Deception and Misreporting in a Social Program

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Abstract

We investigate empirically the extent of misreporting in a poverty-alleviation program in which self-reported information, followed by a household visit, is used to determine eligibility. Underreporting may be due to a deception motive, and overreporting to an embarrassment motive. We find that underreporting of goods and desirable home characteristics is widespread, and that overreporting is common with respect to goods linked to social status. Larger program benefits encourage underreporting and discourage overreporting. The effect of benefits on underreporting is significant under a variety of specifications. We also investigate the effects of education and gender on misreporting.

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1 Introduction

Targeted poverty-alleviation programs rely on the ability to identify the poor population from the non-poor. The costs of identifying the poor are presumably larger in developing countries, where variables such as income cannot be independently verified (for example through tax institutions). As a consequence, targeted programs in developing countries usually rely on information provided by the applicants. It is widely believed that the incentives for underreporting of economic conditions in this context are quite large. Nevertheless, to our knowledge, no previous research has analyzed the extent to which individuals misreport their characteristics when applying for social programs. This lack of research is because the data requirements are severe, e.g. information is needed on what an individual reports to a government agency when applying for a program, as well as independent information on the "true" characteristics of the individual. Such information has been rarely, if at all, available. In this paper, we employ a very unusual database from *Oportunidades*, the Mexican government main poverty-alleviation scheme, to explore quantitatively the extent and causes of misreporting in social programs.

Economists usually assume that individuals tell the truth only if this is incentive-compatible given the material outcomes. According to conventional assumptions, applicants to a social program will understate their material possessions, so long as this increases the probability of benefitting from the program. A corollary of this view is that underreporting should be made costly by the program requirements.[1] If underreporting is not costly, we can expect potential beneficiaries to display a strategic bias toward *deception*. Ethical considerations may act as a counterweight,[2] however, so that the willingness of individuals to deceive for advantage is worth exploring quantitatively.

Research in participation in welfare programs reminds us that very often individuals do not apply to a program even if participation seems convenient given the material outcomes. This has been interpreted by Moffitt (1983) and others as the result of a social "stigma" or utility loss associated with par-

[1] See e.g. Besley and Coate (1992, 1995).

[2] See Bok (1999) for a thoughtful review of the positions of moral philosophers about deception, and the recent article by Gneezy (2005) for some experimental evidence.

ticipation.[3] By analogy, we may expect that even some individuals who are willing to apply may have a bias toward overreporting due to *embarrassment* or stigma associated with the lack of certain material goods.

We use a database from the Mexican program *Oportunidades* to estimate a model of reporting in the context of social programs that encompasses both deception and embarrassment considerations. When this program was introduced in urban localities in 2002, an advertising campaign was carried out to inform potential applicants that registration centers for the program would open during certain dates. Applicants who turned up at the registration centers were asked to provide information on their address and on their household characteristics. Eligibility into the program was determined using the household characteristics to compute a household poverty index.[4] Applicants initially found to be eligible received a household visit during the coming weeks to verify the information given, after which a final classification on eligibility was made.

Our database is rather unique in that it includes not only what individuals reported at the registration center but also what they were actually found to have during the household visit for over a million households. Thus, we can check whether applicants reported correctly, understated or overstated their possessions in answering the questionnaire. Since we can calculate the benefits for each applicant according to program rules, we have information about what was at stake for each applicant.

We find that underreporting is widespread in every item we investigate. Overreporting is common in goods that may have a "status" value. Both underreporting and overreporting are clearly sensitive to material incentives. Larger program benefits encourage underreporting and discourage overreporting. Quantitatively speaking, though, the impact of program benefits on misreporting is not overwhelming at the margin. As an illustration, increasing in 50% the monetary benefits from the program (which were close to 26%

[3] Riphahn (2001) provides some recent evidence on widespread lack of participation in social programs by potential beneficiaries. See also Currie (2004) for a review of the literature on take up.

[4] The weights attached to each answer in the household poverty index were previously determined using a poverty regression similar to the methodology described by Ravallion (1996). The methodology was public (Reglas de Operación 2002) but no the specific weights.

of the households' preprogram expenditure) would increase underreporting in a few percentage points, and would have an even smaller impact in deterring overreporting. There is also evidence that there are forces at work other than material incentives in the decision to misreport or report accurately. For instance, education discourages underreporting and, very clearly, encourages overreporting. This is interesting because education may be linked to socialization and therefore to sensitivity to status considerations. Finally, male applicants underreport more and overreport less than female applicants with respect to "status" goods.

The results suggest that the use of self-reported household characteristics in targeting poverty-alleviation programs can be improved upon by taking into account both under and overreporting in the allocation of weights to household characteristics. For instance, the eligibility index employed by *Oportunidades* officials gave some weight to gas boilers, cars, trucks, and washing machines, all items for which underreporting was rampant. The index also gave weight to toilets, tap water, and concrete floor, items for which overreporting was common. While underreporting may have been corrected at the household visit stage, overreporting is worrisome because it may have led to the exclusion of the program of households that could qualify in principle. It is also a problem that is harder to deal with: If a poverty index accurately identifies goods whose absence is a clear indicator of poverty, it is also probably identifying goods associated with a higher social status.

At a broader level, there has recently been a surge of interest by economists on the issue of deception in a variety of contexts.[5] The experimental work of Gneezy (2005) shows that in two-party interactions people care both about their own gains and about the losses to the other party in deciding whether to deceive. Other experimental studies reviewed by Croson (2005) reveal that deception is widespread and that generally individuals are not sufficiently skeptical of statements they receive from others. Our setup is different in that individuals' deceptive behavior does not create losses for another easily identified individual. Our results on the impact of monetary payoffs on the willingness to deceive may be of particular interest given the high stakes involved for applicants in our database.

[5]Including among others Crawford (2003), Chen (2005), Ettinger and Jehiel (2006), and Kartik, Ottaviani and Squintani (2006).

TABLE I
MISREPORTING IN OPORTUNIDADES[a]

Good	Underreporting (as % of Households Having Good)	Overreporting (as % of Households Not Having Good)	Households Having Good (as % of Total)
Concrete Floor	14.45	25.41	64.25
Tap Water	13.79	31.76	70.27
Toilet	16.58	39.07	73.08
Gas Boiler	73.12	1.07	3.22
Washing Machine	53.46	6.20	8.49
Phone	73.12	1.34	2.64
Car	83.10	0.80	1.54
Truck	81.71	0.61	1.53
Satellite TV	73.91	1.74	2.32
Water Tank	58.44	3.80	5.18
Refrigerator	36.93	12.05	27.07
Gas Stove	24.25	28.56	48.40
Video Recorder	79.73	1.98	3.25

[a]Observations: 74034. Source: *ENCASURB*.

Overreporting may be due at least in part to inaccurate beliefs similar to the overconfidence or "self-serving bias" described by Babcock and Loewenstein (1997) and Camerer (1997) in other contexts. Overreporters may feel better about themselves by believing that their house floor is accurately described as mostly covered by concrete rather than dirt, even if an "impartial" observer, such as the visitor sent by the program, may disagree.

2 Evidence on Misreporting

Table I provides evidence on misreporting in *Oportunidades* from a random sample of 101,803 applicants (10% of the applicants interviewed at the registration centers in 2002). Out of this sample, 74,034 applicants initially qualified and received a household verification visit. We have calculated under and overreporting as follows. For each of the goods or desirable household characteristics g, let A_g be the set of applicants who asserted having the good

at the registration center interview, and let D_g be the set of applicants who denied having the good at the interview. Similarly, let H_g be the set of applicants who were found to have the good at the household visit, and let N_g be the set of applicants who were found not to have the good at the household visit. We define

$$\begin{aligned} \text{underreporting of good } g &= \frac{|D_g \cap H_g|}{|H_g|} \times 100, \text{ and} \\ \text{overreporting of good } g &= \frac{|A_g \cap N_g|}{|N_g|} \times 100. \end{aligned}$$

Underreporting is substantial for every item described in Table I. Since there are a few goods, such as cars, trucks, phones and video recorders, that may have been hidden during the household visit, we may be understating the already high incidence of underreporting for these goods. Other items seem much harder to hide.

Overreporting is substantial only for a few items in Table I, including toilets, tap water, gas stoves, and concrete floor. This list suggests that applicants may have been embarrassed to report the lack of these items, or perhaps unwilling to acknowledge their true household characteristics. Toilets, tap water, and concrete floor are widespread among the urban poor in our sample, and lacking some of these may carry a stigma.[6]

The incentive to lie to participate in the program has been substantial; the average monthly cash benefit of participation for urban households is about 26.4% of the average applicant's household pre-program expenditure in our sample. Cash benefits for participants in *Oportunidades* include a purely unconditional grant (termed "nutrition grant"), plus some grants conditional on the school attendance of the children in the household, as described in Table II. (The program also includes free medical consultations and nutrition supplements.) Since we can calculate the potential cash benefits a household can receive under the program, we have an idea about the incentive to underreport (or the disincentive to overreport) for each applicant. Table III provides some additional information on the applicants and their households. Most

[6]Since overreporting applicants were less likely to qualify for a house visit, we may be understating the incidence of overreporting, and the opposite effect for underreporting. Note, however, that most applicants qualified for a house visit.

TABLE II
MONTHLY CASH BENEFITS OF OPORTUNIDADES[a]

Grants	Nutrition Grant		150	
	Education Grants:	Grade	Boys	Girls
	Primary	3	100	100
		4	115	115
		5	150	150
		6	200	200
	Middle School	7	290	310
		8	310	340
		9	325	375
	High School	10	490	565
		11	525	600
		12	555	635
Maximum Transfer to Household	With High-School Children		1550	
	Other Households		915	
Average Transfer[b]			350	

[a]In Mexican pesos (2002); 11 pesos is approx. US$1. [b]Urban households (2003).

TABLE III
APPLICANTS AND THEIR HOUSEHOLDS[a]

	Verified Group		Not Verified Group	
Household Characteristics	Mean	Std. Dev.	Mean	Std. Dev.
Total Monthly Expenditure (pesos)	1312	766	1405	735
Per Capita Expenditure (pesos)	319	193	382	219
Family Size	4.54	1.93	4.05	1.73
Children from 0 to 5	0.77	0.87	0.49	0.71
Children from 6 to 21	1.90	1.54	1.63	1.37
Applicant Characteristics	Mean	Std. Dev.	Mean	Std. Dev.
Gender (Female=1)	0.94	0.23	0.94	0.24
Age	38.21	14.46	40.47	14.43
Education	4.44	3.39	5.05	3.49
Working	0.27	0.44	0.27	0.44
Married	0.47	0.50	0.50	0.50
Cohabitating	0.22	0.42	0.16	0.36
Look very poor to interviewer	0.37	0.48	0.24	0.43

[a]Observations: 74034 verified and 10424 not verified applicants. Source: *ENCASURB*.

applicants are women, which is consistent with the fact that the transfers under the program are paid to the mother of the household.

Our data source is the *ENCASURB* (Encuesta Socio-Económica de los Hogares Urbanos), the survey used to establish eligibility for *Oportunidades* for urban households. The *ENCASURB* consists of three related questionnaires. The first is the "inclusion questionnaire" which was applied to applicants at the registration center. Applicants were informed whether they qualify or not on the spot. Applicants who qualified for a household visit, and a large fraction of those who did not, were asked to answer a second questionnaire about themselves and their households. Finally, applicants who were initially declared eligible were informed that they would receive a household visit in the next two or three weeks. A "verification questionnaire" (similar to the inclusion questionnaire) was applied during the household visit. Applicants were required to let interviewers into their households to visually inspect their belongings. Table I employs the answers to the first and third questionnaire, and Table III the answers to the second.

In the next section, we model the decision about reporting of an applicant who understands that these reports are used to determine whether her household qualifies or not for a social program.

3 Misreporting and Incentives

Consider an applicant to a social program who is asked to answer a questionnaire about whether the applicant's household has or not some goods or desirable characteristics (e.g. phone, tap water, concrete floor, etc.). Denoting applicants by $a = 1, \ldots, n$ and goods by $g = 1, \ldots, m$, the report of applicant a is a vector $(r_{ag})_{g=1}^{m} \in \{0,1\}^m$, where $r_{ag} = 1$ means "yes" and $r_{ag} = 0$ means "no." In answering the questionnaire, applicant a is aware of the true answers to the questions, which are represented by $(t_{ag})_{g=1}^{m} \in \{0,1\}^m$. We say that applicant a overreports, underreports or reports truthfully with regard to good g if $r_{ag} > t_{ag}$, $r_{ag} < t_{ag}$, or $r_{ag} = t_{ag}$, respectively.

Eligibility into the program is determined in the following manner. There is a vector of weights assigned to each good $(\omega_g)_{g=1}^{m} \in \Re_+^m$, and a cutoff $\rho \geq 0$ (the "poverty line"). With probability $1 - \delta$, the applicant is eligible for the program if $\sum_g \omega_g r_{ag} \leq \rho$. With probability δ, the applicant is eligible if

$\sum_g \omega_g r_{ag} + \sum_g \tau_g (t_{ag} - r_{ag})^+ \leq \rho$. Here, δ represents the probability that there is some effective monitoring of the applicant's household goods, and τ_g represents the penalty the applicant expects to receive for underreporting with respect to good g if monitoring turns out to be effective.[7]

We assume that applicants hold point beliefs about the vector τ_g and believe that ρ is uniformly distributed in some interval $(0, \overline{\rho})$ where $\overline{\rho} \geq \sum_g \max\{\omega_g, \tau_g\}$. Thus, the probability that a is eligible is

$$\begin{aligned} P(r_{ag}, t_{ag}) \equiv (1-\delta)\left(1 - \textstyle\sum_g w_g r_{ag}\right) + \delta\left(1 - \textstyle\sum_g w_g r_{ag} - \sum_g p_g (t_{ag} - r_{ag})^+\right) \\ = 1 - \textstyle\sum_g w_g r_{ag} - \delta \sum_g p_g (t_{ag} - r_{ag})^+, \end{aligned}$$

where $w_g \equiv \omega_g / \overline{\rho}$ and $p_g \equiv \tau_g / \overline{\rho}$.

Let $U(\cdot)$ be the indirect utility function of applicant a, let Y_a be her pre-program income and let B_a be the monetary benefits of participating in the program. The utility gain of being eligible for the program is then

$$\Delta U(Y_a, B_a) \equiv U(Y_a + B_a) - U(Y_a).$$

Finally, let c_{ag} be the utility cost of "concealing" good g or at least trying to do so, which is suffered by applicant a whenever she underreports, and let e_{ag} be the utility cost of "embarrassment" with respect to good g, which is suffered by applicant a whenever reporting not having the good.

The problem of applicant a is then

$$\max_{(r_{ag})_{g=1}^m} \left(P(r_{ag}, t_{ag}) \times \Delta U(Y_a, B_a) - \textstyle\sum_g c_{ag} (t_{ag} - r_{ag})^+ - \sum_g e_{ag} (1 - r_{ag}) \right).$$

From the solution to this problem, the applicant will underreport about good g if $t_{ag} = 1$ and

$$(w_g - \gamma p_g) \Delta U(Y_a, B_a) - (c_{ag} + e_{ag}) \geq 0,$$

and will overreport about good g if $t_{ag} = 0$ and

$$-w_g \Delta U(Y_a, B_a) + e_{ag} \geq 0.$$

[7] Program operating rules state that "Program benefits will be suspended permanently when the family has given false information with respect to their socio-economic conditions" (Reglas de Operación 2002). Interviewers were not instructed to inform applicants about this program rule, though.

We assume that applicants have a constant relative risk aversion utility function with risk parameter σ; that is[8]

$$\Delta U(Y_a, B_a) = \begin{cases} ((Y_a + B_a)^{1-\sigma} - Y_a^{1-\sigma})/(1-\sigma) & \text{if } \sigma \neq 1 \\ \ln(1 + B_a/Y_a) & \text{if } \sigma = 1. \end{cases}$$

We assume further that c_{ga} and e_{ga} depend linearly on a vector of observable applicant characteristics X_a and a random term; that is

$$c_{ag} = -\alpha_g^c - \gamma_g^c X_a + \nu_{ag} \quad \text{and} \quad e_{ag} = \alpha_g^e - \gamma_g^e X_a + \eta_{ag},$$

where ν_{ag} and η_{ag} are random terms. Thus, applicant a will underreport about good g if $t_{ag} = 1$ and

$$\alpha_{1g} + \beta_{1g}\Delta U(Y_a, B_a) + \gamma_{1g} X_a \geq \epsilon_{1g}, \tag{1}$$

and will overreport about good g if $t_{ag} = 0$ and

$$\alpha_{2g} + \beta_{2g}\Delta U(Y_a, B_a) + \gamma_{2g} X_a \geq \epsilon_{2g}, \tag{2}$$

where $\alpha_{1g} = \alpha_g^c - \alpha_g^e$, $\beta_{1g} = w_g - \delta p_g$, $\gamma_{1g} = \gamma_g^c - \gamma_g^e$, $\epsilon_{1g} = \nu_{ga} + \eta_{ga}$, $\alpha_{2g} = \alpha_g^e$, $\beta_{2g} = -w_g$, $\gamma_{2g} = \gamma_g^e$, and $\epsilon_{2g} = -\eta_{ga}$. Under appropriate assumptions on the random terms, and under the assumption that household visits were in fact effective in monitoring misreporting, equations (1) and (2) can be estimated using logistic regressions.

4 Empirical Analysis

We have estimated equations (1) and (2) for different values of the risk-aversion coefficient using the data from *ENCASURB*. We take Y_a to be the expenditure per capita in the household of the applicant, as reported in the second questionnaire, and B_a to be the cash benefit from the program for the household in per capita terms, calculated using the structure of program benefits and the age and schooling levels of household members. By the time the information on expenditure was collected, applicants knew that they

[8] We are sidestepping for simplicity the issue of the different horizon of benefits for different applicants.

qualified for the program and knew also that reported expenditure was not among the criteria for participation in the program. Nevertheless, we may think that a person that under or overreported with respect to goods may be inclined to do the same with respect to expenditure. We thus include among our estimations the case of risk-neutrality ($\sigma = 0$), in which the utility gain of participating is just the benefits from the program for the household in per capita terms.

The independent variables included in the regressions, other than the utility gain $\Delta U(Y_a, B_a)$, are age of the applicant, years of education, gender, whether the applicant works outside his or her house, whether the applicant speaks an indigenous language, per capita expenditure in the applicant's household, and a set of dummies reflecting subjective judgements of the interviewer: whether the applicant looks "very poor," "somewhat poor," or "not poor," and whether the applicant seems to understand the questionnaire.[9][10] We also include registration center fixed effects, in an attempt to deal with unobserved heterogeneity both in who attends different registration centers and in treatment of applicants across centers.

Table IV provides maximum likelihood logit estimates of β_{1g} and β_{2g} for different goods, using as samples respectively those households who were found to have the good and those households who were not. Estimates in Table IV are provided for $\sigma = 1$, which is an intermediate value of risk-aversion among those we explored (we return below to the issue of the appropriate value of the risk-aversion parameter).

In agreement with the model, $\hat{\beta}_{1g}$ is positive and significant in almost every item for both sets of estimations. The results are thus very supportive of the effect of benefits in encouraging underreporting. Note that the few items in which the model performs badly, such as phones and trucks, are among those in which the assumption of effective monitoring of underreporting is suspect.

Also in agreement with the model, $\hat{\beta}_{2g}$ is negative in almost every item.

[9] Interviewers were also asked if they thought the applicants were lying. Consistent with the evidence that individuals are bad at detecting the deception of others (Croson 2005), interviewers answered that they thought they were told the truth in 98.64% of the cases.

[10] It is unlikely to be a misreporting problem for these independent variables, except for expenditure and perhaps education.

TABLE IV

ESTIMATED COEFFICIENT OF PROGRAM BENEFITS ON MISREPORTING[a]

	Underreporting Equation		Overreporting Equation	
	Without	Reg. Center	Without	Reg. Center
Good	Fixed Effects	Fixed Effects	Fixed Effects	Fixed Effects
Concrete Floor	0.2276	0.2210	-0.0654	-0.1435
	(0.0549)	(0.0576)	(0.0590)	(0.0621)
Tap Water	0.2073	0.1447	-0.0322	-0.0876
	(0.0536)	(0.0566)	(0.0591)	(0.0668)
Toilet	0.1602	0.1528	-0.1287	-0.0959
	(0.0486)	(0.0526)	(0.0614)	(0.0663)
Gas Boiler	0.8035	0.5357	-0.3022	-0.2430
	(0.2534)	(0.2786)	(0.1744)	(0.1787)
Washing Machine	0.3853	0.1309	-0.3672	-0.0743
	(0.1278)	(0.1383)	(0.0775)	(0.0790)
Phone	-0.1175	-0.5016	-0.3050	-0.1027
	(0.2547)	(0.2947)	(0.1554)	(0.1587)
Car	1.5715	0.4476	-0.3194	-0.1887
	(0.4904)	(0.6140)	(0.1988)	(0.2025)
Truck	-0.8698	-1.2556	0.0437	0.1574
	(0.3640)	(0.4428)	(0.1954)	(0.1951)
Satellite TV	0.8467	0.0705	0.1800	0.0765
	(0.2906)	(0.4520)	(0.1156)	(0.1231)
Water Tank	0.5520	0.4628	-0.2012	-0.0931
	(0.1663)	(0.1835)	(0.0917)	(0.0952)
Refrigerator	0.5263	0.4305	-0.2225	-0.1055
	(0.0721)	(0.0760)	(0.0591)	(0.0628)
Gas Stove	0.5461	0.4049	-0.3641	-0.3478
	(0.0561)	(0.0603)	(0.0507)	(0.0545)
Video Recorder	0.7300	0.3341	-0.3024	-0.0470
	(0.2791)	(0.3247)	(0.1287)	(0.1306)

[a]With moderate risk-aversion ($\sigma = 1$). Standard errors in parenthesis; observations: 74034.

$\hat{\beta}_{2g}$ is significant at 10% in most items in which it is negative, although significance levels are less impressive in equation (2) than in equation (1). When fixed effects are considered, significance is reduced in most items, except notably in concrete floor. Overall, the results are somewhat supportive of the effect of benefits in discouraging overreporting.

According to the model, for each item g, $\hat{\beta}_{1g}$ should be indicative of what applicants who have this good believe is the weight given to it in the eligibility criterion, adjusted for the penalty expected for lying. In turn, $-\hat{\beta}_{2g}$ should be indicative of what applicants who lack this good believe is the weight given to it in the eligibility criterion. With common beliefs about the weight of good g, we would expect $\hat{\beta}_{1g}$ to be smaller or equal to the absolute value of $\hat{\beta}_{2g}$. The estimates in Table IV do not satisfy this inequality. Perhaps this is not so surprising. There is little reason to expect there to be common beliefs about the weight of each good since the applicants presumably had no access to the "poverty regression" used by the government to determine those weights. With some heterogeneity of beliefs, we may expect the applicant in the margin between underreporting or reporting truthfully to have a larger estimate of the weight of the good in question than the applicant in the margin between overreporting or reporting truthfully.

Table V provides estimates of the relative weight of each good according to the estimated coefficients for equations (1) and (2). Relative weights are calculated as

$$\begin{aligned} \text{weight of good } g \text{ for under-reporters} &= \frac{\max\{0, \hat{\beta}_{1g}\}}{\sum_{g'} \max\{0, \hat{\beta}_{1g'}\}}, \text{ and} \\ \text{weight of good } g \text{ for over-reporters} &= \frac{\min\{0, \hat{\beta}_{2g}\}}{\sum_{g'} \min\{0, \hat{\beta}_{2g'}\}}. \end{aligned}$$

Estimated relative weights for under and over-reporters are not far off for about half the items, in particular when compared with the true relative weights obtained from administrative sources.

Next, we have estimated the effect of a marginal increase in per capita benefits of the program on the number of applicants who underreport as a percentage of those having the good, and on the number of applicants who overreport as a percentage of those lacking the good. For each item g and for

TABLE V
WEIGHTS OF GOODS IN PROGRAM PARTICIPATION[a]

Good	Estimated Weights		True Weights
	Under-Reporters	Over-Reporters	
Concrete Floor	0.035	0.025	0.178
Tap Water	0.032	0.012	0.083[b]
Toilet	0.024	0.049	0.156
Gas Boiler	0.123	0.116	0.286
Washing Machine	0.059	0.141	0.048
Phone	0.000	0.117	0.000
Car	0.240	0.122	0.060[c]
Truck	0.000	0.000	
Satellite TV	0.129	0.000	0.000
Water Tank	0.084	0.077	0.000
Refrigerator	0.080	0.085	0.190
Gas Stove	0.083	0.139	0.000
Video Recorder	0.111	0.116	0.000

[a]With moderate risk-aversion ($\sigma = 1$) and without fixed effects. Observations: 74034.
[b]Only for households having a toilet. [c]Having either a car and no truck, a truck and no car, or both a car and a truck have the same weight.

each applicant a who has this good, the marginal increase in the probability of underreporting is equal to

$$\frac{\partial \Pr(r_{ag} = 0 | t_{ag} = 1)}{\partial \Delta U(Y_a, B_a)} \times \frac{\partial \Delta U(Y_a, B_a)}{\partial B_a}.$$

Thus, the marginal increase in the probability of underreporting for applicant a can be estimated as

$$\hat{\beta}_{1g} f(\hat{\alpha}_{1g} + \hat{\beta}_{1g} \Delta U(Y_a, B_a) + \hat{\gamma}_{1g} X_a) \times (Y_a + B_a)^{-\sigma},$$

where $f(z) \equiv \exp(z)/(1 + \exp(z))^2$.

The marginal increase in underreporting of good g is calculated as one hundred times the value of the expression above evaluated at the mean values of the exogenous variables for those having the good. The marginal increase in overreporting is calculated similarly. Table VI displays the results for four values of the risk-aversion coefficient. The significance levels are those of the

TABLE VI
MARGINAL EFFECT OF PER CAPITA BENEFITS ON MISREPORTING[a]

	Risk Neutrality ($\sigma = 0$)		Low Risk Aversion ($\sigma = 1/2$)	
Good	Effect on Underreport	Effect on Overreport	Effect on Underreport	Effect on Overreport
Concrete Floor	0.0089***	-0.0055	0.0193***	-0.0085
Tap Water	0.0025	0.0068	0.0091**	0.0089
Toilet	0.0006	-0.0047	0.0050	-0.0131
Gas Boiler	0.0304**	-0.0008	0.0704***	-0.0018*
Washing Machine	0.0294***	-0.0044***	0.0633***	-0.011***
Phone	0.0025	-0.0010*	-0.0016	-0.0027**
Car	0.0546***	-0.0011**	0.1154***	-0.0023**
Truck	-0.0150	0.0002	-0.0549**	-0.0001
Cable/Satellite TV	0.0115	0.0007	0.0463	0.0020
Water Tank	0.0307***	-0.0012	0.0685***	-0.0034*
Refrigerator	0.0354***	-0.0025	0.0788***	-0.0078*
Gas Stove	0.0253***	-0.0163***	0.0561***	-0.0356***
VCR	0.0134	-0.0014**	0.0416*	-0.0035**

	Mod. Risk Aversion ($\sigma = 1$)		High Risk Aversion ($\sigma = 2$)	
Good	Effect on Underreport	Effect on Overreport	Effect on Underreport	Effect on Overreport
Concrete Floor	0.0076***	-0.0037	-0.0001	0.0000
Tap Water	0.0068***	-0.0020	0.0001	-0.0001
Toilet	0.0062***	-0.0089**	0.0002**	0.0000
Gas Boiler	0.0386***	-0.0008*	0.0233***	0.0000
Washing Machine	0.0255***	-0.0055***	0.0002	0.0001**
Phone	-0.0056	-0.0010**	-0.0023	0.0000
Car	0.0552***	-0.0006	0.0242**	0.0000
Truck	-0.0298**	0.0001	-0.0031	0.0000
Cable/Satellite TV	0.0435***	0.0008	0.0552***	0.0000
Water Tank	0.0356***	-0.0019**	0.0123**	0.0000
Refrigerator	0.0326***	-0.0067***	-0.0001	-0.0002
Gas Stove	0.0266***	-0.0213***	0.0008***	-0.0022***
VCR	0.0293*	-0.0015**	0.0225***	0.0000

[a]Marginal effects multiplied by 100. Estimation without fixed effects. Observations: 74034.

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

estimated coefficient of $\Delta U(Y_a, B_a)$. In terms of fitting the signs predicted by the model and in terms of significance, the intermediate value of risk-aversion ($\sigma = 1$) does better than the high value ($\sigma = 2$) for both underreporting and overreporting, and does better than the low values ($\sigma = 1/2$ and $\sigma = 0$) for overreporting and at least comparably for underreporting.[11] If we are concerned about the use of expenditure per capita in the calculation of the utility gain, Table VI is reassuring: the results obtained measuring the utility gain as the cash benefit from the program ($\sigma = 0$) are similar in sign and magnitude to those obtained for other assumptions on relative risk-aversion.

Using the estimates for $\sigma = 1$ in Table VI, we have that increasing in 40 pesos (US$ 3.63) the per capita benefits from the program (that is, roughly speaking, increasing the benefits in 50%) would increase underreporting on cars, satellite TV, gas boilers, water tanks and refrigerators in between 1.3% and 2.2%, and would reduce overreporting in gas stoves, toilets, refrigerators and washing machines in between 0.22% and 0.85%.

Of the independent variables other than $\Delta U(Y_a, B_a)$, education is of particular interest. If overreporting is simply or mostly the result of confusion, we would expect more educated applicants to overreport less frequently. On the other hand, if overreporting is at least in part the result of social embarrassment, we could expect more educated applicants to overreport more frequently than others.[12]

The marginal effects of education on misreporting are described in Table VII. The effect on underreporting is calculated as one hundred times

$$\hat{\beta}_{1gs} f(\hat{\alpha}_{1g} + \hat{\beta}_{1g} \Delta U(Y_a, B_a) + \hat{\gamma}_{1g} X_a),$$

evaluated at the mean values of the exogenous variables for those having the good, where $\hat{\beta}_{1gs}$ is the estimated coefficient of school years in equation (1), and the other terms are as defined above. The effect on overreporting is calculated similarly. The results come squarely in favor of the embarrass-

[11] Consumption studies, such as that of Banks et al. (2001) in the UK, estimate the coefficient of relative risk-aversion in around 2. With a different methodology, experimental studies both in the lab (Holt and Laury 2002) and in the field (Tanaka, Camerer and Nguyen 2006) favor estimates of around 1/2.

[12] In the literature on voter turnout, for instance, the empirical fact that more educated people tend to vote more often is interpreted as a result of more educated people being better integrated in society and thus more susceptible to social pressure (Blais 2000).

TABLE VII

MARGINAL EFFECT OF YEARS OF EDUCATION ON MISREPORTING[a]

Good	Effect on Underreporting	Effect on Overreporting
Concrete Floor	-0.5493***	1.1765***
Tap Water	-0.3259***	0.7088***
Toilet	-0.3442***	1.1539***
Gas Boiler	-0.7411**	0.0807***
Washing Machine	0.6396***	0.1243***
Phone	-0.1050	0.0599***
Car	0.0005	0.0402***
Truck	0.2439	0.0187**
Satellite TV	0.2743	0.0386***
Water Tank	-0.9232***	0.2761***
Refrigerator	0.2068*	0.4351***
Gas Stove	-0.2940***	1.0683***
Video Recorder	0.4468*	-0.0231

[a]Marginal effects multiplied by 100.

TABLE VIII

EFFECT OF GENDER ON MISREPORTING[a]

Good	Effect on Underreporting	Effect on Overreporting
Concrete Floor	2.0514***	-3.8949***
Tap Water	2.7901***	-4.9492***
Toilet	3.1347***	-5.8000***
Gas Boiler	-2.9654	0.0014
Washing Machine	3.5373	-0.4977
Phone	6.0261	-0.3415***
Car	8.4165**	0.0223
Truck	9.7598***	0.0163
Satellite TV	-2.0582	0.1664
Water Tank	3.1493	0.3662
Refrigerator	3.6416**	-2.0754***
Gas Stove	4.1881***	-5.0649***
Video Recorder	-3.2651	0.3235

[a]Effect of a male rather than a female applicant multiplied by 100.

Estimations with moderate risk-aversion ($\sigma = 1$) and without fixed effects. Observations: 74034. * significant at 10%; ** significant at 5%; *** significant at 1%.

ment hypothesis. In almost every item, education significantly increases the probability of overreporting.[13]

Finally, we take a look at the effect of gender on misreporting. Since most of the applicants are women, and benefits from the program accrue to the mother of the household, our sample of male applicants is hardly representative. Undaunted, we proceed to calculate the difference in the behavior of male and female applicants with regard to reporting. The effect of gender on the percentage of misreporting is described in Table VIII. Male applicants are significantly more likely to underreport and less likely to overreport in "status" goods and in durable goods, and (curiously) they are also significantly more likely to underreport in cars and trucks.[14]

Of our sample of 74,034 initially eligible households, 64,842 households were classified as eligible after the household visit. Table IX details the incidence of underreporting in households that were rejected by the program and in households that were accepted. Given the high incidence of underreporting in the households that were considered eligible in the end, we seriously doubt that there was any penalty for underreporting per se.

5 Conclusions

How much do applicants to a social program misreport their "true" characteristics when they know this information will be used to determine participation in the program? How sensitive is misreporting to the program benefits? We find that underreporting is widespread. Overreporting is common in goods whose absence in a household is associated with poverty–precisely those goods that are likely to be given some weight in the methodology employed to determine participation in a poverty-alleviation program. Both

[13] A potential criticism of this result is that individuals who overreported with respect to "status" goods may also be inclined to overreport with respect to years of education. We have found, however, that overreporting is more common in goods that most households have, while most applicants seem to have very few years of education.

[14] Evidence on gender differences in preferences is discussed by Eckel and Grossman (2003) and by Croson and Gneezy (2004). Croson and Gneezy (2004) note that research from psychology suggests that men are more overconfident than women, and that women are more sensitive to social cues in determining appropriate behavior. This seems to favor the interpretation of the evidence on overreporting as the result of embarrassment rather than wrong beliefs.

TABLE IX
UNDERREPORTING AND FINAL STATUS OF VERIFIED HOUSEHOLDS

	Rejected from Program			Accepted by Program		
Good	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Concrete Floor	8521	0.20	0.40	39045	0.13	0.34
Tap Water	8045	0.16	0.37	43977	0.13	0.34
Toilet	8526	0.18	0.39	45575	0.16	0.37
Gas Boiler	1290	0.82	0.38	1093	0.64	0.48
Washing Machine	2465	0.71	0.46	3818	0.42	0.49
Phone	1169	0.82	0.38	788	0.59	0.49
Car	752	0.91	0.28	384	0.67	0.47
Truck	690	0.90	0.29	442	0.68	0.47
Satellite TV	788	0.89	0.32	929	0.61	0.49
Water Tank	1929	0.70	0.46	1902	0.46	0.50
Refrigerator	6925	0.53	0.50	13103	0.29	0.45
Gas Stove	8610	0.40	0.49	27201	0.19	0.40
Video Recorder	1293	0.91	0.29	1114	0.67	0.47

Observations: 74034. Source: *ENCASURB*.

underreporting and overreporting are sensitive to program benefits, more so in the case of underreporting.

Though the effect of program benefits on misreporting is not large at the margin, it is significant in the case of underreporting for a variety of specifications. We believe this robust result is important because applicants were aware that underreporting could affect the probability of receiving the program (positively by increasing the probability of qualifying for a household visit and, perhaps, negatively by increasing the probability of being penalized for providing false information) but they were also aware that *underreporting could not affect the benefits from participating in the program.* Thus, if they were expected utility maximizers and did not suffer any disutility for lying per se, their decision to underreport or report truthfully should not have depended at all on the size of the benefits from the program, so long as these were positive. If we stick with the expected utility maximization hypothesis, we must conclude that applicants dislike deception enough to forego some probability of participating in a program with large benefits rather than deceiving.

There is evidence that people deviate from single-mindedly pursuing what is best from the point of view of their individual material interests in a variety of circumstances: they vote even though they know they are not decisive, they give to charities and political organizations, they punish others at a cost to themselves when they believe they have been treated unfairly. We show that people also deviate from single-mindedly maximizing their individual material interests (or rather those of their family) in the setting of a face-to-face interview with a program official. They do so in a manner that is consistent with the existence of a disutility for deceiving others and, possibly, a disutility for reporting the lack of some goods whose possession is widespread among households of a similar social condition.

More generally, our work suggests that the design of social programs can be improved by taking into account current research on the actual behavior of individuals in contexts that combine economic incentives with ethical and social considerations. And vice versa, the understanding of the behavior of individuals in those contexts can be improved by taking a look at the evidence coming from social programs, with their large samples and high stakes.

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