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The impact of microcredit on child education: quasi-experimental evidence from rural China

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Abstract

This paper assesses causal effects of formal microcredit on children's educational outcomes by using household panel data (2000 and 2004) in a poor province of northwest 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 children's schooling years (captured by late entry, failed grades and suspended schooling from time to time) in 2000 only, and no indication of influence on academic performance for both rounds of survey. The dynamic analysis shows progressive treatment effects of microcredit on both longer schooling years and higher average scores. Formal microcredit appears to improve education in the longer term compared to the short term, and hence may have potential in relaxing the grip of educational poverty traps.

Keywords: microfinance, education, regression-discontinuity design, dynamic treatment effect, rural China

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1. Introduction

The first two goals of the Millennium Development Goals (MDGs) attest to the indispensable relevance of education to the course of reducing poverty and achieving desired economic growth and development. In rural China, high educational costs, together with out-of-pocket costs of illness, are reported to be the main causes of poverty (Gustafsson and Li, 2004). Moreover, low education could breed poverty traps. Based on a national household survey in 2002, Knight *et al.* (2009, 2010) find that educational enrolment and investment could be discouraged by households' low income, and less education received by children in turn tends to undermine their capability of generating income in adulthood, which possibly constitutes a poverty trap.

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, coupled with the absence of well-functioning financial markets, lack of credit has been prevailing among Chinese rural households (Park and Ren, 2001). Rui and Xi (2010) estimate a share of 71 percent of credit rationed rural households in 10 representative Chinese provinces in 2003 and the losses of 12-13 percent in income and 15-16 percent in consumption induced by the lack of credit. Using another dataset for northern China in 2008, Dong *et al.* (2010) further show that not only income and consumption, but also agricultural productivity is deterred by 31.6 percent as farming households cannot take full advantage of agricultural inputs and their capabilities and education without sufficient access to capital. In addition to the economic situation, credit and liquidity constraints put huge barriers on children's educational attainment in rural China (Brown and Park, 2002; 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 and Monge-Naranjo, 2011).

Microfinance seems to equip both scholars and practitioners with an innovative instrument to combat poverty and raise economic wellbeing in developing countries where credit constraints are binding; for example, Khandker (2005) for Bangladesh, Imai *et al.* (2010) for India, Lensink and Pham (2012) for Vietnam, and Liverpool and Winter-Nelson (2010) for Ethiopia. For rural China, however, rigorous empirical research on the impact of microfinance is still limited. Li *et al.* (2004) evaluate the impact of a Grameen Bank-style microcredit programme implemented by the United Nations Development Programme (UNDP) in 1999 in southwest China (Sichuan province). They find that access to microcredit did not stimulate asset accumulation as expected, but increased income by bringing more off-farm working opportunities to rural households, typically out-migration. Using a recent panel dataset between 2002 and 2008 in a central province, Li *et al.* (2011a) 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 five percent and consumption by three percent. They also show that the larger the loan size, the more the income and consumption would increase.

These studies appear to reveal a positive role of microcredit in improving the economic situation for Chinese rural households. Falling in this line of research, this paper also deals with the issue of effectiveness of formal microcredit programmes in rural China,[1] but focuses on the non-economic wellbeing of the beneficiaries that is an integral element to reducing chronic poverty: child education.

It is widely acknowledged that microfinance not only brings about more income by allowing households to engage in more profitable production and investment (Weiss and Montgomery, 2005), but also functions as an insurance to mitigate adverse shocks on income and consumption smoothing (Islam and Maitra, 2012) and therefore prevents reduction in educational and health expenditure (Armendáriz de Aghion and Morduch, 2005). However, expanded production and investment materialised by microfinance could pull children out of schools, because the borrowing family may require more child labour for its businesses and/or for substituting parents' care of his/her siblings and housework (Hazarika and Sarangi, 2008).

In accordance with the above two-folded arguments, there is mixed empirical evidence on the impact of microfinance on education. Doan *et al.* (2011) document a positively causal effect of formal microcredit on household educational expenditure in Vietnam. Maldonado and González-Vega's (2008) study shows that microcredit increases child schooling in the Bolivian context. On the contrary, however, Coleman (1999) and Banerjee *et al.* (2009) find no linkage between access to microfinance and higher education expenditure in Thai and Indian slums. Furthermore, the existing studies reveal only short-term evaluation, while 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 horizons. 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 school, although currently 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 and 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. The former refers to the inherent factors that determine the decision to participate in a microfinance programme and can arise from both observed and unobserved reasons for taking a loan. The majority of aforementioned literature (e.g., Doan *et al.*, 2011; Li *et al.*, 2011a; Maldonado and González-

[1] We do not consider non-governmental microfinance institutions (MFIs), considering their limited capacity in making appreciable differences nationwide to rural households' livelihood. As summarised by Zhang *et al.* (2010), the first reason for the limited role of non-governmental MFIs is that most of them locate in affluent provinces of east China, which is likely to incur a programme placement bias and therefore makes the impact evaluation imprecise. Second, the international donor agencies cannot decide their project locations or local partner agencies, which are affected by political interference. Since 2003, only governmental banks and RCCs have been allowed to engage in rural microfinance, while non-governmental MFIs are not legally sanctioned.

Vega, 2008) draws upon a 'quasi-experiment' method, typically instrumental variable estimation, difference-in-difference and propensity score matching approaches. However, many microfinance schemes do not have strictly exogenous criteria to enforce participation (Weiss and Montgomery, 2005) and the matching methods fail to correct for household unobserved characteristics that affect simultaneously participation and the outcomes (Smith and Todd, 2005). To alleviate these problems, recent advancement is the usage of randomised control trials as in Banerjee *et al.* (2009). However, it is costly to implement and panel data for measuring long-term effects are even scarcer than existing survey data. The placement bias refers to the situation that microfinance institutions implement programmes in affluent areas under the pressure of financial self-sustainability, as poorer clients are deemed to have lower demand for credit and higher risk of default. Either of the two problems would make borrowers systematically different from non-borrowers and would therefore bias the estimated impact.

To sum up, much remains to be learned about the role of microfinance in improving education for clients in rural China, nor is the previous finding credible for causal inferences. The aim of this paper is to fill in both gaps and evidence from rural China that will contribute to an improved understanding of the effectiveness of microcredit programmes in developing countries. Specifically, we investigate whether children can benefit from their families' borrowing behaviour through 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 which has overcome the potential effect of placement bias in view of the presence and distribution of microfinance programmes in the area. Selection bias of borrowing households, particularly the unobservables distinguishing borrowers from non-borrowers, is controlled for in static and dynamic regression-discontinuity designs (RDD), respectively. They do not only mimic a quasi-experimental environment with random assignment of the treatment, but also allow 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 only, 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 brought about not only four months more schooling subsequently in 2004, but also a significant rise of average scores, had the household been unable/unwilling to borrow in all subsequent years after 2000. The results of this study serve as inputs to policy makers in constructing 'inclusive financial institutions' that not only alleviate monetary poverty measured by income or consumption, but also improve the general wellbeing of beneficiaries in terms of building up their human capital in the longer term.

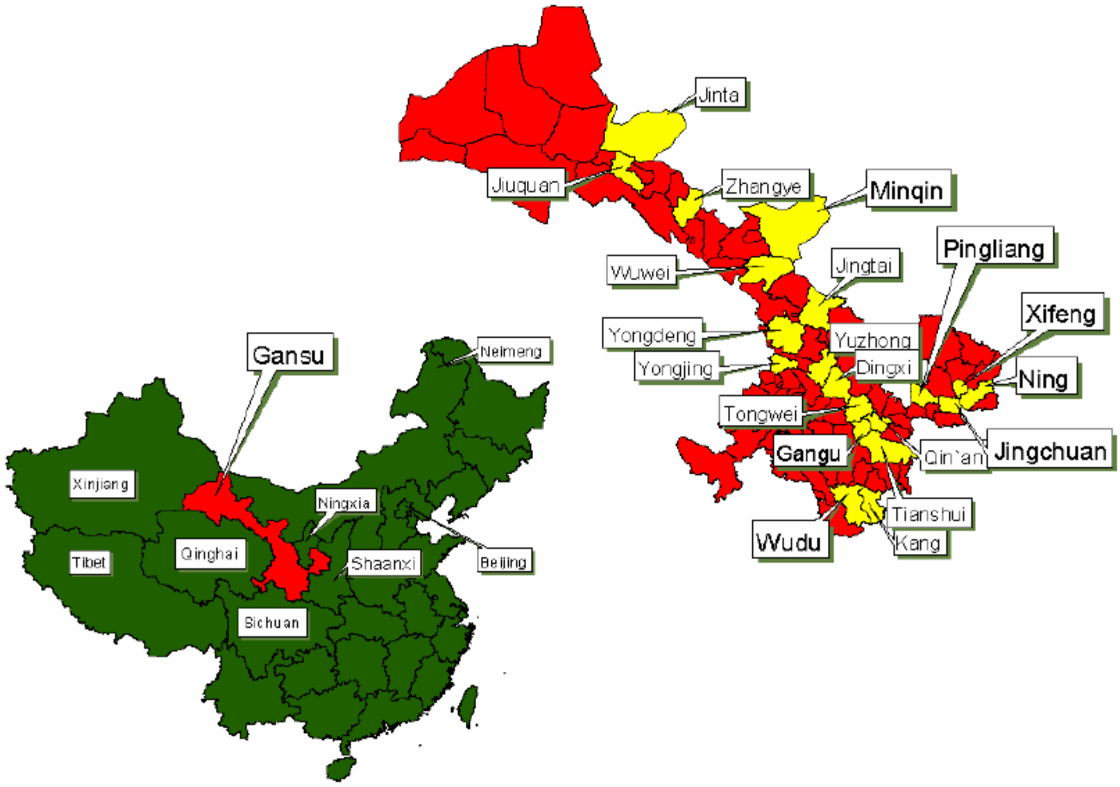
The paper proceeds as follows. The next section describes our dataset. Section 3 sets up the analytical framework. Section 4 justifies our use of RDD and presents the estimation results. Section 5 concludes by discussing some possible implications for policy.

2. Data

2.1. Data source and description of the study areas and population

We employ the Gansu Survey of Children and Families (GSCF) in 2000 and 2004. This longitudinal project collected information on rural children's education and welfare status, as well as background information on their families, teachers, schools, villages and counties. It was initially supported by the World Bank, the Spencer Foundation, and the United States National Institutes of Health, and recently 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. The survey was conducted locally by the National Bureau of Statistics Gansu Branch in collaboration with Northwest Normal University and the Centre for Disease Control. The first survey in year 2000 interviewed 2,000 children aged between nine and 12 equally residing in 100 villages across 20 counties (see Figure 1 for the study areas). 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 percent being boys.

Figure 1: Study areas in the dataset



Source: Map 1 in Hannum and Kong (2007: 8).
Note: Study counties are highlighted in yellow.

As displayed in Figure 1, Gansu is landlocked in northwest China. Only one-third of sample counties have basically plain land, while the rest – two-thirds – are hilly or mountainous. Agriculture dominated productive activities in all sample villages: 88.8 percent of the average per capita net income could be attributed to agriculture; households in 36 out of 100 villages did not engage in non-agricultural work and in the rest of villages, the average share of non-agricultural households within the village was only 6.9 percent.

Gansu has long been one of the poorest provinces in China, with its average rural household per capita net income being within the bottom three out of 31 provinces over the last two decades (1990-2010). Although rural households' income in Gansu is growing steadily over time, they have been increasingly lagging behind many of their counterparts: the average rural household per capita net income in Gansu amounted to 63 percent of the national mean in 1990, but this share consistently declined to 58 percent.[2] In our constructed panel, 30.8 percent of households were poor in 2000 if measuring their per capita consumption against the Chinese government's official poverty line, and this poverty rate dropped to 16.4 percent in 2004 due to increased income and consumption at the national level. If referring to the World Bank international poverty line at US$1.25/day adjusted to the rural-urban price gap in China,[3] the poverty rates were higher in both years (59.3 percent and 35.5 percent, respectively), but a decreasing trend still holds. Moreover, it is notable that the magnitude of poverty reduction rate is higher (23.8 percentage points) at US$1.25/day than that under the Chinese official poverty line (14.4 percentage points). As the latter line is only about two-thirds of the former, this indicates that the not-so-poor households around the international poverty line grew proportionately faster than the ultra-poor. Poverty seemed to be a longer-term phenomenon for households at the bottom of the consumption distribution.

Education in poor rural areas of northwest China is still under-developed. The law of nine-year compulsory education in these areas cannot be fully realised and drop-out occurs frequently.[4] Gansu is no exception.[5] Although the enrolment rate in 2000 was 98.7 percent (1,891 out of 1,916 children),[6] 11.8 percent of them (223 children) left schools in the 2004 re-

[2] Figures in this and the previous sentences are authors' calculation based on data from China Statistical Yearbook 2011 published by the National Bureau of Statistics.

[3] Ravallion and Chen (2007) suggest adjusting the poverty line to the rural-urban price gap in China to measure rural or urban poverty more precisely. Here we adopt their calculation that the same consumption basket in rural areas is 37 percent cheaper than in urban areas.

[4] One may be concerned with possible impact of implementation of this law on our finding in regards to the years of schooling, as implementation of this law would make our estimates of the impact of microcredit imprecise and less valid. However, the law cannot be fully abided by, especially in remote poor areas, and there are many drop-outs and suspended education for various reasons, such as liquidity constraints, poor academic performance and high opportunity costs due to huge migration and increasing wages (Yi *et al.*, 2012). Of these, financial difficulty appears to be a central obstacle – this will be discussed in following paragraphs. According to Yi *et al.*'s (2012) survey for 7,800 rural children from 2009 to 2010, drop-outs were particularly high in junior high schools (14.2 percent) and even higher for students from poor households and those with sick parents, which could breed liquidity constraints. Using the longitudinal National Fixed-point Survey from the Research Center of Rural Economy (RCRE), the Ministry of Agriculture from 1987 to 2002, Sun and Yao (2010) find that only 58.4 percent of rural students who entered primary schools after the launch of the law finished junior high school, i.e. nine-year education. Our data also suggest that, of our sample children, only 12.47 percent in 2000 and 12.53 percent in 2004 had zero educational gap – such an indicator will be explained with greater detail in Section 2.2. In view of this, the law might not substantially bias our estimates of the impact of microcredit on child education, especially in the settings of very poor areas like Gansu.

[5] See Hannum and Kong (2007) for a comprehensive investigation of child education in Gansu province based on the GSCF.

[6] This is largely due to the campaign to eliminate illiteracy in rural areas, which has long been emphasised by the Chinese government. Another reason could be that 71 out of 100 sample villages

visit. In addition to drop-out, being held back happens from time to time among rural students. By 2000, 37.3 percent (707 children) had ever repeated grades in primary schools. Of these children, the majority (84.9 percent) were held back in Grade 1 or 2; 81 percent (573 children) repeated grades once, while at least twice among the rest 19 percent. In 2004, the number of children having repeated grades declined to 483, out of which 100 had been held back at least twice. The majority of those being held back were in Grades 2-4 in primary schools, indicating that many of those reporting repeated grades in 2000 might be held back again over the period of 2000 and 2004. The progression rate from primary to junior high schools was particularly hindered. The median village progression rate was only 41.1 percent. The enrolment age was also largely delayed. Only 13.1 percent of sample children first attended primary school at the age of five or six, while the majority have delayed attendance until the ages of seven to nine.

As a result of financial decentralisation and an education law put into practice in 1995, secondary schools and below began to be financed by local governments. This aggravates education inequality, especially in secondary schooling, as in poor areas local governments are constrained by tight budgets and low capacity (Knight *et al.*, 2011). The proportion of sample schools' expenditure supported by governments decreased quickly, from 16.1 percent in 2000 to 7.7 percent in 2004, while the village-support part increased from 1.8 percent to 8.1 percent. Out of 232 sample schools, 159 (68.6 percent) were responsible for any waiver of students' fees rather than the government. This imposed great pressure on poor villages and schools and would in turn add up to educational inequality. As shown in Table 1, the average educational expenditure for sample children in 2004 was 2.3 times that of 2000. This was driven primarily by more costly secondary education, which was 1.5 times as much in 2004 as in 2000.

The increasing financial burden was ultimately transferred to students. In a country-wide survey from Brown and Park (2002), school fees accounted for half of the rural households' consumption expenditure. In the study areas of Gansu, on average 29.4 percent of our sample households borrowed money through either formal or informal channels, particularly for paying education-related fees. As shown in Table 2, the household educational expenditure per child accounted for 43.78 percent of its per capital net income in 2000 and this share rose dramatically to 64 percent in 2004. The burden for the poor living below the international poverty line was about 10 percentage points higher in both years than for the non-poor.

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run primary schools by themselves and 95 villages reported that children studied in the primary schools locating in the village.

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 & 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 obs.	1891	1682	1854	450	37	1212

Note: (a) 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 no. of enrolled children (column 2). (b) We do not include scholarships or financial assistance, as only three and 23 students in 2000 and 2004 were awarded this, with average values of 13 and 81 *yuan*, respectively. One may be concerned 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 a household financial basis. (c) All monetary variables are translated in real terms at 2004 prices by using the consumer price index in Gansu province. Data on price deflators come from the China Data Centre, University of Michigan.

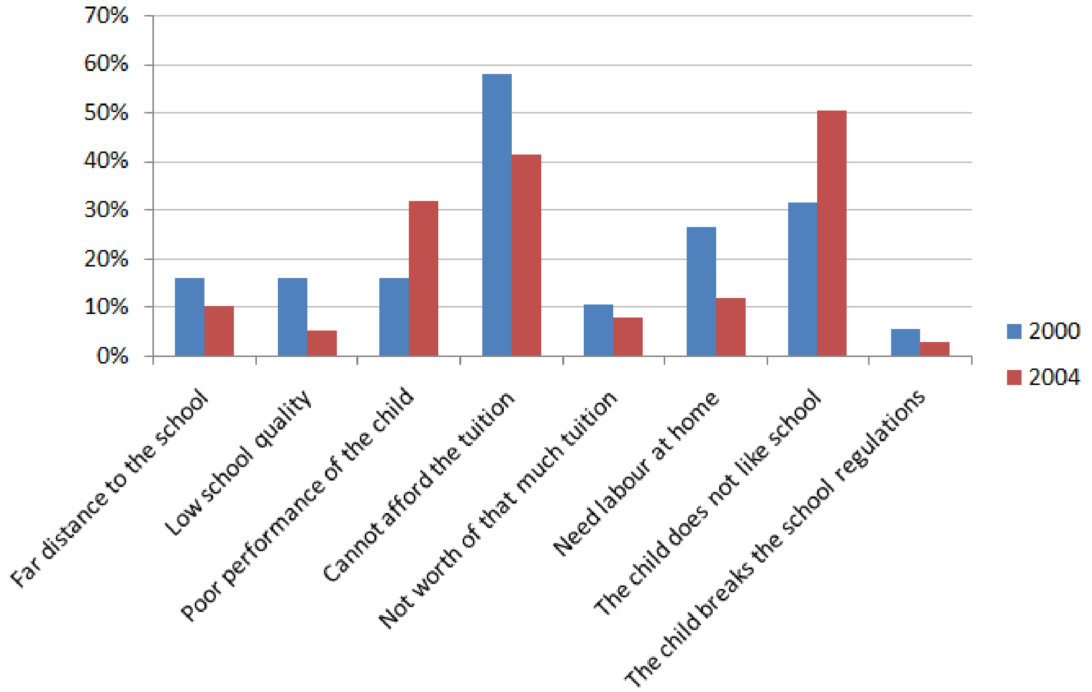
Parents, although poor and credit constrained, do care about children's education. About 97 percent of households deemed that attaining good education is a very important means to leading a happy life. However, increasing educational expenditure incurred drop-outs. As listed in Figure 2, 57.9 percent 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 percent of the drop-outs.

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

Note: (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. (d) All monetary variables are translated in real terms at 2004 prices by using the consumer price index in Gansu province. Data on price deflators come from the China Data Centre, University of Michigan.

Figure 2: Reasons for 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.

RCCs are the largest provider of formal microcredit to rural households in China (Li *et al.*, 2011a, b). They are designed to broaden households' access to credit and have branches in almost every township (Brandt *et al.*, 2003). Participation in RCCs is on a voluntary and household basis. The village client manager and the asset appraisal committee 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.[7] 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. In 2000 and 2004, respectively, 43.3 percent and 35.2 percent took loans. The real average amount of loans borrowed by households was 1,825.8 *yuan* in 2000 and 3413.7 *yuan* in 2004, registering an increase of 87 percent. However, the difficulty of borrowing was intensified over time. In 2004, 60.5 percent 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. Variable description

As our dependent variable, children's educational outcome is measured by two indicators, to 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,

$$schooling\ gap = \max\left\{0, expected\ schooling - observed\ total\ years\ of\ schooling\right\} \qquad (1)$$

where the expected schooling is defined as

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

and

$$observed\ total\ years\ of\ schooling = actual\ (levels)\ years\ of\ schooling - repeated\ grades \qquad (3)$$

The schooling gap reflects the difference between children's actual years of schooling since their first enrolment in primary schools and the desired years of 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.

The most important independent variable of interest is households' borrowing of microcredit. The models used are briefly explained in the next section. Here we introduce first four other categories of control variables that are susceptible to influencing children's educational outcomes based on previous studies (e.g., Brown and Park, 2002; Zhao and Glewwe, 2010). Table 3 presents descriptive statistics for all variables in our analysis.

[7] For detailed information on credit levels, see http://www.gsrcu.com/www/ContentsDisp.asp?id=930&ClassId=14 [in Chinese, accessed June 4, 2012].

Table 3 Descriptive statistics

Variable	2000		2004	
	Mean	S.D.	Mean	S.D.
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
Age	11.042	1.153	15.088	1.159
Gender (boy=1)	0.533	0.499	0.533	0.499
Ethnic minority (yes=1)	0.019	0.136	0.019	0.136
Health status	4.246	0.949	4.090	0.925
Birth order	1.724	0.804	1.718	0.786
Siblings' education	12.590	7.018	1.677	0.809
Child labour	2.878	6.183	8.925	10.356
Attending the nearest school (yes=1)	0.975	0.157	0.922	0.268
Capability of studying	2.954	0.747	3.311	0.937
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 future income	2.032	0.582	2.080	0.589
Women's empowerment on child education	1.886	0.474	1.887	0.502
Ln(hh wealth)	7.089	1.068	7.397	1.084
Ln(tuition fees per child)	5.203	0.611	5.923	0.801
Ln(other education cost per child)	4.491	1.023	5.330	1.264
Teacher's 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 distance to the nearest primary school (km)	0.579	0.876	3.273	3.100
Village distance to the nearest junior middle school (km)	1.051	0.333	4.310	4.235
Age at the first enrolment in the village	6.674	0.672	6.295	0.886
Proceed to secondary education in village[a]	0.892	0.201	0.113	0.162
% of RCCs borrowers in the village	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.

First, we consider the sample child's characteristics, such as age, gender, ethnic identity, health status, whether attending the nearest schools, capability of studying according to the teachers' perception and child labour in terms of the time spent in housework. In particular, we include birth order and siblings' average schooling to reflect the competition of resources for children within the household. We also explicitly control for the sample child's tuition fees and other educational costs separately, considering the educational reform of cancelling tuition fees of primary and secondary education beginning in 2006.

Second, we include parents' educational backgrounds, women's empowerment measured by mothers' power in decision-making relating to children's education, and the household characteristics like the wealth level. It is notable that here we may face difficulties in identification, as higher educational achievement is often associated with more power in families' decision-making and there might be sorting in marriage (Duflo, 2011). Including siblings' average schooling, as mentioned above, could help us address the identification problem. Meanwhile, we also add parents' attitudes towards the sample child's education by using the following two indicators: to what extent they expect the sample child to receive more education; and willingness to provide financial assistance to them in the future. These factors control explicitly for parents' underlying intention for children's education and therefore, help in identifying the impact of parents' education.

Third, the sample children's teachers and schools are taken into account, given that good educational resources are crucial for better educational outcomes (see Glewwe *et al.*, 2011 for a recent comprehensive review). More specifically, we include teachers' educational levels and, at the school level, the student-teacher ratio and the share of unsafe classrooms.

Fourth, the local geographical, cultural and economic situation could also affect child-rearing. Our selected indicators at the village level are distances to the nearest primary or junior high school, average age at the first enrolment, the share of primary students completing junior high schools, the share of households having access to RCCs and the village income per capita.[8]

With these variables on hand, we proceed to set up the RDD to investigate the attributes of children's education outcomes, emphasising the role of household borrowing behaviour of microcredit.

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:

$$p_i = \Pr\left(B_i = 1 | Z_i, Z_v\right) = \Pr\left(\alpha_0 + Z_i\alpha_1 + Z_v\alpha_2 + u_i > 0\right) \qquad (4)$$

where B_i equals 1 when the household actually borrows RCCs and 0 otherwise; 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[9] and other possible determinants based on the past literature. In particular, Z_i denotes household characteristics. First, it includes household size, the

[8] It is notable that although we tried to take into account many possible attributes in order to help purge the causal impact from borrowing behaviour of other potential causes, there are underlying factors that may significantly affect the household's ability and willingness to borrow, but could not be included due to data limitation, like membership in the Communist Party and location near a useful road.

[9] For detailed regulations in Gansu province, see http://www.gsrcu.com/www/ContentsDisp.asp?id=1015&ClassId=48 [in Chinese, accessed June 4, 2012].

number of family members currently not living in the household, the number of employed members, average educational level and age of all household members, household perception of its total income in the previous year (i.e., whether its income was enough to support daily life), the quality of housing, the ratio of irrigated land over the total farmland owned by the household and the wealth status of the household among the village. These are considered by the manager to estimate the client's risk of default and the ability of repayment. Second, Z_i variables are (1) whether borrowing RCCs for educational purposes and (2) availability of informal loans for the households, such as from friends, neighbours and relatives. These are selected out of consideration of household consumption demand for credit and of the fact that informal lending has long been prevailing in rural China. A recent study by Turvey and Kong (2010) finds that about two-thirds of rural households borrow from friends or relatives, given strong trust in Chinese rural communities and informal lending tends to crowd out the borrowing of RCCs. Z_v is a set of village dummies in order to control for common time trend and other unobserved heterogeneity.

The predicted probability of borrowing $\hat{p}_i$ serves as the 'assignment variable'. Given that $\hat{p}_i$ is derived by considering various factors affecting the credit level/limit assigned for the household by the manager and the household's real demand for credit, it can also be understood as an index of the household ability/willingness to borrow. A household is considered to borrow RCCs, that is, receiving the treatment, if its ability/willingness to borrow is higher than 50 percent. Therefore, the probability of being treated can be written by a function that is discontinuous at 0.5.[10]

$$b_i = \begin{cases} 1 \;\; if \;\; \hat{p}_i \geq 0.5 \\ 0 \;\; if \;\; \hat{p}_i < 0.5 \end{cases} \qquad (5)$$

In the context of more than one cross-sectional data, 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 from 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.

[10] We cannot bypass the arbitrariness in defining the assignment threshold. The sensitivity of our estimation to the choice of cut-off will be discussed in Footnote 16 in Section 4.2.

In the presence of self-selection of borrowing RCCs, there are the unobservables relating to both 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 a 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 (a local randomised experiment in Lee and Lemieux, 2010). Therefore, the 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), i.e., the local average treatment effect (LATE).

Based on Eq. (5), the actually observed educational outcome for the sample child *i* at time *t* can be expressed by $y_{it} = b_{it} y_{it}(1) + (1 - b_{it}) y_{it}(0)$, where $y_{it}(1)$ and $y_{it}(0)$ are the outcomes with and without borrowing RCCs, respectively. The outcome y_{it} is written by:

$$y_{it} = \beta_0 + \theta b_{it} + \varepsilon_{it} \tag{6}$$

where $y_{it}(0) = \beta_0 + \varepsilon_{it}$ and $\theta = y_{it}(1) - y_{it}(0)$. This leads to expression of the idea of comparing the treatment and control groups at the cut-off as:

$$\begin{aligned} E(\theta \mid \hat{p}_{it} = 0.5) &= \lim_{\hat{p}\downarrow c} E(y_{it} \mid \hat{p}_{it}) - \lim_{\hat{p}\uparrow c} E(y_{it} \mid \hat{p}_{it}) \\ &= \theta\left[\lim_{\hat{p}\downarrow 0.5} E(b_{it} \mid \hat{p}_{it}) - \lim_{\hat{p}\uparrow 0.5} E(b_{it} \mid \hat{p}_{it})\right] + \left[\lim_{\hat{p}\downarrow 0.5} E(\varepsilon_{it} \mid \hat{p}_{it}) - \lim_{\hat{p}\uparrow 0.5} E(\varepsilon_{it} \mid \hat{p}_{it})\right] \end{aligned} \tag{7}$$

In the quasi-experimental environment around the threshold, households' imprecise control over their ability/willingness to borrow means that $E(\theta \mid \hat{p}_{it})$ and $E(\varepsilon_{it} \mid \hat{p}_{it})$ are continuous at the cut-off (Hahn *et al.*, 2001).[11] It follows Eq. (7) that the LATE is formulated as:

$$\begin{aligned} E[\theta_{it} \mid \hat{p}_{it} = 0.5] &= \lim_{e\downarrow 0} E\left[\theta_{it} \mid b_{it}(0.5+e) - b_{it}(0.5-e) = 1, \hat{p}_{it} = 0.5\right] \\ &= \frac{\lim_{\hat{p}\downarrow 0.5} E[y_{it} \mid \hat{p}_{it}] - \lim_{\hat{p}\uparrow 0.5} E[y_{it} \mid \hat{p}_{it}]}{\lim_{\hat{p}\downarrow 0.5} E[b_{it} \mid \hat{p}_{it}] - \lim_{\hat{p}\uparrow 0.5} E[b_{it} \mid \hat{p}_{it}]} \end{aligned} \tag{8}$$

Empirically, we adopt three different methods to estimate Eq. (8) 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 Eq. (8) are calculated as $\lim_{\hat{p}\downarrow 0.5} E[y_{it} \mid \hat{p}_{it}] = \frac{\sum_{i\in\Psi_t} y_{it} w_{it}}{\sum_{i\in\Psi_t} w_{it}}$,

[11] This assumption will be tested in Section 4.1.

$\lim_{\hat{p}\uparrow 0.5} E[y_{it} \mid \hat{p}_{it}] = \frac{\sum_{i\in\Psi_t} y_{it}(1-w_{it})}{\sum_{i\in\Psi_t}(1-w_{it})}$, $\lim_{\hat{p}\downarrow 0.5} E[d_{it} \mid \hat{p}_{it}] = \frac{\sum_{i\in\Psi_t} b_{it} w_{it}}{\sum_{i\in\Psi_t} w_{it}}$ and $\lim_{\hat{p}\uparrow 0.5} E[b_{it} \mid \hat{p}_{it}] = \frac{\sum_{i\in\Psi_t} b_{it}(1-w_{it})}{\sum_{i\in\Psi_t}(1-w_{it})}$ where the indicator variable $w_{it} = I\{0.5 \le \hat{p}_{it} < 0.5 + h_t\}$ 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*; $\Psi_t = \{i \mid i \in (0.5 - h_t \le \hat{p}_{it} < 0.5 + h_t)\}$ is the sub-sample containing those residing in the vicinity of the cut-off.[12]

Second, in the semi-parametric case, we employ Van der Klaauw's (2008) two-step estimation which offers a useful complement to the LATE by making use of the information of full sample. Specifically, the first step estimates the probability of treatment receipt in a standard probit specification,

$$E[b_{it} \mid \hat{p}_{it}] = \Pr(b_{it} = 1 \mid \hat{p}_{it}) = \gamma \cdot \mathbf{1}(\hat{p}_{it} \ge 0.5) + g(\hat{p}_{it}) \tag{9}$$

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

$$g(\hat{p}_{it}) = \lambda_0 + \lambda_1 \hat{p}_{it} + \lambda_2 \hat{p}_{it}^2 + \left\{\lambda_3(\hat{p}_{it} - 0.5) + \lambda_3(\hat{p}_{it} - 0.5)^2\right\} \cdot \mathbf{1}(\hat{p}_{it} \ge 0.5) \tag{10}$$

In the second step, using Eq. (9) in the outcome regression yields a reduced-form control-function augmented outcome equation:

$$y_{it} = \beta_0 + \theta E[b_{it} \mid \hat{p}_{it}] + X_{it}\beta_1 + X_{ht}\beta_2 + X_{st}\beta_3 + X_{vt}\beta_4 + \pi_t + k(\hat{p}_{it}) + \upsilon_{it} \tag{11}$$

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 $E(\varepsilon_{it} \mid \hat{p}_{it})$ 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 ensure 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

[12] We also estimated the left- and right-hand sides of the limits relative to the cut-off in Eq. (8) by using the triangle kernel putting more weights to observations closer to the cut-off and having proved better properties at boundaries (Ludwig and Miller, 2010). The results are broadly similar to the local Wald estimators in Tables 4-5.

generalised cross-validation of data. $\hat{\theta}$ reflects the average treatment effect defined in Eq. (8) (Van der Klaauw, 2008).

Third, as a variant to Van der Klaauw (2008), we use a standard instrumental variable (IV) approach to estimate Eq. (11). 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) and the independent variables except $E\left[b_{it} \mid \hat{p}_{it}\right]$ in Eq. (11) serving as the included instruments.

It is worth noting 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.[13] Moreover, given the imperfect compliance for those whose ability is higher than 0.5, $\hat{\theta}$ should be treated as a lower bound of the true causal impact of RCCs.

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 their 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 Eq. (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

[13] If estimating the pooled sample instead, $\hat{\theta}$ actually measures an average treatment effect of RCCs in different years within the sample period.

subsequent years, $\left\{b_{i,t-\tau+1},\ldots,b_{it}\right\}$. Adopting Cellini *et al.*'s (2010) dynamic RD strategy, we estimate the ITT by the following outcome regression:

$$y_{it\tau}=\beta_0+\theta_\tau^{ITT}b_{it}+X_{it}\beta_1+X_{ht}\beta_2+X_{st}\beta_3+X_{vt}\beta_4+\pi_t+\mu_\tau+k\left(\hat{p}_{it}\right)+\varepsilon_{it\tau} \tag{12}$$

where μ_τ represents fixed effects for years relative to the borrowing; other variables are defined as before. As shown by the subscripts in Eq. (12), 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 Eq. (12). Taking Cellini *et al.*'s (2010) suggestion, we cluster standard errors by child to account for possible dependence across observations $\left(i,t\right)$ and serial correlation in $\varepsilon_{it\tau}$ caused by the multi-use of observations $\left(i,t\right)$.

As 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*:

$$\begin{aligned}\theta_\tau^{ITT}&\equiv\frac{\mathrm{d}y_{it}}{\mathrm{d}b_{i,t-\tau}}\equiv\frac{\partial y_{it}}{\partial b_{i,t-\tau}}+\left(\frac{\partial y_{it}}{\partial b_{i,t-\tau+1}}\cdot\frac{\mathrm{d}b_{i,t-\tau+1}}{\mathrm{d}b_{i,t-\tau}}+\frac{\partial y_{it}}{\partial b_{i,t-\tau+2}}\cdot\frac{\mathrm{d}b_{i,t-\tau+2}}{\mathrm{d}b_{i,t-\tau}}+\cdots+\frac{\partial y_{it}}{\partial b_{it}}\cdot\frac{\mathrm{d}b_{it}}{\mathrm{d}b_{i,t-\tau}}\right)\\&=\frac{\partial y_{it}}{\partial b_{i,t-\tau}}+\sum_{h=1}^{\tau}\left(\frac{\partial y_{it}}{\partial b_{i,t-\tau+h}}\cdot\frac{\mathrm{d}b_{i,t-\tau+h}}{\mathrm{d}b_{i,t-\tau}}\right)\\&=\theta_\tau^{TOT}+\sum_{h=1}^{\tau}\theta_{\tau-h}^{TOT}\omega_h\end{aligned} \tag{13}$$

where $\theta_0^{ITT}=\theta_0^{TOT}$ and $\omega_h=\dfrac{\mathrm{d}b_{i,t-\tau+h}}{\mathrm{d}b_{i,t-\tau}}$ 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 Eq. (12) with the dependent variable being replaced by $b_{it\tau}$. Reverting Eq. (13) yields our recursive estimates of θ_τ^{TOT}:

$$\theta_\tau^{TOT}=\theta_\tau^{ITT}-\sum_{h=1}^{\tau}\theta_{\tau-h}^{TOT}\omega_h \tag{14}$$

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

4. Estimation results and discussion

We begin by discussing the internal validity of our RDD set-up.[14] We test for the assumptions ensuring successful identification of the treatment effect of borrowing RCCs. The results of the static and dynamic RDD models in Section 3 are then presented in turn.

4.1. Identification problems in RDD

The RDD is regarded as having higher internal validity than other 'quasi-experimental' methods, which, however, needs to be justified as the estimated treatment effect hinges on some important setting of the dataset (Imbens and Lemieux, 2008).

First, to validate the quasi-experimental environment and therefore the causal inferences around the cut-off, the treatment should be randomly assigned for the sub-sample near the cut-off of the assignment variable that we have observed. Households may influence the assignment variable, i.e., their ability/willingness to borrow, through their characteristics and action (e.g., the explanatory variables in Eq. 4), while also experiencing a random unobserved component affecting their chance of having a particular level of ability. This latter makes RDD similar to a randomised experiment in a neighbourhood around the cut-off. Thus, the treatment status should by construct be independent of the pre-determined (baseline) covariates (Lee and Lemieux, 2010), which means that the differences in educational outcomes between borrowers and non-borrowers are not confounded by either observed or unobserved omitted variables. We test for this by re-calculating the local Wald estimator of LATE by adding other covariates in Eq. (6), including the characteristics of the sample children and their families, schools and villages. The results are broadly same as columns 1 and 5 of Tables 4-5,[15] implying that borrowing behaviour is the only source of differential educational outcomes.

Second, the above assumption of randomised experiments at the cut-off also requires that the average educational outcomes for children whose families' abilities to borrow fall barely below the cut-off should ideally form a valid counterfactual to be compared with those in the treated group. It is therefore necessary to investigate whether households can fully 'manipulate' the assignment variable, so that they self-select into groups of borrowers or non-borrowers. If so, borrowers would be systematically different from non-borrowers. We implement the density test formulated by McCrary (2008). The conditional density of household ability index on two potential types of households distinguished by the cut-off is expected to be continuous without manipulation. Therefore, a household experiences equal chance of falling above or below the cut-off, irrespective of the type that it belongs to. As seen in Figure 3, although the estimated conditional density seems to give some indication of discontinuity around the cut-off, the 'jump' of the conditional density function at the cut-off is statistically insignificant in both years. The null hypothesis of zero discontinuity in the

[14] The external validity of RDD in the non-parametric case is less than other 'quasi-experimental' methods, since the LATE draws upon sub-samples close to the cut-off only. However, this may not be a serious problem, as we also employed Van der Klaauw's (2008) semi-parametric estimation and standard IV estimation, taking advantage of full sample and cross-compared our estimation results in Section 4.2.

[15] Results are not shown in the paper, given limited space, but are available upon request from the authors.

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

Independent variable	2000 Local Wald (1)	Two-step CF (2)	Two-step CF (3)	Standard IV (4)	2004 Local Wald (5)	Two-step CF (6)	Two-step CF (7)	Standard IV (8)
$\lim_{\hat{p}\downarrow 0.5} E[y_{it} \mid \hat{p}_{it}] - \lim_{\hat{p}\uparrow 0.5} E[y_{it} \mid \hat{p}_{it}]$	-0.317*				-0.336			
	(0.187)				(0.280)			
$\lim_{\hat{p}\downarrow 0.5} E[b_{it} \mid \hat{p}_{it}] - \lim_{\hat{p}\uparrow 0.5} E[b_{it} \mid \hat{p}_{it}]$	0.110				0.102			
	(0.091)				(0.072)			
$E[\theta_{it} \mid \hat{p}_{it} = 0.5]$	-2.884				-3.276			
	(3.134)				(3.563)			
$\hat{\theta}$		-2.853**	-2.557**	-5.430		-1.260	-0.094	-0.039
		(1.259)	(1.194)	(7.872)		(2.205)	(1.579)	(1.758)
Child characteristics								
Age		0.447***	0.481***	0.526***		0.551***	0.405***	0.402***
		(0.033)	(0.038)	(0.153)		(0.067)	(0.122)	(0.124)
Gender		-0.046	-0.006	0.050		-0.112	0.513***	0.514***
		(0.062)	(0.080)	(0.271)		(0.121)	(0.191)	(0.191)
Health status		-0.127***	-0.135***	-0.161		-0.046	0.071	0.073
		(0.042)	(0.045)	(0.144)		(0.092)	(0.125)	(0.130)
Ethnic minority		-0.218	0.676	0.535		0.289	-0.518	-0.507
		(0.413)	(0.489)	(1.231)		(1.161)	(0.784)	(0.790)
Birth order		0.308***	0.437***	0.606		0.036	0.124	0.125
		(0.080)	(0.091)	(0.484)		(0.134)	(0.205)	(0.204)
Child labour		-0.011*	-0.015**	-0.019		0.007	0.003	0.003
		(0.006)	(0.006)	(0.015)		(0.015)	(0.007)	(0.007)
Capability of studying		-0.068	-0.128*	-0.231		-0.113*	-0.201	-0.200
		(0.052)	(0.067)	(0.314)		(0.065)	(0.126)	(0.126)
Siblings' education		-0.030***	-0.057***	-0.065*		-0.152	0.094	0.092
		(0.007)	(0.012)	(0.038)		(0.132)	(0.275)	(0.273)
Attending the nearest school		-0.607*	-0.489*	-0.342		-0.606	-0.898**	-0.898**
		(0.323)	(0.300)	(0.903)		(0.444)	(0.389)	(0.389)
Parents' characteristics								
Father's education		-0.016	-0.036**	-0.053		-0.052**	-0.049	-0.049
		(0.011)	(0.015)	(0.058)		(0.023)	(0.046)	(0.047)
Mother's education		-0.024***	0.001	0.007		0.013	-0.011	-0.011
		(0.008)	(0.008)	(0.027)		(0.020)	(0.042)	(0.042)
Parents' attitude: child education		-0.169***	-0.155**	-0.157		-0.203	-0.259	-0.256
		(0.052)	(0.061)	(0.188)		(0.148)	(0.273)	(0.274)

Parents' attitude: child's income	-0.019	0.018	0.040	0.075	0.099	0.101
	(0.054)	(0.060)	(0.187)	(0.086)	(0.173)	(0.174)
Women's empowerment on child education	-0.128**	0.008	0.055	-0.012	0.139	0.133
	(0.061)	(0.053)	(0.193)	(0.083)	(0.226)	(0.236)
Household characteristics						
Ln(hh wealth per capita)	0.127*	0.131*	0.250	0.101	0.147	0.146
	(0.070)	(0.070)	(0.361)	(0.107)	(0.131)	(0.132)
Ln(sample child's tuition)	-0.240**	-0.115	-0.112	-0.904***	-0.021	-0.018
	(0.106)	(0.102)	(0.227)	(0.260)	(0.300)	(0.302)
Ln(sample child's other edu. costs)	-0.122**	-0.086*	-0.143	-0.065	-0.225	-0.229
	(0.053)	(0.048)	(0.199)	(0.113)	(0.163)	(0.168)
Teacher and school characteristics						
Teachers' average edu.		-0.243**	-0.252		-1.137***	-1.137***
		(0.060)	(0.178)		(0.159)	(0.159)
Student-teacher ratio		-0.020***	-0.032		0.0003	0.0002
		(0.008)	(0.036)		(0.006)	(0.006)
% unsafe classrooms		0.159	-0.023		1.055**	1.048**
		(0.229)	(0.793)		(0.410)	(0.415)
Village characteristics						
Distance to the nearest primary school		-0.037	-0.051		0.215	0.213
		(0.055)	(0.139)		(0.140)	(0.149)
Distance to the nearest junior middle school		-0.118	-0.164		0.047	0.048
		(0.102)	(0.370)		(0.050)	(0.052)
Age at the first enrolment		0.164**	0.179		-0.189	-0.186
		(0.069)	(0.195)		(0.228)	(0.233)
Proceed to secondary education		0.278	0.269		-2.672**	-2.688**
		(0.380)	(1.074)		(1.046)	(1.063)
% of RCCs borrowers		-0.625***	-0.728		-0.889	-0.916
		(0.238)	(0.780)		(1.555)	(1.602)
Ln (village per capita income)		-0.021	-0.009		-0.077	-0.076
		(0.024)	(0.068)		(0.054)	(0.056)
School dummies	Yes			Yes		
Village dummies	Yes			Yes		
County dummies		Yes	Yes		Yes	Yes
R^2	0.488	0.453	0.338	0.792	0.766	0.765

Note: ***, ** and * denote 1%, 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).

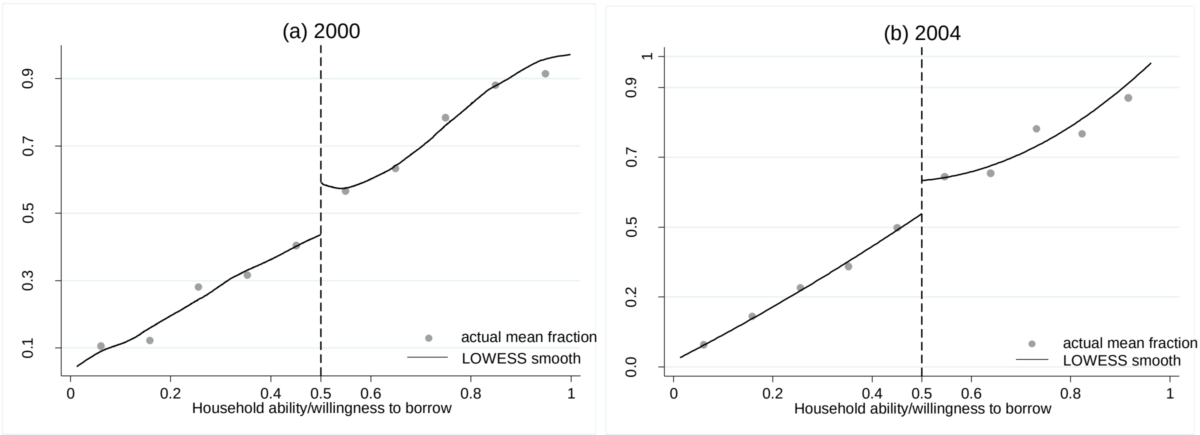
Table 5: FRD estimates of impacts of borrowing RCCs on the average score

Independent variable	2000 Local Wald (1)	Two-step CF (2)	Two-step CF (3)	Standard IV (4)	2004 Local Wald (5)	Two-step CF (6)	Two-step CF (7)	Standard IV (8)
$\lim_{\hat{p}\downarrow 0.5} E[y_{it} \mid \hat{p}_{it}] - \lim_{\hat{p}\uparrow 0.5} E[y_{it} \mid \hat{p}_{it}]$	2.412				0.167			
	(1.976)				(2.012)			
$\lim_{\hat{p}\downarrow 0.5} E[b_{it} \mid \hat{p}_{it}] - \lim_{\hat{p}\uparrow 0.5} E[b_{it} \mid \hat{p}_{it}]$	0.074				0.052			
	(0.077)				(0.076)			
$E[\theta_{it} \mid \hat{p}_{it} = 0.5]$	32.775				3.186			
	(45.926)				(38.772)			
$\hat{\theta}$		19.982	12.465	8.574		11.845	-11.561	-11.740
		(14.358)	(14.054)	(29.275)		(22.190)	(11.811)	(12.203)
Child characteristics								
Age		-0.727**	-1.004**	-0.944		0.365	-0.295	0.012
		(0.333)	(0.426)	(0.601)		(0.639)	(0.917)	(1.159)
Gender		-0.149	-0.453	-0.414		-2.099	-3.458**	-3.701**
		(0.693)	(0.919)	(1.265)		(1.409)	(1.652)	(1.754)
Health status		-0.442	-0.555	-0.569		0.129	-0.113	-0.322
		(0.414)	(0.473)	(0.573)		(0.780)	(0.904)	(1.193)
Ethnic minority		-11.838*	-13.063	-13.141		10.486	1.373	1.200
		(6.347)	(10.173)	(10.601)		(11.230)	(4.092)	(4.838)
Birth order		-0.831	0.084	0.297		-0.386	-3.755*	-4.105*
		(0.832)	(0.964)	(1.805)		(1.097)	(2.051)	(2.365)
Child labour		0.002	0.067	0.062		-0.202	-0.080	-0.085
		(0.051)	(0.055)	(0.064)		(0.155)	(0.071)	(0.066)
Capability of studying		10.280***	10.407***	10.266***		6.313***	5.949***	6.251***
		(0.586)	(0.842)	(1.165)		(0.745)	(0.845)	(0.973)
Siblings' education		0.044	-0.216	-0.227		1.628	2.495	2.638
		(0.066)	(0.154)	(0.170)		(1.425)	(2.388)	(3.116)
Attending the nearest school		-1.894	-2.011	-1.845		2.815	0.201	0.530
		(3.149)	(3.632)	(4.253)		(4.251)	(3.472)	(4.081)
Parents' characteristics								
Father's education		0.144	0.137	0.113		-0.064	-0.543**	-0.585*
		(0.115)	(0.164)	(0.205)		(0.281)	(0.268)	(0.326)
Mother's education		0.045	-0.015	-0.005		-0.102	0.042	-0.029
		(0.094)	(0.088)	(0.098)		(0.217)	(0.304)	(0.389)
Parents' attitude: child's education		0.898	1.417*	1.488*		2.204	-0.131	0.066
		(0.573)	(0.737)	(0.795)		(1.395)	(1.398)	(1.474)

Parents' attitude: child's income	0.373	0.158	0.199	-0.390	-0.386	-0.033
	(0.562)	(0.635)	(0.657)	(0.940)	(1.455)	(1.768)
Women's empowerment on child's	-0.139	-1.510***	-1.431**	0.050	1.777	1.710
education	(0.756)	(0.586)	(0.696)	(0.708)	(1.533)	(1.542)
Household characteristics						
Ln(hh wealth per capita)	-0.476	-0.535	-0.358	0.270	0.260	0.321
	(0.762)	(0.753)	(1.321)	(0.976)	(0.837)	(0.992)
Ln(sample child's tuition)	2.746*	3.483**	3.494**	1.845	0.617	0.855
	(1.600)	(1.585)	(1.589)	(2.059)	(1.771)	(2.085)
Ln(sample child's other edu. costs)	-1.243**	-0.832	-0.920	-0.963	-0.813	-0.966
	(0.513)	(0.652)	(0.809)	(1.202)	(1.191)	(1.229)
Teacher and school characteristics						
Teachers' average education		-0.163	-0.156		1.114	1.070
		(0.745)	(0.807)		(1.020)	(1.083)
Student-teacher ratio		-0.183**	-0.198		-0.029	-0.029
		(0.092)	(0.127)		(0.064)	(0.067)
% unsafe classrooms		-1.889	-2.058		4.056	4.295
		(2.520)	(3.322)		(3.137)	(3.353)
Village characteristics						
Distance to the nearest primary		0.860*	0.877*		1.467	1.837
school		(0.461)	(0.531)		(0.980)	(1.289)
Distance to the nearest junior		0.117	0.120		-0.173	-0.232
middle school		(1.198)	(1.359)		(0.302)	(0.395)
Age at the first enrolment		0.404	0.441		2.302	1.986
		(0.836)	(0.915)		(1.583)	(2.029)
Proceed to secondary education		-6.253	-6.702		-0.790	-0.620
		(4.598)	(4.950)		(8.232)	(8.970)
% of RCCs borrowers		-0.334	-0.734		-8.839	-7.402
		(2.657)	(2.758)		(10.808)	(12.696)
Ln(village per capita income)		-0.270	-0.252		0.146	-0.198
		(0.229)	(0.274)		(0.501)	(0.814)
School dummies	Yes			Yes		
Village dummies	Yes			Yes		
County dummies		Yes	Yes		Yes	Yes
R^2	0.505	0.476	0.392	0.611	0.594	0.467

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

Figure 3: Relationship between actual and the predicted probability of borrowing

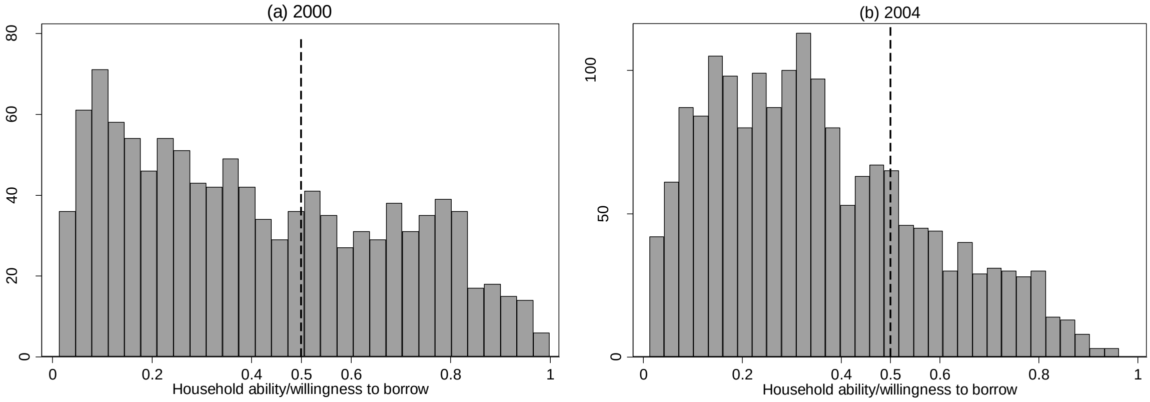


Note: Children are grouped into five bins left and right of the cut-off, respectively. The dot is the cell mean of the indicator for whether the household actually borrows RCCs, which reflects the actual probability of borrowing. The solid line represents predicted probability of borrowing from LOWESS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

estimated density cannot be rejected at all three conventionally statistical levels. This may raise the conjecture that even though households could partially manipulate 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.

The third 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 Eq. (8). Figure 4 clearly illustrates that the higher the ability/willingness, the more likely the household is going to borrow. There is an evident increase of about 10 percent in the actual fraction of households borrowing RCCs when their ability/willingness to borrow exceeds 0.5. 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. Imperfect compliance around the cut-off just conveys the intuition of our use of a fuzzy RDD.

Figure 4: Distribution of household ability/willingness to borrow microcredit



Fourth, other baseline covariates should not suggest discontinuity at the assignment threshold; otherwise the estimated treatment effect will be clouded by other attributes on child education in addition to RCCs. As Figure 3, we inspect possible discontinuity by drawing the local bin averages of each covariate of interests in Eq. (11) against the household ability/willingness index. There is no indication of discontinuity at the threshold.[16] This also echoes our test for the first assumption above, i.e., adding baseline covariates does not significantly alter the estimated treatment effect.

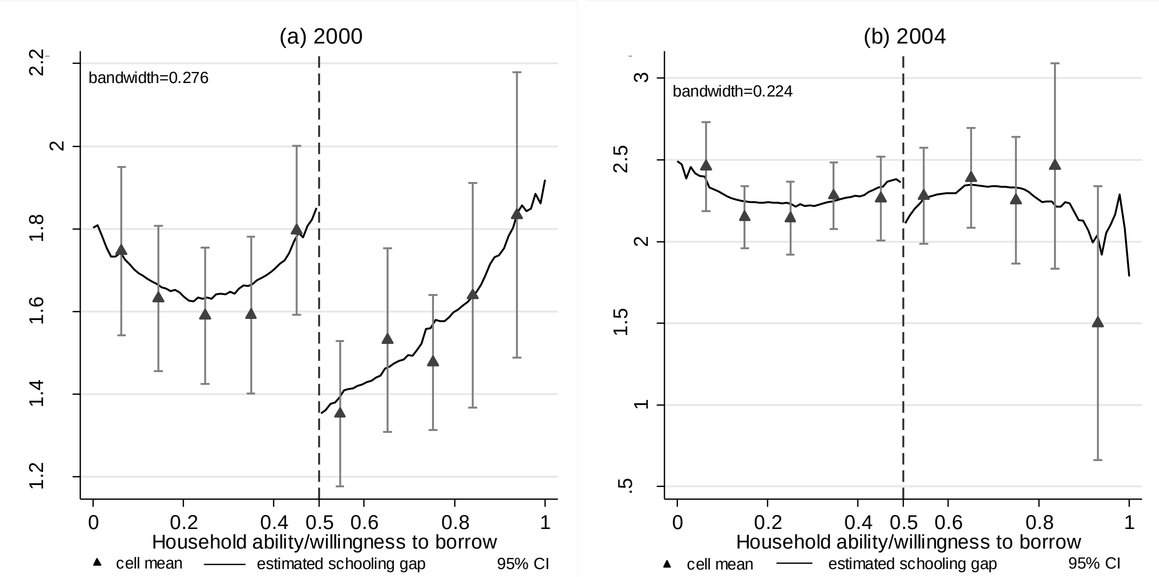
In general, it seems warranted to conclude that our model set-up based on a fuzzy RDD is appropriate in the context of formal microcredit markets in rural Gansu to identify a possibly causal relationship between borrowing RCCs and educational outcomes for borrowers' children.

4.2. Contemporaneous effects of RCCs

We first look at how households' borrowing decisions affect children's education outcomes, without considering the partial compliance amongst the eligible households, i.e., the estimates of the numerator of Eq. (8). Figure 5(a) clearly shows a decrease in children's educational gap in 2000, although borrowing behaviour re-allocated labour within the family by pulling children from studying to housework to compensate other family members' reduced labour input for it, but increased time spent in production activities.[17] Borrowers' children received 0.32 more years of education compared to those born in non-borrowing families (column 1 of Table 4). In view of 11 percent higher probability of receiving the treatment among the eligible households compared with the non-eligible (i.e., the estimated denominator of Eq. (8) listed in column 1 of Table 4), borrowing RCCs could narrow the schooling gap by 2.88 years in 2000 and this finding is robust in various bandwidths (Figure A1(a) in Appendix).[18] We further obtain statistical significance with smaller magnitude of the

[16] Graphs are not shown here, given limited space, but are available from the authors upon request.
[17] The child spent 57.8 percent more time on housework in borrowing households than in non-borrowing ones, and total time spent on production activities by other family members increased by 6.6 percent.
[18] In addition to bandwidth selection, the value of cut-off might also affect our results. We experimented with higher thresholds between 0.5 and 0.7. As expected, higher assignment threshold induced less significant behavioural changes: we observed a smaller 'jump' in the probability of treatment receipt (denominator in Eq. 8), as well as smaller impact on outcomes variables (numerator

Figure 5: Schooling gap as a function of household ability/willingness index



Note: Households are grouped into five bins left and right of 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.

treatment effects of 2.56-2.85 years under the semi-parametric specification (columns 2-3 of Table 4). We can see similar positive effects on schooling in 2004 (as illustrated in Figure 5(b)), while with larger magnitude of 0.34 (column 5 of Table 4). After considering the partial compliance of 10 percent of eligible households, this leads to 3.28 years smaller schooling gap, which is also robust across different bandwidths (Figure A1(b)). It appears that in the circumstances of soaring educational costs, RCCs could assist households more in limiting the child schooling gap. Nevertheless, all estimates in 2004 become statistically insignificant, whichever the model specification is. Since unaffordable fees as a reason for drop-out fell by 16.6 percent between 2000 and 2004 (see Section 2), one can surmise that financial concerns were no longer the biggest obstacle to education in 2004 compared to 2000. Although educational costs rose quickly, many parents would still like to support their children's schooling, and 83.8 percent of parents expected their children to go to university in the future. However, as shown by Figure 2 in Section 2, in 2004, children's dislike of their schools took over from unaffordable costs and emerged as the most frequently reported reason for not attending schools (50.5 percent).

Regarding academic performance, Figure 6(a) reveals an increase in average scores, with the magnitude of 2.41 points (column 1 of Table 5) in case of perfect compliance. The partial compliance leads to 8.57-32.78 points in different model specifications (columns 1-4 of Table 5), however, the estimates are statistically insignificant and highly sensitive to the selection of bandwidth (Figure A2(a)). Evidence in 2004 is mixed. Figure 6(b) suggests

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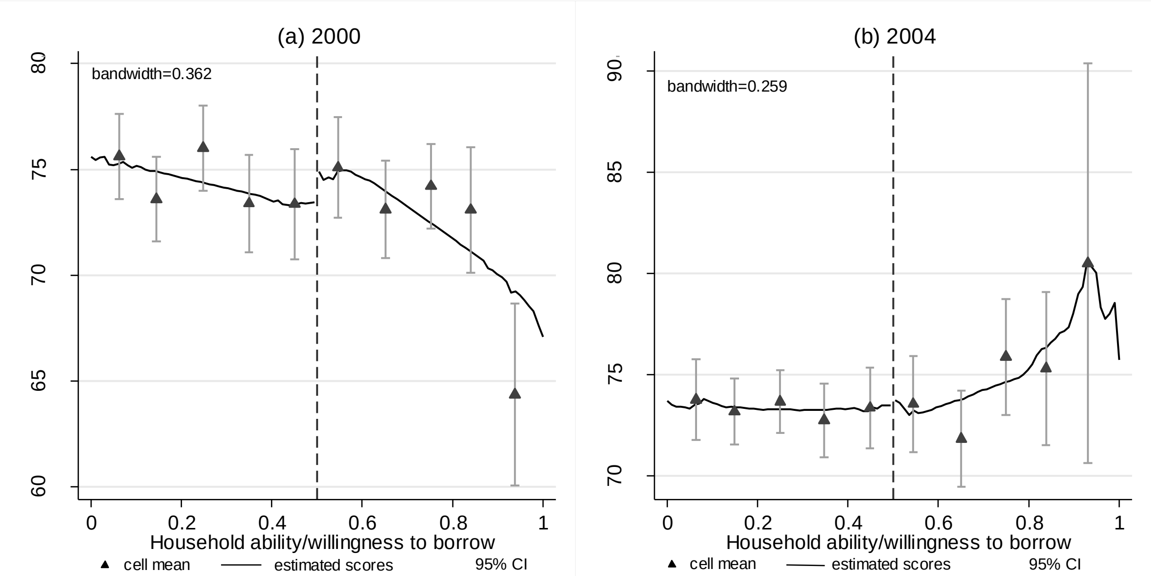
in Eq. 8). As a result, the estimated impact of RCCs on schooling gap and scores reduced substantially under higher values of threshold. For example, if using 0.7 as the cut-off, the estimate of 2.88 years dropped to 0.1.

Figure 6: Scores as a function of household ability/willingness index



Note: See Figure 5.

that there was no difference in scores between borrowers and non-borrowers if all eligible households decided to be debtors to RCCs. This is also confirmed by the estimated magnitude of the changes in average scores, which is 0.17 in column 1 of Table 5. Considering imperfect compliance, i.e., the probability of borrowing among the eligible households was 5.2 percent higher than that of the non-eligible in 2004, the increase in children's average scores varies between 3.19 to 11.85 (columns 5-6 of Table 5) and in some specifications children's scores even dropped among borrowers (columns 7-8 of Table 5). When checking the sensitivity of our estimates to the bandwidth, we also find both positive and negative impacts (Figure A2(b)).

It is worth noting 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 slope right to the cut-off in Figures 5(a) and 6(a) suggests 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 slope 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 for our conjecture can be found from the structure of the usage of the loans. Out of 1,910 households, 528 (27.3 percent) 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, leading to only one-fifth of their total loans at hand for education as opposed to the rest – four fifths – for other activities.[19] In 2004, although the share of borrowers for educational purposes shrank to 22.6

[19] Unfortunately, the data limitations do not allow us to identify for which activity the loan has been used, except education.

percent, 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, as shown in Tables 1-2.[20] The average share of educational borrowing in their total loans rose up to 41.9 percent.

The above analysis suggests that formal microcredit is not automatically a magic bullet to tackle the problems of child education, but needs to be monitored and guided. 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.

Of other explanatory variables, older age and illness of the sample child would make children more likely to drop out. Within the 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 of Table 4) and performed worse in 2004 (columns 7-8 of Table 5) than their elder siblings. Attending the nearest schools could help reduce the schooling gap and this effect could be stronger in 2004 (columns 7-8 of Table 4) than in 2000 (columns 2-3 of Table 4). As in 2004 most 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. Fathers' education was positively correlated with child schooling (columns 3 and 6 of Table 4), as found by Yi *et al.* (2012), and this still holds, even 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; however, this did not necessarily improve children's academic performance (columns 7-8 of Table 5). As predicted, and as the study of Zhao and Glewwe (2010) shows, parents' higher expectation on children's educational attainment could keep children staying longer in schools and encourage them to achieve higher scores. Nevertheless, these effects became statistically insignificant in 2004.

We find that women's empowerment in terms of the position in decision-making on children's education only reduced marginally the schooling gap in 2000[21] (column 2 of Table 4) and was even negatively correlated with children's academic achievements (columns 3-4 of Table 5).[22] By re-estimating columns 2-4 of Table 5 with the interaction between mothers'

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[20] The monetary values in this and previous sentences 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.

[21] One might be concerned with the identification of the impact of women's empowerment on children's educational outcomes, given positive correlation between access to microcredit and women's power within the family found in many developing countries (Duflo, 2011). However, this might not cause serious bias in interpretation in our context, for two reasons. First, formal credits in rural China are in general not gender-targeted, but aimed at expanding credit access to rural households. Second, Li *et al.* (2011c) find that there exists a loan threshold of 30,000 *yuan* (equivalent to 24,324 *yuan* in 2004 prices) beyond which women's power in decision-making begins to rise. In our panel, only 29 and 61 households (1.5 percent and 3.2 percent) in 2000 and 2004 respectively had more formal loans than this threshold.

[22] We also re-estimated columns 2-4 and 6-8 by adding two other indicators of women's empowerment, the power in managing family finances and planting crops, while we did not find statistical significance for them.

education and empowerment as an additional regressor, this latter may be a result of mothers' low educational levels, as indicated by the positive estimated coefficient (0.101): being empowered to the same extent, less-educated women were less capable of helping their children's studies compared with those who were more educated. Moreover, as summarised by Duflo (2011), women's stronger position in decision-making within the family is often related to improvement in child health and the family's' nutrients, health and housing, while at the expense of child education. She points out that the welfare consequences of empowering women depends on which objective is more important for women should they be given more power. The negative effect of women's empowerment on children's academic performance may be explained by the phenomenon that mothers with full control over children's education are more inclined to ask children to earn wage income than those having zero influence on educational decision-making.[23]

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 excpected, better educational quality proxied by teachers' higher educational levels could narrow the children's schooling gap in both 2000 and 2004, 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 five to 10 points.

Among various common factors shared by the villagers, later first enrolment could aggravate children's schooling gap, while pro-education culture represented by a higher share of graduates from primary schools continuing to enter junior high schools substantially reduced the schooling gap in 2004. The distance between the village and schools did not significantly affect child schooling, but living in villages located further from primary schools slightly increased children's average scores in 2000 (columns 3-4 of Table 5). This might be because the 'key schools' having more educational resources and better quality become increasingly scattered in rural areas, as the government began to merge and reduce the quantity of village schools in 2000, in the hope of enhancing each individual school's quality. Attending such 'key schools' would possibly mean higher academic achievement at the expense of longer distance.

4.3. Dynamic effects of RCCs

Households' ability/willingness to borrow RCCs evolves over time according to their observed characteristics and the unobservables. 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

[23] 8.6 percent of mothers with full control over educational decision-making reported that they often asked their children to earn wage income, as opposed to 5.5 percent of those with zero control.

status. This sub-section proceeds to distinguish between short- and long-run impacts 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.[24] For example, gender difference turned out to emerge in schooling gap, with girls lacking behind, but disappeared in average scores. Some attributes lose their explanatory power in Table 6 compared to Tables 4-5, such as child health, whether attending the nearest school, parents' attitudes towards the child's educational achievements, share of unsafe classrooms and the village progression rate to junior high schools. In comparison, some became statistically significant, like two other indicators for school quality.

The negative $\hat{\omega}_1$ in all columns, albeit statistically insignificant, implies that in general those who had borrowed in 2000 would be 2.1-5.2 percent 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 4-5, while borrowing of microcredit appears to improve longer-term education for clients' children. $\hat{\theta}_1^{ITT}$ in columns 1-2 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.

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 percent, ±3 percent and ±1 percent 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 percent, 85.3 percent and 75

[24] An exemption is child labour. In the dynamic analysis (Column 1 of Table 6), one more hour of child labour per week would enlarge the schooling gap by 6.5 days within a year. However, the static analysis (columns 2-3 of Table 4) suggests the opposite: more child labour tended to narrow the schooling gap in 2000. Using data from rural Bangladesh, Islam and Choe (2013) attribute this to the fact that borrowing households use more child labour than non-borrowers. As borrowing microcredit helps to reduce the schooling gap, child labour thus might negatively correlate to the schooling gap. However, this seems counterintuitive – if borrowing is associated with use of child labour, then RCCs will have an adverse effect on attendance, leading to possibly poor performance. But we do not observe statistical significant relationship between borrowing RCCs and child average scores in the static analysis (Table 5).

Table 6 Dynamic treatment effects of borrowing RCCs

Independent variable	Schooling gap			Average scores		
	(1)	(2)	(3)	(4)	(5)	(6)
A. ITT						
$\hat{\theta}_0^{ITT}$	0.046	0.078	0.074	6.267***	-1.591	-1.440
	(0.050)	(0.064)	(0.061)	(2.297)	(1.399)	(1.366)
$\hat{\theta}_1^{ITT}$	-0.278***	-0.299*	-0.288	2.639	4.915*	7.160**
	(0.107)	(0.182)	(0.184)	(2.079)	(2.741)	(2.833)
Child characteristics						
Age	0.287***	0.131***	0.147***	-1.113	0.339	0.484
	(0.073)	(0.024)	(0.025)	(1.401)	(0.463)	(0.498)
Gender	-0.112	-0.092*	-0.069	-1.514	0.411	0.081
	(0.124)	(0.050)	(0.049)	(3.095)	(1.073)	(1.065)
Health status	0.082	-0.008	-0.027	0.509	1.135*	1.284**
	(0.078)	(0.028)	(0.028)	(1.621)	(0.593)	(0.625)
Ethnic minority	-0.139	0.386	0.394	-13.138***	-0.913	1.868
	(0.277)	(0.483)	(0.462)	(5.446)	(4.764)	(4.148)
Birth order	0.062	0.099**	0.084*	3.103	-1.152	-1.680
	(0.167)	(0.049)	(0.049)	(2.660)	(1.138)	(1.164)
Child labour	0.018**	-0.003	-0.002	0.104	-0.051	-0.025
	(0.007)	(0.003)	(0.003)	(0.151)	(0.086)	(0.089)
Capability of studying	-0.002	-0.056*	-0.048	5.138**	4.110***	4.324***
	(0.066)	(0.031)	(0.031)	(2.083)	(0.824)	(0.825)
Siblings' education	-0.453***	-0.015***	-0.013***	-1.189	-0.013	0.093
	(0.164)	(0.005)	(0.005)	(4.098)	(0.113)	(0.114)
Attending the nearest school	0.317	-0.143	-0.121	-8.830	2.931	5.308
	(0.318)	(0.119)	(0.126)	(14.143)	(4.051)	(3.801)
Parents' characteristics						
Father's education	-0.049**	-0.018*	-0.019**	-0.261	0.107	-0.010

	(0.023)	(0.009)	(0.005)	(0.432)	(0.177)	(0.174)
Mother's education	0.017	-0.003	0.001	0.547	0.150	0.183
	(0.024)	(0.005)	(0.005)	(0.567)	(0.115)	(0.112)
Parents' attitude: child's edu.	-0.035	-0.064	-0.061	-4.210	1.544*	1.312
	(0.131)	(0.045)	(0.045)	(4.056)	(0.865)	(0.860)
Parents' attitude: child's inc.	-0.072	0.029	0.028	-1.250	0.450	0.225
	(0.104)	(0.042)	(0.041)	(2.734)	(0.855)	(0.820)
Women's empowerment on child's edu.	0.049	0.036	0.040	4.351***	0.795	0.426
	(0.109)	(0.030)	(0.031)	(1.652)	(0.786)	(0.746)
Household characteristics						
Ln(hh wealth per capita)	0.222	-0.022	-0.036	4.302	1.680	1.889
	(0.174)	(0.074)	(0.074)	(4.470)	(1.496)	(1.584)
Ln(sample child's tuition)	-0.134	-0.098*	-0.134**	5.238*	2.748***	2.568**
	(0.125)	(0.051)	(0.055)	(2.848)	(1.021)	(1.018)
Ln(sample child's other edu. costs)	-0.151**	-0.091***	-0.096***	1.739	0.203	0.172
	(0.067)	(0.028)	(0.029)	(1.611)	(0.524)	(0.574)
Teacher and school characteristics						
Teachers' average edu.		-0.034	-0.027		0.487	0.737
		(0.030)	(0.034)		(0.623)	(0.702)
Student-teacher ratio		-0.001	-0.002		0.039	-0.082
		(0.003)	(0.005)		(0.088)	(0.103)
% unsafe classrooms		0.328***	0.309**		4.538***	2.901
		(0.126)	(0.150)		(1.747)	(2.005)
Village characteristics						
Distance to the nearest primary school		0.026	-0.013		0.479	0.227
		(0.018)	(0.028)		(0.449)	(0.524)
Distance to the nearest junior middle school		0.051	-0.004		0.311	2.231**
		(0.050)	(0.062)		(0.873)	(0.989)
Age at the first enrolment		0.062*	0.048		2.526***	1.035

		(0.032)	(0.036)		(0.687)	(0.761)
Proceed to secondary education		-0.175**	-0.162*		4.981***	6.265***
		(0.076)	(0.087)		(1.835)	(2.079)
% of RCCs borrowers		-0.018	0.080		4.323	3.429
		(0.182)	(0.210)		(3.381)	(4.915)
Ln(village per capita income)		0.010	0.034**		-0.224	-0.101
		(0.010)	(0.016)		(0.251)	(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	-0.021	-0.052	-0.024	-0.021	-0.052
	(0.018)	(0.034)	(0.038)	(0.018)	(0.034)	(0.038)
$\hat{\theta}_1^{TOT}$	-0.277***	-0.297*	-0.285	2.792	4.833*	7.085**
	(0.107)	(0.181)	(0.184)	(2.078)	(2.734)	(2.827)

Note: ***, ** and * denote 1%, 5% and 10% significance levels in turn. Constants, fixed-effects and polynomials are not reported. Standard errors in parentheses are clustered by child and those for the recursive TOT are calculated by the Delta method.

percent, 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 percent, ±3 percent and ±1 percent 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 percent, 29.4 percent and 44.4 percent, 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 Eq. (13). 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. Significantly negative $\hat{\theta}_1^{TOT}$ in columns 1- 2 and positive estimates in columns 5- 6 means 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 paper 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 by a 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 of 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 (three to four months) and an appreciable increase in average scores by five to seven 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.*, 2011a), formal microcredit would contribute to child education more effectively if the targeting problem for the poor could be alleviated.

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Appendix

Figure A.1: Sensitivity of the estimated impact on schooling gap to the bandwidth

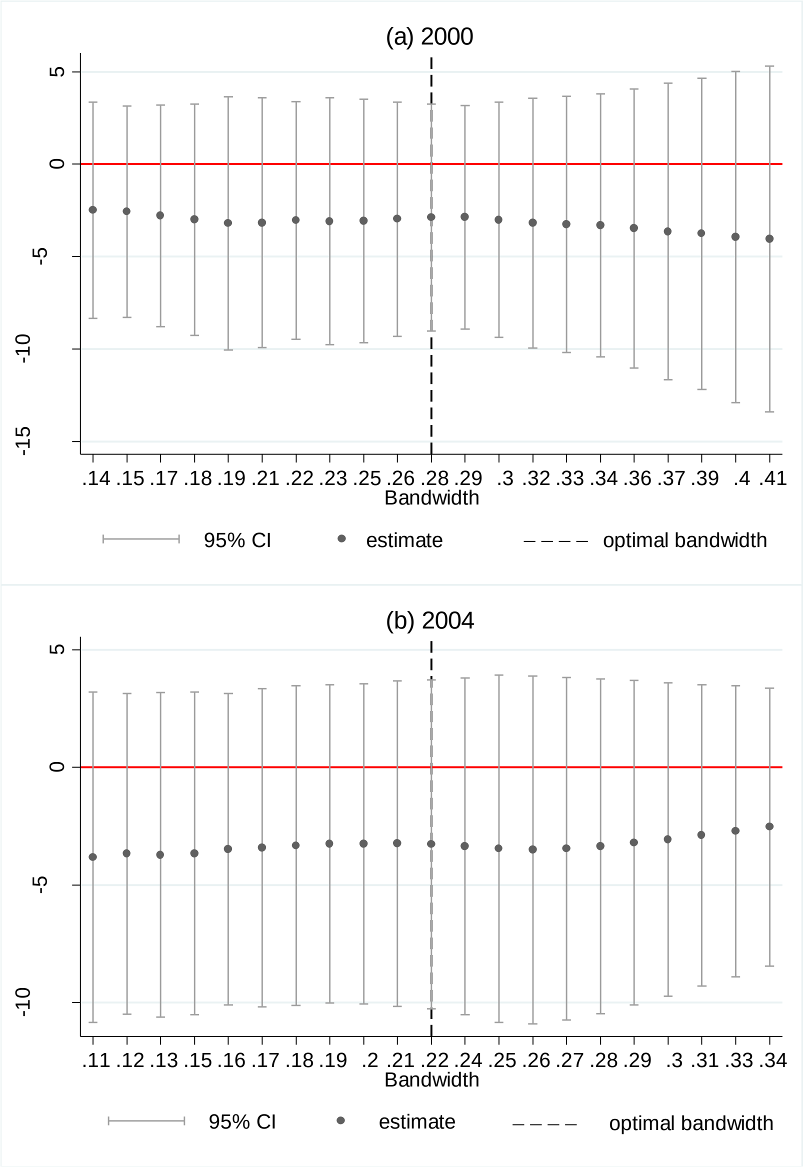

Figure A.2: Sensitivity of the estimated impact on average scores to the bandwidth

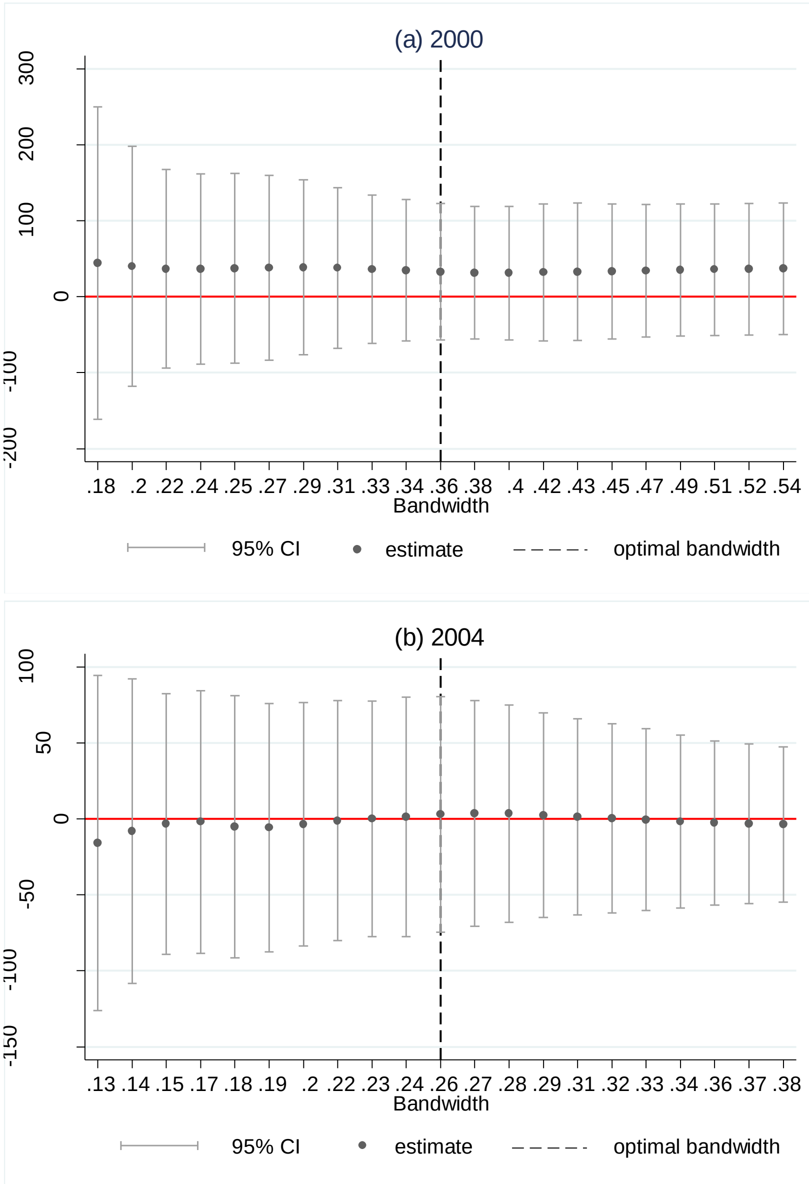

Table A.1: Correlates of the probability of borrowing RCCs

Independent variable	2000	2004
	(1)	(2)
hh size	-0.009 (0.060)	-0.064 (0.068)
No. of members not living in the hh	-0.055 (0.091)	0.048 (0.078)
No. of employed members	-0.108 (0.080)	-0.067 (0.215)
Average edu. of hh members	0.176** (0.077)	-0.119 (0.127)
Average age of hh members	-0.011 (0.009)	0.008 (0.005)
hh wealth status within the village	0.044 (0.069)	-0.069 (0.055)
Quality of housing	-0.092 (0.078)	0.083 (0.081)
Share of irrigated land	0.599 (0.411)	-0.184 (0.210)
Whether borrowing RCCs for child edu. (yes=1)	0.603*** (0.125)	0.548*** (0.087)
Whether hh income was sufficient in the past yr. (yes=1)	-0.239*** (0.083)	-0.235*** (0.065)
Ln(informal credits)	-0.096** (0.044)	-0.173* (0.090)
Village dummies	Yes	Yes
R^2	0.236	0.134

Note: ***, ** and * denote 1%, 5% and 10% significance levels in turn. Heteroscedasticity-robust standard errors are in parentheses. Village dummies and the constants are not reported.

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