SOCIAL NETWORKS AND CHANGES IN CONTRACEPTIVE USE OVER TIME: EVIDENCE FROM A LONGITUDINAL STUDY IN RURAL KENYA*

JERE R. BEHRMAN, HANS-PETER KOHLER, AND SUSAN COTTS WATKINS

The impacts of social networks on changes in contraception in rural Kenya are investigated using special data from a longitudinal household survey. An analytic model, informed by detailed knowledge of the setting, yielded estimates that indicate that (1) social networks have substantial effects even after unobserved factors (e.g., homophily) that may determine social networks are controlled; (2) controlling for these unobserved factors may substantially alter the estimated effects of networks (these controls were not used in previous studies); (3) network effects are important for both men and women; and (4) network effects are nonlinear and asymmetric, suggesting that networks provide information primarily through social learning, rather than by exerting social influence.

Casual observations suggest that individuals make decisions not in social isolation, but in interaction with others. Theoretical analyses of contraceptive choice and fertility dynamics have shown that social interactions can help to explain changes in patterns of fertility or contraceptive behavior (Axinn and Yabiku 2001; Casterline 2001; Kohler 1997, 2000a, 2000b, 2001; Palloni 2001), as well as more general behaviors (Brock and Durlauf 2001; Friedkin 1998; Friedkin and Cook 1990; Schelling 1978), that are otherwise difficult to reconcile with standard individual-centered explanatory frameworks. These theoretical analyses suggest that social networks may work through social influence and social learning (Bongaarts and Watkins 1996; Kohler, Behrman, and Watkins 2001; Montgomery and Casterline 1996). Social influence implies that social networks reinforce or alter norms by providing examples of behavior that may then be considered and copied by others. Social learning reduces the uncertainty associated with innovations, such as family planning, through social networks that provide new information and facilitate evaluation of that information.

There is some evidence to support both casual observations and theoretical expectations. Reports from surveys in many high-fertility countries in the 1960s and 1970s

*Jere R. Behrman, director of the Population Studies Center and W.R. Kenan, Jr. Professor of Economics, University of Pennsylvania, McNeil 160, 3718 Locust Walk, Philadelphia, PA 19104-6297; E-mail: jbehrman@econ.sas.upenn.edu. Hans-Peter Kohler and Susan Cotts Watkins, Department of Sociology, University of Pennsylvania. Kohler gratefully acknowledges the support of the Max Planck Institute for Demographic Research in Rostock, Germany, where he was head of the research group on Social Dynamics and Fertility while this research was conducted. This research was supported, in part, by NIH RO1 HD37276-01 (Behrman and Watkins, co-principal investigators), the TransCoop Program of the German-American Academic Council (Kohler, principal investigator), and NIH P30-AI45008 and the Social Science Core of the Penn Center for AIDS Research (Behrman and Watkins, co-principal investigators on the pilot project). The data used in this article were collected with funding from USAID's Evaluation Project (Watkins and Naomi Rutenberg, co-principal investigators) and the Rockefeller Foundation (for a larger project, including Malawi, with Watkins and Eliya Zulu, co-principal investigators). This article is a revised version of a paper presented at the 2001 meetings of the Population Association of America in Washington, DC, and has benefitted from comments of the participants in the session, particularly John Casterline, Laurie DeRose, Mark R. Montgomery and Mark Pitt, and from the useful comments of the editor and two referees. The three authors contributed equally to this article.

indicated that substantial proportions of respondents heard about modern methods of fertility control from informal sources, such as friends. More recently, analyses of qualitative data from Thailand and Kenya have provided evidence that women chat with each other about family size and family planning (Entwisle et al. 1996; Rutenberg and Watkins 1997; Watkins 2000). Nevertheless, few among the multitude of studies of the determinants of contraceptive use in high-fertility areas have taken the next step and have attempted to estimate causal models to evaluate whether social interaction matters for demographic attitudes and behavior.[1]

Persuasive studies on the consequences of social interaction for contraceptive use in high-fertility areas have been rare for at least two reasons. First, demographers have typically treated individuals as if they were social isolates and thus have not routinely collected data on social interaction. Second, even when measures of social interaction are available, it is difficult to establish causal relations convincingly. A critical problem is that the characteristics of individuals and their socioeconomic context, some of which are not likely to be observed no matter how detailed the data, may influence not only individuals' demographic attitudes and behavior but individuals' interactions with others (for example, see Manski 1993, 1995). In the published empirical studies of social interaction and fertility, however, demographers either have ignored the possibility that social interaction and fertility may be jointly determined or have simply noted the problem in passing.

In our study, we used longitudinal data from a high-fertility area—South Nyanza District, Kenya—that include measures of social networks and contraceptive use. There were three observations over six years for the same respondents, which permitted us to explore the impact of social networks while controlling for unobserved determinants of those networks, such as women's preferences or characteristics of the communities in which they live. In the second section, we use qualitative data to describe the local criteria for selecting network partners for discussing family planning and discuss the quantitative data for our estimates. In the third section, we describe the analytic model that we prefer, on the basis of our detailed knowledge of the process by which networks were selected in the specific context of our study, and present estimates of the effect of social networks on changes in contraceptive use. Our preferred estimates indicate that the causal effects of social networks on contraceptive use are significant and substantial (and are typically larger for men than for women). They also suggest that the failure to control for the choice of network partners, as in the previous literature on the use contraceptives in high-fertility areas, may be misleading. They further indicate that social networks primarily affect respondents' contraceptive choices through social learning by providing information, rather than by exerting social influence. Alternative specifications of key variables and other tests indicate that these results are robust. In the fourth section, we conclude that social networks have effects on contraceptive use in this high-fertility area and that this study provides what are currently the best available estimates of the magnitude of these effects in such a context.

STUDY CONTEXT AND DATA

Context

For this study, we used qualitative and quantitative data that we collected explicitly to analyze the impact of social networks on reproductive behaviors in rural South Nyanza District, Kenya, an area inhabited predominantly by Luos and one in which the use of modern contraception has recently been increasing from previous low levels. Because

1. Among the exceptions are Entwisle et al. (1996), Entwisle and Godley (1998), Kohler, Behrman, and Watkins (2000, 2001), Montgomery and Casterline (1993, 1996), Montgomery and Chung (1994), Munshi and Myaux (2000), Valente and Saba (1998, 2001), and Valente et al. (1997).

both our choice of an analytic model and our interpretation of the results were influenced by specific socioeconomic and cultural characteristics of this context, it is useful to describe the context in some detail.

The decline in fertility in Kenya is an example of dramatic social change (Watkins 2000). In the mid-1980s, Kenya was widely known as the country with the highest fertility in the world; shortly thereafter, the national level of fertility began to decline and continued to do so rapidly. The decline was associated with the widespread adoption of a new reproductive model of a small family achieved through the use of modern methods of family planning to limit fertility. Although perceptions of desirable family size had begun to change before the introduction of family planning programs, it was only after the Kenyan government, at the urging of the international population movement, aggressively promoted modern methods of family planning that fertility began to fall (Watkins and Hodgson 1998). The new methods became available in rural clinics in Nyanza in the late 1980s. Their adoption was slow, however, partly because they were initially perceived as foreign owing to the association of family planning programs with whites and with a Kenyan government dominated by members of another tribe (Weinreb 2001). As the new model of family planning has slowly become domesticated—transformed into a local Luo model—the use of contraceptives has increased and fertility has begun to decline (Watkins 2000).[2]

The domestication of the foreign model of reproduction appears to be occurring partly through social networks in which Luos evaluate the advantages and disadvantages of many versus few children in their current local circumstances; learn from relatives, friends, and neighbors about their own experiences with modern methods of family planning or gossip about the experience of others; and assess their network partners' support of or opposition to the new reproductive model. An excerpt from a focus group (FG) we conducted with women aged 20–29 before the first round of our household survey suggests that network partners are providing information:[3]

> *FG statement 1*: Sometimes we talk when going to fetch water or going to the market.
> *FG statement 2*: The one who uses the pills is the one who will tell you how that pill is affecting her.
> *FG statement 3*: Sometimes we've gone for a meeting, we talk before the meeting starts.
> *FG statement 4*: Some people say pills are not good with you and it makes you so thin. Even thinner than what you were before.
> *FG statement 5*: Some people also say you would give birth to a child with lots of disabilities.
> *FG statement 6*: There was a woman who's been using injections, and she's been having periods twice in a month. This woman really had trouble until she changed to pills. Now she took the medicine, and by the time she was stopping to use pills, she got pregnant, and now she's pregnant.
> *Moderator*: How did you get to know about this?
> *FG statement 7*: We were sitting down, and she was telling how that thing happened to her.

2. In the 1977/78 Kenyan World Fertility Survey, only 1.6% of women in union in Nyanza used some method of contraception other than postpartum abstinence (Brass and Jolly 1993:95). Over the period of our data collection, contraceptive use increased from 12.3% of married women currently using family planning in 1994–1995 to 17.7% in 1996–1997 and 17.4% in 2000. In addition, 23.0% of married women had ever used family planning in 1994–1995, rising to 30.6% in 1996–1997 and 31.0% in 2000.

3. The quality of the recording of the focus groups was not sufficient to permit us to identify the individual speaking; the numbers that we use here refer to the sequence of comments, not who made the comments.

Although many in our sample voiced the desire for a smaller number of children than their mothers bore, there was still considerable ambivalence about the best family size. Modern methods of family planning are of even more intense interest. These methods are coming to be considered less as foreign and more as a sign of modernity whose users are described as "enlightened." Nonetheless, there is a great deal of uncertainty and informal discussion outside the clinics about the effects of these methods on women's bodies (Rutenberg and Watkins 1997). Such discussion is disparaged by Kenyan family planning professionals as circulating "myths and rumors" that inhibit the adoption of modern methods (Rutenberg and Watkins 1997), but it may also circulate information about the actual experiences of others, both satisfactory and unsatisfactory, and permit the participants to assess the extent of contraceptive use, as well as its acceptability, among those with whom they talk.

Luo social networks are constrained toward homogeneity by social, economic, and cultural characteristics of the context. Because transportation is irregular and expensive and there are few telephones, interaction with those outside the area is limited. Within the local communities, there is relatively little economic and social stratification; thus, interaction occurs primarily with those who live in the same context and have similar characteristics. Important exceptions are the temporary migration of men to urban areas for wage labor and the longer-term migration of the more educated, as well as trips by their wives to visit them. Funerals, which are frequent because of the high levels of AIDS, are important in Luo culture. They last for several days and are loci of more heterogeneous interaction because the migrants and other relatives who live elsewhere attend. It is also significant for the characteristics of networks that Luo cultural ideals are of exogamous marriage and patrilocal residence. Most men continue to live in their village of birth—which consist of their relatives—after marriage, but women must alter their networks when they move to their husbands' communities. This gender difference in the formation of networks may result in a gender difference in the impacts of networks if, for example, the impact of a network partner depends on the duration of the relationship. Informal interaction between the sexes is discouraged (Ayodo 1994), and the interactions of women are expected to be different from those of men. When we initially told the Luos on our research team that we were interested in "gossip" of both women and men, they objected: women gossip, they said, but "men discuss." An analysis of our qualitative data showed that both men and women consider decisions of family size to be rightly the province of men, whereas decisions about methods of fertility control are in the women's domain.

The economic and social characteristics of the context, as well as Luo cultural expectations, suggest that the members of social networks are likely to be similar to one another—Luo, poor, and with little education—and thus may be expected to make similar decisions about reproduction. Within the pool of potential network partners, however, it is reasonable to assume that individuals make choices about those with whom they will discuss aspects of reproduction. Two kinds of selectivity appear likely a priori. The first is selectivity motivated by homophily, a preference for network partners with similar characteristics that appears to be an ubiquitous feature of social structure (Blau 1994; Fischer 1982; Katz and Lazarsfeld 1955; Marsden 1988). If a respondent prefers network partners who are much like herself, her behaviors and her network partners' behaviors are likely to be correlated, but there may be no influence either from network partners to the respondent, or vice versa. It is possible to introduce more controls for the observable characteristics of the respondent that may influence her preference for network partners, but there are likely to remain relevant unobservable characteristics of the respondent, such as a preference for network partners who can keep a secret (there are many women who use family planning secretly; see Watkins, Rutenberg, and Wilkinson 1997). The second type of selectivity is strategic: what if respondents who are considering the use of family planning

strategically select network partners who they believe are using family planning themselves? If either homophily or strategic selectivity is present, the direction of causality may be from the respondent to choices of network partners; again, it would be difficult to conclude that networks influence attitudes or behavior.

When selectivity is thought to be present, it is typically on the basis of assumptions about the way people behave, assumptions that are not specific to a particular context. This general approach has led to the development of statistical methods to control for selectivity, with particular attention to instrumental variable models. Recently, however, some methodologists have raised questions about the way these statistical models are often applied (Bound, Jaeger, and Baker 1995; Pearl 2000; Winship and Morgan 1999) and have suggested that the use of these approaches can be improved by turning to data or prior scientific knowledge for a better understanding of how decisions are actually made (Freedman 1999; Manski and Nagin 1998; Stolzenberg and Relles 1997; for an example, see Short et al. 2002).

In previous work using both qualitative and household-survey data, we assessed the nature and likely extent of the selectivity of network partners in the particular context of our surveys (Watkins and Warriner 2000). The qualitative data consisted of 40 semistructured interviews with married women and 40 with married men, as well as nine focus groups, all conducted in the study sites in 1994, before the first household survey. There was considerable evidence that family planning was "in the air," discussed while women were chatting as they sat or walked together, such that even young women who had no interest in family planning learned about others' experiences with it. But we also found evidence of both homophily and strategic selectivity in the choice of network partners. The homophily was in terms not only of observable characteristics, such as age and education, which, in principle, can be controlled for, but also of characteristics that are unlikely to be observed even in the most detailed data sets, as indicated in the following segment from one of the focus groups. When the moderator asked with whom women talk about their problems, the focus-group members responded:

> *FG statement 1*: It is only your husband you can talk to.
> *FG statement 2*: Aah. You can also tell a woman like you. When you are really annoyed, you can go and tell a woman like you.
> *Moderator*: What about you, Jennifer?
> *FG statement 3*: When I am annoyed, sometimes I do not tell my cowife [the Luo word for cowife and sister-in-law is the same]. I go and tell my friend who is a woman like me and who will also tell me about her problems.
> *FG statement 4*: These stories, we normally tell about them with young women just like us.
> *FG statement 5*: And more so to those who keep it a secret, not the talkative ones.

A preference for homophilous network partners also characterizes men's discussions of family planning. Even though men's choices are less constrained because of their greater freedom of movement, the observable characteristics of their network partners are similar to those of women (Watkins and Warriner 2000).

There is also evidence that some respondents strategically initiated a conversation about family planning. In one example from a semistructured interview, a woman attending a funeral noticed that another woman there was taking a pill, suspected it was for family planning, and interrogated her. In another example, the interviewer asked with whom the respondent talked about family planning:

> *Respondent*: I have discussed this with my sister-in-law.
> *Interviewer*: Who started the conversation?

Respondent: I am the one who started.
Interviewer: What did you say?
Respondent: I said that "I am now taking pills to help me space my children. If my husband doesn't make noise [complain], I'll go and stop [childbearing]."
Interviewer: What did she say?
Respondent: She said that it is a good decision. Children will only cause problems, and when they are many, they can't be healthy because the means of looking after them is not something easy.

To assess the extent of strategic selectivity by perceived use of family planning in the choice of partners in family planning networks, we sorted all conversations about family planning reported by the women in the 40 semistructured interviews into three categories. The first category included conversations in which the topic of family planning came up generally in the flow of conversation. The second category included conversations about family planning in which a network partner was deliberately sought out for information (e.g., because she had some experience with family planning or knew something about it). The third category included conversations that could not be placed with certainty. Over half (56%) the respondents talked to others about family planning in the course of general conversations in which the topic of family planning appeared to come up incidentally: it was brought up by others who described their own experiences or gossiped about the experiences of others. About one third (36%) of the conversations could be labeled strategic, and 8% are unclassifiable.

In summary, in Nyanza, network partners for discussions of family planning are selected according to two criteria. One is homophily, some of which can be observed in the quantitative data and some of which is unobserved. The second is strategic, a deliberate selection of network partners on grounds that have to do with family planning; in our data, this criterion accounted for a substantial minority of choices. These results cannot be generalized because they are based on qualitative data from a specific context. For Nyanza, however, they suggest that the choice of an appropriate estimation method should take into account the likelihood that the characteristics of individuals and their socioeconomic context, some of which are not likely to be observed no matter how detailed the data, may influence not only individuals' demographic attitudes and behavior but their interactions with others. Two widely used statistical methods to control for right-side choice variables are instrumental variables and fixed effects. Our detailed knowledge of the context and criteria for selection of network partners led us to conclude that there are no instruments available that are likely to predict sufficiently well the selection of network partners but are not likely to be correlated with the disturbance term in the contraceptive-use equation. Our longitudinal data, however, permitted us to use fixed-effects estimators to control for unobservable characteristics that are likely to influence both contraceptive use and the selection of network partners. These fixed-effects estimates are our preferred estimates. We also provide random-effects estimates that do not take into account the likelihood that the fertility behavior and network partners are jointly determined, to permit a comparison with the type of estimates that predominates in the previous demographic literature on networks. We now turn to the data that we used to estimate this model and then to the model itself.

Data

The data were collected by Watkins and colleagues in the Kenyan Diffusion and Ideational Change Project (KDICP), a longitudinal household survey and a set of semistructured interviews and focus groups that were conducted during 1994–1995, 1996–1997, and 2000 in four rural sublocations (administrative units) in South Nyanza District. We describe the quantitative data briefly here; more details about the sample,

Table 1. Number of Respondents in Each Round of the KDICP Data

Survey Round	Number of Women	Number of Women With Nonmissing Data[a]	Number of Men	Number of Men With Nonmissing Data[a]
Kenya 1 (1994–1995)	923	909	744	572
Kenya 2 (1996–1997)	740	724	565	549
Kenya 3 (2000)	925	884	699	602
Participating in Kenya 1 Through Kenya 3		497 (498)[b]		324
Participating in Kenya 1 and 2		545		408

[a]Women or men with nonmissing information on the variables included in the subsequent regressions (e.g., contraceptive use, education, and marital status).

[b]For ever-used contraception, there are 498 instead of 497 women with nonmissing information.

the data, and analyses of the quality of the data are available at www.pop.upenn.edu/networks.

The first wave of the longitudinal household survey (Kenya 1) was conducted in December 1994 and January 1995 with a sample of 923 women and 744 husbands. The sampling frame was a list of villages in each rural site. From this list, enough villages were randomly selected to provide the desired sample size, consisting of all married women of reproductive age who were present and their husbands if they were living at home (there is much male temporary migration for work, and sometimes wives accompany their husbands to the city). Two years later, the second wave (Kenya 2) of the survey reinterviewed these women and men (and any individuals who were on the first-round sample list who had not been located during that round, but were located in the second round); the third wave followed in January and February 2000 (Kenya 3). Table 1 presents the number of respondents for each of the three rounds of data and the number who are in all three rounds. In total, 497 women and 324 men participated in all three survey waves and reported information on the relevant individual and network characteristics.[4] A comparison of our data for South Nyanza in the first two rounds of our survey with the data collected by the 1993 Kenya Demographic and Health Survey (KDHS 1994) in rural Nyanza Province shows that our data are representative of the province.

4. We conducted our analysis for the subsample of respondents for whom we had data on all three rounds, which raises the possibility of attrition bias. Most studies of attrition (e.g., those published in the special issue of the *Journal of Human Resources* in Spring 1998) have been for longitudinal samples in developed countries. The striking result of these studies is that the biases in estimated socioeconomic relations that are due to attrition are nil or small—despite attrition rates as high as 50% and significant differences between the means of a number of outcome and standard control variables for those lost to follow-up and those who were reinterviewed. Similar results are reported for three developing-country samples in Alderman et al. (2001). We undertook such analyses of attrition for our sample (estimates not presented here but available from the authors on request). We found differences between the respondents and those who were lost to the sample for one or two rounds: the latter tended to have higher education and to be younger, which a priori is plausible if individuals with such characteristics are more likely to be geographically mobile. Despite such differences in the mean characteristics, tests of joint significance for the possibility that the coefficient estimates in multivariate estimates for contraceptive use differ between attritors and respondents indicated no significant differences at the 25% level. Therefore, it appears that in our data, as in the other studies referred to earlier, attrition is selective in the sense that the mean characteristics of those who were not interviewed in all three rounds sometimes differ from those of the respondents who were included in all rounds, but that such differences do not distort significantly the coefficient estimates in multivariate relations for contraceptive use.

Table 2. Summary Statistics

	Women			Men		
Dependent Variable	Kenya 1	Kenya 2	Kenya 3	Kenya 1	Kenya 2	Kenya 3
N	909	724	884	572	549	602
Individual Characteristics at *t*–						
Age	28.70	32.75	34.90	39.19	43.76	45.70
	(7.95)	(8.37)	(8.06)	(11.92)	(13.07)	(12.50)
Not Married	0.00	0.06	0.14	0.00	0.03	0.04
Children ever born	4.47	5.44	5.34	6.11	7.53	7.46
	(3.08)	(3.08)	(3.18)	(5.37)	(6.68)	(5.37)
Has a radio	0.51	0.60	0.63	0.50	0.65	0.73
Has a metal roof	0.20	0.25	0.41	0.19	0.27	0.41
Has at least a primary education	0.80	0.79	0.82	0.90	0.90	0.92
Has a secondary or higher education	0.13	0.14	0.14	0.32	0.28	0.33
Family Planning Variables, Respondent						
Proportion currently using family planning	0.12	0.17	0.17	0.21	0.24	0.20
Proportion ever using family planning	0.23	0.32	0.32	0.25	0.32	0.28
Proportion wanting no more children	0.32	0.39	0.41	0.22	0.28	0.32
Family Planning Network						
Proportion with at least one family planning user in the network	0.75	0.81	0.88	0.71	0.75	0.81
Uncensored size of family planning network	2.88	3.90	4.61	3.42	3.93	5.10
	(2.68)	(3.64)	(4.08)	(3.67)	(4.10)	(4.89)
Censored size of family planning network	2.23	2.54	2.83	2.25	2.39	2.77
	(1.61)	(1.52)	(1.41)	(1.68)	(1.64)	(1.57)
Proportion with more than four network partners	0.23	0.33	0.37	0.29	0.32	0.41
Proportion with at least one family planning user in the network	0.48	0.63	0.61	0.41	0.49	0.47
Number of family planning users in the network (censored)	1.03	1.42	1.24	0.95	1.16	1.05
	(1.31)	(1.40)	(1.30)	(1.36)	(1.42)	(1.37)

Notes: Standard deviations are shown in parentheses. We used the subscript "*t*–" to emphasize that the variables refer to the time before *t*, where *t* refers to the survey wave. The age in Kenya 3 is based on the age in Kenya 1 and Kenya 2 because of problems in the age variable in the Kenya 3 survey. The mean and standard deviation for age therefore excludes respondents who were interviewed in Kenya 3 but not in Kenya 1 and 2.

Table 2 gives summary sample statistics for the variables that we used in our analysis (see the next section). We now briefly discuss the primary variables.

Dependent variable. We focused on whether an individual was currently (at the time of the survey) using contraception. The percentage of women currently using contraception increased from 12% to 17% between Kenya 1 and 2 but stayed at 17% in Kenya 3.

Whether a man reported current contraceptive use ranged from 20% to 24%, with a peak in Kenya 2. The difference in reported levels of contraceptive use by women and men has been found in other data sets, including the KDHS (Ezeh and Mboup 1997; Miller, Zulu, and Watkins 2001). This difference may reflect actual differences associated with men's extramarital sexual relations or with additional wives who do not live in the sample villages. Alternatively, there may be systematic measurement errors. Perhaps, for example, men overreport the use of contraceptives because the use of modern contraceptives is thought to be an indicator of being "modern." On the other hand, women may underreport the use of contraceptives because although they are using modern contraceptives, they do not wish to acknowledge such use to an interviewer because it is a secret from their husbands or their husbands' relatives. Such systematic tendencies to over- or underreport are likely to result in biases in standard estimates of the impact of social networks on contraceptive use—probably upward if there is systematic overreporting and downward if there is systematic underreporting. But to the extent that such systematic tendencies to misreport are constant for an individual over time, they are controlled in our preferred fixed-effects estimates (see the next section).

Individual characteristics. The time-varying variables that we included in our analyses as controls were the number of births before the current period, not being married,[5] having a radio, and having a metal roof. As one would expect, these variables tended to increase between 1994–1995 and 2000. In 2000, women had an average of 5.3 children, approximately 60% had a radio, and about 40% had a metal roof. For our random-effects logit estimates (see the next section), we also included a set of individual characteristics, such as the level of schooling and age, that drop out in the fixed-effects estimates. (Schooling actually increased for a few respondents over time, but the variation is not sufficient to identify the schooling coefficient in the fixed-effects estimates; therefore in the random-effects logits, we used schooling and age as measured in Kenya 2.)

Social networks. In each of the survey waves, we asked the respondents whom they had ever talked with about family planning. The survey questions used the word "chat" to indicate that we were interested not in lectures or counseling sessions at the clinics but, rather, in informal interactions, or "gossip." Family planning is clearly a prominent topic in social interactions. At least 75% of the women reported having discussed family planning with at least one network partner, and these percentages increased to 88% by Kenya 3. On average, in the different survey rounds, women talked with 2.9 to 4.6 network partners about family planning. Some questions were asked about the characteristics of a maximum of four of these partners, including whether the network partner used family planning.[6] This format produced sets of respondent, or ego-centered, networks with up to four network partners.[7] The average size of these "censored" networks in different survey

5. The initial sample was of married women, but those who were subsequently widowed were retained in the sample in subsequent waves (divorced women leave their husbands' homes and therefore generally could not be located).

6. For the network partners' contraceptive use, we did not ask the respondents to distinguish between current use and ever use at the time of the survey because (1) the discontinuation of contraceptives is frequent and (2) the respondents may not have known the current contraceptive-use status of their network partners because the conversations occurred in the past.

7. Our (and others') measurement of network characteristics face several limitations. First, they do not indicate the relative importance of the various discussions for individual women or provide information on the content of the conversations. Second, as noted in the text, we followed the practice common in ego-centered network analyses of asking specific questions about only a subset of network partners for those who reported large networks. Because the choice of whom the respondent discussed in such cases may not be random, there may be biases embedded in the data; however, we conjecture that the conversations that the respondent recalled are more important. (Kohler 1998 found bias in the estimates of density for truncated ego-centered network data.) Third, our network measurements were based on recall and, as with any data reconstructed from memory, potential biases exist. Brewer (2000) and Brewer and Webster (2000) found that respondents tend to forget about a fifth of their personal network partners when asked to list them from memory. Fourth, information on

rounds was between 2.2 and 2.8. About one third of the women reported talking with more than four network partners about family planning. Because of limitations on how long the interviews could last without undue fatigue, no information was collected for network partners beyond the first four. In our analyses, we therefore explored alternative assumptions about the distribution of these behaviors among these network partners on whom we do not have information.

The number of network partners reported in women's family planning networks for whom we have detailed information about contraceptive behavior, their relation to the respondents, and some socioeconomic characteristics, such as education, was 2,039 in Kenya 1; 1,867 in Kenya 2; and 2,626 in Kenya 3.[8] This information about the network partners was reported by the respondents, and the variation in the total number of network partners across survey waves is due primarily to the different number of respondents in the three surveys waves (see Table 1).

A clear pattern in Table 2 is the marked trend toward larger family planning networks. For instance, 25% of women in Kenya 1 reported no communications about family planning, but this percentage declined to 12% in Kenya 3. There are two possible explanations for the increase in family planning networks. First, some women were initially reluctant in Kenya 1 to report that they had talked with anyone: some respondents told the interviewers that they did not want to talk about a friend for fear of "spoiling her name." As the respondents became more familiar with the survey project, this reluctance to report about friends may have decreased in Kenya 2 and Kenya 3. Second, if the respondents did not forget their past conversations, the number of network partners could not decrease over time; the reported increase could be due only to the accumulation of more conversations over time and may not indicate an increased frequency of such conversations.

We believe, however, that the increase in the size of social networks and the number of family planning users in these networks was also due to more frequent interactions about family planning. Although the questions on conversations about family planning in the questionnaire did not specify a time frame for these conversations, it seems as if the respondents primarily included relatively recent interactions among the first four network partners. For instance, the women who participated in all three waves of the survey reported a total of 1,636 network partners with detailed information in Kenya 3, and in 89% (or 1,455 cases), the most recent communication with the respective network partner about family planning occurred after the earlier survey, Kenya 2. In Kenya 2, however, the same respondents already reported a total of 1,422 network partners in their family planning networks. Hence, a net increase of 181 in the number of network partners with detailed information between Kenya 2 and Kenya 3 corresponds to at least 1,422 conversations about family planning during this period.[9] We cannot disentangle the extent to which the recent conversations in Kenya 3 (i.e., the interactions that occurred during the period since Kenya 2) were with network partners who were already part of the networks in Kenya 2. However, it is clear that the respondents referred to relatively recent conversations about family planning, which appear to have been increasing. The increase in the number of contraceptive users in the family planning networks may have occurred because a network

the behavior of network partners was reported by the respondents and thus may be projections of their own behavior (discussed later in the text in regard to systematic measurement errors). To the extent that these measurement problems are persistent over time for each respondent, they are likely to cause biases in standard logit estimates, but to be controlled in our preferred fixed-effects estimates.

8. These are the total number of network partners reported by the respondents. The total number of individuals who are network partners of one or more respondents is smaller if there is overlap in the networks for different women, as there surely is.

9. We know only the lower bound for this number of conversations, because there could have been multiple conversations with the same network partner and some respondents' networks were censored. Table 2 shows that the extent of censoring is increasing over time.

partner, who was already mentioned at an earlier wave, adopted family planning over time or because the women were more likely to encounter family planning users instead of nonusers in their most recent conversations.

One possible problem with our representations of network partners is that they may incorporate either random or systematic measurement error. Random measurement error in a right-side variable, as is well known, biases estimated coefficients toward zero; random measurement error is exacerbated in our preferred fixed-effects estimates (see the next section) because it is larger relative to the deviations from averages on which fixed effects depend than it is relative to the level of the same variables. We were not able to control for random measurement error. Thus, if the respondents reported their total (uncensored) network partners with a true number plus or minus a random term, we underestimated the effect of networks.

Systematic measurement error is more complicated because it can bias the estimates in either direction. For one of our four study sites, we have reports both from the respondents (ego) and from their network partners about their use of contraceptives (White and Watkins 2000). An analysis of these data showed that the respondents appeared to overstate systematically the proportion of their network partners who were using modern contraception. The implications of such overreporting would depend on why it occurs.[10] For example, the respondents may have systematically overreported the use of contraceptives by their network partners because they perceived that the research team favored family planning and wanted the team to learn that their friends were the sort of people of whom the team would approve. This type of systematic overreporting would probably cause an *under*estimate of the true effects of actual contraceptive use by network partners in standard logits. But to the extent that individual respondents overreported the number of their network partners who were users by the same amount in each survey wave (although this amount may differ across respondents), our preferred fixed-effects estimates controlled for this systematic measurement error. Another example of systematic error is if the respondents projected onto their network partners the behaviors that they decided to follow themselves—that is, if they overstated the extent to which their network partners used contraceptives if they decided to use contraceptives themselves (and vice versa). If the reports on network partners' contraceptive use are accurate with regard to what the respondents believed to be true even if there is such a projection, then even in standard logits the reports may capture the true effects of *perceived* contraceptive use among network partners even if they measure with error the partners' actual use of contraceptives.

Because the identification of the network effect in our preferred fixed-effects logits (relation 1 in the next section) is based on within-individual variations in the size and composition of the family planning networks over time, we report in Table 3 summary statistics for the within-individual variation in the primary network representations that were used in the subsequent estimations. These deviations from the within-individual average reflect both increased contraceptive use and increased social interaction with both users and nonusers. Moreover, the standard deviations of these individual-level deviations from the average are relatively constant in each variable across all waves, indicating that in all periods, contraceptive use and our primary network characteristics varied to an approximately equal extent from their individual averages.

In Table 4, we report regressions of the *change* in the number of family planning users and nonusers across the survey waves in the respondents' family planning networks on the initial number of users and nonusers in the networks. The pattern emerging from these regressions is that a smaller initial number of users (nonusers) is strongly associated with larger changes in the number of users (or nonusers) over the intersurvey period.

10. The impression of overreporting may be false because it is possible that some network partners were not using contraception at the time of the survey but had been using contraception earlier when they talked about family planning with the respondent.

Table 3. Summary Statistics for Deviations of the Use of Family Planning, and the Characteristics of the Family Planning Network From the Within-Individual Average in the Three Survey Waves: Only Respondents Who Participated in Kenya 1–Kenya 3

	Women			Men		
Dependent Variable	Kenya 1	Kenya 2	Kenya 3	Kenya 1	Kenya 2	Kenya 3
N	497	497	497	324	324	324
Currently Using Family Planning	–0.039	0.009	0.030	–0.031	0.043	–0.012
	(0.261)	(0.254)	(0.273)	(0.284)	(0.314)	(0.303)
Family Planning Network						
Number of family planning users in the network (censored)	–0.256	0.173	0.082	–0.154	0.130	0.025
	(0.971)	(0.937)	(0.964)	(0.995)	(0.976)	(1.028)
Number of non–family planning users in the network (censored)	–0.143	–0.197	0.340	–0.120	–0.179	0.299
	(1.071)	(1.008)	(1.016)	(1.187)	(1.118)	(1.172)

Moreover, having an initial larger number of nonusers in the network is also associated with larger increases in the number of users. This finding suggests that some respondents "replaced" their nonusers with users or, alternatively, that their network partners adopted contraceptives over time. As is suggested by the qualitative information summarized earlier, funerals seem to be an important catalyst of social interactions about family planning: the respondents in villages with more frequent funerals tended to have more users and nonusers of family planning in their networks. Time-varying individual characteristics, such as having a metal roof or a radio (both signs of relative wealth) or the number of children ever born, do not significantly predict changes in the social networks. In addition, there is some indication that the change in the number of family planning users increases and the change in the number of nonusers decreases with age, which may reflect a life-cycle pattern and/or may be related to the fact that older women interact more frequently with women who are more likely to be users because of their higher age or fertility. Finally, women with a secondary education also tend to have larger changes in the number of users in their networks.[11]

ESTIMATES OF THE IMPACT OF SOCIAL NETWORKS ON CHANGES IN CONTRACEPTIVE USE

Empirical Specification

We posited a logit model for contraceptive use that depends on social networks, characteristics of individuals that are observed in the data, and fixed and stochastic characteristics of individuals and their communities that are not observed in the data:[12]

$$Y_{it} = f(N^{u}_{it-}, N^{n}_{it-}, \mathbf{X_{it-}}, \mathbf{f_i}, e_{it}), \quad (1)$$

11. The dependence of the changes in the number of users or nonusers in the respondents' networks on the initial size and composition of the networks and fixed individual characteristics, such as age and higher education, is likely to be due to individual fixed effects. These effects are removed from relation (1) in the next section in our fixed-effects estimations so they do not cause biases in our preferred estimates.

12. Standard models of fertility determinations, such as the quantity-quality model of Becker and Lewis (1973) and Willis (1973), do not include social networks explicitly; they could be extended, however, to include social networks as sources of information about contraceptives and norms for using them for individuals with heterogeneous preferences in a way that would imply relations, such as relation (1) for the determination of contraceptive use and relation (2) for the selection of social network partners.

Table 4. Women: Regression of Changes in the Number of Family Planning Users and Nonusers in the Network Between Survey Waves on the Initial Number of Users and Nonusers and Personal Characteristics[a]

Dependent Variable	Number of Users	Number of Nonusers
Number of Network Participants Using Family Planning, Time *t*–	–0.72**	0.02
	(0.03)	(0.03)
Number of Network Participants Not Using Family Planning, Time *t*–	0.10**	–0.90**
	(0.03)	(0.03)
Village Average Number of Funerals Attended[b]	0.13**	–0.10**
	(0.03)	(0.04)
Dummy Variable for Not Married, Time *t*	0.37	–0.23
	(0.25)	(0.22)
Children Ever Born, Time *t*	–0.01	0.01
	(0.02)	(0.02)
Respondent Has a Radio, Time *t*	0.11	0.10
	(0.09)	(0.09)
Respondent Has a Metal Roof, Time *t*	0.12	–0.01
	(0.10)	(0.10)
Respondent Has at Least a Primary Education	0.19	0.04
	(0.12)	(0.11)
Respondent Has a Secondary Education	0.29*	–0.19
	(0.14)	(0.13)
Age	0.02*	–0.02*
	(0.01)	(0.01)
(Age/10) Squared	0.00	0.00
	(0.001)	(0.001)
Dummy Variable for Survey Wave Kenya 2	–0.53*	2.47**
	(0.27)	(0.27)
Dummy Variable for Survey Wave Kenya 3	–0.27	1.89**
	(0.25)	(0.26)

Notes: Robust standard errors are used to account for potential heteroscedasticity; they also account for the correlation of residuals for the same individual across time periods.

[a]Changes in the number of users were measured between Kenya 1 and Kenya 2 and between Kenya 2 and Kenya 3; the initial network composition was measured, respectively, at Kenya 1 and Kenya 2.

[b]The village average number of funerals attended in the last month before the survey; this question is available only for Kenya 1 and Kenya 3, and these two measurements were used to predict, respectively, the change in networks between Kenya 1 and 2 and Kenya 2 and 3.

*$p < .05$; **$p < .01$

where Y_{it} is contraceptive use by individual i at time t; N^u_{it-} is number of social network partners for individual i before time t who were reported to be contraceptive users;[13] N^n_{it-} is number of social network partners for individual i before time t who were

13. We used the subscript "t–" to emphasize that the variable N refers to the time before t; we used this notation also for other predetermined variables. Furthermore, the variables can be vectors with more than one

reported *not* to be contraceptive users; $\mathbf{X_{it-}}$ is a vector of other variables for individual i determined before time t (e.g., age, marital status, children ever born, and wealth); $\mathbf{f_i}$ is a vector of unobserved fixed factors that determine contraceptive use by individual i (e.g., unobserved current community characteristics and the persistent part of individual preferences, including those related to social interactions); and e_{it} is a random disturbance term that affects contraceptive use by individual i at time t that is due, for example, to randomness in the availability of contraceptives.

Standard logit estimates of relation (1), as in the previous literature on family planning in high-fertility areas,[14] may be contaminated by omitted variable bias because the representations of social networks before time t, denoted N^u_{it-} and N^n_{it-}, are likely to be correlated with the unobserved fixed factors (f_i) that determine current contraceptive use. This is likely to be the case because of homophily—that is, women choose to talk primarily with women like themselves, as was discussed in the previous section. Women who have a relatively high underlying tendency to use contraceptives because, for example, they prefer to have fewer children than do others are likely to have as network partners other women who are relatively likely to use contraception because they also prefer fewer children.[15] In addition to the prevalence of homophily, as noted in the previous section, about a third of the 40 women in the semistructured interviews chose to discuss contraceptive use with particular women because they believed these women were using contraceptives. Whether because of homophily or such a strategic selection of partners, social networks before time t are likely to have been determined by individual characteristics X_{it-} and unobserved fixed factors f_i, both of which also appear in the contraceptive use Eq. (1), and on other potentially unobserved factors u_i that are uncorrelated with X_{it-} and e_{it}, as represented in the linear approximation in relation (2):

$$N^u_{it-} = gX_{it-} + hf_i + u_{it-} \text{ and}$$

$$N^n_{it-} = g'X_{it-} + h'f_i + u_{it-}', \tag{2}$$

where the primes refer to the different coefficients for the determination of the number of nonusers in the social network compared with the number of users. As a result, the usual estimates of the coefficients of the social network variables in the determination of current contraceptive use in relation (1) are likely to include not only the effect of social networks, but the effect of the correlated parts of the unobserved variables in f_i.

Therefore, to attempt to obtain consistent estimates of the coefficients that measure the impacts of social networks on contraceptive use, we used fixed-effects logits (e.g., Hsiao 1986).[16] These estimates effectively control for unobserved individual

element. For example, for the network variables, we allowed for some nonlinearities by including both a dichotomous variable for whether there is at least one user in a respondent's social network and the number of users beyond one (and likewise for nonusers).

14. See Entwisle et al. (1996), Entwisle and Godley (1998), Kohler et al. (2000, 2001), Montgomery and Casterline (1993, 1996), Montgomery and Chung (1994), Munshi and Myaux (2000), and Valente et al. (1997). In a previously published article (Kohler et al. 2001), we allowed for the number of network partners to be correlated with unobserved factors in the disturbance term and controlled for that possibility by considering only respondents with three or four network partners. But we assumed in that study that network characteristics (namely, network density) are independent of factors in the error term for the relation with contraceptive use. One exception is a study by Montgomery et al. (2001), which adopted a strategy similar to ours using four rounds of longitudinal form 1998–1999 through 2000 in six communities in southern Ghana; that study also found significant effects of social networks on contraceptive use that persist with controls for fixed effects.

15. Because of homophily, there is likely to be a relatively constant subgroup structure in terms of characteristics, including contraceptive use and preferences for family size, as described by Friedkin (1998), even if there are more fluid dyadic reactions within the group.

16. We obtained consistent estimates under the assumption that stochastic shocks that affect network partners *before* time t do not affect *current* contraceptive use at time t. If we had identified instruments for

characteristics, such as underlying preferences for children; unobserved community or subgroup characteristics, such as shared norms; and correlations in disturbances common to individuals who for some observations are respondents and for others are included among network partners. Because the fixed effects control for all fixed characteristics over the period, these estimates effectively focus on the relation between the changes in network partners and the changes in the respondents' use of contraceptives. Much of the previous research used estimation methods that implicitly or explicitly assumed that the selection of network partners is random; thus we also present random-effects logit estimates that permit a comparison of the two approaches.[17]

One important question about social networks that motivated this article is, of course, whether they have a significant impact on contraceptive use in high-fertility contexts, such as in rural Kenya. The fixed-effects logit estimates proposed here provide information with which to answer that question, conditional on the underlying specification. This specification, in contrast to the previous literature, allows for the possibility—which seems plausible—that unobserved factors, such as preferences for having many (or few) children, affect both the decision to use contraceptives and the choice of social network partners. The comparisons with the estimates using the random-effects logits indicate whether it is important to control for relevant unobserved preferences.

Another important question is this: what are social networks doing? Theoretical and empirical analyses suggest that social networks may work through social learning and social influence, that is, through altering or reinforcing norms or through providing new information and facilitating evaluation of that information (Bongaarts and Watkins 1996; Montgomery and Casterline 1996; Moscovici 1985; Schiller 2000). The previous literature on family planning and social networks in high-fertility areas has recognized such possibilities, but generally has not provided empirical evidence of their importance.

We address this question on the basis of the following two considerations (for a related discussion, see also Kohler et al. 2001). First, if information is of primary relevance, then having at least one social network partner with experience using contraceptives is likely to provide a lot of information, but additional partners beyond the first user are likely to provide less additional information. For example, if there are beliefs that the use of contraceptives developed by whites has dire consequences, such as death, for black women, knowing at least one user who has not suffered such consequences provides critical information. The additional information on such a topic provided by subsequent users is likely to be less than that provided by the first partner who is a user. Therefore, testing whether there are nonlinearities—that is, whether the effect of at least one network partner's use of contraceptives is more important than the effects of having additional network partners who use contraceptives—provides some insight into the importance of obtaining information through social learning.

Second, if information is of primary relevance, those who have direct experience—that is, particularly those who are perceived by the respondents to have used contraceptives—are likely to be the most important sources of information. Therefore, a test of whether the impact of users is larger in absolute value than that of nonusers is also a test of the relative importance of information versus normative change.

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instrumental variable estimates (in which the social network representation is purged of its correlation with the compound disturbance term in the relation being estimated by using the predicted value of the variable instead of its actual value), we could have dropped this maintained assumption. But, as noted in the previous section, we did not have variables with the necessary characteristics to serve as identifying instruments.

17. Random-effects logits, rather than standard logits, are necessary because each respondent contributes three observations on social networks and contraceptive use, and the estimation of the coefficients and their standard errors needs to adjust for the correlation in the likelihood scores of these observations. Random-effects logit models achieve this adjustment.

If, in contrast, networks are related primarily to norms and exert social influence, the predictions are different. For example, if the respondents are mimicking their partners' behavior because of social influence, the impact of every partner should be equal in absolute magnitude, but positive for users and negative for nonusers. That is, if the respondents are basically balancing the contradictory information about norms from users versus nonusers, neither nonlinearities nor asymmetries of the types should be expected if information is dominant.

Thus, it appears likely that the role of networks is more important in providing information the greater the effects of the first contraceptive user versus additional contraceptive users and the larger the effects of contraceptive users versus nonusers. To explore such possibilities, we expanded the representation of networks in relation (1) to indicate whether there is at least one user and how many users there are beyond one reported in the social network of each respondent—and likewise for nonusers.

Estimates for Contraceptive Use as Reported by Women

Table 5 presents a set of coefficient estimates for the fixed-effects logit model in relation (1) for women who reported that they were currently using contraceptives. The corresponding odds ratios are equal to the exponent of the coefficients in Table 5.

In our longitudinal approach focusing on changes in the use of contraceptives over time, each respondent contributes three observations to the estimates in Table 5: one each in which the current period is Kenya 1, Kenya 2, and Kenya 3. The number of respondents used for each estimate therefore is 497, equal to the number of respondents observed for all three periods who do not have missing information on key variables (see Table 1). The fixed-effects logit model uses the subset of 156 women who changed contraceptive use at least once during the observation period; women whose contraceptive use was constant over time do not contribute to the likelihood function of the fixed-effects logit model (Hsiao 1986).

Table 5 includes two pairs of estimates for relation (1). Each pair includes our a priori preferred individual fixed-effects estimates and, for comparison, random-effects logit estimates.[18] The two pairs differ only in their representations of social networks: (1) whether at least one network partner uses contraceptives and the number above one of network partners using contraceptives and (2) whether at least one network partner uses contraceptives, the additional number of network partners using contraceptives, whether at least one network partner does not use contraceptives, and the additional number of network partners who do not use contraceptives. For each estimate, the right-side variables include a control for the survey round. Standard errors are reported in parentheses beneath the point estimates. The standard errors of the random-effects logit model account for the correlation in the likelihood scores across survey waves for the same respondent.

The first set of fixed-effects estimates in Table 5 indicates that the odds of a woman currently using contraceptives increase by a factor of more than 2 (exp(.716) = 2.05) if she has at least one contraceptive user among her network partners. This effect is statistically significant and important, especially when considering the relatively modest mean levels of contraceptive use that are summarized in Table 2. The point estimates in this model also indicate that having additional network partners who are users beyond the first one does not significantly increase the odds of currently using family planning, nor

18. Estimating the random-effect model on the same sample as the fixed-effect model, that is, the subset of women with at least one change in family planning use, does not yield unbiased estimates of the coefficients because the estimation is based on a selected subset of individuals—those with at least one change in family planning use. The random-effect estimation does not account for this selection and hence yields distorted estimates when applied to this subset of individuals. Fixed-effect estimation is not affected by this selection because the individual fixed effects absorb the observed and unobserved fixed characteristics of individuals that determine whether a respondent changed family planning use at least once during the period of observations.

Table 5. Women: Fixed-Effect and Random-Effect Logit Models for Currently Using Family Planning With Different Specifications of Network Partners' Family Planning Use (Respondent's Contraceptive Use Measured at Kenya 1, 2, and 3)

Dependent Variable	Fixed-Effects Logit	Random-Effects Logit	Fixed-Effects Logit	Random-Effects Logit
At Least One Family Planning User in the Network	0.72*	0.61*	0.69*	0.49†
	(0.30)	(0.25)	(0.32)	(0.26)
Number of Remaining Family Planning Users in the Network	0.16	0.49**	0.07	0.49**
	(0.12)	(0.10)	(0.14)	(0.11)
At Least One Nonuser in the Network			0.01	0.27
			(0.30)	(0.24)
Number of Remaining Nonusers in the Network			–0.22	–0.19
			(0.16)	(0.13)
Dummy Variable for Not Married, Time *t*–	–0.60	–0.64	–0.59	–0.66
	(0.52)	(0.41)	(0.52)	(0.41)
Children Ever Born, Time *t*–	0.10	0.06	0.12	0.06
	(0.12)	(0.05)	(0.12)	(0.05)
Respondent Has a Radio, Time *t*–	0.41	0.38†	0.39	0.37†
	(0.30)	(0.20)	(0.30)	(0.20)
Respondent Has a Metal Roof, Time *t*–	–0.71*	0.08	–0.73*	0.08
	(0.37)	(0.22)	(0.37)	(0.22)
Respondent Has at Least a Primary Education		0.83**		0.85**
		(0.31)		(0.31)
Respondent Has a Secondary Education		0.61*		0.61*
		(0.28)		(0.28)
Age		0.41**		0.41**
		(0.11)		(0.11)
(Age/10) Squared		–0.59**		–0.59**
		(0.16)		(0.16)
Dummy Variable for Survey Wave Kenya 2	0.35	0.21	0.34	0.21
	(0.24)	(0.22)	(0.24)	(0.21)
Dummy Variable for Survey Wave Kenya 3	0.60*	0.44*	0.63*	0.45*
	(0.29)	(0.22)	(0.30)	(0.23)
Constant		–11.36**		–11.35**
		(1.99)		(1.99)
N (Number of Women, Each Observed at Three Surveys)	156	497	156	497

Notes: The fixed-effect logit model is based only on individuals who changed their contraceptive behavior at least once between Kenya 1 and Kenya 3; women with constant contraceptive use in all three survey waves were dropped in the estimation. We used "*t*–" to emphasize that the variable refers to the time before *t*, where *t* refers to the survey wave.

†$p < .10$; *$p < .05$; **$p < .01$

is this effect substantial (i.e., by a factor of only 1.17 for each additional network partner compared with 2.05 for having the first partner who is a user). Moreover, the respondent's contraceptive use is exclusively related to network partners who are themselves users of contraception. In the second fixed-effects estimate in Table 5, we include nonusers, but these network characteristics do not have significant or substantial effects. The nonlinearities in the effects of partners who are contraceptive users and the asymmetries between the effects of partners who are users versus those who are not both suggest that these networks are primarily providing information through social learning, rather than exerting social influence (see the discussion on specification at the end of the first part of this section).

Our preferred estimates control, as noted, for all unobserved fixed characteristics that may affect both current contraceptive use and social networks. For reasons discussed at the beginning of this section, our *prior* belief is that the fixed-effects logit estimates are preferred over the random-effects logit estimates because our previous analysis of qualitative and quantitative data suggested that in this context, there were likely to be unobserved fixed factors that affect both the contraceptive use Y_{it} and the social network representations N^u_{it-} and N^n_{it-}. However, do these controls make any substantive difference? The answer to this question is definitely yes. The first set of estimates, for example, indicates that the random-effects logit estimates of the increase in the odds of currently using contraceptives that is due to an additional user in a woman's social network is biased upward by over 300% (the random-effects model suggests an increase in the odds of contraceptive use for each user in the social network of 63%, in contrast to the 17% suggested by the fixed-effects estimates). The finding that random-effects logit models misestimate the influence of social networks on contraceptive use suggests that the unobserved individual and community factors, represented by the term f_i in relation (1), are correlated with the respective representation of the social network, N^u_{it-} and N^n_{it-}.

In summary, the estimates in Table 5 suggest that (1) having a contraceptive user in a woman's social network before the time of the survey has a significant and substantial effect on the probability that the woman currently uses contraceptives, even with controls for such unobserved factors that may affect both the propensities to use contraceptives and the nature of the respondent's social network; (2) women are primarily obtaining information from these networks, rather than evaluating desirable and appropriate behavior; and (3) the use of random-effects logits that implicitly assume that network partners are randomly selected, as in the previous literature, results in substantial biases in the estimated impact of prior social networks on women's current contraceptive use.

Tests of Robustness

Table 6 presents a specification test for the findings that social networks exert important influences on contraceptive use by exploring whether *past* contraceptive use predicts *current* social networks and whether such predictions, if significant, remain significant with controls for individual fixed effects. If past contraceptive use were to predict current social networks even with fixed effects, then it would not be possible to interpret comfortably the fixed-effects logit estimates in Table 5 as representing the causal effects of social networks on contraceptive use, rather than reverse causality.

The first estimate in Table 6 is an ordinary least-squares (OLS) estimate in which the current number of social network partners using contraceptives is the dependent variable. Among the right-side variables is contraceptive use at the previous survey round (referred to as time $t - 1$), as well as a number of controls parallel to those included in Table 5 but for both the current period (referred to as time t) and the past period (referred to as time $t - 1$). The estimates suggest that a respondent's past contraceptive use is a powerful predictor of the current number of her network partners who use contraceptives, with a coefficient estimate indicating that past users have 0.63 more current network partners who are users

Table 6. Women: Specification Tests With the Current Number of Network Partners (Total or Family Planning Users) as the Dependent Variable and Past Family Planning Use as the Right-Side Variable (Respondent's Contraceptive Use Measured at Kenya 2 and Kenya 3)[a]

Dependent Variable	OLS: Number of Network Partners Using Family Planning	Fixed Effects: Number of Network Partners Using Family Planning
Using Family Planning, Time $t-1$	0.63**	–0.07
	(0.14)	(0.19)
Dummy Variable for Not Married, Time $t-$	–0.14	–0.56*
	(0.15)	(0.24)
Dummy Variable for Not Married, Time $t-1$	0.50†	0.53†
	(0.26)	(0.32)
Children Ever Born, Time $t-$	0.07†	0.07
	(0.04)	(0.06)
Children Ever Born, Time $t-1$	–0.06	–0.08
	(0.04)	(0.07)
Respondent Has a Radio, Time $t-$	0.19*	0.17
	(0.10)	(0.15)
Respondent Has a Radio, Time $t-1$	0.04	–0.03
	(0.09)	(0.13)
Respondent Has a Metal Roof, Time $t-$	0.21†	–0.21
	(0.12)	(0.18)
Respondent Has a Metal Roof, Time $t-1$	0.00	–0.33
	(0.13)	(0.21)
Respondent Has at Least a Primary Education	0.52**	
	(0.12)	
Respondent Has a Secondary Education	0.27†	
	(0.15)	
Age	0.07	
	(0.05)	
(Age/10) Squared	–0.08	
	(0.07)	
Dummy Variable for Survey Wave Kenya 3	–0.19*	–0.02
	(0.08)	(0.11)
Constant	–0.93	
	(0.80)	
N (Number of Women, Each Observed at Three Surveys)	497	497

Notes: The robust standard errors are used to account for potential heteroscedasticity. For the OLS estimates, these standard errors also account for the correlation of residuals for the same individual across time periods. We used "$t-$" to emphasize that the variable refers to the time before t, where t refers to the survey wave.

[a]The time of measurement is indicated by time t. Past contraceptive use and individual characteristics at the preceding survey wave are indicated as time $t-1$.

$^{\dagger}p < .10$; $^{*}p < .05$; $^{**}p < .01$

than do past nonusers (an estimate that has a *t* ratio of 4.43). Of course, this predictive power may reflect only preferences for homophilous network partners—women who are judged by the respondent to be "like me" are more likely to be users if the respondent is a user (and vice versa)—or other unobserved factors. These possibilities are controlled in the second estimate in Table 6 with individual fixed effects. Once fixed effects to control for unobserved characteristics are introduced in the estimation, the coefficient estimate for past contraceptive use practically disappears. The value becomes much smaller in magnitude (actually slightly negative) and not significantly different from zero even at the 50% level. This comparison suggests that consistent with our interpretation of Table 5, unobserved factors, such as preferences for homophily, are important in understanding the contraceptive use–social network nexus and that estimates that are made without controlling for them may be misleading for identifying causal effects, even if the associations in such estimates using OLS appear strong.

We also conducted several other tests of robustness that we summarize (for brevity, the additional estimates are not presented here, but are available from the authors on request). First, we explored what would happen if we used alternative dependent variables: "ever-used contraceptives"[19] and "the respondent wants no more children." We found similar patterns, although the estimates for wanting no more children are more imprecise than are those for contraceptive use. Second, we explored how the estimates change if all network partners, if any, beyond the four about which the respondent reported contraceptive use were assumed to be users or nonusers. The latter almost does not change the estimates. The former makes them more imprecise and less consistent with the respondents' own contraceptive behavior. The combination suggests that in cases in which there are more than four reported network partners, most of them beyond the fourth are not users. Third, we also estimated linear fixed-effects probability models that yielded substantively and statistically similar results to the fixed-effects logit estimates.

Estimates for Contraceptive Use Reported by Men

All the estimates discussed to this point are for women. Are there parallel results for men? The demographic literature on social networks has focused on women, which may reflect the perception that women are more engaged in such networks or are more central in contraceptive choices than are men. But certainly casual observations in the sample villages suggest that men spend a great deal of time in informal social interactions and report "chatting" about family planning about as much as do women. Table 7 presents estimates for men that are parallel to those in Table 5 for women. These estimates indicate, first, that social networks have a significant positive impact on contraceptive use for men as for women, even when unobserved individual fixed factors are controlled. Despite the frequent protestations by the male respondents that family planning was a "woman's matter" and the local perception that it is women who gossip, not men, the estimates indicate some larger effects for men than for women. This finding may reflect, as was suggested earlier, that the social network partners for men carry more weight in men's decisions because, owing to exogamy and patrilocality, they are likely to have known most of their network partners much longer than the women have known theirs. Second, the fixed-effects estimates differ between men and women in respects other than their magnitude. In particular, those for men indicate greater (and significant) effects of having additional network partners who are contraceptive users beyond the first one. Nevertheless, for men, as for women, the role of networks in providing information appears to

19. For ever use of contraception, the stochastic term in relation (1) is less likely to be independent of the prior social network, as is required for the identification of the causal effect of prior social networks on contraceptive use, than is current contraceptive use.

Table 7. Men: Fixed-Effect and Random-Effect Logit Models For Currently Using Family Planning With Different Specifications of Network Partners' Family Planning Use (Respondent's Contraceptive Use Measured at Kenya 1, Kenya 2, and Kenya 3)

Dependent Variable	Fixed-Effects Logit	Random-Effects Logit	Fixed-Effects Logit	Random-Effects Logit
At Least One Family Planning User in the Network	1.53**	1.44**	1.57**	1.52**
	(0.38)	(0.27)	(0.41)	(0.30)
Number of Remaining Family Planning Users in the Network	0.32*	0.56**	0.30†	0.52**
	(0.14)	(0.11)	(0.17)	(0.12)
At Least One Nonuser in the Network			–0.10	–0.19
			(0.33)	(0.27)
Number of Remaining Nonusers in the Network			0.01	0.03
			(0.18)	(0.14)
Dummy Variable for Not Married, Time *t*–	–2.30	–0.91	–2.29	–0.91
	(1.59)	(0.95)	(1.60)	(0.95)
Children Ever Born, Time *t*–	0.21*	0.01	0.21*	0.01
	(0.10)	(0.02)	(0.10)	(0.02)
Respondent Has a Radio, Time *t*–	0.49	0.23	0.49	0.23
	(0.35)	(0.23)	(0.35)	(0.23)
Respondent Has a Metal Roof, Time *t*–	–0.46	0.22	–0.47	0.23
	(0.43)	(0.25)	(0.44)	(0.25)
Respondent Has at Least a Primary Education		0.86†		0.88†
		(0.51)		(0.51)
Respondent Has a Secondary Education		0.45†		0.45†
		(0.23)		(0.24)
Age		0.11†		0.11†
		(0.06)		(0.06)
(Age/10) Squared		–0.13†		–0.13†
		(0.07)		(0.07)
Dummy Variable for Survey Wave Kenya 2	0.30	0.26	0.29	0.26
	(0.27)	(0.24)	(0.27)	(0.24)
Dummy Variable for Survey Wave Kenya 3	–0.27	–0.15	–0.27	–0.14
	(0.34)	(0.25)	(0.34)	(0.26)
Constant		–6.19**		–6.11**
		(1.47)		(1.48)
N (Number of Males, Each Observed at Three Surveys)	133	324	133	324

Notes: The fixed-effect logit model is based only on individuals who changed their contraceptive behavior at least once between Kenya 1 and Kenya 3; women with constant contraceptive use in all three survey waves were dropped in the estimation. We used "*t*–" to emphasize that the variable refers to the time before *t*, where *t* refers to the survey wave.

†$p < .10$; *$p < .05$; **$p < .01$

dominate. Third, the random-effects estimates for men appear to be less biased, if anything, than are those for women.[20]

Finally, in estimates that are not presented here for brevity, for men, as for women, the social network effects carry over to alternative dependent variables, such as having ever used contraceptives and wanting no more children. (In fact, for the latter there is stronger evidence of significant coefficient estimates for men than for women.)

CONCLUSIONS

Casual observations suggest that individuals make decisions not in social isolation, but in interaction with one another. Social scientists have recently begun to modify accounts of social change that focus on individual actors by taking communities, neighborhoods, and networks into account. Yet the literature does not permit confident inferences regarding the causal effects of social networks because unobserved factors that may directly affect attitudes and behavior may also directly affect choices of the units of social interaction. For example, our previous analyses, using both qualitative and quantitative data, of the process by which network partners are selected suggested that in the specific context in which our data were collected, there was a considerable preference for homophilous network partners, "women like me"; in addition, all those in a network may be exposed to the same market and community constraints and possibilities. If the causal direction is unclear, what has been interpreted as the causal effects of social networks may simply be associations that are due to both contraceptive use and network partners' choices being determined, in part, by unobserved factors, such as preferences.

The availability of unusual longitudinal data and the use of statistical methods that control for unobserved factors provide a unique opportunity to extend the individualistic rational-actor models to incorporate social interaction and to estimate the causal effects of social networks on attitudes and behavior. This article presented such an analysis for contraceptive use in high-fertility rural Kenya. We have four major findings.

First and foremost, our analysis shows that social networks have significant and substantial effects even when we controlled for unobserved factors that may also determine the nature of the social networks. In particular, this study provides what we believe are currently the best available estimates about the effects of social networks on contraceptive use in high-fertility areas.

Second, estimates of the effects of social networks that are based on the implicit assumption that they are determined randomly, as in previous studies, may lead to a substantial misunderstanding of the impact of social networks on individual behaviors. With our data, analyses that did not control for the possibility that both contraceptive behavior and social networks within which this behavior is discussed are partially determined by unobserved factors, such as preferences, appeared to misestimate the effects of networks.

Third, the effects of social networks are not limited to women, even though in local stereotypes women are often characterized as gossiping much more than men. To the contrary, our estimates indicate that, if anything, men are likely to be more influenced by their network partners than are women. This finding may reflect cultural patterns of exogamy and patrilocality that result in men having known their network partners since childhood, whereas women alter their network partners after marriage.

20. The relatively small change in some of the estimates for men suggests that the biases toward zero owing to random measurement error that often have been emphasized in concerns about fixed-effects estimates (e.g., Ashenfelter and Krueger 1994; Griliches 1979) are probably not all that large. This result also gives us more confidence that the differences between the random-effects and the fixed-effects estimates for women are substantially due to control for important unobserved fixed effects and are not just an artifact of random measurement error: we have no reason to expect that random measurement error would be so much larger for women than for men as would be required if that were the explanation for the difference between the random and fixed-effects estimates in Table 5. (Systematic measurement error may differ by gender, but any individual systematic over- or underreporting is effectively part of what is controlled in the fixed-effects estimates.)

Fourth, the effects of social networks that we found contribute to a better understanding of social change. These effects are generally nonlinear and asymmetric. They are particularly large for having at least one network partner who is perceived to be using contraceptives; however, the inclusion of additional networks partners with the same characteristic generally has much smaller (and insignificant) effects (for women). This combination of nonlinearity and asymmetry suggests that the exchange of information constitutes the primary aspect of social interactions about family planning—social learning, not social influence. In addition, the nonlinear and asymmetric pattern of network influences is consistent with stereotypic diffusion models (e.g., Rogers 1995; Valente 1994). If there are just a few who initially adopt an innovation, they have a relatively large influence because they interact with a relatively large number of individuals who have not yet adopted it; in such cases, they provide these individuals with at least one adopter, the influence of whom is relatively large. Thus, adoption initially accelerates. As there are more innovators, however, the marginal influence of yet another adopter eventually starts to decline. Interaction processes therefore suggest that social networks are likely to have large effects on behavior as long as an innovation is not widely disseminated. As innovative behavior increases, the marginal effect of interactions is likely to be much smaller than in the early phase of the diffusion process.

The use of family planning has already increased rapidly worldwide and fertility has begun to decline almost everywhere in developing countries (Bongaarts and Watkins 1996). Although our data are particular to rural Kenya and our analysis is of specific interest to demographers who are interested in diffusion through social interaction, we believe the approach exemplified here is of wider use for those who are interested in social change. In particular, our results suggest that pervasive social change may be stimulated by early and small amounts of women's and men's gossip.

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