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Does child spacing affect children's outcomes?
Evidence from a Swedish reform

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Does child spacing affect children's outcomes? Evidence from a Swedish reform*

by

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Abstract

In this paper, we provide evidence of whether child spacing affects the future success of children. As an exogenous source of variation in child spacing, we make use of the introduction of an administrative rule in the parental leave benefit system in Sweden. This rule made it possible for a woman to retain her previous high level of parental leave benefits, i.e., 90 percent wage replacement, without entering the labor market between births provided that the interval between the births did not exceed 24 months. The rule had a much larger effect on the birth spacing behavior for native-born mothers compared to foreign-born mothers due to their differential attachment to the labor market. We find that the rule caused a reduction in spacing among native-born mothers as compared to the foreign-born mothers. For individuals born by native-born mothers, the reform also caused a decrease in educational attainment. Thus, this suggests that the effect of spacing children closer has a negative impact on children's future outcomes. We provide additional evidence that this is likely due to the strong effects of early environment on the capacity for human skill development as discussed by Knutsen et al. (2006).

Keywords: Child spacing, parental leave, child school performance
JEL-codes: J13, J18

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1 Introduction

This paper empirically investigates whether child spacing, as measured by the birth interval to the nearest younger sibling, has an effect on the older sibling's performance later in life such as educational attainment or school performance.[1] Although, there is a large literature dealing with other family characteristics, such as family size and birth order,[2] there is hardly any work that analyzes the effect of child spacing on the long term outcomes of children.[3, 4] The lack of studies about the effects of birth spacing on child outcomes is surprising given that birth rates are declining and that the average family size is below two children per family in many countries.[5] For example, the average total fertility rate is 1.8 in the OECD countries (Human Development Reports 2005), and in many countries, such as Sweden and the US, there has emerged a "two-child norm" (e.g., David and Sanderson 1987). As a result, families may differ more in the spacing of their children than they do in the number of children and therefore the timing of births is becoming a much more salient issue.

The challenge of estimating the effect of child spacing on child outcomes is, of course, to find an exogenous source of variation in birth spacing since child spacing is likely to be endogenous, i.e., the time intervals between births is partly determined by unobserved parental characteristics.[6] In this paper, we will use as an administrative rule

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[1] Specifically, we analyze the outcomes of first-born and second-born subjects, and we organize the data into families with at least 2 births for first-born and families with at least 3 births for second-born, which is important because it helps defining meaningful child-spacing effects.

[2] See Blake (1989) for book length treatment of the relationship between family size and school performance. The effect of family size on child outcomes has recently become a hot topic. Examples of very recent studies are Angrist et al. (2006), Black et al. (2005), Cáceres-Delpiano (2006), Rosenzweig and Zhang (2006), and Qian (2006). For Swedish evidence, see Grönqvist and Åslund (2007).

[3] To the best of our knowledge, there are only two studies in sociology (Powell and Steelman 1990, 1993) and two studies in economics (Stafford 1987, Holmlund 1984) that correlate measures of child spacing and school performance. However, these studies raise obvious concerns about causality since they do not use any exogenous source of variation in birth spacing. Moreover, they cannot define a meaningful child-spacing effect since their measures of child spacing are flawed. For example, Powell and Steelman use the number of siblings within a particular age range, which means that the "experiment" is not well defined, i.e., treatment occurs before the subjects exist. Furthermore, this measure confounds family size with child spacing.

[4] There is a large literature that investigates whether child spacing affects child mortality. In contrast to this study, this literature does not estimate the impact of the effect of the younger child on the outcome of the older child but the effect on the newly born child instead. See Conde-Agudelo et al. (2006) for a recent meta-study and Setty-Venugopal and Upadhyay (2002) for a survey of studies in developing countries. For a study in economics, see Duflo (1998).

[5] One possible reason for the lack of studies of child spacing on children's future outcomes is that information on child spacing is absent in most available data sets.

[6] There is a large literature in demography and in economics investigating factors related to the timing of births. For work in economics: see for example, Heckman et al. (1985), Heckman and Walker (1990), Newman (1983), and

in Sweden which came into place in 1980 as an exogenous source of variation in child spacing. This rule made it possible for women to retain their previous high level of parental leave benefits (i.e., 90 percent wage replacement) without entering the labor market between births provided that the interval between the births did not exceed 24 months.[7] This administrative rule thus gave a woman a short-term economic incentive to space her children within 24 months in order to avoid the reduction in benefits, i.e., a "speed premium" on further childbearing.[8]

We argue that this rule should *a priori* have a differential impact on child spacing behavior of women from different countries of origin due to their sharp differences in taste for work (e.g., Fernández and Fogli 2009).[9] For example, in 1980 women born in one of the Nordic countries (Sweden, Denmark, Finland, and Iceland) had the highest labor force participation rates among all OECD countries (OECD Labor Market Statistics), suggesting that Nordic-born women should be much more affected by the spacing rule than women from another country of origin. Indeed, we find that native-born mothers (women born in a Nordic country) sharply reduced their birth spacing as compared to foreign-born mothers (women born-outside a Nordic country) after the introduction of the child spacing rule in 1980. Most importantly, these two groups of women (native-born and foreign-born women) had strikingly similar trends in their birth spacing for more than 10 years prior to 1980, which lends credibility to the assumption that foreign-born mothers constitute a valid comparison group for native-born mothers.

Having documented that the child spacing rule had differential impacts on the birth-spacing behavior among native-born and foreign-born women, we turn to the analysis of the long-term outcomes of their children. We mainly look at first and second-born

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Newman and McCulloch (1984). For studies based on Swedish data, see Heckman et al. (1985), Heckman and Walker (1990), and Walker (1986, 1995).

[7] In addition to the change in the administrative spacing rule there were other changes in the parental leave benefits that took place in 1980. The number of parental days increased by 3 months (1 month with 90 percent replacement rate and 2 months with a low flat rate compensation). Moreover, paid leave for taking care of a sick child increased by 1.5 months. These extensions of the parental leave benefits could potentially affect child performance in the long run. However, recent research by e.g., Liu and Nordström Skans (2008), Dustmann and Schönberg (2008) and Wurtz (2007) find no such effects. On the other hand, Carneiro et al. (2009) find large positive effects of increased parental leave on child outcomes. This suggests that, if anything, our negative child-spacing effect is underestimated.

[8] This reform has previously been analyzed by demographers. For example, Hoem (1993) analyzed how the period total fertility rate is affected by the speed premium. See also Andersson (1999, 2002), and Andersson et al. (2006). The analysis in this paper, both regarding the empirical design and the outcomes of interest, differs significantly from their work.

[9] A mother's country of birth is also an immutable characteristic, i.e. it cannot be affected by the treatment itself or by individuals' reaction to the treatment, thereby avoiding the problems of having an endogenous grouping variable as discussed by Heckman (1996) and Blundell et al. (1998).

individuals but we briefly also look at third-born, fourth-born and fifth-born individuals. For first-born children, child spacing is measured by the birth interval between the first-born and second-born child, and we include all families with at least 2 children (2+ sample). For second-born children, child spacing is measured by the birth interval between the second-born and third-born child, and we include all families with at least 3 children (3+ sample). Looking at first-born and second-born subjects, together with the organization of the data into a 2+ and a 3+ sample, is important because it helps defining meaningful child-spacing effects. We show that the shares that have attained a university-preparatory education among individuals with a native-born mother and foreign-born mother closely mirror the pattern of birth spacing. Specifically, *both* the levels and the trends in the educational attainment for birth cohorts younger than 1980 is very similar in the two groups, while for older birth cohorts (1980-1987) educational attainment among individuals born by native-born mothers started to decrease relative to individuals with foreign-born mothers in 1980, the year of the introduction of the child spacing rule. Most importantly, similar patterns are found separately for first-born, second-born, third-born, fourth-born and fifth-born individuals.

Taken together, the striking similarity between the changes that took place in 1980 for both child-spacing and the long-term child outcome suggests that there is a causal relationship between child spacing and child future outcomes. As a result, we argue that the administrative child spacing rule can be used as an instrument for child spacing. According to our instrumental variable estimates, we find that the decrease in child spacing had a non-trivial effect on a child’s future outcome: a one month reduction of a mother’s birth interval due to the administrative spacing rule, implied a 1-2 percentage point decrease in the likelihood of attaining a preparatory-university education. A way to gauge the magnitude of the estimated child-spacing effect is to compare it with the gap in university-preparatory education between girls and boys, which is about 15 percentage points. In other words, the gender difference corresponds to a 7.5-15 months reduction in average child spacing. This in turn should be compared to the fact that the average child spacing was about 46 months before and 38 months after the introduction of the administrative rule in 1980. Thus, the administrative child spacing rule led to 8 months, or 17 percent, reduction in child spacing.

To further investigate the likely mechanisms behind the child-spacing effect and/or any possible confounding factors, we perform a number of tests. First, we conduct tests regarding the comparability of the treatment and comparison groups. Specifically, we show that native-born and foreign-born mothers have similar trends in maternal age at first birth and in the maternal education levels before 1980, which again suggests that foreign-born mothers are an adequate comparison group for native-born mothers.[10] We also show that the estimated child-spacing effect is broadly robust to alterations in the comparison group. For example, we find similar effects when we use, one at a time, women born in Asia, South America, or Europe as the comparison group. In sharp contrast, when we only use mothers from North America as the comparison group there is no child-spacing effect. These findings are reasonable since the countries in Asia, South America, or Europe around 1980 typically had much lower women labor force participation rates than any of the Nordic countries, while United States or Canada only had somewhat lower rates. Thus, one should not expect to find a clear child-spacing effect when mothers from North America constitute the comparison group.

Second, we perform a number of tests to exclude the possibility that the administrative child-spacing rule affected other family outcomes that have been suggested in the literature to affect children's long term outcomes. To begin with, we find no evidence that the child-spacing rule had an effect on completed family size, since native-born mothers and foreign-born mothers have similar trends in family size both before and after 1980. Second, we find no evidence that the child spacing rule affected divorce rates, which otherwise could have led to a negative relationship between child spacing and child outcomes (e.g., Gruber 2004, and Dahl and Moretti 2008).

Third, we analyze whether important differences in a child's upbringing affect the estimated child-spacing effect since this may provide information about the potential mechanism behind the child-spacing effect, as further discussed below. Previous work has suggested that out-of-home care (e.g., Baker et al. 2008) and maternal employment (Ruhm 2004, 2008 and Bernal 2008) are important factors in a child's upbringing that affect child outcomes. We therefore test for whether the child-spacing effect differs

[10] The treatment and comparison groups also have similar trends in paternal age at first birth and paternal education before the introduction of the child spacing rule (due to space constraints, these are not reported in the paper, but are available on request).

across the amount of exposure to out-of-home child care by dividing the sample into cohorts with high and low exposure to out-of home child care. We find little evidence that the child-spacing effect is affected by differences in the exposure to out-of-home child care.[11] We also split the data depending on maternal education since high education will typically be strongly associated with high employment. Again, we find little evidence that the child-spacing effect differs depending on the level of maternal education. Another potential source for creating differences in long-term outcomes of children are differences in the quality or length of primary education (*Grundskola*), grades one through nine, as discussed by Card and Krueger (1996). However, since Swedish primary education is compulsory, free of charge and regulated in a national curriculum, we think that primary education cannot be responsible for the child-spacing effect.[12] Nonetheless, we split the data depending on the average amount of real school expenditure per student during grades one through nine. Again, the estimated child spacing effect differs little between individuals in areas with high or low school spending. The child-spacing effect is also broadly similar across other possible differences in a child's upbringing such as family sizes, the gender of the child, and whether the child is first-born or second-born.

We argue that the insensitivity of the estimated child-spacing effect to important differences in a child's upbringing suggests that the causing factor must have happened in the first years of the child's life. That is because the only crucial factor that seems to have changed for a first-born child or second-born is the presence of a younger sibling that is born much closer, i.e., within two-year interval. There is by now a growing consensus that early childhood experiences may have a uniquely powerful influence on the development of cognitive and social skills. Knudsen et al. (2006), for example, forcefully argue that "a cross-disciplinary examination of research in economics, developmental psychology, and neurobiology reveals a striking convergence on a set of common principles that account for the potent effects of early environment on the capacity for human skill development." Thus, it may therefore be particularly detrimenttal for a child's future development to have another sibling at a very young age since

[11] For an overview of the Swedish child care system, see Gustafsson and Stafford (1996).
[12] For a discussion of the Swedish education system, see Björklund et al. (2005).

when children are very closely spaced then childcare obligations dominate, i.e., a mother must give most of her attention, commitment or energy to the newborn infant.

We argue that this is a likely explanation of our finding for the following reasons. First, the identifying variation in child-spacing effect comes from those women who were encouraged to have the next child within a two-year period in order to take advantage of the administrative spacing rule. Second, most Swedish women stay home with their newborn baby and any older siblings for at as least one year, since paid maternal leave was 360 days in 1980.[13] The long-term maternity leave period combined with the fact that about 90 percent of women breastfeed their newborn the first 3 months and 70 percent up to 6 months,[14] makes it obvious that there will be binding constraints for the time a mother can spend with her older child.[15]

An additional and complementary reason why short birth-spacing may have a negative effect is that pregnancy in itself may affect the quality of parenting since parenting might be poor due to tiredness during and after pregnancy. Having a next child within 24 months may exacerbate the fatigue during and after pregnancy. For example, having two pregnancies close together may cause iron-deficiency or even led to anaemia. Anaemia in a pregnant woman can cause excessive fatigue and stress and make her more susceptible to illness.[16]

To conclude, if close-spacing has an effect on a mothers behavior both before child birth (e.g., fatigue during pregnancy causes worse parenting) and after a child birth (e.g., the time-consuming process of taking care of a newly born leads to less maternal time for the older sibling), then the older sibling can be affected (indirectly or directly) by the subsequent birth of the younger sibling for much more than a year. If this event causes toxic stress in early childhood, where toxic stress refers to strong, frequent or prolonged activation of the body's stress management system, then this can have "disruptive effects on the nervous system and stress hormone regulatory system that can

[13] The entitled number of paid maternal leave days could be extended for a longer period than 360 days if the benefits are used at half or quarter of full time. During this time, mothers that were on maternal leave were not allowed to have their other children in public day care.
[14] Most babies normally breastfeed every 1 1/2 to 2 1/2 hours during the first couple of months
[15] During this time, mothers that were on maternal leave were not allowed to have their other children in the heavily subsidized (90 percent) public day care system. In 1987, for example, only about 7-8 percent of all children aged 0-6 was in private day care while 47 percent was in public day care.
[16] Importantly, however, even when a woman is iron-deficient, medical research shows that the required amount of iron continues to be provided to the placenta and fetus. Otherwise this could potentially explain the negative effect on the second-born child from close birth spacing but clearly not the effect on the first-born child.

damage developing brain architecture and chemistry and lead to life long problems in learning" (Center on the Developing Child at Harvard University 2007).[17]

Our paper contributes to a number of literatures. First, our child-spacing results speak to the current debate of the validity of using twins as an instrument to test the quality-quantity trade-off. For example, Qian (2006) argues that "the occurrence of twins potentially has a direct effect (e.g. birth spacing) on child outcomes in addition to its effect on family size" while Rosenzweig and Zhang (2006) argues that "no evidence is adduced that spacing has significant effects, net of family size, on child quality".[18] Our evidence suggests that child-spacing has an effect.

Second, our results add to the literature investigating the relationship between fertility and economic incentives. Recent work has shown that cash transfers may have an effect of fertility, e.g., Lalive and Zweimüller (2009) and Milligan (2005). However, as discussed by Milligan "the observed response may be transitory rather than permanent; women may have changed the timing of children rather than the eventual size of their families." Similarly, Lalive and Zweimüller (2009) acknowledge that "while we do not observe the completed fertility cycle of mothers, we conclude that it is quite likely that the policy change did not only affect the timing but also the number of births." In our study we use cohorts of women who completed their fertile years at the time when the Swedish administrative child-spacing rule came into place in 1980. In sharp contrast to Lalive and Zweimüller (2009) and Milligan (2005), our results suggest that the parental leave provisions *only* affects the timing of births but not on completed fertility (family size).[19] Our result is therefore consistent with the implications from life-cycle models of fertility as discussed by Hotz et al. (1997). They argue that *transitory* changes in the price of children or parental incomes "may be to shift the *timing* of births over the life-cycle rather than have much, if any, effect on the number of births accumulated."

Our results about economic incentives and fertility, is therefore relevant to the current debate in several countries of how to promote fertility through economic

[17] For more information about the toxic stress on child development see National Scientific Council on the Developing Child (2005) and the references cited therein.
[18] Grönqvist and Åslund (2007) find no effect of family size on child outcomes using the twin-birth design on data from Sweden.
[19] That *lifetime* fertility size is not affected by the reform is perhaps not surprising given that the cohort fertility in Sweden has been strikingly stable. For more than half a century, cohort fertility has varied within a narrow band of 1.9 to 2.1 children per woman as discussed by Walker (1995) and Björklund (2006).

incentives. For example, Germany has recently introduced a speed premium (36 months) on future childbearing similar to the Swedish one as a way to boost fertility.[20] According to our results, the German child-spacing rule is not likely to affect completed fertility but rather to have a negative impact on a child future outcome.

The rest of the paper is structured as follows. In section 2, we discuss the administrative child-spacing rule and provide evidence that it had a differential impact on native and foreign-born mothers. Section 3 presents evidence on the impact of the administrative child-spacing rule on child outcomes. Section 4 presents the results of the effect of child-spacing on child outcomes from using two-stage least squares and Wald estimators where the administrative rule is an instrumental variable for child spacing. In Section 5 we provide additional evidence on the child-spacing effect, while Section 6 concludes.

2 The incentives for child spacing

In this section, we discuss the parental leave benefit system and the administrative rule that provides the incentive for close child spacing in Sweden.[21] We also present evidence that the administrative rule had differential impacts on child spacing of native-born and foreign-born women.

The Swedish parental benefit system was introduced in 1974 and it was the first program of its kind among western welfare democracies. Before 1974, women were entitled to maternity allowances at the event of childbirth but now, either parent could receive payment to stay at home and care for the newborn child, although mothers continued to use the bulk of paid leave opportunities. The benefit level was 90 percent of foregone earnings with eligibility based on the parent’s individual earnings 9 consecutive months or 12 out of 24 months preceding the birth-related withdrawal. Those who did not fulfil this requirement instead received a low flat rate. In 1980, the total benefit period was 12 months; 9 months with a 90 percent replacement rate plus three additional months at a low flat rate.

[20] See Spiess and Wrohlich (2006) for a discussion of the parental leave benefit reform in Germany.

[21] Family policy in Sweden is characterized by flexible parental-leave regulations, generous parental leave allowances, right to part-time work, and high supply of publicly-financed day care for children. See Björklund (2006) for an overview of family policies in Sweden.

The rules that determine parental leave benefits in Sweden also have an element that creates a kind of "speed premium" on further childbearing. Since benefits are earnings-related, a period of no work or only part-time work after a birth would usually reduce the benefit level after a subsequent birth. However, in 1980 it became possible for women to retain her previous high level of benefits without entering the labor market between births provided that the interval between the births did not exceed 24 months.[22] Thus, this gave a woman a short-term economic incentive to space her children within 24 months in order to avoid the reduction in benefits, i.e., a speed premium on further childbearing.

Here it is important to point out that it was the authorities, rather than politicians, who determined these rules concerning the practical implementation of the parental leave system.[23] Therefore, one cannot claim that politicians deliberately created incentives for the close spacing of children. Thus, there are no obvious political economy issues which otherwise may be a potential problem when using a policy change as an exogenous source of variation (Besley and Case 2000).

Figure 1 shows the child spacing behavior in Sweden during 1968 to 1992. This figure shows that until 1980, the average spacing between two consecutive siblings was between 45-47 months, while it sharply decreased to about 37 months in 1990. Thus, the average child spacing was reduced with more than 20 percent over this period. This lends some support to that it was the administrative rule that came into place in 1980 that caused the reduction in child spacing. However, this evidence is only suggestive since it is based on a pre and post comparisons. A more compelling identification strategy is to use a differences-in-differences method which critically depends on a suitable variable being available to classify observations into the control and treatment groups. We will argue that a mother's country of birth is a useful way of classifying individuals into treatment and control groups since: (i) they should on *a priori* grounds be

[22] From 1974 to 1979, a mother could also abstain from earnings and yet retain the right to a previous benefit level for subsequent births. In 1974 the interval between births could not exceed 12 months, while in 1978 and 1979 the interval was 15 months. Thus it may be possible that this rule could have affected the spacing decisions of a small fraction of mothers even before 1980.

[23] The Swedish Government controls the authorities by each year drafting a set of appropriation instructions (regleringsbrev), which specifies the goals for each authority for the coming year and how much money is at their disposal. The Government has no right to instruct authorities in how to implement a certain law or how to decide in a particular matter. This is known as ministerial rule and is prohibited in Sweden. As a result, public administration and state agencies in particular, have a high degree of independence and decentralisation.

differently affected by the administrative child-spacing rule, and (ii) the country of birth is exogenous with respect to the administrative reform.

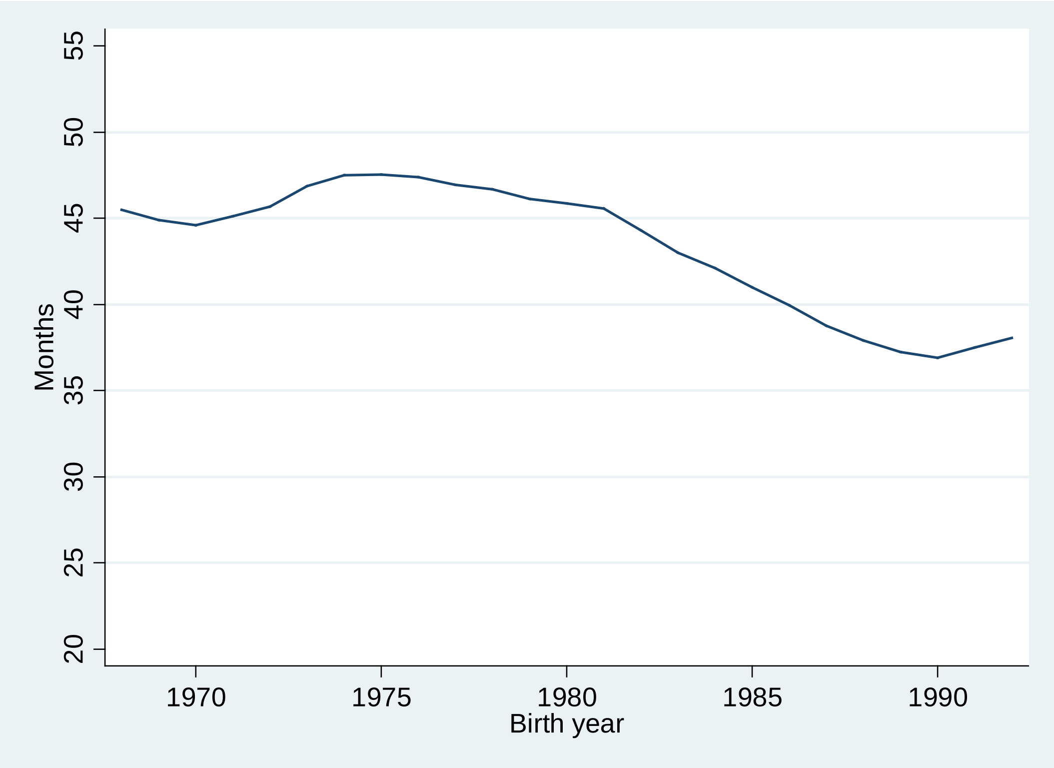

Figure 1 Average child spacing 1968–92 by birth year of children

Note: Child spacing is based on the time difference in birth between the child and the subsequent sibling. All birth orders are included.

To begin with, as noted previously native-born mothers should have *relatively* stronger incentives for closer spacing than foreign-born mothers since they are more strongly attached to the labor market than foreign-born mothers. In other words, both native-born and foreign-born mothers are affected by the child spacing rule but to very different degrees. Thus, both groups are therefore treated but we continue to label the native-born mothers as the treatment group and the foreign-born mothers as the comparison or control group.

Table 1 shows the labor force participation rates for native-born and foreign-born women for the years 1979 and 1985. The upper panel shows the figures for women in childbearing ages (i.e., women aged 16-44) and the figures for women with children less than seven years old. *Table 1* reveals that labor participation rates are significantly higher for native-born than foreign-born mothers for both categories of women. This is also the case both before as well as after the change in the administrative rule in 1980. For example, native-born women had a labor participation rate of 75 percent compared to only 61 percent for foreign-born women for those aged 16-44 in 1979.

Table 1. Labor force participation rates (n percent)

	1979	1985
	Women aged 16-44	
Native-born	75	79
Foreign-born	61	63
	Women with children under 7	
Native-born	79	80
Foreign born	58	59

The markedly lower participation rates for foreign-born women are also consistent with information provided by country specific labor market surveys (OECD Labor Market Statistics).[24] *Figure 2* displays the labor force participation rates for a number of OECD countries for the year 1980. Sweden has the highest rate followed by the other Scandinavian countries. Thus, all other OECD countries have lower labor force participation rates than the Nordic countries. According the labor market survey, the average labor force participation rates for the treatment group vary between 62-76 percent. For the remaining OECD countries the corresponding rates vary between 33-60 percent.

[24] The participation rate is defined as female labor force of all ages divided by female population 15-64 years old.

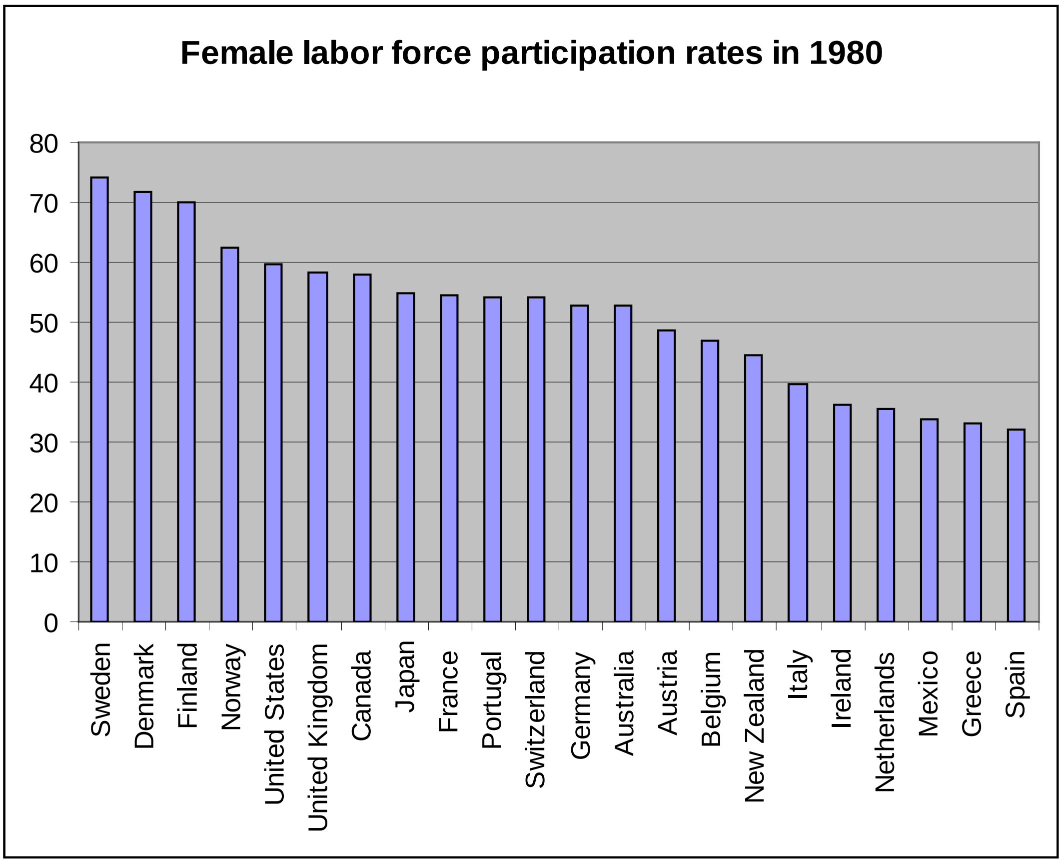

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Figure 2 Female labor force participation rates in 1980

Classifying women into treatment and control groups based on their country of birth can therefore also be thought of as capturing different cultural norms for women's decision to work as discussed by Fernández and Fogli (2009). Consequently, if we instead would have categorized mothers treatment status based on the female labor force participation in their country of origin, we would still have classified mothers born in the Nordic countries as "more treated" and mother born outside the Nordic countries as "less treated". For reasons discussed below, we therefore define the treatment group as women born in the Nordic countries (Sweden, Finland, Norway, Denmark and Iceland) while the comparison group consist of those women born outside the Nordic countries.

Mothers' country of birth is also a useful way of classifying individuals into treatment and control groups since country of birth is an immutable characteristic and can not be affected by the treatment itself or by individuals' reaction to the treatment. If we classify the treatment and control group based on a measure of the actual labor market status we would run into problems of having an endogenous grouping variable since the administrative reform is likely to affect a mother's labor force participation. This type of problem has been previously discussed in the labor supply literature where income has been used to classify individuals into treatment and control groups (e.g., Heckman 1996 and Blundell et al. 1998).[25] As a result of defining treatment status on the basis of a mother's country of birth, and not the actual labor force participation status, the reduced form relationship between a mothers outcome and the administrative spacing rule is therefore like an "intention to treat" effect. Nonetheless, under the assumption that the administrative child-spacing rule had no effect on child outcomes other than decreasing child spacing, the effect of child spacing on future child outcomes can still be estimated using an instrumental variables method.

Due to confidentiality reasons, Statistics Sweden does not provide information about an individual's country of birth. Instead, information about origin is provided at a country group level, consisting of 10 country regions. *Table 2* displays information about the region of birth for the native-born mothers (the treatment group) and foreign-born mothers (the control group), respectively. The information in *Table 2* is based on the first-born sample (2+ sample). As discussed above, Native-born mothers are defined to be born in Sweden or in some of the other four Nordic Countries (i.e., Denmark, Finland, Norway and Iceland) since women in the Nordic countries have very high labor market attachments. *Table 2* shows that 95 percent of the native-born mothers are born in Sweden. Foreign-born mothers are classified into eight different groups by Statistics Sweden, namely EU 15 (i.e., the non-Nordic member countries in the European Union before the enlargement in 2004), Europe (i.e., European countries not including EU15), Africa, North America, South America, Asia, Oceania, and Soviet Union. *Table 2* reveals that of the total of 25,325 of foreign-born mothers in our sample, 56 percent of foreign-born mothers are born in a European country (i.e., EU 15 or Europe), 27 percent are born in an Asian country, while the others are born in some of the other remaining groups. In the following, it is important to keep in mind that we need to have enough observations before and after 1980 in both the control and treatment groups since we use a differences-in-differences design. Before 1980, there

[25] Heckman (1996) criticizes Eissa (1995) who use of women's income as a grouping variable. Since women may switch groups as a result of the tax reform, this leads to biased estimates of the behavioral effect of the reform.

are 339,007 and 15,601 observations in the treatment and control group respectively, while after there are 198,286 and 9,724 in the treatment and control groups respectively. The issue of sample size in the control group is going to be important when we analyze sub-samples of the data and when we look at second-born children, the 3+ sample. For example, there are only 709 observations, whereof 395 are for the period after 1980, when mothers from North America are used as the comparison group.

Table 2. Mothers' region of birth by first born child

	Frequency	Percentage
	Native-born mothers	
Sweden	511,156	95.1
Other nordic countries	26,137	4.9
Total sum	537,293	
	Foreign-born mothers	
EU 15	4,673	18.4
Europe	9,500	37.5
Africa	1,029	4.1
North America	709	2.8
South America	2,277	9.0
Asia	6,839	27.0
Oceania	78	0.3
Soviet Union	220	0.9
Total sum	25,325	

Notes. - These groups are taken from the classification used by Statistics Sweden. Nordic includes: Denmark, Norway, Finland, and Iceland, EU 15 is equal to the 15 member states of the European Union but excluding Denmark Finland and Sweden. Europe does not include EU15 and the Nordic Countries. The remaining groups are self explanatory.

Figure 3 shows the distribution of the year of immigration to Sweden. It is interesting to note that about two thirds of the foreign-born mothers immigrated to Sweden before the introduction of the speed-premium rule in 1980. *Figure 4* displays how the composition of the region of birth among foreign-born mothers by year of birth of the children has evolved over time. For ease of exposition, we have grouped the eight regions of birth into four groups: EU15, Europe, Asia, and a group consisting of the remaining five regions with the smallest number of immigrant mothers. *Figure 4* reveals that the proportion of the Asian group has increased over time while the group from Europe has decreased. The proportion of mothers born in EU 15 and in the remaining group of countries has remained more or less constant. Importantly, there are no sharp changes in the composition of region of birth around the year of the introduction of the child spacing rule in 1980, which otherwise could have led to problems with our identification strategy.

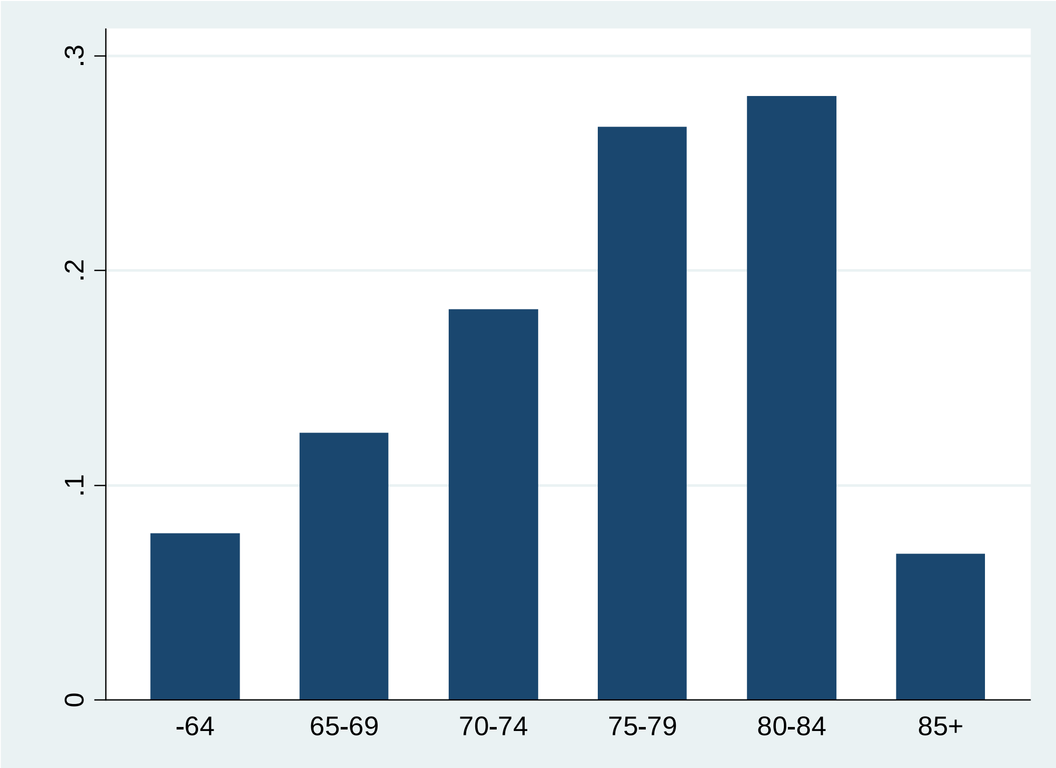

Figure 3 Distribution of immigration year of foreign-born mothers

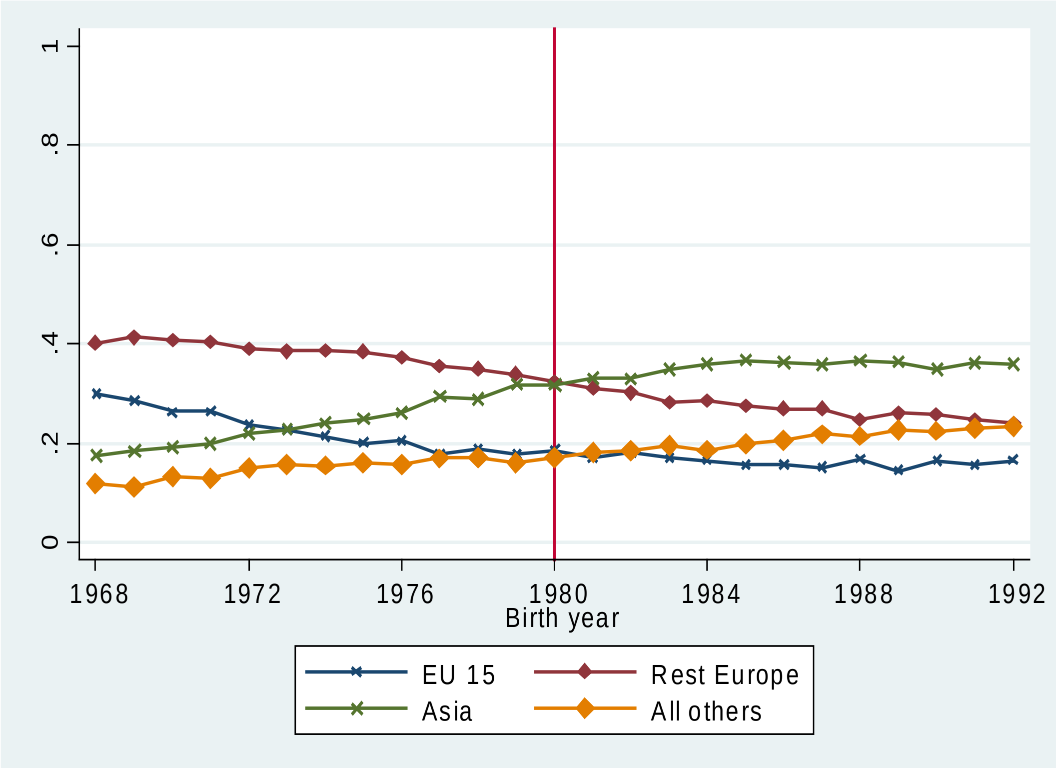

Figure 4 Mothers region of birth by birth year of children

It is however not enough that country of birth is a useful way of classifying women into treatment and control groups; it must also be the case that these two groups should be comparable across time, although they may have different time-invariant characteristics. In other words, the two groups should have parallel trends in outcome variables, such as child spacing, in absence of the intervention (i.e., the parallel trend assumption).

Figure 5 shows the development of average child spacing for the treatment and control groups from 1968 to 1992 by birth year of the children. This figure shows that the two groups have more or less parallel trends in child spacing until 1980, the year of the introduction of the administrative rule, but that they start to diverge subsequently.

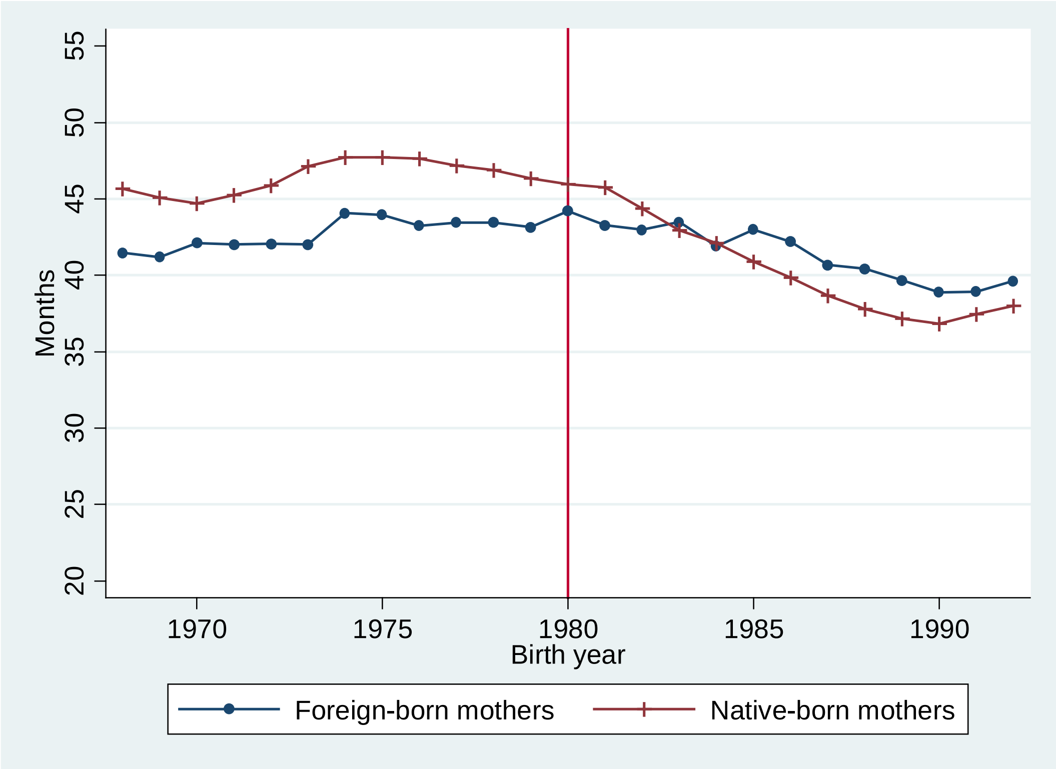

Figure 5 Child spacing of foreign-born and native-born mothers by birth year of children

It is also possible to perform a statistical analysis of whether these two groups actually have parallel trends in child spacing before 1980 by using a differences-in-differences framework. *Table 3* reports OLS estimates of an unconstrained set of interactions between the treatment group indicator (children with native-born mothers) and time effects (year of birth fixed effects), i.e.,

(1) $$Spacing_{igt} = \sum_{t=1969}^{1992} (native_g \times \lambda_t)\beta_t + \lambda_t + \theta native_g + \nu_{igt}$$

where $native_g$ is an indicator if individual i has a native-born mother. The coefficients of interests are the β's, i.e., the effects of the full set of year-native interactions, with 1968 as the base year. These year-native interactions describe the change in the child-spacing behavior of native-born mothers relative to foreign-born mothers. Since the reform came into affect in 1980, we expect that the β's should be close to zero before 1980, unless the parallel trend assumption does not hold. The results in Column 1 suggest a rather large and statistically significant decline in child spacing with little evidence of pre-existing trends (i.e., before 1980). Specifically, we cannot reject that the β's are zero before the treatment but conclude that the β's are jointly statistically significant from zero after 1980, which can be seen from the F-tests with their corresponding p-values within parentheses.

As a way of illustrating the main message from the statistical analysis in *Table 3*, *Figure 6* shows the estimated native-year interactions from Column 1 with the corresponding 95 percent confidence intervals. *Figure 6* shows that the two groups have parallel trends in child spacing for as long as 12 years (1968 to 1979) before the administrative rule came into place in 1980. Moreover, in 1980, there is a significant change in child spacing behavior between the two groups where the native-born mothers decreased their spacing relative to foreign-born mothers. After 1985, the two groups seem to have similar child spacing trends, which is quite reasonable since one can expect that the level of child spacing continues to adjust for both groups only until they reach their new equilibrium levels. The adjustment in the level of child spacing seems to be fairly rapid since it was completed in five years time, i.e., from 1980 to 1985.

Table 3. Estimated native-year effects on child spacing

Effect	(1)	(2)	(3)
Native ´ 1969	-0.31	-0.36	
	(.81)	(0.81)	
Native ´ 1970	-1.59	-1.43	
	(0.78)	(0.79)	
Native ´ 1971	-0.94	-0.90	
	(0.77)	(0.77)	
Native ´ 1972	-0.38	-0.46	
	(0.77)	(0.77)	
Native ´ 1973	0.93	0.96	
	0.76	(0.77)	
Native ´ 1974	-0.57	-0.75	
	(0.77)	(0.78)	
Native ´ 1975	-0.41	-0.85	
	(0.75)	(0.76)	
Native ´ 1976	0.22	-0.35	
	(0.75)	(0.75)	
Native ´ 1977	-0.50	-1.35	
	(0.75)	(0.76)	
Native ´ 1978	-0.76	-1.76	
	(0.75)	(0.76)	
Native ´ 1979	-1.00	-2.08	
	(0.74)	(0.75)	

Effect	(1)	(2)	(3)
Native ´ 1980	-2.43	-3.67	-2.78
	(0.74)	(0.75)	(0.48)
Native ´ 1981	-1.73	-3.14	-2.26
	(0.73)	(0.74)	(0.48)
Native ´ 1982	-2.79	-4.18	-3.31
	(0.73)	(0.75)	(0.49)
Native ´ 1983	-4.70	-6.17	-5.30
	(0.74)	(0.75)	(0.49)
Native ´ 1984	-3.99	-5.55	-4.69
	(0.74)	(0.75)	(0.49)
Native ´ 1985	-6.29	-7.74	-6.88
	(0.74)	(0.76)	(0.50)
Native ´ 1986	-6.57	-8.12	-7.25
	(0.74)	(0.76)	(0.51)
Native ´ 1987	-6.20	-7.67	-6.81
	(0.77)	(0.79)	(0.55)
Native ´ 1988	-6.82	-8.19	-7.33
	(0.79)	(0.80)	(0.56)
Native ´ 1989	-6.71	-8.02	-7.16
	(0.82)	(0.84)	(0.62)
Native ´ 1990	-6.23	-7.45	-6.60
	(0.88)	(0.89)	(0.69)
Native ´ 1991	-5.67	-6.93	-6.08
	(0.93)	(0.95)	(0.76)
Native ´ 1992	-5.80	-7.15	-6.30
	(1.04)	(1.05)	(0.89)
Controls	No	Yes	Yes
F-test	17.28	19.7	55.3
R^2	0.0230	0.0262	0.0261
Observations	1,147,456	1,147,456	1,147,456

Note.- Robust standard errors are reported in parentheses. The table reports year-native interactions in regressions that include native and year of birth dummies. The *F*-test is a test for whether the year-native interactions are jointly significantly different from zero after the introduction of the administrative child-spacing rule in 1980. Controls include mother's level of education, and full set of interactions between a mothers region of birth and the year of immigration.

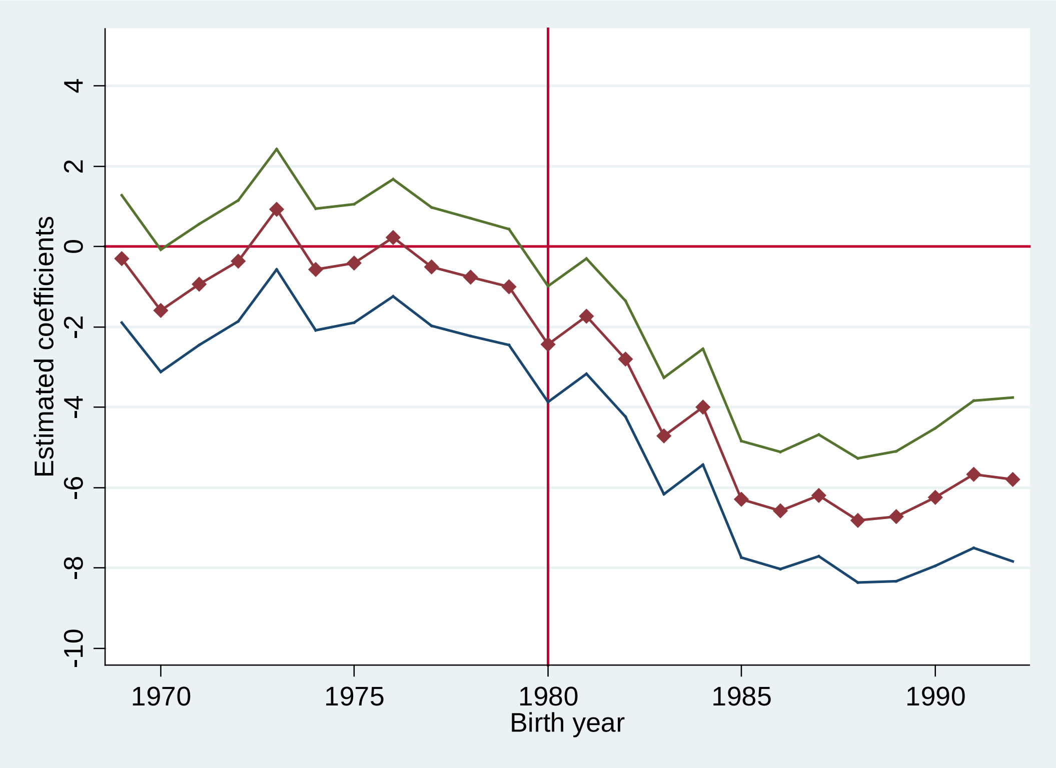

Figure 6 Effects of native-year interactions on child spacing by birth year of children

Note:- The estimated native-year effects are from Column 1 in Table 3.

We can get additional support for the claim that the introduction of the child-spacing rule caused the change in child spacing by looking at the distributions of child spacing before and after 1980, separately for the control and treatment groups. *Figure 7* shows that the distribution of child spacing for children with foreign-born mothers is only somewhat affected after 1980 as compared to before. In sharp contrast, the distribution for native-born mothers has clearly shifted to the left after 1980 as displayed in *Figure 8*. The shift in distribution seems to be particularly pronounced for spacing levels around 24 months.

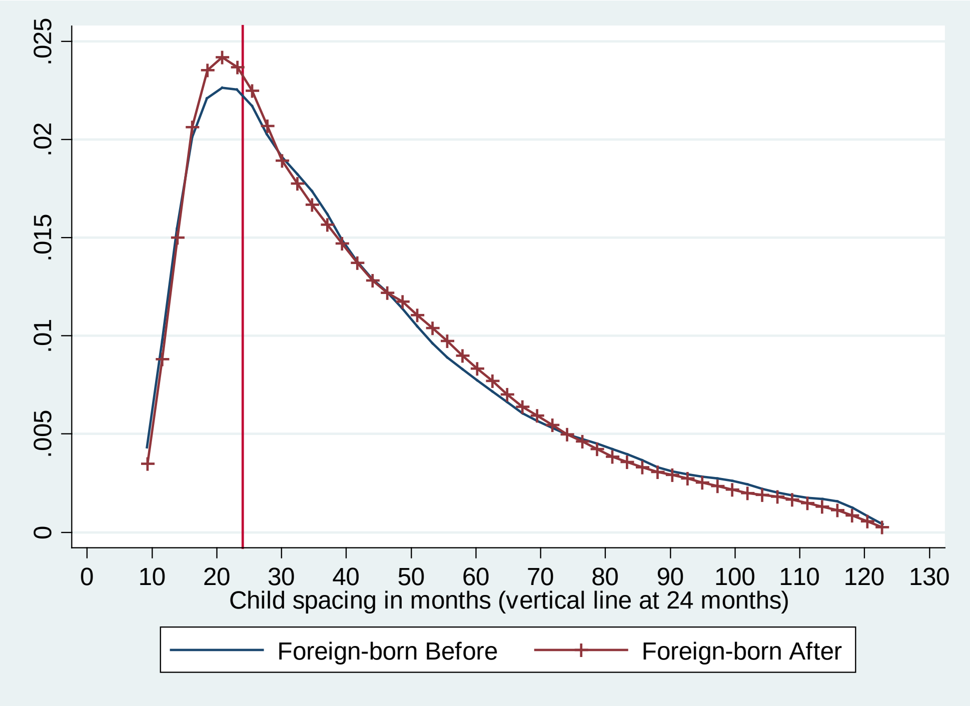

Figure 7 Comparison of estimated kernel densities of child spacing for foreign-born mothers

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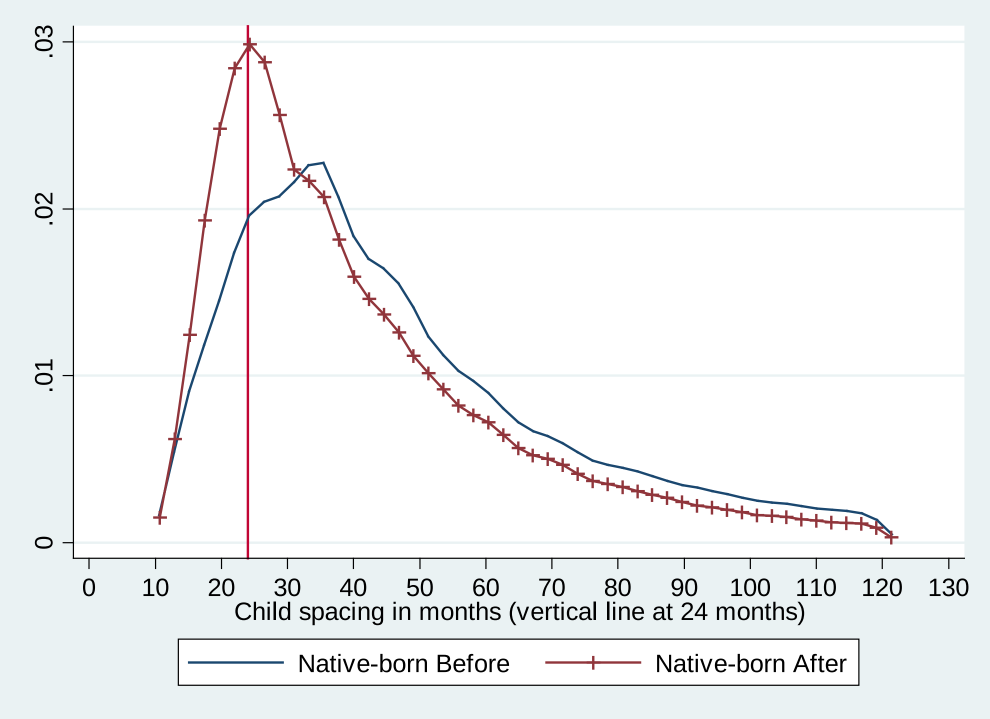

Figure 8 Comparison of estimated kernel densities of child spacing for native-born mothers

Yet another way of illustrating that it was the child spacing rule that affected child spacing behavior is to look at the share of women that gave birth to the next child within 24 months. *Figure 9* shows that in 1968, 20 percent of foreign-born mothers had their second child within 24 months. The corresponding figure for native-born mothers is only 11 percent. Most importantly, however, is that the trends are roughly the same until 1980 when the share of mothers that gave birth to the second child within 24 months starts to increases among the native-born mothers relatively to foreign-born mothers. *Figure 10* shows the estimated year-native interactions from the regression model in equation (1), but where the dependent variable is now an indicator taking the value one if the next child is born with 24 months. *Figure 10* shows that the treatment and the control groups have similar trends until 1980 but where the share of native-born mothers having a second child within 24 months sharply increases afterwards.

To conclude, the child spacing patterns as displayed in Figures 5-10 strongly suggest that it was the introduction of the speed-premium rule that caused the shift in the distribution of child spacing for native-born mothers.

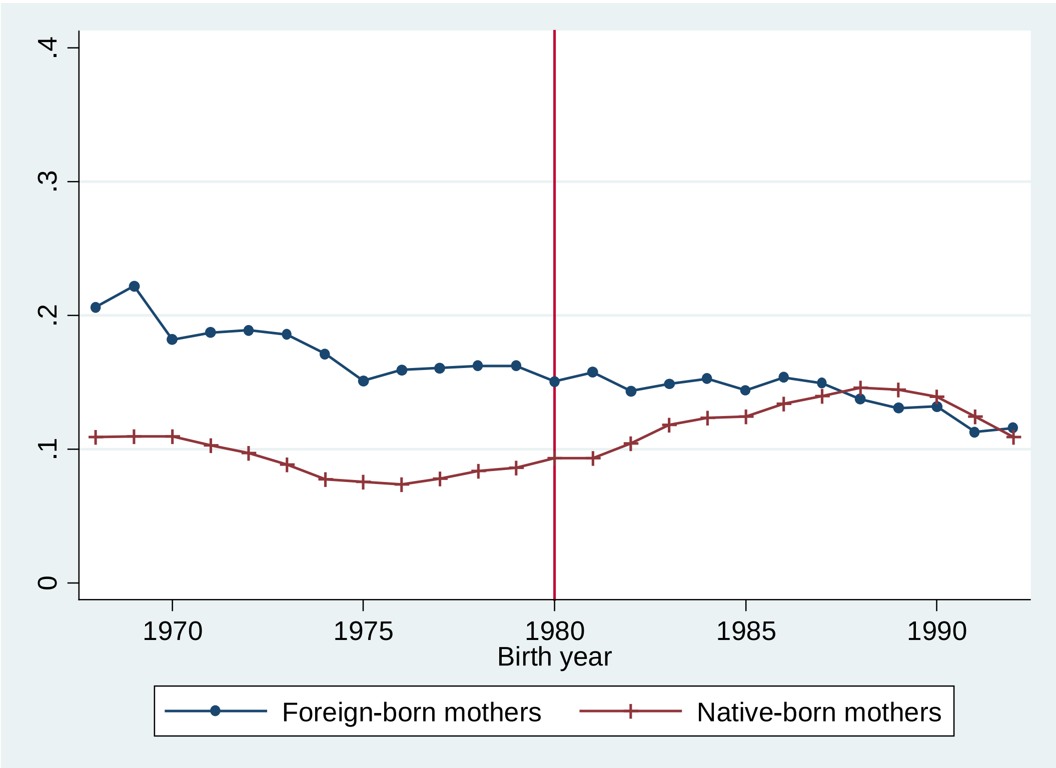

Figure 9 Share of mothers that gave birth to her next child within 24 months by birth year of children

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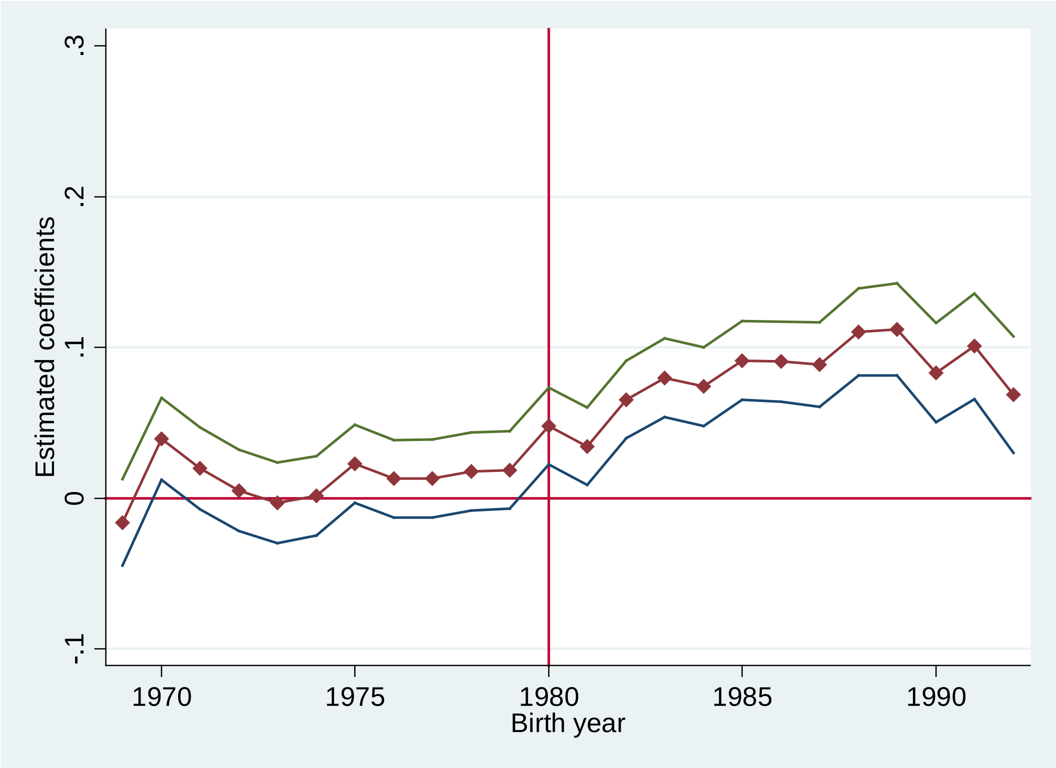

Figure 10 Effects of native-year interactions on the share of mothers who gave birth to her next child within 24 months by birth year of children

To further probe the comparability of the treatment and control groups, we look at the development of maternal age at first child birth and years of schooling for native-born and foreign-born mothers, respectively. *Figure 11* and *Figure 12* show highly similar trends in the maternal age and years of schooling before the administrative child spacing rule that was introduced in 1980. However, native-born mothers' age at first birth started to decrease relative to foreign-born mothers in 1980. This is not surprising since the child-spacing rule is likely to affect the timing of *all* births due to the incentives for women to bunch their births together. In other words, since our measure of child spacing is defined as the difference between maternal age at her second and first births, then if child spacing is affected then maternal age at first birth is also likely to be affected. This implies that maternal age at first birth cannot be used as a control variable since it is endogenous and would therefore bias the estimate of the treatment effect (Angrist and Pischke 2009).

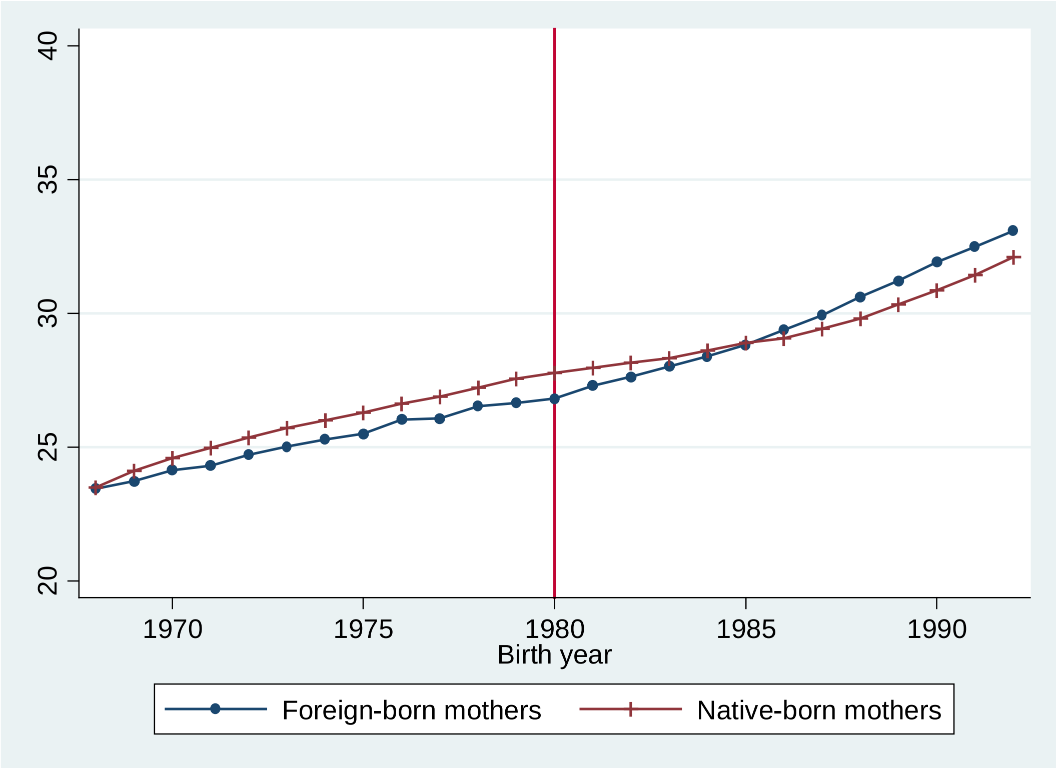

Figure 11 Maternal age at first birth by birth year of children

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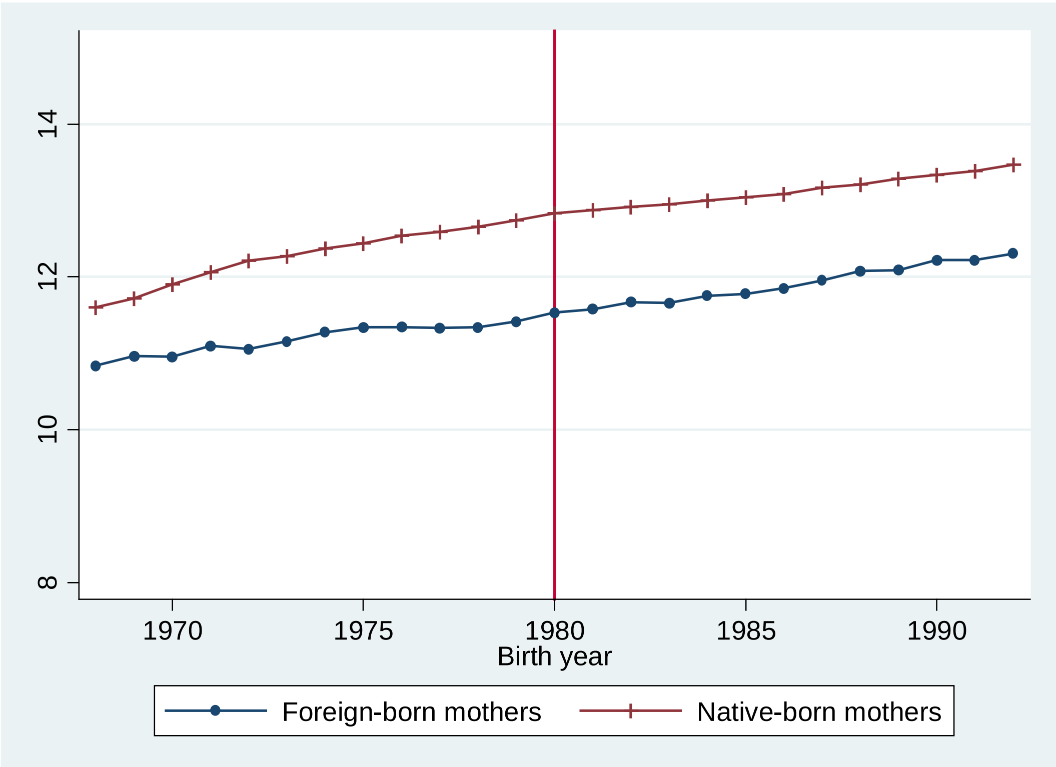

Figure 12 Educational attainment of mothers by birth year of the child

The evidence provided above suggests that native-born and foreign-born mothers have remarkably similar trends in several characteristics before 1980, such as child spacing, years of schooling, and maternal age. Nevertheless, one might still worry about compositional changes in the control group since the foreign-born mothers have emigrated from very different groups of countries. One way of addressing this is to add a number of controls that is not affected by the child-spacing rule. We therefore control for a mother's educational attainment and a *full* set of interactions between the region of birth and the year of immigration in specification (1).

Most of the estimated child spacing effects are hardly affected as can be seen by comparing Column 2 with Column 1 in *Table 3*. However, the estimates for 1978 and 1979 are now significantly different from zero but still rather small which suggests that there was a small change in child spacing before 1980. As discussed by Hoem (1993), during the years 1978 and 1979, women could still retain her previous high level of benefits without entering the labor market between births provided that the interval between the births did not exceed 15 months. Achieving such a tight spacing of children is biologically difficult and not desired by many parents. Thus, we still use 1980 as the date of treatment although a small fraction of women may have taken advantage of the tighter birth interval before 1980.

That most of the estimates of child spacing effects hardly change when pre-treatment controls are included in equation (1), suggests that compositional bias is not an important issue in our context. Column 3 shows the results when we impose the restriction that all β's are zero before 1980. The F-statistics is 55.3, which, anticipating the instrumental variable approach, suggests that the set of instrumental have enough explanatory power as to avoid problems of weak instruments.

Another way of addressing the comparability of the treatment and control groups is to restrict the sample of foreign-born mothers to, say, only those who emigrated from a country within EU15, since these women may be more comparable to native-born women on *a priori* grounds. As shown further below, the estimate of the child spacing effect is robust to alterations of the regions of birth included in the control group.

Another issue is that the administrative child spacing rule may not only have affected child spacing but also completed family size.[26] This would raise concerns about the exclusion restriction of our instrument – the administrative spacing rule – in the child outcome equation. To address whether the reform had an impact on family size we have looked at completed family size before and after the reform for native-born and foreign-born mothers, respectively. *Figure 13* displays the development of completed family size across the treatment and the control groups by birth year of children. This figure shows that they have parallel trends during the whole period, i.e., both before as well as after 1980. In addition, we have also estimated the following differences-in-differences specification for family size:

$$Familysize_{igt} = \alpha + \lambda_t + \delta native_g + \beta 1[year \geq 1980, native = 1] + u_{igt}$$

where 1[.] is an indicator function. We cannot reject that $\beta=0$, since $\hat{\beta}$=0.03 with a standard error of 0.11.

[26] Milligan (2005) and Lalive and Zweimuller (2009) find evidence suggesting that policy reforms affects fertility but they cannot discriminate whether this is due to a timing effect or a due to a family size effect since they do not have data on completed fertility.

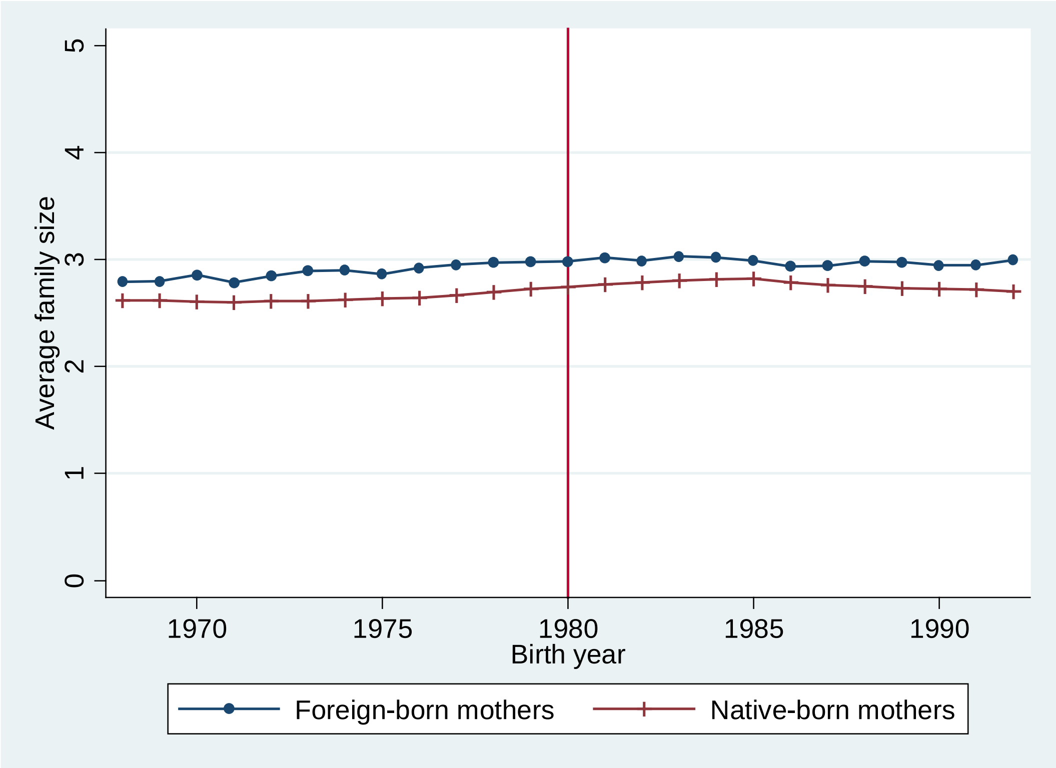

Figure 13 Average completed family size by birth year of children

To further address the question whether the reform affected completed family size we also investigate if there were any change in the family size distribution before and after the reform. *Figure 14* shows the family size distribution for native-born mothers before and after the reform. The fraction of families with two children is the same before and after the reform. In fact, according to *Figure 14*, it is hard to find any evidence that family size increased after the reform. If anything, it looks like one child families have increased slightly after the reform and that family sizes of 4 or larger have become less frequent. This pattern is also present if we look at the family size distribution before and after the reform for foreign-born mothers, shown in *Figure 15*. *Figure 16* shows the difference in the family size share after and before the reform for native-born and foreign-born mothers, respectively. As *Figure 16* clearly shows, the fraction of one-child families increased almost to the same extent for both native-born and foreign-born mothers. For family sizes of 2-4 there are roughly no changes at all except for the fraction of families with a least 5 children, which decreased after the reform for both groups. Given the fact that family sizes of five and larger are uncommon, the results clearly show that family size was hardly affected by the “speed premium rule”.

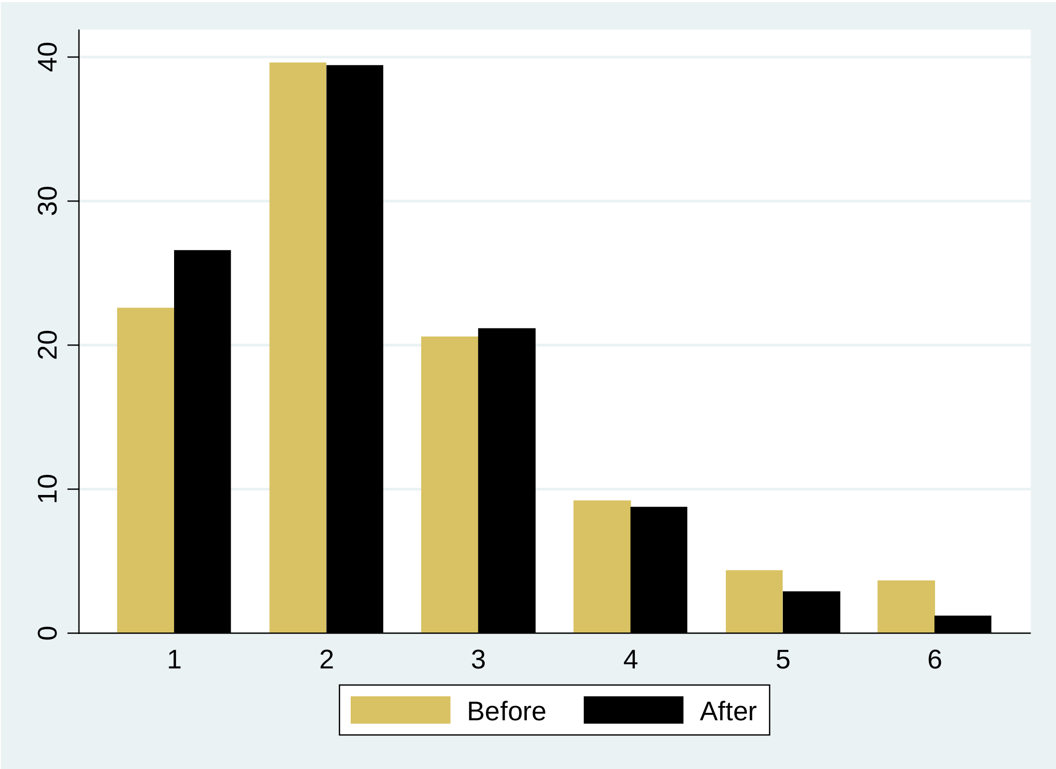

Figure 14 Distribution of completed family sizes before and after the reform: Native-born mothers

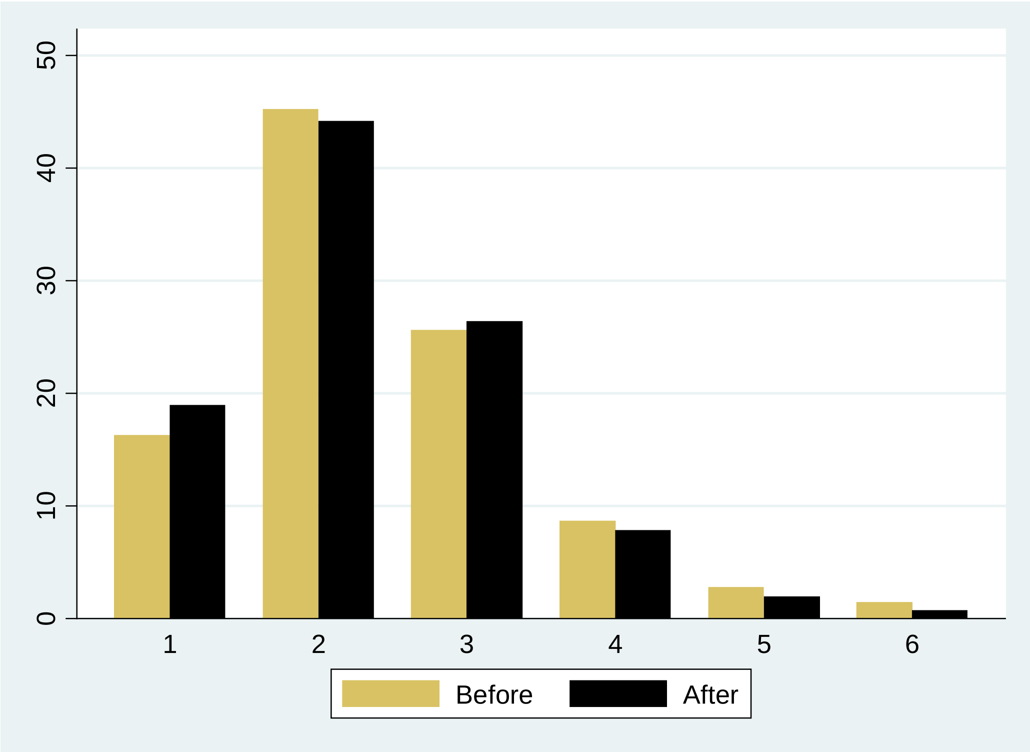

Figure 15 Distribution of completed family sizes before and after the reform: Foreign-born mothers

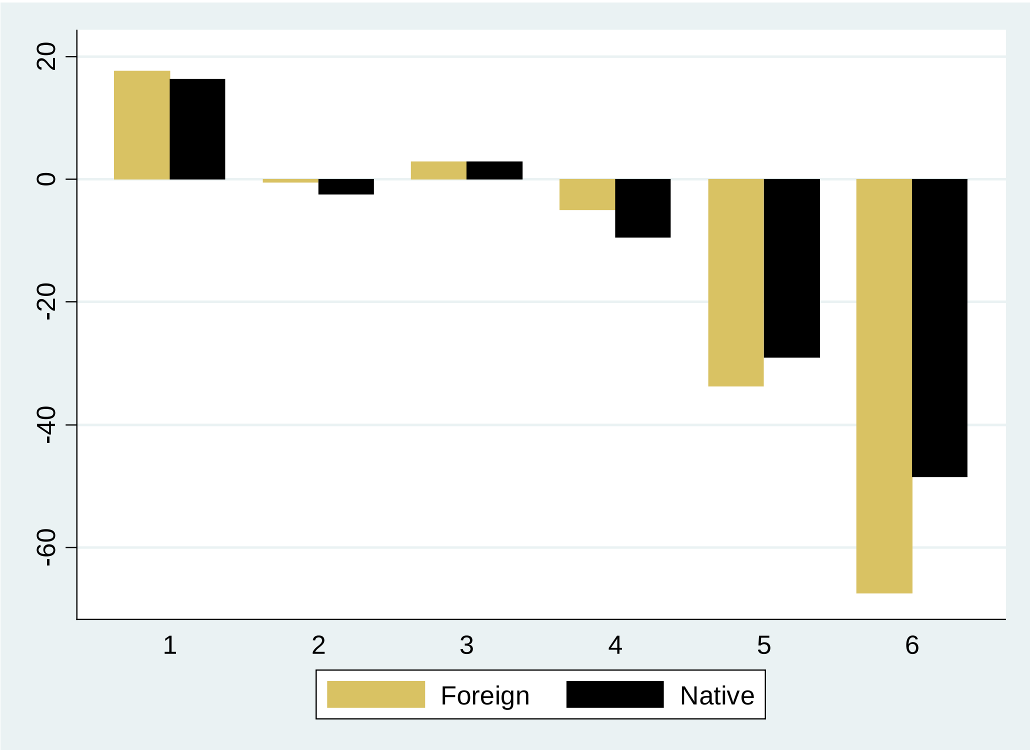

Figure 16 Comparison of the percentage changes (before vs. after 1980) in family size distributions for foreign-born and native-born mothers, respectively

3 The effect of the speed-premium on child outcomes

In this section we provide evidence of the effect of the administrative rule – the speed premium – on child outcomes. We use the Multi Generation Population Register matched with the longitudinal data base LOUISE. The data were provided by Statistics Sweden. LOUISE is a register based data set on the total Swedish population which includes information on, among other things, income and education. The Multi Generation Registers include identifiers so that we can match parents to their biological children and siblings to each other. Consequently, and quite importantly, the information on child spacing, birth order and number of children is not conditional on having found the siblings in the other parts of the data set, which otherwise is the case in most other available micro data sets, since it is directly recorded for each mother.

When matching children to parents we use the mother identifier since almost all children have grown up with a mother. We restrict the analysis to all first-born and second-born individuals born between 1968 and 1988 due to limitations of the child outcome data. As noted previously, the spacing for first-born children is measured by the birth interval between the first and second-born child, while for second-born children child spacing is measured by the birth interval between the second and third-born.[27] In other words, we work with two main analysis samples. One consists of first-born subjects in families with two or more births (2+ sample). The second sample consists of second-born subjects in families with three or more births (3+ sample). Data therefore consists of repeated cross-section of mothers' first-born or second-born child since each mother is only observed in one year. Since we study the outcomes of children born before the second or third birth we avoid any selection problems due to differential preferences of family size.[28] We also restrict our sample to mothers who are born before 1965 in order to look at completed family size.

For the 2+ sample, the treatment group will therefore consist of all first-born subjects with native-born mothers with a family size of two or more, while the comparison group will be all first-born individuals with foreign-born mothers, also with at least two

[27] We also exclude observations where child spacing is less than 1 year (around 0.10 percent of the population) and more than 10 years (almost 5 percent of the sample). For children born in 1960-1995 there are around 16 percent where we have no information on mother country of birth (of those children with missing information on mothers' country of birth, 91 percent are born before 1972.

[28] This is analogous to the sample criteria used by Angrist et al. (2006) and Black et al. (2005).

children. Similarly, for the 3+ sample, the treatment group consists of all second-born subjects with native-born mothers with a family size of three or more, while the control group will be all second-born individuals with foreign-born mothers in families with at least 3 births.

The main child outcome measure used in this paper is university-preparatory educational attainment which individuals typically obtain at the age of 19 in Sweden. The information on educational attainment is only available for individuals born up to 1987 since educational attainment is measured in 2006. Many individuals are therefore still in the educational system. For example, the 1987 birth cohort is 19 years old in 2006. To avoid any censoring problems, we therefore use university-preparatory education as our educational attainment outcome.

The Swedish schooling system can briefly be described in the following way. Primary and middle schooling (*Grundskola*), grades one through nine, is compulsory. Although there has been a growth of state financed private schools recently, public schools, free of tuition, are still most common. Only a handful of tuition charging schools exists. The final grades from the ninth year in compulsory school are used for admission to secondary school education (*Gymnasieskolan*). Around 90 percent of the pupils continue on to secondary school which basically consists of two tracks, vocational and academic (university-preparatory). The grades from secondary school are used for admission to higher education (colleges and universities). Generally, those individuals who complete a university-preparatory education will do that directly after graduation from compulsory school, and university-preparatory education is typically three years.

We measure university-preparatory education as whether an individual has attained a three year secondary school education that qualifies for further academic studies at a university. All the main tracks included in a university-preparatory education i.e., science, social sciences, and business administration, are included in this definition. Individuals who already have attained a higher education that requires a three year of university-preparatory education are of course also defined as having attained a university-preparatory education. Having a university-preparatory education is very highly correlated with having a university degree (i.e., number of years of schooling). Using the university-preparatory education measure for the mothers, where most of them are likely to have completed their education, we find that those who have a university-preparatory education have 2 more years of schooling in 2003.

Starting the analysis with first-born children, i.e., using the 2+ sample, *Figure 17* shows the development in the share with a university-preparatory education during the period 1968-1987, separately for first-born children with native-born and foreign-born mothers, respectively. It shows that the treatment and the control groups have strikingly similar levels and trends until the introduction of the child spacing rule in 1980 when the levels starts to diverge. In other words, the evolution in the educational attainment is the basically the same for 12 years (i.e., 1968 to 1979) for the treatment and control groups.

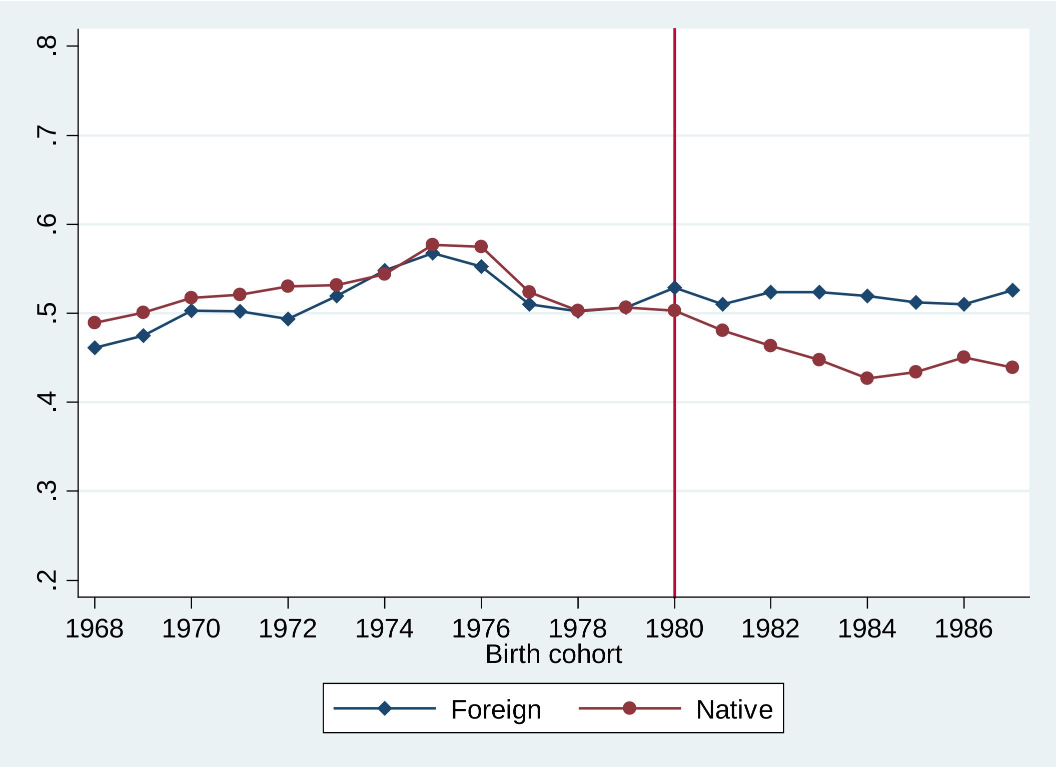

Figure 17 Share of first-born children who have attained a university preparatory education. Families with at least 2 children

Again, we make a statistical test of whether the two groups have parallel trends by using an identical differences-in-differences specification as equation (1) but where a dummy for having a university-preparatory education is the dependent variable instead of child spacing, i.e.,

(2) $$Education_{igt} = \sum_{t=1}^{T} (native_g \times \lambda_t)\beta_t + \lambda_t + \theta native_g + \nu_{igt}$$

Table 4 shows the results from this regression. Looking at Column 1 in *Table 4*, results strongly suggest a statistically significant decline in the share of university-preparatory education after 1980 for the treatment group with little evidence of an existing pre-reform trend (i.e., before 1980). Specifically, we cannot reject that the β's are zero before the treatment but conclude that the β's are jointly statistically significant from zero after 1980, which can be seen from the F-tests with corresponding p-values within parentheses. Moreover, when we add a number of controls for a mother's educational attainment and a full set of interactions between the region of birth and the year of immigration, as a way of addressing compositional changes within the control group as discussed previously, the effects are hardly affected as can be seen in Column 2. This suggests again that compositional bias is not an important issue in our context. Column 3 shows the results when we impose the restriction that all β's are zero before 1980. In order to illustrate the main point from the regression model in equation (2), *Figure 18* shows the estimated native-year interactions from Column 1 in *Table 4* with the corresponding 95 percent confidence intervals. *Figure 18* shows that we cannot statistically reject that the two groups have similar trends in the share of individuals who have attained a university-preparatory education before 1980, while the two groups have differential trends thereafter since there is a decrease in educational attainment for the treatment group relative to the control group.

This striking similarity of the pattern of educational attainment in *Figure 18* with the pattern in child spacing in *Figure 6*, strongly suggests that there is a causal relationship between child spacing and child long-term outcomes.

Turning to the outcome of the second-born in families with at least 3 births, i.e., the 3+ sample, *Figure 19* shows the development of university-preparatory education. Again, we see that the control and treatment groups have roughly similar levels and trends until 1980. *Table 5* shows the results from the statistical test, while *Figure 20* displays the estimated native-year interactions from Column 1 in *Table 5* with the corresponding 95 percent confidence intervals. *Figure 20* shows that we cannot statistically reject that the two groups have similar trends in the share with a university-preparatory education before 1980, while we conclude that the two groups have differential trends thereafter since there is a decrease in educational attainment for the treatment group relatively to the control group after 1980.

Table 4. Estimated native-year effects on university preparatory education for first-born children (families with at least 2 children)

Effect	(1)	(2)	(3)
Native ´ 1969	0.00	-0.00	-
	(0.02)	(0.02)	
Native ´ 1970	-0.01	-0.03	-
	(0.02)	(0.02)	
Native ´ 1971	-0.01	-0.02	-
	(0.02)	(0.02)	
Native ´ 1972	0.01	-0.00	-
	(0.02)	(0.02)	
Native ´ 1973	-0.01	-0.03	-
	(0.02)	(0.02)	
Native ´ 1974	-0.03	-0.04	-
	(0.02)	(0.02)	
Native ´ 1975	-0.01	-0.03	-
	(0.02)	(0.02)	
Native ´ 1976	-0.00	-0.02	-
	(0.02)	(0.02)	
Native ´ 1977	-0.01	-0.04	-
	(0.02)	(0.02)	
Native ´ 1978	-0.02	-0.04	-
	(0.02)	(0.02)	
Native ´ 1979	-0.02	-0.05	-
	(0.02)	(0.02)	
Native ´ 1980	-0.04	-0.06	-0.03
	(0.02)	(0.02)	(0.01)
Native ´ 1981	-0.05	-0.08	-0.05
	(0.02)	(0.02)	(0.01)
Native ´ 1982	-0.09	-0.11	-0.09
	(0.02)	(0.02)	(0.01)
Native ´ 1983	-0.10	-0.12	-0.09
	(0.02)	(0.02)	(0.02)
Native ´ 1984	-0.12	-0.13	-0.11
	(0.02)	(0.02)	(0.02)
Native ´ 1985	-0.11	-0.13	-0.10
	(0.02)	(0.02)	(0.02)
Native ´ 1986	-0.09	-0.09	-0.07
	(0.02)	(0.02)	(0.02)
Native ´ 1987	-0.11	-0.12	-0.09
	(0.02)	(0.02)	(0.02)
Controls	No	Yes	Yes
F-test	F=6.62	F=7.81	F=17.19
P-value	(0.0000)	(0.0000)	(0.0000)
R^2	0.0070	0.0582	0.0582
Observations	562,618	562,618	562,618

Note.- Robust standard errors are reported in parentheses. The table reports year-native interactions in regressions that include native and year of birth dummies. The *F*-test is a test for whether the year-native interactions are jointly significantly different from zero after the introduction of the administrative child-spacing rule in 1980. Controls include mother's level of education, and full set of interactions between a mothers region of birth and the year of immigration.

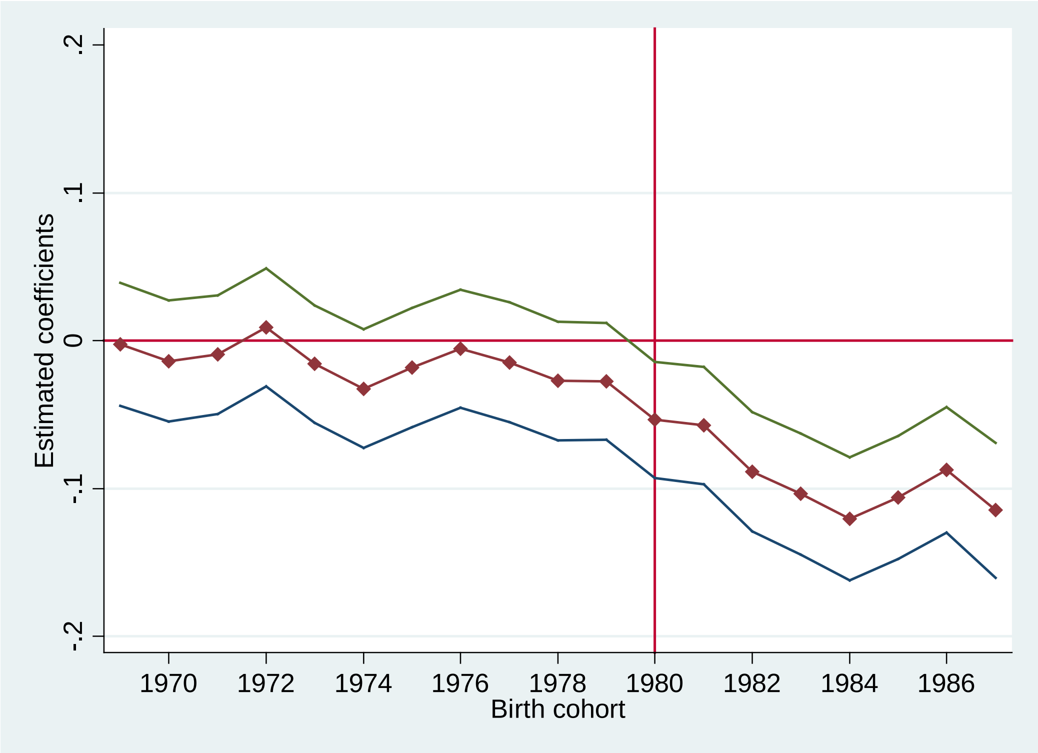

Figure 18 Estimated native-year effects on the share of first-born children who have attained a university preparatory education. Families with at least 2 children

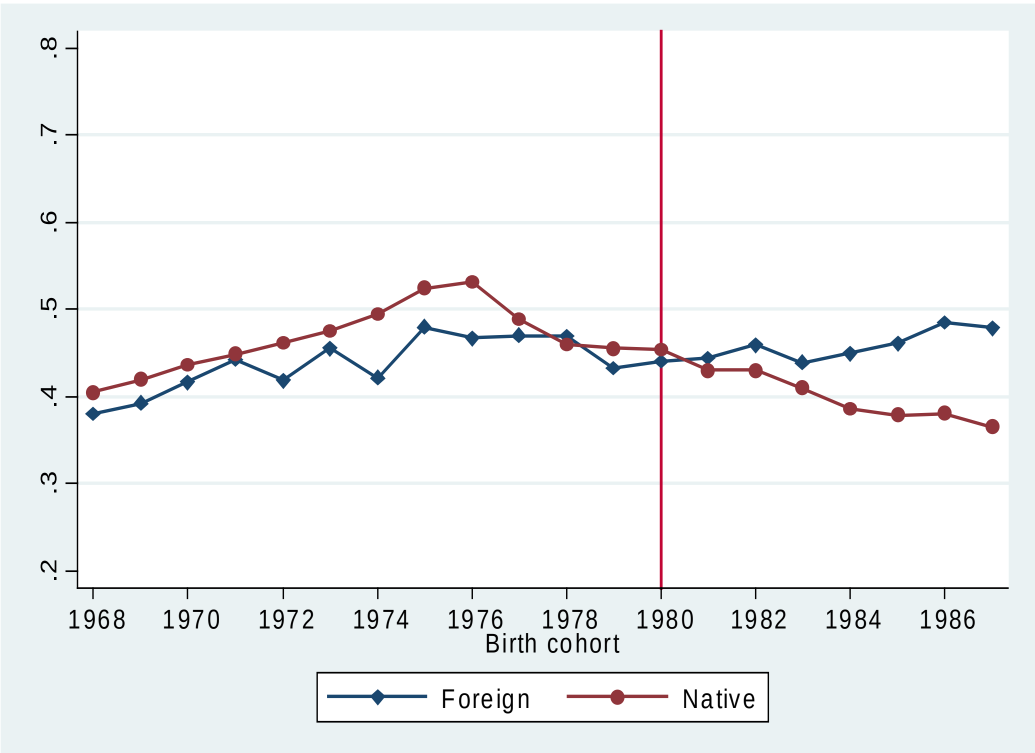

Figure 19 Share of first-born children who have attained a university preparatory education. Families with at least 3 children

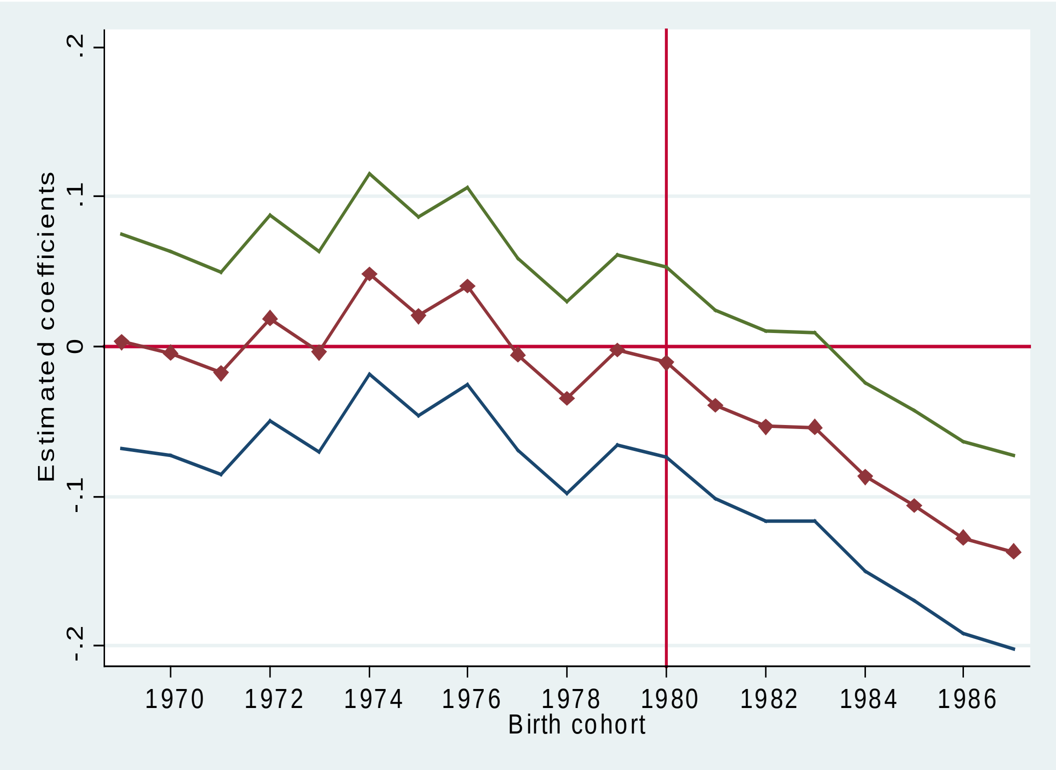

Figure 20 Estimated native-year effects on the share of first-born children who have attained a university preparatory education. Families with at least 3 children

Again, when we add a number of controls for a mother's educational attainment and a full set of interactions between the region of birth and the year of immigration, the effects are hardly affected as can be seen in Column 2. This again suggests that compositional bias is not an important issue in our context. That compositional changes do not seem to be important is not surprising since we have already shown previously that native-born and foreign-born mothers have parallel trends in both maternal age and educational attainment before the reform. Furthermore, looking at the development of characteristics of biological fathers (age and years of schooling) we find that native-born and foreign-born fathers have parallel trends in such characteristics during the whole sample period, 1968-87.[29]

[29] Results are available from the authors upon request. It is noteworthy that about 30 percent of the foreign-born mothers – where we can identify a father to the child – are married to a male born in Sweden. Thus, the control group does not only consist of mothers living in families where both parents are immigrants. This suggests that the control group might be more similar to the treatment group than if we would have used mothers only married to an immigrant male.

Table 5. Estimated native-year effects on university preparatory education for second-born children (families with at least 3 children)

Effect	(1)	(2)	(3)
Native ´ 1969	0.00	0.02	
	(0.04)	(0.04)	
Native ´ 1970	-0.01	-0.00	
	(0.04)	(0.04)	
Native ´ 1971	-0.02	-0.02	
	(0.04)	(0.04)	
Native ´ 1972	0.02	0.02	
	(0.04)	(0.04)	
Native ´ 1973	-0.01	-0.02	
	(0.04)	(0.04)	
Native ´ 1974	0.05	0.04	
	(0.04)	(0.03)	
Native ´ 1975	0.02	0.01	
	(0.04)	(0.03)	
Native ´ 1976	0.04	0.02	
	(0.03)	(0.03)	
Native ´ 1977	-0.01	-0.01	
	(0.03)	(0.03)	
Native ´ 1978	-0.03	-0.06	
	(0.03)	(0.03)	
Native ´ 1979	-0.00	-0.02	
	(0.03)	(0.03)	
Native ´ 1980	-0.01	-0.03	-0.03
	(0.03)	(0.03)	(0.02)
Native ´ 1981	-0.04	-0.06	-0.05
	(0.03)	(0.03)	(0.02)
Native ´ 1982	-0.05	-0.06	-0.06
	(0.03)	(0.03)	(0.02)
Native ´ 1983	-0.06	-0.07	-0.07
	(0.03)	(0.03)	(0.02)
Native ´ 1984	-0.09	-0.09	-0.08
	(0.03)	(0.03)	(0.02)
Native ´ 1985	-0.10	-0.12	-0.12
	(0.03)	(0.03)	(0.02)
Native ´ 1986	-0.13	-0.15	-0.14
	(0.03)	(0.03)	(0.02)
Native ´ 1987	-0.14	-0.15	-0.14
	(0.03)	(0.03)	(0.02)
Controls	No	Yes	Yes
F-test	F=5.35	F=5.15	F=12.33
P-value	(0.0000)	(0.0000)	(0.0000)
R^2	0.0082	0.0718	0.0718
Observations	227,540	227,540	227,540

Note.- Robust standard errors are reported in parentheses. The table reports year-native interactions in regressions that include native and year of birth dummies. The *F*-test is a test for whether the year-native interactions are jointly significantly different from zero after the introduction of the administrative child-spacing rule in 1980. Controls include mother's level of education, and full set of interactions between a mothers region of birth and the year of immigration.

4 The impact of child spacing on university-preparatory education

In this section we present results of the effect of child spacing on child outcomes. Under the assumption that the administrative child-spacing rule – the speed premium – had no effect on child outcomes other than decreasing child spacing, we can use this administrative rule to construct instrumental variable estimates of the impact of child spacing on child outcomes. For example, using a single indicator for before and after the introduction of the speed premium rule we can construct a simple Wald/IV estimate, i.e.,

$$\hat{\beta}^{IV} = \frac{(\overline{Y}^{native,after} - \overline{Y}^{native,before}) - (\overline{Y}^{foreign,after} - \overline{Y}^{foreign,before})}{(\overline{Spacing}^{native,after} - \overline{Spacing}^{native,before}) - (\overline{Spacing}^{foreign,after} - \overline{Spacing}^{foreign,before})}$$

Since we have many post-treatment years we can also estimate the effect of child spacing on child outcomes using a Two-Stage Least Square (2SLS) method. In the 2SLS approach, we would use all post treatment native-year interactions as instrumental variables instead of only one instrument as in the Wald method. However, the 2SLS method may lead to the problem of weak instruments if some of the individual instruments are weak as discussed by Andrews and Stock (2006). The Wald approach has the advantage of avoiding the problem of many weak instruments since it only uses a single and strong instrument. We will therefore present results from both the Wald and the 2SLS approaches.

We will cluster the standard errors at mothers' birth region-year level to account for potentially correlated effects among mothers from the same birth region. Since data is repeated cross-section of mothers' first-born child or second-born child and each mother is only observed in one year, this implies that serial correlation in individual outcomes is unlikely to be a problem. Since there are 10 birth regions and 20 years there are 200 birth-region-years, this will provide a sufficient number of clusters for the clustering estimator to have good properties as discussed by Angrist and Pischke (2009).

Before showing the results from the Wald/IV and the 2SLS approaches, we present results from OLS regressions, as a benchmark for assessing biases or potential heterogeneity in the estimated child-spacing effect. *Table 6* displays the results for university-preparatory education for first-born children (i.e., using the 2+ sample) and second-born children (3+ sample), respectively. Without any controls, the OLS estimate is -0.00094 for first-born children (Column 1). This means that for first-born children one month *shorter* birth interval will lead to an almost 0.1 percent *higher* probability of having a university-preparatory education. When we add controls for the maternal level of education and a full set of interactions between a mothers region of birth and the year of immigration, as a way of addressing compositional changes within the control group, the effects become slightly lower in absolute terms as can be seen in Column 2. The estimated effects for second-born children are smaller but still significantly different from zero (see Columns 2 and 4).

Turning to the instrumental variable approach, *Table 7* displays the results from the Wald and the 2SLS methods for first-born children. The 2SLS estimate is 0.020 while the Wald/IV estimate is 0.022 in the specification without any additional control variables. Thus, one month *shorter* birth interval will decrease the probability of obtaining a university-preparatory education by around 2 percentage points. These estimates are also statistically different from zero and of the opposite sign from the OLS estimates in *Table 6*. This suggests that the OLS estimate is strongly biased or that there are strong non-linearities in the treatment effect. On one hand, one reason for the bias in the OLS estimates is that high ability parents choose to bunch their children closely together as a way to avoid too many breaks in their job marker careers. On the other hand, the treatment effect may be highly non-linear as the result of this paper indicates. We have therefore allowed for non-linear effects in the OLS specifications and results (not reported) suggest that spacing closer than 24 months affects children more negatively than longer spacing.

Table 6. OLS estimates University preparatory education

	2+ sample		3+ sample	
Child spacing	-0.00094***	-0.00064***	-0.00012***	-0.00010***
	(0.00003)	(0.00003)	(0.00004)	(0.00004)
Controls	No	Yes	No	Yes
R^2	0.0082	0.0587	0.0078	0.0714
Observations	562,618	562,618	227,540	227,540

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses. Controls included are year of birth fixed effects, mother's level of education, and full set of interactions between a mothers region of birth (10 regions) and the year of immigration.

Table 7. University preparatory education: 2SLS and Wald/IV estimates. 2+ sample

	2SLS		Wald/IV	
	(1)	(2)	(3)	(4)
Child spacing	0.020***	0.017***	0.022***	0.019***
	(0.004)	(0.002)	(0.005)	(0.003)
Controls	No	Yes	No	Yes
First-stage F-test	6.51	10.18	19.71	56.15
Observations	562,618	562,618	562,618	562,618

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses. Controls included are year of birth fixed effects, mother's level of education, and full set of interactions between a mothers region of birth (10 regions) and the year of immigration.

When we add controls for the mother's level of education and full set of interactions between a mother's region of birth and the year of immigration the effects become slightly lower as can be seen in Columns 2 and 4. Looking at the First-stage F-statistics from the 2SLS and Wald/IV estimators, they suggest that the Wald/IV estimator may be preferred from a weak instrument point of view since the F-statistics is twice as large as the F-statistics from the 2SLS estimator. Nevertheless, there seems to be small differences regarding the point estimate of the two estimators and their associated standard errors.

Turning to second-born children, *Table 8* displays the results from the Wald and the 2SLS methods. The 2SLS estimate is 0.018 which is the same as the Wald/IV estimate in the specification without any additional control variables; both estimates are statistically different from zero. Thus, one month *shorter* birth interval will decrease the probability of obtaining a university-preparatory education by around 1.8 percentage points. The effects of child spacing on university-preparatory education are slightly reduced when adding the controls (see Columns 2 and 4). Based on the First-stage F-statistics, the Wald/IV estimator may again be preferred to the 2SLS estimator from a weak instrument point of view. To avoid any problems of many weak instruments we will in the subsequent analyses only report the Wald/IV estimates.

Table 8. University preparatory education: 2SLS and Wald/IV estimates. 3+ sample

	2SLS		Wald/IV	
	(1)	(2)	(3)	(4)
Child spacing	0.018***	0.014***	0.018***	0.014***
	(0.003)	(0.002)	(0.004)	(0.002)
Controls	No	Yes	No	Yes
First-stage F-test	10.84	9.30	16.27	41.38
Observations	227,540	227,540	227,540	227,540

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses. Controls included are year of birth fixed effects, mother's level of education, and full set of interactions between a mothers region of birth (10 regions) and the year of immigration.

Table 9. Different control groups: Wald/IV for university preparatory education 2+ sample

	EU 15	Europe	Africa	North America	South America	Asia
			2+ sample			
Child spacing	0.013**	0.040**	0.026	-0.071	0.014***	0.014***
	(0.006)	(0.018)	(0.014)	(0.130)	(0.005)	(0.002)
Observations in control group	4,673	9,500	1,029	709	2,277	6,839
	541,966	546,793	538,322	538,002	539,570	544,132
			3+ sample			
Child spacing	-0.0028	0.024	0.020***	-0.0062	0.017	0.015***
	(0.0050)	(0.024)	(0.006)	(0.0240)	(0.016)	(0.002)
Observations in control group	1,947	2,868	649	306	1,003	4,477
Total observations	218,146	219,067	216,848	216,505	217,202	220,676

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses. Controls included are year of birth fixed effects, mother's level of education, and full set of interactions between a mothers region of birth (10 regions) and the year of immigration.

We now turn to additional analyses and examine whether the child spacing effects is sensitive to the definition of the control group, i.e., subjects with foreign-born mothers. This group is heterogeneous with respect to region of birth. Nonetheless, data from ILO (2001) shows that almost all countries in these regions had smaller or much smaller female labor force participation rates among ages 25-54 than the treatment group in 1980. Results from Wald estimations of the effect of child spacing on university-preparatory education for first-born children, using mothers from different birth regions as control groups, separately, are shown in the upper panel of *Table 9*. The lower panel of *Table 9* displays the corresponding results for second-born children. As can be seen in *Table 9*, the Wald estimates are similar to the previously estimated child-spacing effects as presented in *Table 7* and 8, except when mothers born in North America are used as the control group (see Column 4). This is consistent with the hypothesis that mothers' with a high labor force attachment do not constitute a relevant control group since mothers from North America have a high labour force participation rate. Thus, they are affected by the speed premium rule and can therefore be considered treated in the same way as mothers from the Nordic countries. It should be pointed out, however, that mothers from North America are relatively few, 709 observations in total. Note also that the number of observations is also relatively small when mother from Africa and South America are used as comparison groups. Thus, it is not surprising that the standard errors are somewhat smaller compared to the estimations reported in *Table 7* and 8 where the full samples are used.

Taken together, we conclude that estimated child spacing effect is broadly robust to alterations in the comparison group. For example, we find similar effects when we only use women born in Asia, South America, or Europe. In sharp contrast, when we use North America there is no child spacing effect.

In *Table 10* and 11, we test whether the child-spacing effect differ across families of different sizes, for first-born and second-born children, respectively. To avoid any sample selection problems due to differential preferences of family size, we restrict the sample to families with at least n births and study the outcomes of children born before the n birth. Specifically, we look at samples with 3 or more births and 4 or more births. For comparison, Column 1 restates the Wald/IV estimates with control variables from

Table 7. We finally also examine whether there are gender differences in the effect of the child spacing on child outcomes. The first two columns of *Table 12* show the Wald estimates for girls and boys, separately, using first-born children. Columns 3 and 4 show the corresponding estimates using second-born children.

The general conclusion from estimations of heterogeneous effects with respect to family size and gender is that we find that the negative child-spacing effect on educational attainment is broadly similar across family sizes and the gender of the child.

Table 10. Different family size: Wald/IV estimates for university preparatory education 2+ sample

	Two children or more	Three children or more	Four children or more
	(1)	(2)	(3)
Child spacing	0.019***	0.021***	0.022***
	(0.003)	(0.004)	(0.004)
Controls	Yes	Yes	Yes
First-stage F-test	56.15	37.47	41.54
Observations	562,618	236,909	70,021

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses. Controls included are year of birth fixed effects, mother's level of education, and full set of interactions between a mothers region of birth (10 regions) and the year of immigration.

Table 11. Different family size: Wald/IV estimates for university preparatory education 3+ sample

	Three children or more	Four children or more
	(2)	(3)
Child spacing	0.014***	0.021***
	(0.002)	(0.004)
Controls	Yes	Yes
First-stage F-test	41.38	40.15
Observations	227,540	69,445

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses. Controls included are year of birth fixed effects, mother's level of education, and full set of interactions between a mothers region of birth (10 regions) and the year of immigration.

Table 12. Female versus males: IV estimates

	University preparatory education 2+ sample		University-preparatory education 3+ sample	
	Female	Male	Female	Male
Child spacing	0.020***	0.016***	0.017***	0.009***
	(0.003)	(0.002)	(0.003)	(0.003)
Controls	Yes	Yes	Yes	Yes
First-stage F-test	47.48	36.76	52.29	17.37
Observations	275,546	288,072	110,241	117,299

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses. Controls included are year of birth fixed effects, mother's level of education, and full set of interactions between a mothers region of birth (10 regions) and the year of immigration.

Table 13. University preparatory education: Wald/IV estimates. Third, fourth, fifth-born children

	Third-born	Fourth-born	Fifth-born
	(1)	(2)	(3)
Child spacing	0.013***	0.014***	0.010**
	(0.003)	(0.004)	(0.004)
First-stage F-test	24.79	42.86	17.83
Observations in control group	4,647	1,973	838
Observations	63,351	17,919	5,609

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses. Controls included are year of birth fixed effects, mother's level of education, and full set of interactions between a mothers region of birth (10 regions) and the year of immigration.

5 Additional evidence

In this section, we provide further evidence on the child-spacing effect. Specifically, we make seven additional tests. First, we examine whether the child-spacing effect is present in the samples with third-born, fourth-born and fifth-born children. Second, we test whether the child-spacing effect differs depending on the availability of out-of-home care. Third, we test whether the child-spacing effect differs depending on the mother's level of education. Fourth, we test whether the divorce rates differ between native-born and foreign-born mothers before and after the introduction of the child-spacing rule in 1980. Fifth, we estimate the child-spacing effect for individuals raised in areas with high or low school expenditures. Sixth, we check whether the administrative spacing rule affected child-spacing shorter than 15 months. Finally, we estimate the child-spacing effect on another measure of educational performance, namely final grades in compulsory school (at age 15).

We begin by estimating the child-spacing effect for third-born, fourth-born and fifth-born individuals. As before, child-spacing is measured by the birth interval between the younger and older child, and we analyze the outcome of the older child. The sample sizes will of course be much smaller for higher parities than for first-born or second-born, but there is still interesting to know whether the child-spacing effect is still present in these samples because that may provide evidence about the likely mechanism behind the spacing effect. For example, if all individuals are affected similarly by the introduction of the child-spacing rule this would strengthen our interpretation that the spacing effect is due to the strong effects of early environment on the capacity for human skill development as discussed by Knutsen et al. (2006). *Figure 21-Figure 23* show the development in the share with a university-preparatory education for third-born, fourth-born and fifth-born, respectively, during the period 1968-1987. For all three groups, figures show that the treatment and the control groups have similar levels and trends until the introduction of the child spacing rule in 1980 when the levels starts to diverge. This is exactly the same pattern as previously found for first-born and second-born individuals. *Table 13* shows that the estimate child-spacing effects are very similar across all samples – for the third-born the estimate is 0.013, for the fourth-born

0.014, and for the fifth-born 0.010. All the estimates are also statistically different from zero.

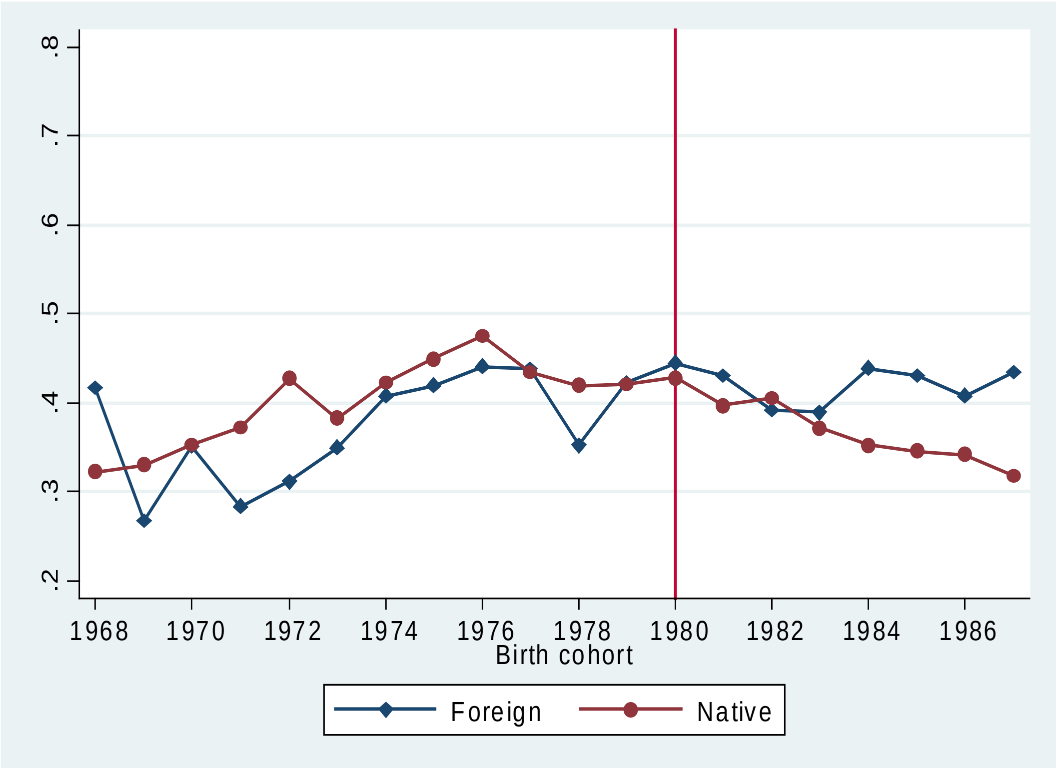

Figure 21 Share of third-born children who have attained a university preparatory education. Families with at least 4 children

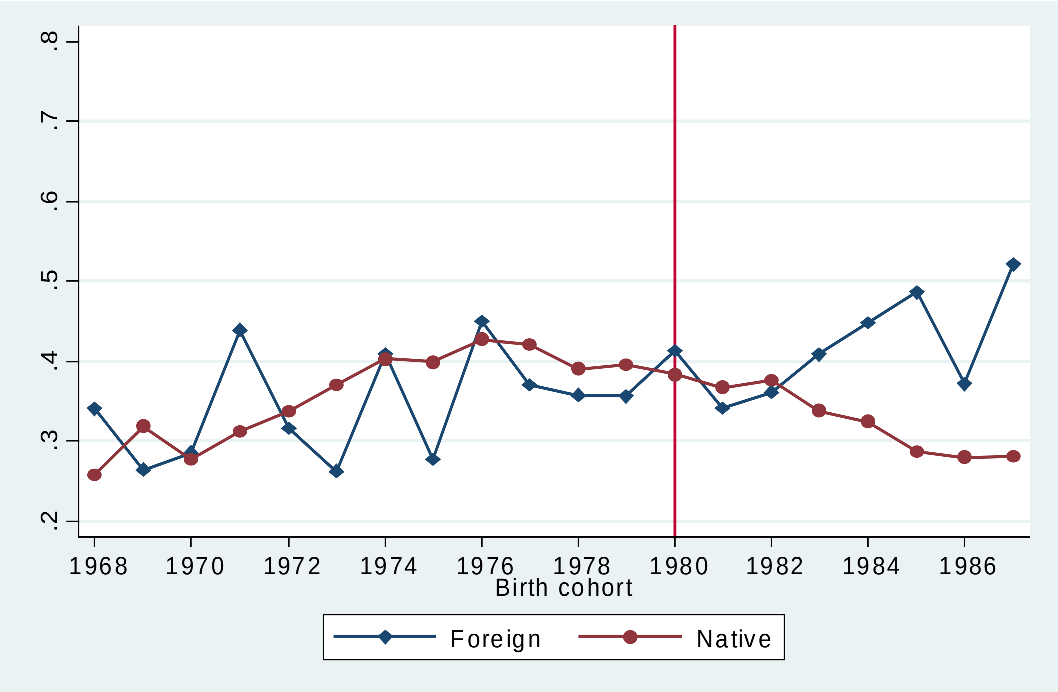

Figure 22 Share of fourth-born children who have attained a university preparatory education. Families with at least 5 children

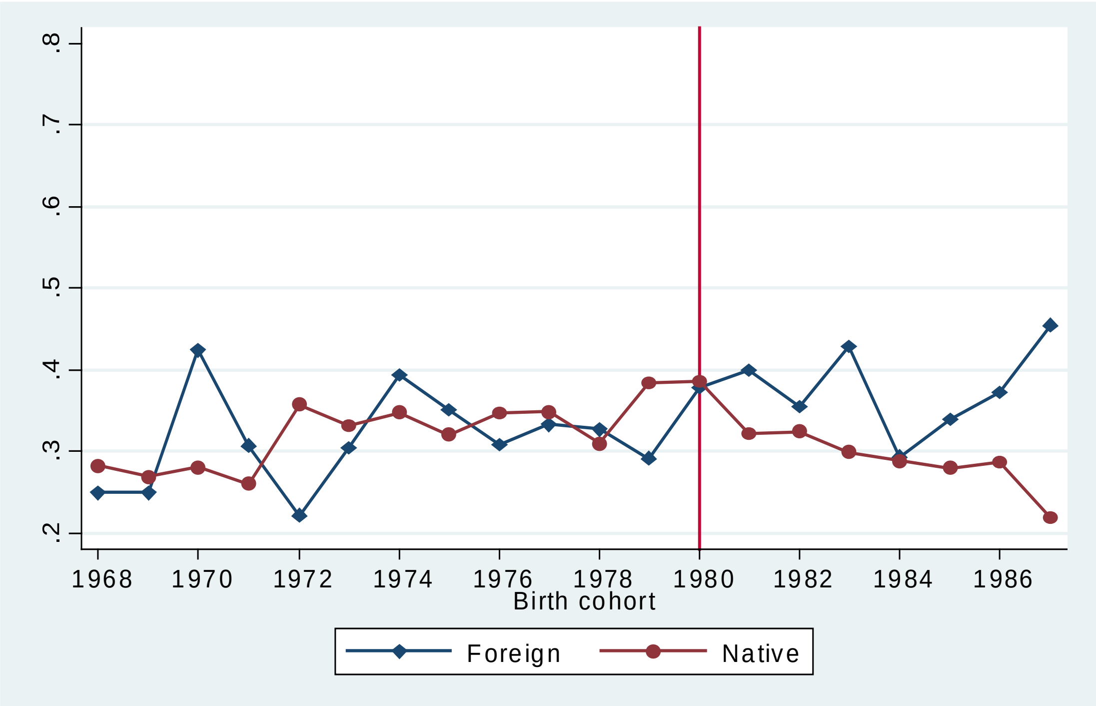

Figure 23 Share of fifth-born children who have attained a university preparatory education. Families with at least 6 children

Turning to the second test, out-of-home care may be a factor that may affect a child's outcome. For example, Baker et al. (2008) find evidence that the introduction of universally accessible child care in Canada has negative effects on a variety of child outcomes. Thus, our child-spacing effect may therefore be confounded by differences in out-of-home care for children. Alternatively, a child-spacing effect that occurs at a young age may be reinforced (or mitigated) by a child's exposure to out-of home care.[30] To investigate this issue, we divide the sample into two groups: one group where the availability of out-of-home care is high and another group where it is low. In Sweden, the bulk of out-of-home child care is heavily subsidized (90 percent) and publicly provided.[31] The public day care is provided at the municipality level which offers two modes of official child care arrangements: centre and family care.[32] Swedish child care is regarded as being of high quality. We have data on the number of slots in centre care and family day care by municipality and birth cohorts during the period 1974-87. Thus, we can construct a measure of the average exposure to out-of-home care for children aged 0-6 that varies across cohorts and municipalities. For example, the 1974 birth cohort in the municipality Härjedalen had the lowest exposure to out-of-home care, namely 3.7 percent while the 1984 birth cohort in Sundbyberg had the highest exposure, namely 72.2 percent. In order to have roughly similar sample sizes in the groups with high and low availability of out-of-home day care, the group with high availability is defined as having at least 41 percent of exposure.[33] The average exposure in the two groups is 50 and 28 percent, respectively. *Table 14* presents the results. We find similar child-spacing estimates with the exception for second-born children in low availability out-of-home care environments. That the availability of out-of-home care has little consequence for the estimated child-spacing effect suggests that the child-spacing effect occurred before the child was exposed to out-of-home day care. Moreover, since out-of-home day care is strongly correlated with maternal employment, this also suggests that maternal employment has little impact on the child-spacing effect.

[30] For information about Swedish child care, see Gustafsson and Stafford (1994).
[31] Only about 7 percent of children aged 0-6 has private day care.
[32] As of 2009, there are 290 municipalities.
[33] The reason is that 41 percent of exposure divides the sample in two halves of equal size.

Table 14. Child spacing effect and the availability of out-of-home day care

	University preparatory education 2+ sample		University-preparatory education 3+ sample	
	High availability of out-of home-day care Mean=50%	Low availability of out-of home-day care Mean=28%	High availability of out-of home-day care Mean=50%	Low availability of out-of home-day care Mean=28%
Child spacing	0.021***	0.028*	0.026***	0.20
	(0.008)	(0.015)	(0.010)	(1.00)
First-stage F-test	12.81	2.62	6.11	0.00
Observations in control group	9,740	5,401	5,442	2,246
Observations	180,704	191,301	83,301	81,102

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses.

Table 15. Child spacing effect and mothers level of education

	University preparatory education 2+ sample		University-preparatory education 3+ sample	
	High	Low	High	Low
Child spacing	0.012*	0.026***	0.020*	0.018***
	(0.006)	(0.006)	(0.012)	(0.004)
First-stage F-test	5.83	15.62	4.53	13.97
Observations in control groups	5,531	19,794	1,938	9,403
Observations	171,084	391,534	68,159	159,381

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses. High education is defined as at least two-years of post-secondary education.

Turning to whether the child-spacing effect is affected by the mother's level of education. The idea here is that mothers' education is strongly correlated to maternal employment since highly educated women are more likely to work. If there are small differences in estimated effects of child spacing between mothers with high and low education this would also support the hypothesis that the child-spacing effect is derived from the child's early environment. *Table 15* displays results separately for highly and low educated mothers where high education is defined as having at least two years of post-secondary education. Again, the child spacing-effect is broadly similar across education levels of the mother. For first-born children the estimated effect of child spacing for highly educated mothers is 0.012, and for low educated mothers the estimated effect is 0.026. As regards, the second-born children the corresponding estates are 0.020 and 0.018.

We also test for whether divorce may be a confounding factor behind the child spacing effect since some studies have found evidence that divorce may affect child outcomes (e.g., Gruber 2004 and Dahl and Moretti 2008). *Figure 24* shows that native-born mothers and foreign-born mothers have strikingly similar trends in the divorce rates, as measured in 1990, before and after the introduction of the child spacing rule in 1980. This suggests that divorce is not confounding our estimates.

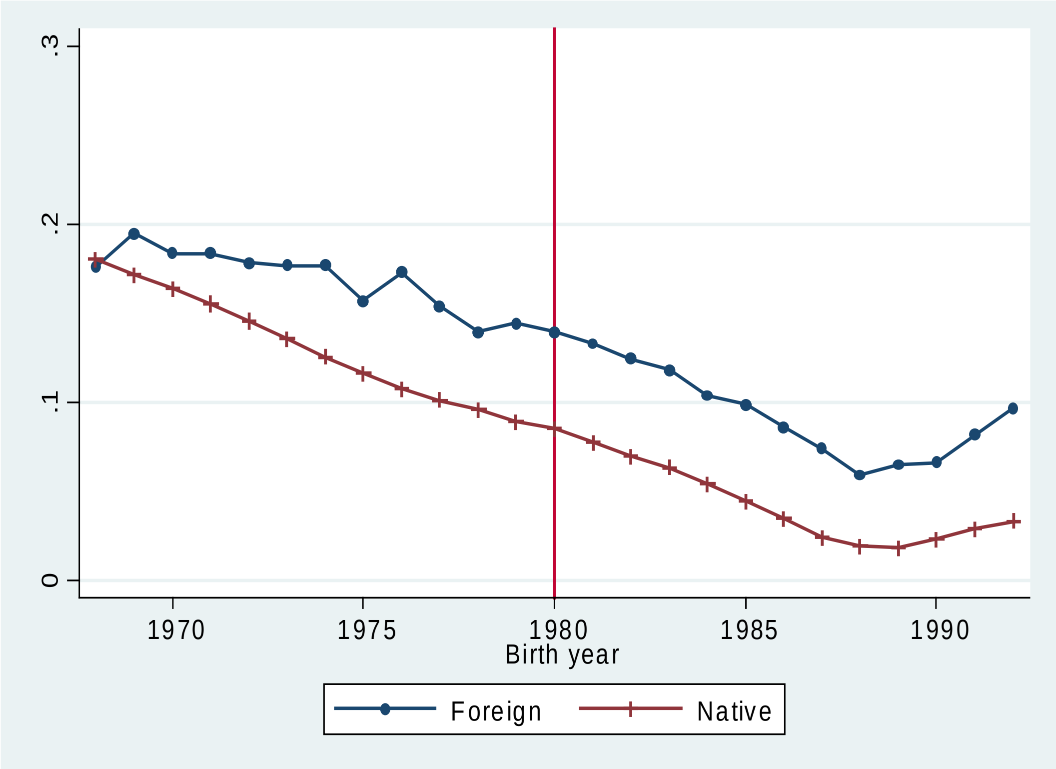

Figure 24 Mothers divorce rates by birth year of the child

As another check, we test whether differences in school resources might explain the child-spacing effect. We have yearly data on schools expenditures from all municipalities. We can therefore construct an average real spending per student by cohort and by municipality. For example, for the 1974 birth cohort in the municipality of Stockholm, we take average of per pupil spending in Stockholm during the years 81 to 89 (compulsory schooling years grade 1 to grade 9 for 1974 birth cohort). Using this measure of school resources, we split the data into two samples depending on median of school spending. *Table 16* displays the results. For first-born children the estimated child-spacing effects are very similar for areas with high school spending and low school spending, 0.019 and 0.018, respectively. The estimated child-spacing effects for second-born are also similar but also less precisely estimated than for first-born children.

Table 16. Pupil spending in primary school

	University preparatory education 2+ sample		University-preparatory education 3+ sample	
	High spending	Low spending	High spending	Low spending
Child spacing	0.019***	0.018**	0.027	0.032
	(0.007)	(0.008)	(0.022)	(0.052)
First-stage F-test	8.89	8.35	1.96	0.45
Observations in control group	8,547	6,603	4,648	3,040
Observations	185,909	186,096	82,236	82,167

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses.

Table 17. Final grades at compulsory school: 2SLS and Wald/IV estimates. 2+ sample

	2SLS		Wald/IV	
	(1)	(2)	(3)	(4)
Child spacing	0.580***	0.532***	0.619***	0.524***
	(0.184)	(0.109)	(0.225)	(0.122)
Controls	No	Yes	No	Yes
First-stage F-test	4.05	9.28	15.80	50.79
Observations	482,531	482,531	482,531	482,531

Note. - Standard errors clustered at the mothers' birth region-year level are reported in parentheses. Controls included are time fixed effects, mother's level of education, and full set of interactions between a mothers region of birth and the year of immigration.

We also look at whether the administrative rule affected child spacing intervals shorter than 15 months. The reason for this test is that very short interpregnancy intervals have been associated with an increased risk of adverse perinatal outcomes (Conde-Agudelo et al. 2006). Thus, if higher order births are directly affected by close birth spacing this may then explain the negative child-spacing effects for higher order births, although this cannot clearly explain the effect on first-born. *Figure 25* shows the share of native-born women with births interval closer than 24 months, closer than 15 months and closer than 12 months, respectively. This figure shows that the introduction of the child-spacing rule only affected intervals between 15 to 24 months, which suggests that adverse perinatal outcomes cannot explain the negative child-spacing effect since it is mainly shorter birth intervals than 24 months that has been associated with an increased risk of adverse outcomes.

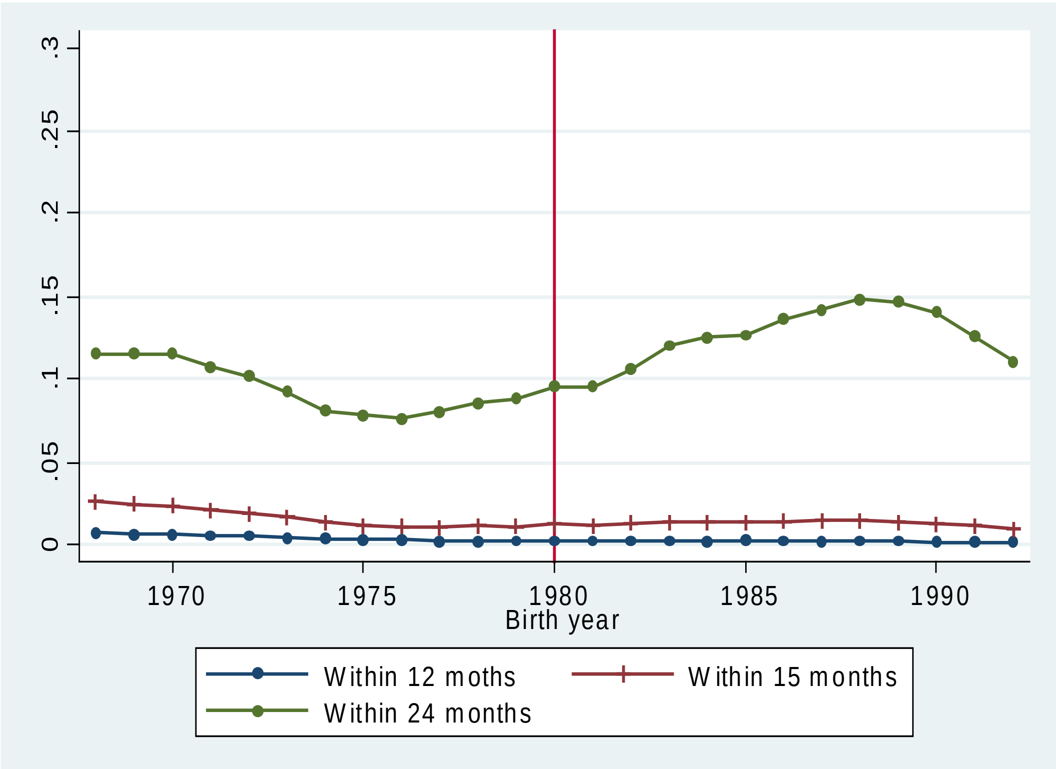

Figure 25 Share of native-born mothers that gave birth to her next child within 24 months, 15 months and 12 moths, respectively

Finally, we estimate the child-spacing effect on another measure of educational performance, namely final grades in compulsory school. These individuals are 15 or 16 years old when they graduate since Sweden has 9 years of compulsory schooling and individuals' typically start at age 7. This outcome is expressed in terms of percentile scores. *Table 17* shows the results for the 2+ sample. The estimated child-spacing effect is about 0.6 percentile scores. A way to gauge the magnitude of the estimated child-spacing effect is to compare it with the gap in percentile scores between girls and boys, which is about 12 percentile scores. In other words, the gender difference corresponds to a 20 months reduction in average child spacing, which should be compared with the 7.5-15 months reduction in average child spacing when university preparatory education was used as the outcome of interest. Thus, although that the estimated child-spacing effect for final grades in compulsory schooling is somewhat smaller than for university preparatory education, it is still reassuring that the have the same signs.[34]

[34] Following a suggestion of Peter Fredriksson, we have also used the grade measure as a control variable in our previous IV approach where we use university preparatory education as the outcome of interest. In this case, the estimated child-spacing effect goes from 2.1 to 1.6. The reason for controlling for final grades, despite the obvious endogeneity problem, is that there has been a change in the grading system during the sample period, and this change could therefore be responsible for the finding of a negative child-spacing effect. However, since the estimated effect is only marginally affected this cannot be an explanation for the estimated child-spacing effect.

6 Discussion and conclusion

In this paper we have estimated a negative association between very close child spacing (less than 2 years) and long-term outcomes of children as measured by educational attainment. We argue that this is a causal relationship since we use a credible source of exogenous variation in spacing, i.e., an administrative child-spacing rule which made it possible for a woman to retain her previous high level of parental leave benefits, i.e., 90 percent wage replacement, without entering the labor market between births provided that the interval between the births did not exceed 24 months. We argue that this rule should *a priori* have a differential impact on child spacing behavior of women from different countries of origin due to sharp difference in taste for work. Specifically, in 1980 women born in one of the Nordic countries (Sweden, Denmark, Finland, and Iceland) had the highest labor force participation rates among all other countries, suggesting that Nordic-born women should be much more affected by the spacing rule than women from another country of origin. Indeed, we find that native-born mothers (women born in a Nordic country) sharply reduces their birth spacing as compared and foreign-born mothers (women born-outside a Nordic country) after the introduction of the child spacing rule in 1980 (see *Figure 5*). Most importantly, native-born and foreign-born mothers have strikingly similar trends in child spacing from 1968 to 1980, which strongly suggests that foreign-born women is a valid comparison group for native-born mothers. Equally importantly, we also show that the levels and trends in the educational attainment for children with a native-born mother are highly similar to the educational attainment for children with foreign-born mothers for birth cohorts born between 1968 and 1980. For later birth cohorts, however, there is a decrease in the educational attainment for children with native-born mothers, both for first-born and second-born individuals (see *Figure 17* for first-born, and *Figure 18* for second-born, *Figure 21* for third-born, *Figure 22* for fourth-born and *Figure 23* for fifth-born). Thus, there is a strong association between the differential change in birth spacing in 1980 and the change in the educational attainment for children.

To further probe whether this association reflects a causal relationship between child-spacing and the long term child outcome, we perform a number of different tests. First, native-born and foreign-born women have similar trends in maternal age at first birth and maternal education before the introduction of the child spacing rule in 1980, which again suggests that foreign-born mothers is valid comparison group for native-born mothers.

Second, the estimated child spacing effect is broadly robust to alteration in the comparison group. For example, we find similar effects when we only use women born in Asia, South America, or Europe. In sharp contrast, when we use North America there is no child spacing effect. These findings are reasonable since the countries in Asia, South America, or Europe around 1980 typically had much lower female labor force participation rates than any of the Nordic countries, while United States or Canada only had just somewhat lower rates. Thus, one should not expect to find a child-spacing effect when the North America sample is used as a comparison group for the Nordic-born women.

Third, the child spacing rule does not affect completed family size, which suggest that is the change in timing of births rather than a change in completed fertility that is responsible for the association child-spacing and the long term child outcome. Fourth, the child spacing rule does not affect the mothers' divorce rates, which otherwise could have been a mediating factor. Fifth, we find similar negative-child spacing effects for both boys and girls. Sixth, the child-spacing effect is also similar for women with high or low maternal education, which suggests that maternal employment is not likely to be a mediating factor since education levels are typically highly correlated with maternal employment. Seventh, the child-spacing effect is also broadly similar for children with high exposure and low exposure to out-of-home child care, which suggests that neither out-of home child care, nor maternal employment (out-of-home care is almost by definetion associated with maternal employment) could be mediating factors. Eight, the child-spacing effect is also similar in areas where school resources are high or low.

Taken together, the above results suggest that one plausible explanation for the consistent finding of a negative child-spacing effect is that a child's development may be particularly vulnerable to changes in their environment at an early age, i.e., the subsequent birth of a younger sibling within a two-year interval. There is by now a growing consensus that early childhood experiences may have a uniquely powerful influence on the development of cognitive and social skills (Knudsen et al., 2006). Thus, it may therefore be particularly detrimental for a child's future development to have another sibling at a very young age since when children are very closely spaced, childcare obligations dominate, i.e., a mother must give most of her attention, commitment or energy to the newborn infant. If this event causes toxic stress in early childhood, where toxic stress refers to strong, frequent or prolonged activation of the body's stress management system, then this can have "disruptive effects on the nervous system and stress hormone regulatory system that can damage developing brain architecture and chemistry and lead to life long problems in learning" (Center on the Developing Child at Harvard University 2007).[35]

[35] For more information about the toxic stress on child development see National Scientific Council on the Developing Child (2005) and the references cited therein.

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