SIBLING RIVALRY, RESIDIENTIAL RIVALRY, AND CONSTRAINTS ON THE AVAILABILITY OF CHILD LABOR*

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Sibling rivalry occurs when siblings compete for parental investments. We examine how rivalry among biological siblings, who may not be co-resident, differs from rivalry among co-resident children and how this affects school enrollment for children in Burkina Faso. We test the hypothesis that the value of child labor in home production contributes to rivalry by comparing households that differ in their access to child fostering networks. Fostering moves child labor between residences, decoupling a child's location from the value of their time. We find rivalry influences enrollment only in families who do not foster and are thus constrained in their ability to equalize child labor supply and demand. In those households, the relative productivity of resident children impacts time allocation decisions and subsequently enrollment. We find no evidence of rivalry in unconstrained households. Thus, sibling rivalry is better understood as residential rivalry, stemming from constraints on child labor availability.

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Efforts to promote universal primary education largely focus on school costs, quality, and access. Within-family considerations are also important. How and why household and especially sibling composition influences investments in education has received substantive attention. In a classical model with complete markets, sibling composition impacts education through the present value of the child's future economic contribution to the family or differences in the returns to education caused by learned behaviors (Butcher and Case 1994). Models that allow for heterogeneous preferences of multiple agents 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 child income generation potential (Manacorda 2004) or relative returns to investments (Garg and Morduch 1998).

We argue that comparative advantage in household-based production is a key cause of differences in education among co-resident children. Household-based production is central in the economic lives of the world's poor (Banerjee and Duflo 2007), and it is the main user of

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child labor. In the Millennial UNICEF MICS survey project, more children work in household-based production than attend school. 86 percent of children 10-14 help with the family farm, business, or provide unpaid household services for the household where they live. Children are nine times more likely to participate in home-based economic activity than wage employment outside the home.[1] The value of their time in household production is the opportunity cost of education. With a diminishing marginal product of child labor in household production, the greater the endowment of child labor in the household, the lower the opportunity cost of education. We use the phrase "residential rivalry" for this phenomenon that the composition of resident children in a family impacts education by altering the opportunity cost of time in school.

Testing residential rivalry is difficult, because the opportunity cost of time in school is unobservable.[2] 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 or receive children. When households can send or receive children, the opportunity cost of schooling depends on the value of child labor among households that can exchange children rather than the productivity of children present in the household. We measure composition by the sex ratio of co-resident children. In data from rural Burkina Faso, we find that education is influenced by the sex composition of resident children in households that do not participate in fostering networks and thus are constrained in their ability to send or receive child labor. We find no relationship between sex composition and education in unconstrained households. The differences between constrained and unconstrained households in the response of education to sex composition are substantive. 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 in constrained (non-fostering) households (26 percent of mean enrollment in our setting) but has no substantive effect on schooling in unconstrained households.

There are three main contributions of our finding of residential rivalry from constraints on the availability of child labor. First, this is novel evidence supporting the importance of the opportunity cost of time in household-based production for education. Many child labor studies argue that girls have both an advantage in household-based work and document their more

[1] Authors' calculation from the MICS2 microdata available at http://www.childinfo.org/

[2] If all types of labor that could be used in household-based production are perfect substitutes in all tasks, labor markets are complete, and the household either sends labor to the formal labor market or hires labor in, then the opportunity cost of education is a transformation of the wage paid. We discuss this further in Section 1.

intensive involvement in household-based 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 made it difficult to establish a connection between engagement in household-based production and education.

Second, our finding that having relatively more resident girls increases education is similar to findings in the sibling rivalry literature (Garg and Morduch 1998; Morduch 2000). However, 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 that induce parents to invest in human capital based on their offspring's relative returns to education. Having more low return children (girls) reduces competition for scarce resources and raises investments for all children in their human capital. Our data contain information on both siblings and resident children. When we consider all biological siblings, we find no correlation between education and sex composition. We observe a correlation between education and the sex composition of young siblings close in age, but young siblings are typically 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 study is thus supportive of others (Parish and Willis 1993, Thomas et al. 2004, Cox and Fafchamps 2008, and Vogl 2010) that emphasize the important role adjustments in household composition play in determining 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 fostering in Africa. 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 in Burkina Faso as well as the rest of Africa.[3] The larger the fostering network, the weaker is the connection between education and household labor supply and demand. This view of fostering as a solution to the missing market for labor in household-based activity is useful in interpreting the existing literature on fostering. While the role fostering plays in promoting schooling is well documented, many studies such as Ainsworth (1996) and Grimard (2000) emphasize the importance of household labor demand in the decision

[3] In the data used in this study, during the three years prior to the survey, 37 percent of households fostered a child (17.8 percent sent a child, 14.9 percent received a child, and 4.3 percent both sent and received a child). This rate of child fostering is consistent with Demographic and Health Survey data from other African nations (Vandermeersch 1997). For example, she documents a fostering rate of 21 percent in Burkina Faso, 26 percent in Cote d'Ivoire, 15 percent in Mali, 25 percent in Niger, and 32 percent in Senegal.

to receive foster children. Others such as Butcher (1993), Zimmerman (2003), and Akresh (2009) highlight sex imbalances within the household and income shocks as reasons for sending children, especially girls. If movements in children equilibrate the value of child labor within a network, both supply and demand factors are important determinants of fostering. Their relative importance depends on the distribution of child endowments and complementary inputs.

A challenge with establishing a causal link between sex composition and education in families that differ in their fostering status is that the decision to foster is driven by factors correlated with education. Part of our contribution comes from a robustness check where we attempt to address the endogeneity of fostering, an issue that is neglected in much of the literature on the consequences of fostering. The cultural history of Bazega province provides a useful source of variation in fostering. For historical reasons discussed 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 fostering exchanges. While separately, each of these characteristics may affect education, we argue it is plausible that the interaction of a lineage group's proclivity for fostering and the availability of network members elsewhere has no direct correlation with education except through how this interaction influences child fostering. Our finding that residential rivalry occurs only in households that do not foster is also present in the data when we use variation in fostering from this instrument.

The remainder of the paper is organized as follows. Section 1 defines the concept of residential rivalry and motivates the test within this study. Section 2 describes the Burkina Faso household survey data used in the analysis. Section 3 documents the existence of residential rivalry and the relationship between that rivalry and child fostering. Section 4 concludes.

1. Theoretical Framework

Residential rivalry is the phenomenon that the co-residency of children impacts education by altering the opportunity cost of time in school. Appendix 1 contains a more formal and complete articulation of a theoretical model that generates our test of residential rivalry. This section highlights the key ideas and assumptions behind our definition of residential rivalry.

We have in mind a model where a single agent decides to allocate his endowment of child time between schooling and production of a non-traded good, both of which give the agent utility. The opportunity cost of schooling is then the agent's valuation of the marginal product of labor in household production. The agent is constrained to use labor that is resident in the

household in the production of this non-traded good, and labor exhibits positive, diminishing returns. Residences with more labor to produce the non-traded good have lower opportunity costs of time in school.

Suppose there are two types of labor, boys and girls. The agent treats all children of a given type identically. Both types are perfect substitutes in the production of the non-traded good subject to a productivity shifter. Assume girls have an advantage at producing the non-traded good.[4] Suppose the agent's return to education is the same for boys and girls. Because of their advantage in production, girls receive less education. Residences with more girl labor available will have more education, because the additional girl time in home production lowers the opportunity cost of time in school. We distinguish between this residential rivalry effect (working through the opportunity cost of time in school) and the sibling endowment effect stemming from the presence of more labor leaving the household wealthier.

Residential rivalry does not depend on biological relationship except when those biological siblings are co-resident. Hence, it differs from the phenomenon usually called "sibling rivalry". In the model of Garg and Morduch (1998), sibling rivalry occurs because of credit constraints. Parents cannot borrow against future earnings to finance a child's education and do not have enough cash on hand to finance the optimal amount of education for each child. Hence, siblings compete for scarce resources to finance their education. All siblings are better off if there is less within-household competition because the household is endowed with more girls who have lower returns to education (an assumption in the sibling rivalry model). Residential rivalry occurs even with perfect credit markets since it stems from the inability to substitute for resident labor in the production of the non-traded good.

Residential rivalry disappears as we relax the constraint to use resident labor in the non-traded good's production. In the extreme, suppose the agent could use hired-in labor for the non-

[4] 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, not dowries, at the time of marriage, it is plausible that heads are wealthier if endowed with more girls.

traded good's production. Then the opportunity cost of education depends on labor market wages, not residency. Thus, we can test residential rivalry by comparing households that differ in whether they are constrained to use endowment labor in production of the non-traded good. Our test for residential rivalry mirrors Benjamin's (1992) test of the separation hypothesis for farm household models. He tests whether farm labor decisions depend on household composition. We test whether education (and implicitly its opportunity cost) depends on which children are resident for households constrained in their ability to bring in or send out child labor.

Parents are typically hesitant to send their children to work in strangers' homes. In our setting in rural Burkina Faso, most foster children move within extended-family networks where the network offers the child some protection compared to unrelated hosts. Hence, the variation in whether the agent is constrained in the availability of labor for the production of the non-traded good comes from whether the agent has access to a fostering network. We expect education to depend on what type of children are present in households that are constrained to use endowment labor by virtue of their exclusion from fostering networks, and we expect to see no relationship between residency and education in households that are part of a fostering network.

Our test of residential rivalry assumes there are inputs into household activities for which the agent cannot hire in labor. This assumption is in Becker (1965) and subsequent work on home production. An inability to hire labor for household activities may be due to information problems, social stigma, cash constraints, or the low economic value of many of these services. Paid employment is rarely seen in our study area, and all tests that we know of the competitive markets hypothesis in agriculture in rural Burkina Faso reject the separation hypothesis (Udry 1996). Thus, moving children between residences through fostering is one of the few ways a household can adapt composition to its economic needs. Certainly adults could also move, but there are many reasons why children might have comparative advantage in moving for these purposes compared to adults.

2. Burkina Faso Child Fostering Survey (BCFS)

This study focuses on the education of children ages 6 to 13 in the Burkina Faso Child Fostering Survey (BCFS). We focus on these ages as they are the key primary school ages in Burkina Faso, and child labor laws there distinguish 14 year olds (who can legally 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 Burkina Faso, located approximately 50 miles

from the country capital. The data were collected by Akresh in 2001 and were used in studies on the determinants of child fostering (Akresh 2009) and the impacts of fostering on schooling (Akresh 2008). 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 for 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 the child's residency status at the time of the survey. In addition, for every household, all survey questions in the household roster were asked about any person who had lived in the household for at least 4 months at any point during the 3 years before the survey. These questions include the age, gender, education, current school enrollment status, and number of months the individual lived outside of the village during the 3 years prior to the survey. These two unique aspects of expanding the targets for the household roster (all biological children and anyone resident during the 3 years prior to the survey) differ from traditional household surveys in developing countries. They allow us to measure the differential impacts of *sibling* versus *residential* sex composition. In contrast, most previous studies are limited to studying the sex composition only of siblings who are resident at the time of the survey.

Second, the BCFS collects information about every household's kinship lineage group. All households in the survey are Mossi ethnicity, the largest ethnic group in Burkina Faso with 40 percent of the population. Starting around the 15th century, the Mossi conquered large swaths of territory due to their extensive cavalry and created a prosperous empire that lasted until the colonial period (Tauxier 1917; Skinner 1962). The Mossi are divided into 2 main kinship lineage groups. The first are the Nakomse, translated as "people of power", who are descendants of the cavalry that conquered the region's other inhabitants (Hammond 1959). 62 percent of households in our sample are of this lineage group. The second are the descendants of the farmers who had originally owned the lands. This second 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 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 may 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 residence location for everyone in the household head's immediate family network, defined to include parents, brothers, sisters, and adult children. Our identification comes from combining the lineage group and extended family information to gauge the availability of network members who might participate in a fostering exchange.

Table 1 illustrates how 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 who are girls.[5] We define residency as being present for at least 4 out of 12 months in a year, although for those children present, 91 percent of them were resident all 12 months.[6] Each panel in the table is restricted to different groups of children age 6-13 to highlight how sibling sex composition differs by residence and biological relationship to the household head. Panel A focuses on children 6-13 of the household head. Panel B focuses on the 90 percent of children 6-13 of the household head who 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 residency environment for non-resident children of the household head, so columns 6 and 7 of panel A are missing.

Most of our discussion focuses on resident children 6-13 of the household head (panel B). These children average more than 8 siblings and slightly more than half are girls. The fraction female is mechanically higher for boys than girls.[7] 70 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 permanently leave home until they are older. The average number of co-resident children under 18 is about 2 percent larger than the number of siblings under 18 indicating that receiving children is slightly

[5] We define siblings as biological children of the household head. Columns 2 and 3 include all siblings regardless of sibling residence or age. Columns 4 and 5 restrict calculations to only siblings under 18 regardless of sibling residence. Columns 6 and 7 only include co-resident children under 18 regardless of their biological relationship to the child.

[6] This residency definition was adopted to synchronize with the child fostering residency definition used in the fieldwork and developed in consultation with local partners. The 4 month residency requirement was adopted in the fieldwork so that children who changed residences during school vacations and other short term movements would not be codified as fostered.

[7] 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.

more prevalent than sending. This net inflow is weighted towards girls as the fraction female is closer to parity when considering co-residency rather than siblings.

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 this information for resident children who are not a child of the household head. The difference between panel B and C is due to the 32 percent of resident children who are not children of the household head. These children are more likely to be female and tend to live in households with fewer children present and a more equal gender balance. This is important because we will find that it is the movement of girls that appears to relax the constraint that produces residential rivalry.

3. Empirical Findings

3.1 Residential Rivalry

Studies of the impact of siblings on education typically use the number of siblings and the number of sisters that a child has to explain different schooling outcomes as follows:

$$(1) \qquad e_{ihv} = \kappa + \omega_0 S_{ih} + \omega_1 F_{ih} + \alpha_0 X_{ih} + \alpha_1 Z_h + \lambda_v + \varepsilon_{ihv}$$

where e_{ihv} is the educational outcome for child i in household h resident in village v, S_{ih} is the number of siblings the child has, F_{ih} is 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, λ_v is a village fixed effect, and ε_{ihv} is a random, idiosyncratic error term. The interpretation of ω_0 is the change in e_{ihv} associated with an additional male sibling. The interpretation of ω_1 is the change in e_{ihv} 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 explicitly relax it.

In columns 1 and 2 of Table 2, we report estimates of ω_0 and ω_1 from eq. (1). The 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.[8] We find no

[8] 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. Note that the data do not contain any information on child labor activities.

evidence of substantive sibling endowment effects. 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. In our data, 24 percent of children 6-13 are enrolled. Results are consistent in the specification that restricts the sample to the household head's children ages 6-13 who are present in 2000 (column 2) and for the sample that does not include this residency restriction (column 1).

While standard theory on sibling endowments (including sibling rivalry) argues that the biological relationship is the critical one, most previous research is unable, due to data limitations, to examine this relationship for non-resident biological siblings. For this reason, the sample in columns 1 and 2 differs from typical sibling rivalry regressions. In columns 3 and 4, we limit the sibling counts to siblings aged 0-18, regardless of residency. Additional male siblings lower the likelihood of enrollment, while another biological sister under 18 raises the likelihood of enrollment. The challenge in interpreting columns 3 and 4 is that when we restrict our sibling measures to children under 18 that also restricts our attention largely to co-resident siblings. 90 percent of these under 18 siblings are also co-resident. Hence, columns 3 and 4 could imply sibling endowment effects that only occur among siblings who are close in age or it could be evidence of residential rivalry as sibling age and cohabitation are strongly correlated.

We find clearer evidence supporting residential rivalry in Table 3. In column 1, we show a sibling rivalry regression for typical datasets with limited information. We restrict the number of siblings and number of female siblings to resident biological siblings under 18 because in most datasets, information is only available about resident child siblings. Results in column 1 are consistent with Table 2 column 4 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 child relationships must be defined relative to the household head.[9] We observe similar evidence of gender rivalry whether our units of observation are children of the household head (column 1) or all children (column 2) and whether we focus on resident female siblings or resident females. Magnitudes in column 2 are slightly smaller than column 1. However, 80 percent of the additional children in column 2 are in households participating in fostering exchanges where, as we discuss below, there is no evidence

[9] Of the 269 children ages 6-13 added to the sample, 214 are fostered, 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.

of rivalry. Column 3 again restricts the sample to children of the household head as in column 1. We find a similar impact on enrollment due to resident female children as we found in column 1 due to resident female biological siblings. Because most 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 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 sibling rivalry models or the model in section 1.1) do not posit that such effects should only influence education for children close in age. In fact, most evidence of how siblings interact shows older offspring supporting younger offspring (Parish and Willis 1993, Vogl 2010). Nevertheless, one could modify sibling endowment models to posit 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.

3.2 Child Fostering and Rivalry

To distinguish how biological siblings and resident children influence education differently, we must address how these non-biological siblings became part of the household. In Table 4, we examine how residential rivalry differs across foster and non-foster households. For children of the household head, we find similar effects of adding male siblings in both foster and non-foster households. However, residential rivalry appears only to exist in households that have not recently fostered a child. For households that have 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 18 and school enrollment. For non-foster households, 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. Results are similar if the regression sample is restricted to only children of the household head (column 4). Non-fostering households are

constrained in their ability to easily obtain child labor so the relative productivity of resident children then impacts time allocation and enrollment decisions.

3.3 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 enrollment decision. Although studies of fostering typically compute counterfactuals for foster children by examining non-fostered children, foster 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.[10] 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 the number of extended family members living outside the village and use the interaction as our instrument for fostering. Let N_h indicate a non-fostering household. $Nakomse_h$ indicates a household from the Nakomse kinship group. R_k is the fostering rate for the household's kin group k. M_h is the household's number of extended family members that live outside the village (we include the total number of extended family members as a household level control in the vector Z_h). We estimate a pseudo first stage regression of N_h on $Nakomse_h$, M_h, and the interaction of M_h and R_k, as well as all controls included in equation (1):

$$(2) \quad N_{ihvk} = \pi_0 + \pi_1 M_h + \pi_2 Nakomse_h + \pi_3 M_h * R_k + \pi_4 S_{ih} + \pi_5 F_{ih} + \pi_6 X_i + \pi_7 Z_h + \lambda_v + \upsilon_{ihvk}$$

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 (2). We use $\tilde{N}_{ihk}$,

[10] Our definition of a foster household in this paper is a household that has sent or received a child during the 3 years prior to the survey. Results are qualitatively similar and still statistically significant if we define a foster household as one that has ever sent or received a child. We use the 3-year window as our preferred definition because we are trying to capture recent child availability and 3 years was the window used in the fieldwork tracking phase.

$\tilde{N}_{ihk} * S_{ih}$, and $\tilde{N}_{ihk} * F_{ih}$ as instruments in estimating equation (1) modified to allow different residential rivalry effects by household fostering status. Specifically, we modify equation (1) as:

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

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.

We assume a person's lineage group's proclivity to foster interacts with the availability of fostering opportunities to determine whether fostering exchanges occur without separately affecting schooling. Since fostering is more prevalent with more extended family members, we expect the interaction of the clan group and the number of extended family members to increase the chance the household does not foster as the saliency of the clan influence is less important if 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 the average fostering rate of the other lineage group members excluding the household's own observation. Nakomse are more likely to foster. More extended family members outside of the village increases fostering. Our instrument, the interaction of the two, increases the likelihood households do not foster. Put another way, being Nakomse attenuates the extended family member importance. Having more 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-level fostering rates to construct the instruments. It is possible there is heterogeneity in kinship group behavior across villages. Columns 3 and 4 contain results where we instrument for non-foster status with village-level kinship fostering rates rather than using the fostering rate for the entire kinship group, again omitting a household's own observation. 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 (3) 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.

Results in Table 6 show we cannot reject the null that residential rivalry in non-fostering households is equally salient for boys and girls. However, the magnitudes of the residential rivalry effects in non-fostering households are larger for girls. Columns 1 to 4 contain results for boys; columns 5 to 8 contain results for girls. The regressions within each gender are organized as in Table 5. Columns 1 and 2 for boys (5 and 6 for girls) instrument for non-fostering using the clan group-wide variation in fostering rates. Columns 3 and 4 (7 and 8 for girls) use 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 is unrelated to schooling for boys or girls in foster 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 exogenous, and our attempt to address the endogeneity of fostering is unprecedented in the fostering literature.

We use the number of siblings of the household head as an instrument for the number of resident children. We assume the number of siblings of the household head affects the head's desired family size but is not determined by contemporaneous economic circumstances. In using this instrument for this robustness check, we make the strong assumption that the latent socio-economic factors that lead to the head's sib size do not persist to influence his choice of family size. We implement the instrument in a similar way to how we instrumented for foster status. We also still instrument for foster status. Appendix Table 2 contains the pseudo-first stages for fostering and number of resident children. The household head's number of siblings 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 (3). 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.

Addressing the endogeneity of the number of resident children (still treating their sex composition as exogenous) has little 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 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 school could then feedback into fertility decisions. When we instrument for number of children, in non-foster households we observe larger negative effects of boys on education and larger positive effects of girls on education compared to instrumenting only for foster status (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 protect education and attenuate rivalry. Constrained households in part choose fertility aware of latent labor demand and potential educational effects. The instrument works off a different dimension. It leverages the size of the head's family growing up, which we think informs his expectations about what size family he wants. That is, the instrument works off the non-economic dimension of the choice of number of children. There are apt to be economic costs of this choice, and these costs are reflected in the larger Table 7 estimates.

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 and might also offer insurance or financial support. We assume this value of siblings is captured by controls for the number of extended family members and the number outside the village. 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.

4. Conclusion

This paper examines how rivalry among biological siblings and co-resident children affects educational investments for households in rural Burkina Faso. Resident children influence educational investments differently than biological siblings because of the household's demand

for services that it cannot acquire through local labor and product markets. 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 and residential sex composition on schooling investments. We find little evidence to support what is generally referred to in the literature as sibling rivalry. However, residential rivalry, which is the idea that having a greater share of resident children with an advantage in household-based production increases education by altering the opportunity cost of time in school, appears far more relevant in our setting. Enrollment is higher in households where a larger fraction of the resident children are girls.

As the hiring of child labor is rare in this region, child fostering substitutes for the hiring 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 adjust 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 biological 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 consider the robustness of our findings by instrumenting 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 (column 1 of table 7), the thought experiment of changing a resident child from a boy to a girl in a non-fostering household raises the likelihood of enrollment for all children by 17 percentage points, 70 percent of mean enrollment.

Education policy in poor countries often focuses on improving the schooling of girls, which generally lags far behind that of boys. By highlighting the distinction between sibling and residential rivalry, we provide an explanation for boys' higher enrollment rates that focuses on household production and the availability of child labor rather than on parent preferences or

higher male returns to education. This distinction is important because overall economic development or affirmative action programs aimed at persuading parents to enroll girls may be less effective than interventions 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.

Appendix 1: Sibling Endowment Effects and Residential Rivalry

In this appendix, we illustrate how siblings and co-resident children can differentially affect time allocation in households. We consider two types of children (boys and girls) and focus on how the mix of child types affects education. In Section A1.1, we add siblings and education into a standard separable farm household model. Siblings create a sibling endowment effect. Households with an endowment of the relatively more valuable type are wealthier. In Section A1.2, 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 one type of labor is more productive), 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 A1.1 and A1.2 assume the endowment of child types is given. In Section A1.3, 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.

A1.1 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 follow convention in the child labor literature (Basu and Van 1998, Baland and Robinson 2000) and treat the labor supply of adults as inelastic with respect to choices over child time allocation. Each household has a single head who is a unitary decision-maker. The head is endowed with N offspring. A fraction g are girls. Household preferences are 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.[11] The household has multiple

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

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 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* 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:

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

subject to

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

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

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

$$c_i, e_i, L_i^o, L_i^W, L_i^h \geq 0, i \in \{m,f\} \tag{5}$$

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 labor use in household production resource constraint. It assumes boys and girls are perfect substitutes in production and hired in labor is a perfect substitute for family labor. In our analysis, a central point is the extent of substitutability between boys and girls in household production. Hence, we introduce a productivity shifter a, with $a>0$, that reflects how many girls one unit of boy labor is equal 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:

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

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

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

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

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

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. Footnote 4 in the main text highlights why girls might be more productive workers in home production and why families endowed with more girls may be wealthier.

A1.2 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 $\boldsymbol{\epsilon}$, and that utility is additively separable in each of its arguments. Thus, we write the head's utility function as:

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

We consider the case of no external labor market so the head can neither hire in 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. The equation (4) time constraints 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:

$$\begin{aligned} \max_{e_m, e_f} & \; v\big(N * (g * (1 - e_f) + (1-g) * a * (1 - e_m))\big) \\ & + N\left(g * r(e_f) + (1-g) * r(e_m)\right) \end{aligned} \tag{9}$$

Assuming interior solutions,[12] 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 $r_f' = v'$.[13] In the Section A1.1 model, this is also true, but in that model the value of labor's marginal product in home production is market determined. In this 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, which induces more schooling. 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:

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

[12] Conceptually in the model, education is time not working and includes time in class or studying and leisure time.
[13] 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) $v'(L) = \beta_0 + \beta_1 * L$

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

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.[14] 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.

A1.3 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 model above, if households are unconstrained in their ability to bring in new members, the gender mix will depend on the shape of the

[14] 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 $L_f^o - a\frac{\alpha_0}{\alpha_1}\left(\frac{\alpha_1}{\alpha_0} + 1 - a - a e_f \frac{\alpha_1}{\alpha_0}\right)$. 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 in Burkina Faso move between households, it is likely within an extended family network. This implies there is a finite supply of male and female children. The shadow price of boys and girls is determined as a result of supply and demand in the network. We define the equilibrium shadow price of a boy as θ_m and a girl as θ_f. Suppose the price of girls and boys are paid out of consumption. We assume each separate residence faces its own budget constraint:

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

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 who 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 A1.1 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 A1.1. 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.

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

	Number of Sampled Children (1)	Siblings		Siblings Under 18		Co-resident Children Under 18	
		Average Number (2)	Fraction Female (3)	Average Number (4)	Fraction Female (5)	Average Number (6)	Fraction Female (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**	0.035***
		[0.012]	[0.013]
Number of Resident Children, Ages 0-18		-0.020**	-0.022**
		[0.008]	[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** [0.039]	0.076* [0.043]	0.103*** [0.038]	0.083** [0.041]
(Non-foster Household) * (Number of Resident Children, Ages 0-18)	-0.010 [0.023]	0.025 [0.028]	-0.009 [0.024]	0.020 [0.027]
Number of Resident Female Children, Ages 0-18	-0.010 [0.018]	0.006 [0.024]	-0.011 [0.019]	0.003 [0.022]
Number of Resident Children, Ages 0-18	-0.014 [0.013]	-0.040** [0.019]	-0.026* [0.016]	-0.039** [0.020]
Non-foster Household	-0.269** [0.140]	-0.528*** [0.169]	-0.609** [0.265]	-0.533** [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

	Boys Only				Girls Only			
Dependent Variable: School Enrollment	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** [0.085]	0.191* [0.107]	0.273* [0.144]	0.264* [0.157]
(Non-foster Household) * (Number of Resident Children, Ages 0-18)	-0.078 [0.064]	-0.077 [0.086]	-0.141 [0.102]	-0.135 [0.124]
Number of Resident Female Children, Ages 0-18	-0.007 [0.070]	0.000 [0.064]	-0.048 [0.094]	-0.046 [0.068]
Number of Resident Children, Ages 0-18	-0.020 [0.062]	-0.039 [0.056]	-0.009 [0.084]	-0.007 [0.054]
Non-foster Household	-0.218 [0.156]	-0.395** [0.192]	-0.667* [0.405]	-0.437 [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. The F-statistic for the 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).

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* [1.037]	1.481*** [0.305]	-2.261* [1.180]	1.578*** [0.333]				
(Village Kinship Group Fostering Rate) * (Number of extended family members outside the village)					-0.038 [0.081]	0.049*** [0.017]	-0.042 [0.089]	0.066*** [0.020]
Number of Brothers and Sisters of Household Head	-0.078** [0.034]	0.005 [0.009]	-0.076** [0.037]	0.004 [0.009]	-0.076** [0.035]	0.004 [0.009]	-0.074* [0.039]	0.004 [0.009]
Nakomse Kinship Group Dummy	0.616 [0.388]	-0.499*** [0.112]	0.741* [0.417]	-0.549*** [0.126]	-0.038 [0.236]	-0.005 [0.064]	-0.002 [0.243]	-0.032 [0.071]
Number of extended family members outside the village	0.768** [0.367]	-0.519*** [0.110]	0.882** [0.421]	-0.559*** [0.120]	0.090 [0.057]	-0.015 [0.016]	0.097 [0.060]	-0.025 [0.018]
Number of extended family network members	0.002 [0.043]	-0.005 [0.013]	-0.007 [0.043]	-0.0004 [0.014]	0.004 [0.044]	-0.006 [0.014]	-0.004 [0.043]	-0.002 [0.015]
Number of Resident Female Children, Ages 0-18	1.136*** [0.052]	-0.021 [0.013]	1.117*** [0.054]	-0.032** [0.015]	1.155*** [0.051]	-0.034** [0.013]	1.139*** [0.055]	-0.045*** [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. The F-Statistics for the joint significance of the instruments in columns 1- 8 are 4.98, 11.78, 4.06, 11.29, 2.38, 4.35, 1.85, and 5.62 respectively. Data source: Burkina Child Fostering Survey (BCFS).