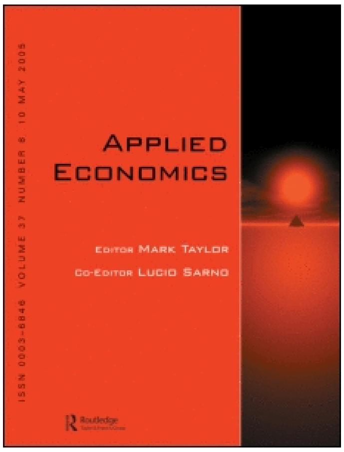

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Intra- and inter-household externalities in children's schooling: evidence from rural residential neighbourhoods in Bangladesh

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Intra- and inter-household externalities in children's schooling: evidence from rural residential neighbourhoods in Bangladesh

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This article tests for neighbourhood effects on children's schooling, using unique data on rural residential neighbourhoods from Bangladesh. We find that school completion of children is positively and significantly affected by the mean grade completion of other children in the neighbourhood. We then present three pieces of evidence that suggest that the social effect offers a valid explanation. Firstly, the evidence we find of inter-household externalities is not driven out by control for a host of neighbourood and household attributes. Secondly, the result remains robust to neighbourhood composition effects: it is unchanged as we purge our main sample of the households within the neighbourhood that are potentially linked in terms of their recent history of partition. Thirdly, a similar peer effect is found for adults who completed schooling before the introduction of existing educational reforms in rural areas suggesting that the observed effect of growing up in educated neighbourhood does not merely capture the influence of common exposure to various government educational interventions. As a by-product, the article also provides evidence of intra-household externality in children's schooling, net of neighbourood externalities. We conclude by discussing the implication of these findings for education policy design.

I. Introduction

The importance of the family in the process of human capital formation is well recognized in development literature. However, families in poorer countries could be in a much weaker position to aid children's schooling through the provision of a supportive learning environment at home. In developing countries, school quality is low, adult illiteracy is very high and households investment in children are often credit constrained. The social environment, in such a setting, can help relax some constraints via proximity to educated individuals in the residential neighbourhood.[1] If the social environment is not perfectly correlated with household wealth, removing the credit constraints can only partly make up for differences in educational outcomes: children from dysfunctional neighbourhoods, with a poor social environment, may continue to demand/acquire less education. Hence, a greater understanding of the social process within the neighbourhood that mediates schooling should form an important consideration. However, the impact of social effects on schooling, arising via interactions with neighbours in the social space, is under-researched for developing countries.[2] The objective of this article is to test for social externalities in human capital production in a developing country, with low average level of schooling, Bangladesh. We examine the extent to which an increase in schooling of adults and other children in the residential neighbourhood affects children's school completion in rural Bangladesh. We find significant evidence of inter-household externalities: children's education benefits positively from the mean education of their neighbouring peers. In addition to neighbourhood externalities, we also find evidence of intra-household education externalities, net of neighbourhood effects: children of uneducated parents have more schooling when co-residing with educated adults in the household.

There are three pieces of evidence that suggest the social effect as the principal explanation for inter-household effects. First, our evidence of peer effect is not driven out by control for a host of attributes such as income, landholding, and parental background at the neighbourhood and household levels. Furthermore, the results remain robust to neighbourhood composition: it is unchanged as we purge our main sample of households within the neighbourhood that are potentially linked in terms of their recent history of partition. Finally, peer effect in our data could be owing to common exposure to educational reforms introduced by the government in the study area. Yet a similar effect is found for adults who completed schooling before the introduction of the reforms. Our analysis of the long-term impact of schooling of neighbourhood peers bears out our claim of inter-household externality in school completion. This is investigated in a model of decision to participate in wage work for a sample of adults, treating school completion as an endogenous covariate. We find that the mean schooling of neighbouring children during an individual's school years increases the likelihood of wage work participation in adulthood, via boosting school completion: the education of neighbours during the childhood serves as instrument for own school completion in our wage participation model.

In Bangladesh, educational investment is widely considered as an important strategy to reduce poverty (Wodon, 2000; Asadullah, 2006b). According to recent research, educational investment not only leads to higher wage earnings in the labour market (Asadullah, 2006a), it also boosts agricultural production in rural Bangladesh (Asadullah and Rahman, 2009). Therefore, increasing the level of school completion remains an important development goal for the government. To this end, a sizeable economics literature on school participation and attainment of children in Bangladesh is available (e.g. Asadullah, 2005, 2008; Asadullah *et al*., 2007). Whilst such literature highlights various demand and supply side determinants of children's schooling choices, none of the studies explores the role of externalities (beyond parental education) in children's schooling. Hence, our study hopes to fill an important gap in the literature.

The rest of the article is organized as follows. Section II discusses the empirical strategy and analytical framework in detail and revisits the issue of identification of social effects in the context of demand for schooling. Section III discusses the data. Section IV provides a discussion of the main findings. Section V presents the conclusion.

II. Analytical Framework: Neighbourhoods and Educational Production

A primary and perhaps the most relevant social space in a rural context is defined in terms of proximity of individuals by their place of residence. A common measure of neighbourhood (defined as such) in rural Bangladesh is '*bari*' i.e. a cluster of households. These household clusters are widely observed all over rural Bangladesh, where individuals usually enjoying some form of kinship set up households next to each other around the common yard. A typical *bari* consists of two parts: *bhitor bari* (i.e. in-house) and *bahir bari* (i.e. out-house). *Bhitor-bari* houses kitchen and residential dorm. Nonrelatives visiting the *bari* are only allowed in *bahir bari*. The out-house consists of

[1] Sociologists have discussed a variety of ways through which educational and socio-economic background of constituent members in the social space benefits children's schooling outcomes. For a review, see Mayer and Jencks (1989).
[2] An exception is Weir (2007), which addresses some aspects of the issue using household survey data from rural Ethiopia.

a visitor room, otherwise known as *kachari ghar*, which is commonly located next to the common pond (i.e. outer pond) is meant for common usage by all *bari* males and visitors. Among other significant features, *bari* members usually cooperate with each other in times of crisis. Depending on the nature of the underlying kin relations, *baris* share among themselves various resources. The common resources include pond(s) of the *bari*, outhouse for guests, in–out approach road(s) of the *bari*, graveyard, at times prayer room/mosque attached to the *bari*, etc.

The literature on neighbourhood effects speculates about various routes via which externalities within the *bari* can be expected to increase school completion. According to Manski (1993, 2000), externalities could take the form of an endogenous social effect (i.e. children's education in the *bari* being influenced by the mean schooling of other children in the *bari*) and/or a contextual effect (i.e. children's education in the *bari* being affected by the mean characteristics of children and/or their parents in *bari*). While sociological studies speculate about the various processes that could potentially generate social effects in a neighbourhood, most of these processes can be explained in terms of preference and constraint of individuals (Manski, 2000).

A correlation between a child's schooling and education of neighbouring peers, namely an endogenous social effect, could arise when greater school completion among children relaxes some constraints in educational production for the index child. Such an interaction is facilitated by *lessons swapping* among peers in the *bari*. When school attainment is, *inter alia*, reliant on home assignments, children in proximate grades may collude in preparation of such assignments.[3] The burden of home assignments is usually progressive across grades so that at higher grades, benefits of cooperation are larger, particularly among children of credit-constrained families. Hence, higher participation of older or same aged children in relevant grades (predicted by their age) enlarges the pool of 'school-success-specific' knowledge within the *bari*, leading to positive constraint interactions. Alternatively, elder children in higher grades could share their accumulated knowledge acting as mentors or simply pass on their notes and other tangible school inputs to children enrolled in immediately lower grades.

Similarly, a contextual social effect arises when children's interactions with educated adults in the neighbourhood relax some constraints underlying the educational production function. For example, neighbouring educated mothers in the *bari* could substitute for own mother's education, particularly when one's own mother has little or no education. Given the distinction between proximate uneducated and isolated uneducated in society, what matters in children's education is not the *actual* but rather the *effective* level of education of own mothers, defined in terms of mother's proximity to other educated adults within the household and outside, in neighbouring households (Basu and Foster, 1998).[4]

To summarize, all the above possibilities imply that whilst studying educational outcomes in developing countries, one needs to expand the educational production function by accounting for the socio-economic mix of the children in their residential neighbourhood. This point is emphasized throughout the article. We are interested in assessing whether school completion of a child is affected by that of other children and adults in the reference population, i.e. the *bari*. The econometric framework for estimating the production function accounting for neighbourhood characteristics is discussed in the next section.

Empirical strategy: testing for education spillover

We employ a simple linear regression model for years of school completion where individual schooling varies linearly with the mean outcomes in the group, with mean exogenous attributes of the *bari*, and with other personal attributes that may be common across all group members. Such a model is specified below:

$$S_{bi} = \beta E_b[S_{bi}] + \gamma E_b[x_{bi}] + \delta x_{bi} + \alpha_b + \varepsilon_{bi} \qquad (1)$$

where S_{bi} is grade attainment and $E_b[S_{bi}]$ is the corresponding group level (i.e. the mean schooling of children in b-th *bari*) analogue. x_{bi} proxies for individual-specific characteristics (e.g. gender, religion, parental education etc.) of i-th child in b-th *bari*. $E_b[x_{bi}]$ proxies for the mean characteristics (e.g. mean education of adult male/female in the *bari*) of b-th *bari* members.[5] α_b is unobserved heterogeneity common across children within the b-th *bari* and ε_{bi} is a random error term.

[3] However, children enrolled in the same grade in a school may also compete and decide not to cooperate.

[4] The framework is extendable beyond household, to neighbourhood and can be disaggregated by characteristics of the literate household member such as gender and age (Basu and Foster, 1998).

[5] In the calculation of *bari* averages we take out the index child and all his/her other family members. Hence, despite being calculated at the *bari* level, for each child $E_b[S_{bi}]$ and $E_b[x_{bi}]$ vary across households within the *bari*.

In Equation 1, the social effect is captured altogether by $E_b[S_{bi}]$ and $E_b[x_{bi}]$. In Manski's (1993) terminology, a significant β is a measure of endogenous social effect. On the other hand, the contextual social effect is captured by the coefficient on $E_b[x_{bi}]$. Given that x_{bi} are pre-determined, contextual effects are exogenous in educational production. Furthermore, *baris* are often formed on the basis of kinship. In all such cases, α_b is significant: children of immediate kins residing in the same *bari* are likely to share or inherit common characteristics. Poor control of α_b or the 'correlated effect' (in Manski's term) leads to nonsocial effects, the presence of which biases the estimated endogenous and contextual effects.

However, even if no correlated effects are present in the data, β is not identified in a simple linear regression (unless additional assumptions are made), due to what Manski refers to as the *reflection problem*. Data on outcomes do not reveal whether group behaviour actually affects individual behaviour or whether group behaviour is simply an aggregation of individual behaviour. This suggests that the endogenous effect cannot be separated from various contextual effects. One solution to identify β is obtained by adopting a dynamic model using retrospective data on group variables. If there is a lag in the transmission of the social effect (i.e. children learn from the educational experiences of their preceding reference group), then one can include the lagged group mean schooling as a regressor in addition to the education of their own cohorts. In the case of the former, nonsocial factors remain contemporaneous while the social effect acts on the individual with a lag. Apart from yielding a crude identification strategy, this also bypasses Manski's *reflection problem* if the effect of 'education of own cohort' is absent: contextual effects become separable from endogenous effect. The last point becomes evident as we re-formulate our structural equation by including education of an older cohort of peers in Equation 1:

$$S_{bi} = \beta E_b[S_{bi}] + \theta E_b\left[S_{b(i+j)}\right] + \gamma E_{bh}[x_{bi}] + \delta x_{bi} + \alpha_b + \varepsilon_{bi} \quad (2)$$

here, $E_b[S_{bi}]$ is the mean peer schooling of own cohort defined over peers in the *bari* who are of same age or at most 2 years younger/older than the index child. $E_b[S_{b(i+j)}]$ refers to the mean peer schooling of the older cohort. The older cohort consists of those children who are 3–5 years older than the index child (hence j ranges from 3 to 5). If the 'own cohort' effect is absent (i.e. $\beta = 0$), Equation 2 reduces to the following:

$$S_{bi} = \theta E_b\left[S_{b(i+j)}\right] + \gamma E_{bh}[x_{bi}] + \delta x_{bi} + \alpha_b + \varepsilon_{bi} \quad (3)$$

Assuming no correlated effects, the effect of endogenous interactions due to education of an older cohort is then separable from other contextual interaction effects in Equation 3 on the ground that mean education of older cohort is invariant to the education of the index child. However, the omission of the 'own cohort' effect leads to an omitted variable type of bias so that Equation 3 remains the preferred specification (over Equation 1). This specification distinguishes between endogenous effects arising from own and older cohorts of peers in the *bari*. Resulting regression estimates of social effects therefore should be taken as evidence of association without implying any strict causal relation.

The structural Equation 2 describes endogenous social effects in terms of mean schooling of peers. Such linear model of social effects, however, is too simple to fully unpack the processes underlying a social effect. To better explore the 'black-box of social interactions', we expand Equation 2 by including additional variables on the right hand side so as to distinguish between different types of constraint interactions. On specific *bari* characteristics, in addition to 'mean schooling' of peers of own and lagged cohort in the *bari*, we include two additional regressors to distinguish between various underlying mechanisms via which social interaction effects arise; these are discussed below.

Birth order within *bari*. Children may benefit from interactions with other children in higher grades via direct mentoring or through inheritance of school study materials (and school resources such as textbooks). Given the prospect of such an externality arising from the presence of *bari* children in sufficiently higher grades, the relative ranking of a child in the grade distribution of his peers should be taken into account. As an exogenous proxy for this, we use 'birth order within *bari*' of the index child. Excluding own siblings, this variable gives the birth order of a child among all the *bari* children (aged 6–17 years).

Maximum grade among peers. Control for maximum grade attainment among peers allows separating the effect of role models by older children in higher grades. Hence, we expect a positive effect of this variable.

Turning to contextual effects arising from adult education in the *bari*, the most prominent is that of the effect of neighbouring mothers as home tutors. To test this effect of *bari* mothers' education, we include two dummies, each identifying the presence of at least one: (i) grade 1–4 educated mother in *bari* and (ii) grade 5+ educated mother in *bari*.

The distinction between mothers and nonmothers is because educated nonmothers may respond differently compared to those who are mothers of school-aged children. The focus on mothers as a source of externality is consistent with the literature on social benefits of female education. Basu and Foster (1998) also point out that at the margin, the external effects of education/literacy are likely to be larger if the source of the externality is a female rather than a male. In our model, to further purge the effects of mother-specific variables from that of other educated adults in the *bari*, we additionally control for the presence of at least one nonmother with (i) 1–4 years of schooling, (ii) 5 or more years of schooling and (iii) mean schooling of adult males in the *bari*. This allows us to net out the contribution of nonmothers and adult males in the *bari*.

Given the close link between households and residential neighbourhood (i.e. *bari*), the impact of intra- and inter-household externalities should be studied together. Hence, in addition to inter-household externality, we also look at *intra-household externality*. We interact own mother's education dummies (i.e. mother with no education and mother with grade 1–4 equivalent education) with a dummy that identifies whether there is at least one primary school educated adult in the household. A significant positive coefficient on the two resulting interaction terms would imply that children of less-educated but proximate mothers (i.e. those with co-resident educated nonspouse adult householders) have more schooling relative to that of similarly educated but isolated mothers. Our simultaneous focus on intra- and inter-household externalities is thus likely to yield better estimates of these two types of externalities to the extent that common *bari* environment is driven by common family background effects.[6]

Additional tests of education spillovers

Despite the proposed regression specification, our empirical strategy is likely to be fraught with the problem of 'correlated effects', arising due to the composition of the neighbourhood and omission of common correlates of schooling. First, children in the neighbourhood may have similar schooling outcomes if they belong to equally well-resourced households and household income remains poorly measured. Second, inter-household correlation in schooling could capture the possibility that some of the peers are related by blood i.e. a sub-sample of households in the neighbourhood comprise extended families. Below, we outline two tests to assess the robustness of our findings to these issues. An additional test of the educational spillover is also discussed.

Accounting for inter-household ties. The composition of neighbourhood in our analysis poses two challenges. The *bari*, our measure of neighbourhood, consists of households whose heads are usually related by blood or affinal connections. The inter-household relations inside the *bari* are either defined by immediate kinship or by heterogeneous kinship among constituent households. Following this, the observed correlation of schooling among *bari* members may potentially capture nothing but inter-household ties. For instance, couples living in close proximity (i.e. in the same *bari*) to their parents may be enjoying all the benefits of joint residence in a single household headed by their parents.[7]

To this end, we create a sample (of 'nonlinked' households) that consists of households, which are not related to each other in the same *bari* in terms of their recent history of household partition. In the other sample (i.e. 'linked' household sample), heads of households currently in the same *bari* co-resided in the same household in the recent past and were therefore likely to have retained close economic ties. We then split our sample observations between 'linked' and 'nonlinked' sample, and estimate the regression models separately to observe whether the externality effect differs across inter-household kin ties. In particular, we test whether evidence of social effect persists once we purge our main sample off the linked households. The exact sample construction rule is discussed in detail later on in the article.

Unaccounted income effects. We test whether externality (owing to one's proximity to educated nonspouse adults within and outside the household) is capturing an unobserved wealth effect or operates via enhancing household income. We do this via additionally controlling for per capita expenditure whilst estimating an instrumental variable model of

[6] Support for such a specification also comes from Ginther *et al.* (2000) who find that the coefficients on neighbourhood variables tend to fall in value and lose statistical significance as the specification of family variables become more complete.
[7] Apart from the issue of resource pooling among 'linked' households, separation of linked households is important for another reason. Some education may have been provided during the period when the linked household members were still co-resident. Even if it is the case that an index child is enrolled in school only in post-partition period, correlation with schooling of children in other households today may simply capture the common home learning environment shared in their pre-school years. Since most of the children (aged 6–17 years) were not born in 1982, this, however, is less of an issue to the extent most of the schooling occurred soon after 1982.

grade completion treating expenditure as an endogenous regressor. This also improves our estimates of inter-household externalities to the extent that incomplete control for income creates an omitted variable bias.

Alternative test of inter-household externality effects. If *bari* spillover effects experienced during one's childhood are genuine, we would expect children from better educated *baris* to perform better in the labour market, than those from educationally depressed *baris*. Therefore, testing for labour market success of children who grew up in *baris* with higher schooling would suffice as an indirect and additional test of inter-household externality within the *bari*. To this end, we exploit the existence of retrospective data on *bari* schooling (or social environment) of individuals currently in the labour market. We use this data to construct instruments for schooling attainment, which is modelled as an endogenous determinant of wage work participation.

For our model of wage work participation, we estimate an IV-probit regression where the dependent variable takes the value one for wage work and is zero otherwise. The IV-probit model is based on Amemiya's Generalized Least Squares (AGLS) estimators for probit with endogenous regressors and is more efficient than the two-stage conditional likelihood model (2SCML) proposed by Rivers and Voung (1988). The endogenous regressors are treated as linear functions of the instruments and the other exogenous variables and corrected SE are reported.

Apart from the endogenous schooling variable, the regressions control for individual and household characteristics. We use the following lagged variables as instruments for own education variable: (i) mean peer schooling of boys in the own cohort, (ii) mean peer schooling of girls in the own cohort, (iii) mean peer schooling of older cohort and (iv) mean schooling of adults (i.e. individuals aged 18–65 year olds) in the *bari*. If the retrospective *bari* variables turn out to be (jointly) significant as excluded instruments for 'years of school completion' in our model, we can claim to have evidence of social effects arising from one's residence in better-educated *baris*. This test is important for an additional reason. Evidence of peer effect in children's education could capture correlated effects in the form of common exposure to educational interventions introduced during 1990–1996. However, individuals who are currently in the labour market attended school before such reforms were introduced. Hence, significance of peer variables (i.e. excluded instruments) in the first-stage regression for this cohort would serve as a partial test of the hypothesis of 'correlated effect' related to common participation in educational intervention programs.

III. Data Description

This research combines data on residential neighbourhoods from two independent censuses and one sample survey all conducted in the Matlab Thana, located in South-East of Dhaka, the capital of Bangladesh. Most of the data primarily came from the The Matlab Socio-Economic Census (henceforth MSEC) 1996, conducted by the International Centre for Diarrhoeal Disease Research, Bangladesh (ICDDR, B), covered a population of 25 000 households in 141 villages in the Matlab Thana. Census records are preserved by the Centre in the form of a Demographic Surveillance System (DSS). Our choice of a random sample of *baris* from the census database has been guided by the availability of data from an independent survey on the same population for the year 1996, as well as an earlier census for the year 1982. These include the Matlab Health and Socio-Economic Survey (MHSS) 1996 and the MSEC 1982. Conditioning the primary sample selection on the MHSS data enables us to complement our analysis of MSEC data with detailed individual, household and community level information available from the MHSS.

Individuals in the censuses and the survey population can be linked at the individual, household and *bari* level using unique *bari*, person and household identification number. We therefore merged information on sample individuals from the three sources and created three unique samples to estimate our empirical models as discussed in the earlier section. The first two consist of all children of school age whereas the third sample comprises a sample of adults in the *bari* for whom we have retrospective information on the characteristics of their childhood neighbours. The exact rules for construction of these samples along with a description of the characteristics of the resulting samples are discussed below.

Construction of the working sample

Our *primary sample* consists of individuals drawn from the MSEC 1996 population. Since the MSEC data could be used along with the MHSS, we have extracted the MSEC data from the DSS database as follows. The MHSS surveyed a random sample of 2687 *baris* in the Matlab Thana area, treating the *bari* as the Primary Sampling Unit (PSU).[8] We have therefore selected these *baris* to form our random sample. Next, records on all household and individuals in the sample *baris* were extracted. This yielded data on 79 094 individuals residing in 14 869 households. A total of 527 *baris* (or 19.61% of all *baris*) comprise single households. The rest consists of two or more households. On average, there are nine households in a *bari*. The size of the *bari* can also be judged in terms of number of resident children of school age. On average, the number of (6–17 years old) children per *bari* is 16.3 with a range of 1–143 and median of 13. The summary statistics of the MSEC sample data are presented in Table A1.

A *second sample* is constructed by combining the MSEC 1996 data with that of the MHSS. The MSEC 1996 does not provide data on expenditure or income. However, such data is available from the MHSS. Taking advantage of the overlap in the survey population and timing, we merge the MHSS sample households with the MSEC 1996 records. Given that the MHSS sampled only two households per *bari*, the resulting sample is much smaller in size: the MSEC–MHSS-linked sample contains 4993 children in 2156 male-headed households in 1494 *baris*.

The *third sample* for our analysis is constructed by linking data on individuals aged 20–32 years in MSEC 1996 with their neighbourhood records in MSEC 1982, preserved in the DSS database.[9] Unique identification numbers were used from the DSS database to link individuals and households across the two censuses. This yielded a sample of 9749 individuals for whom we have data on their occupational status and socio-economic profile of their residential neighbourhoods.

IV. Results and Discussion

Table 1 presents OLS estimates of the determinants of grade completion using the MSEC 1996 data. The regressions are estimated over a sample of children aged 6–17 years.[10] The regressors are age, sex, religion and birth order of the child; characteristics of the household head (e.g. age, sex, education, occupation); education and age of the head's spouse and household landholdings (homestead and cultivable).[11] We also control for various *bari* characteristics such as landholdings (homestead and cultivable), total number of children aged 0–5 years and 6–17 years and total number of adults (aged 18+ years) in the *bari*. It may be recalled that the *bari* level variables discard all individuals in the index household for whom the variable is being calculated. Hence, all the *bari* variables reflect the neighbourhood effect net of own/household effect.

All regressions include village 'Fixed Effects' (FEs) which control for village level variation in school quality. Regression results in Table 1 are reported separately for full, male and female samples. For each sample, specification (1) refers to Male-Headed Households (MHHs) whereas specification (2) corresponds to Female-Headed Households (FHHs).[12] Only findings specific to our variables of interest are discussed below in detail. In the light of this, we first discuss the findings on *bari*-related externalities, which are then followed by the results on within-household externality.

Main results

Birth order in the *bari*. This variable takes the value one for the eldest child in the *bari*. We find a significant positive coefficient of *birth order* for all samples, indicating that younger *bari* children tend to achieve higher grade completion. Although defined

[8] First, one household was drawn from each of the *bari*s that consisted of a single household. A total of two households were sampled from the remaining *bari*s, each of which had more than one household. This led to data on a total of 24 266 individuals in 4364 households. Detailed information on the MHSS 1996 is available in Rahman *et al.* (2001).

[9] We focus on this age group to ensure that all individuals in our 1996 sample were of school age. However, focusing on this age group implies that our dependent variable, grade completion, is left censored. The resulting estimates of the education spillover effects (using censored data) are likely to be biased downward; without accounting for censoring in the data would yield a lower bound of the true spillover effects.

[10] Given this age range, censoring of the dependent variable remains an issue, either due to nonenrollment or current enrollment (so that last grade completed is yet to be observed). To correct for potential bias due to nonenrollment in school, one may estimate a sample selection model. However, in the absence of convincing exclusion restriction, we have not pursued this route. Hence, our results need to be interpreted with a degree of caution.

[11] Controlling for mother/head's spouse age is important because older mothers are likely to have greater experience as a child carer or home tutor.

[12] The separation of the full sample data by gender of the head is unavoidable because in almost 95% of cases, female-heads do not have their spouse present in data (either because they are divorced or the male partner is located outside the study area). Splitting sample observations by gender is important because there may be gender differences in the ability to benefit from social interactions.

Table 1. OLS estimates of determinants of grade completion (children aged 6–17 years)

	Full sample		Boys		Girls	
	MHHs	FHHs	MHHs	FHHs	MHHs	FHHs
Age	0.243 (9.57)**	0.391 (6.02)**	0.290 (7.75)**	0.502 (5.10)**	0.179 (5.21)**	0.267 (3.05)**
(Age squared)/100	0.596 (5.54)**	0.107 (0.39)	0.364 (2.31)*	−0.341 (0.84)	0.905 (6.21)**	0.603 (1.63)
Female	0.020 (0.92)	0.063 (1.14)				
Hindu	0.108 (2.18)*	−0.140 (0.88)	0.286 (3.91)**	0.109 (0.47)	−0.054 (0.81)	−0.450 (2.05)*
Birth order in family	−0.167 (12.61)**	−0.199 (5.16)**	−0.158 (8.10)**	−0.152 (2.55)*	−0.184 (10.38)**	−0.240 (4.72)**
Education of household-head	0.062 (14.18)**		0.069 (10.64)**		0.055 (9.35)**	
Head's spouse has no education	−0.813 (19.07)**		−0.792 (12.68)**		−0.826 (14.34)**	
Head's spouse has grade 1–4 education	−0.340 (5.62)**		−0.339 (3.76)**		−0.355 (4.41)**	
Head's spouse has no education and proximate	0.362 (11.01)**		0.329 (6.81)**		0.394 (8.86)**	
Head's spouse has grade 1–4 education and proximate	−0.026 (0.41)		0.006 (0.06)		−0.027 (0.31)	
Head has no education		−1.184 (14.55)**		−1.249 (10.16)**		−1.084 (9.91)**
Head completed grade 1–4		−0.564 (5.22)**		−0.607 (3.56)**		−0.530 (3.75)**
Head has no education and proximate		0.670 (7.80)**		0.880 (6.85)**		0.499 (4.28)**
Head has 1–4 grade and proximate		0.249 (1.55)		−0.059 (0.24)		0.513 (2.41)*
Birth order in *bari*	0.030 (12.96)**	0.027 (3.48)**	0.021 (6.35)**	0.027 (2.21)*	0.039 (12.47)**	0.029 (2.75)**
Mean peer schooling, girls of own cohort	0.118 (15.60)**	0.105 (5.54)**	0.100 (9.16)**	0.087 (3.07)**	0.134 (12.97)**	0.143 (5.45)**
Mean peer schooling, boys of own cohort	0.109 (14.15)**	0.053 (2.67)**	0.107 (9.56)**	0.048 (1.66)***	0.112 (10.58)**	0.047 (1.67)***
Mean peer schooling, girls of older cohort	0.050 (7.66)**	0.052 (3.18)**	0.038 (4.02)**	0.043 (1.72)***	0.061 (6.94)**	0.058 (2.62)**
Mean peer schooling, boys of older cohort	0.041 (6.73)**	0.056 (3.56)**	0.039 (4.36)**	0.060 (2.58)*	0.045 (5.46)**	0.043 (1.98)*
Maximum schooling among peers	−0.014 (1.45)	−0.013 (0.47)	−0.026 (1.77)***	−0.001 (0.02)	−0.003 (0.25)	−0.034 (0.92)
Mean schooling of male adults in *bari*	0.035 (5.25)**	0.056 (3.19)**	0.038 (3.96)**	0.013 (0.48)	0.028 (3.16)**	0.096 (4.12)**
Presence of (grade 1–4) educated mother in *bari*	0.026 (1.02)	0.108 (1.62)	0.024 (0.63)	0.120 (1.22)	0.038 (1.10)	0.119 (1.31)
Primary educated mother in *bari*	−0.023 (0.85)	0.001 (0.01)	−0.057 (1.42)	0.107 (1.02)	0.007 (0.19)	−0.066 (0.66)
Primary educated adult female in *bari*	−0.037 (1.14)	0.117 (1.27)	−0.005 (0.10)	0.148 (1.06)	−0.064 (1.46)	0.106 (0.85)
Grade 1–4 educated female adult in *bari*	−0.006 (0.16)	−0.087 (0.87)	0.004 (0.07)	−0.206 (1.34)	−0.008 (0.17)	−0.023 (0.17)
Adjusted R^2	0.60	0.58	0.57	0.54	0.64	0.65
Village FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	18 906	3464	9701	1794	9205	1670

Notes: Absolute value of *t*-statistics in parentheses. The regression constant is suppressed. Regressions also control for total number of children aged 0–5 years and 6–17 years; total number of adults in the *bari*; head's occupation; mother's (head's spouse's) age, religion; dummies to control for missing data on mean peer schooling variables (Since neighbourhood variables are missing for single-household *baris*, inclusion of such dummy variable essentially controls for single-household *baris* in our sample); head's spouse and age of *bari* children's education; landholding of the index household and other households in *bari*.

*, ** and *** denotes significance at 5, 1 and 10% levels, respectively.

with respect to other children in the *bari*, *birth order within bari* potentially varies for every child within the family. Hence, we also estimated an alternative specification by fully controlling for (i) household-level FEs and (ii) *bari*-level FEs (results not reported). In both cases, the coefficient remains highly significant and positive. Its inclusion somewhat reduces the effect of birth order within the family indicating that part of the disadvantage of being the youngest in the family is offset by the advantage of being younger among *bari* peers. To the extent that the potential role model effect is captured by the maximum grade of *bari* children in our model, the positive effect of *birth order within the bari* is attributable to the prospect of tutoring by older peers within the *bari*.

Mean education of peers. Four key variables of interest in this study are mean peer schooling of boys and girls of their own cohort and that of the older cohort in the *bari* (all being potential proxies for endogenous social effects). Whilst all the four variables are highly significant, those of particular interest relate to the effects of the older cohort variables. As discussed earlier, these serve as superior proxies for endogenous social effects. Turning to the gender effect, there is no statistical difference between the effects of mean education of male and female peers. The effect does not vary across the sample of boys and girls. Similarly, boy–boy and/or girl–girl interaction does not appear to be statistically different from boy–girl and/or girl–boy interaction.

Maximum schooling among peers. Contrary to our expectation, maximum grade completed among *bari* children has a negative, albeit insignificant effect on individual grade completion. This is suggestive of the absence of any potential role model effect arising from the presence of peers in higher grades.

***Bari* adult female education.** The two dummy variables – the presence of at least one mother with grade 1–4 equivalent education and one with primary grade completion in the *bari* – have no significant effect. This result is not surprising since, *a priori*, neighbouring educated parents act as home teachers only if own parents are relatively less educated. As an alternative test, we re-ran the regressions by splitting the sample of children by educational status of the household-head (results suppressed). None of the *bari* female adult education variables were significant for the sample of households headed by educated males whereas for the sample of households with uneducated heads, presence of at least one (grade 1–4) educated mother has a strong positive effect.[13] This confirms our prior that the positive externality due to proximity of educated mothers in the neighbourhood exists but is exclusive to children of uneducated parents.

Intra-household education externality. Turning to household specific variables, the coefficients on '*head's spouse has no education*' and '*head's spouse has grade 1–4 education*' are negative and highly significant (Table 1; MHHs sample).[14] This confirms that children of less-educated mothers have less schooling. However, coefficients on the interaction terms (i.e. '*head's spouse has no education & proximate*' and '*head's spouse has primary education & proximate*') are positive and significant. This suggests that the disadvantage of being born to uneducated mothers is partially offset in presence of co-resident primary educated adults in the household. To be precise, children of *isolated* mothers are worse off than that of *proximate* uneducated mothers. This finding is robust to control for *bari* FEs (regression results available upon request) and hence suggestive of 'intra-household externality' in education. However, much of the intra-household externality (as reported in our study) is likely to be specific to joint families. Using Indian data, Foster and Rosenzweig (2002) find considerable evidence that joint family children have higher educational attainment than those in nuclear families. Nevertheless, the sources of variation in the characteristics of these nonspouse adult members in extended families could be endogenous in educational production to the extent that the incident of co-residence (and hence the availability of additional educated persons) itself is driven by the demand for household specific public goods such as education (Foster, 1998). Hence, care is needed in interpreting the finding on intra-household externality.

V. Additional Tests

Splitting sample by inter-households ties

It may be recalled that household-heads in *baris* are often related via kinship. The heads of all or a subset

[13] Education of *bari* 'nonmother female' adults still have no effect.

[14] Although the effect of mother's education is not necessarily exogenous to the extent that male heads with higher taste for educated children marry educated female so that mother education merely captures unobserved taste for education of the head.

of households in a given *bari* may be related as brothers or father and sons or mother and sons (Rahman *et al.*, 2001). Besides, it may be that a set of household-heads in a *bari* who are all brothers in 1996 (or two household-heads in 1996 are related as father and the son) have resided in the same household in the recent past, say, in 1982. Since these household partitions occurred after 1982 (the year for which we also have MSEC data), by 1996 many of them may be retaining some of the earlier links that they enjoyed as members of same household. If so, the estimates of the *bari* social effect may simply serve as a proxy for the benefit of joint residence in a single household (such as sharing a public good like education). If so, the separation of households 'linked' by their history of partition from the 'nonlinked' households allows a way to further minimize the kinship-related 'correlated effects' (e.g. characteristics common to cousins) which may otherwise bias our estimate of *bari* social effects.

Information on household partition is not directly recorded in our data. To identify and separate out these households from others (i.e. which have not split) within a given *bari*, we follow a rule adopted by Foster (1993). According to Foster, a household partition takes place if two individuals enumerated in a given household in period $t-1$ were observed to be household-head in period t. Hence, if two heads from the same *bari* in 1996 are found to be co-resident of a household in 1982, then we assume that household partition has taken place.[15] Then we separate these recently partitioned households (whose heads were in the same households in 1982 but separated thereafter to form new households) from the rest of our MSEC 1996 sample. The main sample purged of the 'linked' households yields the sample of 'nonlinked' households. Note that household partition referred to above is different from the case of new headship, where there is merely a transfer of headship, for example, following the death of the earlier household-head unless another member from the same household co-exists in the same *bari* in 1996 but as the head of a separate household.[16] In addition, there may still be household partitions where parted household is located outside the *bari*. But we do not look at that as we are primarily interested in the relation between households within the *bari*, not outside.

There are some notable differences in the characteristics of the 'linked' versus 'nonlinked' sample. 'Linked' households appear to be headed by younger adults. For 'nonlinked' households, mean school completion of children is 2.46 compared to 1.93 for 'linked' households and the difference is statistically significant (*p*-value 0.00). Household-heads in 'linked' sample also have less schooling. These differences are interesting for the following reason. Foster and Rosenzweig (2002), in their study of the determinants of household division in India, find that intra-household inequality in schooling increases the probability of household division. Conditional on mean and variance of schooling, increase in maximum schooling decreases the probability of a household split. The division of households thus contributes to a reduction in within-household inequality, but increasing differentials in inter-household average schooling. Hence, higher mean schooling attainment of children in our sample of 'nonlinked' households may be capturing the fact that these households are also more likely to have sustained as joint families compared to the sample of 'linked' households (which are more likely to have experienced a dissolution and hence, nuclear).

Regression results for 'nonlinked' and 'linked' sample households are reported in Table 2. The respective *bari* variables for 'nonlinked' ('linked') households are generated using information on all other households in the *bari* which are 'nonlinked' ('linked') to the index household. For the sake of brevity, we report estimates focusing on the male head's spousal education (instead of the own mother's education). The first two columns in Table 2 correspond to 'nonlinked' sample, whilst the last two report results for the 'linked' sample.

Both the *bari* and village FEs models for both samples yield significant evidence of intra-household education externality. However, on inter-household externality, all the *bari* variables related to female adult schooling are insignificant for the 'nonlinked' sample. The effects of mean schooling of *bari* boys and girls are highly significant and positive. Since this sample excludes 'linked' household children, estimates for 'nonlinked' sample are less likely to be contaminated by common characteristics as would have prevailed due to the presence of children related

[15] This approach discards all households set up outside the study area i.e. Matlab Thana. These are treated as events of migration.
[16] In this scenario, Foster (1993) further distinguishes between 'new household' and 'inherited household'. If the original head was still in the study area and lived in the household of one of the new heads, then that new head is assumed to have inherited a household. Such a distinction between inherited and new households is not made in our analysis for we do not focus on the impact of partition.

Table 2. FE and OLS estimates of determinants of grade completion for 'nonlinked' and 'linked' households (children aged 6–17 years)

	Nonlinked		Linked	
	FE	OLS	FE	OLS
Age	0.328 (10.49)**	0.271 (8.42)**	0.273 (6.26)**	0.214 (4.78)**
(Age squared)/100	0.518 (3.88)**	0.562 (4.17)**	0.749 (3.86)**	0.949 (4.87)**
Female	0.027 (0.93)	0.050 (1.83)***	−0.089 (2.18)*	−0.098 (2.52)*
Birth order in family	−0.165 (8.80)**	−0.153 (9.06)**	−0.155 (5.50)**	−0.142 (5.65)**
Education of household-head	0.049 (7.61)**	0.062 (11.46)**	0.062 (5.76)**	0.067 (7.98)**
Head's spouse has no education	−0.916 (14.08)**	−0.943 (17.15)**	−0.531 (5.87)**	−0.611 (8.22)**
Head's spouse has grade 1–4 education	−0.379 (3.89)**	−0.422 (5.09)**	−0.184 (1.59)	−0.216 (2.24)*
Head's spouse has no education and proximate	0.367 (7.77)**	0.402 (9.87)**	0.302 (3.77)**	0.280 (4.29)**
Head's spouse has grade 1–4 education and proximate	−0.023 (0.23)	0.000 (0.00)	−0.014 (0.10)	−0.021 (0.19)
Mean peer schooling, girls of own cohort		0.119 (13.70)**		0.112 (8.21)**
Mean peer schooling, boys of own cohort		0.081 (9.17)**		0.097 (7.30)**
Mean peer schooling, girls of older cohort		0.059 (7.28)**		0.058 (4.22)**
Mean peer schooling, boys of older cohort		0.032 (4.24)**		0.034 (2.55)*
Maximum schooling among peers		−0.034 (2.85)**		−0.017 (1.02)
Birth order in *bari*		0.042 (9.92)**		0.060 (7.76)**
Mean schooling of male adults in *bari*		0.043 (5.50)**		0.029 (3.02)**
Grade 1–4 educated mother in *bari*		−0.011 (0.34)		0.124 (2.58)**
Primary educated mother in *bari*		−0.009 (0.25)		0.048 (0.94)
Primary educated adult female in *bari*		−0.048 (1.24)		−0.023 (0.26)
Grade 1–4 educated female adult in *bari*		−0.004 (0.09)		0.157 (1.60)
Adjusted R^2	0.51	0.59	0.50	0.60
Bari FEs	Yes	No	Yes	No
Village FEs	No	Yes	No	Yes
N	12 729	12 729	5166	5166

Note: Same as Table 1.

as first cousins and so on.[17] For the 'linked' sample, education of *bari* mothers also exerts significant positive influence. These findings are consistent with Foster (1993). Using a sample from Matlab censuses 1974 and 1982, Foster finds that recently partitioned households within *baris* are significantly linked: children's education is affected by characteristics of other linked households in the *bari* which co-existed as joint-families in 1974. However, the distribution of resources within *bari* also matters, controlling for linked household FEs.

Intra- and inter-household externalities: income effects?

The externality due to one's proximity to educated adults (in the household and outside in the *bari*) may capture an income effect: uneducated parents may earn more in the labour market due to their proximity to other educated adults in the household/neighbourhood (Basu *et al.*, 2002). Social interactions then boost schooling merely via relaxing the family budget constraint. If so, superior control for household income (proxied by household per capita expenditure or LnPCE) would predict a fall in the size of the externality estimates. Subsequently, we test this hypothesis using the MSEC–MHSS-linked sample data. The regression results are reported in Table A2. With the inclusion of LnPCE and controlling for *bari* FEs, uneducated spouse's proximity to primary educated nonspouse adult in the household continues to exert a positive significant effect on grade completion (specification 1). However, the

[17] In our analysis, children in linked (nonlinked) households benefit from interactions with children from linked (nonlinked) households only. This imposes a restriction that nonlinked (linked) household children are outside the social space of children from linked (nonlinked) households.

effect becomes statistically insignificant once we treat LnPCE as endogenous (specification 2).[18] It seems that externality benefit of having educated nonspouse adults in the household is being transmitted via an increase in per capita expenditure.

To observe whether inter-household externality arising from the education of *bari* individuals are also capturing some income effects, we turn to columns (3) and (4), which respectively treat LnPCE as an exogenous and endogenous regressor. Reassuringly, in both specifications, most of our earlier evidence of social effects remain statistically significant and preserve their expected signs. Mean schooling of *bari* girls and boys are all significant and positive.

We further investigate the above results by re-estimating our model separately for the sample of 'nonlinked' and 'linked' households (see Tables A4 and A5). For the 'nonlinked' sample, mean schooling of children and adults in the *bari* continue to exert a significant positive influence. Particularly reassuring is the significance of the effect of the older cohort of boys and girls. Turning to intra-household externalities, we also find a significant effect of uneducated mothers on their children's education when they co-reside with primary educated nonspouse adults in the household. However, evidence supporting within-household education spillover is absent in the sample of 'linked' households.

Additional test of inter-household education externality

The evidence of neighbourhood effect, even if robust to host of controls for household and neighbourhood attributes and composition, could arguably be driven by common exposure of children in same neighbourhoods to public education programmes. Two such programs are Food-For-Education (FFE) and Female Secondary Stipend (FSS), which have been instrumental in increasing school participation in rural Bangladesh in recent years. However, both of these programs were introduced during the 1990s. Looking at school attainment of adults therefore provides a way to circumvent this problem as they were not exposed to any of these interventions. Any evidence of peer effect for the adult sample therefore cannot be attributed to common participation in public education programmes.

To this end, we estimate a model of wage work participation (via IV-probit regressions) for a sample of adults aged 20–32 years where schooling is treated as an endogenous regressor. Peer effect is investigated in the first-stage regression where retrospective measures of *bari* schooling serve as instruments for an individual's educational attainment. The regressions also control various individual characteristics such as age, age squared, religion and gender. Additionally, household landholding is included as a regressor. We use four lagged variables as instruments for years of schooling: (i) mean peer schooling of boys in the own cohort, (ii) mean peer schooling of girls in the own cohort, (iii) mean peer schooling of older cohort and (iv) mean schooling of adults (i.e. individuals aged 18–65 year olds) in the *bari*. These variables were calculated using MSEC 1982 data.

Using the *bari* characteristics as instrumental variables for the endogenous education variable in the wage participation regression is somewhat similar to the practice of using family background variables as IV for grade completion in an individual wage regression. There is ample application of such instruments in the economics literature on returns to education. As with family background variables, the exogeneity of *bari* variables as IVs is not always obvious since these instruments are not generated by any natural or quasi-natural experiments. Given these potential problems, we do not claim that the IV estimates reported in this section decisively solve the endogeneity problem of schooling variable in wage participation. It is simply meant to be illustrative of the hypothesis that *bari* schooling in early childhood generates *spillover* effects which are reflected in greater labour force participation in later years via increased schooling attainment.

The determinants of wage work participation from the probit and IV-probit models are reported in Table 3. All the estimates reported correspond to the marginal effects instead of regression coefficients. The bottom panel reports result of a test of exogeneity for the probit model with an endogenous

[18] As excluded instruments, we use total market value of assets and total number of cows owned by the household (see column (5)). These instruments are highly significant in the first stage and comfortably pass the validity test. Despite the fact that our excluded instruments pass the validity test, we are cautious in being conclusive about these IV results. The use of cows as instrument, for example, could be contested. For farm households, however, this variable potentially reflects demand for child labour and hence *a priori*, could be disputed as a choice for valid exclusion restriction.

Table 3. Probit and IV-probit estimates of determinants of wage–work participation (adults aged 20–32 years)

	Probit	IV-Probit	First stage schooling
Age	0.001 (1.73)***	0.001 (1.86)***	−0.124 (11.83)**
Hindu	−0.030 (5.77)**	−0.029 (5.59)**	−0.188 (2.07)*
Female	−0.143 (22.84)**	−0.143 (22.77)**	−0.221 (3.14)**
Married	−0.022 (4.18)**	−0.019 (3.58)**	−0.425 (5.11)**
Years of schooling	0.012 (21.52)**	0.015 (8.61)**	
Household size	−0.005 (1.05)	−0.005 (1.07)	0.189 (2.42)*
Total homestead land of household	−0.000 (0.30)	−0.000 (1.08)	0.030 (13.30)**
Total cultivable land of household	−0.000 (3.63)**	−0.000 (3.98)**	0.006 (20.05)**
Mean peer schooling, boys of own cohort in 1982			0.092 (3.73)**
Mean peer schooling, girls of own cohort in 1982			0.134 (4.54)**
Mean peer schooling, older cohort in 1982			0.140 (6.53)**
Mean schooling of adults in *bari* in 1982			0.466 (20.27)**
Pseudo/adjusted R^2	0.23	0.17	0.24
Smith–Blundell test of exogeneity (p-values)		.059	
N	9749	9749	9749

Notes: Absolute value of t-statistics in parentheses. The endogenous variable in the IV-Probit model is 'years of schooling'. All the regression estimates correspond to marginal effects (instead of regression coefficients). Regression intercept suppressed.
*, ** and *** denote significance at 5, 1 and 10% levels, respectively.

regressor proposed by Smith and Blundell (1986).[19] Rejection of the null indicates that schooling is endogenous and hence, the IV-probit is preferred over the standard probit estimator. Both probit and IV-probit estimates yield positive and significant effects of the schooling variable. All the other regressors have usual signs. The highly significant effect of the four lagged schooling variables in the first stage is consistent with our earlier evidence of significant positive effect of *bari* schooling on children's grade completion.

VI. Conclusion

This article has provided a detailed account of social determinants of children's school completion in rural Bangladesh. We have tested for the presence of inter-household externalities in children's grade completion within rural residential neighbourhoods in Bangladesh. We find some evidence of inter-household externalities: mean schooling of boys and girls in the *bari* raise grade attainment of an individual child. These effects are significant and large irrespective of whether we focus on the mean schooling of peers of own or older cohorts in the *bari*.

There are three pieces of evidence that espouse social effect as an explanation for the neighbourhood effects reported in this study. First, the results remain unchanged even when we account for neighbourhood composition. Households in a *bari* are often potentially linked in terms of their recent history of partition. Recently partitioned households often maintain significant socio-economic ties so that their presence in the data is a potential source of 'correlated' (nonsocial) effects, which masks genuine social effect. Yet, regression results yield significant coefficient on mean schooling of own and older cohorts of peers in the *bari* even when we purge our sample of the 'linked' households. This finding remains unchanged to further control for household expenditure. Second, the finding of positive peer effect is robust to control for host of household and neighbourhood characteristics. In particular, we allow for endogeneity of household expenditure by re-estimating schooling regressions in an instrumental variable framework. However, this does not drive out our result of inter-household externality.

Finally, we demonstrate that the evidence on peer effect is not driven by educational-reform related 'correlated effects' e.g. all children from some neighbourhoods benefit from public interventions, such as the FFE programme. To this end, we test for peer effects for an older cohort of individuals who completed schooling at a time when these educational interventions were nonexistent.

[19] The test involves specifying that the exogeneity of schooling variable is under suspicion. Under the null hypothesis, the probit model is appropriately specified with all explanatory variables as exogenous. Under the alternative hypothesis, the suspected endogenous schooling variable is expressed as linear projections of a set of instruments (including *bari* mean schooling variables), and the residuals from those first-stage regressions are added to the model. Under the null hypothesis, these residuals should have no explanatory power.

Using a *sample of adults* for whom retrospective records on their childhood residential neighbourhoods is available, we test whether mean education of peers during their school years affects labour market participation decisions via increasing school attaining of an index adult. First, we estimate a simple model of wage work participation, where an individual's educational attainment is endogenous. Subsequently, we test the hypothesis that the mean schooling of peers in the *bari* during an individual's childhood are valid and strong instruments for own schooling. Our analysis confirms this hypothesis. The significance of peer variables in the first-stage regression of schooling, thus once again suggest the presence of a social effect (externality) originating from the schooling of peers in the neighbourhood. This lends further support to the evidence of peer effects in children's schooling for the *sample of children* currently of school age in our data.

In addition to the evidence of inter-household education externalities, we find considerable evidence of intra-household externality enjoyed by children of uneducated mothers due to their proximity to primary educated adults in the household. This evidence is sufficiently robust to fully control for neighbourhood FEs; within-household externality is larger when the male spouse is also uneducated. Our finding of within-household externality is particularly important in rural Bangladesh, where the overall literacy rate (among those aged 7 years and above) is as low as 44.3% (in 1995) and 28.5% for females.

The suggestive evidence presented in this article has important policy implications for education policy design in developing countries. The evidence of intra-household externality – children of uneducated parents are better off when co-residing with educated nonparent adults in the same household – implies that traditional public interventions (such as cash subsidies, stipends, fee waivers and so on) to attract and retain the 'difficult-to-reach' at risk children from poor households to schools may be complemented by more longer term policy of improving the schooling levels of the adult population. This can be accomplished via educational investment in low-skilled parents, particularly uneducated mothers. Any externality arising from such investments are, nevertheless, likely to be captured by own children only, which may not benefit neighbouring children of school age, as evidenced from the insignificant effect of educated adult (female) neighbours in the *bari* on children's schooling. The same is, however, not true if a child within the neighbourhood is targeted for an educational intervention. Policies designed at increasing schooling of one child (in educationally deprived neighbourhoods) has important spillover effects on schooling outcomes of other children in the *bari* with a feedback to further boost schooling of the targetted student. Our evidence of significant positive effect of mean schooling of neighbourhood children on individual grade completion supports this hypothesis.

To conclude, traditional public interventions (such as conditional cash transfers, fee waivers and so on) to attract and retain a child in school is also likely to benefit other neighbouring children of school age. In other words, policies designed at increasing schooling of one child in educationally deprived neighbourhoods has important spillover effects on schooling outcomes of other children in the *bari* with a feedback to further boost schooling of the targeted student. Our evidence of significant positive effect of mean schooling of *bari* children on individual grade completion supports this hypothesis. That said, clearer evidence of the social effect is necessary to ascertain whether the observed effect, indeed reflects endogenous social effects, as opposed to contextual effects. This is a challenge that future research on social interactions in educational production should address for developing countries.

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Appendix

Table A1. Means statistics of sample observations and variable definition (children aged 6–17 years)

Variable	Definition	Mean	SD
Grade	Years of grade completed in school	2.34	2.41
Age	Measured in years	11.27	3.42
Female	=1 if female	0.49	0.50
Hindu	=1 if Hindu	0.12	0.32
Birth order in family	Birth order of index child within family 'eldest child has smallest value'	1.88	1.01
Household variables			
Age of household-head	In years	46.43	11.30
Household-head is female	= 1 if household-head is female	0.15	0.36
Education of household-head	Years of education completed by household-head	2.95	3.53
Head has no education	=1 if head has no education	0.49	0.50
Head completed grade 1–4	=1 if head has grade 1–4 education	0.18	0.39
Head has no education but proximate	=1 if head uneducated and a primary educated adult present in family	0.13	0.34
Head has 1–4 grade and proximate	=1 if head educated 1–4 grade co-resides with a primary educated adult in the family	0.08	0.26
Household-head is agriculturist	=1 if head is in agriculture	0.30	0.46
Household-head is day labourer	=1 if head is a day labourer	0.11	0.31
Age of head's spouse	Head's spouse's age	31.96	16.99
Data on age of head's spouse missing	=1 if head's spouse age data missing	0.18	0.38
Head's spouse has no education	=1 if head's spouse has no education	0.53	0.50
Head's spouse has grade 1–4 education	=1 if head's spouse has grades 1–4 education	0.12	0.33
Head's spouse has no education and proximate	=1 if uneducated spouse co-resides with a primary educated adult in the household	0.21	0.41
Head's spouse has grade 1–4 education and proximate	=1 if 1–4 grade educated spouse co-resides with a primary educated adult in family	0.07	0.26
Total homestead land of household	Household-owned homestead land (in decimals)	10.77	14.55
Total cultivable land of household	Household-owned cultivable land (in decimals)	63.37	108.97

(*continued*)

Table A1. Continued

Variable	Definition	Mean	SD
Bari variables			
Mean peer schooling, girls of own cohort	Own cohort includes any child aged at most 2 years older/younger	1.97	2.13
Mean peer schooling, boys of own cohort	Own cohort includes any child aged at most 2 years older/younger	2.01	2.10
Mean peer schooling, girls of older cohort	Older cohort includes any peer older than the index child by 3–5 years	2.67	2.77
Missing, female	=1 if data is missing for 'mean peer schooling, girls of older cohort'	0.29	0.45
Mean peer schooling, boys of older cohort	Older cohort includes any peer older than the index child by 3–5 years	2.86	2.86
Missing, male	=1 if data is missing for 'mean peer schooling, boys of older cohort'	0.26	0.44
Maximum schooling among peers	Maximum grade completed among *bari* children (aged 6–17)	6.24	2.31
Birth order in *bari*	Birth order of the index child among peers in *bari*	8.51	9.70
Mean schooling of adults in *bari*	mean schooling of *bari* adults	3.14	2.06
Mean schooling of male adults in *bari*	mean schooling of *bari* adult males	3.93	2.54
Grade 1–4 educated mother in *bari*	=1 if at least one 1–4 grade educated mother present in *bari*	0.52	0.50
Primary educated mother in *bari*	=1 if at least one 5+ grade completed mother present in *bari*	0.55	0.50
Primary educated adult female in *bari*	=1 if at least one 1–4 grade educated *bari* nonmother female present	0.19	0.40
Grade 1–4 educated female adult in *bari*	=1 if at least one 5+ grade completed *bari* non-mother female present	0.14	0.34
Number of children, 0–5	Total no. of 0–5 years olds in *bari*	7.62	8.88
Number of children, 6–17	Total no. of 6–17 years olds in *bari*	16.41	15.49
Number of children, 18+	Total no. of 18 years or older individuals in *bari*	27.03	25.24
Mean homestead land of *bari*	Average homestead land owned by other HHs in *bari* (in decimals)	10.36	10.00
Mean cultivable land of *bari*	Average cultivable land owned by other HHs in *bari* (in decimals)	59.74	69.40
N		22370	

Notes: (a) Sample excludes heads aged below 22 years to separate nonstudents. (b) Nonmother females are female adults (aged 18+) in *bari* with no own children present in their respective household.

Table A2. Neighbourhood FEs estimates of determinants of grade completion of children (MHHs only)

	All MHHs	MHHs with uneducated heads
Age	0.316 (12.95)**	0.332 (9.48)**
(Age squared)/100	0.541 (5.12)**	−0.039 (0.26)
Female	0.006 (0.26)	−0.009 (0.28)
Birth order in family	−0.177 (12.26)**	−0.184 (8.23)**
Age of household−head	−0.002 (1.19)	−0.005 (1.54)
Education of household-head	0.055 (10.92)**	
Household-head is agriculturist	−0.062 (1.93)***	−0.004 (0.07)
Household-head is day labourer	−0.362 (8.35)**	−0.187 (3.11)**
Age of head's spouse	0.004 (1.51)	0.002 (0.38)
Head's spouse has no education	−0.771 (15.77)**	−0.802 (7.63)**
Head's spouse has grade 1–4 education	−0.348 (5.04)**	−0.386 (2.95)**
Head's spouse has no education and proximate	0.342 (9.19)**	0.506 (8.89)**
Head's spouse has grade 1–4 education and proximate	0.009 (0.12)	0.187 (1.12)
Adjusted R^2	0.52	0.35
Bari FEs	Yes	Yes
N	18 906	8856

Notes: Absolute value of *t*-statistics in parentheses. Regression constant has been suppressed. All the specifications include *bari* fixed effects and dummy for missing data of mothers/male head's spouse.
*** and ** denotes significance at 10 and 1% levels, respectively.

Table A3. OLS, FE and IV estimates of determinants of grade completion of children using linked MHSS-Census sample (MHHs only)

	(1) FE	(2) IV-FE	(3) OLS	(4) IV	(5) First stage (LnPCE)
Female	0.104 (2.21)*	0.109 (2.29)*	0.097 (2.27)*	0.106 (2.39)*	−0.018 (1.38)
Birth order in family	−0.223 (6.01)**	−0.250 (6.46)**	−0.187 (7.16)**	−0.200 (7.36)**	−0.005 (0.58)
Education of household-head	0.066 (4.26)**	0.058 (3.61)**	0.073 (8.16)**	0.065 (6.80)**	0.014 (5.10)**
Head's spouse has no education	−0.500 (3.30)**	−0.235 (1.32)	−0.547 (6.22)**	−0.089 (0.87)	−0.332 (12.69)**
Head's spouse has grade 1–4 education	−0.322 (1.42)	−0.088 (0.36)	−0.208 (1.67)***	0.125 (0.95)	−0.211 (5.61)**
Head's spouse has no education and proximate	0.275 (2.38)*	0.163 (1.32)	0.274 (4.14)**	0.092 (1.23)	0.142 (7.13)**
Head's spouse has grade 1–4 education and proximate	0.150 (0.63)	−0.007 (0.03)	0.051 (0.39)	−0.106 (0.77)	0.118 (2.99)**
LnPCE	0.430 (5.36)**	1.170 (4.40)**	0.606 (13.20)**	1.431 (8.61)**	
Value of household asset					0.113 (16.93)**
Total cows owned by household					0.040 (8.10)**
Birth order in *bari*	0.040 (5.97)**	0.041 (6.00)**	0.045 (7.23)**	0.045 (6.92)**	0.000 (0.14)
Mean peer schooling, girls of own cohort			0.104 (7.99)**	0.103 (7.63)**	−0.001 (0.31)
Mean peer schooling, boys of own cohort			0.064 (4.80)**	0.060 (4.33)**	0.006 (1.61)
Mean peer schooling, girls of older cohort			0.077 (5.87)**	0.068 (5.01)**	0.008 (1.96)***
Mean peer schooling, boys of older cohort			0.070 (5.76)**	0.067 (5.31)**	0.005 (1.36)
Maximum schooling among peers			−0.031 (1.86)***	−0.036 (2.09)*	0.006 (1.20)
Mean schooling of male adults in *bari*			0.022 (2.04)*	0.018 (1.61)	0.005 (1.53)
Grade 1–4) educated mother in *bari*			0.041 (0.79)	0.058 (1.08)	0.005 (0.29)
Primary educated mother in *bari*			0.018 (0.33)	0.037 (0.65)	−0.010 (0.64)
Primary educated adult female in *bari*			0.114 (1.73)***	0.150 (2.21)*	−0.048 (2.45)*
Grade 1–4 educated female adult in *bari*			0.031 (0.40)	0.041 (0.53)	−0.046 (1.99)*
Adjusted R^2	0.60	–	0.62	–	0.32
Bari FEs	Yes	Yes	No	No	No
Village FEs	No	No	Yes	Yes	Yes
Exogeneity test of LnPCE	–	0.003	–	0.00	–
Over-identification test	–	0.88	–	0.50	–
N	4993	4993	4993	4993	4993

Notes: Absolute value of *t*-statistics in parentheses. Regression constant has been suppressed. The regressions also control for total number of children aged 0–5 years and 6–17 years, total number of adults in the *bari*, religion and head's occupation, age and spousal age. Also included are dummies to control for missing data on mean peer schooling variables, head's spouse age and number of cows.

*, ** and *** denote significance at 5, 1 and 10% levels, respectively.

Table A4. OLS, FE and IV estimates of determinants of grade completion of children using linked MHSS–MSEC sample 'nonlinked' households (MHHs only)

	(1) FE	(2) IV-FE	(3) OLS	(4) IV	(5) First stage (LnPCE)
Age	0.395 (6.30)**	0.382 (6.03)**	0.273 (4.46)**	0.251 (4.01)**	0.016 (0.91)
(Age squared)/100	0.459 (1.78)***	0.483 (1.86)***	0.600 (2.33)*	0.636 (2.42)*	−0.008 (0.10)
Female	0.097 (1.69)***	0.104 (1.80)***	0.093 (1.74)***	0.101 (1.84)***	−0.014 (0.87)
Birth order in family	−0.149 (3.19)**	−0.170 (3.54)**	−0.161 (4.78)**	−0.175 (5.05)**	−0.004 (0.45)
Education of household-head	0.064 (3.22)**	0.060 (2.98)**	0.060 (5.31)**	0.053 (4.39)**	0.016 (4.79)**
Head's spouse has no education	−0.807 (4.02)**	−0.633 (2.90)**	−0.731 (6.43)**	−0.273 (2.15)*	−0.362 (11.07)**
Head's spouse has grade 1–4 education	−0.312 (1.06)	−0.164 (0.54)	−0.275 (1.64)	0.048 (0.28)	−0.230 (4.69)**
Head's spouse has no education and proximate	0.405 (2.93)**	0.328 (2.28)*	0.315 (3.87)**	0.173 (1.97)*	0.112 (4.72)**
Head's spouse has grade 1–4 education and proximate	−0.093 (0.31)	−0.222 (0.72)	0.097 (0.56)	−0.015 (0.08)	0.100 (1.97)*
LnPCE	0.400 (3.90)**	0.850 (3.54)**	0.599 (10.30)**	1.277 (6.49)**	
Value of household asset					0.117 (13.90)**
Total cows owned by household					0.049 (7.52)**
Birth order in *bari*	0.056 (6.39)**	0.057 (6.42)**	0.054 (6.21)**	0.052 (5.88)**	0.002 (0.83)
Mean peer schooling, girls of own cohort			0.095 (6.12)**	0.097 (6.11)**	−0.005 (1.02)
Mean peer schooling, boys of own cohort			0.040 (2.56)*	0.040 (2.50)*	0.002 (0.43)
Mean peer schooling, girls of older cohort			0.091 (5.60)**	0.086 (5.21)**	0.002 (0.40)
Mean peer schooling, boys of older cohort			0.060 (3.96)**	0.058 (3.78)**	0.002 (0.49)
Maximum schooling among peers			−0.049 (2.36)*	−0.058 (2.72)**	0.014 (2.39)*
Mean schooling of male adults in *bari*			0.036 (2.71)**	0.029 (2.13)*	0.009 (2.42)*
Grade 1–4 educated mother in *bari*			0.052 (0.79)	0.068 (1.01)	0.010 (0.52)
Primary educated mother in *bari*			−0.004 (0.05)	−0.009 (0.13)	0.019 (0.94)
Primary educated adult female in *bari*			0.038 (0.46)	0.078 (0.92)	−0.034 (1.42)
Grade 1–4 educated female adult in *bari*			0.034 (0.34)	0.043 (0.42)	−0.050 (1.71)***
Adjusted R^2	0.61	–	0.62	–	0.35
Bari FEs	Yes	Yes	No	No	No
Village FEs	No	No	Yes	Yes	Yes
Exogeneity test of LnPCE	–	0.08	–	0.00	–
Over-identification test	–	0.25	–	0.11	–
N	3337	3337	3337	3337	3337

Notes: Absolute value of *t*-statistics in parentheses. Regression constant has been suppressed. Columns (1) and (2) also controlled for religion, two dummies for head's occupation (whether a day labourer or an agriculturist) and spouse's age. The regressions in columns (3)–(5) also control for total number of children aged 0–5 years and 6–17 years, total number of adults in the *bari*, religion and head's occupation and spouse's age. Also included are dummies to control for missing data on mean peer schooling variables, head's spousal age and number of cows.

*, ** and *** denotes significance at 5, 1 and 10% levels, respectively.

Table A5. OLS, FE and IV estimates of determinants of grade completion of children using linked MHSS–MSEC sample 'linked' households (MHHs only)

	(1) FE	(2) IV-FE	(3) OLS	(4) IV	(5) First stage (LnPCE)
Age	0.136 (1.32)	0.187 (1.65)***	0.210 (2.26)*	0.205 (2.14)*	0.001 (0.05)
(Age squared)/100	0.938 (2.20)*	0.746 (1.59)	0.795 (1.99)*	0.797 (1.95)***	−0.007 (0.06)
Female	0.032 (0.34)	0.025 (0.25)	0.026 (0.32)	0.037 (0.43)	−0.029 (1.23)
Birth order in family	−0.397 (5.09)**	−0.397 (4.81)**	−0.159 (2.99)**	−0.168 (3.08)**	−0.009 (0.60)
Age of household-head	−0.014 (0.81)	−0.008 (0.45)	0.002 (0.24)	−0.007 (1.30)	−0.004 (1.49)
Education of household-head	0.063 (1.66)***	0.048 (1.15)	0.079 (4.23)**	0.087 (4.61)**	0.001 (0.20)
Head's spouse has no education	0.259 (0.79)	0.702 (1.55)	−0.479 (2.83)**	−0.080 (0.37)	−0.300 (6.11)**
Head's spouse has grade 1–4 education	−0.713 (1.51)	−0.099 (0.15)	−0.360 (1.65)***	−0.082 (0.35)	−0.162 (2.53)*
Head's spouse has no education and proximate	−0.262 (0.90)	−0.080 (0.24)	0.180 (1.20)	−0.076 (0.38)	0.262 (6.06)**
Head's spouse has grade 1–4 education and proximate	0.493 (0.90)	0.470 (0.81)	0.009 (0.03)	−0.229 (0.80)	0.197 (2.62)**
LnPCE	0.245 (1.29)	2.091 (1.69)***	0.549 (5.68)**	1.285 (3.03)**	
Value of household asset					0.099 (7.48)**
Total cows owned by household					0.021 (2.53)*
Birth order in *bari*	−0.029 (0.82)	−0.019 (0.51)	0.036 (1.21)	0.041 (1.34)	−0.008 (0.93)
Mean peer schooling, girls of own cohort			0.108 (3.74)**	0.114 (3.82)**	−0.008 (0.94)
Mean peer schooling, boys of own cohort			0.142 (5.13)**	0.133 (4.65)**	0.010 (1.29)
Mean peer schooling, girls of older cohort			0.039 (1.21)	0.026 (0.77)	0.012 (1.31)
Mean peer schooling, boys of older cohort			0.022 (0.74)	0.035 (1.12)	−0.011 (1.27)
Maximum schooling among peers			−0.041 (1.27)	−0.048 (1.45)	0.011 (1.14)
Mean schooling of male adults in *bari*			0.027 (1.45)	0.023 (1.20)	0.005 (0.90)
Grade 1–4 educated mother in *bari*			−0.026 (0.23)	−0.093 (0.77)	0.122 (3.79)**
Primary educated mother in *bari*			0.180 (1.59)	0.197 (1.71) +	−0.009 (0.27)
Primary educated adult female in *bari*			0.366 (1.88)***	0.433 (2.13)*	−0.119 (2.09)*
Grade 1–4 educated female adult in *bari*			0.235 (0.94)	0.199 (0.77)	−0.019 (0.25)
Adjusted R^2	0.57	–	0.62	–	0.27
bari FEs	Yes	Yes	No	No	No
Village FEs	No	No	Yes	Yes	Yes
Exogeneity test of LnPCE	–	0.11	–	0.068	–
Over-identification test	–	0.85	–	0.015	–
N	1347	1347	1347	1347	1347

Notes: Absolute value of *t*-statistics in parentheses. Regression constant has been suppressed. Columns (1) and (2) also controlled for religion, head's occupation and spouse's age. The regressions in columns (3)–(5) also control for total number of children aged 0–5 years and 6–17 years, total number of adults in the *bari*, religion and head's occupation and spouse's age. Also included are dummies to control for missing data on mean schooling of peers, head's spousal age and number of cows.
***, * and ** denotes significance at 10, 5 and 1% levels, respectively.