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Targeting with Agents†

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Targeting assistance to the poor is a central problem in development. We study the problem of designing a proxy means test when the implementing agent is corruptible. Conditioning on more poverty indicators may worsen targeting in this environment because of a novel tradeoff between statistical accuracy and enforceability. We then test necessary conditions for this tradeoff using data on Below Poverty Line card allocation in India. Less eligible households pay larger bribes and are less likely to obtain cards, but widespread rule violations yield a de facto allocation much less progressive than the de jure one. Enforceability appears to matter. (*JEL* D12, I32, I38, O12, O15)

Which households should be eligible for social assistance? Targeting is a central problem in public economics, particularly for developing countries. Because these countries do not have reliable data on the income or consumption of their citizens, they often rely instead on "proxy means tests," or categorizations of households into eligible and ineligible groups based on easier-to-observe characteristics. For example, households that own color televisions might be ruled ineligible. A large literature has developed showing how to design optimal PMTs by applying statistical decision theory to household survey data.[1] In this paradigm, "the optimal policy equates the marginal reduction in poverty from a further indicator being used with its marginal administrative cost" (Besley and Kanbur 1990, 14).[2]

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[1] See Ravallion (1989); Ravallion and Chao (1989); Besley (1990); Besley and Kanbur (1990); Glewwe (1992); Ravallion and Sen (1994); Grosh and Baker (1995); Schady (2000); Park, Wang, and Wu (2002). See Grosh (1994) and Coady, Grosh, and Hoddinott (2004) for reviews of targeting in practice.

[2] Sophisticated applications may also take into account the distortion of household incentives that targeting generates (Mirrlees 1971). An alternative approach to targeting is to impose requirements on beneficiaries that make the program "self-targeting" (Besley and Coate 1992), generating a different set of agency problems (Niehaus and Sukhtankar 2011).

In practice, however, the rule implemented may differ from the rule designed. Research on corruption has provided many examples of ways in which the officials who implement social programs bend or break the rules. They divert transfers from the intended recipients (Reinikka and Svensson 2004; Olken 2006), inflate claims about program participation (Niehaus and Sukhtankar 2011), demand bribes to issue permits to eligible recipients (Svensson 2003), and take bribes to issue permits to ineligible recipients (Bertrand et al. 2007). The optimal response to such problems may involve not only tougher enforcement but also changing the very nature of the task assigned (Banerjee 1997; Banerjee, Hanna, and Mullainathan 2011).

Motivated by these observations, we ask a simple but important question: how should targeting rules be designed when they must be implemented by corruptible agents? We study a model in which a principal with progressive preferences defines a targeting rule to be implemented by a subordinate official. The official may have distinct allocative preferences—for example, he may wish to give slots to voters—or be tempted to demand bribes. The principal's capacity to discipline the official is limited, perhaps because arbitrarily large punishments are not available (Becker 1968; Mirrlees 1999). The official therefore sets a schedule of household-specific bribe prices (possibly equal to 0) that optimally trade off his allocative preferences, bribe rents, and expected penalties. This schedule then determines the allocation of slots and rents.

We use this framework to examine properties of optimal targeting rules. The most striking lesson is that conditioning eligibility on an additional poverty indicator can strictly worsen targeting. This is true even though from a purely statistical perspective the additional indicator can only help. Of course, if the indicator is not perfectly verifiable then one might expect these gains to be diluted because of the monitoring problem. What we show is that they may in fact be reversed. The reason is that the additional indicator affects not only who is eligible (the statistical effect) but also how verifiable the (in)eligibility of other inframarginal households is (the enforcement effect). If the enforcement effect is sufficiently negative it may trump the positive, statistical effect.

A concrete example may help illustrate this. Suppose that households with paved floors are ineligible. Some of these households are poor, so this rule is imperfect. On the other hand, a third party can verify ineligibility simply by observing a paved floor. Anticipating this, the official may be reluctant to sell slots to ineligible households. Now consider refining the rule so that households with paved floors are eligible unless they also have a television set. Statistically speaking this may be an improvement if most of the newly eligible households are poor. For a third party to verify ineligibility, however, he must now verify that a household has both assets. Verifying both facts is harder than verifying just the first, so the official need be less apprehensive about giving (or selling) slots to ineligible households. If this enforcement effect is strong enough it may more than offset the statistical gains.[3]

The possibility that more targeting could backfire raises the questions whether and when this is likely to happen in practice. We make progress on these issues in

[3] There is also a positive effect on enforcement for households that were previously eligible. Our formal result identifies cases where the net effect on enforcement is negative.

two ways. First, we identify and test three conditions that must hold in order for targeting to backfire. The first two conditions are simply that enforcement be weak and that the official's preferences be poorly aligned with the principal's. The third, more interesting condition characterizes the technology of enforcement: enforcement must work in such a way that *degrees* of (in)eligibility, and not just (in)eligiblity per se, influence the official's choices. If any one of these conditions does not hold then the rule designer can safely ignore the agency layer.

We test these conditions in the context of Below Poverty Line (BPL) card allocation in Karnataka, India. BPL cards are India's most important targeting mechanism; participation in a wide range of public schemes, including the Targeted Public Distribution System (TPDS), is restricted to cardholders. Different states use different proxy means tests to allocate BPL cards. In Karnataka the PMT consists of a series of exclusion restrictions. For example, a household that owns a water pump or an automobile is ineligible. Local officials are responsible for implementing this rule, subject to monitoring by back-checking teams.

To understand how BPL card allocation works we collected original survey data on over 14,000 households in rural Karnataka. Our data have several novel features relative to earlier analysis of targeting. Most importantly, they include both households' statutory eligibility and their actual BPL status, letting us estimate rule violations. They also include the prices charged to 93 percent of BPL card recipients (and 73 percent of households overall), letting us examine the role of bribery in the allocation process.

Our data suggest that enforcement is weak. We estimate that 70 percent of the ineligible households in our sample have BPL cards, while 13 percent of eligible households do not. Overall 48 percent of the households in our sample are misclassified, and eligible households are only 21 percent more likely than ineligible ones to hold cards. Bribes are commonplace—75 percent of households report paying a price above the statutory fee—but interestingly the mean (conditional) overpayment is only Rs 14, and ineligible households pay only Rs 3 more on average than eligible ones, also consistent with weak enforcement.

The data also suggest that rule violations are not driven primarily by officials trying to improve targeting using their own soft information. In regressions controlling for eligibility, income plays a small or insignificant role in predicting prices and allocations. Overall, while statutory eligibility is correlated -0.55 with log income, the correlation with the actual allocation of BPL cards is only -0.23. Strikingly, this is weaker than the correlation between income and a number of individual, readily observable criteria. For example, ownership of a water pump and having a gas connection are correlated -0.32 and -0.30 with income, respectively. The allocation of BPL cards would thus be more progressive if the government could enforce an eligibility rule based solely on one of these relatively coarse criteria.

Finally, we find that eligibility is not a sufficient statistic; instead, degrees of ineligibility matter for predicting both prices and allocations. The price a household reports being charged for a BPL card increases monotonically with the number of eligibility rules it violates, while the probability that the household holds a BPL card decreases monotonically with the number of violations. This is important as it suggests the presence of enforcement effects that could be exploited by a sophisticated rule designer.

Our first set of empirical results thus provide support for the three qualitative conditions that must hold in order for targeting to backfire. Given this, we also conduct a second, quantitative exercise: we assess the welfare gains from status quo targeting as opposed to the simplest alternative, universal eligbility. This comparison is particularly relevant in the Indian context, where eligibility was universal until 1997 but has been targeted since. We estimate that imperfect enforcement substantially enlarges the set of social welfare functions for which the principal prefers universal eligibility to targeting.

Interestingly, our emphasis on enforceability parallels a recent shift within Indian policy debates. While early critiques of BPL policy focused on statistical accuracy (Sundaram 2003), more recent analyses have argued that misimplementation is as important a constraint on performance (Hirway 2003). Dreze and Khera (2010) have recently proposed using dramatically simpler targeting criteria, such that every household can attribute its inclusion or exclusion to a single criterion, on the grounds that this would reduce fraud. Our model provides one formal justification for their idea.

Our empirical results extend a line of work by Alderman (2002) and Olken (2005) documenting how local officials use discretionary power over the allocation of welfare benefits. In particular, Alderman (2002) finds evidence that officials target "soft" measures of poverty as well as easily observable ones. This is analogous to our finding that income has some predictive power even conditional on observable characteristics. Because eligibility is nondiscretionary in our setting, however, we are able to go further, measuring eligibility and examining whether the de facto allocation is *on net* more or less progressive than the de jure one. Our paper also fits within a broader literature on targeting that has examined how targeting performance differs with village characteristics (Galasso and Ravallion 2005; Bardhan and Mookherjee 2006c), with government as opposed to NGO implemention (Banerjee et al. 2009), and with rule-based as opposed to community-based procedures (Alatas et al. forthcoming). Finally, our analysis builds on the broader decentralization literature in emphasizing the potential tradeoff between the benefits of local information and the risks of elite capture (Bardhan 2002; Bardhan and Mookherjee 2000, 2005, 2006a, 2006b). Indeed, our empirical comparison between the de jure and de facto allocations of BPL cards speaks directly to this core question.[4]

The rest of the paper proceeds as follows: Section I develops the theoretical apparatus necessary to think about targeting with agents; Section II describes the empirical context in which we work and the data we collected; Section III analyzes targeting and rent extraction in this setting; Section IV presents counterfactual simulations, and Section V concludes.

[4] The paper also relates indirectly to work on geographic targeting (Schady 2000; Park, Wang, and Wu 2002) in that geographic targeting may be particularly attractive in weakly-enforced settings.

I. Targeting with Agents

A. *An Example*

We begin with a simple example before presenting our full theoretical apparatus. Our goal is to explain, in a loose but easy-to-follow way, how "more targeting" could backfire. Some elements of the story are necessarily left imprecise for the time being.

Consider a set of households of measure 2, half of whom are rich and half poor. A principal wishes to allocate slots among these households; he obtains a net benefit $b > 0$ for each poor household that obtains a slot, but incurs a net cost $c > 0$ for each rich household that does so. The actual allocation of slots must be implemented by an agent, who observes each household's type. The principal can instruct the agent to give slots to all households (universal eligibility), no households (no program), or only to poor households (targeting).

First consider the (standard) case in which the principal's monitoring technology allows him to perfectly discipline the agent. Then the optimal policy is clearly to instruct the agent to give slots to all poor households and no rich ones, yielding a payoff of b. Equivalently, suppose the agent's preferences are the same as the principal's; then the principal can again simply instruct him to give slots to the poor.

Now suppose that enforcement is weak and that the agent has distinct allocative preferences, or is tempted to extract rents by charging bribes. In this case the principal's instructions may not be perfectly implemented. In particular, suppose that under universal eligibility each household obtains a slot with probability q_U, while under targeting the eligible poor (ineligible rich) obtain slots with probability q_E (q_I). Then targeting increases the principal's payoff if and only if

$$(q_U - q_I)c - (q_U - q_E)b > 0. \tag{1}$$

The first term captures how making rich households ineligible affects their likelihood of obtaining slots (i.e., inclusion errors). If enforcement is at all effective then this likelihood must fall $(q_U > q_I)$, so that the effect is positive. This is the benefit of better statistical targeting. The contribution of the second term, on the other hand, is more subtle. This term captures how making rich households ineligible affects the likelihood that *poor* households obtain slots (i.e., exclusion errors). There could be no effect; if verifying that a poor household is poor is just as easy as verifying that it exists then we would expect to see $q_E = q_U$. On the other hand, verifying poverty could be harder than verifying existence; an auditor would need to produce hard evidence of income or assets, depending on how poverty is defined. In this case the official would worry less about denying a slot to a poor household under targeting than under universal eligibility, so that $q_E < q_U$. This generates a negative enforcement effect which, if strong enough, could more than offset the statistical gains from targeting.

Though stylized, this example suggests that using additional targeting criteria could be counterproductive if three conditions hold: enforcement is weak, the agent's preferences are misaligned with the principal's, and the ability to detect misallocation depends on the details of the rule. The theoretical analysis that follows will show that these conditions are indeed necessary (Proposition 1) and provide an example in which simpler rules do indeed perform better (Proposition 2). The empirical analysis in Section III will then test Proposition 1's conditions using data on the BPL card allocation in Karnataka.

B. *The Agent*

A principal wishes to allocate slots among a set of households. Household i has income $y_i \in \{\underline{y}, \overline{y}\}$ and other characteristics $\mathbf{x}_i \in \mathbf{X}$ which are potentially correlated with income: for example, one component of $\mathbf{x}_i$ might indicate whether or not household i owns a color television.[5] Finally, household willingness to pay for a slot, v_i, is distributed exponentially with rate parameter $1/\eta_i$, where $0 < \underline{\eta} \leq \eta_i \leq \overline{\eta} < \infty$.[6]

Let $F(y, \mathbf{x}, \eta)$ be the joint distribution of these household attributes. We model variation in the elasticity of demand η_i in order to allow for unobservable heterogeneity when we turn to empirical applications in Section III, but we will abstract from it in presenting the main theoretical results in Sections IC–ID.

By treating household types $\mathbf{x}_i$ as exogenous we implicitly abstract from behavioral distortions introduced by the targeting rule. For example, if owning a television makes one ineligible then households have disincentives to buy televisions. This is an important simplification, since rules that are easier to enforce could also be more distortionary. They could also be less distortionary—for example, universal eligibility is both the least distortionary rule and the simplest. Either way, readers should interpret the exogeneity of types as an expositional assumption and keep this caveat in mind throughout.

The principal cannot observe income directly but must define a proxy means test in terms of more readily observable characteristics. Formally, a targeting rule is a subset $R \subseteq \mathbf{X}$, with the interpretation that household i is eligible if and only if $\mathbf{x}_i \in R$. The rule is implemented by an official who observes $(y_i, \mathbf{x}_i, \eta_i)$, though not the idiosyncratic valuation v_i. The official's payoff depends both on the allocation of slots and on his own net income Y. If $a_i \in \{0,1\}$ indicates whether household i obtains a slot then the official's payoff is

$$U(Y, \{a_i\}) = Y + \underline{\alpha}\int_{y_i=\underline{y}} a_i \, di + \overline{\alpha}\int_{y_i=\overline{y}} a_i \, di. \tag{2}$$

The parameters $(\underline{\alpha}, \overline{\alpha})$ measure the official's distributive preferences. An official with $\underline{\alpha} = \overline{\alpha} = 0$ simply maximizes his own income; this could be the case if the official were indifferent to distributional considerations or simply did not observe household incomes. High $\underline{\alpha}\,(\overline{\alpha})$, on the other hand, captures a strong preference for giving slots to the poor (rich). For example, an official motivated by electoral issues

[5] We use a binary indicator of poverty for simplicity, sidestepping issues of relative poverty measurement that are not central to the argument. This corresponds to the special case P_0 of the class of poverty measures defined by Foster, Greer, and Thorbecke (1984).

[6] In our application to BPL cards, demand heterogeneity may come from a number of sources. For example, some households value the commodity mix it provides more than others; some expect to actually receive more of their legal allotment than others; some are credit-constrained and thus unable to purchase the full allotment more often than others; some value the time they must spend waiting to collect their rations more than others, etc.

might place a high value on giving out slots to all voters (high $\underline{\alpha}$ and $\overline{\alpha}$). We can thus capture the uncertainty about the intrinsic and extrinsic motives of local officials that has dominated the debate over decentralizing welfare. As Jean Dreze and Amartya Sen put it,

> *"The leaders of a village community undoubtedly have a lot of information relevant for appropriate selection. But in addition to the informational issue, there is also the question as to whether the community leaders have strong enough motivation—or incentives—to give adequately preferential treatment to vulnerable groups. Much will undoubtedly depend on the nature and functioning of political institutions at the local level, and in particular on the power that the poor and the deprived have in the rural community."* (*Dreze and Sen* 1989, *quoted in Bardhan and Mookherjee* 2006c)

Given these preferences, the official may be tempted to break the targeting rule R. If he does break the rule with respect to household i he is detected and punished with probability $\pi(a_i, \mathbf{x}_i, R)$, which reflects the structure of monitoring and the likelihood with which rule breaking by the agent can be conclusively proved. We assume that rule abidance is never punished $\big(\pi(a, \mathbf{x}, R) = 0$ if $a = 1(\mathbf{x} \in R)\big)$, while rule violations are always punished with some positive probability $\big(\pi(a, \mathbf{x}, R) > 0$ if $a \neq 1(\mathbf{x} \in R)\big)$. Punishment consists of a (monetized) fine $f > 0$, which should be interpreted broadly to include career concerns, psychic costs, etc. The fine can be interpreted as an (inverse) measure of discretion: as $f \rightarrow 0$ the official can choose the allocation of slots freely, while as $f \rightarrow \infty$ adherence to the rules becomes paramount.

If the principal could make f arbitrarily large, then he could perfectly enforce any targeting rule. In practice, however, there are limits on how harshly corrupt officials can be punished. In part this reflects norms of proportionate punishment. In corruption-prone societies it also reflects limits on the size of the penalty that a supervisor or a court can be trusted to levy without themselves becoming vulnerable to subversion (Glaeser and Shleifer 2003). It is common in India for higher-ranked officials to intervene and protect lower-ranked officials from punishment for corruption. Ultimately, the effective strength of enforcement is an empirical question.

The official allocates slots by establishing a menu of type-specific prices $p(y_i, \mathbf{x}_i, \eta_i) \geq 0$.[7] We interpret prices broadly as including nonmonetary transfers: for example, an official might give a slot to a friend in anticipation of having this favor returned. Note also that "pricing" slots is consistent with rule abidance, as the official could set the price equal to 0 for eligible households and $+\infty$ for ineligible ones. His problem is

$$(3) \quad \max_{\{p_i\}} \int (1 - G(p_i \mid \eta_i))[p_i - c(y_i, \mathbf{x}_i)]\, dF(y_i, \mathbf{x}_i, \eta_i) \text{ such that } p_i \geq 0 \,\forall\, i,$$

[7] We do not model the creation of additional rules by the official as a screening device (Banerjee 1997).

where $G(\cdot \mid \eta)$ is the exponential CDF with rate parameter $1/\eta$ and the implicit marginal cost $c(y_i, \mathbf{x}_i)$ of providing a slot is

$$(4) \quad c(y_i, \mathbf{x}_i) \equiv f[\pi(1, \mathbf{x}_i, R) - \pi(0, \mathbf{x}_i, R)] - \underline{\alpha} 1(y_i = \underline{y}) - \overline{\alpha} 1(y_i = \overline{y}).$$

This cost consists of two components. First, allocating a slot to household i may either increase or decrease expected penalties, depending on whether or not $\mathbf{x}_i \in R$. Second, implicit costs are lower to the extent that the official directly values allocating slots to households with income level y_i.

Pointwise maximization of (3) yields the monopolist's markup equation[8]

$$(5) \quad p^*(y_i, \mathbf{x}_i, \eta_i) = \max\{0, c(y_i, \mathbf{x}_i) + \eta_i\}.$$

From this it follows directly that the probability household i obtains a slot is

$$(6) \quad \mathbf{P}(a_i = 1 \mid \mathbf{x}_i, y_i, \eta_i) = 1 - G\big(\max\{0, c(y_i, \mathbf{x}_i) + \eta_i\} \mid \eta_i\big).$$

Prices increase in income y_i (conditional on η_i, and $\mathbf{x}_i \in R$,) if and only if the official has progressive preferences ($\underline{\alpha} > \overline{\alpha}$). Similarly prices weakly decrease in eligibility $1(\mathbf{x}_i \in R)$ (conditional on η_i, and y_i,) and strictly decrease if and only if penalties are positive ($f > 0$). Since household-level demand is decreasing in price, conditional on η_i, corresponding opposite results on quantities follow directly. The targeting rule R thus influences the final allocation of slots indirectly by determining the official's willingness to accept payment from each household. For example, giving a slot to an ineligible household is potentially costly and the official must obtain a larger bribe to be willing to do so. Exactly how much more depends on the details of enforcement summarized by π; two ineligible households may face different prices if one is "riskier" from the official's point of view. Of course, if the official has strong incentives to give slots to everyone ($\underline{\alpha}, \overline{\alpha} \gg 0$) then rule violations will be widespread but bribes low.

Note also that for f sufficiently large all eligible households receive slots (at price 0), and as $f \to \infty$ the number of ineligible households that receive slots approaches 0.[9] This reflects the fact that there are no deep informational constraints in the model: since any particular rule violation is punished with some positive probability, the principal could obtain arbitrarily close compliance if arbitrarily harsh punishments were available (Mirrlees 1999; Becker 1968).

[8] Substituting household i's price elasticity of demand p_i/η_i into the familiar markup equation $(p - c)/p = -1/\epsilon$ and imposing $p_i \geq 0$ yields (5).

[9] The latter is a limit result because of the simplifying assumption that the demand shocks v_i are unbounded. If the v_i were bounded above then there would be some finite f that eliminates inclusion errors.

C. *The Principal*

The principal has progressive preferences: he values a unit of surplus transferred to a poor (rich) household at $\underline{\omega}\,(\overline{\omega})$ with $\underline{\omega} > \overline{\omega}$. We normalize the cost of providing a slot to either type of household to 1. We also fix $\eta_i = \eta$ for the rest of this section; we will reintroduce heterogeneous demand elasticities in our empirical application. The interesting case is that in which $\underline{\omega} > 1/\eta > \overline{\omega}$, so that the principal's expected return from giving a slot to a poor (rich) household is positive (negative). The principal's payoff as a function of the price schedule $\{p_i\}$ charged to households is

$$V(\{p_i\}) = \int_{y_i=\underline{y}} 1(v_i > p_i)\big(\underline{\omega}(v_i - p_i) - 1\big)\, dF(y_i, \mathbf{x}_i, \eta_i) + \int_{y_i=\overline{y}} 1(v_i > p_i)\big(\overline{\omega}(v_i - p_i) - 1\big)\, dF(y_i, \mathbf{x}_i, \eta_i). \tag{7}$$

By exploiting properties of the exponential distribution we can write this as

$$V(\{p_i\}) = (\underline{\omega}\eta - 1)\int_{y_i=\underline{y}} \exp\left\{-\frac{p_i}{\eta}\right\} dF(y_i, \mathbf{x}_i) + (\overline{\omega}\eta - 1)\int_{y_i=\overline{y}} \exp\left\{-\frac{p_i}{\eta}\right\} dF(y_i, \mathbf{x}_i). \tag{8}$$

This can be interpreted as a loss function parametrized by the cost $\underline{\omega}\eta - 1 > 0$ of excluding a poor household and the cost $1 - \overline{\omega}\eta > 0$ of including a rich one. Note that the loss function depends both on how well targeted benefits are (the proportion that go to the poor) and also on the overall scale of benefit provision. (Ravallion 2009).

The existing literature has studied the case where the agent is completely honest, or $p_i = 0$ for all eligible households and $p_i = +\infty$ for all ineligible ones. In that case the principal's problem is

$$\max_{R \in \mathcal{P}(\mathbf{X})} (\underline{\omega}\eta - 1)\int_{y_i=\underline{y}} 1(\mathbf{x}_i \in R)\, dF(y_i, \mathbf{x}_i) + (\overline{\omega}\eta - 1)\int_{y_i=\overline{y}} 1(\mathbf{x}_i \in R)\, dF(y_i, \mathbf{x}_i). \tag{9}$$

Here the analogy to statistical decision theory is exact. In this case the costs and benefits of adding indicators to a targeting rule are well understood: "more information is generally better than less, though there are diminishing returns" (Grosh and Baker 1995, ix), while "the beauty of using just a few indicators is that administrative costs are kept low" (Besley and Kanbur 1990, 13). When the principal *cannot* rely on the agent to behave honestly, however, he must take into account the more complex reactions of the agent's optimal price schedule $\{p_i^*\}$ to the choice of targeting rule. We wish to understand whether and how this affects the value of targeting on more indicators.

D. *Rule Design: When Is More Information Better?*

We begin our analysis of these issues by providing conditions under which agency constraints do *not* affect the rule design problem. These will form the basis of our diagnostic empirical work below.

PROPOSITION 1: *Let R^* be statistically optimal in the sense that it solves* (9). *Then:*

(i) *As $f \to \infty$ the payoff from R^* approaches the constrained optimal payoff.*

(ii) *As $\underline{\alpha} \to \infty$ while $\overline{\alpha} \to -\infty$ the payoff from R^* approaches the constrained optimal payoff.*

(iii) *If $\underline{\alpha} = \overline{\alpha}$ and there exists $\tilde{\pi}$ such that $\pi(a_i, \mathbf{x}_i, R) = \tilde{\pi} \cdot 1(a_i \neq 1(\mathbf{x}_i \in R))$ then rule R^* yields at least as high a payoff as any other nontrivial rule.*

PROOF:

All proofs are in Appendix A.

The first part of this proposition simply says that, as one would expect, when enforcement is sufficiently strong the principal cannot do better than use the statistically optimal rule. The second says that when the agent's preferences are closely aligned with the principal's then, again, the principal can do no better than use the statistically optimal rule; indeed, the agent will "overrule" any attempt to impose a less accurate one.

The third part of the proposition is the most interesting, as it establishes a link between the technology of enforcement and the rule design problem. The condition $\pi(a_i, \mathbf{x}_i, R) = \tilde{\pi} \cdot 1(a_i \neq 1(\mathbf{x}_i \in R))$ for all R describes an environment in which the principal's ability to verify a household's (in)eligibility does not depend on the nature of the eligibility rule. This would hold if, for example, the principal audited a fraction of households and these audits verified all of a household's characteristics $\mathbf{x}_i$. In this case the official's probability of punishment depends only on whether a rule has been broken, and not by "how much." This feature shuts off enforcement effects: changing one household's eligibility status has no effect on the likelihood that any other household gets a slot, and so targeting is again a purely statistical exercise.

In Section IA we outlined what might happen when these conditions fail, and conjectured that this might make targeting relatively unattractive compared to universal eligibility. We can now make this argument precise. In that example the set of household types was "rich or poor" $(\mathbf{X} = \{\underline{y}, \overline{y}\})$ and the relevant targeting options were simply targeting and universal eligibility. To pin down the agent's preferences, let $\underline{\alpha} = \overline{\alpha} = 0$ so that the agent cares only about profit. To pin down enforcement, let the probability that the principal verifies a household's existence be π_e (possibly equal to 1) while the probability that he verifies a household's type be $\pi_t \leq \pi_e$. Substituting the pricing equation (5) into the principal's value function (8) we can write the principal's gain from targeting as opposed to universal eligibility as proportional to[10]

$$(10) \qquad [\exp\{f\pi_e/\eta\} - \exp\{-f\pi_t/\eta\}](1 - \overline{\omega}\eta) - [\exp\{f\pi_e/\eta\} - \exp\{f\pi_t/\eta\}](\underline{\omega}\eta - 1).$$

As expected the first term is positive: targeting lowers the probability that a rich household obtains a slot. If verifying household types is as easy as verifying their existence ($\pi_t = \pi_e$), then the second term vanishes. If it is harder ($\pi_t < \pi_e$), however, then targeting decreases the probability that poor households obtain slots, even though they remain eligible. If exclusion errors are sufficiently costly relative to inclusion errors ($\underline{\omega}$ is large relative to $\overline{\omega}$), then the constrained optimal policy will be universal eligibility—even though in this example targeting is always optimal under perfect enforcement.

We can also examine this tradeoff in the context of incremental changes to a given rule. To illustrate this, let the space of household types be a product space $\mathbf{X} = X^1 \times X^2$ of two household asset holdings, which we will call "land" and "jewelry." Recycling notation, let F be the joint distribution of x_i^1 and x_i^2, F_1 and F_2 the marginal distributions, and F_{12} the distribution of the sum $x_i^1 + x_i^2$. We assume that $\mathbb{P}(x^1 + x^2 \leq k \,|\, x^1 = x)$ is strictly decreasing in x for any k; this holds if x^1 and x^2 are independently distributed, for example, but rules out very strong negative correlations. The principal considers as poor agents whose total assets $x_i^1 + x_i^2$ fall below some threshold y^*. The statistically optimal rule is therefore

$$(11) \qquad R_{12} \equiv \{\mathbf{x} : x^1 + x^2 \leq y^*\},$$

which achieves perfect targeting in the absence of agency concerns. Among rules that condition only on x^1, a natural candidate is

$$(12) \qquad R_1 \equiv \{\mathbf{x} : x^1 \leq x^{1*}\}$$

for some threshold value x^{1*}, which makes eligible all households with sufficiently low land holdings.[11] Note that both R_{12} and R_1 are examples of "scoring" rules, i.e., can be written as $R = \{x : \sum_{n=1}^{N} h_n(x^n) < 0\}$ for some collection of functions $\{h_n\}$.

[10] The mapping between this expression and the notation in equation (1) is as follows: $b = \omega\eta - 1, c = 1 - \overline{\omega}\eta$, $q_U \propto \exp\{f\pi_e/\eta\}$, $q_E \propto \exp\{f\pi_t/\eta\}$, and $q_I \propto \exp\{-f\pi_t/\eta\}$.

[11] Ravallion (1989) and Ravallion and Sen (1994) study land-based targeting.

Scoring rules are widely used in practice and include the BPL rule we study below. The analysis that follows generalizes immediately to other linear scoring rules.[12]

Interestingly, the optimal land threshold turns out to be the same regardless of how effective enforcement is:

LEMMA 1: *Fix any* $\phi_1 > 0$ *and let* x^{1^*} *satisfy*

$$\mathbb{P}(x^1 + x^2 \leq y^* | x^1 = x^{1^*})\underline{\omega} + \left(1 - \mathbb{P}(x^1 + x^2 \leq y^* | x^1 = x^{1^*})\right)\overline{\omega} = 1/\eta,$$

or $x^{1^*} = 0$ *if that equation has no solution. Then the rule* R_1 *defined by threshold* x^{1^*} *is uniquely optimal within the class of rules that condition only on* x^1.

In other words, the expected welfare gain from giving a slot to a marginal household should just equal the cost of the slot. This is obvious in the perfect-enforcement case; the more interesting point is that it continues to hold with imperfect enforcement.

Now consider an agent who cares only about maximizing profits ($\underline{\alpha} = \overline{\alpha} = 0$). To parameterize enforcement, suppose that the principal observes the value of characteristic $j \in \{1,2\}$ for household i with independent probability ϕ_j.[13] If the principal observes enough to determine that the household has been incorrectly classified then he fines the agent f.[14]

We can now formalize the idea that conditioning on more indicators may yield strictly worse results if they are hard to verify:

PROPOSITION 2: *Given a fixed rule* R *that conditions nontrivially on* x^2, *there exists* $\phi_2^*(R) > 0$ *such that if* $\phi_2 < \phi_2^*(R)$ *then rule* R_1 *yields a strictly higher payoff than* R.

The intuition for this result rests on the same tradeoff between statistical accuracy and enforceability, but because the type space $X_1 \times X_2$ is larger there are more effects to keep track of. Figure 1 summarizes these effects. It plots the type space partitioned into regions defined by the two candidate targeting rules. Solid lines separate the households that are eligible and ineligible under the two rules; dotted lines separate households whose eligibility is the same but who face different equilibrium prices. These prices are determined by the difference in the official's probability of punishment induced by giving a household a slot $\left(\pi(1, \mathbf{x}_i, R) - \pi(0, \mathbf{x}_i, R)\right)$; this difference is positive for eligible households and negative for ineligible ones. The figure plots this difference in each region of the graph, first for rule R_{12} and then for rule R_1.

The tradeoff between statistical accuracy and enforceability boils down to a comparison between two groups of regions. In one group—regions A, D, and

[12] One can see this using a change of variables argument: if the principal considered as poor households for whom $\beta_1 x_i^1 + \beta_2 x_i^2 < y^*$, we can introduce new variables $\tilde{x}_i^n = x_i^n / \beta_n$ and continue as before.

[13] The analysis extends to the case where these events are not perfectly independent, at the cost of notational clutter.

[14] We focus in this example on top-down enforcement. If households that were illegally excluded could complain, then the probabilities of detecting inclusion and exclusion errors would of course be asymmetric.

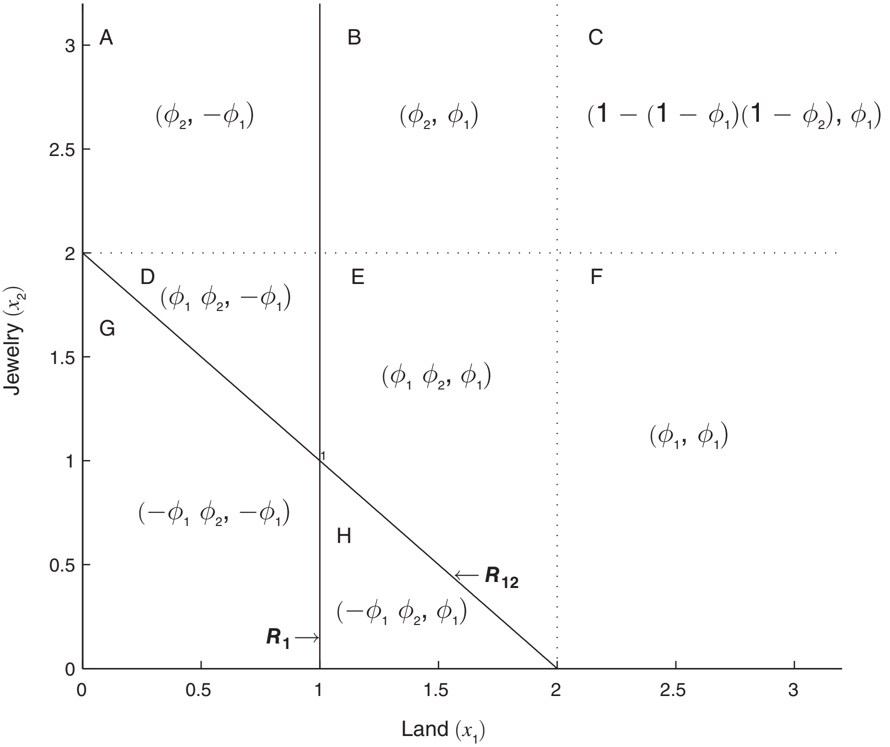

Figure 1. Targeting on Two Asset Measures

Notes: Plots the household type space, with landholdings on the x-axis and jewelry holdings on the y-axis. The space is partitioned into regions defined by the targeting rules R_{12} and R_1. The value of the enforcement effect $\pi(1, \mathbf{x}_i, R) - \pi(0, \mathbf{x}_i, R)$ is displayed within each region, first for rule R_{12} and then for rule R_1.

H—the statistically optimal rule R_{12} does unambiguously better than the simpler rule R_1 because it correctly defines poor households as eligible and rich households as ineligible. For example, prices are higher for the (rich) households in region A under R_{12} than under R_1. In a second group—regions B, C, E, and G—the two rules agree on eligibility but are differentially enforceable. For example, to verify the eligibility of poor households in region G the principal must observe land holdings under R_1 but *both* land holdings and jewelry holdings under R_{12}. In general, the two rules cannot be ranked in terms of enforceability—the simpler rule R_1 is easier to enforce in region G but harder to enforce in region C, for example. As jewelry holdings become hard to verify $(\phi_2 \to 0)$, however, R_1 is as or more enforceable than R_{12} in every region. This fact drives Proposition 2.[15]

To understand the result it may be helpful to contrast it with more familiar intuitions about multitasking. The issue here is not how strong to make incentives: the

[15] It is a corollary that within any finite set of alternative rules there exists a bound ϕ_2^* below which R_1 is optimal. There does not exist a uniform bound ϕ_2^* below which R_1 performs better than *any* rule for the technical reason that one can construct infinite sequences of rules $\{R^t\}$ that approximate R_1 arbitrarily, so that the principal's payoff does not converge uniformly.

agent is risk-neutral, a perfect performance measure R_{12} is available, and conditional on using it the principal would like to make incentives as strong as possible. The issue is rather that when the strength of incentives is constrained (f is bounded) the optimal choice of a performance measure depends both on how well correlated it is with the principal's objective function (statistical accuracy) and also how responsive it is to the agent's "effort" (enforceability).

Similar logic shows why allowing penalty levels to vary with the nature of the rule violation would not affect the result. To see this, suppose the principal can define variable fines $f(a_i, \mathbf{x}_i, R)$ that depend on the nature of the rule violation. For a rule like R_{12} that perfectly targets the poor it will always be optimal to enforce as aggressively as possible, i.e., set fines at the upper bound for any violation. Reinterpreting the fixed f in this example as the upper bound, we can interpret the calculated performance of R_{12} as the best it can ever do, while the calculated performance of R_1 is a *lower* bound on how well it can do after possibly re-optimizing fines.[16]

E. *The Costs and Benefits of Delegation*

Stronger enforcement always helps the principal if he has a statistically perfect targeting rule available, but it is less clear whether this holds more generally. Intuitively, an official with progressive preferences may bend the rules precisely in order to improve on them, and the principal might not wish to discourage this kind of behavior. Our final result formalizes this intuition:

PROPOSITION 3: *Let the probability of detecting a violation be constant* $(\pi(a_i, \mathbf{x}_i, R) = \pi > 0$ *whenever* $a_i \neq 1\,(\mathbf{x}_i \in R))$. *If R perfectly targets the poor then* $\partial V/\partial f \geq 0$. *If R does not perfectly target the poor, so that there are some ineligible poor and some eligible rich, then there exist a scalar f^* and functions $\underline{\alpha}^*(f)$ and $\overline{\alpha}^*(f)$ such that if $f > f^*$, $\underline{\alpha} > \underline{\alpha}^*(f)$, and $\overline{\alpha} < \overline{\alpha}^*(f)$ then $\partial V/\partial f < 0$.*

The mechanics of this result are that, for large fines and a highly progressive official, almost all the eligible poor and almost none of the ineligible rich will have slots. The marginal effects of increased fines f then become concentrated among the ineligible poor and the eligible rich. Because these groups are statistically mistargeted by the rule, stronger enforcement of the rule among them has negative effects on the principal's payoff. This logic mirrors that in multitasking agency theories: a targeting rule R that imperfectly targets the poor is an imperfect measure of performance, and attaching strong incentives to such a measure may distort the agent's behavior away from productive actions he would otherwise have taken (Holmstrom and Milgrom 1991; Baker 1992).

The notion that weak enforcement could be optimal may seem counterintuitive given that weak enforcement is often cited as a key governance challenge in developing countries. Yet one can think of weak enforcement as simply a less extreme version

[16] Note also that nothing above contradicts the revelation principle, which states that the set of optimal mechanisms includes one in which the agent faithfully reports all his information but does not imply that transfers in an optimal mechanism are sensitive to every facet of this report.

of outright decentralization, which can be advantageous in some circumstances (Bardhan 2002; Bardhan and Mookherjee 2005). Whether or not weak enforcement yields better or worse targeting is thus an unresolved empirical question.

II. Empirical Context and Data Collection

A. *Targeting India's Poor*

India's BPL system has become the focal point of a long-standing debate over how to best target the poor. Prior to 1997 India operated a universal Public Distribution System (PDS) intended to provide basic commodities to all Indian households at subsidized prices. To accomplish this the government created a vast system of procurement and distribution. The Food Council of India purchased grain from farmers and stored it at government-owned warehouses; subsequently, these commodities were allocated to each state based on prior years' consumption levels and distributed through a system of about 400,000 Fair Price Shops (FPS), each one servicing several villages. At the FPS, households purchased rice, wheat, sugar, and kerosene at uniform prices below those on the open market.

In 1997 the government judged the PDS too costly to support and introduced poverty targeting. Under the Targeted Public Distribution System, all households in India are classified as being below-poverty-line (BPL) or not. Each BPL household is entitled to defined quantities of basic commodities at subsidized prices typically equal to about half of what it costs the government to purchase and distribute them. In contrast, above-poverty-line (APL) households pay prices approximately equal to the government cost, which are also very close to market prices. The Indian Planning Commission estimated that in 2001 the effective annual subsidy to BPL card holders in Karnataka from grain purchases was Rs 294 (Programme Evaluation Organization 2005). Many other social programs are also now targeted to BPL households—for example, the cards give access to advantageous loans for agricultural activities, education scholarships, medical benefits, housing schemes, and distributions of bicycles, books, clothes, soap, salt, oil, and tea.

Identifying BPL households has thus become a central task for welfare policy in India. The central government conducts surveys approximately every five years to identify the number of households it thinks are BPL in each state and then allocates funding for social programs in proportion to these numbers. The states can use their own criteria to actually allocate BPL cards, however (and the states usually estimate their poverty counts to be much higher than the central government's figures). Dreze and Khera (2010) estimate that 33 percent to 34 percent of Indian households held BPL cards as of 2005.

Our empirical work is set in Karnataka, where the most recent round of BPL surveys was held in 2007. A household was legally eligible if it did *not* have any of the following:

- Annual income more than Rs 17,000 in urban areas or Rs 12,000 in rural areas.
- A telephone (land line or mobile).
- A two-, three-, or four-wheeler (e.g., motorcycle, auto-rickshaw, or car).

- A gas connection.
- A color TV.
- More than 5 acres of dry land.
- A water pump set.
- A household member who is a salaried government employee.

Note that this rule is a special case of the widely used class of "scoring" rules—in this case households must receive a score of zero to be eligible. It is also interesting that while some of the eligibility criteria seem plausibly verifiable (e.g., land holdings or status as a government employee), others appear harder to prove.

The actual process of allocating BPL cards begins with a state-mandated survey to determine which households are eligible for BPL cards. Surveys are conducted by government officials at the level of the Gram Panchayat (GP), a collection of several villages. The official in charge was usually the village accountant, but may also in some cases have been the GP Secretary, Anganwadi (health) worker, or a local school teacher. Regardless of the exact identity of the official, he or she would typically have both "hard" and "soft" information about the poverty and other characteristics of households in the Panchayat.[17]

The legally mandated process for ascertaining BPL eligibility involved several additional steps. After the initial government survey was completed, the BPL eligibility list for each village was compiled at the taluk (subdistrict) level. The compiled lists were then remitted to the corresponding GP's to verify that households disclosed asset ownership and wealth truthfully to the initial government survey team. GP officials were supposed to organize a meeting of all registered voters (a "Gram Sabha") in order to read aloud the eligibility list, give the community a chance to dispute any categorization, and resolve such disputes on the spot. Finally, the revised list should have been posted at a well-known place in each village for several days before being finalized and remitted to the taluk, which then proceeded to issue BPL cards. In most of Karnataka, temporary ration cards were issued in 2007 and households were in principle allowed an additional opportunity to appeal their eligibility status in the period before permanent ration cards were issued in 2008. Finally, in addition to these "bottom-up" checks the state government sent out teams to resurvey a sample of households and check that the targeting rules were correctly implemented.

B. *Cross-Checking BPL Allocations*

Because BPL cards are valuable, officials had incentives to break both targeting rules and process rules. To understand how the BPL allocation works in practice we therefore need independent data on household characteristics and BPL status. We collected such data as part of a quality of life survey in Karnataka in early 2008. We constructed our sample in two stages. First we selected villages; in most districts we drew a proportional random sample of villages, while in Raichur we

[17] Note that de jure households do not need to apply for a BPL card. In programs with an application requirement targeting depends both on administrative decisions and on household's self-selection into the applicant pool (Coady and Parker 2009; Baird, McIntosh, and Özler 2009).

sampled from among villages that had been part of an earlier experiment.[18] We then randomly selected 21 households from each village, sampling from the state governments list of all households that had been identified in the BPL survey. Our surveyors were not always able to complete interviews with all of the 21 assigned households, either because the household had migrated or because no one was at home during the day; in these cases we randomly selected replacement households for them to interview. In the event that both the originally sampled household and the backup could not be interviewed, fewer than 21 households were surveyed. In total we surveyed 14,074 households, or an average of 17 households per village.

The primary objective of the survey was to obtain independent measures of both BPL eligibility and BPL card ownership in order to measure the extent of misclassification. We structured the survey instrument carefully to encourage veracity. Questions about the BPL eligibility criteria were posed early in the survey along with other similar quality of life questions, and the surveyors did not refer to them as eligibility criteria. Questions about card ownership and other politically sensitive questions were located at the end of the survey to avoid influencing responses to the questions about eligibility criteria.[19] While every effort was made to ensure accuracy, our data will inevitably contain some measurement error, particularly for hard-to-measure items like income. We discuss below how this affects the interpretation of each of our results.

In addition to our core data on BPL eligibility and card ownership, we also collected information on the process through which cards were allocated and on respondent's understanding of the allocation rules. We were particularly interested in understanding the prices households paid for BPL cards. The state of Karnataka fixed the fee for issuing a BPL card at Rs 5, but given the discretionary power local officials have we expected to see higher prices charged in practice. We therefore asked households both about the "official fee" necessary to obtain a card and also about any "extra fee" they were charged. Responses to the later question may need to be interpreted with care, but as respondents' anonymity was assured they had no reason to be concerned about faithfully reporting the prices they faced.

Table 1 presents basic descriptive information about the households in our survey. The majority are illiterate (61 percent) and Hindu (95 percent), and a large minority come from a scheduled caste or tribe (40 percent).

III. BPL Targeting in Practice

A. *How Are BPL Cards Allocated?*

Households generally report low adherence to the statutory allocation procedures. Fifty percent of respondents remembered being surveyed by someone to

[18] The experiment involved providing a random subsample of villages with information about the BPL eligibility criteria. Sadly this treatment was found to have no effect on any measured outcome. We include village fixed effects in all our regression specifications, so any unnoticed effects of the experiment should not influence our results. Results are also qualitatively similar if we simply exclude the experimental villages (22 percent of sampled villages).

[19] In addition to regular BPL cards there are variants (Antyodaya Anna Yojane and Annapurna cards) for households that are not only below the poverty line but also disadvantaged in other ways, e.g., widowed or elderly. We collected data on each type of card but treat them symmetrically in the following analysis.

TABLE 1—BASIC HOUSEHOLD CHARACTERISTICS

Variable	Percent	Observations
Religion		13,717
Other	5%	
Hindu	95%	
Caste		13,601
Scheduled caste	26%	
Scheduled tribe	14%	
General	60%	
Household head marital status		13,361
Married	81%	
Never married	1%	
Widowed	18%	
Divorced	0%	
Household head education		13,357
Illiterate	61%	
Less than primary	5%	
Primary	10%	
Middle	13%	
Matriculate	7%	
Intermediate	2%	
BA/BSc	1%	
MA/MSc	0%	
Professional degree	0%	
Household head gender		13,381
Male	83%	
Female	17%	

determine eligibility. Thirteen percent of respondents were aware of a Gram Sabha meeting held to discuss BPL eligibility; 25 percent remembered at least one Gram Sabha meeting held in the last two years but said it did not cover BPL eligibility, and the remaining 62 percent did not recall any Gram Sabha meeting having been held in the past two years. Conditional on being among the 13 percent of respondents who did recall a Gram Sabha held to discuss BPL eligibility, only 16 percent said that families had an opportunity to object to their eligibility status at this meeting. Finally, only 2 percent of respondents said that a list of eligibility assignments was posted somewhere in the village. Of course, some respondents may simply have forgotten events that actually did take place. We cannot rule this out conclusively, though given the salience of the BPL process—which takes place once every five years—we suspect that it is not the entire explanation.

We also asked respondents about their familiarity with the eligibility criteria. The top panel of Table 2 gives the percentages of respondents that correctly answered the question “Is a family eligible for a BPL card if it has X” for various criteria. Not all of the criteria are actual eligibility criteria; several are placebos. Accuracy rates vary from 19 percent to 77 percent and the (unweighted) average accuracy rate across the criteria is 50 percent, exactly what respondents would have achieved by random guessing. Overall only 35 percent of respondents described themselves as familiar with the eligibility rules, and only 17 percent reported that they knew what to do if they disagreed with the way they were categorized. Overall, awareness of the eligibility criteria is low.

Table 2—Households Are Unfamiliar with Eligibility Rules

Is a family eligible if it has...	Correct answer	Percent correct
Water pump	No	57%
Jewelry worth more than Rs 8,000	Yes	38%
Motorized vehicle	No	41%
Bicycle	Yes	73%
Member with monthly salary over Rs 1,000	No	41%
Electricity	Yes	77%
More than 3 hectares of dry land	No	35%
Black and white TV	Yes	65%
Telephone	No	19%
Are you...		Percent agreeing
Familiar with the eligibility criteria?		35%
Aware how to object?		17%

Notes: The first ten rows report the percentage of households that correctly identified whether or not the given condition makes a household legally ineligible for a BPL card. The latter rows report the percentage of households that agreed with the given statements.

B. *How Effective is Enforcement?*

If enforcement is sufficiently strong then statistically optimal rules are also constrained optimal (Proposition 1, part 1). To test this condition we turn next to data on the actual allocation of BPL cards. Table 3 cross tabulates our measure of BPL eligibility and actual BPL card possession. The data suggest that rule breaking is widespread. 13 percent of households legally eligible for a BPL card do not have one, and 70 percent of households ineligible for a card have one nevertheless. In total we estimate that 48 percent of the households in our sample are misclassified.

To the extent that our data on asset holdings contain measurement error they may overestimate the extent of misclassification. One reason to think this is not the whole story is that most of the violations we detect are inclusion errors; for these to be the result of measurement error, it would have to be the case that our survey teams mistakenly recorded that households possessed assets they did not (as opposed to simply missing assets that they held). We can also construct more conservative estimates of inclusion error based on subsets of the criteria that are easier to measure. If we ignore income, we estimate that 53 percent of households are ineligible and that of these 68 percent have BPL cards. If we focus solely on the five criteria that are arguably least likely to be mismeasured—ownership of a vehicle, a phone, a gas connection, a color TV, or a water pump—we still estimate that 43 percent of households are ineligible and that 66 percent of these hold BPL cards. Finally, our figures—while stark—are consistent with the findings of smaller-scale studies of BPL allocations in other states.[20]

[20]For six villages in Gujarat following the 1997 BPL Census, Hirway (2003) estimates that 10 percent to 15 percent of eligible households did not receive cards, while 25 to 35 percent of ineligible households received cards. For eight villages in Rajasthan following the 2002 BPL Census, Khera (2008) estimates that 44 percent of eligible households did not receive BPL cards while 23 percent of ineligible households did receive them. Ram, Mohanty, and Ram (2009) report high rates of violations of individual eligibility criteria in the India-wide National Family and Health Survey-3. In a distinct setting, Camacho and Conover (2011) document suspicious patterns in official records for Colombia's poverty-targeting scheme SISBEN which are consistent with manipulation, though they do not directly measure rule violations.

TABLE 3—OFFICIAL RULES ARE FREQUENTLY VIOLATED

	Ineligible	Eligible	Total
No BPL card	2,560	652	3,212
	(30%)	(13%)	(24%)
Has BPL card	5,862	4,419	10,281
	(70%)	(87%)	(76%)
Total	8,422	5,071	13,493

Note: Column percentages in parentheses, e.g., 70 percent of ineligible households have BPL cards.

Another way to assess the strength of enforcement is to examine the frequency of bribery. If enforcement were perfect, no households would pay bribes—eligible households would receive cards for free, while inelible households would be unable to obtain cards at any price. A large proportion of households in our survey—73 percent of all households and 93 percent of BPL card recipients—reported the price they faced to obtain a card. We define the total price as the sum of the reported "official fee" and any "extra" fee reported (7 percent of households reported an "extra" fee). Among those who reported a price, 75 percent reported one above the statutory maximum fee of Rs 5 (0.2 percent reported one below Rs 5) with a maximum bribe of Rs 305. The mean bribe is small, however, at Rs 9, or Rs 14 conditional on being positive.[21] This suggests nonmonetary factors play an important role in the allocation of BPL cards—officials may trade benefits for votes, for example.

We can examine pricing and allocation together using the corresponding equations implied by our model. Letting h index households and v index villages, we can write the pricing equation (5) as

$$p_{hv} = f[\pi(1, \mathbf{x}_{hv}, R) - \pi(0, \mathbf{x}_{hv}, R)] + (\underline{\alpha} - \overline{\alpha})1(y_{hv} = \overline{y}) - \underline{\alpha} + \eta_{hv} \tag{13}$$

whenever prices are positive for household h in village v.[22] Note that the strength of enforcement f appears here multiplied by the term $[\pi(1, \mathbf{x}_{hv}, R) - \pi(0, \mathbf{x}_{hv}, R)]$, which is increasing in eligibility. This says that the partial correlation between eligibility and prices (or quantities) can be interpreted as a measure of the strength of enforcement.

To estimate this relationship we adapt the model to our data as follows. First, since we observe a continuous measure of household income y_{hv} we replace the indicator $1(y_{hv} = \overline{y})$ with $\log y_{hv}$. Second, we know little about the enforcement term $[\pi(1, \mathbf{x}_{hv}, R) - \pi(0, \mathbf{x}_{hv}, R)]$ except that it should be negative for eligible households

[21] These figures are small both in absolute terms and relative to our best estimates of the benefits of holding a BPL card. Using self-reported data on commodities purchased at the Fair Price Shop, prices paid, and corresponding market prices, we estimate that the mean BPL household in our sample receives an implied subsidy of Rs 201 per month while the mean APL household receives Rs 67, which puts the implied value of a BPL card at Rs 133 per month. Inequality between the bribe price and the value of a good is a common feature of illicit markets and sometimes called the "Tullock paradox." See Bardhan (1997).

[22] We report OLS estimates that ignore the fact that self-reported prices are left-censored at Rs 5, the true statutory fee. Tobit estimators that account for this suffer from an incidental parameters problem and are only consistent as the number of observations per village grows. Nevertheless we did estimate Tobit models and obtained estimated coefficients similar to and slightly larger than those reported below.

and positive for ineligible ones; we therefore experiment with functions $h(\cdot)$ of the eligibility criteria including a simple eligibility indicator and the number of criteria violated.[23] Third, we augment the model with village fixed-effects λ_v, which absorb institutional variation across villages and isolate variation in decision making by the same officials within villages.[24] This yields

$$p_{hv} = fh(\mathbf{x}_{hv}) + (\underline{\alpha} - \overline{\alpha})\log y_{hv} + \lambda_v + \eta_{hv}. \tag{14}$$

Intuitively, this equation says that bribe prices should be driven by eligibility if the official perceives rule breaking as costly ($f > 0$) and by income if the official has redistributive preferences ($\underline{\alpha} > \overline{\alpha}$). Analogous opposite results follow for the probability that household *hv* holds a BPL card.[25]

Table 4 presents estimates of equation (14) and analogous linear probability models for BPL card ownership. Panel A focuses on reported prices; panels B and C focuses on BPL status, with panel B restricting the estimation sample to households that reported prices for comparability with panel A. We focus for now on column 1, which simply relates prices and quantities to eligibility. Consistent with the model, ineligible households pay significantly higher prices for BPL cards. The point estimate is small, however: ineligible households pay Rs 3 more on average, which suggests that while officials are cognizant of the costs of breaking the rules they perceive these as being small. Classical measurement error in our eligibility variable could be part of the explanation for this small point estimate. Note, however, that given a *maximum* reported overpayment of Rs 305 the range of the dependent variable is itself inconsistent with large eligibility effects. Effects on quantities are also small, with ineligible households 1 percent less likely to hold BPL cards. Estimates for the full sample are larger, with ineligible households 21 percent less likely to hold cards, but still much smaller than the 100 percent difference that would obtain under perfect enforcement.

C. *How Progressive Are Officials' Preferences?*

Even in a weakly-enforced environment, there may be little downside to using statistically optimal rules if officials themselves have progressive preferences (Proposition 1, part 2). In this case officials would simply target the poor using their own, soft information. For example, an official might give a BPL card to a household with a water pump that was once well-off but had recently fallen on hard times.

[23] We also estimated a variety of models in which specific violations and combinations of violations were allowed to have distinct effects. The conclusions we report below were robust to these variations (available on request).

[24] An additional motivation for including village fixed effects is that, anecdotally, some villages in Karnataka were told that they should not allocate BPL cards to more than 60 percent of households. One can show that if the official faces a binding quantity constraint then optimal pricing is the same as in equation (5) except that prices are augmented by the Lagrange multiplier λ on the quantity constraint, which would vary by village.

[25] This approach can be seen as a micro-founded analogue to the reduced-form specifications of Alderman (2002) who regresses welfare receipts on household expenditure while conditioning on a set of more readily observable attributes that could in principle have been included in a PMT.

Table 4—Eligibility, Income, Prices, and Allocations

Regressor	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Prices							
Ineligible	2.935		0.868	2.002			
	(0.509)***		(0.668)	(0.477)***			
No. violations		1.277	1.059		1.029	1.281	1.009
		(0.253)***	(0.343)***		(0.298)***	(0.255)***	(0.296)***
No. placebo violations						−0.222	−0.35
						(0.343)	(0.366)
Log annual income				1.564	0.946		1.05
				(0.507)***	(0.577)		(0.611)*
Observations	9,608	9,608	9,608	9,608	9,608	9,608	9,608
R^2	0.007	0.011	0.011	0.01	0.012	0.011	0.012
Panel B. Quantities							
Ineligible	−0.013		0.003	−0.006			−0.005
	(0.003)***		(0.005)	(0.003)*			(0.005)
No. violations		−0.008	−0.009		−0.006	−0.008	−0.006
		(0.002)***	(0.002)***		(0.002)***	(0.002)***	(0.002)***
No. placebo violations						−0.005	−0.004
						(0.003)*	(0.003)
Log annual income				−0.012	−0.006		
				(0.005)***	(0.005)		
Observations	9,608	9,608	9,608	9,608	9,608	9,608	9,608
R^2	0.002	0.006	0.006	0.004	0.006	0.006	0.006
Panel C. Quantities							
Ineligible	−0.215		0.008	−0.107			
	(0.01)***		(0.012)	(0.01)***			
No. violations		−0.097	−0.099		−0.079	−0.096	−0.081
		(0.003)***	(0.004)***		(0.004)***	(0.003)***	(0.004)***
No. placebo violations						−0.038	−0.03
						(0.005)***	(0.006)***
Log annual income				−0.146	−0.062		−0.054
				(0.009)***	(0.009)***		(0.009)***
Observations	13,183	13,183	13,183	13,183	13,183	13,183	13,183
R^2	0.065	0.145	0.145	0.109	0.151	0.15	0.154

Notes: The unit of observation in all regressions is a household. The outcome is the price the household faced for a BPL card in panel A and an indicator equal to one if the household obtained a BPL card in panels B and C. The estimation sample includes all households that reported BPL card prices in panels A and B, and all households in panel C. "Ineligible" is an indicator equal to one if the household violates any eligibility criteria; "violations" is the number of criteria it violates; "placebo violations" is the number of assets the household holds that do not disqualify it. All specifications include village fixed effects. Robust standard errors clustered at the village level are presented in parentheses.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

If violations of this sort were typical, then we would expect to see them produce a more progressive allocation of BPL cards.

The data show that the eligibility rule itself does a credible job of targeting the poor in the traditional, statistical sense. The raw correlation between eligibility and log income is a healthy −0.55 and the within-village correlation is nearly as

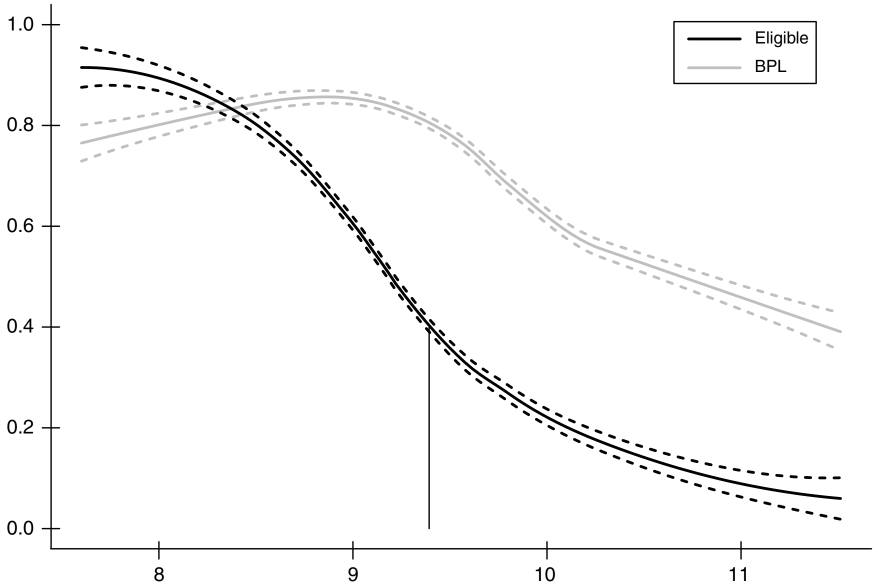

Figure 2. Eligibility Is More Progressive than BPL Status

Notes: Plots nonparametric regressions of an indicator for statutory eligibility (Eligible) or actual BPL status (BPL) against log income. For this graph statutory eligibility is defined ignoring the income threshold at Rs 12,000 per month, which is plotted separately. The domain of the plot is the 1 percent-trimmed sample income distribution. Ninety-five percent confidence intervals are indicated by dashed lines.

strong at -0.52. One might worry that even an honest official could not implement targeting on hard-to-observe criteria like the Rs 12,000 income threshold, but after dropping this criterion the correlation is still a healthy -0.42. In contrast, actual BPL status is correlated -0.23 with log income. Strikingly, even drastically simplified rules that drop all but *one* of the eligibility criteria still outperform the actual allocation: log income is correlated -0.37 with owning a phone, -0.32 with owning a water pump, -0.31 with owning more than 5 acres of land, and -0.30 with having a gas connection. As these are highly observable characteristics (in the case of land ownership and gas connections there exist independent records that could be used for cross-checking) it seems implausible that the BPL rule performs poorly solely because officials are unable to implement it. Figure 2 provides a nonparametric look at how rule violations affect the distribution of BPL cards. It shows that the poorest households are slightly more likely to be eligible than to have BPL cards, while for richer households this relationship is reversed. There is little evidence that rule violations are driven by officials' desire to improve targeting.

While each of these individual correlations is undoubtedly affected by measurement error, the conclusion that the actual allocation is less progressive than the statutory one is less likely to be. Note that measurement error in income will tend to affect both correlations with eligibility and with BPL status, but not to reverse their ordering. To reverse this ordering it would have to be the case that the single BPL card ownership variable is measured with much more error than the collection of variables which go into our measure of BPL eligibility.

Turning to a multivariate analysis, columns 4 and 5 of Table 4 report the results of regressions that include both functions of eligibility criteria and also the logarithm of annual household income. The coefficient on income serves as a test of the joint hypothesis that officials have "soft" information about household poverty and use this to target BPL cards. The results are generally mixed. Among households who reported prices, higher income is associated with higher prices and a lower probability of holding a BPL card, but these results are insignificant once we control for the number of criteria violated. Only in the full sample does income consistently negatively predict BPL status, and here the estimated effect is small: doubling log income has a smaller effect than increasing the number of violated eligibility criteria by one. Thus, while there is some evidence for soft targeting it appears insufficient to generate a progressive final allocation.[26]

D. *Do Degrees of (In)eligibility Matter?*

The most interesting feature of Proposition 1 is part 3, which says that even in the worst-case scenario where enforcement is weak and officials do not have progressive motives it may *still* be optimal for the rule designer to ignore the agency problem. Whether this is true hinges on the technology of enforcement, and in particular on whether enforcement works in such a way that simply *being* (in)eligible determines a household's likelihood of getting a slot, and not *how* (in)eligible the household is. In this case changing one household's eligibility status has no effect on the likelihood of other household's obtaining slots. This makes the agency problem ignorable from the perspective of rule design.

Figure 3 summarizes the (unconditional) relationship between degrees of ineligibility, prices, and the probability of holding a BPL card. Prices steadily increase and the probability of holding a BPL card steadily decrease as the number of eligibility criteria a household violates increases. This is consistent with the idea that it is not simply whether a household is ineligible but how ineligible it is that matters.

To examine this relationship more closely, column 3 of Table 4 includes both an ineligibility indicator and the number of eligibility criteria violated. If officials perceive all rule violations as being equally risky then we should find that the exact number of violations is unimportant once we control for eligibility. If degrees of ineligibility matter, on the other hand, then the number of violations should play a role even conditional on ineligibility. The data support the latter hypothesis: moving from 0 to 1 violation appears to raise prices and lower the likelihood of obtaining a BPL card by roughly the same amount as moving from 1 to 2, from 2 to 3, and so on.

[26] Since income and ineligibility are positively correlated with each other and have similar effects on bribe prices, one interpretation concern is that an incorrect choice of functional form for one could generate a spurious result for the other. We experimented with a full set of nonparametric indicators for every possible combination of rule violations and obtained essentially identical results for income. Similarly, we experimented with higher-order polynomials in log income and obtained essentially identical results for violations. Functional form does not appear to be an issue.

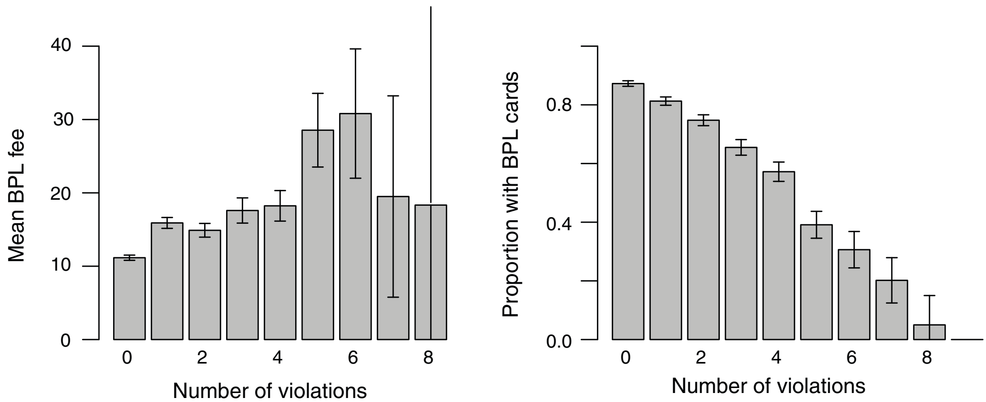

Figure 3. Prices and Allocations Are Monotone in Violations

The main outstanding concern with these estimates is that there may be variation in households' willingness to pay η_{hv} that is observed by the official but not by us. If this unobserved willingness to pay were positively related to violations then this could explain why degrees of ineligibility are associated with higher prices. Such a positive relationship could arise due to variation in credit constraints. Alternatively, one could imagine a negative correlation given that the goods provided through the TPDS are thought to be inferior goods. Unfortunately, with cross-sectional data we do not have plausible instruments for $\mathbf{x}_{hv}$ or y_{hv} to help in testing these hypotheses. We can, however, implement a placebo test. If the eligibility criteria are predicting prices because they are correlated with an unobservable demand shifter, then we should find that ownership of other similar assets which are *not* eligibility criteria should predict prices in a similar way. Our survey collected data on three such criteria: whether the household had electricity, a black and white television, and a bicycle. We therefore include these separately from the true eligibility criteria and see whether the estimated price effects match.

Columns 6 and 7 include the number of placebo criteria the household violates as a predictor. In contrast to real violations, placebo violations negatively predict prices and this effect is not statistically significant. This supports the view that rule violations matter because they raise the official's perceived cost of allocating a BPL card to the household. For the coefficient on true violations to be driven by an omitted variable, that variable would have to be correlated with prices and with true violations but uncorrelated with placebo violations.

We also estimate whether placebo violations are correlated with BPL card ownership, though here it is less clear what to expect: if placebo violations are correlated with both demand shifters and prices, then their effects on card ownership are ambiguous. Placebo violations negatively predict BPL card ownership in the sample of households who reported prices, but this is never more than marginally significant. Only in the full sample do placebo violations significantly predict allocations; here the magnitude of this relationship is about one-third that of the coefficient on true violations, which remains strongly significant. One way of reading these results is that placebo violations appear correlated with whether

TABLE 5—ARE HOUSEHOLDS DECEIVING OFFICIALS?

Regressor	(1)	(2)
Number violations	−0.104 (0.004)***	−0.098 (0.006)***
Visited by official	0.052 (0.012)***	
Visited × violations	0.012 (0.005)**	
Number violations known		0.007 (0.004)*
Violations known × violations		0.000 (0.002)
Observations	13,173	13,145
R^2	0.152	0.147

Notes: (1) The unit of observation in all regression is a household. The outcome is an indicator equal to one if the household obtained a BPL card. (2) Robust standard errors clustered at the village level are presented in parentheses.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

or not a household obtained a price "quote" at all, but not with the magnitude of that quote.

E. *Corruption or Fraud?*

The evidence thus far suggests that violations of Karnataka's BPL targeting rule are due to the corrupt behavior of the officials in charge. Yet might not some of these violations reflect fraudulent misrepresentations by the households themselves?[27]

Some of the evidence is directly inconsistent with this view. First, fraud should not lead to the exclusion of eligible households. Second, we have seen that most households must pay a bribe to obtain a BPL card and that bribe prices are systematically related to household's eligibility status, consistent with rent-seeking by officials. Third, the fact that households on average know *nothing* about the eligibility criteria suggests they are unlikely to be systematically gaming the process.

To further investigate the fraud hypothesis we use our data on which households were visited by a government official to ascertain their status. If the issue is that officials are not catching rule violations because they are not conducting proper inspections, then we should see that (i) households that were visited are less likely to receive BPL cards, and (ii) this effect is stronger for households that violate more rules. Column 1 of Table 5 shows that the opposite is true in both cases: households that were visited are *more* likely to obtain a BPL card, and this effect is stronger for less eligible households. This strongly suggests that visits are less about inspection than about negotiation.

[27] For example, Martinelli and Parker (2009) show that households' self-reported eligibility for Progressa/Opportunidades differs from eligibility as assessed by officials in follow-up visits. It is important to note that the BPL allocation process differs in that it does not include an initial self-report.

We can also examine whether eligibility violations matter less for households that are better informed about the eligibility criteria. The idea behind this test is that if households are fraudulently concealing characteristics that make them ineligible, then households that are better informed should be able to do this more effectively. Column 2 of Table 5 shows that this is not the case. Eligible households that are better informed about the rules, in the sense that they correctly identify more of the actual exclusion restrictions as such, are slightly more likely to obtain cards. The effect of information is no stronger, however, for ineligible households. Knowledge of the rules thus does not appear to be especially useful in allowing ineligible households to obtain cards.

IV. Would a Universal PDS Outperform the Targeted PDS?

The evidence we have examined thus far—evidence of weak enforcement, misaligned preferences, and "degrees" of eligibility—opens the door for considering targeting rules that are statistically less precise but easier to enforce. One would like to go further and estimate exactly which rules would perform best. The ideal way of doing so would of course be to experiment with different rules. Given that such experiments may be slow to materialize, if only because of the associated political risks, it is worth asking what we can say using the observational data at hand.

We focus here on a simple exercise: comparing targeting to universal eligibility. This question is motivated by the actual history of the Public Distribution System in India, which was a universal system prior to 1997. It also has the attractive feature that we need make only one extrapolative assumption in order to answer it. In particular, to compare targeting to universal eligibility we need to evaluate the planner's welfare function (8) under both regimes and under alternative assumptions about enforcement. This requires (i) predicting the allocation $\{a_i\}$ of slots under each scenario, and (ii) defining a social welfare function to evaluate these allocations.[28]

To predict allocations under perfect enforcement we simply set $a_i = 1$ for eligible households under targeting and $a_i = 1$ for all households under universal eligibility. We already observe the allocation of slots under status quo enforcement and targeting in our data. This leaves the task of predicting the allocation that we would observe under status quo enforcement but universal eligibility. The model predicts that all households should be at least weakly more likely to obtain a BPL card under universal eligibility than under targeting—this is true for ineligible households because they become eligible, and for eligible households because it becomes easier to establish their eligibility. Thus, we again set $a_i = 1$ for those households in our sample that obtained BPL cards. We then assume that each non-BPL household would have the same nonzero probability of obtaining a card under universal eligibility.

[28] Of course, this comparison does not rule out the possibility that targeting rules other than the one currently in use might be optimal. Predicting the performance of other targeting rules would require much more heroic extrapolative assumptions: one would need to estimate a structural version of the enforcement term $\pi(1, \mathbf{x}_i, R) - \pi(0, \mathbf{x}_i, R)$ in our model and then simulate the impacts of all conceivable rules R.

To parametrize social welfare we again suppose that the planner categorizes households into rich and poor, and we assume that the poor are those with incomes below the Rs 12,000 threshold specified in Karnataka's rule. Calculations using welfare weights that vary arbitrarily with income are straightforward to conduct but more complicated to exposit. We can then continue to represent the planner's preferences as

$$(15)\quad V(\{p_i\}) = \frac{\underline{\omega}\eta - 1}{1 - \overline{\omega}\eta}\int_{y_i=\underline{y}} 1(a_i = 1)\, dF(y_i, \mathbf{x}_i) - \int_{y_i=\overline{y}} 1(a_i = 1)\, dF(y_i, \mathbf{x}_i).$$

This expression is unique up to the relative welfare weight $r = (\underline{\omega}\eta - 1)/(1 - \overline{\omega}\eta)$ which the principal places on the poor. Intuitively, changes in the allocation of slots will be evaluated differently depending on how much the principle cares about inclusion errors relative to exclusion errors. We will therefore treat r as an unknown parameter and ask for what values of r the principal prefers targeting to universal eligibility. $r = 1$ corresponds to the symmetric case in which the net benefit of transferring a dollar of surplus to the poor is just equal to the net cost of transferring a dollar of surplus to the rich. When $r > 1$ ($r < 1$), on the other hand, the principal is relatively more concerned about exclusion (inclusion) errors.

Our calculations imply that in the case of perfect enforcement the planner prefers targeting if $r \leq 1.59$ and universal eligibility if $r > 1.59$. Intuitively, a planner who cares primarily about getting benefits to the poor will be more willing to tolerate the additional inclusion errors that universal eligibility generates. The same intuition holds in the case of status quo enforcement as well, but the tradeoff shifts: we calculate that the planner prefers targeting if $r \leq 1.36$ and universal eligibility if $r > 1.36$. Imperfect enforcement thus expands the set of social welfare functions for which universal eligibility is optimal: for $r \in [1.36, 1.59]$ targeting would be optimal if enforcement were perfect but, given the targeting failures we see in our data, universal eligibility is better in practice. This result is consistent with the intuitions we began with in Section IA and perhaps gives some sense of the potential magnitude of enforcement effects.

V. Conclusion

Accurately targeting resource transfers to the poor is one of the most pressing problems in international development. The predominant approach to targeting is to perform a statistical analysis of data from household surveys to define a proxy means test that is as tightly correlated with poverty as possible. This approach may fail to achieve the desired results, however, when the implementation of the targeting rule is delegated to corruptible agents. We study the problem of designing targeting rules subject to this agency constraint. Our main theoretical finding is that conditioning a targeting rule on an additional household characteristic, though it always improves statistical performance, may strictly reduce the principal's payoff because of novel effects on the *enforceability* of the rule.

Turning to data on the performance of a key proxy means test in India, we find evidence of weak enforcement. Rule breaking appears widespread and the ultimate allocation of benefits is substantially less progressive than it would have been had the rules been faithfully implemented. Targeting rules do appear to influence the bribe prices that officials charge to households, consistent with the existence of some enforcement, but the effects are small, consistent with enforcement being weak. We infer that this is an environment in which it may be important to design targeting rules that are relatively easy to enforce. Interestingly, Dreze and Khera (2010) have proposed reforming targeting policy in India for exactly this reason.

Of course, another implication of our results is that unobserved factors play a large role in determining the allocation of benefits within villages. Future work could fruitfully seek to characterize this process. One hypothesis, suggested by the work of Alatas et al. (forthcoming), is that local decision makers have their own notions of "poverty" that differ from those of the government. Alternatively, the dictates of electoral competition may determine who receives benefits and who does not.

Appendix A

PROOF OF PROPOSITION 1:

As a reminder we restate the principal's objective function

$$\text{(A1)} \qquad V(\{p_i\}) = (\underline{\omega}\eta - 1)\int_{y_i=\underline{y}} \exp\left\{-\frac{p_i}{\eta}\right\} dF(y_i, \mathbf{x}_i) + (\overline{\omega}\eta - 1)\int_{y_i=\overline{y}} \exp\left\{-\frac{p_i}{\eta}\right\} dF(y_i, \mathbf{x}_i)$$

and the traditional "statistical" optimization problem

$$\text{(A2)} \qquad \max_{R\in\mathcal{P}(\mathbf{X})} (\underline{\omega}\eta - 1)\int_{y_i=\underline{y}} 1(\mathbf{x}_i \in R)\, dF(y_i, \mathbf{x}_i) + (\overline{\omega}\eta - 1)\int_{y_i=\overline{y}} 1(\mathbf{x}_i \in R)\, dF(y_i, \mathbf{x}_i).$$

Parts 1 and 2:—Fix any eligibility rule R. As $f \rightarrow \infty$ the price p_i charged to eligible households approaches 0, while that charged to ineligible households approaches ∞. Since $\exp\{-p_i/\eta\}$ is dominated by the constant function $g(\cdot) = 1$ the dominated convergence theorem applies and the principal's objective function approaches the maximand in (A2). Hence asymptotically R can do no better than a solution to (A2). Alternatively as $\underline{\alpha} \rightarrow \infty$ and $\overline{\alpha} \rightarrow -\infty$ the prices charged to poor households approach 0 while those charged to rich households approach ∞. Again applying the dominated convergence theorem, the principal's payoff approaches $(\underline{\omega}\eta - 1)\int_{y_i=\underline{y}} dF(y_i, \mathbf{x}_i)$ regardless of R—in other words, the choice of a rule becomes irrelevant.

Part 3:—Suppose $\underline{\alpha} = \overline{\alpha}$ and there exists $\tilde{\pi}$ such that $\pi(a_i, \mathbf{x}_i, R) = \tilde{\pi} \cdot 1(a_i \neq 1(\mathbf{x}_i \in R))$ for all R. Then after some manipulation the principal's payoff can be written as

$$\text{(A3)} \qquad A\left[(\underline{\omega}\eta - 1)\int_{y_i = \underline{y}} 1(\mathbf{x}_i \in R)\, dF(y_i, \mathbf{x}_i) + (\overline{\omega}\eta - 1)\int_{y_i = \overline{y}} 1(\mathbf{x}_i \in R)\, dF(y_i, \mathbf{x}_i)\right] + B,$$

where A and B are constants that do not depend on R. Thus solutions to (A2) also solve the more constrained problem.

PROOF OF LEMMA 1:

For convenience define $\rho(x) \equiv \mathbb{P}(x^1 + x^2 \leq y^* \mid x^1 = x)$. If the principal makes a household with $x_1 = x$ eligible, then that household will receive a slot with probability $\exp\{-(\eta - \phi_1 f)/\eta\}$ while if they are ineligible they will receive a slot with probability $\exp\{-(\eta + \phi_1 f)/\eta\}$. The difference in the principal's payoff induced by making such a household eligible is proportional to

$$\text{(A4)} \qquad \left[\exp\left\{-\frac{1}{\eta}(\eta - \phi_1 f)\right\} - \exp\left\{-\frac{1}{\eta}(\eta + \phi_1 f)\right\}\right] \times \left((\underline{\omega}\eta - 1)\rho(x) + (\overline{\omega}\eta - 1)(1 - \rho(x))\right),$$

which is positive if and only if $\rho(x)\underline{\omega} + (1 - \rho(x))\overline{\omega} \geq 1/\eta$. This along with the monotonicity of $\rho(x)$ implies that the strictly optimal rule among those that condition on x_1 only is $R_1 \equiv \{\mathbf{x} : x_1 \leq x_1^*\}$ where x_1^* is defined by $\rho(x_1^*)\underline{\omega} + (1 - \rho(x_1^*))\overline{\omega} = 1/\eta$.

PROOF OF PROPOSITION 2:

Consider any rule R which conditions nontrivially on x^2 in the sense that there is a positive-measure subset $S \subseteq X_1$ within which the eligibility status of households depends on x^2. Define E and I the (possibly empty) subsets of $X_1 \backslash S$ within which all households are eligible and ineligible, respectively. As $\phi_2 \to 0$ prices in regions E, S, and I approach $\exp\{-(\eta - \phi_1 f)/\eta\}$, $\exp\{-1\}$, and $\exp\{-(\eta + \phi_1 f)/\eta\}$ respectively.

The argument in Lemma 1 shows that there exists x^{1*} such that the principal obtains strictly positive expected utility from giving slots to households with $x_i^1 < x^{1*}$ and strictly negative expected utility from giving slots to households with $x_i^1 > x^{1*}$. Thus if there are any households in $(X_1 \backslash E) \cap [0, x^{1*})$, then he can do strictly better (asymptotically) by expanding E to $[0, x^{1*})$, raising these households' probability of obtaining slots from $\exp\{-1\}$ or $\exp\{-(\eta + \phi_1 f)/\eta\}$ to $\exp\{-(\eta - \phi_1 f)/\eta\}$. Similarly, if there are any households in $(X_1 \backslash I) \cap (x^{1*}, \infty)$ he can do strictly better (asymptotically) by expanding I. Since S contains a positive mass of households, at least one of these two modifications is possible; together they yield R_1 and a strictly higher payoff.

PROOF OF PROPOSITION 3:

Given $\pi(a_i, \mathbf{x}_i, R) = \pi > 0$ whenever $a_i \neq 1(\mathbf{x}_i \in R)$ the official's problem amounts to choosing prices for the four categories defined by the product of rich/poor and eligible/ineligible. Call these categories $\{EP, IP, ER, IR\}$. His equilibrium choices are

$$p_{EP} = \max\{0, \eta - \pi f - \underline{\alpha}\} \tag{A5}$$

$$p_{IP} = \max\{0, \eta + \pi f - \underline{\alpha}\} \tag{A6}$$

$$p_{ER} = \max\{0, \eta - \pi f - \overline{\alpha}\} \tag{A7}$$

$$p_{IR} = \max\{0, \eta + \pi f - \overline{\alpha}\}. \tag{A8}$$

Let m_x denote the mass of households in category x and $\omega_x \in \{\underline{\omega}, \overline{\omega}\}$ the appropriate welfare weight for each group. The principal's payoff as a function of f satisfies

$$V(f) = \sum_{x \in \{EP, IP, ER, IR\}} \exp\left\{-\frac{p_x}{\eta}\right\}(\omega_x \eta - 1) \tag{A9}$$

$$\frac{\partial V(f)}{\partial f} = -\frac{1}{\eta} \sum_{x \in \{EP, IP, ER, IR\}} \exp\left\{-\frac{p_x}{\eta}\right\}(\omega_x \eta - 1)\frac{\partial p_x}{\partial f}. \tag{A10}$$

If $\underline{\alpha} > \eta - \pi f$ then $p_{EP} = 0$ and so among the poor stronger enforcement can only hurt, by raising p_{IP}. If $\overline{\alpha} < -\eta + \pi f$ then all rich households face strictly positive prices and so the contribution of the rich to $\partial V / \partial f$ is

$$\frac{\pi}{\eta}\left[\exp\left\{-\frac{p_{ER}}{\eta}\right\}m_{ER} - \exp\left\{-\frac{p_{IR}}{\eta}\right\}m_{IR}\right](\overline{\omega}\eta - 1), \tag{A11}$$

which is strictly negative provided that

$$\exp\left\{\frac{p_{IR} - p_{ER}}{\eta}\right\} > \frac{m_{IR}}{m_{ER}} \Leftrightarrow f > \frac{\eta}{2\pi}\log\left(\frac{m_{IR}}{m_{ER}}\right). \tag{A12}$$

On the other hand if the rule perfectly targets the poor then $m_{IP} = m_{ER} = 0$ and it is easy to see that $\partial V / \partial f \geq 0$.

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