Does Parental Leave Affect Fertility and Return-to-Work? Evidence from a "True Natural Experiment"

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Abstract We study the causal effects of changes in parental leave provisions on fertility and return-to-work behavior. We exploit a policy change that took place in 1990 in Austria which extended the maximum duration of parental leave from the child's first to the child's second birthday. As parental leave benefits can be automatically renewed when a new mother is still on leave from a previous child, this created a strong incentive to "bunch" the time off work in case of multiple planned children and/or to increase fertility. We study the quantitative effect of this incentive using an empirical strategy which resembles a true experimental set-up very closely. In particular, assignment to treatment is random and treated and controls face (almost) identical environmental conditions. We find that treated mothers have a 4.9 percentage points (or 15 percent) higher probability to get an additional child within the following three years; and a 3.9 percentage points (or 10 percent) higher probability in the following ten years. This suggests that not only the timing but also the number of children were affected by the policy change. We also find that parental leave rules have a strong effect on mothers' return-to-work behavior. Per additional months of maximum parental leave duration, mothers' time off work is reduced by 0.4 to 0.5 months. The effects of a subsequent policy change in 1996 when maximum parental leave duration was reduced from the child's second birthday to the date when the child became 18 months old brought about no change in fertility behavior, but a labor supply effect that is comparable in magnitude to the one generated by the 1990 policy change. This can be rationalized by the incentives created through automatic benefit renewal.

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1 Introduction

Can parental leave policies stimulate fertility? Total fertility rates have been strongly decreasing since the 1960s and have reached levels below replacement in almost all OECD countries. In times of aging societies the implications of declining births for the population structure are dramatic and policies to turn around this trend are high on the agenda. Hence precise estimates on how and to which extent parental leave and other family polices can affect fertility behavior are of enormous interest for economic policy.

The present paper exploits a parental leave policy change that allows us to estimate its causal effects rather precisely. The policy change was implemented on July 1, 1990 in Austria and extended the maximum duration of parental leave from the child's *first* to the child's *second* birthday. This change created a strong incentive for new mothers to plan additional children or to get planned children more quickly. The reason why this incentive was particularly strong, is a special feature of the Austrian parental leave system: eligibility to parental leave is tied to previous work experience but this requirement is abandoned and parental leave is *automatically renewed* provided the new birth occurs when the mother is still on parental leave from a previous child. Before the policy change, automatic renewal was of minor significance, simply because it is biologically difficult to give birth to a further child within a year's time. After the policy change, automatic renewal became not only biologically easily possible but also highly attractive. This is because automatic renewal implies that the work requirement is abandoned during times when a mother's disutility of work is particularly high, i.e. when the children are still very small. This creates not only an incentive to "bunch" planned births and the associated time off work but may also induce women to get more children.

To assess how increased parental leave duration affected fertility and return-to-work choices we rely on a comparison of two interesting groups of young mothers: "treated" young mothers who delivered their baby in *July* 1990 and "control" young mothers who delivered their baby in *June* 1990. Such a comparison has two attractive features which imply that our empirical strategy resembles a true experimental set-up very closely. First, assignment of a mother to one of the two groups is purely random and endogenous selection does not play a role. This is because parents could not anticipate the parental leave reform when they made their fertility choices. The political discussion about the reform started in mid November 1989. At that time, however, the babies born in June/July 1990 were already conceived. Hence selectivity on the basis of parents' deliberate choices can be ruled out.[1] Furthermore, selectivity on the basis of fine-tuning the due date cannot be a problem either. This is because the 1990 parental leave reform created an incentive to postpone the date. While bringing the date forward is possible (by inducing a birth or by a cesarean section), postponing it is impossible on biological grounds.

A second attractive feature of this empirical strategy is that the environments in which members of the two groups are making their fertility and return-to-work choices are almost identical. Much of our empirical analysis will focus on the probability of getting an additional child and the probability

[1]One could argue that assignment to treatment is non-random as the political discussion might have affected the decision to abort an already started pregnancy. Notice, however, that until early April 1990 it was neither clear *when* the parental leave reform will take place nor *under which conditions* the reform would be implemented. Hence, even if the political discussion had an impact on abortions, it is not clear why the two groups should have been affected differently.

of returning to work during the 36 months following the June/July 1990 birth. This implies that any observed differences between treated and controls cannot be attributed to differences in environmental conditions between the two groups. In fact, during the 36 months over which we follow the mothers' behavior, treated and controls have *identical* conditions in 35 out of the 36 months following the birth. This allows us to adopt a (sharp-design) regression discontinuity approach (Hahn, Todd, and Van der Klaauw, 2001) in which the required assumptions are satisfied in an almost ideal way. In sum, our study is one of the rare cases in a socio-economic context where a policy change created a "true natural experiment".

We note that the effect estimated from a comparison of these two groups captures the causal effect of incentives created by *automatic renewal* of parental leave. The overall effect of the policy change, however, is likely to be larger. Our estimated effect is based on a situation where, for any *subsequent* birth, the more generous post-reform rules apply not only to treated but also to control young mothers. This implies that our estimate does not take account of the *direct* effect of the more generous parental leave rules resulting from longer job protection and longer benefit duration. To the extent that these more generous rules have a direct effect on fertility, the overall effect of the policy change will be larger than the automatic renewal effect we estimate here.

There are two further respects in which the present study contributes to the literature. First, the 1990 policy change was followed by a further reform in 1996 that reduced maximum parental leave duration from 24 to 18 months. This yields further informative variation in parental leave duration. In particular, the 1996 reform allows us to study whether the effects of parental leave duration are linear (in the sense that the six-month decrease of the 1996 reform lead to responses of half the size as the twelve-months increase of the 1990 reform) or whether such changes involve important non-linearities. We note further that studying such big variation in parental leave durations is potentially highly relevant as previous studies have found that short parental leave mandates do not change behavior whereas longer mandates do (Baker and Milligan, 2005). If parental leave policies are an effective family policy instrument, the Austrian policy change should produce substantial changes in mothers' fertility and work behavior.

A second respect in which the present study contributes to the literature is a unique and very informative data set, the Austrian Social Security Database (ASSD). This database registers not only the births but also the take-up of parental leave benefits and the work history of individuals. This is particularly favorable in the present context, as our goal is to study the joint fertility and work decisions of mothers. Furthermore, the ASSD covers the universe of all Austrian employees which allows us to draw very specific samples. In sum, the ASSD provides more detailed information on the effects of parental leave policies than most data sets used in previous studies.

Our findings are as follows. *First*, the twelve-months extension of parental leave enacted with the 1990 policy reform had a strong impact on the probability of having a further child. The percentage women who got an additional child within the next 36 months is about 5 percentage points (or 15 percent) higher when parental leave lasts until the child's second birthday as opposed to the child's first birthday. However, the six-months reduction in maximum parental leave duration enacted with the 1996 policy reform, did not change the share of women with additional children. These seemingly contradictory results can be rationalized by the incentives of automatic renewal of parental leave benefits.

Second, we find that both policy changes had a significant impact on mothers' return-to-work behavior. It turns out that, per additional month of maximum parental leave duration, the time off work increases by 0.4 to 0.5 months. The return-to-work patterns are strongly determined by parental leave rules – with particularly strong increases in the percentage women back at work when the maximum parental leave ends. We do not observe any asymmetries between the extension and the reduction of parental leave effects.

Third, for the 1990 policy change, we find that fertility behavior is not only affected in the short run but we also see long-term effects of the policy change. Among women eligible to the more generous parental leave rules, a substantially higher percentage had an additional child even ten years after the birth of the first child. Hence, not only the spacing between planned births, but most likely also the number of additional birth was affected. (Obvious data constraints do not allow a corresponding analysis for the 1996 policy change).

The paper is organized as follows. In the next section, we briefly discuss related previous literature. Section 3 discusses fertility related family-policies in Austria and describes in detail the 1990 and 1996 parental leave policy changes. Section 4 presents and discusses our empirical strategy. The empirical results of the extension and shortening of parental leave are presented in sections 5 and 6. Section 7 concludes.

2 Previous literature

Our paper relates to various strands of the existing literature. We briefly review (i) studies that look at the effect of parental leave (and other family policies) on fertility behavior, (ii) studies that analyze the implications of parental leave for labor supply and return-to-work behavior, and (iii) studies that analyze the joint determination of labor supply and fertility choices.

There is a small literature that studies the role of parental leave policies for fertility outcomes. A study closely related to the present analysis is Hoem (1993) who looks at the effects of a "speed premium" introduced in Sweden during the late 1970s and early 1980s. Similar to the present paper, these rules let parents retain their parental leave benefits provided an additional child came in time. Hoem (1993) find that this speed premium lead to substantial increases in Swedish fertility rates. Unlike the present paper, which relies on longitudinal individual data, Hoem's (1993) analysis is based on vital statistics disaggregated by cohort, parity, and age of previous child. Hoem's analysis relies on a pre-post comparison (where many other things may change), so the precise quantitative effect of the successive policy changes is hard to deduce. In contrast, the present paper takes particular care in estimating a causal effect of the policy change.

A further closely related paper is Piketty (2003). Similar to the present analysis, he uses a policy change, the 1994 extension of the French Parental Education Benefit, to study the implications for both fertility and labor supply behavior. The reform allowed women to claim benefits at the birth of their second child (rather than at the birth of their third child as under previous legislation). This policy change induced a large fall in participation but only a small increase in fertility.

Averett and Whittington (2001) study whether the adoption federal Family and Medical Leave Act in 1993 did affect fertility rates in the U.S. The Act guaranteed 12 weeks of unpaid parental leave to workers in larger firms (more than 50 employees). They find that maternity leave provisions significantly increase the probability of birth, an effect which is larger for higher birth parities. Obviously, this study suffers from potential selection bias as women with desired fertility may look for jobs providing parental leave. However, Averett and Whittington (2001) find that a woman's desire for children does not seem to be an important mechanism by which women are sorted into jobs with parental leave provisions. Büttner and Lutz (1990) analyze the impact of a similar policy change that took place in 1976 in East Germany (the former German Democratic Republic) where paid parental leave was increased from originally 18 weeks to 26 weeks (52 weeks for mothers with two or more children) which lead to a remarkable increase in fertility rates in the following years.

To our knowledge, the only study that has looked at the effect of the 1990 change in Austrian parental leave legislation is the study of Hoem, Prskawetz, and Neyer (2001). They show that the 1990 reform has had an impact on the likelihood of *third* births. The paper by Hoem et al. (2001) differs from our study in various respects. First, their study is to understand the relative importance of fertility determinants in Austria, including educational attainment and women's labor force status, in addition to the role of family policy. In contrast, our focus are the detailed incentives and effects of the 1990 parental leave reform. Second, they use survey data whereas our study is based on social security register data. The advantage of our data set is that it provides us with a larger number of observations and lets us focus on much more informative samples. Third, we do not only study the implications of the 1990 extension, but also study the effects of the subsequent 1996 reduction in parental leave duration. Finally, and most importantly, we take particular care to precisely estimate the causal effect of the policy changes that result from the possibility of automatic parental leave renewal.

Further related studies have looked at the impact of other pronatalist family policies on fertility. Milligan (2005) studies the introduction of the Allowance for Newborn Children in Quebec, Canada, which pays up to C$8,000 to parents in the time following the birth of a child. He finds a significant impact on fertility and also heterogenous effects across subgroups. Hardoy and Schøne (2005) analyze the fertility implications of the Norwegian cash-for-care subsidy which reduces the cost of children for mothers with limited access to public child care. It turns out that the introduction of the subsidy lead to delays in subsequent births.

Another related literature has studied the impact of the U.S. welfare reform on fertility rates, with mixed evidence. Surveys of Hoynes (1997) and Moffitt (1998) either see no or only a weak and little robust impact of welfare reform on fertility. This appraisal is supported by more recent evidence of Kearney (2004) who studies the consequences of the implementation of family cap policies. These rules were implemented as part of the U.S. welfare reform and deny or reduce transfer payments to welfare recipients with additional births. No significant reduction of fertility rates of women aged 15-34 could be detected. This evidence is supported by Joyce, Kaestner, Korenman, and Henshaw (2004) who find that women with a high risk of recipiency showed a similar fertility behavior in family cap states and in states that did not introduce the cap. Joyce, Kaestner, and Korenman (2002) find little effect of the Personal Responsibility Work Opportunity Reconciliation Act which strongly altered the financial incentives to bear children out-of-wedlock. They find no systematic impact of the Act on out-of-wedlock fertility. However, Rosenzweig (1999) finds that high welfare benefit levels and low marital prospects induce young women to bear children out of wedlock.

Also other studies have found significant impacts of financial incentives on fertility behavior. Whittington, Alm, and Peters (1990) and Whittington (1992) study the impact on fertility rates of the U.S. personal tax exemption of dependants which provides an implicit subsidy for each child of. It turns out that the personal exemption has a positive and significant effect on the US national birth rate. Similar evidence is found by Zhang, Quan, and Van Merrbergen (1994) for Canada and by Ermisch (1988) for the U.K. Gauthier and Hatzius (1997), using panel data for 22 OECD countries, show that family benefits have a significant (albeit not very large) effect on fertility rates. The study by Boyer (1989) looks at the effects of the Old Poor Laws of 19th century Great Britain and finds that child allowances caused the birth rates to increase.

A more extensive literature has assessed the role of parental leave policies for labor supply and return-to-work decisions of young mothers. Studies focusing on the U.S. have either used state variation in parental leave provisions or have analyzed the impact of the 1993 U.S. Family and Medical Leave Act. Klerman and Leibowitz (1997) find that changes in maximum parental leave durations across U.S. states does affect the duration of leaves but does not signficantly affect female employment. Klerman and Leibowith (1999) find that the introduction of the U.S. Family and Medical Leave Act in 1993 did not have any major impact on the return-to-work behavior of young mothers. A more recent study by Baum (2003) confirms this result. The 12 week duration granted by the 1993 Leave Act appears to be too short, parental leave is unpaid, and other rules (minimum employer size) excludes many woman from the rules. In contrast, Berger and Waldfogel (2004) find that there is closer relationship between maternity leave coverage and mothers' labor force participation.

Parental leave rules in other countries are more generous and hence more likely to have an impact on the labor supply behavior of mothers. Baker and Milligan (2005) exploit the substantial variation in parental leave provisions over time and across Canadian provinces (ranging from 17 and 70 weeks) to study how such policies affect the amount of time that mothers spend with their children. They find both short and long mandates increase job continuity (i.e. the fraction of women returning to their pre-birth employer). However, only long (but not short) leaves the time off work. This is important, as most welfare-enhancing effects of parental leave legislation work via its effects on the amount of time that mothers spend with their very small children.

Ruhm (1998) as well as Ruhm and Teague (1997) show, using panel data on parental leave provisions across European countries, that womens' attachment to the labor force increases as with the duration of parental leave mandates. Pylkkänen and Smith (2004) find that the impact of parental leave and other family policies has a strong impact on mothers' career interruptions in Denmark and Sweden. Furthermore, the more flexible Swedish system – where parents can split parental leave and times on partenal leave can be saved now and used up later – lets women adopt more strongly to policy changes as opposed to Denmark where the parental leave system does not allow for such flexibility. Ondrich, Spiess, Yang, and Wagner (1999) show that changes in maximum leave durations in Germany in the

1980s had a significant impact on the time that mothers spend with their children.

A final related strand of literature aims at understanding the role of financial incentives for women's joint labor supply and fertility decisions. These studies have typically relied on the estimation and/or simulation of structural models. Early approaches include Moffitt (1984), Rosenzweig and Schultz (1985), Hotz and Miller (1988), Heckman and Walker (1990), see also the survey of Hotz, Klerman, and Willis (1997). More recent ones are Francesconi (2002), Del Bocca (2002) and Del Boca, Pasqua, and Pronzato (2004). Keane and Wolpin (2002a,b) estimate and simulate a dynamic model of fertility and welfare take up. A recent paper by Laroque and Salanie (2005), also based on the structural approach, models in much detail the French tax-benefit system and its implications for female participation and fertility. Their results and simulations suggest that financial incentives play a sizable role in determining fertility decisions in France.

3 The institutional environment

3.1 The Austrian parental leave system

The Austrian system was introduced in 1957 when mothers were protected from dismissal of the previous job for a period of 6 months. Two major reforms took place in 1961 when the maximum duration of parental leave was extended up until the child's first birthday and a means-tested transfer payment proportional to the unemployment benefit was introduced; and in 1974 when the transfer became flat rate and independent of household income.

The rules that were in place during the 1990s required a minimum employment experience prior to birth. Women taking up parental leave for the first time, had to have worked (and paid social security contributions) for at least 52 weeks during the two years prior to birth. For mothers with at least one previous take-up of parental leave the employment requirements is reduced to 20 weeks of employment during the last year prior to the subsequent birth. Parental leave comes with two major advantages. First, mothers are eligible to a flat rate transfer amounting to about 340 Euros per month (in 1990) or about 31 % of gross median earnings of female workers. Benefits are not means tested and not taxed implying a median *net income* replacement ratio of more than 40 %. Women without a partner or with a low-income partner are eligible to higher benefit levels (Sonderunterstützung). Second, parental leave provides protection of a mother's previous job. A mother can return to her previous employer and is protected from dismissal during the first 4 weeks after returning to work.[2]

Parental leave is preceded by a period of *maternity protection* intended to protect the health of mother and child around the period of confinement. This period of *maternity protection* lasts for 16 weeks (usually 8 weeks before and 8 weeks after the actual birth). During this period women are not only insured against the risk of dismissal but get an associated transfer equal to the average wage rate over the last quarter prior to the birth. Formally, the parental leave period starts when the maternity protection period ends (see Figure 1).

A specific feature of the Austrian parental leave system is the possibility of automatic parental leave

[2]The effective duration of job protection is much longer than these four weeks. In Austria, layoffs are suject to advance notice regulations implying that a mother's job is protected for several months after returning from parental leave.

renewal in case the mother is still on leave from a previous child. In that case, the parental leave benefits (that is, job protection and parental leave transfer) are extended without any further work requirement. The work requirement is even abandoned if the new birth follows closely after a previous birth. Provided that a new maternity protection period starts within 6 weeks after the formal termination of a previous leave, take-up of a new parental leave is possible without any further employment requirement. As maternity protection starts 8 weeks before the due date, the rules effectively imply that automatic renewal possible until 14 weeks after the formal termination of a previous parental leave period (see Figure 1). If a new birth occurs after this date, the new mother is required to have worked for at least 20 weeks to requalify for a new parental leave period.

Figure 1

Prior to July 1, 1990, the maximum duration of parental leave ended with the child's first birthday. Automatic benefit renewal played only a minor role, simply because it is biologically difficult to get a further child within the required period (= one year plus 14 weeks). Planning an additional child very quickly after the expiration of the previous parental leave involves the risk of not qualifying for automatic renewal and of being forced to work for 20 weeks to requalify. As the disutility of work is very high when children are very small, mothers planning a further child had an incentive to postpone childbearing under the parental leave rules prior to July 1990.

After July 1, 1990, the maximum duration of parental leave was extended until the child's second birthday. Just like before, parental leave may end earlier for mothers taking up the old job or for mothers getting an additional child. Clearly, the rules after July 1990 give quite different incentives. Having a second child within a period of two year plus 14 weeks is easily possible from a biological point of view. In other words, parental leave rules after July 1990 provide an incentive to chronologically "bunch" a given number of planned children and the associated time off work. Moreover, the change in parental leave duration creates an incentive to increase the number of births.[3]

A further policy change took place *after July 1, 1996*. After that date, the maximum duration of parental leave still lasted until the child's second birthday. However, the new rules required that at least 6 months of the leave had to be taken by the father. As fathers' take-up of parental leave is negligible, the 1996-reform effectively implied a reduction in maximum parental duration from the child's second birthday to the date when the child became 18 months old. Notice that automatic benefit renewal is still a relevant option under these new conditions. Having a further baby within 18 months plus 14 weeks after the previous birth is still biologically easily possible. To take advantage of automatic renewal, the spacing between two subsequent births has to become shorter. As a result, the 1996 policy

[3]The 1990 policy reform came with several additional changes. The system was changed from a "maternity" to a "parental" leave system. Not only the child's mother but also the father could go on parental leave. However, this is of no practical consequence. In 1990 less than 1 % of fathers took advantage of that possibility. A second change was that women in farm households and family businesses as well as women who did not meet the employment requirements became eligible to a transfer equal to 50 % of regular parental leave benefits up until the child's second birthday. This is of no importance in the present analysis because we confine ourselves to study behavior of female dependent employees. Furthermore, the reform made it possible to take part-time parental leave, either between child's first and second birthday (by both parents at the same time) or between child's first and third birthday (only one parent or both parents alternating).

change may even further enhance the "bunching" incentive.[4] The 1996 reform brought also a slight increase in previous employment requirements for second and subsequent birth. Instead of originally 20 weeks within the last year, women had to spend 26 weeks within the last year in employment. This further enhances the incentive to take advantage of automatic renewal of parental leave benefits.

3.2 Other fertility related family policies

Besides parental leave benefits, fertility-related family policies in Austria consist of a broad set of measures that we only briefly discuss here. Parents are eligible are *child benefits* (Familienbeihilfe). There is universal eligibility to these benefits (meaning that all parents with sufficiently long residence in Austria are eligible). These benefits amounted to about 95 Euros per month for each child below age 10, and to 110 Euros per month for each child between ages 10 and 19). The tax system has *tax deductions for children* (Kinderabsetzbeträge), that increase with the number of children. Furthermore there is a *birth benefit* (Geburtenbeihilfe) of Euro 1090 paid to mothers in several steps upon medical inspections between the child's birth and its fourth birthday. The supply of *child care facilities* for small children is rather low. According to OECD (Employment Outlook 2001) the proportion of children under age 3 enrolled in formal child-care arrangements was only about 4 % in 1998 which is very low be international standards. [5]

While the most significant changes in fertility-related family policies during the 1990s concerned changes in parental leave legislation, several other minor changes were made with respect to other family policies. In 1997 the birth benefit was abolished. In 1998 there was a major effort by the central government to improve the supply of childcare facilities in public kindergardens (Kindergartenmilliarde). However, this increase in government spending was targeted towards the age group 4-6 rather than the very small children.

Figure 2 clearly shows that times when average parental leave duration is high are times when total fertility is high. However, the picture does not tell a very clear story. First, the correlation between average duration of parental leave and the number of births is far from clear. In the late 1980s parental leave was still low, but the number of births was close to the 90,000 threshold. In the late 1990s, when both maximum and average parental leave duration was more 50 % higher, the number of births was considerably lower. Second, parental leave is clearly not the only determinant of fertility behavior. Other things were changing as well. Most importantly, labor force participation rates (women ages 15-64) rose from less than 50 % in the mid 1980s to more than 60 % by the year 2000 (OECD

[4] A further major policy change took place in 2002. There were two major changes. First, transfer payments (Kinderbetreuungsgeld) was increased and granted for a period of 3 years (rather than at most two years as under previous rules). Furthermore, transfer payments become became independent of previous work requirements (so also other previously non-covered group became eligible). Third, mothers could go on leave for two years again and, during the leave period, earn labor income up to 14,500 Euros during the period when benefits are drawn. We do not discuss these changes in more detail, as our data do not allow us to study this most recent change.

[5] For instance, the comparable number for the U.S. is 54 %, for Denmark, Norway and Sweden 64 %, 40 %, and 48 %, respectively. Germany, and southern European countries have similarly low levels of child care facilities for kids under age 3 (These number include both public and private child care provision such as group care in child-care centres, residential care, childminders based in their own home, care provided by person who are not a family-member; see OECD Employment Outlook 2001).

Employment Outlook, 1993, 2004) and were monotonically increasing throughout the 1990s. Third, and most obviously, the evolution of births is not only the effect of fertility behavior (that is, the propensity to become a child by the typical woman in the relevant age group) but also the result of demographics (that is, the number of potential mothers at a particular date). This suggests that a closer look at the data is necessary to shed light on the role of parental leave legislation on fertility behavior.

Figure 2

4 Data

In order to assess the effect of parental leave legislation on fertility behavior and labor supply we use information from the Austrian social security dataset (ASSD). The ASSD consists of administrative individual register data collecting information relevant for old-age social security benefits. As these benefits depend on individuals' earnings and employment histories, the data set reports these histories for the universe of Austrian private sector workers. Furthermore, not only employment histories, but also times with childbearing and -rearing ("Kinderersatzzeiten") are relevant for old-age social security benefits. In other words, the data set reports high-quality information on the number of births and the take-up of parental leave benefits by female employees.

The ASSD has several obvious advantages which will be of particular importance for the empirical strategy developed below. First, the data set covers the *universe* of the private sector employees in Austria implying we can rely on large samples, even when very specific groups are considered. Second, the data reports, on a daily basis, the occurrence of a birth and take-up (and durations) of maternity protection and parental leaves since the year 1972. This allows us to determine precisely both the parental leave eligibility status as well as the maximum duration of parental leave of mothers. Third, as all employment and earnings over an individual's life cycle are reported in the data, we can look in a very detailed way at the joint distribution of fertility and labor supply behavior of mothers over extended time periods.

A disadvantage of the ASSD is incomplete recording of *previous* births. The condition for a birth being registered in the ASSD is that a woman had to be employed at least once prior to this birth. If no such previous employment spell exists, it is random whether or not a birth is recorded in the data. However, once a birth has been registered in the data, all further births are also registered. This means for all mothers in our data, we can precisely determine the *subsequent* fertility life cycle. However, the randomness in birth reporting of previously never-employed women implies that we cannot precisely determine the *parity* of a birth. To make things precise, consider a woman who gets a child at age 30 and who was employed prior to birth. In that case the current birth and all subsequent births are registered in the data. Assume this women has entered the ASSD at age 25 (because she was never employed before that age) was continuously employed until age 30. While the birth at age 30 is observed in the data for sure, we cannot be certain that this is the woman's actual first birth. This woman could have given birth to children before entering the ASSD at age 25. More generally, if we observe x previous births in the data, we know that any subsequent birth is of parity x *or higher*.

As a result, we cannot condition the likelihood of a subsequent birth in an exact way on the number of previous births. This is important, since demographic research has typically found then number of previous children as one of the most important determinants of the likelihood of an additional child. While this is a drawback of our analysis, we can still precisely determine the likelihood of an additional child unconditional on the number of previous birth. As we will see below these indicators are quite informative on the causal effect of changes in parental leave legislation.

5 Empirical strategy: regression discontinuity analysis

The focus of this paper is to assess the effects of the duration of parental leave for spacing and number of subsequent children as well as the mothers' return-to-work behavior. Let T denote the date of birth of the previous child. Let Y indicate whether an additional birth occurs within 36 months after date T, i.e. $Y = 1$ if the mother gives birth to an additional child, and $Y = 0$ otherwise.[6] Let D indicate a mother's treatment status, i.e. $D = 1$ if parental leave lasts until the previous child's second birthday, and $D = 0$ if parental leave lasts until the previous child's first birthday.

Assignment to treatment is a discontinuous function of the date of birth T. All mothers giving birth to a child on or after July 1, 1990 (t_0) are entitled to the prolonged parental leave duration, whereas all mothers giving birth to a child earlier are denied the extended parental leave duration. We draw two samples from our raw data. The *treated or "before" sample* consists of women who gave birth to a child between June 1 and June 30, 1990; the *control or "after" sample* consists of women who gave birth to a child between July 1 and July 30, 1990. Because the ASSD reports all births occurring in the universe of all females employed in the private sector we end up with a sufficiently large data set amounting to 6218 observations.

While our data set does not report the parental leave eligibility status directly, we observe actual parental leave take-up. Thus, we can investigate how strongly the duration of parental leave changes as a function of calendar time. Figure 3 reports average durations of parental leave durations by *birthdays.* The data show very clearly that, within the first three years immediately after the birth, mothers who deliver their baby in June 1990 are on parental for roughly one year. In contrast, mothers who gave birth in July 1990 the corresponding number is almost twice as high. In particular there is no trend in average parental leave durations *within each month.*[7] So we can be quite confident that assignment to treatment changes sharply on July 1, 1990.

Figure 3

Thus $E(D|T = t_0 + \epsilon) = 1$ and $E(D|T = t_0 - \epsilon) = 0$, i.e. assignment to treatment is "sharp" in the

[6] We focus on the a space of 36 months between births because parental leave coverage differs between mothers at most for a period of 28 months between births. Thus, only the first three years between births can directly be affected by differences in parental leave legislation. A sensitivity analysis discusses the results when the space between births is extended to 120 months or 10 years.

[7] Notice that take-up of parental leave is itself an endogenous variable. However, as the majority mothers use up the eligibility period, this indicator is informative on the treatment intensity. Notice further that the take-up indicator in Figure 3 reports take up of parental leave benefits during the first three years. Because more than one birth may take place during that interval, the indicator shown in Figure 1 may be larger than the maximum duration of a single leave.

terminology of Hahn et al. (2001).[8] Thus, an intuitively appealing contrast that infers the causal effect of extended parental leave on the spacing of births is the following

$$E(Y|T = t_0 + \epsilon) - E(Y|T = t_0 - \epsilon)$$

It can be shown that for $\epsilon > 0$ sufficiently small, this contrast identifies the average effect of offering extended parental leave duration on the spacing of births for mothers giving birth when parental leave was extended (Hahn et al. 2001).[9]

There are several reasons making us confident that a comparison of these two groups is informative on the *causal* effect of parental leave legislation on fertility and return-to-work supply behavior. A *first reason* comes from inspection of the observed characteristics of the two groups. If there is random selection into treatment status, we would expect no major differences in the means of observed characteristics between the two groups. Table 1 shows that, in fact, the two groups are very similar with respect to observed characteristics. In both samples, almost 80 percent of all births occurred in mothers' age group 20-29 and about 13 percent at ages 30 and above. Both groups were roughly 1.5 years in employment and .12 years in unemployment during the past two years. All other job characteristics, like average wages (and their standard deviation), white collar employment, and the distribution of previous employment across industries and regions are almost identical between the two groups. While the two groups are very similar, they are not identical. Our analysis below will therefore use regression analysis which controls for these individual characteristics.

Table 1

A *second reason* refers to the way the treatment status is assigned to individuals. As we focus on births that took place during the rather short period in June and July 1990, this comes close to a random assignment of treatment status to individuals. To study the information that potential parents got via the public debate on the upcoming parental leave reform, we performed a content analysis of the major Austrian newspapers. The public discussion started in November 11, 1989 – seven and a half months prior to the final implementation of the change. At that time it was neither clear whether, when, and how extended parental leave would actually be introduced. While, on November 15, 1989, the press announced that the extended parental leave might be introduced on July 1, 1990, on November 16, 1989, 21 pro-business members of the parliament announced that they would block a law extending parental leave. On January 5, 1990, the headline of an article of the "Neue AZ" regarding family policy announced that the policy of "Extension of Parental Leave Has Failed". It took until April 5, 1990, that the press finally declared that the ruling coalition (social-democrats and conservatives) had found a political compromise. In sum, the chronology of the public parental leave reform debate suggests that it was unclear until 3 months prior to the policy change whether and under which conditions the

[8]Note that in the analysis, we treat time as discrete with the smallest time unit equal to 1 day. This guarantees, that the density of births at t_0 is non-zero.

[9]When assignment to treatment is sharp, $E(Y|T = t_0 + \epsilon) - E(Y|T = t_0 - \epsilon) = E(Y_1 - Y_0|T = t_0 + \epsilon) + E(Y_0|T = t_0 + \epsilon) - E(Y_0|T = t_0 - \epsilon)$ with Y_0 denoting the space between births with $D = 0$ and Y_1 denoting the space between births with $D = 1$. For $\epsilon > 0$ sufficiently small, this contrast identifies the average effect of treatment at calendar time t_0 – $E(Y_1 - Y_0|T = t_0)$ – provided that $E(Y_0|T)$ is continuous in t_0.

parental leave would be extended. Hence it is impossible that the fertility decisions for the June/July 1990 births were influenced by parents' anticipation of the 1990 policy change.

While selection on the basis of deliberate fertility choices cannot be relevant, one might still wonder why we observe substantially more births in July than we do in June (see Table 1, 3 bottom rows). The number of births over the 30 days interval June 1 to June 30 observed in the data is 2979 whereas the corresponding number for July 1 to July 30 is 3239, or 8.7 percent higher. This most likely reflects seasonality. Births in July exceed the number of births in June in any given year in the period 1986-1995. Moreover, both in 1986 and 1995 (when no family policy changes were enacted) the number of births reported in the ASSD is 9 percentage points in July compared to June. Nevertheless, we will perform sensitivity tests comparing births that occur, respectively, in the first/second half of June and the first/second half of July.

A *third reason* why our results are reliable comes from the time-proximity of the sample selection dates of the pre- and post-observations relative to the (comparably long) time interval over which we observe mothers' fertility and labor supply behavior. While samples are selected over two successive months, we will focus on women's fertility and return-to-work behavior over the first 36 months following the birth. Hence any differences between treated and controls cannot be attributed to differences in environmental conditions by the two groups. In fact, during the 36 months over which we follow mothers' behavior, treated and controls have *identical* environmental conditions in 35 out of the 36 months following the birth. Hence a typical problem that contaminates pre-post comparisons, cannot be of importance in the present context.

6 Extending parental leave: the July 1990 policy change

We are now ready to discuss the impact of parental leave duration on fertility and return-to-work behavior. We restrict our focus in two respects. First, we study only the behavior of mothers after their *first birth that is observed in the data.* (While below we will refer to this event as a woman's "first birth" we have to be aware that the true first birth is not observed in the data due to imperfect reporting of women's previous birth histories). The reason for this restriction is that we expect stronger effects of parental leave for low birth parities but the effects for higher parities is likely to be quantitatively less important. Second, we will concentrate on fertility and work behavior during the first 36 months after a woman's first birth. During this time, the differences in incentives between treated and controls are strongest, hence the incentive created by the policy change should work most strongly during this interval. Note, however, that the adopted changes in parental leave policies may also change life-cycle behavior of treated mothers so that not only the spacing but also the number of additional births may be affected. In a sensitivity analysis we will explore how the change in parental leave legislation affects fertility and return-to-work behavior over longer time horizons.

Figure 4 presents first evidence on the effects of parental leave policies on fertility behavior. The vertical axis measure the fraction of women with an additional child within the 36 months following the mid 1990 birth. The data points reported in the Figure are bi-weekly *backward* moving averages for births in June; and biweekly *forward* moving averages for births in July. (The averaging removes the noise in high-frequency day-to-day fertility data; forward/backward averaging ensures that the same parental leave rules apply for each July/June data point in Figure 4). The probability that a woman in the control sample (mothers with a birth in June 1990) gives birth to an additional child within the 36 months following June 1990 is .320; the corresponding probability in the treated sample is .367, or about 15 percent higher. For the control sample, the biweekly (backward) moving average is below .33 for each day in June. For the treated, the biweekly (forward) average is above .35 for each day in July. Hence the effect seems quite robust and does vary only slightly over the particular (biweekly) time window that we adopt.[10]

Figure 4

Table 2 presents the results from regression analysis using the indicator variable "additional child within 36 months" as the dependent variable (columns 1 and 2). The first column gives the coefficients from a regression of the additional-child indicator on a mother's treatment status without any further explanatory variables. This is just summarizing what we have seen in Figure 4 into a single number: the (unconditional) probability to get a further child is 4.7 percentage points (or 14.7 percent) higher for treated than for controls. In columns (2) we include additional explanatory variable into the linear probability model. This leaves our results unchanged. The point estimate of the treatment indicator is even slightly higher than before (4.9 percentage points - or a 15.3 percent increase in the additional-child probability).

As mentioned above, our goal is not only to understand how parental leave rules affect women's fertility behavior. We also want to see how these rules impact on the return-to-work process of mothers. Columns 3 and 4 presents first evidence on this question by regressing a mother's employment status 36 months after the mid 1990 birth on the treatment indicator. The results in column 3 clearly shows that return to work is strongly and significantly affected by parental leave rules. 36 months after the birth the fraction of women back at work is 9.7 percentage points (or 15.7 percent) lower for treated than for controls. Including additional regressors (column 4) leaves this result unchanged, the point estimate is of exactly the same magnitude as before. However, including additional regressors strongly increases the explanatory power of the linear probability model. Age, previous employment status, and the characteristics of the previous job have a significant impact on the decision to return to work after childbearing.

Columns 1 to 4 suggest that both fertility and return to work are strongly affected by parental leave rules. An indicator that summarizes the joint labor supply and fertility effects asks: How many women still stay at home with a single child? Columns 5 regresses this indicator on treatment status, column

[10] We have also investigated the assumption that the probability of having an additional child is continuous in calendar time by comparing the 14 day backward moving average for each day between June 1 and June 14 to the 14 day forward moving average for each day between June 1 and June 14. The idea of this contrast is the following. Treatment status of mothers is identical for each day in the backward moving average and in the forward moving average. Yet, the backward moving average and the forward moving average differ with respect to 13 out of 14 calendar days. Nevertheless, we find that the difference between the backward moving average and the forward moving average estimate of the probability of having an additional child is small. Almost all days between June 1 and June 14 are characterized by a difference between the backward and the forward moving average estimate that is smaller than 1 percentage point. This is evidence supporting the assumption that $E(Y|T)$ is continuous in the period June 1 to June 14.

6 includes additional control variables. Extending parental leave from the child's first to its second birthday reduces the fraction of women without an additional child that are still out of labor force after 36 months. Quantitatively, the effect is strong: the fraction being 3.5 percentage points (or 12.4 percent) lower for the treated than for the controls. Just like before, including additional regressors leaves the picture unchanged but improves the goodness of fit of the linear probability model.

Table 2

How robust are these estimates? In order to check this, Table 3 reports results from variations of models in columns 2, 4, and 6 of Table 2. (The row A in Table 3 simply reproduces the previous results for ease of comparison). One possible objection is that the effects measured here are, in fact, not (or not only) the result of changes in legislation but also the results of "seasonality" (mothers who give birth in July are of a different type than mothers who give birth in June). This objection may be relevant as we observe a much higher number of births in July as compared to June. To check whether such seasonality affects our results we reran all regression for the year for births in June versus July 1989. During the year 1989 no policy change took place and any difference between June and July births is purely seasonal. Row B in Table 3 shows that the birth month in 1989 does not have any significant impact on fertility and return-to-work behavior over the following 36 months.

Table 3

A further interesting issue relates to the life-cycle effects of changes in parental leave rules. In row C of Table 3 we present evidence on regressions when the dependent variables are measured ten years (rather than three years) after the mid 1990 birth. With respect to fertility, the estimated coefficient is still highly significant and quantitatively important. Ten years after the mid 1990 birth, the fraction of women with an additional child is still 3.9 percentage points higher in the treated group. This suggests that the effects of the 1990 parental leave reform were highly persistent. In other words, changes in parental leave rules did not only affect the spacing between a given number of planned births but also the number of subsequent births. Interestingly, the differences in labor supply are much lower after ten years. However, the fraction of women that are kept off the labor market as a result of the more generous parental leave rules are still almost 2 percentage points. Finally, and in line with the fertility results, the fraction of women with either an additional child or with a job ten years after the mid 1990 birth is significantly higher for the treated groups again suggesting highly persistent effects of changes in parental leave rules.

A final interesting sensitivity analysis conducted in Table 3 concerns the magnitude of the estimated effects. Is a 5 percentage point increase in the fraction of additional children small or large? To get a sense for the order of magnitude we compare in row D the estimated effect with the estimated effect that we get when we compare the two birth cohorts July 1990 and July 1993. If we see differences in behavior (over the following 36 months) between these cohorts, this most likely reflects a trend – there was no major change in parental leave rules and fertility-related family policies between July 1990 and June 1996. The results indicate that, among the July 1993 cohort, the fraction if women with an additional child within the next three years is 2.9 percentage points lower than in the July 1990 cohort. Neither the percentage women back to work nor the percentage women still off work without an additional child changes during this three-years time interval (see columns 2 and 3). Suppose our fertility estimate in column 1 reflects the true fertility trend in the 1990s. This implies would imply that the fraction of mothers giving birth to an additional child within the next 36 months was decreasing by roughly 1 percentage point per year. In other words, the increase in the generosity of parental leave rules in 1990 has neutralized between 4 and 5 years of this downward trend (depending on whether we rely on the the 120-months estimate of row C or the 36-months estimate of row A).

In Table 4 we investigate the issue of possible unequal birth behavior in June and July in more detail. We have already seen in Table 3 that, for the year 1989, the additional-child probability does not change between months June and July. Still, one might wonder why there are more births in July 1990 than in June 1990. In Table 1, we have seen that the difference in the number of births is minor when the second half of June is compared to the first half of July. However, there is a big difference in the number of births between the first half of June and the second half of July. If the reason is seasonality, mothers who give birth in early June may be of a different type than mothers who give birth in late July. In order to test the robustness of our estimates with respect to such seasonality we check whether treatment effects depend on the within-month timing of births. We divide the samples according to whether the birthday occurred in the first (between 1st and 15th) or the second (between 16th and 30th) day of the respective month and rerun the same linear probability model we ran in column 2, Table 2 for the whole sample. Results in Table 4 suggest that the within-month timing of births does not drive the results. Irrespective of the particular subperiods on which our treatment and control samples are based, the coefficient is of the same magnitude, between .047 and .050. Hence heterogeneous treatment effects based on seasonality can be ruled out.

Table 4

So far, our above analysis has analyzed fertility and labor supply behavior by looking a fertility and work status at a certain point in time after the mid 1990 birth. Our focus was 36 months after that date, because during the first three years after the birth, parental leave rules provide the most significant difference in incentives between treated and controls. However, we have not yet looked at the dynamics *within* this period. The hypothesis we put forward above is the extension of parental leave that took effect on July 1, 1990 gives an incentive to bunch childbearing. Automatic renewal together with the longer duration of parental leave under the new rules make it easily possible to time an additional child such that there is no necessity to return to work between two subsequent births. This implies we should observe most of the additional births that we see under the new rules (Figure 4 above) between the child's first and second birthday.

To shed light on this timing issue we draw in Figure 5 the additional-child "hazard" rate. This is the likelihood that, conditional upon having no child up to month $t-1$, a woman gets a further child during month t. For obviously reasons, the hazard rate is very small, before month 12, both for the treated and the control sample. After that date, the two graphs start to diverge. The control group has a somewhat higher hazard value between months 12 and 16, whereas the treated group takes over after month 18 up until month 28. The difference between the two groups is highest during months 22 to 25, when the additional birth hazard is almost twice as large for the treated. After month 28 no further differences between the two groups are observed.

Figure 5

The evidence on group differences in the spacing of births perfectly fits our hypothesis. To ensure that a further parental leave can be taken up without the necessity to return to work a woman has to get an additional child in time. This explains why the additional birth hazard is so much higher in the treated group during months 21 to 24. Note further that additional child hazard rates during the first four months after parental leave has lapsed are high, both for the control group (during months 13 to 16) and for the treated group (months 25 to 28). This pattern can also be rationalized by the parental leave rules. Recall that the rules grant automatic renewal of parental leave without any new work requirement provided the mother a new maternity protection period starts within 14 weeks after the formal termination of the previous leave. Hence the higher hazard between during months 25 to 27 is the results of incentives created through by the possibility of automatic renewal.

Figure 6 translates the additional child hazard into the probability that a woman has had an additional child at some date t (1 minus the "survivor" function). The probability is higher for controls up to month 21, but then the situation turns and the treated group has the higher number of birth. The difference accumulates to more than 5 percentage points, and holds at 4.7 percentage points at month 36.

Figure 6

Our goal is to shed new light on the *joint* behavior of woman's fertility and return-to-work decisions and how parental leave legislation interferes with these decisions. In this context, it is interesting to see how birth events change mothers' work behavior and how such behavior is affected by parental leave rules. The graphs in Figure 7 show the proportion of women that have returned to work up to month t. We see that about 10 percent of all mothers return to work very quickly. However, most other mothers stay off work during the first year after a birth. Between months 2 and 11 the percentage working is still well below 20 percent, both for the treated and the controls. In month 12, when parental leave lapses for the controls, we observe a substantial jump in the fraction of women who are back at work. The ratio increases from 19 percent at the end of month 11 to 42 percent at the end of month 12. At that date, the percentage women back at work increases only slightly for the treated group increasing to values slightly above 20 percent. The end of parental leave, however, leads to a substantial jump for the treated group, from about 25 percent at the end of month 23 to more than 40 percent at the end of month 24. After parental leave has run out, we see a continuous increase in the percentage woman back at work which accumulated to about 10 percentage points between months 24 and 36, for both the treated and the controls.

Figure 7

Many women do not return to work because they want to have a further child. In order to learn something about the extent to which parental leave legislation keeps mothers from working we have to look at the percentage mothers who *neither returned to work nor had a further child* (Figure 8). This indicator is shaped mainly by return-to-work behavior at early months but increasingly by fertility behavior at later months. For both groups this fraction is large until month 11 whereas between months

12 and 23 the percentage women off work without a further child is substantially smaller for the controls than for the treated. However, after month 24 more women among the treated havegot an additional child so that there is a smaller percentage of treated women who neither have job nor a child during the third year following June/July 1990.

Figure 8

We can look into the interactions between birth and return-to-work behavior in more detail by focusing on the difference between treated and controls with respect to the joint distribution of the additional-child transition and the return-to-work transition during the three years after birth. Table 5 shows that the big difference between the two groups, 13.2 percentage points, concerns the fraction of mothers who got an additional child but who did not return to work. This is perfectly compatible with the incentives implied by automatic benefit renewal. Mothers who gave birth to an additional child between the previous child's first and second birthday are still eligible to parental leave benefits 36 months after the previous birth. In contrast the fraction of women who had an additional child and who already returned to work is 8.6 percentage points lower among the treated. There is only a minor difference in the fraction of women who returned to the job without an additional child. Finally, the fraction of women without a child who did not return to work is 3.5 percentage points smaller among the treated than among the controls 36 months after the June/July birth.

Table 5

Figures 5 to 8 discuss in detail the spacing between births and jobs during the 3 years following a first child. However, we did not account for observed characteristics, which may potentially affect the differences between the two groups. Moreover, the effect of extended parental leave on the space between births is ambiguous since extended parental leave tends to decrease the additional child hazard in the first 18 months after birth increasing it only thereafter (Figure 5). Table 6 provides a regression analysis that investigates the net effect of extended parental leave on the space between births, on the duration until women return-to-work, and on the duration of the time period without birth nor job. In column 1, the dependent variable in these regressions is the number of months that a mother is observed without a further child during the 36 months following the birth (the spacing between births). In columns 2 and 3 the dependent variables are, respectively, the number of months a woman spends off work (the spacing of jobs) and the number of months a woman stays at home with a single child.[11]

Table 6

The more generous parental leave rules that were introduced in July 1990 decreased the average distance between births by .464 months. This is clearly the effect of two changes that go in opposite directions. On the one hand, women get more children during the 36 months following the birth. On the other hand, for women who plan an additional birth under the new rules, the spacing can become

[11]It can be shown that these regressions measure the area between the respective survivor functions in Figure 6, Figure 7, and Figure 8. Intuitively, this is true because expected duration of stay in a state is the integral with respect to elapsed duration t of the probability of surviving in that state until t.

bigger without loosing continuous access to parental leave benefits. The net effect is a slight decrease in the spacing between subsequent birth during the first 36 months after the first birth.

In contrast to the spacing between birth, the spacing between jobs is the result of two effects that go in the same direction. On the one hand, under the new rules there is smaller fraction of women returning to work within 36 months following the first birth. This increases the average length of time off work. On the other hand, to get continuous access to parental leave benefits the spacing between jobs for women who are giving birth to a child can be longer under the new rules. In sum, the spacing between jobs is much larger under the new rules. The coefficient in column 2 of Table 6 suggests that treated mothers stay almost 4.7 months longer off work than control mothers (with identical observable characteristics).

Similarly, the number of months a woman spends off work without having a further child is 2.9 months higher under the new rules (see column 3 in Table 6). While a smaller fraction women with no additional child is still out of labor force after 36 months, the average duration in such circumstances is much smaller under the old rules during the 36 months after birth. The point estimates are rather robust. Just like in previous regressions, excluding control variables does neither change the point estimates not levels of significance.

7 Shortening maximum parental leave: the 1996 reform

We have seen that the 1990 parental leave reform had substantial effects on women's fertility and labor supply behavior after their first birth. It is interesting to check whether we see effects of comparable magnitude once the maximum duration of parental leave is again reduced. The 1996 policy reform brought such a reduction by six months. (Recall that the maximum duration of parental leave continued to terminate with the child's second birthday, but now at least six months had to be take by either parent. As almost no fathers take up parental leave, the effective maximum duration to be taken up by the mother typically ended when the child's became 1 1/2 years old).

We now study the impact of this effective *reduction* in maximum parental leave on women's fertility and work behavior. In in Panel A of Table 7, we present the coefficients estimated from the linear probability model using the additional-child indicator (row 1), the return-to-work indicator (row 2), and the either-child-or-work indicator (row 3) as the dependent variable. Panel B of Table 7 reports the corresponding estimates for the corresponding durations until these events. For ease of comparison the first column in Table 7 reproduces the results from the effects of the twelve-months extension of parental leave presented above. The second column in Table 7 present the coefficients of our main interest.

Table 7

Interestingly, our results show that shortening maximum parental leave duration by six months has no impact on the probability of having an additional child. 36 months after a previous birth, the probability of having an additional child is the same for treated and controls (conditional on observed characteristics). Furthermore, the spacing between subsequent birth occurs .271 months earlier - although the coefficient is not statistically significant (see the coefficient in panel B, first row, second column).

We note that these two results can be rationalized by the new rules introduced with the 1996 policy change. Parents who plan an additional child and who want to take advantage of continuous take-up of parental leave have to make sure that the additional birth occurs before the first child becomes 18 months plus 14 weeks old. Under the 1990 rules this had to occur before the first child become 24 months plus 14 weeks old. In other words, the 1996 rules introduced a tighter time constraint for parents planning to combine an additional child with continuous take-up of parental leave benefits. While the time constraint is tighter under the 1996 rules, it is still biologically feasible to meet this constraint (and different from for the pre-1990 rules where continuous take-up was biologically almost impossible). Figure 9 shows that, in fact, the share of mothers with an additional child is higher under the 1996 rules as compared to the 1990 rules between months 14 and 28. Furthermore from inspection of the hazard rates (not shown here) it is obvious that this difference is due to substantially higher additional birth hazards during months 14 to 18, the months immediately before maximum parental leave terminate under the 1996 rules.

Figure 9

It is further interesting to analyze how the policy change affected the return-to-work process. Table 5 (panel A row 2 column 2) it is shown that the percentage mothers back at work after 36 months is 5.4 percentage points higher under the 1996 rules as compared to the 1990 rules. Interestingly, our estimate of the six months reduction in maximum parental leave duration is about half the size as the estimate we got for the twelve months increase in maximum duration (see column 1). This near-linearity between maximum parental leave duration and labor supply is further confirmed by our job-spacing regressions (panel B, row 2). The 1996 lead to a reduction in the average time out of labor force (spacing between jobs) of about 2.5 months. This compares to an increase in time distance between jobs of 4.7 months – as a result of the 12 months extension of maximum parental leave duration with the 1990 rules. In sum our estimates suggest that, per additional month of maximum parental leave duration, the percentage mothers back to work within 36 months decreases by about 0.8 to 0.9 percentage points; and the average duration off work increases by 0.4 to 0.5 months.

The share of women still off work without a further child after 36 months is not significantly different under the 1996 rules as compared to the 1990 rules. While more mothers get back to work, no change in fertility is observed. However, as shown in panel B, the new rules significantly reduce the time that mothers spend at home without an additional child is reduced by 2.35 months.

8 Conclusions

This paper presents new evidence on the effect of parental leave policies on fertility and return-to-work behavior. To estimate the causal impact of parental leave rules we exploit a policy change in Austria, enacted on July 1, 1990 which increased the maximum duration of parental leave from the child's first to the child's second birthday. An important feature of the Austrian parental leave system – which plays a central role in our empirical strategy – is the possibility of automatic renewal of parental leave benefits as long as the mother is still on parental leave from a previous child. The policy change created a strong incentive for women planning two or more children to chronologically "bunch" births and the associated time off work. Moreover, by strongly reducing the costs of additional children, the policy change did not only create an incentive to decrease the space between births, but also to increase the number of birth.

To assess the causal effect created by the policy change through automatic parental leave renewal, we adopt a regression discontinuity approach. The assumptions needed for this approach are satisfied in an almost ideal way in the present context. First, the policy change was enacted on July 1, 1990, at 0:00 am, so that the regression discontinuity design is "sharp" (in the sense of Hahn et al., 2001). Our estimates rely on a comparison of subsequent fertility and return-to-work behavior of mothers with a birth in June 1990 to those with a birth in July 1990 so that our comparison is in the close "neighborhood" of the treatment cut-off. Second, assignment to treatment is purely random, as parents with children born in June/July 1990 could not anticipate the parental leave policy change. Exact date and details of the change were not clear until early April 1990 – but at that date the June/July 1990 children were already conceived. Third, while our control/treatment samples is based on pre-post policy births, the conditions under which all mothers in our sample made their subsequent fertility and return-to-work decisions are almost identical. Most of our analysis focuses on behavior during the 36 months following the June/July 1990 birth. Hence treated and control mothers face identical conditions during 35 out of 36 months.

Our findings indicate that the Austrian 1990 parental leave reform had a strong impact on the probability of having an additional child. The additional-child probability is significantly higher for the treated than for the controls, both in the short- and in the long-run. During the first three years following the 1990 birth, this probability is about 5 percentage points (or 15 percent) higher for the treated; and during the first 10 years the probability is still about 4 percentage points higher for the treated. This suggests that the parental leave reform did not only decrease the spacing but did also increase the number of births.

Our estimates of the effects of the parental leave reform on fertility decisions are a lower bound. Our estimated effect is informative on the causal effect of incentives created by automatic parental leave renewal. The overall effect of the policy change, however, is likely to be larger. The only difference is that the maximum parental leave duration for the *1990 birth* ends with the child's second treated mothers whereas it already ends with the child's first birthday for control mothers. In the present context, the more generous post-reform rules apply to both treated and control mothers for any *subsequent birth*. However, the overall effect should also include the direct effect of job protection and longer benefit duration. To the extent that these have a direct effect on fertility, the overall effect of the policy change will be larger.

Our analysis also evaluates a subsequent policy change that took place in 1996 which effectively reduced the maximum duration of parental leave from 24 months to 18 months. We find that this policy change did not lead to a change in the additional-child probability of the treated mothers. This suggests that the main effect of the policy change works via the automatic-renewal meachanism built in the Austrian parental leave system. Getting an additional child within 18 months is biologically possible and mothers took advantage of this possibility after the 1996 reduction in parental leave. However, this effect refers to the short-run. It may well be that the reduction had long-run effects on fertility choices.

(As our data end in 2001 the long-run fertility effect cannot be estimated).

Finally, both policy changes had a significant impact on return-to-work behavior. Per additional month of maximum parental leave duration, the time off work increases by 0.4 to 0.5 months. No major difference between the effects of the 1990 increase and the 1996 decrease in maximum parental leave can be detected.

Suppose the policy objective is to maximize the number of births and to minimize the amount of time that women spend off work. Under such objectives, our results suggest that the rules introduced in 1996 optimize on this objective. Clearly, policy objectives are not as simplistic and many more dimensions play a role. On the one hand, the amount of time mothers spend with their children might have a significant impact on child development – which would call for a longer duration of parental leaves (for evidence along these lines see Berger, Hill, and Waldfogel, 2005, Gregg, Washbrook, Propper, and Burgess, 2005, and Tanaka 2005). On the other hand, a long duration of parental leave might significantly weaken the labor market position of mother or mothers to be (either because after returning back to work mothers are more likely to be fired by their employers as soon as the job protection period has expired; or because employers may anticipate the higher costs of longer parental leaves and may become more reluctant in the first place to hire women with high probabilities of childbearing).

Furthermore, it is als important to note that parental leave legislation is just one of a broad set of fertility-related family policies. We have studied two successive policy changes during a period where no significant changes in other fertility-related family policies took place. While this is a nice set-up for policy evaluation we have to be cautious in extrapolating our results to other environments. There may be important interaction effects with other policies, in particular supply of child care facilities for very small children (or lack thereof, as in the case of Austria); or other financial incentives (such child benefits, tax deductions for children, and social transfers directly related to a birth – where Austrian family policies are comparably generous). How an isolated variation in parental leave policies affect women's behavior will depend on a country's particular family-policy mix. Our analysis shows, however, that parental leave provisions are a potentially very important element in the family policy / fertility nexus.

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Figure 1:
The Timing of Maternity Protection (MP) and Parental Leave (PL) in Austria

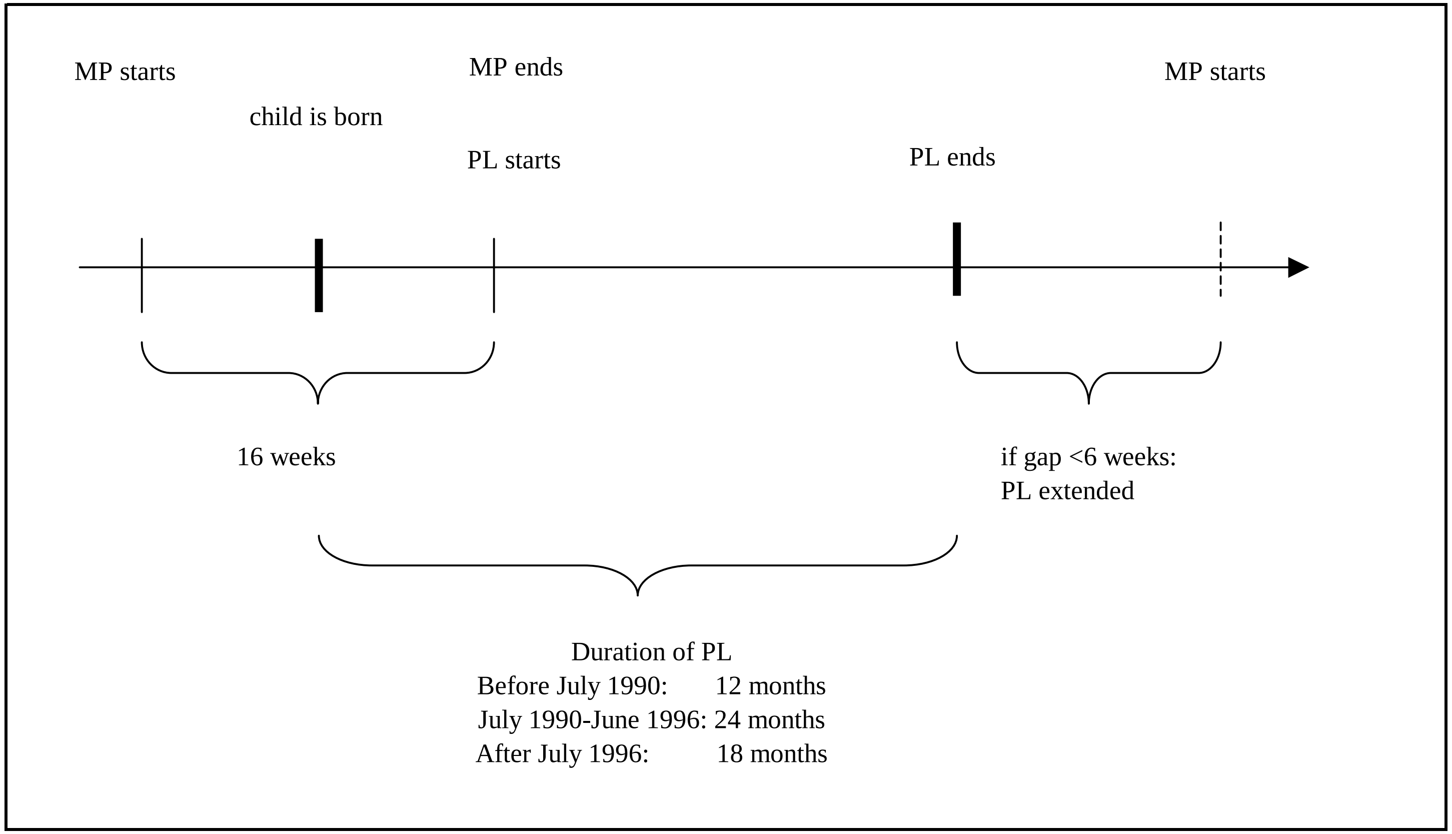

Figure 2:
Children Born and Parental Leave Duration, Austria 1987-1999

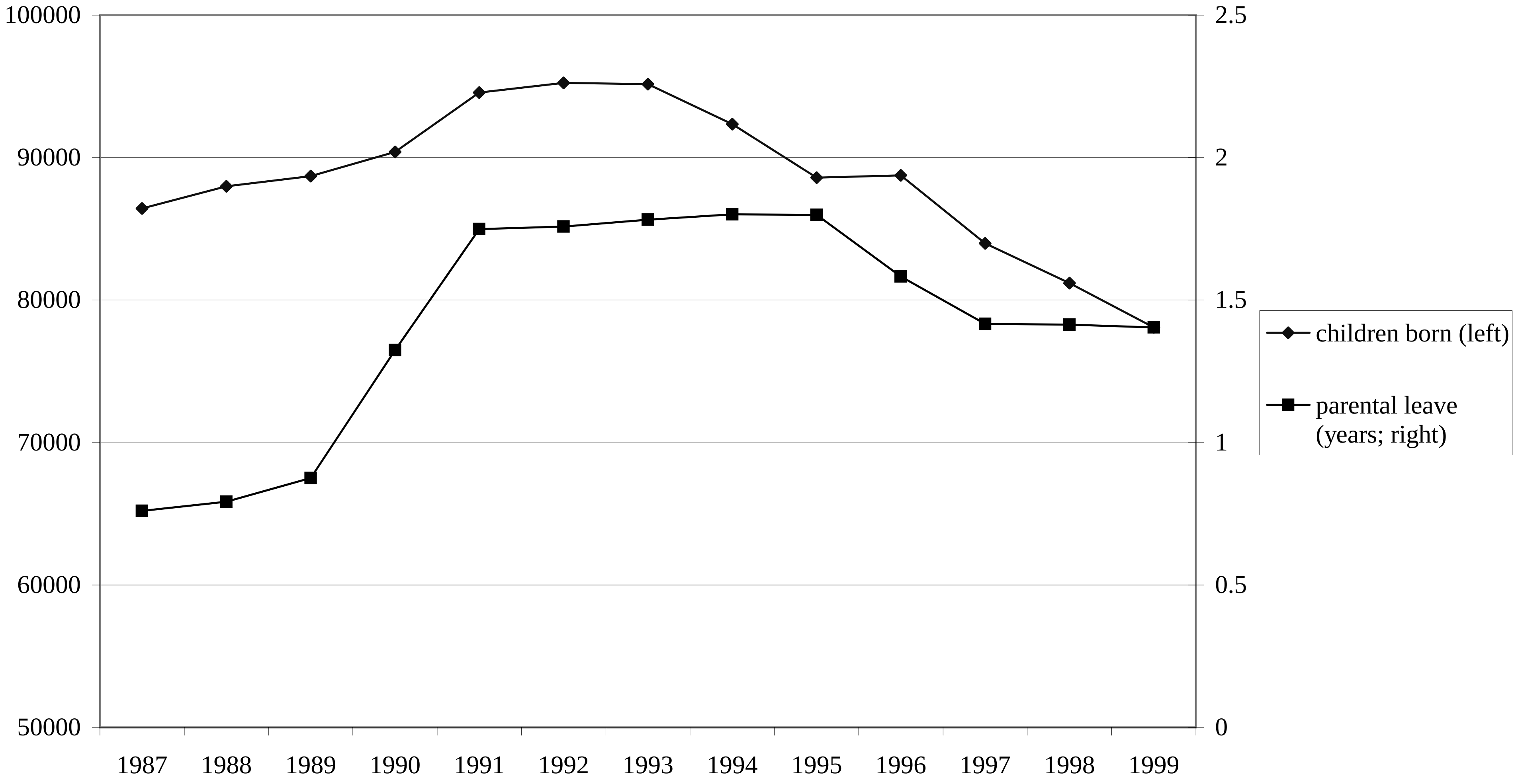

Figure 3:
Parental Leave Duration (Years) in the Discontinuity Sample

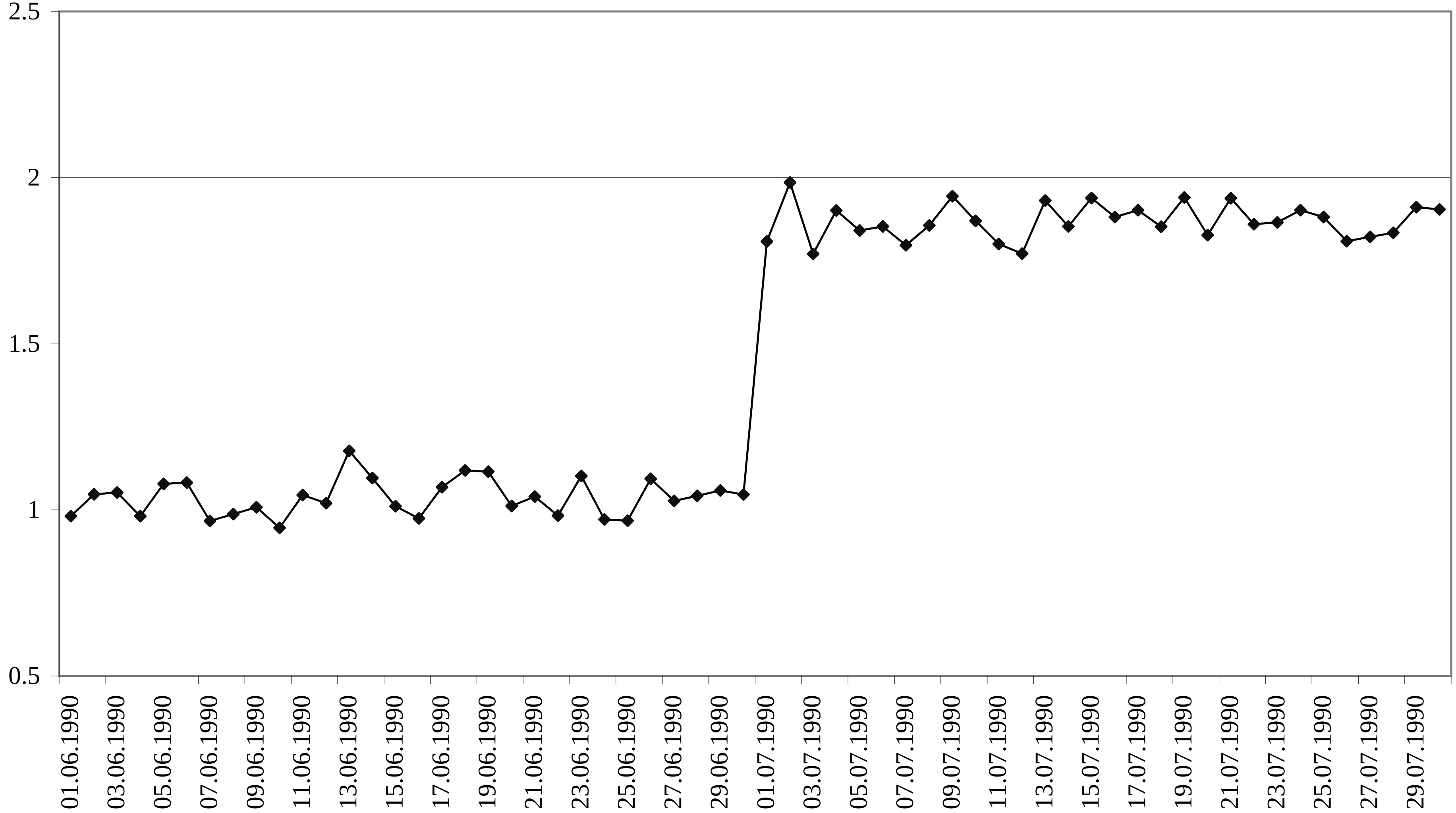

Figure 4:
The Probability of an Additional Birth Within 3 Years
14 Day Moving Average
(Backward in June, Forward in July)

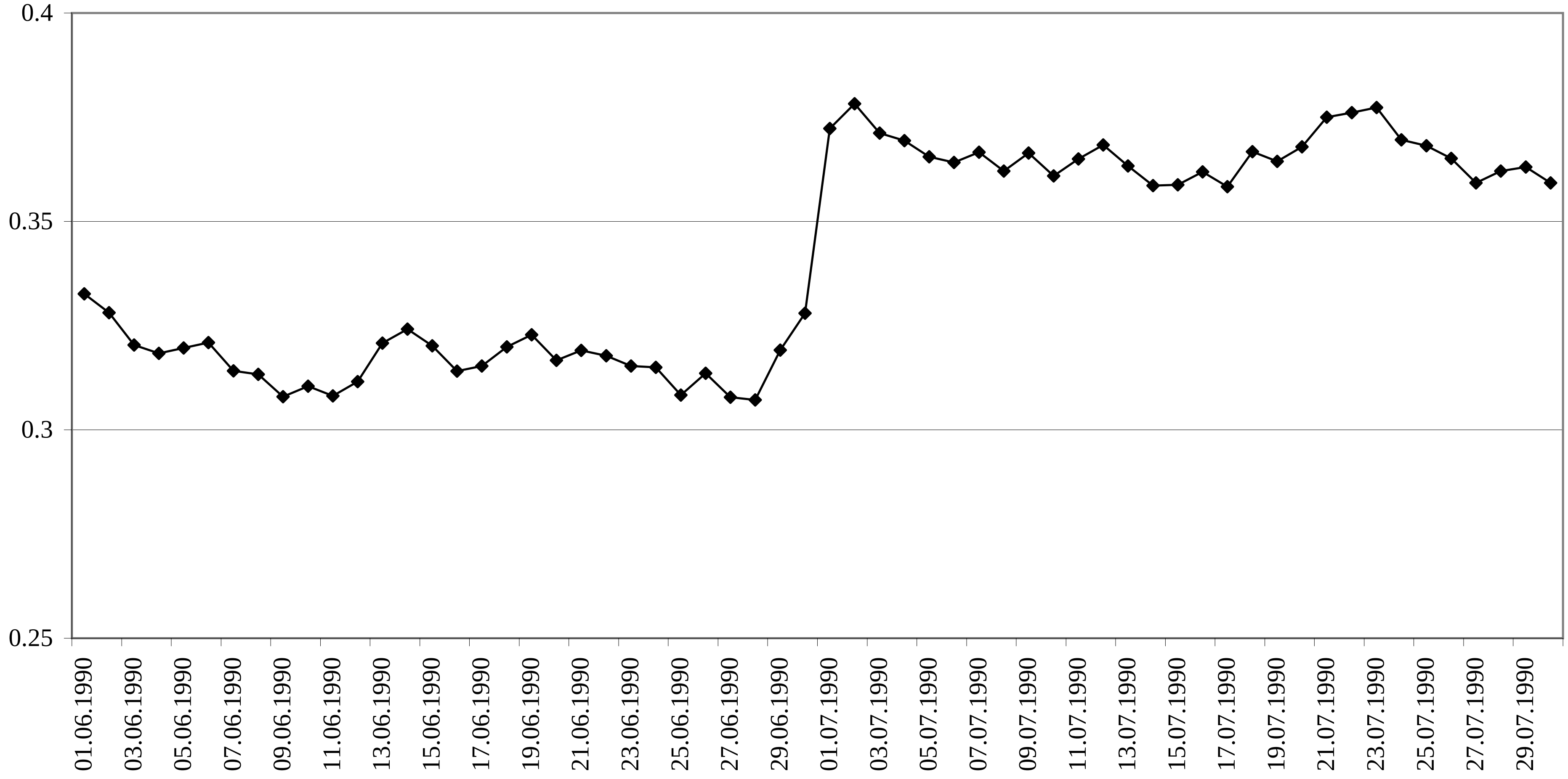

Figure 5:
The Additional Birth Hazard when Parental Leave is Extended

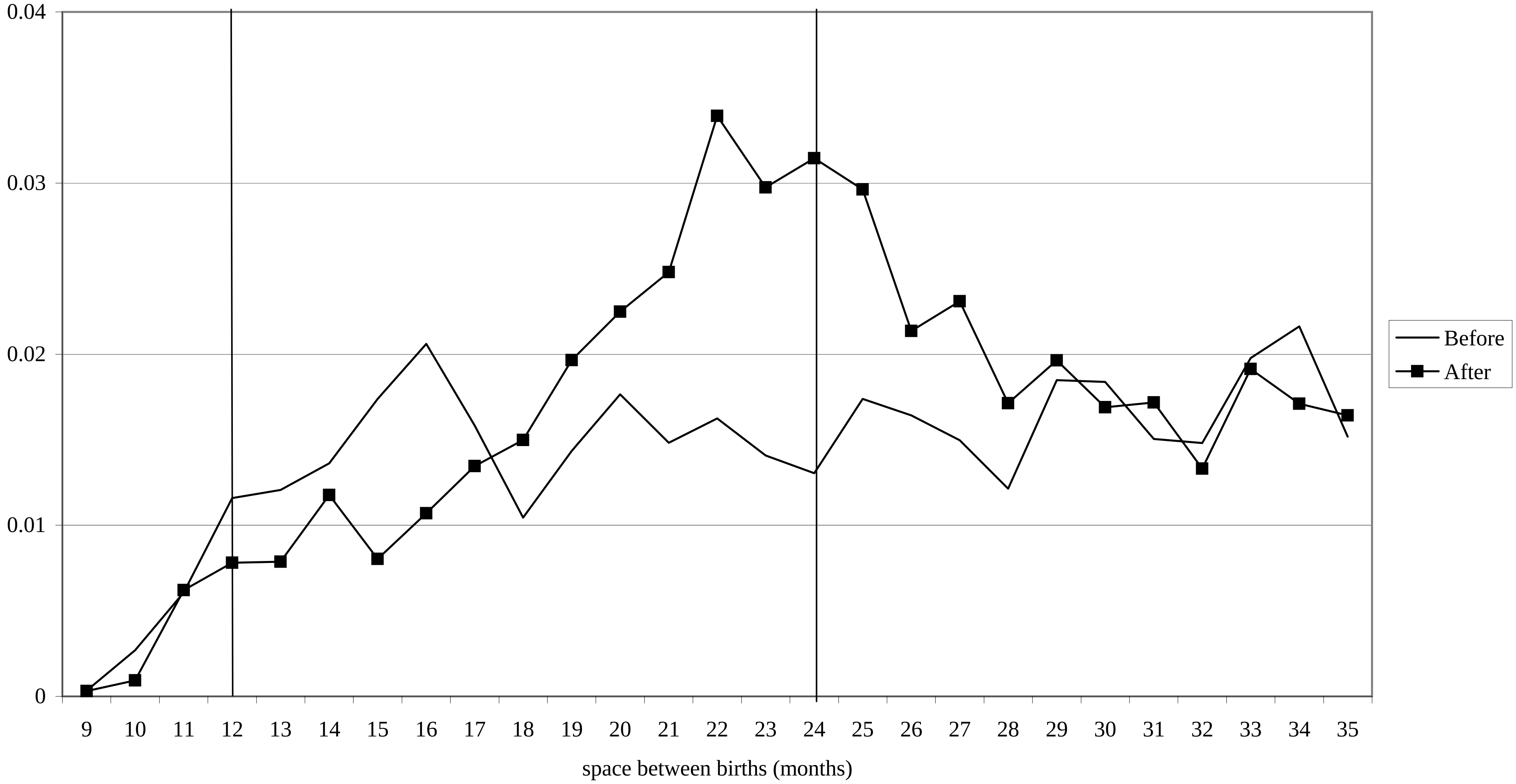

Figure 6:
The Share with an Additional Birth when Parental Leave is Extended

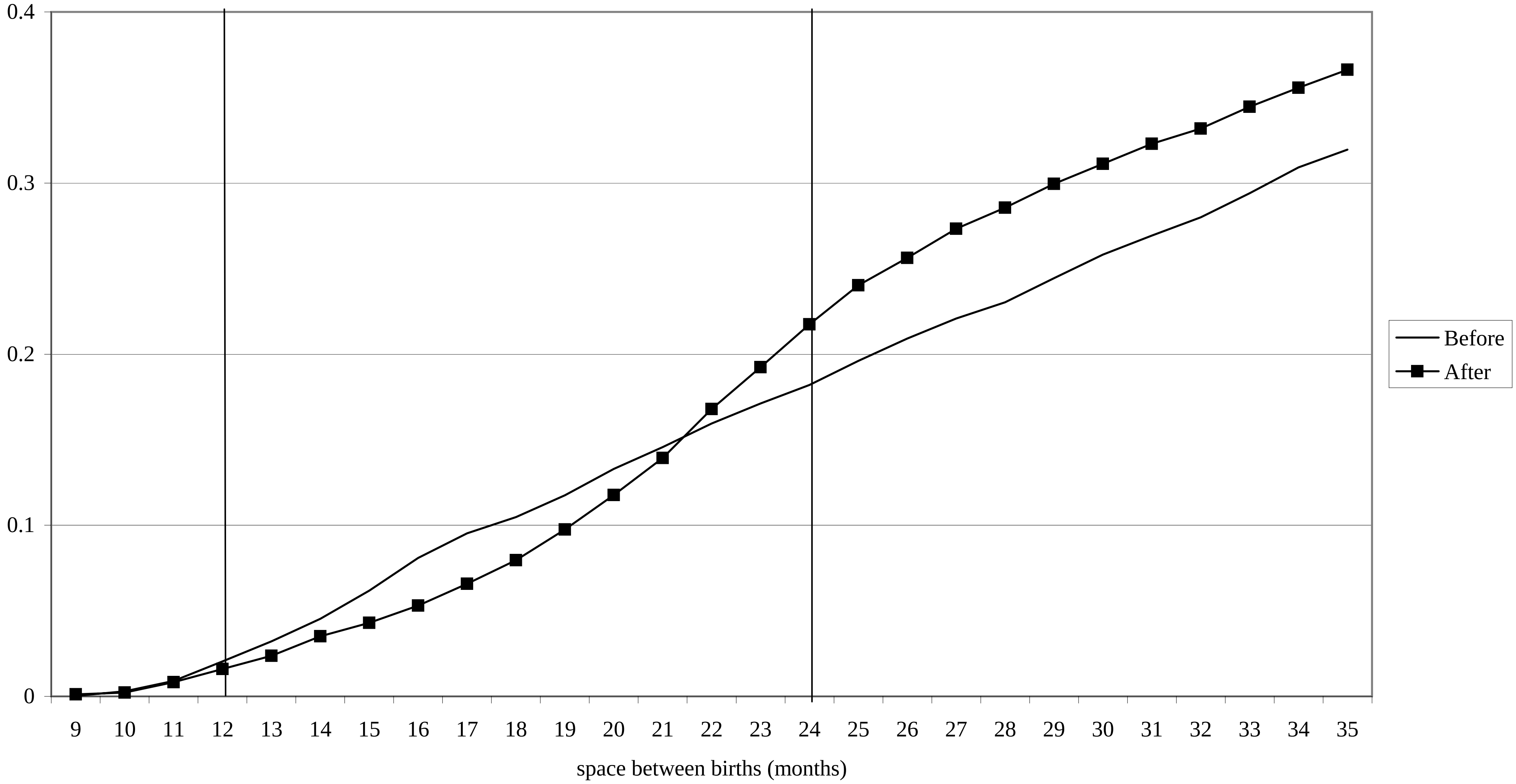

Figure 7:
The Share Returning to Work when Parental Leave is Extended

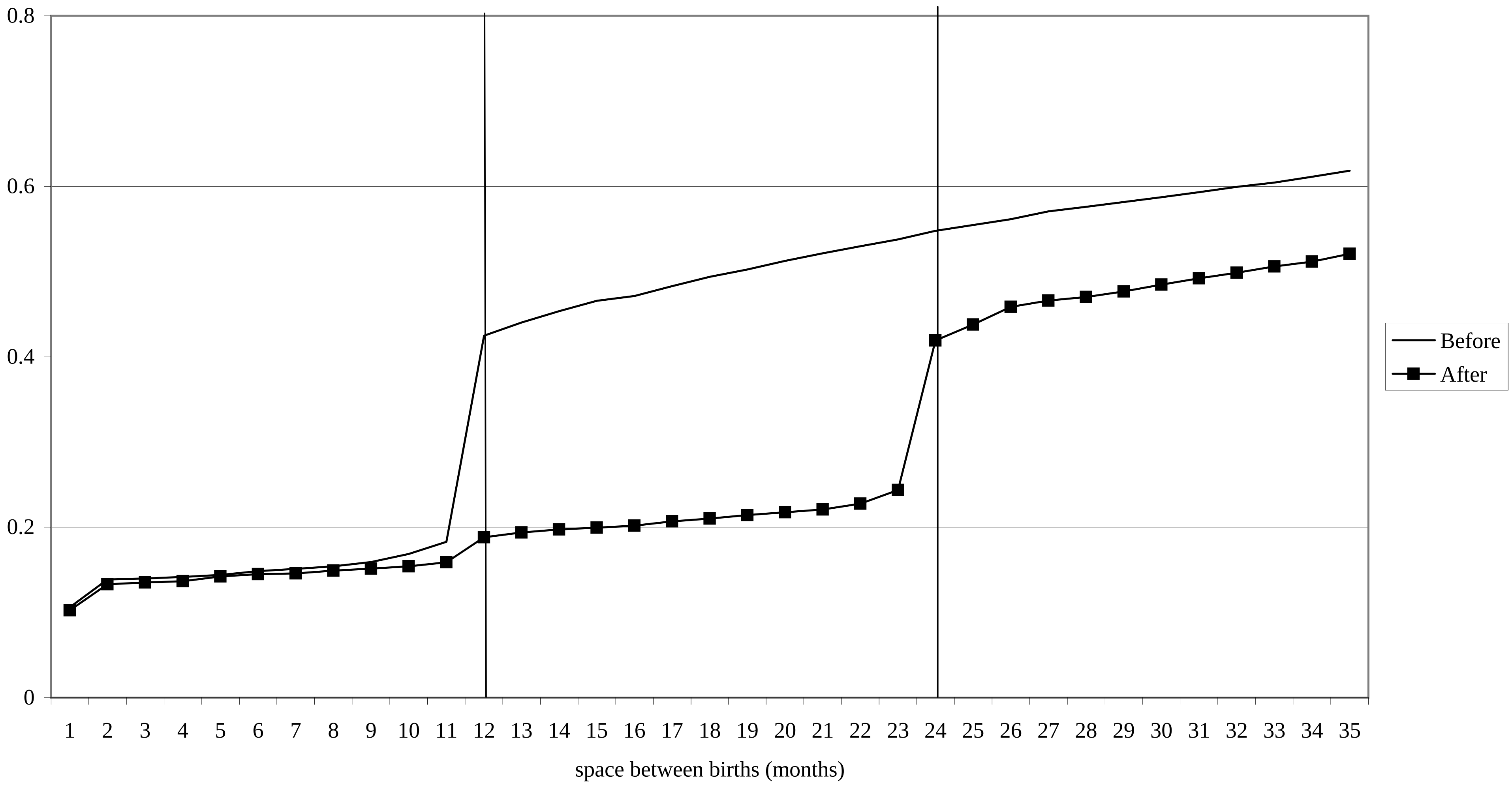

Figure 8:
The Share with Neither Birth nor Return to Work when Parental Leave is Extended

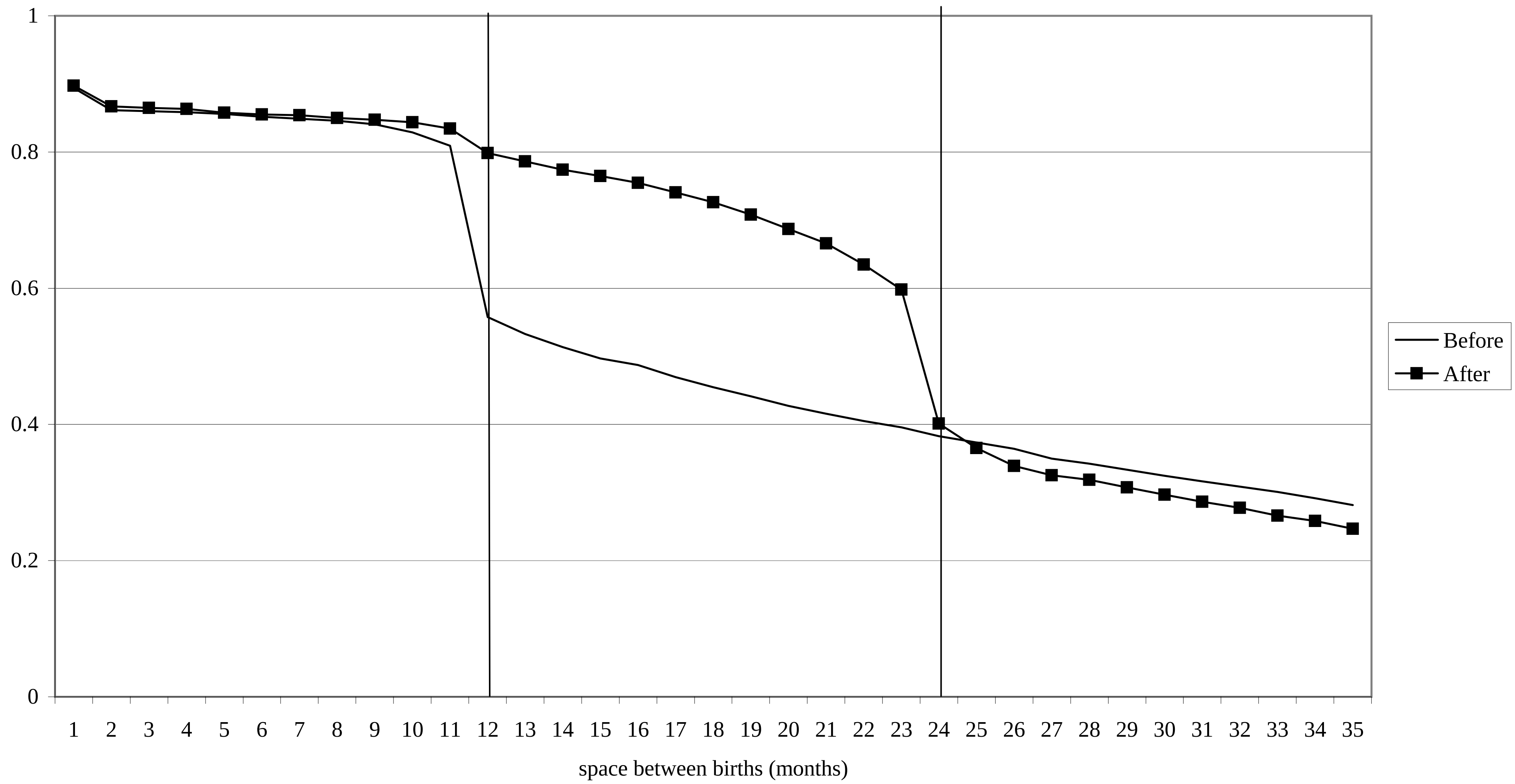

Figure 9:
The Share with an Additional Child when Parental Lave is Reduced

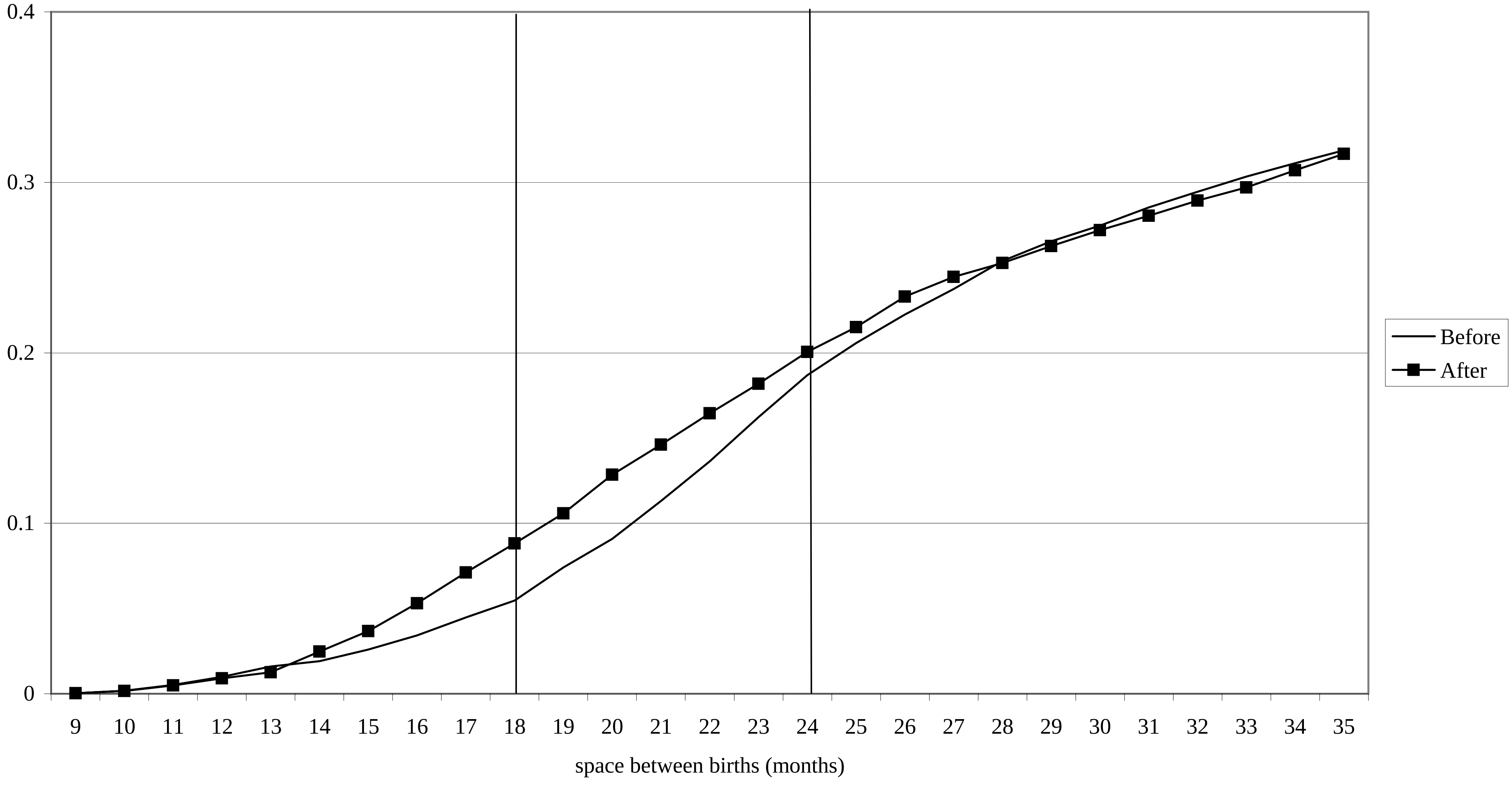

Table 1:
Descriptive Statistics of the Discontinuity Sample

		Before		After	
Parental Leave (Years)		1.037	(0.542)	1.869	(0.560)
Age					
	15-19	0.102		0.093	
	20-24	0.440		0.422	
	25-29	0.341		0.347	
	30-34	0.087		0.108	
	35-39	0.029		0.030	
Employment Prior to Birth (Years)		1.548	(0.485)	1.541	(0.491)
Unemployment prior to Birth (Years)		0.116	(0.268)	0.116	(0.269)
Wage in Previous Job (Nominal 100 AS)		4.887	(2.229)	4.894	(2.251)
Wage Missing		0.016		0.020	
White Collar		0.565		0.560	
Region					
	Vienna	0.217		0.213	
	Lower Austria	0.142		0.129	
	Burgenland	0.019		0.027	
	Upper Austria	0.150		0.152	
	Styria	0.094		0.086	
	Carinthia	0.076		0.069	
	Salzburg	0.074		0.077	
	Tyrol	0.092		0.090	
	Vorarlberg	0.042		0.050	
	Missing	0.093		0.108	
Industry					
	Food	0.040		0.033	
	Textiles	0.066		0.071	
	Construction	0.018		0.021	
	Trade	0.205		0.188	
	Recreation	0.100		0.105	
	Finance	0.039		0.031	
	Cleaning	0.047		0.042	
	Health	0.074		0.080	
	Education	0.020		0.023	
	Social Insurance	0.103		0.105	
	Other / Missing	0.287		0.301	
N(obs.)		2979		3239	
	Total				
	Day 1-15	1466		1546	
	Day 16-30	1513		1693	

Notes: Standard Deviation in Parentheses. Before refers to eligible women giving birth to a child in the period from June 1,1990 to June 30, 1990. After refers to eligible women giving birth to a child in period from July 1, 1990 to July 30, 1990.

Source: Own Calculation, based on discontinuity sample from ASSD.

Table 2:
The Effect of Extended Parental Leave Duration on Fertility and Labor Supply

Three Years after First Birth	(1) Birth	(2) Birth	(3) Job	(4) Job	(5) Neither	(6) Neither
After (July 1 - July 30, 1990)	0.047	0.049	-0.097	-0.097	-0.035	-0.037
	(0.012)***	(0.012)***	(0.013)***	(0.012)***	(0.011)***	(0.011)***
Age (15-19 Years)						
20-24 Years		0.051		-0.107		0.058
		(0.022)**		(0.023)***		(0.020)***
25-29 Years		0.035		-0.111		0.092
		(0.023)		(0.025)***		(0.022)***
30-34 Years		-0.071		-0.161		0.180
		(0.027)***		(0.030)***		(0.027)***
35-44 Years		-0.164		-0.100		0.163
		(0.034)***		(0.042)**		(0.039)***
Employment Prior to Birth (Years)		0.004		0.085		-0.065
		(0.015)		(0.016)***		(0.015)***
Unemployment prior to Birth (Years)		-0.006		-0.140		0.100
		(0.026)		(0.028)***		(0.027)***
White Collar		0.003		0.080		-0.063
		(0.016)		(0.016)***		(0.015)***
Wage in Previous Job (Nominal 100 AS)		0.007		0.014		-0.009
		(0.003)**		(0.004)***		(0.003)***
Region	No	Yes	No	Yes	No	Yes
Industry	No	Yes	No	Yes	No	Yes
Constant	0.320	0.202	0.618	0.558	0.282	0.355
	(0.009)***	(0.038)***	(0.009)***	(0.040)***	(0.008)***	(0.037)***
Observations	6218	6218	6218	6218	6218	6218
R-squared	0.00	0.03	0.01	0.08	0.00	0.05

Notes: Robust standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.
Source: Own Calculation, based on discontinuity sample from ASSD.

Table 3:
Sensitivity Analysis

Three Years after First Child Was Born	(1) Birth	(2) Job	(3) Neither
A. Baseline Results: July 1-30, 1990 vs June 1-30, 1990	0.049	-0.097	-0.037
	(0.012)***	(0.012)***	(0.011)***
B. Seasonality: July 1-30, 1989 vs June 1-30, 1989	-0.011	0.008	-0.009
	(0.012)	(0.012)	(0.011)
C. Long-term Effects: Ten Years after First Child Was Born	0.039	-0.018	-0.024
	(0.012)***	(0.009)**	(0.006)***
D. Trend: July 1993 vs July 1990	-0.029	0.001	0.000
	(0.012)**	(0.012)	(0.011)

Notes: Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. Each cell in table is the "After" coefficient from a separate regression.

Source: Own Calculation, based on discontinuity sample from ASSD.

Table 4:
Sensitivity Analysis: Births in first / second half of month

	After Period	
	July 1 - July 15	July 16 - July 30
Before Period		
June 1 - June 15	0.050	0.047
	(0.017)	(0.017)
June 16 - June 30	0.050	0.047
	(0.017)	(0.017)

Notes: Table reports the coefficient that measures the change in the probability of an additional child when comparing the "After Period" to the "Before Period" in a regression controlling for control variables listed in Table 2.

Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%.

Source: Own Calculation, based on discontinuity sample from ASSD.

Table 5:
Interactions between birth and return-to-work behavior

Three Years after First Child Was Born	Before	After	Difference
Neither additional birth nor return-to-work	28.16	24.67	-3.49
No additional birth and return-to-work	39.88	38.72	-1.16
Additional birth but no return-to-work	10.00	23.25	13.25
Additional birth and return-to-work	21.95	13.37	-8.58
Total	100.00	100.00	0.00

Notes: Table reports the joint distribution between the first birth and first return-to-work transition during the three year period after first child was born. Independence rejected at all conventional levels of significance ($p<.001$). MNL analysis indicates that log odds of "Additional birth no return-to-work" are singificantly higher and "additional birth and return-to-work" are significantly lower than the baseline "neither additional birth nor return-to-work" after extending parental leave.

Source: Own Calculation, based on discontinuity sample from ASSD.

Table 6:
The Effects of Extended Parental Leave Duration on the Spacing of Births and Jobs

	(1)	(2)	(3)
Space between first birth and	Subsequent Birth	Subsequent Job	Subsequent Birth or Job
After (July 1 - July 30, 1990)	-0.464	4.697	2.919
	(0.178)***	(0.298)***	(0.283)***
Age (15-19 Years)			
20-24 Years	-0.558	4.288	3.613
	(0.331)*	(0.601)***	(0.565)***
25-29 Years	-0.243	3.589	3.381
	(0.348)	(0.631)***	(0.597)***
30-34 Years	0.894	4.245	4.528
	(0.410)**	(0.757)***	(0.724)***
35-44 Years	1.983	3.389	4.220
	(0.522)***	(1.095)***	(1.062)***
Employment Prior to Birth (Years)	0.024	-2.005	-1.745
	(0.233)	(0.418)***	(0.403)***
Unemployment prior to Birth (Years)	0.064	4.499	4.041
	(0.409)	(0.636)***	(0.635)***
White Collar	0.172	-2.495	-2.171
	(0.243)	(0.393)***	(0.373)***
Wage in Previous Job (Nominal 100 AS)	-0.073	-0.504	-0.419
	(0.050)	(0.094)***	(0.088)***
Region of Employer	Yes	Yes	Yes
Industry	Yes	Yes	Yes
Constant	32.950	21.322	19.925
	(0.584)***	(1.055)***	(1.011)***
Observations	6218	6218	6218
R-squared	0.02	0.19	0.16

Notes: Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. Space is measured in months and censored at 36 months.

Source: Own Calculation, based on discontinuity sample from ASSD.

Table 7:
Comparing The Extension to The Reduction of Parental Leave Duration

	Extending Parental Leave from 1st to 2nd birthday	Reducing Parental Leave from 2nd birthday to 18 months
A. Three years after first birth		
Share with additional child	0.049	0.000
	(0.012)***	(0.011)
Share with job	-0.097	0.054
	(0.012)***	(0.012)***
Share with neither	-0.037	-0.014
	(0.011)***	(0.009)
B. Space between first birth and subsequent		
Birth	-0.464	-0.271
	(0.178)***	(0.170)
Job	4.697	-2.541
	(0.298)***	(0.277)***
Birth or Job	2.919	-2.354
	(0.283)***	(0.251)***

Notes: Robust standard errors in parentheses, * significant at 10%; ** significant at 5%; *** significant at 1%. Each cell in table is the "After" coefficient from a separate regression.

Source: Own Calculation, based on discontinuity sample from ASSD.

Table A1:
Full Results, Table 2

	(1)	(2)	(3)	(4)	(5)	(6)
3 Years After First Child Was Born	Child	Child	Job	Job	Neither	Neither
After (July 1 - July 30, 1990)	0.047	0.049	-0.097	-0.097	-0.035	-0.037
	(0.012)***	(0.012)***	(0.013)***	(0.012)***	(0.011)***	(0.011)***
Age (15-19 Years)						
20-24 Years		0.051		-0.107		0.058
		(0.022)**		(0.023)***		(0.020)***
25-29 Years		0.035		-0.111		0.092
		(0.023)		(0.025)***		(0.022)***
30-34 Years		-0.071		-0.161		0.180
		(0.027)***		(0.030)***		(0.027)***
35-44 Years		-0.164		-0.100		0.163
		(0.034)***		(0.042)**		(0.039)***
Employment Prior to Birth (Years)		0.004		0.085		-0.065
		(0.015)		(0.016)***		(0.015)***
Unemployment prior to Birth (Years)		-0.006		-0.140		0.100
		(0.026)		(0.028)***		(0.027)***
White Collar		0.003		0.080		- 0.063
		(0.016)		(0.016)***		(0.015)***
Wage in Previous Job (Nominal 100 AS)		0.007		0.014		-0.009
		(0.003)**		(0.004)***		(0.003)***
Region of Employer (Missing)						
Vienna		0.008		-0.052		0.007
		(0.027)		(0.027)*		(0.024)
Lower Austria		0.113		-0.064		-0.021
		(0.029)***		(0.029)**		(0.025)
Burgenland		-0.027		-0.013		0.003
		(0.044)		(0.045)		(0.041)
Upper Austria		0.051		-0.136		0.059
		(0.028)*		(0.028)***		(0.026)**
Styria		0.019		-0.151		0.081
		(0.031)		(0.031)***		(0.029)***
Carinthia		-0.013		-0.061		0.016
		(0.032)		(0.032)*		(0.029)
Salzburg		0.013		-0.198		0.123
		(0.032)		(0.033)***		(0.031)***
Tyrol		-0.002		-0.145		0.099
		(0.031)		(0.031)***		(0.029)***
Vorarlberg		0.179		-0.163		-0.001
		(0.038)***		(0.038)***		(0.033)
Industry (Other / Missing)						
Food		0.017		0.013		-0.011
		(0.034)		(0.035)		(0.032)
Textiles		0.028		-0.048		0.018
		(0.028)		(0.029)*		(0.027)
Construction		0.027		0.028		0.002
		(0.045)		(0.046)		(0.043)
Trade		0.013		-0.049		0.031
		(0.020)		(0.021)**		(0.019)
Recreation		0.042		0.059		-0.053
		(0.025)*		(0.026)**		(0.024)**
Finance		-0.004		0.064		-0.027
		(0.034)		(0.035)*		(0.032)
Cleaning		0.015		0.049		-0.031
		(0.032)		(0.035)		(0.032)
Health		0.043		0.097		-0.088
		(0.026)		(0.026)***		(0.022)***
Education		0.019		0.199		-0.090
		(0.043)		(0.039)***		(0.035)**
Social Insurance		0.026		0.157		-0.095
		(0.024)		(0.022)***		(0.020)***
Wage Missing		0.072		0.039		-0.048
		(0.051)		(0.054)		(0.050)
Constant	0.320	0.202	0.618	0.558	0.282	0.355
	(0.009)***	(0.038)***	(0.009)***	(0.040)***	(0.008)***	(0.037)***
Observations	6218	6218	6218	6218	6218	6218
R-squared	0.00	0.03	0.01	0.08	0.00	0.05

Notes: Robust standard errors in parentheses
* significant at 10%; ** significant at 5%; *** significant at 1%
Source: Own Calculation, based on discontinuity sample from ASSD.