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

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

A longstanding question in the economics of the family is the relationship between sibship size and subsequent human capital formation and economic welfare. If there is a "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. Regression estimates and a large theoretical literature suggest this is indeed the case. This paper provides new evidence on the child-quantity/child-quality trade-off. Our empirical strategy exploits exogenous variation in family size due to twin births and preferences for a mixed sibling-sex composition, as well as ethnic differences in the effects of these variables. We use these sources of variation to look at the causal effect of family size on completed educational attainment, fertility, and earnings. For the purposes of this analysis, we constructed a unique matched data set linking Israeli Census data with information on the demographic structure of families drawn from a population registry. Consistent with other recent findings in this area, our results show no evidence of a quantity-quality trade-off, though there is some evidence that first-born girls from large 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 connection between family size and economic circumstances is one of the most enduring questions in social science. The earliest theoretical discussion of the role of family size in the determination of living standards was probably Malthus, who famously argued that family size responds to income shocks in a manner that keeps living standards at a constant subsistence level. Beginning with Becker and Lewis (1973) and Becker and Tomes (1976), economists have replaced the Malthusian model with a theoretical framework that sees both the number of children and parental investment per child as choice variables that respond to economic forces. Part of this agenda is an attempt to reconcile the apparent paradox of declining family size in the face of economic growth with the superficially plausible presumption that children are indeed a normal good. The notion of a quantity-quality trade-off appears to provide this reconciliation: as parents get richer they demand children of higher "quality," (i.e., children who are more productive), without necessarily demanding more of them. In fact, because increases in quality can be interpreted as making children more expensive, the quantity-quality trade-off explains why families might get shrink as parents get richer.

On the policy side, the notion that smaller families and slower population growth are essential for development motivates many governments and international agencies to promote, or even, in the case of China, to require smaller families.[1] While this policy position often seems based on naive empiricism, both the Malthusian and the Becker and Lewis (1973) models provide some theoretical support for a pessimistic view. A negative causal relation between child quantity and parental investment also comes out of a number of sophisticated theoretical recent analyses of the role of the demographic transition in economic development (e.g., Galor and Weil, 2000; Hazan and Berdugo, 2002, and Moav, 2005). On the other hand, the newer

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

theories focus primarily on the quantity and quality implications of human capital accumulation. In these models, the effects of population-control efforts and similar policy interventions are less clear cut, a point we return to after presenting results.

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

This paper provides new evidence on the quantity-quality trade-off using exogenous variation in family size. Our approach adapts and extends the Angrist-Evans (AE-98) IV strategy to the estimation of effects of family size on various measures of "quality" that might be affected by the home environment. In particular, we look at the effect of third and higher births on first- and second-born children's completed schooling, adult earnings, and on marital status and fertility. Two of the instruments used here, as in AE-98, are dummies for multiple births at second birth and a dummy for same-sex sibling pairs in families with two or

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

more children. For the purposes of this analysis, we use a specially-constructed link between Israeli Census micro data and information on family background derived from the population registry.

We also extend the AE-98 identification strategy in a number of ways. First, in addition to looking at families with two or more children, we exploit multiple third births and the effects of sibling-sex composition in families with three or more children. We also introduce a new source of exogenous variation in family size based on sharp differences in the effects of multiple births and sex-composition across ethnic groups in the Israeli population. For example, the effect of having a multiple birth has a much larger effect on Jews of European or North American origin than on Jews of Asian or North African origin, since the latter chose to have large families even in the absence of a multiple birth. On the other hand, an all-female sibling sex composition leads to sharp rise in the number of children born to the Asia-Africa group, with relatively little effect on the fertility of ethnic Europeans and Israeli natives. Thus, our sample and identification strategy allow us to juxtapose the results from a number of different groups, using fertility shocks of different sorts and sizes, and over differing ranges of variation. Remarkably, all of this evidence points in the same direction: an exogenous increase in family size at second and higher births appears to have little effect on first and second born children, with the possible exception of an increase in the likelihood that female children marry.

Our paper comes on the heels of a substantial and growing literature attempting to link multiple births with measures of child quality. Rosenzweig and Wolpin (1980) appear to have been the first to use multiple births to estimate a child-quantity/child-quality trade-off. More recent estimates using multiple births include Duflo (1998), who looks at effects on child mortality in Indonesia; Black, Devereux, and Salvanes (2004), who estimate effects on education in Norway; and Caceres (2004), who look at effects on private school enrollment in US Census data. Caceres (2004) also estimates effects on dropout status, teen pregnancy, and parents' marital status.

An important difference between our study and these earlier papers is that we have a wider range of outcomes than has been previously available for a research design of this type. Our outcomes come from a unique data set we constructed for the purposes of this project linking the 1983 and 1995 Israeli Census micro samples, which provide information on education, work, earnings, marriage, and childbearing, with detailed sibling information from the population registry. A second key difference is that the ethnic diversity of the Israeli population allows us to compare effects across families of different sizes and from different cultural traditions. Of particular interest here are results for the Asia-Africa subsample, that is, Sephardic Jews of North African and Middle Eastern origin. Those in this group have demographic and social characteristics much closer to developing country populations than do native-born Israelis or Israelis of European and North American stock. Finally, ours appears to be the first study to use sibling-sex composition to estimate the quality-quantity trade-off for a wide range of outcomes, or with a strong and well-documented first-stage.[3]

The paper is organized as follows. The next section describes the census-registry link and the construction of our more-than-two (2+) and more-than-three (3+) analysis samples. Section III discusses first-stage estimates and Section IV presents the main OLS and 2SLS results. We discuss the relation between our findings and earlier work in Section V, focusing on the question of whether the case for an empirical quantity-quality trade-off should be evaluated in terms of parental inputs or child outcomes. Finally, Section VI concludes and suggests directions for further work.

II. Data and Samples

The main sources of data used here are the 20% public-use micro-data samples from the 1995 and 1983 Israeli censuses, linked with information on parents from the population registry. The Israeli census micro files are 1-in-5 random samples that include information collected on a fairly detailed long-form

[3]Black, Devereux, and Salvanes (2004) briefly mention a failed effort in this direction. Conley and Glauber (2004) report estimates using sex-composition IVs, looking at grade retention and private school attendance. Lee (2004) uses preferences for male children to construct an instrument for family size in Korea, where preferences for boys (as opposed to a mixed sibling-sex composition) are strong. Another related study is Qian (2004), who exploits urban-rural differences in China's one-child policy as a source of exogenous variation in family size.

questionnaire similar to the one used to create the PUMS files for US censuses.[4] The set of Jewish long-form respondents aged 18-60 provides our initial study sample. In the discussion that follows, we refer to these individuals as "subjects," to distinguish them from their parents and siblings, for whom we also collected data. Information on parents and siblings was obtained from the Israeli population registry maintained by the ministry of the interior. Conditional on an ongoing confidentiality review, the registry is available for use on a per-project basis inside the Central Bureau of Statistics (CBS) in a restricted-access Research Data Center. The link from census to registry is necessary for our purposes because in a sample of adult respondents, most of who no longer live with their parents and siblings, the census provides no information about sibship size, multiple births, or sibling sex composition.[5]

The Israeli population registry, our source of information on families of origin, contains updated administrative records on 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 our analysis file proceeded by first using our subject's 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

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[4]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.

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

not these children appear in the census. In this manner we were able to observe the sex and birth dates of most adult census respondents' siblings.

Match Rates and Sample Selection Issues

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

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

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

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

The final sample restriction retains only first and second-born subjects since these are the people exposed to the natural experiments exploited by the twins and sex-composition research design. 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.

Description of Analysis Samples

We work with two analysis samples, both described in Table 2. The first sample consists of first-born subjects in families with two or more *births* (the 2+ sample, N=89,445). The second sample consists of first- and second-born subjects in families with three or more *births* (the 3+ sample, N=65,671 first-born and 53,070 second-born). These samples are defined conditional on the number of births instead of the number of children so that multiple-birth families can be included in the analysis samples without affecting the sample selection criteria. Twin subjects were dropped from both samples, however.[6]

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.[7]

The Jewish Israeli population is often grouped by ethnicity, with Jews of African and Asian origin

[6]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.

[7]The 2+ sample naturally includes the 3+ sample. 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.

(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. Larger families in the AA population are also reflected in the statistics on number of children. Average family size ranges from 3.6 in the 2+ sample to 4.2 in the 3+ sample (4.3 for second-borns). In the AA subsample, however, the corresponding family sizes are about 4.3 and 4.7.

Table 2 also reports statistics on the variables used to construct instrumental variables. The twin rate was 9/10 of one percent at second birth in the 2+ sample and 1 percent at third birth in the 3+ sample, with similar rates in the AA and full samples.[8] As expected, about 51 percent of births are male, regardless of birth order. Consequently, about half of the 2+ sample was born into a same-sex sibling pair and about one-quarter of the 3+ sample was part of a same-sex threesome.

The outcome variables described in Table 2 measure subjects' educational attainment, labor market status and earnings, marital status and fertility, and the characteristics of subjects' spouses. Most Israelis are high school graduates, while 20 percent are college graduates. In the AA subsample, however, proportion of college graduates is much lower. Most of our subjects were working at the time they were interviewed and earned about 3000 shekels (about 1000 dollars) per month on average (including zeros). About 40 percent of subjects were married, though marriage rates are higher in the AA subsample. Table 2 also reports select descriptive statistics on spouses' characteristics in the sample of married subjects.

III. First-stage Estimates

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

[8]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.

*Twins First-Stage*s

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

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

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

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

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

The twins-2 main effect, α_0, captures the effect of a multiple birth in the non-AA population, while the interaction term, α_1, measures the AA/non-AA difference.[9] The estimates in column 2 show that non-AA family size goes up by about .63 in response to a multiple birth (similar to the AE-98 first stage), while AA family size increases by only .63-.45=.18. Both α_0 and α_1 are very precisely estimated.

The remaining columns of Table 3 report the first-stage effect of a multiple third birth in the 3+ sample. Twins-3 effects were estimated in the 3+ sample by replacing t_{2i} with t_{3i}, a dummy for multiple third births, in equations (1) and (2). These results are reported in columns 3-4 for first-borns and columns 5-6 for

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

the pooled sample of first- and second-borns. The first stage effect of a multiple birth is bigger in the 3+ sample than in the 2+ sample because the desire to have additional children diminishes as family size increases. For the same reason, the effect of t_{3i} differs less by ethnicity in the 3+ sample than in the 2+ sample, though, as the estimates in column 6 show, there is still a significant difference by ethnicity when first and second born subjects are pooled.

Sibling-Sex Composition First-stages – 2+ Sample

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

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

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

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

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

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.[10]

The same-sex first-stage effects estimated using equation (3a) in the 2+ sample, reported in column 1 of Table 4a, are on the order of .08 children. This increase is due to an increase of a little over .03 in the likelihood of having more than two children, as well as smaller increases in the likelihood of having more than

[10]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.

3 and more than 4 children, as can be seen in columns 4, 7, and 8, which report effects on dummies, $d_{ki}=1[c_i>k]$, for k=2, 3, and 4. Same-sex sibship at first and second birth has an impact on the likelihood of having 4 or more children because, with probability one-half at each birth, families with a same-sex sibship outcome in earlier births find themselves with an all-boy or all-girl sibship at the next birth as well. Thus, $s_{12i}=1$ shifts the distribution of fertility to the right in addition to increasing the likelihood that families have more than two children.[11]

Sex-composition effects estimated using equation (3b), allowing for separate two-boys and two-girls coefficients, are reported in columns 2 and 3 of Table 4a. In addition to allowing different effects for boys and girls, the results reported in column 3 are from models that incorporate AA interaction terms, as with the twins estimates discussed above. The effect of two girls is .11 (s.e.=.015) while the effect of two boys is only .039 (s.e.=.015). The results allowing different effects by ethnicity generate an effect of two girls equal to .086 (s.e.=.017) in the non-AA population, with the AA effect of two girls is larger by .055 (s.e.=.032). In contrast, the two boys effect is only .056 (s.e.=.017) in the non-AA population, with the AA two-boys effect is *smaller* by .042 (s.e.=.031). As a result, the AA population appears to increase childbearing in response to the birth of two girls but not in response to the birth of two boys.

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

[11]The first-stage effect of an instrument on c_i in the 2+ sample can be shown to be the sum of the first stage effects on d_{ki}; k=2, . . . (Angrist and Imbens, 1995). In contrast with the results reported here, Angrist and Evans (1998) found similar same-sex effects on completed fertility and on the probability of having more than two children, i.e. on d_{2i},, and therefore chose the latter as the endogenous variable of interest. This difference is due to the fact that even in the face of a same-sex threesome, few relatively few American couples are motivated to try again. Because of the substantially larger first-stage for c_i in the Israeli context, we use completed fertility as the endogenous variable.

children.[12] Effects in the non-AA population drop off more sharply as the number of children increases, and are similar for two boys and two girls. If anything, the non-AA population seems to increase childbearing more sharply in response to two boys than to two girls.

Sibling-Sex Composition First Stages – 3+ Sample

The sex-composition first-stage in the 3+ sample captures the effect of an all-boy or all-girl triple, controlling for the sex-composition of earlier births. Because a same-sex triple occurs only in families with same-sex pairs at first and second births, the model conditions on b_{12i} and g_{12i}, as well as a subject-sex main effect. An additional variable included in these models is a dummy for the sex of the third child, an effect which is defined conditional on a *mixed-sex* sibling pair at first and second birth (because for families with b_{12i}=1, the boy-third effect is the same as having an all-male triple, while for families with g_{12i}=1, the boy-third effect is the same as having an all-female triple). The resulting model can be written as follows (we spell out only the model that allows for separate all-male and all-female effects):

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

where b_{123i} and g_{123i} are indicators for all-male and all-female triples and b_i is subject sex (i.e., b_{1i} for first-borns and b_{2i} for second-borns).[13]

The first-stage effects using equation (4) in the 3+ sample and the corresponding effects after incorporating the AA interactions are reported in Table 4b. Estimates for the first-borns sample are reported in

[12]This increase, while counterintuitive at first blush, can be attributed to the fact that in the AA population that ends up with an all-girl triple, the likelihood of having more children is very large. Note that $P[c_i>k|\ c_i\geq k-1]=P[c_i>k|\ c_i\geq k]P[c_i\geq k|\ c_i\geq k-1]$. The first stage effect can be written using the operator Δ_s, where this denotes the same-sex contrast in the relevant conditional probability of increasing fertility. We have $\Delta_s P[c_i>k|\ c_i\geq k-1] = \Delta_s\{P[c_i>k|\ c_i\geq k]P[c_i\geq k|\ c_i\geq k-1]\}=\Delta_s P[c_i>k|\ c_i\geq k]P[c_i\geq k|\ c_i\geq k-1, s_i=1]+\Delta_s P[c_i\geq k|\ c_i\geq k-1]P[c_i>k|\ c_i\geq k, s_i=0]$. For AA, the incremental effects $\Delta_s P[c_i>k|\ c_i\geq k]$ and $\Delta_s P[c_i\geq k|\ c_i\geq k-1]$ are both substantial at k=3,4.

[13]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. There are at most 7 linearly independent terms in any saturated model that conditions on the sex-composition of the first three children, but we have 6. A saturated model is obtained by replacing the single term $(1-s_{12i})b_{3i}$ with two terms, with $b_{1i}(1-b_{2i})b_{3i}$ and $b_{2i}(1-b_{1i})b_{3i}$. In practice, this matters little.

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

Other columns in the table show the effect of sibling sex composition on the probability of having more than 3 and more than 4 children in the sample of first-borns and in the pooled sample of first- and second-borns. Since results in both samples are similar, we discuss effects on fertility increments for first-borns only. These results are summarized in Figure 2, which reports first-stage estimates of the effects of three-boys/girls (b_{123i} / g_{123i}) on d_{ki} for k=3 to 10, along with the associated confidence bands. The estimates are from a model that conditions on b_{12i}, g_{12i} and b_i, and are estimated separately for the AA and non-AA population. In the AA population, b_{123i} increases the likelihood of having 4 or more children, with a small and marginally significant effect on the likelihood of having 5 or more children. The effect of three boys is similar in the AA and non-AA population. In contrast, the effect of three girls differs considerably by ethnicity. 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}.

The Boy-3rd Instrument

The fourth and fifth rows of Table 4b show the effect of having a boy at third birth (b_{3i}) in families with a mixed-sex sibship at first and second birth. A boy at third birth reduces childbearing in families that already have one boy by .080 (s.e.=.018). Results allowing different coefficients by ethnicity generate an effect of -.053 (s.e.=.023) in the non-AA population, while the AA interaction term adds a further .054

(s.e.=.035) to this reduction, though the difference between AA and non-AA is not significant. The Boy-3rd effect potentially provides an additional source of exogenous variation in fertility, beyond pure sex-composition effects. We therefore add this to the instrument list for some of the 2SLS specifications discussed below. Figure 3 summarizes the effects of b_{3i} on fertility increments for families with a mixed-sex sibship at first and second birth, separately by ethnicity. In the AA population, b_{3i} reduces the likelihood of having more than 4 children as well as the likelihood of higher order births, up to k=7, beyond which the effect is no longer significant. In the non-AA population, b_{3i} reduces the likelihood of having 4 or more children, with no significant effect at higher order births.

Pooled First-Stages Using the Full Set of Instruments

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

IV. OLS and 2SLS Estimates

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

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

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

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

The 2+ Sample

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

In contrast with the pattern of adverse effects generated by the OLS estimates, 2SLS estimates show zero or even positive effects. These results appear in columns 3-7 of Table 5, which report 2SLS estimates for different sets of instruments. For example, the estimated effect on schooling using twins instruments with AA interaction terms, reported in column 4, is .101 (s.e.= .13). The corresponding estimates using sex-composition instruments with AA interaction terms, reported in column 6 is .222 (s.e.=.176). Combining both twins and sex-composition instruments generates an estimate of .15 (s.e.=.104), reported in column 7. Interestingly, the combination of instruments generates a substantial gain in precision relative to the use of each instrument set separately. So much so that the estimated schooling effect in the first row of column 7 is significantly different from the corresponding OLS estimate of −.145. Likewise, there is a precisely estimated zero effect for matriculation status, a key educational milestone in the Israeli milieu.[14]

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 question of how many IV estimates are indeed different from the OLS benchmark. As it turns out, the estimates in column 7, constructed by pooling twins and sex-composition instruments with AA interaction terms, meet this standard of precision remarkably often. In particular, 12 of the 18 parameters presented in this column are estimated precisely enough that the associated 95% confidence interval exclude the corresponding OLS estimates. Moreover, estimates of effects on the level and quality of schooling are very close to zero. The least precise estimates are those for the subject's own labor market outcomes and his or her spouse's outcomes. This echoes similar results reported by Black , Devereux, and Salvanes (2005), who also found insignificant but imprecise effects of family size on earnings.

A second set of noteworthy findings are those for marriage and fertility. The IV estimates of effects on marital status suggest that subjects from larger families are more likely to be married and got married sooner. With both twins and sex-composition instruments, the estimates for marital status on census day are significantly different from zero and substantially larger than the corresponding OLS estimates. These marriage effects are paralleled by (and are perhaps the cause of) an increase in fertility: the combination-IV estimate of the effect on the probability of having any children is 0.078, four times larger then the

[14] 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.

corresponding OLS estimate, 0.019. Estimates of effects on a dummy for having two or more children show a similar pattern. In addition to the likelihood that increased marriage rates increase fertility, these fertility effects may reflect an intergenerational causal link in preferences over family size, a possibility suggested by Fernandez and Fogli (2005).

The last set of results in the table is for spousal characteristics. These results are for the sample of married individuals only and are therefore potentially affected by selection bias. At the same time, these results are of interest as an alternative measure of child quality, beyond human capital and labor market variables. One possible consequence of larger sibship sized is reduced parental investment in attributes that are rewarded in the marriage market. Consistent with this notion, and with the other OLS estimates in the table, the OLS estimates in the lower panel of Table 5 suggest that first-borns from larger families are married to spouses with lower level schooling, lower labor force participation and lower earnings. Again, however, the IV estimates in columns 3-6 show no significant relation and many effects of the opposite sign.

The 3+ Sample

Estimates in the 3+ sample are broadly similar to those for the 2+ sample, though there are some noteworthy differences. The 3+ results for first-borns are reported in Table 6a, which has a structure similar to Table 5. The OLS results in Tables 5 and 6 are virtually identical, though this is not surprising given the fact that the first-borns in the 3+ sample are a sub-sample (73 percent) of the first-borns in the 2+ sample. The 2SLS estimates in the 3+ sample exploit different sources of variation than in the 2+ sample (twins at 3rd birth instead of second; same-sex triples instead of pairs), so here we might expect some differences. The first key finding, however, is preserved: 2SLS estimates using both twins and sibling-sex composition generate no evidence of an effect on human capital or labor market variables.

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

One difference between the results for first-borns in the 2+ sample and the results for first-borns in the 3+ sample is that the marriage effects are smaller and less consistently significant. In particular, while the twins estimates are significantly different from zero for marital status on census day, the twins instruments fail to generate a significant estimate on marital status at age 21. Using sex-composition instruments, this pattern is reversed, with a (marginally) significant estimate on marriage at age 21 results, while these same instruments generate a zero effect on marriage on census day. On the other hand, while the estimates in the 3+ sample are no longer consistently significant, they still point in the direction of increased marriage.

A second difference between the results in Tables 5 and 6a is that some of the estimated effects on subjects' fertility in the 3+ sample are negative and significant (e.g., −.125 with a standard error of .058 in column 7). This negative effect comes from a negative effect on the probability of having 3 or more children; effects on the probability of having any children or having more than two children are small and not significantly different from zero. Below we explore the marriage and fertility results further by looking at separate results for samples of men and women.

Adding the second-borns to the first-borns in the 3+ sample roughly doubles the sample size. The OLS results are essentially unchanged by this addition, as can be seen in column 1 of Table 6b, which reports results using both first and second-borns. The IV estimates in Tables 6a and 6b are also broadly similar. One important, though unsurprising, difference is that the estimates in Table 6b are more precise. As a result, two thirds of the estimates in column 7 generate 95 percent confidence intervals that exclude the corresponding OLS estimates. A further change from Table 6a is that the total fertility estimate is smaller and no longer significantly different from zero, while the marriage estimates using sex-composition instruments with AA interaction terms are now significant or close to it for both marital status outcomes (column 5). The combined-instruments estimate for early marriage are also significant (e.g., .035 with s.e.=.017 in column 7), though considerably smaller than the corresponding estimate in Table 5. On balance therefore, the evidence for at least a modest marital status effect is reasonably consistent across Tables 5, 6a, and 6b.

Analyses by ethnicity and gender

Large numbers of Sephardic Jews came to Israel from the Arab countries of Asia and North Africa in the 1950s. Initially, the Total Fertility Rate (TFR) of the AA population in Israel was 5-6, similar to that in many developing countries, while the TFR for Israeli Jews of European origin was just above 3. By the late 1990s, however, the TFR of the non-religious AA population had fallen to 2.2, only slightly higher than that of other non-religious Jewish groups (Friedlander, 2002). The sharp decline in TFR among the AA population occurred without government encouragement, and in the face of pronatalist tax and housing policies (Okun, 1997). In addition to having higher fertility, at least until recently, the AA group is less educated and has lower earnings 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.

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

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

Also of interest are separate results for men and women, especially in view of the apparent effects on marital status reported in Tables 5 and 6. As with the results by ethnicity, the separate results by gender were constructed using the full set of instruments, including Boy-3rd in the 3+ sample. These results are reported in Table 8. The OLS estimates in Table 8 are almost identical for men and women. As with the estimates by ethnicity, the 2SLS estimates by gender show no evidence of an adverse effect on schooling or labor market variables for either group. In this case, some of the estimates on schooling variables for men are positive and significant. An especially important result in this context is that the increase in marriage rates is much more pronounced in the female sample.

V. Comparison with Previous Findings and Theoretical Implications

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

Two other recent studies have used IV methods with US census data to look at the effects of sibship size on schooling and private school enrollment among youth still co-resident with parents. Using twins instruments, Caceres (2004) finds a negative effect of family size on private school enrollment in some specifications and samples (as well as effects on room-sharing and parental divorce) but no effect on measures of human capital such as schooling or dropout status. Conley and Glauber (2004) use sibling-sex composition instruments to estimate effects on grade-retention and private school enrollment. Their results point to negative effects, though elements of their research design make this finding difficult to interpret.[15] Finally, Qian (2004) uses variation in China's one-child policy to estimate the effects of family size on school enrollment in China. Perhaps surprisingly, her estimates suggest that relaxation of the one-child policy increased the enrollment rates of first-born children.

Consistent with this related work, and against a background of our own OLS estimates showing strong adverse effects, our IV strategies generate little evidence for a quantity-quality trade-off in the sense of a causal link between sibship size and outcome variables describing the human capital, earnings, or social status of first- and second-born children. This suggests the OLS effects reflect substantial omitted variables bias.[16] Our results reinforce and broaden the earlier findings in this area by simultaneously drawing on a number of sources of variation and including evidence from various fertility increments and from different family types. On the other hand, we do find some evidence of a possible "crowding effect" in the form of accelerated

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

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

marriage rates for girls from large families.

The first question this set of findings raises is what might account for the absence of a causal link between sibship size and child welfare, at least as measured here. One possibility is that 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. 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). Evidence on this point is difficult to obtain since consumption data rarely come in samples large enough or with the kind of retrospective family information needed to replicate our natural-experiments research design. Weighing against the "less leisure, more work" theory, however, are the AE-98 results showing no effect of additional childbearing on husbands' labor supply and a sharp negative effect of childbearing on wives labor supply and earnings.

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

A third sort of explanation for the absence of a causal link between sibship size and the outcomes studied here might be called "marginally ineffective or irrelevant inputs." Using research designs similar to ours, Caceres (2004) and Glauber and Conley (2005) both find some evidence for a decreased likelihood of private school enrollment. Caceres also finds that children in larger families are more likely to share a room.

This can be seen as paralleling the results reported here suggesting that girls from larger families marry sooner, since the latter may reflect a desire to leave a relatively crowded household. The private school, marriage, and room-sharing effects reflect changes in parental inputs. In practice, these inputs may matter little for children's life chances. For example, parents may incorrectly believe that a private school education is better (perhaps due to a misleading peer correlation) and children almost certainly prefer more space to less. But in the long run, these factors may be more consumption than investment, contributing little to human capital and life chances. Given the findings for these few inputs and the absence of significant or credible effects on longer-term outcomes, the notion that parents adjust inputs with low investment value appears to get some support.

Theoretical interpretation

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

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

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

VI. Summary and Directions for Further Work

We use a unique data set combining census and population registry data to study the quantity-quality trade-off, where this is defined as a causal link running from sibship size in the family of origin to human capital and economic and social status later in life. Our research design exploits variation in fertility due to multiple births and preferences for a mixed sibling-sex composition, along with ethnicity interactions and preferences for a male child at third birth. The evidence is remarkably consistent across research designs and samples: while all instruments exhibit a strong first-stage relation, and OLS estimates are strongly negative, IV estimation generates no evidence for negative consequences of increased sibship size on outcomes. We do find, however, some evidence that girls from larger families marry sooner.

The results reported here are consistent with the findings from a number of recent studies using data from America, Norway, and China to explore the same sort of questions. What might explain the failure of an empirical quantity-quality trade-off to appear? One possibility is that the cost of children is borne by reducing parental consumption while holding quality constant. Another is that mothers' withdrawal from the labor force in response to childbirth is ultimately a net plus for older siblings. In future work, we plan to investigate these possibilities with the aid of information from other samples and for additional outcome variables. On the data collection side, we hope to be able to use a number of strategies to improve match rates between the census and population registries and within families.

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Table 1: Match Rates and Sample Selection

	Israeli-born Father		Foreign-born Father			
			Foreign-born subejct		Israeli-born Subject	
	Subject born<1955	Subject born ≥ 1955	Subject born<1955	Subject born ≥ 1955	Subject born<1955	Subject born ≥ 1955
	(1)	(2)	(3)	(4)	(5)	(6)
			A. 1995 census			
All subjects	9,453	56,534	118,633	72,340	58,767	161,331
Matched to registry (N,%)	9,057 95.8%	54,073 95.6%	115,123 97.0%	68,788 95.0%	57,098 97.2%	156,096 96.8%
Matched mother + siblings (N,%)	1,573 16.6%	50,597 89.5%	7,600 6.4%	32,472 44.9%	11,351 19.3%	139,783 86.6%
Selected sample						
Mothers born ≥ 1930 whose age at 1st birth ε [15,45]	494	48,683	1,166	26,217	2,556	119,928
of which: Israeli born mothers or immigrants who arrived since 1948 and before the age of 45	419	47,022	1,127	22,704	2,211	115,783
of which: first and second borns from 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 from families with 2 or more births	276	5,834	906	5,889	1,869	21,744
Estimated fertility coverage: 79%						

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

Table 2: Analysis Samples

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

Table 2 (cont.)

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

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

Table 3: Twins First Stage

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

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

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

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

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

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

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

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

Table 5: Results for First Borns in 2+ Sample

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

Notes: The table reports means of the dependent variables (column 1) and coefficients on number of children for OLS models (column 2) and 2SLS models using different sets of instruments (columns 3-7). 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. Regression estimates are from models that include the control variables specified in Table 3. Robust standard errors are reported in parenthesis.

Table 6a: Results for First Borns in 3+ Sample

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

Notes: The table reports means of the dependent variables (column 1) and coefficients on number of children for OLS models (column 2) and 2SLS models using different sets of instruments (columns 3-7). Instruments with an 'AA' suffix are interaction terms with an AA dummy. The sample includes first borns from families with 3 or more births as decribed in Table 1. Regression estimates are from models that include the control variables specified in Table 3. Robust standard errors are reported in parenthesis.

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

Outcome	OLS (1)	2SLS: twins (2)	2SLS: twins twinsAA (3)	2SLS: girl123 boy123 (4)	2SLS: girl123, boy123 girl123AA, boy123AA (5)	2SLS: all (6)	2SLS: all +boy3, boy3AA (7)
Schooling							
Highest grade completed	-0.143	0.167	0.187	-0.108	-0.054	0.076	0.080
	(0.005)	(0.119)	(0.111)	(0.134)	(0.120)	(0.081)	(0.077)
Years of schooling ≥ 12	-0.031	0.025	0.025	0.001	-0.007	0.016	0.012
	(0.001)	(0.019)	(0.018)	(0.023)	(0.022)	(0.014)	(0.013)
Matriculation certificate	-0.033	0.057	0.065	-0.019	0.008	0.036	0.042
	(0.001)	(0.026)	(0.026)	(0.030)	(0.027)	(0.019)	(0.018)
Some College (age ≥ 24)	-0.021	0.061	0.062	-0.047	-0.022	0.010	0.002
	(0.001)	(0.037)	(0.036)	(0.030)	(0.025)	(0.021)	(0.018)
College graduate (age ≥ 24)	-0.015	0.065	0.068	-0.056	-0.031	0.003	-0.001
	(0.001)	(0.036)	(0.037)	(0.026)	(0.022)	(0.019)	(0.016)
Labor Market Outcomes (age ≥ 22)							
Worked during the year	-0.027	0.029	0.034	0.038	0.035	0.037	0.039
	(0.001)	(0.025)	(0.024)	(0.029)	(0.027)	(0.018)	(0.017)
Weekly labor force participation	-0.023	0.028	0.035	-0.001	-0.006	0.011	0.018
	(0.001)	(0.025)	(0.024)	(0.028)	(0.026)	(0.018)	(0.016)
Hours worked last week	-1.41	2.38	2.53	1.44	1.56	2.29	2.11
	(0.05)	(1.48)	(1.45)	(1.36)	(1.28)	(0.97)	(0.89)
Monthly earnings (in 1995 Shekels)	-185	46.6	63.4	127	115	103	107
	(6.77)	(207)	(206)	(176)	(162)	(131)	(122)
Ln(monthly earnings)	-0.027	0.017	0.011	0.013	0.031	0.038	0.019
	(0.002)	(0.048)	(0.049)	(0.050)	(0.046)	(0.033)	(0.031)
Marriage							
Married on census day	0.020	0.022	0.016	0.019	0.041	0.022	0.015
	(0.001)	(0.018)	(0.018)	(0.024)	(0.022)	(0.014)	(0.013)
Married by age 21 (age ≥ 21)	0.024	0.029	0.027	0.044	0.050	0.039	0.035
	(0.001)	(0.021)	(0.023)	(0.031)	(0.030)	(0.019)	(0.017)
Fertility (women only)							
Number of own children	0.118	-0.069	-0.077	-0.079	-0.043	-0.058	-0.076
	(0.004)	(0.066)	(0.061)	(0.069)	(0.064)	(0.045)	(0.043)
Any children	0.023	0.020	0.014	-0.003	0.012	0.013	0.010
	(0.001)	(0.029)	(0.028)	(0.025)	(0.022)	(0.017)	(0.016)
2 or more children	0.025	-0.002	-0.005	-0.007	0.007	0.002	-0.005
	(0.001)	(0.026)	(0.024)	(0.023)	(0.021)	(0.016)	(0.015)
3 or more children	0.026	-0.050	-0.047	-0.042	-0.037	-0.041	-0.041
	(0.001)	(0.020)	(0.019)	(0.021)	(0.020)	(0.014)	(0.013)
Spouse's Outcomes (for married)							
Years of schooling	-0.166	0.539	0.552	-0.325	-0.233	0.106	0.095
	(0.008)	(0.308)	(0.294)	(0.225)	(0.201)	(0.162)	(0.145)
Weekly labor force participation	-0.023	0.043	0.037	0.026	0.010	0.020	0.012
	(0.001)	(0.034)	(0.033)	(0.024)	(0.020)	(0.017)	(0.016)
Monthly earnings (in 1995 Shekels)	-217	429	426	-889	-442	-151	-250
	(9.0)	(350)	(345)	(414)	(322)	(230)	(202)

Notes: The table reports means of the dependent variables (column 1) and coefficients on number of children for OLS models (column 2) and 2SLS models using different sets of instruments (columns 3-7). Instruments with an 'AA' suffix are interaction terms with an AA dummy. The sample includes first and second borns from families with 3 or more births as decribed in Table 1. Regression estimates are from models that include the control variables specified in Table 3. Standard errors are clustered by mother's ID.

Table 7: All-instrment 2SLS Specification By Ethnicity

	Israel/Europe				Asia/Africa			
	2+		3+		2+		3+	
	1st borns		1st borns	1st+2nd	1st borns		1st borns	1st+2nd
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Schooling								
Highest grade completed	-0.156	0.189	0.240	0.202	-0.134	0.108	0.076	-0.013
	(0.008)	(0.119)	(0.156)	(0.116)	(0.007)	(0.207)	(0.139)	(0.101)
Years of schooling ≥ 12	-0.031	0.013	0.013	0.028	-0.025	-0.015	0.020	-0.009
	(0.001)	(0.019)	(0.024)	(0.019)	(0.001)	(0.036)	(0.025)	(0.018)
Matriculation certificate	-0.042	0.038	0.072	0.069	-0.028	0.022	0.012	0.027
	(0.002)	(0.030)	(0.037)	(0.029)	(0.001)	(0.045)	(0.028)	(0.022)
Some College (age ≥ 24)	-0.039	0.063	0.012	0.010	-0.018	0.054	0.014	-0.002
	(0.002)	(0.038)	(0.052)	(0.038)	(0.001)	(0.047)	(0.027)	(0.020)
College graduate (age ≥ 24)	-0.026	0.060	0.006	0.006	-0.012	0.053	0.004	-0.006
	(0.002)	(0.034)	(0.049)	(0.035)	(0.001)	(0.041)	(0.023)	(0.017)
Labor Market Outcomes (age ≥ 22)								
Worked during the year	-0.038	0.015	0.077	0.080	-0.016	0.061	0.030	0.017
	(0.002)	(0.029)	(0.038)	(0.028)	(0.001)	(0.048)	(0.028)	(0.021)
Weekly labor force participation	-0.035	0.002	0.093	0.091	-0.012	0.043	-0.020	-0.015
	(0.002)	(0.030)	(0.039)	(0.029)	(0.001)	(0.048)	(0.027)	(0.021)
Hours worked last week	-2.08	0.01	4.28	4.27	-0.73	1.02	1.03	0.69
	(0.09)	(1.72)	(2.17)	(1.68)	(0.07)	(2.34)	(1.34)	(1.05)
Monthly earnings (in 1995 Shekels)	-262	369	345	330	-132	87	30	-8
	(13)	(273)	(347)	(257)	(10)	(293)	(208)	(138)
Ln(monthly earnings)	-0.033	0.057	-0.006	-0.001	-0.022	-0.190	0.006	0.014
	(0.005)	(0.073)	(0.075)	(0.056)	(0.003)	(0.101)	(0.044)	(0.036)
Marriage								
Married on census day	0.034	0.088	-0.013	-0.009	0.010	0.046	0.052	0.037
	(0.001)	(0.022)	(0.027)	(0.021)	(0.001)	(0.038)	(0.024)	(0.018)
Married by age 21 (age ≥ 21)	0.040	0.085	0.017	0.012	0.015	0.140	0.046	0.047
	(0.002)	(0.028)	(0.032)	(0.024)	(0.001)	(0.054)	(0.028)	(0.022)
Fertility (women only)								
Number of own children	0.148	0.120	-0.189	-0.130	0.088	0.130	-0.093	-0.054
	(0.008)	(0.074)	(0.088)	(0.065)	(0.006)	(0.116)	(0.075)	(0.056)
Any children	0.030	0.063	-0.019	-0.011	0.015	0.130	0.043	0.020
	(0.002)	(0.032)	(0.038)	(0.029)	(0.002)	(0.116)	(0.027)	(0.021)
2 or more children	0.032	0.064	-0.013	-0.024	0.018	0.115	-0.004	0.001
	(0.002)	(0.028)	(0.035)	(0.025)	(0.002)	(0.047)	(0.026)	(0.019)
3 or more children	0.032	0.008	-0.070	-0.044	0.021	0.062	-0.065	-0.040
	(0.002)	(0.021)	(0.028)	(0.019)	(0.002)	(0.042)	(0.024)	(0.018)
Spouse's Outcomes (for married)								
Years of schooling	-0.213	0.112	0.050	0.129	-0.156	-0.150	-0.002	-0.046
	(0.014)	(0.301)	(0.388)	(0.275)	(0.011)	(0.439)	(0.239)	(0.168)
Weekly labor force participation	-0.039	-0.012	-0.010	-0.019	-0.013	0.038	0.047	0.016
	(0.002)	(0.035)	(0.048)	(0.034)	(0.002)	(0.042)	(0.027)	(0.018)
Monthly earnings (in 1995 Shekels)	-331	-76	10	-187	-163	274	-211	-300
	(17)	(433)	(635)	(407)	(13)	(665)	(329)	(241)

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

Table 8: All-instrument 2SLS Specification By Sex

	Males				Females			
	2+		3+		2+		3+	
	1st borns		1st borns	1st+2nd	1st borns		1st borns	1st+2nd
	OLS	2SLS	2SLS	2SLS	OLS	2SLS	2SLS	2SLS
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Schooling								
Highest grade completed	-0.171	0.217	0.297	0.284	-0.118	0.158	0.061	-0.027
	(0.008)	(0.154)	(0.190)	(0.138)	(0.007)	(0.140)	(0.124)	(0.093)
Years of schooling ≥ 12	-0.036	0.028	0.023	0.027	-0.021	-0.008	0.009	-0.005
	(0.001)	(0.028)	(0.032)	(0.025)	(0.001)	(0.021)	(0.021)	(0.016)
Matriculation certificate	-0.029	0.013	0.055	0.070	-0.037	0.048	0.014	0.029
	(0.001)	(0.038)	(0.039)	(0.031)	(0.001)	(0.032)	(0.027)	(0.021)
Some College (age ≥ 24)	-0.022	0.013	0.025	0.039	-0.023	0.086	0.010	-0.016
	(0.001)	(0.045)	(0.050)	(0.034)	(0.001)	(0.041)	(0.028)	(0.022)
College graduate (age ≥ 24)	-0.014	0.025	0.032	0.030	-0.016	0.075	-0.005	-0.014
	(0.001)	(0.038)	(0.044)	(0.030)	(0.001)	(0.037)	(0.024)	(0.019)
Labor Market Outcomes (age ≥ 22)								
Worked during the year	-0.023	0.013	0.028	0.032	-0.025	0.043	0.054	0.035
	(0.001)	(0.031)	(0.035)	(0.025)	(0.002)	(0.036)	(0.028)	(0.022)
Weekly labor force participation	-0.020	0.031	0.005	0.025	-0.020	-0.003	0.026	0.014
	(0.001)	(0.033)	(0.036)	(0.025)	(0.002)	(0.036)	(0.027)	(0.021)
Hours worked last week	-1.28	0.17	1.51	1.83	-1.09	0.27	2.13	1.56
	(0.08)	(2.60)	(2.21)	(1.77)	(0.07)	(1.60)	(1.29)	(1.01)
Monthly earnings (in 1995 Shekels)	-233	278	439	169	-128	259	53	53
	(13)	(453)	(425)	(299)	(09)	(210)	(160)	(119)
Ln(monthly earnings)	-0.026	0.063	0.051	0.000	-0.026	-0.025	-0.007	0.012
	(0.003)	(0.085)	(0.069)	(0.050)	(0.003)	(0.071)	(0.044)	(0.038)
Marriage								
Married on census day	0.018	0.046	0.036	0.016	0.022	0.091	0.028	0.020
	(0.001)	(0.025)	(0.027)	(0.020)	(0.001)	(0.027)	(0.023)	(0.017)
Married by age 21 (age ≥ 21)	0.015	-0.019	0.005	-0.007	0.028	0.165	0.050	0.054
	(0.001)	(0.021)	(0.023)	(0.018)	(0.002)	(0.039)	(0.029)	(0.023)
Fertility (women only)								
Number of own children	-	-	-	-	0.110	0.115	-0.125	-0.076
					(0.005)	(0.064)	(0.058)	(0.043)
Any children	-	-	-	-	0.019	0.078	0.023	0.010
					(0.001)	(0.026)	(0.021)	(0.016)
2 or more children	-	-	-	-	0.023	0.061	-0.006	-0.005
					(0.001)	(0.024)	(0.021)	(0.015)
3 or more children	-	-	-	-	0.026	0.006	-0.068	-0.041
					(0.001)	(0.019)	(0.019)	(0.013)
Spouse's Outcomes (for married)								
Years of schooling	-0.122	-0.674	0.836	0.570	-0.219	0.273	-0.262	-0.142
	(0.011)	(0.358)	(0.419)	(0.278)	(0.013)	(0.343)	(0.253)	(0.174)
Weekly labor force participation	-0.019	-0.063	0.036	0.019	-0.026	0.059	0.020	0.008
	(0.002)	(0.055)	(0.062)	(0.042)	(0.001)	(0.032)	(0.023)	(0.016)
Monthly earnings (in 1995 Shekels)	-130	-711	393	232	-316	295	-431	-391
	(09)	(236)	(341)	(237)	(18)	(533)	(407)	(272)

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

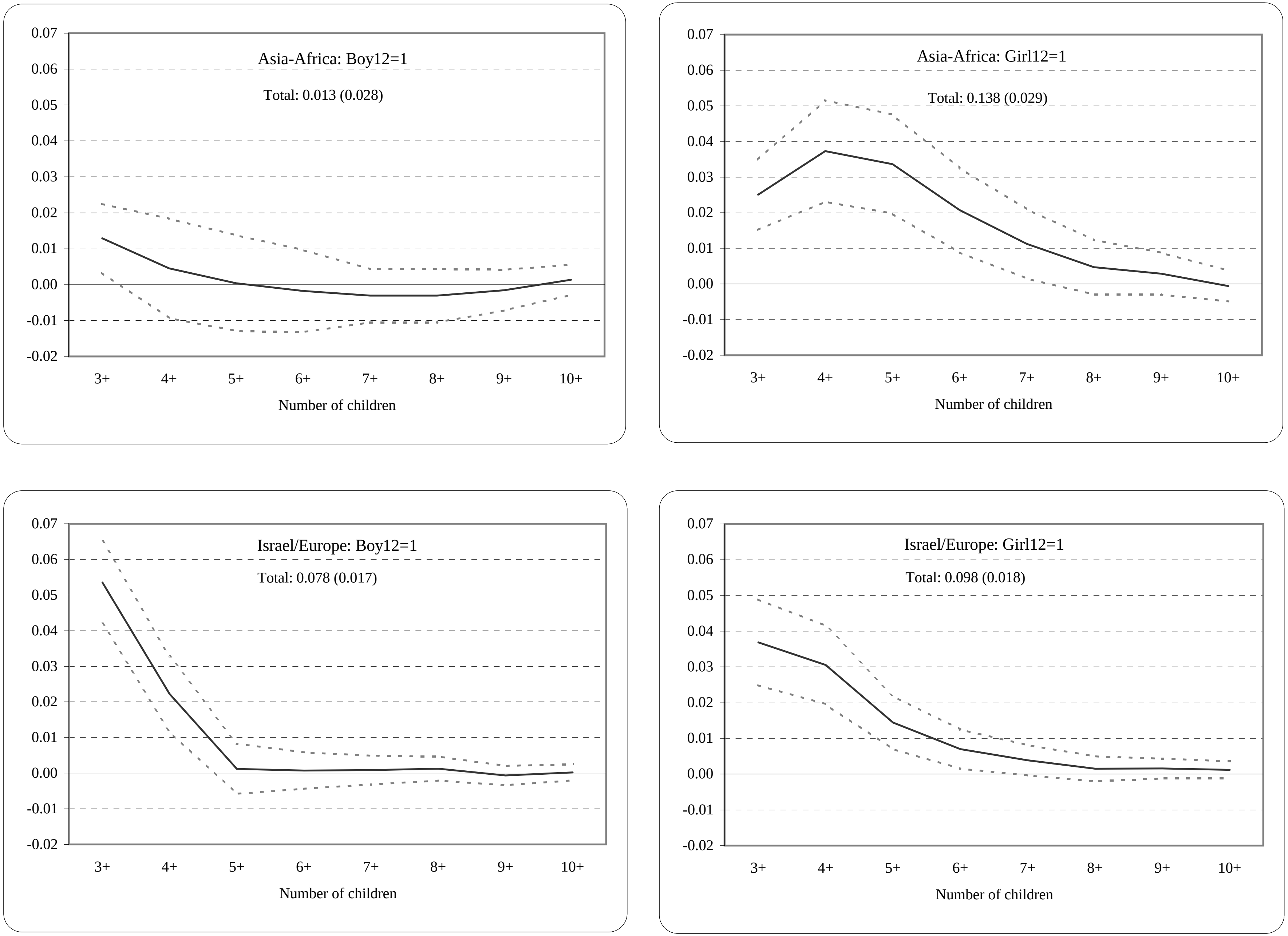

Figure 1: First-borns 2+ sample. First stage effects by ethnicity and type of sex-mix.

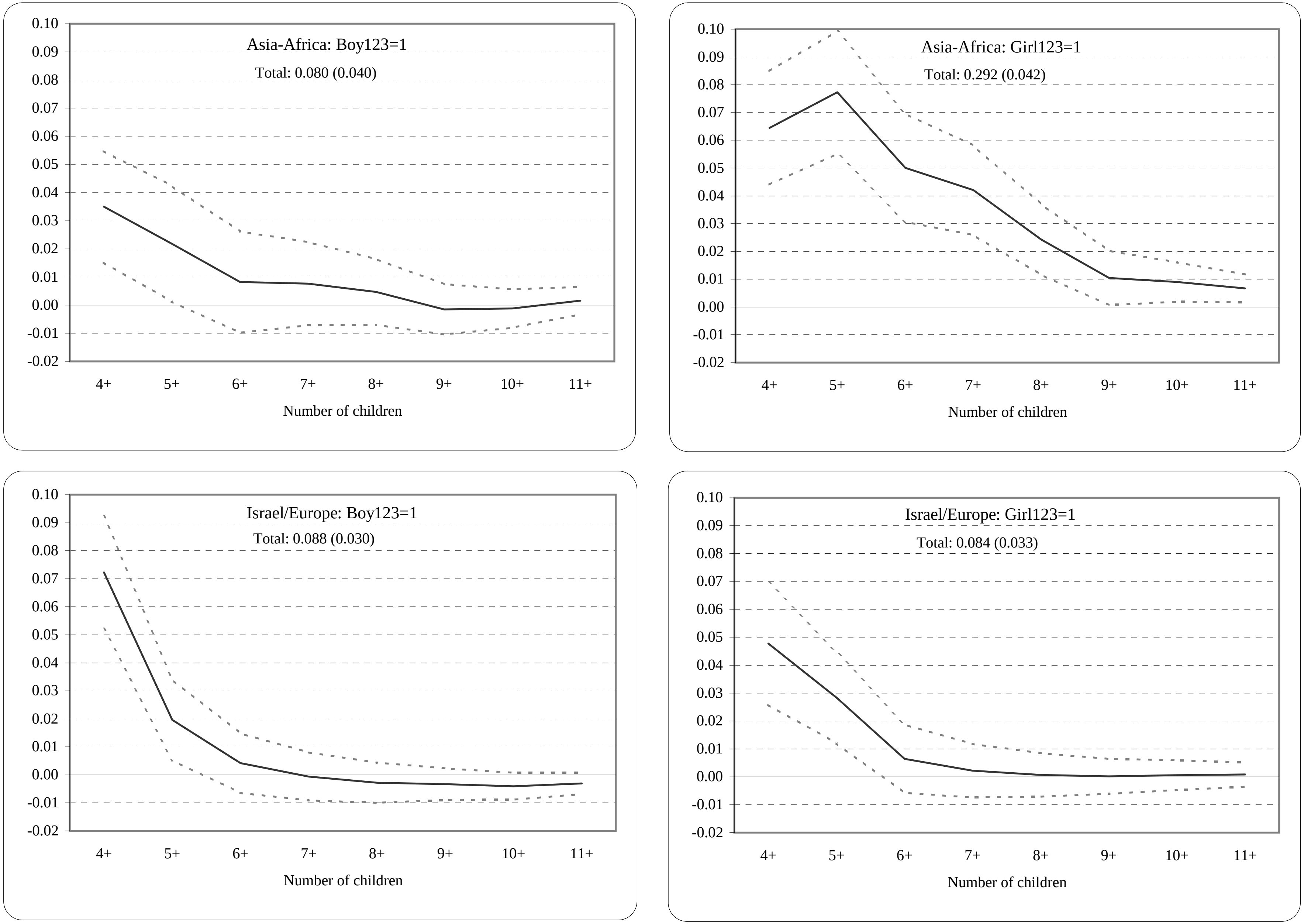

Figure 2: First borns 3+ sample. First stage effects by ethnicity and type of sex-mix (conditional on samesex12=1).

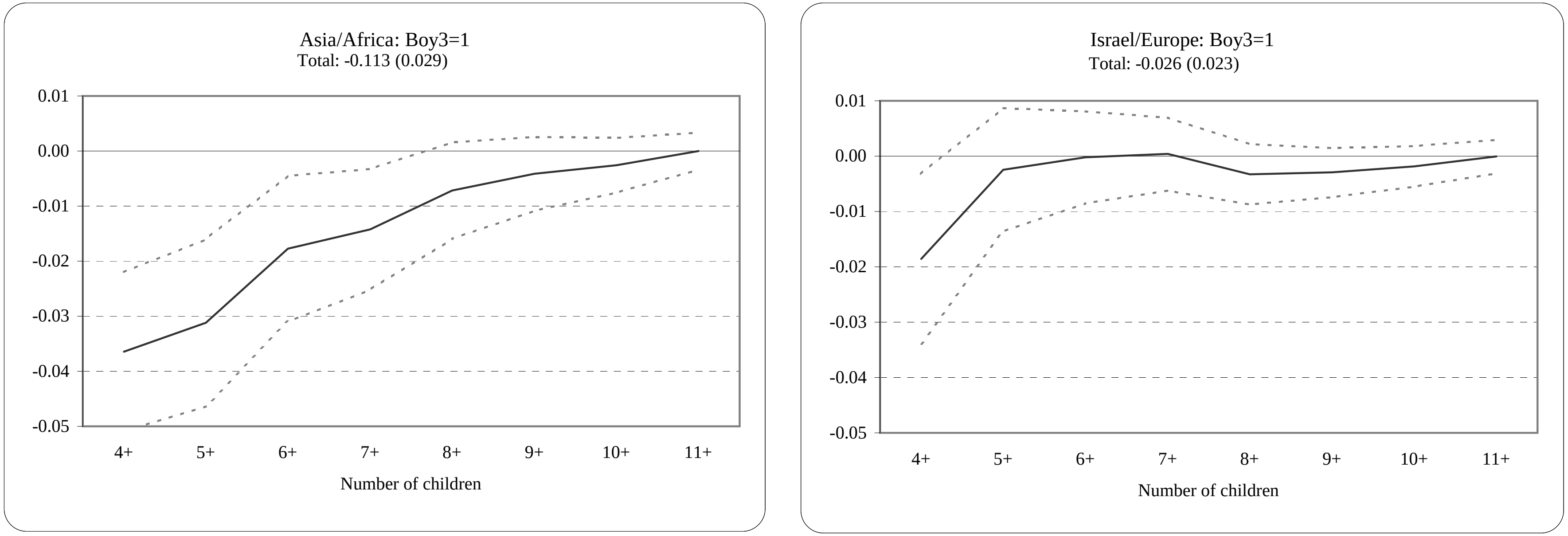

Figure 3: First borns 3+ sample. First stage effects of Boy3 (conditional on samesex12=0)

Table A1: Pooled First Stage

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

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