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Josh Angrist
Victor Lavy
Analía Schlosser

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Forschungsinstitut
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Institute for the Study
of Labor

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Josh Angrist
MIT, NBER and IZA Bonn

Victor Lavy
Hebrew University and NBER

Analía Schlosser
Hebrew University

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IZA

P.O. Box 7240
53072 Bonn
Germany

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

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ABSTRACT

New Evidence on the Causal Link between the Quantity and Quality of Children*

A longstanding question in the economics of the family is the relationship between sibship size and subsequent human capital formation and economic welfare. If there is a causal "quantity-quality tradeoff," then policies that discourage large families should lead to increased human capital, higher earnings, and, at the macro level, promote economic development. Ordinary least squares regression estimates and a large theoretical literature suggest that this is indeed the case. This paper presents new evidence on the child-quantity/child-quality trade-off. Our empirical strategy exploits exogenous variation in family size due to twin births and preferences for a mixed sibling-sex composition, as well as ethnic differences in the effects of these variables and preferences for male births in some ethnic groups. We use these sources of variation to look at the causal effect of family size on completed educational attainment, fertility, and earnings. For the purposes of this analysis, we constructed a unique matched data set linking Israeli Census data with information on the demographic structure of families drawn from a population registry. Our results show no evidence of a quantity-quality trade-off, though some estimates from one subsample suggest that first-born girls from large families marry sooner.

JEL Classification: J13, J24, O12

Keywords: fertility, quantity-quality trade-offs, instrumental variables

Corresponding author:

Joshua D. Angrist
Department of Economics
MIT
50 Memorial Drive
Cambridge, MA 021242-1347
USA
Email: angrist@mit.edu

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FAMILY PLANNING: THE WAY TO PROSPERITY.
(A SLOGAN FOUND ON THE BACK OF INDONESIA'S FIVE-RUPIAH COIN)

I. Introduction

The question of how family size affects economic circumstances is one of the most enduring in social science. The earliest theoretical discussion of the role of family size in the determination of living standards was probably by Malthus, who famously argued that family size responds to income shocks in a manner that keeps living standards at a constant subsistence level. Malthusian stagnation is a crude general equilibrium phenomenon, divorced from any optimizing behavior. Beginning with Becker and Lewis (1973) and Becker and Tomes (1976), however, economists developed a rich theoretical framework that sees both the number of children and parental investment per child as household choice variables that respond to economic forces. Part of this agenda is an attempt to reconcile the apparent paradox of declining family size in the face of economic growth with the superficially plausible presumption that children are a normal good. The notion of a quantity-quality trade-off appears to provide this reconciliation: as parents get richer they demand children of higher "quality," (i.e., children who are more productive), without necessarily demanding more of them. In fact, because increases in quality can be interpreted as making children more expensive, the quantity-quality trade-off explains why families might get smaller as parents get richer.

On the policy side, the notion that smaller families and slower population growth are essential for economic development motivates many governments and international agencies to promote, or even to require smaller families.[1] While this policy position often seems to be based on naive empiricism, both the Malthusian and the Becker and Lewis (1973) models provide some theoretical support for the view that large families keep living standards low. A negative causal relation between child quantity and

[1] In addition to China, examples of government-sponsored family planning efforts include a forced-sterilization program in India and the aggressive public promotion of family planning in Mexico and Indonesia. These episodes are recounted in Weil (2005; Chapter 4), which also mentions the antinatalist slogan on the Indonesian Rupiah. Bongaarts (1994) notes that by 1990, 85 percent of people in the developing world lived in countries where the government considered the rates of fertility too high.

parental investment also comes out of a number of sophisticated theoretical recent analyses of the role of the demographic transition in economic development (e.g., Galor and Weil, 2000; Hazan and Berdugo, 2002, and Moav, 2005). On the other hand, newer theories focus primarily on the quantity and quality implications of human capital accumulation. In these models, the effects of population-control efforts and similar policy interventions are less clear cut. Moreover, with perfect capital markets, parental investment in their children's human capital should be unresponsive to family size.

Most of the scholarly evidence pointing to an empirical quantity-quality trade-off comes from the widely observed negative association between family size on one hand and schooling or academic achievement on the other. For example, Leibowitz (1974) and Hanushek (1992) find that children's educational attainment and achievement growth are negatively correlated with family size. Many other micro-econometric and demographic studies show similar relations.[2] The principal problem with research of this type is that such associations need not be indicative of a causal relation. The fact that people raised in large families end up with less schooling than those raised in smaller families need not be due to family size *per se*. Rather, this correlation may simply reflect differences in parental education, earnings potential, or other unobserved factors that affect both fertility and the home environment. The likelihood of omitted variables bias in estimates of the effects of childbearing is highlighted by Angrist and Evans (1998), who used multiple births and preferences for a mixed sibling-sex composition to construct instrumental variables (IV) estimates of the effect of family size on mothers' labor supply. IV estimates using both twins and same-sex instruments, while still negative, are considerably smaller than the corresponding OLS estimates.

This paper provides new evidence on the quantity-quality trade-off using exogenous variation in family size. In particular, we look at the effect of third and higher-order births on first- and second-born

[2] See, e.g., the recent review by Schultz (2005). Johnson (1999) notes that the relation between family size and economic well being or growth is less clear cut at the time series or cross-country level. In contrast with Hanushek (1992), Guo and Vanwey (1999) show that control for family effects eliminates the relation between sibship size and intellectual development.

children's completed schooling, adult earnings, and on marital status and fertility. These are all important long-run "quality" indicators that are likely to be affected by the home environment. Two of the instruments used here, as in Angrist and Evans (1998), are dummies for multiple births at second birth and a dummy for same-sex sibling pairs in families with two or more children.

We also extend the sex-composition and twins identification strategies in a number of ways. First, in addition to looking at families with two or more children, we exploit multiple third births and the effects of sibling-sex composition in families with three or more children. We then introduce a new source of exogenous variation in family size based on sharp differences in the effects of multiple births and sex-composition across ethnic groups in the Israeli population. For example, multiple births have a much larger effect on Jews of European or North American origin than on Jews of Asian or North African origin, since the latter chose to have large families even in the absence of a multiple birth. On the other hand, an all-female sibling sex composition leads to sharp rise in the number of children born to the Asia-Africa group, with relatively little effect on the fertility of ethnic Europeans and Israeli natives. Finally, we exploit preferences for boys at higher order births in the Asia-Africa subsample.[3]

As a result of this rich variation, our sample and identification strategies allow us to juxtapose the results from a number of different groups, using fertility shocks of different sorts and sizes, and over differing ranges of variation. Remarkably, all of this evidence points in the same direction: exogenous increases in family size have little effect on first and second born children, with the possible exception of an increase in the likelihood that older girls marry.

Our paper is related to a burgeoning empirical literature attempting to link multiple births with measures of child quality. Rosenzweig and Wolpin (1980) appear to have been the first to use multiple births to estimate a child-quantity/child-quality trade-off. More recent estimates using multiple births

[3] Traditional Jewish preferences over sibling sex-composition can be traced back to the Mishna (Oral law): A man shall not stop having children until he has two. Beit Shamai (a relatively strict rabbinic tradition) says two sons, while Beit Hillel (a more forgiving rabbinic tradition) says a boy and a girl. As it is written in Genesis, 'male and female he created them.' (Mishna Nashim - Yebhamoth 6:7).

include Duflo (1998), who looks at effects on child mortality in Indonesia; Black, Devereux, and Salvanes (2004), who estimate effects on education in Norway; and Caceres (2004), who looks at effects on private school enrollment in US Census data. Caceres (2004) also estimates effects on dropout status, teen pregnancy, and parents' marital status.

An important difference between our study and these earlier papers is that we have a wider range of outcomes than has been previously available for a research design of this type. Our outcomes come from a unique data set we constructed for the purposes of this project linking the 1983 and 1995 Israeli Census micro samples, which provide information on education, work, earnings, marriage, and childbearing, with detailed sibling information from the population registry. A second key difference is that the ethnic diversity of the Israeli population allows us to compare effects across families of different sizes and from different cultural traditions. Of particular interest here are results for the Asia-Africa subsample, that is, Sephardic Jews of North African and Middle Eastern origin. Those in this group have demographic and social characteristics much closer to developing country populations than do native-born Israelis or Israelis of European and North American stock.[4] Finally, ours appears to be the first study to use sibling-sex composition to estimate the quality-quantity trade-off for a wide range of outcomes, or with a strong and well-documented first-stage.[5]

The paper is organized as follows. The next section describes the census-registry link and the construction of our more-than-two (2+) and more-than-three (3+) analysis samples. Section III discusses first-stage estimates and Section IV presents the main OLS and 2SLS results. We discuss the relation between our findings and earlier work in Section V, focusing on the question of whether the case for an

[4] Overall, Israel circa 1975, when the subjects we study were growing up, was an upper middle income country, with GDP per-capita about like that in Greece and Argentina; see Heston, Summers, and Aten (2002).

[5] Black, Devereux, and Salvanes (2004) briefly mention a failed effort in this direction. Conley and Glauber (2004) report estimates using sex-composition IVs, looking at grade retention and private school attendance, though problems with their research design make their results hard to interpret. Lee (2004) uses preferences for male children to construct an instrument for family size in Korea, where preferences for boys (as opposed to a mixed sibling-sex composition) are strong.

empirical quantity-quality trade-off should be evaluated in terms of parental inputs or child outcomes. Finally, Section VI concludes and suggests directions for further work.

II. Data and Samples

The main sources of data used here are the 20% public-use micro-data samples from the 1995 and 1983 Israeli censuses, linked with information on parents from the population registry. The Israeli census micro files are 1-in-5 random samples that include information collected on a fairly detailed long-form questionnaire similar to the one used to create the PUMS files for US censuses.[6] The set of Jewish long-form respondents aged 18-60 provides our initial study sample. In the discussion that follows, we refer to these individuals as "subjects," to distinguish them from their parents and siblings, for whom we also collected data. Information on parents and siblings was obtained from the Israeli population registry maintained by the Ministry of the Interior. Conditional on confidentiality review, the registry is available for use on a per-project basis inside the Central Bureau of Statistics (CBS) in a restricted-access Research Data Center. The link from census to registry is necessary for our purposes because in a sample of adult respondents, most of whom no longer live with their parents and siblings, the census provides no information about sibship size, multiple births, or sibling sex composition.[7]

The Israeli population registry, our source of information on families of origin, contains updated administrative records for Israeli citizens and residents, whether currently living or dead, including most Israelis who have moved abroad. This data base also includes the Israeli ID numbers held by citizens and temporary residents. ID numbers are issued at birth for the native-born and upon arrival for immigrants. In addition to basic demographic information on individuals (date of birth, sex, country of birth, year of

[6] Documentation can be found at the Israel Social Sciences Data Center web site: http://isdc.huji.ac.il/mainpage_e.html (data sets 115 [1995 demographic file] and 301 [1983 files]). The Census includes residents of dwellings inside the State of Israel and Jewish settlements in the occupied territories. This includes residents abroad for less than one year, new immigrants, and non-citizen tourists and temporary residents living at the indicated address for more than a year.

[7] About 80% of the Israeli population is Jewish. The study sample is limited to Jews because census-to-population-registry match rates are considerably lower for other groups.

immigration, marital status, religion and nationality), the registry records parents' names and registrants' parents' ID numbers.

The construction of an analysis file proceeded by first using subjects' ID numbers to link to non-public-use versions of census long-form files that include ID numbers with registry records for as many subjects as we could find. In a second step, we used the registry to find subjects' mothers. Finally, once mothers were linked to census respondents, we then located all the mothers' children in the registry, whether or not these children appear in the census. In this manner we were able to observe the sex and birth dates of most adult census respondents' siblings.

Match Rates and Sample Selection

Although coverage rates are reasonably good, not all census respondents appear in the population registry. Moreover, among those who can be found, information may be missing for mothers, and among those with mothers in the registry, some or all siblings may be missing. The likelihood of successful matches at each stage of our linkage effort is determined primarily by the inherent coverage limitations of the registry. Israel's population registry was first developed in 1948, not long after the creation of the state of Israel. Census enumerators went from house to house, simultaneously collecting information for the first census and for the administrative system that became the registry. Later, the registry was updated using vital statistics data. Thus, in principle, the sample of respondents available for a census interview in 1983 and 1995 should appear in the registry, along with their mothers' ID numbers, if they were resident in Israel in 1948, born in Israel after 1948, or immigrated to Israel after 1948.

The vast majority of our subjects do indeed appear in the registry. This can be seen in Table 1, the first two rows of which report starting sample sizes and subject-to-registry match rates, grouped according to whether subjects' parents were Israeli born, birth cohort, and whether subjects were Israeli-born (there are two panels in the table, one for each census). Subject-to-registry match rates range from 95-97 percent regardless of cohort and nativity. The first coverage shortfall from our point of view is the failure to obtain an administrative record for subjects' mothers. This failure arises for a number of reasons. First, subject's mothers may have been alive but not at home in 1948 when the registry was created, or the mother may have been deceased. Second, and more importantly for most of our subjects, children are linked to mothers at the time they are born. We are therefore most likely to locate all of a subject's siblings when the subject's mother gave birth to all of her children in Israel.

The second row of each panel in Table 1 describes the impact of these record-keeping constraints on our census-to-registry match rates. The mothers of subjects with Israeli-born fathers were found 90 percent of the time for cohorts born after 1955. On the other hand, for those born before 1955, only 17 percent of mothers were found. Likewise, for those with foreign-born fathers, there is a similar age gradient in mothers' match rates. Even in this group, however, 87 percent of mothers were found for younger Israeli-born subjects in the 1995 census. The 1955 birth cohort marks a useful division for our purposes because mothers of subjects born after 1954 gave birth to most of their children in post-1948 Israel (the mothers in this group were mostly born after 1930, and, assuming childbirth starts at 18, this dates their first births at 1948 or later).

Given the match rates in Table 1, our analysis sample is clearly weighted towards post-1955 cohorts (i.e., 40 or younger in 1995). This accounts for about two-thirds of the 1995 population aged 18-60. Among the children of immigrant fathers, we're also much more likely to find mothers of the Israeli-born. The coverage rates for post-1955 Israeli-born cohorts seem high enough that we are likely to have information on mothers for a representative sample of younger cohorts regardless of fathers' nativity. For the purposes of analysis, we also used information on mothers in the matched sample to discard any remaining mothers who were born before 1930 (as the match rates for this group appeared to be very low anyway). Subjects with mothers whose first birth was before age 15 or after age 45 were also dropped. These further restrictions eliminate almost all subjects born before 1955, primarily because most of those born earlier have mothers born before the 1930 maternal age cutoff. We also restricted the sample of subjects with foreign-born mothers to those whose mothers arrived 1948 or later and before the age of 45 (in this case so that an immigrant mother with children is likely to have come with all her children, who would then have been included in the registry, either in the first census, or at the time IDs were issued to the family).

The final sample restriction retains only first and second-born subjects since these are the people exposed to the natural experiments exploited by the twins and sex-composition research designs Note that the restriction to first and second born subjects naturally eliminates a higher percentage of younger rather than older cohorts. This restriction also has a bigger effect on the Israeli-born children of foreign-born fathers than on other nativity groups, probably because these children were disproportionately likely to have been born to immigrant fathers who arrived with a large wave of immigrants from Asia and Africa in the 1950s. Immigrants from this group typically formed large families after arrival and will therefore have contributed more higher-parity births to the sample.[8]

Description of Analysis Samples

We work with two analysis samples, both described in Table 2. The first sample consists of first-born subjects in families with two or more *births* (the 2+ sample, N=89,445). The second sample consists of first- and second-born subjects in families with three or more *births* (the 3+ sample, N=65,671 first-born and 53,070 second-born). These samples are defined conditional on the number of births instead of the number of children so that multiple-birth families can be included in the analysis samples without affecting the sample selection criteria. Twin subjects were dropped from both samples, however.[9]

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[8] A possible concern in this context is whether match rates are correlated with the twins and sex composition instruments. We cannot check this directly because the instruments can be constructed only for those who are matched. We note, however, that outcome variables are reasonably similar for matched and unmatched individuals in the census files. While there are some differences in outcomes by match-status, these differences are small, variable, and sometimes insignificant. Since, as we show below, the results are consistent across all outcomes, it seems unlikely that selection bias due to differential matching is an important factor.

[9] A 3+ sample defined as including first-born children from families with three or more children instead of three or more births would include all families with multiple second births. Likewise, sibling-sex composition can be defined across births without the need to determine which, say, of two twins, constitutes the second child.

Roughly three-quarters of the observations in each sample were drawn from the 1995 Census. On average, subjects were born in the mid-sixties and their mothers were in their early twenties at first birth. Because out-of-wedlock childbearing is rare in Israel, especially among the cohorts studied here, virtually all subjects in both samples were born to married mothers. Naturally, however, some marriages have since broken up and some wives have been widowed. This is reflected in the 2003 marital status variables available in the registry.[10]

The Jewish Israeli population is often grouped by ethnicity, with Jews of African and Asian origin (AA; e.g., Moroccans), distinguished from Jews of European and North American (EA) origin. The 2+ sample is about 40 percent AA (defined using father's place of birth), while the 3+ sample is over half AA. A preference for larger families in the AA population is also reflected in the statistics on numbers of children. Average family size ranges from 3.6 in the 2+ sample to 4.2 in the 3+ sample (4.3 for second-borns). In the AA subsample, however, the corresponding family sizes are about 4.3 and 4.7.

Table 2 also reports statistics on the variables used to construct instrumental variables. The twin rate was 9/10 of one percent at second birth in the 2+ sample and 1 percent at third birth in the 3+ sample, with similar rates in the AA and full samples.[11] As expected, about 51 percent of births are male, regardless of birth order. Consequently, about half of the 2+ sample was born into a same-sex sibling pair and about one-quarter of the 3+ sample was part of a same-sex threesome.

The outcome variables described in Table 2 measure subjects' educational attainment, labor market status and earnings, marital status and fertility, and the characteristics of subjects' spouses. Most Israelis are high school graduates, while 20 percent are college graduates. In the AA subsample,

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[10] The 2+ sample of first-borns naturally includes the 3+ sample of first-borns. In the 3+ sample, about 10 percent of the first- and second-borns have the same mother (both must appear in the 20% census sample and be in the relevant age range). We therefore cluster analyses that pool parities by mothers' ID.

[11] Note that the second-birth twin rate in the 3+ sample is not comparable to the second birth twin rate in the 2+ sample or the third-birth twin rate in the 3+ sample because the 3+ sample consists of those who had three or more births. Families with a second-born twin need not have a third birth to have three or more children. Families with a second-born twin that have a third birth have at least four children, and hence are relatively rare in the 3+ sample.

however, proportion of college graduates is much lower. Most of our subjects were working at the time they were interviewed and earned about 3000 shekels (about 1000 dollars) per month on average (including zeros). About 40 percent of subjects were married, though marriage rates are higher in the AA subsample. Table 2 also reports select descriptive statistics on spouses' characteristics in the sample of married subjects.

III. First-stage Estimates

The twins and sex-composition first stages are described below in turn. Because the sex-composition model is somewhat more complicated in the 3+ sample, these estimates are discussed in a separate subsection.

*Twins First-Stage*s

A multiple second birth increases the number of siblings in the 2+ sample by about half a child, a statistic reported in column 1 of Table 3, which shows first-stage estimates for the twins experiment. In particular, column 1 reports estimates of the coefficient α in the equation

$$c_i = X_i'\beta + \alpha t_{2i} + \eta_i \qquad (1)$$

where c_i is subject i's sibship size (including the subject), X_i is a vector of controls that includes a full set of dummies for subjects' and subject's mothers' ages, Mothers' age at first birth, mothers' age at immigration (where relevant), fathers' and mothers' place of birth, census year, and a dummy for missing month of birth. The variable t_{2i} indicates multiple second births in the 2+ sample.

The Israeli twins-2 first stage is smaller than the twins-2 first stage of about .6 in the AE-98 sample, reflecting the fact that Israelis typically have larger families than Americans. Multiple births result in a smaller increase in family size when families would have been large even in the absence of a multiple birth. Within Israel, however, there are marked differences in the twins first-stage by ethnicity.

This can be seen in column 2 of Table 3, which reports the twins-2 main effect and an interaction term between twins-2 and a dummy for Asia-Africa ethnicity (a_i) in the equation

$$c_i = X_i'\beta + \alpha_0 t_{2i} + \alpha_1 a_i t_{2i} + \eta_i. \tag{2}$$

The twins-2 main effect, α_0, captures the effect of a multiple birth in the non-AA population, while the interaction term, α_1, measures the AA/non-AA difference.[12] The estimates in column 2 show that non-AA family size goes up by about .63 in response to a multiple birth (similar to the AE-98 first stage), while AA family size increases by only .63-.45=.18. Both α_0 and α_1 are very precisely estimated.

The remaining columns of Table 3 report the first-stage effect of a multiple third birth in the 3+ sample. Twins-3 effects were estimated in the 3+ sample by replacing t_{2i} with t_{3i}, a dummy for multiple third births, in equations (1) and (2). These results are reported in columns 3-4 for first-borns and columns 5-6 for the pooled sample of first- and second-borns. The first stage effect of a multiple birth is bigger in the 3+ sample than in the 2+ sample because the desire to have additional children diminishes as family size increases. For the same reason, the effect of t_{3i} differs less by ethnicity in the 3+ sample than in the 2+ sample, though, as the estimates in column 6 show, there is still a significant difference by ethnicity when first and second born subjects are pooled.

Sibling-Sex Composition First-stage in the 2+ Sample

The sex-composition first stages in the 2+ sample are based on the following two models:

$$c_i = X_i'\beta + \gamma_1 b_{1i} + \gamma_2 b_{2i} + \pi_s s_{12i} + \eta_i \tag{3a}$$

$$c_i = X_i'\beta + \gamma_1 b_{1i} + \pi_b b_{12i} + \pi_g g_{12i} + \eta_i \tag{3b}$$

where b_{1i} (boy-first) and b_{2i} (boy-second) are dummies for boys born at first and second birth, the variable

$$s_{12i} = b_{1i}b_{2i} + (1-b_{1i})(1-b_{2i}),$$

[12] The a_i main effect is included in the vector of covariates, X_i.

is a dummy for same-sex sibling pairs, and

$$b_{12i} = b_{1i}b_{2i} \text{ and } g_{12i} = (1\text{-}b_{1i})(1\text{-}b_{2i})$$

indicate two boys and two girls. Note also that b_{1i} indicates the subject's sex in the 2+ sample, and that $s_{12i} = b_{12i}+g_{12i}$. The first model controls for boy-first and boy-second main effects, while the excluded instrument is a same-sex effect common to boy and girl pairs. The second model allows the effect of two boys and two girls to differ, though one of the boy main effects must be dropped since $\{b_{1i}, b_{2i}, b_{12i}, g_{12i}\}$ are linearly dependent.[13]

The same-sex first-stage effects estimated using equation (3a) in the 2+ sample, reported in column 1 of Table 4a, are on the order of .074 children. This increase is due to an increase of a little over .03 in the likelihood of having more than two children, as well as smaller increases in the likelihood of having more than 3 and more than 4 children, as can be seen in columns 4, 7, and 10, which report effects on dummies, $d_{ki}=1[c_i>k]$, for k=2, 3, and 4. Same-sex sibship at first and second birth has an impact on the likelihood of having 4 or more children because, with probability one-half at each birth, families with a same-sex sibship outcome in earlier births find themselves with an all-boy or all-girl sibship at the next birth as well. Thus, $s_{12i}=1$ shifts the distribution of fertility to the right in addition to increasing the likelihood that families have more than two children.[14]

Sex-composition effects estimated using equation (3b), allowing for separate two-boys and two-girls coefficients, are reported in columns 2 and 3 of Table 4a. In addition to allowing different effects for boys and girls, the results reported in column 3 are from models that incorporate AA interaction

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[13] For example, $g_{12i} = 1\text{-}b_{1i}\text{-}b_{2i} + b_{12i}$. Control for boy-first and boy-second main effects is motivated by the fact that the same-sex interaction term is, in principle, correlated with the main effects (Angrist and Evans, 1998) when the probability of male birth exceeds .5. In practice, however, this matters little because both the correlation is small and because the main effects are small.

[14] The first-stage effect of an instrument on c_i in the 2+ sample can be shown to be the sum of the first stage effects on d_{ki}; k=2, . . . (Angrist and Imbens, 1995). In contrast with the results reported here, Angrist and Evans (1998) found similar same-sex effects on completed fertility and on the probability of having more than two children, i.e. on d_{2i},, and therefore chose the latter as the endogenous variable of interest. This difference may be due to the fact that even in the face of a same-sex threesome, relatively few American couples are motivated to try again or because Angrist and Evans did not observe completed fertility. Because of the substantially larger first-stage for c_i in the Israeli context, we use completed fertility as the endogenous variable.

terms, as with the twins estimates discussed above. The effect of two girls is .11 (s.e.=.015) while the effect of two boys is only .039 (s.e.=.015). Models allowing different coefficients by ethnicity generate an effect of two girls equal to .086 (s.e.=.017) in the non-AA population, while the AA effect of two girls is larger by .055 (s.e.=.032). In contrast, the two boys effect is only .056 (s.e.=.017) in the non-AA population, while the AA two-boys effect is *smaller* by .042 (s.e.=.031). As a result, the AA population appears to increase childbearing in response to the birth of two girls but not in response to the birth of two boys.

The remaining columns of Table 4a show the effect of sibling sex composition on the fertility distribution for fertility increments above two children. These results are summarized in Figure 1, which reports first-stage estimates of effects of b_{12i} and g_{12i} on d_{ki} for k up to 9, along with the associated confidence bands. In the AA population, b_{12i} increases the likelihood that families have 3 or more children, with no significant effect at higher-order births. In contrast, the effect of two girls on d_{ki} actually increases from k=2 to k=3, and then tails off gradually, with a marginally significant effect on the likelihood of having 7 or more children.[15] Effects in the non-AA population drop off more sharply as the number of children increases, and are similar for two boys and two girls. If anything, the non-AA population seems to increase childbearing more sharply in response to two boys than to two girls.

Sibling-Sex Composition First Stages in the 3+ Sample

The sex-composition first-stage in the 3+ sample captures the effect of an all-boy or all-girl triple, controlling for the sex-composition of earlier births. Because a same-sex triple occurs only in families with same-sex pairs at first and second births, the model conditions on b_{12i} and g_{12i}, as well as a subject-sex main effect. An additional variable included in these models is a dummy for the sex of the third child, an effect which is defined conditional on a *mixed-sex* sibling pair at first and second birth

[15] This increase, while counterintuitive at first blush, can be attributed to the fact that in the AA population that ends up with an all-girl triple, the likelihood of having more children is very large.

(because for families with b_{12i}=1, the boy-third effect is the same as having an all-male triple, while for families with g_{12i}=1, the boy-third effect is the same as having an all-female triple). The resulting model can be written as follows (we spell out only the model that allows for separate all-male and all-female effects):

$$c_i = X_i'\beta + \gamma_1 b_i + \delta_b b_{12i} + \delta_g g_{12i} + \gamma_3(1-s_{12i})b_{3i} + \lambda_b b_{123i} + \lambda_g g_{123i} + \eta_i, \quad (4)$$

where b_{123i} and g_{123i} are indicators for all-male and all-female triples and b_i is subject sex (i.e., b_{1i} for first-borns and b_{2i} for second-borns).[16]

The first-stage effects using equation (4) in the 3+ sample and the corresponding effects after incorporating the AA interactions are reported in Table 4b. Estimates for the first-borns sample are reported in columns 1-6. The overall effect of three girls is 0.181 (s.e.=.025), double the effect of three boys, 0.092 (s.e.=.023). As in the 2+ sample, the effect of three girls is bigger in the AA population. The estimate for non-AA is .097 (s.e.=.032) and the increment for AA is .167 (s.e.=.051). In contrast, the effect of three boys is similar in the AA and non-AA population. Columns 7-8 in Table 4b report estimates of equation (4) for total fertility in the pooled sample of first- and second-borns. The estimates are similar to those obtained in the sample of first-borns only, though they are more precise due to the increased sample size.

Other columns in the table show the effect of sibling sex composition on the probability of having more than 3 and more than 4 children in the sample of first-borns and in the pooled sample of first- and second-borns. Since results in both samples are similar, we discuss effects on fertility increments for first-borns only. These results are summarized in Figure 2, which reports first-stage estimates of the effects of three-boys/girls (b_{123i} / g_{123i}) on d_{ki} for k=3 to 10, along with the associated confidence bands. The estimates are from a model that conditions on b_{12i}, g_{12i} and b_i, and are estimated

[16] This model is almost saturated in the sense that it controls for all lower-order interaction terms in the estimation of the effects of the two samesex triples except for one: in the $(1-s_{12i})b_{3i}$ term, we don't distinguish mixed sibling pairs according to whether a boy or girl was born first. A saturated model can be obtained by replacing the single term, $(1-s_{12i})b_{3i}$, with two terms, $b_{1i}(1-b_{2i})b_{3i}$ and $b_{2i}(1-b_{1i})b_{3i}$. In practice, this substitution matters little.

separately for the AA and non-AA population. In the AA population, b_{123i} increases the likelihood of having 4 or more children, with a small and marginally significant effect on the likelihood of having 5 or more children. The effect of three boys is similar in the AA and non-AA population. In contrast, the effect of three girls differs considerably by ethnicity, reaching .29 for three girls in the AA sample. Also in the AA population, the effect of g_{123i} increases from k=3 to k=4 and then diminishes gradually for higher values of k, remaining marginally significant even at k=10. In the non-AA population, in contrast, the effect of g_{123i} is considerably smaller and differs little from the effect of b_{123i}.[17]

The Boy-3rd Instrument

The fifth and sixth rows of Table 4b show the effect of having a boy at third birth (b_{3i}) in families with a mixed-sex sibship at first and second birth. A boy at third birth reduces childbearing in families that already have one boy by .080 (s.e.=.018). Results allowing different coefficients by ethnicity generate an effect of -.053 (s.e.=.023) in the non-AA population, while the AA interaction term adds a further .054 (s.e.=.035) to this reduction, though the difference between AA and non-AA is not significant. The Boy-3rd effect potentially provides an additional source of exogenous variation in fertility, beyond pure sex-composition effects. We therefore add this to the instrument list for some of the 2SLS specifications discussed below. Figure 3 summarizes the effects of b_{3i} on fertility increments for families with a mixed-sex sibship at first and second birth, separately by ethnicity. In the AA population, b_{3i} reduces the likelihood of having more than 4 children as well as the likelihood of higher

[17] A possible concern with the sex-composition instruments is failure of the Imbens and Angrist (1994) monotonicity assumption. Monotonicity, which requires that an instrument only operate on an endogenous variable in one direction, ensures that the IV estimand has a causal interpretation in a world of heterogeneous treatment effects. Because some parents may prefer a mixed sibship while others may prefer same-sex sibships, monotonicity need not hold for sex composition instruments. As a partial check on monotonicity, we estimated the same-sex first stage separately by intervals of individual year of birth, maternal age at first birth, and ethnicity. Only 3 out of 36 cells generated negative estimates and all 16 significant estimates were positive.

order births, up to k=7, beyond which the effect is no longer significant. In the non-AA population, b_{3i} reduces the likelihood of having 4 or more children, with no significant effect at higher order births.

Pooled First-Stages Using the Full Set of Instruments

In an attempt to increase precision, we also estimated specifications that combine twins and sex-composition instruments. In particular, for the 2+ sample, we combined the t_{2i}, g_{12i} and b_{12i} instruments. For the 3+ sample, we combined the t_{3i}, g_{123i}, b_{123i} and $(1-s_{12i})b_{3i}$ instruments, controlling for the characteristics of the first two births (g_{12i}, b_{12i}, t_{2i} and b_i). These models also include AA interactions. Because the results are similar to those reported in Tables 3, 4a, and 4b, the pooled first stage is reported in the Appendix.

Association between Instruments and Covariates

As in the Angrist and Evans (1998) study using sex-composition instruments, there is no relation between sex-mix and any of the background variables or covariates in our matched data set. These results are therefore not reported or discussed in detail to save space. We also replicate the common finding that twin births are associated with older maternal age. For example, the mothers of first-borns and second-borns who had twins at second or third birth were .3-.5 years older at first birth than those who had singletons. Twinning is not otherwise associated with subject demographics with one exception: in the 1995 sample of 2+ subjects, twin rates are higher for younger subjects. Since twins can be identified only when birth records are complete, the fact that the quality of birth records improved considerably for some cohorts seems likely to explain this finding. In any case, the 3+ sample does not exhibit this pattern. Because the results are similar in the 2+ and 3+ samples, the change in quality of birth records seems unlikely to have had a major impact on our findings.

It's also worth noting that multiple-birth enhancing fertility treatments, a possible source of bias when using twins instruments, became available in Israel only in the mid 1970's. The effect of this on twin rates is evident in vital statistics data only from the mid-80's onwards (see, Blickstein and Baor, 2004). Since fewer than five percent of the third-born siblings in our 3+ sample and fewer than one percent of second-born siblings in our 2+ sample were born after 1984, the spread of fertility treatments seems unlikely to be a factor in our analysis.

As a final check on the instruments, we looked for reduced-form relations between the instruments and outcomes in a sub-sample where there is little or no first-stage relation. In particular, as we showed in Tables 3 and 4, there are only modest effects of a multiple second birth on sibship size for AA first-borns in 2+ families. Likewise, a two-boy sex composition has little effect on AA sibship size. Consistent with a causal interpretation of the twins and sex-composition IV estimates, there is no reduced-form relation between the twins and two-boy instruments and any outcome variable in the 2+/AA sample.

IV. OLS and 2SLS Estimates

The causal effect of interest is the coefficient ρ in the model

$$y_i = W_i'\mu + \rho c_i + \varepsilon_i \qquad (5)$$

where y_i is an outcome variable and W_i includes the covariates X_i, as well as instrument-specific controls (e.g., b_i). In models without covariates, 2SLS estimates of this equation capture siblings' weighted average causal response (ACR) to the birth of an additional child – i.e., the effect of going from c_i-1 to c_i, averaged over c_i – for those whose parents were induced to have an additional child by the instrument at hand. The ACR extends the local average treatment effect idea (LATE, Imbens and Angrist, 1994), a causal effect for those induced to increase childbearing by the instrument, to models with variable treatment intensity. The weighting function that lies behind the ACR is proportional to the CDF differences plotted in Figures 1-3 (Angrist and Imbens, 1995). With covariates, the interpretation of the ACR is slightly more elaborate, but the basic idea behind this interpretation is preserved. Of course, OLS estimates of equation (5) need not have a causal interpretation in models with or without controls.[18]

The outcome variables of interest capture the effects of family size on economic well-being and social status. In particular, we look at measures of subjects' educational attainment (highest grade completed, and indicators of high school completion, matriculation status, and college attendance), labor market status (indicators of work last year and last week, labor force participation, and hours worked last week) and earnings, marital status (indicators of being married at census day and married by age 21) and fertility (number of own children and indicators of having any or two or more children), and the characteristics of subjects' spouses (highest grade completed, last week labor force participation and monthly earnings).

The 2+ Sample

As is typical for regressions of this sort, OLS estimates of the coefficient of family size on equation (5) imply adverse effects of increased family size on measures of human capital and economic circumstances. Larger families of origin are also associated with earlier marriage, increased fertility, and marriage to a less educated spouse. These results can be seen in column 2 of Table 5, which presents OLS estimates for first-borns in the 2+ sample (column 1 reports the means). Not surprisingly, given the sample sizes, all the OLS estimates are very precise. Control for covariates reduces but does not eliminate this negative relationship, as can be seen in column 3 of the table.

In contrast with the adverse effects reflected in the OLS estimates of effects on schooling variables, 2SLS estimates show zero or even positive effects. These results appear in columns 4-8 of

[18] Formally, the average causal response identified by instrumental variables in this context is defined as follows: Let $Y_i(n)$ be potential outcome of subject i, when exposed to a sibship of size $n=0,\ldots,J$. Let n_{0i} and n_{1i} be the number of children in the sibship with binary instrument Z_i switched on or off. Then, we have,
$ACR = \sum_j E[Y_i(j)-Y_i(j-1)|\ n_{1i}\geq j > n_{0i}]\omega(j)$; where the weighting function $\omega(j)$ is proportional to $P[n_{0i}<j] - P[n_{1i}<j] = P[n<j|\ Z_i=0] - P[n<j|\ Z_i=1]$. In other words, the weights are the difference in conditional fertility CDFs given the instrument values.

Table 5, which report 2SLS estimates for different sets of instruments. For example, the estimated effect on schooling using twins instruments with AA interaction terms, reported in column 5, is .101 (s.e.= .13). The corresponding estimates using sex-composition instruments with AA interaction terms, reported in column 7 is .222 (s.e.=.176). Combining both twins and sex-composition instruments generates an estimate of .15 (s.e.=.104), reported in column 8. Interestingly, the combination of instruments generates a substantial gain in precision relative to the use of each instrument set separately. So much so that the estimated schooling effect in the first row of column 8 is significantly different from the corresponding OLS estimate of −.145 reported in column 3. Likewise, the estimated effect on matriculation status, a key educational milestone in the Israeli milieu, is small, positive, and reasonably precise.[19]

This discussion highlights the fact that a key concern with the IV analysis is whether the estimates are precise enough to be informative. Of particular interest is the ability to distinguish IV estimates from the corresponding OLS benchmark. As it turns out, the estimates in column 8, constructed by pooling twins and sex-composition instruments with AA interaction terms, meet this standard of precision remarkably often. In particular, 12 of the 18 parameters presented in this column are estimated precisely enough that the associated 95% confidence interval exclude the corresponding OLS estimates reported in column 3. Moreover, estimates of effects on the level and quality of schooling are very close to zero. The least precise estimates are those for the subject's own labor market outcomes and his or her spouse's outcomes. Similar results were reported by Black, Devereux, and Salvanes (2005), who found insignificant but imprecise effects of family size on earnings.[20]

A second set of noteworthy findings are those for marriage and fertility. The IV estimates of effects on marital status suggest that subjects from larger families are more likely to be married and got

[19] Angrist and Lavy (2004) report that even in a sample limited to those with exactly 12 years of schooling, matriculation certificate holders earn 13 percent more.

[20] The desire for precision notwithstanding, a natural question at this point is whether different instrumental variables *should* be pooled in a 2SLS procedure since each instrument potentially generates its own local average treatment effect. In this case, pooling instruments is justified by the desire to pin down what appears to be a common effect (of zero) as precisely as possible.

married sooner. Using both twins and sex-composition instruments, the estimated effects on marital status are significantly different from zero and substantially larger than the corresponding OLS estimates. On the other hand, the marriage effects generated by sex-composition instruments are much larger than the twins estimates, a point we return to below.

The marriage effects are paralleled by (and are perhaps the cause of) an increase in fertility: the combination-IV estimate of the effect on the probability of having any children is 0.078, four times larger then the corresponding OLS estimate, 0.019. Estimates of effects on a dummy for having two or more children show a similar pattern. In addition to the likelihood that increased marriage rates increase fertility, these fertility effects may reflect an intergenerational causal link in preferences over family size, a possibility suggested by Fernandez and Fogli (2005). Again, however, a cautionary note is that the fertility effects come from the sex-composition instruments and not twins. Also, since fertility estimates are based on fertility measures defined as of census day, i.e. 1983 or 1995, they could reflect an effect on earlier childbearing (possibly generated by earlier marriage) and not on completed fertility.

The last set of results in Table 5 is for spousal characteristics. Because the sample in this case is limited to married individuals, these results are potentially affected by selection bias. At the same time, while they should be interpreted with caution, the spousal results are of interest as an alternative measure of child quality, beyond human capital and labor market variables. One possible consequence of larger sibship sized is reduced parental investment in attributes that are rewarded in the marriage market. Consistent with this notion, and with the other OLS estimates in the table, the OLS estimates in the lower panel of Table 5 suggest that first-borns from larger families are married to spouses with fewer years of schooling, lower labor force participation and lower earnings. Again, however, the IV estimates in columns 4-8 show no significant effects, with signs that are more often positive than negative.

The 3+ Sample

Estimates in the 3+ sample are broadly similar to those for the 2+ sample, though there are some noteworthy differences. To save space, and because the resulting estimates are more precise, we focus here on 3+ imates combining first and second borns (the resulting sample is roughly double the sample of first-borns only). Estimates for first and second-borns in the 3+ sample are reported in Table 6; results for first-borns only in the 3+ sample appear in the appendix. The OLS results in Tables 5 and 6 are virtually identical. The 2SLS estimates in the 3+ sample exploit more sources of variation than were used to construct estimates in the 2+ sample (twins at 3rd birth instead of second; same-sex triples instead of pairs), so here we might expect some differences. The first key finding, however, is preserved: 2SLS estimates using both twins and sibling-sex composition generate no evidence of an adverse effect of larger family size on human capital or labor market variables.

Columns 2-6 in Table 6 parallel columns 4-8 in Table 5 in that these columns report results from the same sequence of longer instrument lists, with the modification that the twins estimates are now generated by the event of a multiple 3rd birth and the sex-composition instruments are dummies for same-sex triples. An innovation in Table 6, however, is the addition of column 7 reporting results combining all instruments (with AA interaction terms) and a dummy for boy-3rd (with an AA interaction term). This provides a modest further gain in precision.

Importantly, the marriage effects in the 3+ sample are smaller and less consistently significant than in the 2+ sample. In particular, the twins instruments generate no significant marriage estimates when used alone. Likewise, the sex-composition instruments generally only marginally significant estimates on one marriage outcome in one specification (reported in column 5). Finally, there are no longer any significant fertility effects, except for some marginally significant negative impacts on the probability of having 3 or more children. This pattern of results therefore suggests there may be something special about sex-composition-induced increases in family size in the 2+ sample. Separate results by ethnicity and sex results, discussed in detail below, suggest that a likely explanation for the marriage results in the 2+ sample is the pressure having a second-born sister puts on the eldest girl to marry in traditional families.

Because some concern has been raised about the existence of direct effects of the same-sex instrument on children's outcomes, thereby violating the exclusion restriction for this instrument, we also look at estimates omitting the same-sex instrument. These results, reported in column 8, again provide no evidence of any adverse effects of family size. In general, same-sex instruments appear to generate smaller 2SLS estimates (i..e., closer to zero or less likely to be positive) than do twins instruments or the combination of twins with boy-3rd. This contradicts Rosenzweig and Wolpin's (2000) conjecture regarding possible beneficial effects of having a sibling of the same sex.

An important feature of Table 6 is the relative precision of the 2SLS estimates. For example, two thirds of the estimates in column 7 generate 95 percent confidence intervals that exclude the corresponding OLS estimates. The marriage estimates using sex-composition instruments with AA interaction terms are now significant or close to it for both marital status outcomes (column 5). The combined-instruments estimate for early marriage is also significant (e.g., .034 with s.e.=.016 in column 7). On the other hand, the estimated effects on marital status in the pooled sample of first and second-borns are considerably smaller than the corresponding estimates in Table 5. On balance, therefore, the evidence for family-size effects on marital status comes primarily from the 2+ sample.

Interpreting Average Causal Response in the 2+ and 3+ Samples

An important feature of the results in Tables 5 and 6 is their consistency for most outcomes across instruments, samples, and subjects' birth order. This is important because, as shown in the first-stage plots, the sex-composition instruments shift the fertility distribution over a wide range of parities, with substantial shifts in large families, especially for the AA sample. Twins, in contrast, increase completed fertility close to the parity where a multiple birth occurred. Because these CDF shifts weight increment-specific causal effects in the overall ACR, the twins and sex-composition IV estimates are capturing the effects of different fertility increments. A second and related point is that the fertility shifts induced by both sets of instruments are over very different ranges in the 2+ and 3+ samples.

A third related point in this context is the large difference in the age of older children when a sibling is born due to a multiple birth and when a sibling is born for any other reason. For example, first-born children in the 2+ sample were, on average about 7 years old when a singleton third child was born but only 4 years old when upon the arrival of a third-born twin. Similarly, first-born children in the 3+ sample were, on average 9.5 years old when a singleton fourth child was born but only 7.75 years old when the fourth-born was a twin. A first-born child exposed to a parity-six singleton birth, say, due to sex preferences, was about 12 years old at the time. The range in ages of exposure to increased family size therefore rules out one possible explanation for the absence of adverse effects. In particular, this finding suggests that the absence of quantity-quality effects is not due to the fact that exposure to a larger family matters only for children in a certain age range.

Analyses by Ethnicity and Gender

Large numbers of Sephardic Jews came to Israel from the Arab countries of Asia and North Africa in the 1950s. Initially, the Total Fertility Rate (TFR) of the AA population in Israel was 5-6, similar to that in many developing countries, while the TFR for Israeli Jews of European origin was just above 3. By the late 1990s, however, the TFR of the non-religious AA population had fallen to 2.2, only slightly higher than that of other non-religious Jewish groups (Friedlander, 2002). The sharp decline in TFR among the AA population occurred without government encouragement, and in the face of pronatalist tax and housing policies (Okun, 1997). In addition to having higher fertility, the AA group is less educated and is poorer than other (Jewish) ethnic groups. For example, only 12 percent of AA Jews in our 2+ sample are college graduates, while the overall college graduation rate in the 2+ sample is 20 percent. The gap in living standards by ethnicity is especially large in large households. Among those born in Israel, the average 1990 income in AA households with 5 or more members was about 60 percent of the income of similarly-sized European-American households, only 15% larger than the income of non-Jews (Central Bureau of Statistics, 1992, Table 11.4)

Most relevant for our purposes are the marked differences in the first-stage effects of multiple births and sex composition by ethnicity. The AA group increases fertility relatively little as a consequence of a multiple birth, especially a multiple second birth, since with high probability AA mothers who experienced a multiple birth were going to have more children anyway. On the other hand, an all-female sibship leads to sharply increased birth rates in the AA sample, much more than for other groups. Given the marked differences in fertility rates, socioeconomic status, and first-stage effects by ethnicity, results for the non-AA and AA samples might be expected to be different. To explore this possibility, we estimated models using the full set of instruments (i.e., corresponding to the last column in table 5, and to column 7 in table 6) separately for the AA and non-AA groups. These results are reported in Table 7.

OLS estimates generally show larger adverse effects in the non-AA sample than in the AA sample. In contrast, however, the 2SLS estimates are broadly similar in that the estimates for both the AA and non-AA groups generate no evidence of an effect on human capital or labor market variables. In fact, estimates for some of the schooling and labor market outcomes in the non-AA group are positive and significant (matriculation certificate and labor supply measures). As before, there is evidence for an increase or acceleration in marriage rates in both groups, while the fertility results are more mixed. The negative fertility effect comes mainly from the non-AA group while the positive early marriage effects are much larger in the AA subsample. The lack of an adverse effect of family size on child quality in the AA sample is particularly noteworthy in view of the non-western characteristics of this population and the effort in many developing countries to promote smaller families.

Also of interest in this context are separate results for men and women, especially in view of the effects on marital status discussed above. As with the results by ethnicity, we estimated separate results by gender using the full set of instruments, including Boy-3rd in the 3+ sample. These are reported in

Table 8. The OLS estimates are almost identical for men and women. Again, the 2SLS estimates by gender show no evidence of any adverse effect on schooling or labor market variables for either group. In this case, some of the estimates on schooling variables for men are positive and significant. An especially important result in this context is the finding that the increase in marriage rates is much more pronounced for women. Moreover, as in the earlier pooled analysis, the effects are much larger in the 2+ than in the 3+ sample; in the latter, the effect on early marriage is insignificant and the effect on survey-date marital status is only marginally significant.

A further analysis using the twins and sex-composition instruments separately shows that the marriage effects for women come primarily from the sex-composition instruments (these results are not reported in the tables). This suggests that the marriage results for women in the 2+ sample may be anomalous. A likely explanation is the pressure having a second-born sister puts on the eldest to marry. This is consistent with traditional Jewish values and can be traced back to the Biblical story of Rachel and Leah.[21] The fact that the early marriage effect is larger for the more traditional AA subpopulation is also consistent with this story. The Rachel-and-Leah effect implies a possible violation of the exclusion restriction – though in the 2+ sample only. But these marital status effects may also be a result of crowding. Older daughters in Israel who would like to set up an independent household may be tempted to marry sooner when crowded by younger sisters.

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[21] Leah was the firstborn daughter of Laban, who was Issac's brother-in-law; Rachel was the second-born, and more beautiful, in the biblical account. Jacob, son of Issac, wished to marry Rachel, but was tricked into marrying the first-born by Laban, who claimed that the eldest daughter must marry first but also ultimately allowed Jacob to marry Rachel as well -- after a 2nd long apprenticeship. This well-known story and the associated tradition may explain why the marriage effects are larger in the 2+ sample than in the 3+ sample. In the 2+ sample the marriage effects on girls come from a second-born girl who may be pushing first-born girls to marry sooner. In the 3+ sample, the same-sex effects on girls are generated by the 3rd-born girl (since we control for the sex of the second child), who may generate smaller effects than those generated by the next-oldest sibling.

V. Comparison with Previous Findings and Theoretical Implications

Rosenzweig and Wolpin (1980) used multiple births to study quantity-quality trade-offs in a small sample from India. Their estimates point to a negative effect of multiple births on education, but the Rosenzweig and Wolpin sample consisted of children who may not have completed their schooling, and included children born after the occurrence of a multiple birth (therefore, a selected sample). A more recent study by Black, Devereux and Salvanes (2005) uses a large sample of administrative records to look at the effects of multiple births on schooling and earnings. Controlling for birth order, the occurrence of a multiple birth has no effect on these outcomes. An interesting difference between their results and ours is that the Norwegian families they study are much smaller than those in our sample.

Two other recent studies have used IV methods with US census data to look at the effects of sibship size on schooling and private school enrollment among youth still co-resident with their parents. Using twins instruments, Caceres (2004) finds a negative effect of family size on private school enrollment in some specifications and samples (as well as effects on room-sharing and parental divorce) but no effect on measures of human capital such as schooling or dropout status. Conley and Glauber (2004) use sibling-sex composition instruments to estimate effects on grade-retention and private school enrollment. Their results point to negative effects, though their research design is problematic.[22] Finally, Qian (2004) uses regional and time variation in China's one-child policy, as well as multiple births, to estimate the effects of family size on school enrollment in China. Perhaps surprisingly, her estimates suggest that relaxation of the one-child policy increased the enrollment rates of first-born children.

Consistent with this related work, and against a background of our own OLS estimates showing strong adverse effects, our IV strategies generate little evidence for a quantity-quality trade-off in the

[22] Conley and Glauber (2004) omit the first-stage estimates that lie behind their estimates. The private school estimates in their study are significant only for "later-borns" (i.e., later than 2nd born), a potentially endogenous sample in the sex-composition research design. The effects they report on grade repetition are more precise, but to a surprising extent given the likely size of the fertility first-stage in the 1990 PUMS (presumably the same as reported in Angrist and Evans, 1998).

sense of a causal link between sibship size and outcome variables describing the human capital, earnings, or social status of first- and second-born children. This suggests the OLS effects reflect substantial omitted variables bias.[23] Our results reinforce and broaden the earlier findings in this area by simultaneously drawing on a number of sources of variation and including evidence from various fertility increments and from different family types. On the other hand, we do find some evidence of a possible "crowding effect" in the form of accelerated marriage rates for girls from large families. The fact that the marriage effects are highly localized, however, suggests they may be due to the social pressure a younger sister exerts on the eldest to marry, especially in traditional Jewish households. It seems unlikely this channel would give rise to a spurious absence of quantity-quality effects.

Explanations and Implications for QQ Theory

The first question this set of findings raises is what might account for the absence of a causal link between sibship size and child welfare, at least as measured here. One possibility is that, as far investment in human capital goes, parents use perfect capital markets to fund investment irrespective of resource constraints. It seems unlikely, however, that capital markets are so nearly perfect, especially in Israel during the period we are studying, when even the private market for mortgage financing was not well-developed.

A more relevant possibility is that, in the face of larger families, whether due to an exogenous surprise in the case of twins or in response to an exogenous shift in the preferences for more children due to sex composition, parents adjust on margins other than quality inputs.[24] For example, parents may work longer hours or take fewer or less expensive vacations (i.e., consume less leisure). Parents may also

[23] Shavit and Pierce (1991) present a detailed descriptive analysis of the correlation between sibship size and education for Israeli ethnic groups.

[24] Israel, like many countries, offers tax concessions to larger families in the form of child allowances, but these payments were low during the period subjects in our analysis samples were born (Manski and Mayshar, 2002). We confirmed this in an exploratory analysis allowing changes in eligibility and the level of child allowances across cohorts to interact with the instruments.

substitute away from personal as opposed to family consumption (e.g., by drinking less alcohol). Evidence on this point is difficult to obtain since consumption data rarely come in samples large enough or with the kind of retrospective family information needed to replicate our natural-experiments research design. Weighing against the "less leisure, more work" theory, however, are the AE-98 results showing no effect of additional childbearing on husbands' labor supply and a sharp negative effect of childbearing on wives labor supply and earnings.

The AE-98 results for wives raise the possibility of an explanation linked to female labor supply. Clearly one effect of additional childbearing is to increase the likelihood of at-home child-care for older siblings (an effect also documented by Gelbach, 2002). It may be that home care is better, on average, than commercial or other out-of-home care, at least in the families affected by the fertility shocks studied here. On the other hand, the evidence on this point, mostly coming out of welfare reform efforts to increase employment rates for single mothers, has been mixed, showing both positive and negative effects (see, e.g., Cherlin, 2004). The picture here may become clearer as additional evidence accumulates. In this context, however, it should also be noted that results for women on public assistance need not apply to other groups.[25]

A third sort of explanation for the absence of a causal link between sibship size and the outcomes studied here might be called "marginally ineffective or irrelevant inputs." Using research designs similar to ours, Caceres (2004) and Glauber and Conley (2005) both find some evidence for a decreased likelihood of private school enrollment. Caceres also finds that children in larger families are more likely to share a room. This can be seen as paralleling the results reported here suggesting that girls from larger families marry sooner, since the latter may reflect a desire to leave a relatively crowded household. The private school, marriage, and room-sharing effects reflect changes in parental inputs. In practice, these inputs may matter little for children's life chances. For example, parents may incorrectly believe that a

[25] Estimates of AE-98 type models for samples of Israeli mothers show only modest effects of child-bearing on labor supply; see, e.g., Marmer (2000).

private school education is better (perhaps due to a misleading peer correlation) and children almost certainly prefer more space to less. But in the long run, these factors may be more consumption than investment, contributing little to human capital and life chances. Given the findings for these few inputs and the absence of significant or credible effects on longer-term outcomes, the notion that parents adjust inputs with low investment value appears to get some support.

Another explanation that is consistent with our findings is that having twins or same sex siblings has direct and positive effect on the outcome of interest due to economies of scale. If true then these effects might offset the negative effect of sibship size. As a check on this, we looked for reduced-form effects on outcome variables in sub-samples for which there are little or no first stage effects of the instruments on family size. In particular, there are only modest effects of a multiple second birth on sibship size for AA first-borns in 2+ families. Likewise, a two-boy sex composition has little effect on AA sibship size. The fact that there is no reduced-form relation between the twins and two-boy instruments on any outcome variable in these sub-sample weighs against the possibility of direct positive effects of the kind related to scale economies or any other factor.

The lack of a causal effect of family size on human capital and earnings appears inconsistent with a quantity-quality tradeoff in child-rearing. It should be noted, however, that in the original Becker-Lewis (1973) analysis, the quantity-quality trade-off was motivated as an endogenous shift in response to rising incomes. Becker and Lewis essentially assume that the income elasticity with respect to child quality is greater than that for child quantity, so that increases in income cause parents to shift from quantity to quality. At the same time, it is straightforward to show that exogenous increases in family size in a Becker-Lewis-type setup (due, say to a change in contraceptive costs; p. S283) should reduce child quality since an increase in quantity increase the shadow price of quality. Along these lines, Rosenzweig and Wolpin (1980) similarly interpret the event of a twin birth as capturing the effect of a change in the relative price of quantity (actually a subsidy to the cost of further childbearing; p. 234).

They argue that this price change should reduce quality unless quantity and quality are strong complements in parental utility functions.

A more recent theoretical literature focuses on the interaction between technological change, human capital, and quantity-quality trade-offs. Here the theoretical case for a quantity-quality trade-off is less clear-cut. In Galor and Weil (2000), for example, parents substitute towards quality when the returns to human capital rise. In this sort of model, the effect of exogenous increases in the number of children on quality depends on the form of the utility function and other structural details.

While the quantity-quality tradeoff is less clear-cut in more recent theoretical discussions than in the original Becker framework, the traditional view has nevertheless helped to provide an intellectual foundation for policies that attempt to reduce family size in LDCs. Our results clearly raise questions about the nature and extent of the causal link running from numbers of children to family living standards. Of course, results for Israel, a relatively developed society, need not apply in a developing country setting. At the same time, we estimated effects for a sub-population of Asian and North African origin that has many of the demographic and cultural characteristics of a developing country population. The results for the AA and non-AA populations are similar.

VI. Summary and Directions for Further Work

We use a unique sample combining census and population registry data to study the causal link running from sibship size to human capital and economic and social status later in life. Our research design exploits variation in fertility due to multiple births and preferences for a mixed sibling-sex composition, along with ethnicity interactions and preferences for a male child at third birth. The evidence is remarkably consistent across research designs and samples: while all instruments exhibit a strong first-stage relation, and OLS estimates are substantial and negative, IV estimation generates no evidence for negative consequences of increased sibship size on outcomes. Some estimates suggest that girls from larger families marry sooner, but this seems likely to be an effect specific to first-born girls with a younger sister.

The results reported here provide an unusually broad picture that is consistent with a narrower set of findings from a number of recent studies using data from America, Norway, and China to explore the same sort of questions. What might explain the failure of an empirical quantity-quality trade-off to appear? One possibility is that parents invest in human capital without regard to resource constraints. As noted earlier, this seems unlikely in the Israeli context. Another is that the cost of children is borne by reducing parental consumption while holding quality constant. Mothers' withdrawal from the labor force in response to childbirth may also be a net plus for older siblings, though the extent of this withdrawal in Israel seems to be too small to be a big part of the story. Finally, parents may reduce expenditure on parental inputs or investments that are of low value to children, at least in our sample. In future work, we hope to shed light on these issues by generating new evidence on the effect of childbearing on resource allocation across generations.

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Table 1: Match Rates and Sample Selection

	Israeli-born Father				Foreign-born Father							
					Foreign-born subejct				Israeli-born Subject			
	Subject born<1955		Subject born ≥ 1955		Subject born<1955		Subject born ≥ 1955		Subject born<1955		Subject born ≥ 1955	
	(1)		(2)		(3)		(4)		(5)		(6)	
						A. 1995 census						
All subjects	9,453		56,534		118,633		72,340		58,767		161,331	
Matched to registry (N,%)	9,057	95.8%	54,073	95.6%	115,123	97.0%	68,788	95.0%	57,098	97.2%	156,096	96.8%
Matched mother + siblings (N,%)	1,573	16.6%	50,597	89.5%	7,600	6.4%	32,472	44.9%	11,351	19.3%	139,783	86.6%
Selected sample												
Mothers born ≥ 1930 whose age at 1st birth ε [15,45]	494		48,683		1,166		26,217		2,556		119,928	
of which: Israeli born mothers or immigrants who arrived since 1948 and before the age of 45	419		47,022		1,127		22,704		2,211		115,783	
of which: first and second borns of families with 2 or more births	349		34,778		1,008		15,443		1,937		67,952	
Estimated fertility coverage: 86%												
						B. 1983 census						
All subjects	11,049		12,665		160,459		25,025		66,761		70,662	
Matched to registry (N,%)	9,704	87.8%	10,867	85.8%	140,932	87.8%	20,691	82.7%	60,105	90.0%	62,141	87.9%
Matched mother + siblings (N,%)	1,289	11.7%	9,258	73.1%	7,380	4.6%	14,557	58.2%	10,767	16.1%	50,785	71.9%
Selected sample												
Mothers born ≥ 1930 whose age at 1st birth ε [15,45]	421		7,854		1,065		9,197		2,438		34,560	
of which: Israeli born mothers or immigrants who arrived since 1948 and before the age of 45	318		6,952		1,045		8,913		2,138		32,368	
of which: First and second borns of families with 2 or more births	276		5,834		906		5,889		1,869		21,744	
Estimated fertility coverage: 79%												

Notes: The table reports sample sizes and match rates at each step of the link from census data to the population registry. The target population for linkage consists of Jewish census respondents in 1995 and 1983 aged 18-60. The table also shows the impact of sample selection criteria on sample sizes

Table 2: Analysis Samples

	Full			Asia-Africa		
	2+	3+		2+	3+	
	1st borns (1)	1st borns (2)	2nd borns (3)	1st borns (4)	1st borns (5)	2nd borns (6)
1995 Census	0.758	0.753	0.775	0.706	0.705	0.732
Mother married or widowed in 2003	0.910	0.926	0.932	0.921	0.932	0.937
Endogenous variables						
# of children	3.63	4.22	4.32	4.31	4.67	4.76
More than 2 kids	0.739	-	-	0.867	-	-
More than 3 kids	0.400	0.545	0.572	0.593	0.686	0.704
Family composition						
Twins at second birth	0.009	0.006	-	0.009	0.006	-
Twins at third birth	-	0.010	0.010	-	0.009	0.009
Boy at first birth	0.517	0.518	0.527	0.518	0.518	0.528
Boy at second birth	0.513	0.514	0.507	0.514	0.513	0.504
Boy at third birth	-	0.513	0.516	-	0.508	0.515
Girl12=1	0.233	0.239	0.237	0.232	0.236	0.234
Boy12=1	0.265	0.272	0.272	0.265	0.267	0.267
Girl123=1	-	0.115	0.114	-	0.117	0.113
Boy123=1	-	0.140	0.141	-	0.138	0.138
Control Variables						
Age on census day	26.2	26.4	25.5	27.4	27.5	26.4
Year of birth	1966	1965	1967	1964	1964	1965
Mother's age on census day	49.0	48.8	50.4	49.7	49.5	50.8
Mother's year of birth	1943	1943	1942	1942	1942	1941
Mother's age at 1st birth	22.7	22.2	22.1	22.0	21.7	21.7
Mother's age at immigration (for non-israeli mothers)	17.4	15.7	15.9	15.6	15.4	15.7
Mother's ethnicity						
Israel	0.344	0.354	0.315	0.167	0.161	0.138
Asia-Africa	0.397	0.468	0.507	0.792	0.804	0.830
Former USSR	0.115	0.068	0.064	0.011	0.009	0.007
Europe-America	0.144	0.111	0.114	0.030	0.025	0.025
Father's ethnicity						
Israel	0.274	0.282	0.248	-	-	-
Asia-Africa	0.426	0.500	0.535	1.00	1.00	1.00
Former USSR	0.115	0.068	0.068	-	-	-
Europe-America	0.186	0.149	0.148	-	-	-

Table 2 (cont.)

	Full			Asia-Africa		
	2+	3+		2+	3+	
	1st borns	1st borns	2nd borns	1st borns	1st borns	2nd borns
	(1)	(2)	(3)	(4)	(5)	(6)
Subject ethnicity						
Israel	0.836	0.870	0.887	0.856	0.853	0.878
Asia-Africa	0.061	0.074	0.065	0.144	0.148	0.122
Former USSR	0.066	0.029	0.024	-	-	-
Europe-America	0.037	0.028	0.025	-	-	-
Education Outcomes						
Highest grade completed	12.6	12.5	12.3	12.2	12.1	12.0
Schooling ≥ 12	0.824	0.813	0.802	0.759	0.754	0.752
Matriculation certificate	0.487	0.459	0.429	0.366	0.355	0.338
Some College (age ≥ 24)	0.291	0.262	0.224	0.177	0.136	0.109
College graduate (age ≥ 24)	0.202	0.180	0.153	0.117	0.111	0.093
Labor Market Outcomes (age ≥ 22)						
Worked during the year	0.827	0.820	0.809	0.812	0.787	0.772
Weekly labor force participation	0.817	0.813	0.806	0.813	0.793	0.783
Hours worked last week	32.6	32.4	31.7	32.5	31.7	30.9
Monthly earnings (in 1995 Shekels)	2,997	2,920	2,721	2,847	2,486	2,258
Ln(monthly earnings)	8.08	8.07	8.03	8.07	7.99	7.93
Marriage and fertility						
Married on census day	0.446	0.465	0.418	0.519	0.530	0.478
Married by age 21 (age ≥ 21)	0.172	0.183	0.171	0.198	0.205	0.194
Number of own children (women only)	1.00	1.08	0.98	1.28	1.32	1.20
Spouse's Outcomes (for married)						
Highest grade completed	12.8	12.6	12.5	12.1	12.0	12.0
Weekly labor force participation	0.848	0.837	0.836	0.834	0.830	0.830
Monthly earnings (in 1995 Shekels)	3,421	3,251	3,146	3,067	3,036	3,000
Number of observations	89,445	65,671	53,070	38,063	32,874	28,391

Notes: The table reports descriptive statistics for each of the 3 analysis samples used in the paper. The 2+ sample consists of first-born census subjects from families with two or more births including the subject. The 3+ sample consists of first-born of first- and second-born census subjects from families with three or more births including the subject. The Asia-Africa subsample consists of census subjects whose fathers' ethnicity is identified as Asia-African in the census.

Table 3: Twins First Stage

	2+		3+			
	1st borns		1st borns		1st and 2nd borns	
	(1)	(2)	(3)	(4)	(5)	(6)
Twins-2	0.452	0.627	-	-	-	-
	(0.050)	(0.057)				
Twins-2 x Asia-Africa	-	-0.445	-	-	-	-
		(0.106)				
Twins-3	-	-	0.522	0.583	0.585	0.692
			(0.045)	(0.045)	(0.043)	(0.049)
Twins-3 x Asia-Africa	-	-	-	-0.132	-	-0.225
				(0.094)		(0.086)
Male	-0.018	0.000	0.016	0.018	0.013	0.005
	(0.010)	(0.012)	(0.018)	(0.023)	(0.011)	(0.015)
Male x Asia-Africa	-	-0.041	-	-0.005	-	0.015
		(0.022)		(0.035)		(0.022)
Asia-Africa	0.242	0.267	0.167	0.161	0.084	0.070
	(0.015)	(0.019)	(0.016)	(0.027)	(0.014)	(0.021)

Notes: The table reports first-stage effects on number of children. The sample includes non-twins aged 18-60 in the 1983 and 1995 censuses as decribed in Table 1. In addition to the effects reported, the regressions include indicators for age, missing month of birth, mother's age, mother's age at first birth, mother's age at immigration (where relevant), father's and mother's place of birth, and census year. Regressions for columns 3-6 include also controls for girl12, boy12 and twins at second birth. Regressions for columns 5-6 include also indicators for second born and birth spacing between first and second birth. Robust standard errors are reported in parenthesis. Standard errors in columns 5-6 are clustered by mother's ID.

Table 4a: Sex-Composition First Stage in 2+ Sample (First-borns)

	# of children			More than 2			More than 3			More than 4		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Samesex12	0.074	-	-	0.030	-	-	0.022	-	-	0.010	-	-
	(0.010)			(0.003)			(0.003)			(0.002)		
Girl12	-	0.110	0.086	-	0.028	0.032	-	0.032	0.028	-	0.022	0.013
		(0.015)	(0.017)		(0.004)	(0.006)		(0.004)	(0.005)		(0.004)	(0.004)
Girl12 x Asia-Africa	-	-	0.055	-	-	-0.010	-	-	0.010	-	-	0.022
			(0.032)			(0.007)			(0.009)			(0.008)
Boy12	-	0.039	0.056	-	0.032	0.046	-	0.012	0.018	-	-0.001	-0.002
		(0.015)	(0.017)		(0.004)	(0.005)		(0.004)	(0.005)		(0.003)	(0.004)
Boy12 x Asia-Africa	-	-	-0.042	-	-	-0.032	-	-	-0.013	-	-	0.002
			(0.031)			(0.007)			(0.008)			(0.007)
Boy1	-0.020	0.015	0.012	0.003	0.001	0.000	-0.009	0.002	0.003	-0.008	0.003	0.004
	(0.010)	(0.015)	(0.017)	(0.003)	(0.004)	(0.006)	(0.003)	(0.004)	(0.005)	(0.002)	(0.003)	(0.004)
Boy1 x Asia-Africa	-	-	0.007	-	-	0.002	-	-	-0.002	-	-	-0.002
			(0.031)			(0.007)			(0.008)			(0.007)
Boy2	-0.038	-	-	0.000	-	-	-0.011	-	-	-0.012	-	-
	(0.010)			(0.003)			(0.003)			(0.002)		
Asia-Africa	0.242	0.242	0.236	0.043	0.043	0.053	0.098	0.098	0.100	0.065	0.065	0.061
	(0.015)	(0.015)	(0.024)	(0.004)	(0.004)	(0.006)	(0.005)	(0.005)	(0.007)	(0.004)	(0.004)	(0.006)

Notes: The table reports first-stage effects on number of children and binary indicators for having more than 2, 3 and 4 kids. The sample includes first born non-twins from families with 2 or more births. Regression estimates are from models that include the control variables specified in Table 3. Regressions for columns 1,4,7 and 10 control also for boy at second birth. Robust standard errors are reported in parenthesis.

Table 4b: Sex-Composition First Stage in 3+ Sample (First- and Second-borns)

	First Borns						First and Second Borns					
	# of children		More than 3		More than 4		# of children		More than 3		More than 4	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Girl123	0.181	0.097	0.050	0.044	0.051	0.032	0.185	0.075	0.050	0.043	0.055	0.028
	(0.025)	(0.032)	(0.007)	(0.011)	(0.007)	(0.008)	(0.022)	(0.027)	(0.006)	(0.009)	(0.006)	(0.007)
Girl123 x Asia-Africa	-	0.167	-	0.011	-	0.039	-	0.214	-	0.013	-	0.052
		(0.051)		(0.015)		(0.013)		(0.043)		(0.012)		(0.011)
Boy123	0.092	0.095	0.052	0.067	0.023	0.023	0.063	0.068	0.054	0.067	0.020	0.020
	(0.023)	(0.029)	(0.007)	(0.010)	(0.006)	(0.007)	(0.021)	(0.025)	(0.006)	(0.009)	(0.005)	(0.006)
Boy123 x Asia-Africa	-	-0.007	-	-0.032	-	0.001	-	-0.008	-	-0.027	-	0.000
		(0.047)		(0.014)		(0.012)		(0.041)		(0.012)		(0.011)
Boy3 x (1-samesex12)	-0.080	-0.053	0.007	-0.027	-0.019	-0.009	-0.079	-0.047	-0.030	-0.023	-0.019	-0.007
	(0.018)	(0.023)	(0.005)	(0.008)	(0.005)	(0.006)	(0.015)	(0.019)	(0.004)	(0.007)	(0.004)	(0.005)
Boy3 x (1-samesex12) x Asia-Africa	-	-0.054	-	-0.010	-	-0.020	-	-0.061	-	-0.013	-	-0.023
		(0.035)		(0.010)		(0.009)		(0.030)		(0.009)		(0.008)
Subject = boy	0.014	0.016	0.000	0.003	0.004	0.006	0.013	0.004	-0.002	-0.002	0.004	0.003
	(0.018)	(0.023)	(0.005)	(0.008)	(0.005)	(0.006)	(0.011)	(0.015)	(0.003)	(0.005)	(0.003)	(0.004)
(Subject = boy) x Asia-Africa	-	-0.003	-	-0.005	-	-0.006	-	0.017	-	0.001	-	0.001
		(0.035)		(0.010)		(0.009)		(0.022)		(0.006)		(0.006)
Asia-Africa	0.164	0.182	0.086	0.095	0.062	0.070	0.083	0.096	0.064	0.072	0.045	0.052
	(0.016)	(0.032)	(0.006)	(0.010)	(0.005)	(0.009)	(0.014)	(0.026)	(0.005)	(0.008)	(0.004)	(0.007)

Notes: The table reports first-stage effects on number of children and binary indicators for having more than 3 and 4 kids. The sample for columns 1-6 includes first born non-twins from families with 3 or more births. The sample for columns 7-12 includes first and second born non-twins from families with 3 or more births. Regression estimates are from models that include the control variables specified in table 3. Standard errors in columns 7-12 are clustered by mother's ID.

Table 5: Results for First Borns in 2+ Sample

Outcome	Means (1)	OLS: basic covs. (2)	OLS: all covs. (3)	2SLS -- Instrument list: twins (4)	twins, twinsAA (5)	girl12, boy12 (6)	girl12, boy12, girl12AA, boy12AA (7)	all (8)
Schooling								
Highest grade completed	12.6	-0.252	-0.145	0.152	0.101	0.295	0.222	0.150
		(0.005)	(0.005)	(0.159)	(0.130)	(0.184)	(0.176)	(0.104)
Years of schooling ≥ 12	0.824	-0.037	-0.029	0.023	0.021	-0.009	-0.016	0.003
		(0.001)	(0.001)	(0.027)	(0.021)	(0.028)	(0.028)	(0.017)
Matriculation certificate	0.487	-0.054	-0.033	-0.009	-0.004	0.100	0.077	0.033
		(0.001)	(0.001)	(0.036)	(0.033)	(0.043)	(0.040)	(0.025)
Some College (age ≥ 24)	0.291	-0.049	-0.023	0.012	0.023	0.089	0.089	0.054
		(0.001)	(0.001)	(0.048)	(0.045)	(0.048)	(0.046)	(0.031)
College graduate (age ≥ 24)	0.202	-0.036	-0.015	-0.022	-0.008	0.115	0.115	0.052
		(0.001)	(0.001)	(0.041)	(0.040)	(0.046)	(0.044)	(0.028)
Labor Market Outcomes (age ≥ 22)								
Worked during the year	0.827	-0.025	-0.024	-0.011	0.000	0.063	0.072	0.032
		(0.001)	(0.001)	(0.036)	(0.032)	(0.044)	(0.043)	(0.025)
Weekly labor force participation	0.817	-0.020	-0.020	0.006	0.003	0.018	0.033	0.015
		(0.001)	(0.001)	(0.038)	(0.034)	(0.043)	(0.043)	(0.026)
Hours worked last week	32.6	-1.06	-1.20	-0.76	-0.04	1.46	1.06	0.65
		(0.05)	(0.06)	(2.41)	(2.14)	(2.06)	(1.98)	(1.42)
Monthly earnings (in 1995 Shekels)	2,997	-217	-179	-6.76	55.5	266	430	261
		(7.4)	(8.0)	(362)	(319)	(283.8)	(292)	(209)
Ln(monthly earnings)	8.08	-0.034	-0.025	-0.032	0.007	-0.053	-0.067	-0.026
		(0.002)	(0.002)	(0.095)	(0.085)	(0.092)	(0.083)	(0.057)
Marriage								
Married on census day	0.446	0.023	0.020	0.039	0.056	0.118	0.101	0.074
		(0.001)	(0.001)	(0.028)	(0.025)	(0.034)	(0.032)	(0.019)
Married by age 21 (age ≥ 21)	0.172	0.027	0.022	-0.003	0.021	0.198	0.192	0.107
		(0.001)	(0.001)	(0.035)	(0.031)	(0.047)	(0.046)	(0.025)
Fertility (women only)								
Number of own children	1.00	0.123	0.110	0.182	0.037	0.191	0.178	0.115
		(0.004)	(0.004)	(0.133)	(0.086)	(0.096)	(0.097)	(0.064)
Any children	0.448	0.126	0.019	0.093	0.012	0.136	0.134	0.078
		(0.001)	(0.001)	(0.057)	(0.036)	(0.041)	(0.041)	(0.026)
2 or more children	0.320	0.030	0.023	0.084	0.042	0.080	0.076	0.061
		(0.001)	(0.001)	(0.050)	(0.032)	(0.035)	(0.036)	(0.024)
Spouse's Outcomes (for married)								
Highest grade completed	12.8	-0.325	-0.173	-0.274	-0.155	0.333	0.263	-0.002
		(0.008)	(0.009)	(0.417)	(0.324)	(0.438)	(0.421)	(0.252)
Weekly labor force participation	0.848	-0.023	-0.023	-0.008	-0.012	0.033	0.035	0.011
		(0.001)	(0.001)	(0.052)	(0.039)	(0.040)	(0.039)	(0.028)
Monthly earnings (in 1995 Shekels)	3,241	-281	-223	112	-241	518	425	92.9
		(09)	(10)	(582)	(437)	(720)	(658)	(356)

Notes: The table reports means of the dependent variables (column 1) and coefficients on number of children for OLS models (columns 2-3) and 2SLS models using different sets of instruments (columns 4-8). Instruments with an 'aa' suffix are interaction terms with an AA dummy. The sample includes first borns from families with 2 or more births as decribed in Table 1. OLS estimates for column 2 include indicators for age and sex. Estimates for columns 3-8 are from models that include the control variables specified in Table3. Robust standard errors are reported in parenthesis.

Table 6: Results for First and Second Borns in 3+ Sample

Outcome	OLS all covs. (1)	2SLS -- Instrument list: twins (2)	twins, twinsAA (3)	girl123, boy123 (4)	girl123, boy123, girl123AA, boy123AA (5)	all (6)	all, boy3, boy3AA (7)	twins, twinsAA, boy3, boy3AA (8)
Schooling								
Highest grade completed	-0.143	0.167	0.187	-0.108	-0.054	0.076	0.080	0.175
	(0.005)	(0.119)	(0.111)	(0.134)	(0.120)	(0.081)	(0.077)	(0.101)
Years of schooling ≥ 12	-0.031	0.025	0.025	0.001	-0.007	0.016	0.012	0.017
	(0.001)	(0.019)	(0.018)	(0.023)	(0.022)	(0.014)	(0.013)	(0.017)
Matriculation certificate	-0.033	0.057	0.065	-0.019	0.008	0.036	0.042	0.069
	(0.001)	(0.026)	(0.026)	(0.030)	(0.027)	(0.019)	(0.018)	(0.023)
Some College (age ≥ 24)	-0.021	0.061	0.062	-0.047	-0.022	0.010	0.002	0.024
	(0.001)	(0.037)	(0.036)	(0.030)	(0.025)	(0.021)	(0.018)	(0.027)
College graduate (age ≥ 24)	-0.014	0.053	0.056	-0.057	-0.029	0.006	0.002	0.037
	(0.001)	(0.032)	(0.033)	(0.028)	(0.022)	(0.019)	(0.016)	(0.024)
Labor Market Outcomes (age ≥ 22)								
Worked during the year	-0.027	0.029	0.034	0.038	0.035	0.037	0.039	0.043
	(0.001)	(0.025)	(0.024)	(0.029)	(0.027)	(0.018)	(0.017)	(0.021)
Weekly labor force participation	-0.023	0.028	0.035	-0.001	-0.006	0.011	0.018	0.040
	(0.001)	(0.025)	(0.024)	(0.028)	(0.026)	(0.018)	(0.016)	(0.021)
Hours worked last week	-1.41	2.38	2.53	1.44	1.56	2.29	2.11	2.59
	(0.05)	(1.48)	(1.45)	(1.36)	(1.28)	(0.97)	(0.89)	(1.26)
Monthly earnings (in 1995 Shekels)	-185	46.6	63.4	127	115	103	107	96
	(6.77)	(207)	(206)	(176)	(162)	(131)	(122)	(180)
Ln(monthly earnings)	-0.027	0.017	0.011	0.013	0.031	0.038	0.019	0.005
	(0.002)	(0.048)	(0.049)	(0.050)	(0.046)	(0.033)	(0.031)	(0.042)
Marriage								
Married on census day	0.020	0.022	0.016	0.019	0.041	0.027	0.020	0.006
	(0.001)	(0.018)	(0.018)	(0.024)	(0.022)	(0.014)	(0.013)	(0.016)
Married by age 21 (age ≥ 21)	0.023	0.029	0.028	0.041	0.048	0.037	0.034	0.018
	(0.001)	(0.022)	(0.021)	(0.029)	(0.028)	(0.017)	(0.016)	(0.019)
Fertility (women only)								
Number of own children	0.114	-0.059	-0.067	-0.086	-0.042	-0.054	-0.072	-0.097
	(0.004)	(0.063)	(0.058)	(0.071)	(0.064)	(0.044)	(0.042)	(0.059)
Any children	0.022	0.021	0.015	-0.004	0.013	0.014	0.011	0.006
	(0.001)	(0.028)	(0.027)	(0.025)	(0.023)	(0.017)	(0.016)	(0.025)
2 or more children	0.024	0.000	-0.003	-0.008	0.008	0.003	-0.004	-0.016
	(0.001)	(0.025)	(0.023)	(0.024)	(0.022)	(0.016)	(0.015)	(0.022)
3 or more children	0.025	-0.047	-0.044	-0.044	-0.037	-0.040	-0.040	-0.037
	(0.001)	(0.019)	(0.018)	(0.022)	(0.020)	(0.014)	(0.013)	(0.018)
Spouse's Outcomes (for married)								
Highest grade completed	-0.166	0.539	0.552	-0.325	-0.233	0.106	0.095	0.389
	(0.008)	(0.308)	(0.294)	(0.225)	(0.201)	(0.162)	(0.145)	(0.218)
Weekly labor force participation	-0.023	0.043	0.037	0.026	0.010	0.020	0.012	0.014
	(0.001)	(0.034)	(0.033)	(0.024)	(0.020)	(0.017)	(0.016)	(0.024)
Monthly earnings (in 1995 Shekels)	-217	429	426	-889	-442	-151	-250	-77
	(9.0)	(350)	(345)	(414)	(322)	(230)	(202)	(253)

Notes: The table reports coefficients on number of children for OLS models (column 1) and 2SLS models using different sets of instruments (columns 2-8). Instruments with an 'aa' suffix are interaction terms with an AA dummy. The sample includes first and second borns from families with 3 or more births as decribed in Table 1. Regression estimates are from models that include the control variables specified in Table3. Standard errors are clustered by mother's ID.

Table 7: Full Specification By Ethnicity

	Israel/Europe				Asia/Africa			
	2+		3+		2+		3+	
	1st borns		1st borns	1st+2nd	1st borns		1st borns	1st+2nd
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Schooling								
Highest grade completed	-0.156	0.189	0.240	0.195	-0.134	0.108	0.076	-0.017
	(0.008)	(0.119)	(0.156)	(0.111)	(0.007)	(0.207)	(0.139)	(0.100)
Years of schooling ≥ 12	-0.031	0.013	0.013	0.025	-0.025	-0.015	0.020	-0.009
	(0.001)	(0.019)	(0.024)	(0.018)	(0.001)	(0.036)	(0.025)	(0.018)
Matriculation certificate	-0.042	0.038	0.072	0.067	-0.028	0.022	0.012	0.025
	(0.002)	(0.030)	(0.037)	(0.028)	(0.001)	(0.045)	(0.028)	(0.022)
Some College (age ≥ 24)	-0.039	0.063	0.012	0.016	-0.018	0.054	0.014	-0.002
	(0.002)	(0.038)	(0.052)	(0.038)	(0.001)	(0.047)	(0.027)	(0.020)
College graduate (age ≥ 24)	-0.026	0.060	0.006	0.013	-0.012	0.053	0.004	-0.006
	(0.002)	(0.034)	(0.049)	(0.035)	(0.001)	(0.041)	(0.023)	(0.017)
Labor Market Outcomes (age ≥ 22)								
Worked during the year	-0.038	0.015	0.077	0.077	-0.016	0.061	0.030	0.015
	(0.002)	(0.029)	(0.038)	(0.026)	(0.001)	(0.048)	(0.028)	(0.020)
Weekly labor force participation	-0.035	0.002	0.093	0.089	-0.012	0.043	-0.020	-0.016
	(0.002)	(0.030)	(0.039)	(0.027)	(0.001)	(0.048)	(0.027)	(0.020)
Hours worked last week	-2.08	0.01	4.28	4.14	-0.73	1.02	1.03	0.64
	(0.09)	(1.72)	(2.17)	(1.63)	(0.07)	(2.34)	(1.34)	(1.03)
Monthly earnings (in 1995 Shekels)	-262	369	345	313	-132	87	30	-13
	(13)	(273)	(347)	(248)	(10)	(293)	(208)	(136)
Ln(monthly earnings)	-0.033	0.057	-0.006	-0.004	-0.022	-0.190	0.006	0.012
	(0.005)	(0.073)	(0.075)	(0.055)	(0.003)	(0.101)	(0.044)	(0.036)
Marriage								
Married on census day	0.034	0.088	-0.013	-0.006	0.010	0.046	0.052	0.038
	(0.001)	(0.022)	(0.027)	(0.020)	(0.001)	(0.038)	(0.024)	(0.017)
Married by age 21 (age ≥ 21)	0.040	0.085	0.017	0.014	0.015	0.140	0.046	0.046
	(0.002)	(0.028)	(0.032)	(0.023)	(0.001)	(0.054)	(0.028)	(0.021)
Fertility (women only)								
Number of own children	0.148	0.120	-0.189	-0.127	0.088	0.130	-0.093	-0.051
	(0.008)	(0.074)	(0.088)	(0.065)	(0.006)	(0.116)	(0.075)	(0.055)
Any children	0.030	0.063	-0.019	-0.009	0.015	0.130	0.043	0.021
	(0.002)	(0.032)	(0.038)	(0.029)	(0.002)	(0.116)	(0.027)	(0.020)
2 or more children	0.032	0.064	-0.013	-0.023	0.018	0.115	-0.004	0.001
	(0.002)	(0.028)	(0.035)	(0.024)	(0.002)	(0.047)	(0.026)	(0.019)
3 or more children	0.032	0.008	-0.070	-0.044	0.021	0.062	-0.065	-0.039
	(0.002)	(0.021)	(0.028)	(0.019)	(0.002)	(0.042)	(0.024)	(0.017)
Spouse's Outcomes (for married)								
Highest grade completed	-0.213	0.112	0.050	0.202	-0.156	-0.150	-0.002	-0.049
	(0.014)	(0.301)	(0.388)	(0.272)	(0.011)	(0.439)	(0.239)	(0.164)
Weekly labor force participation	-0.039	-0.012	-0.010	-0.010	-0.013	0.038	0.047	0.015
	(0.002)	(0.035)	(0.048)	(0.033)	(0.002)	(0.042)	(0.027)	(0.018)
Monthly earnings (in 1995 Shekels)	-331	-76	10	-69	-163	274	-211	-296
	(17)	(433)	(635)	(391)	(13)	(665)	(329)	(233)

Notes: The table reports OLS and 2SLS results estimated separately by ethnicity.The 2SLS estimates are from models that include the full set of instruments (i.e. corresponding to column 8 in table 5 and column 7 in tables 6 and A2). Regression estimates are from models that include the control variables specified in Table 3. Robust standard errors are reported in parenthesis. Standard errors for columns 4 and 8 are clustered by mother's ID.

Table 8: Full Specification By Sex

	Males				Females			
	2+		3+		2+		3+	
	1st borns		1st borns	1st+2nd	1st borns		1st borns	1st+2nd
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Schooling								
Highest grade completed	-0.171	0.217	0.297	0.254	-0.118	0.158	0.061	-0.025
	(0.008)	(0.154)	(0.190)	(0.128)	(0.007)	(0.140)	(0.124)	(0.092)
Years of schooling ≥ 12	-0.036	0.028	0.023	0.024	-0.021	-0.008	0.009	-0.005
	(0.001)	(0.028)	(0.032)	(0.023)	(0.001)	(0.021)	(0.021)	(0.016)
Matriculation certificate	-0.029	0.013	0.055	0.061	-0.037	0.048	0.014	0.030
	(0.001)	(0.038)	(0.039)	(0.029)	(0.001)	(0.032)	(0.027)	(0.021)
Some College (age ≥ 24)	-0.022	0.013	0.025	0.037	-0.023	0.086	0.010	-0.012
	(0.001)	(0.045)	(0.050)	(0.032)	(0.001)	(0.041)	(0.028)	(0.022)
College graduate (age ≥ 24)	-0.014	0.025	0.032	0.030	-0.016	0.075	-0.005	-0.012
	(0.001)	(0.038)	(0.044)	(0.028)	(0.001)	(0.037)	(0.024)	(0.019)
Labor Market Outcomes (age ≥ 22)								
Worked during the year	-0.023	0.013	0.028	0.030	-0.025	0.043	0.054	0.035
	(0.001)	(0.031)	(0.035)	(0.023)	(0.002)	(0.036)	(0.028)	(0.022)
Weekly labor force participation	-0.020	0.031	0.005	0.024	-0.020	-0.003	0.026	0.015
	(0.001)	(0.033)	(0.036)	(0.023)	(0.002)	(0.036)	(0.027)	(0.021)
Hours worked last week	-1.28	0.17	1.51	1.79	-1.09	0.27	2.13	1.62
	(0.08)	(2.60)	(2.21)	(1.63)	(0.07)	(1.60)	(1.29)	(1.01)
Monthly earnings (in 1995 Shekels)	-233	278	439	139	-128	259	53	52
	(13)	(453)	(425)	(277)	(09)	(210)	(160)	(118)
Ln(monthly earnings)	-0.026	0.063	0.051	-0.002	-0.026	-0.025	-0.007	0.012
	(0.003)	(0.085)	(0.069)	(0.049)	(0.003)	(0.071)	(0.044)	(0.038)
Marriage								
Married on census day	0.018	0.046	0.036	0.015	0.022	0.091	0.028	0.022
	(0.001)	(0.025)	(0.027)	(0.019)	(0.001)	(0.027)	(0.023)	(0.017)
Married by age 21 (age ≥ 21)	0.015	-0.019	0.005	-0.006	0.028	0.165	0.050	0.054
	(0.001)	(0.021)	(0.023)	(0.016)	(0.002)	(0.039)	(0.029)	(0.023)
Fertility (women only)								
Number of own children	-	-	-	-	0.110	0.115	-0.125	-0.072
					(0.005)	(0.064)	(0.058)	(0.042)
Any children	-	-	-	-	0.019	0.078	0.023	0.011
					(0.001)	(0.026)	(0.021)	(0.016)
2 or more children	-	-	-	-	0.023	0.061	-0.006	-0.004
					(0.001)	(0.024)	(0.021)	(0.015)
3 or more children	-	-	-	-	0.026	0.006	-0.068	-0.040
					(0.001)	(0.019)	(0.019)	(0.013)
Spouse's Outcomes (for married)								
Highest grade completed	-0.122	-0.674	0.836	0.598	-0.219	0.273	-0.262	-0.132
	(0.011)	(0.358)	(0.419)	(0.274)	(0.013)	(0.343)	(0.253)	(0.169)
Weekly labor force participation	-0.019	-0.063	0.036	0.032	-0.026	0.059	0.020	0.006
	(0.002)	(0.055)	(0.062)	(0.040)	(0.001)	(0.032)	(0.023)	(0.016)
Monthly earnings (in 1995 Shekels)	-130	-711	393	205	-316	295	-431	-345
	(09)	(236)	(341)	(227)	(18)	(533)	(407)	(261)

Notes: The table reports OLS and 2SLS results estimated separately for men and women.The 2SLS estimates are from models that include the full set of instruments (i.e. corresponding to column 8 in table 5 and column 7 in tables 6 and A2). Regression estimates are from models that include the control variables specified in Table 3. Robust standard errors are reported in parenthesis. Standard errors for columns 4 and 8 are clustered by mother's ID.

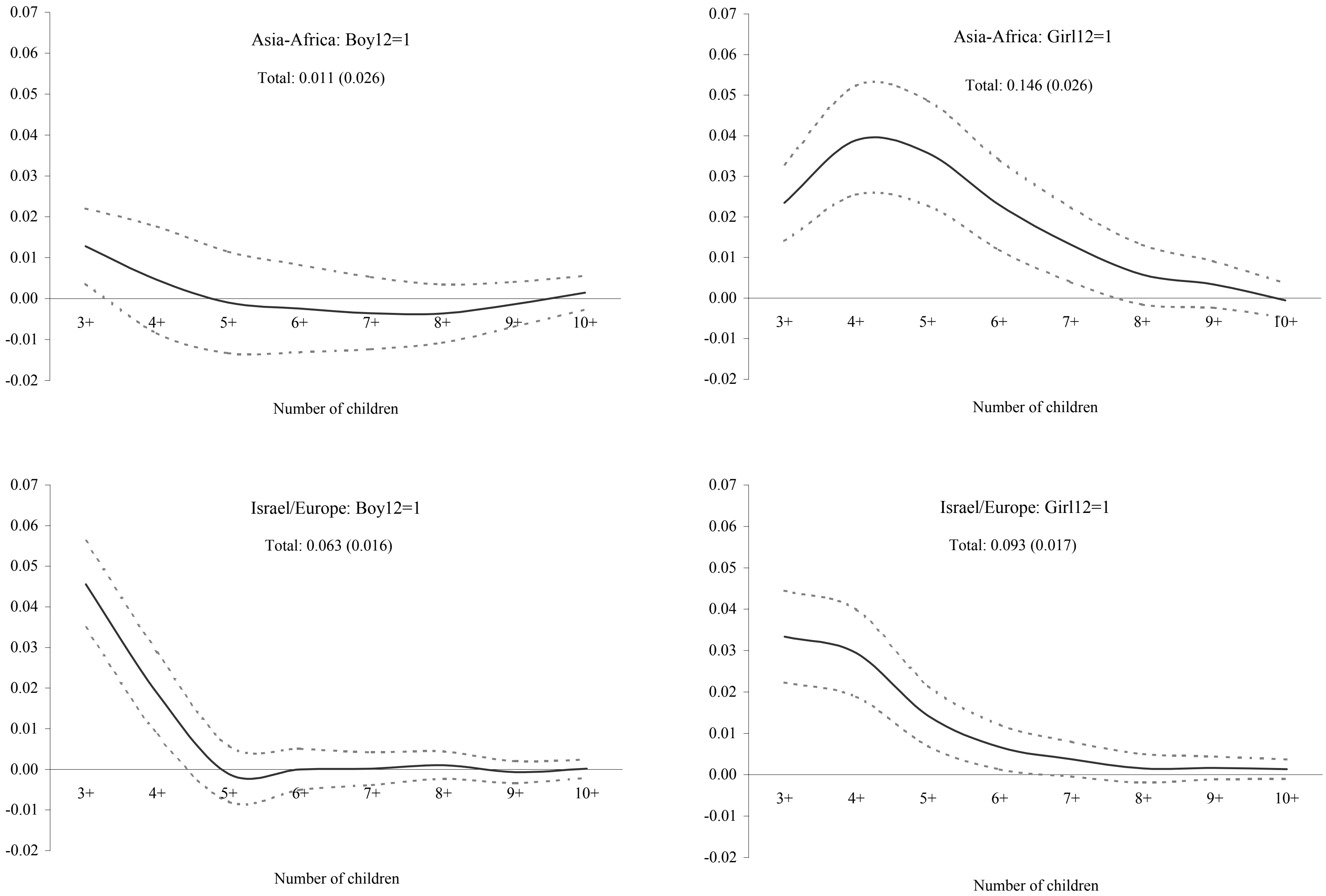

Figure 1: First-borns 2+ sample. First stage effects by ethnicity and type of sex-mix.

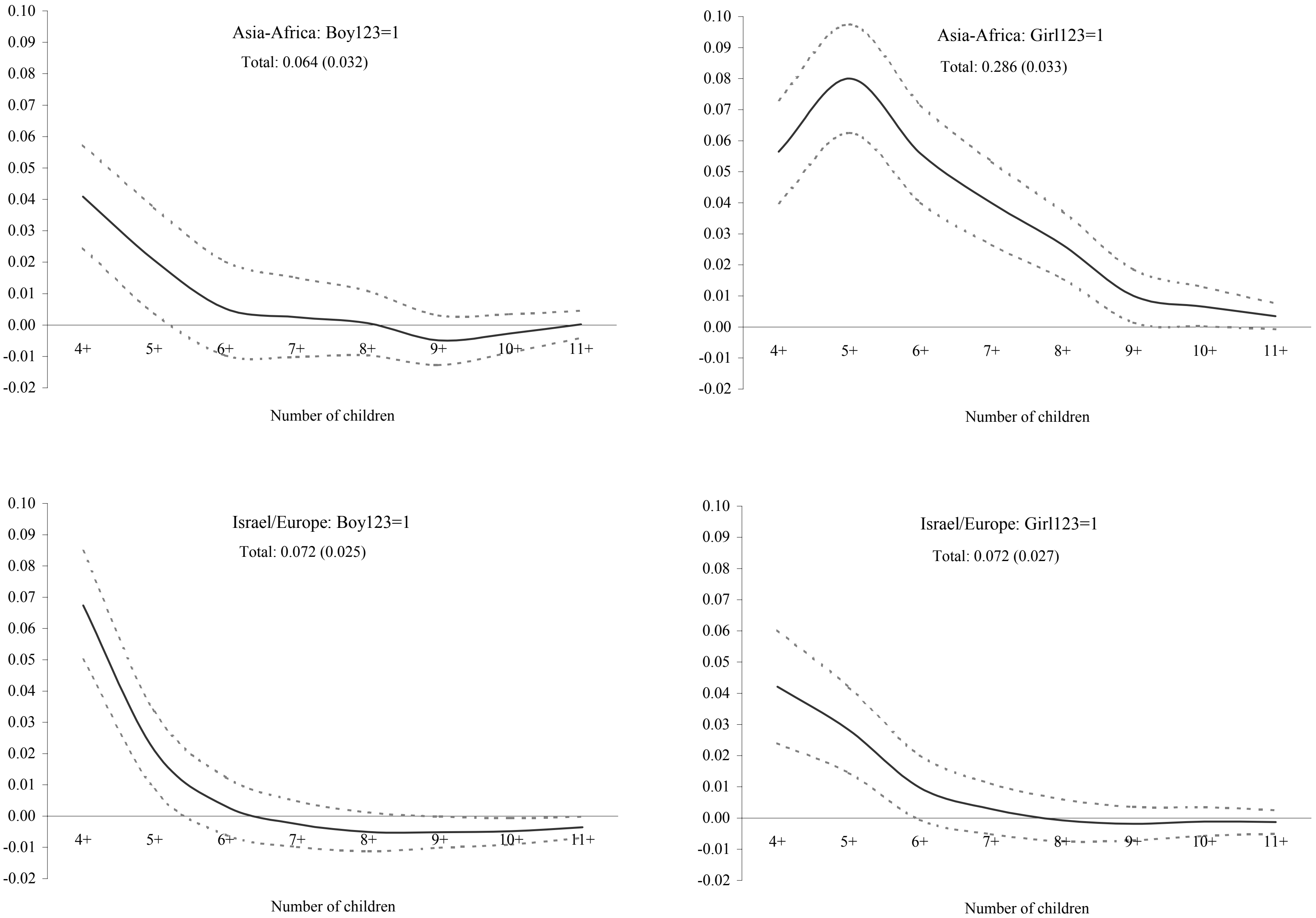

Figure 2: First and second borns 3+ sample. First stage effects by ethnicity and type of sex-mix (conditional on samesex12=1).

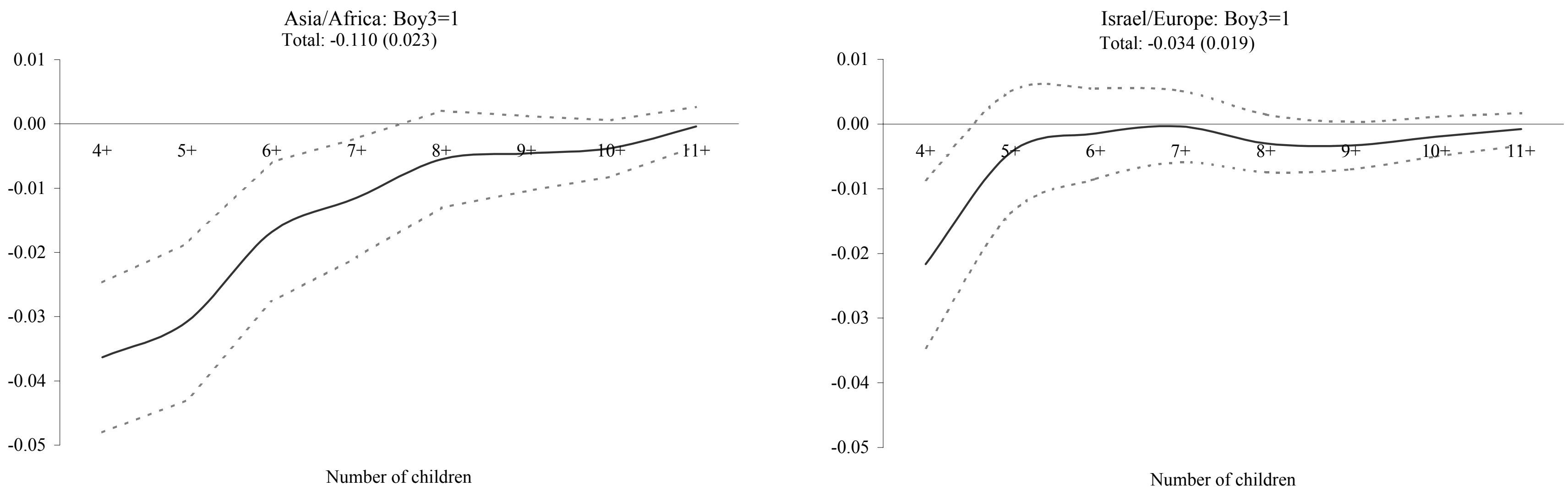

Figure 3: First and second borns 3+ sample. First stage effects of Boy3 (conditional on samesex12=0)

Table A1: Pooled First Stage

	2+	3+	
	1^{st} borns	1^{st} borns	1^{st} and 2^{nd} borns
	(1)	(2)	(3)
Twins	0.640	0.602	0.711
	(0.057)	(0.048)	(0.054)
Twins x Asia-Africa	-0.443	-0.109	-0.199
	(0.106)	(0.100)	(0.094)
Girls	0.090	0.102	0.082
	(0.017)	(0.032)	(0.027)
Girls x Asia-Africa	0.053	0.166	0.210
	(0.032)	(0.051)	(0.043)
Boys	0.061	0.103	0.075
	(0.017)	(0.029)	(0.025)
Boys x Asia-Africa	-0.045	-0.014	-0.146
	(0.030)	(0.047)	(0.040)
Boy3 x (1-samesex12)	-	-0.049	-0.041
		(0.023)	(0.019)
Boy3 x (1-samesex12) x Asia-Africa	-	-0.057	-0.065
		(0.035)	(0.030)
Subect = boy	0.012	0.018	0.005
	(0.017)	(0.023)	(0.149)
(Subject = boy) x Asia-Africa	0.007	-0.005	0.016
	(0.031)	(0.035)	(0.223)
Asia-Africa	0.242	0.190	0.103
	(0.024)	(0.032)	(0.025)

Notes: The table reports first-stage effects on number of children using the full set of instruments. The regression estimates are from models that include the control variables specified in Table 3. Robust standard errors are reported in parenthesis. Standard errors in column 3 are clustered by mother's ID.

Table A2: Results for First Borns in 3+ Sample

Outcome	OLS all covs. (1)	2SLS -- Instrument list: twins (2)	twins, twinsAA (3)	girl123, boy123 (4)	girl123, boy123, girl123AA, boy123AA (5)	all (6)	all, boy3, boy3AA (7)	twins, twinsAA, boy3, boy3AA (8)
Schooling								
Highest grade completed	-0.160	0.322	0.357	-0.099	-0.003	0.148	0.157	0.310
	(0.006)	(0.176)	(0.170)	(0.166)	(0.158)	(0.114)	(0.106)	(0.149)
Years of schooling ≥ 12	-0.033	0.038	0.038	0.007	0.010	0.024	0.022	0.031
	(0.001)	(0.027)	(0.026)	(0.027)	(0.028)	(0.019)	(0.018)	(0.024)
Matriculation certificate	-0.038	0.082	0.093	-0.050	-0.018	0.028	0.038	0.087
	(0.001)	(0.036)	(0.036)	(0.037)	(0.034)	(0.025)	(0.023)	(0.031)
Some College (age ≥ 24)	-0.025	0.138	0.144	-0.044	-0.023	0.028	0.010	0.046
	(0.001)	(0.059)	(0.059)	(0.036)	(0.033)	(0.028)	(0.024)	(0.037)
College graduate (age ≥ 24)	-0.017	0.131	0.137	-0.072	-0.041	0.016	0.004	0.057
	(0.001)	(0.052)	(0.053)	(0.034)	(0.039)	(0.025)	(0.022)	(0.034)
Labor Market Outcomes (age ≥ 22)								
Worked during the year	-0.028	0.038	0.037	0.081	0.074	0.060	0.048	0.020
	(0.001)	(0.037)	(0.036)	(0.037)	(0.036)	(0.026)	(0.023)	(0.031)
Weekly labor force participation	-0.024	0.033	0.034	0.023	0.013	0.022	0.017	0.020
	(0.001)	(0.037)	(0.037)	(0.034)	(0.033)	(0.025)	(0.022)	(0.031)
Hours worked last week	-1.44	2.72	2.69	3.40	3.36	3.29	2.28	1.09
	(0.06)	(2.06)	(2.05)	(1.78)	(1.70)	(1.31)	(1.16)	(1.65)
Monthly earnings (in 1995 Shekels)	-194	140	139	287	212	195	138	55
	(9.0)	(327)	(324)	(253)	(239)	(198)	(181)	(275)
Ln(monthly earnings)	-0.026	0.058	0.055	-0.014	0.001	0.028	0.005	0.009
	(0.003)	(0.069)	(0.070)	(0.056)	(0.051)	(0.042)	(0.039)	(0.061)
Marriage								
Married on census day	0.021	0.048	0.040	0.001	0.023	0.023	0.023	0.028
	(0.001)	(0.027)	(0.027)	(0.030)	(0.028)	(0.020)	(0.018)	(0.024)
Married by age 21 (age ≥ 21)	0.024	0.009	0.012	0.050	0.053	0.034	0.033	0.014
	(0.001)	(0.031)	(0.030)	(0.034)	(0.033)	(0.022)	(0.021)	(0.027)
Fertility (women only)								
Number of own children	0.119	-0.154	-0.160	-0.129	-0.103	-0.127	-0.125	-0.157
	(0.006)	(0.089)	(0.084)	(0.094)	(0.089)	(0.063)	(0.058)	(0.081)
Any children	0.022	0.014	0.008	0.006	0.023	0.017	0.023	0.011
	(0.002)	(0.038)	(0.037)	(0.033)	(0.031)	(0.024)	(0.021)	(0.031)
2 or more children	0.025	-0.012	-0.011	-0.001	0.010	0.001	-0.006	-0.030
	(0.002)	(0.037)	(0.035)	(0.031)	(0.030)	(0.023)	(0.021)	(0.030)
3 or more children	0.026	-0.085	-0.082	-0.069	-0.066	-0.073	-0.068	-0.062
	(0.002)	(0.027)	(0.026)	(0.031)	(0.030)	(0.021)	(0.019)	(0.025)
Spouse's Outcomes (for married)								
Highest grade completed	-0.180	1.00	1.00	-0.379	-0.267	0.181	0.126	0.581
	(0.010)	(0.56)	(0.55)	(0.283)	(0.266)	(0.238)	(0.213)	(0.362)
Weekly labor force participation	-0.025	0.106	0.105	0.044	0.038	0.056	0.038	0.038
	(0.001)	(0.059)	(0.059)	(0.032)	(0.030)	(0.026)	(0.023)	(0.038)
Monthly earnings (in 1995 Shekels)	-241	585	565	-612	-400	-99.0	-118	162
	(11)	(565)	(558)	(536)	(473)	(346)	(303)	(396)

Notes: The table reports coefficients on number of children for OLS models (column 1) and 2SLS models using different sets of instruments (columns 2-8). Instruments with an 'aa' suffix are interaction terms with an AA dummy. The sample includes first borns from families with 3 or more births as decribed in Table 1. Regression estimates are from models that include the control variables specified in Table3. Robust standard errors are reported in parenthesis.