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Children's schooling in Nicaragua: What is the link between educational achievement, borrowing, and gender?

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Abstract

The impact of credit has been widely studied, and yet little is known about the effect of formal versus informal loans. In this paper, we contrast the two and their impact on children's schooling using longitudinal data from Nicaragua. To address endogeneity, we utilize both household fixed effects and locality–year fixed effects. Our results indicate that, on average, children from borrowing households fare worse than children from nonborrowing households, with male borrowers having a disproportionately negative effect on boys, and vice versa for girls and female borrowers. Informal credit is found to have a protective effect on school attendance, but the effects of formal and informal credit on cumulative schooling are found to be statistically equivalent. However, this appears to mask considerable heterogeneity within informal borrowing.

1 | INTRODUCTION

The rhetoric of the early microfinance industry has put strong emphasis on investments in entrepreneurial activities, overlooking the broader importance of credit for the economic development of households (Karlan & Morduch, 2009). In reality, for low-income households, credit provides access to liquidity beyond investments in productive activities. Households with low irregular incomes borrow frequently for many different reasons—to invest in productive activities, build

assets, accumulate large lump sums for long-term investments and life-cycle needs (education, weddings, funerals), smooth consumption over time, and recover after emergencies (Bauchet, Marshall, Starita, Thomas, & Yalouris 2011; Collins, Morduch, & Rutherford 2009; Matin, Hulme, & Rutherford 2002; Todd 1996).

Credit allows resource constrained households to achieve greater economic stability and more command over their financial lives (Collins et al., 2009; Roodman, 2012), which may lead to improved household well-being. Conversely, inappropriate use of credit and unfavorable loan terms can cause over-indebtedness (Schicks, 2013) and severe credit repayment difficulties, leading to debt traps and deeper poverty (Roodman, 2012). While small loans can help cover households' subsistence needs, it is unlikely that they can have a transformational impact on well-being (Banerjee, 2013).

Further differentiating between formal and informal credit sources provides deeper insight into the role of credit for household well-being. More specifically, formal financial institutions—in developed and developing countries alike—have long shied away from loans for consumption smoothing, precisely because this use of credit generally does not increase overall household earnings and often fails to generate even the funds needed for repayment. In the absence of insurance products, households are then forced to turn to informal credit, in some cases at extremely high interest rates or involving coercive repayment schemes. However, some informal credit—for example, through family and friends—may have better terms even than formal loans, and households clearly benefit, at least in the short run, from improved consumption smoothing.

Moreover, there is reason to believe that the source of credit may have differential effects on outcomes, even holding loan terms constant. For example, in the case of education, poor households are more vulnerable to shocks (Morduch, 1994), and expenditure on children's schooling is commonly used to smooth other forms of consumption (Chen, 2011). Moreover, education and future earnings are not accepted as collateral against loans (Dowla, 2011; Jacoby & Skoufias, 1997), leaving informal loans as the only source of credit to both finance and smooth schooling expenses. Similarly, if formal credit is invested in productive activities, this may increase the demand for household labor, with potentially adverse effects on children's schooling (Pitt & Khandker, 1998), despite the (positive) income effect (Islam & Choe, 2013; Maldonado & González-Vega, 2008). Nonmonetary transaction costs associated with formal borrowing (e.g., time spent traveling to financial institutions and processing new loans) may also be significant. While parents are gone, children may be induced to skip school, or required to engage in business or household work. Pressure to avoid default on a loan may induce households to cut back on educational expenses or move children into income-generating activities. Conversely, "credit plus" microfinance programs that provide informational training can alter parents' perceptions about schooling and improve their knowledge about the benefits of education, increasing the level of investment in children's schooling.

Most empirical research, however, focuses on formal credit, despite the prevalence of informal credit and its importance to households' livelihoods in developing countries.[1] More specifically, between 2011 and 2014, borrowing from a financial institution (a bank or another type of financial institution) declined from 11.7 to 8.6 percent and borrowing from private informal lenders declined from 7.0 to 6.5 percent, while borrowing from family and friends increased from 30.2 to 34.9 percent in low-income countries (World Bank, 2012, 2015). Additionally, existing research often compares borrowers utilizing formal sources with a combination of nonborrowers and borrowers utilizing informal sources (Dalla Pellegrina, 2011). It is difficult to interpret this as a measure of credit constraints, given large differences across regions in both access to and terms of informal credit sources. Indeed, this could help explain the heterogeneous effects of microcredit found

throughout the literature; where the impact is small or even negative, it may be that informal credit markets are functioning relatively well, not that broad-based access to credit is not an important channel for development and growth. Alternatively, small or negative effects of formal credit may suggest that conditionality is excessive, and credit could, in fact, be quite transformative if households are given greater latitude. This would also be consistent with findings by De Mel, McKenzie, and Woodruff (2012) showing large and long-lasting impacts of unconditional one-time cash transfers to business owners.

The impact of informal credit, particularly relative to formal credit, remains an under-studied empirical question. To our knowledge, only a few studies have separated the impact of different borrowing channels (Akudugu, 2014; Nguyen & Van den Berg, 2014). Some research has attempted to draw causal inference between credit—both formal and informal—and investments in capital (Ayyagari, Demirgüç-Kunt, & Maksimovic 2010; Kan, 2000), investments in agricultural and nonagricultural productive activities (Dalla Pellegrina, 2011), or household well-being (Akudugu, 2014; Nguyen & Van den Berg, 2014). We have identified one study that investigates whether formal and informal credit impacts child schooling, and whether these effects are differentiated by the child's gender (Doan, Gibson, & Holmes 2014). The study uses cross-sectional data and finds that formal credit positively affects children's schooling, while informal credit has the opposite effect, and girls tend to have better schooling outcomes overall.

However, it is critical to control for unobserved characteristics of the household that are driving the demand for credit, as those seeking formal and informal loans are likely to have differing preferences and investment strategies for child schooling, as well as differing initial resources and endowments. This paper contributes to the literature by investigating the link between formal and informal borrowing and children's schooling in the context of Nicaragua, controlling for unobserved heterogeneity through the use of household fixed effects and time-varying aggregate shocks with locality–year fixed effects. We examine whether children from households utilizing credit have a higher likelihood of attending school, differentiating the effects by both the child's and the borrower's gender, and by informal and formal borrowing. We take a broad perspective on borrowing, which incorporates cash and in-kind borrowing through both formal and informal financial institutions.

Our results suggest that children from borrowing households, on average, have worse schooling outcomes than children from nonborrowing households. Additionally, these effects tend to be gender specific: the impact of borrowing is more adverse when children and borrowers are of the same gender. In our main results, the effect of informal credit cannot be statistically distinguished from formal borrowing. In summary, our findings indicate that credit expansion, despite relaxing constraints for households, may have adverse effects on children's educational outcomes. To counteract these adverse effects, the delivery of credit needs to be accompanied by social policies and programs acknowledging households' need for child labor, and the design of credit programs needs to align with the social objectives of borrowers and go beyond the credit programs.

The paper is organized as follows. Section 2 describes the research context. Section 3 presents the empirical approach. Section 4 describes the data and descriptive statistics. Section 5 presents our main results along with a discussion of robustness. Section 6 concludes.

2 | RESEARCH CONTEXT

2.1 | Gender, formal, and informal borrowing

Women are often the primary beneficiaries of the microfinance industry and informal credit associations: women tend to have higher loan repayment rates (D'Espallier, Guérin, & Mersland 2011), as they are more vulnerable to social pressure (Roodman, 2012), and they are more likely to be responsible for managing finances and investing in household well-being (Pitt & Khandker, 1998). Social investors also get higher intrinsic returns from targeting women (Demirgüç-Kunt, Beck, & Honohan 2008).

Channeling credit through female borrowers can increase women's personal income and assets and give them more control over household financial resources, thus shifting the intrahousehold distribution of power in favor of women. Increased bargaining power of women can then contribute to improvements in children's welfare, including schooling, since women have been found to spend more on children's education (Gitter & Barham, 2008), although there is also some conflicting evidence (Edmonds, 2006).

Alternatively, the moderating effects of gender may be negative. First, despite obtaining credit, women may not have full control over how loans are utilized (Goetz & Gupta, 1996; Kabeer, 2001). Additionally, besides differential preferences towards children's schooling, borrowing by males and females may generate gender-specific demand for child labor. Time and skills of children may be substitutes or complements to their parents' time and skills (Holvoet, 2004; Pitt & Khandker, 1998). When females engage in productive activities outside the house, girls can replace them in household chores and child care, or assist them in productive activities. Similarly, boys can assist adult males in productive activities, or may be expected to take over certain household activities.

Differentiation between formal and informal credit is also important. First, credit market segmentation may arise due to supply-side rationing. Since only borrowers know their true creditworthiness and repayment abilities, there exist informational asymmetries between borrowers and lenders. Overcoming informational asymmetries through screening, monitoring and contract enforcement may be prohibitively costly when borrower's income is low and the institutional environment is weak (Banerjee & Duflo, 2007; Madestam, 2014; Yadav, Otsuka, & David 1992), which makes lending to the poor particularly difficult. Credit rationing is considered the optimal strategy when information asymmetries are severe (Stiglitz & Weiss, 1981). Local informal lenders tend to have an informational advantage over formal lenders, since they possess better information about borrowers' creditworthiness, are better equipped to enforce credit contracts, and are more familiar with the demands of this particular clientele (Carpenter & Jensen, 2002; Pagura & Kirsten, 2006; Zeller, 1994). When borrowers are credit rationed by formal financial institutions, informal lenders serve as an alternative for less creditworthy borrowers.

At the same time, the coexistence of formal and informal credit markets indicates that borrowing occurs simultaneously through both channels (Banerjee & Duflo, 2007). In this case, different motives for borrowing may require different credit sources (Guérin, Roesch, Venkatasubramanian, & D'Espallier 2012). Specifically, formal lenders tend to specialize in production and asset accumulation loans (Mohieldin & Wright, 2000), zero-interest lenders in consumption loans, while moneylenders provide loans for any purpose at a higher cost (Yadav et al., 1992). Consumption spending and emergencies tend to be financed through informal credit (Barslund & Tarp, 2008; Mohieldin & Wright, 2000). As such, different types of educational expenses may be associated with different credit channels.

Finally, access to formal or informal credit tends to be differentiated by gender. In particular, gender norms can create differences in access to informal credit markets (Johnson, 2004). In terms of access to formal credit, historically, women's access to traditional formal credit was limited (Berger, 1989), although more recent research shows that differences in access to formal financial services disappear once appropriate controls are included (Aterido, Beck, & Iacovone 2013). Additionally, even though many microfinance institutions specifically target women and reserve more total resources for female borrowers, they may also allocate smaller loans to females (Godquin, 2004), or charge higher interest rates (Muravyev, Talavera, & Schäfer 2009). Given gender-specific differences in credit accessibility, repayment rates, and preferences, borrower's gender constitutes an additional moderating factor in the relationship between borrowing and schooling.

2.2 | Context: Nicaragua

This paper focuses on Nicaragua between 1998 and 2005, which despite significant economic, social, and political advances, remained among the poorest and least educated countries in Latin America (World Bank, 2008). The percentage of the population living below the national poverty line remained at 48.3 percent in 2005 (World Bank, 1960–2016), and Nicaragua ranked 112 out of 177 countries according to the Human Development Index in 2004 (United Nations Development Programme (UNDP), 2006). In terms of schooling, in 2004, 92.6 percent of primary-school-age children in Nicaragua were enrolled in primary schools (which lasts 6 years), but only 41.8 percent of secondary-school-age children were enrolled in secondary schools (which lasts 5–6 years depending on the track).

The formal financial sector in Nicaragua has been underdeveloped for a long time and experienced an unprecedented growth until 2008. In the absence of strong microfinance regulation, the rapid growth of the microfinance industry and overemphasis on credit led to the credit repayment crisis in 2009, during which several microfinance institutions went out of business. The number of microfinance loans subsequently decreased, limiting financial inclusion among lower income populations. Since the delinquency crisis, the financial industry has been recovering and growing. Our data predate this crisis by several years; however, it is possible that our estimates may understate the positive impact of formal borrowing if the lack of financial regulation led to suboptimal lending practices.

3 | ECONOMETRIC MODEL

To estimate the effects of borrowing on schooling outcomes, we consider two measures: *school attendance* and *schooling gap*. School attendance is a good measure of current educational investments, and takes a value of 1 if a child attended school in the current school year. Schooling gap is calculated as the difference between the highest grade completed and the expected level of schooling based on age (Maldonado & González-Vega, 2008; Islam & Choe, 2013):

$$Expected\ schooling = \begin{cases} 0 & if\ age \leq 7 \\ (age - 7) & if\ 8 \leq age \leq 18 \\ 11 & if\ age > 18 \end{cases}$$

$$Schooling\ gap = max\ \{0, expected\ schooling - actual\ schooling\}.$$

A child who has successfully progressed in school will not have any schooling gap, whereas a child who has not attended school continuously (late entry, grade repetition, expulsion, or voluntary temporary or permanent withdrawal) will have a positive schooling gap. Thus, *schooling gap* is a dynamic measure that accounts for disruptive versus continuous school enrollment over time and cumulative schooling outcomes.[2] Data limitations do not allow us to account for education quality or other schooling inputs (e.g., days attended, supplies, tutoring). Because the typical age at which children start school is 7 years old and the span of complete schooling is 11 years, children between 7 and 17 years old are included in the estimation of *school attendance*. Alternatively, since children are expected to complete the first grade by 8 years old and to graduate from school by 18 years old, children between 8 and 18 years old are the sample of interest for *schooling gap*.

We estimate the reduced form demand for schooling for child i, in household j and municipality k at time t, which can be expressed as

$$Y_{ijkt} = \beta_0 + \beta_1 borrower_{jkt} + \beta_2 X_{ijkt} + \beta_3 H_{jkt} + \beta_4 H^1_{jkt} + \mu_j + \gamma_{kt} + e_{ijkt} \quad (1)$$

where Y represents the schooling outcome (attendance, schooling gap), *borrower* is a dummy variable equal to 1 if a household borrowed in the past 12 months, and 0 otherwise, X represents individual-level time-varying characteristics, and H and H^1 represent household-level time-varying characteristics interacted and not interacted with child's gender, respectively.[3] Heteroskedasticity-robust standard errors are used to account for unequal error variance across different variable values, and standard errors are clustered at the municipality level to account for correlation among households within municipalities.[4]

In the equation above, μ_j is the household fixed effect, γ_{kt} is the urban–municipality–year fixed effect, and ϵ_{ijkt} is an error term adjusted for heteroskedasticity and within-group correlation. The unit of observation is the child, but longitudinal data permit us to include household and urban–municipality–year fixed effects in the equation thus controlling for unobserved characteristics that may affect both educational attainment and borrowing decisions. Household fixed effects account for unobserved household characteristics that are fixed over time, such as intrahousehold attitudes towards schooling, inter-temporal and gender preferences, motivation, entrepreneurship skills, and some elements of access to credit, both formal and informal. Urban–municipality–year fixed effects account for unobserved time-varying conditions at the municipality level, such as systemic shocks, entry/exit in financial markets and among informal lenders, urbanization levels, and access to and quality of schools. Inclusion of the urban/rural division controls for rural-specific shocks (e.g., low agricultural yield because of drought) versus urban-specific shocks (e.g., urban development) within the same municipality.

Additionally, we allow some parameters to differ with the gender of the child by defining

$$\beta_s = \beta^0_s + \left(girl_{ijkt} * \beta^g_s\right) \text{ for } s = 0, 1, 2, 3 \quad (2)$$

where s indexes the parameters defined in Equation 1 (β_0, β_1, β_2, β_3). β^0_s represents the intercept for boys (the base level) for each coefficient, and the intercept for girls relative to boys is β^g_s. Including this interaction allows us to compare the effects of borrowing on schooling outcomes for girls and boys. Beyond differentiating the outcomes by child's gender, we also differentiate the regressor of interest, borrowing behavior within households (β_1), by the gender of the borrower,

the source of the loan, and their interaction:

$$\beta_1 = \beta_1^0 + \left(female_{jkt} * \beta_1^f\right) + \left(informal_{jkt} * \beta_1^i\right) + \left(female_{jkt} * informal_{jkt} * \beta_1^{fi}\right) \quad (3)$$

where *female* is a dummy variable equal to 1 if the borrower was a female, and 0 otherwise, and *informal* takes a value of 1 if a household had informal credit, and 0 otherwise. The reference group is formal credit obtained by a male borrower. Credit from formal sources includes private banks, state banks, nonprofit organizations, financial cooperatives and associations, and credit from informal sources includes friends, relatives, producers' associations, and moneylenders. Both cash and in-kind loans are included.

After decomposing parameter β_1, the marginal effect of borrowing on schooling outcomes for different types of borrowers and children is listed in Table 1. For instance, β_1^0 represents the average schooling of boys in households with a male borrower, relative to nonborrowing households, and $(\beta_1^0 + \beta_1^{0g} + \beta_1^f + \beta_1^{fg})$ is the average schooling of girls from households with a female borrower, again relative to nonborrowing households. The use of interaction terms allows us to test directly for differences in effects of borrowing on schooling of girls and boys, differentiating credit by borrower's gender and loan source. For example, the effect of female- relative to male-borrowing is β_1^f, and the differential effect for girls is β_1^{fg}. Following this logic, we obtain eight different combinations, all of which should be viewed relative to nonborrowing households. Therefore, in this study, we investigate whether borrowing results in better schooling outcomes compared with nonborrowing, and whether the effects of borrowing differ by credit sources, borrower's gender, and child's gender.

Control variables at the individual level (X) include child's age, a quadratic in the child's age, child order within household, and an indicator for whether one of the child's parents is the household head. Control variables at the household level (H) include the number of household members, presence of young children (under 6 years old), presence of elderly (over 60 years old), average number of schooling years of all household adults, area of owned land, income assistance from friends and relatives, and an indicator for female household headship. To allow for a fully flexible specification, these control variables are also interacted with the child's gender. Two control variables at the household level (H^1)—travel time to primary school and participation in educational programs—are not interacted with the child's gender, since the effects of these variables are less likely to be differentiated by the child's gender.

Selection bias on both the demand and supply side of the credit market (Banerjee, Karlan, & Zinman 2015; Islam & Choe, 2013; Pitt & Khandker, 1998) are major concerns for our research. On the demand side, borrowers self-select to apply or not to apply for credit, which implies that borrowers are systematically different from nonborrowers on observable and unobservable characteristics. For example, people who apply for credit may be wealthier, healthier, more educated, as

TABLE 1 Average effects of borrowing on child's schooling

	Girls	Boys
Male borrower	$\beta_1^0 + \beta_1^{0g}$	β_1^0
Female borrower	$\beta_1^0 + \beta_1^{0g} + \beta_1^f + \beta_1^{fg}$	$\beta_1^0 + \beta_1^f$
Male informal borrower	$\beta_1^0 + \beta_1^{0g} + \beta_1^i + \beta_1^{ig}$	$\beta_1^0 + \beta_1^i$
Female informal borrower	$\beta_1^0 + \beta_1^{0g} + \beta_1^f + \beta_1^{fg} + \beta_1^i + \beta_1^{ig} + \beta_1^{fi} + \beta_1^{fig}$	$\beta_1^0 + \beta_1^f + \beta_1^i + \beta_1^{fi}$

Note: Comparison is against nonborrowing.

well as more business savvy, proactive, socially connected, or have different preferences. These unobserved attributes may influence both decisions to apply for loan and the level of investment in children schooling, which can bias the findings. On the supply side, first, placement of credit suppliers is not random. For example, areas with better infrastructure, road access, denser populations, and favorable citizen attitudes are more likely to attract formal financial institutions, and experience a higher demand for credit. Second, lenders apply some criteria to evaluate creditworthiness of loan applicants offering loans to some individuals and denying others. The criteria applied may be systematic, following well-defined eligibility standards, or nonsystematic, where creditors evaluate clients using subjective criteria.

In our case, including control variables helps reduce but cannot entirely eliminate the selection bias. Additionally, household fixed effects control for time-invariant characteristics of the household that may be correlated with credit and schooling, while the municipality–year fixed effects account for time-varying aggregate economic shocks and market conditions. The key identifying assumption then is that changes in household borrowing behavior are not jointly determined with changes in child schooling, conditional on the fixed effects and our control variables. However, there may still be time-varying household- or child-level shocks that affect both credit and schooling decisions. We return to this issue in Section 5 and present several robustness checks to support our main results.

4 | DATA AND SAMPLE

The data come from the Living Standards Measurement Study Survey 1998/99 and 2005 (Encuesta Nacional de Hogares sobre Medición de Nivel de Vida, 1998/99; the Encuesta Nacional de Hogares sobre Medición de Nivel de Vida, 2005) collected by the Nicaraguan National Institute of Statistics and Census (Instituto Nacional de Estadísticas y Census) with technical support from the World Bank. The survey collects household-level data on dwelling characteristics, demographic composition, consumption, business and agricultural activities, financial behaviors, and individual-level data on education and employment.

The panel dataset includes households present in both survey waves. The sample is limited to school-age children (7–18 years old, depending on the model), and consists of children who were present in at least one survey wave; a child may be present in both survey waves or in a single survey wave (as a result of either age restrictions or sample attrition). There is just over 6 percent child-level attrition from the sample; since the majority of these individuals are older children who would have been 17 to 18 years old in 2005, the probable reasons for attrition are migration from the household for work, studies or marriage. We include both households with female and male borrowers in the model, but exclude households with both male and female borrowers so that the coefficients can be clearly interpreted as the effect of each borrower type relative to no borrowing. The proportion of excluded observations equals 5 percent among all children from borrowing households. Both cash and in-kind loans from both formal and informal sources are included.[5] The final sample contains 1,550 households and 8,004 observations at the child–year level. Overall, 24.5 percent of children come from households that obtained at least one loan in the past 12 months, among which 54 percent come from households where credit was obtained by female(s) and 46 percent by male(s). Approximately 51.6 percent of households borrow through formal channels, and 48.4 percent borrow from informal channels.

Table 2 reports descriptive statistics for the sample of households that did not borrow in 1998 and provides a clear indication of how comparable households are in the absence of borrowing.

TABLE 2 Descriptive statistics, 1998

	Household does not borrow in 2005		Household borrows in 2005		
	Sample mean/ proportion	Median	Sample mean/ proportion	Median	Diff.
Individual characteristics					
Male	0.52	1	0.50	1	
Average age	11.97	12	11.96	12	
Household characteristics					
Household size	6.97	6	6.80	6	
Female-headed household	0.29	0	0.28	0	
Average years of schooling of household adults	1.85	1.25	2.05	1.50	
Number of 0–6 years olds	1.44	1	1.35	1	
Number of 18–59 years olds	2.62	2	2.73	2	
Number of 60 and over years olds	0.31	0	0.21	0	***
Urban setting	0.49	0	0.49	0	
Minutes to a health facility	45.27	20.0	39.67	20.0	
Minutes to a primary school	18.31	10.0	16.77	10.0	
Participated in an educational program	0.37	0	0.39	0	
Received remittances from relatives or friends (in the past month)	0.20	0	0.20	0	
Owns a color TV	0.20	0	0.18	0	
Owns a refrigerator	0.13	0	0.14	0	
Owns a bicycle	0.22	0	0.29	0	**
Household is poor (assessed by World Bank)	0.59	1	0.58	1	
Number of adults working last week	1.68	2	1.71	2	
Had a business or worked independently (in the past 12 months)	0.34	0	0.33	0	
Owned land size (in hectares)	5.70	0	4.82	0	
Engaged in agriculture (on own land)	0.26	0	0.20	0	**
Raised livestock (in the past 12 months)	0.39	0	0.33	0	*
Observations (children)	2,454		866		

Note: Statistically significant differences across columns are reported at the 1% (***), 5% (**), or 10% (*) level, based on a *t* test. The data are in the long format. Individual-level characteristics are included for each child. Household-level characteristics are reported for each household.

When limiting our attention to nonborrowing households in 1998 and comparing only on the basis of 2005 borrowing status, we see that there are almost no statistically significant differences in the observable characteristics in 1998.

Intergroup differences are observed in the full sample (Table A in the Online Appendix—for access, see Supporting Information at the end of this paper),[6] as this also includes households that borrowed in 1998 and, therefore, conflates differences across borrowing status with differences across survey periods. Importantly, descriptive statistics for the full sample highlight the need to distinguish borrowing by loan source and borrower's gender. Specifically, borrowing channel and purpose tend to be associated: on average, informal loans are used overwhelmingly to fund emergencies and household expenses, while formal credit sources are roughly equally distributed across agriculture, business, and emergency/household expenses. Households with female borrowers tend to borrow more for nonagricultural activities and home improvement or emergencies, and households with male borrowers tend to borrow more for agricultural purposes. Finally, with one exception, statistically significant differences are observed for outcome variables across all subgroups. Outcome means suggest that children from borrowing households (households with female borrowers, and households with formal loans) generally have better schooling outcomes than children from nonborrowing households (households with male borrowers, and households with informal loans). However, as seen in the next section, drawing conclusions based only on sample means may be misleading, which highlights the need to appropriately account for intergroup differences.

5 | EMPIRICAL FINDINGS

5.1 | Results and discussion

This section presents empirical findings describing average marginal effects of borrowing on schooling outcomes. We estimate regression models reported in Equations 1 to 3, comparing schooling outcomes for girls and boys from nonborrowing households, male versus female borrowers, and formal versus informal borrowers. Both household– and urban–municipality–year fixed effects are included in all specifications, and standard errors are clustered at the municipality level and robust to heteroskedasticity.

When averaged over loan types and gender groups, the effect of borrowing on the schooling gap is positive (Table 3, column 1); that is, children are more likely to have schooling interruptions when the household obtains credit. Adding an indicator to distinguish informal borrowing reduces the magnitude of the coefficient and the precision, though the coefficient on informal borrowing itself is not statistically significant (column 2). Further distinguishing by borrower's gender significantly reduces the precision of the estimates, pointing to heterogeneity effects within these groups (column 3). Indeed, looking at the model that includes child gender and interactions of gender with other covariates (column 4), we find strong evidence of borrower-specific effects, particularly when the borrower and the child are of the same gender. The relative effect of a female borrower is negative and statistically significant, with the opposite being true for girls. On net, boys in households with female borrowers experience no effect on their schooling, while girls experience a net increase of 0.22 years in their schooling gap. Conversely, boys in households with male borrowers experience a net increase of 0.46 years in their schooling gap, and there is no statistically significant effect for girls. Interestingly, we do not observe a corresponding effect for school attendance (column 6). It seems, therefore, that children still attend school when the household takes a loan, but the borrowing activity disrupts their schooling investments such that they are unable to successfully complete the school year.

For informal loans, we observe essentially no statistically significant difference between formal and informal borrowing for children's cumulative schooling, though the point estimates are relatively large in magnitude. These coefficients may be imprecisely estimated because informal

TABLE 3 Effect of borrowing on child outcomes, fixed effects estimates

	A. Schooling gap					B. School attendance[a]	C. Any work last week	D. Any illness last month
Variable	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Effects for boys								
Borrowed last year	0.19***	0.16*	0.19	0.46***	0.34***	–0.04	0.00	0.04
	(0.07)	(0.09)	(0.13)	(0.17)	(0.14)	(0.04)	(0.04)	(0.07)
Female borrower			–0.07	–0.49**	–0.28	–0.03	–0.01	0.00
			(0.18)	(0.23)	(0.18)	(0.05)	(0.06)	(0.08)
Informal loan		0.06	–0.07	–0.27		0.02	–0.03	0.04
		(0.12)	(0.20)	(0.23)		(0.06)	(0.07)	(0.08)
Informal × female			0.23	0.41		0.13*	0.06	–0.04
			(0.25)	(0.33)		(0.07)	(0.10)	(0.09)
Constant	4.70***	4.71***	4.67***	4.73***	4.66***	0.28	0.98***	0.04
	(0.98)	(0.98)	(0.97)	(0.94)	(0.95)	(0.24)	(0.25)	(0.27)
Relative effects for girls								
Borrowed last year				–0.49***	–0.34**	0.04	–0.04	–0.07
				(0.17)	(0.15)	(0.04)	(0.04)	(0.05)
Female borrower				0.74***	0.61***	0.03	0.14**	0.09
				(0.21)	(0.18)	(0.04)	(0.06)	(0.06)
Informal loan				0.34		0.02	0.03	0.09
				(0.24)		(0.05)	(0.06)	(0.10)
Informal × female				–0.27		–0.13**	–0.13	–0.01
				(0.35)		(0.07)	(0.10)	(0.11)
Constant	0.80	0.79	0.78	0.87	0.94	0.03	–1.54***	–0.47
	(1.16)	(1.16)	(1.16)	(1.18)	(1.17)	(0.19)	(0.23)	(0.31)
Observations	7,035	7,035	7,035	7,035	7,035	7,190	7,026	7,034
Number of households	1,429	1,429	1,429	1,429	1,429	1,452	1,429	1,429

Note: Robust standard errors are clustered at the municipality level, and reported in parentheses. Statistical significance: ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$. Model 4 includes household fixed effects, community–year fixed effects, and controls for child age, a quadratic in the child's age, child order, and relation to household head; household size and composition, average schooling of adults, owned land, transfers, and female household headship, all interacted with child gender. Travel time to school and educational programs are not interacted with child gender. Includes children age 8–18 unless otherwise noted. Models 1–4 show the progression of our results as more credit variables are added. Model 5 excludes informal credit. [a]Includes children age 7–17.

borrowing itself can be quite heterogeneous, with some loans taken from moneylenders at very high interest rates, and other loans taken from family members with subsidized or zero interest rates. The effects are generally opposite in sign to the main borrowing effect and smaller in magnitude. If we take the estimates at face value, they suggest that the effects of informal borrowing are generally the same as those for formal borrowing, though more muted. That is, borrowing leads to worse schooling outcomes, particularly for children of the same gender as the borrower, with

formal credit having a more pronounced adverse effect. However, there is some evidence that informal borrowing may improve school attendance, particularly for boys in households with female borrowers, though the coefficients are only marginally significant. This would be consistent with the notion that informal loans—and only informal loans—can be used to help smooth schooling expenditures (i.e., to pay school fees so that a child can still attend), and that boys' schooling is more likely to be protected than girls' schooling.[7]

Omitting informal credit and comparing formal borrowers with both nonborrowers and informal borrowers (Table 3, column 5) does not change the sign of our findings, but reduces the magnitude of coefficients and changes the significance of the coefficient on borrowing by females. On net, the adverse effect of credit on schooling gap is reduced for boys in households with male borrowers (from 0.46 to 0.34 years), and increases for girls in households with female borrowers (from 0.22 to 0.33 years). While these differences are not particularly large in magnitude, ignoring informal credit clearly leads to attenuation bias, consistent with measurement error in the credit activity variables. The point estimates also become less precise, which may lead to incorrect conclusions about the importance of borrowing on schooling outcomes. Moreover, it is important to note that while our results show that informal borrowers should not be casually combined with nonborrowers, the resulting bias depends heavily on context, particularly the strength and depth of financial markets, institutions, and social networks.

Overall, our findings suggest that the impact of borrowing on schooling outcomes is negative and gender specific: schooling outcomes of children are worse when the child and the borrower have the same gender, compared to children from nonborrowing households or children from households with borrowers of the opposite gender. These findings are slightly more pronounced for boys than for girls and are quite large in magnitude. Boys in households with male borrowers have, on average, nearly one-half of a year less schooling (0.46), and girls in households with female borrowers have nearly one-quarter of a year less schooling (0.22), compared to their counterparts in nonborrowing households. The channel of borrowing also seems to matter, though only marginally. The net effects of informal borrowing are the same in sign, but more modest in magnitude. However, our estimates are quite noisy, which makes it difficult to rule out the possibility that certain types of informal borrowing (e.g., from high-interest moneylenders) could have very different effects. Additionally, it appears that informal borrowing may be particularly useful for securing school attendance, perhaps given the greater flexibility to use informal loans for consumption smoothing.[8]

The adverse effects on schooling attainment can be suggestive of a relationship between credit and the demand for child labor. On average, 58 percent of households with female borrowers owned businesses, compared with 39 percent of households with male borrowers and 39 percent of nonborrowing households. Also, 40 percent of households with male borrowers were engaged in agricultural activities on own land, compared with 12 percent of households with female borrowers and 26 percent of nonborrowing households (see Table A in the Online Appendix—for access, see Supporting Information). In short, productive activities of borrowers tend to be gendered, which can require gender-specific child labor. Particularly, girls' time and skills can substitute or add on females' time and skills, and the same relationship holds for boys and adult males. To examine this link, we explore the relationship between borrowing and child labor, defined as performance of any type of work in the past week. This includes working for pay, apprenticeship, helping in the family business, selling goods, or helping on the family farm.

In Table 3 (column 7), we see that girls are indeed more likely to be working when living with a female borrower. The probability that a school-aged girl has done any income-generating work in the last week increases by 14 percentage points relative to boys when a female in the household

takes a loan. Interestingly, there is suggestive evidence that informal borrowing mitigates this—the coefficient on informal borrowing interacted with borrower's gender is roughly the same in magnitude and opposite in sign, though the coefficient is not significant at conventional levels. All other point estimates are very small in magnitude and not statistically significant, suggesting no impact on child labor for boys or even for girls in households with a male borrower. Therefore, while there is some evidence for the education–labor substitution effect, less schooling is not always associated with more child labor. That a significant effect is observed only for girls with female borrowers points to a particular complementarity between girls' labor and women's self-employment activities. There are potentially complex implications here for program design; programs targeting female borrowers may find short-lived effects on female empowerment, if the induced pattern of labor demand interferes with girls' schooling.

Conversely, men's businesses may have a greater specific need for adult male labor. However, if borrowing allows these family businesses to expand, this could also reduce the returns to boys' schooling, to the extent that they expect to take over these businesses later on. This could explain the adverse effect of male borrowing on boys' schooling combined with no change in income-generating activities. Note, however, that we measure child labor only on the extensive margin. It is also possible that children are significantly increasing work hours with household borrowing, even though there are no new children entering the labor force. Additionally, owing to data limitations, we are unable to include in our child labor measure any nonincome-generating activities such as household chores/maintenance and childcare. It is possible then that, as parents spend more time in income-generating activities, children are drawn in primarily to take over unpaid household tasks.

Still, the lack of clear findings on child labor in the presence of a large effect on schooling raises the concern that our results are driven by an unobserved child- or household-specific shock that both deters schooling and motivates households to borrow. For example, serious illness could require large expenditures on medical care and time away from income-generating activities while also interrupting schooling activities. To check this, we look at whether the child experienced any illness in the past month (Table 3, column 8). We find no statistically significant effects of borrowing on child health, and the point estimates are all quite small in magnitude. Thus, our results do not appear to be explained by this type of child-specific shock. In the following subsection, we consider other potential threats to our identifying assumption.

5.2 | Robustness checks

Given the inclusion of fixed effects, the adverse effect of credit on schooling cannot be explained by any unobserved time-invariant household characteristics, or by aggregate economic shocks at the municipality level. However, there remains the possibility that households have experienced some unobserved time-varying shocks that affect both borrowing and schooling.

Of course, it is difficult to test for this directly, nor are there valid instrumental variables for both formal and informal borrowing. Instrumental variables used in other studies are not appropriate in this context. For example, eligibility criterion tied to land ownership has been used extensively as an instrument in the context of microfinance institutions in South Asia (Islam & Choe, 2013; Dalla Pellegrina, 2011; Pitt & Khandker, 1998). However, there is no eligibility rule that is applied universally for formal financial institutions in Nicaragua, and informal lenders do not have any stated eligibility requirements. Finding measures of distance unrelated to schooling decisions also has conceptual and data challenges. The two survey waves lack consistent data on distance measures, such as access to banks or roads, which could potentially correlate with access to credit. At the same time, the distance to a healthcare facility used as an instrument in Gitter and Barham

(2007) may have a direct effect on long-term schooling if health affects the returns to or costs of schooling. In our case, we observe a moderate correlation between distance to health facility and distance to public primary school (0.39).

Instead, we follow two alternative approaches to address potential endogeneity concerns. First, we split the sample based on poverty status (as defined by the World Bank). Because poor households are, by definition, on the brink of subsistence, they are more vulnerable to shocks, and they have fewer or less effective risk-coping mechanisms. Therefore, poor households are more likely to be forced to use child schooling as a consumption-smoothing mechanism, in order to maintain necessities. Similarly, poor households would, on average, be more willing to incur transaction costs to borrow in the event of an adverse shock. Thus, if our results are driven by time-varying household-specific shocks, we should observe more pronounced effects for poor households than for nonpoor households. That is, a shock to a poor household is more likely to induce borrowing and decrease schooling.

In contrast, we see that the effects are, if anything, slightly larger for nonpoor households (Table 4, columns 1 and 2). In both poor and nonpoor households, boys fare worse with male borrowers, and girls fare worse with female borrowers, and the point estimates are remarkably similar, though more precise and slightly larger for the nonpoor households. This provides suggestive evidence that unobserved time-varying shocks are not driving our results, as we would expect poor households, given their proximity to subsistence consumption levels, to respond more strongly to a shock, *ceteris paribus*. There is a marginally significant protective effect of informal borrowing by women, for school attendance, which is less evident for poor households (Table 5, columns 1 and 2). However, the coefficients associated with informal borrowing are not consistently larger for poor households than for nonpoor households, again suggesting that our results are not driven by unobserved household-level shocks. Rather, this may suggest that poor households are more likely to access informal loans with unfavorable terms, while nonpoor households may have strong social and family networks for informal lending. Otherwise, for school attendance, we observe the same effects for formal borrowing across poor and nonpoor households.

Additionally, we conduct robustness checks on more balanced samples. We start off by re-running the analysis limiting the sample to households that did not borrow in 1998, which is mostly balanced on observables (see Table 2). We see that the point estimates remain largely unchanged with regard to sign, though less precise with the smaller number of borrowing households in the sample (Table 4, column 3). An exception is the coefficient on borrowing for school attendance, which moves in the expected direction and is statistically significant at the 5 percent level (Table 5, column 3).

Next, we balance the sample by applying propensity score methods. We estimate propensity scores in two ways: by predicting household's probability of being a borrower in 1998 conditional on selected households characteristics in 1998 that could not be influenced by borrowing, and by predicting the likelihood of borrowing in 2005 given characteristics in 1998 for households that did not borrow in 1998. We use generated propensity scores to weigh each observation and perform the fixed effects analysis on a more balanced sample. This technique allows us to achieve greater similarity between borrowing and nonborrowing households, minimizing the differences on observables between the subgroups, and maintain the use of fixed effects to account for time-invariant unobservable characteristics.

The first reweighted model can be compared with the main model (Tables 4 and 5, column 4). For schooling gap, the coefficients on formal credit are consistent with the main results in terms of both magnitudes and statistical significance. Coefficients on informal borrowing move in the same direction and, additionally, a statistically significant effect for informal credit is found. The

TABLE 4 Effect of Borrowing on Schooling Gap, Fixed Effects Estimates, Alternate Samples and Models

	A. Poor Households (1)	B. Non-Poor Households (2)	C. Non-Borrowing Households in 1998 (3)	D. Re-Weighted (Full Sample)[a] (4)	E. Re-Weighted (Non-Borrowing Households in 1998)[b] (5)	F. Excludes Households with Both Formal and Informal Loans (6)	G. Includes Households with Both Male and Female Borrowers (7)
Effects for Boys							
Borrowed Last Year	0.51* (0.29)	0.60*** (0.21)	0.24 (0.34)	0.51*** (0.16)	0.37 (0.34)	0.44*** (0.18)	0.33** (0.15)
Female Borrower	−0.54* (0.32)	−0.62** (0.30)	−0.77* (0.39)	−0.41** (0.20)	−0.68 (0.41)	−0.50** (0.25)	−0.34 (0.21)
Informal Loan	−0.32 (0.35)	−0.52 (0.33)	0.00 (0.41)	−0.40* (0.23)	−0.08 (0.44)	−0.26 (0.23)	−0.18 (0.20)
Informal*Female	0.65 (0.42)	0.44 (0.43)	0.62 (0.52)	0.51 (0.35)	0.39 (0.52)	0.44 (0.35)	0.33 (0.32)
Constant	−0.57 (1.49)	1.83 (1.46)	4.83*** (1.11)	5.02*** (1.03)	4.51*** (1.20)	4.82*** (1.00)	4.71*** (0.88)
Relative Effects for Girls							
Borrowed Last Year	−0.39 (0.25)	−0.59** (0.29)	−0.44 (0.36)	−0.48*** (0.17)	−0.40 (0.35)	−0.58*** (0.18)	−0.44*** (0.15)
Female Borrower	0.65* (0.35)	0.86*** (0.32)	1.06** (0.38)	0.67*** (0.20)	0.96** (0.39)	0.91*** (0.20)	0.67*** (0.20)
Informal Loan	0.21 (0.33)	0.25 (0.38)	−0.05 (0.45)	0.57** (0.25)	−0.13 (0.44)	0.44* (0.24)	0.34 (0.21)
Informal*Female	−0.19 (0.50)	−0.24 (0.48)	−0.16 (0.49)	−0.54 (0.40)	0.06 (0.48)	−0.46 (0.35)	−0.27 (0.33)
Constant	0.89 (1.84)	1.09 (1.39)	1.21 (1.45)	−0.40 (1.27)	1.36 (1.80)	1.10 (1.21)	0.67 (1.13)
Observations	4,371	2,608	5,530	7,029	5,459	6,877	7,212
Number of Households	772	647	1,106	1,427	1,092	1,399	1,464

Notes: Robust standard errors are clustered at the municipality level, and reported in parentheses. Statistical significance: ***p<0.01, **p<0.05, *p<0.1. Each model includes household fixed effects, community-year fixed effects, and controls for child age, a quadratic in the child's age, child order, and relation to household head; household size and composition, average schooling of adults, owned land, transfers, and female household headship, all interacted with child gender. Travel time to school and educational programs are not interacted with child gender. Includes children age 8-18.

[a]Propensity scores are obtained by regressing household borrowing decisions (in 1998) on household characteristics (in 1998), including demographic characteristics, educational background, geographic setting, and selected wealth measures that could not be influenced by borrowing.

[b]Propensity scores are obtained by regressing household borrowing decisions (in 2005) on household characteristics (in 1998), including demographic characteristics, educational background, geographic setting, borrowing status, poverty status, as well as engagement in agricultural and self-employment activities. Sample is limited to households that did not borrow in 1998.

TABLE 5 Effect of Borrowing on School Attendance, Fixed Effects Estimates, Alternate Samples and Models

	A. Poor Households (1)	B. Non-Poor Households (2)	C. Non-Borrowing Households in 1998 (3)	D. Re-Weighted (Full Sample)[a] (4)	E. Re-Weighted (Non-Borrowing Households in 1998)[b] (5)	F. Excludes Households with Both Formal and Informal Loans (6)	G. Includes Households with Both Male and Female Borrowers (7)
Effects for Boys							
Borrowed Last Year	−0.02 (0.06)	0.00 (0.06)	−0.16** (0.06)	−0.01 (0.04)	−0.15** (0.05)	−0.04 (0.04)	−0.03 (0.04)
Female Borrower	−0.04 (0.11)	−0.06 (0.06)	0.05 (0.09)	−0.06 (0.05)	0.02 (0.08)	−0.02 (0.06)	−0.05 (0.05)
Informal Loan	0.03 (0.08)	−0.05 (0.08)	0.13 (0.10)	−0.03 (0.05)	0.11 (0.09)	0.02 (0.06)	0.04 (0.06)
Informal*Female	0.11 (0.13)	0.18* (0.09)	0.09 (0.13)	0.13** (0.06)	0.11 (0.12)	0.12 (0.07)	0.12* (0.07)
Constant	0.14 (0.17)	0.14 (0.30)	0.29 (0.31)	0.19 (0.22)	0.03 (0.29)	0.18 (0.24)	0.29 (0.23)
Relative Effects for Girls							
Borrowed Last Year	0.04 (0.05)	0.01 (0.05)	0.09 (0.06)	0.00 (0.04)	0.07 (0.05)	0.07* (0.04)	0.04 (0.03)
Female Borrower	0.05 (0.07)	0.02 (0.06)	0.00 (0.07)	0.05 (0.04)	0.00 (0.06)	0.00 (0.04)	0.03 (0.04)
Informal Loan	0.03 (0.06)	0.02 (0.08)	−0.07 (0.08)	0.05 (0.05)	−0.04 (0.07)	−0.01 (0.05)	0.01 (0.05)
Informal*Female	−0.17* (0.10)	−0.10 (0.10)	−0.09 (0.11)	−0.12* (0.07)	−0.09 (0.10)	−0.10 (0.07)	−0.12* (0.06)
Constant	0.43* (0.23)	0.35 (0.26)	0.01 (0.24)	0.31 (0.21)	0.03 (0.29)	0.08 (0.19)	0.07 (0.19)
Observations	4,529	2,603	5,696	7,176	5,620	7,027	7,366
Number of Households	793	649	1,134	1,449	1,120	1,422	1,490

Notes: Robust standard errors are clustered at the municipality level, and reported in parentheses. Statistical significance: ***p<0.01, **p<0.05, *p<0.1. Each model includes household fixed effects, community-year fixed effects, and controls for child age, a quadratic in the child's age, child order, and relation to household head; household size and composition, average schooling of adults, owned land, transfers, and female household headship, all interacted with child gender. Travel time to school and educational programs are not interacted with child gender. Includes children age 7-17.

[a]Propensity scores are obtained by regressing household borrowing decisions (in 1998) on household characteristics (in 1998), including demographic characteristics, educational background, geographic setting, and selected wealth measures that could not be influenced by borrowing.

[b]Propensity scores are obtained by regressing household borrowing decisions (in 2005) on household characteristics (in 1998), including demographic characteristics, educational background, geographic setting, borrowing status, poverty status, as well as engagement in agricultural and self-employment activities. Sample is limited to households.

estimates suggest that informal borrowing by households with male borrowers attenuates the negative effects of formal credit for boys and wipes out any positive effects of formal credit for girls. The coefficients for school attendance maintain statistical significance observed in the main model. The second reweighted model is analogous to the model limited to households that did not borrow in 1998 (Tables 4 and 5, column 5). The results are comparable, although the coefficient on borrowing by females that was only marginally significant becomes statistically insignificant, for schooling gap.

We repeat our analysis dropping the small number of households that report using both formal and informal loans (Tables 4 and 5, column 6). This ensures that our results capture the effects of each loan type separately, rather than conflating the two. The estimates are very similar to our main results, with boys' schooling gap increasing in the presence of a male borrower, and the same for girls with female borrowers. In this case, girls do slightly worse, and they also fare worse with informal borrowing by men. The latter effect may point to credit constraints; households that can borrow both formally and informally tend to have better schooling outcomes than those that are restricted to only informal credit. In this restricted sample, formal loans are also found to increase school attendance for girls, though the effect is only marginally significant. This, again, may point to credit constraints; by excluding households that utilize both formal and informal credit, we can see that those households that are able to satisfy their demand for credit with formal borrowing alone are in a better position to make schooling investments.

Alternatively, instead of dropping households with both male and female borrowers, we keep them and reclassify them as households with male borrowers (Tables 4 and 5, column 7). For the schooling gap, the magnitudes of borrowing coefficients become smaller, especially for boys, and the coefficient on loans taken out by female borrowers becomes statistically insignificant at any conventional level (p-value = 0.107). This could be explained by the fact that the coefficient on borrowing subsumes some of the effect of borrowing by females, making it more difficult to detect statistical significance.

It is also possible that our results may be driven by differential attrition. For example, suppose children in nonborrowing households are more likely to leave or interrupt school but also more likely to leave the household at younger ages. This could generate a positive correlation between borrowing and schooling gap simply because those children with the largest schooling gaps are missing from our data. To check this, we repeat our analysis restricting the sample by age, focusing either on primary school-age children or lowering the age cut-off to 16. This does not substantively change our findings, suggesting that differential attrition across age groups is not driving our results (available upon request).

Finally, we reclassify our borrowing categories to give priority to informal credit, that is, households borrowing from both sources are assigned a value of one for informal loans and a value of zero for formal loans. Unsurprisingly, given the relatively small number of households that utilize both formal and informal loans, this specification too supports our main findings (available upon request).

6 | CONCLUSION

This paper explores the effects of borrowing on children's educational outcomes and focuses on the distinction between credit channels, as well as borrower's and child's gender. Distinguishing between formal and informal credit is important, given that both credit channels are used extensively by households in developing countries. Because they tend to differ in credit arrangements and levels of accessibility by different populations, they may also have different effects on

children's schooling. Similarly, given the historical existence of gender gaps in credit access and educational achievements, the focus on gender is important.

Our results indicate that children from nonborrowing households tend to have better schooling outcomes than children from borrowing households. Additionally, the effects of borrowing on child schooling are heterogeneous across borrower and child gender, as well as borrowing channels.

First, when children and borrowers are of the same gender, borrowing leads to worse schooling outcomes. One potential explanation for these findings is household demand for child labor. Households with female borrowers, on average, owned more businesses than male borrowers or nonborrowers, and households with male borrowers, on average, were more engaged in agricultural activities. These productive activities differentiated by gender may also require gender-specific child labor, where children labor complements or substitutes the adult labor. This supposition is supported by our evidence for girls with female borrowers, though we do not observe a similar effect for boys. However, this may be due to data limitations, given that we only observe child labor on the extensive margin and in income-generating activities. Boys are more likely to be engaged in such activities irrespective of household borrowing, making it more difficult to discern an effect on the extensive rather than intensive margin. It is possible that children in borrowing households must engage more in household chores/maintenance and/or childcare, and therefore accumulate less schooling.

Second, we are unable to provide conclusive evidence regarding the impact of the borrowing channel. Our point estimates are noisy, suggestive of heterogeneity particularly within the category of informal borrowing, but, taken at face value, our estimates suggest that, on average, informal loans lead to adverse effects on cumulative schooling similar to those found for formal credit. However, there is also some evidence that informal borrowing may improve school attendance, though the coefficients are only marginally significant. This result may suggest that the informal credit can protect against risks and consumption fluctuations, helping maintain continued school attendance. A more detailed analysis of the impact of informal credit markets is needed, and our results suggest that survey efforts should take greater care in recording the sources of informal credit as well as the terms of informal loans.

Overall, our findings suggest that while credit is important for the economic development of households, borrowing can also result in the unintended negative consequences for children's schooling, with potentially large inter-generational effects. Three policy implications emerge from this analysis. First, to offset the adverse effects of borrowing, credit expansion needs to be supplemented by other educational and social programs that contribute to improvement of schooling outcomes. Second, the design of credit programs needs to take into account overall household well-being, including the need for child labor. For example, supplementing financial services with social services, or changing parents' perceptions about schooling through informational sessions could positively influence children's schooling. While we find only marginal evidence of the positive effects of the informal credit on schooling, experimenting with different credit arrangements and conditionality can also help microfinance institutions design better credit products that align with both economic and social objectives of households. Finally, greater emphasis on deposits and insurance beyond credit provision would ensure that households have access to the broader range of financial tools.

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ENDNOTES

[1] Informal financial institutions may include relatives, neighbors, friends, employers, landlords, shopkeepers, moneylenders, self-help groups, ROSCAs, ASCAs, or nonfinancial associations of producers, women, or farmers. Formal financial institutions may include commercial, development, agricultural, or Grameen banks, savings and credit cooperatives, NGOs, or microfinance institutions.

[2] As an alternative measure, we also considered "highest grade completed." These results are almost identical to those for schooling gap (though, by construction, they are of the opposite sign). We do not report them, and they are available upon request.

[3] We choose not to use loan amounts, as the identifying assumption for the alternative specification is overly restrictive. Specifically, it assumes that the size of the loan—rather than just the dichotomous decision to borrow—is uncorrelated with unobserved time-varying shocks. Additionally, survey respondents are more likely to make errors in reporting loan amounts, which would increase the measurement error and contribute to biased estimates.

[4] We cluster at the municipality level rather than the household level, per Cameron and Miller (2015), who note that "The consensus is to be conservative and avoid bias and use bigger and more aggregate clusters when possible, up to and including the point at which there is concern about having too few clusters."

[5] The proportion of children from households with both formal and informal loans is 6.5 percent among all children from borrowing households. Households borrowing from both formal and informal sources are assigned a zero value for informal borrowing, as we assume that formal loans take priority.

[6] More detailed summary statistics with median, minimum, and maximum values do not point to extreme values of control variables (available upon request).

[7] When also adding loan amounts as regressors, the main findings for formal borrowing remain unchanged with regard to sign and significance, though the coefficient on the indicator for female borrower is no longer statistically significant. Additionally, the effect of informal credit amount becomes significant for boys' schooling when borrowers are males (available upon request).

[8] Instead of pooling households with male and female borrowers in the same regression model, we also conduct the analysis on two different samples: one including nonborrowing households and households with female borrowers only, and one including nonborrowing households and households with male borrowers only. The results remain unchanged (see Table B in the Online Appendix—for access, see Supporting Information at the end of this paper).

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SUPPORTING INFORMATION

Additional Supporting Information may be found online in the supporting information tab for this article.

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