Access to Credit, Education, and Women's Say in the Household: Evidence from Bangladesh

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ABSTRACT

A substantial literature on women's say in the household focuses on microcredit, but there is little evidence on the relative roles of credit and education. Using household survey data from Bangladesh, we provide a comparative analysis of the effects of education and microcredit on women's decision making power in the household. We implement two econometric approaches: bias adjusted OLS estimator of Oster (2019) that extends the Altonji et al. (2005) approach where selection on observables is used as a guide to selection on unobservables, and doubly robust radius matching estimator of Lechner et al. (2011). The evidence suggests a limited impact of microcredit, consistent with the recent evidence from RCT based studies. In contrast, education is much more important for enhancing women's say in a range of household decisions. There is no significant interaction effect between education and credit. Evidence from Gelbach decomposition suggests that outside employment is an important mediating mechanism, but household wealth and assortative marriage matching on education are not important. The impact of education on women's decision making remains strong even after controlling for these mediating factors, pointing to the importance of other mechanisms such as self-confidence and better negotiation skills of educated women.

Key Words: Women's Empowerment, Household Decision Making, Women's Education, Microcredit, Bangladesh

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Introduction

Bias against women in the family, labor market, and broader society is common in many developing countries. Son preference, sex selective abortion, unequal inheritance laws and customs, restrictions on social and geographic mobility are some of the widely known examples. Ensuring equality of women has been an important development goal in recent decades (Sen 1999; Duflo 2012; Kabeer 2005). Women constitute a disproportionate share of poor and vulnerable people and usually have little say in the household decision making, especially in patriarchal societies.

There is a widely-held view that educating women is among the most important policy instruments for achieving gender equality in the household, market, and social interactions (World Development Report 2012; Sen 1999). There are a number of plausible reasons to expect that higher schooling would affect the decision making power of women within the household. Educated women usually earn higher income which reduces their economic dependence, and makes the threat of divorce (or non-cooperation within the marriage) more credible when facing conflict of preference with husbands or in-laws. Education also makes women more confident, articulate, and better prepared to argue for their preferences and points of view. Amartya Sen writes, "[education] can add to the value of production in the economy and also to the income of the person who has been educated. But even with the same level of income, a person may benefit from education in reading, communicating, arguing, in being able to choose in a more informed way, in being taken more seriously by others and so on" (Sen 1999, p. 294). A woman with more schooling is less likely to be in an arranged marriage without her consent, which may result in better matching and more

balance of power in the household.[2] Another important aspect of education compared to other assets such as land and non-farm business is that it is inalienable, and a husband or in-laws cannot strip her education when a serious conflict arises (Eswaran 2014).

Although a growing literature has focused on identifying factors that can improve women's say in the household, a primary focus has been on the role played by access to microcredit and working outside home (Eswaran 2014; Hashemi et al. 1996; Kabeer 2001; Anderson and Eswaran 2009). The argument that microcredit may improve women's empowerment (or autonomy) is based on the following observations.[3] Women, specially in developing countries, lack access to formal credit markets, which may constrain entrepreneurship and economic independence. Microcredit with its focus on women as the borrowers partly redresses the gender bias in the credit market. Second, standard microcredit programs such as Grameen and BRAC in Bangladesh also incorporate women's empowerment as goals. Third, in group based programs, women can rely on group members for help and advice when there is conflict with husbands or other family members such as mothers-in-law.

Empirical evidence on the effects of microcredit membership on women's decision making power within the household is conflicting. Some researchers find that microcredit enhances women's earning capability, which in turn contributes to their empowerment (Hashemi et al. 1996; Kabeer 2001), while others do not find any positive impact (Goetz

[2]Banerji et al. (2013) and Emran et al. (2014) find that education reduces the probability of arranged marriage for girls in India and Vietnam, respectively. Koenig et al. (2003) provide evidence that educated women in Bangladesh are less likely to be victim of domestic violence.

[3]We use "empowerment" and "autonomy" as interchangeable. However, some authors make a distinction between them; see, for example, Dixon-Mueller (1998). Our interpretation of "empowerment" is closer to that of Kabeer (1999) who emphasizes empowerment as the processes by which those who have been denied the capacity for choice gain this capacity.

and Gupta 1996; Garikipati 2008; Bajracharya and Amin (2013); Banerjee 2013). Home-based activities financed by microcredit NGOs may not be effective in improving women's position in the household, because they are treated as "invisible income", similar to much of women's unpaid work in home goods production and on the farm. However, working outside the home facilitated by education may be of special importance for women's independence and better bargaining power in the household (Sen 1999; Anderson and Eswaran 2009; Kabeer 2016).[4]

This paper has three goals. First, to understand the relative roles of access to credit and education in improving women's decision-making power in the household. To the best of our knowledge, this is the first study to provide comparative estimates of the effects of education and access to microcredit on women's say in the household in a developing country, Bangladesh. Second, we test for possible interaction effects between them: are they complementary, substitutes, or separable?[5] One can make a plausible argument in favor of complementarity between education and credit. The effectiveness of microcredit may depend on the education of a borrower, because a more educated woman is likely to be a better manager of a microenterprise and a more efficient investor. Third, we explore the role of alternative mechanisms including the three most widely discussed in the literature: working outside the home, household wealth, and assortative matching in marriage in mediating the impacts. If the estimated impacts remain robust after controlling for these salient economic mechanisms, it can be interpreted as (indirect) evidence of the importance of other more intangible mechanisms such as self confidence and negotiation skills of an

[4]Kabeer (2016) provides a survey and synthesis of the studies on the impacts of paid and unpaid work on women's empowerment in Bangladesh.

[5]Even when they are separable, omitting one factor may cause omitted variables bias in the estimated effects.

educated women, as noted by Sen (1999).

We develop an empirical strategy to analyze the effects of access to credit and higher education with a focus on possible interaction effects and implement it to understand women's position in the household in Bangladesh using data from Demographic and Health Survey (DHS) 2011. It is difficult, if not impossible, to design randomized interventions or find quasi experimental exogeneous variations that can isolate the effects of credit and education, with possible interaction effect between them. We take advantage of a rich set of recent econometric approaches that tackle biases in OLS estimates due to unobserved ability and preference heterogeneity without imposing exclusion restrictions required in an instrumental variables approach. We use the doubly robust radius matching estimator developed by Lechner et al. (2011) that makes the treatment and comparison groups more comparable using both propensity score matching and regression adjustments. The evidence on balance between the treatment and comparison shows that matching plus regression adjustments eliminate much of the imbalance observed in the simple means. This approach also allows us to test for possible complementarity between access to credit and higher education without imposing any functional form. More importantly, we implement the bias-adjusted OLS estimator developed by Oster (2019) that extends the approach originally due to Altonji et al. (2005). Following Altonji et al. (2005), Oster (2019) uses selection on observables as a guide to selection on unobservables, and provides a method to estimate consistently the omitted variables bias, which, in turn, yields a bias-adjusted estimate net of selection on unobservables.[6]

[6]The existing evidence on the impact of microcredit on women's empowerment in Bangladesh primarily relies on the OLS estimator, and thus, the estimates are likely to be substantially biased because of selection on unobservables.

The estimates from alternative econometric approaches deliver a set of robust conclusions. The evidence suggests a limited impact of access to microcredit on Bangladeshi women's decision making power in the household.[7] This finding is consistent with the evidence from RCT based studies of microcredit in other developing countries.[8] The effects of higher education, in contrast, are positive and significant (at the 5 percent or less levels) for all types of decisions considered: from monetary decisions such as large purchases to the more knowledge-intensive decisions such as children's health care. The effects of education are substantial in magnitude: a woman with primary or more schooling is 8%-11% more likely to have a say in these household decisions compared to a woman without primary schooling. There is no evidence of complementarity between education and credit.

We explore the mechanisms behind the strong impacts of education. The evidence from Gelbach (2016) decomposition suggests that working outside home is an important mediating mechanism for all the decisions considered. Household wealth plays a mediating role only in children's health care decisions, and marriage matching is not significant as a mechanism for any of the decisions. Perhaps, more importantly, the effects of education on the decision making of women remain both numerically substantial and statistically significant (at the 5 percent level) when we include husband's characteristics (as proxies for assortative matching), an indicator of household's wealth status, and a dummy variable indicating whether a woman works outside the home. This can be interpreted as evidence

[7]The evidence suggests that microcredit does not have a significant impact in three out of four decisions we analyze.

[8]See the discussion by Banerjee (2013). The fact that the evidence on the effects of microcredit from our research design reaches conclusions similar to the RCT based studies is reassuring, and suggests that our approach deals adequately with the biases in the OLS estimates. The OLS estimates suggest positive and significant effects of credit consistent with upward bias because of omitted ability and preference heterogeneity.

that education enhances women's decision making power through other channels such as self-confidence, better negotiation skills improving her bargaining power, and comparative advantage in knowledge-intensive decisions.

The rest of the paper is organized as follows. Section 2 provides a discussion of the conceptual issues related to the effects of education and access to credit on women's place in the household. The next section lays out the empirical strategy for estimating the effects of higher education and access to credit on women's decision making power. Section (4) discusses the data and the variables. The main empirical results on the effects of credit versus education are reported in section (5). Section (6) provides a formal test of the null hypothesis that education and credit are separable in determining women's say in the household. We explore the mechanisms in section (7) including the evidence from Gelbach (2016) decomposition. The paper concludes with a summary of the main findings.

2. Conceptual Issues

The literature on women's decision making in the household uses survey questions on decisions regarding own and children's health, geographic mobility, and household purchases (daily needs and large purchases), among other things. It may be useful to consider two channels through which education and credit may affect the decision making role of women in the household. First, it may affect the bargaining power (threat utility) of women, and second, it may affect comparative advantage in making a decision.

The focus of the recent literature has been on the women's threat utility in a bargaining model (Agarwal 1997, Eswaran 2014). The quality of decision making remains a neglected aspect, not considered in many recent papers on women's autonomy in household decision making. It is important to appreciate the role of comparative advantage in this context

as some decisions may be categorized as relatively "knowledge intensive". For example, decisions regarding health are likely to be more "knowledge intensive" in the sense that a more educated person will be able to make a better decision. This implies that a more educated woman may have greater say in these cases even in the absence of any change in her income and bargaining power. In contrast, spending decisions about large (durable goods) purchases may primarily depend on who earns the money and the bargaining power that comes with economic independence. Thus, access to credit is more likely to affect spending decisions when credit financed activities lead to higher income for the borrower women. Another widely used indicator of a woman's bargaining power relates to whether she needs permission for geographic mobility and social interactions.[9] In the DHS 2011 data set, women were asked whether they needed permission for visiting her own family and friends. Decisions regarding mobility are not complex and there is no obvious reasons to expect that a better educated woman would have any comparative advantage in taking such decisions. However, they might be particularly good indicators of a woman's position in the household as she is likely to place a premium on visiting her natal family and conflict of preference between husband and wife may be sharp in this case.[10]

There are two important distinctions between access to credit and education when the focus is on the threat utility of women (compared to her husband or partner). First, education is inalienable, but a business built with microcredit can be expropriated when there is a conflict (or in the event of divorce). Expropriation of property rights to land

[9]Bloom et al. (2001) find that the women with freedom of geographic mobility in India obtained higher levels of antenatal care and were more likely to get safe delivery care.

[10]In an analysis of women's decision making in the households in Sri Lanka, Malhotra and Mather (1997) find that education and employment affect women's say in financial decisions, but have little impacts on the decisions relating to social interactions.

and other assets of women is unfortunately common in a developing country with weak property rights enforcement such as Bangladesh. This implies that the effects of education may be more important than the direct effect of income and assets on the bargaining power of a woman. Second, education is a salient factor in marriage market matching, and more educated women are likely to be married to more educated men. In so far as a more educated husband is more likely to treat his wife better, educated women will benefit from assortative matching on education in the marriage market. Thus, the effects of education include the assortative marriage channel.

3. Empirical Strategy

The literature on the effects of microcredit on women's decision making (empowerment) in the household uses a variant of the following empirical model:

$$D_i^j = \alpha_0 + \alpha_1 M_i + X_i^{'} \Gamma + \varepsilon_i, \tag{1}$$

where D_i^j is a dummy variable taking on the value of 1 when woman i exercises her preference in decision j , M_i is a dummy variable indicating whether she is a microcredit borrower, and X_i is a vector of individual, family, and village characteristics that can affect both the selection into microcredit and women's bargaining power in the household.

When the focus is on estimating the effects of education on women's decision making in the household, one can estimate a similar empirical model:

$$D_i^j = \beta_0 + \beta_1 E_i + X_i^{'} \Phi + \epsilon_i, \tag{2}$$

where E_i is an indicator of the education level of woman i.

Although it is widely argued that both education and access to credit may enhance women's standing in the household and beyond, most of the existing discussions implicitly assume that there is no interaction effect between them. A more general specification suited for understanding the effects of education and access to credit on women's decision making power within the household can be written as:

$$D_i^j = \theta_0 + \theta_1 M_i + \theta_2 E_i + \theta_3 \left(M_i * E_i\right) + X_i^{'}\Pi + \xi_i. \tag{3}$$

If education and access to credit are complementary (substitutes) in strengthening women's say in the household, then we expect $\theta_3 > 0 (< 0)$. Estimating Equation 3, however, poses challenges because of unobserved heterogeneity in ability and preference. The existing literature tries to take advantage of randomized credit interventions or credible policy experiments (such as changes in compulsory schooling) to identify the effects of credit and education separately. However, finding policy experiments or designing an intervention that can create clean exogenous variations powerful enough to identify the effects of both education and access to credit is a daunting task, and may not be feasible in most cases.[11] As noted by Ravallion (2009) and Elbers and Gunning (2014), estimating the effects of unidimensional interventions (either credit, or schooling) may be misleading in ranking alternatives if there are significant interaction effects.

In the empirical analysis, we use a binary indicator of education and split the sample in

[11] Card (1999) notes that randomized interventions face especial challenges in education, because it is impossible to design an intervention that increases schooling of a random student by one year. Also, credit intervention would need to wait many years after the intervention in school which makes such a research project extremely difficult. We are not aware of any randomized interventions designed in this way.

four different groups defined by the pair of binary indicators (M_i, E_i) ; they are $(0,0)$, $(0,1)$, $(1,0)$, $(1,1)$. For example, the group $(0,1)$ comprises of women who are not microcredit borrowers $(M_i = 0)$ and have higher education $(E_i = 1)$. With these mutually exclusive four groups, we can rewrite equation (3) as follows:

$$D_i^J = \delta_0 + \delta_1 D^{01} + \delta_2 D^{10} + \delta_3 D^{11} + X_i' \Pi + \nu_i, \tag{4}$$

where the group (0.0) is the comparison group comprising of women who are low educated and not microcredit members (as it is in equation (3) above), and there are three target group dummies defined for the other three groups. In this framework, complementarity implies $\delta_3 > \delta_1 + \delta_2$, substitutability $\delta_3 < \delta_1 + \delta_2$, and separability $\delta_3 = \delta_1 + \delta_2$. Perhaps the most important advantage of this approach is that we can implement the doubly robust matching estimators developed in the evaluation literature for a binary treatment.[12] As a robustness check, we provide estimates using years of schooling as an indicator of education.

We use a recently developed doubly robust matching estimator that helps reduce the biases in estimates with non-experimental data. In particular, we implement the distance weighted bias corrected radius matching (henceforth BC-RM) due to Lechner et al. (2011). The BC-RM combines the following: (i) weighting of the matched controls within the radius according to their distance to the treated observation, (ii) bias adjustment based on OLS or logit regression depending on the support of the outcome variable (doubly robust), (iii) partially data-driven choice of the radius size as a function of the distances in pair matching

[12]One might argue that we are "throwing away" information by dichotomizing the schooling variable. It is important to appreciate that a continuous treatment places a much higher demand on the data. In the context of evaluating microcredit programs, Morduch and Roodman (2014) provide an excellent discussion on this point, and argue for using a binary indicator of MFI membership instead of the amount of loans as the treatment variable.

and (iv) asymptotically unbiased propensity score trimming to ensure common support in the propensity score across treatment groups.

An advantage of the doubly robust matching estimator in our application is that we can test for the existence of an interaction effect without imposing an arbitrary functional form. To appreciate this, consider a more general specification of the empirical model that allows for both education and credit effects:

$$D_i^j = F\left(M_i, E_i, X_i\right). \tag{5}$$

Assuming that the function $F\left(.\right)$ is twice differentiable, we have the following:

$$\frac{\partial^2 D_i^j}{\partial M_i \partial E_i} \begin{cases} > 0 \quad if\ complementary \\ \\ < 0 \qquad if\ substitutes \end{cases} \tag{6}$$

We can estimate a discrete analog of the cross-partial derivative in inequality (6) above using matching estimators that do not impose any functional form.

The BC-RM estimator relies on the maintained assumption that conditional on the observables included in the model, there are no significant unobservable factors that affect both the selection into the treatment and the outcomes. We relax this assumption by implementing the bias-adjusted estimator due to Oster (2019) that builds on the earlier influential work of Altonji, Elder and Taber (2005) (henceforth AET 2005). The AET (2005) method uses selection on observables as a guide to selection on unobservables, and provides conditions under which a lower bound on the causal effect can be estimated without imposing any exclusion restrictions. The lower bound in AET (2005) is derived under two

assumptions: (i) selection on unobservables is equal to selection on observables and (ii) the R^2 of a hypothetical regression that includes all the relevant variables (observables and unobservables) equals 1, which excludes the possibility of measurement error in the outcome variables or idiosyncratic shocks. A limitation of the AET (2005) approach is that, in a linear OLS regression, it is not possible to calculate a bias adjusted treatment effect. Oster (2019) relaxes the second assumption and, more importantly, provides a way to calculate the bias-adjusted treatment effect for the OLS estimator.

Consider the following modified model, corresponding to equation (4) above:

$$D_i^J = \delta_0 + \delta_1 D_i^{01} + \delta_2 D_i^{10} + \delta_3 D_i^{11} + X_i^{'}\Pi + Z_i^{'}\Phi + \zeta_i, \tag{7}$$

where X_i denotes the vector of observables, and Z_i is the vector of unobservables, ζ_i is the error term capturing measurement error and idiosyncratic shocks, and, as before, the vector of treatment dummies are denoted by D_i. The relationship between selection on observables and selection on unobservables can be written as:

$$\frac{Cov\left(D_i, Z_i^{'}\Phi\right)}{Var\left(Z_i^{'}\Phi\right)} = \mu \frac{Cov\left(D_i, X_i^{'}\Pi\right)}{Var\left(X_i^{'}\Pi\right)}. \tag{8}$$

As discussed by AET (2005) and Oster (2019), it is plausible to take $\mu = 1$ as the upper-bound in most of the cases because the surveys are designed to collect data on the salient determinants of household and individual behavior, and as a result, the observables are likely to dominate the unobservables, implying that $\frac{Cov\left(D_i, X_i^{'}\Pi\right)}{Var\left(X_i^{'}\Pi\right)} \geq \frac{Cov\left(D_i, Z_i^{'}\Phi\right)}{Var\left(Z_i^{'}\Phi\right)}$. In our context, for example, the survey collected data on the land ownership of the household, which is used as a selection criterion by most of the microcredit programs in Bangladesh.

Since land ownership is an important indicator of household wealth, it will capture scarcity in a household which can sharpen the conflicts in decision making. The survey also has information on the availability of primary and secondary schools in a community which are important factors in schooling decisions of girls in Bangladesh.

AET (2005) assumes that the value of R^2 in the hypothetical regression (7) , denoted as R^2_{max}, is equal to 1, i.e., $R^2_{max} = 1$, which does not allow any role for measurement error or idiosyncratic shocks. Oster (2019) extends the approach by assuming that $R^2_{max} < 1$. In Oster's analysis, the value of R^2_{max} is bounded by the estimated R^2 from the data, denoted as $\tilde{R^2}$, from the model in equation (4) above. Based on an analysis of a set of published articles using randomized experiments, she suggests a value of $R^2_{max} = 1.3\tilde{R^2}$ that validates the estimated treatment effects in 90% of the experiments. Oster (2019) develops a consistent estimator of the omitted variables bias and a bias-adjusted OLS estimator under the conditions that $\mu = 1$ and $R^2_{max} = 1.3\tilde{R^2}$. This allows us to provide a bias-adjusted OLS estimate that corrects for selection on unobservables. As a conservative strategy, we will also check the robustness of the conclusions allowing for a larger role for selection on unobservables, considering higher values of μ, up to $\mu = 2$.

4. Data and Variables

The data used for the empirical analysis come from the nationally representative Bangladesh Demographic and Health Survey (BDHS) 2011. The enumeration areas (EAs) of the 2011 population census provided by Bangladesh Bureau of Statistics (BBS) constitute the sample frame for the survey. The 2011 BDHS is a two-stage stratified sample of households. In the first stage, 600 EAs were selected with probability proportional to the EA size, with 207 in urban areas and 393 in rural areas. In the second stage, a systematic sample of about

30 households was selected from each EA. The survey covers a total of 17,141 households from which 17,842 ever-married women aged 13 to 49 were interviewed.

The survey is conducted using four main questionnaires: household, women's, men's, and community questionnaires. The data used in this paper come from the household, women's and community questionnaires. The household questionnaire is designed to collect information on individual characteristics such as his/her age, sex, education, and relationship to the head of the household, and household characteristics such as source of water, type of toilet facilities, materials used to construct the house, and ownership of various consumer goods etc. The women's questionnaire collects information on the respondent's participation in household decision-making, her age, education, religion, reproductive history, use of contraceptive methods, antenatal and delivery care, membership in income generating NGOs, and husband's background. The community questionnaire collects information such as the existence of development organizations, availability, and accessibility of health care and family planning services, main economic activities, and access to roads and electricity.

Our analysis focuses on rural households. Some of the interviewed women are not members of the respective households, and we drop those observations as the household and community characteristics may not be relevant to them. We also drop those women who are not currently married; where the husband's age is missing or where the husband is 25 or more years older than the wife; where the wife is older than the husband; where the respondent's relation to the household head is something other than head herself, wife, daughter or daughter-in-law; or where the household head's age is less than 20 years. That leaves us with a final sample of 9,373 observations. Sample sizes are not the same in all

the estimated equations owing to missing values of the outcome variables.

Our focus is on women's participation in household decision making. In the survey, women are asked "who has the final say" on "respondent visiting her family and relatives," "respondent's own health care," "child health care," and "large household purchase".[13] Responses are "respondent alone," "respondent jointly with husband or others" and "husband or someone else". We construct an indicator variable for each of the decisions that takes on 1 if the respondent woman takes the relevant decision alone or jointly with husband and/or others, and 0 otherwise.

The main regressors of interest are two binary variables - access to credit (M) and education (E). Access to credit takes on 1 if the respondent is a member of any microfinance institution (MFI) such as Grameen Bank, BRAC, ASA, PROSHIKA or other income generating NGOs, and 0 otherwise. Primary schooling is used as the cut-off for education; the education dummy takes on 1 if the respondent has 6 or more years of schooling and 0 otherwise.[14] In all of the regressions, we include a set of individual, household and community characteristics to take into account selection on observables. We include the age of the respondent and age-squared. The effect of age is not unambiguous from a priori considerations. An older woman has more experience as well as a better understanding of her role in the family that can positively affect her participation in decision-making. On the other hand, an older woman may value the traditional role of women, leaving the decision-making

[13]The survey also asked the women about contraceptive choice. We do not include it in the analysis for two reasons. First, 93 percent of women in our data have say in making contraceptive choices suggesting there is little gender bias in this instance. Second, the sample for analysis becomes much smaller when we include contraceptive use as an indicator.

[14]As part of robustness checks, we also provide the Oster (2019) bias corrected OLS estimates from a specification where education is measured by years of schooling instead of a dummy variable indicating more than primary schooling.

matters in the hands of the male members of the family. Also, younger women exposed to changing social norms (for example, through social media) may have a defiant attitude towards the traditional role of women in the society compared to the older women. Religion can play an important role which is captured by a dummy variable for a respondent being Muslim. Access to schools can affect a girl's education significantly, especially in a context where women's geographic mobility is restricted either because of social norms or worry about harassment on the road. We include dummies indicating whether there are primary and secondary schools in a community. The regressions also include indicators of access to markets (distance to the nearest city corporation) that can be important for both education (better returns to education), and cost-benefit of taking microcredit because a well-functioning labor market reduces the demand for microcredit (Emran et al. (2021)).

Summary statistics on the outcome variables in the online appendix Table A.4 show that women's participation rate is 67% in child health care decision, 62% in own health care decision, 61% in deciding when to visit family and relatives, and 58% in large household purchases. When the four indicators are combined, 41% of the women participate in all 4 decisions, 57% in at least 3, 69% in at least 2, and 81% in at least 1 decision. About 43% of women have access to credit, 35% have 6 or more years of schooling and only 13% have both access to credit and 6 or more years of education. The respondents in the survey are women aged between 15 to 49 years and the average age is 31 years. The share of Muslim women in the sample is 90%, most households have less than 0.5 acres of land (83%) and the average number of adult (10 years or older) household members is 4.05. Of the community characteristics, 63% respondents belong to communities with all weather access road, 78% with access to electricity, 82% with primary schools, 36% with high schools, 25% with

post offices and 30% reside within 40 kilometers from the nearest city corporations. The distribution of the sample by treatment status is presented in the online appendix Table A.1, and the mean participation rates in household decisions by treatment status are in the online appendix Table A.2.

5. Empirical Results

We discuss the estimates in two steps. We start with the estimates that rely on conditional independence assumption (CIA); in particular we report estimates from OLS, and the bias corrected radius matching (BC-RM) due to Lechner et al. (2011). We then address potential biases resulting from unobserved heterogeneity in preferences and abilities using Oster (2019) bias adjusted OLS (BA-OLS) estimator.

5.1 Estimates Based on the Conditional Independence Assumption

Table 1 reports the estimates of parameters δ_1, δ_2 and δ_3 in equation (4) above using the OLS, and BC-RM estimators. The OLS estimates, reported in the odd numbered columns, suggest that access to credit among women with low education has a positive impact on women's participation in three out of four decisions: visiting family and relatives, child health care, and large household purchase, but the effect is not significant at the 10 percent level for own health care. The effects of education among women with no access to microcredit are numerically larger and statistically significant at the 1 percent level for all four decisions. The OLS estimates also do not show any evidence of an interaction effect between access to credit and education in so far as women's say in the household decisions is concerned; none of the estimates of parameter δ_3 is statistically significantly different from the additively separable effects $(\delta_1 + \delta_2)$ at the 10 percent level. The OLS estimates

are, however, likely to be substantially biased.

Evidence from the Bias Corrected Radius Matching Estimator

If the biases in the OLS estimates are at least partly eliminated by matching, we would expect better covariate balance between the "treatment" and "comparison" after matching when compared to the raw data. To check covariate balance with and without matching for the BC-RM estimator, we report estimates of standardized bias defined as $SB(\%) = \frac{\overline{X}_T - \overline{X}_C}{\sqrt{\frac{S_T^2 + S_C^2}{2}}} \times 100$, where $\overline{X}_T$ and S_T^2 are the mean and variance of covariate X for treatment group, and $\overline{X}_C$ and S_C^2 are the mean and variance of X for the comparison group, respectively. The standardized percentage biases of all covariates for three treatment groups (credit, education, and both credit and education) are shown in Figure 1, with and without matching. Detailed estimates are presented in Table A.3 in the online appendix. The evidence shows that matching substantially improves covariate balance. All the post-matching bias estimates shown in Figure 1 are within the range of 10%.[15] Additional evidence from a comparison of the raw sample and the matched sample shows much lower values of pseudo R^2 after matching, thus strengthening the evidence from the standardized bias estimates.

The BC-RM estimator relies on a common support assumption which requires that each individual has a positive probability of receiving treatment. Figure AF.1 in the online appendix presents the propensity score distributions for the treatment and control groups. The propensity score distribution suggests that the common support assumption is satisfied for all three types of treatments: access to credit ($M_i = 1$), higher education ($E_i = 1$), and

[15]The standardized bias of 10% is considered to indicate the negligible imbalance in covariates (Normand et al., 2001).

both access to credit and higher education ($M_i = 1, E_i = 1$).

The BC-RM estimates are smaller in magnitude compared with the corresponding OLS estimates (see the BC-RM estimates reported in the even numbered columns in Table 1). This is consistent with the widely held view that OLS estimates of the effects of credit and education are likely to be biased upward. In contrast to the OLS estimates, the BC-RM estimates suggest no significant effect of access to credit on the decisions regarding large purchases or visiting friends and family.[16] The effects of education, however, remain significant at the 5 percent or lower levels for all four decisions according to the BC-RM estimates.

The lower panel of Table 1 reports the effects of credit and education on four different indicators of women's decision making by counting the number of decisions a woman participates in. The evidence shows that the effects of education again dominate that of credit, consistent with the evidence in the top panel. For example, according to the BC-RM estimates, access to credit does not have any significant effect on the probability that a woman participates in all 4 decisions, but higher education (more than primary schooling) has a statistically significant and numerically substantial effect: a 4.6 percentage points increase in the probability of participation.

The joint treatment of education plus credit, however, does not suggest a stronger effect when compared to the effect of education alone, indicating that there is no complementarity between education and access to credit. We present formal tests of the null hypothesis of separability between education and credit in a later section.

[16]This is consistent with the evidence presented by Bajracharya and Amin (2013) that the OLS estimates of the impact of microcredit on domestic violence in Bangladesh are driven by selection biases. They find that the estimates based on propensity score matching do not show any significant differences between households with and without microcredit.

5.2 Addressing Selection on Unobservables: Estimates from Oster (2019) Bias Adjusted OLS (BA-OLS) Estimator

In this section, we discuss estimates from the Oster BA-OLS estimator which relaxes the CIA assumption and addresses selection on unobservables. The estimates are reported in Table 2.

The evidence in Table 2 shows that education has numerically substantial and statistically significant (at the 1 percent level) positive effects across the board. In contrast, access to credit has a positive and significant (at the 5 percent level) effect on only two decisions (large household purchases and children's health care), but no perceptible effect on the other two decisions. The evidence from BA-OLS is different in the case of large purchases compared to the estimate from BC-RM estimator in Table 1 that suggests no significant impact. Given this conflict, the evidence on large purchases is not conclusive and open to different interpretations. We favor a conservative approach and consider the evidence of an impact robust enough only when both BC-RM and BA-OLS estimates lead to the same conclusion.

The positive effect of credit on children's health care reinforces the widely-discussed finding in the literature that women's economic independence leads to reallocation of household budget in favor of health and education of children. However, the recent evidence shows that the positive income effect of microcredit is more likely for only a subset of borrowers who are entrepreneurial (Banerjee et al. (2015)), implying that the magnitude of the impact of credit on women's decision making power is likely to be modest. A comparison of the estimates for credit vs. education supports this interpretation, as the numerical magnitude of the education effect is much larger across the board in Table 2. The differences

between the effects of education and credit are statistically significant at the 5 percent level for all of the decisions. Even for the monetary decisions such as large purchases, the impact of credit is much smaller: a woman with microcredit membership is 3.4% more likely to have a say in decisions regarding large purchases, while a woman with primary schooling is 8.7% more likely to have a say. The evidence is consistent with the idea that the effect of education is likely to be stronger because it captures three mechanisms: (i) the knowledge effect (comparative advantage), (ii) the independent income effect as higher education (more than primary schooling) opens up opportunities in the labor market, and (iii) the more intangible channels such as self-confidence and negotiation skills.

The estimated effects of joint treatment of higher education plus microcredit from the Oster (2019) bias adjusted OLS estimator are reported in the last column of Table 2. The effects of the joint treatment are numerically larger than the corresponding estimates for education in three decisions: large purchases, children's health care, and visiting family and friends, but the differences between education and joint treatment are not statistically significant at the 10 percent level.

The results on the combined indicators of woman's decision making power in the household using the Oster estimator are reported in the lower panel of Table 2. The evidence again supports the primacy of education; for example, while access to microcredit has no effect on the probability of participation in all 4 decisions, a woman with more than primary schooling enjoys a 8.3 percentage points higher probability of such extensive participation in household decision making.

The estimates in Table 2 assume $\mu = 1$, following the suggestions of Oster (2019) and Altonji et al. (2005). As a conservative strategy, we check the robustness of the conclusions

allowing for a larger selection on unobservables. The estimates for $\mu = 1.2, 1.4, 1.6, 1.8, 2.0$ are reported in the online appendix Tables A.5 (for credit) and A.6 (for education). All the conclusions based on Table 2 remain valid.

(6) The Interaction Effect: Test of Separability

As discussed in section (2), education may be complementary to access to credit in increasing women's participation in household decision-making. Comparative advantage derived from increased knowledge may complement access to credit in increasing women's participation in household decision-making. On the other hand, increased earning potential of a higher educated woman in the labor market may act as a substitute for access to credit.

To test the nature of interaction between access to credit and education, we estimate $\frac{\partial}{\partial M_i}F(\cdot)$, the marginal effect of access to credit, for more educated ($E_i = 1$) and less educated ($E_i = 0$) sub-samples separately and take the difference between these two to estimate the discrete analog of $\frac{\partial^2}{\partial M_i \partial E_i}F(\cdot)$. We test of the null hypothesis that the difference is zero, implying that education and credit are separable. The results for the BC-RM and Oster BA-OLS estimators are reported in Table 3.[17] The evidence is very robust that the null hypothesis of separability cannot be rejected for any of the four decisions, and this conclusion is valid across different estimators in Table 3.

The evidence discussed so far relies on a binary classification of educational attainment. To check if the main conclusions are robust when we use years of schooling instead, we report the estimates using Oster (2019) estimator in Table 4. The estimates show that all the conclusions based on the binary educational attainment earlier remain intact.

[17]We omit the simple OLS estimates for the sake of brevity. They are available from the authors.

(7) Mechanisms (Pathways)

Three mechanisms have been widely discussed in the recent economic literature: marriage market matching based on education, work outside the home, and wealth effect. However, as emphasized by Amartya Sen in many of his writings (see, for example, Sen (1999)), the effects of education on other relatively intangible aspects of quality of life may be equally valuable. Education reduces a "fundamental insecurity", especially for women (Sen (1999)), and make them more confident and articulate. More educated women are more likely to know their rights and successfully resist usurpation of their rights. In this section, we explore two questions: (i) the relative roles of the three mechanisms, and (ii) whether there is any significant role left for the intangible mechanisms emphasized by Sen once we control for the three mechanisms noted above.

As a first step, we provide evidence on the link between women's education and the three channels described above. Table 5 reports estimates of the effects of women's education on indicators of matching in marriage (husband's education, age gap with husband, conflict of preference)[18], a dummy for working outside home, and an indicator of household wealth.[19] The OLS estimates, with and without controls, suggest that a higher educated woman is, in general, matched with a more educated husband, and higher education of women increases the probability of working outside home and household wealth (all three are significant at the 1 percent level), reduces the age gap and conflict of fertility preference between

[18]Husband's education is a dummy variable equalling 1 if the husband has 6 or more years of schooling and zero otherwise. The age gap equals the husband's age minus the wife's age. The indicator of conflict of preference is a dummy variable that takes on the value of 1 if a husband wants more children than his wife.

[19]Household wealth is represented by a wealth index reported in the DHS 2011 data. The index is constructed using household asset data via principal components analysis (NIPORT et al. 2013).

husband and wife.[20] The Oster BA-OLS estimates do not find any significant effect of higher education of a woman on the age gap and conflict of preference, suggesting that the impacts found earlier in the OLS estimates are driven by selection on unobservables. In contrast, the impacts of higher education of a woman on household wealth and probability of working outside home remain robust in the BA-OLS estimates. With this evidence, we now turn to the question whether the effects of education on women's say in household decisions found earlier are in fact mediated through these channels, with a focus on the roles of working outside and household wealth.

Matching in the marriage market based on education can lead to a better balance of power between the husband and wife. We test this hypothesis by including a dummy for husband having more than primary schooling, age gap between wife and husband and its squared, and an indicator of conflict of preference. The results using BC-RM and Oster BA-OLS estimators are reported in the first two columns of Table 6. The evidence is striking: the estimated effects of education on women's ability to participate in decision making remains largely unaffected (compare with the estimates in Table 1 and 2), and in some cases, the magnitude of the effect is larger once we control for these variables.

Higher education may facilitate work outside the home through employment in the formal sector such as the garment industry in Bangladesh or in the government supported primary and secondary schools as teachers. This is consistent with the evidence in Table 5. To see if this channel is primarily responsible for the strong effects of education on women's say found earlier, we control for a dummy indicating that a woman was employed outside the home during the last 12 months of the survey date. The results, reported in

[20]The effect is not significant for conflict of preference when we control for credit access.

columns 3 and 4 of Table 6, show that the estimated effects of education do not change in any substantial way, especially for the Oster (2019) estimator (compare to the estimates in column 2 of Table 2).

Columns 5 and 6 in Table 6 report the estimates when we control for a measure of household wealth based on the principal component analysis of a household's assets (see the discussion in the data section).[21] If the competition for limited income sharpens the gender conflicts, we would expect that the more educated households to have fewer such conflicts, because higher educated women live in households with higher wealth in our context, as found in Table 5. The estimates show that the impact of education on decision on children's health care is lower once we control for wealth: the Oster bias adjusted OLS estimate goes down from 0.078 in Table 2 to 0.66 in Table 6. However, the wealth channel is not important for the other three decisions as the bias adjusted OLS estimates of the effects of education are larger or unchanged (compared to Table 2).

The last two columns in Table 6 provide the estimates when the indicators capturing all three mechanisms are added as controls in the regression. The point estimates of the effects of education are larger in magnitude (compared to Table 2) for decisions about large household purchase and visiting friends and family, but slightly smaller for the health related decisions.

Finally, we implement Gelbach (2016) decompositions that use the omitted variables bias formula in an OLS regression to decompose the combined effects of the three mechanisms above. As noted by Gelbach (2016), the standard practice of sequential addition of different variables to check how the estimate of interest (the effect of education) changes

[21] This measure of household wealth is reported in the DHS 2011 survey.

can be misleading because the conclusion depends on the order of addition. The results from Gelbach (2016) decompositions are reported in Table 7, and the evidence shows that working outside the home is a statistically significant mechanism in all decisions and household wealth only for children's health care. This is consistent with existing evidence on the importance of working outside the home for women in Bangladesh to gain autonomy (Eswaran (2014)).

Perhaps, the most important take away from the evidence in Tables 6 and 7 is that the effects of education on women's say in the household do not seem to be driven primarily by the three salient economic channels discussed in the literature. This can be interpreted as suggestive evidence that education improves women's say in the household primarily through more intangible channels such as a sense of security, self-confidence, self-respect, and better negotiation skills as emphasized by Amartya Sen (1999).

(8) Conclusions

We provide an analysis of the relative importance of access to credit and education as instruments for improving women's decision making power within the household in a developing country. We implement the doubly robust matching estimator due to Lechner et al. (2011), and the bias adjusted OLS estimator developed by Oster (2019) to address selection on unobservables. Using data from Demographic and Health Survey 2011 survey, we provide evidence on women's say in household decisions in Bangladesh.

The evidence suggests that access to credit in the form of microcredit membership has only limited impact on the decision making power of women in Bangladesh. In contrast, having primary schooling or more education empowers women in participating in decisions related to household expenditure, own and children's health issues, and social interactions

(visiting friends and family). While the effects on health decisions may reflect comparative advantage in knowledge intensive decisions, it is perhaps more striking that education also enables women to participate in other decisions including visiting her natal family where the conflict of preference between a woman and her husband can be especially sharp. Evidence also suggests that the effects of education and access to credit are additively separable with no significant interaction between them.

We explore the importance of three salient economic mechanisms widely noted in the literature in mediating the impact of higher education: assortative matching in marriage, working outside the home, and wealth effect. Evidence from Gelbach (2016) decomposition suggests that working outside home is important as a mechanism, but we do not find any significant role for marriage market matching or household wealth. Perhaps, more important is the finding that the effects of education remain largely unchanged when we control for indicators of these three mechanisms, suggesting that education enhances women's say primarily through the more intangible mechanisms, making her more confident, articulate, and aware of her legal rights in a patrilineal society.

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Table 1: The marginal effects of access to credit (δ_1), education (δ_2), and joint treatment (δ_3) on women's participation in household decision making.

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent variables	Access to credit (δ_1)		Education (δ_2)		Joint-treatment (δ_3)	
	OLS	BC-RM	OLS	BC-RM	OLS	BC-RM
Visiting Family and Relatives	0.027*	0.015	0.057***	0.046***	0.069***	0.051**
	(0.014)	(0.014)	(0.016)	(0.018)	(0.019)	(0.020)
Own Health Care	0.006	-0.003	0.085***	0.071***	0.063***	0.050**
	(0.013)	(0.014)	(0.016)	(0.018)	(0.017)	(0.020)
Children's Health Care	0.036***	0.031**	0.049***	0.042**	0.084***	0.078***
	(0.014)	(0.014)	(0.016)	(0.017)	(0.018)	(0.019)
Large Household Purchase	0.04***	0.022	0.056***	0.046**	0.071***	0.042**
	(0.014)	(0.014)	(0.017)	(0.018)	(0.018)	(0.020)
Combined indicators: Respondent participates in -						
All 4 decisions	0.027**	0.013	0.055***	0.046***	0.049**	0.030
	(0.014)	(0.015)	(0.018)	(0.017)	(0.020)	(0.020)
At least 3 of 4	0.034**	0.018	0.068***	0.057***	0.088***	0.068***
	(0.014)	(0.014)	(0.017)	(0.018)	(0.019)	(0.020)
At least 2 of 4	0.027**	0.018	0.063***	0.054***	0.080***	0.060***
	(0.013)	(0.013)	(0.016)	(0.017)	(0.016)	(0.019)
At least1 of 4	0.022*	0.016	0.060***	0.049***	0.072***	0.061***
	(0.011)	(0.011)	(0.014)	(0.016)	(0.013)	(0.016)

Notes:

(1) Control variables include respondent's age & age squared, religion (Islam), number of adult household members (10 years or above), less than 0.50 acre of household land ownership; community characteristics - all weather access road, access to electricity, post office, primary and high schools in the community, dummy variables for distance to the nearest city corporations; and district fixed effect.

(2) Access to credit is defined as respondent being a member of income generating organizations such as Grameen Bank, BRAC, Proshika, etc.

(3) Education is a dummy variable that takes on 1 if respondent has more than 5 years of schooling.

(4) BC-RM = Bias Corrected Radius Matching.

(5) Clustered standard errors are in parentheses.

(6) ***, ** and * denote statistical significance at 1%, 5% and 10% level of significance, respectively.

Table 2: The bias adjusted OLS (BA-OLS) estimates of the marginal effects of access to credit (δ_1), education (δ_2), and joint treatment (δ_3) on women's participation in household decision making.

	(1)	(2)	(3)	(4)
Dependent variables	Access to credit (δ_1)	Education (δ_2)	Joint treatment (δ_3)	z-score for $\delta_3 = \delta_2$
Visiting Family and Relatives	0.021	0.088***	0.092***	0.187
	(0.014)	(0.017)	(0.020)	
Own Health Care	0.001	0.112***	0.084***	-1.036
	(0.014)	(0.018)	(0.020)	
Children's Health Care	0.030**	0.078***	0.106***	1.128
	(0.013)	(0.017)	(0.019)	
Large Household Purchase	0.034**	0.087***	0.092***	0.180
	(0.014)	(0.019)	(0.021)	
Combined indicators: Respondent participates in -				
All 4 decisions	0.021	0.083***	0.071***	-0.432
	(0.016)	(0.018)	(0.021)	
At least 3 of 4	0.027*	0.101***	0.113***	0.495
	(0.015)	(0.016)	(0.020)	
At least 2 of 4	0.02	0.094***	0.101***	0.291
	(0.014)	(0.016)	(0.019)	
At least1 of 4	0.017	0.084***	0.089***	0.252
	(0.012)	(0.014)	(0.015)	

Notes:

(1) Control variables include respondent's age & age squared, religion (Islam), number of adult household members (10 years or above), less than 0.50 acre of household land ownership; community characteristics - all weather access road, access to electricity, post office, primary and high schools in the community, dummy variables for distance to the nearest city corporations; and district fixed effect.

(2) Access to credit is defined as respondent being a member of income generating organizations such as Grameen Bank, BRAC, Proshika, etc.

(3) Education is a dummy variable that takes on 1 if respondent has more than 5 years of schooling.

(4) Bootstrapped standard errors from 250 replications are presented in parentheses.

(5) ***, ** and * denote statistical significance at 1%, 5% and 10% level of significance, respectively.

Table 3: Test of complementarity between access to credit and education on women's participation in household decision making.

Dependent variables	(1)	(2)	(3)	(4)	(5)	(6)
	BC-RM			BA-OLS		
	Marginal effect of access to credit		z-score	Marginal effect of access to credit		z-score
	Less Educated	More Educated		Less Educated	More Educated	
Visiting Family and Relatives	0.015	0.040**	1.060	0.021	0.028	0.273
	(0.014)	(0.020)		(0.014)	(0.019)	
Own Health Care	-0.003	-0.001	0.111	0.001	-0.014	-0.633
	(0.014)	(0.019)		(0.014)	(0.019)	
Children's Health Care	0.031**	0.063***	1.370	0.030**	0.051***	0.978
	(0.014)	(0.019)		(0.013)	(0.017)	
Large Household Purchase	0.022	0.025	0.101	0.034**	0.019	-0.673
	(0.014)	(0.020)		(0.014)	(0.018)	
Combined indicators: Respondent participates in -						
All 4 decisions	0.013	0.020	0.296	0.021	0.006	-0.616
	(0.015)	(0.020)		(0.016)	(0.019)	
At least 3 of 4	0.018	0.038*	0.821	0.027*	0.036*	0.355
	(0.014)	(0.020)		(0.015)	(0.020)	
At least 2 of 4	0.018	0.040**	0.983	0.02	0.027	0.334
	(0.013)	(0.018)		(0.014)	(0.019)	
At least 1 of 4	0.016	0.033**	0.887	0.017	0.018	0.021
	(0.011)	(0.016)		(0.012)	(0.016)	

Notes:

(1) Control variables include respondent's age & age squared, religion (Islam), number of adult household members (10 years or above), less than 0.50 acre of household land ownership; community characteristics - all weather access road, access to electricity, post office, primary and high schools in the community, dummy variables for distance to the nearest city corporations; and district fixed effect.

(2) Access to credit is defined as respondent being a member of income generating organizations such as Grameen Bank, BRAC, Proshika, etc.

(3) Education is a dummy variable that takes on 1 if respondent has more than 5 years of schooling.

(4) Clustered standard errors for BC-RM, and bootstrapped standard errors with 250 replications for BA-OLS estimates are in parentheses.

(5) ***, ** and * denote statistical significance level at 1%, 5% and 10%, respectively.

Table 4: The bias adjusted OLS (BA-OLS) estimates of acess to credit, years of schooling and the interaction effects.

	(1)	(2)	(3)
Dependent variables	Access to Credit	Years of Schooling	(Credit × Schooling)
Visiting Family and Relatives	0.037	0.019***	-0.010*
	(0.025)	(0.003)	(0.006)
Own Health Care	0.012	0.020***	-0.008
	(0.026)	(0.003)	(0.005)
Children's Health Care	0.011	0.016***	-0.005
	(0.027)	(0.003)	(0.006)
Large Household Purchase	0.027	0.015***	-0.009
	(0.025)	(0.003)	(0.006)
Combined indicators: Respondent participates in -			
All 4 decisions	0.02	0.016***	-0.006
	(0.024)	(0.003)	(0.007)
At least 3 of 4	0.029	0.019***	-0.008
	(0.026)	(0.003)	(0.006)
At least 2 of 4	0.011	0.018***	-0.007
	(0.024)	(0.003)	(0.005)
At least1 of 4	0.028	0.017***	-0.010**
	(0.018)	(0.003)	(0.005)

Notes:

(1) Control variables include respondent's age & age squared, religion (Islam), number of adult household members (10 years or above), less than 0.50 acre of household land ownership; community characteristics - all weather access road, access to electricity, post office, primary and high schools in the community, dummy variables for distance to the nearest city corporations; and district fixed effect.

(2) Access to credit is defined as respondent being a member of income generating organizations such as Grameen Bank, BRAC, Proshika, etc.

(3) Bootstrapped standard errors from 250 replications are presented in parentheses.

(4) ***, ** and * denote statistical significance at 1%, 5% and 10% level of significance, respectively.

Table 5: Relationship between education, and husband's characteristics, women's working status and household wealth index.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Variables	Husband's education (6+)		Age gap with husband		Husband wants more children		Work outside		Household wealth index	
OLS estimates with dummy variable for respondent having more than 5 years of schooling										
Education (6+)	0.483***	0.491***	-0.600***	-0.291*	-0.020***	-0.007	0.013	0.035***	0.573***	0.553***
	(0.011)	(0.014)	(0.114)	(0.156)	(0.007)	(0.010)	(0.009)	(0.011)	(0.021)	(0.022)
Access to Credit		-0.015		-0.228*		-0.001		0.052***		-0.050***
		(0.010)		(0.134)		(0.008)		(0.009)		(0.013)
Education X Credit		-0.099***		-0.274		-0.004		-0.003		-0.122***
		(0.020)		(0.223)		(0.014)		(0.015)		(0.029)
OLS estimates with respondent's years of schooling										
Years of schooling	0.070***	0.074***	-0.097***	-0.067***	-0.003***	-0.002	0.003**	0.007***	0.085***	0.086***
	(0.001)	(0.002)	(0.015)	(0.021)	(0.001)	(0.001)	(0.001)	(0.002)	(0.003)	(0.003)
Access to Credit		-0.014		-0.171		0.001		0.057***		-0.032**
		(0.011)		(0.168)		(0.010)		(0.011)		(0.015)
Schooling X Credit		-0.007***		-0.040		-0.001		-0.001		-0.013***
		(0.002)		(0.029)		(0.002)		(0.002)		(0.004)
BA-OLS (Oster 2019) estimates with dummy variable for respondent having more than 5 years of schooling										
Education (6+)		5.005		-0.012		0.004		0.053***		0.512***
		(22.212)		(0.178)		(0.010)		(0.012)		(0.042)
Access to Credit		0.039***		-0.111		0		0.042***		0.037**
		(0.014)		(0.151)		(0.008)		(0.010)		(0.017)
Education X Credit		-0.346***		0.369		0.012		-0.056**		-0.422***
		(0.039)		(0.281)		(0.015)		(0.023)		(0.038)
BA-OLS (Oster 2019) estimates with respondent's years of schooling										
Years of Schooling		0.399		-0.034		-0.001		0.010***		0.090***
		(0.489)		(0.026)		(0.001)		(0.002)		(0.007)
Access to Credit		0.097***		0.145		0.008		0.043**		0.165***
		(0.026)		(0.220)		(0.012)		(0.017)		(0.033)
Schooling X Credit		-0.051***		0.110**		0.002		-0.014***		-0.066***
		(0.007)		(0.052)		(0.003)		(0.004)		(0.007)

Notes:

(1) The odd numbered columns do not include any control variables.

(2) The even numbered columns include the same control variables used in the main specification (including district fixed effects).

(3) Clustered standard errors for the OLS and bootstrapped standard errors with 250 replications for BA-OLS estimates are in parentheses.

(4) ***, ** and * denote statistical significance at 1%, 5% and 10% significance level, respectively.

Table 6: The marginal effects of education (δ_2) with different specifications.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Husband charateristics		Work outside		Wealth Status		All	
	BCRM	BA-OLS	BCRM	BA-OLS	BCRM	BA-OLS	BCRM	BA-OLS
Visiting Family	0.053***	0.091***	0.043**	0.085***	0.042**	0.094***	0.047**	0.091***
and Relatives	(0.020)	(0.021)	(0.018)	(0.017)	(0.019)	(0.019)	(0.021)	(0.022)
Own Health Care	0.059***	0.111***	0.069***	0.108***	0.049***	0.112***	0.038*	0.106***
	(0.020)	(0.021)	(0.018)	(0.018)	(0.019)	(0.021)	(0.021)	(0.023)
Children's Health Care	0.038*	0.083***	0.04**	0.077***	0.009	0.066***	0.011	0.072***
	(0.020)	(0.020)	(0.017)	(0.017)	(0.019)	(0.019)	(0.021)	(0.021)
Large Household	0.059***	0.12***	0.041**	0.083***	0.047**	0.104***	0.054**	0.124***
Purchase	(0.021)	(0.023)	(0.018)	(0.019)	(0.020)	(0.022)	(0.021)	(0.024)
Combined indicators: Respondent participates in								
All 4 decisions	0.041**	0.117***	0.042**	0.08***	0.024	0.087***	0.024	0.112***
	(0.020)	(0.023)	(0.017)	(0.018)	(0.019)	(0.021)	(0.021)	(0.024)
At least 3 of 4	0.055***	0.107***	0.052***	0.097***	0.039**	0.107***	0.046**	0.107***
	(0.021)	(0.021)	(0.018)	(0.016)	(0.019)	(0.020)	(0.021)	(0.023)
At least 2 of 4	0.066***	0.103***	0.052***	0.091***	0.048***	0.097***	0.05**	0.101***
	(0.020)	(0.021)	(0.017)	(0.016)	(0.018)	(0.020)	(0.021)	(0.023)
At least 1 of 4	0.045***	0.073***	0.047***	0.082***	0.034**	0.08***	0.027	0.069***
	(0.017)	(0.018)	(0.015)	(0.014)	(0.016)	(0.017)	(0.018)	(0.019)

Notes:

(1) In addition to the control variables in the main specification, columns(1) and(2) include husband's education, age gap with the respondent and the gap squared, and a dummy variable for husband wanting more children; columns (3) and (4) include a dummy variable for the respondent working in the last twelve months; columns (5) and (6) include four wealth status dummy variables for households at the top four quintles in the wealth index distribution; and columns (7) and(8) include all of the additional control variables used in columns (1) to (6).

(2) Control group in each model is D^{00}, that is, the group without access to credit and education.

(3) Clustered standard errors for BRCM and bootstrapped standard errors from 250 replications for BA-OLS are presented in parentheses.

(4) ***, ** and * denote statistical significance at 1%, 5% and 10% significance level, respectively.

Table 7: Gelbach decompositions of the education effects (in percentage points).

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variables	Visiting Family and Relatives	Own Health Care	Children's Health Care	Large Household Purchase	Participate in how many decisions			
					All 4	At least 3	At least 2	At least 1
Main specification	4.77***	6.94***	4.78***	4.59***	3.98***	5.78***	5.83***	5.45***
	(1.226)	1.204)	(1.212)	(1.254)	(1.269)	(1.287)	(1.123)	(0.965)
Main spec.+additional	4.06***	5.83***	3.44**	5.29***	3.65***	5.06***	5.26***	4.54***
control variables	(1.426)	1.369)	(1.364)	(1.374)	(1.341)	(1.456)	(1.313)	(1.165)
Explained by the	0.71	1.11	1.33*	-0.7	0.33	0.72	0.57	0.91
additional controls	(0.713)	0.732)	(0.685)	(0.707)	(0.741)	(0.746)	(0.657)	(0.579)
Husband's	0.59	0.93	0.21	-0.72	-0.33	0.27	0.33	0.77
characteristics	(0.607)	0.628)	(0.550)	(0.608)	(0.623)	(0.588)	(0.524)	(0.536)
Work outside	0.18**	0.26***	0.12*	0.30***	0.29***	0.27***	0.20***	0.12**
	(0.076)	0.094)	(0.063)	(0.105)	(0.110)	(0.099)	(0.073)	(0.050)
Wealth status	-0.07	-0.09	1.00**	-0.28	0.38	0.17	0.04	0.02
	(0.516)	0.588)	(0.507)	(0.558)	(0.575)	(0.583)	(0.526)	(0.448)

Notes:

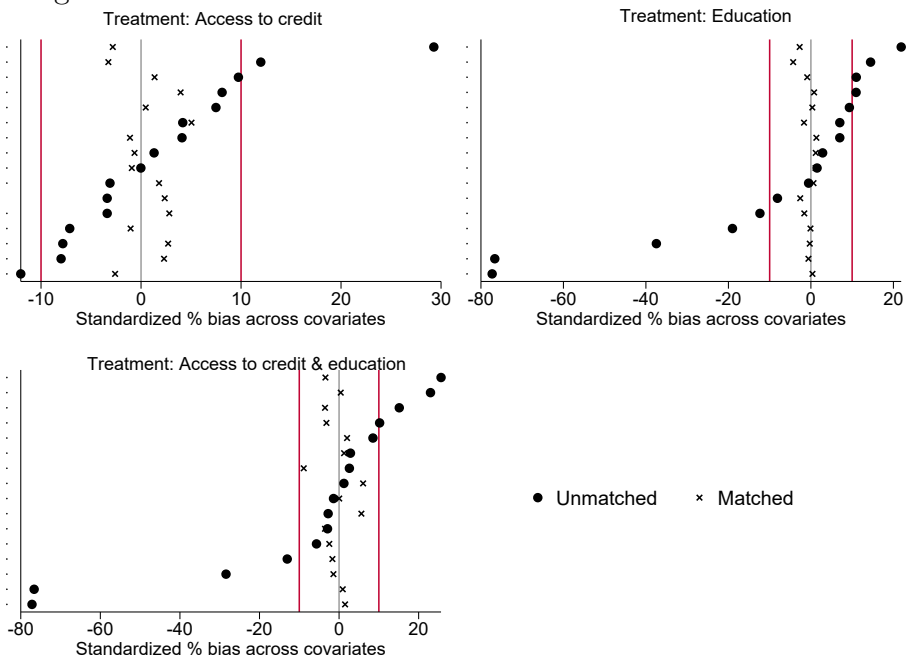
(1) Estimates are converted to percentage points multiplying by 100.

(2) Covariates in the main specification: respondent's age & age squared, religion (Islam), number of adult household members (10 years or above), less than 0.50 acre of household land ownership; community characteristics - all weather access road, access to electricity, post office, primary and high schools in the community, dummy variables for distance to the nearest city corporations; and district fixed effect.

(3) Husband's characterisitics include a dummy for husband having more than primary schooling, age gap and the gap squared, and a dummy variable for husband wanting more chindern than the respondent; working outside is dummy variable if respondent worked outside over the last 12 months; and wealth status is represented by 4 wealth status dummy variables for households at the top four quitiles in household wealth index distribution

(4) ***, ** and * denote statistical significance at 1%, 5% and 10% level of significance, respectively.

Figure 1: Standardized bias of covariates for BC-RM estiamtes.



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Appendix

Table A.1: Distribution of the sample by treatment status.

	$Education = 0$	$Education = 1$	Total
$Access\,to\,credit = 0$	$\boldsymbol{D^{00}}$ **:** 3236	$\boldsymbol{D^{01}}$: 2113	5,349
(%)	(34.52)	(22.54)	(57.06)
$Access\,to\,credit = 1$	$\boldsymbol{D^{10}}$ **:** 2780	$\boldsymbol{D^{11}}$ **:** 1244	4,024
(%)	(29.66)	(13.27)	(42.93)
Total	6,016	3,357	9,373
(%)	(64.18)	(35.81)	(100)

Table A.2: The mean participation rates by treatment status.

Participation	D^{00}		D^{10}		D^{01}		D^{11}		All	
indicators	Obs	Mean	Obs	Mean	Obs	Mean	Obs	Mean	Obs	Mean
Visiting Family and Relatives	3233	0.597	2110	0.578	2779	0.640	1242	0.609	9364	0.607
Own Health Care	3235	0.609	2113	0.630	2780	0.631	1244	0.623	9372	0.622
Children's Health Care	3234	0.650	2110	0.626	2779	0.705	1244	0.682	9367	0.666
Large Household Purchase	3236	0.564	2113	0.546	2780	0.623	1244	0.585	9373	0.581
Combined indicators: Respondent participates in -										
all 4 decisions	3230	0.398	2107	0.384	2778	0.443	1242	0.394	9357	0.408
at least 3 of 4	3230	0.551	2107	0.537	2778	0.604	1242	0.577	9357	0.567
at least 2 of 4	3230	0.678	2107	0.666	2778	0.725	1242	0.707	9357	0.693
at least 1 of 4	3230	0.792	2107	0.792	2778	0.828	1242	0.823	9357	0.807

Table A.3: Standardized % bias in mean of matched and unmatched samples in BC-RM method

Variables	Unmatched/ Matched sample	Treatment: Access to credit		Treatment: Education		Treatment: Access to credit & education	
		%bias	t-stat	%bias	t-stat	%bias	t-stat
Age	Unmatched	1.3	0.5	-77.3	0	-77.2	0
	Matched	-0.7	-0.25	0.4	0.13	1.5	0.41
Age squared	Unmatched	0	-0.01	-76.6	0	-76.6	0
	Matched	-0.9	-0.35	-0.6	-0.22	0.9	0.27
Muslim	Unmatched	-12	-4.68	-8.1	0.003	-28.4	0
	Matched	-2.6	-0.93	-2.6	-0.81	-1.4	-0.31
Household size (10 years or older)	Unmatched	-3.4	-1.31	7	0.011	-2.8	0.397
	Matched	2.8	1.09	1.3	0.4	5.6	1.39
Less than 0.5 acres of land	Unmatched	29.3	11.17	-37.5	0	-5.7	0.086
	Matched	-2.8	-1.33	-0.3	-0.08	-2.5	-0.6
Community has access road	Unmatched	9.7	3.76	14.5	0	23	0
	Matched	1.4	0.51	-4.3	-1.43	0.4	0.11
Community has electricity	Unmatched	8.1	3.13	21.9	0	25.6	0
	Matched	4	1.48	-2.7	-0.97	-3.4	-0.95
Community has post office	Unmatched	4.2	1.61	7	0.012	10.2	0.002
	Matched	5	1.88	-1.7	-0.53	-3.2	-0.76
Community has primary school	Unmatched	-3.4	-1.31	2.8	0.311	2.9	0.395
	Matched	2.4	0.86	1.2	0.38	1.3	0.32
Community has high school	Unmatched	7.5	2.9	9.3	0.001	15.1	0
	Matched	0.5	0.17	0.3	0.11	-3.5	-0.86
Distance to the city corporation: Within 10 to 20 km	Unmatched	-7.1	-2.74	-0.6	0.843	2.6	0.434
	Matched	-1	-0.41	0.6	0.21	-8.9	-2.04
Within 20 to 30 km	Unmatched	-8	-3.08	1.5	0.592	-2.9	0.389
	Matched	2.3	0.93	1.1	0.36	-3.5	-0.88
Within 30 to 40 km	Unmatched	4.1	1.59	11	0	8.5	0.009
	Matched	-1.1	-0.4	-0.9	-0.28	2	0.48
Within 40 to 60 km	Unmatched	-3.1	-1.2	11	0	-1.4	0.675
	Matched	1.8	0.68	0.8	0.25	0	0.01
Within 60 to 100 km	Unmatched	12	4.63	-12.4	0	1.2	0.715
	Matched	-3.3	-1.2	-1.6	-0.53	6	1.51
More than 100 km	Unmatched	-7.8	-3.01	-19	0	-13	0
	Matched	2.7	1.12	-0.1	-0.04	-1.7	-0.47

Notes:

$\%\,bias = \frac{\overline{X}_T - \overline{X}_C}{\sqrt{\frac{S_T^2 + S_C^2}{2}}} \times 100$, where $\overline{X}_T$ and S_T^2 are the mean and variance of covariate X for treatment group, and $\overline{X}_C$ and S_C^2 are the mean and variance of X for the control group, respectively.

Table A.4: Descriptive Statistics.

Variables	Obs	Mean	Std. Dev.	Min	Max
Outcome variables					
Visiting Family and Relatives	9364	0.607	0.488	0	1
Own Health Care	9372	0.622	0.485	0	1
Children's Health Care	9367	0.666	0.472	0	1
Large Household Purchase	9373	0.581	0.493	0	1
Participate in all 4 decisions	9357	0.408	0.491	0	1
Particiaptes in at least 3 decisions out of 4	9357	0.567	0.495	0	1
Participate in at least 2 of 4 decisions	9357	0.693	0.461	0	1
Participate in at least 1 of 4 decisions	9357	0.807	0.395	0	1
Treatment variables					
Access to Credit	9373	0.43	0.495	0	1
Education (more than 5 years of schooling)	9373	0.351	0.477	0	1
(Access to Credit)x(Education)	9373	0.129	0.335	0	1
Control variables					
Age	9373	30.802	8.974	15	49
Age gap with husband	9373	8.992	4.722	0	25
Worked in last 12 months	9373	0.11	0.313	0	1
Husband wants more children	9373	0.094	0.291	0	1
Husband has more than 5 years of schooling	9373	0.346	.476	0	1
Muslim	9373	0.898	0.303	0	1
1=Land owned less than 0.5 acres	9373	0.826	0.379	0	1
Number of household members (10 years or above)	9373	4.023	1.862	1	20
All weather access road	9373	0.631	0.483	0	1
Access to electricity	9373	0.778	0.416	0	1
Community has primary school	9373	0.821	0.384	0	1
Community has high school	9373	0.355	0.479	0	1
Community has post office	9373	0.247	0.431	0	1
Distance to the city corporation: Within 10 km	9373	0.009	0.096	0	1
Within 10 to 20 km	9373	0.076	0.265	0	1
Within 20 to 30 km	9373	0.078	0.268	0	1
Within 30 to 40 km	9373	0.129	0.336	0	1
Within 40 to 60 km	9373	0.253	0.435	0	1
Within 60 to 100 km	9373	0.388	0.487	0	1
More than 100 km	9373	0.066	0.249	0	1

Table A.5: Covariate balance test statistics for BC-RM estimator.

Treatment	Sample	Pseudo R^2	$LR\chi^2$	$p > \chi^2$	Mean Bias
Credit=1, Education=0	Unmatched	0.029	242.74	0.000	7.6
(vs Credit=0, Education=0)	Matched	0.002	15.30	0.503	2.2
Credit=0, Education=1	Unmatched	0.164	1174.57	0	19.8
(vs Credit=0, Education=0)	Matched	0.002	10.91	0.815	1.3
Credit=1, Education=1	Unmatched	0.138	728.56	0	18.6
(vs Credit=0, Education=0)	Matched	0.004	45.63	0.654	2.9

Note: Null hypothesis - covariates are balanced.

Figure A.1: Propensity score distribution by treatment status for BC-RM estimator.

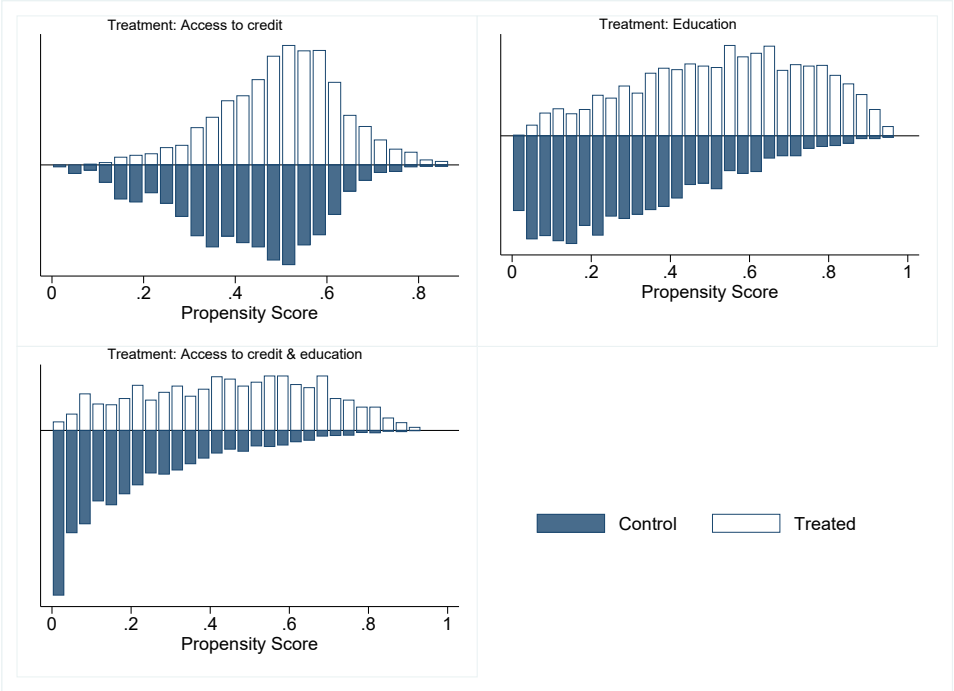

Table A.6: BA-OLS estimates of the marginal effects of access to credit, δ_1 .

μ	Visiting Family and Relatives	Own Health Care	Children's Health Care	Large Household Purchase	Combined indicators: Respondent participates in - All 4 decisions	At least3	At least 2	At least 1
0	0.027	0.006	0.036	0.040	0.027	0.034	0.027	0.022
0.2	0.026	0.005	0.035	0.039	0.026	0.032	0.025	0.021
0.4	0.025	0.004	0.034	0.038	0.025	0.031	0.024	0.020
0.6	0.023	0.003	0.032	0.036	0.024	0.030	0.022	0.019
0.8	0.022	0.002	0.031	0.035	0.022	0.029	0.021	0.018
1.0	0.021	0.001	0.030	0.034	0.021	0.027	0.020	0.017
1.2	0.020	0.000	0.028	0.032	0.020	0.026	0.018	0.016
1.4	0.019	-0.002	0.027	0.031	0.019	0.024	0.017	0.015
1.6	0.018	-0.003	0.026	0.030	0.017	0.023	0.015	0.014
1.8	0.016	-0.004	0.024	0.028	0.016	0.022	0.014	0.013
2.0	0.015	-0.005	0.023	0.027	0.015	0.020	0.012	0.012

Notes:

(1) μ represents the level of selection on unobservables relative to observables.

(2) First row represents OLS estimates.

(3) The maximum R^2in each model is assumed to be $R^2_{max} = 1.3 \times R^2_{OLS}$.

(4) Control variables include respondent's age & age squared, religion (Islam), number of adult household members (10 years or above), less than 0.50 acre of household land ownership; community characteristics - all weather access road, access to electricity, post office, primary and high schools in the community, dummy variables for distance to the nearest city corporations; and district fixed effect.

(5) Control group in each model is D^{00}, that is, the group without access to credit and education.

Table A.7: BA-OLS estimates of the marginal effects of education, δ_2.

					Combined indicators: Respondent participates in -			
μ	Visiting Family and Relatives	Own Health Care	Children's Health Care	Large Household Purchase	All 4 decisions	At least 3	At least 2	At least 1
0	0.057	0.085	0.049	0.056	0.055	0.068	0.063	0.060
0.2	0.063	0.090	0.054	0.062	0.060	0.074	0.069	0.065
0.4	0.069	0.095	0.060	0.068	0.066	0.080	0.075	0.069
0.6	0.075	0.100	0.066	0.074	0.071	0.087	0.081	0.074
0.8	0.081	0.106	0.072	0.080	0.077	0.094	0.087	0.079
1.0	0.088	0.112	0.078	0.087	0.083	0.101	0.094	0.084
1.2	0.094	0.117	0.085	0.093	0.090	0.108	0.100	0.089
1.4	0.102	0.124	0.092	0.101	0.096	0.116	0.108	0.095
1.6	0.109	0.130	0.100	0.108	0.103	0.124	0.115	0.101
1.8	0.118	0.138	0.108	0.116	0.111	0.133	0.124	0.108
2.0	0.127	0.145	0.117	0.126	0.119	0.143	0.133	0.115

Notes:

(1) μ represents the level of selection on unobservables relative to observables.

(2) First row represents OLS estimates.

(3) The maximum R^2in each model is assumed to be $R^2_{max} = 1.3 \times R^2_{OLS}$.

(4) Control variables include respondent's age & age squared, religion (Islam), number of adult household members (10 years or above), less than 0.50 acre of household land ownership; community characteristics - all weather access road, access to electricity, post office, primary and high schools in the community, dummy variables for distance to the nearest city corporations; and district fixed effect.

(5) Control group in each model is D^{00}, that is, the group without access to credit and education.

Table A.8: BA-OLS estimates of the marginal effects of "access to credit and education" on women's participation in household decision making, δ_3.

μ	Visiting Family and Relatives	Own Health Care	Children's Health Care	Large Household Purchase	Combined indicators: Respondent participates in - All 4 decisions	At least 3	At least 2	At least 1
0	0.069	0.063	0.084	0.071	0.049	0.088	0.080	0.072
0.2	0.074	0.067	0.089	0.075	0.053	0.093	0.084	0.075
0.4	0.078	0.071	0.093	0.079	0.058	0.098	0.088	0.078
0.6	0.083	0.075	0.097	0.083	0.062	0.103	0.092	0.082
0.8	0.087	0.079	0.102	0.087	0.067	0.108	0.096	0.085
1.0	0.092	0.084	0.106	0.092	0.071	0.113	0.101	0.089
1.2	0.098	0.088	0.111	0.096	0.076	0.119	0.105	0.093
1.4	0.103	0.093	0.117	0.101	0.082	0.125	0.110	0.097
1.6	0.109	0.098	0.122	0.106	0.087	0.131	0.116	0.101
1.8	0.115	0.103	0.128	0.112	0.093	0.138	0.121	0.106
2.0	0.121	0.109	0.134	0.118	0.099	0.145	0.127	0.111

Notes:

(1) μ represents the level of selection on unobservables relative to observables.

(2) First row represents OLS estimates.

(3) The maximum R^2in each model is assumed to be $R^2_{max} = 1.3 \times R^2_{OLS}$.

(4) Control variables include respondent's age & age squared, religion (Islam), number of adult household members (10 years or above), less than 0.50 acre of household land ownership; community characteristics - all weather access road, access to electricity, post office, primary and high schools in the community, dummy variables for distance to the nearest city corporations; and district fixed effect.

(5) Control group in each model is D^{00}, that is, the group without access to credit and education.

Table A.9: BA-OLS estimates of $[\delta_3 - (\delta_1 + \delta_2)]$.

μ	Visiting Family and Relatives	Own Health Care	Children's Health Care	Large Household Purchase	Combined indicators: Respondent participates in - All 4 decisions	At least 3	At least 2	At least 1
0	-0.009	-0.023	0.007	-0.018	-0.027	-0.008	-0.001	-0.004
0.2	-0.010	-0.026	0.005	-0.021	-0.029	-0.010	-0.003	-0.007
0.4	-0.012	-0.030	0.003	-0.025	-0.031	-0.013	-0.006	-0.010
0.6	-0.014	-0.034	0.001	-0.029	-0.033	-0.017	-0.009	-0.014
0.8	-0.017	-0.039	-0.001	-0.034	-0.035	-0.021	-0.013	-0.019
1.0	-0.020	-0.045	-0.004	-0.041	-0.038	-0.026	-0.017	-0.025
1.2	-0.023	-0.053	-0.008	-0.048	-0.042	-0.032	-0.022	-0.032
1.4	-0.028	-0.063	-0.013	-0.058	-0.047	-0.040	-0.029	-0.041
1.6	-0.034	-0.075	-0.019	-0.070	-0.053	-0.050	-0.038	-0.053
1.8	-0.042	-0.093	-0.027	-0.088	-0.061	-0.063	-0.050	-0.070
2.0	-0.054	-0.124	-0.039	-0.117	-0.073	-0.084	-0.069	-0.103

Notes:

(1) μ represents the level of selection on unobservables relative to observables.

(2) First row represents OLS estimates.

(3) The maximum R^2in each model is assumed to be $R^2_{max} = 1.3 \times R^2_{OLS}$.

(4) Control variables include respondent's age & age squared, religion (Islam), number of adult household members (10 years or above), less than 0.50 acre of household land ownership; community characteristics - all weather access road, access to electricity, post office, primary and high schools in the community, dummy variables for distance to the nearest city corporations; and district fixed effect.

(5) Control group in each model is D^{00}, that is, the group without access to credit and education.