Hitotsubashi Journal of Economics 53 (2012), pp.1-23. 

# SIBSHIP SIZE, BIRTH ORDER, AND CHILDREN'S EDUCATION IN DEVELOPING COUNTRIES: EVIDENCE FROM BANGLADESH

CHEOLSUNG PARK

*College of Economics and Finance, Hanyang University*
*Seoul 133-791, South Korea*
cheolsung@hanyang.ac.kr

AND

WANKYO CHUNG*

*School of Business, Hallym University*
*Chuncheon 200-702, South Korea*
wankyo@hallym.ac.kr

Received January 2011; Accepted November 2011

## *Abstract*

We examine whether the effect of sibship size on education differs by the individual's birth order in low-income countries, using data from Matlab, Bangladesh. Exploiting exposure to the randomized family planning program in Matlab for identification, we find evidence that sibship size has negative effect on education and positive effect on labor force participation of the first- and the second-born children, but no significant effect on education or labor force participation of the later-born children. Ignoring the difference in the effect of sibship size on education by birth order may confound inferences on quantity-quality tradeoff in low income countries.

*Keywords:* Economic Development, Human Capital, Child Quantity, Child Quality
*JEL codes:* I20, J13, O15

## I. *Introduction*

The seminal works by Becker and Lewis (1973) and Becker and Tomes (1976) postulate that an increase in the quantity of children has negative effects on the quality of children. Since the release of the papers, many empirical studies on the quantity-quality relationship have been conducted. Most previous empirical studies assume that the effect of quantity (sibship size) on quality (education) of children is equal across individuals. In this study, we question the assumption and examine whether an increase in sibship size has different effects on school

* Corresponding author. We thank the editor and the anonymous referee for the helpful comments and suggestions on the earlier versions of this paper.

enrollment and years of schooling across individuals by birth order in developing countries. We posit that the cost of a child's education—including foregone earnings of the child—differs by the child's birth order, and that the effect of sibship size is greater for an earlier-born child whose opportunity cost of education is higher than for a later-born child. We also question whether ignoring those differences by birth order confounds the inference on the quantity-quality relationship.

To estimate the effect of sibship size on education of individuals consistently, we employ two-stage least squares estimation method, using exposure to a randomized family planning and reproductive health program—maternal and child health and family planning (MCH-FP) program of Matlab, Bangladesh—as the instrumental variable (IV) for sibship size. This IV has been previously used by Sinha (2005) and Joshi and Schultz (2007), who assume, unlike us, equality of the sibship size effect regardless of the birth order.

Our estimation results indicate that an increase in sibship size has a negative effect on education and a positive effect on labor force participation of the first- and the second-born children, but little effect on education and work of the later-born children. The finding can be explained by parental choice between education and work for the children. As the family size increases, parents need to augment family income to maintain consumption above the subsistence-level. Since earlier-born children are likely to command higher wages in the rural labor market or be more productive than younger ones, parents choose to discontinue the education of earlier-born children and let them work before later-born children. We also find evidence that the assumption of equal effect of sibship size could confound inferences on the quantity-quality tradeoff. The mixed results of the previous studies can be attributed partly to the questionable assumption of equal effect which is not likely to hold especially in low-income countries.

The balance of the paper is organized as follows. Section II discusses the literature related to this study. Section III presents a simple model of parental choice between school enrollment and work of children in a subsistence level economy. It raises the possibility that the effect of sibship size on education differs by the child's birth order. Section IV describes the data and the sample constructed for this study. Section V presents and discusses the estimation results. Section VI concludes the paper.

## II. *Related Literature*

The instrumental variable (IV) estimation method has been used by many recent empirical studies on the effect of the quantity on the quality of children as a strategy to deal with the potential bias arising from endogeneity of quantity of children. The two most popular sources of variations in fertility used as the IV in the literature are twin births (Angrist et al., 2010; Black et al., 2005; Dayioğlu et al., 2009; Rosenzweig and Wolpin, 1980; Rosenzweig and Zhang, 2009) and sex composition of children (Angrist et al., 2010; Black et al., 2005; Conley and Glauber, 2006; Lee, 2008). The two IVs have been used in studies both on developing and developed countries. Population policies in developing countries have also been exploited by researchers as IVs, for example, China's One Child Policy (Qian, 2008; Rosenzweig and Zhang, 2009) and exposure to MCH-FP program of Bangladesh (Joshi and Schultz, 2007; Sinha, 2005).

While the OLS results almost invariably indicate that the quantity and the quality of children are negatively associated, the findings of the causal effect of the quantity on the quality of children have been mixed. Some find it to be negative (Lee, 2008; Rosenzweig and Wolpin, 1980; Rosenzweig and Zhang, 2009), and others insignificant or even positive (Angrist et al., 2010; Black et al., 2005; Dayioğlu et al., 2009; Qian, 2008; Sinha, 2005)[1].

Specifically for Matlab, Bangladeshi, Sinha (2005) uses a sample from the same data set as ours, the Matlab Health and Socioeconomic Survey (MHSS), to study the effect of MCH-FP program on fertility and child labor supply and to estimate the effect of sibship size on school enrollment of children from 10 to 16 years of age. Sinha (2005) also provides evidence on randomness of MCH-FP program assignment. Sinha (2005) finds that exposure to the program significantly reduced lifetime fertility and a significant increase in boys' labor force participation. However, Sinha (2005) does not find that sibship size has any significant effect on school enrollment of the children, with or without accounting for endogeneity of mother's fertility.

Joshi and Schultz (2007) also study the quantity-quality relationship using the MHSS data in addition to the effect of exposure to MCH-FP program on fertility, women's health, earnings and household assets, use of preventive health measures, and the child's health. Aggregating variables by mother, they find evidence that a fertility decline has a significant positive effect on average education of boys aged 9 to 14, but not on education of girls or the older boys.

Another related study is done by Foster and Roy (1997). They use three different data sets (1974 census, 1982 census, and Knowledge, Attitudes, and Practice Survey of 1990) and find some evidence that treatment by MCH-FP is positively correlated with schooling of children aged 8 to 15, and that the number of preschool-age siblings has negative effects on schooling of school-age children. Unlike this study, Sinha (2005) and Joshi and Schultz (2007), Foster and Roy (1997) do not use exposure to MCH-FP as the IV for the sibship size.

This study differs from the three previous studies on the same region of Bangladesh in two important aspects. One is that we examine the quantity-quality relationship in the much larger group of individuals, without restricting our sample to a certain age group. This study, therefore, provides more comprehensive information on the quality-quantity relationship in Matlab, Bangladesh than the previous studies. The other is that we postulate that the effect of sibship size on education may differ by the child's birth order and estimate the relationship between the magnitude of the effect and birth order. In this aspect this study differs not only from the three studies on Matlab, Bangladesh but from most previous studies on quantity-quality relationship.

Our assumption and approach are different from the studies by Black et al. (2005), Emerson and Souza (2008), and Dayioğlu et al. (2009) that estimate the effect of birth order on schooling of a child separately from the effect of sibship size. They assume that the child's birth order affects the level of schooling of the child, but do not assume that sibship size interacts with birth order[2]. Our study extends their ideas, allowing existence of the interactive effect of sibship size and birth order, and examines whether the effect of sibship size differs in

[1] For the more detailed survey of the studies that use data from developing countries, see Schultz (2007).

[2] Black et al. (2005) estimate the effect of sibship size on education of the children born before the twins in the family. If the second and the third children are twins, they estimate the effect of sibship size on the first-born child only. They do not, however, estimate the effect of sibship size separately by the child's birth order if twinning occurs after the second child.

magnitude by the child's birth order.

## III. *How Can Effect of Sibship Size on Education Differ by Birth Order?*

Think of a rural household in a developing country that consists of one parent (decision maker) and $n$ school-aged children. The number of children is assumed to be exogenously given. We assume that the household operates not far from the subsistence level and so that the parent faces severe liquidity constraints due to lack of collateral and has little room for savings. Thus we assume that the parent maximizes per-period utility function. The parent's objective function is

$$V=U[c-g(n)]+\sum_{i=1}^{n} re_i, \quad (1)$$

where $c$ is the total consumption of the household, $g(n)$ is the subsistence level of consumption for the household, $r$ is the positive utility value the parent gets from a child's enrollment in a school, and $e_i$ is the enrollment status (1 if enrolled and 0 otherwise) of child of birth order $i$ or, simply put, child $i$. $r$ is positive possibly for two reasons: the parent may care directly about the child's education and/or about future consumption determined by the child's education. We assume $r$ is equal across the children. We will discuss later the case where the assumption does not hold. We assume that $g'(n)>0$ and $g''(n)<0$ due to economy of scale and that $U(0)=-\infty$, $U'[\cdot]>0$, and $U''[\cdot]<0$.

A child may either enroll in a school or work. If child $i$ is enrolled, it costs the parent $p_i$. If child $i$ works, the child contributes $w_i$ to the household income, either by earning the wage or increasing the family farm output. We assume that $p_1 \geq p_2 \geq \cdots \geq p_n$ and $w_1 > w_2 > \cdots > w_n$. That is, the direct cost of a child's education is not lower than that of his or her younger sibling's education, and earlier-born children command higher wages in the rural labor market or be more productive than later-born children. The latter assumption is made based upon the well-established empirical finding in the literature on child labor that wages increase with the age of child workers which happens probably because the older child is more experienced, more mature, and more able to perform complex task than the later-born and younger siblings (Emerson and Souza, 2008). We assume that the parent has exogenous income of $y$. It implies the following budget constraint for the parent:

$$y+\sum_{i=1}^{n} w_i(1-e_i)=c+\sum_{i=1}^{n} p_i e_i, \quad (2)$$

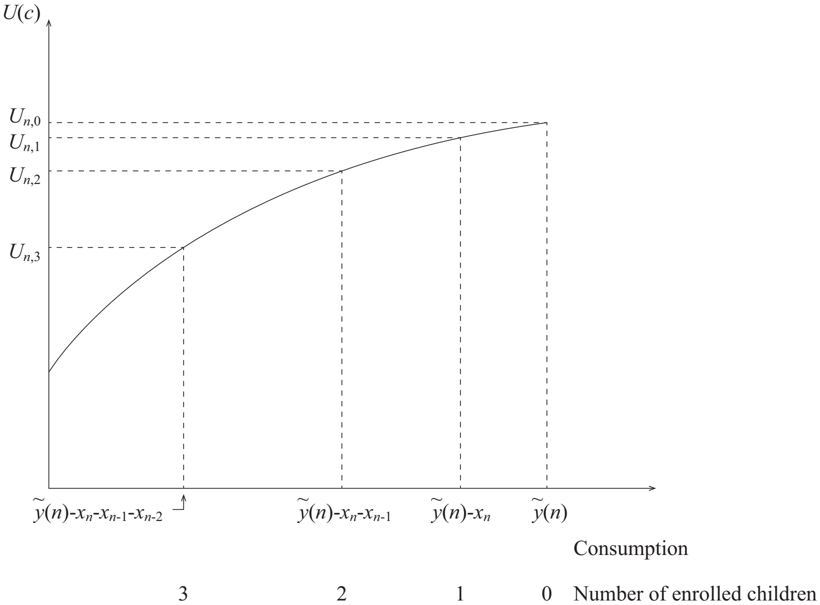
or, equivalently,

$$c=y+\sum_{i=1}^{n} w_i-\sum_{i=1}^{n} e_i x_i,$$

where $x_i=w_i+p_i$ is the total cost of educating child $i$. By the assumptions, $x_1>\ldots>x_n$.

Substituting the budget constraint for $c$ in the objective function (1) and defining $\tilde{y}(n)\equiv y+\sum_{i=1}^{n} w_i-g(n)$, we can rewrite the parental optimization problem as

FIG 1. PARENTAL UTILITY FROM CONSUMPTION $U(c)$




$$\max_{e_1,\ldots,e_n} U\left[\tilde{y}(n)-\sum_{i=1}^{n} e_i x_i\right]+r\sum_{i=1}^{n} e_i.$$

Note that since $x_1 > \ldots > x_n$, whenever $\sum_{i=1}^{n} e_i = j > 0$, $\max[\tilde{y}(n)-\sum_{i=1}^{n} e_i x_i] = \tilde{y}(n)-\sum_{k=n-j+1}^{n} x_k$. This means that whenever the parent chooses to enroll $j$ children in school, the parent will enroll the $j$ youngest children among the $n$ children since it is the cheapest way to do so. This implies that the probability of enrollment and, possibly, the schooling level are likely to be correlated positively with birth order within a household. It also implies that, across multiple households, birth order should have a positive relationship with the probability of school enrollment, after controlling for sibship size and age.

Since the parent will enroll children in school in the reverse birth order, the parental optimization problem can be rewritten into

$$\max_{j} U\left[\tilde{y}(n)-I(j>0)\sum_{k=n-j+1}^{n} x_k\right]+r\cdot j,$$

where $j \in \{0,1,\ldots,n\}$ and $I(\cdot)$ is the indicator function that equals one if the condition in the parenthesis is satisfied and zero otherwise.

Let $U_{n,j}$ denote $U\left[\tilde{y}(n)-I(j>0)\sum_{k=n-j+1}^{n} x_k\right]$, that is, the parental utility from consumption given that $j$ youngest children out of $n$ children are enrolled in school. Then $U_{n,j}-U_{n,j+1}=U\left[\tilde{y}(n)-I(j>0)\sum_{k=n-j+1}^{n} x_k\right]-U\left[\tilde{y}(n)-\sum_{k=n-j}^{n} x_k\right]$ is the loss of utility due to lower consumption induced by enrolling an additional child, or the marginal cost of enrolling a child, given that $j$ children are already enrolled in school. Since the function $U(\cdot)$ is increasing and strictly concave and $x_1 > ... > x_n$, it follows that $U_{n,0} - U_{n,1} < U_{n,1} - U_{n,2} < \cdots < U_{n,n-1} - U_{n,n}$, as shown in Figure 1. That is, the marginal cost incurred to the parent of enrolling an additional child increases with the number of children already enrolled. The marginal benefit of enrolling a child, on the other hand, is the constant $r$. Therefore, if $U_{n,j^*-1} - U_{n,j^*} \leq r < U_{n,j^*} - U_{n,j^*+1}$, the optimal number of children to enroll is $j^*$ and the youngest $j^*$ children are enrolled[3].

The optimal decision rule implies that an upward shift of the marginal cost curve may decrease the number of enrolled children chosen by the parent. Such an upward shift of the marginal cost curve can be caused by an increase of sibship size. To see this, let us compare a family of $n+1$ children with a family of $n$ children. Suppose that the parents have the identical preference and income and that the older $n$ children of the former are identical to the children of the latter. In this case the marginal cost of enrolling child 1 to $n$ is higher for the former family than that of enrolling the corresponding child for the latter family, or using the mathematical notation, $U_{n+1,j} - U_{n+1,j+1} > U_{n,j-1} - U_{n,j}$ for $j = 1, ..., n$. It can be shown as follows. Notice that for $j = 1,...,n$

$$\begin{aligned} U_{n+1,j} &= U\left[\tilde{y}(n+1) - \sum_{k=n-j+2}^{n+1} x_k\right] = U\left[\tilde{y}(n+1) - I(j>1)\sum_{k=n-j+2}^{n} x_k - x_{n+1}\right] \\ &= U\left[\tilde{y}(n) - I(j>1)\sum_{k=n-j+2}^{n} x_k - p_{n+1} - \{g(n+1) - g(n)\}\right] \\ &< U\left[\tilde{y}(n) - I(j>1)\sum_{k=n-j+2}^{n} x_k\right] = U_{n,j-1}. \end{aligned}$$

Furthermore,

$$U_{n+1,j} - U_{n+1,j+1} = U\left[\tilde{y}(n+1) - \sum_{k=n-j+2}^{n+1} x_k\right] - U\left[\tilde{y}(n+1) - \sum_{k=n-j+2}^{n+1} x_k - x_{n-j+1}\right]$$

and

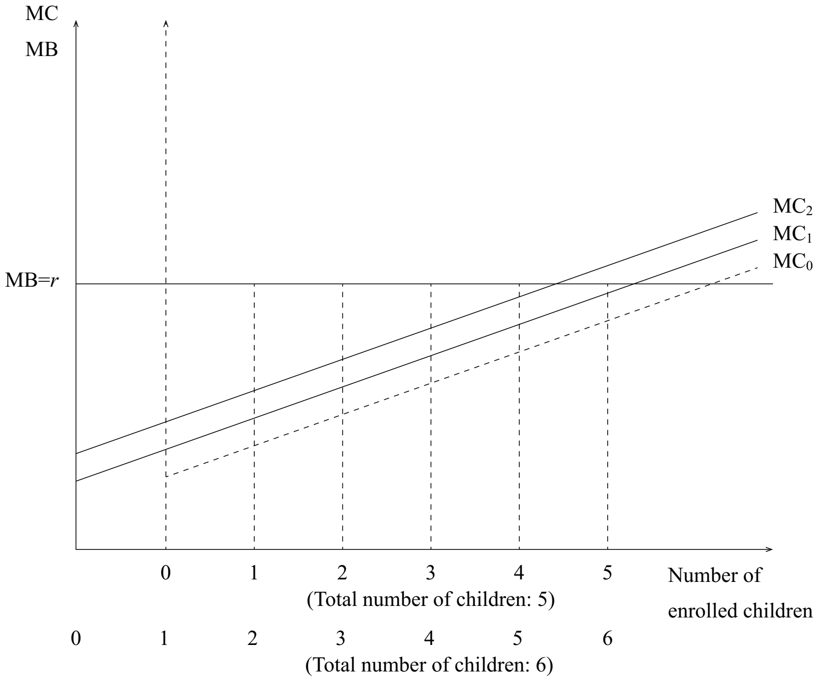
$$U_{n,j-1} - U_{n,j} = U\left[\tilde{y}(n) - I(j>1)\sum_{k=n-j+2}^{n} x_k\right] - U\left[\tilde{y}(n) - I(j>1)\sum_{k=n-j+2}^{n} x_k - x_{n-j+1}\right].$$

Since the function $U[\cdot]$ is increasing and strictly concave, it should be the case that $U_{n+1,j} - U_{n+1,j+1} > U_{n,j-1} - U_{n,j}$.

Using the results above, Figure 2 compares the family of five children with the family of six children. Suppose that for the family of five children the marginal cost curve for enrolling a child is given by the dashed $MC_0$, and thus every child is enrolled in school. For the family of six children the MC curve must be located above $MC_0$. One possibility is $MC_1$, which is not far above $MC_0$. In this case the parents of the bigger family also enroll every child in school, and there is no difference in school enrollment status of child 1 to 5 in each family. However, if the marginal cost curve for the family of six children is $MC_2$, the parent will choose to enroll only five out of six children. In this case the first-born child in the bigger family is disenrolled,

[3] There are cases for corner solutions. If $U_{n,n-1} - U_{n,n} \leq r$, all children are enrolled; if $r < U_{n,0} - U_{n,1}$, no child is enrolled.

FIG 2. EFFECT OF AN INCREASE OF SIBSHIP SIZE ON THE OPTIMAL NUMBER OF ENROLLED CHILDREN




while the others are still enrolled in school.

This suggests that an increase of the sibship size is likely to have the greater negative effect on education of a earlier-born child than that of a later-born child. The intuition behind this result is simple. If an increase of the household size requires more resources diverted to consumption, the parent may choose to make some children to work instead of going to school. In such a case it is most advantageous to the parent to make the oldest child, who commands the highest wage, to quit school and to work.

It should be noted that the theoretical results discussed so far may not hold if the parent values education of older children more than that of younger children. For example, if the parents care more about education of older children than that of younger children, so that $r_1 > r_2 > \cdots > r_n$, the theoretical prediction on the birth order effect is ambiguous (Emerson and Souza, 2008). In a subsistence-level economy, however, it is likely that consideration for the cost of education and forgone income dominates the consideration for the utility value of education. Still, one can argue that in a multi-period setting the parent has an incentive to keep investing in education of earlier-born children of higher ability in expectation of higher return in the future. This argument loses the appeal to a substantial degree, however, if the credit market is imperfect and negative income shocks are frequent, as is common in rural areas in low-income countries.

It has been shown that under the assumptions of our model birth order is likely to be positively correlated with education and the sibship size effect is likely to decrease by birth order. Now we turn our attention to the empirical analysis. We estimate the effect of sibship size on two measures of parental investment in children's education: current school enrollment status for the school-aged (7 to 25) individuals and the number of years of schooling for the older (19 and above) and disenrolled individuals. In our empirical analysis we deal with endogeneity of the sibship size, which is assumed to be exogenous in the theoretical discussion so far, using instrumental variables.

## IV. *Data*

We analyze a sample from the Matlab Health and Socioeconomic Survey (MHSS) of 1996. The MHSS draws a random sample of baris (household clusters) and households from 141 villages of Matlab, Bangladesh, a rural region located about 60 kilometers southeast of Dhaka, the country's capital. The MHSS is a multipurpose and comprehensive survey that collects a broad array of socio-demographic information on individuals, families, and communities.

A special feature of MHSS we exploit for this study is that one half of the main households interviewed by the MHSS are in the treatment group of the randomized family planning program, the Maternal and Child Health and Family Planning (MCH-FP) service, and the other half in the control group. We use the treatment status by MCH-FP service as the IV to deal with endogeneity of sibship size.

### 1. MCH-FP Service

MCH-FP service started in 1978 in 70 treatment villages in Matlab, Bangladesh by the International Center for Diarrhoeal Disease Research, Bangladesh (ICDDR-B). At first the program had provided only family planning services in addition to the government services which both the treatment villages and the other 72 control villages had received. In 1982 one half of the treatment villages started to get additional maternal and child health services such as antenatal care and immunization. In 1986 the additional service has expanded to all the treatment villages (Joshi and Schultz, 2007).

Randomness of program assignment of MCH-FP service has been examined by Sinha (2005) and Joshi and Schultz (2007). Using MHSS data, Sinha (2005, Table 1) has found no significant mean difference in the number of children ever born, education, spousal education, and small farmland ownership among the preprogram cohort—evermarried women aged 40 or older in 1978—across the control and the treatment regions. Furthermore, using the 1974 census data, Joshi and Schultz (2007, Table 2) have found no significant difference in child-woman ratios (proxy for surviving fertility), average years of schooling, and average housing condition (persons in house with tin roof) in 1974 before the program started. The salient preprogram difference between the control and the treatment villages turns out to be the religious composition of the population: the proportion of Hindu families is significantly higher in the treatment villages than in the control villages. All in all the two studies suggest together that the control and the treatment status were assigned at random at least at the beginning of MCH-FP.

The MCH-FP has made significant differences in the contraceptive prevalence rate and the fertility rate between the treatment area and the control area (Foster and Roy, 1997; Sinha, 2005; Schultz, 2009). The contraceptive prevalence rate in the treatment area has increased from 7 percent before the treatment to 57.6 percent in 1990. The contraceptive prevalence rate in the control area was 27.9 percent in 1990. The fertility rate and the child mortality rate have been consistently and substantially lower in the treatment area than in the control area. For example, the annual crude fertility rate in the treatment area has been lower by 2 to 10 births per 1000 population than that in the control area between 1985 and 1996 (Mostafa et al., 1998).

## 2. The Sample

Our sample consists of individuals who meet the following three criteria. First, their biological father must be a household head, and the mother must be 36 to 50 years old at the time of the MHSS survey. We restrict the sample to heads' children, because household decision-making process for investment in non-heads' children may differ from that for heads' children. We exclude children of the younger mothers, because the effect of the parents' yet-to-be-realized 'desired' sibship size on children's education cannot be identified from the effect of 'realized' sibship size on children's education. The children of the mothers older than 50 are excluded because MCH-FP program did not exist while their mother was fertile. Second, individuals must be 7 years or older, as the primary education starts at age 6 in Bangladesh. We do not put any upper limit on age. The oldest individual in the sample turns out to be 34 years old. The individuals in the sample may or may not coreside with their parents. We obtain information on non-coresident children's age, sex, and education from mothers. 6,195 children of 1,350 pairs of parents satisfy the first and the second criteria.

Third, the father should have resided in the village he currently resides in when he was 12 years old or before he got married. 1,530 children of 342 pairs of parents are dropped because they do not satisfy this criterion. This restriction is to reduce the potential bias from endogenous sorting of parents which may have happened between the inception of the MCH-FP program and the survey. Sinha (2005) and Joshi and Schultz (2007) have shown that assignment of control and treatment status was at random at the beginning of MCH-FP program. However, the characteristics of immigrants to the villages may differ systematically by the treatment status. For example, since the MCH-FP services lower the price of fertility control and the cost of improving children's health, parents who have strong taste for children's quality over quantity may be more willing to be in the treatment area than those who have weaker taste for children's quality.

The data indeed show that the characteristics of immigrant parents, who moved into the treatment or the control villages after marriage, are markedly different between the control and the treatment areas. Among the 342 pairs of immigrant parents, the average years of schooling of fathers (mothers) in the treatment area is greater than that of their counterparts in the control area by 2.5 (1.9) years. In contrast, among the native parents, the difference is still positive, but by less than a year. In addition, among the immigrant parents, those who moved into the treatment area are much wealthier than those who moved into the control area, while the difference between the native parents is much smaller. This suggests that endogenous sorting is likely to have happened and that our sample restriction can reduce the effect of sorting. However, it should be noted that since staying is another form of endogenous sorting, we cannot completely rule out unobserved heterogeneity from our sample of natives.

After excluding 155 children who are reported to be born to a mother younger than 16 in order to reduce noise from reporting errors and excluding those who have missing information on any of the necessary variables, we are left with the sample of 4,182 children of 952 pairs of parents. We use two subsamples for our analysis. One is the sample of individuals aged 7 to 25 and the other is the sample of individuals aged 19 and older—the oldest is 34—and not enrolled in any school at the time of the survey. The former, called the 'enrollment sample', is used to estimate the effect of sibship size on school enrollment and the latter, called the 'schooling sample', is used to estimate the effect of sibship size on years of schooling.

We measure an individual's sibship size by the number of siblings who have survived more than a year after being born, since a sibling who had died shortly after the birth is likely to have had little effect on the household resources used by the individual. Since MHSS uses information on vital events from Demographic Surveillance System (DSS) that has been in existence in Matlab since 1966 to reduce reporting errors, the key variable is not likely to suffer from severe measurement error[4].

An individual's birth order is determined by his or her age in comparisons with the ages of the siblings. The deceased siblings, if any, are not counted, since it turns out to be extremely difficult to find the exact pregnancy order or the pregnancy outcome order of non-coresident children[5].

Tables 1 and 2 show the summary statistics of the enrollment sample and schooling sample. The summary statistics are shown separately for those in the control area and those in the treatment area for comparisons. The two tables show that in general the individuals in (or from) the treated villages are more likely to be enrolled in a school and have got, on average, more schooling than those in the control villages. Among the first-born and the second-born individuals, those in the treatment area are more likely to be enrolled in school than those in the control area. Among the later-born individuals, the opposite pattern holds, mainly because those in the control area are younger and thus more likely to be enrolled in school than those in the treatment area due to age. The average years of education, on the other hand, are greater among the individuals in the treatment area than those in the control area across birth orders and sexes, except among second-born women. Males are more likely to be enrolled and better educated than females in both areas. Overall we find that there are unconditional differences in enrollment and schooling between the two areas, but that the differences are not particularly large or strong.

The average age of children in the enrollment sample is about 16 and that in the schooling sample about 24. The average sibship size is 5 to 6 and differs by about 0.6 between the treatment and the control areas. The reported sibship size is close to the total fertility rate of Matlab area in the early 1980s (Mostafa et al., 1998) during which most mothers in our sample were fertile. Children and parents from the treatment area are older than their counterparts from

[4] Through fortnightly visits to households, the DSS has collected information on dates of birth/pregnancy, death, migration, and marriage. The results from DSS are used to correct reporting errors on such events by the MHSS respondents (Rahman et al., 1999). The DSS, however, does not provide information on some early births by older women. Information on those is provided by self-reports alone.

[5] We tried to use the pregnancy order information by matching the reported age of each non-coresident child to the date of a pregnancy outcome reported by the mother. However, match failed for 47% of non-coresident children (576 children). Furthermore, there may be substantial matching errors even among the matched non-coresident children, as the age and the date information is likely to be noisy.

Table 1. Summary Statistics of Enrollment Sample (7≤age≤25)

| | Control Area | Treatment Area | $p$ |
|---|---|---|---|
| Enrollment status (1 if enrolled) | | | |
| Age 7 to 12 | 0.87 | 0.88 | 0.774 |
| Male | 0.89 | 0.88 | 0.890 |
| Female | 0.86 | 0.88 | 0.599 |
| Age 13 to 19 | 0.60 | 0.62 | 0.270 |
| Male | 0.60 | 0.61 | 0.920 |
| Female | 0.59 | 0.64 | 0.140 |
| Age 20 to 25 | 0.18 | 0.19 | 0.855 |
| Male | 0.26 | 0.26 | 0.995 |
| Female | 0.09 | 0.10 | 0.560 |
| Birth order=1 | 0.30 | 0.36 | 0.117 |
| Male | 0.30 | 0.41 | 0.064 |
| Female | 0.30 | 0.31 | 0.773 |
| Birth order=2 | 0.42 | 0.45 | 0.314 |
| Male | 0.45 | 0.47 | 0.610 |
| Female | 0.38 | 0.43 | 0.289 |
| Birth order>2 | 0.69 | 0.63 | 0.002 |
| Male | 0.71 | 0.64 | 0.005 |
| Female | 0.66 | 0.62 | 0.153 |
| Gender (1 if female) | 0.46 | 0.48 | 0.120 |
| Age | 15.62 (4.96) | 16.55 (5.00) | <0.001 |
| Birth order | 3.45 (1.78) | 3.28 (1.71) | 0.004 |
| | [Household-level characteristics] | | |
| Sibship size | 5.77 (1.97) | 5.18 (1.76) | <0.001 |
| Father's years of education | 3.07 (3.96) | 3.73 (4.33) | 0.015 |
| Father's age | 51.72 (7.04) | 51.84 (7.28) | 0.767 |
| Mother's years of education | 1.63 (2.51) | 1.99 (2.72) | 0.036 |
| Mother's age | 42.15 (4.26) | 42.44 (4.34) | 0.325 |
| Religion (1 if Muslim father) | 0.95 | 0.83 | <0.001 |
| Household income | 30732 (42782) | 34101 (49529) | 0.266 |
| Household assets | 415489 (3209118) | 494338 (4168331) | 0.743 |
| No. of members aged 26 to 54 | 1.87 (0.61) | 1.90 (0.70) | 0.538 |
| No. of members older than 54 | 0.50 (0.56) | 0.53 (0.58) | 0.353 |
| No. of schools in village | 1.83 (1.47) | 2.82 (2.06) | <0.001 |
| Number of individuals | 1771 | 1919 | |
| Number of households | 439 | 511 | |

*Note:* Standard deviations in the parentheses. The last column is the $p$-value from the test of the hypothesis that the difference is zero. The test is the $t$ test for means and the binomial probability test for proportions.

the control area.

On average, the parents in the enrollment sample are younger and better educated than those in the schooling sample. This is likely to reflect improvement in education of Bangladeshi population over time. As discussed above, parents in the treatment area are, on average, more educated than those in the control area. For fathers the difference in average years of education is 0.7 for the enrollment sample and 0.4 for the schooling sample, and for mothers 0.4 and 0.3 respectively.

TABLE 2. SUMMARY STATISTICS OF SCHOOLING SAMPLE (19≤AGE≤34 AND UNENROLLED)

| | Control Area | Treatment Area | $p$ |
|---|---|---|---|
| Years of education | 4.26 (4.09) | 4.75 (4.20) | 0.032 |
| Male | 4.76 (4.41) | 5.38 (4.40) | 0.072 |
| Female | 3.78 (3.70) | 4.13 (3.91) | 0.217 |
| | | | |
| Birth order=1 | 4.36 (4.31) | 5.36 (4.41) | 0.008 |
| Male | 5.13 (4.85) | 5.93 (4.54) | 0.162 |
| Female | 3.57 (3.53) | 4.79 (4.22) | 0.012 |
| Birth order=2 | 4.46 (4.20) | 4.48 (4.19) | 0.987 |
| Male | 4.90 (4.26) | 5.49 (4.47) | 0.354 |
| Female | 4.04 (4.12) | 3.56 (3.71) | 0.359 |
| Birth order>2 | 3.96 (3.68) | 4.35 (3.92) | 0.307 |
| Male | 4.16 (3.89) | 4.73 (4.13) | 0.335 |
| Female | 3.78 (3.49) | 3.97 (3.67) | 0.683 |
| | | | |
| Gender (1 if female) | 0.51 | 0.51 | 0.979 |
| Age | 24.02 (3.71) | 24.15 (3.51) | 0.495 |
| Birth order | 2.10 (1.14) | 2.23 (1.27) | 0.051 |
| | [Household-level characteristics] | | |
| Sibship size | 6.27 (1.92) | 5.63 (1.75) | <0.001 |
| Father's years of education | 2.73 (3.73) | 3.08 (3.77) | 0.233 |
| Father's age | 53.85 (6.00) | 54.14 (6.35) | 0.543 |
| Mother's years of education | 1.31 (2.24) | 1.55 (2.30) | 0.167 |
| Mother's age | 43.83 (3.88) | 44.25 (3.79) | 0.172 |
| Religion (1 if muslim father) | 0.95 | 0.84 | <0.001 |
| Household income | 31249 (46134) | 34123 (50512) | 0.453 |
| Household assets | 513903 (3857146) | 602017 (5051485) | 0.802 |
| No. of members aged 26 to 54 | 1.83 (0.66) | 1.84 (0.68) | 0.859 |
| No. of members older than 54 | 0.56 (0.57) | 0.63 (0.57) | 0.153 |
| No. of schools in village | 1.71 (1.41) | 2.93 (2.09) | <0.001 |
| Number of children | 643 | 820 | |
| Number of households | 302 | 347 | |

*Note:* Standard deviations in the parentheses. The last column is the $p$-value from the test of the hypothesis that the difference is zero. The test is the $t$ test for means and the binomial probability test for proportions.

The majority is Muslim in both areas, but the fathers in the control area are more likely to be Muslim than those in the treatment area. Non-Muslims are mostly Hindus. Households in the treatment area has, on average, higher income and greater wealth than those in the control area, but the difference is not statistically significant. Villages located in the treatment area have more schools—primary, secondary, or Islam schools— than those located in the control area. This may contribute to the difference in education between the control and the treatment areas.

## V. *Estimation Results*

The model we estimate is

$$y_i = \alpha_s s_i + \alpha_{sp2} s_i b_{2i} + \cdots + \alpha_{spk} s_i b_{ki} + \alpha_{p2} b_{2i} + \cdots + \alpha_{pk} b_{ki} + \beta \mathbf{X}_i + e_i, \qquad (3)$$

where $y_i$ is the outcome variable (enrollment status and the years of completed schooling), $s_i$ is the sibship size, $b_{2i},...,b_{ki}$ are the birth order dummies, $\mathbf{X}_i$ is the individual and family characteristics assumed to be exogenous, and $e_i$ is the error term that is likely to be correlated with $s_i$ and thus with $s_i b_{2i},...,s_i b_{ki}$. The coefficients of interest are $\alpha_s,(\alpha_s+\alpha_{sp2}),...,(\alpha_s+\alpha_{spk})$.

Birth orders are categorized into five groups (1, 2, 3, 4, and above 4) and $k=5$ if $y_i$ is school enrollment status; they are categorized into three groups (1, 2, and above 2) and $k=3$ if $y_i$ is completed schooling. The birth orders are grouped so that each category has roughly the same number of observations. The numbers of categories differ between the two samples because of the sample size and the age of the individuals in the samples.

The vector of exogenous variables $\mathbf{X}_i$ consists of age dummies, sex dummy, parental education and age, Muslim dummy, household income and assets quartile dummies, number of household members aged 26 to 54 and above 54, and number of schools in the village. Household income and asset quartile dummies are used instead of the levels due to the concern on measurement error. Using the level does not change the results much. Robust standard errors that assume clustering by village are used for the inferences[6].

Since we assume that the sibship size is endogenous, we use IVs for $s_i, s_i b_{2i},...,s_i b_{ki}$. The IVs are the MCH-FP treatment status ($t_i$) and its interactions with the birth order dummies ($t_i b_{2i},...,t_i b_{ki}$).

### 1. The Main Results

Table 3 shows estimated coefficients of the interaction terms of the treatment dummy and birth order dummies in the first stage along with their robust standard errors. The table is divided into columns labeled (A) and (B) by the second-stage outcome variable: (A) for the enrollment status and (B) for the years of completed schooling. For columns (A) there are five equations to be estimated in the first stage and the results are shown in panels [1] to [5]; for columns (B) there are three equations to be estimated in the first stage and the results are shown in panels [1] to [3]. The heading of each panel indicates the second-stage outcome variable and the first-stage dependent variable of the equation estimated.

The results show that the individuals in or from the treated villages have on average 0.4 to 0.7 fewer siblings than those in or from the control villages. In column (A) the coefficients of the IVs are jointly significant at the 1% level in panels [1] to [4] and at the 6% level in panel [5]; in column (B) they are jointly significant at the 1% level in panels [1] and [2] and at the 6% level in panel [3].

Table 3 reports two statistics used to detect the weak IV problem with multiple endogenous variables. One is Anderson canonical correlation LR statistic (Hall et al., 1996) and the other is Cragg-Donald weak instrument statistic (Stock and Yogo, 2002). Based on the Anderson canonical correlation LR statistic, we reject at any significance level that at least one of the canonical correlations between the endogenous variables and the instruments is zero. Furthermore, the Cragg-Donald weak instrument test rejects at the 5% level that our

[6] Given the identifying variation comes from a village-level instrument, the specifications are preferable to be clustered at the village level rather than at the household level.

TABLE 3. FIRST-STAGE REGRESSION RESULTS FOR IVS

| | (A) Enrollment | | (B) Schooling | |
|---|---|---|---|---|
| | Coeff. | Robust s.e. | Coeff. | Robust s.e. |
| [1] Dependent variable: | (A,B) Sibship size | | | |
| Treated | −.5919 | .1380 | −.6280 | .1860 |
| Treated × birth order = 2 | −.0610 | .0659 | −.0064 | .1326 |
| Treated × birth order > 2 | − | − | −.0436 | .2377 |
| Treated × birth order = 3 | −.0376 | .1026 | − | − |
| Treated × birth order = 4 | −.0237 | .1332 | − | − |
| Treated × birth order > 4 | .0841 | .1884 | − | − |
| [2] Dependent variable: | (A,B) Sibship size × birth order = 2 | | | |
| Treated | −.0029 | .0195 | −.000005 | .0331 |
| Treated × birth order = 2 | −.5886 | .1294 | −.6722 | .1649 |
| Treated × birth order > 2 | − | − | .0382 | .0331 |
| Treated × birth order = 3 | .0028 | .0173 | − | − |
| Treated × birth order = 4 | .0127 | .0177 | − | − |
| Treated × birth order > 4 | −.0011 | .0204 | − | − |
| [3] Dependent variable: | (A) Sibship size × birth order = 3 | | (B) Sibship size × birth order > 2 | |
| Treated | −.0083 | .0190 | .0026 | .0489 |
| Treated × birth order = 2 | −.0106 | .0138 | −.0043 | .0311 |
| Treated × birth order > 2 | − | − | −.7164 | .2717 |
| Treated × birth order = 3 | −.5751 | .1146 | − | − |
| Treated × birth order = 4 | .0154 | .0207 | − | − |
| Treated × birth order > 4 | −.0127 | .0219 | − | − |
| [4] Dependent variable: | (A) Sibship size × birth order = 4 | | | |
| Treated | .0011 | .0170 | − | − |
| Treated × birth order = 2 | −.0148 | .0110 | − | − |
| Treated × birth order = 3 | −.0270 | .0140 | − | − |
| Treated × birth order = 4 | −.6129 | .1292 | − | − |
| Treated × birth order > 4 | −.0441 | .0267 | − | − |
| [5] Dependent variable: | (A) Sibship size × birth order > 4 | | | |
| Treated | −.0098 | .0238 | − | − |
| Treated × birth order = 2 | .0015 | .0119 | − | − |
| Treated × birth order = 3 | −.0111 | .0133 | − | − |
| Treated × birth order = 4 | −.0129 | .0195 | − | − |
| Treated × birth order > 4 | −.4340 | .1711 | − | − |
| Anderson LR stat. | 66.31 | | 44.18 | |
| C-D stat. | 13.23 | | 14.57 | |

*Note:* Robust standard errors clustered by village are reported. Estimates of the other coefficients are not shown. Anderson LR statistic is the Anderson canonical correlation LR statistic (Hall et al., 1996) and C-D statistic is the Cragg-Donald weak instrument statistic (Stock and Yogo, 2002).

instruments are weak, given that we accept 10% of the bias by OLS as the maximal bias of 2SLS (Table 1, Stock and Yogo 2002).

Although they are not shown here for brevity, the first-stage results are consistent with general expectations. Sibship size of a female is greater than that of a male, most likely due to Bangladeshi parents' traditional preference for sons. Father's years of education coefficients are estimated to be small and not statistically significant. Mother's years of education coefficients, on the other hand, are estimated to be negative, greater in the magnitude, and statistically

Table 4. Estimated Effects of Sibship Size on Education by Birth Order

| | 2SLS | | OLS | |
|---|---|---|---|---|
| | (1) Enrollment (N=3690) | (2) Schooling (N=1463) | (3) Enrollment (N=3690) | (4) Schooling (N=1463) |
| | (A) Estimated coefficients | | | |
| Sibship size | −.1260 | −1.2103 | −.0295 | −.2738 |
| | (2.33) | (2.41) | (2.98) | (2.66) |
| Sibship size × birth order=2 | .0114 | 1.0819 | .0052 | .1485 |
| | (0.18) | (1.86) | (0.48) | (1.37) |
| Sibship size × birth order>2 | – | 1.1171 | – | .1069 |
| | | (1.49) | | (0.79) |
| Sibship size × birth order=3 | .1973 | – | −.0026 | – |
| | (2.53) | | (0.23) | |
| Sibship size × birth order=4 | .1184 | – | −.0079 | – |
| | (1.62) | | (0.74) | |
| Sibship size × birth order>4 | .1473 | – | .0233 | – |
| | (2.13) | | (1.80) | |
| | (B) Implied sibship size coefficients | | | |
| Birth order=1 | −.1260 | −1.2103 | −.0295 | −.2738 |
| | (2.33) | (2.41) | (2.98) | (2.66) |
| Birth order=2 | −.1145 | −.1284 | −.0243 | −.1253 |
| | (1.90) | (0.27) | (3.39) | (1.20) |
| Birth order>2 | – | −.0932 | – | −.1668 |
| | | (0.17) | | (1.69) |
| Birth order=3 | .0713 | – | −.0321 | – |
| | (1.28) | | (3.90) | |
| Birth order=4 | −.0076 | – | −.0375 | – |
| | (0.16) | | (5.05) | |
| Birth order>4 | .0213 | – | −.0062 | – |
| | (0.32) | | (0.82) | |
| Joint Wald test *p*-value | 0.047 | 0.103 | 0.000 | 0.031 |

*Note:* Robust *t* values are in the parentheses. The other coefficient estimates are not shown.

significant at the 5% level in three of the five first stage results. Muslims have more children than Hindus. Wealthier family have more children. The number of schools in the village is in general positively correlated with sibship size. It may be due to the effect of demand for education or the family may have more children in an environment where educating children is easier or cheaper.

Table 4 shows the main results of this study. It shows estimated coefficients of the interaction terms of sibship size variable and birth order dummies (panel A), and the implied effect of sibship size on enrollment status and completed schooling of an individual by birth order (panel B). Columns 1 and 2 show the 2SLS results and, for comparisons, columns 3 and 4 the OLS results.

The 2SLS results in columns 1 and 2 show clearly that the effect of sibship size on education of the individual changes from being negative, large in magnitude and statistically significant to being small in magnitude and statistically insignificant with increasing birth order,

TABLE 5. EFFECT OF SIBSHIP SIZE ON LABOR FORCE PARTICIPATION BY BIRTH ORDER

| | 2SLS | | OLS | |
|---|---|---|---|---|
| | (1) All (N=3594) | (2) Men (N=1910) | (3) All (N=3594) | (4) Men (N=1910) |
| | (A) Estimated coefficients | | | |
| Sibship size | .1902 | .1601 | .0146 | .0231 |
| | (2.76) | (2.63) | (1.65) | (2.11) |
| Sibship size × birth order=2 | −.0415 | −.1090 | −.0006 | −.0048 |
| | (0.68) | (1.41) | (0.07) | (0.41) |
| Sibship size × birth order=3 | −.1726 | −.1530 | .0186 | .0028 |
| | (2.37) | (1.94) | (1.83) | (0.15) |
| Sibship size × birth order=4 | −.1207 | −.1549 | .0307 | .0235 |
| | (1.43) | (1.72) | (2.63) | (1.47) |
| Sibship size × birth order>4 | −.2325 | −.3145 | −.0103 | −.0138 |
| | (2.06) | (2.32) | (0.77) | (0.89) |
| Anderson LR statistics | 61.01 | 46.51 | NA | |
| Cragg-Donald statistics | 12.14 | 9.18 | NA | |
| | (B) Implied sibship size coefficients | | | |
| Birth order=1 | .1902 | .1601 | .0146 | .0231 |
| | (2.76) | (2.63) | (1.65) | (2.11) |
| Birth order=2 | .1487 | .0511 | .0141 | .0182 |
| | (1.87) | (0.67) | (1.90) | (1.81) |
| Birth order=3 | .0176 | .0071 | .0332 | .0259 |
| | (0.31) | (0.11) | (3.82) | (1.66) |
| Birth order=4 | .0695 | .0052 | .0454 | .0465 |
| | (1.23) | (0.09) | (5.11) | (4.09) |
| Birth order>4 | −.0424 | −.1543 | .0043 | .0092 |
| | (0.45) | (1.21) | (0.43) | (0.78) |
| Joint Wald test *p*-value | 0.056 | 0.075 | 0.000 | 0.000 |

*Note:* Robust *t* values are in the parentheses. The other coefficient estimates are not shown.

as the model in section III predicts. The results in panel (B) show that sibship size has a strong negative effect on the probability of school enrollment of the first- and the second-born children. One more sibling is estimated to reduce the probability by more than 10 percentage points, everything else equal. Sibship size has also a strong negative effect on the years of schooling of the first-born children. An additional sibling is estimated to reduce their schooling by more than a year. Each of the effects is statistically significant at the 6% or smaller level.

On education of the individuals of higher birth orders, however, sibship size does not have statistically significant effect. The magnitudes are much closer to zero. School enrollment status of the third-born and the later-born individuals and the years of schooling of the second-born and the later-born individuals do not appear to be affected by sibship size. The results shown in panel (A) show that the magnitudes of sibship size effect on school enrollment status of the third- and later-born individuals are statistically different from those of the first- and second-born individuals. For the years of schooling, however, coefficients of the interaction terms are not jointly significant.

The joint test of the sibship size coefficient and interaction term coefficients, shown in the last row of Table 4, reveals that sibship size has jointly statistically significant effect on school enrollment at the 5% level. However, effect of sibship size on schooling is marginally insignificant at the 10% level in the joint test, although the effect on schooling of the first-born child is individually significant at the 5% level. This suggests that the effect of increasing sibship size on education may be temporary, except for the first-born child. When subsistence level of consumption is threatened, some families drop children out of school, but they may send them back to school once the economic situation improves. That is, an increase of sibship size causes delays in education, but it may not necessarily lead to less education over the lifetime, especially for the second- and later-born children[7].

Our model in Section III attributes the results in Table 4 to parental choice between work and education of the children and different values of a child's work in the labor market by birth order. According to the model, the effect of sibship size should have different effects not only on education but also on labor market status by birth order. So we have estimated the effect of sibship size on labor force participation—working in family business or for others, or looking for work—using the same setup. Individuals younger than 11 are not used as the labor market status for them is missing in the data. The enrollment sample and the schooling sample are pooled. The results are shown in Table 5. Columns (1) and (3) show the 2SLS and OLS estimation results using both men and women, and columns (2) and (4) the results using only men.

The 2SLS results in panel (B) show that sibship size has positive and statistically significant effects on labor force participation of first-born individuals. For the later-born individuals, the coefficient is far smaller in magnitude and not statistically significant, except for the coefficient of the second birth order dummy in column (1) that is statistically significant at the 7% level. The estimation result is consistent to the result in Table 4, indicating that an increase of sibship size causes the first-born and the second-born individuals to drop out of school and to enter the labor market, possibly temporarily[8].

Comparing the 2SLS results to the OLS results in columns 3 and 4 of Tables 4 and 5, we find that the OLS coefficients of sibship size are much smaller in magnitude than the 2SLS coefficients for first-born individuals and, in some cases, second-born individuals. What is responsible for the difference? First, it is possible that the 2SLS estimates of the coefficients are biased downward due to endogenous sorting. As discussed in Section IV the parents in the treatment area are different from those in the control area in some aspects (e.g., education). These qualities may contribute to increasing the education of the children in the treatment area, while the IV, the treatment area dummy, is negatively correlated with the endogenous variable, i.e., sibship size. It can bias downward the 2SLS estimates of the sibship size effect. However, it does not explain why the coefficient estimates are much smaller in magnitude and even positive for individuals of higher birth order.

---

[7] We have run a regression of 'grade lag' (= expected grade indicated by age-actual grade) on sibship size variable and the other explanatory variables without the interaction terms, instrumenting sibship size by treatment status. The 2SLS coefficient estimate of sibship size is .437 (robust $t$ =2.09) and the OLS counterpart is .128 (robust $t$=3.54). If the dependent variable is replaced by existence of grade lag (= grade lag>0), the 2SLS coefficient is .062 (robust $t$ =2.15) and the OLS coefficient is .006 (robust $t$=1.50). The full results are available upon request.

[8] Another test of the model would be to examine whether individual wages differ by birth order in the labor market. However, it is not feasible since a large number of individuals are family workers and their wage information is missing in the data.

Table 6. Implied Sibship Size Coefficient Estimates by Birth Order: Subsample of Birth Order≤3

| | 2SLS | | OLS | |
|---|---|---|---|---|
| | (1) Enrollment | (2) Schooling | (3) Enrollment | (4) Schooling |
| | (A) Sample restricted to sibship size≥3 | | | |
| Birth order=1 | −.1252 | −1.4023 | −.0297 | −.3240 |
| | (1.87) | (2.71) | (2.54) | (3.00) |
| Birth order=2 | −.1061 | −.1801 | −.0265 | −.1518 |
| | (1.68) | (0.37) | (3.35) | (1.52) |
| Birth order=3 | .0762 | −.2342 | −.0313 | −.1324 |
| | (1.47) | (0.42) | (3.53) | (1.17) |
| Joint Wald test $p$-value | 0.0431 | 0.0542 | 0.0001 | 0.0282 |
| Subsample size | 2043 | 1235 | 2043 | 1235 |
| | (B) Sample restricted to sibship size≥4 | | | |
| Birth order=1 | −.1030 | −.9355 | −.0259 | −.2975 |
| | (1.54) | (2.07) | (2.01) | (2.72) |
| Birth order=2 | −.0879 | −.0474 | −.0252 | −.1659 |
| | (1.50) | (0.10) | (2.66) | (1.61) |
| Birth order=3 | .0710 | .0175 | −.0301 | −.1274 |
| | (1.51) | (0.03) | (2.99) | (1.06) |
| Joint Wald test $p$-value | 0.0895 | 0.2117 | 0.0057 | 0.0550 |
| Subsample size | 1836 | 1183 | 1836 | 1183 |
| | (C) Sample restricted to sibship size≥5 | | | |
| Birth order=1 | −.1491 | −.9256 | −.0207 | −.2522 |
| | (1.63) | (1.68) | (1.10) | (2.18) |
| Birth order=2 | −.1183 | −.0038 | −.0428 | −.1842 |
| | (1.45) | (0.01) | (3.02) | (1.92) |
| Birth order=3 | .0562 | .1550 | −.0399 | −.0963 |
| | (1.07) | (0.23) | (2.95) | (0.74) |
| Joint Wald test $p$-value | 0.1764 | 0.3862 | 0.0038 | 0.1015 |
| Subsample size | 1360 | 1013 | 1360 | 1013 |

*Note:* Robust $t$ values are in the parentheses. The other coefficient estimates are not shown.

Second, it is possible that parents whose first or second child is of high quality have chosen to have more children, so that the quality of the earlier born children is positively correlated with the sibship size. Third, it is also possible that the omitted initial (extended) family wealth, which is positively correlated with both quality and quantity of children, may still cause the bias in OLS, although we control for the current wealth and income using quartile dummies. The positive effect of wealth on education is likely to be greater among earlier born children than among later born children, because earlier born children are first ones to stop going to school among children in times of need.

## 2. Robustness of the Results

A possible criticism of our estimation strategy is that the effect of birth order, the effect of

Table 7. Estimated Coefficients of Sibship Size × Birth Order Dummies from the Fixed-Effect Models

| | IV-FE | | FE | |
|---|---|---|---|---|
| | (1) Enrollment (N=3690) | (2) Schooling (N=1463) | (3) Enrollment (N=3690) | (4) Schooling (N=1463) |
| Sibship size × birth order=2 | .0432 | .5791 | .0019 | .2231 |
| | (0.55) | (1.08) | (0.16) | (2.05) |
| Sibship size × birth order>2 | – | .8390 | – | .1796 |
| | | (1.48) | | (1.52) |
| Sibship size × birth order=3 | .2396 | – | −.0059 | – |
| | (2.88) | | (0.46) | |
| Sibship size × birth order=4 | .1682 | – | −.0151 | – |
| | (1.95) | | (1.07) | |
| Sibship size × birth order>4 | .1874 | – | .0110 | – |
| | (2.02) | | (0.72) | |

*Note:* Robust $t$ values are in the parentheses. The other coefficient estimates are not shown.

sibship size, and their interactions may not be identified with enough precision, since an individual's birth order is by definition positively correlated with the sibship size. To answer this criticism we construct subsamples that are likely to be free from such concern, re-estimate the model, and examine whether the results change in any significant way.

The subsamples we use are constructed using only the first-, the second-, and the third-born individuals. We construct three subsamples of them with the minimum sibship size of three, four, and five. The correlations between birth order and sibship size in the subsamples are far smaller than those in the original whole samples. In the original enrollment sample the correlation is 0.43 and in the original schooling sample 0.27. With the minimum sibship size of three, four, and five, the correlations are 0.12, 0.10, and 0.10 in the enrollment subsamples, and 0.15, 0.14, and 0.13 in the schooling subsamples. If the results with the subsamples differ substantially from the results with the original sample or across the subsamples, we may question if the effects are identified correctly.

The results using the subsamples are shown in Table 6. Each panel of the table shows the implied sibship size coefficient estimates by the birth order in the subsample. As the minimum sibship size increases, the subsample size decreases and the coefficients are estimated with, not surprisingly, less precision. Nevertheless the first two columns of Table 6 show that the 2SLS results with the subsamples are remarkably similar to the 2SLS results with the original sample—the effect of sibship size on education of the third-born individual is not statistically significant in any subsample; the estimated effects of sibship size on education of the first-born individual are always negative and often statistically significant at the 10% or smaller level; and sibship size has the greatest negative effect on education of the first-born individual. The consistency of the results of Table 4 with those of 6 suggests that the relatively high correlation between birth order and sibship size variables in the original sample should not be a major concern in interpreting the estimation results.

As another check of robustness of the results, we estimate the coefficients treating the family as the fixed effect. That is, we assume that the error term $e_i$ contains the family fixed effect and obtain the estimates of the coefficients using within-family variations only. Table 7 shows the fixed-effect estimation results for the key coefficients $\alpha_{sp2},...,\alpha_{spk}$ of the interaction terms of the model (3). If the estimated coefficients of the fixed-effect model differ substantially from those shown in Table 4, we should be concerned about the bias arising from unobserved family heterogeneity[9].

The results in Table 7 column (1) indicate that controlling for the family fixed effect, the effect of sibship size does not differ significantly between the first-born and the second-born individuals, while it differs significantly between the first-born and the third- or the later-born individuals. The same pattern is found in Table 4 column (1) which does not control for the fixed effect. In Table 7 column (2), the difference between the first-born and the second-born individuals is smaller than that between the first-born and the later-born individuals, but none of the coefficient is statistically significant at any conventional level. The results are comparable to those in Table 4 column (2). Columns (3) and (4) of Table 7 are also comparable to columns (3) and (4) of Table 4. All in all, the within-family estimates tell the same story as those in Table 4: the effect of sibship size on education is likely to be smaller for the third- or the later-born individuals than for the first- or the second-born individuals.

One may still question whether the 2SLS results are biased because a child's treatment status is correlated with education through other channels than its effect on sibship size. Joshi and Schultz (2007) indeed have found some differences between women living in the treatment villages and those living in the control villages in some characteristics besides fertility. For example, they found differences in their body weight and degree of participation in group activities (e.g. group savings). However, such maternal characteristics cannot explain why the treatment should have different effects on the child's education by birth order. Furthermore, although MCH-FP includes some health services for the treated population, we find little evidence that health status of the treated children is significantly different from that of the untreated children.

## 3. Consequence of Ignoring Differences of Sibship Size Effect across Birth Orders

The conventional model, for example that of Black et al. (2005), assumes that the effect of sibship size is equal regardless of birth order. At this time it is worth asking how different our estimation results would be if we maintained the assumption of equal effect. To answer the question, we have estimated the model without the interaction terms between sibship size and birth order.

Table 8 shows the estimation results of the key coefficients under the assumption of equal effect: panel (A) for school enrollment status and panel (B) for years of completed schooling. It shows that the 2SLS results are heavily affected by the predominant birth order in the sample. The younger individuals of the enrollment sample are predominantly of high birth orders. The 2SLS result in panel (A-II) is indeed comparable to the estimated sibship size coefficients for individuals of high birth orders. On the other hand, the older ones of the schooling sample are predominantly of low birth orders. The 2SLS result in panel (B-II) is shown to be somewhere between the coefficient for first-born individuals and that for second-born individuals. The 2SLS sibship size coefficients are hardly statistically significant.

The result suggests that the 2SLS results on the quantity-quality tradeoff using data from a

---

9 The coefficient of sibship size $\alpha_s$ is not estimated in the fixed-effect model since it is invariant within a family.

Table 8. Estimated Coefficients of the Sibship Size and the Birth Order Dummy Variables under the Assumption of Equal Effect

| | (A) Enrollment | | | | (B) Schooling | | | |
|---|---|---|---|---|---|---|---|---|
| | Coeff. | S.E. | Coeff. | S.E. | Coeff. | S.E. | Coeff. | S.E. |
| | (I) OLS | | (II) 2SLS | | (I) OLS | | (II) 2SLS | |
| Sibship size | −.0259 | .0048 | −.0200 | .0319 | −.1850 | .0739 | −.4353 | .3423 |
| Birth order=2 dummy | .0110 | .0197 | .0087 | .0224 | −.2893 | .2133 | −.1871 | .2536 |
| Birth order=3 dummy | .0707 | .0245 | .0657 | .0360 | −.5018 | .3621 | −.2711 | .4972 |
| Birth order=4 dummy | .1017 | .0277 | .0937 | .0502 | −.4308 | .4812 | −.0724 | .6496 |
| Birth order=5 dummy | .1478 | .0322 | .1357 | .0732 | −.3525 | .6201 | .1325 | .8424 |
| Birth order=6 dummy | .1340 | .0398 | .1176 | .0958 | −.3139 | 1.2383 | .2973 | 1.3445 |
| Birth order=7 dummy | .2116 | .0431 | .1908 | .1221 | −.8846 | .9725 | .0143 | 1.4713 |
| Birth order=8 dummy | .2320 | .0532 | .2073 | .1356 | − | − | − | − |
| Birth order=9 dummy | −.0240 | .1242 | −.0520 | .1818 | − | − | − | − |
| Birth order=10 dummy | −.1787 | .1893 | −.2127 | .2464 | − | − | − | − |
| Birth order=11 dummy | .5184 | .0591 | .4758 | .2385 | − | − | − | − |
| Birth order=12 dummy | .6922 | .0720 | .6485 | .2428 | − | − | − | − |
| *F*-value for birth order dummies | 13.29 (0.000) | | 7.17 (0.000) | | 0.55 (0.769) | | 0.42 (0.864) | |
| Observations | 3690 [1953; 1737] | | | | 1463 [722; 741] | | | |
| R-squared | 0.4088 | | 0.4085 | | 0.3580 | | 0.3480 | |

*Note:* Standard errors are robust standard errors computed with households as clustering units. Estimation results of the other variables are not shown.

low-income country such as Bangladesh are likely to be sensitive to composition of individuals in the sample. We think that it explains, at least in part, why the evidence provided by the previous studies on the quantity-quality tradeoff in Matlab, Bangladesh (Foster and Roy, 1997; Sinha, 2005; Joshi and Schultz, 2007) has been mixed. The assumption of equal effect across birth orders should be questioned in low income countries.

Table 8 provides another evidence of the effect of birth order on education, controlling for the sibship size. The results in the panel (A) indicate that, controlling for the sibship size, age, and other factors, the probability of school enrollment increases with birth order. Except for a couple of statistically insignificant cases (ninth and tenth birth order) the increase of the school enrollment probability by birth order is largely monotonic in both the OLS and the 2SLS results. In both OLS and 2SLS results the coefficients are jointly significant at the 1% level. Although not shown here, the results for the subsamples of males and females are similar. Note that the overall estimation results of the birth order dummies in the panel (A) are consistent to the prediction of the model in section III.

We cannot claim the same, however, for the results in panel (B). The OLS coefficients of the birth order dummies are negative and their sizes have no clear pattern by birth order. Although the 2SLS results hint at some possibility that the schooling increases with birth order, the estimates are too imprecise for any definite conclusion. They are not jointly significant at any conventional level.

As discussed previously, this seems to indicate that school dropouts are temporary for some individuals, especially among those of low birth orders. Some individuals of low birth orders may drop out of school and work in case of economic difficulty, but they may go back to school once the economic situation improves.

The finding in panel (A) that birth order has a positive effect on school enrollment of children in Bangladesh, controlling for sibship size, is consistent with the findings for several other developing countries. For example, Ejrnæs and Pörtner (2004) have found the same for children in Philippines, and Emerson and Souza (2008) for children in Brazil. Dayioğlu et al. (2009), on the other hand, have found that the birth order effect is U-shaped for children in urban Turkey. These findings in developing countries, including ours, are inconsistent with the finding of Black et al. (2005) that birth order has a negative effect on education of children in Norway, a developed country. In some developing countries the cost of foregone earnings seems to be the predominant factor that affects parental decision on school enrollment of the children.

## VI. *Conclusion*

In this paper, we provide evidence that the effect of sibship size on education of individuals differs by the child's birth order in a low-income country. Our 2SLS results, instrumenting sibship size by exposure to a randomized family planning program in Bangladesh, indicate that an increase in sibship size has significant negative effects on education of first-born and second-born individuals, but insignificant effects on education of later-born individuals. We also find evidence that 2SLS results under the assumption that sibship size effect is equal regardless of birth order are sensitive to the composition of the sample.

Our point estimates of the sibship size effect suggest that addition of one sibling decreases the probability of school enrollment of the first- and the second-born children by about 10 percentage points and completed education of the first-born children by about one full year in Matlab, Bangladesh. In addition, we have found evidence that an increase in sibship size prompts the first-born and the second-born children to participate in the labor market. However, as for later-born children, the sibship size effect on enrollment, schooling and labor force participation is found to be smaller and not statistically significant. These results remain robust even when we use other estimation strategies that are likely to lessen the potential identification problem due to correlation between sibship size and birth order and the potential bias due to unobserved family heterogeneity.

The finding of this study suggests that in a low-income country like Bangladesh, reducing fertility rate is likely to increase parental investment in education of the first- or the second-born children, but unlikely to make much difference to later-born children's education.

## *References*

Angrist, J., V. Lavy, and A. Schlosser (2010), "Multiple Experiments for the Causal Link between the Quantity and Quality of Children," *Journal of Labor Economics* 28 (4), pp.773-824.

Becker, G. S. and H. G. Lewis (1973), "On the Interaction between the Quantity and Quality of Children," *Journal of Political Economy* 81(2), S279-S288.

Becker, G. S. and N. Tomes (1976), "Child Endowments and the Quantity and Quality of Children," *Journal of Political Economy* 84(4), S143-S162.

Black, S. E., P. J. Devereux, and K. G. Salvanes (2005), "The More the Merrier? The Effect of Family Size and Birth Order on Children's Education," *Quarterly Journal of Economics* 120(2), pp.669-700.

Conley, D. and R. Glauber (2006), "Parental Educational Investment and Children's Academic Risk: Estimates of the Impact of Sibship Size and Birth Order from Exogenous Variation in Fertility," *Journal of Human Resources* 41(4), pp.722-737.

Dayioğlu, M., M. G. Kirdar, and A. Tansel (2009), "Impact of Sibship Size, Birth Order and Sex Composition on School Enrolment in Urban Turkey," *Oxford Bulletin of Economics and Statistics* 71, pp.399-426.

Ejrnæs, M. and C. C. Pörtner (2004), "Birth Order and the Intrahousehold Allocation of Time and Education," *Review of Economics and Statistics* 86(4), pp.1008-1019.

Emerson, P. M. and A. P. Souza (2008), "Birth Order, Child Labor and School Attendance in Brazil," *World Development* 36(9), pp.1647-1664.

Foster, A. D. and N. Roy (1997, April), "The Dynamics of Education and Fertility: Evidence from a Family Planning Experiment," Mimeograhed, University of Pennsylvania.

Hall, A. R., G. D. Rudebusch, and D. W. Wilcox (1996), "Judging Instrument Relevance in Instrumental Variables Estimation," *International Economic Review* 37(2), pp.283-298.

Joshi, S. and T. P. Schultz (2007, February), "Family Planning as an Investment in Development: Evaluation of a Program's Consequences in Matlab, Bangladesh," Economics Growth Center Discussion Paper No. 951, Yale University.

Lee, J. (2008), "Sibling Size and Investment in Children's Education: An Asian Instrument," *Journal of Population Economics* 21(4), pp.855-875.

Mostafa, G., M. K. Shaikh, J. K. van Ginneken, and A. M. Sarder (1998), *Demographic Surveillance System-Matlab: Registration of Demographic Events 1996*, Volume 28. Dhaka, Bangladesh: ICCDR,B: Center for Health and Population Research.

Qian, N. (2008, March), "Quantity-quality and the One Child Policy: The Only-child Disadvantage on School Enrollment in Rural China," Memeographed, Brown University.

Rahman, O., J. Menken, A. Foster, C. E. Peterson, M. N. Khan, R. Kuhn, and P. Gertler (1999), "Matlab [Bangladesh] Health and Socioeconomic Survey (MHSS), 1996: Overview and User's Guide," Mimeographed. RAND, CA, USA.

Rosenzweig, M. R. and K. I. Wolpin (1980), "Testing the Quantity-Quality Fertility Model: The Use of Twins as a Natural Experiment," *Econometrica* 48, pp.227-240.

Rosenzweig, M. R. and J. Zhang (2009), "Do Population Control Policies Induce More Human Capital Investment? Twins, Birthweight, and China's 'One Child' policy," *Review of Economic Studies* 76(3), pp.1149-1174.

Schultz, T. P. (2007), "Population Policies, Fertility, Women's Human Capital, and Child Quality.In T. P.Schultz and J. A. Strauss (Eds.)," *Handbook of Development Economics*, Volume 4, Chapter 52, Elsevier B. V.

Schultz, T. P. (2009), "How Does Family Planning Promote Development?: Evidence from a Social Experiment in Matlab, Bangladesh, 1977-1996," Mimeographed, Yale University.

Sinha, N. (2005), "Fertility, Child Work, and Schooling Consequences of Family Planning Programs: Evidence from an Experiment in Rural Bangladesh," *Economic Development and Cultural Change* 54(1), pp.97-128.

Stock, J. H. and M. Yogo (2002), "Testing for Weak Instruments in Linear IV Regression," Technical report, NBER Technical Working Paper 284.