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Estimating Hypothetical Bias in Economically Emergent Africa: A Generic Public Good Experiment

Arthur J. Caplan, David Aadland, and Anthony Macharia

This paper reports results from a contingent valuation based public good experiment conducted in the African nation of Botswana. In a sample of university students, we find evidence that stated willingness to contribute to a public good in a hypothetical setting is higher than actual contribution levels. However, results from regression analysis suggest that this is true only in the second round of the experiment, when participants making actual contributions have learned to significantly lower their contribution levels. As globalization expands markets, and economies such as Botswana's continue to modernize, there is a growing need to understand how hypothetical bias will influence the valuation of public goods.

Key Words: hypothetical bias, public good, willingness to pay, Botswana

Experimental studies of bargaining behavior and public good provision have recently been extended to international and cross-cultural settings. For example, Roth et al. (1991) find that latent cultural differences partially explain observed variation in two-player ultimatum bargaining games, but not in multi-player market behavior. Henrich (2000) finds a similar cultural effect for ultimatum bargaining between a sample of U.S. graduate students and Machiguenga tribesmen in the Peruvian Amazon.[1] In a more recent paper, Ehmke, Lusk, and List (2008) find that hypothetical bias in contingent valuation (CV) differs across location and cultures.

Taken together, these experimental studies suggest that cultural differences can help explain variation in behavior associated with standard bargaining and public good valuation frameworks.[2] The current paper adds to this experimental literature by providing a preliminary test for hypothetical bias in the provision of public goods in economically emergent Africa.[3,4] In this way, our

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[1] Henrich et al. (2001) expand the scope of these findings to 15 small-scale societies in 12 countries on five continents.

[2] To the contrary, Slonim and Roth (1998) and Cameron (1999) find little or no evidence of a cultural effect on ultimatum bargaining behavior. Cardenas and Carpenter (2008) provide an exhaustive survey of field experiments conducted in the developing world. The experiments focus on individual preferences in four general categories: (i) propensity to cooperate in social dilemmas, (ii) trust and reciprocity, (iii) norms of fairness and altruism, and (iv) risk and time preference. They conclude that cooperation does in fact exist in category (i). Macroeconomic conditions impact categories (ii) and (iii). With respect to category (iv), people in developing countries are not necessarily more risk averse, yet impatience results are mixed.

[3] Hypothetical bias is any deviation of an individual's stated willingness to pay (WTP) from his actual WTP due to the hypothetical nature of the good or payment mechanism. Positive (negative) hypothetical bias occurs when stated willingness to contribute is higher (lower) than the actual contribution level. Note that we are careful not to substitute "true willingness to contribute" for "actual contribution level" here, as our econometric model's link with random utility theory (see the Eco-

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study adds to the accumulating body of knowledge about how to test for the effects of different cultural or national identities on economic behavior. As globalization expands markets, and economies such as Botswana's continue to modernize, there is a growing need to understand how cultural factors influence the subjective valuation of public goods.

In contrast to Ehmke, Lusk, and List's (2008) result of *negative* hypothetical bias for university students in Niger, we find evidence of *positive* hypothetical bias in our sample of university students in Botswana. In other words, we find evidence that stated willingness to invest in a public good in a hypothetical setting is higher than actual investment levels. However, results from our analyses suggest that this is true only in the second round of the experiment, when participants making actual contributions have learned to significantly lower their investment levels. These preliminary results suggest that further research regarding the valuation of public goods should target a broader, more representative sample of Botswana's citizens.[5]

The contrasts between Ehmke, Lusk, and List (2008) and this paper also extend to the experimental designs and empirical methodologies. Ehmke, Lusk, and List use a within-subject design, where all 60 participants are confronted with a hypothetical public good choice in the first round of the experiment and then the same participants are confronted with an actual choice in the second round of the experiment. As a result, the authors cannot be sure whether the valuation differences are due to between-round learning or hypothetical versus actual incentives.

Also, Ehmke, Lust, and List choose a multinomial logit model, where the dependent variable represents four potential categories of responses (yes-yes, no-no, yes-no, and no-yes) to a non-randomized bid for the hypothetical and actual scenarios, respectively. Using this type of model, the authors are able to establish the existence of hypothetical bias as well as identify regional effects (i.e., whether there are statistical differences in participation across the locations: Indiana/Kansas in the United States, China, Niger, and France). However, by not having randomized the bid values within each region, the authors are precluded from estimating the magnitude of within-region hypothetical bias.[6]

In this study we use a between-subject design, where our sample of 100 participants is first divided into hypothetical and actual sub-samples, and then each sub-sample participates in two separate rounds of the experiment (the next section provides a detailed description of the experimental design). As a result, we are able to isolate the effect of between-round learning from the effect of hypothetical bias.

We estimate a bivariate probit model to account for possible error correlation between the respondent's first- and second-round investment decisions and find evidence of hypothetical bias in the second round of the experiment. Then, using two separate univariate probit models, we test for symmetry in the between-round learning effect, and find that individuals making actual investments are more likely to switch from having said "yes" to their (randomized) bid in round one to saying "no" in round two. In other words, only individuals in the actual treatment learn that free-

nometric Model section for more detail) assumes truth itself is probabilistic [as Harrison (2006) succinctly puts it, what matters is not truth *per se*, but rather a well-established empirical point of reference]. Further, we acknowledge that reality is context-specific, i.e., dependent upon the social context within which an individual formulates his valuation of the good (Harrison and List 2004, List et al. 2004, Huck and Weizäcker 2002, List 2006, List 2003, and Lusk and Norwood, forthcoming).

[4] We say "preliminary" in order to emphasize the fact that, similar to the vast majority of laboratory studies in the literature, our sample is restricted to a relatively small group of university students (a restriction necessitated by the high cost associated with running public good experiments such as ours). Thus, the existence of hypothetical bias among older and less-educated generations of today remains an open research question. Although a plethora of WTP estimates exist for public goods in developing nations (e.g., see Pearce, Pearce, and Palmer 2002), none that we are aware of, other than Ehmke, Lusk, and List (2008), explicitly address the issue of hypothetical bias. See Murphy et al. (2005) for a meta-analysis of hypothetical bias in stated-preference valuation.

[5] Although exceptions exist (e.g., Carson et al. 1996, Johannesson 1997, Smith and Mansfield 1998, Champ and Bishop 2001, Vossler and Kerkvliet 2003, Johnston 2006, Haab, Huang, and Whitehead 1999, and Smith 1999), our finding of positive hypothetical bias is consistent with the majority of experiments and field surveys in the literature (e.g., List and Gallet 2001, Little and Berrens 2004, Murphy and Stevens 2004, Murphy et al. 2005, Harrison 2006, Harrison and List 2004, Cummings, Harrison, and Rutström 1995, and Harrison and Rutström 2006). Our results are also consistent with the fact that the relevant effects are often not found until a few rounds of the experiment have been completed (Ledyard 1995).

[6] Ehmke, Lusk, and List (2008) also test whether culture affects hypothetical bias using a reduced-form binary logit model, where the dependent variable takes the value of one if the respondent exhibits either positive or negative hypothetical bias (i.e., votes either yes-no or no-yes, respectively) and the value of zero if the person exhibits no hypothetical bias (i.e., votes either yes-yes or no-no). They find that the individuals from more "masculine" and "individualistic" societies are more likely to exhibit hypothetical bias.

riding pays.[7]

In the next section, we discuss the experimental design used in this study to test for hypothetical bias. In the section "Data and Unconditional Tests" we discuss both our sample frame and the data obtained from the public good experiment. This section also provides summary statistics and unconditional tests for the presence of hypothetical bias and between-round learning effects in our sample. Our empirical model is presented in the "Econometric Model" section, followed by a results section. We conclude with a discussion of this study's limitations and avenues for future research.

Experimental Design

As alluded to in the previous section, a primary objective of the experiment is to create a laboratory to test for the magnitude of hypothetical bias in the valuation of a public good. To accomplish this objective, we incorporate several features into the experiment.[8]

First, we elicit values for a "generic" public good that is less prone to "homegrown" assessments by the participants and less affected by the existence of field substitutes. Homegrown values are infused into the experiment by participants from their prior experiences valuing similar public goods (i.e., field substitutes), which are independent from the induced values provided by the experimenter (Harrison 2006, Cummings, Harrison, and Rutström 1995, Smith 1976). In this way, we lessen the chance that our measure of hypothetical bias is confounded by social determinants of the good's value. For example, if we had instead selected "expanded wilderness protection in the Kalahari Desert" or "private funding for secondary education" as the public good for which values were to be elicited, social pressures such as the "purchase of moral satisfaction" (Kahneman and Knetsch 1992) and the "desire to conform socially" (Bernheim 1994) would have been likely to confound our estimates of hypothetical bias.[9] Further, our goal was to maintain as many traditional features of public good experiments as possible, such as induced valuation and the incentive to free-ride. This is perhaps best accomplished by eliciting values for a more generic public good.

Second, we wish to create a scenario that closely mimics how CV surveys have traditionally been conducted in the field.[10] This entails elicitation of maximum willingness to pay (WTP) for a public good without the imposition of a provision-point mechanism, or what Carson and Groves (2007) call a "coercive payment" scheme. A provision-point mechanism typically sets a minimum positive aggregate contribution threshold necessary for provision of the public good (Rondeau, Schulze, and Poe 1999). The main advantage of this type of mechanism is its incentive-compatibility (Carson and Groves 2007, Cummings et al. 1997). However, in cases where a realistic provision point is unknown, which seems to be the predominant case in the CV literature, imposition of such a mechanism is unrealistic. We therefore use a dichotomous-choice donation mechanism so that a minimum positive aggregate contribution threshold is not arbitrarily set prior to eliciting the participants' WTP values.[11]

[7] It may in fact be more correct to end this sentence with "….learn that *more* free-riding pays." The fact that at least some participants in each treatment answer "no" to their bid amounts indicates that free-riding potentially exists in each treatment.

[8] The complete experimental design is provided in the Appendix.

[9] Expanded wilderness protection in the Central Kalahari Desert and private funding of secondary school education are two popular issues in Botswana at the moment. Ehmke, Lusk, and List (2008) use bottled mineral water as their public good because it is available and consumed in each country included in their sample and therefore less likely to induce social pressures. Of course, as Harrison and List (2004) point out, lab experiments, no matter how "sterile," are never completely free of context.

[10] We acknowledge that there are well-known limits to how well a hypothetical treatment in a laboratory experiment can mimic CV surveys. For example, Cummings and Harrison (1994) point out that there is no empirical evidence to suggest that laboratory experiments and CV surveys produce similar results [although Carson and Groves (2007) cite more recent studies that find similarities between CV surveys and experiments]. Carson and Groves (2007) point out that the context within which values are elicited in CV surveys—in particular the degree of consequentiality as perceived by respondents—is an important determinant of whether the values are incentive-compatible [Landry and List (2007) find empirical evidence from a field experiment to support this claim]. On the other hand, CV surveys run the risk of presenting respondents with goods and prices that may be perceived as being implausible or uncertain in terms of their actual costs and the probability of their ever being provided. Harrison and List (2004) reach similar conclusions in their comparisons of CV surveys and field experiments, and extend the catalogue of distinctions between the two methods to differences in sample selection, participant experience and heuristics, nature of the commodity being valued (e.g., the availability of substitutes for the commodity), and the stakes involved. In the end, they argue that experiments and field surveys are meant to be methodologically complementary, not substitutes for one another.

[11] Note that this mechanism effectively sets a provision point at zero, i.e., if no one makes a positive investment in the public good, then the net payout to everyone is zero. See below for more details about the investment decision and what is meant by "net payout." By comparison, Ehmke, Lusk, and List (2008) use a provision-point mechanism in their experiments.

Third, we designed the experiment to test for hypothetical bias in our sample. The existence of hypothetical bias indicates that although individuals may wish to contribute at high levels, they understand the inherent coordination problems and incentives to deviate from the cooperative strategy. Toward this end, half the participants were given the option of contributing to the public good using real money (actual group), while the other half simply stated their hypothetical contribution level (hypothetical group). By contrasting the average contribution levels of the two groups, we are able to directly test for the existence of hypothetical bias.

Fourth, in addition to testing for a between-round learning effect, we provide an information treatment where half the participants read through an example of the experiment themselves and then the researcher quickly re-reads the example out loud. Participants were allowed to ask questions about the experiment at any point in time. Also as part of this information treatment, two sentences were added to the second-to-last paragraph of the example:

> What this row of numbers tells us is that the payout is 5 Pula for a person who chose to invest something and 10 Pula for a person who chose to invest nothing. Now, let's see how much Pula each of the five people participating in this example takes home with them from the experiment.

Participants in the information control group read through the example on their own, without any additional input provided by the researcher and without inclusion of the two sentences above.[12] Inclusion of this treatment in our experimental design reflects a pervasive concern about "information bias" in the CV literature (Ajzen, Brown, and Rosenthal 1996, Smith and Desvousges 1986).

To begin the experiment, each participant was provided with 50 Pula in cash (approximately US$10) with which to make an investment decision in the public good (the money was paperclipped to the experiment's instruction sheet). Participants in the hypothetical treatment were reminded that they would "not be paid anything more or less," while participants in the actual treatment were informed that they were "investing for real." This type of distinction between the hypothetical and actual treatments was reiterated in the directions for the experiment (see Appendix).

As the Payout Chart makes clear, the investment decision incorporates a free-riding incentive and a prisoner's dilemma (as well as the properties of non-exclusion and non-rivalry in consumption). The incentive for free-riding occurs because, all else equal, those who choose not to invest any of their 50 Pula obtain a higher payout than those who choose to invest some positive amount. A prisoner's dilemma occurs because choosing to invest increases the average group investment, which in turn leads to a higher payout for everyone.

As mentioned above, the investment question (see Appendix) is presented in a single-bounded dichotomous-choice format. In the case of the actual treatment (the wording for the hypothetical treatment is similar) the investment question reads, "This question requires a choice for which your net payout from the experiment will ultimately be determined." The bid amounts (used in place of "**XX**") were randomly and uniformly selected from the interval (5, 15, 25, 35, and 45 Pula). Based on her response to her specific bid amount, the participant's latent WTP may then be placed in one of two regions: ($-\infty$, bid amount) in the event of answering "no" to the WTP question, and (bid amount, ∞) in the event of answering "yes."

After answering the investment question (and thus completing round one of the experiment), each participant was provided with a Net Payout Worksheet. The worksheet enabled a participant to calculate her net payout from round one, and thus determine the total amount of money remaining if there was going to be only one payout. In the process of determining her own net payout, the participant also obtained information on the average donation of the group, which in turn could have conditioned her decision to cooperate or not in the next round.

Each participant then repeated the experiment again in round two, facing the same respective bid amount as was randomly drawn in round one. By not varying a given participant's bid amount between rounds we ensured that any change in her response to the investment question would be based solely on any additional information she had gained from completing the Net Payout Work-

[12] The experimental design presented in the Appendix is for the hypothetical and information treatments. The designs for the other treatments are available from the authors upon request.

sheet. In order to mimic typical field-survey conditions, where respondents' calculations are not overseen by the researcher, we purposefully did not check the students' worksheets for any miscalculations. Rather, we created two control variables for our regression analysis based on whether a respondent made any mistake(s) on the worksheet.[13]

Upon completion of round two, a fair coin was flipped to determine which of the two rounds would determine the participants' actual net payout. The participants were informed of the coin-flip procedure prior to beginning round one. The reason for randomizing which net payout would actually be paid, rather than simply basing the payout on round two's outcome, was to induce the students to answer the investment question in round one more seriously than they otherwise might have. Finally, the students answered a series of demographic questions (see the Appendix for the specific wording of the questions).

Data and Unconditional Tests

The experiment was pre-tested with a group of 30 graduate students in the University of Botswana (UB) Business School. Several changes were made to the experimental design as a result of the pre-test, mostly geared toward fine-tuning the instructions. During the week following the pre-test, approximately 100 undergraduate students from the Business School were recruited to participate in the experiment.[14]

The experiment was run in four separate sessions, with approximately 25 students per session.[15] Overall summary statistics for each of the variables are provided in Table 1. As indicated in Table 1, fewer participants answered "yes" to their respective bid amounts in round two of the experiment than in round one (the mean for Yes_1 is larger than the mean for Yes_2). Slightly fewer than half of the participants are male, the average age is approximately 22 years, and most are Botswana citizens in their junior year or below. Few participants classify themselves as being rich in income or as having fathered or mothered a child. The majority consider themselves as being "happy" or "very happy" with their lives. Few participants made "small" or "large" mistakes in calculating their net payouts from round one of the experiment using the Net Payout Worksheet.[16]

Table 2 provides an (unconditional) comparison of the proportions of participants who answered "yes" to their bid amount in rounds one and two of the experiment across the hypothetical (hyp = 1) and actual (hyp = 0) treatments. The comparison between the hypothetical and actual treatments in round one suggests an absence of hypothetical bias (either positive or negative), as the means for Yes_1 (hyp = 0) and Yes_1 (hyp = 1) are not statistically different from one another at the 5 percent significance level. The same comparison for round two, however, shows the existence of positive hypothetical bias since the means of Yes_2 (hyp = 0) and Yes_2 (hyp = 1) are statistically different from one another. Therefore, we find evidence in support of positive hypothetical bias in our sample of UB students, but only after the participants have completed round one of the experiment.

The results in Table 2 can also be used to test for the effect of information that participants received *during* the experiment.[17] Specifically, the mean of Yes_1 (hyp = 1) can be compared with the mean of Yes_2 (hyp = 1) to test for a between-round learning effect in the hypothetical treatment, and the means of Yes_1 (hyp = 0) and Yes_2 (hyp = 0) can likewise be compared for a between-round learning effect in the actual treatment.

The ratio test suggests that participants in the actual treatment responded between rounds by reducing their acceptance of the offered bid: the mean of Yes_2 (hyp = 0) is statistically lower than

[13] For specifics, refer to the definitions of the *smprob* and *bgprob* variables included in Table 1 below.

[14] Our sample was restricted to business students for two reasons. First, this helped reduce the cost of recruiting students to participate in the experiment. Second, it increased the probability that the recruited students would understand the investment nature of the experiment. See Harrison and List (2004) for an insightful discussion about the strengths and weaknesses of using student samples.

[15] The experiment was run on four consecutive days, one session per day, to minimize the potential for students to discuss the experiment with one another. We initially estimated our empirical model with controls for treatment effects and found them to be insignificant, suggesting the absence of a "session effect."

[16] Less than 15 percent of the labor force in Botswana has obtained a tertiary education (World Bank 2009). Based on the 2001 Botswana Census, nationwide there are slightly more females than males, there are slightly more than three children born per woman, and the average age is slightly over 36 years (CIA World Factbook 2006). Mean monthly income is approximately 3,500 Pula, which is slightly less than US$600 at the time of study (World Resources Institute 2009). Thus, by comparison, the average individual in our student sample has fewer children and is younger and poorer than the national average.

[17] Empirical tests for the effect of information provided *prior* to the experiment are discussed in the next section (see Table 4).

Table 1. Variable Names, Definitions, Sample Means and Standard Deviations (N = 102)

Variable Name	Definition	Mean	SD
Yes_1	= 1 if "yes" to bid amount in the first round of the experiment, = 0 otherwise	0.46	0.50
Yes_2	= 1 if "yes" to bid amount in the second round of the experiment, = 0 otherwise	0.38	0.49
τ	= bid amount (5, 15, 25, 35, or 45 Pula)	24.51	14.10
hyp	= 1 if experimental session is hypothetical, = 0 otherwise	0.48	0.50
info	= 1 if additional information about the example was given to participants prior to the actual experiment, = 0 otherwise	0.56	0.50
male	= 1 if male, = 0 otherwise	0.46	0.50
age	= years	22.45	3.69
nation	= 1 if Botswana, = 0 otherwise	0.92	0.27
class	= 1 if in junior year or below, = 0 otherwise	0.83	0.38
gpa	= self-reported cumulative grade point average (5.0 highest)	3.36	0.59
field	= 1 if accounting major, = 0 otherwise (which includes not having declared a major yet and double majors)	0.59	0.49
rich	= 1 if self-reported income is greater than 3,000 Pula per month, = 0 otherwise	0.14	0.35
middle	= 1 if self-reported income is between 1,500 and 3,000 Pula per month, = 0 otherwise	0.54	0.50
risk	= 1 if risk averse, = 0 otherwise	0.42	0.50
child	= 1 if a mother or father, = 0 if not	0.11	0.31
happy	= 1 if "happy" or "very happy" with life, = 0 otherwise (including "unsure")	0.80	0.40
smprob	= 1 if mistake on net payout worksheet did not preclude correct calculation of net payout, = 0 otherwise	0.09	0.29
bgprob	= 1 if mistake on net payout worksheet resulted in incorrect calculation of net payout, = 0 otherwise	0.21	0.41
chgwtpup	= 1 if participant marked "no" to investment question in first round and "yes" to investment question in second round, = 0 otherwise	0.08	0.27
chgwtpdn	= 1 if participant marked "yes" to investment question in first round and "no" to investment question in second round, = 0 otherwise	0.16	0.37
WTP_0	= participant's ideal (open-ended) bid amount (in Pula)	17.17	13.86
sense	= 1 if WTP_0 was not larger than a bid amount that was rejected in both rounds or the second round only, = 0 otherwise	0.80	0.40

the mean of Yes_1 (*hyp* = 0) at the 5 percent level of significance. However, participants in the hypothetical treatment did not systematically change their responses: the mean of Yes_2 (*hyp* = 1) is not statistically different than the mean of Yes_1 (*hyp* = 1). In other words, it appears that participants in the actual treatment learned that cooperation (without coordination) does not pay, but free-riding does.[18]

[18] Although not presented in Table 2, we also compared the means for *prior* information effects (i.e., the means for the information and control groups discussed in the introductory section of this paper). We found no evidence that the prior information mattered.

Table 2. Proportions of Participants Answering "Yes" to the Random Bid (hypothetical vs. actual and first-round vs. second-round)

Treatment	Variable	Average
hyp = 0, round = 1	*Yes* [a]	0.42 [a]
hyp = 1, round = 1	*Yes* [a]	0.51
hyp = 0, round = 2	*Yes* [b]	0.22 [a,b]
hyp = 1, round = 2	*Yes* [b]	0.55 [b]

[a,b] Proportions demarcated with superscript *a* are statistically different from each other at the 5 percent level of significance; similarly for proportions demarcated with superscript *b*. The "across-treatment" test of differences in proportions was carried out using Fisher's exact test. The "within-treatment" test of differences in proportions was carried out using McNemar's test.

Questions naturally arise as to why only participants in the actual treatment were induced to free-ride, and why it was necessary for them to learn to do so. In answer to the first question, factors such as consequentiality, credibility, and plausibility (i.e., the degree to which individuals believe a choice is binding or that their responses will affect policy in any meaningful way) seem to be the most convincing reasons why only participants in the actual treatment were induced to free-ride (Champ et al. 2002, Cummings and Harrison 1994, and Carson and Groves 2007). In our particular case, perhaps participants in the hypothetical treatment viewed the experiment as being inconsequential enough to not consider the option of investing strategically, while participants in the actual treatment not only interpreted their choices as being consequential, but also believed the payoffs to be plausible, and thus credible.

In answer to the second question, experiential learning seems to be the primary explanation in the experimental literature for delayed free-riding behavior (Shogren 2006, Andreoni 1988, Andreoni and McGuire 1993, Marwell and Ames 1981, and Slonim and Roth 1998). For example, Andreoni (1988) finds that free-riding is seldom observed in one-shot games; however, it is often found in finitely repeated games.[19] In the end, it is likely that an interaction of the two effects—consequentiality/plausibility/credibility and learning—best explains both the extent and timing of our free-riding result (Harrison 2006).

Non-parametric mean estimates of WTP are presented in Table 3. We have calculated these estimates using the method proposed by Kriström (1990), with linear interpolation to recover the empirical survival function, and Ayer et al.'s (1955) "pool-adjacent-violator algorithm" to obtain a monotone non-increasing sequence of proportions. The associated standard errors are calculated according to the method proposed by Boman, Bostedt, and Kriström (1999). The WTP estimates concur with the results presented in Table 2. As with the proportion comparisons presented in Table 2, a comparison of the WTP estimates for round two shows evidence of positive hypothetical bias—the respective point estimates 13.31 Pula and 27.19 Pula are statistically different from one another.

The WTP estimates also suggest that participants in the actual treatment responded between rounds by reducing their WTP: the point estimate of 13.31 Pula is statistically lower than 23.17 Pula at the 95 percent level of confidence. However, participants in the hypothetical treatment did not lower their WTP: the point estimate of 27.15 Pula is not statistically different than 27.19 Pula at standard significance levels. Again, participants in the actual treatment learned that free-riding pays.

Econometric Model

To explain the variation in investment decisions, we estimate a bivariate probit model that accounts for possible error correlation between the individual's first- and second-round decisions (Greene 2008):

[19] Kachelmeier and Shehata (1992) report evidence from their experimental lotteries conducted in China that suggests a role for risk aversion in delaying theoretically predicted responses. Andreoni (1995), Houser and Kurzban (2002), and Palfrey and Prisbrey (1997) suggest that confusion on the part of respondents explains the delayed response, while Taylor et al. (2001) and Vossler and McKee (2006) find evidence against the confusion hypothesis.

Table 3. Non-Parametric Estimates of Willingness to Pay (hypothetical vs. actual and first-round vs. second-round)

Treatment	Variable	Mean Estimate (Pula)	Std. Error (Pula)
hyp = 0, Round = 1	*WTP*[a]	23.17[a]	3.27
hyp = 1, Round = 1	*WTP*[a]	27.15	3.01
hyp = 0, Round = 2	*WTP*[b]	13.31[a,b]	2.93
hyp = 1, Round = 2	*WTP*[b]	27.19[b]	3.02

[a,b] Means demarcated with superscript *a* are statistically different from each other at the 5 percent level of significance; similarly for means demarcated with superscript *b*. The statistical tests are standard *t*-tests for differences in means from sub-samples with equal sample sizes (for the within-treatment, across-round comparisons) and unequal sample sizes and unequal variances (for the across-treatment, within-round comparisons). See Hogg and Craig (1978) for the methods used to conduct these tests. The WTP estimates are calculated as in Kriström (1990) and the associated standard errors are calculated according to Boman, Bostedt, and Kriström (1999).

$$(1) \qquad Y^*_{i,j} = X_{i,j}\beta_j + \in_{i,j},$$

where i indexes participants; j = 1,2 denotes the round of the experiment; $X_{i,j}$ is a vector of explanatory variables from Table 1 including the *hyp* treatment effect and bid τ_i; β_j is a vector of the associated coefficients; and the errors $\in_{i,j}$ are assumed to have a bivariate standard normal distribution with correlation parameter ρ. If the latent dependent variable $Y^*_{i,j}$ is greater than zero, then the participant invests τ_i ($Yes_{i,j}$ = 1). If the latent dependent variable $Y^*_{i,j}$ is less than or equal to zero, then the participant does not make the investment ($Yes_{i,j}$ = 0). Each participant can therefore be placed in one of four investment categories: ($Yes_{i,1}$ = 0, $Yes_{i,2}$ = 0), ($Yes_{i,1}$ = 1, $Yes_{i,2}$ = 0), ($Yes_{i,1}$ = 0, $Yes_{i,2}$ = 1), or ($Yes_{i,1}$ = 1, $Yes_{i,2}$ = 1). The probabilities of being in the four investment categories are then used to form and maximize the joint log likelihood function.

Results

Table 4 reports the coefficient estimates and bootstrapped standard errors from the maximum likelihood estimation.[20] We find evidence that the round-one and round-two error terms are positively correlated (i.e., ρ is positive and statistically significant at the 1 percent level), suggesting that the bivariate model is preferred over a univariate model. For round one, only the coefficient estimate for bid τ is statistically significant (at the 10 percent level), implying *inter alia* the absence of hypothetical bias and no effect of prior information (*info*) in round one of the experiment—a result that concurs with the unconditional comparisons and WTP estimates shown in Tables 2 and 3, and discussed in the previous section.[21]

However, the story is different for round two. The coefficients for *info* and *hyp* are both statistically significant (at the 10 percent and 1 percent levels, respectively). Individuals in the hypothetical treatment are more likely to accept the bid than those facing an actual decision of whether to contribute to the public good. In other words, we find evidence of positive hypothetical bias in round two of the experiment—again a result that is consistent with the unconditional comparisons and WTP estimates shown in Tables 2 and 3. We also find that, on average, additional information provided prior to round one of the experiment helps reduce the individual's probability of accepting the bid, but the effect is weaker than for hypothetical bias both in terms of its magnitude and statistical significance.

To further investigate the effect of prior information on the probability of accepting the bid, we

[20] GAUSS version 8.0 is used to estimate the model. We estimate the model with both the full and a reduced set of variables included in Table 1. Since several of the variables were insignificant in those regressions, we dropped the demographic variables from the models presented in Table 4. We also used ordinary least squares to estimate a model where WTP_0 was the regressand, but found that few of the variables could explain variation in the open-ended measure of WTP.

[21] We also estimated a regression of WTP_0 on τ to check for anchoring bias. The coefficient on τ was positive and statistically significant at the 10 percent level, suggesting the existence of anchoring bias in our sample. However, without any variation in τ across rounds of the experiment, we are unable to identify this effect in the dichotomous-choice framework.

Table 4. Bivariate Probit Estimates for Rounds One and Two

Variable	Coefficient (Round 1)	Coefficient (Round 2)
Constant	0.0886 (0.3514)	-0.3576 (0.3639)
info	-0.0025 (0.2560)	-0.3622* (0.2609)
hyp	0.2401 (0.2426)	0.8458*** (0.2667)
τ	-0.0125* (0.0096)	-0.0069 (0.0099)
ρ	0.7741*** (0.0979)	
Log L	-115.0383	
Sample Size	102	

TESTS FOR PRIOR INFORMATION EFFECTS ACROSS TREATMENTS

Treatment	Null Hypothesis	Wald Statistic[a]	P-Value
hyp = 1, round = 1	$\beta_{inf1} = \beta_{inf0}$	0.98	0.32
hyp = 1, round = 2	$\beta_{inf1} = \beta_{inf0}$	0.04	0.85
hyp = 0, round = 1	$\beta_{inf1} = \beta_{inf0}$	0.98	0.32
hyp = 0, round = 2	$\beta_{inf1} = \beta_{inf0}$	2.73*	0.10

[a] Calculated for one linear restriction per hypothesis with sample size 102.

Note: *, **, and *** indicate significance at the 10 percent, 5 percent, and 1 percent level, respectively. Bootstrapped standard errors are in parentheses.

created four separate interactive dummy variables (*info* × *hyp*) for each round. We then re-estimated the model with only these new variables and the bid. Wald tests were performed to test for prior information effects across treatments. The bottom half of Table 4 presents our results. As indicated, prior information had only a slight statistical effect (10 percent level of significance) in the second round on participants in the actual treatment. Accordingly, participants in the actual treatment who had been provided with prior information were less likely to accept their bids than those who had not.[22]

In addition to the bivariate model, we also estimated two separate univariate probit models to check for between-round learning effects for the hypothetical and actual treatments. The results are presented in Table 5. In the first model, we investigate the behavior of individuals who answered "yes" to the initial investment decision (N= 47). The dependent variable measures whether these individuals switched from investing a positive amount in round one of the experiment to investing nothing in round two (i.e., *chgwtpdn* = 1). The coefficient estimate for *hyp* is negative and significant at the 1 percent level of significance. This suggests that individuals making actual investment decisions were more likely to switch from having said "yes" in round one to saying "no" in round two. Similar to our unconditional results in our "Data and Unconditional Tests" section and the results for prior information discussed above, we find evidence that individuals in the actual treatment learned that cooperation does not pay but that free-riding does.

[22] The regression results for this test are available upon request from the authors.

Table 5. Univariate Probit Estimation Sorted by Initial Investment Decision

	MODEL 1: "Yes" to Initial Investment Dependent Variable = *chgwtpdn*	MODEL 2: "No" to Initial Investment Dependent Variable = *chgwtpup*
Explanatory Variable	Coefficient	Coefficient
Constant	-0.2151 (0.6246)	-1.1988 (1.8658)
info	0.7513* (0.4604)	-0.3873 (1.2028)
hyp	-1.5301** (0.7018)	0.4033 (1.7395)
τ	0.0020 (0.0179)	0.0048 (0.0194)
Log L	-22.5976	-21.6425
Sample Size	47	55

*, **, and *** indicate significance at the 10%, 5% and 1% level, respectively. Bootstrapped standard errors are in parentheses. The sample sizes depend on how a respondent answered the investment question in the first round of the experiment. As such, 47 of the 102 participants responded "yes" to their bid value in round 1, and 55 participants responded "no."

In the second model, we investigate the behavior of individuals who answered "no" to the initial investment decision (N = 52). Here the dependent variable measures whether individuals switched from investing nothing in round one of the experiment to investing a positive amount in round two (i.e., *chgwtpup* = 1). The coefficient on *hyp* is positive but not statistically different than zero. This indicates that individuals who invested hypothetically were no more likely to increase their investment between rounds than those who made actual investment decisions. Therefore, we find no evidence that individuals in the hypothetical treatment learned to cooperate any more than those in the actual treatment.

Summary and Conclusions

This paper reports evidence of positive hypothetical bias in a CV-based public good experiment conducted with university students in the African nation of Botswana. To our knowledge, this is the first such evidence of positive hypothetical bias for an African country—the only previous public good experiment, conducted with students in the country of Niger, reports evidence of *negative* hypothetical bias.

The fact that positive hypothetical bias is found through our regression analysis only in the second round of the experiment—after participants have used a worksheet to calculate their respective net payouts from round one—suggests that additional information provided *during* (i.e., between rounds of) the experiment may be an effective method to induce participants making actual investment decisions to reduce their WTP for the public good. However, additional between-round information does not eliminate positive hypothetical bias in the sense that it does not induce participants who are investing hypothetically to similarly reduce their WTP.

The finding that additional information provided during the experiment induces only those participants who are making an actual investment to reduce their WTP for the public good suggests that mitigating hypothetical bias in CV-based research may require additional mitigation measures, such as *ex ante* reminder statements (see Cummings and Taylor 1999, List 2001, Aadland and Caplan 2006) and *ex post* calibration of WTP (List and Shogren 1998, Harrison et al. 1999). With respect to real-payment situations, such as those encountered by charitable organizations, our results suggest that the prior experiences of potential donors are likely to matter. All else equal, those having been solicited more often in the past may be more likely to free-ride on the expected contributions of others. Thus, provision-

point mechanisms, where minimum aggregate contribution thresholds are pre-established, are likely to be necessary in obtaining incentive-compatible pledges of support.

Our findings should be judged with two caveats in mind. First, the sample for the experiment is confined to university business students. Therefore, while it may be representative of that particular subgroup of students, our sample may not be representative of the university student body at large; it certainly is not representative of the Botswana population in general (see footnote 16). Second, Botswana is generally considered to be an economically emergent country, in the sense that its economic growth since independence in 1966 has been both steady and high relative to the vast majority of the world's other developing countries (World Bank Group 2000). Thus, generalizing this paper's results to the rest of Africa, let alone the lesser-developed world at large, is of questionable value.

As a result of these caveats, the role for future research is clear. More public good experiments need to be conducted in Africa and other lesser-developed areas of the world, preferably with larger and more representative samples. Ideally, a variety of public good mechanisms, such as provision and non-provision points, will be tested in the laboratory. As in the more-developed world, results from a broad base of experimental research will then help guide the design of survey instruments for field research throughout the lesser-developed world. Indeed, the current pace at which markets and non-markets (e.g., global externalities) are becoming linked internationally compels us to understand how welfare is determined within a more interconnected world.

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Appendix: Experimental Design

Instructions

You have been given 50 Pula to participate in this experiment. The money is yours to keep. *You will not be paid anything more or less*.

Before the actual experiment begins, a simple example is presented. The purpose of the example is to demonstrate how an individual's "net payout" from the experiment is determined. Net payout is an amount of money that an individual receives based on (i) how much of his own money he chooses to invest, and (ii) how much money everyone else in the room chooses to invest. The actual experiment that you and the other students in this room are going to participate in will begin after you have gone through this example.

EXAMPLE

Suppose there are only five individuals in a room, each of whom has been given 20 Pula. After studying the Payout Chart below, the individuals make the following decisions:

- Individual 1 chooses to invest nothing.
- Individual 2 chooses to invest 5 Pula.
- Individual 3 chooses to invest 10 Pula.
- Individuals 4 and 5 each choose to invest 15 Pula.

These choices result in a total of 45 Pula invested from the five individuals, for an average investment of 45 Pula ÷ 5 individuals = 9 Pula. Based on the Payout Chart below, we can now calculate each individual's net payout.

Payout Chart – This is only an example

	Payout Ranges	
	"*Yes*, I'll invest"	"*No*, I won't invest"
Average Group Investment	Payout (Pula)	Payout (Pula)
Greater than 0 Pula; less than or equal to 10 Pula	5	10
Greater than 10 Pula; less than or equal to 20 Pula	20	25

Begin by noting that for this example the average group investment of 9 Pula is between 0 Pula and 10 Pula in the Payout Chart, so we can focus on the first row of numbers. What this row of numbers tells us is that the payout is 5 Pula for a person who chose to invest something and 10 Pula for a person who chose to invest nothing. Now, let's see how much Pula each of the five

people participating in this example take home with them from the experiment.

Individual 1 chose to invest nothing. He therefore receives a net payout of 10 Pula (10 Pula payout from the Payout Chart above less 0 Pula invested) and he leaves the room with a total of 30 Pula (the 20 Pula he started the experiment with plus his 10 Pula net payout). Individual 2 chose to invest 5 Pula. She therefore receives a net payout of 0 Pula (5 Pula payout from the Payout Chart above less 5 Pula invested) and she leaves the room with a total of 20 Pula. Individual 3 chose to invest 10 Pula. He therefore receives a net payout of -5 Pula (5 Pula payout from the Payout Chart above less 10 Pula invested) and he leaves the room with a total of 15 Pula. Individuals 4 and 5 each chose to invest 15 Pula. They therefore each receive a net payout of -10 Pula (5 Pula payout from the Payout Chart above less 15 Pula invested) and each leaves the room with 10 Pula.

Are there any questions before we begin?

Experiment

Directions. Use the payout chart below to decide whether to *hypothetically* invest some or none of your 50 Pula. If this experiment were for real, your actual net payout would be determined by your own investment choice and the average investment of the group, as was demonstrated in the example. Note that if the total group investment is zero (and thus the average group investment is also zero), the net payout is zero to everyone.

Payout Chart

	Payout Ranges	
	"*Yes*, I'll invest"	"*No*, I won't invest"
Average Group Investment	Payout (Pula)	Payout (Pula)
Greater than 0 Pula; less than or equal to 10 Pula	5	10
Greater than 10 Pula; less than or equal to 20 Pula	20	25
Greater than 20 Pula; less than or equal to 30 Pula	35	40
Greater than 30 Pula; less than or equal to 40 Pula	50	55
Greater than 40 Pula; less than or equal to 45 Pula	65	70
Greater than 45 Pula; less than or equal to 50 Pula	80	85

INVESTMENT QUESTION

This question requires a choice for which your net payout from the experiment would be *hypothetically* determined.

Are you willing to make an investment of *XX* Pula?

Yes ❒

No ❒

NET PAYOUT WORKSHEET

1. Amount of Pula I was asked to invest: _____

   This is the Pula amount that was included in the Investment Question during the experiment.

2. Amount of Pula that I agreed to invest: _____

   If you decided to check the "Yes" box for the Investment Question during the experiment, then re-enter the number that you have written on line 1 above onto line 2. If you checked the "No" box for the Investment Question, then enter 0 on line 2.

3. My payout from the experiment: _____

   This is the number that has been worked out on the board in front of the class and that corresponds to the amount of Pula that you agreed to invest.

4. My net payout from the experiment: _____

   Subtract the amount you have written on line 2 from the amount on line 3. Note that this could be a negative number.

5. The amount of money I leave the experiment with: _____

   Add 50 Pula to the amount on line 4.

Demographic Questions

Please answer the following questions to the best of your ability. These questions are very important to us. Remember that all information is completely anonymous and confidential.

1. Gender:

   ❒ Male ❒ Female

2. Age: _____.

3. Nationality/ethnicity: _____.

4. Class Standing:
   - ❒ First year
   - ❒ Second year
   - ❒ Third year
   - ❒ Fourth year
   - ❒ Graduate

5. Cumulative grade point average: _____

6. Have you declared a major field of study?

   ❒ Yes ❒ No

   If yes, what is your major field of study? ___

7. In which range do you think your monthly consumption expenditure currently falls [consumption expenditure includes money that you spend (and that other people spend to support you) for things like food, clothing, housing, entertainment, cell phone, utility bills, savings at the bank, etc.—it does not include money that you give or lend to other people]?
   - ❒ Less than 1,500 Pula per month.
   - ❒ Greater than 1,500 Pula but less than 3,000 Pula per month.
   - ❒ Greater than 3,000 Pula but less than 4,500 Pula per month.
   - ❒ Greater than 4,500 Pula but less than 6,000 Pula per month.
   - ❒ Greater than 6,000 Pula per month.

8. Which would you choose?
   - ❒ 50 Pula with certainty.
   - ❒ 50% chance of 0 Pula; 50% chance of 100 Pula.
   - ❒ I'm indifferent between the two choices above.

9. Do you have a son or a daughter?

   ❒ Yes ❒ No

10. Please check the box that best describes your current level of happiness in life.
    - ❒ I am very unhappy with my life.
    - ❒ I am unhappy with my life.
    - ❒ I am happy with my life.
    - ❒ I am very happy with my life.
    - ❒ I am uncertain about my happiness in life.

11. If you could have chosen an amount yourself to invest in the experiment that you have just participated in, what would that amount have been (taken from your 50 Pula)? _____

Thank you for participating in this experiment!