INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE

IFPRI

IFPRI Discussion Paper 01534

May 2016

Anchoring Bias in Recall Data

Evidence from Central America

Susan Godlonton

Manuel A. Hernandez

Mike Murphy

Markets, Trade and institutions Division

INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE

The International Food Policy Research Institute (IFPRI), established in 1975, provides evidence-based policy solutions to sustainably end hunger and malnutrition and reduce poverty. The Institute conducts research, communicates results, optimizes partnerships, and builds capacity to ensure sustainable food production, promote healthy food systems, improve markets and trade, transform agriculture, build resilience, and strengthen institutions and governance. Gender is considered in all of the Institute's work. IFPRI collaborates with partners around the world, including development implementers, public institutions, the private sector, and farmers' organizations, to ensure that local, national, regional, and global food policies are based on evidence. IFPRI is a member of the CGIAR Consortium.

AUTHORS

Susan Godlonton is a an associate research fellow in the Markets, Trade and Institutions Division of the International Food Policy Research Institute (IFPRI) and an assistant professor in the Department of Economics at Williams College, Williamstown, MA, US.

.

Manuel A. Hernandez is a research fellow in the Markets, Trade and Institutions Division of IFPRI, Washington, DC.

Mike Murphy is a research analyst in the Markets, Trade and Institutions Division of IFPRI, Washington, DC.

Notices

[1.] IFPRI Discussion Papers contain preliminary material and research results and are circulated in order to stimulate discussion and critical comment. They have not been subject to a formal external review via IFPRI's Publications Review Committee. Any opinions stated herein are those of the author(s) and are not necessarily representative of or endorsed by the International Food Policy Research Institute.

[2.] The boundaries and names shown and the designations used on the map(s) herein do not imply official endorsement or acceptance by the International Food Policy Research Institute (IFPRI) or its partners and contributors.

Copyright 2016 International Food Policy Research Institute. All rights reserved. Sections of this material may be reproduced for personal and not-for-profit use without the express written permission of but with acknowledgment to IFPRI. To reproduce the material contained herein for profit or commercial use requires express written permission. To obtain permission, contact ifpri-copyright@cgiar.org.

Contents

Tables

Figures

ABSTRACT

Understanding the magnitude and source of measurement biases in self-reported data is critical to effective economic policy research. This paper examines the role of anchoring bias in self-reports of objective and subjective outcomes under recall. The research exploits a unique panel survey data set collected over a three-year period from four countries in Central America. It assesses whether respondents use their reported value of specific measures from the most recent survey period as a cognitive heuristic when recalling the value from a previous period, while controlling for the value they reported earlier. We find strong evidence of sizable anchoring bias in self-reported retrospective indicators for both objective measures (household and per capita income, wages, and hours spent on the household's main activity) and subjective measures (reports of happiness, health, stress, and well-being). In general, we also observe a larger bias in response to negative changes for objective indicators and a larger bias in response to positive changes for subjective indicators.

Keywords: anchoring bias; recall data; self-reporting; smallholder farmers; Central America

JEL codes: C8, O12, Q12

ACKNOWLEDGMENTS

This work was undertaken as part of, and partially funded by, the CGIAR Research Program on Policies, Institutions, and Markets (PIM) led by IFPRI. We thank Francisco Olivet, Luis Arteaga, and the Research and Planning team of Central America for their help in collecting data. This paper has not gone through IFPRI's standard peer-review process. The opinions expressed here belong to the authors, and do not necessarily reflect those of PIM, IFPRI, or CGIAR.

1. INTRODUCTION

The literature has acknowledged the potential for recall error in household surveys for several decades (for example, Neter and Waksberg 1964; Evans and Leighton 1995; Sudman et al. 1996).[1] Research has revealed retrospective biases over a wide range of employment, income, and expenditure data obtained from samples of populations in developed countries. Therefore, it is reasonable to anticipate that similar or potentially larger biases may exist in rural populations in developing countries– who typically have lower education levels and face more volatile economic conditions. In this paper, we examine the role of anchoring as a source of measurement error using panel survey data from Central America.

Anchoring bias as conceptualized by Tversky and Kahneman (1974) has been well documented in the economics literature. It has been studied in the context of financial markets (for example, Campbell and Sharpe 2009) and auction prices (for example, Beggs and Graddy 2009) and in the elicitation of willingness-to-pay parameters (for example, Ariely, Loewenstein, and Prelec 2003).[2] Furnham and Chu Boo (2011 have reviewed the literature on anchoring, and anchoring bias has also been addressed in the survey methodology literature. For example, Hurd and others (1998) show that anchoring effects may account for significant differences in responses when using brackets versus open-ended questions on savings and consumption data. Frykblom and Shogren (2000) argue that anchoring effects in discrete choice questions may result from survey framing issues rather than the dichotomous choice per se. However, our understanding of the role of anchoring bias as a source of nonclassical measurement error in retrospective data collection efforts is still quite limited, particularly in low-income settings. We focus on the role of anchoring bias in recall error of both objective and subjective indicators.

This research takes advantage of a novel panel survey data set from the International Food Policy Research Institute's (IFPRI's) Poverty-Sensitive Scorecard pilot program for smallholder farmers in Central America (see Hernandez and Torero 2014). This data set spans four countries—El Salvador, Guatemala, Honduras, and Nicaragua—and includes three rounds of data collected across three years: 2011, 2012, and 2013. It provides concurrent and recall data for objective indicators (monthly total and per capita household income, wages from primary occupation, and hours worked) as well as subjective measures (reports of happiness, health, stress, and overall well-being). We exploit this survey instrument feature to examine the degree and nature of mental anchoring in survey recall data.

In particular, we evaluate whether respondents use their reported value from the preceding wave of the survey as a cognitive heuristic to assist them in recalling the value from a previous period, while controlling for the value they reported in the earlier wave. For both objective and subjective indicators, we consistently find that the anchor value strongly predicts the reported recall value. In all cases, we conclude at conventional statistical levels that the recall measure is largely influenced by the anchor value. Our results provide evidence in favor of recall bias for both objective and subjective indicators. Furthermore, the high degree of mental anchoring is common across all four countries.

The estimation results are robust to a number of different specification checks. In the case of the objective indicators, we are able to exploit the fact that we have two rounds of data with anchoring variables to expand our analysis. First, we combine both waves into a pooled regression, adding a year dummy to account for a time effect. Second, we rerun our initial cross-section using values obtained under two-year (rather than one-year) recall. In the case of our subjective indicators, we perform an additional check to control for potential cap effects; given that the subjective indicators are discrete bounded variables ranging from 1 to 10, floor and ceiling effects may influence our results. For all three robustness checks, we find effects similar to those in our main specification.

[1] See also Bound, Brown, and Mathiowetz (2001) for a more general review of measurement error in survey data.

[2] The willingness-to-pay literature has led to an ongoing debate about whether preferences are in fact consistent and stable (for example, Maniadis, Tufano, and List 2014; Fudenberg, Levine, and Maniadis 2012; Alevy, Landry, and List 2011; Ariely, Loewenstein, and Prelec 2003). In this study, we examine anchoring bias as a source of measurement error rather than contribute to this debate.

We also extend the main analysis to examine whether positive or negative outcome changes across time play a role in anchoring. We find that positive changes are strongly associated with a positive bias while negative changes are strongly associated with a negative bias. This response pattern further supports the interpretation of the observed measurement error as anchoring bias. Interestingly, we find that the magnitude of recall bias is generally larger in response to negative changes for objective indicators but larger in response to positive changes for subjective indicators.

The paper contributes to the growing literature on understanding behavioral biases in self-reported data collection efforts. The last few decades have seen a significant rise in primary data collection efforts, particularly in developing countries. There has also been increasing attention to retrospective panel survey data. Frequently, researchers ask questions about current and past situations in a single wave, thereby reducing survey costs and avoiding attrition issues. Given the simultaneous shift in policy circles toward relying on rigorous evidence to inform decision making, the accuracy of self-reported data collected over different recall periods is critical.

Two recent studies closely related to ours are those by de Nicola and Giné (2014) and Maruyama (2007), who compare retrospective data obtained from survey respondents with an alternative independent measure to estimate recall error. De Nicola and Giné (2014) use records of reported catches by fishing vessels as an independent measure to assess the reliability of data that fishermen self-report in coastal India. Maruyama (2007) compares a retrospective and a concurrent measure of wage earnings among workers in Indonesia using data from the Indonesian Family Life Survey. We follow a validation approach similar to these studies but focus instead on the role of anchoring bias in recall error. We further examine the extent to which respondents may anchor their responses in both objective and subjective measures.

In the case of subjective indicators, it may be expected that such biases are important. Using a sample of women in Texas, Krueger and Schkade (2008) create test-retest validity indicators for a range of subjective indicators as well as education, income, and other standard economic variables. Their results indicate that the subjective indicators are less reliable than the objective measures. However, in our analysis we find more nuanced results. On average, we find a similar pattern of results for objective variables, such as income and primary wage, and subjective variables, such as reported happiness and stress. On the other hand, we generally find that recall bias is larger in the presence of negative differences for objective indicators but larger in the presence of positive differences for subjective indicators.

2. DATA

The data used for this analysis correspond to a panel survey of households that participated in selected agricultural projects as part of IFPRI's Poverty-Sensitive Scorecard pilot program conducted in Central America. The objective of the program was to select demand-driven projects using a scorecard tool that could better link poor smallholder farmers to dynamic markets through extension and training activities. The beneficiaries of each project belonged to an existing farmers' association and were involved in the production of the same crop, which varied by project (including basic grains, fruits and vegetables, coffee and cacao). Hernandez and Torero (2014) provide additional details about the pilot program and selected projects.

Three survey rounds were conducted, one baseline survey at the end of 2011 and two follow-up surveys at the end of 2012 and 2013, respectively, to evaluate the scorecard tool and the impact of the selected projects. In this study, we use the balanced panel of 554 project beneficiaries found in each survey wave. The sample is representative of the selected projects' beneficiaries, who are located across 24 municipalities (distinct administrative areas) within four countries: El Salvador, Guatemala, Honduras, and Nicaragua.[3] Refer to Hernandez and Torero (2015) for further details of the survey sample and attrition over time.[4]

The data collected in the survey include socioeconomic characteristics of respondents, household composition, income and labor activities, household assets and expenditure, and well-being indicators. The data set also has a special feature that makes it particularly useful for testing anchoring bias in recall data. In the second and third survey rounds, households responded to a series of questions regarding, for example, their household income using different recall periods. In 2013, respondents were asked about their household income in 2013 and then shortly afterward about their income in 2012 and 2011; similarly, in 2012, they were asked about their income in 2012 as well as 2011. The intent of this part of the panel survey design was to make some of the key indicators collected comparable between returning and new households that entered the survey.[5]

As shown in Figure 2.1, we have repeated values for different objective and subjective outcomes, obtained using different lengths of recall. Objective indicators include monthly total and per capita household income, wages from primary occupation, and working hours in the household's main activity. For these indicators, we have records of the information collected during each survey round (that is, concurrent data for 2011, 2012, and 2013, respectively) as well as one and two years of data obtained with different recall periods (that is, recall data for 2011 and 2012, respectively, that were collected in 2013). Subjective indicators include measures (on a 10-point scale) of happiness, health, stress, and overall well-being. For these indicators, we collected concurrent data for 2012 and 2013, as well as recall data for 2012 that we collected in 2013; these subjective measures were not collected in the first year.[6]

---

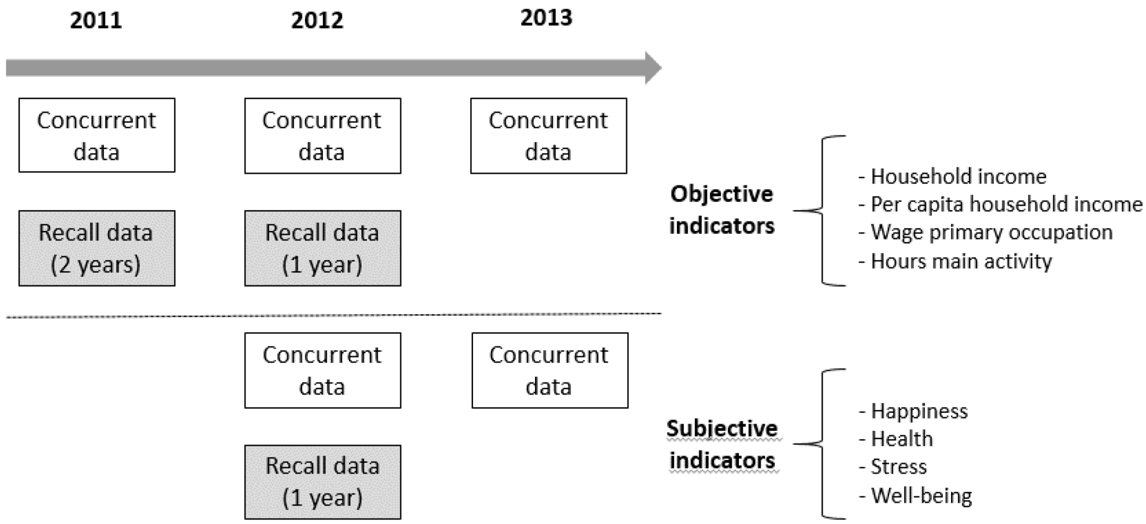
[3] A total of 9.2 percent of the households are from El Salvador, 48.4 percent from Honduras, 16.8 percent from Guatemala, and 25.6 percent from Nicaragua.

[4] The survey sample maintained the proportion of beneficiaries in each project by gender and location (municipality). After each survey wave, there was a 10 percent attrition rate between each survey wave due to internal migration and other factors. However, there were no major differences, at least in observable characteristics, between households that attrited and those that did not.

[5] New households entered the survey in the second and third year as replacements and because the sample was expanded to include households from nearby areas that did not necessarily participate in the program.

[6] Focusing on these indicators also helps us to reduce potential framing effects which could influence our results, which is more common among binary survey questions (see, for example, Goldin and Reck 2015).

Figure 2.1 Concurrent and recall data reported for objective and subjective indicators



Source: Authors.

Table 2.1 presents basic descriptive statistics for the working sample. Panel A reports demographic and household characteristics of the respondents in the last period. The sample is evenly split between men and women, and the average age is 43 years. Individuals in general have not completed primary schooling; on average, they have had 5.3 years of education. The household size is close to six members and the plot size is 2.6 hectares, reflecting the fact that the program targeted poor farmers.

Panel B reports some basic demographic information about the interviewers who collected data from the respondents. The same firm and supervisors in each country conducted the three survey rounds. However, the pool of interviewers across years changed, and the same subjects were not always assigned to the same interviewer. In the first two waves of data collection, about 40 percent were male, while in the third wave 55 percent were male. The average age of the interviewers decreased from approximately 35 to 33 years old between the first and the other two survey rounds. We account for these enumerator characteristics in the regression analysis.

Finally, Panel C and D present summary statistics of the objective and subjective outcomes of interest respectively.[7] Two patterns are worth noting regarding the objective indicators. First, there is a declining trend in the concurrent measures reported after each wave over the period 2011 to 2013. For total and per capita monthly household income, the decline is monotonic over time (from US$421 and US$84, respectively, in 2011 to US$214 and US$45, respectively, in 2013). Primary wages and working hours also decreased (from US$207 to US$161 and from 39.4 hours to 38 hours).[8] Second, and central to this study, the recall measures for 2011 and 2012, captured in 2013, are strikingly similar to the corresponding 2013 concurrent measures in terms of both their mean and their dispersion (standard deviation). For example, the recall 2011 and 2012 per capita income is, on average, US$46 and US$47, respectively, versus US$45 for the concurrent 2013 measure; their standard deviations are US$59, US$66, and US$59, respectively. These patterns provide preliminary support for significant anchoring bias in self-reported data, such as income and wage data, measured in the last wave.

[7] Respondents in Guatemala, Honduras, and Nicaragua reported monetary variables in local currency units. For ease of interpretation, we convert these to US dollars using the corresponding mean exchange rates published by the local central banks for the period of the survey.

[8] We do not believe that the beneficiaries of the program had any incentive to underreport their income or wages over time, given the design of the program. It was clear from the beginning of the program that the allocated grants were only (one-time) start-up grants for selected projects intended to be sustainable in the medium and long term.

Table 2.1 Summary statistics

Variable	Mean	SD	Min	Max
Panel A: Individual characteristics (2013)				
Age	43.016	12.661	17	78
If male	0.504	0.500	0	1
Years of education	5.314	3.399	0	17
Household size	5.745	2.478	1	15
Size of main plot (hectares)	2.616	5.659	0.005	69.888
Proportion missing size of main plot	0.137	0.344	0	1
Panel B: Interview characteristics				
If male interviewer (2011)	0.424	0.495	0	1
If male interviewer (2012)	0.404	0.491	0	1
If male interviewer (2013)	0.549	0.498	0	1
Interviewer age (2011)	34.818	7.266	21	45
Interviewer age (2012)	32.812	7.312	21	46
Interviewer age (2013)	32.948	10.168	20	51
Interview length in minutes (2011)	30.120	13.675	10.633	108.483
Proportion paused (2011)	0.031	0.173	0	1
Interview length in minutes (2012)	38.860	15.190	10.383	117.683
Proportion paused (2012)	0.038	0.191	0	1
Interview length in minutes (2013)	42.667	17.925	11.900	117.967
Proportion paused (2013)	0.049	0.216	0	1
Panel C: Outcomes of interest of objective indicators				
Income (concurrent 2011)	420.789	746.438	0.313	6515.295
Income (concurrent 2012)	322.530	677.670	0.127	8935.777
Income (concurrent 2013)	213.803	262.150	0.048	3151.955
Income (recall 2011)	232.630	309.122	0.052	3902.599
Income (recall 2012)	234.435	311.808	0.050	3475.243
Income per capita (concurrent 2011)	83.922	160.257	0.078	1628.824
Income per capita (concurrent 2012)	67.118	182.318	0.025	2978.593
Income per capita (concurrent 2013)	44.870	58.926	0.006	488.553
Income per capita (recall 2011)	46.050	59.305	0.007	650.433
Income per capita (recall 2012)	47.409	65.561	0.012	695.049
Primary wage (concurrent 2011)	206.529	364.018	0.000	3278.184
Primary wage (concurrent 2012)	155.363	282.484	0.298	3049.053
Primary wage (concurrent 2013)	161.379	496.677	0.048	8663.994
Primary wage (recall 2011)	167.772	463.932	0.000	9366.238
Primary wage (recall 2012)	160.618	441.030	0.050	8936.340
Hours in main activity (concurrent 2011)	39.363	14.619	3	96
Hours in main activity (concurrent 2012)	37.397	15.985	3	96
Hours in main activity (concurrent 2013)	38.032	16.113	0	144
Hours in main activity (recall 2011)	37.818	16.687	1	144
Hours in main activity (recall 2012)	37.426	16.063	1	144

Table 2.1 Continued

Panel D: Outcomes of interest of subjective indicators				
Happiness (concurrent 2012)	8.011	1.835	1	10
Happiness (concurrent 2013)	8.101	1.869	1	10
Happiness (recall 2012)	8.195	1.750	1	10
Health (concurrent 2012)	7.724	1.929	1	10
Health (concurrent 2013)	7.749	2.056	1	10
Health (recall 2012)	7.850	1.855	1	10
Stress (concurrent 2012)	5.581	2.642	1	10
Stress (concurrent 2013)	5.446	2.558	1	10
Stress (recall 2012)	5.542	2.460	1	10
Well-being (concurrent 2012)	7.623	1.976	1	10
Well-being (concurrent 2013)	7.731	2.029	1	10
Well-being (recall 2012)	7.825	1.814	1	10
Observations				554

Source: Authors' calculations.
Notes: Years of education are those reported in 2011. "Proportion paused" indicates interviews that were paused and recommenced. Income and wage variables for Guatemala, Honduras, and Nicaragua were converted to US dollars. Subjective indicators are measured on a 1–10 scale. SD = standard deviation.

Regarding the subjective indicators, we elicited all four of these measures using a 10-point scale, where higher numbers indicate greater magnitude. Higher scores indicate that respondents are better off in terms of happiness, health, and well-being, while the opposite is true for stress. We observe that respondents are marginally better off in terms of the concurrent subjective measures reported in 2012 and 2013. In addition, in most cases, the average recall measures for 2012 are closer to the concurrent measures reported in 2013 than to the measures reported in 2012; this is also indicative of anchoring bias. For instance, the recall 2012 level of happiness is 8.19 versus 8.1 and 8.01 for the concurrent 2013 and 2012 measures, respectively.

The potential anchoring bias in self-reported objective and subjective data is clearer in Table 2.2. Columns (1) and (2) correspond to the difference between the 2013 concurrent measure and the 2012 recall measure (2013 concurrent minus 2012 recall), while columns (3) and (4) pertain to the difference between the 2012 concurrent measure and the 2012 recall measure (2012 concurrent minus 2012 recall). The difference in the 2013 concurrent versus 2012 recall measure is considerably smaller than the difference in the 2012 concurrent versus 2012 recall measure for most of the objective and subjective indicators. In particular, for reported income and wages, the 2012 recall measure is four to seven times closer to the 2013 concurrent measure than to the 2012 concurrent measure; for the subjective indicators (except level of stress) it is one to two times closer. It should also be noted that there is a lower level of dispersion in the difference between the 2013 concurrent and 2012 recall measures than in the difference between the 2012 concurrent and recall measures.

Overall, these summary statistics provide preliminary evidence of substantial anchoring bias in self-reported data across most objective and subjective indicators. We turn next to outlining our empirical approach.

Table 2.2 Differences between concurrent and recall measures

	2013 Concurrent - 2012 Recall		2012 Concurrent - 2012 Recall	
	Mean	SD	Mean	SD
Outcome	(1)	(2)	(3)	(4)
Income	-20.632	243.131	88.096	705.231
Per capita income	-2.539	52.594	19.709	180.144
Primary wage	0.710	288.142	-5.255	522.958
Hours in main activity	0.606	11.781	-0.029	20.198
Happiness	-0.094	1.530	-0.184	2.359
Health	-0.101	1.704	-0.126	2.366
Stress	-0.096	1.960	0.040	3.329
Well-being	-0.094	1.522	-0.202	2.402

Source: Authors' calculations.

Notes: "Concurrent" refers to the value for the stated year as reported in that year. "Recall" refers to the value for 2012 as reported in 2013. Income and wage variables for Guatemala, Honduras, and Nicaragua were converted to US dollars. Subjective indicators are measured on a 1–10 scale. SD = standard deviation.

3. EMPIRICAL APPROACH

The structure of the survey permits us to compare concurrent objective and subjective measures reported in each survey wave with recall measures reported in the last wave. Sometimes, recalling information for an earlier period may have been challenging. In these cases, respondents may use their reported value for the most recent period as a cognitive heuristic to assist in the reporting of the subsequent recall value. This mental anchoring may result, for example, in the provision of a biased estimate of income in the earlier period, since it has been influenced by their reporting for the more recent period. The descriptive statistics discussed above suggest that anchoring bias may play an important role in this context.

Formally, we can conceptualize this bias in terms of a simple linear regression framework. Consider $\hat{x}$ to be the realization of a given variable x, which is observed with measurement error ε:

$$\hat{x} = x + \varepsilon. \tag{1}$$

Following Campbell and Sharpe (2009), we can assume an anchoring variable a based on the reported value from a previous question, which respondents use to supplement their recalling of the realized value $\hat{x}$:

$$\hat{x} = pa + (1 - p)x + \varepsilon, \tag{2}$$

where $0 \leq p \leq 1$ is the weight that the respondent assigns to the anchoring value relative to the true latent variable x; that is, when $p = 1$, the estimate is based solely on the anchoring value, and when $p = 0$, anchoring bias plays no role.

In practice, we can test for anchoring bias in our sample by estimating the following model:

$$y_{ij}^{r} = \alpha + \beta y_{ij}^{a} + \gamma y_{ij}^{c} + \theta Z_{ij} + \kappa_j + \varepsilon_{ij}, \tag{3}$$

where y_{ij}^{r} is the recall value for the variable of interest (for example, household income) reported by individual i from project j for a given previous period (that is, the 2012 income reported in 2013, which we refer to as the 2012 recall income); y_{ij}^{a} is the value of the variable for the most recent period, which serves as the anchor value (that is, the 2013 income reported in 2013, which we refer to as the 2013 concurrent income); y_{ij}^{c} is the value of the variable reported in the previous period, which serves as a proxy for the actual value (that is, the 2012 concurrent income); Z_{ij} is a vector of controls that includes individual, household, and interview characteristics; κ_j is project-level fixed effects; and ε_{ij} is a white-noise error term.

We estimate the same model for both objective and subjective indicators. The parameters of interest in equation (3) are β and γ. The first parameter measures the partial correlation between the value reported for the most recent period and the recall value, while the second parameter captures the correlation between the value reported in the previous period and the recall value. If $\beta > \gamma$, then anchoring bias plays a major role when recalling information. The opposite is true if $\beta < \gamma$. For the objective indicators, we exploit an additional round of data, which permits us to expand the analysis to two-year recall lags.

The estimation of equation (3) poses some issues worth discussing. First, indicators like income may be autocorrelated such that the anchoring, measured through y_{ij}^{a}, could also reflect this correlation. Yet this potential correlation across time should be captured by y_{ij}^{c}. Therefore, the estimated relative weight (β) is expected to be net of this correlation, if any.

Second, the estimated anchoring bias may also reflect recall error resulting from a positive or negative shock recently experienced, for example, a weather shock that affected crop yields and thus the income of smallholder farmers. We test for this potential source of bias when recalling household income by using rainfall variation as an instrument. Given that the primary income source for the surveyed households is agricultural activities, changes in rainfall in the most recent year are likely to affect income in the most recent period and, by construction, should not be correlated with the values reported for the preceding period. Following Miguel, Satyanath, and Sergenti (2004), we construct a variable for the proportionate change in mean rainfall between the current and the previous period for the municipality where the respondent is located.[9] We find that this proportionate change is a valid instrument for the most recent income reported (y_{ij}^{a}), but we fail to reject the null hypothesis of exogeneity of this variable based on the Wu-Hausman test.[10]

Third, in the case of the subjective indicators, the elicitation method using a 10-point scale may introduce bias. This bias arises particularly for individuals reporting values in the lower and upper bounds, because their underlying latent values may lie below or above the possible range. We demonstrate that our results are robust to the exclusion of such individuals.

We also extend our base framework to examine whether the direction and magnitude of the bias differs in response to positive or negative changes in the outcomes of interest across time. To do so, we estimate the following regression:

$$\Delta y_{ij} = \alpha + \beta_1 Pos_{ij} + \beta_2 Neg_{ij} + \theta Z_{ij} + \kappa_j + \varepsilon_{ij}, \tag{4}$$

where Δy_{ij} is the difference between the recall value for 2012 (reported in 2013) and the 2012 concurrent measure; Pos_{ij} is the value of the difference between the 2013 concurrent outcome and the 2012 concurrent outcome if that difference is positive, and 0 otherwise; and Neg_{ij} takes the absolute value of the difference if the difference is negative, and 0 otherwise.

To be consistent with the hypothesis that anchoring bias drives the measurement error observed, we would expect $\beta_1 > 0$ and $\beta_2 < 0$. In other words, if a household exhibits positive changes in the outcome across time we anticipate that the recall bias is positive, and the opposite if the household exhibits a negative change in the outcome. As an additional sensitivity check, we also control for the modal pattern of these differences at the municipality level. Specifically, we control for positive and negative municipality indicators, where the former takes the value 1 when the majority of the people in the municipality, excluding the respondent, report a positive change for the given indicator between 2012 and 2013, and 0 otherwise. In this scenario, the converse would be true for the negative dummy.

---

[9] We obtained the rainfall data at the municipality level from the US Agency for International Development (USAID) Famine Early Warning Systems Network. It corresponds to the Climate Hazards Group InfraRed Precipitations with Stations time-series data, which contain 72 pentadal (five-day) precipitation averages per location per year. The change in rainfall is given by

$$R_{m,t} = \frac{\left(\bar{r}_{m,t} - \bar{r}_{m,t-1}\right)}{\bar{r}_{m,t-1}},$$

where $\bar{r}_{m,t}$ is the average rainfall in municipality *m* at year *t*.

[10] See Appendix, Table A.1.

4. RESULTS AND DISCUSSION

This section presents the estimation results and evaluates robustness. We first discuss the base results of the estimated model specified in equation (3) using concurrent and recall data for 2012 and 2013. We then assess the validity of the results using alternative model specifications and samples.

Base Results

Table 4.1 presents the estimation results for the objective outcomes. Columns (1), (4), (7), and (10) include project-level fixed effects. Columns (2), (5), (8), and (11) add controls for the respondents' demographic and household characteristics, including age, gender, education (years of schooling), household size, and plot size.[11] Columns (3), (6), (9), and (12) add an additional set of variables for the interview characteristics. These include controls for the age and gender of the person conducting the interview and for the duration of the interview.[12] The p-values of the coefficients, reported in parentheses, are clustered at the project level. These values are derived following Cameron and colleagues' (2008) wild bootstrap procedure to better account for the precision of the standard errors in the presence of only eight clusters.

As shown in the table, the coefficient of the anchor variable is positive and statistically significant at conventional levels across specifications and for all outcomes. The point estimate of the anchoring coefficient ranges from 0.65 in the case of working hours to 0.77 in the case of monthly income. These results provide strong evidence that respondents use the reported value for the current period (2013 outcome reported in 2013) to frame their responses in reference to the preceding year (2012 outcome reported in 2013), despite answering a range of other questions between these two reports. In contrast, the coefficient of the concurrent measure (2012 outcome reported in 2012) is consistently close to 0 and not statistically significant. Based on the one-sided Wald tests (p-values) reported at the bottom of the table, we cannot reject the null hypothesis that the anchoring coefficient is greater than or equal to 0.5 (H_0: $\beta \geq 0.5$) at conventional levels of significance for all indicators. It might be easier for an individual to recall some outcomes, such as primary wage, than others, such as total household income, which requires recalling the income of other household members. Yet we observe a significant degree of mental anchoring when reporting all four objective outcomes.

Table 4.2 presents the estimation results for the subjective indicators using the same set of model specifications as those in Table 4.1. Similar to the objective measures, the anchor value has a strong and statistically significant relationship to the recalled subjective measures. The estimated anchoring coefficient ranges from 0.52 for the health indicator to 0.66 for the stress indicator, and in all cases we can conclude that this coefficient is statistically larger than or equal to 0.5. The coefficients of the concurrent measures, in turn, are generally close to 0 and not significant. The concurrent measure is partially correlated with the recall measure only for the well-being indicator, yet this partial correlation is less than one-tenth of the correlation between the anchor and recall measures.

Furthermore, we find that the bias seems slightly stronger for objective outcomes than for subjective outcomes. This result is surprising given the anticipated natural reference dependence when evaluating, for instance, well-being or level of happiness across different periods, as opposed to reporting wages or working hours.[13] Overall, the results provide strong evidence that anchoring bias plays an important role when recalling both objective and subjective outcomes.

[11] We also include an indicator that takes the value 1 when the size of the main plot is missing—for these cases we assign the median plot size—and 0 otherwise.

[12] For cases in which the interview was paused and recommenced, we assign the duration variable the median value. In addition, we include an additional dummy variable, which takes the value of 1 for paused interviews and 0 otherwise.

[13] Yet the lower magnitude of the anchoring bias among subjective indicators may be explained by the caps (1 and 10) in these indicators, as discussed in the following section.

Table 4.1 Estimation results for objective outcomes

Coefficient	Dependent variable											
	Income			Income per capita			Primary wage			Hours main activity		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Anchor value	0.769***	0.764***	0.749***	0.698***	0.694***	0.681***	0.722**	0.720***	0.721**	0.652***	0.663***	0.660***
	(0.004)	(0.000)	(0.000)	(0.004)	(0.004)	(0.000)	(0.012)	(0.004)	(0.016)	(0.000)	(0.000)	(0.008)
Concurrent measure	0.007	0.003	0.002	0.024	0.02	0.019	-0.071	-0.071	-0.067	0.014	0.018	0.018
	(0.840)	(0.984)	(0.836)	(0.520)	(0.441)	(0.391)	(0.105)	(0.129)	(0.328)	(0.555)	(0.383)	(0.523)
Project fixed effects	+	+	+	+	+	+	+	+	+	+	+	+
Individual characteristics	-	+	+	-	+	+	-	+	+	-	+	+
Interview characteristics	-	-	+	-	-	+	-	-	+	-	-	+
p-value (H_0: anchor ≥ 0.5)	0.957	0.959	0.947	0.942	0.945	0.940	0.812	0.810	0.813	0.994	0.995	0.993
Observations	554	554	554	554	554	554	554	554	554	554	554	554

Source: Authors’ calculations.

Notes: “Dependent variable” is the reported value in 2013 for the outcome in 2012. “Anchor value” refers to the value reported in 2013 for the outcome in 2013. “Concurrent measure” refers to the value reported in 2012 for the outcome in 2012. “Individual characteristics” include respondent age, gender, years of education, household size, and size of primary agricultural plot. “Interview characteristics” include age and gender of enumerator and duration of survey. Wild-bootstrapped *p*-values appear in parentheses and are clustered at the project level (eight clusters), following Cameron and others (2008), using 256 iterations. p-value of one-sided Wald test for anchor ≥ 0.5 reported at the bottom also obtained via bootstrapping method. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table 4.2 Estimation results for subjective outcomes

Coefficient	Dependent variable											
	Happy			Healthy			Stress			Well-being		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Anchor value	0.572***	0.572***	0.579***	0.527***	0.516***	0.518***	0.657***	0.658***	0.646***	0.575***	0.573***	0.575***
	(0.000)	(0.008)	(0.004)	(0.004)	(0.004)	(0.008)	(0.008)	(0.004)	(0.000)	(0.000)	(0.004)	(0.000)
Concurrent measure	0.032	0.034	0.035	0.048	0.036	0.035	0.043	0.041	0.052	0.055***	0.057***	0.057**
	(0.285)	(0.312)	(0.340)	(0.316)	(0.348)	(0.297)	(0.320)	(0.355)	(0.281)	(0.008)	(0.004)	(0.047)
Project fixed effects	+	+	+	+	+	+	+	+	+	+	+	+
Individual characteristics	-	+	+	-	+	+	-	+	+	-	+	+
Interview characteristics	-	-	+	-	-	+	-	-	+	-	-	+
p-value (H_0: anchor ≥ 0.5)	0.956	0.962	0.969	0.690	0.611	0.622	0.971	0.972	0.962	0.929	0.926	0.925
Observations	554	554	554	554	554	554	554	554	554	554	554	554

Source: Authors' calculations.

Notes: "Dependent variable" is the reported value in 2013 for the outcome in 2012. "Anchor value" refers to the value reported in 2013 for the outcome in 2013. "Concurrent measure" refers to the value reported in 2012 for the outcome in 2012. "Individual characteristics" include respondent age, gender, years of education, household size, and size of primary agricultural plot. "Interview characteristics" include age and gender of enumerator and duration of survey. Wild-bootstrapped *p*-values, are reported in parentheses and clustered at the project level (eight clusters), following Cameron and others (2008), using 256 iterations. p-value of one-sided Wald test for anchor ≥ 0.5 reported at the bottom also obtained via bootstrapping method. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Additional Estimations

We now examine the sensitivity of our base results. To do this, we run four additional estimations. First, we assess whether our base results differ by country. In particular, we evaluate whether the observed anchoring bias for both objective and subjective measures recurs across individuals in all four countries. Appendix Table A.2 reports the estimation results when we interact the country dummies (Honduras is the base category) with the corresponding anchor measure (outcome reported in 2013) and concurrent measure (outcome reported in 2012). We observe similar anchoring behavior in all four countries. In particular, the magnitude of the association between the anchor value and the recalled objective and subjective outcomes is consistent across countries. We observe a few differences across countries for the association between the 2012 concurrent and recall measures, but these are small and the overall partial correlations are still close to 0. Importantly, the high degree of mental anchoring is not specific to one country but is common among individuals in all surveyed areas.

Second, for the objective variables, we exploit the fact that we have concurrent reports for 2011, 2012, and 2013, as well as recall reports for 2011 and 2012.[14] As a result, we can jointly compare the 2011 recall measure with the concurrent 2012 measure and the 2012 recall measure with the concurrent 2013 measure, while controlling for the corresponding concurrent reports of 2011 and 2012. This one-year recall comparability allows us to create a pooled sample.

Appendix Table A.3 presents the estimation results using this pooled sample, which also accounts for a survey round dummy. We find results similar to those in our base estimations. The coefficient of the variable that serves as an anchor ranges between 0.58 and 0.82, and it is statistically significant for all the objective indicators. We also detect some significant partial correlations between the concurrent measure and the dependent recall variable, though the magnitude of these correlations is very small, close to 0.

Third, we run an additional set of regressions for our objective variables, this time exploring the effect of the anchoring variable on the reported measure under two-year recall. We compare the effect of the individual's reported value for 2013 (as reported in that round) with the measure reported for 2011 in the same survey. One notable difference in this exercise with the base analysis is that the respondent is now answering approximately twice as many questions between the anchoring value (the concurrent report for 2013) and the two-year recall report (the concurrent report for 2011).

Appendix Table A.4 presents these results. It finds results similar to those in Table A.3, with even stronger evidence of anchoring. The anchoring coefficient ranges between 0.69 and 0.93, and a null or very weak correlation exists between the two-year recall report and the outcome reported in 2011. One possibility for stronger anchoring effects in the context of longer recall periods is that respondents may be prone to give more biased answers; because it is harder to recall accurately, they may be more biased by a potential anchor. The stronger effects are interesting given the increased amount of time between the potential anchoring question and the subsequent report under two-year recall (compared with the report under one-year recall); one could anticipate a lower bias in this context.

Fourth, we examine the sensitivity of our subjective indicator results to potential cap effects because these indicators are constructed on a scale of 1 to 10. Accordingly, Appendix Table A.5 presents the estimation results, excluding the cases in which the individuals reported a value of 1 (or 10) in the last survey wave—that is, when the concurrent measure of 2013 is equal to the minimum or maximum value. We find results very similar to those of the original specification, with positive and highly significant anchor values ranging between 0.49 and 0.66. Thus, the anchoring bias among the subjective measures does not seem to be driven by the lower and upper bounds in the scale.[15]

[14] Unfortunately, the subjective well-being questions were not included in the 2011 baseline survey, precluding their use for this portion of our sensitivity analysis.

[15] Note that this exercise helps to rule out the possibility that the caps in the subjective indicators are the source of the anchoring bias. Yet the imposed caps may still limit the magnitude of the bias. For example, individuals might report a value of 9 for their current well-being. Based on this report (used as an anchor), they would like to report a value of 11 for the previous period. But they are constrained to reporting a maximum value of 10; a similar (opposite) reasoning applies if their anchor value is close to 1.

Differential Responses to Reported Outcome Changes

We next extend the main analysis to examine whether the anchoring effect varies based on changes in concurrent reports. We might expect an individual who experienced a positive change in income between 2012 and 2013 to be subject to a greater or lesser degree of anchoring bias than an individual experiencing no change or a negative change. To explore this possibility, we evaluate how a positive or negative change in the concurrent measures between one period and another influences the anchoring bias. As shown in equation (4), the dependent variable in this case is the difference between the recall value for 2012 (reported in 2013) and the 2012 concurrent measure.[16] There are two additional discrete independent variables. One accounts for a positive difference in the concurrent reports (equal to the value of the difference between the 2013 and 2012 concurrent measures when this difference is positive and 0 otherwise). The other variable accounts for a negative difference in the concurrent reports (equal to the absolute value of the difference between the 2013 and 2012 concurrent measures when this difference is negative and 0 otherwise).

In Table 4.3, panel A presents the estimation results for the objective indicators and panel B for the subjective indicators. Two interesting patterns are worth noting. First, for all objective and subjective measures, we observe a strong and statistically significant association between the magnitude of both positive and negative outcome changes and the magnitude of the anchoring bias on the recalled value. The opposite sign of the coefficients for positive and negative outcome changes provides additional support for interpreting the observed bias as anchoring bias. A larger positive (negative) change between the 2012 and 2013 concurrent measures results in a larger positive (negative) difference between the 2012 recall and concurrent measures.

Figures 4.1 and 4.2 provide additional insights about the observed outcome changes and the distribution of the measurement bias (the difference between the recall and concurrent outcome for 2012). Figure 4.1 plots the distribution of the measurement bias for income, distinguishing between individuals reporting positive and negative outcome changes. Figure 4.2 plots the same distributions for the level of happiness. In both cases, we find a leftward distributional shift of the measurement bias for those experiencing a negative outcome change and a corresponding rightward shift for those experiencing a positive outcome change. These patterns further support the opposite direction of the bias when individuals experience positive and negative outcome changes. Moreover, this finding demonstrates that these effects occur throughout the entire distribution of individuals with varying positive (negative) outcome changes and are not driven simply by outlier values.

[16] For individuals reporting no change, the variable takes the value 0. For the income and wage variables, few individuals report no change; for hours in main activity and the subjective indicators, 12 percent to 25 percent of the sample reports no change.

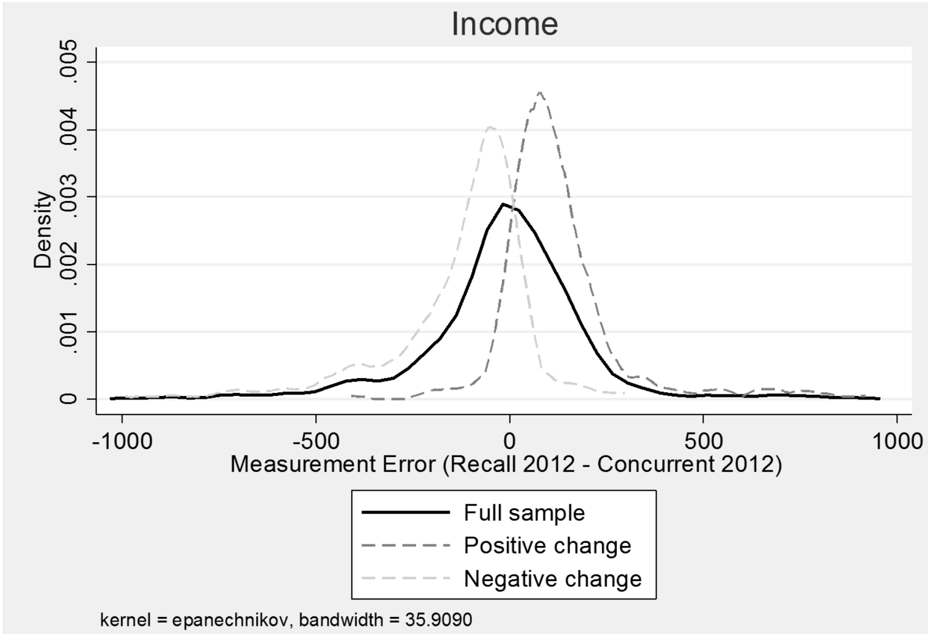
Table 4.3 Estimation results for objective and subjective outcomes, differential responses to changes in concurrent measures

Panel A: Objective outcomes				
Coefficient	**Dependent variable**			
	Income	**Income per capita**	**Primary wage**	**Hours main activity**
	(1)	**(2)**	**(3)**	**(4)**
Positive difference	0.679**	0.543***	0.732*	0.885***
	(0.039)	(0.004)	(0.074)	(0.000)
Negative difference	-1.015***	-1.000***	-0.999**	-0.724***
	(0.008)	(0.008)	(0.016)	(0.008)
Project fixed effects	+	+	+	+
Individual characteristics	+	+	+	+
Interview characteristics	+	+	+	+
p-value (H_0: positive ≥ 0.5)	0.787	0.593	0.813	1.000
p-value (H_0: negative ≤ -0.5)	1.000	1.000	1.000	0.999
p-value (H_0: positive + negative ≤ 0)	1.000	1.000	1.000	0.001***
Observations	554	554	554	554
Panel B: Subjective outcomes				
Coefficient	**Dependent variable**			
	Happy	**Healthy**	**Stress**	**Well-being**
	(1)	**(2)**	**(3)**	**(4)**
Positive difference	1.021***	0.921***	0.717***	0.932***
	(0.004)	(0.000)	(0.008)	(0.004)
Negative difference	-0.525**	-0.547***	-0.872**	-0.583**
	(0.020)	(0.008)	(0.016)	(0.012)
Project fixed effects	+	+	+	+
Respondent characteristics	+	+	+	+
Interview characteristics	+	+	+	+
p-value (H_0: positive ≥ 0.5)	1.000	1.000	0.985	1.000
p-value (H_0: negative ≤ -0.5)	0.672	0.710	0.999	0.934
p-value (H_0: positive + negative ≥ 0)	0.672	0.710	0.001***	0.934
Observations	554	554	554	554

Source: Authors' calculations.

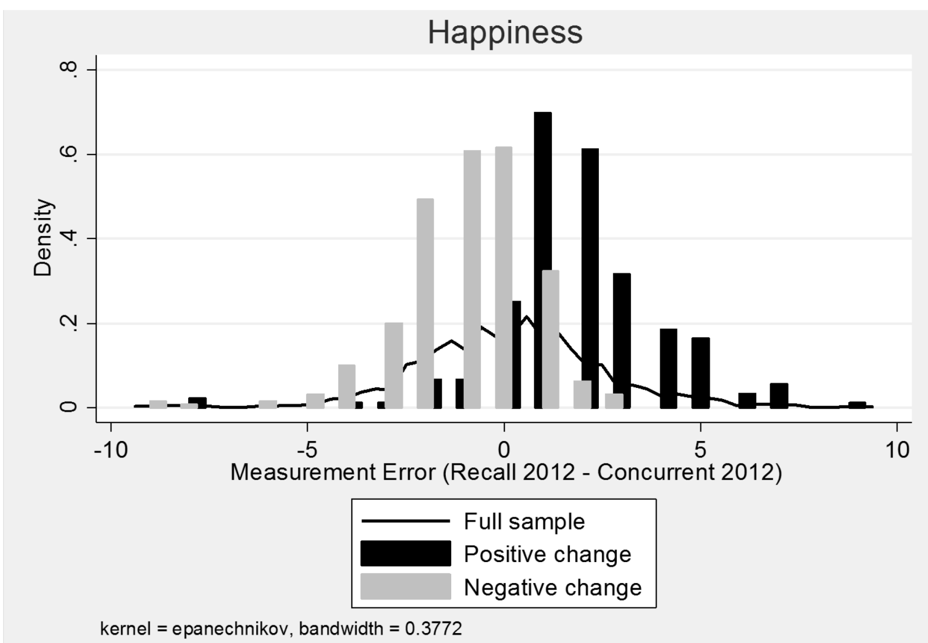
Notes: "Dependent variable" is the difference between the value for 2012 reported in 2013 and the value for 2012 reported in 2012. "Positive difference" is the value of the difference between the 2013 value reported in 2013 and the 2012 value reported in 2012 if that difference is greater than 0; it takes the value of 0 otherwise. "Negative difference" is the difference between these 2013 and 2012 concurrent values (in absolute value) if the difference is less than 0, and 0 otherwise. "Individual characteristics" include respondent age, gender, years of education, household size, and size of primary agricultural plot. "Interview characteristics" include age and gender of enumerator and duration of survey. Wild-bootstrapped *p*-values are reported in parentheses and clustered at the project level (eight clusters), following Cameron and others (2008), using 256 iterations. p-value of one-sided Wald tests reported at the bottom also obtained via bootstrapping method. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Figure 4.1 Distribution of measurement bias for selected objective measures



Source: Authors.
Notes: "Positive change" corresponds to all individuals with a positive change in their concurrent income between 2012 and 2013. "Negative change" corresponds to all individuals with a negative change in their concurrent income between 2012 and 2013. Density distributions were derived using kernel density estimation. For ease of interpretation, respondents reporting a difference in magnitude greater than 1,000 are excluded (31 observations).

Figure 4.2 Distribution of measurement bias for selected subjective measures



Source: Authors.
Notes: "Positive change" corresponds to all individuals with a positive change in their concurrent level of happiness between 2012 and 2013. "Negative change" corresponds to all individuals with a negative change in their concurrent level of happiness between 2012 and 2013. Density distributions were derived using kernel density estimation.

The second interesting pattern is the differential responses in the magnitude of the anchoring bias to positive and negative year-on-year changes in concurrent reports for objective and subjective variables. Interestingly, in the case of the objective outcomes, the magnitude of the bias appears to be stronger for negative than for positive differences in concurrent reports. With regard to the subjective outcomes, we find the opposite relationship. Among the objective indicators, the magnitude of the coefficient is statistically larger for negative differences across all indicators except working hours. Among the subjective indicators, the magnitude of the coefficient is statistically larger for all positive differences except level of stress, for which a negative change actually indicates an improvement for the individual.[17]

Appendix Table A.7 reports the estimation results of equation (4) when further controlling for the modal pattern of outcome changes in the municipality where the respondent resides. We include both a positive and a negative dummy variable that take the value 1 when the majority of the people in the municipality (excluding the respondent) report a positive or negative change in the corresponding variable between 2012 and 2013. We find a pattern of results similar to the one in Table 5. In concurrent reports, the magnitude of the bias is larger for negative differences in the objective measures and larger for positive differences in the subjective measures. In terms of the variables' capturing the municipality modal pattern, in some of the objective and subjective indicators we find a significant correlation between the modal pattern and the magnitude of the bias. However, positive (negative) outcome changes in the vicinity do not necessarily translate into a larger positive (negative) bias.

[17] Similar results are obtained for the subjective indicators when excluding the cases in which the concurrent value reported in 2013 (that is, the anchor value) was either the minimum or the maximum on the 10-point scale. See Appendix Table A.6.

5. CONCLUSION

This paper uses a unique panel survey data set to examine the role of anchoring as a source of recall error in four countries in Central America. We find strong evidence in support of anchoring bias for both objective and subjective indicators. Respondents consistently use their reported value in the last wave (2013) as a cognitive heuristic to assist them in recalling the value from a previous period (2012). The results are robust to alternative model specifications and samples. We further find that, measured concurrently, positive changes in indicators across time strongly correlate with a positive bias, while negative changes strongly correlate with a negative bias. Interestingly, objective indicators show larger biases in response to negative changes, while subjective indicators exhibit larger biases in response to positive changes. This pattern of results complements existing research, which has documented that mood (Bodenhausen, Gabriel, and Lineberger 2000), knowledge (Wilson et al. 1996), and cognitive ability (Bergman et al. 2010) mitigate the magnitude of anchoring bias in different contexts.

We acknowledge some limitations of the results presented. The survey instrument used to generate the data used in this study has a particular structure (that is, an initial set of questions about a given outcome for the most recent period coupled with posterior questions about that outcome in previous periods). This structure enables us to analyze precisely the extent of anchoring bias, but also prevents us from ruling out the possibility that this anchoring would not be present in other survey designs. This feature also prevents us from disentangling whether individuals use their report for 2013 as a reference because it is the most recent (salient) year or because interviewers ask about it first. Similarly, because the survey was carried out with a very specific population (poor smallholder farmers in Central America), the conclusions may not necessarily be generalizable to different populations—whether in terms of location, wealth, or occupation.

Despite these concerns, our data make clear that a naïve analysis using retrospective measures without accounting for the effects of anchoring would provide biased estimates of the effect of a given variable on different objective and subjective measures. In this sense, it is necessary to account for likely cognitive biases that a respondent may be subject to when providing a response, even when considering relatively short reference periods. In agricultural development, this seems especially pertinent. Respondents with low educational levels are frequently asked to recall a series of complex interrelated activities, as well as a wide variety of interactions with labor and other markets, over different recall periods. Beegle, Carleto, and Himelein (2011) and Beegle and others (2014) have documented the particular problems this approach poses when collecting agricultural and consumption data in developing countries.

Overall, this study shows the presence of one particular type of cognitive bias in retrospective data from a household survey. Future research should continue to investigate the potential biases of self-reported retrospective data at a time when such data have become widely used in empirical work, particularly in developing regions. The differential anchoring bias that occurs when researchers compare individuals who have experienced positive versus negative outcome changes across time deserves further analysis, ideally in an experimental setting. Collecting accurate and reliable data, including retrospective data, is critical for effective economic policy research.

APPENDIX: ADDITIONAL ESTIMATIONS

Table A.1 Wu-Hausman test for exogeneity of anchor variable in income and wage regressions

Statistic	Dependent variable	
	Income (1)	Income per capita (2)
	Wu-Hausman test: H_0: variable is exogenous	
F(1,7)	0.078	0.022
Prob. > F	0.79	0.887
	First-stage regressions	
Rainfall instrument	556.949***	128.5834*
	(186.449)	(74.671)
Test of excluded instrument: F(1,7)	10.122	2.271
Prob. > F	0.016**	0.1755

Sources: Authors' calculations.

Notes: Robust standard errors are reported in parentheses, clustered by project. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A.2 Estimation results for objective and subjective outcomes, differentiated effects by country

Panel A: Objective outcomes				
Coefficient	Dependent variable			
	Income (1)	Income per capita (2)	Primary wage (3)	Hours main activity (4)
Anchor value	0.654***	0.658**	0.697*	0.689*
	(0.008)	(0.023)	(0.090)	(0.055)
Concurrent measure	0.083	0.068	-0.326	0.019
	(0.461)	(0.406)	(0.715)	(0.785)
Anchor * El Salvador	-0.117	-0.171	0.260	0.327
	(0.930)	(0.594)	(0.590)	(0.121)
Anchor * Guatemala	0.776	0.732	0.818	0.013
	(0.352)	(0.391)	(0.434)	(0.996)
Anchor * Nicaragua	0.087	-0.023	0.310	-0.203
	(0.844)	(0.797)	(0.402)	(0.262)
Concurrent * El Salvador	-0.032	0.052	0.089	-0.007
	(0.547)	(0.617)	(1.000)	(1.000)
Concurrent * Guatemala	-0.137	-0.107	0.309	-0.033
	(0.273)	(0.250)	(0.715)	(0.605)
Concurrent * Nicaragua	-0.108	-0.068	0.328	-0.012
	(0.258)	(0.391)	(0.738)	(0.895)
Project fixed effects	+	+	+	+
Individual characteristics	+	+	+	+
Interview characteristics	+	+	+	+
p-value (H_0: anchor ≥ 0.5)	0.822	0.812	0.759	1.000
Observations	554	554	554	554

Table A.2 Continued

Panel B: Subjective outcomes				
Coefficient	**Dependent variable**			
	Happy	**Healthy**	**Stress**	**Well-being**
	(1)	**(2)**	**(3)**	**(4)**
Anchor value	0.548	0.501*	0.541***	0.534**
	(0.102)	(0.070)	(0.004)	(0.039)
Concurrent measure	0.052	0.096	0.111*	0.024
	(0.227)	(0.219)	(0.051)	(0.258)
Anchor * El Salvador	0.030	0.294	0.328	0.235
	(0.852)	(0.242)	(0.191)	(0.156)
Anchor * Guatemala	0.153	0.218	0.253	0.044
	(0.422)	(0.609)	(0.332)	(1.000)
Anchor * Nicaragua	0.101	-0.127	0.285	0.053
	(0.156)	(0.328)	(0.145)	(0.719)
Concurrent * El Salvador	-0.091	-0.086	-0.157**	-0.047
	(0.211)	(0.375)	(0.012)	(0.148)
Concurrent * Guatemala	0.002	-0.048	-0.100	0.140
	(1.000)	(0.508)	(0.168)	(0.648)
Concurrent * Nicaragua	-0.063	-0.183*	-0.133	0.115
	(0.586)	(0.086)	(0.113)	(0.180)
Project fixed effects	+	+	+	+
Individual characteristics	+	+	+	+
Interview characteristics	+	+	+	+
p-value (H_0: anchor ≥ 0.5)	0.837	0.509	0.715	0.722
Observations	554	554	554	554

Sources: Authors' calculations.

Notes: "Dependent variable" is the reported value in 2013 for the outcome in 2012. "Anchor value" refers to the value reported in 2013 for the outcome in 2013. "Concurrent measure" refers to the value reported in 2012 for the outcome in 2012. El Salvador, Guatemala, and Nicaragua are dummy variables for each country (Honduras is the base category). "Individual characteristics" include respondent age, gender, years of education, household size, and size of primary agricultural plot. "Interview characteristics" include age and gender of enumerator and duration of survey. Wild-bootstrapped *p*-values are reported in parentheses and clustered at the project level (eight clusters), following Cameron and others (2008), using 256 iterations; p-value of one-sided Wald test for anchor ≥ 0.5 reported at the bottom also obtained via bootstrapping method. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A.3 Estimation results for objective outcomes, pooled regression

Coefficient	Dependent variable			
	Income	Per capita income	Primary wage	Hours main activity
	(1)	(2)	(3)	(4)
Anchor	0.640***	0.575***	0.821***	0.744***
	(0.000)	(0.000)	(0.004)	(0.008)
Concurrent measure	0.024*	0.033**	0.028***	0.02
	(0.070)	(0.047)	(0.004)	(0.281)
Time fixed effects	+	+	+	+
Project fixed effects	+	+	+	+
Individual characteristics	+	+	+	+
Interview characteristics	+	+	+	+
p-value (H_0: anchor ≥ 0.5)	0.828	0.839	0.959	1.000
Observations	1108	1108	1108	1105

Sources: Authors' calculations.

Notes: Pooled regression for values reported over periods 2011 to 2012 and 2012 to 2013. Sample size reflects two periods for each of 554 individuals (three individuals have missing values for "hours main activity" in 2011). "Dependent variable" is the reported value in the current year for the outcome in the preceding year. "Anchor" refers to the value reported in the current year for the outcome in the current year. "Concurrent measure" refers to the value reported in the preceding year for the outcome in the preceding year. "Individual characteristics" include respondent age, gender, years of education, household size, and size of primary agricultural plot. "Interview characteristics" include age and gender of enumerator and duration of survey. Wild-bootstrapped *p*-values are reported in parentheses and clustered at the project level (eight clusters), following Cameron and others (2008), using 256 iterations. p-value of one-sided Wald test for anchor ≥ 0.5 reported at the bottom also obtained via bootstrapping method. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A.4 Estimation results for objective outcomes, two-year recall

Coefficient	Dependent variable			
	Income	Per capita income	Primary wage	Hours main activity
	(1)	(2)	(3)	(4)
Anchor	0.926***	0.746***	0.710**	0.685**
	(0.004)	(0.004)	(0.016)	(0.012)
Concurrent measure	0.006	0.025*	0.118	-0.01
	(0.645)	(0.051)	(0.117)	(0.777)
Project fixed effects	+	+	+	+
Individual characteristics	+	+	+	+
Interview characteristics	+	+	+	+
p-value (H_0: anchor ≥ 0.5)	0.988	0.998	0.789	0.998
Observations	554	554	554	551

Sources: Authors' calculations.

Notes: "Dependent variable" is the reported value in 2013 for the outcome in 2011. "Anchor value" refers to the value reported in 2013 for the outcome in 2013. "Concurrent measure" refers to the value reported in 2011 for the outcome in 2011. "Individual characteristics" include respondent age, gender, years of education, household size, and size of primary agricultural plot. "Interview characteristics" include age and gender of enumerator and duration of survey. Wild-bootstrapped *p*-values are reported in parentheses and clustered at the project level (eight clusters), following Cameron and others (2008), using 256 iterations. *p*-value of one-sided Wald test for anchor ≥ 0.5 reported at the bottom also obtained via the bootstrapping method. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A.5 Estimation results for subjective outcomes, excluding cases when minimum or maximum value reported

Coefficient	Dependent variable			
	Happiness	Health	Stress	Well-being
	(1)	(2)	(3)	(4)
Anchor	0.633***	0.494***	0.664***	0.598***
	(0.000)	(0.004)	(0.004)	(0.008)
Concurrent measure	0.084*	0.055*	0.055	0.062
	(0.078)	(0.074)	(0.387)	(0.141)
Project fixed effects	+	+	+	+
Individual characteristics	+	+	+	+
Interview characteristics	+	+	+	+
p-value (H_0: anchor ≥ 0.5)	0.994	0.473	0.983	0.951
Observations	395	417	478	420

Sources: Authors' calculations.

Notes: The estimations exclude cases in which the minimum or maximum value of the subjective indicator (that is, 1 or 10) was reported in 2013. "Dependent variable" is the reported value in 2013 for the outcome in 2012. "Anchor value" refers to the value reported in 2013 for the outcome in 2013. "Concurrent measure" refers to the value reported in 2012 for the outcome in 2012. "Individual characteristics" include respondent age, gender, years of education, household size, and size of primary agricultural plot. "Interview characteristics" include age and gender of enumerator and duration of survey. Wild-bootstrapped *p*-values are reported in parentheses and clustered at the project level (eight clusters), following Cameron and others (2008), using 256 iterations; p-value of one-sided Wald test for anchor ≥ 0.5 reported at the bottom obtained via bootstrapping method. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A.6 Estimation results for subjective outcomes, differential responses to changes in concurrent measures excluding cases when minimum or maximum value reported

Coefficient	Dependent variable			
	Happiness	Health	Stress	Well-being
Positive difference	1.013***	0.933***	0.823***	0.946***
	(0.000)	(0.000)	(0.008)	(0.008)
Negative difference	-0.642***	-0.649***	-0.840***	-0.686**
	(0.008)	(0.008)	(0.008)	(0.016)
Project fixed effects	+	+	+	+
Individual characteristics	+	+	+	+
Interview characteristics	+	+	+	+
p-value (H_0: positive ≥ 0.5)	1.000	1.000	0.995	1.000
p-value (H_0: negative ≤ -0.5)	0.929	0.959	1.000	0.988
p-value (H_0: positive + negative ≥ 0)	0.929	0.959	0.000***	0.988
Observations	395	417	478	420

Sources: Authors' calculations.

Notes: The estimations exclude cases in which the minimum or maximum value of the subjective indicator (that is, 1 or 10) was reported in 2013. "Dependent variable" is the difference between the value for 2012 reported in 2013 and the value for 2012 reported in 2012. "Positive difference" is the value of the difference between the 2013 value reported in 2013 and the 2012 value reported in 2012 if that difference is greater than 0; it takes the value of 0 otherwise. "Negative difference" is the difference between 2013 and 2012 concurrent values (in absolute value) if the difference is less than 0; it is 0 otherwise. "Individual characteristics" include respondent age, gender, years of education, household size, and size of primary agricultural plot. "Interview characteristics" include age and gender of enumerator and duration of survey. Wild-bootstrapped *p*-values are reported in parentheses and clustered at the project level (eight clusters), following Cameron and others (2008), using 256 iterations-p-value of one-sided Wald tests reported at the bottom also obtained via bootstrapping method. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Table A.7 Estimation results for objective and subjective outcomes, differential responses to changes in concurrent measures controlling for municipality modal outcome pattern

Panel A: Objective outcomes				
Coefficient	**Dependent variable**			
	Income	**Income per capita**	**Primary wage**	**Hours main activity**
	(1)	**(2)**	**(3)**	**(4)**
Positive difference	0.684**	0.540**	0.736	0.881***
	(0.039)	(0.012)	(0.102)	(0.000)
Negative difference	-1.016***	-1.000**	-1.003***	-0.719***
	(0.008)	(0.012)	(0.008)	(0.008)
Positive municipality modal pattern	37.096***	8.624	113.951***	-2.173
	(0.000)	(0.266)	(0.000)	(0.625)
Negative municipality modal pattern	28.101**	6.777	118.649***	-3.749
	(0.016)	(1.000)	(0.000)	(0.398)
Project fixed effects	+	+	+	+
Individual characteristics	+	+	+	+
Interview characteristics	+	+	+	+
p-value (H_0: positive ≥ 0.5)	0.793	0.586	0.819	1.000
p-value (H_0: negative ≤ -0.5)	1.000	1.000	1.000	0.999
p-value (H_0: positive + negative ≤ 0)	1.000	1.000	1.000	0.001***
Observations	554	554	554	554

Panel B: Subjective outcomes				
Coefficient	**Dependent variable**			
	Happiness	**Health**	**Stress**	**Well-being**
Positive difference	1.019***	0.920***	0.714***	0.928***
	(0.000)	(0.004)	(0.008)	(0.000)
Negative difference	-0.521**	-0.546**	-0.864**	-0.581***
	(0.016)	(0.012)	(0.016)	(0.008)
Positive municipality modal pattern	-0.414	-0.04	0.485	0.345
	(0.172)	(0.461)	(1.000)	(0.328)
Negative municipality modal pattern	-0.269***	0.041	0.598	0.277
	(0.008)	(1.000)	(0.680)	(0.766)
Project fixed effects	+	+	+	+
Respondent characteristics	+	+	+	+
Interview characteristics	+	+	+	+
p-value (H_0: positive ≥ 0.5)	1.000	1.000	0.984	1.000
p-value (H_0: negative ≤ -0.5)	0.643	0.699	0.999	0.933
p-value (H_0: positive + negative ≥ 0)	0.643	0.699	0.001***	0.933
Observations	554	554	554	554

Sources: Authors' calculations.

Notes: "Dependent variable" is the difference between the value for 2012 reported in 2013 and the value for 2012 reported in 2012. "Positive difference" is the value of the difference between the 2013 value reported in 2013 and the 2012 value reported in 2012 if that difference is greater than 0; it takes the value of 0 otherwise. "Negative difference" is the difference between the 2013 and 2012 concurrent values (in absolute value) if the difference is less than 0; it is 0 otherwise. "Positive municipality modal pattern" is an indicator variable that takes the value of 1 if the majority of other respondents in the municipality report a positive change in the outcome between 2012 and 2013; it takes the value of 0 otherwise. "Negative municipality modal pattern" is the converse. "Individual characteristics" include respondent age, gender, years of education, household size, and size of primary agricultural plot. "Interview characteristics" include age and gender of enumerator and duration of survey. Wild-bootstrapped *p*-values are reported in parentheses and clustered at the project level (eight clusters), following Cameron and others (2008), using 256 iterations; p-value of one-sided Wald tests reported at the bottom also obtained via bootstrapping method. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

REFERENCES

Alevy, J., C. Landry, and J. A. List. 2011. *Field Experiments of Anchoring of Economic Valuations.* University of Alaska Working Paper 2011-02. Seattle, WA, US: University of Alaska.

Ariely, D., G. Loewenstein, and D. Prelec. 2003. "'Coherent Arbitrariness': Stable Demand Curves without Stable Preferences." *The Quarterly Journal of Economics* 118: 73–105.

Beegle, K., C. Carletto, and K. Himelein. 2011. *Reliability of Recall in Agricultural Data.* World Bank Development Research Group Working Paper WPS5671. Washington, DC: World Bank.

Beegle, K., J. de Weerd, J. Friedman, and J. Gibson. 2014. *The Challenge of Measuring Hunger*. World Bank Policy Research Working Paper WPS6736. Washington, DC: World Bank.

Beggs, A., and K. Graddy. 2009. "Anchoring Effects: Evidence from Art Auctions." *American Economic Review* 99 (3): 1027–1039.

Bergman, O., T. Ellingsen, M. Johannesson, and C. Svensson. 2010. "Anchoring and Cognitive Ability." *Economics Letters* 107 (1): 66–68.

Bodenhausen, G. V., S. Gabriel, and M. Lineberger. 2000. "Sadness and Susceptibility to Judgemental Bias: The Case of Anchoring." *Psychological Science* 11 (4): 320–323.

Bound, J., C. Brown, and N. Mathiowetz. 2001. "Measurement Error in Survey Data." In *Handbook of Econometrics.* Vol. 5, edited by J.J. Heckman and E. Leamer, 3705–3843. Amsterdam: Elsevier.

Cameron, C., J. B. Gelbach, and D. L. Miller. 2008. "Bootstrap-Based Improvements for Inference with Clustered Errors." *The Review of Economics and Statistics* 90 (3): 414–427.

Campbell, S. D., and S. A. Sharpe. 2009. "Anchoring Bias in Consensus Forecasts and Its Effect on Market Prices." *The Journal of Financial and Quantitative Analysis* 44 (2): 369–390.

de Nicola, F., and X. Giné. 2014. "How Accurate Is Recall Data? Evidence from Coastal India." *Journal of Development Economics* 106: 55–65.

Evans, D. S., and L. S. Leighton. 1995. "Retrospective Bias in the Displaced Worker Surveys." *The Journal of Human Resources* 30 (2): 386–396.

Frykblom, P., and J. F. Shogren. 2000. "An Experimental Testing of Anchoring Effects in Discrete Choice Questions." *Environmental and Resource Economics* 16: 329–341.

Fudenberg, D., D. K. Levine, and Z. Maniadis. 2012. "On the Robustness of Anchoring Effects in WTP and WTA Experiments." *American Economic Journal: Microeconomics* 4 (2): 131–145.

Furnham, A., and H. Chu Boo. 2011. "A Literature Review of the Anchoring Effect." *The Journal of Socio-economics* 40: 35–42.

Goldin, J., and D. Reck. 2015. "Framing Effects in Survey Research: Consistency-Adjusted Estimators." Unpublished, Stanford Law School, CA, US.

Hernandez, M. A., and M. Torero. 2014. "Poverty-Sensitive Scorecards to Prioritize Lending and Grant Allocation with an Application in Central America." In *Finance for Food: Towards New Agricultural and Rural Finance*, edited by Doris Köhn, 263–283, New York: Springer.

———. 2015. "A Poverty-Sensitive Scorecard to Prioritize Lending: Evidence from Central America." Unpublished, International Food Policy Research Institute, Washington, DC.

Hurd, M. D., D. McFadden, H. Chand, L. Gan, A. Merrill, and M. Roberts. 1998. "Consumption and Savings Balances of the Elderly: Experimental Evidence on Survey Response Bias." In *Frontiers in the Economics of Aging*, edited by D. A. Wise, 353–392. Cambridge, MA, US: National Bureau of Economic Research.

Krueger, A. B., and D. A. Schkade. 2008. "The Reliability of Subjective Well-Being Measures." *Journal of Public Economics* 92 (8–9): 1833–1845.

Maniadis, Z., F. Tufano, and J. A. List. 2014. “One Swallow Doesn’t Make a Summer: New Evidence on Anchoring Effects.” *American Economic Review* 104 (1): 277–290.

Maruyama, E. 2007. “Remembering Earnings During Unstable Times: Evidence from the Indonesian Family Life Surveys*.*” Unpublished, International Food Policy Research Institute, Washington, DC.

Miguel, E., S. Satyanath, and E. Sergenti. 2004 “Economic Shocks and Civil Conflict: An Instrumental Variables Approach.” *Journal of Political Economy* 112 (4): 725–753.

Neter, J., and J. Waksberg. 1964. “A Study of Response Errors in Expenditures Data from Household Interviews.” *Journal of the American Statistical Association* 59 (305): 18–55.

Sudman, S., N. M. Bradburn, and N. Schwarz. 1996. *Thinking about Answers: The Application of Cognitive Processes to Survey Methodology.* San Francisco: Jossey-Bass.

Tversky, A., and D. Kahneman. 1974. “Judgement under Uncertainty: Heuristics and Biases.” *Science* (185):1124–1131.

Wilson, T. D., C. E. Houston, K. M. Etling, and N. Brekke. 1996. “A New Look at Anchoring Effects: Basic Anchoring and Its Antecedents.” *Journal of Experimental Psychology* 125 (4): 387–402.

RECENT IFPRI DISCUSSION PAPERS

For earlier discussion papers, please go to www.ifpri.org/pubs/pubs.htm#dp.
All discussion papers can be downloaded free of charge.

1533. *Contracting by small farmers in commodities with export potential: Assessing farm profits of lentil growers in Nepal.* Anjani Kumar, Devesh Roy, Gaurav Tripathi, P. K. Joshi, and Rajendra P. Adhikari, 2016.

1532. *Rent dispersion in the US agricultural insurance industry.* Vincent Smith, Joseph Glauber, and Robert Dismukes, 2016.

1531. *Long-term drivers of food and nutrition security.* David Laborde, Fahd Majeed, Simla Tokgoz, and Maximo Torero, 2016.

1530. *Understanding compliance in programs promoting conservation agriculture: Modeling a case study in Malawi.* Patrick S. Ward, Andrew R. Bell, Klaus Droppelmann, and Tim Benton, 2016.

1529. *A model of reporting and controlling outbreaks by public health agencies.* Alexander E. Saak and David A. Hennessy, 2016.

1528. *Boserupian pressure and agricultural mechanization in modern Ghana.* Frances Cossar, 2016.

1527. *Agricultural mechanization and agricultural transformation.* Xinshen Diao, Jed Silver, and Hiroyuki Takeshima, 2016.

1526. *Delegation of quality control in value chains*. Alexander E. Saak, 2016.

1525. *Structural transformation and intertemporal evolution of real wages, machine use, and farm size–productivity relationships in Vietnam.* Yanyan Liu, William Violette, and Christopher B. Barrett, 2016.

1524. *Can contract farming increase farmers' income and enhance adoption of food safety practices?: Evidence from remote areas of Nepal.* Anjani Kumar, Devesh Roy, Gaurav Tripathi, P. K. Joshi, and Rajendra P. Adhikari, 2016.

1523. *Effectiveness of food subsidies in raising healthy food consumption: Public distribution of pulses in India.* Suman Chakrabarti, Avinash Kishore, and Devesh Roy, 2016.

1522. *Findings across agricultural public expenditure reviews in African countries*. Stephen D. Mink, 2016.

1521. *Risk and sustainable crop intensification: The case of smallholder rice and potato farmers in Uganda.* Bjorn Van Campenhout, Emmanuel Bizimungu, and Dorothy Birungi, 2016.

1520. *Varietal integrity, damage abatement, and productivity: Evidence from the cultivation of Bt cotton in Pakistan.* Xingliang Ma, Melinda Smale, David J. Spielman, Patricia Zambrano, Hina Nazli, and Fatima Zaidi, 2016.

1519. *Institutional arrangements to make public spending responsive to the poor—(where) have they worked?: Review of the evidence on four major intervention types.* Tewodaj Mogues and Alvina Erman, 2016.

1518. *A poverty-sensitive scorecard to prioritize lending and grant allocation: Evidence from Central America.* Manuel A. Hernandez and Maximo Torero, 2016.

1517. *Can information help reduce imbalanced application of fertilizers in India?: Experimental evidence from Bihar.* Ram Fishman, Avinash Kishore, Yoav Rothler, Patrick S. Ward, Shankar Jha, and R. K. P. Singh, 2016.

1516. *Pakistan's fertilizer sector: Structure, policies, performance, and impacts.* Mubarik Ali, Faryal Ahmed, Hira Channa, and Stephen Davies, 2016.

1515. *Agriculture-nutrition linkages and child health in the presence of conflict in Nepal.* Elizabeth Bageant, Yanyan Liu, and Xinshen Diao, 2016.

1514. *"As a husband i will love, lead, and provide": Gendered access to land in Ghana*. Isabel Lambrecht, 2016.

1513. *Formal versus informal: Efficiency, Inclusiveness, and financing of dairy value chains in India.* Pratap S. Birthal, Ramesh Chand, P. K. Joshi, Raka Saxena, Pallavi Rajkhowa, Md. Tajuddin Khan, Mohd Arshad Khan, and Khyali R. Chaudhary, 2016.

1512. *Measuring women's disempowerment in agriculture in Pakistan*. Nuzhat Ahmad and Huma Khan, 2016.

1511. *The impact of conditional cash transfer programs on indigenous households in Latin America: Evidence from PROGRESA in Mexico*. Esteban J. Quiñones and Shalini Roy, 2016.

1510. *Why some are more equal than others: Country typologies of food security.* Eugenio Díaz-Bonilla and Marcelle Thomas, 2016.

INTERNATIONAL FOOD POLICY RESEARCH INSTITUTE

www.ifpri.org

IFPRI HEADQUARTERS

2033 K Street, NW
Washington, DC 20006-1002 USA
Tel.: +1-202-862-5600
Fax: +1-202-467-4439
Email: ifpri@cgiar.org