Misreporting in Sensitive Health Behaviors and its Impact on Treatment Effects: An Application to Intimate Partner Violence

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Abstract

Despite the large potential for misreporting, much of the empirical work in economics relies on self-reported data. This problem is more worrisome whenever the respondent is enquired about sensitive topics. Relying on an indirect questioning technique, we measure and characterize misreporting of physical and sexual intimate partner violence. On average, we do not find evidence of misreporting but we uncover strong evidence of non-random measurement error: the anonymity provided by the indirect method allows college-educated women to report higher rates of victimization while no change is observed for the less educated. This systematic misreporting is large enough to reverse the education gradient in violence. We also provide a low-cost solution to correct the biased created, even for causal estimators, under the presence of non-classical measurement error in the dependent variable.

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1 Introduction

Much of the empirical work in economics relies on self-reported data, despite the fact that people make several mistakes when responding to a survey. For different reasons, ranging from random mistakes, limited attention, and lack of recollection to behavioral biases or stigma, respondents give inaccurate answers that introduce measurement error in the data. Misreporting is expected to be even more worrisome whenever the respondent faces questions about sensitive topics such as personal earnings, crime activity, drug use, discrimination, physical appearance, or domestic violence.

While classical measurement error in the dependent variable only affects the precision of the estimates, without additional information the presence of non-classical measurement error makes it impossible to obtain unbiased causal effects of a variable of interest. In the case of risky behaviors such as violence against women or youth crime, for example, the identification of causal relationships is crucial to guide prevention and mitigation efforts.

In recent years, measurement error concerns have been increasingly addressed in the literature. An important share of these studies has made use of administrative records to directly measure and characterize misreporting in sensitive topics such as voting [Rosenfeld et al., 2016], mental health conditions [Bharadwaj et al., 2015], or personal earnings [Gottschalk and Huynh, 2010]. However, in several cases this is not an alternative due to lack of accurate administrative data or self-selection into such reporting.

Using an indirect questioning technique, this paper measures and characterizes misreporting when dealing with a sensitive topic and proposes an alternative to quantify the bias introduced by measurement error in the estimation of treatment effects. As a case study, we focus on the measurement of physical and sexual intimate partner violence (IPV), both due to its saliency as a public health issue and the urgency to generate accurate data on its prevalence to guide policy efforts.

Our focus on violence against women is also extremely timely as a growing number of studies tries to identify the main drivers of this phenomenon [e.g., Angelucci, 2008; Hidrobo

and Fernald, 2013; Haushofer and Shapiro, 2013; Bobonis et al., 2013; Hidrobo et al., 2016] and the impact of programs intended to reduce its prevalence [World Health Organization, 2009]. Several scholars have argued that measures of violence against women could be subject to reporting error [e.g., DeKeseredy and Schwartz, 1998; Ellsberg et al., 2001; Kishor, 2005; Aizer, 2010], but little is known about the magnitude and the characteristics of misreporting in this field. The use of inaccurate self-reported data on victimization could be introducing important distortions in the estimates of treatment effects in the aforementioned studies.

We implement an indirect questioning technique which provides further anonymity to the respondents and compare the prevalence rates of physical and sexual IPV estimated by this method to that obtained from direct questions from the Demographic and Health Surveys (DHS), a global project that is the main source of IPV data [Klugman et al., 2014]. Thus, we are the first to measure misreporting when the direct questions that are currently the gold standard in the measurement of violence against women are used.

In particular, we apply the methodology of list experiments [e.g., Blair and Imai, 2012; Glynn, 2013; Karlan and Zinman, 2012] as well as DHS direct questions to a sample of female clients of a microcredit organization operating in several impoverished peri urban districts of Lima, Peru. We randomize two questionnaires at the individual level. The control group receives the nine direct questions that DHS uses to measure the prevalence of physical and sexual IPV. In addition, the control group receives nine lists of four non-sensitive statements and is asked to provide the number of statements that hold true in each list but not the individual prevalence of each statement. The treatment group does not answer the direct questions but rather answers the list experiment questions provided to the control with an added sensible statement. Thus, the nine lists received by the treatment group have five statements, where the last one refers to a specific act of physical or sexual violence. Randomization guarantees that the average number of neutral statements is equal across treatment arms. Thus, the prevalence rate of a given act of physical or sexual violence is estimated as the difference in the average number of statements that hold true in each list

across treatment arms.

We find no significant differences in reporting of physical and sexual violence across direct and indirect methods. However, we find that the reporting error varies with the level of education: women with completed tertiary education report higher rates of violence under the list experiments than under the direct method. There is no difference for less educated women. The increased report of violent episodes among more educated women under list experiments is large enough to reverse the negative education gradient identified when prevalence rates are measured through direct questions.

We argue that our results have ample applications in settings where the dependent variable suffers from non-random measurement error [Bound et al., 2001; Butler et al., 1987] and where administrative records are not a data source alternative. As a general result, we review the implications of systematic misreporting on the estimation of causal effects. A popular strategy to deal with endogeneity biases has been to rely on the exogenous variation introduced in the variable of interest through a randomized controlled trial (RCT) or a quasi experimental approach using instrumental variables (IV). We show that RCTs and (valid) IVs still yield biased treatment effects in the presence of non-classical measurement error in the outcome variable. In fact, relative to RCTs and IVs, cross-sectional estimates may provide *less* biased estimates when the sign of the bias from omitted variables is opposite to that of the relationship between measurement error and the risk factor.

Our experimental approach provides researchers with a simple and inexpensive strategy to test for measurement error, classical or not, in contexts where administrative records are not available and fieldwork is being conducted. By providing full anonymity to the respondent, we minimize the costs of being exposed as a victim and obtain a benchmark measure that can be used to gauge the characteristics of the reporting error for a given sensitive outcome. Furthermore, our approach allows researchers to directly estimate measurement error in their sample and correct their treatment effect estimates. Our contribution is particularly valuable for the case of violence against women, since there are no previous efforts trying to quantify

the severity and patterns of underreporting in such sensitive behavior nor the implications that misreporting has on the estimation of treatment effects.

The paper is divided in five sections including this introduction. Section two reviews the literature on misreporting when sensitive information is collected. The third section provides details about the case study we implement. It reviews the design of the indirect method we relied upon, describes the data and the sample, provides details on the estimation strategy of the measurement error, and presents the results. The fourth section discusses the implications of these results when trying to causally identify the drivers of IPV, presents simulation results that quantify the magnitude of the bias introduced under different scenarios, and provides practical guidelines to deal with measurement error bias in the estimation of treatment effects. The last section concludes.

2 Misreporting in Sensitive Survey Questions

There is an extensive literature showing that measurement error in survey data on certain topics is not random but rather correlates with an array of characteristics. For instance, income and asset data are prone to systematic measurement error due to distrust, lack of recall, strategic reporting, stigma, or a desire to reduce the interview time, among other reasons. Poorer households, for example, could underreport their income if they perceive the data collected is going to be used to distribute social welfare benefits. At the same time, they tend to have more sources of income at diverse frequency rates, which could lead them to misreport due to lack of perfect recall.[1] In fact, by relying on tax records, Gottschalk and Huynh [2010] shows that there is substantial measurement error in earnings and that this error is correlated with earnings and positively correlated across time.

Health outcomes also suffer from such bias as reviewed in Bound et al. [2001]. For example, Butler et al. [1987] shows evidence of non-classical error in the measurement of arthritis while Johnston et al. [2009] finds a similar pattern in hypertension self-reporting.

[1] Indeed, Meyer et al. [2008] shows that welfare benefits may be misreported.

O'Neill [2012] identifies a negative correlation between self-reported and anthropometric measures of body mass index. More recently, Bharadwaj et al. [2015] relies on administrative records and finds that underreporting in mental health medication is correlated with age, gender, and ethnicity.

Non-classical measurement error in the dependent variable makes it impossible to obtain unbiased causal effects of a particular characteristic or attribute, especially if the outcome is correlated with the latter. For example, a well-known puzzle in the development economics literature is that of an inverse plot size-productivity relationship. Two recent studies [Gourlay et al., 2017; Desiere and Jolliffe, 2017] show that whenever self-reported measures of yields are replaced by more accurate measures, the relationship between plot size and productivity vanishes.

In the case of risky behaviors such as crime or violence, the identification of causal relationships is particularly crucial since these findings tend to guide costly policy efforts and targeting strategies. Even when exogenous variation in the hypothesized risk factor is introduced, misleading conclusions may emerge if the dependent variable is systematically misreported.

The Case of Violence Against Women

Two features of violence against women generate large potential for error in the measurement of prevalence rates: it is usually perpetrated by people they know, mainly their partners or ex partners, and it tends to be invisible as much of it happens behind closed doors and in the privacy of the home.

These features introduce very large costs to self-identify as a victim. First, there is an emotional cost that the woman may face due to her attachment to the offender and the potential sanctions (social or legal) that he may face. Second, a woman may also fear the potential loss of her partner's economic support if her status as a victim is revealed. Third, if exposed, she also faces the risk of retaliation through an escalation of violence against her

or her children. Finally, women may fear stigmatization, either from intrinsic or extrinsic sources [Overstreet and Quinn, 2013].

There is a growing consensus about the best practices on how to ask questions about IPV. They have been compiled and proposed by the WHO Organization et al. [1997]; Ellsberg and Heise [1999]. For example, fieldworkers need to secure a safe place to ask IPV-related questions making sure that women are alone when answering these questions. Also, participants should have several opportunities to respond about issues related to IPV. Generic and subjective questions such as "Have you ever experienced domestic violence?" must be avoided and instead questions should reflect specific episodes of violence.

Despite the use of rigorous ethical and privacy protocols in specialized surveys, respondents may still perceive a degree of risk of being exposed when asked directly about their IPV experience. In fact, since the costs of being exposed are very likely to be heterogeneous, privacy concerns may differentially prevent women from truthfully reporting their previous experience of violence, leading to systematic misreporting. For instance, Ellsberg et al. [2001] argues that when more safety measures for privacy are provided, higher rates for IPV are found, relative to the DHS methodology. However, while suggestive, the authors cannot isolate the fact that the compared surveys where conducted in different years and without an experimental design.

The private nature of the violence implies that administrative records from the police or health establishments may capture a non-random sample of the true cases. Although a few reports may come from third parties, the bulk of the records rely upon the victim's decision to approach the authorities.

Unlike other health outcomes, administrative data cannot provide a benchmark for the "true" measure of violence against women since reporting violence to the authorities also imposes a cost of being exposed. Indeed, this cost may become even higher due to fear or distrust of the authority herself. Using surveys from 24 countries in the DHS program, Palermo et al. [2014] show that only seven percent of women who experienced such violence

made a formal report that would be captured in administrative data (e.g., police, medical, or social services). Most likely, there is selection into reporting since the ones who made an active effort to do so are the ones who face lower exposure costs. In fact, Palermo et al. [2014] shows that reporting depends on women's socioeconomic characteristics such as age, marital status, education, and urban location.

Our paper relies on list experiments, to measure and characterize the reporting error in the prevalence of physical and sexual lifetime experience of IPV as committed by the women's last partner.[2] List experiments provide full anonymity to respondents, which minimizes the costs of being exposed as a victim and/or exposing the aggressor. Thus, we provide a significant contribution to the literature on violence against women by establishing a benchmark and characterizing misreporting.[3]

Similar to Karlan and Zinman [2012], we recruit a large enough sample to only ask the control group about their previous violence experience using face-to-face DHS-type survey questions. This allows us to ensure full protection to the treatment group, who only answers the list experiment questions that include the sensitive statement.

Two recent papers are closely related to our paper: Joseph et al. [2017] and Peterman et al. [2017]. They both rely on list experiments to measure prevalence rates of physical domestic violence. Their contribution is valuable but they have several limitations. First, Joseph et al. [2017] measures prevalence rates at the household level, which implies that the

[2]Recent applications of list experiments include, for example, Karlan and Zinman [2012] to measure loan proceeds from microfinance loans, McKenzie and Siegel [2013] to elicit illegal migration rates, Coffman et al. [2013] to measure the size of LGBT population and anti-gay sentiment, Imai et al. [2014] to examine vote-selling, and Rosenfeld et al. [2016] to study anti-abortion support.

[3]Alternative methods include qualitative approaches as in [Blattman et al., 2016]. The authors combine surveying with ethnographic techniques to uncover misreporting. Their approach does not provide anonymity to the respondents. It is quite expensive since it requires the surveyor team to stay for longer periods in the field and its success depends heavily on the surveyors' ability to make the respondent feel safe and comfortable to truthfully report or reveal her answers or behavior. Surveyors training becomes crucial which only adds to the cost of the fieldwork, making it hard to scale up. There are other indirect questioning techniques such as endorsement experiments or randomized response technique which are often used in the political science literature but that are not appropriate to measure IPV prevalence. The former is not adequate since it is designed to measure attitudes rather than behavior. Randomized response methods do measure behavior but generate high non-response rates since the burden to conduct the randomization is imposed on the respondent. This method can be hard to grasp, even among respondents in developed countries.

respondent is not necessarily a woman. It may be the case that the respondent does not know about the experience of domestic violence for all women in the household or that he is the perpetrator himself. Second, their sensitive statement is quite general (*Has at least one woman member of your household faced physical aggression from her husband anytime during her life?*), greatly departing from the well-established WHO guidelines for the measurement of violence which require asking about several and specific violent events. The same holds for Peterman et al. [2017], who targets women as respondents but uses a general sensitive statement to measure physical violence (*In the last 12 months, have you ever been slapped, punched, kicked, or physically harmed by your partner?*). Third, neither Joseph et al. [2017]'s nor Peterman et al. [2017]'s list experiments allow to accurately measure misreporting in the direct question modules. The latter did not include a direct question equivalent to the sensitive item in the control questionnaire while the former asks the same individual the direct question on violence *before* the indirect question. This could bias both reports since the respondent is no longer protected by the list experiment. Finally, neither study is able to compare their results to the so-called "gold standard" questions from the DHS, which are the best alternative to ask direct questions on violence against women at the moment. Thus, they are not able measure misreporting relative to the *best available* direct reporting method. Our design overcomes these limitations by focusing on women as respondents, following the WHO guidelines for direct questions as well as their privacy and safety protocols throughout the application of the questionnaire, asking the indirect questions to a control group that differs from the treatment group, and comparing the prevalence rates obtained from the indirect method to the ones that come from the DHS direct method.

3 Measuring Reporting Bias in Violence Against Women

3.1 List Experiments: Design

List experiments have been traditionally used to gather opinions and/or record behavior related to inherently sensitive issues which are more prone to underreport. The basic design of a list experiment will feature a control group (C), who is only given a list of S neutral statements, and a treatment group (T), who receives the same list of S statements plus one, where the last one refers to a sensitive issue. Both groups are asked to provide the *number* of statements that hold true, without indicating which ones are in fact true. Below we show how comparison between the average number of true statements across groups yields the prevalence rate of the sensitive statement while providing full anonymity to the respondent.

Let $d_{is} = 1$ if, for individual i, the sth statement is true and zero otherwise. In a list experiment, this is not directly observed. However, we observe the number of responses that hold true for each i denoted as $\sum_s^S d_{is}$ when she belongs to the control group and $\sum_s^{S+1} d_{is}$ is she is in the treatment group. Under the assumption that the inclusion of the sensitive statement does not distort the answers to the neutral statements in the treatment group (no-design effect assumption, see Blair and Imai [2012]), random assignment of the treatment at the individual level implies that:

$$E\left(\sum_s^S d_{is}|T\right) = E_i\left(\sum_s^S d_{is}|C\right)$$

That is, the control group provides the counterfactual of the number of true statements if the treatment group were to receive only S statements. The prevalence rate of the sensitive statement can thus be measured as:

$$\rho = E\left[\left(\sum_s^{S+1} d_{is}|T\right) - \left(\sum_s^S d_{is}|C\right)\right]$$

We apply this methodology to measure prevalence rates of intra-partner physical and

sexual violence during a woman's lifetime as committed by her last partner. In particular, the sensitive statements used in the lists reproduce the ones asked directly in the DHS when trying to directly measure physical and sexual IPV prevalence.

For the list experiments to effectively protect respondents' privacy while providing a good estimator of the prevalence rate, the selection of neutral statements is crucial. In particular, designing the list of statements has to take into account the trade-off between protecting the respondent and reducing the variability of the responses. On one hand, we would like to avoid a neutral list in which a very large share of the population is likely to respond $\sum_s^S d_{is} = S$, i.e. ceiling effect, since the respondent would no longer be protected. A similar situation occurs when the list contains low-prevalence items (i.e., $\sum_s^S d_{is} \approx 0$) that may deter the respondent to answer honestly.

On the other hand, a list that avoids the two problems stated above will most likely introduce greater variability in the responses, which could then increase the variance of the estimator. Glynn [2013] provides some guidance in the development of lists so as to maximize the level of protection while sacrificing little variance. He shows that introducing negative correlation between the responses to the neutral items in the list limits the variability of the responses while minimizing the likelihood of ceiling effects. In Section 3.2 we provide details on the efforts we undertook to minimize extreme values in the sets of statements used while maintaining low levels of variability in the responses.

Even if the instrument is flawless, list experiments pose an important implementation challenge. In that sense, the training of surveyors is fundamental for two reasons. First, to ensure that the experiment is correctly implemented. The respondent may become overwhelmed with the mechanics of the experiment since it is not what the typical question demands from her. If the surveyor is not able to guide the respondent through the methodology, additional measurement biases due to lack of understanding may be introduced. Second, to be able to make the respondent aware of the anonymity provided by the experiment. If the respondent is unable to grasp the full confidentiality provided under the list experiment, the

biases relative to DHS-type questions do not completely disappear. Sub-section 3.2 provides details on the strategies we followed to minimize implementation issues.

The nature of the list experiments in itself imposes a limitation if the use of the data goes beyond the measurement of prevalence rates. Since the data collected under this method does not allow the researcher to link prevalence rates to other respondents' characteristics, the analysis of correlations between the experience of violence and other variables is limited. However, with large enough sample sizes one can measure prevalence rates by sub-samples as we do here (see Section 3.4) and learn more about the correlation between violence and risk factors.

3.2 Sample Description and Data

The population of interest for our study is composed by adult women (aged 18 and above), in Lima, who receive microloans from the Adventist Development and Relief Agency (ADRA), a international non-governmental organization (NGO) running a village banking program in Peru's peri-urban and rural areas. ADRA's clients in Lima are microentrepreneurs from the most impoverished districts such as San Juan de Lurigancho, Villa Maria del Triunfo, Villa El Salvador, Ventanilla, Huaycan, and Los Olivos.

From the total pool of 1873 clients in 112 village banks in ADRA's microcredit program in Lima, we first drop all under-aged clients as well as all women above 65. This leaves us with a remaining universe of 1776 clients. We then draw 6 banks at random and exclude them from the study to be able to pilot the instruments with their members. This leaves us with 1690 clients in 106 banks. Finally, we work with all banks with monthly meetings scheduled during July 2015 which restricts the universe of interest to 1562 women in 98 village banks. We targeted this universe and were able to interview 1223 women between July 1st and August 25th, 2015.

Randomization of the treatment was done at the individual level and was conducted by the surveyor. The questionnaire was implemented via tablets. Due to some initial compli-

cations with the software, we drop a few surveys which were incorrectly assigned to answer the list experiment questions from both treatment arms and are left with a sample of 1078 valid surveys.[4] According to our power calculations, this sample was large enough to detect an effect as small as 0.03 percentage points between the treatment and control groups.[5]

Table A.1 in the Appendix confirms that the randomization was successful. There is only a small significant difference in the share of women that are household heads across treatment arms (at the 5 percent level). All our estimates include a full set of controls, including a binary variable that indicates if the woman is the household head.

Clearly, the implementation of list experiments requires careful preparation in terms of instrument development, the training of surveyors, and tools to secure respondents' adequate understanding of this type of questions. With this in mind, we dedicated special attention to (i) the design of the instrument, (ii) the selection and training of surveyors, and (iii) the application of the instrument.

First, we took special care in the design of the questionnaires. We piloted the non-sensitive statements in a small sample of ADRA's clients who were not part of the experimental sample. We came up with a list of 41 statements and asked 31 individuals to provide a yes/no answer in order to measure the prevalence rates of each statement. The questions were framed without a time horizon to be in line with the sensitive items on violence intended to measure prevalence rates in a woman's lifetime.

The prevalence rates of the non-sensitive statements were useful in two ways. On one hand, they measured the adequacy of the statements for our particular setting. Statements with prevalence rates too close to zero were discarded. On the other hand, the prevalence

[4]During the first three weeks of fieldwork, the randomization process was done by an offline version of the online platform we used to collect the data. Due to some complications with the software, which led some respondents to answer the two versions of the survey, we asked the surveyors to randomize using a pair of marbles from different colors during the rest of the fieldwork.

[5]Using the Peruvian DHS survey, we define the initial violence prevalence rates in the area studied. We decide to focus on one of the least frequently reported acts of violence, forced to have sexual relationships. Initial prevalence rate is set at 0.05 with a standard deviation of 0.2. With the randomization conducted at the individual level, a minimum detectable effect of 0.03 percentage points, a significance level of 10% and power of 0.8, the minimum sample size required was estimated at 550 per treatment arm.

rates helped us decide how to group the statements in sets of four in order to minimize ceiling effects and reduce the variance of the estimator [Glynn, 2013].[6] Table A.2 in the appendix shows the prevalence rates of the 34 statements we kept for the list experiments, after removing those with very small prevalence rates.[7] Table A.3 in the appendix reports the correlation of prevalence rates in each set of statements grouped together.

Compared to similar studies, a key advantage of our paper is a large sample size, which allows us to have separate questionnaires for the treatment and control groups.[8] This reduces potential biases that may be introduced when asking the same respondent both the direct and indirect questions as done in Karlan and Zinman [2012] and Joseph et al. [2017].

The control group replied to a questionnaire that had the module of direct questions on physical and sexual IPV presented before the list experiments section. Both modules were located right after the direct questions on emotional violence. In the treatment group, only the list experiment questions with the added sensitive statement were provided, right after the emotional violence questions (see Table 1). One may argue that the inclusion of the direct questions on physical and sexual IPV in the control group could have biased the responses to the rest of the questions in the survey, including answers to the lists of neutral statements. It could be that the mention of such a sensitive subject made the respondent relive or remember painful experiences and that this feeling lingered throughout the rest of the questionnaire, interfering with the thinking process to arrive to her answers. We argue that, in any case, both groups were somehow influenced by the preceding questions on emotional violence. Moreover, in Table A.4 in the Appendix we test for differences in the answers and non-response rates to the last module across treatment arms. The eight questions in this module refer to client's satisfaction with ADRA. In only one case the answers across treatment and control groups differ significantly but only at the 10% level.

[6]Based on the collected data on the correlation of responses across pairs of statements, we developed an algorithm that tried to induce negative correlation within the list of non-sensitive statements. First, we chose a grouping that minimized correlation between pairs of statements. Second, we grouped pairs of statements based on optimal negative correlations and checked the correlation in the full list was still negative.

[7]Two statements used in the final instrument were not tested in the pilot.

[8]See sample instruments in Appendix C.

Non-response rates are also similar and in only one out of the eight cases the treatment group is statistically less likely to respond. We acknowledge that this test is imperfect since the treatment group was differentially exposed to the IPV questions through the list experiments. For future extensions, we suggest to randomize the order of the direct and indirect questions on IPV in the control questionnaire.

Table 1: Structure of the questionnaire

<table>
<tr><th>Control</th><th>Treatment</th></tr>
<tr><td colspan="2">Consent form and introduction</td></tr>
<tr><td colspan="2">Demographics</td></tr>
<tr><td colspan="2">Memory test</td></tr>
<tr><td colspan="2">Direct questions about emotional violence</td></tr>
<tr><td>Direct questions about physical and sexual violence</td><td rowspan="2">Lists (5) with indirect questions about physical and sexual violence</td></tr>
<tr><td>Lists (4) with neutral statements</td></tr>
<tr><td colspan="2">Satisfaction with ADRA</td></tr>
</table>

Second, we carefully selected a team of female surveyors with previous experience on the topics of gender and gender biased violence. We invited them to a three-day training workshop and selected the top performers in the practice sessions. The workshop itself included a sensitization session provided by a local NGO, *Centro de la Mujer Peruana Flora Tristán*, which works on gender issues and women's empowerment.

Third, we tried to minimize the chances for misunderstanding or confusion when applying the instrument by providing the respondents with visual aids during the interview. Depending on the randomization outcome, the surveyor provided each respondent with a printed copy of the list experiment questions. This allowed respondents to follow the list of statements read to them and helped them remember the number of positive answers as they went along the list. We also tried to minimize potential biases in responses due to fears of having their individual answers revealed to ADRA. As shown in Appendix C.1, the consent form clearly stated that individual answers were not going to be shared with anyone outside

the research team.

Table 2 reports the prevalence rates of ever experiencing different violent acts as collected by DHS surveys. Prevalence of emotional violence against women was collected for the entire sample while only the control group answered the direct questions related to physical and sexual IPV. We included nine different acts of physical and sexual violence as inflicted by their actual or past partner: having her hair pulled; being pushed, shaken, or having something thrown at her; being slapped or having her arm twisted; being punched or hit with something that may have hurt her; being kicked or dragged; being strangled o burnt; being threatened with a knife, gun, or other weapon; being forced to have sex; and being forced to perform sex acts she does not approve of. Based on these DHS questions, we crafted nine corresponding sensitive statements to be added to the lists of neutral statements provided to the control.

Prevalence rates as measured by the direct questions are shockingly high in our sample. Almost 80% of the women in our sample have ever experienced any type of violence, either emotional or physical/sexual. Prevalence rates for any type of emotional violence are about 0.64, close to the 0.62 prevalence rate reported for any type of physical or sexual violent act. Not only are prevalence rates high but those who are victims of violent acts tend to suffer from it quite often as reported in the last column of Table 2.

3.3 Estimation

Let T_i denote the treatment assignment to the list experiment. Also, let D_i be equal to the number of statements that hold true for individual i, where $D_i = \sum_s^S d_{is}$ whenever i is assigned to the control group and $D_i = \sum_s^{S+1} d_{is}$ if i belongs to the treatment group. The difference-in-means estimator ρ approximates the prevalence rate of the sensitive statement included in the list provided to the treatment group:

$$D_i = \alpha + \rho T_i + \xi_i \tag{1}$$

Table 2: Prevalence rates of IPV

	All Sample		Sample w/violence	
	N	Prevalence rate	N	High frequency
Emotional IPV	1078	0.64		
Humiliate	1076	0.38	407	0.32
Insult	1074	0.35	373	0.33
Call lazy	1076	0.27	290	0.28
Threatens to harm	1076	0.15	162	0.38
Threatens to leave	1076	0.32	345	0.32
Physical and sexual IPV	560	0.62	.	.
Pull hair	560	0.31	170	0.24
Push	559	0.46	252	0.19
Slap	559	0.26	147	0.25
Punch	559	0.22	123	0.27
Kick	558	0.15	81	0.37
Strangle	560	0.06	30	0.33
Knife	560	0.06	32	0.22
Forced sex	559	0.23	127	0.36
Unapproved sex practices	558	0.09	51	0.37
IPV	560	0.78		

NOTE: The prevalence of IPV is measured as the prevalence rate of any type of violence, emotional or physical. Similarly, the prevalence of emotional (physical and sexual) IPV is measured as the prevalence of any type of emotional (physical and sexual) aggression. The last column reports the share of women who reported experiencing a given violent incident with high frequency.

Furthermore, let the reported prevalence rates under DHS methods[9] be denoted as p. Thus, we are interested in estimating the level of misreport between the list experiment and the DHS as measured by $(\rho - p)$ and in testing whether this difference is positive and statistically significant. Since the control and treatment groups are, on average, equivalent in terms of their true prevalence rates, $(\rho - p)$ signals the existence of underreporting.

The model estimated with list experiments data can be further extended to capture

[9]These surveys take into account all the ethical guidelines recommended by the World Health Organization to measure violence prevalence rates by having an enumerator ask face-to-face questions about whether the respondent has experienced a list of violent incidents.

prevalence rates for different sub-samples as defined by x_i:

$$D_i = \alpha + \rho T_i + \gamma x_i + \zeta(T_i \cdot x_i) + \xi_i \tag{2}$$

The term $(\rho + \zeta)$ captures the prevalence rate measured by experimental methods among individuals with $x_i = 1$ while ρ will measure the prevalence rate for those with $x_i = 0$.[10] Again, we can compare these prevalence rates to their counterpart measure obtained through direct reporting, p, conditional on x_i.

3.4 Results

Although we execute the nine list experiments to measure prevalence rates of physical and sexual IPV, we decide to analyze the data coming from only seven of these experiments. We drop the data for being pushed, shaken, or having something thrown at and being forced to have sex. Despite our efforts to group non-sensitive statements in a way that minimized ceiling effects and reduced the variance of the estimator, we faced some issues in the lists used in these two cases (see Appendix B for more details). For the remaining lists, we applied the test proposed by Blair and Imai [2012] where the null hypothesis is "no design effect". In all cases, we fail to reject the null at the 5% confidence level (results available upon request).

Our main goal is to measure if there are statistically significant differences in the report of violence across direct and experimental data collection methods. A positive gap between ρ and p would suggest he presence of underreporting.

Table 3 presents the estimated differences between indirect and direct reporting of experience of the different forms of physical and sexual IPV. Significance levels on the last column correspond to a Wald test of the difference between ρ and p for each act of IPV. The last two rows of the table report the results from a joint test of significance of the gap between ρ and p for the seven acts of violence analyzed.

[10] These are the multivariate regression estimators obtained under linearity in x_i and $(T_i \cdot x_i)$ as proposed in Blair and Imai [2012].

Table 3: Difference in estimated prevalence rates of physical and sexual IPV

Violent act	List experiments (ρ)	Direct reporting (p)	($\rho - p$)
Pull hair	0.418	0.311	0.107*
Slap	0.170	0.265	-0.094
Punch	0.174	0.224	-0.049
Kick	0.126	0.145	-0.019
Strangle	-0.022	0.055	-0.077
Knife	0.046	0.057	-0.011
Sex acts	0.052	0.095	-0.043
Joint test			
χ^2		8.12	
Prob $> \chi^2$		0.322	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Estimates of ρ are obtained from a regression that includes surveyor fixed effects as well as controls such as: household head dummy, age, civil status, education level, number of children, Spanish is the woman's mother tongue, working woman, literacy, tenure in ADRA, high average loan size, high savings balance, and an indicator of good memory. Estimates of p are obtained from a regression of the direct answer on a constant.

On average, the results in Table 3 suggest that direct questions used in health surveys do not introduce a bias in measuring the prevalence of violence when compared to experimental methods that provide anonymity to the respondent. For six out of seven acts of physical violence, the prevalence rates obtained through experimental methods do not significantly differ from those measured using direct DHS-type questions. Indeed, the joint test that the seven gaps are different from zero is rejected, providing little evidence to suspect of average reporting biases.

A note on non-response rates is worth including here. In the control group, the non-response rate for the IPV module with the direct questions is 5.4%. List experiments do not lead to a big difference in that respect: the non-response rate for the module with list experiments is 3.9% for the treatment group and close to null for the control group.

The lack of a significant difference in prevalence rates across reporting methods presented in Table 3 does not rule out the potential for misreporting among specific groups. More vulnerable groups with higher costs of being exposed could be more likely to truthfully report violence under the indirect method due to the provision of full confidentiality. We

next explore such potential outcomes relying on Equation (2).

Keep in mind that, although we are able to explore differential misreporting by characteristics of the respondent, our study was not designed to identify the forces that are driving the results. In other words, we slice the data in different ways to check if systematic misreporting is identified for the case of physical and sexual IPV. Since the costs of exposure are likely to vary by the level of economic and social empowerment, civil status, and the number of children of the victim, we designed the survey instrument to be able to test for differences across these characteristics. However, we remain agnostic as to how these costs vary according to the observable characteristics of the woman. For example, more economically empowered women may be more likely to report truthfully since they do not fear the loss of economic support of their partner. But they may also be more likely to underreport if the burden of stigmatization is greater among them.

We find evidence of misreporting among the most educated women in the sample. Table 4 shows that there are large positive gaps in the prevalence rates reported under indirect and direct methods in the group of women with complete tertiary education. The joint significance test of the gaps confirms that there is systematic misreporting in this group, which is not identified in the group of less educated women.

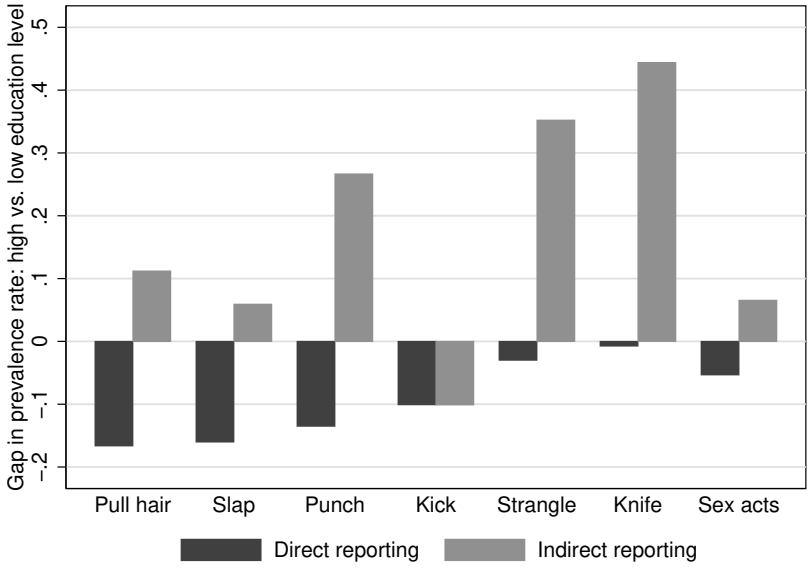
Interestingly, the measured bias among the most educated women is large enough to reverse the education gradient in violence. Figure 1 reports the difference in prevalence rates across the groups of high and low education levels for each reporting method. Under direct methods, this gap is negative for all seven acts of violence, implying that prevalence rates are higher for the least educated women. This negative correlation between education level and prevalence rates disappears once indirect methods are used. The gap in prevalence rates across education levels turns positive for all but one act of violence under indirect methods, revealing a positive correlation between education and experience of physical and sexual IPV. Once the costs of being exposed are minimized, women with complete tertiary education exhibit higher prevalence rates of physical and sexual IPV than less educated

Table 4: Difference in estimated prevalence rates of physical and sexual IPV by education level

	Less than tertiary education				Tertiary education			
Violent act	ρ	p	$(\rho - p)$		ρ	p	$(\rho - p)$	
Pull hair	0.398	0.340	0.058		0.510	0.173	0.336	**
Slap	0.160	0.293	-0.133	*	0.219	0.133	0.086	
Punch	0.126	0.247	-0.121		0.393	0.112	0.281	*
Kick	0.144	0.163	-0.019		0.043	0.062	-0.019	
Strangle	-0.086	0.061	-0.146	**	0.267	0.031	0.236	*
Knife	-0.034	0.058	-0.093		0.410	0.051	0.359	***
Sex acts	0.040	0.104	-0.065		0.105	0.051	0.054	
Joint test								
χ^2		10.62				22.02		
Prob $> \chi^2$		0.156				0.003		

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Estimates of ρ are obtained from a regression that includes surveyor fixed effects as well as controls such as: household head dummy, age, civil status, education level, number of children, Spanish is the woman's mother tongue, working woman, literacy, tenure in ADRA, high average loan size, high savings balance, and an indicator of good memory. Estimates of p are obained from a regression of the direct answer on a constant. Differential effects by education level are obtained from the model in (2).

Figure 1: Gap in IPV Prevalence Rates across Education Levels by Reporting Method



NOTE: The gap reported in each bar is the difference in prevalence rates across the groups of women with high and low education. High education level is defined as completed tertiary education.

women.

Surprisingly, no other measure of empowerment is correlated with significant biases in the report of violence at the 95% confidence level. Table 5 reports the joint significance tests that the bias in the seven acts of physical and sexual IPV is different from zero by sub-samples. While some modest differences emerge at the 10% in the sub-samples of single women and those with worse standing in ADRA (i.e., lower loan size, lower savings balance, and lower tenure), these do not seem to follow a clear pattern as in the case of education. Table A.6 in Appendix A shows that even though the biases are jointly and significantly different from zero, no specific bias among single women is statistically significant. Moreover, the differences identified by standing in ADRA seem to favor *overreporting* under direct methods among clients with worse standing, but this pattern is only barely significantly different from zero for two acts of violence (see Table A.11 in Appendix A).

We argue that the effect among more educated women is not capturing a better understanding of the list experiment questions since there are no significant biases for other characteristics that may proxy better understanding of the methodology (see Tables A.7 and A.8 in Appendix A). As mentioned above, putting forward an explanation for why education level is the only characteristic that generates systematic misreporting in our sample goes beyond the scope of this paper. Our goal is to use this case study to highlight potential problematic patterns of non-random misreporting in survey data. With the limited data collected in our survey and the lack of random variation in the characteristics of respondents, we cannot fully pin down the underlying sources of misreporting among more educated women. Nevertheless, below we try to provide an explanation for the differential importance of the costs of exposure by education level and present some suggestive evidence along those lines.

In most policy forums, women empowerment is considered a powerful tool to reduce the prevalence of IPV. However, both theoretical and empirical work show that the relationship between empowerment indicators such as education and the probability of being a victim of IPV is ambiguous. On one hand, greater access to information among more educated

Table 5: Joint significance test of $(\rho - p)$: Heterogeneous effects

	χ^2	Prob $> \chi^2$
Age		
<50	4.124	0.765
50+	8.219	0.314
Civil status		
Single	13.436	0.062
Married	4.318	0.742
Education level		
Less than tertiary	10.617	0.156
Completed tertiary	22.018	0.003
Mother tongue		
Spanish	10.934	0.142
Other language	7.306	0.398
Memory test		
Low score	3.993	0.781
High score	6.598	0.472
Household head		
Not the head	8.781	0.269
Head	4.729	0.693
Employment		
Does not work	6.218	0.515
Works	6.481	0.485
Standing in ADRA		
Young client	13.30	0.065
Mature client	6.64	0.467

NOTE: Joint test that the seven biases are different from zero. See Table 4 for details about the regressions. Mature clients are those with loan size and savings balance above the 75th percentile and a tenure greater than two loan cycles.

women may change their attitudes towards social and gender norms, which can make them less tolerant of male dominance and violent behavior at home. Moreover, under assortative matching, women with more years of schooling are more likely to find partners who are also more educated and exposed to more equal social and gender norms.

On the other hand, greater returns and better access to job market opportunities among highly educated women may lead to different equilibria within the household. Intra-household bargaining models predict that, as long as education increases their outside option, more educated women should see violence experience reduced when compared to less educated ones [Farmer and Tiefenthaler, 1996]. However, instrumental theories of IPV highlight the use of violence by men in order to control resources at home [Eswaran and Malhotra, 2011]. Depending on the context, this backlash effect may undo the positive effects of empowerment through education on IPV.

In our sample, prevalence rates estimated through indirect methods show a negative relationship between education level and IPV experience. The only negative channel that can explain this pattern is the presence of a backlash effect. Indeed, Table A.12 exhibits the presence of gender norms that favor gender equality in highly educated women's households. We also see that women with complete tertiary education have partners that treat them better and are less controlling (see Table A.13). More educated women in our sample are also 5.8 percentage points more likely to work, which is suggestive of better job market opportunities available to them. The negative gradient on education that we uncover can only be explained by a greater propensity of partners to exert violence as a way to extract additional available resources, which counteracts the positive expected effects of empowerment through education.

Now, what makes it more costly for highly educated women in our sample to expose their partners? First, there is no reason to believe that emotional attachment to the partner should be differential across education levels. Second, more educated women should fear *less* the potential loss of their partners' economic support, which would make them more

prone to be honest when reporting directly. We speculate that both stigma concerns and fear of retaliation could be greater burdens when reporting directly among the more educated. Exposure to more equal gender norms increases the costs imposed by stigma [11]. Moreover, the backlash effect can make fear of retaliation more intense [e.g., Macmillan and Gartner, 1999].

4 Non-Classical Measurement Error in the Outcome

Our results show that, on average, there is no evidence of misreporting of physical and sexual IPV experience. However, the provision of anonymity through list experiments exposes the presence of non-classical measurement error. More educated women underreport when using DHS-type direct questions, the current gold standard and the most common way to measure violence in applied research.

This finding has extremely important implications on the empirical literature that tries to identify the main drivers and triggers of violence against women. In a context where evidence is increasingly being used to move into action in the policy arena, our results are particularly important as they show that targeting strategies and prevention and mitigation programs may be designed with the wrong parameters in mind.

4.1 The Data Generating Process

To understand the implications of the presence of non-classical error in the measurement of an outcome, we consider a simple model. Suppose that a researcher wants to estimate β in the following model:

$$y_i = \beta x_i + \epsilon_i \qquad i = 1, \ldots, N. \tag{3}$$

[11] See, [Lindbeck et al., 1999] for an example of how social norms and stigma are related in the case of welfare state.

In our particular case of interest, y_i would capture a measure of IPV and x_i would represent women's education, her income, or any other "risk factor" explored in the literature. The error term is assumed to distribute $N(0,1)$. For simplicity, (3) assumes that y_i and x_i are measured in deviations from the mean and ignores the role that other variables can play in explaining violence against women.[12]

Now consider a case when y_i is measured with some noise. The researcher observes $\tilde{y_i}$ instead if the true value y_i:

$$\tilde{y_i} = y_i + \omega_i$$

Let x_i be measured without error and define it as follows:

$$x_i = \gamma\epsilon_i + \tau_i$$

That is, the risk factor is correlated with ϵ_i whenever $\gamma \neq 0$, introducing endogeneity in the estimation of β. Let $\tau_i \sim N(0,\kappa)$ so that $\text{var}(\tau_i) = \kappa\text{var}(\epsilon_i)$.

Now, we model measurement error as a mix between a classical component and a non-classical one:

$$\omega_i = \phi x_i + \nu_i \tag{4}$$

where $\nu_i \sim N(0,1)$.

4.2 Causal Estimation under Endogeneity and Measurement Error Biases

Whenever x_i is correlated with ϵ_i ($\gamma \neq 0$) and measurement error is non-classical ($\phi \neq 0$), then $E(\omega_i) = 0$. However, two types of biases are introduced in the estimation of β using cross-sectional data:

[12]Bound et al. [1994] provide a general framework where x_i is a vector instead of a scalar.

$$\begin{aligned}\hat{\beta}_{\text{OLS}} &= \beta + \frac{\text{cov}(\epsilon_i, x_i)}{\text{var}(x_i)} + \frac{\text{cov}(\omega_i, x_i)}{\text{var}(x_i)} \\ &= \beta + \gamma \frac{\text{var}(\epsilon_i)}{\text{var}(x_i)} + \phi \end{aligned} \tag{5}$$

where the second term in (5) captures the endogeneity bias and the third one corresponds to the non-classical measurement error bias.

4.3 Implications on Current Evidence

Several papers in the literature have tried to estimate (3) via ordinary least squares using only cross-sectional variation to identify the impact of risk factors on violence against women.[13] More recent papers tried to deal with the limitations of this approach by trying to reduce or eliminate the endogeneity bias. One of the most common approaches has been the use of exogenous variations coming from RCTs, specially those providing conditional cash transfers (CCT) to women as part of antipoverty programs.[14] Other studies have tried to look at the impact of social norm interventions provided under an experimental design (see Pronyk et al. [2006] and World Health Organization [2009]). Another common strategy to deal with endogeneity problems is the use IV techniques. For example, Erten and Pinar [forthcoming] use a school reform in Turkey as an instrument to evaluate the impact of women's education on the prevalence of violence.

By introducing random (or exogenous) variation in x_i, these papers are able to convincingly set $\gamma \frac{\text{var}(\epsilon_i)}{\text{var}(x_i)} = 0$. However, if x_i in itself makes women more likely to misreport violence, the bias stemming from measurement error does not go away. This is very likely to occur in the context of CCT programs since the cash transfer tends to come within a bundle of other

[13] See Jewkes et al. [2002], Koenig et al. [2003], Breiding et al. [2008], Fulu et al. [2013], where demographic and socioeconomic variables are considered among a long list possible risk factors. See Capaldi et al. [2012] for a recent review.

[14] See Hidrobo and Fernald [2013], Hidrobo et al. [2016], Haushofer and Shapiro [2013], Angelucci [2008], and Bobonis et al. [2013], among others. See also De Koker et al. [2014] for a review of RCT papers in the United States.

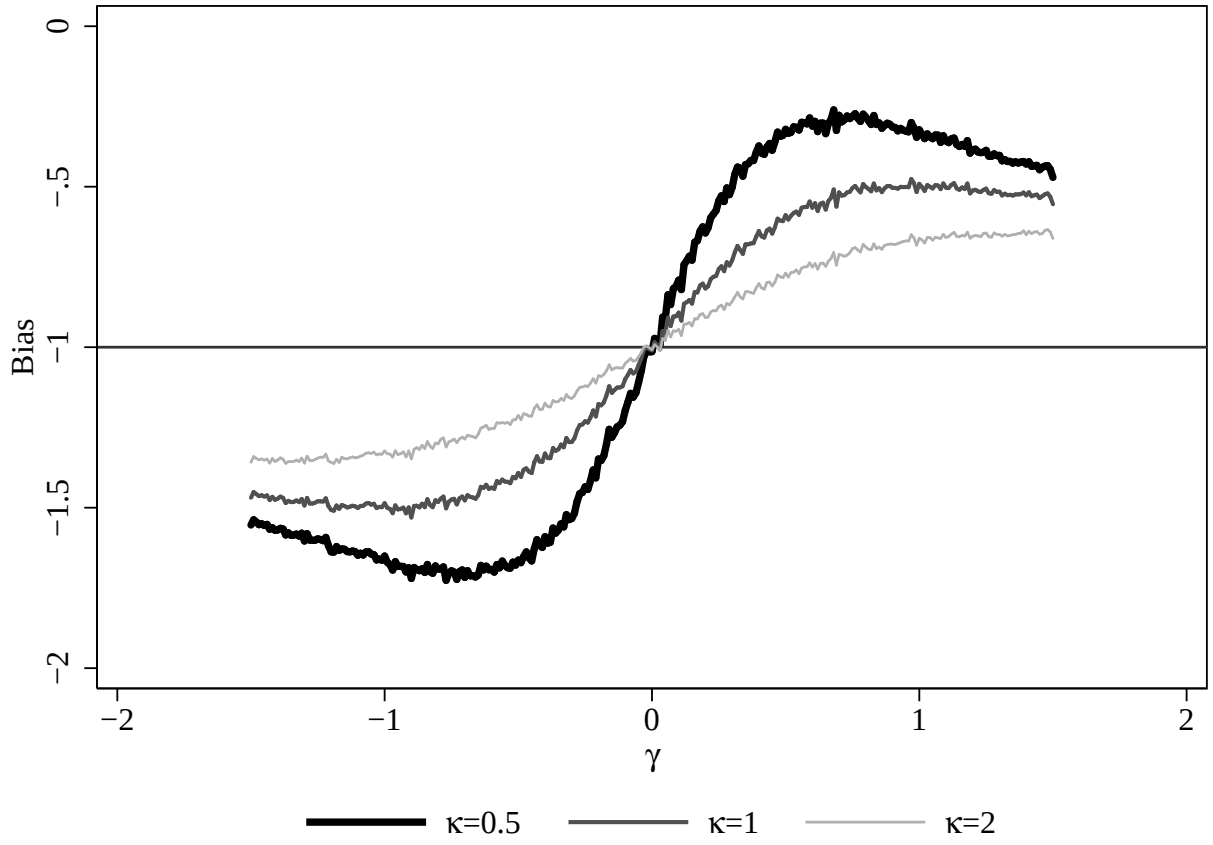
program components that may provide the recipient with information, changes in what is socially acceptable, or changes in the costs of being exposed. The same applies to education as the increase in human capital could translate into access to more information, exposure to different social norms, better access to labor market opportunities, to name a few of the factors that may affect the report of IPV.

Thus, non-classical measurement error imposes a limit to the gains that randomization or IV provide to obtain less biased estimates of treatment effects. Since ϕ in (5) does not go away under these methodologies, the estimate of β could be still far off from its true value. In fact, OLS may yield *less* biased estimates of β whenever the sign of the correlation between x_i and ϵ_i is opposite to that of the correlation between x_i and ω_i. For instance, if education creates a stigma so that more educated women underreport violence ($cov(\omega_i, x_i) < 0$), as shown in our list experiments, but education is positively correlated with unobserved ability, as expected in human capital models (e.g., Card [2001]), the two biases partially cancel out.

We conduct simulations relying on the data generating process outlined in sub-section 4.1 to provide a better sense of the conditions that yield less biased estimates of β in the case of OLS when compared to RCTs and IVs. In Figure 2, we set the non-classical measurement error that remains in RCT and IV methods to -1 ($\phi = -1$) and plot the bias obtained through OLS for different values of κ and γ. Clearly, a necessary condition to get smaller biases under OLS than RCTs and IVs is that γ and ϕ have opposite signs. Moreover, $\gamma \frac{\text{var}(\epsilon_i)}{\text{var}(x_i)} - \phi$ becomes close to zero whenever γ increases relative to ϕ, and more so whenever κ is higher. That is, with observational data and when γ and ϕ have opposite signs, the best a researcher can hope for is that endogeneity levels are worrisome (low κ) so that they can neutralize the non-classical measurement error.

We argue that the list experiment used in our study provides an inexpensive way to directly measure ϕ and correct biased estimates from RCTs or IV methods. Based on the study's budget and sample size, the cost per women to conduct our experiment was close to US$8. For projects already conducting fieldwork, as those implementing a RCT, the cost

Figure 2: Bias in OLS estimates ($\gamma \frac{\text{var}(\epsilon_i)}{\text{var}(x_i)} + \phi$) with $\phi = -1$



of adding the questions required to conduct a list experiment is even smaller. From (4), notice that ϕ is the slope of the relation between the risk factor of interest (x_i) and the measurement error in the dependent variable (ω_i). By conducting an experiment similar to ours researchers can directly estimate ω_i and obtain ϕ by correlating it with x_i. This will allow them to compute the bias in their estimates of β. In the next section we show how to estimate ϕ in the presence of non-linear (and non-classical) measurement error in the dependent variable.

4.4 Non-Linear Measurement Error

In the previous section, we consider the possibility of a linear source of non-classical measurement error as in Blattman et al. [2016]. We extend this case to consider non-linear and non-classical measurement error as the one we identify in our sample. We redefine the measurement error introduced in equation (4) as follows:

$$\omega_i = \pi_i(\phi x_i + \nu_i) + (1 - \pi_i)(\nu_i) \tag{6}$$

where $\pi_i = I[x_i > \mu_x]$ and $\mu_x = \bar{\mu}$. In this case, measurement error in the dependent variable is related to x_i in a non-linear way. As in our case study, the indicator function activates whenever the woman has completed tertiary education, i.e., has accumulated years of schooling above $\bar{\mu}$.

In this new framework, the OLS estimator of β becomes:

$$\begin{aligned} \beta_{OLS} &= \beta + \gamma\frac{\text{var}(\epsilon_i)}{\text{var}(x_i)} + \phi\frac{\text{cov}(x_i, \pi_i x_i)}{\text{var}(x_i)} \\ &= \beta + \gamma\frac{\text{var}(\epsilon_i)}{\text{var}(x_i)} + \phi E(\pi_i) \end{aligned} \tag{7}$$

Thus, when the measurement error is not linear, the bias of the OLS estimator still depends on ϕ as before but now it is also affected by the relative size of the group that generates non-classical measurement error. As an example, we provide an estimate of the bias remaining when estimating treatment effects of college education on IPV using RCT or IV methods. Using the findings from Table 4 and the fact that 17.5 percent of the women in our sample completed college, we can estimate ϕ for a given act of IPV during a woman's lifetime: the bias due to measurement error in β is 0.049 ((0.336-0.058)*0.175) in the case of having her hair pulled and 0.079 in the case of being attacked with a knife. Although we have no way to pin down the bias due to endogeneity, we provide $\hat{\beta}_{OLS}$ corresponding to education level in the case of these two acts of violence in our sample as a reference: -0.143

and 0.009 for having her hair pulled and being attacked with a knife, respectively.

5 Conclusion

Our paper uses indirect questioning methods to measure misreporting in sensitive topics. In particular, we study the case of physical and sexual IPV as committed by the woman's last partner and rely on list experiments to provide full anonymity in its report.

We are the first to measure misreporting of IPV when direct health survey questions, the current gold standard, are used. We find that, on average, there are no significant differences in direct versus indirect reporting. Furthermore, our results show that underreporting in our sample is concentrated among women with complete tertiary education, who do not fit the typical victim stereotype. This has important implications on the invisibility of violence that certain groups may suffer and the targeting efforts conducted to prevent and combat IPV. More educated women seem to face larger costs of being exposed and thus require higher levels of privacy and confidentiality to make them feel safe enough to report victimization truthfully. Since this pattern is not identified among more empowered women as measured by other proxies, we speculate that more educated women are more prone to face higher stigma costs and greater fear of retaliation related to a backlash effect.

Our contribution goes beyond our particular application to IPV. When (quasi) random assignment in the risk factor is introduced, non-classical measurement error in the dependent variable biases the estimates of treatment effects. We show that under certain conditions, randomization (and instrumental variables) could lead to even larger biases compared to cross-sectional studies. We provide a solution to correct biased causal effects under the presence of non-classical measurement error in the dependent variable. Paired with instrumental variable techniques or randomized controlled trials that deal with endogeneity biases, our approach offers the potential to estimate unbiased treatment effects at a very low cost.

We acknowledge that the external validity of our results is limited. However, in a setting

with high prevalence rates, such as the one studied here, it would have been more difficult to identify underreporting since the local social norms could be more accepting of violence. But even in this setting we are able to find evidence of misreporting for a particular group. Further research should explore whether the misclassification is larger in areas with lower prevalence rates and if the heterogeneous effects vary by context. This is particularly urgent given the growing number of studies on IPV that try to estimate treatment effects with outcome variables that seem to be systematically misreported.

For studies examining the impact of risk factors on violence against women as well as for studies analyzing any other sensitive behavior in settings where administrative records are not reliable, we advocate for the inclusion of list experiment questions in the survey instruments used by researchers during data collection efforts. This will allow them to measure the magnitude of the bias in the estimated treatment effects introduced by non-classical measurement error based on the risk factor of interest.

It is worth highlighting that our design was implemented at a very low cost: we were able to collect 1221 surveys at a cost of US$8 per woman. This means that there are potentially important savings from this method when compared to other procedures [Blattman et al., 2016] that require intensive qualitative approaches. This opens up the possibility to replicate our design with other samples with different contextual characteristics.

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A Additional Figures and Tables

Table A.1: Summary Statistics and Balance Check

	Control	(T-C)	N
Demographic Characteristics			
Age	43.825	0.903	1078
	[11.604]	[0.693]	
Married	0.798	-0.007	1078
	[0.402]	[0.025]	
Literate	1.959	0.002	1078
	[0.199]	[0.012]	
Spanish is not mother tongue	0.114	0.019	1078
	[0.318]	[0.020]	
Household head	0.313	0.07	1078
	[0.464]	[0.029]**	
Works	0.73	0.005	1078
	[0.444]	[0.027]	
Less than complete primary	0.109	0.017	1078
	[0.312]	[0.020]	
Primary education	0.266	-0.036	1078
	[0.442]	[0.026]	
Secondary education	0.45	-0.019	1078
	[0.498]	[0.030]	
Higher education	0.175	0.039	1078
	[0.380]	[0.024]	
Number of children	2.987	-0.013	1076
	[1.891]	[0.102]	
Number of children under 12 under her care	0.897	-0.025	1060
	[1.641]	[0.083]	
Memory test: % words remembered right after	0.85	0.026	1078
	[0.357]	[0.021]	
Memory test: % words remembered at the end	0.489	0.038	1078
	[0.500]	[0.030]	
Always lived in current locality	0.632	-0.028	1078
	[0.483]	[0.030]	
Financial Situation			
Average loan size in past 4 cycles	1552.664	8.921	1025
	[1178.413]	[72.065]	
Average savings balance in past 4 cycles	791.688	77.259	1025
	[861.449]	[63.958]	
High loan size and high savings balance	0.284	0.038	1078
	[0.451]	[0.028]	
Partner's characteristics			

Continued on next page

	Control	(T-C)	N
Jealous when speaking to other men	0.979	0.195	1077
	[7.224]	[0.488]	
Accuses her of being unfaithful	0.452	0.521	1078
	[4.196]	[0.420]	
Prevents her from visiting or being visited by friends	0.801	-0.203	1077
	[7.233]	[0.408]	
Limits contact with family	1.096	-0.511	1078
	[9.310]	[0.477]	
Wants to know where she is at all times	0.828	-0.34	1077
	[5.909]	[0.251]	
Does not trust her with money	0.428	0.374	1077
	[4.199]	[0.375]	
Humiliates her in public	0.555	0.018	1078
	[4.196]	[0.261]	
Calls her ignorant or idiot	0.538	0.37	1078
	[4.196]	[0.375]	
Calls her lazy, useless, or sleepy	0.45	0.006	1078
	[4.196]	[0.261]	
Threatened to harm her or someone close to her	0.512	-0.368	1078
	[5.913]	[0.250]	
Threatened to leave, take children, or cut off financial support	0.68	-0.362	1078
	[5.910]	[0.251]	
Survey Application			
Interruption by men	0.045	0	1078
	[0.207]	[0.013]	
Interruption by partner	0.007	-0.003	1078
	[0.084]	[0.004]	
Presence partner	0.018	-0.006	1078
	[0.133]	[0.007]	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.2: Prevalence rates of non-sensitive statements in the pilot

Have you ever	Mean	S.D.
made improvements to your dwelling?	0.774	0.425
traveled with your family on vacation? *	0.613	0.495
seen any soap opera? **	1.000	0.000
lost your cell phone? **	0.645	0.486
reared farm animals for consumption?	0.613	0.495
felt insecure in your neighborhood?	0.710	0.461
paid rent for the place where you live?	0.548	0.506
run out of money to cover the household's monthly expenses?	0.710	0.461
bought any high-end clothes?	0.290	0.461
been part of a Christian church?	0.484	0.508
purchased a TV with HD?	0.290	0.461
witnessed robberies in your neighborhood?	0.516	0.508
been robbed on the street?	0.516	0.508
seen *Al fondo hay sitio*? * [a/]	0.903	0.301
had to truncate your studies to care for your family?	0.742	0.445
pursued a technical degree?	0.387	0.495
read *El Comercio*? ** [b/]	0.645	0.486
helped your children with their homework?	0.968	0.180
participated in other microfinance programs?	0.645	0.486
had multiple businesses at the same time?	0.387	0.495
experienced that your business' sales are insufficient to cover your household expenses?	0.516	0.508
had insurance from ESSALUD, the armed forces or the police?	0.323	0.475
suffered from a serious medical condition that has required medical assistance?	0.677	0.475
bought expensive clothes?	0.226	0.425
traveled with your children?	0.839	0.374
played any games on your cell phone? *	0.290	0.461
visited the cathedral of Lima? **	0.677	0.475
used the subway as a means of transportation?	0.290	0.461
traveled with your friends?	0.323	0.475
participated in a committee or association in your neighborhood?	0.548	0.506
been to the movies with your family?	0.452	0.506
been out for a walk with your children?	0.968	0.180
bought new clothes for your children on important dates (Christmas, birthdays, etc.)? *	0.968	0.180
had problems with your partner because of money issues?	0.839	0.374

NOTES: * These statements are the ones in the 2nd list experiment question (push). ** These statements are the ones in the 8th list experiment question (forced sex).

[a/] *Al fondo hay sitio* is a very popular soap opera than run for several years in Peru.

[b/] *El Comercio* is one of the most read newspapers in the country, particularly in Lima.

Table A.3: Correlation of prevalence rates among non-sensitive statements

	1a	1b	1c	1d
1a	1.00			
1b	-0.29	1.00		
1c	0.12	-0.03	1.00	
1d	0.33	0.10	-0.34	1.00

	2a	2b	2c	2d
2a	1.00			
2b	-0.29	1.00		
2c	-0.08	0.23	1.00	
2d	-0.03	-0.06	-0.26	1.00

	3a	3b	3c	3d
3a	1.00			
3b	-0.29	1.00		
3c	-0.12	-0.16	1.00	
3d	0.34	-0.29	-0.35	1.00

	4a	4b	4c
4a	1.00		
4b	-0.29	1.00	
4c	0.25	-0.02	1.00

	5a	5b	5c	5d
5a	1.00			
5b	-0.37	1.00		
5c	-0.07	0.22	1.00	
5d	-0.06	-0.07	-0.37	1.00

	6a	6b	6c	6d
6a	1.00			
6b	-0.28	1.00		
6c	-0.23	-0.10	1.00	
6d	-0.05	0.14	-0.31	1.00

	7a	7b	7c	7d
7a	1.00			
7b	-0.54	1.00		
7c	0.15	0.03	1.00	
7d	0.09	-0.13	-0.28	1.00

	8a	8b	8c	8d
8a	1.00			
8b	-0.13	1.00		
8c	-	-	-	
8d	0.07	0.50	-	1.00

	9a	9b	9c
9a	1.00		
9b	-0.24	1.00	
9c	-0.04	-0.11	1.00

NOTE: Questions 4 and 9 include only 3 statements because the fourth one used in these questions did not come from the list of statements tested in the pilot. In question 8, statement c had a prevalence rate of 1.

Table A.4: Difference in Responses and Non-Response Rates to the Last Module Across Treatment Arms

	Control	(T-C)	N
Differences in answers			
Satisfied with training	0.813	0.008	1077
	[0.391]	[0.024]	
Satisfied with family talks	0.834	0.014	1076
	[0.373]	[0.022]	
Satisfied with sports events	0.592	-0.025	1076
	[0.492]	[0.030]	
Satisfied with loans	0.871	-0.007	1076
	[0.335]	[0.021]	
Likely to stay in VB	0.793	-0.024	1068
	[0.405]	[0.025]	
Likely to recommend ADRA to others	0.953	-0.025	1076
	[0.211]	[0.014]*	
Likely to assume role in VB committee	0.494	0.031	1073
	[0.500]	[0.031]	
Differences in no-response rates			
Satisfied with training	0	0.002	1078
	[0.000]	[0.002]	
Satisfied with family talks	0.002	0	1078
	[0.042]	[0.003]	
Satisfied with sports events	0.002	0	1078
	[0.042]	[0.003]	
Satisfied with loans	0	0.004	1078
	[0.000]	[0.003]	
Likely to stay in VB	0.007	0.004	1078
	[0.084]	[0.006]	
Likely to recommend ADRA to others	0.002	0	1078
	[0.042]	[0.003]	
Likely to assume role in VB committee	0.005	-0.001	1078
	[0.073]	[0.004]	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A.5: Difference in estimated prevalence rates of physical and sexual IPV by age

	< 50 years old			50+ years old		
Violent act	ρ	p	$(\rho - p)$	ρ	p	$(\rho - p)$
Pull hair	0.386	0.304	0.082	0.477	0.324	0.153
Slap	0.151	0.251	-0.100	0.206	0.293	-0.087
Punch	0.185	0.213	-0.028	0.155	0.245	-0.090
Kick	0.115	0.124	-0.009	0.146	0.187	-0.041
Strangle	0.023	0.048	-0.026	-0.105	0.069	-0.174
Knife	0.007	0.048	-0.042	0.118	0.074	0.044
Sex acts	0.011	0.059	-0.048	0.127	0.166	-0.039
Joint test						
χ^2		4.12			8.22	
Prob > χ^2		0.765			0.314	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Estimates of ρ are obtained from a regression that includes surveyor fixed effects as well as controls such as: household head dummy, age, civil status, education level, number of children, Spanish is the woman's mother tongue, working woman, literacy, tenure in ADRA, high average loan size, high savings balance, and an indicator of good memory. Estimates of p are obained from a regression of the direct answer on a constant. Differential effects by education level are obtained from the model in (2).

Table A.6: Difference in estimated prevalence rates of physical and sexual IPV by civil status

	Single			Married		
Violent act	ρ	p	$(\rho - p)$	ρ	p	$(\rho - p)$
Pull hair	0.547	0.345	0.201	0.386	0.302	0.084
Slap	0.195	0.354	-0.159	0.164	0.242	-0.078
Punch	0.144	0.336	-0.193	0.182	0.195	-0.013
Kick	0.263	0.214	0.049	0.092	0.128	-0.036
Strangle	0.039	0.133	-0.094	-0.037	0.036	-0.073
Knife	0.072	0.097	-0.025	0.039	0.047	-0.008
Sex acts	0.106	0.133	-0.026	0.038	0.085	-0.047
Joint test						
χ^2		13.44			4.32	
Prob > χ^2		0.062			0.742	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Estimates of ρ are obtained from a regression that includes surveyor fixed effects as well as controls such as: household head dummy, age, civil status, education level, number of children, Spanish is the woman's mother tongue, working woman, literacy, tenure in ADRA, high average loan size, high savings balance, and an indicator of good memory. Estimates of p are obained from a regression of the direct answer on a constant. Differential effects by education level are obtained from the model in (2).

Table A.7: Difference in estimated prevalence rates of physical and sexual IPV by mother's tongue

	Spanish				Other language		
Violent act	ρ	p	$(\rho - p)$		ρ	p	$(\rho - p)$
Pull hair	0.444	0.315	0.129	*	0.239	0.281	-0.043
Slap	0.142	0.258	-0.116	*	0.368	0.317	0.050
Punch	0.138	0.216	-0.078		0.423	0.281	0.142
Kick	0.083	0.138	-0.055		0.426	0.203	0.223
Strangle	-0.048	0.054	-0.103		0.160	0.063	0.098
Knife	0.057	0.056	0.000		-0.030	0.063	-0.092
Sex acts	0.044	0.083	-0.038		0.103	0.190	-0.088
Joint test							
χ^2		10.93				7.31	
Prob $> \chi^2$		0.142				0.398	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Estimates of ρ are obtained from a regression that includes surveyor fixed effects as well as controls such as: household head dummy, age, civil status, education level, number of children, Spanish is the woman's mother tongue, working woman, literacy, tenure in ADRA, high average loan size, high savings balance, and an indicator of good memory. Estimates of p are obained from a regression of the direct answer on a constant. Differential effects by education level are obtained from the model in (2).

Table A.8: Difference in estimated prevalence rates of physical and sexual IPV by memory

	Bad memory				Good memory		
Violent act	ρ	p	$(\rho - p)$		ρ	p	$(\rho - p)$
Pull hair	0.477	0.350	0.127		0.362	0.270	0.092
Slap	0.253	0.262	-0.009		0.091	0.267	-0.176 *
Punch	0.247	0.248	-0.001		0.105	0.198	-0.093
Kick	0.165	0.155	0.011		0.088	0.135	-0.047
Strangle	0.006	0.063	-0.057		-0.049	0.047	-0.096
Knife	0.061	0.073	-0.013		0.032	0.040	-0.008
Sex acts	0.137	0.116	0.021		-0.029	0.073	-0.102
Joint test							
χ^2		3.99				6.60	
Prob $> \chi^2$		0.781				0.472	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Estimates of ρ are obtained from a regression that includes surveyor fixed effects as well as controls such as: household head dummy, age, civil status, education level, number of children, Spanish is the woman's mother tongue, working woman, literacy, tenure in ADRA, high average loan size, high savings balance, and an indicator of good memory. Estimates of p are obained from a regression of the direct answer on a constant. Differential effects by education level are obtained from the model in (2).

Table A.9: Difference in estimated prevalence rates of physical and sexual IPV by household head status

	Household head				Not the household head		
Violent act	ρ	p	$(\rho - p)$		ρ	p	$(\rho - p)$
Pull hair	0.455	0.275	0.180	**	0.348	0.389	-0.040
Slap	0.174	0.240	-0.066		0.163	0.320	-0.157
Punch	0.136	0.197	-0.061		0.246	0.282	-0.035
Kick	0.131	0.112	0.019		0.117	0.218	-0.102
Strangle	-0.012	0.026	-0.038		-0.041	0.120	-0.161
Knife	0.057	0.034	0.023		0.025	0.109	-0.083
Sex acts	0.024	0.065	-0.041		0.103	0.160	-0.057
Joint test							
χ^2		8.78				4.73	
Prob $> \chi^2$		0.269				0.693	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Estimates of ρ are obtained from a regression that includes surveyor fixed effects as well as controls such as: household head dummy, age, civil status, education level, number of children, Spanish is the woman's mother tongue, working woman, literacy, tenure in ADRA, high average loan size, high savings balance, and an indicator of good memory. Estimates of p are obained from a regression of the direct answer on a constant. Differential effects by education level are obtained from the model in (2).

Table A.10: Difference in estimated prevalence rates of physical and sexual IPV by employment

	Does not work			Works		
Violent act	ρ	p	$(\rho - p)$	ρ	p	$(\rho - p)$
Pull hair	0.468	0.351	0.117	0.400	0.296	0.104
Slap	0.207	0.272	-0.065	0.157	0.262	-0.105
Punch	0.339	0.252	0.087	0.114	0.213	-0.099
Kick	0.070	0.185	-0.116	0.146	0.130	0.016
Strangle	0.014	0.086	-0.072	-0.035	0.044	-0.079
Knife	0.062	0.066	-0.004	0.040	0.054	-0.014
Sex acts	0.039	0.113	-0.073	0.056	0.088	-0.032
Joint test						
χ^2		6.22			6.48	
Prob $> \chi^2$		0.515			0.485	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Estimates of ρ are obtained from a regression that includes surveyor fixed effects as well as controls such as: household head dummy, age, civil status, education level, number of children, Spanish is the woman's mother tongue, working woman, literacy, tenure in ADRA, high average loan size, high savings balance, and an indicator of good memory. Estimates of p are obained from a regression of the direct answer on a constant. Differential effects by education level are obtained from the model in (2).

Table A.11: Difference in estimated prevalence rates of physical and sexual IPV by standing in ADRA

	Young client				Mature client		
Violent act	ρ	p	$(\rho - p)$		ρ	p	$(\rho - p)$
Pull hair	0.408	0.309	0.099		0.466	0.322	0.144
Slap	0.147	0.279	-0.133	*	0.282	0.189	0.093
Punch	0.194	0.237	-0.042		0.081	0.156	-0.075
Kick	0.114	0.152	-0.038		0.180	0.111	0.069
Strangle	-0.061	0.062	-0.122	*	0.158	0.022	0.135
Knife	0.091	0.064	0.027		-0.162	0.022	-0.184
Sex acts	0.023	0.090	-0.067		0.186	0.122	0.064
Joint test							
χ^2		13.30				6.64	
Prob $> \chi^2$		0.065				0.467	

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. OLS estimates. Mature clients are those with loan size and savings balance above the 75th percentile and a tenure greater than two loan cycles. Estimates of ρ are obtained from a regression that includes surveyor fixed effects as well as controls such as: household head dummy, age, civil status, education level, number of children, Spanish is the woman's mother tongue, working woman, literacy, tenure in ADRA, high average loan size, high savings balance, and an indicator of good memory. Estimates of p are obained from a regression of the direct answer on a constant. Differential effects by education level are obtained from the model in (2).

Table A.12: Partner's Participation in Household Chores, by Women's Education Level

Husband helps with...	Less than tertiary education	Tertiary education	Difference	
Laundry	0.18	0.31	-0.13	***
Preparing meals	0.11	0.18	-0.07	***
Minor repairs	0.55	0.60	-0.05	*
Taking care of the family	0.36	0.45	-0.09	***
Taking care of the sick	0.27	0.35	-0.08	**
Cleaning	0.20	0.32	-0.12	***

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. Means reported correspond to the proportion of households in which partner collaborates with given chore. Significance levels obtained from a two-sample t test.

Table A.13: Partner Behavior, by Women's Education Level

Husband...	Less than tertiary education	Tertiary education	Difference	
Get jealous when she talks to men	0.44	0.41	0.02	
Accuses her of being unfaithful	0.27	0.15	0.12	***
Stops her from seeing friends	0.27	0.16	0.11	***
Limits visits/contacts with family	0.23	0.13	0.10	***
Always wants to know where she is	0.49	0.45	0.04	
Does not trust her with money	0.26	0.15	0.11	***

NOTE: * significant at 10%; ** significant at 5%; *** significant at 1%. Means reported correspond to the proportion of households in which partner behaves as stated. Significance levels obtained from a two-sample t test.

B Ceiling Effects

Although using a very small sample (31 observations), the pilot data allows us to measure the prevalence of each non-sensitive statement before designing the list experiments. Relying on this data, we grouped statements in sets of 4 while trying to minimize ceiling effects and reduce the variance of the estimator (see sub-section 3.2). Since we had to construct 9 sets of 4 non-sensitive statements simultaneously, we relied on an algorithm that tried to minimize these two problems for the 9 sets of statements altogether. Thus, the final grouping we obtained may have been more conducive to generate ceiling effects in certain questions.

In particular, we believe that there may be a higher propensity to yield ceiling effects in the questions related to push and forced sex. Table B.1 reports some statistics on the prevalence rates of the sets of non-sensitive statements with data from the pilot. The first column reports the mean prevalence of the 4 statements, while the second and third report the standard deviation and the 75th percentile of this 4 prevalence rates. The non-sensitive statements grouped with the sensitive ones on pushing and forced sex have very high average prevalence rates and low variance. Moreover, the 75th percentile of prevalence rates for these sets of 4 statements is very high, which shows that many statements in these groups have high prevalence rates. In fact, one of the statements grouped with forced sex has a prevalence rate of 1 ("ever watched a soap opera").

In what follows, we discard the results on these two acts of violence. We focus on the acts of violence related to the other seven list experiment questions that seem more robust to biases in the instrument design.

Table B.1: Prevalence of 4 non-sensitive statements by question

	Distribution of prevalence		
Statements grouped with:	Mean	SD	p(75)
Slap	0.419	0.083	0.484
Kick	0.500	0.194	0.661
Knife	0.508	0.152	0.597
Pull Hair	0.613	0.411	0.968
Push	0.694	0.310	0.935
Strangle	0.694	0.150	0.790
Forced sex	0.742	0.173	0.839

NOTE: Columns 1-3 report means, standard deviations, and the 75th percentile for the prevalence rates of each sample of 4 non-sensitive statements. Only 3 out of the 4 statements grouped with punch and sex acts come from the pilot and are thus not reported.

C Sample Instruments

C.1 Informed Consent

Thanks for agreeing to talk to me. My name is ... I work as a surveyor for the University of Connecticut and the Inter-American Development Bank, who are performing a study about female microentrepreneurs in Peru. I kindly request your participation in this interview. While I read the instructions and questions, please tell me whether there is anything that you do not understand.

You have been selected to participate in this interview as a client of ADRA Peru. We are interviewing ADRA's clients in the districts of Metropolitan Lima to collect information about the current situation of their families. I would like to ask you some questions about you, your household, and the decisions that are taken in your family. The interview lasts for about 15 minutes and your participation is voluntary. I will write down your answers as we move forward. I ask you to please answer honestly and with the best of your knowledge. If you agree to participate, you can decide to end the interview at any moment or to skip any question that you would prefer not to answer.

Your answers are anonymous and their confidentiality will be protected at every moment. Your name will not be used in any report and your answers will not be delivered to any person that does not belong to the research team. The analysis of the data that we collect will allow us to understand the current situation of women and their families in the area. If you have any question, you can ask me or other person in our research team. If you have any problem or inquiry about this study after the interview, please feel free to contact: Jorge Agüero or Veronica Frisancho, the researchers in charge of the project.

C.2 IPV and List Experiment Modules

C.2.1 Control Questionnaire: Direct Questions on Physical and Sexual IPV

1	Has your partner ever pulled your hair?
2	Has your partner ever pushed, shaken, or thrown something at you?
3	Has your partner ever slapped you or twisted your arm?
4	Has your partner ever punched you or hit you with something dangerous?
5	Has your partner ever kicked or dragged you?
6	Has your partner ever tried to strangle or burn you?
7	Has your partner ever threatened you with a knife, gun, or other weapon?
8	Has your partner ever forced you to have sexual intercourse when you did not want to?
9	Has your partner ever forced you to practice sexual practices that you do not approve of?

C.2.2 Control Questionnaire: List Experiment Questions

I am going to read to you a list of statements. Could you please tell me how many of them are true? Do not tell me which ones are true, only how many of them are true.

Have you ever...?

1. (a) Purchased a TV with HD
   (b) Been out for a walk with your children
   (c) Helped your children with their homework
   (d) Bought expensive clothes
2. (a) Played any games in your cellphone
   (b) Purchased new clothes for your children on important dates (e.g. Christmas, birthdays, others)
   (c) Traveled with your family on holidays
   (d) Seen *Al fondo hay sitio*[15]
3. (a) Pursued a technical degree
   (b) Experienced that your business' sales are insufficient to cover your household expenses
   (c) Traveled with friends
   (d) Been to the movies with your family
4. (a) Witnessed robberies in your neighborhood
   (b) Been robbed on the street
   (c) Had insurance from ESSALUD, the armed forces, or the police
   (d) Been depressed
5. (a) Felt insecure in your neighborhood
   (b) Had multiple businesses at the same time
   (c) Reared farm animals for consumption
   (d) Used the subway as a means of transportation
6. (a) Run out of money to cover the household's monthly expenses
   (b) Traveled with your children
   (c) Been part of a Christian church
   (d) Had to truncate your studies to care for your family
7. (a) Paid rent for the place where you live
   (b) Participated in other microfinance programs
   (c) Bought high-end clothes
   (d) Participated in a committee or association in your neighborhood
8. (a) Lost your cell phone

[15] *Al fondo hay sitio* is a very popular soap opera than ran for several years in Peru.

(b) Read *El Comercio*[16]

(c) Seen any soap opera

(d) Visited the Lima's cathedral

9. (a) Made improvements to your dwelling

(b) Had problems with your partner because of money issues

(c) Received a loan from *Mi Banco*

(d) Suffered from a serious medical condition that has required medical assistance

C.2.3 Treatment Questionnaire: List Experiment Questions

I am going to read to you a list of statements. Could you please tell me how many of them are true? Do not tell me which ones are true, only how many of them are true.

Have you ever...?

1. (a) Purchased a TV with HD

(b) Been out for a walk with your children

(c) Helped your children with their homework

(d) Bought expensive clothes

(e) Had your hair pulled by your partner?

2. (a) Played any games in your cellphone

(b) Purchased new clothes for your children on important dates (e.g. Christmas, birthdays, others)

(c) Traveled with your family on holidays

(d) Seen *Al fondo hay sitio*[17]

(e) Been pushed, shaken, or thrown something at you by your partner?

3. (a) Pursued a technical degree

(b) Experienced that your business' sales are insufficient to cover your household expenses

(c) Traveled with friends

(d) Been to the movies with your family

(e) Been slapped or had your arm twisted by your partner?

4. (a) Witnessed robberies in your neighborhood

(b) Been robbed on the street

[16] *El Comercio* is one of the most read newspapers in the country, particularly in Lima.

[17] *Al fondo hay sitio* is a very popular soap opera than ran for several years in Peru.

(c) Had insurance from ESSALUD, the armed forces, or the police
(d) Been depressed
(e) Been punched or hit with something dangerous by your partner

5. (a) Felt insecure in your neighborhood
(b) Had multiple businesses at the same time
(c) Reared farm animals for consumption
(d) Used the subway as a means of transportation
(e) Been kicked or dragged by your partner

6. (a) Run out of money to cover the household's monthly expenses
(b) Traveled with your children
(c) Been part of a Christian church
(d) Had to truncate your studies to care for your family
(e) Had your partner trying to strangle or burn you

7. (a) Paid rent for the place where you live
(b) Participated in other microfinance programs
(c) Bought high-end clothes
(d) Participated in a committee or association in your neighborhood
(e) Been threatened with a knife, gun, or other weapon by your partner

8. (a) Lost your cell phone
(b) Read *El Comercio*[18]
(c) Seen any soap opera
(d) Visited the Lima's cathedral
(e) Been forced to have sexual intercourse when you did not want to by your partner

9. (a) Made improvements to your dwelling
(b) Had problems with your partner because of money issues
(c) Received a loan from *Mi Banco*
(d) Suffered from a serious medical condition that has required medical assistance
(e) Been forced to practice sexual practices that you do not approve of by your partner

[18] *El Comercio* is one of the most read newspapers in the country, particularly in Lima.