ORIGINAL ARTICLE

Sisters, Not Brothers: Gender and the Household Division of Educational Resources

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CITATION (APA STYLE)
Sayedahmed, D. (2026). Sisters, Not Brothers: Gender and the Household Division of Educational Resources. *International Journal of Gender, Knowledge and Social Futures, 1(1).* https://doi.org/10.30546/200310.010.2026.0046

ABSTRACT

Female education has become one of development policy's most familiar devices. Educate girls, the argument goes, and early childbearing falls. Beneath that proposition there are two questions that receive noticeably less attention: who inside the household gets the education in the first place, and how much causal work can schooling itself really do? This paper examines both using the Cape Area Panel Study and sibling sex composition as a source of variation in the educational attainment of young adults aged 18 to 26 in metropolitan Cape Town, South Africa. The first-stage results reveal a striking gender asymmetry. Young women raised with any sisters complete 0.78 fewer years of schooling on average, controlling for household size and family background. Male schooling, on the other hand, barely moves with sibling composition. The sign differs across countries and cohorts, and the gender asymmetry appears more stubborn. Collectively with the wider literature, that instability points toward a gendered household allocation rule: girls' educational investment appears more contingent on family composition, more available for adjustment when circumstances inside the household change. Sisters expose an inequality they did not create. The second stage results complicate another familiar story. Once education is instrumented, the large negative relationship between schooling and teenage pregnancy shrinks sharply and loses statistical significance. The Cape Town evidence therefore places gender inequality at two distinct points in the development story: in the household distribution of education itself, and in the distance between educational resources and reproductive agency. Gender responsive policy has reason to look further inside the household. Increasing what families have available is only part of the question. Who gets what still remains to be a question of its own.

Keywords *Sibling Sex Composition; Female Education; Early Fertility; Instrumental Variables; South Africa*

ACKNOWLEDGMENTS I am grateful to Prof. Matthieu Chemin, Prof. Franque Grimard, and Prof. Shelley Clark of McGill University for directing me to the Cape Area Panel Study. Waves 1–3 (2002–2005) were collected by the University of Cape Town and the University of Michigan, funded by the US National Institute for Child Health and Human Development and the Andrew W. Mellon Foundation. Wave 4 (2006) was collected by the University of Cape Town, the University of Michigan, and Princeton University, with major funding from the National Institute on Aging through Princeton University and NICHD through the University of Michigan. An earlier version of this paper was submitted as a master's thesis in Economics at McGill University in 2013.

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1. Introduction

"What circumstances limit women's liberty and can she overcome them?"

— Simone de Beauvoir, The Second Sex.

Education occupies a peculiar place in development policy. It is something to be distributed, often unequally, and something expected to do considerable work once acquired. For girls and women especially, schooling has accumulated an extraordinary number of jobs. It is expected to improve employment and earnings, delay childbearing, improve health, alter fertility, and carry human-capital gains into the next generation. Some of these relationships are remarkably persistent in the data (Schultz, 1993, 2008). They have also helped turn female education into a policy device. Years of schooling become something policymakers can pull to move outcomes elsewhere, fertility among them (Schultz, 1997). Yet education has to reach the girl before it can perform any of this work. Who gets how much of it is already a distributional question.

The evidence linking women's education and fertility is certainly persuasive about association. Cause is another matter, and policy has rarely been especially patient about the difference. More educated women tend to have fewer children, and the relationship survives a remarkable number of controls for family and community background. Still, correlations carry baggage. Omitted variables are one obvious problem (Berger & Leigh, 1989). Ability is another (Griliches, 1977). Then there is a confounder especially difficult to wave away: how people value the future relative to the present. Fuchs (1982) raised the role of discount rates decades ago, and the problem remains awkward. A young woman inclined to invest heavily in a future self may remain in school longer and postpone childbearing for reasons rooted in that same disposition. Education and fertility then move together beautifully in the data. The beauty of the relationship tells us very little about what produced it. Treat education as exogenous and that entire difficulty disappears by assumption. Conveniently. Does education itself account for some of the difference observed in early childbearing?

This study takes that question to metropolitan Cape Town, South Africa. Using an instrumental variables (IV) approach, it asks how much confidence the causal story around the education-fertility relationship can really bear. The empirical design takes its cue from Butcher and Case (1994), who used the sex composition of a woman's siblings as an instrument for schooling in the United States. Here, the outcome changes too. Butcher and Case were interested in earnings. This paper turns to teenage pregnancy. Sibling sex composition offers something unusually difficult to find in this literature: variation in schooling that the woman herself did not choose. She plainly had no hand in whether her siblings were born male or female. That gets the analysis some distance. The exclusion restriction remains the difficult part. Family composition can alter household resources, parental expectations, domestic responsibilities, supervision, perhaps even the small and largely invisible negotiations through which girls' time gets allocated inside a household. Some of those pathways could conceivably reach early childbearing without travelling through education first. Section 5 takes that problem seriously because the credibility of the exercise depends on it.

Furthermore, there's another intriguing aspect of sibling composition that delves deeper within the household dynamics. This paper examines how parents invest in children's education, how resources get divided between sons and daughters, and what those choices may eventually mean for adolescent girls. The consequences can arrive early. Teenage pregnancy may keep girls at home during precisely those early career years when young men begin moving into work and accumulating experience. Poorer employment prospects later on may make education for girls appear less worthwhile to parents in the first place. Girls receive fewer opportunities because their prospects are poor; their prospects remain poor partly because those opportunities were withheld. A rather nasty circle. In a setting where

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household resources are tight, the question of how far a child goes in school can remain substantially with parents, even while female employment expands and girls spend more years in education. Constraints rarely have much reason to fall evenly.

The Cape Area Panel Study (CAPS) makes it possible to examine these questions with unusually rich longitudinal data, following a large and representative sample of young people in metropolitan Cape Town through adolescence and into adulthood. There is a second reason for bringing the Butcher and Case strategy here. Since 1994, sibling sex composition has had a surprisingly long life as an instrument, while doubts about its stability have followed it around almost from the beginning. Does the instrument behave similarly once it leaves the institutional environment in which it was first identified? Butcher and Case found that American women raised only with brothers received more education than women raised with at least one sister, controlling for household size. Kaestner (1997) could not reproduce the result for a later United States birth cohort. Hauser and Kuo (1998) found fairly weak support. Evidence from China sends the sign elsewhere again. Cape Town adds another version: having sisters reduces female educational attainment by 0.78 years, while male schooling appears remarkably indifferent to sibling sex composition and family size. The specific Butcher and Case brother effect is difficult to see here.

Crucially, the instability interests this paper as much as the coefficient itself. Across these settings, the direction of the sibling effect on female schooling changes, sometimes considerably, while female schooling continues to respond to household composition in ways that male schooling often does not. That recurring gender asymmetry is part of what makes the instability informative. Household allocation rules change too: whose schooling is worth paying for, whose labour is needed, who takes care of whom, what parents imagine a daughter's education will eventually return. The instrument therefore arrives in Cape Town carrying a social question alongside the econometric one. Its credibility has to be established in the setting where it is used. Its first stage may also tell us something about whose education becomes adjustable inside that setting.

Also, the second-stage estimates add another complication. Education has no statistically significant effect on teenage pregnancy in the Cape Town sample once schooling is instrumented. The OLS estimates tell a much more dramatic story, with a substantially larger negative relationship between female schooling and teenage pregnancy. Breierova and Duflo (2004) find education to be an unimportant determinant of fertility before age 25 in Indonesia, while Osili and Long (2008) find almost the reverse empirical pattern in Nigeria: their IV estimate is much larger than the corresponding OLS estimate. Cape Town therefore enters an already unsettled causal literature. Female education has nevertheless become a familiar instrument of fertility policy, sometimes to the point that years of schooling are spoken about as though they operate rather mechanically on early childbearing. Add schooling here, receive fewer births there. The appeal is obvious. The correlations are strong, and educating girls is something worth doing anyway.

In sum, the contribution of the paper sits across these two results. The first stage suggests that household composition is part of the machinery through which educational resources are allocated, with girls' schooling more exposed to that machinery than boys. Across countries and cohorts, the direction of the sibling effect changes while the gendered sensitivity of educational investment appears more persistent. That instability is itself informative. It shifts attention from the particular sibling configuration toward the allocation rule underneath it, and toward whose schooling becomes adjustable when circumstances inside the household change. The second stage puts pressure on the assumption that additional schooling will travel straightforwardly into lower teenage pregnancy. Together, the two results locate gender inequality at different points in the development story. One concerns the distribution of an educational resource inside the household. The other concerns how far that resource can travel into a reproductive outcome shaped by a much wider social world. For gender responsive policy, the distinction matters. A

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household can be poor and allocate scarce resources unequally. It can become somewhat less poor and continue allocating them unequally. Increasing the size of the household pie tells us surprisingly little about how the pie will be divided. The allocation rule deserves attention of its own.

The rest of the paper proceeds as follows. Section 2 reviews the literature on sibling composition and education, together with work on education's non-market returns through fertility. Section 3 introduces the Cape Town data and patterns of educational attainment. Section 4 presents the first-stage analysis. Section 5 considers endogeneity and the second-stage estimates. Section 6 discusses the findings and their implications. Section 7 addresses limitations and future research. Section 8 concludes.

2. Literature Review

2.1 Sibling Composition and Educational Attainment

Usually, economic models of household behaviour begin with a family allocating scarce resources among its children. Educational attainment, from this perspective, emerges partly from decisions made inside the household, where parents weigh competing claims on a limited budget (Becker, 1981; Behrman, Pollak, & Taubman, 1982). Once resources are scarce, those decisions acquire a gender. Expected returns matter. So do borrowing constraints, domestic responsibilities, birth order, and assumptions about which child will eventually support the household. A daughter and a son can therefore place very different claims on the same educational budget. The arithmetic of the household is rarely just arithmetic. Early developing-country evidence made this visible. Parish and Willis (1993), studying Taiwan, find that brothers reduce siblings' education and that credit constraints and birth order sharpen the effects, particularly for girls. Older sisters in poor households can increase younger siblings' schooling through their own contributions to the family. Salaff (1981) describes a starker version in Hong Kong: older daughters leave school and enter work while younger brothers continue their education. Tang (1981) captures the uncomfortable logic rather memorably. One of the best things that can happen to a boy, after having wealthy and educated parents, is having an older sister. A grim compliment to the sister.

Additionally, later work carried the resource-allocation argument across very different settings. Studies from Ghana, Türkiye, Bolivia, Vietnam, and China find consequences associated with sibship size, birth order, or sibling sex composition, often where resources are already thin (Garg & Morduch, 1998; Morduch, 2000; Dayıoğlu et al., 2009; Zeng et al., 2012; Zheng, 2015; Vu & Tran, 2021). The direction moves around. In China, for example, brothers can reduce female educational attainment while sisters release resources that might otherwise be absorbed by sons (Zheng, 2015). Family size is changing alongside these allocation problems. Declining fertility can loosen some resource constraints while making the sex and position of each child more consequential inside a smaller household (Ibhate, Opoggen, & Bondoi, 2025). A preference for sons has rather different arithmetic in a family expecting two children than in one expecting six. Butcher and Case (1994) produced the result that gave sibling sex composition another empirical life. In the United States, women raised only with brothers obtained more education than women raised with sisters, while comparable effects did not appear for men. The result proved restless. Kaestner (1997) finds essentially no sibling-sex effect among white adults in a later American cohort and a reversed sign among Black adults. Hauser and Kuo (1998) find weak and inconsistent evidence across several datasets. Conley (2000) proposes another mechanism altogether, with opposite-sex siblings depressing attainment regardless of the respondent's sex. Same family variable. Several empirical lives.

Still, evidence outside the United States makes a universal interpretation harder. Chinese studies find brothers reducing educational attainment and sisters increasing it, particularly for women and rural

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households (Lu & Treiman, 2008; Zheng, 2015). More recent evidence makes the gendered allocation mechanism harder to miss. Also, Liu et al. (2025) find that China's 1.5-child policy widened the educational gender gap by disproportionately reducing female attainment, with eldest daughters who acquired younger brothers facing the largest losses. Resource dilution explains part of the effect. So does care. Older daughters absorb responsibilities inside the household that their brothers do not absorb in the same way. More recent work gets closer still to the institutions and relationships underneath these coefficients. Collins (2025) shows that inheritance customs mediate sibling-gender effects on schooling. Zang et al. (2023) find sibling spillovers especially pronounced among disadvantaged families.

Furthermore, and fascinatingly, Brenøe (2022) finds that women raised with brothers develop more traditional gender roles later in life, while Barclay and Bygren (2026) move beyond years of schooling and find sibling composition associated with gender typical educational choices among some groups. That is, the sibling can affect resources. Sibling effects can also reach beyond years of schooling into what education eventually looks like. Likewise, older siblings influence younger siblings' college and major choices across several institutional settings (Altmejd et al., 2021), while gender-atypical fields of study remain patterned by social-class background and the gendered environments in which educational choices are made (Seehuus, 2019). The sibling can also enter the social world in which particular educational futures begin to feel ordinary. That social world matters because a brother or sister is never merely a sex indicator once placed back inside a household. Sibling relationships themselves sit within wider cultural and family systems (Garner & Manuel-Scott, 2026). Expectations about earnings, inheritance, care for parents, domestic work, marriage, and the value attached to educating a daughter can all give the same sibship configuration a different meaning somewhere else. Years of schooling capture the eventual educational outcome while revealing slight about how education rearranges gender relations and agency underneath it, a problem that also appears in Peppin Vaughan's (2025) review of women's education and sex selection.

Hence, the instability across studies may contain information of its own. Sibship composition operates inside systems of gender socialisation and household allocation whose rules vary across institutional settings. Its coefficient can change sign as those rules change. The wandering sign may be telling the story. What persists more often is the gendered sensitivity underneath it: *female schooling appears responsive to household composition in settings where male schooling moves sizeably less.* Read this way, the question shifts from which sibling is "good" or "bad" for a girl's education. Household resources reach children through allocation rules already carrying expectations about gender. Sibship composition may reveal whose education becomes available for adjustment when those resources are divided. A sister coefficient can therefore tell us something larger than competition between sisters. It can expose a setting in which girls' educational investment is more negotiable. That asymmetry is itself part of the gender inequality under study here.

2.2 Non-Market Returns to Education: Reduced Fertility

In addition, and separately, education can reach fertility through several routes. More educated women may choose fewer children while investing more in each child, the familiar quantity-quality mechanism (Becker & Lewis, 1973). Education can improve child survival and alter desired family size (Lam & Duryea, 1999; Schultz, 1993), while contraceptive knowledge and effective use provide another route (Rosenzweig & Schultz, 1985, 1989). Earnings matter too, since education can raise the opportunity cost of childbearing. More recent fertility theory has widened the frame considerably, bringing parental time, bargaining within couples, labour-market institutions, family policy, and investment in children into the same fertility decision (Doepke et al., 2023). Education can enter several of these at once. How far, and in which direction, depends on what else is going on. The descriptive relationship is striking. Mothers with more education tend to have fewer children, and Schultz (1993) identifies this as one of the major

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findings in research on women's education. Causality is where the ground gets softer. Compulsory-schooling and school-expansion designs have produced reductions in early fertility in some settings and small, heterogeneous, or differently signed estimates elsewhere (Breierova & Duflo, 2004; Black et al., 2008; McCrary & Royer, 2011; Fort, Schneeweis, & Winter-Ebmer, 2016). Kim (2023) reaches much the same conclusion from the wider literature. Education can alter health, child survival, fertility preferences, contraceptive knowledge, and women's control over births. The weight carried by each channel moves with institutions and stages of economic development.

Consequently, developing-country IV evidence makes the variation especially difficult to ignore. Breierova and Duflo (2004) exploit Indonesia's large primary-school construction programme, drawing on the regional and cohort variation in schooling generated by the programme and originally developed as an identification strategy in Duflo (2001). Average parental education matters for very early fertility, before age 15. By age 25, the effect becomes unimportant - already, the timing is doing something interesting. Moreover, Osili and Long (2008) reach a very different result in Nigeria, where their TSLS estimates suggest a substantial causal reduction in births before age 25. More recent estimates have hardly settled the matter. Wang (2025) finds smaller and less uniform effects in Malawi, while Chen and Guo (2022) uncover considerable heterogeneity following China's compulsory-schooling reform. A causal effect exists in parts of this literature. Its size, timing, and reach have proven remarkably sensitive to where and how education changes. Part of the difficulty lies in the outcome called fertility. DeCicca and Krashinsky (2023), using Canadian compulsory-schooling reforms, find education compressing the fertility distribution: women become more likely to have at least one child and less likely to have multiple children. European research similarly distinguishes educational effects on fertility timing from effects on eventual family size (Vasireddy et al., 2023). Work from China and Taiwan adds further variation across cohorts and stages of the reproductive life course (Piotrowski & Tong, 2016; Zhang & Zhao, 2023; Liu & Zhao, 2024; Zheng, 2024; Shen, 2025). "Fertility" begins to carry rather a lot. First birth. Teenage pregnancy. Births before age 25. Completed fertility. Timing. Distribution. A coefficient attached to one can slide surprisingly easily into a policy claim about another.

Therefore, literature leaves education operating through mechanisms whose strength depends heavily on context. The inverse education-fertility relationship remains descriptively powerful. Its causal interpretation travels less comfortably. Education reaches labour-market opportunities, marriage and family formation, knowledge about reproduction, household bargaining, and the economic cost of children. These pathways can reinforce one another. They can also pull fertility in different directions. A clean coefficient has a lot of disorder behind it. There is another problem, especially important once the question is framed around women's education. Fertility is frequently measured at the level of the woman while the decision itself may be made somewhere much more crowded.

In addition and importantly, it should be noted that feminist economics has long pushed analysis inside that household space. Women's access to resources and women's control over those resources cannot simply be treated as equivalent (Mason, 1986). Naturally this is crucial as fertility is one of the key decision focuses within a household, where individual resources enter relationships structured by bargaining and authority. Van Hoyweghen et al (2025), studying rural Senegal and Uganda, find that individual fertility preferences cannot simply stand in for household choices. Decision-making itself varies sharply by setting. In Uganda, fertility choices reflect a balance between spouses' preferences. In Senegal, husbands' preferences dominate.

Yet, this complicates a simple education-to-fertility pathway. Schooling can change a woman's preferences, knowledge, opportunities, expectations from work, perhaps her bargaining position inside the household as well. The eventual fertility decision may still be made in a space where her preference carries limited weight. Additional schooling then has quite a distance to travel before it reaches a birth

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outcome, through a partner, through a household, through the economic possibilities surrounding that household. This matters for the interpretation of causal estimates. A weak effect of schooling can coexist with substantial value in women's education when reproductive decisions themselves are relational and unequally held. Null instrumented estimates of the kind reported below become less puzzling once that distance is taken seriously.

3. Data and Patterns of Educational Attainment

3.1 Data

Two specific data requirements shaped this study. First, the young adults surveyed needed to be old enough to have completed a meaningful amount of education. Second, information was needed on the number and sex composition of the siblings in the households in which they were raised. Many datasets widely used to study education are poorly suited to this purpose because they record current household characteristics without recovering the sibship in which respondents grew up. The Cape Area Panel Study (CAPS) makes this analysis possible, as CAPS is a longitudinal survey that followed a large and representative sample of young adults in metropolitan Cape Town through the transitions from adolescence into adult life (University of Cape Town & University of Michigan, 2012). The study commenced in 2002, when approximately 4,800 young people between the ages of 14 and 22 were interviewed in Wave 1, together with information on their households. Young adults were selected from the household roster collected during the household interview. Subsequent waves followed the original young adult sample. By Wave 4 in 2006, the young adults and their households had been interviewed repeatedly as they moved through schooling, work, family formation, and other early-adult transitions. Wave 4 also included a short questionnaire covering children born to female young adults. The present analysis uses Waves 1 through 4, covering 2002 to 2006 (Lam et al., 2008). CAPS subsequently continued to Wave 5 in 2009, which falls outside the period examined here. The Wave 1 household roster records the relationship of each household member to the young adult respondent. Variables describing the size of the sibship in which respondents were raised and the sex composition of their siblings were constructed from this roster.

The young adult data also provide detailed information on parental education, years of education completed, and pregnancy incidence, including live births and whether a respondent had ever been pregnant. Indicators for parental education and a per capita household income-based poverty measure control for family socioeconomic status. **Table 1** reports the summary statistics and provides further detail on the variables used in the analysis.

Table 1. Summary statistics, Cape Area Panel Study (CAPS), 2002–2006

	Ages 18–26 inclusive	
Variable	Males	Females
Years of education completed	10.17 (2.20)	10.62 (1.94)
Any sisters	0.71 (0.44)	0.73 (0.44)
Percent sisters in sibship	0.33 (0.21)	0.34 (0.24)
Number of sisters	1.13 (0.99)	1.19 (1.01)
Number of brothers	1.14 (0.96)	1.09 (0.95)
Number of siblings	2.27 (1.20)	2.29 (1.23)
High school degree, mother	0.19 (0.39)	0.18 (0.38)
College degree, mother	0.08 (0.28)	0.08 (0.27)
Completed education, mother	9.00 (2.86)	8.76 (3.05)

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	Ages 18–26 inclusive	
Variable	**Males**	**Females**
High school degree, father	0.17 (0.37)	0.18 (0.38)
College degree, father	0.08 (0.27)	0.08 (0.28)
Completed education, father	8.94 (2.95)	8.93 (3.05)
Mother works	0.47 (0.49)	0.46 (0.49)
Father laborer	0.07 (0.25)	0.08 (0.26)
Father professional	0.13 (0.33)	0.12 (0.32)
Oldest sibling	0.35 (0.46)	0.35 (0.42)
Poor household	0.17 (0.37)	0.16 (0.36)
Age in 2006	21.57 (2.47)	21.63 (2.40)
Observations	**1,561**	**1,878**

Notes: Standard deviations in parentheses. All means weighted using sample weights. The sample is restricted to adolescents aged 18–26 inclusive with a positive amount of completed education and at least one sibling. Percent sisters in sibship is the ratio of the number of sisters to the total number of siblings within the household, including the respondent, according to the Wave 1 household roster. Mother high school degree = 1 if the mother's highest level of education is Grade 12 or above; = 0 otherwise. Mother college degree = 1 if the mother's highest level of education is Grade 16 or above (diplomas and certificates); = 0 otherwise. Father's education is defined analogously. Mean completed education for the mothers and fathers of both female and male adolescents is roughly nine years, which covers primary and middle school levels in full. Racial composition of the sample: black 0.45 (males) and 0.48 (females); coloured 0.44 and 0.42; white 0.11 and 0.10. *Source:* Author's calculations using CAPS, Waves 1–4 (2002–2006).

Table 1 shows that male and female adolescents have a similar number of siblings on average. The tendency to belong to a poor household is also similar across genders, at around 17 percent, which is unsurprising given that the data come from metropolitan Cape Town rather than South Africa as a whole. Parents of the adolescents are relatively unlikely to have completed high school or college, though almost all have completed primary and middle school, again unsurprising given the metropolitan setting. Therefore, the key take away from this table to keep in mind when reading the rest of the study that follows below is that the sample splits almost evenly between adolescents with and without sisters, so the sibling contrast is not a rare-cell artefact; parental education averages roughly nine years, placing most households at the primary-to-middle-school margin where schooling decisions bind hardest.

3.2 Educational Attainment of Adolescent Females and Males

Table 2 continues the summary statistics in terms of the educational levels completed by the young adults. Most Cape Town young adults have completed primary and middle school, so there is almost no variation at those levels. High school and college degrees are much less common. The data show that adolescent females are slightly more likely than males to complete high school (0.376 versus 0.348), whereas college degree completion is roughly the same for both genders.

Table 2. Education levels completed, by sex

Level of education completed	Females	Males	Observations (F / M)
Primary school degree	0.993 (0.082)	0.975 (0.156)	1,878 / 1,561
Middle school degree	0.870 (0.339)	0.802 (0.398)	1,878 / 1,561
High school degree	0.376 (0.487)	0.348 (0.474)	1,658 / 1,233
College degree	0.181 (0.385)	0.193 (0.390)	1,082 / 796

Notes: Standard deviations in parentheses. Fractions of young adults at each level of education, conditional on completing that level. All means weighted using sample weights. Primary = 1 if the level of education completed by the last wave is Grade 6 or above; middle school = 1 if Grade 9 or above; high school = 1 if Grade 12 or above; college degree = 1 if 20 or above (undergraduate diploma or certificate from a university); = 0 otherwise. Mean completed education is 10.62 years for females (SD = 1.94) and 10.17 years for males (SD = 2.20), covering primary and middle school levels in both cases. *Source:* Author's calculations using CAPS, Waves 1–4 (2002–2006).

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The second stage tends to command the attention in studies like this one, with all its intricacies of endogeneity and the exclusion restriction, yet first stage also has its complications before one reaches to the second stage stories, therefore needs care and attention. That is, in this case, the number of siblings in a family is highly correlated with other measures of family background that can affect educational attainment. Regressions of completed education on the number of siblings, parents' education, socioeconomic status, birth order, and religion (**Table A1** in the Appendix) isolate each variable's independent effect. And in contrast to Butcher and Case's (1994) findings for the United States, an additional sibling in Cape Town is associated with an increase in educational attainment of roughly two fifths of a year for male adolescents and three fifths of a year for female adolescents. This may suggest that adolescent siblings play the role of a supportive cushion for each other in obtaining additional schooling, which may partly explain South Africa's general upward trend in secondary school enrolments (**Figure 1** in the Appendix). However, an additional sibling becomes disadvantageous once the number of siblings is squared. The significant independent positive effect of sibship size reported in **Table A1** vanishes for males in the first-stage analysis once control variables are added, but the positive effect of the number of siblings remains significant for females even after controls (**Table 3**). Moreover, parents' educational attainment positively and significantly influences schooling for both males and females. For female adolescents, the effect of the mother's education appears larger than that of the father's, which may reflect greater maternal influence on adolescent daughters or the fact that the mother's education partly proxies for household wealth (Butcher & Case, 1994). All in all, indeed, family background variables are important determinants of schooling, and these characteristics are controlled for in the regressions that follow. So, all in all, the key take away regarding Table 2 to keep in mind before the next table is that young women and men reach primary and middle school at similar rates, but the sample thins out at every higher step, which is the range where sibling composition turns out to do its work.

4. The Impact of Sibling Composition on Educational Attainment: First-Stage

4.1 Specifications

The sex composition of siblings has a significant effect on the education of young adult females. The results in **Table 3** show that females' schooling is significantly influenced by both family size and sibling sex composition. Males' schooling, however, is unaffected by both. Family composition may influence educational attainment through different mechanisms. If the introduction of a sister changes a daughter's reference group within a household, an indicator for the presence of any sisters should be significant in regressions of completed education (Butcher & Case, 1994). If instead borrowing constraints cause females' educational decisions to depend on the overall sex composition of their siblings, or if girls and boys have different effects on a family's budget constraint, educational attainment should depend on the percentage of children within the household, including the respondent, who are female. The results of adding each of these controls are presented in **Table 3**.

Table 3. First stage: sibling sex composition and educational attainment

	Females				Males			
Explanatory variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Any sisters	−0.776***			−0.979***	−0.036			−0.615
	(0.204)			(0.340)	(0.211)			(0.393)
Any brothers		−0.075				−0.253		

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	Females				Males			
Explanatory variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
		(0.161)				(0.241)		
Percentage of siblings female			−0.781**	0.463			0.253	1.217*
			(0.361)	(0.587)			(0.359)	(0.668)
Number of siblings	0.707***	0.822**	0.473***	0.716***	0.474	0.594	0.442	0.559
	(0.190)	(0.351)	(0.176)	(0.191)	(0.421)	(0.400)	(0.412)	(0.417)
Number of siblings squared	−0.144***	−0.161**	−0.109**	−0.147***	−0.112	−0.125	−0.108	−0.128
	(0.049)	(0.073)	(0.047)	(0.049)	(0.079)	(0.077)	(0.078)	(0.080)
Control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,647	1,647	1,647	1,647	1,357	1,357	1,357	1,357
R^2	0.151	0.153	0.147	0.150	0.167	0.165	0.165	0.161

Notes: Dependent variable: years of education completed by the last wave. Robust standard errors in parentheses; ***p< 0.01, **p < 0.05, *p < 0.1. All regressions are weighted using sample weights and are cluster-adjusted. All specifications include controls for poor household, oldest sibling, race, religion, parents' high school and college degrees, father professional, father laborer, mother working, age, age², age³, and an intercept. The sample is restricted to Cape Town adolescents aged 18–26 with at least one sibling and a positive amount of education. Sibling information is obtained from the Wave 1 household roster. Percentage of siblings female is the ratio of the number of sisters to the total number of siblings, including the respondent. Mean completed education is 10.62 years for females (SD = 1.94) and 10.17 years for males (SD = 2.20), covering primary and middle school levels. *Source:* Author's calculations using CAPS, Waves 1–4 (2002–2006).

The educational attainment of males does not appear to depend on sibling composition, as the coefficients on both family variables, any sisters and percentage female, are mostly insignificant. Unlike in Butcher and Case (1994), males' education does not remain orthogonal to sibling composition once both an indicator for any sisters and an interaction between any sisters and the number of siblings is allowed; the coefficient on the interaction term becomes significantly negative. The presence of any sisters has a significant impact on females' education. The addition of a sister reduces a female's education by 0.78 years, holding family size and background constant. This reduction is much larger than Butcher and Case's (1994) estimate of −0.30 for women in the United States. As in Butcher and Case (1994), the influence operating through the presence of any sisters appears more persistent than that operating through the percentage of siblings who are female. In column 3, when the percentage female variable is added by itself, it is significantly and negatively correlated with girls' education. When it is added alongside the any sisters indicator, however, it loses significance while the presence of any sisters remains strong (column 4). Particularly for females from small families, the any sisters indicator and the percentage female variable are highly correlated; in the absence of the former, the latter may be picking up its effect. Adding both variables tests their relative effects. The any sisters indicator dominates, with a large and significant negative coefficient of −0.979 and a t-statistic of 2.88. The percentage female coefficient is not small (0.463) and turns positive, but it is insignificant, with a t-statistic of 0.79.

Butcher and Case (1994) can state that women raised only with brothers receive significantly more education than women raised with any sisters. Although the coefficient on any sisters is negative and significant in both Cape Town and the United States, their coefficient on the any brothers indicator for women is positive and marginally significant (0.227, standard error 0.134), whereas the corresponding coefficient here is negative and highly insignificant (−0.075, standard error 0.161). One may therefore state that adolescent girls raised with brothers tend to receive more education than girls raised with any sisters in Cape Town, but not with the confidence available to Butcher and Case (1994). In metropolitan Cape Town there also appears to be a hint of negative gender bias in girls' educational attainment. For boys, the positive effect of the percentage of siblings who are female is consistent regardless of whether the any sisters indicator is included, and it becomes significant when that indicator is added. For adolescent females, the coefficient on any sisters is negative and significant regardless of the presence of

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the percentage female variable. The results in **Table 3** suggest that sisters negatively affect each other's educational attainment.

In brief, before moving on to the next table, the key take away from the first stage effect of sibling sex composition on educational attainment in Table 3 is that any sisters cut a young woman's schooling by roughly 0.78 to 0.98 years and the effect survives every specification, while the any-brothers coefficient is small and insignificant, so the Butcher and Case "brothers help" result does not reappear here; boys are unmoved throughout.

4.2 Sibling Sex Composition and Educational Degrees

The indicator for the presence of any sisters is next used to analyse the impact of sibling sex composition on the educational degree attainments of female young adults reported in **Table 2**. Hence, the next table, which is **Table 4** reports regressions at three points of degree attainment: completing high school, earning a college degree including certificates and diplomas, and earning a bachelor's degree together with postgraduate degrees. Levels below high school are excluded owing to the lack of variation there. Males' educational attainment appears orthogonal to sibling sex composition, so the analysis focuses on adolescent females. Please note that having established in **Table 3** that it dominates the alternative measures of sex composition, we carry forward only the any sisters indicator.

Table 4. First stage analytics continued: sibling sex composition on educational degrees, young adult females

	Binary dependent variable		
Explanatory variables	**High school degree (1)**	**College degree (2)**	**College/grad degree (3)**
Any sisters	−0.249**	0.407**	0.759**
	(0.112)	(0.205)	(0.296)
Number of siblings	0.401***	−0.416*	−0.611
	(0.150)	(0.237)	(0.561)
Number of siblings squared	−0.087**	0.083	0.126
	(0.037)	(0.052)	(0.101)
Control variables	Yes	Yes	Yes
Observations	1,658	1,082	955
R^2	0.102	0.101	0.101

Notes: Robust standard errors in parentheses; ***p < 0.01, **p < 0.05, *p < 0.1. All regressions are weighted using sample weights and are cluster-adjusted. Controls are as in Table 3. High school degree = 1 if the female's highest level of education completed by the last wave is Grade 12 or above; = 0 otherwise. College degree (column 2) = 1 if the respondent attended and completed any form of higher education after high school, including diploma and certificate programs; = 0 otherwise. College/graduate degree (column 3) = 1 if the respondent completed an undergraduate Bachelor's degree from a university or a postgraduate degree; = 0 otherwise. Two definitions of a college degree are used because metropolitan settings offer many distinct types of higher education programs.
Source: Author's calculations using CAPS, Waves 1–4 (2002–2006).

As in **Table 3**, in the above table, sisters significantly reduce the probability that adolescent females obtain a high school degree, by roughly 25 percent on average. Strikingly however, having a sister significantly increases the likelihood that a young adult female obtains any form of higher education degree, such as a diploma or certificate (column 2), and also increases the likelihood of obtaining a bachelor's or master's degree from a university (column 3). This offsetting pattern, a negative effect at the high-school margin alongside a positive one at the tertiary level, is obscured in the continuous years-of-education measure of **Table 3**, where the two effects partly cancel, and is visible only when attainment is disaggregated by degree. In metropolitan Cape Town, sisters therefore appear to influence each other's chances of obtaining college degrees positively. One possible explanation is that older sisters participate in the labour force and financially support their sisters in obtaining college degrees. Higher education degrees such as bachelor's degrees, diplomas, and certificates tend to be highly valued

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by employers in urban areas, and such degrees can generate substantial returns in terms of access to prestigious job opportunities in metropolitan areas, especially for females.

It is reasonable to note that higher education degrees tend to function as luxury goods for many individuals, given high indirect costs such as the opportunity cost of forgone employment and high direct costs such as tuition. **Table 4** shows that the effect of the number of siblings on the likelihood of obtaining college, diploma, and certificate degrees becomes negative for adolescent females. In contrast to the supportive cushion role that siblings appear to play in attaining additional years of education in **Table 3**, siblings quickly begin to reduce the likelihood of obtaining higher education degrees. The supportive cushion interpretation remains doubtful when the number of siblings is squared, since the positive coefficients are not significant. In summary, the key take away from Table 4 of sibling composition on educational degrees is that disaggregating by degree reveals a reversal a years-of-schooling measure would hide: sisters lower the probability of finishing high school yet raise the probability of completing a college credential, so the sibling penalty is concentrated at the secondary margin and flips at the tertiary one.

4.3 Other Economic Outcomes and the Exclusion Restriction

If the presence of a sister changes a female's educational goals, it may also affect other aspects of her economic and social life. To this end, **Table 5** presents estimations of the effect of any sisters on two objective outcome variables, currently working and ever married, as well as one subjective outcome variable, the age at which the respondent expects to first marry. **Table 5** also explores the influence of any sisters on whether a female's parents were ever married. These variables may be expected to be linked to both schooling decisions and sibling composition. Importantly, **Table 5** is also reported to comment on the exclusion restriction requirement for the potential instruments - the any sisters indicator, the number of siblings, and the number of siblings squared. That is, the instruments should not influence the main outcome variable, the incidence of teenage pregnancy, other than through the endogenous regressor, education. Therefore, the below table checks whether there are other channels through which the instruments might reach the outcome. Because if one of the instruments influences the incidence of teenage pregnancy other than through educational attainment, the causality argument becomes doubtful, and here we analyse and verify for this and confirm that it is not the case.

Table 5. Sibling composition and other socioeconomic outcomes, young adult females

	Socioeconomic outcomes			
	Currently working	Ever married	Age expected to first marry	Parents ever married
Mean of dependent variable	0.321	0.071	39.41	0.751
	(0.465)	(0.257)	(26.31)	(0.422)
Any sisters	0.018	0.280	−3.137	−0.213
	(0.160)	(0.203)	(2.954)	(0.221)
Number of siblings	0.472	−0.331	1.656	0.427
	(0.332)	(0.441)	(6.246)	(0.304)
Number of siblings squared	−0.078	0.094	−0.228	−0.082
	(0.068)	(0.088)	(1.282)	(0.061)
Observations	1,344	1,664	1,663	1,647

Notes: Standard deviations of dependent variable means and robust standard errors of coefficients in parentheses; ***p < 0.01, **p < 0.05, *p < 0.1. The sample is restricted to adolescents with at least one sibling. All regressions include an intercept, age, age², age³, and indicators for religion, race, poor household, oldest child, and parents' highest level of education completed. Sibling information comes from the Wave 1 household roster. Currently working = 1 if Wave 1 work status indicates full-time, part-time, or combined employment; = 0 otherwise; these regressions also control for mother working, father laborer, and father professional. Ever married = 1 if the respondent reported having married by the last wave; = 0 otherwise. Age expect to first marry is a subjective continuous

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variable from the Wave 1 young adult data. Parents ever married = 1 if the respondent reported in Wave 1 that her parents had been married to each other; = 0 otherwise. *Source:* Author's calculations using CAPS, Waves 1–4 (2002–2006).

Hence, according to the reporting in **Table 5**, no effect of any sisters is found on the social and economic outcomes. There is no effect on the probability of currently working or of having ever married, and any sisters appears uncorrelated with the subjective measure of expected age at first marriage and with whether the respondent's parents were ever married. Therefore, the potential instruments are uncorrelated with other social and economic outcome variables that could influence fertility decisions is reassuring for the exclusion restriction. Sibling sex composition influences females' schooling but does not appear to influence other relevant socioeconomic outcomes. This suggests that sibling sex composition may be useful as an instrument in estimating non-market returns to education, such as delays in teenage pregnancy. So, in summary, the key take away regarding Table 5, which is the exclusion restriction checks is that sibling composition shows no systematic association with the other social and economic outcomes that could reach fertility on their own, which is reassuring for the exclusion restriction, though it speaks only to the channels that are observable.

5. Education and Early Fertility: Sibling Sex Composition as an Instrument

5.1 Justifying the Instruments and Tackling Endogeneity

A simple OLS regression of teenage pregnancy incidence on education raises substantial endogeneity concerns. Such a regression omits unobservable variables such as preferences, discount rates, parental care, and wealth, each of which may influence both the endogenous regressor and the dependent variable. For example, individual preferences or discount rates may positively affect educational attainment - if patience levels are unobserved and high, but while negatively affecting a female's pregnancy decisions. This would overstate the negative impact of education on teenage pregnancy, and thus the nonmarket return to education. Beyond omitted variable bias, the research question also carries the risk of bias from potential simultaneous causation between schooling and early fertility. Measurement error poses a further risk: completed education may be reported with error, leading to attenuation bias in the estimated nonmarket return. If completed education is underreported, measurement error may appear in the pregnancy regression as a higher likelihood of teenage pregnancy, and hence a lower nonmarket return to education. To address these potential biases, the instrumental variables methodology is used. In an ideal setting, a randomized controlled trial would be preferable for inferring causality, but such a design is highly challenging for a fertility related question of this kind.

Specifically, the results in **Table 3** and **Table 4** suggest that sibling sex composition may be useful as an instrument in studying the effects of education on teenage pregnancy incidence. The first-stage analysis in **Table 3** confirms the relevance of all the potential instruments, such as, the any sisters indicator, the number of siblings, and the number of siblings squared, to years of education completed for young adult females. All three are significantly correlated with years of completed education for adolescent females, satisfying the relevance condition required of a valid instrument. The exclusion restriction requires that sibling sex composition not appear in the causal model of interest in its own right, and hence that it be excluded from the teenage pregnancy regression. Alternative channels through which the instruments might influence the outcome are examined in **Table 5**. The channels checked there are not, of course, the only possible stories; further mechanisms could contaminate the second stage and cast doubt on the exclusion restriction. Because one's sibling sex composition is unlikely to be correlated with personal attributes other than through the household upbringing itself, sibling sex composition generates exogenous variation in education that can be used to estimate the impact of education on teenage pregnancy.

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Claiming that a female's sibling sex composition is entirely exogenous may appear a strong assumption. One could argue that in Cape Town individuals have brothers or sisters for underlying reasons, or that parents prefer to have daughters or sons for particular reasons - preferring sons to contribute to household income through labour force participation, or daughters to help with domestic work. Parents may have control over the number of children they have, but their control over the gender composition of those children is likely to be very limited. In this specific context it therefore seems reasonable to treat the exogeneity requirement as satisfied. The exogeneity assumption is nevertheless untestable, given the unobservable determinants of teenage pregnancy such as discount rates and preferences. The sexes of siblings may be used as instruments in measuring the nonmarket return to education if they help determine the amount of education a female receives while remaining orthogonal to her preferences or discount rates. Equivalently, sibling sex composition may serve as an instrument if it is correlated with educational attainment and uncorrelated with the source of the bias, whether omitted variables or measurement error. Patience level bias - omitted and unobserved high patience appearing as lower teenage pregnancy probability through more education - and measurement error - appearing as higher teenage pregnancy probability through underreported schooling - may have offsetting effects on the noninstrumented estimate. Whether the OLS estimate is too large or too small is therefore an empirical matter.

Therefore, the below **Table 6** presents results of teenage pregnancy regressions, with and without instrumenting for years of completed education. The dependent variable is the incidence of teenage pregnancy, constructed from the data using both ever having been pregnant and ever having reported a live birth by the last wave.

Table 6. Second stage: OLS and two-stage least squares estimations of education's impact on fertility, young adult females

	Dependent variable: incidence of teenage pregnancy			
Explanatory variables	**OLS (1)**	**Reduced form (2)**	**TSLS (3)**	**TSLS (4)**
Years of education	−0.247***	—	−0.028	−0.063
	(0.041)		(0.087)	(0.093)
Any sisters	−0.076	−0.014	—	—
	(0.152)	(0.146)		
Number of siblings	0.282	0.033	0.011	—
	(0.305)	(0.295)	(0.034)	
Number of siblings squared	−0.051	−0.001	−0.004	—
	(0.061)	(0.060)	(0.010)	
Control variables (as in first stage)	Yes	Yes	Yes	Yes
Observations	1,653	1,653	1,653	1,653
R^2	0.278	0.228	—	—
Hansen J overidentification test (p-value)	—	—	—	Cannot reject (0.553)

Notes: Robust standard errors in parentheses; ***p < 0.01, **p < 0.05, *p < 0.1. The sample comprises young adult females aged 18–26 inclusive with a positive number of years of education and at least one sibling. All regressions are weighted using sample weights and are cluster-adjusted. Controls include an intercept, age, age², age³, and indicators for religion, poor household, oldest child, race, and parents' highest level of education completed. The binary dependent variable equals 1 if the respondent ever reported a pregnancy or live birth by the last wave in 2006; = 0 otherwise. Column 3 instruments years of education with the any sisters indicator alone. Column 4 instruments with any sisters, the number of siblings, and the number of siblings squared. The reduced form is reported as a robustness check: an instrument added to the outcome regression of interest should have no explanatory power. The Hansen J statistic reports the chi-squared(2) p-value for the test of overidentifying restrictions; the null that the instruments are uncorrelated with the second-stage residual cannot be rejected. *Source:* Author's calculations using CAPS, Waves 1–4 (2002–2006).

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The first column above presents an OLS estimate of the nonmarket return to education for adolescent females in Cape Town. The estimated reduction in the likelihood of teenage pregnancy of roughly 25 percent is large and negative. An indicator that the young adult female had a sister is insignificant in the teenage pregnancy regression once completed education is included as an explanatory variable. The reduced form estimation, with completed education omitted, suggests that the presence of a sister insignificantly reduces the likelihood of teenage pregnancy by roughly 0.014 ($p = 0.91$), regardless of the underlying explanation. The OLS and reduced form columns confirm that the any sisters indicator does not appear in its own right in the teenage pregnancy regression, which is reassuring for the exclusion restriction. The number of siblings and the number of siblings squared likewise have no explanatory power in the second stage. Two sets of instrumental variables estimates are presented. In column 3, years of completed education is instrumented using only the any sisters indicator. The nonmarket return to education remains a reduced likelihood of teenage pregnancy, but this negative effect is not significant (coefficient −0.028, $p = 0.82$). The second set of instruments comprises the any sisters indicator, sibship size, and sibship size squared. The IV estimate remains highly insignificant in this specification, though slightly more negative (coefficient −0.063, $p = 0.50$).

In summary, the key take away from the Table 6, which is the second stage analysis of education on early fertility, is that once education is instrumented, its effect on early childbearing between ages 18 and 26 is insignificant, while the large negative ordinary least squares estimate reveals itself as substantially biased, pointing to omitted variables rather than measurement error as the source of the gap.

5.2 Reliability and Endogeneity

Subsequently, at least two complications remain for the two-stage least squares results. The first concerns the exclusion restriction itself. Sibling sex composition may reach teenage pregnancy through an unobservable characteristic, including a young woman's preferences or the discount rate through which she weighs present and future outcomes. An indicator for the presence of a sister would then have its own route into the second-stage equation. If sisters, brothers, or household size altered preferences towards childbearing, expectations around marriage, or the weight attached to future outcomes, some independent relationship with teenage pregnancy incidence might be expected. **Table 5** provides little observable evidence of such a pathway in Cape Town. Reassuring, up to a point. The table can look only where the data allow it to look. Currently working, ever married, and expected age at first marriage offer imperfect glimpses of preferences and intertemporal choices. The underlying preferences and discount rates remain unobserved. The exclusion restriction therefore remains an identifying assumption. Table 5 provides evidence around its plausibility. It cannot establish it.

The second complication concerns the large distance between the OLS and TSLS estimates. Classical measurement error in schooling would ordinarily attenuate the estimated education coefficient towards zero. The movement here goes in the other direction. The TSLS estimates in **Table 6** are considerably less negative than the OLS estimates, and the statistically significant relationship found under OLS disappears after instrumenting. This pattern is more consistent with downward omitted variable bias arising from characteristics associated with both schooling and teenage pregnancy incidence. Discount rates are one candidate. Preferences another. Family circumstances imperfectly captured by the available controls could do similar work. The OLS coefficient appears to be carrying rather more than education. Moreover, there is also a substantive reason to take the IV null seriously. Fertility decisions may be made within relationships and households whose preferences do not coincide neatly with those of the individual woman whose schooling is being instrumented. Van Hoyweghen et al. (2025) show in rural Senegal and Uganda that women's fertility preferences do not simply stand in for household choices, while the location of decision-making itself varies across settings. Ji et al. (2015) similarly find that intergenerational ties and parental influence shape childbearing intentions in a low fertility context.

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Schooling may alter what a young woman knows, expects, or wants. The eventual fertility decision can still have other people in it. This offers one plausible context for interpreting a weak instrumented relationship between education and early childbearing. The Cape Town data cannot identify that mechanism directly.

The comparison with earlier causal studies also requires some precision about the outcome being estimated. Osili and Long (2008) find a substantially larger TSLS than OLS effect of female schooling on births before age 25 in Nigeria, supporting a causal reduction in early fertility. Breierova and Duflo (2004) find education important for very early fertility before age 15 in Indonesia, with little evidence of an effect before age 25. Other natural experiments produce estimates that vary across settings and definitions of fertility (Black et al. 2008; McCrary & Royer, 2011; Fort, Schneeweis, & Winter-Ebmer, 2016). The Cape Town estimates fall at the weaker end of this literature: Table 6 provides no statistically significant evidence that additional schooling reduces teenage pregnancy incidence among the young women in this sample. And teenage pregnancy is the outcome here. Births before age 25, fertility timing, completed fertility, and the distribution of births capture other parts of the reproductive life course. Recent work makes the distinction consequential (DeCicca & Krashinsky, 2023; Vasireddy et al., 2023). The Cape Town estimates should therefore be read for what they identify: the relationship between instrumented schooling and teenage pregnancy in this population and period.

All things considered, the direction of movement from OLS to TSLS gives little reason to regard measurement error in schooling as the main source of the divergence. OLS produces the more negative coefficient. Instrumenting sharply reduces the estimated relationship and removes its statistical significance. Downward omitted variable bias is the more plausible interpretation, arising from characteristics associated with both educational attainment and teenage pregnancy incidence that remain outside the observed controls. Some can be named, preferences, discount rates, family circumstances. Others cannot. That uncertainty belongs in the interpretation of the estimate.

6. Discussion

The Cape Town results refuse the symmetry implied by a simple model of household resource allocation. Male schooling is largely unmoved by sibling sex composition. Female schooling responds. Young women raised with any sisters complete 0.78 fewer years of education, holding household size and family background constant. The any brothers coefficient is small, negative, and statistically insignificant. This differs from the influential Butcher and Case (1994) finding that women raised only with brothers receive more education than women raised with sisters. Cape Town supplies considerably firmer evidence of a sister effect than of any corresponding brother advantage. That is a vital distinction, as a straightforward resource dilution account would suggest that additional children place pressure on a finite household budget and that schooling responds accordingly. The Cape Town pattern is more peculiar. Sons' schooling barely responds to sibling sex composition. Daughters' schooling does. Birth order offers little explanation either, consistent with Hauser and Sewell (1985). Something more particular appears to be happening around daughters and around the conditions under which their education is allocated.

Especially the negative sister coefficient needs careful interpretation. Girls are not somehow producing other girls' educational disadvantage. Sisters expose a mechanism they did not create. Their presence changes the household configuration within which educational resources, responsibilities, expectations, and perhaps future returns are assessed. Garner and Manuel-Scott (2026) are useful here because they place sibling relationships back inside the wider family and cultural systems that give those relationships meaning. A sister is never merely another female child added to a regression. She enters a household

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already carrying ideas about daughters, care, domestic labour, marriage, paid work, and what an education for a girl is eventually expected to yield. The gender inequality may therefore lie partly in *adjustability*. One child's educational investment can be relatively protected while another child's becomes available for revision when household circumstances change. In Cape Town, female schooling appears to occupy the more negotiable position. Another daughter enters the household and something shifts around daughters. The data cannot reveal precisely what. Domestic responsibilities may be redistributed. Expected returns may change. Parental aspirations may move. Ideas about marriage or work may enter. The empirical result can establish whose schooling moves with sizably greater confidence than it can establish why.

This is also where Cape Town speaks to the instability in the wider sibling-composition literature. Butcher and Case (1994) find an apparent advantage for American women raised with brothers. Kaestner (1997) fails to reproduce that pattern in a later American cohort and finds the sign reversed among Black adults. Hauser and Kuo (1998) report weak and inconsistent evidence across datasets. Chinese evidence sends the coefficient elsewhere again, with brothers absorbing educational resources and sisters sometimes associated with greater female attainment (Zheng, 2015). Collins (2025) shows that inheritance practices mediate sibling gender effects on schooling across sub-Saharan Africa. Brenøe (2022) finds sibling gender reaching beyond years of schooling into women's later gender conformity. Barclay and Bygren (2026) locate another effect in gender typical educational choices. The drifting sign is therefore worth taking seriously. It may contain more information than a search for a universally favourable sibling configuration allows. Sibling composition enters households whose allocation rules already carry assumptions about daughters and sons. Those rules vary across place and time. So does the coefficient. What appears more persistent across several settings is the gendered sensitivity underneath it: female educational outcomes respond to sibling composition while male outcomes are often more insulated. Cape Town adds developing country evidence to that pattern.

This is one of the paper's central contributions. The question raised by sibling composition becomes larger than whether brothers or sisters are beneficial for girls' schooling. The more consequential question is *whose education becomes available for adjustment when household circumstances change.* A negative sister coefficient can then be read as evidence about the position occupied by girls' education inside the household allocation process. Educational inequality does not require girls always to receive less than boys. It can also reside in the greater contingency of girls' resources, in their exposure to changes elsewhere in the household. Girls can reach relatively high levels of schooling overall and still occupy the less protected position at the margin. **Table 4** makes that story stranger, and more interesting. Sisters reduce the probability of completing high school while increasing the probability of obtaining higher education credentials among women who reach that stage. The relationship between sisters and schooling therefore changes across educational margins. One possibility is that sisters become resources for one another later in the educational trajectory, through information, financial support, example, or labour-market participation. The data cannot distinguish among these mechanisms. Still, the reversal cautions against treating "the sister effect" as a single process operating identically throughout a young woman's education. Gendered household relations can constrain at one point and support at another. The continuous years-of-schooling measure partially conceals this movement.

The second-stage results locate the gender problem somewhere else again. Under OLS, education appears to reduce teenage pregnancy substantially. Once schooling is instrumented, the coefficient contracts sharply and loses statistical significance. Cape Town therefore joins an already unsettled causal literature. Osili and Long (2008) find a sizeable causal reduction in births before age 25 in Nigeria. Breierova and Duflo (2004) find effects concentrated at very early ages in Indonesia. Black et al. (2008), McCrary and Royer (2011), and Fort, Schneeweis, and Winter-Ebmer (2016) produce results that vary

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across institutional settings and definitions of fertility. More recent work has made the instability harder to dismiss. DeCicca and Krashinsky (2023) find education changing the distribution of fertility in Canada, while Vasireddy et al. (2023) show how educational gradients differ across fertility timing and quantum in Europe. The Cape Town estimate belongs specifically to teenage pregnancy among these young women, in this place and period. It says little about completed fertility, eventual family size, or fertility much later in the life course. Within those boundaries, however, the result has a gender implication that matters. Schooling is an individual resource. Reproductive decisions do not necessarily reside wholly with the individual woman. Van Hoyweghen et al. (2025) show how fertility preferences and decision-making authority can be distributed across partners and households. Ji et al. (2015) similarly locate reproductive intentions within intergenerational relationships. Additional schooling may change what a young woman knows, earns, expects, imagines, or wants from her future. Whether those changes reach a reproductive outcome depends partly on the social relations through which that outcome is decided.

The distance between educational attainment and the conditions under which women can use it has a longer history in the economics of gender. Goldin (2006, 2021) traces enormous gains in women's education and career expectations alongside inequalities that emerge around family formation, caregiving, and the organisation of work. The Cape Town result sits much earlier in the life course, in a very different institutional setting, with the inequality appearing long before the career versus family problem Goldin examines. The underlying question nevertheless has a family resemblance. An educational resource can expand what is possible for a young woman while the social arrangements surrounding that resource continue to shape how far it travels.

Collectively, the first and second stages locate gender inequality at two different points in the development policy story. Before education is acquired, household allocation appears to shape how securely educational resources are attached to daughters. After education is acquired, the route from schooling to reproductive outcomes passes through relationships and institutions that schooling itself does not necessarily rearrange. Education enters a gendered household at both ends of the empirical exercise. This has consequences for how female education is used in development policy. Expanding girls' access to schooling remains valuable in itself, quite apart from any effect on fertility. Yet increasing the resources available to a household does not tell us how those resources will be divided, and increasing a young woman's schooling does not tell us how much authority she will hold over decisions elsewhere in her life. Gender responsive policy therefore has reason to look at allocation rules as well as resource levels. At the domestic responsibilities that make girls' schooling interruptible. At the expectations governing what a daughter's education is thought to return. And eventually at the harder question running underneath the second stage: *what has to be true for an educational resource to become meaningful agency?*

Perhaps that is the more uncomfortable policy implication of the Cape Town results. Female education has often been asked to perform an extraordinary amount of development work. Reduce fertility. Improve child health. Raise household income. Transform the next generation. The findings here suggest that some of the constraints attributed to insufficient education may sit in the gendered relations surrounding education itself. Asking schooling to undo those relations leaves the allocation rule remarkably undisturbed.

7. Limitations and Future Research

Several limitations define how far the Cape Town results can travel. The first concerns the instrument. Sibling sex composition is plausibly exogenous to a young woman's own choices, while the exclusion

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restriction remains an identifying assumption. Table 5 provides some reassurance by showing little observable relationship between sibling composition and proxies for preferences or intertemporal choices. Those proxies are limited. Preferences, discount rates, family expectations, and other characteristics capable of influencing both schooling and early childbearing remain partly unobserved. The TSLS estimates should therefore be read as conditional on the exclusion restriction holding in this setting.

The first-stage results raise a related issue. Sibling sex composition has no universal educational meaning. The literature itself makes that clear. Brothers are associated with greater female schooling in Butcher and Case (1994), little or inconsistent change in later United States studies, and lower female attainment in parts of the Chinese literature. Cape Town produces another configuration. This limits the portability of the instrument across institutional settings. It also limits the portability of the substantive coefficient. The estimate that sisters reduce female schooling by 0.78 years describes this sample and the household allocation environment in which these young women grew up. It should not be read as a general effect of sisters on girls' education. CAPS imposes further boundaries. The sample follows young adults in metropolitan Cape Town during a particular historical period, with the analysis drawing on Waves 1 through 4 from 2002 to 2006. Cape Town is a highly unequal urban setting with a social and institutional history that gives household resources, schooling, labour market prospects, race, and gender a particular shape. Results from this population may travel imperfectly to rural South Africa, to other South African cities, or to younger cohorts whose educational and reproductive environments have changed. The longitudinal structure of CAPS is a considerable strength. Its geographical and historical specificity remains part of what the estimates mean.

The fertility outcome is narrower still. Teenage pregnancy captures one portion of the reproductive life course. It cannot establish effects on completed fertility, eventual family size, later birth timing, or the distribution of births across adulthood. This distinction matters given recent evidence that education can affect different fertility margins in different ways (DeCicca & Krashinsky, 2023; Vasireddy et al., 2023). The absence of a statistically significant IV effect on teenage pregnancy in Cape Town therefore belongs to teenage pregnancy. Extending that result to fertility more generally would ask the data to answer a larger question than the one estimated here. The analysis also observes the outcome of household allocation more clearly than the allocation process itself. The first stage identifies a gender asymmetry: female schooling responds to sibling composition while male schooling is comparatively stable. CAPS cannot show exactly what happens inside the household to produce that asymmetry. Transfers between siblings, domestic work, parental expectations, caregiving, bargaining over school expenses, and beliefs about daughters' future returns are possible mechanisms. The present data do not separate them. The interpretation developed here, that girls' educational investment may occupy a more adjustable or negotiable position within the household, should therefore be understood as an interpretation of the observed pattern rather than a directly measured household rule.

The same caution applies to reproductive agency. The second-stage null is consistent with a literature in which fertility decisions are relational and decision-making authority can extend beyond the individual woman (Ji et al., 2015; Van Hoyweghen et al., 2025). CAPS does not directly identify bargaining power or establish that household decision-making explains the weak IV estimate. That possibility helps locate the result within a wider gender literature. It remains a mechanism for future investigation. These limitations also suggest where the research can go next. Data that follow educational investments inside households, rather than observing only their eventual outcomes, would make it possible to examine how money, time, domestic labour, expectations, and caregiving shift when household composition changes. Following sisters and brothers jointly would be especially useful. So would designs capable of separating educational access from educational security: whether a child receives schooling, and how

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readily that schooling is interrupted or reduced when the household comes under pressure. The distinction may matter particularly for girls.

Future work could also connect educational allocation more directly to reproductive agency. Measures of fertility preferences alongside partner and parental preferences, bargaining power, contraceptive autonomy, marriage expectations, and control over household resources would allow the pathway from schooling to early childbearing to be examined rather than inferred. Replication across South African settings and more recent cohorts would show whether the Cape Town gender asymmetry persists as family size, schooling, labour markets, and gender expectations change. The coefficient may move again. Understanding what moves with it may prove more useful than searching for a sibling effect that stays the same everywhere.

8. Conclusion

This study begins with two questions that are often treated separately. Who receives education inside the household, and what education can subsequently be expected to do. In Cape Town, the answers meet. Female schooling responds to sibling sex composition while male schooling remains comparatively stable. Young women raised with sisters complete fewer years of education on average. Once schooling is instrumented, its large negative relationship with teenage pregnancy shrinks sharply and loses statistical significance. The first result draws attention to a form of gender inequality that average educational attainment can conceal. Girls' schooling appears more contingent on household composition, more available for adjustment as circumstances inside the family change. Sisters do not create this inequality. Their presence exposes an allocation rule under which daughters' educational investment appears less protected. Across settings, the coefficient moves. Who benefits from a particular sibling configuration changes with it. The more persistent question concerns whose education becomes negotiable in the first place. The second result takes the argument somewhere else. Education is a resource, and acquiring a resource does not settle how much power it will carry into the rest of a woman's life. The question is an old one in gender studies and social thought. Beauvoir's concern with the circumstances that limit women's freedom remains strangely close to the problem here: schooling may widen the field of possibility while the conditions surrounding a woman continue to govern what can be made of it (de Beauvoir, 1949/2011). Schooling enters reproductive decisions already crowded with relationships, household authority, economic possibilities, and institutions. How far schooling travels remains an empirical question.

For gender responsive policy, that indeed naturally matters. Expanding girls' education remains essential. So does asking who receives those educational resources, how securely they are attached to girls, and what has to be true for education to become meaningful agency. Education is asked to carry an extraordinary amount of development policy on its shoulders. Yet, as shown here, some of that weight belongs elsewhere. In the household. In the allocation rule. In the structures surrounding the girl who gets an education. Therefore, who gets what still remains a question of its own.

Appendix

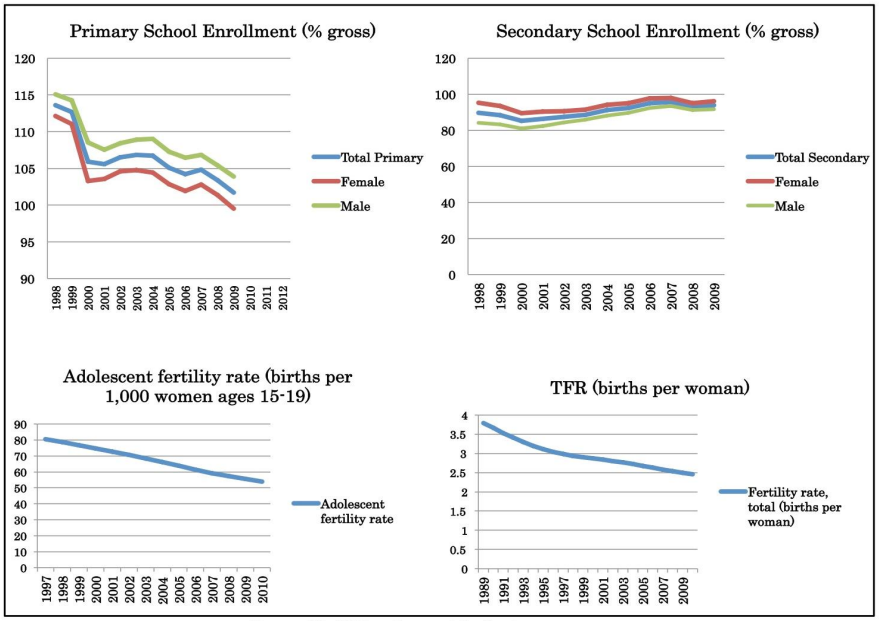
A.1 Background on South Africa: General Trends in Education and Fertility

The South African case is a fascinating one. The country has made significant progress since 1994 towards achieving gender parity in basic education. Indeed, it has gone beyond parity, to the extent that girls now make up most enrolments in secondary schools, though enrolment in primary schools remains lower for females **(Figure 1).** Pregnancy is among the major concerns that can threaten the gains

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achieved in schools (Panday et al., 2009). The incidence of teenage pregnancy undermines efforts to ensure that female adolescents remain in school. Teenage fertility in South Africa has been declining over time **(Figure 1).** Declines in overall fertility rates have been attributed, among other factors, to increased access to information on reproductive health and rights and to improved contraceptive use. Despite the reported decline in teenage fertility rates, the high proportion of unintended pregnancies among teenagers remains a serious problem (Panday et al., 2009). Pregnant teenagers face serious health, socioeconomic, and educational challenges. South Africa has the lowest fertility rate in mainland sub-Saharan Africa; while adolescent fertility has been declining over time, it has done so more slowly than overall fertility. Teenage fertility declined by 10 percent between 1996 (78 per 1,000 females) and 2001 (65 per 1,000 females), with a further decline to 54 per 1,000 females reported in the 2007 Community Survey (Panday et al., 2009; Statistics South Africa, 2008).

Figure 1. Trends in school enrolment and fertility in South Africa.



Source: World Development Indicators

Notes. Panels show primary school enrolment (% gross, 1998–2012), secondary school enrolment (% gross, 1998–2009), the adolescent fertility rate (births per 1,000 women ages 15–19, 1997–2010), and the total fertility rate (births per woman, 1989–2009).
Source: Created by the author using the World Development Indicators; Panday et al. (2009); Statistics South Africa (2008).

The key take away from Figure 1, which is the composite WDI panel (enrolment and fertility trends, South Africa) is the context for the setting, which is that South African primary and secondary enrolment sit near or above gender parity across the study window while adolescent and total fertility decline steadily, so the sample is drawn from a period of expanding schooling and falling fertility, exactly the concomitance the paper interrogates for cause.

Furthermore, **Table A1** below reports the full baseline specification: a single regression per sex of completed years of education on the entire control set. It establishes that family background - parental education, household poverty, and sibship size - significantly predicts schooling, justifying the inclusion of these controls throughout **Tables 3** to **6**. It also shows the raw effect of sibship size before the sibling sex composition variables are introduced: the number of siblings enters positively and significantly for both sexes, while the negative coefficient on its square indicates the benefit diminishes as families grow.

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Table A1. Years of completed education, by sex

	Ages 18–26 inclusive	
Explanatory variables	**Males**	**Females**
Number of siblings	0.417** (0.199)	0.595*** (0.177)
Number of siblings squared	−0.102** (0.050)	−0.128*** (0.050)
High school degree, mother	1.177*** (0.093)	1.470*** (0.085)
College degree, mother	1.547*** (0.130)	1.882*** (0.119)
Completed education, mother	0.223*** (0.016)	0.223*** (0.013)
High school degree, father	1.324*** (0.096)	1.451*** (0.084)
College degree, father	1.550*** (0.125)	1.812*** (0.111)
Completed education, father	0.198*** (0.016)	0.203*** (0.012)
Oldest sibling	0.154 (0.162)	−0.021 (0.154)
Poor household	−0.202** (0.092)	−0.556*** (0.159)
Father professional	0.071 (0.088)	0.025 (0.072)
Father laborer	−1.093** (0.491)	−0.097 (0.409)
Mother works	0.112** (0.056)	0.142 (0.121)
Catholic	0.522*** (0.178)	0.301** (0.140)
White	0.545*** (0.081)	0.781*** (0.096)
Age	0.132*** (0.033)	0.055* (0.028)
Observations	**1,570**	**1,880**

Notes: Dependent variable: years of completed education. Robust standard errors in parentheses; ***p < 0.01, **p < 0.05, *p < 0.1. All regressions are weighted using sample weights. The sample is restricted to adolescents with completed education greater than zero and at least one sibling. Mother high school degree = 1 if the mother's highest level of education is Grade 12 or above; = 0 otherwise. Mother college degree = 1 if the mother's highest level of education is Grade 16 or above (diploma or certificate programs); = 0 otherwise. Father's education is defined analogously. *Source:* Author's calculations using CAPS, Waves 1–4 (2002–2006).

The key take away from Table A1, which is the full baseline regression, by sex, is that the diagnostic that justifies the control set, which is that family background predicts schooling strongly for both sexes, and the raw sibship size effect is positive and significant here, before the composition variables enter and reshape it in **Table 3** in section 4 above.

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