-birth wage<sup>c</sup></b>						
% in Sample	16.49	33.52		13.13	36.87	
Active (age 17)	0.029 (0.026)	0.008 (0.011)	0.453	−0.023 (0.020)	0.031*** (0.011)	0.021
Active (age 23)	−0.029 (0.052)	0.022 (0.023)	0.372	−0.046 (0.048)	0.037 (0.026)	0.129
Active (age 17-23)	−0.025 (0.033)	0.017 (0.016)	0.251	−0.008 (0.029)	0.037** (0.017)	0.172
Non-disabled (age 23)	0.008 (0.021)	0.017 (0.011)	0.727	−0.016 (0.015)	0.028*** (0.009)	0.010
Fit for military (boys)	−0.031 (0.085)	0.097** (0.047)	0.183	0.033 (0.080)	0.148*** (0.049)	0.222
<b>Earns <math>\geq</math> 75% of pre-birth wage<sup>d</sup></b>						
% in Sample	18.11	31.89		11.50	38.49	
Active (age 17)	0.011 (0.027)	0.010 (0.011)	0.973	−0.006 (0.021)	0.028** (0.011)	0.147
Active (age 23)	−0.007 (0.056)	0.027 (0.022)	0.564	−0.049 (0.046)	0.034 (0.028)	0.122
Active (age 17-23)	−0.016 (0.034)	0.018 (0.016)	0.369	−0.014 (0.028)	0.037** (0.018)	0.123
Non-disabled (age 23)	0.003 (0.022)	0.021** (0.010)	0.469	−0.008 (0.016)	0.024** (0.009)	0.077
Fit for military (boys)	−0.060 (0.091)	0.113** (0.046)	0.089	0.050 (0.075)	0.132*** (0.050)	0.364

*Notes:* Each coefficient represents a separate 2SLS regression, with years on parental leave instrumented by the assignment to the reform based on data from *ABR*, *ASSD* and *Ministry*. We use a sample of children born in Austria in June/July 1989/1990, excluding children born  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad, maternal age groups, premature births and birth-year and birth-month fixed-effects. Robust standard errors are shown in parentheses. \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Prob>F(chi2) of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. <sup>b</sup>Maternal characteristics indicate a low/high propensity of being employed  $\geq 360$  days in second year after childbirth, low/high according to median prediction (0.19). <sup>c</sup>Maternal characteristics indicate a low/high propensity of being full-time employed (with  $\geq 75\%$  of pre-birth earnings) in January following the child's first birthday, low/high according to median prediction (0.18). <sup>d</sup>Maternal characteristics indicate a low/high propensity of being employed with  $\geq 50\%$  of pre-birth earnings in January following the child's first birthday, low/high according to median prediction (0.25).

**Table A.5: Family size**

	All communities	Communities with nursery	Communities w/o nursery	P-value $\Delta^a$
<b>Number of children<sup>b</sup></b>				
1 year after birth	0.001 (0.005)	−0.002 (0.010)	0.002 (0.006)	0.799
2 years after birth	0.042** (0.020)	0.015 (0.036)	0.052** (0.024)	0.381
3 years after birth	0.058** (0.026)	0.093** (0.047)	0.042 (0.031)	0.358
4 years after birth	0.062** (0.029)	0.100* (0.052)	0.044 (0.034)	0.364
5 years after birth	0.072** (0.031)	0.102* (0.057)	0.058 (0.037)	0.520
6 years after birth	0.086*** (0.033)	0.096 (0.061)	0.079** (0.039)	0.819
7 years after birth	0.080** (0.035)	0.118* (0.064)	0.061 (0.041)	0.453
8 years after birth	0.086** (0.036)	0.114* (0.067)	0.071* (0.043)	0.583
9 years after birth	0.082** (0.037)	0.135** (0.069)	0.058 (0.044)	0.342
10 years after birth	0.076** (0.038)	0.136* (0.071)	0.048 (0.045)	0.294
11 years after birth	0.079** (0.039)	0.152** (0.073)	0.046 (0.047)	0.221
12 years after birth	0.068* (0.040)	0.158** (0.074)	0.028 (0.048)	0.141
13 years after birth	0.072* (0.041)	0.172** (0.075)	0.027 (0.048)	0.104
14 years after birth	0.078* (0.042)	0.166** (0.076)	0.038 (0.049)	0.157
15 years after birth	0.073* (0.042)	0.167** (0.077)	0.031 (0.050)	0.140
16 years after birth	0.075* (0.043)	0.163** (0.079)	0.036 (0.051)	0.172
17 years after birth	0.068 (0.043)	0.155** (0.079)	0.029 (0.051)	0.178

*Notes:* Each coefficient represents a separate 2SLS regression with years on parental leave instrumented by the assignment to the reform based on data from the *ABR* and the *ASSD*. We use a sample of mothers giving birth to their first child in Austria in June/July 1989/1990, excluding mothers giving birth  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad, maternal age groups, premature birth and birth-year and birth-month fixed-effects. Robust standard errors are shown in parentheses. \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. <sup>b</sup>The number of children are measured at the first child's birthday in each year.

**Table A.6: Maternal employment**

	All communities	Communities with nursery	Communities w/o nursery	P-value $\Delta^a$
<b>Mother is employed<sup>b</sup></b>				
1 year after birth	0.004 (0.009)	0.006 (0.017)	0.004 (0.011)	0.904
2 years after birth	−0.316*** (0.021)	−0.322*** (0.040)	−0.313*** (0.025)	0.846
3 years after birth	−0.045* (0.024)	−0.060 (0.045)	−0.037 (0.028)	0.661
4 years after birth	−0.026 (0.025)	−0.056 (0.046)	−0.009 (0.029)	0.382
5 years after birth	−0.012 (0.025)	−0.004 (0.047)	−0.014 (0.029)	0.859
6 years after birth	−0.003 (0.025)	0.075 (0.047)	−0.031 (0.030)	0.054
7 years after birth	−0.008 (0.025)	0.031 (0.046)	−0.020 (0.030)	0.354
8 years after birth	−0.030 (0.025)	−0.013 (0.046)	−0.034 (0.030)	0.694
9 years after birth	−0.017 (0.025)	−0.010 (0.045)	−0.019 (0.030)	0.869
10 years after birth	−0.002 (0.025)	0.014 (0.045)	−0.007 (0.030)	0.694
11 years after birth	0.018 (0.024)	−0.018 (0.044)	0.034 (0.029)	0.324
12 years after birth	−0.002 (0.024)	−0.017 (0.043)	0.006 (0.029)	0.656
13 years after birth	−0.015 (0.024)	−0.016 (0.043)	−0.015 (0.029)	0.976
14 years after birth	0.016 (0.023)	0.010 (0.042)	0.018 (0.028)	0.879
15 years after birth	0.002 (0.023)	0.013 (0.041)	−0.003 (0.028)	0.748
16 years after birth	0.004 (0.023)	0.011 (0.041)	0.001 (0.027)	0.833
17 years after birth	0.021 (0.022)	0.006 (0.041)	0.027 (0.027)	0.670

*Notes:* Each coefficient represents a separate 2SLS regression with years on parental leave instrumented by the assignment to the reform based on data from the *ABR* and the *ASSD*. We use a sample of mothers giving birth to their first child in Austria in June/July 1989/1990, excluding mothers giving birth  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad, maternal age groups, premature birth and birth-year and birth-month fixed-effects. Robust standard errors are shown in parentheses. \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. <sup>b</sup>Maternal employment is measured in January before the child's birthday in each year (i. e., we measure earnings when the child is 0.5, 1.5, . . . , 16.5 years old).



**Table A.7: Maternal full-time employment**

	All communities	Communities with nursery	Communities w/o nursery	P-value $\Delta^a$
<b>Mother works full-time<sup>b</sup></b>				
1 year after birth	−0.004 (0.006)	−0.008 (0.011)	−0.002 (0.007)	0.642
2 years after birth	−0.165*** (0.018)	−0.239*** (0.035)	−0.134*** (0.021)	0.009
3 years after birth	−0.003 (0.020)	−0.059 (0.040)	0.023 (0.023)	0.076
4 years after birth	0.043** (0.022)	0.009 (0.043)	0.063** (0.025)	0.271
5 years after birth	0.039* (0.022)	0.012 (0.044)	0.053** (0.026)	0.423
6 years after birth	0.024 (0.023)	0.029 (0.045)	0.026 (0.026)	0.944
7 years after birth	0.024 (0.023)	−0.011 (0.046)	0.042 (0.027)	0.321
8 years after birth	0.024 (0.024)	−0.057 (0.046)	0.059** (0.027)	0.030
9 years after birth	0.044* (0.024)	−0.047 (0.047)	0.084*** (0.028)	0.017
10 years after birth	0.042* (0.025)	−0.022 (0.048)	0.071** (0.029)	0.096
11 years after birth	0.041 (0.025)	−0.053 (0.048)	0.080*** (0.029)	0.018
12 years after birth	0.034 (0.025)	−0.078 (0.049)	0.081*** (0.030)	0.005
13 years after birth	0.030 (0.026)	−0.035 (0.048)	0.058* (0.030)	0.102
14 years after birth	0.058** (0.026)	−0.008 (0.048)	0.087*** (0.030)	0.098
15 years after birth	0.044* (0.026)	−0.004 (0.048)	0.063** (0.031)	0.241
16 years after birth	0.028 (0.026)	−0.029 (0.048)	0.051* (0.031)	0.160
17 years after birth	0.040 (0.026)	0.009 (0.048)	0.054* (0.031)	0.421

*Notes:* Each coefficient represents a separate 2SLS regression with years on parental leave instrumented by the assignment to the reform based on data from the *ABR* and the *ASSD*. We use a sample of mothers giving birth to their first child in Austria in June/July 1989/1990, excluding mothers giving birth  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad, maternal age groups, premature birth and birth-year and birth-month fixed-effects. Robust standard errors are shown in parentheses. \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. <sup>b</sup>The mother works and earns a daily wage of at least  $\geq 75\%$  of her average pre-birth earnings (over the last two years before birth) in January before the child's birthday in each year (i. e., we measure earnings when the child is 0.5, 1.5, ..., 16.5 years old).

**Table A.8: Family status — full sample**

	All communities	Communities with nursery	Communities w/o nursery	P-value $\Delta^a$
<b>Currently married, full sample<sup>b</sup></b>				
1 year after birth	−0.019 (0.024)	0.014 (0.045)	−0.030 (0.029)	0.411
2 years after birth	0.003 (0.024)	0.047 (0.045)	−0.012 (0.029)	0.269
3 years after birth	0.011 (0.024)	0.069 (0.045)	−0.011 (0.029)	0.133
4 years after birth	0.006 (0.024)	0.041 (0.045)	−0.006 (0.029)	0.373
5 years after birth	0.006 (0.024)	0.054 (0.044)	−0.011 (0.028)	0.222
6 years after birth	0.013 (0.024)	0.050 (0.045)	0.001 (0.028)	0.360
7 years after birth	0.024 (0.024)	0.069 (0.045)	0.009 (0.028)	0.257
8 years after birth	0.025 (0.024)	0.060 (0.045)	0.015 (0.028)	0.400
9 years after birth	0.021 (0.024)	0.061 (0.045)	0.008 (0.029)	0.313
10 years after birth	0.024 (0.024)	0.058 (0.045)	0.014 (0.029)	0.407
11 years after birth	0.023 (0.024)	0.039 (0.045)	0.021 (0.029)	0.744
12 years after birth	0.030 (0.024)	0.050 (0.045)	0.027 (0.029)	0.663
13 years after birth	0.026 (0.024)	0.044 (0.045)	0.024 (0.029)	0.709
14 years after birth	0.022 (0.024)	0.041 (0.045)	0.020 (0.029)	0.702
15 years after birth	0.021 (0.025)	0.025 (0.045)	0.025 (0.029)	0.901
16 years after birth	0.025 (0.025)	0.034 (0.045)	0.027 (0.029)	0.934
17 years after birth	0.023 (0.025)	0.030 (0.045)	0.026 (0.029)	0.996

*Notes:* Each coefficient represents a separate 2SLS regression with years on parental leave instrumented by the assignment to the reform based on data from the *ABR*, the *ASSD* and the *AMR/ADR*. We use a sample of mothers giving birth to their first child in Austria in June/July 1989/1990, excluding mothers giving birth  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad, maternal age groups, premature birth and birth-year and birth-month fixed-effects. Robust standard errors are shown in parentheses. \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. <sup>b</sup>Currently married.

**Table A.9: Family status — cond. on being married at birth**

	All communities	Communities with nursery	Communities w/o nursery	P-value $\Delta^a$
<b>Currently married, cond. on being married at birth<sup>b</sup></b>				
1 year after birth	−0.000 (0.004)	−0.007 (0.009)	0.003 (0.003)	0.256
2 years after birth	−0.000 (0.006)	−0.017 (0.013)	0.008 (0.006)	0.081
3 years after birth	−0.002 (0.009)	0.004 (0.020)	−0.005 (0.009)	0.670
4 years after birth	−0.007 (0.011)	−0.009 (0.025)	−0.005 (0.011)	0.890
5 years after birth	0.002 (0.013)	0.013 (0.030)	−0.001 (0.013)	0.667
6 years after birth	0.006 (0.016)	0.011 (0.035)	0.006 (0.016)	0.899
7 years after birth	0.020 (0.018)	0.022 (0.038)	0.022 (0.018)	1.000
8 years after birth	0.022 (0.019)	0.019 (0.041)	0.026 (0.020)	0.885
9 years after birth	0.023 (0.021)	0.031 (0.043)	0.023 (0.022)	0.865
10 years after birth	0.033 (0.022)	0.048 (0.046)	0.029 (0.024)	0.719
11 years after birth	0.028 (0.023)	0.018 (0.048)	0.037 (0.025)	0.727
12 years after birth	0.032 (0.024)	0.036 (0.051)	0.035 (0.026)	0.980
13 years after birth	0.024 (0.025)	0.042 (0.052)	0.020 (0.028)	0.709
14 years after birth	0.022 (0.026)	0.040 (0.054)	0.017 (0.029)	0.700
15 years after birth	0.014 (0.027)	0.016 (0.056)	0.016 (0.030)	1.000
16 years after birth	0.017 (0.028)	0.025 (0.057)	0.017 (0.031)	0.896
17 years after birth	0.014 (0.029)	0.009 (0.059)	0.021 (0.032)	0.853

*Notes:* Each coefficient represents a separate 2SLS regression with years on parental leave instrumented by the assignment to the reform based on data from the *ABR*, the *ASSD* and the *AMR/ADR*. We use a sample of mothers giving birth to their first child in Austria in June/July 1989/1990, excluding mothers giving birth  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad, maternal age groups, premature birth and birth-year and birth-month fixed-effects. Robust standard errors are shown in parentheses. \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. <sup>b</sup>Currently married in the sample of mothers who have been married at birth.

**Table A.10: Family status — cond. on not being married at birth**

	All communities	Communities with nursery	Communities w/o nursery	P-value $\Delta^a$
<b>Currently married, cond. on not being married at birth<sup>b</sup></b>				
1 year after birth	−0.001 (0.017)	0.015 (0.023)	−0.006 (0.021)	0.522
2 years after birth	0.037 (0.023)	0.094*** (0.034)	0.018 (0.029)	0.090
3 years after birth	0.049* (0.026)	0.115*** (0.039)	0.026 (0.033)	0.080
4 years after birth	0.043 (0.028)	0.075* (0.044)	0.033 (0.035)	0.458
5 years after birth	0.033 (0.029)	0.078* (0.046)	0.019 (0.036)	0.311
6 years after birth	0.041 (0.030)	0.070 (0.047)	0.033 (0.037)	0.532
7 years after birth	0.049 (0.030)	0.098** (0.048)	0.033 (0.037)	0.285
8 years after birth	0.049 (0.030)	0.079 (0.049)	0.040 (0.037)	0.519
9 years after birth	0.039 (0.031)	0.072 (0.049)	0.029 (0.037)	0.484
10 years after birth	0.036 (0.031)	0.049 (0.050)	0.034 (0.037)	0.805
11 years after birth	0.038 (0.031)	0.043 (0.049)	0.039 (0.038)	0.956
12 years after birth	0.047 (0.031)	0.047 (0.049)	0.050 (0.037)	0.967
13 years after birth	0.047 (0.031)	0.029 (0.049)	0.057 (0.037)	0.650
14 years after birth	0.041 (0.031)	0.022 (0.049)	0.052 (0.037)	0.635
15 years after birth	0.046 (0.031)	0.016 (0.049)	0.060 (0.037)	0.481
16 years after birth	0.050* (0.031)	0.028 (0.049)	0.063* (0.037)	0.570
17 years after birth	0.047 (0.030)	0.039 (0.049)	0.055 (0.037)	0.797

*Notes:* Each coefficient represents a separate 2SLS regression with years on parental leave instrumented by the assignment to the reform based on data from the *ABR*, the *ASSD* and the *AMR/ADR*. We use a sample of mothers giving birth to their first child in Austria in June/July 1989/1990, excluding mothers giving birth  $\pm 5$  days around the cutoff date. Each specification controls for the child's sex, low maternal SES, whether the mother was born abroad, maternal age groups, premature birth and birth-year and birth-month fixed-effects. Robust standard errors are shown in parentheses. \*, \*\* and \*\*\* indicate statistical significance at the 10-percent, 5-percent and 1-percent level. <sup>a</sup>Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. <sup>b</sup>Currently married in the sample of mothers who have not been married at birth.