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STUDIES ON THE AGRICULTURAL
AND FOOD SECTOR
IN TRANSITION ECONOMIES

Lena Kuhn

THE BRINK OF POVERTY

Efficiency and effectiveness of
targeted social assistance for
poverty reduction in rural China

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THE BRINK OF POVERTY

Efficiency and effectiveness
of targeted social assistance
for poverty reduction in rural China

by Lena Kuhn

The king said, "May I hear from you what the true royal government is?" "Formerly," was the reply, "King Wen's government of Qi was as follows: The husbandmen cultivated for the government one-ninth of the land; the descendants of officers were salaried; at the passes and in the markets, strangers were inspected, but goods were not taxed: there were no prohibitions respecting the ponds and weirs; the wives and children of criminals were not involved in their guilt. There were the old and wifeless, or widowers; the old and husbandless, or widows; the old and childless, or solitaries; the young and fatherless, or orphans – these four classes are the most destitute of the people, and have none to whom they can tell their wants, and King Wen, in the institution of his government with its benevolent action, made them the first objects of his regard [...]." The king said, "O excellent words!" Mencius said, "Since your Majesty deems them excellent, why do you not practice them?" "I have an infirmity," said the king; "I am fond of wealth."

**Mencius to King Hui of Liang; Book Mengzi (ca. 300 BC),
translation by James Legge (1990)**

SUMMARY

Despite economic growth and extensive development projects, many marginalized Chinese rural households remain trapped in poverty. To safeguard basic livelihood and decrease absolute poverty, the Chinese Rural Minimum Living Standard Scheme (RMLSS) provides income transfers to rural households below an absolute income threshold. For budgetary reasons and incentive considerations, policy makers usually limit the target group and strive for strict exclusion of non-eligible beneficiaries. Political economists however have been disagreeing about the significance of the restrictiveness and accuracy of this so-called targeting for the respective program's anti-poverty effect. The dissertation approaches this question by an in-depth analysis of the impact of narrowly targeted financial transfers on poverty reduction in emerging economies along the example of the Chinese RMLSS. Of interest are the accuracy and effectiveness of the RMLSS transfers, but even more the influence of the targeting method and efficiency in reducing rural absolute poverty.

The dissertation focuses on an empirical analysis of the effect of benefit allocation, varying targeting methods, poverty lines and program coverage on poverty reduction of the rural *dibao* scheme. After the first introductory chapter, the second chapter sets the stage by introducing the social assistances system in question and providing specific background information that is necessary for the later, comprehensive evaluation. In Chapter 3, the major targeting mechanisms are analyzed from a theoretical point of view and contrasted with socioeconomic conditions and policy environment found in the Chinese countryside.

For the main quantitative analysis in chapter four, the thesis first measures the benefit incidence of the RMLSS in a nationally representative sample of nearly 5000 rural households. Further, a statistical

approximation to the anti-poverty impact of *dibao* grants is calculated. In a second step, efficiency and effectiveness of other distributional schemes, most prominently geographical targeting, demographic targeting and proxy means testing, are simulated. The simulated distribution schemes are compared with the actual distribution of RMLSS in terms of their benefit-incidence, receiver operating characteristic (ROC) and various poverty indicators. Further, all scenarios are benchmarked against the worst and best cases of random distribution and perfect information scenario. Several simulations finally test the robustness of the compared distribution scheme against changes in other program parameters, for instance the size of the overall budget and the amount of individual transfers.

The results suggest that poverty identification was rather inaccurate at the time of data collection, resulting in a considerable amount of leakage of funds to households above the regional poverty line and exclusion of households below the local poverty line. While mistargeting mostly occurred close to the poverty line, funds were also misappropriated to clearly non-eligible households. Meanwhile, the anti-poverty effect of social transfers was quite low, poverty reduction among the sample households reaching only 2.5%. More comprehensive simulations however showed that the inaccuracy of beneficiary identification was only accountable for a small share for the lack in anti-poverty effect. Moreover, targeting efficiency could only be marginally improved by switching to other targeting methods like demographic targeting or proxy means testing. Much more effective were changes in the volume of program: When transfers were lifted to the actual depth of poverty, the simulated poverty reduction more than quadrupled to 10.8%.

The difficulties of achieving higher targeting efficiency are set out in the fifth chapter of the thesis, in which the reasons for decreased accuracy and anti-poverty impact are traced through a case study conducted by the author in eight Chinese townships. Several elements of institutional analysis are implemented to depict possible factors for the low anti-poverty effect of the *dibao* program. General patterns showed that inefficient targeting was often connected to technical difficulties

of income measurement, nepotism and the relative scarcity of transfer and administrative funds. Poor township finance explained understaffing and patchy monitoring of policy implementation in many of the visited townships. Scarcity of transfer funds seemed to be due to the limitation of transfer quotas, which however could also occur in regions with apparently better financial situation. Quota limitations clearly fostered hardly transparent allocation mechanisms, for instance the preselection of potentially eligible applicants by village leaders or the community, rotation or splitting of funds, tightening eligibility criteria or dissuading villagers from application.

The thesis closes with several policy suggestions. Considering the existing budget constraint, considerable administrative costs of targeted anti-poverty schemes and unsustainable implementation mechanisms, the author suggests employing available resources into universal schemes. For instance, an extension of social security like health insurance or comprehensive pension schemes would already tackle the major causes of poverty. Other measures like intensifying efforts to increase the targeting efficiency, volume or coverage of narrowly targeted income transfers, as suggested by recent government policies, meanwhile do not seem recommendable.

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LIST OF ABBREVIATIONS

AUC	Area under the curve
CFPS	China Family Panel Studies
CGSS	China General Social Survey
CHFS	China Household Finance Survey
CHIP	Chinese Household Income Project
CHNS	China Health and Nutrition Survey
DFA	Discriminant function analysis
FPR	False positive rate
GDP	Gross domestic product
IQR	Interquartile range
LISA	Local indicators of spatial association
MCA	Ministry of Civil Affairs
NIC	Newly industrialized countries
OECD	Organization for Economic Co-operation and Development
OLS	Ordinary least squares
PCA	Principal component analysis
PGI	Poverty gap index
PHR	Poverty headcount ratio
PMT	Proxy-means testing
PW	Piecewise regression
QR	Quantile regression
RMB	Renminbi (Chinese Yuan)
RMLSS	Rural Minimum Living Standard System
ROC	Receiver operating characteristic
TDI	Targeting differential
TPR	True positive rate
WGI	Welfare gap index

1 INTRODUCTION

1.1 MOTIVATION AND STATE OF RESEARCH

Throughout history, governmental welfare systems often served as means to maintain social stability, secure political influence and achieve state legitimation. While a dominant role of religious groups or individual benefactors for poverty alleviation can be an indicator for a weak or fragmented government, lavish state welfare was often perceived as display for a stable and capable state authority: For example in ancient Rome, free or subsidized grain rations were granted to every citizen who was willing to queue up at the state granaries (Rowland 1976). This de facto unconditional basic income became one fundamental column of ancient Rome's domestic policy, providing political power and legitimacy to those magistrates or rulers that were able to guarantee a steady grain supply (Rickman 1980).

Beyond political motives, welfare programs may pursue poverty alleviation or even sustainable poverty reduction. From the theoretical point of view, public welfare can alleviate or prevent chronic or transitory incapability of earning income and thus decrease poverty levels significantly (Subbarao 1997). A meta-study summarized past experience with social assistance programs in several countries and found an anti-poverty impact via improved health and nutrition, improved education and decreased child labor (International Labour Organization 2010). However, in contrast to poverty alleviation, true poverty reduction (i.e. lifting individuals or households above the poverty lines) is far more difficult to achieve and depends strongly on program design. Not always does the cost of a welfare system reflect in proportional and immediate poverty reduction, which exposes state welfare to frequent criticism (see for instance Midgley 2010)

Nevertheless, social systems of modern welfare states offer a wide range of support. Among OECD countries, social expenditure reached 21.6% of the total GDP in 2014 (OECD 2015). In general, social expenditure comprises medical care, sickness leave benefit, unemployment benefits,

old age benefits, benefits for those injured during employment, family benefits, maternity benefits, invalidity benefits and benefits for widows, orphans of deceased breadwinners (International Labour Organization 1952). We can distinguish a) social insurances like unemployment, health or pension insurances, and b) social assistance, non-contributory transfer payments to deprived households. While rising GDPs were followed by expanding social systems also in newly-industrialized countries, the extent of support varies considerably across regions and countries. For instance, expenditure on public social security in the BRICS countries (Brazil, Russia, India, China and South Africa) range from 2.6% in India to 21.3% to Brazil in 2014 (International Labour Organization 2015).

One common characteristic of most modern and historical welfare systems is the restriction of beneficiaries by so-called targeting of transfers. Even where political stakes are high, the considerable cost of universal system may ultimately become overwhelming. As we learn from antique sources, growing urbanization inflated the number of beneficiaries of Roman grain doles to about 300.000 by the reign of Emperor Augustus, corresponding to about 60% of the city's population.¹ At this point, the system put such a strain to the state budget that the rulers attempted to limit the number of beneficiaries. Remarkably, this limitation apparently did not took place by introducing a systematic form of means-testing, but only by excluding non-citizens or drawing lots (Rowland 1976; Rickman 1980; Erdkamp 2013).

Budgetary considerations are not the sole motivator for restricting access to welfare benefits. Under a limited social welfare budget, many policy makers decide for narrow, means-based targeting by selecting only the most needy beneficiaries for transfers. This restriction implies a higher share of benefits attributed to the truly poor and thus intuitively maximize the anti-poverty effect (Sen 1995). Universal transfers, in contrast, lead to a high marginal tax rate and thus high costs of redistribution (see formal explanation in Akerlof 1978). A further argument is the presumed incentive effect of income transfers. Universally provided

1 Based on census data discussed in Turchin and Scheidel (2009).

transfers (i.e. income insurance) are perceived to encourage behavior that promotes long-term poverty, for instance by lowering incentives to provide labor (Lemieux and Milligan 2008). These and similar reflections led to the introduction of the *Poor Law Amendment Act* of 1834, a very restrictive system of state welfare in 19th century Great Britain: Under the impression of rising welfare expenses and Malthus' and Ricardo's works on overpopulation and the effect of social assistance on wage levels, politicians focused on maintaining labor incentives to able-bodies by restricting eligibility to its absolute minimum. In fact, the targeting of welfare transfers was so strict that funds could only be attained by giving up all personal autonomy and subduing to the "poor administration": The so-called workhouses in Victorian England offered a living standard considerably below that of workers of the lowest income, at the same time obliging able-bodied residents to hard labor and mandating the separation of family members into different quarters (Barr 1993). The extremely low levels of transfers and the repelling character of the working houses, vividly depicted in the novels of Charles Dickens, certainly provided a disincentive to rely on state welfare.

Finally, also normative reflections may influence the extent of targeting efforts. For instance (Rothstein 2001) discuss how the "substantial justice" of a system may be questioned with regard to the accountability of the poor for their predicament. Ultimately, the decision for and the extent of targeting also depends on the political attitude of decision makers in the debate over individual vs. communal responsibility, as for instance raised by Oorschot (2002). These considerations may result in various equilibria concerning the size of the redistributive sector and eventually explain different typologies of welfare state regimes (e.g. Korpi and Palme 1998 or Esping-Andersen 1990).

Emerging economies face a particular dilemma in designing their social system: With rising welfare levels, these countries typically experience a divergence of income levels, which generates a geographically less homogenous sphere of potential beneficiaries. This development may have a detrimental effect on the efficiency of inclusive and comprehensive welfare programs. Therefore, many argue that the restriction

of program access and more efficient targeting of social assistance are a necessity (see for instance Milanović 1995). In contrast to developed countries, however, emerging economies often yet lack a comprehensive filing system. Thus, so-called verified means tests along tax records or labor contracts are not possible (Klasen and Lange 2015). Also, financial and administrative capacities are still more limited than in high-income countries. Therefore, this thesis argues that narrowly and individually targeted programs, which require a large amount of information, financial resources and administrative capacity, are a considerable challenge for Newly Industrialized Economies (NIEs). High administrative costs may painfully cut into the available budget, while implementation errors are likely to weaken the performance of the system. The actual anti-poverty impact of narrowly targeted social assistance in emerging economies is therefore of particular interest.

China is one of the emerging economies that put increasing effort in the construction of comprehensive social security. Since the Reform and Opening Movement, tremendous success has been made in reducing poverty levels. The Chinese government is determined to completely remove extreme poverty², which is mostly located in rural areas, by 2020 (State Council 2011). With increased off-farm labor and regular remittances from migrant workers however, income levels have become increasingly heterogeneous also in the country side, so that regional development programs are not perceived anymore as a sufficient solution to the remaining absolute poverty. Therefore, Chinese government has begun to highlight the importance of “precise anti-poverty policy” (*jingzhun fupin* 精准扶贫) to “bite down the tough bone” of remaining poverty (The Economic Daily 2015). This transition from regional universal to narrowly targeted anti-poverty policy is in line with the main argument of this thesis and poses an interesting object to study the challenges of social assistance under transition. In rural areas, one specific program of direct social transfers is intended to alleviate and reduce poverty, the Rural Minimum

2 The current Chinese rural poverty line of 2300 RMB per capita and year is equivalent to about 1.7 PPP- $\$$ daily income. The current international poverty line meanwhile is set at 1.9 PPP- $\$$.

Living Standard System (in the following referred to also by its Chinese acronym, the rural *dibao* 低保). Although ambitious, the rural *dibao*'s actual influence on poverty levels has been doubted. Quantitative studies agree upon that, despite the legislators' efforts and improvements, a considerable amount of program funds is captured by non-eligible household, while an even higher number of eligible households are not covered (Liu 2008; Zhang and Xu 2010; Yi and Zhang 2011).

In summary, focused social assistance is often considered an important tool to sustainable poverty reduction. Also in the Chinese case, policy makers seem determined to eradicate rural poverty among others by the means of selective income transfer programs. In contrast to mere poverty alleviation, the anti-poverty effect of targeted programs depends on administrative and budget constraints, which may not be sufficient in structurally disadvantaged rural areas. To guarantee efficacious and sustainable anti-poverty policy in rural China, not only the design of social assistance, but also its implementation shall be critically analyzed. Following the assumption that individually targeted programs are challenging especially in Newly Industrialized Countries, the targeting mechanism of income transfers shall be in the center of interest. Therefore, this work will examine the connection of different targeting methods on poverty reduction, but also discuss the general suitability of targeted social transfers for poverty reduction as such, since, as Martin Ravallion puts it, "[...] better targeting is not seen as being desirable in its own right, but rather as an instrument for reducing poverty" (Ravallion 2009b: 206).

1.2 LITERATURE REVIEW

Generally, we can differentiate targeting or "tagging" literature into two major strands, a) the political economy of targeting, and b) practical evaluations of the accuracy of various targeting methods and their impact on specific program outputs. In addition, this section features a review of case studies on the rural Chinese social assistance system.

1.2.1 The political economy of targeting

Ever since the expansion of social security in Western industrialized countries, leading economists were in disagreement on the fundamental question of whether targeted welfare should be preferred over universal programs. A debate between Wilbur Cohen and Milton Friedman is a suitable representation of the two conflicting standpoints. Wilbur Cohen argued that, for ensuring broad public support, a social security system should target the whole population. Benefits should be positively related to earnings of the individual household, while the minimum benefit level was to be set at the poverty line (Cohen and Friedman 1972: 12). Friedman meanwhile opted for a much narrower targeting of benefits. In his view, social assistance should be strictly provided according to the poverty level of a household to ensure the pro-poor character of expenditure (Cohen and Friedman 1972: 40–41). A summary of the complete debate over universal vs. selective social welfare can be found in Boston and St John (1999) or Besley and Kanbur (1991). In the following, this work will focus on those arguments that are most closely connected to the expected anti-poverty impact of social assistance.

The most straightforward and most frequently addressed aspect is the efficiency argument. If the transfer budget is assumed to be fixed, the anti-poverty effect of a program intuitively depends on the efficiency in the distribution of the available funds. Under poor allocation of the redistributive budget (i.e. taxes), the outcome may be regressive transfers, a setting under which transfers flow from poor households to other poor households or even to households above the poverty line (e.g. Le Grand 1982; Bertram 1988). Targeted social assistance with a strong pro-poor character meanwhile is expected to result in progressive transfers. Especially for scenarios with high poverty rates it is therefore recommended to choose very narrowly targeted programs for high poverty reduction (see for instance Grosh et al. 2008). In simulations using a fixed budget, Klasen and Lange show that narrow and deep targeting achieves the highest poverty reduction (Klasen and Lange 2015).

The assumption of a fixed budget meanwhile is not uncontested. By comparing social programs worldwide, Korpi and Palme (1998) find a negative relationship between the size of the budget and the degree of low-income targeting. In these cases, the saved budget is not used to increase per-capita transfers, i.e. narrow targeting becomes or remains shallow, leading to low anti-poverty effects among beneficiaries. Ravallion (1999, 2004) compares various country cases and finds that budget cuts result in a faster reduction of spending to the poor than to the non-poor. Apparently, the non-poor are more effective in protecting their interests under contracting budgets.

Also the assumption that narrow targeting by default results in budgetary savings is contested. The main critique is that administrative costs of narrow targeting are neglected. Typically, finer targeting requires more intense selection and monitoring and thus boosts administrative costs, which again cut into the available budget (Besley and Kanbur 1991).

Klasen and Lange (2016) use Bolivian and Indonesian data to compare various targeting settings under the assumption of a more flexible budget. They find that a focus on targeting efficiency does not maximize anti-poverty impact. Settings meanwhile that fix the number of beneficiaries at the level of the actual headcount achieve higher poverty reduction. Ravallion however acknowledges that a potential trade-off of efficiency against effectiveness should not be generalized but depends on individual policy setting (Ravallion 2003). For instance Coady et al. (2004) found 25% of 85 targeted programs to be regressive in a way that random distribution would have achieved better results.

Another issue are monetary and social costs of targeting at the side of recipients. The social stigma of social assistance may deter potential applicants and is certainly intensified by strict monitoring under narrowly targeted programs. Besley and Coate (1992) meanwhile argue that leakage of funds to the non-poor may actually contribute to the stigmatization of recipients. Furthermore, social assistance stigma might be used for efficient self-targeting of applicants and thus more efficient distribution of funds. However, the degree of social stigma depends on social and cultural settings. In the case of China, Zhou (2012) does not find

considerable welfare stigma of social assistance reception. Meanwhile, the same study identifies transaction costs that are connected with program participation as a strong deterrent factor, an argument which is also supported by Besley (1990). In any case, both factors may contribute to decrease monitoring costs of a narrowly targeted transfer system.

Another concern is the political cost of social assistance: Universal systems generally feature a higher marginal tax rate than a targeted system, under which few selected individuals are given a different tax schedule than the general population (Akerlof 1978). Gelbach and Pritchett (2002) draw on game theory to discuss the problem of lacking political support of tax-payers for narrowly targeted transfer programs, to which they contribute, but usually don't benefit from. Therein, they are supported by Donder and Hindriks (1998), who analyze potential voting equilibria over the degree of targeting. They find that tax-payers' support erodes quickly when the size of the targeted population falls below a certain level. Rothstein (2001) however states that the preferences of voters and tax-payers can result in multiple equilibria concerning the level of targeting. A summary of the discussion can for instance be found in Smolensky et al. (1995).

Targeting may also impact the economic cost of social assistance, particularly via work incentives and labor provision. While many studies find empirical support for the decreased labor supply under increased levels of welfare transfers (Erbenova et al. 1998; Börsch-Supan 2000; Krueger and Meyer 2002; Lemieux and Milligan 2008), there is also contrary evidence (i.e. Lubyova and van Ours 1998; Ardington et al. 2009). As Moffit (2002) states, evidence is quite ambivalent such that both negative and positive effects on labor supply are conceivable, depending on beneficiaries' characteristics like gender or age (Kits et al. 2015).

Meanwhile, few empirical works weigh the mentioned effects against each other. Van de Walle (1998) for instance discusses the administrative costs of fine targeting against the higher benefit-incidence of pro-poor policy and recommends a two-pronged approach, combining finely-targeted programs with more general components of social security.

1.2.2 Program evaluations

Among program evaluations, this study differentiates between targeting assessments and impact evaluations. Targeting assessments theoretically and empirically analyze targeting schemes' efficiency in identifying the target population.³ While there are numerous works analyzing specific targeting mechanisms (e.g. Baker and Grosh 1994; Grosh and Baker 1995; Ahmed and Bouis 2002; Bigman and Srinivasan 2002; Conning and Kevane 2002; Rai 2002; Chinsinga 2005; Kidd and Wylde 2011), fewer approaches provide an empirical assessment or comparison of several mechanisms. Most prominently, Coady et al. (2004) provide a thorough theoretical overview and empirical meta-assessment of targeting approaches worldwide. Fizbein and Schady (2009) compare several conditional cash transfer programs with regards to the empirical assessment of benefit allocation. Mills et al. (2015) discuss various targeting methods and mechanisms from a theoretical point of view. Sabates-Wheeler et al. (2014) empirically compare targeting mechanisms, but focus only on categorical mechanisms.

Targeting measures alone however may be misleading. As discussed above, the connection between transfer accuracy and poverty reduction is less clear-cut than one would assume (see also Ravallion 2009b; Klasen and Lange 2016). An extensive strand of literature therefore conducts direct impact evaluations of specific programs, a comprehensive overview of which is for instance given by a meta-analysis published by IEG (2011) and Barrientos et al. (2010). Bastagli et al. (2016) list evaluation of cash-transfer programs, while a review of conditional cash transfer program evaluations is given in the appendix of Fizbein and Schady (2009). Therefore, this study refrains from providing a further extensive list. Generally, existing studies vary in terms of their study design (experimental vs. non-experimental), evaluation criteria (e.g. education, income, consumption, health, nutrition, or labor market participation) and evaluation methods (random control trials in experimental setups,

³ For a very comprehensive list and analysis of existent studies see also Devereux et al. (2015).

difference-in-difference method, regression discontinuity approaches, benefit incidence analysis etc.). Most studies focused on one specific program, comparative assessments featured only rarely, for instance in Fizbein and Schady (2009) or Rawlings and Rubio (2005). The most recent meta-study (Bastagli et al. 2016) identified 44 works assessing the impact of cash-transfer programs on monetary poverty, nine of which concentrate on Foster–Greer–Thorbecke (FGT) measures of poverty and only one being concerned with the role of targeting for program outcomes (Merttens et al. 2015).

1.2.3 Case studies China

Among the mentioned studies focusing on middle-income countries, there are several works on general aspect of poverty and social security in transition economies (Milanović 1995; Hutton and Redmond 2000), however only few contributions focus on theoretical challenges of social assistance in transition or emerging countries (Alexandrova and Braithwaite 2000; Milanović 2000). Social assistance in China is in the center of several academic English-language evaluative studies, the majority of which however focusing on social assistance in urban areas (e.g. Ravallion 2007; Wang 2007; Solinger 2008; Gao et al. 2009; Ravallion 2009a; Gustafsson and Deng 2011; Lu 2013; Ravallion and Chen 2015). A considerable number of Chinese contributions study particular aspects of implementation of the rural system, but usually from a very theoretical point of view and without comprehensive empirical data. Examples are Deng and Wang (2008) or Li and Jiang (2012), who discuss various implementation problems and formulate policy suggestions, or the qualitative analysis of nepotisms in *dibao* distribution by Liu (2013), Guo (2009) or Liu (2008), who reflect on the political role of *dibao* distribution. Zhang and Lou (2006) discuss the demographic categorization of *dibao* recipients, while Liu (2012) focuses on the question of limited funds. Further, media reports provide anecdotic evidence on certain malfunctions of the system, e.g. Zhang and Jiao (2008). Among the few profound empirical

studies are a report by the Asian Development Bank (2010) on local pilots in the early 2000s that was supplemented with an additional publication by the same authors (Zhang et al. 2012). In a short quantitative study, Yi and Zhang (2011) analyze benefit incidence and subjective impact of *dibao* grants in a small sample of rural households. Yang (2012) is mainly concerned with the adequacy of poverty lines and grant level, using published macro-data on rural households an *dibao* expenditure, while Ye (2014) reviews the subjective satisfaction of *dibao* recipients. Quantitative information is also provided in a report by the National Audit Office (2012), which is evaluating the usage of social security funds. A World Bank report from 2011 is one of the few English-language publication to focus on the rural system by evaluating the system's design and implementation in one exemplary province (The World Bank 2011). Liu (2014) traces the intellectual origins of the *dibao* system. The most recent English contribution is Golan et al. (2014), who match household survey data with published administrative data to conduct an impact assessment and analyze targeting efficiency. Li and Ying (2014) are concerned specifically with methodological aspects of targeting *dibao* funds.

1.3 RESEARCH AIM AND OBJECTIVES

As presented above, Chinese policy maker view the targeting of income transfers as decisive factor for the anti-poverty effect of social assistance in rural areas. A strong marker for this political line is the current Chinese social policy of "precise anti-poverty policy", which increases efforts into targeting social assistance to the remaining group of household in absolute poverty. However, the importance of targeting for increased anti-poverty effect remains yet to be confirmed by empirical evidence. It has to be tested whether the anti-poverty effect of rural *dibao* can be increased by more precise targeting or if other requirements have to be met instead or additionally. Thus, it will be necessary to assess not only if a) the current policy actually has an effect on poverty levels, but also b)

whether this effect is influenced by the targeting method, or if there are other, more important factors at play.

In detail, the first aim is to assess whether the current form of social assistance is providing meaningful support to lifting the most deprived stratum of the rural population above the poverty line. To systematically evaluate the current role of social assistance policy for poverty reduction in rural China, several research objectives have to be met. The first objective is to analyze the efficiency of available mechanisms in classifying households along a specific poverty threshold. The second objective is to determine the effectiveness of social assistance transfers in decreasing poverty levels. The Chinese state council (2007: 1) defines the main aim of the social assistance as to “[...] settle nationwide the problem of food and clothing/shelter of the rural poor [...]”. For this assessment, the anti-poverty effect of *dibao* transfers is defined as the absolute or relative number of households that are lifted above a pre-defined poverty line under the assumption of a specific budget limit. Under restricted funds, the effectiveness of anti-poverty programs clearly depends on the efficient deployment of available means, leading to the maximization of the desired output under given inputs⁴. In this context, efficiency of targeting is therefore understood as the accurate identification of eligible household through a specific targeting mechanism. As undesired outputs in the narrow sense one can consider the waste of funds to non-eligible households and the exclusion of eligible households from the system. In the wider sense, undesired outputs may also be incentive costs, social and political costs (Coady et al. 2004: 7–10).

The second aim is to test the general assumption that the method of selecting targets for social assistance transfers is truly the main factor for achieving accurate identification of beneficiaries (efficiency) and thus maximizing the anti-poverty effect (effectiveness) or whether there are other, more important factors. The first objective is to identify potential systematic impediments against these goals from a theoretical

4 According to Drucker (1963), the terms efficiency and effectiveness should be understood as “doing the right things” and “doing things right”, respectively. Thus, an ineffective policy or targeting method could still be implemented efficiently, and vice versa.

perspective. For instance, it shall be assessed whether the design of implementation directives is compatible with the thrust of the program. A further focus is on the compatibility of program implementation with the incentives to those involved in the administration and implementation of these directives. The second objective is to compare the accuracy of poverty classification and the anti-poverty effect of the current and alternative targeting mechanisms. For this comparison, the classification accuracy and the anti-poverty effect of potential targeting schemes shall be contrasted with the results of the earlier evaluation of the current scheme.

Thirdly, this study aims at providing policy options for the further development of the current targeting scheme. Taking into consideration the existent constraints in terms of information and administration, this study will discuss options of how the fundamental dilemma of social assistance in rural China can be overcome. The objectives are to identify key areas for improvement and pointing out concrete political measures to mitigate problems of rural social assistance identified during the analysis. An important aspect therein is to formulate policy suggestions with relevance not only for the exemplary case of rural China, but also for the many newly industrialized countries facing similar challenges concerning information access and administrative capacity.

1.4 RESEARCH APPROACH

For meeting the research aims as presented above, this thesis follows a mixed methods approach as suggested by Creswell (2013). Mixed methods in this context mean to employ data collected with quantitative and qualitative methods either in concurrent or sequential form to analyze a common problem or close aspects of one research question. Described as the “third methodological revolution” (Tashakkori and Teddlie 1998), they supplement qualitative and quantitative analysis methods by integrating information from both sources instead of perceiving them as

antagonistic systems. The approach is especially appealing for applied research and evaluation tasks, where strictly quantitative or qualitative perspectives are often insufficient (Kuckartz 2014). Receiving increased popularity among researcher of various disciplines, mixed-methods approaches have been applied in poverty research as well (Roelen and Camfield 2015).

In fact, the presented research topic has multiple facets and the connected research tasks and questions are diverse. There are some rather clear-cut questions on effectivity and efficiency of different targeting mechanisms, which are approached best with quantitative methods. For assessing the degree of accurately distributed funds, this thesis mainly adopts benefit incidence analysis, which measures the incidence of a program's benefits across income groups at fixed inputs, for instance fixed program expenditure (see Van de Walle 1998a for a general introduction and Lanjouw et al. 1998 for an exemplary application). Further, this study employs ROC-curves (Receiver operating characteristics) as a measure for accuracy of identification of households that are considered poor under different poverty-cut-thresholds (Thompson and Zucchini 1989; Wodon 1997). Poverty impact analysis finally will allow to benchmark different targeting mechanisms against each other with respect to the desired output of poverty reduction. The data for this analysis is taken from two large household datasets with reliable income figures, the Chinese Household Income Project (CHIP) and the China Family Panel Studies (CFPS). Detailed information on the data source for quantitative analysis is provided in section 4.2.

The analysis of underlying problems for deficiencies in efficiency and the formulation of detailed policy suggestion will require additional qualitative evidence. For inquiring into the implementation and intrinsic problems of means testing and community targeting mechanisms in China, this thesis employs an embedded multiple case study as suggested by Yin (2003b). This case study uses techniques of institutional analysis, a tool which can "[...] help the analyst structure thinking about the complex relationships and processes within organizations upon which reforms depend" (The World Bank 2003: 14).

Special focus lies for instance upon performance incentives, access to relevant information, and financial and personal capacity. Generally, institutional analysis requires three types of necessary information:

- 1) Background information on key stakeholders, in this case foremost recipient and non-recipient households in rural villages, village leadership and administrators on several levels of regional administration. This analysis is based on primary data from two waves of closed interviews with households and village leaders in five Chinese provinces conducted in 2012 and 2014. The main sections of interest give information on welfare characteristics of household, *dibao* reception, and some key facts and assessment concerning the implementation of *dibao* policy.
- 2) In-depth interviews with key informants from these stakeholder groups, which were conducted in the form of semi-structured interviews with stakeholders from the original household survey conducted in two focus provinces in 2014. During these interviews, individual assessments concerning the implementation and the accuracy of targeting were of main interest.
- 3) Triangulation and cross-reference with information from secondary data, for instance government policy, regulations and work manuals, press articles, expert interviews or macro data from the Ministry of Civil Affairs. Details on the case study data is featured in section 5.2.

In meeting the research objectives along the described research approach, this study will be structured as follows: After the first introductory chapter, the second chapter sets the stage by providing specific background information, which is necessary for the later evaluation and introducing the social assistances system in question. In Chapter 3, different targeting mechanisms are analyzed from a theoretical point of view. Chapter 4 features the quantitative analysis of targeting efficiency and accuracy under different targeting regimes, while chapter 5 enquires into the reasons for the observed targeting patterns along a case study featuring quantitative and qualitative data. In chapter 6, results are discussed and processed into concrete policy suggestions.

1.5 CONTRIBUTION OF THE THESIS

This thesis aspires to contribute to existent research with respect to its research focus and questions, the data base and technical content. As pointed out during literature review, targeting literature usually provides either a discussion of the political economy of social assistance targeting or empirical analysis of existent social assistance systems. A holistic analysis of underlying reasons for malfunctions and the discussion of alternative targeting methods however are essential for the development of generalizable policy recommendations. The combination of theoretical and empirical evidence is also necessary to not only interpret the impact of a program, but also discuss fundamental questions of political economy over the optimal level of inclusiveness of a social assistance program. This thesis aspires at closing this gap by combining a comprehensive quantitative study on the targeting and impact of social assistance in rural areas of China with a careful theory-based qualitative analysis of the underlying mechanisms.

Unlike many previous studies, this assessment shall be undertaken with respect to theoretical consideration, local socio-cultural and administrative peculiarities and statistical challenges of poverty classifications. Identifying, explaining and offering solutions to the structural problems of social assistance distribution in rural China will also further the discussion of the specific challenges of social assistance system in Newly Industrialized Economies. Moreover, this thesis analyses the suitability of different targeting methods for poverty classification from both theoretical and empirical perspective. At the same time, also the importance of narrowly targeting to poverty reduction as such shall be re-evaluated. Herein, this thesis does not venture the debate whether “targeting matters”, which would culminate in often political and normative discussions over the universality of income transfers and unconditional basic incomes. Instead, it is rather interested in whether targeting method and targeting accuracy matter for poverty reduction in rural China. To solve this question, the thesis will analyze a larger number of classification

approaches, which was not undertaken in previous research to the author's knowledge.

Literature review showed that for the case of Chinese rural social assistance, empirical assessments are rare. Therefore, this thesis puts a strong focus on contrasting theoretical considerations with a large amount of empirical information on social assistance distribution, impact and causes for mistargeting. Specifically, this thesis aspires to shed light onto the object of research by providing original primary data on household, village and township level in addition to existing datasets stemming from large-scale household surveys. Further, the study draws strongly on the knowledge of local experts and involves their expertise into hypotheses building and questionnaire design. Finally, the thesis pays heed to the need for combining quantitative and qualitative empirical evidence in answering the posed research question by selecting a mixed-methods-approach. This methodological approach promises answers concerning both the accuracy and effect of social assistance targeting, but also provides insights into mechanisms and causes of mistargeting, which will be important for formulating well-founded policy suggestions.

While in-sample accuracy of poverty classification or income estimation has been tested frequently, the quality of estimation outside the calibration sample has often been neglected by existing literature on the topic. Therefore, one technical novelty of this thesis is the critical assessment of the accuracy of out-of-sample prediction quality of income estimation. Another technical focus of this thesis is on the potential non-linearity of income estimation in terms of the parameters. For testing the potential non-linearity of estimation parameters at different segments of income distribution, this thesis also conducts semi-parametric spline regression, which is the first application of that kind to the author's knowledge.

2 WELFARE AND SOCIAL ASSISTANCE IN RURAL CHINA

The following chapter will provide some background information on the situation of income and poverty structures in rural China, followed by a brief history of rural social assistance in general and the political framework of the program under review, the Rural Minimum Living Standard System. Thus, this section provides basic information as a starting point for the later theoretical and empirical analysis.

2.1 POVERTY AND INCOME DISTRIBUTION IN CHINA

In China, absolute poverty (defined as an average per capita income of below 1.9 PPP\$) is mainly a rural phenomenon. While absolute poverty in urban areas had been practically eradicated by 2012, the poverty rate in rural areas was still above 10% (see figure 1).

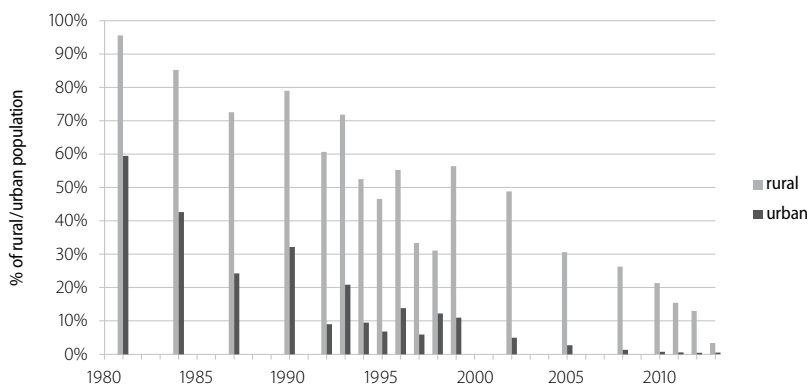


Figure 1: Absolute Poverty in China, 1981–2013 (in % of the total population)

Own illustration. Data source: The World Bank 2014

Note: Poverty line at 1.9 PPP\$ (2011) per day

The reasons for this rural-urban gap lie in the past: Since the initiation of the reform and opening movement in the 1980s, urban wages and living standards have been propelled by economic growth. Rural areas meanwhile could profit to a lesser degree from the economic reforms and growth due to resource constraints and geographical disadvantages. Despite the initiation of development programs since the 1980s, rural poverty and the general educational, health and nutritional gap remained striking (Zhang et al. 2003). Voting down the local development

differences “by feet”, as described by Tiebout (1956), is prevented by the *hukou* (户口) system. This system of residence registration impedes permanent migration especially between the countryside and cities, as citizens need an official registration in the respective location to get full access to the social system, public services and housing markets. An urban registration, however, can usually only be gained through birth or marriage with a registered citizen. With low profits in agriculture and a lack of local job opportunities, especially young people engage in seasonal migration to urban centers, where industry and service sector continue to offer working opportunities even for unqualified labor. In effect, the contribution of agricultural production to per capita household incomes dropped to 34.4% in 2012, compared to values of over 60% in the mid-1980s (National Bureau of Statistics China 2015b).

2.2 THE EMERGENCE OF SOCIAL SECURITY AND SOCIAL ASSISTANCE IN CHINA

First contact with the emerging welfare systems in Western countries during the early 20th century inspired Chinese reformers to reflect on a comprehensive welfare system. Realizing that modernization and economic development brought about massive imbalances in the distribution of wealth, the founding father of Chinese Republic, Sun Yatsen, saw an obligation of a modern state to leveling these inequalities. After the overthrow of monarchy and foundation of the Chinese Republic in 1912, public welfare (*minsheng zhuyi* 民生主义) was included into the “Three Principles of the People”, a cornerstone of political ideology (Sun 1953). However, “public welfare” remained but a political concept and was not yet to be translated into a practical policy. Only after the foundation of the People’s Republic of China in 1949, true efforts into the installment of a social welfare system were taken by the Communists, though mostly

focused on the urban centers. Noteworthy are the introduction of a health insurance and in-kind support for the poorest individuals (Wang 2008).

Massive regional imbalances in recent economic development brought the improvement of rural livelihood into the focus of policy-makers. By definition of the World Bank, poverty reduction work comprises a multitude of infrastructural measures and development of human and social capital, but also individual poverty alleviation measures (The World Bank 2013). While public investment for instance into the improvement of rural infrastructure contributed to decreasing structural deficits, individual factors like high dependency on agricultural production and low education and nutrition deficits continued to hinder socio-economic development (Fan et al. 2002; Glauben et al. 2012; Zhang et al. 2013b). Consequently, China boosted educational expenditure, expenditure in the health system and into social security. In 2013, the biggest components of social security and employment expenditure were transfers to social insurance funds, social assistance and social housing programs (China State Finance Magazine 2014: 312–317). However, with a share of 3.7% of GDP in 2011, Chinese social expenditure was considerably below the average of World Bank upper-middle income countries, which lay at 8.1% in the period 2008–2013 (International Labour Organization 2015).

Comprehensive monetary social assistance in rural areas was one of the last pillars of social security in contemporary China. In fact, it roots in the painful restructuring of Chinese state-owned companies starting in the mid-1990s, during which officially 28.18 million workers of state-owned-enterprises were laid off. Under the pressure of massive surplus labor, some of these laid-off workers failed to find a new job in the formal or informal sector (Naughton 2007). To alleviate the resulting poverty, the government of Shanghai pioneered by introducing an urban welfare system. Other industrial regions that were hit more severely by these structural changes than agricultural regions, soon implemented similar schemes. In 1999, the Central Government decided to adopt the successful pilot as national policy under the name of “Urban Minimum Living Standard System”. Households with rural registration (*hukou*) were not covered by the new welfare system, because farm land and family

support were perceived as sufficient income insurance. In practice, the traditional intergenerational contract often failed due to demographic change and outmigration of young rural population to the cities. Consequently, rural citizens without the physical means to work their plot and lacking support by relatives ended up in desolate circumstances. Several regions therefore introduced similar social assistance programs for rural residents too, yet locally financed and heterogeneous in program design (Zhang and Xu 2010). By 2007, the Rural Minimum Living Standard System (*Nongcun jumin zui di shenghuo baozhang* 农村居民最低生活保障, RMLSS or “rural *dibao*”) was introduced nationwide and replaced or supplemented insufficient local anti-poverty programs and basic national welfare systems (Saunders and Shang 2001; Liu 2014).

2.3 DESIGN OF THE RURAL MINIMUM LIVING STANDARD SYSTEM (RMLSS)

The rural *dibao* program grants monthly transfers to members of poor households, poverty being defined by an absolute threshold on the annual per capita net income. In 2013, the system’s total coverage reached nearly 53.9 million persons, while total expenses added up to \$25 billion (PPP) (Ministry of Civil Affairs 2014b). With a total coverage of about 8.6% of the rural population, the rural *dibao* system exceeded urban social assistance coverage by far (see figure 2).

Generally, the *dibao* program aims at households or individuals who receive a per capita annual net income below a local income line, also called *dibao* standard (*dibao biaozhun* 低保标准), and can’t be supported by family members. In addition to this poverty threshold, regionally defined criteria concerning the absence of household assets (especially those considered luxury assets), lifestyle, and labor potential regulate program admission (State Council 2007).

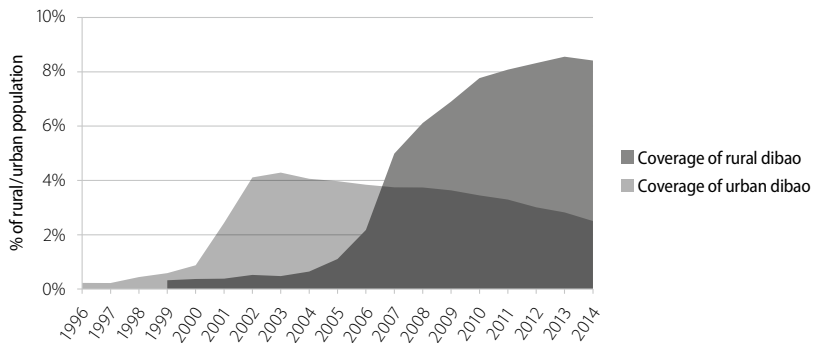


Figure 2: Coverage of urban and rural *dibao*, 1996–2014

Data source: National Bureau of Statistics China 2015a

The responsibilities that come with this program design are divided among different administrative levels. The five official administrative levels of the Chinese government are the central government, provinces, prefectures, counties and townships. Further, rural townships are responsible for administrative villages (*xingzheng cun* 行政村), which consist of several so-called small groups or neighborhoods (*cunmin xiaozu* 村民小组). Even though villages and neighborhoods are not classified as official administrative levels (Chinese Government Web 2014), they are involved in several tasks of the system's implementation.

At the beginning of each *dibao* granting wave (usually annually), households or individuals file an application at the village council or directly at the township office of civil affairs. Information on applicants and the final decision on recipients are published on the village community board to expose it to community evaluation (Zhang et al. 2012: 161). Another form of community control is a process called “democratic discussion” (*minzhu pingyi* 民主评议), in which a council consisting of administrators and villagers’ representatives debates and votes on the validity of the applicants’ claims. The councils’ findings, which are of mere

recommendatory nature, are supposed to be forwarded to township and county offices of the Ministry of Civil Affairs together with the results of direct evaluation and the application itself. The households' statements are verified by officials (usually from township or county level), who inspect the applicant households. Based on their assessment, a final decision on the approval of grants should be made by district- or county-level administrative bodies. This division of tasks is known as the "2–3 system", since reviews and decisions about applications are supposed to be handled only on the 2nd and 3rd administrative levels above the village level (township and county).

After the granting process concludes, funds are transferred to the villagers' accounts, usually accounts opened for the distribution of health insurance, pensions or various agricultural subsidies. If the application is not filed for an individual but a whole household, county government may grant benefits to more than one household member. The sum of transfers per household are not only affected by the number of entitlements (also called "quota") in that household, but also the *dibao* class of the recipient(s): Many provinces classify beneficiaries in *dibao*-classes according to the nature and depth of their poverty, entailing different levels of income transfers. In order to avoid idleness, fraud or crowding out of family support by state transfers, national regulations mandate regular monitoring by township and county administrative staff and, where necessary, the suspension or cuts of grants (State Council 2007; Zhang et al. 2012).

Many details like the sequence of the targeting process, participants of democratic discussion, duration of publication, the level of grants inside each *dibao*-class or the frequency of monitoring are specified at the province, district or even county level, and may vary considerably. First, major regional differences exist concerning the administrative level at which the *dibao* standard is decided. While the municipalities of Beijing and Shanghai both issue uniform standards, ten provinces allow city or county governments to freely determine the *dibao* standard. In five provinces, the provincial government attributes a local standard according to the respective region's development level. In 14 provinces, including this

study's research regions, a minimum *dibao* standard is settled by provincial government, but can be further increased at the local level (Ministry of Civil Affairs 2014b; Sichuan Provincial People's Government 2015). Second, there does not seem to be a uniform method for calculating this *dibao* standard. In practice, it is defined either by i) a commodity-basket-type calculation, ii) as a ratio of the official minimum wage, income or consumption expenditure of that region, iii) according to the rural poverty line issued by the National Bureau of Statistics, or by a mixture of all these methods. Reportedly, this standard is sometimes also adjusted to the fiscal capacity of local government, as lower *dibao* standards imply less recipients and lower expenses (Yang 2011; Zhang et al. 2012). Consequently, considerable variation between *dibao* standards could be observed inside many provinces, most prominently in high-income provinces (Jiangsu, Zhejiang and Guangdong), but also in lower-middle-income provinces where *dibao* standards could be determined locally (Anhui and Sichuan). Third, there are differences in demographic and welfare eligibility criteria. For instance, Sichuan and Shaanxi regulations allow local governments down to the county level to add further items to the list of admission criteria, which not only decreases the system's transparency, but might also be used to reduce the number of potential recipients.

3 CHALLENGES OF WELFARE TARGETING

The following chapter examines the targeting problem from a theoretical point of view. Specifically, it will take a critical look at the targeting methods and their respective strengths and weaknesses, which may impact the accuracy of *dibao* targeting and the efficiency of *dibao* transfers as such. By providing impressions on the structural differences of several targeting mechanisms, this theoretical assessment is the foundation for the subsequent empirical analyses. For the quantitative analysis in section 4, this chapter will provide directions for designing a proxy means testing scenario and hypotheses on the anti-poverty effect of the tested targeting methods. For section 5, this theoretical analysis will deliver working hypotheses on the reasons for mistargeting under the current targeting scheme. However, the section is not only a general introduction into the theoretical background of different targeting approaches, but will also compare the theoretical considerations with realities in the research region. The information for this comparison mostly stems from official macro data and evidence from academic literature and government documentation.

Following the widely acknowledged definition by Coady et al. (2004), there are three main methods to target welfare benefits: Individual assessment, self-selection and categorical methods (see figure 3). Self-targeting occurs under settings where the program design itself creates disincentives to non-targeted households or individuals, for instance through social stigma induced by the application, the inferiority of benefits or a high amount of red tape (Ravallion 2003b; Coady et al. 2004), for which the previously mentioned British *Poor Law Amendment Act* is a striking example. However, since the effect of self-targeting is difficult to be simulated, this option will be excluded from the later analysis. Instead, this study will focus on individual assessment and categorical targeting.

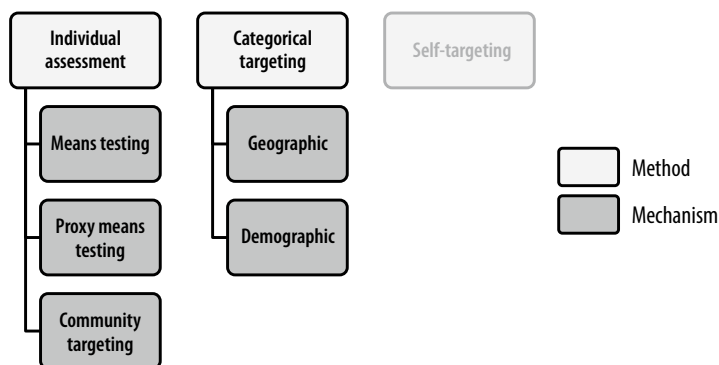


Figure 3: Targeting methods and mechanisms

Source: Coady et al. 2004

Categorical targeting means to select beneficiaries by their affiliation to specific population subsamples. This study focuses on two categorical mechanisms: For **geographic targeting**, all inhabitants of a specific region are selected as program targets, while **demographic targeting** focuses on specific population groups, for instance elderly or single mothers.

With rising income heterogeneity, policy-makers often prefer targeting methods that focus on the eligibility of specific individuals and households by **individual assessment**. For individual assessment, there are three common mechanisms: The most straightforward option, means testing, determines eligibility by directly assessing the relevant household information (for social transfer programs household income or expenditure), but requires high administrative capacity and good documentation of income. For proxy means testing, on the other hand, evaluators try to specify a model predicting the household material welfare by some items proxying household income or expenditure. On basis of the estimated income, eligibility and hence program participation of specific households will be determined. In practice, an estimation model is fitted in a small but nationally representative dataset and thereafter transferred to the population of interest. The detailed methods for estimating income with proxy variables will be introduced at a later point of this study. The third option for individual assessment is community targeting, where community leaders or social workers decide about recipients based on their inside knowledge.

3.1 DIRECT INCOME MEASUREMENT – MEANS TESTING

Means testing refers to the direct assessment of household welfare by surveying all income or expenditure components of a household. On the one hand, direct welfare evaluation provides the most accurate information on the financial means and eligibility of an application. On the other hand, the obvious problem is the difficulty of procuring accurate information on potential recipients' income level (Besley 1990). Income measurement is not only one of the most costly targeting methods (Devereux et al. 2015), but there are also considerable technical requirements for successful implementation of a means-tested program, as pointed out by a World Bank study: First, income has to be well documented and

verifiable, second, the population needs to be generally literate and third, administrative personnel highly qualified for conducting sophisticated income measurement (Coady et al. 2004). Unfortunately, not all of these conditions might be fulfilled in remote rural areas of China. Accordingly to literature, especially three major factors might compromise the accuracy of collected income data: (1) interviewers' own difficulties to cover all possible income sources; (2) respondents' lacking knowledge and/or ability to calculate the total household income, especially if long recall periods are used; (3) the incentive to households to conceal or understate income if they assume that the data might be used for instance for tax collection or program targeting (McKay 2000a: 95; Moore et al. 2000).

One example for implementation of a means-tested social assistance program is the Brazilian *Bolsa Familia* program, which provides cash-transfers for families whose self-reported incomes are below a certain threshold, conditional on their children's school attendance. The general impact of this program seems to be rather impressive, as compared to other conditional cash transfers programs (Rocha and Soares 2010). Meanwhile, among conditional cash-transfer programs in Latin American countries, *Bolsa Familia* did seem most prone to manipulation and fraud by applicants due to its reliance of self-reported income, which was neither documented nor verified by a central authority (Handa and Davis 2006). In the following, this thesis will discuss the relevance of the three mentioned potential weaknesses – definition, collection and reliability of income data – for the Chinese case. In addition, the problem of data falsification shall be addressed shortly.

3.1.1 Definition of income components

As mentioned, one challenge of income measurement is the coverage of all important income components. The Chinese National Bureau of Statistics of China defines rural net income as “[...] the total income of the permanent residents of the rural households during a year after the deduction of the expenses for productive and non-productive business

operation, the payment for taxes and the payment for collective units for their contracted tasks [...]” (National Bureau of Statistics China 2002). In detail, the total income includes wage income, income from business activities, property income and transfer income, but excludes sold goods and income from loans (National Bureau of Statistics China 2011).

In practice, national governments regulations relegate to province governments for determining income components that are relevant for measuring eligibility (State Council 2007). Shaanxi government, for instance, defines a list of relevant income sources, but also leaves room for additional components to be added by district or county regulations (Shaanxi Province Government 2010). County level government further expands the list and excluded certain item. At large, regional definitions correspond to the national definition. One county for instance defines: labor income (including migration labor); income from self-employment; business and management income; income from individual business, rent and lending or sublease business; gains from renting out family assets, interests on loans, dividends or bonuses; allowance or child support from legal supporter, foster persons etc.; inheritances and gifts; allowance for retired workers, discharge allowance, personal injury compensation or livelihood allowance; other optional components (Yan’an Ansai Peoples’ Government 2012). Comparing this list with official guidelines on household income collection, the selection of items seems cogent and consistent with the program’s aims.

The flexibility of the list of items included or excluded for income calculation does not seem to be required by interregional development heterogeneities. However, this flexibility might only be problematic for data collection if it is intentionally manipulated to control the number of beneficiaries or if it creates confusion between the government levels. Thus it seems that the crucial challenge may not be in the definition of income components but rather on whether all items in this list are actually included during data collection. This question shall be approached in the following section.

3.1.2 Quality of data collection

A first condition for income data collection is the transparency and measurability of different income components. Especially farm incomes and in-kind incomes may be difficult to quantify for inexperienced evaluators. Further, income sources with higher variability, for instance business incomes, seem more prone to intentional underreporting than regular wage income (Moore et al. 2000). Indeed, agriculture is still a major income source in the Chinese countryside: In 86.6% of the total 2028 households from the CCAP 2012 household sample, at least one family member was still working on the household plot. In addition, a growing share of off-farm-work can be observed in rural China (Glauben et al. 2008; Wang et al. 2011). Indeed, 24.9% of the households ran their own business, while in 22.2% of the households at least one household member received in-kind salary payments from an off-farm job. All in all, the structure of rural incomes may facilitate underreporting or measurement errors.

Another factor influencing the quality of data collection is the choice of the recall-period: Literature regards twelve months as appropriate reference period for agriculture, where income usually is generated in a lump sum during harvest time. For more regular inflow of wage, business and private transfer income, usually a shorter reference period is chosen to avoid the problem of underestimation and omission of individual components (Vijverberg and Mead 2000: 115; Schaffner 2000: 226–228; McKay 2000b: 289). In accordance to the seasonal character of agricultural income, the eligibility threshold itself is given in per capita household income per year. In *dibao* application forms from the research region, recall periods varied from six months to one year. As long as agriculture is still the main income source, this period seems rather appropriate. However, the share of agricultural incomes has been decreasing rather continuously since the 1980s, without a visible slowing down of this trend (figure 4). This development might necessitate reconsidering the length of the recall period.

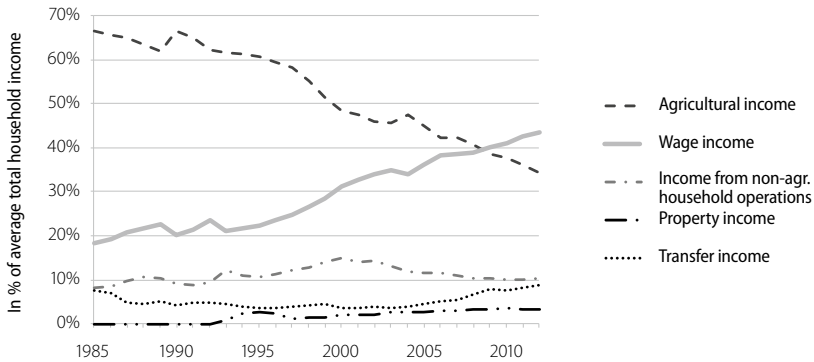


Figure 4: Income composition of rural households, 1985–2012

Own illustration. Data source: National Bureau of Statistics China 2015a

Another pitfall of data collection is the qualification and aptitude of evaluators. Income collection for the rural *dibao* system is mostly implemented by village and township personnel instead of trained and specialized evaluators like in national household surveys. Typically, many administrators on village level and even on township level have a low level of school education, and only a small fraction is actually acquainted with basic statistics (Holz 2002). Other technical challenges contribute to the exclusion of very low and very high incomes even in data from the National Bureau of Statistics, thus leading for instance to understatement of national inequality figures (Bramall 2001).

3.1.3 Reliability of household statements

Concerning income measurement, also the reaction of interviewees towards sensitive questions is an important issue. In fact, interviewees in some cultures were found to be more sensitive to income-related questions than even to inquiries on their sexual behavior (Tourangeau and

Yan 2007). Some types of income may be more prone to underreporting than others: One recent study found that in U.S. household surveys, the self-employed underreport their income by about 30% (Hurst et al. 2015). Similarly, a meta-study by Moore et al. (2000) finds relative low misreporting of wages and salaries, but considerable misreporting of transfer and especially asset income. Especially tax reasons may motivate household members to underreport their income. Gosh and Glewwe (2000) mention that respondents may answer untruthfully if they even suspect a connection to income taxes. In the case of the *dibao* program, the connection between income statements and eligibility for monetary transfers is well known and poses a clear incentive to underreport household income.

In the Chinese case, the incentive given by program design to understate income meets with the opportunity to do so. The first factor is the difficulty to measure and to disclose income from sources other than from documented employment (Moore et al. 2000: 26). While wage income became the most important income source of rural households in 2009, it is still responsible for only 44% of rural incomes. Also property income and transfer income shares are rising (see figure 4). In summary, the growing ratio of less verifiable income components might make it easier for households to conceal important shares of household incomes from administrators.

Also the size of the informal sector will be a serious obstacle in verifying households' statements, as income from informal activities is not monitored by the state and therefore can't be verified. Indeed, the informal sector in China is still large: Summarizing official numbers on urban employment, a 2009 study estimated that in urban centers about 120 million rural migrant workers were informally employed in 2006 (Huang 2009). In another study in six large Chinese cities, 44.5% of the migrant workers were found to be employed in the informal sector. Another 37.3% of those employed in the formal sector were working informal jobs (Park et al. 2012).

Documentation of income, registration of employees for social insurance reasons or disclosure of information on financial accounts can decrease the leeway for tax fraud and income data. Through extensive

targeting and monitoring efforts, only 3.4% of German recipients of minimum living income were found to have concealed part of their income to attain social assistance in 2010 (Bundesagentur für Arbeit 2012). In rural China, the current level of informal labor and lack of income documentation leads us to expect a larger share of fraud in means-testing. Especially for the informal sector and income remittance by family members working abroad, income concealment is likely to be high.

3.1.4 Political interference

Also political interference has to be considered as a potential bias of direct income data collection, which is relevant for the quality of direct income assessment by official sources, but should also be kept in mind for the later discussion of community targeting. Since years, there have been certain doubts about the reliability of official Chinese economic data. According to a diplomatic cable published on Wikileaks in 2007, Prime Minister Li Keqiang himself labeled Chinese GDP data as “[...] ‘man-made’ and therefore unreliable.” In his opinion, all figures except electricity consumption, volume of rail cargo and data on disbursed loans could not be used to assess economic growth but were for reference only, especially the GDP data (Randt 2007). Also among academics, there is a lively discussion on the accuracy of official Chinese macro data, on which Young (2003) provides a comprehensive overview. Further inquiry into this topic seems necessary, as inaccuracies in official macro data might impact the accuracy of poverty thresholds and shed doubt on the integrity of household-level data, which is passed on to higher government levels during the application process. However, it is essential to differentiate between coordinated manipulation by central level and local misreporting of data.

Systematic falsification of aggregate output and growth data on national level has been discussed by Western media, but is mostly rejected by academic studies (e.g. Holz 2003; Chow 2006; He and Sun 2013). Instead, data corruption is assumed to occur on the grass-root level or during transmission along several government levels (Cai 2000;

Koch-Weser 2013). Generally, local falsification of statistical data is not regarded a centrally coordinated process, but seems to happen mostly due to personal incentives of local administrators: The performance of administrators at most levels of government (except village level) is evaluated by a complex system assessing the implementation of state policy and personal performance (Heberer and Trappel 2013). The specific evaluation criteria often prioritize the implementation of growth-enhancing policy and generation of local economic growth (O'Brien and Li 1999). Consequently, some authors argue that biased income or growth data originate mostly in the desire to please higher government levels and polish individual performance records, especially in backward regions (Koch-Weser 2013; Wallace 2016). At the same time, local leaders might also feel implicit or explicit pressure by their superiors to report progress on rural development to higher government levels. Superiors, finally, might be reluctant to point out bogus numbers as not to raise doubt about their own achievements. In fact, the conditionality of careers of local administrators to performance statistics is so strong and data manipulation so common that it formed a figure of speech, "Cadres produces statistics, statistics produce cadres" (*guan chu shuzi, shuzi chu guan* 管出数字, 数字出官) (Cai 2000). Ultimately, numbers might even be boosted on each level of reporting hierarchy, thereby multiplying the reporting bias. In a 2012 sample of village data, five village leaders frankly stated that township level had asked them to report higher average income data. On average, these leaders were required to overstate observed income by 64%. It is therefore rather likely that also in the case of *dibao*, administrators try to gloss over economic underperformance by overstating household income and understating local poverty rates (Center for Chinese Agricultural Policy 2012).

All in all, the collection of reliable income data in rural China is problematic for various reasons, and requires specialized evaluators and a thorough questionnaire design. These requirements may be given for focused or centralized studies, but most likely not for decentralized, casual evaluations by untrained local administrators.

3.2 GEOGRAPHIC TARGETING

As mentioned above, geographic targeting is a viable option if poverty is highly determined by regional fixed effects and thus welfare is expected to be rather homogenous within regions sharing the same geographic effects. For instance poverty maps can help in identifying target regions (e.g. Bigman and Srinivasan 2002). Indeed, theory points to strong effects of geography on local welfare levels. For instance, advocates of geographic poverty traps argue that development might indeed be influenced directly by the effect of climate on health and agricultural productivity and the significance of access to a coastline or rivers for trade and innovation (e.g. Bloom et al. 1998; Gallup and Sachs 1999; Sachs and Malaney 2002). Additionally, remote areas might be at disadvantage whenever a strong political focus on the development of strategic coastal or urban centers is present. In late 19th century, resource extraction from underdeveloped regions fueled development of urban and coastal regions in China, while service provision to more backward regions was neglected, a mechanism described in detail by Pomeranz (1993). Other studies provide empirical indications for certain regional factors in rural China contributing to poverty or preventing households from leaving poverty (Jalan and Ravallion 2002; Wan and Zhou 2005; Olivia et al. 2011; Glauben et al. 2012).

To judge whether geographic targeting of social assistance is truly justifiable, it is necessary to find empirical evidence for a nationwide connection between geographic location and poverty levels. This could be achieved by comparing the welfare levels of regions sharing similar geographic conditions. As argued in the previous chapter, reliable figures on poverty incidence for local levels are hard to come by for practical reasons and due to the highly political character of these figures. To gain some basic understanding of the spatial characteristics of income, instead information on *dibao* reception rates among rural population is used. For assessing regional characteristics of *dibao* reception, *dibao* macro data from 2014 was contrasted with 2010 census data on the number of regionally registered population and regional population growth rates, which were extrapolated to 2014 level by rural population growth rates.

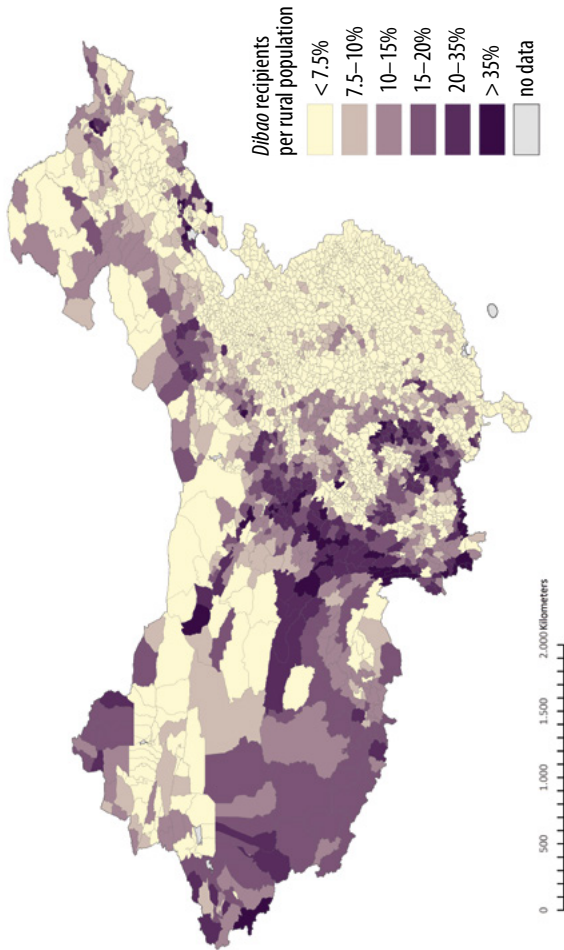


Figure 5: Coverage of the Rural Minimum Living Standard Scheme (2014)

Own calculations and illustration, *dibao* data from Ministry of Civil Affairs 2014a, population data from National Bureau of Statistics China 2010

Thus, it is possible to generate information about *dibao* reception rates per rural population across Chinese counties, the second lowest administrative level. Only for a few counties, information was missing and had to be supplemented with data from the next administrative level, districts. Mapping the distribution of *dibao* beneficiaries leaves the impression of a considerable spatial component of *dibao* reception.

As displayed in figure 5, the coverage of the rural *dibao* system is highest in the Western, mountainous regions of China and remote border regions. While in some Western regions up to 50% of the rural population received social assistance, coverage in industrial Eastern counties rarely exceeded 10% of the rural population in 2013.

Having linked geospatial information to *dibao* reception rates, it is possible to conduct a test for spatial autocorrelation. This test assesses whether *dibao* reception in certain regions correlate significantly with direct neighboring regions, which are assumed to share the same geographic location. Put in other words, the linear association between a variable's value and the same variable's values in the neighboring locations is tested. The univariate Moran's I is a measure of global spatial autocorrelation in one variable at a fixed time period, which ranges from -1 (perfect dispersion) to $+1$ (perfect correlation), the value 0 indicating random distribution (Moran 1950). For local analysis, the Moran's I is decomposed into its regional contributions, generating the Local Moran test statistic (Anselin 1995). The spatial relationship among the regions is defined by a queen contiguity weights matrix, which determines neighborhood status by common boundaries and vertices (see figure 6).

For *dibao* reception rates, the local Moran's I takes the value of 0.56, which is significantly different from zero and thus indicates a certain positive spatial autocorrelation of social assistance. For more detailed analysis, it is necessary to look into the regional composition, illustrated by the LISA (Local Indicators of Spatial Association) cluster map, in which regions with statistically significant autocorrelations are indicated

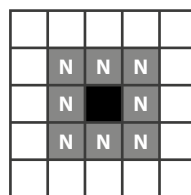


Figure 6: Queen contiguity

Own illustration

(see figure 7). Indeed, there is a significant positive correlation in the underdeveloped Western areas of China, where poverty seems to be most prevalent and *dibao* reception rates are generally high (high-high correlation). At the same time, there is a positive correlation of low *dibao* reception (low-low) in the coastal, mostly metropolitan areas. However, in northern and central regions, there is few spatial autocorrelation of *dibao* reception to be found (grey areas). Put in other words, in these regions, *dibao* reception was not correlated to the *dibao* reception rates in adjacent regions. While this does not rule out the existence of geographical factors influencing poverty and consequently the distribution of social assistance, many counties in the more developed regions were unaffected by their immediate neighbors' social assistance reception levels, even though they must have shared many geographical characteristics.

In addition to the lack of spatial autocorrelation of social assistance, one should also mention within-region inequality. Even though poverty and inequality numbers are difficult to come by in rural areas for reasons discussed earlier, there has been some research on the question of Chinese rural inequality. Past and recent studies agree that income or consumption inequality in rural China has been on a constant rise since the advent of economic reforms (Fan et al. 2002; Benjamin et al. 2005; Xing et al. 2009; Li et al. 2015). Altogether, the lack of spatial autocorrelation in some regions and increasing rural welfare inequality generates strong doubts about the general efficiency and effectiveness of geographic targeting.

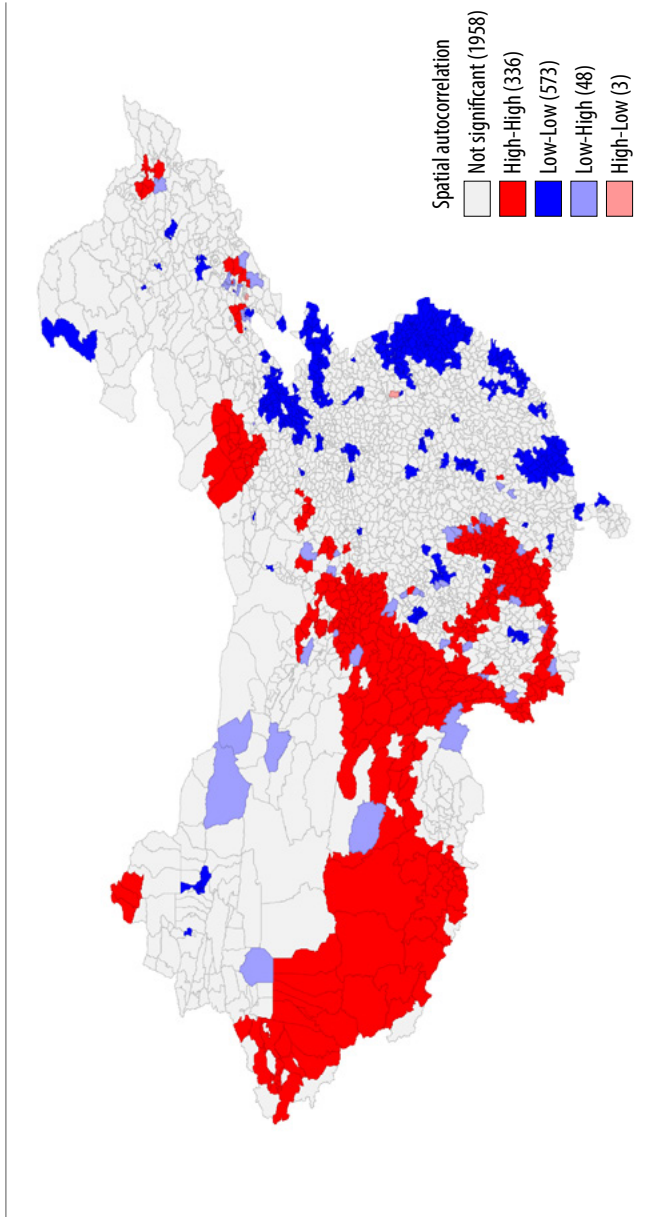


Figure 7: LISA cluster map, 999 permutations (*dibao* coverage rate)

Source: own calculations, *dibao* data from Ministry of Civil Affairs 2014a, population data from National Bureau of Statistics China 2010

3.3 DEMOGRAPHIC TARGETING

Demographic targeting is a form of categorical targeting, where eligibility is determined by attribution to certain demographic groups, for instance an age threshold for pensions or child allowances, an attested handicap for disability grants or gender for certain microcredit programs. While some programs are distributed solely by demographic criteria (Devereux et al. 2015), many countries chose to combine demographic targeting methods with other identification methods (Fizbein and Schady 2009). On the one hand, this targeting method is popular for its administrative simplicity and might be useful under circumstances where administrative funds and capacity are low. On the other hand, the accuracy of demographic targeting crucially depends on the correlation between poverty and the selected demographic marker. While demographic targeting might work very accurately in identifying specific population strata, there may occur considerable inaccuracies in classifying household or individuals according to their poverty status (Devereux et al. 2015). In other words, this form of targeting is only appropriate when specific population strata are symmetrically affected by poverty incidence (Coady et al. 2004). Past experience from other countries shows that in transition economies, demographic characteristics cease to be very accurate poverty classifiers (Habibov and Fan 2006). Also, it was found that targeting based on demographic criteria only may reduce exclusion but leads to considerable leakage of funds to the non-poor. Following this logic, demographic targeting might be problematic especially under scarce financial resources (Grosch and Leite 2014).

Important poverty triggers in rural China are loss of labor ability and increasing health cost due to old age in combination with lack of support by family members (e.g. Cai et al. 2012). Also empirical evidence supports the role of age as determinant of transitory and long-term poverty (Jalan and Ravallion 1998; Glauben et al. 2012). To test the association between demographic classifiers and poverty classification, a larger sample of nearly 5000 rural Chinese households from 2009 (see section 5.2 for a detailed data description) was tested for the connection between

Table 1: Demographic characteristics and poverty status (n=4,924)

Demographic categories	Poverty rate	Pearson's correlation coefficient
Sample average	9.04%	
Family type: Single or couple, no children	12.03%	0.0743 ***
Family type: Couple and one child	7.14%	-0.0035
Family type: Couple and two children	6.21%	-0.0388 ***
Family type: Couple and more than two children	12.30%	0.0181
Family type: Single parent and children	8.22%	-0.0096
Family type: Three generations under one roof	7.86%	-0.017
Age household head <20	10.34%	0.0049
Age household head: 20–29	7.93%	-0.009
Age household head: 30–39	6.78%	-0.0369 ***
Age household head: 40–49	5.75%	-0.0764 ***
Age household head: 50–59	7.19%	-0.036 **
Age household head: 60–69	14.32%	0.0775 ***
Age household head: 70–79	27.04%	0.1512 ***
Age household head >=80	32.14%	0.0609 ***
Gender of household head	8.89%	-0.0075
Family member with handicap	14.04%	0.1152 ***
Households without labor force	30.98%	0.1508 ***

Own calculations. Data Source: Institute of Social Science Survey of Peking University 2010

Note: Significance levels *** p<0.01, ** p<0.05, * p<0.1

demographic classes and poverty incidence along local poverty thresholds. Table 1 shows poverty levels of different demographic groups as well as the correlation between group membership and poverty status, measured by the Pearson correlation coefficient. Poverty rates were significantly higher for childless adults, households with elderly household heads and households with at least one handicapped family member. In fact, poverty rates were significantly positively correlated with certain demographic properties, for instance childless individuals, couples or households with households heads aged 60 years and older, or households with handicapped members. If these patterns hold beyond the limited CFPS sample, demographic targeting might help to identify eligible households. However, even in the most vulnerable demographic group (households with a household head > 80 years) “only” a third of the households were in poverty. Thus, purely demographic targeting could in fact lead to considerable leakage of funds to the non-poor. Meanwhile, the actual impact of these misgivings on targeting accuracy and anti-poverty effect of social assistance remains to be assessed empirically.

3.4 COMMUNITY TARGETING

By the definition by Coady et al. (2004), community targeting is a mechanism of individual assessment where the classification of applicants is not conducted by external administrators but by community groups or community leaders. In the case of rural *dibao*, community targeting comprises three main elements: Direct eligibility assessment by village leaders⁵, evaluation of applications during “democratic meetings” among villagers or villager representation and village leadership, and the implicit

5 Admittedly, village leaders fulfil task during *dibao* implementation that are commissioned by the township administration, and therefore are government agents. The classification of village leaders as “community” is still justified since the author found that most of village leaders’ actions were not provided for by official regulations. Furthermore, village level is in fact not an official government level. Finally, village leaders are elected by the community and their main function within the community isn’t related to *dibao* policy. According to the definition by Coady et. al (2004: 14), village leaders’ role in the targeting process is therefore categorized as community targeting.

assessment of applicants and recipients via public announcement of applicants and beneficiaries on village bulletin boards.

First of all, this mechanism is an attractive alternative for its (seemingly) lower implementation costs. In China, the implementation of public investments, for instance infrastructure projects, turned out to be cheaper when administered on local level (Wong et al. 2013). Also community targeting of social assistance may decrease administrative costs due to the lower involvement of external administrators and evaluators. More importantly, the identification of targets might not only be cheaper but also more accurate when done on the grass-root level (Coady et al. 2004). Arguably, community members or their leaders in small communities are better informed about true welfare levels among households than their principals at higher government levels. Also in China, decentralized policy implementation may in the best case lead to deployment of public goods and resources that corresponds better to local needs (Yi et al. 2011).

However, community targeting also has certain disadvantages, which are connected to its highly decentralized implementation. First of all, decentralization of anti-poverty program delivery may result in higher capture by local elites and interregional misappropriation of program funds (Conning and Kevane 2002; Bardhan and Mookherjee 2005). This is closely related to a key argument of principal agent theory that agents have an informational advantage over their principals but don't necessarily share the principal's interest. Therefore, these agents may hide information, characteristics, or might even act to the principal's disadvantage (Ross 1973; Stiglitz 1975). Another risk factor of decentralized social assistance targeting is the question of policy implementation through village leaders, who, in their function as Lipsky's "street-level bureaucrat" (Lipsky 2010), may enjoy considerable discretion in adapting central policy to perceived or actual local needs. Indeed, there is evidence for a significant implementation gap for instance for Chinese environmental policy (Ran 2013) or social assistance programs in other countries (Chinsinga 2005).

Therefore, community targeting typically finds application only where other targeting mechanisms are not feasible for lack of capital and/or trained administrators, if it is incompatible with specific program's

thrust, or if other programs, like geographic targeting, would result in high leakage to non-poor groups. Since the rural *dibao* policy also contains components of community targeting, the relevance of these two major caveats for the Chinese case shall be analyzed more in detail.

3.4.1 Interregional misappropriation and scarcity of funds

While interregional distribution of funds may impact all kinds of anti-poverty programs, it certainly is symptomatic for programs relying on community targeting due to the distinct decentralized character of this targeting mechanism. Since the classification of households is mostly done on village and township level, community targeting depends more than other forms of targeting on the availability of grants and administrative funds on the local level. Therefore, these issues shall be discussed specifically for community targeting, even though other forms of targeting may be subject to the same challenges.

For discussing the financial side of program implementation, it is useful to cast a glance on the general situation of financial federalism: During the past 40 years, the share of expenditure by local government levels in total national expenditure has been steadily increasing (see figure 8). With the current level of local expenditure, the People's Republic of China can be considered a fiscally highly decentralized country, even compared to federal countries like the U.S., Russia or Germany (Zhang 2006; Xu 2011). The picture changes though when looking at the revenue side: Due to a tax reform in early 1994, the share of revenue in total national revenue to be retained by local government levels fell sharply below the share of expenditure during the 1990s. Until about 2010, local shares in overall revenue remained on a level of around 44%, increasing again only since the past five years (see figure 8). These numbers clearly show a strong vertical fiscal imbalance between central and local governments, as an increasing share of expenditure is done by local levels, while revenue is increasingly

located at central government level. An additional strain on local budgets is the tax-exemption of household-level agricultural production, which weakened the tax-base of agriculture-based regions with little industry from which to collect tax revenue (Zhang 2006).

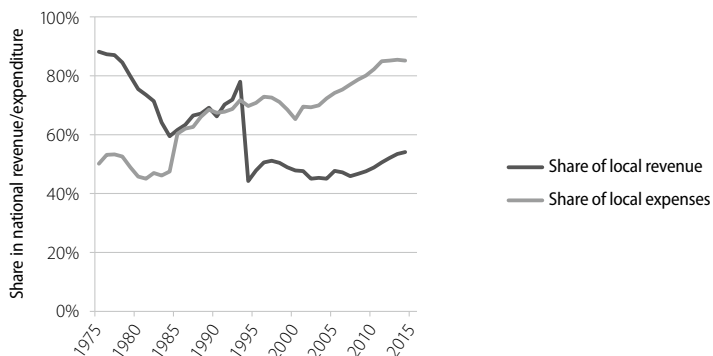


Figure 8: Share of local expenditure and revenue in the national budget

Own illustration. Data source: National Bureau of Statistics China 2015a

This vertical fiscal imbalance means that local governments, especially in agricultural regions, are depending on intergovernmental tax transfers (Kennedy 2007a) for providing social services and implementing public programs like the rural *dibao*. To enable the nationwide implementation of social assistance even in underdeveloped regions, about two-thirds of the total *dibao* expenses (60.1% in 2011) are provided by the central government. The remainder is supplied by the province, district or county levels, whereas township and village levels are exempt from any financial responsibilities (Zhang et al. 2012; Ministry of Finance of the People’s Republic of China 2013; Ministry of Civil Affairs 2014b). According to regulations, each autumn civil affair bureaus on each of the listed government levels have to estimate next year’s budget requirements for the *dibao* program and report it to the responsible financial departments. Financial

departments at each level incorporate these funds into the budget plan, which is then presented to the respective levels' People's Congresses for approval. The amount of the central government's subsidies is based on information like the number of *dibao* recipients in the respective region, *dibao* standards, subsidy levels and budget surplus carried forward from the previous year (Ministry of Finance of the People's Republic of China and Ministry of Civil Affairs 2012).

However, existent studies question whether the redistribution of funds is sufficient to make up for fiscal imbalances (e.g. Wang and Herd 2013; Liu et al. 2014). In some cases, intergovernmental transfers were found to be politically influenced and thus not equalizing but rather increasing fiscal imbalances (Huang and Chen 2012). Lin and Wong (2012) exemplarily analyzed the effect of rural transfer payments and subsidies in the light of intergovernmental transfers and found that intergovernmental transfers could not make up for imbalances in local governments' budgets due to inaccurate or inadequate redistribution of funds. Consequently, local governments in poorer region faced serious financial constraints, leading to regressive transfers (the poorer the region, the lower the transfers per capita) and reduced redistributive impact of the pro-poor (Lin and Wong 2012). Also in the case of rural *dibao*, a certain share of funds still has to be covered locally, even though the payments from the central government help to equalize regions of different welfare levels. Considering the results of Lin and Wong (2012), also the redistribution of funds for the *dibao* program may be inadequate. Furthermore, administrative costs related to program implementation may not be covered by central government transfers but have to be met with local funds (Ministry of Finance of the People's Republic of China and Ministry of Civil Affairs 2012), which places additional burden especially on the budgets of poor regions.

3.4.2 Local capture and nepotism

With the advent of community-driven development approaches, researchers soon cautioned against the danger of capture and nepotism by local elites (e.g. Platteau and Gaspart 2003). Especially in unequal communities, the risk of elite capture in the course of poverty programs was increased (Darmawan and Klasen 2013). Literature specifically mentions the incentive structure of rural cadres as one factor producing an imbalanced implementation of certain policy goals (Ran 2013).⁶ Generally, the Chinese central government has means at its disposal to encourage compliance by monitoring administrators' performance. The performance of local cadres down to the township level is measured through a complex evaluation system assessing both policy implementation (目标考核, *mubiao kaohe*) and personal performance (Heberer and Trappel 2013). However, prioritization of specific political goals by policymakers at higher governmental levels might lead to unbalanced incentives for local leaders. Consequently, local leaders' compliance might vary depending on the particular policy area (Edin 2003; Kennedy 2007b). As long as higher governmental levels still prioritize economic development, the incentive for local leaders to focus on social policy implementation might be low (see O'Brien and Li 1999). In addition, the size of the incentive is influenced by the extent to which the successful implementation of this policy is measurable (Göbel 2011). Unfortunately, the success of *dibao* implementation turns out to be rather hard to quantify, especially compared to economic performance. Moreover, local leaders might choose to put their resources into projects that are more likely to succeed (also termed "sure policy" by Heberer and Trappel 2013: 1061) or more prestigious, or more likely to yield immediate results (Eaton and Kostka 2014).

Moreover, there are considerable incentives for village administrators not to comply with regulations concerning the distribution of *dibao* funds. Beyond any electoral consideration (in contrast to village party secretaries, village leaders are elected rather than appointed), rural social

⁶ The following section features results also published in a journal article by the author (Kuhn et al. 2016a).

traditions seem to be a dominant factor: Traditional Chinese society, just as many other kinship-based societies with strong rural foundations, is closely knit through constant reciprocity between its members. This flow of favors is the basis of a network of social connections, *guanxi* (关系), which bears a strong emotional component (Kipnis 1997) but is clearly also based on shared interests and material benefits (Yang 1994). A large pool of good social connections upon which one can occasionally draw to redeem favors is of eminent importance for gaining access to scarce resources (Yang 1994). Indeed, a dense social network might help not only to avoid falling into poverty (Garcia and Kazepov 2002), but also to secure social assistance in situations of need. Meanwhile, social and moral necessity require dedicating large amounts of time to meeting the expectations and needs of one's kin and friends in order not to damage one's social reputation. Failing these expectations would indicate either a lack of authority to command scarce goods or the violation of implicit rules of reciprocity, and lead to a loss of "face" (脸, *lian* or 面, *mian*; see Ho 1976: 873; Yan 1996). According to Sahlins (1972: 207), a person of rank in small kinship-based communities is expected to provide for the community. Conversely, this exercised generosity further strengthens the rank system. Following this logic, *guanxi* requirements also change the incentive system of village leaders as their leadership entails implicit liabilities in the form of material or immaterial favors to their kin, friends, and clients. Even though similar patron-client relations also exist in other rural societies (Powell 1970), they seem to be especially persistent in rural China, having adapted to severe social and political transformations (Oi 1989, 1989; Esherick and Rankin 1990; Kipnis 1997; Yang 2002). Indeed, recent studies – such as Liu (2013) – report on the distribution of social assistance funds to kin and clients of the respective administrative agents (人情低保, *renqing dibao*). With increasing coverage of the system, the distribution of surplus funds may form an additional source of power for village leaders (Liu 2008; Guo 2009). It is even argued that the distribution of government funds like *dibao* grants have become a replacement for the power that village leaders lost with the abolishment of agricultural taxes, which traditionally were collected by local leaders (Liu 2008).

In effect, village leaders, who are the only actors with clear information about the true welfare level of households, sometimes have strong incentives to avoid community conflict and maintain “face” by supporting kin and friends. These motives might override the weaker incentives set by the central government to comply with regulations concerning the distribution of *dibao* funds. Beyond inaccurate policy implementation or the concealment of households’ true welfare level, village leaders might actively favor their clients to the disadvantage of other households. At this point, this study is not even taking into account the problem of leaders capturing funds for their private benefit, as has been described for instance by Zhang and Jiao (2008). Admittedly, some village leaders might just as well not take advantage of their superiors’ weak control, but rather comply with targeting regulations out of a sense of professional ethics, without necessarily receiving direct compensation or appreciation; they may even dedicate an excess of resources to their work (Dilulio, John D Jr 1994). Also, there have been indications that village-level elections, which were introduced during the past years, have increased compliance and responsiveness of local leaders concerning public investment (Luo et al. 2010). Hence, as both compliance and non-compliance are well defensible on behavioral grounds, their factual relevance remains to be empirically assessed.

3.5 PROXY MEANS TESTING

Another method of assessing individual eligibility without having to rely on income data collection, crude demographic criteria or opaque community targeting is proxy means testing (PMT). For this method, household welfare is estimated by easily observable proxy variables that either reflect or influence household monetary welfare, for instance the living arrangements, housing, household assets or human capital.⁷ Especially in recent years, PMT gained certain popularity for estimating regional poverty levels (Hentschel et al. 2000; Minot 2000; Alderman et al. 2002; Elbers et al. 2003; Otter 2009; Van Edig, Xenia Felice 2010; Olivia et al. 2011), but also for the estimation of welfare levels of individual households for targeting reasons (Grosh and Baker 1995; Grootaert and Braithwaite 1999; Ahmed and Bouis 2002; Castaño 2002; Houssou et al. 2007; Sharif 2009; Azevedo and Robles 2013; Bakshoodeh 2013).

The most frequent argument in favor of PMT is that the measurement of observable household characteristics is less time-consuming and requires less training for evaluators. Also, misreporting on the side of households might be less of a problem for observable household characteristics than for income (Coady et al. 2004). However, if for instance the ownership of a specific asset was known to be weighted strongly in determining income levels, households might signal neediness by selling or hiding that specific asset. Indeed, respective reports can be found for the cases of Colombia (Camacho and Conover 2011). According to Glewwe (1992: 303), this fraudulent conduct can be controlled to a certain degree by not revealing the exact modelling parameters to local administrators and applicants.

Keeping the detailed selection criteria secret however may produce a different problem. If the mechanisms and criteria of beneficiary selection criteria are unknown to the general public, the targeting outcome may

7 We recall that demographic targeting also uses demographic characteristics to identify targets. However, demographic targeting is distinctly different in the sense that characteristics like gender or age are directly used as eligibility criteria, while proxy means testing aims at determining eligibility along estimated household income, expenditure or other quantifiable measures of monetary welfare.

be rather elusive for community member and, in the worst case, endanger social cohesion (Adato et al. 2000). The problem is especially present under narrow targeting connected with a large share of exclusion, or if the estimation model produces inaccurate estimates, leading to inclusion of rich households. An existent PMT system, Mexico's former social assistance system PROGRESA in fact seemed to be so arbitrary that community members attributed selection as beneficiary to divine fate or perceived the targeting process as a lottery (Adato et al. 2000: 85). Considering these problems, the accuracy of PMT is not only an intrinsic goal but also an important factor for public support for any policy targeted by it.

In addition to these problems, which are not exclusive to PMT (see for instance a summary by Kidd and Wylde 2011), there are even stronger misgivings on statistical challenges of income and poverty estimation. In the following, three major problems of PMT are presented. The first is the choice of proxy variables, which concerns all models of income and poverty estimation. The second problem, the recreation of variance, concerns only the case of income estimation. The third problem of external validity concerns all parametric approaches to poverty classification or income estimation to the same degree.

3.5.1 Recreating variation in household incomes⁸

The total variation of household incomes can be defined as the variance of income Y around its mean \bar{Y} , the total sum of squares:

$$TSS = \sum (Y_i - \bar{Y})^2 \quad (1)$$

The challenge of income estimation consists in that it is hardly possible

⁸ The following section bases on a working paper by the author discussing the internal and external validity of income estimation and poverty classification via regression analysis (Kuhn et al. 2016b).

to identify all factors influencing and reflecting observed income. Either, important household characteristics are missing in the dataset, or the existence of specific factors is unknown to the econometrician. Therefore, only a part of variation can be explained, while a certain share of total variation (TSS), the residual variation, remains unaccounted for. According to Pindyck and Rubinfeld (1991), we write:

$$\sum (Y_i - \bar{Y})^2 = \sum (Y_i - \hat{Y}_i)^2 + \sum (\hat{Y}_i - \bar{Y})^2 \quad (2)$$

Total variation = Residual variation + Explained variation

Depending on the size of the residual variation, estimated income \hat{Y} will therefore typically feature a lower variation than the observed income. The practical implication of this problem can be illustrated by comparing the distribution of observed income with the distribution of predicted income as it is produced by a very simple OLS regression with CFPS data (see section 4.2 for data description). As depicted in figure 9, the variance of estimated incomes (transparent bars) is considerably lower than that of observed income (solid bars). Thus, incomes are generally overestimated in the left tail of income distribution. Similarly, incomes in the right tail are overestimated (though, due to the slightly biased estimator, to a lesser degree than in the left tail). On the quality of income estimation at the extremes of income distribution see also Kidd and Wylde (2011).

Also other PMT applications typically produce overestimation of low incomes and thus underestimation of poverty levels (e.g. Grosh and Baker 1995; Grosse et al. 2009; Kidd and Wylde 2011; Bakshoodeh 2013). In some cases however, estimated poverty levels actually turned out to be higher than observed poverty levels (Otter 2009; Christiaensen et al. 2012). These exceptional cases might be explained by either a better set of explanatory variables, which decreased unobserved variation in the lower income tail, or a biased model that underestimated incomes systematically, shifting the mean of the income distribution to the far left. Alternatively, the log-normality assumption of OLS estimation may be violated by a skewed distribution of log-income in the selected sample.

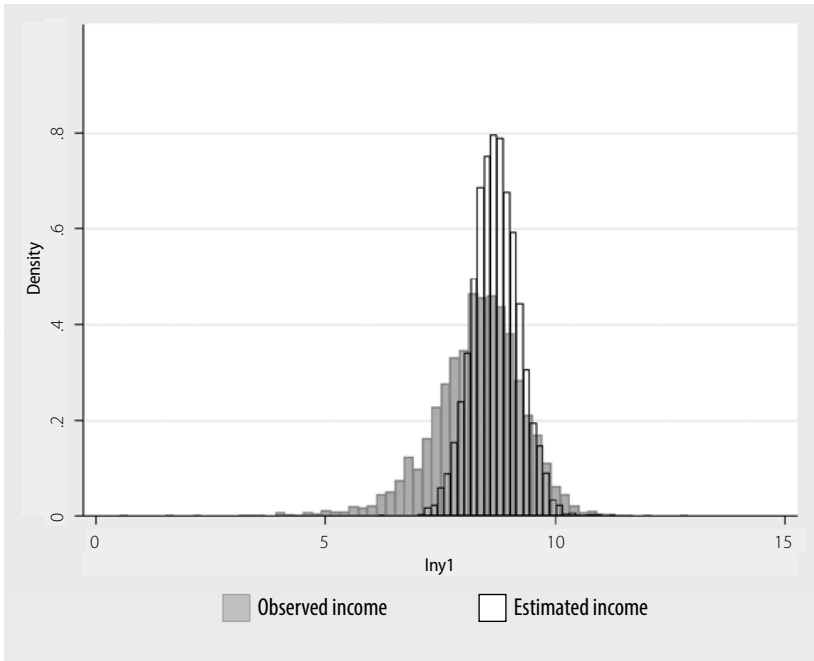


Figure 9: Systematical over- and underestimation of income

Own illustration. Data source: Institute of Social Science Survey of Peking University 2010

However, overestimation of incomes at the low tail of income distribution might occur not only for statistical but also for very practical reasons, for instance the unalterable character of certain household and family characteristics. While a sudden loss of labor force due to illness or retirement does not necessarily reflect in household characteristics (e.g. living space or the household's demographic composition), it will certainly decrease income drastically. This difference between current income and long-term household welfare as reflected by visible household assets is a topic that requires specific discussion in the following section.

3.5.2 The difference between monetary welfare and income

Looking for proxies for monetary welfare, researchers often select variables that describe the location of a household, the housing and dwelling characteristics, or ownership of household durables (Grosh and Baker 1995). But as pointed out before, there is a distinct difference between the *level* of household welfare and the current *flow* of household income to be measured. Especially housing, dwelling and household durables are, among other factors, a function of past flows of income and expenditure, but not necessarily a strong correlate of present income levels (e.g. Klasen 2000; Filmer and Pritchett 2001; Harttgen et al. 2013). Therefore, some household characteristics may be excellent proxies for the current welfare level, but still be misleading for estimating the current income level of that same household. The crucial problem is the asymmetrical adjustment of durables and fixed assets to changes in household income. Empirical evidence points out that consumption of durables and fixed assets seems less sensitive to anticipated or real negative changes as compared to positive changes. Further, permanent income changes have a larger effect on consumption than transitory changes (Jappelli and Pistaferri 2010). Therefore, a relatively strong rise of consumption following anticipated or real increases in income, and smaller decrease of consumption under negative income shocks are to be expected. Conversely, negative income shock does not always involve an immediate decrease of household assets due to their cumulative character. Furthermore, purchase of certain assets like TV or mobiles might be strongly influenced by preferences and relative prices, and therefore not strictly correlate with growth of incomes (Harttgen et al. 2013: 58). PMT models that focus on variables that are only reflecting the material welfare of households (e.g. ownership of durables, dwelling) could thus generate inaccurate estimations of current household income. Therefore, an accurate PMT model should by all means also include variables that reflect of influence the ability of a household to generate income.

3.5.3 External validity of income estimation

The third difficulty of OLS-based income estimation is the external validity of prognostic information, the accuracy of prediction outside the calibration sample. The discussion of external validity roots in prognostic problems in health and consumer studies (Campbell and Stanley 1966; Calder et al. 1982; Justice et al. 1999), but is relevant for nearly all estimation problems. Meanwhile, this thesis argues that the question of external validity has specific implications for social assistance targeting and thus requires a more detailed focus: As described above, one of the downsides of PMT is the obscurity of the targeting process to the concerned communities. In previous cases it could be observed that public acceptance of social transfers was weakened considerably by high rates of mistargeting. Therefore, this thesis argues that it is vital that a PMT model still performs well once it leaves the statistician's hands and is extended beyond the calibration sample because external validity is not only important for targeting accuracy but also ensures the necessary public and political support. In the author's view, the topic of external validity of PMT for poverty and income estimation has not received due attention. Among those papers conducting PMT-based income estimation or poverty targeting, only half actually test model transfers, mostly for estimation of regional averages (small area estimation). Household-level estimation including a discussion of external validity however could only be found in three papers, among them only one study testing the robustness of a model to transfers between different waves or sample regions (see table 2).

For evaluating the external validity of income estimation, this study draws on a systematic that was developed for evaluating medical prognoses by Justice et al. (1999). According to them, external validity of prognostic systems stipulates that the accuracy of prognosis does not deteriorate if transported across time (historic transportability), space (geographic transportability) or samples with different data collection methods (methodologic transportability). During these transports, several problems add to the inaccuracy of in-sample estimation of income and poverty status, the **estimation error**, which was discussed in the previous two sections.

Table 2: Literature review of income estimation model transfers

	Model transfer	Type of transfer	Prediction Level
Ahmed and Bouis 2002	-		
Alderman et al. 2002	yes	Survey-Census	Regional
Kidd and Wylde 2011	-		
Bakshoodeh 2013	-		
Castaño 2002	-		
Christiaensen et al. 2012	yes	Between waves	Regional
Coady and Parker 2009	-		
Elbers et al. 2003	yes	Survey-Census	Regional
Grootaert and Braithwaite 1999	-		
Grosh and Baker 1995	-		
Grosse et al. 2009	yes	Survey-Census	Regional
Hentschel et al. 2000	yes	Survey-Census	Regional
Houssou et al. 2010	-		
Johannsen 2009	-		
Klasen and Lange 2015	yes	Between waves	Household level
Minot 2000	yes	Survey-Census	Regional
Olivia et al. 2011	yes	Survey-Census	Regional
Otter 2009	yes	Survey-Census	Regional
Sharif 2009	yes	In-sample	Household
Stifel and Christiaensen 2007	yes	Between waves	Regional
Van Dinh, Thi Tuyet et al. 2009	-		
Van Edig, Xenia Felice 2010	yes	Between waves and samples	Household

Source: own compilation

Literature frequently mentions two main sources of errors during models transports beyond the calibration dataset. First of all, **differences in the sampling** of household surveys may impact the external validity of income estimation and poverty classification (Kidd and Wylde 2011). If, for instance, a variable's explanatory power is specifically strong

in explaining income differences of poor households, it might receive a high weight during calibration in a sample containing numerous poor households. If the produced parameter is then used to predict income levels in a sample mainly consisting of rich households, for which this relation between the explanatory variable and income does not exist, the resulting estimation would be biased.⁹ Therefore, it is vital that the calibration sample appropriately reflects the national income distribution and also covers households at the extreme of income distribution. The sampling technique and sampling weights play an important role in fitting an estimation model of a sufficient external validity. In theory, it is possible to determine structural sampling differences by comparing the respective information on household characteristics. However, it will be very difficult to approach the (representative) population structure completely. In earlier years, even the official NBS sample seemed to be plagued by problems of sample representativeness (Bramall 2001). Therefore, it can be assumed that the out-of-sample prediction in this study will also suffer from a certain bias induced by sampling differences.

Second, **differences in the survey design and implementation** can impact item-nonresponse, unit-nonresponse of specific income groups, and response accuracy. Item-nonresponse is an important issue especially for income data in the calibration dataset, since income is, as mentioned above, a very sensitive topic across cultures. Item- or unit-nonresponse of specific income groups are especially critical, as they might compromise the sampling and add to the sampling differences mentioned above, if not controlled for by attrition weights.

Several factors of survey design may influence the quality of the dataset (see Rossi et al. 1983 for a complete overview). Among these factors are the survey mode, for instance the decision between direct, telephone or self-administered interviews (Aquilino 1994). But even details like the sequence of questions (Rossi et al. 1983: 302–304) or the difference

9 An additional reason for the necessity of representative samples is the problem of endogeneity in the estimation model, which does not allow for model transfers between unrepresentative samples (Cameron and Trivedi 2005: 818–819). The question of endogeneity will be discussed more in detail in the following section.

between account-keeping data collection and recall-income collection (Moore et al. 2000) may impact the data. Other items are implementation errors on the side of evaluators, either because of insufficient training or insufficient performance of evaluators. As survey design, training processes and monitoring mechanisms vary between surveys, the size and direction of these implementation errors will also differ between surveys. Possible ways in which interviewers may have an effect on nonresponse or response quality may be for instance the interviewers' behavior (Durrant et al. 2010), interviewer characteristics, or the size of the assignment per interviewer (Singer et al. 1983). Other factors like the intentional falsification of answers by interviewers are connected to interviewer characteristics as well (Schraepler and Wagner 2005), but can be detected through monitoring of the data collection process by the survey organizers. Differences in these factors between the datasets in question may have a strong effect on the accuracy of out-of-sample predictions.

In summary, an unbiased out-of-sample prediction does require the best possible model fit in the calibration data while at the same time ensuring that the model is valid beyond the calibration sample, i.e. avoid overfitting. Therefore, it is important to eliminate sample and survey implementation difference between calibration and evaluation sample. The standardization of sampling and income data collection between statistical units might significantly reduce this mistake. The correction and abatement of model transfer biases after data collection however are not the focus of this work. Instead, the following quantitative assessment will test the size of the impact of model transfers on the accuracy of classification and the level of poverty reduction.

3.6 SUMMARY

This chapter discussed the particular strengths and risks of a selection of targeting mechanisms, namely individual assessment via income measurement or proxy means testing, geographic targeting, demographic targeting and community targeting. Strong differences between the methods might lead to different performance in terms of accuracy of poverty identification and anti-poverty effect. While major leakage can be expected for geographic and demographic targeting due to the current stage of socio-economic and administrative development in the research region, community targeting is likely to be distorted by socio-cultural peculiarities and adverse incentives to local administrators. Furthermore, the previous analysis advised caution when specifying a PMT model. Particular care should be taken concerning three components. First of all, income estimation might fail in estimating current income levels and instead focus on welfare stock as a function of past income. Further, variable selection may impact the variance of estimated incomes. Since it is anticipated that the variance of expected income will be low no matter how careful the variable selection, any assessment of PMT models should be prepared to control for overestimation of incomes at the lower tail of income distribution. Finally, it should be made sure to test for the external validity of the estimation results. The extent and impact of the theoretical challenges on the targeting of social assistance will be subject to empirical assessment in chapter 5.

4 QUANTITATIVE EVALUATION OF *DIBAO* EFFICIENCY AND EFFECTIVENESS

The following chapter contains an empirical analysis of the existing targeting regime with regards to the efficiency of its benefit allocation and its immediate anti-poverty impact. Further, this quantitative analysis will compare the efficiency and impact of the existing targeting scheme with alternative targeting mechanisms in order to evaluate to what degree the anti-poverty impact of social assistance is truly depending on the respective targeting mechanism. The following chapter is structured into five main parts: The analysis method, a description of the data base, the presentation of the analysis results including some robustness tests, and a summary.

4.1 METHOD

In this chapter, this thesis will assess the efficiency and effect of the current *dibao* targeting and compare it with alternative targeting methods. The following section presents methodological aspects of the assessment and a comparison of targeting efficiency and anti-poverty effect. Special attention is paid to the specification of the methodologically demanding income estimation models used for poverty classification via proxy means testing (PMT).¹⁰

4.1.1 Income and poverty estimation methods

Among the analyzed targeting approaches, the methodological settings of PMT are most challenging. Since current income is the main eligibility criterion for the RMLSS, the estimation will be specified to approximate current net income (or income-based poverty status) instead of consumption expenditure. For income or poverty estimation itself, several methods are conceivable:

Ordinary least squares (OLS) regression is among the most widely used models for income estimation (Grosh and Baker 1995; Struyk and Kolodeznikova 1999; Sharif 2009; Klasen and Lange 2015). In most PMT-type income simulations (e.g. Hentschel et al. 2000), household income enters the OLS regression in log-transformation. On the one hand, this approach is straightforward and produces continuous absolute income estimates. Further, the generated estimation parameters can easily be applied to estimate income in other samples. On the other hand, simple linear regression might be accurate in predicting the mean of observed income, but very likely be inaccurate in estimating incomes at the low tail of income distribution (see chapter 3.5).

¹⁰ This section also bases on a working paper on internal and external validity of income estimation (Kuhn et al. 2016b), which puts a stronger focus on the detailed description of the estimation process.

Quantile regression (QR) seeks to minimize the absolute deviations from the median or any other quantile τ of the dependent variable (Koenker and Bassett 1978; Koenker and Hallock 2001). For income estimations, this method has been employed for instance by Muller and Bibi (2010) or Houssou et al. (2010). To obtain an estimate of the conditional expectation function $E(Y|x)$, the following minimization problem has to be solved:

$$\arg \min_{\beta \in \mathbb{R}} \sum_{i=1}^n p_{\tau}(y_i - x'_i \beta) \quad \text{if } \tau \in \{0,1\} \quad (3)$$

where p_{τ} is the so-called “check function”, which weights the residuals by τ if positive and by $1-\tau$ if negative:

$$p_{\tau}(z) = z(\tau - 1(z \leq 0)). \quad (4)$$

Generally, this method has the appeal that it is more robust to outliers in the dependent variable. The big advantage for this study is that it is possible to estimate parameters at a specific, critical sample segment. Since this study is mostly interested in incomes around the poverty threshold, quantile regression is specified to estimate the 9th percentile of income distribution, which corresponds to the average poverty headcount. However, this specification likely decreases accuracy of estimation at the higher quantiles.

If it is assumed that parameters are not constant over the whole range of income values observed in the sample, also piecewise linear regression (PW10) is an interesting estimation method. Piecewise regression basically means to conduct separate linear regression for different welfare segments. Other than the quantile regression specified above, piecewise regression might achieve accurate estimation across the whole distribution, not only at a specific quantile. The crucial part is the identification of the points at which the slope or the level of the regression line changes, the so-called breakpoints or segment borders. This study aims at generating segments as a function of explanatory variables, as suggested by

Muggeo (2003). Unfortunately, there are no natural breakpoints available, critical values at which the relationship with explanatory variables changes by some natural mechanism. In this case, it is recommended to use quantiles as breakpoints, also called quantile knot sequence (Keele 2008; Harrell 2001: 23). This study assumes ten income segments separated by knots $k=9$ at the respective quantiles of predicted income values estimated by a single OLS.¹¹ The downside of piecewise linear regression is that the resulting estimates are not continuous across the whole distribution at the segment borders. To restore a continuous distribution, this study employs a spline regression, a technique which is described more in detail in Appendix A.

Among binary choice models, the most common is probit regression, which estimates the probability (between 0 and 1) of households being situated below an absolute or relative poverty line by a set of independent variables (Grootaert and Braithwaite 1999; Minot 2000; Houssou et al. 2007). While estimation might be more accurate in classifying households into poor and non-poor, it does not produce income estimates but only a poverty probability score conditional on a pre-defined poverty line.

Closely related to probit regression is a two-group discriminant function analysis (DFA), which calculates a discriminant score by deriving a variate by a linear combination of independent variables (Rogers et al. 2006). As described in Hair et al. (2006), the variates' weights are calculated in a way that maximizes the differences between the two groups (in this case: poor and non-poor as defined by the absolute poverty threshold). A linear discriminant function can be written as

$$Z_k = a + w_1x_{1k} + w_2x_{2k} + \dots + w_nx_{nk} \quad (5)$$

where the discriminant score Z_k for each observation k (in this case: household) is defined by an intercept a , and the independent variables X_{ik} are weighted by discriminant weights W_i , whose size are relative to

11 Due to this approach of breakpoint selection, piecewise regression unfortunately can't be transferred beyond the calibration sample without further adjustments.

the discriminatory power of variable X_i concerning group membership. The probability of being member of the group(s) in question rises with increasing discriminant score. Using this property, one can use discriminant function analysis to estimate the poverty status of households, but also rank them according to their discriminant score. The disadvantage of discriminant analysis is its relatively high sensitivity regarding violations against the basic assumptions of normality, linearity and multi-collinearity. However, discriminant analysis is more appropriate for a multitude of groups and thus might be advantageous for attributing households to income deciles (Hair et al. 2006).

Principle component analysis (PCA) is a method to reduce dimensionality in a set of correlating variables and has found wide application for welfare estimation since income data for calibration is not necessary (Lai 2003; McKenzie 2005; Vyas and Kumaranayake 2006; Harttgen and Vollmer 2011; Harttgen and Klasen 2012). After transposing the matrix of possibly correlating variables, the linear combination of values that accounts for the largest share of variation in the data (the first principal component) can be used as a proxy for an underlying latent variable. In this case, this latent variable is monetary household welfare. The welfare score can be translated into household income by adapting a modification of an approach presented by Harttgen and Vollmer (2011), which is based on the assumption that the ranking of households according to the asset index is the same as the ranking according to (unobserved) income.

In the first step, standardized PCA scores $p_{i,st}$ are generated:

$$p_{i,st} = \frac{p_i - \bar{p}}{\sigma_p}, \quad (6)$$

where p_i stands for the score of the first principal component of household i , \bar{p} its sample average and σ_p its standard deviation. The second step generates income estimates by recreating the mean and distribution of regional income:

$$\hat{Y} = p_{i,st} \cdot s_X + \bar{X}, \quad (7)$$

where \bar{X} and s_x are the sample average and standard deviation of observed income. While in this case, the data provides the necessary information on mean income and income distribution for the sample in question, real-life applications would require suitable macro data for conducting such a transformation.¹²

There are two important reasons to include all of the listed estimation techniques in the following impact analysis. First, there is simply no agreement in literature about the most suitable model. As shown in table 3, each estimation method has strengths and weaknesses concerning income and poverty estimation. Differences mainly concern data requirements and model assumptions, but also the generated output. For instance, some of the estimation techniques produce more accurate estimates at certain segments of income distribution, while others aspire to produce accurate estimates across the whole sample. Second, and more important, it is unknown how the different approaches will affect the poverty classification and consequently the simulated anti-poverty effect. Since it is one aim of this thesis to assess the suitability of the PMT for targeting issues, the following simulation of targeting via proxy means testing should be robust to changes of the specific estimation technique. Therefore, this study will not decide for one estimation method prematurely.

Meanwhile, some estimation methods will be excluded for practical reasons: Multidimensional deprivation indices for instance assign scores for deprivations concerning specific welfare dimensions and gained increasing popularity for determining the poverty status of households. The resulting scores are aggregated either as an average of the analyzed components or by subjective or computational weights (e.g. principle component scores) (Klasen 2000; Alkire and Foster 2011). This method was not considered for this study due to the difficulty of selecting cut-offs and weights of sufficient external validity.

12 A closely related technique is a utility function approach proposed by Po et al. (2012). This method is advantageous for income estimations across years or countries, which however is not the focus of this study.

Table 3: Summary of different estimation techniques

Model	Advantages	Disadvantages
OLS regression	<ul style="list-style-type: none"> • Technically least demanding 	<ul style="list-style-type: none"> • Estimation is likely to be most accurate at the mean of observed income only.
Quantile regression	<ul style="list-style-type: none"> • Focus on specific income segment possible 	<ul style="list-style-type: none"> • Estimation is likely to be biased for income segments distant to the selected quantile
Piecewise regression	<ul style="list-style-type: none"> • Tackles non-linearity in parameters 	<ul style="list-style-type: none"> • Higher sensitivity to sample transfers due to difficulty of transferring the breakpoints
Probit regression	<ul style="list-style-type: none"> • Focus on a specific segment of income distribution 	<ul style="list-style-type: none"> • Does not produce income estimates but only probability of attribution to a group
Discriminant function analysis	<ul style="list-style-type: none"> • Appropriate for a multitude of groups • Advantageous for attributing households to income deciles 	<ul style="list-style-type: none"> • Higher sensitivity regarding violations against the basic assumptions of normality, linearity and multi-collinearity.
Principle components analysis	<ul style="list-style-type: none"> • Non-parametric approach, • no fitting process required, • no "transfer"-bias as such possible • No observed income necessary 	<ul style="list-style-type: none"> • Does not estimate household income but a general welfare index • Average mean and dispersion of income in the research region have to be known. • Assumes log-normal distribution of income

Source: own compilation

The specification of the non-PMT methods are methodologically less demanding: For geographic targeting, the sample counties are ranked by their reported GDP per capita in the respective year and all households in the poorest two counties are classified as poor. The classification threshold is chosen in a way that the number of targeted households approximates the amount of beneficiary households identified by the actual targeting process (i.e. the number of observed *dibao* households). As mentioned before, it is important to identify the same number of beneficiaries with all targeting mechanisms and PMT methods in order to maintain comparability in terms of anti-poverty effect. For the same motive, demographic

eligibility criteria are set in a way that a similar number of households are identified as poor than it is the case with the other targeting mechanisms. In accordance with theoretical evidence and the main demographic correlates of poverty status featured in table 1 (p.43), all households with zero labor force are classified as poor. In addition, households are included that have just one laborer, but another handicapped household member and a head of household who is older than 75 years.

4.1.2 Variable selection for proxy means testing

The second key challenge of PMT is to find proxy variables that promise high predictive capacity, but also ensure practicability and parsimony. A too wide selection of explanatory variables might lead to overfitting of the estimation model (Christiaensen et al. 2012) and increase implementation cost through extensive data collection efforts. However, a certain number of variables is necessary to approximate the variance of the continuous income distribution, especially since many explanatory variables are discrete. Another practical reason is that the choice of only one or a few proxy variables could quickly lead to households providing false signals to evaluators (Grosh and Baker 1995). Therefore, the set of explanatory variables should not be too small either.

As mentioned in the data section, CFPS provides a wide selection of potential explanatory variables that are, according to literature, connected with household income in a formative or reflective way. In addition, province-level variables can be gathered from official yearbooks by the National Bureau of Statistics China (2015a). A small selection of county level information is provided by the CFPS working group. Since county-identifiers are not provided by the survey organizers, it is not possible to merge the household data with additional external information on country characteristics.

Variables reflecting household welfare are, for practical reasons, mainly visible and easily observable household assets. For the measurement of living standard level or inequality, the construction of asset indices

has been discussed widely (Klasen 2000; Montgomery et al. 2000; Filmer and Pritchett 2001; Sahn and Stifel 2003; McKenzie 2005). However, it should be noted that the ownership of certain “luxury” assets, for instance mobiles or TV sets, can be biased by rising living standards or individual preferences. Other assets are unsuitable proxies for income since they are provided by the government uniformly on community-level, e.g. electricity (Harttgen et al. 2013: 41).

However, income estimation purely based on household assets is not recommendable due to the conceptual difference between income and welfare levels of households. Filmer and Pritchett (2001: 116) argue that assets and household expenditure are both proxies for the household’s long-run economic status, though not necessarily for each other. While the asset index captures the past and current investment in household assets, the net income or expenditure level may be less correlated to past income levels. In a very similar line, Harttgen et al. (2013) highlight the difference between consumption expenditure as a flow of values, and assets, which are in fact a cumulative stock. This difference can explain why pure asset indices were found to be actually rather unprecise in identifying levels of household income or consumption (Montgomery et al. 2000). In order to capture not only long-term, accumulated household welfare but current flows of income, it is necessary to also include variables and household characteristics that are stronger related to current income, for instance labor force and place of employment. According to literature, poverty and low income in rural China was found to mainly originate in the household composition or the availability of family labor (Xing et al. 2009; Glauben et al. 2012), the education of household members (Wan and Zhou 2005; Xing et al. 2009; Glauben et al. 2012), land endowment (Wan and Zhou 2005; Xing et al. 2009), and regional effects (Jalan and Ravallion 2002; Wan and Zhou 2005; Olivia et al. 2011; Glauben et al. 2012). Therefore, variables from all these dimensions were considered for the estimation model.

In composing the final set of variable, their statistical power in predicting income and poverty status are also of interest. However, regression coefficients are of limited meaning and hardly allow a causal

interpretation in this case: As was mentioned before, the variable set also comprises household assets that rather reflect than form household income. Inclusion of reflective variables however might violate the assumption of exogeneity of explanatory variables, thus confounding causal links between dependent and independent variables could cause problems of endogeneity. This means that structural or causal interpretation of OLS is not advisable, since this requires that the conditional mean of the error term equals zero. If $E[y|\mathbf{x}]$ is nonlinear in \mathbf{x} , the model loses its value for causal interpretation. However, it is still the best linear prediction, which is also termed a non-structural/reduced form interpretation (Cameron and Trivedi 2005: 68–69; Shmueli 2010). Since the aim is not to interpret/confirm causal factors for income differences or poverty but only to predict variance in income levels, endogeneity and omitted variable bias are therefore less problematic (Grosh and Baker 1995; Grootaert and Braithwaite 1999; Filmer and Pritchett 2001; Ahmed and Bouis 2002). However, incorrectly specified conditional means do not allow for unrepresentative sampling. Put in other words, PMT requires sampling weights since otherwise the estimated parameters will not be consistent beyond an unrepresentative calibration sample (Cameron and Trivedi 2005: 818–819).

Following a World Bank approach (Stifel and Christiaensen 2007), this study employed a stepwise regression process to systematically measure the impact of removing or adding single variables to the variable set. Keeping in mind the concerns about statistical variable selection leading to overfitting of an estimation model (Wooldridge 2012: 626), the tests for predictive power of variables were not conducted in the CFPS sample but in the CHIP survey sample. Only those variables were discarded that did not increase the overall explanatory power of the model for any of the estimation methods. Since the main focus of this study is on income prediction at the lower tail of income distribution, also a few variables were excluded from the system that performed well in predicting high incomes but failed in reflecting lower levels of income. The forward-stepwise selection procedure was conducted for several estimation models (probit, OLS-regression, piecewise regression, quantile regression and

PCA). The main criteria for evaluating the predictive power were the Akaike information criterion (AIC) and the F-statistics comparing the restricted model and the unrestricted model (for Probit mostly AIC). Also mistargeting was considered as a selection criterion.

The final selection includes 28 explanatory variables, which are featured in table 4. Only PCA uses a reduced set of variables, excluding characteristics which might not always be correlated positively with the income level of households such as binary identifiers of specific family types. In total, there were 4924 rural households for which there was sufficient information to conduct income estimation. With respect to practicability, only variables were selected that are routinely part of household surveys and are relatively easy to collect. With respect to parsimony, this range of selected variables corresponds to other estimation approaches and is the result of a process of eliminating all variables that did not contribute to the prediction power under any testing scenario.

In an OLS regression, most of the selected variables are significantly correlated with the log-income (see Appendix B). Especially variables determining the labor potential of a household were significantly related to household income, for instance the household size (negatively), the ratio of full labor equivalent in the household, the number of migrant workers, the sum of non-agricultural workers or the maximum of education level in the household. Further, several measures of regional development are significantly correlated to the households' income, for instance the respective county's urbanization rate. At this point however, it is necessary to recall that the set of explanatory variables includes both formative and reflective indicators. Due to the resulting endogeneity issues, the correlation coefficients featured in appendix B are very likely biased estimators and should not be used to infer any causal interpretation between income and the explanatory variables.

Table 4: Descriptive statistics of dependent and independent variables

Dimension	Variable	Label	Obs.	Mean	Std. Dev.
Income	Y1	Per capita household income	4924	6332.80	10302.03
	X11	Number of household members	4924	4.23	1.83
Demography ⁵	X12	Ratio elderly to total household members ¹	4924	0.16	0.27
	X13	Share of children (0–5 years) in total hh members	4924	0.06	0.11
	X14	Share of children (6–15 years) in total hh members	4924	0.11	0.16
	X15_1	Family type: Single or couple (0/1)	4924	0.34	0.47
	X15_2	Family type: Couple and one child (0/1)	4924	0.00	0.05
	X15_3	Family type: Couple and two children (0/1)	4924	0.13	0.34
	X15_4	Family type: Couple and more than two children (0/1)	4924	0.02	0.16
	X15_5	Family type: Single parent and children (0/1)	4924	0.10	0.30
	X15_6	Family type: Three generations under one roof (0/1) ²	4924	0.15	0.35
	X15_7	Family type: Others(0/1)	4924	0.25	0.44
	Labor	X22	Ratio of full labor equivalent in household ³	4924	0.69
X23		Total male labor force	4924	1.74	0.82
X24		Sum of labor experience (in years) of members >16 years	4924	83.67	41.23
X24b		Sum of squared labor experience (in years)	4924	3697.13	2578.89
X25		Number of migrant workers	4924	0.52	0.94
X26		Number of non-agricultural workers	4924	0.47	0.75
Health	X31	Household member with handicap (0/1)	4924	0.30	0.46
Housing and assets	X42	Housing value in RMB (log)	4924	10.69	1.51
	X51	Ownership of motorbike(s) (number)	4924	0.61	0.66
	X52	Ownership of car(s) (number)	4924	0.12	0.37
Education	X53	Ownership of color TV(s) (number)	4924	1.25	0.68
	X62	Maximum years of education among household members ⁴	4924	7.28	4.44
Regional Development	X91	Province average per capita GDP (log)	4924	9.98	0.45
	X93	County urbanization rate	4924	0.20	0.17
	X94	County's rate of labor age population	4924	0.60	0.06
	X95	County's mean years of education of labor age population	4924	8.35	1.07
	X97	County employment rate of labor age population	4924	0.70	0.09

¹ According to retirement age: Individuals >60 years; ² Adults living with their children and at least one of their parents;

³ Labor equivalent assigned according to Yi and Zhang (2011); ⁴ Only including those with labor force;

⁵ Demographic variables were excluded for income estimation via PCA.

Data Source: Institute of Social Science Survey of Peking University 2010

4.1.3 Determining the grants

A further issue concerns the sum of *dibao* grants of targeted household, which is integral information to simulate the poverty effect of *dibao* transfers. In practice, the sum of *dibao* grants per household is determined by two parameters: a) the level of grants per individual recipient, and b) the number of recipients per household, as one or several household members can receive benefits, once the household applied successfully.

In the community questionnaire, the level of grant per recipient is reported for each of the villages. Only for a few observations, this number had to be imputed as an average of the respective county. Further, the CFPS data contains information about the number of official recipients among beneficiary households, however only for households targeted by the current scheme. For the simulation of *dibao* distribution by other methods, the number of beneficiaries per household had to be imputed: In the CFPS, generally, a connection between household size and number of individual recipients (on average 1.31 household members) can be observed. Following these observed patterns, the quotas were assigned as illustrated in table 5.

Table 5: Numbers of beneficiaries per targeted household

Number of household members	Number of quotas
1	1
2	1.18
3	1.39
4	1.51
5–7	1.5
>7	1
1 adult with children*	1

*Households with only one adult never received more than 1 quota

4.1.4 Choice of poverty and eligibility thresholds

Two other important choices are the poverty and eligibility thresholds. First, it is necessary to set a poverty threshold to determine a household's "true" poverty status along its observed income. Using an uniform poverty line (for instance the national poverty line for rural areas) for all households in the sample would not honor the differences in price levels across regions, which were diagnosed for instance by Biggeri et al. (2016). Unfortunately, the Chinese government so far has not been issuing any local poverty lines. Therefore, province-level *dibao* standards (the official eligibility threshold for program admission), which usually are situated considerably below the national rural poverty line, will serve as absolute poverty line. There are two advantages to this solution:

First, these *dibao* standards consider regional differences in prices and development levels (Ministry of Civil Affairs 2010).¹³ Second, the use of the *dibao* standard will allow evaluating the targeting accuracy of the current scheme along national regulations. The second choice is on the eligibility threshold, along which the PMT methods will determine households' eligibility for *dibao* benefits. For several reasons, this study did not choose an absolute income threshold, but a relative eligibility threshold set at the respective regions *dibao* coverage rate: For determining eligibility along estimated income, an absolute threshold set at the level of *dibao* standards would be problematic because income estimation very likely does not preserve the distribution of observed income. Since low incomes tend to be overestimated by PMT approaches (see section 3.5.1),

13 To incorporate the effect of lower consumption expenditure of children and economies of scale in household expenditure, poverty classification and welfare measurement usually employ adult equivalence scales. The most common approaches weight children as a fraction of adults due to their lower consumption of food and other commodities. General economies of scale are incorporated by decreasing weights for additional adult household members (Deaton 1997). While the use of equivalence scales has been suggested for the rural *dibao* policy, it has not yet been implemented (The World Bank 2011). Poverty classification based on comparing households' adult equivalent incomes with thresholds defined in per capita income would therefore bias the assessment of the actual *dibao* targeting performance. Therefore, poverty lines are given as household income per capita and not per adult equivalent.

very few households would in fact qualify for *dibao* benefits. Therefore, this study uses a poverty classification, which does not require absolute accuracy of income estimated but merely accurate ranking of households. Relative poverty lines set at a certain percentage of the sample's median income depend on the accuracy of the absolute value of the median of estimated income as well. Instead, this study uses an eligibility threshold set at a specific quantile of the sample population. First, this approach allows poverty classification even under biased estimation of absolute incomes. Second, by choosing relative poverty lines, it is possible to keep the number of recipients (and the expenditure) constant, which will allow a fair comparison of the different targeting mechanisms. Third, relative poverty lines allow including also probit and DFA models, which both are not directly generating income estimates or scores that can be transformed into income values. For these estimation models, households are ranked by their probability score of being poor. As mentioned above, there is no reliable information about local poverty levels as orientation for determining the exact location of the regional poverty threshold. Instead, the relative poverty line will be set at the respective regions *dibao* coverage rate, which also retains comparability of the hypothetical distribution with the distribution of actual *dibao* funds. To make sure that no major bias is induced by this approach, this thesis will additionally test a specification where the relative eligibility threshold is set at the sample's poverty rate (along observed household income) in the respective region.

4.1.5 Evaluation criteria

For the following analysis, the main criteria of interest are the accuracy of income estimation, poverty classification and benefit allocation, and the anti-poverty effect of transfers. The following two sections present the selected measures, along which the actual and simulated targeting models will be assessed.

4.1.5.1 Criteria for the accuracy of income estimation and poverty classification

As presented above, the different estimation models produce either income estimates or poverty classification. The accuracy of income prediction or ranking of households is relevant for assessing the efficiency of *dibao* allocation along those estimation models that actually produce income estimations. For all the other scenarios that only estimate poverty status (including the targeting mechanism currently in practice) or generate a welfare score, just the assessment of poverty status prediction will be relevant.

One straightforward indicator for income estimation accuracy is the sample's mean square error (MSE) in estimating incomes Y of households i across the sample population n .

$$MSE = \frac{1}{n-1} \sum_{i=1}^n (Y_i - \hat{Y}_i)^2 . \quad (8)$$

However, this measure does not capture the efficiency of targeting over income strata and is very sensitive to outliers. Therefore, the second measure is the relative estimation error at the respective segment border, given as

$$\varepsilon_{Q_i} = \frac{\hat{Y}_{Q_i} - Y_{Q_i}}{Y_{Q_i}}, \quad (9)$$

where $\hat{Y}_{Q_i} - Y_{Q_i}$ is the difference between estimated and observed income at the respective quantile Q_i in relation to the observed income at that quantile.

For measuring the accuracy of income ranking prediction, Kendall's rank correlation coefficient τ seems suitable. Kendall's τ is a measure for the correlation between predicted and actual income rank (Kendall 1938) and is defined as

$$\tau = \frac{n_c - n_d}{\frac{n(n-1)}{2}}. \quad (10)$$

Here, n_c indicates the number of concordant pairs, $(Y_j - Y_i)(\hat{Y}_j - \hat{Y}_i) > 0$, for which households' ranks i and j by observed income Y and estimated income \hat{Y} agree. n_d indicates the number of non-concordant pairs, defined as $(Y_j - Y_i)(\hat{Y}_j - \hat{Y}_i) < 0$. The resulting coefficient lies between -1 and 1 , $\tau=1$ indicating perfect agreement in ranks, and $\tau=-1$ indicating perfect disagreement.

Literature provides a large number of potential measures for poverty classification, for instance summarized by Ravallion (2009b). Commonly, the accuracy of poverty classification is assessed by measuring the incidence of incorrect identification of non-poor households (inclusion error, a type 2 error) and the non-identification of poor households (exclusion error, a type 1 error) as for instance defined by Cornia and Steward (1993). A convenient way to visualize these two errors is the cross-tabulation of the poverty status as characterized by dichotomous (poor/non-poor) classification reporting relative errors of inclusion and exclusion (see table 6).

Table 6: Calculating targeting evaluation indicators

		Household poverty status as of observed income	
		Eligible	Non-eligible
Predicted household poverty status	Eligible	$a = \sum I(\hat{z}_i < cutoff, z_i \in \{intended\ targets\})$ <p>Successful targeting</p>	$b = \sum I(\hat{z}_i < cutoff, z_i \notin \{intended\ targets\})$ <p>Absolute inclusion error ("Leakage")</p>
	Non-eligible	$c = \sum I(\hat{z}_i \geq cutoff, z_i \in \{intended\ targets\})$ <p>Absolute exclusion error</p>	$d = \sum I(\hat{z}_i \geq cutoff, z_i \notin \{intended\ targets\})$ <p>Successful targeting</p>

Note: A similar table is introduced for instance by Grosh and Baker (1995). Assuming that \hat{z}_i is the predicted income of household i , z_i is the observed income of household i . $I(\cdot)$ is an indicator function where 1=the condition in brackets is true and 0=the condition in brackets is false.

The latter analysis is mainly interested in the relative misidentification in the targeting process. The relative inclusion error (also labeled "leakage" in targeting problems), can be written as the rate of "non-eligible beneficiaries" in all beneficiaries:

$$Relative\ Leakage = \frac{b}{a+b}, \tag{11}$$

where a is the number of eligible beneficiaries and b the number of non-eligible beneficiaries.

The relative exclusion error is defined as the rate of "eligible non-beneficiaries" c in the "total eligible population" ($a+c$).

$$Relative\ Exclusion = \frac{c}{a+c} \tag{12}$$

The so-called targeting differential (TDI), proposed by Ravallion (2000), takes into account both exclusion and leakage simultaneously. Even though also this targeting measure fails at weighting both targeting errors equally (Klasen and Lange 2016), it was found to be the only targeting measure sufficiently related to poverty impact of a specific transfer program (Ravallion 2009b). In detail, the TDI measures the difference between the coverage rate among poor and the participation rate of the non-poor. The TDI, which takes on larger values with improved targeting, is written as:

$$TDI = \frac{\sum I(z_i < cutoff, \hat{z}_i < cutoff)}{\sum I(z_i < cutoff)} - \frac{\sum I(z_i \geq cutoff, \hat{z}_i < cutoff)}{\sum I(z_i \geq cutoff)}. \quad (13)$$

The previous three targeting measures have the common property to put equal attention to each case of mistargeting. In reality though, mistargeting close to the poverty line may be considered a less severe problem. Therefore, this thesis suggests a measure which incorporates the distance of a wrongly identified household to the poverty threshold, which is henceforth called the mistargeting severity index (MSI). This measure is a modification of the poverty gap index (PGI, see Sen 1976; Ravallion 2009b) and measures the relative distance to the poverty line of those households that were wrongly identified as poor or non-poor (i.e. both leakage and exclusion cases). Algebraically, this index can be written as

$$MSI = \frac{1}{n} \sum_{i=1}^n \frac{|p_r - y_i|}{p_r}, \quad (14)$$

where n is the sample population, p_r is the poverty line of province r and y_i the income of a specific household i .

4.1.5.2 Criteria for evaluating the anti-poverty effect

Poverty reduction as the desired effect of social assistance can be measured in absolute terms as the number of poor households that have been lifted out of poverty by *dibao* transfers. Algebraically, this is given as

$$\Delta Poverty = p_0 - p_1, \quad (15)$$

which is the difference between the number of households whose income is below the local poverty threshold before (p_0) and after (p_1) the transfer of *dibao* grants. Further, this survey reports the reduction of the sample's poverty headcount ratio (PHR) as the relative change in the rate of households under the poverty line among the sample population n :

$$\Delta PHR = \frac{\frac{p_0}{n} - \frac{p_1}{n}}{\frac{p_0}{n}}. \quad (16)$$

4.2 DATA

As described above, the aim of this section is to compare the internal validity of income estimation for PMT with other targeting methods and actual distribution of *dibao* funds. To accomplish this task, this study requires a nationally representative dataset containing passable income data, enough household characteristics for income estimation, and information on actual *dibao* distribution. To check the external validity, an additional dataset is necessary, which has to contain the same variables on household characteristics than the primary data set, but not necessarily information on *dibao* reception. Upon careful consideration, the China Family Panel Studies (CFPS), a comprehensive national program to reflect changes in China's society, economy, population, education and health mainly under the leadership of the Institute of Social Science Survey of Peking University (2010) was chosen as baseline sample for the quantitative analysis. The CFPS sample covers rural and urban households of 25 Chinese provinces, municipalities and autonomous regions, which can be sorted into four regions (East, Center, Coastal West and Municipalities, see figure 10).

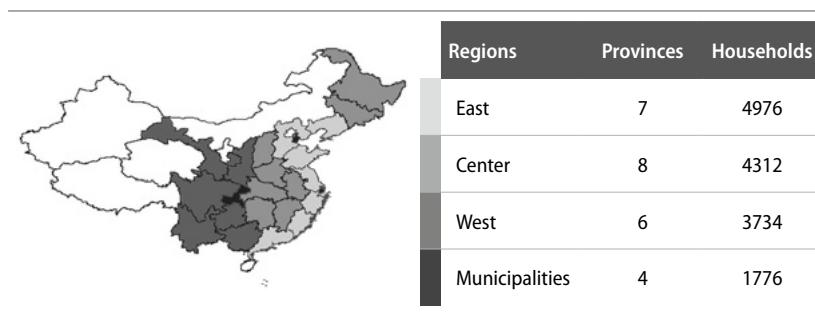


Figure 10: Sample distribution of CFPS (unweighted)

Own illustration

The total sample comprises 14,960 households, among them 7638 rural households with valid income information. To generate a nationally representative sample and in order to take regional development differences into account, the survey organizers employ three-stage probability-proportional-to-size sampling (PPS) with implicit stratification. In this process, the administrative units are sorted according to their economic development level reflected by GDP or, where unavailable, the rate of rural population and population density (Xie 2012a, 2012b). The CFPS survey organizers state that these sampling weights restore national representativeness in geographic terms. Arguing with the strong regional effect of development, the organizers further claim that thereby also representativeness in economic terms is achieved (Xie 2012c).

Several reasons led to the choice of this dataset. Generally, there exist several sets of academic household surveys that contain the necessary information for conducting quantitative analysis (table 7). Two of the featured studies focus on household income and expenses: the Chinese Household Income Project (CHIP) and the China Household Finance Survey (CHFS). Two other surveys are concerned with general household characteristics, but also collect comprehensive income data, the China General Social Survey (CGSS) and the China Family Panel Studies (CFPS). The China Health and Nutrition Survey (CHNS) is specialized rather in questions of household health. In general, there exist considerable differences between these datasets concerning the surveys' spatial coverage, sampling frame, sampling size and data collection methods: The CHNS for instance covers only a fraction of the country's 31 provinces while CGSS contains all of them.¹⁴ Also the sample size differs greatly, from only 3000 households in the CHNS and up to 13000 in the CHIP dataset. Most of the datasets assign population weights (Xu et al. 2012; Zhang et al. 2013a; Gustafsson et al. 2014).

14 With reference to the difficult political and practical research environment in some Autonomous Regions (especially Tibet and Xinjiang) the CFPS does not cover all province-level regions. The national representativeness of the data in the relevant properties however is not affected due to the low population density in these regions.

Table 7: Rural income datasets¹⁵

Survey name	CFPS <i>China Family Panel Studies</i>	CHIP <i>China Household Income Project</i>	CHNS <i>China Health and Nutrition Survey</i>	CGSS <i>China General Social Survey</i>	CHFS <i>China House- hold Finance Survey</i>
Survey wave	2009	2007	2009	2009	2010
Number of provinces	25	16	9	31	25
Number of rural households	7,638	13,000	3,007	4,561	3,244
Average p.c. rural income	6,027 (5,153.2)	5,096 (4,140.4)	8,872 (5,153.2)	6,523 (5,153.2)	9,594 (5,919.0)
GINI (total)	0.49	0.38	0.49	0.48	0.62
Poverty headcount index	25.64%	17.26%	16.96%	25.06%	26.26%
Poverty gap (average normalized)	0.11	0.06	0.13	0.10	0.20
Rate of <i>dibao</i> participants	5.0% (6.9%)	2.0% (5.0%)	–	–	–

Note: Except for CGSS, income is expressed as unweighted net income. For CGSS, there was only gross income available. Values in brackets are the national averages for the respective year as given by the National Bureau of Statistics.

Own illustration. Data source:

Institute of Social Science Survey of Peking University 2010; China Institute For Income Distribution 2012; Renmin University of China and Hongkong University of Science and Technology 2015

Most critical for this study are the surveys' differences in terms of income measurement. Data from the CHIP differs from all other surveys as the organizers decided to collect income the account-keeping method. For this approach, sample households are required to keep account of income or expenditure over a period of several months (Luo et al. 2013). Income data in the other surveys was collected by the traditional method of recall-type interviews, where respondents are asked

¹⁵ A more detailed comparison of available Chinese income data is given by a unpublished study by researchers from Beijing Normal University (Luo 2016).

by external evaluators to recall different components of income and expenditure during the past period. In general, there is no clear agreement between scientists on the superiority of account-keeping or recall-type income measurement (CGSS 2010; Ding 2012). According to Moore et al. (2000) and the organizers of CHIP (2012), this book-keeping method is able to improve the recall accuracy of expenditure or income, whereas recall-type technique might lead to omission of certain income components through respondents' lack of knowledge of important components, income fluctuation between different months and the omission of income received in the past. Further, account-type data collection might reduce the concealment of income: Since information is collected over a longer period, households would be required to constantly and systematically underreport income. On the other hand, the long duration of account-type data collection may increase attrition, especially among the employed and high-income households (Moore et al. 2000; Ravallion 2003a).¹⁶ A further issue is the inclusion of in-kind transfers, which is also handled differently across the surveys (Gustafsson et al. 2014).

For this study, the representativeness of the sample concerning poverty, income distribution and income in the low tail are the most important criteria. Indeed, averages of monetary welfare and poverty vary considerably between the surveys (see table 7). From among the featured datasets, CHNS and CHFS measured the highest average incomes. Especially the underrepresentation of poor households might be responsible for underestimation of income and poverty (Xu et al. 2012; Yue and Li 2013a; Zhang et al. 2013a). Also, the focus of the CHNS is not specifically on income measurement but on health issues (Gustafsson et al. 2014). Since this thesis' special interest is income in the low-tail of income distribution, CHNS and CHFS were ruled out for this project. Whereas CGSS has the advantage of large spatial coverage and provides average income measurement that seems rather close to the official measurement, important variables for targeting via proxy means testing are missing. This

16 For a detailed comparison of CHIP and CHFS data see Yue & Li (2013a, 2013b), Gan (2013a, 2013b) and Luo (2016).

leaves us with data from the two surveys CHIP and CFPS. Concerning the poverty structure, the poverty ratio (poverty headcount index PHI) is not a very suitable selection criterion though, as there is no consensus on the “true” level of national poverty. Official estimations range between 5.21% and 12.04% for 2009 (National Bureau of Statistics China 1992–2010, 2000–2014). However, the average *dibao* reception rate in rural population is closer to the total national average (6.9% in 2009 and 5.0% in 2007) in CFPS as compared to CHIP. In the end, this representativeness in terms of *dibao* recipients was the decisive criterion for choosing the CFPS over the CHIP dataset.

The data structure of the CFPS sample imposes certain limitations. For instance, there was no information on the value of a specific asset to capture depreciation or different cost of purchase. Therefore, the weights designed to specific household assets might be biased for assets of very high or very low market value. The inclusion of other demographic variables though should reduce the extent of potential bias. A further problem is that the required explanatory variables were available only on household level, while in practice, both households and individuals can apply. However, the ability of family and household members to provide for the applicant is a relevant eligibility criterion, therefore the household level seem a valid unit for classification purposes.

The data from the CHIP survey meanwhile was used for testing external validity of income and poverty estimation. For 8000 households from nine provinces, a special questionnaire designed by CHIP covered additional, more detailed questions on household characteristics (China Institute For Income Distribution 2012).

Own population sampling weights were added to this smaller dataset to restore its representativeness in terms of the population share of the geographical region: For statistical reasons, China is often divided into the four regions (see also figure 10) due to the country’s strong spatial socioeconomic differences. In reference to this division, the population weight is defined as the stratum’s population share divided by the stratum’s sample share, multiplied by the ratio of the national population to the total sample size.

$$w_i^k = \frac{s^k N}{s^k n}, \quad (17)$$

where S^k is the population share of region k in the national population N , and s^k is the population share of the sample region k in the overall sample population n . This weighting approach is discussed more in detail in Li et al. (2015).

4.3 RESULTS

In the first part of this results section, the accuracy of the current targeting scheme and hypothetical accuracy of alternative targeting mechanisms are assessed. In the second section, the actual and hypothetical anti-poverty effect of the selected targeting mechanism will be tested. As mentioned, several targeting scenarios will be benchmarked against the current targeting mechanism: Among them are geographical targeting (assignment to all households in the poorest two counties), distribution according to one demographic criterion, and several forms of parametric and non-parametric PMT (probit, discriminant function analysis, OLS, piecewise regression, quantile regression, and principal components analysis). In addition, these targeting schemes will also be compared with purely random selection of beneficiaries and distribution under perfect information about the welfare level of households serving as benchmarks specified at the lowest and highest boundary of targeting accuracy.

4.3.1 Efficiency of income estimation and poverty classification

Table 8 depicts several measures of prediction accuracy with respect to income prediction and income rank prediction for those four estimation models that actually produce estimations of income or income rank, ols, piecewise regression, quantile regression and PCA.¹⁷ Probit and discriminant function analysis are not reviewed at this point, since both methods only estimate the probability of a household being located below a specific poverty line. The table contains measures for the rank correlation of observed and predicted income (Kendall's τ), the total sample's average squared estimation error ($\bar{\varepsilon}_i^2$) and relative accuracy of income estimation at the respective segment border (ε_{Q_i}).

Among the featured models, PCA produces the least accurate rank estimates, according to the Kendall's τ displayed in table 8. The other estimation models are very similar concerning their rank correlation. While PCA again produces the largest overall errors ($\bar{\varepsilon}_i^2$), OLS regression performs best. However, OLS regression is by no means the most accurate technique to estimate low incomes. At the decile thresholds at the low tail of income distribution, we can observe by far the smallest error for quantile regression and the largest error for OLS regression and PCA. Meanwhile, PCA and OLS regression produce the smallest error at the 9th decile, while quantile regression produces exorbitant estimation errors at the high tail of income distribution. These results show clearly how the relative assessment of estimation methods depends on the intended application of estimation results.

¹⁷ Even though PCA produces only welfare scores, pseudo income estimates can be generated with the procedure described earlier.

Table 8: Accuracy of income estimation

	ols	pw10	qr	pca
Kendall's τ	0.376	0.376	0.351	0.272
MSE	94,581,016	97,277,200	120,733,064	163,391,808
$\epsilon_{(0.1)}$	1.749	0.719	0.488	0.815
$\epsilon_{(0.2)}$	1.041	0.304	0.595	0.211
$\epsilon_{(0.3)}$	0.744	0.176	0.626	0.106
$\epsilon_{(0.4)}$	0.523	0.033	0.666	0.022
$\epsilon_{(0.5)}$	0.380	0.085	0.681	0.101
$\epsilon_{(0.6)}$	0.271	0.127	0.693	0.178
$\epsilon_{(0.7)}$	0.155	0.266	0.710	0.229
$\epsilon_{(0.8)}$	0.062	0.297	0.725	0.242
$\epsilon_{(0.9)}$	0.086	0.381	0.745	0.190

Data Source: Institute of Social Science Survey of Peking University 2010

More interesting for this study though is the targeting accuracy. In table 9, three selected criteria of targeting efficiency are reported. In the reduced CFPS sample, 9% of the rural households were classified as poor according to their reported income, while only 4.7% of the sample households (231 households) received rural *dibao* payments. This discrepancy led to an extreme exclusion rate of 89.2% under the current scheme (“actual”), a total of 397 poor households that were not covered by rural *dibao*. At the same time, the majority of recipient households (79.2%) were not situated below the poverty line. In total, this meant an incidence rate of mistargeting of 11.8%.

Under the given financial constraint (i.e. keeping the total expenditure and the quota per province fixed) and perfect information about the welfare level of households (scenario “perfect”), nearly half of the poor could have been assigned *dibao* quotas, reducing exclusion to 52.8%. Even under perfect targeting, the limitation of quotas thus led to the exclusion of 235 eligible households. The leakage of funds in the “perfect” scenario can be explained by the use of relative poverty thresholds: The

21 leakage households lived in comparatively rich regions, where an income level just above the poverty line made them belong to the poorest households in that area. Since benefits were distributed by a relative regional poverty threshold, these households were identified as poor (since they belonged to the poorest quantile of that region) even though they would not have qualified for benefits under an absolute poverty line.

The performance among the other models was ambivalent. When it comes to exclusion, leakage and total mistargeting, geographic targeting is less accurate even than random targeting. The weak performance of geographic targeting however is no surprise, since it was previously assumed that income distribution would be too heterogeneous already in China for adaption of this targeting mechanism. Unexpectedly, the current targeting mechanism ("actual"), a mixture of means testing and community targeting with additional demographic components performs worse according to most criteria than even the extremely simple demographic targeting mechanism. The least exclusion was produced by the PMT methods, while demographic targeting features the least leakage. The targeting differential (TDI), which takes both estimation errors into account, is lowest for geographical and random targeting and highest for probit regression. The TDI of the actual targeting process was only at about half the level of the best performing PMT methods.

Table 9: Accuracy of poverty classification (n=4,924)

	Exclusion		Leakage		TDI
	absolute	in %	absolute	in %	
actual	397	89.2%	183	79.2%	0.067
demo	381	85.6%	139	68.5%	0.113
geo	418	93.9%	240	89.9%	0.007
probit	373	83.8%	159	68.8%	0.126
ols	375	84.3%	161	69.7%	0.121
pw	374	84.0%	160	69.3%	0.124
qr	375	84.3%	161	69.7%	0.121
pca	381	85.6%	216	77.1%	0.096
discr	382	85.8%	168	72.7%	0.104
random	431	96.9%	223	94.1%	-0.018
perfect	235	52.8%	21	9.1%	0.467

Data Source: Institute of Social Science Survey of Peking University 2010

To fully understand the consequence of the choice of targeting mechanism on the distribution of funds, it is necessary to analyze the income rank and income level of households that were incorrectly classified. Table 10 displays the prevalence of total misallocation, exclusion and leakage of current *dibao* targeting across income strata, which are defined by province-level welfare deciles. The first finding is that indeed a considerable share of leakage happened in the 2nd to 4th income segment, i.e. close to the respective poverty threshold. Fortunately, the probability of leakage decreased in the higher deciles. However, some “leakage” households (10.38% of total leakage) were even found in the top two deciles of total income distribution, which clearly is outside any tolerance range. For exclusion, the picture is clearer: While the overwhelming majority of exclusion cases took place in the first income decile, the probability of exclusion of poor households (error likeliness) increased with proximity to the poverty line. However, the accuracy of poverty classification among regional income deciles does not seem a very suitable tool because of the

fact that in very rich regions, household could be situated in the bottom decile and still be situated above the poverty line (and vice versa).

The differentiation between mistargeting around the poverty line and more extreme cases of mistargeting is crucial for assessing the accuracy of targeting methods. As could be seen in table 10, the assessment of mistargeting along income deciles is not very helpful due to varying poverty thresholds. To truly learn about the severity of misclassification, the distance of each misclassified household to the local poverty threshold is decisive.

Table 10: Accuracy of poverty classification among regional income deciles

Income segment	Leakage			Exclusion frequency		
	Absolute incidence	Decile share	Error likelihood	Absolute incidence	Decile share	Error likelihood
1	10	5.46%	7.69%	328	82.62%	87.47%
2	38	20.77%	8.74%	55	13.85%	98.21%
3	24	13.11%	5.02%	14	3.53%	100.00%
4	27	14.75%	5.49%	0	0.00%	.
5	23	12.57%	4.70%	0	0.00%	.
6	15	8.20%	3.03%	0	0.00%	.
7	12	6.56%	2.42%	0	0.00%	.
8	15	8.20%	3.07%	0	0.00%	.
9	14	7.65%	2.83%	0	0.00%	.
10	5	2.73%	1.04%	0	0.00%	.

Note: „Error-likeness“ for the 3rd decile adapted to smaller group size.
Data Source: Institute of Social Science Survey of Peking University 2010

Table 11 lists the various targeting scenarios concerning the absolute gap between observed income and the *dibao* standard (in 1000 RMB) in case of misclassification and the Mistargeting Severity Index (MSI) as defined in section 4.1.5:

Table 11: Gap between absolute poverty threshold and observed income

		actual	demo	geo	probit	ols	pw10	qr	pca	discr	random
Absolute gap (in 1000 RMB)	Total	760	857	970	577	563	567	606	905	666	1,341
	Exclusion	225	223	258	225	222	222	226	230	227	253
	Leakage	535	634	712	353	340	344	380	675	439	1088
Relative gap (MSI)	Total	14.7%	14.1%	21.8%	10.4%	10.3%	10.4%	11.0%	17.2%	12.3%	25.1%
	Exclusion	3.6%	3.6%	4.1%	3.6%	3.5%	3.5%	3.6%	3.7%	3.6%	4.1%
	Leakage	241%	261%	330%	150%	148%	150%	162%	236%	189%	441%

Data source: Institute of Social Science Survey of Peking University 2010

Unsurprisingly, the maximum of total welfare gaps could be observed for random distribution (1,341.000 RMB). Among non-randomly targeted transfers, the sum of welfare gaps was highest for geographical targeting (about 72% of that of random targeting) while the lowest sum of welfare gaps was produced by OLS and piecewise regression (about 42–43% of that of random targeting). The variation however mostly stems from leakage gaps, which are ranging between 712.000 RMB (geographic) and 340.000 RMB (OLS). Exclusion, on the other hand, varied to a lesser degree because the range of possible incomes below the poverty threshold was smaller. Here, the actual distribution mechanism outperformed not only geographic targeting but also several PMT techniques.

The MSI measures the average income gap in relation to the actual poverty line. As can be seen in the lower section of table 11, piecewise regression once again performs best among the featured models. Whereas the models' differences according to exclusion are not large (due to the fact that the bandwidth of income levels below the poverty line is much smaller than above), distinct differences can be found for the

leakage-MSI: As expected, random and geographic targeting perform worst. For the actual targeting process, misclassified households' average distance to the poverty line was more than two times the value of the local poverty line. Only demographic and geographic targeting performed worse, for the latter of which the income gap of benefits was more than three times the poverty threshold.

Graphically, the welfare gap can also be displayed using boxplots, which show the 25% quantile, the median and the 75% quantile of the total distribution (interquartile range, IQR) against the minimum and maximum of welfare gaps (see figure 11). The IQR was largest for demographic targeting and actual targeting. However, demographic targeting had the highest maximum distance to the poverty line among leakage household. While statistical dispersion measured by IQR was lower for geographic targeting and targeting by PCA, these scenarios still allowed for considerable outliers. The lowest maximum values can be observed for the OLS-based parametric income estimation, especially quantile regression, which also had the smallest IQR. Thus we can explain why demographic targeting and PMT via quantile regression produced large welfare gap while their targeting performance was relatively good: Put simply, these methods worked rather well in identifying poor households, but also included a certain amount of extreme under- and over-estimations (or classifications). This property might make them seem like unappealing techniques when only looking at the sum of welfare gaps.

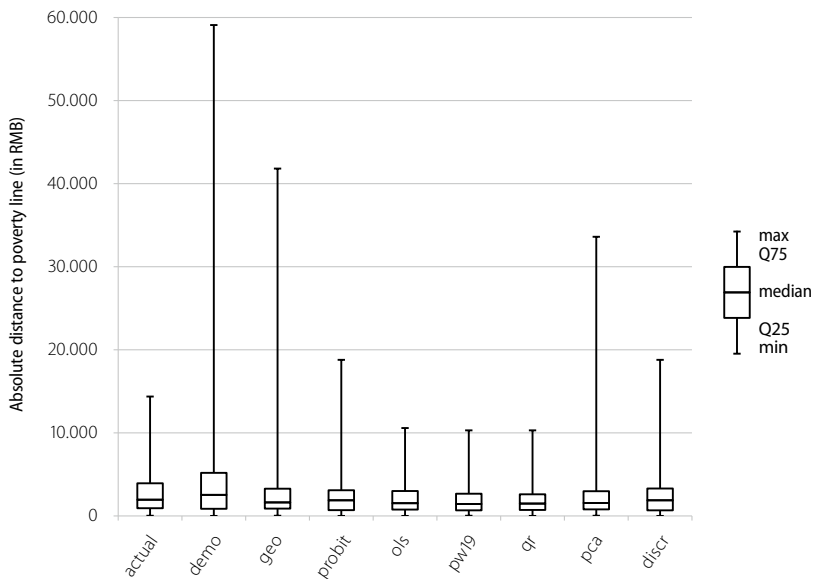


Figure 11: Boxplot of income gaps across targeting methods

Own illustration. Data source: Institute of Social Science Survey of Peking University 2010

A basic benefit incidence analysis can help to assess the pro-poorness of *dibao* transfers by observing the distribution of funds across local income deciles. This method is generally seen as an informative assessment tool for the distributional incidence of program benefits by analyzing the allocation of benefits across population groups according to their provision cost (Demery 2003) and has been in use since the late 1970s (e.g. Selowsky 1979). However in this case, the provision cost is simply the *dibao* grant itself. The administrative cost of provision is not included due to the problems of establishing an isolated number: The total administrative cost of the civil affairs administration, which amounted to 49 billion RMB in 2013, does not only contain administrative expenses of the rural *dibao* but also other programs and responsibilities, which the bureaus for civil affairs is handling at the same time (China State Finance Magazine 2014).

For this study, the distribution of expenditure for *dibao* grants across income groups is used as measure for the pro-poorness of the *dibao* program. As displayed in table 12, the actual distribution achieved a rather low distribution of funds to the poorest 20% (42.1%) but also only moderate spill-over to the richest 20% of the population (7.7%). The largest share of funds to the poorest 10% in every province was assigned by piecewise regression (33.3%), the smallest by geographic and random targeting (14.2% and 7.0%, respectively). Among the PMT scenarios, PCA again performed worst (25.8%). The highest cumulative share to the poorest 30% was provided by probit regression (68.2%), the lowest by geographic targeting (40.4%). Furthermore, considerable spill-over effects occur for geographic and demographic targeting: 11.1% and 10.6%, respectively, were allocated to the richest 20% of all households. The least spillover to the richest 20% is produced by the PMT-models, foremost piecewise regression and OLS. All in all, demographic targeting and probit regression attributed the largest share of funds to the poor, while the distribution was least pro-poor under the actual targeting and PCA.

Table 12: Distribution of *dibao* funds across observed provincial income groups (n=4,924), values in %

		1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th	poor
actual	suc.	25.6	16.5	13.3	10.7	10.0	4.9	4.8	6.5	5.8	1.9	22.0
	cum.	25.6	42.1	55.3	66.0	76.0	80.9	85.7	92.2	98.1	100.0	
demo	suc.	33.2	19.7	11.5	5.2	9.1	3.7	2.9	4.1	1.4	9.2	31.0
	cum.	33.2	52.9	64.4	69.5	78.6	82.4	85.3	89.4	90.8	100.0	
geo	suc.	14.2	14.5	11.7	13.9	11.8	7.6	7.9	7.4	6.5	4.6	10.3
	cum.	14.2	28.7	40.4	54.2	66.0	73.6	81.5	88.9	95.4	100.0	
probit	suc.	32.3	21.3	14.5	11.4	6.5	4.6	4.4	3.3	1.6	0.0	30.6
	cum.	32.3	53.6	68.2	79.5	86.1	90.7	95.1	98.4	100.0	100.0	
ols	suc.	32.9	21.4	12.7	11.2	7.2	5.5	5.5	2.3	0.3	0.9	29.4
	cum.	32.9	54.3	67.1	78.3	85.4	91.0	96.4	98.7	99.1	100.0	
pw10	suc.	33.3	20.9	12.7	11.3	7.2	5.0	5.5	2.3	0.3	1.4	29.8
	cum.	33.3	54.2	67.0	78.3	85.5	90.5	96.0	98.3	98.6	100.0	
qr	suc.	32.0	20.9	13.9	9.4	8.8	4.2	5.3	2.8	1.5	1.3	29.9
	cum.	32.0	52.8	66.7	76.1	84.9	89.1	94.4	97.2	98.7	100.0	
pca	suc.	25.8	16.7	13.0	13.9	7.8	7.5	5.6	5.1	1.3	3.4	22.4
	cum.	25.8	42.5	55.4	69.3	77.1	84.6	90.1	95.3	96.6	100.0	
discr	suc.	29.2	21.4	15.4	10.2	9.2	5.0	3.2	4.4	0.9	1.3	26.3
	cum.	29.2	50.6	66.0	76.1	85.3	90.3	93.5	97.8	98.7	100.0	
perfect	suc.	100.0	0	0	0	0	0	0	0	0	0	90.9
	cum.	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	100.0	
random	suc.	7.0	9.5	9.9	9.7	12.3	11.0	11.7	10.7	8.7	9.7	5.9
	cum.	7.0	16.5	26.3	36.1	48.4	59.3	71.0	81.7	90.3	100.0	

Note: Poverty defined by an absolute local poverty threshold

Data Source: Institute of Social Science Survey of Peking University 2010

The same information can be visualized by concentration curves (see figure 12), which plot percentiles of the population against their respective cumulative share of transfers awarded to them. Fortunately, none of the scenarios is regressive, i.e. features an inverse concentration curve.

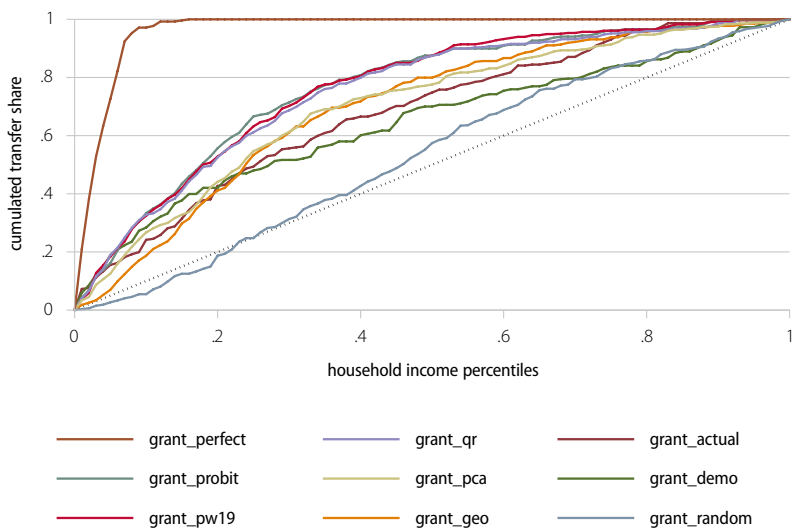


Figure 12: Concentration curves of *dibao* grant allocation

Own illustration. Data source: Institute of Social Science Survey of Peking University 2010

4.3.2 Effectiveness of rural *dibao* in terms of poverty reduction

The inefficiencies of current and potential targeting schemes are very likely to impact the effect of social transfers. However, Ravallion (2009b) argues that most indicators for targeting efficiency are only approximate predictors for a system's anti-poverty effect. Therefore, this study simulates the poverty impact under the hypothetical targeting schemes featured above and compares it with the effect of the actual distribution of benefits (under the existing budget restrictions). The outcome of interest is the impact of social assistance transfers on poverty levels, poverty being defined as per capita family net income (less actual *dibao* transfers) below the regional *dibao* standard. Both the absolute and relative changes in poverty rates induced by allotting *dibao* transfers are reported. Furthermore, the effect of transfers on the income inequality, measured by the GINI-index, will be compared. Undeniably, statistical approximation omits for instance the effects of crowding out of family support or own labor, but also indirect anti-poverty effects of transfers or trickle-down effects in a community, which are unknown. Also irregularities in the targeting implementation, which might differ across targeting methods, can't be incorporated in this simulation, but have to be addressed at a later point.

Table 13 features the anti-poverty effect of transfers in the baseline scenario. Generally, the statistical anti-poverty effect in this sample was rather small, both in absolute and relative terms. However, certain differences could be observed across targeting methods: The highest anti-poverty effect was generated by distribution according to demographic criteria: Due to the distinct pro-poor allocation of funds, transfers targeted by demographic criteria could reduce the poverty rate by 7.2% and move 32 households out of poverty. Among PMT scenarios, quantile regression performed best, reaching a poverty reduction of 5.8% (=26 households). The lowest performance was achieved by geographic targeting and the actual transfers, which only lifted a handful of households out of poverty

(2.5% and 2.7%, respectively). The effect on inequality was practically negligible as none of the models could decrease the high GINI by more than one percentage point.

Table 13: Anti-poverty effect of rural *dibao* transfers (n=4,924)

	Without	Actual	Demographic	Geographic	Proxy-Means testing						Perfect	Random
					Probit	OLS	PW10	QR	PCA	DFA		
Poverty reduction relative	–	2.5%	7.2%	2.7%	5.4%	4.3%	4.5%	5.8%	3.4%	4.9%	11.7%	0.7%
Poverty reduction absolute	–	11	32	12	24	19	20	26	15	22	52	3
Poverty rate after <i>dibao</i>	9.0%	8.8%	8.4%	8.8%	8.5%	8.7%	8.6%	8.5%	8.7%	8.6%	8.0%	9.0%
GINI	0.495	0.482	0.480	0.481	0.481	0.481	0.481	0.481	0.482	0.481	0.481	0.482

Data source: Institute of Social Science Survey of Peking University 2010

Considering these small differences between the tested targeting methods, it is questionable whether accuracy of targeting is the only problem for poverty reduction. This notion is supported when looking at the effect of target model “perfect”. Even under perfect poverty classification (i.e. under perfect knowledge about the poverty status of every household), the available funds could generate an anti-poverty reduction of merely 11.7% (or 52 households). The only explanation is that either the total sum of available transfers was insufficient to provide for all poor (program coverage) or that the individual grant were not high enough to get large or very poor households above the poverty line (grant level).

4.3.3 Robustness tests

Above it was shown that the presented targeting approaches lead to different degrees of anti-poverty effect. In the following, it shall be tested how the anti-poverty impact of *dibao* transfers is influenced by a) more flexible grant height per recipient and b) higher program coverage (as the total amount of recipients admitted to the program). These two tests are intended to provide additional information to this section's fundamental question: Is the low performance of the featured models a signal that policy makers should put more efforts into developing a more accurate identification strategy or are other changes in program design recommendable? A third robustness test is conducted to c) test whether the usage of absolute poverty lines/eligibility thresholds may change the assessment of the mentioned targeting methods, efficiency and effectiveness of the mentioned models. Table 14 illustrates the four main scenarios that will be presented additionally to the baseline scenario in the previous section, including a test for external validity, which is presented in the next section. Since there were large similarities between DFA and probit, and OLS and PW19, the robustness analysis will be conducted only for a reduced number of PMT models. The final robustness test analyzes the d) external validity of targeting by PMT methods by model transfers between two independent datasets.

Table 14: Scenarios for measuring the anti-poverty impact

	Grants level	Program coverage	Poverty line	Model transfer
Baseline scenario	Village average	231 households (=4.7%)	Relative	No
Scenario 2: "Flexible funds"	Individual poverty gap	231 households (=4.7%)	Relative	No
Scenario 3: "High coverage"	Village average	444 households (=9%)	Relative	No
Scenario 4: Absolute poverty line	Village average	231 households (=4.7%)	Absolute	No
Scenario 5: Model transfer	Village average	231 households (=4.7%)	Relative	Yes

Source: own compilation

4.3.3.1 Robustness against changes of grant height

In the simulation described above (henceforth: the baseline scenario), the grant height was uniform across the recipients of a village. Officially however, the grants are supposed to be high enough to cover entirely the gap between true income and *dibao* standard. Thus, many regions over the past years decided to introduce *dibao* classes, according to which recipients are granted different levels of transfers according to their depth of poverty. To test whether the baseline's scenario assumption of flat-rate-type grants may influence the previous evaluation of poverty impact, it is necessary to rerun the simulation with different grant levels. This robustness test assumes lenient budget control in the sense that the grant sum is fully flexible but the total number of recipient households still remains at the original level of 231 households.

Concerning the grant levels, full responsiveness to the actual depth of poverty is assumed. The depth of poverty in this case is defined as the gap between observed income and the respective local *dibao* standard. Obviously, under imperfect targeting methods, the true depth of poverty would be unknown. In reality, a similar arrangement could be reached if the targeted households (and only them!) are subject to a detailed means-test by trained evaluators to ascertain their true income level and the required grants.¹⁸

Table 15 presents the potential anti-poverty effect of *dibao* grants under a policy setting where the number of recipients is fixed but grants are optimally adapted to the depth of poverty. The scenario "perfect" in this case again describes the poverty effect under perfect knowledge about the poverty status like in the baseline scenario. Since in this case, every targeted household could receive benefits that lifted it across the poverty line, the "perfect" scenario reached a poverty reduction of 47.2%. As can be seen, the increase of *dibao* grants for targeted beneficiaries improved the effect of *dibao*, especially for those targeting scenarios that

¹⁸ It needs to be stressed that in the scenario of fully flexible grants, leakage of funds is implicitly ruled out: Households that are targeted by estimated income receive still receive zero grants, as their income gap according to observed income is negative.

performed best in identifying low-income households (probit and piecewise regression models). This time, also the difference in effectivity between PMT-models and alternative models was more distinct. In the best case, *dibao* transfers were able to reduce poverty by 16.2% (probit), which corresponds to still 1/3 of the anti-poverty effect achieved by the “perfect” scenario. In the “flexible funds” scenario, actual targeting reached a poverty reduction of 10.8%.

Table 15: Anti-poverty effect of rural *dibao* transfers, “flexible funds” scenario (n=4,924)

	Without	Actual	Demographic	Geographic	Proxy-Means testing				Perfect	Random
					Probit	PW10	QR	PCA		
Poverty reduction relative	–	10.8%	14.4%	6.1%	16.2%	16.0%	15.7%	14.4%	47.2%	3.1%
Poverty reduction absolute	–	48	64	27	72	71	70	64	210	14
Poverty rate after <i>dibao</i>	9.0%	8.06%	7.74%	8.49%	7.58%	7.60%	7.62%	7.74%	4.77%	8.75%
GINI	0.495	0.482	0.481	0.482	0.481	0.481	0.481	0.481	0.477	0.482

Data source: Institute of Social Science Survey of Peking University 2010

4.3.3.2 Robustness against changes of program’s coverage

The previous analysis assumed a form of narrow targeting restricted with regards to the total number of targeted households: For the baseline scenario and the “flexible grants scenario”, the number of targets was fixed at 231 households. Without such a limitation, a larger share of the population could be reached, however giving up the original budget control.¹⁹ Because it is hard to predict how suitable the mechanisms are in

¹⁹ Also from a theoretical point of view, the available budget is assumed to increase with growing coverage due to larger political support for broadly targeted programs (see for instance Klasen and Lange 2016: 19 f).

classifying larger shares of the population, the change in the coverage of the program might impact the relative performance of different targeting scenarios. Thus the results presented above should be tested for robustness to an increase of the program size. Originally used in clinical medicine (Zweig and Campbell 1993), the so called Receiver operating characteristic (ROC) analysis is an interesting tool for illustrating the trade-off between decreasing exclusion by increasing the program scope and the accompanying surge of leakage.

The ROC-curve displays the rate of true positive classifications over all eligible households (true positive rate, TPR) against the rate of false positive classifications over all non-eligible households (false positive rate, FPR). In the following, the true positive rate TPR (also called the sensitivity of the classification in Wodon 1997) and the false positive rate FPR are defined as

$$TPR = \frac{a}{a+c} \text{ and } FPR = \frac{b}{b+d} \quad (18)$$

Where a is the number eligible beneficiaries, b is the number of non-eligible beneficiaries, c is the number of eligible non-recipients, and d is the number of non-eligible non-beneficiaries (also compare table 6).

Previous approaches have used ROC-curves to compare the accuracy of different poverty classification models (Wodon 1997; Klasen and Lange 2015). This study is also interested in yet another practical application of ROC-curves. Plotting TPR against FRP traces and displays the amount of additional leakage that Chinese policy-makers have to expect if they venture to decrease exclusion by successively increasing the program coverage (i.e. the number of targeted households). Figure 13 displays the ROC-curves for increasing levels of program coverage for four different targeting mechanisms. Each increase of program coverage between zero and 100% of the population resulted in an increase of both true and false classifications, which was plotted along the y-axis and the x-axis, respectively. The grey line (random) represents the bottom line of perfectly random beneficiary identification, for which each increase in sensitivity (TPR) is reflected by the same increase in leakage (FPR). The area under

the curve (AUC) can be used to describe the average performance of a classifier and ranges from zero to one. In this sense, probit regression performs best on average among the available models (AUC=0.78), signifying that its average marginal leakage is smallest. Among the featured models, the area under the curve is smallest for PCA (AUC=0.63). On average, the ROC-curves support the previous findings on the relative performance of the different PMT-models. However, it should be noted that the ranking is not uniform across all the whole range of possible program sizes: For instance, quantile regression and piecewise regression slightly outperform each other at different levels of program size.

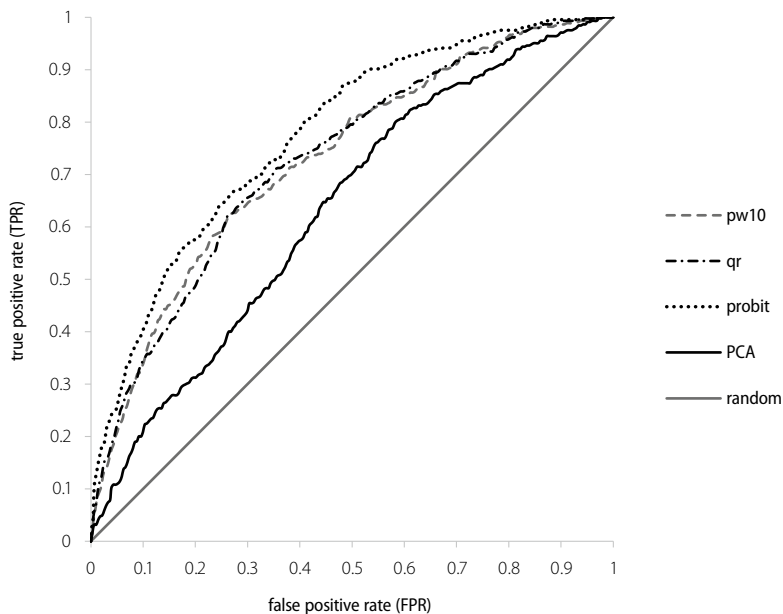


Figure 13: ROCs along varying program size (0–100% of the population)

Own illustration. Data source: Institute of Social Science Survey of Peking University 2010

Additionally, the slope of the ROCs allows some careful speculation concerning the “optimal” program size: As the slope of the curve is especially steep at the extreme left, an increase in program size in that section would decrease exclusion significantly without translating into corresponding increase of leakage in this case. A turning point is only reached where the slope is 45 degrees and the curve has a maximum orthogonal distance to the “random targeting” reference line. For piecewise regression, this point is reached at about 1580 recipients (corresponding to about 35% of the sample population), when the rate of false positive classifications (FPR) increases faster than the rate of correctly identified poor (TPR). This turning point represents the maximum program size still sensible from the perspective of the marginal leakage rate.

However, program coverage of 35% of the rural population is not realistic, given that the Chinese policy makers mandate rather narrow targeting. A politically more enforceable benchmark might be the sample’s poverty headcount (without current transfers), which is about 9% or 444 households²⁰. In the next step, it is simulated how much an increase in program size up to the current poverty headcount would influence the anti-poverty effect of the transfers.

Table 16 shows the simulated anti-poverty performance of the chosen models under this scenario. Due to the flat rate grants, even in the best case of full knowledge on the poverty status of the sample households only 29.2% of the poor could be lifted out of poverty. The best alternatives achieved 7.2–8.5% of poverty reduction, which is considerable increase as compared to the baseline scenario. However, the additional poverty reduction produced by expanding the program varied between the models. The poverty reduction of funds targeted with unsuitable methods could hardly be improved by expanding the coverage.

20 The poverty level was again calculated by using local *dibao* standards as absolute poverty lines.

Table 16: Anti-poverty effect of rural *dibao* transfers, “high coverage” scenario (n=4,924)

	Without	Actual *	Demographic	Geographic	Proxy-Means testing				Perfect	Random
					Probit	PW10	QR	PCA		
Poverty reduction relative	–	2.5%	9.0%	2.9%	8.3%	7.2%	8.5%	3.8%	29.2%	2.2%
Poverty reduction absolute	–	11	40	13	37	32	38	17	130	10
Poverty rate after <i>dibao</i>	9.0%	8.81%	8.23%	8.77%	8.29%	8.39%	8.27%	8.69%	6.40%	8.83%
GINI	0.495	0.482	0.479	0.481	0.479	0.479	0.479	0.481	0.478	0.481

Note: Columns marked with * feature results from the baseline scenario
 Data source: Institute of Social Science Survey of Peking University 2010

4.3.3.3 Robustness against changes of the absolute poverty line

As indicated above, the use of absolute poverty lines might impact the accuracy of certain income estimation models due to their tendency to decrease variance of estimated incomes. Therefore, the above analysis assumed a relative poverty line to identify poor households. While this approach is more reliable for PMT, it is not strictly in line with actual eligibility criteria, which define poverty by absolute income and not as a specific quantile of income distribution. Thus, it is necessary to test whether the previous results hold also for the case of an absolute eligibility threshold, for instance the regional poverty line. Specifically, it would be interesting to learn about the impact of using an absolute poverty line on the incidence of leakage and exclusion and the anti-poverty effect of the models and their relative performance. In detail, exclusion and leakage under the absolute national poverty lines and under relative poverty lines are reported at fixed percentages of the sample median or at specific income quantiles.

Figure 14 and figure 15 display the relative incidence of exclusion and leakage, respectively, under various absolute and relative poverty lines. In both figures, absolute poverty lines range between 1000 and 4400 RMB, which corresponds to the 10th to the 50th percentile of population as used for the relative poverty lines.

Figure 14 displays how exclusion changes if poverty and eligibility are determined by an absolute poverty line (left graph) instead of the original relative threshold (right graph). Except for quantile regression, the use of absolute poverty lines increased the exclusion in the sample for OLS, pw19, and PCA up to nearly 100%. Apparently, this increase was because low incomes were on average overestimated so that fewer households were classified as poor under low absolute poverty lines. However, the listed estimation models are affected differently by this effect. While exclusion rates of PCA and piecewise regression soon drop to moderate levels, OLS regression continues to produce high exclusion. Comparing with the relative estimation errors at quantile thresholds featured in table 8, these different reactions can be explained by the models strengths and weaknesses in estimating absolute incomes across different income segments. The only exception is quantile regression, which produces very low exclusion rates due to its high accuracy at the low tail of income.

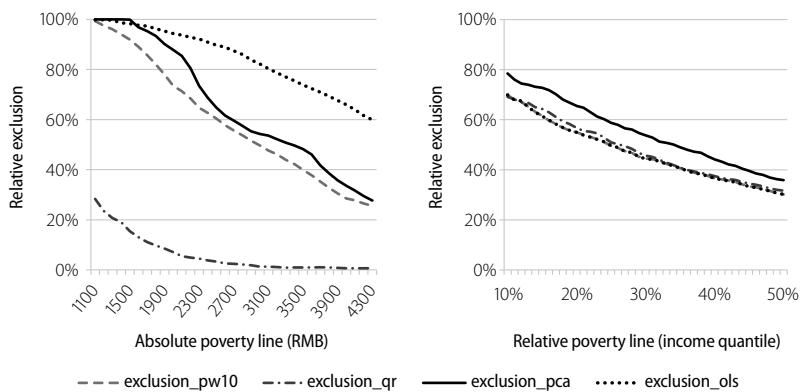


Figure 14: Exclusion under shifting absolute and relative poverty lines

Own illustration. Data source: Institute of Social Science Survey of Peking University 2010

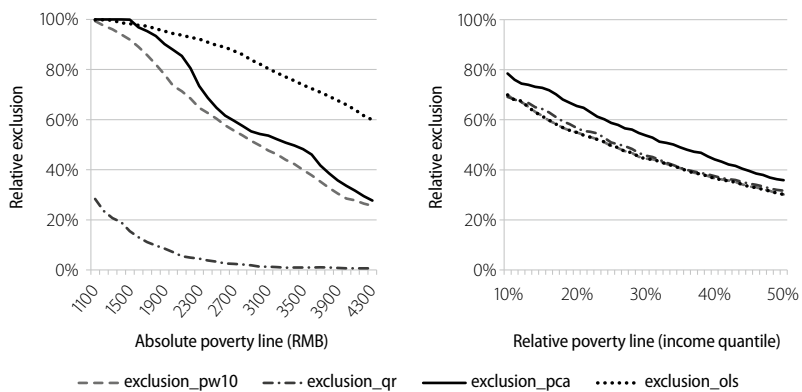


Figure 15: Leakage under shifting absolute and relative poverty lines

Own illustration. Data source: Institute of Social Science Survey of Peking University 2010

The reverse results could be found for leakage (see figure 15), which was considerably lower for the absolute poverty in the low range for the same reason: Low incomes were systematically overestimated so that barely any households would have been admitted to the system. Those few that qualify for transfers under this setting are mostly very deep in poverty. Therefore, the results at very low poverty lines are hardly meaningful. At higher poverty lines, the differences between the estimation methods became more pronounced. The exorbitant leakage rates of quantile regression along absolute poverty lines are the price for the method's strong focus on the low tail of income. The other three methods produced similar leakage as under relative poverty lines, PCA again performing a little worse than OLS and piecewise regression. Again, these differences in performance are closely linked to the ability of the models to estimate absolute incomes in different income segments (see table 8).

All in all, the section above supports the previous statement that absolute poverty lines may be problematic for narrow targeting by PMT techniques because of the extreme exclusion rates. Further, the tendency of the specified quantile regression to reduce exclusion at the cost of leakage is becoming very distinct in the figures above. Also the trade-off between leakage and exclusion becomes more evident under absolute poverty lines. In this setting, model choice depends on political preference of leakage and exclusion: While OLS regression minimizes leakage, quantile regression reduces exclusion. A compromise is piecewise regression, which returns relatively moderate levels of both leakage and exclusion.

4.3.3.4 External validity of proxy means testing

However, these results have practical implications only to a limited degree, since in real life applications, estimations are conducted outside the calibration sample. Model transfers from calibration to estimation sample are, as discussed above, likely to increase estimation errors. Therefore, this study conducts a final robustness test by simulating a model transfer

using a sample from the 2007 CHIP household survey as calibration sample to estimate income in the 2009 CFPS sample. This model transfer simulates a worst case scenario, in which calibration and estimation sample differ in terms of time of data collection, income collection method (account-keeping instead recall-type survey) and sample regions. Thus, the model transport is expected to induce a bias in efficiency and effectivity. In detail, the regression parameters estimated in the CHIP dataset were transferred to the CFPS data to estimate income and poverty status, which then were used as a basis for *dibao* targeting. Due to the fact that PCA makes no reference to or use of observed income data, there is in fact no true model transfer as the welfare score is generated independently for every new sample. For methodological reasons, model transfers could also not be conducted for piecewise regression.

Table 17: Out-of-sample prediction accuracy (fitted in CHIP data)

	probit	ols	qr
Kendall's τ		0.298	0.281
$\bar{\epsilon}^2$		96,790,672	117,141,336
$\epsilon_{.}(0.1)$		1.646	0.030
$\epsilon_{.}(0.2)$		0.952	0.292
$\epsilon_{.}(0.3)$		0.686	0.389
$\epsilon_{.}(0.4)$		0.455	0.467
$\epsilon_{.}(0.5)$		0.309	0.524
$\epsilon_{.}(0.6)$		0.192	0.570
$\epsilon_{.}(0.7)$		0.063	0.617
$\epsilon_{.}(0.8)$		0.045	0.657
$\epsilon_{.}(0.9)$		0.201	0.715
Exclusion	90.3%	90.3%	90.3%
Leakage	81.4%	81.4%	81.4%
BPCA	-171	-171	-171

Data source: Institute of Social Science Survey of Peking University 2010

As illustrated in table 17, the model transfer did not necessarily produce less accurate income estimation. In fact, the accuracy of income estimation increased for quantile regression both across the whole sample and at the quantiles. However, these numbers are misleading since they are mainly a result of on average less overestimation of low incomes. At the same time, the transported estimation could not capture high incomes, which lead to strongly increased leakage. While estimated incomes were on average closer to the observed value at the low tail of income distribution, the quality of ranking decreased, as reflected by the much lower rank correlation. Consequently, also exclusion of beneficiaries increased.

Thus the anti-poverty effect of the parametric estimation models is likely to suffer from the decreasing accuracy of income and poverty prediction, too. As can be seen in table 18, the anti-poverty-effect of all the targeting techniques that are based on parametric regression (probit, OLS and quantile regression) decreased considerably after the model transfer. Ultimately, a model transport can reduce the accuracy of income and poverty prediction to such a degree that the PMT-targeted transfers' anti-poverty effects drop to the level of actual transfers and considerably below demographic targeting. The anti-poverty effect of OLS regression, the PMT technique that was least impacted by the model transfer, dropped to 3.1% or 14 households, which was only a little higher than baseline geographic targeting and the actual poverty classification. In terms of GINI reduction, none of the techniques reaches any reduction worth mentioning. Assuming that in reality PMT-targeting always implies a model transfers, the tested PMT models would not bring about significant improvement in contrast to the actual targeting and be inferior to demographic targeting.

Table 18: Anti-poverty effect of rural *dibao* transfers, model transfer scenario (n=4,924)

	Without	Actual *	Demographic *	Geographic *	Proxy-Means testing				Perfect *	Random *
					Probit	OLS	QR	PCA*		
Poverty reduction relative	-	2.5%	7.2%	2.7%	2.2%	3.1%	2.2%	3.4%	11.7%	0.7%
Poverty reduction absolute	-	11	32	12	10	14	10	15	52	3
Poverty rate after <i>dibao</i>	9.0%	8.8%	8.4%	8.8%	8.8%	8.8%	8.8%	8.73%	8.0%	9.0%
GINI	0.495	0.482	0.480	0.481	0.482	0.482	0.482	0.482	0.481	0.482

Note: Columns marked with * feature results from the baseline scenario for comparison

Data source: Institute of Social Science Survey of Peking University 2010

All in all, it is arguable whether PMT is an acceptable tool for poverty classification under lab-conditions. The picture further deteriorates as soon as the fitted model is transferred outside the estimation sample. In a study by the author of this thesis, the PMT proved to be instable also for transfers between the mentioned surveys CFPS and CHIP and an additional datasets mentioned above, the CGSS. Further isolated transfers across time and survey region showed that the neither the survey wave nor the survey region contributed the main share of estimation errors, but rather the transfer between surveys of different income composition and income collection methods (Kuhn et al. 2016b). Therefore, any practical implementation would require that the calibration sample and the estimation sample are as close as possible in terms of sampling structure and income data collection. Otherwise, external validity is expected to be low and the model transfer is likely going to further decrease the accuracy of poverty classification.

4.4 SUMMARY

The analysis above aimed at testing whether social assistance is truly able to mitigate poverty in a considerable way or if is only achieving certain alleviation of the consequences of poverty. For this end, the current and alternative targeting schemes were tested concerning their accuracy in predicting income and poverty status and achieving poverty reduction. Further, several adjustments with regards to the grant height, amount of recipients and location of the poverty line were tested. Finally, the robustness of the income estimation and poverty classification to transfers of the estimation model beyond the calibration sample was tested.

This quantitative analysis produced several outcomes. First, this chapter showed that the allocation of funds is in fact rather inaccurate as compared to the policy goals. In addition, the potential anti-poverty effect as defined by local poverty lines was rather marginal as long as the official average grants were used. These results confirm that the targeting method may influence the accuracy and anti-poverty effect of *dibao* transfers. Judging from a basic benefit-incidence analysis, for instance demographic targeting and proxy means testing in this sample improved the accuracy of allocation and thus the anti-poverty effect as compared to the current targeting scheme. Considering the problem of low external validity of income estimation, the value of PMT for real-life application is however uncertain. Meanwhile, specifications in the robustness test show that a bigger share of variation in relevant outcomes was achieved by increasing the program coverage (i.e. the amount of recipients) and especially the grants per recipient. In both cases, the accuracy and impact of *dibao* transfers increased considerably.

At this point, this thesis contrasted the actual policy with a numerical simulation of other targeting methods to analyze the anti-poverty effect of rural *dibao*. Still, this analysis is incomplete as we have to assume that the actual *dibao* allocation was affected by several of the implementation issues described in the theoretical analysis. Therefore, it is necessary to a) find out whether there was indeed an implementation bias for the current targeting and b) whether this bias may also have impacted the implementation of the other targeting methods.

5 CASE STUDY ON
SOCIAL ASSISTANCE
IMPLEMENTATION

During quantitative analysis in the previous section, low performance of the actual targeting method could be measured. As discussed before, the direction and extent of these implementation issues is interesting for the assessment of the current policy as well as for comparison with alternative targeting methods. Therefore, the following chapter shall inquire into the reasons for the low performance of the current targeting method by drawing on a case-study in a few selected regions. In its course, this chapter will provide insights into why the current targeting mechanism may be inefficient and whether the identified defects may impact other targeting methods as well. In doing so, this analysis will provide further reference points for deriving policy options.

The case study combines quantitative data from two larger household surveys (Institute of Social Science Survey of Peking University 2010; Center for Chinese Agricultural Policy 2012, 2014) and qualitative data from semi-structured interviews with focus households from selected villages of the original sample. In the following, a short section describes the sampling and data collection process. Details on the underlying data will be given in the second section. In the third section, the results of the mostly qualitative analysis are presented.

5.1 CASE STUDY DESIGN

In the following, three main components of the case-study setup are presented: a) the study questions and propositions, b) the selection of units and cases of analysis, and c) the analysis strategy.

As mentioned before, the case study was designed to supply reasons for mistargeting under the current scheme and assess whether these factors might impact other targeting schemes as well. During theoretical analysis, several challenges of community targeting and means testing were discussed and can be summarized in three focus areas: First of all, interregional appropriation of funds is expected to impact the implementation of the distinctly decentralized policy. Second, this study will focus on local capture and nepotism due to the strong involvement of

local authorities. A third weak spot of the current *dibao* policy may be the practical implementation of income measurement.

For studying the implementation of *dibao* targeting, three levels were analyzed as an embedded, multiple cases study: a) Households, b) village leaders as the grass-root level administrators of national policy, and c) township government acting as the link between national government and village administrators. The multiple case design requires the situation to be examined in no less than two provinces with heterogeneous implementation of national regulations, which allows us to encounter the desired contrasting conditions and produces substantial analytical benefits. The embedded design means to collect information on different statistical and administrative levels, which is necessary for testing and contrasting the information given by up- and downstream levels of local government and households in the third step of institutional analysis. Case selection is certainly one of the pitfalls of the case study approach, as it may lead to the under- or overstatement of specific causal links (George and Bennett 2005: 22 ff.). In contrast to the quasi-nationally representative, structured household surveys conducted by the Center for Chinese Agricultural Policy (2012, 2014), only a small number of sites could be visited for direct semi-structured interviews by this thesis' author. All in all, semi-structured interviews were conducted with seven township-level administrators, eight village leaders and members of 43 households in the two provinces Sichuan and Shaanxi. The small size of this sample admittedly offers just a glimpse on the large variety of regions in China. Even though the interview partners were selected from a sample that was in itself fairly representative for rural living conditions, the focus sample admittedly can't claim the same in statistical terms. Nevertheless, several steps were taken to reduce the sample selection bias as far as possible: The selection of the case study sites (i.e. townships) offers a certain variation in local conditions and legislation and represents a spectrum of rather typical cases: While the local *dibao* standard in 2012 lay between 1,860 and 2,700 RMB per capita and year in these regions ($\approx 500\text{--}730$ PPP\$), the average per capita income in the eight selected villages lay between 1,200 RMB and 6,500 RMB per year ($\approx 320\text{--}1760$ PPP\$), and *dibao* covered between 1.6%

and 7.4% of the villagers. In addition to the economic situation, differences could be found in distance to urban centers or natural conditions for agriculture.

Yin proposes several techniques for case study analysis, among them pattern matching, time series analysis, cross-case synthesis, and explanation building. The latter is an iterative process of comparing initial statements or propositions against various cases that lead to their stepwise revision or refinement, thus building an explanation for specific observations and develop ideas for further study (Yin 2003b: 120–122). The explanation building technique is especially suitable for settings in which a set of causal links “explaining” a phenomenon are complex and difficult to measure. As for other techniques of case study analysis too, it is essential to attending to all available evidence, addressing rival interpretations, making use of expert knowledge and being aware of the state of art in the subject of interest (Yin 2003b: 137). For this study, a sequence of iterative checks and verifications of hypothesis on the causes for mistargeting under the current allocation scheme was implemented. The first step was extensive literature research followed by expert interviews to identify potential causes for mistargeting. These initial hypotheses were contrasted with the data collected during a larger household survey. This household survey did not only deliver information that helped to re-evaluate the initial hypothesis, but also formed the basis for the semi-structured interviews with key stakeholders on household level and among village leadership and township administrators. During these interviews, the hypotheses were again re-evaluated. As a result, several theses could be rejected, while other findings led to the addition of new causal links. Finally, the initial hypotheses were condensed to three major, interlinked factors, which were robust to any further re-evaluation.

5.2 DATA

For implementing this analysis strategy, the study draws on several data sources. During literature research, not only academic publications, but also media reports were included. To gain access to a wide selection of reports, the author screened the Chinese Press Agency *Xinhuanet*, the news section of the Chinese search engine *Baidu* and *GoogleNews*. Subsequently, expert interviews with several Chinese scientists and administrators were conducted (Qin 2014; Li 2014; Anonymous 60 2014; Feng 2014; Wu 2014; Anonymous 59 2014; Yang 2014). During those interviews, different aspects of the policy funding, policy design and implementation of national and provincial policy at the local level were discussed.

After literature review and expert interviews, several hypotheses on the causes of current mistargeting were built and prepared for re-evaluation with quantitative data. The starting point is a 2011 dataset that originates from the *Rural Public Investment Survey* (PI), which was conducted by the Center for Chinese Agricultural Policy (2012) in the form of structured interviews. The PI data provides the necessary background information on household characteristics and *dibao* transfers of about 2000 households and the respective villages and townships in five Chinese provinces, among which there were 186 *dibao* recipients. However, this first dataset does not include very detailed information on the program's implementation and targeting mechanisms. To test initial hypothesis about mistargeting of funds, 1240 of the original sample households (among them 119 *dibao* recipients) were revisited during a 2014 follow-up survey in 62 villages (Center for Chinese Agricultural Policy 2014). The author developed a short questionnaire on detailed implementation of *dibao* policy and potential defaults on household or village level (see Appendix C for the questionnaire).

From this data frame, the author conducted semi-structured interviews with households, village leaders and township administrators along standardized questionnaires (see Appendix D). For interviews, the author contacted 58 key stakeholders in eight of the previous sample's townships in the provinces of Sichuan and Shaanxi, where poverty

is most prominent and the *dibao* system was of major importance. The interview questions were developed together with academics working in that specific field and validated by discussing them with two former staff of civil affairs bureaus. For testing the propositions concerning efficiency of *dibao* policy, several questions to households and village leaders aimed at measuring the perception of mistargeting in the respective community, perceived reasons for that mistargeting and the implementation of specific components of *dibao* policy that were previously proposed as problematic. The open household interviews were intended to collect additional ideas and impressions about *dibao* policy implementation and distribution of funds that might have been omitted in the previous surveys. Village leaders were questioned on monitoring, availability of *dibao* funds, public discussion of *dibao* targets and the publication of the *dibao* recipients' identity on village level.

In contrast to quantitative, structured interviews, the presence of the researcher itself is of higher importance for semi-structured or open qualitative interviews since the interviewer has to react to unexpected turns of the conversation and deviated from the previous structure (Yin 2003b: 58). Moreover, Yin (2003a) also recommends to include direct observations during the interviews, e.g. socioeconomic conditions of the research region, administrative seats, general village economy, living conditions of the neighborhood and the home of interviewees that were contrasted with information on livelihood given by the households. Therefore, it was important that the author of this thesis was leading the semi-structured interview herself. To bridge language barriers and avoid excessive cultural bias, a local student acted as intermediary during household interviews. Further, the interviewer and the accompanying student each took notes during the interviews, which were compared directly after the interviews. Finally, audio records of the interviews were taken with permission of the interviewees, transcribed by native speakers, and compared with the notes taken during the interviews.

5.3 RESULTS

In the following, three strands of explanations for inefficiencies of *dibao* policy are presented, the allocation and scarcity of *dibao* funds, problems of nepotism and local implementation of income measurement.

5.3.1 Scarcity of funds

Theoretical analysis provided clues for the regional distribution and availability of *dibao* funds causing various problems for *dibao* efficiency. In the following, this study differentiates between scarcity of administrative funds and lack of grants themselves, both of which having quite different causal explanations and consequences for the implementation of *dibao* policy.

5.3.1.1 Scarcity of administrative funds

In interviews with township administrators, the lack of personnel to implement the *dibao* policy as decreed by national and regional governments was repeatedly raised. In fact, the visited townships' governments had assigned the whole township's *dibao* work to only one or two administrators in the township governments. These administrators were responsible for 15 villages and about 1,800 recipients in the average sample township, not to mention an unknown number of rejected applications. One bureau had tried to hire an office assistant for months, but failed to find qualified candidates willing to take such a troublesome job for the low wage that local government could offer (Anonymous 07 2014). A statement by a township officer gives a credible impression of the resulting workload:

“The biggest problem with the dibao work is the lacking manpower. I am working in the MCA office since 2012 and I am

the only one working on dibao. There are three items which I have to take care of: Civil affairs (including handicapped people), traffic/road security, and dissemination of laws [to village leadership]. On top of this, I am also the contact cadre for a specific village in the township. That means I have to attend all the meetings in this village, including the meetings in the village small groups. During the application phase of dibao, I have to visit the villages during the day and come back in the evening to put the application documents in order. // (Anonymous 02 2014)

The main reason for this understaffing can be assumed in the poor financial situation of some rural townships. As argued above, poor regions typically have a weak tax-base and high *dibao* coverage leading to considerable administrative effort. Kennedy (2007a) argues that many townships in poorer regions have become “administrative shells” since the first wave of tax reforms, not being able to deliver even basic public services. Further, literature suggests that from 2000 on a considerable downsizing of local governments might have aggravated the understaffing (Li 2006). While townships are receiving considerable financial transfers from higher government levels to conduct *dibao* work, these funds are by definition earmarked for the usage as *dibao* grants and are not to be used for covering administrative costs that arise with the policy implementation. Therefore, fiscal reforms during the past few years may be one important reason for *dibao* inefficiencies.

This lack of funds and personnel on township and county level was found to result in deficient implementation of important program components. For instance, the implementation of policy monitoring seemed patchy in the larger townships, especially for more remote villages. According to township governments, monitoring *dibao* recipient households is supposed to not only prevent fraud by households but also serve the purpose of supervising village leaders’ implementation of the system. Leaders in 55 (= 89%) of the follow-up sample villages stated that ex-post monitoring was taking place (usually once per year), though in 23 (= 41%)

of these villages this monitoring was solely done on the village level (by village leaders, village committee members, party secretaries, or members of the village representation). This procedure not only contradicts the 2–3 system (which mandates not to conduct selection and monitoring of targets at the village level), but also precludes the possibility of monitoring village leaders' performance. Further, survey responses on the household level left the impression that household visits and monitoring by the local township and county were not as comprehensive as demanded by regulations: in 2013, 39 (=33%) of the 119 *dibao* households were not examined by township or county officials during the application. Ex-post monitoring by county or township administrators was not conducted in 67 (=56%) of the households. In fact, for some of these households monitoring might not have been scheduled because the unalterable character of their poverty rendered repeated inspections superfluous (e.g. recipient has a permanent handicap). Still, it can be assumed that this overall low frequency of monitoring was connected with the high work-load of township personnel. In fact, administrators in one township quite frankly admitted that they could not conduct the annual monitoring for most households due to understaffing:

“At the moment the problem of dibao work is that the targets have to be evaluated once each year [...]. The manpower is seriously insufficient. To reduce the amount of work, we collect data basically only for those 20% dibao households that are new in the system.”

(Anonymous 03 2014)

In other regions, township administration usually did not schedule any visits on village level at all:

“Usually, township level in this district usually does not visit the countryside for on-spot inspection, only in special cases they will go to the villages for inspection.”

(Anonymous 01 2014)

After on-spot data collection, some other implementation problems turned out to be quite unconnected to financial constraints: As mentioned, villages and townships are instructed to publish details on applicants and recipients at publication boards in the vicinity of the town hall. Villagers are encouraged to anonymously report any irregularities, especially suspicions of fraud, via a direct telephone number to the township Bureau of Civil Affairs. In 46 (=74%) of the villages, information on applicants was published in this way, while practically all villages (95%) published a list of recipients.

However, the secluded situation of some town halls, the rather short duration of publication and disinterest of villagers often reduced its effect. In consequence, about half of the villagers the author talked to in semi-structured interviews had not seen any *dibao* information on the publication board. Further, reporting might be reduced by the (sometimes well-founded) fear that complaints would not remain anonymous in a small community. This inhibition was further increased if, as one township leader reported, complaints could not be filed anonymously:

“It is possible to file [a complaint], then we will make an inspection [...]. We will find out [about irregularities]. All I need is to know who complains about whom, I need real names to reach the people. If I can't reach the people, there is no way I can inspect.”
(Anonymous 06 2014)

In consequence, complaints about irregularities in the distribution of funds were, according to sources in the township governments, rather rare.

5.3.1.2 Limitedness of *dibao* grants

Not only the finance of administrative costs, but also the finance of *dibao* grants themselves seemed patchy in some regions. Earlier research (Li and Jiang 2012; Liu 2012) and expert interviews indicated that *dibao*

funds or the number of *dibao* beneficiaries were sometimes limited. However, none of the official regulations mentions this so-called limitation of quotas (*ming'e youxian* 名额有限)²¹. Instead, the main principle of *dibao*, “distribution according to need” (*yingbao jinbao* 应保尽保) was highlighted, the assignment of grants to all eligible applicants without any general restriction of funds.

Several reasons for the existence of such limitations are conceivable. First of all, sheer miscalculations during the budget-planning process is possible, but can't be responsible for systematic underfunding. Second, poverty statistics communicated to higher government levels, according to which *dibao* funds are distributed across regions, might underreport income levels (see section 3.1.4). In that case, the responsible officers would need to match the number of *dibao* applications to the reported poverty rate and accept that the annual funds transferred from higher government level according to these numbers are insufficient. A third reason might be that *dibao* funds are limited for the purpose of serving as an implicit relative poverty line (Yang 2014). This idea is based on the situation that it is usually easier to rank households than to classify them along an absolute poverty line. Attributing a village-specific quota implies that village leaders do not need to measure the absolute income exactly but simply have to compare the welfare level of applicants and distribute quotas to the poorest among them. This argument will be pursued in section 5.3.3.

For shedding light on this issue, the potential limitation of *dibao* grants was a focus topic during the 2014 structured, quantitative interviews. In about 29 villages (=47%) of the follow-up sample, village leaders reported that a fixed quota for *dibao* existed in their village, either in terms of number of recipients or amount of total funds. Also during the semi-structured interviews, four out of seven administrators on township

21 *Ming'e* 名额 is usually translated as “quota”, which is a rather ambivalent term. Literally, it reads as “name list” and should rather be understood as the number of persons admitted to some system or program. In practice though, this term was also used in the sense of an entitlement to some benefit that can be obtained (*dedao ming'e* 得到名额) or be (or not be) available (*ming'e yong wanle* 名额用完了).

level openly admitted in interviews that the number of recipients was limited in their region. In two further cases, township governments claimed that grants were unlimited, but were later contradicted by village leaders who confirmed the existence of quotas.

The consequences of the limitation of funds depend on the configuration of this limitation. In some of those 2012 sample villages that seemed to have limited *dibao* funds, this limitation was linked to the general economic situation of the village (i.e. the quota was distributed in relation to the average income or other development measures). In these cases, the limitation of funds per region need not have a detrimental effect per se because the limitation was at least fairly responsive to poverty levels. In other cases, however, there was no connection between estimated poverty levels and *dibao* quota, as one township officer admitted:

“The quota is decided according to the population or the total area of one village through [county and township] Party Committee meetings.”
(Anonymous 05 2014)

In fact, the quota was distributed as a fixed percentage of village population in 38% of all villages. Evidently, such a quota might lead to undercoverage in particularly poor villages and excess distribution of surplus funds in richer villages. Therefore, it is not surprising that in 20 (=69%) of the 29 sample villages that had limited funds, village leaders reported that this quota was insufficient to provide for all applicants that they perceived as eligible, and that, as a consequence, certain households had to be excluded from the system. On household level, the consequences of these funding shortages were visible, too. In the sample of 1240 households (Center for Chinese Agricultural Policy 2014), non-applicants were asked about the reason for desisting from applying for *dibao* funds, which are illustrated in figure 17 (p. 132): Among 1249 households, 845 households had not filed an application since they felt they did not fulfil the eligibility criteria. From among the remaining 173 households that had not filed an application, 27 had done so because they were informed that funds were used up in their village and no new beneficiaries would be admitted.

The scarcity of funds often generated the need to make ends meet on village level, as statements from village leaders and township administrators underline:

“There are a lot of people who need dibao, but the quota and the capital is limited, there is no way to satisfy everyone. [...] The main difficulty of our work is the fact that the quota is limited; we can't completely achieve the yingbao jinbao principle²².” (Anonymous 10 2014)

“The other [problem] is the quota, which is limited to 9% [of village population]. I will give the example of one village leader's experience. He told me that they have many cases of serious diseases but they didn't have sufficient quota [for covering all of them]. Therefore, this village leader was facing a lot of conflict in the village.” (Anonymous 06 2014)

In the case-study villages, several ways could be observed in which village leaders coped with scarce funds. First of all, village authorities seemed to actively persuade households not to apply or informed households about the limited character of funding: In 26 cases, households had refrained from applying since they knew or assumed all available spots were already taken. Five households reported that quotas were distributed without any formal application and thus did not bother to apply at all. In other cases, village leaders seemed to introduce additional eligibility criteria to further reduce the list of potential applicants: In 24 cases, households did not apply because they wrongly assumed that ownership of land or young age would exclude them from the system, which hints at village leaders tightening eligibility criteria to regulate the number of applicants (on the reasons for non-application see figure 17, p. 132).

22 应保尽保 yingbao jinbao: “everyone in need will receive funds”

Moreover, the author also learned that some village leaders felt need to fit the number of applications that would be forwarded to the township level to the assigned quota:

“The townships total quota is binding. The total sum of funds is also fixed. Therefore, we need to adapt the number of people in the dibao classes according to the quota which is fixed both in number of total funds and number of recipients.”
(Anonymous 11 2015)

Some villagers reported that village leadership indeed conducted a pre-selection based on their perception of eligibility, even though the village level is generally not to be involved into the processing of applications:

“First of all, everyone can apply. Then, the village leader decides who is the poorest. Afterwards, the village leaders will pass the name list of dibao recipients to the township.”
(Anonymous 45 2014)

While immediate rejections of applications on village level are not strictly according to official *dibao* policy and decrease the transparency of the process, targeting may still be accurate as long as the assessment of village leaders is correct and strictly and exclusively based on eligibility criteria. Also villagers, when asked about the reasons for the rejection of their applications, often mentioned alleged ineligibility as sole explanation (see figure 18, p. 134). However, a certain number of households stated that their application got rejected due to scarce quota (14 cases), which shows that the approach described by Anonymous 11 above is not an exception.

Another way to control the number of applicants was through the “democratic discussion”. In some cases, the assembly was assigned to elect those households that are allowed to apply for funds instead of simply giving their evaluation on the applications that have already been filed:

“A council of all households in the small group comes together to discuss who is supposed to receive dibao. In a discussion, they compare the different households to see who is poorer than the other. After that, the village cadre, the villager representation, the small group leader and the small group committee discuss in a common meeting and decide who is getting dibao and about the according dibao class [determining the amount of dibao]. After that, they [the households] have to write an application letter.”
(Anonymous 33 2014).

If the number of recipients could not be controlled, village leaders were creative in finding other solutions: In some villages, the low average amount of transfers raised the suspicion that leadership, attempted to prevent discord among villagers by splitting benefits of the nominal recipients among all households he or she perceived as needy. In case-study interviews, five villagers reported that only the most deprived households received social assistance regularly. The remaining quotas rotated between the remaining households on annual basis, while year after year the same households were reported as official recipients to the higher authorities. In these extreme cases, villagers were not even included in the application process:

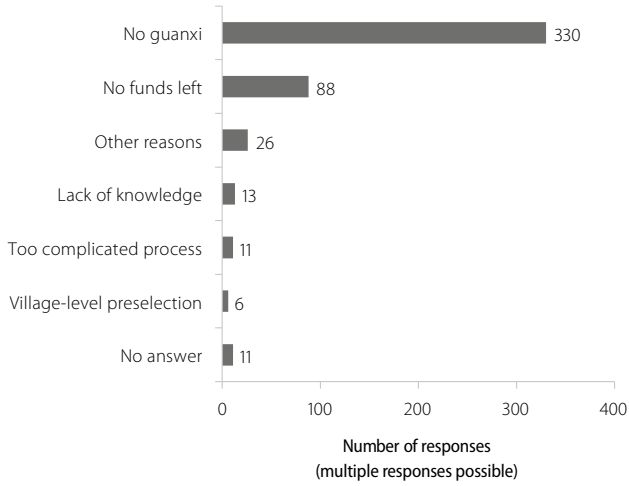
“Actually, we do not need to apply for dibao. The village leader chose me and then applied for dibao [in my name]. But the money for my quota goes to other people in this village, too.”
(Anonymous 44 2014)

While scarce quota might be a problem for other forms of targeting as well, it is a crucial problem for community targeting. For this form of targeting, village leadership is a bottleneck for grant applications due to their ability to regulate the number of applicants by estimating income in a very unstandardized, obscure and often subjective way. In the following, the specific problems of nepotism in the connection with community targeting will be discussed.

5.3.2 Problems of nepotism

Principal-agent theory and *guanxi* research gave the idea that nepotism might be a major problem of community targeting. Indeed, a first round of interview questions on household levels shows that villagers themselves shared this view. In the bigger follow-up survey with 1240 households, more than one third of the respondents reported incidence of mistargeting in their village. Leakage of *dibao* funds was reported by 432 households (35%), while exclusion errors were stated by 465 households (38%). Asked about the major reasons for mistargeting of funds in their village, the most frequent response pointed towards the importance of personal relationships in attaining one of the limited *dibao* “vacancies” (see figure 16). Cases of exclusion were to 68% perceived as originating from a lack of personal relationship (*guanxi*). Other, less frequently mentioned reasons were a lack of resources on village level, lack of knowledge on the side of potential recipients, village-level preselection, and the perception that the application process was too complicated. Cases of leakage were nearly exclusively perceived as stemming from good personal relations (90% of the responses), either with local decision-makers or the local community, which decided on recipients in the so-called democratic evaluation.

Exclusion



Leakage

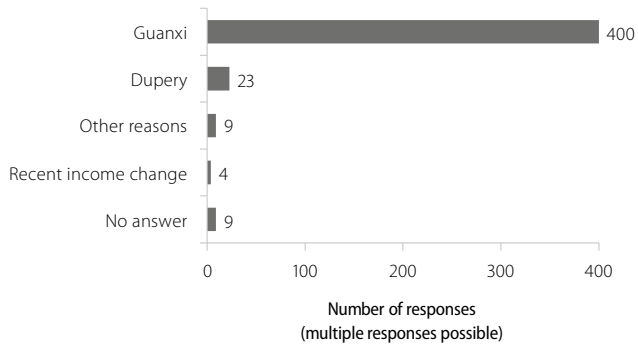


Figure 16: Perceived reasons for mistargeting of funds

Own illustration. Data source: Center for Chinese Agricultural Policy 2014

These statements are a first clue that nepotism might indeed be a main problem of program implementation. However, these numbers only describe very individual impressions, potentially made by persons that were not very acquainted with the system. Therefore, the sample was split into applicants and non-applicants and asked separately for reasons for non-application and non-admission, respectively. Indeed, non-eligibility was the main reason that made villager refrain from application (see figure 17). From among those cases not connected to eligibility, 23% respondents had not applied because they believed the social connections to be insufficient (41 cases). In further 16 cases, funds were distributed in an informal fashion outside the official application system, as indicated in the previous section.

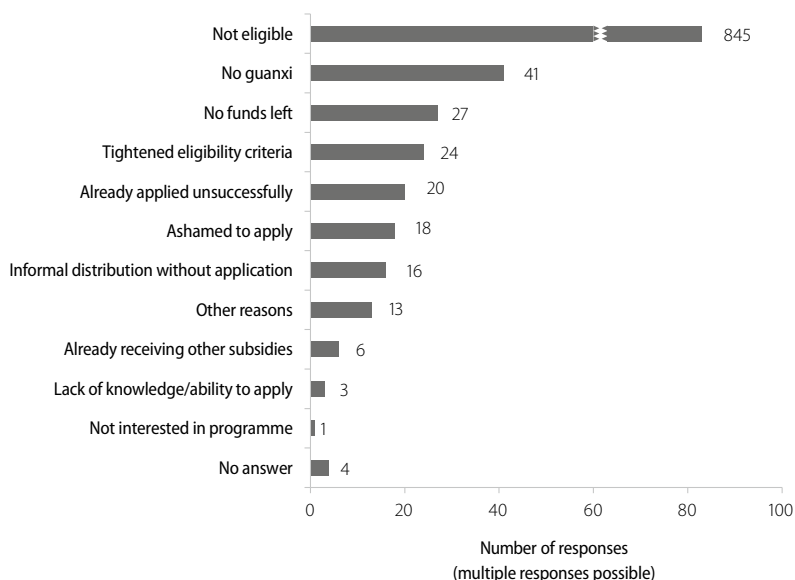


Figure 17: Reasons for non-application of households (follow-up sample, 2014)

Own illustration. Data source: Center for Chinese Agricultural Policy 2014

Again, it has to be highlighted that distribution outside the application system does not necessarily mean misappropriation of funds but seemed to be fairly accurate in some cases. Still, the lack of transparency certainly is a breeding ground for nepotism and irregularities of all sorts. This thesis argues that increasing transparency, for instance by including all villagers into the “democratic meeting” meetings, significantly improves the satisfaction of villagers with *dibao* distribution. On the one hand, better knowledge of the system might help to mitigate the suspicions and perception of injustices on the side of the villagers. During the interviews, especially those villagers without clear knowledge about the distribution were most suspicious of irregularities:

“Do you know who got *dibao* this year” – “No, no one knows.”
– “Do you know the official requirements for being eligible?”
– “Requirements? What requirements? You have to take the back door, you need connections!” – “But didn’t you just say you don’t know who got the *dibao*? How do you know people are pulling their relations?” – “We are the local villagers, so we know the realities. You come from the city, so you do not know how things work here.” (Anonymous 46 2014)

On the other hand, it seemed that regular meetings between villagers were reflecting the development of civic virtues and grass-roots level democracy in that village. Thereby, lacking knowledge might simply reflect low transparency and accountability of local governments, as for instance a statement of a villager suggested:

“The village leadership alone is deciding on who is getting the money. This way, people who have good relationship with the leaders could get the money. [...] The name list [of recipients] is never shown to the public, and the villagers never vote to participate in the meeting of the *dibao* system!” (Anonymous 47 2014)

In fact, many villagers shared the opinion that nepotism did not only matter during the application process but also during the decision over applications and the rejection of applications through village or township authorities. From among the 68 households that were not rejected for eligibility reasons, 18 assumed that their application was unsuccessful due to a lack of social connection that would have allowed them to attain one of the scarce quotas (see figure 18).

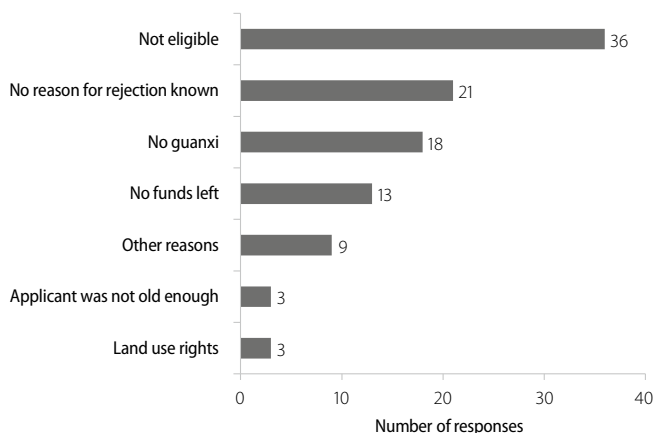


Figure 18: Reported reasons for rejection of applications (follow-up sample, 2014)

Own illustration. Data source: Center for Chinese Agricultural Policy 2014

In that context, the aforementioned “democratic discussion”, reportedly held in 96% of the villages, might actually be a double-edged tool. First of all, the general public only participated if these meetings were held at the level of neighborhoods (*cunmin xiaozu* 村民小组) instead of the administrative village (*xingzheng cun* 行政村). Notably, the information level of villagers was much higher in those villages where meetings were held on small group level. Even though this measure increased transparency of the process, administrators of several townships reported that small-group-level “democratic discussion” resulted in grim fights

between villagers over the distribution of funds. In one village, local leaders stated that votes were often given to well-connected and popular households (instead of needy households) and resulting conflicts had to be mediated by village or even township personnel. Especially in villages with limited funds and inappropriate information about true welfare levels, “democratic evaluation” turned out to be susceptible to *gaunxi*-induced irregularities, as villagers reported:

“The people who participate in the voting meetings can’t really understand the actual poverty situation of each of the households that are put up for election. In addition, before the election no one really contributed detailed information about the real situation of the households put up for election. This results in the situation that each person that is voting will favor those household they are good friends with.”
(Anonymous 16 2014)

All in all, it seems rather challenging to conduct community evaluation in rural China without igniting inner-community conflict, clientelism and corruption. While community evaluation has potential for increasing the transparency of the targeting process and public participation, hitherto experience is often sobering. Villagers’ perception of biasedness of the targeting outcome and village and township administrators’ indications of practical problems were supplemented by the evaluators’ own observations of random cases of present or recent leakage and exclusion among the sample households. Assuming that the observed patterns of social interaction and power relation are to a certain degree generalizable to other backward rural areas in China, the difficulties of implementing community targeting could explain the comparatively low performance of the current targeting scheme in these regions.

5.3.3 Local implementation of means testing

A third problem in the present targeting system is the implementation gap in remote rural areas with rather homogeneous income structure, a view which is for instance shared by a township administrator:

“The most difficult part, if you ask me, is the classification of households because the gap between poor and rich is not large.” (Anonymous 06 2014)

As mentioned before, in poor rural communities large shares of income are from agricultural activity and remittances from migrant workers, both of which are hard to determine by outsiders. This dilemma was also confirmed by township administrators:

“The biggest challenge in the dibao work at the moment is the estimation of the household income, as income of rural households originates from farming and part-time jobs which is hard to clearly define since there is no fixed income. Especially for old people it’s difficult since their own income is very small and the support by their children is not fixed and hard to determine, which makes it difficult to count it into the old people’s household income.” (Anonymous 01 2014)

On village level, the training of administrators seems to be the crucial point for income evaluation. Income measurement in the rural *dibao* program is implemented by elected village leaders or local party members. In fact, in only one of the case-study villages, leaders claimed to have received training in how to specifically measure household income. Others admitted quite frankly:

“Every six months or each year once, we visit the households to monitor their poverty status. [...] We did never receive any kind of dibao training, we just do an on-spot household evaluation.”
(Anonymous 08 2014)

In one case, the village leader had enjoyed a specific training and was more informed about the official regulations concerning income collection:

“The village organizes on-spot examinations, looking for people that have more than 1800 RMB yearly per person income, an AC or other expensive consumption goods. Those [households] that don't come up to the requirements receive an advisory explanation and we allow them to withdraw their application.”
(Anonymous 09 2014)

Trained cadres or township leaders on the other hand were informed about the most important income components:

“About the income measurement: They [village leaders] include into the calculation all the income from farming and husbandry – all kind of agricultural income, plus the income from working on a job. The per capita family income is calculated by dividing through the total number of household members.”
(Anonymous 03 2014)

In some cases, however, the *dibao* standard was not even mentioned by village leaders. Instead, demographic criteria seemed to be the focus of poverty classification:

“The dibao targets are mainly households with old people with weak mind and people with serious diseases.”
(Anonymous 10 2014)

“Who can usually be a recipient of dibao? People who have diseases, especially long-term diseases – for instance one villager is having tracheitis.” (Anonymous 13 2014)

While this definition is not wrong as such, the omission of other components reflecting financial neediness is obvious. The dismissal of means-testing (i.e. income measurement) by the street level bureaucrats seemed to be often due to practicability rather than to sloppy implementation, as following statement by a village leader suggests:

“The dibao standard here is below 2000 yuan. However, it is hard to calculate the income. [...] Normally the village committee knows much about the current situation of this village. They know who has lower income and who got a severe disease. Another way is through [let the households present you] the hospital fees of severe diseases.” (Anonymous 12 2014)

The subjective assessment of poverty status was indeed rather widespread. Several village leaders were rather confident about being able to assess relative poverty just by their knowledge of the village situation:

“We check whether they are poor or not, but basically we know who needs the money.” (Anonymous 14 2014)

“For example, if a household’s yearly income suddenly goes up, the people’s representation knows that, they know how much you earned a year. They subtract the expenses and if you get below that standard, you get [dibao]. If you don’t come [below] the standard, you don’t get it.” (Anonymous 15 2014)

Judging from these statements, many local leaders in underdeveloped areas are following a rather ad-hoc strategy for measuring income and poverty. This is supported by the assessment of Cai (2000), who described income determination in rural areas (for general statistical reasons, not in the context of *dibao*) as “30% statistics, 70% estimation”. In this case, the *dibao* standard (i.e. the absolute poverty line) would be merely of symbolic nature.

In summary, unsuitable *dibao* regulations lead to an implementation gap as some village leaders perceived the absolute admission threshold as impracticable. Instead, some village leaders were found to determine eligibility in a relative fashion by simply comparing the living standard of villagers and using severe disease or old age as an additional qualification criterion. By ranking households against each other and selecting the poorest number according to the available funds, a relative poverty line is actually introduced. This practice is an example for the *Realpolitik* of policy implementation in rural China mentioned by Li (2006), where local administrators oppose the top-down approach of Chinese policy making by adapting central policy to the realities of the grassroots level.

5.4 SUMMARY

In the previous case study, several elements of institutional analysis were implemented for learning about possible causal factors for the low anti-poverty effect of the *dibao* program. Using the qualitative evidence from all available sources, a pattern of causal factors could be developed that shows how different factors may influence the accuracy of *dibao* allocation separately or together. Figure 19 displays the mechanism as it presents itself in this case study: Poor township finance explained understaffing and patchy monitoring of policy implementation in many of the visited townships. Poor township finance seemed to be connected to the limitation of quota, though quota could also occur in regions with apparently better financial situation. Quota limitations however clearly fostered the implementation of hardly transparent allocation mechanisms,

for instance the preselection of potentially eligible applicants by village leaders or the community, rotation or splitting of funds, tightening eligibility criteria or dissuading villagers from application.

At the one hand, mistargeting may therefore be directly caused by limitations on the total number of recipients or available funds. In those cases, where the quota limit is set according to the financial situation of an administrative region, the quasi-relative poverty line could be situated considerably above or below the *dibao* standard, creating leakage and exclusion, respectively. In some remote and underdeveloped regions, bad finance and thus low monitoring efforts met with traditional clientelist power structures and obscure household classification practices. Certainly, each of these three factors may occur independently and does not necessarily have to lead to exclusion or leakage. However, this thesis argues that it was primarily the unfortunate combination of those three factors that strengthened the likeliness of mistargeting of *dibao* funds in the visited sites.

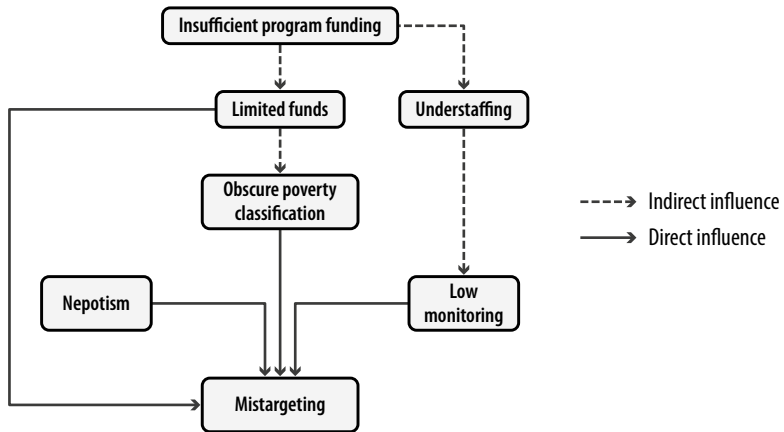


Figure 19: Causal links for mistargeting

Own illustration

These findings are in line with some of the assumptions that had been extracted from the theoretical analysis in Chapter 3. For instance, village and township administrators confirmed the problem of measuring income in rural areas. Further, qualitative data confirmed the proneness of community targeting to defects caused by nepotism and clientelism among members of small communities. However, mistargeting in this specific sample did seem to happen rather at the lower strata of income distribution and rarely around the richest members of a community. At the same time, the qualitative analysis uncovered several problems that are not necessarily connected to the targeting method, for instance the limitation of *dibao* funds, understaffing of township offices and, as a result, low monitoring efforts. Switching the targeting method would therefore not necessarily improve the targeting accuracy as these funding issues would remain.

The causal links as proposed above may explain the low accuracy of social assistance targeting in the quantitative household sample from the CHFS survey. Key results are supported by the observations of a larger audit by the National Audit Office in 2012 among 8101 villages. The auditors found leakage of 1.382 billion RMB in 3.23 million cases due to insufficient evaluation and monitoring efforts or because of manipulation by applicants. In about 9% of the villages, targeting was not done according to regulations, while in 2.6% of the villages, the village leader distributed *dibao* funds at his/her own discretion (National Audit Office 2012). While there is no empirical prove of the causal link between the composite of general problems of community targeting and means testing, administrative flaws and limited funding, all evidence from theory, quantitative and qualitative data suggests so. Thus it can be assumed that the incidence of mistargeting in the CFPS sample was not a singularity but might in fact be symptomatic for remote and underdeveloped regions.

6 DISCUSSION

The qualitative and quantitative evidence presented in the previous chapter will now be used to discuss the initial research questions on the efficiency of the current *dibao* policy and the question whether improvements to the targeting mechanism are recommendable.

6.1 DOES RURAL *DIBAO* MATTER? – ACCURACY AND ANTI-POVERTY IMPACT OF TARGETED TRANSFERS

To achieve the first aim of this thesis, the assessment of accuracy and the anti-poverty impact of current social assistance transfers in China, chapter 4 contained a detailed analysis of the distribution of actual transfers and alternative targeting models. Further, a statistical approximation to the anti-poverty impact of *dibao* grants among a sample of nearly 5000 rural households was calculated. Finally, the reasons for decreased accuracy and anti-poverty impact were traced through a case study in eight Chinese townships. The results suggest that poverty identification at the time of data collection was rather inaccurate, resulting in a considerable amount of leakage of funds to households above the regional poverty line and exclusion of households below the local poverty line. While mistargeting mostly occurred close to the poverty line, funds were also misappropriated to clearly non-eligible households.

At the same time, the anti-poverty effect of social transfers was quite low. Assuming flat rate transfers as they were implemented in the early years of the system, poverty reduction among the sample households and along local poverty lines reached only 2.5%. When transfers were lifted to the actual depth of poverty, the anti-poverty effect increased considerably, reaching a hypothetical poverty reduction of 10.8%. This finding indicates that the poverty impact was clearly hampered by the level of *dibao* transfers, which were often not meaningful enough to significantly decrease poverty levels among beneficiaries. The evident problem is that without a systematic collection of income data, the actual depth of poverty is unknown to administrators. As was discussed above, there are serious practical challenges and costs connected to income collection. *Dibao* classes with different levels of transfers, which were introduced lately, seem to be a more handy solution (Yang 2014).

Practical reasons for this low accuracy of the current targeting scheme, a combination of means-testing and community evaluation,

presented themselves during theoretical and empirical assessment. According to theory, income data can be biased by a faulty definition of its components, the low quality of data collection processes itself, the lack of reliability of household statements and political interference. According to qualitative evidence, the major impediment against income assessment by program evaluators was the composition of rural incomes, which often consisted of undocumented income and thus was hardly quantifiable. Further, the insufficient training of village-level evaluators decreased the quality of data collection. Meanwhile, there was no clear evidence on direct political interference during income data collection. Intentional and unintentional underestimation of poverty levels by local evaluators might lead to artificially low *dibao* quota in poor regions, yet there is no data or empirical evidence for systematical patterns.

Concerning community targeting, theoretical and empirical evidence both point towards local power structures, nepotism and clientelism as the main sources for malfunctions. On the one hand, there was a strong subjective perception of misappropriation of funds due to social relationship between community evaluators and applicants among villagers. During qualitative interviews, this perception was further supported by villagers as well as village leaders and township leaders. Apparently, social connections influenced the reception of *dibao* funds both directly (through active influence by village leaders in favor of well-connected community members) and indirectly (through settings in which socially well-situated households had an advantage in attaining *dibao* quotas, for instance during open votes on recipients).

6.2 DOES THE TARGETING METHOD MATTER?

A second aim of this thesis was to determine whether the targeting method is truly a decisive factor for achieving a poverty-reducing social assistance system. The quantitative analysis included a set of alternative targeting mechanisms to be compared with the original targeting scheme.

As hinted by theory, geographic targeting turned out to be an unsuitable option, at least under the selected mode, considering the increasing heterogeneity of incomes in the Chinese countryside. Instead, the so-called proxy-means-testing has many advocates among researchers and administrators. One major technical problem of PMT was the low variance of estimated incomes, which this study controls for by applying relative instead of absolute poverty lines. The second challenge was that, depending on the choice of explanatory variables, proxy means testing might in fact measure the households' long-term economic status (as a function of past incomes) and not the current income flow. This problem can be reduced, even though not completely solved, by specifically choosing variables that reflect a household's current ability to generate income. Finally, theoretical evidence pointed out serious troubles concerning the external validity of estimation. Despite sampling weights and great efforts in specifying estimation models that might be fairly generalizable beyond the calibration sample, the external validity of the estimation was poor. The reasons were sampling and methodological differences between the different samples, which could not be entirely corrected for ex-post. Thus, the low external validity of proxy means testing limits its employment for poverty estimation. Specific technical requirements to improve external validity are a nationally representative calibration sample and explanatory variables, which are valid predictors across all regions and population strata.

In the setting of this study, demographic targeting along labor force and age of the household head was more accurate in identifying household below the poverty line and reducing poverty than sophisticated proxy means testing methods. This finding to some extent contradicts literature, which argues that estimation models with lower complexity may be more practical but also perform less well in the context of poverty targeting (Sabates-Wheeler et al. 2015). In the baseline scenario (see table 13, p.100), demographic targeting resulted in an increase of the anti-poverty effect of 191% (or additional 21 households lifted out of poverty) as compared to the actual targeting scheme. However, the quite foreseeable downside of demographic targeting, the large amount

of funds leaking to non-poor households, once again reminds of the fatal trade-off between leakage and exclusion in the targeting of a program, which can't be overcome by a simple change of targeting method.

Considering these findings, would an alternative targeting approach bring about significant improvements in terms of poverty reduction? Purely numerical simulations of poverty-impact alone are not sufficient to answer this question. In fact, the actual distribution was troubled by a number of implementation mistakes, which may also impact other targeting mechanisms but could not be incorporated into the simulation. Ultimately, the question whether a change in a targeting method may increase poverty reduction strongly depends on additional factors: As indicated by qualitative data, the distributive outcome of the current scheme was found to be mainly influenced by the four factors nepotisms/clientelism, nontransparent or badly defined targeting processes, low monitoring of the targeting process, and the limitation of funds.

During the analysis of theoretical information and qualitative data, it could be shown that *clientelism and nepotism* are at play especially under decentralized policy implementation. While most targeting mechanisms require a certain degree of cooperation of local actors or institutions, community targeting assigns the selection of targets de facto to the village level. Thus, it can be assumed that the problem of clientelism may be present for all targeting methods in inverse proportion to the level of policy centralization and strongest for community targeting. Also *transparency* and clarity of targeting processes might increase with centralization of tasks, as there are no local amendments to processes and rules. Regardless of the targeting method, the communication of relevant information to local stakeholders is of course a necessity. However, also methodological details are important for transparency of targeting processes: While demographic targeting offers transparency through the use of fixed and unalterable demographic criteria, PMT-testing was criticized in this respect by literature due to its mathematical complexity and the necessity of keeping the detailed classification criteria confidential. The transparency of community targeting again is highly depending on the detailed implementation: If the community was represented by

local office holders without involvement of the community, transparency could be very low indeed. Theoretical and qualitative evidence suggested that the necessity of *monitoring* targeting efforts increases with the amount of involved actors: Thus mistargeting due to lacking implementation of monitoring measures may again be most distinct for community targeting and means testing on village level, nevertheless it can be assumed to be present with other forms of targeting as well.

Following these considerations, it seems that a shift away from the current means testing and community targeting towards demographic targeting may increase the efficiency of social assistance due to reduced leeway for clientelism, higher transparency and facilitated monitoring. The effect of a change in targeting method however vitally depends on other problems, which are unrelated to the choice of targeting method.

The *limitation of dibao funds* for instance is not necessarily connected to the choice of targeting method. While this limitation of funds serves as a control mechanism and an absolute poverty line (Yang 2014), there were indications by both applicants and administrators on the insufficiency of program coverage. During resulting fights over the scarce *dibao* quota, weak community members were naturally at disadvantage. As the analysis of ROC-curves showed, program size in the study sample could theoretically be increased up to a third of the rural population until an increase in program coverage leads to more additional leakage than the respective decrease in exclusion. Even by increasing program coverage just up to the sample's overall poverty level of 9%, a notable decrease of exclusion and increase of anti-poverty effect could be achieved, though only for the more efficient targeting methods. On average, the increased program scope improved the anti-poverty effect by about 67% (13 additional household above the poverty line) as compared to the baseline scenario with flat-rate grants. Another factor of fund shortage is the insufficient level of *dibao* grants: In the sample used for this analysis, the poverty gap among beneficiary households often seemed larger than the average transfers. In another study, recipients themselves evaluated the impact of grants to their livelihood as mediocre at best (Yi and Zhang 2011). Thus, it can be assumed that both targeting accuracy and program

impact are influenced by fund shortages, which are very likely not connected to the explicit targeting methods. Indeed, this study's quantitative simulations showed that a change in transfer height had a profound impact on the program's poverty reduction, increasing the anti-poverty effect of the actual targeting scheme by 336% (or 37 additional households lifted out of poverty).

Finally, one should mention the *administrative costs* connected to different targeting methods, which could not be taken into account in this study due to data requirements. Therefore, we can only infer from previous studies: As measured by Devereux et al. (2015) for a transfer program in Zambia, the implementation costs were lowest for geographic and community-based targeting (except random allocation, which obviously is only a theoretical benchmark). Means-testing was the most costly option, being nearly three times as expensive as geographic targeting. The costs per beneficiary for PMT and categorical targeting meanwhile was about twice as high than that of geographic targeting, however still being still considerably cheaper than means testing. A shift away from the current means-testing would therefore very likely mean a reduction of implementation cost and thus – assuming a fixed total budget – a relative increase in *dibao* funds for distribution among beneficiaries.

6.3 DOES THE TARGETING EFFICIENCY MATTER?

The anti-poverty impact of social assistance is determined by various factors, some of which – like the height of grants or the size of the program – were unrelated to the choice of targeting methods. Other aspects may vary between targeting methods, for instance unknown implementation cost of the specific programs or the incentives set to involved actors. Also identification accuracy can be influenced by the choice of targeting method. In the presented simulation however, the additional poverty reduction achieved by the choice of the targeting method was lower

than expected. On the one hand it is certainly legit to state that targeting methods do matter for the effect of social assistance. On the other hand, with the current set of targeting methods it was not possible to achieve the desired increase in effectiveness of transfers only by increasing the identification accuracy. Therefore, the critical question may rather be: Does the targeting efficiency matter for poverty reduction?

As pointed out early in this study, redistributive policy has always been accompanied by discussions over its inclusiveness. Core arguments for targeting are the limitation of public funds, the assumed increase of poverty reduction by efficient distribution of funds, incentive effects, considerations of political support and more normative attitudes over accountability and individual responsibility of the poor. On the other hand, several researchers have been arguing that policy makers are often putting too high priority on avoiding leakage (type 2 error) and therein lose perspective of the aim to reach full coverage of the eligible poor (see for instance Cornia and Stewart 1993). Other recent studies argue that targeting accuracy (i.e. the reduction of targeting errors of both types) may be a misleading criterion for assessing the anti-poverty effect of a program (Ravallion 2009b).

The evidence presented in this study could not dispose of arguments that caution against narrowly targeted programs (Van de Walle 1998b). Judging from the findings of qualitative analysis, administrative effort of identifying eligible recipients and moral misconduct seem to be inversely related to restrictiveness of the program. In villages where the number of recipients was restricted at a level below the actual poverty incidence, social assistance became a scarce resource, the fight over which disadvantaged individuals and regions were least likely to win. Also quantitative results rather speak in favor of moderate targeting. For all analyzed targeting models, narrow targeting resulted in extreme exclusion rates, thus failing the aim of meaningful poverty reduction. Instead, the optimal program size from the perspective of the marginal leakage rate was at a poverty threshold of about 30%. Unless program coverage exceeded the critical level of 30% of the total population, poverty reduction increased with growing program coverage. Among the analyzed models,

the largest anti-poverty reduction could be reached by demographic targeting, which allows for considerable leakage in terms of absolute expenditure. Thus, the analyzed case could not support the intuitive assumption that minimizing leakage warrants higher poverty reduction. In this light, an overly strong focus on increasing the accuracy of the current targeting method may not sufficiently credit the actual nexus of the problem.

6.4 POLICY SUGGESTIONS

Under the presented evidence, three policy approaches to the targeting problem in rural China can be offered, which vary in their degree of entailing adjustments to the current policy. The two most conventional approaches would be to either improve the current targeting scheme in details (for instance monitoring or program size), or to change the current targeting mechanism altogether. A third, more radical approach, offers to rethink the policy concept of social assistance in underdeveloped rural regions as such.

6.4.1 Strategy 1: Improvement of the current scheme

The first and most straightforward course of action is to enforce policy implementation through village leadership. As shown by Heberer and Trappel (2013), cadre evaluation can serve as both an incentive (since evaluation results are decisive for further career options) and a control mechanism (by defining leaders' scope of action, communicating the central government's expectations, and disciplining cadres). Introducing direct evaluation of village cadres with a strong emphasis on policy implementation goals could shift local administrators' incentives (see e.g. Wang and Zhang 2004). Some township governments appointed

non-local cadres to be in charge for one specific village – so-called “stationed cadres” (*Zhucun ganbu* 驻村干部) – to conduct regular external *dibao* monitoring. While these cadres have more access to information than normal township administrators, they do not experience the same level of social obligation and pressure felt by those leaders who permanently reside in the village. The downside of monitoring, cadre evaluation, or external supervision is these measures’ considerable cost, for which especially poor townships and counties do not seem to be able to raise the necessary funds. Many township governments are already understaffed and struggle with the current range of monitoring tasks. One alternative to centralized monitoring is to more deeply involve villagers in assessing the eligibility of *dibao* applicants, as was also proposed by O’Brien and Li (1999). In fact, Chinese central government is increasingly promoting the idea of getting villagers more deeply involved in policy assessment, a topic already addressed in other policy contexts (Heberer and Trappel 2013). Concerns over resulting community conflicts, confidentiality and stigmatization of recipients however remain considerable. In any case, information on program mechanisms, eligibility criteria, and recipients has to be communicated to villagers, whose knowledge of the issue was often found to be rather poor. The “democratic discussions” helped to spread information and prevent any major leakage of funds, but only when a substantial number of villagers participated. Traditional publication boards could serve the same purpose, but only if they become more accessible to remote households or if they are modernized – for instance using push notifications that are received via SMS by all registered users. Such a system does not require modern smartphones and could increase the dissemination of necessary information on application periods, criteria, applicants, and meetings; it might also prevent misconceptions and resulting conflicts between community members.

However, changes in the incentive structure will prove ineffective as long as village leaders do not have the financial means at hand to admit all eligible applicants. Therefore, a second measure that might help to increase accuracy of transfers is the abolishment of any limitation in terms of *dibao* recipients or funds. Such limitation contradicts the principle of

yingbao jinbao, according to which all eligible households should receive transfers. However, we should take into account a practical reason for which the local governments might decide to limit funds: an upper limit of recipients may be intended not only to guard against leakage of funds (Zhang et al. 2012) but also to serve as a relative poverty line. This relative poverty line can ease the targeting process for local leadership in regions where administrative capacity is low if we assume that ranking households according to their relative wealth is an easier task than assessing each applicant's poverty status according to his or her respective income level. In any case, it is necessary to at least ensure that this quota is closely and only (!) connected to the respective region's poverty level. The quota should never be influenced by the insufficiency of local funds, as this would clearly contradict the principle of *yingbao jinbao*. In the same fashion, administrative funds should be adapted to local poverty levels and, where necessary, be co-funded by higher governmental levels. Closely connected is the level of transfers, which needs to be adapted to the depth of poverty of applicant households. The innovation of *dibao* classes are one step into the right direction, even though their assignment was found to be quite arbitrary in some regions. The expansion of program coverage for increasing the anti-poverty impact is another approach, which was already raised by previous works, but clearly depends on the elasticity of local budgets and sufficient accuracy in distribution the additional funds.

6.4.2 Strategy 2: Changing the targeting mechanism

The case of means-tested rural social assistance is a good example for a setting in which ambitious policy design can also impede accurate implementation, a line of argumentation which was proposed by Li (2006). Examples from high-income countries show that social assistance based on means testing remains difficult to operationalize, even if there is comprehensive income documentation. Taxation and social transfers conditional

on income level pose persistent incentives to conceal income and hide in the informal sector. Given the lack of centralized collection of income data (e.g. for tax reasons), the witnessed difficulties of implementing a means-based social assistance in rural Chinese areas are not surprising. Customized income assessment, an alternative to community targeting, is costly, error-prone, labor intensive for administrators at the local level and requires proper training of evaluators. Further, self-reported income data is rendered rather useless by the clear incentive for households to understate their income. Thus, it seems that the use of income-based eligibility assessment for rural Chinese households without documented income should be reconsidered in general. However, what are alternative targeting mechanisms if one would like to avoid giving too much power into the hands of community members and their leaders?

Parametric PMT models were more accurate than a nonparametric model like PCA in terms of their in-sample accuracy and resulting anti-poverty effect. Their sensitivity to model transfers however reduced their accuracy and anti-poverty effects considerably and thus were no improvement as compared to the actual targeting scheme. Therefore, parametric PMT models can only be applied if there are no major sampling differences between the calibration and the estimation sample and the data is collected in a very similar way, ideally by the same survey organizer. Most important in this concern is that the calibration sample is nationally representative in terms of income distribution, poverty incidence and other population characteristics. One non-parametric PMT method was PCA, which outperformed the other PMT models in terms of external validity and could reach a certain anti-poverty impact under flexible funds. The seemingly low accuracy of poverty classification might be due to the fact the PCA is the only PMT strategy that does not rely on observed income at all and rather estimates the welfare level than the household income. Because of the decreased performance of PCA under absolute poverty lines with narrow targeting, as shown in the robustness tests, this method requires the availability of a relative poverty line.

However, all featured PMT-based targeting mechanisms shared a relatively high incidence of false classification regardless their specification.

While a certain anti-poverty effect can still be achieved, the low accuracy of targeting quite likely would be problematic under real-life condition. As the author could also observe during the case study, an obscure targeting process can lead to discord and envy between beneficiaries and non-beneficiaries in the Chinese case as well. In the Mexican case, the seemingly arbitrary selection of beneficiaries for the transfer program PROGRESA raised evaluators' apprehension that social assistance might in fact endanger social cohesion (Adato et al. 2000). Judging by this thesis' evaluation, a pure proxy-means tested program in China might ultimately share the fate of the Mexican PROGRESA of rather arbitrary poverty classification and thus fading public support.

As an alternative, many local leaders did rather emphasize demographic eligibility criteria. The connection between demographic characteristics and poverty seems to be strong in remote rural areas of China and might justify demographic targeting. On the one hand, this form of categorical targeting is simple and cheaper, since it avoids measuring income and focuses on visible, easily quantifiable demographic characteristics. In contrast to the PMT models, demographic targeting does not rely on weighting parameters that might be biased by model transfers. On the other hand, demographic targeting requires clear, objective, and universal definitions of admission criteria, a point in which the current scheme leaves considerable room for interpretation. One example is the important exclusion criterion of family members' ability to provide support: At what income level or degree of relationship should a person be expected to support his or her relatives? What degree of handicap or disease results in total loss of labor capacity and qualifies a household for income transfers? The current regulations clearly allow considerable leeway for local implementation. While this flexibility acknowledges local heterogeneity, it also decreases transparency and might cause additional distortions.

As found by benefit incidence analysis, simulated demographic targeting allocated considerable shares of income to non-poor households, which left fewer funds to households in the lowest two deciles. Despite its high spillovers, demographic targeting still had the largest anti-poverty

impact across all models under fixed flat-rate transfers and was among the most powerful mechanism under the “flexible transfers” assumption. Therefore, a well-defined system of demographic targeting can be recommended for poverty targeting, if policy makers are prepared to tolerate some spill-over to households above the poverty line. In light of the considerable administrative effort and monitoring cost spent of avoid leakage of funds, this strategy might actually save program funds. Indeed, the seemingly inconsistent argument for more lenient handling of leakage is supported by some researchers, who argue that certain spill-over might actually increase political support of the middle-class for redistributive programs (Korpi and Palme 1998; Moene and Wallerstein 2001; Gelbach and Pritchett 2002).

6.4.3 Strategy 3: Rethinking social assistance in rural China

The results above showed that an improvement of the targeting accuracy may help to achieve a more meaningful anti-poverty effect. Unfortunately, even the unrealistic case of perfect information about the income level did not mean that all household could be lifted out of poverty. First of all, quantitative and qualitative analysis showed that policy-makers face considerable administrative and financial challenges to increase the effect and accuracy of the *dibao* policy. Further, literature repeatedly raised various arguments against narrowly targeted programs and excessive targeting efforts in general, among them the considerable administrative cost or the stigmatization of recipients. A further argument may be the serious individual consequences of wrongful exclusion of poor households, which might occur under very narrow targeting.²³ Therefore, this thesis proposes to subject the rural *dibao* policy as such to a critical discussion. Put in other words, the crucial question might not be if social assistance

23 A recent example is the suicide of an impoverished farmer family in the province of Gansu. The case reached high public attention since the family was allegedly expelled from the social assistance system earlier due to the ownership of three plowing cattle (see for instance Beijing Morning Post 2016).

is *done right*, but whether narrowly targeted, income-based social assistance transfers are effective per se, if they are in fact *the right thing to do*.

The starting point for this consideration is that in the majority of cases *dibao* recipients are in poverty for a reason: Usually, participants or their relatives were seriously ill, permanently handicapped or too frail to work in the cities or to till the soil. In some other cases, *dibao* households were in poverty because they faced exceptional expenses, usually for the higher education of their children, serious diseases of relatives or a high dependency ratio due. In few cases, agricultural households were vulnerable due to the small size of the family plot or total loss of land usage rights. Other than in industrialized countries, poverty of able-bodied due to unemployment is yet a very rare phenomenon in rural areas due to ample job opportunities in the urban centers. In fact, unemployment-induced poverty did not occur to the author in a single case during the research for this study. The rather high accuracy of demographic targeting, which bases on exactly these classifications, supports this line of argumentation.

Social policy worldwide usually differentiates between unemployed that are generally fit for work, and unemployed that lost their labor force, for instance because of disabilities or handicaps. While able-bodied are covered by unemployment insurance and assisted in finding new employment, those unable to work are provided for with other elements of social security and social insurance according to their status.²⁴ It is therefore argued that the newly introduced pension insurance, health insurance, orphan's allowance, and disability pension would in fact cover an overwhelming share of the rural poor. Increasing the benefits to specific groups identified not by their income level but their capabilities to generate income (i.e. elderly farmers, handicapped and seriously ill) would improve their financial situation and avoid letting them fall into poverty in the first place. Other triggers for poverty, such as ruinous education cost could be covered by extending existent scholarship programs, while

24 The challenges for implementing such a narrowing of social assistance is documented for urban area, where the higher government's demand of identifying and sorting out of able-bodied beneficiaries was met only hesitantly by local administrators (Solinger and Jiang 2016).

vulnerability to natural disasters might be decreased by improved agricultural insurance (see for instance Barnett et al. 2008). Unemployment insurance, on the other hand, may be relevant in urban centers or for landless households.

Following this line of thought, this study suggests to carefully consider the possibility of redeploying the considerable expenses and administrative expenses for rural *dibao* (except maybe for cases of transitory, exceptional deprivation not covered by the above mentioned programs) to boost more universal schemes of social security, foremost pension insurance and health insurance. Admittedly, one might argue that the rearrangement of social security could turn out more costly than the previous system, especially during a transitional phase. However, China is certainly at a stage where administrative and financial capacity (assuming adequate horizontal and vertical balancing of budgets) is sufficient to sustain the transition. Ultimately, the “precise anti-poor policy” movement could be taken as an opportunity to improve the classification of the roots of poverty and fighting them accordingly, instead of further intensifying costly program targeting. Integrating the system of rural social assistance, basically an unsteady trickle of mere alms to destitute households, into a more comprehensive and sustainable approach of social security could significantly increase the attractiveness of the rural areas and be a step towards the aim of developing a true “new socialist countryside”.

6.5 RESEARCH LIMITATIONS

During the course of this thesis, several research limitations were identified that may influence the evaluation of the results. Even though the author took several measures to ensure that the dataset reflected the actual situation as close as possible, some problems concerning the national representativeness of the used dataset could not be fully dispelled. Further, as Coady et al. (2004) or Milanović (2000) point out, there is no targeting mechanisms promising highest effectivity under all settings, since the accuracy of targeting largely depends on the socioeconomic

conditions under which it is implemented. Therefore, greatest care was given to the choice of explanatory variables and their specification in the estimation model. Still, the assessment of various classification mechanisms does not raise any claim on general validity. In order to generalize the results beyond this case study, they should be contrasted with findings from different specifications in datasets from other regions.

Another possible factor influencing the choice of targeting method is the respective administrative costs, which the presented targeting methods may entail. Unfortunately, there was no reliable information on region- and program-specific implementation costs for conducting for instance a cost-efficiency analysis. For China, this kind of information can only be gathered in close cooperation with the Chinese Ministry of Civil Affairs, which could not be established at the time of data collection.

Methodologically, the choice between two scenarios of grant levels – village level flat-rate and full flexibility to the poverty gap – may seem limited. As pointed out above, those two settings were the closest approximation possible, facing the unknown determinants of *dibao* classes in practice. However, both extreme scenarios delivered similar results concerning the relative assessment of targeting mechanisms and income estimation models. Also, due to the lack of panel data, it is not possible to trace the indirect, but only the immediate anti-poverty effect of transfers. Further research in this direction needs to overcome the existing data restrictions of relatively large intervals between survey waves and high attrition, but also the methodological problem that admission to the social assistance system is not an exogenous factor.

6.6 CONCLUSION

The discussion on social assistance and income transfers is often dominated by the desire of policy-makers to achieve perfect allocation of transfers. Social assistance policy usually contains sophisticated mechanisms of excluding non-eligible households from benefits. The considerable administrative and financial requirements of this approach however are often underestimated. Currently, Chinese government invests considerable resources into achieving “precise poverty policy” in rural China. Therefore, this thesis analyzed the accuracy and anti-poverty effect of social transfers and critically reflected on the role of targeting mechanism for poverty reduction. To assess the anti-poverty effect of the current and alternative targeting schemes and gather information on the reasons for observed inefficiencies, the author employed a mixed methods approach combining quantitative and qualitative evidence from the Chinese countryside.

In a sample of 4924 rural households, the targeting of social assistance transfers was found to be rather inaccurate, both concerning leakage of funds and exclusion of eligible households. The leakage of funds usually occurred around the poverty line and thus might in fact help to prevent poverty. By failing to cover eligible households adequately, however, the scheme’s effect on poverty levels was meager. Further, the scarcity of *dibao* funds resulted in exclusion of households in especially deprived regions. Quantitative evidence showed that the low anti-poverty effect of transfers could only partly be improved by better knowledge on the depth of poverty among recipient households, information which none of the proxy-means testing schemes was very successful in providing anyway. The second reason for the low anti-poverty effect was that transferred benefits often were not substantial enough to lift all of the beneficiaries across the poverty line. Qualitative evidence further indicated considerable nepotism and clientelism during community evaluation, obscure targeting processes and low monitoring of the process due to lack of funds and personnel on the side of township and county administrations. While some of these deficits could be solved by changing the

targeting mechanism, others, like low administrative capacities, pertain to all targeting schemes.

Policy suggestions range from the improvement of community targeting by lowering the level of evaluation to village small groups and introducing modern means of information like push-notifications to more profound changes: As quantitative analysis revealed, most of the analyzed methods suffer from problems of external validity and fail in identifying poor household reliably. The most recommendable alternative mechanism was to select targets according to clearly defined demographic characteristics. Considering the strong demographic character of poverty however, this thesis also suggests to rethink poverty alleviation and poverty reduction through *dibao* transfers in general. Since reasons for poverty are in general connected to health and age, the considerable effort and cost of social assistance targeting might also be redirected to other forms of social security, which work rather like an insurance against falling into poverty in the first place. In any case however, an increase of grant levels to the actual depth of poverty is necessary for reaching true poverty reduction.

This study filled a research gap in providing an assessment of the efficiency and effect of social assistance in rural China, taking into account the socio-cultural and administrative peculiarities and connecting theoretical evidence with original quantitative and qualitative empirical evidence from the research region. In addition to original empirical evidence, the thesis made sure to draw on regional resources by involving local experts into hypotheses building and questionnaire design. Methodologically, it provided new insight by testing also the effect of out-of-sample prediction, a problem which has been largely neglected by existing literature on the topic. Another methodological focus was on the inclusion of several methods of income estimation, among them a rather novel application of semi-parametric spline regression for income estimation.

The results of this thesis lay out options for further research. First, additional insights into the cost-efficiency of several targeting strategies in the Chinese countryside might be realized by seeking respective

information from the Chinese Ministry of Civil Affairs. Second, this study only measured the pure monetary impact of social transfers on poor households. Indirect poverty reduction via health and nutrition, education and child labor, utilization of productive capacity, consumption, and facilitation of social inclusion and cohesion would inevitably be the next step. The compilation of time-series would give interesting insights into the long-run benefits of targeted social assistance. In the future, technical progress and the advent of “big data” might provide new options to the described targeting challenge. For instance satellite images may help to identify marginalized regions and households, as proposed by Jean et al. (2016). While these options may solve old problems of data reliability and cost, they may bring up new challenges, for instance on data protection and the invasiveness of these identification strategies. The assessment and weighting of these approaches may become the task of future research.

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APPENDIX A: PIECEWISE REGRESSION

According to Seber and Wild (1989), it is highly advisable to smoothen the transition between the different income segments in piecewise regression to recreate a continuous distribution. The necessity to introduce some smoothing factor is illustrated in figure 20, which shows the exemplary case of a univariate regression of household income on of housing value with a linear fit and a piecewise linear fit at two breakpoints (10 and 15 log housing value). At the first breakpoint, the slope of the estimation changes, while a distinct level change occurs at the second breakpoint. Especially the level change at the second breakpoint obviously requires some smoothing mechanism.

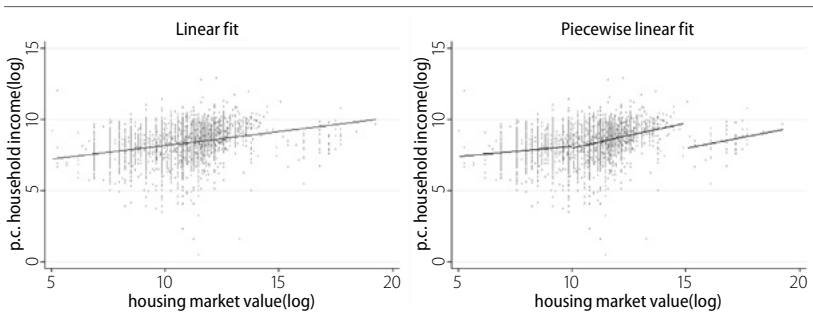


Figure 20: Univariate linear regression vs. piecewise linear regression

Own illustration. Data source: Institute of Social Science Survey of Peking University 2010

A solution to this problem is piecewise polynomial regression of the 3rd order (cubic splines), also called spline regression. Not only will spline regression generate nonlinear piecewise estimates that agree at the breakpoints, but also will the curve be smooth, i.e. be continuous in the first and second derivatives. While this piecewise polynomial regression

provides a more local fit than simple polynomial regression, it does not have the explicit local fit of for instance bin-smoothers, running-mean smoothers or kernel-smoothers (Hastie and Tibshirani 1990: 21–24).

As described in Montgomery et al. (2001) or Hastie and Tibshirani (1990), a spline regression along h knots (i.e. segment borders) $t_1 < t_2 < \dots < t_h$ can be written as

$$E(y) = \beta_0 + \sum_{j=1}^3 \beta_j x^j + \sum_{i=1}^h \beta_i (x - t_i)_+^3, \quad (\text{A.1})$$

where

$$(x - t_i)_+ = \begin{cases} (x - t_i) & \text{if } x - t_i > 0 \\ 0 & \text{if } x - t_i \leq 0 \end{cases}. \quad (\text{A.2})$$

However, the calculation of cubic splines can get out of hand with increasing number of knots, as the number of continuity restrictions in formula A.1 increases. A more stable model for settings in which there is a larger number of breakpoints or knots are the so-called basis splines (B-splines) (Hastie and Tibshirani 1990: 22–26). According to Durrleman and Simon (1989: 554), B-splines do not perform well for a small number of knots, thus five or more knots are desirable. The detailed algebraic properties of B-splines can be found in Boor (2001). A more accessible introduction in Montgomery et al. (2001: 230) reads:

$$E(y) = \sum_{i=1}^{h+4} \gamma_i B_i(x), \quad (\text{A.3})$$

where $\gamma_i, i = 1, 2, \dots, h + 4$ are the parameters to be estimated.

$$B_i(x) = \sum_{j=i-4}^j \left[\frac{(x - t_j)_+^3}{\prod_{\substack{m=i-4 \\ m \neq j}}^i (t_j - t_m)} \right], i = 1, 2, \dots, h + 4 \quad (\text{A.4})$$

Usually, the internal knots are supplemented with external knots $t_0 = x_{min}$ and $t_{h+1} = x_{max}$ indicating the endpoints of the distribution. As shown in figure 21, spline regression smooths level and slope changes at the thresholds and generates continuous estimates of income for the univariate example introduced above.

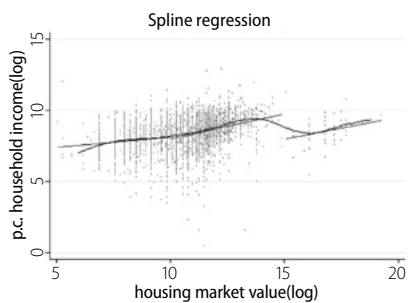


Figure 21: Univariate spline regression

Own illustration. Data source: Institute of Social Science Survey of Peking University 2010

APPENDIX B: OLS REGRESSION RESULTS (CFPS DATA)

Variables	Labels	y1	lny1
x11	Number of household members	-140.158 (390.913)	-0.117*** (0.021)
x12	Ratio of elderly in total household members ¹	-910.777 (1,261.609)	-0.242** (0.103)
x13	Share of children (0–5 years) to total household members	-6,607.793 (5,121.534)	0.038 (0.233)
x14	Share of children (6–15 years) to total household members	-5,557.023 (5,762.064)	0.498** (0.205)
x15_1	Family type: Single or couple (0/1)	990.109* (559.822)	0.061 (0.051)
x15_2	Family type: Couple and one child (0/1)	1,429.733 (2,154.083)	0.305 (0.249)
x15_3	Family type: Couple and two children (0/1)	862.667 (1,654.959)	-0.017 (0.053)
x15_4	Family type: Couple and more than two children (0/1)	-527.762 (562.681)	-0.073 (0.091)
x15_5	Family type: Single parent and children (0/1)	-485.726 (589.145)	-0.122* (0.064)
x15_6	Family type: Three generations under one roof (0/1) ²	475.708 (1,217.540)	-0.044 (0.057)
x22	Ratio of full labor equivalent in household ³	564.838 (1,527.701)	0.559*** (0.160)
x23	Total male labor force	-1,051.239 (938.252)	0.024 (0.035)
x24	Sum of labor experience (in years) of members >16 years	-22.717 (18.479)	0.000 (0.002)
x24b	Sum of squared labor experience (in years)	0.216 (0.361)	0.000 (0.000)

Variables	Labels	y1	lny1
x25	Number of migrant workers	57.342 (272.835)	0.076*** (0.027)
x26	Number of non-agricultural workers	1,342.834** (556.149)	0.151*** (0.021)
x31	Household member with handicap (0/1)	-642.023 (692.563)	-0.029 (0.064)
x42	Log of housing value (RMB)	548.943*** (211.446)	0.096*** (0.013)
x51	Ownership of motorbike(s) (number)	1,624.411* (850.112)	0.165*** (0.027)
x52	Ownership of car(s) (number)	1,107.162 (736.190)	0.101** (0.042)
x53	Ownership of color TV(s) (number)	2,423.360*** (588.837)	0.182*** (0.026)
x62	Maximum years of education among household members ⁴	146.080** (68.484)	0.031*** (0.005)
x91	Province average per capita GDP (log)	-53.806 (693.269)	0.065 (0.051)
x93	County urbanization rate	2,248.111 (1,454.859)	0.525*** (0.163)
x94	County's rate of labor age population	15,261.197** (6,154.271)	2.134*** (0.420)
x95	County's mean years of education of labor age population	-1,016.294*** (314.088)	-0.059** (0.027)
x97	County employment rate of labor age population	-4,235.443** (2,027.018)	-0.124 (0.277)
Constant	Constant	1,340.370 (7,865.962)	5.199*** (0.621)
Observations		4,952	4,952
R ²		0.074	0.252

Robust standard errors in parentheses; *** p<0.01, ** p<0.05, * p<0.1;

¹ According to retirement age: Individuals > 60 years;

² Adults living with their children and at least one of their parents;

³ Labor equivalent assigned according to Yi and Zhang (2011);

⁴ Only including those with labor force;

⁵ Demographic variables were excluded for the PCA model

Data Source: Institute of Social Science Survey of Peking University 2010

APPENDIX C:
QUESTIONNAIRES
STRUCTURED
INTERVIEWS (2014)

C-1: Household questionnaire

问题 (Question)	单位 (Coding/unit)
1. 2013 年你是低保户吗? Were you a dibao household in 2013?	1=是; 2=否) 跳到 6 题 1=yes; 2=no → jump to question 6
2. 2013 年一共拿到几个月的低保补助? For how many months did you receive dibao in 2013?	1=全年; 2=临时 (请说明几个月!) 1=the whole year; 2=temporary (specify amount of months)
3. 2013 年一共拿到多少低保补助?	元 (RMB)
4. What was the total sum of dibao grants your household received in 2013?	0=从未; 1=一次; 2=两次; 3=多于两次
2013 年申请低保过程中, 是否需要县或者乡镇部门复查? Were you inspected during the application process by township of government administration in 2013?	0=never; 1=once; 2=twice; 3=multiple times
5. 2013 年变低保户以后, 是否需要县或者乡镇部门复核? Did you have an inspection after the application process by township of government administration?	0=从未; 1=一次; 2=两次; 3=多于两次) 跳到 7
6. 2013 年本人或者家里其他人申请低保了吗?	0=never; 1=once; 2=twice; 3=multiple times → jump to question 7
6a. 如果申请, 低保为什么没有得到批准? (主要原因) If you applied, why was your application unsuccessful?	1=是; 2=否) 跳到 6b 1=yes; 2=no → jump to question 6b 1=收入不符合低保标准; 2=有劳动力; 3=低保金额用完了; 4=申请程序出了问题; 5=他们没说为什么; 6=其他 (请说明) 4=there was a problem in the application process; 5=they did not tell; 6=other (specify)
6b. 如果没申请, 为什么没申请? (主要原因) Why didn't you apply?	1=收入差不多够了; 2=不知道怎么办; 3=觉得申请程序太复杂; 4=觉得补助太少; 5=以前申请过但是没有批准; 6=贫困但觉得不好意思; 7=其他 (请说明) 1=income is enough; 2=don't know how to apply; 3=application is too troublesome; 4=grants are insignificant; 5=applied unsuccessfully before; 6=eligible but ashamed; 7=other (specify)
7. 2013 本村是否有农户拿到低保, 但是您觉得不符合条件? Were there any dibao households in this village this year that you don't consider eligible?	1=有; 2=没有) 9 题 1=yes; 2=no → jump to question 9
8. 这个农户为什么能够拿到低保? What's the reason, in your opinion?	1=弄虚作假申请低保; 2=有关系; 3=变低保户以后改善; 4=其他 (请说明) 1=dupery; 2=relationships; 3=sudden income change; 4=other
9. 2013 本村是否有农户确实贫困, 但没有拿到低保? Were there any poor households in this village this year that didn't receive dibao?	1=有; 2=没有) B1 表 1=yes; 2=no → jump to question B1
10. 这个农户为什么没拿到低保? What's the reasons, in your opinion?	1=这个农户不知道低保; 2=这个农户觉得申请手续太复杂; 3=名额用完; 4=这个农户没什么关系; 5=这个农户最近才遇到困难; 6=其他 (请说明) 1=lack of knowledge; 2=application complicated; 3=quota finished; 4=no relationship; 5=sudden income change; 6=other (specify)
11. 拿不到低保与土地有关系吗? Does dibao reception have a connection with land ownership?	1=有关系 (请解释); 2=没关系 1=yes (specify); 2=no

C-2: Village leader questionnaire

问题 [Question]	单位 [Coding/unit]
1. 村委会会把申请低保农户的有关信息向社会公布吗? <i>Does the village council publish the relevant information on dibao applicants?</i>	1=是; 2=否 1=yes; 2=no
2. 本村是否有一个民主评议小组, 对低保户逐一评议? <i>Does this village have an evaluation group discussing the dibao households?</i>	1=有; 2=没有 1=yes; 2=no
3. 本村把农村低保的对象、补助有关信息向社会公布吗? <i>Does the village council publish the relevant information on dibao recipients?</i>	1=是; 2=否 1=yes; 2=no
4. 2013年本村的低保有限制吗? <i>Were there any limitation concerning dibao in 2013?</i>	1=低保资金有限; 2=低保对象数量有限; 3=没有限制, 符合条件都收到补助; 4=其他 (请说明) 1=dibao funds are limited; 2=number of dibao targets is limited 3=no limitation; 4=other (specify)
5. 2013年本村的名额限制是多少? <i>What was this village's dibao quota in 2013?</i>	个 number
6. 2013年, 本村是否有农户因为名额不够没办法申请或者没收到低保? <i>Were there eligible households in 2013, to which you could not give dibao to eligible applicants because of the quota limitation?</i>	1=是; 2=否 1=yes; 2=no
7. 您如何分配本村的低保资金? <i>How do you distribute this village's dibao quota?</i>	1=低保资金就是平均分配给所有贫困户的农户; 2=补助配合低保标准和这农户的收入的区别; 3=按低保档次; 4=县镇政府向各户分配具体的补助; 5=其他 (请说明) 1=evenly across all poor households; 2=according the difference between dibao standard and family income; 3=according to dibao classes; 4=grant is fixed by county of township administration; 5=other (specify)
8. 农户变低保户以后复核吗? (临时低保对象以外) <i>Do you conduct monitoring of dibao recipients? (except temporary recipients)</i>	0=没有复核; 1=如果有变化或怀疑; 2=一年一次; 3=一年两次; 4=根据低保档次; 5=其他 (请说明) 0=never; 1=in case of changes or doubt; 2=once a year; 3=twice a year; 4=according to dibao class; 5=other (specify)
9. 谁复核低保对象的情况? <i>Who conducts the monitoring?</i>	1=县官员; 2=镇官员; 3=村领导; 4=村委会; 5=其他 (请说明) 1=county administrator; 2=township administrator; 3=village leader; 4=village council; 5=other (specify)
10. 本村的低保每月多少钱? <i>What is the dibao grant per month in this village?</i>	元 RMB
11. 低保名额/低保金和土地 (如征占) 有关系吗? <i>Does the dibao quota/dibao reception have any connection to land?</i>	1=是; 2=否 1=yes; 2=no

APPENDIX D:
QUESTIONNAIRES
SEMI-STRUCTURED
INTERVIEWS (2014)

D-1: Household questionnaire (dibao recipients)

<p>1. How is the village situation, how are your neighbors doing?</p>	
<p>Expected content</p> <p><i>Income/Relative welfare</i> <i>Health</i> <i>Infrastructure</i> <i>Agriculture</i></p>	<p>Specific questions</p> <p>What are the main income sources in your household? 本村的农户主要的收入来源有哪些? Are there very poor households in village? What are the reasons for their poverty? 本村有没有特别贫困的家庭? 贫困的原因是什么? Are there any quite rich households in village? What are their sources of income? 有没有特别富裕的家庭? 他们为什么能够这么富? Did your village face any development problems recently (Specify)? 最近本村在提高农民收入方面遇到了特别的困难? 怎么样 的困难? Did you participate in any village meetings last year? (e.g. street repair meeting) 你去年是否参加了村里面组织的开会,你印象中开会的内容包括了哪些问题 和事情 (比如关于修路等项目)? Did you cast your vote the last time the village leader was elected? 上次选举村领导你投票了没有?</p>
<p>2. How is your household doing, how are you doing yourself?</p>	
<p>Expected content</p> <p><i>Income</i> <i>Health</i> <i>Education</i> <i>Marriage</i> <i>Agriculture</i></p>	<p>Specific questions</p> <p>How is your health? 你身体健康最近怎么样? How are your children (school attendance, work, marriage etc.)? 孩子怎么样 (上学, 工作, 结婚等)? How is the situation of agriculture? 种地怎么样? How much land do you work on? 你家有多少田地? How is your income situation this year? 今年的总体收入情况怎么样? What was your main source of income this year? 今年主要收入来源是什么? Are there any specific challenges or problems to your livelihood recently? 最近你的生活有没有什么特别的困难或难处? Are you receiving any government subsidy or assistance at the moment? 你有没有获得任何政府补助或者补贴?</p>
<p>3. I am specifically interested in the rural dibao system. Can you tell me something about how application works here?</p>	
<p>Expected content</p> <p><i>Application process</i> <i>Eligibility</i> <i>Grants</i> <i>Distribution</i> <i>Monitoring</i></p>	<p>Specific questions</p> <p>Have you ever been a dibao household? if yes, when did you receive? 你曾经吃过低保, 什么时候? For how long? (number of months) 吃低保吃了多久? (几个月) How did you obtain your dibao quota? What was the reason you received dibao? 你们家的低保名额是怎么获得的? 为什么能吃低保? Do you have to file a new application every year? 你必须每年重新申请吃低保的吗? Did you apply yourself? 你亲自申请了没有? How much dibao grants did you receive last year in total? 去年一共获得低保补助是多少? What do you buy with the additional money (food, other)? 每个月的低保补助都花到哪儿去了? How much money do you need here to pay one month's living? 本地农民一个人一个月的生活费大概是多少钱? (包括食物, 电费, 水费, 衣服等)</p>

D-1: (continued)

<p>4. Can you tell me something about the distribution of dibao in this village?</p>	
<p>Expected content</p> <p><i>Public announcement of dibao households</i> <i>Democratic consultation</i> <i>Distribution</i> <i>Monitoring</i></p>	<p>Specific questions</p> <p>Do you know who got dibao last year in this village? 你知道去年本村有几个人吃了低保? (一个人多?)</p> <p>How do you know? 你是怎么知道的?</p> <p>Have you seen the notices on dibao on the village's publication board? 你看见过本村关于低保的公示栏?</p> <p>Have you heard that dibao issues are discussed in the village meetings? (i.e. democratic discussion) 你是否听说过村里就低保问题进行开会讨论?</p> <p>Did you ever get any monitoring visits by township or county officials? 乡镇干部是否来你家了解过家庭收入情况?</p> <p>When and how often did they visit the last time you got dibao? 最近吃低保那一年上面政府来你们家就家庭困难情况调查了解过几次?</p> <p>What did the monitors ask about? 来调查走访的干部问你哪些内容?</p> <p>What kind of documents did they ask for? 他们就你的低保进行走访调查要求你们家提供了哪些证明材料了?</p>
<p>5. Are you satisfied with the dibao system? Is it a useful policy to you?</p>	
<p>Expected content</p> <p><i>Efficiency of policy</i> <i>Mistargeting</i> <i>Improvement</i></p>	<p>Specific questions</p> <p>Should higher government increase additional money to dibao or rather strengthen the pension insurance scheme? 你觉得上级政府更应该增加低保的资金投入还是增加养老金的资金投入?</p> <p>Should higher government increase additional money to dibao or rather strengthen the health insurance scheme? 你觉得上级政府更应该增加低保的资金投入还是增加医疗保险的资金投入?</p> <p>Should higher government increase additional money to dibao or rather invest it into village infrastructure (streets, schools...)? 你觉得上级政府更应该增加低保的收入还是增加修路或道路交通, 盖学校等项目投入?</p> <p>Was there something about the dibao standards, the evaluation process, the level of benefits or their distribution, which you were not quite satisfied with? What specifically? 关于低保的认定标准, 认定程序, 补助资金, 分配等方面你有没有什么不太满意吗? 具体意见?</p> <p>What is your opinion on the distribution of dibao funds in this village? 你关于本村的低保资金的分配有哪些想法?</p>
<p>6. Personal attitude</p>	
<p>Expected content</p> <p><i>Attitude</i></p>	<p>Specific questions</p> <p>How do you agree with the following statement: "The government should take more responsibility to ensure that everyone is provided for?" 你是否同意下列的说法: 政府应该保尽保, 尽量照顾到每个需要得到帮助的农户?</p> <p>How do you agree with the following statement: Most people would try to take advantage of you if they got a chance? 你同意下列的说法: 有机会的话, 大部分的人会利用你来获取一些好处?</p> <p>Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people? 一般说, 你觉得可以相信别人, 还是应该提防别人?</p>

D-2: Household questionnaire (non-recipients)

Expected content	Specific questions
<p>1. How is the village doing, how your neighbors?</p> <p><i>Income</i> <i>Relative welfare</i> <i>Health</i> <i>Infrastructure</i> <i>Agriculture</i></p>	<p>What are the main income sources in your household? 本村的农户主要的收入来源有哪些? Are there very poor households in village? What are the reasons for their poverty? 本村有没有特别贫困的家庭? 贫困的原因是什么? Are there any quite rich households in village? What are their sources of income? 有没有特别富裕的家庭? 他们为什么能够这么富? Did your village face any development problems recently? (specify) 最近本村在提高农民收入方面遇到了是否遇到了特别的困难? 怎么样的困难? Did you participate in any village meetings last year? (e.g. street repair meeting) 你去年是否参加了村里面组织的开会,你印象中开会的内容包括了哪些问题和事情?(比如关于修路等项目) Did you cast your vote the last time the village leader was elected? 上次选举村领导你投票了没有?</p>
<p>2. How is your household doing, how are you doing yourself?</p> <p><i>Income</i> <i>Health</i> <i>Education</i> <i>Marriage</i> <i>Agriculture</i></p>	<p>How is your health? 你身体最近怎么样? How are your children (education, work, marriage)? 孩子怎么样(上学, 工作, 结婚等)? How is the situation of agriculture, how much land do you work on? 种地的收入怎么样, 田地是多少? How is your income situation at the moment? 今年的总收入情况怎么样? Where does your income come from, mainly? 今年主要收入来源是什么? Are there any specific challenges or problems to your livelihood recently? 最近你的生活有没有什么特别的困难或难处 Are you receiving any government subsidy or assistance at the moment? 你有没有获得任何政府补助, 补贴?</p>
<p>3. I am specifically interested in the dibao system. Can you tell me something about the system works here? How is the process on village level in contrast to the official regulation?</p> <p><i>Application process</i> <i>Eligibility</i> <i>Grants</i> <i>Distribution</i></p>	<p>Do you know the requirements for receiving dibao? 你知道低保的条件是什么? Have you ever been a dibao household? 你曾经吃过低保, 什么时候? How much money do you need here to pay for one month's living? 当地一个月的生活费大概是多少? (包括食物, 电费, 水费, 衣服等) Why didn't you apply for dibao? 你为什么没申请低保? Why did you not get dibao the last time you applied? 你为什么上次申请没成果了? Who is deciding on dibao distribution? 谁来决定哪些农户可以获得低保? How much money do dibao recipients get on average, in your opinion? 你觉得一个月的低保补助是多少?</p>

D-2: (continued)

<p>4. Can you tell me something about the distribution of dibao in this village?</p> <p><i>Public announcement</i> <i>Dem. consultation</i> <i>Monitoring</i></p>	<p>Do you know who got dibao last year in this village? 你知道去年本村有几个人吃了低保? (一个人多?) How do you know? 你是怎么知道的?</p> <p>Have you seen this village's publication board on dibao? 你看见过本村关于低保的公示栏?</p> <p>Have you heard about democratic consultation? 你是否听说过村里面就低保问题进行开会讨论?</p>
<p>5. Do you believe the dibao program can decrease poverty?</p> <p><i>Satisfaction</i> <i>Efficiency</i> <i>Mistargeting</i> <i>Improvement</i></p>	<p>Should higher government increase additional money to dibao or rather strengthen the pension insurance scheme? (explain advantages and disadvantages) 你觉得上级政府更应该增加低保的资金投入还是增加养老金投入? (对象可能不太一样, 低保也是为了年轻人遇到了困难, 养老保险是为了所有退休的人, 不管富于还是贫困)</p> <p>Should higher government increase additional money to dibao or rather strengthen the health insurance scheme? (explain advantages and disadvantages) 你觉得上级政府应该增加低保的资金投入还是增加医疗保险的资金投入? (对象可能不太一样, 低保是为了所有贫困人家, 医疗保险就是为了生病的人, 不管贫困还是富于)</p> <p>Should higher government increase additional money to dibao or rather invest it into village infrastructure (streets, schools...)? (explain advantages and disadvantages) 你觉得上级政府应该增加低保的收入还是增加修改道路交通, 盖学校等项目投入? (低保就是为了贫困人, 修路, 修学校是为了所有的村民)</p> <p>Was there something about the assessment standards, the evaluation process, the dibao benefits or distribution, which you were not quite satisfied with? What specifically? 关于低保的认定标准, 认定程序, 补助资金, 分配等方面你有没有什么不太满意吗? 具体意见?</p> <p>What is your opinion on the distribution of dibao funds in this village? 你关于本村的低保资金的分配有哪些想法?</p>
<p>6. Personal attitude</p> <p><i>Attitude</i></p>	<p>How do you agree with the following statement: "The government should take more responsibility to ensure that everyone is provided for"? 你是否同意下列的魔法政府应该报低保, 尽量照顾到每个需要得到帮助的农户?</p> <p>Do you think most people would try to take advantage of you if they got a chance? 你同意下列的魔法: 有机会的话, 大部分的人会利用你来获取一些好处?</p> <p>Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people? 一般说, 你觉得可以相信别人, 还是应该提防别人?</p>

D-3: Village leader questionnaire

Expected content	Specific questions
<p>1. Extent of poverty in this village</p> <p>Headcount</p> <p>Depth</p> <p>Reasons of poverty</p>	<p>How many people under the rural poverty line in this village? 本村有多少人在贫困线之下?</p> <p>Main source of income in this village? 你们村集体主要有哪些收入来源? 村民的主要收入来源有哪些?</p> <p>General income situation in this village? 你们村的经济状况在本乡镇处于什么水平?</p> <p>How many poor households are there in this village? 本村比较贫困户有多少?</p> <p>How many dibao recipients last year? 去年的低保户有多少?</p>
<p>2. Could you explain to me how rural dibao is implemented in this village? How is the process from application to distribution?</p> <p>Application</p> <p>Income measurement</p> <p>Decision</p> <p>Distribution</p>	<p>How often/when can households apply? (e.g. once a year)? 农民每隔多长时间申请一次? 每次申请的时间是否在相对固定时间段?</p> <p>Can a household receiving dynamic dibao apply a second year? 村民能否因为家庭困难而申请临时性的低保?</p> <p>Exclusion criteria (apart from income)? 有没有设定标准来优先排除某些农户没有资格纳入低保?</p> <p>Who is testing the eligibility of applicants during the application process? 你们审查认定农户低保的工作流程是怎样的? 审查的内容有哪些?</p> <p>Is there any training concerning the work of identifying dibao households? 有没有开展有关低保户认定工作的培训?</p> <p>Are the recipients attributed to certain classes with fixed transfer rates? 有没有根据贫困程度设定不同的低保档次? 不同的低保档次的补助标准是多少?</p> <p>What are the criteria for these classes? 不同档次的低保认定标准是什么?</p> <p>How are households getting the money (picking it up, village leader distributing, transfer on account)? 农户如何领取低保补助?</p> <p>What is the role of the stationed cadre in this process? 包村或蹲点负责村级事务的乡镇干部在低保政策实施过程中具体做了哪些辅助工作?</p> <p>What is your role as village leader in this process? 在低保政策实施过程当中, 村领导具体负责做了哪些工作?</p>
<p>3. What are challenges in implementing such a complex system?</p> <p>Dupery</p> <p>Exclusion</p> <p>Income measurement</p> <p>Nepotism?</p> <p>Identifying poor</p> <p>Monitoring</p>	<p>In other regions we heard that fraud on dibao is quite a problem, how is the situation here? 你觉得在本县范围内是否存在有骗取低保补助的现象?</p> <p>Can you cover all poor in this area? 是否做到了低保户应保尽保? Why is that not possible? 没有实现的话, 原因是什么?</p> <p>Are there any problems in identifying dibao recipients? 是否存在在低保户对象认定方面存在困难?</p> <p>Do you conduct any monitoring of identified recipients? What is the frequency of monitoring? 是否对于已经认定的低保对象进行复查? 多久复查一次?</p> <p>Are there democratic evaluations being held, are the results published? 低保户的认定是否经过了民主评议? 是否进行了公示?</p> <p>Are there democratic evaluations being held, are the results published? 低保户的内容是什么? (申请的对象, 对象的情况, 低保对象, 低保补助) Content of publication? (applicants, conditions of applicants, recipients, transfer sum) 公示的内容是什么? (申请的对象, 对象的情况, 低保对象, 低保补助) Where is the publication board? 公示栏在哪儿?</p> <p>What if there are more poor households than quotas? 如果家庭贫困的农户数量超过上级政府分配的名额, 你们是怎么处理的?</p> <p>How do you identify households with economic difficulties? 你是怎么识别认定经济困难户的?</p> <p>Is a dibao household monitored once his application was approved? 低保对象一旦确定以后是否对低保户进行跟踪调查?</p>
<p>4. What could be done to improve these aspects you have just spoken about?</p> <p>Program design</p>	<p>Are there any aspects in process/dibao standard/grant amount/increase of quotas that should be adjusted? 低保申请程序、低保标准、低保补助金额是否有所调整?</p>

D-4: Township government questionnaire

Expected content	Specific questions
<p>1. Could you tell me something about the extent and nature of poverty in this region?</p> <p><i>Headcount</i> <i>Depth</i> <i>Reasons of poverty</i></p>	<p>Is this Township an official poverty region? 这个县是否是一个贫困县? How many people under the rural poverty line in this township? 这个乡有多少人在贫困线之下? Are there any region-specific reasons development barriers or factors? 制约这个地区经济发展的障碍或因素有哪些? Any villages where poverty is more serious than in other regions? Why is that? 有没有哪些村的人均收入低于国家贫困线? 为什么?</p>
<p>2. Could you explain to me how rural dibao is implemented in this region? How is the process from application to distribution?</p> <p><i>Application</i> <i>Income measurement</i> <i>Decision</i> <i>Distribution</i></p>	<p>How often/when can households apply? Are there specific application phases? 农民每隔多长时间申请一次? 每次申请的时间是否在相对固定时间段? Exclusion criteria (apart from income)? 有没有设定标准来优先排除某些农户没有资格纳入低保? Can you explain the schedule of dibao targeting? How does household examination work? 你们审查认定农户低保的工作流程是怎样的? 审查的内容有哪些? Is there any training about how to identify dibao recipients? 有没有开展有关低保户认定工作的培训? Has the township office any option to adapt the dibao standard to local conditions? 乡镇能不能根据本乡镇实际情况适当调整低保标准? What was the percentage of dibao households in this region? Are there big differences? 通常情况下低保农户所占的比例是多少? 各个村低保农户所占的比 例有没有差别? Is there an electronic filing system for dibao administration? 你们有没有关于低保政策实施的电脑管理系统? Are there different classes of dibao? What are the respective transfer rates? 有没有根据贫困程度设定不同的低保档次? 不同的低保档次的补助标准是多少? Who decides about average transfers in these classes? (township/county level) 在设定不同的低保档次补助标准方面谁有决定权? What are the criteria for these classes? 不同档次的低保认定标准是什么? How are households getting the money (picking it up, village leader distributing, transfer on account)? 农户如何领取低保补助? What is the role of the stationed cadre in this process? 包村或蹲点负责村级事务的乡镇干部在低保政策实施过程中具体做了哪些辅助工作? What is the role of the village leader in this process? 在低保政策实施过程当中, 村领导具体负责做了哪些工作?</p>
<p>3. Could you tell me something about the coverage of rural dibao in your region?</p> <p><i>Dibao recipients</i> <i>Amount and structure of government funds</i></p>	<p>How much were the total dibao funds of this township last year? 去年本地发放的低保补助总资金是多少? How much were the administrative costs? 低保的管理成本是多少? How big is the dibao quota that your township got assigned by county government last year? 去年本乡的低保名额是多少? Does your township government have to contribute any own funds? 乡镇政府的是否有相应的低保匹配资金吗? What is the process of the distributing the quotas for dibao to the villages (process: meeting with village leaders, only the MCA involved etc.)? 你能不能介绍一下如何分配低保名额到各个村? Based on which aspects do you distribute the funds to the villages (according to average income, population, other...)? 你怎么把低保资金分配向农村(按人均收入, 人口, 其他的)? Were the quotas sufficient last year? 去年的名额是否够用?</p>

D-4: (continued)

<p>4. What are challenges in implementing such a complex system?</p> <p><i>Dupery</i> <i>Exclusion</i> <i>Measurement of income</i> <i>Nepotism?</i> <i>Identifying poor</i> <i>Monitoring</i></p>	<p>In other regions we heard that fraud on dibao is quite a problem, how is the situation here? 你觉得在本县范围内是否存在有骗取低保补助的现象? Can you cover all poor in this area? 是否做到了低保户应保尽保? If that was not possible, why was this the case? 没有实现的话, 原因是什么? Are there any problems in identifying dibao recipients? 是否存在低保户对象认定方面存在困难? Do you conduct any monitoring of identified recipients? What is the frequency of monitoring? 是否对于已经认定的低保对象进行复查? 多久复查一次? Are there democratic evaluations being held, are the results publications? 低保户的认定是否经过了民主评议? 是否进行了公示? Content of publication? (applicants, conditions of applicants, recipients, transfer sum) 公示的内容是什么? (申请的对象, 对象的情况, 低保对象, 低保补助) Where is the publication board? 公示栏在那儿? Were there ever any complaints about the system or the targeting? 有没有人因为对低保补助政策向你或者上级部门反映不满意意见?</p>
<p>5. What could be done to improve these aspects you have just spoken about?</p> <p><i>Change in process, dibao standard, grant amount, funding for this Bureau</i></p>	<p>5. What could be done to improve these aspects you have just spoken about?</p> <p>Would any changes in the process/dibao standard/poverty line/grant amount/funding for this bureau/more dibao funding in general help? 乡镇政府在低保标准, 贫困线, 补助标准等方面是否可以进行适当调整? Is the number of officers responsible for dibao and the related workload sufficient in this office? 负责低保工作的办公人员及相关的日常管理行政经费是否充足?</p>

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