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MULTIPLE EXPERIMENTS FOR THE CAUSAL LINK BETWEEN THE QUANTITY AND QUALITY OF CHILDREN

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Multiple Experiments for 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 welfare. If there is a causal "quantity-quality trade-off," 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 suggests that this is indeed the case. This paper presents new evidence on the child-quantity/child-quality trade-off using quasi-experimental variation 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. For the purposes of this analysis, we constructed a unique matched data set linking Israeli Census data on human capital, earnings, and other outcomes with information on the structure of families drawn from a population registry. Our sample includes groups with very high fertility. An innovation in our econometric approach is the juxtaposition of results from multiple instrumental variables (IV) strategies, capturing the effects of fertility over different ranges for different sorts of people. To increase precision, we also develop an estimator that combines different instrument sets across partially-overlapping parity-specific sub-samples. The resulting variety of evidence addresses the question of the external validity of a given set of IV estimates. Our results are remarkably consistent in showing no evidence of a quantity-quality trade-off across samples and experiments. We do find, however, that girls from larger families marry sooner.

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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. Beginning with Becker and Lewis (1973) and Becker and Tomes (1976), economists have 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. An important implication of this framework is that exogenous reductions in family size should increase parental investment in children, thereby improving human capital and welfare. By the same token, events that lead to otherwise unplanned increases in family size should reduce parental investment and therefore reduce infra-marginal "child quality."

On the policy side, the view that smaller families and slower population growth are essential for economic development motivates many international agencies and some governments to promote, or even to require, smaller families. In addition to China's One Child Policy, 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.[1] Bongaarts (1994) notes that by 1990, 85 percent of people in the developing world lived in countries where the government considers fertility to be too high. The Becker and Lewis (1973) model, as well as recent economic analyses of the role of the demographic transition, provide additional theoretical support for the view that large families keep living standards low (e.g., Galor and Weil, 2000; Hazan and Berdugo, 2002, and Moav, 2005).

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

[1] These episodes are recounted in Weil (2005; Chapter 4), which also mentions the anti-natalist slogan on the Indonesian Rupiah.

micro-econometric and demographic studies show similar relations.[2] The principal problem with research of this type is the likelihood of omitted variables bias in estimates of the effects of childbearing. This is highlighted by Angrist and Evans (1998), who constructed instrumental variables (IV) estimates of the effect of family size on mothers' labor supply. IV estimates, 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 low- and high-fertility sub-samples. We begin by looking at the effect of third and higher-parity births on first- and second-born children's completed schooling, labor market status, adult earnings, and marital status and fertility. These are all important long-run "quality" indicators that are likely to be affected by the home environment. Effects on marriage and fertility also play a role in some theories of the demographic transition (Lutz and Skirbekk, 2005).

Two of the instruments used here are dummies for multiple second births and a dummy for same-sex sibling pairs in families with two or more children, as in Angrist and Evans (1998). But we also extend the sex-composition and twins identification strategies in three ways. First, we 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. Second, as an alternative to instruments based on sex-mix, we exploit preferences for boys at higher order births in some ethnic groups.[3] Third, we combine twins and sex-composition instruments at different parities to produce more precise IV estimates and increase the range of variation covered by our experiments. This parity-pooled analysis includes third and fourth-born children.

[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.

[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).

Another important innovation in our analysis relative to earlier quasi-experimental studies is the combination of evidence from multiple sources of variation. This is important for a number of reasons. First, both twins and sex-composition instruments are potentially subject to omitted variables biases. For example twin rates vary with maternal characteristics like age at birth and race, and twin births affect child spacing and child health in a manner that seems likely to accentuate any negative effects of childbearing. Instrumental variables derived from sibling sex composition are not subject to these considerations, though sex-composition may affect outcomes due, say, to economics of scale through room-sharing (as suggested by Rosenzweig and Wolpin, 2000). A comparison of twins and sex-composition estimates therefore provides a specification check since the omitted variables bias associated with each type of instrument should act differently. The use of instruments based on preferences for male children *per se* also provides a simple check on IV estimates derived from sex-mix.

A second consideration arises from that fact that the estimates generated by any particular IV strategy are immediately relevant only for the sorts of people affected by that instrument (Imbens and Angrist, 1994). Moreover, in models with variable treatment intensity, IV results are specific to the range of variation induced by the instrument (Angrist and Imbens, 1995). As noted by Moffit (2005), these limitations lead to concerns about the external validity of any particular set of IV estimates. Our analysis addresses these concerns by juxtaposing results from different quasi-experimental research designs. On one hand, as we show below, twins instruments identify the effect of treatment on the non-treated since compliance is perfect when a multiple birth occurs. On the other hand, the average causal response due to a twin birth is for a one-child shift at the parity of occurrence. Sex-composition instruments, in contrast, shift the fertility distribution at parities as high as nine. Moreover, the ethnic composition of same-sex compliers (in the sense of Angrist, Imbens, and Rubin, 1996) tends to vary in a manner opposite to that for twins. We therefore argue that the fact that IV estimates affecting different people and inducing differing ranges of variation generates similar results, as ours do, provides considerable evidence for the external validity of our estimates.

Our paper is related to a burgeoning empirical literature that uses multiple births to estimate the causal effects of family size. 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 include Duflo (1998), who looks at effects on child mortality in Indonesia and Caceres (2004), who looks at effects on private schooling and school completion in US Census data. Qian (2004) uses regional and time variation in China's one-child policy to construct instruments, as well as multiple births, to estimate the effects of family size on school enrollment in China. An especially compelling recent study, by Black, Devereux, and Salvanes (2005), uses twins to estimate effects on education and earnings in Norway. As in our paper, Black, *et al* (2005) look at human capital variables with a large administrative sample. In contrast with the original Rosenzweig and Wolpin study, this recent literature has uncovered surprisingly little evidence for an adverse effect of family size on human capital.[4]

To the best of our knowledge, none of the previous work has attempted to combine or reconcile evidence from multiple natural experiments. Our paper also differs from Caceres (2004) and Black, *et al* (2005) in that we study a higher-fertility population with demographic and social characteristics much closer to developing country populations. Of particular interest is the Asia-Africa (AA) subsample, that is, Sephardic Jews of North African and Middle Eastern origin. Sephardic Jews are poor relative to the Israeli average and have very large families.[5] We also look briefly at a smaller sample of Israeli Arabs, a mostly Muslim population with very high fertility.

On the methodological side, our paper has important features in common with Oreopoulos (2006), who compares IV estimates of the returns to schooling using changes in compulsory schooling

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[4] A potential problem with the original Rosenzweig and Wolpin (1980) study is the use of a sample that includes the outcomes of children born after the occurrence of a twin birth. Conley and Glauber (2005), who find some negative effects of family size using sex-composition instruments, is similarly subject to selection bias in that they include the endogenously selected outcomes of children born after the sex-composition experiment occurs. As in Black, et al (2005) and Caceres (2004), our research design eliminates this type of selection bias by looking only at children affected by family-size shocks that occurred at higher parities.

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

laws in different countries. Oreopoulos argues that this comparison can be used to gauge the importance of treatment-effect heterogeneity when the size of the compulsory-schooling first stage varies. In contrast with Oreopoulos' analysis, our study involves a collection of differing types of experiments, which, in addition to affecting different sorts of people, lead to differing ranges of variation in an ordinal fertility variable. A final contribution stems from the relative precision of our estimates. Having established that different instruments and samples generate broadly similar effects, we develop a simple two-stage least squares (2SLS) procedure that combines parity-specific instrumental variables estimates into a single estimate that is more precise than the twins estimates reported by Black, *et al* (2005).

The next section describes the data and the construction of the analysis samples. Section III discusses the first-stage estimates and their implications for treatment effect heterogeneity and nonlinearity, while Section IV presents the main OLS and 2SLS results. On balance, the result reported here offer little evidence for an effect of family size on schooling, work, or earnings, though we do find some effects on girls marital status, age at marriage, and fertility. Section V discusses possible explanations for these findings while 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 and siblings 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,

[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.

for whom we also collected data. 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]

Match Rates and Sample Selection

The vast majority of our census subjects appear in the population 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 a mother may have been deceased. Second, children are more likely to be linkable to parents and siblings when a 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

[7] About 80% of the Israeli population is Jewish. Although we briefly discuss a handful of estimates for Arab subjects, the main study sample is limited to Jews because census-to-population-registry match rates are considerably lower for other groups. Additional information related to data set construction appears in the data appendix.

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 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. 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]

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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

Description of Analysis Samples

We work with two main analysis samples, both described in Table 2. One 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=65,673 first-born and 52,964 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]

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.

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who are matched. We note, however, that outcome variables are reasonably similar for matched and unmatched individuals in the census files, though there are some significant differences. These differences are small and variable across outcomes, however. 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.

[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.

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. Most Israelis are high school graduates, while 20 percent are college graduates. In the AA subsample, however, the 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 45 percent of subjects were married, though marriage rates are higher in the AA sub-sample.

III. First-stage Estimates, Interpretation, and Instrument Validity

Different instruments generate different average causal effects. Of particular importance in this context are: (a) the links between first-stage effects and the subpopulations affected by each underlying natural experiment, and (b) the relation between first-stage effects and the range of variation induced by each instrument. These points are detailed below.

A. Twins First-Stages

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

[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.

$$c_i = X_i'\beta + \alpha t_{2i} + \eta_i \tag{1a}$$

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} (which we call twins-2) 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 Angrist and Evans (1998) 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

$$c_i = X_i'\beta + \alpha_0 t_{2i} + \alpha_1 a_i t_{2i} + \eta_i. \tag{1b}$$

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-.48=.15. 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

[12] The a_i main effect is included in the vector of covariates, X_i. Note that the covariate effects, all labeled 'β', differ as the first-stage specification and sample change.

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.

Heterogeneity and Non-linearity in the Response to a Multiple Birth

The difference in first stage effects across ethnic groups has a simple structural interpretation in the average causal response (ACR) framework laid out by Angrist and Imbens (1995). To see this, define potential endogenous variables C_{0i} and C_{1i} to be the number of children a woman would have if a generic binary instrument, Z_i, is equal to zero or one. Because we observe C_{0i} for those with Z_i equal to zero and C_{1i} for those with Z_i equal to one, the realized number of children is

$$c_i = C_{0i} + (C_{1i} - C_{0i})\, Z_i.$$

For a model without covariates, the IV estimand using this instrument is the Wald estimator (see, e.g., Angrist, 1991):

$$\beta_w = \frac{E[y_i|\, Z_i=1] - E[y_i|\, Z_i=0]}{E[c_i|\, Z_i=1] - E[c_i|\, Z_i=0]}$$

where y_i is the outcome variable. The observed y_i is related to potential outcomes, $Y_i(j)$, where j indexes possible values of $c_i = 0, 1, 2, \ldots, J$; as follows:

$$y_i = Y_i(0) + \textstyle\sum_j [Y_i(j) - Y_i(j-1)]1[c_i \geq j], \tag{2}$$

where the summation is from $j=1, \ldots, J$.

A linear constant-effects model imposes the restriction, $Y_i(j) - Y_i(j-1) = \rho$, for all i and j, in which case the Wald estimator equals this parameter. More generally, Angrist and Imbens (1995) show that

$$\beta_w = \textstyle\sum_j E[Y_i(j) - Y_i(j-1)|\, C_{1i} \geq j > C_{0i}]\omega(j); \tag{3}$$

where the weighting function, $\omega(j)$, is

$$\omega(j) = P[C_{1i} \geq j > C_{0i}] / \{\textstyle\sum_j P[C_{1i} \geq j > C_{0i}]\}$$

Thus, the Wald estimator is a weighted average causal response (ACR) for people from families induced by an instrument to go from having fewer than j to at least j children, weighted over j by the probability of crossing this threshold.[13]

It is straightforward to show that the denominator normalizing the weights, $\omega(j)$, is the Wald first-stage. In other words,

$$E[c_i | Z_i=1] - E[c_i | Z_i=0] = E[C_{1i}-C_{0i}] = \sum_j P[C_{1i} \geq j > C_{0i}].$$

This relation is important because we can think of individuals with $C_{1i} \geq j > C_{0i}$ for any j in the support of c_i as *compliers* in the sense of Angrist, Imbens, and Rubin (1996). In this context, the subpopulation of compliers consists of individuals who switch from having fewer than j to at least j children because of the instrument. Differences in the size of the first stage across demographic or ethnic groups measure differences in the probability of compliance between these groups.

As a practical matter, we can use the ratio of first stages for the AA and overall sample to measure the likelihood that twins-2 compliers are of AA ethnicity. To see this, note that

$$E[C_{1i}-C_{0i} | a_i = 1] / E[C_{1i}-C_{0i}] = \sum_j (P[a_i = 1 | C_{1i} \geq j > C_{0i}] / P[a_i = 1])\omega_j,$$

where the weights, $\omega_j = P[C_{1i} \geq j > C_{0i}] / \sum_j P[C_{1i} \geq j > C_{0i}]$, sum to one. Thus, the ratio of the first-stage for the AA subsample to the overall first-stage summarizes the extent to which compliers are AA, relative to the population proportion AA. The fact that AA family size increase by only .15 in response to a second twin birth while the overall first stage is .44 therefore means that the population of twins compliers is less than half as likely to be AA as the overall population. In contrast, sex-composition compliers are disproportionately likely to be AA, as we show below.

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[13] The assumptions that lay behind the ACR theorem are: (a) Potential outcomes and treatment assignments are independent of the instrument; (b) The instrument moves fertility in one direction only (monotonicity), i.e., $C_{1i} \geq C_{0i}$,] With covariates, the interpretation of the ACR is more elaborate, but the basic idea is preserved. 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.

A second important relevant feature of the twins identification strategy, also derived from the ACR interpretation of the Wald estimand, is the fact that twins estimates capture the causal effect of childbearing in a narrow range. Figure 1, which plots first-stage estimates of the effect of twins-2 and twins-3 on $\{d_{ji} \equiv 1(c_i \geq j);\ j=1,\ldots,11\}$, along with the associated confidence bands, documents this. The normalized CDF differences plotted in Figure 1 are the ω_j in the ACR decomposition of β_w in equation (3). The figure therefore implies that twins instruments capture an average causal effect over a range of fertility variation that is close to the parity of the multiple birth. For example, a multiple third birth increase the likelihood of having a fourth child by about .35 in the Asia-Africa 3+ subs-sample, with a much smaller effect on the likelihood of having a fifth child and no significant effect at higher parities (see the lower left panel of figure 1).[14]

The last important econometric feature of the twins estimates is that they approximate a weighted average of the causal *effect of treatment on the non-treated.* In other words, the subpopulation of compliers affected by the twins-2 instrument is the entire population with two children. Likewise, the twins-3 instrument captures causal effects on the entire population with three children. To see this, note first that $P[C_{1i} \geq 3 > C_{0i}] = P[C_{0i} = 2]$, since $C_{1i} \geq 3$ and $C_{0i} \geq 2$ for everybody in the 2+ sample. Moreover, as Figure 1 shows, $P[C_{1i} \geq j > C_{0i}]$, is close to zero for $j > 3$ since a multiple second birth has little effect on childbearing at higher parities. Therefore, $\beta_w = E[Y_i(3)-Y_i(2) | C_{0i}=2]$. Finally, because Z_i is independent of potential outcomes and potential treatment assignments, $\beta_w = E[Y_i(3)-Y_i(2) | C_{0i}=2, Z_i=0]$. But this is the same as $E[Y_i(3)-Y_i(2) | c_i=2]$, because all those with two children have singleton births and $C_{0i}=2$. A similar line of reasoning leads to the conclusion that the twins-3 estimator in the 3+ sample identifies $E[Y_i(4)-Y_i(3) | c_i=3]$.

Non-twins instruments identify average causal effects that differ in two ways from the effects captured by twins. On one hand, the compliers population is less complete; not all the non-treated are

[14] The twins-2 (twins-3) instrument engenders small shifts in fertility at parities beyond 3 (4) because a multiple birth leads to tighter spacing, thereby lengthening the biological window for continued childbirth. .This is most likely to relevant for the ultra-orthodox minority who have very high fertility.

affected by sex-composition. On the other hand, as we show below, the range of fertility variation induced is often quite a bit wider. In particular, sex-composition instruments (including a dummy for 3rd-born male children) shift the fertility distribution over a wider range than does a multiple birth, especially in the event of an all-female sibship.

B. Sibling-Sex Composition First-stages

Sex-composition first stages in the 2+ sample were estimated using the following two models:

$$c_i = X_i'\beta + \gamma_1 b_{1i} + \gamma_2 b_{2i} + \pi_s s_{12i} + \eta_i \tag{4a}$$

$$c_i = X_i'\beta + \gamma_1 b_{1i} + \pi_b b_{12i} + \pi_g g_{12i} + \eta_i \tag{4b}$$

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

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

is a dummy for same-sex sibling pairs, and

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

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.[15] We also report results from models with AA interaction terms, as in Table 3.

The first-stage effect of s_{12i} in the 2+ sample, reported in column 1 of Table 4, is .073 children. The AA interaction term in this case is essentially zero, so that in contrast with the twins first-stage, the overall sex-composition effect in the 2+ sample is the same for the AA and non-AA populations.

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[15] For example, $g_{12i} = 1-b_{1i}-b_{2i} + b_{12i}$. 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.

In models with common effects across ethnic groups, two girls increases family size by .11 (s.e.=.015) while the effect of two boys is .039 (s.e.=.015). This can be seen in columns 3 and 4 of Table 4, which report estimates of π_b and π_g in equation (4b). Models allowing different coefficients by ethnicity generate a two-girls effect equal to .088 (s.e.=.017) in the non-AA population, while the effect of two girls in the AA sample is larger by .051 (s.e.=.028). In contrast, the two boys effect is only .055 (s.e.=.016) in the non-AA population, and the AA two-boys effect is *smaller* by .038 (s.e.=.026). 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 sex-composition first-stage in the 3+ sample captures the effect of an all-boy or all-girl triple on first- and second-born subjects, controlling for the sex-composition of earlier births. The first-stage therefore conditions on b_{12i} and g_{12i}, as well as a subject-sex main effect and a birth order dummy. Additional variables included in these models are dummies for the sex of the third child, an effect which is defined conditional on a *mixed-sex* sibling pair at first and second birth (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 notation only for the model that allows for separate all-male and all-female effects):

$$c_i = X_i'\beta + \gamma_1 b_i + \delta_b b_{12i} + \delta_g g_{12i} + \gamma_3(1-s_{12i})b_{3i} + \lambda_b b_{123i} + \lambda_g g_{123i} + \eta_i, \qquad (5)$$

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 term b_{3i} (boy-3) is also used as an instrument, though we postpone a discussion of the associated first stage for the moment. The sex-composition effects in this model are reported in columns 5 through 12 of Table 4.

[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.

The overall same-sex effect in the 3+ sample is .12 among first- and second-borns. This can be seen in column 9 of Table 4 (results for first-borns only, reported in column 5-8, are similar). The AA interaction term generates a large ethnic differential in sex-composition effects. For example, the same-sex effect among first- and second-born non-AA subjects, reported in column 10 of Table 4, is .070 (s.e.=.019), while the AA subsample responds to a same-sex triple by more than twice as much. This again contrasts with the twins estimates, where first-stage effects are smaller in the AA subsample.

First-stage effects in the 3+ sample show large differences when stratified by both sex and ethnicity, as can be seen in columns 7-8 and 11-12 of Table 4. The overall effect of three girls on first- and second-borns is 0.183 (s.e.=.022), almost triple the corresponding effect of three boys, 0.065 (s.e.=.021). The effect of three girls is also much larger in the AA population. The estimate for non-AA in column 12 is .072 (s.e.=.027) and the increment for AA is .217 (s.e.=.043), so that the effect of three girls in the first- and second-born AA subsample is .29 (.26 for first-borns only). This is considerably larger than the twins effect on AA subjects in the 2+ sample.

Heterogeneity and Non-linearity in the Response to Sibling-sex Composition

The difference in first-stage effects by AA status documented in Table 4 shows that the population of sex-composition compliers is disproportionately more likely to be of AA background. This is especially true for the response to an all-girl sibship. For example, the two-girl effect on AA fertility is .14, while the EA effect is about .09. The AA differential in the effects of sex-composition on family size is largest for the response to same-sex triples. This pattern stands in marked contrast to the composition of twins-compliers, among which the AA subsample is under-represented. Thus, any comparison of twins and sex-composition IV estimates is implicitly a comparison for very different groups.

A second noteworthy distinction between the sex-composition and twins first-stages is in the different ranges of effects traced out by the two types of instruments. As we noted above, the twins-2 instrument in the 2+ sample increases family size from 2 to 3 with relatively little effect at higher parities, while the twins-3 in the 3+ sample primarily increases family size from 3 to 4, with virtually no other impact on fertility. In contrast, a same-sex sibship leads some families to keep having children at higher parities in pursuit of a more balanced sex composition.

The distribution shift due to sex-composition in the 2+ sample is documented in Figure 2, which reports first-stage estimates of effects of b_{12i} and g_{12i} on $d_{ji} \equiv 1(c_i \geq j)$, for j up to 11, 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 effects at higher-order births. In contrast, the effect of two girls on d_{ji} increases from j=2 to j=3, and then tails off gradually, with a marginally significant effect on the likelihood of having 7 or more children. 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.

The CDF differences plotted in Figure 2 imply that sex-composition instruments capture an average causal effect which reflects the effect of having as many as seven children in the AA population and as many as six children in the non-AA population. The *range* of fertility variation induced by sex composition is even wider in the 3+ sample. This can be seen in Figure 3, which reports CDF differences in response to b_{123i} and g_{123i}, along with the associated confidence bands. The figure shows that, 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}.

C. The Boy-3 Instrument

The bottom rows of columns 5-12 in Table 4 show the effect of having a boy at third birth in families with a mixed-sex sibship at first and second birth. We expect the boy-3 instrument to operate through preferences for male children that are common in more traditional Israeli households. In addition to providing additional variation, the boy-3 instrument is useful because it is implicitly used only for families with a mixed-sex sibship at parities one and two. The boy-3 instrument is therefore unlikely to be subject to the same violations of the exclusion restriction as instruments derived from sex-mix.

A boy at third birth reduces childbearing in the families of first- and second-borns with a mixed-sex sibship by .077 (s.e.=.015). Models allowing different coefficients by ethnicity generate an effect of -.044 (s.e.=.019) in the non-AA population, while the AA interaction term adds a further .064 (s.e.=.030) to this reduction. Figure 4 summarizes the effects of b_{3i} on fertility increments separately by ethnicity. The sample used to construct this figure includes both first- and second-borns.

Figure 4 shows that, as with the sex-mix instruments, boy-3 affects fertility over a wider range than do multiple births. In the AA population, in particular, b_{3i} reduces the likelihood of having more than 4 children as well as the likelihood of higher order births, up to 7, beyond which the effect is no longer significant. In the non-AA population, on the other hand, b_{3i} reduces the likelihood of having 4 or more children, with no significant effect at higher order births.

D. Instrument Validity

A possible concern in any study using IV is failure of the instruments to be independent of potential outcomes, either because of confounding or violations of the exclusion restriction. As in the Angrist and Evans (1998) study using sex-composition instruments, however, 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 replicated 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 cohorts. Since twins can be identified only when birth records are complete, the fact that the quality of birth records improved over time 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. Effects of confounding factors might therefore be expected to surface in such samples. Consistent with a causal interpretation of the twins and sex-composition IV estimates, however, 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

When estimated using separate 2+ and 3+ samples, the causal effect of interest is the coefficient ρ in the model

$$y_i = W_i'\mu + \rho c_i + \varepsilon_i \tag{5}$$

where y_i is an outcome variable and W_i includes the covariates X_i, as well as instrument- and sample-specific controls (e.g., b_i). As discussed in the previous section, 2SLS estimates of this equation capture siblings' weighted average causal response to the birth of an additional child for those whose parents were induced to have an additional child by the instrument at hand. The outcome variables measure human capital, economic well-being, and social circumstances. 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 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 an indicator of having any children).

A. The 2+ Sample

As is typical for regressions of this sort, OLS estimates of the coefficient on family size in equation (5) indicate a negative association between family size and measures of human capital and economic circumstances. Larger families of origin are also associated with earlier marriage and increased fertility. 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 negative OLS estimates, 2SLS estimates point to zero or even positive effects. These results appear in columns 4-8 of Table 5, which report 2SLS estimates for different sets of instruments. For example, the effect on schooling estimated using twins instruments with AA interaction terms, reported in column 5, is .105 (s.e.= .131). The corresponding estimates using sex-composition instruments with AA interaction terms, reported in column 7 is .222 (s.e.=.176).

To increase precision, we also estimated specifications that combine twins and sex-composition instruments *within* a given sample (in this case, 2+) to produce a single, more efficient IV estimate.

Although each instrument potentially generates its own local average treatment effect, the combination of instruments in this context can be justified by the desire to pin down what appears to be a common effect (of zero) as precisely as possible.

Combining both twins and sex-composition instruments generates an estimate of .16 (s.e.=.106), reported in column 8.[17] The combination of instruments generates a substantial gain in precision relative to the use of each instrument set separately; the 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.[18]

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, 7 out of 9 estimates of effects on non-marriage and fertility outcomes presented in this column are estimated precisely enough that the associated 95% confidence interval exclude the corresponding OLS estimates reported in column 3. Moreover, most estimates of effects on the level and quality of schooling are very close to zero. A few of the estimated effects on matriculation rates and college attendance are significant and positive, though given the large number of reported effects, this may be a chance finding.

A second set of noteworthy results 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 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.

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[17] The combined first-stages are reported in the appendix.

[18] 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.

On the other hand, the marriage effects generated by sex-composition instruments are 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 0.079, four times larger then the corresponding OLS estimate, 0.019. 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).

B. The 3+ Sample

Estimates in the 3+ sample, reported in Table 6, are broadly similar to those for the 2+ sample, though there are some noteworthy differences. Columns 2-6 in Table 6 parallel columns 4-8 in Table 5 in that they report results from a similar sequence of instrument lists, with the modification that the twins instruments were generated by the event of a multiple 3rd birth and the sex-composition instruments are dummies for same-sex triples. A further change in Table 6 is the addition of a column (7) which reports results combining all instruments (with AA interaction terms) and a dummy for boy-3 (also with an AA interaction term). This addition provides a modest further gain in precision.

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, 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. Moreover, as in Table 5, a few of the estimated effects on schooling outcomes are positive and (marginally) significant, though the significant estimates are fewer and smaller in this case. The marriage effects in the 3+ sample are also smaller and less consistently significant than in the 2+ sample. In particular, the twins instruments generate no significant marriage estimates when used alone, though they are still positive. Likewise, there are no longer any significant fertility effects.

As a check on the exclusion restrictions for sex-composition instruments, we also looked at estimates omitting these instruments but retaining boy-3. 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-3. This is inconsistent with Rosenzweig and Wolpin's (2000) conjecture regarding possible beneficial effects of having a sibling of the same sex. The boy-3 instrument may also have direct effects, as suggested by Butcher and Case (1994) for girls, but others have found little evidence for this (e.g., Kaestner, 1997).

Interpreting Average Causal Response

The main body of results in Tables 5 and 6 is largely consistent across instruments, samples, and subjects' birth order. This is important because, as shown in the previous section, different instruments shift the fertility distribution very differently for different ethnicities. Moreover, sex-composition instruments shift fertility over a wide range of parities, with substantial shifts in large families, especially for the AA sample. Twins instruments, in contrast, increase completed fertility close to the parity where a multiple birth occurred. The twins and sex-composition IV estimates therefore capture the effects of different fertility increments. A related point is that the fertility shifts induced by both sets of instruments are over very different ranges in the 2+ and 3+ samples. Over-identification tests generate a formal measure of the equality of a set of IV estimates in models with multiple instruments (see, e.g., Angrist, 1991). Although not reported here in detail, we note that over-identification tests for the 2SLS estimates in Tables 5 and 6 show no evidence of significant differences across instrument sets.

A final observation worth making 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 in our research design 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.

C. Combining 2+, 3+, 4+, and 5+ Samples

An important feature of Table 6 is the relative precision of the 2SLS estimates. Two thirds of the estimates in column 7, which reports the results of estimates pooling all instruments *within* the 3+ sample, generate 95 percent confidence intervals that exclude the corresponding OLS estimates. The most precise estimate of effects on highest grade completed has a standard error of .076, while the most precisely estimated effect on earnings is estimated with a standard error of .030 (column 7). This is somewhat less precise than the estimated effects on years of schooling reported in Black, Devereux, and Salvanes (2005), which have standard errors on the order of .05 for schooling and .02 for earnings.

To further increase precision we also pooled estimates *across* the 2+, 3+, and two higher-parity samples. For example, we constructed a single twins-IV estimate using t_{2i}, t_{3i}, t_{4i}, and t_{5i} as instruments in a data set that implicitly stacks the 2+, 3+, 4+, and 5+ samples, while restricting the IV estimates from the different parity-specific sub-samples to be the same. Because the instrument list and conditioning variables are different in each parity-specific subsample, this procedure requires a modification of conventional 2SLS.

The Parity-Pooled Setup

Our pooled analysis works with the union of subjects from 2+, 3+, 4+, and 5+ sub-samples. The total sample therefore includes individuals who are first-born subjects in the 2+ sample, first- and second-born subjects in the 3+ sample, first-through-third-born subjects in the 4+ sample, and first-through-fourth-born subjects in the 5+ sample. In other words, the sample includes all birth orders up to p−1 from families with at least p children, for p≤5. The p+ sub-samples are not mutually exclusive; for example, a given first-born subject in the 5+ sample must also be a member of the 2+, 3+, and 4+ sub-samples.

The restriction that motivates pooled estimation is that the causal effect of childbearing is a constant, denoted ρ_0 (Tables 5 and 6 suggest ρ_0=0). In terms of potential outcomes, we have

$$Y_i(j) = Y_{0i} + \rho_0 \cdot j. \tag{6}$$

In addition, let $Y_{0i} = X_i'\mu_0 + \upsilon_i$ denote the regression of Y_{0i} on X_i in the population from which the parity-pooled sample is drawn. The residual, υ_i, is orthogonal to X_i in this population by construction. The observed outcome, y_i, is linked to this causal model by

$$y_i = X_i'\mu_0 + \rho_0 c_i + \upsilon_i. \tag{7}$$

Note that the residual, υ_i, may be correlated with c_i.

The following Lemma provides the econometric justification for pooled estimation:

LEMMA. Let d_{pi} denote membership in a p+ sample and let Z_{pi} denote an instrumental variable satisfying $Z_{pi} \perp\!\!\!\perp Y_{0i} \mid W_{pi}, d_{pi}=1$, where W_{pi} includes X_i, plus possibly additional instrument-specific controls. Let $Z_{pi}^* = Z_{pi} - W_{pi}'\Gamma$ where Γ is the coefficient vector from a regression of Z_{pi} on W_{pi}' in the p+ population. Assume there is a first stage for Z_{pi}, i.e., $E[Z_{pi}^* c_i| d_{pi}=1] \neq 0$. Then $E[d_{pi}Z_{pi}^* \upsilon_i]=0$ where υ_i is the error term in (7) and the expectation is taken in the population containing subjects of birth order up to p−1 from families with at least p children.

PROOF: $E[d_{pi}Z_{pi}^*\upsilon_i] = E[d_{pi}Z_{pi}^*(y_i - X_i'\mu_0 - \rho_0 c_i)] = E[Z_{pi}^*(y_i - X_i'\mu_0 - \rho_0 c_i)| d_{pi}=1]P[d_{pi}=1]$. Note that $E[Z_{pi}^* X_i| d_{pi}=1]=0$ by construction. Given the constant-effects causal model, (6), and the conditional independence assumption at the beginning of the lemma, $\rho_0 = E[Z_{pi}^* y_i | d_{pi}=1] / E[Z_{pi}^* c_i | d_{pi}=1]$. Therefore $E[Z_{pi}^*(y_i - \rho_0 c_i) | d_{pi}=1]=0$. ♦

This Lemma shows how a common causal parameter can be estimated in a parity-pooled sample. For example, we can combine t_{2i} in the 2+ sample and t_{3i} in the 3+ sample. The data set required for this is the union of the 2+ and 3+ samples, i.e., first-borns in the 2+ sample, and second-borns in the 3+ sample (first-borns in the 3+ sample are included in the 2+ sample). After partialing out the relevant set of covariates as described in the lemma, $d_{2i}[t_{2i}]^*$ and $d_{3i}[t_{3i}]^*$ are valid instruments for equation (7) in the pooled {2+ U 3+} sample. Similarly, we can combine $d_{2i}[b_{12i}]^*$, $d_{2i}[g_{12i}]^*$, $d_{3i}[b_{123i}]^*$, $d_{3i}[g_{123i}]^*$, and $d_{3i}[(1-s_{12i})b_{3i}]^*$, where the first-step regression-adjustment of each instrument accounts for the fact that sex-composition instruments involve different sets of controls in the 2+ and 3+ sample, in addition to the set of common covariates, X_i.

Before turning to a discussion of parity-pooled empirical results, we briefly discuss first stage relations in higher parity samples, focusing on the sex-composition instruments. A full set of first-stage estimates, including those for t_{4i}, and t_{5i}, is given in the appendix. Figures 5-8 report the effects of sex composition on fertility in the 4+ and 5+ samples, using a format similar to the one used in Figures 3 and 4. The figures document the fact that sex mix sharply increases family size in these samples. Effects are again larger for all-female than for all-male sibships and for the AA population. In the AA samples, an all-girl sibship increases the likelihood of family sizes as large as nine. The largest first-stage effect in this context is .36 (s.e.=.082), as a result of five girls. On the other hand, and all-male sibship still increases fertility in both the 4+ and 5+ samples. The effect of a multiple fourth birth, reported in column 5 of Appendix Table A1, is almost one child for non-AA Jews in the 4+ sample. For this group, the twins experiment amounts to a randomized trial with perfect compliance.

Parity-Pooled Results

The empirical strategy using parity-pooled samples leads to a considerable gain in precision, while most of the estimated effects on outcomes other than marriage and fertility remain small and insignificant. This can be seen in Table 7, which reports pooled results using twins instruments in columns 1-3, results pooling sex composition instruments in columns 4-6, and the results of pooling all instruments in columns 7-9. The table shows results from three samples for each instrument set: the union of subjects from the 2+ and 3+ sub-samples, the union of subjects from 2+, 3+ and 4+ sub-samples, and the union of subjects from 2+, 3+, 4+, and 5+ sub-samples. For example, the estimated effect on highest grade completed using all available twins instruments in the union of the 2+, 3+, 4+, and 5+ samples is .031 (s.e.=.055), shown in the first row of Table 5. The corresponding estimate using all available sex composition instruments is .054 (s.e.=.068), in column 6.

The estimates combining both twins and sex composition instruments in the union of 2+, 3+, 4+, and 5+ samples, reported in column 9 of Table 7, are the most precise we have been able to construct. For example, the estimated effect on highest grade completed is .040 (s.e.=.043), in comparison with .072 (s.e.=.076) in Table 6. Similarly, the estimate for effects on log monthly earnings is .001, similar to the estimate in Table 6, with a standard error down from .030 to .019. Both of these estimates are slightly more precise than the most precise estimates of effects on years of schooling and on log earnings reported by Black, Deverux, and Salvanes (2005). Eight out of 9 estimates of effects on non-marriage and fertility outcomes in column 9 of Table 7 generate confidence intervals that exclude the corresponding OLS estimates with covariates, reported in column 1.

Most of the parity-pooled estimates of effects on marriage and some of the effects on fertility remain at least marginally significantly different from zero. For example, the estimated effect on marriage using twins instrument in the pooled 2+,3+,4+, and 5+ sample is .023 (s.e.=.010), and the corresponding estimate using sex-composition instruments is .045 (s.e.=.012). While sex composition instruments generate larger effects on marriage than do the twins instruments, the fact that this effect turns up in both IV strategies suggests the IV estimates reflect the causal effect of childbearing and not just a propensity for older girls to marry in response to the birth of a younger sister, a point discussed further in the next section.

D. Analyses by Ethnicity and Gender

Results by Ethnicity

Large numbers of Sephardic Jews came to Israel from the Arab countries of Asia and North Africa in the 1950s. Although fertility among Sephardic Jews ultimately fell to close to the Israeli average, the AA cohorts in our sample come from much larger families than other Jews. While almost 60 percent of AA Jews in the 2+ sample come from families with 4 or more children, only 26 percent of other Jews in the sample come from families this large.

In addition to having higher fertility, the AA group is less educated and 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 big in larger 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). These differences suggest estimates in the AA sub-sample may be especially relevant for poorer populations.

OLS estimates by ethnicity, reported in columns 1 and 5 of Table 8, generally show somewhat larger adverse effects on schooling and labor market outcomes in the non-AA sample than in the AA sample. All of the 2SLS estimates in Table 8 are for the full parity-pooled sample including the union of subjects from 2+, 3+, 4+, and 5+ families. The resulting 2SLS estimates by ethnicity generate no evidence of an effect on human capital or labor market variables for either ethnic group. The most precise 2SLS estimates use the full set of instruments, with results reported in columns 4 and 8. For example, the estimated effect on highest grade completed using all instruments in the non-AA sample is.043 (s.e.=.064), while the corresponding estimate for AA is .031 (s.e.=.057). The estimated effects on log earnings from the same specification is .047 (s.e.=.036) for non-AA and -.023 (s.e.=.022) for AA.

As in the sample that does not differentiate by ethnicity, there is again evidence for an effect of family size on marriage rates or timing in both groups. For example, the estimated effects on marriage using all instruments are .030 (s.e.=.011) for non-AA subjects and .035 (s.e.=.010) for AA subjects. Effects on early marriage are almost identical in the two groups. The effects on fertility are mostly insignificant but still consistently positive in both samples, with similar magnitudes in the combined-instrument specification.

Most of our empirical analysis focuses on Jewish subjects because the sample size for Jews is large, match rates between the Census and registry are higher for Jews than non-Jews, and birth records are more complete for Jews. For example, the probability of finding mother and siblings for Arab subjects appearing in the 1995 Census (almost all of whom are Israeli-born) is about 70 percent for Arabs born 1955 or later. The corresponding match rate for Israeli-born Jews is almost 90 percent. Unpublished tabulations from the Central Bureau of Statistics suggest that, for this population about 60 percent of Arab births are located, in contrast with 87 percent of births for Jews.

These data shortcomings notwithstanding, the Arab minority in Israel is of considerable substantive interest due to their high fertility and relatively low living standards. The sibship size for Muslem Arabs in our 2+ and 3+ samples is about 7 children. A second factor of interest is the lower level of social services available to Israeli Arabs (who were under military rule until 1966). This group may be especially likely to reduce parental investment in response to the resource constraints induced by exogenous fertility shocks. We therefore report a reduced set of estimates for Arabs, focusing on the largest parity-pooled sample, but including only subjects whose mothers were born after 1940. These restrictions generate a sample with better fertility coverage and higher match rates.[19]

OLS estimates for the Arab sample, reported in column 9 of Table 8, are negative, though the OLS estimate for highest grade completed is considerably more negative for Arabs than for Jews, while the OLS estimates for labor market outcomes are less. None of the corresponding 2SLS estimates of effects on Arabs schooling and the labor market outcomes' are significantly different from zero, though they are also imprecise. Twins instruments generate imprecise estimates in this case because the twins first stage is smaller and less robust for Arabs than for Jews due to Arabs' larger family size. On the other hand, sex composition instruments generate reasonably precise estimates, as can be seen in column 11. The resulting estimates for highest grade completed and log earnings are -.021 (s.e.=.082) and -.040 (s.e.=.028). Estimates combining both instrument sets, reported in column 12, are similar and slightly more precise than those in column 11. A final set of noteworthy results from the Arab sample are the positive and significant effects on marriage and fertility in columns 11-12. On balance, therefore, the results for Arabs are consistent with those for Jews.

Effects on Men and Women

Also of interest are separate estimates for men and women, especially in view of the effects on marital status discussed above. We therefore estimated separate models by sex using the full set of instruments in the largest parity-pooled sample, with results reported in Table 9. The OLS estimates are similar for men and women. Again, however, 2SLS estimates by sex show no evidence of negative effects on schooling or labor market variables for either group. The estimated effects on men are mostly positive; two are close to marginally significant

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[19] The sample also omits Druze and Arabs born abroad or whose mothers were born abroad. Subjects with mothers whose first birth was before age 15 or after age 45 were also dropped. We chose a 1940 birth cohort cutoff for mothers because match rates improve sharply for children born after 1960. The resulting sample of first, second, third, and fourth borns includes 23,591 individuals with estimated fertility coverage of 65 percent.

2SLS estimates of effects on marriage rates are more pronounced for women than for men, and more precise. For example, the effect on women estimated using both twins and sex composition instruments, reported in column 8, is .04 (s.e.=.009), while the corresponding effect for men, reported in column 4, is .021 (s.e.=.011). Moreover, the estimated effects on early marriage for women are on the order of 6-7 percentage points and significantly different from zero whether estimated using twins or sex-composition instruments (see columns 6-8). In contrast, the corresponding estimates for men are negative and insignificant.

The consistency and relative precision of results across instrument sets suggests that early marriage may indeed be a consequence of increased family size, especially for older daughters. The marriage effects seem to generate a small effect on fertility as well (also apparent in Table 7). Stronger marriage effects for women may reflect the fact that marriage is the main route to an independent household for girls in traditional Jewish families. Moreover, older daughters in Israel may be tempted to marry sooner when crowded by younger sisters. This is consistent with traditional Jewish values and can be traced back to the Biblical story of Rachel and Leah's joint betrothal to Jacob. We might therefore expect marriage effects estimated using sex-composition instruments to be larger than effects estimated using twins instruments, as seems to be the case.

V. Possible Explanations

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 increases the shadow price of quality. Along these lines, Rosenzweig and Wolpin (1980) interpret twin births as 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. While the quantity-quality tradeoff is less clear-cut in more recent theoretical discussions, the traditional view provides an intellectual foundation for policies that attempt to reduce family size in LDCs.

The first question our findings raise is what might account for the absence of a causal link between sibship size and child welfare. A definitive answer to this question must await future empirical research. Here, we briefly review a number of possible explanations. One theoretical 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 financial markets were 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.[20] For example, parents may work longer hours or take fewer or less expensive vacations (i.e., consume less leisure). Parents may also substitute away from personal as opposed to family consumption (e.g., by drinking less alcohol). Direct evidence on this point is difficult to obtain since consumption data rarely come in the form needed to replicate our research design.

The Angrist-Evans (1998) 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, estimates of AE-98 type models for samples of Israeli mothers show only modest effects of child-bearing on labor supply (Marmer, 2000). A related channel is the effect of family size on marital stability (Heaton, 1990).

A third sort of explanation for the absence of a causal link between sibship size and the outcomes studied here might be called "marginally irrelevant inputs." Using research designs similar to ours,

[20] 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, 2003). We confirmed this in an exploratory analysis allowing changes in eligibility and the level of child allowances across cohorts to interact with the instruments.

Caceres (2004) finds 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, consistent with crowding effects that might lead to earlier marriage. Private school attendance, room-sharing, and early marriage (at least for girls) may matter little for human capital and earnings outcomes. A final explanation that is consistent with our findings is that the presence of siblings directly enhances child welfare, perhaps because children with siblings benefit socially or take on more responsibility sooner. This conjecture is consistent with Qian's (2004) IV estimates for China, which show that the presence of a younger sibling increases older children's school enrollment.

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, economic well-being, and family structure later in life. Our research design exploits a wide range of variation in fertility due to multiple births and preferences for a mixed sibling-sex composition, along with ethnicity interactions and preferences for male children. The natural experiments embodied in these IV strategies have been shown to be driven by different ethnic groups and to reflect a wide range of fertility variation. Our strategy of combining evidence from different natural experiments reinforces the external validity of each underlying IV strategy.

The evidence reported here 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. The estimates do suggest, however, that girls from larger families marry sooner. This marriage effect may have a modest effect on fertility, but it does not appear to reduce schooling, employment, or earnings. In future work, we hope to shed light on possible explanations by generating new evidence on the effect of family size on resource allocation across generations.

DATA APPENDIX: ADDITION DETAILS ON RECORD LINKAGE

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 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.

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.

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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 (1)	Subject born ≥ 1955 (2)	Subject born<1955 (3)	Subject born ≥ 1955 (4)	Subject born<1955 (5)	Subject born ≥ 1955 (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	232	3,657	730	3,425	1,519	12,095
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 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	0.74	1.00	1.00	0.87	1.00	1.00
More than 3 kids	0.400	0.545	0.573	0.593	0.686	0.704
Family composition						
Twins at second birth	0.009	0.006	0.000	0.008	0.006	0.000
Twins at third birth	0.007	0.010	0.010	0.008	0.009	0.009
Boy at first birth	0.517	0.518	0.527	0.518	0.518	0.528
Boy at second birth	0.514	0.515	0.507	0.516	0.514	0.504
Boy at third birth	0.515	0.515	0.517	0.509	0.509	0.516
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	0.115	0.114	0.117	0.117	0.113
Boy123=1	0.140	0.140	0.140	0.138	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	49.1	48.8	50.4	49.7	49.5	50.7
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	0.805	0.830
Former USSR	0.115	0.068	0.064	0.011	0.009	0.007
Europe-America	0.144	0.111	0.113	0.030	0.025	0.025
Father's ethnicity						
Israel	0.836	0.869	0.887	0.856	0.852	0.878
Asia-Africa	0.426	0.501	0.535	1.000	1.000	1.000
Former USSR	0.114	0.068	0.068	-	-	-
Europe-America	0.186	0.149	0.148	-	-	-

Table 2 (cont.)

	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)
Subject ethnicity						
Israel	0.836	0.869	0.887	0.856	0.852	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	0.027	0.025	-	-	-
Education Outcomes						
Highest grade completed	12.6	12.5	12.3	12.2	12.1	12.0
Schooling $\geq$ 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 $\geq$ 24)	0.291	0.262	0.224	0.177	0.169	0.143
College graduate (age $\geq$ 24)	0.202	0.180	0.153	0.117	0.111	0.093
Labor Market Outcomes (age $\geq$ 22)						
Worked during the year	0.827	0.820	0.809	0.812	0.809	0.798
Hours worked last week	32.6	32.4	31.7	32.5	32.4	31.7
Monthly earnings (in 1995 Shekels)	2997	2920	2721	2847	2820	2621
Ln(monthly earnings)	8.08	8.07	8.03	8.07	8.07	8.02
Marriage and fertility						
Married on census day	0.446	0.465	0.418	0.519	0.530	0.479
Married by age 21 (age $\geq$ 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
Number of observations	89,445	65,673	52,964	38,063	32,875	28,357

Notes: The table reports descriptive statistics for the main 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- 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-Africa 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	0.437 (0.050)	0.625 (0.057)	-	-	-	-
Twins-2 x Asia-Africa	-	-0.484 (0.105)	-	-	-	-
Twins-3	-	-	0.522 (0.045)	0.583 (0.045)	0.585 (0.043)	0.692 (0.049)
Twins-3 x Asia-Africa	-	-	-	-0.132 (0.094)	-	-0.226 (0.086)
Male	-0.018 (0.010)	0.000 (0.012)	0.016 (0.018)	0.018 (0.023)	0.014 (0.011)	0.006 (0.015)
Male x Asia-Africa	-	-0.041 (0.022)	-	-0.005 (0.035)	-	0.015 (0.022)
Asia-Africa	0.242 (0.015)	0.267 (0.019)	0.166 (0.016)	0.161 (0.027)	0.083 (0.014)	0.069 (0.021)

Notes: The table reports first-stage effects of twins-2 and twins-3 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 4: Sex-Composition First Stages

	2+				3+							
	First Borns				First Borns				First and Second Borns			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Samesex	0.073	0.071	-	-	0.133	0.095	-	-	0.120	0.070	-	-
	(0.010)	(0.012)			(0.017)	(0.022)			(0.015)	(0.019)		
Samesex x Asia-Africa	-	0.005	-	-	-	0.076	-	-	-	0.097	-	-
		(0.022)				(0.034)				(0.029)		
All girls	-	-	0.110	0.088	-	-	0.180	0.095	-	-	0.183	0.072
			(0.015)	(0.017)			(0.025)	(0.032)			(0.022)	(0.027)
All girls x Asia-Africa	-	-	-	0.051	-	-	-	0.169	-	-	-	0.217
				(0.028)				(0.051)				(0.043)
All boys	-	-	0.039	0.055	-	-	0.092	0.095	-	-	0.065	0.070
			(0.015)	(0.016)			(0.023)	(0.029)			(0.021)	(0.025)
All boys x Asia-Africa	-	-	-	-0.038	-	-	-	-0.005	-	-	-	-0.009
				(0.026)				(0.047)				(0.041)
Boy3 x (1-samesex12)	-	-	-	-	-	-	-0.080	-0.053	-	-	-0.077	-0.044
							(0.018)	(0.023)			(0.015)	(0.019)
Boy3 x (1-samesex12) x Asia-Africa	-	-	-	-	-	-	-	-0.054	-	-	-	-0.064
								(0.035)				(0.030)

Notes: The table reports first-stage effects of sex-composition and boy-3 on number of children. The sample for columns 1-4 includes first born non-twins from families with 2 or more births. The sample for columns 5-8 includes first born non-twins from families with 3 or more births. The sample for columns 9-12 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 the notes to Table 3. Standard errors in columns 9-12 are clustered by mother's ID.

Table 5: Estimates 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	0.174	0.105	0.294	0.222	0.160
		(0.005)	(0.005)	(0.166)	(0.131)	(0.184)	(0.176)	(0.106)
Years of schooling ≥ 12	0.824	-0.037	-0.029	0.030	0.024	-0.009	-0.015	0.007
		(0.001)	(0.001)	(0.028)	(0.021)	(0.028)	(0.028)	(0.017)
Matriculation certificate	0.487	-0.054	-0.033	-0.004	0.001	0.100	0.077	0.035
		(0.001)	(0.001)	(0.038)	(0.033)	(0.043)	(0.040)	(0.025)
Some College (age ≥ 24)	0.291	-0.049	-0.023	0.017	0.026	0.089	0.089	0.057
		(0.001)	(0.001)	(0.052)	(0.046)	(0.048)	(0.046)	(0.032)
College graduate (age ≥ 24)	0.202	-0.036	-0.015	-0.021	-0.006	0.115	0.115	0.054
		(0.001)	(0.001)	(0.045)	(0.041)	(0.046)	(0.044)	(0.028)
Labor Market Outcomes (age ≥ 22)								
Worked during the year	0.827	-0.025	-0.024	-0.005	0.002	0.062	0.072	0.034
		(0.001)	(0.001)	(0.038)	(0.033)	(0.043)	(0.043)	(0.026)
Hours worked last week	32.6	-1.06	-1.20	-0.97	0.00	1.46	1.06	0.51
		(0.05)	(0.06)	(2.58)	(2.18)	(2.06)	(1.98)	(1.45)
Monthly earnings (in 1995 Shekels)	2997	-217.0	-179.1	-7.7	73.0	266.7	429.1	264.1
		(7.4)	(8.0)	(394.1)	(324.5)	(283.6)	(292.1)	(214.2)
Ln(monthly earnings)	8.08	-0.034	-0.025	-0.045	0.021	-0.052	-0.067	-0.025
		(0.002)	(0.002)	(0.107)	(0.088)	(0.092)	(0.082)	(0.059)
Marriage and fertility								
Married on census day	0.446	0.023	0.020	0.043	0.060	0.118	0.101	0.078
		(0.001)	(0.001)	(0.029)	(0.025)	(0.034)	(0.032)	(0.020)
Married by age 21 (age ≥ 21)	0.172	0.027	0.022	-0.006	0.024	0.197	0.192	0.110
		(0.001)	(0.001)	(0.037)	(0.032)	(0.047)	(0.046)	(0.026)
Number of own children	1.00	0.126	0.110	0.171	0.039	0.191	0.178	0.116
		(0.004)	(0.005)	(0.131)	(0.086)	(0.096)	(0.097)	(0.064)
Any children	0.448	0.029	0.019	0.090	0.013	0.135	0.134	0.079
		(0.001)	(0.001)	(0.056)	(0.036)	(0.041)	(0.041)	(0.026)

Notes: The table reports means of dependent variables in column 1 and OLS estimates of the coefficient on family size in columns 2-3. 2SLS estimates using different sets of instruments appear in 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 Table 3. Robust standard errors are reported in parenthesis.

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

		2SLS -- Instrument list						
Outcome	OLS all covs. (1)	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	-0.116	-0.067	0.069	0.072	0.171
	(0.005)	(0.117)	(0.110)	(0.134)	(0.120)	(0.080)	(0.076)	(0.101)
Years of schooling ≥ 12	-0.031	0.024	0.025	0.000	-0.009	0.009	0.006	0.016
	(0.001)	(0.019)	(0.018)	(0.023)	(0.022)	(0.014)	(0.013)	(0.017)
Matriculation certificate	-0.033	0.057	0.065	-0.020	0.007	0.038	0.042	0.068
	(0.001)	(0.026)	(0.025)	(0.030)	(0.027)	(0.018)	(0.017)	(0.023)
Some College (age ≥ 24)	-0.021	0.059	0.060	-0.051	-0.025	0.011	0.003	0.029
	(0.001)	(0.036)	(0.036)	(0.031)	(0.025)	(0.021)	(0.018)	(0.027)
College graduate (age ≥ 24)	-0.014	0.052	0.055	-0.060	-0.032	0.004	0.001	0.030
	(0.001)	(0.032)	(0.032)	(0.028)	(0.022)	(0.018)	(0.016)	(0.024)
Labor Market Outcomes (age ≥ 22)								
Worked during the year	-0.027	0.029	0.034	0.033	0.031	0.032	0.035	0.038
	(0.001)	(0.025)	(0.024)	(0.029)	(0.027)	(0.018)	(0.017)	(0.021)
Hours worked last week	-1.40	2.35	2.51	1.31	1.44	1.94	1.79	2.08
	(0.05)	(1.45)	(1.43)	(1.36)	(1.28)	(0.94)	(0.88)	(1.23)
Monthly earnings (in 1995 Shekels)	-184.5	47.2	63.9	118.8	109.2	90.0	93.1	76.3
	(6.8)	(204.1)	(203.8)	(176.5)	(162.2)	(128.7)	(120.8)	(175.7)
Ln(monthly earnings)	-0.027	0.015	0.010	0.011	0.029	0.020	0.002	-0.021
	(0.002)	(0.048)	(0.048)	(0.050)	(0.046)	(0.033)	(0.030)	(0.041)
Marriage and fertility								
Married on census day	0.020	0.021	0.015	0.024	0.045	0.029	0.022	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	0.046	0.052	0.039	0.036	0.024
	(0.001)	(0.022)	(0.021)	(0.029)	(0.028)	(0.017)	(0.016)	(0.019)
Number of own children	0.114	-0.062	-0.068	-0.082	-0.037	-0.052	-0.069	-0.101
	(0.004)	(0.062)	(0.058)	(0.071)	(0.064)	(0.044)	(0.042)	(0.056)
Any children	0.022	0.021	0.015	-0.002	0.016	0.015	0.013	0.010
	(0.001)	(0.028)	(0.026)	(0.025)	(0.023)	(0.017)	(0.016)	(0.023)

Notes: The table reports OLS estimates of the coefficient on family size in column 1. 2SLS estimates using different sets of instruments appear in 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 described in Table 2. Regression estimates are from models that include the control variables specified in Table 3. Standard errors are clustered by mother's ID.

Table 7: Estimates for Parity-Pooled Samples

	Twins			Sex composition			All		
	2+, 3+	2+, 3+, 4+	2+, 3+, 4+, 5+	2+, 3+	2+, 3+, 4+	2+, 3+, 4+, 5+	2+, 3+	2+, 3+, 4+	2+, 3+, 4+, 5+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Schooling									
Highest grade completed	0.145	0.060	0.031	0.036	0.036	0.054	0.092	0.050	0.040
	(0.079)	(0.059)	(0.055)	(0.093)	(0.074)	(0.068)	(0.060)	(0.046)	(0.043)
Years of schooling ≥ 12	0.022	0.002	-0.006	-0.013	-0.015	-0.010	0.006	-0.005	-0.008
	(0.013)	(0.011)	(0.010)	(0.016)	(0.014)	(0.013)	(0.010)	(0.008)	(0.008)
Matriculation certificate	0.036	0.009	-0.001	0.037	0.026	0.013	0.036	0.016	0.005
	(0.019)	(0.014)	(0.012)	(0.021)	(0.016)	(0.014)	(0.014)	(0.010)	(0.009)
Some College (age ≥ 24)	0.041	0.010	0.004	0.002	0.009	0.002	0.017	0.010	0.003
	(0.026)	(0.016)	(0.014)	(0.019)	(0.015)	(0.014)	(0.016)	(0.011)	(0.010)
College graduate (age ≥ 24)	0.026	0.012	0.011	0.007	0.011	0.007	0.014	0.012	0.010
	(0.023)	(0.014)	(0.012)	(0.017)	(0.013)	(0.012)	(0.014)	(0.010)	(0.009)
Labor Market Outcomes (age ≥ 22)									
Worked during the year	0.016	-0.007	-0.001	0.044	0.004	0.011	0.031	-0.002	0.005
	(0.018)	(0.014)	(0.013)	(0.020)	(0.016)	(0.014)	(0.013)	(0.010)	(0.009)
Hours worked last week	1.30	0.37	0.66	1.25	-0.41	0.11	1.27	0.00	0.40
	(1.09)	(0.76)	(0.69)	(0.98)	(0.77)	(0.71)	(0.72)	(0.54)	(0.49)
Monthly earnings (in 1995 Shekels)	27.6	-37.4	36.6	187.5	48.8	57.2	118.7	5.3	47.0
	(157.8)	(121.8)	(107.1)	(132.5)	(97.5)	(88.8)	(101.6)	(78.6)	(70.2)
Ln(monthly earnings)	0.001	0.009	0.012	-0.019	-0.017	-0.010	-0.008	-0.004	0.001
	(0.039)	(0.030)	(0.027)	(0.035)	(0.028)	(0.027)	(0.026)	(0.021)	(0.019)
Marriage and fertility									
Married on census day	0.030	0.029	0.023	0.046	0.053	0.045	0.037	0.039	0.033
	(0.013)	(0.010)	(0.010)	(0.017)	(0.013)	(0.012)	(0.010)	(0.008)	(0.007)
Married by age 21 (age ≥ 21)	0.024	0.023	0.020	0.078	0.058	0.052	0.052	0.039	0.035
	(0.016)	(0.012)	(0.012)	(0.021)	(0.017)	(0.015)	(0.013)	(0.010)	(0.009)
Number of own children	-0.011	0.037	0.046	-0.012	0.016	0.016	-0.012	0.025	0.030
	(0.045)	(0.040)	(0.038)	(0.050)	(0.039)	(0.036)	(0.034)	(0.028)	(0.026)
Any children	0.020	0.017	0.021	0.040	0.049	0.042	0.031	0.033	0.032
	(0.020)	(0.014)	(0.013)	(0.018)	(0.015)	(0.013)	(0.013)	(0.010)	(0.009)

Notes: The table reports 2SLS estimates of parity-pooled models using different sets of instruments and sub-samples. For example, the columns headed "2+, 3+" report results from the union of 2+ and 3+ samples. Standard errors are clustered by mother's ID.

Table 8: Full Specification By Ethnicity

	Israel/Europe				Asia/Africa				Arabs			
	OLS (1)	Twins (2)	Sex comp. (3)	All (4)	OLS (5)	Twins (6)	Sex comp. (7)	All (8)	OLS (9)	Twins (10)	Sex comp. (11)	All (12)
Schooling												
Highest grade completed	-0.117 (0.006)	0.030 (0.068)	0.126 (0.170)	0.043 (0.064)	-0.117 (0.005)	0.031 (0.094)	0.026 (0.073)	0.031 (0.057)	-0.291 (0.013)	-0.520 (0.319)	-0.021 (0.082)	-0.049 (0.080)
Years of schooling ≥ 12	-0.029 (0.001)	-0.003 (0.012)	0.015 (0.028)	0.000 (0.011)	-0.023 (0.001)	-0.008 (0.018)	-0.019 (0.014)	-0.014 (0.011)	-0.039 (0.002)	-0.036 (0.044)	-0.015 (0.012)	-0.016 (0.012)
Matriculation certificate	-0.038 (0.001)	0.007 (0.017)	0.075 (0.043)	0.017 (0.015)	-0.023 (0.001)	-0.012 (0.019)	0.002 (0.015)	-0.003 (0.012)	-0.027 (0.002)	-0.014 (0.043)	-0.004 (0.011)	-0.005 (0.011)
Some College (age ≥ 24)	-0.033 (0.001)	0.011 (0.021)	0.008 (0.040)	0.010 (0.019)	-0.014 (0.001)	-0.008 (0.017)	0.004 (0.014)	-0.001 (0.011)	-0.013 (0.001)	0.002 (0.032)	-0.005 (0.009)	-0.005 (0.009)
College graduate (age ≥ 24)	-0.021 (0.001)	0.016 (0.019)	0.030 (0.036)	0.019 (0.017)	-0.010 (0.001)	0.003 (0.015)	0.004 (0.012)	0.003 (0.009)	-0.008 (0.001)	-0.022 (0.023)	-0.003 (0.008)	-0.005 (0.007)
Labor Market Outcomes (age ≥ 22)												
Worked during the year	-0.039 (0.001)	-0.012 (0.018)	0.016 (0.035)	-0.007 (0.016)	-0.014 (0.001)	0.013 (0.018)	0.009 (0.016)	0.011 (0.012)	-0.007 (0.002)	0.020 (0.037)	-0.010 (0.011)	-0.008 (0.010)
Hours worked last week	-2.23 (0.07)	0.41 (1.01)	-0.01 (1.81)	0.30 (0.87)	-0.67 (0.05)	0.98 (0.94)	0.06 (0.76)	0.45 (0.59)	-0.15 (0.07)	0.58 (1.73)	-0.19 (0.44)	-0.15 (0.43)
Monthly earnings (in 1995 Shekels)	-239.1 (8.6)	93.5 (175.9)	203.1 (272.3)	119.6 (147.0)	-120.5 (6.5)	-37.6 (123.2)	39.8 (92.1)	7.0 (74.2)	-23.0 (6.3)	39.5 (111.5)	-84.8 (38.5)	-77.1 (37.1)
Ln(monthly earnings)	-0.026 (0.003)	0.043 (0.043)	0.058 (0.070)	0.047 (0.036)	-0.023 (0.002)	-0.018 (0.034)	-0.026 (0.028)	-0.023 (0.022)	0.001 (0.003)	-0.005 (0.095)	-0.040 (0.028)	-0.036 (0.027)
Marriage and fertility												
Married on census day	0.034 (0.001)	0.029 (0.012)	0.035 (0.028)	0.030 (0.011)	0.010 (0.001)	0.014 (0.017)	0.046 (0.013)	0.035 (0.010)	0.018 (0.002)	-0.017 (0.040)	0.022 (0.011)	0.020 (0.011)
Married by age 21 (age ≥ 21)	0.039 (0.001)	0.032 (0.016)	0.039 (0.034)	0.034 (0.014)	0.014 (0.001)	0.011 (0.017)	0.056 (0.017)	0.038 (0.012)	0.021 (0.002)	-0.036 (0.046)	0.055 (0.015)	0.048 (0.014)
Number of own children	0.149 (0.006)	0.035 (0.047)	0.023 (0.108)	0.032 (0.043)	0.078 (0.004)	0.073 (0.065)	0.011 (0.039)	0.030 (0.034)	0.090 (0.008)	0.129 (0.152)	0.105 (0.044)	0.106 (0.042)
Any children	0.034 (0.002)	0.025 (0.018)	0.022 (0.038)	0.024 (0.016)	0.015 (0.001)	0.026 (0.022)	0.042 (0.014)	0.038 (0.012)	0.014 (0.002)	-0.029 (0.043)	0.031 (0.014)	0.025 (0.013)

Notes: The table reports OLS and 2SLS results from models estimated separately by ethnicity. The 2SLS cstimates are from models estimated for the full parity-pooled samples (i.e. corresponding to columns 3,6, and 9 in table 7). Standard errors are clustered by mother's ID.

Table 9: Full Specification By Sex

	Males				Females			
	OLS (1)	Twins (2)	Sex comp. (3)	All (4)	OLS (5)	Twins (6)	Sex comp. (7)	All (8)
Schooling								
Highest grade completed	-0.150	0.040	0.206	0.078	-0.096	0.027	0.015	0.020
	(0.005)	(0.080)	(0.157)	(0.073)	(0.005)	(0.069)	(0.073)	(0.050)
Years of schooling ≥ 12	-0.033	-0.003	0.022	0.002	-0.019	-0.004	-0.018	-0.011
	(0.001)	(0.015)	(0.031)	(0.014)	(0.001)	(0.012)	(0.013)	(0.009)
Matriculation certificate	-0.023	0.012	0.060	0.023	-0.033	-0.018	0.003	-0.007
	(0.001)	(0.018)	(0.033)	(0.016)	(0.001)	(0.016)	(0.016)	(0.011)
Some College (age ≥ 24)	-0.017	0.007	0.015	0.010	-0.019	0.002	-0.002	0.000
	(0.001)	(0.020)	(0.027)	(0.016)	(0.001)	(0.018)	(0.015)	(0.012)
College graduate (age ≥ 24)	-0.011	0.017	0.027	0.021	-0.013	0.007	0.004	0.006
	(0.001)	(0.017)	(0.023)	(0.014)	(0.001)	(0.017)	(0.014)	(0.011)
Labor Market Outcomes (age ≥ 22)								
Worked during the year	-0.024	0.002	0.041	0.013	-0.021	-0.006	0.006	0.000
	(0.001)	(0.016)	(0.027)	(0.014)	(0.001)	(0.018)	(0.017)	(0.012)
Hours worked last week	-1.31	0.80	-0.05	0.56	-1.01	0.35	0.17	0.23
	(0.06)	(1.10)	(1.64)	(0.91)	(0.05)	(0.79)	(0.77)	(0.55)
Monthly earnings (in 1995 Shekels)	-219.6	-28.7	305.3	65.5	-109.1	73.3	-0.5	33.2
	(8.7)	(192.6)	(296.0)	(162.1)	(5.4)	(88.9)	(81.1)	(59.6)
Ln(monthly earnings)	-0.026	-0.017	0.023	-0.001	-0.024	0.049	-0.012	0.015
	(0.002)	(0.039)	(0.050)	(0.031)	(0.002)	(0.034)	(0.031)	(0.023)
Marriage and fertility								
Married on census day	0.016	0.020	0.021	0.021	0.022	0.025	0.053	0.040
	(0.001)	(0.012)	(0.023)	(0.011)	(0.001)	(0.014)	(0.013)	(0.009)
Married by age 21 (age ≥ 21)	0.013	-0.015	-0.006	-0.012	0.027	0.060	0.069	0.065
	(0.001)	(0.011)	(0.019)	(0.010)	(0.001)	(0.019)	(0.019)	(0.013)
Fertility (women only)								
Number of own children	-	-	-	-	0.101	0.046	0.016	0.030
					(0.003)	(0.038)	(0.036)	(0.026)
Any children	-	-	-	-	0.020	0.021	0.042	0.032
					(0.001)	(0.013)	(0.013)	(0.009)

Notes: The table reports OLS and 2SLS results from models estimated separately by sex. The 2SLS estimates are from models estimated for the full parity-pooled samples (i.e. corresponding to columns 3, 6, and 9 in table 7). Standard errors are clustered by mother's ID.

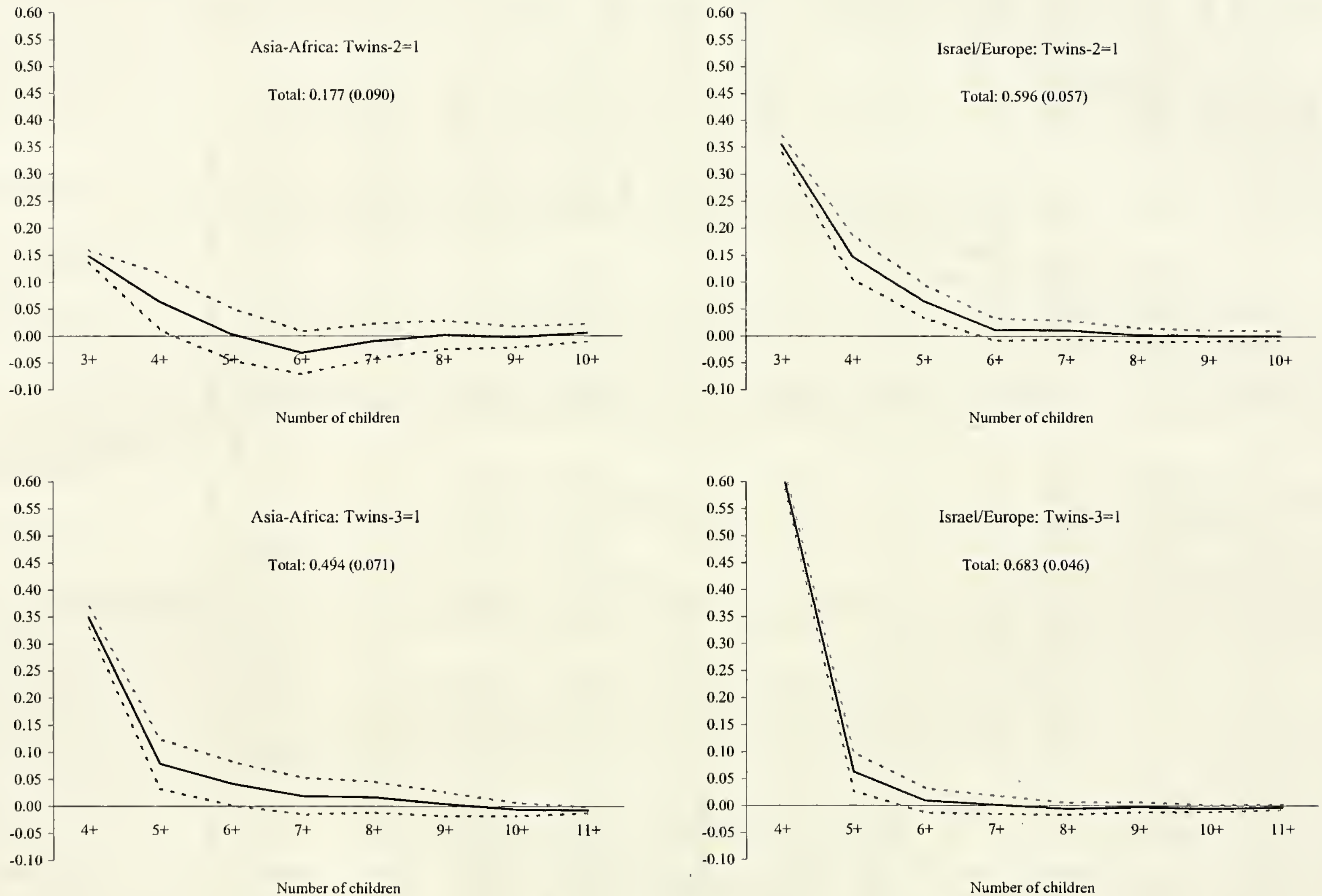


Figure 1: First borns in the 2+ sample, first stage effects of twins-2 (top panel). First and second borns in the 3+ sample, first stage effects of twins-3 (bottom panel).

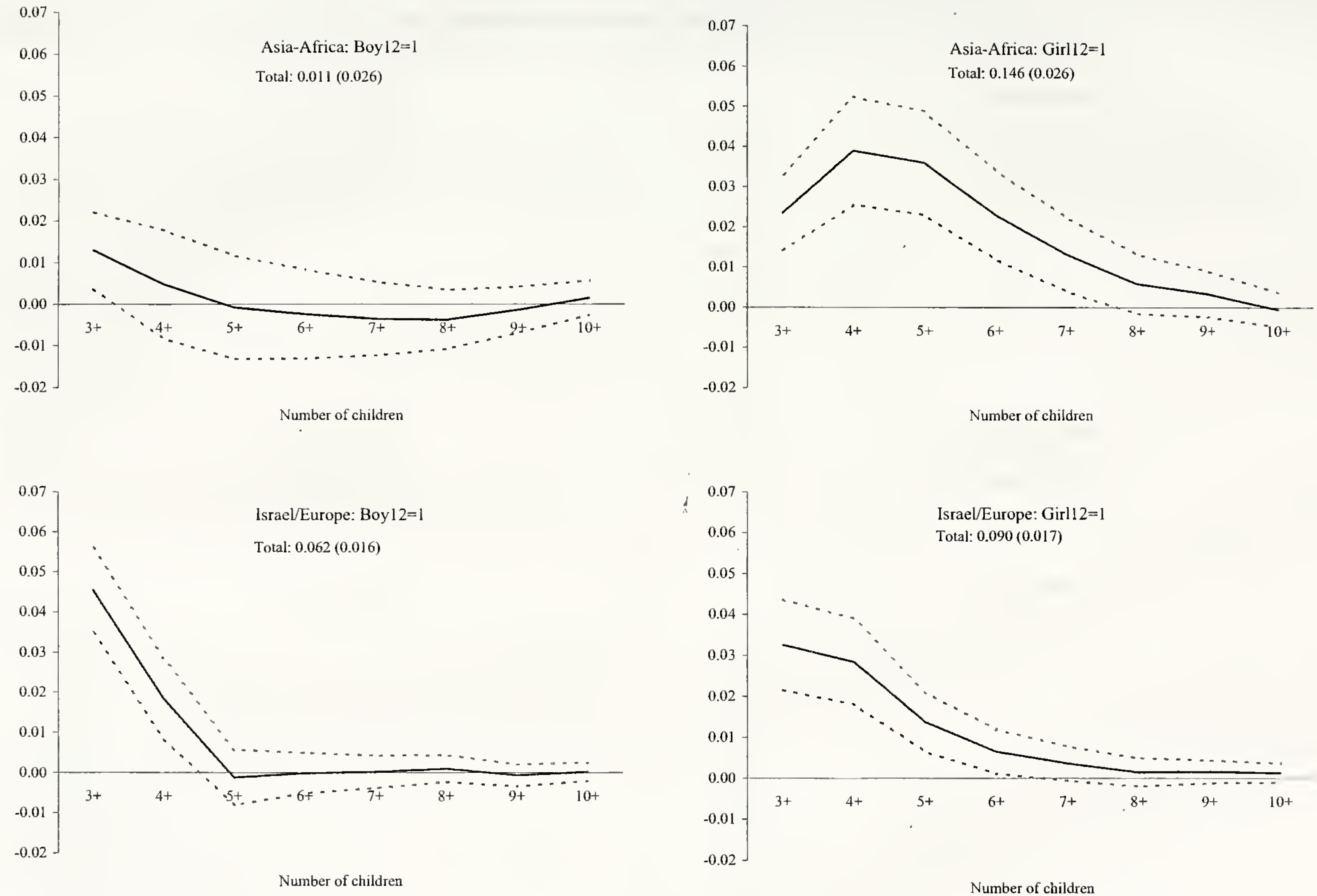


Figure 2: First-borns 2+ sample. First stage effects by ethnicity and type of sex-mix.

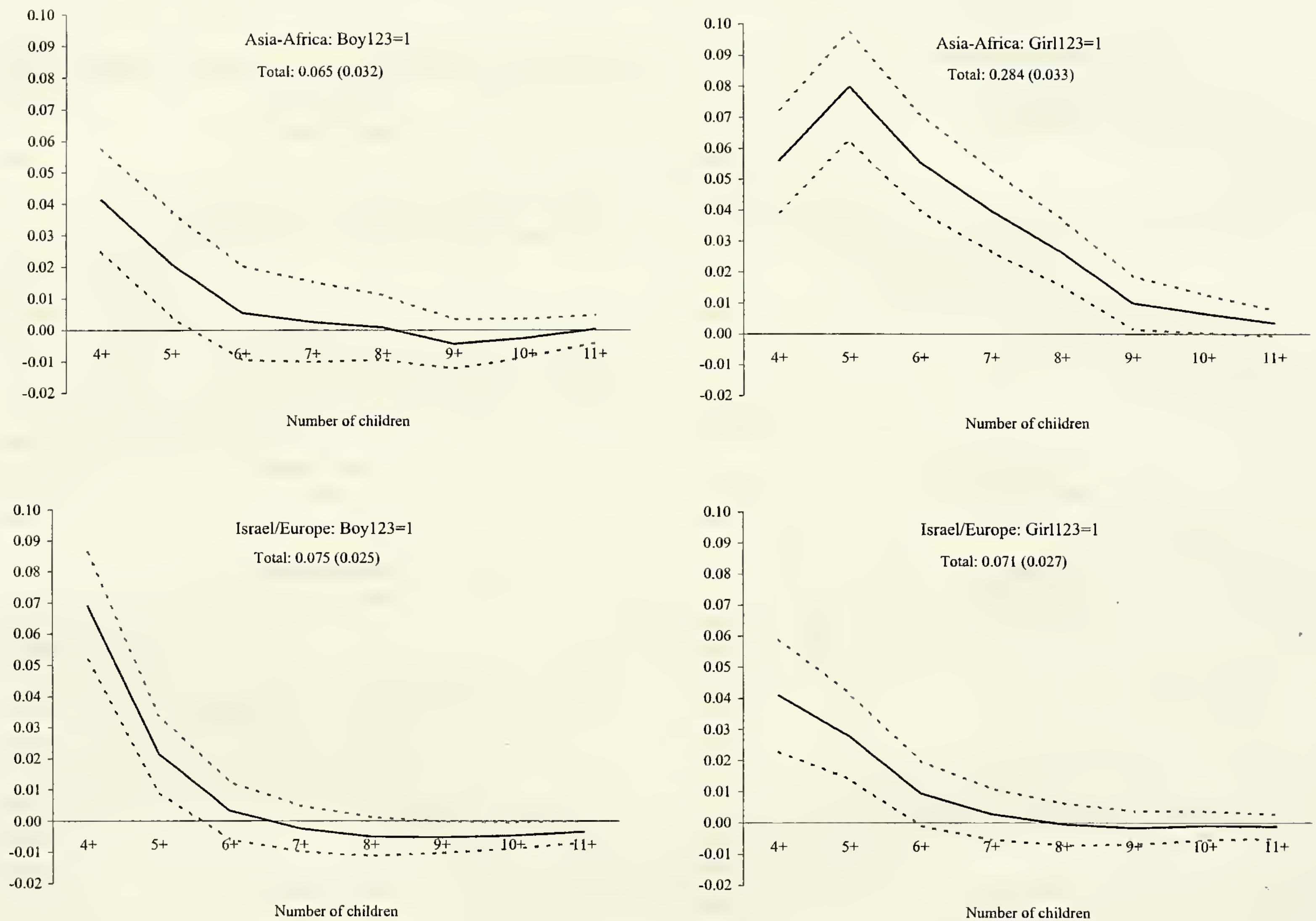


Figure 3: First and second borns 3+ sample. First stage effects by ethnicity and type of sex-mix.

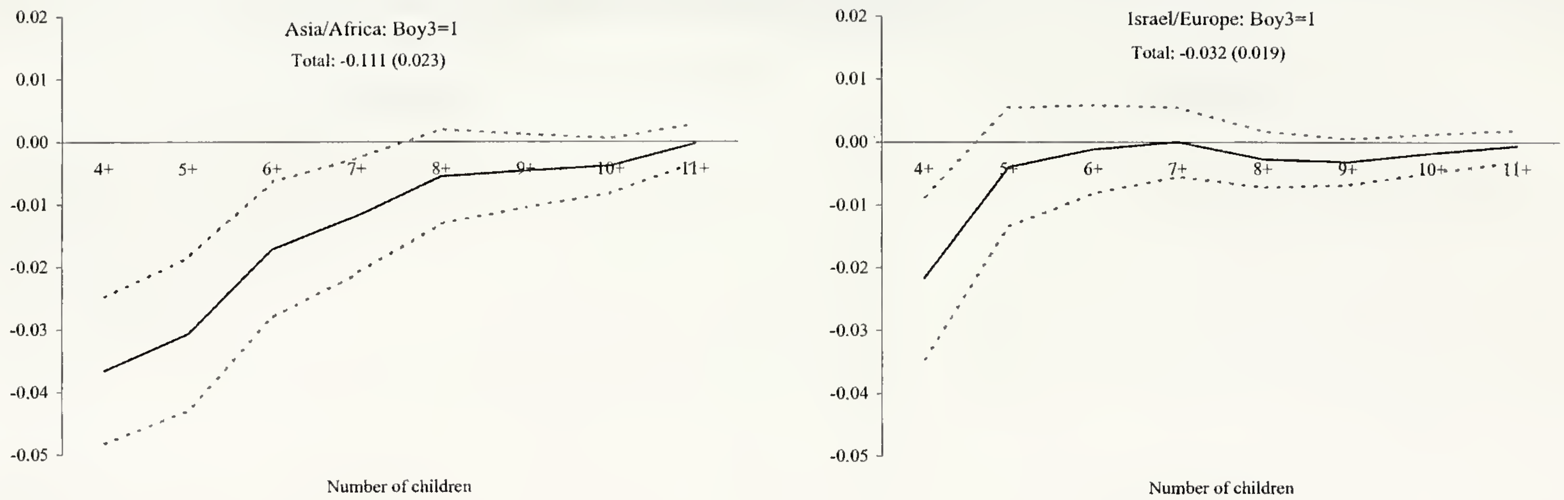


Figure 4: First and second borns 3+ sample. First stage effects of Boy3.

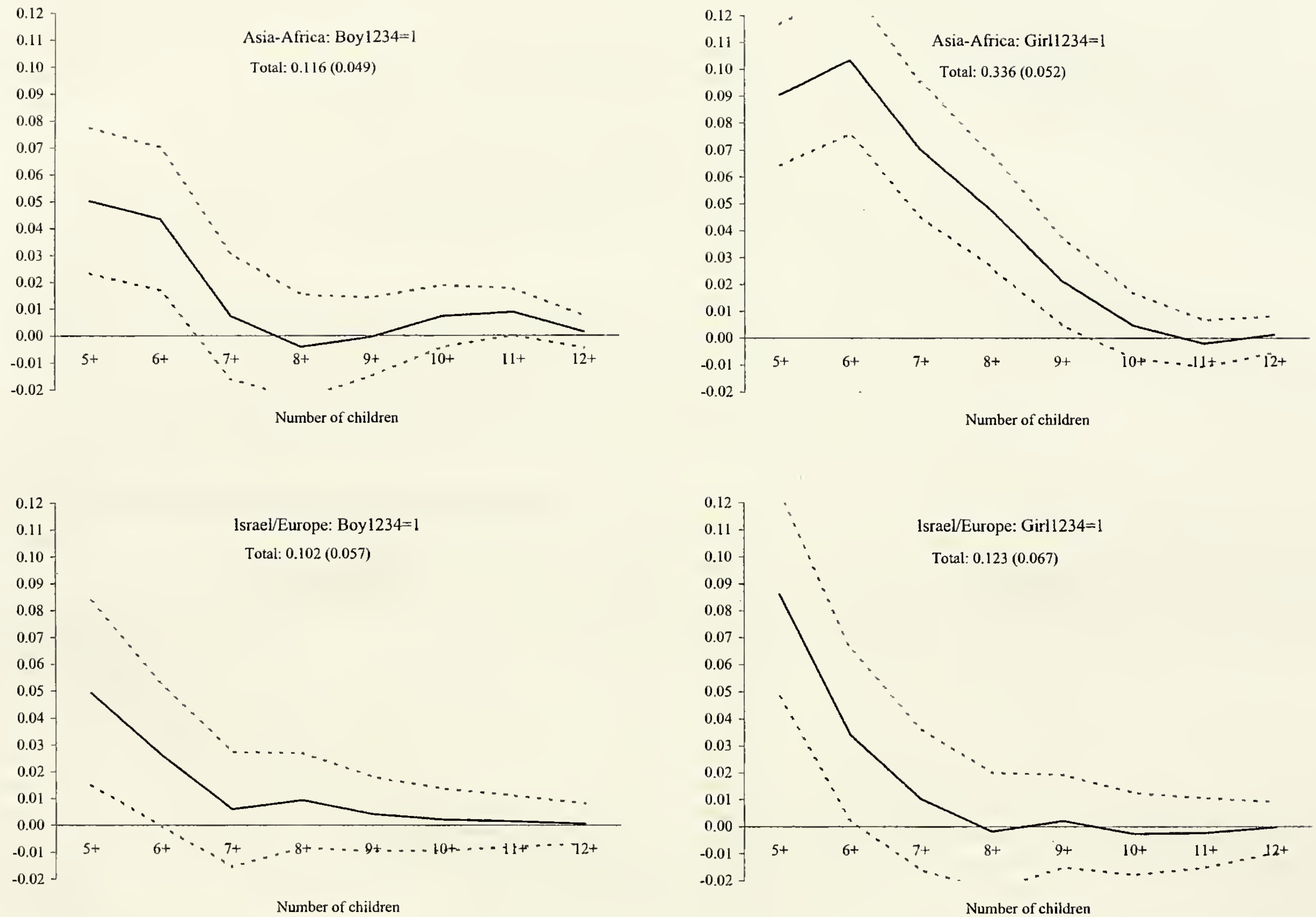


Figure 5: First, second, and third borns 4+ sample. First stage effects by ethnicity and type of sex-mix.

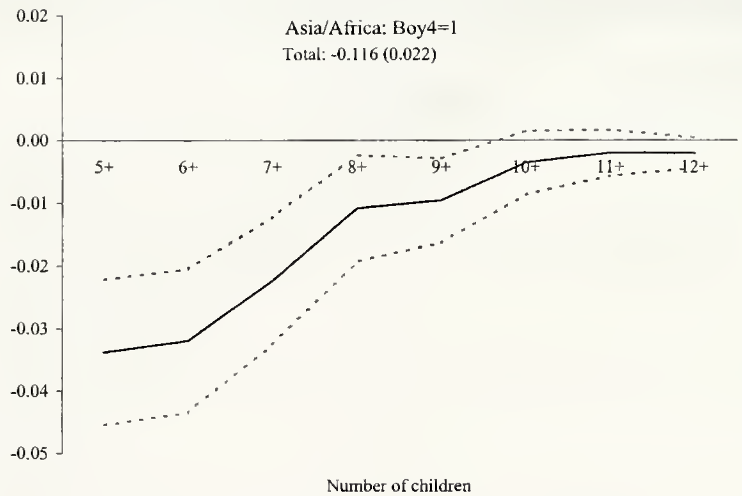


Figure 6: First, second, and third borns 4+ sample. First stage effects of Boy4.

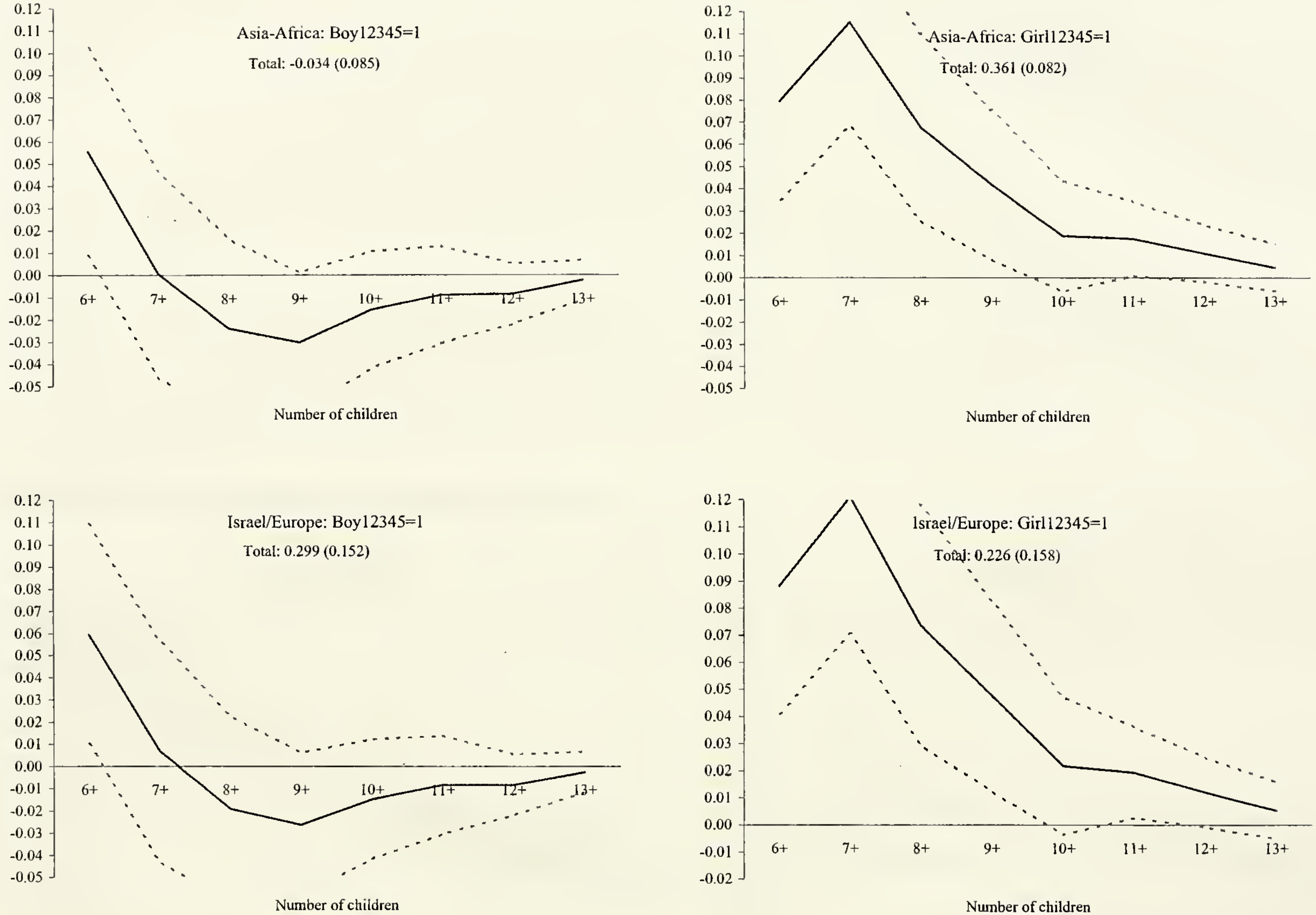
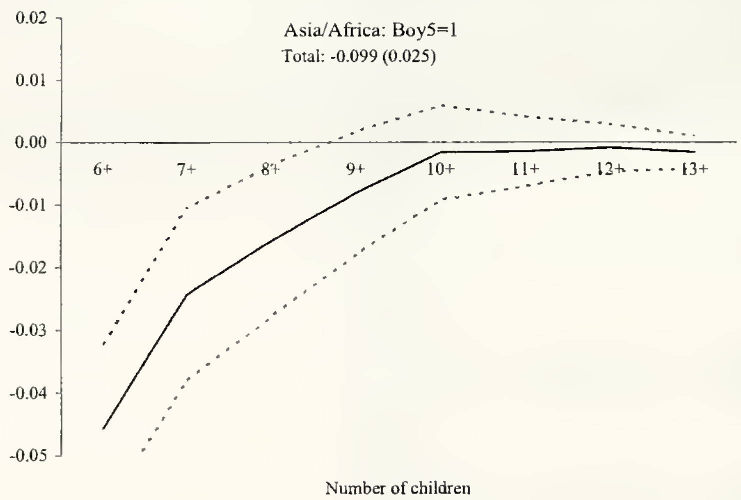


Figure 7: First, second, third, and fourth borns 5+ sample. First stage effects by ethnicity and type of sex-mix.



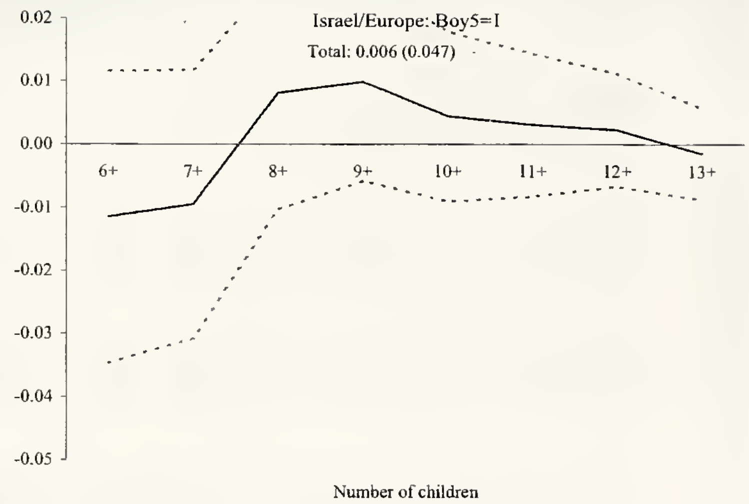


Figure 8: First, second, third, and fourth borns 5+ sample. First stage effects of Boy5.

Table A1: Pooled First Stage

	2+	3+		4+		5+	
	1^{st} borns	1^{st} borns	$1^{st}+2^{nd}$	1^{st} borns	$1^{st}+2^{nd}+3^{rd}$	1^{st} borns	$1^{st}+2^{nd}+3^{rd}+4^{th}$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Twins	0.637	0.594	0.698	0.879	0.998	0.629	0.714
	(0.057)	(0.045)	(0.049)	(0.114)	(0.110)	(0.232)	(0.162)
Twins x Asia-Africa	-0.482	-0.138	-0.228	-0.135	-0.291	-0.052	-0.159
	(0.105)	(0.094)	(0.086)	(0.148)	(0.137)	(0.268)	(0.196)
All Girls	0.090	0.102	0.079	0.158	0.127	0.147	0.242
	(0.017)	(0.032)	(0.027)	(0.083)	(0.068)	(0.212)	(0.161)
All Girls x Asia-Africa	0.052	0.165	0.212	0.202	0.212	0.173	0.123
	(0.032)	(0.051)	(0.043)	(0.105)	(0.085)	(0.245)	(0.180)
All Boys	0.061	0.103	0.077	0.096	0.102	0.489	0.307
	(0.017)	(0.029)	(0.025)	(0.073)	(0.058)	(0.218)	(0.155)
All Boys x Asia-Africa	-0.045	-0.011	-0.015	0.067	0.007	-0.535	-0.336
	(0.031)	(0.047)	(0.041)	(0.095)	(0.076)	(0.247)	(0.176)
Boy at last birth	-	-0.048	-0.038	-0.024	-0.024	0.039	0.031
(for mixed sex in n-1 births)		(0.023)	(0.019)	(0.036)	(0.029)	(0.067)	(0.048)
Boy at last birth x Asia-Africa	-	-0.057	-0.067	-0.085	-0.087	-0.112	-0.126
(for mixed sex in n-1 births)		(0.035)	(0.030)	(0.045)	(0.036)	(0.075)	(0.054)
Subect = boy	0.013	0.018	0.006	0.021	0.025	-0.023	0.002
	(0.017)	(0.023)	(0.015)	(0.045)	(0.024)	(0.090)	(0.039)
(Subject = boy) x Asia-Africa	0.007	-0.005	0.016	0.005	0.010	0.054	0.018
	(0.031)	(0.035)	(0.022)	(0.055)	(0.028)	(0.100)	(0.043)
Asia-Africa	0.242	0.189	0.103	-0.001	-0.054	-0.276	-0.253
	(0.024)	(0.032)	(0.026)	(0.056)	(0.041)	(0.111)	(0.072)

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. Regressions for columns 4-7 also include controls for twins and sex-composition at lower parity births. Regressions for columns 3,5, and 7 also include controls for birth order and spacing. Robust standard errors are reported in parenthesis. Standard errors in columns 3, 5, and 7 are clustered by mother's ID.