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. In the model we propose and estimate, 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. We also estimate the costs of lying and embarrassment for different goods, and show that the embarrassment cost for lacking a good is proportional to the percentage of households who own the good.

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. 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-

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[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 propose and estimate a one-agent decision model, where underreporting is imputed to financial incentives, truthful reporting to a cost for lying, and overreporting to an embarrassment motive. While simple, the model captures the relevant trade-offs for an applicant to a social program that uses self-reported information. An applicant who has some desirable household characteristic will report not having it if the increase in the probability of getting into the program by lying, multiplied by the program benefits,

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[3] Riphahn (2001) provides some recent evidence on widespread lack of participation in social programs by potential beneficiaries. See also Currie (2006) 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 general methodology was public (Reglas de Operación 2002) but not the specific weights.

exceeds the cost of lying. An applicant who does not have some desirable household characteristic will report having it if the embarrassment cost exceeds the increase in the probability of getting into the program by telling the truth, multiplied by the program benefits.

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% 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.

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. Our analysis is necessarily limited to applicants to the poverty-alleviation program; there may be possibly a large number of households with stronger embarrassment motives who do not even apply to the program.

At a broader level, there has recently been a surge of interest by econo-

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mists 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, so that the unwillingness to lie cannot be attributed to altruism.

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) 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.

Our results about the costs of lying and embarrassment are very valuable to the extent that it is unusual to find field data (as opposed to experimental data) on communication games. A preference for telling the truth is important for the working of many social institutions in different settings, from poverty measurement to opinion polls for policy purposes to the justice system. Our results illustrate that not only financial motives but also social and psychological motives play a role in determining whether individuals tell the truth or not. Social and psychological motives may deter individuals from lying for financial advantage, and conversely, a social motive like embarrassment may induce individuals to overreport in spite of financial costs.[6] The results thus provide concrete evidence on the relative importance of financial versus social and psychological motives for truthtelling behavior.

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[5]Including among others Crawford (2003), Chen (2005), Ettinger and Jehiel (2006), and Kartik, Ottaviani and Squintani (2007).

[6]This point is reminiscent of the costly management of "social information" (the information the individual directly conveys about himself) described by Goffman (1963).

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

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\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 a concrete floor are widespread among the urban poor in our sample, and lacking some of these may carry a stigma.

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* (see Table II) include grants conditional on the school attendance of the children in the household which vary by the grade of schooling and gender of each child as well as a fixed grant linked to regular health clinic visits by household mem-

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*.

bers. The program also includes free medical consultations and nutrition supplements.[7] 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 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.

There are a couple of potential selection issues in relation to our data. We study the reporting of only those individuals who apply, whereas it is commonly the case that many eligible households do not apply to targeted social programs.[8] In addition, not all households who apply were verified and this is likely directly related to their reporting. While most individuals are initially declared eligible, note that underreporting can only increase the likelihood of being declared eligible at the module whereas overreporting can

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[7]There has been a good deal of research estimating the impact of program benefits on participating households in rural areas in terms of education and health, including Schultz (2004), Behrman, Sengupta and Todd (2005), Gertler (2004), and Todd and Wolpin (2006), among others.

[8]We are studying the application decision in related work. In a sample of poor urban areas approximately 65 percent of eligible households applied to the program. Of those not applying, 84 percent can be attributed to lack of knowledge of the program or the lack of a registration module in the vicinity.

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Table II

Monthly Cash Benefits of Oportunidades[a]

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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	With High-School Children			1550
to Household	Other Households			915
Average Transfer[b]				350

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

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Table III

Applicants and their Households[a]

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	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*.

reduce the likelihood. Thus, in our sample of verified individuals, ceteris paribus, we are more likely to have underreporters than overreporters. This is of course important not only for potential selection, but for the implication that those who overreport may not receive benefits precisely because of their overreporting.

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.[9] If the applicant expects that effective verification will result at least in the correction of underreporting, we would have $\tau_g \geq \omega_g$.[10]

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[9] Applicants may have believed that either there was not going to be a verification visit with some probability, or that interviewers would not be careful. These beliefs, of course, may turn out to be incorrect ex post.

[10] Program operating rules state that "Program benefits will be suspended permanently when the family has given false information with respect to their socioeconomic conditions"

We assume that applicants hold point beliefs about the vectors $(\varpi_g)_{g=1}^m$ and $(\tau_g)_{g=1}^m$. We also assume that applicants believe that ρ is uniformly distributed in some interval $(0, \overline{\rho})$ where $\overline{\rho} \geq \sum_g \max\{\omega_g, \tau_g\}$. Thus, according to the applicant's beliefs, the probability that a is eligible is[11]

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

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 truthfully not having the good.

The problem of applicant a is then

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

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

$$(w_g - \delta p_g)\Delta U(Y_a, B_a) \geq c_{ag}.$$

That is, an applicant who has good g will report not having it if the increase in the probability of getting into the program by lying $(w_g - \delta p_g)$, multiplied by the program benefits, exceeds the cost of lying.

(Reglas de Operación 2002). Interviewers were not instructed to inform applicants about this program rule, though. As discussed later on, apparently there was no penalty other than the correction of underreporting.

[11] Note that the probability of being eligible is additively separable in the reports about the different goods. Separability follows from the assumption of uniform beliefs about the cutoff for eligibility into the program, and it is useful in terms of estimating the model.

Similarly, the applicant will overreport about good g if $t_{ag} = 0$ and

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

That is, an applicant who does not have good g will report having it if the embarrassment cost exceeds the increase in the probability of getting into the program by telling the truth (w_g), multiplied by the program benefits.

We assume that applicants have a constant relative risk aversion utility function with risk parameter σ; that is[12]

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

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

$$c_{ag} = \alpha_{cg} + \gamma_{cg} X_a + \epsilon_{cag} \quad \text{and} \quad e_{ag} = \alpha_{eg} + \gamma_{eg} X_a + \epsilon_{eag},$$

where ϵ_{1ag} and ϵ_{2ag} are random terms independently distributed across goods and across people according to logistic distributions with location parameter 0 and scale parameter s_{1g} and s_{2g}, respectively.

Thus, applicant a will underreport about good g if $t_{ag} = 1$ and

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

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

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

where $\alpha_{1g} = -\alpha_{cg}/s_{1g}$, $\beta_{1g} = (w_g - \delta p_g)/s_{1g}$, $\gamma_{1g} = -\gamma_{cg}/s_{1g}$, $\epsilon_{1ag} = \epsilon_{cag}/s_{1g}$, $\alpha_{2g} = \alpha_{eg}/s_{2g}$, $\beta_{2g} = -w_g/s_{2g}$, $\gamma_{2g} = \gamma_{eg}/s_{2g}$ and $\epsilon_{2ag} = -\epsilon_{eag}/s_{2g}$. Note that the error terms have a standard logistic distribution. Under the assumption that household visits were in fact effective in monitoring misreporting, equations (1) and (2) can then be estimated using logistic regressions.

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[12] We are sidestepping for simplicity the issue of the different horizon of benefits for different applicants, according to the age and school grade of their children. If household a has a horizon of benefits T_a and discount factor β_a, then its expected benefit of participating should be multiplied by $(1 - \beta_a^{T_a+1})/(1 - \beta_a)$. If this term is similar for all applicants (e.g. they all discount the future heavily) ignoring it is just a normalization of the utility function.

4 Empirical Analysis

We have estimated equations (1) and (2) for the different goods, using as samples respectively those households who were found to have the good and those households who were not, with 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 potential 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 pre-program (Table II). 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.[13] 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.

Maximum likelihood estimation of equations (1) and (2) yield values of σ near one (that is, between 0.7 and 1.3) for most goods.[14] For the purpose of comparability across goods, in what follows we focus on estimates based on $\sigma = 1$. We also discuss estimates based on other values of σ.

By the time the information on expenditure was collected, applicants knew that they 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

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[13]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.

[14]There are a few exceptions: equations (1) and (2) yield values of σ below 0.5 for car, and equation (1) yield values of σ above 1.7 for gas boiler, phone, video recorder and satellite TV.

thus include among our estimations the case of risk-neutrality ($\sigma = 0$), in which the utility gain of participating does not depend on the expenditure per capita in the household of the applicant.

4.1 Utility Gains and Applicants' Beliefs

Table IV provides maximum likelihood 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 $\sigma = 1$.

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. $\hat{\beta}_{2g}$ 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 good g, we have $\beta_{1g}/(-\beta_{2g}) \leq s_{2g}/s_{1g}$. In Table IV we have that $\hat{\beta}_{1g}$ is larger than $\hat{\beta}_{2g}$ in absolute value for almost every good, implying that s_{1g} is smaller than s_{2g}. Recall that the variance of a logistic distribution is proportional to the scale parameter. Thus, Table IV suggests that for almost every good the variance of the error term in the cost of lying is smaller than the variance of the error term in the embarrassment cost.

Table V provides estimates of the relative weight of each good according to the estimated coefficients for equations (1) and (2). We assume that underreporters believe that the penalty for underreporting is proportional to the weight of each good. In terms of the discussion in the previous paragraph, the assumption here is that the variance of ϵ_{cag} is the same across goods, and

Table IV

Estimated Coefficient of Utility Gains on Misreporting[a]

Good	Underreporting Equation		Overreporting Equation	
	Without Fixed Effects	Reg. Center Fixed Effects	Without Fixed Effects	Reg. Center 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.

TABLE V

WEIGHTS OF GOODS IN PROGRAM PARTICIPATION[a]

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

[a]With moderate risk-aversion ($\sigma = 1$) and without fixed effects. Observations: 74034.

similarly for ϵ_{eag}. Relative weights are calculated as

$$\text{weight of good } g \text{ for under-reporters} \quad = \quad \frac{\max\{0, \hat{\beta}_{1g}\}}{\sum_{g'} \max\{0, \hat{\beta}_{1g'}\}}, \text{ and}$$

$$\text{weight of good } g \text{ for over-reporters} \quad = \quad \frac{\max\{0, -\hat{\beta}_{2g}\}}{\sum_{g'} \max\{0, -\hat{\beta}_{2g'}\}}.$$

(We censor in each case the two estimated coefficients with incorrect signs from the point of view of the model.) 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 and omitted in the table). Note that, since applicants were unlikely to have access to the weights employed by the program, the assumption that applicants had common beliefs about the weights is only a useful simplification.

4.2 Effect of Program Benefits on Misreporting

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

We evaluate the expression above at the mean values of the exogenous variables for those having the good, and multiply this value by one hundred to obtain the marginal effect of the program benefits on underreporting. The marginal effect on overreporting is calculated similarly. (Note that, consistently with Table I, we define underreporting of good g as the number of applicants who underreport as a percentage of the number of applicants who have this good, and define overreporting similarly.)

Table VI displays the results for four values of the risk-aversion coefficient. The significance levels are those of the 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.[15] If we are concerned about the use of reported expenditure per capita in the calculation of the utility gain, Table VI is reassuring: the results obtained measuring the utility gain as the cash

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[15]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.

TABLE VI

MARGINAL EFFECT OF PER CAPITA BENEFITS ON MISREPORTING[a]

| | Risk Neutrality ($\sigma = 0$) | | Low Risk Aversion ($\sigma = 1/2$) | |
| | Effect on | Effect on | Effect on | Effect on |
Good	Underreport	Overreport	Underreport	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***	-0.0110***
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 ($\sigma = 2$) | |
| | Effect on | Effect on | Effect on | Effect on |
Good	Underreport	Overreport	Underreport	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 on probability of misreporting multiplied by 100. Estimation without fixed effects. Observations: 74034.

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

TABLE VII

EFFECT OF EDUCATION ON MISREPORTING

| | Effect of Education[a] on | |
Good	Underreport	Overreport
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 effect of years of education on probability of misreporting 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%.

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 percent points, and would reduce overreporting in gas stoves, toilets, refrigerators and washing machines in between 0.22 and 0.85 percent points.

4.3 Effect of Education on Misreporting

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

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frequently than others.[16]

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 embarrassment hypothesis. In almost every item, education significantly increases the probability of over-reporting. The effect is stronger for goods whose absence is more likely to be embarrassing, including toilets, concrete floor and tap water.[17]

4.4   Was There a Penalty for Underreporting?

We analyze here whether verification was enforced, i.e. whether households misreporting in the registration center and found to be ineligible in the verification were actually excluded from the program. Table VIII shows benefi-ciary status by eligibility classification in the registration center and in the verification. As expected, those found to be initially eligible in the regis-tration center and who continue to be eligible after the verification (63,243 households) are very likely to become beneficiaries. We turn now to house-holds who were classified initially eligible at the registration center and found to be ineligible after the verification (10,791 households). Surprisingly, 31 percent of these households did in fact become beneficiaries. So, while the verification was effective at excluding a majority of households who misre-ported enough in the registration center to appear eligible when they were

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[16]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).

[17]A potential criticism of these results is that individuals who overreported with respect to some 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. Since most applicants have very few years of education, having little education is unlikely to be embarrassing.

TABLE VIII
FINAL STATUS OF VERIFIED HOUSEHOLDS

Eligibility Status	Became Program Beneficiary
Eligible in Reg. Center but not in Verification	31.3%
Eligible in Reg. Center and in Verification	97.6%
Underreported	93.8%
Did Not Underreport	98.1%

Observations: 74034.

in fact ineligible, an important minority were able to underreport and get into the program anyway. In terms of the model, the true probability that verification is effective is about 0.687.

A second question is whether there is any evidence that there was a penalty for underreporting after the household visit,[18] beyond recalculating the score. Looking at the sample of households declared eligible at the verification visit, there is a small difference in the percentage of households that became beneficiaries between those who underreported in the registration center and those who did not. While this might imply there was at least a minimum penalty to underreporting beyond correcting characteristics, the (small) difference may also reflect differing take up rates between the two groups for other reasons. Thus, there is little evidence of a rigorous or uniform penalty applied to underreporting beyond correction of characteristics.

4.5 The Costs of Lying and Embarrassment

According to our model, the utility cost of lying about good g for applicant a if the applicant has this good is

$$c_{ag} = \alpha_{cg} + \gamma_{cg}X_a + \epsilon_{cag} = s_{1g}(-\alpha_{1g} - \gamma_{1g}X_a + \epsilon_{1ag}),$$

where ϵ_{1ag} has a standard logistic distribution.

We can calculate the expected utility cost of lying for an applicant as

$$\text{E}(\max\{s_{1g}(-\alpha_{1g} - \gamma_{1g}X_a + \epsilon_{1ag}), 0\}).$$

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[18]Program rules might suggest this (see footnote 10).

Note that we bound the cost from below by zero, i.e. we interpret a negative realization of $-\alpha_{1g} - \gamma_{1g}X_a + \epsilon_{1ag}$ as meaning a zero cost of lying or no disincentive to lie.[19] Solving the expectation we obtain that the expected utility cost of lying for an applicant that has good g is

$$-s_{1g}(\alpha_{1g} + \gamma_{1g}X_a) + s_{1g}\ln(1 + \exp(\alpha_{1g} + \gamma_{1g}X_a)).$$

Using equivalent variations and assuming $\sigma = 1$, the monetary cost of lying is

$$c_{ag}^m = \bar{Y}_a \exp\left[-s_{1g}(\alpha_{1g} + \gamma_{1g}X_a) + s_{1g}\ln(1 + \exp(\alpha_{1g} + \gamma_{1g}X_a))\right] - \bar{Y}_a,$$

where $\bar{Y}_a$ is the total household expenditure of applicant a. Intuitively, c_{ag}^m represents how much money the household should receive for an applicant to be willing to conceal having good g.

Similarly, the monetary cost of embarrassment for an applicant who does not have good g is

$$e_{ag}^m = \bar{Y}_a \exp\left[s_{2g}(\alpha_{2g} + \gamma_{2g}X_a) + s_{2g}\ln(1 + \exp(-\alpha_{2g} - \gamma_{2g}X_a))\right] - \bar{Y}_a.$$

Intuitively, e_{ag}^m represents how much money the household should receive for the applicant to declare, embarrassingly, not having good g.

In order to calculate the costs of lying and embarrassment, we need to have some estimate of the scale parameters s_{1g} and s_{2g}. Recall that the applicant will overreport if

$$e_{ag} \geq w_g \Delta U(Y_a, B_a)$$

or equivalently if

$$s_{2g}\beta_{2g}\Delta U(Y_a, B_a) + s_{2g}(\alpha_{2g} + \gamma_{2g}X_a - \epsilon_{2ag}) \geq 0.$$

We can estimate the coefficients β_{2g}, α_{2g} and γ_{2g}, but not the "original coefficients" $s_{2g}\beta_{2g}$, $s_{2g}\alpha_{2g}$ and $s_{2g}\gamma_{2g}$ (see e.g. Train 2003). Note that according

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[19]It is simple to check that the likelihood function associated with equation (1) is the same whether we assume that ϵ_{1ag} has a standard logistic distribution or that ϵ_{1ag} has a density identical to the standard logistic density for $\epsilon_{1ag} \geq \alpha_{1g} + \gamma_{1g}X_a$ and a point mass at $\epsilon_{1ag} = \alpha_{1g} + \gamma_{1g}X_a$, as long as $\beta_{1g} > 0$.

to the model $-s_{2g}\beta_{2g}$ is equal to $\omega_g/\bar{\rho}$, the weight of good g according to applicants' beliefs multiplied by the density representing the beliefs of applicants about the poverty line. We know, however, the true weights of the goods ω_g^T and the true value of the poverty line ρ^T. We can approximate s_{2g} if we assume that applicants' beliefs are roughly correct, as follows.

First, we assume the expected value of the poverty line according to the applicants' beliefs is equal to the true value of the poverty line. That is, $\bar{\rho}/2 = \rho^T$. Second, we assume that the applicants guess correctly the sum of the true weights of all the goods we consider, even if not necessarily the true weight of any particular good. That is, $\sum_g \omega_g = \sum_g \omega_g^T$. These two assumptions together imply $\sum_g s_{2g}\beta_{2g} = \sum_g -\omega_g^T/\bar{\rho} = \sum_g -\omega_g^T/2\rho^T$. Finally, we assume that s_{2g} is the same for every good, or equivalently that the variance of ϵ_{2ag} is the same for every good. This implies $s_{2g} = \left(\sum_{g'} s_{2g'}\beta_{2g'}\right) / \sum_{g'} \beta_{2g'}$ for all g. Thus, s_{2g} can be estimated as

$$\hat{s}_{2g} = \left(\sum_{g'} -\omega_{g'}^T/2\rho^T\right) / \sum_{g'} \min\{0, \hat{\beta}_{2g'}\} \text{ for all } g.$$

(We censor the two estimated coefficients with incorrect signs from the point of view of the model.)

We proceed similarly with respect to s_{1g}. Consistent with the discussion in Section 4.4, we assume that applicants believe that the only penalty for underreporting, in case verification is effective, is to correct underreporting (i.e. $\tau_g = \omega_g$), and guess correctly the probability that verification is effective. This implies $\sum_g s_{1g}\beta_{1g} = \sum_g (\omega_g^T - \delta^T \omega_g^T)/\bar{\rho} = (1-\delta^T)\sum_g \omega_g^T/\bar{\rho}$, where δ^T is the true probability that verification is effective. Thus, s_{1g} can be estimated as

$$\hat{s}_{1g} = (1 - \delta^T)\left(\sum_{g'} \omega_{g'}^T/2\rho^T\right) / \sum_{g'} \max\{0, \hat{\beta}_{1g'}\} \text{ for all } g.$$

According to the poverty regression actually used by the program, households received a number of points for lacking certain goods and also a number of points related to family size and region. To calculate $\hat{s}_{1g}$ and $\hat{s}_{2g}$, we take ω_g^T to be the number of points that a household gets toward qualifying for the program for lacking good g, and we take ρ^T to be the maximum number of points that a household gets for reasons other than lacking goods, plus the number of points a household gets for lacking all goods, minus the number of points required to qualify for the program.

TABLE IX

MEDIAN MONETARY COST OF LYING AND EMBARRASSMENT[a]

| Good	Cost of Lying	Cost of Embarrassment
Concrete Floor	29.25	35.89
Tap Water	29.46	45.37
Toilet	26.61	61.88
Gas Boiler	4.50	1.24
Washing Machine	8.60	7.41
Phone	3.26	1.39
Car	3.49	0.91
Truck	1.32	0.58
Satellite TV	5.15	1.55
Water Tank	7.83	4.32
Refrigerator	14.96	15.82
Gas Stove	21.22	45.40
Video Recorder	3.50	2.28

[a]In Mexican pesos (2002); 11 pesos is approx. US\$1. With moderate risk-aversion ($\sigma = 1$) and without fixed effects. Observations: 74034.

Intuitively, to estimate $\hat{s}_{1g}$ and $\hat{s}_{2g}$ we assume that applicants understand the weight of goods vis-a-vis other criteria in eligibility to the program, but are uncertain about the poverty line so they believe that by declaring any good they are reducing the probability of being declared eligible.

Table IX offers the median monetary costs of lying and embarrassment. The cost of lying for different goods varies from close to 0 to about 2.68 US dollars, and the cost of embarrassment for the lack of different goods varies from close to 0 to about 5.63 US dollars. Note that the order of magnitude of the cost of lying depends on the assumed applicants' beliefs about the probability that verification would be effective. As an upper bound, if applicants did not expect verification to be effective at all, our estimates of the cost of lying would increase threefold.

As illustrated in Figure I, both the cost of lying and the cost of embarrassment about goods are positively correlated with the percentage of households who have each good. The correlation between embarrassment costs and the percentage of households who have each good is consistent with the idea that embarrassment is more severe with respect to goods which are widespread in

FIGURE I
COST OF LYING AND EMBARRASMENT AND HOUSEHOLDS
HAVING THE GOOD
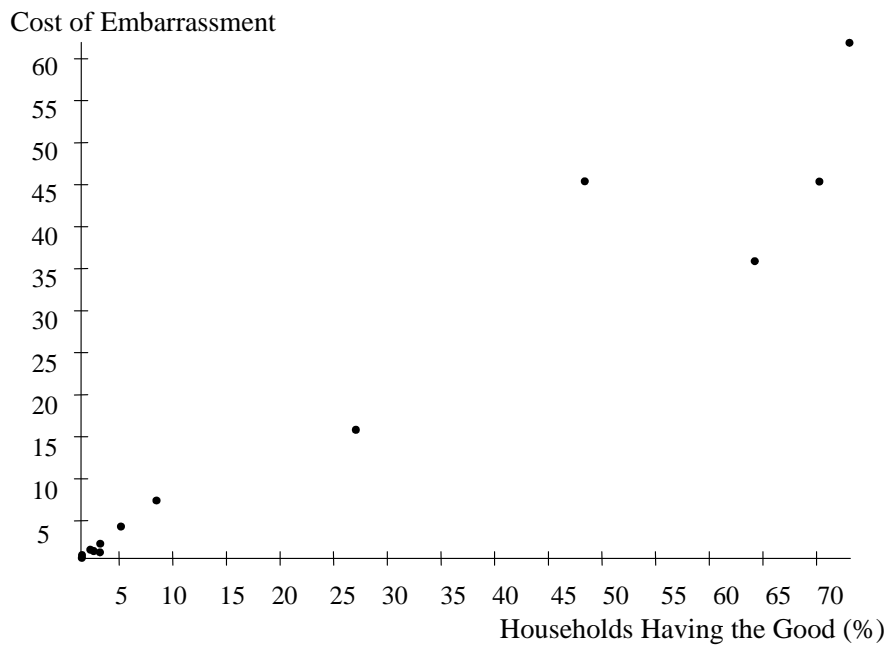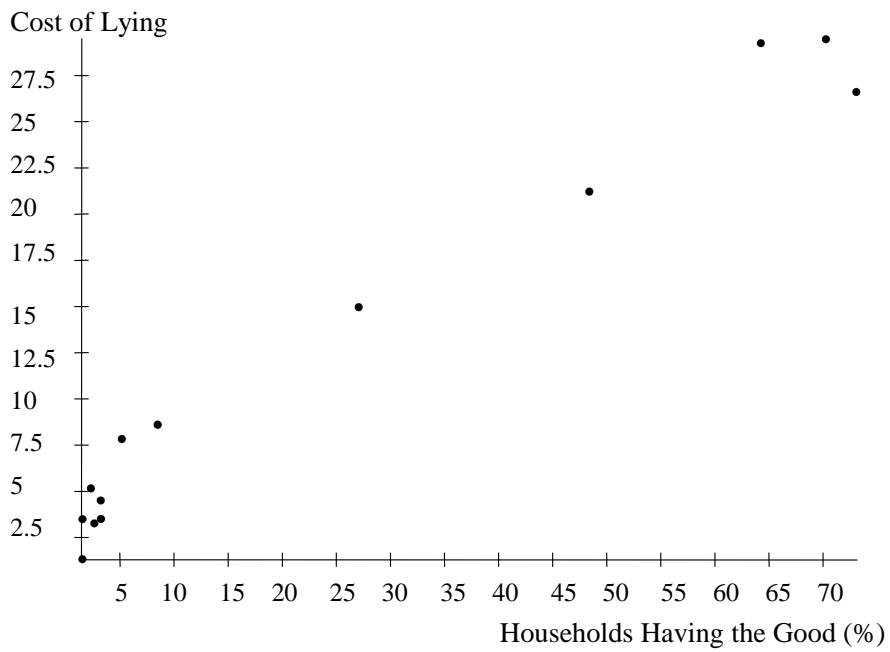
the social group of the applicant. The correlation between lying costs and the percentage of households who have each good is a bit more surprising, and seems to indicate that it is easier to lie with regards to goods that are uncommon in the social group of the applicant.

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 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 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.

From the viewpoint of the design of the process of selection of beneficiaries for targeted social programs, our research brings good and bad news. Among the good news, underreporting is widespread but not overwhelming, except for a few goods. Among the bad news, overreporting is common for a few goods, which implies that self-reporting may lead to the exclusion of deserving households. There is a wide variation in the incidence of misreporting for different goods. Thus, applicants' reports can be useful in the context of the selection of beneficiaries as long as the weights assigned to different goods take into account the incidence of misreporting.

The crux of the "poverty regression" methodology is the use of data on household characteristics which are presumably observable and are statistically correlated with income. Our research points out that using the applicants' reports with weights obtained from a poverty regression is limited by the extent and variance across goods of misreporting. Moreover, finding the optimal weights is complex because the incidence of misreporting will not be constant with changes in the targeting methodology. That is, the issue of determining the optimal weights is not purely statistical but involves a strategic component.

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 evidence coming from social programs offers a window to understanding the behavior of individuals in those contexts.

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