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

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First version: November 30, 2006
This version: November 30, 2007

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 women 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 decrease in a number of educational outcomes (years of schooling, post-secondary educational attainment and final grades in compulsory school). Thus, our instrumental variable estimates show that the effect of spacing children closer has a negative impact on children's future outcomes. We also find evidence that girls are more negatively affected than boys of being closely spaced.

[Θ] We thank Mårten Palme for providing us with the data. We also are grateful for comments from Josh Angrist, Anders Björklund, Peter Fredriksson, Per Johansson, David Strömberg, Olof Åslund and seminar participants at IIES, IFN, Institute for Futures Studies, Uppsala University, University of Amsterdam, and European Society of Population Economics Conference (Chicago 2007).
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1. Introduction

This paper empirically investigates whether child spacing − the time interval from one child's birth date until the next child's birth date − has an effect on children's performance later in life as measured by school performance and educational attainment. Although, there is a large literature that deals with the effect of other family characteristics, such as family size and birth order, on child outcomes,[1] there are hardly any work that deals with this issue.[2, 3]

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.[4] 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. As an illustration of the salience of child spacing, a Google search on "child spacing" gave about 141, 000 hits while the related concept "birth spacing" gave about 158,000 hits.[5]

The effect of child spacing on child outcomes is not only of interest in its own right, but the issue is at the heart of 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". This paper presents evidence that cast doubt of the validity of using twin births as an instrument for family size.[6]

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[1] 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).

[2] There are only few studies in sociology (e.g., Powell and Steelman 1990, 1993) and in economics (Stafford 1987, and Holmlund 1984) that correlates 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, and in contrast to this study, these work only have very crude measures of child spacing such as the number of siblings within a particular age range.

[3] There is a large literature that investigates whether child spacing affects child mortality. For example, 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).

[4] 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.

[5] The Google search was made in June 17, 2007.

[6] Grönqvist and Åslund (2007) find no effect of family size on child outcomes using the twin-birth design on data from Sweden.

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 parental choices. Clear evidence of this endogeneity problem is provided by the large literature investigating factors related to the timing of births in demography and in economics.[7] Consequently, one cannot simple regress child outcomes on child spacing since child spacing will be correlated with unobserved parental characteristics.

The contribution of this paper is to use an administrative child-spacing rule in Sweden, together with a very large administrative data set of the entire Swedish population over an extended period of time, to investigate whether child spacing has a causal impact on child outcomes.[8] From 1980, an administrative rule made it possible for women 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. 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. Due to differential attachment to the labor market, the child-spacing rule had much larger effect on the child spacing of native-born mothers than foreign-born mothers (i.e., those women born outside the Nordic countries).

As a result, it is possible to use an instrumental variable approach when estimating the effect of child spacing on child outcomes where we argue that the administrative rule constitutes a valid instrumental variable for child spacing conditional on group and time-fixed effects, i.e., cohort-specific effects. Specifically, we estimate an OLS regression of child spacing (the birth interval between the first-born individual and its second-born sibling) on a dummy for having a native-born mother, a full set of cohort-fixed effects, and indicator for being in the group affected by the policy (individuals born after 1980 by native-born mothers). We then use the predicted values from this regression in a second step where we now regress the individuals' outcomes (i.e., number of years of schooling, final grades in compulsory school and whether they have a post-secondary education) on a dummy for having a native-born mother and a full set of cohort time fixed effects. In other words, the identification assumption is that the policy reform is orthogonal to the individuals' outcomes.

[7] For work in economics: see for example, Heckman *et al*. (1985), Heckman and Walker (1990), Newman (1983), and Newman and McCulloch (1984). For studies based on Swedish data, see Heckman *et al*. (1985), Heckman and Walker (1990), and Walker (1986, 1995).
[8] This reform has been studied previously by demographers. See for example Hoem (1993), Andersson (1999, 2002), and Andersson *et al*. (2006).

Although we cannot directly test the identifying assumption, an implication of our difference-in difference/IV approach is that the two groups (individuals with native-born or foreign-born mothers) should have similar trends in both child spacing and outcomes before the child spacing rule was introduced. In fact, we provide extensive evidence that the two groups have parallel pre-treatment trends in both child spacing and in the child outcomes for an extended period of time (as long as 12 years). Moreover, native- and foreign-born mothers also have similar pre treatment trends in mothers' age at first birth and the number of years of schooling. Furthermore, we find no evidence that the child spacing rule had an effect on completed fertility (family size) which otherwise would have raised concerns about the validity of our exclusion restriction. Perhaps most importantly, we construct a refutability test by comparing the child outcomes for with native-born or foreign-born mothers in one-child families. Since one-child families should not be affected by the child spacing rule but otherwise be exposed to the same unobservables as families with two or more children, they provide a litmus test of whether individuals with foreign-born mothers constitute a valid control group for individuals with native-born-mothers. It is reassuring that individuals in one-child families have similar trends in child outcomes both before and after the introduction of the child spacing rule. Taken together, these results strongly support the validity of using second generation immigrants (i.e., individuals with foreign-born mothers) as a control group for individuals with native-born mothers.

We find that a decrease in child spacing has non-trivial effects. For example, according to our instrumental variable estimate a mother that decreases her birth interval with almost four months due to the administrative spacing rule would, on average, lead to a reduction of about 0.08 standard deviations in the grades of her first-born child. This is about one third of the effect found in Project Star when comparing the performance of students that was randomly assigned to either a small class or to a large class (Krueger 1999). We also find evidence that girls are more negatively affected than boys of being closely spaced.

One possible explanation for the negative effect of close spacing is through dilution of parental resources. When children are closely spaced, childrearing obligations dominate, i.e., parents can not give their undivided attention, commitment or energy to any one child. Close spacing may also drain economic resources. This is line of reasoning consistent is with the quality-quantity models of Becker and Lewis (1973) and Becker and Tomes (1976) since, as Hanushek (1992) pointed out, decreased child spacing acts like an increase in family size because smaller spacing means a lower probability of being in a small family and less attention and resources from parents. A negative effect of close child spacing is also consistent with Blake's (1981, 1989) resource dilution theory.[9]

In the Swedish context, a mother's time constraint is probably the more important explanation for the negative impact of close spacing on child outcomes since primary and secondary education is free of charge in Sweden. Moreover, there were very limited possibilities of child care services around the time of the introduction of the administrative spacing rule since there was only publicly provided day care and the demand for these services was very high compared to the supply.[10] For example, in a survey in 1980 made by Statistics Sweden,[11] there was a demand for 348,000 (i.e., 48 percent of the children aged 0 to 6) slots while the supply was only 211,000.[12] As a consequence of the shortage of the day care slots, women were forced to take care of their own children. Thus, if a woman chooses to have her children close spaced then the first-born child will experience a dramatic decrease in the mother's attention when the second-child arrives. Our evidence that first-born girls are much more negatively affected than first-born boys is also consistent with the time constraint explanation since girls typically require less attention from their mothers than boys due to their faster rate of development of various skills in their early childhood. Thus, a boy child (less than 2 years) would therefore require more attention than a girl child when the second-child arrives.

Another issue is that the negative effect of having children closely spaced may be highly non-linear. As noted previously, the identifying variation in child spacing 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. It may be the case that shortening the spacing below two years is particularly detrimental to a child future development. Our analysis cannot address this issue since we lack any exogenous source in child spacing of variation in this range. Consequently, we cannot address questions about the optimal spacing choice. Nevertheless, the child spacing rule had a dramatic impact on spacing choices of a very large fraction of women in Sweden which increases the external validity of our findings.

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[9] Another theory is the confluence model by Zajonc and Markus (1975) and Zajonc (1976). However, this theory predicts a positive relationship between long-term child outcomes and close spacing.

[10] For a detailed description of the Swedish child care and family policies circa 1980, see Kamerman and Kahn (1981).

[11] Barnomsorgsundersökningen, 1980, Statistiska meddelanden, S 1980:20, Statistiska centralbyrån.

[12] As of 2006, more than 84 perecent of the children aged 1-6 attend child care. Sweden's child care policy is also interesting in its own right since it is an example of a childcare system were the government heavily subsidizes day care activities for young children with working mothers (e.g., local governments spend about 2 percent of GDP on day care). The Swedish child care system has therefore received some attention among economists (e.g., Becker (2005), and Rosen (1996, 1997)).

Although, we stress the results from our IV approach the reduced form outcomes, i.e., the effect of the child spacing rule on fertility behavior and the effect of the child spacing rule on child outcomes, are also of independent interest.[13] There is, for example, a current discussion in several countries of how to promote fertility through economic incentives. Germany has recently introduced a speed premium (36 months) on future childbearing similar to the Swedish one. Our results suggest that such a policy would have no effect on completed fertility (family size) i.e. but only an impact on the timing of births instead.[14] Moreover, the German child-spacing rule is also likely to have a negative impact on child outcomes as well.

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 present the results of the effect of child-spacing on child outcomes from using two-stage least square and Wald estimators where the administrative rule is an instrumental variable for child spacing, while Section 5 concludes.

[13] In contrast to the IV approach, the reduced form outcomes do not require an exclusion restriction to have a causal interpretation; only that the instrument (child spacing rule) is as good as randomly assigned.
[14] See Spiess and Wrohlich (2006) for a discussion of the parental leave benefit reform in Germany.

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.[15] 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 of 24 months preceding the birth-related withdrawal. Those who did not fulfill 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.

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.[16] Thus, this gives 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 practical implementation of the parental leave system.[17] Therefore, one cannot claim that politicians deliberately created incentives for the close spacing of children.

[15] 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.

[16] 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.

[17] 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 in general, and state agencies in particular, have a high degree of independence and decentralisation.

Thus, there are no 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 child spacing behavior in Sweden. This figure show 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 comparison. 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 differently affected by the speed premium rule and (ii) that the country of birth is exogenous with respect to the administrative reform.

To begin with, 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 and foreign born mothers are affected by the child spacing rule but to 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 control group. Table 1 shows the labor force participation rates for native-born and foreign-born women. The upper panel shows the figures for those in childbearing ages, i.e., those women aged 16-44, and the lower panel for those women with children less than seven years of age. Table 1 reveals that labor participation rates are significantly higher for native than foreign-born women for both categories of women; both before and 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.

The markedly lower participation rate for foreign-born women is also consistent with information provided by country specific labor market surveys. In 1980, the OECD countries had on average a labor force participation rate of 51 percent while Sweden had the highest rate (OECD Labor Market Statistics).[18] As a result, our way of classifying women into treatment and control groups based on their country of birth can therefore be thought of as

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

capturing different *cultural* norms for women's decision to work or not as discussed by Fernández (2007). In other words, we would have come to the same classification of the control and treatment groups, if we instead would have used female labor force participation (LFP) in the country of birth as a cultural proxy for the work behavior of women, and categorized mother's treatment status based on countries with high and low female LFP.[19]

Mother's country of birth is also a useful way of classifying individuals into treatment and control groups since country of birth is an immutable characteristics, i.e. cannot be affected by the treatment itself or by individuals reaction to the treatment. In other words, if we instead should classify the treatment and control group based on a measure of labor force attachment instead (e.g., mothers who work or do not work) 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 is being used as a grouping variable in difference-in-differences approaches (e.g., Heckman 1996 and Blundell *et al*. 1998).[20] Since the treatment status is assigned on the basis of a mother's country of birth, the effect of the administrative rule on the mother's outcome is therefore like an "intention to treat" effect. Thus, the reduced form effect measures the differential impact of the child spacing rule on the child spacing behaviour of native versus foreign-born mothers.

It is not enough that country of birth is a useful way of classifying individuals 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 the outcome absent the intervention, i.e., the parallel trend assumption.

Figure 2 shows the development of child spacing between first-born and second-born children for the two groups from 1968 to 1992. This figure shows that the two groups seem to 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. 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 difference-in-difference framework. Table 2 reports OLS

[19] Labor force participation rates in 1980: Australia 52.7, Austria 48.7, Belgium 47, Canada 57.8, Denmark 71.8, Finland 70.1, France 54.4, Germany 52.8, Greece 33, Ireland 36.3, Italy 39.6, Japan 54.8, Luxembourg 39.9, Mexico 33.7, Netherlands 35.5, New Zealand 44.6, Norway 62.3, Portugal 54.3, Spain 32.2, Sweden 74.1, Switzerland 54.1, United Kingdom 58.3, United States 59.7. The Nordic countries (Sweden, Denmark, Finland and Norway) have the highest labor force participation rates.

[20] Heckman (1996) criticizes Eissa's (1995) use of income as a grouping variable sine some women may switch groups as a result of the tax reform, leading to biased estimates of the behavioral effect of the reform.

estimates of an unconstrained set of interactions between the treatment group indicator (children with native-born mothers) and time effects, i.e.,

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

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 (i.e., the parallel trend assumption). 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, Figure 3 shows the estimated native-year interactions from Column 1 in Table 2 with the corresponding 95 percent confidence intervals. Figure 3 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 relatively to foreign-born mothers. After 1985, the two groups seem to have similar child spacing trends which are 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 a five years time, i.e., from 1980 to 1985.

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 4 shows that the distribution of child spacing for children with foreign-born mothers is only somewhat affected after the reform as compared to before the reform. In sharp contrast, the distribution for native-born mothers has clearly shifted to the left after the reform in 1980 as displayed in Figure 5. The shift in distribution seems to be particularly pronounced for spacing levels around 24 months. Taken together, these facts strongly suggest that it is the introduction of the speed-premium rule that has caused the shift in the distribution of child spacing for native-born mothers.

To further probe the comparability of the treatment and control groups, Figures 6 and 7 display mother's age at first birth and the number of years of schooling for these two groups. These figures show remarkably similar trends in mother's age at first birth and number of years of schooling before the administrative child spacing rule was introduced in 1980. However, native born mother's age at first birth started to decrease relatively to foreign-born mothers in 1980. This is not surprising since a mother's age at first birth is likely to be affected by the spacing rule. In other words, since child spacing is defined as the difference between a mother's age at 2nd birth and her 1st birth, then if child spacing is affected by the child spacing rule then mother's age will also be affected. As a result, a mother's age at birth cannot be used as a control variable in the analysis of the effect of child spacing on child outcomes since it will be endogenous.

There are also other factors that are likely to be endogenous to the child spacing rule such as employment status or earnings. Figures 8 and 9 show log earnings and the employment rates, both measured in 2003, for these two groups. Figure 9 reveals that there is a larger difference in employment rates between native and foreign born mothers who gave birth to their first-born child before 1980 than those who gave birth to their first-born child after 1980. This suggests that the child spacing rule may have had an affect on mother's labor supply decisions. These types of variables should therefore not be used as control variables; only characteristics that were determined before the treatment, i.e., the child spacing rule, should be used as control variables (Rosenbaum 1987).

One such pre-treatment characteristic is a mother's country of birth. Grouped information about the region of birth for the treatment group, i.e., native-born mothers, and the control group, i.e., foreign-born mothers is provided in Table 3. 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 similar and high labor market attachments (see footnote 16). Table 3 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 American, South America, Asia, Oceania, and Soviet Union. Table 3 reveals that of the total of 33,382 of foreign-born mothers in our sample, 55 percent of foreign-born mothers are born in a European country (i.e., EU 15 or Europe), 26 percent are born in an Asian country, while the others are born in some of the other remaining groups. Figure 10 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 of the introduction of the speed-premium rule in 1980. Figure 11 displays how the composition of the country of birth among foreign born mothers has changed 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 11 reveals that proportion of the Asian group has increased over time while the rest of Europe has decreased and EU 15 and the shares of the other groups have remained more or less constant.

One might question whether foreign-born mothers constitute a valid control group for native-born mothers since the foreign-born mothers have emigrated from very different groups of countries. However, the evidence provided above suggests that they have remarkably similar trends in most of the characteristics before 1980, such as child spacing, years of schooling, and mother's age at first birth. Nevertheless, one might still worry about compositional changes in the control group. One way of addressing this is to 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 in specification (1). The estimated child spacing effects are hardly affected as can be seen in Column 2 in Table 2. This 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 problem of comparability of the treatment and control groups is to restrict the sample of foreign-born mothers to, say, only those who emigrated from a European country, (i.e., EU15 and Europe) since these women may be more comparable to native-born women on *a priori* grounds. As shown below, the results are robust to various restrictions on which countries to include in the control group.

Another issue is that that the administrative child spacing rule may have affected not only child spacing but also completed family size.[21] This would raise concerns about the excludability of our instrument in the child outcome equation. In Table 4, we provide

[21] Milligan (2005) and Lalive and Zweimuller (2005) find evidence in support for 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.

information about family size in the treatment and control groups. The table shows that the most frequent family size for natives and immigrants is two, 42 and 36 percent, respectively. Moreover, the distributions of family size across these two groups are also rather similar.

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 12 display the development of average family size across the treatment and the control group. This figure shows that they have parallel trends. In addition, we have also estimated the following difference-in-difference specification for family size:

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

where 1[.] is an indicator function. We cannot reject that $\beta=0$, since $\hat{\beta}=0.03$ with a standard error of 0.11. 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 women as discussed by Walker (1995) and Björklund (2006). Moreover, there is *a priori* no reason to suspect that the speed premium should cause *lifetime* fertility (i.e., family size) to change due to its specific design. This line of reasoning is also consistent with the implications from life-cycle models of fertility as discussed by Hotz, Klerman and Willis (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."

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 data matched with the longitudinal data base LOUISE and the *Årskurs 9 registret*, which includes final grades in compulsory school.[22] All data was provided by Statistics Sweden. We restrict the analysis to all 1st born individuals born between 1968 and 1988 due to limitations of the child outcome data.[23] Child spacing is measured by the birth interval between 1st and 2nd born.[24] Since we restrict the sample to families with at least 2 births and study the outcomes of children born before the 2nd birth we avoid any selection problems due to differential preferences of family size.[25] We also restrict our sample to mothers who are born before 1965 in order to look at completed family size.

Our treatment group will therefore consist of first-born subjects with native born mothers with a family size of two or more, while the control group will be first-born individuals with foreign-born mothers, also with at least two children. In other words, second generation immigrants will be the comparison group for individuals with native-born mothers. Second-generation immigrants is also likely to be a useful comparison group since they will share the *same* intuitional setting (e.g., school system, family policies etc.) as individuals born by native-born mothers. As a matter of fact, we will provide evidence below that second generation immigrants are likely to be a valid control group by comparing child outcomes in one child families. Since one-child families should not be affected by the child spacing rule but otherwise be exposed to the same unobservables as families with two or more children, they provide a litmus test of whether individuals with foreign-born mothers constitute a valid control group for individuals with native-born-mothers.

[22] LOUISE is a register based data set on the total Swedish population which includes information, 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.

[23] The change in administrative rule took place in 1980 and since we only have child outcome data for those born before 1986 for post-secondary education those born before 1989 for final grades in compulsory school, the data does not allow one to look at the outcomes for 2nd and 3rd born children.

[24] We also require child spacing to be larger than 1 year (around 0.10 percent of the population) and less 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 and these individuals will not be included in the analysis anyway).

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

We will use number of years of schooling, post-secondary education, and final grades in compulsory school for first-born individuals as the child outcomes of interests. We only have information on educational attainment for individuals born up to 1985 and data on grades for those born between 1976 and 1988.

Starting with the number of years of schooling as the child outcome of interest, Figure 13 shows the development during the period 1968-1985, separately for children with native-born and foreign-born mothers, respectively. It shows that younger cohorts in both the treatment and the control groups have less years of schooling. This is not surprising since many of them have still not finished college or university. However, the important thing to note in Figure 13 is that both groups seem to have similar trends in the average years of schooling until the introduction of the child spacing rule in 1980 when the difference in the levels starts to converge. In other words, the evolution in the average years of schooling is the basically the same for 12 years (i.e., 1968 to 1979) for the treatment and control group.

We can make a statistical test of whether the two groups have parallel trends by using a difference-in-difference specification, namely to estimate a similar specification as equation (1) but where the child outcome is the dependent variable instead of child spacing, i.e.,

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

Table 5 shows the results from this regression. Looking at Column 1 in Table 5 suggest a statistically significant decline in the number of years of schooling with little evidence of pre-existing (i.e., before 1980) trends. 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, the effects are hardly affected as can be seen in Column 2. This 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. In order to illustrate the main message from the regression (2), Figure 14 shows the estimated native-year interactions from Column 1 in Table 5 with the corresponding 95 percent confidence intervals. Thus, Figure 14 shows that we cannot statistically reject that the two groups have similar trends in the number of years of schooling before 1980 while the two groups have differential trends thereafter since there is a decrease in the years of schooling for the treatment group relatively to the control group. This suggests, anticipating the instrumental variable results, that the reduction in child spacing for native-born mothers also caused a reduction in the number of years of schooling for their first-born children as compared to the first-born children with foreign-born mothers.

Turning to the second child outcome, post-secondary education is defined as an indicator variable taking the value 1 if the individual has obtained an education level higher than secondary school given that this post-secondary education is at least two years (0 otherwise). Figure 15 shows the development of the average share with a post-secondary education during the period 1968-1985, separately for children with native-born and foreign-born mothers, respectively. It shows that younger cohorts in both groups have less post-secondary education. Again, this is not surprising since many of them have still not finished college or university. However, the important thing to note is that both groups have similar trends in post-secondary education before the reform in 1980 where the individuals with native-born mothers have a higher share with post-secondary education than individuals with foreign-born mothers. In contrast, after the reform it is the individuals with foreign-born mothers that have a higher share of post-secondary education than the individuals with native-born mothers. Table 6 shows the results from the statistical test, specification (2), while Figure 16 displays the estimated native-year interactions from Column 1 in Table 6 with the corresponding 95 percent confidence intervals. Table 6 and Figure 16 show that we cannot statistically reject that the two groups have similar trends in post-secondary education before 1980, while we conclude that the two groups have differential trends subsequently since there is a decrease in educational attainment for the treatment group relatively to the control group after 1980. Once again, this suggests, anticipating the instrumental variable results, that the reduction in child spacing for native-born mothers also caused a reduction in post-secondary education for their first-born children as compared to the first-born children with foreign-born mothers.

Turning to third child outcome, final grades in compulsory school will be expressed in terms of z-scores. We made this choice since there is major change in the grading system during the period of study but also since we like to compare the size of the spacing effect to other studies of school performance. Until 1997 the grading system was based on a five-grading scale where a student could receive a score ranging from 1 to 5 in each subject. In data we have information on each individual’s average score in the final grade. In 1998, the grading system was changed to a scale between zero and 320. Figure 17 shows the development of the z-scores for children with native-born mothers relative to foreign-born mothers. As shown in Figure 17, before the reform children with native-born mothers have about 0.07-0.10 standard deviations higher average grades than children with foreign-born mothers but after the reform this difference decreases and from 1984 and onwards they have even better grades than individuals with foreign-born mothers. Thus, the deterioration in final grades before and after the reform for individuals with native-born mothers is consistent with the previous results about years of schooling and post-secondary education, which again suggests that close child spacing is causally related to children's future performance.

Table 7 shows the results from the statistical test, specification (2), while Figure 18 displays the estimated native-year interactions from Column 1 in Table 7 with the corresponding 95 percent confidence intervals. Table 7 and Figure 18 show that we cannot statistically reject that the two groups have similar trends in final grades in compulsory school before 1980 (although this statement is only based on four pre treatment years since grades are only available for the 1976 to 1988 birth cohorts) while 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. 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, 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.

Refutability test: Child outcomes for one child families

Taken together, the results presented above suggest that the administrative rule that came into place in 1980 was responsible for the change in the child outcomes between these two groups of individuals. Nevertheless, there may be many other social, economic and political forces changing over time that may affect the groups differently. For example, individuals with foreign-born mothers could be differently affected by economic shocks, e.g., the severe economic downturn in the early 1990s in Sweden, than subjects with native-born mothers. There could also be that different kinds of policies, such as family policy, that could have differential impact on the outcomes of children in these two groups. Specifically, if there were other family policies that changed simultaneously with the introduction of the child spacing rule reform in 1980, and which would have had differential impact on native and foreign-born mothers, this would be detected by the refutability test.

In order to address if there are such confounding factors that could potentially explain our result, we construct a "refutability" test.[26] Arguably, individuals in one-child families should be exposed to more or less the same unobserved factors as subjects in families with two or more children with the important exception that one-child families should not be affected by the child-spacing rule, i.e., there is no child spacing effect for one-child families. As a result, one could test whether the child outcomes evolve differently for subjects with native-born and foreign-born mothers in one-child families. Since one-child families should not be affected by the administrative rule, there should be no effect of speed premium for this group unless there are important unobserved confounders. Thus, we can estimate equation (2) for the subpopulation of one child families and test whether the β's should is zero before and after 1980. Table 8 and Figures 19-21 show the estimated year×native interaction effects using number of years of schooling, post-secondary education and final grades in compulsory school, together with a corresponding 95-percent confidence intervals. As can be seen from the three figures, the β's are zero before and after the introduction of the speed premium, i.e., second generation immigrants in one-child families have the same trend in child outcomes as compared to individuals with native-born mothers. Thus, this lend strong support to that, in families with more than one child, individuals with foreign-born mothers also constitute a valid control group for subjects with native-born-mothers.

[26] See Angrist and Krueger (1999) for a discussion of refutability tests.

4. The impact of child spacing on child outcomes

In this section we present results of the effect of child spacing on child outcomes. Under the assumption that the administrative child-spacing rule – 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 year $\times$ native 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 the local government level since they are in charge of providing compulsory schooling and that may cause individual outcomes to be correlated across time for a specific locality.[27] Since there are almost 300 local governments this will provide a sufficient number of clusters for the clustering estimator to have good properties as discussed by Bertrand *et al*. (2004). We have also clustered the standard errors on year×group as a way to correct for the Moulton problem as discussed by Donald and Lang (2006). In this case we typically get somewhat smaller standard errors than those presented in the tables.

Before showing the results from the Wald/IV and the 2SLS approaches, we present results from OLS regressions, as a benchmark for assessing potential biases in the OLS approach. Table 9 displays the results for years of schooling, post secondary education as well as final grades in compulsory school. Without any controls, the OLS estimate is -0.006 for years of schooling (Column 1), -0.001 for post secondary education (Column 3) and -0.004 for final grades in compulsory school (Column 5). This means that one month *shorter* birth

[27] See Björklund *et al*. (2005) for a discussion of the Swedish school system.

interval will lead to 0.006 *more* years of schooling, a 0.01 percent *higher* probability of having a post secondary education and that the grades will be a 0.004 higher standard deviation higher. When we add controls for the mother's 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 are hardly affected as can be seen in Columns 2, 4 and 6.

Turning to the instrumental variable approach, Table 10 displays the results from the Wald and the 2SLS methods for years of schooling. The 2SLS estimate is 0.085 while the Wald/IV estimate is 0.095 in the specification without any additional control variables. Thus, one month *shorter* birth interval will lead to almost one tenth of a year *shorter* education. These estimates are also statistically different from zero and of the opposite sign from the OLS estimates in Table 9. This suggest that the OLS estimate are strongly biased and not to be trusted. One reason for the different sign of the OLS estimates is that high ability parents choose to bunch there children closely together as a way to avoid too many breaks in there job marker careers. When we add controls for the mother's level of education and full set of interactions between a mothers region of birth and the year of immigration the effects are hardly affected 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 seem to be small differences regarding the point estimate of the two estimators and their associated standard errors.

Turning to the second child education outcome measure, Table 11 displays the results from the Wald and the 2SLS methods for post-secondary education. The 2SLS estimate is 0.020 while the Wald/IV estimate is 0.025 in the specification without any additional control variables; both estimates are statistically different from zero. Thus, one month *shorter* birth interval will lead to 2-2.5 percent *lower* probability of getting a post-secondary education. Neither for post-secondary education the effects are hardly affected when adding the control variables (see Columns 2 and 4). Based on the F-statistics, the Wald/IV estimator may be preferred to the 2SLS estimator from a weak instrument point of view.

Table 12 shows the results from the Wald and the 2SLS methods for final grades in compulsory school. Again, we see that the 2SLS and Wald/IV estimates are very different from their OLS counterparts in Table 9. The 2SLS and the Wald/IV estimates without controls are 0.018 and 0.023, respectively. The estimated effects when adding the controls are hardly affected as can be seen in Columns 2 and 4. From the weak instrument point of view, the Wald/IV estimator is again to be preferred. Nevertheless, there seem to be small differences regarding the point estimate of the two estimators and their associated standard errors.

Another issue related to whether one should use a single instrument or multiple instruments in our IV specifications is the interpretation of the estimated effect. When the effects are heterogeneous and the instrument is binary, the Wald parameter typically has a LATE interpretation (Angrist and Imbens 1994). In our case, the Wald estimate measures the average effect for those children with mothers who react to the incentives created by the administrative rule by spacing their children within the required 24 months period. Since different cohorts may differ from each other with respect to individual characteristics, the child spacing effects may be heterogeneous across cohorts. We can use the 2SLS-estimator and perform Hansen's J-test for overidentifying restrictions on all child outcomes to test for heterogeneous effects since each native-cohort interaction from 1980 and onwards are used as instruments. Results indicate that the IV-estimates are statistically equal to each other since we cannot reject the null hypothesis of the overidentfying restriction test for any of the child outcome maesures. This implies that or previous results can be generalized to all children with mothers who space their children within 24 month due to the administrative rule, independent on which cohort they belong to.

In Tables 13-15, we test whether the child-spacing effect differ across families of different sizes. 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 the different sub-samples we only report the Wald/IV estimates as to avoid problems of many weak instruments. For comparison, Column 1 restates the Wald/IV estimates with control variables from Tables 10-12.

Starting with years of schooling as the child outcome of interest, the estimates are 0.095 for all family sizes, 0.125 for family with 3 or more children, and 0.13 for families with at least four children. Thus, the child-spacing effect increases slightly with family size. Table 13 also shows that the first-stage *F*-statistics is always much larger than 10 suggesting that there are no problems with weak instruments.

As regards, post-secondary education the estimates are 0.024 for all family sizes, 0.026 for family with 3 or more children, and 0.023 for families with at least 4 children. Again, the *F*-statistics is always much larger than 10. Turning to final grades in compulsory school, the child spacing effect is 0.029 for families with at least three children and 0.036 for those with four or more children. This again suggests that the effect is somewhat larger in larger families (the F-statistics are also in this case always larger than 10).

Next, we investigate whether effects are sensitive to the choice of control group. We choose to disaggregate the control group into two groups: mothers born in European country and non-European country, respectively. We avoid further disaggregating of the control groups since this would imply rather few observations per year-cohort cell. Table 16 reports the distribution of observations per year separately for the treatment group and the two control groups. The year/cohort size is between 24,000 and 35,000 for the treatment group while it is only between 300 and 1,100 for the two separate control groups. Thus, the combined control group used in the previous analysis consists of cell sizes of 800 to 1,800.

Table 17-19 show estimations when mothers born in either a European or in non-European country are used as the control groups. Again, for comparison, Column 1 restates the Wald/IV estimates with control variables from Tables 10-12**.** For years of schooling and post-secondary education the effect is marginally larger when only mothers from Europe are used as the control group. Interestingly, the F-statistics is much larger when mothers from non-European countries are used as the control group.

Turning to the final grades in compulsory school, estimated effects are hardly affected by the choice of control group. Again, the F-statistic is much higher when mothers from non-European countries are used as control group.

We also report results from whether there are gender differences in effect of the child spacing on child outcomes. Tables 20 show the results for boys and girls without any additional controls while Table 21 includes controls. From these tables one can clearly see that the estimates for girls are at least twice as large the corresponding estimates for boys.

6. Conclusion

In this paper we have discovered a negative association between close child spacing and children's future outcomes, i.e., school performance and 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 *a priori* should have a differential impact on mothers of different origin of births due to the different labor market attachments. We also made a number of robustness checks of our results. First, the reform *only* affected the spacing behavior and not completed family size which lends credibility to the exclusion restrictions of the administrative reform. Second, the child-spacing effect is robust to controlling for compositional changes (mother's educational attainment and a full set of interactions between the region of birth and the year of immigration) in the treatment and control groups. Third, the child-spacing effect is present in families of all sizes. Finally, and perhaps most importantly, in contrast to families with 2 or more children there is no differential trend in the outcome for children with native-born mothers compared to foreign-born mothers in families with only one child, which suggests that it is child-spacing and not any unobserved factor that affects the outcome for children with two or more siblings. Taken together these results strongly suggest that child spacing is causally related to children's future success.

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Table 1. Labor force participation rates

	1979	1985
Women aged 16-44		
Native-born	0.75	0.79
Foreign-born	0.61	0.63
Women with children under 7		
Native-born	0.79	0.80
Foreign born	0.58	0.59

Table 2. 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)	
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 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.

Table 3. Mothers' region of birth

	Frequency	Percentage
	Native-born mothers	
Sweden	647,155	95.0
Nordic	33,833	5.0
Total sum	647,155	
	Foreign-born mothers	
EU 15	6,610	19.8
Europe	11,703	35.1
Africa	1,511	4.5
North America	1,130	3.4
South America	3,175	9.5
Asia	8,847	26.5
Oceania	119	0.4
Soviet Union	287	0.9
Total sum	33,382	

Notes. - These groups are taken from the classification used by Statistics Sweden. Nordic includes: Denmark, Norway, Finland, and Island, 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.

Table 4. Number of children in the family (by family and treatment status)

Number of children	Treatment group: Native-born mothers Mean family size=2.7		Control group: Foreign-born mothers Mean family size=2.9	
	Frequency	Percentage	Frequency	Percentage
1	188,613	9.3	14,162	12.5
2	852,271	41.9	40,453	35.7
3	626,087	30.8	28,224	24.9
4	237,746	11.7	15,302	13.5
5	80,112	3.9	7,799	6.9
6	28,279	1.4	3,887	3.4
7	10,177	0.5	1,853	1.6
8	4,310	0.2	1,011	0.9
9	1,849	0.1	416	0.4
10+	2,267	0.1	264	0.2
Total sum	2,031,711		113,371	

Table 5. Number of years of schooling

Effect	(1)	(2)	(3)
Native × 1969	.082	0.05	
	(.095)	(.091)	
Native × 1970	-.039	-.109	
	(.094)	(.091)	
Native × 1971	.056	-.045	
	(.094)	(.091)	
Native × 1972	.127	-.014	
	(.094)	(.091)	
Native × 1973	.202	.063	
	(.091)	(.089)	
Native × 1974	-.038	-.182	
	(.092)	(.090)	
Native × 1975	.044	-.075	
	(.090)	(.088)	
Native × 1976	.177	.036	
	(.090)	(.089)	
Native × 1977	.232	.047	
	(.094)	(.092)	
Native × 1978	.089	-.081	
	(.094)	(.092)	
Native × 1979	.116	-.097	
	(.091)	(.090)	
Native × 1980	-.051	-.228	-.190
	(.088)	(.088)	(.058)
Native × 1981	-.207	-.404	-.366
	(.087)	(.087)	(.057)
Native × 1982	-.189	-.408	-.372
	(.082)	(.083)	(.052)
Native × 1983	-.283	-.484	-.448
	(.079)	(.080)	(.046)
Native × 1984	-.144	-.327	-.292
	(.079)	(.080)	(.047)
Native × 1985	-.381	-.610	-.574
	(.071)	(.073)	(.034)
Controls	No	Yes	Yes
F-test	14.66	18.01	53.40
P-value	(0.0000)	(0.0000)	(0.0000)
R^2	0.2185	0.2837	0.2837
Observations	540,420	540,420	540,420

Note.– Robust standard errors are reported in parentheses. The table reports year × native interactions in regressions that include native and year 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.

Table 6. Post-secondary education

Effect	(1)	(2)	(3)
Native × 1969	.003	-.001	
	(.018)	(.018)	
Native × 1970	-.002	-.011	
	(.018)	(.018)	
Native × 1971	.005	-.007	
	(.018)	(.018)	
Native × 1972	.017	-.001	
	(.018)	(.018)	
Native × 1973	.044	.024	
	(.018)	(.018)	
Native × 1974	.005	-.016	
	(.018)	(.018)	
Native × 1975	.020	.003	
	(.018)	(.018)	
Native × 1976	.025	.005	
	(.018)	(.018)	
Native × 1977	.025	-.004	
	(.018)	(.018)	
Native × 1978	.005	-.022	
	(.018)	(.018)	
Native × 1979	.006	-.031	
	(.017)	(.017)	
Native × 1980	-.032	-.062	-.056
	(.017)	(.017)	(.011)
Native × 1981	-.079	-.109	-.103
	(.016)	(.017)	(.011)
Native × 1982	-.062	-.095	-.089
	(.015)	(.015)	(.008)
Native × 1983	-.062	-.091	-.085
	(.014)	(.014)	(.007)
Native × 1984	-.073	-.098	-.092
	(.014)	(.015)	(.008)
Native × 1985	-.083	-.116	-.110
	(.015)	(.015)	(.009)
Controls	No	Yes	Yes
F-test	7.6	11.5	56.3
P-value	(0.0000)	(0.0000)	(0.0000)
R^2	0.1015	0.1562	0.1562
Observations	543,449	543,449	543,449

Note.– Robust standard errors are reported in parentheses. The table reports year × native interactions in regressions that include native and year 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.

Table 7. Final grades in compulsory school

Effect	(1)	(2)	(3)
Native × 1977	-.039	-.044	
	(.043)	(.041)	
Native × 1978	-.039	-.050	
	(.041)	(.040)	
Native × 1979	.004	-.020	
	(.041)	(.040)	
Native × 1980	-.040	-.047	-.018
	(.041)	(.040)	(.031)
Native × 1981	-.082	-.094	-.066
	(.042)	(.041)	(.032)
Native × 1982	-.095	-.112	-.084
	(.041)	(.041)	(.032)
Native × 1983	-.082	-.091	-.063
	(.042)	(.041)	(.032)
Native × 1984	-.166	-.168	-.140
	(.043)	(.042)	(.034)
Native × 1985	-.102	-.145	-.117
	(.043)	(.043)	(.036)
Native × 1986	-.117	-.139	-.110
	(.046)	(.045)	(.037)
Native × 1987	-.098	-.091	-.063
	(.050)	(.050)	(.043)
Native × 1988	-.121	-.125	-.097
	(.052)	(.050)	(.044)
Controls	No	Yes	Yes
F-test	2.2	2.6	3.3
P-value	(0.0194)	(0.0049)	(0.0005)
Observations	327,906	327,906	327,906

Note.– Robust standard errors are reported in parentheses. The table reports year × native interactions in regressions that include native and year 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.

Table 8. Refutability test

Effect	Number of years of schooling	Post-secondary education	Final grades in compulsory school
Native × 1969	-.098	-.007	
	(.17)	(.032)	
Native × 1970	.059	.004	
	(.16)	(.031)	
Native × 1971	.141	.045	
	(.16)	(.030)	
Native × 1972	.124	.017	
	(.16)	(.031)	
Native × 1973	.136	.047	
	(.16)	(.031)	
Native × 1974	.089	.019	
	(.16)	(.031)	
Native × 1975	.239	.048	
	(.16)	(.030)	
Native × 1976	.097	.020	
	(.16)	(.031)	
Native × 1977	.204	.035	.161
	(.16)	(.030)	(.066)
Native × 1978	.041	.009	.058
	(.16)	(.030)	(.067)
Native × 1979	.020	-.010	.024
	(.16)	(.030)	(.064)
Native × 1980	-.003	-.017	-.008
	(.16)	(.030)	(.068)
Native × 1981	-.022	-.022	.037
	(.15)	(.028)	(.066)
Native × 1982	-.048	-.004	.020
	(.15)	(.025)	(.068)
Native × 1983	-.018	-.018	-.004
	(.14)	(.025)	(.069)
Native × 1984	.164	-.007	.023
	(.14)	(.025)	(.068)
Native × 1985	-.047	-.025	.003
	(.13)	(.026)	(.068)
Native × 1986			-.008
			(.067)
Native × 1987			-.081
			(.071)
Native × 1988			.037
			(.077)
Controls	No	No	No
F-test	1.50	0.55	0.41
P-value	(0.17)	(0.77)	(0.93)
R^2	0.1717	0.0645	0.0009
Observations	143,810	145,205	84,844

Note.– Robust standard errors are reported in parentheses

Table 9. OLS estimates

	Number of years of schooling		Post-secondary education		Final grades in compulsory school	
Child spacing	-0.006	-0.004	-0.001	-0.0007	-0.004	-0.0033
	(0.0004)	(0.0003)	(0.0001)	(0.00004)	(0.0001)	(0.0001)
Controls	No	Yes	No	Yes	No	Yes
R^2	0.22	0.28	0.10	0.16	0.01	0.10
Observations	540,420	540,420	543,449	543,449	327,906	327,906

Note. – Standard errors clustered at the local government 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.

Table 10. Number of years of schooling: 2SLS and Wald/IV estimates

	2SLS		Wald/IV	
	(1)	(2)	(3)	(4)
Child spacing	0.085	0.090	0.095	0.095
	(0.011)	(0.010)	(0.016)	(0.011)
Controls	No	Yes	No	Yes
First-stage F-test	24.9	15.3	20.1	45.6
Observations	540,420	540,420	540,420	540,420

Note. – Standard errors clustered at the local government 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.

Table 11. Post-secondary education: 2SLS and Wald/IV estimates

	2SLS		Wald/IV	
	(1)	(2)	(3)	(4)
Child spacing	0.020	0.020	0.025	0.024
	(0.002)	(0.002)	(0.003)	(0.002)
Controls	No	Yes	No	Yes
First-stage F-test	24.2	12.9	19.2	40.2
Observations	543,449	543,449	543,449	543,449

Note. – Standard errors clustered at the local government 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.

Table 12. Final grades in compulsory school: 2SLS and Wald/IV estimates

	2SLS		Wald/IV	
	(1)	(2)	(3)	(4)
Child spacing	0.018	0.020	0.023	0.025
	(0.005)	(0.005)	(0.006)	(0.007)
Controls	No	Yes	No	Yes
First-stage F-test	14.1	10.4	46.2	69.1
Observations	327,906	327,906	327,906	327,906

Note. – Standard errors clustered at the local government 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.

Table 13. Different family size: Wald/IV estimates for number of years of schooling

	2+child family	3+child-family	4+-child-family
	(1)	(2)	(3)
Child spacing	0.095	0.125	0.130
	(0.011)	(0.018)	(0.024)
Controls	Yes	Yes	Yes
First-stage F-test	45.6	39.5	89.6
Observations	540,420	228,924	69,561

Note. – Standard errors clustered at the local government 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.

Table 14. Different family size: Wald/IV estimates for post-secondary education

	2+child family (1)	3+child-family (2)	4+-child-family (3)
Child spacing	0.024	0.026	0.023
	(0.002)	(0.003)	(0.005)
Controls	Yes	Yes	Yes
First-stage F-test	40.2	41.7	101.2
Observations	543,449	230,388	70,038

Note. – Standard errors clustered at the local government 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.

Table 15. Different family size: Wald/IV estimates for final grades in compulsory school

	2+child family (1)	3+child-family (2)	4+-child-family (3)
Child spacing	0.025	0.029	0.036
	(0.007)	(0.012)	(0.016)
Controls	Yes	Yes	Yes
First-stage F-test	69.1	23.6	15.5
Observations	327,906	140,636	39,154

Note. – Standard errors clustered at the local government 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.

Table 16. Number of first-born each year for the treatment and the control groups.

Year of birth	Treatment group	Control group	
	Individuals with native-born mothers	Individuals with mothers born in a European country	Individuals with mothers born in a non-European country
1968	32,270	912	353
1969	30,957	971	408
1970	32,497	1,011	436
1971	34,588	1,042	521
1972	34,495	1,055	592
1973	33,857	1,035	631
1974	33,708	995	668
1975	32,032	986	696
1976	29,556	990	723
1977	28,310	859	741
1978	26,592	886	777
1979	27,066	878	899
1980	27,901	883	899
1981	26,954	804	920
1982	26,681	754	840
1983	26,398	657	785
1984	26,277	651	755
1985	26,269	627	787
1986	26,453	569	688
1987	25,209	436	516
1988	23,983	377	420

Table 17. Different control groups: Wald/IV estimates for number of years of schooling

	All (1)	European (2)	Non-European (3)
Child spacing	0.095	0.101	0.093
	(0.011)	(0.045)	(0.010)
Controls	Yes	Yes	Yes
First-stage F-test	45.6	14.5	63.7
Observations	540420	529477	526050

Note. – Standard errors clustered at the local government 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.

Table 18. Different control groups: Wald/IV estimates for post-secondary education

	All (1)	European (2)	Non-European (3)
Child spacing	0.024	0.032	0.021
	(0.002)	(0 .012)	(0.002)
Controls	Yes	Yes	Yes
First-stage F-test	40.2	13.4	61.7
Observations	543,449	532283	528873

Note. – Standard errors clustered at the local government 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.

Table 19. Different control groups: Wald/IV estimates for final grades in compulsory school

	All (1)	European (2)	Non-European (3)
Child spacing	0.025	0.024	0.024
	(0.007)	(0.015)	(0.006)
Controls	Yes	Yes	Yes
First-stage F-test	69.1	10.5	81.7
Observations	327,906	320818	320384

Note. – Standard errors clustered at the local government 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.

Table 20. Girls versus boys: IV estimates

	Years of schooling		Post-secondary education		Final grades in compulsory school	
	Girls	Boys	Girls	Boys	Girls	Boys
Child spacing	0.147	0.054	0.035	0.018	0.036	0.014
	(0.023)	(0.024)	(0.006)	(0.003)	(0.011)	(0.006)
Controls	No	No	No	No	No	No
First-stage F-test	13.7	23.2	12.9	23.2	21.7	50.0
Observations	262420	278000	263,775	279,674	161,519	166,387

Note. – Standard errors clustered at the local government 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.

Table 21. Girls versus boys: IV estimates including controls

	Years of schooling		Post-secondary education		Final grades in compulsory school	
	Girls	Boys	Girls	Boys	Girls	Boys
Child spacing	0.135	0.064	0.032	0.017	0.032	0.012
	(0.019)	(0.014)	(0.005)	(0.002)	(0.005)	(0.004)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
First-stage F-test	19.1	75.5	16.8	71.9	19.5	54.4
Observations	262420	278000	263,775	279,674	161,519	166,387

Note. – Standard errors clustered at the local government 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.

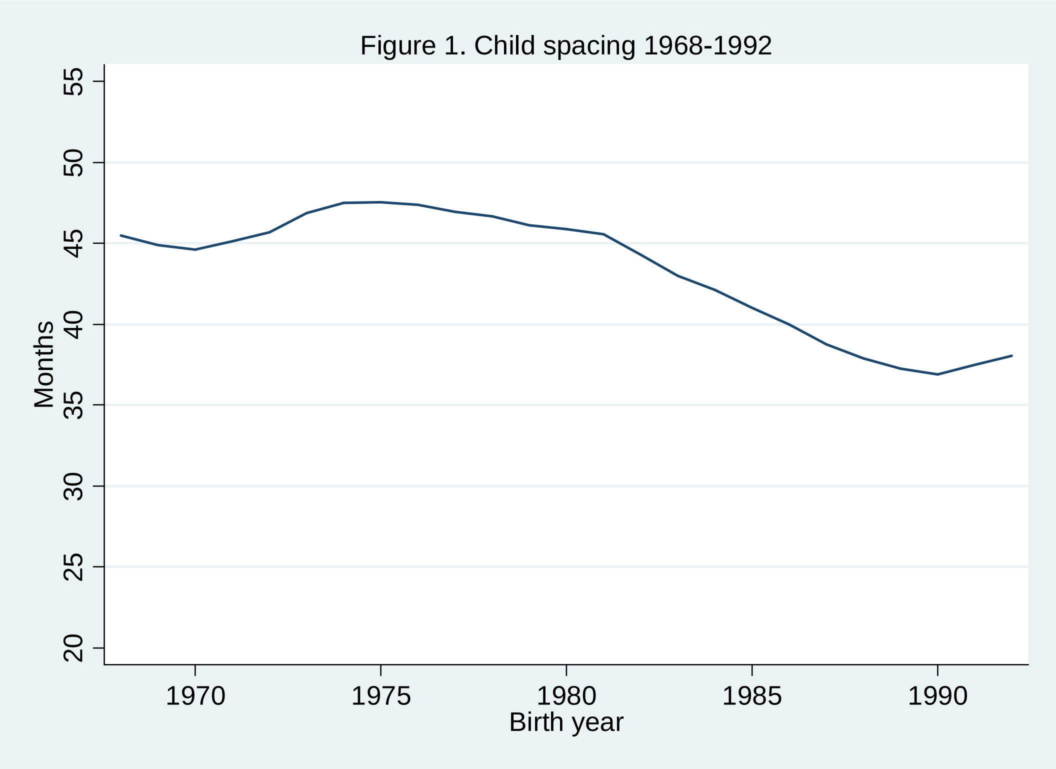


Figure 1. Child spacing 1968-1992
Months
55
50
45
40
35
30
25
20
1970
1975
1980
1985
1990
Birth year

Figure 2. Child spacing

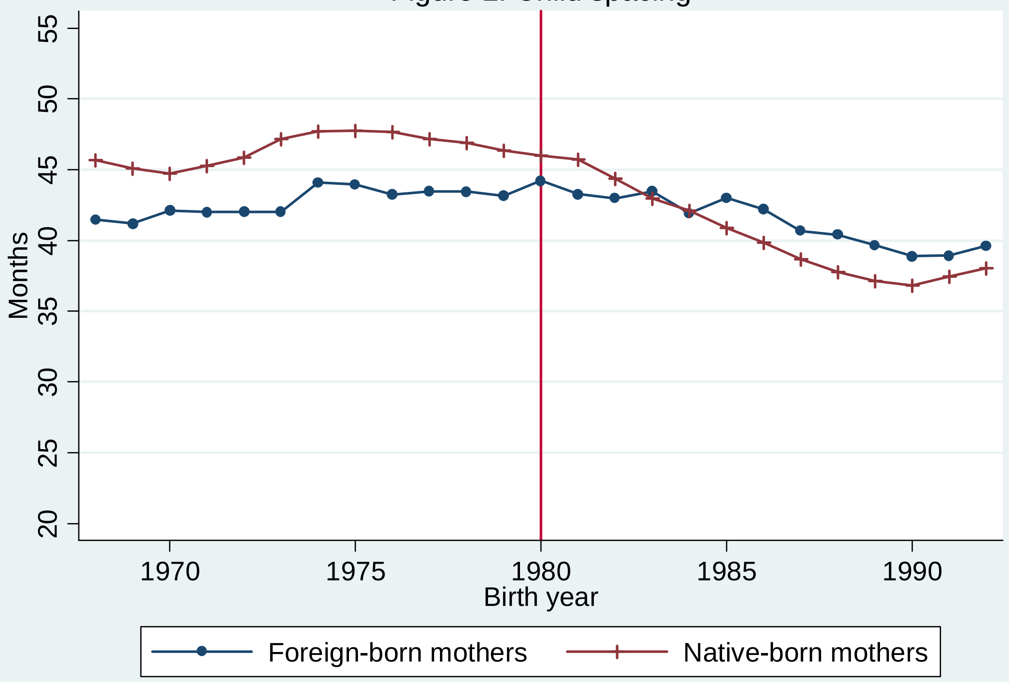


Figure 3. Effects of native year interactions on child spacing
95 percent confidence intervals

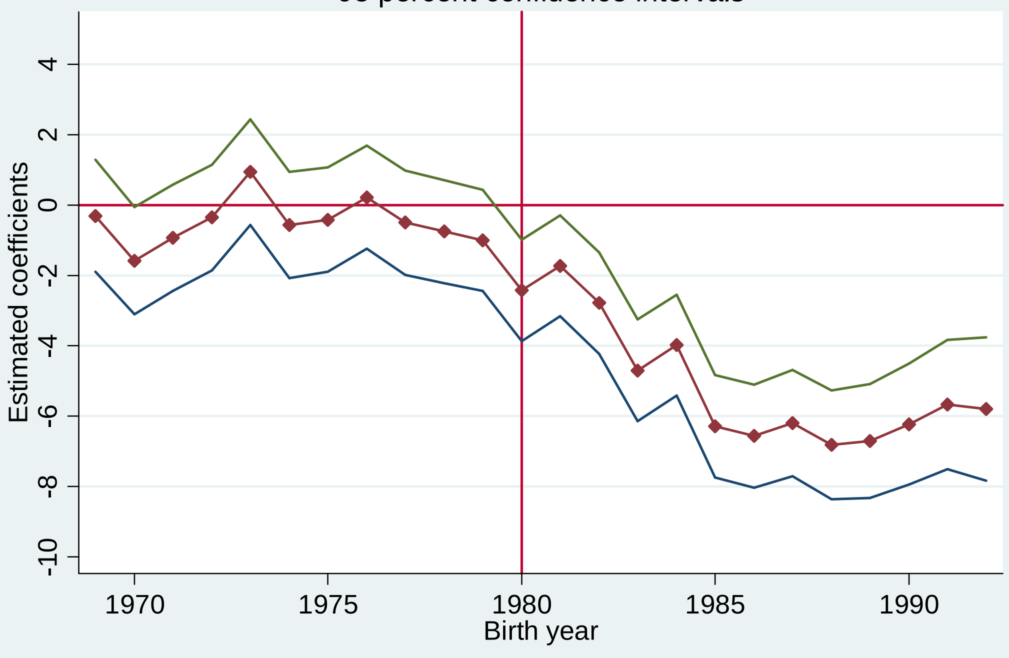


Figure 4. Kernel estimates of child spacing

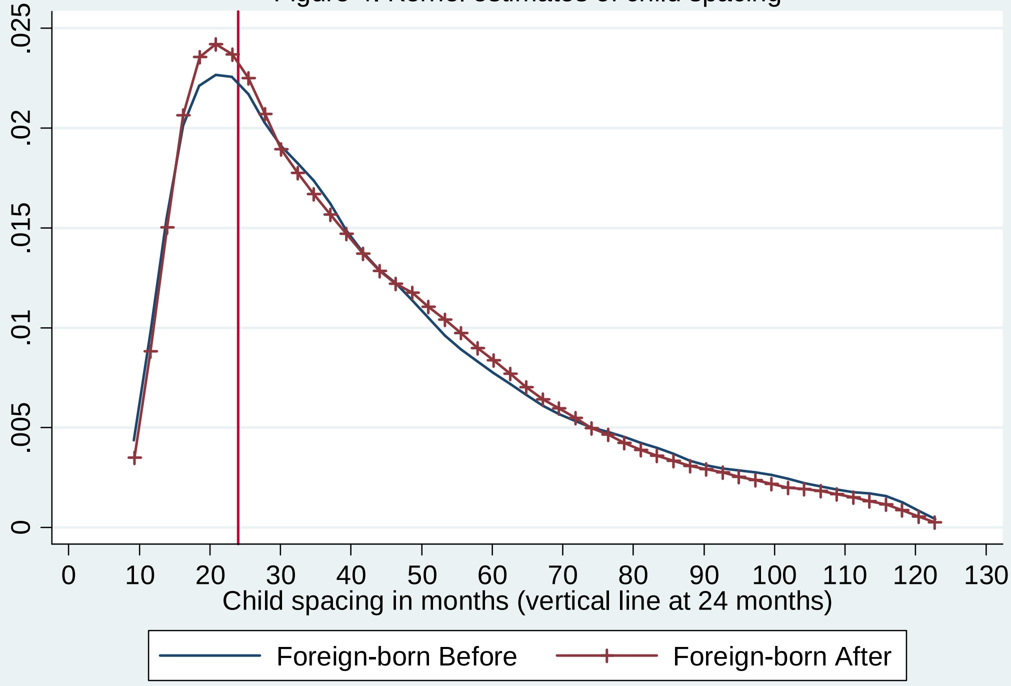


Figure 5. Kernel estimates of child spacing

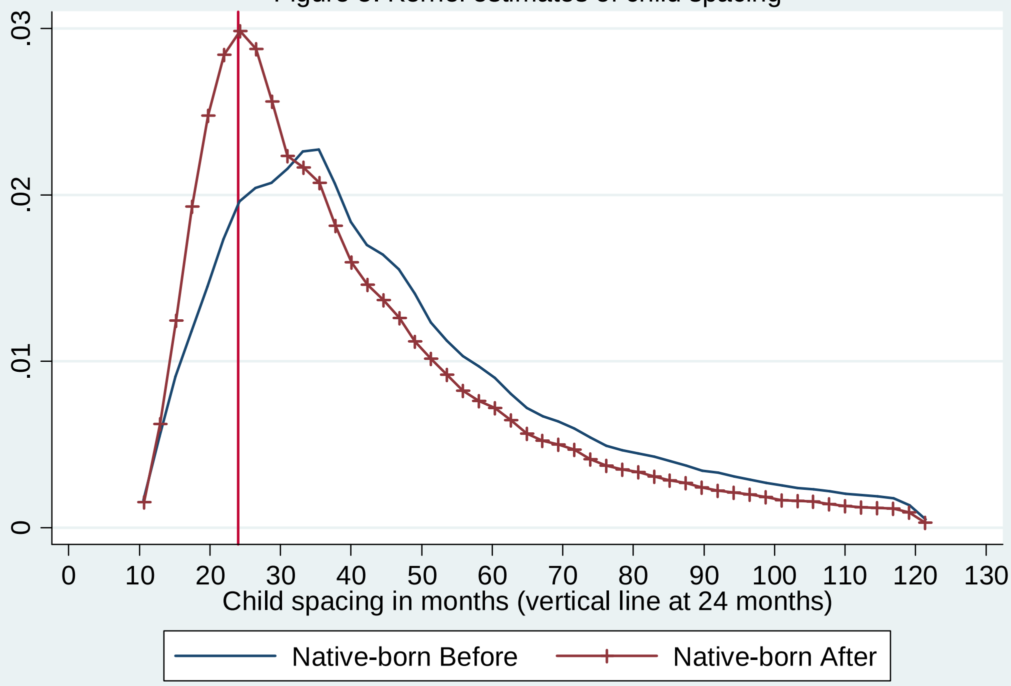


Figure 6. Mother's age at first child birth

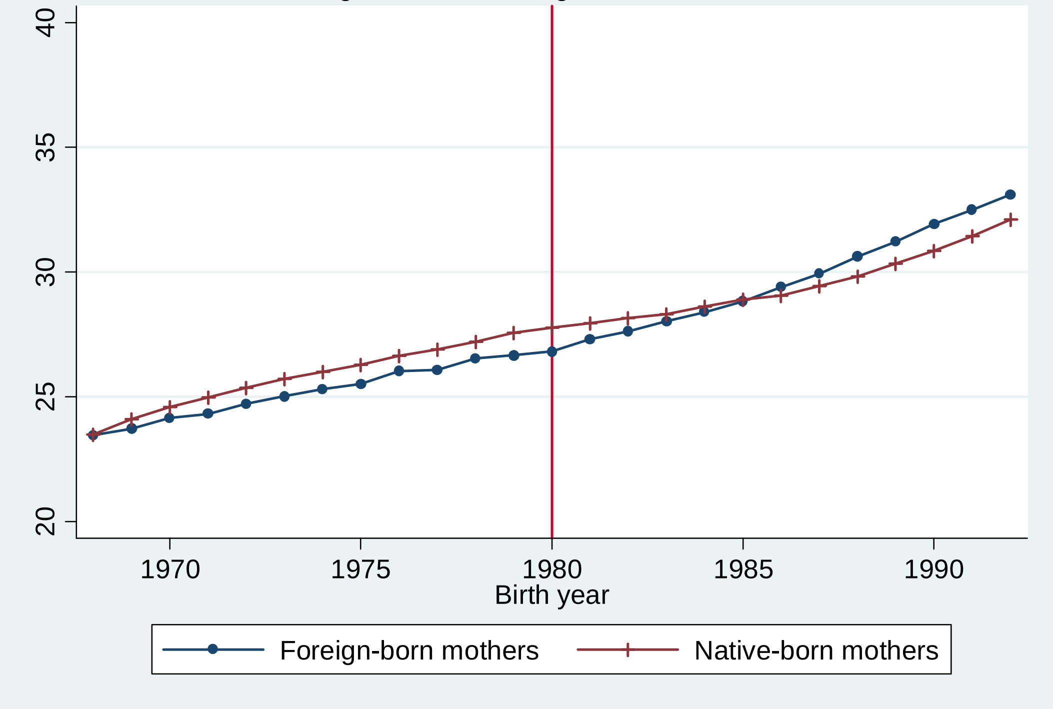


Figure 7. Mother's years of schooling

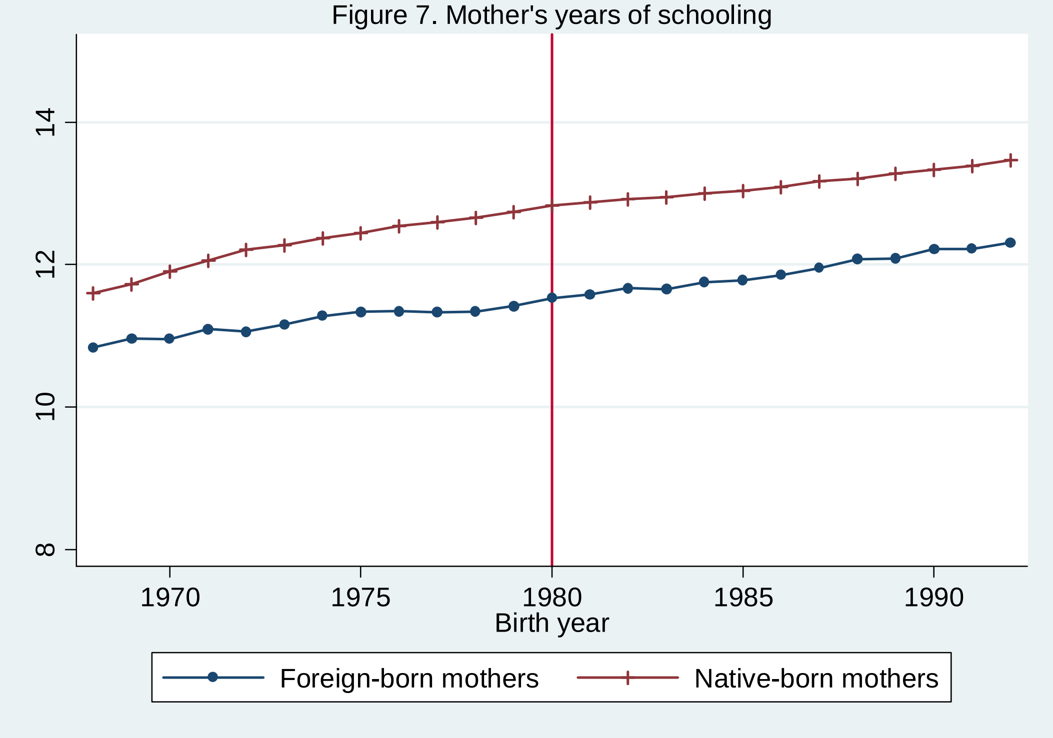


Figure 8. Mother's annual log earnings

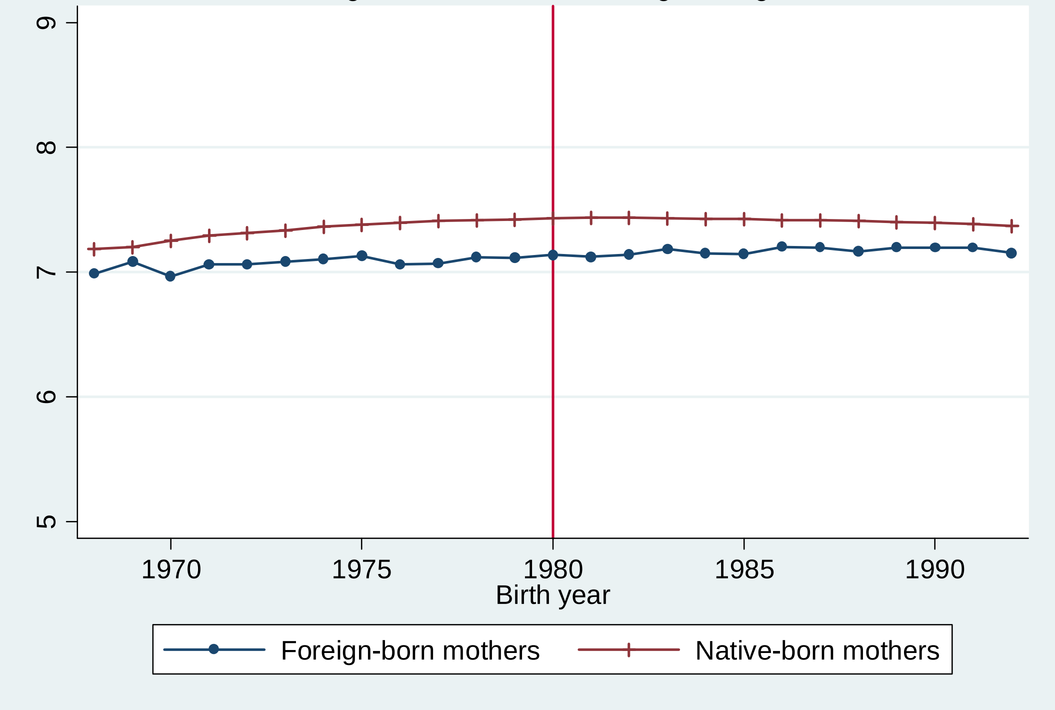


Figure 9. Mother's employment rates

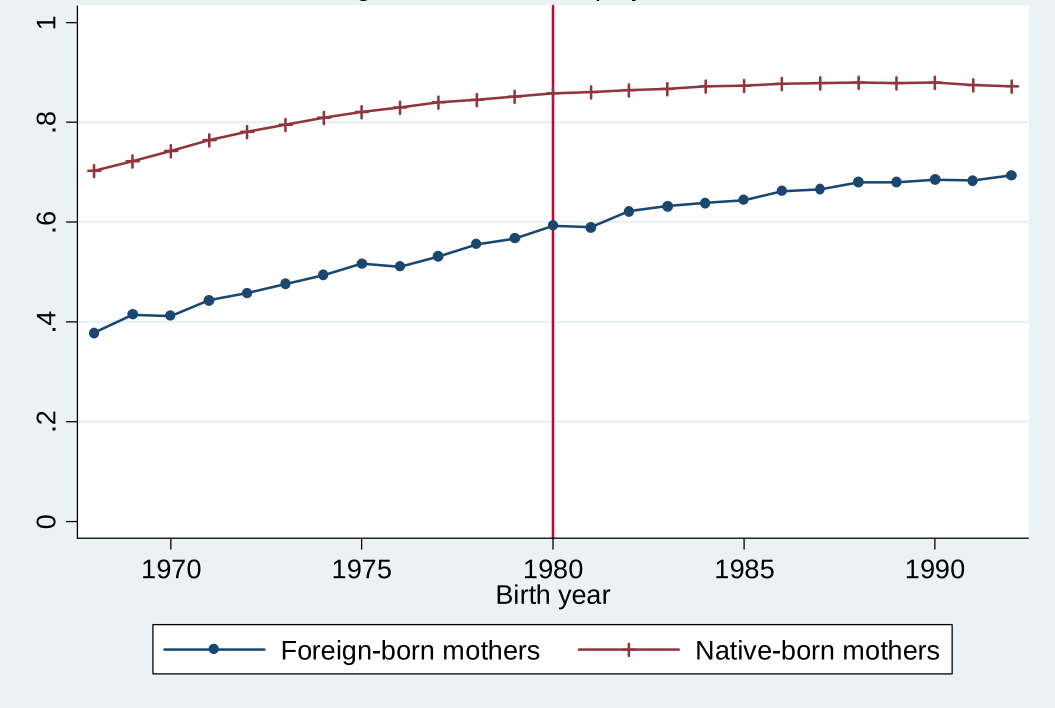


Figure 10. Distribution of immigration year

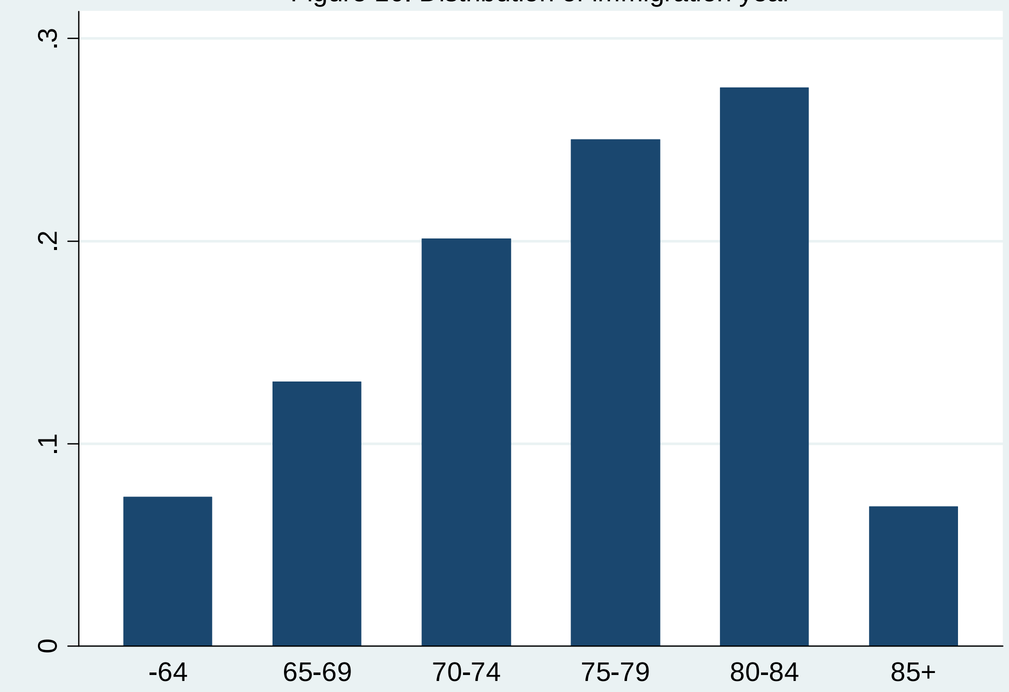
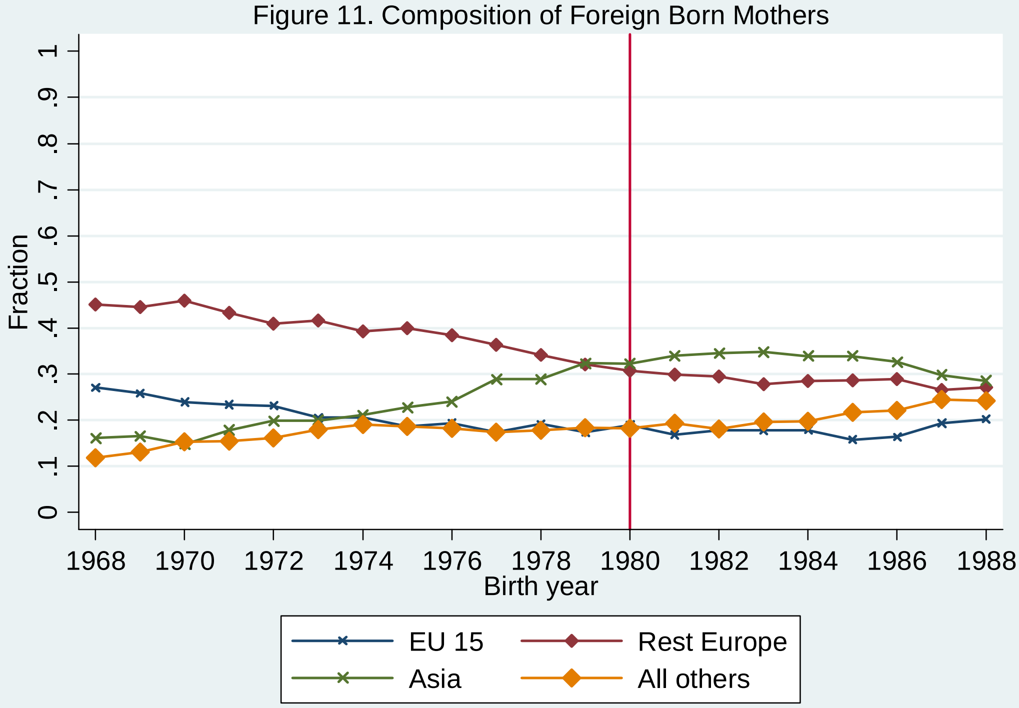


Figure 11. Composition of Foreign Born Mothers



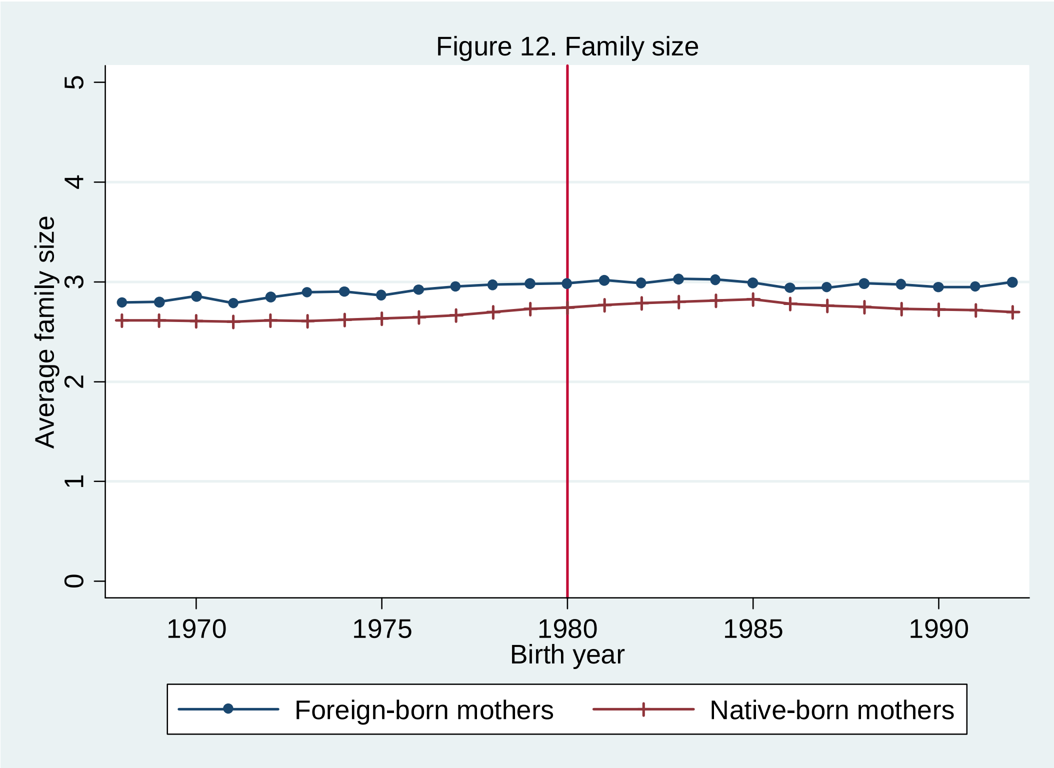


Figure 12. Family size
Average family size
0
1
2
3
4
5
1970
1975
1980
1985
1990
Birth year
Foreign-born mothers
Native-born mothers

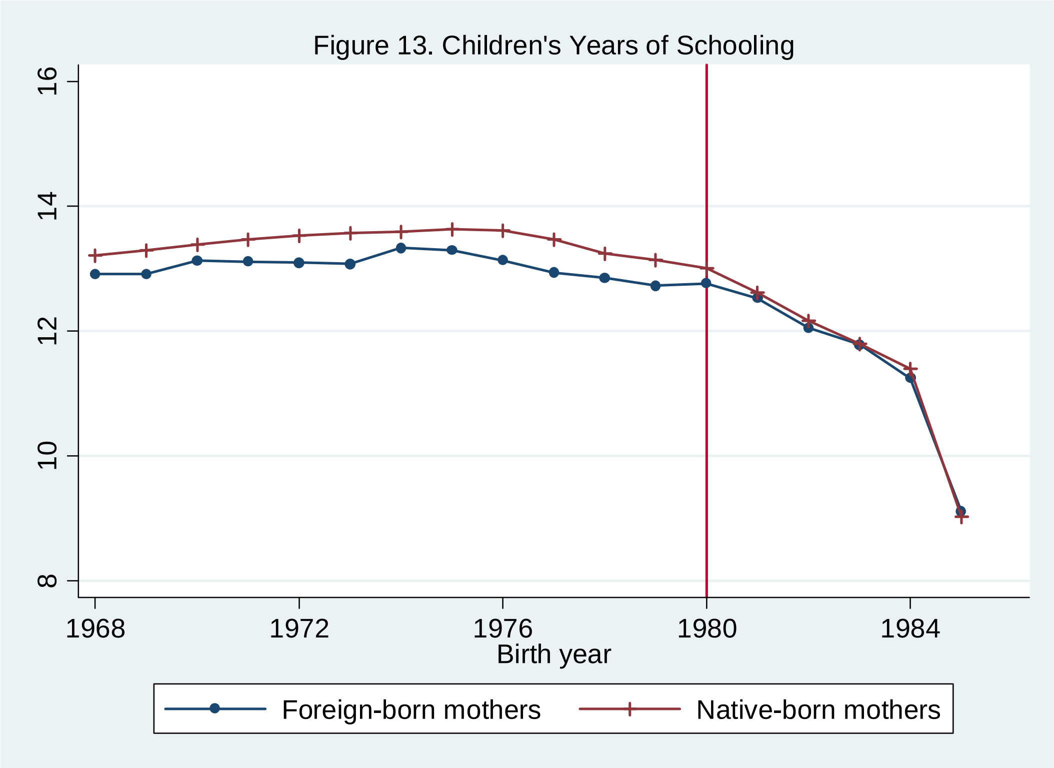


Figure 13. Children's Years of Schooling
16
14
12
10
8
1968
1972
1976
1980
1984
Birth year
Foreign-born mothers
Native-born mothers

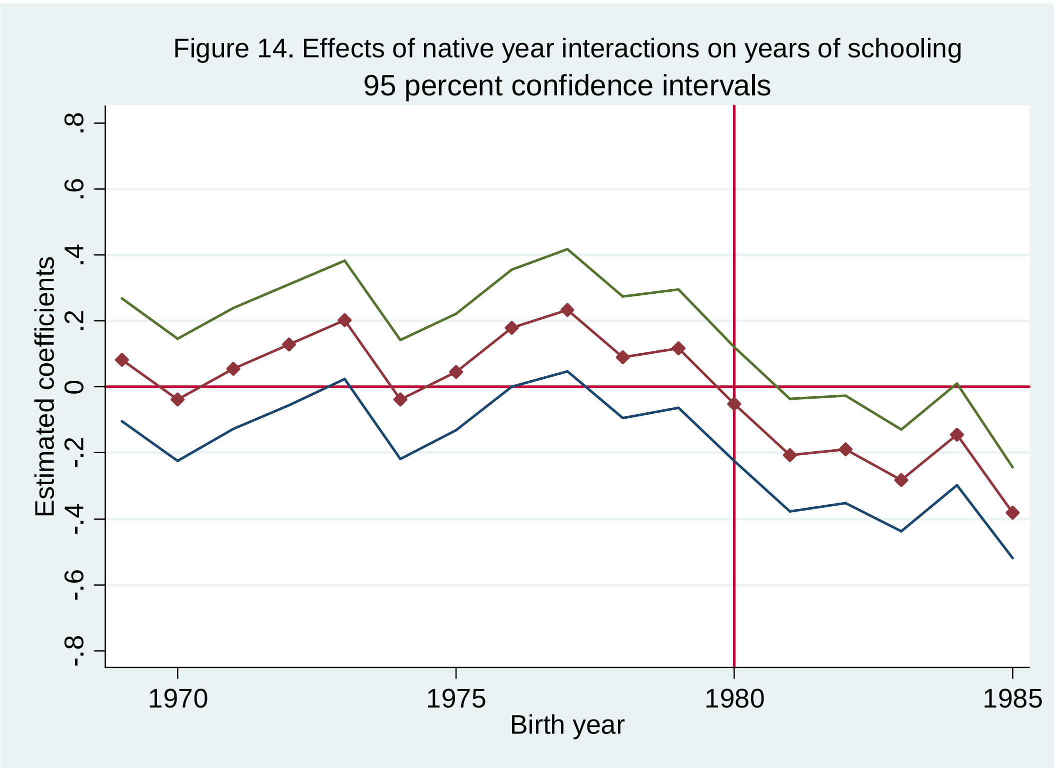


Figure 14. Effects of native year interactions on years of schooling
95 percent confidence intervals
Estimated coefficients
.8
.6
.4
.2
0
-.2
-.4
-.6
-.8
1970
1975
1980
1985
Birth year

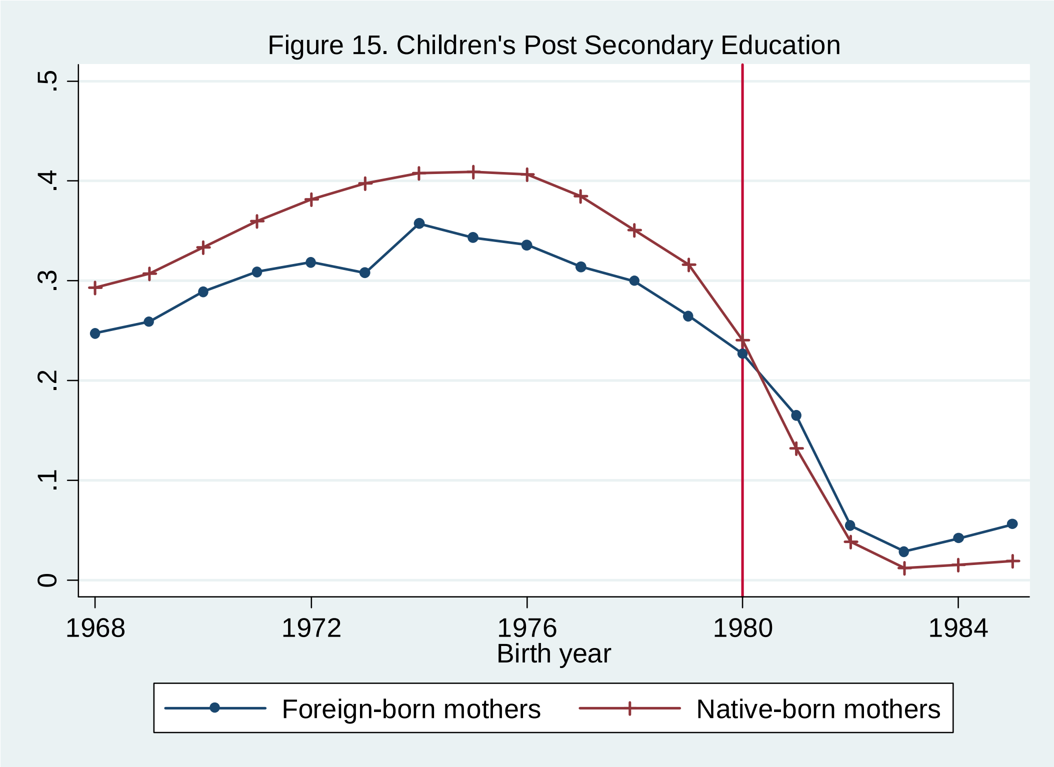


Figure 15. Children's Post Secondary Education

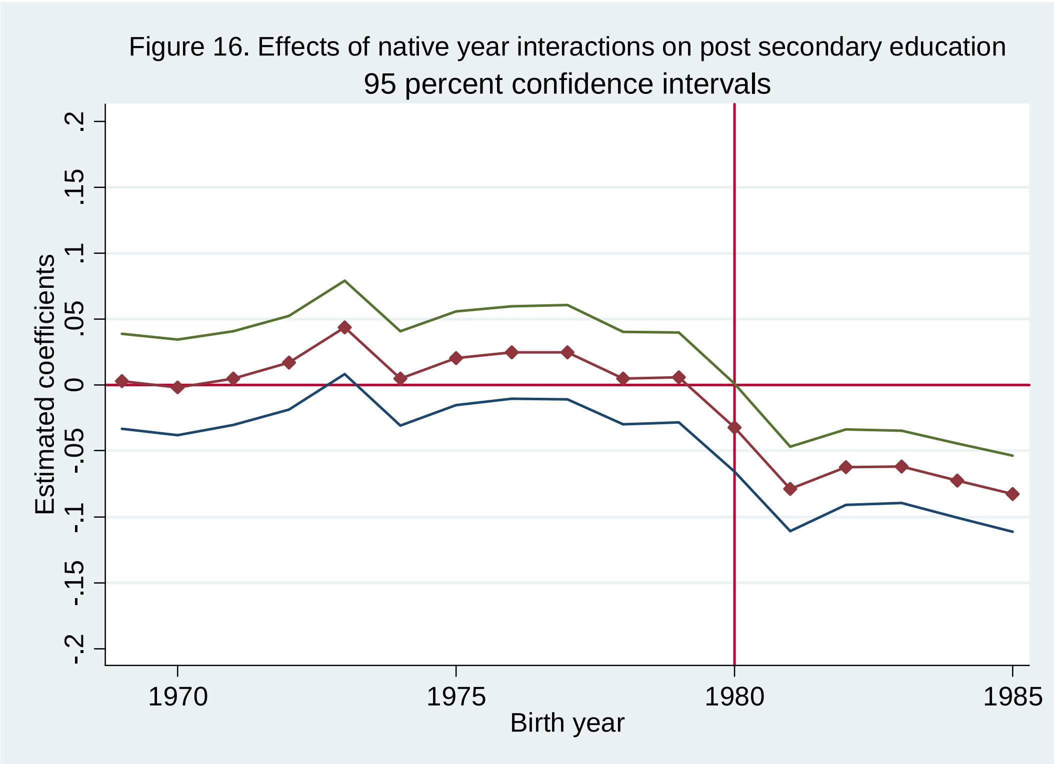


Figure 16. Effects of native year interactions on post secondary education
95 percent confidence intervals

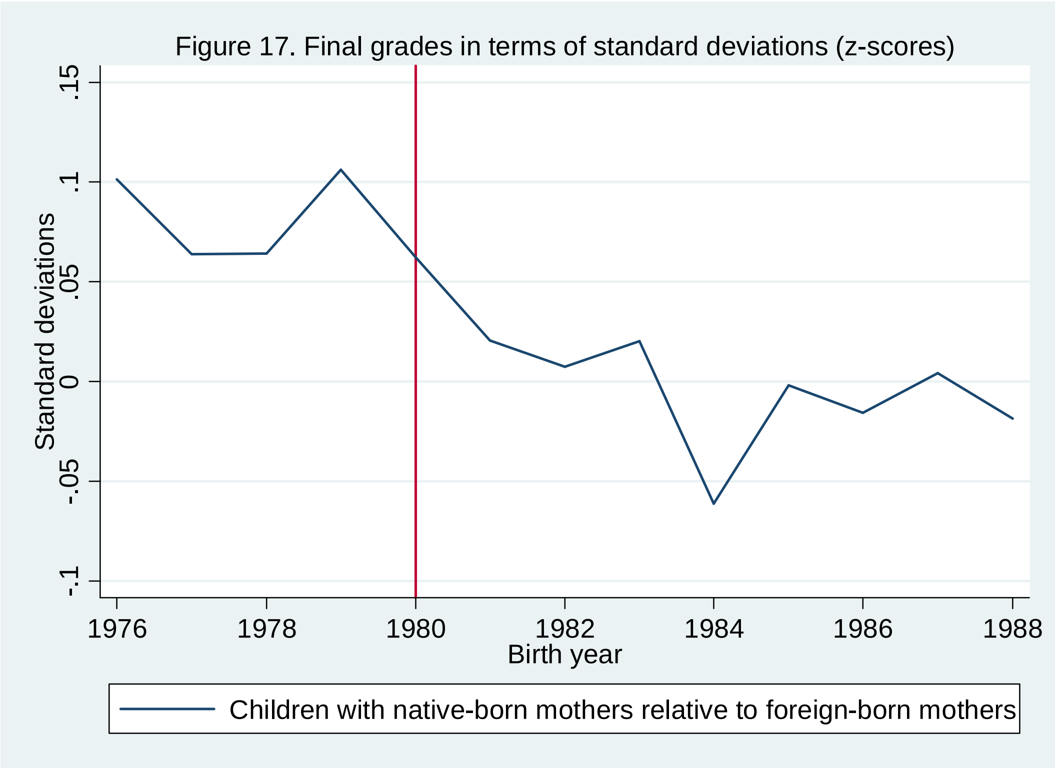


Figure 17. Final grades in terms of standard deviations (z-scores)
Standard deviations
-.1
-.05
0
.05
.1
.15
1976
1978
1980
1982
1984
1986
1988
Birth year
Children with native-born mothers relative to foreign-born mothers

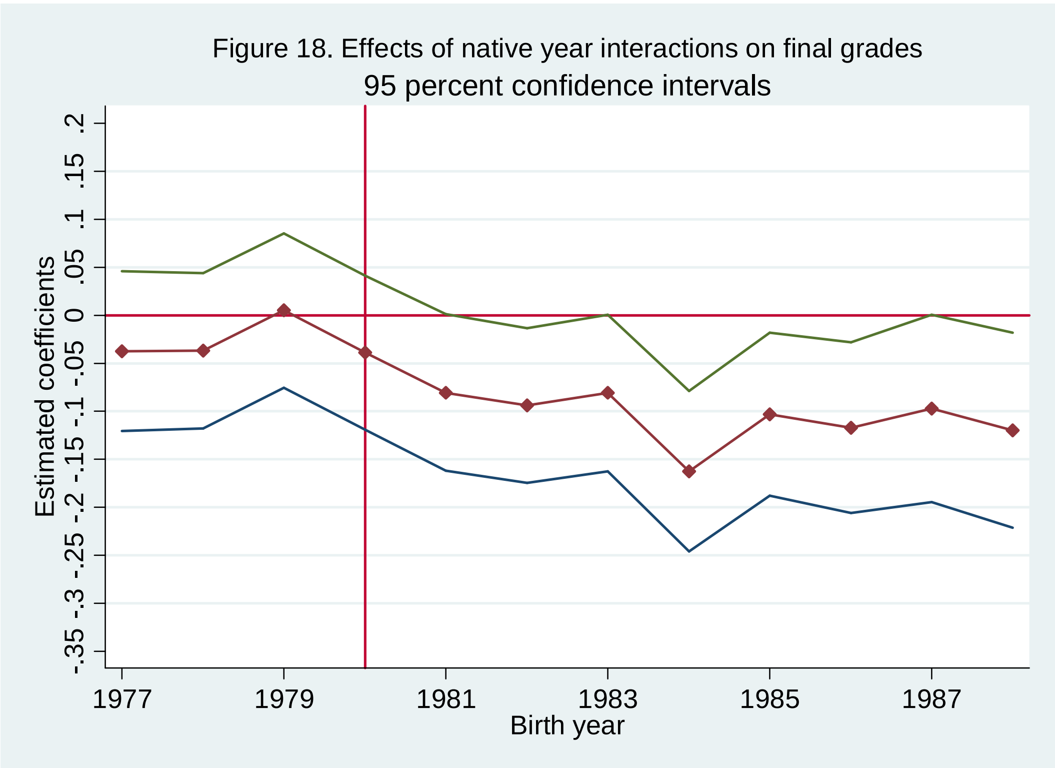


Figure 18. Effects of native year interactions on final grades
95 percent confidence intervals
Estimated coefficients
-.35 -.3 -.25 -.2 -.15 -.1 -.05 0 .05 .1 .15 .2
1977
1979
1981
1983
1985
1987
Birth year

Figure 19. Effects of native year interactions on years of schooling
95 percent confidence intervals

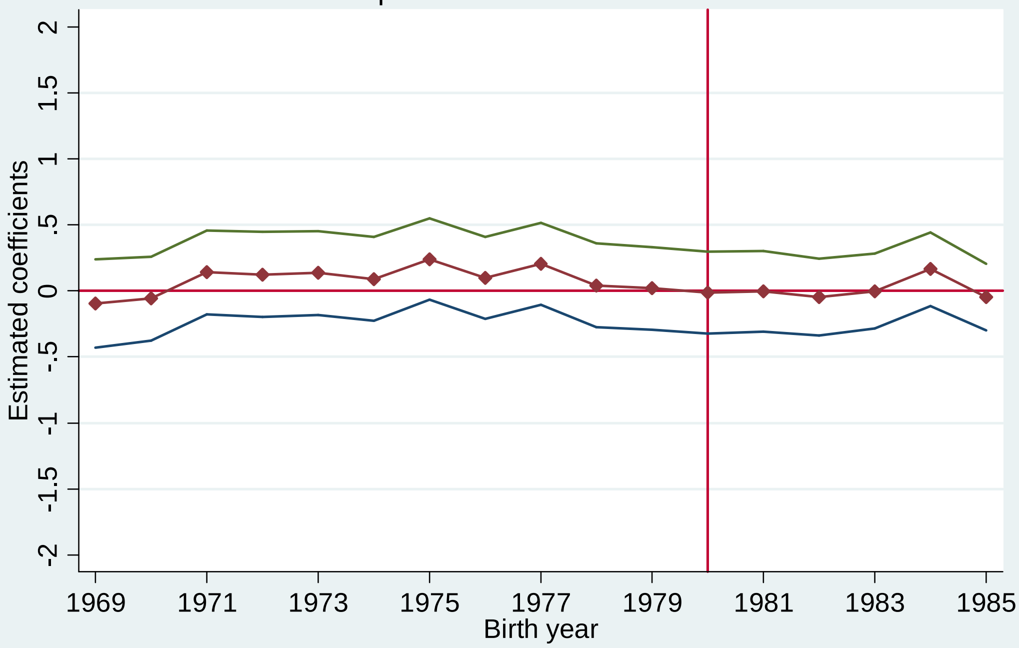


Figure 20. Effects of native year interactions on post secondary education
95 percent confidence intervals

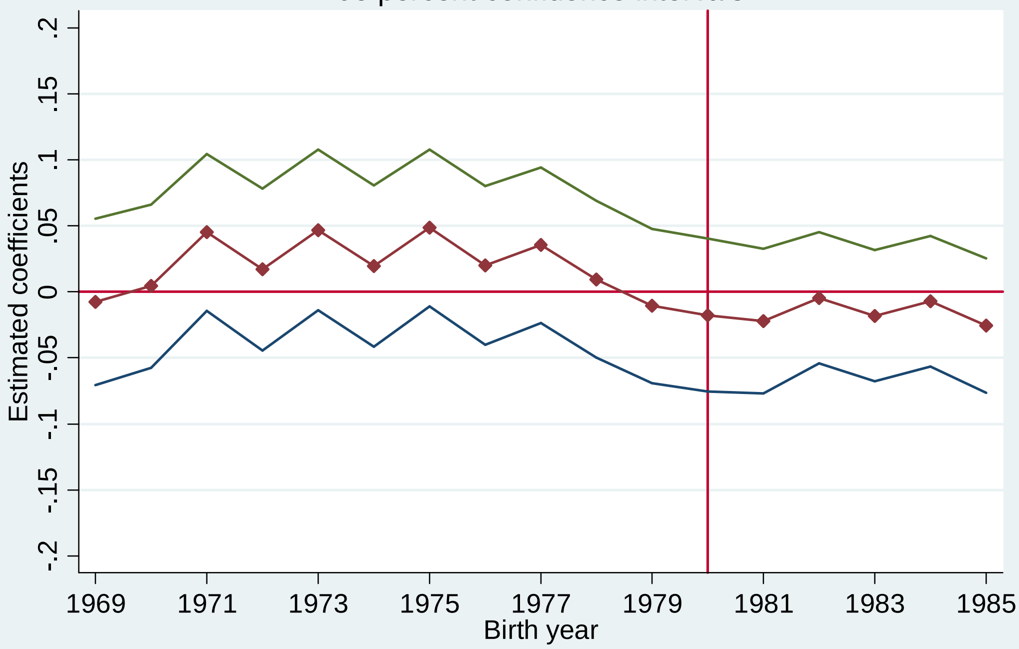


Figure 21. Effects of native year interactions on final grades
95 percent confidence intervals