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Benefit Incidence with Incentive Effects, Measurement Errors and Latent Heterogeneity

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Abstract

Empirical studies of tax and benefit incidence routinely ignore behavioral responses and measurement errors. This paper offers an econometric method of estimating the mean benefit withdrawal rate (marginal tax rate) allowing for incentive effects, measurement errors, and correlated latent heterogeneity in incidence. Under the method's identifying assumptions, a feasible instrumental variables estimator corrects for incentive effects and measurement errors, and provides a bound for the true value when there is correlated incidence heterogeneity. A case study for a large cash transfer program in China indicates that past methods of assessing benefit incidence using either nominal official rates or raw tabulations from survey data are deceptive. The program entails a nominal 100 percent benefit withdrawal rate—a poverty trap. However, the paper finds that the actual rate is much lower, and clearly too low in the light of the literature on optimal income taxation. The paper discusses likely reasons based on the qualitative observations.

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1. Introduction

Most rich countries today have extensive welfare systems for which poverty reduction is an important objective and most emerging middle-income countries are embarking on new social policies with explicit antipoverty objectives. Past discussions of all such policies have fallen into two main camps. According to one camp, the incentive effects of targeted policies are so large that the policies end up creating poverty by discouraging the efforts of poor people to escape poverty by their own means. By contrast, the second camp has largely ignored incentive effects or down-played their importance. At various times, both camps have influenced social policy.

Famously, concerns about incentives were prominent in the early nineteenth century debates on England's Poor Laws, which provided targeted relief to the poor. The Poor Laws went back to around 1600, but their pinnacle was clearly the *Speenhamland System* of 1795, which aimed to guarantee a minimum income through a sliding scale of wage supplements (Himmelfarb, 1984). The view that such policies create poverty was endorsed by prominent classical economists, including Malthus (1806) and Ricardo (1817).[2] Significant reforms to the Poor Laws were implemented in 1834, including the repeal of *Speenhamland*.

While the Poor Laws debate was hugely influential on social policy, the evidence appears to have been largely based on easily manipulated anecdotes and characterizations, with flimsy claims of attribution.[3] The arguments were somewhat one-sided, and many potential economic benefits were ignored, as argued by Solar (1995). Nonetheless, the policy debate soon spread widely and has echoed over the last 200 years. Motivated by the debates on England's Poor Laws in the early 19th century, and influenced by the writings of prominent British economists, similar debates were going on in the US, with calls for reforms to cut the rising cost of relief efforts largely motivated by claims about incentive effects (Klebaner, 1964). In modern times, Murray (1984) and others mounted an influential critique of US welfare policies in which similar concerns about adverse incentive effects loomed large. Moffitt (1992, 2002) and others noted the paucity of good evidence on incentive effects. And, while modern debates on social policy have certainly had more evidence to draw on than was the case in the 19th century debates on the Poor Laws, strong policy positions have persisted independently of the evidence.[4]

[2] For further discussion of this debate in Ravallion (2013).
[3] The ale-house figured prominently in the anecdotes about behavioral responses; see Furniss (1920, Ch.6).
[4] In the context of the 1980s debates on US welfare policy see Ellwood and Summers (1986).

While the first camp emphasizes incentives, often based on rather little evidence, the second camp has essentially ignored such concerns, and with no stronger empirical basis. With the availability of new micro data sets there has been a huge expansion in studies of tax and benefit incidence. It has become routine in such studies to ignore incentive effects, and this is true in countries at all levels of development.[5] Consider what is clearly the most common method of assessing benefit incidence, by which one studies how mean transfer receipts (or tax payments) vary by class intervals of households ranked by their "net income," defined as observed total income minus transfers received and/or taxes paid. Various "targeting measures" are then calculated (as reviewed in Ravallion, 2009b). This method (or some variation on it) is what Bourguignon and Pereira Da Silva (2003, p.9) term the "accounting method." The method has the attraction of simplicity, in that the calculations are straightforward. However, net income (so calculated) need not accord well with income in the absence of intervention given behavioral responses. The potential for bias in assessments of benefit incidence is plain.

Reviews of past literature using the accounting method have warned that incentive effects are being ignored (see, for example, van de Walle, 1998). However, it has been argued that the method provides a "satisfactory short-cut for the study of a policy's distributional impact" (to quote just one example, from Sahn and Younger, 2003, p.29). The main defense is what can be dubbed the fixed-income assumption. This says that people have little ability to influence their income and so incentive effects are minimal. In supporting this view, applications in developed countries have pointed to the relative inflexibility of working hours to justify non-behavioral incidence analysis; see, for example, Kakwani (1986, p.117) in the context of Australia. However, it is unclear that this is plausible even in rich countries, especially given that part-time work has become more common, and the assumption is hardly plausible in developing countries with large informal sectors. While the best examples in the literature are explicit about their assumptions, non-behavioral benefit incidence analyses continue in applied work, and often uncritically. It would be hard to exaggerate the policy influence that these empirical studies have had across the world.[6]

[5] The examples, of which there are many, include Kakwani (1986), Atkinson and Sutherland (1989), Sahn and Younger (2003), Bourguignon et al. (2003), Goni et al. (2011), and Ben-Shalom et al. (2012). Reviews of studies of benefit incidence in developing countries can be found in van de Walle (1998) and Demery (2003).

[6] Coady et al. (2004a,b) and Grosh et al. (2008) survey existing programs in developing countries. Virtually all of the work covered by these meta studies has ignored incentive effects.

One way to better inform these policy discussions is to study behavioral responses directly, such as by looking for labor supply effects.[7] This is certainly of interest, although (especially in a developing country setting) one would need to allow for other sources of such effects in addition to labor supply. (Labor force participation—the extensive margin of the labor supply response—is clearly also relevant, but so too is self-employment, migration and transfer behavior.) However, this approach does not tell us directly about benefit incidence, which is arguably the main thing of interest to policy makers.

This paper focuses instead on the problem of estimating the mean Benefit Withdrawal Rate (BWR), given by the average rate at which transfer receipts respond to differences in household income—the marginal tax rate. This can be interpreted as a measure of targeting performance, telling us how much transfer receipts decline with higher pre-transfer income. Focusing on the BWR also allows us to draw on simulation results from the literature on optimal income taxation in which the marginal tax rate is the key policy parameter of interest.

As is recognized in the literature, the BWR is a key parameter for any social policy.[8] Yet while incentive effects have motivated the calculations of BWRs, they have been routinely ignored in the estimation methods, in common with the benefit incidence literature more broadly. Past methods have either calculated conditional means of actual transfers/taxes at each level of net income or calculated the transfers/taxes implied by the formal rules. Yet behavioral responses are clearly relevant to estimating the BWR by either method. So too are measurement errors in the statistical methods. As Ravallion (2008) argues, what is identified as "imperfect targeting" in social programs could simply reflect such errors. Neither of the two camps described above have paid much attention to this problem. We study the bias in statistical estimates of the BWR induced by latent incentive effects and income measurement errors.

The paper also identifies a third source of bias, which we call correlated incidence heterogeneity. This arises when there are idiosyncratic differences in the BWR, correlated with income. For example, on moral grounds, program administrators in practice may resist cutting benefit levels of the poorest family when its income rises slightly. The extent of this problem will naturally vary with the amount of local administrative discretion in implementation.

[7] Examples of this approach (spanning various approaches) include Atkinson (1995, Chapter 7), Sahn and Alderman (1995), Bingley and Walker (1997), Milligan (2005), Lemieux and Milligan (2008), Skoufias and Di Maro (2008) and Fan (2010).

[8] See, for example, Moffitt (2002), Holt and Romich (2007) and Maag (2012).

The paper proposes an econometric estimator for the mean BWR for an antipoverty program. To assure that our proposed method is operational, we constrain it to use essentially the same data as the statistical accounting method. We do assume that the micro data are available. Our key identifying assumption can be thought of as a more general, and more plausible, version of the fixed income assumption described above, widely used in the literature. We relax the fixed-income assumption to allow only certain income components to be fixed, which become the instrumental variables for total income net of transfers/taxes. Correlated incidence heterogeneity can still leave a bias in our estimator, by creating correlations between the instrumental variables and the error term. We argue that this extra bias can be signed under the assumption that if the data we have are consistent with the program's aim of reducing poverty then the unobserved differences in incidence (stemming from heterogeneity in BWRs) will also be consistent with that objective. In other words, if what we observe indicates that the program reduces poverty then it is assumed that this is also true of the things we do not observe.

These ideas are applied to a major new antipoverty program in China. In an effort to address new concerns about unemployed and vulnerable workers, and the social instability that they might create, the central government introduced the *Minimum Livelihood Guarantee* program, popularly known as the *Dibao* (DB) program, in 1999.[9] The program's design is outlined in various documents of the State Council and it is administered by the Ministry of Civil Affairs (MOCA). By 2009 the program covered 23 million people.[10]

This is China's version of *Speenhamland*.[11] The DB program aims to provide locally-registered urban households with an income per person below predetermined local DB "poverty lines" (*Dibao xian*) with a transfer payment sufficient to bring their incomes up to that line.[12] So this is a program for which one's prior, based on the scheme's deign, would be that there are large incentive effects. Indeed, taken literally, the program's design implies that participants face a 100% marginal tax rate in that a small increase in non-program income will result in an equal reduction in program receipts. Incentives to escape poverty will be weak or absent.

[9] *Dibao* started in Shanghai in 1993, spread to other cities, and became a national policy in 1997, with formal State Council regulations issued in 1999. On the history and politics of the program see Hammond (2009, 2011).
[10] In 2007 a new rural version of the *Dibao* program emerged. World Bank (2010) studies this program in its early stages in four provinces.
[11] It is also reminiscent of Britain's Supplementary Benefit introduced after the Second World War, whereby income top-ups aimed to assure that all incomes reached the poverty line.
[12] 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.

However, there are many reasons why the actual BWRs on an antipoverty program may differ from the nominal rate.[13] While the State Council's proclamations imply a BWR on DB of 100% there is scope for local discretion and innovation.[14] Our results suggest that the way the DB program operates in practice through its local-level implementation greatly attenuates the incentive effects implied by its formal design. Thus the official nominal rules appear to be highly deceptive about actual incidence. While in theory, DB imposes a 100% marginal tax rate on participants, the reality on the ground is a much lower rate. Using our data on DB participants and a matched comparison group of non-participants, we estimate that the BWR is only about 12-14% per annum. We find a higher BWR in richer cities, peaking at 27% for Beijing. It appears that (even in Beijing) the incentives built into the program as it works in practice are unlikely to create a poverty trap. Indeed, when viewed in the light of the literature on the optimal design of targeted programs, the program's BWR appears to be too low.

The following section examines the problems of estimating the BWR and describes our solution. Section 3 describes the *Dibao* program and our data. Section 4 presents our results, also comparing our estimate of the BWR with the non-behavioral method. We then offer some observations on the implications of our findings in Section 5. Section 6 concludes.

2. Theory and methods of estimating the benefit withdrawal rate

One can define the "benefit incidence" of a specific set of transfers (or taxes) as the mapping from incomes in the absence to those transfers to the transfer payments received. With little loss of generality we can think of this mapping as some unknown smooth function giving the transfer to household *i*, denoted T_i, with income in the absence of transfers Y_i^*; let this function be $\phi_i(Y_i^*)$. Note that the function varies, allowing transfer receipts to vary at a given Y_i^*. (A special case identifies the benefit incidence as the conditional mean transfer received at given income, i.e., the regression function of T_i on Y_i^*.) We can then define the BWR as the local slope of the tangent to $\phi_i(Y_i^*)$, as given by $-\beta_i$ in the equation for the tangent:[15]

$$T_i = \alpha_i + \beta_i Y_i^* \qquad (1)$$

[13] Moffitt (2002) makes this point in the context of welfare policies in the US.
[14] This has been noted by Hammond (2009, 2011) and Duckett and Carrillo (2011).
[15] Notice that there is no error term in this equation since it holds by definition.

To illustrate, consider the extremes of "perfect targeting" and "no targeting." With perfect targeting everyone is brought up to a minimum income level, *Z*, depending on their current income, i.e., $-\beta_i = 1$ for all *i* for which $Y_i^* \leq Z$ and $T_i = 0$ otherwise. Such transfers will protect from income poverty, but create a poverty trap. Without rationing by the government, the cost of the program can be expected to rise above the aggregate poverty gap. On top of the fact that those receiving payments will have little or no incentive to work or acquire income from some other source, at least some of those not initially eligible (because their income exceeds the poverty line) will see an attractive income-leisure trade-off in that they could have only slightly lower income by working less and so becoming eligible.

At the other extreme, a "basic income scheme" provides a fixed cash transfer to every person, whether poor or not, i.e., $T_i = Z$ ($\beta_i = 0$) for all *i*.[16] This idea has spanned policy-oriented discussions in both rich and poor countries.[17] There are no incentive effects of the transfers since there is no action that anyone can take to change their transfer receipts. But nor is there any purposive targeting to poor people. While a basic income is unlikely to alter incentives to work (say), a complete assessment must take account of the method of financing the transfers, and once one allows for financing, the incentive and information issues re-emerge. Proposals in developed countries have typically allowed for financing through a progressive income tax scheme (such as in Meade, 1972), in which case the idea becomes formally similar to the Negative Income Tax (Friedman, 1962), though the modes of administration may differ.

In practice, we expect most programs to fall somewhere between these extremes. Intuitively, we can think about the policy choice as that of setting the trade-off between two main objectives of a social policy: First, the policy can provide insurance, by assuring that current incomes do not fall below some crucial level. Second, the policy can try to help assure that poor people break out of poverty when the opportunity arises. The former aspect can be termed protection, while the second is promotion.[18] The perfect targeting case is clearly good for protection, but bad for promotion. The basic income idea is good for promotion but is unresponsive to shocks and so offers less protection. Any imaginable targeted program will face a trade-off between these two objectives.

[16] This has been called many things including a "poll transfer," "guaranteed income," "citizenship income" and an "unmodified social dividend."
[17] See Meade (1972), Atkinson and Sutherland (1989), Atkinson (1995), Raventós (2007) and Bardhan (2011).
[18] Applying a useful distinction made by Drèze and Sen (1989) and formalized by Ravallion et al., (1995).

How do we determine the BWR is in practice? Here we only attempt to estimate the mean BWR ($-\beta$), so we re-write (1) as:

$$T_i = \alpha + \beta Y_i^* + \varepsilon_i \quad (2)$$

Here $\varepsilon_i = (\beta_i - \beta)Y_i^* + \alpha_i - \alpha$ and $E(\varepsilon_i | Y_i^*) = 0$ by construction. We cannot estimate (2) as a linear regression since Y_i^* is unobserved. Common practice in benefit incidence studies is (in effect) to replace Y_i^* by actual (observed) income net of transfers received from the program. Mean transfer receipts (or tax payments) are then tabulated against net income. A linear "benchmark model" for estimating the BWR consistent with this practice would then entail running an ordinary least squares (OLS) regression, giving transfer receipts conditional on income net of transfers:

$$T_i = \alpha + \beta(Y_i - T_i) + \mu_i \quad (3)$$

Here Y_i denotes the observed (survey-based) total income. Equation (3) can be interpreted as the linear regression corresponding to the long-standard non-behavioral method of assessing benefit incidence, as described in the introduction.

Equation (3) can be derived from (2) by postulating a behavioral model for incomes. As noted in the Introduction, past non-behavioral benefit incidence studies for developed countries have argued that the rigidity of working hours makes an incentive effect on labor supply unlikely (see, for example, Kakwani, 1986). By similar logic, measurement errors are not presumably of much concern. As we have noted, this fixed-income assumption is questionable with reference to all income sources, though defensible for some sources. This is a clue for identification. In particular, we postulate two components of income net of transfers: Component 1 comprises those income sources that are unaffected by the program and measured accurately, while Component 2 comprises sources that are influenced by behavioral responses to the transfers and are also measured with error. Candidates for Component 1 are formal (regular salaried) income and property income while Component 2 includes earnings from casual work (not regular salaried work), self-employment income, and private transfers.

Assuming that transfer receipts displace Component 2 linearly at a rate π_i for household *i* and allowing for classical measurement errors we can write:

$$Y_i = Y_i^* + (1 - \pi_i)T_i + \upsilon_i \quad (0 \le \pi_i \le 1) \quad (4)$$

where $E(\upsilon_i | T_i, Y_i^*) = 0$.[19] If there is no (income-relevant) behavioral response to the program then $\pi_i = 0$ for all *i*, in which case income net of transfers ($Y_i - T_i$) is a valid proxy for Y_i^* (with only measurement error to worry about). At the other extreme, when $\pi_i = 1$, extra transfer income displaces other income one-for-one, i.e., there is no impact of the program. Between these extremes, we can expect that income net of transfer receipts will be affected by the program through its incentive effects such as on labor supply decisions.

With the behavioral model in (4) it can be seen that the estimable model in (3) is related to the theoretical model in (2) through the properties of the error term, which takes the form:

$$\mu_i = \beta\pi_i T_i - \beta\upsilon_i + \varepsilon_i \tag{5}$$

Given this structure to the error term we can readily derive the following expression for the asymptotic value of the OLS regression coefficient:

$$\text{Plim}\,\hat{\beta}_{OLS} = \beta\left[1 - \gamma + \frac{Cov(\pi_i T_i, Y_i - T_i)}{Var(Y_i - T_i)}\right] + \frac{Cov(\varepsilon_i, Y_i - T_i)}{Var(Y_i - T_i)} \tag{6}$$

where $\gamma \equiv Var(\upsilon_i)/Var(Y_i - T_i)$ is the share of the variance in observed net incomes accountable to measurement errors. As equation (6) makes clear, the usual attenuation bias due to measurement errors is augmented by two further sources of bias. First, there is an incentive effect stemming from any correlation between net income and the effect of transfer receipts on other income. Second, there is a source of bias stemming from correlated incidence heterogeneity—specifically a non-zero correlation between the differences in BWRs across households and their net incomes.[20] Let us examine these biases in turn.

If the incentive effect is stronger at higher incomes ($Cov(\pi_i T_i, Y_i - T_i) > 0$) then it will work to offset the attenuation bias. A sufficiently positive correlation between the incentive effect and income would eliminate the bias due to measurement error (although this would be something of a fluke event). A negative correlation would strengthen the attenuation bias. On *a priori* grounds it is unclear what direction of bias is most likely due to incentive effects.

[19] We confine attention to classical measurement errors. More generally, transfer receipts could also be measured with error and this error could well be correlated with the measurement error in incomes, which could either offset the attenuation bias or magnify it (depending on the sign of this correlation).

[20] This is an example of what sometimes called "correlated random effects" in the literature. In the context of impact evaluation this is essentially what Heckman et al.(2006) call "essential heterogeneity."

There is a stronger *a priori* case for signing the second source of bias (on top of that due to measurement errors). If the observable data are consistent with the aim of the program to reduce poverty then it would be reasonable to assume that this is also true of the unobservables; specifically, that the latent variation in the scheme's BWR will be such that poorer households end up with larger transfers, i.e., $Cov(\varepsilon_i, Y_i - T_i) < 0$. We can call this the assumption of latent pro-poor incidence. Under this assumption, OLS will over-estimate the BWR in the absence of either income measurement errors or incentive effects. More generally, the net bias is unclear.

A special case lends itself to a straightforward interpretation. Suppose that both the BWR and the incentive parameter are constant across households. Then it is readily verified that: [21]

$$\text{Plim}\,\hat{\beta}_{OLS} = \frac{\beta(1-\gamma)}{1-\beta\pi} \tag{7}$$

For $\beta < 0$ and $\pi > 0$ it can be seen that OLS will be biased downwards for the BWR, and this holds even without income measurement errors. The two sources of bias work in the same direction leading OLS to underestimate the BWR.

How might the bias be removed? Some common methods using panel data would be ill-advised. For example, notice that the structure of the error term in equation (5) does not suggest that a household fixed effect specification would provide a good estimate since the sources of bias are not constant over time. Indeed, one may well expect an even lower signal-to-noise ratio in such an estimator ($Var(\Delta\upsilon_i)/Var(\Delta Y_i - \Delta T_i) > \gamma$) and (hence) greater bias than for OLS.[22]

We will instead use an Instrumental Variables (IV) estimator under our assumption that both the incentive effects and the measurement errors are confined to Component 2 of income. Recall that Component 1 is assumed to be both measured accurately and not prone to incentive effects of the program. If incentive effects and income measurement errors are the only source of bias (in other words, there is no heterogeneity in the BWR) then these income sources can be used as the IVs for income net of transfers. It is important that Component 1 income is a good predictor of total income to avoid the weak instruments problem; if the IVs are weak then there is no reason to suppose that the IV estimator is less biased than OLS.

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[21] To derive this expression note first that the probability limit of the OLS regression coefficient ($\hat{\beta}_{OLS}$) is (as usual) $\beta + Cov(\mu_i, Y_i - T_i)/Var(Y_i - T_i)$. The second term can be written as $\beta(\pi\hat{\beta}_{OLS} - \gamma)$. On solving we obtain equation (7).

[22] For further discussion of this source of bias in fixed-effects estimators see Deaton (1995).

Correlated incidence heterogeneity can invalidate the IVs. If the BWR varies systematically, then the IVs based on Component 1 are likely to be correlated with the error term through their non-zero covariance with the ε_i's. To help address this concern, we exploit our panel data by using the lagged values of Component 1 income sources as the IVs.

However, while this will go some way toward reducing the bias, we acknowledge that (as in other applications using lagged values as IVs) positive serial correlation in incomes can jeopardize this identification strategy. However, under the assumption above of latent pro-poor incidence, the unobserved differences in transfer receipts will tend to favor poor people. Then we can expect that our IVs are negatively correlated with the error term. Having removed the bias due to incentive effects and measurement errors (under our assumptions), the true value of the mean BWR ($-\beta$) will then be lower than our IV estimate.[23]

By bounding the bias in the IV estimator under our assumption of latent pro-poor incidence we will still be able to draw a robust policy implication in our application to follow.

3. Background and data on China's *Dibao* program

Economic transformation has come with a dramatic change in the nature of social protection in China, which has switched from employer-based security arrangements (the so-called "iron rice bowl") to an increasing role of government at all levels. This has been a policy response to fundamental changes in the urban labor markets, themselves stemming from policy changes (World Bank, 2007, 2010; Ravallion, 2013b). A new form of open unemployment—found in many other economies but new to China—emerged in urban China in the late 20th Century, in the wake of retrenchments of workers in un-profitable state-owned enterprises since the mid-1990s. Not surprisingly, those least able to work, including the disabled or unhealthy, tended to be the first to go. Having previously been protected by their work units, they were now exposed to market forces. Using a 2000 survey, Appleton et al. (2002) found persistently high unemployment amongst retrenched workers, who tended to have fewer skills, less education, poorer health, and to be women and middle-aged. Less secure, less regular forms of part-time work have also become more common in urban China (Solinger, 2002; Park and Cai, 2011).

[23] Note that (as usual) Plim $\hat{\beta}_{IV} = \beta + Cov(Z_i, \mu_i)/Cov(Z_i, Y_i - T_i)$ where Z is the IV.

The *Dibao* program is China's main social policy response. However, not unlike most past policy debates on antipoverty policy in England, Europe and North America (as discussed in the Introduction), there is little or no evidence on the actual incentive effects in practice of this major new social program. The main problem identified in past research on the program is seemingly weak coverage of the eligible participants rather than leakage to ineligible participants (Chen et al., 2008; Gao et al., 2009). However, this assessment ignores incentive effects and measurement errors. Coverage may be weak but expanding coverage would be ill-advised if in fact the program is creating a poverty trap.

The data structure we devised for studying this program is somewhat unusual. Similarly to other researchers, we could not obtain access to the complete micro data for China collected by the National Bureau of Statistics (NBS) Under Chinese law NBS cannot provide those data. So we could not estimate a censored regression model (such as a Tobit model) of DB receipts on a sample of the entire urban population (or even selected provinces). However, with NBS's cooperation, we could obtain a sub-sample of actual and potential DB participants for selected cities and follow up this sample for re-interviewing.

As our sample for estimating the BWR for the DB program we use actual participants or likely participants for seven cities, as given in Table 1, which gives sample sizes and participation rates by city. The seven cities were chosen to span China's main geographic areas as well as representing a range of city sizes. In terms of growth rates, they also span a wide range. In identifying actual or likely DB participants we are explicitly excluding the vast majority of urban Chinese for whom participation in the DB program is unimaginable as they have incomes well beyond the DB poverty lines, and are very unlikely to ever need the program. On *a priori* grounds, DB receipts and (of course) the BWR can be set to zero for them.

We drew our sample from the 2007 Urban Household Short Survey (UHSS) by China's NBS. The UHSS is an unusual survey. It is the first step in constructing the sample for the regular Urban Household Survey (UHS), which has a much longer questionnaire, but much smaller sample. The big advantage of the UHSS here is that its sample size allows us to capture a significant number of DB participants, to be interviewed further. Also, while the UHSS is a relatively short survey, it allows us to measure a fairly wide range of household characteristics including income by source. The UHSS is unlikely to give as accurate a measure of total income as obtained from surveys that use more detailed questions on income by source, such as NBS's

smaller UHS. However, the latter survey includes too few DB households for our purposes. Chen et al. (2006) describe the survey data in greater detail.

The population with which we are concerned is all actual or "potential" participants in a targeted antipoverty program. We can readily identify actual participants (denoted D=1) but potential participants are more difficult. We used a model of participation conditional on covariates X to identify non-participants with a probability of participation greater than some critical value, $P(D_i = 1 \mid X_i) > P^{\min}$. The sample we use is thus all those households who are actual participants in DB in the base year plus a sample of the same size comprising those with the highest predicted probability of participation based on their covariates in that survey round.

We drew samples of all 1,040 *Dibao* participant households and 1,029 "high propensity" non-participants from the 2007 UHSS for seven cities. The high propensity households were those with the highest propensity scores (predicted probabilities) for DB participation in the UHSS, based on probit using a large number of explanatory variables; this was essentially the same probit reported in Chen et al. (2006). These 2069 households were resurveyed in 2009 and 2010. The surveys were done by the Urban Household Survey Division of the NBS. In addition we had numerous informal, open-ended, interviews in 2007, 2009 and 2010 with DB officials (central and local) and DB households in Beijing, Chongqing, Tianshui and Wuhan.

The UHSS measured household income from responses to a series of questions on income by broad categories (formal salary income, business income, casual work, self-employment, private transfers and DB). Measurement errors in the reported incomes must be anticipated although more so for some components than others. It should also be noted that survey-based incomes may differ from income at the time of assignment for DB eligibility. Checks on the latter are done by local authorities/neighborhood committees and there is also a community appeals process. And it should be noted that there is more than one way to assess "income." For example, there are differences in the time period deemed relevant (current income vs. longer-term income). Possibly DB officials use a different time period to the survey.

Table 2 summarizes the overall participation rates and the entry and exit rates for the combined sample of actual (2007) participants and the high-propensity participants. There is significant "stickiness" as indicated by the dominant diagonals in these joint distributions. Over the period 2007-10, we find that a greater number of households left the program than joined. This was confirmed by administrative data from MOCA.

Given the low entry rate from the initial sample of likely participants we will also estimate the mean BWR for sub-samples of participants only, defined as those who were found to have participated in at least one survey round.

4. Estimation results

The simple OLS coefficient of DB receipts on income net of DB implies a BWR of 6.5% (t=5.4) (Table 3). Adding time effects this rises slightly, to 7.2% (t=5.1). We also give results for subsamples of DB participants only. The sub-sample gives similar results to the full sample. So instead of a 100% marginal tax rate these calculations suggest a rate of only around 7%. However, this may well be a large under-estimate, as discussed in Section 2.

Table 3 also gives results for household fixed-effects regressions of DB receipts on income net of DB using our three-year panel. This gives an even lower BWR of 3.1% (t=-3.9) using all households in sample and 4.9% (t=-2.3) using only those households who receiving DB income at least once (Table 3). However, as discussed in Section 2, we expect that greater noise in the changes over time may well be imparting a downward bias in the fixed effects estimator.

The IV results are in Table 4. Recall that the IVs are lagged regular salaried income and lagged property income. These were very significant predictors for net income in the first stage regressions.[24] The IV estimator gives an appreciable higher BWR of 12-14%, roughly double the OLS estimate. As noted in Section 2, we cannot rule out remaining bias due to correlated incidence heterogeneity, stemming from a positive serial correlation in incomes and latent heterogeneity in the BWRs. Under our assumption of a latent pro-poor incidence (consistent with its objective of reducing poverty), the true value of the mean BWR will be no greater than our IV estimate. Although the BWR implied by our IV estimator is about twice the OLS estimate, it is still appreciably lower than the value of 100% implied by the scheme's design.

Table 5 gives both estimates by city. (We only give results for the whole sample given the sample sizes.) The IV estimate of the BWR exceeds OLS for all except Pingliang. We see a marked variation across cities with the IV estimates ranging from 6% to 27%. For the IV estimates (but not OLS) there is a positive correlation between the BWR and mean income (from Table 1); the correlation coefficient is 0.67 which is significant at the 10% level. Of course, with

[24] On the full sample, the F-statistic for the regression of income net of DB on the two IVs was 80.61, with prob.<0.00005; for the participant sub-sample it was 38.03 with prob.<0.00005.

only seven cities one should be cautious here, but it is at least suggestive that richer cities tend to put higher weight on protection. Ravallion (2009a) also shows that richer cities of China tend also to have more generous DB lines. These observations for China are consistent with the idea that the demand for social insurance tends to be greater in richer economies.[25]

We tested a number of variations. In one we allowed for other sources of heterogeneity in DB transfer receipts by adding a quadratic function of the propensity scores for DB participation by regressing on a broad set of covariates (similarly to the covariates used in the probits reported in Chen et al., 2006). The mean BWR was slightly lower (in absolute value), though still significantly different from zero. We also tested for lagged income effects by adding the lagged net income as a regressor; we give these results for the IV estimator in Table 4. There is a small lagged effect, but the overall results are similar.

As an aside, given these findings, it is of obvious interest to ask how much the program helped in protecting China's poor from the Global Financial Crisis of 2009. Some straightforward simulations are suggestive. The top panel of Table 6 gives the actual transitions above and below a fixed poverty line that we set at 1.5 times the local DB line. The second panel gives the results when we set all DB payments to zero in 2009, while the third panel gives the results when we set DB receipts in 2009 to their 2007 values. We see that, despite the low BWR, DB payments still protected some households from falling into poverty during the crisis. Only 32 households fell into poverty (by this definition) between 2007 and 2009. In the absence of DB payments the number would have been 225—11% of the sample. However, in assessing the responsiveness of the scheme to shocks the changes in DB payments are more relevant. Then we see that program had less impact, with 71 households falling into poverty without the changes in DB payments, as compared to the observed count of 32 (Table 5, lower panel).

5. Implications and insights from qualitative observations

We focus on two implications. The first concerns standard (non-behavioral) benefit incidence calculations. As noted in the introduction, past applications have ignored or downplayed behavioral responses, sometimes arguing that their non-behavioral incidence calculations are a good approximation (though it has rarely been clear why). Our results illustrate

[25] This is born out in cross-country evidence on spending on social insurance and also with inter-temporal comparisons for the US (Krueger and Meyer, 2002).

the potential for a large discrepancy between the BWR implied by a scheme's formal rules and its actual implementation. Simulation methods based on nominal rates and rules could be especially deceptive in settings in which there is considerable local discretion in implementation. (We return to this point in the context of DB.)

A higher BWR for a given aggregate transfer and given distribution of income in the absence of the program clearly implies larger transfers to the poorest households. To gauge how much difference this makes, consider the average transfer payment to someone at zero income in the absence of the program ($E(T|Y^* = 0)$)—the intercept in the benefit incidence function. It is readily verified that the derivative of this expected value with respect to the BWR is simply the overall mean income in the absence of the program.[26] This is not, of course, data, but a fair approximation is overall mean income, which is 975 yuan per month for urban China (Table 1). Thus an increase in the estimated BWR of 0.08 (implied by the switch to our IV estimate for the "anytime" participants) yields an increase in the mean transfer payment to someone at zero income of 78 yuan per month, over three-quarters of the mean DB payment. This is clearly a sizeable impact, leading us to question past claims in the literature that using net income (ignoring incentive effects) provides a good approximation.

The second implication concerns reform of the DB program. Here we can draw on past research on BWRs for targeted programs. The BWRs found in practice have spanned a wide range from negative values to 100%.[27] However, a strand of the literature has attempted to identify optimal BWRs. Intuitively, as long as both protection and promotion are valued, the optimal benefit withdrawal rate on a scheme such as *Dibao* is unlikely to be unity, but nor is it likely to be close to zero. Can the range be narrowed further?

Three papers in the literature are especially relevant (all influenced by the seminal paper by Mirrlees, 1971). First, Kanbur, Keen and Tuomala (1994) study the optimal design of a stylized program aiming to minimize a measure of poverty when there is an incentive effect on labor supply, and they come to the conclusion that the optimal BWR would be around 60-70%.[28] By contrast, our preferred IV estimates suggest that the BWR for DB is 12-14%. Even in the

[26] To verify this, note first that $\overline{T} = (\alpha + \beta\overline{Y}^*)P$ where P is the overall participation rate. Then $E(T|Y^* = 0) = (\overline{T}/P) - \beta\overline{Y}^*$ as claimed.

[27] See Moffitt (2002) and Maag et al (2012) with reference to US programs.

[28] Naturally such calculations require functional-form assumptions. Kanbur et al. assume Cobb-Douglas preferences, implying an elasticity of substitution between consumption and leisure of unity.

highest-income city in our study, Beijing, the rate is 27%. And, under our assumption of latent pro-poor incidence, these BWRs overestimate the true values of the mean BWR.

Second, Saez (2002) simulates optimal transfers for a range of parameter combinations for a utilitarian social welfare function (rather than a poverty measure as in Kanbur et al). Under the combinations Saez considers plausible and for a moderate to high aversion to inequality the implied marginal tax rates on the poor are still higher than we find for the DB program.

Third, the same conclusion is reached based on the results of Kanbur and Tuomala (2011) who consider various social welfare objectives, including "Rawlsian" maximin, a poverty reduction objective and "charitable conservatism," whereby one puts positive weight on the non-poor but is indifferent to inequality amongst them. Under their chosen parameterizations, all three objective functions imply marginal tax rates on the poorest half or so of the population that are appreciably higher than we find for the DB program.

So, in the light of these results from the optimal taxation literature, our findings suggest that the BWR for DB is too low—even though its design would suggest the opposite. Incentive arguments do not suggest that the DB program should be perfectly targeted as long as promotion is valued alongside protection. However, our results suggest that DB payments do not respond adequately to changes in household income from other sources. While such a low BWR makes it unlikely that the program would provide a serious disincentive for earning extra income, it raises concerns about how well the program reaches the poorest and how well it adapts to changes in household needs. This raises doubts about how well the program is addressing uninsured risk and transient poverty. In short, adverse incentives do not appear to be a problem in this program, but protection from poverty is a concern.

We can make a number of further observations to help understand this finding, including from our field work. While the design of the scheme suggests that the center puts a high weight on protection, it must rely on local implementing agents and the information provided by actual or potential recipients. Like many social spending programs in China and elsewhere, *Dibao* relies heavily on decentralized implementation. While the national and provincial governments provide guidelines and co-financing (to all except the well-off coastal provinces), the selection of beneficiaries is under municipal control. Each municipality determines its own DB line and finances the transfers in part at least from local resources. Claimants must apply to the local (county-level) MOCA office for DB assistance, and they typically do this through their local

community committee, which administers the program on a day-to-day basis. There is also a community-level vetting process whereby the names of proposed participants are displayed on local notice boards and community members are encouraged to identify any undeserving applicants. However, there is still scope for participants to hide some sources of income, such as transfers from friends or relatives.

Our qualitative observations from our field work suggest that local agents actively "smooth" DB payments and participation. For example, we were told by local MOCA officials that they often allow DB benefits to continue for some period after a participating family finds extra work. Local officials are clearly aware of the incentive problem, and expressed concerns both DB participants becoming too dependent on transfers from the program, with too little incentive to work.[29] There appears to be ample scope for local discretion in implementation so as to provide enhanced work incentives.

The implicit preferences of local officials appear to be closer to a promotion objective than the protection objective of the central government. It is seen as unacceptable at local level to cut DB payments to poor people one knows (possibly quite well) when their income rises. Resistance naturally comes from participants too. And our field work suggested that there can be frictions in the entry of new participants, such as due to costs of finding and obtaining information and checking. Stigma effects cannot be ruled out either.

There are also insights into the findings and implications of some past research on the *Dibao* program. The program has been found to be quite good at avoiding leakage to the non-poor. DB recipients are more likely to be poor and unemployed (Wang, 2007; Chen et al., 2008; Gustafsson and Deng, 2011). Coverage of the poor is clearly the bigger problem. The authorities know this as the "ought to protect, not protecting" (*yingbao weibao*) problem (Hammond, 2009). Despite the program's aims, it is clearly not reaching the majority of those households with a reported income (net of DB) below the DB line (Chen et al., 2008; Chen and Ravallion, 2011). Gao et al. (2009) estimate that the program only reaches half of its intended beneficiaries. Chen et al. (2008) estimate that the program is only covering about one-eighth of the aggregate income gap relative to the DB lines. The benefit levels for retrenched workers are clearly well below

[29] Hammond (2009, p.185) also notes that local officials expressed concerns about dependency on DB based on his field work.

their prior wages. It is the program's weak coverage and low benefit levels the scheme's impact on poverty is modest (Ravallion, 2009b).

This pattern of restricted coverage identified in the literature on DB may well be a sensible response by the authorities to the incentive concerns about the program's design. If indeed the program was creating a poverty trap, then rationing access would be the only way of dealing with the problem without more fundamental reforms in the program's design. Absent such reforms, one would be loath to expand coverage to all who claim eligibility.

While we agree that one should be concerned about expanding coverage if the program is generating a poverty trap, our results do not suggest that this is an important concern in practice. Expanded coverage would probably not entail significant efficiency costs due to the program's incentive effects. The greater concern would appear to be whether the scheme is adjusting flexibly enough to household income shocks to provide adequate protection. That would call for design changes to assure a higher BWR than appears to be the case at present. A combination of such design changes and expanded coverage would be needed to assure greater poverty impact.

6. Conclusions

Past discussions of redistributive policies, including antipoverty programs, have been somewhat polarized between those who emphasize incentive effects and those who ignore them. Most empirical studies of tax and benefit incidence have used either statistical methods based on survey data or simulations (assuming that the policy works as it is designed) to study incidence relative to incomes net of taxes paid or benefits received. As is well recognized in principle, these methods ignore behavioral responses. The statistical method also ignores income measurement errors, although that problem is less well recognized. Nonetheless, these practices largely continue uncritically in applied work on benefit incidence—work that has also had a great deal of policy impact.

We have offered a new applicable approach to estimating average benefit incidence that can be implemented with essentially the same data as prevailing methods, but without ignoring incentive effects and measurement errors. The point of departure from past work is that we focus directly on what is arguably the key policy parameter, namely the benefit withdrawal rate (or marginal tax rate). Our key assumption is that incentive effects and classical measurement errors only impact certain income components but that these still have predictive power for isolating

exogenous variation in total income net of transfers/taxes. This justifies a straightforward instrumental variables estimator for the mean benefit withdrawal rate.

We have also pointed to another distinct source of bias in the standard practice, stemming from correlated differences in the program's idiosyncratic benefit withdrawal rates. Correlated incidence heterogeneity casts doubt on the exclusion restriction for our instrumental variables estimator. Nonetheless, we have argued that, as long as this source of heterogeneity reflects latent pro-poor targeting (consistent with the observed data) then we can interpret our preferred estimate of the mean benefit withdrawal rate as an upper bound to the true value.

To illustrate our approach, we have used a specially designed and commissioned survey to study what is probably the largest cash transfer programs in the world (in terms of coverage), namely China's *Dibao* program. Our results suggest a sizeable bias in the benefit incidence picture that is implied by either the formal rules or the usual statistical practice of calculating conditional means at different net incomes. In the present application, our estimated mean benefit withdrawal rate is much lower than the formal rules suggest, yet about double that implied by the standard statistical approach.

By focusing on the key parameter for policy design, we can also offer some insights for policy reform in the light of the literature on optimal taxation. The central government's design for *Dibao* aims to use means-tested transfers to assure that no registered urban resident has an income below a stipulated "*Dibao* poverty line." In theory this is ideal for protection but bad for promotion given that it imposes a 100% marginal tax rate on poor participants—a poverty trap. However, we find no sign of this in the data. Indeed, if anything the benefit withdrawal rate is too low, implying that the scheme is relatively unresponsive to income changes. Incentives for "promotion" are strong, but performance in "protection" is weak. The reason is found in local implementation practices. Local agents implicitly put a far higher weight on promotion than implied by the central government's design for the scheme (though we find that cities with higher average income tend to put a higher weight on protection).

Our key policy conclusion is that the *Dibao* program is unlikely to provide a strong disincentive for earning extra income among participants. Incentive effects appear to be more serious than presumed by standard non-behavioral incidence analysis but still much less severe than basic incentive theory would suggest given the program's design on paper. Indeed, our

findings suggest that reforms to the program should strive for a higher benefit withdrawal rate in local implementation, alongside expanded coverage.

Of course, this is just one application of our proposed method. We cannot guarantee that the method will work well in other settings. Importantly, it must be plausible that at least some significant income sources can be treated as exogenous and measured accurately. Otherwise, there is a danger of a weak-instruments problem, whereby a low correlation between the instruments and total income artificially inflates the estimated mean benefit withdrawal rate. While the method works well in our setting, it is an empirical question whether it will be applicable in other settings. But that is not hard to test.

Table 1: 2007 UHSS sample for seven cities and 2007 summary data

City	Sample size	Of which DB households	% of DB households	DB spending per capita of participants (Yuan per month)	DB line (Yuan per month)	Mean income (Yuan per person per month)
Beijing	33286	352	1.06	269.29	327.78	1835.00
Shenyang	12080	207	1.71	144.24	244.69	1129.73
Jinan	8000	195	2.44	132.81	249.09	1022.53
Wuhan	4689	205	4.37	116.81	219.38	1054.30
Chongqing	14324	1128	7.87	104.26	178.33	1021.73
Tianshui	912	134	14.69	102.79	148.00	555.50
Pingliang	820	221	26.95	103.89	138.00	531.44
Urban China	493975	16365	3.31	101.74	182.40	974.67

Note: DB spending and poverty line data from MOCA. The last row gives aggregates for all of urban China, not just the seven cities listed.

Table 2: Exit and entry from the *Dibao* program 2007-2010

(a) 2007 as the base year

		DB in 2009?			DB in 2010?		
		No	Yes	Total	No	Yes	Total
DB in 2007?	No	931	98	1,029	820	99	919
	Yes	216	824	1,040	272	675	947
	Total	1,147	922	2,069	1,092	774	1,866

(b) 2009 as the base year

		DB in 2010?		
		No	Yes	Total
DB in 2009?	No	959	59	1,018
	Yes	133	715	848
	Total	1,092	774	1,866

Table 3: Estimated benefit withdrawal rates using OLS

	Whole sample	Participants only
Simple OLS regression	-0.065***	-0.065***
	(0.012)	(0.023)
With year effects	-0.072***	-0.067***
	(0.014)	(0.025)
With both year and household fixed effects	-0.031***	-0.049***
	(0.008)	(0.021)

Note: Regression coefficient of DB receipts on income net of DB. Standard errors in parentheses. ***: Significant at < 1%. "Participants" are defined as those who participated at least once.

Table 4: Instrumental variables regressions using lagged exogenous income sources as IVs

	Whole sample		Participants only	
Income net of DB	-0.1205***	-0.1179***	-0.1446***	-0.1377***
	(0.009)	(0.009)	(0.020)	(0.019)
Lagged income net of DB	n.a.	-0.0022	n.a.	-0.0047***
		(0.010)		(0.001)
No. obs.	3935	3935	2245	2245
F	178.91	95.69	55.73	84.46
(prob.)	(0.000)	(0.000)	(0.000)	(0.000)

Note: BWR estimated by regressing DB payments on income net of DB using lagged formal salary and lagged property income as the IVs. Robust standard errors in parentheses.***: Significant at < 1%.

Table 5: Estimated benefit withdrawal rates by city

Regression coefficient of DB receipts on income net of DB	n	OLS	IV
Beijing	1242	-0.0574***	-0.2727***
		(0.0213)	(0.045)
Shenyang	918	-0.0955***	-0.1817***
		(0.009)	(0.024)
Jinan	856	-0.0718***	-0.0846***
		(0.011)	(0.029)
Wuhan	846	-0.0885***	-0.1438***
		(0.008)	(0.028)
Chongqing	1187	-0.0460***	-0.0633**
		(0.004)	(0.030)
Tianshui	480	-0.0444***	-0.1535***
		(0.016)	(0.056)
Pingliang	475	-0.1227***	-0.1073**
		(0.018)	(0.045)

Note: Whole sample. BWR estimated by regressing DB payments on income net of DB. IV estimates use lagged formal salary and lagged property income as the IVs. Robust standard errors in parentheses. ***: significant at 1%; **significant at 5%; *: Significant at < 10%.

Table 6: Poverty transitions with and without the *Dibao* program

		Actual transitions in 2009		Simulated with DB=0 in 2009		Simulated with Δ DB=0 in 2009	
		Above line	Below line	Above line	Below line	Above line	Below line
2007	Above line	1944	32	1751	225	1905	71
	Below line	84	9	52	41	63	30

Note: The cut-off line is set at 1.5 times the local DB line.

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