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How Can We Measure the Causal Effects of Social Networks Using Observational Data? Evidence from the Diffusion of Family Planning and AIDS Worries in South Nyanza District, Kenya

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How Can We Measure the Causal Effects of Social Networks Using Observational Data? Evidence from the Diffusion of Family Planning and AIDS Worries in South Nyanza District, Kenya

by

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Abstract: This study presents estimates that social networks exert causal and substantial influences on individuals' attitudes and behaviors. The study explicitly allows for the possibility that social networks are not chosen randomly, but rather that important characteristics such as unobserved preferences and unobserved community characteristics determine not only the outcomes of interest but also the informal conversational networks in which they are discussed. Longitudinal survey data from rural Kenya on family-planning and AIDS are used to estimate the impact of social networks while controlling for their unobserved determinants. There are four major findings: First, the endogeneity of social networks can substantially distort the usual cross-sectional estimates of network influences. Second, social networks have significant and substantial effects even after controlling for unobserved factors that may determine the nature of the social networks. Third, these network effects generally are nonlinear and asymmetric. In particular, they are relatively large for individuals who have at least one network partner who is perceived to be using contraceptives or or to be at high risk of HIV/AIDS, which is consistent with S-shaped diffusion models that have been emphasized in the literature. Fourth, the effects of networks are not confined to the use of family planning by women, the focus of much of the literature on networks in demography, but appear to be more general, influencing responses to HIV/AIDS, and influencing men as well as women.

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SECTION 1: INTRODUCTION

The aim of this paper is to demonstrate empirically and convincingly that a different picture of the dynamics of social change emerges if individuals are considered not in isolation, but as members of social networks. To achieve this goal requires unusual data as well as the use of recent advances in techniques of modeling that permit making causal inferences from observational data. We thus exploit a longitudinal data set consisting of repeated measures of social networks and individual attitudes and behavior over time, and use statistical models that take into account the possibility that the choice of network partners is neither random nor fully exogenously determined, but is influenced by factors that are not observed in the data.

Although the fathers of social science in the late nineteenth and early twentieth centuries were deeply concerned with the relationship between the individual and the group (e.g. Durkheim, Marx, the Chicago School in Sociology, Veblen), by the 1950s the focus of social scientists was on individuals. An explosion of individual- and household-based cross-sectional surveys and the development of high speed computers facilitated analyses of information on individuals abstracted from their social setting and protected from the passage of time. Individuals were questioned about their own attributes and attitudes, and one variable was related to others measured at the same time. To the extent that the social setting was captured, it was primarily in terms of administrative boundaries such as nations or cities. More recently, however, there has been an increase in attempts to understand the influence of settings that are more local, and perhaps more meaningful to women and men as they proceed day-to-day through their lives, such as their local communities and their personal networks. The literature that presents these attempts has considered social change through the mechanism of the diffusion of innovations (Rogers and Kincaid 1981; Coleman, Katz and Menzel 1966; Strang and Tuma 1993), the existence of thresholds or tipping points that lead to a divergence in the dynamics of collective and individual decision processes (Schelling 1978; Gladwell 2000, Valente 1994), and the roles of social networks that lead to embedded social actors (Granovetter 1973; Burt 1992). Among the more vibrant related research areas are those of demographers studying diffusion (Bongaarts and Watkins 1996; Montgomery and Casterline 1996; Entwisle et al 1996; Kohler 2000a, 2000b, forthcoming; Kohler, Behrman and Watkins 2000; Axinn and Yabiku 2001), attempts by sociologists to modify rational actor models by embedding the actors in a social context (Granovetter 1973; Coleman 1986; DiMaggio 1997; Smelser and Swedberg 1994), and attempts by economists to incorporate the effects of neighbors and neighborhoods into individualistic neoclassical models (Borjas 1995; Brock and Durlauf 2001; Duncan and Aber 1997).

Persuasive empirical studies on the consequences of social networks for attitudes and behavior are rare, however, for at least two reasons. First, because contemporary social scientists have typically treated individuals as if they were social isolates, they have not routinely collected data on social networks. Second, even when measures of social networks are available, it is difficult to establish causal relations convincingly. A critical problem is that characteristics of an individual and her social setting may influence her attitudes and behavior, but also her choice of community and personal network. Thus, any similarity between the attitudes and behaviors of individual actors and their network partners may not reflect social influences, but constitute merely a reflection of similar determinants of behavior (Manski 1993). While this problem is sometimes acknowledged, few studies have addressed it explicitly.

A focus on neighborhoods, communities or networks thus raises general problems of establishing causality in a particularly acute form. It is reasonable to assume that actors have more choice in the selection of their work and residential communities or in the selection of people with whom they discuss matters important to them than in their choice of their age, gender, race or even class. Causal inferences obtained from standard statistical models that include these latter characteristics are therefore tenable because these characteristics are unlikely to be determined by unobserved factors that influence the outcome variable of interest and simultaneously the key explanatory variables included in the model. On the other hand, casual observation suggests that choice influences the selection of neighborhoods, communities and personal networks, albeit under constraints that may be either flexible or relatively constrictive. If choice is important, then the conclusion that local social settings or networks influence individuals is not necessarily tenable. There is considerable evidence that neighborhoods are composed of people who are more similar to each other than they are to those in neighborhoods with different characteristics (Lieberson and Waters 1988; Massey and Denton 1993). Equally unsurprising is evidence that the choice of network partners is guided by homophily, the preference for interaction with those much like onself (Katz and Lazarsfeld 1955; Blau 1994; Fischer 1982; Marsden 1987; Watkins and Warriner 2000). If the factors that influence the choice of local social settings or networks are available in the data, they can be controlled for in statistical models. Often, however, they are not observed, either because the information was not collected or because the factors are very difficult to observe. Examples of the latter are a preference for living near those who share similar moral values that are often difficult to articulate on a survey, or a preference for chatting with those who can be trusted to be discreet. If these unobserved determinants of neighborhood or peer-group choice are directly related to the outcome variable of interest – and not only indirectly through their effect on the neighborhood or peer-group – then standard analyses of neighborhood or peer-group effects will provide a distorted picture of their causal relevance for individual behavior.

Statistical procedures have been developed to attempt to control for selection on the basis of unobserved characteristics, including fixed effects, random effects and instrumental variables. These have been used in many studies in the social sciences, with some practitioners claiming that standard methods that do not control for unobserved factors can lead to substantial biases in estimation.[1] These methods, however, are quite demanding of data. To estimate models with random or fixed effects, for instance, requires multiple observations of potentially relevant attitudes or behaviors.

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[1] A few examples include Angrist (1990), Angrist and Krueger (1992), Alderman, Behrman, Lavy and Menon (2001), Ashenfelter and Krueger (1994), Behrman and Deolalikar (1990), Behrman, Foster and Rosenzweig (1997), Behrman, Foster, Rosenzweig and Vashishtha (1999), Behrman and Rosenzweig (1999, 2001), Behrman, Rosenzweig and Taubman (1994, 1996), Behrman and Taubman (1976), Behrman and Wolfe (1987, 1989), Budig and England (2001), Conley and Bennett (2000), England, Farkas, Kilbourne and Dou (1988), Foster and Rosenzweig (1994, 1995, 1996), Kohler, Skytthe and Christensen (2001), Jasso (1985), Pitt, Rosenzweig and Gibbons (1993), Rosenzweig and Schultz (1983, 1987), Rosenzweig and Wolpin (1986, 1995).

Consider an example from demography, where there is currently considerable interest in the effects of social interaction on fertility (Bongaarts and Watkins 1996; Montgomery and Casterline 1996; Entwisle et al 1996; Kohler 1997; 2000a, 2000b, forthcoming; Kohler, Behrman and Watkins 2000; Axinn and Yabiku 2001), as well as on other demographic outcomes such as migration (Massey and Espana 1987; Massey and Espinosa 1997). To study the impact of social interaction on contraceptive use using fixed effect models, multiple observations per individual of contraceptive use and of social interaction are required. Such data have rarely been collected. To estimate models using instrumental variables requires instruments that are convincingly related to measures of social interaction but not to the stochastic term in the equation determining contraceptive use. Such instruments are infrequently available. As a result, in the few studies of social interaction and demographic behavior, analysts either ignore the possible joint determination of measures of social interaction and fertility or qualify their approach by mentioning the problem in passing.

In this paper, we provide an example of the way that social interaction influences social change. Although our example comes from demography, our approach is relevant to a wide range of questions about neighborhood, community and network effects that are of interest to social scientists. In addition, we provide what we believe are the best estimates the influence of social networks on behavior and attitudes regarding contraceptive use and AIDS. We use data from South Nyanza District, Kenya, where both modern contraception and AIDS are innovations introduced in the past decade. Important features of these data include that they provide detailed information about the interactions in social networks about AIDS and family planning, and that they provide multiple observations over a five-year period. In particular, information about family planning networks, fertility attitudes and behavior are measured for the same respondents at three points in time, and for the AIDS attitudes and AIDS social networks at two points in time. The data therefore provide longitudinal, detailed and direct evidence about the social context that potentially affects individuals' attitudes and behaviors, and such information is essential for establishing a link between the social context and individual's behavior and attitudes (for a recent study that stresses the critical importance of these aspects, see Axinn and Yabiku 2001). Moreover, our data permits 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 Section 2, we present our analytic framework, which shows that unobserved characteristics are indeed likely to affect women's contraceptive use <u>and</u> their choice of network partners. For simplicity of exposition, we develop the analysis in this section focusing on women and contraceptive use, but the approach is generalizable to other outcomes. In Section 3 we discuss the context and data for our empirical analysis. In Section 4, we compare estimates of the effect of social networks on contraceptive use, with and without control for individual fixed effects. We find that the results differ substantially depending on whether unobserved fixed factors that might affect both the choice of network partners and contraceptive use are controlled. Our preferred estimates indicate that the causal effects of social networks on contraceptive use are significant and substantial (and larger for men than for women) even with our controls. Alternative specifications of key variables and other tests suggest that these results are robust. In Section 5, we present similar estimates in an analysis of the influence of network partners on the degree to which a respondent considers herself or himself to be at risk of AIDS. In Section 6, we conclude that social networks have effects on fertility- and AIDS-related attitudes and behaviors, and that this study provides what are currently the best available estimates of the magnitude of these effects.

SECTION 2: ANALYTIC FRAMEWORK, EMPIRICAL SPECIFICATION, AND ESTIMATION ISSUES

The choice of empirical specifications and estimation methods with which to examine the possible impact of social networks on contraceptive use is influenced by analysts' assumptions about the process through which networks are formed, though often these assumptions appear to be conditioned strongly by data and software availability. The approach used in the demographic literature is to assume, usually implicitly, that it is acceptable to treat networks as if they were formed randomly.[2] That is, it is assumed that there are no unobserved characteristics of the women such as their preferences, no unobserved prices, no unobserved community characteristics, etc., that affect contraception use directly and that also are correlated with the empirical representation of their social networks. There are two reasons to expect that these assumptions are often violated: 1) the reported characteristics of network partners and the reasons respondents chose some but not others, and 2) the logic of models of fertility behavior. We consider each of these in turn.

Reported Reasons for Choice of Network Partners: The validity of the statistical models that we consider below to control for selectivity is difficult to evaluate because the basic assumptions and predictions of these models are rarely challenged or tested (Manski 1995; Stolzenberg and Relles 1997; Winship and Morgan 1999; Freedman 1999; Rosenzweig and Wolpin 2000). Some critics suggest that these models can be improved by turning to "data or prior scientific knowledge" (Freedman 1999:19; see also Manski and Nagin 1998) in order to understand the process by which decisions are made. Thus, for the same sample that we use in the analyses in this paper, Watkins and Warriner (2000) use household survey data and semi-structured interviews to understand better the underlying process by which network partners are determined. Their analysis of the survey data shows that the networks are relatively homogeneous compared to networks in more economically and socially stratified countries: for example, the respondent and his or her network partners tend to be similar in terms of their age, education, and economic status. Other analyses show that the networks are relatively dense and network partners are therefore likely to know each other as well as the respondent (Kohler, Behrman and Watkins 2001).

This homogeneity of networks is undoubtedly due in part to the constraints imposed by the

[2] See 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), and Valente, *et al.* (1997). In Kohler, Behrman and Watkins (2001) we allow for the number of network partners to be correlated with unobserved factors in the disturbance term and control for that possibility by considering only respondents with three or four network partners. But we assume in that study that network characteristics (namely, network density) is independent of factors in the error term for the contraceptive use relation. One exception in the work underway by Montgomery, et al. (2001) that was presented at the PAA 2001 meetings, but for which we have seen only some of the preliminary tables, not the paper per se.

context of rural Kenya, where relatively homogenous populations live in small communities (see Section 3). Importantly, however, the primary criterion for the choice of network partners appeared to be a preference for talking with homophilous others , i.e. "Women like me". Our qualitative data collected in S. Nyanza District, consisting of 40 semistructured interviews with women and 40 with men, support this, and make it clear that "a woman like me" is one similar to the respondents in characteristics such as "whether she can keep a secret" that are not observable in our data.

The most direct illustration of possible biases in the usual estimates of the impact of networks on fertility attitudes and behavior is whether network partners are selected in part with respect to what the respondent believes to be their attitudes toward or use of family planning. If a respondent who, for example, is considering the use of family planning, deliberately chooses to talk with someone she or he believes has personal experience with these methods, even when the correlation between the use of family planning by the respondent and her network partners is strongly positive, the causal direction may not be only from the network to the respondent but substantially or even totally the reverse. The assumption that women deliberately select network partners with respect to their perceived use of family planning is reasonable, and consistent with a view that as rational actors women search for information that will help them to make decisions about fertility control. To examine this empirically, Watkins and Warriner drew on a set of semi-structured interviews conducted with 40 women. They found that about 20 percent of the 40 women gave no indication about the motivation for the selection of network partners. About 30 percent indicated that they choose a particular woman with whom to discuss family planning because she was believed to use family planning. For the other half of the interviews, the respondent made it clear that such strategic selection did not occur. In these cases, the topic of family planning was likely to occur as women were talking together as they walked to get water or firewood, or sitting and chatting at home, such that even women who were not considering the use of family planning would learn about its perceived advantages and disadvantages.

From these studies, thus, the process of network selection in our data appears to be dominated by homophily, with the strategic selection of network partners because they are believed to use family planning as a somewhat less important criterion. But both homophily and the strategic selection of network partners mean that it cannot be assumed that networks are chosen randomly.

Standard Models of Fertility Behaviors with Extensions to Include Social Networks: The standard models of fertility determination such as the quantity-quality model of Becker and Lewis (1973) and Willis (1973) do not include social networks explicitly, though some of this literature does recognize social influence in determining preferences (e.g., Easterlin, Pollak and Wachter 1980). But sketching out how these standard models might be extended to include social networks is useful; we follow demographic convention in using women as the actors (Watkins 1993) and we focus on the use of contraception.

We begin with a model of fertility that assumes that women maximize their own preferences for the quantity and quality of their children, their own health and other outcomes that they can influence. We then extend this standard model by proposing that social interaction also enters into their preference functions: for example, a woman may enjoy social interaction in itself, she may obtain social status from her number of children relative to that of her network partners, or she may be uncomfortable when she deviates from the expectations of appropriate behavior held by her social group. In addition, social interactions may provide information about whether it is socially acceptable to use modern contraceptive methods or the consequences of specific methods (e.g. the impact of Depoprovera versus pills on one's body). Preferences differ across women; for example, given the same opportunities, some women choose to use modern methods of contraception while others use traditional methods, and some spend more time in social interaction than others.

The constraints under which the women make choices can be collapsed into two groups, those that characterize the "production" of children and other outcomes and those that characterize the constraints under which this production occurs:

1. Production functions: Among the production functions that form the basis for the standard fertility model are those for births (e.g. the impact of contraceptive use on the probability of conception) and for child quality (e.g. the impact of resources devoted to children). Information production functions are also relevant: they permit extending the standard model to characterize the impact of social networks on information related to a number of behaviors and outcomes, including knowledge of social norms regarding contraceptive use.
2. Resource constraints: The most common resource constraints depicted in standard fertility models are those of money and time. Many fertility models assume a full-income (i.e., covering all resources under the control of the women, including their time) budget constraint that incorporates explicitly the use of the resources under the women's control and resources that are available to her (which may be positive or negative, depending on whether a woman is a net provider of resources to others or a net receiver of such resources). The availability of resources is often depicted as depending on market prices (current and expected in the future), but the model can be extended such that the availability of resources also depends on social interactions in the present as well as those that are expected in the future, such as agreement about household sharing rules or customs and about the allocation of community resources, as well as the channels through which resources from other households can flow.

Within this extended model, maximization of women's preferences subject to such constraints leads to reduced-form relations that determine *inter alia* contraceptive use and social networks. Each of these reduced-form relations includes on the right-side all of the variables that are predetermined from the point of view of the women at the time of the current period's decisions: all preferences, all intrahousehold channels and community characteristics, all current and expected prices, etc. Because both contraceptive use and social networks are dynamic, these relations need to be updated for assets that are carried over time (e.g., the size and composition of the groups in which such social interactions occur may change, the number of surviving children).

From the perspective of whether a woman uses contraceptives at time t, the central past asset for this paper is her social network prior to time t. In our framework, this network was established prior to time *t* and influenced by the same set of predetermined variables that affected the woman when she made her contraceptive decisions prior to time *t*. Therefore many of the determinants of social networks prior to time *t* are likely to be the same variables as those on the right-side of the relation determining contraceptive use at time *t* because they are fixed or slowly adjusting over time (e.g., the woman's schooling, her expectations regarding future prices and the interfamilial and community resources on which she can draw, the persistent part of her preferences).

Unfortunately, most of these variables can not be observed in our or in any other data sets. Therefore the reduced-form relation for contraceptive use at time *t* as a function of the social network prior to time *t* includes in the disturbance term all those factors that are on the right-side of the reduced form for such contraceptive use, and many of these are likely to be correlated with determinants of network characteristics prior to *t*. As a result, if the estimation procedure does not control for these unobserved right-side factors that determine contraceptive use at time *t* and are correlated with the unobserved right-side factors that determined social networks prior to time *t*, the estimated impact of social networks prior to time *t* on contraception use at time *t* is biased. This is the case because the estimated impact includes not only the causal effect of interest–here, the impact of prior social networks on current contraception use-- but also the effect of all unobserved factors in the disturbance term of the equation that determines contraceptive use that are correlated with social networks determined prior to time *t*. Such a bias may be positive or negative, depending on the direction of the impact of the unobserved factors on contraceptive use versus the direction of their impact on social networks prior to time *t*. Within this framework, thus, social networks would not seem likely *a priori* to be random.

Specification of Empirical Relations that We Estimate and Estimation Issues: Based on the empirical investigations of network partners' characteristics and the sketch of a common model of fertility behavior that we extended to incorporate social networks, we posit that prior social networks are <u>not</u> likely to be random in the sense of being independent of disturbance terms in relations for the estimation of contraceptive use at time *t*. Therefore we use an empirical specification of the relation determining contraceptive use in which there is explicit recognition that, in addition to observed right-side variables (including social networks prior to time *t*), there are unobserved factors. A first-order linear approximation to the linear probability model for contraceptive use under these assumptions is:

$$Y_{it} = \mathrm{a}\, N_{it-} + \mathrm{b}\, X_{it-} + f_i + e_{it}, \qquad (1)$$

where Y_{it} is contraceptive use by individual *i* at time *t*; N_{it-} is the social network for individual *i* prior to time *t* (we use the subscript "t-" to emphasize that the variable *N* refers to the time prior to *t*; we use this notation also for other predetermined variables); X_{it-} is a vector of other state variables for individual *i* determined prior to time *t* (e.g., age, marital status, children ever born, women's schooling, wealth); f_i is a vector of unobserved fixed factors that determine contraceptive use by individual *i* (e.g., the woman's and her children's frailties and intelligence, unobserved current community characteristics, expectations regarding future prices and interfamilial and community resources on which the woman can draw, the persistent part of her preferences); and e_{it} is an random disturbance term that affects contraceptive use by individual *i* at time *t* due, for example, to randomness in the availability of contraceptives or to price shocks that are deviations from the long-run secular price trends.

The basic estimation problem is that the representation of social networks prior to time t (N_{it-}) is likely to be correlated with the unobserved fixed factors (f_i) that determine current contraceptive use. Social networks prior to time t N_{it-} are likely to have been determined by individual characteristics X_{it-} and unobserved fixed factors f_i, both of which appear also in the contraceptive use equation (1), and additionally 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):

$$N_{it-} = g\, X_{it-} + h\, f_i + u_i. \quad (2)$$

As a result, the ordinary least squares (OLS) estimate of the coefficient of social networks, a, in the determination of current contraceptive use in relation (1) includes not only the effect of social networks, but also the effect of the correlated parts of the unobserved variables in (f_i). In particular, in the simplest case in which there is no X_{it-} in Eq. (1), the estimate of a equals the true value of a plus the effect of f_i times the correlation between f_i and N_{it-}.

To obtain consistent estimates of the coefficient a, which measures the impact of social networks on contraceptive use, it is necessary to break the correlation between the term representing social networks and the compound disturbance term including both fixed and random elements. The two dominant statistical methods discussed in the literature for doing so are (1) instrumental variables and (2) fixed effects.[3]

For instrumental variable estimates, the right-side variable of interest (in this case, 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. The identifying instrument(s) needed for this prediction must satisfy three conditions: (i) they must be sufficiently correlated with the representation of social networks that they are being used to predict, (ii) they must not be correlated with the compound disturbance term in the relation determining contraceptive use, and (iii) they must not be included in the contraceptive use relation.[4] The logic of the model sketched out above means that fixed determinants of contraceptive use in general also are determinants of social networks, so fixed individual, family, household, community or community variables (e.g., women's schooling, compound land rights, community location and environment) are not good identifying

[3] In principal a nonstatistical way to have this correlation be zero is to conduct an experiment in which women are assigned randomly social networks. For this, as for many other questions in the social sciences, it would seem very difficult if not impossible to conduct such an experiment. But the experimental ideal is behind many recent discussions of how to obtain estimates of causal effects from behavioral data (see surveys and discussions in Manski 1995; Heckman 1997; Heckman, et al. 1999; Stolzenberg and Relles 1997; Winship and Morgan 1999; and Rosenzweig and Wolpin 2000).

[4] The use of instruments that do not satisfy one of these conditions (or satisfy the third by some arbitrary exclusion from the contraceptive use relation) can result in estimates that are worse than those that would be obtained without estimates. Because many studies have apparently used such instruments, some criticize the instrumental variable approach – rather than the inappropriate use of the method.

instruments. Variables that are correlated over time (e.g., age, secular trends in prices, interhousehold relations, and community characteristics) do not satisfy the second condition. Unanticipated deviations from such variables ("shocks") in the past (e.g., price shocks) satisfy conditions (ii) and (iii). It is an empirical question of whether they satisfy condition (i). Our prior is that for the long-run decisions related to fertility of interest to us in this study, such shocks may not satisfy condition (i). Even if long-run prices are important, short-term fluctuations may not be. Unfortunately, we have no variables that permit us to explore the possible significance of such shocks.

To control for individual fixed effects, a dummy variable for each individual in the sample can be included (thus obtaining estimates of the f_i for each of the *i* individuals) or relation (1) can be differenced over time (which eliminates all fixed effects in the disturbance term because their differences are zero). Either statistical procedure requires multiple observations on the variables that are observed in relation (1) which, if the unit of observations is individuals, almost always means observations over time. The observations on the critical observed variables – Y_{it} and N_{it-} in relation (1) – moreover must vary over time for enough individuals in the sample to permit estimation of the impact of the latter on the former. We focus on such individual fixed effects estimates below to assess the impact of social networks on contraceptive use. Under the assumptions that we have indicated above (with some caveats to which we turn next), these estimates are of the causal effects of social networks on contraceptive use.

Determinants of changes in the size and composition of social networks: To identify the causal effect of social networks on contraceptive use in the fixed effect estimation of relation (1) we rely on variations in the size and composition of a respondent's social network over time -- in our case by changes of the variable N_{it-} across the three survey waves – Kenya 1, Kenya 2 and Kenya 3. Moreover, in order that the fixed effect estimation of relation (1) reveals the correct social network effect, these variations in N_{it-} must not be correlated with the period-specific random disturbance term e_{it} in relation (1) that affects contraceptive use by individual *i* at time *t*. In particular, variations in the social network size and composition that are consistent with these conditions include: (a) Some women have more and some have fewer encounters with others with whom they had not previously discussed family planning but with whom such discussions occurred by chance and, given our estimates, these new network partners were contraceptive users or previous network partners who did not use contraception but subsequently adopted it. (b) The composition of respondent's social networks at the beginning of the each inter-survey period, i.e., at Kenya 1 for the period Kenya 1 – Kenya 2 or at Kenya 2 for the period Kenya 2 – Kenya 3, differ because respondents had differential opportunities or incentives to interact about family planning with others in the past. This differential "stock" of network partners is likely to be correlated with the fixed effects f_i in relation (1). This differential stock of past interactions leads to different opportunities for new interactions during the period between surveys. For instance, in Section 3 we present evidence that the increase in network partners (or users among them) is inversely related to the initial number of network partners, which is plausible because the probability of a chance conversation in the course of daily life (for example, while fetching water or going to the grain mill) seems to be greater over an interval the fewer such conversations one has had in the past. Similarly, we find evidence that the change in the number of network partners is positively related to events that plausibly increase opportunities to increase interaction (e.g., deaths, funerals, and similar other events that lead to social gatherings). Because the fixed effect estimation accounts for differences in the stock of network partners at the beginning of each inter-survey period, the differential changes in the size and composition of network partners that result from differences in the stock of prior interactions constitute important sources of variation in network size and composition that allow us to identify the network effect in the fixed effect estimation of relation (1).[5] The same argument holds if changes in the social network depend on age or some other fixed individual characteristic such as education (which, however, in fact does not vary substantially in our sample across the survey period).

Nevertheless, an important maintained assumption in our analyses is that the representation of social networks, or the change in social networks between surveys, is independent of whatever shocks are in the random part of the disturbance term, e_{it}.[6] A few potential variables that *a priori* may seem to violate this assumption, however, turn out to be innocuous. Particularly important in this context are several life-cycle factors, such as increases in age or duration of marriage, that may lead to increased incentives for using family planning and interacting with others about family planning. If the relevant life-cycle variables approximately linearly enter relations (1) and (2), which determine the contraceptive use and our network representation, then the fixed effect estimation subsume the systematic influence of these life-cycle changes on both family planning use and the social network representation in the fixed effect and these changes do not distort the resulting fixed effect estimates of the network influences.

The relevance of including fixed effects in the estimation is revealed in our estimates below by four aspects of those estimates. First, there is the question of whether – conditional on our assumptions – the control for fixed effects makes any difference in the estimates. Second, we also perform Hausman specification tests that reveal whether the OLS estimates are consistent as compared to the fixed effect estimates, which are consistent under our model assumptions. Third, we regress changes in the network representation N_{it-} between surveys on the size and composition of the social network at the beginning of

[5] In the fixed effect estimates, thus, the social network variable effectively is the deviation from the individual average and is correlated with the individual fixed effect. But that does not cause biases in the estimates because, conditional on the functional form in relation (1), the disturbance term in the fixed effect estimate of relation (1) does not include the individual fixed effect (even though if relation (1) is estimated without control for fixed effects, the disturbance term does include the unobserved fixed characteristics, which are correlated with the social network variable and therefore cause the bias on which we are focusing).

[6] If we had good measure of such short-run shocks (e.g., short-run deviations from secular trends in prices or in local supplies of contraceptives) we could include the current value of such shocks in relation (1) to purge the disturbance term of this effect and/or use lagged values of such shocks that are not correlated with the current values to instrument the social network variable in relation (1), which would eliminate this potential source of bias in the estimates (e.g., Alderman, *et al.* 2001). Unfortunately, as noted above in our discussion of the instrumental variable alternative, we do not have observations on such variables. However, as we also note in that discussion, our *prior* is that such short-run shocks are not likely to be major factors in the determination of decisions that have substantial long-run implications and therefore are likely to be dominated by expectations regarding longer-run prices, interhousehold relations and community characteristics (the persistent part of which the fixed effect estimates control for perfectly). But this is a maintained hypothesis that we are not able to test in this study.

this period and individual characteristics. If these regressions reveal that the initial stock of social interaction matters, then it indicates that there are differential opportunities for new social interactions between surveys depending on how many conversations the respondent already had in the past. Moreover, if in these regressions the individual characteristics are insignificant, this supports our hypothesis that changes in the social networks over time are no longer correlated with individual fixed effects. Fourth, we also present a specification test in which the dependent variable is future network characteristics and among the right-side variables is current contraceptive use. Logically, if the causal influences are from social networks to respondent's current contraceptive use, the latter variable should not affect future network characteristics. But current contraceptive use may be significant in OLS estimates of this specification because it proxies for unobserved fixed effects. If so, it will not be significant in fixed effects estimates.

Some Further Caveats and Robustness Tests: We also consider some further aspects of the assumed specification that might be violated and the extent that we can explore the robustness of our estimates to the violation of these assumptions.

Specification of the dependent variable: We have focused in this section on current use of contraceptives as reported by women as the dependent variable. To see how robust are our estimates to this choice, we also undertake estimates for two other family-planning-related variables as reported by women (ever-use of contraceptives, want no more children), current contraceptive use as reported by men and an a variable that captures the extent to which women perceive themselves to be at risk of AIDS (discussed in Section 3).

A further question concerns measurement error in the dependent variable. Usually this is ignored because random measurement error in the dependent variable does not cause biases. But, based on comparisons between reported contraceptive use by men versus women, it appears that there may be some systematic measurement errors that result in men giving higher reports than women (e.g., see the summary statistics for contraceptive use given in Table 2 below; see also Miller, Zulu and Watkins forthcoming). Perhaps, for example, men over-report use because the use of modern contraceptives is thought to be an indicator of being "modern." Or, on the other hand, women may under-report use because, though they are using modern contraceptives, they do not wish to acknowledge such use given some question about how socially acceptable is such use. Such systematic tendencies to over- or under-report are likely to result in biases in OLS estimates of the impact of social networks on contraceptive use – probably upward if there is systematic over-reporting and downwards if there is systematic under-reporting. But to the extent that for an individual such systematic mis-reporting tendencies are constant over time, they are controlled in the fixed effect estimates.

Specification of the social network variable: We firstly consider alternative empirical representations of social networks: the number of network partners who use contraceptives and the number of network partners not using contraceptives for the dependent variables related to family planning, and the number of network partners with high perceived AIDS risk and the number of network partners with low perceived AIDS risk for the dependent variables related to concern about AIDS.

Secondly, we consider nonlinear effects – in particular, whether the marginal impact of having one network partner with a given behavior or characteristics (e.g., using contraceptives, having high perceived AIDS risk) is different from those of having more network partners with such characteristics. Thirdly, we consider how our estimates vary when we make opposite extreme assumptions regarding whether network partners who are not among the four for whom information was collected on contraceptive use either all are non-users or all are users (see the discussion of the network partner data in Section 3).

An additional possible problem regarding our representation of network partners is that it may incorporate random or systematic measurement error. Random measurement error in a right-side variable, as is well-known, biases estimated coefficients towards zero and is exacerbated in fixed effects estimates because such measurement error 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 are not able to control for random measurement error. So to the extent that when we ask a respondent how many network partners does she have and she responds with the true number plus or minus a random term, we underestimate the effect of networks. Systematic measurement error would occur if, for example, respondents systematically under-report their number of network partners because they simply do not recall them all or they over-report contraceptive use by their network partners because they perceive the research team to favor family planning and they wish the team to learn that their friends are the sort of people of whom the team would approve. Systematic measurement error can bias the estimates in either direction, depending on its nature. If respondents systematically understate their number of network partners and do so more the larger is the true number of their network partners, for example, the result is likely to be an upward bias in the estimated impact of the number of network partners in OLS estimates. (Intuitively, the variable that is used in the estimates is smaller than the true number but it represents the effect of the larger number by attributing greater than true effects to the reported number.) If respondents systematically overstate the proportion of their network partners who are using modern contraception, as seems to be the case in our data (White and Watkins 2000), the result is likely to be a downward bias in the estimated influence of contraceptive use by network partners. To the extent that individual respondents always misreport their number of network partners by the same amount (though this amount may differ across respondents), our individual fixed effects control perfectly for systematic measurement error.

<u>Assumption about the distribution of the disturbance term</u>: For the results on which we focus in the text, as in this section, we use a linear probability model because the role of fixed effects is more transparent in this linear approximation. But for dichotomous dependent variables the assumptions of normality and homoscedasticity of the disturbance term are violated. We adjust for these violations of the classical OLS model by using robust standard errors, and with this modification the linear probability model (with fixed effects) provides consistent estimates of the parameters in relation (1) and their standard errors. In addition, we present in the appendix fixed effect logit and random effect logit estimates for comparison. The former is analogous to the fixed effect regression, while the latter is

analogous to OLS. Despite the fact that the fixed effect logit estimates are based only on the subset of women who change their contraceptive behavior over time, which is a disadvantage of this method as compared to the linear probability model, we note here that these alternative estimates indicate the same patterns of coefficients as are discussed in the text on the basis of linear probability models.

SECTION 3: STUDY CONTEXT AND DATA

Context: Our empirical analysis uses data that we collected in rural South Nyanza District, Kenya. The vast majority of the inhabitants in this area and of the survey respondents are Luo, a group characterized by exogamous marriage and patrilocal residence. Men who are *de jure* residents of a village are related to each other through a common ancestor. Women, however, must adjust their networks at marriage when they move to their husband's community, although they retain links with their natal families who often reside in other parts of Nyanza Province.

Most residents are engaged primarily in subsistence agriculture, supplemented by small-scale business, some wage labor, and occasional remittances from urban relatives. Cash necessary for such expenses as school fees and clothing is obtained from remittances, wage labor, or, especially for women, small scale retailing (e.g. buying bananas in a larger market and reselling them locally). Almost all respondents live in mud huts with thatched roofs, although some have more costly metal roofs. Although most men and women have attended school, few in our sample have studied beyond the primary grades. Only 13.4 percent of the 926 women whom we interviewed in 1994 had been to school beyond the primary grades. Those with more education tend to seek work in the cities, particularly Nairobi and Mombasa; those who do not find jobs in the cities often return to the rural areas and are engaged in much the same activities as those who have never been to school. The area has experienced less socioeconomic development than most other parts of Kenya, in part due to the history of political opposition in a country where patron-client relations based on ethnicity are important for the distribution of governmental resources (Weinreb forthcoming).

The characteristics of Luo social networks are constrained by the setting in which social interaction takes place: these include poor means of communication with those outside the area, the relative lack of economic and social stratification, and normative rules. Yet none of our sample sites are completely socially isolated. In the first round of our household survey conducted in 1994/95 (and described in more detail below), one half of the respondents reported owning a radio and 37 percent of women and 61 percent of men had spent six months or more after marriage outside their village, usually in a city. Women leave the area to visit natal kin living elsewhere in Nyanza or a husband who has migrated for work. Funerals, which are frequent due to the high level of AIDS, bring relatives and friends to the area for a few days. Nonetheless, because transportation is irregular and expensive and telephones very few, frequent social interaction is largely restricted to interactions with other members of the local community.

In South Nyanza, both modern methods of family planning and AIDS arrived in the past decade. The model of a small family achieved through the use of modern methods of family planning was introduced by the Kenyan government at the urging of the international population movement in the

1960s and 1970s, but only reached Nyanza through local clinics in the late 1980s and early 1990s (Watkins and Hodgson 1997). This model was initially perceived as a model of reproduction foreign to Nyanza because it was associated with whites and with a Kenyan government dominated by members of other ethnic groups than the Luo. That foreign model has slowly become domesticated–transformed into a local Luo model–and contraceptive use has increased and fertility has begun to decline (Watkins 2000).[7] The domestication of the foreign model of reproduction appears to have occurred in part through local 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 to assess their network partners support of or opposition to the new reproductive model. Although many in our sample voice desires for a smaller number of children than their mothers bore, there is still considerable ambivalence about the best family size. Modern methods of family planning are of even more intense interest, and there is a great deal of discussion, especially in the women's networks, about the effects of these methods on women's bodies (Rutenberg and Watkins 1997).

Much the same process appears to be occurring with AIDS. Although the first case of AIDS was officially diagnosed in 1985, as late as 1994 when we conducted exploratory fieldwork we heard little about it, and when it was mentioned it was as a disease that was relevant to those who lived in towns, not in rural villages. By the time of the second round, AIDS was far more evident (funerals frequently disrupted the data collection, and deaths to our sample indicated a doubling of death rates for adult men) and had come to be seen as a local problem. More than 85 percent of women knew about at least one recent death for which they suspect AIDS as a potential cause, and more than 30 percent knew about more than five such cases. AIDS was a frequent topic in social interactions: over three-quarters of the women had talked with at least one person about AIDS. Many respondents had come to perceive that they themselves were at risk of AIDS and were quite worried , and over two-fifths of the women have talked with at least one network partner who felt herself to be at a moderate or great risk of infection . Respondents know how AIDS is transmitted, and are primarily concerned about transmission through sex. In interaction with their social networks, they are considering strategies for prevention. These strategies are quite gendered. Men's primary strategy concerns extramarital partners: they exchange information with network partners about the likelihood that a potential network partner is infected, as well as assessing the social acceptability of condom use with partners perceived not to be safe (Watkins and Schatz 2001). Women's primary concern is that they will be infected by their husbands, who many suspect may have extramarital sexual relations. Because condom use to prevent the transmission of STDs or HIV is considered incompatible with marriage (only 1 percent of the husbands in our survey said that condoms could be used in marriage or even in a regular relationship), their strategies are

[7] Nevertheless, contraceptive prevalence is relatively low compared to some other parts of Kenya. Our household survey found 12.3 percent of married women were currently using family planning in 1994/95, 17.7 percent in 1996/97 and 17.4 percent in 2000. In addition, 23.0 percent of married women had ever used family planning in 1994/95, rising to 30.6 percent in 1996/97 and 31.0 percent in 2000.

directed toward persuading their husband to eschew extramarital relations. We have little information on what husbands and wives say to each other regarding AIDS in South Nyanza, but in semi-structured interviews with 150 couples in Malawi, a country which in relevant respects is quite similar to Kenya, we asked how these conversations arose and what was said (this study is described in http://www.pop.upenn.edu/networks). There is a clear pattern in the form and content of these discussions. Both husband and wife typically described the conversation as introduced by the wife, often at a peaceful time such as after dinner, and provoked by an event such as a funeral or seeing someone who had symptoms believed to characterize someone with AIDS (e.g. extreme thinness), hearing something about AIDS on the radio or at a clinic, or by a conversation with a network partner. The wife then focused on the risk that AIDS would leave their children orphans, and they tactfully picture the solution as joint–"we must take care." The similarity across couples in the way the wives introduce the topic of risk and focus on the children suggests that women's strategies with respect to their husbands may be a product of discussion with their their network partners.

Data: The data were collected by Watkins and colleagues in the Kenyan Diffusion and Ideational Change Project (KDICP). The KDICP consists of a longitudinal household survey, and a set of semi-structured interviews and focus groups that were collected during 1994/95, 1996/97 and 2000 in four rural sublocations (administrative units) in South Nyanza District, Nyanza Province, Kenya. We describe the data briefly here; more details, the data, and analyses of data quality 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 of the rural sites. 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 re-interviewed these women and men (and any individuals who were on the first round sample list, had not been located during that round, but were located in the second round), followed by a third wave in January and February 2000 (Kenya 3). Some respondents were present at the time of all three waves, some only at one. Table 1 gives the number of respondents for each of the three data rounds and the numbers who are in all three rounds. In total, 497 women (324 men) participated in all three survey waves and reported information on the relevant individual and network characteristics (498 women for the dependent variable having ever-used contraception), and 545 women (408 men) participated in the last two rounds of the data collection that contained questions pertaining to AIDS. 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.

TABLE 1 ABOUT HERE

Table 2 gives summary sample statistics for the variables that we use in our analysis, which we now briefly discuss.

TABLE 2 ABOUT HERE

Dependent variables (Y_{it}): For family planning we focus on whether a woman is currently (at the time of the survey) using contraception. The proportion of women currently using contraception increased from 12 to 17 percent between Kenya 1 and 2, but stayed at 17 percent in Kenya 3. We also consider for comparative purposes whether the woman has ever used contraception and whether she wants more children. Whether women have ever used family planning also increased between Kenya 1 and Kenya 2 but then remained at the same level as in Kenya 2 for Kenya 3. The stated desire for no more children increased slightly throughout the period, though at a lower rate between Kenya 2 and Kenya 3 than between Kenya 1 and Kenya 2. In addition, we consider whether a man reports current contraceptive use, which ranges between 20 and 24 percent. As we discussed in Section 2, the higher level of contraceptive use for males may indicate systematic misreporting of family planning use by males or by females.

For the AIDS analyses we use the respondent's perceived risk of AIDS–often believed to be a determinant of protective behavior-- as the dependent variable.[8] Data on respondents' perceived risk ("worry") of becoming infected with AIDS were collected as a categorical variable with four options: none (0), some (1), moderate (3) and great (4); we construct a continuous variable using the values indicated in parentheses. The summary statistics in Table 2 reflect a gender difference in perceived AIDS risk, with women reporting more concern than men. Substantial changes in perceptions of risk between Kenya 2 and Kenya 3 are associated with other changes between the two surveys: for example, those women who reported that their husband was unfaithful in Kenya 2 but faithful in Kenya 3 perceived their risk to have declined, whereas those who had come to believe their spouse was having extramarital sexual relations became more worried (Watkins and Schatz 2001).

The specification of relation (1) is developed in terms explicitly of current use of contraceptives, but the details in regard to timing carry over directly to wanting more children and the extent to which respondents perceive themselves to be at risk of AIDS. The use of this specification for whether she has ever used contraceptives, however, is more problematic because this dependent variable refers not only to current but also to past behavior. Therefore stochastic shocks that affected social networks in the past also affected the dependent variable, which may result in spurious correlations that could confound the identification of causal effects. Despite these caveats we have undertaken estimates with this dependent variable for comparison.

Individual characteristics (X_{it-}): The time-varying variables that are included in our analyses as controls include the number of births prior to the current period, not being married,[9] having a radio and

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[8] Alternative dependent variables might include numbers of extramarital sexual partners and condom use, but our evidence suggests that the reporting of these variables is quite unreliable.

[9] 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' home and therefore generally could not be located).

having a metal roof. As one would expect, these variables tend to increase between 1994/95 and 2000. Women in 2000 tended to have about 5.3 children, approximately 60 percent had a radio and about 40 percent a metal roof. For our OLS estimates we also include a set of individual characteristics, such as the level of schooling and age, that drop out in the fixed effects estimates. (Schooling actually increases for some respondents over time, but the variation is not sufficient to identify the schooling coefficient in the fixed effects estimates; therefore in the OLS regressions we use the schooling and age as measured in Kenya 2).

Social networks: In the each of the survey waves we asked respondents with whom they had talked about family planning. In Kenya 2 and Kenya 3 we also asked respondents with whom they had talked about AIDS risks. The survey questions in both cases used the word "chat" in order to indicate that we were interested not in lectures or counseling sessions at the clinics, but rather in informal interactions. Both family planning and AIDS are clearly prominent topics in social interactions. In both of these rounds at least 75 percent of the women reported having discussed family planning or AIDS with at least one network partner, and these percentages increased to 88 percent by Kenya 3. On average, women talked with 2.9 to 4.6 network partners about family planning (3.9 to 4.6 for AIDS). Detailed questions were asked about the characteristics of a maximum of four of these partners, including (for the family planning networks) whether the network partner used family planning and (for the AIDS networks) what is the network partner's perceived degree of worry about AIDS.[10] This format produced sets of respondent, or ego-centered, networks with up to four network partners.[11] The average size of these "censored" networks is between 2.2 to 2.8 for family planning and between 2.4 to 2.9 for AIDS. About one third of women talked to more than four network partners about family planning, and more than one half of the women talked to more than four others about AIDS. Due to limited time in the questionnaire, no detailed information on family planning and AIDS related behaviors and altitudes were collected for network partners beyond the first four. In our analyses below, we will therefore explore

[10] For the network partners' contraceptive use, we did not ask respondents to distinguish between current use and ever use at the time of the survey; because contraceptive discontinuation is frequent and because the conversations occurred in the past, the respondent may not have known the current contraceptive use status of her network partners. For the network partners' perceived risk ("worry") of becoming infected with AIDS, on the basis of exploratory analyses, we dichotomized this into "low" levels of worry (none and some perceived risk) and "high" levels of worry (moderate and great).

[11] Our (and others') measurement of network characteristics face several limitations. (1) They do not indicate the relative importance of the various discussions for individual women or provide information on the content of the conversations. (2) As noted in the text, we follow the practice common in ego-centered network analyses of asking specific questions about only a subset of network partners for those who report large networks. Because the choice of whom the respondent discusses in such cases may not be random, there may be biases embedded in the data, we would conjecture that the conversations that the respondent recalls are more important. (Kohler 1998 found bias in the estimates of density for truncated ego-centered network data). (3) Our network measurements are based on recall and, as with any data reconstructed from memory, potential biases exist. Brewer and Webster (2000) and Brewer (2000) found that respondents tend to forget about a fifth of their personal network partners when asked to list them from memory. To the extent that these measurement problems are persistent over time for each respondent, as discussed above, they are likely to cause biases in OLS estimates, but be controlled in the fixed effects estimates. Nevertheless, our network measurements and estimates based on them must be interpreted with some caution.

alternative assumptions about the distribution of these behaviors/attitudes among these network partners on whom we do not have detailed information.

In total, there are 2039 network partners in women's family planning networks in Kenya 1, 1,867 in Kenya 2, and 2626 in Kenya 3 for whom we have detailed information about contraceptive behavior, their relation to the respondent, and some socioeconomic characteristics such as education. This information about the network partners is reported by the respondent, and the variation in the total number of network partners across survey waves is largely due to the different number of respondents in the three surveys waves (see Table 1). In Kenya 2 and Kenya 3 respondents were also asked the same questions about their informal conversations about AIDS (they were not asked in Kenya 1 because, as noted earlier, at the time of our exploratory fieldwork in early 1994 AIDS was not an open topic of discussion). There were a total of 1730 network partners in women's AIDS network in Kenya 2 and 2689 in Kenya 3.

A clear pattern in Table 2 is the marked trend towards larger family planning and AIDS networks. For both males and females, the fraction of respondents who had conversations about AIDS and family planning has been increasing over time. For instance, while 25 percent of women in Kenya 1 reported no communications about family planning, this fraction declined to 12 percent in Kenya 3. The reasons for this increase are twofold. First, if respondents do not forget their past conversations, the number of network partners can not decrease over time and the reported increase could be only due to the fact of accumulating more conversations over time without indicating an increased frequency of such conversations. We believe, however, that the increase is social network size and the number of family planning users in these networks is also due to more frequent interactions about family planning and AIDS. Although the questions about conversations about family planning or AIDS in the questionnaire did not specify a time frame for these conversations, it seems as if respondents primarily include relatively recent interactions among the first four network partners about whom detailed information was collected. For instance, the female respondents 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 percent (or 1,455 cases) the most recent communication with the respective network partner about family planning has been in the period since 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 (we only know the lower bound since their could have been multiple conversations with the same network partner, and also some respondents' networks have been censored; Table 2 shows that this extent of censoring is increasing over time). We cannot disentangle for our data to which extent the recent conversations in Kenya 3, i.e., the interactions that have occurred during the period since Kenya 2, are with network partners who were already part of the network in Kenya 2. However, the above data about the most recent conversations clearly indicate that respondents referred to relatively recent conversations about family planning. Changes in the size and composition of the family network therefore seem to reflect the extent of recent conversations about family planning, and we therefore believe that the information about network partners in Table 2 indicates an increase in the frequency of interactions about family planning and not merely the accumulation of more network partners due to a longer time span in which interactions may have occurred. (Similar analyses for the other periods and networks show results that are comparable to the above family planning example.) The increase in the number of contraceptive users in the family planning networks can therefore occur because a network partner, who was already mentioned at an earlier wave, adopts family planning over time or because women are more likely to encounter family planning users instead of non-users in their most recent conversations.

Because the identification of the network effect in the fixed effect regression (1) is based on within-individual variations in the size and composition of the family planning or AIDS network over time, we report in Table 3 summary statistics for the within-individual variation in the primary network representations that are used in the subsequent estimations. For family planning, these deviations from the within-individual average reflect the change towards an increased level of current use across individuals, and they reflect the increased social interaction with both users and non-users. Moreover, the standard deviation of these individual-level deviations from the average is relatively constant in each variable across all waves, indicating that in all periods the contraceptive use and our primary network characteristics varied to an approximately equal extent from their individual averages. For AIDS, the summary statistics in Table 3 indicate a very weak trend towards a reduced perception of AIDS risk for respondents, and tendency towards an increased number of network partners who perceive respectively a moderate/high or low/no AIDS risk.

TABLE 3 ABOUT HERE

In Table 4 we further report regressions of the *change* in the number of family planning users and non-users between surveys in the respondent's family planning network on the initial number of users and non-users in the network. The pattern emerging from these regressions is that a smaller initial number of users (non-users) is strongly associated with larger changes in the number of users (or non-users) inter-survey period. Moreover, having an initial larger number of non-users in the network is also associated with larger increases in the number of users. This suggests that some respondents "replace" their non-users with users, or alternatively, that the respective network partners adopt contraceptives over time. Moreover, funerals seem to be an important catalyst of social interactions about family planning, and respondents in villages with more frequent funerals, many of which are due to AIDS related deaths, tend to have more users and non-users of family planning in their networks. Time-varying individual characteristics, such as having a metal roof or 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 non-users 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 due to their higher age or fertility. Finally, women with secondary education also tend to have larger changes in the number of users over time.

TABLE 4 ABOUT HERE

These changes in the number of users and non-users in respondents networks between survey periods is consistent with our theoretical discussion in Section 2 about the determinants of network change. Moreover, the dependence of the changes in the number of users or non-users in respondent's networks on the initial size and composition of the network and fixed individual characteristics such as age and higher education is likely to be due to individual fixed effects f_i. These effects, however, are removed from relation (1) in our fixed effect estimations.

Analysis of sample attrition: As noted, we conduct our analysis for a subsample of respondents for whom we have data on all three rounds (see Table 1). In particular, the analyses for family planning are based on 55 percent of the sample population contacted in Kenya 1, and the analyses for AIDS are based on 75 percent of the respondents who participated in Kenya 2 when AIDS related questions were introduced in the questionnaire. Many analysts have the intuition that attrition is likely to be selective on characteristics such as schooling and thus that high attrition is likely to bias estimates made from longitudinal data. Most studies of attrition that we have found are for large longitudinal samples in developed countries, several of which appeared in a special issue of *The Journal of Human Resources* (Spring 1998) on "Attrition in Longitudinal Surveys." The striking result of these studies is that the biases in estimated socioeconomic relations due to attrition are small**—**despite attrition rates as high as 50 percent and with significant differences between the means of a number of outcome and standard control variables for those lost to follow-up and those who were re-interviewed.

We undertake similar analyses of attrition for our sample. We find that there are a fair amount of differences between our sample respondents for this study and those lost to the sample for at least one but not all three rounds: the latter tend to have higher education and be younger, for example, which *a priori* is plausible if women with such characteristics are likely to be more geographically mobile (Appendix Table A1). Some of the differences in the means are significant. But, as noted in the literature for developed countries to which reference is made above, differences in means of observed characteristics of those not continuously in the sample versus respondents for all sample rounds does not necessarily mean that there are differences in the coefficient estimates of the relations of interest. To explore this possibility we conduct the Becketti, Gould, Lillard, and Welch (1988) (BGLW) test in which we estimate for each round of data the OLS linear probability model for contraceptive use, but we let each of the coefficient estimates possibly differ between attritors and the respondents for whom we have data in all three rounds (Appendix Table A2).[12] Tests of joint significance for the possibility that the coefficient estimates differ between attritors and respondents indicate no significant differences (the tests are not significant at a 25 percent or higher level). It therefore appears in our data, as in the corresponding analyses for developed country data (e.g., Panel Study on Income Dynamics), that

[12] This test does not, of course, control for individual fixed effects and cannot be conducted with individual fixed effects because such estimates can not be made for those absent for a survey round. The maintained assumption, therefore, is that the bias due to unobserved variables is identical for all members of the sample, whether or not they were present at all three survey rounds.

attrition is selective in the sense that mean characteristics of those who were not interviewed in all three rounds sometimes differ from the respondents who are included in all three rounds. However, such differences do not distort significantly the coefficient estimates in these multivariate relations, and the estimated coefficients tend to be robust despite substantial attrition in our longitudinal survey.

SECTION 4: ESTIMATES OF THE IMPACT OF SOCIAL NETWORKS ON FERTILITY-RELATED BEHAVIORS AND ATTITUDES

Table 5 gives a set of estimates for the linear probability model in relation (1) for women reporting currently using contraceptives. The number of respondents used for each estimate is 497. Each respondent contributes three observations: one in which the current period is for Kenya 3, and two additional observations in which the current period is for Kenya 1 and Kenya 2. There are four pairs of estimates that differ only in their representations of social networks:

(1) number of network partners using contraceptives,

(2) number of network partners using contraceptives and number of network partners not using contraceptives,

(3) whether at least one network partner uses contraceptives and number above one of network partners using contraceptives, and

(4) whether at least one network partner uses contraceptives, number above one of network partners using contraceptives, whether at least one network partner does not use contraceptives and number above one of network partners who do not use contraceptives.

Each pair includes our *a priori* preferred individual fixed effects estimates for relation (1) and, for comparison, OLS estimates. For each estimate the right-side variables include a control for the survey round. Robust standard errors are reported in parentheses beneath the point estimates. These account for potential heteroskedasticity and, for the OLS estimates, the correlation in the disturbances for the same individual across time. The corresponding fixed effect (and random effect) logit estimates are presented in Appendix Table A3. The interpretations of these logit estimates agree with those of the linear probability model, on which we focus in the text.

TABLE 5 ABOUT HERE

The individual fixed effects estimates indicate that the probability of women currently using contraceptives increases significantly by a little more than 0.038 for every network partner on the average who is a user. However this average effect primarily is the effect of having at least one contraceptive user among the network partners. Having one contraceptive user among the network partners increases the probability of the female respondent being a current user significantly by about 0.054. The point estimate indicates that having additional network partners who are users beyond the first one increases the probability of use by about 0.032 for each additional network partner. Moreover, the respondent’s contraceptive use is exclusively related to network partners who are themselves users of contraception. In none of the fixed effect estimates in Table 5 does the number of non-users of family planning, or the presence of at least one such non-user, have a significant or substantial effect.

The estimated fixed effect coefficients in Table 5 for the influence of family planning users in the social network on the respondent's own contraceptive use *prima facie* may seem small -- significantly different from zero but not very substantial. For instance, the increase in the probability of using contraceptives equals 0.05 if one has a network partner who uses contraceptives, controlling for all fixed characteristics that might affect both social networks and contraceptive use. Does this constitute a small or a large effect? Our answer to this question is that, in comparison with the mean levels of contraceptive use that are summarized in Table 2, this is a substantial effect, increasing the probability of use by roughly one third at the sample means.

Our preferred estimates control, as noted, for all unobserved fixed characteristics that might affect both current contraceptive use and social networks. For reasons that we discuss in Section 2 our *prior* is that the fixed effects estimates are preferred over the OLS estimates due to unobserved fixed factors that would seem to affect both. In addition, Hausman specification tests for all models in Table 5 reject the null hypothesis that the OLS models are appropriate and these tests therefore strongly suggest the necessity to account for unobserved individual characteristics through fixed effects (or alternative methods).

Do our controls make any substantive difference? The answer to this question is definitely yes. The first set of estimates, for example, indicates that the OLS estimates of the average effect of contraceptive users in social networks on women's current contraceptive use is biased upward by 180 percent. The third and fourth sets of estimates indicate that the OLS estimates understate substantially the effect of having at least one contraceptive user in one's social relative network relative to that of having subsequent network partners who are users. Furthermore, the fourth set of estimates indicates that OLS estimates distort the impact of having non-users in the network, and make it appear that effect is weakly significant even though it is not in fixed effects estimates. The finding that OLS overestimates the influence of social networks on contraceptive use also indicates that the unobserved individual and community factors, represented by the term f_i in relation (1), are positively correlated with the respective representation of the social network, N_{it-}. In the first and second set of estimates in Table 5, for instance, the direction of change between the fixed effect and OLS estimates suggests that women with unobserved characteristics in f_i, that increase her probability of contraceptive use, also tend to have more social network partners who are users of family planning and fewer who are non-users (although the latter effect is weak).

In summary, the estimates in Table 5 suggest that (i) having a contraceptive user in a woman's social network prior to the time of the survey has a significant and substantial effect on the probability that she currently uses contraceptives even with control for such unobserved factors that may affect both the propensities to use contraceptives and social networks, and (ii) OLS estimates based on the assumption that social networks are random result in substantial biases in the estimated impact of prior social networks on women's current contraceptive use.

The estimates in Table 5 include exploration of some aspects of the representation of social networks. We also have explored some other aspects of the specification, as noted in Section 2. For example, we investigate what happens if all of the network partners who are mentioned beyond the first four (the only ones for whom contraceptive use was asked) are assumed to be non-users or, alternatively, users. As noted in Section 3, between 23 and 37 percent of the respondents mentioned more than four network partners but, due to the need to keep the interview of manageable length, information was asked (including contraceptive use) only for the first four network partners mentioned.

Table 6 presents the fixed effects estimates with the second representation of social networks in Table 5 for (i) the extreme assumption that all network partners beyond the first four are not users, (ii) the opposite extreme assumption that all network partners beyond the first four are contraceptive users, and (iii) an intermediate assumption that all network partners beyond the first four are users or non-users in the same proportion as the first four network partners. Our intuition is that the first assumption is likely to be much closer to reality than the second or third assumption given (a) the usage rates reported by the respondents themselves in the sample and (b) a probable tendency to mention first those network partners who are users (even though there was no instruction to do so) because conversations with such network partners may have seemed more relevant in a context where outsiders have come to ask questions (see Miller, Watkins and Zulu, forthcoming). The estimates in Table 6, particularly for the first assumption, are basically consistent with those in Table 5. The first one yields an identical significant coefficient estimate for the number of users in the network as in the corresponding model in Table 5. The coefficient estimate on the number of nonusers remains insignificant but becomes much smaller, as would be expected given the increase in the mean of this variable with this adjustment. The last assumption, a proportional distribution of network partners beyond the fourth, is also consistent with the estimates in Table 5, although there is a substantial reduction in the coefficient (from .038 to .013) due to the increase in the mean and variation of the number family planning users in the network under this assumption; nevertheless, the effect of interacting with family planning users remains significant even in this specification. The second assumption, on the other hand, yields a much smaller and less significant (nonzero only at the 14 percent level) coefficient estimate for the number of network partners who are users, apparently because the mean number of users under this extreme assumption is much higher (so the coefficient estimate declines to obtain the same effect) and because the highly unlikely assumption that all network partners above four are users introduces a lot of noise into this variable (which, to the extent that it is random, biases the coefficient estimate towards zero and lessens the precision).

TABLE 6 ABOUT HERE

Another aspect of the specification that we have explored is whether the results are robust if we use alternative dependent variables. Table 7 presents estimates parallel to those in Table 5, but with having ever-used contraceptives as the dependent variable instead of currently using contraceptives. As we discuss in Sections 2 and 3, on *a priori* grounds we prefer the dependent variable “currently using contraceptives” because the stochastic term in relation (1) for this variable is more 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. But the estimates in Table 7 suggest that very similar results to those in Table 5 are obtained if the dependent variable is ever having used contraceptives. The estimated network effect is somewhat larger than in the estimates for current use, and at least one user in the network is not more important than the remaining number of users. However, similar to the earlier results, OLS substantially overestimates and in part mis-represents the effects of social networks on women's contraceptive use. The analogous results for the dependent variable indicating that a woman wants no more children, which are not presented here, also provide consistent estimates to those reported in Table 5 and 6, although the estimates for wanting no more children are more imprecise than those for contraceptive use.

TABLE 7 ABOUT HERE

A different sort of specification test that we mention in Section 2 is to explore whether past contraceptive use predicts current social networks and whether such predictions, if significant, remain significant with control for individual fixed effects. The first estimates in Table 8 is the 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 (referred to as time *t*) and the past (referred to as time *t* - 1) period. The estimates suggest that a respondent's past contraceptive use is a powerful predictor of the current number of her network partners who use contraception, with a coefficient estimate indicating that past users have 0.63 more current network partners who are users than do past nonusers (an estimate that has a t ratio of 4.43). Similarly, past contraceptive use is a powerful predictor of the current size of the family planning network. Of course, this predictive power may only reflect preferences for homophilous network partners – women who are "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 estimates in Table 8 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 percent 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 estimates that are made without controlling for them may be very misleading for identifying causal effects, even if the associations in such estimates appear strong.

TABLE 8 ABOUT HERE

All of the estimates that we have discussed to this point are for family-planning related variables for females. A related question is whether there are parallel results for males. The demographic literature on social networks has tended to focus on females, which may reflect a perception that females are more engaged in such networks or more central in contraceptive choices. But certainly casual observations in the sample villages suggest that males spend a lot of time in informal social interactions, and they report "chatting" about family planning about as much as do females. Table 9 presents estimates for males parallel to those in Table 5 for females (The corresponding random and fixed effect logit models are presented in Appendix Table A4 and yield very similar results). Comparisons between these two sets of estimates for males indicate: (1) Social networks have significant positive impact on contraceptive use for males as for females, even when unobserved individual fixed factors are controlled. Despite the frequent protestations by 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 much larger effects for males than for females. (2) The fixed effect estimates differ in some other respects besides the magnitude between males and females. In particular, those for males indicate greater (and significant) effects of having additional network partners who are users beyond the first one. (3) The OLS estimates for males appear to be less biased – sometimes substantially less – than are those for females.[13]

TABLE 9 ABOUT HERE

Finally we note that in estimates that are not presented for brevity, for males, as for females, the social network effects carry over to other dependent variables, such as having ever used contraceptives and wanting no more children (in fact in the latter with stronger evidence of significant coefficient estimates than for females).

SECTION 5: ESTIMATES FOR CONCERN ABOUT AIDS RISKS

In our discussion of the context in South Nyanza in Section 3 we mentioned substantial informal discussion of AIDS, provoked by worries about the risk of contracting AIDS. Given the frequency of funerals, it is not surprising that only a minority of the respondents reported that they were "not at all" worried about AIDS, and that the topic was discussed intensively with network partners and with spouses. Conversations about AIDS with network partners occurred in much the same circumstances as those about family planning, for example while women are going about their daily activities or visiting with each other in their homes. Although AIDS is perceived to be very different from family planning–AIDS is associated with promiscuity, whereas contraception is considered a family matter--the characteristics of the network partners in the AIDS conversational networks are very similar to those in the family planning conversational networks (Watkins and Warriner 2000).[14]

The data that we collected on concerns about AIDS and AIDS social networks in Kenya 2 and Kenya 3 permit us to investigate whether social networks have an impact on being worried about AIDS.

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[13] The relatively small change in the estimates for males suggests that the biases towards zero due to random measurement error that often have been emphasized in concerns about fixed effects estimates (e.g., Griliches 1979, Ashenfelter and Krueger 1994) probably are not all that large. This also gives us more confidence that the differences between the OLS and the fixed effects estimates for females are substantially due to control for important unobserved fixed effects and not just an artifact of random measurement error because we have no reason to expect that random measurement error would be so much larger for females than for males as would be required were that the explanation for the difference between the OLS and fixed effects estimates in Table 5. (Systematic measurement error may differ by gender but, as noted in Section 2, any individual systematic over- or under-reporting is effectively part of what is controlled in the fixed effects estimates.)

[14] The AIDS network has substantially more men (26 percent of women's AIDS network partners are men, compared to 4 percent men in the family planning network); they are also more likely to be better educated than the respondent's family planning network, less likely to have a similar level of education as the respondent (more better educated and more less well educated), more likely to be in frequent contact (daily or weekly) with the respondent, and more likely to be friends (rather than confidantes or acquaintances).

The role of social networks in affecting concerns about and behaviors related to AIDS, of course, is of substantial interest in itself. In addition this exploration permits comparisons with the investigation in Section 4 above about the impact of social networks on family planning. The dependent variable that we use in this section is the respondent's perceived risk of becoming infected with AIDS. Similar to our earlier analyses, we represent social networks by the extent to which the respondent's network partners are reported to be worried about AIDS (although in what follows we will refer to the network partners perceptions of risk, this perception is reported by the respondent). In particular, we include the number of network partners who perceive either no or only a small risk of AIDS infection, and we include the number of network partners who perceive moderate or great risks of getting AIDS. The analyses also contain all of the other right-side controls that we include for the analysis in Section 4.

Table 10 presents estimates for women's perceived AIDS risk of getting AIDS. These estimates are parallel to the second and fourth groups of estimates in Table 5 for current contraceptive use, with the exception that the dependent variable is only available at two instead of three survey waves. The analyses are therefore based on all women who participated in both Kenya 2 and 3. The results in Table 10 suggest that, parallel to the estimates for contraceptive use in Section 4, (1) there are substantial and significant social network effects on the perception of AIDS risk, (2) OLS estimates lead to biased estimates of these networks effects, and (3) the basic network effects depend on having at least one person with high perceived AIDS risk.

TABLE 10 ABOUT HERE

But there are also some noteworthy differences between these estimates and the ones in Table 5 for current contraceptive use. First, there is evidence of significant impact in the fixed effects estimates of both types of network partners, not only those with high perceived risks. These effects are smaller in absolute magnitude for those partners with low perceived risks than the effects of those with high perceived risks, particularly for the dependent variable being one's own perceived AIDS risk. In other words, the effects are not symmetrical: conversations with network partners who are worried heighten the respondent's perception that she is at risk of AIDS, whereas conversations with network partners who are not worried have weaker effects in reducing her concern. Second, for the own perceived AIDS risk dependent variable, the OLS biases tend to be downward rather than upward. For the second set of estimates, for example, the OLS estimates of the absolute magnitude of having at least one network partner with high (low) risks are 20-33 percent below the fixed effects estimates.[15] This suggests that the correlation of unobserved individual characteristics that affect positively one's own perceived AIDS risk with the number of network partners with either high or low risk is negative. This is consistent with a situation in which women who currently are relatively concerned talk a lot with others, perhaps hoping to learn something that would help them formulate strategies of protection that would keep risks

[15] As in Section 4 for the male estimates, these results reinforce our interpretation that random measurement error in measuring network characteristics is not a major part of the difference between our OLS and fixed effects estimates. The effect of random measurement error is to bias the fixed effects results towards zero, so it can not dominate if the fixed effects estimates are larger in absolute magnitude than the OLS estimates.

relatively low – although these conversations have the effect of increasing their anxiety. Women who are relatively unconcerned about their risks despite perceiving them to be high, however, may not try to learn as much from network partners as do those who are less fatalistic because there is less expected payoff to doing so.

6. CONCLUSIONS

Casual observations suggest that individuals do not make decisions in social isolation, but in interaction with each other. 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 might directly affect attitudes and behavior might also directly affect the units of social interaction. For example, women with preferences for network partners like themselves may interact with similar women, and all of those in a network may be exposed to the same market and community constraints and possibilities. Thus, what has been interpreted as the causal effects of social networks may simply be associations because the causal direction is unclear.

The availability of an unusual longitudinal data and uses of statistical methods that control for unobserved factors provide an unusual opportunity to extend the individualistic rational actor and neoclassical models to incorporate social interaction, and to estimate the causal effects of social networks on attitudes and behavior. Our major findings are:

First, and foremost, our analysis shows that social networks have significant and substantial effects even when we control for unobserved factors that also may 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 family planning and AIDS- related behaviors and attitudes. In addition, a corollary of our results is that network analyses predicated on the assumption that social networks are random are likely to be misleading.

Second, 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 or with high perceived AIDS risk. The inclusion of additional networks partners with the same characteristic or with the opposite characteristic generally has much smaller or insignificant effects.[16] The combination of this form of nonlinearity and asymmetry is consistent with the usual stereotypic diffusion models (e.g., Rogers 1995). If there are just a few who initially adopt an innovation, they have relatively large influence through networks because they interact with relatively large numbers of individuals who have not yet adopted; in such cases they provide these individuals with at least one adopter, the influence of which 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

[16] While these are generalizations, there are some exceptions, such as the impact of additional network partners using contraceptives beyond one on male contraceptive use.

social networks are likely to have large effects on attitudes and 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.

Third, the effects of networks are not confined to the use of family planning by women, the focus of much of the literature on networks in demography, but appear to be more general, influencing responses to HIV/AIDS, and influencing men as well as women.

Fourth, estimates of the effects of social networks based on the assumption that they are determined randomly, as in previous studies, may lead to substantial misunderstanding regarding the impact of social networks on individual behaviors. This is the case because such estimates ignore that there may be important factors such as homophily that determine social networks. For instance, women who are more likely to want to restrict their childbearing and thus use contraceptives are more likely to have in their social networks women who also use contraceptives.

Although the data are particular to rural Kenya and our examples are of specific interest to demographers interested in diffusion through social interaction, we believe the approach exemplified here is of wider use for those interested in social change. In particular, our results support the expectations in the literature on threshold models that a small amount of social interaction can facilitate and promote substantial social change. Specifically, 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). Our results suggest that the rabidity of these changes is related to only small amounts of interaction. Interaction with at least one network partner is critical, with additional network partners being much less important. These results also offer grounds for some cautious optimism to those concerned with the widespread pandemic of AIDS because social interaction may stimulate a potential for rapid change in relevant attitudes and preventive behavior.

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Table 1: Number of respondents in each of the rounds of the KDICP data

	number of women	number of women with non-missing data[a]	number of men	number of men with non-missing data[a]
Kenya 1 (1994/95)	923	909	744	572
Kenya 2 (1996/97)	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

Notes: (a) women/men with non-missing information on the variables included in the subsequent regressions (e.g., contraceptive use, education, marital status, etc.)
(b) For ever used contraception, there are 498 instead of 497 women with non-missing information

Table 2: Summary Statistics

	Females			Males		
	Kenya 1	**Kenya 2**	**Kenya 3**	**Kenya 1**	**Kenya 2**	**Kenya 3**
N	909	724	884	572	549	602
Individual Characteristics at t-						
Age	28.7	32.75	error	39.19	43.76	error
	(7.95)	(8.37)	in var!	(11.92)	(13.07)	in var!
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 radio	0.51	0.60	0.63	0.50	0.65	0.73
Has metal roof	0.20	0.25	0.41	0.19	0.27	0.41
Has at least primary schooling	0.80	0.79	0.82	0.90	0.90	0.92
Has secondary or higher schooling	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 chidren	0.32	0.39	0.41	0.22	0.28	0.32
Family planning (fp) network						
Proportion with at least one nwp in fp 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 4 network partners	0.23	0.33	0.37	0.29	0.32	0.41
Prop. with at least one nwp fp user in network	0.48	0.63	0.61	0.41	0.49	0.47
Number of fp users in 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)
Perceived AIDS risk, respondent						
Proportion perceiving no risk	--	0.25	0.20	--	0.28	0.21
Proportion perceiving small risk	--	0.35	0.44	--	0.38	0.53
Proportion perceiving moderate risk	--	0.26	0.27	--	0.23	0.22
Proportion perceiving great risk	--	0.14	0.09	--	0.11	0.04
AIDS network						
Prop. with at least one nwp in AIDS network	--	0.76	0.88	--	0.83	0.91
Uncensored size of AIDS network	--	3.90	4.62	--	3.96	5.10
		(3.62)	(4.08)		(4.08)	(4.90)
Censored size of AIDS network	--	2.38	2.91	--	2.70	3.26
		(1.61)	(1.42)		(1.52)	(1.27)
Proportion with more than 4 network partners	--	0.51	0.53	--	0.43	0.47
Proportion with at least one nwp who perceives moderate or great AIDS risk	--	0.42	0.53	--	0.48	0.48
Number of nwp who perceive moderate or great AIDS risk	--	0.91	1.06	--	1.09	0.93
		(1.28)	(1.24)		(1.37)	(1.19)
Proportion with at least one nwp who perceives no or small Aids risk	--	0.47	0.70	--	0.55	0.77
Number of nwp who perceive no or small AIDS risk	--	0.98	1.61	--	1.19	2.07
		(1.27)	(1.40)		(1.36)	(1.47)

Notes: Standard deviations in parentheses

Table 3: Summary statistics for deviations of family planning use, family planning network characteristics, AIDS perception and AIDS network characteristics from the within-individual average in the three survey waves
Only respondents who participated in Kenya 1 -- Kenya 3 (family planning) or Kenya 2 -- Kenya 3 (AIDS)

	Females			Males		
	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 (fp) network						
Number of fp users in 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-fp users in 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)
N	--	545	545	--	--	--
Perceived AIDS risk, respondent[a] (scaled 1 through 4)	--	0.005	-0.005	--	--	--
		(0.585)	(0.585)			
Aids network						
Number of nwp who perceive moderate or great AIDS risk	--	-0.095	0.095	--	--	--
		(0.839)	(0.839)			
Number of nwp who perceive no or small AIDS risk	--	-0.300	0.300	--	--	--
		(0.903)	(0.903)			

Notes: (a) because there are only two survey waves with AIDS information, the summary for the deviations from the within-individual mean are equal in Kenya 2 and Kenya 3 are equal (with the exception of sign differences)

Table 4: Females -- regression of changes in the number of family planning users and non-users in network between survey waves on the initial number of users and non-users and personal characteristics (Changes in the number of users are measured between Kenya 1 and Kenya 2, and between Kenya 2 and Kenya 3; the initial network composition is measured respectively at Kenya 1 and Kenya 2)

	number of users	**number of non-users**
# of nwp using family planning (fp), time t-	-0.722	0.015
	(0.035)**	(0.033)
# of nwp not using family planning (fp), time t-	0.102	-0.899
	(0.034)**	(0.033)**
village average number of funerals attended[a]	0.134	-0.095
	(0.034)**	(0.035)**
dummy for not married, time t	0.372	-0.226
	(0.251)	(0.225)
children ever born, time t	-0.008	0.008
	(0.020)	(0.020)
Respondent has radio, time t	0.110	0.095
	(0.085)	(0.089)
Respondent has metal roof, time t	0.122	-0.009
	(0.104)	(0.104)
Respondent has at least primary education	0.192	0.041
	(0.118)	(0.112)
Respondent has secondary education	0.289	-0.190
	(0.142)*	(0.130)
age	0.016	-0.019
	(0.008)*	(0.008)*
(age/10) squared	0.00002	0.000
	-0.0006	(0.000)
Dummy for survey wave Kenya 2	-0.534	2.465
	(0.267)*	(0.269)**
Dummy for survey wave Kenya 3	-0.273	1.886
	(0.253)	(0.260)**

Notes:
p-values: + < .1; * < .05; ** < .01
Robust standard errors are used to account for potential heteroscedasticity; these robust standard errors additionally account for the correlation of residuals for the same individual across time periods
a) the village average number of funerals attended in the last month prior to the survey; this question is only available 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.

Table 5: Females -- linear probability models for currently using family planning with different specifications of network partners' family planning use. Respondent's contraceptive use is measured at K1, K2 and K3.

Method	**Fixed Effects**	**OLS**	**Fixed Effects**	**OLS**	**Fixed Effects**	**OLS**	**Fixed Effects**	**OLS**
# of nwp using family planning (fp), time t-	0.0385 (0.0091)**	0.0703 (0.0085)**	0.0328 (0.0098)**	0.0673 (0.0086)**				
# of nwp not using family planning (fp), time t-			-0.0120 (0.0079)	-0.0076 (0.0063)				
at least one fp user in network					0.0542 (0.0251)*	0.0279 (0.0201)	0.0508 (0.0276)+	0.0151 (0.0225)
# number of remaining fp users in network					0.0324 (0.0146)*	0.0871 (0.0137)**	0.0241 (0.0157)	0.0876 (0.0146)**
at least one non-user in network							-0.0028 (0.0290)	0.0334 (0.0269)
# number of remaining non-users in network							-0.0177 (0.0136)	-0.0201 (0.0114)+
dummy for not married, time t	-0.0586 (0.0457)	-0.0699 (0.0343)*	-0.0592 (0.0458)	-0.0703 (0.0343)*	-0.0567 (0.0459)	-0.0716 (0.0343)*	-0.0569 (0.0459)	-0.0740 (0.0343)*
children ever born, time t	0.0099 (0.0107)	0.0043 (0.0048)	0.0109 (0.0107)	0.0046 (0.0048)	0.0095 (0.0107)	0.0046 (0.0048)	0.0106 (0.0107)	0.0047 (0.0048)
Respondent has radio, time t	0.0363 (0.0255)	0.0291 (0.0210)	0.0371 (0.0255)	0.0301 (0.0211)	0.0355 (0.0255)	0.0310 (0.0209)	0.0362 (0.0255)	0.0316 (0.0210)
Respondent has metal roof, time t	-0.0752 (0.0381)*	0.0245 (0.0273)	-0.0759 (0.0381)*	0.0252 (0.0274)	-0.0766 (0.0381)*	0.0265 (0.0273)	-0.0785 (0.0380)*	0.0265 (0.0272)
Respondent has at least primary education		0.0717 (0.0269)**		0.0744 (0.0270)**		0.0703 (0.0268)**		0.0728 (0.0270)**
Respondent has secondary education		0.0828 (0.0396)*		0.0828 (0.0395)*		0.0801 (0.0393)*		0.0794 (0.0393)*
age		0.0375 (0.0093)**		0.0376 (0.0093)**		0.0387 (0.0091)**		0.0389 (0.0091)**
(age/10) squared		-0.0522 (0.0125)**		-0.0526 (0.0125)**		-0.0544 (0.0123)**		-0.0546 (0.0123)**
Dummy for survey wave Kenya 2	0.0307 (0.0216)	0.0167 (0.0207)	0.0319 (0.0217)	0.0173 (0.0207)	0.0298 (0.0216)	0.0192 (0.0208)	0.0310 (0.0216)	0.0198 (0.0208)
Dummy for survey wave Kenya 3	0.0647 (0.0255)*	0.0411 (0.0230)+	0.0715 (0.0262)**	0.0452 (0.0232)+	0.0636 (0.0256)*	0.0448 (0.0232)+	0.0705 (0.0262)**	0.0466 (0.0233)*
Constant		-0.6800 (0.1608)**	0.0545 (0.0578)	-0.6728 (0.1608)**		-0.6872 (0.1590)**		-0.6932 (0.1585)**
N (number of women, each observed at three surveys)	497	497	497	497	497	497	497	497

Notes:
p-values: + < .1; * < .05; ** < .01
Robust standard errors are used to account for potential heteroscedasticity (especially in the linear probability model). For OLS estimates these robust standard errors additionally account for the correlation of residuals for the same individual across tim time periods.

Table 6: Females -- linear probability models for currently using family planning with different assumptions about the contraceptive use of network partners without detailed information in the KDICP survey

Method	**Fixed Effects**	**Fixed Effects**	**Fixed Effects**
Assumption about family planning use of network partners without detailed information (i.e., the fifth and higher network partner mentioned by the respondent)	NWP > 4 are not fp users	NWP > 4 are fp users	NWP > 4 are distributed proportionally[a]
# of nwp using family planning (fp), time t- (among network partners with detailed information)	0.0382 (0.0097)**		
# of nwp not using fp, time t-, assuming all nwp > 4 are not users	-0.0040 (0.0039)		
# of nwp using fp, time t-, assuming all nwp > 4 are users		0.0062 (0.0041)	
# of nwp not using family planning (fp), time t- (among network partners with detailed information)		-0.0244 (0.0079)**	
# of nwp using fp, time t-, assuming proportional distribution of nwp > 4			0.0132 (0.0053)*
# of nwp not using fp, time t-, assuming proportional distribution of nwp > 4			-0.0099 (0.0046)*
	-0.0749 (0.0454)+	-0.0763 (0.0455)+	-0.0755 (0.0454)+
Children ever born, time t-	0.0083 (0.0111)	0.0088 (0.0112)	0.0090 (0.0112)
Respodent has radio, time t-	0.0239 (0.0263)	0.0295 (0.0261)	0.0292 (0.0264)
Respondent has metal roof, time t-	-0.0686 (0.0374)+	-0.0689 (0.0380)+	-0.0732 (0.0386)+
Dummy for survey wave Kenya 2	0.0246 (0.0222)	0.0282 (0.0220)	0.0265 (0.0223)
Dummy for survey wave Kenya 3	0.0661 (0.0268)*	0.0729 (0.0271)**	0.0713 (0.0271)**
N (number of women, each observed at three surveys)	457	457	453

Notes:
p-values: + < .1; * < .05; ** < .01
(a) The network partners without detailed information are split into fp users and non-users according to the proportion observed among the four network partners with detailed information for the respondent
Robust standard errors are used to account for potential heteroscedasticity (especially in the linear probability model).
For OLS estimates these robust standard errors additionally account for the
correlation of residuals for the same individual across time periods

Table 7: Females -- linear probability models for having ever-used family planning with different specifications of network partners' family planning use. Respondent's contraceptive use is measured at K1, K2 and K3.

Method	Fixed Effects	OLS	Fixed Effects	OLS	Fixed Effects	OLS	Fixed Effects	OLS
# of nwp using family planning (fp), time t-	0.0460 (0.0096)**	0.0873 (0.0091)**	0.0477 (0.0105)**	0.0946 (0.0094)**				
# of nwp not using family planning (fp), time t-			0.0033 (0.0090)	0.0185 (0.0084)*				
at least one fp user in network					0.0400 (0.0276)	0.0767 (0.0272)**	0.0303 (0.0299)	0.0580 (0.0304)+
# number of remaining fp users in network					0.0484 (0.0146)**	0.0915 (0.0144)**	0.0536 (0.0164)**	0.1079 (0.0162)**
at least one non-user in network							0.0289 (0.0308)	0.0624 (0.0321)+
# number of remaining non-users in network							-0.0058 (0.0153)	0.0033 (0.0146)
dummy for not married, time t	-0.0151 (0.0540)	0.0373 (0.0477)	-0.0150 (0.0540)	0.0381 (0.0475)	-0.0159 (0.0541)	0.0369 (0.0478)	-0.0165 (0.0540)	0.0349 (0.0479)
children ever born, time t	0.0292 (0.0127)*	0.0175 (0.0063)**	0.0289 (0.0127)*	0.0167 (0.0063)**	0.0293 (0.0127)*	0.0176 (0.0063)**	0.0291 (0.0127)*	0.0167 (0.0063)**
R has radio, time t	0.0388 (0.0275)	0.0524 (0.0258)*	0.0386 (0.0275)	0.0499 (0.0257)+	0.0392 (0.0276)	0.0529 (0.0257)*	0.0394 (0.0276)	0.0507 (0.0256)*
R has metal roof, time t	-0.0732 (0.0387)+	0.0510 (0.0333)	-0.0730 (0.0387)+	0.0492 (0.0331)	-0.0726 (0.0389)+	0.0514 (0.0334)	-0.0732 (0.0389)+	0.0497 (0.0331)
R has at least primary education		0.1359 (0.0369)**		0.1294 (0.0369)**		0.1356 (0.0370)**		0.1289 (0.0370)**
R has secondary education		0.1299 (0.0463)**		0.1298 (0.0462)**		0.1292 (0.0464)**		0.1271 (0.0463)**
age		0.0407 (0.0133)**		0.0404 (0.0132)**		0.0410 (0.0132)**		0.0413 (0.0131)**
(age/10) squared		-0.0555 (0.0184)**		-0.0547 (0.0183)**		-0.0561 (0.0183)**		-0.0559 (0.0181)**
Dummy for survey wave Kenya 2	0.0637 (0.0224)**	0.0422 (0.0221)+	0.0634 (0.0225)**	0.0408 (0.0222)+	0.0640 (0.0224)**	0.0428 (0.0221)+	0.0640 (0.0225)**	0.0425 (0.0222)+
Dummy for survey wave Kenya 3	0.0640 (0.0263)*	0.0278 (0.0258)	0.0621 (0.0274)*	0.0178 (0.0263)	0.0645 (0.0263)*	0.0287 (0.0259)	0.0619 (0.0274)*	0.0182 (0.0263)
Constant		-0.8016 (0.2232)**		-0.8192 (0.2224)**		-0.8034 (0.2227)**		-0.8367 (0.2202)**
N (number of women, each observed at three surveys)	498	498	498	498	498	498	498	498

Notes:

p-values: + < .1; * < .05; ** < .01

Robust standard errors are used to account for potential heteroscedasticity (especially in the linear probability model). For OLS estimates these robust standard errors additionally account for the correlation of residuals for the same individual across time periods

Table 8: Females -- specification tests with current number of network partners (total or family planning users) as dependent variable and past family planning use as right-side variable
Respondent's contraceptive use is measured at K2 and K3, and 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

Method	**OLS**	**Fixed Effects**	**OLS**	**Fixed Effects**
Dependent Variable	**number of network partners using fp**	**number of network partners using fp**	**number of network partners**	**number of network partners**
Currently using family planning, time t - 1	0.6276	-0.0695	0.3612	-0.0096
	(0.1416)**	(0.1920)	(0.1084)**	(0.1415)
Dummy for not married, time t-	-0.1415	-0.5593	-0.1032	-0.4562
	(0.1532)	(0.2442)*	(0.1693)	(0.2127)*
Dummy for not married, time t -1	0.5044	0.5309	0.2928	0.1647
	(0.2592)+	(0.3191)+	(0.2528)	(0.3605)
Children ever born, time t-	0.0671	0.0733	0.1261	0.1412
	(0.0362)+	(0.0565)	(0.0358)**	(0.0557)*
Children ever born, time t - 1	-0.0604	-0.0818	-0.0987	-0.0875
	(0.0381)	(0.0697)	(0.0399)*	(0.0628)
Respondent has radio, time t-	0.1921	0.1691	0.2272	-0.1508
	(0.0964)*	(0.1489)	(0.1131)*	(0.1733)
Respondent has radio, time t - 1	0.0373	-0.0316	0.1417	-0.0584
	(0.0910)	(0.1393)	(0.0996)	(0.1433)
Respodent has metal roof, time t-	0.2128	-0.2106	0.1813	-0.3093
	(0.1191)+	(0.1843)	(0.1251)	(0.1735)+
Respondent has metal roof, time t - 1	0.0041	-0.3320	0.0353	-0.2688
	(0.1307)	(0.2104)	(0.1370)	(0.1984)
Respondent has at least primary schooling	0.5222		0.6439	
	(0.1232)**		(0.1476)**	
Respondent has secondary scholling	0.2687		0.0119	
	(0.1478)+		(0.1442)	
Age	0.0740		0.0316	
	(0.0475)		(0.0526)	
(Age/10) squared	-0.0763		-0.0442	
	(0.0655)		(0.0730)	
Dummy for survey wave Kenya 3	-0.1892	-0.0235	0.3550	0.5186
	(0.0796)*	(0.1139)	(0.0808)**	(0.1082)**
Constant	-0.9319		0.9710	
	(0.8018)		(0.8989)	
N (number of women, each observed at three surveys)	497	497	497	497

Notes:
p-values: + < .1; * < .05; ** < .01
Robust standard errors are used to account for potential heteroscedasticity (especially in the linear probability model).
For the OLS estimates these standard errors additionally account for the correlation of residuals
for the same individual across time periods

Table 9: Males -- linear probability models for currently using family planning with different specifications of network partners' family planning use. Respondent's contraceptive use is measured at K1, K2 and K3.

Method	Fixed Effects	OLS	Fixed Effects	OLS	Fixed Effects	OLS	Fixed Effects	OLS
# of nwp using family planning (fp), time t-	0.0974 (0.0131)**	0.1268 (0.0103)**	0.0956 (0.0135)**	0.1252 (0.0107)**				
# of nwp not using family planning (fp), time t-			-0.0034 (0.0093)	-0.0040 (0.0078)				
At least one fp user in network					0.1484 (0.0428)**	0.1469 (0.0372)**	0.1561 (0.0454)**	0.1595 (0.0397)**
# number of remaining fp users in network					0.0777 (0.0229)**	0.1186 (0.0198)**	0.0719 (0.0239)**	0.1123 (0.0206)**
At least one non-user in network							-0.0238 (0.0446)	-0.0324 (0.0417)
# number of remaining non-users in network							0.0015 (0.0198)	0.0051 (0.0166)
Dummy for not married, time t-	-0.1532 (0.0912)+	-0.0535 (0.0571)	-0.1525 (0.0913)+	-0.0528 (0.0572)	-0.1539 (0.0897)+	-0.0524 (0.0561)	-0.1510 (0.0899)+	-0.0506 (0.0560)
Children ever born, time t-	0.0030 (0.0032)	0.0010 (0.0020)	0.0030 (0.0032)	0.0010 (0.0020)	0.0029 (0.0032)	0.0011 (0.0020)	0.0029 (0.0032)	0.0009 (0.0020)
Respondent has radio, time t-	0.0182 (0.0373)	0.0199 (0.0257)	0.0180 (0.0373)	0.0195 (0.0258)	0.0207 (0.0374)	0.0196 (0.0256)	0.0201 (0.0376)	0.0186 (0.0258)
Respondent has metal roof, time t-	-0.0113 (0.0463)	0.0299 (0.0329)	-0.0100 (0.0465)	0.0308 (0.0331)	-0.0138 (0.0462)	0.0296 (0.0330)	-0.0117 (0.0464)	0.0311 (0.0331)
Respondent has at least primary schooling		0.0751 (0.0322)*		0.0774 (0.0328)*		0.0735 (0.0322)*		0.0769 (0.0328)*
Respondent has secondary schooling		0.0667 (0.0346)+		0.0681 (0.0348)*		0.0665 (0.0347)+		0.0679 (0.0347)*
Age		0.0114 (0.0060)+		0.0112 (0.0060)+		0.0113 (0.0060)+		0.0112 (0.0060)+
(Age/10) squared		-0.0128 (0.0059)*		-0.0127 (0.0059)*		-0.0127 (0.0059)*		-0.0127 (0.0059)*
Dummy for survey wave Kenya 2	0.0457 (0.0291)	0.0326 (0.0281)	0.0459 (0.0292)	0.0329 (0.0281)	0.0437 (0.0292)	0.0320 (0.0281)	0.0441 (0.0293)	0.0324 (0.0282)
Dummy for survey wave Kenya 3	0.0002 (0.0306)	-0.0153 (0.0281)	0.0016 (0.0309)	-0.0135 (0.0280)	-0.0009 (0.0305)	-0.0157 (0.0280)	0.0012 (0.0308)	-0.0137 (0.0279)
Constant		-0.2695 (0.1474)+	0.0810 (0.0389)*	-0.2604 (0.1479)+		-0.2708 (0.1480)+		-0.2586 (0.1483)+
N (number of males, each observed at three surveys)	324	324	324	324	324	324	324	324

Notes:

p-values: + < .1; * < .05; ** < .01

Robust standard errors are used to account for potential heteroscedasticity (especially in the linear probability model). For the OLS estimates these robust standard errors additionally account for the correlation of residuals for the same individual across time periods

Table 10: Females -- linear regression for female respondent's perceived risk of getting AIDS with different specifications of network partners' risk perceptions, and linear probability model for variable whether the respondent has talked with spouse about AIDS. Respondent's risk perception and communication with spouse are measured at K2 and K3.

Method	**Fixed Effects**	**OLS**	**Fixed Effects**	**OLS**
Dependent Variable	**Perceived AIDS risk**	**Perceived AIDS risk**	**Perceived AIDS risk**	**Perceived AIDS risk**
# of nwps with moderate or high perceived risk, time t-	0.1742	0.1618		
	(0.0318)**	(0.0237)**		
# of nwps with no or low perceived risk, time t-	-0.0448	-0.0737		
	(0.0286)	(0.0212)**		
at least one nwp with moderate or high perceived risk, time t-			0.4213	0.3336
			(0.0960)**	(0.0752)**
# of remaining nwps with moderate or high perceived risk, time t-			0.0641	0.0812
			(0.0535)	(0.0401)*
at least one nwp with no or low perceived risk, time t-			-0.1825	-0.1237
			(0.0988)+	(0.0801)
# of remaining nwps with no or low perceived risk, time t-			-0.0079	-0.0646
			(0.0467)	(0.0356)+
Dummy for not married, time t-	0.1725	0.1894	0.1737	0.1952
	(0.1814)	(0.0976)+	(0.1814)	(0.0975)*
Children ever born, time t-	-0.0111	0.0115	-0.0135	0.0109
	(0.0412)	(0.0146)	(0.0417)	(0.0146)
Respondent has radio, time t-	-0.1164	-0.0881	-0.1031	-0.0858
	(0.1027)	(0.0632)	(0.1017)	(0.0630)
Respondent has metal roof, time t-	-0.0020	0.0430	-0.0159	0.0392
	(0.1276)	(0.0670)	(0.1261)	(0.0669)
Respondent has at least primary schooling		0.1417		0.1406
		(0.0784)+		(0.0777)+
Respondent has secondary schooling		-0.1180		-0.1213
		(0.0876)		(0.0882)
Age		0.0216		0.0234
		(0.0274)		(0.0275)
(Age/10) squared		-0.0475		-0.0503
		(0.0373)		(0.0374)
Dummy for survey wave Kenya 3	-0.0190	-0.0206	-0.0282	-0.0312
	(0.0617)	(0.0528)	(0.0618)	(0.0528)
Constant		1.9248		1.8864
		(0.4756)**		(0.4778)**
N (number of women, each observed at two surveys)	545	545	545	545

Notes:

p-values: + < .1; * < .05; ** < .01

Robust standard errors are used to account for potential heteroscedasticity (especially in the linear probability model). For the OLS estimates these robust standard errors additionally account for the correlation of residuals for the same individual across time periods

Table A1: Females -- Comparison of characteristics of women who participate in all three waves of the KDIC survey as compared to attritors, i.e., women who participate in at least one but not all three rounds

	Kenya 1		Kenya 2		Kenya 3	
	Women in K1--K3	Attritors	Women in K1--K3	Attritors	Women in K1--K3	Attritors
Females						
N	497	414	497	231	497	387
Individual Characteristics						
Age	29.96	27.31*	33.27	31.69*		
	-8.05	(7.63)	(8.27)	(8.56)		
Not Married			0.06	0.07	0.14	0.13
Children ever born	5.13	3.70*	5.69	4.82*	6.23	4.20*
	(3.14)	(2.84)	(3.14)	(2.91)	(2.93)	(3.11)
Has radio	0.52	0.49	0.60	0.60	0.63	0.63
Has metal roof	0.23	0.16*	0.28	0.21*	0.44	0.36*
Has at least primary schooling	0.77	0.85*	0.78	0.82	0.77	0.89*
Has secondary or higher schooling	0.12	0.15	0.13	0.16	0.12	0.16*
Family planning variables, respondent						
Proportion currently using family planning	0.13	0.11	0.18	0.16	0.20	0.13*
Proportion ever using family planning	0.24	0.22	0.34	0.27	0.34	0.29
Proportion wanting no more chidren	0.38	0.25*	0.42	0.32*	0.49	0.30*
Family planning (fp) network						
Proportion with at least one nwp in fp network	0.72	0.78*	0.81	0.81	0.90	0.86*
Uncensored size of family planning network	2.80	2.96	3.88	3.96	4.88	4.28*
	(2.73)	(2.62)	(3.63)	(3.64)	(4.09)	(4.06)
Censored size of family planning network	2.15	2.32	2.53	2.57	2.97	2.65*
	(1.64)	(1.57)	(1.53)	(1.51)	(1.35)	(1.46)
Proportion with more than 4 network partners	0.23	0.24	0.33	0.34	0.41	0.33*
Prop. with at least one nwp fp user in network	0.45	0.52	0.62	0.65	0.63	0.58
Number of fp users in network (censored)	0.99	1.08	1.41	1.43	1.32	1.14*
	(1.30)	(1.31)	(1.42)	(1.36)	(1.34)	(1.24)

Notes: Standard deviations in parentheses; (*) indicates that difference between attritors and non-attritors is statistically significant at the 5% level

Table A2: BGLW test for attrition effects for females. OLS estimates of linear probability model for currently using contraception in Kenya 1, Kenya 2 and Kenya 3

Survey wave	**Kenya 1**	**Kenya 2**	**Kenya 3**
Indicator for attritor	-0.2050	-0.3474	-0.0909
	(0.2717)	(0.4449)	(0.1833)
# of nwp using family planning (fp), time t-	0.0498	0.0735	0.0842
	(0.0149)**	(0.0214)**	(0.0158)**
Interaction of above variable with "attritor"	0.0093	-0.0153	0.0079
	(0.0199)	(0.0255)	(0.0215)
# of nwp not using family planning (fp), time t-	-0.0067	0.0171	0.0256
	(0.0108)	(0.0181)	(0.0110)*
Interaction of above variable with "attritor"	0.0019	-0.0253	-0.0254
	(0.0146)	(0.0212)	(0.0154)+
Dummy for not married, time t-	--	-0.0462	0.0362
		(0.0979)	(0.0537)
Interaction of above variable with "attritor"	--	0.0253	-0.1284
		(0.1184)	(0.0688)+
Children ever born, time t-	0.0124	0.0054	0.0040
	(0.0076)	(0.0102)	(0.0064)
Interaction of above variable with "attritor"	-0.0015	-0.0093	0.0046
	(0.0105)	(0.0122)	(0.0094)
Respondent has radio, time t-	-0.0416	-0.0135	-0.0339
	(0.0289)	(0.0490)	(0.0348)
Interaction of above variable with "attritor"	0.0793	0.0486	0.0651
	(0.0414)$^+$	(0.0600)	(0.0503)
Respondent has metal roof, time t-	-0.0144	0.0525	0.0666
	(0.0426)	(0.0675)	(0.0393)+
Interaction of above variable with "attritor"	-0.0035	-0.0100	-0.0357
	(0.0604)	(0.0805)	(0.0534)
Respondent has at least primary schooling	0.0633	0.0905	0.0643
	(0.0440)	(0.0469)+	(0.0503)
Interaction of above variable with "attritor"	-0.0187	-0.0488	-0.0154
	(0.0616)	(0.0632)	(0.0666)
Respondent has secondary schooling	0.0390	0.0998	0.0470
	(0.0486)	(0.0871)	(0.0511)
Interaction of above variable with "attritor"	-0.0325	0.0339	0.1136
	(0.0741)	(0.1073)	(0.0825)
Age	0.0241	0.0073	0.0069
	(0.0113)*	(0.0214)	(0.0051)
Interaction of above variable with "attritor"	0.0133	0.0238	0.0012
	(0.0176)	(0.0259)	(0.0074)
(Age/10) squared	-0.0402	-0.0122	-0.0057
	(0.0169)*	(0.0295)	(0.0053)
Interaction of above variable with "attritor"	-0.0217	-0.0270	0.0000
	(0.0264)	(0.0354)	(0.0075)
Constant	-0.3498	-0.1802	-0.2476
	(0.1753)*	(0.3679)	(0.1056)*
N	911	728	884
test-statistic of F-test that all interaction variables are zero (df in parentheses)	0.57 (9, 891)	0.66 (10, 706)	1.18 (10, 862)

Notes:
p-values: + < .1; * < .05; ** < .01
Robust standard errors are used to account for potential heteroscedasticity in the linear probability model

Table A3: Females -- 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 is measured at K1, K2 and K3.

Method	Fixed Eff. Logit	Random Eff. Logit	Fixed Eff. Logit	Random Eff. Logit	Fixed Eff. Logit	Random Eff. Logit	Fixed Eff. Logit	Random Eff. Logit
# of nwp using family planning (fp), time t-	0.2877	0.5169	0.2402	0.4963				
	(0.0806)**	(0.0652)**	(0.0941)*	(0.0750)**				
# of nwp not using family planning (fp), time t-			-0.1023	-0.0457				
			(0.1060)	(0.0838)				
At least one fp user in network					0.7160	0.6056	0.6890	0.4884
					(0.3000)*	(0.2454)*	(0.3194)*	(0.2598)+
# number of remaining fp users in network					0.1575	0.4907	0.0710	0.4898
					(0.1178)	(0.0953)**	(0.1431)	(0.1121)**
At least one non-user in network							0.0146	0.2686
							(0.3021)	(0.2357)
# number of remaining non-users in network							-0.2231	-0.1936
							(0.1615)	(0.1292)
Dummy for not married, time t-	-0.6492	-0.6389	-0.6296	-0.6396	-0.6038	-0.6368	-0.5865	-0.6619
	(0.5152)	(0.4081)	(0.5119)	(0.4085)	(0.5179)	(0.4085)	(0.5176)	(0.4098)
Children ever born, time t-	0.1111	0.0627	0.1218	0.0641	0.1045	0.0625	0.1203	0.0646
	(0.1207)	(0.0474)	(0.1213)	(0.0475)	(0.1211)	(0.0475)	(0.1218)	(0.0475)
Respondent has radio, time t-	0.4386	0.3811	0.4339	0.3856	0.4073	0.3764	0.3914	0.3748
	(0.2992)	(0.2032)+	(0.2997)	(0.2036)+	(0.3000)	(0.2039)+	(0.3012)	(0.2038)+
Respondent has metal roof, time t-	-0.7074	0.0874	-0.7004	0.0925	-0.7138	0.0819	-0.7268	0.0754
	(0.3676)+	(0.2188)	(0.3700)+	(0.2192)	(0.3659)*	(0.2196)	(0.3714)*	(0.2200)
Respondent has at least primary schooing		0.8259		0.8404		0.8297		0.8479
		(0.3064)**		(0.3082)**		(0.3072)**		(0.3085)**
Respondent has secondary schooling		0.6042		0.6065		0.6110		0.6145
		(0.2822)*		(0.2828)*		(0.2835)*		(0.2832)*
Age		0.4160		0.4166		0.4137		0.4135
		(0.1143)**		(0.1144)**		(0.1146)**		(0.1144)**
(Age/10) squared		-0.5909		-0.5924		-0.5868		-0.5854
		(0.1619)**		(0.1621)**		(0.1624)**		(0.1620)**
Dummy for survey wave Kenya 2	0.3610	0.2124	0.3550	0.2134	0.3475	0.2090	0.3351	0.2084
	(0.2347)	(0.2147)	(0.2345)	(0.2148)	(0.2362)	(0.2151)	(0.2364)	(0.2149)
Dummy for survey wave Kenya 3	0.6318	0.4482	0.6648	0.4667	0.6012	0.4428	0.6328	0.4517
	(0.2897)*	(0.2243)*	(0.2926)*	(0.2271)*	(0.2922)*	(0.2250)*	(0.2957)*	(0.2277)*
Constant		-11.3573		-11.3060		-11.3635		-11.3545
		(1.9897)**		(1.9934)**		(1.9931)**		(1.9914)**
N (number of women, each observed at three surveys)	156	497	156	497	156	497	156	497

Notes:
p-values: + < .1; * < .05; ** < .01
Fixed effect logit model is based only on individuals who change their contraceptive behavior at least once between Kenya 1 and Kenya 3; women with constant contraceptive use in all three survey waves are dropped in the estimation

Table A4: Males -- 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 is measured at K1, K2 and K3.

Method	Fixed Eff. Logit	Random Eff. Logit	Fixed Eff. Logit	Random Eff. Logit	Fixed Eff. Logit	Random Eff. Logit	Fixed Eff. Logit	Random Eff. Logit
# of nwp using family planning (fp), time t-	0.6029	0.7718	0.6186	0.7791				
	(0.0956)**	(0.0722)**	(0.1180)**	(0.0831)**				
# of nwp not using family planning (fp), time t-			0.0266	0.0153				
			(0.1163)	(0.0851)				
At least one family planning user in network					1.5320	1.4414	1.5682	1.5178
					(0.3806)**	(0.2771)**	(0.4136)**	(0.3005)**
# number of remaining fp users in network					0.3198	0.5599	0.2972	0.5234
					(0.1395)*	(0.1063)**	(0.1682)+	(0.1241)**
At least one non-user in network							-0.0964	-0.1893
							(0.3321)	(0.2677)
# number of remaining non-users in network							0.0108	0.0333
							(0.1824)	(0.1411)
Dummy for not married, time t-	-2.0669	-0.9034	-2.0608	-0.9061	-2.3048	-0.9135	-2.2927	-0.9065
	(1.3665)	(0.9341)	(1.3580)	(0.9336)	(1.5915)	(0.9455)	(1.6037)	(0.9484)
Children ever born, time t-	0.2307	0.0112	0.2296	0.0113	0.2131	0.0138	0.2128	0.0130
	(0.1022)*	(0.0242)	(0.1023)*	(0.0243)	(0.1029)*	(0.0245)	(0.1034)*	(0.0245)
Respondent has radio, time t-	0.3919	0.2415	0.3933	0.2426	0.4867	0.2341	0.4873	0.2268
	(0.3390)	(0.2281)	(0.3391)	(0.2282)	(0.3517)	(0.2303)	(0.3513)	(0.2303)
Respondent has metal roof, time t-	-0.2993	0.2304	-0.3061	0.2280	-0.4638	0.2188	-0.4665	0.2258
	(0.4199)	(0.2435)	(0.4208)	(0.2439)	(0.4343)	(0.2460)	(0.4359)	(0.2460)
Respondent has at least primary schooling		0.9056		0.9005		0.8610		0.8786
		(0.5074)+		(0.5086)+		(0.5132)+		(0.5131)+
Respondent has secondary schooling		0.4534		0.4485		0.4458		0.4538
		(0.2319)*		(0.2334)+		(0.2340)+		(0.2353)+
Age		0.1112		0.1118		0.1080		0.1067
		(0.0618)+		(0.0619)+		(0.0621)+		(0.0620)+
(Sge/10) squared		-0.1318		-0.1323		-0.1278		-0.1266
		(0.0670)*		(0.0671)*		(0.0673)+		(0.0672)+
Dummy for survey wave Kenya 2	0.2841	0.2830	0.2861	0.2823	0.2969	0.2639	0.2928	0.2646
	(0.2649)	(0.2332)	(0.2653)	(0.2333)	(0.2706)	(0.2352)	(0.2709)	(0.2351)
Dummy for survey wave Kenya 3	-0.2690	-0.1303	-0.2772	-0.1357	-0.2724	-0.1478	-0.2651	-0.1426
	(0.3382)	(0.2522)	(0.3400)	(0.2540)	(0.3418)	(0.2535)	(0.3437)	(0.2556)
Constant		-6.0591		-6.0967		-6.1928		-6.1074
		(1.4580)**		(1.4736)**		(1.4700)**		(1.4820)**
N (number of males, each observed at three surveys)	133	324	133	324	133	324	133	324

Notes:

p-values: + < .1; * < .05; ** < .01

Fixed effect logit model is based only on individuals who change their contraceptive behavior at least once between Kenya 1 and Kenya 3; women with constant contraceptive use in all three survey waves are dropped in the estimation