Social Capital and International Migration: A Test Using Information on Family Networks[1]

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This article uses a multistate hazard model to test the network hypothesis of social capital theory. The effects of family network ties on individual migration are estimated while controlling for measured and unmeasured conditions that influence migration risks for all family members. Results suggest that social network effects are robust to the introduction of controls for human capital, common household characteristics, and unobserved conditions. Estimates also confirm the ancillary hypothesis, which states that diffuse social capital distributed among community and household members strongly influences the likelihood of out-migration, thus validating social capital theory in general and the network hypothesis in particular.

Demonstrating the superiority of one theoretical claim over another is always difficult, and opportunities to conduct critical tests are rare, even in the natural sciences where experimental methods prevail. The network

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hypothesis of social capital theory offers a particular dilemma. Its leading prediction is that people who are socially related to current or former migrants have access to social capital that significantly increases the likelihood that they, themselves, will migrate. This hypothesis is not new. Indeed, it has a respectable historical tradition and continues to be invoked to explain the magnitude of migration flows as well as the concentration of certain types of migrants in particular locations. The logical and historical foundations of the hypothesis and a summary of a number of newer formulations and applications throughout the world are thoroughly covered elsewhere (see Massey et al. [1998], for a review).

Despite the fact that the hypothesis has been sustained in a surprisingly large number of studies and in diverse social and geographic settings, no test has yet established its veracity *compared with other theories* that predict the same outcomes. In this article, we employ an infrequently used model and statistical tool to conduct a systematic test of social capital theory, one that confirms the latter's validity while simultaneously casting doubt on competing explanations.

SOCIAL CAPITAL THEORY

The economist Glenn Loury (1977) introduced the concept of social capital to designate a set of intangible resources in families and communities that help to promote the social development of young people, but it was the sociologist Pierre Bourdieu (1986) who pointed out its broader relevance to human society. According to Bourdieu and Wacquant (1992, p. 119), "Social capital is the sum of the resources, actual or virtual, that accrue to an individual or a group by virtue of possessing a durable network of more or less institutionalized relationships of mutual acquaintance and recognition."

The key characteristic of social capital is its convertibility—it may be translated into other forms of capital, notably financial (Harker, Mahar, and Wilkes 1990). People gain access to social capital through membership in interpersonal networks and social institutions and then convert it into other forms of capital to improve or maintain their position in society (Bourdieu 1986; Coleman 1988). Although Portes and Sensenbrenner (1993) point out that social capital may have negative as well as positive consequences, theorists have generally emphasized the positive role it plays in the acquisition and accumulation of other forms of capital (see Coleman 1990), an emphasis that has been particularly strong in migration research.

Migrant networks are sets of interpersonal ties that connect migrants, former migrants, and nonmigrants to one another through relations of kinship, friendship, and shared community origin. Network connections increase the likelihood of international migration because they lower the costs and risks of movement and increase the expected net returns to migration. Having a tie to someone who has migrated yields social capital that people can draw upon to gain access to an important kind of financial capital, that is, high foreign wages, which offer the possibility of accumulating savings abroad and sending remittances home.

As early as the 1920s, sociologists recognized the importance of networks in promoting international movement (see Thomas and Znaniecki 1918–20; Gamio 1930). Although Taylor (1986, 1987) characterized network ties as a source of "migration capital," Massey et al. (1987, p. 170) appear to have been the first to label migrant networks specifically as a source of social capital. Following Coleman's (1990, p. 304) dictum that "social capital . . . is created when the relations among persons change in ways that facilitate action," they identified migration itself as the catalyst for change. Everyday ties of friendship and kinship provide few advantages, in and of themselves, to people seeking to migrate abroad. Once someone in a person's network migrates, however, the ties are transformed into a resource to gain access to foreign employment and the money that it brings. Each act of migration creates social capital among people to whom the new migrant is related, thereby raising their own odds of out-migration (Massey et al. 1987; Massey, Goldring, and Durand 1994).[2]

Thus, although there are a number of alternative renditions of the same idea, the key hypothesis is that social networks connections create conditions that facilitate the migration of others (decreasing costs, augmenting potential streams of future income, reducing risks, transmitting information). As a result, individuals who are related to migrants will, ceteris paribus, be more likely to migrate themselves. In what follows, we oftentimes refer to the observable correlation of migration risks across members of a social group as the "apparent" network effect since the correlation may also be observed in the absence of any social capital embedded in relations within a network, as described below.

COMPETING EXPLANATIONS

Despite the cogency of this argument, there are several plausible alternative explanations for the fact that people related to migrants are more

[2] Due to space constraints, we can only discuss a few testable propositions about migration risks derived from recent research on social networks as social capital. The literature is broad and rich and covers experiences in very diverse geographic settings. For comprehensive reviews, see Massey et al. (1998) and also in Hugo (1981).

likely to migrate themselves. Whereas some of these explanations predict a close association between the migratory behavior of individuals connected by close family or household ties, others account for the commonality of migratory behavior within a broader set of people linked more loosely by kinship, friendship, or community origin. In either case, however, the association of migration risks among individuals who belong to a social group is expected not—as the social capital hypothesis would have it—because the behavior of one influences the behavior of the others via the formation of social capital, but due to the influence of conditions that are *shared* by individuals in the group. In what follows, we review the most important competing explanations for apparent network effects.

Human Capital

To the extent that people living in the same social group share characteristics that influence the costs and benefits of international migration, the conventional human capital model predicts that migration decisions will be correlated among friends, relatives, and even community members. The key argument is that the migratory behaviors are correlated because they share *common characteristics and constraints* that influence the expected net return to migration, and, hence, the likelihood of its occurrence. According to this line of reasoning, if one could somehow remove the influences of shared human capital characteristics, then the association between migratory behaviors of related individuals would be reduced or eliminated.

Joint Decision Making

Unlike the foregoing explanation, the model of family income maximization assumes that household members jointly formulate a strategy to maximize household (rather than individual) income. The family collectively chooses members to move in a particular order so as to earn the highest *total* household income, yielding an apparent "chain migration effect," whereby the migration of one household member seems to raise the likelihood that others will follow. In reality, however, the observed network effect does not stem from the effect of household members on one another, but from the correlation of behaviors within the household as a result of joint decision making to develop a common strategy that governs individual actions.

A prominent version of this model, proposed by Borjas and Bronars (1991), assumes that household members jointly formulate an optimal allocation of family workers to potential productive activities, including migration. Depending on whether the income at potential destination areas is distributed more or less equally than in the origin area, the first link in the migration chain is selected with great care. No matter which member goes first, however, the family knows that migration costs for subsequent members will be less than for the first migrant. A jointly maximizing family incorporates this knowledge into its decision before anyone migrates, picking an optimal chaining pattern that amortizes the costs of migration over all family members.

Thus, the observed correlation of migratory behavior among individuals within the same household may only reflect the fact that they are responding jointly to common conditions that impinge on the household exogenously, yielding another version of the "common cause" hypothesis already mentioned. No value is assigned to network relationships themselves. Rather, members of a household share an elevated risk of migration because they formulate a common strategy in response to a single set of economic exigencies, not because social ties facilitate migration. Note, however, that in contrast to human capital theory, this theory does not necessarily predict a *positive* correlation between the migration risks of different household members, as the coordination of behavior to maximize income could require some members to stay at home while others are selected to migrate. This may occur, for example, when a household owns a productive enterprise that calls for overseeing by trusted family members. In this case, some family members must remain while others will be free to migrate.

Risk Diversification

In contrast to the neoclassical economic model developed by Borjas and Bronars, the new economics of labor migration model proposed by Stark and others postulates that households operate not only to maximize income, but also to *minimize risk* (David 1974; Stark 1991). According to this conceptualization, migration offers a means of diversifying income to manage households' risk exposure. In the same way that investors diversify their holdings to limit their exposure to loss, households diversify the allocation of workers to different productive activities in different places. The strategy requires only that earnings at points of origin and destination be uncorrelated, or better yet, inversely correlated. Given a negative association between business cycles in sending and receiving societies, a household will not be greatly harmed by recession at home, since one or more family members will be earning high wages abroad and can remit a portion of their earnings back to the household.

Social networks render migration practical as a means of risk diversification (Taylor 1986). When migrant networks are well-developed, they put a destination job within easy reach of most community members, making emigration a reliable and relatively risk-free resource (Massey et al. 1987). As a result, migration is more likely under conditions of strong than weak network ties. As in the Borjas and Bronars model, diversification may necessitate different timings of movement for different individuals, possibly yielding a negative correlation between migration decisions within households.

Selection

One final explanation for network effects rests on the fact that people become enmeshed in social networks through nonrandom selection processes. Social and economic variables that determine a person's network membership simultaneously influence the propensity to migrate, thus creating a spurious association between the two outcomes. According to this line of reasoning, the migration of one household member does not influence others' migration risks. Rather, the observed association is due to a common underlying process of selection. Such a mechanism is particularly plausible where there is a substantial amount of room for personal choice to operate, as in networks based on friendship or, to a lesser extent, on shared community of origin. It is much less likely that this mechanism will be of any significance when social networks are based on kin ties.[3] As in the joint decision-making model, the selection hypothesis does not assign any intrinsic value to social relations themselves but underscores the importance of common underlying processes that simultaneously influence decisions made by different family members.

THE BURDEN OF PROOF

Whereas social capital theory hypothesizes that movement by one person *directly influences* the odds of movement by others within the social network, we have specified four equally plausible mechanisms leading to the same prediction: that people within common social groupings are subject to common human capital influences; that moves may be coor-

[3] Admittedly, even within nuclear families where membership is not a matter of choice, some selection forces can operate. In fact, it is known that health status conditions are partially shared by members of the same family or those living in the same household and, in turn, that health status affects the risks of migration. In this case, selection of migrants on health status creates a correlation between their migration experience. It should be noted, however, that this situation is indistinguishable from one where family or household members have similar human capital, with health status being one of the defining elements of human capital. It then follows that if we are able to reduce the influence of unmeasured factors shared by members of the household, we will simultaneously reduce selection effects due to health conditions.

dinated as a result of a joint household decision to maximize income; that moves may be linked as a result of a joint household strategy to diversify risk; and that moves are interrelated because factors that select individuals into common social networks also select on the propensity to migrate.

How can we tell these alternative explanations apart? Inferences about network effects are typically based on qualitative or quantitative studies, which show that having a tie to a current or former migrant raises a person's odds of out-migration, controlling for the influence of various individual, household, and community characteristics. In quantitative studies, for example, a dichotomous indicator of migration is regressed on a set of measured covariates plus one or more network indicators that are defined a priori—whether certain family members are current or past migrants, the number of friends or acquaintances who have ever migrated, the fraction of a community's inhabitants with prior migrant experience, and so on. If the network indicator displays a positive association with the odds of out-migration—either in the cross section (Espinosa and Massey 1998) or longitudinally (Massey and Espinosa 1997)—then ceteris paribus one infers a network effect (i.e., that the social tie has operated directly to promote the subject's migration). This commonly used strategy, however, has three distinct shortcomings.

Spuriousness and Selection

The conventional strategy does not rule out common effects. In order to infer the existence of a direct effect, as claimed by the social capital theory, it is essential to remove the influence of conditions that are common to individuals in a network. Since two individuals linked by kinship or friendship will typically share common characteristics that influence migration, these must be controlled before any causal influence can be assigned to the network tie per se. Although many characteristics are easily measured and can thus be included in statistical models as controls, inevitably some common factors are not so easily measured (health status, attitudes, motivations, beliefs) and are not so easily subject to statistical control. In the presence of unmeasured heterogeneity, the usual method of inferring network effects is not sufficient to eliminate competing explanations.

By the same token, rarely if ever are potential selection effects addressed at all. Although this is much less of a problem when social networks under observation are defined by household or kin ties, selection may have some influence within networks that emerge in other social domains.

Completeness

Without exception, conventional efforts to test the social capital/network model have relied on a single test to assess the direction and magnitude of the association between migratory behaviors of individuals within a social network. Yet the validity of social capital theory cannot rest on a single test. This is because the theory implies and predicts other empirical regularities that should be assessed as well. Insofar as these predicted regularities are not observed, or if observed are inconsistent with competing theories, they can be used to falsify social capital theory or eliminate rival explanations

Multiplicity of Social Networks

Conventional strategies typically only probe the significance of one realm of social relations at a time. This practice is usually associated with shortcomings in the information available to researchers and can lead to inconclusive results, particularly when no tests of alternative predictions are simultaneously carried out. For example, even if all shared conditions among related individuals can be measured and statistically controlled, both the joint maximization and risk diversification perspectives still predict a correlation between migratory behaviors *within households*. If these theoretical accounts are valid, some or all of the association of migration risks among members of a household-based network that remain after controlling for shared conditions may simply reflect the fact that household members act collectively to derive a joint strategy of migration that they subsequently implement.

Thus, even if it were possible to strip the observed relationship of the effects of shared conditions, methodologies typically used to infer network effects cannot eliminate the counter-hypotheses of risk diversification and joint decision making. A strong means of adjudicating between these counter-hypotheses and social capital theory is to demonstrate that, net of shared conditions, there is an association between migration risks of individuals who share the same social networks *but not the same family or households* (see above). This is a demanding test because it requires information on network connections across several social domains.

In the absence of conditions to implement a strong test, one can derive and verify the validity of *corollaries* from social capital theory that are not predicted by the joint-decision and risk diversification models. Thus, to the extent that we eliminate the second weakness—testing corollaries is a means of achieving completeness—we may also be able to eliminate or attenuate the relevance of the third shortcoming. It should be noted, however, that this is a weaker means of discriminating between theories than the one involving joint evidence from different social domains. In what follows, we develop a model that enables us to bypass the spuriousness and selection shortcoming. Because we only have information pertaining to a single social domain (the family), we cannot eliminate the multiplicity of social networks shortcoming. Instead, we are able to test three corollaries from social capital theory neither of which is implied by competing models. This is a solution to the completeness shortcoming and provides a weak solution to the multiplicity of social networks problems.

MODELS AND ESTIMATIONS OF SOCIAL NETWORKS EFFECTS

We adapt recently developed hazards techniques to derive models capable of eliminating rival explanations (Clayton 1978; Hougaard 1986; Clayton and Cuzick 1985; Yashin and Iachine 1997). These models permit us to retrieve fixed and time-dependent effects on the *joint migratory risks* of two members of a social dyad while simultaneously controlling for the effect of unmeasured common conditions. They establish a relation between the *timing* of movement by each party in the social relationship to four basic factors: (1) measured conditions characteristics of each individual; (2) common measured conditions and characteristics; (3) unmeasured common conditions and characteristics; and (4) the effect of the migration of one member of the pair on the timing of migration by the other. Our basic methodological problem is how to distinguish between these various effects, determine their direction, and estimate their magnitude.

Although this problem is certainly not new in social science, its solution is not obvious and requires the application of special models and procedures. Neither the theories discussed above nor the models we introduce below necessitate that we focus only on the timing of *first* migration, but doing so offers the advantage of not requiring us to model an interrelated sequence of events or to fine-tune data on the timing of first, second, third, and higher order moves. To be sure, a thorough test of competing explanations ultimately *should* examine such sequences and their interrelations. Our objective is more modest: we only evaluate whether or not the initial migration of one family member influences the timing of movement by another.

A Naive Model

Let us begin with the simplest case. Suppose that $Y_{ij}(t)$ is a dichotomous variable representing the *first* migratory experience of individual i in social group j at time t. It attains a value of "1" if the first migration occurred by time t and "0" otherwise, where t represents the time elapsed since a suitably defined point of origin for the first migration process. People within social group j are related to one another and could be expected to influence one another's behavior. If one person in j migrates, then we hypothesize that the risks of first migration increase for other members of the social group because of the theoretically expected mutual influences derived from social capital theory. The social bonds that define membership in j constitute connections within the migrant network.

For example, j may indicate membership in a household wherein people are related by kinship. Migratory behavior of various members of the household, husbands and wives, fathers and sons, brothers and sisters, and so on are thus related to each other. Given this conceptualization of network migration, we can specify the following simple model:

$$Y_{ij}(t) = \alpha X_{ij} + \beta Z_j + \gamma M_j + \epsilon_{ij}, \qquad (1)$$

where X_{ij} is a vector of characteristics for individual i in household j, Z_j stands for shared characteristics of the household that may affect migration risks of all its members, M_j is a vector of indicators of migration experience of other household members, α, β, and γ are vectors of effects, and ϵ_{ij} is an error term. The α-effects are associated with individual characteristics, the β-effects are associated with conditions shared by members of the household, and the γ-effects measure the influence of migration of other members of the household. That is, β-effects and γ-effects are estimates of the contributions of shared conditions and family networks respectively.

The model presented in equation (1), however, presents a number of serious estimation problems. The most important is the likely existence of unmeasured characteristics correlated with M_j. Relevant unmeasured factors are common conditions that should have been included in the subvector Z_j, but which have not been measured for some reason (e.g., cost, practicality, convenience). The consequence of such omissions is inconsistent estimates of γ, the effects of migration experience among members in the kin network. More generally, the presence of unmeasured common causes makes infeasible the identification of social network effects.

A Bivariate Hazards Model

Bivariate hazards models were developed to study two survival processes affected by common conditions as well as mutual influences. Suppose two individuals in a household have migration risks (or hazards) defined by $\mu_{1j}(t_1|X_{1j},Z_j,W_j)$ and $\mu_{2j}(t_2|X_{2j},Z_j,W_j)$. In this notation, X_{ij} refers to a vector of individual characteristics, either fixed or time-dependent, that are associated with each member of the pair in household j; Z_j includes common characteristics of the household or community, again either fixed or time-dependent; and W_j contains unmeasured fixed characteristics of the household. These risks are expressed by the following two equations:

$$\mu_{1j}(t_1) = \alpha_1 X_{1j} + \beta_1 Z_j + \delta W_j + \epsilon_1, \tag{2}$$

$$\mu_{2j}(t_2) = \alpha_2 X_{2j} + \beta_2 Z_j + \delta W_j + \epsilon_2. \tag{3}$$

Although this model allows the estimation of effects (α's and β's) net of the influence of common measured and unmeasured characteristics, it suffers from two key limitations. First, using procedures developed by Clayton (1978) and Clayton and Cuzick (1985), the model is theoretically estimable *only in the absence of reciprocal influences* (Mare and Palloni 1988). If such reciprocal influences are strong—and this is precisely the social capital hypothesis—then the estimates of the effects of covariates will be inconsistent. Second, in order to find a tractable solution, Clayton postulates a parametric form to describe the effect of unmeasured conditions, W_j. The problem is that the estimates of the α's and β's are very sensitive to the actual specification of this distribution.

A Flexible Multistate Model

To estimate the effects of reciprocal behavioral influences in the process of first migration while simultaneously controlling for the influence of shared conditions, we propose a multiple hazards model. This model improves on the naive approach by virtue of its power to control for measured and unmeasured conditions. It improves upon the bivariate parametric approach in that it enables us to retrieve estimates of reciprocal influences that are not sensitive to parametric specification of unmeasured conditions.

Again, we consider the case of paired household members and for simplicity work with the example of two siblings. As illustrated in figure 1, at any time t, we can identify four distinct states with respect to the timing of first migration by two siblings: neither sibling has migrated, the oldest sibling has migrated but not the younger one, the youngest sibling has migrated but not the oldest, and both siblings have migrated. Hazards associated with flows into and out of the four states can be represented either parametrically or nonparametrically, and each hazard is defined as a function of both individual and shared characteristics:

$$\mu_{1j}(t_1) = \alpha_1 X_{1j} + \beta_1 Z_j + W_j + \epsilon_1, \tag{4}$$

$$\mu_{2j}(t_2) = \alpha_2 X_{2j} + \beta_2 Z_j + W_j + \epsilon_2, \tag{5}$$

$$\mu_{3j}(t_3) = \alpha_3 X_{3j} + \beta_3 Z_j + W_j + \epsilon_3, \tag{6}$$

$$\mu_{4j}(t_4) = \alpha_4 X_{4j} + \beta_4 Z_j + W_j + \epsilon_4, \tag{7}$$

$$\mu_{5j}(t_5) = \alpha_5 X_{5j} + \beta_5 Z_j + W_j + \epsilon_5, \tag{8}$$

where the four hazards correspond to the four states just described, X_{ij} refers to the measured characteristics of person i in household j, Z_j indicates measured common characteristics of household j, and W_j indicates unmeasured household characteristics. As before, α_i and β_i, are vectors of parameters

This representation yields an adequate basis for estimation provided we can resolve one minor difficulty. This occurs when the hazard for one member of the pair is effectively zero even though the hazard for the other is not—for example, if an older sibling is exposed to a nonzero risk of migrating but a younger sibling is not exposed at all (because he or she is too young). This circumstance violates the proportionality of hazards assumption and leads to inconsistent estimates. To resolve the problem, we start the clock of the process only *after* the youngest member of the sibling pair reaches the minimum age for migration, which we assume to be 15 (see below).

This model specification has several appealing features. First, because the units of observation are pairs of actors rather than individuals (e.g., older and younger siblings), the representation of hazards tolerates the inclusion of unobserved characteristics associated with the pair, thus facilitating estimation while controlling for shared unmeasured conditions. Second, the unmeasured component can be represented either as parametric or nonparametric rather than being constrained to a narrow range of parametric forms (as in the bivariate hazard model). Third, and most important, the effects of the timing of the event for one member of the pair on the timing of the event for the other can be evaluated in rather simple ways. For example, if the initial migration of the oldest sibling has an important influence on the timing of first migration of a younger sibling, then the difference between $\mu_{2j}(t_1)$ and $\mu_{4j}(t_4)$ should be discernible from the parameters for the corresponding baseline hazards (when all relevant characteristics and their effects are the same across siblings and

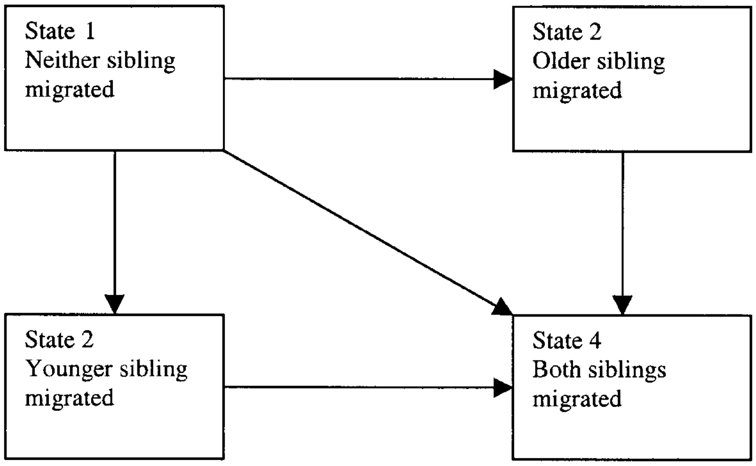

FIG. 1.—A multistate representation of siblings' migration

transitions). Alternatively, we can use global likelihood ratio statistics to test the equality of parameters for the two baseline hazards.

Using the Multistate Model to Discriminate between Competing Theories

The data available to us pertains to households. As a consequence, we apply the multistate model described above to estimate social networks effects in only one social domain, the coresidential family. In particular, we investigate the effects of one sibling's migration on other siblings' migration risks. The multistate model enables us to estimate these effects while controlling for measured characteristics reflecting human capital (of siblings and households) and for unmeasured conditions shared by siblings. Thus, the estimated effects are net of the influence of spurious association and of selection effects triggered by some or all of the unmeasured shared conditions (see n. 2). The direction of these effects, their magnitude, and their significance provide evidence for social networks effects and enable us to test social capital theory.

Because of the nature of the data, however, we cannot deploy a strong test to adjudicate between social capital theory, on the one hand, and joint decision-making and risk diversification theories, on the other. This strong test requires us to show that social networks effects prevail in some social domains other than the family. We can, however, use a weak test and check the validity of corollaries derived from social capital theory that are not consistent with competing theories. This is a minimum requirement to distinguish social capital theories from the other theories.

The first corollary can be formulated as follows: to the extent that social capital can be deployed to resolve problems faced by migrants, apparent network effects should be greater during periods when migration becomes more costly and difficult. In particular, if social capital indeed becomes more valuable during periods of tighter border enforcement, we should observe larger effects of social networks. In the case of Mexico-U.S. migration, for example, we would gain greater confidence in our hypothesis if the size of network effects increased *after* the passage of the Immigration Reform and Control Act of 1986, which launched a substantial build-up of enforcement resources along the Mexico-U.S. border, criminalized the hiring of undocumented migrants, and authorized the U.S. Department of Labor to expand internal inspections (Singer and Massey 1998; Phillips and Massey 1999).[4]

The second corollary regards the influence of social capital located *outside* the household on individual migration risks. Individuals who live in communities where migration is more prevalent are more likely to participate in nonfamily networks involving migrants and, consequently, to tap sources of social capital located outside the household. It follows that the persistence of effects of community migration prevalence on individual migration risks (after controlling for measured and unmeasured individual and household conditions) is prima facie evidence of the importance of social capital over and above factors implied by the joint decision-making and risk diversification theories. Indeed, neither of these perspectives predicts an effect for social capital located *outside* the household (once household conditions are controlled).

The third corollary involves the migration experience of the household head. As mentioned before, the correlation of migration risks across siblings, net of the effects of measured and unmeasured conditions, is also expected under the joint household and risk diversification theories because under these theories risks of migration are subject to household coordination. However, if we are able to control for migration behavior of other members of the household, we would expect that the apparent social network effects disappear: if the only factor accounting for correlation between migration risks of any two siblings is the existence of household coordination, it should vanish once we control for the migration behavior of *all* household members. This is a tall order, so we only pursue a shortcut and control for migration behavior of the father. Under social

[4] Full documentation of these data, the questionnaires, and the samples, along with the files themselves, are available from the MMP Web site: http://www.pop.upenn.edu/mexmig/

capital theory, father's previous migration also creates social capital and should exert an effect on the migration risks of both siblings, *but it should not do so by attenuating the effects of one sibling's migration on the migration risks of the other.*

In what follows, we show that our estimates support the existence of social networks effects. We do not take this as evidence that human capital theory is irrelevant, but as an indication that social capital is also a strategic condition promoting the process of migration. Similarly, because we are not able to implement a strong test to discriminate between social capital and the other competing theories, we can only claim that the observed social network effects are weakly distinguishable from effects that would be observed if the processes of joint decision making or risk diversification took place in the absence of social capital effects

DATA, MEASURES, AND METHODS

Our data come from the Mexican Migrant Project (MMP), whose database at the time of the analysis included samples of 39 communities located in the states of Jalisco, Michoacán, Guanajuato, Nayarit, and Zacatecas. Together, these states constitute a region (Western Mexico) that historically has sent a majority of migrants to the United States (Durand, Massey, and Zenteno 2001). The data set also includes one additional community from the state of Guerrero, a newer migrant-sending location in the central region to the south of Mexico City (other communities from this region are in the process of being added to the file).

Characteristics of the Data

Respondents were interviewed in 1982–83 and in successive years from 1987 to 1995 using an ethnosurvey questionnaire that collected information about the social, economic, and demographic characteristics of the head, the spouse, the head's children, and other household members (see Massey and Zenteno 2000). Information was compiled for *all* children of the household head regardless of age or where they lived (determining independently whether each child still lived in the respondent household). Among the data gathered from each son or daughter was the date of his or her first trip to the United States. Each household head also provided a complete life history that included separate histories of marriage, fertility, labor, home ownership, land ownership, and business ownership.

Within each community, the typical sample consisted of 200 households, although in smaller settlements, fewer households were chosen, and in some cases, larger samples were compiled. Sampling frames were constructed by conducting a house-to-house census at each site. Usually an entire town or city was canvassed, but in large urban areas, this was not possible and specific working-class neighborhoods were demarcated and sampled instead. Sampling fractions ranged from 0.029 to 0.803 and averaged about 0.228. Refusal rates varied from 0 to 0.152 and average 0.062. Higher refusal rates generally occurred because of local political circumstances rather than suspicions about the study per se.

In choosing the communities, investigators sought to include a range of population sizes, ethnic compositions, and economic bases. Communities were *not* chosen because they were thought to contain U.S. migrants, and the data set in fact includes a wide range of migratory prevalence ratios, ranging from one community where just 9% of adults have been to the United States to another where 60% have migrated (Massey et al. 1994). Although the sample is not strictly representative of the states of western Mexico, it nonetheless incorporates a broad cross section of households and communities in the region and yields a sample of U.S. migrants whose characteristics are remarkably similar to those enumerated in representative surveys (Zenteno and Massey 1999; Massey and Zenteno 2000).[5]

Since our method focuses on the migratory behavior of sibling pairs, we select all households containing at least two siblings over the age of 15, assuming that beyond this age people tend to make their own migration decisions rather than simply following their parents. From these households, we select the oldest sibling as a reference point and randomly choose a younger sibling for the second person in the pair. To the extent that the age distribution of siblings in a household is related to their migratory behavior (via fertility effects or the age distribution of parents), our sample of pairs may be biased somewhat by selection, but we do not see this as a serious problem. We limit our analysis to siblings enumerated in Mexico to yield comparable measures of employment and occupational status for all brothers and sisters. Cases where one of the sibling pair was born in the United States were excluded to eliminate return migration as an extraneous effect. Finally, so as to focus on recent migratory experience

[5] This corollary is not only a logical deduction from social capital theory, but is also suggested by past research (Donato, Durand, and Massey 1992; Massey and Espinosa 1997; Singer and Massey 1998). Accordingly, restrictive policies implemented in the wake of the Immigration Reform and Control Act of 1986 (IRCA) have had little effect in reducing the odds of leaving on an initial undocumented trip, taking an additional trip without documents, or crossing the border surreptitiously. These outcomes could only occur if the effect of social networks became stronger after IRCA's implementation, thus offsetting the increase in detection equipment and enforcement personnel along the border. To test this idea, we will divide years of observation into those before IRCA (1986 or earlier) and those afterward (1987+) and check whether or not the effect of having a tie to a migrant sibling increases over time.

TABLE 1
Observed Transition Matrix of Male Sibling Pairs Selected from 39 Mexican Communities

State of Origin	State of Destination (1)	(2)	(3)	(4)
Frequencies:				
1. Neither brother migrated	2,248	324	342	24
2. Older brother migrated	. . .	438	0	206
3. Younger brother migrated ...			307	35
4. Both brothers migrated			. . .	265
Crude probabilities:				
1. Neither brother migrated	.765	.110	.116	.008
2. Older brother migrated	. . .	.680	.000	.320
3. Younger brother migrated ...			.898	.102
4. Both brothers migrated			. . .	1.00

and minimize recall error, we deleted cases where the oldest sibling was over age 50.

These deletions give us a sample of 3,258 sibling pairs. Because we know the date of each person's first trip to the United States, we can derive a transition matrix that counts the frequency of moves associated with each pathway depicted in figure 1. The frequencies of this transition matrix are displayed in the upper panel of table 1. Beginning when the youngest sibling turns 15, we follow both persons in the pair and observe the timing of first migration. Cases of left censoring (where the older sibling migrated before the younger one reached age 15) originate in state 2 and do not violate the assumption of proportional hazards. Of these left-censored cases, 204 remained in state 2 for the entire observation period and 90 proceeded to state 4 (i.e., the younger sibling migrated).

In total, we counted 4,189 interstate transitions among sibling pairs during the observation period. The diagonal of the transition matrix contains instances of state *immobility*. In 2,248 cases, neither sibling migrated between the time the youngest turned 15 and the survey date. In 265 cases, both siblings migrated in the same year; in 307 cases, the younger sibling migrated first whereas the older never left; and in 438 cases, the older sibling migrated first while the younger never left.

The off-diagonal cells, in contrast, indicate moves *between* states during the observation period. Of the 931 changes of state catalogued in the observation period, there were 324 moves from state 1 to state 2 (neither migrated to oldest migrated), 342 moves from state 1 to state 3 (neither migrated to youngest migrated), and 24 moves from state 1 to state 4 (neither migrated to both migrated). In addition, 206 moved from state

2 to state 4 (youngest followed oldest), and 35 went from state 3 to state 4 (oldest followed youngest).

Dividing cell frequencies by the row totals yields crude probabilities associated with each transition.[6] These are shown in the lower panel of table 1. Among those pairs reaching age 15 without either sibling migrating (yielding no social capital for either person to draw upon), younger and older siblings had roughly *the same* likelihood of migrating: the probability that the older sibling left first was 0.110, whereas the likelihood that the younger left first was 0.116. Having an older migrant sibling, however, *almost triples* the likelihood that the younger sibling will migrate, raising the exit probability from 0.116 to 0.320. This higher risk of out-migration means that having a tie to an older migrant sibling significantly lowers the waiting time to first migration. The effect of a network tie to a migrant sibling appears to be asymmetrical with respect to age, however, as having a *younger* sibling with U.S. experience does *not* increase the likelihood of out-migration for the older sibling. Indeed, at 0.102, the probability remains roughly the same as for those lacking social capital entirely (0.110 and 0.116).

In general, these transition probabilities are consistent with the network hypothesis of social capital theory. As we have pointed out, however, they are also consistent with other plausible explanations. In order to isolate the independent effect of social capital, we need to control for the effects of individual and shared characteristics using the multistate hazard model derived above. To identify the model in equations (4)–(7), we selected a set of tractable indicators of individual and shared characteristics from the MMP data set.

Measures

Table 2 shows means and standard deviations for *individual* characteristics of the siblings, including gender, education, and occupational status. Table 3 displays means and standard deviations for shared conditions. These include traits of the household head that are assumed to be fixed (gender, education, and occupational status), as well as time-varying indicators of household wealth (ownership of farmland, real estate, or business enterprises).

To assess variability of individual human capital, we use the educational and occupational status of siblings. A comparison between figures in tables 2 and 3 shows that educational levels improve sharply between parental and sibling generations. Whereas nearly two-thirds of household heads

[6] These probabilities are "crude" because they are calculated without proper adjustments for competing risks.

TABLE 2
INDIVIDUAL CHARACTERISTICS OF PERSONS IN SIBLING PAIRS SELECTED FROM 39 MEXICAN COMMUNITIES

	YOUNGER SIBLING		OLDER SIBLING	
VARIABLE	Mean	SD	Mean	SD
Gender:				
Male	.502	.500	.510	.500
Education:				
0–3 years	.153	.360	.211	.408
4–9 years	.615	.487	.534	.499
10+ years	.232	.422	.256	.436
Occupational status:				
Unemployed	.313	.464	.336	.473
Unskilled	.236	.425	.220	.415
Skilled	.299	.458	.370	.473

NOTE.—For fixed covariates evaluated at the time of the baseline survey, gender = 1 male, 0 otherwise; education = 1 if 4 or more years, 0 otherwise; occupation = 1 if skilled, 0 otherwise. N of sibling pairs = 3,258.

(64.9%) had three or fewer years of schooling, among their offspring, only 21.1% of older siblings and 15.3% of younger siblings had such low levels of education. Likewise, whereas roughly a quarter of all siblings had 10 or more years of schooling (23.2% of the younger ones and 25.6% of those who were older), the figure for household heads was only 5.2%. By contrast, the distribution by occupation is more favorable to fathers than siblings, probably reflecting longer labor force experience among parents. In fact, a substantial number of the siblings are still only teenagers, so it should not surprise us that their unemployment rates are much higher than their parents. Whereas roughly a third of the siblings were unemployed at the time of the survey (31.3% of the younger and 33.6% of the older siblings), only 23% of household heads lacked a job. Nearly half of all household heads (49.4%) held a skilled occupation, compared with only 37% of older siblings and 30% of younger siblings.[7]

[7] In these analyses, we consider employment and occupational status as a single variable because the structure of the data prevents their separation. The "unemployed" category includes those jobless but looking for work, but also homemakers, students, and people out of the labor force for other reasons. Siblings and parental occupational status at the time of interview are not the ideal measures of human capital we seek since they may reflect the acquisition of skills that resulted from migration itself. To the extent that improvements in occupational skills are related to underlying abilities and resourcefulness, however, the indicator will perform its role, albeit crudely. If, however, changes are strongly related to migration experience and only partially reflect the influence of innate skills and abilities, the effects of this variable will be inconsistent. Our efforts to include a time-dependent version of individuals' occupation were not successful due to the relatively large number of cases with unknown values for periods

TABLE 3
HOUSEHOLD CHARACTERISTICS OF SIBLING PAIRS SELECTED FROM 39 MEXICAN COMMUNITIES

Variable	Mean	SD
Head's gender:		
Male	.850	.150
Head's education:		
0–3 Years	.649	.477
4–9 Years	.299	.458
10+ Years	.052	.222
Head's occupational status:		
Unemployed	.229	.420
Unskilled	.277	.448
Skilled	.494	.500
Household wealth:		
Owns farm land	.156	.363
Owns real estate	.671	.470
Owns business	.361	.480
Social networks:		
Father a migrant	.350	.102
Prevalence in community	.420	.110

NOTE.—For fixed covariates, head's gender = 1 male, 0 otherwise; head's education = 1 if 4 or more years, 0 otherwise; head's occupation = 1 if skilled, 0 otherwise. For time-dependent covariates, farm land = 1 if own farmland, 0 otherwise; real estate = 1 if owns real estate, 0 otherwise; owned business = 1 if owned business, 0 otherwise; father's migration = 1 if father experienced migration, 0 otherwise; prevalence of migration = 1 if prevalence of adult migration in community exceeds first quartile of distribution (for year of exposure). N of sibling pairs = 3,258.

Although gender is not a human capital variable proper, it represents an important individual characteristic that proxies for the different roles assigned to males and females in Mexican society (Lewis 1960, p. 54–68; Diaz 1966, pp. 76–93; Foster 1967, pp. 55–86). Traditionally, it is men who migrate first, and when women do migrate, they typically go either as members of a couple or a larger family unit (Massey et al. 1987; Rouse 1991, 1992; Durand and Massey 1992). Thus, although having a Y chromosome does not endow individuals with naturally low or high levels of human capital, female gender nonetheless operates as a constraint by raising the emotional and social costs of migration (Alarcón 1992; Hondagneu-Sotelo1994; Goldring 1995). As expected, the gender distribution of siblings is relatively balanced.

Age is a potentially important individual variable because it reflects

before the baseline survey. Another limitation of these measures of social capital is that we cannot gauge joint effects of quality and quantity of social capital since we do not have suitable measures to do so.

accumulated experience and belongs in the model as a dimension of human capital. The ages of individual siblings are not entered as covariates when modeling transitions originating in state 1 because the process is assumed, by definition, to start at age 15. Thus, the baseline risk reflects effects of age, including those associated with human capital traits that are not measured by education or occupation. Preliminary analyses with controls for sibling's age at first trip in transitions originating in states 2 and 3 showed that its effects were trivial and were dropped from further consideration.[8]

As argued earlier, gender captures unmeasured factors likely to influence the propensity to migrate. In keeping with Mexico's patriarchal culture, the vast majority of household heads in our sample are male. Typically females are designated as heads only when the male is absent because of death, divorce, or abandonment, leaving the household vulnerable to risk of poverty. Other things equal, therefore, one might expect siblings from female-headed households to experience a higher risk of outmigration.

Three of the common conditions are time-varying covariates. First, timing of father's migration is entered to check the validity of the second corollary. It is also a condition that affects the social capital for children in the household and is thus expected to have its own influence on migration risks.[9]

Second, to test for the validity of the third corollary, we include a measure of the prevalence of migration within the community. The MMP data allow the computation of a time-varying estimate of the proportion of community members age 15+ who have ever been to the United States (see Massey, Goldring, and Durand 1994). From this information, we are able to construct a time-dependent covariate that attains a value of "1" if, at the beginning of any calendar year of exposure to migration risks, the prevalence of migration experience among those age 15+ in the community of residence migration exceeded the first quartile of the distribution. We use a time-dependent covariate because aggregate migration experience in the community changes over time as a result of individual migration experiences. The corollary asserts that if social capital theory holds, prevalence of migration in the community should exert an influence

[8] Other individual variables of theoretical interest that proved to be of trivial empirical significance in the models, are birth order of the youngest child and documentation status (on first trip) of either sibling. They too were dropped from consideration.

[9] Note that father's migration is one way of producing a female-headed household; therefore, it is a necessary control to assess the net effects, if any, of female headedness. As was the case for siblings' documentation on first trip, father's documentation was dropped from the analysis since it did not contribute significantly to it.

on the individual propensity to migrate *over and above* the effects of network ties within the household.[10]

Third and finally, possession of assets in the form of property is introduced as a control since such household characteristic could inhibit migration among some of the children (the youngest in a stem-family system) and promote it among others (the older ones). Since assets are subject to change as a function of migration experience, the variables for possession of property are time dependent and defined as of the beginning of each year of exposure to migration.

Model Estimation

We estimate the model using CTM, a maximum-likelihood program developed by George Yates, James Heckman, and James Walker precisely to estimate generalized continuous hazard models. We employ a Weibull baseline hazard to represent the time dependence of the risk of migrating, a flexible, monotonic functional form that requires only a level and a slope parameter. To test the sensitivity of our estimates to this functional form, we tried different specifications (piecewise exponential, Gompertz, and quadratic functions), but our main results proved robust to changes in the baseline hazard.

The multistate model posits different baselines and effects for each transition. Since there are five different transitions, the simplest model requires 10 parameters to describe all five baseline hazards. For each individual variable we enter, there are potentially 10 different effects (one for each sibling in each of five transitions), whereas for each shared condition there are five parameters to estimate (one per transition). Estimates can thus proliferate very rapidly, but this turns out to be completely unnecessary. In fact, extensive tests of different model specifications showed that constrained models offer a parsimonious representation of the data. Constrained models are those where the effects of individual and shared characteristics are invariant across transitions and where the effects of individuals' characteristics on his or her own migration risks are the same as those on the sibling's migration risks.

[10] In early analyses, we also introduced controls for community size as another common characteristic, but these were ultimately dropped for lack of significance.

TABLE 4
BASIC RESULTS FOR MULTISTATE HAZARD MODELS OF INCREASING COMPLEXITY

Model	N	Parameters Estimated	Log Likelihood	χ^2	df	p-value
Model 1 (baseline)	3,258	10	−2,439.0	...	...	...
Model 2 = 1 + human capital:						
Unconstrained	3,258	25	−2,215.6			
Constrained	3,258	13	−2,221.2	11.6	12	.59
Model 3 = 2 + shared conditions:*						
Unconstrained	3,180	55	−2,205.3			
Constrained	3,180	19	−2,208.1	5.6	36	.99
Model 4 = 3 + father's migration and prevalence:†						
Unconstrained	3,180	63	−1,976.1			
Constrained	3,180	21	−1,983.2	14.2	42	.98
Model 5 = 4 + unmeasured heterogeneity:						
Unconstrained	3,180	65	−1,949.1			
Constrained	3,258	23	−1,958.2	18.2	42	.98

NOTE.—df and χ^2 reported only when goodness of fit of alternative models is carried out (unconstrained versus constrained models). χ^2 based goodness-of-fit test across models can also be performed except for the model 5 within which none of the others nests. Constrained models force estimates of a characteristic to be identical across all transitions. Individual characteristics are constrained to have identical effects across siblings from the start (tests not shown).

* Models 4 and 5 disregard transition 1 to 4.

† Models 4 and 5 are estimated on a reduced number of cases, a result of deletions due to missing information over time on shared characteristics, father's date of first migration, or community of residence.

RESULTS

Testing Model Constraints

Table 4 displays values of the log-likelihoods, chi-square statistics, degrees of freedom, and p-values for the main models we estimate.[11] The first model is our baseline model, and it only includes parameters for the five baseline hazards. The second model adds indicators of human capital; the third adds conditions shared by the siblings; the fourth adds two dichotomous (and time-varying) indicators, one for timing of father's mi-

[11] All our tests are based on likelihood ratio statistics, a reasonable choice when testing hypotheses that involve nested models. Alternative statistics (Akaike criterion and BIC) were also calculated but produced identical results, and we do not show them here. Although we are able to retrieve estimates for all 5 transitions in models 1–3, the increasing complexity of models 5 and 6 leads to unstable estimates for the parameters of the transition from state 1 to state 4. This is because of the very small number of events associated with the transition (see table 1). Throughout our discussion, we omit display of estimates for the transition even when it was possible to obtain them.

TABLE 5
ESTIMATES OF PARAMETERS FOR BASELINES HAZARD

	TRANSITION				
	1 to 2	1 to 3	2 to 4	3 to 4	1 to 4
Model 1:					
Intercept ...	−2.25*	−1.79*	−.73*	−1.96*	−5.00*
	(.09)	(.07)	(.12)	(.27)	(.40)
Slope	−.31*	.08	−.17*	−.10	−.41
	(.06)	(.06)	(.08)	(.18)	(.23)
Model 2:					
Intercept ...	−2.66*	−2.28*	−1.13*	−2.21*	−5.00*
	(.10)	(.09)	(.12)	(.26)	(.29)
Slope	−.25*	.16	−.07	−.06	−.41
	(.06)	(.06)	(.07)	(.18)	(.23)
Model 3:					
Intercept ...	−2.92*	−2.53*	−1.33*	−2.62*	−5.00*
	(.06)	(.06)	(.23)	(.68)	(.39)
Slope	−.25*	.16*	−.07	−.02	−.41
	(.06)	(.06)	(.07)	(.18)	(.23)
Model 4:					
Intercept ...	−3.07*	−2.71*	−2.09*	−2.62*	...
	(.16)	(.15)	(.17)	(.35)	...
Slope	−.21*	.18*	−.07	−.09	...
	(.06)	(.06)	(.08)	(.18)	...
Model 5:					
Intercept ...	−3.08*	−1.59*	−.11	−2.79*	...
	(.18)	(.23)	(.36)	(.36)	...
Slope	−.18*	.46*	.49*	−.004	...
	(.06)	(.09)	(.15)	(.18)	...

NOTE.—SEs are given in parentheses. Model 1 is the baseline model; 2 adds controls for human capital; 3 adds controls for shared conditions; 4 adds controls for social networks; 5 adds controls for heterogeneity.
* $P < .05$.

gration and one for community migration prevalence; and the last model introduces a control for unmeasured shared conditions (unmeasured heterogeneity). For models 2–5, we also estimate the corresponding constrained model where effects of variables are set to be identical across transitions. In all cases, the chi-square statistic for goodness of fit leads to acceptance of the hypothesis that effects are indeed identical across transitions or to acceptance of the constrained models. The estimates of parameters of the baseline hazards for each model are in table 5. (Table 7, below, displays the estimates associated with each of the variables included models 2–5.)

Testing the Main Hypothesis of Social Capital Theory

Resting on the simplified representation made possible by the parsimony of constrained models, we turn now to a test of the first hypothesis derived from the social capital theory. If social capital theory is valid, we expect to observe a marked difference in the risks of out-migration for individuals whose sibling has migrated compared with those whose sibling has not, *even after controlling for observed individual and shared conditions*. There are two procedures for doing this in the multistate model. The first is to test for the significance of differences between baseline parameters before and after controls for individual and shared characteristics are introduced. The second is to perform global likelihood ratio tests to assess the goodness of fit of models with constrained baseline parameters. We use each of these in turn.

Our purpose is to show that prior migration of a sibling exerts an important effect on the migration risk of the other. To evaluate this claim, we ask the following counterfactual: How much larger would the migration risk be for an individual whose sibling has not migrated if he or she had migrated? If the focal individual is a younger sibling, the answer is given by the relative hazard, or the ratio $\mu_{24}[t; g_{24}(X)]/\mu_{13}[t; g_{13}(X)]$, where $g_{ij}(X)$ is a linear function of all parameters associated with of all measured characteristics of the focal individual include in vector X. In the constrained models we are using, $g_{24}(X) = g_{13}(X)$ so that the relative hazard is equal to $\exp(\alpha_{24} + \beta_{24} \times \ln t)/\exp(\alpha_{13} + \beta_{13} \ln t)$, where the α and β are the Weibull parameters of the baselines. When the β's are similar to each other, the relative hazard is just a function of the α's.

Table 5 shows the estimated parameters for the baseline model (model 1) along with those estimated after successive controls are introduced. We are interested in comparing intercepts and slopes for pairs of transitions. For example, comparing the intercept (slope) of the transition from state 2 to state 4 with the intercept (slope) of the transition from state 1 to state 2 provides information about the relative magnitude of the risks of migration among younger siblings whose older sibling migrated first and among younger siblings whose older siblings migrated after they did (or not at all).

The top panel of table 5 captures in parametric form the transition processes already described by the raw probabilities shown in table 1. Recall that this table offered evidence that network ties *do* increase the likelihood of first migration. The probability of out-migration among those with an older migrant sibling was nearly three times that of individuals lacking this tie. Moreover, a tie to an older migrant sibling was more powerful in promoting out-migration than a tie to a younger migrant sibling. We also see this pattern in the parameters estimated for model 1.

The underlying hazard for transition 2 to 4 (migration risk for younger siblings whose older sibling migrated first) is considerably greater than that for transition 1 to 2 (migration risk for older siblings whose younger sibling has not migrated) or 1 to 3 (migration risk for younger sibling whose older sibling has not migrated). In fact, the intercept for 2 to 4 transition (−0.73) is significantly above either of the latter two intercepts (−2.25 or −1.79). Moreover, the value of the intercept (−1.96) for the 3 to 4 transition (risk for older siblings if younger migrated first) is much closer to the intercept for the 1 to 2 transition than to the intercept of the 3 to 4 transition. This may reflect the fact that a network tie to an older sibling is more powerful in promoting out-migration than a tie to a younger migrant sibling, an age asymmetry observed if older siblings are endowed with more resources by virtue of their migration than are their younger siblings. The estimated slopes suggest that the higher hazard for transition 2 to 4 decays more slowly than for the 1 to 2 transition. The last pair of slope coefficients is not significantly different from zero, indicating that the hazard does not change strongly as siblings age.

The remaining panels in the table show changes to baseline parameters as successive controls are introduced. If social capital theory is correct, then we expect the hazard for the 2 to 4 transition (indicating the presence of an older migrant sibling) to remain significantly above either the 1 to 2 or 1 to 3 transition (where there is no migrant sibling) *despite controls for human capital variables*. This is precisely what we observe in model 2. Whereas the 1 to 2 and 1 to 3 intercepts are well below negative two (−2.66 and −2.28, respectively), the 2 to 4 intercept is significantly higher at −1.13 ($P < .001$). The slope coefficient for the 2 to 4 transition reveals a hazard curve that is significantly flatter than the 1 to 2 transition. The slope in the latter case (−0.25) indicates a relatively rapid decay in the hazard of out-migration with age, whereas the slope coefficient of −0.07 for the 2 to 4 transition is not significantly different from zero and suggests a constant hazard of out-migration with age. Thus, compared to those who lack this source of social capital, having an older migrant sibling exposes individuals to a higher hazard of out-migration over a longer period of time. And although differences between intercepts for the 2 to 4 transition and for the 1 to 2 and 1 to 3 transitions are somewhat diminished by the introduction of human capital controls, the gap is still large and highly significant (−1.13 for the 2 to 4 transition versus −2.66 and −2.28 in the 1 to 2 and 1 to 3 transitions, respectively). The introduction of human capital controls, however, does enhance slightly the differences in slopes.

Adding controls for shared conditions in model 3 and two time-dependent controls for network effects (timing of father's migration and migration prevalence in the community) in model 4 does not change the estimates very much. As shown in table 5, although the value of the intercept for the 2 to 4 transition first falls slightly to −1.33 and then to −2.09, it remains significantly above the value of the intercepts for the 1 to 3 transition (−2.53 and −2.71) and for the 1 to 2 transition (−2.92 and −3.07). The slope coefficients hardly experience any changes.

The last model in table 5 introduces controls for unmeasured shared conditions that are likely to affect the migration risks of both siblings (unmeasured heterogeneity). To avoid estimates that are overly sensitive to distributional assumptions, we estimate the multistate model allowing nonparametric heterogeneity.[12] Specifically, we postulate the existence of more than one latent subgroup with distinctive risks of first migration. Despite multiple attempts at increasing its number, our best behaved and most parsimonious model suggested the existence of only two latent subgroups: one composing an estimated 34% of the exposed pairs with higher than average risks of first migration, and a second subgroup composing about 66% of the population of exposed pairs with lower than average risks. If the differences in baseline migration risks between younger siblings whose older siblings have (or have no) migration experience is due to unmeasured shared conditions, the introduction of a control for such sources will lead to attenuation of differences among the baselines. If, on the other hand, social capital theory has some validity, one would expect those controls to leave the differences unchanged.

Remarkably, once unobserved heterogeneity is controlled, the higher intercepts associated with social capital not only persist, but are strengthened. Having an older sibling who has been to the United States *substantially* increases the chances of international migration. Whereas the intercept for the 2 to 4 transition is −0.11, those associated with transitions 1 to 2 and 1 to 3, both involving no family network ties, are considerably lower at −3.08 and −1.59, respectively ($P < .001$). As before, network effects are asymmetrical with respect to age: those with a younger migrant sibling share about the same risk of migration as those lacking migrant siblings.

Allowing for unobserved heterogeneity has a much stronger effect on

[12] There is a different type of heterogeneity, the so-called mover-stayer type of heterogeneity, for which we also estimated parameters. This type of heterogeneity captures the possibility that a subset of individuals have a zero-valued hazard of migration, that they will not migrate no matter what. We estimated models using sequentially one and the other type of heterogeneity (we cannot estimate them both simultaneously since this creates identification problems). The inferences drawn from each were not dissimilar, though the values of the estimates of parameters were different. Since unmeasured heterogeneity that does not postulate the existence of a set of individuals with zero risk of migration is more realistic, we decided to present only the corresponding results.

the relative size of the slopes. Indeed, the introduction of this control completely changes the effect of duration in the 2 to 4 transition, switching its sign from −0.07 to 0.49, a figure virtually identical to the value estimated for transition 1 to 3. In both cases, the risk of out-migration tends to *increase* with age, at least up to age 50 when our observation stops. This makes the task of identifying the residual effects of social capital very simple, as the only difference between the risks for transitions 1 to 3 and 2 to 4 is the relative size of the constants. Thus, despite the fact that models 1 to 4 fail to account for the existence of the two underlying subgroups with different migration risks, their estimates are quite robust. Even if a failure to control for unobserved heterogeneity conceals an important difference in the shape of transitions, this simply reinforces the idea that there are strong social capital effects that cannot be imputed to unmeasured shared conditions.

The second procedure to test for differences in the baseline hazards consists of a sequence of likelihood ratio tests that assess whether setting equality constraints on the estimates of the baseline hazards for pairs of transitions leads to changes in the goodness of fit of the model. We perform these tests using a slight variation of the most complete model (namely, model 5) and refer to it as the unconstrained model, or model U. The specification for U is slightly more parsimonious than model 5 in that we constrain all three effects of the covariates for household property to be identical.[13]

After estimating U, we proceed to estimate four sets of constrained models and to calculate chi-square statistics comparing the constrained model with U. The first set is for "same-sibling" comparisons and corresponds to migration risks of the younger sibling only. The set includes models C1 and C2. The former constrains the intercept and slope of transition 1 to 3 to be equal to the intercept and slope of transition 2 to 4, whereas the latter constrains only the slopes of the transitions to be identical. The second set is also for "same-sibling" comparisons but applies to the older sibling. This set includes models C3 and C4, which are analogous to C1 and C2 but refer to transitions 1 to 2 and 3 to 4, respectively. The third and fourth sets are for "cross-sibling" comparisons. The third set includes models C5 and C6 for the contrast between transitions 1 to 2 and 2 to 4. Model C5 constrains intercepts and slopes to be identical, whereas model C6 constrains only the slopes to be equal. The fourth set

[13] The estimated effects of each of the three dummies for household property are very similar to each other (see table 7, model 5), so the loss in fit by using U is trivial. The chi-square statistic for the constrained and unconstrained models is 0.54 with 2 degrees of freedom, a statistic's value not significantly different from 0 even with a liberal significance of 0.05.

TABLE 6
SEQUENTIAL LOG-LIKELIHOOD RATIO TESTS

Model + Constraint	Log Likelihood	χ^2	Transitions Involved in Constraints	Parameters Constrained	*df*
U	1,886.7			None	
C1	1,898.8	24.2	1–3 vs. 2–4	Interc.+ slope	6
C2	1,887.7	2.0	1–3 vs. 2–4	Slope only	1
C3	1,887.4	1.4	1–2 vs. 3–4	Interc.+ slope	6
C4	1,886.9	.4	1–2 vs. 3–4	Slope	1
C5	1,891.2	5.4	1–3 vs. 3–4	Interc.+ slope	6
C6	1,889.4	5.2	1–3 vs. 3–4	Slope	1
C7	1,901.2	29.0	1–2 vs. 2–4	Interc.+ slope	6
C8	1,897.6	21.8	1–2 vs. 2–4	Slope	1
C9	1,898.8	24.0	1–3 vs. 2–4 & 1–2 vs. 3–4	Interc.+ slope	4
C10	1,904.7	36.0	1–3 vs. 3–4 & 1–2 vs. 2–4	Interc.+ slope	4

NOTE.—See text for definition of models and contrasts. All tests based on models that exclude transitions 1 to 4.

includes models C7 and C8, which are associated with transitions 1 to 3 and 3 to 4.

Table 6 displays the values of the log-likelihood of models with gradually increasing constraints. “Same-sibling” contrasts are easy to interpret: they reveal that the fit of the constrained model suffers greatly in the case of the younger sibling but not at all in the case of the older sibling. In fact, the chi-square statistic is 24.2 for model C1, but only 1.4 for model C3. It is also clear that the differences in baseline risks are overwhelmingly associated with differences in intercepts, not in slopes, since the log likelihood of the model with a constrained slope is almost identical to the log likelihood of the unconstrained model. This result indicates that, as expected by social capital theory, there are important differences in the hazards even after controlling for measured and unmeasured conditions. The degradation of goodness of fit across the constrained model reveals the importance of kin ties for the migration of younger but not necessarily older siblings.

“Cross-sibling” comparisons are slightly more complicated to interpret since differences in baselines may also be a consequence of the difference in migration risks between the eldest sibling and any other sibling in the household, not just of kin effects. For example, the test for model C7, associated with a chi-square value of 29.0 and 2 degrees of freedom, reveals that there are important differences between transitions 1 to 2 and 2 to 4, and that such differences are overwhelmingly the result of slope differences (positive for transition 2 to 4 and negative for transition 1 to 2). However, this could result from (*a*) different social status and migration-related roles of eldest siblings in Mexican households; (*b*) differences between experiences of siblings with and without ties to one with migration experience; or, lastly, (*c*) to a combination of both these mechanisms. Although the same considerations apply to model C5, the test reveals that the constrained model fits the data well and that there are no significant differences between the slope and intercept of the corresponding transitions (1 to 3 and 3 to 4). Models C9 and C10 include simultaneously "same-sibling" and "cross-sibling" contrasts and are, therefore, summaries of the differences just examined.

In sum, whether we use the coarse procedure of comparing baseline hazards or the more robust strategy of assessing goodness of fit by constraining parameters, we arrive at the same conclusion: that there are important differences, precisely in the direction predicted by social capital theory, between the risks of migration of individuals whose siblings have and have not migrated.

Testing Corollaries

The first corollary of social capital theory suggests that *differences* in migration risks between those with access to social capital (having sibling or father who has migrated) and those without it (having no sibling or father who has migrated) should *increase* during periods of stricter immigration enforcement. In order to test this idea, we estimate a model that contains a time-dependent dummy variable that assumes a value of 1 if the year of exposure to migration for each pair of siblings took place after 1986, when the Immigration Reform and Control Act was passed, and 0 otherwise. We expect the effects of this variable to be strong for transitions 2 to 4 and 3 to 4 but much less so for transitions 1 to 2 and 1 to 3. The results (not shown) indicate that although the dummy variable is properly signed (negative effects on migration risks in all transitions), it has no discernible effects at all in the *differences* between the hazards for transitions 1 to 3 and 2 to 4 on the one hand, and between 1 to 2 and 3 to 4, on the other.[14]

To evaluate the validity of the second and third corollaries, we use table 7. This table displays estimates of effects (and standard errors) for the constrained versions of models 2–5. The second corollary implies that the time-dependent variable for timing of father's migration has a significant effect and that it does not alter the differences in risks between siblings with and without access to social capital (migration experience of sibling).

[14] A better test than the one performed would have been to test estimate two models, one for the period before IRCA and one for the period after. Regrettably, the number of events induced by the partition is too small in each case, and estimates are difficult to obtain or are unstable.

TABLE 7
Parameter Estimates for Controls Added in Successive Phases of Estimation

Control Variable	Model 2 B	Model 2 SE	Model 3 B	Model 3 SE	Model 4 B	Model 4 SE	Model 5 B	Model 5 SE
Individual traits:								
Schooling 4+	−.34*	.14	−.43*	.09	−.44*	.09	−.43*	.11
Skilled occupation	−.94*	.12	−.81*	.07	−.76*	.07	−.78*	.09
Male	1.60*	.14	1.48*	.48	1.50*	.08	1.74*	.09
Household characteristics:								
Head schooling 4+			.33*	.12	.15*	.07	.14	.09
Head skilled occupation			.03	.07	.09	.07	.12	.08
Head male			.06	.10	−.24*	.11	−.27	.11
Owns farm land			−.04	.10	−.02	.09	−.05	.11
Owns real estate			−.09	.07	−.08	.07	−.07	.09
Owns business			.11	.07	.08	.08	.09	.08
Social networks:								
Prevalence					.61*	.07	.73*	.09
Father a migrant					.71*	.07	.89*	.09
Factor loadings for unmeasured heterogeneity:								
State 1 to state 2							−.44*	.02
State 1 to state 3							−2.48*	.33
State 2 to state 4							−2.99*	.32
State 3 to state 4							32.50	86.00
Probability							.34*	.04

* $P < .05$.

We have already shown that the latter part of this proposition is indeed confirmed by the data (likelihood ratios test corresponding to model 4 in table 6). The regression coefficients in table 7 show that the first part of the proposition is also true. Note that the estimated coefficient associated with having a migrant father is 0.89 meaning that the risk of migrating for any sibling whose father has already migrated is 2.43[exp(.89)] times as high as the risk for any sibling whose father has not migrated. This large effect is likely to occur because father's migration is also a source of social capital. This finding is harder to justify though not inconsistent with the household joint decision-making or diversification theories since neither suggests a positive correlation of migration risks among *all* members of a household.

The third corollary implies significant effects of prevalence of migration experience in the community, above and beyond the social capital effects of siblings' migration. Table 7 indicates that this is in fact the case: the independent effects of living in a high prevalence migration community are of the order of 0.73 meaning that the risks of migrating for any siblings are twice as high [exp(.73)] as in areas with lower prevalence of migration. Again, one would not expect this result under the joint household decision-making or risk diversification models, once we control for all relevant household and individual conditions.

IMPLICATIONS OF RESULTS

To illustrate the effects of social capital, we use the parameter estimates of model 5 in table 7 to generate three outcomes: waiting time to first migration, proportion not migrating by age 30, and median age at first migration. We estimate these quantities under two assumptions—that the subject *does* and *does not* have an older sibling with U.S. experience—and we use the parameters for transitions 1 to 3 and 2 to 4, respectively. We compute statistics using conventional life table methods using the parameter values shown in table 7. For the sake of illustration, we design four different population profiles reflecting different combinations of values for the control variables (which are applied to the coefficients in table 5).

The first profile assumes an unskilled, uneducated male whose household head is similarly unskilled, uneducated, and without property. Moreover, the head has not been to the United States and resides in a community with little migratory experience. The second profile assumes the same male sibling and household head, except that now we assume the head has been to the United States and lives in a community with many migrants. The next two profiles are for male siblings who are educated, skilled, and whose household heads are likewise educated, skilled, and property-owning. In the third profile, the head has not migrated to the United States, and the community has few international migrations. In the fourth profile, the head is a previous migrant head, and the community has a high prevalence of migration.

Outcomes associated with these profiles are presented in table 8. The upper panel shows what happens in the absence of a tie to a migrant sibling, and the lower panel reveals what happens when an older sibling has already migrated. No matter what profile is assumed, having an older migrant sibling (i.e., a network tie yielding social capital) substantially reduces the waiting time to migration, lessens the percentage who have not migrated by age 30, and lowers the median age of first migration. The first two columns, for example, correspond to the socioeconomic profile of the person generally most at risk of migrating to the United States: an unskilled and uneducated man without property. In profile 1, he is assumed to lack network ties, either through the household head or the wider community. Under these circumstances, if one's older sibling were to migrate, the waiting time to first migration would be cut in half

TABLE 8
EXPECTED LIFE TABLE PARAMETERS FOR YOUNGER SIBLINGS' TIME TO FIRST MIGRATION WITH AND WITHOUT AN OLDER MIGRANT SIBLING ASSUMING DIFFERENT POPULATION PROFILES

	PROFILE NUMBER			
	1	2	3	4
Without social capital:*				
Years to first migration	6.8	1.9	12.6	2.6
%nonmigrant at age 30	24.0	.0	45.0	2.0
Median age at first migration ...	20.2	15.6	26.6	16.0
With social capital:*				
Years to first migration	3.1	.9	6.5	1.8
%nonmigrant at age 30	5.0	.0	24.0	1.0
Median age at first migration ...	16.7	15.0	20.0	15.5

NOTE.—For profiles 1 and 2, siblings = uneducated, unskilled, male; head = uneducated, unskilled, no property. Profile 1 shows data for nonmigrant heads and low migration prevalence; profile 2 shows data for migrant heads and high migration prevalence. For profiles 3 and 4, siblings = educated, skilled, male; head = educated, skilled, property. Profile 3 shows data for nonmigrant heads and low migration prevalence; profile 4 shows data for migrant heads and high migration prevalence.
* Social capital indicates an older migrant sibling.

(from 6.8 to 3.1 years), the percentage nonmigrant by age 30 would drop from 24% to 5%, and the median age at first migration would fall from 20.2 to 16.7. Thus, having a family network tie substantially quickens the transition to international migration.

If one assumes that the sibling lives in a household where the head has migrated and in a community characterized by a high prevalence of U.S. migrants, then out-migration becomes virtually inevitable in any event, although the transition is again more rapid for those who have an older migrant sibling than for those who do not. In the former case, the average number of years to first migration is just 0.9, the median age of departure is 15, and the percentage who have not migrated by age 30 is 0.

A similar contrast is observed among those with more education and occupational skills, generally persons who would be assumed to be less prone to international migration. Among such people living in households without a migrant head and in a community with low migration prevalence, the absence of a tie to a migrant sibling yields a waiting time of 12.6 years to first migration, with 45% not migrating by age 30 and a median age at migration of 26.6. In contrast, the simple addition of an older migrant sibling lowers the waiting time to 6.5 years, reduces the percentage nonmigrant by age 30 to 24%, and cuts the median age at migration to 20. Although the presence of a migrant father and a high prevalence of community migration once again dominate in determining these statistics, the existence of a migrant sibling tie nonetheless works to speed up the transition to international migration, reduce the age of first departure, and lower the percentage who never migrate.

CONCLUSION

By specifying and estimating a flexible multistate hazard model, we sought to overcome some important limitations of prior research on migrant networks and social capital. Although earlier studies show that having a social tie to someone with migrant experience significantly raises the likelihood of out-migration, they failed to control for the effects of common causes (unobserved heterogeneity), possibly yielding overestimates of apparent network effects. Prior studies have likewise been unable to eliminate competing explanations derived from neoclassical economic theory and the new economics of labor migration, both of which predict a correlation between the migratory behavior of household members but do not posit the existence of social capital or network effects.

Our work has been successful in eliminating common causality and selectivity as competing explanations for family-based network effects. Estimates from our multistate hazard model show that having an older sibling who has been to the United States triples the likelihood of migrating to the United States and that this differential in the odds of movement persists when controls for human capital, common conditions, and unobserved heterogeneity are introduced. Overall, the estimated effects suggest very sharp differences in the behavior of people who are and are not exposed to migratory behavior through a tie to a migrant sibling.

We cannot implement a strong test to rule out competing explanations drawn from the other two competing theoretical models. Nonetheless, the fact that the apparent network effects pertain not only to close ties within households, but also to diffuse ties within communities confirms a prediction derived from social capital theory but not neoclassical economics or the new economics of labor migration. Moreover, the migration-inducing effect of a tie to an older migrant sibling is not reduced by high prevalence of migration in the community. Finally, although a father's migration experience exerts powerful influences on the migration risks of both siblings, it does not alter the differences in risks between individuals with and without a migrant sibling.

Despite these supportive findings, considerable work remains to be done to confirm the validity of social capital as a useful theoretical concept. For example, networks based on kinship are not necessarily the most efficient or most salient in shaping migration decisions. Indeed, networks based on much weaker ties of friendship or acquaintance may be equally or more important than kinship ties in determining the odds of out-migration. Although we have clearly demonstrated the importance of siblings as an important source of social capital, this connection represents only one strand in a much larger and potentially more powerful fabric of social relations affecting migration.

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