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

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Abstract

This paper tests for neighborhood 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 neighborhood 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 paper also provides evidence of intra-household externality in children's schooling, net of neighborhood externalities. We conclude by discussing the implication of these findings for education policy design.

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1 Introduction

The importance of the family in the process of human capital formation is well recognised 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 neighbourhood¹. 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 countries². The objective of this paper 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

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.

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3 addition to neighbourhood externalities, we also find evidence of intra-household education
4 externalities, net of neighbourhood effects: children of uneducated parents have more
5 schooling when co-residing with educated adults in the household.
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10 There are three pieces of evidence that suggest the social effect as the principle
11 explanation for inter-household effects. First, our evidence of peer effect is not driven out by
12 control for a host of attributes such as income, landholding, and parental background at the
13 neighbourhood and household levels. Furthermore, the results remain robust to
14 neighbourhood composition: it is unchanged as we purge our main sample of households
15 within the neighbourhood that are potentially linked in terms of their recent history of
16 partition. Finally, peer effect in our data could be owing to common exposure to educational
17 reforms introduced by the government in the study area. Yet a similar effect is found for
18 adults who completed schooling before the introduction of the reforms. Our analysis of the
19 long-term impact of schooling of neighbourhood peers bears out our claim of inter-
20 household externality in school completion. This is investigated in a model of decision to
21 participate in wage work for a sample of adults, treating school completion as an
22 endogenous covariate. We find that the mean schooling of neighbouring children during an
23 individual's school years increases the likelihood of wage work participation in adulthood,
24 via boosting school completion: the education of neighbours during the childhood serves as
25 instrument for own school completion in our wage participation model
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48 In Bangladesh, educational investment is widely considered as an important strategy
49 to reduce poverty (Asadullah, 2006; Wodon, 2000). According to recent research,
50 educational investment not only leads higher wage earnings in the labour market (Asadullah,
51 2006), it also boosts agricultural production in rural Bangladesh (Asadullah and Rahman,
52 2008). Therefore, increasing the level of school completion remains an important
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3 development goal for the government. To this end, a sizeable economics literature on school
4 participation and attainment of children in Bangladesh is available (e.g. Asadullah, 2005;
5 Asadullah et al., 2007; Asadullah, 2008). Whilst such literature highlights various demand
6 and supply side determinants of children's schooling choices, none of the studies explores
7 the role of externalities (beyond parental education) in children's schooling. Hence, our
8 study hopes to fill an important gap in the literature. Detailed knowledge of the determinants
9 of school participation is important
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20 The rest of the chapter is organised as follows. Section 2 discusses the empirical
21 strategy and analytical framework in detail and revisits the issue of identification of social
22 effects in the context of demand for schooling. Section 3 discusses the data. Section 4
23 provides a discussion of the main findings. Section 5 presents the conclusion.
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29 **2 Analytical Framework: Neighbourhoods and Educational Production**

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31 A primary and perhaps the most relevant social space in a rural context is defined in terms
32 of proximity of individuals by their place of residence. A common measure of
33 neighbourhood (defined as such) in rural Bangladesh is "*bari*" i.e. a cluster of households.
34 These household-clusters are widely observed all over rural Bangladesh, where individuals
35 usually enjoying some form of kinship set up households next to each other around the
36 common yard. A typical *bari* consists of two parts: *bhitor bari* (i.e. in-house) and *bahir bari*
37 (i.e. out-house). *Bhitor-bari* houses kitchen and residential dorm. Non-relatives visiting the
38 *bari* are only allowed in *bahir bari*. The out-house consists of a visitor room, otherwise
39 known as *kachari ghar*, which is commonly located next to the common pond (i.e. outer
40 pond) is meant for common usage by all *bari* males and visitors. Among other significant
41 features, *bari* members usually cooperate with each other in times of crisis. Depending on
42 the nature of the underlying kin relations, *baris* share among themselves various resources.
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3 The common resources include pond(s) of the *bari*, outhouse for guests, in-out approach
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5 road(s) of the *bari*, graveyard, at times prayer room/mosque attached to the *bari* etc.
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8 The literature on neighbourhood effects speculates about various routes via which
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10 externalities within the *bari* can be expected to increase school completion. According to
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12 Manski (1993; 2000), externalities could take the form of an endogenous social effect (i.e.
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14 children's education in the *bari* being influenced by the mean schooling of other children in
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16 the *bari*) and/or a contextual effect (i.e. children's education in the *bari* being affected by
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18 the mean characteristics of children and/or their parents in *bari*). While sociological studies
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20 speculate about the various processes that could potentially generate social effects in a
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22 neighbourhood, most of these processes can be explained in terms of preference and
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24 constraint of individuals (Manski, 2000).
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29 A correlation between a child's schooling and education of neighbouring peers,
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31 namely an endogenous social effect, could arise when greater school completion among
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33 children relaxes some constraints in educational production for the index child. One such an
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35 interaction is facilitated by *lessons swapping* among peers in the *bari*. When school
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37 attainment is, inter alia, reliant on home assignments, children in proximate grades may
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39 collude in preparation of such assignments³. The burden of home assignments is usually
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41 progressive across grades so that at higher grades, benefits of cooperation are larger,
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43 particularly among children of credit constrained families. Hence, higher participation of
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45 older or same aged children in relevant grades (predicted by their age) enlarges the pool of
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47 "school-success-specific" knowledge within the *bari*, leading to positive constraint
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49 interactions. Alternatively, elder children in higher grades could share their accumulated
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58 ³ However, children enrolled in the same grade in a school may also compete and decide not to cooperate.
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3 knowledge acting as mentors or simply pass on their notes and other tangible school inputs
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5 to children enrolled in immediately lower grades.
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8 Similarly, a contextual social effect arises when children's interactions with educated
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10 adults in the neighbourhood relax some constraints underlying the educational production
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12 function. For example, neighbouring educated mothers in the *bari* could substitute for own
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14 mother's education, particularly when one's own mother has little or no education. Given
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16 the distinction between proximate uneducated and isolated uneducated in society, what
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18 matters in children's education is not the *actual* but rather the *effective* level of education of
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20 own mothers, defined in terms of mother's proximity to other educated adults within the
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22 household and outside, in neighbouring households (Basu and Foster, 1998)⁴.
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27 To summarise, all the above possibilities imply that whilst studying educational
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29 outcomes in developing countries, one needs to expand the educational production function
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31 by accounting for the socio-economic mix of the children in their residential neighbourhood.
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33 This point is emphasised throughout the paper. We are interested in assessing whether
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35 school completion of a child is affected by that of other children and adults in the reference
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37 population i.e. the *bari*. The econometric framework for estimating the production function
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39 accounting for neighbourhood characteristics is discussed in the next section.
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43 **2.2 Empirical Strategy: Testing for Education Spillover**

44 We employ a simple linear regression model for years of schooling completion where
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46 individual schooling varies linearly with the mean outcomes in the group, with mean
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48 exogenous attributes of the *bari*, and with other personal attributes that may be common
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50 across all group members. Such a model is specified below:
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57 ⁴ The framework is extendable beyond household, to neighbourhood and can be disaggregated by
58 characteristics of the literate household member such as gender and age (Basu and Foster, 1998).
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$$S_{bi} = \beta E_b[S_{bi}] + \gamma E_b[x_{bi}] + \delta x_{bi} + \alpha_b + \varepsilon_{bi} \quad (1)$$

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* members⁵. α_b is unobserved heterogeneity common across children within the b-th *bari* and ε_{bi} is a random error term.

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 non-social 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

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 the $E_b[S_{bi}]$ and $E_b[x_{bi}]$ vary across households within the bari.

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, non-social 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):

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

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 to 5 years older than the index child (hence j ranges from 3-5). If the "own cohort" effect is absent (i.e. $\beta = 0$), equation (2) reduces to the following:

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

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.

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3 The structural equation (2) describes endogenous social effects in terms of mean
4 schooling of peers. Such linear model of social effects, however, is too simple to fully
5 unpack the processes underlying a social effect. To better explore the “black-box of social
6 interactions”, we expand equation (2) by including additional variables on the right hand
7 side so as to distinguish between different types of constraint interactions. On specific *bari*
8 characteristics, in addition to “mean schooling” of peers of own and lagged cohort in the
9 *bari*, we include two additional regressors to distinguish between various underlying
10 mechanism via which social interaction effects arise; these are discussed below.
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22 (i) *Birth order within bari*: Children may benefit from interactions with other
23 children in higher grades via direct mentoring or through inheritance of school study
24 materials (and school resources such as textbooks). Given the prospect of such an externality
25 arising from the presence of *bari* children in sufficiently higher grades, the relative ranking of
26 a child in the grade distribution of his peers should be taken into account. As an exogenous
27 proxy for this, we use “birth order within *bari*” of the index child. Excluding own siblings,
28 this variable gives the birth order of a child among all the *bari* children (aged 6-17 years).
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39 (ii) *Maximum grade among peers*: Control for maximum grade attainment among
40 peers allows separating the effect of role models by older children in higher grades. Hence,
41 we expect a positive effect of this variable.
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46 Turning to contextual effects arising from adult education in the *bari*, the most
47 prominent is that of the effect of neighbouring mothers as home tutors. To test this effect of
48 *bari* mothers’ education, we include two dummies, each identifying the presence of at least
49 one: (i) grade 1-4 educated mother in *bari* and (ii) grade 5+ educated mother in *bari*.
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55 The distinction between mothers and non-mothers is because, educated non-mothers
56 may respond differently compared to those who are mothers of school aged children. The
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3 focus on mothers as a source of externality is consistent with the literature on social benefits
4 of female education. Basu and Foster (1998) also point out that at the margin, the external
5 effects of education/literacy are likely to be larger if the source of the externality is a female
6 rather than a male. In our model, to further purge the effects of mother-specific variables
7 from that of other educated adults in the *bari*, we additionally control for the presence of at
8 least one non-mother with (i) 1-4 years of schooling (ii) 5 or more years of schooling and
9 (iii) mean schooling of adult males in the *bari*. This allows us to net out the contribution of
10 non-mothers and adult males in the *bari*.
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22 Given the close link between households and residential neighbourhood (i.e. *bari*),
23 the impact of intra and inter-household externalities should be studied together. Hence, in
24 addition to inter-household externality, we also look at *intra-household externality*. We
25 interact own mother's education dummies (i.e. mother with no education and mother with
26 grade 1-4 equivalent education) with a dummy that identifies whether there is at least one
27 primary school educated adult in the household. A significant positive coefficient on the two
28 resulting interaction terms would imply that children of less-educated but proximate mothers
29 (i.e. those with co-resident educated non-spouse adult householders) have more schooling
30 relative to that of similarly educated but isolated mothers. Our simultaneous focus on intra
31 and inter-household externalities is thus likely to yield better estimates of these two types of
32 externalities to the extent that common *bari* environment is driven by common family
33 background effects⁶.
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50 **2.3 Additional Tests of Education Spillovers**

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56 ⁶ Support for such a specification also comes from Ginther et al. (2000) who find that the coefficients on
57 neighbourhood variables tend to fall in value and lose statistical significance as the specification of family
58 variables become more complete.
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3 Despite the proposed regression specification, our empirical strategy is likely to be fraught
4 with the problem of “correlated effects”, arising due to the composition of the
5 neighbourhood and omission of common correlates of schooling. First, children in the
6 neighbourhood may have similar schooling outcomes if they belong to equally well-
7 resourced households and household income remains poorly measured. Second, inter-
8 household correlation in schooling could capture the possibility that some of the peers are
9 related by blood i.e. a sub-sample of households in the neighbourhood comprise of extended
10 families. Below, we outline two tests to assess the robustness of our findings to these issues.
11 An additional test of the educational spillover is also discussed.

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25 **i. Accounting for Inter-household Ties:** The composition of neighbourhood in our
26 analysis poses two challenges. The *bari*, our measure of neighbourhood, consists of
27 households whose heads are usually related by blood or affinal connections. The inter-
28 household relations inside the *bari* are either defined by immediate kinship or by
29 heterogeneous kinship among constituent households. Following this, the observed
30 correlation of schooling among *bari* members may potentially capture nothing but inter-
31 household ties. For instance, couples living in close proximity (i.e. in the same *bari*) to their
32 parents may be enjoying all the benefits of joint residence in a single household headed by
33 their parents⁷.

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46 To this end, we create a sample (of “non-linked” households) that consists of
47 households which are not related to each other in the same *bari* in terms of their recent

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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.

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3 history of household partition. In the other sample (i.e. “linked” household sample), heads
4 of households currently in the same *bari* co-resided in the same household in the recent past
5 and were therefore likely to have retained close economic ties. We then split our sample
6 observations between “linked” and “non-linked” sample, and estimate the regression models
7 separately to observe whether the externality effect differs across inter-household kin ties. In
8 particular, we test whether evidence of social effect persists once we purge our main sample
9 off the linked-households. The exact sample construction rule is discussed in detail later on
10 in the paper.
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22 **ii. Unaccounted Income Effects:** We test whether externality (owing to one’s
23 proximity to educated non-spouse adults within and outside the household) is capturing an
24 unobserved wealth effect or operates via enhancing household income. We do this via
25 additionally controlling for per capita expenditure whilst estimating an instrumental variable
26 model of grade completion treating expenditure as an endogenous regressor. This also
27 improves our estimates of inter-household externalities to the extent that incomplete control
28 for income creates an omitted variable bias.
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39 **iii. Alternative Test of Inter-household Externality Effects:** If *bari* spillover effects
40 experienced during one’s childhood are genuine, we would expect children from better
41 educated *baris* to perform better in the labour market, than those from educationally
42 depressed *baris*. Therefore, testing for labour market success of children who grew up in
43 *baris* with higher schooling would suffice as an indirect and additional test of inter-
44 household externality within the *bari*. To this end, we exploit the existence of retrospective
45 data on *bari* schooling (or social environment) of individuals currently in the labour market.
46 We use this data to construct instruments for schooling attainment which is modelled as an
47 endogenous determinant of wage work participation.
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3 For our model of wage work participation, we estimate a IV-Probit regression where
4 the dependent variable takes the value one for wage work and is zero otherwise. The IV-
5 Probit model is based on Amemiya's Generalised Least Squares (AGLS) estimators for
6 probit with endogenous regressors and is more efficient than the two stage conditional
7 likelihood model (2SCML) proposed by Rivers and Young (1988). The endogenous
8 regressors are treated as linear functions of the instruments and the other exogenous
9 variables and corrected standard errors are reported.
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20 Apart from the endogenous schooling variable, the regressions control for individual
21 and household characteristics. We use the following lagged variables as instruments for own
22 education variable: (i) mean peer schooling of boys in the own cohort, (ii) mean peer
23 schooling of girls in the own cohort, (iii) mean peer schooling of older cohort and (iv) mean
24 schooling of adults (i.e. individuals aged 18-65 year olds) in the bari. If the retrospective
25 bari variables turn out to be (jointly) significant as excluded instruments for "years of school
26 completion" in our model, we can claim to have evidence of social effects arising from
27 one's residence in better-educated baris. This test is important for an additional reason.
28 Evidence of peer effect in children's education could capture correlated effects in the form
29 of common exposure to educational interventions introduced during 1990-1996. However,
30 individuals who are currently in the labour market attended school before such reforms were
31 introduced. Hence, significance of peer variables (i.e. excluded instruments) in the first stage
32 regression for this cohort would serve as a partial test of the hypothesis of "correlated
33 effect" related to common participation in educational intervention programs.
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52 **3.3 Data Description**

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55 This research combines data on residential neighbourhoods from two independent censuses
56 and one sample survey all conducted in the Matlab Thana, located in South-East of Dhaka,
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3 the capital of Bangladesh. Most of the data primarily came from the The Matlab Socio-
4 Economic Census (henceforth MSEC) 1996, conducted by the International Centre for
5 Diarrhoeal Disease Research, Bangladesh (ICDDR,B), covered a population of 25,000
6 households in 141 villages in the Matlab Thana. Census records are preserved by the Centre
7 in the form a Demographic Surveillance System (DSS). Our choice of a random sample of
8 *baris* from the census database has been guided by the availability of data from an
9 independent survey on the same population for the year 1996, as well as an earlier census
10 for the year 1982. These include the Matlab Health and Socio-Economic Survey (MHSS)
11 1996 and the MSEC 1982. Conditioning the primary sample selection on the MHSS data
12 enables us to complement our analysis of MSEC data with detailed individual, household
13 and community level information available from the MHSS.
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29 Individuals in the censuses and the survey population can be linked at the individual,
30 household and bari level using unique bari, person and household identification number. We
31 therefore merged information on sample individuals from the three sources and created three
32 unique samples to estimate our empirical models as discussed in the earlier section. The first
33 two consist of all children of school age whereas the third sample comprises of a sample of
34 adults in the *bari* for whom we have retrospective information on the characteristics of their
35 childhood neighbours. The exact rules for construction of these samples along with a
36 description of the characteristics of the resulting samples are discussed below.
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48 **3.1 Construction of the Working Sample**

49 Our *primary sample* consists of individuals drawn from the MSEC 1996 population. Since
50 the MSEC data could be used along with the MHSS, we have extracted the MSEC data from
51 the DSS database as follows. The MHSS surveyed a random sample of 2687 *baris* in the
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3 Matlab Thana area, treating the bari as the primary sampling unit (PSU)⁸. We have therefore
4 selected these bari to form our random sample. Next, records on all household and
5
6 individuals in the sample bari were extracted. This yielded data on 79094 individuals
7
8 residing in 14869 households. A total of 527 *baris* (or 19.61% of all *baris*) comprise of
9
10 single households. The rest consists of two or more households. On average, there are 9
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12 households in a *bari*. The size of the *bari* can also be judged in terms of number of resident
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14 children of school age. On average, the number of (6 to 17 years old) children per *bari* is
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16 16.3 with a range of 1 to 143 and median of 13. The summary statistics of the MSEC sample
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18 data are presented in Appendix Table 1.
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25 A *second sample* is constructed by combining the MSEC 1996 data with that of the
26
27 MHSS. The MSEC 1996 does not provide data on expenditure or income. However, such
28
29 data is available from the MHSS. Taking advantage of the overlap in the survey population
30
31 and timing, we merge the MHSS sample households with the MSEC 1996 records. Given
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33 that the MHSS sampled only 2 households per *bari*, the resulting sample is much smaller in
34
35 size: The MSEC-MHSS linked sample contains 4993 children in 2156 male-headed
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37 households in 1494 bari.
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41 The *third sample* for our analysis is constructed by linking data on individuals aged
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43 20-32 years in MSEC 1996 with their neighbourhood records in MSEC 1982, preserved in
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45 the DSS database⁹. Unique identification numbers were used from the DSS database to link
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47 individuals and households across the two censuses. This yielded a sample of 9749
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51 ⁸ First, one household was drawn from each of the *baris* that consisted of a single household. A total of two
52 households were sampled from the remaining *baris*, each of which had more than one household. This led to
53 data on a total of 24266 individuals in 4364 households. Detailed information on the MHSS 1996 is available
54 in Rahman et al. (2001).

55 ⁹ We focus on this age-group to ensure that all individuals in our 1996 sample were of school-age. However,
56 focusing on this age-group implies that our dependent variable, grade completion, is left censored. The
57 resulting estimates of the education spillover effects (using censored data) are likely to be biased downward;
58 without accounting for censoring in the data would yield a lower bound of the true spillover effects.
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3 individuals for whom we have data on their occupational status and socio-economic profile
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5 of their residential neighbourhoods.
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7 8 **4 Results and Discussions** 9

10 Table 1 presents OLS estimates of the determinants of grade completion using the MSEC
11 1996 data. The regressions are estimated over a sample of children aged 6-17 years¹⁰. The
12 regressors are age, sex, religion and birth order of the child; characteristics of the household
13 head (e.g. age, sex, education, occupation); education and age of the head's spouse and
14 household landholdings (homestead and cultivable)¹¹. We also control for various *bari*
15 characteristics such as landholdings (homestead and cultivable), total number of children
16 aged 0-5 years and 6-17 years and total number of adults (aged 18+ years) in the *bari*. It
17 may be recalled that the *bari* level variables discard all individuals in the index household
18 for whom the variable is being calculated. Hence, all the *bari* variables reflect the
19 neighbourhood effect net of own/household effect.
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34 All regressions include village “fixed effects” (FEs) which control for village level
35 variation in school quality. Regression results in Table 1 are reported separately for full, male
36 and female samples. For each sample, specification (1) refers to male-headed households
37 (MHHs) whereas specification (2) corresponds to female-headed households (FHHs)¹². Only
38 findings specific to our variables of interest are discussed below in detail. In the light of this,
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48 ¹⁰ Given this age range, censoring of the dependent variable remains an issue, either due to non-enrolment or
49 current enrolment (so that last grade completed is yet to be observed). To correct for potential bias due to non-
50 enrolment in school, one may estimate a sample selection model. However, in the absence of convincing
51 exclusion restriction, we have not pursued this route. Hence, our results need to be interpreted with a degree of
52 caution.

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

55 ¹² The separation of the full sample data by gender of the head is unavoidable because in almost 95% of cases,
56 female-heads do not have their spouse present in data (either because they are divorced or the male partner is
57 located outside the study area). Splitting sample observations by gender is important because there may be
58 gender differences in the ability to benefit from social interactions.
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we first discuss the findings on *bari*-related externalities, which are then followed by the results on within-household externality.

For Peer Review

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 & proximate	0.362 (11.01)**		0.329 (6.81)**		0.394 (8.86)**	
Head's spouse has grade 1-4 education & 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 & proximate		0.670 (7.80)**		0.880 (6.85)**		0.499 (4.28)**
Head has 1-4 grade & proximate		0.249 (1.55)		-0.059 (0.24)		0.513 (2.41)*
Birth order in <i>bari</i>	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 <i>bari</i>	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 <i>bari</i>	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 <i>bari</i>	-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 <i>bari</i>	-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 <i>bari</i>	-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 Fixed Effects	yes	yes	yes	yes	yes	yes
N	18906	3464	9701	1794	9205	1670

Note: Absolute value of t-statistics in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. 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 variables¹³; head's spouse and age of

¹³Since neighbourhood variables are missing for single-household *baris*, inclusion of such dummy variable essentially controls for single-household *baris* in our sample.

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3 *bari* children's education; landholding of the index household and other households in *bari*. **4.1 Main**
4 **results**

5 *Birth order in the bari*: This variable takes the value 1 for the eldest child in the *bari*. We
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7
8 find a significant positive coefficient of *birth order* for all samples, indicating that younger
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10 *bari* children tend to achieve higher grade completion. Although defined with respect to
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12 other children in the *bari*, *birth order within bari* potentially varies for every child within the
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14 family. Hence, we also estimated an alternative specification by fully controlling for (i)
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16 household-level fixed effects and (ii) *bari*-level fixed effects (results not reported). In both
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18 cases, the coefficient remains highly significant and positive. Its inclusion somewhat
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20 reduces the effect of birth order within the family indicating that part of the disadvantage of
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22 being the youngest in the family is off-set by the advantage of being younger among *bari*
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24 peers. To the extent that the potential role model effect is captured by the maximum grade of
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26 *bari* children in our model, the positive effect of *birth order within the bari* is attributable to
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28 the prospect of tutoring by older peers within the *bari*.
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34 *Mean education of peers*: Four key variables of interest in this study are mean peer
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36 schooling of boys and girls of their own cohort and that of the older cohort in the *bari* (all
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38 being potential proxies for endogenous social effects). Whilst all the four variables are
39
40 highly significant, those of particular interest relate to the effects of the older cohort
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42 variables. As discussed earlier, these serve as superior proxies for endogenous social effects.
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44 Turning to the gender effect, there is no statistical difference between the effects of mean
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46 education of male and female peers. The effect does not vary across the sample of boys and
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48 girls. Similarly, boy-boy and/or girl-girl interaction does not appear to be statistically
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50 different from boy-girl and/or girl-boy interaction.
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55 *Maximum schooling among peers*: Contrary to our expectation, maximum grade completed
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57 among *bari* children has a negative, albeit insignificant effect on individual grade
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3 completion. This is suggestive of the absence of any potential role model effect arising from
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5 the presence of peers in higher grades.
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8 *Bari adult female education:* The two dummy variables – the presence of at least one mother
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10 with grade 1-4 equivalent education and one with primary grade completion in the bari --
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12 have no significant effect. This result is not surprising since, a priori, neighbouring educated
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14 parents act as home teachers only if own parents are relatively less-educated. As an
15
16 alternative test, we re-ran the regressions by splitting the sample of children by educational
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18 status of the household-head (results suppressed). None of the bari female adult education
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20 variables were significant for the sample of households headed by educated males whereas
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22 for the sample of households with uneducated heads, presence of at least one (grade 1-4)
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24 educated mother has a strong positive effect¹⁴. This confirms our prior that the positive
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26 externality due to proximity of educated mothers in the neighbourhood exists but is
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28 exclusive to children of uneducated parents.
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34 *Intra-household education externality:* Turning to household specific variables, the
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36 coefficients on “*head’s spouse has no education*” and “*head’s spouse has grade 1-4*
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38 *education*” are negative and highly significant (Table 1; MHHs sample)¹⁵. This confirms
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40 that children of less-educated mothers have less schooling. However, coefficients on the
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42 interaction terms (i.e. “*head’s spouse has no education & proximate*” and “*head’s spouse*
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44 *has primary education & proximate*”) are positive and significant. This suggests that the
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46 disadvantage of being born to uneducated mothers is partially offset in presence of co-
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48 resident primary educated adults in the household. To be precise, children of *isolated*
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55 ¹⁴ Education of bari “non-mother female” adults still have no effect.

56 ¹⁵ Although the effect of mother’s education is not necessarily exogenous to the extent that male heads with
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58 higher taste for educated children marry educated female so that mother education merely captures unobserved
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60 taste for education of the head.

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3 mothers are worse than that of *proximate* uneducated mothers. This finding is robust to
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5 control for *bari* fixed-effects (regression results available upon request) and hence
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7 suggestive of “intra-household externality” in education. However, much of the intra-
8
9 household externality (as reported in our study) is likely to be specific to joint families.
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11 Using Indian data, Foster and Rosenzweig (2002) find considerable evidence that joint
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13 family children have higher educational attainment than those in nuclear families.
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15 Nevertheless, the sources of variation in the characteristics of these non-spouse adult
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17 members in extended families could be endogenous in educational production to the extent
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19 that the incident of co-residence (and hence the availability of additional educated persons)
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21 itself is driven by the demand for household specific public goods such as education (Foster,
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23 1998). Hence, care is needed in interpreting the finding on intra-household externality.
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29 **5 Additional Tests**

30 **5.1 Splitting Sample by Inter-Households Ties**

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32 It may be recalled that household-heads in *baris* are often related via kinship. The heads of
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34 all or a subset of households in a given *bari* may be related as brothers or father and sons or
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36 mother and sons (Rahman et al., 2001). Besides, it may be that a set of household-heads in a
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38 *bari* who are all brothers in 1996 (or two household-heads in 1996 are related as father and
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40 the son) have resided in the same household in the recent past, say, in 1982. Since these
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42 household partitions occurred after 1982 (the year for which we also have MSEC data), by
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44 1996 many of them may be retaining some of the earlier links that they enjoyed as members
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46 of same household. If so, the estimates of the *bari* social effect may simply serve as a proxy
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48 for the benefit of joint residence in a single household (such as sharing a public good like
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50 education). If so, the separation of households “linked” by their history of partition from the
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52 “non-linked” households allows a way to further minimise the kinship-related “correlated
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3 effects” (e.g. characteristics common to cousins) which may otherwise bias our estimate of
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5 bari social effects.
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8 Information on household partition is not directly recorded in our data. To identify
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10 and separate out these households from others (i.e. which have not split) within a given bari,
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12 we follow a rule adopted by Foster (1993). According to Foster, a household partition takes
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14 place if two individuals enumerated in a given household in period $t-1$ were observed to be
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16 household-head in period t . Hence, if two heads from the same bari in 1996 are found to be
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18 co-resident of a household in 1982, then we assume that household partition has taken
19
20 place¹⁶. Then we separate these recently partitioned households (whose heads were in the
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22 same households in 1982 but separated thereafter to form new households) from the rest of
23
24 our MSEC 1996 sample. The main sample purged of the “linked” households yields the
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26 sample of “non-linked” households. Note that household partition referred to above is
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28 different from the case of new headship, where there is merely a transfer of headship, for
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30 example, following the death of the earlier household-head unless another member from the
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32 same household co-exists in the same *bari* in 1996 but as the head of a separate household¹⁷.
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34 In addition, there may still be household partitions where parted household is located outside
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36 the *bari*. But we do not look at that as we are primarily interested in the relation between
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38 households within the *bari*, not outside.
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46 There are some notable differences in the characteristics of the “linked” vs. “non-
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48 linked” sample. “Linked” households appear to be headed by younger adults. For “non-
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50 linked” households, mean school completion of children is 2.46 compared to 1.93 for
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53 ¹⁶ This approach discards all households set up outside the study area i.e. Matlab Thana. These are treated as
54 events of migration.

55 ¹⁷ In this scenario, Foster (93) further distinguishes between “new household” and “inherited household”. If the
56 original head was still in the study area and lived in the household of one of the new heads, then that new head
57 is assumed to have inherited a household. Such a distinction between inherited and new households is not
58 made in our analysis for we do not focus on the impact of partition.
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3 “linked” households and the difference is statistically significant (p-value 0.00). Household-
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5 heads in “linked” sample also have less schooling. These differences are interesting for the
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7 following reason. Foster and Rosenzweig (2001), in their study of the determinants of
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9 household division in India, find that intra-household inequality in schooling increases the
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11 probability of household division. Conditional on mean and variance of schooling, increase
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13 in maximum schooling decreases the probability of a household split. The division of
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15 households thus contributes to a reduction in within-household inequality, but increasing
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17 differentials in inter-household average schooling. Hence, higher mean schooling attainment
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19 of children in our sample of “non-linked” households may be capturing the fact that these
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21 households are also more likely to have sustained as joint families compared to the sample
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23 of “linked” households (which are more likely to have experienced a dissolution and hence,
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25 nuclear).

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32 Regression results for “non-linked” and “linked” sample households are reported in
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34 Table 2. The respective *bari* variables for “non-linked” (“linked”) households are generated
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36 using information on all other households in the *bari* which are “non-linked” (“linked”) to
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38 the index household. For the sake of brevity, we report estimates focusing on the male
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40 head’s spousal education (instead of the own mother’s education). The first two columns in
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42 Table 2 correspond to “non-linked” sample, whilst the last two report results for the “linked”
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44 sample.

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48 Both the *bari* and village fixed-effects models for both samples yield significant
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50 evidence of intra-household education externality. However, on inter-household externality,
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52 all the *bari* variables related to female adult schooling are insignificant for the “non-linked”
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54 sample. The effects of mean schooling of *bari* boys and girls are highly significant and
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56 positive. Since this sample excludes “linked” household children, estimates for “non-linked”
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3 sample are less likely to be contaminated by common characteristics as would have
4 prevailed due to the presence of children related as first cousins and so on¹⁸. For the
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8 “linked” sample, education of *bari* mothers also exerts significant positive influence. These
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10 findings are consistent with Foster (1993). Using a sample from Matlab censuses 1974 and
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12 1982, Foster finds that recently partitioned households within *baris* are significantly linked:
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14 children’s education is affected by characteristics of other linked households in the *bari*
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16 which co-existed as joint-families in 1974. However, the distribution of resources within
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18 *bari* also matters, controlling for linked household fixed effects.
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56 ¹⁸ In our analysis children in linked (non-linked) households benefit from interactions with children from
57 linked (non-linked) households only. This imposes a restriction that non-linked (linked) household children are
58 outside the social space of children from linked (non-linked) households.
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Table 2: FE and OLS estimates of determinants of grade completion for “non- linked” and “linked” households (Children aged 6-17 years)

	“Non-linked”		“Linked”	
	FE	OLS	FE	OLS
Age	0.328	0.271	0.273	0.214
	(10.49)**	(8.42)**	(6.26)**	(4.78)**
(Age squared)/100	0.518	0.562	0.749	0.949
	(3.88)**	(4.17)**	(3.86)**	(4.87)**
Female	0.027	0.050	-0.089	-0.098
	(0.93)	(1.83)+	(2.18)*	(2.52)*
Birth order in family	-0.165	-0.153	-0.155	-0.142
	(8.80)**	(9.06)**	(5.50)**	(5.65)**
Education of Household-head	0.049	0.062	0.062	0.067
	(7.61)**	(11.46)**	(5.76)**	(7.98)**
Head’s spouse has no education	-0.916	-0.943	-0.531	-0.611
	(14.08)**	(17.15)**	(5.87)**	(8.22)**
Head’s spouse has grade 1-4 education	-0.379	-0.422	-0.184	-0.216
	(3.89)**	(5.09)**	(1.59)	(2.24)*
Head’s spouse has no education & proximate	0.367	0.402	0.302	0.280
	(7.77)**	(9.87)**	(3.77)**	(4.29)**
Head’s spouse has grade 1-4 education & proximate	-0.023	0.000	-0.014	-0.021
	(0.23)	(0.00)	(0.10)	(0.19)
Mean peer schooling, girls of own cohort		0.119		0.112
		(13.70)**		(8.21)**
Mean peer schooling, boys of own cohort		0.081		0.097
		(9.17)**		(7.30)**
Mean peer schooling, girls of older cohort		0.059		0.058
		(7.28)**		(4.22)**
Mean peer schooling, boys of older cohort		0.032		0.034
		(4.24)**		(2.55)*
Maximum schooling among peers		-0.034		-0.017
		(2.85)**		(1.02)
Birth order in <i>bari</i>		0.042		0.060
		(9.92)**		(7.76)**
Mean schooling of male adults in <i>bari</i>		0.043		0.029
		(5.50)**		(3.02)**
Grade 1-4 educated mother in <i>bari</i>		-0.011		0.124
		(0.34)		(2.58)**
Primary educated mother in <i>bari</i>		-0.009		0.048
		(0.25)		(0.94)
Primary educated adult female in <i>bari</i>		-0.048		-0.023
		(1.24)		(0.26)
Grade 1-4 educated female adult in <i>bari</i>		-0.004		0.157
		(0.09)		(1.60)
Adjusted R ²	0.51	0.59	0.50	0.60
<i>Bari</i> Fixed Effects	Yes	No	Yes	No
Village Fixed Effects	No	Yes	No	Yes
N	12729	12729	5166	5166

Note: Same as Table 1.

5.2 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 Appendix Table 2. With the inclusion of LnPCE and controlling for *bari* fixed-effects, uneducated spouse's proximity to primary educated non-spouse adult in the household continues to exert a positive significant effect on grade completion (specification 1). However, the effect becomes statistically insignificant once we treat LnPCE as endogenous (specification 2)¹⁹. It seems that externality benefit of having educated non-spouse 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 regressors. 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.

¹⁹ 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.

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3 We further investigate the above results by re-estimating our model separately for
4 the sample of “non-linked” and “linked” households (see Appendix Tables 4 and 5). For the
5 “non-linked” sample, mean schooling of children and adults in the bari continue to exert a
6 significant positive influence. Particularly reassuring is the significance of the effect of the
7 older cohort of boys and girls. Turning to intra-household externalities, we also find a
8 significant effect of uneducated mother on their children’s education when they co-reside
9 with primary educated non-spouse adults in the household. However, evidence supporting
10 within-household education spillover is absent in the sample of “linked” households.
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21 **5.3 Additional Test of Inter-household Education Externality**

22 The evidence of neighbourhood effect, even if robust to host of controls for household and
23 neighbourhood attributes and composition, could arguably be driven by common exposure
24 of children in same neighbourhoods to public education programs. Two such programs are
25 food-for-education (FFE) and female secondary stipend (FSS) which have been instrumental
26 in increasing school participation in rural Bangladesh in recent years. However, both of
27 these programs were introduced during the 1990s. Looking at school attainment of adults
28 therefore provides a way to circumvent this problem as they were not exposed to any of
29 these interventions. Any evidence of peer effect for the adult sample therefore cannot be
30 attributed to common participation in public education programs.
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45 To this end, we estimate a model of wage work participation (via IV-Probit
46 regressions) for a sample of adults aged 20-32 years where schooling is treated as an
47 endogenous regressors. Peer effect is investigated in the first stage regression where
48 retrospective measures of bari schooling serve as instruments for an individual’s educational
49 attainment. The regressions also control various individual characteristics such as age, age
50 squared, religion and gender. Additionally, household landholding is included as a regressor.
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3 We use four lagged variables as instruments for years of schooling: (i) mean peer schooling
4 of boys in the own cohort, (ii) mean peer schooling of girls in the own cohort, (iii) mean
5 peer schooling of older cohort and (iv) mean schooling of adults (i.e. individuals aged 18-65
6 year olds) in the bari. These variables were calculated using MSEC 1982 data.
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12 Using the bari characteristics as instrumental variables for the endogenous education
13 variable in the wage participation regression is somewhat similar to the practice of using
14 family background variables as IV for grade completion in an individual wage regression.
15 There is ample application of such instruments in the economics literature on returns to
16 education. As with family background variables, the exogeneity of bari variables as IVs is
17 not always obvious since these instruments are not generated by any natural or quasi-natural
18 experiments. Given these potential problems, we do not claim that the IV estimates reported
19 in this section decisively solve the endogeneity problem of schooling variable in wage
20 participation. It is simply meant to be illustrative of the hypothesis that bari schooling in
21 early childhood generates *spillover* effects which are reflected in greater labour force
22 participation in later years via increased schooling attainment.
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Table 3: Probit and IV-Probit estimates of determinants of wage-work participation (Adults aged 20-32 years)

	Probit	IV-Probit	1st 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 <i>Bari</i> 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

Note: Absolute value of t-statistics in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. The shaded area indicates variables that have been treated as endogenous and subsequently, instrumented. All the regression estimates correspond to marginal effects (instead of regression coefficients). Regression intercept suppressed.

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 regressor proposed by Smith and Blundell (1986)²⁰. Rejection of the null indicates that schooling is endogenous and hence, the IV-probit is preferred over the

²⁰ 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.

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3 standard probit estimator. Both probit and IV-probit estimates yield positive and significant
4 effects of the schooling variable. All the other regressors have usual signs. The highly
5 significant effect of the four lagged schooling variables in the first stage is consistent with
6 our earlier evidence of significant positive effect of bari schooling on children's grade
7 completion.
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10 11 12 13 14 15 **6. Conclusion**

16
17 This paper has provided a detailed account of social determinants of children's school
18 completion in rural Bangladesh. We have tested for the presence of inter-household
19 externalities in children's grade completion within rural residential neighbourhoods in
20 Bangladesh. We find some evidence of inter-household externalities: mean schooling of
21 boys and girls in the *bari* raise grade attainment of an individual child. These effects are
22 significant and large irrespective of whether we focus on the mean schooling of peers of
23 own or older cohorts in the *bari*.
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34 There are three pieces of evidence that espouse social effect as an explanation for the
35 neighbourhood effects reported in this study. First, the results remain unchanged even when
36 we account for neighbourhood composition. Households in a *bari* are often potentially
37 linked in terms of their recent history of partition. Recently partitioned households often
38 maintain significant socio-economic ties so that their presence in the data is a potential
39 source of "correlated" (non-social) effects which masks genuine social effect. Yet,
40 regression results yield significant coefficient on mean schooling of own and older cohorts
41 of peers in the *bari* even when we purge our sample of the "linked" households. This finding
42 remains unchanged to further control for household expenditure. Second, the finding of
43 positive peer effect is robust to control for host of household and neighbourhood
44 characteristics. In particular, we allow for endogeneity of household expenditure by re-
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3 estimating schooling regressions in an instrumental variable framework. However, this does
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5 not drive out our result of inter-household externality.
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8 Finally, we demonstrate that the evidence on peer effect is not driven by educational-
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10 reform related “correlated effects” e.g. all children from some neighbourhoods benefit from
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12 public interventions, such as the FFE program. To this end, we test for peer effects for an
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14 older cohort of individuals who completed schooling at a time when these educational
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16 interventions were non-existent. Using a *sample of adults* for whom retrospective records on
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18 their childhood residential neighbourhoods is available, we test whether mean education of
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20 peers during their school years affects labour market participation decisions via increasing
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22 school attainment of an index adult. First, we estimate a simple model of wage work
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24 participation, where an individual’s educational attainment is endogenous. Subsequently, we
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26 test the hypothesis that the mean schooling of peers in the *bari* during an individual’s
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28 childhood are valid and strong instruments for own schooling. Our analysis confirms this
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30 hypothesis. The significance of peer variables in the first-stage regression of schooling, thus
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32 once again suggest the presence of a social effect (externality) originating from the
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34 schooling of peers in the neighbourhood. This lends further support to the evidence of peer
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36 effects in children’s schooling for the *sample of children* currently of school age in our data.
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43 In addition to the evidence of inter-household education externalities, we find
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45 considerable evidence of intra-household externality enjoyed by children of uneducated
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47 mothers due to their proximity to primary educated adults in the household. This evidence is
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49 sufficiently robust to fully control for neighbourhood fixed effects; within-household
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51 externality is larger when the male spouse is also uneducated. Our finding of within-
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53 household externality is particularly important in rural Bangladesh, where the overall
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3 literacy rate (among those aged 7 years and above) is as low as 44.3 per cent (in 1995) and
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5 28.5 percent for females.
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8 The suggestive evidence presented in this paper has important policy implications
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10 for education policy design in developing countries. The evidence of intra-household
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12 externality – children of uneducated parents are better off when co-residing with educated
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14 non-parent adults in the same household.-- implies that traditional public interventions (such
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16 as cash subsidies, stipends, fee waivers and so on) to attract and retain the “difficult-to-
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18 reach” at risk children from poor households to schools may be complemented by more
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20 longer-term policy of improving the schooling levels of the adult population. This can be
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22 accomplished via educational investment in low skilled parents, particularly uneducated
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24 mothers. Any externality arising from such investments are, nevertheless, likely to be
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26 captured by own children only, which may not benefit neighbouring children of school age,
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28 as evidenced from the insignificant effect of educated adult (female) neighbours in the *bari*
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30 on children’s schooling. The same is, however, not true if a child within the neighbourhood
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32 is targeted for an educational intervention. Policies designed at increasing schooling of one
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34 child (in educationally deprived neighbourhoods) has important spillover effects on
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36 schooling outcomes of other children in the *bari* with a feedback to further boost schooling
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38 of the targeted student. Our evidence of significant positive effect of mean schooling of
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40 neighbourhood children on individual grade completion supports this hypothesis.
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48 To conclude, traditional public interventions (such as conditional cash transfers, fee
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50 waivers and so on) to attract and retain a child in school is also likely to benefit other
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52 neighbouring children of school age. In other words, policies designed at increasing
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54 schooling of one child in educationally deprived neighbourhoods has important spillover
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56 effects on schooling outcomes of other children in the *bari* with a feedback to further boost
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3 schooling of the targeted student. Our evidence of significant positive effect of mean
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5 schooling of *bari* children on individual grade completion supports this hypothesis. That
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7 said, clearer evidence of the social effect is necessary to ascertain whether the observed
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9 effect, indeed reflects endogenous social effects, as opposed to contextual effects. This is a
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11 challenge that future research on social interactions in educational production should
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13 address for developing countries.
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29 disclaimers apply.
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**Appendix Table 1: 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 & 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 & 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 & 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
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 <i>bari</i> children (aged 6-17)	6.24	2.31
Birth order in <i>bari</i>	Birth order of the index child among peers in <i>bari</i>	8.51	9.70
Mean schooling of adults in <i>bari</i>	mean schooling of <i>bari</i> adults	3.14	2.06
Mean schooling of male adults in <i>bari</i>	mean schooling of <i>bari</i> adult males	3.93	2.54
Grade 1-4 educated mother in <i>bari</i>	=1 if at least one 1-4 grade educated mother present in <i>bari</i>	0.52	0.50
Primary educated mother in <i>bari</i>	=1 if at least one 5+ grade completed mother present in <i>bari</i>	0.55	0.50
Primary educated adult female in <i>bari</i>	=1 if at least one 1-4 grade educated <i>bari</i> non-mother female ⁽¹⁾ present	0.19	0.40
Grade 1-4 educated female adult in <i>bari</i>	=1 if at least one 5+ grade completed <i>bari</i> non-mother female present	0.14	0.34
Number of children, 0-5	Total no. of 0-5 years olds in <i>bari</i>	7.62	8.88
Number of children, 6-17	Total no. of 6-17 years olds in <i>bari</i>	16.41	15.49
Number of children, 18+	Total no. of 18 years or older individuals in <i>bari</i>	27.03	25.24
Mean homestead land of <i>bari</i>	Average homestead land owned by other HHs in <i>bari</i> (in decimals)	10.36	10.00
Mean cultivable land of <i>bari</i>	Average cultivable land owned by other HHs in <i>bari</i> (in decimals)	59.74	69.40
N		22370	

Note: (1) Sample excludes heads aged below 22 years to separate non-students. (2) Non-mother females are female adults (aged 18+) in *bari* with no own children present in their respective household.

Appendix Table 2: Neighbourhood fixed-effects 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 & proximate	0.342 (9.19)**	0.506 (8.89)**
Head's spouse has grade 1-4 education & proximate	0.009 (0.12)	0.187 (1.12)
Adjusted R ²	0.52	0.35
Bari Fixed Effects	Yes	Yes
N	18906	8856

Note: Absolute value of t statistics in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. Regression constant has been suppressed. All the specifications include bari fixed effects and dummy for missing data of mothers/male head's spouse.

Appendix Table 3: OLS, FE and IV estimates of determinants of grade completion of children using linked MHSS-Census sample (MHHs only)

	(1)	(2)	(3)	(4)	(5)
	FE	IV-FE	OLS	IV	1 st 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 & 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 & 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 Fixed Effects	Yes	Yes	No	No	No
Village Fixed Effects	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

Note: Absolute value of t-statistics in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. 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.

Appendix Table 4: OLS, FE and IV estimates of determinants of grade completion of children using linked MHSS-MSEC sample “non-linked” households (MHHs only)

	(1)	(2)	(3)	(4)	(5)
	FE	IV-FE	OLS	IV	1 st 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 & 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 & 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 Fixed Effects	Yes	Yes	No	No	No
Village Fixed Effects	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

Note: Absolute value of t-statistics in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. 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.

Appendix Table 5: OLS, FE and IV estimates of determinants of grade completion of children using linked MHSS-MSEC sample “linked” households (MHHs only)

	(1)	(2)	(3)	(4)	(5)
	FE	IV-FE	OLS	IV	1 st 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 & 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 & 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 Fixed Effects	Yes	Yes	No	No	No
Village Fixed Effects	No	No	Yes	Yes	Yes
Exogeneity test of LnPCE	-	.11	-	.068	-
Over-identification test	-	.85	-	.015	-
N	1347	1347	1347	1347	1347

Note: Absolute value of t-statistics in parentheses. + significant at 10%; * significant at 5%; ** significant at 1%. 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.