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May 2017

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May 18, 2017

Abstract

We provide a novel interpretation of the estimated treatment effects from evaluations of parental leave reforms. Accounting for the counterfactual mode of care is crucial in the analysis of child outcomes and potential mediators. We evaluate a large and generous parental leave extension in Austria exploiting a sharp birthday cutoff-based discontinuity in the eligibility for extended parental leave and geographical variation in formal childcare. We find that estimated treatment effects on long-term child outcomes differ substantially according to the availability of formal childcare and the mother's counterfactual work behavior. We show that extending parental leave has significant positive effects on children's health and human capital outcomes only if the reform induces a replacement of informal childcare with maternal care. 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. 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 strong public and private debate about who should provide care to young children (Gregg and Waldfogel, 2005). This question has gained importance against the background of increasing female labor force participation and the absence of grandparents within the household, who have traditionally provided informal childcare. Governments in the Western world have responded to these developments by offering two alternative institutions: formal childcare and parental leave (PL) policies. However, these two institutions promote competing models of family organization. Proponents of PL policies implicitly assume that mothers, and more recently also fathers, are the best caregivers. By contrast, advocates of formal childcare prefer children to spend time in a nursery and parents to participate in the labor market.

In this paper, we provide a novel interpretation of the estimated treatment effects from evaluations of PL reforms. We show that accounting for the counterfactual mode of care is crucial. In our evaluation of a large and generous PL extension in Austria, the estimated treatment effects on long-term child outcomes differ substantially according to the availability of formal childcare, and the mother's counterfactual work behavior. Both factors determine the counterfactual mode of care. In our analysis, we implicitly assess the effectiveness of formal childcare and PL in promoting child development relative to informal care arrangements. While these two institutions have thus far been evaluated in two — hardly connected — strands of the economics literature, we argue that a joint evaluation provides an improved understanding of the determinants of child development and long-term outcomes.

In Austria, PL has been a right for mothers since 1957. In 1990, paid and job-protected PL was extended by 12 months, meaning that mothers of children born on June 30 or earlier were eligible for one year of PL, while mothers who gave birth on July 1 or later were entitled to take PL until the child's second birthday. Around the 1990 reform, the availability of formal childcare for under-three-year-olds (provided by nurseries) varied substantially across communities. At that time, two-thirds of the population lived in a community without a nursery. 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, mostly provided by grandparents. This setting provides us with the opportunity to shed light on the effects of PL policies across varying counterfactual modes of childcare. The core of the study is thus to evaluate whether the PL reform had different effects on child outcomes depending on the counterfactual mode of care.

This PL 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 takeup rates were almost universal (Lalive and Zweimüller, 2009). By combining various sources of administrative data, we investigate how the PL extension affected children's educational, labor market, and health-related outcomes in communities with and without nurseries. We also show that this sample split is not confounded by other community characteristics. Moreover, detailed data on the mother's work history allow us to approximate the mother's counterfactual work behavior. A further sample split by this dimension isolates the counterfactual mode of care. 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 mediators is important to fully understand how PL policies affect child development.

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

On average, we find that the reform improved child health outcomes, but had no effect on educational and labor market outcomes. These average effects mask substantial heterogeneity. While we find little variation across socio-economic status (SES) and the child's sex, we observe strong heterogeneity according to the availability of formal childcare and the mother's counterfactual work behavior. The effects on child outcomes are zero (or negative) for children in communities with nurseries and positive in communities without nurseries. These effects are driven by mothers, who would have been working in the counterfactual situation with short PL. 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 main 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 finding indicates that care provided by mothers (or nurseries) is superior to informal care arrangements. The prolonged PL duration also led to small changes in household income. However, we find no evidence for the relevance of this income effect.

Our results on potential channels fully confirm and expand the analyses by Lalive and Zweimüller (2009) and Lalive, Schlosser, Steinhauer and Zweimüller (2014), who investigate 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 eight 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 PL extension brought about a significant rise in subsequent fertility among affected mothers and altered the spacing of births (examined up to 10 years after childbirth). As our analysis of 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).

Only one study has thus far investigated the effects of this PL reform in Austria on child outcomes. By using data from the OECD's Programme for International Student Assessment (PISA), Danzer and Lavy (forthcoming) find no significant *intention-to-treat effects* (ITTs) of the PL extension on proficiency scores in mathematics, reading and science at age 15. However, their subgroup analysis uncovers significantly positive effects on PISA scores for children (especially boys) of highly educated mothers. By contrast, the PL 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 PL takeup, maternal employment, siblings, family status, or childcare attendance, the authors focus on ITTs and cannot explore and test potential mediators through which the reform may have affected child outcomes. Furthermore, it is unclear whether the effects on schooling outcomes 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 on this topic by conducting one of the most comprehensive long-run studies of a PL reform on child outcomes. Our analysis is facilitated by the clear design of the policy reform and unique and superior data available in Austria. We highlight the role of the counterfactual mode of care and provide the first evidence on how it shapes the effect of extended PL on child outcomes. Moreover, we provide new and important insights by carefully discussing and assessing potential mediators through which the reform might have affected child outcomes.

[Table 1]

Design-based papers, most of which focus on Nordic countries, have exploited unanticipated changes in paid PL to evaluate the importance of such leave and early maternal employment for child development (see the overview in Table 1). The large majority of these studies focus on schooling outcomes; only two studies estimate the long-run effects on adult labor market outcomes (Carneiro, Løken and Salvanes, 2015; Dustmann and Schönberg, 2012). One group of studies examines PL extensions within the first year after birth. The dominant finding of these studies is that PL in the first year has no effect on child development captured by educational and labor market outcomes (see Rasmussen (2010) for Denmark, Baker and Milligan (2010, 2015) for Canada, Dahl, Løken, Mogstad and Salvanes (2016) for Norway (1987-1992) and Dustmann and Schönberg (2012) for Germany). The only exception is the study by Carneiro et al. (2015), who document significant positive effects on long-term child outcomes of the 1977 reform in Norway. Another two studies examine PL extensions in the second and third years 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 of variation are institutional differences in the PL 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 PL takeup. Many existing studies are restricted to estimating the ITTs on children, as they cannot match a child with maternal information. Since we have perfect information on PL takeup, we can estimate *local average treatment effects* (LATE) for most of our outcomes. Studies also differ in their ability to control for seasonal and age effects. Another potential explanation is that treatment effects depend on the counterfactual mode of care. This explanation has been largely ignored by previous research on PL. Our findings point to the importance of the counterfactual mode of care.[1]

Second, we contribute to the literature on the role of childcare for child development and human capital formation. In particular, we add to the scarce quasi-experimental evidence on the impact of childcare for children below the age of three. Most of the literature on childcare focuses on children aged three and above and provides mixed results. Some studies suggest that increased informal childcare (i.e., non-center-based care provided by grandparents, relatives, or child-minders) has negative effects on child outcomes compared with parental care, while formal (i.e., center-based) childcare has no adverse effects (e. g., Bernal and Keane, 2011; Datta-Gupta and Simonsen, 2010). The quasi-experimental evidence from Norway and Germany points towards the positive effects of formal childcare on child outcomes (e. g., Havnes and Mogstad, 2011; Cornelissen et al., 2016; Gathmann and Sass, 2012), whereas analyses in Canada and the United States come to the opposite conclusion (e. g., Baker et al., 2008; Herbst, 2013) or find only positive effects for children from particularly disadvantaged households (Fitzpatrick, 2008; Kottelenberg and Lehrer, 2017). Only a few studies provide evidence for children *below the age of three*.[2] While Felfe and Lalive (2014) and Drange and Havnes (2015) find positive effects for early center-based care on child development, Fort et al. (2016) report negative effects, particularly for girls. Again, differences in the counterfactual mode of care and quality of formal childcare might explain the conflicting results, making it difficult to draw general conclusions from the existing evidence. Better knowledge and more evidence for the impact

[1] A recent paper by Kline and Walters (2016) shows that the impact of Head Start on test scores depends on the counterfactual mode 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.

[2] We focus here on the evidence from universal childcare programs in developed countries. Preschool and childcare programs in developing countries are often targeted at the low-income population and provide not only day care but often also include nutritional programs (see, for instance, Noboa-Hidalgo and Urzùa, 2012; Behrman et al., 2004). Also see Elango et al. (2016) for a recent summary of the evidence on universal and targeted programs in developed countries.

on the very young are urgently needed, as about one-third of under-three-year-olds in OECD countries attend formal childcare and this upward trend is expected to continue.[3]

Third, we show that PL policies and formal childcare are important aspects of the early childhood environment, thereby contributing to the literature that emphasizes the importance of this environment for the production of human capital (Cunha et al., 2006). Indeed, our findings add to the dynamic ongoing policy debate (Elango et al., 2016; Rossin-Slater, 2017). Countries invest heavily into PL benefits and formal childcare. Among OECD countries, average public spending on maternity and PL cash benefits was 0.38 percent of national GDP.[4] By comparison, total family-related public expenditure comprised on average 2.24 percent of GDP, while expenditure related to early childhood education and care amounted to 0.71 percent.[5] Given the increasing shares of working mothers and under-three-year-olds enrolled in formal day-care centers, our analysis therefore provides unique and timely insights into the interplay of maternal employment, PL policies, and formal childcare.

The remainder of the paper is organized as follows. Section 2 provides details on the Austrian PL reform and other relevant aspects of the institutional setting. Section 3 introduces our data. Section 4 presents our research design. We also define our treatment, assignment, and outcome variables, present our estimation strategy, and discuss the identifying assumptions in this section. Section 5 presents our results. First, we show the effects of the PL reform on child outcomes in general and with regard to the counterfactual mode of care. We further discuss the importance of the availability of a nursery relative to other community characteristics. We then present evidence on various potential mediators such as fertility behavior, maternal labor supply, and family stability. Lastly, we provide several robustness checks. Section 6 concludes.

2 Institutional background

In this section, we describe the PL system before and after the 1990 reform. To enhance the understanding of the Austrian institutional background, we also provide information on female labor force participation, the availability and characteristics of formal childcare, and the use of informal care.

[3]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.

[4]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.

[5]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).

2.1 Austrian PL system and the 1990 reform

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

The eligibility criteria for PL and associated transfer payments as well as the maternity leave regulations remained unaffected by the reform. Maternity leave, which precedes PL, mandates a compulsory leave period of eight 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 their average net earnings of the preceding 13 weeks (*Wochengeld*). To become eligible for PL, mothers needed 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), 20 weeks of equivalent employment during the last 52 weeks were sufficient. During the PL period, eligible women received a monthly transfer payment of €352 (in 2015 values).[6] This corresponded to about 40 percent of net median female earnings. As a side effect, the 1990 PL extension also prolonged the automatic renewal period during which mothers were allowed to transition from one PL spell to the next without fulfilling the work criteria. Since we focus on first-born children, this aspect of the reform affects our analysis only indirectly.[7]

Several features of the reform make it particularly suitable for our analysis, as they allow us to identify 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 PL extension was announced and implemented at relatively short notice. It passed the Austrian parliament only in April 1990 and 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 to exploit the more generous PL regime. Indeed, we find no evidence that parents postponed their delivery date. *Third*, the reform affected the vast majority of mothers, since almost all first-time mothers were eligible and PL takeup among eligible mothers was almost universal. *Fourth*, the reform increased the average PL duration substantially.

[6]Non-married mothers who did not live in the same household with the child's father and who did not receive sufficient child support from him, and married mothers whose husbands earned no or low income received about 50 percent higher assistance.

[7]The automatic renewal period elapsed 3.5 months after the expiration of the maximum PL. To benefit from this PL renewal, pre-reform (post-reform) mothers had to give birth to another child within 15.5 (27.5) months of the previous birth. Lalive and Zweimüller (2009) show that this change in the automatic renewal regulation affected the timing and spacing of second births.

2.2 Female labor force participation

In 1990, about 64 percent of all Austrian women between the ages of 25 and 54 participated in the labor market, a rate lower than those in Scandinavian and Anglo-Saxon countries, comparable to that in Germany, and well above those in Southern Europe.[8] For the population of women without children (between the ages of 25 and 40) participation rates were substantially higher, namely 88.3 percent.[9] Hence, the vast majority of these women should have been eligible for PL and thus affected by the reform. According to data taken 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 (*the Austrian Social Security Database, ASSD*) based on precise information on prior employment.

2.3 Formal and informal childcare

The Austrian system of formal childcare distinguishes between facilities for children below the age of three (nurseries, *Kinderkrippe/Krabbelstube*) and for those aged three to six (kindergarden, *Kindergarten*). While the vast majority of communities have offered a kindergarden since the 1980s, the local availability of nurseries has been traditionally much lower. In 1990, about 2 percent of communities had nurseries. The existing nurseries were predominantly in more densely populated areas. Therefore, the share of the covered population (around 33 percent) was substantially larger than the share of communities (see Table 2). This fact created a regional dispersion in the local availability of nurseries that we exploit in the following analysis. Importantly, the supply of nurseries was stable in the years around the reform.

[Table 2]

The upper panel of Figure 1 plots the crude enrollment rates of children below one year, between the ages of one and two and between the ages of two and three for communities *with* nurseries. We calculate these crude rates by using data on the number of enrolled children by age and community and the number of children in the respective birth cohort and community.[10] These crude rates cannot be directly applied to our estimation sample, since we cannot distinguish between children's parity, country of birth, and their mothers' employment status. In

[8] 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).

[9] Own calculations based on Austrian Census data from the year 1991. The corresponding participation rates for women with one and two children were 78.0 and 57.8 percent, respectively.

[10] 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.

our estimation sample, we include only first-born children born in Austria whose mothers were eligible for PL. We expect substantially higher enrollment rates for this group.

Between 1988 and 1990, the crude enrollment rate was below 0.5 percent for children under one year, around 8 percent for one-year-old children, and around 17 percent for two-year-old children. In the two years after the PL extension in 1990, the enrollment rate of one-year-olds decreased substantially, while it remained constant for the other two groups. In 1992, the year in which the figure for one-year-olds represents the first complete post-reform cohort (born between January and December 1991), the enrollment rate dropped by half. This finding is in line with the notion that the PL extension induced a substitution of formal childcare with maternal care for one-year-olds.

[Figure 1]

Since only the children of working mothers attended formal childcare, enrollment rates for this group of the population should actually have been higher.[11] We approximate the enrollment rates for (full-time) employed mothers by using only children of (full-time) employed mothers as a denominator. 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 of the crude rate. Only 35 percent of these mothers were employed (21 percent were employed full-time) even in the absence of the PL entitlement. This leads to adjusted enrollment rates of around 22 percent and 35 percent between 1988 and 1990 for children of employed and full-time employed mothers, respectively (see the lower panel of Figure 1). Thus, the PL reform induced a replacement of formal childcare with maternal care for a substantially higher share of children than suggested by the crude 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 two educators per group. On average, 1.5 of these graduated from a college for nursery education (ISCED level 4B degree).[12]

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 two-year-olds was about €136 (in 2015 prices) per child, considering a standard care arrangement of about four to six hours per day. For children

[11] One prerequisite for attending formal childcare is that both parents work (typically at least 20 hours per week).

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

having lunch at the childcare facilities, average expenditure rose to about €220. These costs correspond to about 10 to 17 percent of the average monthly earnings of women at that time.[13]

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

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

3 Data

We construct our main data set by combining various administrative data sources. In our main data set, we observe the universe of births with detailed information on families' SES. Most importantly, we can follow the mother and child over time along different aspects of life. The *ASSD* provides information on the mother's eligibility for PL, her actual takeup, her return to work behavior, the child's labor market behavior, and any other event relevant for pension claims such as periods of military service.[14] The *Austrian Birth Register* enables us to closely track subsequent fertility behavior. The *Austrian Marriage Register* and *Austrian Divorce Register* document any change in marital status. Finally, the 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 to generate our outcome variables, our treatment and assignment variables, and a comprehensive set of covariates. At the family level, our outcome variables include current family size, maternal

[13] According to our estimates from the Austrian Microcensus 1995, average monthly earnings of employed women in childbearing age (aged 20 to 45) were about €1,304, when considering only women working 35 hours or more, this average wage was about €1,461.

[14] 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).

labor supply, and marital status. Finally, we define a number of children's medium- and long-term labor market and health outcomes. Starting from the universe of children born in Austria, we restrict our analysis to first-born singleton children of mothers aged between 15 and 45 years at the time of birth and eligible for PL.[15]

To obtain educational outcomes, we use PISA data from 2003 and 2006 and data from the *Educational Register* of the city of Linz (EducReg).[16] These data sets have several drawbacks compared with our main data set. First, these data do not cover the universe of births. PISA includes a representative sample of about 5,000 children aged 15/16 years at the time of testing. Thus, in PISA 2006 the 1990 birth cohort was sampled and in PISA 2003 we observe the 1987 birth cohort. The EducReg includes all children residing in Linz. Second, since these data do not include information on the mother's eligibility or actual PL takeup, we can only estimate the ITTs. However, given the high takeup rate, the difference between the ITT and 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 the month of birth is available).[17] Fourth, the set of covariates is smaller. Fifth, the PISA and EducReg lack information on community of birth. Thus, we have to implicitly assume that we observe children (and mothers) in their community of birth. While we know that nurseries were available in Linz in 1990, we have to proxy for childcare availability in our analysis of the PISA data. We use the number of inhabitants in the community in which the school is located and assume that communities with more than 100,000 inhabitants had a nursery in 1990, whereas communities with fewer than 100,000 did not.

4 Research design

We estimate the effect of the PL extension by combining an RDD with a DiD approach. In this setup, the treatment resembles a prolonged duration of paid and job-protected 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). To identify the treatment effect, we exploit the discontinuity in the PL duration at the reform date and compare the 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 (1987 when using PISA data) as a control cohort.

[15] There is no difference in the share of twins or multiple births before and after the reform cutoff date.

[16] 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.

[17] Since the EducReg provides no information on the child's country of birth, we exclude all students with foreign language or citizenship. This sample restrictions aims to exclude children, who were potentially not exposed to the Austrian PL system. Austria witnessed a large influx of migrants post 1993.

4.1 Treatment and assignment variables

The treatment variable is defined as the PL duration. The maximum PL duration is 10 months before and 22 months after the reform. Assignment into treatment depends on whether a child is born in the post-reform period (on July 1, 1990 or later). Panel A of Figure 2 depicts the relationship between assignment and treatment for eligible mothers. It plots, by birthdate, the average PL duration measured in days excluding the post-birth maternity leave period. In 1990, we observe a distinct jump in the average PL duration from 285 to 590 days. Given that the average post-birth maternity leave period is about 56 days, we can conclude that the reform increased the average PL duration almost from one year to almost two years. By way of comparison, the average PL duration in 1989 has no intra-year variation at all.

[Figure 2]

Eligibility and takeup rate This clean and large jump in the PL duration results from two facts, which make this reform particularly useful from a methodological perspective. First, the share of mothers eligible for PL before and after the reform is high (about 90 percent). Panel B of Figure 2 depicts the share of eligible mothers pre- and post-reform. Second, the actual takeup of PL is almost universal in both periods (around 97 percent). Panel C of Figure 2 refers to the takeup rate among eligible mothers. In both years, there is no discontinuous change in the respective share around the cutoff date.

4.2 Outcome variables

Children's educational outcomes First, we analyze the PISA test scores in the fields of mathematics, science, and reading. 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 educational tracks. Higher secondary schools (the *high track*) comprise grades 5 to 12/13, 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 an 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.[18] In the PISA sample, which covers students in grade 9

[18]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).

and is representative of Austria, about 63 percent of students are in the high track.[19] Table 3 summarizes these and all the other outcome variables.

[Table 3]

Children's labor market outcomes The majority of students who graduate from the low track enter the workforce at around age 16, ideally via the dual education system, or as unskilled workers. We analyze children's labor market outcomes 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 in education (school, apprenticeship, or university), employed, in military (or alternative civilian) service, or on maternity leave/PL. Inactive children are either unemployed, only marginally employed, disabled, on long-term sick leave, or on rehabilitation.

In particular, we define binary variables capturing children's activity status at the ages 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 ages of 17 and 23 (87 percent on average) and a binary variable for children active in each period during this age range. The latter variable has a mean of 0.49. 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 wages.

Children's health outcomes We use binary outcome variables to assess children's health. The first variable assesses the disability status up to the age of 23. We exploit the available information on the receipt of family allowance, which is granted for any child with a physical or mental disability. We define two binary variables. First, we code the variable 'non-disabled' equal to one if the child has never been disabled up to 23 years of age (93.5 percent). Second, we code a binary variable 'capable of work' equal to one, if the child has not been classified as permanently unable to work by the regional chief medical officer (98.6 percent).

A further health variable indicates whether male children are fit for military service. This is derived from the ASSD, which provides information on whether a man has served in the military or carried out alternative civilian service until the age of 23. In Austria, all male citizens are subject to compulsory military service and must enlist for examinations within a year of 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

[19] Data for the school year 2006/07 show that after grade 8 about a third of graduates from the low track transfer to the high track (Schneeweis and Zweimüller, 2012).

measuring the extensive and intensive margin. The extensive margin is captured by binary indicators coded 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 to measure 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 two years before birth).[20] 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 examine 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 10 years after parity one is about 1.9. 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 married.

4.3 Econometric model

We exploit the sharp birthday cutoff-based discontinuity in the eligibility for extended PL to estimate the treatment effects on all the outcomes discussed above. While the relationship between assignment and treatment is strong, it is not fully deterministic; hence, we set up a *fuzzy RDD*. We use 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:

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

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

In the first-stage equation (1), the dependent variable is PL_i, the actual PL duration measured in years. The assignment variable T_i is coded one if a child is born in the post-reform period. The vector of covariates X_i comprises information on maternal age at birth (15-20 years, 21-25 years, ... 41-45 years), maternal SES,[21] 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 1990. To account for any unobserved characteristics that follow a seasonal pattern between

[20] 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.

[21] We form two groups based on education and pre-birth earnings. We classify mothers as low SES all mothers who completed compulsory schooling or who completed apprenticeship training or intermediate vocational school and have below median pre-birth earnings. High SES mothers are all mothers with at least higher school or who completed apprenticeship training or intermediate vocational school and above median pre-birth earnings.

children born in June and July, we pool information on unaffected control cohorts from 1989.[22] Thus, we use information on all births from June and July 1990 and June and July 1989. This data structure allows us to control for birth year effects (δ_{1y}) and birth month effects (θ_{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 by being assigned to the new regulations. In our complementary data sets, which we use to examine educational outcomes, we do not observe the actual PL duration. Therefore, we estimate the following ITT:

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

Three conditions need to hold for $\hat{\beta}_2$ to be informative about the causal effect of an additional year of PL. First, assignment to the increased PL duration T_i must predict actual takeup 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 takeup rate at the cutoff (see 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 assignment increases the average PL duration by 0.813 years or 297 days. The estimated coefficient is highly statistically significant with an F-statistic of about $6,600$. This coefficient is stable across sub samples.[23]

The inability to precisely manipulate assignment into treatment is the key identifying assumption behind any RDD. Public discussion about the potential reform of the PL system started in November 1989 (Lalive and Zweimüller, 2009). By April 5, 1990, the Austrian government had 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.[24] Figure 3 shows that the average number of births per day does not vary around the cutoff date. Thus, there is no evidence of manipulations of the birthdate. Still, to be on the safe side, we exclude births five days before or after the cutoff date.[25] This so-called

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

[23] The largest difference is observed between mothers with low and high SES, for whom we obtain coefficients of 0.838 and 0.781, respectively.

[24] This could apply to planned Caesarian sections and induced labor. 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.

[25] One of the first studies to demonstrate marginal timing of births due to financial incentives is Dickert-Conlin and Chandra (1999) for the US.

doughnut sample should be free of any sorting. Depending on the outcome under consideration, our sample has around 9,000 observations (see Table 3 for more details).

[Figure 3]

Whether assignment is correlated with any outcome-determining factor is not fully testable; however, it is reassuring that none of our covariates changes discontinuously around the cutoff. Figure 4 plots the daily averages of all covariates and other pre-determined variables between May and September 1990. More formally, we test for differences in the means of the covariates and other predetermined variables between not-assigned (child born in June 1990) and assigned families (child born in July 1990). Table 4 shows no quantitatively important differences (see the fifth column). Few differences are statistically significant (see the sixth column), but there is no evidence of 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 and not by other formal or informal caregivers. This LATE is a weighted average of the LATEs for three subpopulations: (i) the LATE for children who switch from informal care to maternal care, (ii) the LATE for children who switch from formal care to maternal care, and (iii) the LATE for children who do not experience a change, since their mothers provide maternal care in the second year of the child's life independent of the PL entitlement. The latter children experience only an increase in family income (see Section 5.2). 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 for working mothers, we exploit the 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 (i.e. grandparents). In communities with nurseries, the counterfactual could be formal childcare, informal care arrangements, or a combination of both. We presuppose that a large proportion of working first-time mothers would have relied on formal care arrangements during their child's second year of life in the absence of the reform.

However, given that enrollment in formal care was not universal, we cannot claim that it 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 across communities with and without nurseries by splitting our sample along this dimension. It turns out that this dimension, combined with mothers' counterfactual work behavior, 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 educational, labor market, and health outcomes. We study how the expansion in PL affected children by exploring the relative importance of time with the mother compared with income effects. Moreover, we provide evidence that the availability of formal childcare is responsible for the observed treatment effect and not a confounding factor at the community level. Second, we examine the treatment effects on family size, maternal labor supply, and family stability up to 17 years after birth. Any significant behavioral response 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

Average effects Table 5 presents the estimation results for the educational, labor market, and health-related outcomes of children for the full sample. 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 the average effects (the ITT for educational outcomes and the LATE for all other outcomes). The PL extension has no effect on educational and labor market outcomes and significantly positive effects on health. Children whose mothers are exposed to extended PL are more likely to be capable of work (plus 1.7 percentage points) and treated boys are more likely to be fit for military service (plus 9.2 percentage points). The positive effect of extended PL on health is most likely to be driven by the higher likelihood of (appropriate) early intervention. Indeed, US studies provide evidence that the early identification of impairment can improve adult health outcomes (Campbell et al., 2014).

[Table 5]

Effects according to the availability of formal childcare 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 the test scores in science and reading differ significantly between communities. In communities with childcare facilities, the coefficients are negative for all three subjects, although statistically significant only in reading. By contrast, we obtain positive and statistically significant effects in communities where childcare is unavailable. The positive coefficients on the test scores amount to about one quarter of the standard deviations in these variables. In addition to the test scores, we investigate high track attendance in different grades; however, we do not find any significant effects in grade 9. By contrast, significant negative effects are obtained for high track attendance in grades 5 and 8 in Linz (a community with a nursery).

For labor market and health outcomes, we find a similar pattern. In communities with nurseries, the estimated coefficients are mostly negative; however, with one exception, they are 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. At age 17, our activity measure is driven by education. Treated children in communities without a nursery are more likely to attend a school or apprenticeship training. For age 23, the coefficients in the regressions for specific kinds of activities (e.g., education and employment) and wages (conditional on employment) are not significant. At this age, it is unclear whether being in education or being employed is better. 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 apprenticeship training. Being employed at that age would be a positive outcome for children who would be inactive, still in school, or attending apprenticeship training in the counterfactual situation; on the contrary, it would be a negative outcome for children who would attend university in the counterfactual situation. Similarly, the wage results are also difficult to interpret, since we assess wages only for the selected group of employed individuals. Because of these shortcomings, we focus on our activity measures henceforth. Being active as opposed to 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 3 percentage points, 2.5 percentage points and 12.2 percentage points in these areas for the outcomes *non-disabled*, *capable of work* and *fit for military*, respectively. These effects are sizeable and amount to 3.2, 2.5 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. This strongly suggests that the counterfactual mode of care plays an important role. The positive treatment effects in communities where formal childcare is not available suggests 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 a sensitivity analysis for our main outcome variables. The results are robust to the exclusion of individual covariates (sex, maternal SES, maternal migration background, premature birth, and maternal age), the inclusion of maternal pre-birth occupation and wages, and the inclusion of children born five days either side of the cutoff date. Probit and IV-Probit regressions yield marginal effects that are similar in size to the coefficients obtained in the linear regressions.

We also analyze potential heterogeneities with respect to maternal SES and the sex of the child (see Table A.2 in the Appendix). These results show no systematic pattern with respect to maternal SES. However, our results suggest that the reform was somewhat more beneficial for boys which is in line with previous evidence for Austria (Danzer and Lavy, forthcoming).

5.2 Counterfactual work behavior and time with the mother

The estimated effects capture two important 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 between two groups of mothers. For 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 prolonged job protection.[26] By contrast, for mothers who changed their return-to-work behavior following the reform, the treatment effect captures a likely decrease in income as well as a prolonged period with the child. For these children, the mode of care also changes. Owing to missing information on their counterfactual return-to-work behavior, we cannot differentiate between these two groups of mothers or cleanly disentangle the effects of income and the change in the mode of care. However, we use complementary estimation strategies to uncover the dominant forces 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 the child's age in communities with and without nurseries. A similar pattern is observed in both types of communities. During the first year of the child's life, only 3 percent of mothers were employed and thus not on PL. For pre-reform mothers, employment increased only to about 35 percent in the second year of the child's life, implying that 65 percent

[26]The extended job protection might foster the medium-run labor market attachment of these women.

of mothers stayed at home with their child even in the absence of a PL entitlement. Thus, for around 35 percent of children in our sample, the reform induced prolonged maternal care, while the reform did not change the duration of maternal care for the remaining 65 percent of children.

[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 second year, and subtract the yearly amount of PL benefits.[27] 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 an income in the second year, the reform either increased or decreased disposable income depending on 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, about 35 percent of the children in our sample experienced more time with the mother and potentially lower household income, while 65 percent experienced no change in the time with the mother but the family had a higher income. This raises the question of 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 predetermined observable characteristics and propose the following strategy:

- First, we use pre-reform mothers and estimate their propensity of being employed in the second year of the child's life as a function of their characteristics (e.g., education and pre-birth earnings).
- Second, by using these characteristics, we predict the propensity to work in the second year for the full sample of mothers (pre- and post-reform).
- Third, we split the sample at the median 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 (or to work full-time), we estimate a linear probability model of employment in the second year on maternal characteristics and birth outcomes.[28] As

[27] In this calculation we make the simplifying assumption that all mothers stay at home during the entire second year.

[28] These variables are mostly taken from the *Austrian Birth Register* and characterize the time before birth. We use information on 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).

expected, mothers with a higher education and higher pre-birth earnings are more likely to work in the second year of the child's life. Moreover, foreign-born mothers and mothers with a religion other than Roman Catholic have a higher propensity to work. Overall, a higher propensity to work is positively correlated with our measure of maternal SES. 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 the summary statistics of maternal characteristics for women with a low and a high propensity to work (full-time) in the second year of the child's life.

[Table 6]

Table 6 summarizes the separate estimations for mothers whose characteristics indicate a low and a high propensity to work (in communities with and without nurseries). This set of estimations provides the key result of our analysis. The positive effects obtained for children in communities without a nursery are driven by mothers with a high propensity to work (i.e., who would have worked in the second year if PL would have lasted only one year). Strikingly, most of the coefficients are twice as large as those 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 likelihood that the child is not disabled is raised by 5.9 percentage point. The probability of being fit for military service is increased by almost 18 percentage points. By contrast, the coefficients for low propensity mothers in communities without a nursery are almost zero (with one exception) and not statistically significant. This finding suggests that the income effect is of secondary importance. In communities with nurseries, none of the coefficients is — irrespective of the mother's propensity to work — statistically significant. These results are robust to alternative classifications of maternal employment.[29]

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 replaced informal care arrangements with maternal care. This finding suggests that time with the mother is the driving force behind our results on child development. Concerning the income effects, we conclude that the PL 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.

[29] 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 (≥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.

5.3 Confounding community characteristics

Our estimated treatment effects strongly differ according to the availability of formal childcare at the community level. This heterogeneity may be explained by the counterfactual mode of care. However, other community characteristics may be crucial in shaping the treatment effects of PL. Indeed, communities with and without nurseries differ in many aspects such as the level of urbanization, age structure of the population, preponderance of conservative values, and female participation in the labor market. The number of inhabitants per square kilometer is around 51 in communities with nurseries and around 2 in those without. The share of children below the age of 15 is 0.13 in communities with nurseries and 0.18 in communities without. The percentage of Roman Catholic inhabitants is 69 in communities with childcare and 91 in those without. Finally, the female employment rate is 64 percent where nurseries are available and 52 percent where they are unavailable.

To shed light on the importance of other potentially confounding community characteristics, we pursue the following strategy. We create indicators for the low and high values of these community characteristics based on their median in the pooled sample (communities with and without nurseries) and split each sample according to 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 if the availability of formal childcare is indeed decisive, then the estimated effects should not only be significant in the sample of typical, but also in that of atypical communities. Examples of atypical communities are communities without formal childcare, but with a high level of urbanization, a low share of children below age 15, a low share of Roman Catholics, or a high female employment rate. 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]

Table 7 shows the estimation results for typical and atypical communities without nurseries for the sample of all mothers (upper Panel), mothers with a high work propensity (middle Panel) and mothers with a low work propensity (lower Panel). In the first two columns, we compare communities with high and low population density. We find comparable effects in typical and atypical communities (the estimates do not differ significantly from each other). Next, we investigate the age structure of the population by using the share of inhabitants below the age of 15. We find significant effects for typical and atypical communities. For activity at age

17 and age 17-23, the effects are even larger in atypical communities. Furthermore, we check whether conservative values of the population might confound our estimates. By using the share of Roman Catholics as an indicator of traditional family values, we again find positive effects for typical and atypical communities. Finally, we focus on differences in the female employment rate. Again, positive effects are found in typical and atypical communities. The separate estimations for mothers according to their work propensity in the child's second year of life confirm the results obtained so far: the positive effects of the reform stem from children with high work propensity mothers. These effects are observed in all types of communities without nurseries.

This exercise provides supporting evidence for our hypothesis that the availability of formal childcare and its consequences for the counterfactual mode of care drive the effects of PL as opposed to other correlated community characteristics.

5.4 Fertility, maternal labor supply, and family stability

PL policies affect the relative costs of child-bearing and may therefore alter fertility decisions. Indeed, the 1990 Austrian PL 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 may alter specialization within the household, the bargaining power of spouses, and marital stability. To shed some light on the effects of these potential mediators, we estimate the effects of the reform on family size, maternal employment, and family stability.

Family size Figure 7 shows the estimated coefficients and 95 percent confidence intervals obtained by 2SLS regressions of PL duration on family size up to 17 years after treatment for women in communities with and without nurseries. Table A.5 in the Appendix provides the full regression results.

[Figure 7]

We find that the PL reform significantly increased fertility in communities where childcare is available. The coefficient of the number of children ranges from 0.09 in the third year of the child's life to 0.16 in year 17. Thus, the PL extension induced the birth of 16 additional children per 100 women within 17 years. Given the 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 because of the reform. However, the reform had no effects on completed fertility in these communities.

Maternal labor supply Extended PL and the resulting increase in family size might negatively affect maternal labor supply. Figure 8 shows the estimated coefficients of PL on the probability that the mother is employed (upper panel) or full-time employed (lower panel) in the years of the child's life. Tables A.6 and A.7 in the Appendix show the full estimation results.

[Figure 8]

The additional year of PL has similar negative effects on maternal labor supply in the second year of the child's life in both subsamples (32 and 31 percentage points in communities with and without nurseries, respectively). These results confirm the descriptive evidence presented in Section 4. After the extended PL period has expired, we find no significant effects on maternal labor supply. Given the sizeable fertility effects of the reform, the results show that treated mothers in communities with nurseries are able to reconcile family life and employment relatively quickly.

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. By 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. Extended PL should help women return to work. In communities without nurseries, women may be more likely to use their right to return to their job after two years as opposed to one year. Furthermore, the somewhat reduced spacing of births between the first and second children might reduce the overall absence from work, thereby assisting 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 mothers are able to work more in the absence of child development problems.

Family stability Figure 9 summarizes the results on family status and Tables A.8 - A.10 in the Appendix provide 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 were and were not married at the time of birth.[30] No significant results are obtained for mothers married at birth, indicating that the probability of divorce has not been influenced by the PL reform. For mothers unmarried at birth, the reform

[30] 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).

increased the probability of getting married in communities with nurseries in the first years after birth. The coefficients are statistically significant up to seven years after birth and fade away thereafter. This result is in line with our estimations of 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 in labor supply on the extensive margin, no effect on long-run full-time employment, and an increased propensity to get married in the medium term. Mothers in communities without formal childcare reacted very differently to the reform. Apart from a differential spacing between births, these women did not alter their fertility decisions. Further, while they did not change their labor market participation, they increased their full-time employment in the medium and long run. 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. Our evidence suggests that the counterfactual mode of care drives this heterogeneity. In this final section, we explore the role of potential mediators. As discussed in the previous section, the reform influenced family size, maternal employment, and family stability. In communities with nurseries, the PL reform increased fertility by around eight percent. Furthermore, mothers reduced their short-run labor supply at the extensive margin. Thus, reduced parental monetary and time investments into the child because of a quantity/quality tradeoff 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 second children somewhat.

[Table 8]

Table 8 shows the sensitivity of our estimates for children's labor market and health outcomes to the inclusion of family size and maternal (full-time) employment in the second, third, fifth and 10th years of the child's life. Considering 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 driven by maternal full-time employment and income effects, the coefficients of 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 in communities with nurseries, controlling for family size might alter the estimated effects.

It turns out that 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 10th years of the child's life (see the first panel of Table 8).[31] Moreover, none of the positive coefficients in communities with nurseries becomes larger or statistically significant. Most negative coefficients become even larger in absolute terms. In communities without nurseries, the coefficients of PL do not change either.

Turning to the second panel of Table 8, we see that the estimated effects are also robust to the inclusion of maternal employment in the second, third, fifth, and 10th years of the child's life. 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 the 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 finding 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 mediators of the PL reform. The estimated treatment effects on children most likely have other origins, namely, PL 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 from our baseline model and all the robustness tests, showing the treatment effect heterogeneity according to the availability of nurseries across all estimation models.

[Figure 10]

6 Conclusions

This paper provides a novel interpretation of the estimated treatment effects from evaluations of PL reforms. We show that accounting for the counterfactual mode of care is decisive. In our evaluation of a large PL extension in Austria, the estimated treatment effects on child outcomes differ substantially according to the availability of formal childcare and the mother's counterfactual work behavior. Both factors determine the counterfactual mode of care.

We find that in communities without formal childcare, children have significantly better outcomes after the PL extension. This treatment effect of the reform is particularly strong for

[31] 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.

those children whose mothers would have been working in the counterfactual situation with short PL. These results strongly suggest that children benefit from the switch from informal care provided by grandparents or other relatives (i.e., the counterfactual mode of care) to maternal care. We conclude that informal care arrangements do not provide the same fruitful environment for children in their second year of life. This finding is in line with existing evidence showing that childcare stability (e.g., the number of different care arrangements over time, daily stability, including predictable routines and structures) is important for child development (Morrissey, 2009). Another potential channel is that children with impairments are less likely to receive (optimal) early intervention under informal care arrangements. Grandparents — the most important providers of informal care — are on average less educated than mothers, may be unable to identify the need for intervention, or hold more traditional and less beneficial views about childrearing.

By contrast, for children in communities with formal childcare, we find mostly zero (and some negative) treatment effects of the PL extension. Here, the counterfactual mode of care is not uniquely defined, but typically a nursery. Thus, with comparably less power, we conclude that the switch from formal care to maternal care has no robust effects on long-term child outcomes. This finding is in line with the literature on formal childcare, which finds zero (or positive) effects of formal childcare and mostly negative effects of informal childcare compared with maternal care.

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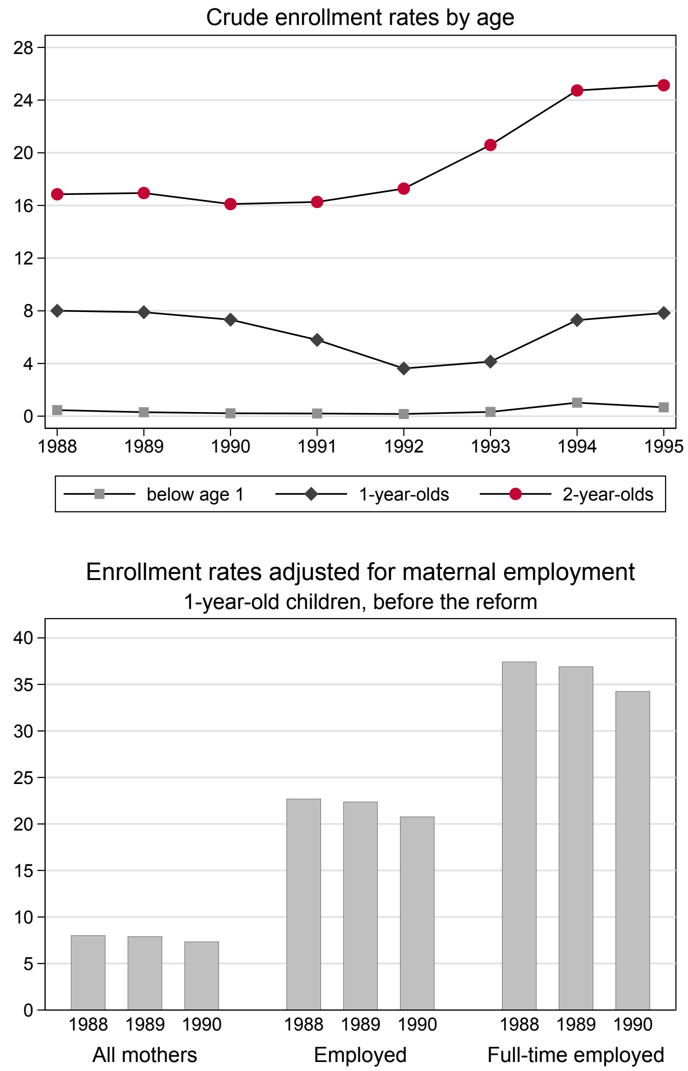
7 Figures (to be placed in the paper)

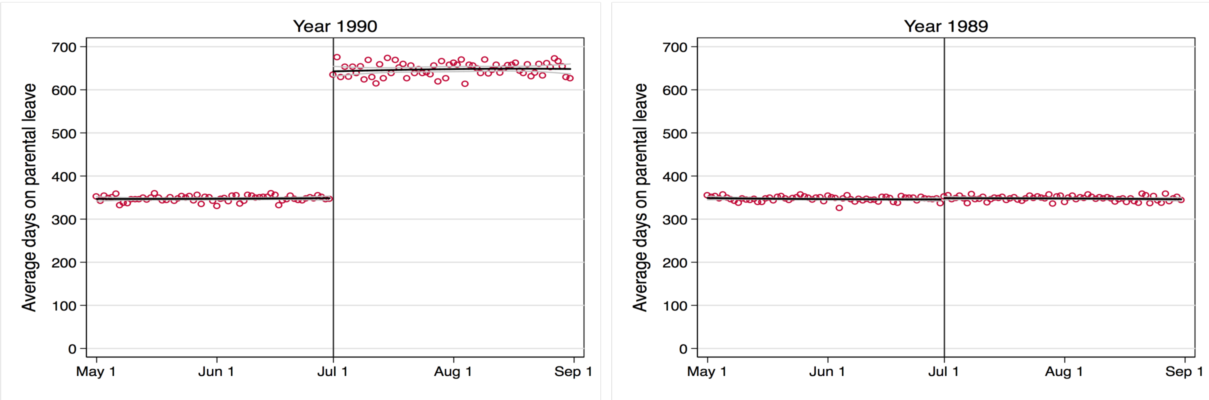
Figure 1: Enrollment rates in formal childcare



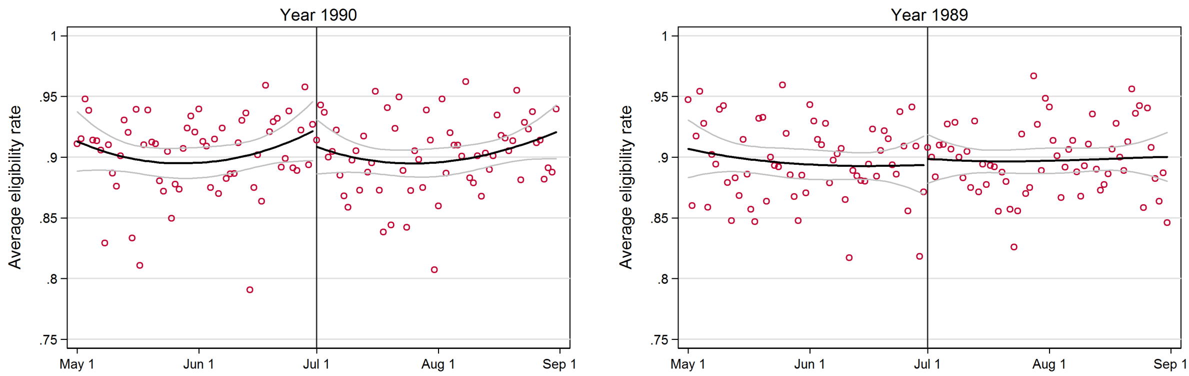
Notes: Own calculations based on data from *Kindertagesheimstatistik* (Statistics Austria, Statcube, retrieved on November 17, 2016) and the *Austrian Birth Register*. All figures refer to communities with a nursery. Crude enrollment rates are calculated as the ratio between the number of enrolled children 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 calender 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 adjusted for 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: PL duration, eligibility and takeup rate

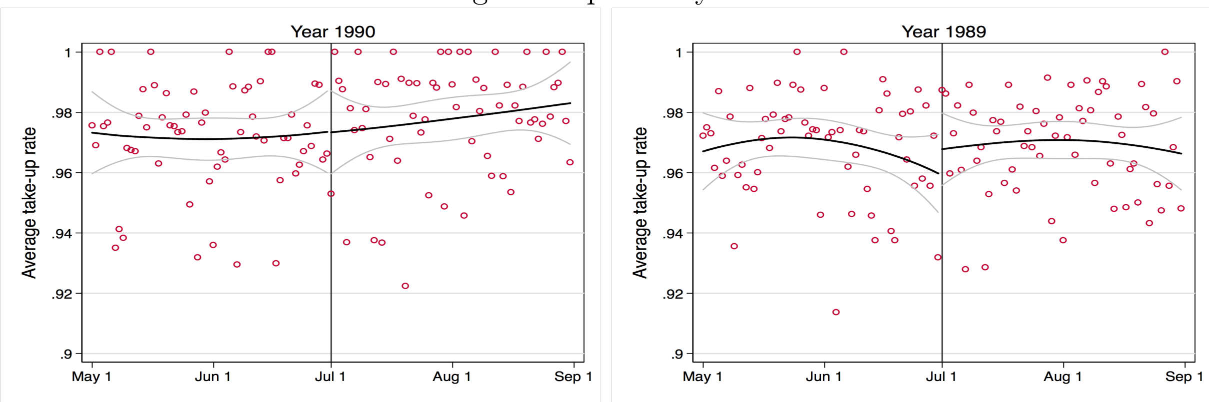
Panel A: Average PL duration by date of birth



Panel B: Average eligibility rate by date of birth

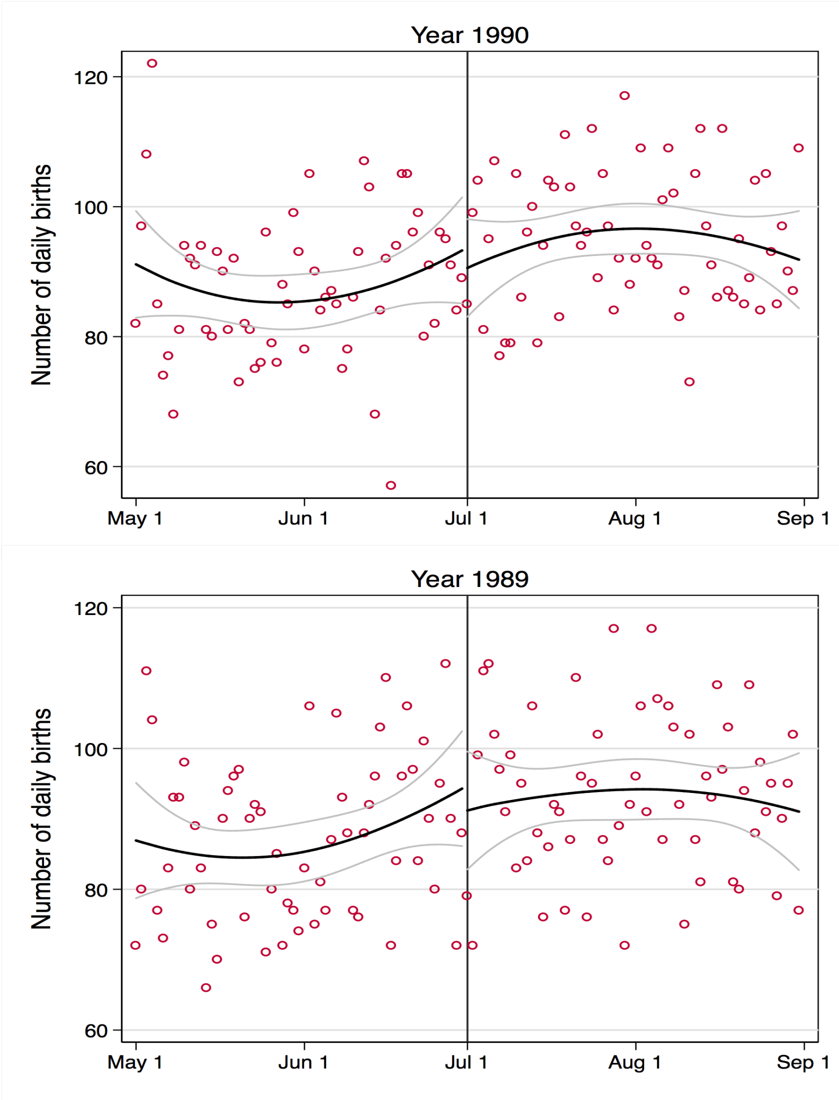


Panel C: Average takeup rate by date of birth



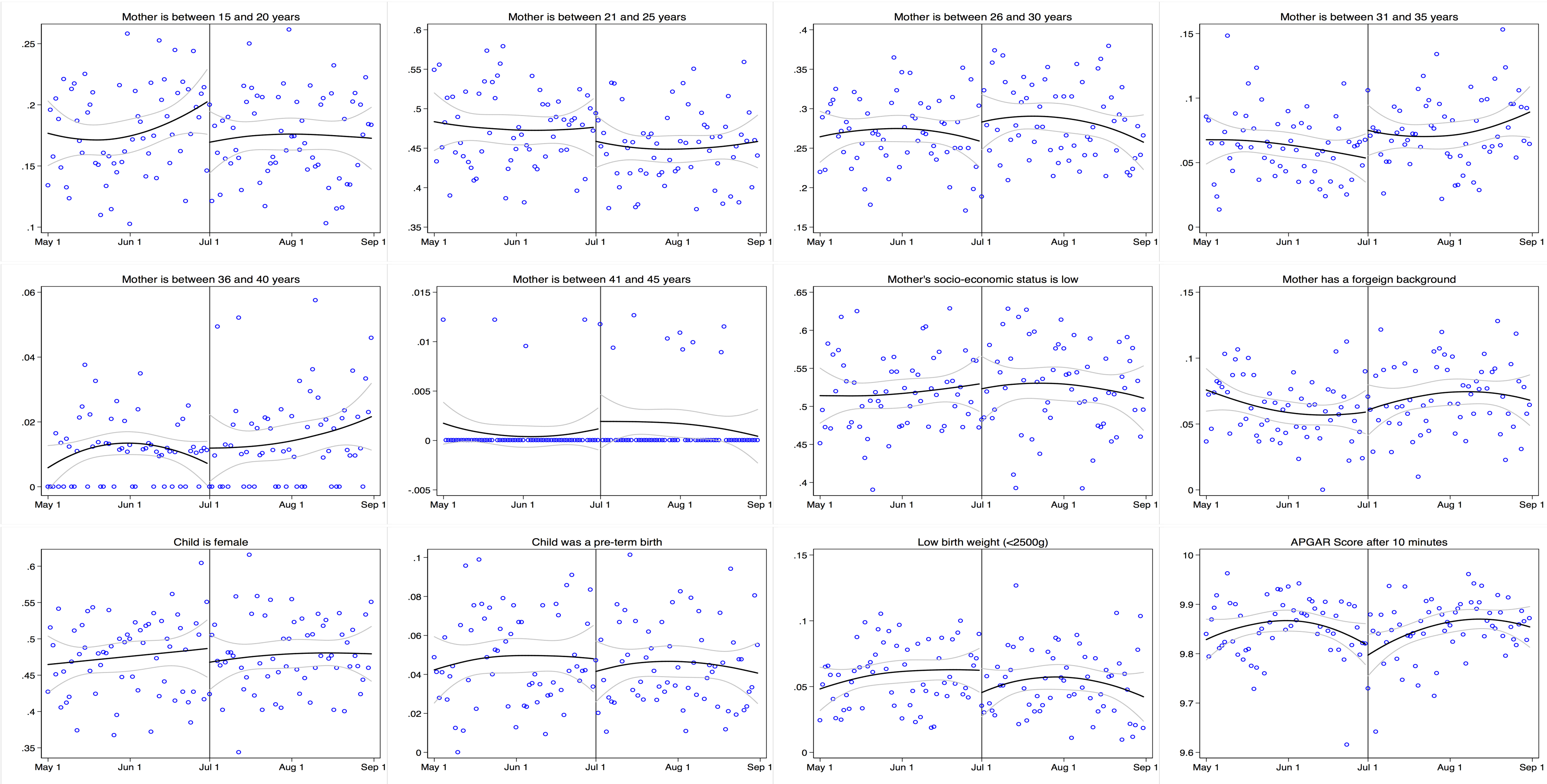
Notes: These figures show daily averages (by birthdate) of three different variables for the period from May to September in the year of the reform (1990), and in the year before the reform (1989) with a second degree polynomial fit. Panels A depicts the average days on PL. This resembles our first-stage relationship. Panels B depicts the average eligibility rate. Panels C depicts the average takeup rate.

Figure 3: Density of the assignment variable (number of of daily births)



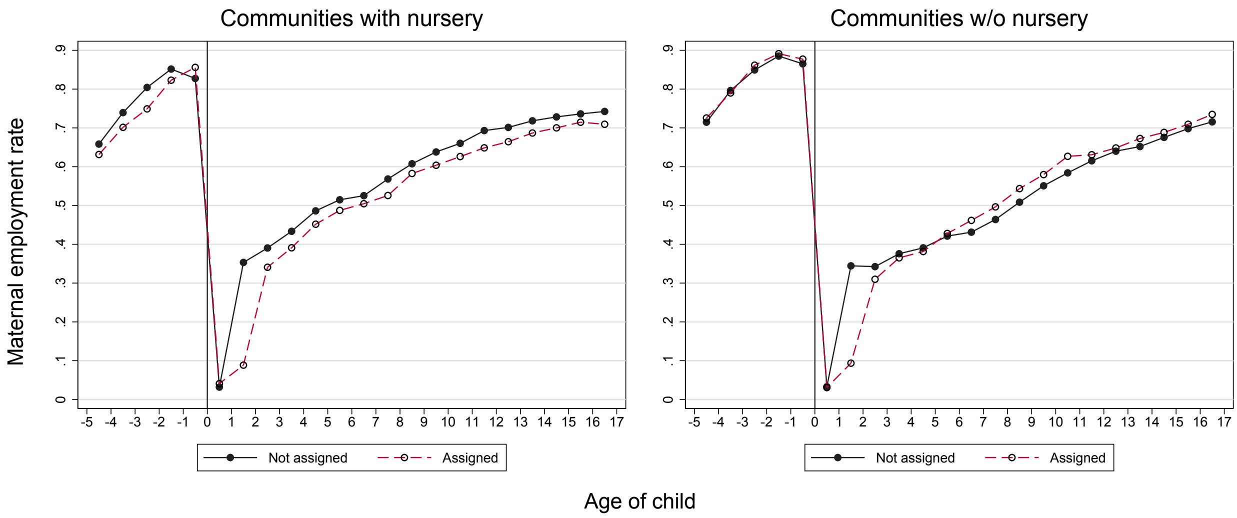
Notes: These figures show the number of daily births in the period from May to September in the year of the reform (upper Panel), and in the year before the reform (lower Panel) with a second degree polynomial fit. The figures shows no evidence of discontinuity at the cutoff birthday date on July 1.

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



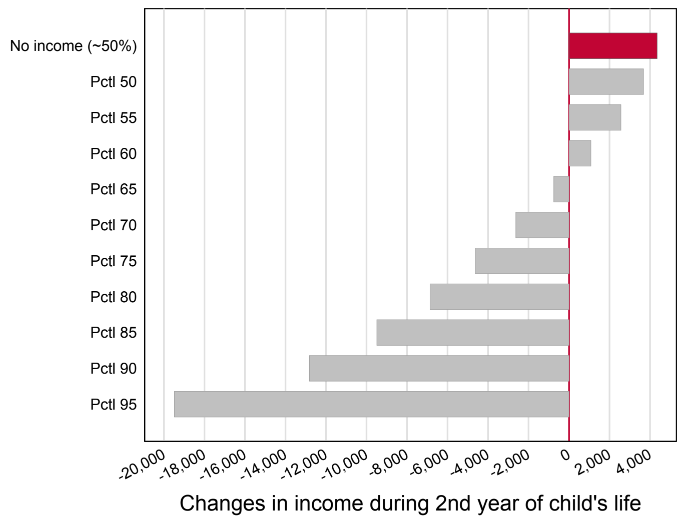
Notes: These figures show the daily averages of all covariates and a number of other pre-determined variables in the period from May to September 1990. There is no indication of a discontinuity at the at the cutoff birthday date on July 1.

Figure 5: Return to work



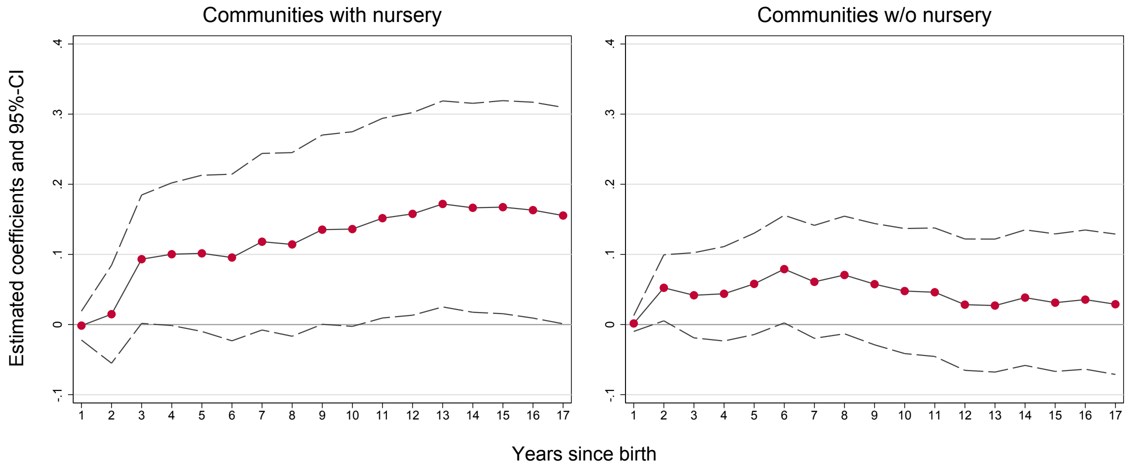
Notes: Employment rates are calculated for our sample of PL eligible first time mothers whose children were born in June/July 1989/1990 (excluding ±5 days around the cutoff). Employment is measured on January 1st in each year and shown separately for not assigned (pre-reform) and assigned (post-reform) mothers in communities with and w/o nursery.

Figure 6: Income



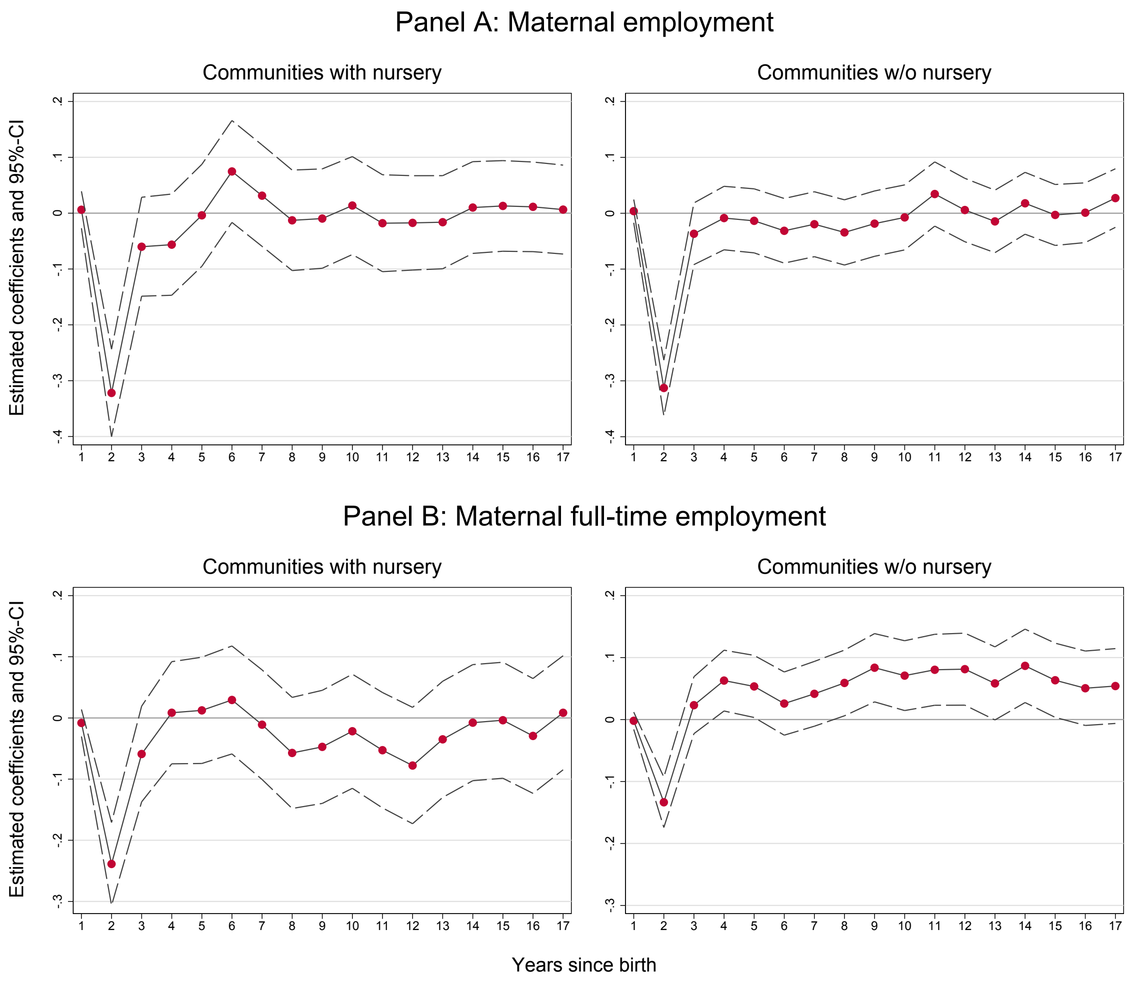
Notes: Mean hypothetical income changes due to the reform are shown for mothers at different points in the income distribution. After the reform most mothers did not work in the second year of the child's life, losing labor income, but received PL benefits. These income changes are approximated as follows: using our sample of PL eligible first time pre-reform mothers, we measure real net earnings in the second year of the child's life (gross earnings minus social insurance contributions minus income taxes) and subtract PL benefits.

Figure 7: Family size up to 17 years after childbirth



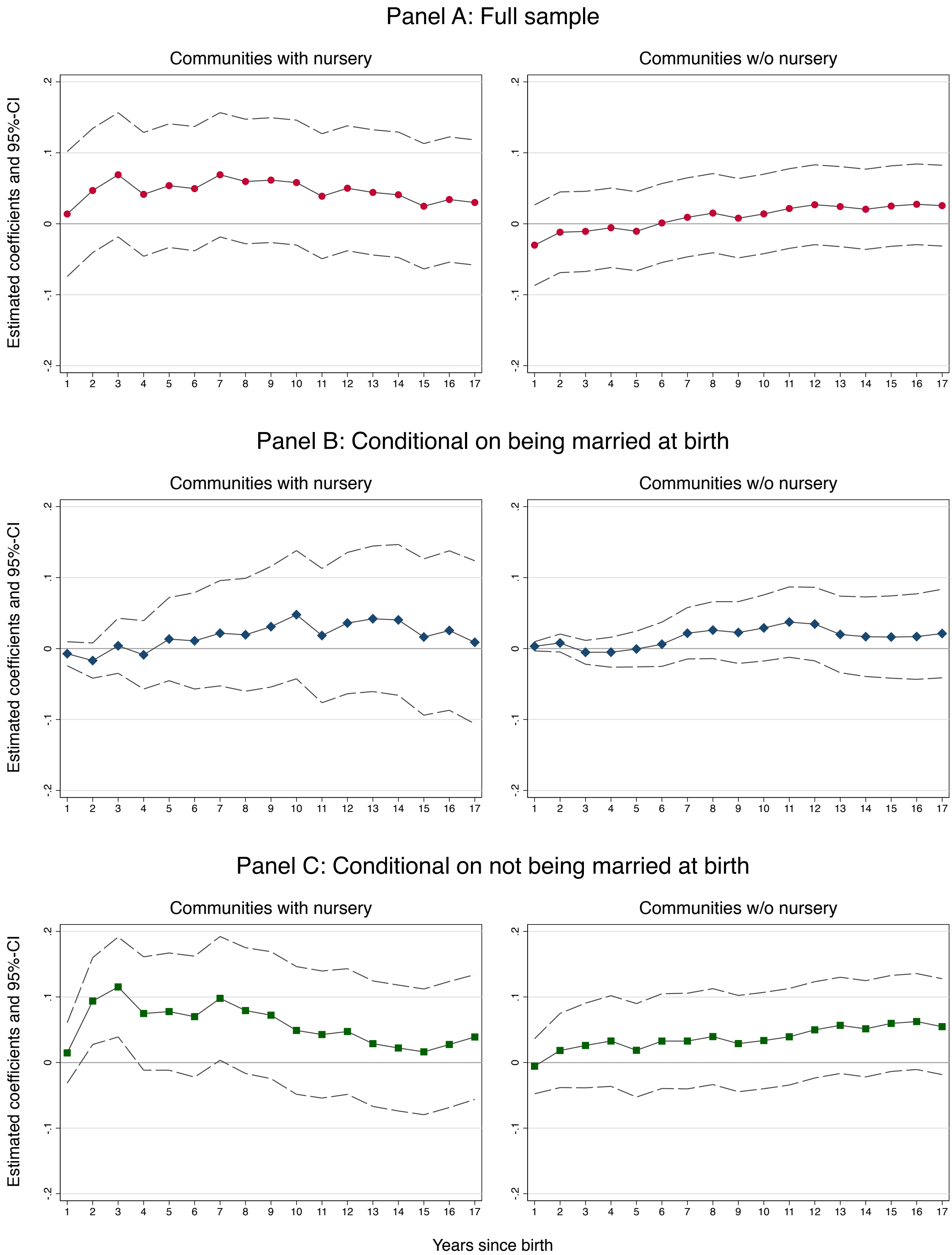
Notes: This figure shows the estimated coefficients and 95% confidence intervals from separate 2SLS regressions with years on PL instrumented by the assignment to the reform. Family size is measured as the number of children at the first child's birthday in each year. See Table A.5 in the Appendix for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

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



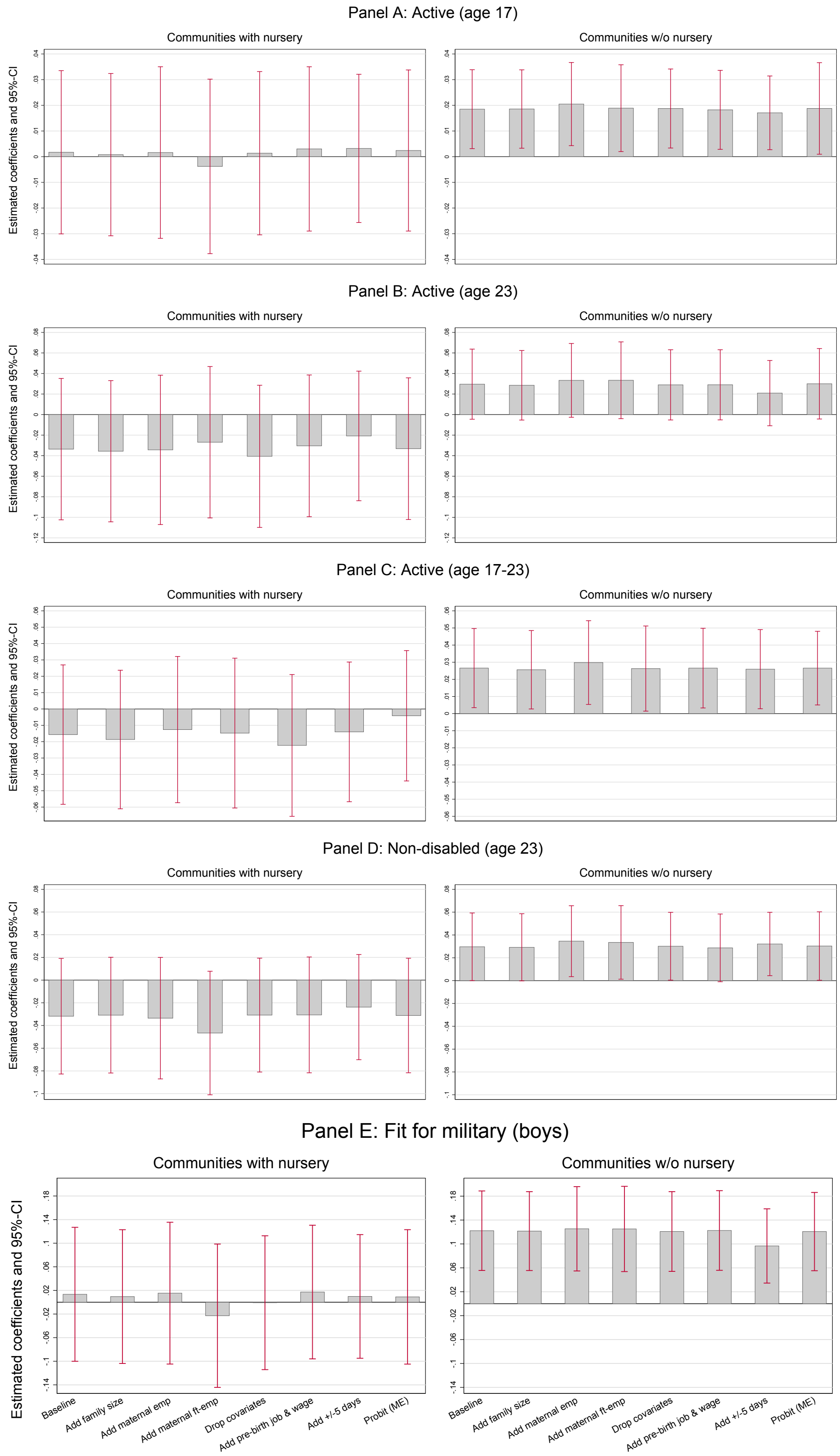
Notes: These figures show the estimated coefficients and 95% confidence intervals from separate 2SLS regressions with years on PL instrumented by the assignment to the reform. Maternal labor supply is measured as the probability to be employed (Panel A) and the probability to be employed full-time (Panel 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 PL 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: PL 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. *Data*: survey data (NLSCY)	Small and mostly insignificant results. *Heterogeneity*: 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. *Data*: survey data (NLSCY)	No significant positive effects. Small negative effects on vocabulary scores. *Heterogeneity*: Same across subgroups (gender, parental education).	see Baker and Milligan (2010)
Carneiro, Løcken and Salvanes (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 *Data*: Administrative data	Significant positive effects: Reduced drop-out rates and increased earnings, college attendance, completed years of schooling and IQ (males) *Heterogeneity*: 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 Salvanes (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 *Data*: Administrative data	No significant effects. *Heterogeneity*: 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. *Data*: PISA	No significant average effects. *Heterogeneity*: 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) *Data*: Administrative data	No or extremely small effects. Expansion from 18 to 36 months slightly negative effects. *Heterogeneity*: 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 *Data*: Administrative data	No significant effects. *Heterogeneity*: 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 *Data*: Administrative data, PISA	No significant effects *Heterogeneity*: Same across subgroups (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 - PL; PISA - Programme for International Student Assessment.

Table 2: Availability of kindergardens and nurseries, 1988–1995

Year	1988	1989	1990	1991	1992	1993	1994	1995
Kindergarden available:								
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
Nursery available:								
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[a]	N	All Mean	Communities **with** nursery Mean	Communities **w/o** nursery Mean
Child:						
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[b]), on maternity/PL 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 (age 23)	Binary indicator equal to one if child is *not* disabled until age 23.	Ministry	8,495	0.935	0.925	0.940
Capable of work (age 23)	Binary indicator equal to one if child is *not* unable to work due to disability until age 23.	Ministry	8,495	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
Mother:						
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
Family:						
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 ($t = 10$)[c]	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: [a]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*. [b]This type of employment contract is for jobs with a low number of working hours and low pay and covers only accident insurance. [c]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 Mean	N	Sample of post-reform mothers Mean	N	Diff.	P-value
Covariates:						
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
Other pre-determined variables:						
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 ±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 Δ^a
Education outcomes (ITT)^b				
Test score math (age 15/16)	13.168	−11.833	21.712*	0.179
	(10.880)	(21.499)	(12.536)	
Test score science (age 15/16)	11.487	−27.516	23.435*	0.032
	(11.274)	(20.280)	(12.821)	
Test score reading (age 15/16)	9.806	−39.839*	27.161**	0.005
	(11.159)	(20.338)	(12.636)	
High track grade 9 (age 14/15)	0.022	0.027	0.012	0.891
	(0.048)	(0.095)	(0.056)	
High track grade 5 (age 10/11)^c		−0.311***		
		(0.087)		
High track grade 8 (age 13/14)^c		−0.213**		
		(0.091)		
Labor market outcomes (LATE)				
Active (age 17)	0.014*	0.002	0.019**	0.351
	(0.007)	(0.016)	(0.008)	
In education (age 17)	0.013	−0.004	0.019**	0.235
	(0.008)	(0.018)	(0.009)	
Active (age 23)	0.011	−0.034	0.030*	0.107
	(0.016)	(0.035)	(0.017)	
In education (age 23)	−0.001	−0.011	0.002	0.789
	(0.022)	(0.044)	(0.026)	
Employed (age 23)	0.003	−0.028	0.017	0.431
	(0.025)	(0.049)	(0.029)	
Log wage (age 23)	−0.012	−0.032	−0.005	0.656
	(0.025)	(0.053)	(0.027)	
Active (age 17-23)	0.014	−0.016	0.027**	0.088
	(0.011)	(0.022)	(0.012)	
Always active (age 17-23)	−0.009	−0.096**	0.028	0.026
	(0.026)	(0.047)	(0.031)	
Health outcomes (LATE)				
Non-disabled (age 23)	0.011	−0.032	0.030**	0.041
	(0.013)	(0.026)	(0.015)	
Capable of work (age 23)	0.017***	−0.003	0.025***	0.061
	(0.006)	(0.013)	(0.007)	
Fit for military (boys)	0.092***	0.013	0.122***	0.105
	(0.029)	(0.058)	(0.034)	

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 ±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 PL 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. [a]Prob>F(chi2) of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. [b]In the PISA sample, we stratify the sample by school location and assume that communities with ≥100,000 inhabitants had a nursery. [c] In the EducReg Linz sample we do not control for whether the mother was born abroad.

Table 6: Child outcomes by predicted maternal propensity of being employed in second year[a]

	Low work propensity			**High work propensity**		
	Communities **with** nursery	Communities **w/o** nursery	P-value Δ^b	Communities **with** nursery	Communities **w/o** nursery	P-value Δ^b
% in Sample	16.52	33.49		13.10	36.90	
Active (age 17)	0.015	0.004	0.687	−0.010	0.037***	0.052
	(0.025)	(0.011)		(0.021)	(0.012)	
Active (age 23)	0.000	0.003	0.971	−0.063	0.065**	0.021
	(0.051)	(0.023)		(0.049)	(0.027)	
Active (age 17-23)	−0.038	0.004	0.248	0.009	0.055***	0.179
	(0.033)	(0.016)		(0.029)	(0.017)	
Non-disabled (age 23)	−0.002	0.006	0.865	−0.053	0.059***	0.006
	(0.040)	(0.020)		(0.034)	(0.023)	
Fit for military (boys)	0.072	0.079*	0.937	−0.044	0.177***	0.023
	(0.082)	(0.046)		(0.083)	(0.051)	

Notes: Each coefficient represents a separate 2SLS regression, with years on PL 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 ±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. [a]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). [b]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

	Population density[b]			Share of children[c]			Share of catholics[d]			Female employment[e]		
	Low	High		Low	High		Low	High		Low	High	
Community type[a]	Typical	**Atypical**	P-value Δ	**Atypical**	Typical	P-value Δ	**Atypical**	Typical	P-value Δ	Typical	**Atypical**	P-value Δ
All mothers												
% in Sample	70.59	29.41		29.6	70.38		30.13	69.87		67.32	32.68	
Active (age 17)	0.018**	0.021	0.890	0.056***	0.006	0.017	0.025	0.016*	0.617	0.017*	0.024	0.692
	(0.009)	(0.015)		(0.019)	(0.008)		(0.015)	(0.009)		(0.009)	(0.015)	
Active (age 23)	0.039*	0.006	0.428	0.013	0.037*	0.574	0.023	0.034*	0.764	0.039*	0.010	0.446
	(0.020)	(0.035)		(0.037)	(0.019)		(0.034)	(0.020)		(0.021)	(0.032)	
Active (age 17-23)	0.023*	0.036	0.650	0.086***	0.005	0.006	0.055**	0.015	0.135	0.014	0.052**	0.149
	(0.014)	(0.024)		(0.026)	(0.013)		(0.023)	(0.014)		(0.014)	(0.022)	
Non-disabled (age 23)	0.024	0.046*	0.502	0.062*	0.019	0.249	0.048*	0.022	0.429	0.027	0.038	0.726
	(0.018)	(0.028)		(0.033)	(0.017)		(0.027)	(0.018)		(0.018)	(0.029)	
Fit for military (boys)	0.132***	0.115*	0.822	0.124*	0.125***	0.985	0.166***	0.107***	0.435	0.122***	0.126**	0.953
	(0.040)	(0.065)		(0.065)	(0.040)		(0.064)	(0.040)		(0.041)	(0.061)	
High work propensity[f]												
% in Sample	66.64	33.36		36.39	63.61		35.67	64.33		61.67	38.33	
Active (age 17)	0.031**	0.051***	0.394	0.084***	0.014	0.015	0.037*	0.038***	0.969	0.041***	0.029	0.639
	(0.015)	(0.020)		(0.026)	(0.012)		(0.021)	(0.014)		(0.013)	(0.023)	
Active (age 23)	0.080**	0.031	0.393	0.037	0.082***	0.431	0.056	0.074**	0.751	0.070**	0.057	0.818
	(0.032)	(0.048)		(0.047)	(0.032)		(0.044)	(0.033)		(0.032)	(0.046)	
Active (age 17-23)	0.059***	0.048	0.771	0.113***	0.024	0.023	0.079***	0.044**	0.351	0.041*	0.075**	0.366
	(0.021)	(0.031)		(0.033)	(0.020)		(0.030)	(0.021)		(0.021)	(0.030)	
Non-disabled (age 23)	0.056*	0.068*	0.802	0.102**	0.036	0.194	0.041	0.071**	0.516	0.066**	0.047	0.688
	(0.028)	(0.039)		(0.043)	(0.027)		(0.034)	(0.031)		(0.028)	(0.041)	
Fit for military (boys)	0.205***	0.147*	0.578	0.161*	0.188***	0.798	0.198**	0.172***	0.806	0.150**	0.226***	0.466
	(0.064)	(0.083)		(0.084)	(0.064)		(0.083)	(0.064)		(0.064)	(0.083)	
Low work propensity[f]												
% in Sample	74.16	25.84		23.48	76.52		25.11	74.89		72.44	27.56	
Active (age 17)	0.008	−0.008	0.530	0.018	0.000	0.560	0.013	0.001	0.628	−0.002	0.017	0.436
	(0.012)	(0.022)		(0.028)	(0.011)		(0.023)	(0.012)		(0.013)	(0.020)	
Active (age 23)	0.010	−0.012	0.709	−0.014	0.009	0.723	−0.014	0.010	0.677	0.018	−0.049	0.206
	(0.026)	(0.053)		(0.060)	(0.024)		(0.053)	(0.025)		(0.027)	(0.046)	
Active (age 17-23)	−0.005	0.036	0.305	0.049	−0.006	0.206	0.032	−0.005	0.362	−0.006	0.025	0.403
	(0.018)	(0.036)		(0.040)	(0.017)		(0.036)	(0.018)		(0.018)	(0.032)	
Non-disabled (age 23)	−0.001	0.019	0.677	0.008	0.007	0.985	0.058	−0.010	0.162	−0.001	0.023	0.602
	(0.023)	(0.040)		(0.051)	(0.021)		(0.044)	(0.023)		(0.023)	(0.039)	
Fit for military (boys)	0.085*	0.095	0.934	0.059	0.085*	0.815	0.130	0.066	0.562	0.106**	0.007	0.340
	(0.051)	(0.103)		(0.102)	(0.051)		(0.099)	(0.051)		(0.053)	(0.089)	

Notes: Each coefficient represents a separate 2SLS regression, with years on PL 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 ±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. [a]Communities are stratified based on the median of the respective community characteristic in the overall sample of communities with and w/o nurseries. [b]The mother lived in a community with a low/high number of inhabitants per square-kilometer, low/high according to median (0.946). [c]The mother lived in a community with a low/high share of children aged 0-15, low/high according to median (0.180). [d]The mother lived in a community with a low/high share of catholics, low/high according to median (0.926). [e]The mother lived in a community with a low/high female employment rate, low/high according to median (0.528). [f]Maternal characteristics indicate a high/low propensity of being employed >0 days in the second year after childbirth.

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

	All communities	Communities **with** nursery	Communities **w/o** nursery
Adding family size[a]			
Active (age 17)	0.014*	0.001	0.019**
	(0.007)	(0.016)	(0.008)
Active (age 23)	0.010	−0.036	0.029*
	(0.016)	(0.035)	(0.017)
Active (age 17-23)	0.012	−0.019	0.026**
	(0.010)	(0.022)	(0.012)
Non-disabled (age 23)	0.011	−0.031	0.029*
	(0.013)	(0.026)	(0.015)
Fit for military (boys)	0.090***	0.010	0.122***
	(0.029)	(0.058)	(0.034)
Adding maternal employment[b]			
Active (age 17)	0.016**	0.002	0.020**
	(0.008)	(0.017)	(0.008)
Active (age 23)	0.014	−0.034	0.033*
	(0.017)	(0.037)	(0.018)
Active (age 17-23)	0.017	−0.013	0.030**
	(0.011)	(0.023)	(0.012)
Non-disabled (age 23)	0.015	−0.034	0.035**
	(0.014)	(0.027)	(0.016)
Fit for military (boys)	0.096***	0.015	0.125***
	(0.031)	(0.061)	(0.036)
Adding maternal full-time employment[c]			
Active (age 17)	0.013*	−0.004	0.019**
	(0.008)	(0.017)	(0.009)
Active (age 23)	0.017	−0.027	0.033*
	(0.017)	(0.038)	(0.019)
Active (age 17-23)	0.015	−0.015	0.026**
	(0.011)	(0.023)	(0.013)
Non-disabled (age 23)	0.010	−0.047*	0.033**
	(0.014)	(0.028)	(0.016)
Fit for military (boys)	0.084***	−0.023	0.125***
	(0.031)	(0.062)	(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 ±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. [a]Additional control variables: family size 2, 3, 5, and 10 years after birth. [b]Additional control variables: binary indicators for maternal employment 2, 3, 5, and 10 years after birth. [c]Additional control variables: binary indicators for maternal full-time employment 2, 3, 5, and 10 years after birth.

Web Appendix

This Web Appendix (not for publication) provides additional material discussed in the unpublished manuscript '*Parental Leave, (In)formal Childcare and Long-term Child Outcomes*' by Natalia Danzer, Martin Halla, Nicole Schneeweis, and Martina Zweimüller.

A.1 Tables

Table A.1: Robustness of child outcomes

	All communities	Communities **with** nursery	Communities **w/o** nursery
No covariates[a]			
Test score math (age 15/16)	8.646	−25.090	19.500
	(11.438)	(23.219)	(13.228)
Test score science (age 15/16)	8.540	−39.560*	22.720*
	(11.865)	(21.371)	(13.352)
Test score reading (age 15/16)	8.180	−50.207**	27.403**
	(12.012)	(21.786)	(13.447)
High track grade 9 (age 14/15)	0.019	0.000	0.012
	(0.051)	(0.097)	(0.061)
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*	0.001	0.019**
	(0.007)	(0.016)	(0.008)
Active (age 23)	0.008	−0.041	0.029*
	(0.016)	(0.035)	(0.017)
Active (age 17-23)	0.011	−0.022	0.027**
	(0.011)	(0.022)	(0.012)
Non-disabled (age 23)	0.011	−0.031	0.030**
	(0.013)	(0.026)	(0.015)
Fit for military (boys)	0.086***	−0.001	0.121***
	(0.029)	(0.058)	(0.034)
Controls for maternal pre-birth job and wage[b]			
Active (age 17)	0.014*	0.003	0.018**
	(0.007)	(0.016)	(0.008)
Active (age 23)	0.010	−0.030	0.029*
	(0.016)	(0.035)	(0.017)
Active (age 17-23)	0.014	−0.014	0.026**
	(0.011)	(0.022)	(0.012)
Non-disabled (age 23)	0.011	−0.031	0.029*
	(0.013)	(0.026)	(0.015)
Fit for military (boys)	0.093***	0.017	0.123***
	(0.029)	(0.058)	(0.034)
Including children born ±5 days around cutoff date			
Active (age 17)	0.013**	0.003	0.017**
	(0.007)	(0.015)	(0.007)
Active (age 23)	0.008	−0.021	0.021
	(0.015)	(0.032)	(0.016)
Active (age 17-23)	0.017*	−0.004	0.027**
	(0.010)	(0.020)	(0.011)
Non-disabled (age 23)	0.016	−0.024	0.032**
	(0.012)	(0.024)	(0.014)
Fit for military (boys)	0.071***	0.010	0.097***
	(0.027)	(0.053)	(0.032)
Probit models for binary outcomes (Marginal effects)			
High track grade 9 (age 14/15)	0.019	0.025	0.010
	(0.048)	(0.092)	(0.056)
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*	0.002	0.019**
	(0.008)	(0.016)	(0.009)
Active (age 23)	0.011	−0.033	0.030*
	(0.016)	(0.035)	(0.017)
Non-disabled (age 23)	0.012	−0.031	0.030**
	(0.013)	(0.026)	(0.015)
Fit for military (boys)	0.090***	0.009	0.121***
	(0.029)	(0.058)	(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 ±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 PL 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. [a]No covariates included other than month and year-of-birth. [b]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[a]			Gender		
	Low	High	P-value Δ^b	Girls	Boys	P-value Δ^c
Education outcomes (ITT)						
Test score math (age 15/16)	5.423	24.734	0.402	3.431	24.033	0.341
	(14.942)	(16.440)		(15.659)	(15.005)	
Test score science (age 15/16)	−1.749	30.545*	0.115	5.341	18.348	0.543
	(14.508)	(15.660)		(15.552)	(15.468)	
Test score reading (age 15/16)	−6.784	32.436*	0.087	−0.484	21.729	0.308
	(15.091)	(16.704)		(15.394)	(15.889)	
High track grade 9 (age 14/15)	0.066	−0.055	0.262	−0.020	0.073	0.368
	(0.066)	(0.079)		(0.069)	(0.071)	
Labor market outcomes (LATE)						
Active (age 17)	0.012	0.016	0.808	0.010	0.019*	0.536
	(0.010)	(0.010)		(0.010)	(0.011)	
In education (age 17)	0.014	0.012	0.916	0.003	0.022**	0.245
	(0.012)	(0.011)		(0.012)	(0.011)	
Active (age 23)	0.010	0.010	0.998	0.021	0.002	0.558
	(0.022)	(0.023)		(0.023)	(0.023)	
In education (age 23)	−0.019	0.021	0.382	0.028	−0.027	0.222
	(0.027)	(0.037)		(0.033)	(0.030)	
Employed (age 23)	0.027	−0.031	0.258	−0.018	0.019	0.474
	(0.033)	(0.039)		(0.038)	(0.034)	
Log wage (age 23)	0.008	−0.041	0.325	−0.061*	0.032	0.057
	(0.032)	(0.039)		(0.036)	(0.033)	
Active (age 17-23)	0.016	0.010	0.783	0.013	0.015	0.910
	(0.015)	(0.015)		(0.015)	(0.015)	
Always active (age 17-23)	−0.033	0.020	0.303	0.001	−0.016	0.750
	(0.034)	(0.039)		(0.037)	(0.035)	
Health outcomes (LATE)						
Non-disabled (age 23)	0.018	0.003	0.548	0.011	0.013	0.949
	(0.018)	(0.019)		(0.018)	(0.019)	
Capable of work (age 23)	0.014	0.021**	0.621	0.018**	0.017*	0.995
	(0.009)	(0.008)		(0.008)	(0.010)	
Fit for military (boys)	0.128***	0.047	0.170		0.092***	
	(0.040)	(0.043)			(0.029)	

Notes: Each coefficient represents a separate regression based on data from *PISA* (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 ±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 PL 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. [a]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). [b]Prob>F(chi2) of difference in coefficients between children of mothers with low/high socio-economic status based on fully interacted regressions. [c]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[a] Low	Employed[a] High	Full-time employed[b] Low	Full-time employed[b] 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. [a]Propensity of the mother of being employed >0 days in the second year of the child's life. [b]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 Δ^a	Communities **with** nursery	Communities **w/o** nursery	P-value Δ^a
Employed during entire second year[b]						
% in Sample	15.30	34.70		14.31	35.68	
Active (age 17)	0.027	0.005	0.400	−0.023	0.034***	0.021
	(0.024)	(0.010)		(0.022)	(0.012)	
Active (age 23)	0.022	0.027	0.919	−0.086*	0.032	0.032
	(0.051)	(0.024)		(0.049)	(0.025)	
Active (age 17-23)	−0.003	0.022	0.501	−0.026	0.033*	0.089
	(0.032)	(0.016)		(0.030)	(0.017)	
Non-disabled (age 23)	0.003	0.011	0.859	−0.065*	0.051**	0.004
	(0.039)	(0.021)		(0.034)	(0.021)	
Fit for military (boys)	−0.024	0.124***	0.115	0.054	0.122**	0.473
	(0.082)	(0.047)		(0.082)	(0.048)	
Earns ≥ 50% of pre-birth wage[c]						
% in Sample	16.49	33.52		13.13	36.87	
Active (age 17)	0.029	0.008	0.453	−0.023	0.031***	0.021
	(0.026)	(0.011)		(0.020)	(0.011)	
Active (age 23)	−0.029	0.022	0.372	−0.046	0.037	0.129
	(0.052)	(0.023)		(0.048)	(0.026)	
Active (age 17-23)	−0.025	0.017	0.251	−0.008	0.037**	0.172
	(0.033)	(0.016)		(0.029)	(0.017)	
Non-disabled (age 23)	0.008	0.017	0.727	−0.016	0.028***	0.010
	(0.021)	(0.011)		(0.015)	(0.009)	
Fit for military (boys)	−0.031	0.097**	0.183	0.033	0.148***	0.222
	(0.085)	(0.047)		(0.080)	(0.049)	
Earns ≥ 75% of pre-birth wage[d]						
% in Sample	18.11	31.89		11.50	38.49	
Active (age 17)	0.011	0.010	0.973	−0.006	0.028**	0.147
	(0.027)	(0.011)		(0.021)	(0.011)	
Active (age 23)	−0.007	0.027	0.564	−0.049	0.034	0.122
	(0.056)	(0.022)		(0.046)	(0.028)	
Active (age 17-23)	−0.016	0.018	0.369	−0.014	0.037**	0.123
	(0.034)	(0.016)		(0.028)	(0.018)	
Non-disabled (age 23)	0.003	0.021**	0.469	−0.008	0.024**	0.077
	(0.022)	(0.010)		(0.016)	(0.009)	
Fit for military (boys)	−0.060	0.113**	0.089	0.050	0.132***	0.364
	(0.091)	(0.046)		(0.075)	(0.050)	

Notes: Each coefficient represents a separate 2SLS regression, with years on PL 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 ±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. [a]Prob>F(chi2) of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. [b]Maternal characteristics indicate a low/high propensity of being employed ≥360 days in second year after childbirth, low/high according to median prediction (0.19). [c]Maternal characteristics indicate a low/high propensity of being full-time employed (with ≥75% of pre-birth earnings) in January following the child's first birthday, low/high according to median prediction (0.18). [d]Maternal characteristics indicate a low/high propensity of being employed with ≥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 Δ^a
Number of children^b				
1 year after birth	0.001	−0.002	0.002	0.799
	(0.005)	(0.010)	(0.006)	
2 years after birth	0.042**	0.015	0.052**	0.381
	(0.020)	(0.036)	(0.024)	
3 years after birth	0.058**	0.093**	0.042	0.358
	(0.026)	(0.047)	(0.031)	
4 years after birth	0.062**	0.100*	0.044	0.364
	(0.029)	(0.052)	(0.034)	
5 years after birth	0.072**	0.102*	0.058	0.520
	(0.031)	(0.057)	(0.037)	
6 years after birth	0.086***	0.096	0.079**	0.819
	(0.033)	(0.061)	(0.039)	
7 years after birth	0.080**	0.118*	0.061	0.453
	(0.035)	(0.064)	(0.041)	
8 years after birth	0.086**	0.114*	0.071*	0.583
	(0.036)	(0.067)	(0.043)	
9 years after birth	0.082**	0.135**	0.058	0.342
	(0.037)	(0.069)	(0.044)	
10 years after birth	0.076**	0.136*	0.048	0.294
	(0.038)	(0.071)	(0.045)	
11 years after birth	0.079**	0.152**	0.046	0.221
	(0.039)	(0.073)	(0.047)	
12 years after birth	0.068*	0.158**	0.028	0.141
	(0.040)	(0.074)	(0.048)	
13 years after birth	0.072*	0.172**	0.027	0.104
	(0.041)	(0.075)	(0.048)	
14 years after birth	0.078*	0.166**	0.038	0.157
	(0.042)	(0.076)	(0.049)	
15 years after birth	0.073*	0.167**	0.031	0.140
	(0.042)	(0.077)	(0.050)	
16 years after birth	0.075*	0.163**	0.036	0.172
	(0.043)	(0.079)	(0.051)	
17 years after birth	0.068	0.155**	0.029	0.178
	(0.043)	(0.079)	(0.051)	

Notes: Each coefficient represents a separate 2SLS regression with years on PL 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 ±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. ^{a}Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. ^{b}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 Δ^a
Mother is employed[b]				
1 year after birth	0.004	0.006	0.004	0.904
	(0.009)	(0.017)	(0.011)	
2 years after birth	−0.316***	−0.322***	−0.313***	0.846
	(0.021)	(0.040)	(0.025)	
3 years after birth	−0.045*	−0.060	−0.037	0.661
	(0.024)	(0.045)	(0.028)	
4 years after birth	−0.026	−0.056	−0.009	0.382
	(0.025)	(0.046)	(0.029)	
5 years after birth	−0.012	−0.004	−0.014	0.859
	(0.025)	(0.047)	(0.029)	
6 years after birth	−0.003	0.075	−0.031	0.054
	(0.025)	(0.047)	(0.030)	
7 years after birth	−0.008	0.031	−0.020	0.354
	(0.025)	(0.046)	(0.030)	
8 years after birth	−0.030	−0.013	−0.034	0.694
	(0.025)	(0.046)	(0.030)	
9 years after birth	−0.017	−0.010	−0.019	0.869
	(0.025)	(0.045)	(0.030)	
10 years after birth	−0.002	0.014	−0.007	0.694
	(0.025)	(0.045)	(0.030)	
11 years after birth	0.018	−0.018	0.034	0.324
	(0.024)	(0.044)	(0.029)	
12 years after birth	−0.002	−0.017	0.006	0.656
	(0.024)	(0.043)	(0.029)	
13 years after birth	−0.015	−0.016	−0.015	0.976
	(0.024)	(0.043)	(0.029)	
14 years after birth	0.016	0.010	0.018	0.879
	(0.023)	(0.042)	(0.028)	
15 years after birth	0.002	0.013	−0.003	0.748
	(0.023)	(0.041)	(0.028)	
16 years after birth	0.004	0.011	0.001	0.833
	(0.023)	(0.041)	(0.027)	
17 years after birth	0.021	0.006	0.027	0.670
	(0.022)	(0.041)	(0.027)	

Notes: Each coefficient represents a separate 2SLS regression with years on PL 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 ±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. [a]Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. [b]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 Δ^a
Mother works full-time^b				
1 year after birth	−0.004	−0.008	−0.002	0.642
	(0.006)	(0.011)	(0.007)	
2 years after birth	−0.165***	−0.239***	−0.134***	0.009
	(0.018)	(0.035)	(0.021)	
3 years after birth	−0.003	−0.059	0.023	0.076
	(0.020)	(0.040)	(0.023)	
4 years after birth	0.043**	0.009	0.063**	0.271
	(0.022)	(0.043)	(0.025)	
5 years after birth	0.039*	0.012	0.053**	0.423
	(0.022)	(0.044)	(0.026)	
6 years after birth	0.024	0.029	0.026	0.944
	(0.023)	(0.045)	(0.026)	
7 years after birth	0.024	−0.011	0.042	0.321
	(0.023)	(0.046)	(0.027)	
8 years after birth	0.024	−0.057	0.059**	0.030
	(0.024)	(0.046)	(0.027)	
9 years after birth	0.044*	−0.047	0.084***	0.017
	(0.024)	(0.047)	(0.028)	
10 years after birth	0.042*	−0.022	0.071**	0.096
	(0.025)	(0.048)	(0.029)	
11 years after birth	0.041	−0.053	0.080***	0.018
	(0.025)	(0.048)	(0.029)	
12 years after birth	0.034	−0.078	0.081***	0.005
	(0.025)	(0.049)	(0.030)	
13 years after birth	0.030	−0.035	0.058*	0.102
	(0.026)	(0.048)	(0.030)	
14 years after birth	0.058**	−0.008	0.087***	0.098
	(0.026)	(0.048)	(0.030)	
15 years after birth	0.044*	−0.004	0.063**	0.241
	(0.026)	(0.048)	(0.031)	
16 years after birth	0.028	−0.029	0.051*	0.160
	(0.026)	(0.048)	(0.031)	
17 years after birth	0.040	0.009	0.054*	0.421
	(0.026)	(0.048)	(0.031)	

Notes: Each coefficient represents a separate 2SLS regression with years on PL 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 ±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. [a]Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. [b]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 Δ^a
Currently married, full sample^b				
1 year after birth	−0.019	0.014	−0.030	0.411
	(0.024)	(0.045)	(0.029)	
2 years after birth	0.003	0.047	−0.012	0.269
	(0.024)	(0.045)	(0.029)	
3 years after birth	0.011	0.069	−0.011	0.133
	(0.024)	(0.045)	(0.029)	
4 years after birth	0.006	0.041	−0.006	0.373
	(0.024)	(0.045)	(0.029)	
5 years after birth	0.006	0.054	−0.011	0.222
	(0.024)	(0.044)	(0.028)	
6 years after birth	0.013	0.050	0.001	0.360
	(0.024)	(0.045)	(0.028)	
7 years after birth	0.024	0.069	0.009	0.257
	(0.024)	(0.045)	(0.028)	
8 years after birth	0.025	0.060	0.015	0.400
	(0.024)	(0.045)	(0.028)	
9 years after birth	0.021	0.061	0.008	0.313
	(0.024)	(0.045)	(0.029)	
10 years after birth	0.024	0.058	0.014	0.407
	(0.024)	(0.045)	(0.029)	
11 years after birth	0.023	0.039	0.021	0.744
	(0.024)	(0.045)	(0.029)	
12 years after birth	0.030	0.050	0.027	0.663
	(0.024)	(0.045)	(0.029)	
13 years after birth	0.026	0.044	0.024	0.709
	(0.024)	(0.045)	(0.029)	
14 years after birth	0.022	0.041	0.020	0.702
	(0.024)	(0.045)	(0.029)	
15 years after birth	0.021	0.025	0.025	0.901
	(0.025)	(0.045)	(0.029)	
16 years after birth	0.025	0.034	0.027	0.934
	(0.025)	(0.045)	(0.029)	
17 years after birth	0.023	0.030	0.026	0.996
	(0.025)	(0.045)	(0.029)	

Notes: Each coefficient represents a separate 2SLS regression with years on PL 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 ±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. [a]Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. [b]Currently married.

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

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

Notes: Each coefficient represents a separate 2SLS regression with years on PL 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 ±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. [a]Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. [b]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 Δ^{a}
Currently married, cond. on not being married at birth[b]				
1 year after birth	−0.001	0.015	−0.006	0.522
	(0.017)	(0.023)	(0.021)	
2 years after birth	0.037	0.094***	0.018	0.090
	(0.023)	(0.034)	(0.029)	
3 years after birth	0.049*	0.115***	0.026	0.080
	(0.026)	(0.039)	(0.033)	
4 years after birth	0.043	0.075*	0.033	0.458
	(0.028)	(0.044)	(0.035)	
5 years after birth	0.033	0.078*	0.019	0.311
	(0.029)	(0.046)	(0.036)	
6 years after birth	0.041	0.070	0.033	0.532
	(0.030)	(0.047)	(0.037)	
7 years after birth	0.049	0.098**	0.033	0.285
	(0.030)	(0.048)	(0.037)	
8 years after birth	0.049	0.079	0.040	0.519
	(0.030)	(0.049)	(0.037)	
9 years after birth	0.039	0.072	0.029	0.484
	(0.031)	(0.049)	(0.037)	
10 years after birth	0.036	0.049	0.034	0.805
	(0.031)	(0.050)	(0.037)	
11 years after birth	0.038	0.043	0.039	0.956
	(0.031)	(0.049)	(0.038)	
12 years after birth	0.047	0.047	0.050	0.967
	(0.031)	(0.049)	(0.037)	
13 years after birth	0.047	0.029	0.057	0.650
	(0.031)	(0.049)	(0.037)	
14 years after birth	0.041	0.022	0.052	0.635
	(0.031)	(0.049)	(0.037)	
15 years after birth	0.046	0.016	0.060	0.481
	(0.031)	(0.049)	(0.037)	
16 years after birth	0.050*	0.028	0.063*	0.570
	(0.031)	(0.049)	(0.037)	
17 years after birth	0.047	0.039	0.055	0.797
	(0.030)	(0.049)	(0.037)	

Notes: Each coefficient represents a separate 2SLS regression with years on PL 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 ±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. [a]Prob>chi2 of difference in coefficients between communities with and w/o nursery based on fully interacted regressions. [b]Currently married in the sample of mothers who have not been married at birth.