NBER WORKING PAPER SERIES

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Working Paper 17165
http://www.nber.org/papers/w17165

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2011

Akresh acknowledges the collaboration between Yale University and l'Institut Supérieur des Sciences de la Population of the University of Ouagadougou that aided the data collection. The fieldwork was funded by the National Science Foundation (Grant No. 0082840), Social Science Research Council, J. William Fulbright Fellowship, National Security Education Program, Institute for the Study of World Politics, and Yale Center for International and Area Studies. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 17165
June 2011
JEL No. O15

ABSTRACT

We consider the influence of household-based production on human capital investment. In data from rural Burkina Faso, we document a positive correlation between the presence of girls and enrollment that disappears in households that are able to send out or receive in children. We argue that the connection between education and the sex composition of co-resident children in households that are constrained in their ability to adjust child labor owes to residential rivalry, the idea that having a greater share of resident children with an advantage in household based production increases education by reducing the within-household equilibrium value of child time.

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1. Introduction

Efforts to promote universal primary education have largely focused on school costs, school quality, and schooling access. However, within-family considerations about whether to enroll in school are also critical. The importance of how and why household and especially sibling composition influences investments in education has been emphasized in the literature in economics and related disciplines. In a classical model with complete markets, sibling composition could affect differences in the returns to education through learned behaviors (e.g. Butcher and Case 1994). Models that allow for heterogeneous preferences of multiple decision-makers generate sibling composition effects on education through agents' tastes for education (Moehling 2005). Liquidity constraints can lead to effects of sibling composition on education that operate through income (Manacorda 2004) or relative returns to investments (Garg and Morduch 1998). In this study, we emphasize the distinction between how sibling and co-resident children influence education. We argue that comparative advantage in household-based production is an important cause of differences in education among co-resident children, a cause that is sometimes misattributed to sibship rather than co-residency. We introduce the phrase "residential rivalry" for the idea that having a greater share of resident children with an advantage in household-based production increases education by reducing the within household equilibrium value of child time.

The concept that the availability of labor within the household affects the opportunity cost of time is in Becker (1965). Rosenzweig (1977) and related studies on the determinants of farm fertility implicitly emphasize its importance for investments in children. Children are extensively involved in work inside the household. In the UNICEF MICS3 survey project from 2005/06, 68 percent of children 6-13 helped with the family farm, business, or provided unpaid household services for the household where they live. Children are nine times more likely to participate in household-based economic activity compared to wage employment outside of the home.[1] With complete labor markets for all household-based tasks, the (shadow) value of child time inside the household depends on local labor markets. Even if there is no child labor market, the substitutability of child and adult time inside the family implies that local labor markets determine the opportunity cost of child time inside the household. Hence, the influence of co-

[1] Authors' calculation from the MICS3 microdata available at http://www.childinfo.org/

resident children on time allocation through within-household labor demand depends on the assumption that there are tasks within the household for which one cannot hire in labor.

Testing this hypothesis that co-residents influence education through the value of time in the household is difficult, as it is unobservable. We propose an indirect test where we compare the elasticity of education with respect to the composition of co-resident children in households that differ in their ability to send out or receive in children by virtue of their participation in child fostering exchanges. We measure composition by the sex ratio of co-resident children. In data from rural Bazega province in Burkina Faso, we find that education is influenced by the sex-composition of resident children in households that are constrained in their ability to send or receive labor and who do not participate in child fostering networks. We find no relationship between sex composition and education in unconstrained households. The differences in the response of education to sex composition are substantive between constrained and unconstrained households. In our most basic specification, the thought experiment of switching a resident child from a boy to a girl increases school enrollment by 6 percentage points or 26 percent of baseline mean enrollment in constrained (non-fostering) households and has no substantive effect (less than 1 percentage point) on schooling in unconstrained households.

There are three main contributions of our finding of residential rivalry due to constraints on the availability of child labor. First, this is novel evidence in support of the importance of the opportunity cost of time in household-based production for education. Many studies in the child labor literature argue that girls have both an absolute and comparative advantage in household-based work and document their more intensive involvement in household activities (Levison and Moe 1998; Glick and Sahn 2000; Edmonds 2007; Del Carpio and Macours 2010; Dammert 2010). However, the simultaneous nature of time allocation decisions has it made it difficult to establish a connection between this engagement in household-based production and education.

Second, our finding that education increases with the number of resident girls is similar to what is generally found in the sibling rivalry literature building from Garg and Morduch (1998) and Morduch (2000), but our results suggest an alternative mechanism to explain these sibling rivalry findings. The usual explanation for sibling rivalry is that rivalry is driven by credit constraints inducing parents to invest in human capital based on the sex composition of their offspring. Having more low return children (girls) reduces competition for scarce resources and raises investments in all children. Our data contain information on all biological siblings, regardless of their residency status. When we consider all siblings, we observe no correlation between sibling sex composition and education. We observe a correlation between the sex composition of young siblings and education, but young siblings are mostly co-resident. Moreover, the fact that sex composition only appears to matter for education in households constrained in their ability to access additional child labor implies that the data in the present case are more consistent with residential rivalry rather than sibling rivalry. Our findings are consistent with other studies such as Parish and Willis (1993), Thomas et al (2004), and Vogl (2010) that emphasize the important role adjustments in household composition play in key determinants of poverty and long-term well-being.

Third, our finding that the constraints that lead to residential rivalry are relaxed by fostering networks contributes to our understanding of the role played by the fostering institution in Africa. Families use fostering in a way that is analogous to how one might use labor markets to meet household labor demands, and our findings are consistent with an evolving shadow price of a child within the network. When families participate in fostering networks, the value of time depends on supply and demand within the network in the absence of the ability to hire in help. The larger the network, the weaker is the connection between household labor supply and demand and education. This endogenous shadow price for a child is useful in interpreting the existing literature on fostering. Studies such as Ainsworth (1996) and Grimard (2000) emphasize the importance of household labor demand needs in the decision to receive in children and other family members. While others such as Butcher (1993), Zimmerman (2003), and Akresh (2009) highlight sex imbalances within the household and income shocks as reasons for sending out children, especially girls. If movements in children equilibrate the shadow value of a child within a network, both supply and demand factors will be important determinants of fostering. Their relative importance will depend on both the distribution of child endowments and complementary inputs.

A challenge with the link between sex composition and education in families that differ in their fostering status is that the decision to foster is driven by factors that will be correlated with education. The endogeneity of fostering has been a major barrier to understand the consequences of fostering in Africa and part of our contribution to the literature on fostering is a solution to this problem. The cultural history of Bazega province provides an unusual opportunity to address the problem of endogeneity in our definition of having an available fostering network. For historical reasons that we discuss in more detail below, kinship lineage groups vary in their proclivity for fostering and households differ in the availability of extended family members outside their village to participate in these fostering exchanges. While separately, each of these characteristics may affect education, we assume that the interaction of a lineage group's measured proclivity for fostering and the availability of network members elsewhere has no direct correlation with education except through how this interaction influences child fostering. Hence, we use this interaction as our source of identification in our test of the hypothesis that co-residents influence education through the value of time in the household. Our findings suggest that households that are unable to participate in fostering have much lower education rates, especially when they are endowed with fewer girls.

The remainder of the paper is organized as follows. Section 2 illustrates residential rivalry within a model of household production. The household survey data from Burkina Faso used in the analysis are described in Section 3. Section 4 documents the appearance of residential rivalry and the relationship between that rivalry and child fostering. Section 5 concludes.

2. Sibling Endowment Effects and Residential Rivalry

In this section, we illustrate how siblings and co-resident children can affect time allocation in a farm household model. We consider two types of children (boys and girls), and our focus is on how the mix of child types affects education. In Section 2.1, we add siblings and education into a standard separable farm household model. Siblings create a sibling endowment effect. Households with an endowment consisting of the relatively more valuable type are wealthier. In Section 2.2, we introduce siblings and education into a non-separable farm household model. Non-separation comes from eliminating the ability to hire in labor. This generates residential rivalry. When households are endowed with relatively more effective labor (because of the greater productivity of one type of labor), the shadow value of labor in the household is lower, inducing more education in equilibrium. Our test of residential rivalry is then a test of the separation hypothesis extended to non-economic activity analogous to Benjamin (1992). Sections 2.1 and 2.2 take the endowment of child types as given. In Section 2.3, we consider how the ability to choose the type of child present affects educational choices. The ability to move children is analogous to the ability to hire in labor. Residential rivalry dissipates while sibling endowment effects persist.

2.1 Sibling Endowments

Sibling endowment effects arise from characteristics of the offspring of a common parent. Sibling rivalry, which comes from credit constraints and different market returns to human capital investment by gender, is one type of sibling endowment effect. In a simple model where we add children and education into a standard farm household model (Bardhan and Udry 1999), the sibling endowment effect arises because families are wealthier when they are endowed with more economically valuable children. Different market returns to education and liquidity constraints are not necessary to generate a sibling endowment effect.

To focus on children, we treat the labor supply of adults as inelastic with respect to choices over child time allocation. It is common to abstract from the adult labor supply problem in models of child labor (Basu and Van 1998, Baland and Robinson 2000). Each household has a single head who is a unitary decision-maker. The head is endowed with *N* offspring a fraction g of which are girls. Suppose that household preferences can be represented by a single utility function defined over the consumption, c, and education, e, of his children: $U(c_m, c_f, e_m, e_f)$, where m denotes boys and f girls.[2] The household has multiple children of each type (boy/girl). To focus on sex composition alone, we assume all children of a given type are treated the same.

Household income is generated by an exogenous income Y, home based production, and by working in the labor market. The exogenous income comes from rents to fixed assets, transfers, and income from inelastic adult labor supply. The household operates a farm or home enterprise with asset endowment *A* and using *L* units of child labor. The household production function $F(L; A)$exhibits positive diminishing marginal product of child labor. Child labor in household production consists of time from each of its own family members (superscript o) and from hired in labor (superscript h). Let L_i^o be the labor supplied to household production by each household member of type *i* where $i \in \{m, f\}$. L_i^h is total hired in labor of type $i \in \{m, f\}$. The household produced good can be transformed into a consumption good that has a relative price *p*. In addition to transforming the household produced good and using the exogenous income *Y*, the household can purchase consumption goods by having each child of gender *i* work L_i^W for hire in the labor market for wage w_i, $i \in \{m, f\}$.

The household's problem is:

$$\max_{c_m, c_f, e_m, e_f, L_m^h, L_f^h} U(c_m, c_f, e_m, e_f; g, N, A, Y) \qquad (1)$$

[2] We replace leisure in the standard model with education as that will be our emphasis rather than labor supply.

subject to

$$p\left((1-g)Nc_m + gNc_f\right) + w_m L_m^h + w_f L_f^h \leq F(L;A) + w_m((1-g)NL_m^W) + w_f gNL_f^W + Y \quad (2)$$

$$L = (1-g)NL_m^o + gNL_f^o + L_m^h + L_f^h \quad (3)$$

$$T_i = e_i + L_i^o + L_i^W, i \in \{m, f\} \quad (4)$$

$$c_i, e_{i,} L_i^o, L_i^W, L_i^h \geq 0, i \in \{m, f\} \quad (5)$$

T_i is the time endowment per child. In treating education as the residual claimant on child time outside of work, it should be thought of as encompassing classroom time, study time, and leisure.

Equation (3) is the resource constraint for labor use in household production. It presumes that boys and girls are perfect substitutes in production and that hired in labor is a perfect substitute for family labor. In our discussion, a central point is the extent to which boys and girls can substitute for one another in household production. Hence, it will be useful to introduce a productivity shifter a, with $a>0$, that reflects how many girls one unit of boy labor is equivalent to. We rewrite (3) in terms of girl equivalent labor:

(3') $L = a(1-g)NL_m^o + gNL_f^o + aL_m^h + L_f^h$.

Labor demand in the wage labor market, where children may work for a wage w_i, is the sum of demand for hired in labor in household production. Equilibrium implies $w_m = aw_f$. Suppressing the subscript on the girl's wage, we can rewrite the budget constraint (2) as:

$$p\left((1-g)Nc_m + gNc_f\right) + awL_m^h + wL_f^h \leq F(L;A) + aw((1-g)NL_m^W) + wgNL_f^W + Y \quad (2')$$

The sibling endowment effect is evident in the full-income constraint. Substituting (3') and (4) into (2') gives the full income constraint:

$$p\left((1-g)Nc_m + gNc_f\right) + wN\left((1-g)ae_m + ge_f\right) \leq \pi + wN\left((1-g)aT_m + gT_f\right) + Y \quad (6)$$

$$\pi \equiv F(L;A) - wL \quad (7)$$

The value of the endowment of children is $wN\left((1-g)aT_m + gT_f\right)$ and will therefore affect all household decisions. Hence, the sex composition of siblings will affect education so long as genders differ in their productivity.

To the extent education is positively correlated with income, we expect more education in families with more valuable sibling endowments. $0<a<1$ implies that families endowed with more girls are wealthier. Therefore, we would see "sibling rivalry" type results (Garg and Morduch 1998, Morduch 2000) in the absence of any market imperfections simply because of the positive correlation between human capital investments and wealth.

Are girls more productive or valuable workers than boys? This modeling assumption is closely related to the issues raised in Pitt, Rosenzweig, and Hassan (2010). They argue that men have comparative advantage in strength oriented tasks compared to women. Child tasks are unlikely to reward "brawn" and hence boys are unlikely to possess the types of advantages we usually assume men have in unskilled labor. Moreover, repetitive tasks require attention and focus, and in many settings girls may be more compliant and amenable to a wider variety of tasks. Gender stereo-typing of tasks like child care, water and wood collection activities, and cooking might also make boys less productive in household production as they would be less willing to engage in such gendered tasks. Of course, most models imply that families with more boys are wealthier because of expected future transfers from higher average returns to education, and we have abstracted from this by leaving education as a matter of preference. However, in the current setting where the returns to education for a male living in rural Burkina Faso are as uncertain as are a head's ability to capture those returns and where families pay bride prices at the time of marriage, it is believable that the sibling endowment effect is such that heads are wealthier if they are endowed with more girls.

2.2 Residential Rivalry

Residential rivalry arises, because families must consider the relative productivity of their children in household-based production. To focus on this aspect of sibling interactions, we make several simplifications to our set-up. We assume parents provide the same consumption to all children and parents receive the same utility from providing consumption to boys as girls. Denote c as total consumption (divided equally among all children) and the head's utility function as $U(c, e_m, e_f)$. Rivalry can arise from differences in returns to education (more precisely, differences in the utility parents receive from educating boys and girls). To focus on production, we assume boys and girls have the same return function, $r_f(\epsilon) = r_m(\epsilon) \equiv r(\epsilon)$ for any level of education ϵ, and that utility is additively separable in each of its arguments. Thus, we write the head's utility function as:

(8) $U(c, e_m, e_f) \equiv u(c) + N\left(g * r(e_f) + (1-g) * r(e_m)\right)$

We consider the case of no external labor market so that the head can neither hire in child labor nor sell child labor to the market, meaning $L_m^h = L_f^h = L_m^W = L_f^W = 0$. We normalize prices to 1 and the time endowment for each type of child is 1 (T=1). Assuming non-satiation, the budget constraint (2) reduces to $c = F(L; A) + Y$ and, in effect, we have a home production model where goods brought in by inelastic adult labor supply are perfect substitutes for those produced by children. Time constraints (4) become $1 = e_i + L_i^o, i \in \{m, f\}$. Labor used in home production is $L = a(1-g)NL_m^o + gNL_f^o$. If we plug in the time constraints, the head's problem becomes a simple problem of choosing education for each gender. Rewriting $u(c) = u(F(L; A) + Y) \equiv v(L)$, we have the household's problem as:

(9)
$$\max_{e_m, e_f} v\left(N * (g * (1-e_f) + (1-g) * a * (1-e_m))\right) + N\left(g * r(e_f) + (1-g) * r(e_m)\right)$$

Assuming interior solutions,[3] the educations of boys and girls are chosen to equalize returns to education with the value of labor's marginal production: $r'_m = av'$ and $r'_f = v'$.[4] In the model of Section 2.1, this is also true, but in that model the value of labor's marginal product in home production is market determined. In the present case, the value of labor's marginal product depends on the endowment of girls (the presence of more girls raises the amount of effective labor present). The greater the amount of effective labor engaged in production, the lower the opportunity cost of education. A lower opportunity cost of education induces more education. Hence, there is both a sibling endowment effect and a change in relative prices that we refer to as a residential rivalry effect.

To make the residential rivalry result more salient, we introduce several functional form assumptions. We assume positive, diminishing returns to education for each type of child:

(10) $r_f'(e) = r_m'(e) = \alpha_0 + \alpha_1 * e \; \forall e \in (0,1)$

[3] Conceptually in the model, education e is time outside of work. It would include classroom time, study time, and leisure time.

[4] By assumption, boys and girls have the same returns functions, but with the same return functions boys and girls will have different levels of education because of their productivity differences in household production. We add the gender subscripts to the partial derivatives to emphasize the difference in returns by gender.

with $\alpha_0 > 0$, $\alpha_1 < 0$, and $|\alpha_1| < |\alpha_0|$. The first two assumptions imply positive diminishing marginal returns to education and the latter ensures there is a positive return to education even when *e=1*. We assume positive, diminishing returns to labor in household production as well:

(11) $\quad v'(L) = \beta_0 + \beta_1 * L$

with $\beta_0 > 0$, $\beta_1 < 0$ and $|\beta_1| < |\beta_0|$. As with education, these assumptions on parameters imply positive, diminishing marginal returns for all L.

Given these assumptions, we can explicitly solve for the amount of education received by both boys and girls. We focus on girls but the intuition is similar for boys. Plugging in the functional form assumptions to the first order conditions for the household's problem and rearranging, we can solve for the choice of education for each girl:

(12)
$$e_f = \frac{\beta_0 - \alpha_0 + \beta_1 Ng + \beta_1 aN(1-g) + \beta_1 a(1-a)\frac{\alpha_0}{\alpha_1}N(1-g)}{\alpha_1 + \beta_1 Ng + a^2\beta_1 N(1-g)}$$

To see residential rivalry, we differentiate equation (12) with respect to *g*:

(13)
$$\frac{\partial e_f}{\partial g} = \left(\frac{\beta_1}{\frac{\alpha_1}{N} + \beta_1 g + a^2\beta_1(1-g)}\right)\left((1-a)\left(1 - a\frac{\alpha_0}{\alpha_1}\right) - e_f(1-a^2)\right) > 0$$

Education for girls increases with the fraction of girls in the residence because of the assumption of diminishing marginal returns to education and labor.[5] The impact of an increase in g is declining in the amount of education chosen for girls and also in the number of children present in the household. More children imply more labor and therefore less of a marginal effect of the additional labor a girl brings to the household. All of these terms are similarly signed for boys although the magnitude of the change in education for boys with the addition of girls will be smaller, because boys receive more education.

2.3 Residential Rivalry with Endogenous Household Composition

If households are free to choose both the number of children present *N* and the type of children present *g*, then residential rivalry dissipates. In the set-up above, if households are unconstrained in their ability to bring in new members, the gender mix will depend on the shape of the

[5] Diminishing marginal returns implies $\beta_1 < 0$ and $\alpha_1 < 0$. Thus, the term in the first parentheses, $\left(\frac{\beta_1}{\frac{\alpha_1}{N}+\beta_1 g+a^2\beta_1(1-g)}\right)$, is positive. The second term can be rewritten as $L_f^o + e_f a^2 + a^2\frac{\alpha_0}{\alpha_1} - a\frac{\alpha_0}{\alpha_1} - a$ or $L_f^o - a\frac{\alpha_0}{\alpha_1}\left(\frac{\alpha_1}{\alpha_0} + 1 - a - ae_f\frac{\alpha_1}{\alpha_0}\right)$. This is positive given the assumptions that a and e_f are less than one and that α_1 is less than α_0 in magnitude.

production function, a, and the shape of the return function, r. Because households would choose to add as many children as possible, we need to add a cost of adding members to limit this.

When children move between households in Burkina Faso, it is most likely within an extended-family network. This implies that there will be a finite supply of male and female children. The shadow price of boys and girls will be determined as a result of supply and demand within the network. We represent the equilibrium shadow price of a boy as θ_m and a girl as θ_f. Suppose the price of girls and boy are paid out of consumption. The budget constraint becomes:

(14) $$c = F(L; A) + y - N(1-g)\theta_m - Ng\theta_f$$

These shadow prices are equilibrium outcomes within the family network. Hence, they implicitly incorporate the constraint on the availability of each type of child within the network.

Suppose that a household head receives utility from educating children that are present regardless of whether they are his direct genetic offspring or children fostered in from his extended family network. Heads choose to add boys and girls until their net return (return to education plus return to labor) is equal to their shadow price in the network. Central to the residential rivalry result is that labor productivity in the household is an endogenous function of the prevalence of girls. When we add in a price to having boys and girls resident, we return to a situation analogous to that of Section 2.1 where labor markets and endowments determine the education of girls and boys. In the present case, the value of boys and girls in the fostering network plays a role analogous to the labor market in Section 2.1. Sibling endowment effects persist, but the productivity of children in the household no longer depends on the household's endowment alone. Of course, fostering networks are not infinitely large, so the endowment of sexes in a household may be correlated with the shadow value of both types of children in the network. For this reason, we emphasize that mobility of children and therefore child labor within a fostering network attenuates residential rivalry rather than completely eliminates it.

3. Burkina Faso Child Fostering Survey (BCFS)

This study focuses on the educational status of children ages 6 to 13 in the Burkina Faso Child Fostering Survey (BCFS). We focus on children 6-13 as those are the key primary school ages in Burkina Faso, and child labor laws in Burkina Faso distinguish 14 year olds (who can legally work in most types of work) from 13 year olds (who cannot). BCFS consists of interviews with 606 randomly selected households in 15 randomly selected villages in Bazega province in central Burkina Faso. The survey data were collected by Akresh in 2001 and have been used in studies on the determinants of child fostering (Akresh 2009) and the impacts of fostering on schooling (Akresh 2008). Bazega province is located approximately 50 miles from the Burkina Faso capital, Ouagadougou. Households in this region are predominantly subsistence farmers growing sorghum and groundnuts and have an average annual household income of $158.

There are several unique aspects of the BCFS data that are important in our analysis. First, we can distinguish between biological siblings and other residents. For every household, all survey questions in the household roster were asked about every biological child of the household head, regardless of whether the child was currently resident at the time of the survey. In addition, for every household, all survey questions in the household roster were asked for any individual who had lived within the sampled household for at least four months at any point during the three years prior to the survey. These survey questions include the age, gender, educational attainment, current school enrollment status, and the number of months the individual lived outside of the village during the three years prior to the survey (1998, 1999, and 2000). These two unique aspects of expanding the targets for the household roster (all biological children and anyone resident during the three years prior to the survey) differ from traditional, multipurpose household surveys in developing countries. In our case, they allow us to measure the differential impacts of *sibling* sex composition and *residential* sex composition. In contrast, most studies in this literature are limited to studying the sex composition only of siblings who are resident at the time of the survey.

Second, the BCFS collects detailed information about every household's kinship lineage group. This data is necessary for our identification strategy. Almost all households in the survey are of the Mossi ethnicity, which is the largest ethnic group in Burkina Faso with about 40 percent of the population. Starting around the 15th century, the Mossi were able to conquer large amounts of territory due to their extensive cavalry, and they created a prosperous empire that lasted until the colonial period (Tauxier 1917; Skinner 1962). Mossi society is divided into two main kinship lineage groups. The first are the Nakomse, translated as "people of power", who are descendants of the cavalry that conquered the other inhabitants of the Mossi region (Hammond 1959). Approximately 62 percent of the households in our sample are of this lineage group. The second group is the descendants of the farmers who had originally occupied and owned the lands. This second lineage group can be further sub-divided into blacksmiths, traders, and farmers (Skinner 1964). Historically, the Nakomse were more likely to foster, and these differences persist through to the time of the BCFS. 38.6 percent of Nakomse households foster children compared to 35.9 percent for the non-noble kin. While household surveys might collect information on an individual's ethnic group, no other surveys in Burkina Faso have this detailed information on an individual's lineage, information that is critical to our estimation strategy.

Third, the BCFS collects detailed information on occupation, marital status, education, demographic characteristics, and location of residence for every individual in the household head's immediate family network, which is defined to include his parents, brothers, sisters, and adult children. Our identification comes from combining the information on lineage with this information on the extended family living outside the head's village to gauge the availability of network members that might participate in a fostering exchange.

Table 1 illustrates how the additional data collection in the BCFS changes our understanding of a child's environment. The columns in the table present, for different sibling and residence definitions, the average number of siblings and the fraction of those siblings that are girls.[6] For this project, we define residency as being present for at least four out of twelve months in a year, although for those children that were present, 91 percent of them were resident for all 12 months.[7]

Each panel in the table is restricted to different groups of children age 6 to 13 to highlight how sibling sex composition differs by residence and biological relationship to the household head. The top panel (A) focuses on children 6-13 of the household head. The middle panel (B) restricts further to the resident children 6-13 of the household head. 90 percent of children 6-13 of the household head are resident. Boys are more likely to be resident than girls. Overall, the sibling setting for children of the household head and for resident children looks similar. This should be expected as resident and non-resident children have the same siblings. We do not observe the new residency environment for non-resident children of the household head, so columns 6 and 7 of panel A are missing.

Most of our discussion will focus on resident children 6-13 of the household head, panel B. These children average more than eight siblings and slightly more than half are girls. The

[6] Columns 2 and 3 include all living siblings, regardless of sibling residence or sibling age. Columns 4 and 5 restrict calculations to only living siblings under age 18, regardless of sibling residence status. Finally, Columns 6 and 7 restrict calculations to only co-resident children under age 18, regardless of their biological relationship to the child.
[7] This residency definition was adopted to synchronize with the child fostering residency definition used in the fieldwork and developed in consultation with local partners.

fraction female will mechanically be higher for boys than girls.[8] Seventy percent of the siblings of children 6-13 are under 18. A comparison of siblings under 18 (columns 4 and 5) compared to residents under 18 (columns 6 and 7) is informative for our analysis below. Much of the difference between these two groupings owes to fostering. Child marriage is rare, and children do not typically leave home for good until they are older. On average the amount of inflow of children exceeds outflow so that the number of co-resident children under 18 is larger than the number of siblings under 18. This net inflow appears weighted towards girls as the fraction female is closer to parity when considering co-residency rather than siblings.

The bottom panel C of Table 1 focuses on all resident children 6-13. Our detailed sibling measures are only available relative to the household head, so we do not have sibling information for resident children who are not children of the household head. The difference between panel B and C is driven by households with resident children who are not children of the household head. 32 percent of resident children are not children of the household head. These resident children 6-13 that are not children of the household head are more likely to be female than male. Interestingly, they tend to be living in houses with fewer children present and a more equal gender balance. This is important, because we will find that it is the movement of girls in particular that seem to relax the constraint that produces residential rivalry.

4. Residential Rivalry in Burkina Faso

4.1 Sibling Endowment Effects and Residential Rivalry

We follow the empirical approach of sibling rivalry studies in our analysis.[9] Studies of the impact of sibling endowments on human capital typically use counts of the number of siblings and the number of sisters that a child has to explain different schooling outcomes as follows:

(15)
$$e_{ih} = \kappa + \omega_0 S_{ih} + \omega_1 F_{ih} + \alpha_0 X_{ih} + \alpha_1 Z_h + \varepsilon_{ih}$$

where e_{ih} is the educational outcome for child i in household h, S_{ih} is a count of the number of siblings the child has, F_{ih} is a count of the number of female siblings the child has, X_{ih} is a vector of individual characteristics such as age and gender that might influence parental investments, Z_h is a vector of household characteristics, and ε_{ih} is a random, idiosyncratic error term. The interpretation of ω_0 is the change in e_{ih} associated with an additional male sibling. The

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[8] If the typical head has 9.3 children, 5 of whom girls, when we condition on being a boy, that boy will have 8.3 siblings, 5 of whom are girls. When we condition on being a girl, she will have 8.3 siblings, 4 of whom are girls.

[9] The final, published Garg and Morduch paper only reports anthropometric outcomes, but earlier versions and Morduch (2000) as well as those studies cited in the introduction also look at education.

interpretation of ω_1 is the change in e_{ih} associated with the thought experiment of converting a sibling from a male to a female. $\omega_0 + \omega_1$ is then the change in e_{ih} associated with adding an additional female sibling. The standard approach in the literature takes current family size and composition as given at the time of the enrollment decision. In our initial regressions exploring sibling endowments we maintain this assumption, but in later estimations focused on residential rivalry, we are able to explicitly relax it.

We find little evidence of substantive sibling endowment effects. This finding is in columns 1 and 2 of Table 2 where we report estimates of ω_0 and ω_1 from eq. (15). These regressions examine the relationship between schooling, the number of siblings, and the number of female siblings, using data on all siblings, regardless of sibling age or residence.[10] There is no additional schooling associated with adding a female sibling. The thought experiment of converting a male sibling to a female is associated with elevated schooling, but the coefficient is small in magnitude and not statistically significant. Twenty-four percent of children 6-13 are enrolled in school in our data. Column 1 includes children of the household head 6-13 regardless of their residence status. Column 2 restricts the sample to children 6-13 of the household head who are present in the head's house in the year preceding the survey. We observe similar findings in column 2 as with column 1.

While standard theory on sibling endowments (including sibling rivalry) argues that the biological relationship is the critical one, most previous research on this issue is unable, due to data limitations, to examine this relationship for all biological siblings. For this reason, the data used in columns 1 and 2 differ from typical sibling rivalry regressions. In columns 3 and 4 of Table 2, we limit the sibling counts to siblings aged 0-18, again regardless of residency status. Additional male siblings lower the likelihood of school enrollment. The number of biological sisters under age 18 is correlated with a higher likelihood of school enrollment.

The challenge in interpreting columns 3 and 4 is that when we restrict our measures of siblings to children under 18, we are also restricting our attention largely to co-resident children. Roughly 90 percent of these under age 18 siblings are also co-resident. Hence, columns 3 and 4

[10] All regressions in the paper include village fixed effects, child age and gender dummies, and controls for household head's gender, education, marital status, and age. In Table 2, Columns 2 and 4 restrict the estimation sample to only children of the household head ages 6 to 13 who are present in 2000, while Columns 1 and 3 do not include this additional residency restriction.

could imply sibling endowment effects that only occur among siblings that are close in age or it could be evidence of residential rivalry as sibling age and cohabitation are strongly correlated.

We find clearer evidence in favor of residential rivalry in Table 3. However, we first show in column 1 a typical sibling rivalry regression for datasets with limited information. The counts of the number of siblings and number of female siblings are restricted to biological siblings under age 18 who are resident. In most datasets, this is the only information available about siblings and siblings are only identifiable based on their relationship to the household head. The results in column 1 are consistent with Column 4 in Table 2 showing a sibling rivalry effect of 4.0 percentage points or 17 percent of the base enrollment.

In Column 2, we relax the restriction that all children and relationships must be defined relative to the household head. Of the additional 269 children 6-13 added to the column 2 sample, 214 are fostered children, with neither the mother nor father present in the household. For the other 55 children, their mother, but not their biological father, is present in the household. We observe similar evidence of gender rivalry regardless of whether we focus on children of the household head or all children as our units of observation and regardless of whether we focus on resident female siblings or resident females. Magnitudes in column 2 of Table 3 are slightly smaller than column 1. However, 80 percent of the additional children brought into column 2, compared to column 1, are in households participating in fostering exchanges where there is no evidence of residential rivalry as we discuss below. Column 3 again restricts the sample to children of the household head, as in column 1. We find a similar pattern of results when we include all resident children as we found when examining only biologically related, resident children in column 1. Because 68 percent of all residents are siblings, we cannot conclude that column 3 implies that the column 1 results are driven by residential rivalry rather than sibling rivalry, but it highlights how we observe similar schooling patterns regardless of whether we consider siblings or all children present. We fail to reject the null that additional resident females have the same effect on schooling as do additional resident female siblings (t-statistic of 0.34).

Our results so far are consistent with residential rivalry and a modified sibling endowment effect. Most theories that imply sibling endowment effects (such as models of sibling rivalry or the model in section 2.1) do not posit that such effects should only influence education for children who are close in age. In fact, most evidence of how siblings support each other relies on older offspring supporting younger offspring (Parish and Willis 1993, Vogl 2010).

Nevertheless, one could modify models of sibling endowment effects to posit that they are only salient for children close in age. Hence, testing between residential rivalry and sibling endowment effects requires comparing households that differ in whether they are constrained to use endowment labor for household production.

4.2 Child Fostering and Rivalry

In distinguishing between how biological siblings and resident children influence education differently, we must address the issue of how these non-biological siblings became part of the household. Since the formal hiring of child labor is non-existent in this region, child fostering is the principal way children move between residences, and it is quite prevalent, both in Burkina Faso as well as the rest of Africa.[11]

In Table 4, we examine how residential rivalry differs across foster and non-foster households. We find fairly similar effects of adding male siblings for children of the household head in both foster and non-foster households. However, residential rivalry appears to only exist in households that have not recently participated in fostering exchanges. For households that have recently sent or received a child (during the three years prior to the survey), there is no evidence of residential rivalry, as we find a weak and statistically insignificant relationship between the number of female resident children and schooling. This contrasts with those households that have not recently been involved in a child fostering exchange, as there is a strong positive relationship between the number of female resident children under age 18 and school enrollment. Converting a resident male child to a resident female child is correlated with a 6 percentage point higher likelihood of enrollment, or 27 percent of the base enrollment for non-foster households. Results are similar if the regression sample is restricted to only children of the household head (Column 4). For households that are constrained in their ability to easily obtain child labor (non-fostering households), the relative productivity of resident children will then impact time allocation decisions and school enrollment.

4.3 Child Fostering and Rivalry with Endogenous Fostering and Household Composition

A household's decision to send or receive a child is not random and could be related to observable or unobservable factors that also influence the educational enrollment decision.

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[11] Of the 606 surveyed households, during the three years prior to the survey 37 percent of these households had fostered a child (17.8 percent sent out a child, 14.9 percent received in a child, and 4.3 percent had both sent and received a child). This rate of child fostering is consistent with Demographic and Health survey data from other African countries (Vandermeersch 1997).

Although studies of fostering typically compute counterfactuals for fostered children by examining non-fostered children, there are ample reasons why fostering households may differ in ways that are difficult to observe and are correlated with educational outcomes.

We instrument for a household's foster status with the interaction of the household head's kinship lineage group's proclivity for fostering and the availability of extended family members outside the village with which to foster.[12] Different lineage groups have distinct proclivities to foster, yet this would not be a suitable instrument for fostering as it fails to satisfy the exclusion restriction. Lineage groups may differ in preferences for education or have substantially different endowments that could directly influence enrollment. Similarly, the number of extended family members living outside of the respondent's village will influence the availability of fostering opportunities, but these family members may provide information, resources, and other opportunities that directly correlate with schooling.

In our empirical work, we control directly for kinship lineage group and for the number of extended family members living outside the village and use the interaction as our instrument for fostering. Specifically, let N_h denote an indicator that is 1 if a household does not participate in a fostering relationship. $Nakomse_h$ is a dummy that a household belongs to the Nakomse kinship group. R_k is the fostering rate for the household's kin group k. M_h is the number of extended family members for the household that live outside the village (we include the total number of extended family members as a household level control in the vector Z_h). The pseudo first stage is a regression of N_h on all controls included in equation (15) as more fully detailed in the notes to Table 2 (and including village fixed effects):

(16)
$$\begin{aligned} N_{ihk} &= \pi_0 + \pi_1 M_h + \pi_2 Nakomse_h + \pi_3 M_h * R_k \\ &+ \pi_4 S_{ih} + \pi_5 F_{ih} + \pi_6 X_i + \pi_7 Z_h + \upsilon_{ihk} \end{aligned}$$

Conditional on the kinship group, there is no variation in the clan fostering rate so inclusion of $Nakomse_h$ also controls for R_k. Denote $\tilde{N}_{ihk}$ as the predicted values from (16). We use $\tilde{N}_{ihk}$, $\tilde{N}_{ihk} * S_{ih}$, and $\tilde{N}_{ihk} * F_{ih}$ as instruments in estimating equation (15) modified to allow there to be

[12] Our definition of a fostering household throughout this paper is that the household has sent or received a child during the three years prior to the survey. Results are qualitatively similar and sometimes more significant if we define a fostering household as one who has ever sent a child out or received a child in. We choose the 3 year window as our preferred definition as we are trying to capture more recent child availability.

different residential rivalry effects by the household's fostering status. Specifically, we modify equation (15) as:

(17) $$e_{ihk} = \kappa + \omega_0 S_{ih} + \omega_1 F_{ih} + \omega_2 N_{ihk} + \omega_3 S_{ih} * N_{ihk} + \omega_4 F_{ih} * N_{ihk} + \Psi_{ihk} + \varepsilon_{ihk}$$

with $\Psi_{ihk} \equiv \alpha_0 X_{ih} + \alpha_1 Z_h + \pi_1 M_h + \pi_2 Nakomse_h$. We treat $N_{ihk}, S_{ih} * N_{ihk}, F_{ih} * N_{ihk}$ as endogenous.

Our idea is that an individual's lineage group's proclivity to foster will interact with the availability of fostering opportunities to determine whether fostering exchanges occur without separately affecting schooling. Because fostering is more prevalent with more extended family members, we expect the interaction of the clan group effect and the number of extended family members to increase the chance the household does not participate in fostering as the saliency of the clan influence becomes less important when more extended family members are available. Thus, the instrument should positively predict non-fostering status. Columns 1 and 2 in Appendix Table 1 contain the pseudo-first stage estimates of the determinants of non-fostering status using our instrument. The lineage group's fostering rate is estimated using the fostering rates of the other lineage group members (excluding the household's own observation). Nakomse are more likely to foster. Additional extended family members outside of the village increases fostering participation. The interaction of the two, our instrument, increases the likelihood that a household does not foster. Put another way, being Nakomse attenuates the extended family member importance. Having extended family members attenuates the Nakomse effect.

Table 5 results show that the impact of females on education is entirely in households who do not foster, even after we instrument for non-fostering status as described above. In column 1, we include all children 6-13 present in the household. Column 2 is restricted to children of the household head. The evidence for residential rivalry is similar for both samples. In column 1, the thought experiment of changing a resident boy to a girl in a fostering household is a slight reduction in education that is small and insignificant. For a non-fostering household, this thought experiment of changing a resident child from a boy to a girl raises school enrollment by 8 percentage points. This is a large effect, a third of the average enrollment rate in our sample.

Columns 1 and 2 use clan-wide fostering rates in the construction of instruments. It is reasonable to expect there could be heterogeneity in kinship group behavior across geographic space in our study area. Columns 3 and 4 contain results where we instrument for non-foster status with village-level kinship group fostering rates (again, omitting the household's own observation) rather than using the fostering rate for the kinship group as a whole. The pseudo-first stages are in columns 3 and 4 of Appendix Table 1. The advantage of using this village * kinship group level fostering rate is that there is more variation in the instrument. The disadvantage is that it is more apt to capture village characteristics that happen to be correlated with kinship group. The second stage results of estimating (17) with these instruments (columns 3 and 4 in Table 5) are slightly larger in magnitude but are qualitatively similar to what we observed in columns 1 and 2 with the kinship group-wide variation.

The data do not reject the null that residential rivalry in non-fostering households is equally salient for boys and girls. However, the magnitudes of the effects of residential rivalry in non-fostering households are larger for girls. These results are in Table 6. Columns 1 to 4 contain results for boys 6-13. Columns 5 to 8 contain results for girls 6-13. The organization of regressions within each gender is identical to Table 5. Columns 1 and 2 for boys (5 and 6 for girls) instrument for non-fostering using the kinship group-wide variation in fostering rates. Columns 3 and 4 (7 and 8 for girls) use the village-level clan fostering rates. The magnitudes are consistent with the hypothesis that girls benefit more from having more girls in non-fostering households, but the data cannot reject equality. The sex composition of resident children seems unrelated to schooling for both boys and girls in fostering households.

These findings of residential rivalry in Tables 5 and 6 attempt to address the endogeneity of fostering, but they do not address the fact that the number of children is also a choice. This deficiency is not unique to our study. Most of the literature on sibling interactions typically treats number of siblings and their gender as effectively randomly assigned, and our attempt to address the endogeneity of fostering is unprecedented in the fostering literature.

We make use of the fact that the number of siblings of the household head will affect the head's desired family size, but will not be determined by contemporaneous economic circumstances. Thus, we use the number of siblings of the household head as an instrument for the number of resident children. We implement the instrument in an analogous way to how we instrumented for fostering status, while also instrumenting for fostering status. Appendix Table 2 contains the pseudo-first stages for both foster status and the number of resident children. The number of siblings of the household head predicts the number of resident children but not foster status (we always condition on the number of extended family members outside the village). We use the predicted values of the pseudo-first stages to construct instruments for non-foster status, number of children resident, and all of the associated interactions exactly as we did for non-foster status alone in equation (17). Second stage results are in Table 7. Columns 1 and 2 use the kinship group level variation as in columns 1 and 2 of Table 5. Columns 3 and 4 use the village * kinship group variation as in columns 3 and 4 of Table 5.

The attempt to address the endogeneity of the number of resident children (still treating their sex composition as exogenous) has little substantive effect on our estimates for fostering households. This is unsurprising as our theory posits little link between education and child presence in unconstrained households.

It is non-fostering households where endogenous fertility decisions especially could be most relevant as these households are constrained to make education decisions based on shadow wages for labor on hand. Expected shadow values of time and returns to education could then feedback into fertility decisions. When we instrument for number of children present, in non-foster households we observe larger negative effects of boys on education and larger positive effects of girls on education compared to instrumenting for fostering status alone (comparing Tables 5 and 7). This difference between findings with endogenous child counts (Table 5) and exogenous IV estimates (Table 7) is consistent with endogenous decisions about fertility and household composition that serve to protect education and attenuate rivalry. Constrained households in part choose their fertility aware of latent labor demand and potential educational decisions. The instrument is working off a different dimension. It is leveraging the size of the head's family growing up, which we think will inform his expectations about what size family he would like to have. That is, the instrument is working off the non-economic dimension of the choice of number of children. There are apt to be economic costs of this choice, and the cost of that non-economic choice is what the larger estimates in Table 7 reflect.

Of course, there are reasons to be concerned with treating the size of the head's sibling cohort as exogenous to education decisions. Latent economic factors persist over time, so the same factors that determined the head's parents' fertility decisions may affect his. Siblings provide information about labor market opportunities and returns to education. We assume this informational value of siblings is already captured by controls for the number of extended family members and the number outside the village. Siblings might also offer insurance or financial support. Again, we rely on controlling for extended family members to capture this. Another way to articulate our assumption is that extended family members influence education in all the same ways as siblings might, except the head's siblings also inform about latent tastes for children.

5. Conclusion

This paper examines how rivalry among biological siblings and co-resident children affects educational investments for households in rural Burkina Faso. We develop a theoretical model that emphasizes household production and constraints on the availability of child labor to highlight how biological siblings influence educational investments differently from other resident children. A unique household survey conducted by one of the authors allows us to measure the educational enrollment status of all biological children of the household head, rather than just those who are resident, and this enables us to examine the differential impacts of sibling sex composition and residential sex composition on schooling investments. We find little evidence to support what is generally referred to in the literature as sibling rivalry, but residential rivalry, which is the idea that having a greater share of resident children with an advantage in household-based production increases education by reducing the within household equilibrium value of child time, appears far more relevant. School enrollment is higher in households where a larger fraction of the resident children are girls.

As the formal hiring of child labor is rare in this region, child fostering substitutes for the hiring in of labor and is the dominant way children move between households. For those households that have recently participated in child fostering exchanges, there is no empirical evidence of residential rivalry. Consistent with our model, in the non-fostering households that are unable to obtain child labor, the relative productivity of resident children will then impact time allocation decisions and subsequently school enrollment. The key to distinguishing the residential rivalry explanation from other explanations for sex composition effects on education that work through siblings is our ability to differentiate siblings from co-residents and our ability to compare households that differ in whether they can access child labor through fostering.

Given that a household's decision to foster a child is correlated with factors that influence education, we instrument for the household's foster status using the interaction of a kinship lineage group's measured proclivity for fostering and the availability of extended family network members living outside the village. We only observe residential rivalry in households that are constrained in their ability to satisfy child labor demands and do not participate in fostering relationships. In our preferred instrumental variables specification, the thought experiment of changing a resident child from a boy to a girl raises the likelihood of school enrollment for all children by 17 percentage points, 70 percent of average enrollment.

Much of education policy in poor countries is focused on improving the schooling of girls, which generally lags far behind that of boys. By highlighting the distinction between sibling and residential rivalry, we are able to provide an explanation for the higher enrollment rates of boys that is not based on parent preferences or higher male returns to education but is focused on household production and the availability of child labor. This distinction is important because general economic development or affirmative action programs aimed at persuading parents to enroll girls might be less effective than investments that allow for substituting girl's time in home production. Furthermore, targeting those isolated households without extensive extended family networks will have a larger impact because children in those constrained households are most at risk of reduced human capital investments due to residential rivalry.

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Table 1: Sibling and Residence Composition of Children Ages 6-13

	Number of Sampled Children	Siblings		Siblings Under 18		Co-resident Children Under 18	
		Average Number	Fraction Female	Average Number	Fraction Female	Average Number	Fraction Female
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Children 6-13 of the Household Head							
Males	449	8.209	0.547	5.601	0.538	n/a	n/a
Females	492	8.152	0.531	5.799	0.514	n/a	n/a
Total	941	8.180	0.538	5.705	0.525	n/a	n/a
Panel B: Resident Children 6-13 of the Household Head							
Males	417	8.297	0.547	5.643	0.538	5.847	0.531
Females	426	8.204	0.534	5.883	0.519	5.962	0.509
Total	843	8.250	0.540	5.764	0.528	5.905	0.520
Panel C: All Resident Children 6-13							
Males	546	n/a	n/a	n/a	n/a	5.740	0.509
Females	566	n/a	n/a	n/a	n/a	5.631	0.505
Total	1112	n/a	n/a	n/a	n/a	5.684	0.507

Notes: N/A indicates not applicable, as full details on siblings were only collected for children of the household head. Columns 2 and 3 restrict the calculations to counts of all siblings of the child, regardless of residence status or sibling age. Columns 4 and 5 restrict the calculations to counts of all siblings under age 18, regardless of residence status. Columns 6 and 7 restrict the calculations to all co-resident children under age 18, regardless of biological relationship to the child. Data source: Burkina Child Fostering Survey (BCFS).

Table 2: Sibling Rivalry Regressions, Children Ages 6-13

Dependent Variable: School Enrollment	Child of Household Head, Ages 6-13	Child of Household Head, Ages 6-13, Child Present in 2000	Child of Household Head, Ages 6-13	Child of Household Head, Ages 6-13, Child Present in 2000
	(1)	(2)	(3)	(4)
Number of Female Biological Siblings, All Ages	0.013	0.016		
	[0.011]	[0.012]		
Number of Biological Siblings, All Ages	-0.013	-0.013		
	[0.008]	[0.009]		
Number of Female Biological Siblings, Ages 0-18			0.035***	0.037***
			[0.013]	[0.014]
Number of Biological Siblings, Ages 0-18			-0.028***	-0.027***
			[0.009]	[0.010]
Village Fixed Effects?	Yes	Yes	Yes	Yes
Child Controls?	Yes	Yes	Yes	Yes
Household Head Controls?	Yes	Yes	Yes	Yes
Number of Children	941	843	941	843

Notes: Robust standard errors in brackets, clustered at household level. * significant at 10%; ** significant at 5%; *** significant at 1%. Child controls include child age and gender dummies. Household head controls include head's gender, education, marital status, and age. The regression sample in Columns 1 and 3 includes all children ages 6-13 of the household head, while in Columns 2 and 4 it is restricted to resident children ages 6-13 of the household head. Data source: Burkina Child Fostering Survey (BCFS).

Table 3: Residential Rivalry Regressions, Children Ages 6-13

Dependent Variable: School Enrollment	Child of Household Head, Ages 6-13, Child Present in 2000 (1)	Child Ages 6-13, Child Present in 2000 (2)	Child of Household Head, Ages 6-13, Child Present in 2000 (3)
Number of Resident Female Biological Siblings, Ages 0-18	0.040*** [0.015]		
Number of Resident Biological Siblings, Ages 0-18	-0.030*** [0.011]		
Number of Resident Female Children, Ages 0-18		0.028** [0.012]	0.035*** [0.013]
Number of Resident Children, Ages 0-18		-0.020** [0.008]	-0.022** [0.009]
Village Fixed Effects?	Yes	Yes	Yes
Child Controls?	Yes	Yes	Yes
Household Head Controls?	Yes	Yes	Yes
Number of Children	843	1112	843

Notes: Robust standard errors in brackets, clustered at household level. * significant at 10%; ** significant at 5%; *** significant at 1%. Child controls include child age and gender dummies. Household head controls include head's gender, education, marital status, and age. The regression sample in Columns 1 and 3 is restricted to resident children ages 6-13 of the household head, while in Column 2 it includes all resident children ages 6-13. Data source: Burkina Child Fostering Survey (BCFS).

Table 4: Residential Rivalry Regressions, Children Ages 6-13, By Household Foster Status

Dependent Variable: School Enrollment	Foster Household		Non-Foster Household	
	Child Ages 6-13, Child Present in 2000	Child of Household Head, Ages 6-13, Child Present in 2000	Child Ages 6-13, Child Present in 2000	Child of Household Head, Ages 6-13, Child Present in 2000
	(1)	(2)	(3)	(4)
Number of Resident Female Children, Ages 0-18	0.009	0.028	0.064***	0.065***
	[0.015]	[0.021]	[0.019]	[0.021]
Number of Resident Children, Ages 0-18	-0.012	-0.030*	-0.034**	-0.027*
	[0.011]	[0.016]	[0.014]	[0.016]
Village Fixed Effects?	Yes	Yes	Yes	Yes
Child Controls?	Yes	Yes	Yes	Yes
Household Head Controls?	Yes	Yes	Yes	Yes
Number of Children	599	418	513	425

Notes: Robust standard errors in brackets, clustered at household level. * significant at 10%; ** significant at 5%; *** significant at 1%. Child controls include child age and gender dummies. Household head controls include head's gender, education, marital status, and age. The regression sample in Columns 1 and 2 is restricted to only foster households that had either sent or received a child during the three years prior to the survey, while the sample in Columns 3 and 4 is restricted to non-fostering households. Columns 1 and 3 include all resident children ages 6-13, while Columns 2 and 4 restrict the sample to resident children ages 6-13 of the household head. Data source: Burkina Child Fostering Survey (BCFS).

Table 5: Residential Rivalry Regressions, Children Ages 6-13, Instrumenting for Household Foster Status

Dependent Variable: School Enrollment	Child Ages 6-13, Child Present in 2000 (1)	Child of Household Head, Ages 6-13, Child Present in 2000 (2)	Child Ages 6-13, Child Present in 2000 (3)	Child of Household Head, Ages 6-13, Child Present in 2000 (4)
(Non-foster Household) * (Number of Resident Female Children, Ages 0-18)	0.094**	0.076*	0.103***	0.083**
	[0.039]	[0.043]	[0.038]	[0.041]
(Non-foster Household) * (Number of Resident Children, Ages 0-18)	-0.010	0.025	-0.009	0.020
	[0.023]	[0.028]	[0.024]	[0.027]
Number of Resident Female Children, Ages 0-18	-0.010	0.006	-0.011	0.003
	[0.018]	[0.024]	[0.019]	[0.022]
Number of Resident Children, Ages 0-18	-0.014	-0.040**	-0.026*	-0.039**
	[0.013]	[0.019]	[0.016]	[0.020]
Non-foster Household	-0.269**	-0.528***	-0.609**	-0.533**
	[0.140]	[0.169]	[0.265]	[0.240]
Instrument Variation	Kin	Kin	Kin*Village	Kin*Village
Village Fixed Effects?	Yes	Yes	Yes	Yes
Child Controls?	Yes	Yes	Yes	Yes
Household Head Controls?	Yes	Yes	Yes	Yes
Number of Children	1055	795	1055	795

Notes: Robust standard errors in brackets, clustered at household level. * significant at 10%; ** significant at 5%; *** significant at 1%. Child controls include child age and gender dummies. Household head controls include head's gender, education, marital status, and age. In Columns 1 and 2, we instrument for the potentially endogenous variable indicating household foster status using the interaction of a kinship lineage group's measured proclivity for fostering and the availability of extended family network members living outside the village. In Columns 3 and 4, we use the interaction of a kinship lineage group's within-village measured proclivity for fostering and the availability of extended family network members living outside the village. Sample size is reduced in these columns due to missing information on kinship lineage group for some households. Columns 1 and 3 include all resident children ages 6-13, while Columns 2 and 4 restrict the sample to resident children ages 6-13 of the household head. The F-statistic for the excluded instruments in Columns 1-4 (respectively 22.64, 20.42, 8.13, and 10.92) are above the threshold that would indicate a potential weak instrument bias. Data source: Burkina Child Fostering Survey (BCFS).

Table 6: Residential Rivalry Regressions, Children Ages 6-13, Instrumenting for Household Foster Status, By Gender

Dependent Variable: School Enrollment	Boys Only				Girls Only			
	Child Ages 6-13, Child Present in 2000	Child of Household Head, Ages 6-13, Child Present in 2000	Child Ages 6-13, Child Present in 2000	Child of Household Head, Ages 6-13, Child Present in 2000	Child Ages 6-13, Child Present in 2000	Child of Household Head, Ages 6-13, Child Present in 2000	Child Ages 6-13, Child Present in 2000	Child of Household Head, Ages 6-13, Child Present in 2000
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Non-foster Household) * (Number of Resident Female Children, Ages 0-18)	0.085	0.064	0.068	0.082	0.094**	0.099**	0.115**	0.106**
	[0.073]	[0.070]	[0.065]	[0.062]	[0.041]	[0.046]	[0.046]	[0.047]
(Non-foster Household) * (Number of Resident Children, Ages 0-18)	-0.019	0.020	-0.019	0.009	-0.010	0.006	-0.007	0.004
	[0.036]	[0.041]	[0.033]	[0.040]	[0.025]	[0.030]	[0.029]	[0.033]
Number of Resident Female Children, Ages 0-18	-0.004	0.014	0.003	0.006	-0.015	-0.020	-0.013	-0.020
	[0.035]	[0.036]	[0.030]	[0.032]	[0.023]	[0.029]	[0.026]	[0.030]
Number of Resident Children, Ages 0-18	-0.010	-0.036	-0.021	-0.027	-0.010	-0.022	-0.025	-0.029
	[0.022]	[0.026]	[0.025]	[0.026]	[0.016]	[0.021]	[0.020]	[0.025]
Non-foster Household	-0.056	-0.312*	-0.24	-0.233	-0.381*	-0.613**	-0.813***	-0.803***
	[0.172]	[0.185]	[0.335]	[0.279]	[0.198]	[0.272]	[0.288]	[0.299]
Instrument Variation	Kin	Kin	Kin*Vil.	Kin*Vil.	Kin	Kin	Kin*Vil.	Kin*Vil.
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Head Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Children	524	397	524	397	531	398	531	398

Notes: Robust standard errors in brackets, clustered at household level. * significant at 10%; ** significant at 5%; *** significant at 1%. Child controls include child age dummies. Household head controls include head's gender, education, marital status, and age. In Columns 1, 2, 5, and 6, we instrument for the potentially endogenous variable indicating household foster status using the interaction of a kinship lineage group's measured proclivity for fostering and the availability of extended family network members living outside the village. In Columns 3, 4, 7, and 8, we use the interaction of a kinship lineage group's within-village measured proclivity for fostering and the availability of extended family network members living outside the village. Odd-numbered columns include all resident children ages 6-13, while even-numbered columns restrict the sample to resident children ages 6-13 of the household head. Columns 1-4 are restricted to only boys, while Columns 5-8 are restricted to only girls. Data source: Burkina Child Fostering Survey (BCFS).

Table 7: Residential Rivalry Regressions, Children Ages 6-13, Instrumenting for Household Foster Status and Number of Children

Dependent Variable: School Enrollment	Child Ages 6-13, Child Present in 2000 (1)	Child of Household Head, Ages 6-13, Child Present in 2000 (2)	Child Ages 6-13, Child Present in 2000 (3)	Child of Household Head, Ages 6-13, Child Present in 2000 (4)
(Non-foster Household) * (Number of Resident Female Children, Ages 0-18)	0.177**	0.191*	0.273*	0.264*
	[0.085]	[0.107]	[0.144]	[0.157]
(Non-foster Household) * (Number of Resident Children, Ages 0-18)	-0.078	-0.077	-0.141	-0.135
	[0.064]	[0.086]	[0.102]	[0.124]
Number of Resident Female Children, Ages 0-18	-0.007	0.000	-0.048	-0.046
	[0.070]	[0.064]	[0.094]	[0.068]
Number of Resident Children, Ages 0-18	-0.020	-0.039	-0.009	-0.007
	[0.062]	[0.056]	[0.084]	[0.054]
Non-foster Household	-0.218	-0.395**	-0.667*	-0.437
	[0.156]	[0.192]	[0.405]	[0.289]
Instrument Variation	Kin	Kin	Kin*Village	Kin*Village
Village Fixed Effects?	Yes	Yes	Yes	Yes
Child Controls?	Yes	Yes	Yes	Yes
Household Head Controls?	Yes	Yes	Yes	Yes
Number of Children	1055	795	1055	795

Notes: Robust standard errors in brackets, clustered at household level. * significant at 10%; ** significant at 5%; *** significant at 1%. Child controls include child age dummies. Household head controls include head's gender, education, marital status, and age. We instrument for the potentially endogenous variable indicating household foster status using in columns 1 and 2 the interaction of a kinship lineage group's measured proclivity for fostering and the availability of extended family network members living outside the village and in columns 3 and 4 the interaction of a kinship lineage group's within-village measured proclivity for fostering and the availability of extended family network members living outside the village. We also instrument for the potentially endogenous number of children using the number of brothers and sisters of the household head. In column 1, the F-statistic for the excluded instruments in the foster household status regression is 11.78 and in the number of children regression is 4.98. In column 2, the corresponding F-statistics are 11.29 and 4.06, respectively. In column 3, the corresponding F-statistics are respectively 4.35 and 2.38, while in column 4, the corresponding F-statistics are 5.62 and 1.85. Columns 1 and 3 include all resident children ages 6-13, while Columns 2 and 4 restrict the sample to resident children ages 6-13 of the household head. Data source: Burkina Child Fostering Survey (BCFS).

Appendix Table 1: Non-linear Instrumental Variables, First Stage Regressions for Table 5

IV Regression:	Table 5 Column 1	Table 5 Column 2	Table 5 Column 3	Table 5 Column 4
Dependent Variable: Non-Foster Household	(1)	(2)	(3)	(4)
(Kinship Group Fostering Rate) * (Number of extended family members outside the village)	1.418***	1.480***		
	[0.298]	[0.328]		
(Village Kinship Group Fostering Rate) * (Number of extended family members outside the village)			0.048***	0.064***
			[0.017]	[0.019]
Nakomse Kinship Group Dummy	-0.479***	-0.517***	-0.006	-0.032
	[0.112]	[0.126]	[0.063]	[0.070]
Number of extended family members outside the village	-0.495***	-0.521***	-0.012	-0.020
	[0.108]	[0.119]	[0.016]	[0.018]
Number of extended family network members	-0.004	-0.0004	-0.005	-0.002
	[0.012]	[0.013]	[0.013]	[0.014]
Number of Resident Female Children, Ages 0-18	0.013	0.016	0.007	0.012
	[0.020]	[0.021]	[0.020]	[0.021]
Number of Resident Children, Ages 0-18	-0.030**	-0.043***	-0.036***	-0.050***
	[0.013]	[0.015]	[0.013]	[0.015]
Village Fixed Effects?	Yes	Yes	Yes	Yes
Child Controls?	Yes	Yes	Yes	Yes
Household Head Controls?	Yes	Yes	Yes	Yes
Number of Children	1055	795	1055	795

Notes: Robust standard errors in brackets, clustered at household level. * significant at 10%; ** significant at 5%; *** significant at 1%. Child controls include child age and gender dummies. Household head controls include head's gender, education, marital status, and age. Data source: Burkina Child Fostering Survey (BCFS).

Appendix Table 2: Non-linear Instrumental Variables, First Stage Regressions for Table 7

	Table 7 Column 1	Table 7 Column 1	Table 7 Column 2	Table 7 Column 2	Table 7 Column 3	Table 7 Column 3	Table 7 Column 4	Table 7 Column 5
Dependent Variable:	Number of Resident Children	Non-Foster Household	Number of Resident Children	Non-Foster Household	Number of Resident Children	Non-Foster Household	Number of Resident Children	Non-Foster Household
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(Kinship Group Fostering Rate) * (Number of extended family members outside the village)	-1.963*	1.481***	-2.261*	1.578***				
	[1.037]	[0.305]	[1.180]	[0.333]				
(Village Kinship Group Fostering Rate) * (Number of extended family members outside the village)					-0.038	0.049***	-0.042	0.066***
					[0.081]	[0.017]	[0.089]	[0.020]
Number of Brothers and Sisters of Household Head	-0.078**	0.005	-0.076**	0.004	-0.076**	0.004	-0.074*	0.004
	[0.034]	[0.009]	[0.037]	[0.009]	[0.035]	[0.009]	[0.039]	[0.009]
Nakomse Kinship Group Dummy	0.616	-0.499***	0.741*	-0.549***	-0.038	-0.005	-0.002	-0.032
	[0.388]	[0.112]	[0.417]	[0.126]	[0.236]	[0.064]	[0.243]	[0.071]
Number of extended family members outside the village	0.768**	-0.519***	0.882**	-0.559***	0.090	-0.015	0.097	-0.025
	[0.367]	[0.110]	[0.421]	[0.120]	[0.057]	[0.016]	[0.060]	[0.018]
Number of extended family network members	0.002	-0.005	-0.007	-0.0004	0.004	-0.006	-0.004	-0.002
	[0.043]	[0.013]	[0.043]	[0.014]	[0.044]	[0.014]	[0.043]	[0.015]
Number of Resident Female Children, Ages 0-18	1.136***	-0.021	1.117***	-0.032**	1.155***	-0.034**	1.139***	-0.045***
	[0.052]	[0.013]	[0.054]	[0.015]	[0.051]	[0.013]	[0.055]	[0.014]
Village Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Child Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Head Controls?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Number of Children	1055	1055	795	795	1055	1055	795	795

Notes: Robust standard errors in brackets, clustered at household level. * significant at 10%; ** significant at 5%; *** significant at 1%. Child controls include child age and gender dummies. Household head controls include head's gender, education, marital status, and age. Data source: Burkina Child Fostering Survey (BCFS).