TI 2015-073/V
Tinbergen Institute Discussion Paper

The Effect of Sibling's Gender on Earnings, Education and Family Formation

Noemi Peter[1]
Petter Lundborg[2]
Dinand Webbink[3]

[1] *University of Bern, Switzerland;*
[2] *Lund University, Sweden;*
[3] *Erasmus School of Economics, Erasmus University Rotterdam, the Netherlands.*

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Noemi Peter - Petter Lundborg - Dinand Webbink *

This version: May 2015

Abstract

We examine how the gender of a sibling affects earnings, education and family formation. Identification is complicated by parental preferences: if parents prefer certain sex compositions over others, children's gender affects not only the outcomes of other children but also the very existence of potential additional children. We address this problem by looking at dizygotic twins. In these cases, the two children are born at the same time, so parents cannot make decisions about one twin based on the gender of the other twin.

We find that the gender of the sibling influences both men and women, but in a different way. Men with brothers earn more and are more likely to get married and have children than men with sisters. Women with sisters obtain lower education and give birth earlier than women with brothers. Our analysis shows that the family size channel cannot explain the findings. Instead, the most likely explanation is that siblings affect each other via various social mechanisms.

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*Noemi Peter: University of Bern; Petter Lundborg: Lund University and Institute for the Study of Labor (IZA); Dinand Webbink: Erasmus University Rotterdam, Tinbergen Institute and Institute for the Study of Labor (IZA). Corresponding author: Noemi Peter (noemi.peter@vwi.unibe.ch).
We are grateful to Hessel Oosterbeek, Erik Plug, Paul J. Devereux, Zsombor Cseres-Gergely and to seminar participants in Amsterdam, at the 2012 Summer Workshop of the Institute of Economics, Hungarian Academy of Sciences and at the ESPE Annual Conference 2014 for their comments and suggestions.

1 Introduction

Family environment has long been considered to be a determinant of children's long term outcomes (Haveman and Wolfe, 1995). The influence of close family members is of particular interest since they play a significant role in most people's lives. This applies among others to siblings. According to time use surveys, children spend more out-of-school time with their siblings than with anyone else (McHale and Crouter, 1996). Research on sibling relationships shows that siblings provide reference points to each other, both in childhood and in adulthood (Adams, 1999; Keim *et al.*, 2009). While these observations suggest that siblings have a big potential to have an impact, little is known about the effect of their specific characteristics. We try to shed more light on this by studying the role of one particular factor: we examine how the gender of a sibling affects individual's education, earnings and family formation.

We focus on the gender of the sibling because a large literature shows that gender differences are prevalent. Gender differences can be observed in such important areas as family formation, labor market, personality traits and preferences. For example, women marry and have children at a younger age, are less likely to get promotions and earn less than men (Copen *et al.*, 2012; Baizán *et al.*, 2003; Bertrand, 2010). As compared to men, women are also less competitive, more risk averse, less likely to negotiate and more socially minded (Croson and Gneezy, 2009; Bertrand, 2010). In short, males and females are different in many regards, which implies that brothers and sisters are different too. Thus, the gender of the sibling might be one of the factors that matters, especially since the outcome variables that we examine relate to labor market success and family formation.

The first studies on the role of siblings' gender were done by psychologists who wanted to know how it affects child development.[1] The focus has been on the impact of older siblings' gender because older siblings are typically more dominant in sibling relationships (Tucker *et al.*, 2010). The results of this literature indicate spillover effects, as they show that children with (older) sisters are more feminine/less masculine than children with (older) brothers. Economists examined whether there is an impact on educational outcomes. None of the studies found an impact on white men, while results on white women were controversial.[2] Butcher and Case (1994) found that women with any sisters attained lower education than women with only brothers. Their preferred explanation was that the presence of a second daughter changes the reference group for the first: the girls are grouped together and get lower educational standards than boys. However, their results on women could not be replicated by Kaestner (1997) and Hauser and Kuo (1998).

These mixed findings make it difficult to draw conclusions from the previous literature. Further, the above studies controlled for family size, treating it as a fixed variable. However, this assumption was questioned in the seminal paper of Angrist and Evans (1998), which shows that family size

[1]The pioneers were Koch (1955) and Brim (1958). See Rust *et al.* (2000) for a review and further analysis.

[2]We focus on results on whites from Western countries. For results on other populations, see e.g. Parish and Willis (1993), Morduch (2000) and Chen *et al.* (2013).

depends on children's sex composition. This is because parents may prefer certain sex compositions over others, so the gender of the children affects subsequent fertility decisions. This has two important consequences for empirical identification. First, family size is an outcome variable and therefore controlling for it leads to "bad control bias" (Angrist and Pischke, 2009). Second, a selection bias can arise, irrespective of whether family size is controlled for or not. This is because parental preferences imply that the gender of an earlier born child influences the selection of a potential later born child into the sample. As we show in Section 2, this can lead to biased estimates since parental preferences may affect children's outcomes as well.

We apply an empirical approach that circumvents these problems. Using a sample of dizygotic twins from Sweden, we compare men (women) with co-twin brothers to men (women) with co-twin sisters.[3] We examine whether the gender of the co-twin has an impact on their education, earnings and family formation. Our identification strategy exploits the fact that twins are born at the same time. This implies that there is no selection bias in this setup: parents cannot make decisions about one twin based on the gender of the other twin, given that the twins are born only minutes apart.[4] Family size can still be affected, because parents can choose the number of additional children. We avoid the bad control problem because we do not control for family size but treat it as a potential channel.

It is important that we can distinguish the twins by zygosity. While the sex of dizygotic co-twins is random, the sex of monozygotic twins is always the same as the sex of their co-twin.[5] Thus, if we did not differentiate by zygosity, the coefficient of the co-twin's gender could pick up potential differences between dizygotic and monozygotic twins. We avoid this problem by restricting the sample to dizygotic twins.

While economists have long used twin samples, our approach is different from what is typical in the literature. Most researchers apply twin fixed effects, as they want to exploit that monozygotic twins are very similar to each other. We point out that twins have another advantage. Since they are born at the same time, we can use them to avoid the above-mentioned selection bias. Thus, we exploit twins in a non-traditional way. Related to our paper is a recent study by Gielen *et al.* (2015), who also compare same-sex and opposite-sex twins to each other.[6] However, they cannot differentiate the twins by zygosity and therefore they cannot analyze dizygotic twins separately. [7]

We note that a sibling's gender can have an impact in various ways. Effects can arise via childhood

[3]We follow the previous literature and analyze men and women separately.

[4]Selective abortion and IVF techniques were not available in the time period that we examine.

[5]See Section 2 for more details on this issue.

[6]Chen *et al.* (2013) is also interested in the impact of siblings's gender, although in a different context (they look at a traditionally pro-male Asian society). After completing the first draft of our paper, we found out that an earlier version of their paper used a twin design similar to ours (Chen *et al.*, 2008). However, they could not differentiate the twins by zygosity. In the later version of their paper (Chen *et al.*, 2013) they do not use the twin design any more but apply a different strategy.

[7]In addition, their starting point is also quite different. Studies on animals conclusively demonstrate that in some species, testosterone can transfer between littermates in utero (Ryan and Vandenbergh, 2002). It has been hypothesized that this mechanism may also apply to human twins (Tapp *et al.*, 2011). Based on this hypothesis, Gielen *et al.* (2015) interpret the estimates as the impact of prenatal testosterone. We will discuss this issue in detail in Section 5.3.

spillovers, reference point considerations or specific changes in parental behavior - we will refer to all of these processes collectively as "social mechanisms" (see also Section 5.1). In addition, there could be an impact via the above-mentioned family size channel as well. Thus, a particularly interesting question is whether the estimates are driven by family size or there are other effects over and above that. We address this issue in detail in Section 5.2.

We find that the gender of the co-twin influences both men and women, but in a different way. Men with brothers earn more and are more likely to get married and have children. In case of women, there is an impact on education and age at first birth: women with sisters obtain lower education and give birth earlier. Our analysis reveals that the results can not be explained by the family size channel. Instead, the most likely explanation is that social mechanisms are at work. These mechanisms can operate in case of singletons as well, although their effect might be weaker, especially as far as the influence of younger siblings is concerned.

We contribute to the literature in several ways. We look at a wider range of outcomes than previous studies. We also point out the methodological challenges of empirical identification and offer a solution. As a result, we provide new insights into the impact of siblings' gender. In addition, our paper has important implications for research on other family factors. In particular, children's sex composition is often used as an instrument to identify the impact of family size. We point out several factors that can question the validity of this instrument and therefore suggest to use alternative approaches instead (see Section 5.2).

The paper unfolds as follows. Section 2 explains our empirical approach in more detail and Section 3 describes the data. We present the results in Section 4 and discuss potential explanations in Section 5. Finally, Section 6 concludes.

2 Empirical strategy

Several studies show that the gender of current children influences parity progression, that is, the probability of having additional children (see Angrist and Evans, 1998; Black *et al.*, 2005; Conley and Glauber, 2006; Åslund and Grönqvist, 2007; Dahl and Moretti, 2008; Cools and Kaldager Hart, 2015). In developed countries, the most common finding is that parents of two boys or two girls are more likely to have additional children than parents of a boy and a girl. Clearly, this indicates that there are parental preferences for children's sex composition. At the same time, the exact nature of these preferences is less obvious.

For example, the above phenomenon might arise because some families have a preference for gender mix. However, another possibility is that nobody prefers a gender mix, but some families have a preference for boys while others have a preference for girls. According to this explanation, some parents with same-sex kids will proceed to a next child because their first two children are not from the preferred sex. Families with a boy and a girl will not proceed, because they have a child from the preferred gender for sure. Note that for this explanation to be true, it is not necessary to observe differential progression after the first child. If the number of boy-preferring families equals the number of girl-preferring families, the same number of families will proceed to a second child after a first-born boy than after a first-born girl.

As this example demonstrates, parental preferences can be of various types and different preferences may cancel each other out in aggregate figures. Therefore parental preferences can always be present, even when there is no difference in parity progression ratios.[8] This means that we should always keep in mind that comparing individuals with an older brother to individuals with an older sister can lead to biased results. This is because families that proceed to a next child after having a boy may have different preferences than families that proceed after a girl. If people with different preferences raise their children differently, the estimates will be biased.

This intuition corresponds to a selection bias problem, as in Heckman (1979). To show this in detail we need to consider a set of equations. We will analyze men and women separately so parameters can be different by gender. Nonetheless, for the ease of exposition we omit subscripts for own gender. We start with the equation for the latent variable Y_i^*:

$$Y_i^* = \alpha + \beta^o G_i^o + \gamma X_i + U_i \tag{1}$$

where Y_i^* is the outcome of individual i, G_i^o is the gender of the older sibling and X_i denotes observable exogenous covariates. U_i consists of other relevant variables with $E[U_i] = 0$. Parental preferences are denoted by P_i and they are included in U_i. They are relevant because parents with different preferences may raise their children differently. They are unobservable, so they have to be in U_i instead of X_i. Nonetheless, this does not bias in the estimation of equation (1) because G_i^o and P_i are independent, due to the random assignment of gender.[9]

The problem is that we cannot estimate equation (1) because Y_i^* is a latent variable. Instead of observing Y_i^*, we observe

$$Y_i = \begin{cases} Y_i^* & if \quad S_i = 1 \\ missing & if \quad S_i = 0 \end{cases} \tag{2}$$

That is, we observe outcomes for a selected sample: only for those people who were actually born

[8]Parity progression ratios provide one-sided information: they can be used to show that some kind of parental preferences are present, but they cannot be used to prove the absence of all kind of preferences. It is easy to create examples where there is no differential progression at all but parental preferences are still present. For example, suppose that the same number of families prefer boys and girls. In addition, suppose that these families want to have *at most* two kids. It is clear that in this case, there will be no differential progression, neither at parity 1, nor at parity 2, even though parental preferences are present.

[9]Medical technologies for sex selection were unavailable in the time period that we examine. As far as natural sex selection is concerned, Wilcox *et al.* (1995) found that the timing of intercourse in relation to ovulation has no significant effect. Similarly, Gray *et al.* (1998) found no evidence for the hypothesis that maternal hormones influence sex selection. All in all, the established view among biologists is that sex is essentially random (Reece *et al.*, 2010, p. 290).

($S_i = 1$). This selection depends on both parental preferences (P_i) and the gender of the older child:

$$S_i = f(P_i, G_i^o) \tag{3}$$

If we try to estimate the parameters using the selected sample, we need to consider the expected value of Y_i conditional on $S_i = 1$:

$$E[Y_i|S_i = 1, G_i^o, X_i] = \alpha + \beta^o G_i^o + \gamma X_i + E[U_i|S_i = 1, G_i^o, X_i] = \alpha + \beta^o G_i^o + \gamma X_i + E[U_i|f(P_i, G_i^o) = 1, G_i^o, X_i] \tag{4}$$

The last term makes the selection bias visible. We condition on both $S_i = 1$ and G_i^o. Since selection depends on both P_i and G_i^o, this implicitly defines P_i: only certain parents will proceed to a next child after they have a girl (boy). Recall that U_i includes P_i because parental preferences may affect how children are raised. This implies that $E[U_i|f(P_i, G_i^o) = 1, G_i^o = 1, X_i] \neq E[U_i|f(P_i, G_i^o) = 1, G_i^o = 0, X_i]$, so the estimates suffer from selection bias.

We circumvent this problem by using a sample of dizygotic (DZ) twins. Since twins are born at the same time, parents cannot make decisions about one twin based on the sex of the other twin. This means that there is no selection bias in this setup. Thus, we can estimate the following equation, for men and women separately:

$$Y_i = \alpha + \beta^{co} G_i^{co} + \gamma X_i + U_i \tag{5}$$

where Y_i denotes the outcome of twin i, G_i^{co}denotes the gender of the co-twin and X_i denotes exogenous covariates (birth year fixed effects). We will estimate standard errors such that we allow clustering at the level of the family of origin.

We do not control for family size because that could lead to bad control bias. In principle this means that the estimates have to be interpreted as a total impact that includes the family size channel. Nonetheless, our setting has two important features that make the family size channel less likely to act. First, twins are not necessarily the first children in the family. Therefore their sex might not be so influential, given that family size depends on the sex composition of all existing children. Second, twins already increase family size substantially, so we can expect their sex to have little additional impact. Indeed, in Section 5.2 we analyze family size separately as an outcome and find that the gender of the co-twin has no significant effect on it in our sample. This means that our estimates are not driven by the family size channel. Note that with a different identification strategy this might be more difficult to assess. For example, suppose that one were to analyze whether the gender of younger children affects the outcomes of older children. This empirical approach would also avoid the selection bias, since the gender of younger children cannot affect the existence of older children retroactively. However, in that case it could be more ambiguous whether the estimates reflect anything more than the family size channel.

We estimate equation (5) under the assumption that the gender of the co-twin is random. This is why we focus on dizygotics only. Just like two singleton siblings, DZ twins result from the separate fertilization of two different ova by two different sperms (Phillips, 1993). Their sex is determined the same way as the sex of singletons: the offspring will be male (female) if the sperm bears a Y (X) chromosome. Since each ovum is fertilized by a different sperm, the sex of a DZ co-twin is just as random as the sex of a singleton sibling. Thus, the probabilities of dizygotic males and females are thought to be approximately equal, independently of the sex of the other twin.[10] Indeed, the share of same-sex co-twins is fifty percent in our sample of dizygotic twins, for both men and women.

The case of monozygotic (MZ) twins is different because they arise when one zygote splits into two genetically identical units. This implies that MZ twins are all same-sex. Thus, if we did not differentiate by zygosity, differences between opposite-sex and same-sex twins could reflect not only the impact of the sibling's gender, but also the impact of having a genetically identical sibling or other potential differences between monozygotic and dizygotic twins. We circumvent this problem by restricting the main estimation sample to DZ twins.

3 Data

Our data comes from the SALT project (Screening Across the Lifespan Twin Study) of the Swedish Twin Registry (STR) (see Lichtenstein *et al.*, 2002). The aim of the SALT was to survey all Swedish twins born in 1926-1958, irrespective of the sex composition of the twin pair.[11] For these cohorts, the STR had records of every twin birth from the national birth register.[12] They contacted all available twins for a phone interview. The process started with a pilot in 1996-1997 and then full scale data collection took place in 1998-2002. The oldest cohorts were surveyed first, followed by younger cohorts.[13] The interviewers tried to get as many responses as possible; e.g. people were called back

[10]This view is so widely accepted that researchers typically calculate the number of dizygotics within the twin population as twice the number of opposite-sex twins (Benirschke *et al.*, 2012). This is the so-called Weinberg's differential rule (WDR). Although James (1979) raised questions about this method, several studies found that it is supported empirically (Vlietinck *et al.*, 1988; Husby *et al.*, 1991; Fellman and Eriksson, 2006). Thus, Elston *et al.* (2002) conclude that this is a reasonable approximation and actual deviations are small.

[11]The SALT sample contains twins from cohorts 1886-1925 as well. However, these cohorts were screened only partially; many twins were left out because they did not take part in previous surveys. The early surveys targeted only certain sex compositions (typically same-sex pairs). In turn, the SALT sample contains disproportionately few opposite-sex twins from cohorts 1886-1925. We do not use these early cohorts in our research, only the complete cohorts of 1926-1958.

[12]This is remarkable as many twin registries have to enroll twins in alternative ways instead. For example, at the start of this project we obtained data from the Australian Twin Registry (ATR). However, it turned out that the ATR used specific advertisements for recruitment so that the sample composition of same-sex and opposite-sex twins was different. After discovering this problem, we abandoned the Australian sample. Nonetheless, it is reassuring that the preliminary results on that sample were quite similar to the results obtained here.

[13]Of course "younger" is meant in relative terms; participants are 41-74 years old. The STR focused on these ages because they wanted to screen for some diseases. A population above 40 is actually quite fortunate for our purposes, since fertility is typically completed by that age. It could have been a disadvantage for the analysis of income, but as we explain soon, we collected information on income from earlier years as well. Thus, we can look at income in the working ages.

at least five times if they were unavailable.

In short, the SALT was a systematic screening of all available twins. Unfortunately, we do not have access to the universe of twins, so we cannot compare respondents to the rest of the twins. However, we know that the response rate was remarkably high: 74 percent. Thus, the STR managed to survey a large sample of the underlying population. Recall that the use of a survey is essential for our purposes: otherwise, we could not differentiate between MZ and DZ twins.

The SALT determines zygosity based on questions about intrapair similarity in childhood. This classification was validated in the pilot: 13 DNA-markers were analyzed in blood samples. Zygosity assignment proved correct in 99% of the cases (see Lichtenstein *et al.*, 2002).

The survey provided information on marital status, children and siblings as well. In case of marriage, we use the dummy "ever married" that takes one if the individual has ever been married or cohabited and zero otherwise. The questions on children and siblings refer to biological children/siblings (other than the co-twin) who are still alive. We look at the number of children to analyze fertility.[14] To analyze timing, we calculate age at first birth from the birth year of children. In addition, we also acquired information on age at menarche (first menstruation) from the survey, to address whether fertility patterns are led by a biological channel.

For the analysis of education and income we obtained registry data. Less than 0.1% of the observations were lost due to the merger with registers. The source of the education data is the education register (utbildningsregistret, UREG). Years of schooling is calculated from obtained degree. The income data comes from the national register on taxable income (IoT, Inkomst- och taxeringsregistret). This is *not* based on individuals' self-reports but on information collected from the employers and other agencies providing taxable benefits. We use records from years 1971-2007 (all converted to 2010 prices).[15] The income variable measures total taxable labor income in the given year: it includes wages, income from self-employment, pensions, sickness benefits and other taxable benefits.[16] From any given year, we use the available income records of those who are aged between 25 and 64 in that year. The main source of income in this age range is typically the labor market.

Since we have income records from many years, we have several observations per person. We

[14] We imputed data for childless women. This is because women who never gave birth got 'missing' assigned for the children variables, even though their number of children should be zero. We do not have access to the variable on childbirth so we do not know which observations are truly missing and which ones should be recorded as zero. However, the questions on children follow directly the ones on siblings, so we inferred who could have answered them. In particular, a woman is assumed to have zero children if she answered the question on siblings but not the question on children. Since data on men is complete, we can check the accuracy of our strategy. The results are reassuring: 99.7% of men who answered the question on siblings also answered the question on children.

[15] In fact, the income data starts in 1968. However, after inspecting the data, we had concerns about sample selection in the first 3 years. In 1968-1970, 16.24% of the observations are missing, whereas data is almost complete after that period. Therefore we decided to use only the 1971-2007 data in the analysis.

[16] Many benefits became taxable in 1974 and therefore they are included in the income measure since then. This means that our variable was somewhat more restrictive in the pre-1974 years. Nonetheless, recent work by Björklund *et al.* (2009) suggests that this is not a major concern, as benefits constitute a very small share of total earnings. In any case, our results remain qualitatively the same if we restrict income data to 1974-2007.

take their average to get a permanent income measure for each individual. We use the log of this measure, ln(permanent income) as outcome variable. Note that people may have zero income in one year or another; however, almost nobody has zero income in all years. Therefore we lose hardly any observations when we take the log of permanent income.[17] On the other hand, we averaged over different number of observations in different cohorts. This implies potential heteroskedasticity, even though cohort fixed effects are included in the regressions. We will use robust standard errors to take this into account. In addition, later we will also show results from a pooled regression on ln(income). In this case, we pool the different years instead of taking the average, so more observations fall out due to zero income.[18] We run a pooled regression on the extensive margin as well. That is, we analyze whether there is an impact on the probability of having positive income.

Descriptive statistics are shown in Table 1. The first column shows the characteristics of DZ twins, our main estimation sample. The second and third column breaks down the sample by the gender of the co-twin. OS DZ denotes dizygotic twins whose co-twin is of the opposite sex, while SS DZ denotes dizygotic twins whose co-twin is of the same sex. Recall that the SALT surveyed individuals, not twin pairs, so inclusion in the sample is not conditional on the participation of the co-twin. In line with the previous discussion, we can see that the share of same-sex co-twins is approximately fifty percent, among both men and women. [19]

[17] We lose only 0.08% of women. We do not lose any men.

[18] This share is still quite low, since the income measure includes all kinds of benefits. It is 4.46% in case of women and 0.76% in case of men.

[19] There are somewhat more women in the sample, as they are more likely to respond to surveys (see Singer *et al.*, 2000). Note that we analyze men and women separately, so this will simply mean that the analysis of the latter will be based on more observations.

Table 1: Descriptive statistics

	All DZ twins	OS DZ twins	SS DZ twins
Panel A: Men			
Same-sex co-twin	0.50	0	1
Ever married	0.88	0.87	0.89
Any kids	0.82	0.82	0.83
Number of kids	1.89	1.86	1.91
Age at first birth	27.87	27.88	27.85
Number of siblings	2.00	2.01	1.99
Years of schooling	11.11	11.11	11.11
Ln (permanent income)	12.44	12.43	12.44
Number of observations	13664	6890	6774
Panel B: Women			
Same-sex co-twin	0.50	0	1
Ever married	0.90	0.90	0.90
Any kids	0.87	0.88	0.87
Number of kids	1.99	1.98	2.00
Age at first birth	24.94	25.07	24.82
Age at menarche	13.45	13.42	13.47
Number of siblings	2.04	2.05	2.03
Years of schooling	11.06	11.12	10.99
Ln (permanent income)	11.92	11.93	11.91
Number of observations	14950	7522	7428

Note: Marriage includes cohabitation.

4 Results

In this section we show results on the total impact of the co-twin's gender. These estimates can be the result of several mechanisms; we will discuss them after the main findings are presented.

The main results are reported in Table 2. For men, having a brother instead of a sister has no impact on education. This is consistent with the findings of previous literature. However, there is still an impact on income, suggesting that non-cognitive factors play a role. The gender of the co-twin also affects the probability of family formation: those with brothers are more likely to get married and have children. As a result, they have more children.[20]

The results on women show a different pattern. There is an impact on education: having a sister instead of a brother decreases years of education. This is in line with the findings of Butcher and Case (1994). The coefficient on income is also negative, but it is not statistically significant. There is no impact on the probability of family formation, but there is one on timing: age at first birth is lower for

[20]There is no significant difference in the intensive margin, that is, in the number of children for those who have at least one child. However, this estimate would be difficult to interpret as this sample is conditional on a variable that is significantly affected (having any children). For the same reason, we do not interpret the estimates on age at first birth.

those with sisters. Thus, women have children earlier and obtain lower education if they have a sister instead of a brother.

Table 2: Estimates of the effect of a same-sex co-twin on education, income and family formation, on the sample of DZ twins

	Education	Ln(permanent income)	Ever married	Any kids	Number of kids	Age at first birth
Panel A: Men						
SS co-twin	-0.002	0.013*	0.013**	0.013**	0.044**	-0.029
	(0.048)	(0.007)	(0.006)	(0.007)	(0.022)	(0.101)
N	13664	13664	13560	13434	13430	10979
Panel B: Women						
SS co-twin	-0.098**	-0.011	-0.001	-0.005	0.013	-0.211**
	(0.043)	(0.010)	(0.005)	(0.006)	(0.020)	(0.087)
N	14950	14938	14873	14756	14756	12846

Note: The standard errors shown in parentheses are robust and allow clustering by the family of origin. All regressions include a constant and cohort fixed effects. Education is measured as years of schooling. Permanent income is average income in years 1971-2007 (if subject is aged 25-64 in the given year). The variable "ever married" takes one if the individual has ever been married or cohabited and zero otherwise. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

To see whether these results are robust, we check whether they are sensitive to alternative specifications or definitions (see Table 3). For education, we analyze whether the gender of the co-twin affects the probability of having more than primary education (that is, more than 9 years of schooling). In case of income, we run pooled regressions on ln(income) and on the probability of having positive income. In these regressions we have several observations for every individual. Although this increases sample size, the standard errors do not shrink because of the clustering. To avoid the overrepresentation of those who could be observed for more years than others, we weight in these pooled regressions with the inverse of the number of observations. In this way we ensure that every individual is represented equally.[21] The pooled regressions also include year fixed effects.

In case of the family formation variables, we change the definition of the outcome variables. So far we examined whether the respondent has ever been married, but now we examine the probability of being married at the time of the interview. We redefine the variables "any kids" and "number of kids" such that they count only if the respondent lists the birth date of the kids as well. As far as age at first birth is concerned, we check whether the results are led by a few outliers whose first kid was born at an unusually early or late age. That is, we drop those whose age at first birth was less than 14 or more than 40. We present all the robustness checks in Table 3.

[21]Nonetheless, the results are qualitatively the same in unweighted regressions, where each observation is represented equally, not each individual.

Table 3: Robustness of the estimates of the effect of a same-sex co-twin on education, income and family formation, on the sample of DZ twins

	More than primary ed.	Prob. of income>0	ln(income)	Married	Any kids	Number of kids	Age at first birth
Panel A: Men							
SS co-twin	-0.006	0.001	0.013*	0.018***	0.013*	0.044**	-0.124
	(0.008)	(0.001)	(0.007)	(0.007)	(0.007)	(0.022)	(0.090)
N	13664	421434	418217	13560	13352	13352	10706
Panel B: Women							
SS co-twin	-0.018**	-0.002	-0.007	0.000	-0.004	0.013	-0.220**
	(0.008)	(0.002)	(0.008)	(0.007)	(0.006)	(0.020)	(0.085)
N	14950	461791	441173	14873	14720	14720	12801

Note: The standard errors shown in parentheses are robust and allow clustering by the family of origin. All regressions include a constant and cohort fixed effects. More than primary education means more than 9 years of schooling. The second and third columns show pooled regressions on the probability of having positive income and on ln(income). These pooled regressions also include year fixed effects and are weighted such that every person is represented equally. Married takes one if the individual is currently married or cohabiting and takes zero for those who have never been married or whose marriage ended. Any kids and number of kids are counted only if the respondent lists the birth date of the kids as well. Age at first birth is counted only if it is between 14 and 40. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

It can be observed that all the results are robust. While there is no impact on men's education, women with sisters are less likely to have more than primary education. The pooled regression on ln(income) leads to the same conclusion as the estimates on ln(permanent income). There is no impact on the extensive margin, that is, on the probability of having positive income. The results on family formation are also in line with Table 2.

For the sake of comparison, it could be interesting to see how the results would look like if we could not differentiate the twins by zygosity. Therefore we report results on the sample of all twins (including MZs) in the Appendix, in Table 5. The results are quite similar in case of men. However, they are very different in case of women. The coefficients are insignificant and very close to zero for all outcomes, including education and age at first birth. Thus, the estimates on women would be quite misleading if we had no information on zygosity.

5 Potential explanations

There are several potential explanations for the results of Section 4. In the first subsection, we discuss how effects can arise via various social mechanisms. In the second subsection, we address the role of family size. It is important to examine this factor as a potential channel separately since we did not control for it in the estimations. In the third subsection, we examine testosterone transfer as another potential channel.

5.1 Social mechanisms

There are several specific mechanisms behind the collective term “social mechanisms”. We discuss them now in turn, building on previous research on this topic. We evaluate their plausibility by analyzing whether their predictions are consistent with our results.

First, recall that the child development literature finds that those with brothers (sisters) become more masculine (feminine) (Rust *et al.*, 2000). Therefore gender differences in personality traits and preferences may affect the siblings. For example, men are less risk averse, more competitive, more willing to negotiate, less socially minded, less agreeable and less neurotic than women (see the review of Bertrand, 2010)[22]. If some of these preferences and traits spill over to the sibling, it might explain why those with brothers have higher income.[23] Moreover, previous research suggests that the impact will be more pronounced for men. Of these six non-cognitive factors, three have been found to affect the earnings of men and women differently: negotiation, agreeableness and neuroticism. Bowles *et al.* (2007) find that women who initiate negotiations receive worse evaluations than men who initiate negotiations, at least when the evaluators are men. Similarly, Säve-Söderbergh (2007) finds that employers reward self-promoting less in case of women. Mueller and Plug (2006) find that being less agreeable and less neurotic are both associated with higher earnings in case of men, but not in case of women. Thus, an impact on men’s income is more likely and this is indeed what we see.

A second possibility is the reference point argument. Research on adult siblings shows that people view their sibling’s outcomes as reference points (Adams, 1999; Keim *et al.*, 2009). Therefore they do not want to lag behind their siblings. This is in line with Kahneman and Tversky (1984): there is loss aversion for outcomes below the reference point. This can apply to labor market outcomes and family life as well. Indeed, Kuegler (2009) finds that individuals are less satisfied with their life if their sibling earns more than they do. In addition, Dahl *et al.* (2014) find that men are more likely to take paternity leave if their brother was exogenously induced to do so, which is also consistent with the reference point mechanism.

The gender of the sibling is relevant for two reasons. First, rivalry is stronger among same-sex siblings (Adams, 1999). This is especially true with respect to family formation, since gender roles are quite different in that respect (Keim *et al.*, 2009). This means that we can expect earlier family formation and higher marriage and fertility rates among same-sex twins, and perhaps better labor market outcomes as well. The second reason to expect gender to matter is that gender differences imply different reference points. If the labor market outcomes of men are better, brothers will represent a higher reference point than sisters. Similarly, as women marry and have kids at an earlier age, we

[22]Agreeableness and neuroticism are the two personality traits from the so-called “Big Five” that consistently show gender differences.

[23]Such spillovers can occur in several ways. Sex composition affects the type of common play, which can then affect the acquisition of certain traits (Stoneman *et al.*, 1986). Access to toys is also affected, since parents buy sex-typed toys for their children (Fisher-Thompson, 1993; Nelson, 2005). Yet another option is that parental behavior differs by sex composition (e.g. boys might be encouraged more to compete with each other).

can expect sisters to decrease age at first birth.[24]

We consider the predictions of the reference point mechanism clear when same-sex rivalry and gender differences do not go in opposite directions. This means that for earnings, we can only predict the impact on men: those with brothers should earn more. This is consistent with our findings. In terms of marriage and fertility rates, we expect both men and women to perform better if they have a same-sex sibling. However, only women are expected to also give birth earlier. The predictions on timing are in accordance with our results, but the predictions on marriage and fertility are only in line with the findings of men. As far as education is concerned, Butcher and Case (1994) argue that women with sisters obtain lower education because they get a lower educational reference point. This is possible, since women used to be lower educated than men.[25] However, same-sex rivalry would go in the opposite direction, so this prediction is less clear-cut. Nonetheless, it is possible that there is an indirect effect: women with sisters may obtain lower education because they give birth earlier. The educational prediction is more clear for boys: we can expect men with brothers to have higher education. We do not see such a pattern in the data, perhaps because rivalry among men is not focused on education.[26]

Finally, effects can arise indirectly, via differential parental investments. Previous research shows that the gender of the child influences the support given for family formation. In particular, parents give more support to their daughter than to their son, especially when grandchildren arrive (Pollet *et al.*, 2009). This implies that there is a substitution effect from sisters: if someone has a sister instead of a brother, he/she gets less support from his parents to form his own family. The prediction from this mechanism is clear: sisters negatively affect the family formation of their siblings. This prediction is consistent with the results on men. On women, we do not see a negative impact. However, recall that the reference point mechanism predicted a positive impact on women. Thus, the null results on women can simply reflect the fact that the two opposing forces cancel each other out.

To sum up, spillovers in childhood, reference point considerations and differential parental investment can all have an impact. While the available data does not allow for a direct test of these social mechanisms, it is possible to examine their plausibility by analyzing their predictions. We found that the predictions of the first mechanism are in line with the results. If we combine the predictions of the second and the third mechanisms, the joint predictions are also close to what we observe in the data. Therefore we conclude that social mechanisms are a promising candidate to explain the findings.

[24] The mean age at first marriage is about two and a half years lower for women than for men in Europe (UNECE, 2012). The difference in median age at first marriage is 2.5 years in the US (Copen *et al.*, 2012). Similar gaps can be found in age at first birth, since age at marriage and age at first birth are highly related (see Baizán *et al.*, 2003).

[25] By now, the gender gap in education closed or even reversed in most developed countries. However, in cohorts born in the first half of the 20th century, women were typically lower educated than men (Ganguli *et al.*, 2011).

[26] Studies by educational scientists indicate that boys find rivalry in educational performance “uncool” (see e.g. Epstein *et al.*, 1998 and Warrington *et al.*, 2000).

5.2 Family size

In this section, we focus on family size. This channel is relevant only if two conditions are met: 1) the sex composition of the twins affects family size and 2)family size affects the outcomes of the twins. We can examine the first condition empirically. As the first column of Table 4 shows, there is no impact on the number of siblings. This is in line with our expectations: since twins already increase family size substantially, the sex composition of the pair does not increase family size further. In addition, parity progression is influenced by the sex composition of all existing children, while twins are typically not the first birth. Indeed, more than 60 percent of our sample has at least one older sibling. Of course one can argue that our measure is imprecise, since only those siblings are listed who are still alive at the time of the interview. However, this means that the share of those with older siblings is potentially even higher, which makes our argument even stronger. Nonetheless, we investigated whether the results change if we drop the oldest cohorts, since the survival problem should be mitigated in this case. Our conclusions remained the same.[27] Thus, the data does not support the family size explanation.

The results in Table 4 are one way to address the family size channel. Another way is to consider the second condition, that is, to examine whether we can expect family size to have an impact on labor market outcomes and family formation. In this respect it is very informative to look at Scandinavian studies that use twin births as instruments for family size. In particular, Black *et al.* (2005) finds that education, earnings and teen births are all unaffected by family size in Norway. Åslund and Grönqvist (2010) reach similar conclusions about years of schooling and labor market outcomes in Sweden. This shows that even if we had no information on the number of siblings, the family size channel would not have been a plausible explanation for our findings on these outcomes. With respect to fertility, the above-mentioned studies provide no guidance, but as we will see below, we can learn more about that from a different approach.

The other common strategy used to estimate the impact of family size relies on sibling sex composition as an IV. At this point a brief methodological discussion is warranted. Recall from Section 2 that a selection bias can arise when we compare children with an older brother to children with an older sister. In addition, the validity of the sex composition instrument is questioned if social mechanisms are present. These considerations suggest to treat results from this strategy with caution. It seems best to focus on reduced form estimates that were obtained on first-borns only and interpret them as the total impact of a younger sibling's gender. Such estimates would provide an interesting comparison for our estimates on the impact of a co-twin's gender.

In this spirit it is worthwhile to discuss a recent paper by Cools and Kaldager Hart (2015) on fertility outcomes. Using a sample of Norwegian singletons, they examine how sibling sex composition

[27] We tried dropping everyone born in the 1920s, then even those born in the 1930s. The highest coefficient we got in any of these trials was 0.02, with a p-value of 0.622.

affects fertility.[28]

They find that first-born men have more children if their second-born sibling is of the same sex, while the gender of the second-born sibling has no impact on the fertility of first-born women. Thus, their results are in the same direction as our findings. Now let us compare the magnitudes: their reduced-form estimate for the fertility of first-born men is a bit below 0.03, whereas it is 0.044 in our twin sample.[29] It seems difficult to argue that family size is affected more by the sex of the twins than by the sex of the first two children. Thus, if our results reflected the impact of family size, the estimates of Cools and Kaldager Hart (2015) should be bigger than ours and not the other way around. In contrast, the smaller estimates of Cools and Kaldager Hart (2015) are consistent with explanations based on social mechanisms, since those mechanisms are likely to be weaker when they come from younger siblings towards older children.

Table 4: Potential channels on the sample of DZ twins

	Number of siblings	Age at menarche
Panel A: Men		
SS co-twin	-0.020	
	(0.036)	
N	13409	
Panel B: Women		
SS co-twin	-0.015	0.038
	(0.036)	(0.027)
N	14663	14283

Note: The standard errors shown in parentheses are robust and allow clustering by the family of origin. All regressions include a constant and cohort fixed effects. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

5.3 Testosterone transfer

Let us turn now to another potential explanation. A recent paper by Gielen *et al.* (2015) also compares the earnings of SS and OS twins to each other. In line with our results, they find that a co-twin brother increases men's earnings.[30] However, they interpret the estimates as the impact of prenatal testosterone (T). This interpretation is based on the assumption that T can transfer between co-twins in utero. This is the so-called Twin Testosterone Transfer (TTT) hypothesis. Male fetuses produce much higher levels of T, especially in weeks 10-20 of gestation (Baron-Cohen *et al.*, 2004). Therefore

[28]Originally they use sibling sex composition as an IV for family size, but we focus on reduced form results here.

[29]We calculated the reduced form estimates from the IV estimates and the first stage results.

[30]Gielen *et al.* (2015) only look at earnings and they do not differentiate by zygosity. Recall that the inclusion of MZ twins does not change the results of men substantially (see Table 5). However, the inclusion of MZ twins does bias the coefficient in case of women such that brothers seem to have a more negative impact. Indeed, this bias is reflected in their estimates on women.

if T could transfer, those with a male co-twin would be exposed to higher prenatal T than those with a female co-twin.

The inspiration for this hypothesis comes from animal studies. In case of rodents such as mice and rats, direct measures of prenatal T levels show that T can transfer between littermates (e.g. vom Saal and Bronson, 1980; Even *et al.*, 1992). In line with this, rodents that developed between male fetuses differ from rodents that developed between female fetuses in several aspects (Ryan and Vandenbergh, 2002). Most of the differences relate to genital morphology, physiology and reproductive characteristics, as the primary function of prenatal T is the sexual differentiation of the reproductive system.[31]

The impetus to the TTT hypothesis was given by Miller (1994), who speculated that T may transfer in case of humans as well and argued that human twins should be studied to uncover potential signs of T transfer.[32] Several papers examined the outcomes of twins in this vein - for reviews, see Cohen-Bendahan *et al.* (2005a) and Tapp *et al.* (2011). Both reviews find that the results of this literature lack consistency.

The main idea behind the TTT hypothesis is that extrapolations from animals to humans might be possible. To address this issue, we compare our results on fertility to the results of animal studies on reproduction. The focus of these studies has been on females because they produce much lower levels of prenatal T and hence an extra dose is expected to affect their development more. The findings reveal that females enter puberty later and have lower fertility if they develop between male fetuses instead of female fetuses (Ryan and Vandenbergh, 2002). One might suspect that the results on puberty can be extrapolated, since we found that women with co-twin brothers give birth later than women with co-twin sisters. Fortunately, we can examine this explicitly because we have information on age at menarche (first menstruation). As we can see from Table 4, the coefficient is insignificant and its sign is opposite to the prediction of this hypothesis. Thus, the findings on the timing of puberty do not carry over to our sample. Similarly, there is no effect on the fertility of women.

As far as males are concerned, animal studies produced mixed results. Some researchers found that developing between male fetuses instead of female fetuses leads to enhanced sexual performance (Clark *et al.*, 1992). However, others found significant results in the opposite direction (vom Saal *et al.*, 1983). Given these controversial findings, it is difficult to assess whether our results on men are in line with animal studies.

Our analysis so far found little support for the TTT channel. On the other hand, one can argue that our approach has its shortcomings. Most notably, our evidence is indirect: similarly to the TTT literature, we draw inferences from postnatal outcomes because we do not have information about actual prenatal T levels. While this is clearly a limitation, we note that our conclusion is consistent

[31] In the presence of high prenatal T, male genitalia develop. In the absence of high prenatal T, female genitalia develop, together with the internal reproductive organs (Cohen-Bendahan *et al.*, 2005a).

[32] Although some other researchers expressed similar views already earlier (e.g. Resnick *et al.*, 1993), Miller's study drew the most attention and it is considered to be the seminal paper of the TTT literature.

with medical research that tests the TTT hypothesis directly. In particular, Abeliovich *et al.* (1984) measured T levels in the amniotic fluid of twin fetuses. They found no evidence of T transfer: T levels were not elevated for twins with a male co-twin.[33]

The importance of direct T measurement was emphasized by Miller (1994) as well, but it seems that he was unaware of the results of Abeliovich *et al.* (1984). Instead, he based his argument on Meulenberg and Hofman (1991), who found higher maternal T levels in pregnancies with a male fetus. He interpreted this as a sign that T can transfer, at least from the fetus to the mother. However, several other studies failed to find a difference in maternal T levels by the sex of the fetus (Glass and Klein, 1981; Rodeck *et al.*, 1985; van de Beek *et al.*, 2004; Cohen-Bendahan *et al.*, 2005b; Toriola *et al.*, 2011). In fact, studies that measured T in both amniotic fluid and maternal serum found no correlation between the two measures (Rodeck *et al.*, 1985; van de Beek *et al.*, 2004). Hence T transfer between the fetus and the mother seems to be doubtful.

To summarize, we discussed three competing explanations for the findings in our data and also related them to results from the literature. With respect to T transfer, we found that the results of the animal literature do not carry over to women and the picture is mixed in case of men. In addition, studies that measure T levels directly obtain results that are not consistent with the hypothesis of T transfer. Therefore we conclude that there seems to be little support for the TTT channel. We reached a similar conclusion in the previous subsection about family size. That channel was also not consistent with the findings in our data, nor with findings from other studies. All in all, our analysis suggests that it would be difficult to explain the results without social mechanisms.

6 Conclusion

Using a sample of dizygotic twins from Sweden, we examined whether the gender of the co-twin has an impact on individuals' education, earnings and family formation. We find that men and women are both affected, but in different ways. Men with brothers have higher earnings, are more likely to form a family and have more children. Women with sisters have lower education and give birth earlier. The effect sizes are small but comparable to the impact of other family factors.[34] Our analysis showed that social mechanisms are the most likely explanation for the results.

Can we expect similar effects among singleton siblings? On the one hand, twins are probably closer to each other, so effects might be smaller between singletons. On the other hand, previous research on siblings indicates that the asymmetry of sibling relationships matters. Since there is a

[33]Their data shows that the mean amniotic T level (pg/ml) of female fetuses with a female (male) co-twin is 113 (105). The mean amniotic T level (pg/ml) of male fetuses with a female (male) co-twin is 315 (301). The difference between male and female fetuses is significant. The differences by the sex of the co-twin are not significant, neither for males, nor for females (and they are not even in the direction predicted by the TTT hypothesis).

[34]Holmlund *et al.* (2011) find that the causal effect of an additional year of parental schooling on children's schooling is around 0.10, which is exactly the magnitude that we find. The causal effect of family size is small and insignificant in most studies (see also Section 5.2).

leader-follower relationship between older and younger siblings, older siblings have a bigger impact on younger siblings than the other way around. Twins are born at the same time, so their status is more equal. This predicts that the impact of a co-twin is smaller than the impact of an older sibling but bigger than the impact of a younger sibling. Taken together, these considerations suggest that our estimates are an upper bound for the impact of a younger sibling's gender. However, the impact of an older sibling's gender might be smaller or bigger than the impact of a co-twin's gender. Our prediction for the impact of younger siblings is in line with the results of Cools and Kaldager Hart (2015). As we discussed in Section 5.2, they find that the gender of the second-born sibling affects the fertility of the first-born sibling the same way as the gender of the co-twin affects the other twin. The only difference is that the effect is smaller, just as we expected.

It is worth to discuss external validity in a more general sense as well. Twins are an obvious choice if we want to model two singleton siblings born at the same time. However, one can worry that twins might be too special so that the results do not carry over to the general population. This can be mitigated somewhat by the fact the we look at a time period when IVF technology was unavailable. Nonetheless, the concern remains valid and it also underlines that every empirical strategy has its limitations. Comparing results with studies that use a different strategy can help in this respect. The above-mentioned study by Cools and Kaldager Hart (2015) is a good example since they examined singletons. As their results on fertility are in line with our estimates, it seems that our findings are not specific to twins.

While our paper contributes to knowledge about the long-term effects of a sibling's gender, there are several questions that remain open. This is particularly true with respect to the mechanisms behind our results. We found that there are various social mechanisms that are promising candidates to explain the findings. However, our analysis on this topic remained indirect as we do not have direct measures of the underlying variables. An interesting direction for future research could be to obtain such measures and test the potential explanations directly.

Appendix

Table 5: Estimates of the effect of a same-sex co-twin on education, income and family formation on all twins, irrespective of zygosity

	Education	Ln(permanent income)	Ever married	Any kids	Nr of kids	Age at first birth
Panel A: Men						
SS co-twin	0.065	0.014**	0.010**	0.009	0.036*	-0.018
	(0.043)	(0.006)	(0.005)	(0.006)	(0.020)	(0.090)
N	18314	18314	18157	17986	17981	14681
Panel B: Women						
SS co-twin	0.000	0.001	-0.003	-0.008	0.000	-0.045
	(0.038)	(0.008)	(0.005)	(0.005)	(0.018)	(0.077)
N	20371	20358	20271	20117	20115	17464

Note: The standard errors shown in parentheses are robust and allow clustering by the family of origin. All regressions include a constant and cohort fixed effects. Education is measured as years of schooling. Permanent income is average income in years 1971-2007 (if subject is aged 25-64 in the given year). The variable "ever married" takes one if the individual has ever been married or cohabited and zero otherwise. Significance levels: *** p<0.01, ** p<0.05, * p<0.1

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