Social Learning, Social Influence and Projection Bias:
A caution on inferences based on proxy–reporting of peer behavior

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Abstract

This paper explores the consequences of conflating social learning and social influence concepts and of the widespread use of proxy-reported behavioral data for accurate understanding of learning from others. Our empirical analysis suggests that proxy-reporting is more accurate for new innovations, about which social learning is more plausible, than for mature technologies. Furthermore, proxy-reporting errors are correlated with respondent attributes, suggesting projection bias. Self- and proxy-reported variables generate different regression results, raising questions about inferences based on error-prone, proxy-reported peer behaviors. Self-reported peer behavior consistently exhibits statistically insignificant effects on network members' adoption behavior, suggesting an absence of social effects.

Introduction

In economic learning models, agents generally learn from their own experience ("learning by doing"), from observation of others ("learning from others"), or both (Foster and Rosenzweig 1995; Armantier 2004). Learning from others generates a social multiplier effect in the diffusion of innovations that can be exploited as a cost-effective mechanism for disseminating technological innovations promoted by extension services in developing countries. The fundamental idea is that individuals learn from and imitate the behavior of others within their social network, as suggested by the well-known contagion model for adoption of innovations (Rogers 1995). These network-mediated effects are often decomposed into social learning effects and social influence effects, where the former refers to active information search and construction of rational beliefs, while the latter refers to the desire to avoid conflicts with others by harmonizing one's own beliefs with prevailing beliefs within one's reference group (Montgomery and Casterline 1996). However, the distinction between these two phenomena has been underappreciated by economists, whose interest in the importance and mechanisms of learning from others has grown rapidly in recent years (e.g., Conley and Udry 2001, 2007; Behrman, Kohler and Watkins 2002; Miguel and Kremer 2003; Munshi 2004; Mwakubo *et al*. 2004; Bandiera and Rasul 2006; Moser and Barrett 2006). This paper explores the consequences of conflating these two concepts and of the widespread use of proxy-reported behavioral data for accurate understanding of learning from others.

If an agent has a reason to believe that learning about an innovation may be useful, she may then start looking for others who have either information about or, especially, personal experience with it. In the process of gathering information about peer behavior and performance, the agent forms beliefs about the innovation that may develop over time; those beliefs influence one's choice of whether or not to adopt an innovation oneself.

Two pieces of behavioral information may be of particular interest when considering whether an innovation is worth putting effort into learning about. The first is how many others have taken up the innovation in question, i.e., the population adoption rate among social network members, in the village, or in whatever the relevant reference group is (Ueda 2002). This indicates how interesting others, on average, judge the innovation. Population-level observations are associated with learning through “social influence”, which concerns the establishment of social norms with which people conform or comply, at least to some degree. In order to conform to prevailing social norms, people need knowledge only about aggregate measures of peer behavior, but not about the specific behaviors of individual network members.

The second potentially valuable piece of behavioral information concerns whether certain significant others – e.g., “opinion leaders”– have adopted it (Feder and Savastano 2006). “Social learning” reflects this more individually-precise process of information exchange between persons who specifically identify and evaluate each other’s contributions to the information generation process. This more active social learning often involves seeking out socially distant people with whom one otherwise has infrequent contact, so-called “weak ties” one mobilizes selectively (Granovetter 1973). To identify useful partners for active social learning, agents need precise knowledge about the behavior of at least some others, and the active social learning process itself typically involves exchange of reasonably precise information about each other’s experiences with the technology in question among learning partners. Therefore active social learning should be associated with precise knowledge about the behavior of at least a subset of one’s social network members.

Observing significant others, perhaps especially opinion leaders, may be a relatively effortless way of monitoring the introduction of new innovations, while monitoring population adoption rates may confirm whether one's own judgment is at odds with prevailing views about an innovation. These two processes may be independent of each other, so that people may engage in both, either, or neither. Indeed, it is possible that acquisition of these two pieces of information relies on different information sources or different learning processes, returning us to the distinction between social learning and social influence. Thus, it is possible that people have a relatively good idea about population adoption rates without knowing the exact adoption status of many, if any, specific individuals, and vice versa.

We can make inferences about active social learning versus social influence from two dimensions of the information people have about peer behavior. We propose that having precise information on others' individual-level behaviors, and/or about peer behavior that is difficult to observe, provides evidence of active social learning. Conversely, if farmers possess precise information about peer behavior only at the aggregate level, and/or only about behavior that is easy to observe, it would seem to signal a process of social influence, rather than of active social learning. A combination of accurate knowledge of individual-specific as well as aggregate behaviors, and of both easy- and difficult-to-observe technologies would indicate that both processes are at play.

This distinction between social learning and social influence is important not only to how researchers conceptualize the interpersonal diffusion of information concerning innovations, but also to the design of technology dissemination policies (e.g., through extension services) and to the data researchers use to identify "learning from others" effects. The policy implications arise because standard designs for extension policy implicitly assume agents are active learners who absorb specific information from change agents (e.g., extension officers or model farmers) and thereby create a critical mass of adopters, whose collective experience then diffuses more passively through processes of social influence. A target group that mainly conforms to prevailing practices may be harder to reach because they respond less to precise information about an innovation than to the belief that a consensus exists in favor of the innovation within their particular reference group. If previous adopters do not generate this sense of consensus, passive learning may fail and even effective technologies may not diffuse through the population. The implication is that information aimed at an actively learning target group should emphasize the attributes of the innovation itself, while information aimed at passive learners should rather emphasize attributes of the innovation's adopters and encourage people to identify with the adopters, i.e., choose to make adopters their reference group.

The expected observational outcome of social learning is that behavior is correlated within social networks. Thus, one way of approaching this question empirically is to test for a positive relationship between a subject's technology adoption choice and the density of other adopters in her social network. However, social learning is not the only possible explanation for correlated behavior; Manski (1993) identifies three sources of this observational outcome. They are (a) correlated effects (members of the same social network tend to share the same environment), (b) exogenous effects (they also tend to be similar, e.g., due to endogenous network formation), and (c) endogenous effects (they influence each other through social interaction), the effect we are interested in. When correlated behavior is observed, it remains to be determined which one(s) of these effects explain this outcome.

But the appearance of correlated behavior may also be a result of measurement error. This problem is especially likely when peer behavior is proxy-reported, a data collection method often employed when researchers lack the means to use better methods. Proxy-reported peer behavior is often correlated with the research subject's behavior due to "projection" or "false consensus" bias (Ross, Greene, and House 1977). This can arise when those who have (not) adopted the innovation tend to mistakenly over-report (under-report) the incidence of adoption in their social network. When this is the case, what appears to be correlated behavior is rather an illusion.

The problem of measurement error in proxy-reporting of peers' behaviors has been highlighted in, for example, research on the returns to education (Ashenfelter and Krueger 1994). But prevailing empirical practices suggest it remains underappreciated in the economic literature on learning. The issue is that researchers who study learning from others need information about subjects' peers' behaviors. In many cases, the research subjects themselves are the source of this information via proxy-reporting of peer behavior, despite longstanding warnings about problems associated with such data (Ashenfelter and Krueger 1994; Montgomery and Casterline 1996). Recent examples of social network studies that use proxy-reporting of peer behavior include several eminent scholars and widely cited papers (e.g., Behrman *et al.* 2002, Bandiera and Rasul 2006). These examples highlight the importance of investigating the problems associated with using proxy-reported peer behavior in regressions to measure social learning effects.

Statistics and Econometrics

The inferences we make in this paper are based on two aspects of proxy-reported peer behavior. The first is whether proxy-reported data are accurate measures of peer behavior, which we assess using simple statistics. The second is the econometric consequences of using the proxy-reported variable as opposed to the self-reported variable, assuming the self-reported variable reflects the truth. In this section, we briefly describe our statistical measures and econometric procedures.

Determine the Accuracy of X^P as a Measure of X^S

There exists an extensive literature about measurement errors in surveys. Two distinct perspectives on measurement errors can be identified in this literature; Biemer and Stokes (1991) refer to these as the “sampling” and the “psychometric” perspectives. The former is concerned with econometric consequences of errors introduced through the sampling procedure; the latter concerns variance in responses. Our concern is with response errors, the latter.

Estimation of response errors requires repeated observation of the same variable. In our case, we have two independent but reasonably contemporaneous observations of the variable of interest, i.e., whether our respondent’s peer is using a particular NRM practice on his or her farm. We will refer to respondents as “ego” and the network member they identify as “alter”. We use X^S to represent alter’s self-reported peer behavior, and X^P for ego’s proxy-reported value of the same variable. We assume self-reported data are superior to proxy-reported data, and therefore use these as the “gold standard measure” (Biemer and Woltman 2001) of true peer behavior. We seek to determine to what degree X^P is an accurate measure of X^S. In this analysis, we use two statistical measures:- measurement bias and the reliability ratio.

Measurement bias is a well-known statistic. Under the assumption that X^S is the truth, the difference in expected value between the two data sources, $E[X^P] – E[X^S]$, is a consistent estimator for this bias, B. To test the significance of the estimated measurement bias, we use the standard variance of an estimator to calculate a t statistic. The estimated bias, variance and associated t statistic are thus given by equations (1)-(3), where N is the number of observations.

$$B = E[X^P] - E[X^S] \qquad (1)$$

$$\text{Var}[\text{E}(\text{X}^{\text{S}})] = \frac{E[X^S]*(1-E[X^S])}{N-1} \qquad (2)$$

$$\text{t} = \frac{B}{\sqrt{Var[E(X^S)]}} \qquad (3)$$

We define the reliability ratio, R, following Boozer and Goldstein's (2003) comparison of self-and cross-reported consumption data within households in Ghana. This definition is simply the slope of the univariate regression of X^P on X^S, i.e., the covariance of the two measures normalized by the estimated variance of the true measure, as given by (5). Note that X^S is distributed Bernoulli (p), where $E[X^S]$ is an estimator of p.

$$\text{R} = \frac{Cov[X^P, X^S]}{Var[X^S]} \qquad (4)$$

$$\text{Var}[\text{X}^{\text{S}}] = \text{E}[\text{X}^{\text{S}}]*(1-\text{E}[\text{X}^{\text{S}}]) \qquad (5)$$

We explore independence between the reporting error and typical explanatory variables used in regressions to elicit social network effects on technology adoption by regressing this measurement error (i.e., $X^P – X^S$) on a set of such variables. One specific effect we look for is projection bias, i.e., a positive and statistically significant correlation between the measurement error and ego's own behavior. Since X^P and X^S are binary variables taking the values 0 and 1, this difference can take three values: -1, 0, and 1. Thus, we use a multinomial probit estimator to search for such relationships.

Does Bias in Proxy-Reporting Influence Statistical Inference?

Researchers who study social network effects on respondents' behavior often use proxy-reporting of peer behavior because it is an easy, low-cost way to collect data on the behavior of survey respondents' peers. The alternative would be to track down and interview the peers identified by the survey respondents – a technique known as "snowball sampling" – but that is often logistically and financially infeasible. The consequences of using proxy-reported data may vary, depending on the particular circumstances of each study, as well as the research question and design. However, our regression results offer a cautionary example of the bias that can result from this widespread practice.

In order to explore the effects of using proxy-reported variables in place of self-reported ones in estimating the determinants of farmer technology adoption, we ran series of regressions in which the only difference is the variable used to represent peer behavior. Since the dependent variable, respondents' self-reported own adoption behavior, is binary, taking unit value if the respondent uses the NRM practice in question and zero otherwise., we use a standard binomial probit estimator. We ran two series of regressions, one with the ego-alter dyad as the unit of analysis, the other with only ego as the unit of analysis. In the former, each respondent appears the same number of times as the number of social network contacts the data contain information about, which represents the observed density of the respondent's network. To ensure each respondent carries equal weight in the regressions, each observation is weighted by the inverse of the observed network density. In the latter series of regressions, we faced a different problem - a very small sample, only 56 observations.

In the dyad-specific regressions, the same variable appears on both sides of the equality sign, albeit from different observations: ego's self-reported adoption behavior, and alter's adoption behavior, either self-reported or proxy-reported. Manski (1993) showed that in linear regressions, this model is not identified. In particular, the parameter on the alter behavior variable (interpreted as evidence of endogenous social interactions) is not identified. Interpreting this coefficient is difficult for two reasons: (a) the coefficient may be picking up unobserved correlated or exogenous effects (an omitted variables bias), and (b) since ego is herself a member of the reference group, she contributes directly to group attributes that are assumed to influence her behavior, so it is impossible to determine whether behavior is correlated because alter influences ego *or vice versa*. The latter is the essence of the reflection problem Manski (1993) highlighted.

This identification problem may be less pronounced in binary response models, due to their non-linear nature. Brock and Durlauf (2007) showed that in binary response models with social interactions, partial identification may be achieved under assumptions that are plausible in our context. Their analysis focuses on the relationship between observed and unobserved attributes of ego and the reference group. If assignment to groups is random and there are no unobserved group-level contextual effects, the coefficient on the alter behavior variable is identified up to scale (their Proposition 1). In the presence of unobserved group-level contextual effects, the coefficient is not identified (Proposition 2). However, partial identification may be achieved with certain restrictions on the relationship between observables and unobservables. We assume that in our regressions, partial identification of this parameter is achieved[1]. (Since that is probably the best we can do, we follow Manski's (2003) advice and accept the remaining ambiguity as something with which we must live.)

Data

The data were collected during 2003-2004 in two sites in Kenya: Manyatta Division in Embu District (Eastern Province), and the former Madzuu Division (now divided between several new political entities) in Vihiga District (Western Province), in the country's central and western highlands, respectively. The sample of research subjects consists of 120 households in each site who had been randomly sampled from Division household rosters and previously surveyed under a separate research project. All households are active farming units, but some households have substantial non-farm income sources as well.

We ran two different data collection exercises among the same households. The first survey aimed to enumerate respondents'[2] social networks of different sorts and to collect information on respondents' perceptions of their network contacts' use of any of four different natural resource management (NRM) practices that may improve soil fertility and agricultural productivity: terracing, fallowing, use of organic fertilizers, and planting crops in deep, manure-filled pits, a practice known by the Swahili word *tumbukiza*, which means "submerge". We describe these four NRM practices in a bit more detail below.

The survey respondents ("egos") were asked to identify the people with whom they engage in borrowing and lending (their "transfers" network), the people they (currently) "like" to discuss issues of farming with (their "information" network), and their neighbors, i.e., the household head of each farm bordering the respondent's. Respondents were asked a set of questions about each of these network contacts (labeled "alters"). For each alter, irrespective of network, respondents were asked whether they had discussed the four NRM practices under study with alter, and whether alter had adopted the practice.

Questions used to elicit names of respondents' social network members are often referred to as "name generators", and may not have any other purpose than helping respondents select a manageable subset from a normally very numerous set of social network contacts. In our case, the three categories of people were chosen to enable us to test the relative importance of social influence and social learning. To do so, we wanted to analyze social network effects controlling for purposive communication (information networks) and economic ties, in particular being dependent on alter for economic favors like cash loans (transfer networks). Neighbors were included as a control group. Of course, these networks were overlapping.

A subset of the sample (56 respondents) – one village from each site – was selected for a follow-up round of "snowball sampling" in which alters identified in the first round were tracked down and interviewed using the same questionnaire to which egos responded in the first round. This method permits us to check the accuracy of respondents' (egos') perceptions of the technology adoption behaviors of members of their social networks (alters).

The 56 primary respondents and 267 secondary respondents generated a total of more than 1200 relations amongst each other, i.e., where both ego and alter were among the respondents (the "snowball sample", based on alter self-reporting). This study also uses data collected for the earlier study. These data are only available for primary respondents (egos). When secondary respondents are excluded, the data comprise 654 relations[3].

The meaning of being "an adopter" of a technology was explained to respondents as "knowing it and using it, either continuously or intermittently". Thus, the adoption variables should not depend on the respondent's ability to observe short-term changes in use of a particular practice, thus temporal mismatch should not explain much misreporting of alters' behavior. This is most relevant for organic fertilizer application, which often depends on the availability of suitable organic fertilizers. Many respondents reported having difficulties getting enough organic fertilizers to enable continuous use. Using this interpretation of what it means to be an adopter, having temporarily abandoned organic fertilizer application due to non-availability is not considered disadoption.

The Natural Resources Management Practices Under Study

Before presenting the first set of empirical results, let us briefly describe the four technologies studied. By design, these differ in the ease with which outsiders might observe the practice and with respect to the prevalence of each practice within the population. In a matrix of high/low ease of observation and high/low prevalence, each of the technologies can be assigned to a separate cell (Figure 1).

Terracing was introduced in Kenya nearly a century ago by the British colonial government in response to severe land degradation in parts of the highlands. Despite the heavy-handed interventions of the colonial government and some policy reversals in the immediate post-independence period, adoption of terracing increased steadily as farmers discovered the benefits of terracing (Kamar, Mburu and Thomas 1999). Adoption of terracing has therefore likely reached its long-term equilibrium level, at around 90% adoption in both sites. Terracing is easy to observe as it involves clear, prominent alterations of the physical landscape, so one would expect passive observation of terracing to be relatively accurate and unbiased.

Fallowing was an important component of the traditional slash-and-burn agriculture practiced in the past. Increasing population pressure on the land has led to increased continuous cultivation of land and a decline in fallowing (Drechsel *et al*. 2001). Fallows are more common in Embu (31%) than in Vihiga (23%), reflecting a higher population density in the latter. Like terracing, fallowing is easy to observe by any passerby, especially in the high population density areas we study, where unexploited private land is relatively uncommon.

For generations, farmers in the Kenyan highlands have incorporated organic matter into their cultivated plots as a way of boosting soil fertility. Thus organic fertilizer use is a mature technology, like terracing. Almost all (96% of) respondents apply at least one type of organic fertilizers[4]. However, many have very limited access to organic material for production of organic fertilizers, so quantities applied are often quite small, making observation of organic fertilizer application difficult.

Tumbukiza represents deep incorporation of organic fertilizer into the soil on a small piece of land, thereby increasing the long-term effects of the treatment but requiring significantly more labor effort than top dressing with organic or inorganic fertilizer. This relatively new practice is being promoted by the government extension service in the study areas. But most survey respondents were unfamiliar with *tumbukiza*. In Embu, about 45% of our respondents practiced *tumbukiza*, while in Vihiga, only 13% did. Indeed, a large proportion of respondents, especially in the Vihiga site, had never heard about the practice and did not know anyone who practiced it. *Tumbukiza*, like organic fertilizer application, can be difficult to observe passively, thus one might reasonably expect relatively greater inaccuracy in proxy-reporting of this practice.

Results

Our discussion of results will first examine the accuracy of proxy-reported data as measures of true peer behavior, measured by the bias and reliability of the variable as a predictor of self-reported NRM technology adoption data. Then we discuss inference errors associated with using proxy-reporting data in lieu of self-reported data on peer behavior in regressions.

The accuracy of reported data as measures of peer behavior can be inferred from Table 1. The joint distribution of proxy-(X^P) and self-(X^S) reported data is described by the three statistics $E[X^P]$, $E[X^S]$, and $E[X^P*X^S]$. Using these statistics, it is possible to calculate all other statistics in this table.

Bias

$E[X^P – X^S]$ measures bias in proxy-reporting. This bias is small for all four NRM practices, as it never exceeds five percent, although it is statistically significantly different from zero in three out of four cases. The largest biases are found for the practices terracing (- 4.7%) and organic fertilizer application (- 3.7%), both of which are significantly (at the 1% level) underreported by respondents. The bias is positive and smaller, only 2.7%, and likewise statistically significant (at the 5% level) for fallowing. Only for *tumbukiza* is $E[X^P]$ an unbiased estimate of the aggregate adoption rate. Although proxy-reported data on peer NRM adoption behavior is statistically significantly biased, the data nonetheless suggest that respondents understand prevailing adoption patterns; observing aggregate behavior within a deviation of plus/minus five per cent is not bad. Unfortunately, respondents were not given an opportunity to report their beliefs about aggregate behavior, since the survey instrument contained no such question. That information could have been useful for comparison with reports about the behavior of named individuals and the result of aggregating those reports.

When researchers collect survey data, we hope our respondents will use the “I don’t know” option when that is the truth, and we should expect to get some such responses. In this study, the rate of “I don’t know” responses was low – in the raw data the rate of missing observations is only 2-3, 7 and 11% for alter adoption of terracing and organic fertilizer application, fallowing and *tumbukiza*, respectively. These low rates of “I don’t know” responses may indicate unwillingness among respondents to disappoint the interviewer with a non-response. If this interpretation is correct, many respondents may have merely guessed the behavior of some of their social network contacts. It is impossible to determine the expected outcome of such behavior if guessing is entirely random.

But one may hypothesize candidate guessing strategies. One strategy isto give a positive response (“Yes, alter is an adopter”) with frequency f, based on fairly good knowledge about the aggregate adoption rate, α. Then, the expected rate of correct responses will be:

$$E[\text{correct response}] = f\alpha + (1 - f)(1 - \alpha) \qquad (6)$$

We can easily determine that the optimal f will be 1 if the known adoption rate is greater than 50%, and 0 if it is less than 50%. Thus, extensive strategic guessing based on knowing the aggregate adoption rate would bias responses upwards for prevalent practices, and downwards for the less prevalent ones. The observed biases in Table 1 are not consistent with this type of strategic guessing.

Another possibility is that respondents have no idea what the aggregate adoption rate is, and guess based on the assumption that their own behavior is congruent with the behavior of the majority (i.e., projection of own behavior onto others). In that case, respondents will report that all network contacts whose behavior they don’t know do as they do themselves. If network density and the percentage of network contacts whose adoption behavior is unknown to the respondent are independent of the respondent's own adoption behavior, this guessing strategy would be impossible to detect in aggregate statistics, and would generate data with the correct aggregate rate of positive responses. But it would generate data where (proxy-reported) alter behavior is correlated with the respondent's own behavior. Based on the information presented in Table 1, we cannot rule out this possibility. We investigate below whether our data are consistent with this type of behavior.

Reliability Ratio

The estimated reliability ratios reported in table 1 reveal a low correlation between proxy-and self-reported behavior. The reliability ratios range from 5.1% for terracing up to 10.2% for *tumbukiza*. While the bias indicates that, on average, proxy-reporting errors largely cancel out, providing a reasonably accurate view of aggregate behavior, the reliability ratios suggest that, in general, respondents do not accurately link behaviors to specific individuals within their social network.

Note that *tumbukiza*, the practice with the highest estimated reliability ratio and the only one with no bias in aggregate proxy-reporting, is the one practice that is both difficult to observe and of limited prevalence. The relatively greater accuracy – at both aggregate and individual levels – of this practice in spite of its low prevalence and limited observability would seem to suggest greater effort expended to collect information on a lesser known and hard-to-observe innovation, i.e., possible evidence of active social learning. Fallowing, the other practice of limited prevalence in this sample, was also reported more precisely – at both aggregate (bias) and individual (reliability ratio) levels – than the two most common practices, terracing and organic fertilizer application. Conversely, although terracing is the one practice that is both easy to observe and prevalent, proxy-reporting of terracing is the least accurate, whether measured by bias or the estimated reliability ratio. Apparently, it serves no purpose for these respondents to invest effort in obtaining precise information about the prevalence of terracing among their social network members.

Compared to the findings of other studies, these reliability ratios are very low. Ashenfelter and Krueger (1994) report the reliability of twins' proxy-reporting each other's schooling level. They find reliability ratios of 88-92%. The findings of Biemer and Woltman's (2001) study on the race question with a multiple-response option introduced in the US Census 2000 are more mixed. Here, self-reported race data from a 1998 Dress Rehearsal Census are compared with the same variable reported in a repeat survey. Reliability of self-reported White or Black race is good at 96.4 and 93.8%, respectively. But for other races, reliability is considerably lower, and the reliability ratio for respondents ticking multiple races is only 28.1%. This low measure cannot be explained by respondents ticking different combinations of races, since all combinations of multiple responses were clustered into one category. These respondents altered between ticking several or only one race.

Boozer and Goldstein (2001) use household consumption data from Ghana to calculate a poverty measure. The data were collected by interviewing both spouses in the sample households, asking both spouses to report both their own and each other's consumption. The article does not report reliability ratios, but states that although the two spouses' reports are highly correlated for many goods, proxy-reported consumption of some goods, especially foods consumed off-farm, contained "essentially no information". Differences between men's and women's reporting were large enough to generate different poverty measures, with women's reports leading to higher measured poverty rates than men's reports. Boozer and Goldstein suggest the differences between men's and women's reporting reflect the existence of separate spheres for men and women, with consumption undertaken in the exclusive spheres being private information.

Our findings suggest that while our Kenyan respondents may have a reasonably good sense of aggregate NRM behaviors, the accuracy of perceptions of individual-specific behaviors is low. Moreover, the accuracy of proxy-reporting at both aggregate and individual levels is higher for the techniques that are less prevalent and/or harder to observe. Since this runs contrary to the result that would obtain for equal effort expended in data collection on each technique, this strongly suggests farmers exert more information search effort and pay closer attention to individual-specific details on technologies they find particularly interesting.

The independence of proxy-reporting errors

Studies of social network effects on technology adoption often use explanatory variables that reflect attributes of the respondent, who is expected to be learning about a technology, attributes and behaviors of alters, from whom the respondent is expected to be learning, and attributes of the technology itself. However, information on the behaviors of respondents' social network members typically comes from proxy-reporting. If there exist systematic proxy-reporting errors, as the previous two sub-sections document, this raises the possibility that those errors might be correlated with right-hand side variables in the adoption regression and thereby bias the estimated relationships between covariates of interest and respondent adoption behaviors. Or worse, errors may be correlated with the dependent variable itself. Because our data include rich information about respondents, their social network members and interactions between them, we can run standard adoption regressions to test for correlated behavior with respect to uptake of the NRM practices we study. Furthermore, because we have both proxy- and self-reported observations of alters' behaviors, we can test explicitly the hypothesis that proxy-reporting errors are independent of the regressors of interest.

To test for independence between the proxy-reporting error and explanatory variables of interest, we ran a series of multinomial probit regressions of this error on key regressors.

Summary statistics for those variables are presented in table 2. These include ego and alter attributes, ego's technology adoption choices, and a variable representing whether ego and alter have discussed the technology in question. Ego and alter attributes include ego's gender (0 = male, 1 = female), and whether alter is a family member, member of ego's information network, a neighbor, or a public employee. The latter is the category in the data with the highest income and social status. Thus, public employees may be considered "significant others", and are possibly people other villagers try to imitate. We see that 43% of egos are female, and that only 4% of alters are public employees. Family members, information network members, and neighbors constitute 23%, 37%, and 47% of alters, respectively. The technology adoption rates among respondents are about the same as for alters, found in table 1. There appears to be a close relationship between adoption rates and rates of having discussed a technology, but people are disproportionately more likely to discuss the less prevalent practices, confirming our inferences above with respect to the bias and reliability ratio estimates.

Table 3 a-b) reports the results of regressing proxy-reporting errors on the selected covariates. Neither ego's gender nor alter being a family member is statistically significant in these regressions, with one exception. It surprises us a bit that reporting is not more accurate for family members, given respondents' generally closer interaction with these alters. The general absence of a gender effect is interesting, but not unexpected. To some extent farming is a gendered activity in the study sites; in many sample households men work off-farm, while women take care of the farming enterprise. Thus, one might have expected that women were better informed about farming activities in their social environment. But the sample was explicitly selected to be made up of only active farmers, so both male and female respondents were active farmers. However, females were more likely to over-report alters' adoption of *tumbukiza*. This may suggest that this technology interests women more than men, although they are not more likely to be practicing it, as regressions reported below show.

Alter's geographic proximity or membership in ego's information network is only weakly related to proxy-reporting errors. Since the excluded category is transfers networks, which we might expect to generate less accurate information than having neighboring farms or explicit information exchange, this result is mildly surprising, indicating that particular sub-networks do not generate systematically more accurate information. It is possible that different types of information flow through different sub-networks, and the relationship between types of information and particular sub-networks has not been captured by this study.

Reporting on public employees' behavior is generally more accurate, as seven of eight coefficient estimates are negative, although typically imprecisely estimated. However, the one positive estimated effect (on positive errors related to *tumbukiza*) is statistically significant and of statistically equivalent magnitude to the one significant negative estimated effect (on positive errors related to terracing). Given the very small number of public employees in respondents' social networks, one needs to be cautious about inference; but these results are consistent with the idea that people pay closer attention to the behaviors of higher profile individuals within the community. Thus social learning is likely most accurate from elite farmers or others who naturally attract attention within the community.

Respondents' adoption status is not independent of proxy-reporting errors with respect to peers' adoption choices. Six out of eight coefficient estimates have signs that are consistent with the hypothesis that errors in proxy-reported alter behavior are positively correlated with respondent behavior, three of them statistically significantly. The two coefficient estimates whose signs are inconsistent with this hypothesis, are both insignificantly different from zero. The implication, of course, is that adopters tend to overstate the likelihood that members of their social networks likewise practice the same technology.

The effect of ego and alter having discussed the technology is even stronger in reducing false negatives and increasing false positives in proxy-reporting. In all four cases, the likelihood of a false negative is statistically significantly lower for respondents who report having discussed the technology with alter than for those who do not, while in two of four cases false positives are statistically significantly more likely for respondents who have discussed the technology. Discussion of a technology appears to be easily conflated with one's partner's adoption of the technology.

Overall, there are highly statistically significant relationships – as reflected in the χ^2 test statistic of the "all slopes equal zero" null hypothesis – in each regression of proxy-reporting errors on standard variables that one might reasonably include in an adoption regression specification that allows for possible social learning effects. Of particular note, the results with respect to the latter two variables generate a clear suggestion of possible projection (or false consensus) bias, the routine misattribution to others of one's own behavior, an effect that is substantially reinforced by having discussed the topic[5]. Furthermore, these effects are most pronounced with respect to the least widely adopted technologies – fallowing and *tumbukiza* – for which strongly significant false positive rates are associated with respondent having adopted or discussed the technology with her peer and discussion with peers is also associated with significant reduction in the rate of false negatives in proxy-reporting. Of course, these are the technologies for which the value of accurate social learning is potentially greatest. Furthermore, this indicates that positive proxy-reporting bias is associated with being an active learner, one who experiments with the technology, and also discusses it with others. Failure to recognize the apparent projection bias in proxy-reporting of technology adoption behavior can thereby lead to exaggeration of social learning effects.

The robustness of estimated "learning from others" effects

Our analysis thus far has established that proxy-reporting of peer technology adoption behavior tends to be mildly biased overall, but relatively weakly correlated with individual-specific behaviors, with errors that are statistically significantly related to respondent and alter attributes and, especially, to respondent's own adoption behavior and past discussion of the technology with the network contact. This suggest a pattern of social influence based on reasonably accurate perception of overall adoption behaviors in one's reference population, but with perceptions of the behaviors of specific network members that are highly imprecise and plagued by projection bias. This does not necessarily imply, however, that econometric estimates of "learning from others" effects in standard technology adoption regressions are seriously biased.

In this section we explore that issue directly, testing whether such estimates are robust to the substitution of self-reported data on peer behavior for more widely employed proxy-reported data. We ran two series of regressions, where we first used the ego-alter dyads as our unit of analysis, then repeated similar regressions using only ego as the unit of analysis, and let social networks be represented by aggregate data. In both series of regressions we used five variables representing respondent attributes, adding a variable representing alter adoption – using either the proxy-reported or the self-reported variable – to measure social network effects. In addition, we added a dyad-specific variable representing whether ego and alter had discussed the technology in question in the series of regressions using dyads as the unit of analysis. Interaction terms between alter adoption and alter identity (family member, membership in information network and neighbor) were considered but found insignificant, so we do not report those results. The only two exceptions are mentioned in a note below.

Summary statistics for data used in the first set of regressions (based on observations of dyads) are presented in table 4. We used a smaller sample here than in the previous regressions, since we wanted to use variables representing respondent characteristics that were only available for primary respondents. This precluded using the snowball sample. Due to the near universal adoption of organic fertilizer application, that variable had to be omitted from regressions. We dropped all observations with missing variables, to ensure all regressions were identical, with the only exception being the one variable we substituted between replications.

Table 5 a) presents the results of regressing ego's technology adoption on alter's proxy-reported adoption (X^P), combined with various ego characteristics, and whether the two had discussed the technology in question. Table 5 b) presents regressions using the exact same variables, with the only difference that alter adoption is self-reported (X^S). Let us first quickly summarize the significant coefficient estimates on the respondent characteristics variables, which are statistically insignificantly different from one another whether one use the proxy-reported or self-reported value of alter adoption. Farmers operating more land are uniformly and significantly more likely to adopt each of the three technologies. Otherwise, age, gender, education and labor supply effects vary in sign and significance across the three technologies. As these effects are not our focus, the main point is that these estimates are unaffected by one's choice of alter adoption variable.

The differences arise purely with respect to the estimated social effects. In table 5 a) the coefficient estimate on the proxy-reported alter adoption variable is highly significant in all three regressions, suggesting that alter adoption has a significant and strong effect on ego's adoption decision (assuming no correlated or exogenous group effects). The effect appears to be strongest for terracing and *tumbukiza*. For the same two technologies, coefficient estimates on the "discuss" variable are also highly significant, but the effect is weaker than for alter adoption. These results suggest that respondents have adopted these practices after learning about them through their social networks. However, in table 5 b) coefficient estimates on the (self-reported) alter adoption variable is not significant in any of the three regressions, showing that actual behavior is not correlated within networks.[6] The fact that the self-reported alter adoption variable is consistently insignificant means we can rule out omitted variables bias due to unobserved, group-level correlates.

The χ^2 test statistic at the bottom of table 5 b) is a test of equality between coefficient estimates on self-and proxy-reported peer behavior in a model that includes both variables separately, but otherwise is identical to those reported in the table. For terracing, the test statistic does not permit rejection of the null of equal coefficients on the self-and proxy-reported variables; but for fallowing and *tumbukiza* we do reject that null, very strongly for the latter. That means the difference (the bias) is statistically significant for the technologies where it is most likely that respondents are actually monitoring peer behavior relatively closely, and proxy-reporting is most accurate. This is the opposite of our finding in table 1, and demonstrates that a small aggregate bias can conceal biases that are highly significant and correlated with other variables likely to be included as explanatory variables in regressions.

In the final series of regressions we present here, we let either the number of adopters among sampled social network contacts, or their share of the total number of sampled network contacts (both self- and proxy-reported), represent alter behavior. Thus, we considered the collective influence of social network contacts on each respondent, who was the unit of analysis. Descriptive statistics for the data used in these regressions are presented in table 6 and results of the regressions in table 7a-b). The combination of a small sample and near universal adoption of the two practices, terracing and organic fertilizer application, made using these practices in regressions impossible. Therefore, only two practices are included in this series of regressions, fallowing and *tumbukiza*.

The variables representing gender, age, and farm size are weakly significant in some of the regressions, with the expected signs. But the strongest effects are associated with alter adoption, but *only when alter adoption is proxy-reported*. This confirms our findings in the regressions above, where the pattern is exactly the same.

Discussion

Interpretation of these coefficient estimates benefits from knowing the villages where our data were collected quite well, so we have a relatively good understanding of the technologies under study, and of where the study sites stand regarding the process of their adoption. Thus, in our view, for terracing and fallowing it is very unlikely that social learning is going on, since they are well-established, traditional practices that have long since reached their plateau level of adoption. Instead, for these two practices adoption is more likely a result of respondent attributes, including characteristics of the respondent's household and land holding.

The insignificant coefficient estimates on self-reported peer behavior for these technologies in regressions are consistent with our prior expectations, while the positive and significant coefficient estimates on proxy-reported peer behavior variables are clearly a result of measurement error, which is most likely due to projection bias in proxy-reporting. However, analysis of the accuracy of proxy-reporting suggest that sample respondents are better informed about individual-level peer behavior regarding fallowing than terracing, suggesting that knowledge about idle land, i.e., land in fallow, is interesting to sample respondents for some reason that is unknown to us.

For *tumbukiza* social learning may be a reasonable interpretation for a positive and significant coefficient estimate on the peer behavior variable, since this practice is truly a novelty in the study sites, and there is reason to believe there is social learning going on regarding this practice in the study sites. This practice was included in the survey because it is being promoted as a new innovation by the extension service, and we expected to find that farmers were engaged in active social learning about it. And analysis of the accuracy of proxy-reporting showed that respondents were slightly better informed about individual-level peer behavior regarding this technology than the others in the study. We interpret this finding as evidence that some active social learning may be occurring among sample respondents. But this is the only result that supports the social learning hypothesis - results from regressions follow the exact same pattern for *tumbukiza* as for the other two technologies, so the regressions do not support a social learning hypothesis even for *tumbukiza*.

Summary and Conclusions

We have found that proxy-reporting is less biased and more reliable regarding the adoption of the two NRM practices, fallowing and *tumbukiza,* than for the other two practices under study. The latter is consistent with active social learning about a new and little known innovation, while the former is an indication that respondents have a reason (social learning is probably not that reason) why they are interested in the prevalence of fallowing among their social network members. But the active social learning hypothesis is not supported by the regression analyses we also performed.

Although proxy-reporting of peers' adoption of these two practices appears to be unbiased in the aggregate, errors in proxy-reporting are not random, but rather significantly correlated with respondent attributes. We also found that proxy-reporting is biased in a manner that is consistent with projection bias, or false consensus bias (Ross, Greene, and House 1977). Whether they are adopters or not, respondents appear to believe their social network members tend to do like themselves.

When we used proxy- and self-reported information about peers' adoption behavior in regressions, we found that the proxy-reported variable was significant in all of the regressions. The typical interpretation of such results is that respondents' behavior is influenced by the behavior of their social network members. This is taken as evidence of social learning. However, when self-reported variables were used in otherwise identical regressions, they were consistently not significant, suggesting that the adoption behavior of social network contacts has *no* effect on respondents' adoption decisions. In fact, adoption behavior is not correlated within these networks. An alternative and congruent interpretation of the results for proxy-reported peer behavior may be that respondents' behavior is influenced by their *beliefs* about alter behavior, but in our view that is also unlikely. Our interpretation of this result is that proxy-reported peer behavior reflects that respondents believe their social network members behave as they do themselves, i.e., they project their own behavior on others.

Based on this result, we suspect that in some cases when studies have found evidence of passive social learning, what was really going on was respondents producing proxy-reported data about peer behavior that was influenced by projection bias. Inferences made about passive social learning may have been mistaken, and may have influenced policy design, leading to poorly designed technology dissemination programs that did not ensure that information would reach passive learners.

Notes

1. Analysis of network formation in our data has shown that networks are stratified by productive assets, i.e., assortative matching (Hogset 2005). According to Brock and Durlauf's (2007) Propositions 9-10, with non-random assignment to groups and assortative matching, partial identification is achieved.

2. Within each household, we interviewed the person who had the main responsibility for day-to-day farm management decisions. In many households both spouses were farm managers, either working together, or having separate enterprises. Therefore, about one tenth of the sampled households were selected for interviews with both spouses, selected among those where both spouses were farm managers.

3. Eleven secondary respondents who were members of primary respondents' households were retained in the data used in regressions on dyads, since the household data used here also cover these individuals.

4. We used a relatively broad definition of organic fertilizers: farmyard manure, compost, and mulching using organic matter collected on or off-farm.

5. If we omit the variable reflecting whether ego and alter have discussed the technology, the estimated reduction of false negatives and increase in false positives associated with ego's own adoption choice become greater in magnitude and statistical significance. These separate regression results are available from the authors by request.

6. In regressions not reported here, the coefficient estimate on an interaction term between alter adoption of fallowing (self-reported) and alter being a neighbor, was positive and weakly significant, as was the coefficient estimate on an interaction term between alter adoption of *tumbukiza* (self-reported again) and alter being a family member. This suggests that there may be selective, weak social learning among geographic neighbors and family, although these results could also be due to unobserved correlated effects.

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Table 1. Bias and reliability ratios of proxy-reporting compared with self-reporting

Measure	**Terracing**	**Fallowing**	**Organic fertilizers**	***Tumbukiza***
$E[X^P]$	0.839	0.294	0.924	0.272
$E[X^S]$	0.886	0.267	0.961	0.277
$E[X^P*X^S]$	0.749	0.098	0.892	0.096
$E[X^P – X^S]$	-0.047***	0.027**	-0.037***	-0.005
	(0.009)	(0.013)	(0.006)	(0.013)
Reliability Ratio	0.051	0.098	0.080	0.102
Number of observations	1196	1175	1189	1146

Standard errors in parentheses

*, **, *** statistically significant difference at 10%, 5% and 1% level, respectively

Table 2. Descriptive statistics for snowball sample

Variable	# obs	Mean	Std. Dev.
Ego is female	1203	0.43	0.50
Alter is a family member	1203	0.23	0.42
Alter is a member of ego's information network	1203	0.37	0.48
Alter is a neighbor	1203	0.47	0.50
Alter is a public employee	1203	0.04	0.20
Respondent (ego) practices/uses			
Terracing	1189	0.91	0.29
Fallowing	1173	0.27	0.45
Organic fertilizers	1164	0.95	0.21
Tumbukiza	1144	0.28	0.45
Ego and alter have discussed the technology			
Terracing	1189	0.80	0.40
Fallowing	1173	0.37	0.48
Organic fertilizers	1164	0.84	0.37
Tumbukiza	1144	0.31	0.46

Table 3. Patterns in proxy-reporting errors.

(Multinomial probit estimation. No error is the base outcome).

a) Easy-to-observe	**Terracing**		**Fallowing**	
Error	**Negative**	**Positive**	**Negative**	**Positive**
Ego is female	0.190	0.213	0.033	0.031
	(0.157)	(0.196)	(0.158)	(0.169)
Alter is a family member	-0.010	-0.161	-0.212	-0.010
	(0.178)	(0.235)	(0.194)	(0.175)
Alter is a member of ego's	-0.304	0.041	0.326*	-0.036
information network	(0.188)	(0.211)	(0.173)	(0.174)
Alter is a neighbor	-0.263	0.058	-0.033	-0.327**
	(0.165)	(0.200)	(0.158)	(0.165)
Alter is a public employee	-0.319	-1.538***	0.121	-0.445
	(0.398)	(0.532)	(0.495)	(0.357)
Ego has adopted technology	-0.592**	-0.150	0.228	0.833***
	(0.272)	(0.286)	(0.194)	(0.170)
Ego and alter have discussed	-1.217***	0.096	-0.298*	0.994***
technology	(0.184)	(0.217)	(0.175)	(0.164)
Embu site	0.465***	0.932***	-0.513***	-0.617***
	(0.176)	(0.196)	(0.169)	(0.162)
Constant	-0.273	-2.391***	-0.765***	-1.134***
	(0.330)	(0.459)	(0.219)	(0.210)
Number of observations	1189		1173	
Wald $\chi^2(16)$	122.76***		117.88***	

b) Difficult-to-observe	**Organic fertilizers**		***Tumbukiza***	
Error	**Negative**	**Positive**	**Negative**	**Positive**
Ego is female	0.239	0.143	0.128	0.463***
	(0.208)	(0.262)	(0.167)	(0.176)
Alter is a family member	0.113	0.235	0.162	0.022
	(0.222)	(0.352)	(0.208)	(0.205)
Alter is a member of ego's	-0.360	0.411	-0.029	0.160
information network	(0.227)	(0.308)	(0.184)	(0.193)
Alter is a neighbor	0.123	0.477	-0.427***	-0.022
	(0.226)	(0.290)	(0.162)	(0.175)
Alter is a public employee	-0.752	-0.705	-0.651	1.249***
	(0.672)	(0.571)	(0.445)	(0.285)
Ego has adopted technology	-0.248	0.122	-0.047	0.696***
	(0.454)	(0.594)	(0.219)	(0.192)
Ego and alter have discussed	-1.638***	-0.163	-0.858***	1.375***
technology	(0.241)	(0.285)	(0.210)	(0.185)
Embu site	0.868***	-0.452*	1.520***	1.198***
	(0.295)	(0.265)	(0.197)	(0.253)
Constant	-1.439***	-2.697***	-1.796***	-3.170***
	(0.489)	(0.835)	(0.211)	(0.285)
Number of observations	1164		1144	
Wald χ^2 (16)	115.03***		302.01***	

Robust standard errors in parentheses

*, **, *** statistically significant difference at 10%, 5% and 1% level, respectively

Table 4. Descriptive statistics for primary respondents (446 observations)

a) Binary variables (0 = No, 1 = Yes)

Variable	**Mean**	**Std. Dev.**	**Variable**	**Mean**	**Std. Dev.**
Ego is female	0.53	0.50	Ego has not completed primary school	0.20	0.40
Respondent (ego) practices/uses			Ego and alter have discussed		
Terracing	0.95	0.22	Terracing	0.84	0.37
Fallowing	0.34	0.47	Fallowing	0.35	0.48
Tumbukiza	0.25	0.43	*Tumbukiza*	0.31	0.46
Alter practices/uses (proxy-reported)			Alter practices/uses (self-reported)		
Terracing	0.79	0.41	Terracing	0.89	0.31
Fallowing	0.30	0.46	Fallowing	0.29	0.45
Tumbukiza	0.18	0.38	*Tumbukiza*	0.30	0.46

b) Continuous variables

Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Ego's age	53.59	14.88	19	80
Household size	5.29	2.11	1	12
Area of cultivated land (acres)	2.27	2.63	0.2	15

Table 5. NRM adoption and peer behavior, proxy-or self-reported

(Probit estimation, 446 observations)

a) Proxy-reported peer behavior	**Terracing**	**Fallowing**	***Tumbukiza***
Ego is female	0.402*	0.164	-0.015
	(0.206)	(0.170)	(0.174)
Ego's age	0.008	0.021***	-0.027***
	(0.007)	(0.006)	(0.005)
Not completed primary school	-1.129***	-0.495***	0.746***
	(0.228)	(0.192)	(0.211)
Household size	0.062	0.057	-0.149***
	(0.048)	(0.036)	(0.034)
Area of cultivated land (acres)	0.028	0.221***	0.206***
	(0.031)	(0.042)	(0.033)
Ego and alter have discussed	0.966***	0.091	0.565***
the technology	(0.298)	(0.194)	(0.186)
Alter has adopted the technology	0.864***	0.539***	1.048***
(X^S)	(0.289)	(0.205)	(0.217)
Constant	-0.213	-2.471***	0.413
	(0.614)	(0.495)	(0.428)
Wald $\chi^2(7)$ test statistic	58.80***	48.32***	179.33***

Robust standard errors in parentheses

*, **, *** statistically significant difference at 10%, 5% and 1% level, respectively

b) Self-reported peer behavior	**Terracing**	**Fallowing**	***Tumbukiza***
Ego is female	0.471**	0.141	-0.040
	(0.197)	(0.166)	(0.176)
Ego's age	0.012*	0.021***	-0.025***
	(0.006)	(0.006)	(0.006)
Not completed primary school	-0.927***	-0.528***	0.789***
	(0.206)	(0.189)	(0.206)
Household size	0.057	0.046	-0.147***
	(0.043)	(0.035)	(0.036)
Area of cultivated land (acres)	0.013	0.218***	0.199***
	(0.027)	(0.042)	(0.036)
Ego and alter have discussed	1.314***	0.414***	1.125***
the technology	(0.238)	(0.155)	(0.172)
Alter has adopted the technology	0.287	0.063	0.119
(X^P)	(0.324)	(0.171)	(0.180)
Constant	-0.452	-2.337***	0.342
	(0.625)	(0.477)	(0.425)
Wald $\chi^2(7)$ test statistic	59.14***	45.94***	156.29***
Testing self-vs. proxy-reported adoption variables			
$\chi^2(1)$ test statistic	1.81	3.63*	10.62***

Robust standard errors in parentheses

*, **, *** statistically significant difference at 10%, 5% and 1% level, respectively

Table 6. Descriptive statistics for sub-sample (56 observations)

a) Binary variables (0 = No, 1 = Yes)

Variable	**Mean**	**Std. Dev.**
Ego is female	0.55	0.50
Not completed primary school	0.21	0.41
Respondent (ego) practices/uses		
Fallowing	0.34	0.48
Tumbukiza	0.21	0.41

b) Continuous variables

Variable	**Mean**	**Std. Dev.**	**Min**	**Max**
Ego's age	53.96	14.12	19	80
Household size	5.27	2.23	1	12
Number of adopters in network				
Fallowing (proxy-reported)	2.11	2.15	0	9
Tumbukiza (proxy-reported)	1.13	1.81	0	7
Fallowing (self-reported)	2.05	1.41	0	7
Tumbukiza (self-reported)	2.02	1.70	0	8
Share of adopters in network				
Fallowing (proxy-reported)	0.32	0.30	0	1
Tumbukiza (proxy-reported)	0.17	0.26	0	0.875
Fallowing (self-reported)	0.31	0.19	0	1
Tumbukiza (self-reported)	0.29	0.23	0	0.75

Table 7. NRM adoption and peer behavior, proxy or self-reported

(Probit estimation, 56 observations)

a) Fallowing	**Self-reported**		**Proxy-reported**	
	Number	**Share**	**Number**	**Share**
Ego is female	0.728*	0.723*	0.510	0.591
	(0.432)	(0.435)	(0.446)	(0.440)
Ego's age	0.029*	0.028*	0.030*	0.027
	(0.017)	(0.016)	(0.017)	(0.017)
Not completed primary school	-0.344	-0.319	-0.131	-0.175
	(0.527)	(0.519)	(0.529)	(0.541)
Household size	0.070	0.086	0.102	0.121
	(0.096)	(0.093)	(0.095)	(0.099)
Area of cultivated land (acres)	0.220*	0.230*	0.210*	0.234*
	(0.125)	(0.118)	(0.115)	(0.124)
Alter adoption	-0.192	0.334	0.212**	1.611**
	(0.169)	(1.041)	(0.096)	(0.715)
Constant	-2.852**	-3.376***	-3.826***	-3.921***
	(1.295)	(1.281)	(1.289)	(1.300)
LR $\chi^2(6)$ test statistic	14.46**	13.13**	18.32***	18.46***

Robust standard errors in parentheses

*, **, *** statistically significant difference at 10%, 5% and 1% level, respectively

b) *Tumbukiza*	**Self-reported**		**Proxy-reported**	
	Number	**Share**	**Number**	**Share**
Ego is female	-0.031	-0.048	-0.007	0.010
	(0.457)	(0.454)	(0.490)	(0.506)
Ego's age	-0.025	-0.027	-0.028	-0.033
	(0.019)	(0.019)	(0.020)	(0.021)
Not completed primary school	0.651	0.634	0.631	0.607
	(0.581)	(0.616)	(0.633)	(0.663)
Household size	-0.102	-0.111	-0.144	-0.157
	(0.099)	(0.098)	(0.115)	(0.122)
Area of cultivated land (acres)	0.178	0.185	0.171*	0.188*
	(0.124)	(0.130)	(0.100)	(0.107)
Alter adoption	0.118	0.715	0.305***	2.562***
	(0.137)	(1.226)	(0.113)	(0.871)
Constant	0.187	0.400	0.358	0.563
	(1.248)	(1.196)	(1.222)	(1.245)
LR $\chi^2(6)$ test statistic	11.96*	11.54*	19.16***	21.57***

Robust standard errors in parentheses

*, **, *** statistically significant difference at 10%, 5% and 1% level, respectively

		Ease of Observation	
		Easy	**Difficult**
Prevalence	**Widespread**	Terracing	Organic fertilizer application
	Limited	Fallowing	*Tumbukiza*

Figure 1: Four natural resources management practices