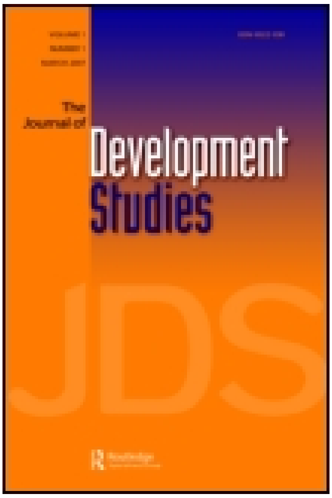

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The Impact of Microcredit on Child Education: Quasi-experimental Evidence from Rural China

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ABSTRACT *This article assesses causal effects of formal microcredit on children's educational outcomes by using household panel data (2000 and 2004) in north-west rural China. The unobservables between borrowers and non-borrowers are controlled in static and dynamic regression-discontinuity designs. The static analysis reveals significant positive impact of microcredit on schooling years in 2000 only, and no influence on academic performance for either wave. The dynamic analysis shows progressive treatment effects on both longer schooling years and higher average scores. Formal microcredit improves education in the longer term compared to the short term, and hence may help relaxing the grip of educational poverty traps.*

1. Introduction

The first two goals of the Millennium Development Goals (MDGs) attests to the indispensable relevance of education to the course of reducing poverty and achieving desired economic growth and development. In rural China, high educational expenditure, together with out-of-pocket cost of illness, is reported to be the main causes of poverty (Gustafsson & Li, 2004). Moreover, low education could breed poverty traps, as found by Knight, Li, & Deng (2010) based on a national household survey in 2002.

Credit constraints – a major factor that stifles growth of income – would further aggravate the above vicious circle. Under the government's rigorous intervention on financial institutions in rural sectors and coupled with the absence of well-functioning financial markets, lack of credit has been prevailing among Chinese rural households. Rui and Xi (2010) and Dong, Lu, and Featherstone (2010) show that lack of credit retards growth of income, consumption and agricultural productivity in China. In addition to economic situation, credit and liquidity constraints put huge barriers in children's educational attainment in rural China (Yi et al., 2012). Given the importance of early childhood investment, the constraints on credit for education faced by Chinese rural households are likely to impair the lifetime accumulation of human capital for the children and therefore, incur inter-generational transfer of poverty (Lochner & Monge-Naranjo, 2011).

Microfinance has been tagged as a developmental strategy that possesses an innovative instrument to combat poverty and raise economic well-being in developing countries where credit constraints are binding – for example, Imai and Azam (2012) for Bangladesh, Imai, Arun, & Annim (2010) for India, and Lensink and Pham (2012) for Vietnam. For rural China, however, rigorous empirical research on the impact of microfinance is not as much as that for other developing economies. Among the limited

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existing literature, Li, Rozelle, & Zhang (2004) evaluate the impact of microcredit in south-west China and find that access to microcredit leads to out-migration. Using a recent panel dataset between 2002 and 2008 in a central province, Li, Gan, & Hu (2011) find that participation in Rural Credit Cooperatives (RCCs), which are the largest formal microcredit providers in rural China, could increase borrowing households' annual income by approximately 5 per cent and consumption by 3 per cent. They also show that the larger the loan size, the more the income and consumption would increase.

It is widely acknowledged that the effect of microfinance transcends income generation (Weiss & Montgomery, 2005), as it provides insurance to mitigate adverse shocks on income and consumption smoothing (Islam & Maitra, 2012) and therefore prevents reduction in educational and health expenditure (Armendáriz de Aghion & Morduch, 2005). However, expanded production and investment materialised by microfinance could pull children out of schools or lower their scores because the borrowing family may require more child labour for its businesses and/or for substituting parents' care of his/her siblings and housework (Islam & Choe, 2013).

In accordance with the above twofold arguments, there is mixed empirical evidence on the impact of microfinance on education. Doan, Gibson, & Holmes (2011) document a positive causal effect of formal microcredit on household educational expenditure in Vietnam. Maldonado and González-Vega (2008) show that microcredit increases child schooling in the Bolivian context. On the contrary, Coleman (1999) and Banerjee, Duflo, Glennerster, & Kinnan (2013) find no linkage between access to microfinance and higher education expenditure in Thailand and Indian slums. A caveat to these existing studies is that they focus on only short-term impact. Little attention has been put on the medium- or long-term impact of borrowing behaviour. Islam (2011) argues that it may take time for households to build reputation for a large loan to be invested and the returns to an investment may also vary in different time horizon. Thus, consistent and repeated microfinance loans may be particularly relevant to education investment. It might be the case that obtaining a loan in one year makes households earn more in the following years and, therefore, their children would be able to stay longer in the school, rather than being pulled out for expanded family business.

At the same time, there are methodological flaws revolving around the assessment of the impact of microfinance, which results in many inconclusive findings on the outcome of microfinance (Hermes & Lensink, 2011). Credible impact evaluation of microcredit programmes relies on addressing two key challenges: selection bias for the individual; and the non-random placement bias for the microcredit programme. Either of the two problems would make borrowers systematically different from non-borrowers and therefore bias the estimated impact. This is because many microfinance schemes do not have strictly exogenous criteria to enforce participation (Weiss & Montgomery, 2005) and, therefore, quasi-experimental approaches especially, matching methods fail to correct for household unobserved characteristics that affect simultaneous participation and the outcomes (Smith & Todd, 2005). To alleviate these problems, use of randomised control trials is on the ascendency (Banerjee et al., 2013). However, it is costly to implement and the panel data for measuring long-term effects are even much scarcer than the existing survey data.

To add to our knowledge on the effect of formal microcredit programmes in rural China, this article focuses on the non-economic well-being of beneficiaries that is an integral element to reducing chronic poverty: child education. In this article, we investigate whether children can benefit from their families' borrowing behaviour of formal microcredit in both the short and medium term. The analysis is based on household panel data in Gansu, a poor and land-locked province in northwest China. The microcredit programme in our analysis is RCCs, which are the largest provider of formal microcredit to rural households in China (Li et al., 2011). It is designed to broaden households' access to credit and has branches in almost every township (Brandt, Park, & Wang 2003), which helps overcome potential placement bias. Selection bias of borrowing households is controlled for in static and dynamic regression-discontinuity designs (RDD) respectively. In addition to mimicking a quasi-experimental environment with random assignment of the treatment, RDD allows us to distinguish between immediate and prolonged effects of borrowing behaviour.

We find a causal impact of accessing formal microcredit on schooling by nearly three years in 2000, but no influence on children's academic performance for both rounds of the survey. When taking into account the progressive effects of obtaining loans, previous borrowing behaviour in 2000 led to four months more schooling and higher average scores by four to sevenpoints in 2004. The results of this study serve as inputs to policy-makers in constructing 'inclusive financial institutions' that alleviate monetary poverty measured by income or consumption and also improve general well-being of the beneficiaries in terms of building up their human capital in the longer-term.

The article proceeds as follows. The next section describes our dataset. Section 3 sets up the analytical framework. Section 4 justifies our use of RDD and presents the estimation results. Section 5 concludes by discussing some possible implications for policy.

2. Data

2.1 Data Source

We employ the Gansu Survey of Children and Families (GSCF) in 2000 and 2004.[1] It has been supported by the United Kingdom Economic and Social Research Council/Department for International Development (ESRC/DfID) Joint Scheme for Research on International Poverty Reduction, and conducted locally by the National Bureau of Statistics Gansu Branch. The first wave in year 2000 interviewed 2,000 children aged between 9 to 12 equally residing in 100 villages across 20 counties. The same 1,918 children were re-interviewed in 2004. We include those with full information on variables of our interests in the constructed panel. This leads to 1,916 observations in our analysis in each wave, with 53.3 per cent being boys.

Education in poor rural areas of north-west China is still under-developed.[2] The average educational expenditure for sample children was 2.3 times in 2004 (475.52 yuan) as in 2000 (206.64 yuan in Table 1).[3] This was driven primarily by more costly secondary education, which was 1.5 times in 2004 as in 2000. The increasing financial burden was ultimately transferred to students. Of our sample households, 29.4 per cent borrowed money through either formal or informal channels, particularly for paying education-related fees. The household educational expenditure per child accounted for 43.78 per cent of its per capital net income in 2000, and this share rose dramatically to 64 per cent in 2004 (Table 2). Burden for the poor living below the international poverty line was about 10 percentage points higher in both years than for the non-poor.

Table 1. Average educational expenditure for enrolled sample children, yuan

	All enrolled		Enrolled primary students		Enrolled secondary students	
	2000	2004	2000	2004	2000	2004
Tuition and textbooks	103.25	238.05	101.96	115.30	168.26	282.88
Stationary	22.45	52.74	22.34	29.04	27.63	61.40
Food, accommodation and transport	11.63	100.01	10.52	12.20	66.87	132.08
Supplemental lessons	0.35	10.82	0.30	0.88	2.33	14.45
Uniform	54.54	56.60	53.44	49.19	81.50	58.44
Other educational costs	14.42	17.30	13.84	6.52	43.77	21.23
Total	206.64	475.52	202.42	213.13	390.37	570.48
No. of observations	1891	1682	1854	450	37	1212

Notes: As not all enrolled sample children in 2004 reported their school types, the sum of enrolled primary and secondary students in 2004 (columns 4 and 6) is less than the total number. of enrolled children (column 2). We do not include scholarships or financial assistance as only 3 and 23 students in 2000 and 2004 were awarded this with average values of 13 and 81 *yuan* respectively. One may concern that merit–based financial aid would change students' behaviour, for example, increasing their efforts, and therefore upwardly bias the estimated treatment effects. This is less of a problem in our analysis as scholarships are given on the household financial basis.
Source: Data on price deflators come from the China Data Centre, University of Michigan. All monetary variables are translated in real terms at 2004 prices by using the consumer price index in Gansu province.

Table 2. Household educational expenditure, yuan

	Full sample		Poor households[a]	
	2000	2004	2000	2004
Household educational expenditure per child (semester)[b]	203.56	522.22	171.46	322.26
Household per capita net income (annual)	731.96	1289.77	515.21	736.01
Average educational burden[c] (%)	43.78	64.03	53.50	73.12

Notes: [a]Poor households are those whose consumption per capita is below the US$1.25/day adjusted to rural–urban price gap in China.
[b]The figure is calculated as the average educational expenditure of all enrolled children in the household.
[c]The figure is calculated as the average ratio of household per capita educational expenditure in two semesters (assuming child attendance of a complete academic year) over household per capita annual net income across all sample households.
Source: Data on price deflators come from the China Data Centre, University of Michigan. All monetary variables are translated in real terms at 2004 prices by using the consumer price index in Gansu province.

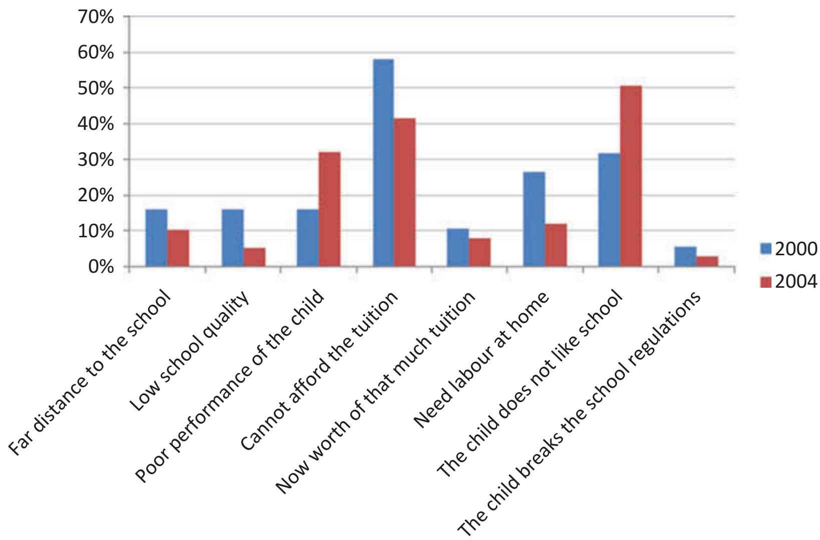

Figure 1. The reasons of suspending education.
Note: The height of the bar represents the share of those reporting that the relevant concern is the reason of their drop-out in all students currently suspending their education.

Parents, albeit poor and credit constrained, do care about children's education. Ninety-seven per cent of households attributed attaining good education to a very important means for a happy life in adulthood. However, increasing educational expenditure incurred drop-outs. As shown in Figure 1, 57.9 per cent of those suspending education in 2000 reported that one of the reasons for drop-out is unaffordable educational fees. This share declined in 2004, which was possibly due to households' increased income, but still explained 41.3 per cent of the drop-outs.[4]

The participation in RCCs is on the voluntary and household basis. The village client manager and the committee of asset appraisal first evaluate the credit qualification (five levels) of intended households, including household demographic and economic information and past history of borrowing, and then issue accordingly a certificate of loan stating the maximum amount of loans permitted. The loan allowed by RCCs varies from 1,000 to 50,000 yuan in the study areas, supporting clients' various activities including both production and consumption plans.[5] The client decides whether, when and how much to borrow within their credit limit. The repayment is on a quarterly basis and the certificate of loan is re-evaluated annually according to the client's up-to-date situation. Households in our panel showed relatively high access to RCCs: 43.3 per cent and 35.2 per cent took loans in 2000 and 2004 respectively. The average loan size was 1,825.8 yuan in 2000 and 3,413.7 yuan in 2004, registering an increase of 87 per cent. However, the difficulty of borrowing was intensified over time. In 2004, 60.5 per cent of sample households felt that it was difficult to obtain loans from RCCs compared to the situation three years ago. This might explain the lower share of borrowers in 2004 than in 2000.

2.2 Construction of Variables

We use two indicators to measure children's educational outcomes and take into account both quantity and quality of education: schooling and academic performance. For the former, we follow Maldonado and González-Vega (2008) and construct a variable of schooling gap to capture the phenomena of late entry, failed grades and suspended schooling from time to time as described in Section 2.1. That is:

$$\textit{schooling gap} = \max\{0, \textit{expected schooling} - \textit{observed total years of schooling}\} \tag{1}$$

where the expected schooling is defined as:

$$\textit{expected schooling} = \begin{cases} 0 & \textit{if age} \leq 6 \\ (\textit{age} - 6) & \textit{if } 7 \leq \textit{age} \end{cases} \tag{2}$$

and:

$$\textit{observed total years of schooling} = \textit{actual (levels) years of schooling} - \textit{repeated grades} \tag{3}$$

The schooling gap reflects the difference between children's actual years of schooling since their first enrolment in primary schools and the desired years schooling at their age. It is zero for those successfully completing their education without any late entry, repeated grades or drop-out. The presence of any one of these problems will make the value of schooling gap positive. For academic performance, we use the average score of the sample child's Chinese and mathematical tests in the most recent semester they attended.[6] Table 3 presents complete descriptive statistics.

3. Methodology

3.1 Regression-Discontinuity Design with a Time-Variant Assignment Variable

Given that RCCs do not set a single criterion for lending but their managers evaluate the risk of intended households, we first construct an 'assignment variable' in the literature of RDD which determines the household's treatment receipt of RCCs. Specifically, we estimate households' borrowing behaviour by a standard probit regression:[7]

$$p_i = \mathbf{1}(\alpha_0 + Z_i\alpha_1 + Z_v\alpha_2 + u_i > 0) \tag{4}$$

where p_i takes the value of one if the household i is a debtor to RCCs at time t and zero otherwise. The selection of explanatory variables is compatible with the regulations by which the client manager issues RCCs[8] and other possible determinants based on the past literature. In particular, Z_i denotes household characteristics, including factors considered by the manager to estimate the client's risk of default and the ability of repayment, whether borrowing RCCs for educational purposes and availability of informal loans for the households such as from friends, neighbours and relatives. Z_v is a set of village dummies in order to control for common time trend and other unobserved heterogeneity.

Table 3. Descriptive statistic

Variables	2000 Mean	2000 SD	2004 Mean	2004 SD
Schooling gap	1.707	1.143	2.282	1.916
Average scores	73.179	13.279	73.310	13.551
Microcredit (yes=1)	0.457	0.498	0.352	0.478
Child characteristics				
Age	11.042	1.153	15.088	1.159
Gender (boy=1)	0.533	0.499	0.533	0.499
Health status	4.246	0.949	4.090	0.925
Ethnic minority (yes=1)	0.019	0.136	0.019	0.136
Birth order	1.724	0.804	1.718	0.786
Child labour	2.878	6.183	8.925	10.356
Capability of studying	2.954	0.747	3.311	0.937
Siblings' education	12.590	7.018	1.677	0.809
Attending the nearest school (yes=1)	0.975	0.157	0.922	0.268
Patents' characteristics				
Father's education	6.954	3.518	6.818	3.962
Mother's education	3.875	4.345	4.200	3.629
Parents' attitude: child education	3.567	0.704	3.825	0.473
Parents' attitude: child's income	2.032	0.582	2.080	0.589
Women's empowerment on child education	1.886	0.474	1.887	0.502
Household characteristics				
Ln(household wealth per capita)	7.089	1.068	7.397	1.084
Ln(sample child's tuition)	5.203	0.611	5.923	0.801
Ln(sample child's other education costs)	4.491	1.023	5.330	1.264
Teacher and school characteristics				
Teacher's average education	12.025	0.949	14.152	1.160
Student–teacher ratio	24.241	8.704	22.496	13.290
% of unsafe classrooms	0.198	0.297	0.226	0.353
Village characteristics				
Distance to the nearest primary school (km)	0.579	0.876	3.273	3.100
Distance to the nearest junior middle school (km)	1.051	0.333	4.310	4.235
Age at the first enrolment	6.674	0.672	6.295	0.886
Proceed to secondary education[a]	0.892	0.201	0.113	0.162
Percentage of RCCs borrowers	0.591	0.286	0.301	0.242
Ln(village per capita income)	5.754	2.651	6.352	2.346

Note: [a]It is proxied by the enrolment rate of junior high school in village in 2000 and the share of completing junior high school in village primary students in 2004. Due to data limitations, we cannot obtain exactly same indicators in two surveys.

The predicted probability of borrowing $\hat{p}_i$ is an index of the household ability/willingness to borrow and is our 'assignment variable'. A household is considered to borrow RCCs (that is, receiving the treatment), if its ability/willingness to borrow is higher than 50 per cent. Therefore, the probability of being treated can be written by a function that is discontinuous at 0.5.

$$b_i = \mathbf{1}(\hat{p}_i \geq 0.5) \tag{5}$$

In the context of more than one cross-sectional datum, the treatment receipt of RCCs hinges on household changing ability/willingness to borrow over time relative to the cut-off that confines the outcome of whether or not the household would borrow (Van der Klaauw, 2008). Here, we follow Van der Klaauw (2008) and repeat the above estimation for each round of the surveys to obtain the household ability, $\hat{p}_{it}$, which could vary over time for the same household i. By doing so, we actually treat two surveys separately as if they are independent of each other. We will take into account dynamic treatment in the next sub-section.

The description of RCCs in Section 2 suggests that households do not necessarily borrow up to their credits limits, although their qualification allows this (higher than 0.5). The imperfect compliance among those 'eligible' clients conforms to a 'fuzzy' regression-discontinuity design (FRD in Imbens and Lemieux [2008]), which will be elaborated in the rest of this sub-section.

In the presence of self-selection of borrowing RCCs, there are the unobservables relating to both the households' ability/willingness to borrow and their actual treatment status. However, since households are unable to precisely control for their ability/willingness to borrow, everyone close to the assignment threshold would have similar chance of having their ability index higher or lower than 0.5. In other words, borrowing RCCs is randomly assigned for households whose predicted probability of borrowing is within a narrow interval around c, which is akin to a quasi-experiment (that is, a local randomised experiment). Therefore, causal impact of households' borrowing of RCCs on their children's education outcomes can be identified locally by comparing those children with their families' predicted probability of borrowing barely passing the threshold c (the treatment group) with those barely below it (the control group), that is, the local average treatment effect (LATE). This leads to expression of the idea of comparing the treatment and control groups at the cut-off as:

$$\begin{aligned} E(\theta\,|\,\hat{p}_{it}=0.5) &= \lim_{\hat{p}\downarrow c} E(y_{it}\,|\,\hat{p}_{it}) - \lim_{\hat{p}\uparrow c} E(y_{it}\,|\,\hat{p}_{it}) \\ &= \theta\left[\lim_{\hat{p}\downarrow 0.5} E(b_{it}\,|\,\hat{p}_{it}) - \lim_{\hat{p}\uparrow 0.5} E(b_{it}\,|\,\hat{p}_{it})\right] + \left[\lim_{\hat{p}\downarrow 0.5} E(\varepsilon_{it}\,|\,\hat{p}_{it}) - \lim_{\hat{p}\uparrow 0.5} E(\varepsilon_{it}\,|\,\hat{p}_{it})\right] \end{aligned} \tag{6}$$

In the quasi-experimental environment around the threshold, households' imprecise control over their ability/willingness to borrow means that $E(\theta\,|\,\hat{p}_{it})$ and $E(\varepsilon_{it}\,|\,\hat{p}_{it})$ are continuous at the cut-off (Hahn, Todd, & van der Klaauw 2001). It follows Equation (6) that the LATE is formulated as:

$$\begin{aligned} E[\theta_{it}\,|\,\hat{p}_{it}=0.5] &= \lim_{e\downarrow 0} E[\theta_{it}\,|\,b_{it}(0.5+e) - b_{it}(0.5-e) = 1,\ \hat{p}_{it}=0.5] \\ &= \frac{\lim_{\hat{p}\downarrow 0.5} E[y_{it}\,|\,\hat{p}_{it}] - \lim_{\hat{p}\uparrow 0.5} E[y_{it}\,|\,\hat{p}_{it}]}{\lim_{\hat{p}\downarrow 0.5} E[b_{it}\,|\,\hat{p}_{it}] - \lim_{\hat{p}\uparrow 0.5} E[b_{it}\,|\,\hat{p}_{it}]} \end{aligned} \tag{7}$$

Empirically, we adopt three different methods to estimate Equation (7) in an effort to attain robustness. First, Hahn et al.'s (2001) 'local Wald' estimator is employed for the non-parametric case. Drawing only upon information of observations in the neighbourhood of the cut-off, the limits in Equation (7) are calculated as $\lim_{\hat{p}\downarrow 0.5} E[y_{it}\,|\,\hat{p}_{it}] = \frac{\sum_{i\in\Psi_t} y_{it}w_{it}}{\sum_{i\in\Psi_t} w_{it}}$, $\lim_{\hat{p}\uparrow 0.5} E[y_{it}\,|\,\hat{p}_{it}] = \frac{\sum_{i\in\Psi_t} y_{it}(1-w_{it})}{\sum_{i\in\Psi_t}(1-w_{it})}$, $\lim_{\hat{p}\downarrow 0.5} E[d_{it}\,|\,\hat{p}_{it}] = \frac{\sum_{i\in\Psi_t} b_{it}w_{it}}{\sum_{i\in\Psi_t} w_{it}}$ and $\lim_{\hat{p}\uparrow 0.5} E[b_{it}\,|\,\hat{p}_{it}] = \frac{\sum_{i\in\Psi_t} b_{it}(1-w_{it})}{\sum_{i\in\Psi_t}(1-w_{it})}$ where the indicator variable $w_{it} = I\{0.5 \leq \hat{p}_{it} < 0.5 + h_t\}$ defines whether the observation lies above the cut-off with the optimal bandwidth h_t selected by Imbens and Kalyanaraman's (2009) procedures at time t; $\Psi_t = \{i\,|\,i \in (0.5 - h_t \leq \hat{p}_{it} < 0.5 + h_t)\}$ is the sub-sample containing those residing in the vicinity of the cut-off.

Second, in the semi-parametric case, we employ Van der Klaauw's (2008) two-step control function (CF) estimation. Specifically, the first step estimates the probability of treatment receipt in a standard probit specification:

$$E[b_{it}\,|\,\hat{p}_{it}] = \Pr(b_{it}=1\,|\,\hat{p}_{it}) = \gamma \cdot \mathbf{1}(\hat{p}_{it} \geq 0.5) + g(\hat{p}_{it}) \tag{8}$$

where γ measures the discontinuity at the cut-off; $g(\hat{p}_{it})$ is a quadratic piecewise function parameterised by:

$$g(\hat{p}_{it}) = \lambda_0 + \lambda_1 \hat{p}_{it} + \lambda_2 \hat{p}_{it}^2 + \left\{ \lambda_3(\hat{p}_{it} - 0.5) + \lambda_3(\hat{p}_{it} - 0.5)^2 \right\} \cdot \mathbf{1}(\hat{p}_{it} \geq 0.5) \tag{9}$$

In the second step, using Equation (8) in the outcome regression yields a reduced-form control-function augmented outcome equation:

$$y_{it} = \beta_0 + \theta E[b_{it} \,|\, \hat{p}_{it}] + X_{it}\beta_1 + X_{ht}\beta_2 + X_{st}\beta_3 + X_{vt}\beta_4 + \pi_t + k(\hat{p}_{it}) + \upsilon_{it} \tag{10}$$

where X_{it} and X_{ht} represent sample children and their families' characteristics described in Section 2.2; the school and village information is controlled by X_{st} and X_{vt}; and π_t denotes the time fixed effects. We further include a control function $k(\hat{p}_{it})$ for $E(\varepsilon_{it} \,|\, \hat{p}_{it})$ to control for the potential association between the household ability/willingness to borrow and children's educational outcomes. $k(\hat{p}_{it})$ ought to be a smooth and continuous function to insure that in the absence of the treatment, the educational outcomes are a smooth function of the ability/willingness to borrow and hence, deferential educational outcomes are the only source of discontinuity around the cut-off. Empirically, we let it take a semi-parametric form, $k(\hat{p}_{it}) \approx \sum_{j=1}^{J} \eta_j \hat{p}_{it}^j$, to accommodate non-linearity, where the power J is left determined by generalised cross-validation of data. $\hat{\theta}$ reflects the average treatment effect defined in Equation (7) (Van der Klaauw, 2008).

Third, we use a standard instrumental variable (IV) approach to estimate Equation (10). The instruments consist of the random treatment assignment b_{it} acting as the excluded instrument for households' observed borrowing status of RCCs (Hahn et al., 2001; Van der Klaauw, 2008) and the independent variables except $E[b_{it} \,|\, \hat{p}_{it}]$ in Equation (10) serving as the included instruments.

The non-parametric (local Wald) and the semi-parametric CF estimators are our fuzzy-RDD estimators. The former uses only the sub-sample whose ability is around the cut-off, while the latter makes use of the information of the full sample and allows estimation for other correlates of child education (Van der Klaauw, 2008). The standard IV is used as robustness checks. Also note that as the above estimation is implemented to each survey separately, in response to year-to-year variation in households' ability/willingness to borrow relative to the cut-off, $\hat{\theta}$ captures essentially a short-term effect of obtaining RCCs on children's education outcomes.[9] Moreover, $\hat{\theta}$ should be treated as a lower bound of the true causal impact of RCCs in the presentence of partial compliance.

3.2 Dynamic Regression-Discontinuity Design

In the context of panel data, multiple treatments become available. The dynamics in b_{it} means that a household which did not borrow RCCs before might change its mind in subsequent years. The causal effect of RCCs therefore contains two different kinds given its nature of voluntary borrowing: the intent-to-treat (ITT) effect and the treatment-on-the-treated (TOT) effects. ITT exogenously makes a household able and willing to borrow RCCs in one year and compares the eligible borrowers with non-eligible borrowers at the threshold, leaving the household's observed borrowing behaviour of RCCs in subsequent years as it is. By contrast, TOT prohibits new borrowers. It measures the effect of borrowing τ years ago on the child's current educational outcomes, had the household been unable to obtain RCCs in all subsequent years.

The LATE defined in Equation (8) equals the ITT divided by the fraction of individuals induced to borrow RCCs at the cut-off of their ability/willingness index. In the context of dynamic treatment assignment, however, TOT might be a more relevant indicator, considering that in the presence of voluntary borrowing of RCCs, those who have not participated in RCCs will never be required to borrow. Moreover, one cannot conclude the role of RCCs by looking at the ITT only, as the estimated impact of RCCs in later years might be overshadowed by the cumulative effect of loans having been obtained before. By exploiting the panel data, we are able to disentangle the cumulative impact of

RCCs on child education from the average treatment effect in Section 3.1, that is, the dynamic ITT and TOT effects separately over time.

Suppose that the child *i*'s educational outcomes are measured in year *t*, while the RCCs became available $\tau = \{0, 1, 2, \ldots, T\}$ years ago for the family h with the child i. The family h decides whether to borrow at $t - \tau$, $b_{i,t-\tau}$, and has a record of borrowing behaviour in subsequent years, $\{b_{i,t-\tau+1}, \ldots, b_{it}\}$. Adopting Cellini, Ferreira, & Rothstein's (2010) dynamic RD strategy, we estimate the ITT by the following outcome regression:

$$y_{it\tau} = \beta_0 + \theta_\tau^{ITT} b_{it} + X_{it}\beta_1 + X_{ht}\beta_2 + X_{st}\beta_3 + X_{vt}\beta_4 + \pi_t + \mu_\tau + k(\hat{p}_{it}) + \varepsilon_{it\tau} \tag{11}$$

where μ_τ represents fixed effects for years relative to the borrowing; other variables are defined as above. As shown by the subscripts in Equation (11), the observations in the standard panel data need to be rearranged in the form of child-calendar-years relative to the borrowing. In other words, there could be multiple observations in the new dataset for the same child in the same calendar year but with different time elapsed compared to the year of borrowing. The OLS is applied to the rearranged dataset to estimate Equation. (11). Taking Cellini et al.'s (2010) suggestion, we cluster standard errors by child to account for possible dependence across observations (i, t) and serial correlation in $\varepsilon_{it\tau}$ caused by the multi-use of observations (i, t).

As in Cellini et al. (2010), we then expand the equation of the definition of the ITT effect of the household's initial borrowing decision in year $t - \tau$ on its child's education outcomes at t:

$$\begin{aligned}
\theta_\tau^{ITT} \equiv \frac{\mathrm{d}y_{it}}{\mathrm{d}b_{i,t-\tau}} &\equiv \frac{\partial y_{it}}{\partial b_{i,t-\tau}} + \left(\frac{\partial y_{it}}{\partial b_{i,t-\tau+1}} \cdot \frac{\mathrm{d}b_{i,t-\tau+1}}{\mathrm{d}b_{i,t-\tau}} + \frac{\partial y_{it}}{\partial b_{i,t-\tau+2}} \cdot \frac{\mathrm{d}b_{i,t-\tau+2}}{\mathrm{d}b_{i,t-\tau}} + \cdots + \frac{\partial y_{it}}{\partial b_{it}} \cdot \frac{\mathrm{d}b_{it}}{\mathrm{d}b_{i,t-\tau}} \right) \\
&= \frac{\partial y_{it}}{\partial b_{i,t-\tau}} + \sum_{h=1}^{\tau} \left(\frac{\partial y_{it}}{\partial b_{i,t-\tau+h}} \cdot \frac{\mathrm{d}b_{i,t-\tau+h}}{\mathrm{d}b_{i,t-\tau}} \right) \\
&= \theta_\tau^{TOT} + \sum_{h=1}^{\tau} \theta_{\tau-h}^{TOT} \omega_h
\end{aligned} \tag{12}$$

where $\theta_0^{ITT} = \theta_0^{TOT}$ and $\omega_h = \frac{\mathrm{d}b_{i,t-\tau+h}}{\mathrm{d}b_{i,t-\tau}}$ measures the effect of borrowing RCCs in year $t - \tau$ on the probability of borrowing RCCs again h years later. $\hat{\omega}_h$ can be obtained by estimating Equation (11) with the dependent variable being replaced by $b_{it\tau}$. Reverting Equation (12) yields our recursive estimates of θ_τ^{TOT}:

$$\theta_\tau^{TOT} = \theta_\tau^{ITT} - \sum_{h=1}^{\tau} \theta_{\tau-h}^{TOT} \omega_h \tag{13}$$

Arguably the TOT effects depend only on the time elapsed since the borrowing behaviour at $t - \tau$, that is, h, while irrelevant to the time t or the history of past borrowing behaviour.

4. Estimation Results and Discussion

4.1 Identification in RDD

For a valid 'quasi-experimental' environment, the treatment should be randomly assigned for the sub-sample near the cut-off of the assignment variable that we have observed. This requires that the average educational outcomes for children whose families' abilities of borrowing falls barely below the cut-off should ideally form a valid counterfactual to be compared with those in the treated group. We implement the density test formulated by McCrary (2008). Figure 2 shows no significant 'jump' of the conditional density function at the cut-off is statistically insignificant in both years. The null hypothesis of zero discontinuity in the estimated density cannot be rejected at all three conventional

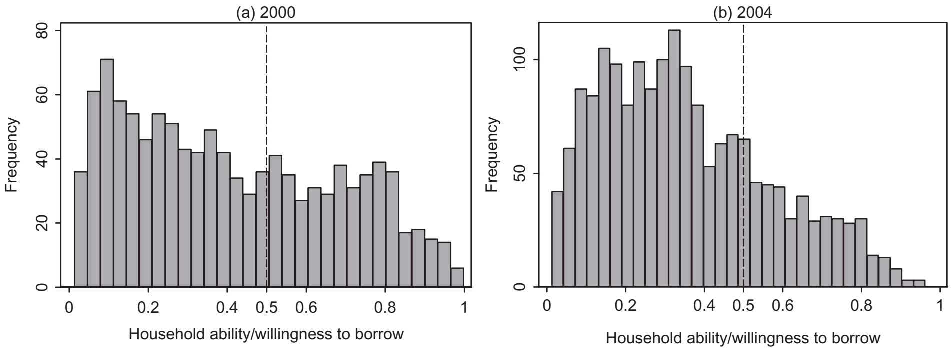

Figure 2. Distribution of household ability/willingness of borrowing microcredit.

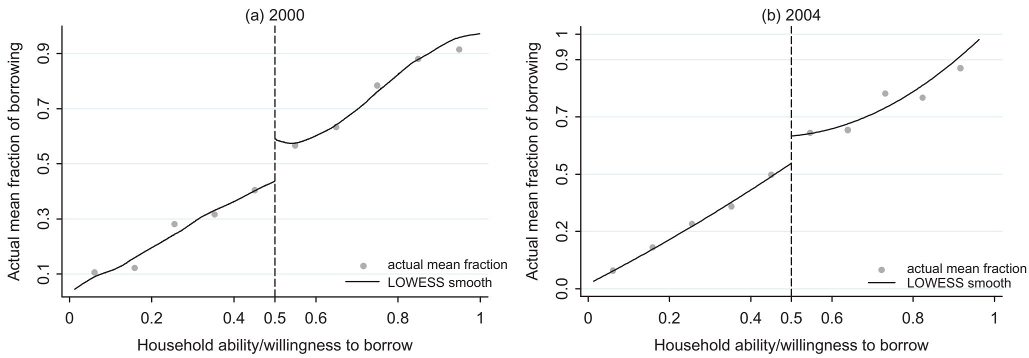

Figure 3. The relationship between actual and the predicted probability of borrowing.
Notes: Children are grouped into 5 bins left and right to the cut-off respectively. The dot is the cell mean of the indicator for whether the household actually borrows RCCs, which reflects the actual probability of borrowing. The solid line represents predicted probability of borrowing from LOWESS smoothing of those actual probabilities.

statistical levels. This may raise the conjecture that even though households could partially manipulate whether to be or not to be borrowers, no completely endogenous sorting can be inferred. According to McCrary (2008), the identification of the treatment effect under the RDD is valid.

Another crucial pre-requisite of RDD is that discontinuity in households' observed borrowing behaviour occurs at the assignment threshold 0.5. Only on observing a 'jump' in households' decision-making on borrowing can we distinguish and compare the treated and control groups and guarantee a non-zero denominator in Equation (7). Figure 3 illustrates that the higher the ability, the more likely the household is going to borrow.[10] There was an increase of 10–11 per cent in the actual fraction of households borrowing RCCs when their ability/willingness to borrow exceeds 0.5.[11] Moreover, non-zero fraction of borrowing for those barely below the assignment threshold means that some 'ineligible' households also engage in borrowing. Once crossing the cut-off, not all households begin immediately to borrow microcredit, as the actual fraction of borrowing only gradually converges to 1 along with households' higher ability/willingness. This means that our constructed assignment variable $\hat{p}_{it}$ only explains part of households' borrowing behaviour and some other unobservables also affect the household decision-making. This, together with imperfect compliance around the cut-off, just conveys the intuition of our use of a fuzzy RDD rather than a sharp one.

4.2 Contemporaneous Effects of RCCs

Consistent with our observation in Figure 4, borrowing RCCs narrowed the schooling gap by 2.88 years in 2000 (column 1, Table 4) and this finding is robust across various bandwidths (Figure A1(a)

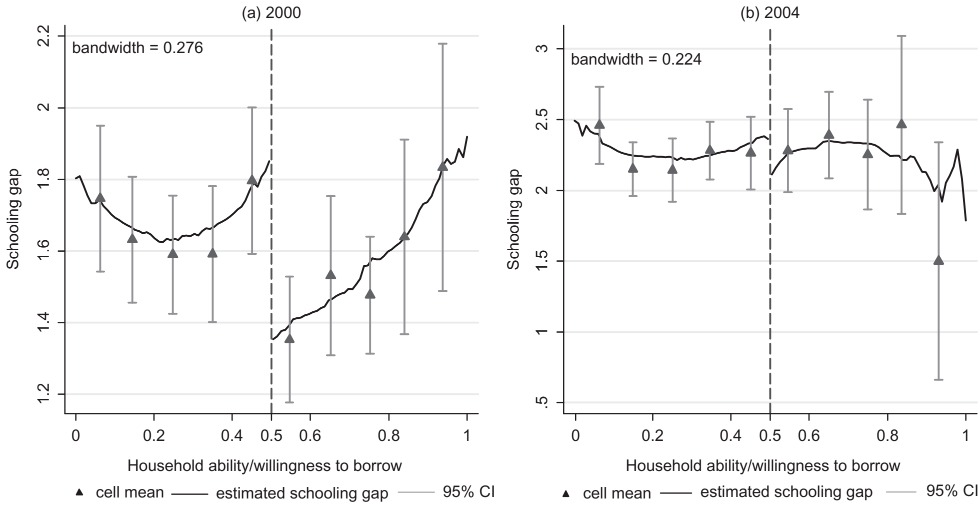

Figure 4. Schooling gap as a function of household ability/willingness index.
Notes: Households are grouped into five bins left and right to the cut-off respectively. The triangle measures the average child education for those falling in the same bin. The solid line is the kernel-weighted linear regression of the cell averages.

in the Online Appendix).[12] We further obtain statistical significance with smaller magnitude of the treatment effects of 2.56–2.85 years under the semi-parametric specification (columns 2–3, Table 4), implying that in the circumstances of soaring educational costs, RCCs could assist households more in limiting child schooling gap. It appears that in the circumstances of soaring educational costs, RCCs could assist households more in limiting child schooling gap. By contrast, all estimated treatment effects in 2004 are statistically insignificant (columns 4–6, Table 4), although their positive magnitude emerges under different bandwidths (Figure A1(b) in the Online Appendix). Significant positive impact in 2000 but not in 2004 may be linked to the reasons of drop-outs. Families reported financial difficulties as the main reason of the drop-out in 2000, while financial concerns were no longer the biggest obstacle to education in 2004, since those reporting unaffordable fees as a reason for drop-out fell by 16.6 per cent. Actually many parents still would like to support their children's schooling, although educational costs rose quickly. The proportion of parents who expected their children to go to universities in the future was 83.8 per cent. However, as shown by Figure 1, in 2004 children's dislike of their schools took over from unaffordable costs and emerged as the most frequently reported reason of not attending schools (50.5%).[13] Consequently, the positive impact of microcredit on schooling gap was only significant in 2000.

Consistent with Figure 5, obtaining RCCs loan substantially increase children's average scores by 12.5–32.8 points in 2000 (columns 1–3, Table 5) but without statistical significance. In 2004 the evidence is quite mixed. Both positive and negative effects were observed and the estimators were not robust across various bandwidths (Figure A2(b) in the Online Appendix). Parents' responses to children's academic performance and our proxy for academic performance may help explain this insignificant role. Borrowing families in general did not respond actively when children did not do well in the exams (regardless of which module). For example, 81.6 per cent (72.7%) of non-borrowing parents said they would contact teachers for children's unsatisfactory scores, while 79.8 per cent (67.8%) of borrowing parents would do so in 2000 (2004). Of non-borrowing parents, 75.7 per cent or 82.4 per cent would spend more time in helping children's homework or pay more attention to children's activities, while only 73.8 per cent and 81.7 per cent of borrowing parents, respectively, would do so. Borrowing parents leaned more towards limiting children's extracurricular activities (53%), compared to non-borrowing parents (50%). Moreover, we used the average scores of Chinese and maths to proxy for academic performance, while leaving other courses such as English, physics

Table 4. FRD estimates of impacts of borrowing RCCs on the schooling gap

	2000			2004		
	Local Wald	Two–step CF	Two–step CF	Local Wald	Two–step CF	Two–step CF
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
$\underset{\sim}{E}[\theta_{it} \mid \{\widehat{p}_{it} = 0.5]$	–2.884 (1.534)*			–3.276 (3.563)		
$\widehat{\theta}$		–2.853 (1.259)**	–2.557 (1.194)**		–1.260 (2.205)	–0.094 (1.579)
Child characteristics						
Age		0.447 (0.033)***	0.481 (0.038)***		0.551 (0.067)***	0.405 (0.122)***
Gender		–0.046 (0.062)	–0.006 (0.080)		–0.112 (0.121)	0.513 (0.191)***
Health status		–0.127 (0.042)***	–0.135 (0.045)***		–0.046 (0.092)	0.071 (0.125)
Ethnic minority		–0.218 (0.413)	0.676 (0.489)		0.289 (1.161)	–0.518 (0.784)
Birth order		0.308 (0.080)***	0.437 (0.091)***		0.036 (0.134)	0.124 (0.205)
Child labour		–0.011 (0.006)	–0.015 (0.006)**		0.007 (0.015)	0.003 (0.007)
Capability of studying		–0.068 (0.052)	–0.128 (0.067)*		–0.113 (0.065)*	–0.201 (0.126)
Siblings' edu.		–0.030 (0.007)***	–0.057 (0.012)***		–0.152 (0.132)	0.094 (0.275)
Attending the nearest school		–0.607 (0.323)*	–0.489 (0.300)*		–0.606 (0.444)	–0.898 (0.389)**
Parents' characteristics						
Father's education		–0.016 (0.011)	–0.036 (0.015)**		–0.052 (0.023)**	–0.049 (0.046)
Mother's education		–0.024 (0.008)***	0.001 (0.008)		0.013 (0.020)	–0.011 (0.042)
Parents' attitude: child edu.		–0.169 (0.052)***	–0.155 (0.061)**		–0.203 (0.148)	–0.259 (0.273)
Parents' attitude: child's inc.		–0.019 (0.054)	0.018 (0.060)		0.075 (0.086)	0.099 (0.173)
Women's empowerment on child edu.		–0.128 (0.061)**	0.008 (0.053)		–0.012 (0.083)	0.139 (0.226)
Household characteristics						
Ln(hh wealth per capita)		0.127 (0.070)*	0.131 (0.070)*		0.101 (0.107)	0.147 (0.131)
Ln(sample child's tuition)		–0.240 (0.106)**	–0.115 (0.102)		–0.904 (0.260)***	–0.021 (0.300)
Ln(sample child's other edu. costs)		–0.122 (0.053)	–0.086 (0.048)*		–0.065 (0.113)	–0.225 (0.163)
Teacher and school characteristics						
Teachers' average edu.			–0.243 (0.060)**			–1.137 (0.159)***
Student–teacher ratio			–0.020 (0.008)***			0.0003 (0.006)
% unsafe classrooms			0.159 (0.229)			1.055 (0.410) **

(*continued*)

Table 4. (*Continued*)

	2000			2004		
	Local Wald	Two–step CF	Two–step CF	Local Wald	Two–step CF	Two–step CF
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
Village characteristics						
Distance to the nearest primary school			–0.037 (0.055)			0.215 (0.140)
Distance to the nearest junior middle school			–0.118 (0.102)			0.047 (0.050)
Age at the first enrolment			0.164 (0.069)**			–0.189 (0.228)
Proceed to secondary education			0.278 (0.380)			–2.672 (1.046)**
% of RCCs borrowers			–0.625 (0.238)***			–0.889 (1.555)
Ln(village per capita income)			–0.021 (0.024)			–0.077 (0.054)
School dummies		Yes			Yes	
Village dummies		Yes			Yes	
County dummies			Yes			Yes
R^2		0.488	0.453		0.792	0.766

Note: ***, ** and * denote 1%. 5% and 10% significance levels in turn. Constants and dummies for the schools, villages and counties aie not reported. Standard errors are in parentheses and clustered by the household ability/willingness to borrow in order to mitigate possible misspecification problems, as suggested by Lee & Card (2008).

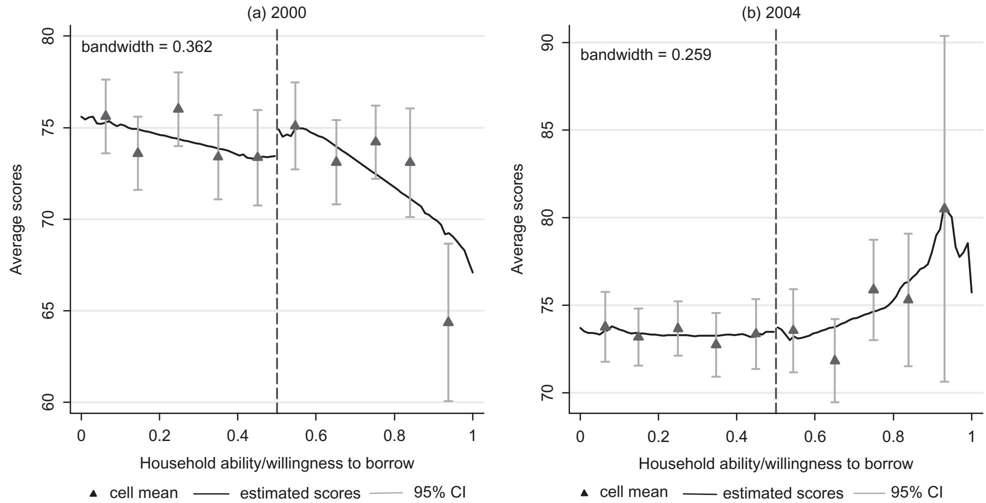

Figure 5. Average scores as a function of household ability/willingness index.
Notes: Households are grouped into five bins left and right to the cut-off respectively. The triangle measures the average child education for those falling in the same bin. The solid line is the kernel-weighted linear regression of the cell averages.

and chemistry due to lack of data. Significant positive influence would not emerge if borrowing families invest in other modules rather than Chinese or maths. This situation may be particularly relevant to the 2004 wave. We do find 17.7 per cent of borrowing families sent their children to make-up classes, as opposed to 14.1 per cent among non-borrowing families. However, 3.6 perc cent and 1.4 per cent more borrowing families would opt for English and physics respectively, but 3.4 per cent and 1.8 per cent fewer of them would choose Chinese and maths. Consequently, borrowing RCCs did not affect significantly children's academic performance measured by average scores of Chinese and maths.

It is also notable that the positive effects of RCCs on schooling and academic performance in 2000 appear to exist only in a narrow neighbourhood of the threshold. The downward slopes right to the cut-off in Figures 5(a) and 6(a) suggest that children with treatment received less schooling and lower scores as the household ability/willingness to borrow kept growing among eligible households. By contrast, the upward slopes in Figures 5(b) and 6(b) indicates that in 2004 the more able and willing the recipients of the treatment, the longer schooling and higher scores the children would achieve, although there is no significant jump in children's educational outcomes brought about immediately by the treatment. These observations raise a conjecture that the most able borrowers in 2000 might have used the loans in other places rather than in education, while those with the highest ability in 2004 might have invested more in children's education. Evidence to our conjecture can be found from the structure of the usage of the loans. Out of 1,910 households, 528 (27.3%) borrowed money particularly to pay school fees in 2000. Among them, those eligible for the assignment had invested 442.8 yuan on average in education – only one-fifth of their total loans for education.[14] In 2004, although the share of borrowers for educational purposes shrank to 22.6 per cent, the mean educational loans among educational borrowers with higher ability than 0.5 was nearly quadrupled to 1,652.3 yuan, possibly in response to the increased school costs. The average share of educational borrowing in their total loans rose to 41.9 per cent.

The above analyses show that formal microcredit is not automatically a magic bullet to tackle the problems of child education, but needs to be monitored and guided.[15] Imai et al. (2010) and Imai and Azam (2012) also find that the impact on different welfare indicators hinges on the different usage of microloans in the Indian and Bangladesh contexts respectively.

Table 5. FRD estimates of impacts of borrowing RCCs on the average score

	2000			2004		
	Local Wald	Two-step CF	Two-step CF	Local Wald	Two-step CF	Two-step CF
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
$E[\theta_{it} \mid \widehat{p}_{it} = 0.5]$	32.775 (45.926)			3.186 (38.772)		
$\widehat{\theta}$		19.982 (14.358)	12.465 (14.054)		11.845 (22.190)	–11.561 (11.811)
Child characteristics						
Age		–0.727 (0.333)**	–1.004 (0.426)**		0.365 (0.639)	–0.295 (0.917)
Gender		–0.149 (0.693)	–0.453 (0.919)		–2.099 (1.409)	–3.458 (1.652)**
Health status		–0.442 (0.414)	–0.555 (0.473)		0.129 (0.780)	–0.113 (0.904)
Ethnic minority		–11.838 (6.347)*	–13.063 (10.173)		10.486 (11.230)	1.373 (4.092)
Birth order		–0.831 (0.832)	0.084 (0.964)		–0.386 (1.097)	–3.755 (2.051)*
Child labour		0.002 (0.051)	0.067 (0.055)		–0.202 (0.155)	–0.080 (0.071)
Capability of studying		10.280 (0.586)***	10.407 (0.842)***		6.313 (0.745)***	5.949 (0.845)***
Siblings' edu.		0.044 (0.066)	–0.216 (0.154)		1.628 (1.425)	2.495 (2.388)
Attending the nearest school		–1.894 (3.149)	–2.011 (3.632)		2.815 (4.251)	0.201 (3.472)
Parents' characteristics						
Father's education		0.144 (0.115)	0.137 (0.164)		–0.064 (0.281)	–0.543 (0.268)**
Mother's education		0.045 (0.094)	–0.015 (0.088)		–0.102 (0.217)	0.042 (0.304)
Parents' attitude: child's edu.		0.898 (0.573)	1.417 (0.737)*		2.204 (1.395)	–0.131 (1.398)
Parents' attitude: child's inc.		0.373 (0.562)	0.158 (0.635)		–0.390 (0.940)	–0.386 (1.455)
Women's empowerment on child's edu.		–0.139 (0.756)	–1.510 (0.586)***		0.050 (0.708)	1.777 (1.533)
Household characteristics						
Ln(hh wealth per capita)		–0.476 (0.762)	–0.535 (0.753)		0.270 (0.976)	0.260 (0.837)
Ln(sample child's tuition)		2.746 (1.600)*	3.483 (1.585)**		1.845 (2.059)	0.617 (1.771)
Ln(sample child's other edu. costs)		–1.243 (0.513)**	–0.832 (0.652)		–0.963 (1.202)	–0.813 (1.191)
Teacher and school characteristics						
Teachers' average edu.			–0.163 (0.745)			1.114 (1.020)
Student-teacher ratio			–0.183 (0.092)**			–0.029 (0.064)
% unsafe classrooms			–1.889 (2.520)			4.056 (3.137)

(*continued*)

Table 5. (*Continued*)

	2000			2004		
	Local Wald	Two-step CF	Two-step CF	Local Wald	Two-step CF	Two-step CF
Independent variable	(1)	(2)	(3)	(4)	(5)	(6)
Village characteristics						
Distance to the nearest primary school			0.860 (0.461)*			1.467 (0.980)
Distance to the nearest junior middle school			0.117 (1.198)			–0.173 (0.302)
Age at the first enrolment			0.404 (0.836)			2.302 (1.583)
Proceed to secondary education			–6.253 (4.598)			–0.790 (8.232)
% of RCCs borrowers			–0.334 (2.657)			–8.839 (10.808)
Ln(village per capita income)			–0.270 (0.229)			0.146 (0.501)
School dummies		Yes			Yes	
Village dummies		Yes			Yes	
Comity dummies			Yes			Yes
R^2		0.505	0.476		0.611	0.594

Note: ***, ** and * denote l%. 5% and 10% significance levels in turn. Constants and dummies for the schools, villages and counties are not reported. Standard errors are in parentheses and clustered by the household ability/willingness to borrow in order to mitigate possible misspecification problems, as suggested by Lee and Card (2008).

Of other explanatory variables, older age and illness of the sample child would make children more likely to drop out. Within family competition for limited educational resources matters for both schooling gap and academic performance. Children with later birth order were less educated in 2000 (columns 2–3, Table 4) and performed worse in 2004 (column 6, Table 5) than their elder siblings. Attending the nearest schools could help reduce the schooling gap, and this effect could be stronger in 2004 than in 2000. As in 2004, 99 per cent of sample children were at the age of secondary education;, this implies that attending the nearest schools could facilitate more secondary education compared to primary education. After controlling for parents' attitudes towards education through variables of their expectations on the child's highest educational achievement and the siblings' educational levels of the sample child, parents' education was positively correlated with child schooling (Table 4), as in Yi et al. (2012), but the estimators are not consistently significant across specifications. Parents' higher expectation on children's educational attainment could keep children staying longer in schools in 2000. Such an impact is also found in Zhao and Glewwe (2010). However, the 2004 estimator is insignificant and there is only weak evidence of positive association between parents' expectation and children's academic performance (column 3, Table 5).

Women's empowerment only reduced marginally the schooling gap in 2000 (column 2, Table 4) and was even negatively correlated with children's academic achievements (column 3, Table 5). Instead of taking care of education, 8.6 per cent of mothers in GSCF with full control over children's education directly asked children to work for income, as opposed to 5.5 per cent of those having zero influence in educational decision-making.

Higher tuition fees and other unspecified educational costs were associated with smaller schooling gap and better performance in 2000 only. Brown and Park (2002) derive similar results. They attribute this to the fact that schools with better educational quality usually have higher charges for students. As excepted, better educational quality proxied by teachers' higher educational levels narrowed the children's schooling gap in both years, and the magnitude in 2004 (–1.137) was more than four times as big as that in 2000 (–0.24 to –0.25). This reflects parents' increased consideration of educational quality when deciding whether to send their children to schools. Nevertheless, the quality of schools' physical resources did not exhibit significant impact on children's academic performance, which alternatively is strongly affected by child capability for studying with the marginal effects varying from 5 to 10 points.

4.3 Dynamic Effects of RCCs

Households' ability/willingness to borrow RCCs evolves over time according to their observable and unobservable characteristics. Consequently, a previous borrower (non-borrower) may become a non-borrower (borrower) and/or households may retain previous decisions. In the presence of multiple treatments, the causal impact of RCCs derived from our static analysis in Section 4.2 is a mixed outcome of households' dynamic treatment status. This sub-section proceeds to distinguish between short- and long-run effects of microcredit by drawing upon the dynamic FRD in Section 3.2.

Table 6 presents the estimation results. The general finding on explanatory variables in the static RDD is also confirmed in the dynamic analysis, although a few lose or gain statistical significance. For example, gender difference turned to emerge in schooling gap with girls lacking behind, but disappeared in average scores.

The negative $\hat{\omega}_1$ in all columns, albeit statistically insignificant, implies that in general those who had borrowed in 2000 would be between 2.1 per cent and 5.2 per cent less likely to enter the second bids in 2004. The contemporaneous ITT effects of microcredit, $\hat{\theta}_0^{ITT}$, become insignificant compared to those in Tables 4 and 5, while borrowing of microcredit appears to improve longer-term education for clients' children. $\hat{\theta}_1^{ITT}$ in columns (1) and (2) of Table 6 suggests that children of clients in 2000 would enjoy 3.3–3.6 months less educational gap and 4.9–7.2 higher average scores in 2004 compared to their counterparts in the non-client families, leaving the 2000 clients to make subsequent decisions on borrowing as they wish.

Table 6. Dynamic treatment effects of borrowing RCC

	Schooling gap			Average scores		
Independent variable	(1)	(2)	(3)	(4)	(5)	lui
A. ITT						
$\hat{\theta}_0^{ITT}$	0.046 (0.050)	0.078 (0.064)	0.074 (0.061)	6.267 (2.297)***	–1.591 (1.399)	–1.440 (1.366)
$\hat{\theta}_1^{ITT}$	–0.278 (0.107)***	–0.299 (0.182)*	–0.288 (0.184)	2.639 (2.079)	4.915 (2.741)*	7.160 (2.833)**
Child characteristics						
Age	0.287 (0.073)***	0.131 (0.024)***	0.147 (0.025)***	–1.113 (1.401)	0.339 (0.463)	0.484 (0.498)
Gender	–0.112 (0.124)	–0.092 (0.050)*	–0.069 (0.049)	–1.514 (3.095)	0.411 (1.073)	0.081 (1.065)
Health status	0.082 (0.078)	–0.008 (0.028)	–0.027 (0.028)	0.509 (1.621)	1.135 (0.593)*	1.284 (0.625)**
Ethnic minority	–0.139 (0.277)	0.386 (0.483)	0.394 (0.462)	–13.138 (5.446)***	–0.913 (4.764)	1.868 (4.148)
Birth order	0.062 (0.167)	0.099 (0.049)**	0.084 (0.049)*	3.103 (2.660)	–1.152 (1.138)	–1.680 (1.164)
Child labour	0.018 (0.007)**	–0.003 (0.003)	–0.002 (0.003)	0.104 (0.151)	–0.051 (0.086)	–0.025 (0.089)
Capability of studying	–0.002 (0.066)	–0.056 (0.031)*	–0.048 (0.031)	5.138 (2.083)**	4.110 (0.824)***	4.324 (0.825)***
Siblings' edu.	–0.453 (0.164)***	–0.015 (0.005)***	–0.013 (0.005)***	–1.189 (4.098)	–0.013 (0.113)	0.093 (0.114)
Attending the nearest school	0.317 (0.318)	–0.143 (0.119)	–0.121 (0.126)	–8.830 (14.143)	2.931 (4.051)	5.308 (3.801)
Parents' characteristics						
Father's education	–0.049 (0.023)**	–0.018 (0.009)*	–0.019 (0.005)**	–0.261 (0.432)	0.107 (0.177)	–0.010 (0.174)
Mother's education	0.017 (0.024)	–0.003 (0.005)	0.001 (0.005)	0.547 (0.567)	0.150 (0.115)	0.183 (0.112)
Parents' attitude: child's edu.	–0.035 (0.131)	–0.064 (0.045)	–0.061 (0.045)	–4.210 (4.056)	1.544 (0.865)*	1.312 (0.860)
Parents' attitude: child's inc.	–0.072 (0.104)	0.029 (0.042)	0.028 (0.041)	–1.250 (2.734)	0.450 (0.855)	0.225 (0.820)
Women's empowerment on child's edu.	0.049 (0.109)	0.036 (0.030)	0.040 (0.031)	4.351 (1.652)***	0.795 (0.786)	0.426 (0.746)
Household characteristics						
Lu(hh wealth per capita)	0.222 (0.174)	–0.022 (0.074)	–0.036 (0.074)	4.302 (4.470)	1.680 (1.496)	1.889 (1.584)
Ln(sample child's tuition)	–0.134 (0.125)	–0.098 (0.051)**	–0.134 (0.055)**	5.238 (2.848)*	2.748 (1.021)***	2.568 (1.018)**
Ln(sample child's other edu. costs)	–0.151 (0.067)**	–0.091 (0.028)***	–0.096 (0.029)***	1.739 (1.611)	0.203 (0.524)	0.172 (0.574)
Teacher and school characteristics						
Teachers' average edu.		–0.034 (0.030)	–0.027 (0.034)		0.487 (0.623)	0.737 (0.702)
Student-teacher ratio		–0.001 (0.003)	–0.002 (0.005)		0.039 (0.088)	–0.082 (0.103)
% unsafe classrooms		0.328 (0.126)***	0.309 (0.150)		4.538 (1.747)**	2.901 (2.005)
Village characteristics						
Distance to the nearest primary school		0.026 (0.018)	–0.013 (0.028)		0.479 (0.449)	0.227 (0.524)
Distance to the nearest junior middle school		0.051 (0.050)	–0.004 (0.062)		0.311 (0.873)	2.231 (0.989)**
Age at the first enrolment		0.062 (0.032)*	0.048 (0.036)		2.526 (0.687)***	1.035 (0.761)
Proceed to secondary education		–0.175 (0.076)**	–0.162 (0.087)*		4.981 (1.835)***	6.265 (2.079)***

(*continued*)

Table 6. (*Continued*)

	Schooling gap			Average scores		
Independent variable	(1)	(2)	(3)	(4)	(5)	lui
% of RCCs borrowers		–0.018 (0.182)	0.080 (0.210)		4.323 (3.381)	3.429 (4.915)
Ln(village per capita income)		0.010 (0.010)	0.034 (0.016)**		–0.224 (0.251)	–0.101 (0.298)
School fixed-effects	Yes			Yes		
Village fixed-effects	Yes			Yes		
Year fixed-effects	Yes	Yes	Yes	Yes	Yes	Yes
Elapsed years since last borrowing	Yes	Yes	Yes	Yes	Yes	Yes
County fixed-effects			Yes			Yes
R^2	0.518	0.437	0.474	0.936	0.971	0.975
B. Recursive TOT						
$\hat{\omega}_1$	–0.024 (0.018)	–0.021 (0.034)	–0.052 (0.038)	–0.024 (0.018)	–0.021 (0.034)	–0.052 (0.038)
$\hat{\theta}_1^{TOT}$	–0.277 (0.107)***	–0.297 (0.181)*	–0.285 (0.184)	2.792 (2.078)	4.833 (2.734)*	7.085 (2.827)**

Note: ***, ** and * denote l%, 5% and 10% significance levels in nun. Constants, fixed-effects and polynomials are not reported. Standard errors in parentheses are clustered by child and those for die recursive TOT are calculated by die Delta method.

Prior to discussing the TOT estimates, we first examine the nature of our data on the dynamics of treatment receipt, in order to better understand how households build or lose their ability/willingness to borrow over time and this induced changes in treatment status. We compare past borrowing behaviour in 2000 for sub-samples lying within ±5 per cent, ±3 per cent and ±1 per cent of the cut-off in 2004. The 2000 recipiency rate of households just above the cut-off in each of the three sub-groups is 57.8 per cent, 85.3 per cent and 75 per cent respectively, and exceeds that of those just below by –31.2, 3.2 and 8.3 percentage points in turn. Similarly, we restrict samples to those within ±5 per cent, ±3 per cent and ±1 per cent of the cut-off in 2000. The 2004 recipiency rate of households just above the cut-off in each of the three sub-groups is 29.6 per cent, 29.4 per cent and 44.4 per cent respectively, and exceeds that of those just below the cut-off by 6.6, 12.7 and 44.4 percentage points in turn.

These observations bear out two implications. First, $\hat{\theta}_0^{ITT}$ is an average outcome of the cumulative effect of repeated borrowing and newly gaining RCCs status, while $\hat{\theta}_1^{ITT}$ measures the consequence of both retaining debtors and those playing a 'one-shot game' by exiting RCCs after 2000. This fleshes out our model set-up for ITT in Equation (12). Second, the cumulative impact of borrowing may explain more proportion of the ITT effects than new entry or exit. Overall, the hybrid nature of the variation in the household treatment status makes disentangling TOT from ITT effects not only important but also necessary to let our estimation results better inform policy.

Based on the estimates of $\hat{\theta}_0^{ITT}$ and $\hat{\theta}_1^{ITT}$, we disentangle the long-term impact of a single bid, $\hat{\theta}_1^{TOT}$, by exogenously authorising the borrowing of microcredit in 2000 but prohibiting subsequent borrowing behaviour thereafter. $\hat{\theta}_1^{TOT}$ is significantly negative in columns (1) and (2) but positive in columns (5) and (6), meaning that families' borrowing behaviour in 2000 alone was sufficient to narrow their children's schooling gap by 3.3–3.6 months and to improve children's average scores by 4.8–7 points. Moreover, broadly similar magnitude of $\hat{\theta}_1^{TOT}$ and $\hat{\theta}_1^{ITT}$ echoes the above exploratory analysis that protracted effects of microcredit dominate the immediate impact of borrowing behaviour in benefiting child education.

5. Conclusion

This article provides empirical evidence on the welfare effectiveness of microcredit programmes in the context of an under-developed province of China, Gansu. It assesses the causal impact of borrowing formal microcredit on children's educational outcomes in a quasi-experimental environment. We further distinguish between the immediate effects of microcredit for borrowers' welfare and longer-term consequences of borrowing behaviour.

Borrowing microcredit can bring about immediate consequences to narrowing child schooling gap in 2000 with the magnitude of nearly three years. This positive treatment effect becomes statistically insignificant over time, although RCCs appear to play a greater role in improving schooling (with 0.4 more years in 2004) when households are faced with escalating educational costs. There is no indication on improved academic performance for borrowers. On identifying causal relationship running from formal microcredit to schooling gap, pushing the lever of microcredit upwards can improve children's schooling, which in turn is likely to help Chinese rural households break the vicious circle between insufficient education and poverty and establish a virtuous circle by removing educational exclusion.

We indeed find indication on such a positive role of formal microcredit in the longer-term. When incorporating the dynamics in treatment assignment, previous borrowing behaviour takes over the role of current involvement. Irrespective of whether we prohibit households from subsequent bids, the initial borrowers' children appear to enjoy a modest reduction in schooling gap by one semester (3–4 months) and an appreciable increase in average scores by 5–7 points. While formal financial institutions need to guide the usage of microcredit especially for the most able/willing households in order to channel the loans to educational purposes for their children, policy addressing rising educational costs for poor rural households for its own sake should be emphasised. Microfinance would function better to improve welfare for its clients had education policy been designed concurrently. Moreover, considering that most clients of RCCs are relatively rich households (Li et al., 2011), formal microcredit would better contribute to child education if the targeting problem for the poor could be alleviated.

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Notes

1. There is another wave in 2007, which has not been released. It will be worthwhile to investigate more waves when they are available, in order to confirm the causal impact found in this article in a longer term.
2. The law of nine-year compulsory education in these areas cannot be fully realised and drop-out occurs frequently. Gansu is no exception to this. One reason is political decentralisation and the role of dual identity of local governments in China: local government pursue socio-economic targets based on the central government's national plan, but has immense discretion over which laws to implement (Birney, 2014). The other reason might be financial decentralisation. Secondary schools and below have been financed by local governments since 1995. This aggravates education inequality, especially in secondary schooling, as in poor areas local governments are constrained with tight budget and low capacity (Knight, Sicular, & Yue 2011).
3. Monetary values throughout this article are in real terms in 2004 prices according to the authors' calculations. Price deflators are the rural CPI in Gansu province, which come from the China Data Centre at the University of Michigan.
4. It is worth noting that all reasons are from the demand side, while the GCSF did not collect information on supply shocks, that is, whether the drop-outs are due to close of schools. However, we find that 29 per cent of sample children who attended primary schools in both 2000 and 2004 (128 out of 438 sample children) changed schools. All of those who attended middle schools in both waves (35 sample children) changed their schools in 2004. Therefore, Figure 1 might also reflect some supply shocks, which we are unable to separate from the genuine demand-side reasons.
5. For detailed information on credit levels, see http://www.gsrcu.com/www/ContentsDisp.asp?id=930&ClassId=14 (in Chinese, accessed 4 June 2012).
6. There are other courses in the curricular, such as music and natural study in primary education, and chemistry and physics in secondary education. However, it is difficult to run them in poor areas without necessary facilities and qualified teachers and the GSCF did not record relevant scores, either. Chinese and maths are the most important courses prior to higher education in China. Particularly in the GSCF, 87.5 per cent of children in 2000 were in the age of primary education and would enter into secondary education in 2004. The entrance exam for secondary education only consisted of Chinese and maths.
7. Estimation results are shown in Table A1 in the Online Appendix. It is notable that independent variables in Equation (4) may also affect the loan size. Preferably one could estimate Equation (4) jointly with the loan size determination regression to account for the fact that households end up borrowing may have different loan limits. Unfortunately, the GSCF only collected the total amount of loans from both RCCs and other financial institutions. We are unable to take into account the impact of loan size of RCCs on child education.
8. For detailed regulations in Gansu province, see http://www.gsrcu.com/www/ContentsDisp.asp?id=1015&ClassId=48 (in Chinese, accessed 3 December 2013).
9. If estimating the pooled sample instead, $\hat{\theta}$ actually measures an average treatment effect of RCCs in different years within the sample period.
10. Using higher thresholds than 0.5 enlarges discontinuity in borrowing behaviour in Figure 3. Discontinuity achieves its maximum at the threshold of 0.65 and then diminishes and finally disappears beyond 0.75. This is predictable, as more able households would be more likely to borrow, but those who are most able may have already borrowed and therefore do not suggest significant changes in their borrowing behaviour.
11. The discontinuities in Figure 3 are not statistically significant with the standard errors being 0.091 and 0.072 in 2000 and 2004 respectively. Insignificance might be caused by the fact that only a small number of observations (137 out of 1,196 households) reside around the cut-off defined by the optimal bandwidth. This in turn yields less efficient treatment effects (columns 1 and 4, Tables 4 and 5) as insignificant discontinuities enter the denominator in Equation (7).
12. We also checked how the choice of threshold would affect the estimated treatment effect by calculating the marginal threshold treatment effect (MTTE) formulated by Dong and Lewbel (2012). In the case of schooling gap in 2000, we calculated the MTTE around 0.5 (in the range below 0.75) as –3.635. It implies that a slightly higher threshold than 0.5 but smaller than 0.75 would result in larger estimated treatment effects on schooling gap in absolute terms and that a marginal increase in the threshold, say from 0.5 to 0.51, would enlarge the estimated treatment effect of borrowing on schooling gap

by 0.036 years, that is, narrowing the schooling gap from 2.88 years (column 1, Table 4) to 2.916 years. This supports our previous conjecture that the threshold of 0.5 yields a lower bound of the treatment effect. Moreover, as the discontinuity could exist until 0.75, the maximum treatment effect on schooling gap would be 3.78 years.

13. The main reason of their dislike of their schools might be their low academic performance, especial in secondary education in rural China (Yi et al., 2012). Using the GCSF plus case studies, Hannum, An, & Cherng (2011) also find that low scores in high school entrance exams deter rural children's educational progression. Another reason might be large opportunity costs of secondary education – de Brauw and Giles (2008) find a robust negative relationship between migrant opportunity with potential higher income and high school enrolment in rural China.
14. Unfortunately, the GSCF does not allow us to identify for which activity the loan has been used except education.
15. The IV estimation results are reported in Table A2 in the Online Appendix. Both static and dynamic findings in Tables 4–6 are broadly supported, but IV yields larger standard errors as also found by Van der Klauuw (2008).

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