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**Impacts of Household Credit on Education and Healthcare Spending
by the Poor in Peri-urban Areas in Vietnam**

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Abstract

There is debate about whether microfinance has positive impacts on education and health for borrowing households in developing countries. To provide evidence for this debate we use a new survey designed to meet the conditions for propensity score matching (PSM) and examine the impact of household credit on education and healthcare spending by the poor in peri-urban areas of Ho Chi Minh City, Vietnam. In addition to matching statistically identical non-borrowers with borrowers, our estimates also control for household pre-treatment income and assets, which may be associated with unobservable factors affecting both credit participation and the outcomes of interest. The PSM estimates of binary treatment effect show significant and positive impacts of borrowing on education and healthcare spending. However, multiple ordered treatment effect estimates reveal that only formal credit has significant and positive impacts on education and healthcare spending, while informal credit has insignificant impacts on the spending.

JEL Classification

C14; C21; H81

Keywords

matching
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1. Introduction

Microfinance has increasingly attracted attention from the global development community because it is considered a powerful tool in poverty alleviation strategies in developing countries (Microcredit Summit 2004). A common argument for microfinance is that it may help keep household production stable and mitigate adverse shocks; thus it helps to prevent school dropout and reduction in spending on healthcare (Armendariz and Morduch 2005; Dehejia and Gatti, 2002; Edmonds, 2006; Jacoby and Skoufias, 1997; Maldonado and Gonzalez-Vega, 2008; Ranjan, 2001). The effects on education and health are critical to sustainable poverty reduction since they affect the quality of human capital formation and the productivity of future generations.

There is debate about the impact of microfinance (Cull, Kunt and Morduch 2009) including its impact on education and healthcare of borrowing households. If, for example, access to credit raises female economic activity it may lead to children being taken out of school to replace maternal inputs in the care of younger siblings or to work in expanded household businesses. The debate has resulted from mixed evidence on microcredit impacts. On the one hand, microcredit has positive impacts on education, for example Pitt and Khandker (1998) find girls receive more schooling if households borrow from the Grameen Bank. On the other hand, some studies find no effects or adverse effects on child education (Hazarika and Sarangi 2008; Islam and Choe 2009; Morduch 1998). Likewise, in terms of health, Pitt, Khandker, Chowdhury and Millimet (2003) find higher weight-for-age and height-for-age amongst children of Grameen Bank borrowers, but Coleman (1999 2006) finds negative impacts of microcredit on healthcare spending by households in Northeast Thailand.

One difficulty in evaluating the impact of microcredit is that borrowers and non-borrowers typically differ in both observable and unobservable characteristics. The borrowers may self-select into borrowing activities due to their better characteristics. This makes it hard to form a counterfactual of what would have happened to the borrowers in the absence of credit and clouds interpretation of any estimated treatment effects. If studies fail to correct for this self-selection problem, the estimates will give naïve and overestimated results of the impact (Coleman 2006). One estimation approach that may better suit this problem is propensity score matching (PSM) where treatment effects are estimated by simulating a randomized experiment, matching households in the treated group with households in the control group that are as alike as possible – based on observable factors. It is then assumed that the matched households would have no systematic differences in response to the treatment, so they provide a valid counterfactual. Proponents state that PSM can replicate benchmarks from randomized experiments when used appropriately (Dehejia and Wahba 2002).

In this paper, a new survey, designed by the first author to meet the conditions under which PSM works well, is used to examine the impact of household credit on education and healthcare spending by the poor in peri-urban areas of Ho Chi Minh City, Vietnam. In addition to matching statistically identical non-borrowers with borrowers, our estimates also control for household pre-treatment income and assets. These pre-treatment variables may be associated with unobservable factors affecting both credit participation and the outcomes of interest, so inclusion of these variables helps deal with the self-selection problem that may have biased some previous estimates of microcredit impacts.

In addition to the use of PSM, two other important features of the current analysis warrant comment. *First*, our evidence comes from a newly industrializing peri-urban area on the outskirts of a city of over seven million people. In contrast, most studies of microcredit impacts have been for rural households.¹ Poverty is becoming more urban and the poor are urbanizing more rapidly than the population as a whole (Ravallion, Chen and Sangraula, 2007). Thus, it is important that studies of microcredit expand to cover urban areas. The impacts of microcredit may differ between urban and rural areas, particularly for our outcomes of interest, since human capital is typically the most important household assets in urban areas and is rewarded more than in rural areas (Goetz and Rupasingha, 2004; Sicular et al, 2007). Also, urbanites consume less from own production and rely more on the market; so the influence of idiosyncratic shocks like illness and loss of employment may be larger in urban areas than in rural areas. Household credit can be a useful tool to fill the income gap created by the shocks; thus, in urban areas credit may be used to support consumption expenditure on healthcare, school fees and food rather than production expenses as found in rural areas (Barslund and Tarp 2007; Johnson and Morduch 2007).

The *second* important feature of this analysis is that it considers both formal and informal credit. Most previous studies examine the impacts of formal or program credit but do not consider effects that credit from other sources has on the outcomes of interest (Coleman 1999, 2006; Khandker, 2005; Morduch 1998; Pitt and Khandker 1998). Hence, the estimated treatment effects may include both those from the program participation and also those from other credit provided by relatives, friends, neighbours and informal moneylenders. On the other hand, our survey captures all sources of credit and the results reported below compare the effects of formal and informal credit. Access to formal credit is often influenced by policy makers, but there is less leverage over informal credit, so distinguishing their separate impacts is of interest.

¹ Previous studies in Vietnam just focused on the rural areas (e.g., Quach, Mullineux and Murinde, 2005; Nguyen 2008).

The remainder of the paper is organized as follows. Section 2 reviews previous studies of household credit impacts on education and healthcare. Section 3 discusses the estimation methodology. The empirical results are reported in Section 4. The final section presents concluding remarks.

2. Previous Literature

Credit may affect household demand for education and health in two ways (Armendariz and Morduch 2005, p. 201). On the one hand, microcredit may help households earn higher income, which raises consumption and increases the demand for healthcare and children's education. On the other hand, if microcredit causes higher female employment, it then may decrease children's schooling if children have to replace mothers' input into the care of younger siblings or work in enlarged household businesses.

There is mixed evidence on these potentially opposing effects. Inadequate schooling in poor countries is often attributed to lack of access to credit since households facing adverse shocks and having insufficient access to credit may pull children out of schools to reduce household expenditure and increase labour income by increasing working hours, including child labour (Dehejia and Gatti 2002; Edmonds, 2006; Jacoby and Skoufias 1997; Kurosaki, 2002; Ranjan 2001). In addition, borrowing households may take children out of school to work in family businesses (Hazarika and Sarangi 2008) because small loans, a typical type of loan for poor households, are often associated with higher interest rates and short-term repayment conditions; the loans therefore require high returns to repay (high) interest rates to lenders. To meet these requirements, poor borrowers may reduce their costs by using their own labour, which may include child labour. For example, Beegle, Dehejia and Gatti (2004) in a study on Vietnam find households who borrowed from higher interest rate sources use more child labour.

Impacts on health and education may also interact. For example, if borrowing enables parents to provide medicines promptly once children are sick, then it may shorten sickness time and keep children at school. Healthier children may have better school performance, which helps keep children at school longer so they more productive adults. In contrast, lower school achievement and attendance are associated with child malnutrition (Glewwe, Jacoby, and King 2000). Healthcare services such as pasteurization, health insurance, family planning and pregnant-mother care are observed to be consumed more by microfinance clients than non-clients (CGAP, 2003).

3. Analytical Framework

3.1 Data

A sample of 411 borrowing and non-borrowing households was interviewed in early 2008 in the peri-urban District 9, Ho Chi Minh City (HCMC) Vietnam.² Since our focus is on microcredit impacts on poor households, the sample was selected from a list of poor households whose initial income per capita was below the HCMC general poverty line of VND 6 million (approximately US\$1 per day).³ We employed a two-step sampling, first selecting wards and then households. The target sample size was set at 500 households, including 100 reserves, to achieve a realised sample of 400. In fact, 411 households were successfully interviewed, accounting for 26% of the total number of poor households in each of the selected wards in the district. The interviewed sample provides 304 borrowing households and 107 non-borrowing households, with 2,062 members, 955 (46.3%) males and 1,102 (53.7%) females. The sample is likely to be representative for the poor group whose initial income per capita is below the poverty line at the survey time in the district but will not be representative for Ho Chi Minh City nor for Vietnam.

The survey was designed to collect data on household and individual demographic-economic variables, commune characteristics, household durable and fixed assets, child schooling and education expenditure, healthcare, food, non-food, housing expenditure, and borrowing activities. We also utilised GPS receivers to collect data on locations of households and facilities in order to measure distances from each household to facilities.

3.2 Impact Evaluation Problems

The most difficult part of credit impact evaluations is to separate the causal effect of credit from selection and reverse causation biases which are very common to nearly all statistical evaluations (Armendariz and Morduch 2010). To net out the treatment effects from other factors, requires answering the question of how borrowers would have done without any credit participation (Armendariz and Morduch 2005, 2010). This question is not easy to answer because researchers are unable to observe the virtual outcomes needed to construct such a counterfactual.

Formally, estimating the impact of credit participation is to measure the difference in the outcome between treatment and control groups, that is, $E(Y|D=1) - E(Y|D=0)$ where Y is the outcome, and D is the treatment taking value 1 if receiving treatment and 0 if otherwise. The difference in the outcome, however, may result from differences in observable characteristics, differences in unobservable characteristics, or from the treatment (credit participation). Estimates will be biased if one does not control for the differences in

² HCMC has 24 Districts. District 9 has the 5th lowest population density, with a population of 227,816 (in 2008).

³ The list was provided by the District Department of Labour, Invalids and Social Affairs.

observable and unobservable characteristics. The differences in the observable characteristics cause ‘overt bias’, which can be removed by controlling for observables (X_i) in estimation models (Lee, 2005). Thus, the impact is now $E(Y|D=1, X_i) - E(Y|D=0, X_i)$. However, the estimated impact may also include a ‘hidden bias’ resulting from unobservable characteristics. Design-based studies such as those with a randomised selection of treatment and control groups can help in this regard because the randomization enables us to cancel out the differences in both observable and unobservable characteristics between the two groups. But in credit impact evaluation, it is very hard to conduct the randomization with human subjects due to motivation and contamination problems (Mosley 1997).

Therefore, there are usually some problems in measuring the impact using non-experimental data because of non-random placement of credit programs and self-selection into credit participation by borrowers. The estimates of the causal effect can have selection bias if credit participation is correlated with unobserved characteristics that also affect the outcomes. For instance, households that are better motivated to invest in children’s schooling may have higher demand for credit. Without an adequate measure of motivation, this omitted factor may make an observed correlation between credit and schooling seems like a causal effect.

For our sample, the non-random placement of credit borrowing is not an important issue because all the surveyed households in the sample have income per capita under VND6,000 thousand, so are eligible for preferred credit (i.e. subsidised interest and easy conditions) from government funds. Selection by informal lenders and self-selection into credit borrowing due to unobservables, however, may occur. If data on pre-treatment variables of interest are available, researchers may examine differences in these variables in order to see whether there is a positive or negative selection on unobserved characteristics, conditional on the observed characteristics. If Y_0^T and Y_0^C are the outcomes for treated and control groups at time 0 (before the treatment), and after controlling for the observables, $E(Y_0^T | D=1, X_i) \neq E(Y_0^C | D=0, X_i)$, one should suspect unobservable confounders are affecting the treatment and outcomes, i.e. there exists ‘hidden bias’ caused by the unobservable confounders. Lee (2005, p. 125) recommends that controlling for Y_0 (together with X_i on the right hand side) may to some extent reduce the hidden bias. In our case, we do not have pre-treatment data on the variables of interest but we could use pre-treatment (baseline) income per capita as a control variable, as suggested by Mosley (1997), Heckman and Smith (1999), and McKenzie, Gibson and Stillman (2010).

$$Y_{ij,t-1} = \alpha + \beta.D_{ij,t} + \lambda.X_{ij,t} + \varepsilon_{ij,t-1} \quad (1)$$

where Y_{ij} is the outcome of interest of household i in ward j ; D is a dummy variable representing if a household borrows (1) or not (0), X is a set of unchanged (or little changed)

control variables over time (household characteristics). The coefficient β shows whether borrowers have higher or lower income per capita than non-borrowers prior to participating in the borrowing activities, conditional on their observed characteristics. If β is positive, that means a positive selection on unobserved attributes exists, borrowers tend to be richer than non-borrowers, which will lead the non-experimental estimators to overstate the impact of credit participation.

3.3 Methods for Measuring Impacts

Experimental data are not available in our case, and thus we need to employ non-experimental methods. The non-experimental methods try to construct counterfactual outcomes for borrowers as if they had not borrowed, and then compare the current outcome with the counterfactual.

Quasi-experimental Methods

In the experimental method, the control group is similar to the treatment group in terms of both observed and unobserved attributes by using the randomization procedure (Bryson, Dorsett, and Purdon, 2002). In contrast, the quasi-experimental method tries to create a comparable control group by asking: ‘what would the treatment group have done without the treatment?’ (Armendariz and Morduch 2005, 2010). To do so, there are two widely used approaches: matching and difference-in-differences estimator (DD). In our case, we do not apply the DD because data for the estimator are not available, hence we will only discuss the matching estimator.

Matching selects non-participants who have similar observed characteristics to participants in order to generate a control group. Matched comparison and treatment groups are now similar in terms of observed characteristics (Dehejia and Wahba, 1999, 2002). The main advantage of the matching method is that one can draw on existing data sources, so it is quicker and cheaper to implement. Nevertheless, matching does not control for unobservable characteristics that may cause selection bias, and as a result, the reliability of estimates is reduced or sensitive (Smith and Todd, 2005). The most widely used matching method is propensity score matching. Other methods of matching on each X (covariate matching) create a problem of high dimensionality which requires large datasets.

The propensity score matching (PSM) method first estimates the propensity score for each participant and non-participant on the basis of observed characteristics, and then compares mean outcome of participants with that of the matched (similar in terms of scores) non-participants. In other words, the purpose of the PSM is to select comparable non-borrowing households among all non-borrowing households to generate a control group, and then compare the outcome of the treatment and matched control groups. The crucial

assumption is that amongst non-borrowers, those with the same or similar characteristics to borrowers should have the same outcomes as what the borrowers would have had without credit participation. This assumption is called unconfoundedness or conditional independence assumption (CIA) (Rosenbaum and Rubin, 1983). The underlying point of this PSM is that control and treatment units with the same propensity score have the same probability of assignment to the treatment as in randomised experiments (Dehejia and Wahba 1999).

The PSM method may produce estimates with low bias if datasets satisfy three conditions (Dehejia and Wahba 2002): (i) data for treatment and control groups are collected using the same questionnaire; (ii) both treatment and control groups are drawn from the same locality; and (iii) the dataset contains a rich set of variables relevant to modelling credit participation and the outcomes. The similarity of treatment and control groups in terms of observable characteristics will increase the likelihood of getting matched and reduce the bias. Since all surveyed households of the current study were poor prior to credit participation, the PSM method should produce less biased estimates than for a sample of the general households whose income per capita may be highly divergent. Heckman, Ichimura and Todd (1997) argue that a subpopulation of treated units is often of more interest than the overall population; and Dehejia (2005) emphasizes the better feasibility of the PSM method if applied to subgroups.

The PSM method allows control for potential bias such as non-placement and self-selection on observed characteristics into program participation (Dehejia, 2005; Dehejia and Wahba, 2002). However, this method still fails to control for unobservable characteristics which may create the hidden bias because the scores are calculated on the basis of observed characteristics only. Dias, Ichimura and Berg (2007) argue that if the treatment assignment and the outcome are affected by unobservables, the matching may give biased results because the method is unable to control for them. Observed characteristics may not fully capture individual motivation, ability and skills which may affect the treatment participation. Success of the PSM depends on how close the control group is to the treatment group in terms of space and time, and the two groups should have as little baseline difference as possible (Lee, 2005).

Non-experimental Methods

To estimate impact of credit, one may utilize simple regression equation is as follows:

$$Y_{ij} = \alpha + \beta.D_{ij} + \gamma.X_{ij} + \varepsilon_{ij} \quad (2)$$

where Y_{ij} is the outcome of interest of household i in ward j ; D_{ij} is a dummy representing if a household borrows (1) or does not (0); and X_{ij} is a set of control variables. OLS estimation to equation (2) assumes that all differences (except for the credit participation status) between

borrowers and non-borrowers affecting outcomes can be captured by the regressors X_{ij} in an OLS regression, and the coefficient of interest, β , just reflects the impact of credit participation and not any omitted variable bias.

However, the selection into credit participation on unobserved characteristics may create a non-zero correlation between ε_{ij} and D_{ij} . Therefore, selection bias on unobservable characteristics is beyond OLS estimator's accountability. The OLS estimates may be therefore biased. To correct for the biases caused by correlation between error term and credit participation, IV method is a potential candidate especially for cross sectional data. The IV method needs good instruments which predict the participation but do not affect the outcome of the treatment. When using IV models, one should bear in mind that the tests for validity of instruments and weak instruments are very important. When instruments are valid but weak, the IV estimator may be even more biased than the OLS estimators (Murray, 2006; Stock and Yogo 2002).

Our potential instruments are household assets acquired over 24 months prior to the survey, pre-treatment income per capita, and distance to the nearest bank or credit institution. These variables may affect credit participation but not outcomes. We conducted the under-identification, over-identification and weak identification test. The tests show that our instrument candidates are weak, and hence the IV estimates may be highly upward biased.

We also implemented the Hausman test, the test results accept the hypothesis that the difference between conventional method estimates and IV estimates is not systematic. So we are able to conclude that the instruments are weak and it is not appropriate to apply IV models in our study. In addition, these instruments may not have valid exclusion restriction if they partly affect both the credit participation and the outcomes (education and healthcare expenditure). For instance, households having shorter distance to the nearest bank also have shorter distance to schools and healthcare centres because banks, schools and healthcare centres are typically located in community/ward centres. As a result, the distance to the closest bank, as an IV, may influence both credit participation and outcomes (education expenditure and healthcare expenditure).⁴

4. Empirical Results

In this section, we start with a simple test for self-selection into credit participation in Sub-section 4.1. Sub-section 4.2 presents PSM estimates of the impact on education and healthcare expenditure. Sub-section 4.3 applies a simple strategy to detect unobserved selection bias by employing the multiple treatment effect method.

⁴ We do not report test results in this paper but they will be provided upon request.

4.1 Self-selection into credit participation

As discussed in sub-section 3.2, in this sub-section we conduct the test for positive selection by regressing pre-treatment income on credit participation status, conditional on household observed characteristics, as in the equation 1. We observe a positive selection of borrowers (positive β). The borrowers and non-borrowers are observed to be different in terms of not only observed characteristics such as age, household size, and location (Appendix 1) but also in terms of unobservable characteristics (Table 1). Conditional on the household head's gender, age, education, and marital status, and on household size and ward dummies, the pre-treatment income difference is VND171 thousand and is statistically significant at the 10% level. In logarithms (the last column of Table 1), borrowers' pre-treatment income is observed to be 7% higher than that of non-borrowers (statistically significant at the 5% level).

Table 1: Testing for positive selection into credit participation (OLS estimation)

Explanatory variables	No control	Controls(1)	Controls(2)
Credit participation (yes=1)	86.86 (0.86)	170.72 (1.81)+	0.068 (2.05)*
Head's gender (male=1)		44.39 (0.45)	0.017 (0.53)
Household head's age		40.37 (1.97)*	0.013 (1.89)+
Head's age squared		-0.36 (2.00)*	-0.000 (1.90)+
Head's education (years of schooling)		0.50 (0.04)	0.001 (0.17)
Head's marital status (married=1)		65.77 (0.58)	0.016 (0.42)
Household size in logarithm		-180.11 (2.16)*	-0.065 (2.45)*
Long Truong ward		-918.24 (7.20)**	-0.226 (4.98)**
Long Phuoc ward		-238.79 (1.87)+	-0.020 (0.44)
Phuoc Binh ward		-609.26 (4.40)**	-0.119 (2.44)*
Constant	3,505.50 (39.26)**	3,034.35 (5.08)**	7.918 (38.24)**
R-squared	0.002	0.202	0.160
Observations	411	411	411

Notes: Robust t statistics in parentheses; +significant at 10%; *significant at 5%; **significant at 1%; Dependent variable is the pre-treatment income per capita (in VND1,000) in Control and Controls(1), and in natural logarithm in Controls(2). The ward TNPA is set as a reference dummy for other wards.

Income per capita prior to credit participation may capture a host of unobservable attributes (e.g. entrepreneurial ability, skills, motivation) which affect outcomes of credit participation such as education and healthcare expenditure, and also affect the likelihood of credit participation. In other words, the hypothesis that the borrowers are self-selected in terms of the unobservable characteristics is plausible. Therefore, non-experimental estimators

that fail to control for unobservables might overestimate impacts. But controlling for the initial variables such as income and assets may reduce the bias caused by the unobservable attributes (Mosley, 1997, p. 14). Indeed, Dehejia and Wahba (1999, 2002) state that PSM can be a reliable estimator if the pre-treatment earnings are controlled.

4.2 PSM estimation

In this section, kernel (with the default bandwidth of 0.06) and radius matching (with the default radius of 0.1) results of the credit impact on education and healthcare expenditure are discussed. The sets of controlling covariates should meet conditions of matching controlling variables discussed in Imbens (2004), Lee (2005), Rosenbaum and Rubin (1983) among others. In some cases, interaction terms were also used to achieve balancing in estimating the propensity scores. Appendix 4 presents discussion on how we chose covariates in the score estimation stage.

Impact on education expenditure

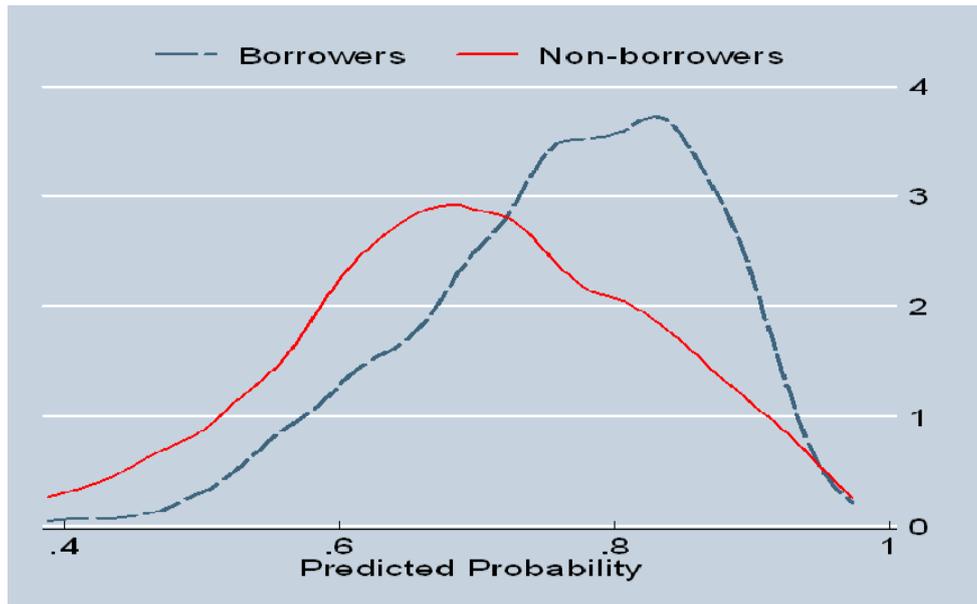
Our base specifications (S_1 and S_3 in Table 2) use a set of covariates of household characteristics such as house head's gender, age, education and marital status, school-aged child ratio, number of children and ward dummies to estimate the scores. Though we do not have panel data to apply the difference-in-difference matching estimator which is believed to be considerably better than cross-sectional matching estimators, inclusion of the pre-treatment household income and assets may reduce bias associated with unobservable characteristics (Imbens and Wooldridge 2009; Mosley 1997). The credit effects when pre-treatment income and assets are included in the matching are reported in the second (S_2) and fourth row (S_4) of Table 2. The purpose of changes in model specifications between S_1 and S_3 , and between S_2 and S_4 is to check the sensitivity of the effect.

Figure 1 shows the kernel densities of the propensity scores when pre-treatment income and assets are included alongside the other controlling variables (S_4 in Table 2). Our matching satisfies the overlap and common support assumption. The figure illustrates a substantial overlap in the distributions. The propensity scores range from 0.418 to 0.943 and from 0.174 to 0.940 for borrowers and non-borrowers, respectively,⁵ and⁶ but the means of scores are not much different (0.761 and 0.675 for borrower and non-borrower groups, respectively). The following estimation of the average treatment effect is restricted to the area of common support, where the two distributions overlap. Thus, some non-borrowers who are quite unlike the borrowers are not used in the comparison.

⁵ Probit estimation for constructing propensity scores is reported in Appendix 2.

⁶ Some studies suggest that the estimation should be in the range of 0.1 to 0.9, but there are 44 observations having scores greater than 0.9 (about 11% of the sample); if dropped, the estimates will be misleading (Crump *et al.* 2009).

Figure 1: Propensity of Scores for Borrowers and Non-borrowers to Estimate ATT for education expenditure



Note: The propensity scores of control units outside the common support were cut off.

Table 2: The average treatment effect on monthly average education expenditure in VND1,000 using matching estimators with whole sample

Control variables in the propensity score estimation	Treated/ controls	Kernel matching	Radius matching
Head's gender, head's age, head's education, marital status, school-aged child ratio, and ward dummies (S_1)	304/107	92.696 (31.967)**	98.696 (32.393)**
$S_2=S_1$ plus initial income in log, initial assets in logarithm	304/101	85.020 (34.027)*	93.022 (31.506)**
Head's gender, head's age, head's education, marital status, number of children from 6 to 18, and ward dummies (S_3)	304/107	87.447 (33.875)**	93.179 (34.182)**
$S_4=S_3$ plus initial income in log, initial assets in logarithm	304/101	81.232 (34.621)*	86.861 (34.448)*

Notes: Bootstrapped standard errors in parentheses with 1,000 repetitions, statistically significant at 10% (+); 5%(*); 1%(**). Only few households (10 households) have more than or equal 4 children aged 6 to 18 years old, to get balanced easier we group them into households having 4 kids. S_i are model specifications.

The estimates of the average treatment effect of credit participation on the treated (ATT) are reported in Table 2 for the whole sample.⁷ There is little difference in results between the two matching approaches used. Matching just on household characteristics and location dummies (S_1 and S_3), the effect of credit is observed to be statistically significant at the 1% level. After including the pre-treatment income and assets (S_2 and S_4) the estimated impact of credit participation on education spending declines but is still significant at the 5% level.

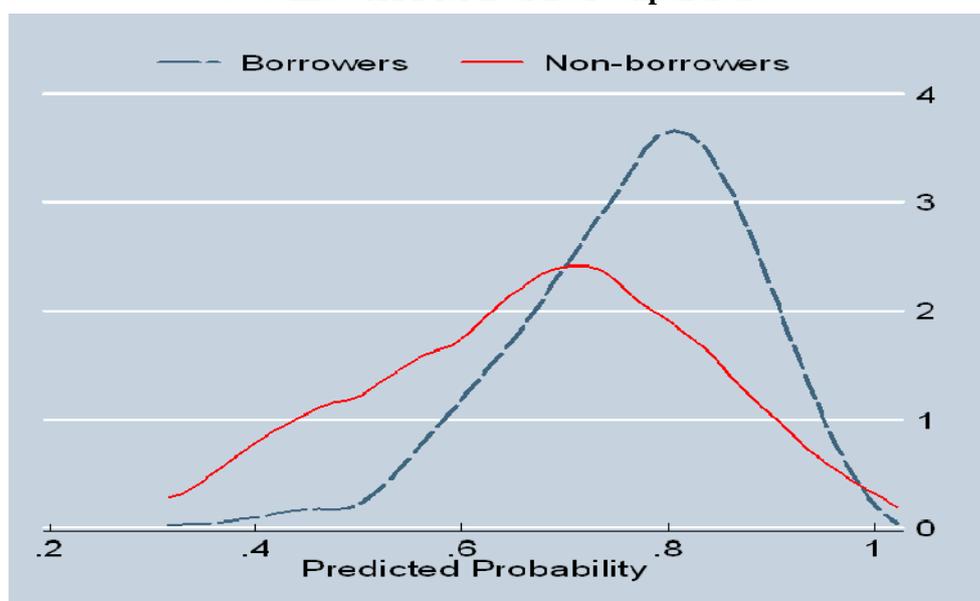
⁷ Estimations of the whole sample and a sub-sample of households having school-age children give very similar results since PSM selects similar non-borrowers in the control group to construct the counterfactual outcomes.

According to these PSM estimates, the borrowers on average spent about VND81 to VND99 thousand more on education per month than do their similar non-borrower counterparts. These estimates are lower than those from the Tobit model (which were about 119 to VND133 thousand, equivalent to about US\$7.1 to US\$7.8).

Impacts on healthcare expenditure

Figure 2 shows the kernel densities of the propensity scores estimated for evaluating the impact of credit on healthcare expenditure. The scores are from when the pre-treatment income and assets are included alongside the other controlling variables in constructing the matches (S_4 in Table 3). The propensity scores range from 0.348 to 0.989 for borrowers and from 0.195 to 0.962 for non-borrowers.⁸ The estimation of the average treatment effect is restricted to the common support.

Figure 2: Propensity of scores for borrowers and non-borrowers to estimate ATT for healthcare expenditure⁹



Note: The propensity scores of control units outside the common support are cut off.

The estimates of credit impact on healthcare expenditure are reported in Table 3. The estimates show that the effect of credit participation on healthcare expenditure is positive and statistically significant no matter which set of covariates and which matching approach are used. Borrowers spent about at least VND93 thousand more on healthcare than similar non-borrowers did.

⁸ The Probit estimation for constructing propensity scores is reported in Appendix 3.

⁹ The sets of variables used for estimating scores to draw Figures 1 and 2 are different. Each set of the variables should affect both credit participation and outcomes (education expenditure in Figure 1 and healthcare expenditure in Figure 2). That is why two figures are slightly different.

Table 3: The average treatment effect on monthly average healthcare expenditure in VND1,000 using matching estimators

Control variables in the propensity score estimation	Treated/ controls	Kernel matching	Radius matching
Specification 1 (S ₁)	304/101	112.277 (48.711)*	111.267 (49.422)*
S ₂ =S ₁ plus initial income in log, initial assets in logarithm	304/97	93.082 (55.382)+	94.016 (56.441)+
Specification 3 (S ₃)	304/107	122.047 (46.442)**	131.161 (44.413)**
S ₄ =S ₃ plus initial income in logarithm, initial assets in log	304/102	108.313 (50.301)*	112.895 (48.612)*

Notes: Bootstrapped standard errors in parentheses with 1000 repetitions, statistically significant at 10% (+); 5%(*); and 1%(**). S₁: Head's gender, head's age, head's education, marital status, household size in log, head's age*gender, ward dummies. S₃: Head's gender, head's education, marital status, dummy of child below 6, number of children from 6 to 18 years old, persons from 18 to 60 years old, dummy of older than 60 years old, head's age*education, and ward dummies.

The matching estimates should be less biased than OLS estimates because matching compares borrowers only with similar non-borrowers. Nevertheless, the 'similarity' of non-borrowers to borrowers is built on observed characteristics, so bias may still exist if unobservables affect both treatment participation and outcomes of interest. The assumption is easily violated if we are unable to control for all variables, especially the unobservables that affect both the treatment participation and outcomes (Bryson, Dorsett, and Purdon, 2002). However, since we focus only on the poor, the disparity in unobservables between borrowers and non-borrowers may not be so large. Furthermore, we also controlled for household pre-treatment income and assets which are more likely to be associated with some unobservable attributes such as motivation, entrepreneurial ability and skills. As a result, the bias may be reduced and the reliability of the matching estimates improved.

4.3 Multiple ordered treatment effect

In this sub-section, multiple treatment effects are estimated to contrast the impacts of informal and formal credit on education and healthcare expenditure. An additional advantage of multiple treatment effects is that they may help to detect potential bias associated with unobservable characteristics, which estimates of binary treatment effects are unable to deal with (Lee, 2005). This usage follows from a suggestion of Lee (2005) to explore the presence of selection bias by checking whether the main scenario of treatment effect is coherent with auxiliary findings. Specifically, applying the multiple ordered treatment effects in the current context treats credit from formal sources (F) as a full treatment, and credit from informal sources (I) as a partial treatment.¹⁰ When the treatment level is increased, the effect will

¹⁰ Mean of accumulated loans *per household* is VND8,317 (about US\$500) and VND15,135 thousand (about US\$920) for informal and formal credit respectively, and average size *per loan* is VND5,229 thousand (about USD317) and 9,327 thousand (about USD566) for informal and formal credit respectively.

become stronger (a good treatment). In contrast, if the treatment is reduced, then the effect will be weaker (a bad treatment). Assume that our expectation is a positive effect, but is not confirmed by multiple ordered treatment effects, then the initial causal findings (from binary treatment) are questionable and may be due to some unobserved attributes (Lee 2005, p. 119). On the other hand, if there is no hidden bias, the treatment effect of the full treated group (F) is expected to be higher than that of the partial treated group (I), and in turn the effect (outcome) of group (I) is greater than that of the non-borrower group (N), controlling for the same set of covariates X_i .

One may question that the counterfactuals of the informal and formal groups are different, so their treatment effects are not comparable. To overcome this issue, we directly compare the informal and formal credit groups, set either of them as a control group and if the estimation outcome is consistent with the multiple treatment effect, then the unobserved confounder will be confirmed.

The estimations of the multiple treatment effects using the PSM method can employ the conventional matching estimators (Rosenbaum and Rubin 1985). In the first stage of score estimation, the multinomial Logit (or Probit) model is used (Lechner 2001). If the treatment is logically ordered, the ordered Logit/Probit is applied instead (Imbens, 2000). Nevertheless, the multinomial or ordered Logit/Probit are quite burdensome, hence a series of binary treatment estimations may be used instead (Caliendo and Hujer 2005; Imbens and Wooldridge 2009; Lechner 2001). We follow this strategy and in turn compare the formal credit group with the non-borrowing group, the informal credit group with the non-borrowing group, and the formal credit group with the informal credit group.

Estimates of the multiple treatment effects on education expenditure are reported in Table 4. The estimation procedure is similar to binary treatment effects in Sub-section 4.2. In S_1 and S_3 , household characteristics are used to construct the scores, then pre-treatment income and assets are controlled for in S_2 and S_4 . The estimated impacts for informal credit are in columns 2 and 3, and the estimates for formal credit effect are in columns 4 and 5.

The estimates show that informal credit has no significant effect on household education expenditure. In contrast, formal credit strongly affects education expenditure. Both kernel and radius matching estimators display similar estimates that are statistically significant at the 1% level. To guard against the higher impact of formal credit on education expenditure being attributed to better household characteristics (higher pre-treatment income and assets), we included these variables in the first stage of propensity score matching.

Table 4: The average treatment effect on monthly average education expenditure in VND1,000 using matching estimators with whole sample

Control variables in the propensity score estimation	Informal credit vs. Non-borrowers		Formal credit vs. Non-borrowers		Formal vs. Informal
	ATTK	ATTR	ATTK	ATTR	ATTR
Specification 1 (S ₁)	35.283 (38.173)	26.968 (37.641)	152.813 (47.642)**	159.717 (46.162)**	111.607 (44.662)*
Specification 2 (S ₂)	10.963 (40.052)	13.056 (39.539)	148.027 (46.321)**	146.784 (48.596)**	117.417 (48.373)*
Specification 3 (S ₃)	33.991 (37.867)	24.652 (36.579)	144.884 (46.097)**	159.113 (44.351)**	108.720 (42.935)*
Specification 4 (S ₄)	7.750 (39.834)	13.440 (38.605)	145.492 (45.875)**	148.440 (48.368)**	118.657 (50.221)*

Notes:

Bootstrapped standard errors in parentheses with 1,000 replications, statistically significant at 10% (+); 5%(*); 1%(**).

S₁: Head's gender, head's age, head's education, marital status, ward dummies, school-aged child ratio, and head's age*head's gender.

S₂: Head's gender, head's age, head's education, marital status, ward dummies, school-aged child ratio, head's age*head's education, initial income in logarithm, initial assets in logarithm.

S₃: Head's gender, head's age, head's education, marital status, ward dummies, number of children aged 6 to 18 years old, and head's age*head's gender.

S₄: Head's gender, head's age, head's education, marital status, ward dummies, number of children aged 6 to 18 years old, head's age*education, initial income in logarithm, initial assets in logarithm.

A further step to confirm the absence of hidden bias is to directly compare impacts of formal credit (a higher level of treatment) to informal credit (a lower level of treatment). Estimates of the difference between the formal and informal credit are shown in the last column of Table 4. The estimates are consistent across the specifications of the matching variables. The higher credit level (treatment level) leads to a greater positive impact; suggesting that serious bias due to unobservables is not detected. Consequently, the positive treatment effect of credit on education expenditure appears to be collaborated.

Likewise, we look at the impact on healthcare spending of formal and informal credit, and the difference in impacts of formal and informal credit. The impact estimates of informal credit and formal credit on healthcare expenditure are reported in Table 5. The results of the difference in impacts between formal and informal credit are presented in the last column of Table 5. The impact of informal credit is positive but only marginally significant at the 10% level. In contrast, the impact of formal credit on healthcare is more than double the effect of informal credit, although not precisely estimated (statistically significant at the 10 percent-level).

Table 5: Average treatment effect on the monthly average healthcare expenditure in VND1,000 using matching estimators

Control variables in propensity score estimation	Informal credit vs. Non-borrowers		Formal credit vs. Non-borrowers		Formal vs. Informal
	ATTK	ATTR	ATTK	ATTR	ATTR
Specification 1 (S ₁)	77.197 (45.833)+	77.037 (41.612)+	192.648 (95.163)*	198.287 (98.337)*	175.762 (85.766)*
S ₂ =S ₁ plus initial income in log, initial assets in log	65.709 (43.060)	68.638 (40.846)+	165.153 (96.364)+	183.121 (92.470)*	178.730 (92.515)+
Specification 3 (S ₃)	59.844 (45.626)	71.473 (42.105)+	200.227 (97.934)*	198.616 (97.505)*	162.392 (92.509)+
S ₄ =S ₃ plus initial income in log, initial assets in log	60.404 (44.646)	66.845 (44.254)	195.088 (97.652)*	194.632 (96.055)*	161.437 (97.067)+

Notes:

Bootstrapped standard errors in parentheses with 1,000 replications, statistically significant at 10% (+); 5% (*); 1% (**). In the last column of S₁ and S₄, the interactions are dropped to get balanced in the estimation of propensity scores.

S₁: Head's gender, head's education, marital status, head's age, household size in logarithm, ward dummies, head's age*gender.

S₃: Head's gender, head's education, marital status, dummy of child below 6 years old, number of children aged 6 to 18 years old, number of persons aged 18 to 60 years old, dummy of older than 60 years old, ward dummies, marital status*head's gender.

Using multiple ordered treatment effects can either undermine (if unobserved biases are present) or enhance (if no unobserved biases) findings of the initial binary treatment effect. While the multiple treatment effect method itself is unable to overcome unobservable bias, it helps to avoid being misled in interpreting binary treatment effect estimates (Lee, 2005, p. 121). In the current case, the higher treatment level has greater positive impacts on healthcare and education expenditure, suggesting that there are no other potential factors or confounders affecting credit participation and healthcare/education expenditure. As a result, the positive treatment effects of credit on healthcare and education are confirmed.

5. Concluding Remarks

This paper presents estimates of the impacts of credit participation on the poor's education and healthcare expenditure in peri-urban areas of HCMC, Vietnam using data from a new survey designed to meet the conditions for the PSM method.

The PSM estimates of the average treatment effect on the treated show that borrowers spent more on education and healthcare than their similar non-borrowers. Credit participation has highly positive and significant effects on the poor's healthcare and education spending in the peri-urban areas. The PSM estimates may be less biased than OLS estimates because PSM compares borrowers with similar non-borrowers. We focus on the poor so that the disparity between treatment and control units is little. We also controlled for the pre-treatment income which is more likely to be associated with some main unobservable

attributes such as motivation, entrepreneurial ability and skills. Therefore, our estimation strategy is likely to reduce the bias and improve the reliability of the matching estimates. Furthermore, all the treated units are within the common support and no treated units are dropped when estimating the ATT effect, thus our estimates may not be misleading.

This study also employs the multiple treatment effects and shows that only formal credit impacted positively and significantly on household education and healthcare spending. The ordering of results suggests that no other important unobserved factors substantially affected credit participation and the outcomes; hence the reported effects of household credit on education and healthcare spending may be robust.

APPENDICES

Appendix 1: Descriptive statistics and *t*-values for equal means by borrowing status

Variables	Borrowers		Non-borrowers		<i>t</i> -value
	Mean	Std.Dev	Mean	Std.Dev	
<i>Pre-treatment or fixed variables</i>					
Head's gender (male=1)	0.507	0.501	0.505	0.502	0.03
Head education (year)	4.911	3.35	4.664	3.76	0.60
Married (yes=1)	0.648	0.478	0.607	0.491	0.74
Head's age	52.901	13.97	59.467	15.46	3.87**
Household size	5.191	2.343	4.523	2.597	2.34*
Children below 6 years old (yes=1)	0.309	0.463	0.178	0.384	2.89**
Children from 6 to 18 years old	1.118	1.024	0.869	1.100	2.05*
Persons from 18 to 60 years old	3.230	1.694	2.692	1.793	2.71**
Older-than-60 persons (yes=1)	0.352	0.478	0.533	0.352	3.25**
Distance to nearest bank (Km)	2.226	2.098	1.804	1.900	1.93+
Distance to nearest market (Km)	1.409	1.032	1.085	0.872	3.10**
Have a phone (yes=1)	0.809	0.394	0.644	0.481	3.18**
Internet/newspapers (yes=1)	0.053	0.224	0.037	0.191	0.68
Have a TV and radio (yes=1)	0.944	0.230	0.925	0.264	0.66
Durable and fixed assets acquired over 24 months prior to the survey	849,924	821,335	786,097	795,593	0.71
Pre-treatment income per capita	3,592	814	3,505	925	0.86
<i>Post-treatment variables</i>					
Total monthly food expenditure	2,122.6	1,247	1,874.3	1,355	1.66+
Total monthly non-food expenditure ^(a)	1,525.3	1,612	1,206.2	1,309	2.04*
Total monthly education expenditure	269.10	332	155.25	239	3.80**
Total monthly education expenditure ^(b)	324.67	347	234.51	267	2.21*
Total monthly health care expenditure	299.67	582	220.84	552	1.25
Total monthly housing expenditure ^(c)	199.39	274	145.64	163	2.41*
Monthly expenditure (food, nonfood, education, healthcare, housing)	4,416.1	2738	3,602.2	2,597	2.75**
Monthly expenditure per capita	918.18	589	878.41	533	0.60

Notes: *t*-value statistically significant at 10% (+), 5% (*), and 1% (**); assets, income, and expenditures are in VND 1,000. ^(a)This includes daily and yearly non-food expenditure excluding health, education and housing expenditure; ^(b)for a sub-sample of households having children below 18 years old; ^(c)this includes garbage disposal, electricity bill, drinking and water bill, housing maintenance expenses. Exchange rate USD/VND=16,481 in 2008.

Appendix 2: Probit estimation for constructing the propensity scores to estimate impacts on education expenditure for the whole sample

Control variables	Model specification			
	(1)	(2)	(3)	(4)
Head's gender (male=1)	-0.0910 (0.55)	-0.0926 (0.55)	-0.1002 (0.60)	-0.1051 (0.63)
Household head's age	-0.0165 (3.14)**	-0.0157 (2.94)**	-0.0175 (3.44)**	-0.0170 (3.29)**
Head's education (years)	0.0151 (0.67)	0.0132 (0.58)	0.0162 (0.71)	0.0143 (0.62)
Head's marital status (married=1)	-0.0767 (0.44)	-0.1038 (0.59)	-0.0960 (0.55)	-0.1270 (0.71)
School child ratio	0.5207 (1.34)	0.6705 (1.70)+		
Children from 6 to 18			0.1105 (1.62)	0.1373 (1.98)*
Pre-treatment income in log		0.5527 (1.90)+		0.5593 (1.92)+
Pre-treatment assets in log		0.0627 (1.20)		0.0653 (1.25)
Long Truong Ward	0.5473 (2.78)**	0.6253 (2.91)**	0.5404 (2.75)**	0.6166 (2.86)**
Long Phuoc Ward	0.4852 (2.49)*	0.4820 (2.44)*	0.4761 (2.44)*	0.4704 (2.39)*
Phuoc Binh Ward	0.2571 (1.23)	0.2725 (1.23)	0.2191 (1.04)	0.2242 (1.01)
Constant	1.1461 (2.86)**	-4.2521 (1.68)+	1.2127 (3.19)**	-4.2458 (1.68)+
LR χ^2	26.86	32.29	27.70	33.34
Prob $>\chi^2$	0.000	0.000	0.000	0.000
Observations	411	411	411	411

Notes:

Absolute value of z statistics in parentheses, + significant at 10%; * significant at 5%; ** significant at 1%; among 411 households, there are 304 borrowing households and 107 non-borrowing households. The ward TNPA is set as a reference dummy for other wards.

Appendix 3: Probit estimation for constructing the propensity scores to estimate impacts on health care expenditure

Control variables	Model specification			
	(1)	(2)	(3)	(4)
Head's gender (male=1)	-0.8468 (1.50)	-0.7924 (1.40)	-0.0963 (0.57)	-0.1096 (0.64)
Head's age (year)	-0.0312 (3.98)**	-0.0302 (3.83)**		
Head's education (years)	0.0112 (0.48)	0.0099 (0.42)	0.0622 (0.96)	0.0581 (0.90)
Head's marital status (married=1)	-0.3759 (1.93)+	-0.3876 (1.97)*	-0.1372 (0.77)	-0.1556 (0.87)
Household size in logarithm	0.6680 (4.28)**	0.6957 (4.37)**		
Child below 6 years old (yes=1)			0.3426 (2.02)*	0.3146 (1.84)+
Children aged 6 to 18			0.1194 (1.74)+	0.1426 (2.05)*
Persons aged 18 to 60			0.0967 (2.06)*	0.0977 (2.05)*
Older than 60 person (yes=1)			-0.3832 (2.06)*	-0.3779 (2.03)*
Pre-treatment income in logarithm		0.6096 (2.04)*		0.5950 (2.02)*
Pre-treatment assets in logarithm		0.0249 (0.46)		0.0334 (0.64)
Head's age*gender	0.0143 (1.42)	0.0130 (1.29)		
Head's age*education			-0.0007 (0.64)	-0.0007 (0.60)
Long Truong Ward	0.4348 (2.16)*	0.5547 (2.52)*	0.5245 (2.60)**	0.6387 (2.91)**
Long Phuoc Ward	0.4086 (2.05)*	0.4100 (2.04)*	0.5036 (2.56)*	0.5049 (2.54)*
Phuoc Binh Ward	0.0366 (0.17)	0.0837 (0.37)	0.1223 (0.55)	0.1665 (0.71)
Constant	1.3743 (2.54)*	-4.0387 (1.56)	0.0202 (0.08)	-5.3013 (2.11)*
LR χ^2	46.45	51.25	36.99	41.81
Prob $>\chi^2$	0.000	0.000	0.000	0.000
Observations	411	411	411	411

Notes:

Absolute value of z statistics in parentheses, + Significant at 10%; * significant at 5%; ** significant at 1%; among 411 households, there are 304 borrowing households and 107 non-borrowing households.

Appendix 4: Choice of covariates for the propensity score estimation

In the PSM method, choosing covariates is important because they affect the estimation outcomes. According to Lee (2005, p. 44), chosen covariate X_i must be pre-treatment and affect both outcome (Y) and the treatment (D – credit participation). In addition, to avoid the causality bias, X_i should not be affected by D, hence post-treatment covariates should not be controlled for because they will remove part (or all) of the effect of D on Y.

The unconfoundedness assumption or conditional independence assumption (CIA) (Rosenbaum and Rubin, 1983) implies that the observable control covariates should not be affected by treatment, and the outcomes of interest are independent of treatment assignment. Thus, included variables should either be fixed over time or be measured before the treatment intervention (Caliendo and Kopienig 2008, p. 38). The pre-treatment measured variables also must not be affected by anticipation of the treatment participation (Imbens, 2004).

Furthermore, variables should be excluded if they are either unrelated to the outcome or not proper covariates of the treatment participation decision model (Bryson et al, 2002; Rubin and Thomas, 1996). A variable that affects only credit participation but not treatment outcome is not necessary to control for because the outcome of interest is not affected by this variable. On the other hand, if a variable affects only the outcome but not the treatment participation, one should not control for since the variable will not make any significant differences between the treatment and control groups. Consequently, only variables that influence simultaneously the participation decision and outcome should be included in the score estimation stage (Bryson et al, 2002, p. 24).

Finally, Dehejia and Wahba (1999) and Heckman, Ichimura and Todd (1997) state that exclusion of important variables could seriously increase bias in estimates. But a covariate is not, or only weakly, correlated with outcomes and the treatment may reduce precision of estimates (Imbens, 2004, p. 23). In the presence of uncertainty, however, it is better to include too many rather than too few covariates (Bryson et al, 2002, p. 25).

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