Neighbors, Social Interactions, and Learning HIV Results

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Abstract

How do neighbors positively or negatively influence individuals living in rural Malawi to attend VCT centers to learn their HIV results? Using GIS data of location of homes and distance to other neighbors, we measure the social network effects of neighbors' VCT attendance on individuals own attendance. This paper utilizes a randomized experiment that encouraged individuals and their neighbors to learn their HIV results. Using the fact that neighbors randomly received monetary incentives of varying amounts to learn their HIV status, the results in this paper indicate positive effects of neighbors attending VCT clinics on others living nearby. We find differential effects by religious networks as well as by initial level of social interaction.

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1. Introduction

Social networks can have both a positive and negative influence on medical and health care decisions as well as the use of health-promoting services. For example, networks can positively influence individuals to seek cancer screening (Suarez 1994), recruit and influence friends to use contraception (Speizer, Tambashe et al. 2001), utilize health services (Deri 2005), receive a flu vaccination (Rao et al. 2006) or influence family planning choices or sexual behavior (Casterline 2001; Kincaid 2000; Kohler et al. 2001; Montgomery and Casterline 1996; Morris and Kretzschmar, 1997; Valente et al. 1997; Helleringer and Kohler 2005; Behrman et al. 2006; Campbell et al. 2002, Munshi 2000). Networks may also have a negative effect on health behavior. Miguel and Kremer (2006) find that social learning about de-worming drugs in Kenya may have actually lowered subsequent purchases: increased number of peers using de-worming drugs lowered others' infection risk and thus lowered the benefit of purchasing such drugs.[3]

This paper evaluates the impact of social networks on the decision to learn HIV results after being tested. There are abundant claims based on qualitative or anecdotal evidence that people are afraid of learning their HIV results. This could be due, in part, to the fact that individuals often overestimate the risk that they face and expect to receive an HIV-positive diagnosis (Anglewicz and Kohler 2005; Bignami Van-Assche, Anglewicz et al. 2005). It is theoretically ambiguous whether the strategic complementarities of others' attendance at VCT centers are positive or negative. They may be positive if, for example, neighbors provide additional emotional support that reduces psychological costs, or if there are economies of scale of travel costs. Alternatively, they may be negative if increased numbers of neighbors observing attendance at the VCT center results in higher psychological costs (i.e. due to stigma). It is often suggested by policy makers that one important barrier to testing and learning HIV results is social stigma; a great deal of financial and human

[3] Within the fields of education and crime there is a wider literature on the effects of social networks, see for example Sacerdote 2000; Figlio 2003; Angrist and Lang 2002; Evans, Oates, and Schwab 1992; Gaviria and Raphael 2001; Hoxby 2000; Zimmerman 2002.

resources have been devoted to de-stigmatization and HIV testing awareness campaigns. (See HITS-2000 Investigators, 2004; Mugusi et al. 2002; Ginwalla 2002; Baggaley 1998; Hutchinson 2004; Ford 2004; Coulibaly 1998; Kalichman 2003; and Wolff 2005). However, there has been surprisingly little rigorous research quantifying or identifying these claimed negative social network effects on seeking HIV results. For this reason, it is important to understand how social interactions affect individuals' motivation to learn their HIV results.

Measuring the extent to which social networks affect decision making is challenging because social group formation is usually endogenous, complicating causal interpretation: that is, if belonging to a social group is a matter of deliberate choice, it is difficult to assign causality to the impact of the group itself (Manski 1993). In addition, individuals may make simultaneous decisions affecting each other making it difficult to determine the causal behavior; this is often called the reflection problem. A few studies have utilized natural or field experiments in which social groups were randomly assigned (e.g. dorm room assignments, see Kremer et al; Rao et al. 2006; Sacerdote 2001; Zimmerman 2003; or random allocation of de-worming medicine, see Miguel and Kremer 2006). Other strategies have used natural experiments or constructed instrumental variables to identify the causal effects of peer behavior (Figlio 2003). This paper adds to this literature by analyzing an experiment that randomized the allocation of monetary incentives to multiple individuals in villages in rural Malawi to learn their HIV results after being tested. The monetary incentives serve as exogenous instruments for individuals living in the same communities to learn their HIV results, thereby permitting a causal analysis of the effects of social networks.

In this paper, we examine several aspects of social networks and the decision to learn HIV results. First, we measure the effects of neighbors living within close geographic proximity on an individual learning her HIV results. We find modest effects of neighbors living within 0.5 kilometers: a 10 percent increase of neighbors attending the VCT (approximately 9 additional neighbors) increases the probability of obtaining HIV results by 1.3 percentage points. These effects

are strongest from neighbors living within the closest proximity. We cannot reject that the effects among men are significantly different from those among women.

We find that intensity of social interactions within a community also is important, but only for women. For women, the frequency of social interactions within the village as measured by attendance to community events (for example, weddings and political meetings), has important effects on increasing the size of the peer effect for obtaining HIV results. However, we cannot distinguish this impact as being due to the causal effect of more social interactions or to selection – that those who choose to participate more are also those who may be more affected by neighbors obtaining their HIV results. The results, however, are driven mainly by participation in village drama meetings rather than participation in any other type of village activity, possibly suggestive that the selection, rather than the causal impact of the intensity of group participation, may be driving these results among women. There is no additional effect of frequency of social interactions within the village on the peer effects among men.

Using detailed data of church and mosque membership, we examine how members of the same and different religious group affect one another. We find no significant effects of religious networks (i.e. those that attend the same church or mosque) on learning HIV results. There are no significant effects among those of different religious affiliations (e.g., Christian or Muslim) or after controlling for religious participation (i.e., attendance in the last week).

Lastly, we quantify the trade-off between personal incentives to learn their HIV results and social networks, using the randomized design of the experiment. We find that social networks matter most among those whose personal incentives to obtain their HIV results are low. Among those offered no financial incentives to learn HIV results, a 10 percent increase of neighbors attending the VCT increases the probability of obtaining HIV results by 2.3 percentage points, roughly double the average effect. The effect of neighbors on own attendance decreases as an individual's own incentive increases – there is almost no effect among those receiving over one dollar of incentive.

2. Experiment and Data

2.1 Survey and Experimental Design

The Malawi Diffusion and Ideational Change Project (MDICP) is conducted in rural Malawi and is a collaborative project between the University of Pennsylvania and the Malawi College of Medicine. It is an on-going study of men and women randomly selected from 125 villages in the districts of Rumphi, Mchinji, and Balaka, located in the north, central, and southern regions respectively. Approximately one in four households in each village were randomly selected to participate, and ever-married women and their husbands from these households were interviewed in 1998, 2001, 2004, and 2006. In 2004, an additional sample of adolescents (ages 15-24) residing in the original villages was added to the sample. In addition, in 2004, all of those who participated in the survey were offered free tests for HIV and three other sexually transmitted infections, in their homes. The test results were available to respondents approximately 2 months after they were taken from respondents while they were processed at the laboratory in the capital city. This paper uses the survey data and HIV data collected in 2004.

Across the three districts, 2894 respondents accepted a test for at least one sexually transmitted disease. The HIV prevalence rate was 6.4 percent (7.5 percent rates for females, 5.5 percent for males). The level of HIV infections in the MDICP sample is considerably lower than national prevalence rates, a typical finding when prevalence data from antenatal clinics are compared with prevalence data from a cross-sectional population-based study (Mishra 2006; Boerma 2003; Garcia-Calleja 2006). In our longitudinal data, downward biases of HIV prevalence rates may also be due to death and migration (discussed below), as well as the fact that the data includes the additional sample of unmarried respondents and married adolescents (the prevalence in the adolescent sample is 1.7%).

The experimental design involved offering monetary incentives to encourage respondents to obtain their HIV test results. After taking the test samples, nurses gave each respondent a voucher

redeemable upon obtaining their HIV results. Voucher amounts were randomized by letting each respondent draw a token indicating a monetary amount out of a bag. Vouchers ranged between one and three dollars; the average total voucher amount was 104 kwacha (or approximately one dollar), worth approximately a day's wage. The distribution of vouchers was monitored to ensure that each nurse followed the rules of randomization. Overall, 20 percent of respondents received no monetary incentive. Vouchers were given in the privacy of a respondent's home, although there were no specific instructions to keep the information about the voucher amount private.

Approximately two months after collecting samples, test results became available and temporary counseling centers consisting of small portable tents were placed randomly throughout the districts, stratified by village. Based on their geo-spatial (GPS) coordinates, respondents' households in villages were grouped into zones, and within each zone a tent location was randomly selected. There were 16 different VCT zones across all three districts with an average of 177 people in each VCT zone. The average distance to a center was short, approximately 2 km, and over 95 percent of those tested lived within five kilometers. The VCT zones are relatively heterogeneous – while on average approximately 46 percent of the sample is male, this varies from 39 – 53 percent across zones. The religious composition also varies across and within in zones. For example, although 22 percent of the sample is Muslim some zones have no Muslim representation and others – in the southern region – are as high as 75 percent Muslim.

Respondents were personally informed of the time and location of their assigned center (open Monday through Saturday from eight in the morning until seven in the evening) and centers were operational for approximately one week. Respondents were allowed to attend any of the VCT centers but were only informed of the location and time of their assigned center (less than six percent of respondents went to a different center than the one to which they were assigned). Couples were not

informed of their results together, and results were verbally told to each respondent. Respondents could only redeem their voucher after they heard their results.[4]

2.2 Data

The sample used for analysis in this paper consists of those individuals who accepted an HIV test in 2004. These data and the experimental design are discussed extensively in Thornton (2008), however, it is worth briefly mentioning a few key issues. First, although the original sample in 1998 was randomly drawn, sample attrition across waves of data collection affects the degree to which this sample is representative. The primary reason for attrition across all waves of data is migration (Obare 2007); in 2004, 18 percent of those interviewed in 2001 were away or had moved which could affect the external validity of the study. However, these data in both the MDICP sample as well as the sub-sample we use for analysis are similar to those found in a recent population based survey in Malawi along all basic demographic characteristics (not shown, MDHS 2004). Test refusals may also be a threat to external validity: approximately 9 percent of those approached refused to be tested for HIV. However, in comparison to other studies, this is a relatively low refusal rate, which may be due to the use of saliva rather than blood in the testing.

The main sample for this paper consists of those who accepted an HIV test in 2004 and had basic covariates of HIV positive or negative results (not including those tested as indeterminate), age, and village ID. This results in 2825 total observations. The sample is 46 percent male with an average age of 33; 71 percent of the respondents were married at the time of the survey interview; and the average number of years of education was 3.3. There are large differences in ethnicity and religion across the three districts: the Chewas in Mchinji and the Tumbukas in Rumphi are primarily

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[4] The timing of VCT center attendance varied substantially by VCT zone. In some zones, the majority of respondents attended in the first several days while in other zones, there were several days with low attendance. With only 16 VCT centers it is difficult to establish the main determinants of differential timing patterns across VCT zones.

Christian, and the Yaos in Balaka practice Islam. The majority of the respondents are subsistence farmers producing primarily for home consumption, although some grow cash crops. The majority of the respondents, 76 percent, had been sexually active in the past year.

At the time of the survey, GPS coordinates were recorded at each respondent's house. Of the sample of tested respondents, 92 percent had GPS coordinates. Those missing coordinates were given imputed values equal to the mean latitude and mean longitude of those with GPS coordinates in the village. We then identify non-spousal non-resident neighbors living within similar VCT zones. We estimate effects of neighbors living within the same VCT zone, within 0.5 kilometers, or within 0.2 kilometer radii bands of each respondent who tested for HIV. For some individuals living on the border of a VCT zone, some neighbors who live within 0.5 kilometers or within 0.2 kilometer bands, may reside in a different VCT zone. However, we restrict the analysis to only neighbors living within the same VCT zone because it is more reasonable to expect peer effects from knowledge of the VCT location, economies of scale, or social stigma to operate among those attending the same tent. There may also have been spillovers across VCT zones but we do not measure that in this paper. On average, there were 91 non-spousal neighbors living within 0.5 kilometers. There were 31 non-spousal neighbors in the first 0.2 kilometer band that tested for HIV, 38 in the second 0.2 kilometer band, and 43 in the third 0.2 kilometer band. Although we are able to identify spousal relationships, we cannot determine whether respondents are related to one another in other ways.

We further distinguish those who are more or less active within their communities with a measurement of social intensity. Respondents were asked "how many times in the past month did you attend a [funeral/ drama performance/ beer brewing place/ place where there is dancing/ periodic market]?". Respondents were also asked "how many times in the past year did you attend a [wedding/ political meeting/ drama about family planning or HIV/AIDS]?". We investigate differential effects of others' obtaining their HIV results by those who participate more or less in each of these activities. To do this, we create a standardized average of participation (standardized to

monthly activities for each sub-activity). Note that due differences in the frequency of the activity (monthly or yearly), comparing activities across question type may result in making incorrect conclusions due to framing rather than differences in frequency of the activities themselves. However, we assume that these framing effects are at least consistent across respondents in making the standardization. The most common community activities were attending funerals, markets, and political meetings. Respondents attended on average 2.8 funerals per month, 2.5 markets, and 2.6 political meetings. Other activities were attended less frequently On average, respondents attended approximately 1.4 different activities. This average measure is used to distinguish differential peer-effects among those with more or less levels of community participation.

An additional set of information that allows for measuring social networks is the religious membership of each respondent. Each respondent was asked about his or her religious affiliation as well as his or her specific place of worship. These data were coded to match individuals to their exact church or mosque. There were a total of 171 different churches or mosques identified, with an average of 11 different congregations in each VCT zone. The distribution of congregations varies substantially across Christian and non-Christian denominations. The majority (XX percent) of the Muslims live in the Southern region; in that region there is an average of XX mosques in each VCT zone. Congregations are used to link respondents to social network groups. Approximately 10 percent of the respondents had no data about their place of worship, either because they had no place of worship or they did not respond to the question (treatment of missing data is discussed below).

3. Estimation Strategy and Results

3.1 Estimation Strategy

The difficulties of identifying and measuring social network effects was first outlined by Manski (1993) who differentiated endogenous peer effects from exogenous peer effects. In this paper, the endogenous peer effect is the effect of neighbors obtaining their HIV results on others obtaining HIV

results. The exogenous peer effect is the effect of neighbors' background characteristics (such as attitude toward HIV or education) on others obtaining HIV results. Identification is further complicated by correlated effects – that individuals have self-selected into peer or network groups based on similar characteristics. Disentangling these various effects is one of the biggest challenges in the social networks literature and previous strategies have either utilized experiments (natural or those initiated by the researcher) that randomly assign peer groups or instruments for peer behavior. To estimate the effect of neighbors learning HIV results, we utilize the latter strategy, where exogenous incentives to learn HIV status are instruments for neighbors learning HIV results. Because neighbors randomly and independently received different levels of financial incentives to learn their HIV results, their behavior is exogenously affected and we can distinguish the impact of neighbors behavior on others. However, while we can identify peer effects, we cannot distinguish why peer effects may be larger or smaller, i.e., distinguishing selection from exogenous background characteristics of neighbors.

Our main specification estimating the effect of neighbors' VCT attendance on an individual's own VCT attendance is:

$$(1) \qquad \text{GotResults}_{ij} = \alpha + \beta \% \, \text{NeighborsGot}_{ij} + X'_{ij}\mu + \varepsilon_{ij}$$

where *GotResults* is an indicator whether individual \`*i*' in village \`*j*' attended the VCT center. The main independent variable, *NeighborsGot* is equal to the fraction of tested neighbors who attended the VCT center.

It is worth briefly mentioning the level of aggregation of the analysis. Previous research has found the size of the social group may have important effects on the size of the social network effect measured in the analysis. Glaeser, Sacerdote and Scheinkman 2002 present a discussion of the social multiplier effect where social network effects are larger when measured among larger groups of aggregation. In the case of this paper, one possible analytical exercise is to identify the impact of

those living within the same VCT zone on others' attendance. Due to the social multiplier effect, using this large level of aggregation may overstate micro-effects of individuals on learning HIV results. In our main specification, we examine the impact of neighbors living within 0.5 kilometers from one another on the decision to learn HIV results (corresponding to approximately half of the neighbors residing within one VCT zone– on average approximately 91 neighbors).[5] In order to test whether peer effects are more concentrated among those living in closer proximity, we present results for neighbors who live within 0.2 kilometer mutually exclusive radii bands from the respondent's household. When estimating peer effects among individuals belonging to the same church/mosque, we estimate the peer effects among all of those individuals living in the same VCT zone, rather than restricting the geographical space to within 0.5 kilometers. If we were to maintain our geographical restriction, the estimated impact of those attending the same place of worship would be confounded with the effect of those living within the same area. We also present placebo test results where we look at the impact of those living in different VCT zones where we would expect no significant effect of neighbors.

Each specification includes demographic controls of HIV status, age, age-squared, gender, years of education, number of assets, an indicator if the respondent received a positive-valued incentive, the amount of the incentive and district fixed effects. We cluster our standard errors by village. In addition, we include in each specification a control for the total number of neighbors in the reference groups. This controls for potential differential effects of social networks among those living among more or less densely populated areas.

Because the percent of neighbors attending the VCT is endogenous (and may also be affected by the respondent herself), we use an instrumental variables strategy to identify the causal effect of neighbors attending the VCT center, relying on the fact that neighbors received different values of

[5] Recall that in 1998 approximately one in four households were randomly sampled and it is reasonable to expect that the households that tested for HIV are evenly distributed with density roughly proportional to all the households within the sample villages.

monetary incentive and these incentives had a strong influence their the decision to attend the HIV results center. We instrument %*NeighborsGot* by a spline function of the percent of neighbors randomly assigned the various incentive amounts within the reference group; in the main specification, the reference group are those living within 0.5 kilometers. In particular, the first stage is:

$$(2)\ \%NeighborsGot_{ij} = \alpha + \beta_1 \%10-50_{ij} + \beta_2 \%50-100_{ij} + \beta_3 \%100-200_{ij} + \beta_4 \%200-300_{ij} + X_{ij}'\mu + \varepsilon_{ij}$$

In this specification, the omitted category is the percent of neighbors within the reference group receiving no incentive (zero valued incentive). The percent of neighbors receiving each of the other specified valued incentives (in Kwacha) are included as the instruments.

There is a large effect of the percent of neighbors receiving various amounts of incentives on the percent of neighbors attending the VCT center. Appendix A shows the first stage estimate for the neighbors residing within 500 meters of each respondent, indicating the large effects of percent of neighbors receiving incentives on the percent of neighbors obtaining HIV results. The F-statistic for the pooled male and female regression is 270 (Column 1).

The main assumption for our identification strategy is that neighbors' incentives do not have a direct effect on others' attending the HIV results centers. Because the vouchers were given in the privacy of each respondent's home, this is a reasonable assumption. However, it is possible that this assumption is violated for those living within the same household due to pooling of household income therefore we exclude all spouses and co-residents from the network analysis and only focus on non-spousal neighbors. It is also possible that individuals may have had behavioral responses to their neighbors' incentives, especially if the relative incentive amount in comparison to neighbors was an important determinant of obtaining HIV results. Given the nature of our social network data, we cannot test whether it is the actual amount of the incentive or the relative difference between the individual's incentive amount and her neighbors that is crucial in determining attendance at the VCT.

For our IV strategy, our assumption is that there is no direct effect of neighbors' incentives on VCT attendance.

In other specifications, the reference group of neighbors or peers may refer to subgroups such as females and males living within 500 meters of the respondent (rather than the total number of neighbors). As discussed above, other specifications involve other reference groups such as 0.2 kilometer bands or neighbors attending the same place of worship who live in the same VCT zone. In this case, the set of instruments corresponds to the reference group. For example, in the case of the 0.2 kilometer bands, the set of instruments include a set of splines for the percent of neighbors within each of the 0.2 kilometer bands who received the various levels of incentives.

It is important to note that those receiving varying incentives amounts had balanced baseline characteristics. Table 2 presents OLS regressions of baseline demographic data on having a positive-valued voucher, the amount of the voucher, and living over 1.5 kilometers from the VCT center. In most instances, there are no significant correlations between incentive amount and demographic data, although in some cases there are statistically significant differences. For example, those receiving positive incentive amounts were approximately 2 years older and were 5 percentage points more likely to be sexually active. However, these differences are not large in magnitude; the fact that the data is generally balanced on observables reassures us that the randomization occurred correctly and that the randomization is likely to have created balanced groups on unobservables.

Another important consideration is that in some specifications, some individuals will have no neighbors in their network. For example, when estimating the impact of peers living within 0.5 kilometers of an individual, there may be some individuals who have no neighbors living within that proximity. In that case, instead of coding those individuals with missing neighbors attendance (because they had no neighbors in that group), those individual's neighbor's VCT attendance is coded as zero percent attendance. This is due to the fact that if there are no neighbors in the

individuals' reference group, there will be no effect of those missing neighbors' VCT attendance on own attendance.

3.2 Results

Table 3 presents the OLS and IV estimates of the effect of neighbors living within 0.5 kilometers' attendance on respondents' own attendance at the HIV results centers. Columns 1-3 presents the OLS estimates of the effect of neighbors' attendance on respondents' VCT attendance. The coefficients are all positive and significant, of approximately the same magnitude. The IV estimates are slightly larger than the OLS estimates: the OLS coefficient for women is 0.101 while the IV coefficient is 0.134 (Columns 2 and 5). For the men, the OLS coefficient is 0.116 and the IV is 0.112 (Columns 3 and 6).

Column 4 presents the pooled IV results for men and women. The coefficient implies that increasing the proportion of one's neighbors that attend the VCT center by 10 percent increases the respondents' probability of attending by 1.24 percentage points. The results are similar for men and for women; 1.34 percentage points for women and 1.12 percentage points for men (Columns 2 and 3). We cannot reject the hypothesis that the effects among women are different than those among men (F-statistic 0.466, not shown).

These results are similar to other findings of peer effects. For example, Sacerdote (2000) found a point estimate of 0.131 of the effect of mean fraternity membership on a dorm floor in college on the likelihood of being in a fraternity. Rao et al. (2007) found slightly larger effects of students' flu vaccine decisions on their friends with a point estimate of 0.82; the effects in Rao et al. may be larger due to the fact that they observe specific friendship groups. Using neighbors living in close proximity as we do here may underestimate the effects of specific friends.

In addition to potential gender differences in the response to neighbors attending the VCT centers, there may be differential responses to neighbors of different genders (Moore 1990; for

Malawi, see Anglewicz and Kohler 2007; Gerland 2005). For example, it may be that men and women only respond to those of the same gender, because same-sex neighbors may be more influenced by each other. To test this, we separate neighbors living within 0.5 kilometers into gender subgroups: female neighbors living within 0.5 kilometers of each respondent and male neighbors living within 0.5 kilometers of each respondent; we then estimate the separate effects of male and female neighbors' VCT attendance on respondents learning their HIV results. Each independent variable – percent of females obtaining results and percent of males obtaining results – are instrumented with the percent of females receiving various incentive amounts and the percent of males receiving the various incentive amounts. The results in Table 4 indicate larger coefficients for neighbors who are female, although the coefficient is not statistically significant and the difference between males and females is also not statistically significant. The larger coefficient on female neighbors holds for both males and females (Columns 2 and 3). It is important to note that the set of instruments in Table 4 are different than those in Table 3 because each the spline of the percent of neighbors receiving differing amounts of incentives is calculated separately by gender in Table 4. The F-statistic of excluded instruments being equal to zero in the first stage are also reported.

There is an extensive literature that suggests that closer peers tend to be concentrated within a close geographical proximity of the individual (Conley and Udry 2000). We choose 500 meters as our main specification; however, we test alternative specifications to examine neighbors living within an even closer proximity. We examine the impact of neighbors' living within the 0.2 kilometer radii bands, from .2 meters to 1.0 kilometer.[6] The percent of neighbors living within each band is instrumented with the percent of the neighbors within that band receiving various amounts of incentives. The IV estimates are presented in Table 5. These results indicate that the peer effect is concentrated among those living closest to each individual rather than neighbors residing further

[6] Miguel and Kremer (2004) measure externalities using similar methodology. The correlation between average neighbors' and own distance to centers is 0.22, 0.18, 0.30 and between neighbors' incentives and own incentives is 0.02, 0.03, and 0.04 for radius bands of 0.2, 0.4, and 0.6 respectively.

away. The positive spillovers are present for both men and women (although statistically significant among men) and suggest that on average those with an additional 10 percent (approximately an additional 3 individuals) of their closest neighbors attending the VCT center are 0.63 percentage points more likely to attend the VCT; the results are similar among men and women. As a robustness check of these estimates, neighbor bands were drawn at further distances as a proxy: starting from 1 kilometer away from the individual. Those results indicated no significant effects of neighbors living further away on own attendance (not shown). Other specifications were also tested, for example, widening the bands to 100m and 300m bands which are generally consistent with the main results presented here – individuals living in close proximity have the largest positive impact on the respondent (not shown).

In these estimates of the effects of neighbors, we cannot rule out the possibility that these positive spillovers are driven by the fact that family members reside close to one another and influence one another's behavior. We can, however, rule out that this is not driven by individuals residing in the same household as the respondent and their spouse(s) because spouses have been excluded from the analysis. In addition, although we instrument the percent of the neighbors attending the VCT center with exogenously assigned incentives, we are unable to determine if the positive peer effects are due to the neighbors themselves, or due to correlated characteristics where individuals endogenously selected neighbors or places to live.

Social Intensity

We also explore how individuals with differing levels of social interactions respond differentially to neighbors' VCT attendance. We measure how individuals with differing levels of social participation respond to others attending the VCT. As described above, individuals were asked about their attendance to weddings, funerals, beer halls, markets, dances, HIV/AIDS meeting, drama groups and political meetings in either the last month or past year. Standardizing social participation

to times attending a meeting in the past month, we construct a “social intensity” index that measures the average number of times that an individual attended each social event. The index ranges from zero to just over 15 with an average of 1.43; a higher level indicates more social interaction within the community. We might expect that this social intensity index would be in important mediating factor for the effects of neighbors on own VCT attendance because those who interact more in the community may have more possibilities for being influenced by others. Alternatively, those who interact more in the community may be the type of person who is more influenced by others. Table 6 presents our preferred specification of the impact of neighbors living within 0.5 kilometers VCT attendance including an interaction with the social intensity index. There are striking differential results by gender and we focus on the disaggregate effects: for women, controlling for social intensity and the interaction of social intensity of the percent of neighbors attending the results center, there is no direct effect of neighbors per se. The peer effect operates entirely through social intensity (Column 2). That is to say, only among those who are more active in social groups in their communities (as measured by number of times participating in outside groups), the more neighbors' VCT attendance has an impact on own VCT attendance. Among women who have no outside community group participation, neighbors have no significant impact on own VCT attendance.

For men, social intensity has no impact on the network effects on VCT attendance: the interaction term is essentially zero (Column 3).

It is important to note that the result of differential networks effects among those with higher social intensity could be due either to the fact that more social participation and interactions increases the effect of networks on VCT attendance, or due to selection – those women who are more likely to be affected by others might also be the ones who are more likely to participate more frequently in social groups. Whether the result is causal or due to selection has important implications for whether it makes sense to promote interventions or programs targeting women's health utilization or not.

To examine these effects further, we disaggregate social participation and examine each activity separately. Table 7 Panel A presents the interaction of social intensity for each activity and neighbors' VCT attendance among women. The largest effect of social intensity is among those with higher levels of participation in drama groups (Columns 2 and 7). There is a small, although significant interaction of level of participation in political meetings and in attending a market (Columns 5 and 8). There are no differential responses to neighbors' obtaining their HIV results with participation in other activities. It is important to note that participation in each of the various activities were not highly correlated with one another. Appendix B presents the correlation coefficients separately for females and males. Thus, individuals who are more likely to attend more of one type of activity (e.g., drama groups) are not those who are also more likely to attend more of another activity (e.g., political meetings). This suggests that the large effects of social intensity found mainly among those participating in drama groups may be due to a selection argument – those who attend drama groups may be more influenced from other. Note that omitting drama group participation from the main regression of Table 6, column 2 reduces the coefficient from 0.136 (standard error 0.065) to 0.086 (standard error 0.049).

Religious Affiliation

Geographic proximity may not capture some of the more intricate networks occurring in rural Malawian villages. Networks may form through community organizations (such as clubs or community groups), or through participation in local activities (such as farming or going to market) as discussed above. Unfortunately, detailed network data on exact friendships are unavailable for these data. However, we have detailed information on one potential network – religious membership. Individuals were asked which religious organization they belonged to. Religious affiliation has been broadly categorized into Christian (Church of Central Africa Presbyterian (CCAP), Catholic, Other Christian Baptist, Anglican, Pentecostal, 7th Day Adventist, and Indigenous

Christian, and Muslim (Quadriya and Sukut). Eight percent of the main sample had no information about their religious participation. Of those with religious participation data, 99 percent reported attending a religious service or being a member of a religious group. Specific church membership includes individuals who attend the same congregation (either church or mosque); this was matched through the identification of the name of the congregation, and the name of the pastor or imam. Religious participation is important among the sample: out of all of those with a religious affiliation, 89 percent of respondents reported attending a religious service (either church or mosque) in the past month, with 63 percent attending in the past week. Religious affiliation may represent a fairly stable social network: only 12.8 percent reported attending a different congregation in the past year.

Table 8 presents the IV estimates of the effect of neighbors' VCT attendance among those living within the same VCT zone by religious affiliation and church/mosque membership. We include those living within the same VCT zone to include the maximum number of neighbors attending the same congregation. We do not present the specifications among neighbors living within 0.5 kilometers of each individual to avoid confounding close-proximity effects with congregation network effects.

Each independent variable of the percent of neighbors attending the VCT is instrumented with the percent of those individuals receiving various incentive amounts (equation 2). The pooled regression including all religions includes a religious denomination fixed effects (Catholic, other Christian, Muslim, non-Christian, or no religion).

Columns 1-3 presents the IV results of the impact of neighbors VCT attendance belonging to the same church/mosque on own VCT attendance for the pooled male and female sample as well as disaggregated by gender. While the coefficients of the effect of the percent of neighbors in the same church/mosque attending the VCT are positive, there are no statistically significant effects.

We can also explore whether more frequent attendance at church has a differential impact on the effect of social networks, as it did among women for community activities. In this case, the

interaction between attending a religious service in the past week and percent of neighbors in the same church/mosque obtaining their HIV results is negative among women (Table 8, Columns 4 - 6). It is essentially zero among men. While the coefficient among women is not statistically significant at traditional levels, it may be that there is a slight negative effect of social networks among those of the same religious group, of those who attend more regularly. However, this interpretation should be viewed with caution as the standard errors of the estimate are high.

We also measure the impact of VCT attendance among who are not in the same church/mosque (Table 8, Columns 10-12). There is no difference between those attending the same church and those who do not attend the same difference on the level of peer effect on obtaining HIV results.

Religious organizations have been both faulted and credited with their responses to the HIV/AIDS epidemic. The responses of religious leaders has varied and there are examples of both positive and negative effects of religious groups on levels of stigma, adoption of safe sexual behavior, and community support. The lack of a positive effect of within church neighbors may suggest social pressure or potentially stigma among females to attend the VCT with their close religious network partners.

Individual Incentives vs. Peer Effects

The fact that individuals were given incentives to attend the VCT clinic not only allows for instrumenting neighbors' VCT behavior, but also allows for quantifying the tradeoff between individual incentives and peer effects to learn HIV results. Offering individuals any positive incentive increased the likelihood of obtaining results by almost 36 percentage points (not shown). Among those who received some positive valued incentive, receiving an additional ten cents of monetary incentive increased the likelihood of obtaining results by 12 percentage points.

The social network effects can be similarly quantified: among those receiving no monetary incentive, a 10 percent increase of neighbors attending the results centers increases the likelihood of attending by 2.1 percentage points. The network effect is significantly larger among those who received no monetary incentive than those who received some monetary incentive. Individual regressions similar to Table 3 Column 4 among those receiving different amounts of incentives indicate large effects among those offered up to 100 Kwacha, leveling off after this amount (not shown). This suggests that for programs aimed at increasing uptake of VCT services, offering each individual a monetary incentive may not be the optimal policy, rather offering a few people monetary incentives would yield larger effects through the social network effects. This is similar to the findings in Duflo and Saez (2003) who find large spillover effects in giving some individuals information about a retirement benefits fair.

4. Conclusion

Understanding why people choose to learn their HIV results is important both theoretically for social scientists and for public policy. In the past several years, governments, NGO's, as well as academics, have emphasized the importance of voluntary testing and counseling (VCT) as a strategy for treating HIV-positive individuals (Ainsworth and Oliver 1997). In addition, other health programs target visits to clinics or other community health events such as vaccination days or baby weighing days.

This paper shows social network effects of neighbors VCT attendance on others' VCT attendance. These effects are strongest among closest neighbors living within 200 meters or approximately the 30 closest households. Social intensity is important for women, although not for men.

Individuals make decisions related to their health based on information, costs, benefits, as well as their social networks. The largest determinants of obtaining HIV results in this study was due to financial incentives; however, it may not always be feasible to subsidize HIV testing, or other

health programs for that matter. In that case, treating some key individuals in communities and allowing those individuals to be catalysts for others may be the most optimal way of increasing health utilization. Who these key individuals are, for example, first movers in the decision to learn HIV results in this study, is the subject of future research.

This paper adds to the growing literature quantifying the impact of social networks using randomized designs. In general, peer-effects of social networks is highly contextualized. In some cases, studies have found large effects of peer-effects in the choice to adopt new technology, or attend informational sessions (Oster and Thornton 2008; Duflo and Saez 2003). In other cases, peer-effects have been found to be quite low (Duflo et al. 2006, Miguel and Kremer 2004). Understanding how these effects operate is important for targeting programs and policies that can assist in improving the speed of adoption or learning about health and health related information.

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