Local or outsider interviewer? An experimental evaluation.

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ABSTRACT

Matching of interviewers and respondents along demographic characteristics is a popular practice in survey research. Interviewer-respondent matching is further enhanced in survey endeavors where interviewers are recruited from the community being studied. The effects of employing local as opposed to outsider interviewers have never been experimentally evaluated. In this paper we report results from a survey experiment that we conducted in the Dominican Republic to measure these effects. Respondents were significantly more likely to report contraceptive use to outsiders, who also recorded significantly higher amounts of remittances received by the household as well as significantly higher outflows (monetary and in-kind) from the household. Respondents were also more likely to tell outsider interviewers that they had heard of presumably famous, but fictitious, people, and presented themselves as more tolerant of stigmatized groups when interviewed by outsiders. We explore possible mechanisms leading to these differences, and believe that misreporting affects both interviewer types, depending on the type of question.

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Background

The relationship between interviewers and respondents has long been a topic of concern in the survey literature. The longstanding dominant view is that there should be no pre-existing relationship between interviewer and respondent – that is, no relationship that predates the interview interaction. This dominant view is driven by fears that pre-existing relationships could generate response bias. Therefore, standard survey interviewing practice ensures that the interviewer is a stranger to the respondent. This practice is so entrenched in the survey ethos that methodology manuals rarely bother to make the point explicit, i.e., there are no statements calling to "make sure your interviewers do not know your respondents" in survey interviewing manuals. Some methodologists explicitly caution, however, against excess familiarity, or "over-rapport" (Miller 1952). Elsewhere we have referred to this principle as the "stranger-interviewer norm" (Weinreb, Sana & Stecklov 2011).

This practice is at odds with strategies and practices of ethnographic fieldwork where much effort is spent to localize or contextualize the researcher. The ethnographic approach, in fact, is premised on the idea that ethnographers begin their work by moving "as strangers into a small and culturally alien community" (Stocking 1983:7) with the goal of ultimately achieving "the status of an intimate, an insider" (Agar 1980:59). Even within standard survey practice, the stranger-interviewer norm seems somehow at odds with an increasingly prevalent view that favors interviewer-respondent matching along certain demographic characteristics such as gender or ethnicity (Schaeffer, Dykema, and Maynard 2010). There is also increasing use of interviewers who have preexisting relationships to the community, if not to the respondents themselves (e.g. Marcelli et al 2009a, 2009b, Parrado, McQuiston & Flippen 2005), an approach that responds to findings from Josephson (1970), who reported high levels of community resistance to Columbia University researchers (and their staff) who were detached from the community under study (in Harlem) and were perceived as unwanted intruders. Each of these slight deviations from the stranger-interviewer norm is premised on the idea that reducing social distance between interviewer and respondent can also improve the validity of survey data. In essence, the ultimate purpose of this approach is to induce the respondent to feel that the interviewer is less of a stranger. In combination with the stranger-interviewing norm, the practice seems to call for an interviewer who must be a stranger while, to the extent possible, appearing not to be so. The underlying epistemological tension is obvious. If respondents must feel that the interviewer is not a stranger, there must be some advantage in the interviewer not being a stranger.

Attempts to reduce social distance between interviewer and respondent in social surveys seem, in recent years, to have been maximized with the adoption of community-based survey research (CBSR), sometimes referred to as community-based participatory research especially if it involves other methods of data collection beyond surveys. In this approach, the surveyed population is geographically concentrated and fairly homogeneous. The researchers establish a working relationship with community-based organizations in a way that maximizes the benefits for both the research and the community. Since tangible or easily measurable benefits for the community make its participation in CBSR more likely, it is not surprising that most research using CBSR has been related to health. In developed societies, CBSR appears to be particularly apt to reach ethnic minorities or immigrant populations (e.g. Marcelli et al. 2009a 2009b; Parrado, McQuiston & Flippen 2005).

A common denominator undergirding CBSR studies is the employment of members of the community as interviewers. In fact, many studies that would not qualify as CBSR (for lacking any meaningful involvement of community organizations) have also employed members of the community under study as interviewers. Since these are largely inexperienced in the task at hand, training them to properly follow interview protocols raises a number of challenges. This appears to be the most recognizable challenge of CBSR and the employment of community interviewers in general. According to its advocates, however, CBSR advantages are more significant. They include the identification and correction of problems with the survey design at an early stage of the research process, access to otherwise difficult-to-reach populations, presumed improvements in the validity of survey responses due to higher trust in the interviewers, and a higher likelihood that the research has direct benefits for the subjects in the study or the members of the community.

In any event, there has been little if any experimental evaluation of the presumed advantages of employing members of the community as interviewers in social surveys. Instead, these have been largely assumed. The only study of which we are aware that has compared the performance of interviewers who were members of the community under study (or "indigenous" interviewers) with that of professional outsider interviewers was reported by Holbrook, Farrar and Popkin (2006) and involved a survey of a Chicago public housing development. Overall, Holbrook and colleagues found that indigenous interviewers neither increased survey participation nor obtained more honest responses than outsider interviewers. In fact, when there was a significant difference concerning sensitive questions it was the outsider interviewers who appeared to have elicited more honesty from the respondents. The authors speculate that greater social distance (between the respondents and the outsider interviewers as opposed to indigenous interviewers) may have actually been a favorable factor in this case. Of course, assuming this conclusion to be accurate, which is difficult to tell since the assignment of interviewers to respondents was not randomized as it would be in a true experiment, it is far from clear that the effects of the locality of interviewers would be similar in a public housing development in Chicago and other settings in the United States or elsewhere. There is in fact good reason to expect that the willingness to accurately report behavior and attitudes to locals and outsiders will vary across societies. Respondents in developing countries, particularly in rural or provincial settings where relations tend to be more dense and less socially and economically differentiated, might tend to be more suspicious of outsiders than those in more developed societies.

Research question

In this paper we test the proposition that the employment of members of the community as interviewers in survey research improves the quality of the data when compared to data collected by outsider interviewers. We do so by means of a methodological experiment. In earlier evidence presented at the PAA using a different (and partially overlapping) component of this same data collection project (Weinreb, Sana and Stecklov 2011), we found mixed evidence in support and against the stranger-interviewer norm. The present analysis also shows mixed results, in this case in support or against the use of locals as interviewers.

Data

We report results from an NIH-funded methodological experiment that we conducted in the summer of 2010 in a provincial town of the Dominican Republic that we will call San Benito. We hired six experienced interviewers from the capital city of Santo Domingo and brought them to San Benito, where we also recruited 24 locals for the task after two selection stages. In a first stage, 32 out of 64 applicants were selected, based on their credentials, individual interviews with the field supervisors, and a brief evaluation of their knowledge of the town and their ability to read maps. After four days of training (which also included the interviewers from Santo Domingo), the prospective local interviewers were given a test. Based on the test results, 24 were hired. The local interviewers ranged in age from 18 to 37, with a mean of 24.04. The outsider interviewers were 28 to 45 years old, with a mean of 33.50. Since the hiring of the locals followed a protocol aimed at choosing those of highest ability, and the hiring of the outsiders put a premium in hiring interviewers with substantial prior experience in survey research, the age distribution of the two groups were bound to be different. Possible implications for the analysis will be discussed below.

All respondents were women in the 20-50 age range. The questionnaire instrument included questions on an array of topics typical of social surveys in developing countries, such as demographic information, household composition, property ownership, income, remittances, family planning, and child vaccinations. In addition, we specifically designed questions on self-representation, tolerance, and other topics, aimed at capturing social desirability bias.

The experimental design ensured that our local interviewers were assigned to both respondents whom they personally knew and respondents with whom they had no prior relationship. For the present analysis we only include the latter interviews, in addition to all interviews conducted by the outsider, experienced interviewers from Santo Domingo. The appendix provides more detail on the experimental design, useful to understand the differences between the sample used in this paper and that used in Weinreb et al. (2011), where the goal was to explicitly test the stranger-interviewer norm.

Analytical approach

As a first step, we conduct t-tests of differences of means where the two groups are respondents interviewed by locals and those interviewed by outsiders. Considering that 24 of our 30 interviewers were novices, interviewer effects are to be expected. We compute intraclass correlation coefficients (ICCs) from simple variance component models to examine possible interviewer effects. Based on the results, we graphically explore to what extent age of the interviewer may have been a factor affecting results. Our next step is to present results from regression analyses.

Our respondents are clustered within interviewers and failure to control for this clustering can lead to misleading conclusions regarding the effects of interviewer characteristics (Daily & Claus 2001, Schaeffer et al. 2010). This calls for either multilevel models (where level-2 is interviewers) or for regression models that correct for clustering. Sample size proved to be problematic when running multilevel models leading, for example, to very near-zero between-interviewer variances in some cases. Likelihood ratio tests suggested that in at least half of our models a regular regression with correction for clustering

offered a better fit. In addition, when we ran both multilevel and regular models on the same variables, results were qualitatively similar. In the interest of simplicity, therefore, we only report results from regular models adjusted for clustering of respondents within interviewers. The main focus of these models is to evaluate the effect of local as opposed to outsider interviewer on an array of key variables, subject to standard controls, including age of the interviewer.

Results

Initial look: t-tests

For most of the questions we asked, differences in the means of responses given to local and outsider interviewers did not differ significantly. Refusal rates were quite small (1.94% for outsiders, 2.91% for locals) and not statistically significant. We limit our analysis below to variables where we found some possible effects of type of interviewer.

Table 1 shows results from t-tests for the difference in means between the two groups of interviewers, as well as ICCs. Variables are grouped into those referring to property ownership, contraception, vaccination of children, household income, receipt of remittances from abroad, household outflows (estimated total value of outflows in currency and in-kind), self-representation and tolerant views.

The t-tests show that respondents were more likely to report owning the home where they lived to outsider interviewers, but more likely to report owning other property to local interviewers. Results for use of contraception are striking. Respondents reported 66% contraceptive use to outsiders, but only 44% to locals. This was largely an effect of respondents married or in unions reporting much higher contraceptive use to outsiders: 82% vs. 50%. The former figure is even higher than the 72.9% recorded for women in unions in the country as a whole by the 2007 Demographic and Health Survey (CESDEM & Macro International 2008). (The same DHS recorded 54.0% among all women aged 15-49 and 61.6% among all sexually active women of that age.) However, it is close to the figure reported for women in unions in the province where San Benito is located—in fact, one of the highest contraceptive prevalence rates in the country. (We are not disclosing the actual figure so as to conceal the location of San Benito.) Reports on child vaccination show that outsider interviewers get higher vaccination reports on the measles vaccine. This difference is significant but small sample size (few children lack the vaccine) might be problematic. (The effect disappears in the regression models shown below.)

Differences in household income are significant, with respondents reporting higher income to outsiders, but only in the logged specification of income. Differences in the amount of remittances received[1], with those interviewed by outsiders reporting much higher amounts, are significant both when the sample is restricted to remittance recipients and when zeroes are included. The same is true for outflows, where again amounts reported to outsiders are higher.

We included a few questions to capture the respondent's presentation of self, including a series of questions that asked the respondent whether she had heard of specific "famous" people. While the list

[1] There is no significant difference between local and outsider interviewers in the percent of households that report receiving remittances: 25.44 and 23.14 percent, respectively.

did indeed include famous persons, it also included two made-up names. In both cases, respondents talking to outsider interviewers were significantly more likely to report that they had heard of these fictitious people.

Finally, we also report results from the three variables addressing tolerance. While the overall level of tolerance was rather low, respondents were consistently more likely to report tolerant views on homosexuals, Haitians[2], and prostitutes to outsider interviewers than to local interviewers. The table shows the t-test and results for an additive index variable where higher values indicate more tolerant views and where, again, respondents appear to be significantly more tolerant when interviewed by outsiders.

Intraclass Correlation Coefficients

The intraclass correlation coefficient (ICC), here calculated as the proportion of the residual variance accounted for the between-group variance in simple variance components models without covariates, is often used to explore the possible presence of group-specific effects, in this case interviewer effects. Since respondents and interviewers were systematically randomized, there is no reason to expect real differences between respondents interviewed by different interviewers. In other words, a reasonable expectation is to find ICCs close to zero. As the ICC begins to rise, questions about interviewer effects emerge.

The rightmost columns of Table 1 show ICCs calculated separately for local and outsider interviewers. Since all the local interviewers were inexperienced, we would expect to find more variability among responses collected by local than outsider interviewers. For most variables under analysis, this expectation is confirmed by larger ICCs for locals. All ICCs computed for outsider interviewers seem reasonable, with some of those computed for local interviewers appearing fairly high. In particular, we suspect interviewer effects on the data on contraceptive use and remittances. We now take a closer look at these variables.

Contraceptive Use and Remittances

Figure 1 plots the percent of women (married or in union) using contraception, by interviewer and age of the interviewer. Taken together, the data seem to indicate that respondents are more likely to disclose contraceptive use to older interviewers, but it also appears that respondents are more likely to disclose contraceptive use to outsider interviewers. Within group, and in particular if we consider only the outsider interviewers, an effect of age is not fully clear. In fact, the coefficient for age of the interviewer is not significant in the models that we show below, but this does not solve the ultimate restriction that there are no outsider interviewers younger than 28 and only three (out of 24) local interviewers over 30.

The problem is similar in the case of remittances received by the household, shown in Figure 2—limited to those interviewers who elicited at least two responses on remittance amount. There seems to be an

[2] Widespread racism combined with the visibility of Haitian labor migrants has long made Haitians the target of animosity among a substantial proportion of Dominicans.

effect of age and there seems to be an effect of interviewer type. Again, data availability is limited by age of the interviewer. Yet, age of interviewer will in fact be nonsignificant in the regression models below, suggesting that type of interviewer is more substantial to explain differences in reported remittance levels.

Consistent with what we would expect based on the ICCs, average responses collected by local respondents, on these two variables, vary wildly—perhaps more strikingly on contraceptive use. The highest ICC reported on Table 2 corresponded to remittance amounts when zeroes are included, that is, when the sample includes all those who reported that they receive remittances rather than only those who disclosed the amount. A substantial minority of respondents who admitted remittance receipt did not disclose the amount received: about 40% of those interviewed by locals and about 30% of those interviewed by outsiders, a difference that was not statistically significant (not shown). The large ICC for the variable that includes those who did not disclose the amount results from, again, pronounced variability among local interviewers in their success eliciting responses. Figure 3 illustrates this point.

Statistical Regressions

Table 2 reports the coefficient for a dummy variable for local interviewer in regressions predicting the same array of variables used in Table 1. Models vary: OLS for continuous variables, Tobit for remittances and outflows when they include the left-censored cases[3], and Logit for dichotomous dependent variables. In addition to type of interviewer, the independent variables include age of the interviewer, age and education of the respondent, household income (except for the regression on income itself), and whether the household was in the claimed sample (see Appendix).

Results concerning contraception, remittances, outflows and tolerant views are generally statistically significant, in the same direction as in the t-tests. The coefficient for local interviewer is marginally significant in the regressions on home ownership, self representation and some specifications of tolerant views and remittances, and it is not statistically significant for the remaining variables. As advanced above, age of the interviewer, included among the controls, was not significant in the regressions on contraceptive use and remittances.

Interactions

In order to explore possible mechanisms for the differences that we found, we re-ran the statistical models testing interactions of type of interviewer with income, education, and age of interviewer. In virtually all cases the interaction terms were inconsequential, but Table 3 reports results from two models where the interaction in question was statistically significant or marginally significant.

As income increases respondents report lower amounts of both remittances and outflows to local than to outsider interviewers. Or, rather, they report increasingly larger amounts to outsiders while not varying much their responses to local interviewers. Figures 4 and 5 illustrate the predictions.

[3] For remittances, as explained above, and for outflows in a similar way: those who reported that their households send currency or goods to other households but did not disclose the amount were coded as zeroes.

Considering that the average household income in the sample is approximately 15,300 Dominican pesos, the results are noteworthy.

Discussion

Outsider interviewers were more often told that the respondents own the home where they lived. The difference is statistically significant, but the magnitude of the difference is rather moderate: 65% vs. 58%. If the difference is not due to chance (and it might likely be, as in the regression model the coefficient for local is marginally significant), we suspect that some respondents chose to represent themselves as successful home owners to outsiders. We believe that the opposite bias is less likely, as neighbors and other locals are likely to know who owns their homes, a fact that might dissuade the respondent from lying to a local interviewer.

Outsider interviewers recorded, beyond doubt, higher reports on contraceptive use than local interviewers: 66% versus 44% among all women in the sample. Among married women and women in unions, the gap was 82% versus 50%, with no significant difference among women not married or in unions. The current estimate of contraceptive use for women of the same age as our respondents who are married or in unions in the Dominican Republic is 73%. The statistically significant difference persists after key controls are included in regression models. Our initial suspicion is that there are two biases at work. The first is that respondents are overreporting contraceptive use to the outsider interviewers. This could be motivated by a widely publicized family planning campaign run by the national government—although this campaign is mainly intended to eliminate unwanted teen pregnancies. It is possible that, with our outsider interviewers being from Santo Domingo (a fact explicitly disclosed at the beginning of the interview during the interviewer's self introduction) some of the respondents thought it to be a good idea to report behavior consistent with that promoted by the central government. On the other hand, we also suspect respondents are underreporting contraceptive use to local interviewers, perhaps in consonance with conservative social views at the local level and for fear of gossip. In addition, we uncovered that respondents tend to report lower contraceptive use to younger interviewers, or at least to *some* younger interviewers. Since no outsider interviewer was too young, it is prudent to leave it as an open question whether age of the interviewer might genuinely affect responses, rather than their status of locals—the fact that the regression model fails to compute statistical significance for the age of the interviewer might be an artifice of the quasi-separation of data concerning age of the interviewer.

Interestingly, there are no significant differences in income reported to local and outsider interviewers once controls are included in the regressions, but remittance amount does seem to be a sensitive issue that produces different responses. Respondents appear to be underreporting remittance amounts, and by a large margin, when interviewed by locals. In addition, they seem to underreport more as their income increases, so that predicted values of remittances among those interviewed by locals do not vary much by income, while predicted remittances rise markedly with income when the interviewer is an outsider. We found the same pattern concerning outflows. In brief, respondents disclose both smaller inflows (of remittances) and smaller outflows when the interviewer is local.

Results on contraception, remittances and household outflows suggest that there are limits to the presumed greater trust that can be generated by interviewers who are not complete strangers—in that they are, in this case, residents of the same provincial town. Questions remain on whether age of the interviewer is a major factor, in particular concerning contraception, as San Benito is a relatively conservative, traditional setting in the Dominican countryside. Fear of gossip is a likely culprit when it comes to the difference in data reported on financial flows.

Judging from results on the questions on famous people and tolerance, in addition to some possible overreporting of contraceptive behavior to outsider interviewers, social desirability bias seems to operate in favor of local interviewers—that is, greater bias when the interviewer is an outsider. Thus, respondents clearly lied more to outsiders when there was potential for embarrassment from not having heard of presumably famous people. In addition, respondents showed themselves as significantly more tolerant when interviewed by the outsiders from Santo Domingo. We suspect that in both cases respondents chose to present themselves as more cosmopolitan and more attuned with the preferences of the residents of the capital city, represented by the interviewers with whom they were talking.

We must also note that for a large array of variables that would be likely candidates for differences by type of interviewer we found no such effect. Refusal rates were similar, and so were the proportions admitting receipt of remittances. A number of other questions that we asked to capture social desirability bias, related to abortion, religious views and illegal behaviors, showed no significant differences in responses given to local and outsider interviewers. Neither did questions on income, even when government assistance was included. Asking the respondents to show the interviewer their national IDs produced again no difference between locals and outsiders in respondent cooperation.

In short, for now, we think our results point in two opposite directions. When there are significant differences between data reported to local and to outsider interviewers (and in most cases there are not) outsider interviewers seem to receive more honest reports on some sensitive questions, while local interviewers seem to collect responses that are less prone to social desirability bias.

Our study improves on prior work in this area in that we collected our data in a systematic way to test interviewer effects, with proper random assignment of interviewers to respondents within each of the two strata. Prior evaluations of interviewer effects have typically been ex-post, as a bi-product of studies that pursued mainly nonmethodological goals. Because of our focus on one type of effect (locality) we eliminated variation in setting (all interviews were in the same town) and in gender of both the interviewer and the respondent. We acknowledge that there is some risk that our results are then too specific, and replication in other settings would be desirable.

Appendix

Our local interviewers were asked to list women eligible for the survey (aged 20-50) living in the community, and they were then asked to identify their houses. These houses became what we referred

to as the "claimed" stratum, with the rest of dwellings in the town being the "unclaimed" stratum. All the households in the claimed stratum were interviewed, together with a sample of households from the unclaimed stratum. Interviewer assignments to respondents in both claimed and unclaimed households were fully randomized. In the claimed stratum, about one-third of households were assigned to "insiders", that is, the interviewers who claimed them—when more than one interviewer had claimed one of these households, the interviewer was selected at random. Another third was assigned at random to local interviewers who had not claimed them. We refer to these as "local-strangers". The other third were randomized among the interviewers from Santo Domingo—the "outsiders". By design, the unclaimed stratum could not have insider interviews. Households in the unclaimed stratum were randomized among locals (necessarily local-strangers) and outsiders.

Table A1 below shows the different types of interviews given this design, clarifying the difference between the sample used here and the sample employed in the earlier paper (Weinreb et al. 2011). Because the goal in that paper was to evaluate different levels of insiderness, and in particular the stranger-interviewer norm, that analysis focused on the claimed sample only. Interviews in the unclaimed sample were not included so as to avoid potentially significant selectivity issues. In the present paper we discard all insider interviews and we have no particular reason to discriminate between the claimed and unclaimed sample. In this case, all local-stranger and outsider interviews are included and result in a considerably larger sample size than Weinreb et al. (2011).

Table A1. Survey design: interview types

	Stratum	
Interviewer	claimed	unclaimed
local	insider #	
local	local-stranger # *	local-stranger *
outsider	outsider # *	outsider *

* Included in this paper's analysis. (N=986)

Included in Weinreb, Sana & Stecklov (2011) (N=509)

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Table 1. Means, t-tests, and Intraclass Correlation Coefficients from Variance Components Models

	local	outsider	N local	N outsider	p-value from ttest	ICC local	ICC outsider
PROPERTIES							
Home owned by household member	0.582	0.649	631	353	0.039	0.000	0.000
Household member owns a different property	0.201	0.139	628	353	0.015	0.023	0.026
CONTRACEPTION							
Currently using contraception	0.440	0.658	527	330	0.000	0.197	0.008
Currently using contraception, married or in union	0.501	0.823	373	209	0.000	0.228	0.067
VACCINATION OF CHILD No. 1							
Vaccinated against measles	0.910	0.987	122	76	0.027	0.000	0.000
HOUSEHOLD INCOME							
Excluding government assist. (DR pesos, monthly, logged)	9.058	9.222	504	320	0.020	0.037	0.018
Excluding government assist. (DR pesos, monthly, logged)*	9.020	9.213	498	319	0.004	0.031	0.014
REMITTANCES FROM ABROAD (disclosed amounts only)							
Total value in DR pesos, Jan - Jun 2010**	23,785	47,503	50	34	0.019	0.149	0.000
Total value in DR pesos, logged, Jan - Jun 2010**	9.475	9.986	50	34	0.080	0.083	0.000
REMITTANCES FROM ABROAD (zeroes included) (a)							
Total value in DR pesos, Jan - Jun 2010**	7,341	19,696	162	82	0.003	0.279	0.032
OUTFLOWS (if >0)							
Total value in DR pesos, Jan - Jun 2010***	5,024	8,005	136	79	0.015	0.036	0.000
Total value in DR pesos, logged, Jan - Jun 2010***	7.727	8.168	136	79	0.026	0.056	0.043
OUTFLOWS (zeroes included) (b)							
Total value in DR pesos, Jan - Jun 2010***	4,466	7,353	153	86	0.011	0.058	0.000
SELF-REPRESENTATION							
Has heard of Luis Comodoro	0.104	0.220	632	354	0.000	0.000	0.044
Has heard of Wilson Medina Gomez	0.187	0.347	631	352	0.000	0.028	0.030
TOLERANT VIEWS							
Positive about homosexuals	0.200	0.333	630	354	0.000	0.026	0.024
Positive about Haitians	0.191	0.288	629	354	0.000	0.072	0.038
Positive about prostitutes	0.115	0.186	627	354	0.002	0.033	0.015
Overall tolerance index (ranges from 0 to 3)	0.507	0.808	627	354	0.000	0.065	0.002

NOTE: ICCs calculated using xtmixed, separately for locals and outsiders, no covariates.

* Excluding those with monthly income in DR pesos >=100,000.

** Excluding outlier at $US72,000.

*** Excluding one outlier with outflows>100,000 DR pesos.

(a) It includes those who reported remittance receipt but did not disclose the amount.

(b) It includes those who reported outflows but did not disclose the value.

Table 2. Effect of local interviewer in models controlling for clustering, with selected covariates

	coeff. for local	p-value
PROPERTIES		
Home owned by household member	-0.375	0.082
Household member owns a different property	0.052	0.891
CONTRACEPTION		
Currently using contraception	-0.972	0.002
Currently using contraception, married or in union	-1.431	0.001
VACCINATION OF CHILD No. 1		
Vaccinated against measles	-0.972	0.337
HOUSEHOLD INCOME		
Excluding government assist. (DR pesos, monthly, logged)	-0.064	0.546
Excluding government assist. (DR pesos, monthly, logged)*	-0.110	0.248
REMITTANCES FROM ABROAD (disclosed amounts only)		
Total value in DR pesos, Jan - Jun 2010**	-38119	0.005
Total value in DR pesos, logged, Jan - Jun 2010**	-1.070	0.002
REMITTANCES FROM ABROAD (zeroes included) (a)		
Total value in DR pesos, Jan - Jun 2010**	-36024	0.101
OUTFLOWS (if >0)		
Total value in DR pesos, Jan - Jun 2010***	-4078	0.026
Total value in DR pesos, logged, Jan - Jun 2010***	-0.410	0.164
OUTFLOWS (zeroes included) (b)		
Total value in DR pesos, Jan - Jun 2010***	-3916	0.009
SELF-REPRESENTATION		
Has heard of Luis Comodoro	-0.595	0.073
Has heard of Wilson Medina Gomez	-0.474	0.084
TOLERANT VIEWS		
Positive about homosexuals	-0.633	0.039
Positive about Haitians	-0.293	0.449
Positive about prostitutes	-0.583	0.092
Overall tolerance index (ranges from 0 to 3)	-0.237	0.016

† OLS for continuous variables, tobit for remittances and outflows models including zeroes, logit for dichotomous variables. In addition to the dichotomy for local vs. outsider, predictors include age of interviewer, age and education of respondent, household income (except for the regression on log og income) and sample stratum.

* Excluding those with monthly income in DR pesos >=100,000.

** Excluding outlier at $US72,000.

*** Excluding one outlier with outflows>100,000 DR pesos.

(a) It includes those who reported remittance receipt but did not disclose the amount.

(b) It includes those who reported outflows but did not disclose the value.

Table 3. Models with Interactions, Remittances and Outflows

	Dependent Variable			
	Remittances		Outflows	
	OLS on Rem>0 (a)		OLS on outflows>0 (b)	
Variables	Coeff.	Robust Std. Error	Coeff.	Robust Std. Error
Local	3319.8	19926.5	-522.3	1688.3
Age of interviewer	-1643.7 *	594.4	-68.5	98.7
Age	-527.0	502.5	104.2 ^	59.3
Education	677.5	1726.6	503.2 **	107.6
Household income	2.5 ^	1.4	0.2 *	0.1
Local * income	-2.6 ^	1.3	-0.2 ^	0.1
Claimed sample	-1067.6	13468.3	1441.6	1129.1
Constant	74300.0 *	26409.0	-3780.7	2865.1
N	74		197	
R-sq or pseudo R-sq	0.229		0.237	

** $p \leq .01$

* $.01 < p \leq .05$

^ $.05 < p \leq .10$

(a) A Tobit model including those who reported receiving remittances but did not disclose the amount as left-censored observations produced no significant coefficients and the pseudo R-sq was 0.0054. Of a total sample size of 205, 131 cases in that model were left-censored.

(b) A Tobit model including those who reported giving away money or goods but did not disclose the value of these outflows (19 cases) produced approximately similar coefficients and standard errors and qualitatively similar p-values, with a pseudo R-sq of 0.0128.

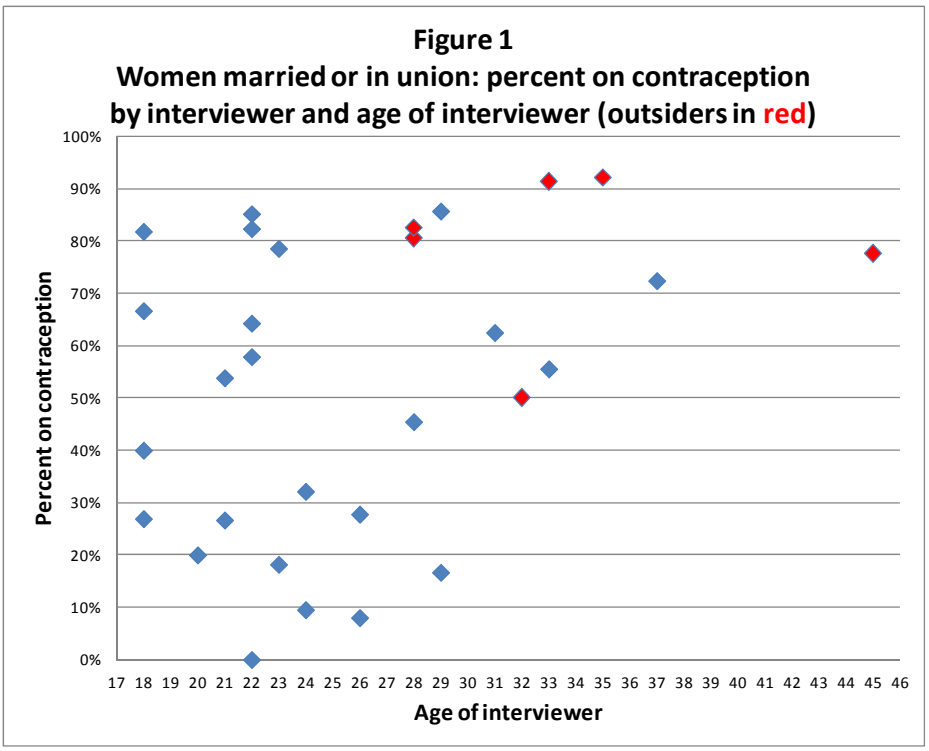
Figure 1
Women married or in union: percent on contraception
by interviewer and age of interviewer (outsiders in red)
Percent on contraception
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 35 36 37 38 39 40 41 42 43 44 45 46
Age of interviewer

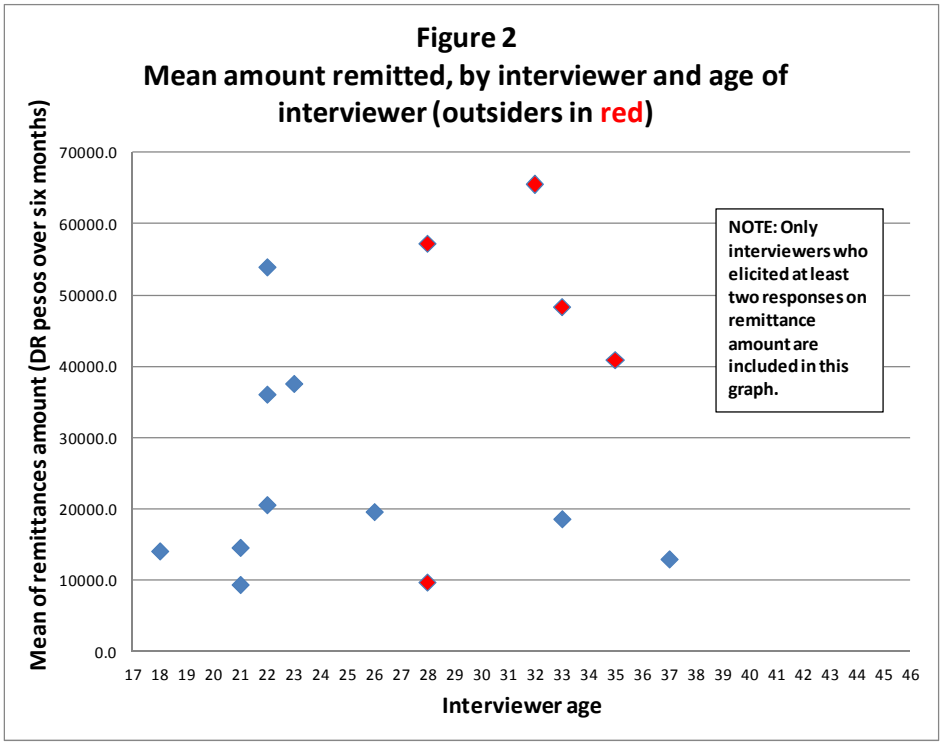
Figure 2
Mean amount remitted, by interviewer and age of
interviewer (outsiders in red)
Mean of remittances amount (DR pesos over six months)
70000.0
60000.0
50000.0
40000.0
30000.0
20000.0
10000.0
0.0
17 18 19 20 21 22 23 24 25 26 27 28 29 30 31 32 33 34 35 36 37 38 39 40 41 42 43 44 45 46
Interviewer age
NOTE: Only interviewers who elicited at least two responses on remittance amount are included in this graph.

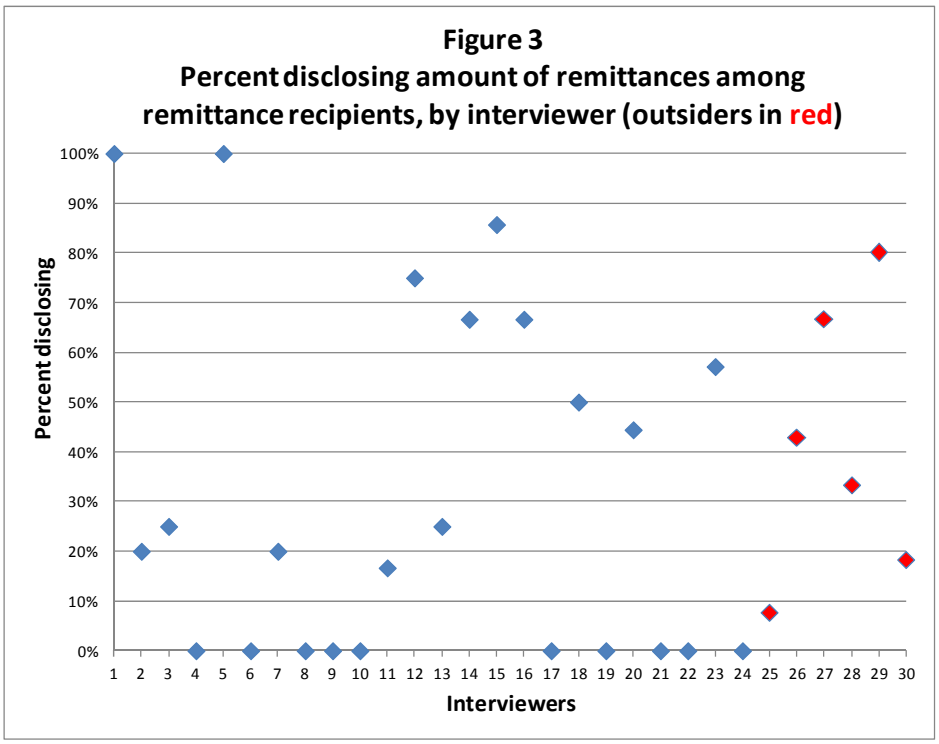
Figure 3
Percent disclosing amount of remittances among remittance recipients, by interviewer (outsiders in red)
Percent disclosing
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26 27 28 29 30
Interviewers

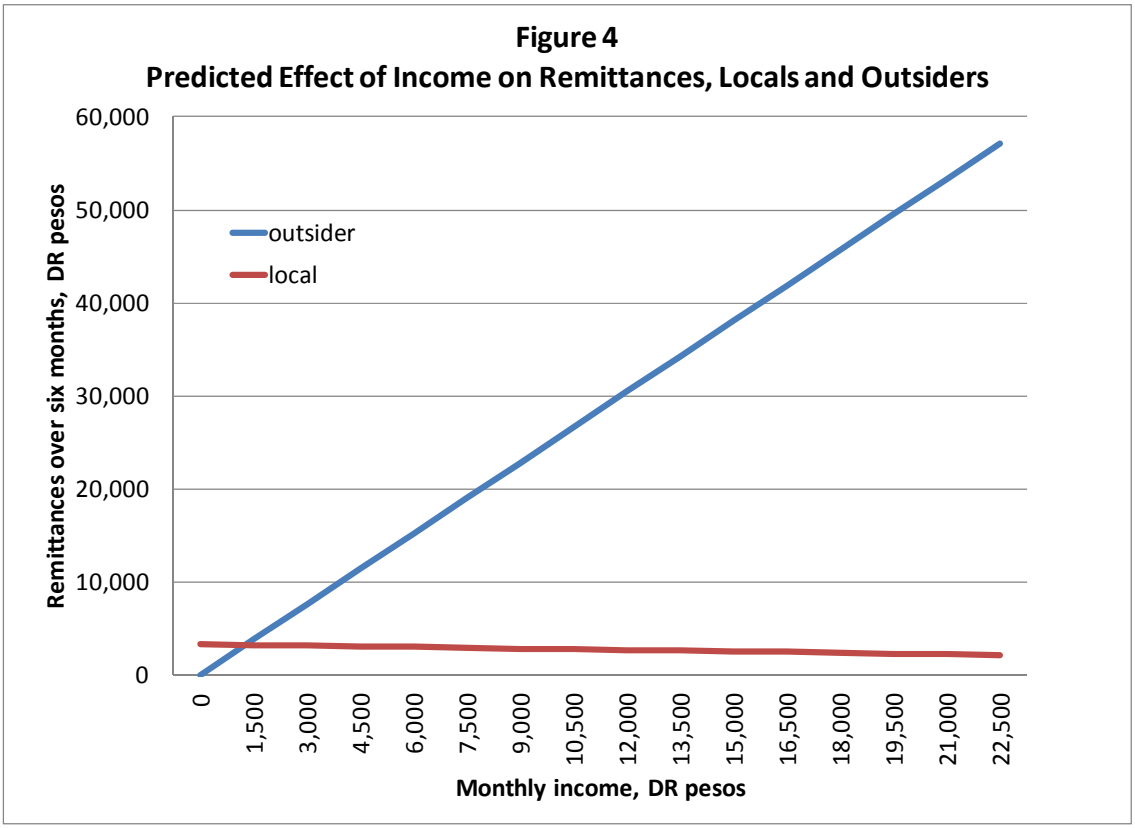
Figure 4
Predicted Effect of Income on Remittances, Locals and Outsiders
Remittances over six months, DR pesos
60,000
50,000
40,000
30,000
20,000
10,000
0
outsider
local
0
1,500
3,000
4,500
6,000
7,500
9,000
10,500
12,000
13,500
15,000
16,500
18,000
19,500
21,000
22,500
Monthly income, DR pesos

Figure 5
Predicted Effect of Income on Remittances, Locals and Outsiders