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Anders Björklund
Donna K. Ginther
Marianne Sundström

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Forschungsinstitut
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of Labor

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Anders Björklund
SOFI, Stockholm University
and IZA

Donna K. Ginther
University of Kansas

Marianne Sundström
SOFI, Stockholm University

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IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

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ABSTRACT

Does Marriage Matter for Children? Assessing the Causal Impact of Legal Marriage*

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. 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 policy relevant question 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 confirm the expected pattern that children with married parents do better than children with cohabiting parents. However, once we control for observable family background, or use instrumental-variables estimation to compare the outcomes for those children whose parents married due to the reform with those children whose parents remained unmarried, the differences disappeared. A supplementary sibling difference analysis also supports the conclusion that the differentials among children of married and cohabiting parents reflect selection rather than causation.

JEL Classification: J10, J12, J13, J18

Keywords: family structure, marriage, child well-being, educational attainment

Corresponding author:

Anders Björklund
Swedish Institute for Social Research (SOFI)
Stockholm University
SE-10691 Stockholm
Sweden
E-mail: anders.bjorklund@sofi.su.se

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1. 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 has launched a USD1.5 billion drive for promotion of 'healthy marriages' (*The New York Times* January 14, 2004). 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. 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 in such ways 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 and the accompanying marriage boom 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.

The second feature of Sweden that allows us to conduct this study is its administrative register data. These data sources allow us to 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 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 no 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. An instrumental-variable analysis with the marriage boom as an instrument for marriage also leads us to conclude that the observed differentials are due to selection rather than being caused by marriage per se. A supplementary sibling analysis which exploits the variation between siblings in the proportion of childhood lived with married and cohabiting parents leads to the same conclusion.

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.

2. 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 signal a greater commitment. Also, the expected duration of a marriage is longer than that of a consensual union. These aspects together with the legal arrangement of marriage may provide for the four functions of marriage mentioned by Weiss (1997) and lead to greater investments in 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. In addition, using U.S. data, Stratton (2004) finds that cohabiting households engage in less intrahousehold specialization than married households. 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 a parent has an incentive to under-invest in their children and free-ride 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, any effect on child outcomes would be the result of selection 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 all of this research does not control for selection into

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

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. However, 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. 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 differentials.

3. Cohabitation and marriage in Sweden

3.1 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 at first birth but cohabiting 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.

3.2 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. There are, however, substantial differences if the union breaks up or one of the partners dies, if the couple has children together or prior to their union, or if they have savings or property. The differences are summarized in Table 1. A crucial difference between married

[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).

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 as 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.

Finally, cohabiting couples do not automatically inherit each other. Cohabiting partners may write testaments in favor of each other, 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 continues to be widow's/widower's pensions available from collective bargaining agreements, however, the availability and size of such pensions differ across sectors. Thus, these legal implications should affect the incentives to marry differently for different groups. We should expect

[4] However, if the house/apartment was bought by one of the partners, the other one has to buy the owner off.
[5] The tax on (any) inheritance was abolished from January 1, 2005.

the selection into marriage and cohabitation to be non-random processes, and as a result, married and cohabiting parents should differ.[6]

3.3 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 with certain transitional provisions. 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-- receive an adjustment pension for up to 12 months.

The adjustment pension depends upon the age of the children and the income of the deceased, thus, 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 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 who have 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

[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.

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 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.

Although 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 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 to 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 to the question why they were not planning to marry was that they could not afford

[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, 2003).

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 constitutes a quasi-natural experiment that will enable us to examine the causal marginal effect of marriage on child outcomes.

4. Data and methods

4.1 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. [10]

Our outcome variable is grade point average (GPA) at age 16.[11] The grades at age 16 are the final grades from compulsory school that 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

[10] We restrict our random sample to the cohorts 1972-1987 because the outcome variable is only available from the 1972 cohort to present and because we want to make sure these children are exposed to the policy change in 1989.
[11] A weakness of the otherwise very rich Swedish register data is that it has no other measure of educational outcomes before age 16.

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.[12] 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.[13] 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 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.[14] The explanatory variables include child's gender and 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).

[12] 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.
[13] 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.
[14] Since we have information on whether parents were married in 1970 we also obtain marital status for those who married before 1968.

For the supplementary analysis of sibling differences, we construct a sample by combining information on the children in our random sample born in 1977-84 who had any full siblings born in 1972-87 (about 68,000 children), with information on these siblings (about 94,000 children), which creates a sample of over 162,000. We then create the explanatory variable 'proportion of childhood lived with married parents' based 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.

4.3 Estimation strategies

Our approach is based on the assumption that marriage is not randomly assigned and we pose the following research question: How does a marginal increase in the exposure to married parents affect the educational outcomes of children? In other words, suppose parents are initially cohabiting, if they marry, how does parents' marital status affect children's GPAs?

We begin by examining statistics on the correlation between parents' marital status and children's GPA in a random sample of Swedish children. Let M_i be an indicator for the marital status of parents of child *i*, and let y_i be the outcome (GPA) of child *i* with u_i as the random error term. The relationship between parents' marital status and the outcome *y* is given by:

$$y_i = \alpha + M_i\delta + u_i \qquad (1)$$

Since marriage is not randomly assigned, δ will not identify the causal effect of marriage.

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 Z_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 Z_i are considered exogenous marriages where cohabiting parents respond to the change in incentives to marry. The identifying assumption is that Z_i, marriages during the marriage boom, are uncorrelated with children's GPAs. Given this assumption we can estimate equation (1') by including Z_i directly in the model.

$$y_i = \alpha + Z_i\delta + u_i \qquad (1')$$

Even 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.

Returning to equation (1), we can use marriages that occurred during the marriage boom, as an instrument for all marriages for those who were at risk of marriage after the Widow's Pension Reform was enacted. IV models require an instrument that is correlated with the probability of marriage and uncorrelated with children's GPAs. Of course, marriages that occur during the marriage boom are perfectly correlated with the probability of marriage. We maintain that these marriages are exogenous, and any exogenous variable can be an instrument for itself. In addition, not all cohabiting couples with children chose to marry during the boom.[15]

The previous discussion did not allow for additional covariates. We can estimate equation (2) below using two-stage least squares:

$$y_i = \alpha + X_i'\beta + M_i\delta + u_i \qquad (2)$$

The first stage regresses the marriage indicator on all the exogenous variables in the model and an indicator for whether or not the couple married during the boom in the end of 1989. Although equation (2) imposes the assumption that the treatment effect of marriage is constant, several authors including Imbens and Angrist (1994), Angrist (2004), and Heckman, Urzua and Vytlacil (2006), have relaxed the assumption of constant causal effects in IV estimation. In the presence of 'essential heterogeneity' where individuals sort on the gain from marriage, the IV does not identify the constant causal effect. The parents who married in the fall of 1989 are 'compliers' as discussed in Angrist, Imbens and Rubin (1996)—they married because of eligibility for the Widows Pension or in response to the marriage boom. In addition, Imbens and Angrist (1994) show that δ may be interpreted as the

[15] We could have used eligibility for the Widow's pension as an instrument for marriage. However, this instrument would not have allowed us to examine the sizable number of bandwagon marriages in our sample. In fact, in our sample there are more parents who marry and are not eligible for the Widow's pension than those that are.

local average treatment effect (LATE). Thus, LATE estimates of (2) capture the mean effect of the marriage treatment on the GPAs of children whose parents responded to the marriage boom. In fact, Heckman, Urzua and Vytlacil (2006) show that LATE is a discrete version of the marginal treatment effect (MTE) first discussed by Björklund and Moffitt (1987).

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 who qualified for the widow's pension and thus, had a financial incentive to get married, the "Treatment Sample." The second group consisted of those women born in 1945 or later who would not qualify for the widow's pension, but jumped on the marriage bandwagon, the "Bandwagon Sample." The Bandwagon Sample 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. The Treatment Sample had a well-defined financial incentive to marry. We estimate IV regressions for both groups because the incentives to marry may have differed significantly between them.

In addition to the IV-estimates, we estimate sibling fixed-effects models as a supplementary analysis. 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. Thus, they exploit another variation other than that created by the policy change. Although fixed-effects models have these advantages they are subject to limitations as well. In particular, fixed-effects estimates can be biased by measurement error. We expect measurement error to be less problematic in this case since most of our data are from population registers, as opposed to being self-reports subject to recall bias.

5. Findings

5.1 Descriptive statistics, OLS regressions and instrumental-variables estimates

We start by presenting some descriptive statistics. Table 2 focuses on the children who lived with both biological parents in 1990 (our full estimating sample) 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) 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 the timing of parents' marriage and children's GPAs for the full random sample using family background controls in four different specifications. In model A we include our indicators for gender, year of birth, and month of birth. In model B we add controls for parents' ages and whether or not the parents were teenagers when the child was born. In model C, we control for parents' income, and in model D we include the number of full and half siblings (mother's side and father's side), and indicators for birth order. 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 four 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 that this 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. 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 not do better than those whose parents remained unmarried.

Next, we estimate OLS regressions of the relationship between children's GPA and parents' marriage and marriage duration. Results are presented in Table 4. We specify four different models with marriage as a dummy variable; we then replace marriage with the duration of childhood until age 16 that a child's parents are married taking account of date of any divorce in four additional specifications.[16] In model A marriage is positively and significantly correlated with the child's GPA as is marriage duration. In model B the coefficient drops somewhat for both outcomes. Turning to model C, we see that controlling for parents' income does not change the estimated association between children's GPA and marriage and marriage duration much. In model D when we include siblings and birth order indicators, the estimated coefficient for marriage is now two-thirds of that in model A, but remains statistically significant at the one percent level. However the coefficient on marriage duration remains almost constant once these variables are added. Taken together, the results in Table 4 show that marriage is positively correlated with children's GPAs.

Results in Tables 5 through 7 limit the sample to those children whose parents were cohabiting in June 1988 when the Widow's Pension Reform was passed into law. Table 5 contains estimates of reduced-form, first-stage and Wald-IV regressions of the effect of marriage and marriage duration on children's GPAs. Panel I of Table 5 shows a positive and significant effect of marriage and marriage duration on children's GPAs in all four reduced-form models. The first stage estimate indicates that a majority of those who married in the sample did so during the marriage boom. The Wald-IV estimates are positive and significant and larger in magnitude than the reduced form estimates in Models C and D.. Panel II of Table 5 limits the analysis to the Treatment Sample, the children whose mothers who

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

would be eligible for the widow's pension if they were married prior to 1990. The estimated effect of marriage and marriage duration on GPA is much smaller in magnitude than in Panel I, and the coefficients are not statistically significant. The first stage estimates show that almost all of the couples who married did so during the marriage boom. The Wald-IV estimate is also quite small and not significant. In contrast, Panel III shows the parameter estimates for the Bandwagon Sample. Similar to Panel I, we find a positive and significant effect of marriage on children's GPA in all four models, and in the Wald-IV estimate. The Wald-IV estimates suggest that marriage and marriage duration may have a causal effect on children's GPAs for the Bandwagon Sample. However, these estimates have not been adjusted for additional covariates.

Table 6 contains the two-stage least squares estimates of the effect of marriage and marriage duration on children's GPA for the Treated Sample. We estimate the same four models as in Tables 3 and 4, but this time we use the marriage boom as an instrument for the endogeneity of marriage along with other exogenous variables in specifications A through D. In all specifications, marriage and marriage duration have no significant effect on children's GPAs. Taking the results in Tables 3 through 6 together, we find little evidence that marriage has a causal effect on children's GPAs. The marriage by parents responding to financial incentive appears to provide no advantages to children. These results stand in contrast to those in Table 7 which presents the two-stage least squares estimates for the Bandwagon Sample. Specifications A through C show a positive and significant effect of marriage and marriage duration on children's GPAs. As parent's age and income are added to the model (C), the estimated effect falls by about half. Once controls are added for siblings and birth order to specification D, the coefficients on marriage and marriage duration become quite small and are no longer significantly different from zero. Given that the Bandwagon Sample may have had little or no financial incentive to marry, the positive effect of marriage observed in models A through C likely reflect selection; as more covariates are added to the two-stage least squares estimates, the effect of marriage on children's GPAs is negligible, indicating that much of the effect of marriage on children's

educational outcomes results from selection on observables in the Bandwagon Sample. Thus, we interpret the results in Table 7 as being consistent with no causal effect of marriage on children's GPAs.

5.2 Robustness checks

A. Two-Stage Least Squares

We estimate the two-stage least squares 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 8.[17] In this table, the effect of marriage is not statistically significant in any of the Treatment Sample specifications, but the results vary depending on the subsample used from the Bandwagon Sample. We begin by splitting the sample by the sex of the child. Unlike previous estimates, we find that marriage has a small positive and significant effect for boys across all four specifications. However, for girls, the effect of marriage on GPA is not statistically significant once controls are added in specifications B through D. 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 in the Bandwagon Sample would reflect selection rather than causation. Also, if it were a causal effect one would expect to find it in the Treatment Sample as well.

Second, since the younger cohorts of children have spent a larger fraction of their childhood 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 for the older cohorts in the Bandwagon Sample which is at odds with the expectation.

Third, we eliminate children whose parents had divorced by 1991. Again we find a positive and significant impact of marriage on children's GPAs across all four specifications. This suggests

[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.

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, since firstborn children, on average, do better in school than their younger siblings and since firstborns spend a larger fraction of childhood with cohabiting parents this could be driving our results. To investigate this possibility we limit our sample to the firstborns. The results are highly similar to those shown for all children in Table 6 and 7 so the firstborns are not driving our results.

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 in the Bandwagon Sample given that a child’s family is above or below median income. However, it is interesting to note that the point estimates for those children from families with above median income in the Treatment Sample are significantly larger than for those below the median. However neither set of estimates is statistically significant.

B. Sibling Fixed-Effects

As a supplementary analysis we exploit 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.

In addition, to investigate whether younger siblings who lived their whole childhood with married parents (whether until age 16 or until 1990) gain educational advantages over their older siblings who lived longer with cohabiting parents we include a dummy variable (in Model 2) 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 9 and show no impact of the proportion lived with married parents on children's educational outcomes as measured by their GPAs at age 16. Moreover, the estimates of Model 2 and 3 indicate that siblings who lived their whole childhood with married parents (whether until age 16 or until 1990) 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.

6. 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, and 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.

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 is due to selection on observables rather than to causation. We also estimated OLS-regressions of relationship between children's GPA and parental marriage regardless of timing and marriage duration. The results suggest that parental marriage is positively correlated with higher GPAs for Swedish children and reflects the fact that the majority of parents of the children in our sample married before the end of 1989.

Next, we used instrumental-variables techniques to identify the causal effect of marriage. The marriage boom at the end of 1989 induced two types of marriages—those in which the women financially benefited by being included in the widow's pension (Treatment Sample) and those where there was no clear financial incentive to marriage (Bandwagon Sample). We find no significant effect of marriage on children's GPAs in the Treatment Sample. However, we do find a significant and positive effect of marriage in three of four specifications using the Bandwagon Sample. Once we include birth order and sibship size in these regressions, the relationship is no longer statistically significant. Taking the results together, we find little evidence that marriage has a causal effect on children's GPAs. In particular, the marriage by parents responding to financial incentive appears to provide no advantages to children. In contrast to our main results, we do find that marriage has a positive effect for boys, but not for girls, in the Bandwagon Sample. For the Treatment Sample we found no difference by child's sex. One possible interpretation of this finding, which is in line with

previous research, is that fathers are more involved in parenting when they have a son, which increases both marital quality and the GPAs of boys. The finding would then reflect selection rather than causation. In fact, research on family structure effects on children has so far paid little attention to differences by child's sex, but this might certainly be worthwhile avenue for future research.

As a supplementary analysis 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. Similar to the instrumental-variables methods, we find no statistically significant effect of marriage on children's educational outcomes.

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 child 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 less 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 for children than education 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 gender. Children born 1978-87 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	Dad's Earnings 1985	Mum's Earnings 1985	# full sibs, half sibs (mum), half sibs (dad) 1990	Mum dies 1991-2000	Dad dies 1991-2000	Marriage duration years [a]
1. Married before birth of child [N=80,666]	0.089	29.5	32.1	1236	595	1.69	0.009	0.015	15.5
	(0.971)			(590)	(366)	0.10			
						0.13			
2. Married after birth but before fall 1989 [N=23,906]	-0.095	25.8	28.4	1087	581	1.51	0.005	0.009	13.1
	(1.000)			(427)	(305)	0.16			
						0.16			
3. Married Fall 1989--mum born in 1945 or later [N=9,468]	-0.124	27.4	30.3	1064	591	1.42	0.005	0.010	9.7
	(1.029)			(375)	(300)	0.14			
						0.17			
4. Married Nov-Dec 1989 --mum born before 1945 [N=188]	-0.138	37.8	37.9	1178	609	0.61	0.016	0.037	6.8
	(0.978)			(489)	(375)	0.83			
						0.49			
5. Not married in 1989, mum born in 1945 or later [N=16,683]	-0.216	26.9	29.7	986	599	1.29	0.007	0.016	0
	(1.065)			(445)	(313)	0.26			
						0.28			
6. Not married in 1989, mum born before 1945 [N=518]	-0.147	38.3	37.4	1143	632	0.59	0.029	0.048	0
	(1.108)			(561)	(448)	1.13			
						0.61			
All who live with bio parents in 1990 [N=131,429]	0.000	28.4	31.0	1164	593	1.58	0.007	0.014	14.5 [b]
	(1.000)			(541)	(345)	0.14			
						0.16			

[a] No. of years parents were married during the child's first 16 years.
[b] Average for parents with positive marriage duration.

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

	A	B	C	D
Married Before Birth[a]	0.315**	0.225**	0.199**	0.205**
	[0.009]	[0.009]	[0.009]	[0.009]
Married After Birth[a]	0.128**	0.161**	0.141**	0.098**
Before Fall 1989	[0.010]	[0.010]	[0.010]	[0.010]
Married Fall 1989 [a]	0.090**	0.072**	0.072**	0.037**
Mom Born >1944	[0.013]	[0.013]	[0.013]	[0.013]
Married Fall 1989[a]	0.126	-0.045	-0.006	-0.006
Mom Born <1945	[0.067]	[0.068]	[0.065]	[0.063]
Mother's Age [b]		0.148**	0.116**	0.129**
		[0.007]	[0.007]	[0.007]
Father's Age [b]		0.013*	0.009	0.024**
		[0.005]	[0.005]	[0.005]
Father's Income [b]			0.903**	0.814**
			[0.049]	[0.045]
Mother's Income [b]			1.899**	1.533**
			[0.071]	[0.064]
Full Siblings				-0.018**
				[0.004]
Half-Sibs (Mother)				-0.270**
				[0.007]
Half-Sibs (Father)				-0.117**
				[0.006]
First Born				0.304**
				[0.012]
Second Born				-0.024
				[0.013]
Third Born				-0.015
				[0.013]
Fourth Born				-0.038*
				[0.015]
Sex	Yes	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes	Yes
Month of birth	Yes	Yes	Yes	Yes
Teen Mother	No	Yes	Yes	Yes
Teen Father	No	Yes	Yes	Yes
Parents' Age Interacted	No	Yes	Yes	Yes
Observations	131429	131429	131429	131429
R-squared	0.06	0.08	0.11	0.14

[a] Reference group is children whose cohabiting parents had not married before 1990. [b] 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 and marriage duration
Full Cross-Sectional Sample

	A	B	C	D	A	B	C	D
Marriage	0.254**	0.192**	0.170**	0.153**				
	[0.009]	[0.008]	[0.009]	[0.008]				
Marriage Duration					0.023**	0.017**	0.016**	0.015**
					[0.000]	[0.000]	[0.000]	[0.000]
Mother's Age[a]		0.157**	0.124**	0.139**		0.146**	0.114**	0.132**
		[0.007]	[0.007]	[0.007]		[0.007]	[0.007]	[0.007]
Father's Age[a]		0.015**	0.011	0.026**		0.013*	0.009	0.025**
		[0.005]	[0.005]	[0.005]		[0.005]	[0.005]	[0.005]
Father's Income[a]			0.918**	0.837**			0.893**	0.816**
			[0.049]	[0.046]			[0.048]	[0.045]
Mother's Income[a]			1.895**	1.553**			1.908**	1.547**
			[0.070]	[0.064]			[0.071]	[0.064]
Full Siblings				-0.012**				-0.018**
				[0.004]				[0.004]
Half-Sibs (Mother)				-0.273**				-0.264**
				[0.007]				[0.007]
Half-Sibs (Father)				-0.118**				-0.111**
				[0.006]				[0.006]
First Born				0.294**				0.290**
				[0.012]				[0.012]
Second Born				-0.017				-0.027*
				[0.013]				[0.013]
Third Born				-0.005				-0.016
				[0.013]				[0.013]
Fourth Born				-0.030				-0.037*
				[0.015]				[0.015]
Sex	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month of birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Teen Mother	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Teen Father	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Parent's Age Interacted	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Observations	131429	131429	131429	131429	131429	131429	131429	131429
R-squared	0.05	0.05	0.11	0.14	0.06	0.08	0.11	0.14

[a] Specification includes squared term. Robust standard errors in brackets. * significant at 5%; ** significant at 1%

Table 5. OLS and Wald IV Estimates of Marriage and Marriage Duration on Children's GPA For those at Risk of Marriage in June, 1988

	A	B	C	D	Wald
I. Full Sample					
Marriage	0.096**	0.084**	0.076**	0.044**	0.086**
	[0.012]	[0.012]	[0.012]	[0.012]	[0.015]
First Stage--Boom[a]					0.842**
					[0.003]
Marriage Duration	0.012**	0.010**	0.009**	0.006**	0.009**
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]
First Stage—Boom[a]					7.900**
					[0.044]
Observations	29944	29944	29944	29944	29944
II. Treatment Sample					
Marriage	0.055	0.027	0.034	-0.008	0.003
	[0.085]	[0.080]	[0.078]	[0.074]	[0.091]
First Stage--Boom[a]					0.945**
					[0.010]
Marriage Duration	0.007	0.005	0.005	0.001	0.000
	[0.011]	[0.011]	[0.011]	[0.010]	[0.013]
First Stage--Boom[a]					6.369**
					[0.201]
Observations	728	728	728	728	728
III. Bandwagon Sample					
Marriage	0.098**	0.086**	0.077**	0.046**	0.088**
	[0.012]	[0.012]	[0.012]	[0.012]	[0.015]
First Stage--Boom[a]					0.839**
					[0.003]
Marriage Duration	0.012**	0.010**	0.009**	0.006**	0.009**
	[0.001]	[0.001]	[0.001]	[0.001]	[0.002]
First Stage--Boom[a]					7.917**
					[0.044]
Observations	29216	29216	29216	29216	29216

Notes: Specifications described in Table 3. Robust standard errors in brackets. * significant at 5%; ** significant at 1%. [a]Estimates for the first stage

Table 6. Two-Stage Least Squares Estimates of Effect of Marriage and Marriage Duration on Children's GPA for Treated Sample

	A	B	C	D	A	B	C	D
Marriage	0.051	0.018	0.022	-0.020				
	[0.089]	[0.088]	[0.085]	[0.082]				
Marriage Duration					0.007	0.003	0.003	-0.003
					[0.013]	[0.013]	[0.012]	[0.012]
Mother's Age[a]		-0.014	-0.084	-0.059		-0.014	-0.083	-0.060
		[0.332]	[0.319]	[0.311]		[0.332]	[0.319]	[0.311]
Father's Age[a]		-0.138	-0.139	-0.149		-0.139	-0.140	-0.149
		[0.082]	[0.083]	[0.082]		[0.082]	[0.083]	[0.082]
Father's Income[a]			2.459**	2.495**			2.455**	2.498**
			[0.909]	[0.920]			[0.912]	[0.924]
Mother's Income[a]			-1.158	-1.326			-1.156	-1.327
			[1.503]	[1.444]			[1.504]	[1.445]
Full Siblings				0.091				0.091
				[0.113]				[0.113]
Half-Sibs (Mother)				-0.181**				-0.181**
				[0.037]				[0.037]
Half-Sibs (Father)				-0.073				-0.073
				[0.047]				[0.047]
First Born				-0.050				-0.051
				[0.134]				[0.134]
Second Born				-0.284				-0.284
				[0.176]				[0.176]
Third Born				-0.261				-0.262
				[0.288]				[0.288]
Fourth Born				-0.818				-0.819
				[0.475]				[0.475]
Sex	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month of birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Teen Mother	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Teen Father	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Parent's Age Interacted	No	Yes	Yes	Yes	No	Yes	Yes	Yes
First Stage--Boom[b]	0.945**	0.944**	0.943**	0.940**	6.369**	6.466**	6.450**	6.416**
	[0.010]	[0.010]	[0.011]	[0.011]	[0.201]	[0.182]	[0.184]	[0.187]
Observations	728	728	728	728	728	728	728	728
R-squared	0.10	0.12	0.18	0.23	0.10	0.12	0.18	0.23

[a] Specification includes squared term. Robust standard errors in brackets. * significant at 5%; ** significant at 1%.

[b] Estimates for the first stage report the effect of the Nov. – Dec. 1989 boom on marriage and marriage duration.

Table 7. Two-Stage Least Squares Estimates of Effect of Marriage and Marriage Duration on Children's GPA for Bandwagon Sample

	A	B	C	D	A	B	C	D
Marriage	0.093**	0.053**	0.052**	0.021				
	[0.015]	[0.015]	[0.015]	[0.015]				
Marriage Duration					0.010**	0.006**	0.005**	0.002
					[0.002]	[0.002]	[0.002]	[0.002]
Mother's Age [a]		0.153**	0.126**	0.109**		0.153**	0.125**	0.109**
		[0.014]	[0.014]	[0.014]		[0.014]	[0.014]	[0.014]
Father's Age [a]		0.026*	0.022*	0.031**		0.026**	0.022*	0.031**
		[0.011]	[0.011]	[0.011]		[0.014]	[0.011]	[0.011]
Father's Income[a]			1.355**	1.181**			1.353**	1.180**
			[0.083]	[0.081]			[0.084]	[0.081]
Mother's Income[a]			2.622**	1.866**			2.622**	1.865**
			[0.232]	[0.234]			[0.232]	[0.234]
Full Siblings				-0.005				-0.005
				[0.008]				[0.008]
Half-Sibs (Mother)				-0.264**				-0.264**
				[0.012]				[0.012]
Half-Sibs (Father)				-0.102**				-0.102*
				[0.013]				[0.013]
First Born				0.231**				0.230**
				[0.028]				[0.028]
Second Born				-0.083**				-0.083**
				[0.030]				[0.030]
Third Born				-0.055				-0.055
				[0.032]				[0.032]
Fourth Born				-0.148**				-0.149
				[0.043]				[0.043]
Sex	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Month	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Teen Mother	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Teen Father	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Parent's Age Interacted	No	Yes	Yes	Yes	No	Yes	Yes	Yes
First Stage--Boom[b]	0.839**	0.842**	0.840**	0.835**	7.917**	7.893**	7.886**	7.784**
	[0.003]	[0.003]	[0.003]	[0.003]	[0.044]	[0.045]	[0.045]	[0.046]
Observations	29215	29215	29215	29215	29215	29215	29215	29215
R-squared	0.05	0.08	0.10	0.14	0.05	0.08	0.10	0.14

[a] Specification includes squared term. Robust standard errors in brackets. * significant at 5%; ** significant at 1%

[b] Estimates for the first stage report the effect of the Nov.–Dec. 1989 boom on marriage and marriage duration.

Table 8. Two-Stage Least Squares Robustness Checks of Effect of Marriage Children's GPA

	Treatment Sample				Bandwagon Sample			
Girls Only	**A**	**B**	**C**	**D**	**A**	**B**	**C**	**D**
Marriage	0.222	0.188	0.204	0.118	0.067**	0.027	0.028	-0.005
	[0.120]	[0.120]	[0.115]	[0.116]	[0.020]	[0.020]	[0.020]	[0.020]
First Stage—Boom[a]	0.952**	0.955**	0.956**	0.948**	0.846**	0.850**	0.849**	0.845**
	[0.027]	[0.027]	[0.027]	[0.027]	[0.005]	[0.005]	[0.005]	[0.005]
Observations	349	349	349	349	14434	14434	14434	14434
R-squared	0.10	0.11	0.18	0.25	0.01	0.04	0.07	0.11
Boys Only								
Marriage	-0.102	-0.141	-0.152	-0.180	0.118**	0.079**	0.074**	0.044*
	[0.131]	[0.131]	[0.128]	[0.122]	[0.022]	[0.022]	[0.022]	[0.022]
First Stage--Boom[a]	0.950**	0.946**	0.943**	0.941**	0.840**	0.843**	0.841**	0.835**
	[0.023]	[0.023]	[0.023]	[0.023]	[0.005]	[0.005]	[0.005]	[0.005]
Observations	379	379	379	379	14781	14781	14781	14781
R-squared	0.07	0.11	0.16	0.21	0.01	0.04	0.07	0.1
Younger: Born 1983-87								
Marriage	0.239	0.276	0.240	0.282	0.063**	0.014	0.015	-0.007
	[0.205]	[0.194]	[0.180]	[0.190]	[0.019]	[0.019]	[0.018]	[0.018]
First Stage--Boom[a]	0.949**	0.950**	0.947**	0.931**	0.816**	0.820**	0.819**	0.815**
	[0.049]	[0.050]	[0.050]	[0.052]	[0.005]	[0.005]	[0.005]	[0.005]
Observations	160	160	160	160	19458	19458	19458	19458
R-squared	0.22	0.25	0.30	0.365	0.049	0.079	0.109	0.15
Older: Born 1977-82								
Marriage	-0.010	-0.052	-0.030	-0.066	0.139**	0.117**	0.110**	0.071*
	[0.098]	[0.10]	[0.098]	[0.091]	[0.025]	[0.025]	[0.025]	[0.025]
First Stage--Boom[a]	0.954**	0.954**	0.954**	0.954**	0.892**	0.894**	0.890**	0.883**
	[0.018]	[0.018]	[0.018]	[0.018]	[0.005]	[0.005]	[0.005]	[0.005]
Observations	568	568	568	568	9757	9757	9757	9757
R-squared	0.09	0.11	0.17	0.22	0.05	0.07	0.10	0.12
No Divorce								
Marriage	0.038	0.006	0.013	-0.023	0.136**	0.090**	0.088**	0.055**
	[0.091]	[0.091]	[0.088]	[0.084]	[0.016]	[0.015]	[0.015]	[0.015]
First Stage--Boom[a]	0.949**	0.947**	0.945**	0.942**	0.856**	0.859**	0.857**	0.851**
	[0.018]	[0.018]	[0.018]	[0.018]	[0.004]	[0.004]	[0.004]	[0.004]
Observations	703	703	703	703	26442	26442	26442	26442
R-squared	0.1	0.13	0.19	0.23	0.05	0.08	0.11	0.14
First Borns								
Marriage	0.030	0.010	0.009	0.000	0.108**	0.065**	0.057**	0.002
	[0.125]	[0.122]	[0.117]	[0.109]	[0.021]	[0.021]	[0.020]	[0.020]
First Stage--Boom[a]	0.966**	0.962**	0.958**	0.958**	0.828**	0.829**	0.827**	0.820**
	[0.012]	[0.013]	[0.014]	[0.014]	[0.003]	[0.003]	[0.004]	[0.004]
Observations	415	415	415	415	16972	16972	16972	16972
R-squared	0.09	0.13	0.20	0.27	0.05	0.09	0.11	0.14

Table 8. (Continued)

Less than Median Income								
Marriage	0.003	0.000	0.005	-0.043	0.069**	0.042*	0.045*	0.015
	[0.127]	[0.128]	[0.129]	[0.126]	[0.020]	[0.020]	[0.020]	[0.020]
First Stage--Booma	0.966**	0.963**	0.959**	0.958**	0.845**	0.849**	0.845**	0.840**
	[0.023]	[0.023]	[0.023]	[0.023]	[0.005]	[0.005]	[0.005]	[0.005]
Observations	347	347	347	347	17925	17925	17925	17925
R-squared	0.14	0.15	0.20	0.25	0.05	0.06	0.07	0.10
Greater than Median Income								
Marriage	0.072	0.042	0.101	0.087	0.078**	0.049*	0.073**	0.041
	[0.112]	[0.112]	[0.105]	[0.105]	[0.023]	[0.023]	[0.023]	[0.022]
First Stage--Boom[a]	0.931**	0.934**	0.931**	0.924**	0.839**	0.842**	0.841**	0.837**
	[0.026]	[0.026]	[0.026]	[0.026]	[0.006]	[0.006]	[0.006]	[0.006]
Observations	381	381	381	381	11290	11290	11290	11290
R-squared	0.15	0.19	0.26	0.30	0.05	0.08	0.10	0.14

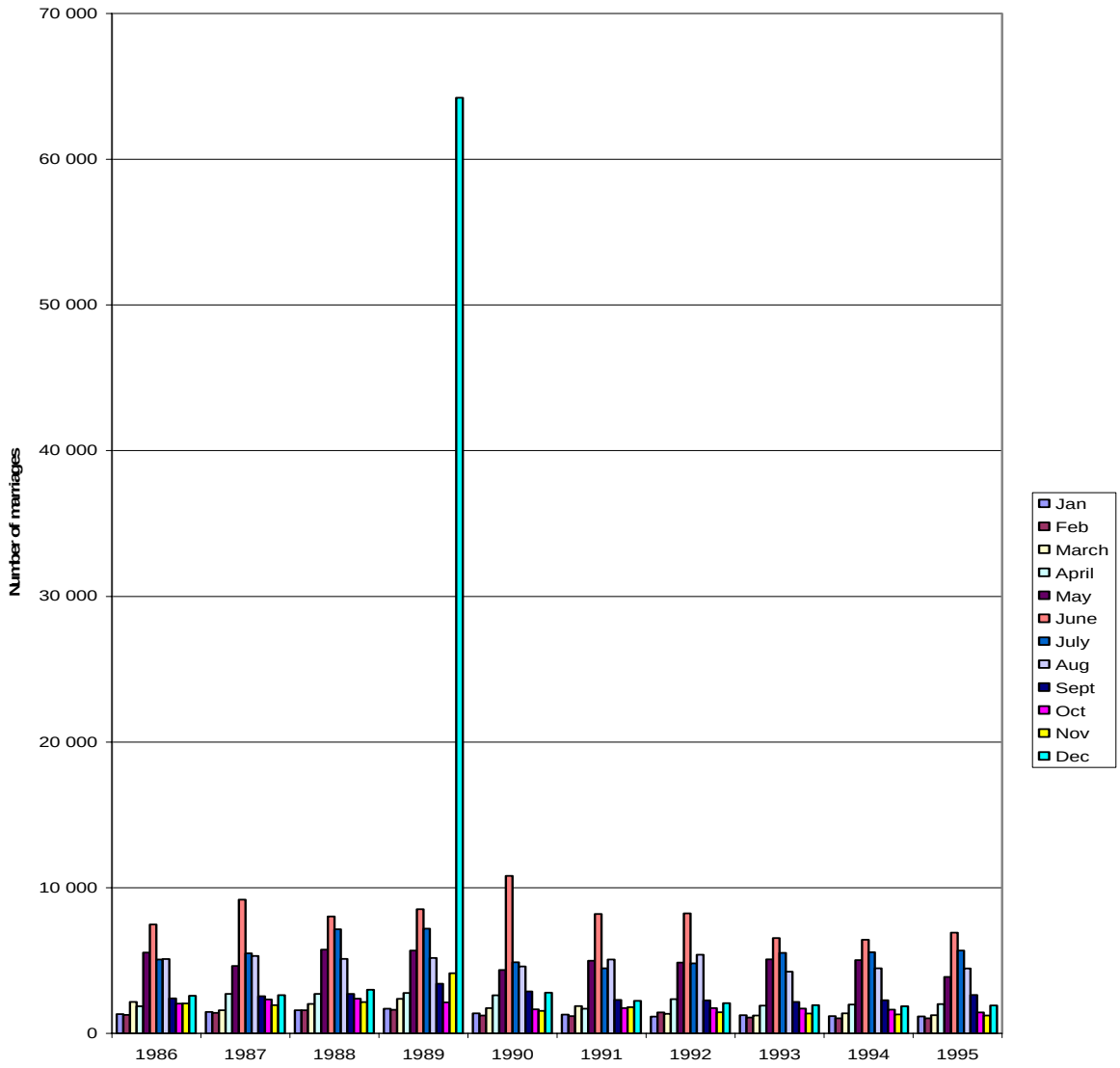
Notes: Specifications described in Table 7. Robust standard errors in brackets. * significant at 5%; ** significant at 1%. [a]Estimates for the first stage report the effect of the Nov–Dec 1989 boom and the exogenous variables on marriage.

Table 9. 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]	-0.019	0.007	-0.058
	[0.033]	[0.038]	[0.038]
Lived with married parents whole childhood		-0.014	0.006
		[0.010]	[0.010]
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	0.092	0.092	0.099

[a] The proportion lived with married parents has been computed as the proportion of years up to age 16 parents were married. Robust standard errors in brackets.* p < .01.

Figure 1. Marriages in Sweden per month 1986-1995



Source: Statistics Sweden