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Di Bao: A Guaranteed Minimum Income in China's Cities?

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Abstract: Concerns about incentives and targeting naturally arise when cash transfers are used to fight poverty. We address these concerns in the context of China's *Di Bao* program, which uses means-tested transfers to try to assure that no registered urban resident has an income below a stipulated "poverty line." There is little sign in the data of poverty traps due to high benefit withdrawal rates. Targeting performance is excellent by various measures; indeed, *Di Bao* appears to be better targeted than any other program in the developing world. However, all but one measure of targeting is found to be uninformative, or even deceptive, about impacts on poverty. We find that the majority of the poor are not receiving help, even with a generous allowance for measurement errors. While on paper, *Di Bao* would eliminate urban poverty, it falls well short of that ideal in practice.

Keywords: Urban poverty, cash transfers, behavioral responses, targeting, China

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1. Introduction

While economic reforms and structural changes in the Chinese economy have meant high rates of economic growth, it is believed that certain sub-groups have been adversely affected or have been unable to participate in the new economic opportunities due to their lack of skills, long-term illness or disability. The collapse of the old safety-net provided by guaranteed employment has left some households vulnerable. Some of the "left behind" households started poor and some became poor, even though aggregate poverty rates have tended to fall over time.[2] Urban areas have figured prominently in these concerns about the "new poor."

The "Minimum Livelihood Guarantee Scheme," popularly known as *Di Bao* (DB), has been the government's main response to this new challenge.[3] The scheme started in Shanghai in 1993, then becoming a national policy with formal regulations issued by the State Council in 1999. (Here we are only concerned with urban DB; a rural version of the program is planned and has started in some provinces.) The program expanded rapidly once it became national policy and by 2003 participation had leveled off at 22 million people, representing 6% of urban residents, at a cost of about 0.1% of GDP (O'Keefe, 2004). The scheme is administered by the Ministry of Civil Affairs (MoCA).

Di Bao aims to provide a transfer to all registered urban households with incomes below a DB line set at the municipal level.[4] The aim is to close the gap between the recipient's income

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[2] On China's progress against poverty since reforms began around 1980 see Ravallion and Chen (2005).

[3] A useful overview of the *Di Bao* program in the context of overall social assistance policy in China can be found in O'Keefe (2004).

[4] "Registered" urban residents are those with an official registration for urban residence. There are also non-registered urban residents, who are often recent migrants from rural areas. Although it is not an issue we have been able to address in this study, we would hypothesize that the fact that the program is confined to households with urban registration is constraining its ability to reduce urban poverty. In testing that hypothesis one would clearly have to consider the possible incentive effects on migration decisions. This is a topic for future research.

and the local DB line (hereafter the "DB gap"), so that a minimum income is guaranteed. However, very little is known about the performance of the program in reaching the poor, even though it is evidently one of the largest cash transfer programs in the developing world. On paper, the program eliminates poverty (at least by its own definition of who is poor). But how close does it come to this ideal in practice?

This paper offers the first systematic assessment of *Di Bao*'s performance, based on independently-collected household survey data. We use the program as a case study for addressing a number of long-standing concerns about how effective transfer programs are in reducing poverty in developing countries. We focus on two issues that have clouded inferences from past work. Firstly, the performance of a program such as *Di Bao* will depend in part on behavioral responses. Yet in assessing targeting performance and poverty impacts it is common practice to simply deduct transfers received from post-transfer income to estimate pre-transfer income. Here there are concerns that recipients' labor supply or private transfer receipts will fall in response to DB, such that the net income gains are lower than the actual money received. On paper, the design of DB implies high marginal tax rates, which suggests that there may be strong incentive effects, which could undermine the program's effectiveness against poverty. The literature on the design of such programs suggests that the benefit withdrawal rate (BWR) — the amount by which the transfer payment falls for each extra unit of pre-transfer income — should be positive, but less than one. For programs aiming to reduce poverty a BWR around one half is consistent with evidence on the relevant income elasticity of labor supply (Kanbur et al., 1995). Taken literally, DB's aim of exactly filling the poverty gaps implies a BWR that is too high. However, it should not be assumed that any program operates exactly the way it is designed. There are many ways that the local administrators can dampen the marginal tax rates to avoid adverse incentive effects, such as by delaying the withdrawal of benefits when DB participants get a new job. There are reports from field work that this happens in practice (O'Keefe, 2004). Whether the incentive problems are a concern in reality is an empirical question.

Secondly, there are concerns about how "targeting performance" has been assessed in past work.[5] A large share of the attention of policy-makers has gone into achieving better targeting, in the sense of concentrating benefits on the poor, notably by avoiding leakage to the non-poor. Various measures of targeting have been used in past work, and these are typically interpreted as measures of a program's performance in "..directing benefits toward poorer members of the population" (Coady et al., 2004a, p.81). However, while it is widely agreed in this literature that the objective is to maximize the impact on poverty,[6] it is far from clear that any of the prevailing targeting measures provide a useful indicator for that objective. Indeed, there can be no guarantee that better targeting by these measures will enhance a programs' impact on poverty.[7] We consider a range of measures found in the literature, and explore their relevance to the performance of the DB program in achieving it objective of eliminating poverty.

Another common problem in past methods of assessing targeting performance is that the survey-based income measure may not coincide with the income concept used for targeting, thus clouding inferences. We address this problem by assessing performance against alternative income concepts, including a new method by which an "income" proxy is calibrated to the program's observed assignment.

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[5] These two issues are not of course independent; incentives depend on how transfers are targeted; assessments of targeting need to take account of incentive effects.

[6] See, for example, the discussion in Coady et al., (2004b, Chapter 2).

[7] See, for example, the results of Ravallion and Datt (1995) and Murgai and Ravallion (2005). For overviews of the generic issues raised by this class of policies see Besley and Kanbur (1993), Cornia and Stewart (1995), van de Walle (1998) and Ravallion (2005). The discussion in van de Walle (1998) — preceding Coady et al., (2004) by six years in the same journal — would surely lead one to question the relevance of the targeting measures used in the latter paper.

After describing our data for China's 35 largest cities in section 2, we outline our model of program participation in section 3. We then look for evidence of behavioral responses in section 4. The targeting performance of DB and its impact on poverty are the subjects of section 5. Section 6 concludes.

2. Data

We use China's Urban Household Short Survey (UHSS) for 2003/04. The UHSS was done by the Urban Household Survey Division of the National Bureau of Statistics (NBS) as a first step in constructing the (smaller) sample for the regular Urban Household Survey (UHS), which has a much longer questionnaire. We use the UHSS sample for the 35 largest cities, giving a total sample of 76,000, varying from 450 (in Shenzhen) to 12,000 (in Beijing). For these 35 cities, the definitions of geographic areas in the UHSS coincide with those for the DB lines and the entire data set has been cleaned by NBS staff and made available for this research.[8]

While the UHSS is a relatively short survey, it allows us to measure a fairly wide range of household characteristics. The survey also included household income, as obtained from a single question, "What is your household's total income?" (though respondents were also asked how much of their income came from wages). This is unlikely to give as accurate a measure of income as the UHS, which builds up its income aggregate from many questions. So we must expect measurement errors. Questions were added to the survey on subjective perceptions of welfare, namely a question on whether the respondent felt that the household's income was

[8] Outside these 35 cities, the local DB lines are not coded or use different codes, and in many cases use different boundaries to the geographic areas used by UHSS. So it is not feasible to assign DB lines to households outside the 35-cities sample. A further problem is that the bulk of the UHSS data outside the 35 cities has not been cleaned and local-level NBS staff were still working on the data at the time of writing. However, we cleaned the data ourselves for incomes and DB receipts for the full sample. We provide selected results from the full sample in an Appendix.

adequate for their needs, and whether income was improving over time. And we added questions to the UHSS on DB participation and income received from DB, for the purpose of this paper. However, this only includes the cash transfer from DB. It appears that some local governments also provide non-monetary benefits to DB participants, such as health-care and schooling entitlements, and sometimes a discount for the cost of utilities (notably in the north). We do not have data on these extra DB benefits. The UHSS was done during 2003 and 2004. The surveys in Beijing, Fujian, Hainan provinces and Kunming (the capital city of Yunnan province) were finished in 2003 while all others finished in 2004.

Another problem is that we do not have a municipal cost-of-living index. The DB lines may well reflect cost-of-living differences, but they will also reflect other variables, including local fiscal capacity. We will discuss the likely biases due to this problem.

3. Model of *Di Bao* participation

In using survey data to assess targeted transfer programs it is generally assumed that the income as measured in the survey is the same income measure used in implementing the program. This is a questionable assumption from three points of view. Firstly, there must be a strong presumption that income is measured with error in any survey; there are the usual reporting errors, but on top of this there are the likely extra errors in using a single income question, as well as the fact that the survey was done after the program was assigned, so the survey-based income net of DB receipts may differ from the income observed at the time the program was assigned (after the checks made by local authorities).

Secondly, potential participants face an incentive to misreport their incomes; possibly the survey-based incomes are more accurate. The DB program does not rely solely on self-reported incomes. Local authorities and neighborhood committees try to assure that recipients are genuinely eligible, taking account of other factors such as financial assets, consumer durables and housing conditions. There is also a community-appeals process, which includes the posting of applicants' names in a public place for two weeks. The national guidelines say that DB recipients are expected to work on "community services;" this would help screen the poor, although it is unclear whether work requirements are enforced locally (O'Keefe, 2004). Field studies in a few specific locations have revealed some possible concerns about income miss-reporting; for example, there are reports from qualitative research in Dalian that some people deliberately under-reported their incomes to obtain assistance (Daoshun and Tuan, 2004).

Thirdly, it is important to note that there is more than one way to measure "income." One source of differences between survey-based incomes and those used to target the program is the time period over which income is measured. Current income can differ from long-term income; a young well educated family may have low current income but be on a rising trajectory with good future prospects. Anecdotal evidence also suggests that local authorities may not measure current income the same way households would report their income. Based on informal interviews with DB participants in Liaoning, Hussain (2002) reports that local authorities would measure income for DB purposes as if the family was receiving all the benefits it was entitled too, ignoring the fact that the family was not in fact receiving those benefits.

The upshot of these considerations is that the allocation of DB is determined by a latent income measure. We assume that the program is allocated according to an unobserved money-metric of welfare given by:

$$\ln Y_i^* = \alpha\phi(\ln Y_i) + \pi X_i + \varepsilon_i \tag{1}$$

Here Y is the observed measure of income net of DB receipts, which can enter nonlinearly through a strictly increasing parametric function ϕ, X is a vector of other factors, which may also reflect measurement error in the observed, survey-based, income and ε_i is a normally distributed error term with zero mean and variance σ_ε^2. A household is eligible for the program if (and only if) $Y_i^* < Z_i$, which is the local DB line (depending on where household *i* lives). We assume that any household who is deemed eligible for DB will accept the transfer. Define a dummy variable that takes the value $D_i = 1$ if household *i* receives the program and $D_i = 0$ if not. The probability of participating is then given by:

$$\Pr(Y_i^* < Z_i) = F[(\ln Z_i - \alpha\phi(\ln Y_i) - \pi X_i)/\sigma_\varepsilon] \tag{2}$$

where *F* is the standard normal distribution function. We can then use a probit to estimate the parameters of (1) (normalized by σ_ε). Notice that the probability of participation is a strictly increasing function of the expected value of the proportionate DB gap, $E(\ln Z_i / Y_i^*)$. This assumes that the program works the way it is intended to. A more general model would allow for a more complicated selection process, as would arise from differences in the power of individuals to affect their DB participation, independently of their income. However, it is not clear that one can identify any variable that would influence "power' independently of income, so the more general model is not empirically distinguishable from the above model.

The *X*'s in equation (1) should clearly include geographic effects, since location can influence living standards independently of other household characteristics, including income. The municipality is the obvious geographic unit. We allow a complete set of municipality effects by *m*-1 dummy variables for the *m* municipalities (each with its own DB line). However, the DB line is constant within municipalities, so we cannot identify the coefficient on $\ln Z_i$ (the inverse of σ_ε) in (2) separately to the geographic effects. The vector *X* includes variables related to the dwelling and the observable characteristics of the household.

The detailed estimates for the probits are in the Appendix; results are given there with and without the net income variable (which enters as a quadratic in log income), given the concerns about its endogeneity. (We return to this issue.) Controlling for household income per capita, we find that DB participation is more likely for larger households, living in smaller dwellings, who do not own their dwelling, have an "old style" toilet, are still using coal for cooking, have no heating, no computer, have a female head of household, have a disabled or sick head of household, or a head with little schooling or who works in services or social security/welfare, or a head who is retired, works at home, has been laid off or is unemployed. DB households have lower financial wealth, are more likely to feel that their income is "less than they need to make ends meet," are more likely to think that their income has improved, have a lower share of wages in income, have more unemployed or students in the household but fewer retired people. Most cities have significantly lower participation rates than Beijing, controlling for household characteristics. (Later we investigate the differences across cities.)

It appears that the program is putting heavier weight on certain characteristics, such as poor dwelling attributes and lack of financial wealth, than is implicit in household income per person from the UHSS. To the extent that these effects reflect measurement errors in incomes <u>or</u> a broader concept of "income" that is motivating the program's targeting at local level, it can be argued that the program is doing a better job of reaching the poor than our calculations based solely on the survey-based incomes would suggest. We return to this point in section 5.

There are also indications that the program is doing better at reaching the chronically poor than those who may be vulnerable to poverty in the future. This is suggested by the fact that people who feel that they are on a downward trajectory are <u>less</u> likely to get support from DB.

It is clear from the above results that DB participants are a highly selected sub-sample. The Appendix gives the frequency distribution of the probit's predicted probabilities ("propensity scores") according to whether the sampled household participated in DB. We find that the sample of non-participants is heavily skewed toward zero probability of participating in DB. There are clearly a great many households in the sample who have negligibly low probabilities of participating in the program. However, there is a region of common support, in that there are at least some non-participants with similar propensity scores to all the participants.

4. Behavioral responses

One cannot assess a programs' targeting performance and impacts on poverty without taking a position on the behavioral responses to the program that influence the net income gains. However, assessing behavioral responses to a program such as DB without longitudinal (panel) data is difficult.[9] With only a single cross-sectional survey it is hard to be confident in the results, given the likelihood of omitted variables correlated with both program placement and the behaviors of interest. However, it is still worth seeing whether there are indications in our data of behavioral responses to the program. The key thing we are looking for is any sign that the program had an impact on the incomes of participants net of the transfers they received.

We use two approaches that can at least throw some light on whether there are likely to be significant behavioral responses relevant to our later assessment of targeting performance and impacts on poverty. First we estimate the marginal tax rate, to see if this is high enough to warrant concerns about behavioral impacts. Then we use a non-experimental evaluation method, which estimates impacts against a matched comparison group.

[9] And even with longitudinal data there can be severe identification problems; for further discussion see Ravallion (2005b, sections 7 and 8).

4.1 Benefit withdrawal rate

The design of DB intends that the benefits received will decrease as income rises, so that (in theory at least) participants face a positive marginal tax rate. Indeed, if DB exactly fills the gap between current non-DB income and the DB line (as is the scheme's aim) then participants will face no incentive to work. Earned income net of DB will fall to zero (assuming that work yields disutility). The program will have created a poverty trap, whereby participants do not face an incentive to raise their own incomes, because of the loss of benefits under DB.

The extent to which this is a real problem in practice is unclear. Benefits are unlikely to be withdrawn quickly. There are reports that at least some local authorities allow DB benefits to continue for some period after the participant finds a job (O'Keefe, 2004). Observations from field work also indicate that a notion of "imputed income" was used in a number of provinces. This was a notional level of income that reflected the potential income given the household labor force; this was apparently done with the aim of reducing work disincentives.[10] The program also appears to be targeted on the basis of other variables besides income, such as disability. This too could reduce the marginal tax rate facing participants.

Since we do not have panel data we cannot observe what happens when benefits are given or withdrawn. The best we can do is use the cross-sectional variance to identify the marginal tax rate. We can estimate the benefit withdrawal rate (BWR) by regressing the per capita DB payment received on income per person less DB receipts, with a complete set of dummy variables for municipalities (to capture the differences in the generosity of the program). The implied BWR is very low, at -0.0012 (t-ratio=-17.51, n=76,808). The estimate is also low if one allows for censoring; using a tobit regression, the estimate was -0.004 (t=-76.23).

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[10] This is based on a personal communication with Philip O'Keefe.

Estimating the tobits separately for each municipality, we obtained statistically significant BWRs in all cases, but all were very low, with none higher (in absolute value) than -0.001.

However, there must be a presumption of bias in these estimates, due to measurement error in incomes. There is the usual source of measurement error in asking incomes using only one question, plus the fact that income net of DB payments will probably underestimate income in the absence of DB if there are behavioral responses. To address this concern, we use an Instrumental Variables Estimator (IVE), in which the same set of regressors used in modeling DB participation in the last section are used as instrumental variables (IV) to estimate the BWR. (Note that in this case we only want to know the unconditional regression coefficient of DB payments on pre-DB income, so the instrumental variables are automatically excluded from the main regression of interest. However, the conditional BWR is unidentified.) When we do this, the estimated BWR is -0.0021 (t=-28.33). We also repeated these calculations separately for each municipality, using the IVE for the full sample in each municipality. The estimates were significantly negative for all municipalities and ranged from -0.0102 to -0.0001.

These calculations suggest that the marginal tax rate is very small, even allowing for measurement error in incomes. It thus appears unlikely that the program would provide any serious disincentive for earning income. However, at the same time, such a low benefit withdrawal rate raises concerns about how well the program reaches the poorest and how well it adapts to changes in household needs. These observations reinforce the aforementioned concern about how well the program is addressing transient poverty.

4.2 Mean impacts on net income relative to a matched comparison group

Another test for behavioral responses is by comparing net income for the DB sub-sample with a matched comparison group. There would (of course) be a strong presumption of selection bias if we were to use non-participants as the comparators. To address this concern we use propensity score matching to select the comparison group from the set of non-participants.[11] Predicted values (the propensity scores) from the probit are used for matching.[12] Using a light survey instrument will no doubt leave biases in these estimators.[13]

Given that the program is means tested it is tempting to include income as a predictor of participation in matching. The problem in doing so is that we would then be using the outcome variable (income net of DB) as one of the predictors for estimating impact on that same outcome variable! The results are only unbiased if it is assumed that there are no behavioral effects, which is what one is trying to test. It is not clear what one would then conclude from the results. In general, the direction of bias in the impact estimator cannot be determined.[14] To avoid this problem we should exclude income from the probit (using the regression in the Appendix), though then we run even higher risk that we have selection bias based on unobserved variables in the matching.

When we exclude income from the probit used to estimate the propensity scores we find that the mean income (net of DB) of participants is significantly lower than income of the matched comparison group of non-participants. Income minus DB receipts is 1417 Yuan lower for the DB participants (with a bootstrapped standard error of 270 Yuan using 100 replications) while mean DB receipts are 270 Yuan. It is not believable that receiving an extra 270 Yuan

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[11] On the theory of propensity score matching see Rosenbaum and Rubin (1983). For an application to a similar problem to the present one see Jalan and Ravallion (2003).

[12] We match each treatment household with the five closest propensity scores. We did not need to drop any observation from the treatment group. The STATA program, *nnmatch*, was used; results were checked against the program *Psmatch2*.

[13] For evidence on this point in the context of estimating behavioral responses to a cash transfer program in Argentina see Ravallion et al. (2005).

[14] We will have underestimated the income net of DB for participants given that they attenuated their labor supply (or received less transfer income) but through the miss-matching we will probably have also underestimated the income of the comparison group (since we over-estimate the propensity scores for treatment units).

would result in a reduction in pre-transfer income of 1417 Yuan; indeed, it would not seem plausible that the income loss exceeded 270 on average. This suggests that sizeable selection bias remains in matched comparisons that do not use income as one of the predictors for DB participation.

It is of interest to at least see what happens if we use income minus DB as a predictor for participation and then test whether there is any significant difference in income net of DB between participants and the matched comparison group. If we did find such a difference then it would clearly be inconsistent with our maintained assumption that the gain from the program is simply the transfer received from DB.

Performing this test, we found that DB participants had a slightly higher income net of DB than the matched comparison group (using net income as a predictor for participation). However, the difference was small and not significantly different from zero; we obtained a difference in mean income of 33 Yuan per person per year, with a bootstrapped standard error (using 100 replications) of 64 Yuan. So the data are internally consistent with the presumption that the income gain is simply the DB payment, though this is clearly a weak test given that the matching is only strictly valid under the assumption that there is no impact on net income.

We think it unlikely that single-difference matching is able to deal well with the selection bias in this case. It remains unclear that there is any defensible identification strategy for estimating impacts on net income with these data. However, these observations from the cross-sectional data do not reveal any compelling signs of behavioral responses that would lead one to question whether the income gain is less than the transfer payment.

5. Targeting and impacts on poverty

We first examine the targeting performance of the DB program, using various measures found in the literature. We then turn to the impacts on poverty. Finally, we examine robustness to measurement errors. Following the results of the last section, we assume that income in the absence of DB is given by the survey-based total income less the amount received from the program. However, we consider alternative welfare indicators that may be less vulnerable to measurement error than our survey-based measures of incomes.

5.1 Performance in reaching the poor

Various measures of "targeting performance" are found in the literature, though rarely is much critical attention paid to the properties of these measures. . The first measure we consider would appear to be the most popular one in both the literature and policy discussions. The measure is the share of total DB payments going to those with pre-transfer income $Y<Z$, the appropriate DB poverty line for that household; thus our first measure is $SHARE \equiv T(Y<Z)/T$ where $T(Y<Z)$ is the total transfer received by those with $Y<Z$ and T is the total transfer. *SHARE* is simply an ordinate of the concentration curve, $C(p)$, giving the cumulative share of transfers going to the poorest p% of the population.

In the special case of a uniform transfer — in which all recipients receive the same amount — *SHARE* becomes what we call the "targeting rate" (*TR*), i.e., the proportion of DB recipients with net income below the DB line; $TR \equiv N(D=1, Y<Z)/N(D=1)$, where $N(D=1, Y<Z)$ is the number of people who are both poor and receiving DB, while $N(D=1)$ is the number of DB participants. Analogously to the targeting rate, we can define the "coverage rate," $CR \equiv N(D=1, Y<Z)/N(Y<Z)$, where $N(Y<Z)$ is the number of people with $Y<Z$. Higher coverage is not normally thought of as better "targeting," though it could clearly matter to

the impacts on poverty.[15] There is another aspect of "coverage" that is relevant to this program, namely how well the program performs in filling the DB gap. We also measure the aggregate transfer to the DB poor as a proportion of the aggregate DB gap.

The second main measure we use is the concentration index (*CI*), as widely used in studying fiscal incidence amongst other applications.[16] Instead of focusing on one point on the concentration curve, this index measures the area between the curve and the diagonal (along which everyone receives the same amount). So $CI \equiv 2\int_0^1 C(p)dp - 1$.[17] The index is bounded above by 1 (at which point the poorest person received all DB payments) and below by -1 (the richest person receives all DB).

Our third main measure of targeting performance is that used by Coady, Grosh and Hoddinott (2004a,b), which we call the *CGH* measure. This is simply *SHARE* normalized by $H \equiv N(Y < Z)/N$, which is the DB poverty rate ("headcount index") for a population of size *N*. In other words, $CGH \equiv C(H)/H$. Thus *CGH* measures targeting performance relative to what would be found under a uniform allocation of the budget (whereby everyone gets the same amount, whether poor or not). Coady et al., choose this measure for its convenience, given that the objective of their meta-study is to compare performance across as many programs and countries as possible; both *SHARE* and *H* are readily available from past studies of the performance of targeted program. (This fact points to the popularity of *SHARE* in past empirical work on targeting performance.)

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15 See Cornia and Stewart (1995) argue that there has been excessive emphasis in policy discussions on "type 1 errors of targeting" (incorrectly classifying a person as poor) relative to "type 2 errors" (incorrectly classifying a person as not poor). This distinction is implicit in assessments of impacts on poverty; see the discussions in Ravallion and Datt (1995) and van de Walle (1998).

16 On the concentration index and its properties see Kakwani (1980) and Lambert (1993).

17 To assure that all our measures of targeting are aligned in the same direction, we multiply the standard definition of the concentration index by -1. To calculate *CI* from our micro data we use the convenient regression-based method outlined in Jenkins (1988) (following Kakwani, 1980.)

However, normalizing by *H* makes *CGH* a quire different measure to *SHARE* (or *CI*). To help understand the difference, consider a transfer scheme operating in two cities and giving all participants the same sum of money. In city A all the scheme's transfers go to the poorest 20% of the population, and the overall poverty rate is 50%. In city B all the transfers go to the poorest 40% and the poverty rate is 10%. A far higher share of the transfers go to the poor in city A (*SHARE*=100% in A, versus 25% in B). City A also has the higher concentration index (*CI*=0.8 in A versus 0.6 in B). By contrast, it is in city B where the scheme is deemed to be better targeted by the *CGH* measure (*CGH*=2.5 for B versus 2 for A).

The fourth measure we consider is the "targeting differential" (*TD*) proposed by Ravallion (1998) and developed further by Galasso and Ravallion (2005). *TD* is the difference between the DB participation rate for the poor and that for the non-poor:

$$TD \equiv \frac{N(D=1, Y<Z)}{N(Y<Z)} - \frac{N(D=1, Y \geq Z)}{N(Y \geq Z)} \tag{3}$$

When only the poor get DB and all of them are covered, *TD* = 1, which is the measure's upper bound; when only the non-poor get the program and all of then do, *TD* = -1, its lower bound. (In the two cities example above, *TD*=2/3 for city B and 0.4 for A.) Alternatively, we can define *TD* as the difference between the mean DB payment received by the poor (i.e., all those with $Y<Z$, whether or not they actually receive DB) and that received by the non-poor (all those for whom $Y \geq Z$) where the difference is normalized by the mean transfer payment (over all recipients). We call this TD^*.[18] Notice that when all recipients get the same transfer, $TD=TD^*$. It turns out later that the choice between *TD* and TD^* makes little or no difference to our results. Since *TD* is easier to interpret we shall focus on this measure.

[18] Unlike *TD*, TD^* can exceed unity, though this appears to be unusual (we found only one case in the 35 cities in our sample).

Unlike the preceding measures, *TD* directly reflects the program's coverage of the target group. In fact it can be readily shown that $TD = (CR - P)/(1 - H)$ where $P \equiv N(D = 1)/N$ is the overall participation rate. (The corresponding formula for TD^* is $TD^* = (CGH - 1)P/(1 - H)$.)

On implementing these measures on the UHSS data, we find that 7.7% of the population had a net income (observed income *minus* DB receipts) below the relevant DB line (Table 1). So the program's total participation is equivalent to about half of the eligible population, defined as those with income below the DB line for the relevant municipality. However, there is some leakage to ineligible households, as can be seen in Table 1.[19] About 40% of DB recipients are ineligible according to these data (0.43=1.69/3.91), giving *TR*=0.57. Almost three-quarters of those who are eligible are not being covered by the program (0.71=5.48/7.71), i.e., *CR*=0.29.

We find that *SHARE*=64%, *CI*=0.78 and *CGH*=8.3. This is excellent targeting performance by international standards. For example, Coady et al., (2004a,b) provide estimates of *CGH* for 122 programs across 48 developing countries. Argentina's *Trabajar* program has a *CGH*= 4.0, making it the best performer by this measure amongst all programs surveyed by Coady et al.[20] The median *CGH* across the 122 programs is 1.25. By this measure, DB is a clear outlier in targeting performance internationally.

Turning to our fourth measure of targeting performance we find that while 29% of the poor receive DB, this is only true of about 2% of the non-poor; thus we find that *TD*=0.27. The mean DB payment across all those with $Y<Z$ is 87.61 Yuan per person per year, while the

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[19] These are sample means. If one weights by city population then the proportion receiving DB rises to 4.69% of which 2.64% had incomes below the DB line. The estimated proportion below the DB line falls slightly, to 7.64%, of which 5.00% were not receiving DB.

[20] *Trabajar* is a combination of a workfare program and social fund, whereby participants are offered low-wage work to do things of value to poor communities; see Jalan and Ravallion (2003). Coady et al., calculate *CGH* from *Trabajar* from Jalan and Ravallion (2003) who estimate that 80% of *Trabajar* participants come form the poorest 20% of the Argentine population ranked by income net of *Trabajar* receipts (which are roughly constant across recipients). The corresponding *CGH* for the poorest decile is much higher, at about 6.0, though still less that for DB.

corresponding mean for those with $Y \geq Z$ is 4.15. The overall mean DB payment across all recipients is 270.33. So TD^*=0.31.

These calculations indicate that, while the program is very well targeted to the poor, it falls well short of perfect targeting (*TD*=1) in which all of the poor and only the poor are covered, as would be implied by the programs' design. Another way to see this is to calculate the total receipts for those with net income below the DB line. We then find that only 12.1% of the aggregate DB gap is filled by the program. DB is a long way off reaching its own aim of bringing everyone up to the DB line.

The weak coverage of the program — in terms of both coverage of those living below the DB line and coverage of the DB gap — is naturally limiting its impact on poverty, despite excellent targeting in the sense of avoiding leakage to the non-poor. Table 2 gives various poverty measures before and after DB transfers. We provide three poverty measures: the headcount index, the poverty gap index (*PG*) and the squared poverty gap index (*SPG*).[21] We give these measures for both the population as a whole (participants plus non-participants) and for participants only. To test robustness to the location of the DB lines, Figure 1 gives the empirical cumulative distribution functions of income (normalized by the relevant DB lines) with and without DB receipts for both participants and the full (35-city) sample. For this purpose, the households are ranked by income normalized by the relevant DB line.

We find that the program is having a sizeable impact on poverty amongst the participants. The proportion of the participant population falling below the DB line is 45% with DB transfers, but it would have been 57% without them. However, the impact on poverty in the population as

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[21] The poverty gap index gives the mean distance below the poverty line as a proportion of that line (the mean is taken over the whole population, counting the non-poor as having zero gap); for the squared poverty gap index the individual poverty gaps are weighted by the gaps themselves, so as to reflect inequality amongst the poor (Foster et al., 1984).

a whole is much less. The proportion falling below the DB lines falls from 7.7% to 7.3% after DB transfers. Proportionate impacts are slightly higher for *PG* than the headcount index and slightly higher again for *SPG*; this indicates that the program has increased the mean income of those below the DB line and reduced inequality amongst them.

We should not be too surprised that the best targeted program in the developing world has so little impact on poverty. Measures of targeting performance (such as *CI* and *CGH*) tell us nothing about coverage (in either of the aspects relevant to DB) and it is clearly the weak coverage that is reducing impact on poverty. We will return to this point when we come to look at the city-level results.

5.2 Robustness to income measurement errors

As we have emphasized, there is likely to be measurement error in the survey-based incomes. We consider two alternative methods of assessing targeting performance and coverage.

Implicit Di Bao gap: Our model in section 2 postulates that the DB program is assigned according to a latent income Y^* rather than the reported income *Y*. Taken literally, this model assigns participation to unit *i* if and only if $Y_i^* < Z_i$. If the model is right then we can interpret the propensity scores as a monotonic increasing function of expected value of the latent (proportionate) DB gap, $E(\ln Z_i / Y_i^*)$. Thus it can be argued that the estimated propensity scores provide a better measure of eligibility than survey-based incomes, in that they reflect the *X*'s that matter to Y^*, independently of observed income. We call this the propensity-score test for eligibility. Note that this almost certainly entails a generous allowance (from the point of view of the DB program) for measurement error in our survey-based incomes, since eligibility is calibrated to covariates of actual participation. If substantial miss-targeting is still indicated using our propensity-score test then there must be a strong presumption that this is true in reality, for the actual (but unobserved) model of program assignment.

Table 3 gives the results analogous to Table 1 for our propensity-score test of eligibility. As one would expect, we find that the coverage rate is higher than that based on the survey incomes, with 50% of eligible households receiving DB (as compared to 28% based on Table 1). The extent of leakage to the non-poor is slightly higher, however, with 49% of those receiving DB being ineligible based on our propensity score test (as compared to 43% based on survey incomes, as in Table 1). The eligible population (the highest 3.9% of propensity scores) receives 61.5% of DB payments, implying a *CGH* measure of 15.7.

On the basis of these results, it cannot be argued that the extent of leakage and incomplete coverage found in Table 1 is entirely due to discrepancies between the latent income measure used by the DB program and the reported incomes in our survey data.

Subjective welfare: Possibly a better indicator of need is the respondents' own assessments of their economic welfare, in response to the question as to whether their income is adequate for their needs. Table 4 cross-tabulates the answers against receipt of DB. We find that 81% of the population living in households receiving DB considered their income to be less than adequate for their needs, while this was true of 30% of the population as a whole. So self-assessments of economic welfare suggest that the program is even better targeted than do the survey-based incomes or our propensity-score test. However, coverage is even weaker based on self-assessed welfare; indeed, almost 90% of those who feel that their income is inadequate do not receive DB. Many of these households would not be considered eligible by MoCA based on objective criteria. However, these calculations do suggest that the DB program is covering only a small proportion of those who feel that their incomes are inadequate for meeting their needs.

Both these alternative methods confirm that the program performs relatively well in avoiding leakage to ineligible households. They also confirm that there is considerable under-coverage of those in need — despite the programs' stated aim of covering all eligible households — although the extent of this varies greatly according to our method of assessing who is eligible.

5.3 Targeting and poverty impacts across cities

Anti-poverty programs such as DB rely on decentralized financing and implementation. Heterogeneity in outcomes across geographic areas is to be expected; there will, of course, be differences in local resources and administrative capabilities, but there will also be (less obvious) differences in the local political economy. Here we aim only to describe the differences in DB performance across municipalities,[22] and to use these differences to assess how well prevailing targeting measures perform in predicting impacts on poverty. Of course, if we know the impacts on poverty — which we agree to be the objective — then we don't need the targeting measures. However, since these measures are widely used in assessing anti-poverty programs and in comparative work, it is of interest to test their value as indicators.

As can be seen from Table 5, there is considerable variation across municipalities in targeting performance. *SHARE* varies from 31% to 98%; *CI* varies from 0.64 to 0.93, *CGH* varies from 2.8 to 18.8,[23] while *TD* varies from 0.06 to 0.53. However, some cities do much better by some measures than others; as one would expect, *SHARE* is positively correlated with *CI* (r=0.50). *CI* is also positively correlated with *CGH* (r=0.34). However, *SHARE* is negatively correlated with *CGH* and *TD*, though the correlations are not significant. (As expected, *TR* and *SHARE* are highly correlated (r=0.80) as are *TD* and TD^* (r=0.88).) *TD* is highly correlated with

[22] The Appendix gives more detail on the results by city.

[23] All except one city (Kunming) have a *CGH* higher than the best performing program, *Trabajar*, surveyed by Coady et al. (2004).

the coverage rate (r=0.98); this is to be expected given that $TD = (CR - P)/(1 - H)$ (section 5.1). The other measures by contrast are only weakly correlated with *CR* (r = -0.28, -0.40 and -0.28 for *SHARE*, *CI* and *CGH* respectively).

Impacts on poverty also vary (Table 5). Subtracting the post-DB poverty rate from the pre-DB rate, the drop in the headcount index varies from 0.0% to 1.5% points. The impacts on poverty are highly correlated with the program's coverage rate (r=0.66, 0.71 and 0.66 for the impacts on *H*, *PG* and *SPG* respectively.) Given this fact, it is not surprising that the he measure of targeting performance that best predicts the program's impacts on poverty is *TD*. Strikingly, we find no sign of a positive correlation between the impacts on poverty and any of our measures of targeting performance <u>except</u> for *TD*; for *SHARE* the correlation coefficient with the impacts on the headcount index is -0.08, which is not significant, while for *CI* and *CGH* the correlation coefficients with poverty impacts are <u>negative</u> (r = -0.40 and -0.41 respectively). Only for *TD* is the correlation positive (r=0.61). Figures 2 and 3 plot the impacts on the headcount index against the *CGH* and *TD* measures respectively.

If one puts the competing targeting measures in a regression for the impacts on poverty, only *TD* has a significant positive coefficient (Table 6, left panel).[24] None of the other targeting measures have any predictive power for the program's impacts on any of the three poverty measures. The extra information contained in *TD* is on the extent of coverage; the other measures predict poverty impacts poorly because they focus exclusively on the programs' ability to concentrate its benefits amongst the poor.

[24] We use the impact on the log of the poverty measure. Using instead the impacts on the levels of the poverty measures also indicated that *TD* is by far the strongest predictor, but also suggested a significant negative effect for *CGH* (for all there poverty measures). However, this is probably deceptive given that *CGH* is normalized by the pre-DB headcount index. Note also that we do not include both *TR* and *SHARE* since they are so highly correlated, and similarly for *TD* and TD^*.

However, the picture changes dramatically when the poverty impacts are normalized by the DB transfer per capita, to give a cost-effectiveness ratio. None of our measures of targeting have significant pair-wise correlations with the cost-effectiveness ratios for the headcount index, though *SHARE* does have significant positive correlations with the cost-effectiveness ratios for *PG* and *SPG* (r=0.65 and 0.59 respectively). We find that *CGH* is negatively correlated with the cost-effectiveness ratios for *PG* and *SPG* (r= -0.44 in both cases). This pattern is echoed by the joint tests in the right panel of Table 6. For the headcount index, the targeting measures are jointly insignificant (F=1.48; prob.=0.23). For the two poverty gap measures, *SHARE* clearly emerges as the best (positive) predictor (Table 6); *CI* and *TD* have no predictive power for cost-effectiveness while *CGH* turns out to be a negative predictor.

Finally it is of interest to note that cities with higher public spending on DB tend to have higher impacts on poverty and better targeting performance, as measured by *TD*. The correlation coefficients between DB transfer payment per capita (of the population) and the impacts of *H, PG* and *SPG* are 0.80 or higher and for *TD* the correlation is 0.73.[25] Figure 2 shows the relationship for *TD*. This is consistent with evidence for anti-poverty programs in other settings suggesting that targeting performance tends to improve as programs expand, and to deteriorate in fiscal contractions; it appears that the early benefits tend to be captured more by the non-poor while it is the poor who are first to bear the costs of contractions (Ravallion, 2004).

6. Conclusions

We have focused on two prominent concerns about the use of cash transfers for fighting poverty: behavioral effects and targeting performance. Armed with an unusually large

[25] By contrast, the other targeting measures tend to be negatively correlated with DB spending, though only significantly so for *CI* (r = -0.52)

household survey, we have explored these concerns for one of the largest means-tested cash transfer schemes in the world (though a scheme about which remarkably little has previously been known).

In aiming to provide a guaranteed minimum income to all registered families in urban China, there is naturally a concern about behavioral responses to the *Di Bao* program; the work disincentives implied by how the scheme operates in theory suggest that counterfactual pre-transfer income will exceed observed income minus transfer receipts. Indeed, a strict interpretation of the design of *Di Bao* implies that it would create a virtual poverty trap, in that participants face a 100% marginal tax rate. Yet we find no evidence consistent with that implication, even when we allow for income measurement errors (which could lead one to underestimate the scheme's benefit withdrawal rate). Our results confirm qualitative observations from field work suggesting that the way the program operates in practice attenuates the incentive effects implied by its design. Indeed, when viewed in the light of the literature on the optimal design of targeted programs, our results suggest that the program's rate of benefit withdrawal is probably too low.

Nonetheless, the program appears to be very good at avoiding leakage to the non-poor. Coverage is clearly the bigger problem. Despite the program's aims, our survey data indicate that it is not reaching about three-quarters of those households with an income below the *Di Bao* line. And it is only covering about one eighth of the aggregate income gap relative to the *Di Bao* lines. While in theory, this program would eliminate poverty (based on the *Di Bao* lines), in practice the impact is small.

Measurement errors are a serious concern, as in any survey-based assessment of targeting performance. We have proposed new methods of testing robustness to the likely sources of error. However, even with a seemingly generous allowance for the fact that our survey-based incomes need not accord with the targeting criteria used by the program, we find that half the eligible population is not being covered. Also, there are signs that the program is doing better at reaching the chronically poor than the transiently poor, which will impede its ability to act as a safety net.

Performance in both targeting and reducing poverty varies greatly across the municipalities in charge of implementing *Di Bao*. However, the cities that are better at targeting DB are generally not the ones where the scheme has the most impact on poverty. Our comparison of poverty impacts across China's cities reveals that only one of the various measures of targeting performance found in the literature has significant power in predicting the program's impact on poverty. Prevailing measures of targeting performance that put heavy weight on the program's ability to concentrate benefits on the poor are largely irrelevant to the scheme's total impact on poverty, though one of these measures has some value as an indicator of cost-effectiveness. In considering future efforts to achieve a greater impact on poverty from this program, policy makers should focus instead on assuring more complete coverage of the poor.

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Table 1: Leakage and coverage of the *Di Bao* program based on observed incomes

% of population	Net income below DB line		Total
	Yes	No	
Receiving DB	2.22	1.69	3.91
Not receiving DB	5.48	90.60	96.09
Total	7.71	92.29	100.00

Note: n=76,443 (for the 35 municipalities).

Table 2: Impacts of *Di Bao* on aggregate poverty measures for urban China

	Di Bao poverty rate (%)	
	Before *Di Bao* (income net of DB receipts)	After *Di Bao* (income including DB receipts)
(a) Population (participants + non-participants)		
Headcount index (%)	7.71	7.26
Poverty gap index (%)	2.28	2.06
Squared poverty gap index (x100)	1.02	0.88
(b) Participants only		
Headcount index (%)	56.85	45.49
Poverty gap index (%)	19.92	14.23
Squared poverty gap index (x100)	10.21	6.44

Table 3: Leakage and coverage using the propensity score as an indicator of the *Di Bao* gap

	Eligible based on propensity score		Total
	Yes (highest 3.91% of $\hat{P}$'s)	No (the rest)	
Receiving DB	1.97	1.92	3.91
Not receiving DB	1.93	93.79	96.09
Total	3.91	95.71	100.00

Note: Column total include cases in which the data for estimating propensity score are missing.

Table 4: Targeting performance based on self-rated welfare

	Income is deemed to be less than adequate for needs	Income is just right	Income is more than enough for needs	Total
Receiving DB	3.16	0.67	0.08	3.91
Not receiving DB	26.36	40.14	29.58	96.09
Total	29.53	40.81	29.66	100.00

Note: n=76,443 (for the 35 municipalities).

Table 5: Targeting and impacts on poverty by city

	Targeting measures					Poverty impacts					
	TR	*SHARE*	*CI* (x100)	*CGH*	*TD* (x100)	*H*		*PG*		*SPG*	
						Pre-DB	Post-DB	Pre-DB	Post-DB	Pre-DB	Post-DB
Beijing	41.96	54.78	86.10	12.71	23.11	4.31	3.83	1.09	0.95	0.44	0.38
Tianjin	46.57	61.52	82.40	10.29	45.21	5.98	5.37	1.39	1.11	0.54	0.37
Shijiazhuang	65.24	64.55	81.52	8.05	25.52	8.02	7.97	2.34	2.20	1.04	0.93
Taiyuan	62.40	73.98	89.99	13.38	27.07	5.53	5.23	1.30	1.15	0.62	0.52
Huhehaote	55.88	55.03	73.12	5.96	6.00	9.24	9.16	3.19	3.13	1.55	1.50
Shenyang	75.49	82.28	85.12	6.97	29.00	11.81	10.67	3.24	2.87	1.41	1.16
Dalian	74.32	79.24	79.49	5.28	17.06	15.02	14.72	4.62	4.40	2.09	1.91
Chuangchun	43.69	65.53	87.49	8.51	22.31	7.70	7.65	2.15	1.95	1.04	0.84
Harbin	65.24	65.46	73.67	4.92	23.21	13.30	12.27	4.03	3.72	1.74	1.56
Shanghai	21.17	31.09	76.88	12.49	49.28	2.49	2.06	0.58	0.41	0.22	0.13
Nanjing	63.29	74.61	85.66	13.74	29.93	5.43	5.03	1.35	1.17	0.51	0.41
Hangzhou	66.67	86.53	89.36	18.61	9.05	4.65	4.54	1.18	1.13	0.54	0.51
Ningbo	57.97	68.82	91.75	14.61	28.78	4.71	4.15	1.27	1.03	0.56	0.44
Hefei	70.20	81.25	88.04	8.77	41.05	9.26	9.04	2.56	2.24	1.03	0.83
Fuzhou	50.00	42.25	68.70	18.78	20.19	2.25	2.25	0.52	0.49	0.18	0.17
Xiamen	79.41	72.08	87.22	10.48	24.08	6.88	6.50	1.91	1.81	0.79	0.74
Nanchang	55.26	64.10	82.66	8.36	29.83	7.67	7.19	2.10	1.89	0.88	0.73
Jinan	84.05	85.97	86.59	8.27	34.75	10.39	9.52	2.95	2.56	1.28	1.03
Qingdao	77.65	83.75	88.82	10.05	14.44	8.33	8.19	2.40	2.24	1.03	0.90
Zhengzhou	81.61	82.56	84.79	12.47	16.14	6.62	6.43	2.07	1.96	0.94	0.87
Wuhan	75.28	84.07	89.32	9.05	43.73	9.29	8.60	2.86	2.47	1.29	1.03
Changsha	48.21	50.07	76.95	7.64	40.99	6.55	6.07	2.09	1.85	0.92	0.78
Guangzhou	76.71	74.73	85.41	12.60	16.59	5.93	5.55	1.78	1.65	0.83	0.73
Shenzhen	37.50	39.08	92.74	15.15	15.09	2.58	2.58	0.92	0.86	0.42	0.40
Nanning	78.76	86.24	83.73	6.50	20.81	13.26	13.04	5.06	4.95	2.57	2.48
Haikou	95.65	97.70	93.19	5.24	8.00	18.64	18.62	6.18	6.10	2.95	2.87
Chongqing	61.61	72.98	74.59	4.17	37.07	17.49	16.51	5.74	4.85	2.66	2.04
Chengdu	57.89	38.34	81.59	7.34	19.55	5.22	5.06	1.63	1.57	0.78	0.74
Guiyang	58.98	71.23	85.57	6.55	30.78	10.87	10.36	3.75	3.32	1.83	1.50
Kunming	33.18	52.30	63.71	2.75	53.31	12.07	10.61	4.03	3.20	2.10	1.54
Xian	25.53	60.37	84.77	16.52	22.92	3.99	3.51	1.02	0.76	0.41	0.25

Lanzhou	68.04	65.92	84.43	7.36	39.16	8.44	7.86	2.36	2.01	1.08	0.84
Xining	59.17	62.12	86.33	9.82	36.12	6.14	5.90	1.91	1.73	0.92	0.79
Yinchuan	54.02	60.29	80.05	9.37	37.28	8.16	7.83	2.62	2.32	1.34	1.10
Wulumuqi	67.01	76.48	92.51	13.58	21.53	5.63	5.48	1.91	1.76	1.01	0.85
Sample mean	56.85	63.82			27.04	7.71	7.26	2.28	2.06	1.02	0.88
Pop. mean*	56.20	64.33			30.37	7.45	6.96	2.19	1.93	0.98	0.81

Note: *TR* is % of DB recipients with $Y<Z$; *SHARE* is the % of DB payments going to those below the DB line; *CGH* is *SHARE* normalized by DB poverty rate; *CI* is the concentration index (see text); *TD* is the difference in participation rates between the poor and non-poor; *H* is the headcount index; *PG* is the poverty gap index and *SP*G is the squared poverty gap. * weighted using 2003 populations by city.

Table 6: Which measure of targeting best predicts poverty impacts of *Di Bao*?

	Impact on (log) poverty measure:			Impact on cost-effectiveness ratio (impact on poverty per unit transfer)		
	Headcount index	Poverty gap index	Squared poverty gap	Headcount index	Poverty gap index	Squared poverty gap
Constant	-0.001	-0.056	-0.131	0.016	-0.003	-0.010
	(-0.012)	(-0.469)	(-0.709)	(0.309)	(-0.191)	(-0.683)
SHARE	-0.016	-0.053	-0.039	0.056	0.027	0.020
	(-0.316)	(-0.870)	(-0.340)	(2.033)	(4.025)	(2.726)
CI	-0.029	0.021	0.075	-0.022	0.019	0.032
	(-0.181)	(0.122)	(0.264)	(-0.230)	(0.953)	(1.483)
CGH	0.003	0.005	0.007	-0.055	-0.080	-0.108
	(1.071)	(1.669)	(1.203)	(-0.455)	(-2.219)	(-3.316)
TD	0.250	0.496	0.753	0.035	0.005	-0.006
	(4.927)	(7.571)	(6.689)	(1.135)	(0.814)	(-1.101)
R^2	0.438	0.629	0.542	0.165	0.550	0.514

Note: t-ratios in parentheses based on White standard errors. Coefficients scaled up by 100 for cost-effectiveness ratio.

Figure 1: Impacts of the program on poverty

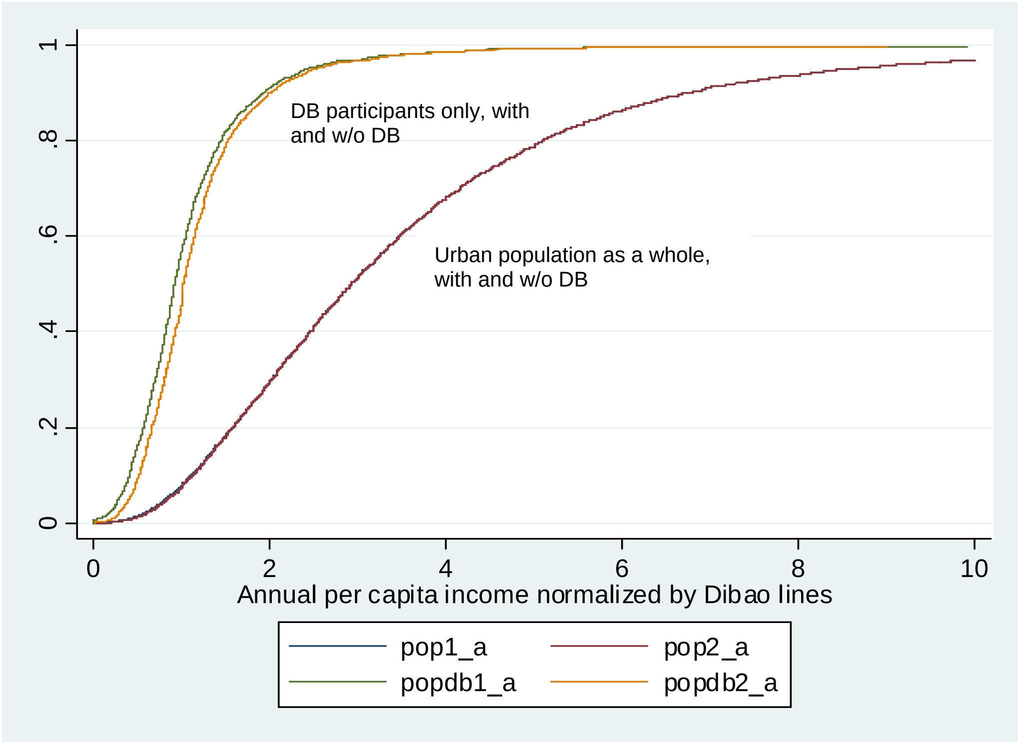


Figure 2: Impact of *Di Bao* on poverty plotted against the CGH measure of targeting performance across cities

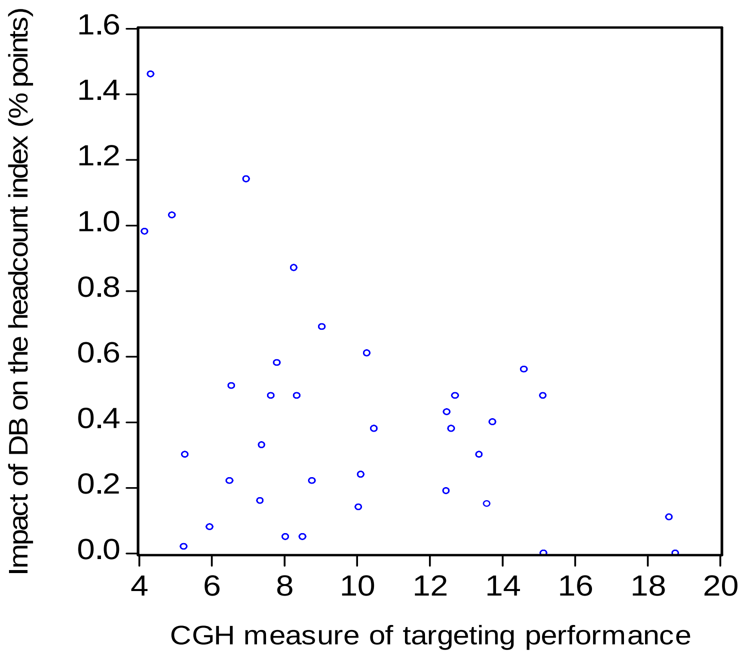


Figure 3: Impact of *Di Bao* on poverty plotted against the targeting differential

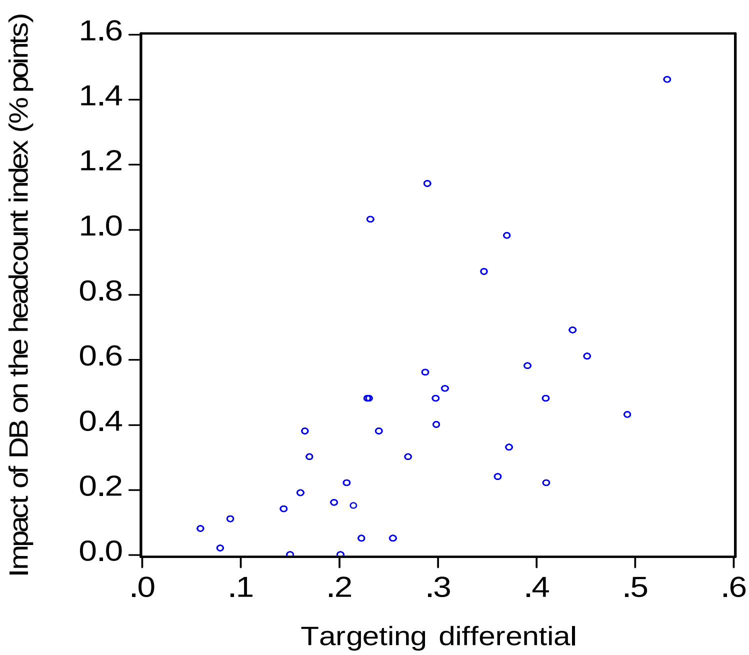
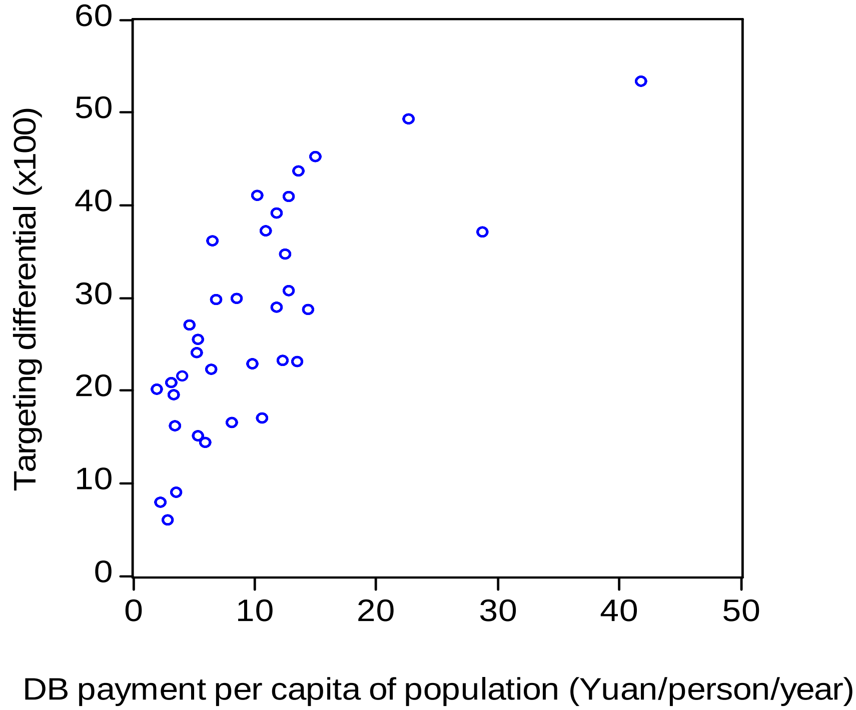


Figure 4: Targeting differential plotted against Di Bao spending per capita across cities



Appendix

Covariates of participation: Table A1 gives selected characteristics of DB recipients and the population as a whole, using the sample for the 35 municipalities. We only include households who are registered to live in the city of residence; all others — those with rural registration or registration in another city — are excluded from the program. (We checked and these groups were not receiving DB payments.) We see from Table A1 that participating families have slightly more children, smaller dwellings, are more likely to have someone with a long-term illness, are less likely to have a retired person but are more likely to have someone unemployed or to be supporting a student. The head of a DB household tends to have less schooling and is more likely to be female. DB households have an appreciably lower mean income than the population as a whole.

Table A2 gives the probits for participation, as discussed in section 3 of the main text.

Figure A1 gives the frequency distributions of the propensity scores. Panel a gives the frequency distribution for DB participants, while panel b gives it for non-participants. Panel c gives a "blow-up" of panel b, given that the heavy skewness in panel b makes it hard to see that there is common support.

Selected results for full urban sample: The full UHSS covered 265,000 urban households nationally, across 30 provinces (excluding Tibet). However, the bulk of the analysis in this paper is only feasible for the sub-sample of 76,000 households in the 35 major metropolitan areas. This Addendum gives some calculations on the poverty impacts for the full sample. Since DB lines cannot be matched properly outside the 35 cities, we cannot use these lines for the full sample. We thus ignore differences in the DB lines when comparing incomes.

Figure A2(a), which gives the CDF of the net incomes of participating households versus the CDF of population as a whole (using the full sample). We see that DB participants are appreciably poorer than the population as a whole. For example, at a poverty line for which 10% of the urban population is deemed to be poor, this is true of 70% of DB participants nationwide.

Figure A2(a) also tells us the impact on poverty, by giving the CDF of the gross incomes of participating households versus the corresponding CDF for their net incomes using the full sample. For comparison purposes, the figure also gives the CDF for the population as a whole. Consider, for example, the poorest 8% of the population in terms of net income (corresponding to the poverty rate for net income below the DB line in the 35-city sample). After DB payments, this falls to 7% — very similar to the impact we find for the 35-city sample. To allow for a wide range of possible poverty lines we can calculate the CDF of the gross incomes of the population and compare this to the corresponding CDF for their net incomes. Panel (a) of Figure A2 gives the results. We see that the program has a marked poverty-reducing impact amongst participants over all poverty lines, but the impact on poverty within the urban population as a whole is very small (as indicated by how close the lower two distributions are in Figure A2; Figure A2(b) gives a blow-up of the lower segment.

Detailed results on targeting and poverty impacts by city: Table A3 gives summary statistics cby city and further calculations relevant to the targeting performance.

Table A1: Characteristics of DB recipients compared to population as a whole

	Receiving DB?				Test for diff. in means	Population	
	No		Yes				
	Mean	St.dev.	Mean	St.dev.	(t-ratio)	Mean	St.dev.
H'hold size (no.)	2.95	1.01	3.08	1.09	-6.54	2.95	1.01
Area of dwelling	68.46	27.57	49.84	23.83	35.77	67.76	27.66
Year of dwelling	1988.47	21.12	1983.50	17.51	12.43	1988.28	21.02
Children (no.)	0.45	0.55	0.57	0.59	-11.72	0.45	0.55
Disabled (no.)	0.03	0.19	0.27	0.54	-60.15	0.04	0.22
Long-term sickness (no.)	0.17	0.49	0.39	0.70	-23.29	0.18	0.50
Retired (no.)	0.66	0.81	0.28	0.56	25.03	0.64	0.81
Unemployed(no.)	0.28	0.55	0.89	0.82	-57.42	0.30	0.57
Homeworker (no.)	0.09	0.29	0.19	0.44	-17.96	0.09	0.30
Students (no.)	0.44	0.55	0.65	0.60	-20.01	0.45	0.56
Head's years of schooling	11.17	3.66	8.42	3.58	39.60	11.07	3.70
Male head	0.71	0.45	0.63	0.48	9.89	0.71	0.45
Age of head	50.65	14.16	51.10	12.99	-1.69	50.67	14.12
Computer (no)	0.43	0.55	0.08	0.29	32.43	0.42	0.55
Wage ratio	0.68	0.39	0.46	0.40	29.38	0.67	0.39
Di Bao receipts per person(Yuan/Year)	0.00	0.00	270.33	350.48	n.a.	10.58	86.91
Net income per person (Yuan/year)	10236.94	9377.70	2934.10	2620.57	42.42	9951.14	9315.28
Sample size	73920		2888			76808	

Table A2: Probits for DB participation

	Coefficient	t-ratio	Coefficient	t-ratio
Log income per capita	0.2725	1.09	n.a.	
Squared log income per capita	-0.0668	-4.08	n.a.	
Log household size	0.2326	5.63	0.3100	8.09
Log area of dwelling	-0.1813	-5.24	-0.2517	-7.62
Year of dwelling construction	0.0009	1.10	0.0020	2.12
Type of house. Default: one story house				
Cheap dwelling	0.0064	0.11	0.0388	0.66
Apartment	-0.0474	-0.68	0.0220	0.32
Single house	-0.2276	-1.54	-0.2784	-1.98
Ownership of house. Default: rented public dwelling				
Rented private house	-0.1340	-1.92	-0.1622	-2.46
Self designed and owned	-0.1465	-2.00	-0.0592	-0.83
Inherited or bought a long time ago	-0.0612	-1.08	0.0231	0.43
Owned apartment/house	-0.2832	-5.23	-0.3071	-5.91
Owned designated low income dwelling	-0.0469	-0.71	-0.0237	-0.38
Owned through employer subsidy	-0.1495	-4.54	-0.1537	-4.84
Other	-0.0826	-0.94	-0.0568	-0.68
Type of toilet. Default: old style				
Public	-0.1127	-1.83	-0.1422	-2.38
Regular toilet	-0.0205	-0.60	-0.0408	-1.26
other	-0.2145	-2.61	-0.1981	-2.44
Type of fuel. Default: electricity				
Pipe gas	0.1061	1.2	0.0271	0.33
Liquefied petroleum gas	0.0651	0.76	0.0410	0.52
Coal	0.3273	3.47	0.3921	4.46
Other	-0.3639	-1.86	-0.3134	-1.71
Type of heating. Default: no heating				
Heating equipment	-0.0203	-0.41	-0.0584	-1.24
Air conditioning.	-0.3379	-6.11	-0.3752	-7.1
Electric heading	-0.1800	-2.49	-0.2020	-2.99
Type of bath. Default: Integrated bath room				
Shower or bathtub	-0.1574	-1.18	-0.0831	-0.66
Other	-0.0273	-0.2	0.1119	0.88
Additional house owned. Default: no				
One	-0.0444	-0.75	-0.1576	-2.81
Two or more	0.2005	1.43	0.0420	0.31
Sharing dwelling with other family	0.1438	2.02	0.1149	1.7
Sharing house with another family	-0.0048	-0.05	-0.0438	-0.46
No computer	0.3191	7.57	0.4450	11.22
Male hh head	-0.1039	-3.76	-0.0928	-3.53

Default head age>60				
Age of head < 20	0.5902	1.56	0.8190	2.71
20-30	-0.2952	-2.29	-0.3476	-2.89
30-40	-0.0936	-1.53	-0.0757	-1.3
40-50	0.0261	0.48	0.0642	1.23
50-60	0.0499	1.04	0.0806	1.75
Health: Default: head is healthy				
Disabled	0.8504	16.41	0.9112	18.4
Sick	0.3232	8.88	0.3589	10.24
Years of schooling of head	-0.0149	-3.37	-0.0269	-6.64
Head’s type of employer: Default: government				
Public service	-0.0696	-0.57	-0.2258	-2.16
State-owned enterprises	-0.1827	-1.73	-0.3603	-4.05
Collective enterprises	0.2205	1.89	0.1070	1.04
Share holding enterprises	-0.0161	-0.13	-0.1659	-1.51
Private enterprises	0.1894	1.7	0.0564	0.6
Foreign or joined enterprises	0.2382	1.18	-0.0362	-0.19
Self-employed	0.0466	0.43	-0.0991	-1.05
Others	0.3766	3.39	0.2959	3.1
Sector of head. Default: agriculture.				
Mining	0.3114	1.65	0.1342	0.74
Manufacturing	0.0837	0.86	-0.0557	-0.69
Construction	0.1882	1.62	0.0447	0.43
Transportation	0.0516	0.48	-0.1588	-1.71
Information	0.2442	1.02	-0.0404	-0.19
Retail and whole sale	0.1187	1.16	0.0050	0.06
Tertiary	0.2675	2.16	0.1332	1.21
Banking	0.5134	1.93	0.1884	0.78
Insurance	0.3779	1.05	0.1282	0.4
Real estate	0.2364	1.13	-0.0008	0
Law	0.0882	0.17	-0.2682	-0.58
Accounting	-0.1159	-0.47	-0.2995	-1.24
Leasing and commercial service	0.1501	0.76	-0.0133	-0.07
Technological research	0.3087	1.36	0.0587	0.28
Environment	0.0119	0.06	-0.2148	-1.14
Services- agencies	0.4576	2.92	0.3165	2.21
Tourism	0.2107	0.6	0.1362	0.44
Service-others	0.2964	3.1	0.1708	2.15
Education	0.1271	0.63	-0.1503	-0.79
Health	0.2077	1.23	-0.0053	-0.03
Social security and welfare	0.6911	2.98	0.5427	2.56
Publication	-0.0083	-0.02	-0.2364	-0.57
Entertainment	0.0859	0.25	0.0515	0.16

Culture, sports etc.	0.4005	1.8	0.1680	0.82
Public management	0.2454	2.3	-0.0084	-0.09
Occupation of head. Default: manager				
Senior professionals	-0.1010	-0.36	-0.3269	-1.3
Junior and middle level professionals	-0.3455	-2.03	-0.4066	-2.65
General managerial staff	-0.1736	-1.72	-0.1533	-1.94
Worker	0.0123	0.14	0.1219	1.73
Others	-0.0355	-0.36	0.0953	1.18
Default: head is working				
Retired	0.3082	3.95	0.1760	2.34
Homeworker	0.4121	4.38	0.2587	2.91
Laid off[26]	0.3314	4.71	0.2208	3.28
Early retired	0.0009	0.01	-0.2907	-3.26
Unemployed	0.3843	5.38	0.3291	4.82
Student	0.5666	1.53	0.3105	0.91
Other	0.3881	5.57	0.4922	7.6
Assets: Default: Financial assets<10000				
Financial assets 10000-30000	-0.4156	-8.17	-0.5592	-11.62
Financial assets 30000-50000	-0.4542	-4.46	-0.6583	-7.25
Financial assets 50000-100000	-0.2933	-2.3	-0.5603	-4.76
Default income less than needed				
Just right	-0.2911	-9.56	-0.4704	-16.78
Surplus	-0.4041	-5.44	-0.7047	-10.92
Default income has improved				
No change	-0.2563	-6.82	-0.1669	-4.72
Worse	-0.4071	-9.89	-0.2619	-6.79
Wage ratio	-0.5899	-16.12	-0.6692	-18.9
Share of retired in h'hold	-1.2260	-11.07	-1.5904	-14.8
Share of homeworker	-0.2957	-2.56	0.2302	2.16
Share of unemployed	0.2130	2.79	0.5641	7.86
Share of student	0.5245	6.14	0.7343	8.83
Share of children	-0.1558	-1.71	0.1202	1.35
City dummy. Default:Beijing				
Tianjin	-0.0681	-0.97	0.0835	1.23
Shijiazhuang	-0.2388	-2.78	0.1582	2
Taiyuan	-0.5976	-5.42	-0.1291	-1.28
Huhehaote	-1.3597	-11.08	-0.9894	-8.55
Shenyang	-0.5294	-7.89	-0.1236	-1.94
Dalian	-0.5717	-7.65	-0.2926	-4.12
Chuangchun	-0.3419	-3.74	0.0560	0.67
Harbin	-0.5388	-7.81	-0.0884	-1.39

[26] Laid off from SOE with Xia Gang subsidies.

Shanghai	0.7573	8.8	0.6718	7.95
Nanjing	-0.1851	-2.18	-0.0224	-0.28
Hangzhou	-0.3990	-2.72	-0.3229	-2.34
Ningbo	0.0490	0.36	0.1137	0.86
Hefei	0.1415	1.34	0.4473	4.37
Fuzhou	-0.5173	-3.8	-0.3696	-2.83
Xiamen	-0.3417	-2.32	-0.3485	-2.53
Nanchang	-0.2752	-2.77	0.1444	1.57
Jinan	-0.4849	-6.06	-0.1259	-1.67
Qingdao	-0.7061	-5.58	-0.4098	-3.45
Zhengzhou	-0.7907	-6.65	-0.4027	-3.64
Wuhan	-0.0319	-0.42	0.3230	4.42
Changsha	0.0645	0.84	0.2039	2.8
Guangzhou	-0.6260	-5.01	-0.5750	-4.96
Shenzhen	0.3040	0.97	0.1588	0.64
Nanning	-0.5367	-4.17	-0.0279	-0.24
Haikou	-1.1193	-7.41	-0.6251	-4.67
Chongqing	0.0532	0.7	0.5052	7.05
Chengdu	-0.6369	-3.95	-0.2739	-1.87
Guiyang	-0.6384	-6.52	-0.2210	-2.45
Kunming	1.0858	10.24	1.2130	12.44
Xian	-0.3491	-2.64	-0.0884	-0.69
Lanzhou	-0.4723	-5.36	-0.0964	-1.19
Xining	-0.5285	-4.84	-0.2250	-2.27
Yinchuan	-0.1434	-1.55	0.2284	2.75
Wulumuqi	-1.0720	-8.75	-0.6375	-5.96
Constant	0.3995	0.22	-4.2944	-2.29
# of obs.	76443		76489	
Pseudo R^2	0.4718		0.4187	

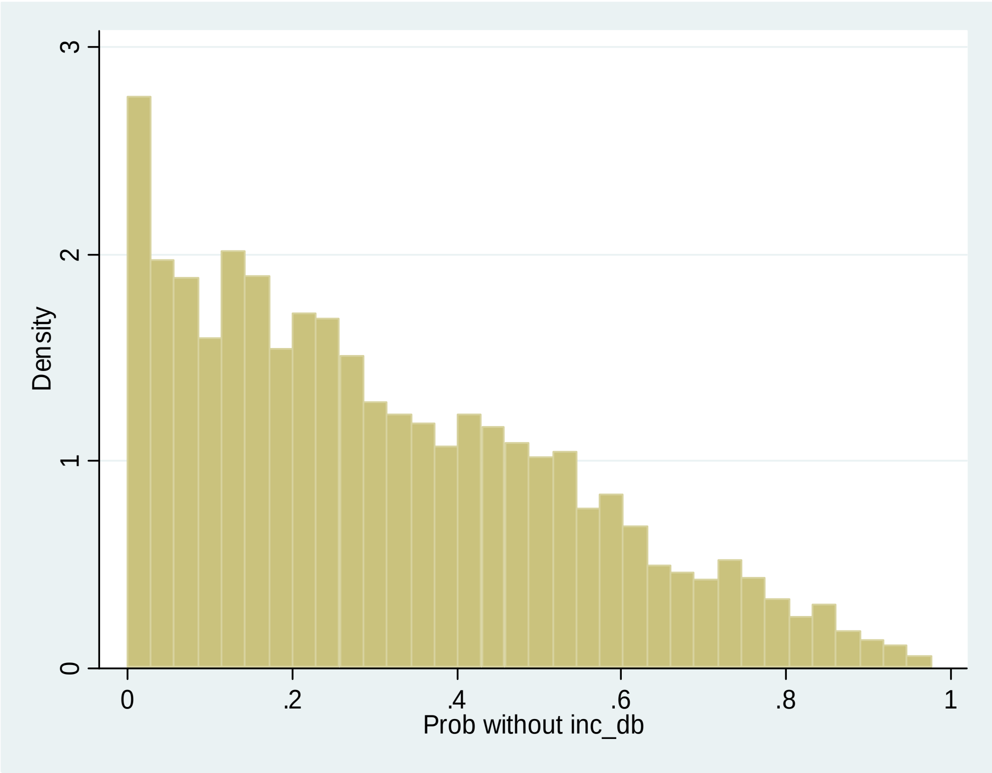
Table A3: Summary statistics and measures of targeting and coverage by city

	(1) Mean income (Yuan per person per year)	(2) DB line (Yuan per person per year)	(3) DB participa-tion rate (%)	(4) DB payment per recipient (Yuan per person per year)	(4) *CR*: % of those with *Y*<*Z* who receive DB	(5) *TR*: % of DB recipients with *Y*<*Z*	(6) *SHARE*: % of DB payments going to those below the DB line	(7) *TD*: Difference in participation rates between poor and non-poor
Beijing	13357	3480	2.53	535.20	24.64	41.96	54.78	23.11
Tianjin	9789	2892	6.26	239.88	48.76	46.57	61.52	45.21
Shijiazhuang	8001	2460	3.29	162.76	26.76	65.24	64.55	25.52
Taiyuan	7855	2052	2.49	187.16	28.06	62.40	73.98	27.07
Huhehaote	7441	2160	1.08	260.72	6.53	55.88	55.03	6.00
Shenyang	6345	2460	4.74	249.51	30.31	75.49	82.28	29.00
Dalian	78355	3312	3.67	288.75	18.17	74.32	79.24	17.06
Chuangchun	7380	2028	4.40	146.80	25.00	43.69	65.53	22.31
Harbin	6812	2400	5.15	239.03	25.28	65.24	65.46	23.21
Shanghai	13767	3480	6.41	353.98	54.46	21.17	31.09	49.28
Nanjing	11557	2880	2.66	320.67	30.96	63.29	74.61	29.93
Hangzhou	14882	3420	0.65	549.09	9.27	66.67	86.53	9.05
Ningbo	15846	3120	2.42	596.35	29.85	57.97	68.82	28.78
Hefei	8211	2520	5.66	179.62	42.91	70.20	81.25	41.05
Fuzhou	10452	2520	0.93	213.97	20.66	50.00	42.25	20.19
Xiamen	14615	3480	2.13	245.90	24.55	79.41	72.08	24.08
Nanchang	7227	1980	4.44	153.10	31.98	55.26	64.10	29.83
Jinan	8597	2496	4.39	284.37	35.53	84.05	85.97	34.75
Qingdao	9235	2760	1.59	372.01	14.83	77.65	83.75	14.44
Zhengzhou	7732	2400	1.33	260.37	16.40	81.61	82.56	16.14
Wuhan	8410	2640	5.59	244.03	45.25	75.28	84.07	43.73
Changsha	10770	2400	6.02	212.28	44.32	48.21	50.07	40.99
Guangzhou	14039	3600	1.31	623.32	16.92	76.71	74.73	16.59
Shenzhen	26036	3600	1.08	497.90	15.79	37.50	39.08	15.09
Nanning	7573	2280	3.66	85.26	21.71	78.76	86.24	20.81
Haikou	8039	2652	1.58	139.33	8.09	95.65	97.70	8.00
Chongqing	6007	2220	12.13	236.99	42.72	61.61	72.98	37.07
Chengdu	9701	2136	1.84	182.13	20.37	57.89	38.34	19.55
Guiyang	7521	1872	6.20	206.62	33.63	58.98	71.23	30.78
Kunming	7231	2280	26.81	155.91	73.68	33.18	52.30	53.31
Xian	7901	2160	4.08	240.90	26.09	25.53	60.37	22.92
Lanzhou	6895	2064	5.08	232.62	40.93	68.04	65.92	39.16
Xining	7505	1860	3.92	165.51	37.83	59.17	62.12	36.12
Yinchuan	7515	2040	6.09	179.06	40.33	54.02	60.29	37.28
Wulumuqi	8351	1872	1.87	215.72	22.18	67.01	76.48	21.53
Sample mean	9951	2715	3.91	270.33	28.87	56.85	63.82	27.04
Pop. mean*	10172	2761	4.69	307.53	32.80	56.20	64.33	30.37

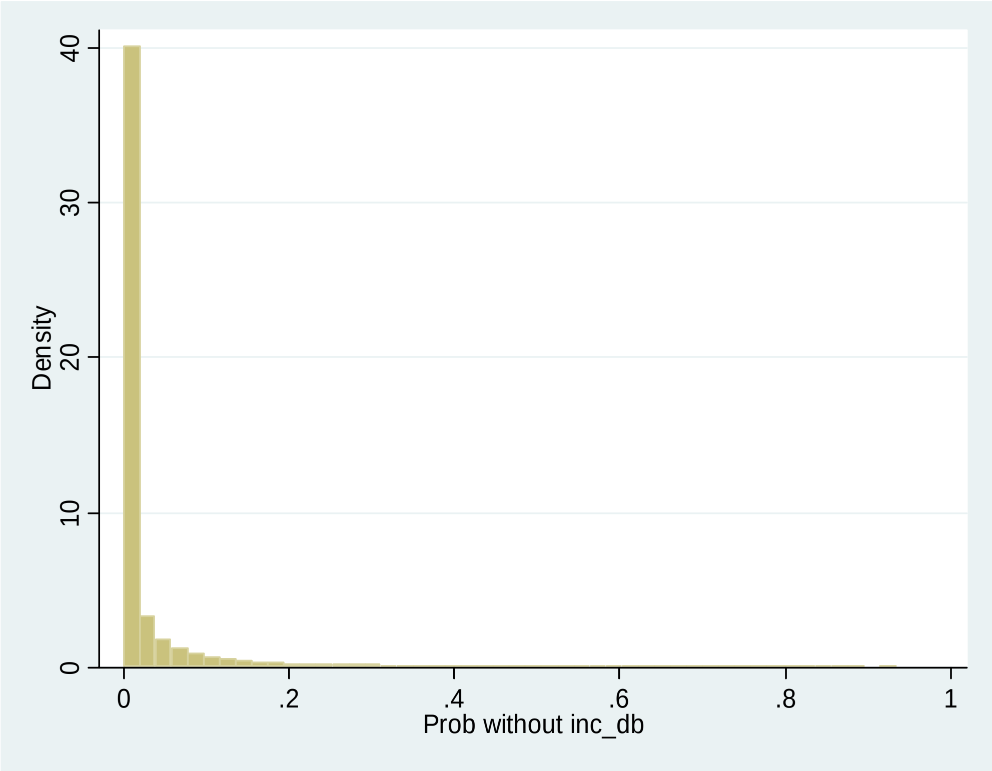
* weighted using 2003 populations by city.

Figure A1: Frequency distributions for propensity scores

(a) DB participants



(b) Non-participants



(c) Blow-up of the lower part of panel (b)

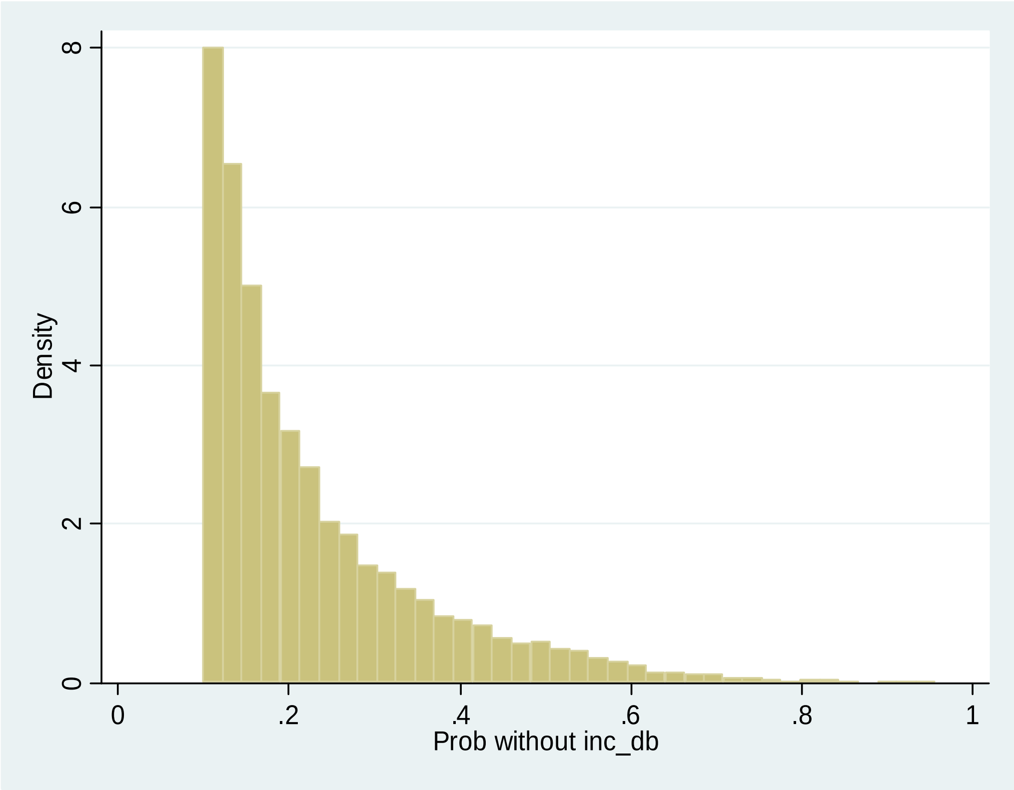
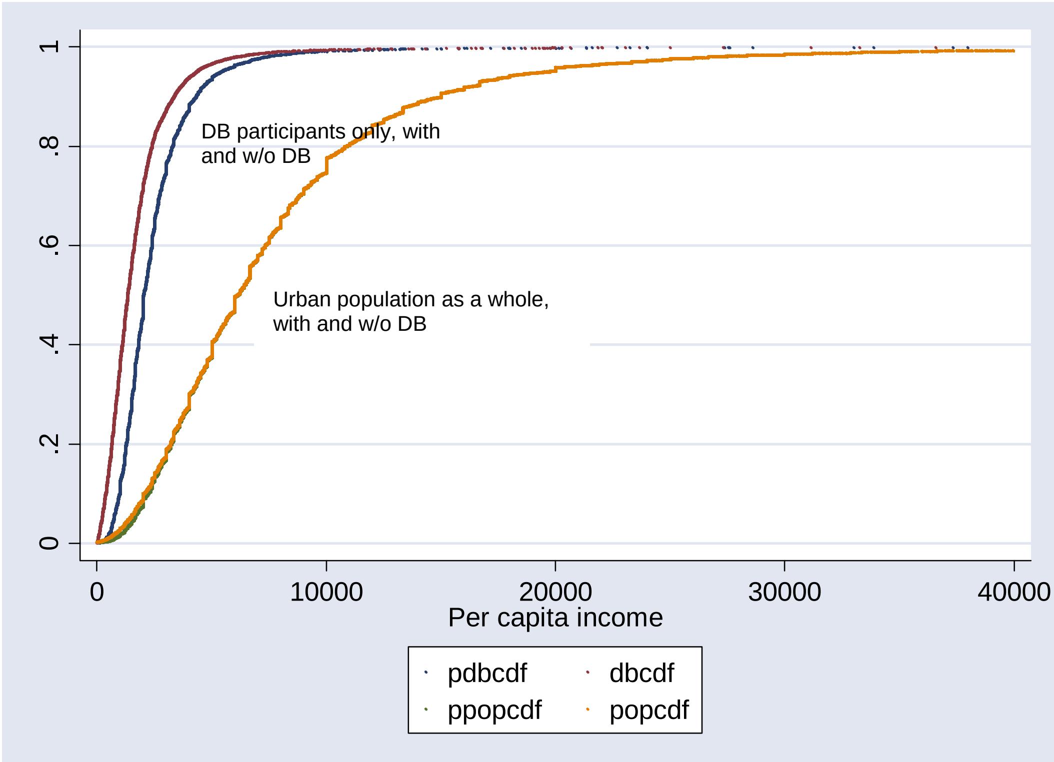

Density
0
2
4
6
8
0
.2
.4
.6
.8
1
Prob without inc_db

Figure A2: Impacts of the program on poverty

(a) Whole distribution



(b) Blow-up of lower segment of the population CDF