

Do survey interviewers really have to be strangers?

Mariano Sana, Vanderbilt University

Alexander A. Weinreb, University of Texas at Austin

Guy Stecklov, Hebrew University

Submitted to IUSSP 2013 Conference

Abstract

A standard methodological assumption in social and demographic surveys is that interviewers should be strangers to respondents. Bias is feared should the interviewer and respondent know each other prior to the interview. We refer to this assumption as the “stranger-interviewer norm” and note that it has never been systematically tested. In order to subject it to scrutiny, we fielded an experimental survey in a town of the Dominican Republic countryside in the summer of 2010. We employed a mix of local and outsider interviewers and hired enough locals to produce an adequate number of “insider” interviews, that is, interviews where interviewer and respondent knew each other. Systematic randomization in our sampling design gives our survey the rigor of an experiment. In this paper we use these data to evaluate how the accuracy of responses varies by the level of familiarity between interviewer and respondent, as we were able to validate a number of survey questions by checking official documents that confirmed (or disconfirmed) respondents’ reports. At the time of this submission our analyses have failed to find support for the stranger-interviewer norm. There is instead some evidence that respondents are more cooperative with interviewers when these are not outsiders.

This is work in progress. Please do not cite.

Background and Research Question

Demographic surveys in less developed countries (LDCs) often recruit local interviewers. There is, of course, a broad spectrum of research projects that collect socioeconomic, demographic, and health data in LDCs. At one end of this spectrum, the most established endeavors, such as the Demographic and Health Surveys (DHS), benefit, after years and several rounds of surveying in most countries, from the availability of experienced field workers native to the countries where the survey is implemented. At the other end, researchers with more limited budgets (and often, admittedly, limited fieldwork experience) set out to undertake surveys in LDCs, sometimes in remote rural locations. When budgets are more or less tight, hiring and training local interviewers is advantageous: interviewers without prior survey experience cost less, and transportation costs for field teams are reduced.

Employment of local interviewers opens the door to an often neglected possibility, especially when the survey setting is a small town, rural village, or an otherwise sparsely populated area. In other words, those areas where fertility and mortality are highest, health indicators are most worrisome, and the benefits of development are only slowly trickling down—that is, those areas for which demographic data are most needed and where demographic surveys are often implemented. Specifically, the possibility exists that interviewers find themselves collecting data from people they know or with whom they are more or less familiar. To what extent this actually happens we cannot tell because normally no records are kept. But it is certainly bound to happen when the survey setting is small and the interviewers are recruited locally. Weinreb (2006), for example, reports data from a survey in Kenya where the field manager did keep records on the relationship between interviewer and respondent after serendipitously realizing that in some cases they actually knew each other prior to the survey interaction. In that survey, 23 percent of interviews had happened between interviewers and respondents who knew each other. According to standard methodological advice, this is to be avoided, and interviewers should be strangers to respondents. Yet, this proposition has never been systematically tested. Do interviewers really need to be strangers?

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.

The stranger-interviewer norm was never tested with experimental data, systematically collected, until very recently. The only examination with non-experimental data was provided by Weinreb (2006). With survey data we gathered in the Dominican Republic by means of a carefully designed experiment, we have tackled various questions concerning the effects on data quality of various degrees of familiarity between interviewer and respondent.

Our purpose in this paper is to address validity as it varies by the degree of mutual familiarity between interviewer and respondent prior to the interview interaction. Elsewhere we have addressed item non-response rates and response patterns, including social desirability bias and other identifiable differences by type of interview, as well as a detailed analysis of the sensitive questions in the self-administered portion of our survey instrument (Weinreb, Sana & Stecklov 2011; Sana, Stecklov & Weinreb 2012; Rodriguez, Sana & Sisk 2012). In this paper we directly address the issue of accuracy in survey data, and we have the validation tools to do so confidently. Do stranger interviewers indeed collect more accurate survey data than interviewers who are familiar to the respondent?

Data

We collected our data in a Dominican town that we refer to as San Benito. San Benito is a mid-size municipal capital with social and economic indicators that approach the average for the Dominican Republic. We recruited 24 local interviewers out of a pool of 64 applicants, in addition to bringing in six experienced survey interviewers from Santo Domingo, the country’s capital—some of the local interviewers had experienced in collecting data from respondents, although not in social survey settings. All the interviewers were female with at least a high school diploma—most of them had a college degree or were currently enrolled in college. The youngest one was 18 years old and the oldest one 46. All of them, experienced and inexperienced alike, were trained together.

Once training was completed, the 24 local interviewers were asked to list all the women between ages of 20 and 50 (i.e. eligible for the survey) who lived in the town and they knew well or were familiar with, in addition to being able to identify their houses—fellow interviewers excluded. We refer to this process as “claiming” houses (or respondents). All lists combined, and without counting duplications, produced a total of 635 local women eligible for the survey. The field team had reasonably accurate and detailed maps of San Benito, secured from the Dominican census office, which included all properties¹. The next step was to identify the houses of the listed women on San Benito’s map, a process that took about a week, during which members of the field team canvassed the town with the local interviewers, armed with copies of the map, one small group at a time.

The end product of this process was the splitting of the town, for survey purposes, into two strata: claimed (the households marked on the map as a result of the process outlined above) and unclaimed (all other households in town). Because the claimed stratum was composed of respondents whom the interviewers knew, the possibility exists that the two strata differ in perhaps important ways. This compels us to control for stratum or conduct separate analyses for the two strata, although the differences in means between respondents in both strata tend to be small. For example, they are nearly the same mean age. Respondents in the claimed stratum are slightly more educated.

There were a total of 5,469 properties listed on the map, although we could not be sure if all of them were residential dwellings or if they were occupied. After the claiming process, then, the claimed stratum consisted of 635 houses/properties and the unclaimed stratum of 4,834. We decided to interview everyone in the claimed stratum, and a random sample of the unclaimed. In addition, to ensure proper randomization and avoid potential sample selection bias (or, in this case, stratum selection bias), we randomized interviewer assignments within both strata, subject to the restriction that interviews in the claimed stratum be divided equally among the three types of interviews that we were seeking to conduct, that is:

- a) Insider interviews, where interviewer and respondent knew each other prior to the survey interview,
- b) Local-stranger interviews, where the interviewer was local but there was no prior familiarity between the two parties, and
- c) Outsider interviews, conducted by the interviewers from Santo Domingo, who knew no one in town.

It should be obvious, then, that by definition there cannot be insider interviews in the unclaimed stratum.

¹ “Mapa de catastro” in Spanish, sometimes translated into English as “survey map”.

Once the survey process was underway, it became clear that some houses in the claimed stratum had been misidentified, that there had been more duplication than we were aware of, that some claimed respondents were no longer in town and some were away temporarily, and so on. However, refusal rates were fairly small (3.5% for outsider interviews, and below 2% for local-stranger and insider interviews) and we still secured over 200 insider interviews. All in all, the final sample size for completed interviews was 1,207. Table 1 shows the breakdown by stratum and type of interview.

Table 1. Sample size by stratum and type of interview

Stratum	Type of interview			Total
	Insider	Local-stranger	Outsider	
Unclaimed	0	455	216	671
Claimed	219	179	138	536
Total	219	634	354	1207

Survey instrument and validation data

The survey instrument was a composite of questions typically asked in social and demographic surveys in less developed countries. The topics included household demographic data, characteristics of the residence, wealth and assets, income, inflows and outflows of money and goods, family planning, detailed information on small children, religious beliefs and activities, attitudinal questions, and a set of sensitive questions collected by means of a separate, self-administered questionnaire.

Direct validation is certainly difficult to achieve. For example, in our initial design we assumed we would be able to validate children's school records or respondents' claims to property ownership. In the field we verified that both were optimistic expectations. Yet, we did manage to include in the survey instrument 16 items subject to direct validation, admittedly, by fairly candid means. In order to this, we simply asked respondents about information that could be validated with two documents that the same respondents were supposed to possess: the Dominican ID card (or *cédula*), and vaccination cards for small children. The relevant questions were asked in the process of the interview and documentation to validate the responses was requested after all the information had been collected. We anticipated that asking the respondents to produce documentation listing information that they had already provided would appear strange to them. Our interviewers, however, reported limited levels of voiced discomfort on the part of the respondents and, when asked, justified their request for documentation (as per their training instructions) in vague terms, speculating that the researchers wanted to test how well people remember information. In the end, our interviewers were able to look at the *cédula* of 831 of our 1,207 respondents, but open refusal to show the document was recorded in only 45 cases. There were indeed many reasons for the *cédula* not to appear. While the veracity of respondents' answers could of course not be verified, they cited the *cédula* being at another location, being lost or misplaced, never having had one, or they simply looked for it and reported not to be able to find it.

Data subject to validation on small children corresponded only to children between 18 months and their fifth birthday. There were 309 respondents who had at least one child in this age range, but only 10 had two. No respondent had more than two children this age. Respondents were less challenged finding the vaccination cards for their children, and being willing to show them to the interviewers, than they were at finding and showing their own *cédulas*. More than 85 percent of respondents with small children produced the vaccination cards. Our interviewers were trained to help respondents understand vaccination recommendations, for which they needed to see the vaccination cards. This undoubtedly must have helped gained cooperation on the part of respondents.

Of the 16 survey items subject to validation, four corresponded to the respondent's own information, six to information on one of the children of eligible age (if any), and six to information on the second child of eligible age (if any). The items, together with the validation documentation used, are listed in Table 2.

Table 2. Validation items

Concerning	Item	Validation document
Respondent		
	Birth date	<i>cédula</i>
	Birth place (town and municipio)	<i>cédula</i>
	"Official" skin color	<i>cédula</i>
	Blood type	any available
Child between 18 months and fifth birthday		
	Birth date	vaccination card
	Birth weight	any available*
	TB vaccine (yes/no)	vaccination card
	No. doses of tetanus vaccine	vaccination card
	No. doses of polio vaccine	vaccination card
	Measles vaccine (yes/no)	vaccination card

* Sometimes, but not always, recorded on the vaccination card.

Proportions validated

While the total of items subject to validation is 16, this high number applies only to the 10 respondents who had two children of eligible age. At the other extreme, for the 898 respondents who had no child of eligible age, the maximum possible number of validations is four: the four items concerning the respondents themselves. In 202 cases we were able to validate nothing at all, reducing our working sample to 1,005 cases. Table 3 lists the proportion of items that, given eligibility, we were able to validate, by stratum and type of interview, as well as the number of respondents for whom these proportions apply—i.e. the number of respondents for whom we could validate at least one item.

In turn, tables 4 and 5 report the same information for respondent-only validation variables (the top panel in Table 2) and for children-only validation variables (the bottom panel in Table 2).

Table 3. Means of proportion validated by stratum and type of interview

Stratum	Type of interview			Total
	Insider	Local-stranger	Outsider	
Unclaimed		0.744	0.741	0.743
N		377	176	553
Claimed	0.805	0.787	0.809	0.800
N	192	157	103	452
Total	0.805	0.757	0.766	0.769
N	192	534	279	1005

Table 4. Means of proportion validated by stratum and type of interview, respondent-only validation variables*

Stratum	Type of interview			Total
	Insider	Local-stranger	Outsider	
Unclaimed		0.747	0.751	0.748
N		374	173	547
Claimed	0.816	0.798	0.822	0.811
N	189	157	100	446
Total	0.816	0.762	0.777	0.777
N	189	531	273	993

Table 5. Means of proportion validated by stratum and type of interview, children-only validation variables*

Stratum	Type of interview			Total
	Insider	Local-stranger	Outsider	
Unclaimed		0.788	0.709	0.757
N		56	36	92
Claimed	0.726	0.745	0.752	0.740
N	24	25	18	67
Total	0.726	0.775	0.723	0.750
N	24	81	54	159

Analytical strategy

We conduct two sets of analyses. In our first set, the unit of analysis is the respondent and the variable

of focus is an aggregate measure of successfully validated matches. In the second set, the unit of analysis is matches, which are clustered within respondents—in theory up to 16 per respondent.

For our first set of analyses we classified all the items that we were able to validate for each respondent into matches and mismatches. We then computed the count of successful matches and the proportion of matches. We used the count of matches to run models for count dependent variables (Poisson, Negative Binomial Regression), subject to exposure varying by respondent (number of items that we could validate), but this proved to be a futile endeavor. Convergence problems forced us to abandon this attempt.

Far more fruitful was to use the proportion of matches as the dependent variable in regression models for sample selection bias, where the selection effect was given by our ability to be able to validate items at all. We report results from these models below.

Our second set of analyses is work in progress. For this set of analyses, we expanded the layout of our dataset so that each observation becomes a person-match, and then we run standard logistic regression models on the likelihood of validated matches. We do not present results from it here but we will have results available by the time of the IUSSP conference. Our preliminary work seems to suggest that results will not be substantially different from those obtained from the models for sample selection bias on the variable for proportion of validated matches.

First Preliminary Results

We conducted regression models for sample selection bias, following Heckman's MLE methods. The dependent variable is proportion of validated matches. The selection effect is given by the fact that, in order to compute the proportion of matches, we need to be able to validate at least one item. As specified above, in 202 cases we were unable to do this, and it is possible that there is selection bias influenced, among other factors, by type of interview. Our identifying variable in the selection equation (labeled "cooperative") is a dichotomy with value 1 if the interviewer reported, once the interview was completed, that the respondent had been cooperative all along. There is an obvious endogeneity possibility here, as interviewers should have been likely to classify someone not producing documentation, and therefore someone for whom we could not validate any item, as "uncooperative". As this is work in progress we expect to produce a more persuasive identification variable for the selection model by the time we present results at the conference. Yet, we did verify that this variable has no significant effect on the likelihood of a validated item being a match, a requirement for a good identification variable in the selection equation.

Table 6 presents the estimated coefficients from Heckman's MLE model for selection bias, using proportion of validated matches on all available items. We present three models. Model 1 does not control for stratum, Model 2 adds a control for stratum, and Model 3 runs the model on the claimed stratum only.

Table 6

Heckman selection model, MLE estimation, for proportion of
validated matches, all 16 validation variables

	Model 1		Model 2		Claimed stratum only	
Outcome equation						
Insider interview	0.031	^	0.008		0.008	
	(0.017)		(0.018)		(0.018)	
Educational level	0.043	**	0.042	**	0.034	**
	(0.005)		(0.005)		(0.007)	
Age	-0.002	**	-0.002	**	-0.004	**
	(0.001)		(0.001)		(0.001)	
Known to have lied	-0.040	*	-0.039	*	-0.070	*
	(0.016)		(0.016)		(0.030)	
Has small children	-0.026		-0.025		-0.044	
	(0.017)		(0.017)		(0.027)	
Claimed stratum			0.034	*		
			(0.016)			
Constant	0.647	**	0.638	**	0.766	**
	(0.042)		(0.042)		(0.065)	
Selection equation						
Cooperative	0.474	**	0.474	**	0.548	**
	(0.102)		(0.101)		(0.176)	
Insider interview	0.313	*	0.295	^	0.420	**
	(0.128)		(0.152)		(0.158)	
Local-stranger interview	0.141	^	0.143	^	0.433	*
	(0.082)		(0.085)		(0.181)	
Educational level	-0.041		-0.042		-0.071	^
	(0.028)		(0.028)		(0.043)	
Age	-0.007		-0.007		-0.015	
	(0.006)		(0.006)		(0.009)	
Known to have lied	0.076		0.076		-0.064	
	(0.097)		(0.096)		(0.157)	
Has small children	0.296	*	0.297	*	0.357	^
	(0.117)		(0.117)		(0.189)	
Claimed stratum			0.028			
			(0.114)			
Constant	0.799	*	0.793	*	1.104	*
	(0.311)		(0.319)		(0.508)	
athrho	0.185	*	0.180	*	0.290	**
	(0.074)		(0.074)		(0.111)	
rho	0.182		0.178		0.282	

	(0.072)	(0.072)	(0.102)
lambda	0.043	0.042	0.063
	(0.018)	(0.018)	(0.023)
No. of observations	1184	1184	526
Censored observations	197	197	82
	-	-	-
Log Likelihood	492.83	491.06	169.46
LR test of independence of the two eqs., chi- sq(1)	6.17 *	5.97 *	6.80 **

** p<.01; * p<.05; ^ p<0.10

Robust Standard errors in parenthesis (adjusting for clustering of respondents within interviewers).

[A side not on the variable presented as “known to have lied”... Respondents were asked to say whether they had heard of a number of famous people. They were then given the names of these people, one at a time, nine in total. The person enumerated fifth and the person enumerated last were made-up names. The rest were either internationally famous people (such as Michael Jackson) or persons famous in the Dominican Republic (such as the country’s first lady). The idea is to tempt respondents to, out of embarrassment from not knowing these presumably famous personalities, report that they had in fact heard of them. Those who responded that way on at least one of the two made-up famous people were classified as “known to have lied”.]

The variable of focus for us is type of interview. There is clear evidence of selection bias given by rho being positive and the LR test at the bottom of the table being significant. In addition, we see that type of interview is, across all three models and with various levels of statistical significance, an important factor to explain this selection mechanism. We were statistically more likely to be able to validate information at all when the interviewer was an insider, or a local-stranger, rather than an outsider.

For the outcome equation (top panel of the table) we collapsed local-stranger and outsider interviews together as the reference category. We did so because our examination of the data, both at the descriptive and multivariate level, revealed that the only discernible difference by type of interview on the likelihood of successful matches was between insider interviews and everyone else. Yet, we only get a marginally significant result when we do not control for stratum. It is clear, however, that the model is misspecified without a control for stratum. Models 2 (with such control) and 3 (on only the claimed stratum) show the effect of insider interview disappear.

Because of the possibility that the questions on small children could be biasing results in some way (to begin, the presence of these children more than doubles the number of variables subject to validation, perhaps increasing the likelihood of validation in the first place) we repeated these models selecting only the four respondent-only validation items.

Results, presented in Table 7, are substantially similar to those presented in Table 6.

Table 7

Heckman selection model, MLE estimation, for proportion of validated matches, respondent-only validation variables

	Model 1		Model 2		Claimed stratum only	
Outcome equation						
Insider interview	0.034	*	0.008		0.008	
	(0.016)		(0.017)		(0.017)	
Educational level	0.047	**	0.046	**	0.039	**
	(0.005)		(0.005)		(0.007)	
Age	-0.002	**	-0.002	**	-0.003	*
	(0.001)		(0.001)		(0.001)	
Known to have lied	-0.047	**	-0.045	**	-0.074	*
	(0.017)		(0.017)		(0.031)	
Has small children	0.002		0.003		-0.001	
	(0.017)		(0.017)		(0.022)	
Claimed stratum			0.039	**		
			(0.015)			
Constant	0.623	**	0.613	**	0.731	**
	(0.046)		(0.046)		(0.068)	
Selection equation						
Cooperative	0.465	**	0.465	**	0.535	**
	(0.094)		(0.094)		(0.158)	
Insider interview	0.294	*	0.277	^	0.406	**
	(0.137)		(0.155)		(0.154)	
Local-stranger interview	0.177	*	0.179	*	0.480	**
	(0.082)		(0.087)		(0.169)	
Educational level	-0.034		-0.035		-0.058	
	(0.029)		(0.028)		(0.041)	
Age	-0.005		-0.005		-0.014	
	(0.006)		(0.006)		(0.009)	
Known to have lied	0.085		0.085		-0.034	
	(0.087)		(0.087)		(0.149)	
Has small children	0.121		0.122		0.121	
	(0.118)		(0.118)		(0.178)	
Claimed stratum			0.027			
			(0.119)			
Constant	0.682	*	0.676	*	0.980	*
	(0.300)		(0.308)		(0.453)	
athrho	0.199	*	0.194	*	0.272	**
	(0.079)		(0.078)		(0.100)	
rho	0.196		0.191		0.266	

	(0.076)	(0.075)	(0.093)
lambda	0.049	0.047	0.060
	(0.020)	(0.019)	(0.022)
No. of observations	1184	1184	526
Censored observations	209	209	88
	-	-	-
Log Likelihood	552.46	550.36	189.48
LR test of independence of the two eqs., chi- sq(1)	6.26 *	6.13 *	7.40 **

** p<.01; * p<.05; ^ p<0.10

Robust Standard errors in parenthesis (adjusting for clustering of respondents within interviewers).

Tentative conclusion (for now)

Focusing on type of interview, so far we have that familiarity between interviewer and respondent (as shown in the selection equation from the Heckman models) had a positive effect on our ability to validate responses in the first place. What this specifically means is that the respondents were more likely to produce the requested documentation that we needed to undertake validation. Granted, if the documentation did not appear, we cannot be sure of why. It might be that the respondent does not want to cooperate, or that she is sloppy with her records, or that she genuinely could not find them, or that she knows she lied and does not want the interviewer to notice, or a number of others reasons. Whatever it is, and even being unable to pinpoint it, we do know one thing: the appearance of those records was a good thing, and they were more likely to appear when the interviewer was an insider, or a local-stranger, than an outsider.

Once records are available, however, and even after adjusting for this selection bias, we found no effect of type of interview on proportion of items positively validated after controlling for stratum. In other words, this is a tie. As far as we can show, there is no evidence that neither insiders nor local-strangers nor outsiders can elicit more honest or accurate responses from respondents. This runs counter to the stranger-interviewer norm, which assumes that the perfect interviewer should be a stranger and respondents would be less likely to answer truthfully if the interviewer is someone they know.

This is, again, work in progress. We cannot yet report on our second set of analyses, where the focus is on individual validation matches rather than aggregate validation as discussed so far. We will have a refined product by the time of the IUSSP conference.

References

Agar, Michael. 1980. *The Professional Stranger: An Informal Introduction to Ethnography*. New York: Academic Press.

- Josephson, Eric. 1970. "Resistance to Community Surveys". *Social Problems* 18(1), 117-129.
- Marcelli, Enrico, Louisa Holmes, David Estella, Fausto da Rocha, Phillip Granberry, and Orfeu Buxton. 2009a. *(In)Visible (Im)Migrants: The Health and Socioeconomic Integration of Brazilians in Metropolitan Boston*. San Diego, CA: Center for Behavioral and Community Health Studies, San Diego State University.
- Marcelli, Enrico, Louisa Holmes, Magalis Troncoso, Phillip Granberry, and Orfeu Buxton. 2009b. *Permanently Temporary?: The Health and Socioeconomic Integration of Dominicans in Metropolitan Boston*. San Diego, CA: Center for Behavioral and Community Health Studies, San Diego State University.
- Miller, S. M. 1952. "The Participant Observer and Over-Rapport." *American Sociological Review* 17: 97-99.
- Parrado Emilio A., Chris McQuiston, and Chenoa A. Flippen. 2005. "Participatory survey research - Integrating community collaboration and quantitative methods for the study of gender and HIV risks among Hispanic migrants." *Sociological Methods & Research* 34(2):204-239.
- Rodriguez, Leslie, Mariano Sana, and Blake Sisk. 2012. Self-administered questions and interviewer-respondent familiarity". Presented at the annual meeting of the Southern Demographic Association, Williamsburg, VA.
- Sana, Mariano, Guy Stecklov, and Alexander A. Weinreb. 2012. "Local or outsider interviewer? An experimental evaluation." Paper presented at the Annual Meeting of the Population Association of America, San Francisco, CA.
- Schaeffer, Nora Cate, Jennifer Dykema, and Douglas W. Maynard. 2010. "Interviews and Interviewing", pp.437-470 in J. Wright and Peter Marsden (ed.) *Handbook of Survey Research*. 2nd edition.
- Stocking, George W. 1983. "History of Anthropology: Whence/Whither." Pp. 3-12 in *Observers Observed: Essays on Ethnographic Fieldwork*, ed. George W. Stocking. Madison, WI: University of Wisconsin Press.
- Weinreb, Alexander A. 2006. "The Limitations of Stranger-Interviewers in Rural Kenya." *American Sociological Review* 71: 1014-1039.
- Weinreb, Alexander A., Mariano Sana, and Guy Stecklov. 2011. "Do insiders or outsiders collect better data? Results from a methodological experiment". Paper presented at the Annual Meeting of the Population Association of America, Washington D.C.