Decentralizing Eligibility for a Federal Antipoverty Program

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Decentralizing Eligibility for a Federal Antipoverty Program:  
A Case Study for China

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**Abstract:** In theory, the informational advantage of decentralizing the eligibility criteria for a federal antipoverty program could come at a large cost to the program’s performance in reaching the poor nationally. Whether this happens in practice depends on the size of the local income effect on the eligibility cut-offs. China’s *Di Bao* program provides a case study. Poorer municipalities are found to adopt systematically lower thresholds—roughly negating inter-city differences in need for the program and generating considerable horizontal inequity, whereby poor families in rich cities fare better. The income effect is not strong enough to undermine the program’s overall poverty impact; other factors, including incomplete coverage of those eligible, appear to matter more.

**Keywords:** Cash transfers, decentralization, poverty lines, targeting, China

**JEL:** H70, I32, I38, O18

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1. **Introduction**

The public finance literature recommends that redistributive transfers aiming to reduce poverty should be the responsibility of the central government in a federal system. The main argument against decentralizing such programs is that doing so will induce migration responses, which will be costly and undermine the redistributive effort.

This policy recommendation is not being followed in many countries. It is quite common for central governments to decentralize key aspects of the implementation and funding of their anti-poverty programs, particularly (though not only) in developing countries. Typically the center continues to provide broad guidelines and at least partial co-funding, but is relieved of the need to decide on the specific beneficiaries of that funding. Informational asymmetries have been the main justification for such decentralized redistributive policies. Advocates argue that, for the purpose of assessing eligibility, local agents are better informed than the center about local conditions. These informational problems are believed to have special salience in developing countries. However, the literature has also pointed out that the same informational problems create prospects for capture by local elites, subverting the center’s aims.

Another important stylized fact about developing countries is the presence of large geographic disparities in average incomes. As this paper will argue, these disparities can be associated with perverse geographic inequities in the outcomes of a decentralized anti-poverty program. Indeed, it is quite possible to find that the induced inter-jurisdictional disparities in program spending far exceed even large disparities in mean incomes. Then, under certain conditions, decentralization can severely limit the scope for reducing poverty when judged by consistent national criteria. The gains to the center in devolving power over beneficiary selection may come at a high price in terms of the program’s overall impact on poverty.

The essence of the problem is that local agents, who must typically commit at least some resources to the program, need not share the center’s goals. Their budget-constrained choices can then undermine the program’s performance against poverty nationally. For certain preferences of local agents, the government of a poor area will deliberately understate its poverty, as an adaptation to its budget constraint. Geographic inequity arises in that poor areas

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2. The classic exposition is Oates (1972), although also see the more qualified view in Oates (1992).
spend less on their poor. Horizontal inequity also emerges, in that equally-poor people are treated differently depending on where they live. Developing countries might then be better advised to follow the more standard recommendations from the public finance literature to centralize the key design parameters of their redistributive policies — although for rather different reasons to the traditional efficiency arguments based on migration responses.

Such concerns are not new. In the past they have been seen to yield a compelling equity case for central action, aiming to assure that *ex ante* equals are treated equally by the fiscal system (as advocated by Buchanan, 1950). The idea is that the center should correct for the inequities by differential cost-sharing or intergovernmental transfers. However, the extent to which such corrective policies are feasible in practice remains a moot point, given the same information asymmetries that have motivated the decentralization of anti-poverty programs. Indeed, as this paper will show, the information needed to eliminate the bias against poor areas *ex post* is even more demanding than that needed to directly implement the center’s preferred program. And the fact that poor areas tend to have poor services in so many developing countries is hardly suggestive of strong geographic redistribution of spending and fiscal burdens. Political influence on the outcomes can also be expected, and it would not be too surprising if this favored better-off areas. The case for believing that cost-sharing or transfers can solve the problem is far from obvious.

The paper studies these issues in the context of a specific anti-poverty program in which means-tested transfers aim to bring everyone up to an assured minimum income. In an effort to redress China’s sharply rising income inequality and signs of weak social protection for vulnerable groups, the central government introduced the *Di Bao* (DB) program in 1999. The program aims to provide all urban households who are registered in the locality of residence with a transfer payment sufficient to bring their incomes up to a predetermined poverty line. Obtaining registration in a new location is generally a difficult and lengthy process in China (not least for the poor), so in practice DB eligibility is confined to well-established local residents. The program started in Shanghai in 1993, where it was deemed to be a success and so became a

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5 In the context of China, the redistributive impact of the existing system of intergovernmental transfers is known to be quite weak; see Tsui (2005), Shen et al. (2006) and Shah and Shen (2006).

6 For a review of the literature on political influences on intergovernmental transfers for the purpose of regional equalization see Khemani (2006).
national (federal) program, with formal regulations issued by the State Council in 1999. The program then expanded rapidly and by 2003 participation had leveled off at 22 million people per year.

Like many areas of social spending in China, Di Bao relies on decentralized implementation. While the national and provincial governments provide guidelines and co-financing, the selection of beneficiaries is under municipal control. Individual municipalities determine their DB line and finance the transfers in part from local resources. The center provides some guidance on how DB lines are to be set. References are made in the regulations to the need to assure that basic consumption needs are met, given prevailing prices, but mention is also made of local fiscal constraints (O’Keefe, 2004; World Bank, 2007). Claimants must apply to the local (county-level) Civil Affairs office for DB assistance, and they typically do this through their local residential committee, which administers the program on a day-to-day basis. There is also a community vetting process whereby the list of proposed participants is displayed on notice boards and community members are encouraged to identify any undeserving applicants.

In 2003-04, about 60% of the program’s cost was financed by the center. The share varied across provinces, although data are not available on the precise shares. A State Council Circular in 2000 says that “…central finance will render support to areas with financial difficulties at its discretion,” and a further 2001 State Council circular clarifies that central funding was available for provinces with financial difficulties and high demand for DB. World Bank (2007, p.11) reports that “the share of central financing relative to in-province financing for 2002 ranged from zero in coastal provinces to 100 percent in Tibet, and 88 percent in Ningxia.” This suggests an effort to set higher central cost shares in poorer provinces. However, in the context of public spending generally, it is also known that the terms of intergovernmental transfers are subject to a process of political negotiation that is not seen to typically favor poorer provinces (Shen et al., 2006). It would be surprising if DB was immune to these political effects.

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7 Generic concerns have been voiced in the literature about the implications of China’s high level of fiscal decentralization for the country’s poor areas; see, amongst others, West and Wong (1995), Park et al., (1996), Kanbur and Zhang (2005), Shen et al. (2006) and Zhang (2006).

8 This raises concerns about stigma effects. World Bank (2007) reports results of a survey of DB participants in Liaoning province, which found that only 10 percent were ashamed or uncomfortable with disclosure of their household information in the application process. However, there may well be a selection bias in this calculation, if those deterred by public disclosure chose not to participate in DB.

9 This information is from correspondence with Philip O’Keefe at the World Bank.
The fact that local authorities retained power over the DB thresholds undoubtedly reflects in part the center’s lack of information on differences in the cost of basic needs in different cities. Government officials (in interviews with the author) said that the advantage of involving local community groups in this process is their greater knowledge of local conditions, including the cost of living. However, it appears likely that the center also felt that there were limits to how much it could credibly control the local authorities, even with good information. The history of the program — notably the fact that DB had emerged from a local initiative — appears to have also influenced the extent of decentralization in implementing the scaled-up national version. Central officials said that local municipalities retained the right to set their own DB lines, given that it started as a local program and the municipalities co-finance the program.  

However, in my interviews with central MOCA officials, they also recognized the likelihood that poorer municipalities may choose lower real DB lines, given their lack of resources. The officials considered this to be an undesirable feature of the program; in other words, the central authorities appear to see the program’s objective as reducing absolute poverty nationally, rather than relative poverty as judged by each locality. The authorities hoped that more favorable cost-sharing arrangements in poor cities would help avoid the problem. This paper will try to see whether that is in fact the case.

The paper begins by outlining a stylized model of the program. The model demonstrates just how much decentralized beneficiary selection can reduce the program’s overall poverty impact as judged by consistent national criteria. In one example, a central budget sufficient to eliminate poverty leaves 90% of the problem untouched when program implementation is decentralized under a fixed cost-sharing rule; this holds even with perfect targeting (according to local eligibility criteria) within all jurisdictions. Furthermore, in this model, the vertical and horizontal inequities come hand-in-hand; the only way to assure equal treatment of *ex ante* equals is to eliminate the inequality in provision between rich and poor areas.

The paper then studies the *Di Bao* program using a household survey that is representative at the level of each of China’s 35 largest cities, allowing city-level analysis. These data are used to explore the inter-city differences in spending and other program parameters, and to examine the implications for the program’s impacts on poverty. The results

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10 This type of central reliance on local governments is a long-standing feature of China’s social policies.
indicate that poorer municipalities tend to set less generous eligibility criteria, which attenuates, but does not eliminate, the program’s efficacy in reaching poor municipalities. However, the extent to which decentralized eligibility attenuated the program’s impact turns out to be very small. While there are a number of concerns about measurement errors, it appears likely that the way the program has operated in practice has helped reduce the cost of decentralized eligibility criteria to the program’s performance in reaching poor areas and poor people nationally. There is also evidence of horizontal inequity, in the form of large inter-city differences in the probability of participation at given (observable) household characteristics.

2. Theoretical model of the antipoverty program

The following model is a stylized version of the scheme that will be studied empirically later in the paper. It is assumed that the central government’s objective for the program is to provide cash transfers sufficient to bring everyone in municipality \( j = 1, \ldots, n \) up to an income level \( Z^*_j \), sufficient to not be deemed “poor”. In keeping with the fact that this is a federal program, poverty is defined in absolute terms, so that two people with the same real income are treated the same way no matter where they live. Thus \( Z^*_j \) is the cost of a reference level of welfare (utility), which is fixed nationally. By an appropriate choice of a cost-of-living index for normalizing both incomes and poverty lines, we can set \( Z^*_j = Z^* \) for all \( j \).

The resulting public expenditure will be distributed across municipalities such that the higher their poverty gap, the higher their spending allocation. Spending per capita in municipality \( j \) with income distribution \( F_j(y) \) is:

\[
C^*_j = \int_0^{Z^*} (Z^* - y) dF_j(y) = (Z^* - \bar{Y}_j^Z) H^*_j
\]

where \( H^*_j = F_j(Z^*)(>0) \) is the proportion below the poverty line (the “headcount index” or “poverty rate”) and \( \bar{Y}_j^Z \) is the mean income of the poor when the poverty line is \( Z^* \). The cost of the program is implicitly a function of all parameters of the distribution function, \( F_j(y) \).

These include the mean, \( \bar{Y}_j \), and the distribution of incomes relative to the mean, which is taken
to be fully described by a vector of parameters, $L_j$, representing the Lorenz curve in municipality $j$. $C_j^*$ is also a function of $Z_j^*$ at a given $F_j(y)$. It is convenient to re-write (1) as:

$$C_j^* = C(\bar{Y}_j, L_j, Z_j^*)$$ (2)

The problem is that the center does not have the information needed to implement this ideal program. It has access to a national sample survey that includes household incomes or expenditures but it can only observe the nominal distribution of income in those provinces or municipalities for which the survey has sufficient sample size to be considered representative. It is implausible that most national surveys would be representative at the levels of government at which one would probably want to implement such a program, to exploit local information for assigning eligibility. And there are differences across municipalities in the cost-of-living and other sources of heterogeneity in the money needed to achieve a given level of welfare — differences that are unobserved by the center. For example, it is still quite rare to have spatial cost-of-living indices. Additionally, there are likely to be idiosyncratic differences in needs (even without price differences). Climate and the existence other public programs are examples.

Decentralized implementation entails that the center gives each municipality the power to select beneficiaries, but requires co-financing to help control the program. Local agents are instructed to fill the poverty gaps but are free to determine the local poverty line. Total spending on the program in municipality $j$ is given by $C(\bar{Y}_j, L_j, Z_j)$ where $Z_j$ is the municipality’s chosen Di Bao poverty line. I shall close off the possibility of re-location in response to the variation in $Z_j$ across municipalities. This can be rationalized by either prohibitive costs of moving or residency requirements (whereby only long-standing residents are entitled to the program).

How will local spending vary with mean income? Intuitively, we might expect two effects working in opposite directions. On the one hand, a poorer municipality will have fewer resources for fighting poverty — call this the “resources effect”. On the other hand, a municipality with low mean income will tend to have a high poverty rate; call this the “needs effect.” The qualifier “tend to” is important, however, since there can also be a “distributional effect,” potentially offsetting the tendency for municipalities with a lower mean income to have a higher poverty rate. To see the various factors that come into play more clearly, differentiate (2) w.r.t. the mean, as follows:
\[
\frac{dC(\overline{Y}_j, L_j, Z_j)}{dY_j} = \left[ \frac{dC(\overline{Y}_j, L_j, Z_j)}{dY_j} \right]_{Z=\text{const.}} + H_j \frac{\partial Z_j}{\partial Y_j} \tag{3}
\]

where \( H_j = F_j(Z_j) \). The first term on the RHS is the needs effect and the second is the resources effect. The needs effect can be broken down as:

\[
\left[ \frac{dC(\overline{Y}_j, Z_j, L_j)}{dY_j} \right]_{Z=\text{const.}} = \left( \frac{\partial C}{\partial Y_j} \right)_{L=\text{const.}} + \frac{\partial C}{\partial L_j} \frac{dL_j}{dY_j} \tag{4}
\]

where

\[
\left( \frac{\partial C}{\partial Y_j} \right)_{L=\text{const.}} = -\int_0^{H_j} \frac{\partial y_j(p)}{\partial Y_j} dp = -\frac{H_j \overline{Y}_j}{Y_j} = -\omega_j \tag{5}
\]

where \( y_j(p) \) is the quantile function (inverse of the distribution function, \( p = F_j(y_j) \)) and \( \omega_j \) (\( 0 < \omega_j < 1 \)) is the income share of the poor.\(^{11}\) The first term on the RHS of (4) is unambiguously negative but the second term — the distributional effect given by the product of the two gradient vectors, \( \partial C / \partial L_j \) and \( dL_j / dY_j \) — could have either sign. I will say that the expansion path for spending is “distribution neutral” if this aggregate distributional effect is zero.

To direction and size of the resources effect depends on the scheme’s design and the behavior of local agents, which we now consider. A key design feature is that the center sets the share of the program cost to be financed locally; that share is denoted \( \alpha_j \), where \( 0 < \alpha_j \leq 1 \) for all \( j \). The center chooses \( \alpha_j \) to assure that the central budget is not exceeded. (The differential cost shares can also be chosen to help control local choices, as discussed later.) Income of the municipality net of spending on the program is \( \overline{Y}_j - \alpha_j C_j \), where \( \overline{Y}_j \) is gross income. The program’s local income share is \( s_j = \alpha_j C_j / \overline{Y}_j \).

In characterizing the behavior of local government agents, it can be presumed that they do not care solely about reducing poverty, which they must balance against the burden of co-financing. It is assumed that each municipality has preferences over spending on the program

\(^{11}\) The derivation of equation (5) exploits the fact that, on holding the Lorenz curve constant (intuitively, holding inequality constant), it must be the case that all income levels change at the same proportionate rate, implying that the quantile function has an elasticity of unity with respect to the mean, i.e., \( \partial \ln y(p) / \partial \ln \overline{Y} = 1 \). Also note that \( C_j = \int_0^{H_j} (Z - y_j(p)) dp \).
and other uses of local income, both valued positively. These preferences can be taken to embody the local political economy, in that different local municipalities are taken to have different preferences, which reflect the local political and economic factors that influence the trade-offs drawn between spending on the anti-poverty program and other uses of public money.

In rationalizing the assumption that local authorities value spending on poverty reduction, we can either imagine that they care intrinsically about their impact on poverty or that it is seen to be instrumentally important. The latter case rests on the fact that the program attracts resources from the center, given the co-financing feature. Reaching a larger slice of the local population through the anti-poverty program may well buttress the position of local authorities, making it more likely that they stay in power.\textsuperscript{12} It is assumed that the program’s local impact on poverty is measured by the poverty gap (consistently with that objective).

More formally, let each municipality have a preference ordering over the two “goods”: local spending on the program and income net of local program spending, as represented by the function:

\[ W_j = W_j(\bar{Y}_j - \alpha_j C_j, C_j) \tag{6} \]

The function \( W \) is assumed to be strictly increasing in both arguments. The conditions for an optimum with respect to \( C_j \) (or, equivalently, \( Z_j \)) are that:\textsuperscript{13}

\[ \alpha_j W_{j\bar{Y}} (\bar{Y}_j - \alpha_j C_j, C_j) = W_{jC} (\bar{Y}_j - \alpha_j C_j, C_j) \tag{7.1} \]

\[ \alpha_j^2 W_{j\bar{Y}Y} - 2\alpha_j W_{j\bar{Y}C} + W_{jCC} < 0 \tag{7.2} \]

(Subscripts on \( W \) denote partial derivatives.) Implicitly differentiating (7.1) with respect to \( \bar{Y}_j \):

\[ \frac{dC_j}{d\bar{Y}_j} = \frac{\alpha_j W_{j\bar{Y}} - W_{j\bar{Y}C}}{\alpha_j^2 W_{j\bar{Y}Y} - 2\alpha_j W_{j\bar{Y}C} + W_{jCC}} \tag{8} \]

The direction of the municipal income effect in equation (8) is ambiguous, under the assumptions made so far. However, four special cases will help interpret the result in (8).

\textsuperscript{12} It is not only in a democracy that public authorities gain from such behavior. A city government in China that was widely seen to neglect its local population would be unlikely to stay in power very long.

\textsuperscript{13} The problem is formally identical to a model of consumer behavior in which \( \alpha \) is interpretable as the relative price of spending on the poverty-reduction program. Without the co-financing requirement the municipality will choose a corner solution in which all its residents are deemed to be “poor.”
Case 1: Suppose that higher municipal income lowers the marginal welfare of program spending ($W_{yc} < 0$) and that the municipality’s objective is linear in income ($W_{yr} = 0$). Then it is immediate from (8) that $dC_j / d\bar{H}_j < 0$; poorer cites will spend more on the program.

Case 2: Suppose instead that $W(.)$ is separable between the two types of spending ($W_{yc} = 0$) and has strictly diminishing returns to income ($W_{yr} < 0$). (Separability can be weakened to $W_{yc} > \alpha_j W_{yr}$.) Then $dC_j / d\bar{H}_j > 0$; poorer cities will spend less on the program, in marked contrast to the centralized program.

Case 3: Now add to Case 2 the assumption of linearity in spending on poverty ($W_{cc} = 0$). Then the income effect on spending is simply the inverse co-financing share:

$$\frac{dC_j}{d\bar{H}_j} = \frac{1}{\alpha_j} \geq 1$$

(9)

Not only will the resources effect dominate, but the total income effect will be no less than unity. At a 50% cost share (say), program spending will rise by $2 for each $1 gain in mean municipal income. Furthermore, local spending on the program could be highly income elastic; the income elasticity is simply the inverse of the share of local income devoted to the program ($s_j$).

Table 1 gives a numerical example. There are two regions, one “poor,” one “rich.” Given the parameter values in Table 1, filling the poverty gaps relative to a single national (real) poverty line would require $10 per capita in the rich region and $135 in the poor region. 90% of the national poverty gap (the population-weighted aggregate of $(Z_s - \bar{Z}_j)H_j$ across the two regions) is in the poor region. Under the “Case 3” welfare function $400\ln(\bar{Y}_j - 0.5C_j) + C_j$ (with a 50% cost share), the decentralized version of the program (at the same cost to the center) will entail that all the program’s budget ends up going to the rich region, with none to the poor region. Instead of eliminating absolute poverty, as judged by the national poverty line, the decentralized program will leave 90% of the problem untouched.

Case 4: A further insight into just how powerful the resources constraint can be is obtained if we combine Case 3 with the assumption of distribution neutrality ($dL_j / d\bar{H}_j = 0$). Then one obtains the following simple formulae for the decomposition in equation (3):
This suggests that differences in needs may well play a very modest role under Case 4. In a municipality with the typical amount of income inequality and a medium size program, $\omega_j$ will be quite small — unlikely to exceed 0.05. With a 50% cost-share, the resources effect will be 2.05, utterly swamping the needs effect.

A further implication of the existence of a municipal income effect on the poverty line is that the decentralized program will generate horizontal inequity, meaning that people who are identical ex ante are not treated equally under the program ex post. This happens in two ways. Firstly, when the income effect on the poverty line is positive there will be people living in poor municipalities who are left out of the program, but would be covered if they lived in a sufficiently better-off area. This stems from the region of non-overlapping support (in the income dimension) induced by the income gradient of the poverty line. Secondly, participants within the region of common support who are at the same pre-intervention income will have different poverty gaps and (hence) receive different transfers, depending on where they live.

In principle, such geographic inequities (both vertical and horizontal) can be redressed by a differential cost-sharing arrangement. To see what would be required, note that $Z_j$ satisfying equation (7.1) can be written as: $Z_j = Z_j(\bar{Y}_j, \alpha_j)$. Consider the conditional cost share, $\alpha_j^* = \alpha_j^*(\bar{Y}_j, Z^*)$, defined implicitly by $Z^* = Z_j(\bar{Y}_j, \alpha_j^*)$. If the center sets $\alpha_j^*$ then it will assure that, under decentralization, each municipality chooses the national poverty line, $Z^*$. (In the numerical example in Table 1, local cost shares of 0.37 and 0.73 for the poor and rich regions respectively will induce them to choose the center’s preferred spending levels under decentralization.) Note that when the center has set the cost shares $\alpha_j^*, j=1,...,n$, there will be no municipal income effect on the poverty lines.

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14 By contrast, “vertical inequality” here refers to differences to differences in transfer receipts between individuals at different levels of income (irrespective of where they live).
However, the data requirements for such a cost-sharing formula are far from innocuous. The function $Z_j(.)$ varies across jurisdictions according to the distribution of income as well as any idiosyncratic factors in preferences. Indeed, with less information than is needed to work out the $\alpha_j^*$s, the center could impose its ideal program at local level. This suggests that the cost-sharing arrangements found in practice may be subject to severe information and computational constraints on the extent to which the biases against poor areas can in fact be eliminated.

The rest of this paper will explore these issues in the context of China’s Di Bao program. After describing the data in the next section, section 4 examines the municipal income effect on program spending, and implements the decomposition into a “needs effect” and “resources effect,” as defined above. The key finding is that a strong local resources effect (operating through the setting of local eligibility criteria) is essentially neutralizing the program’s ability to reach poor municipalities.

These findings raise two further empirical issues taken up in sections 5 and 6. The first concerns the implications for the program’s overall goal of reducing urban poverty; section 5 shows that the resources effect attenuated the scheme’s overall impact on poverty, but that this effect was quantitatively small; incomplete coverage and too low a benefit level were the more important reasons for the program’s low overall impact on poverty. The second issue concerns the implications for horizontal equity; consistent with the arguments above, section 6 shows that the decentralization of eligibility criteria generated considerable horizontal inequity, whereby the poor living in relatively rich cities received more help from the program than otherwise identical families in poor cities.

3. Data

The present empirical analysis is based on two data sources. The first is the available set of (both published and unpublished) administrative records for the program. Most importantly, the administrative records provided the data on the local poverty lines, which could be mapped to the city-level for the largest 35 municipalities, which are the setting for this study. However, independent data on DB spending at municipal level was not available.

The second data source is China’s Urban Household Short Survey (UHSS) for 2003/04. UHSS was done by the Urban Household Survey Division of the National Bureau of Statistics.
(NBS) as a first step in constructing the sample for the regular Urban Household Survey (UHS), which has a much longer questionnaire, but much smaller sample size. This paper uses the UHSS sample for the 35 largest cities, giving a total sample of 76,000 households. The big advantage of the UHSS over alternative survey data sets in this context is that its large sample size allows it to be representative at the level of each of the 35 largest cities; the sample sizes vary from 450 (in Shenzhen) to 12,000 (in Beijing). Thus one can expect to be able to make reasonably reliable inter-city comparisons, though (of course) sampling and non-sampling errors must still be expected. For the 35 cities with adequate sample sizes, the definitions of geographic areas in the UHSS also coincide exactly with those for the DB lines. The entire data set has been cleaned by NBS staff and made available for this research. While the UHSS is a relatively short survey, it allows us to measure a fairly wide range of household characteristics including income. Chen et al. (2006) describe the survey data in greater detail. Table 2 gives summary statistics by city. Note that UHSS did not exist in time for using it in the design of the DB program. In particular, DB lines had been set prior to the survey.

Five data problems are notable. First, the urban surveys done by NBS are thought to under-represent the urban poor, notably the “floating population”—rural migrants to urban who still have rural registration. This problem arose from the fact that the sample frame of the NBS surveys was based on registration-based, rather than based on street addresses. This problem has become less serious since street-address sampling was introduced into the urban surveys after 2002, but it is thought by some observers that a bias remains. The problem is of less concern in the present context, given that rural migrants are not eligible for the program.

Second, the survey measured household income from responses to the single question “What is your household’s total income?” (although respondents were also asked how much of their income comes from wages). Responses to this question are unlikely to give as accurate a measure of income as obtained from surveys that base the income aggregates on many detailed questions, such as NBS’s UHS, although this survey is too small for city-level analysis. To some extent, the measurement errors will average out at city level, but errors are still to be expected. I will note some implications of this problem along the way, and provide some robustness tests.

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; a further problem is that the bulk of the UHSS data outside the 35 cities has not been cleaned.
Third, there is no municipal cost-of-living index for China. The DB lines may well reflect (at least in part) cost-of-living differences. I will discuss the likely biases due to this problem, and argue that the main results are robust.

Fourth, given that municipal program data were not available, I have no choice but to estimate program spending by the survey responses on income received from the program. This excludes administrative costs. But probably more worrying is that self-reported DB receipts are likely to be measured with error. If these are classical (white-noise) errors then they will lower the explanatory power of the regressions reported below, but not create biases. However, one cannot rule out the possibility of non-classical errors. I will comment on the implications.

Finally, the fact that this is a single cross-sectional survey limits the possibilities for allowing for behavioral responses at household level to DB payments (such as through effects on labor supply). Chen et al. (2006) provide a number of tests for behavioral responses, which do not suggest they are present to any significant degree, although the lack of longitudinal data limits the power of these tests. In measuring poverty impacts of the program I shall assume that the income gain is the DB transfer payment.

Related to these data concerns, there is an issue of whether, in studying city-level income effects on DB spending, one should use (gross) mean income of a city or mean income net of DB payments. Net income is only the obvious choice if measurement errors are ignored. Given that gross income is obtained from a single question on income, it is unclear whether all income sources are properly accounted for in the households’ responses. And the problems in measuring net income are compounded by the likely measurement errors in self-reported transfer receipts form DB. Under these conditions, subtracting mean DB spending at city level from mean reported gross income may actually add to the bias in estimating the income effect on spending due to measurement errors. In the following analysis I tried both net income and gross income, and found that the choice made negligible difference (given the size of DB payments). The city level results reported in the next two sections use gross income.

4. Cross-city evidence for the Di Bao program

The survey-based incomes and recorded DB payments do not suggest that the Di Bao program is working in practice as was intended by the program’s design. This is evident in Figure 1, which compares the estimated DB gaps (distance below the DB line as a proportion of
the DB line) with DB spending levels across municipalities (also normalized by the DB line). If the program worked the way it was designed and incomes were measured accurately then there should be a perfect positive linear relationship; instead we find a small negative correlation \( r = -0.20 \). However, there is undoubtedly considerable noise in Figure 1 due to measurement errors in both the estimated DB gaps and in DB spending based on self-reported DB receipts.

The model in section 2 showed that, if the program worked in practice the way its design intended, then the income effect on program spending is the net outcome of two opposing effects, the needs effect (whereby poorer municipalities have a greater poverty problem to be addressed) and the resources effect (whereby poorer municipalities have fewer resources for covering their share of the cost). The relative strength of these two effects depends on design features of the program and the objectives of local agents. Given that the program does not appear to be working as it was intended, there may well be other sources of municipal income effects on program spending, such as differences in administrative capabilities and the possibility of an income effect on the locally optimal level of redistribution; for example, under certain conditions poorer provinces will be less effective in targeting their poor (Ravallion, 1999). I will be able to introduce a richer set of potential covariates for DB participation using the micro data (section 6), but for now I focus on the inter-city relationship between DB spending and mean income.

Let us first consider the bivariate relationship. Across the 35 cities, the regression coefficient of log DB spending per capita on log mean income is -0.220, but it is not significantly different from zero \( (t=-0.66) \). Figure 2 plots the data. (The correlation coefficient is -0.098.) If one drops the richest city, Shenzhen, then the estimated income elasticity falls to -0.150 \( (t=-0.31) \). There is also a strong positive income effect on DB expenditure per DB recipient, which has an elasticity of about unity to city income; the regression coefficient of the log DB payment per recipient on log mean income of the city is 0.977 \( (t=5.18) \).

In theory, the level of DB spending should also vary according to the chosen DB poverty line and differences in the distribution of incomes (Section 2). To allow for distributional effects I used the standard deviation of incomes within each municipality.\(^{17}\) I initially used a cubic in log \( Z \), but the higher-order terms were individually and jointly insignificant (prob. values around 0.16). All t-ratios in this paper are based on White standard errors, corrected for heteroscedasticity. With only 35 observations, there are limits to how many distributional parameters one can allow for. I also tried the coefficient of variation, but the standard deviation fitted the data better.
0.5), so I opted for the following regression of log DB spending per capita ($S$) on both log mean income, the standard deviation ($SD$) and the log DB line:\^1^8

$$\ln S_j = 9.443 - 2.386 \ln \bar{V}_j + 0.113 SD_j + 1.720 \ln Z_j + \hat{\epsilon}_j, \quad R^2=0.147; \quad n=35 \quad (11)$$

(The estimates changed very little on dropping Shenzhen.)

Equation (11) suggests the presence of both the needs effect (a lower mean income and more unequal distribution generate higher spending at a given DB line) and the resources effect (through the choice of the DB line). Recalling the theoretical analysis in section 2, the total income elasticity of spending combines three effects: a direct needs effect, an effect via the variance of incomes (the distributional effect) and a resources effect via the DB line. Grouping the former two channels together as the needs effect, it is also of interest to estimate the “partial reduced form” regression of spending on mean income and the DB line:

$$\ln S_j = -0.468 - 0.925 \ln \bar{V}_j + 1.401 \ln Z_j + \hat{\epsilon}_j, \quad R^2=0.075; \quad n=35 \quad (12)$$

On estimating a similar specification for log DB payments per recipient ($S/P$, where $P$ is the DB participation rate) the standard deviation was statistically insignificant ($t=0.27$), so I dropped it giving:

$$\ln (S_j / P_j) = -6.571 + 0.488 \ln \bar{V}_j + 0.971 \ln Z_j + \hat{\epsilon}_j, \quad R^2=0.568; \quad n=35 \quad (13)$$

Notice that the income effect has switched sign in going from (12) to (13). This clearly stems from a negative income effect on DB participation. The estimated elasticity of the DB participation rate to mean income is -1.197 (with a t-ratio of -3.85). Figure 3 plots the relationship found in the data. The elasticity is even higher (in absolute value) if one controls for the DB poverty line; the income elasticity of participation then rises to -1.413 ($t=-3.31$).

At the same time, there is evidence of a strong positive income effect on the DB line. The regression coefficient of the log DB line on log mean income is 0.503, which is significantly different from zero at the 1% level ($t=6.92$) but also significantly less than unity ($t=6.84$). Figure 4 gives the scatter plot. Dropping Shenzhen, the estimated income elasticity of the DB line is 0.579 ($t=8.36$).

\^1^8 The causal interpretation of this regression is questionable given that the DB line is jointly determined with program spending. Nor is there any valid instrumental variable, given that anything that influenced the DB line would presumably also influence spending conditional on the DB line. However, the aim here is only test for a conditional income effect, at a given DB line.
Thus the small total income effect on spending is the outcome of a negative needs effect at a given DB line (an elasticity of about -0.9) and a positive resources effect operating through the local choice of a DB line (an elasticity of 0.704=1.401x0.503, using (12)). On balance, half (=0.971x0.503/0.977) of the income elasticity of DB payments per recipient in equation (13) is attributable to the positive income elasticity of the DB lines.

These regressions assume homogeneity in city size. Against this, there may be fixed administrative costs, yielding scale economies of city size, or congestion effects on the administrative capabilities, yielding diseconomies. While larger cities do tend to have higher mean income, the correlation coefficient is small (the regression coefficient of log population size on log mean income is 0.220, with a t-ratio of 0.51), so only small biases can be expected in estimating the income effects on spending and the DB lines. Controlling for city size, the income elasticity of spending is -0.335, but is still not significantly different from zero (t=-1.08) and the income elasticity of the DB poverty line conditional on city size is 0.493 (t=8.82). In both cases, a significantly positive city-size effect was also evident, controlling for mean income.

The above results are based on the DB payments recorded in the UHSS. As was clear from Figure 1, there are large deviations between the observed levels of DB receipts in the UHSS and the measured poverty gaps. This undoubtedly reflects errors of targeting in the program’s implementation, although measurement errors are probably also playing a role. It is of interest to compare the regressions for recorded DB spending above with the results one would have expected if the program worked as intended and the measurement errors could be treated as white noise. Using the survey-based DB gaps to estimate (1), the analogous results to equations (11) and (12) are:

$$\ln \hat{C}_j = 11.753 - 2.974 \ln \bar{Y}_j + 0.094 SD_j + 2.374 \ln Z_j + \hat{\epsilon}_j \quad R^2=0.486 \quad (14.1)$$

$$\ln \hat{C}_j = 3.561 - 1.761 \ln \bar{Y}_j + 2.089 \ln Z_j + \hat{\epsilon}_j \quad R^2=0.364 \quad (14.2)$$

Here $\hat{C}_j$ is the cost of filling the DB gap, based on income net of DB payments. On balance (factoring in the income effect on the DB line) the estimated total income elasticity is negative and significant (-0.710, t=-2.95). The income gradient in the DB gaps is larger than found in the recorded DB payments.

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19 I also tried squared and cubed terms in log Z but these were (highly) insignificant.
These regressions imply that if the program had in fact filled the DB gaps, as intended, then we would have seen a negative income gradient, even allowing for positive income effect on the DB eligibility thresholds. The needs effect would have dominated. This suggests that the ways in which the program in practice differed from its intended (theoretical) ideal acted to diminish its efficacy in reaching poor areas, by enhancing the relative importance of the resources effect on spending.

However, one should be cautious about the regressions (11)-(14). The errors in measuring DB spending based on self-reported DB receipts in the UHSS will attenuate the income gradient if poor respondents tend to under-state their true DB receipts. And, of course, income measurement errors are still influencing the results (in all these regressions). The net bias is unclear. If one over- (under-) estimates mean income then one is likely to over- (under-) estimate the poverty gap, suggesting that (14.1) and (14.2) overestimate the true income gradient. However, measurement errors in the survey-based data on municipal incomes are likely to create an attenuation bias in the income elasticities of both the DB gaps and the DB poverty line.

One check for bias due to income measurement errors is to assume that these errors do not alter the income ranking of cities, and that the income rank has no independent effect on spending (and so can be excluded from the regression for spending). Under these assumptions, the rank can be used as the IV for measured income. On doing so, the income elasticity of spending rises (becomes more negative). For example, the IVE for equation (12) is:

\[
\hat{\varepsilon}_{1641} = 0.20, \quad \hat{\varepsilon}_{5641} = 0.10, \quad \hat{\varepsilon}_{0841} = 0.08, \quad \hat{\varepsilon}_{1640} = 0.06.
\]

\[
R^2 = 0.043; \quad n=35 \quad (15)
\]

The IVE for the income elasticity of the DB poverty line rises slightly, to 0.530 (t=7.27). On balance, the total income elasticity of spending to rises to -0.416 (from -0.220) but is still not significantly different from zero (t=-0.97). Other results were similarly robust to using income rank as the IV. Bias will remain to the extent that income measurement errors affect the rank order of cities by income.

There is another source of bias in the regressions reported in this section, due to omitted inter-city differences in the cost-of-living (COL). Consider the reduced form income elasticity of DB spending; let the true income elasticity be \( \delta_i \) in:

\[
\ln S_j = -0.610 - 1.564\ln \bar{Y}_j + 2.164\ln Z_j + \hat{\varepsilon}_j \quad R^2=0.043; \quad n=35
\]

The first stage regression (of \( \ln \bar{Y}_j \) on the income rank) had an \( R^2 \) of 0.81.

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20 The first stage regression (of \( \ln \bar{Y}_j \) on the income rank) had an \( R^2 \) of 0.81.
\[
\ln\left(\frac{S_j}{COL_j}\right) = \delta_0 + \delta_1 \ln\left(\frac{\bar{Y}_j}{COL_j}\right) + \nu_j
\]

(16)

where \(COL_j\) is the latent COL index for city \(j\). One estimates instead \(\ln S_j = \delta_0 + \delta_1 \ln \bar{Y}_j + \mu_j\). The bias only goes to zero as \(\delta_1\) goes to unity, or the income elasticity of the cost-of-living goes to zero.

A clue to the extent of this bias can be found in the provincial cost-of-living indices estimated by Brandt and Holz (BH) (2006). These are not ideal; the most recent estimate is for 2000 and they are for all urban areas of a province rather than the 35 cities studied here. The OLS elasticity of the BH urban COL index across provinces to mean (nominal) income across the 35 cities studied here is 0.213 (\(t=6.44\)). Deflating both DB spending and mean incomes by the BH index gives an income elasticity of -0.486; this is higher (in absolute value) than the unadjusted estimate, although it is still not significantly different from zero (\(t=-1.12\)). On re-estimating (11) and (12) using the BH deflators one obtains:

\[
\ln\left(\frac{S_j}{COL_j}\right) = 9.428 - 2.357 \ln\left(\frac{\bar{Y}_j}{COL_j}\right) + 0.117\left(\frac{SD_j}{COL_j}\right) + 1.682 \ln\left(\frac{Z_j}{COL_j}\right) + \hat{\epsilon}_j
\]

\(R^2 = 0.152\) (17.1)

\[
\ln\left(\frac{S_j}{COL_j}\right) = 0.260 - 1.035 \ln\left(\frac{\bar{Y}_j}{COL_j}\right) + 1.433 \ln\left(\frac{Z_j}{COL_j}\right) + \hat{\epsilon}_j
\]

\(R^2 = 0.095\) (17.2)

The results in (11) and (12) are found to be reasonably robust, though the distributional effect is no longer significant at the 5% level.

Ignoring the COL differences probably leads to an overestimation of the true real income gradient of the DB lines, given that the COL is positively correlated with mean income. Using the BH index for the city’s province as the deflator for each city I find that the elasticity of real DB line to mean real income is 0.384 (\(t=4.40\)). The difference is not large; the income elasticity of the DB line falls from about 0.5 to 0.4.

Allowing for cost-of-living differences across cities will probably also yield a higher (real) income gradient in DB participation. That will be the case if the COL has a (positive) income elasticity less than unity (so that cities are not re-ranked in terms of incomes when one adjusts for the cost-of-living differences).\(^{21}\) Again we can find a clue to the extent of the bias if

\(^{21}\) To see why, suppose that the true income elasticity of the DB participation rate is \(\gamma_j\) in:

\[
\ln P_j = \gamma_0 + \gamma_1 \ln\left(\frac{\bar{Y}_j}{COL_j}\right) + \nu_j
\]

while the estimated regression is

\[
\ln \bar{P}_j = \gamma_0 + \gamma_1 \ln \bar{Y}_j + \mu_j
\]
we use the provincial cost-of-living indices estimated by Brandt and Holz. Using the BH index I find that the elasticity of DB participation to mean income rises to -1.410 (t=3.65) (instead of -1.197 using the nominal data). The BH deflators suggest a slightly lower income elasticity of DB payments per recipient of 0.925 (t=4.18) as compared to the unadjusted estimate of 0.977.

In summary, the above results suggest that both the needs effect and resources effects are present, but with roughly offsetting effects. At a given poverty line, richer cities have lower participation rates and spend less on the program (though more per recipient). Although it does not dominate the needs effect, the countervailing resources effect is evident, in that a higher municipal mean income tends to come with a more generous DB line. The resources effect is strong enough to roughly cancel out the needs effect — largely neutralizing the program’s ability to reach poor municipalities.

5. Impacts on poverty

Despite the program’s aim of eliminating urban poverty, the overall impact appears to be modest. In the same sample survey used here, Chen et al. (2006) find that the poverty gap index, based on income net of DB receipts, is 2.28%; on adding in DB payments it only falls to 2.06%. 22 (Amongst participants only, the corresponding figures are 19.92% and 14.23%; the higher index for participants reflects the programs’ targeting to the poor.) The mean poverty gap as a proportion of the DB poverty lines (as given by the poverty gap index divided by the headcount index.) fell from 0.296 to 0.284.

The scheme is under-funded relative to its aim; the population-weighted mean of the DB payment (per DB recipient) as a proportion of the DB line is 0.108 — slightly more than one third of the aggregate DB gap. Furthermore, and despite the excellent targeting performance, the impact on poverty fell well short of the potential, given the budget outlay. The above calculations imply that if all of the payments made under the program had gone to the DB poor

\[ \mu_j = -\gamma_1 \ln \text{COL}_j + \nu_j \]

The OLS estimate of \( \gamma_1 \) converges in large samples to \( \gamma_1(1 - \delta) \)

where \( \delta \) is the elasticity of \( \text{COL}_j \) to \( \overline{y}_j \). Thus one underestimates \( \gamma_1 \) given that \( 1 > \delta > 0 \).

22 The poverty gap index is the mean distance below the poverty line as a proportion of the line (where the mean is taken over the whole population, counting the non-poor as having zero poverty gaps.) The national value of the index is thus the population-weighted mean of \( \frac{C_j}{Z_j} \).
then the aggregate poverty gap would have fallen by 36% (=0.108/0.296), instead of the actual
decline of only 4% (=1-0.284/0.296). The scheme has clearly fallen well short of its potential.

What role has the program’s decentralized eligibility played in this weak overall
performance against poverty? In particular, how much greater would the program’s impact on
poverty have been if all cities had used the same poverty line, set at a level that would have
entailed the same aggregate level of public spending? The previous section studied the
relationship between program spending and the DB line; for notational brevity we can
summarize the empirical relationship by a function \( S_j(Z_j) \) that gives the level of program
spending in city \( j \) when \( Z_j \) is the local DB line. I shall assume that the function \( S_j(.) \) remains
the same for each \( j \) when a single national poverty line is imposed. In other words, the
municipalities behave the same way; all that changes is the poverty line.

What common poverty line would they confront? I define the budget-neutral national
poverty line, \( Z^* \), such that \( ES_j(Z^*) = ES_j(Z_j) \). Thus, given the behavior of municipalities, the
aggregate spending at \( Z^* \) is the same as under the decentralized eligibility thresholds. (Note that
\( Z^* < \bar{Z} \), the mean poverty line, for \( S_j(.) \) strictly concave.) Suppose now that the level of
spending \( S_j \) yields a poverty impact of \( I_j(S_j) \). Define \( \Delta_j \equiv I_j(S_j(Z^*)) - I_j(S_j(Z_j)) \), which is
the impact gain (or loss) in \( j \) induced by the common poverty line. The contribution of
variability in \( Z_j \) to the aggregate impact \( E[I_j(S_j)] \) can then be measured by \( E(\Delta_j) \). Note that,
while \( E[I_j(S_j(\bar{Z}) - I_j(S_j(Z_j)))] > 0 \) if \( I \) is strictly concave in \( Z \), the fact that \( Z^* < \bar{Z} \) implies that
\( E(\Delta_j) \) could be positive or negative.

To implement this measure, we need an estimate of the poverty impact of program
spending. If the program worked exactly as it was intended then program spending itself gives
the reduction in the aggregate poverty gap due to the program. However, although targeting is
excellent, there is still sizeable leakage of benefits to the nonpoor. To allow for this, I postulate
that the program’s actual impact on the poverty gap index depends on program spending as:

\[
\ln(PG_{0i} / PG_{1i}) = \delta_0 + \delta_i \ln S_i + \mu_i
\]

(18)

Here \( PG_{1i} \) and \( PG_{0i} \) are the post-DB and pre-DB values of the poverty-gap index. I tested an
augmented version of this specification with controls for the (log) pre-DB poverty measure but
this was insignificant. I also tested for effects of differences across municipalities in the program’s targeting performance using (alternately) the share of DB benefits going to the poor, the normalized share and the overall concentration index, but none of these were significant (including when interacted with DB spending); this is consistent with the finding of Ravallion (2007) that the program’s poverty impacts are uncorrelated with targeting performance across municipalities. I also tested a specification including DB spending per DB participants and (log) participation rate as separate regressors but I could not reject the null that the coefficients are equal. A squared term in ln$S$ and interaction effects with the pre-DB poverty measure and with the measures of targeting performance also turned out to be insignificant. The only significant effect was an interaction effect between spending and the pre-DB poverty rate, giving the estimated specification:

\[
\ln(PG_{0i}/PG_{1i}) = -0.067 + (0.110 - 0.028 \ln PG_{0i}) \ln S_i + \hat{\mu}_i \quad R^2 = 0.803 \tag{19}
\]

The elasticity of poverty impact with respect to program spending varies from 0.101 to 0.221 with a mean of 0.154 and tends to be lower in poorer municipalities.

We can now quantify how much greater the poverty impact would have been without the variation in DB poverty lines arising from decentralized eligibility. Using equations (11) and (19), the difference between the program’s poverty impact \(\ln(PG_{0i}/PG_{1i})\) at the mean DB line and its value at the actual line of each municipality is \(\hat{\Delta}_i = 1.72(0.11 - 0.028 \ln PG_{0i}) \ln(Z^*/Z_i)\). The value of \(Z^*\) is 2666.3 (as compared to a mean \(Z\) of 2715 from Table 2). The value of \(\hat{\Delta}_i\) varies from -0.06 to 0.06, with a mean of 0.007; by contrast the mean of \(\ln(PG_{0i}/PG_{1i})\) at the actual poverty lines is 0.115.

The upshot of these calculations is that, while the post-DB poverty gap index would be lower without the variation in DB lines (holding total program spending constant), the extra poverty impact is likely to be very small. The more important reason for the program’s low overall impact on poverty is its incomplete coverage of those below the local DB lines and that the DB payments are too low to assure that the DB line is reached. (Recall that the poverty measures reported at the beginning of this section imply that only 12% of the aggregate $Di Bao$

\[\text{The formula is } Z^* = \left[\overline{S}/M(S_j/Z_{j}^{1.72})\right]^{1/1.72} \text{ where } \overline{S} \text{ is (population-weighted) mean spending and } M(.) \text{ denotes the (population-weighted) mean of the term in parentheses.}\]
gap is being filled by the program.) The weak coverage of the poor partly reflects the fact that
non-registered local residents are not covered by the program. The heavy reliance on self-
selection by beneficiaries may also dull the program’s ability to cover all of those eligible.

6. Horizontal inequity across cities

Recall that horizontal inequality is an implication of a positive income effect on the Di Bao
poverty lines across cities (Section 2). To assess the extent of this problem, define a dummy
variable, \( D_i = 1 \) if household \( i \) receives DB and \( D_i = 0 \) if not, and let \( X_i \) be a vector of relevant
“non-income” factors, including location. The probability of participating in DB is:

\[
Pr(D_i = 1) = N[\phi(\bar{Y}_i) + \beta X_i]
\]

(20)

where \( N \) is the standard normal distribution function (so that equation 20 is estimated as a probit)
and \( \phi(.) \) is a parametric nonlinear function; on experimenting with different functional forms, I
chose a quadratic function of \( \ln \bar{Y}_i \), based on the goodness-of-fit.

The \( X \)'s in (20) should include geographic effects, because location can influence living
standards independently of other household characteristics, including income. A complete set of
municipality effects is allowed for, by including 34 dummy variables for the 35 cities (Beijing is
taken to be the reference).\(^{24}\) The vector \( X \) also includes variables related to the dwelling and the
observable characteristics of the household, as might be deemed relevant to local assessments of
“need.” Discussions with MOCA officials indicated that household assets play an important role
independently of income.

The probit estimates of the municipality effects are given in Table 3. Results are given
with and without controls for other non-income household characteristics.\(^{25}\) However, in the
following discussion I shall use the results with those controls.

There is a positive correlation between the municipal effects in Table 3 (the regression
coefficients on the municipal dummy variables) and the DB lines (Figure 5). The regression
coefficient of the municipal effect on the log DB line is 0.903 (t=2.93). From Figure 5, the city
of Kunming is an outlier; possibly the survey has over-sampled DB participants in Kunming.

\(^{24}\) Note that the DB line is constant within municipalities, so a regression coefficient for the DB line
cannot be identified separately from the geographic effects.

\(^{25}\) The coefficients on the extra control variables are omitted to save space. Complete results for the
control variables can be found in Chen et al. (2006).
Dropping Kunming, the regression coefficient rises to 1.001 with a t-ratio of 3.40. However, it is also evident that there are locational factors being captured by the city effects besides differences in the DB lines; the last regression has an $R^2=0.249$. The municipal effects could well be picking up omitted, geographically associated, household characteristics.

While the (unconditional) participation rate falls as city income rises (Section 4), the opposite is true for participation conditional on income and other characteristics. The regression coefficient of the municipal effects on log mean income is 0.502 and is significant at the 2% level ($t=2.52$); if I drop Kunming then the regression coefficient rises to 0.605 ($t=3.30$).\(^{26}\)

These effects remain reasonably robust when one adds the controls for other “non-income” factors (the second specification in Table 3).\(^{27}\) With the full set of controls, the regression coefficient of the municipal effects on the log DB line is 0.709 with a t-ratio of 1.99, which is not quite significant at the 5% level. However, dropping Kunming, the regression coefficient rises to 0.814 with a t-ratio of 2.39. Again, the city effects are quantitatively large.

So one finds that, at given observed household characteristics, the higher the mean income of the city of residence the better one’s chance of accessing the program. The differences in the size of the municipal effects on participation in Table 3 are quantitatively significant. This can be seen if we ask what income difference would compensate for the difference in the city coefficients holding the probability of participation constant. The existence of the quadratic term complicates the calculation, but simply graphing the predicted scores from Table 3 is sufficient to demonstrate the point. Figure 6 gives the predicted scores for selected cities. Consider, for example, one of the richest cities, Shanghai, and one of the poorest, Nanchang (Table 2). It can be seen that, over the interval in which the scores overlap, the compensating difference in log income is about unity. In other words, a household in Shanghai with more than double the income of an observationally identical household in Nanchang would achieve the same probability of participation.

This effect largely operates through the fact that richer cities set higher DB lines. There are no statistically convincing signs that the income effect operates independently of the DB line;

\(^{26}\) As we have noted, data are not available on the inter-city differences in the cost of living. However, by similar reasoning to section 2, it can be argued that this data problem will lead us to underestimate the real income gradient in the conditional city effects on DB participation.

\(^{27}\) The control variables included household demographics, age of head, education attainments, size, age, quality and ownership status of dwelling, selected consumer durables, health status of head, financial assets, occupation and sector dummy variables; details are available from the author.
on including the DB line as a control variable, the regression coefficient of the city effect on log mean income drops to about half its value and is not significantly different from zero.

So there are convincing signs of horizontal inequity in the program. Holding other observed characteristics constant, people in better off cities (in terms of mean income) are more likely to receive help from the program.

7. **Conclusions**

Decentralized implementation of an anti-poverty program relieves the center of the need to identify eligible recipients, which local authorities may well be in a better position to do. However, decentralization has its costs too — costs that may well be hidden from the center. The literature has pointed to concerns about capture by local elites and migration responses to decentralized antipoverty programs.

This paper has focused on another concern, stemming from the fact that the choices made by local authorities in deciding who is eligible need not be consistent with the center’s objectives and will typically be constrained by local resources. Even without local-capture problems, the geographic inequities under decentralization can so diminish a program’s impact that the informational advantage of decentralization becomes a moot point. Furthermore, the information needed for setting corrective cost-sharing or inter-jurisdictional transfers is no less demanding than required for a fully centralized scheme. In short, there is no *a priori* reason to presume that decentralized implementation dominates centrally-imposed eligibility criteria, albeit based on imperfect information.

It is an empirical issue just how much decentralized eligibility attenuates a program’s ability to reach the poor nationally, though there has been very little research on that issue. China’s *Di Bao* program provides an interesting case study. This is an ambitious attempt to eliminate extreme income poverty in urban China, using geographically decentralized implementation of cash transfers aiming to guarantee a minimum income. Each municipality is free to decide who is eligible, by setting its own minimum income.

On combining evidence from an unusually large household survey (representative for each of the 35 largest cities) with administrative data on the poverty lines chosen by local authorities, the paper finds that better-off cities are able to support higher poverty lines for program eligibility and hence higher participation rates at given levels of need. The local
resource constraint greatly diminished the program’s ability to reach poor areas — roughly cancelling the effect of the inter-city differences in need for the program. The overall cross-city income gradient in program spending is still negative, although small and statistically insignificant. The variation in poverty lines associated with the decentralized eligibility criteria attenuated the program’s overall poverty impact, but this effect turns out to be quantitatively small relative to the problems of leakage to ineligible households and (more importantly) incomplete coverage of those eligible.

As a consequence of the income effect on the eligibility thresholds, equally poor families in different cities have very different levels of access to the program, with the poor in poor cities typically faring the worst. This happens even though the center provides some degree of differential cost-sharing favoring poorer municipalities. The extent of this horizontal inequality suggests that it may create incentives for migration by China’s poor. For now, the country’s registration system and the low level of *Di Bao* payments is constraining these incentives for migration. However, looking forward, likely reforms to the registration system (notably to free up the country’s labor markets) and efforts to expand outlays and coverage will probably require a more unified, and horizontally equitable, program of social assistance.
References


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<th>“Rich” region</th>
<th>“Poor” region</th>
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<tbody>
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<td>Population share</td>
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<td>Mean income ($Y$)</td>
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<td>Mean income of the poor ($Y^Z$)</td>
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<td>Spending under centralized program to fill poverty gaps ($C(Z^*)$)</td>
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<td>Center’s share of cost</td>
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**Notes:** (i) Local agent’s welfare function is $400 \ln[\bar{Y}_j - 0.5 \cdot C_j(Z_j)] + C_j(Z_j)$, implying welfare-maximizing spending levels of $C(Z_j) = 2\bar{Y}_j - 400$; (ii) the center’s aggregate spending is $60$ per capita in both cases.
Table 2: Summary statistics by city

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<tr>
<th>City</th>
<th>Mean income (Yuan per person per year)</th>
<th>DB line (Yuan per person per year)</th>
<th>DB participation rate (% pop.)</th>
<th>DB payments per DB recipient (Yuan per person per year)</th>
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Sources: (1), (3) and (4) are calculated from the UHSS, while (2) is from administrative records of the DB program; see section 3 for details.
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Figure 1: Dibao gaps compared to payments

![Graph showing Dibao payments per recipient normalized by Dibao line vs. Dibao gap (pre-Dibao income) normalized by Dibao line.]

Figure 2: *Di Bao* spending plotted against mean income; 35 main urban areas of China

![Graph showing log mean income of city vs. log DB payments per capita. A point labeled 'Shenzhen' is visible.]
Figure 3: The municipal income effect on DB participation

![Graph showing the municipal income effect on DB participation.](image)

Figure 4: Di Bao lines against mean incomes

![Graph showing Di Bao lines against mean incomes.](image)
Figure 5: Municipal-effects on participation in the *Di Bao* program from Table 3 plotted against the *Di Bao* line

Figure 6: Selected city effects on DB participation as a function of income