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

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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 which 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 rule had a large effect on the birth spacing behavior of native-born mothers while foreign-born mothers were largely unaffected due to their differential attachment to the labor market. We find that the rule caused a 4-7 months 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 0.04-0.012 standard deviation decrease in final grades in compulsory school and a 3-8 percentage point reduction in post-secondary education as compared to individuals born by foreign-born mothers. 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.

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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]

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

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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 a 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.

[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 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.

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 mainly affected the child spacing of native-born mothers while foreign-born mothers (i.e., those women born outside the Nordic countries) were largely unaffected.

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., 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. Although we cannot directly test the identifying assumption, an auxiliary implication is that the two groups (individuals with

[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).

native-born or foreign-born mothers) should have similar trends in both child spacing and outcomes before the policy reform. In fact, we provide extensive evidence that the two groups seem to have parallel pre-treatment trends. Moreover, we have made a number of additional robustness checks. Taken together, these specification checks strongly support the validity of the instrument.

We find that a decrease in child spacing has non-trivial effects. According to our IV 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 a 1/3 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).

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]

The results from our analysis of the Swedish speed premium on future childbearing also have some policy implications since there is a current discussion in several countries of how to promote fertility through economic incentives. For example, Germany has recently introduced a speed premium (36 months) on future childbearing. Our results suggest that such a policy would have no effect on family size, i.e. completed fertility, but a large impact on the timing of births instead.[10]

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 individuals' educational outcomes. Section 4 present the results of the effect of child-

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

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.

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.[11] We also present evidence that the administrative rule had different impacts on child spacing for 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.[12] 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. Therefore, one cannot claim that politicians deliberately created incentives for the close spacing of children. 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).

[11] 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.

[12] 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.

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. This lends support to that it was the administrative rule that came into place in 1980 that caused the sharp 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 stronger incentives for closer spacing than foreign-born mothers since they are more strongly attached to the labor market than foreign-born mothers as can be seen from Table 1, which 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 0.75 compared to 0.61 for foreign-born women for those aged 16-44 in 1979.

Turning to the second point about the exogeneity of classifying individuals into treatment and control groups with respect to the policy reform, we avoid problems of endogeneity when we use country of birth as the grouping variable in our differences-in-differences design.[13] In other words, if we should divide 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 similar problems, as discussed by Heckman (1996) and Blundell *et al.* (1998), of using income as a grouping variable in the labor supply literature, since the administrative reform is likely to affect mothers' labor force participation.

Nonetheless, 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 the two groups should have parallel trends in the outcomes absent the intervention, i.e., the parallel trend

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

assumption. Figure 2 shows the development of child spacing between first-born and second-born children for the two groups. This figure shows clearly that they have parallel trends until 1980, the year of the introduction of the administrative rule, but that they start to diverge subsequently. Moreover, looking at the distributions of child spacing before and after 1980 separately for the control and treatment groups we can get additional support for that the administrative rule caused the change in child spacing. Figure 3 shows that the distribution of child spacing for children with foreign-born mothers is basically unchanged 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 4. 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. Before we provide a more elaborate statistical analysis on the effect of the speed-premium rule on child spacing we describe our data in some detail.

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 (all data provided by Statistics Sweden).[14] 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 1st born individuals born between 1968 and 1988 due to limitations of the child outcome data.[15] Child spacing is measured by the birth interval between 1st and 2nd born.[16] We also restrict our sample to mothers who are born before 1965 in order to look at completed family size.

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 2. Native-born

[14] LOUISE is a register based data set on the total Swedish population which includes information, among other things, income and education.

[15] 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 2[nd] and 3[rd] born children.

[16] 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).

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 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 American, South America, Asia, Oceania, and Soviet Union. Table 2 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. It is also interesting to note that 66 percent (of the total sample) of the foreign-born mothers immigrated to Sweden before of the introduction of the speed-premium rule in 1980. Figure 5 shows how the composition has changed over time in the control group. For ease of exposition, we have grouped the 8 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. In Table 3 we also provide 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.

We are now ready to provide the results from the statistical analysis, i.e., the difference-in-difference approach. Table 4 reports OLS 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. 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.[17] 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 may have enough explanatory power as to avoid problems of weak instruments. As a way of illustrating the main message from Table 3, Figure 6 shows the estimated native-year interactions from Column 1 with the corresponding 95 percent confidence intervals.

Another issue is that that the administrative rule may have affected not only child spacing but also completed family size.[18] This would raise concerns about the excludability of our instrument 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 7 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

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[17] Another control variable which one might think would be relevant when estimating child outcome equations is mother's age at child birth. This is, however, not an appropriate control variable since mother's age at child birth is by construction an endogenous variable. To see this, note that child spacing is defined as the difference in mother's age at the birth of her second and first child. Consequently, if child spacing is affected by the reform then the mother's age will be affected too, by construction. If the effect of child spacing is driven by a change in mother's age, one has to argue that a difference in mother's age of less than a year would has an important impact on child outcome. This is not comparable to controlling for mother age as in the literature studying effects of family size on child outcome using twins as an exogenous variation. Controlling for mother's age is more relevant in the family size literature since mothers who have twins are on average substantially older than those who have not twins.

[18] 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.

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 will use final grades in compulsory school and post-secondary education for first-born individuals as the child outcomes of interests.

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). We only have information on education attainment for individuals born up to 1985. Figure 8 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. 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. 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.

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 9 shows the development of the z-scores for children with native-born mothers relative to foreign-born mothers. As shown in Figure 9, 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 postsecondary education, which again suggests that close child spacing is causally related to children's future performance.

We can again use 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.,

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

Table 5 shows the results for post secondary education and Table 6 for final grades in compulsory school. Looking at Column 1 in Table 5 suggest a rather large and statistically significant decline in post secondary education 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.[19] 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. For purpose of illustration, Figure 10 shows the estimated native-year interactions from Column 1 with the corresponding 95 percent confidence intervals.

Turning to the final grades in compulsory school as the child outcome of interest, Table 6 gives similar results as the ones from Table 5, namely that there are no pre-existing trend, although this conclusion is only based on four pre treatment years since grades are only available for the 1976 to 1988 birth cohorts, and 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. For illustrative purpose, Figure 11 shows the estimated native-year interactions from column 1 with the corresponding 95 percent confidence intervals.

[19] As discussed in footnote 17, a mother's age at birth is not an appropriate control variable since it is per definition endogenous. If the effect of child spacing is driven by a change in mother's age rather than spacing itself, one has to argue that a difference in mother's age of less than a year would has an important impact on child outcome. We find this argument implausible. Moreover, the issue of controlling for mothers age at birth is not comparable to the literature studying effects of family size on child outcome using twins as an exogenous variation. Controlling for mother's age is more relevant in the family size literature since mothers who have twins are on average substantially older than those who have not twins.

Taken together, these results suggest that the administrative rule that came into place in 1980 was responsible for the change in the outcome between these two groups of individuals. Nevertheless, there may be other aggregate factors that could cause individuals born by foreign-mothers to perform differently than individuals with native-born mothers. For example, foreign-born mothers could be differently affected by common factors than native-born mothers (e.g., the severe economic downturn in the early 1990s in Sweden, changes in other family policies) which may have differential impact on the outcomes of their children. We construct a "refutability" test to address if there are such unobserved factors that could potentially explain our result.[20] One could argue that one-child families with native born or foreign-born mothers should be exposed to more or less the same unobserved factors as native-born of foreign-born mothers with two or more children. In other words, one could test whether child outcomes differs between children with native-born or 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. Figures 12 and 13 show the estimated year×native interaction effects using child post-secondary education and final grades in compulsory school, together with a corresponding 95 percent confidence intervals. As can be seen from the two figures, the β's are zero before and after the introduction of the speed premium. Thus, this lend strong support to that the administrative child spacing rule is causally related to the child outcomes.

[20] 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. If we assume 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.[21] 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 7 displays the results for both post secondary education and final grades in compulsory school. Without any controls, the OLS estimate is -0.001 for post secondary education (Column 1) and -0.004 for final grades in compulsory school (Column 3). This means that one month *shorter* birth interval will lead to a 0.01 percent *higher* probability of

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

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 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 and 4.

Turning to the instrumental variable approach, Tables 8 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. Thus, one month *shorter* birth interval will lead to 2-2.5 percent *lower* probability of getting a post-secondary education. These estimates are also statistically different from zero and of the opposite sign from the OLS estimates in Table 7. This suggest that the OLS estimate are strongly biased and not to be trusted. One reason for the different sign of the OLS estimate 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 other child outcome, Table 9 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 7. The 2SLS estimate is 0.018 and the Wald/IV estimate is 0.023 in the specifications without additional control variables. 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 four 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.

In Tables 10 and 11, we test whether the child-spacing effect differ across families of different sizes. For the different subsamples of family size 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 8 and 9. Starting with post secondary education as the child outcome of interest, 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. Thus, the child-spacing effect does not vary with family size. Table 10 also shows that the first-stage *F*-statistics is always much larger than 10 suggesting that there are no problems with weak instruments. 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 suggests that the effect is somewhat larger in large families. Again, there is no problem of weak instruments since the first-stage *F*-statistics is larger than 10 in all specifications.

We also report results from whether there are gender differences in effect of the child spacing on child outcomes. Tables 12 show the results for boys and girls without any additional controls while Table 13 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. Mother's region of birth

	Number of mothers	Percentage of total
	Native-born mothers	
Sweden	647,155	95.0
Nordic	33,833	5.0
Total sum	647,155	100
	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	100

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 3. Distribution of family size

Number of children	Native-born mothers Mean family size=2.7		Foreign-born mothers Mean famly size=2.9	
	Number of mothers	*Percentage of total*	*Number of mothers*	*Percentage of total*
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	100	113,371	100

Table 4. 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 5. 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 6. 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 7. OLS estimates

	Post-secondary education		Final grades in compulsory school	
Child spacing	-0.001	-0.0007	-0.004	-0.0033
	(0.0001)	(0.00004)	(0.0001)	(0.0001)
Controls	No	Yes	No	Yes
R^2	0.1030	0.1569	0.0073	0.1044
Observations	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 8. 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 9. 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 10. Different family size: Wald/IV estimates for post-secondary education

	All family sizes (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 11. Different family size: Wald/IV estimates for final grades in compulsory school

	All family sizes (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 12. Girls versus boys: IV estimates

	Post-secondary education		Final grades in compulsory school	
	Girls	Boys	Girls	Boys
Child spacing	0.035	0.018	0.036	0.014
	(0.006)	(0.003)	(0.011)	(0.006)
Controls	No	No	No	No
First-stage F-test	12.9	23.2	21.7	50.0
Observations	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 13. Girls versus boys: IV estimates including controls

	Post-secondary education		Final grades in compulsory school	
	Girls	Boys	Girls	Boys
Child spacing	0.032	0.017	0.032	0.012
	(0.005)	(0.002)	(0.005)	(0.004)
Controls	Yes	Yes	Yes	Yes
First-stage F-test	16.8	71.9	19.5	54.4
Observations	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.

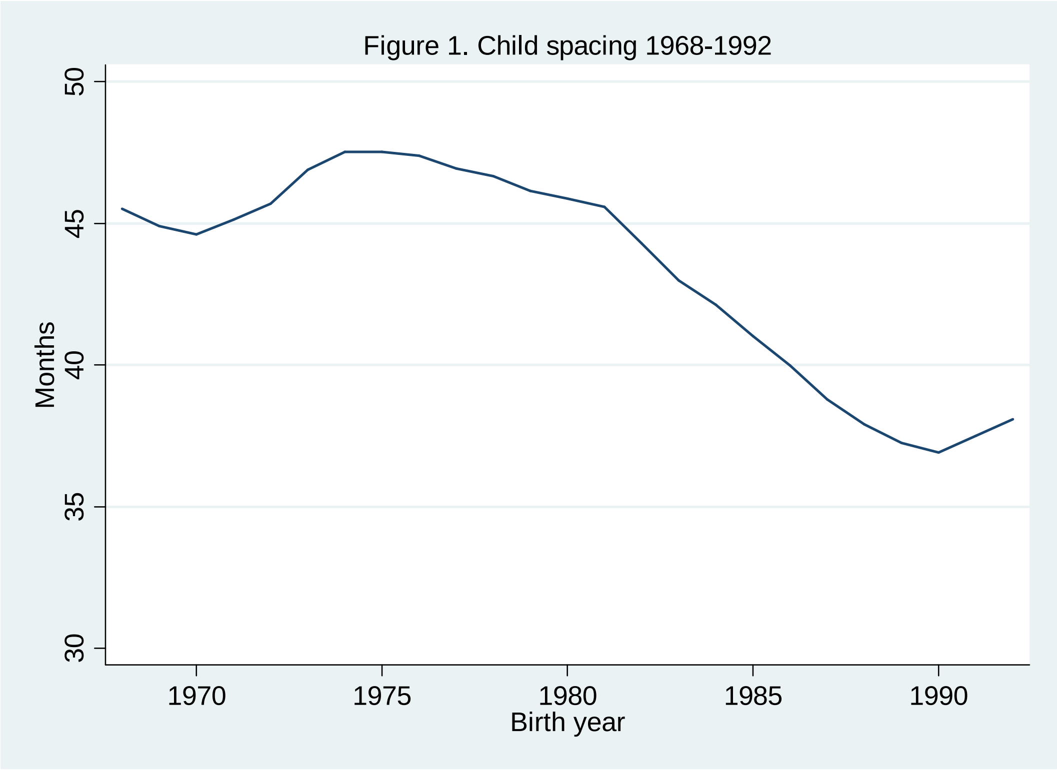
Figure 1. Child spacing 1968-1992
Months
30
35
40
45
50
1970
1975
1980
1985
1990
Birth year

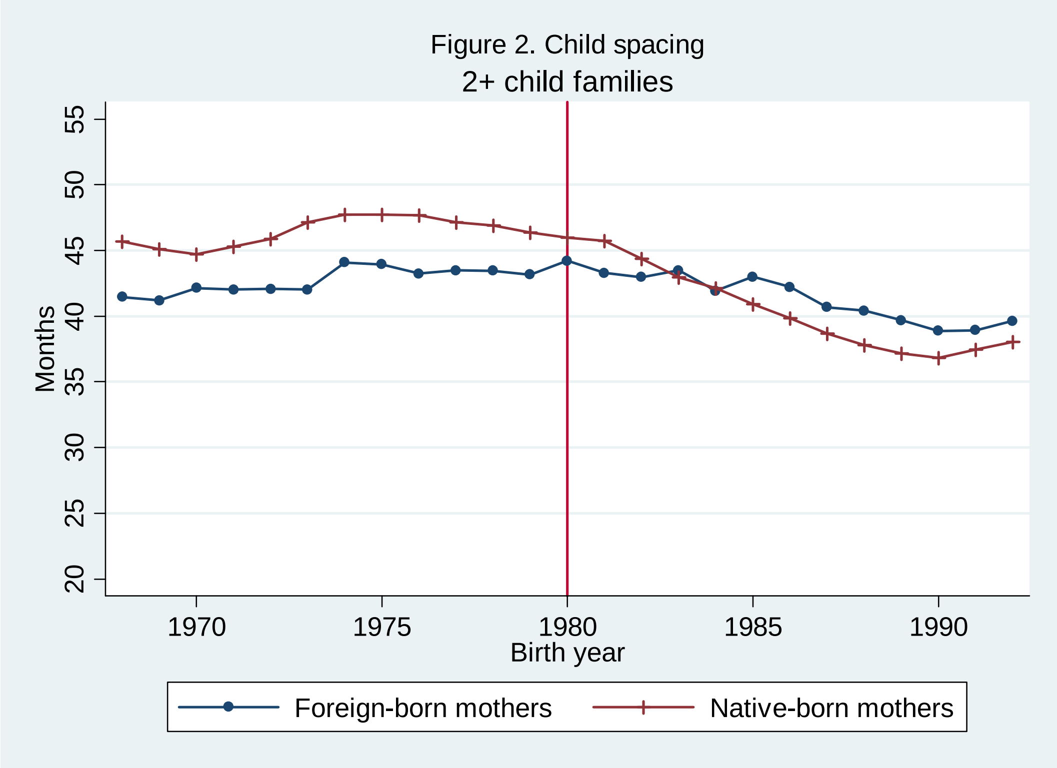
Figure 2. Child spacing
2+ child families
Months
20
25
30
35
40
45
50
55
1970
1975
1980
1985
1990
Birth year
Foreign-born mothers
Native-born mothers

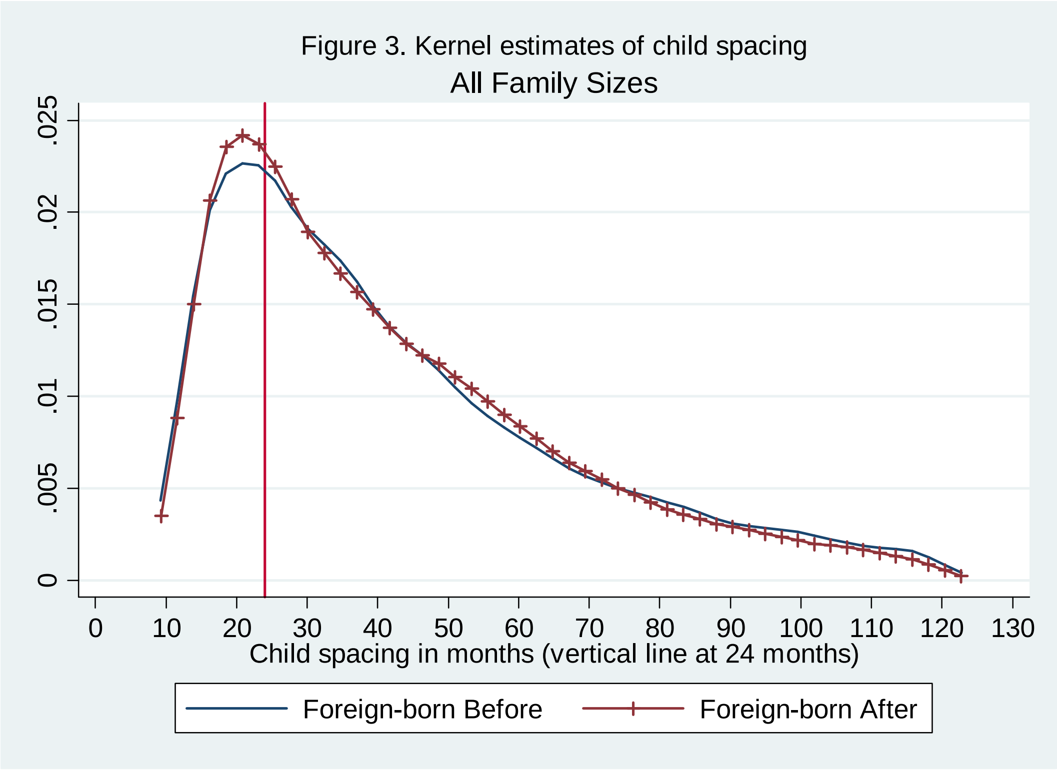
Figure 3. Kernel estimates of child spacing
All Family Sizes
0
.005
.01
.015
.02
.025
0 10 20 30 40 50 60 70 80 90 100 110 120 130
Child spacing in months (vertical line at 24 months)
Foreign-born Before
Foreign-born After

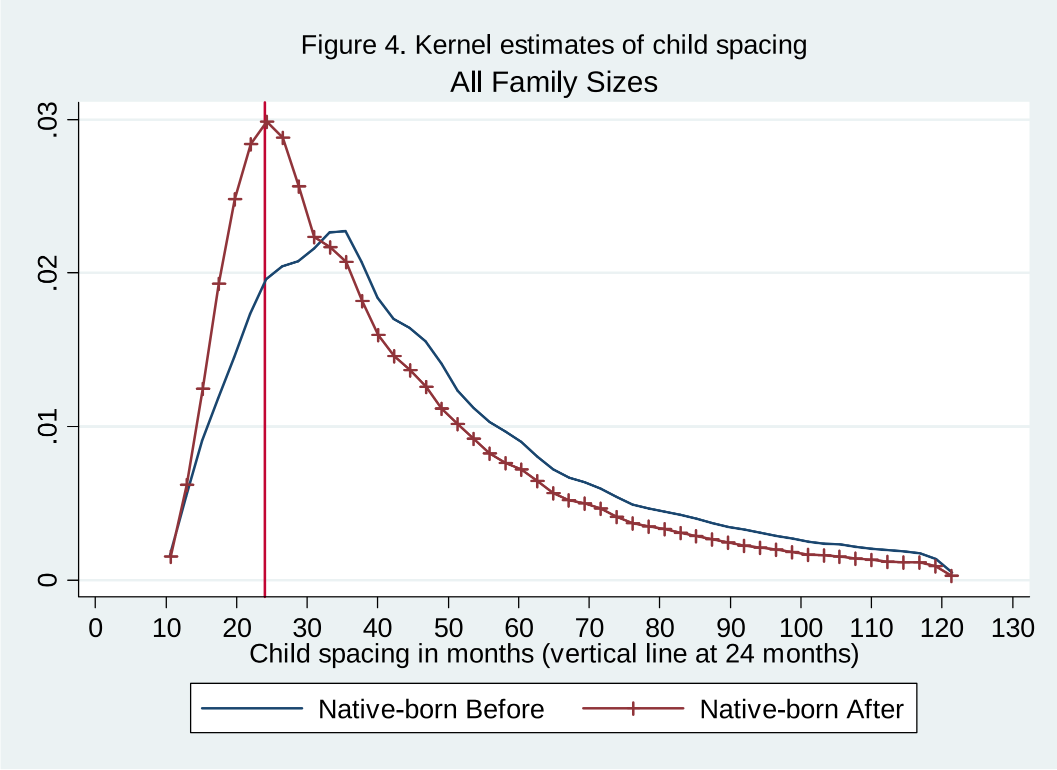
Figure 4. Kernel estimates of child spacing
All Family Sizes
0
.01
.02
.03
0 10 20 30 40 50 60 70 80 90 100 110 120 130
Child spacing in months (vertical line at 24 months)
Native-born Before
Native-born After

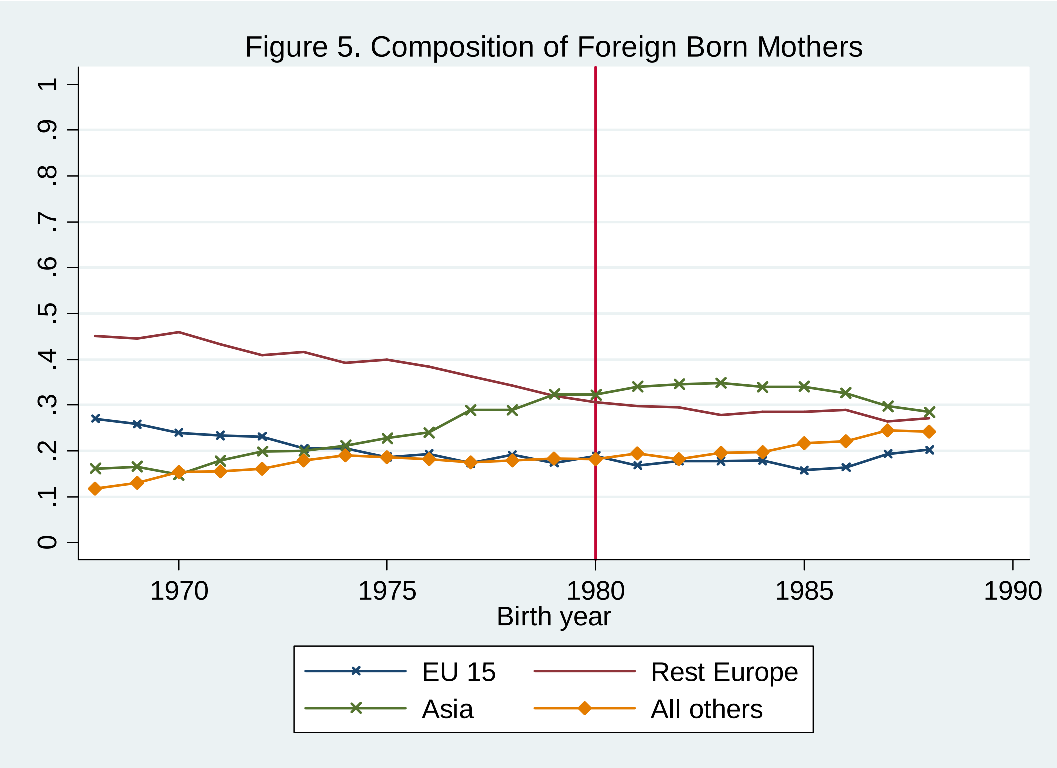
Figure 5. Composition of Foreign Born Mothers
0
.1
.2
.3
.4
.5
.6
.7
.8
.9
1
1970
1975
1980
1985
1990
Birth year
EU 15
Rest Europe
Asia
All others

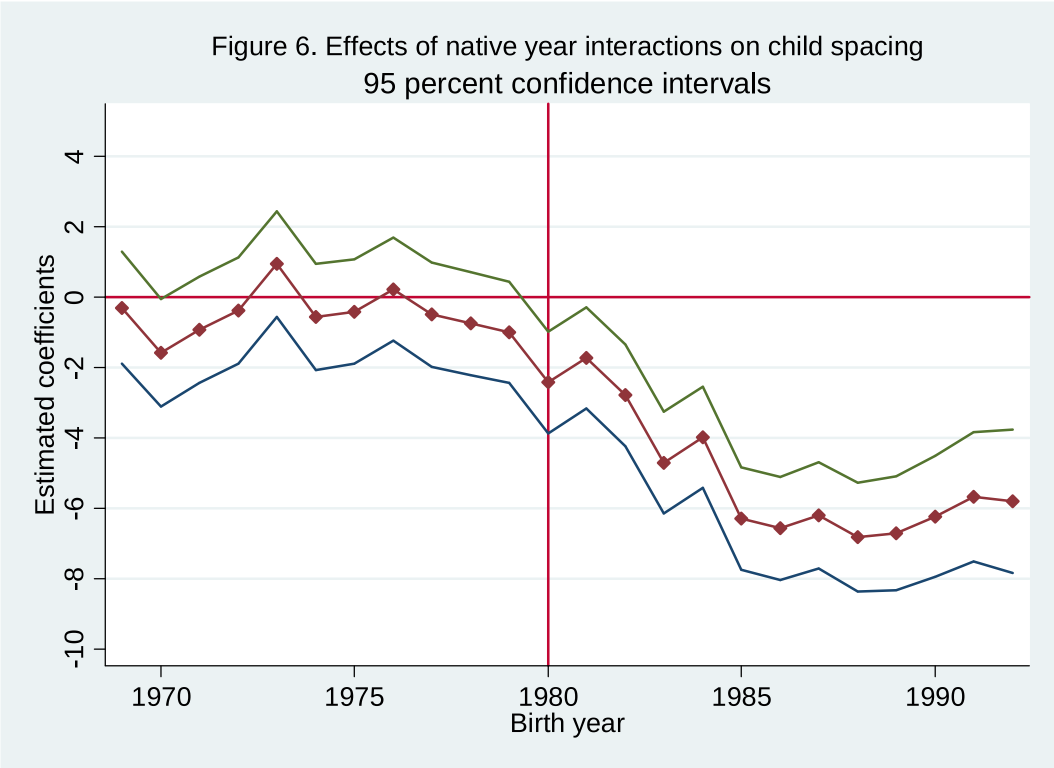
Figure 6. Effects of native year interactions on child spacing
95 percent confidence intervals
Estimated coefficients
-10
-8
-6
-4
-2
0
2
4
1970
1975
1980
1985
1990
Birth year

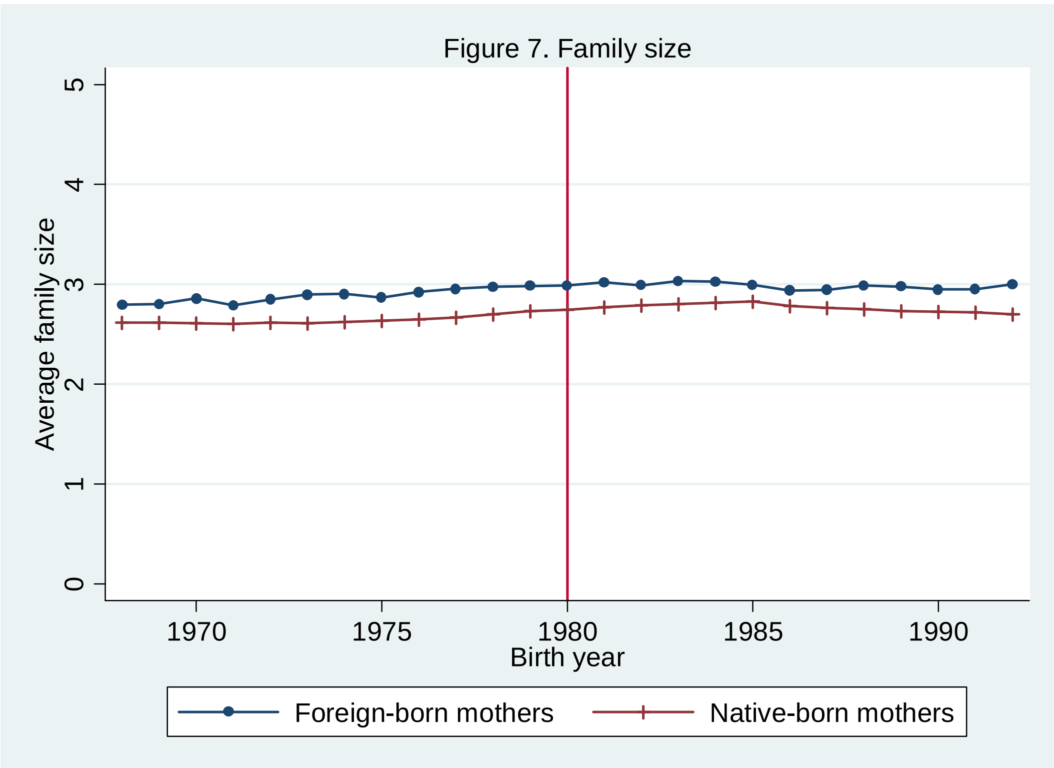
Figure 7. Family size
Average family size
0
1
2
3
4
5
1970
1975
1980
1985
1990
Birth year
Foreign-born mothers
Native-born mothers

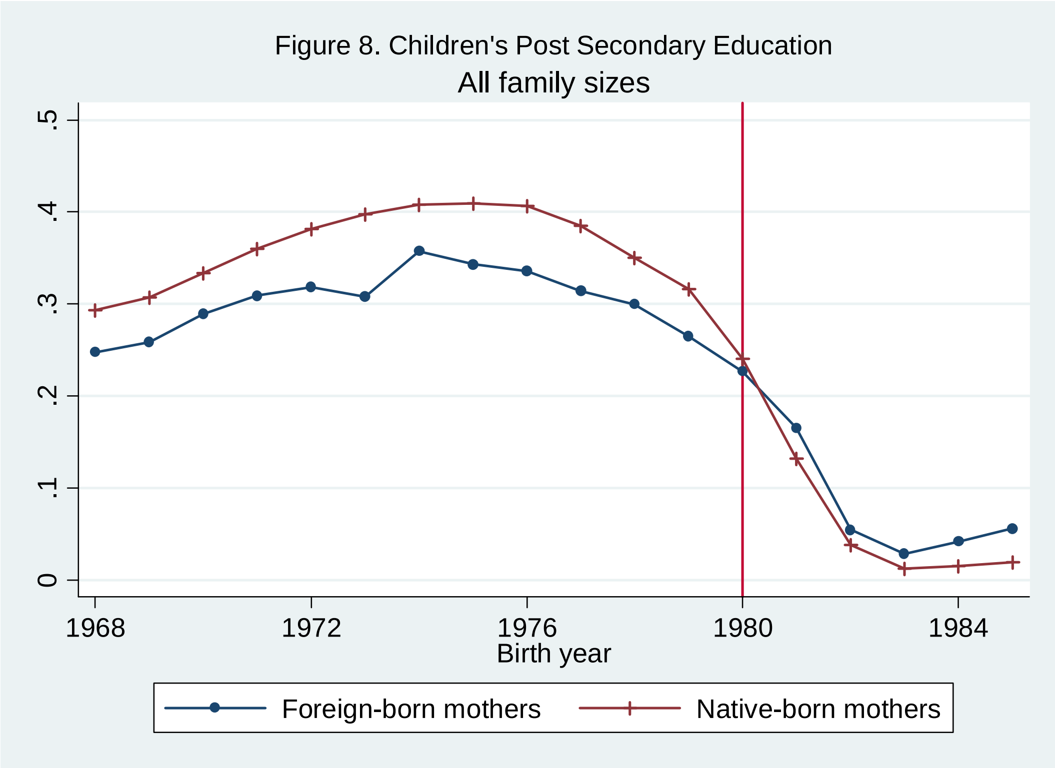

Figure 8. Children's Post Secondary Education
All family sizes
0
.1
.2
.3
.4
.5
1968
1972
1976
1980
1984
Birth year
Foreign-born mothers
Native-born mothers

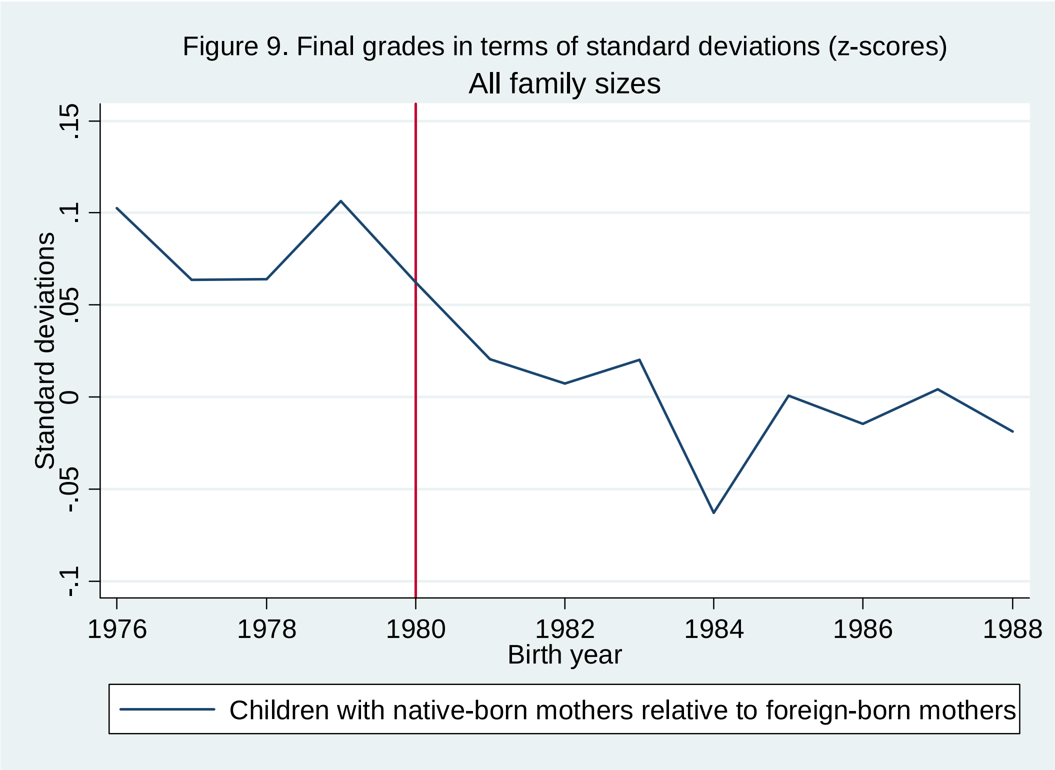
Figure 9. Final grades in terms of standard deviations (z-scores)
All family sizes
Standard deviations
-.1
-.05
0
.05
.1
.15
1976
1978
1980
1982
1984
1986
1988
Birth year
Children with native-born mothers relative to foreign-born mothers

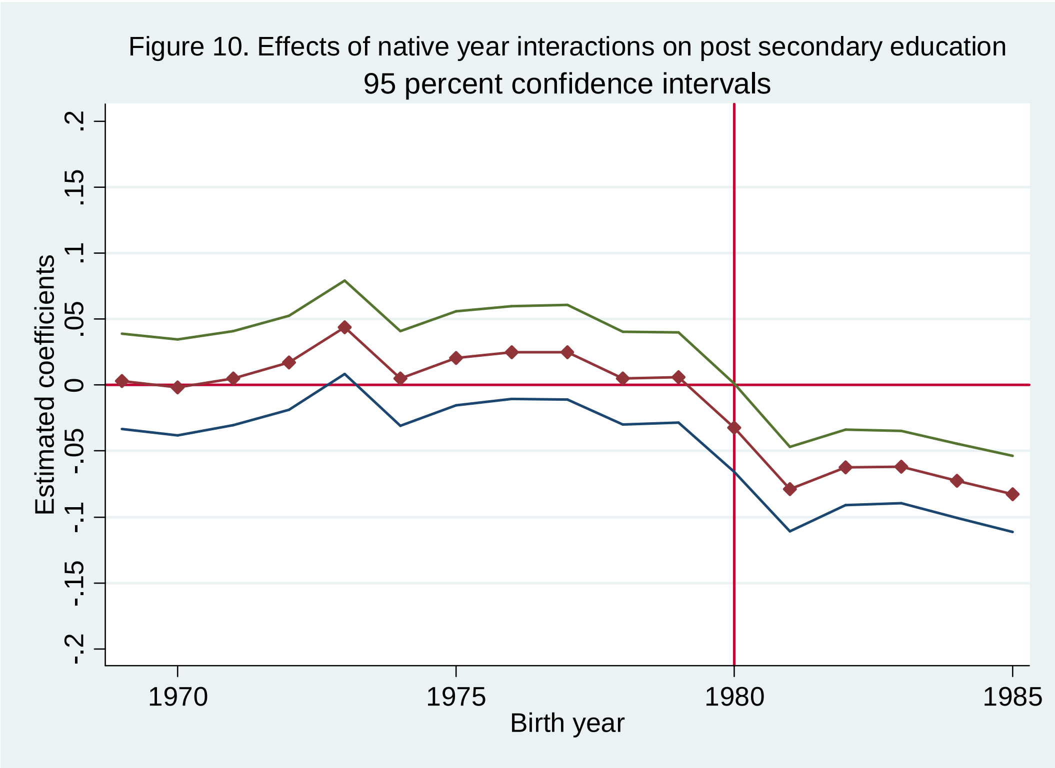
Figure 10. Effects of native year interactions on post secondary education
95 percent confidence intervals
Estimated coefficients
-.2
-.15
-.1
-.05
0
.05
.1
.15
.2
1970
1975
1980
1985
Birth year

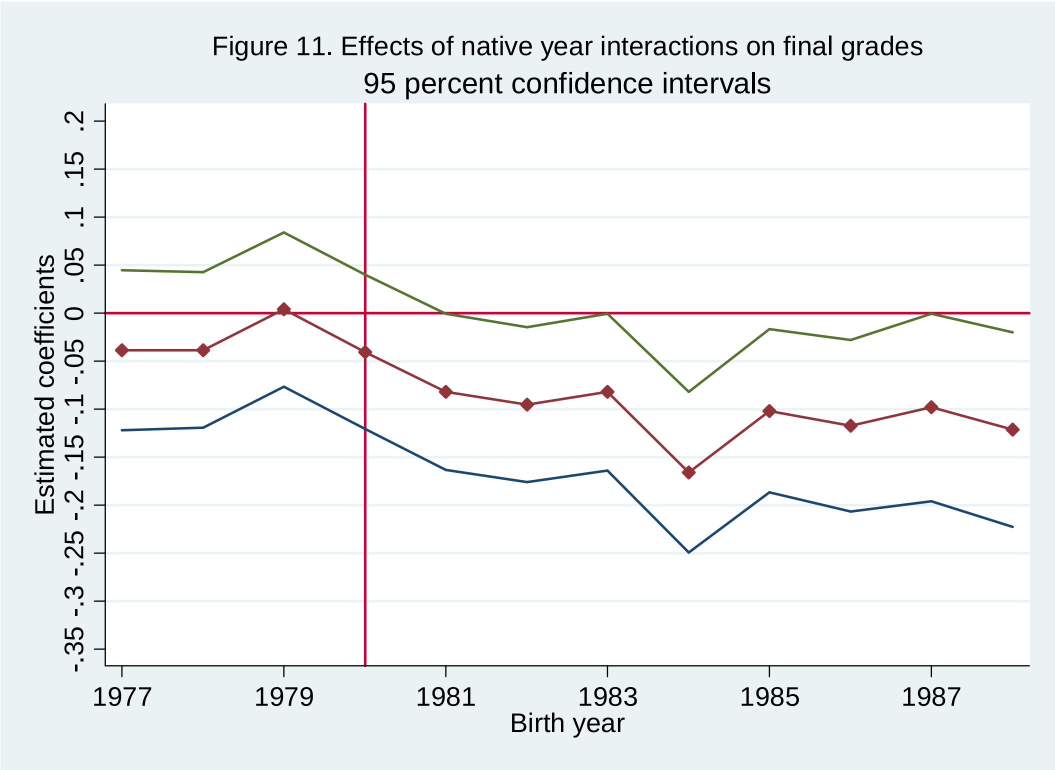
Figure 11. Effects of native year interactions on final grades
95 percent confidence intervals
Estimated coefficients
-.35 -.3 -.25 -.2 -.15 -.1 -.05 0 .05 .1 .15 .2
1977 1979 1981 1983 1985 1987
Birth year

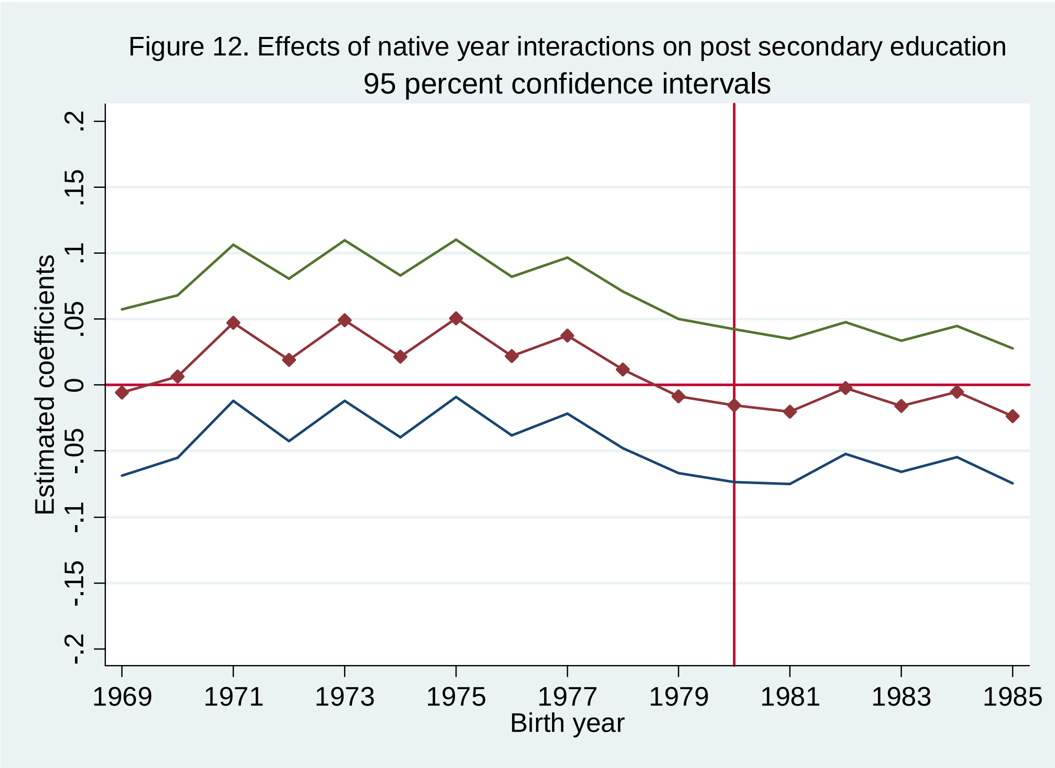
Figure 12. Effects of native year interactions on post secondary education
95 percent confidence intervals
Estimated coefficients
-.2 -.15 -.1 -.05 0 .05 .1 .15 .2
1969 1971 1973 1975 1977 1979 1981 1983 1985
Birth year

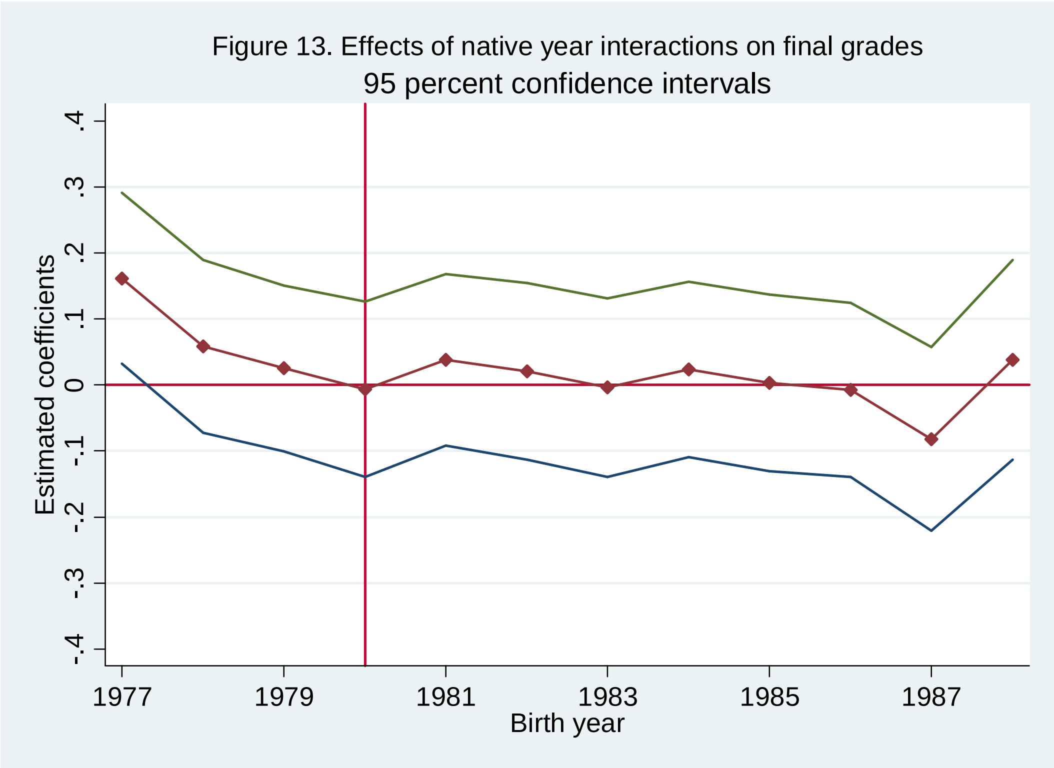

Figure 13. Effects of native year interactions on final grades
95 percent confidence intervals
Estimated coefficients
-.4
-.3
-.2
-.1
0
.1
.2
.3
.4
1977
1979
1981
1983
1985
1987
Birth year