Swedish Institute for Social Research (SOFI)

Stockholm University

WORKING PAPER 3/2010

DOES MARRIAGE MATTER FOR CHILDREN? ASSESSING THE IMPACT OF LEGAL MARRIAGE IN SWEDEN

by

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March 31, 2010

JEL Codes: J10, J12, J13, J18
Keywords: family structure, marriage, child well-being, educational attainment

Acknowledgments: Ginther, Björklund, and Sundström acknowledge financial support from NICHD Grant R03HD048931. Björklund, and Sundström acknowledge financial support from the Swedish Council for Working Life and Social Research (FAS). We thank Joshua Angrist, Suzanne Bianchi, Christopher Bollinger, Ronald Caldwell, Jonathan Gruber, James Heckman, Peter Skogman Thoursie, seminar participants at IUPUI and Uppsala University, the 2005 NBER Summer Institute Children's Workshop and the 2007 ESPE-meetings for helpful comments on earlier drafts of this paper. This is a substantially revised version of IZA Discussion Paper no. 3189.

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ABSTRACT:

This paper examines whether parental marriage confers educational advantages to children relative to cohabitation. We exploit a dramatic marriage boom in Sweden in late 1989 created by a reform of the Widow's Pension System that raised the attractiveness of marriage compared to cohabitation to identify the effect of marriage and the effect of selection bias on marriage estimates. Sweden's rich administrative data sources enable us to identify the children who were affected by parental marriage due to this marriage boom. Our analysis addresses the question of whether marginal marriages created by a policy initiative have an impact on children. Using grade point average at age 16 as the outcome variable, we first show the expected pattern that children with married parents do better than children with cohabiting parents. However, once we control for observable family background and compare the outcomes for children whose parents married due to the reform with those for children whose parents remained unmarried, the differences largely disappear. The results from a sibling difference analysis are consistent with the conclusion that the differentials among children of married and cohabiting parents reflect selection rather than causation.

INTRODUCTION

Is marriage good for children? Should public policy stimulate couples to marry in order to counteract the trend towards cohabitation that is taking place in most Western countries? These are important questions and currently on the policy agenda in many countries. For example, in the U.S. the Bush administration launched a USD 1.5 billion drive for the promotion of 'healthy marriages' (*The New York Times,* January 14, 2004). These policies were prompted by studies that show marriage is correlated with good outcomes for children (e.g. McLanahan and Sandefur 1994) as well as economic theory. The economic theory of the family suggests that parental marriage may have positive effects on children. In his survey of the literature, Weiss (1997) notes that marriage serves four main functions: 1) Division of labor within the household that gives rise to specialization in market work and home production; 2) Coordination of investment activities; 3) Sharing in public goods, such as children; and 4) Risk pooling. Marriage thus allows for coordinated investment by parents in children's human capital.

Despite the increasing prevalence of cohabitation, research on the impact of marriage compared with cohabitation on children is scarce, but suggests that cohabitation may have adverse outcomes for children (Graefe and Lichter 1999, Manning 2002, Smock and Gupta 2002, Bumpass and Lu 2000, Manning and Lichter 1996). However, these studies are rarely designed to properly address the issue of causality and self-selection. Further, as stressed recently by Stevenson and Wolfers (2007), "it is important to differentiate between the average marriage – which is likely to be a happy one – and the marginal marriage that may be created or spared by government policy, which may be quite different".

Our research offers a contribution to this scarce literature with a study of Sweden, where cohabitation is more common than anywhere else in the world. We exploit two unique features of Sweden. First, we make use of a dramatic marriage boom that took place in the last two months of 1989 due to a reform of the widow's pension. In the fall of 1989, it became apparent that the rules governing the widow's pension would be changed so that it would be beneficial to marry before the

end of the year. Formally, these benefits were confined to women born before 1945, but information in media about the reform was so vague that it also stimulated many women who were born in 1945 or later to marry. In all, there was a 21-fold increase of marriage rates in Sweden in the last two months of 1989. These marriages constitute marginal marriages that came about by means of a change in public policy.

Administrative register data available in Sweden allow us to conduct this study. Using these data we can identify the children of the parents who married before and during the boom in 1989, as well as the children whose parents remained unmarried. We also know from census data whether the children lived with their biological parents or not. In addition, a population register identifies the siblings of these children. A special education register of the Swedish population provides our outcome variable: the grade point average (GPA) at age 16, grades which are compulsory for entry into high school (*gymnasium*). Our main analysis sample is a 20 percent random sample of all Swedish children born in 1977-87, in all over 130,000 observations.

Our initial cross-section regressions show that legal marriage is positively correlated with children's GPA. However, when we extend the analysis to control for observable family background factors, we find little difference between the GPAs of the children whose parents were attracted into marriage by the reform and those of the children whose parents remained unmarried. We then examine the extent of selection bias in estimates of the effect of marriage and find that almost all of the positive effect of marriage on children's GPAs can be explained by selection. However, marriage duration does appear to confer a positive effect on children after controlling for observables. Next, we employ family fixed effects that show that all of the effect of marriage on children's GPAs can be explained by unobserved and time-invariant family characteristics. Taken together, our results show that marriage does not have a causal impact on children's GPAs in Sweden.

The paper proceeds as follows: Section 2 presents our theoretical perspectives. Section 3 describes the trends in cohabitation and marriage, discusses the legal differences between marriage

and cohabitation in Sweden and describes the marriage boom in the end of 1989. In Section 4 we present our data and our estimation strategies. Section 5 presents our findings. Section 6 concludes and discusses whether the results for Sweden can be generalized to other countries, such as the United States.

THEORETICAL PERSPECTIVES AND PREVIOUS STUDIES

Unlike cohabitation, which ends when one partner moves out, marriage requires a legal separation of property and custody rights, making it more difficult to dissolve. Thus, it could be that marriage signals a greater commitment, and the expected duration of a marriage is longer than that of a consensual union. These factors, together with the legal arrangement of marriage, may provide for the four functions of marriage discussed by Weiss (1997) and lead to greater investments in children. Lundberg (2002, 2008) and Ginther and Sundström (2010) model specialization in the household. This work shows that more-committed relationships (like marriage) are more likely to specialize, and specialization may confer advantages to children. We know, for example, that in Sweden among employed mothers of children below age 10, the fraction working part time was 62 percent among married mothers, but only 35 percent among cohabiting mothers in 1990 (Swedish Level of Living Survey 1991).[1] Further, Sundström and Duvander (2002) find that married fathers used a larger share of the parental leave for newborn children than cohabiting fathers, net of earnings and other factors. Using U.S. data, DeLeire and Kalil (2005) compare the expenditure behavior of cohabiting and married couples with children. They find that cohabiting couples spend more on adult goods such as tobacco and alcohol, and less on children's educational expenditures, than married couples. These results indicate that marriage may provide for greater investments in children.

If both parents value a child's well-being, then investments in children by one parent may create a positive externality for the other parent. The absence of legal marriage may create a coordination failure, where one parent has an incentive to under-invest in their children and free-ride

[1] We are grateful to Elin Olsson for help with these computations.

off of the investments of the other parent. Thus, the legal status of the parents' relationship may lead to better outcomes in the case of marriage. However, based on economic theory we should expect parents to self-select into the type of union that maximizes their utility and that of the child. Hence, it is plausible that any effect on child outcomes would be the result of positive selection into marriage, rather than causation.

Despite the potential dissimilarity in their impact, few studies examine the differences in the effect of cohabitation and marriage on child outcomes. Research on the impact of cohabitation on children is limited in the United States, and none of this research controls for selection into cohabitation or marriage. Further, we are unaware of any studies of the impact of cohabitation on children in Sweden. When U.S. researchers have compared outcomes for children in married biological parent unions with those for children in all cohabitating unions, children in cohabiting families fare worse (Manning 2002, Brown 2004). Several U.S. studies indicate that the negative associations found between child outcomes and cohabitation may reflect the lesser stability of cohabiting unions and the lower educational attainment and earnings of the adults in these families (Manning and Lichter 1996, Graefe and Lichter 1999, Bumpass and Lu 2000, Manning 2002, Smock and Gupta 2002, Manning 2002, Manning, Smock and Majumdar 2004, Acs and Nelson 2002, Acs and Nelson 2004a, Acs and Nelson 2004b). Other research has shown mixed results on the correlation between children's well-being and living in cohabiting families.

However, Manning (2002) argues that for causal purposes it would be more productive to make comparisons between cohabiting biological parents (cohabiting parents) and married biological parents (married parents), and cohabiting partners and stepparent families. When Manning makes these distinctions, she finds no significant differences in behavior outcomes and school achievement for children living with cohabiting compared with married parents. Brown (2004) makes the same comparison. In contrast, she finds that young children and adolescents of cohabiting parents have lower school engagement and more behavior problems than children of married parents. Yet, the

association with cohabitation becomes insignificant for young children when parental education and resources are included in the specifications. Manning and Lamb (2003) examine adolescent well-being in cohabiting partner, stepparent, and married families. They find worse behavioral and academic outcomes for adolescents in cohabiting partner families when compared with stepfamilies. Hofferth (2006) shows that children of unmarried biological parents have worse behavioral outcomes than children with married biological parents. These studies based on U.S. data are plagued by relatively small numbers of cohabiting biological parents and no controls for selection into marriage. Yet, taken together, this research shows that there are descriptive differences in outcomes between children in cohabiting and married families. The challenge is to understand the sources of these differences.

COHABITATION AND MARRIAGE IN SWEDEN

Trends in Cohabitation and Marriage in Sweden

Cohabiting unions are more common in Sweden than anywhere else in the industrialized world, although levels in Denmark now come rather close. Marriage rates have been declining since the late 1960s, while cohabitation rates have been rising. At the same time, the duration of cohabitation has increased. For example, among women born in the late 1940s about half had married their partner after three years of cohabitation, while this was the case for only about one-tenth of women born in the late 1960s – after five years of cohabitation about two-thirds and one-third of the respective cohorts had married (Bracher and Santow 1998).

Thus, cohabitations in Sweden are stable and relatively long-lasting unions. These unions are, however, less stable than formal marriages, and break-up rates have increased over cohorts. For example, about one-tenth of the first consensual unions for women born in the late 1940s were dissolved within three years, while this was true for about one-fourth of the first unions for women born in the mid-1960s (Hoem B. 1995). In spite of elevated marriage rates for pregnant cohabiting women, the majority of women are not formally married, but cohabiting at first birth in Sweden.

Births to non-cohabiting, unmarried women are rare (less than 10 percent of all births). Sweden is probably unique in the industrialized world in having a lower median age for women at first birth than at first marriage.[2] For the children this implies that there is no stigma associated with being born to cohabiting parents.

Legal Differences Between Cohabitation and Marriage in Sweden in 1989[3]

It is commonly believed that there are only minor differences in the legal implications of marriage and cohabitation in Sweden. However, in the case of the union breaking up or if one of the partners dies, there are substantial differences between the two if the couple has children together or prior to their union, and if they have savings or property. The differences are summarized in Table 1. A crucial difference between married spouses and cohabitants is that married spouses are obliged under the law to support each other according to their ability. Further, for a child of married parents, paternity is automatically attributed to the husband of the mother, and the couple will have joint custody of the child. But if the parents are unmarried or cohabiting, the father has to acknowledge paternity, and they only have joint custody of the child if they both agree to that, which most couples do.

Moreover, in a consensual union there is no community property, like there is in marriage. The 1988 "cohabitation-law" stipulates that if cohabitants split-up, what they have acquired for common use should be divided between them. This applies to dwellings, provided they have been acquired for common use. In the event of a separation, according to the law, the partner who is most in need of the apartment/house should have it, regardless of who bought it.[4] Private property, such as stock and bank savings, is not divided. This is true also for property that was acquired before cohabitation, and for property that has been acquired for private use. This is in contrast to the equal division of community property that takes place when a married couple divorce.

[2] Both medians have been increasing, the former from 25.0 years in 1980 to 26.2 years in 1993 and to 28.4 years in 2001 and the latter from 25.6 years to 27.4 years and to 29.6 years in the same years.
[3] This Section draws on Agell (1982, 1989), Insulander-Lindh & Thunberg (1996) and Ståhlberg (2004).
[4] However, if the house/apartment was bought by one of the partners, the other one has to buy the owner off.

Finally, cohabiting couples do not automatically inherit each other. Cohabiting partners may write testaments in favor of one another, but bequests are taxed.[5] Survivors from a cohabiting union have never been entitled to widows' or widowers' pension in the public supplementary pension system, but under certain very specific circumstances, they were eligible in the general pension scheme. Those who received a widow's/widower's pension prior to 1990 and those who were eligible under the pre-1990 rules (see Section 3.3) still receive their pensions and will do so as long as they live. There continue to be widow's/widower's pensions available from collective bargaining agreements, however, the availability and size of such pensions differs across sectors. Thus, these legal implications should affect the incentives to marry differently for different groups. We should expect the selection into marriage and cohabitation to be non-random processes, and as a result, married and cohabiting parents should differ.[6]

The Swedish Widow's Pension Reform and the Marriage Boom in 1989

In the summer of 1988 the Swedish parliament enacted a reform abolishing the widow's pension beginning in January 1990. Under the old system, if a woman's husband (and certain cohabiting partners) died, she was entitled to a widow's pension for the rest of her life. The pension was based on the husband's retirement income. A widow who was below the general retirement age of 65 received 40 percent of his retirement income. According to the transitional provisions, after age 65 a widow would receive the difference between the widow's pension and her own pension. This system was replaced in 1990 by a system where children of the deceased receive child pensions at most until age 18 and the surviving partner—both sexes, married or cohabiting—receives an adjustment pension for up to 12 months.

The adjustment pension depends upon the age of the children and the income of the deceased, and it is not an unconditional right like the widow's pension. Survivors receive the adjustment pension as long as they have children below age 12. For example, if the husband died in 1990 and the couple

[5] The tax on (any) inheritance was abolished from January 1, 2005.
[6] Henz and Sundström (2001) show, for example, that married mothers were more highly educated and older at first birth, on average, than cohabiting mothers. The differences between the two groups have increased over time.

had a 16-year old child, the child would receive a child pension until age 18 and the widow would get the Adjustment pension for a maximum of 12 months. Survivors with no children at home could get the Adjustment pension for a maximum of ten months. In sum, the Adjustment pension is only available for about one year, or until the youngest child turns 12, whereas the widow's pension was for life. The change in the widow's pension was particularly disadvantageous for women with older children or no children at home.

While the parliament's decision certainly was no secret, its significance was not immediately realized. On the contrary, it was not until the fall of 1989 that the implications of the transitional provisions gradually transpired. Importantly for our analysis, the main impact of these provisions was that all non-married women born before 1945 could gain rights to the widow's pension by marrying before the end of 1989.[7] In addition, some women who were born in 1945 or later and who had children with their cohabitant could improve their rights to a widow's pension by marrying before 1990, but the entitlement was much more restrictive than for older women.[8] The effect of the policy change was dramatic. The propensity to marry sky-rocketed in December 1989, especially for cohabiting couples. Figure 1 shows that the number of marriages increased from an average of 3,000 in previous Decembers to 64,000 in December 1989, a 21-fold increase.[9] It is clear from the graph that the marriage boom did not appreciably decrease marriages in the subsequent years, which is evidence that the marriages in the end of 1989 really were marginal marriages that would have been unlikely without the reform.

Though marriage rates in November and December 1989 were particularly elevated for women over 45 (Hoem 1991, Figure 2 and 3), they were also very high for younger women, who would not financially benefit from marrying. We can interpret the latter change as a "bandwagon" effect--couples who held more or less vague plans of marrying in the future, stopped putting it off and

[7] The transitional provisions for women born in 1945 or later were more restrictive and more complicated.
[8] For women born in 1945 or later who married before 1990 the widow's pension is based on the husband's accumulated retirement income at the end of 1989. Essentially, in order to have an impact on any widow's pension, the husband had to have earned a sizeable income for at least ten years before 1990.
[9] For further analyses of the marriage boom, see Hoem (1991) and Andersson (1998, 2004).

married because so many other couples were doing so. Alternatively, they may have found it too time consuming to find out whether the woman would be eligible for a widow's pension and simpler just to marry. In line with this interpretation, there was abundant misreporting and confusion in the media over who would benefit from marrying and who would not. For example, the Swedish newspaper *Västerbotten-kuriren* on November 12, 1989 wrote that women born **after** 1945 who have children with their cohabitant must marry before the turn of the year to be entitled to a widow's pension. This information was clearly at odds with the Widow's Pension Reform. Still another interpretation of the "bandwagon" effect is that the marriage boom made it less expensive to marry since it became acceptable to marry without having a costly reception. In fact, the most common answer among cohabiting women at the time, to the question of why they were not planning to marry was that they could not afford the wedding they wished to have (Hoem B 1995). Although many ineligible couples married in December, 1989, Figure 1 shows no corresponding decrease in marriages in the early 1990s. This suggests that the "bandwagon" marriages were marginal marriages as well. This dramatic response to the change in the Widow's Pension System created an exogenous change in both the timing and the number of marriages that will enable us to examine the marginal effect of marriage on child outcomes.

DATA AND METHODS

Data

For most of the analysis we use a random sample of children born in Sweden in 1977-87 drawn from the population registers of Statistics Sweden. The data sample 20 percent of Swedish children born each year (approximately 20,000 children per year) and their siblings. These data are combined with family and individual information from the bidecennial censuses from 1980, 1985 and 1990, and from Statistics Sweden's special multigenerational register. The multigenerational register identifies biological parents and siblings. We include only children living with both biological parents in our analysis, information we obtain from the censuses. We also impose the restriction that the

parents were born in Sweden. All these requirements leave us with about 14,000 children of each cohort, in total over 130,000 children.

Our outcome variable is grade point average (GPA) at age 16.[10] The grades at age 16 are the final grades from compulsory school, which are used for entrance to different high-school tracks and are therefore vital for pupils. Further, there are compulsory national tests (in math, Swedish and English) aimed at guiding teachers' grading so that grades should be comparable across the whole country. For the cohorts covered by our study, Statistics Sweden has collected the grades at age 16 for all students who have graduated from a school in the country and made the data available for research purposes (*Årskurs 9 registret*).

To construct a useful outcome variable from this information, we must overcome two problems.[11] First, all pupils do not follow the same study tracks through compulsory school; for example in some fields of study there are both advanced and elementary level courses. We avoid this problem by only using the fields of study that all pupils study. These are Swedish, natural science and social science.[12] Second, the grading system underwent a major change during the period of our study. Through graduation year 1997 Sweden had a so-called relative grading system ranging from 1 to 5. The goal was that the national average should be 3.0 with standard deviation 1. In practice the averages in most fields of studies were between 3.1 and 3.2. For this period we simply use the pupil's average grade and standardize it by the overall mean and standard deviation in our sample. From graduation year 1998 and onwards, Sweden has had a so-called criterion referenced grading system with grades at four levels: IG (not pass), G (pass), VG (pass with distinction) and MVG (pass with special distinction). For entrance to high school these grades are valued 0, 10, 15 and 20 points. We

[10] A weakness of the otherwise very rich Swedish register data is that it has no other measure of educational outcomes before age 16.
[11] Another problem in using grade data is that pupils with immigrant background often study special courses from which the grades are not comparable to the rest of the population. By only including Sweden-born pupils with Sweden-born parents, we avoid this problem.
[12] Some schools apply an overall grade in science and social science, whereas other apply separate grades in biology, physics, chemistry in science and in geography, history and social issues in social science.

use these weights to compute a GPA for each student and standardize by the mean and the standard deviation in our sample, based on the grading system under which the student was tested.

We create marital history for the parents of the children using information from population records and the censuses. We have information on all changes in marital status since 1968 and the exact date of these changes. The explanatory variables include: child's gender, year and month of birth, father's and mother's age, parents' earnings and the sibling composition of the household (his children, her children, and their joint biological children).

For the supplementary analysis of sibling differences, we combine information on the children in our sample who had any full siblings born in 1972-87 (about 68,000 children) with information on these siblings (about 94,000 children).[13] We then create the explanatory variable 'proportion of childhood lived with married parents' based on the parents' marriage duration, computed in the same way as in the preceding analysis. This variable thus takes values from 0 to 1. We define childhood as up to and including age 16 since our outcome variable is GPA at age 16.

Estimation Methods

We are interested in estimating the impact of marriage on children's educational outcomes. In other words, suppose parents are initially cohabiting, if they marry, how does parents' marital status affect children's educational attainment measured by GPAs? To fix ideas, let Y_i be the outcome (GPA) of child *i*, let M_i be an indicator for the marital status of parents of child *i*, B_i be the effect of marriage on the outcome, and let u_i be the unobserved component of the potential GPA outcome. The relationship between parents' marital status and the outcome *Y* is given by:

$$Y_i = \alpha + M_i B_i + u_i \qquad (1)$$

Since marriage is not randomly assigned, estimating a regression using all married and unmarried parents will not identify *B*.

[13] Information on siblings is only available for those in the random sample who were born in 1977-84.

Importantly, the effect of marriage on child outcomes, B_i, can be assumed to vary across families, that is, to be heterogeneous, which motivates the subscript *i*. This benefit might very well be zero for many parents, for example, those who are quite ignorant about the marriage contract. But the effect may be positive for those who consider the marriage contract useful for raising children. To make this idea explicit we specify the beneficial effect:

$$B_i = \overline{B} + e_i$$

where $\overline{B}$ is the mean effect in the total population and e_i is the individual deviation from the mean.

Next, we turn to the decision to marry. Since parents care about the educational outcomes of their children and since they may expect marriage to affect these outcomes, their decision will be a function of this expectation. In addition, there may be other benefits and costs associated with marriage, such as gains from specialization within the household. We classify these as 'other net benefits', and denote them OB_i. These other net benefits are heterogeneous in the same way as the child outcome benefits:

$$OB_i = \overline{OB} + v_i$$

If these other net benefits are scaled in the same way as the child effects, we can formulate the marriage decision:

$$\begin{aligned} &\text{Marry:} && (M = 1) \text{ if } B_i + OB_i \geq 0 && (2) \\ &\text{Do not marry:} && (M = 0) \text{ if } B_i + OB_i < 0. \end{aligned}$$

We can see from the equations in (2) that the decision to marry is a function of both the pure benefit of marriage and the other net benefits related to selection.[14]

Based on this framework we can analyze heterogeneous effects of marriage on child outcomes. We distinguish between (i) the average effect of marriage in the population, (ii) the average effect of marriage on the self-selected, and (iii) the marginal effect of marriage on those induced to marry by the pension reform. The average effect of marriage in the population is $E(B_i) = \overline{B}$. This would be

[14] This framework is very similar to that used in Björklund and Moffitt 1987 to discuss marginal treatment effects.

the effect of marriage on the GPA of children if parents were randomly assigned to marriage and other benefits did not affect the decision to marry, such that OB_i=0. However, this is not often the case.

Since many marriages are self-selected, some of them before the birth of the child, we can identify the average effect of marriage on the self-selected, for children whose parents married before the policy change:

$$B_i = E(B_i \mid B_i + OB_i \geq 0) = \overline{B} + E(e_i \mid e_i + v_i) \geq -(\overline{B}_i + \overline{OB}_i) \qquad (3)$$

It follows from equation (3) that this effect is larger than the average effect in the population because of self-selection into marriage based on the impact on children.

Finally, we consider the marginal effect of marriage for children whose parents married due to the policy-induced increase in 'other net benefits' of marriage. For those who do not marry prior to the policy change, $B_i + OB_i < 0$. Suppose that the government institutes a reform that affects OB_i for certain families near the margin, such that $OB_i' > OB_i$. and $B_i + OB_i' \geq 0 > B_i + OB_i$. It is clear from equation (3) that for this group,

$$B_i = E(B_i \mid B_i + OB_i' \geq 0) = \overline{B} + E(e_i \mid e_i + v_i) \geq -(\overline{B}_i + \overline{OB}_i')$$

However, for the population the average of net benefits is smaller than for those who self-selected marriage such that $E(OB_i') = \overline{OB}' < \overline{OB} = E(OB_i)$ and the estimated effect of the marginal marriage is lower $\overline{B} + \overline{OB}' < \overline{B} + \overline{OB}$.

In the case of Sweden, the change in the Widow's Pension System provides a quasi-natural experiment that allows us to examine the effect of marriage on children's outcomes. Let $MBOOM_i$ be an indicator variable for whether parents married in the fall of 1989 as part of the marriage boom brought about by the Widow's Pension Reform. In this case $MBOOM_i$ are considered marginal marriages where cohabiting parents respond to the change in incentives to marry. The identifying assumption is that $MBOOM_i$, marriages during the marriage boom, are exogenous to children's GPAs. Given this assumption we can estimate the effect of M_i, marriages that occur outside of the

marriage boom, and $MBOOM_i$, marriages that occur in response to the change in the Widow's Pension system in equation (4).

$$Y_i = \beta X_i + \gamma M_i + \phi MBOOM_i + \varepsilon_i . \qquad (4)$$

The difference in the estimated coefficients on M_i and $MBOOM_i$ provides a measure of selection bias on the effect of marriage on children's GPAs.

It is possible, however, that the marriages that occurred during the marriage boom also reflect positive selection by parents into marriage. But, most likely, these marriages result less from positive selection than marriages that took place before the marriage boom. It follows that the coefficient on $MBOOM_i$ provides an upper bound on the effect of marriage on children's GPAs and the difference between the coefficients on M_i and $MBOOM_i$ will understate the extent of selection bias.[15]

Up to now, this discussion has assumed that marital status has an effect on children's GPAs. Alternatively, the duration of the child's exposure to marriage may have an impact on educational outcomes. Although marriages in response to the boom may reflect selection to some degree, the effect of marriage duration may be considered somewhat more exogenous. For example, if the cohabiting couple intended to marry sometime in the future, it is clear from the data that the marriage boom changed the timing of the marriage for many couples. Thus, estimates of the effect of marriage durations resulting from the marriage boom should be exogenous.

In addition to the cross-sectional estimates, we estimate sibling fixed-effects models of the effect on children's GPA of the proportion of childhood lived with married parents. The effect of marriage is identified by differences in siblings' exposure to the duration of married parents; this results from the fact that Swedish parents often do not marry until after the first or second child is

[15] Instead of entering marriages that occurred during the marriage boom directly into the regression, one could use the marriage boom as an instrument for marriage for all couples who were at risk of getting married after the Widow's Pension reform was passed. This would require assuming that all marriages during the boom were uncorrelated with child outcomes, $COV[MBOOM_i, u_i] = 0$ but correlated with the probability of marriage. Clearly, marriages that occurred during the boom would be perfectly correlated with marriage. However, it is not clear that $COV[MBOOM_i, u_i] = 0$, thus we use this alternative approach.

born (or not at all). These methods are advantageous because they allow us to control for unobserved, or family-specific time-invariant factors that may be correlated with the marriage decision and observed outcome. Fixed-effects models allow us to use the entire sample of cohabiting couples who decide to marry at some point during their child's lifetime. Although fixed-effects models have these advantages they are subject to limitations as well. The fixed-effects assumption might be invalid because marriage can be a response to economic conditions or changes in incentives (such as the Widow's pension policy change). Thus, the fixed-effects methodology does not correct for bias due to unobservable characteristics that are correlated with marital status but change over time. We address the short-comings in the fixed-effects methodology by including separate variables for proportions of childhood with married parents depending on when parents got married. To fix ideas, let

$$Y_{ij} = \alpha_{ij} + BM_{ij} + u_{ij}$$

where the Y_{ij} is the outcome (GPA) of child *j* in family *i*, M_{ij} is a measure of the proportion of childhood lived with married parents for child *j*, α_{ij} is the unobserved family effect for child *j,* and *u* is the disturbance term. The fixed effect model assumes that $\alpha_{ij} = \alpha_i$ for all children in the family. If this assumption holds, then estimating

$$\Delta Y_i = B\Delta M_i + \Delta u_i$$

identifies the causal effect of marriage on a child's GPA. However if $\alpha_{ij} \neq \alpha_i$, fixed effects estimates will remain biased. We address this by including different indicators of the proportions of childhood with married parents for the children whose parents married before the marriage boom and for those whose parents married during the marriage boom in the fixed-effects estimator

$$\Delta Y_i = \beta\Delta X_i + \gamma\Delta M_i + \phi\Delta MBOOM_i + \Delta u_i$$

thus, controlling for unobserved components of selection and differences between the two-types of marriage. If the coefficients on M_i and $MBOOM_i$ are significantly different from one another, this

would indicate that the fixed-effects assumption is most likely invalid, and selection remains a problem. However, if there is no significant difference in these coefficients, it would suggest that marriage is highly correlated with unobserved family characteristics and fully addressed by the fixed-effects approach.

As noted above, the Widow's Pension Reform caused a boom in two types of marriages. The first group consisted of the women born prior to 1945, older mothers, who qualified for the widow's pension and thus, had a financial incentive to get married. The second group consisted of those women born in 1945 or later, younger mothers, who would not qualify for the widow's pension, but jumped on the marriage bandwagon. Younger mothers may have intended to get married at some point, and just decided to get married because everyone else was doing so, or alternatively because they may have believed they would qualify for the widow's pension. Older mothers had a well-defined financial incentive to marry. We estimate regressions for both groups because the incentives to marry may have differed significantly between them.

FINDINGS

Descriptive Statistics and OLS Regressions

We start by presenting some descriptive statistics. Table 2 focuses on the children in our full estimating sample (who lived with both biological parents in 1990) and displays means and frequencies for family background variables, as well as mean standardized GPAs at age 16 by parents' marital status: if parents were married and when they married. We see that children of parents who married before they were born had significantly higher GPAs on average than all the other groups of children, but among the other groups of children there were no significant differences in GPAs. Second, children whose parents married before birth (Group 1), married before the fall of 1989 (Group 2), or married in the fall of 1989, but did not qualify for the widow's pension (Group 3) all had more full siblings and fewer half siblings in 1990 than those in the remaining groups, which reflects the more stable family situation for the former.

Table 3 presents OLS estimates of the relationship between children's GPAs and whether parents are married or not. It also presents OLS estimates of the relationship between children's GPAs and the timing of parents' marriage, and for the full random sample using family background controls in six different specifications. In Model A we include our indicators for gender, year of birth, and month of birth. Subsequent models add controls for parents' ages, whether or not the parents were teenagers when the child was born, parents' income, number of full and half siblings (mother's side and father's side), and indicators for birth order. The first row of Table 3 shows the effect of marriage relative to cohabitation for all marriages regardless of timing. Marriage increases children's GPA by one-fourth of a standard deviation in Model A. The effect falls significantly as more control variables are included in the specification, but remains positive and significant. The next four rows of Table 3 show the effect of the timing of parent's marriage on children's GPAs. Relative to cohabitation, the coefficient for marriage prior to birth is large and statistically significant, as is the one for marriage after birth, but prior to 1989 in all models. We tested whether the coefficient for marriage prior to birth was significantly different from other marriage indicators and found this to be the case for all specifications. This suggests that marriages before birth have a different association with children's outcomes than post-birth marriages, and likely reflects selection into marriage based on parental commitment. In addition, the coefficient for marriage is positive and significant for those whose parents' married in the fall of 1989, but the mother did not qualify for the widow's pension, although the coefficient is significantly lower than that of marriages that occurred prior to 1989. In contrast, we find no statistically significant correlation between GPAs and marriage for children whose mothers qualified for the widow's pension. Thus, children whose parents married in the fall of 1989 in response to the financial incentives did no better than those whose parents remained unmarried.

It is interesting to note that the additional covariates in Table 3 meet our expectations. Parental age and income are positively associated with children's GPAs. Children from larger families have lower GPAs, especially if they live with half-siblings. Finally, younger siblings have

lower GPAs relative to the oldest sibling. In the remainder of the paper, we omit coefficients on these covariates in order to focus the discussion on the effect of marriage and marriage duration on children's educational outcomes.

Next, we estimate OLS regressions of the relationship between children's GPA and parents' marriage duration for five of the six previous models **(**Table 4**).** The first row shows the effect of the duration of childhood until age 16 that a child's parents are married on children's GPAs.[16] Similar to the results in Table 3, marriage duration has a positive and significant effect on GPAs. As control variables are added to the models, the coefficient on marriage duration falls by one-third. The next four rows of Table 4 show the effect of marriage duration by the timing of the marriage. As before, the coefficients on marriage duration for marriages prior to 1989 are positive, statistically significant, and larger than the effect of marriages that occurred during the 1989 boom. Marriage duration has no significant effect on children's GPAs for those with mothers born before 1945. The estimates in Tables 3 and 4 suggest that selection bias may explain part of the effect of marriage on children's educational outcomes.

Does Selection Bias Explain the Effect of Marriage on Children's GPAs?

We now consider the extent of the selection bias by limiting the sample to parents who were unmarried (cohabiting) at the birth of their children and including indicators for whether the parents married prior to 1989 or during the marriage boom. We limit the sample to unmarried parents in order to understand the impact of parents becoming married on children's educational outcomes. If there is no selection bias in the effect of marriage, the difference between these coefficients will be zero. If we assume that marriages during the boom are completely exogenous, then this coefficient would represent the causal effect of marriage. Alternatively, if marriages during the boom to some degree reflect positive selection this coefficient can be interpreted as an upper bound on the causal effect of marriage on children's GPAs.

[16] Consistent with the classification in Table 2 and 3, we classify parents as married only if they married prior to 1990.

Table 5 estimates five specifications of the effect of marriage and marriage duration on children's GPAs using the specification given by equation (1'). The first panel presents estimates for the entire sample of parents who were unmarried at the time of their child's birth and includes indicators for marriage before and during the marriage boom. Estimates in the first panel indicate a substantial degree of positive selection bias: we test the equality of the marriage coefficients and reject this at ($p<.01$) for all specifications using the marriage dummy and reject this at ($p<.01$) for specifications C-F using marriage duration. Marriage before the boom significantly increases children's GPAs, however the coefficient drops as more control variables are added to the model. The same is true for marriages during the boom, however, the coefficient decreases by 75 percent between Models A through F and is only statistically significant at the 10 percent level in Model F. This suggests that much of the estimated impact of marriage can be attributed to positive selection on unobservables as well as selection on the observables that we include in the models. The estimates on marriage duration reflect a similar pattern, however, the increase in marriage duration has a positive and significant effect on children's GPAs for those married during the boom after controlling for all observables (Model F). This coefficient is about half the size of parents who married before the boom.

The next two panels of Table 5 present separate estimates for children whose mothers were eligible for the widow's pension (born < 1945) and those with ineligible mothers (born ≥ 1945). Interestingly, we find no statistically significant effect of marriage or marriage duration for children with older mothers in panel 2. This suggests that marriages in response to the widow's pension reform are exogenous at least for the older mothers and have no causal effect on children's educational outcomes. These estimates may also reflect the fact that a larger fraction of the older mothers, than of the younger ones, married before the child was born. In our sample, 83 percent of older mothers were married before the child was born compared with 60 percent of younger mothers.

The final panel of Table 5 presents results for children of younger mothers and reflects similar patterns and coefficients as panel 1. Taken together, the results in Table 5 indicate that positive selection into marriage explains most of the effect of marriage on children's GPAs. After controlling for a full set of observable characteristics (Model F) marriage has only a marginally significant impact on children's GPAs. In contrast, marriage duration, the length of a child's exposure to marriage, does have a positive impact on children's GPAs after controlling for all observables. However, the coefficient is about half the size of marriages that occurred before the boom. Next, we control for family fixed effects in order to determine whether unobserved family characteristics explain the effect of marriage duration on children's GPAs.

Controlling for Family Fixed-Effects

We continue the analysis by exploiting the variation between siblings in proportion of childhood lived with married and cohabiting parents. Most siblings differ in the proportion of childhood lived with married or cohabiting parents since the majority of Swedish couples begin their union by cohabiting and often do not marry until after their first child is born. We investigate whether children whose parents were married during a greater fraction of their childhood have more favorable educational outcomes than those whose parents were cohabiting during a greater fraction of their childhood. To that end, we compare the GPAs at age 16 for full siblings born in Sweden in 1972-87 who grew up with both biological parents and whose parents were either married or cohabiting. We exploit this difference by constructing the variable proportion of childhood lived with married parents, which takes the values from 0 to 1. We define childhood as up to and including age 16 since our outcome variable is GPA at age 16, and compute marriage duration in the same way as in the preceding analysis. As before, we include indicators for whether the parents married before or during the marriage boom.

In addition, to investigate whether younger siblings who lived their whole childhoods with married parents gain educational advantages over their older siblings who lived longer with cohabiting

parents we include a dummy variable (in Models 2 and 3), which equals one if the proportion with married parents equals one and zero otherwise. Finally, as second or third born children are more likely than first-borns to have lived with married parents we include controls for birth order (in Model 3).

The resulting family fixed-effects estimates are presented in Table 6 and show no impact of the proportion lived with married parents on children's educational outcomes as measured by their GPAs at age 16 for marriages that occurred before and during the marriage boom. Moreover, the estimates of Model 2 and 3 indicate that siblings who lived their whole childhoods with married parents have no educational advantages over siblings who lived longer with cohabiting parents. Further, since firstborns, on average, do better in school than their younger siblings and also spend a greater fraction of childhood with cohabiting parents we control for birthorder in Model 3. However, the resulting estimates still indicate no impact on GPAs of proportion of childhood lived with married parents.

Taken together, the results in Tables 5 and 6 suggest that positive selection into marriage explains all of the estimated effect of marriage and marriage duration on children's GPAs. In our final set of analyses we conduct robustness checks.

Robustness Checks

We return to the sample used in Table 5 to estimate OLS models of the effect of marriage on children's GPAs with subsamples of the data in order to evaluate the robustness of our results in Table 7.[17] In this table, the effect of marriage is not statistically significant for children of mothers born before 1945. Marriages that occurred before the boom are positive and statistically significant for all subsamples of younger mothers, however, marriages during the boom vary in statistical significance by the sample. We begin by splitting the sample by the sex of the child. Unlike previous estimates, we find that marriage during the boom has a small positive and significant effect for boys across all

[17] We estimated the same models using marriage duration instead of marriage and the results are qualitatively the same. These estimates are available from the authors by request.

four specifications and were significant for specifications E and F at ($p<.05$) where the full set of control variables were included. However, for girls, the effect of marriage on GPA is not statistically significant once controls are added in specifications E and F. This may be because fathers tend to be more involved in parenting when they have a son which, in turn, may increase marital quality (as well as the GPAs of boys) as suggested by Lundberg and Rose (2003). If this is the case, the significant effect for boys would reflect selection rather than causation. Also, if it were a causal effect one would expect to find it for the children of older mothers as well.

Second, since the younger cohorts of children have spent a larger fraction of their childhoods with parents who married during the marriage boom, we would expect the effect on their GPAs to be stronger than for the older cohorts, if marriage matters. Therefore, we divide the sample into the cohorts born in 1983-87 and those born in 1977-82. We find a positive and significant effect of marriage during the boom for the older cohorts ($p<.10$ for specifications E and F), which is at odds with this expectation.

Third, we eliminate children whose parents had divorced by 1991. Again we find a positive and significant impact of marriage during the boom on children's GPAs across all four specifications. This suggests that "healthy marriage" may have a causal impact on child outcomes relative to marginal or troubled marriages. Thus, it could be that it is the quality of the relationship that matters, and we expect relationship quality to influence selection into marriage.

Fourth, we examine whether children living in blended families are affected differently by marriage by eliminating all families with half-siblings. We find no significant effect of marriage during the boom on children who live with parents and full siblings only. This suggests that marriage may confer advantages to children in blended families relative to cohabitation.

Fifth, we examine whether the estimated effect of marriage differs given the level of total family income. We find no qualitative difference in the estimated effect of marriage during the boom given that a child's family is above median income. However, children whose parents have below

median income and marry during the boom experience a positive effect of marriage on GPA. This suggests that marriage may facilitate investments in children by low-income families, although the estimated effect is half that of children whose parents marry prior to 1989.

Overall, the results in Table 7 support our earlier findings that almost all of the estimated effect of marriage can be explained by positive selection into marriage.

CONCLUSIONS

This research poses the question: for a child who lives with both biological parents, does it matter if parents are legally married or living in a consensual union? More specifically we ask: Should public policy encourage couples to marry to counteract the worldwide trend towards cohabitation? We examine the impact of marginal marriages brought about by a policy change in Sweden. We use register data from Sweden on a random sample of more than 130,000 children born in 1972-1987 to answer these questions. More precisely, we use a marriage boom in the end of 1989 created by the Widow's Pension Reform to identify the causal effect of marriage on children's educational outcomes and the degree to which selection bias explains the effect of marriage on children's educational outcomes.

We began by estimating cross-section regressions of the association between children's educational outcomes, as measured by their GPA at age 16, and the timing of parents' marriage with family background controls included. We find a positive association with marriage for children whose parents had married before their birth, after their birth but before the fall of 1989, and those who married in the fall of 1989 but did not qualify for the widow's pension. However, for children whose parents married in the fall of 1989, marginal marriages that were in response to the financial incentives of the policy change, we find no such educational advantage over children whose parents continued to cohabit without formal marriage. These findings suggest that positive association between marriage and children's education may be due to selection on observables rather than to causation. We also estimated OLS-regressions of relationship between children's GPA and marriage

duration. After controlling for the full set of observable characteristics, marriage duration has a positive and significant effect on children's GPAs for all groups with the exception of mothers born before 1945.

Next, we limit the sample to children whose parents were unmarried at their birth and estimate the effect of marriage and marriage duration on children's GPAs controlling for whether the parents married prior to or during the marriage boom. The difference between these coefficients provides an estimate of the effect of selection bias, and the coefficient on marriages during the boom can be interpreted as the upper bound on the causal effect of marriage on children's GPAs. We find that almost all of the estimated effect of marriage and marriage duration on children's educational outcomes results from positive selection. After controlling for a full set of observable characteristics only marriage duration remains significant at conventional levels, suggesting that length of exposure to marriage may have a causal effect on children's GPAs.

To examine whether unobserved family characteristics explain this effect, we exploited the variation among siblings in proportion of childhood lived with married and cohabiting parents, created by the fact that over half of all first-born children in Sweden are born in cohabiting unions, to estimate sibling fixed-effects models of the effect of marriage on children's GPAs. We find no statistically significant effect of marriage on children's educational outcomes. A series of robustness checks using the sample of children whose parents were unmarried at their birth support our preliminary results. For children with mothers born before 1945 we find no impact of marriage, neither before the boom, nor during the boom, for any subgroup of children. In contrast, children with younger mothers whose parents married before the boom have higher GPAs relative to children with cohabiting parents in all subgroups, while among those whose parents married during the boom only those whose parents earn less than median income or did not divorce have higher GPAs relative to children with cohabiting parents.

We interpret our results as showing that there is no causal effect of marriage on children's educational outcomes, and that much of the apparent benefit of parental marriage is due to selection. Our findings are consistent with McLanahan and Sandefur's (1994) hypothesis that it is the biological relationship of parents to children that matters most for child outcomes. Our results provide bad news for policy-makers who seek to enact policies to promote marriage. For marriage to have a positive impact on child outcomes, it seems necessary that parents marry because they want to, not because they respond to a policy incentive.

Although we find no evidence of a causal effect of marriage on children's educational outcomes in Sweden, several caveats are clearly in order. First, we cannot rule out the possibility that marriage has causal effects in countries where cohabitation is less prevalent than it is in Sweden. For example, while the practice of granting alimony upon divorce was abandoned decades ago in Sweden, it is still sometimes practiced in the U.S., which makes marriage and cohabitation differ more than in Sweden. Second, it is also possible that being born to unmarried parents carries more of a stigma for children in the U.S. than it does for children in Sweden. Third, we cannot rule out the possibility that marriage has causal effects on other outcomes than education for children in the Swedish context. Finally, our study compares the effect of only married biological parents to cohabiting biological parents on children's educational outcomes. Our results do not generalize to blended families with non-biological cohabiting or married parents.

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Table 1: Differences in Legal Arrangement of Marriage and Cohabitation in Sweden

Legal Arrangement:	**Marriage:**	**Cohabitation:**
Obligation to Support Spouse	• Yes: Spouses obligated to support one another	• No: Partners not obligated to support one another
Paternity	• Husband of mother is granted paternity	• Biological father must legally recognize child
Custody	• Joint Custody	• Requires agreement by parents
Taxation	• Property Income and Wealth Taxed Jointly	• Property Income and Wealth Taxed Jointly if share children < 18 years
Community Property	• Yes	• No
Inheritance	• Spouses automatically inherit	• Written testament required for inheritance
Dissolution Costs	• Dissolution requires legal costs	• Limited or no legal costs

Table 2. Descriptive statistics by parents' marital status and timing. Children born 1977-87 living with both biological parents in 1990. (Standard deviations in parentheses).

Parental marital Status	GPA standardized	Mums age at child birth	Dads age at child birth	Child's age at parents' marriage	Dad's Annual Earnings 1985, Ths SEK	Mum's Annual Earnings 1985, Ths SEK	# full sibs, half sibs (mum), half sibs (dad) 1990	Mum dead 1991	Dad dead 1991	Marriage duration years [a]
1. Married before birth of child [N=80,593]	0.081	29.5	32.1	0	124	60	1.69	0.009	0.015	15.5
	(0.934)				(59)	(37)	0.10			
							0.13			
2. Married after birth but before fall 1989 [N=23,873]	-0.107	25.8	28.4	2.4	109	58	1.51	0.005	0.010	13.1
	(0.937)				(43)	(31)	0.16			
							0.16			
3. Married Fall 1989--mum born in 1945 or later [N=9,453]	-0.139	27.4	30.3	5.7	106	59	1.42	0.005	0.010	9.7
	(0.958)				(37)	(30)	0.14			
							0.17			
4. Married Nov-Dec 1989 --mum born before 1945 [N=188]	-0.138	37.8	37.9	9.2	118	61	0.61	0.016	0.037	6.8
	(0.978)				(49)	(37)	0.83			
							0.49			
5. Not married in 1989, mum born in 1945 or later [N=16,665]	-0.226	26.9	29.7		99	60	1.29	0.007	0.016	0
	(1.013)				(44)	(31)	0.26			
							0.28			
6. Not married in 1989, mum born before 1945 [N=516]	-0.176	38.3	37.4		114	63	0.59	0.029	0.048	0
	(1.001)				(56)	(45)	1.14			
							0.61			
All who live with bio parents in 1990 [N=131,288]	-0.009	28.4	31.0		116	59	1.58	0.008	0.014	14.5 [b]
	(0.954)				(54)	(35)	0.14			
							0.16			

Table 3 Cross-section estimates of association between and married parents and between children's GPA and timing of parents' marriage. Full Cross-Sectional Sample

	A	B	C	D	E	F
All with married parents[ab]	0.257**	0.195**	0.172**	0.177**	0.138**	0.154**
	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]	[0.008]
Married Before Birth[a]	0.319**	0.229**	0.203**	0.212**	0.165**	0.207**
	[0.008]	[0.008]	[0.008]	[0.009]	[0.008]	[0.008]
Married After Birth[a]	0.129**	0.161**	0.141**	0.145**	0.122**	0.097**
Before Fall 1989	[0.010]	[0.010]	[0.009]	[0.009]	[0.009]	[0.009]
Married Fall 1989 [a]	0.087**	0.068**	0.067**	0.070**	0.034**	0.032**
Mom Born >1944	[0.012]	[0.012]	[0.012]	[0.012]	[0.012]	[0.012]
Married Fall 1989[a]	0.140*	-0.026	0.012	0.004	0.044	0.026
Mom Born <1945	[0.067]	[0.067]	[0.065]	[0.065]	[0.063]	[0.063]
Mother's Age [c]		0.152**	0.120**	0.118**	0.099**	0.133**
		[0.007]	[0.006]	[0.006]	[0.006]	[0.006]
Father's Age [c]		0.010*	0.006	0.005	0.002	0.021**
		[0.005]	[0.005]	[0.005]	[0.005]	[0.005]
Father's Income [c]			0.910**	0.909**	0.859**	0.821**
			[0.048]	[0.048]	[0.046]	[0.045]
Mother's Income [c]			1.916**	1.866**	1.736**	1.552**
			[0.070]	[0.070]	[0.068]	[0.063]
Full Siblings				-0.017**	-0.043**	-0.016**
				[0.003]	[0.003]	[0.003]
Half-Sibs (Mother)					-0.230**	-0.272**
					[0.006]	[0.006]
Half-Sibs (Father)					-0.095**	-0.119**
					[0.005]	[0.006]
Second Born						-0.327**
						[0.007]
Third Born						-0.320**
						[0.007]
Fourth Born						-0.343**
						[0.012]
Fifth Born						-0.305**
						[0.012]
Sex	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes	Yes	Yes	Yes
Month of birth	Yes	Yes	Yes	Yes	Yes	Yes
Teen Mother	No	Yes	Yes	Yes	Yes	Yes
Teen Father	No	Yes	Yes	Yes	Yes	Yes
Parents' Age Interacted	No	Yes	Yes	Yes	Yes	Yes
Observations	131288	131288	131288	131288	131288	131288
R-squared	0.07	0.09	0.12	0.12	0.14	0.16

[a] Reference group is children whose cohabiting parents had not married before 1990. [b] Specification includes all controls in Model A – F. Coefficients not shown. [c]Specification includes squared term. Robust standard errors in brackets. * significant at 5%; ** significant at 1%

Table 4 Cross-section estimates of association between children's GPA and parents' marriage duration. Full Cross-Sectional Sample [a]

	A	C	D	E	F
Duration all marriage[b]s	0.023**	0.016**	0.017**	0.013**	0.015**
	[0.000]	[0.000]	[0.001]	[0.000]	[0.000]
Dur Marr Before Birth[b]	0.024**	0.016**	0.017**	0.013**	0.016**
	[0.000]	[0.000]	[0.001]	[0.001]	[0.001]
Dur Marr After Birth[b]	0.014**	0.014**	0.014**	0.012**	0.010**
Before Fall 1989	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Dur Marr Fall 1989 [b]	0.014**	0.011**	0.012**	0.007**	0.007**
Mom Born >1944	[0.001]	[0.001]	[0.001]	[0.001]	[0.001]
Dur Marr Fall 1989[b]	0.026*	0.010	0.008	0.013	0.010
Mom Born <1945	[0.010]	[0.009]	[0.009]	[0.009]	[0.009]
Observations	131288	131288	131288	131288	131288
R-squared	0.07	0.12	0.12	0.14	0.16

[a] Specifications as in Table 3 but Model B omitted. [b] Reference group is children whose cohabiting parents had not married before 1990. [c] Specification includes squared term. Robust standard errors in brackets. * significant at 5%; ** significant at 1%

Table 5.Cross-section estimates of association between timing of parents' marriage, marriage duration and children's GPA. Children whose parents were cohabiting (not married) before their birth by age of the mother.[a]

All	**A**	**C**	**D**	**E**	**F**
Married before boom [b]	0.130***	0.133***	0.131***	0.117***	0.0922***
	[0.0098]	[0.0096]	[0.0097]	[0.0096]	[0.0095]
Married during the boom [b]	0.0876***	0.0575***	0.0561***	0.0237**	0.0225*
	[0.0121]	[0.0118]	[0.0118]	[0.0117]	[0.0116]
R-squared	0.055	0.116	0.116	0.136	0.157
Before boom: duration	0.0121***	0.0114***	0.0113***	0.0099***	0.0080***
	[0.0007]	[0.0007]	[0.0007]	[0.0007]	[0.0007]
During boom: duration	0.0123***	0.0083***	0.0082***	0.0045***	0.0045***
	[0.0012]	[0.0011]	[0.0012]	[0.0011]	[0.0011]
R-squared	0.057	0.118	0.118	0.137	0.157
Observations	50695	50695	50695	50695	50695
Mum born<1945					
Married before boom [b]	0.0714	0.0480	0.0468	0.0521	0.0465
	[0.0622]	[0.0608]	[0.0608]	[0.0585]	[0.0586]
Married during the boom [b]	0.0783	0.0718	0.0711	0.0117	0.0183
	[0.0795]	[0.0756]	[0.0754]	[0.0736]	[0.0734]
R-squared	0.087	0.160	0.162	0.213	0.218
Before boom: duration	0.0051	0.0031	0.0030	0.0032	0.0026
	[0.0044]	[0.0043]	[0.0043]	[0.0042]	[0.0042]
During boom: duration	0.0115	0.0094	0.0092	0.0014	0.0022
	[0.0113]	[0.0105]	[0.0105]	[0.0102]	[0.0102]
R-squared	0.087	0.160	0.162	0.212	0.218
Observations	1140	1140	1140	1140	1140
Mum born ≥1945					
Married before boom[b]	0.133***	0.134***	0.132***	0.117***	0.0925***
	[0.0100]	[0.0098]	[0.0098]	[0.0097]	[0.0096]
Married during the boom[b]	0.0893***	0.0572***	0.0560***	0.0237**	0.0222*
	[0.0123]	[0.0119]	[0.0119]	[0.0118]	[0.0117]
R-squared	0.054	0.116	0.116	0.135	0.157
Before boom: duration	0.0121***	0.0114***	0.0113***	0.0099***	0.0080***
	[0.0007]	[0.0007]	[0.0007]	[0.0007]	[0.0007]
During boom: duration	0.0123***	0.0083***	0.0082***	0.0045***	0.0045***
	[0.0012]	[0.0011]	[0.0012]	[0.0011]	[0.0011]
R-squared	0.057	0.118	0.118	0.137	0.157
Observations	49555	49555	49555	49555	49555

[a] Specifications as in Table 3, Model B omitted. [b] Reference group is children whose cohabiting parents had not married before 1990. Robust standard errors in brackets. *significant at 10%, **significant at 5%; ***significant at 1%

All: Equality between the coefficients for marriages before and during boom rejected at p <.01 for A-F. Duration: equality rejected at p < .01 for C-F, not for A.
Older mums: Equality not rejected for any specification, neither for marriage, nor for duration.
Younger mums: Equality rejected at p<.01 for A-F. Duration: equality rejected at p < .01 for C-F, not for A.

Table 6. Fixed-effect estimates of the relationship between the proportion of childhood lived with married parents and educational outcome for full siblings born in Sweden in 1972-87. Dependent variable: Grade Point Averages at age 16.

GPA	Model 1	Model 2	Model 3
Prop. lived with married parents[a]			
Parents married before Fall 1989	-0.037	-0.004	0.044
	(0.039)	(0.048)	(0.048)
Parents married Nov-Dec 1989	0.024	-0.020	0.073
	(0.058)	(0.068)	(0.068)
Lived with married parents whole childhood[b]			
Parents married before Fall 1989		-0.014	0.007
		(0.011)	(0.011)
Parents married Nov-Dec 1989		0.090	0.022
		(0.080)	(0.080)
Oldest child			0.132**
			(0.007)
Youngest child			-0.051**
			(0.009)
Sex	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes
Month of birth	Yes	Yes	Yes
# Families	74,706	74,706	74,706
# Observations	162,234	162,234	162,234
#Identifying observations	24,508	24,508	24,508
R-square within	0.0923	0.0923	0.0985

[a] The proportion lived with married parents has been computed as the proportion of years up to age 16 parents were married. [b] Indicator = 1 if child lived with married parents during the whole childhood, =0 otherwise. Robust standard errors in brackets.** p < .01.

Table 7. Sensitivity analysis: Cross-sectional estimates of relationship between timing of parents' marriage and children's GPA for subgroups of children whose parents were cohabiting before their birth by age of the mother.

	Mum born<1945				Mum born ≥1945			
Girls Only	**A**	**C**	**E**	**F**	**A**	**C**	**E**	**F**
Married before boom [b]	0.081	0.054	0.049	0.046	0.114**	0.114**	0.098**	0.074**
	[0.089]	[0.087]	[0.083]	[0.084]	[0.014]	[0.014]	[0.014]	[0.014]
Married during the boom [b]	0.193	0.190	0.112	0.115	0.072**	0.041*	0.006	0.005
	[0.112]	[0.106]	[0.104]	[0.105]	[0.018]	[0.017]	[0.017]	[0.017]
Observations	556	556	556	556	24383	24383	24383	24383
R-squared	0.041	0.116	0.179	0.185	0.010	0.075	0.097	0.119
Boys Only								
Married before boom [b]	0.057	0.039	0.052	0.047	0.151**	0.155**	0.138**	0.111**
	[0.089]	[0.087]	[0.085]	[0.085]	[0.014]	[0.014]	[0.014]	[0.014]
Married during the boom [b]	-0.011	-0.021	-0.068	-0.062	0.107**	0.073**	0.041*	0.039*
	[0.113]	[0.108]	[0.105]	[0.105]	[0.017]	[0.017]	[0.017]	[0.017]
Observations	584	584	584	584	25172	25172	25172	25172
R-squared	0.042	0.137	0.181	0.187	0.011	0.075	0.094	0.116
Younger: Born 1983-87								
Married before boom [b]	0.077	0.115	0.047	0.047	0.118**	0.115**	0.105**	0.090**
	[0.182]	[0.185]	[0.172]	[0.172]	[0.014]	[0.014]	[0.013]	[0.013]
Married during the boom [b]	0.279	0.264	0.197	0.237	0.069**	0.030	-0.000	0.007
	[0.192]	[0.171]	[0.181]	[0.185]	[0.016]	[0.016]	[0.016]	[0.015]
Observations	205	205	205	205	27521	27521	27521	27521
R-squared	0.163	0.231	0.301	0.319	0.050	0.111	0.134	0.153
Older: Born 1977-82								
Married before boom [b]	0.067	0.025	0.044	0.036	0.155**	0.160**	0.138**	0.099**
	[0.067]	[0.065]	[0.063]	[0.064]	[0.014]	[0.014]	[0.014]	[0.014]
Married during the boom [b]	0.032	0.037	-0.014	-0.011	0.125**	0.102**	0.067**	0.047*
	[0.087]	[0.084]	[0.081]	[0.081]	[0.019]	[0.018]	[0.018]	[0.018]
Observations	935	935	935	935	22034	22034	22034	22034
R-squared	0.080	0.160	0.215	0.220	0.064	0.128	0.142	0.165
No Divorce								
Married before boom [b]	0.068	0.045	0.054	0.048	0.159**	0.151**	0.132**	0.103**
	[0.063]	[0.062]	[0.059]	[0.059]	[0.010]	[0.010]	[0.010]	[0.010]
Married during the boom [b]	0.067	0.063	0.007	0.014	0.131**	0.093**	0.057**	0.053**
	[0.082]	[0.078]	[0.076]	[0.076]	[0.013]	[0.012]	[0.012]	[0.012]
Observations	1096	1096	1096	1096	43932	43932	43932	43932
R-squared	0.096	0.169	0.220	0.224	0.059	0.119	0.136	0.158
No Half Siblings								
Married before boom [b]	0.130	0.093	0.081	0.062	0.106**	0.121**	0.124**	0.100**
	[0.098]	[0.098]	[0.098]	[0.098]	[0.011]	[0.011]	[0.011]	[0.011]
Married during the boom [b]	-0.014	-0.014	-0.036	-0.036	0.059**	0.023	0.025	0.022
	[0.123]	[0.123]	[0.123]	[0.124]	[0.014]	[0.013]	[0.013]	[0.013]
Observations	393	393	393	393	38524	38524	38524	38524
R-squared	0.102	0.142	0.151	0.161	0.058	0.124	0.124	0.151

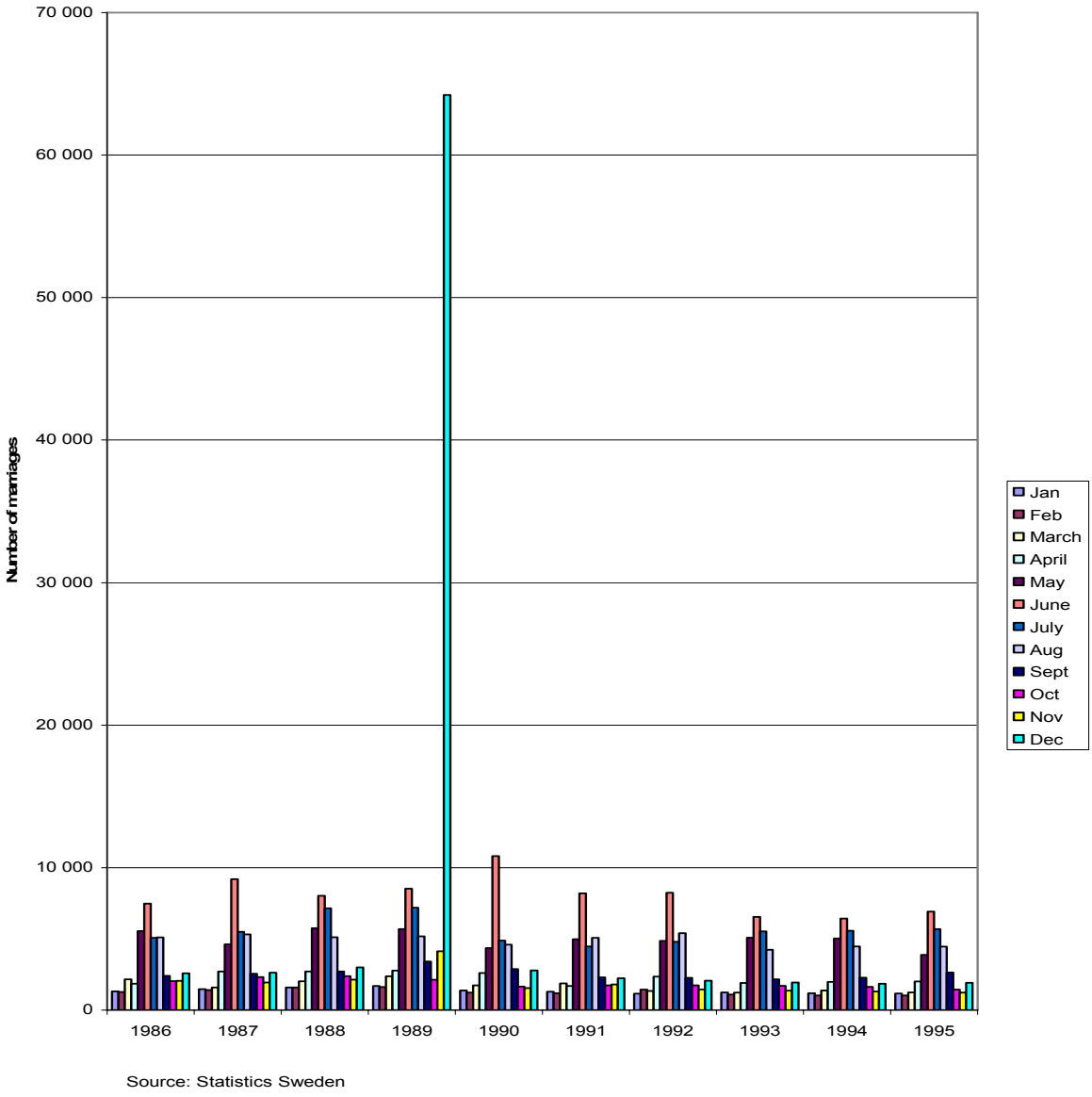
Table 7. (Continued)

	A	C	E	F	A	C	E	F
Less than Median Income								
Married before boom [b]	0.071	0.080	0.064	0.059	0.106**	0.138**	0.124**	0.096**
	[0.076]	[0.073]	[0.072]	[0.072]	[0.014]	[0.014]	[0.014]	[0.013]
Married during the boom [b]	0.092	0.134	0.081	0.090	0.079**	0.077**	0.046**	0.043**
	[0.095]	[0.089]	[0.088]	[0.088]	[0.017]	[0.017]	[0.017]	[0.016]
Observations	703	703	703	703	24644	24644	24644	24644
R-squared	0.112	0.217	0.249	0.253	0.058	0.108	0.125	0.151
Greater than Median Income								
Married before boom [b]	0.017	0.020	0.063	0.053	0.123**	0.136**	0.118**	0.096**
	[0.104]	[0.106]	[0.101]	[0.104]	[0.014]	[0.014]	[0.014]	[0.014]
Married during the boom [b]	-0.007	0.006	-0.062	-0.053	0.067**	0.049**	0.016	0.014
	[0.133]	[0.136]	[0.136]	[0.136]	[0.017]	[0.017]	[0.017]	[0.017]
Observations	437	437	437	437	24911	24911	24911	24911
R-squared	0.122	0.148	0.227	0.237	0.056	0.078	0.100	0.118

Notes: Specifications described in Table 3 Robust standard errors in brackets. * significant at 5%; ** significant at 1%.

For children with mums born < 1945 equality between the two marriage coefficients could not be rejected for any subgroup or specification.
For children with mums born ≥1945 equality between the two marriage coefficients was rejected at p< .01 for all subgroups and specifications except for No Divorce Mod. A which was rejected at p < .05 and for Older: Born 1977-82 Mod. A and Less than Median Income Mod. A for which equality could not be rejected.

Figure 1. Marriages in Sweden per month 1986-1995



Source: Statistics Sweden